

Attachment 3

Containing Exhibits 24-37

County of Santa Clara

Comment to DHS Docket No. USCIS–2010–0012

Inadmissibility on Public Charge Grounds

Exhibit 24

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

Trends in Noncitizens' and Citizens' Use of Public Benefits Following Welfare Reform

1994-97

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Background

With the enactment of the 1996 welfare reform act,⁽¹⁾ Congress imposed broad new restrictions on legal immigrants' access to public benefits, set new time limits on refugees' eligibility for many federal benefits, and introduced new bars on the access of "unqualified immigrants" to services.⁽²⁾ But perhaps more important than these changes in eligibility are welfare reform's chilling effects which may discourage immigrants from using health, nutrition, or other types of benefits, despite the fact that many remain eligible. These effects originate, among other things, in confusion on the part of immigrants and providers about who is eligible for benefits and in fears relating to the application of the public charge doctrine.⁽³⁾

An earlier study by the Urban Institute found evidence of such chilling effects in Los Angeles County.⁽⁴⁾ In that study, approved applications of legal noncitizen families for Medi-Cal and Temporary Assistance for Needy Families (TANF) fell 71 percent between January 1996 and January 1998, while there was no decline among citizens. The drop occurred even though there was no change in legal immigrants' eligibility for these programs in California and denial rates in the county remained steady during the period examined.

In this brief report we use the Census Bureau's Current Population Survey (CPS) to document *national* trends in immigrants' use of public benefits in the period following welfare reform. Specifically, we examine changes in participation between 1994 and 1997 reflected by the March CPS.⁽⁵⁾ During 1994 changes in welfare rules were just beginning to be broadly debated. By the end of 1997, welfare reform had been in place for a year and a half, although full implementation was not complete. In addition, the CPS for both years provides comparable data on benefit use for the entire nation.

The current analysis builds on methods developed by the Urban Institute over the past decade that permit us to distinguish refugees, naturalized citizens, and temporary immigrants from other legally present immigrants.⁽⁶⁾ Such distinctions are important for two reasons. First, conventional comparisons between the benefit use rates of natives and the foreign born mask substantial variation in rates and trends among substantively different segments of the foreign-born population. Second, following welfare reform, citizenship status has become an increasingly important determinant of eligibility for public benefits.

We should emphasize that most legal immigrants and refugees remained eligible for welfare and Medicaid benefits throughout the period examined (1994 through 1997).⁽⁷⁾ The same cannot be said for federal food stamps, however: many legal immigrants' eligibility was supposed to end as of September 1997, while new noncitizen applicants became ineligible in October 1996. Finally, while most immigrants arriving after welfare reform's enactment are barred from federal means-tested public benefits for at least five years,⁽⁸⁾ these "future" immigrants represented a small share of the noncitizen population at the time of the March 1998 CPS.

Principal Findings

- When viewed against the backdrop of overall declines in welfare receipt for all households, ***use of public benefits among noncitizen households (9) fell more sharply (35 percent) between 1994 and 1997 than among citizen households (14 percent)***. These patterns hold for welfare (defined here as TANF, SSI, and General

Assistance), food stamps, and Medicaid.

- **Refugees experienced declines (33 percent) that were at least as steep as those within the noncitizen population**--despite the protections for refugees incorporated into welfare reform and the fact that few refugees had lost their eligibility for benefits by March 1998.⁽¹⁰⁾
- **For low-income populations (i.e., with incomes below 200 percent of poverty), program usage also fell faster for noncitizen than citizen households.**
- **Welfare use in noncitizen households with children also fell faster (36 percent) than in households with children where all adults are citizens (23 percent).**
- One result of these trends is that **noncitizens accounted for a disproportionately large share of the overall decline in welfare caseloads that occurred between 1994 and 1997.** While 23 percent of the drop in welfare caseloads can be ascribed to noncitizens, they represented only 9 percent of households receiving welfare in 1994.
- **Welfare use among elderly immigrants and naturalized citizens did not appear to change between 1994 and 1997.**
- **When welfare use among all households is examined, noncitizen participation levels were higher than citizens' in both 1994 and 1997,** despite rapid declines in noncitizen use rates. But when we look at **poor households (i.e., with incomes under 200 percent of poverty), noncitizens' participation rates in 1994 were no different from those of citizens; by 1997, however, levels had declined so that noncitizens had lower participation rates than citizens** (14.5 versus 17.9 percent). When we examine **poor households with children, noncitizen rates were lower for both 1994 and 1997--falling to almost half of the level of citizens in 1997** (14.0 versus 25.8 percent).
- **Neither naturalization nor rising incomes accounted for a significant share of noncitizens' exits from public benefit use.**

In the following section we examine patterns of benefit use in three different ways. First, we examine benefit use by *household*, disaggregating by all households, by households with incomes below 200 percent of poverty, and by those containing children. We then present findings for *individuals*, distinguishing use patterns for working-age adults (age 18 to 64) and the elderly (age 65 and over). We conclude the section by disaggregating trends by *legal status*, most notably program participation by refugees and naturalized citizens. In each instance, differing units of observation reveal different relative levels and trends in benefit use by citizens and noncitizens.

The analysis examines the use of welfare, food stamps, and Medicaid. While complete results are set out in the [figures and tables](#) included in this report, selected outcomes are highlighted in the narrative below. Since trends in food stamp and Medicaid use generally parallel those for welfare, we usually report results for only welfare.

DETAILED FINDINGS

A. Household-Level Analyses

Relative benefit use rates for citizens and noncitizens differ greatly depending on whether we focus on all households, poor households, or households with children. The reasons for these differences are straightforward. Poor households are far more likely to be eligible for and use benefits, and noncitizens are more likely to be poor. Fifty-four percent of noncitizen households have incomes below 200 percent of poverty, compared with 31 percent of citizen households ([Table 4](#)).

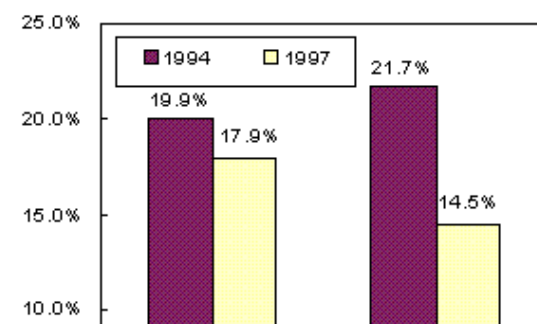
While controls for poverty have occasionally been taken into account in discussions of immigrant welfare use,⁽¹¹⁾ differences between households with and without children have been less frequently invoked. Yet such differences are significant because households with children are considerably more likely to use benefits, and immigrant households are more likely to contain children. Of households headed by noncitizens of working age, 55 percent include children, compared with 35 percent of comparable citizen households. As we report below, when we separately control for poverty and the presence of children, differences in program use rates between citizens and noncitizens diminish and, in some instances, disappear altogether. When we control for both poverty and the presence of children, noncitizen use of benefits is consistently lower than that of citizens, both before and after welfare reform.

All Households. Welfare receipt by noncitizen households fell much faster (35 percent) than citizens' receipt (14 percent) between 1994 and 1997. However, despite these steeper declines, noncitizen use of welfare remained higher than citizens' in 1997--9.0 versus 6.7 percent ([Table 1](#) and [Figure 1](#)). By 1997, far more immigrants had lost their eligibility for food stamps than welfare. But our data show that noncitizens' participation in each program declined at roughly the same rate, with a marginally faster decline in welfare than in food stamps (35 percent versus 30 percent).

Households below 200 Percent of Poverty. The picture of higher welfare use by noncitizens shifts significantly when we control for poverty. By 1997, noncitizens with incomes below 200 percent of poverty had use rates that were significantly *lower* than citizens' rates--14.5 versus 17.9 percent ([Chart A](#)). Here again, noncitizen participation rates dropped faster than citizens' between 1994 and 1997 (33 versus 10 percent). For both food stamps and Medicaid, noncitizens' use also fell faster than citizens' during the same period, so by 1997 relative participation levels within each program were effectively the same for both groups ([Table 1](#) and [Figure 1](#)).

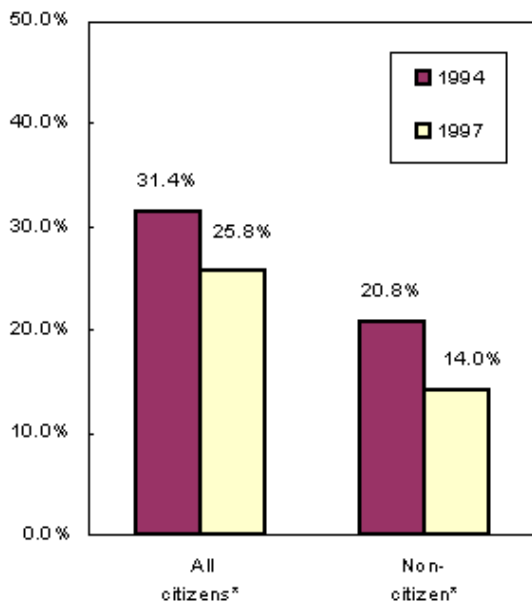
Households with Children. When we control for the presence of children, we also see declines to the point where noncitizen welfare usage rates in households with children are not significantly different from citizens' rates (8.9 versus 9.6 percent, in [Table 3](#)). Rates for both declined rapidly between 1994 and 1997--35 percent for noncitizens and 23 percent for

Chart A. Percent of Households Receiving Welfare:
Income below 200 Percent of Poverty



citizens--but the difference in the rate of decline is not statistically significant (Table 3 and Figure 3).

Chart B. Percent of Households With Children Receiving Welfare: Income Below 200 Percent of Poverty



In households with noncitizen adults, many of the children are citizens, in most cases because they were born in the United States. In fact, there is at least one citizen child in 85 percent of noncitizen households with children. These mixed-status households are of substantial demographic importance in the United States, as about one in 10 American children lives in a household where one or more of the parents is a noncitizen and one or more of the children is a citizen.⁽¹²⁾

Households with Children and with Incomes below 200 Percent of Poverty. Given the large share of noncitizen households that are poor and contain children, one approach to assessing relative benefits is to control for both poverty and the presence of children. When we do so, we see much lower use among noncitizen households, both before and after welfare reform. With the rapid declines that occurred for both groups, noncitizen welfare use in 1997 is about half the rate for citizens--14.0 versus 25.8 percent. (See Chart B, Table 3, and Figure 3.)

B. Individual-Level Analyses

Individual-level analyses of welfare use among citizens and noncitizens produce results that differ from results of aggregate household-level analyses because of patterns of welfare reporting in the CPS, differences in welfare use, and structural differences in the populations.⁽¹³⁾ "Welfare use" for an individual in the CPS is defined as having income from TANF,

General Assistance, or SSI; a household "uses welfare" if anyone in the household has welfare income. One issue in reconciling individual and household use rates, as well as comparing survey with administrative data, is the fact that income data in the CPS is collected only for persons age 15 and over. Thus, if a child is receiving public assistance income, the income will either be ascribed to the parent or missed.

This income-based measure of welfare use means that most households report welfare participation as though there were only a single welfare recipient. For example, a single mother with two children receiving TANF income is only counted as one welfare unit or recipient, not three. The individual-level analysis differs from the household approach by ascribing use to only the reported welfare recipient, not to other household members who are not reported as receiving welfare. One reason it is important to examine individual level benefit use is that noncitizen households are significantly larger than citizen households.

Because there are almost twice as many adults (197 million, see detailed table B) as households (103 million, see Detailed Table A), welfare use rates for individuals should be lower than for households. In addition, noncitizen households are larger than citizen households and are more likely to contain children. Thus, we would expect larger differences in usage rates between households and individuals among noncitizens; the data support this.

Our analysis below focuses on two important subpopulations: working-age adults, age 18 to 64, and the elderly, age 65 and over.

Working-Age Adults. Working-age noncitizens' use of welfare fell roughly three times faster than citizens' between 1994 and 1997--41 versus 15 percent (Table 2). A similar pattern is evident for Medicaid.⁽¹⁴⁾ By 1997, there is no statistical difference between citizen and noncitizen participation rates for welfare (4.0 versus 3.3 percent) or Medicaid (6.7 versus 7.2 percent). See Table 2 and Figure 2.

Elderly Immigrants. In sharp contrast to most of the other components of the analysis reported here, we find no statistically significant decline in either welfare or Medicaid use on the part of elderly noncitizens. In fact, we find no significant change in welfare receipt among the elderly overall, regardless of citizenship status (Table 2 and Figure 2).

Elderly noncitizen use of welfare and other benefits is much higher than is the case for citizens. In 1997, only 3.7 percent of elderly citizens used welfare, compared to 19.0 percent of noncitizens (Table 2 and Figure 2). Higher use of welfare (primarily SSI) and Medicaid among elderly noncitizens can be attributed to the fact that many have not worked in the United States long enough to qualify for Social Security or Medicare. Moreover, the absence of a decline in usage since 1994 may be explained, at least in part, by the restoration of SSI benefits to pre-enactment immigrants.

One result that emerges from the analysis is an *apparent* rise between 1994 and 1997 in the number and share of *naturalized* elderly receiving welfare benefits--from 99,000 or 5.9 percent, to 167,000 or 9.0 percent (Detailed Table B). During the same period, there is a commensurate decline in the number of *noncitizen* elderly receiving benefits (from 213,000 to 163,000). One hypothesis is that the decline in noncitizen participation for this subpopulation may be attributable in part to naturalization. However, these numerical changes are not statistically significant, so the results cannot be treated as definitive.⁽¹⁵⁾

C. Benefit Use by Immigrant Status

Historically, immigration status has been a strong predictor of immigrant use of public benefits. This is partly because

the foreign-born population consists of groups with varied eligibility for benefits. While naturalized citizens and refugees are eligible for benefits on the same terms as native-born citizens, legal permanent residents' use of benefits has been conditioned by deeming and public charge restrictions; temporary immigrants and the undocumented are largely barred from services. Further, the socioeconomic characteristics of the groups differ substantially, resulting in different needs.⁽¹⁶⁾ The importance of disaggregating the foreign-born population by status can be seen in the analysis of household use rates (Table 1).

Foreign-Born Population. Between 1994 and 1997, welfare use in households headed by all foreign-born persons fell by 21 percent or 2.5 percentage points (from 11.7 percent to 9.2 percent). However, this general trend masks very different levels and trends among the various immigrant groups.

Refugees. Refugees, who have historically had the highest levels of public benefit use among the foreign born, account for 8 percent of immigrant-headed households, but for 21 percent of immigrants' welfare use (Detailed Table A). Their use rate remained high--24.5 percent in 1997--but even this level represented a decline of 8.8 percentage points from the pre-reform level of 33 percent (Chart C).

Naturalized Citizens. Naturalized citizens, who have historically had the lowest levels of public benefit use among legally present immigrant populations, represent the other extreme. While naturalized citizens make up 41 percent of immigrant households, they account for only 31 percent of immigrants' welfare use. Their use of benefits was virtually identical to that of native citizens and did not change significantly between 1994 and 1997.

Noncitizens. The residual foreign-born subpopulation, noncitizens, had a large decline of 4.9 percentage points to 9.0 percent in 1997, but their use rate remained somewhat higher than that of citizens.

General Observations

Chilling Effects versus Eligibility Changes

- Because comparatively few legal immigrants were ineligible for public benefits as of December 1997, it appears that the steeper declines in noncitizens' than citizens' use of welfare, food stamps, and Medicaid owe more to the "chilling effect" of welfare reform and other policy changes than they do to actual eligibility changes. In addition, the fact that welfare use among noncitizens dropped as steeply as food stamp use (where new restrictions extended far more broadly) suggests that eligibility changes in one program may chill noncitizens' use of other programs. Over time, eligibility changes will become more important as most immigrants admitted after August 22, 1996, will be ineligible for most means-tested public benefits for at least five years after their entry to the country.

Noncitizens Do Not Appear to Be Naturalizing to Retain Benefits

- The consistently low share and number of naturalized immigrants who receive benefits indicate that few immigrants are becoming citizens in order to retain benefits. If most immigrant benefit recipients were naturalizing to retain benefits, the number of naturalized citizens receiving benefits would have grown substantially more than it did. ⁽¹⁷⁾

Rising Incomes Do Not Explain Lower Program Participation Rates among Noncitizens

- One possible explanation for the faster declines in program participation among noncitizens than among citizens could be that incomes are rising faster for noncitizens. To address this issue, we use demographic standardization techniques (described below) to partition changes in program participation over the 1994-97 period into the share attributable to changes in income and the share attributable to changes in income-specific participation rates. Our analysis finds that most of the change is *not* due to rising incomes. Only 6 percent of the decline in welfare use among noncitizens, versus 30 percent of the decline among citizens, can be explained by rising incomes; similar results hold for food stamps and Medicaid (Table 4).

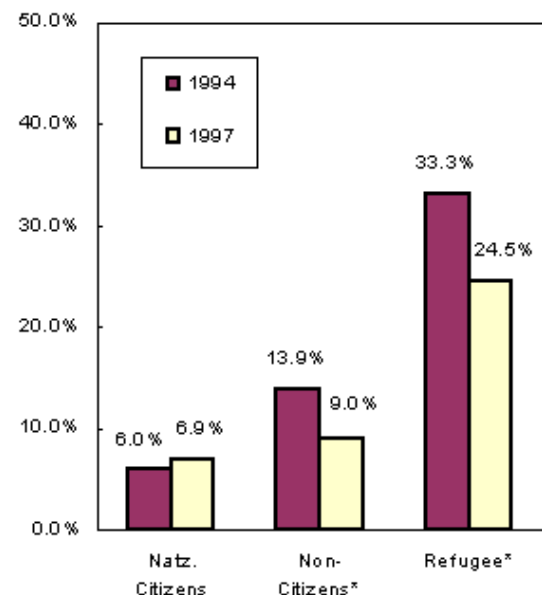
Reduced Use of Health and Other Benefits among Populations Not the Focus of Welfare Reform

- While the apparent decline in welfare use among noncitizens of working age may be an intended and positive policy outcome, these data also reveal sharp declines in the use of health, nutritional, and cash assistance within populations that are thought to be more vulnerable and were not a focus of welfare reform. These vulnerable populations include refugees and the citizen children of noncitizens. As the data presented above indicate, there have been sharp declines in benefits use for both populations. At the same time, health uninsurance rates for noncitizens (46 percent) remain much higher than among citizens (16 percent).⁽¹⁸⁾ Persistently high uninsurance rates, coupled with the decline in program participation documented here, have broad implications for targeting outreach efforts to expand enrollment in programs like California's Healthy Families initiative and for other state efforts to expand enrollment in Medicaid and the Child Health Insurance Program.⁽¹⁹⁾

Noncitizens Do Not Demonstrate a Greater Propensity to Receive Benefits

- The data indicate that higher benefit use rates on the part of noncitizen versus citizen households are due to the fact that immigrant households are poorer and more likely to contain children, not because noncitizens have a greater

Chart C. Percent of Immigrant Households Receiving Welfare, by Immigration Status



disposition towards receiving benefits. In fact, among poor households with children, immigrants have lower use rates for welfare, food stamps, and Medicaid both before and after welfare reform.

Notes on the Analysis

Legal Status Distinction. This analysis employs imputation techniques developed by the Urban Institute that make it possible to assign some legal statuses (notably refugee and nonimmigrant status) to foreign-born persons included in the Current Population Survey. These techniques enable us to disaggregate changes in immigrants' benefit use by legal status--despite the fact that the CPS only distinguishes citizens from noncitizens. Disaggregating the immigrant population in this manner is important because usage patterns vary considerably by legal status and, following welfare reform, legal status has become an increasingly important determinant of immigrants' eligibility for public benefits. Specifically, the rules below have applied following welfare reform:

- *Naturalized citizens* remain eligible for public benefits on the same terms as native-born citizens.
- *Noncitizen refugees* retain eligibility for means-tested federal benefits, including food stamps, Child Health Insurance Program (CHIP), Medicaid, TANF, and SSI, for five to seven years following their entry into the United States.
- *Legal immigrants entering before August 22, 1996*, retain eligibility for several major means-tested public benefit programs: Medicaid, TANF, and the Child Health Insurance Program. Each is jointly funded by the states and the federal government. Welfare reform gave the states the option to extend TANF and Medicaid to pre-enactment legal immigrants and virtually all states have chosen to do so.

Pre-enactment legal immigrants' eligibility for *federal* food stamps remains limited to the elderly, disabled, and children. California and other states have extended state-funded food stamps to working-age immigrants left out by the federal program.

While pre-enactment immigrants' eligibility for SSI was largely eliminated by the 1996 legislation, it has been restored to disabled or elderly immigrants who were receiving SSI when welfare reform passed or who subsequently become disabled.

- *Legal immigrants entering after August 22, 1996*, have been barred from receiving federal means-tested benefits, including TANF, Medicaid, CHIP, SSI, and food stamps, for at least five years after entry and effectively until they naturalize.
- *Undocumented immigrants* remain generally ineligible for most major public benefits.

Partitioning the Change in Welfare Use. The percentage of a group (e.g., citizens, noncitizens, natives) using welfare can be thought of as the product of two sets of percentages or rates: the income distribution of the group and the percentage of each specific income category who receive welfare. To cite a specific, but simplified, example using data from [tables 1](#) and 4, 6.7 percent of citizen households received welfare in 1997. We arrived at this percentage from the following calculation: 17.9 percent of citizen households with incomes below 200 percent of poverty received welfare, and these households represent 30.7 percent of citizen households; of the 69.3 percent of citizen households with incomes above 200 percent of poverty, only 1.7 percent receive welfare. Thus, $6.7 = 17.9 \times 0.307 + 1.7 \times 0.693$. For noncitizen households, 9.0 percent received welfare in 1997 ($9.0 = 14.5 \times 0.541 + 2.6 \times 0.459$). In other words, poor noncitizen households are less likely to use welfare than poor citizen households (14.5 percent versus 17.9 percent), but poor households are much more common among noncitizens than citizens (54.1 percent versus 30.7 percent), so overall noncitizen households are more likely to use welfare than are citizen households.

We can think of two extreme explanations for the change in welfare use between 1994 and 1997. At one extreme, the 35 percent reduction in use for noncitizens (from 13.9 to 9.0 percent) could occur because the rate of welfare use at every income level for noncitizens fell by 35 percent; were this to occur, the overall rate decline would be explained completely by changes in usage rates. On the other hand, the rate of welfare use could stay constant for each income group of noncitizens, but incomes could rise so that more of the noncitizen population fell into higher income groups which use less welfare. In this case, the overall rate decline would be due entirely to changes in income level. In practice, neither extreme occurs; a combination of the two accounts for the change.

A demographic technique called standardization permits us to partition the overall change in welfare use into a portion attributable to changes in usage rates and a portion attributable to changes in income distribution. The standardization requires four sets of percentages, two for each year: the percentage of the population falling into each income category in 1994 and 1997 (i.e., the income distributions), and the percentage of each income category receiving welfare in 1994 and 1997 (i.e., the use rates). For the partition results shown in [Table 4](#), we use eight household income categories: incomes less than 50% of poverty, 50-74%, 75-99%, 100-124%, 125-149%, 150-174%, 175-199%, and 200% of poverty or more.

The 1997 income distribution multiplied by the 1994 detailed use rates gives the percentage that would have received welfare in 1997 if use rates had not changed (i.e., only incomes had shifted). Subtracting this hypothetical rate from the actual 1997 overall use rate gives a measure of change attributable to income changes. We can calculate another measure of the income effect by subtracting the actual 1994 overall use rate with the hypothetical rate computed with the 1994 income distribution and the 1997 detailed use rates. The average of these two estimates is the amount of change between 1994 and 1997 attributable to changes in income. Any remaining change is the share attributable to changes in welfare usage patterns.⁽²⁰⁾

Coverage of welfare use in the CPS. Finally, we should note that the CPS data on benefits use employed in this analysis were reported by the Census Bureau. Both welfare use and welfare income are known to be underreported, possibly substantially, in the CPS. We do not correct for either type of underreporting in our analysis. Further, the data have not been adjusted to take into account program eligibility rules or the misreporting of public benefit use on the part of immigrants and native citizens. Our uncorrected comparisons assume, in effect, that reporting patterns did not change between the 1995 and 1998 CPS. Despite these limitations, CPS data are conventionally used to characterize trends in

benefit use. ⁽²¹⁾ There is no reason to believe that the trends documented in this report are biased or otherwise invalid.

Tables and Figures

Table 1. Percent of Households Receiving Welfare, Food Stamps, and Medicaid, by Citizenship of Household Head and by Poverty Status: 1994 and 1997

Population	All Households				Households Below 200% of Poverty			
	Percent with Any Participation				Percent with Any Participation			
	1994	1997	'94-'97 Change		1994	1997	'94-'97 Change	
Amt.			Pct.	Amt.			Pct.	
Welfare (AFDC/TANF, SSI, GA)								
Total	8.3%	6.9%	-1.4% *	-16%	20.3%	17.9%	-2.5% *	-12%
Citizen	7.8%	6.7%	-1.1% *	-15%	19.9%	17.9%	-2.0% *	-10%
Native	7.9%	6.6%	-1.2% *	-16%	20.2%	18.1%	-2.1% *	-10%
Naturalized**	6.0%	6.9%	0.9%	--	13.5%	14.9%	1.4%	--
Noncitizen**	13.9%	9.0%	-4.9% *	-35%	21.7%	14.5%	-7.2% *	-33%
Refugee	33.3%	24.5%	-8.8% *	-27%	49.1%	40.2%	-9.0%	--
Foreign-Born	11.7%	9.2%	-2.6% *	-22%	21.2%	16.6%	-4.6% *	-22%
Noncitizen --Citizen Difference	6.1% *	2.4% *	-3.7% *	(x)	1.8%	-3.5% *	-5.2% *	(x)
Food Stamps								
Total	9.0%	7.1%	-1.9% *	-22%	24.3%	20.6%	-3.7% *	-15%
Citizen	8.5%	6.8%	-1.8% *	-21%	23.8%	20.5%	-3.3% *	-14%
Native	8.7%	6.8%	-1.8% *	-21%	24.2%	20.8%	-3.4% *	-14%
Naturalized**	5.5%	5.4%	0.0%	--	15.0%	14.8%	-0.2%	--
Noncitizen**	15.4%	10.8%	-4.6% *	-30%	26.3%	19.1%	-7.1% *	-27%
Refugee	35.3%	22.1%	-13.2% *	-37%	52.0%	41.9%	-10.1%	--
Foreign-Born	12.5%	9.3%	-3.2% *	-26%	24.7%	19.4%	-5.2% *	-21%
Noncitizen --Citizen Difference	6.8% *	4.0% *	-2.8% *	(x)	2.4% *	-1.4%	-3.8% *	(x)
Medicaid								
Total	14.3%	13.2%	-1.1% *	-8%	31.3%	30.5%	-0.8%	--
Citizen	13.5%	12.6%	-0.9% *	-7%	30.3%	30.0%	-0.3%	--
Native	13.5%	12.5%	-1.0% *	-7%	30.5%	30.1%	-0.4%	--
Naturalized**	11.9%	13.6%	1.7%	--	23.8%	28.3%	4.5%	--
Noncitizen**	26.5%	20.8%	-5.7% *	-22%	39.8%	32.0%	-7.8% *	-19%
Refugee	42.5%	35.8%	-6.7%	--	58.8%	58.5%	-0.3%	--
Foreign-Born	21.3%	18.7%	-2.6% *	-12%	36.1%	32.7%	-3.4% *	-9%
Noncitizen --Citizen Difference	13.0% *	8.2% *	-4.8% *	(x)	9.5% *	2.0%	-7.5% *	(x)

* Significant at p < 0.10.

** Excludes refugees and non-immigrants. See text for definition.

-- Change not significant.

(x) Not applicable.

Source: Urban Institute tabulations from March Current Population Surveys of 1995 and 1998, with imputations for refugees and non-immigrants. See [Detailed Table A](#) for population data.

Table 2. Percent of Individuals Participating in Welfare and Medicaid, by Citizenship and Age: 1994 and 1997

Some noncitizen** adults	1.4%	-0.7%	-2.1%	(x)	-10.7% *	-11.8% *	-1.1%	(x)
All noncitizen** children	-3.9% *	-5.4% *	-1.4%	(x)	-19.9% *	-19.9% *	0.0%	(x)
Some citizen children	2.4% *	0.0%	-2.3%	(x)	-8.9% *	-10.5% *	-1.6%	(x)
Food Stamps								
Households with children	16.0%	12.3%	-3.6% *	-23%	39.6%	32.9%	-6.7% *	-17%
All citizen adults	15.3%	12.1%	-3.2% *	-21%	40.9%	34.7%	-6.2% *	-15%
Some noncitizen** adults	19.2%	13.6%	-5.6% *	-29%	30.7%	23.3%	-7.4% *	-24%
All noncitizen** children	9.8%	7.4%	-2.4%	--	14.0%	12.0%	-2.0%	--
Some citizen children	20.8%	14.5%	-6.4% *	-31%	33.9%	25.1%	-8.8% *	-26%
Difference from "all citizen" households								
Some noncitizen** adults	3.9% *	1.5%	-2.4%	(x)	-10.2% *	-11.4% *	-1.2%	(x)
All noncitizen** children	-5.5% *	-4.6% *	0.9%	(x)	-26.9% *	-22.7% *	4.2%	(x)
Some citizen children	5.5% *	2.4% *	-3.1% *	(x)	-7.0% *	-9.6% *	-2.6%	(x)
Medicaid								
Households with children	17.5%	14.4%	-3.1% *	-18%	38.4%	33.5%	-4.8% *	-13%
All citizen adults	16.8%	14.0%	-2.7% *	-16%	39.2%	35.1%	-4.1% *	-11%
Some noncitizen** adults	21.1%	16.2%	-4.8% *	-23%	31.1%	24.9%	-6.1% *	-20%
All noncitizen** children	15.7%	10.7%	-4.9%	--	21.8%	15.4%	-6.4%	--
Some citizen children	22.0%	17.0%	-5.0% *	-23%	32.8%	26.5%	-6.4% *	-19%
Difference from "all citizen" households								
Some noncitizen** adults	4.3% *	2.2% *	-2.1%	(x)	-8.2% *	-10.2% *	-2.0%	(x)
All noncitizen** children	-1.1%	-3.3%	-2.2%	(x)	-17.5% *	-19.7% *	-2.2%	(x)
Some citizen children	5.2% *	3.0% *	-2.2%	(x)	-6.4% *	-8.7% *	-2.2%	(x)

* Significant at p < 0.10. ** Excludes refugees and non-immigrants. See text for definition. -- Change not significant. (x) Not applicable. Source: Urban Institute tabulations from March Current Population Surveys of 1995 and 1998, with imputations for refugees and non-immigrants. Universe is households headed by persons 18-64 with children under age 18. See [Detailed Table C](#) for population data.

Table 4. Partition of 1994–97 Change in Household Participation in Welfare, Food Stamps, and Medicaid into Portions Due to Changes in Poverty Prevalence and Welfare Use Rates, by Citizenship								
Population	Percent of Households				Partition of '94-'97 Change in Participation***			
	Below Specified Level of Poverty		Participating in Program		Total Change	Due to Use Rates	Due to Poverty Rates	Pct. of Change from Usage
	1994	1997	1994	1997				
Welfare (AFDC/TANF, SSI, GA)								
100 Percent of Poverty								
Total	13.9%	12.7%	8.3%	6.9%	-1.4% *	-0.9%	-0.5%	66%
Citizen	13.0%	11.8%	7.8%	6.7%	-1.1% *	-0.7%	-0.5%	60%
Native	13.0%	11.7%	7.9%	6.6%	-1.2% *	-0.7%	-0.5%	59%
Naturalized**	11.0%	12.5%	6.0%	6.9%	0.9%	--	--	--
Noncitizen**	27.3%	25.9%	13.9%	9.0%	-4.9% *	-4.6%	-0.3%	94%
Refugee	31.9%	25.4%	33.3%	24.5%	-8.8% *	-5.3%	-3.5%	60%

Foreign-Born	21.4%	20.4%	11.7%	9.2%	-2.6% *	-2.2%	-0.3%	87%
Food Stamps								
125 Percent of Poverty								
Total	18.9%	17.5%	9.0%	7.1%	-1.9% *	-1.4%	-0.6%	70%
Citizen	17.7%	16.4%	8.5%	6.8%	-1.8% *	-1.2%	-0.6%	68%
Native	17.8%	16.3%	8.7%	6.8%	-1.8% *	-1.2%	-0.6%	66%
Naturalized**	16.2%	17.6%	5.5%	5.4%	0.0%	--	--	--
Noncitizen**	35.7%	34.8%	15.4%	10.8%	-4.6% *	-4.1%	-0.5%	89%
Refugee	41.1%	32.7%	35.3%	22.1%	-13.2% *	-8.4%	-4.8%	64%
Foreign-Born	28.5%	27.5%	12.5%	9.3%	-3.2% *	-2.7%	-0.5%	85%
Medicaid								
200 Percent of Poverty								
Total	34.3%	32.1%	14.3%	13.2%	-1.1% *	-0.5%	-0.7%	42%
Citizen	32.8%	30.7%	13.5%	12.6%	-0.9% *	-0.3%	-0.6%	28%
Native	32.9%	30.5%	13.5%	12.5%	-1.0% *	-0.3%	-0.7%	31%
Naturalized**	32.2%	33.6%	11.9%	13.6%	1.7%	--	--	--
Noncitizen**	55.9%	54.1%	26.5%	20.8%	-5.7% *	-5.1%	-0.6%	90%
Refugee	60.6%	49.9%	42.5%	35.8%	-6.7%	--	--	--
Foreign-Born	47.1%	45.0%	21.3%	18.7%	-2.6% *	-2.1%	-0.6%	78%

* Significant at p < 0.10.

** Excludes refugees and non-immigrants. See text for definition.

*** Partition uses poverty-specific welfare rates for intervals of 25 percentage points for 50–200 percent of poverty (i.e., <50%, 50–74%, 75–99%, 100–124%, 125–149%, 150–174%, 175–199%, 200% or more). See text for details.

-- Total change not significant.

Source: Urban Institute tabulations from March Current Population Surveys of 1995 and 1998, with imputations for refugees and non-immigrants.

Detailed Table A. Household Receipt of Welfare, Food Stamps, and Medicaid, by Citizenship of Household Head and by Poverty Status: 1994 and 1997

(Populations in thousands)

Population	All Households						Households Below 200 Percent of Poverty					
	Program Participation in Household?						Program Participation in Household?					
	1994			1997			1994			1997		
	Yes	Total	% Yes	Yes	Total	% Yes	Yes	Total	% Yes	Yes	Total	% Yes
Welfare (AFDC/TANF, SSI, GA)												
Total	8,188	99,106	8.3%	7,091	102,584	6.9%	6,903	33,960	20.3%	5,877	32,888	17.9%
Citizen	7,257	92,989	7.8%	6,400	96,169	6.7%	6,084	30,525	19.9%	5,287	29,493	17.9%
Native	7,031	89,248	7.9%	6,096	91,748	6.6%	5,920	29,320	20.2%	5,065	28,007	18.1%
Naturalized*	225	3,741	6.0%	304	4,421	6.9%	163	1,205	13.5%	222	1,486	14.9%
Noncitizen*	723	5,209	13.9%	474	5,246	9.0%	632	2,910	21.7%	411	2,841	14.5%
Refugee	202	606	33.3%	213	872	24.5%	180	367	49.1%	175	435	40.2%
Foreign-Born	1,157	9,858	11.7%	995	10,837	9.2%	983	4,640	21.2%	811	4,881	16.6%
Food Stamps												
Total	8,949	99,106	9.0%	7,263	102,584	7.1%	8,240	33,960	24.3%	6,773	32,888	20.6%
Citizen	7,924	92,989	8.5%	6,500	96,169	6.8%	7,275	30,525	23.8%	6,044	29,493	20.5%
Native	7,720	89,248	8.7%	6,261	91,748	6.8%	7,095	29,320	24.2%	5,824	28,007	20.8%
Naturalized*	204	3,741	5.5%	239	4,421	5.4%	180	1,205	15.0%	220	1,486	14.8%

Noncitizen*	801	5,209	15.4%	567	5,246	10.8%	764	2,910	26.3%	543	2,841	19.1%
Refugee	214	606	35.3%	193	872	22.1%	191	367	52.0%	182	435	41.9%
Foreign-Born	1,228	9,858	12.5%	1,003	10,837	9.3%	1,145	4,640	24.7%	949	4,881	19.4%
Medicaid												
Total	14,189	99,106	14.3%	13,523	102,584	13.2%	10,630	33,960	31.3%	10,027	32,888	30.5%
Citizen	12,533	92,989	13.5%	12,102	96,169	12.6%	9,243	30,525	30.3%	8,852	29,493	30.0%
Native	12,088	89,248	13.5%	11,499	91,748	12.5%	8,956	29,320	30.5%	8,432	28,007	30.1%
Naturalized*	445	3,741	11.9%	603	4,421	13.6%	287	1,205	23.8%	420	1,486	28.3%
Noncitizen*	1,379	5,209	26.5%	1,090	5,246	20.8%	1,158	2,910	39.8%	910	2,841	32.0%
Refugee	258	606	42.5%	312	872	35.8%	216	367	58.8%	254	435	58.5%
Foreign-Born	2,101	9,858	21.3%	2,024	10,837	18.7%	1,674	4,640	36.1%	1,595	4,881	32.7%

Detailed Table B. Individual Welfare and Medicaid Participation, by Citizenship and Age: 1994 and 1997

Population	Individual Participation in Program, <i>Age 18–64</i>						Individual Participation in Program, <i>Age 65 and Over</i>					
	1994			1997			1994			1997		
	Yes	Total	% Yes	Yes	Total	% Yes	Yes	Total	% Yes	Yes	Total	% Yes
Welfare (AFDC/TANF, SSI, GA)												
Total	7,856	160,217	4.9%	6,632	165,329	4.0%	1,549	31,350	4.9%	1,419	32,082	4.4%
Citizen	6,939	146,620	4.7%	6,047	151,104	4.0%	1,277	30,207	4.2%	1,160	31,037	3.7%
Native	6,808	141,270	4.8%	5,908	144,513	4.1%	1,178	28,536	4.1%	993	29,188	3.4%
Naturalized*	132	5,350	2.5%	139	6,591	2.1%	99	1,671	5.9%	167	1,849	9.0%
Noncitizen*	651	11,713	5.6%	385	11,772	3.3%	213	1,019	20.9%	163	857	19.0%
Refugee	257	1,340	19.2%	197	1,890	10.4%	60	124	48.1%	96	188	51.0%
Foreign-Born	1,048	18,947	5.5%	725	20,817	3.5%	372	2,814	13.2%	426	2,894	14.7%
Medicaid												
Total	12,698	160,217	7.9%	11,372	165,329	6.9%	2,919	31,350	9.3%	2,901	32,082	9.0%
Citizen	11,011	146,620	7.5%	10,165	151,104	6.7%	2,551	30,207	8.4%	2,528	31,037	8.1%
Native	10,773	141,270	7.6%	9,886	144,513	6.8%	2,364	28,536	8.3%	2,251	29,188	7.7%
Naturalized*	238	5,350	4.4%	279	6,591	4.2%	186	1,671	11.1%	276	1,849	14.9%
Noncitizen*	1,211	11,713	10.3%	853	11,772	7.2%	287	1,019	28.2%	242	857	28.2%
Refugee	443	1,340	33.1%	336	1,890	17.8%	81	124	64.8%	131	188	69.7%
Foreign-Born	1,925	18,947	10.2%	1,486	20,817	7.1%	554	2,814	19.7%	650	2,894	22.4%

Detailed Table C. Welfare, Food Stamp, and Medicaid Participation of Households with Children, by Citizenship of Adults and Children and by Poverty Status: 1994 and 1997 (Populations in thousands)

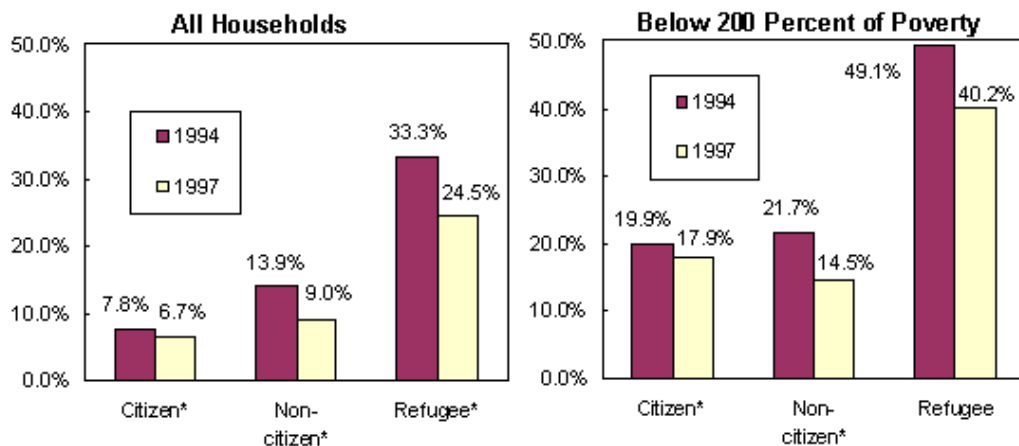
Households with children	4,713	36,784	12.8%	3,600	37,286	9.7%	4,160	13,818	30.1%	3,156	13,138	24.0%
All citizen adults	4,031	32,498	12.4%	3,142	32,747	9.6%	3,550	11,294	31.4%	2,755	10,670	25.8%
One or more noncitizen* adults	524	3,785	13.8%	346	3,880	8.9%	466	2,243	20.8%	304	2,165	14.0%
All noncitizen* children	47	560	8.5%	21	497	4.2%	42	359	11.6%	18	297	5.9%
One or more citizen children	476	3,225	14.8%	325	3,383	9.6%	424	1,885	22.5%	287	1,868	15.3%
Food Stamps												
Households with children	5,872	36,784	16.0%	4,601	37,286	12.3%	5,477	13,818	39.6%	4,323	13,138	32.9%
All citizen adults	4,969	32,498	15.3%	3,949	32,747	12.1%	4,622	11,294	40.9%	3,702	10,670	34.7%
One or more noncitizen* adults	726	3,785	19.2%	526	3,880	13.6%	689	2,243	30.7%	504	2,165	23.3%
All noncitizen* children	55	560	9.8%	37	497	7.4%	50	359	14.0%	36	297	12.0%
One or more citizen children	671	3,225	20.8%	489	3,383	14.5%	639	1,885	33.9%	469	1,868	25.1%
Medicaid												
Households with children	6,447	36,784	17.5%	5,368	37,286	14.4%	5,302	13,818	38.4%	4,405	13,138	33.5%
All citizen adults	5,453	32,498	16.8%	4,596	32,747	14.0%	4,433	11,294	39.2%	3,748	10,670	35.1%
One or more noncitizen* adults	797	3,785	21.1%	630	3,880	16.2%	697	2,243	31.1%	540	2,165	24.9%
All noncitizen* children	88	560	15.7%	53	497	10.7%	78	359	21.8%	46	297	15.4%
One or more citizen children	709	3,225	22.0%	576	3,383	17.0%	619	1,885	32.8%	495	1,868	26.5%

* Excludes refugees and non-immigrants. See text for definition.

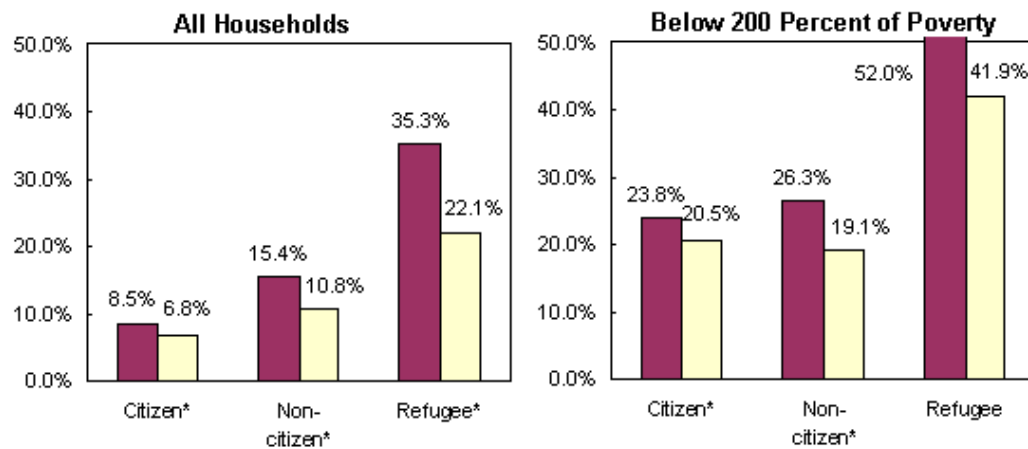
Source: Urban Institute tabulations from March Current Population Surveys of 1995 and 1998, with imputations for refugees and non-immigrants.

Figure 1. Percent of Households Receiving Welfare, Food Stamps, and Medicaid, by Nativity of Household Head and by Poverty Status

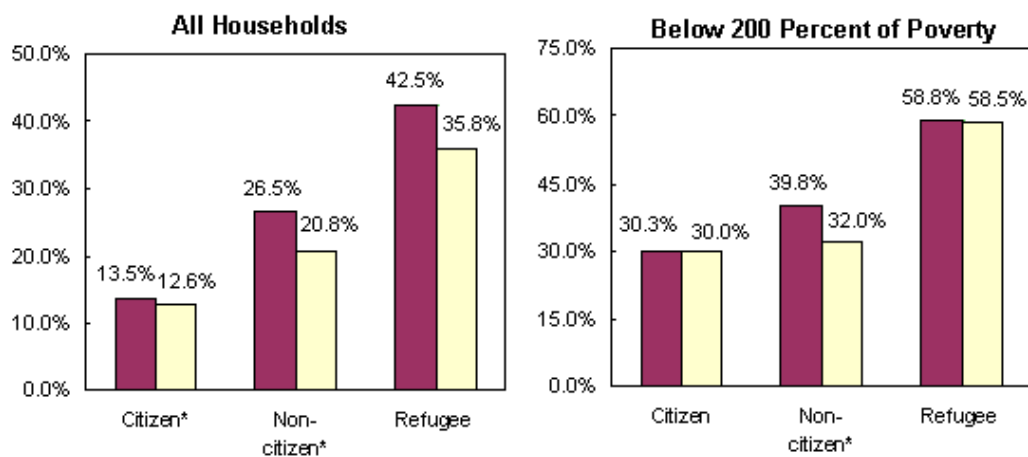
A. Percent of Households Receiving Welfare



B. Percent of Households Receiving Food Stamps



C. Percent of Households Receiving Medicaid



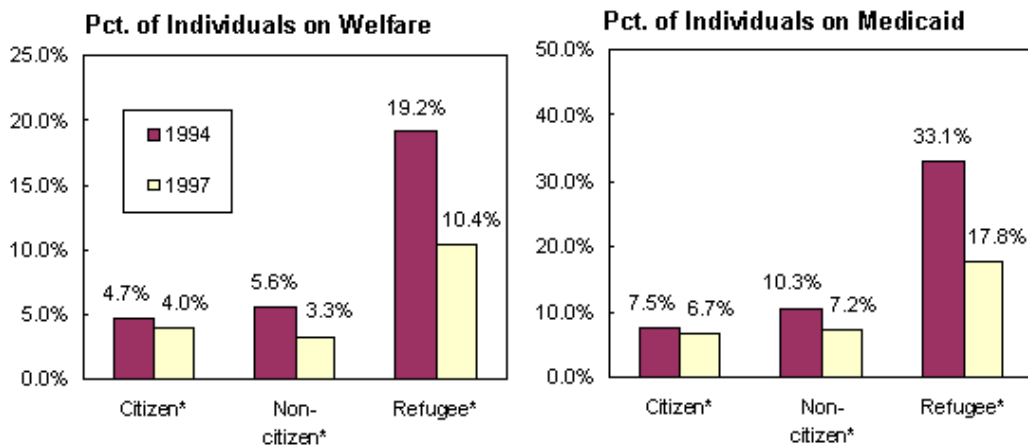
Source: [Table 1](#).

Note: Noncitizen group excludes refugees and non-immigrants. See text.

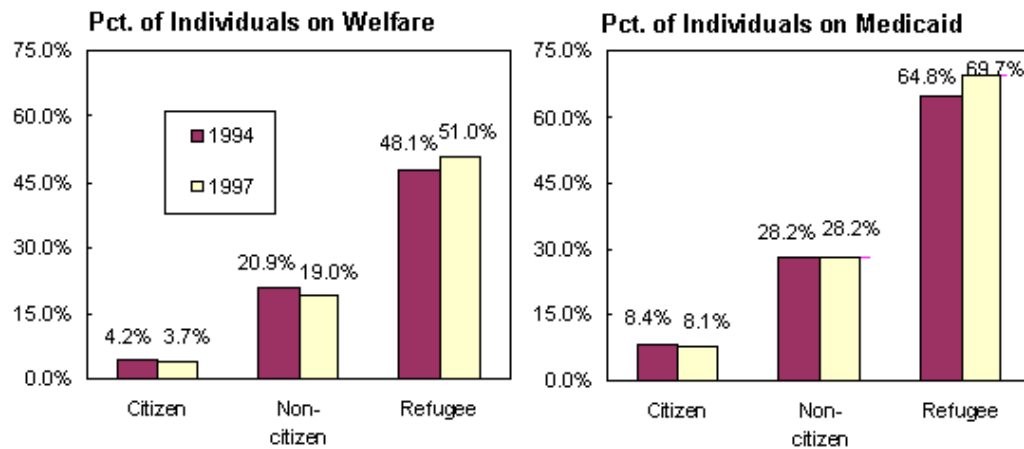
* 1994-97 change is significant at $p < 0.10$.

Figure 2. Percent of Individuals Participating in Welfare and Medicaid, by Age and Citizenship: 1994 and 1997

A. Age 18-64



B. Age 65 and Over



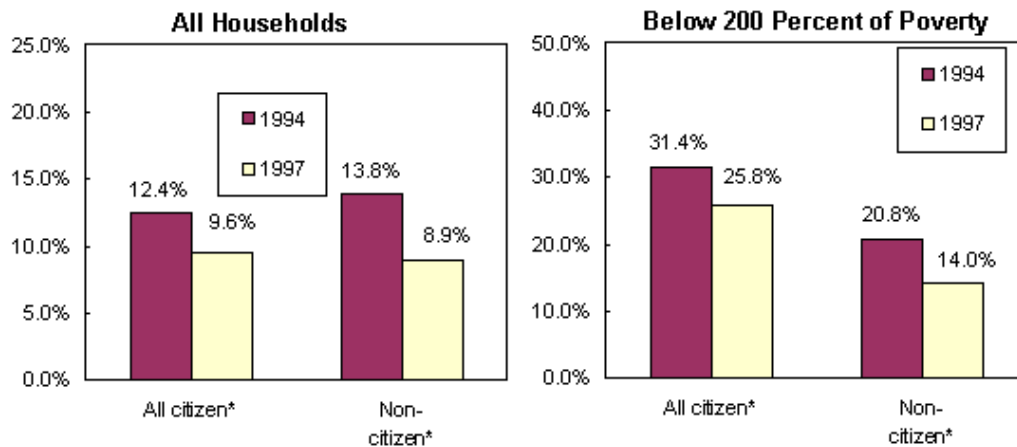
Source: Table 2.

Note: Noncitizen group excludes refugees and non-immigrants. See text.

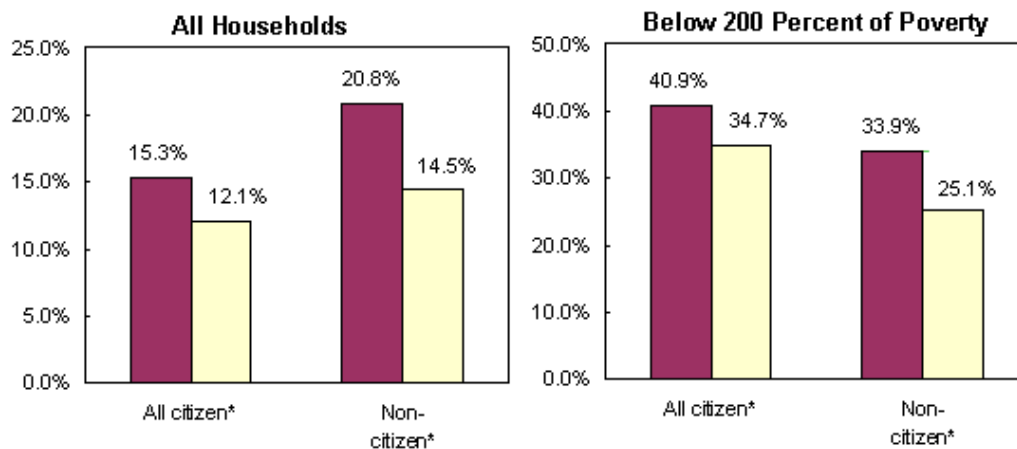
* 1994-97 change is significant at $p < 0.10$.

Figure 3. Percent of Households with Children Receiving Welfare, Food Stamps, and Medicaid, by Citizenship of Adults and Children and by Poverty Status: 1994 and 1997

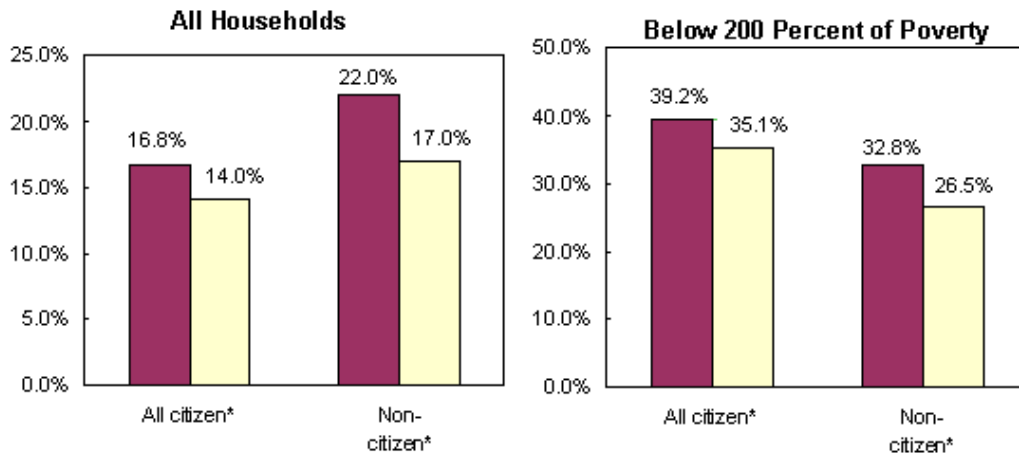
A. Percentage of Households Receiving Welfare



B. Percentage of Households Receiving Food Stamps



C. Percentage of Households on Medicaid



Source: [Table 3](#). Includes only households with children headed by persons 18-64 years old.
 Note: Noncitizen group excludes refugees and non-immigrants. See text for definition of groups.
 * 1994-97 change is significant at $p < 0.10$.

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Notes

1. The Personal Responsibility and Work Opportunity Reconciliation Act, Pub. L. 104-193 (1996).
 2. While most of these unqualified immigrants are undocumented immigrants, many unqualified immigrants are legally present in the United States and have work authorization. See, generally, Michael Fix and Wendy Zimmermann, "The Legacies of Welfare Reform's Immigrant Restrictions," *Interpreter Releases*, November 16, 1998.
 3. "Public charge" is a term used by the Immigration and Naturalization Service (INS) and the State Department to describe someone who is, or is likely to become, dependent on public benefits. Public charge considerations have historically been a factor in the admissibility of aliens (i.e., grant of a green card) and, only rarely, in the deportation of aliens who have been in the United States less than five years.
- In the past several years, public charge has been inappropriately invoked in some instances where noncitizens have attempted to reenter the United States and where immigrants have sought to naturalize. In some cases, noncitizens seeking to adjust status, naturalize, or reenter the country have been asked to repay public benefits. The legality of compelling repayment in these contexts is suspect.
4. See Wendy Zimmermann and Michael Fix, ["Declining Immigrant Applications for Medi-Cal and Welfare Benefits in Los Angeles County,"](#) The Urban Institute, July 1998.
 5. The CPS collects information on program use and income in March for the preceding *calendar* year. Thus, the information collected in the March 1995 CPS pertains to calendar year 1994; and the March 1998 CPS, to calendar year 1997. Throughout this document, we use the CPS data from March 1995 and 1998 for reference years 1994 and 1997. The data from the March 1995 CPS have been reweighted to correct for an error in the official weights (Jeffrey S. Passel and Rebecca Clark, ["Immigrants in New York: Their Legal Status, Incomes, and Taxes,"](#) The Urban Institute, April 1998).

The CPS data used here are not corrected for underreporting of welfare use or welfare income. (See "[Coverage of Welfare Use in the CPS](#).")

6. See Passel and Clark, 1998.

7. Welfare reform gave the states the option of barring legal immigrants in the United States before August 22, 1996, from TANF and Medicaid. However, virtually all states extended benefits to these pre-enactment immigrants. See Fix and Zimmermann, 1998.

8. "Federal means-tested public benefits" have been determined to be Temporary Assistance for Needy Families (TANF), Medicaid, the Child Health Insurance Program (CHIP), Supplemental Security Income (SSI), and food stamps.

9. Unless otherwise noted, the comparisons of citizens and noncitizens exclude refugees and temporary immigrants (i.e. "nonimmigrants" according to immigration law), which are treated separately. *Citizens* include natives, persons born in Puerto Rico and other outlying areas, and immigrants who have acquired citizenship through naturalization. *Noncitizens* include aliens admitted as lawful permanent residents and undocumented immigrants.

10. A refugee is defined legally as a person outside his/her country of nationality who is unable to return because of a well-founded fear of persecution. Because their departure from their home country is involuntary and unplanned and because many suffer physical or mental trauma, refugees have been made eligible for most public benefits from the date of their arrival.

We assign refugee status based on country of birth and period of entry to the United States. For persons entering after 1980, we define a "refugee country" as one where refugees and asylees account for more than 40 percent of total admissions of legal permanent residents, refugees, and asylees during any two-year period. See Passel and Clark, 1998.

11. See, for example, *From Generation to Generation, The Health and Well-Being of Children in Immigrant Families*, National Research Council, Institute of Medicine, National Academy Press, Washington D.C., 1998.

12. See Fix and Zimmermann, 1998.

13. See Jennifer Van Hook, Jennifer E. Glick, and Frank D. Bean, "Public Assistance Receipt Among Immigrants and Natives: How the Unit of Analysis Affects Research Findings," *Demography* 36 (1, February 1999): 111-20.

14. In the CPS, food stamp usage is a household-level variable, so we do not report individual usage patterns.

15. They are, however, consistent with administrative data, specifically the Food Stamp Program Quality Control data for fiscal years 1994 and 1997.

16. See, for example, Passel and Clark, 1998.

17. The data in [Detailed Table B](#) show that the number of naturalized citizens age 18 and over reporting welfare income rose by only 75,000 between 1994 and 1997, while the number of naturalized citizens increased by 1,419,000. During that period, the number of noncitizens reporting welfare use fell by 316,000.

18. P. Fronstin, "Sources of Health Insurance and Characteristics of the Uninsured: An Analysis of the March 1998 Current Population Survey," EBRI Issue Brief 204, December 1998.

19. We should note that declines in Medicaid caseloads affect more populations than just noncitizens and can also be viewed as an unintended effect of welfare reform. See Marilyn R. Ellwood and Leighton Ku, "Welfare and Immigration Reforms: Unintended Side Effects for Medicaid," *Health Affairs* 17 (3, May/June 1998): 137-51.

20. This second difference can be shown to be equivalent algebraically to the average of two separate estimates of change attributable to difference in usage rates. One measure compares the actual 1997 overall use rate with a hypothetical rate computed as the 1997 detailed use rates multiplied by the 1994 income distribution. The other subtracts actual 1994 overall use rate from the hypothetical rate based on the 1994 detailed use rates and the 1997 income distribution. For more information on standardization and partition, see Prithwis Das Gupta, *Standardization and Decomposition of Rates: A User's Manual*, U.S. Bureau of the Census, Current Population Reports, Series P23-186, Washington, D.C.: U.S. Government Printing Office, September 1993.

21. See, for example, Fronstin, 1998. Planned Urban Institute reports that take into account patterns of underreporting and program eligibility rules are expected in the summer of 1999.

Other Publications by the Authors

- [Michael E. Fix](#)
- [Jeffrey S. Passel](#)

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Exhibit 25

County of Santa Clara
Comment to DHS Docket No. USCIS-2010-0012
Inadmissibility on Public Charge Grounds

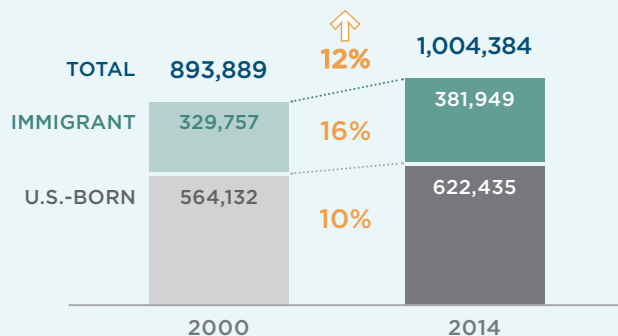
NEW AMERICANS IN SAN JOSE AND SANTA CLARA COUNTY

A SNAPSHOT OF THE DEMOGRAPHIC AND ECONOMIC
CONTRIBUTIONS OF IMMIGRANTS

POPULATION GROWTH

In 2014, the population of **San José** reached more than
one million people
for the first time. The immigrant population also reached
an all-time high — more than
38%
of the population
are immigrants.

In **Santa Clara County**, the overall
population reached close to
two million people
in 2014, with immigrant residents
making up
38%
of the population.



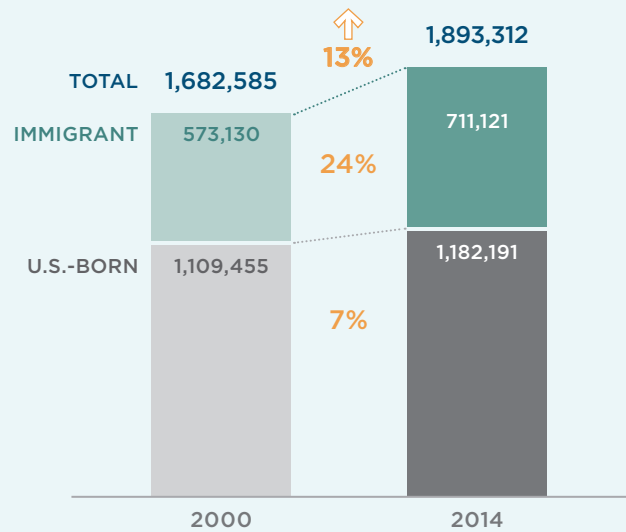
Overall, **San José's** population grew by 12.4% between 2000 and 2014, from almost 894,000 to 1 million. **Immigrants were responsible for 47.2% of this growth.** During this period, the immigrant population of San José grew by 15.8%, from just under 330,000 to almost 382,000. Meanwhile, the U.S.-born population of the city grew less, by 10.3% between 2000 and 2014.

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POPULATION GROWTH, CONTINUED

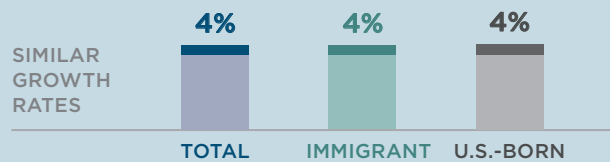
As a result of the increase in immigrants between 2000 and 2014, we estimate that 56,896 U.S.-born people were drawn to Santa Clara County.

Between 2000 and 2014, **Santa Clara County** grew by 12.5%, from 1.68 million to more than 1.89 million people, with the **immigrant population accounting for 65.5% of this growth**. During this period, the immigrant population of Santa Clara County grew by 24.1%, from 573,000 to more than 711,000. Meanwhile, the U.S.-born population grew by only 6.6%, from 1.11 million to 1.18 million between 2000 and 2014.

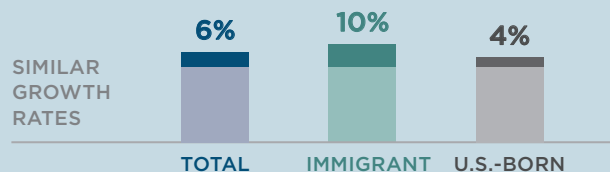


POPULATION GROWTH (2009–2014)

San José grew by 4% between 2009 and 2014, from 965,000 to 1 million, with immigrants responsible for **35.9% of this growth**. The immigrant population increased by 3.9%, from 367,000 to almost 382,000 in 2014. Meanwhile, the U.S.-born population of San José also increased at a similar rate, growing by 4.3%, from almost 597,000 to more than 622,000 in 2014.



Santa Clara County's overall population increased by 6.1%, from 1.78 million in 2009 to 1.89 million in 2014, with immigrants accounting for **62.8% of this growth**.¹ During this period, the immigrant population in Santa Clara County grew by more than 10%, from just under 643,000 in 2009 to more than 711,000 in 2014. This far outpaced growth in the U.S.-born population, which increased by 3.5% between 2009 and 2014.



¹ Population growth includes any new residents from outside of Santa Clara County.

MIGRATION AND COUNTRIES OF ORIGIN

TOP 10 COUNTRIES OF ORIGIN OF IMMIGRANTS IN SANTA CLARA CO.

Mexico	139,831	(19.7% of all immigrants)
India	110,121	(15.5%)
Vietnam	99,691	(14%)
China (incl. Hong Kong)	77,358	(10.9%)
Philippines	59,758	(8.4%)
Taiwan	27,637	(3.9%)
Korea	17,406	(2.4%)
Iran	14,091	(2%)
Japan	11,256	(1.6%)
Russia	10,188	(1.4%)

Of the almost 19,000 immigrant residents of Santa Clara County that moved to the area in the past year, more than **51% came from India** (9,753 residents), **27.8% from China** (incl. Hong Kong and Macau), and **7.8% from Japan**. Canada (7.7%) and Mexico (5%) were also popular countries of origin for recent arrivals.



ECONOMIC CONTRIBUTIONS

In 2014, immigrants in Santa Clara County contributed an estimated

\$77 billion
to the county's economy

through their consumption and tax contributions.

Given their income, we estimate that the immigrant population of Santa Clara County contributed almost

\$3 billion in state and local taxes

in 2014, including property, income, sales, and excise taxes levied by either the State of California or by municipal governments.³ Immigrant households also contributed

\$7 billion in federal taxes,

as well as

\$5 billion
to Social Security and

\$1 billion
to Medicare.

The immigrant population in Santa Clara County wields considerable economic power. In 2014, immigrants held

\$27 billion
in spending power

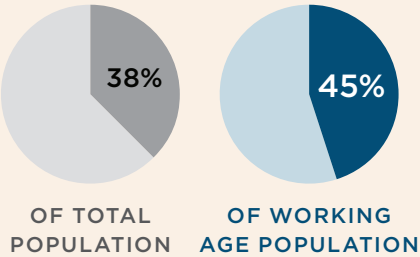
46.4% of the county's total spending power.²

² Based on tax rates from Institute on Taxation and Economic Policy (2015) "Who Pays? A Distributional Analysis of the Tax Systems in All Fifty States."

³ Based on tax rates from Institute on Taxation and Economic Policy (2015) "Who Pays? A Distributional Analysis of the Tax Systems in All Fifty States."

LABOR MARKET

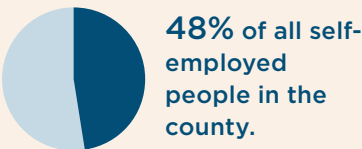
While immigrants make up 37.6% of Santa Clara County's population, they make up an even larger share of the working age population. Immigrants make up 45% of all people ages 25 to 65.



Immigrants also make up a disproportionate share of the labor force in the county:

47%
of all employed people are immigrants.

Immigrants are overrepresented among the self-employed, making up



They are also slightly more likely than the U.S.-born to be entrepreneurs, with 8.9% of all immigrant workers running their own business, compared to 8.5% of all U.S.-born workers.

Immigrants play a critical role in some of California's most important industries that continue to look for talented workers. In the top 5 industries in Santa Clara County by number of workers — Manufacturing, Professional and Technical Services, Healthcare and Social Assistance, Retail, and Arts and Hospitality — immigrants make up significant numbers, if not the majority share, of workers.



In 2014, **unemployment levels** for workers in these key industries were around or lower than California's overall unemployment rate of 7.1%. They were:



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LABOR MARKET, CONTINUED



In **information and computer technology (ICT) related industries**, immigrants make up a majority of all workers, two-thirds (66%) of workers are immigrants.

Unemployment for ICT workers was very low in 2014, with only 3% of ICT workers out of work.

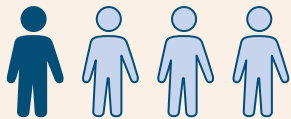
Similarly, immigrants are also more likely to work in several other industries, including...

Professional & Technical Services: 1.44x more likely

Construction: 1.2x more likely

Accommodation & Food Service: 1.2x more likely

Given their distribution in the economy, immigrants in Santa Clara County are **1.68 times more likely to work in manufacturing** than the U.S.-born.



In other words, while nearly **1 in 4 immigrant** workers work in manufacturing...



...less than **1 in 7 U.S.-born** workers do.

Looking specifically at advanced manufacturing industries, we find that immigrants also play a large role in supporting this sector of the economy. Overall, **immigrants made up 60.2% of the advanced manufacturing workforce**. In the 5 largest segments by number of workers, immigrant workers made up:

Computer & Electronics Manufacturing	65.2%
Chemical Product Manufacturing	54.4%
Fabricated Metal Products	51.7%
Medical Equipment	50.6%
Motor Vehicle, Aircraft, and Ship Buildings	36.8%

EDUCATION LEVEL

While Santa Clara is already one of the most well-educated counties in the United States, the county's immigrant population has particularly high rates of educational attainment.

The share of the immigrant population aged 25 and up with **at least a bachelor's degree** is 49.8%. Meanwhile, 46.1% of the U.S.-born population over the age of 25 has at least a bachelor's degree.

46% of the U.S.-born population

50% of immigrants

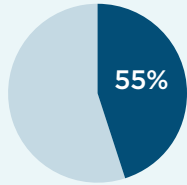
More than 1 in 4 immigrant residents over the age of 25 has an **advanced degree** (26%). This is higher than the share of U.S.-born residents, 18.4%, or less than 1 in 5, who have an advanced degree.

18% of the U.S.-born population

26% of immigrants

ENTREPRENEURSHIP

As of 2014, there were **more than 23,500 immigrant business owners** in San José, representing 54.6% of all entrepreneurs.



**OF SAN JOSE
ENTREPRENEURS
ARE IMMIGRANTS**

Overall, immigrants in San José are slightly more likely to be self-employed than the U.S.-born, with

1 in 10 immigrants (10%)

working for their own business, while

less than 1 in 10 (9%) U.S.-born workers

is self-employed.

Self-employed immigrants in San José have a

combined annual business income over

\$743 million,

a figure that totals more than half of all business income for self-owned businesses in the city.

Santa Clara County is home to 14 Fortune 500 companies as of 2015. Of these firms, 50% were founded either by immigrants or by the children of immigrants.

These companies combined had annual revenues of

\$393 billion

in FY2014.

According to the 2012 Survey of Business Owners, there were 3,033 Hispanic-owned businesses in Santa Clara County.

These businesses employed

24,464 people

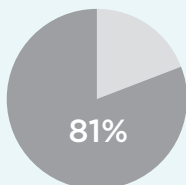
in the county and had combined total revenues of almost **\$4.2 billion**.

Across the county, immigrant entrepreneurs are **particularly represented** in several industries:

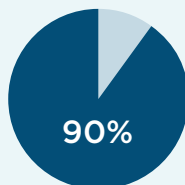
Retail Trade	72.2%
Manufacturing	63.4%
Accommodation & Food Services	60%
Construction	59%
Healthcare & Social Assistance	42.8%

LANGUAGE

As of 2014, 81.1% of immigrant children--those under the age of 18--in Santa Clara County lived in households that spoke a language other than English at home. Meanwhile, for immigrant adults over the age of 18, 90.2% lived in households that spoke a language other than English at home.

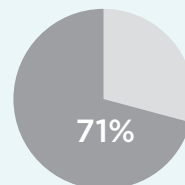


**OF IMMIGRANT
CHILDREN SPEAK A
LANGUAGE OTHER
THAN ENGLISH AT
HOME**

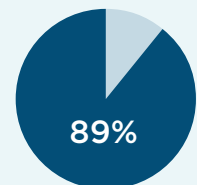


**OF IMMIGRANT
ADULTS SPEAK A
LANGUAGE OTHER
THAN ENGLISH AT
HOME**

When looking just at naturalized citizens, we find that 89.2% of adults and 71% of children under the age of 18 speak a language other than English at home.



**OF CHILDREN
SPEAK A LANGUAGE
OTHER THAN
ENGLISH AT HOME**



**OF ADULTS
SPEAK A LANGUAGE
OTHER THAN
ENGLISH AT HOME**

LONG-TERM BENEFITS OF INTERNATIONAL STUDENTS REMAINING IN THE COUNTY AFTER GRADUATION

In Fall 2014, there were

12,375 students

at Santa Clara County area universities who were **international students**.

These international students supported an **estimated 4,702 jobs** in Santa Clara County and **contributed over**

\$425 million

to the local economy by virtue of their spending and tuition payments.⁴

If Santa Clara County retains

one-half,

or 6,188, of its international students after graduation with bachelor's degrees or higher,

3,183 local jobs

will be created within six years.⁵

Such retention will also boost the metro area's

GDP per capita by \$552

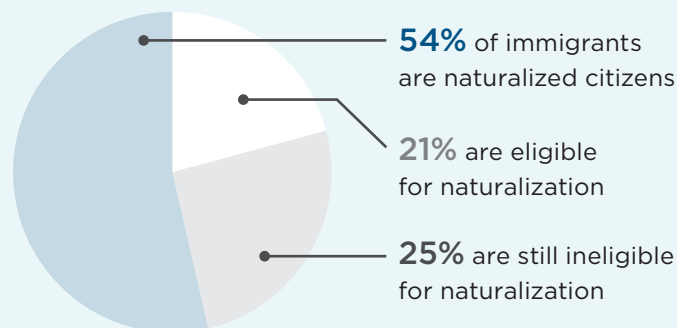
within the next 30 years,⁶ and increase the population by

23,863 people

within the next 50 years.⁷

NATURALIZATION/CITIZENSHIP

As of 2014, of the over 711,000 immigrant residents of Santa Clara County, 53.8% were naturalized citizens. Meanwhile, a further 147,000 immigrant residents, or 20.8% of the entire immigrant population, are estimated to be eligible for naturalization. More than 180,000, or about 25.4% of the immigrant population in 2014 was still ineligible for naturalization.



⁴ Estimates come from NAFSA's factsheet series, "Benefits from International Students." <https://istart.iu.edu/nafsa/reports/state.cfm?state=CA&year=2014>

⁵ Curtis Simon, "Human Capital and Metropolitan Employment Growth," Journal of Urban Economics 43, (1998).

⁶ Rita Ray, "Effect of Education on Gross Domestic Product: A Case Study from US 'Mid-West'," International Review of Business Research Papers, Vol. 10-1. (March, 2014).

⁷ Edward L. Glaeser, Giacomo A. M. Ponzetto, and Kristina Tobio, "Cities, Skills and Regional Change," Regional Studies, Vol. 48-1, (January, 2014).



Exhibit 26

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds



Immigrants in California

California has long been home to the largest number of immigrants in the United States, having accepted early settlers along their seaports as far back as the 17th century. Immigrants now account for over one quarter of the state's population and comprise nearly 34 percent of the entire labor force. As workers, business owners, taxpayers, and neighbors, immigrants are an integral part of California's diverse and thriving communities and make extensive contributions that benefit all.

More than a quarter of California residents are immigrants, while nearly one in four residents is a native-born U.S. citizen with at least one immigrant parent.

- In 2015, 10.7 million immigrants (foreign-born individuals) comprised 27.3 percent of the population.¹
- California was home to 5.3 million women, 4.9 million men, and 449,878 children who were immigrants.²
- The top countries of origin for immigrants were Mexico (40 percent of immigrants), the Philippines (8 percent), China (5.9 percent), Vietnam (4.8 percent), and India (4.5 percent).³
- In 2016, 9.3 million people in California (23.8 percent of the state's population) were native-born Americans who had at least [one immigrant parent](#).⁴

Nearly half of all immigrants in California are naturalized U.S. citizens.

- 5.3 million immigrants (49.7 percent) had naturalized as of 2015,⁵ and 2.2 million immigrants were eligible to become [naturalized U.S. citizens](#) in 2015.⁶
- Two-thirds (66.7 percent) of immigrants reported speaking English "well" or "very well."⁷

Immigrants in California are distributed across the educational spectrum.

- More than a quarter of adult immigrants had a college degree or more education in 2015, while over a third had less than a high school diploma.⁸

Education Level	Share (%) of All Immigrants	Share (%) of All Natives
College degree or more	27.4	35.1
Some college	18.6	35.3
High school diploma only	19.6	21.5
Less than a high-school diploma	34.4	8.0

Two million U.S. citizens in California live with at least one family member who is undocumented.

- 2.4 million [undocumented immigrants](#) comprised 22 percent of the immigrant population and 6 percent of the total state population in 2014.⁹
- 4.7 million people in California, including 2 million born in the United States, lived with at least one [undocumented family member](#) between 2010 and 2014.¹⁰
- During the same period, nearly one in five children in the state was a U.S.-citizen living with at least one undocumented family member (2.4 million children in total).¹¹

Nearly 200,000 Deferred Action for Childhood Arrivals (DACA) recipients live in California.

- As of 2016, 64 percent of [DACA-eligible immigrants](#) in California, or 242,339 people, had applied for DACA.¹²
- An additional 120,000 residents of the state satisfied all but the educational requirements for DACA, and another 62,000 would be eligible as they grew older.¹³

Immigrants make up more than a third of the labor force in California and are integral to a range of industries.

- 6.6 million immigrant workers comprised 33.9 percent of the labor force in 2015.¹⁴

- Immigrant workers were most numerous in the following industries:

Industry	Number of Immigrant Workers
Manufacturing	930,261
Health Care and Social Assistance	874,169
Accommodation and Food Services	691,552
Retail Trade	663,977
Construction	524,665
Analysis of the U.S. Census Bureau's 2015 American Community Survey 1-year PUMS data by the American Immigration Council.	

- The largest shares of immigrant workers were in the following industries:¹⁵

Industry	Immigrant Share (%) (of all industry workers)
Agriculture, Forestry, Fishing & Hunting	66.9
Manufacturing	45.6
Administrative & Support; Waste Management; and Remediation Services	43.2
Other Services (except Public Administration)	39.7
Accommodation and Food Services	39.2
Analysis of the U.S. Census Bureau's 2015 American Community Survey 1-year PUMS data by the American Immigration Council.	

Immigrants are an integral part of the California workforce in a range of occupations.

- In 2015, immigrant workers were most numerous in the following occupation groups:¹⁶

Occupation Category	Number of Immigrant Workers
Office and Administrative Support	675,184
Sales and Related	641,880
Production	596,157
Management	587,525
Building and Grounds Cleaning & Maintenance	580,164
Analysis of the U.S. Census Bureau's 2015 American Community Survey 1-year PUMS data by the American Immigration Council.	

- The largest shares of immigrant workers were in the following occupation groups:¹⁷

Occupation Category	Immigrant Share (%) (of all workers in occupation)
Farming, Fishing, and Forestry	77.1
Building and Grounds Cleaning & Maintenance	61.7
Production	53.3
Construction and Extraction	43.0
Computer and Mathematical Sciences	41.3
Analysis of the U.S. Census Bureau's 2015 American Community Survey 1-year PUMS data by the American Immigration Council.	

- Undocumented immigrants comprised 9 percent of the state's workforce in 2014.¹⁸

Immigrants in California have contributed tens of billions of dollars in taxes.

- [Immigrant-led households in the state paid](#) \$56.5 billion in federal taxes and \$26.4 billion in state and local taxes in 2014.¹⁹
- Undocumented immigrants in California paid an estimated \$3.2 billion in [state and local taxes](#) in 2014. Their contribution would rise to \$3.7 billion if they could receive legal status.²⁰
- [DACA recipients](#) in California paid an estimated \$534.1 million in state and local taxes in 2016.²¹

As consumers, immigrants add hundreds of billions of dollars to California's economy.

- California residents in immigrant-led households had \$238.7 billion in [spending power](#) (after-tax income) in 2014.²²

Immigrant entrepreneurs in California generate billions of dollars in business revenue.

- Nearly 880,000 immigrant business owners accounted for 38.2 percent of all self-employed California residents in 2015 and generated \$21.8 billion in business income.²³
- In the following California metropolitan areas in 2015, at least one in six business owners was an immigrant. Immigrants accounted for:
 - 40.2 percent of business owners in the Los Angeles/Long Beach/Anaheim metro area,
 - 24.1 percent in Riverside/San Bernardino,
 - 17.4 percent in Sacramento/Arden/Arcade/Roseville,
 - 34.3 percent in San Diego/Carlsbad/San Marcos,
 - 31.2 percent in San Francisco/Oakland/Fremont, and
 - 42.4 percent in San Jose/Sunnyvale/Santa Clara.²⁴

Endnotes

1. “Foreign born” does not include people born in Puerto Rico or U.S. island areas or U.S. citizens born abroad of American parent(s). U.S. Census Bureau, 2015 American Community Survey 1-Year Estimates. The American Immigration Council elected to use data from the 2015 ACS 1-Year estimates wherever possible to provide the most current information available. Since these estimates are based on a smaller sample size than the ACS 5-year, however, they are more sensitive to fluctuations and may result in greater margins of error (compared to 5-year estimates).
2. Children are defined as people age 17 or younger. Men and women do not include children. Ibid.
3. Analysis of the U.S. Census Bureau’s 2015 American Community Survey 1-year PUMS data by the American Immigration Council.
4. Analysis of data from the 2016 Current Population Survey by the American Immigration Council, using IPUMS-CPS. Sarah Flood, Miriam King, Steven Ruggles, and J. Robert Warren, *Integrated Public Use Microdata Series, Current Population Survey: Version 5.0* [dataset] (Minneapolis, MN: University of Minnesota, 2017).
5. 2015 ACS 1-Year Estimates.
6. Augmented IPUMS-ACS data, as published in “State-Level Unauthorized Population and Eligible-to-Naturalize Estimates,” Center for Migration Studies data tool, accessed August 2017, data.cmsny.org/state.html.
7. Figure includes immigrants who speak only English. Data based on survey respondents age 5 and over. Analysis of 2015 ACS 1-Year Estimates by the American Immigration Council.
8. Data based on survey respondents age 25 and older. Ibid.
9. Pew Research Center, “U.S. unauthorized immigration population estimates,” November 3, 2016, www.pewhispanic.org/interactives/unauthorized-immigrants/.
10. Silva Mathema, “State-by-State Estimates of the Family Members of Unauthorized Immigrants,” University of Southern California’s Center for the Study of Immigrant Integration and the Center for American Progress, March 2017, www.americanprogress.org/issues/immigration/news/2017/03/16/427868/state-state-estimates-family-members-unauthorized-immigrants/.
11. American Immigration Council analysis of data from the 2010–2014 ACS 5-Year, using Silva Mathema’s “State-by-State Estimates of the Family Members of Unauthorized Immigrants” and IPUMS-USA. Steven Ruggles, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek, *Integrated Public Use Microdata Series: Version 7.0* [dataset] (Minneapolis, MN: University of Minnesota, 2017).
12. “DACA-eligible” refers to immigrants who were immediately eligible to apply for DACA as of 2016. Migration Policy Institute analysis of U.S. Census Bureau data from the 2014 American Community Survey (ACS), 2010–14 ACS pooled, and the 2008 Survey of Income and Program Participation (SIPP), as cited in “Deferred Action for Childhood Arrivals (DACA) Data Tools,” accessed June 2017, www.migrationpolicy.org/programs/data-hub/deferred-action-childhood-arrivals-daca-profiles.
13. Ibid.
14. Analysis of 2015 ACS 1-year PUMS data by the American Immigration Council. Categories are based on the 2012 North American Industry Classification System (NAICS), www.census.gov/eos/www/naics/index.html.
15. Ibid.
16. Analysis of 2015 ACS 1-year PUMS data by the American Immigration Council. Categories are based on the 2010 Standard Occupational Classification (SOC) system, www.bls.gov/soc/major_groups.htm.
17. Ibid.
18. Pew Research Center, “U.S. unauthorized immigration population estimates,” 2016.
19. New American Economy, *The Contributions of New Americans in California* (New York, NY: August 2016), 5, <http://www.newamericaneconomy.org/research/the-contributions-of-new-americans-in-california>.
20. Institute on Taxation & Economic Policy (ITEP), *Undocumented Immigrants’ State & Local Tax Contributions* (Washington, DC: March 2017), 3, www.itep.org/undocumented-immigrants-state-local-tax-contributions-2/.
21. ITEP, *State & Local Tax Contributions of Young Undocumented Immigrants* (Washington, DC: April 2017), Appendix 1, <https://itep.org/state-local-tax-contributions-of-young-undocumented-immigrants/>.
22. New American Economy, *The Contributions of New Americans in California*, 4.
23. “Business owners” include people who are self-employed, at least 18 years old, and work at least 15 hours per week at their businesses. Analysis of 2015 ACS 1-year PUMS data by the American Immigration Council.
24. American Immigration Council analysis of 2016 CPS data. Flood, King, Ruggles, and Warren, *IPUMS CPS* dataset.

Exhibit 27

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

Civic Contributions: Taxes Paid by Immigrants in the Washington, DC, Metropolitan Area

May 2006

By

Randy Capps, Everett Henderson

The Urban Institute

Jeffrey S. Passel

Pew Hispanic Center

Michael Fix

Migration Policy Institute



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Executive Summary

The Washington, DC, metropolitan area is home to over 1 million immigrants, who composed one-fifth of the area's total population in 2004. The metropolitan area is relatively affluent and boasts a strong economy that attracts large numbers of immigrants for jobs at both the high- and low-skilled ends of the labor market. Immigrants in the Washington area come from more diverse countries of origin than is the case nationally, and a relatively high share come from origins with above average incomes. Whether higher or lower skilled, immigrants contribute strongly to the region's economy, purchasing power, and tax base.

Immigrant households in the Washington, D.C., metropolitan area had a total income of \$29.5 billion in 1999–2000, and they paid \$9.8 billion in taxes. This represents 19 percent of the region's total household income and 18 percent of all taxes paid. Our estimate of the amount of taxes paid by immigrants is an underestimate, because it is based on 1999–2000 data, and the number of immigrants in the region has grown from 850,000 to at least 1.2 million since that time.

Although immigrant households on average have lower incomes than native-born Washington, D.C., area households, they pay nearly the same share of their incomes in taxes. Some groups of immigrants—the most educated and highest earners—actually pay more in taxes than natives, on average. On the other hand, less-educated immigrants and those without permanent legal status have considerably lower incomes and pay a lower share of their incomes in taxes than natives.

This report estimates the taxes paid by immigrants in the Washington, D.C., area in 1999–2000 and documents their

demographics, household composition, income, and dispersal across jurisdictions in the region. The findings in this report are based mostly on analysis of 2000 U.S. Census data, because the census provides the most recent comprehensive data that allow disaggregation by country of origin groups and by many of the region's local jurisdictions. The demographic data in the report are updated through 2004 using the U.S. Current Population Survey. We calculate taxes at both the individual level (e.g., income and payroll taxes) and the household level (e.g., property taxes), but aggregate them up to the household level. Throughout the report we refer to households headed by immigrants (whether citizens, legal immigrants, or unauthorized migrants) as “immigrant households” and compare their incomes and tax payments to households headed by native-born U.S. citizens. Following are highlights of key findings from the report:

Immigrant households in the metropolitan area paid almost \$10 billion in taxes in 1999–2000, and they paid taxes at nearly the same rate as native households.

For the taxes we calculated at all levels of government, immigrant households paid \$9.8 billion, or 17.7 percent of total taxes paid by metropolitan-area residents in 1999–2000 (\$55.2 billion). Immigrants' share of taxes was virtually the same as their share of the total population (17.4 percent), but below their share of total income (19 percent) and of all households (21 percent). Immigrant households paid a slightly lower share of their incomes in taxes (28 percent) than native households (31 percent), in part because they had lower average incomes. Overall,

however, our research suggests that immigrants pay taxes roughly in proportion to their share of the overall population.

Immigrant households in the Washington, DC, metropolitan region had a total income of \$29.5 billion in 1999–2000, and they paid \$9.8 billion in taxes. This represents 19 percent of the region's total household income and 18 percent of all taxes paid.

The federal government collects nearly three-quarters of taxes paid by both immigrants and natives. In 1999–2000, \$7 billion, or 72 percent of all taxes paid by immigrant households, were federal: income, Social Security, and Medicare. Another \$1.6 billion (17 percent) were state taxes— income, sales, automobile, cigarettes, and alcohol—and the remaining \$1.1 billion (12 percent) were local taxes: property, income, sales, automobile, telephone, and utility. The distribution of taxes across federal, state, and local governments was similar for natives.¹

¹ Counties collect income taxes in Maryland, but the Virginia jurisdictions do not collect income taxes; all income taxes collected in the District of Columbia were categorized as state taxes. Virginia (but not Maryland) counties collect sales and automobile taxes.

Immigrant households in Fairfax and Montgomery counties—the two largest jurisdictions—paid 60 percent of all taxes paid by the metropolitan area’s immigrant households.

In 1999–2000, immigrant households in Fairfax County paid \$3.2 billion in taxes, or 32 percent of the regional total. Immigrant households in Montgomery County paid \$2.8 billion—28 percent of the total. Immigrants in the District of Columbia and Prince George’s County each paid about \$1 billion in taxes (10 percent of the regional total in each jurisdiction). Total tax payments were much lower in other jurisdictions because they had far fewer immigrants. This pattern may change in the future, however, as the population of immigrants is growing most rapidly in outer-ring suburban jurisdictions such as Loudoun County.

Immigrants accounted for nearly one-quarter of all taxes paid by households in Fairfax and Montgomery counties.

In 1999–2000, immigrant households paid 24 percent of all taxes paid by households in Montgomery County and 22 percent in Fairfax County. These two jurisdictions also had the highest shares of immigrants in their populations. Immigrant households paid 19 percent of taxes in Arlington, 16 percent in the District of Columbia, 15 percent in Prince George’s County, and 13 percent in Alexandria and Prince William County. In all jurisdictions, the share of taxes paid by immigrant households was only slightly below their share of

total income and of all households.

Immigrant households in Northern Virginia and suburban Maryland pay a significant share of taxes collected by the states of Virginia and Maryland.

In 1999–2000, immigrant households in the Northern Virginia jurisdictions paid 8 percent of all state taxes paid by all households across the state of Virginia: \$810 million out of \$9 billion. Immigrant households in suburban Maryland also paid 8 percent of all taxes paid by households across the state: \$560 million out of \$6.2 billion.² Thus, the taxes paid by immigrants in Northern Virginia and suburban Maryland not only support local government services but also strongly support state services across both Virginia and Maryland.

Households headed by immigrants who are naturalized citizens pay roughly the same taxes as households headed by native-born citizens, while households headed by non-citizens pay lower taxes.

In 1999, households headed by naturalized citizens—immigrants who in most cases have been in the United States at least five years and passed the citizenship exam—had an average income of \$91,000, above that for households headed by native-born citizens (\$88,000). Naturalized citizen households paid roughly the same amount in taxes as native households (\$31,000). Households headed by legal permanent residents—“green card” holders admitted as refugees, for employment, or to be reunified

with family members—had slightly lower average incomes (\$79,000) and paid lower average taxes (\$27,000). Households headed by unauthorized immigrants (i.e., those who entered the country illegally or overstayed valid visas) or by immigrants with Temporary Protected Status (those granted temporary work authorization based on war or natural disaster in their home countries, mostly El Salvador) had the lowest average incomes (\$53,000) and paid the lowest average taxes (\$13,000). Because many unauthorized immigrants are paid in cash and therefore do not pay Medicare, Social Security, or federal, state, or local income taxes, any change in immigration policy that would grant them legal work authorization would significantly raise their tax contributions.³

Households headed by immigrants from most regions of the world pay taxes similar to those paid by native households.

In 1999, average incomes were higher for households headed by immigrants from India (\$112,000), Europe (\$100,000), and the Middle East (\$95,000) than for households headed by native-born citizens. Households headed by immigrants from these regions also paid higher taxes than native households. Moreover, households headed by immigrants from South America and from China, Vietnam, and other parts of Asia had only slightly lower incomes and paid only slightly lower taxes than native households. The households with the lowest incomes were those

³ Our tax models assume that just over one-half (55 percent) of unauthorized immigrants paid income, Social Security, and Medicare taxes, as many worked under the table. We assume, however, that all unauthorized immigrants paid the other forms of taxes not directly related to work authorization. We also assume 100 percent tax compliance for immigrants with Temporary Protected Status, because they had legal work authorization.

Some counties but not others in both states collect taxes on telephones and utilities, as does the District of Columbia. For a full listing of taxes calculated at the federal, state, and local levels, see table 1.

² When households in jurisdictions outside the Washington metropolitan area were included, the share of state taxes paid by immigrant households was 11 percent in Virginia and 12 percent in Maryland.

headed by immigrants from El Salvador (\$51,000), other Central American countries (\$55,000), and sub-Saharan African countries (\$56,000); these were also the households with the lowest average tax payments. Despite this variation by origin, all groups of immigrants made significant tax contributions in 1999–2000.

Better-educated households pay more in taxes, regardless of whether they are immigrant or native households.

The 1999 average income for households headed by immigrants without a high school degree was only \$47,000—or less than one-half the income of households headed by immigrants with a four-year college degree (\$103,000). The average income for households headed by natives without a high school degree (\$39,000) was even lower, and was only about one-third of that for households headed by natives with college degrees (\$122,000). Average 1999–2000 taxes were three times lower for households headed by immigrant high school dropouts than households headed by college-educated immigrants (\$12,000 versus \$36,000), while

among native households this ratio was about four times lower (\$11,000 versus \$45,000). Thus, investments in adult education—for both natives and immigrants—will likely pay off in terms of higher tax revenues in the future.

English-speaking immigrant households pay higher taxes than native households, but households headed by immigrants who speak little or no English pay much lower taxes.

In 1999–2000, the average income of English-speaking immigrant households was higher than that for native households (\$97,000 versus \$88,000), and English-speaking immigrant households paid higher taxes on average (\$34,000 versus \$31,000). Immigrant households where English was not the primary language but the household head spoke English very well had average income and tax payments nearly as high as those for native households. On the other hand, immigrant households where the head did not speak English at all had much lower average income (\$41,000) and tax payments (\$11,000). These findings suggest

that immigrants from English-speaking countries and those who learn English well contribute as strongly to the tax base as native-born residents of the Washington region. For those immigrants who do not speak English well, investment in English language instruction will likely raise their future income and tax contributions.

Taken together, these findings show the importance of immigrants to the regional economy and tax base. As the share of immigrants in the region's population increases—from 18 to 20 percent between 2000 and 2004—their contributions to the tax base will continue to increase. All immigrants pay substantial shares of their incomes in taxes, and in most cases these shares are close to those paid by natives. The best-educated immigrants—those from India, Europe, and the Middle East—actually earn more income and pay higher taxes than natives. Continued flows of these highly skilled immigrants into the region represent an economic and fiscal bonus to the area's state and local governments.

In 1999–2000, immigrant households in Fairfax County paid \$3.2 billion in taxes, or 32 percent of the regional total. Immigrant households in Montgomery County paid \$2.8 billion—28 percent of the total. Immigrants in the District of Columbia and Prince George's County each paid about \$1 billion in taxes (10 percent of the regional total in each jurisdiction).

Foreword

Few issues in the current debate over comprehensive immigration reform are as controversial and contested as the fiscal impacts of illegal immigrants on the communities within which they live and the United States as a whole. The costs and benefits of immigrants have become a hot political topic recently, owing in part to the rapid rise in immigration—both legal and illegal—over the past couple of decades. Costs such as public education, health care, and social services are frequent topics of concern and debate when it comes to recent immigrants (Camarota 2004); however, the sizeable contributions of immigrants to the economy and tax base are often underestimated or overlooked (Kasarda and Johnson 2005). This report does not offer estimates of the overall economic impact of immigration on the Washington, D.C., area; instead, it focuses on one element of immigrants' impact that is very important for local officials and policymakers: their tax contributions. Because immigrants have become such a large population, their tax payments have become a significant part of state and local budgets as well as federal tax revenues generated in the area.

A recent survey from the Pew Hispanic Research and Hispanic Center (Kohut and Suro 2006) finds 56 percent of respondents nationwide believe that immigrants do not pay their fair share of taxes. Only one-third of respondents believe that immigrants do so.⁴ But our analysis in this report suggests that immigrants in the Washington, D.C., metropolitan

area do pay their fair share of taxes, at least in comparison to natives. In fact, we find no substantial differences in the average tax payments or share of income paid in taxes between natives and immigrants, with one important exception: the unauthorized population.

Our analysis focuses on the Washington metropolitan region, a region that emerged during the 1990s as an important new immigrant gateway and settlement area. Between 1990 and 2000, the foreign-born in Washington grew by almost 70 percent, with much of that growth coming in the large, comparatively wealthy and close-in suburbs—especially Fairfax and Montgomery counties (Price et al. 2005). This rapid growth and suburbanization reflect nationwide patterns in immigrant settlement and give this report a salience beyond the Washington area (Singer 2004).

Moreover, the report is being released at a time of intense political ferment as Congress is debating reforms that could increase the number of both temporary and permanent immigrants entering the country, place a significant share of the unauthorized population on a path to legal status, and stiffen federal and state enforcement of the nation's immigration laws. One question underlying the debate is whether immigrants pay their own way—and the degree to which native taxpayers are subsidizing their presence. Again, our analysis suggests that immigrants in the Washington metropolitan area are paying their own way and are not heavily subsidized by the native-born population.

The central purpose of this report is to estimate the amount of taxes paid by immigrants living in the metropolitan area. In the Washington, D.C., area, as nationally, immigrants are predominantly of young working

age and—with the exception of some unauthorized immigrants—pay the same income and payroll taxes as native-born citizens. Very few are yet retired or collect pensions, Social Security, or other retirement benefits. Additionally, since the area is home to so many relatively well-paid immigrants, they in most cases pay income and payroll taxes at nearly the same rates as citizens.

Immigrants pay several other types of taxes as well, and these other taxes tend to be more important sources of revenue at the state and local level. Many immigrants own homes and pay state and local property taxes; others—like all renters—pay property taxes indirectly through their rent payments. Immigrants consume the same goods and services as everyone else, and therefore also pay state and local sales taxes. Many immigrants own cars and pay automobile taxes in the jurisdictions that collect them. In this report we sum up all taxes paid by immigrants and natives in the national capital area, including income, Medicare, and Social Security taxes paid to the federal government as well as income, property, sales, automobile, tobacco, alcohol, and utility taxes paid to the District of Columbia, Maryland, Virginia, and most major suburban jurisdictions. While not exhaustive of all taxes and fees, our methodology captures the vast majority of taxes paid by individuals in the metropolitan area.⁵

Several general points that emerge from this study bear emphasizing:

- All immigrant groups, regardless of legal status, pay substantial

⁴ The survey also had a subsample in the Washington, D.C., metropolitan area, where 46 percent of respondents said that immigrants do not pay their fair share in taxes, but 41 percent said they believe that immigrants do pay their fair share.

⁵ Where possible, we adjust our tax estimates to match reported total individual tax contributions across jurisdictions. For a full list of taxes included in our analysis and how they are calculated, see the methodology section of this report.

amounts of federal, state, and local taxes.

- Contrary to public perception noted above, on average, immigrant households paid roughly the same share of taxes as they represent of the total regional population. Since there are so many immigrants in suburban Maryland and Northern Virginia, immigrants living in these areas provide substantial support for services across the states of Maryland and Virginia through their state tax payments.
- Tax payments are correlated with citizenship, legal status, education levels, and English-speaking ability. Hence, immigrants who are unauthorized, who have less than a high school education, and who are limited English proficient pay lower taxes on average than other immigrant populations. Offering a path to legalization and increasing educational attainment and English skills would probably increase these groups' fiscal contributions.
- While the national capital region is in many ways representative of the nation as a whole, it is also distinct in some ways. Unlike many metropolitan areas, no single national origin group dominates the immigrant population (as Mexicans do in California and Texas, for example). Many members of the region's largest group—Salvadorans—have been granted what is known as Temporary Protected Status, which contemplates their eventual return to their home country but also authorizes them for work and enhances their tax contributions.

Approach

This study adapts analytic methods used in previous Urban

Institute studies of taxes paid by immigrants in New York State and Los Angeles (Passel and Clark 1998; Clark and Passel 1993). We start with detailed data on individual households drawn from the Census 2000 5-Percent Public Use Microdata Sample—the largest public use data set with information on U.S. households and their income and assets. We use techniques pioneered by the Urban Institute and Pew Hispanic Center to assign legal status to the immigrants in the data set, such as naturalized citizens, legal permanent residents, refugees, legal temporary migrants, unauthorized migrants, and immigrants with Temporary Protected Status.

We estimate the tax contributions for each household based on the household's income, demographic composition, and other characteristics, including home ownership, rent/mortgage payments, other housing expenses, and automobile ownership. We adjust the initial estimates to account for deficiencies in census data (such as underreporting of income and under-sampling of immigrants) and to ensure that the total tax contributions we estimate for each jurisdiction agree with the reported amounts of taxes collected. Using this method, we are able to develop estimates of several different taxes paid by immigrants for each major jurisdiction in the national capital region.

We estimate three broad categories of tax payments, and our estimates are developed for individuals, families, and households depending on the appropriate unit for each tax. Payroll taxes (Social Security and Medicare) are estimated on an individual basis from information on wage and salary income as well as self-employment income. Income taxes (federal, state, and local) are estimated for "filing units" (essentially nuclear family

units) using estimates of taxable income based principally on the census but augmented to account for income undercounted by the census. Property and consumption tax estimates are modeled at the household level based on income data from the census and consumption models developed from the Consumer Expenditure Survey (CES). Consumption taxes we estimate at the state and local level—depending on the jurisdiction—include general sales, automobile, alcohol, and tobacco. The tax estimates for all three categories are aggregated to households and reported at this level.

Income and tax estimates are reported for geographic areas identifiable in the census data that correspond to political jurisdictions within the Washington, D.C., metropolitan area. The District of Columbia is covered as a single area in the study. We report taxes for the following Virginia jurisdictions: Alexandria City, Arlington County, Fairfax County (including the cities of Fairfax and Falls Church), Fredericksburg City (including Spotsylvania and Culpeper counties), Loudoun County (including Fauquier, Clarke, and Warren counties), Stafford County (including King George County), and Prince William County (including Manassas and Manassas Park cities). The Maryland areas reported are Prince George's County, Montgomery County, Frederick County, Charles County, and Calvert County (including St. Mary's County).

The main goal of the study is to estimate all types of taxes in all jurisdictions for households headed by immigrants, and to compare their tax payments with payments of households headed by natives. We also calculate tax payments separately for immigrant households according to their citizenship and legal status, and country or region of birth.

Demographics of the National Capital Region's Foreign-Born Population

The Washington, D.C., metropolitan area is home to over 1 million immigrants, who composed one-fifth of the area's total population in 2004. The metropolitan area is relatively affluent and boasts a strong economy that attracts large numbers of immigrants for jobs at both the high- and low-skilled ends of the labor market. As we will show in this report, immigrants in the Washington area come from more diverse countries of origin than is the case nationally, and a relatively high share of the region's immigrants come from origins with above average incomes.

While we provide more recent data on the demographics of immigrants, we use the 2000 U.S. Census to calculate taxes for several reasons. First, the census provides the most recent data with comprehensive coverage and large samples of immigrant populations.⁶ Second, the census includes geographic detail that permits us to examine the District of Columbia and most major suburban jurisdictions in Maryland and Virginia separately. Third, using the census, we can disaggregate income and taxes paid for several different country and region-of-origin groups within the area's jurisdictions. Fourth, the census offers detail on income, housing characteristics, and automobile ownership—all important inputs for our tax estimation models. Finally, we have assigned legal status to non-citizen immigrants in the census data, allowing us to estimate taxes paid by different groups of

Between 2000 and 2004, the area's total population grew by 21 percent or about 1 million—from 4.8 to 5.8 million—but the foreign-born population grew much faster—by 36 percent, from 850,000 to 1.16 million. Immigrants represented 18 percent of all the metropolitan area's residents in 2000 and 20 percent in 2004.

immigrants according to their status, such as naturalized citizens, temporary legal residents, legal permanent residents, unauthorized migrants, and refugees (for definitions of these groups, see figure 1).⁷

⁶ Nonetheless, our estimates build in an assumed undercount of up to 10 percent for certain immigrant groups in the 2000 Census.

⁷ For a full description of our legal status assignment methodology, see the methodology section of the report.

Figure 1: Definitions of Citizenship and Legal Status Categories Used in the Report

Non-citizens

- *Legal permanent residents (LPRs)* are legally admitted to live permanently in the United States through qualifying for immigrant visas abroad or adjustment to permanent resident status in the United States. LPRs are issued documentation commonly referred to as “green cards,” although the cards have not been green for many years. Almost all LPRs are “sponsored” (i.e., brought to the United States) by close family members or employers.
- *Refugees and asylees* are granted legal status due to persecution or a well-founded fear of persecution in their home countries. Refugee status is granted before entry to the United States. Refugee status may be granted to a group of persons, although each individual must also qualify for the status. Asylees must meet the same criteria regarding fear of persecution. Unlike refugees, asylees usually arrive in the country without authorization (or overstay a valid visa), later claim asylum, and are granted their legal status while in the United States. After one year, refugees and asylees are generally eligible for permanent residency, and then for naturalization five years after that. Almost all “adjust” their status and become LPRs, although they retain certain rights—for instance, eligibility for major federal benefit programs—by virtue of their designation as refugees or asylees. In this report, we categorize all immigrants who enter as refugees or are granted asylum as refugees, even after they become LPRs or citizens.
- *Temporary legal residents* have been admitted to the United States for a temporary or indefinite period, but have not attained permanent residency. Most are people who have entered for a temporary period, for work, as students, or because of political disruption or natural disasters in their home countries. Some seek to stay for a permanent or indefinite period and have a “pending” status that allows them to remain in the country and often to work but does not carry the same rights as legal permanent residency.
- *Unauthorized migrants* do not possess a valid visa or other immigration document, because they entered the United States illegally (usually across the Mexican border), stayed longer than their temporary visas permitted, or otherwise violated the terms under which they were admitted. A small number eventually adjust their status and attain legal residency after a sponsorship petition has been filed by a relative, spouse, or employer.
- *Temporary protected status (TPS)* was granted to several hundred thousand migrants who fled wars and natural disasters during the 1980s and 1990s. Most TPS migrants entered the country illegally or overstayed valid visas—and thus were once part of the unauthorized population—but later applied for and received TPS. A large majority of TPS migrants are from El Salvador, although small numbers of migrants from other Central American and African countries have also received TPS.

Citizens

- *Naturalized citizens* are former LPRs who have become U.S. citizens through the naturalization process. Typically, LPRs must be in the United States for five or more years to qualify for naturalization, although immigrants who marry citizens can qualify in three years, and some small categories qualify even sooner. LPRs must take a citizenship test—in English—and pass background checks before qualifying to naturalize.
- *Native-born citizens.* All people born in the United States (including the children of non-citizen parents) are granted birthright citizenship, regardless of their parents’ birthplace or legal status. Native-born citizens also include people born in Puerto Rico, U.S. Virgin Islands, other U.S. territories and possessions, and those born in foreign countries to a U.S. citizen parent.

Trends in the Size of the Region's Foreign-Born Population

Over the past few years the Washington, D.C., area has experienced rapid growth in its total population, and the share of the population composed of immigrants has also risen. Between 2000 and 2004, the area's total population grew by 21 percent or about 1 million—from 4.8 to 5.8 million—but the foreign-born population grew much faster—by 36 percent, from 850,000 to 1.16 million. Immigrants were 18 percent of all the metropolitan area's residents in 2000 and 20 percent in 2004. The Washington, D.C., area also has a higher share of immigrants than the overall U.S. population (12 percent in 2004), but the region's foreign-born share is lower than in such major immigration gateway cities such as New York, Los Angeles, and Miami.

As the foreign-born share of the region's population increases, so will their tax and other economic contributions. Since we use 2000 data to develop our tax estimates, and the immigrant population has grown by more than one-third since that time, our estimates likely *underestimate* substantially immigrants' current tax contributions.

Origins of the Region's Immigrants

The national capital area's foreign-born population is somewhat more diverse than the immigrant population nationally, and this diversity has important implications for immigrants' family income and tax contributions. In the Washington, DC, area, the two largest source regions for immigrants were Latin America (40 percent) and Asia (32 percent) in 2000 (figure 2). Central America accounted for 24 percent of the region's immigrants, and El Salvador was the source country

for 13 percent. Immigrants from Africa and the Middle East made up 15 percent and those from Europe and Canada the remaining 13 percent (for a detailed list of origins for the region's immigrants, see appendix table 1). Nationally, about one-half of immigrants come from Latin American and one-quarter from Asia, but shares from Africa and the Middle East are much lower (under 5 percent). One important contrast between the Washington, D.C., region and the nation is that Mexico is the most common sending country nationally (30 percent), while El Salvador is the most common country regionally. Another important difference is the relatively high shares of Asian, Middle Eastern, and African immigrants in the area. As we shall see later in the report, Asian and Middle Eastern immigrants earn relatively high incomes, and so their overrepresentation in the Washington, D.C., area is part of why the region's immigrant population is relatively affluent and the tax base relatively strong.

Figure 2: Region of Origin for Washington, DC, Metropolitan Area Immigrants, 2000



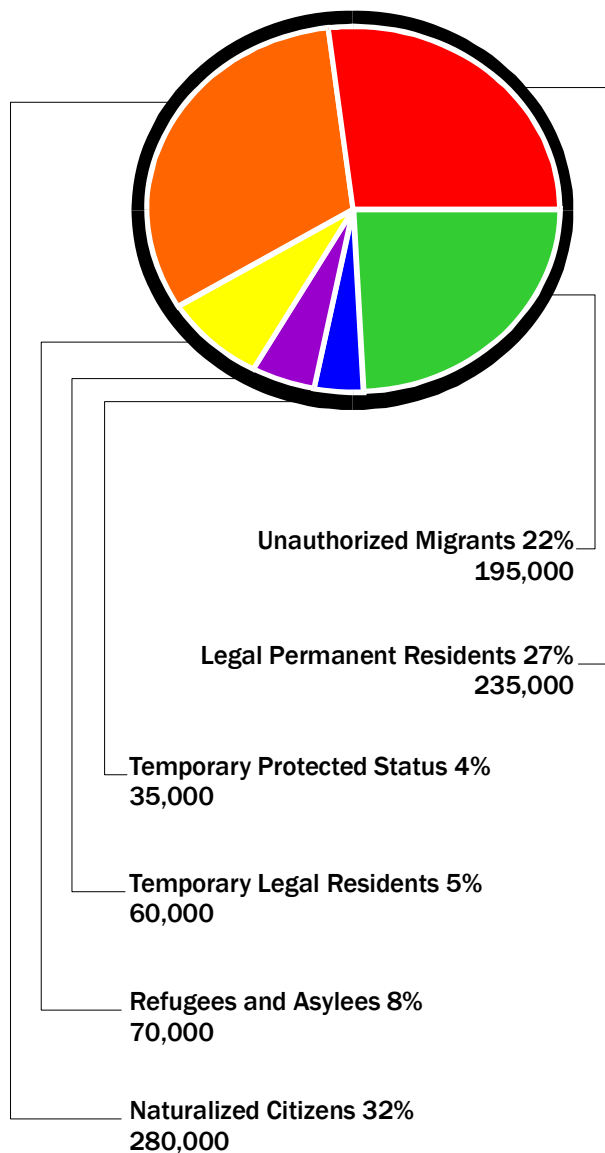
Total of 857,000 Foreign-Born in April 2000

Source: Urban Institute and Pew Hispanic Center analysis of U.S. Census data, 2000.

Citizenship and Legal Status of the Region's Immigrants

Washington, D.C.'s immigrant population includes a mix of legal and unauthorized immigrants, as well as those who have become U.S. citizens (for definitions of the citizenship and legal status groups analyzed in this report, see figure 1). In 2000, 26 percent of the region's immigrants were unauthorized immigrants and immigrants from Central America who had Temporary Protected Status (figure 3). This share was near the national average in 2000 (27 percent). Unauthorized immigrants are those who enter the United States illegally, overstay valid temporary visas, or otherwise violate immigration laws. The U.S. Congress granted Temporary Protected Status (TPS) to immigrants from Central America who fled civil wars and natural disasters, mostly during the 1980s. TPS was renewed for about 300,000 immigrants—mostly from El Salvador and Honduras—in early 2006 (U.S. Citizenship and Immigration Services 2006). The share of immigrants who have TPS is relatively high in Washington, D.C., because the region has so many immigrants from Central America.⁸ Nonetheless, there is a significant unauthorized population—i.e., those lacking any form of legal documentation—in the metropolitan area, and the vast majority are immigrants from Latin American countries.

Figure 3: Citizenship and Legal Status of Washington, DC, Metropolitan Area Immigrants, 2000



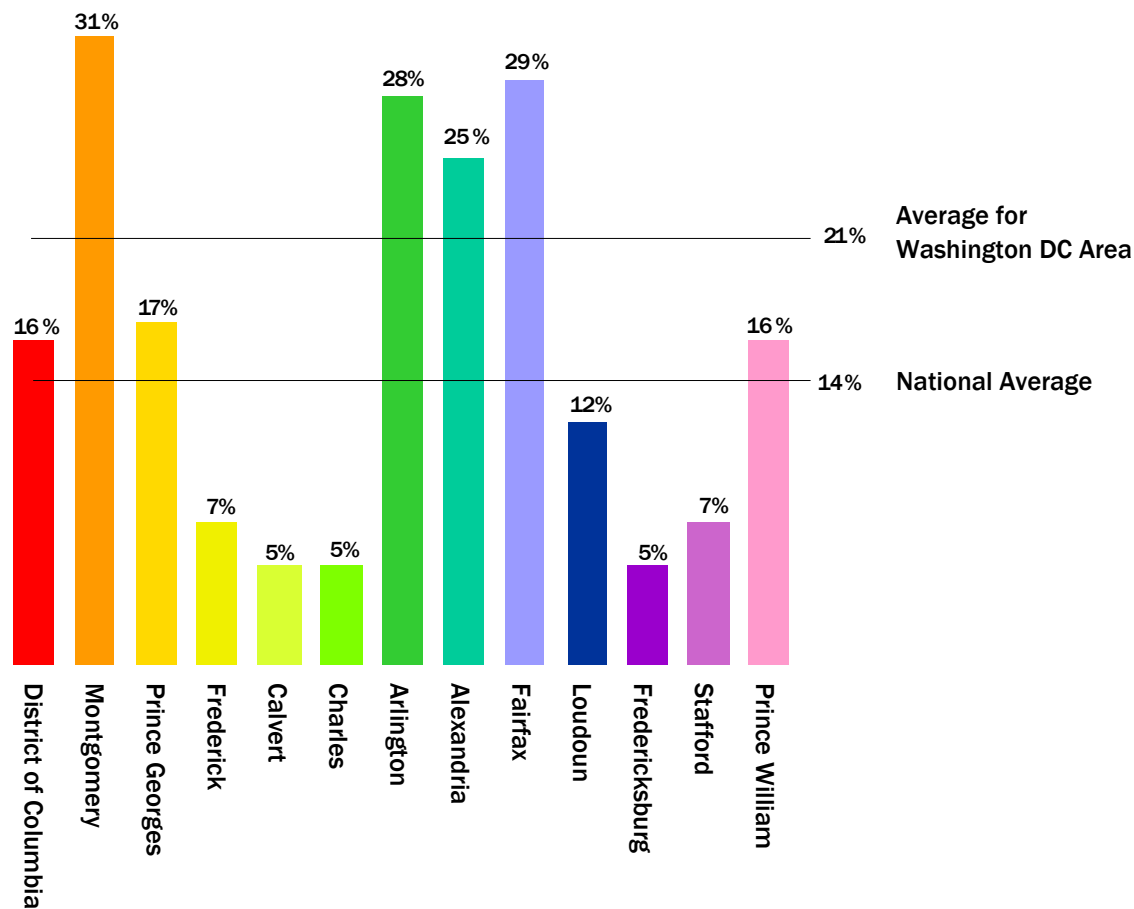
Total of 870,000 Foreign-Born in April 2000

⁸ We estimated that 54 percent of all the immigrants from El Salvador we assigned "unauthorized" status actually had TPS. Thus, out of the 65,000 "unauthorized" migrants from El Salvador in the Washington, D.C., metropolitan area in 2000, we consider 35,000 to have TPS and the remaining 30,000 to be truly unauthorized. There were very small numbers of unauthorized migrants from other countries, and so we did not develop an estimate of the TPS population for any other country.

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the Census.

Note: The data displayed in this chart are adjusted for estimated undercount of immigrants in the census and therefore vary substantially from the official census data used elsewhere in this report.

Figure 4: Foreign-born Share of Total Population by Jurisdiction, Washington, DC, Metropolitan Area, 2000



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the Census.

Note: Counties are grouped according to 2000 Census Public Use Microdata Areas. Some groups of counties include counties outside the official Washington, DC, metropolitan statistical area.

The share of immigrants who were legal permanent residents (LPRs) in 2000 (27 percent) was nearly the same as the undocumented share and again matched the national pattern. LPRs are “green card” holders or immigrants who have been legally admitted to the country and can apply for citizenship after several years. A higher share of the region’s immigrants (32 percent) were naturalized immigrants—those LPRs who had already applied for and received citizenship. Finally, in the national capital area as nationally, very small shares of immigrants were refugees—those who have been admitted based on

fear of persecution—or temporary residents such as students and temporary workers (for a detailed description of the citizenship and legal status of the population and households by jurisdiction, see appendix table 2). Many of the region’s students and temporary workers come from Asian countries such as India and China.

Distribution of Immigrants across the Region

The foreign-born are most highly concentrated in Washington, DC’s

most affluent inner-ring suburbs, slightly less concentrated in the District of Columbia and Prince George’s County, and least concentrated in outer-ring suburbs. In 2000, immigrants were slightly more likely than the native-born to live in suburban Virginia, but slightly less likely to live in the District of Columbia. About the same share of immigrants and natives (44 percent) lived in suburban Maryland (appendix table 2). The vast majority of immigrants (70 percent) lived in the three most populous suburban counties: Fairfax (29 percent), Montgomery (27 percent), and Prince George’s (14 percent).

Fairfax and Montgomery County each had more than 100,000 households headed by immigrants; Prince George's County was home to 49,000 immigrant households. The District of Columbia also had a high number of immigrant households: 39,000. The absolute number of immigrant households was much lower in most other jurisdictions. As we shall see later in the report, total taxes paid by immigrant households follow a similar pattern, with the vast majority of taxes generated in Fairfax, Montgomery, and Prince George's counties as well as the District of Columbia.

The share of households headed by immigrants in 2000 was highest and exceeded the metropolitan-area average in inner-ring suburban towns and counties: Montgomery County (31 percent), Fairfax County (29 percent), Arlington (28 percent), and Alexandria (25 percent), Prince George's County (17 percent) and the District of Columbia (16 percent) and had foreign-born household shares below the metropolitan average of 21 percent (figure 4). All the outer-ring counties except Prince William had foreign-born household shares below the *national* average of 14 percent.⁹

Other studies have documented the slowing growth of foreign-born population inside the District of Columbia and inner-ring suburbs such as Arlington and Alexandria, along with the rising share of immigrants in the outer-ring suburbs (Singer 2003). Due mostly to the cost of housing, less affluent immigrants tend to concentrate in less expensive housing within the

District of Columbia, Prince George's County, eastern Montgomery County, and parts of Fairfax County. More affluent immigrants, however, are dispersed throughout the more expensive suburban areas, with the largest numbers in Fairfax County, Montgomery County, and Arlington County. Immigrant populations are growing most rapidly, however, in counties farther out in the metropolitan area, for instance Loudoun, Prince William, and Howard. It is likely that the number of immigrants and their tax payments have grown significantly everywhere since 2000, the year on which our tax estimates are based. But the number of immigrants has grown most rapidly in the outer-ring counties, where tax payments of immigrants have also likely grown the most since 2000.¹⁰

Immigrant Household Size and Income

The taxes paid by immigrant households are determined in large part by their incomes, which in turn are affected strongly by the number of wage earners in the household. Immigrant households are generally larger than native households—in the Washington, DC, area as nationally—and they include more adults as well as more children on average. The average size of immigrant households in the Washington metropolitan region in 2000 was 3.2 persons, 29 percent higher than the average size of native households (2.5 persons). There were an average of 2.3 adults per immigrant household, compared with 1.8 adults in native households (appendix table 3).

Immigrant households also had more children on average than native households: 0.8 versus 0.6.¹¹ Households headed by unauthorized or TPS immigrants were the largest, with an average 3.6 persons (2.5 adults and 1.1 children). But all other types of immigrant households were also larger on average than native households.

Larger household size means that immigrant households have more earners than native households on average and that their total incomes receive a substantial boost from the additional earners. In 1999, immigrant households in the Washington metropolitan area had an average income of \$78,000, about 89 percent of the average income for native households (\$88,000).¹² But average income *per person* for immigrant households was just \$24,000, only 69 percent of the average for natives (\$36,000).

There is substantial variation in the average incomes of immigrant households by their citizenship and legal status. Despite their higher average size, households headed by unauthorized immigrants had the lowest average income (\$53,000), while income for households headed by naturalized citizens (\$91,000) was actually higher than that for native households (figure 5). Although naturalized citizen households had a total income that was 104 percent of the average for native households, their average income *per person* (\$31,000) was only 87 percent of the average for native

¹¹ These averages include households without children, so the average for households with children would be substantially higher.

¹² In our analysis we calculated mean income instead of median income. Mean income is necessary in order to calculate the average share of taxes paid by households (which is equivalent to mean taxes paid divided by mean income). The mean income is substantially higher than the median income for all households, regardless of nativity, because the highest income households are weighted more heavily when calculating the mean than the median.

⁹ The outer-ring suburban counties were grouped using 2000 Census Public Use Microdata Areas (PUMAs), and in some cases these PUMAs include counties outside the official Washington, DC, metropolitan statistical area. We drew our boundaries based on PUMAs to be as inclusive as possible. For a full list of counties and groups of counties included in the report, see the methodology section of this report.

¹⁰ Unfortunately, there are no data comparable to the 2000 Census that would enable us to develop more recent tax estimates for all the jurisdictions in this report. The Census Bureau has been conducting the American Community Survey (ACS) every year since 2002, and the data are directly comparable to the 2000 Census. County and city-level estimates from the ACS will not be available until 2007 or 2008, however.

Figure 5: Mean Household Income by Household Nativity, Citizenship and Legal Status, Washington, DC, Metropolitan Area, 1999 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the Census.

Note: Income figures are reported for the year before the census was taken (1999) and are shown here rounded to the nearest thousand dollars.

households per person (appendix table 3). For all types of immigrant households, the ratio of total income to native households is higher than the ratio of personal income to natives, meaning that the larger number of adult workers in immigrant households boosts their income regardless of citizenship and legal status. Nonetheless, unauthorized and TPS households have by far the lowest incomes—mostly because unauthorized immigrants earn the lowest wages (Capps et al. 2003). Legal temporary migrant households also have relatively low incomes, because about one-half of the temporary residents are students and only work part time, often at relatively low-wage jobs.

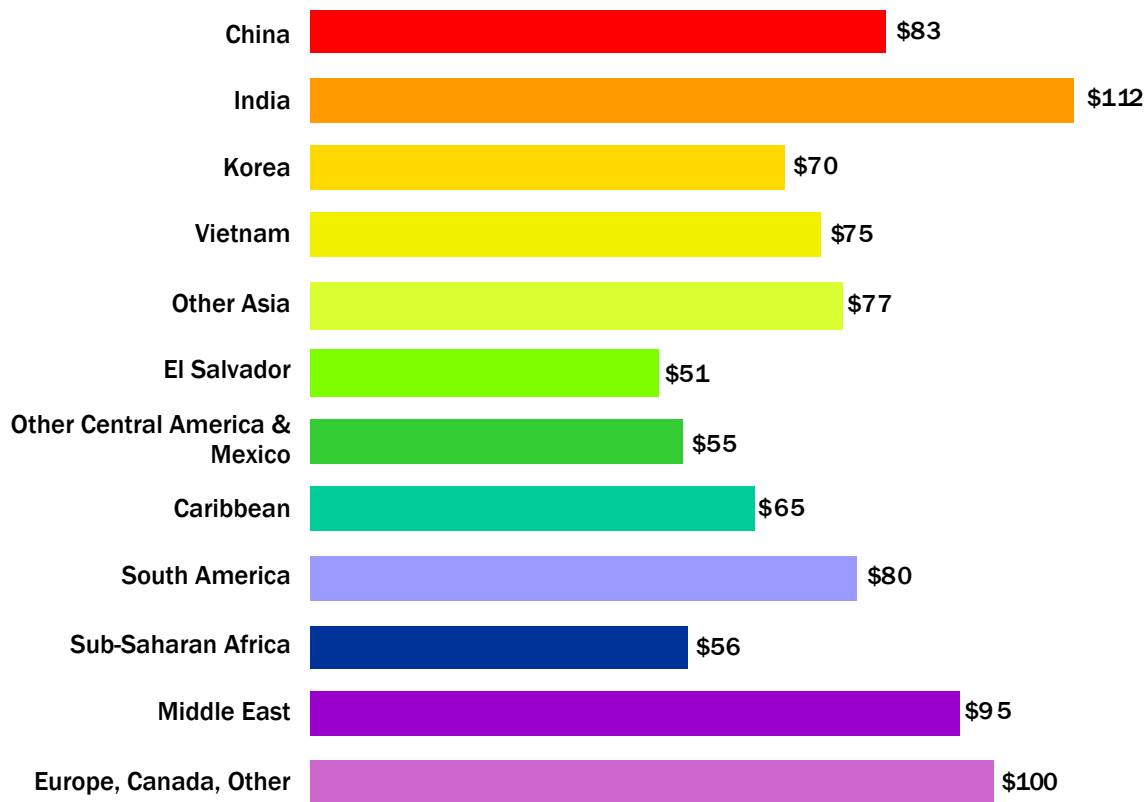
Immigrants from all regions of the world except for Europe and Canada live in households that are larger than native households (appendix table 4). In 2000, households headed by immigrants from El Salvador were the largest, with 4.4 persons on average (3

adults plus 1.4 children). Households headed by immigrants from other Central American countries—including Mexico—were the next largest (4.2 persons), followed by Vietnamese households (3.7 persons) and South American households (3.3 persons). Latin American and Asian immigrant households were the largest, followed by African, Middle Eastern, and, finally, European and Canadian households. European and Canadian households were actually slightly smaller than native households (2.4 versus 2.5 persons on average). Even European and Canadian households had more adults on average than native households (1.9 versus 1.8); their lower total household size was due to fewer children.

There is also substantial variation in household income by region of birth; in fact, some European and some Asian immigrant groups have average household incomes

that are higher than those of native households (figure 6). In 1999, average incomes were higher than natives' incomes for households headed by immigrants from India (\$112,000), Europe and Canada (\$100,000), and the Middle East (\$95,000). At the other end of the spectrum, the average income was only about one-half as high for households headed by immigrants from El Salvador (\$51,000), other Central American countries, (\$55,000) and sub-Saharan African (\$56,000). The tremendous variation in income will also be reflected in the amount of taxes paid by immigrants from different parts of the world, as we will see later in this report. For all immigrants from all regions of the world, however, the ratio of household income to natives' income is substantially higher than the ratio of *per person* income, due to higher average household size.

Figure 6: Mean Household Income by Household Place of Birth, Washington, DC, Metropolitan Area, 1999 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Income figures are reported for the year before the census was taken (1999) and are shown here rounded to the nearest thousand dollars.

Taxes Paid by Immigrants in the National Capital Region

Immigrant households living in the Washington, DC, metropolitan area pay taxes roughly in proportion to their share of total income and of all households in

the region. Immigrant households paid a total of \$9.7 billion in taxes at the federal, state, and local levels in 1999–2000 (table 1, page 18). This amounted to about 18 percent of the taxes paid by all households in the region (\$55 billion). Immigrant households had a total income of \$29 billion, or 19 percent of the regional total for all households (\$158 billion), in

1999. There were 380,000 immigrant households, constituting 21 percent of the region's total of 1.8 million households in 2000 (appendix table 5).

Table 1: Total Taxes Paid by Immigrant Households in the Washington, DC, Metropolitan Area, by Type of Tax, circa 1999–2000

Tax	Immigrant Households*	All Households*	% from Immigrants
Total, all taxes	9,771	55,174	17.7%
Federal Taxes	7,030	40,183	17.5%
Income Tax	3,924	24,366	16.1%
Social Security ^a	2,420	12,225	19.8%
Medicare ^a	686	3,592	19.1%
State Tax	1,643	9,425	17.4%
Income tax	1,079	6,328	17.0%
Sales tax ^b	418	2,326	18.0%
Auto taxes and fees ^c	120	592	20.4%
Tax on cigarettes and alcohol ^d	26	179	14.7%
Local Tax	1,098	5,566	19.7%
Real property tax ^e	627	3,145	19.9%
Income tax ^f	226	1,254	18.0%
Sales tax ^g	76	400	19.0%
Auto taxes and fees ^h	121	511	23.6%
Utility and telephone taxes ⁱ	48	256	19.0%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

*Indicates figures in millions of dollars.

^a Includes employer-paid share.

^b State portions of general sales tax (including food), restaurant tax, and sales tax on alcohol (VA).

^c Includes state gasoline tax and state car registration, but does not include excise tax on purchases.

^d Includes special state tax on alcohol and cigarettes, but does not include profit from state store sales (VA).

^e Includes estimated payments on behalf of renters; includes state portion in Maryland and all of DC's residential property tax.

^f Maryland only—county share.

^g Includes local cigarette and tobacco tax, and estimated state share of general tax.

^h Includes "car tax" plus local gasoline tax and local car registration in Virginia, but does not include excise tax on purchase.

ⁱ Includes 911 fee, local phone tax, and any "tax" on utilities (not operating profit). Does not include federal/state excise taxes. Some of the utility taxes are levied on suppliers but are passed through to the consumer.

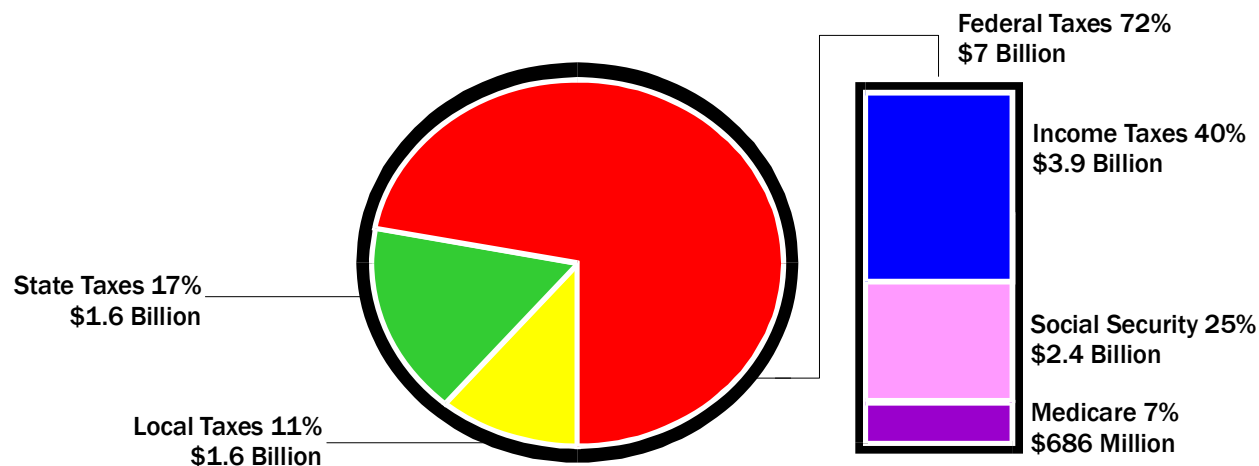
Federal taxes account for almost three-quarters of taxes collected from both immigrant and native households in the metropolitan area. In 1999–2000, 72 percent of all the taxes paid by immigrants in the region went to the federal government (figure 7). These taxes included income, Social Security,

and Medicare.¹³ State taxes— income, sales, automobile, and cigarettes/alcohol—accounted for another 17 percent of all taxes paid by immigrant households. Local income, property, automobile, utility, and telephone

taxes accounted for the remaining 11 percent. (For a complete listing of the taxes we calculated and how we calculated them, see the methodology section of this report.)

¹³ We included the employer contributions to Social Security and Medicare in these totals.

Figure 7: Federal, State, and Local Shares of Taxes Collected from Immigrant Households in the Washington, DC, Metropolitan Area, circa 1999–2000



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Immigrant households paid a relatively low share of income and other progressive taxes, but a higher share of taxes that are less progressive. In 1999–2000, immigrant households paid 16 percent of all federal, 17 percent of state, and 18 percent of local income taxes (figure 8, page 20). Immigrant households on average have lower incomes than native households, so they pay a lower share of these progressive taxes because the tax rates are higher at higher income levels. Immigrant households paid a higher share of Medicare and Social Security taxes (19 and 20 percent); these are flat-rate taxes that are capped at a certain income level. Immigrant households also paid a relatively high share of state and local sales and other consumption taxes—with the exception of state taxes on alcohol and tobacco. Despite these variations, however, immigrant households paid taxes roughly in proportion to their share of total regional income (19 percent) and all households (21 percent).

Overall, immigrant households pay taxes at roughly the same rate as native households in the national capital region. Their tax rates are

slightly lower because their incomes are lower, and this lowers the rates on the progressive income taxes.¹⁴ The average immigrant household paid \$26,000 in federal, state, and local taxes in 1999–2000 (appendix table 6). The average total tax payment by immigrant households amounted to 83 percent of the average for native households: \$31,000. Immigrant households paid 28 percent of their incomes in taxes, compared to an average of 31 percent for native households.¹⁵

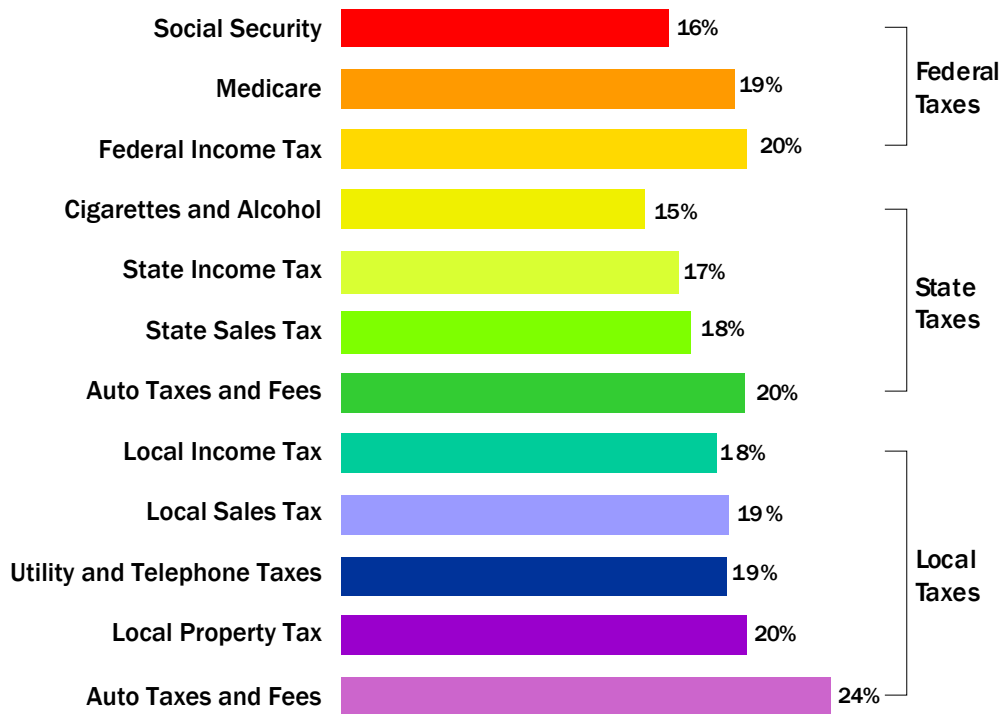
¹⁴ Tax payments are also lower for immigrant households because some unauthorized immigrants work under the table and do not pay income or payroll taxes. We assume that 55 percent of unauthorized immigrants pay Medicare, Social Security, and federal, state, and local income taxes. We also assume that 100 percent of unauthorized immigrants pay property and consumption taxes.

¹⁵ When calculating the share of household income paid in taxes, we exclude the employer share for Medicare and Social Security.

Immigrant Contributions to the Federal, State, and Local Tax Bases across Jurisdictions in the National Capital Region

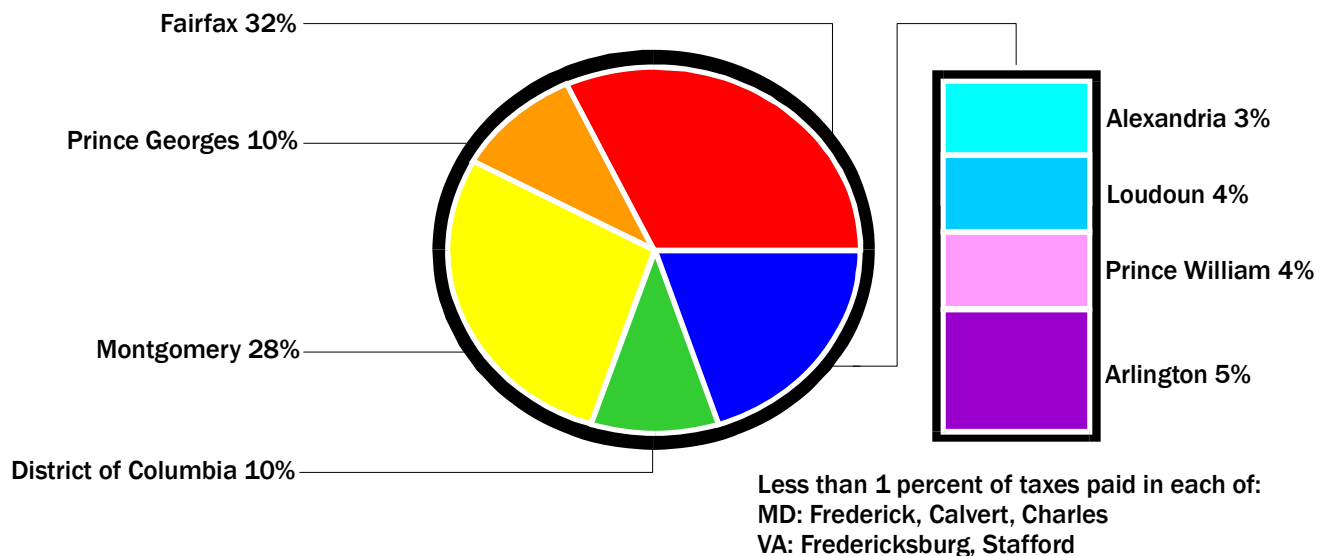
Immigrant households are a significant part of the tax base in all parts of the Washington, DC, region, but they are the largest share and contribute the most in taxes to the region's two largest jurisdictions: Fairfax and Montgomery counties. In 1999–2000, immigrant households in Fairfax and Montgomery counties accounted for 60 percent of all taxes paid by immigrant households in the region (figure 9). Immigrant households living in Fairfax County paid a total of \$3.2 billion in federal, state, and local taxes in 1999–2000, while those in Montgomery County paid \$2.8 billion in taxes (appendix table 7). These two counties also have the largest absolute numbers and highest shares of immigrants, as discussed in the previous section of this report.

Figure 8: Share of Taxes Paid by Immigrant Households in the Washington, DC, Metropolitan Area, by Type of Tax, circa 1999–2000



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Figure 9: Distribution of Taxes Paid by Immigrant Households across Jurisdictions in the Washington, D.C., Metropolitan Area, circa 1999–2000



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

The District of Columbia and Prince George's County each accounted for 10 percent of all the taxes paid by immigrant households. In 1999–2000, immigrant households living in each of these two jurisdictions paid about \$1 billion in taxes. Immigrant households in the smaller jurisdictions of Alexandria and Arlington generated relatively low shares of the total because they had fewer immigrant households. The lowest shares of taxes came from the outlying jurisdictions, where immigrants represented a relatively low share of the total population.

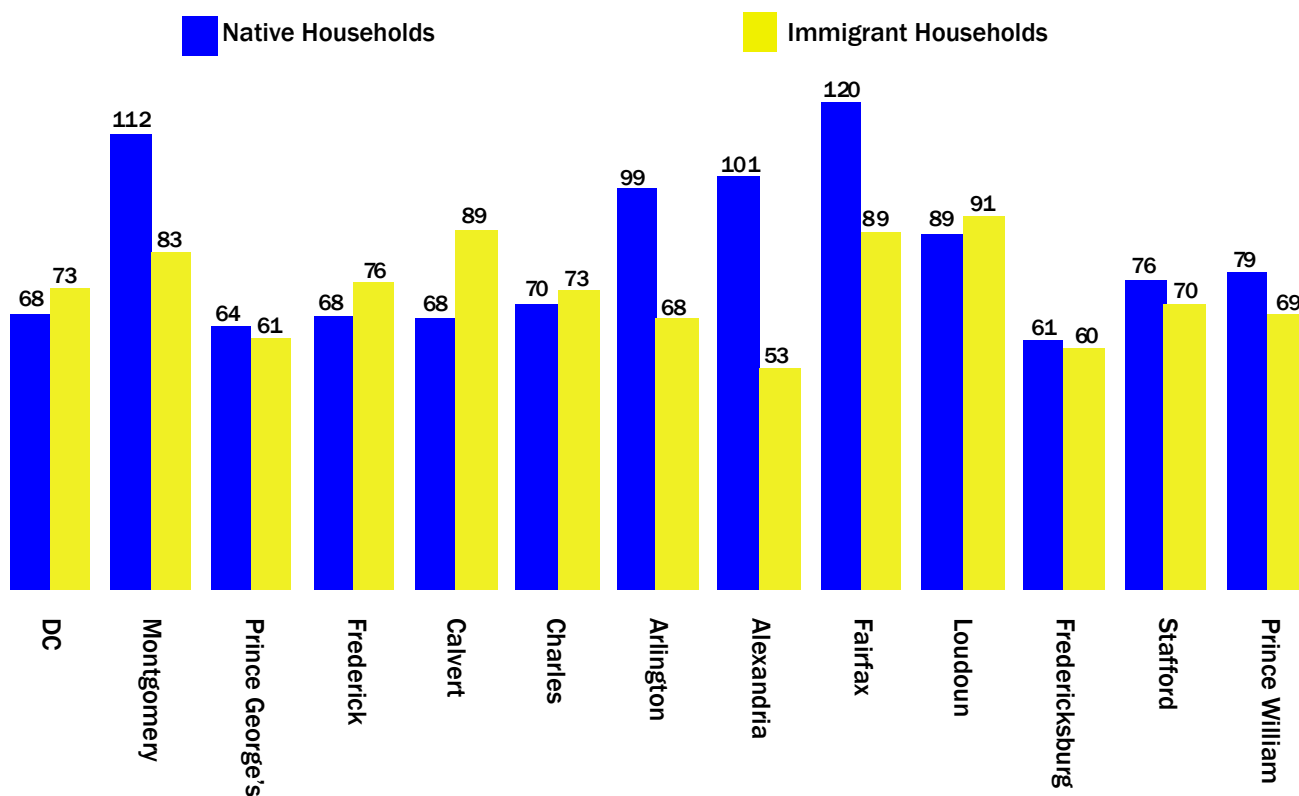
Fairfax and Montgomery counties have among the highest average household incomes for both

immigrants and natives, and that is another part of the explanation for the large share of taxes collected from these jurisdictions. Immigrant households in these two jurisdictions had average 1999 incomes (\$89,000 in Fairfax and \$83,000 in Montgomery County) that were higher than the average for the region (\$78,000). Native households, however, had far higher incomes, averaging \$120,000 in Fairfax and \$112,000 in Montgomery County—these were by far the highest average incomes of any jurisdiction in the region (figure 10). The relative affluence of these two districts, combined with the sheer size of their foreign-born populations, explains why

immigrants there contribute so much to the region's tax base.

Immigrant households in the next two largest regions had relatively low average 1999 incomes (\$73,000 in the District of Columbia and \$61,000 in Prince George's County), but native households also had lower than average incomes in these jurisdictions. In fact, the average income of native households in the District of Columbia (\$68,000) was actually lower than that for immigrant households. Immigrant households also had incomes higher than native households in many outlying jurisdictions (such as Frederick, Calvert, Charles, and Loudoun counties).

Figure 10: Mean Household Income for Immigrant and Native Households in the Washington, DC, Metropolitan Area, by Jurisdiction, circa 1999–2000 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Income figures are reported for the year before the census was taken (1999) and are shown here rounded to the nearest thousand dollars.

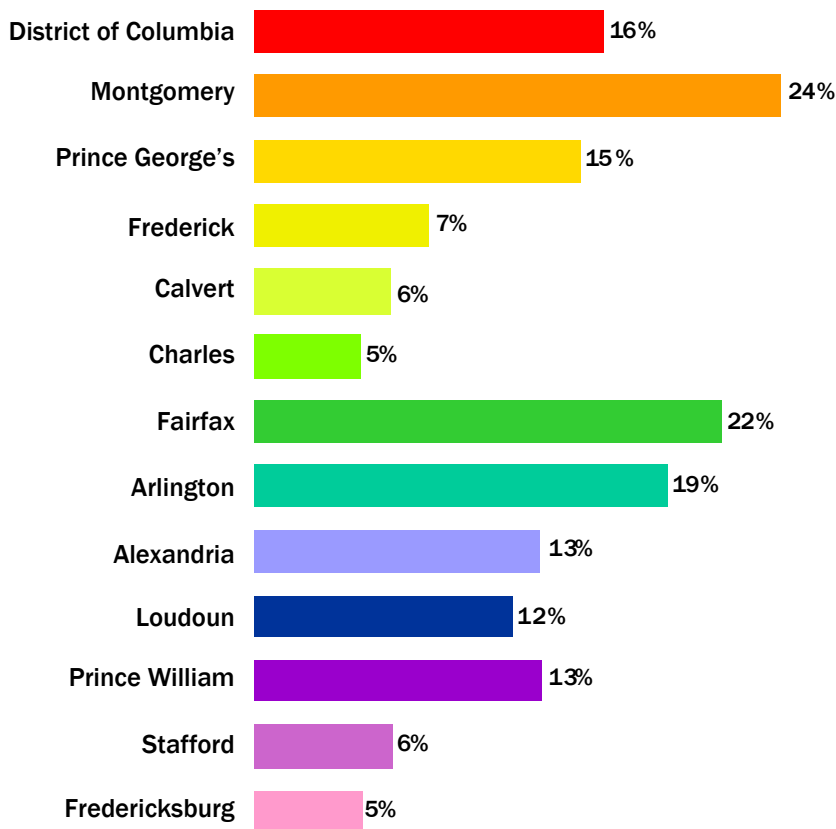
The relative gap in native versus immigrant household income was highest, by contrast, in Arlington and Alexandria, which are home to relatively affluent native but relatively poor immigrant populations. In Alexandria, the average income for immigrant households (\$53,000) was only about one-half the average for native households (\$101,000) in 1999.

Immigrant households pay taxes in rough proportion to their shares of all households in those jurisdictions where their incomes are about the same as natives, but a lower proportion where they have substantially lower incomes. In 1999–2000, immigrant households paid nearly one-quarter of all taxes paid by households in Montgomery and Fairfax counties (figure 11).

However, their share of taxes paid was below their share of all households in these two jurisdictions (24 versus 31 percent in Montgomery County and 22 versus 28 percent in Fairfax County). The large gap in income between immigrant and native households in these jurisdictions is part of the explanation. Immigrant households made up 28 percent of all households in Arlington but only paid 19 percent of taxes generated there. In Alexandria, they represented 25 percent of all households but only paid 13 percent of all taxes. The gap between immigrants' share of taxes paid and their share of households is largest in Arlington and Alexandria because the gap in income between immigrants and natives is largest in these two jurisdictions.

Immigrant households pay taxes roughly equivalent to their shares of all households in the other jurisdictions, where their incomes are similar to that of native households. In the District of Columbia, immigrants paid the same share of taxes as their share of households in 2000 (16 percent). Immigrant households also paid taxes in the same proportion as their share of all households in many of the outlying counties, including Loudoun (12 percent), Frederick (7 percent), Charles (5 percent), and Fredericksburg (5 percent). In Prince George's County, immigrant households paid 15 percent of taxes and made up 13 percent of all households, and in Prince William County they paid 13 percent of taxes and made up 16 percent of all households.

Figure 11: Share of Taxes Paid by Immigrant Households in the Washington, DC, Metropolitan Area, by Jurisdiction, circa 1999–2000



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Employer contributions to Social Security and Medicare are excluded from total taxes when calculating share of household income.

Immigrant households pay taxes roughly according to their share of the population, whether these are taxes at the federal, state, or local level. In fact, immigrant households tend to pay a slightly higher share of local taxes than state or federal taxes, because the local taxes are less progressive. In 1999–2000, immigrant households paid 17 percent of all federal and state taxes paid by households; they paid 20 percent of local taxes (appendix table 8). In the District of Columbia, immigrant households paid 16 percent of federal and state but 17 percent of local taxes. In suburban Maryland, they paid 18 percent of federal and state but 19 percent of local taxes. In Northern Virginia, they paid 17 percent of federal, 18 percent of state, and 21 percent of local taxes. Thus, immigrants contribute most strongly to the tax base at the local level.

Immigrants living in suburban Washington jurisdictions also contribute strongly to the tax bases of both Maryland and Virginia. In 1999–2000, immigrant households living in the

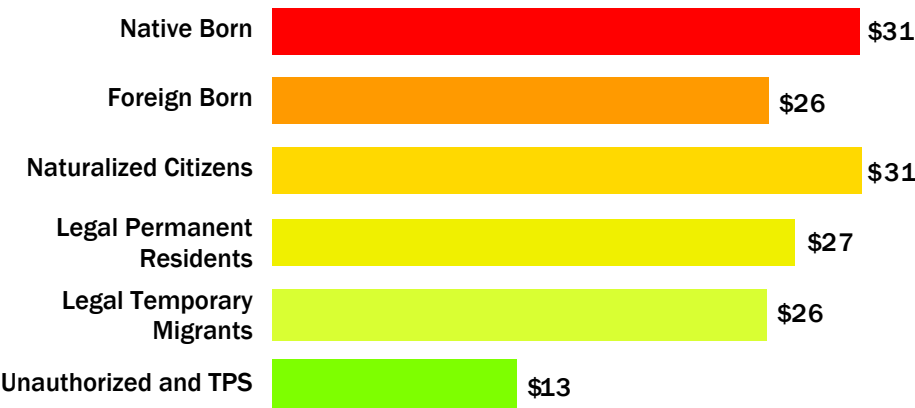
suburban Maryland counties paid \$560 million in state taxes; this was equivalent to 8 percent of the \$6.2 billion in state taxes paid by all households across the state of Maryland. Similarly, immigrant households in Northern Virginia paid about 8 percent of all household taxes collected by the state of Virginia (\$810 million out of \$9 billion). When immigrant households in jurisdictions outside the Washington metropolitan area were included in the total, the share of state taxes paid by immigrant households was 11 percent in Virginia and 12 percent in Maryland.

Variation in Tax Payments by Household Citizenship and Legal Status

Households headed by naturalized citizens pay roughly the same amount in taxes and at the same rate as households headed by U.S.-

born citizens in the Washington metropolitan area. In 1999–2000, the average immigrant household headed by a naturalized citizen paid \$31,000 in taxes—the same as the average native-headed household (figure 12). Naturalized citizen households paid 30 percent of their income in taxes, only slightly below the rate for native households (31 percent, see appendix table 6). Naturalized citizens are the immigrants who have been in the United States the longest and are the best integrated: they must in most cases reside in the country at least five years and take an English language test before becoming citizens. As shown earlier in the report, naturalized citizen households also have the highest income among immigrant households—their average income actually exceeded that for native-born households in 1999. Thus, the most integrated immigrants pay taxes at the same rate as natives, suggesting that over time immigrants’ tax contributions will reach parity with natives.

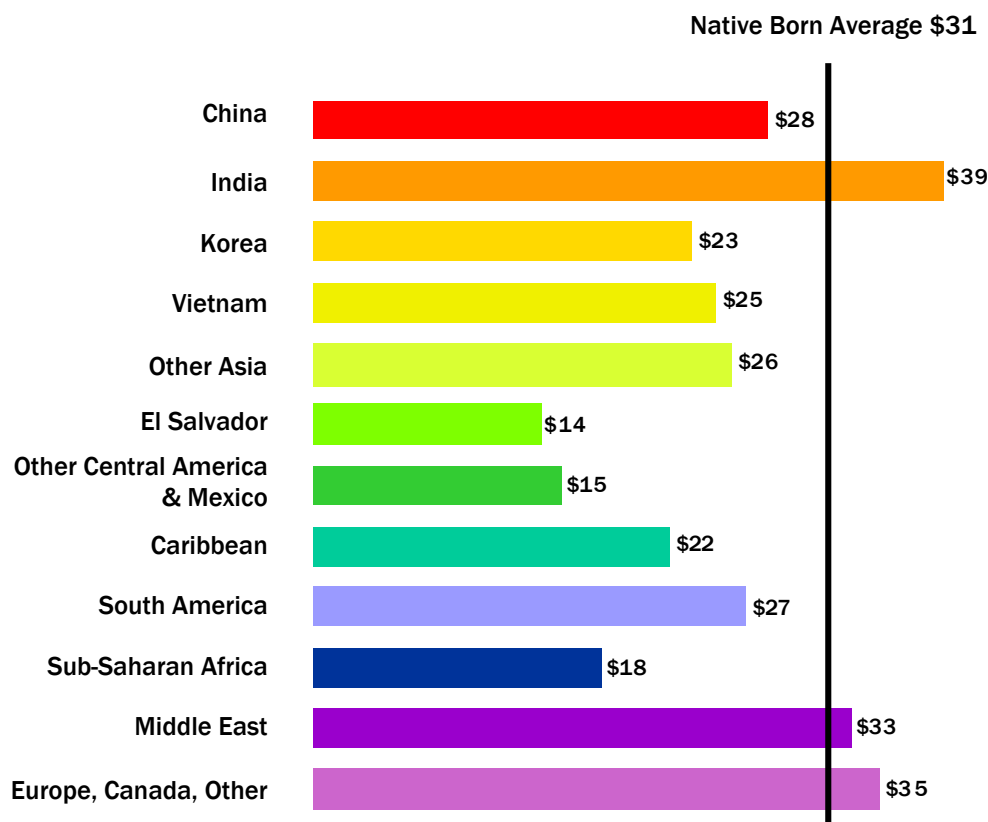
Figure 12: Taxes Paid by Households in the Washington, DC, Metropolitan Area, by Household Nativity, Citizenship, and Legal Status, circa 1999–2000 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Average tax payment rounded to the nearest thousand dollars.

Figure 13: Taxes Paid by Households in the Washington, DC, Metropolitan Area, by Household Place of Birth, circa 1999–2000 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Average tax payment rounded to the nearest thousand dollars.

Legal permanent residents (i.e., green card holders) and legal temporary migrants—students and temporary workers—also pay taxes at nearly the same rate as natives. In 1999–2000, households headed by permanent residents paid \$27,000 in taxes, while those headed by temporary migrants paid \$26,000. The average income of these households was substantially below that for native households in 1999. Permanent resident households paid 29 percent of their income in taxes—just below the rate for native households—while temporary migrant households paid 33 percent of their income in taxes—higher than the rate for natives.

Unauthorized and Temporary Protected Status (TPS) migrants are the one group of immigrants

that pay substantially lower amounts of tax. In 1999–2000, households headed by unauthorized and TPS migrants paid \$13,000 in taxes, and this amounted to only 19 percent of their average household income. Unauthorized and TPS migrants had substantially lower average incomes in 1999 (\$53,000 versus \$78,000 for immigrant households overall). Furthermore, our tax models assume that only 55 percent of unauthorized individuals pay Social Security, Medicare, and federal, state, and local income taxes, since many of them work under the table and are paid in cash.¹⁶ Our tax models also assume that 100 percent of

the unauthorized pay the other taxes we calculated, because these other taxes are not collected through employment. Nonetheless, the likelihood that many unauthorized immigrants do not pay income or payroll taxes substantially reduced their tax contributions at the federal, state, and local levels. Thus, any extension of legal work authorization—whether temporary or permanent—would give a substantial boost in tax revenue to jurisdictions across the Washington metropolitan area.

¹⁶ We assume, however, that 100 percent of TPS migrants pay these taxes, because all TPS migrants have temporary work authorization.

Variation in Tax Payments by Immigrants' Country and Region of Origin

Immigrants from several different regions of the world pay higher taxes than natives in the Washington, DC, area. In 1999–2000, the largest average tax contribution came from households headed by Indian immigrants (\$39,000), followed by European and Canadian immigrant households (\$35,000) and Middle Eastern immigrant households (\$33,000); all these households paid higher taxes on average than native households (figure 13).

The share of income paid in taxes was also just about as high for Indian immigrants (30 percent), European and Canadian immigrants (30 percent), and Middle Eastern immigrants (31 percent) as it was for native households (appendix table 6). Average tax payments from

households headed by Chinese immigrants (\$28,000) and South American immigrants (\$27,000) were not far below the average for natives; they paid 28 and 29 percent of their income in taxes, respectively. The pattern of tax payments by region of origin reflects the pattern of household income, as Indian, European, and Middle Eastern immigrants had the highest average household incomes in 1999.

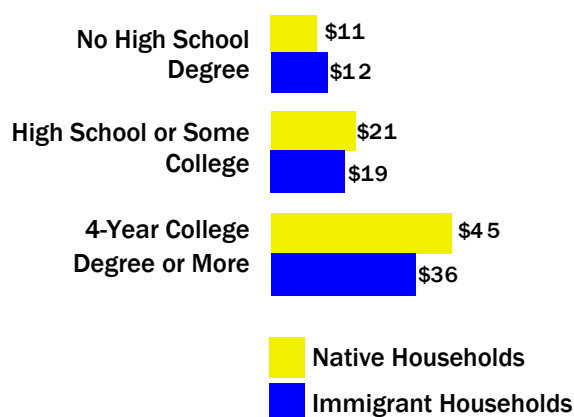
The lowest tax payments came from households headed by immigrants from El Salvador, other Central American countries, sub-Saharan Africa, and the Caribbean. In 1999–2000, Salvadoran households paid \$14,000 in taxes, and other Central American households paid \$15,000; both these groups paid 21 percent of household income in taxes, on average. Sub-Saharan African immigrant households paid \$18,000 and about 26 percent of their average income in taxes, while Caribbean households paid \$22,000 (28 percent of their income in taxes). Once again, the pattern of tax payments matches

the pattern of income, with the lowest-income households paying the lowest share of their income in taxes. Moreover, immigrants from El Salvador and other Central American countries pay a still lower share of taxes because a relatively high share of this population is unauthorized, and the unauthorized pay a relatively low share of income in taxes.

Variation in Tax Payments by Educational Attainment and English Proficiency

Differences in human capital are also strongly associated with variations in income and tax payments, as households headed by well-educated immigrants who speak English have the highest incomes and tax payments. In fact, the variation by education in income and tax payments follows the same pattern for both native and immigrant households in the national capital region (figure 14).

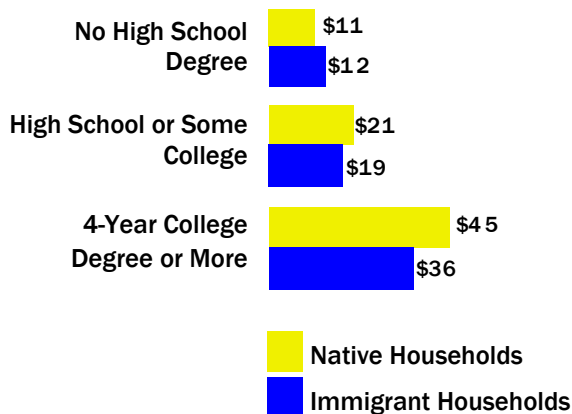
Figure 14: Mean Household Income by Nativity and Educational Attainment of Household Head, Washington, DC, Metropolitan Area, 1999 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Income figures are reported for the year before the census was taken (1999) and are shown here rounded to the nearest thousand dollars.

Figure 15: Average Tax Payment for Households in the Washington, DC, Metropolitan Area, by Nativity and Educational Attainment of Household Head, circa 1999–2000 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Tax payments are rounded to the nearest thousand dollars.

In 1999, households headed by immigrants with a four-year college degree or more education had an average income of \$103,000, more than twice the income for households headed by immigrants without a high school degree (\$47,000). Native households show an even steeper income gradient across educational categories: average income was about three times as high as for households headed by natives with a college degree (\$122,000) as those headed by high school dropouts (\$39,000). Among households headed by college graduates, average income was substantially higher for native households than immigrant households. On the other hand, average income was actually higher for immigrants than natives in households headed by high school dropouts.

Tax payments follow a similar pattern: better-educated households pay higher taxes whether they are headed by immigrants or natives. In 1999–2000, the average tax payment was three times as high for households headed by immigrants with a four-year college degree as

for those headed by immigrants without a high school degree: \$36,000 versus \$12,000 (figure 15). Among native households, those headed by college graduates paid four times as much tax on average as those headed by high school dropouts (\$45,000 versus \$11,000). Differences in tax payments between native and immigrant households were very slight at lower levels of educational attainment; the only substantial difference was among the best educated—and highest income—group of households.

The share of income paid in taxes was also similar between immigrant and native households within these educational attainment categories. Immigrant households headed by college graduates paid 30 percent of their income in taxes, versus 32 percent of comparable native households (appendix table 9). At the other end of the spectrum, immigrant households headed by high school dropouts paid 21 percent of income in taxes, compared with 24 percent of native households. A significant share of the least-educated immigrant households were unauthorized and therefore

less likely to pay income and payroll taxes.

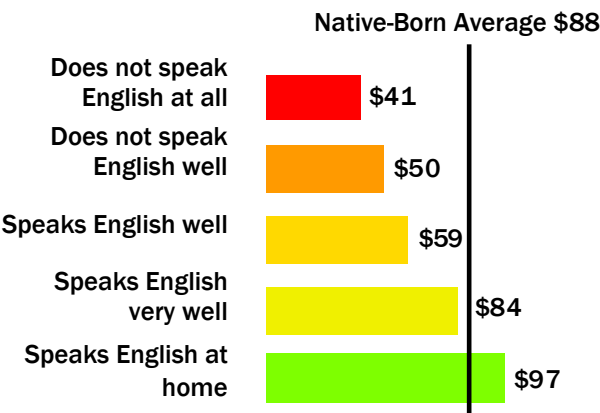
Income and tax payments also vary significantly by the English proficiency of immigrants, as English proficiency is an important measure of immigrant integration and correlates strongly with educational attainment. In 1999, average income for English-speaking immigrant households exceeded that for natives: \$97,000 versus \$88,000 (figure 16). The average income for households headed by immigrants who speak another language but also speak English very well (\$84,000) was almost as high as that for native households, but income dropped off steeply with declining English proficiency.¹⁷ The average income for immigrant households whose heads did not speak English at all (\$41,000) was less than one-half the average for

¹⁷ The census asks respondents whether English or another language is spoken in the home, and for all those households where another language is spoken, how well each household member over age 5 speaks English. We did not analyze data on English proficiency for native households, because English is the language spoken in almost all native households.

those whose head spoke English very well.

Due to their relatively high incomes, English-speaking immigrant households pay more in taxes than native households in the Washington, D.C., area. In 1999–2000, English-speaking immigrant households on average paid \$34,000 in taxes, versus \$31,000 for native households (figure 17). Immigrant households whose heads spoke English very well paid \$29,000 in taxes, near the average for natives. Tax payments for immigrant households whose heads did not speak English well (\$13,000) or at all (\$11,000) were only about one-third to one-half as high as taxes paid by households with higher levels of English proficiency. English-speaking immigrant households paid 30 percent of their income in taxes, compared with 29 percent for households whose head spoke English very well and just 21 percent for those whose head did not speak English well or at all (appendix table 9). Thus, immigrants’ income and tax payments vary greatly with their English proficiency and educational attainment.

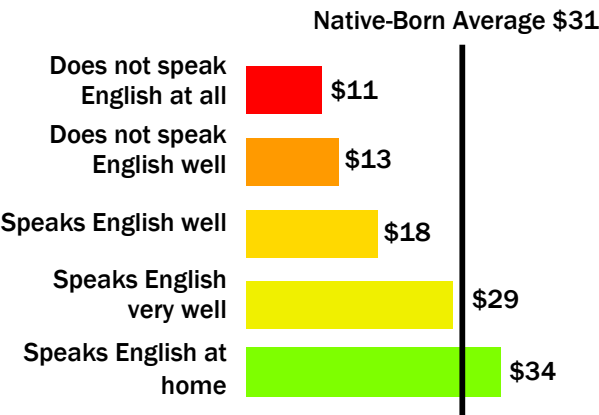
Figure 16: Mean Income for Immigrant Households in the Washington, DC, Metropolitan Area, by English Proficiency of Household Head, 1999 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Income figures are reported for the year before the census was taken (1999) and are shown here rounded to the nearest thousand dollars.

Figure 17: Average Taxes Paid for Immigrant Households in the Washington, DC, Metropolitan Area, by English Proficiency of Household Head, circa 1999–2000 (Thousands of Dollars)



Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

Note: Tax payments are rounded to the nearest thousand dollars.

***In 1999-2000,
immigrant households
in the Washington, DC
metropolitan area paid
nearly \$10 billion, or
about 18 percent of all
taxes paid by
households in the
region—roughly in
proportion to their
share of the
population...
immigrants have, on
the whole, successfully
integrated into the
region's economy and
tax base.***

Conclusions

Immigrant households in the Washington, D.C., metropolitan area paid nearly \$10 billion, or about 18 percent of all taxes paid by households in the region (\$55 billion), in 1999–2000.

Immigrants at that time represented about 18 percent of the total population, so they paid taxes roughly in proportion to their share of the population. Immigrants paid almost one-quarter of all household taxes generated in the region's two largest jurisdictions—Fairfax and Montgomery counties—but they contributed substantially to the tax bases of the region's smaller jurisdictions as well. Moreover, immigrant households in suburban Maryland and Northern Virginia contributed a significant share of all household taxes collected by the states of Maryland and Virginia (about 8 percent in each state). Since 2000, the number of immigrants has grown dramatically—from 850,000 to over 1.2 million—and their share has increased from 18 to 20 percent of the region's population, suggesting their tax contributions have risen dramatically as well.

The Washington metropolitan area has attracted a diverse group of immigrants from regions across the globe, and has been particularly successful in attracting well-educated immigrants.

Immigrants have on the whole successfully integrated into the region's economy and tax base. The average household income for immigrants in 1999 was almost as high as that for natives (\$78,000 versus \$88,000), and average tax payments for 1999–2000 were also similar: \$31,000 for native versus \$26,000 for immigrant households. The most integrated households paid taxes at the same level and rate as native households. This included households where the head had become a naturalized U.S. citizen as well as English-speaking households and those headed by

immigrants with college degrees. These findings suggest several overall recommendations.

Recommendations

- *Continue to welcome immigrants to the Washington, DC, area, because they are a significant and growing part of the region's economy and tax base. A large majority of the region's immigrants are citizens or legal non-citizens, and they pay taxes at nearly the same rate as native households.*
- *Continue support for expansion of biotechnology, telecommunications, and other highly skilled sectors of the economy that attract the highest-paid immigrants.*
- *Allow time for immigrants to become integrated into the region's economy. As immigrants spend more time in the United States, they mostly learn English and become U.S. citizens. Once they become citizens and learn to speak English very well, immigrants pay taxes at the same levels and rates as U.S. natives.*
- *Support English as a second language, adult education, and other workforce development programs for immigrants because they have the potential to raise immigrants' earnings and tax contributions. The additional state and local tax contributions of immigrants who obtain higher-paying jobs will more than offset the costs of these programs.*
- *Grant temporary or permanent work authorization to undocumented immigrants, because, once authorized, a higher share would pay income and payroll taxes. State and local governments would also benefit because their income tax collections would increase alongside those of the federal government.*

Methodology

The general approach used in this study is a microsimulation of tax payments for 1999–2000 using data for individuals and households from the 2000 Census. We use a tax simulator for estimating income tax payments and models developed from survey data as a basis for sales and other consumption taxes. Administrative data on taxes collected from federal, state, and local governments provide control totals to ensure that the estimates for each tax, totaled by area across all households, conform to actual tax collections.

We estimate three broad categories of tax payments, and our estimates are developed for individuals, families, and households depending on the appropriate unit for each tax. Payroll taxes (Social Security and Medicare) are estimated on an individual basis from information on wage and salary income as well as self-employment income. Income taxes (federal, state, and local) are estimated for “filing units” (essentially nuclear family units) using estimates of taxable income based principally on the census but augmented to account for income undercounted by the census. Property and consumption tax estimates are modeled at the household level based on income data from the census and consumption models developed from the Consumer Expenditure Survey (CES). Consumption taxes we estimate at the state and local level—depending on the jurisdiction—include general sales, automobile, alcohol, and tobacco. The tax estimates for all three categories are aggregated to households and reported at this level.

Income and tax estimates are reported for geographic areas identifiable in the census data that correspond to political jurisdictions within the Washington, D.C., metropolitan area. The District of

Columbia is covered as a single area in the study. We report taxes for the following Virginia jurisdictions: Alexandria City, Arlington County, Fairfax County (including the cities of Fairfax and Falls Church), Fredericksburg City (including Spotsylvania and Culpeper counties), Loudoun County (including Fauquier, Clarke, and Warren counties), Stafford County (including King George County), and Prince William County (including Manassas and Manassas Park cities). The Maryland areas reported are Prince George’s County, Montgomery County, Frederick County, Charles County, and Calvert County (including St. Mary’s County).

The main goal of the study is to estimate all types of taxes in all jurisdictions for households headed by immigrants, and to compare their tax payments with payments of households headed by natives. We also calculate tax payments separately for immigrant households according to their citizenship and legal status, country or region of birth, educational attainment, and English proficiency.

Data Obtained from the 2000 Census

The population data for this study come from the 2000 Census 5-percent Public Use Microdata Sample (PUMS). This data set has individual records for a 5-percent sample of the entire U.S. population with detailed information on demographic and housing characteristics. Although not the most recent data, the 5-percent PUMS is by far the largest data set available, making disaggregation by jurisdiction and country of origin possible. While the Census Bureau’s American Community Survey, which started in 2003, has collected comparable and more recent data, sufficient data to produce analyses similar to those described in this report will not be available until at least 2008.

The census is divided into household and individual-level records. We used the following household-level items for this study, mostly to calculate property taxes:

- housing tenure (owner/renter)
- type of housing
- mortgage payments
- rent
- estimated housing value
- household spending on mortgages, utilities, insurance, and property taxes

The individual-level socioeconomic data we used include the following:

- age
- sex
- marital status
- race/Hispanic origin
- relationship to household head
- school enrollment
- information on employment, including labor force participation, class of worker (self-employed or working for an employer), occupation, and industry of employment

We estimated income and payroll taxes as well as consumption patterns—used later to calculate consumption taxes—based on the eight types of income counted in the census:

- wages, salary, commissions, bonuses, and tips
- net self-employment income
- interest, dividends, net rental income, royalty, estate, and trust income
- Social Security income including disability and Railroad Retirement
- Supplemental Security Income (SSI)
- public assistance income (other than SSI)
- retirement, survivor, or disability income (other than Social Security)
- all other income.

Assignment of Legal Status to Non-citizens in the Data

The census provides some information related to immigration and citizenship, but no direct information on the legal status of non-citizens. The data items in the census include:

- country of birth;
- year of entry to the United States; and
- U.S. citizenship (but not legal status).

Starting with these census data items, the methodology for developing estimates by legal status proceeds in several steps. (See Passel, Van Hook, and Bean 2004, 2006; and Passel and Clark 1998.) The first stage involves making demographic estimates of legal foreign-born residents of the United States and key states. These estimates are discounted for legal immigrants omitted from the census and compared with the initial census-based estimates of the immigrant population to measure the number of unauthorized migrants included in the census. In this variant of the “residual” methodology, we produce estimates by age, sex, and period of entry for 35 countries or regions of birth and six states (California, New York, Texas, Florida, Illinois, and New Jersey) and the balance of the United States. Using information on census coverage of the foreign-born population, we inflate the estimates of legal and unauthorized migrants for census omissions to produce estimates of those included in the census as well as the total number in the country (or state), by up to 10 percent.

Note that there are no special control totals or estimates for immigrant populations for the Washington, D.C., metropolitan area. The control totals used in this report are based on our national-level estimates.

In the second stage of the estimation process, individual respondents in the census are assigned to various legal statuses using a combination of (1) deterministic edits based on correspondence between the respondent’s characteristics and admission criteria; (2) inference based on family relationships, country or region of origin, and date of arrival in the United States; and (3) probabilistic methods. The iterative assignment process produces national and state-level estimates consistent with the demographic estimates of legal and unauthorized migrants included in the census. After the assignment process, each individual in the census has been assigned to one of the following statuses:

1. Legal permanent resident (LPR) alien—persons admitted for permanent residence or persons with “green cards.”
2. Naturalized citizens—persons admitted as LPRs who have acquired U.S. citizenship through the naturalization process.
3. Refugee and asylee aliens—persons admitted for permanent residence as refugees or through the asylum process (regardless of current status).
4. Naturalized refugees and asylees—persons admitted as refugees or through the asylum process who have acquired U.S. citizenship through the naturalization process.
5. Legal temporary migrants or “non-immigrants”—persons legally admitted on a temporary basis for specified periods and specified purposes (including work). The largest groups of non-immigrants are foreign students and various types of “guest workers.” The visa categories counted as legal for the estimates include A, F, G, H-1B, some H-2s, some Js, L, M, N, O, P, and R.

6. Unauthorized migrants—foreign-born persons who are not in groups 1–5. This category includes individuals who entered the U.S. clandestinely, individuals who entered with fraudulent documents, and individuals who entered legally but either overstayed the period of their visa or otherwise violated their terms of admission.
7. U.S. natives—persons born as U.S. citizens regardless of place of birth. Most are born in the United States or its territories or possessions, but some are born in foreign countries to parents who are U.S. citizens.

The assignment process is designed so that the number of individuals assigned as legal immigrants (i.e., the sum of groups 1–4) and unauthorized (i.e., group 6) agrees as closely as possible with the demographic and residual estimates at the state level (for six states and the balance of the nation), for adults by sex and children, and for four country-of-birth groups.

The final stage of the estimation process involves adjusting the weights for legal and unauthorized migrants to account for census undercounts of these populations. The adjustment factors are based on the demographic residual estimation process described earlier. The resulting adjusted population figures for legal immigrants and unauthorized migrants are equal to the demographic estimates developed in the first stage of the estimation process. As with the status assignments, the resulting estimates incorporate the demographic estimates for six states and the balance of the nation, for adults by sex and children, and for four country-of-birth groups.

Identification of Temporary Protected Status Migrants.

Persons with Temporary Protected Status (TPS) are included among the persons classified as “unauthorized migrants” in the

estimation and assignment process. Persons with TPS generally entered the United States without permission, but have been allowed to stay in the country on a temporary basis because conditions in their home country are too dangerous or unstable for them to return. In the late 1990s, the U.S. Congress granted TPS to immigrants from Central America who fled civil wars and natural disasters. TPS was renewed for about 300,000 immigrants—mostly from El Salvador and Honduras—in early 2006 (U.S. Citizenship and Immigration Services 2006). The share of immigrants who have TPS is relatively high in Washington, D.C., because the region has so many immigrants from Central America.

The various TPS populations have not been classified as “legal” because deficiencies in the available data do not permit accurate tracking of the groups and because of other measurement problems. Further, individuals with TPS would generally want to take part in the various legalization programs being proposed in 2006. However, the TPS group differs from almost all other unauthorized migrants in that the TPS group has work authorization and is “known” to immigration officials. Some observers (e.g., Martin 2005) have argued that the TPS group should be grouped with the LPRs and refugees.

Immigrants from El Salvador are the single largest group of TPS migrants as well as the largest single nationality group in the Washington, D.C., metropolitan area. We estimated the number of immigrants with TPS in the Washington, D.C., area based on the share of unauthorized immigrants from El Salvador that we believe had TPS in 2000. The U.S. Immigration and Naturalization Service (2002) estimated that, nationally, about 263,000 immigrants from El Salvador had applied and were eligible for TPS as of June 10, 2002. This amounted to 54 percent of all the immigrants from

El Salvador to whom we assigned unauthorized status to in our 2002 data from the U.S. Current Population Survey. We applied this percentage to the number of unauthorized immigrants from El Salvador we calculated in the 2000 Census (65,000) to get an estimate of 35,000 TPS migrants. We consider the remaining 30,000 unauthorized. In the census data for the Washington region, there were very small numbers of unauthorized migrants from other countries that were eligible for TPS (less than 1,000), and so we did not develop an estimate of the TPS population for any other country.

Despite their legal work authorization, we group Salvadorans who may have TPS with the unauthorized migrants. They require some special treatment in some of the tax estimation procedures noted below.

Assignment of Household Nativity, Citizenship, and Legal Status

Since we aggregate taxes to the household level, we created a summary variable for the nativity and legal status of each household based on the nativity and legal status of the household head and the spouse (if one is present). The assignments proceed hierarchically such that each household is assigned a single nativity/status group even if the household contains adults with different legal statuses. The hierarchy is as follows:

- unauthorized migrant—either the head or spouse is unauthorized;
- refugee alien—either the head or spouse is a refugee alien, but neither is unauthorized;
- legal permanent resident (LPR) alien;
- naturalized refugee;

- naturalized non-refugee;
- native;
- legal temporary migrant or nonimmigrant—both head and spouse (if present) are legal temporary migrants.

Thus, a household where the head is a naturalized citizen and the spouse is a refugee is classified as a refugee household. However, if the spouse is native, then the household is classified as a naturalized citizen household. In most households with spouses, the spouse has the same status as the household head. Virtually all native households with spouses have native spouses. If the head is native and the spouse is foreign-born, the household is given the status of the spouse in all cases except those with legal temporary migrant spouses. However, for individual cases where a foreign-born individual has characteristics that would lead to classification as a legal temporary migrant, marriage to a native would lead to classification of the migrant as something other than a legal temporary migrant.

Assignment of Region and Place of Birth to Immigrant Households

We also disaggregate tax payments by immigrants’ place of birth in this report. In order to do this, we calculate a “household country of birth” based on the country of birth of either the household head or spouse. The household country of birth is the head’s country of birth *if and only if* the head’s legal status is the same status that we assigned to the entire household. If the household head has a different legal status than the household overall, then the household’s country of birth is the spouse’s country of birth, since, by definition, we assigned the spouse’s legal status to the entire household in such cases. For example, an LPR household with the head born in Mexico and the

spouse in El Salvador is classified as a Mexican LPR household if the head is an LPR. On the other hand, an LPR household where the head is Mexican but a naturalized citizen and the spouse is an LPR born in El Salvador would be classified as an LPR household and assigned El Salvador as the household country of birth.

Identification of Geographic Areas

Metropolitan areas are defined by the Census Bureau according to standards defined by the Office of Management and Budget (OMB). For the District of Columbia, Maryland, and Virginia, the geographic “building blocks” for metropolitan areas are counties and, in some cases in Virginia, independent cities. In this report, we use the term “Washington, D.C., metropolitan area” to refer to the Census Bureau’s “Washington-Arlington-Alexandria, DC-VA-MD-WV Metropolitan Statistical Area.” The jurisdictions included in the metropolitan area are listed to the right.

In the 2000 Census 5-percent PUMS, it is not possible to delineate areas that correspond exactly to the metropolitan area. Each household in the 5-percent PUMS is assigned to a Public-Use Microdata Area (PUMA). The PUMAs in the 5-percent data are defined so that each contains at least 100,000 persons. Each PUMA is wholly contained within a single state. In general, PUMAs consist of census tracts within a single county (or city), entire counties (or cities), or aggregations of entire counties (or cities). In this report, we aggregate PUMAs to correspond approximately to political jurisdictions within the Washington, D.C., metropolitan

Washington-Arlington-Alexandria, DC-VA-MD-WV Metropolitan Statistical Area

Washington-Arlington-Alexandria, DC-VA-MD-WV Metropolitan Division

District of Columbia, DC
Calvert County, MD
Charles County, MD
Prince George’s County, MD
Arlington County, VA
Clarke County, VA
Fairfax County, VA
Fauquier County, VA
Loudoun County, VA
Prince William County, VA
Spotsylvania County, VA
Stafford County, VA
Warren County, VA
Alexandria City, VA
Fairfax City, VA
Falls Church City, VA
Fredericksburg City, VA
Manassas City, VA
Manassas Park City, VA
Jefferson County, WV

Bethesda-Frederick-Gaithersburg, MD Metropolitan Division

Frederick County, MD
Montgomery County, MD

(See <http://www.census.gov/population/estimates/metro-city/03mfips.txt> for definitions and constituent areas.)

area. (See U.S. Census Bureau 2005 for description of the PUMS and definitions of the areas and variables; maps of PUMAs are available at

<http://www.census.gov/geo/www/maps/puma5pct.htm>.) The 13

reporting areas we use, their constituent political jurisdictions, and corresponding PUMAs are defined to the right.

These areas form a very close approximation to the officially designated Washington, D.C., metropolitan area. We omit Jefferson County, WV, which had a population of 42,190 according to 2000 Census Summary File 3 data available on *American Factfinder* (U.S. Census Bureau n.d.). The areas designated as “**” in the list to the right (St. Mary’s County, MD; Culpeper County, VA; and King George County, VA) are not part of the official Washington, D.C., metropolitan area. These three counties contain 132,276 persons according to the Summary File 3 data. Thus, the official metropolitan area contains 4,796,183 persons whereas our areas contain 4,891,269 persons or 2.0 percent more people. The correspondence for the foreign-born population is even closer because the marginal areas contain very few immigrants. Our reporting areas include 832,284 immigrants according to the census tabulations, or only 0.4 percent more than the 829,310 in the official area.

Estimation of Income Taxes

Federal and state income taxes were estimated using *IncTaxCalc* (Bakija 2004). *IncTaxCalc* is a SAS program that calculates taxes using “all major features of federal and state income tax laws” for years from 1900 through 2002. We used the October 10, 2004, version of the program (with the author’s permission) to compute income taxes with data from the 2000 Census 5-percent PUMS. To ensure that the tax estimates

agreed with official data for taxes paid, we compared the results of *IncTaxCalc* with data from Internal Revenue Service (IRS) data for tax year 1999. These data, from *Statistics of Income (SOI)*, provide data at the state level broken down by reported income level on the number of returns files, types of returns, income reported (by type), amount of deductions and exemptions, and income tax liability (Internal Revenue Service 1999). Use of *IncTaxCalc* requires that the data be arrayed by filing units (not households). The income data reported in the census does not correspond exactly to the types of income reported on tax returns. Further, the aggregate income in the census tends to be less than the amounts of income reported on tax returns, with most of the shortfall coming on high-income and very high income tax returns. Accordingly, we first constructed tax filing units and then allocated the income reported in the census to the types of income required by tax returns. Finally, we adjusted the reported incomes so that the aggregate totals for the District of Columbia, Maryland (the entire state, not just the Washington, D.C., metropolitan area), and Virginia (the entire state) agreed closely with the reported SOI totals. These adjusted incomes were used in the rest of the study to calculate spending patterns and consumption taxes.

Assignment of Tax Filing Units. For tax filing units, we constructed “minimal household units” (MHUs) by editing census data on relationship to household heads and families. Minimal household units are defined as nuclear families—married couples or single individuals with dependent children (if present). MHUs differ from census-defined family units in that related adults other than spouses are considered part of a census family, but not an MHU. For example, a husband, wife, two minor children, and the wife’s mother are considered a single family, but are treated as two

PUMAs

Washington

Washington, DC (State 11)
PUMAs (5): 00101–00105

Montgomery

Montgomery County, MD (State 24)
PUMAs (7): 01001–01007

Prince George’s

Prince George’s County, MD (State 24)
PUMAs (7): 01101–01107

Frederick

Frederick County, MD (State 24)
PUMAs (1): 00300

Calvert

Calvert County, MD (State 24)
St. Mary’s County, MD (State 24)**
PUMAs (1): 01500

Charles

Charles County, MD (State 24)
PUMAs (1): 01600

Arlington

Arlington County, VA (State 51)
PUMAs (1): 00100

Alexandria

Alexandria City, VA (State 51)
PUMAs (1): 00200

Fairfax

Fairfax County, VA (State 51)
Falls Church City, VA (State 51)
Fairfax City, VA (State 51)
PUMAs (5): 00301–00305

Loudoun

Loudoun County, VA (State 51)
Clarke County, VA (State 51)
Fauquier County, VA (State 51)
Warren County, VA (State 51)
PUMAs (1): 00600

Fredericksburg

Fredericksburg City, VA (State 51)
Spotsylvania County, VA (State 51)
Culpeper County, VA (State 51)**
PUMAs (1): 00900

Stafford

Stafford County, VA (State 51)
King George County, VA (State 51)**
PUMAs (1): 00800

Prince William

Prince William County, VA (State 51)
Manassas City, VA (State 51)
Manassas Park City, VA (State 51)
PUMAs (2): 00501–00502

MHUs. (The wife's mother is a separate MHU, but not a separate "family.") In defining MHUs, we treat all children under 18 as part of the parent's MHU regardless of the child's income; also, children up to age 25 who are enrolled in school or have minimal income are treated as part of the parent's MHU. All children (under 18) are placed in an MHU, even if there is no parent in the household. Specifically, such children are considered part of the household head's MHU.

In general, each MHU is a tax filing unit. However, when the children (or other dependents) in an MHU have income of their own, an MHU may file more than one return. In general, the additional returns filed by children are so-called "0-exemption" returns. The tax simulator does not actually simulate all the separate returns filed by minors and does not generate a return for every MHU with no net tax liability.

In the aggregate, the tax simulator generates about 5.9 million returns (weighted) from the 2000 Census 5-percent PUMS for D.C., Maryland, and Virginia combined. This figure is 2.4 percent below the

6.0 million shown by the SOI. The major source of this (small) discrepancy is a shortfall of about 360,000 returns from filing units with incomes less than \$20,000 and, especially, low-income returns from the District of Columbia. Since these filing units have little or no tax liability in the aggregate, this discrepancy in returns does not translate into large discrepancies in tax liability.

The results from *IncTaxCalc* (with the income adjustments noted below) show a simulated federal income tax liability of \$46.6 billion; this figure is only 0.9 percent below the \$47.1 billion in tax payments for the three states from the SOI. The simulated values for each state are also quite close to the SOI data: 0.8 percent higher than the SOI figure for Washington, D.C.; 0.7 percent lower in Maryland; and 1.3 percent lower in Virginia. Only data for the MHUs in PUMAs within the Washington, D.C., metropolitan area are reported in this study.

Adjustment of Reported Income. Several different adjustments are required to the reported income in the census to bring the income data (and the resulting tax

estimates) into line with the data reported in the SOI. First, some types of income are not reported separately in the census. Interest income and dividends are combined into a single census income type, but they are reported separately and receive different treatment in the tax code. Capital gains income is not collected separately in the census; it is sometimes grouped with interest income and is often ignored in census reports. Several types of income (including wage and salary income) are "top coded" in the census. As a result, the aggregates of these incomes fall well short of the adjusted gross income (AGI) reported by SOI. A number of different assumptions were tested before we arrived at the following adjustment to reported income.

Interest, Dividends, and Capital Gains. In the aggregate, these three types of income are severely underreported in the census. Further, they are not disaggregated. To bring the totals in line with the SOI reports and to generate the different types of income, we made the following adjustments to the census data:

Multiplier for Total Interest, Dividend, Capital Gains:

<u>Adjusted Gross Income</u>	<u>DC</u>	<u>MD</u>	<u>VA</u>
<\$30K	1.8	2.0	1.7
\$30-150K	1.9	1.4	1.5
\$150K+	2.1	2.6	2.45

Allocation of Adjusted Total to Types of Income:

<u>Type of Income</u>	<u>DC</u>	<u>MD</u>	<u>VA</u>
Interest	41%	44%	48%
Dividend	23%	22%	21%
Capital Gains	35%	34%	31%

Business (Self-Employment) Income. There is considerably more self-employment income reported in the census than on federal income tax returns. Accordingly, the reported incomes must be adjusted downward to agree with the SOI figures:

Multiplier (Discount Factor) for Total Self-Employment Income:

<u>Adjusted Gross Income</u>	<u>DC</u>	<u>MD</u>	<u>VA</u>
<\$30K	.86	.82	.84
\$30-150K	.61	.61	.63
\$150K+	.48	.47	.48

Wage and Salary Income. Again, adjustments are required to bring the census figures in line with the SOI totals. At the lower end of the income spectrum, the adjustments account for a small degree of over-reporting in the census:

Low-end Wage and Salary Income Multipliers:

<u>Adjusted Gross Income</u>	<u>DC</u>	<u>MD</u>	<u>VA</u>
<\$30K	1.0	.95	.95
\$30–75K	.95	.95	.95
\$75–150K	1.0	1.0	1.0
\$150–300K	1.1	1.0	1.0

Then, filing units with AGI in the range of \$75,000–\$150,000 had their wage and salary income increased somewhat and “spread out” using the following adjustments:

For DC & Virginia:

New W&S = \$75,000 + 2.0 * (Old W&S – \$75,000) if Old W&S > \$75,000;
New W&S = Old W&S if Old W&S <= \$75,000.

For Maryland:

New W&S = \$75,000 + 1.25 * (Old W&S – \$75,000) if Old W&S > \$75,000;
New W&S = Old W&S if Old W&S <= \$75,000.

At the high end of the income distribution, however, two factors must be taken into account. First, there is a significant amount of underreporting, so the incomes must be adjusted upward. Second, because of “top-coding,” there is a clustering of incomes at a single value. Accordingly, the second part of the adjustment process is designed to spread out the high incomes across a broader range of values. The adjustment changes the census wage and salary income to a new value defined as:

$$\text{New W\&S} = \$100,000 + \text{Multiplier} * (\text{Old W\&S} - 100,000)$$

The multipliers vary from 0.333, which reduces wage and salary income to a value close to where the top coding begins, at \$175,000, to 25.0, which produces wage and salary income near \$6 million. For each jurisdiction, this adjustment is applied to units with (a) initial AGI of \$300,000–\$500,000; and (b) wage and salary income of \$300,000–\$450,000. The multiplier for each filing unit that qualifies for an adjustment is chosen using a random number. The average multiplier is about 1.4.

Tax Compliance of Unauthorized Immigrants. For native and legal immigrant MHUs, all filing units with an estimated tax liability are

assumed to file tax returns. However, for unauthorized migrant MHUs, we assume that only some of the taxes are paid. There is a significant amount of evidence that many unauthorized migrants are working “on the books.” That is, they have Social Security numbers, either fake or belonging to someone else, and are having income and payroll taxes taken out of their wages. This evidence includes an accumulation of tax payments in accounts with the Social Security Administration that either do not exist or where the name of the filer does not match the name on the account. In addition, a significant number of income tax returns are being filed with Individual Tax Identification Numbers, but these numbers do not entitle the holder to work in the United States (Milbourn 2004). See Camarota (2004) and Passel and Clark (1998) for a review of the evidence.

Based on the available evidence and values used by similar studies, we assumed that 55 percent of unauthorized migrant filing units actually paid income taxes (and had payroll taxes withheld). We implemented this adjustment through the weights used to compute aggregate taxes paid and the average income tax payment per household or payroll tax per individual. Because a significant share of the migrants we classified

as “unauthorized” in the Washington DC, area are actually authorized to work because they have TPS, we adjust the compliance rate for unauthorized filing units from El Salvador. For immigrants with TPS, we assume a compliance rate of 100 percent. Since our calculations estimate that about 54 percent of Salvadorans we classified as undocumented actually had TPS in 2000, we use a compliance rate of 79 percent for unauthorized Salvadorans overall.

Estimation of State Income Taxes. State income taxes are estimated for filing units with the same tax simulator as used for federal income taxes. The adjustments noted above were incorporated into the state tax models. The same compliance rates were implemented for unauthorized migrant filing units. No additional alignment was required for these taxes.

Estimation of Local Income Taxes. There are no additional local income taxes in Washington, D.C., or Virginia. In Maryland, however, local income taxes are paid with state taxes. They are calculated as fixed rates and shares of state taxes paid, with the rates and fractions varying by county. The local rates and shares of state taxes are as follows:

<u>Area</u>	<u>Rate</u>	<u>Share of State Tax</u>
Montgomery	3.01%	65.393%
Prince George's	3.01%	64.476%
Frederick	2.52%	53.736%
Charles	2.52%	53.639%
Calvert-St. Mary's	2.71%	57.801%

To align the tax collections with administrative data, the local taxes were discounted by about 1 percent.

Estimation of Payroll Taxes

Payroll taxes on wage and salary income are calculated with a straightforward formula. The Social Security tax on wage and salary income for 1999 is computed as 6.2 percent of wage and salary income, up to a maximum take of \$4,501.20 (equal to 6.2 percent of \$72,600). The employer pays an equal amount, and we include this in our calculations as a tax paid by the employee. For self-employment income, the individual pays both the employee and employer share. No more than \$72,600 (of combined wage-salary and self-employment income) is subject to this tax.

The Medicare tax is 1.24 percent of wage and salary income, but unlike the Social Security tax, there is no maximum. Again, the employer share is equal to the employee share. For self-employment income, the individual pays both shares.

The Social Security and Medicare taxes are also discounted for unauthorized migrants to account for payment "off the books" using the same assumptions as we use for income taxes.

Estimation of Property Taxes

The census collected data on property tax payments by households who own their own homes. We used these figures straight from the census without

any major adjustments. For renters, however, we calculated property tax as a share of the rent paid. We estimated the amount of tax paid on rental units and credited the tenant with payment of these taxes. The value of rental property is estimated using a capitalization rate of 8.5 percent and assuming that half the rent paid goes for upkeep. The tax rates for each jurisdiction were then applied to the estimated market value to obtain the estimated property tax payments attributable to renters.

In Maryland, property tax payments are split among the state, counties, and some smaller jurisdictions. Our calculation of property tax takes into account all three governmental levels. However, in allocating real property tax payments to jurisdictions we treat all the real property tax in Maryland as a "local" (not "state") tax.

Virginia has a personal property tax on automobiles. The tax rate is set by each city and county. We computed the average tax paid per car by estimating the number of cars in each jurisdiction from the census data and the aggregate taxes paid from administrative data. Each household was assumed to pay the average tax on each car owned by members of the household.

Estimation of State and Local Consumption Taxes

We calculated sales and other consumption taxes based on estimates of household spending patterns along with state and local tax rates for the particular spending items. Spending patterns

were estimated using data from census and the 2000 CES.

Estimation of Household Spending. We began by estimating the total income available to households for consumption after they paid income taxes, payroll taxes, and housing costs. We used total household income as adjusted for our income tax calculations (see section on income tax estimation above). Household expenditures on mortgage or rent, utilities, property taxes, and insurance were deducted. We also deducted payroll along with federal, state, and local income taxes. Using the remaining income available to households ("after-tax" income), we estimated their spending on various items using average values from the CES. The spending categories we estimated are the following:

- food away from home (restaurants)
- alcohol
- telephone
- tobacco
- household expenditures
- medical and education expenses
- food at home
- charitable contributions
- pensions and retirements
- gasoline
- vehicle expenses
- public transportation

We calculated average household expenditures for each category for households with and without automobiles, disaggregated along the following dimensions:

Household race/ethnicity

- White, non-Hispanic*
- Black, non-Hispanic
- Hispanic
- Asian-Pacific Islander, non-Hispanic

- American Indian-Alaska Native, non-Hispanic

Household “type”

- married couple without children*
- married couple with children
- married couple, other
- single parent (with children)
- single person
- other

Household income

- Net loss*
- \$0–10,000
- \$10,000–\$20,000
- \$20,000–\$30,000
- \$30,000–\$40,000
- \$40,000–\$50,000
- \$50,000–\$60,000
- \$60,000–\$75,000
- \$75,000–\$100,000
- \$100,000 or more

Because of sample-size limitations in the CES, three two-way tables with average shares of after-tax income spent in each category were estimated with the CES microdata. For each table, dummy-variable regression equations were computed (omitting the categories designated as “*” in the list above). The three regressions were averaged to provide a single

equation to estimate the shares of after-tax income that each household spent in the designated categories. Because some households have extremely high incomes, maximum spending values were set for each category so that aggregate spending was in line with the available administrative data for the District of Columbia, Maryland, and Virginia.

Consumption Taxes Estimated. The following state and local taxes were estimated using our household spending estimates:

- general sales tax
- alcoholic beverages (taxes on beer, spirits, wine, and champagne, but not profit from state-owned stores in Virginia)
- cigarette tax
- sales tax on food (Virginia only)
- sales tax on restaurant meals (above regular sales tax)
- gasoline tax
- automobile registration, inspection, and drivers’ licenses

- automobile sales tax (but not excise tax)
- utility taxes (gas, water, electricity, heating oil, including “pass-through” of taxes on suppliers, but not profits of government-owned utilities)
- telephone tax (911 fees and local phone tax, but not federal/state excise taxes)

Tax rates for each state and locality, where applicable, were applied to the estimated amounts spent for each household to provide initial estimates of each tax collected for each jurisdiction. These tax rates were adjusted slightly (by factors ranging from 0.80 to 1.25) to bring the estimates of taxes collected in line with the actual tax collections. The final amounts of taxes paid were then estimated with the adjusted rates. Every household is assumed to pay the same tax rate on estimated amounts spent. Unlike the payroll taxes, there is no discount for unauthorized migrant households.

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Appendix Tables

**Appendix Table 1: Detailed Country and Region of Origin for
Washington, D.C., Metropolitan Area Immigrants, 2000**

(Populations in thousands)

Country or Region of Birth	Metropolitan Area				Maryland Counties@		Virginia Counties@	
	Total	District of Columbia	Maryland Counties	Virginia Counties	Mont- gomery	Prince George's	Arlington	Fairfax+
All Foreign-Born	857	76	367	414	233	109	55	251
Europe/Canada/Other	114	15	48	51	35	--	5	29
Europe	99	12	42	44	32	--	--	25
Germany	17	--	--	--	--	--	--	--
United Kingdom	19	--	--	--	--	--	--	--
Former USSR	15	--	--	--	--	--	--	--
Other Europe	48	--	--	--	--	--	--	--
Canada*	12	--	--	--	--	--	--	--
Oceania/Other	(b)	--	--	--	--	--	--	--
Latin America	338	38	146	154	82	59	29	78
Central America**	202	23	78	101	41	34	18	49
El Salvador	111	15	41	55	24	16	11	28
Mexico	38	--	17	19	--	10	--	--
Guatemala	21	--	--	11	--	--	--	--
Honduras	14	--	--	--	--	--	--	--
Other Central Amer.	16	--	--	--	--	--	--	--
Caribbean	51	--	34	--	15	17	--	--
Jamaica	18	--	13	--	--	--	--	--
Other Caribbean	33	--	21	--	--	--	--	--
South America	85	--	34	44	25	--	10	25
Bolivia	20	--	--	15	--	--	--	--
Colombia	11	--	--	--	--	--	--	--
Peru	21	--	--	12	--	--	--	--
Other South Amer.	34	--	17	12	--	--	--	--
Asia & Africa	406	24	173	209	116	50	20	144
Asia (South & East)	277	12	111	154	79	25	14	112
China#	44	--	25	17	19	--	--	12
India	48	--	23	24	16	--	--	18
Korea	49	--	(x)	31	13	--	--	25
Pakistan	17	--	--	13	--	--	--	--
Philippines	34	--	15	17	--	--	--	11
Vietnam	40	--	12	26	--	--	--	21
Other S. and E. Asia	45	--	16	27	--	--	--	17
Middle East##	49	--	18	29	15	--	--	20
Iran	19	--	--	10	--	--	--	--
Other Middle East	31	--	10	18	--	--	--	--
Africa (Sub-Saharan)	79	--	45	26	21	23	--	12
Ethiopia***	18	--	--	--	--	--	--	--
Nigeria	13	--	10	--	--	--	--	--
Other Africa	48	--	27	17	--	--	--	--

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

- * Includes North America. # Includes Hong Kong, Taiwan, and some smaller islands.
- ** Includes Mexico. ## Includes North Africa and Southwest Asia (to Iran).
- *** Includes Eritrea. @ Only counties with 50,000 or more foreign-born population shown separately.
- Fewer than 10,000 persons (weighted).

(Share of County Foreign-Born Population)

Country or Region of Birth	Metropolitan Area				Maryland Counties@		Virginia Counties	
	Total	District of Columbia	Maryland Counties	Virginia Counties	Mont- gomery	Prince George's	Arlington	Fairfax
All Foreign-Born	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
Europe/Canada/Other	13.3%	19.1%	13.0%	12.4%	15.0%	--	9.9%	11.0%
Europe	11.5%	16.4%	11.6%	10.6%	13.6%	--	--	9.9%
Germany	2.0%	--	--	--	--	--	--	--
United Kingdom	2.2%	--	--	--	--	--	--	--
Former USSR	1.7%	--	--	--	--	--	--	--
Other Europe	5.6%	--	--	--	--	--	--	--
Canada*	1.4%	--	--	--	--	--	--	--
Oceania/Other	(b)	--	--	--	--	--	--	--
Latin America	39.4%	49.7%	39.7%	37.2%	35.3%	54.0%	52.7%	31.0%
Central America**	23.5%	29.8%	21.2%	24.5%	17.7%	31.2%	32.6%	19.0%
El Salvador	13.0%	19.2%	11.3%	13.4%	10.5%	15.1%	20.1%	11.0%
Mexico	4.5%	--	4.5%	4.7%	--	9.2%	--	--
Guatemala	2.5%	--	--	2.7%	--	--	--	--
Honduras	1.6%	--	--	--	--	--	--	--
Other Central Amer.	1.9%	--	--	--	--	--	--	--
Caribbean	5.9%	--	9.3%	--	6.6%	16.0%	--	--
Jamaica	2.1%	--	3.6%	--	--	--	--	--
Other Caribbean	3.9%	--	5.7%	--	--	--	--	--
South America	9.9%	--	9.2%	10.7%	10.9%	--	18.9%	10.0%
Bolivia	2.3%	--	--	3.7%	--	--	--	--
Colombia	1.3%	--	--	--	--	--	--	--
Peru	2.4%	--	--	2.8%	--	--	--	--
Other South Amer.	3.9%	--	4.8%	2.8%	--	--	--	--
Asia & Africa	47.3%	31.2%	47.3%	50.4%	49.7%	46.0%	36.1%	57.0%
Asia (South & East)	32.4%	16.4%	30.2%	37.2%	34.1%	23.0%	24.4%	44.0%
China#	5.2%	--	6.7%	4.1%	8.3%	--	--	4.0%
India	5.6%	--	6.2%	5.7%	7.1%	--	--	7.0%
Korea	5.7%	--	(x)	7.4%	5.4%	--	--	10.0%
Pakistan	2.0%	--	--	3.0%	--	--	--	--
Philippines	4.0%	--	4.1%	4.2%	--	--	--	4.0%
Vietnam	4.7%	--	3.2%	6.4%	--	--	--	8.0%
Other S. and E. Asia	5.2%	--	4.3%	6.4%	--	--	--	6.0%
Middle East##	5.7%	--	4.8%	7.0%	6.4%	--	--	7.0%
Iran	2.2%	--	--	2.5%	--	--	--	--
Other Middle East	3.6%	--	2.7%	4.4%	--	--	--	--
Africa (Sub-Saharan)	9.2%	--	12.3%	6.2%	9.2%	21.2%	--	4.0%
Ethiopia***	2.1%	--	--	--	--	--	--	--
Nigeria	1.5%	--	2.8%	--	--	--	--	--
Other Africa	5.6%	--	7.4%	4.0%	--	--	--	--

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

* Includes North America.

Includes Hong Kong, Taiwan, and some smaller islands.

** Includes Mexico.

Includes North Africa and Southwest Asia (to Iran).

*** Includes Eritrea.

@ Only counties with 50,000 or more foreign-born population shown separately.

-- Fewer than 10,000 persons (weighted).

**Appendix Table 2: Citizenship and Legal Status by Jurisdiction for
Washington, D.C., Metropolitan Area Immigrants, 2000**

(Populations and Households in thousands)

Jurisdiction	Total	Native-Born	Foreign-Born					
			Foreign-Born Total	Percent of Total	Naturalized Citizens	Legal Permanent Residents	Legal Temporary Migrants	Unauthorized and TPS
Population by Individual Status								
Metropolitan Area	4,925	4,068	857	17.4%	310	271	47	229
Washington, DC	574	498	76	13.3%	20	26	6	23
Maryland	2,166	1,799	367	16.9%	142	115	19	91
Virginia	2,185	1,771	414	19.0%	147	130	22	115
Maryland Counties								
Montgomery	877	644	233	26.6%	93	72	14	54
Prince George's	806	690	117	14.5%	41	37	5	34
Frederick	197	188	9	4.5%	4	3	1	1
Calvert	164	159	4	2.7%	2	1	0	1
Charles	121	117	4	3.0%	2	1	0	0
Virginia Counties & Cities								
Arlington	194	138	55	28.6%	13	20	4	18
Alexandria	129	96	34	26.0%	8	11	1	12
Fairfax	1,010	759	251	24.8%	97	76	14	63
Loudoun	270	245	25	9.2%	10	8	1	6
Fredericksburg	143	138	5	3.3%	2	1	0	2
Stafford	108	104	4	4.1%	3	1	0	0
Prince William	332	291	41	12.3%	14	12	1	14
Distribution Across Areas of Population by Individual Status								
Metropolitan Area	100%	100%	100%	(x)	100%	100%	100%	100%
Washington, DC	12%	12%	9%	(x)	7%	10%	12%	10%
Maryland	44%	44%	43%	(x)	46%	42%	41%	40%
Virginia	44%	44%	48%	(x)	48%	48%	47%	50%
Maryland Counties								
Montgomery	18%	16%	27%	(x)	30%	27%	29%	24%
Prince George's	16%	17%	14%	(x)	13%	14%	10%	15%
Frederick	4%	5%	1%	(x)	1%	1%	1%	1%
Calvert	3%	4%	1%	(x)	1%	0%	0%	0%
Charles	2%	3%	0%	(x)	1%	0%	0%	0%
Virginia Counties & Cities								
Arlington	4%	3%	6%	(x)	4%	7%	9%	8%
Alexandria	3%	2%	4%	(x)	3%	4%	3%	5%
Fairfax	21%	19%	29%	(x)	31%	28%	30%	28%
Loudoun	5%	6%	3%	(x)	3%	3%	2%	2%
Fredericksburg	3%	3%	1%	(x)	1%	0%	0%	1%
Stafford	2%	3%	1%	(x)	1%	0%	0%	0%
Prince William	7%	7%	5%	(x)	5%	5%	1%	6%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

Some "counties" contain small populations from additional jurisdictions. See text for definitions.

* Legal temporary (nonimmigrant) population not shown separately, but included in totals.

Means shown in light text based on fewer than 5,000 weighted households; those in *italics* on 5,000-10,000 weighted households.

(x) Not applicable.

(Share of County Foreign-Born Population)

Jurisdiction	Total	Native-Born	Foreign-Born					
			Foreign-Born Total	Percent of Total	Naturalized Citizens	Legal Permanent Residents	Legal Temporary Migrants	Unauthorized and TPS
Households by Status of Head/Spouse								
Metropolitan Area	1,845	1,467	379	20.5%	157	124	18	80
Washington, DC	249	210	39	15.6%	12	14	3	10
Maryland	783	623	160	20.4%	70	53	7	31
Virginia	813	633	180	22.1%	75	57	8	40
Maryland Counties								
Montgomery	327	225	102	31.1%	45	33	4	19
Prince George's	288	239	49	17.0%	19	17	2	11
Frederick	70	65	5	6.5%	3	1	0	0
Calvert	57	54	3	4.5%	2	1	0	0
Charles	42	39	2	5.3%	1	1	0	0
Virginia Counties & Cities								
Arlington	87	63	24	27.9%	7	8	2	7
Alexandria	63	47	16	25.4%	5	5	1	5
Fairfax	368	263	105	28.6%	47	32	5	21
Loudoun	97	86	12	12.1%	6	4	0	2
Fredericksburg	51	49	3	4.9%	1	1	0	0
Stafford	36	33	3	7.3%	2	1	0	0
Prince William	111	93	18	15.9%	8	5	0	4
Distribution of Households by Status Across Areas								
Metropolitan Area	100%	100%	100%	(x)	100%	100%	100%	100%
Washington, DC	14%	14%	10%	(x)	8%	12%	16%	12%
Maryland	42%	42%	42%	(x)	44%	43%	37%	38%
Virginia	44%	43%	48%	(x)	48%	46%	47%	49%
Maryland Counties								
Montgomery	18%	15%	27%	(x)	29%	27%	25%	23%
Prince George's	16%	16%	13%	(x)	12%	13%	10%	14%
Frederick	4%	4%	1%	(x)	2%	1%	1%	1%
Calvert	3%	4%	1%	(x)	1%	1%	0%	0%
Charles	2%	3%	1%	(x)	1%	1%	0%	0%
Virginia Counties & Cities								
Arlington	5%	4%	6%	(x)	5%	7%	12%	8%
Alexandria	3%	3%	4%	(x)	3%	4%	3%	6%
Fairfax	20%	18%	28%	(x)	30%	26%	29%	26%
Loudoun	5%	6%	3%	(x)	4%	3%	2%	2%
Fredericksburg	3%	3%	1%	(x)	1%	1%	1%	1%
Stafford	2%	2%	1%	(x)	1%	1%	0%	0%
Prince William	6%	6%	5%	(x)	5%	4%	1%	6%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

Some "counties" contain small populations from additional jurisdictions. See text for definitions.

* Legal temporary (nonimmigrant) population not shown separately, but included in totals.

Means shown in light text based on fewer than 5,000 weighted households; those in *italics* on 5,000-10,000 weighted households.

(x) Not applicable.

**Appendix Table 3: Household Type and Composition for Foreign-Born
Households in the Washington, D.C., Metropolitan Area,
by Household Legal Status and Country of Birth, 2000**

(Populations and households in thousands)

Population and Measure	Total	Native-Born	Foreign-Born Households				
			Foreign-Born Total	Naturalized Citizens	Legal Permanent Residents	Legal Temporary Migrants	Unauthorized and TPS
Households	1,845	1,467	379	157	124	18	80
Couples	938	696	242	102	90	10	41
Male Heads	355	292	63	19	15	5	25
Female Heads	552	478	74	37	20	3	14
Persons	4,824	3,617	1,208	462	413	45	287
Heads/Spouses	2,784	2,163	621	259	214	28	121
Other Adults Males	428	280	148	45	48	4	50
Other Adult Females	381	261	119	46	40	3	30
Children	1,232	912	320	112	111	11	85
Native-Born	1,152	907	245	103	88	2	52
Foreign-Born	80	6	74	9	24	9	33
Percent Native							
All Persons	82%	99%	31%	40%	32%	6%	20%
Adults	79%	99%	15%	24%	15%	1%	3%
Children	94%	99%	77%	92%	79%	21%	61%
Persons per Household							
Total	2.61	2.47	3.19	2.94	3.33	2.55	3.59
Adults	1.95	1.84	2.34	2.23	2.43	1.94	2.52
Children	0.67	0.62	0.84	0.71	0.90	0.62	1.07
Mean Income							
Per Household	85,700	87,700	77,900	91,000	78,600	66,500	53,300
Per Person	32,800	35,600	24,400	30,900	23,600	26,100	14,800
Ratio to Natives							
Household Income	98%	100%	89%	104%	90%	76%	61%
Income Per Person	92%	100%	69%	87%	66%	73%	42%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Appendix Table 4: Household Type and Composition for Foreign-Born Households in the Washington, D.C., Metropolitan Area, by Household Country and Region of Birth, 2000

(Populations and households in thousands)

Population and Measure	All Foreign-Born	Asia	China*	India	Korea	Vietnam	Other Asia**	Latin Amer.	El Salv.	Cent. Amer. & Mexico	Carib-bean	South America	Other	Africa+	Middle East++	Europe Canada, Other
Households	379	117	20	21	21	14	41	127	36	29	27	36	134	36	25	73
Couples	242	84	14	16	15	10	30	75	22	18	13	22	83	18	17	47
Male Heads	63	15	3	3	2	2	5	24	9	6	4	5	23	9	4	10
Female Heads	74	18	3	2	4	2	7	28	5	4	9	9	28	9	3	16
Persons	1,208	369	56	63	63	53	133	476	157	120	81	118	362	111	73	178
Heads/Spouses	621	202	34	36	36	24	71	202	57	47	40	58	217	54	43	120
Other Adult Males	148	41	5	7	7	9	14	79	30	25	9	15	28	12	6	11
Other Adult Females	119	36	5	5	6	7	13	55	19	13	9	14	29	13	5	11
Children	320	91	12	15	15	13	35	140	51	36	23	31	88	33	20	36
Native-Born	245	68	10	12	11	10	25	108	39	26	20	22	70	25	16	30
Foreign-Born	74	23	2	3	4	3	10	33	11	9	4	9	19	8	4	6
Percent Native																
All Persons	31%	27%	26%	25%	27%	25%	30%	31%	28%	29%	39%	30%	36%	30%	34%	41%
Adults	15%	12%	10%	9%	12%	9%	15%	12%	5%	11%	20%	16%	22%	11%	17%	31%
Children	77%	75%	80%	77%	73%	76%	72%	77%	78%	74%	85%	71%	79%	75%	80%	82%
Persons per Household																
Total	3.19	3.14	2.82	3.05	3.03	3.68	3.22	3.75	4.41	4.16	3.02	3.31	2.70	3.10	2.92	2.43
Adults	2.34	2.37	2.20	2.33	2.30	2.80	2.36	2.64	2.99	2.92	2.15	2.44	2.04	2.19	2.13	1.93
Children	0.84	0.77	0.62	0.72	0.74	0.88	0.85	1.11	1.42	1.23	0.87	0.87	0.66	0.91	0.78	0.49
Mean Income																
Per Household	77,900	82,800	82,500	112,000	70,300	74,800	77,300	63,200	50,800	55,400	65,300	80,400	87,500	56,200	94,500	100,400
Per Person	24,400	26,300	29,200	36,700	23,200	20,300	24,000	16,900	11,500	13,300	21,600	24,300	32,400	18,100	32,400	41,400
Ratio to Natives																
Household Income	89%	94%	94%	128%	80%	85%	88%	72%	58%	63%	74%	92%	100%	64%	108%	114%
Income Per Person	69%	74%	82%	103%	65%	57%	68%	47%	32%	37%	61%	68%	91%	51%	91%	116%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

* Includes Hong Kong and Taiwan.

** Countries east of and including Afghanistan and Pakistan.

+ Sub-Saharan countries.

++ North African countries and southwest Asian countries west of and including Iran and Iraq.

Appendix Table 5: Total Household Income and Taxes Paid in the Washington, D.C., Metropolitan Area, by Household Nativity, Legal Status, and Place of Birth circa 1999–2000

Household Nativity, Legal Status and Place of Birth	Household Taxes*			Household Income			Households	
	Amt.**	Pct.	Ratio to HH %	Amt.**	Pct.	Ratio to HH %	No.#	Pct.
Total	55,174	100.0%	(x)	158,160	100.0%	(x)	1,845	100.0%
Native-Born	45,402	82.3%	104%	128,674	81.4%	102%	1,467	79.5%
Foreign-Born	9,771	17.7%	86%	29,486	18.6%	91%	379	20.5%
Naturalized Citizens	4,933	8.9%	105%	14,299	9.0%	106%	157	8.5%
Legal Permanent Residents	3,360	6.1%	91%	9,752	6.2%	92%	124	6.7%
Legal Temporary Migrants	460	0.8%	87%	1,179	0.7%	78%	18	1.0%
Unauthorized and TPS	1,019	1.8%	43%	4,255	2.7%	62%	80	4.3%
Asia	3,285	6.0%	94%	9,723	6.1%	97%	117	6.4%
China***	563	1.0%	94%	1,652	1.0%	96%	20	1.1%
India	802	1.5%	129%	2,321	1.5%	131%	21	1.1%
Korea	481	0.9%	77%	1,472	0.9%	82%	21	1.1%
Vietnam	360	0.7%	84%	1,070	0.7%	87%	14	0.8%
Other Asia##	1,079	2.0%	87%	3,207	2.0%	90%	41	2.2%
Latin America	2,467	4.5%	65%	8,029	5.1%	74%	127	6.9%
El Salvador	494	0.9%	46%	1,816	1.1%	59%	36	1.9%
Other C. Amer., Mexico	428	0.8%	49%	1,604	1.0%	65%	29	1.6%
Caribbean	586	1.1%	73%	1,747	1.1%	76%	27	1.5%
South America	959	1.7%	90%	2,863	1.8%	94%	36	1.9%
Other	4,019	7.3%	100%	11,735	7.4%	102%	134	7.3%
Sub-Saharan Africa	649	1.2%	61%	2,014	1.3%	66%	36	1.9%
Middle East+	834	1.5%	111%	2,367	1.5%	110%	25	1.4%
Europe Canada, Other	2,536	4.6%	116%	7,353	4.6%	117%	73	4.0%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

* Includes employer-paid FICA and Medicare tax.

** In millions of dollars.

In thousands.

*** Includes Hong Kong and Taiwan.

Countries east of and including Afghanistan and Pakistan.

+ North African countries and southwest Asian countries west of and including Iran and Iraq.

Appendix Table 6: Mean Household Income and Taxes Paid in the Washington, D.C., Metropolitan Area, by Household Nativity, Legal Status, and Place of Birth, circa 1999–2000

Household Nativity, Legal Status and Place of Birth	Mean Household Income	Mean Household Taxes Paid*	Percent of Income Paid**
Native-Born	87,700	30,957	31%
Foreign-Born	77,900	25,804	28%
Naturalized Citizens	91,000	31,405	30%
Legal Permanent Residents	78,600	27,094	29%
Legal Temporary Migrants	66,500	25,948	33%
Unauthorized and TPS	53,300	12,756	19%
Asia	82,800	27,962	28%
China***	82,500	28,134	28%
India	112,000	38,697	30%
Korea	70,300	22,997	28%
Vietnam	74,800	25,141	27%
Other Asia#	77,300	25,994	28%
Latin America	63,200	19,416	25%
El Salvador	50,800	13,835	21%
Other Central America, Mexico	55,400	14,782	21%
Caribbean	65,300	21,901	28%
South America	80,400	26,914	29%
Other	87,500	29,965	30%
Sub-Saharan Africa	56,200	18,100	26%
Middle East+	94,500	33,295	31%
Europe, Canada, Other	100,400	34,635	30%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

* Includes employer-paid FICA and Medicare tax.

** Excludes employer-paid FICA and Medicare tax.

*** Includes Hong Kong and Taiwan.

Countries east of and including Afghanistan and Pakistan.

+ North African countries and southwest Asian countries west of and including Iran and Iraq.

Appendix Table 7: Total Household Income and Taxes Paid for Immigrant Households in the Washington, D.C., Metropolitan Area, by Jurisdiction, circa 1999–2000

Jurisdiction	Household Taxes Paid*				Household Income			Households	
	Amt.**	Pct.	Ratio to HH %	% Pd. In Taxes#	Amt.**	Pct.	Ratio to HH %	No.***	Pct.
Metropolitan Area	9,771	100.0%	(x)	28.1%	29,486	100.0%	(x)	379	100.0%
Washington, DC	1,006	10.3%	100%	30.8%	2,858	9.7%	94%	39	10.3%
Maryland	3,972	40.7%	96%	27.5%	12,135	41.2%	98%	160	42.2%
Virginia	4,793	49.0%	103%	28.0%	14,494	49.2%	103%	180	47.5%
Maryland Counties									
Montgomery	2,769	28.3%	106%	27.9%	8,419	28.6%	106%	102	26.8%
Prince George's	961	9.8%	76%	26.5%	2,983	10.1%	78%	49	12.9%
Frederick	112	1.2%	96%	26.7%	345	1.2%	97%	5	1.2%
Calvert+	77	0.8%	117%	28.6%	225	0.8%	114%	3	0.7%
Charles	52	0.5%	91%	26.4%	162	0.5%	93%	2	0.6%
Virginia Counties & Cities+									
Arlington	536	5.5%	85%	27.6%	1,658	5.6%	87%	24	6.5%
Alexandria	271	2.8%	66%	26.9%	842	2.9%	68%	16	4.2%
Fairfax	3,151	32.2%	116%	28.6%	9,374	31.8%	115%	105	27.7%
Loudoun	356	3.6%	117%	28.1%	1,065	3.6%	116%	12	3.1%
Fredericksburg	46	0.5%	71%	25.5%	150	0.5%	77%	3	0.7%
Stafford	56	0.6%	84%	25.4%	182	0.6%	89%	3	0.7%
Prince William	376	3.8%	82%	25.0%	1,223	4.1%	89%	18	4.7%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

* Includes employer-paid FICA and Medicare tax.

Excludes employer-paid FICA and Medicare tax.

** In millions of dollars.

*** In thousands.

**Appendix Table 8: Taxes Collected in the Washington, D.C., Metropolitan Area,
by Type of Tax and Jurisdiction, circa 1999–2000**

(Taxes in millions of dollars)

Jurisdiction	All Taxes	Federal Taxes				State Taxes					Local Taxes					
		Total	Income	Social Security ^a	Medi-care ^a	Total	Income	Sales tax ^b	Auto taxes/ fees ^c	Tobacco/ Alcohol	Total	Real property ^e	Income ^f	Sales tax ^g	Auto taxes/ fees ^h	Utility/ tele-phone ⁱ
Taxes Paid by All Households																
Metropolitan Area	55,174	40,183	24,366	12,225	3,592	9,425	6,328	2,326	592	179	5,566	3,145	1,254	400	511	256
Washington, DC	6,200	4,160	2,652	1,166	341	1,728	1,177	480	49	21	312	229	0	0	0	84
Maryland	21,764	15,800	9,208	5,141	1,450	3,147	2,016	755	277	100	2,817	1,473	1,254	0	0	90
Virginia	27,209	20,223	12,505	5,918	1,801	4,550	3,134	1,091	266	59	2,436	1,443	0	400	511	82
Maryland Counties																
Montgomery	11,737	8,628	5,587	2,313	728	1,647	1,121	362	114	51	1,462	718	705	0	0	39
Prince George's	6,207	4,396	2,197	1,755	444	905	548	232	96	28	907	506	353	0	0	48
Frederick	1,555	1,127	576	437	114	243	142	65	28	9	185	107	76	0	0	1
Calvert+	1,279	940	489	359	93	190	118	42	23	7	149	80	68	0	0	1
Charles	987	709	360	278	71	163	87	54	16	5	115	62	52	0	0	1
Virginia Counties and Cities+																
Arlington	2,863	2,176	1,406	589	182	480	337	113	23	7	207	116	0	40	49	2
Alexandria	2,043	1,520	1,009	390	122	333	237	76	16	5	190	86	0	29	34	41
Fairfax	14,527	10,795	6,953	2,911	931	2,413	1,688	572	123	30	1,319	787	0	222	282	28
Loudoun	2,975	2,208	1,295	702	210	503	339	123	35	6	265	162	0	38	60	5
Fredericksburg	993	729	378	278	73	176	111	45	18	2	88	52	0	16	19	0
Stafford	891	661	349	246	66	154	101	39	13	2	76	45	0	17	13	0
Prince William	2,916	2,133	1,116	802	215	490	322	123	39	6	293	195	0	38	53	7

(Taxes in millions of dollars)

Jurisdiction	All Taxes	Federal Taxes				State Taxes					Local Taxes					
		Total	Income	Social Security ^a	Medi-care ^a	Total	Income	Sales tax ^b	Auto taxes/fees ^c	Tobacco/Alcohol	Total	Real property ^e	Income ^f	Sales tax ^g	Auto taxes/fees ^h	Utility/tele-phone ⁱ
Taxes Paid by Immigrant Households																
Metropolitan Area	9,771	7,030	3,924	2,420	686	1,643	1,079	418	120	26	1,098	627	226	76	121	48
Washington, DC	1,006	678	416	202	60	276	186	80	7	3	53	40	0	0	0	13
Maryland	3,972	2,874	1,556	1,031	287	562	357	135	55	14	536	292	226	0	0	19
Virginia	4,793	3,478	1,952	1,187	339	806	536	203	58	9	509	295	0	76	121	17
Suburban Maryland																
Montgomery	2,769	2,015	1,138	682	195	384	251	88	35	9	370	199	159	0	0	11
Prince George's	961	682	327	281	74	141	84	37	16	4	138	77	55	0	0	7
Frederick	112	82	41	33	9	17	10	5	2	0	13	8	5	0	0	0
Calvert+	77	57	32	20	6	11	7	3	1	0	8	4	4	0	0	0
Charles	52	38	17	16	4	9	5	3	1	0	6	4	3	0	0	0
Northern Virginia																
Arlington	536	395	235	126	35	94	63	23	6	1	47	25	0	8	13	1
Alexandria	271	190	97	73	19	46	29	12	4	1	35	15	0	5	9	7
Fairfax	3,151	2,289	1,319	750	220	525	354	130	35	6	337	198	0	51	81	7
Loudoun	356	263	145	91	27	59	39	15	4	1	34	21	0	5	7	1
Fredericksburg	46	34	17	13	4	8	5	2	1	0	4	3	0	1	1	0
Stafford	56	41	19	17	4	10	6	3	1	0	5	3	0	1	1	0
Prince William	376	266	120	116	30	65	40	18	6	1	45	30	0	6	9	1

Jurisdiction	All Taxes	Federal Taxes				State Taxes					Local Taxes					
		Total	Income	Social Security ^a	Medi-care ^a	Total	Income	Sales tax ^b	Auto taxes/ fees ^c	Tobacco/ Alcohol	Total	Real property ^e	Income ^f	Sales tax ^g	Auto taxes/ fees ^h	Utility/ tele-phone ⁱ
Percent of taxes paid by immigrants across jurisdictions, levels of government																
Metropolitan Area	18%	17%	16%	20%	19%	17%	17%	18%	20%	15%	20%	20%	18%	19%	24%	19%
Washington, DC	16%	16%	16%	17%	18%	16%	16%	17%	15%	14%	17%	17%	N/A	N/A	N/A	15%
Maryland	18%	18%	17%	20%	20%	18%	18%	18%	20%	14%	19%	20%	18%	N/A	N/A	21%
Virginia	18%	17%	16%	20%	19%	18%	17%	19%	22%	16%	21%	20%	N/A	19%	24%	21%
Maryland Counties																
Montgomery	24%	23%	20%	29%	27%	23%	22%	24%	31%	19%	25%	28%	23%	N/A	N/A	29%
Prince George's	15%	16%	15%	16%	17%	16%	15%	16%	17%	13%	15%	15%	16%	N/A	N/A	15%
Frederick	7%	7%	7%	8%	8%	7%	7%	7%	6%	5%	7%	7%	7%	N/A	N/A	7%
Calvert+	6%	6%	7%	5%	6%	6%	6%	6%	5%	4%	6%	5%	6%	N/A	N/A	5%
Charles	5%	5%	5%	6%	6%	5%	5%	5%	6%	4%	6%	6%	5%	N/A	N/A	5%
Virginia Counties and Cities+																
Arlington	19%	18%	17%	21%	19%	20%	19%	21%	27%	18%	23%	22%	N/A	21%	26%	28%
Alexandria	13%	13%	10%	19%	16%	14%	12%	16%	25%	13%	19%	17%	N/A	16%	25%	18%
Fairfax	22%	21%	19%	26%	24%	22%	21%	23%	29%	20%	26%	25%	N/A	23%	29%	27%
Loudoun	12%	12%	11%	13%	13%	12%	11%	12%	12%	11%	13%	13%	N/A	12%	12%	13%
Fredericksburg	5%	5%	5%	5%	5%	5%	4%	5%	5%	5%	5%	5%	N/A	5%	5%	N/A
Stafford	6%	6%	6%	7%	7%	6%	6%	7%	7%	6%	7%	8%	N/A	7%	7%	N/A
Prince William	13%	12%	11%	15%	14%	13%	12%	14%	16%	13%	15%	15%	N/A	15%	16%	16%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public Use Microdata Sample with an adjustment for legal and unauthorized immigrants omitted from the census.

****** First column is percentage of federal taxes within the jurisdiction. State and local taxes are combined for the District of Columbia.

a Includes employer-paid share.

b State portion of general sales tax (including food), restaurant tax, and sales tax on alcohol (VA).

c Includes state gasoline tax and state car registration, but does not include excise tax on vehicle purchases.

d Includes special state tax on alcohol and cigarettes, but does not include profit from state store sales (VA).

e Includes estimated payments on behalf of renters; includes state portion in Maryland and all of DC's residential property tax.

f Maryland only—county share of state income tax.

g Includes local cigarette and tobacco tax, and estimated local share of general tax.

h Includes “car tax” plus local gasoline tax and local car registration in Virginia, but does not include excise tax on purchase.

i Includes 911 fee, local phone tax, and any “tax” on utilities (not operating profit). Does not include federal/state excise taxes. Some of the utility taxes are levied on suppliers but are treated as pass-throughs to the consumer.

Appendix Table 9: Mean Household Income and Taxes Paid in the Washington, D.C., Metropolitan Area, by Nativity, Educational Attainment, and English Proficiency of Household Head, circa 1999–2000

Nativity, Educational Attainment, and English Proficiency of Household Head	Mean Household Income	Mean Household Taxes Paid*	Percent of Income Paid**
Native-Born	87,700	30,957	31%
No High School Degree	39,187	10,846	24%
High School, Some College	63,165	21,295	28%
4- Year College Degree or More	122,048	44,668	32%
Foreign-Born	78,600	25,804	28%
No High School Degree	46,789	12,148	21%
High School, Some College	59,683	18,995	26%
4- Year College Degree or More	103,354	36,145	30%
Speaks English at Home	96,916	33,634	30%
Speaks Another Language			
Speaks English Very Well	84,286	28,889	29%
Speaks English Well	58,909	17,655	25%
Speaks English Not Well	49,676	13,099	21%
Speaks English Not at All	40,834	11,360	21%

Source: Urban Institute and Pew Hispanic Center tabulations from Census 2000 5-Percent Public-Use Microdata Sample (PUMS) with an adjustment for legal and unauthorized immigrants omitted from the Census.

Notes:

* Includes employer-paid FICA and Medicare tax.

** Excludes employer-paid FICA and Medicare tax.

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Exhibit 28

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

Original Investigation

Quality of Health Insurance Coverage and Access to Care for Children in Low-Income Families

Amanda R. Kreider, BS, BA; Benjamin French, PhD; Jaya Aysola, MD, MPH; Brendan Saloner, PhD; Kathleen G. Noonan, JD; David M. Rubin, MD, MSCE

IMPORTANCE An increasing diversity of children's health coverage options under the US Patient Protection and Affordable Care Act, together with uncertainty regarding reauthorization of the Children's Health Insurance Program (CHIP) beyond 2017, merits renewed attention on the quality of these options for children.

OBJECTIVE To compare health care access, quality, and cost outcomes by insurance type (Medicaid, CHIP, private, and uninsured) for children in households with low to moderate incomes.

DESIGN, SETTING, AND PARTICIPANTS A repeated cross-sectional analysis was conducted using data from the 2003, 2007, and 2011-2012 US National Surveys of Children's Health, comprising 80 655 children 17 years or younger, weighted to 67 million children nationally, with household incomes between 100% and 300% of the federal poverty level. Multivariable logistic regression models compared caregiver-reported outcomes across insurance types. Analysis was conducted between July 14, 2014, and May 6, 2015.

EXPOSURES Insurance type was ascertained using a caregiver-reported measure of insurance status and each household's poverty status (percentage of the federal poverty level).

MAIN OUTCOMES AND MEASURES Caregiver-reported outcomes related to access to primary and specialty care, unmet needs, out-of-pocket costs, care coordination, and satisfaction with care.

RESULTS Among the 80 655 children, 51 123 (57.3%) had private insurance, 11 853 (13.6%) had Medicaid, 9554 (18.4%) had CHIP, and 8125 (10.8%) were uninsured. In a multivariable logistic regression model (with results reported as adjusted probabilities [95% CIs]), children insured by Medicaid and CHIP were significantly more likely to receive a preventive medical (Medicaid, 88% [86%-89%]; $P < .01$; CHIP, 88% [87%-89%]; $P < .01$) and dental (Medicaid, 80% [78%-81%]; $P < .01$; CHIP, 77% [76%-79%]; $P < .01$) visits than were privately insured children (medical, 83% [82%-84%]; dental, 73% [72%-74%]). Children with all insurance types experienced challenges in access to specialty care, with caregivers of children insured by CHIP reporting the highest rates of difficulty accessing specialty care (28% [24%-32%]), problems obtaining a referral (23% [18%-29%]), and frustration obtaining health care services (26% [23%-28%]). These challenges were also magnified for privately insured children with special health care needs, whose caregivers reported significantly greater problems accessing specialty care (29% [26%-33%]) and frustration obtaining health care services (36% [32%-41%]) than did caregivers of children insured by Medicaid, and a lower likelihood of insurance always meeting the child's needs (63% [60%-67%]) than children insured by Medicaid or CHIP. Caregivers of privately insured children were also significantly more likely to experience out-of-pocket costs (77% [75%-78%]) than were caregivers of children insured by Medicaid (26% [23%-28%]; $P < .01$) or CHIP (38% [35%-40%]; $P < .01$).

CONCLUSIONS AND RELEVANCE This examination of caregiver experiences across insurance types revealed important differences that can help guide future policymaking regarding coverage for families with low to moderate incomes.

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Until the Patient Protection and Affordable Care Act (hereafter Affordable Care Act) was enacted in 2010, children in families with low to moderate incomes could receive subsidized health insurance through either Medicaid or the Children's Health Insurance Program (CHIP). The Affordable Care Act added a third option through the creation of qualified health plans (QHPs), which are sold on the federal and state exchanges and subsidized for individuals and families with incomes of up to 400% of the federal poverty level (FPL). This new insurance option for children in families with low to moderate incomes has stimulated a debate regarding children's health insurance coverage. Some hypothesize that the QHPs, born from the commercial market, might eclipse the need for CHIP coverage although the Affordable Care Act maintained both options. A recent, short-term extension of CHIP funding by Congress through 2017 now prolongs this debate.¹

The early experience with QHPs has been equivocal. While QHPs are required to include a package of essential health benefits, including pediatric benefits, these benefits are not defined consistently across states.² In addition, the QHPs reflect benefit and cost-sharing standards in the private market. Despite the Affordable Care Act's cost-sharing subsidies, families in the private market have experienced higher costs compared with families with CHIP coverage.³⁻⁸ Children enrolled in Medicaid and CHIP also are reported to have more comprehensive benefit packages than privately insured children, which is especially so for children enrolled in Medicaid, while access to dental, vision, and developmental services is mandatory for children under the Medicaid Early and Periodic Screening, Diagnostic, and Treatment program, it is not required in stand-alone CHIP plans.⁹ However, lower reimbursement rates in Medicaid have been linked to reduced access to health care providers, particularly for specialty services in pediatrics, compared with private insurance.¹⁰⁻¹² Finally, private insurance can subject families to greater out-of-pocket costs than public insurance^{10,13}; while cost-sharing is allowed in CHIP and Medicaid, family contributions to costs cannot exceed 5% of income.^{14,15}

In the midst of uncertainty about the future of children's health insurance coverage, we must better understand coverage quality and associated access to care for children in families with low to moderate incomes. Such data are crucial as nearly 40% of children in the United States lived in households with incomes between 100% and 300% of the FPL by 2013.¹⁶ Few studies have explored differences in quality of care for demographically similar families with coverage via CHIP vs Medicaid^{10,13,17-20}; therefore, differences in comprehensiveness and quality between these 2 publicly financed programs require clarity. We analyzed the National Surveys of Children's Health (NSCH) from 2003, 2007, and 2011-2012 to provide a comprehensive comparison by insurance coverage type of caregiver-reported experiences with care for children in families with low to moderate incomes.

Methods

Design and Participants

We conducted a repeated cross-sectional analysis using public- and restricted-use data from the 2003, 2007, and 2011-2012

At a Glance

- Data from the National Survey of Children's Health were used to derive public insurance eligibility status and compare access to and use of health care across 4 insurance coverage types (Medicaid, the Children's Health Insurance Program [CHIP], private insurance, and uninsured) for children in households with low to moderate incomes.
- Children insured by Medicaid or CHIP experienced greater access to preventive medical (Medicaid and CHIP, 88%) and dental (Medicaid, 80%; CHIP, 77%) care than did privately insured children (medical, 83%; dental, 73%).
- Children with all types of insurance experienced challenges in accessing specialty care, with as many as 1 in 4 children having difficulty seeing a specialist; however, these challenges were amplified for children insured by CHIP (28%) and for privately insured children with special health care needs (29%).
- Caregivers of privately insured children were much more likely to experience out-of-pocket costs (77%) than were caregivers of children insured by Medicaid (26%) or CHIP (38%).

NSCH surveys.²¹ Analysis was conducted between July 14, 2014, and May 6, 2015. The NSCH is a nationally representative, telephone-based survey conducted by the National Center for Health Statistics (the 2003 and 2007 surveys were landline-based samples; the 2011-2012 survey included a cellular telephone subsample). The NSCH included questions about children's health status, access to and use of health care, insurance status, demographics, and household information, including household educational level, household income, and family structure. A knowledgeable adult, typically a parent, provided information about the sampled children. Analysis of deidentified data from the survey is exempt from federal regulations for the protection of human research participants.

The analysis focused on families with incomes between 100% and 300% of the FPL because within this income segment, children could qualify for either Medicaid or CHIP depending on their state's eligibility thresholds.²² Children from Vermont were excluded owing to special eligibility rules that made determination of public coverage type difficult. Children from Tennessee were excluded owing to unavailable Medicaid-to-Medicare fee index data. Children with functional limitations were excluded because they were more likely to have non-income-based eligibility for Medicaid (eg, disability), making classification of public insurance type difficult. Subgroup analyses were performed among young children (aged ≤5 years) and children with special health care needs; these analyses provide additional context regarding the experience of groups likely to have greater needs for health care services.

Classification of Insurance Status

Using caregiver-reported measures of current insurance status from the NSCH, children were classified as uninsured, publicly insured, or privately insured. The NSCH does not separately ascertain enrollment in Medicaid vs CHIP. Thus, to disaggregate children likely to be enrolled in Medicaid vs CHIP, we obtained restricted income and household size data through an agreement with the National Center for Health Statistics Research Data Center. Using these data, we calculated each household's poverty status as a percentage of FPL. We then linked

that poverty status to the public insurance income eligibility thresholds relevant to the child's state and age group within the given survey year, using eligibility information from the Kaiser Family Foundation.²³⁻²⁵ To examine possible misclassification of coverage type, we compared our state-level, NSCH-derived enrollment estimates with administrative enrollment data from the Centers for Medicare & Medicaid Services for each year.^{19,26} Estimates of Medicaid enrollment were consistently lower than rates reported by the Centers for Medicare & Medicaid Services, consistent with prior research.²⁷ However, we obtained high correlation coefficients between simulated and actual enrollment totals across states and time within programs (Medicaid, 0.90; CHIP, 0.98). Uninsured children were included in the analyses as a reference point with which to interpret the level of access and use of health care services experienced by insured children.

By including data across 3 years of NSCH surveys, we sought to exploit differences within and across states in income-based eligibility requirements over time. Many states changed these requirements between 2003 and 2012. For example, a 7-year-old child in Missouri with a household income of 175% of the FPL would have been eligible for Medicaid in 2003 but by 2007 would instead have been eligible for CHIP. A similar child in Arizona would have qualified for CHIP in 2007 but would not have been eligible for any public insurance after the state froze CHIP enrollment in 2010.⁷

Outcomes

We assessed the following family-reported outcomes: access to and use of primary and specialty care, unmet health care needs, out-of-pocket costs, care coordination, and satisfaction with care. Measures of access to and use of care included receipt of a preventive medical and dental visit within the last 12 months, having a personal physician or nurse, and having a usual source of health care (excluding multiple health care providers or emergency department visits). Specialty care outcomes, including caregiver-reported problems, seeing a specialist, or obtaining a referral in the last 12 months, were assessed for children who required these services. Unmet health care needs included any unmet medical or dental needs. Caregiver-reported measures of satisfaction with care included whether the child's insurance always met his or her needs, whether the insurance always allowed the child to see necessary health care providers, and whether the caregiver was ever frustrated in obtaining health care services for the child. The out-of-pocket cost measure included the presence of any out-of-pocket costs (not including health insurance premiums). We also included derived measures of whether the child received effective care coordination (inclusive of care coordination and communication across health care providers) and family-centered care (caregiver reported a trusting, collaborative, working partnership with child's health care providers during a recent visit).^{28,29} For more information on outcome measures, see the eAppendix in the [Supplement](#).

Statistical Analysis

Multivariable logistic regression models compared child-level outcomes across insurance types. All models adjusted for calendar year, income strata (100%-150%, >150%-200%, and

>200%-300% of the FPL), child-level demographic and household characteristics (age, sex, race/ethnicity, special health care needs, household educational level, family structure, and urbanicity as measured by residence in a metropolitan statistical area), and state-level characteristics (Medicaid-to-Medicare fee index, poverty rate, and unemployment rate). State-level characteristics were included to adjust for the generosity of public insurance and account for other state-specific economic factors that could affect the access environment. Within each income stratum there were children who were enrolled in Medicaid and CHIP, both within and across states. Subanalyses were conducted to examine outcomes stratified by income; these subanalyses included an interaction term between insurance type and income strata. A robust variance estimator accommodated the correlation due to clustering of children within states. Models were properly weighted to accommodate the complex survey design and nonresponse. Model estimates were used to generate predicted probabilities of each outcome by insurance type, standardized by child and state characteristics. Important differences were identified based on a combination of several criteria: statistically significant differences in odds ratio contrasts between Medicaid, CHIP, and private insurance ($P < .05$); clinically relevant differences across insurance types in the adjusted marginal probabilities of the outcome; and consistency in results across outcome domains to mitigate the problem of multiple comparisons. All analyses were performed in Stata, version 13 (StataCorp), including the svy suite of commands.

Results

The study sample was 80 655 children, weighted to 67 million children nationally. Among the 80 655 children, 51 123 (57.3%) had private insurance, 11 853 (13.6%) had Medicaid, 9554 (18.4%) had CHIP, and 8125 (10.8%) were uninsured. Privately insured children were more likely than other children to be white, come from 2-parent households, and come from households where a caregiver's educational level was higher than high school (**Table 1**). Medicaid-insured children were younger, on average, and publicly insured children were more likely to have special health care needs than other children.

Predicted probabilities of outcomes by insurance type, standardized by child and state characteristics, are presented in **Table 2**. For ease of interpretation, these predicted probabilities are described as predicted percentages below. Results stratified by FPL are presented in eTables 1 through 9 in the [Supplement](#).

Standardized Estimates of Preventive and Specialty Care

In a multivariable logistic regression model (with results reported as adjusted probabilities [95% CIs]), 88% (86%-89%) of children insured by Medicaid and 88% (87%-89%) of those insured by CHIP had a preventive medical visit compared with 83% (82%-84%) of privately insured children ($P < .001$ for both contrasts). In addition, publicly insured children were more likely than privately insured children to receive a preventive dental visit: 80% (78%-81%) of children insured by Medicaid

Table 1. Demographic and Household Characteristics of Children From the National Survey of Children's Health^a

Characteristic	Unweighted No. (Weighted %) ^b			
	Uninsured (n = 8125)	Medicaid (n = 11 853)	CHIP (n = 9554)	Private Insurance (n = 51 123)
Age, y				
≤5	2143 (27.6)	5602 (53.8)	2731 (27.0)	15 811 (31.5)
6-11	2598 (32.8)	3197 (24.0)	3529 (38.2)	16 239 (33.1)
12-17	3384 (39.6)	3054 (22.2)	3294 (34.7)	19 073 (35.4)
Female sex	3856 (46.2)	5893 (50.0)	4622 (49.1)	24 924 (49.1)
Race/ethnicity				
White	4589 (44.1)	5592 (44.0)	5089 (39.6)	37 348 (65.7)
Black	644 (13.6)	2098 (21.3)	1606 (19.7)	3884 (11.0)
Hispanic	2136 (36.4)	2628 (25.1)	2051 (33.0)	5483 (15.1)
Other	756 (5.9)	1535 (9.6)	808 (7.7)	4408 (8.2)
Special health care needs	824 (9.8)	2139 (17.0)	1967 (17.5)	7399 (14.3)
Household educational level ^c				
Less than high school	890 (13.9)	1244 (12.9)	894 (13.6)	1292 (3.5)
High school	2484 (36.2)	4012 (38.5)	3058 (38.4)	9862 (24.3)
More than high school	4751 (49.9)	6597 (48.6)	5602 (47.9)	39 969 (72.2)
Household income				
>100% to 150% of FPL	2615 (38.3)	8407 (77.7)	3516 (39.0)	6697 (15.9)
>150% to 200% of FPL	2390 (29.2)	2757 (19.0)	4404 (43.9)	11 702 (24.2)
>200% to 300% of FPL	3120 (32.5)	689 (3.3)	1634 (17.1)	32724 (59.8)
Family structure				
Two parent-biological or adopted	5206 (63.3)	5917 (51.9)	4689 (52.5)	36 263 (69.8)
Two parent-stepparent	745 (10.2)	1000 (9.1)	981 (10.7)	4106 (9.4)
Single mother-no father present	1584 (20.2)	3809 (31.3)	2802 (27.4)	8482 (16.4)
Other	590 (6.3)	1127 (7.7)	1082 (9.3)	2272 (4.4)
Resides in an MSA ^d	5265 (80.2)	7822 (74.7)	6536 (83.1)	34 111 (78.5)

Abbreviations: CHIP, Children's Health Insurance Program; FPL, federal poverty level; MSA, metropolitan statistical area.

^a Includes all children surveyed in the National Surveys of Children's Health in 2003, 2007, and 2011-2012 for whom insurance type could be reliably classified and who were eligible for the study (ie, between 100% and 300% of the FPL and did not have functional limitations).

^b Percentages are weighted to represent the population of noninstitutionalized children 17 years and younger in 48 states and the District of Columbia.

^c Highest educational level achieved by any member of the household.

^d Measure of urbanicity.

and 77% (76%-79%) of those insured by CHIP had such a visit compared with 73% (72%-74%) of privately insured children ($P < .001$ and $P = .001$, respectively). Uninsured children were substantially less likely than insured children to receive preventive care visits, have a personal physician or nurse, or have a usual source of care ($P < .001$ for all contrasts).

In contrast to preventive care, children with all insurance types experienced challenges in specialty care access, but those insured by CHIP had more difficulty accessing specialty care than did privately insured children. For example, 15% (13%-18%) of privately insured children and 18% (14%-23%) of Medicaid-insured children had difficulty obtaining a referral when needed compared with 23% (18%-29%) of those insured by CHIP ($P = .01$ and $P = .11$, respectively). Similarly, across all insurance types, more than 1 in 5 families needing specialty care had difficulty obtaining access, with children insured by CHIP having modestly higher rates of difficulty (28% [24%-32%]) compared with children enrolled in Medicaid ($P = .06$) and private insurance ($P = .03$).

Standardized Estimates of Perception of Unmet Needs

Unmet medical and dental needs were uncommon for insured children: only 2% (2%-2%) of privately insured children, 2% (2%-3%) of those insured by Medicaid, and 3% (3%-4%) of those insured by CHIP had unmet medical needs compared with 10% (8%-12%) of uninsured children ($P < .001$

for all 3 contrasts). This pattern was similar for unmet dental needs. However, children insured by Medicaid and CHIP were more likely to have insurance that always met their needs (Medicaid, 78% [76%-80%]; CHIP, 78% [75%-80%]) than were privately insured children (73% [72%-75%]) ($P = .002$ and $P = .004$, respectively). The caregivers of more than 80% of children across all insurance types reported satisfaction with the ability to see needed health care providers (Medicaid, 82% [80%-84%]; CHIP, 84% [82%-86%]; and private insurance, 83% [82%-84%]).

Standardized Estimates of Care Coordination, Satisfaction With Care, and Out-of-Pocket Costs

Twenty percent (17%-23%) of caregivers of children enrolled in Medicaid reported frustration obtaining health care services compared with 23% (21%-24%) for privately insured children and 26% (23%-28%) for those insured by CHIP. Respondents insured by CHIP were significantly more likely to report such frustration than those with Medicaid plans ($P = .004$). Approximately 70% of insured respondents received care coordination when needed across all plan types (Medicaid, 72% [68%-75%]; CHIP, 68% [65%-72%]; and private insurance, 70% [68%-72%]) compared with less than half (47% [41%-53%]) of uninsured respondents ($P < .001$ for all 3 contrasts). A similar pattern was found for receipt of family-centered care. In contrast, caregivers of privately insured children had the highest

Table 2. Adjusted Probability of Outcomes for Entire Cohort^a

Outcome	Adjusted Probability (95% CI)			
	Uninsured	Medicaid	CHIP	Private Insurance
Preventive and specialty care				
Reported receiving ≥1 preventive medical visit	0.67 (0.65-0.69)	0.88 (0.86-0.89) ^b	0.88 (0.87-0.89) ^b	0.83 (0.82-0.84)
Reported having a personal physician or nurse	0.73 (0.70-0.75)	0.89 (0.88-0.90)	0.89 (0.88-0.91)	0.89 (0.89-0.90)
Reported receiving ≥1 preventive dental visit ^c	0.54 (0.52-0.57)	0.80 (0.78-0.81) ^b	0.77 (0.76-0.79) ^b	0.73 (0.72-0.74)
Reported having a usual source of health care ^d	0.80 (0.78-0.83)	0.93 (0.92-0.95)	0.94 (0.93-0.95)	0.94 (0.93-0.95)
Reported a problem seeing a specialist ^e	0.33 (0.25-0.40)	0.23 (0.19-0.26)	0.28 (0.24-0.32) ^f	0.22 (0.20-0.24)
Reported a problem obtaining a referral ^{d,e}	0.27 (0.16-0.37)	0.18 (0.14-0.23)	0.23 (0.18-0.29) ^f	0.15 (0.13-0.18)
Unmet health care needs				
Reported an unmet medical need	0.10 (0.08-0.12)	0.02 (0.02-0.03) ^g	0.03 (0.03-0.04) ^b	0.02 (0.02-0.02)
Reported an unmet dental need ^c	0.12 (0.10-0.13)	0.04 (0.03-0.04) ^g	0.05 (0.04-0.06)	0.04 (0.04-0.05)
Reported insurance always meets child's health care needs ^d	NA	0.78 (0.76-0.80) ^b	0.78 (0.75-0.80) ^b	0.73 (0.72-0.75)
Reported insurance always allows child to see needed health care providers ^d	NA	0.82 (0.80-0.84)	0.84 (0.82-0.86)	0.83 (0.82-0.84)
Care coordination, satisfaction, and out-of-pocket costs				
Reported frustration obtaining health care services ^h	0.61 (0.56-0.66)	0.20 (0.17-0.23) ⁱ	0.26 (0.23-0.28)	0.23 (0.21-0.24)
Received effective care coordination when needed ^{d,e}	0.47 (0.41-0.53)	0.72 (0.68-0.75)	0.68 (0.65-0.72)	0.70 (0.68-0.72)
Received family-centered care ^d	0.49 (0.45-0.53)	0.67 (0.64-0.69)	0.66 (0.64-0.69)	0.66 (0.65-0.68)
Reported out-of-pocket costs for child's health care ^{d,j}	NA	0.26 (0.23-0.28) ^{b,i}	0.38 (0.35-0.40) ^b	0.77 (0.75-0.78)

Abbreviations: CHIP, Children's Health Insurance Program; NA, not applicable.

^a Derived from National Survey of Children's Health data from 2003, 2007, and 2011-2012. Predicted probabilities were estimated from a logistic regression model with a robust variance estimator. All models adjusted for calendar year, child-level demographic and household characteristics (age, sex, race/ethnicity, special health care needs, household income, household educational level, family structure, urbanicity as measured by metropolitan statistical area), and state-level characteristics (Medicaid-to-Medicare fee index, poverty rate, and unemployment rate). Estimates were weighted to represent the population of noninstitutionalized children 17 years and younger in 48 states and the District of Columbia.³⁰

^b Significantly different from private insurance; $P < .01$.

^c Models for dental health outcomes excluded children younger than 1 year.

^d These questions were asked only in the 2007 and 2011-2012 National Surveys of Children's Health.

^e These questions were asked only for the subset of children who needed the service (ie, needed to see a specialist, needed a referral, or needed care coordination).

^f Significantly different from private insurance; $P < .05$.

^g Significantly different from CHIP; $P < .05$.

^h This question was asked only in the 2011 National Survey of Children's Health.

ⁱ Significantly different from CHIP; $P < .01$.

^j Out-of-pocket costs do not include premiums or costs paid by insurance.

prevalence of out-of-pocket costs (77% [75%-78%]) compared with caregivers of children insured by Medicaid (26% [23%-28%]; $P < .001$) and CHIP (38% [35%-40%]; $P < .001$).

Young Children and Children With Special Health Care Needs

The analysis of children with special health care needs revealed additional challenges for privately insured children (Table 3). For example, 29% (26%-33%) of caregivers of privately insured children reported a problem accessing a specialist compared with caregivers of children insured by CHIP (25% [20%-31%]; $P = .25$) and Medicaid (20% [15%-25%]; $P = .007$). In addition, only 63% (60%-67%) of respondents with privately insured children reported that their insurance always met their needs, well below that observed in CHIP (73% [68%-77%]; $P = .006$) and Medicaid (76% [71%-81%]; $P < .001$). Thirty-six percent (32%-41%) of respondents with privately insured children expressed frustration obtaining health care services compared with 28% (21%-34%) of those insured by Medicaid ($P = .05$). Finally, caregivers of privately insured children were most likely to experience out-of-pocket costs (80% [78%-83%]) compared with caregivers of

children insured by CHIP (40% [35%-46%]; $P < .001$) and Medicaid (23% [18%-27%]; $P < .001$).

In contrast, caregivers of younger children (aged ≤5 years) reported access challenges for specialty care that were similar to the aggregate responses reported above, with an exception: caregivers of younger children with both private insurance (18% [13%-22%]; $P = .01$) and CHIP (19% [12%-26%]; $P = .01$) reported greater problems obtaining referrals than did those insured by Medicaid (9% [5%-13%]) (Table 4). In addition, less than half of children aged 5 years or younger with private insurance received a preventive dental visit (48% [46%-50%]) compared with those insured by Medicaid (56% [52%-59%]; $P = .001$) and CHIP (60% [56%-64%]; $P < .001$). Caregivers of privately insured children (21% [19%-24%]; $P = .05$) and those with children insured by CHIP (27% [22%-32%]; $P = .001$) had higher rates of frustration obtaining health care services than did those with children insured by Medicaid (16% [13%-20%]). Similar to other families, caregivers with young children experienced the highest likelihood of out-of-pocket costs with private insurance (71% [68%-74%]) compared with children insured by CHIP (27% [22%-32%]; $P < .001$) and Medicaid (21% [17%-25%]; $P < .001$).

Table 3. Adjusted Probability of Outcomes for Children With Special Health Care Needs^a

Outcome	Adjusted Probability (95% CI)			
	Uninsured	Medicaid	CHIP	Private Insurance
Preventive and specialty care				
Reported receiving ≥1 preventive medical visit	0.76 (0.70-0.82)	0.92 (0.90-0.94)	0.91 (0.89-0.94)	0.90 (0.89-0.92)
Reported having a personal physician or nurse	0.80 (0.75-0.85)	0.93 (0.91-0.95)	0.93 (0.91-0.95)	0.94 (0.92-0.95)
Reported receiving ≥1 preventive dental visit ^b	0.63 (0.55-0.71)	0.86 (0.83-0.89) ^c	0.83 (0.80-0.86)	0.81 (0.78-0.83)
Reported having a usual source of health care ^d	0.84 (0.77-0.90)	0.97 (0.95-0.98)	0.94 (0.92-0.96) ^c	0.97 (0.96-0.98)
Reported a problem seeing a specialist ^e	0.53 (0.39-0.67)	0.20 (0.15-0.25) ^f	0.25 (0.20-0.31)	0.29 (0.26-0.33)
Reported a problem obtaining a referral ^{d,e}	0.49 (0.28-0.71)	0.20 (0.13-0.27)	0.22 (0.16-0.29)	0.20 (0.15-0.24)
Unmet health care needs				
Reported an unmet medical need	0.17 (0.12-0.23)	0.04 (0.02-0.06)	0.06 (0.04-0.07) ^c	0.03 (0.02-0.04)
Reported an unmet dental need ^b	0.17 (0.13-0.22)	0.05 (0.03-0.06) ^g	0.08 (0.05-0.10)	0.06 (0.05-0.08)
Reported insurance always meets child's health care needs ^d	NA	0.76 (0.71-0.81) ^f	0.73 (0.68-0.77) ^f	0.63 (0.60-0.67)
Reported insurance always allows child to see needed health care providers ^d	NA	0.81 (0.77-0.86)	0.77 (0.73-0.82)	0.76 (0.72-0.79)
Care coordination, satisfaction, and out-of-pocket costs				
Reported frustration obtaining health care services ^h	0.76 (0.65-0.88)	0.28 (0.21-0.34) ^c	0.33 (0.27-0.39)	0.36 (0.32-0.41)
Received effective care coordination when needed ^{d,e}	0.43 (0.29-0.57)	0.65 (0.59-0.72)	0.64 (0.58-0.70)	0.62 (0.58-0.66)
Received family-centered care ^d	0.48 (0.38-0.58)	0.74 (0.69-0.79) ^g	0.67 (0.62-0.72)	0.67 (0.64-0.71)
Reported out-of-pocket costs for child's health care ^{d,i}	NA	0.23 (0.18-0.27) ^{f,j}	0.40 (0.35-0.46) ^f	0.80 (0.78-0.83)

Abbreviations: CHIP, Children's Health Insurance Program; NA, not applicable.

^a Derived from National Survey of Children's Health data from 2003, 2007, and 2011-2012. Predicted probabilities were estimated from a logistic regression model with a robust variance estimator. All models adjusted for calendar year, child-level demographic and household characteristics (age, sex, race/ethnicity, household income, household educational level, family structure, urbanicity as measured by metropolitan statistical area), and state-level characteristics (Medicaid-to-Medicare fee index, poverty rate, and unemployment rate). Estimates were weighted to represent the population of noninstitutionalized children 17 years and younger in 48 states and the District of Columbia.³⁰

^b Models for dental health outcomes excluded children younger than 1 year.

^c Significantly different from private insurance; $P < .05$.

^d These questions were asked only in the 2007 and 2011-2012 National Surveys of Children's Health.

^e These questions were asked only for the subset of children who needed the service (ie, needed to see a specialist, needed a referral, or needed care coordination).

^f Significantly different from private insurance; $P < .01$.

^g Significantly different from CHIP; $P < .05$.

^h This question was asked only in the 2011 National Survey of Children's Health.

ⁱ Out-of-pocket costs do not include premiums or costs paid by insurance.

^j Significantly different from CHIP; $P < .01$.

Discussion

This study examined the experiences with health insurance coverage for families with incomes between 100% and 300% of the FPL and found consistently high levels of preventive care receipt for all insured children. However, preventive medical and dental visits were more prevalent for children insured by Medicaid and CHIP than for privately insured children. These findings are consistent with other published studies and demonstrate reassuringly high rates of access to dental care for children insured by Medicaid and CHIP.^{10,13,17,18} However, as many as 1 in 4 caregivers reported difficulty accessing specialty care and frustration obtaining health care services, with some evidence of greater difficulty among those enrolled in CHIP. In addition, nearly one-third of caregivers of privately insured children with special health care needs reported such challenges. This finding is consistent with a recent study of children with special health care needs that found greater adequacy of coverage in public insurance than in private insurance.³¹ Finally, caregivers of privately insured children were substantially more likely to experience out-of-pocket costs than

were those with children insured by Medicaid or CHIP, with the lowest likelihood of out-of-pocket costs being for those covered by Medicaid.

The implications of our findings are best considered within the shifting landscape of children's insurance coverage. Under the Affordable Care Act, QHPs are expanding the availability of private insurance for families with low incomes, but the early experiences with QHPs have been mixed. First, QHPs are required to include 10 essential health benefits. However, a recent review of state benchmark plans (on which QHPs are based) revealed that no plan included a definition of pediatric services, one of the required benefits.² Second, cost sharing has been found to be higher in QHPs, mirroring trends in the private insurance market.⁵ Third, new practices in the private market (specifically, tiering of provider networks) are a concern in QHPs. These practices could adversely affect specialty access in pediatrics due to unique shortages of specialty health care providers and concentrations of such providers in children's hospital networks.^{2,8}

Our findings provide empirical data for the ongoing debate about subsidized coverage for children. The high reported rates of preventive care receipt and perception of

Table 4. Adjusted Probability of Outcomes for Children 5 Years and Younger^a

Outcome	Adjusted Probability (95% CI)			
	Uninsured	Medicaid	CHIP	Private Insurance
Preventive and specialty care				
Reported receiving ≥1 preventive medical visit	0.80 (0.76-0.83)	0.94 (0.93-0.95)	0.94 (0.93-0.96)	0.93 (0.92-0.94)
Reported having a personal physician or nurse	0.79 (0.75-0.82)	0.90 (0.88-0.92)	0.91 (0.89-0.93)	0.91 (0.89-0.92)
Reported receiving ≥1 preventive dental visit ^b	0.35 (0.30-0.39)	0.56 (0.52-0.59) ^c	0.60 (0.56-0.64) ^c	0.48 (0.46-0.50)
Reported having a usual source of health care ^d	0.87 (0.83-0.91)	0.93 (0.91-0.95) ^e	0.96 (0.95-0.98)	0.94 (0.93-0.96)
Reported a problem seeing a specialist ^f	0.31 (0.16-0.47)	0.19 (0.14-0.24) ^e	0.29 (0.20-0.38) ^g	0.20 (0.17-0.23)
Reported a problem obtaining a referral ^{d,f}	0.31 (0.15-0.48)	0.09 (0.05-0.13) ^{e,g}	0.19 (0.12-0.26)	0.18 (0.13-0.22)
Unmet health care needs				
Reported an unmet medical need	0.08 (0.05-0.11)	0.02 (0.01-0.03) ^e	0.04 (0.02-0.06)	0.03 (0.02-0.04)
Reported an unmet dental need ^b	0.06 (0.04-0.08)	0.02 (0.01-0.03)	0.03 (0.02-0.04)	0.03 (0.02-0.03)
Reported insurance always meets child's health care needs ^d	NA	0.82 (0.78-0.85)	0.83 (0.79-0.87) ^g	0.78 (0.76-0.80)
Reported insurance always allows child to see needed health care providers ^d	NA	0.86 (0.82-0.89)	0.89 (0.87-0.92)	0.87 (0.85-0.89)
Care coordination, satisfaction, and out-of-pocket costs				
Reported frustration obtaining health care services ^h	0.57 (0.46-0.67)	0.16 (0.13-0.20) ^{g,i}	0.27 (0.22-0.32)	0.21 (0.19-0.24)
Received effective care coordination when needed ^{d,f}	0.55 (0.41-0.69)	0.74 (0.69-0.80)	0.65 (0.58-0.73)	0.71 (0.66-0.75)
Received family-centered care ^d	0.62 (0.55-0.70)	0.72 (0.68-0.75)	0.69 (0.65-0.74)	0.72 (0.70-0.75)
Reported out-of-pocket costs for child's health care ^{d,j}	NA	0.21 (0.17-0.25) ^{c,e}	0.27 (0.22-0.32) ^c	0.71 (0.68-0.74)

Abbreviations: CHIP, Children's Health Insurance Program; NA, not applicable.

^a Derived from National Survey of Children's Health data from 2003, 2007, and 2011-2012. Predicted probabilities were estimated from a logistic regression model with a robust variance estimator. All models adjusted for calendar year, child-level demographic and household characteristics (age, sex, race/ethnicity, special health care needs, household income, household educational level, family structure, urbanicity as measured by metropolitan statistical area), and state-level characteristics (Medicaid-to-Medicare fee index, poverty rate, and unemployment rate). Estimates were weighted to represent the population of noninstitutionalized children 17 years and younger in 48 states and the District of Columbia.³⁰

^b Models for dental health outcomes excluded children younger than 1 year.

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ⁱ Significantly different from CHIP; $P < .01$.

^j Out-of-pocket costs do not include premiums or costs paid by insurance.

Medicaid and CHIP coverage meeting children's needs, together with concerns about limited access and increased cost sharing in private plans, might caution against calls for expanded private (ie, QHP) coverage for children and substantiate advocacy for extending CHIP coverage beyond 2017. However, this study uncovered some challenges in access to services and specialty care for both children with CHIP coverage and privately insured children with special health care needs. Although the etiology of these challenges is not well understood, these findings suggest that Medicaid might serve children in families with low to moderate incomes better than other coverage types.

Nonetheless, strengthened insurance exchanges could provide an option for families with low to moderate incomes to purchase coverage. The goal would be the creation of a continuum of family coverage, from Medicaid plans to QHPs, but concerns about access to specialty care, affordable dental coverage, and cost sharing would require attention. One way to ensure the comprehensiveness of coverage currently available in states' Medicaid programs would be to require QHPs to match those programs' benefit and cost-sharing provisions. Finally, creating protections in the QHP market by limiting tiering of regionally scarce specialty pedi-

atric health care providers could curtail the concerns about access to specialty care reported for children with special health care needs in this study.

We acknowledge the following limitations. First, the survey reported point-in-time insurance measures coupled with income and coverage quality measures from the prior 12 months. It is possible that coverage and eligibility at the time of the survey was not reflective of families' experiences throughout the year. Second, there was potential for misclassification of insurance type. We attempted to minimize misclassification by excluding children with functional limitations, whose Medicaid eligibility might have been based on disabilities. In addition, while our methods for classifying children into Medicaid and CHIP coverage have not been externally validated, we were reassured that the correlations between our NSCH-derived enrollment estimates for CHIP and Medicaid and administrative estimates across states and time were very high. Nevertheless, the extent to which some children's insurance type was misclassified might have biased results toward the null. Third, the amount of out-of-pocket expenses would have been preferable to the binary variable for any out-of-pocket costs used in our analysis. Others have reported much

higher out-of-pocket expenses among families with private insurance vs CHIP coverage.⁷ Fourth, while we adjusted for state-by-state differences in Medicaid payments and poverty, we acknowledge that there might have been systematic unmeasured differences across states (eg, strength of the safety net) that could limit the generalizability of our national estimates and overstate the influence of insurance type on access to health care within some geographic areas.

Conclusions

The findings of this study reveal important differences between insurance coverage options for families with low to moderate incomes in preventive care, access to specialty care, and cost sharing. Ongoing evaluation is needed to ensure that policy decisions are responsive to these differences so that historical strides made in pediatric health coverage do not recede.

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Author Contributions: Ms Kreider and Dr French had full access to all the data in the study and take responsibility for the integrity of the data and the accuracy of the data analysis.
Study concept and design: Kreider, French, Aysola, Noonan, Rubin.
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Exhibit 29

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

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CHILDHOOD MEDICAID COVERAGE AND LATER LIFE HEALTH CARE UTILIZATION

Laura R. Wherry
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Working Paper 20929
<http://www.nber.org/papers/w20929>

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Childhood Medicaid Coverage and Later Life Health Care Utilization
Laura R. Wherry, Sarah Miller, Robert Kaestner, and Bruce D. Meyer
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ABSTRACT

Policy-makers have argued that providing public health insurance coverage to the uninsured lowers long-run costs by reducing the need for expensive hospitalizations and emergency department visits later in life. In this paper, we provide evidence for such a phenomenon by exploiting a legislated discontinuity in the cumulative number of years a child is eligible for Medicaid based on date of birth. We find that having more years of Medicaid eligibility in childhood is associated with fewer hospitalizations and emergency department visits in adulthood for blacks. Our effects are particularly pronounced for hospitalizations and emergency department visits related to chronic illnesses and those of patients living in low-income neighborhoods. Furthermore, we find evidence suggesting that these effects are larger in states where the difference in the number of Medicaid-eligible years across the cutoff birthdate is greater. Calculations suggest that lower rates of hospitalizations and emergency department visits during one year in adulthood offset between 3 and 5 percent of the initial costs of expanding Medicaid.

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I. Introduction

One of the goals of publicly-subsidized health insurance is to improve the health of those without insurance. The argument underlying this policy is straightforward: health insurance provides the means to use more, and more timely, medical care, and because of this greater use of care, health is improved. As a result, those who gain coverage may need fewer expensive hospital and emergency department visits later in life due to their improved health, and these long term changes in utilization may partially or completely offset the initial cost of insurance provision. While the intuition behind this argument is strong, the empirical evidence to support it is relatively weak. For example, the Oregon Medicaid Experiment did not find significant health benefits from health insurance during the first two years of coverage and found that the provision of Medicaid increased, rather than decreased, the use of costly hospital and emergency department care (Finkelstein et al. 2012, Taubman et al. 2014).¹

One limitation of the literature in this area is its relatively short time horizon. Most studies seek to link health insurance to health contemporaneously, or for a few subsequent years. However, the health benefits of insurance may be cumulative and revealed only after a sustained period of insurance and regular use of medical care. In addition, certain types of medical care focus on protecting the patient from future health risks and the payoffs from these types of preventive services may not be evident until later in life. In both scenarios, shorter windows of analysis may not be adequate to identify the health benefits insurance.

A second limitation of studies evaluating contemporaneous effects of public health insurance on hospitalizations and emergency department visits is that they are unable to isolate the potential health benefit of insurance from the increased access that results from insurance. For example, they cannot separate an improvement in health due to insurance that could ultimately result in fewer hospitalizations from the concurrent access effects of insurance that lower out-of-pocket costs and induce greater use of care including hospitalizations. Even if an individual's health improves as a result of public insurance coverage, the access effect may dominate in the short term, leading to higher utilization of medical services.

¹ Medicaid coverage was associated with improvements in self-reported health, but no change in physical and clinical health measures (Finkelstein et al. 2012, Baicker et al. 2013).

In this paper, we address these issues by examining whether the expansion of Medicaid in the late 1980s and early 1990s improved the health later in life of those affected. Specifically, we exploit plausibly exogenous variation by birthdate in the cumulative number of years an individual was eligible for public health insurance coverage. To phase in the Medicaid expansions, Congress specified that several eligibility expansions for low-income children applied only to children born after September 30, 1983. As a result, children born before September 30, 1983 experienced lower rates of Medicaid eligibility and fewer Medicaid-eligible years in childhood than children born immediately following the cutoff. This discontinuity in eligibility was first identified and used by Card and Shore-Sheppard (2004) to examine contemporaneous changes in insurance coverage. Wherry and Meyer (forthcoming) later demonstrated that the policy led to cumulative differences in childhood eligibility. They estimated that a child in a family with income just under the Federal Poverty Level (FPL) gained approximately five additional years of Medicaid eligibility during childhood if she were born on October 1, 1983 rather than September 30, 1983. Black children were particularly likely to benefit from the Medicaid expansions, gaining on average more than twice the number of Medicaid-eligible years of white children.

We exploit this policy discontinuity as a source of exogenous variation in Medicaid eligibility in order to evaluate the long-term effects of Medicaid. The outcomes we examine are hospital and emergency department (ED) visits derived from administrative data from all states that make such data available. These databases capture the universe of hospitalizations or ED visits in each state for a given year and provide sufficiently large sample sizes to detect changes in utilization among the young (e.g., age 25) population that we study. In addition to birth year and month, the data also provide information on other patient characteristics including race and median income of the zipcode of residence. This information allows us to examine changes in hospitalization and ED use among groups that were especially likely to be affected by the change in Medicaid policy.

An important contribution of our study is that we are able to isolate the potential health benefits of insurance from any access effects of health insurance on hospitalizations and ED visits. We are able to disentangle the health effect of insurance on utilization from changes in out-of-pocket costs by analyzing the hospitalizations and use of ED care of young adults later in life, when

there are no longer policy-driven differences in Medicaid eligibility or out-of-pocket costs between our treatment and control groups that could drive utilization patterns. We examine the effects of coverage one year after the cohorts have experienced the additional Medicaid coverage (at age 15) and ten years later (at age 25), allowing us to capture both immediate and longer-term effects.

Our study is also informative on the dynamic technology of the production of health, a topic of great recent research interest. For example, Heckman (2007) models current capabilities, which include cognitive and non-cognitive ability and health, as a function of initial values and all past and current investments. While he points to several features of this function that research has elucidated, other features have not been empirically determined. Understanding this process is important for example when designing policies to ameliorate disadvantage. We are able to estimate the effects of investments in child health for a disadvantaged population in the pre-teen and early teen years on health over ten years later. We thus advance our understanding of the process of how health at one age is altered by investments at another.

We find no immediate effects of the expansions on health care utilization at age 15. However, we find sizeable effects of Medicaid eligibility in childhood on hospitalizations and emergency department visits at age 25 among black cohorts who gained coverage. Black cohorts born immediately after the cutoff are estimated to experience approximately 7 to 15 percent fewer hospitalizations and 2 to 5 percent fewer emergency department visits at age 25 relative to those born just before the cutoff. Our results are particularly pronounced for hospitalizations and emergency department visits related to chronic illnesses and among patients from low-income zip codes. Additionally, our analysis suggests that these effects are largest in states where the discontinuity in the cumulative number of Medicaid eligible years is greatest. We do not find reductions in the utilization of non-blacks (who experienced smaller gains in Medicaid eligibility at the birth date cutoff), nor do we find effects for hospitalizations related to appendicitis or injury, two conditions that are unlikely to be affected by access to care in childhood. Placebo tests using earlier birth cohorts to estimate breaks at non-discontinuity points indicate that any discontinuities at these false “cutoff” points are small relative to the effects we estimate at the true birth date cutoff.

Our results provide several insights that are relevant to current policy debates surrounding the provision of public health insurance and the role of government in expanding coverage. First, our estimates indicate that between 3 and 5 percent of the initial cost of the Medicaid expansions for children were “offset” by lower hospitalization and emergency department usage at age 25 alone, with a substantial fraction of these cost savings accruing to the government in the form of lower hospital payments for publicly-insured patients. If these effects persist, then the size of the cost offset is likely to be even greater. Second, our results highlight the importance of evaluating these programs over a longer time period. Indeed, we find no impact of Medicaid coverage in our analysis of the “immediate” effect at age 15, but do find effects later in life at age 25. These findings suggest that the benefits of insurance may only materialize over a long horizon.

II. Background

High-quality analyses of Medicaid eligibility expansions for children consistently show that Medicaid increases health care utilization, including hospitalizations, in the short term.² However, there are fewer studies of the effects of gaining Medicaid on children’s health and the evidence from this literature is mixed. A number of studies using parental reports of child health find no evidence of improvement under public insurance, while several papers document significant declines in child mortality.³ Thus, the effect of gaining health insurance on health remains an important but unanswered question.

One limitation of studies seeking to assess the effect of insurance on health is that they examine how coverage affects health, for example, as measured by hospital admissions, immediately after or within a few years of the coverage expansion. If the health benefits of insurance are realized later, then a contemporaneous or short-run analysis may miss much of the effect of insurance. An

² See evidence of increased hospital use in Dafny and Gruber (2005), Currie and Gruber (1996a) and Boudreaux, Golberstein, and McAlpine (2014). In addition, Currie and Gruber (1996a), Card and Shore-Sheppard (2004), and Currie, Decker and Lin (2008) present evidence indicating an increase in annual doctor visits under expanded public insurance.

³ For example, Currie, Decker and Lin (2008), Currie and Gruber (1995), De La Mata (2012), and Racine et al. (2001) find no change in subjective measures of child health such as child health status and activity limitations. Meanwhile, Currie and Gruber (1996a, 1996b), Goodman-Bacon (2014), Howell et al. (2010), and Wherry and Meyer (forthcoming) find significant effects on infant or child mortality. Not all studies, however, find mortality improvements; Decker, Almond, and Simon (2015) find no evidence of changes in maternal and child mortality under the rollout of Medicaid. See Howell and Kenney (2012) for further discussion of this literature.

emerging literature on the longer-term effects of health insurance coverage in childhood on later life outcomes has begun to address this issue. Wherry and Meyer (forthcoming) examine the later life mortality of cohorts born before and after the 1983 cutoff date specified in many expansions of Medicaid. They provide evidence linking this increase in childhood eligibility to a later decline in teenage mortality for black children who were more likely to gain eligibility under the expansions than white children. Boudreaux, Golberstein and McAlpine (2014) use variation in the timing of the introduction of the Medicaid program across states in the 1960s to identify long-term effects among cohorts with different exposure to the program. They find that those who gained access to Medicaid early in childhood were less likely to report having a chronic illness as an adult. Brown, Kowalski, and Lurie (2015) also use state-level variation in the timing of the Medicaid expansions for children in the 1980s to examine long-term effects and find that cohorts who gained coverage have higher wages, receive lower earned income tax credit payouts, have higher graduation rates and lower mortality as adults. Also relying on state-level variation, Cohodes et al. (forthcoming) find that cohorts who gained coverage in childhood as a result of these Medicaid expansions have higher educational attainment, and Miller and Wherry (2015) find that cohorts whose mothers had higher eligibility rates for prenatal coverage while the cohort was in utero had better health outcomes and fewer hospitalizations in adulthood related to chronic health conditions, as well as higher rates of high school graduation.

In this paper, we add to this literature by exploiting the discontinuity in Medicaid eligibility and coverage among those born around September 30, 1983 to study the effect of childhood Medicaid eligibility on hospitalizations and emergency department visits at age 25. There is only limited study of the long term health effects of Medicaid eligibility, so our paper adds to this literature. In addition, with the exception of Wherry and Meyer (forthcoming), all previous papers on the long-term effects of Medicaid coverage use state and year level variation in Medicaid policy to examine long-term outcomes. Although this empirical approach has been used many times in the literature, some authors have pointed out its limitations (e.g., the estimates tend to be sensitive to the inclusion of state-specific trends; see Dave et al. 2008). The regression discontinuity design we employ allows us to examine the effects of childhood Medicaid coverage in a way that is arguably more credible because it does not rely on using policy changes at the state level as an instrument for eligibility.

III. The Policy Discontinuity

Discontinuity in Eligibility

Prior to the 1980s, eligibility for Medicaid for non-disabled children was primarily limited to children in families receiving cash welfare under the Aid to Dependent Families with Children (AFDC) program. Recipients of AFDC benefits were primarily single-mother families with very low income levels, often well below the poverty line.⁴ Beginning in the mid-1980s, Congress took steps to expand eligibility for Medicaid to children not participating in AFDC who would otherwise be ineligible for Medicaid benefits. In a series of legislative acts, eligibility for Medicaid was expanded to all children with family incomes at or below the poverty line, regardless of family structure or participation in the AFDC program.

In an effort to phase in changes in Medicaid eligibility, Congress specified that many of the legislative changes applied only to children born after September 30, 1983. This provision meant that children born just before and after this birthdate cutoff faced very different eligibility criteria for Medicaid during their childhood years. Wherry and Meyer (forthcoming) simulate childhood eligibility for public health insurance for cohorts born on either side of this birthdate cutoff.⁵ They show that this unique feature of the expansions led to a large discontinuity in the number of years of Medicaid eligibility during childhood for cohorts born at this birthdate.

Given the nature the expansions, the discontinuity was largest for children with family incomes below the poverty line and above AFDC income levels. Figure 1 displays the average number of years of childhood eligibility for public insurance by birth month cohort for children in families

⁴ Income limits for the AFDC program were established by states and ranged from 14 to 79 percent of the federal poverty line in 1989 (U.S. General Accounting Office 1989).

⁵ The authors use a random sample of children of ages 0-17 from each year of the 1981-1988 March Supplement to the Current Population Survey (CPS) and estimate eligibility for this pooled sample if born in each month between October 1979 and September 1987. They employ detailed federal and state public health insurance eligibility rules for the years 1979 to 2005 to estimate eligibility status for each month during childhood through age 17. This simulation holds family characteristics, including state of residence, family structure and size, parent employment and family income, constant over the child's lifetime. See Wherry and Meyer (forthcoming) for additional information.

with incomes below 150 percent of the federal poverty line. The magnitude of the discontinuity in childhood eligibility at the September 30, 1983 cutoff is largest for children in families with incomes between 75 and 100 percent of the poverty line. The gain represents an additional 4.6 years of eligibility during childhood. Children with incomes between 50 and 75 percent of poverty, as well as those with incomes between 25 and 50 percent of poverty, also experience sizeable gains with an additional 3.4 and 2.0 years of eligibility, respectively.

Figure 2 reveals that the gain in eligibility was primarily concentrated at ages 8 to 14 for children born immediately after the birthdate cutoff. This graph plots the share of the September versus October 1983 birth cohorts eligible for public health insurance at each age during childhood by race. Eligibility levels are similar for the two cohorts prior to age 8 and again starting at age 15. These cohorts were approximately 8 years of age at the implementation of the Omnibus Budget Reconciliation Act of 1990 (OBRA90), which required all state Medicaid programs to cover children under age 19 born after September 30, 1983. This legislation was responsible for most of the discontinuity. Later, the State Children's Health Insurance Program (CHIP) authorized state expansions of public health insurance to children in higher income families. The CHIP expansions served to close the gap in public eligibility for cohorts born on either side of the cutoff at around age 15. We examine health care utilization for cohorts born just before and after September 30, 1983 following the differential gain in Medicaid eligibility at age 15 and then 10 years later at age 25.

We also examine differential effects of the expansions by race and by state of residence. Black children were more likely to gain eligibility under the expansions (Table 1) due to their family incomes. On average, black children born in October versus September 1983 were 17 percentage points more likely to gain Medicaid eligibility. Among those who were made Medicaid-eligible, the average gain in eligibility throughout childhood was 4.9 years. This is over twice the average years of eligibility gained by non-black children, who experienced a 9 percentage point gain in eligibility across the birth date threshold that led, on average, to 4.4 additional Medicaid-eligible years throughout childhood.⁶

⁶ Wherry and Meyer (forthcoming) estimate eligibility gains by child race and state of residence using a similar methodology but rely on a national sample that draws children from the CPS by race and state cells. The estimates presented here differ slightly in that they rely on state-specific

The gain in Medicaid eligibility for children born after the cutoff also varied by state due primarily to differences in Medicaid policies in place before the expansions and differences in state socioeconomic characteristics.⁷ For example, the impact of the requirement that states cover all poor children would depend on both prior eligibility levels determined by the state's AFDC eligibility threshold (e.g. 14% FPL vs. 79% FPL) and the concentration of children in the state with family incomes between AFDC eligibility thresholds and the poverty line.

Table 2 presents estimates of the average eligibility gain at the cutoff for each state in our study.⁸ These estimates were calculated using state-specific samples of children from the CPS and therefore capture the average magnitude of the discontinuity at the cutoff given a state's eligibility rules and distribution of family characteristics. The size of the discontinuity in eligibility varies from 0.05 years of eligibility in California to 1.33 years of eligibility in Arkansas.⁹ We use this variation to estimate differential effects across states associated with the policy change.

Discontinuity in Coverage and Utilization

In addition to a demonstrated discontinuity in childhood eligibility, we also measure any corresponding discontinuity in childhood coverage. It is important to bear in mind, however, that even if they did not take active steps to enroll in the program, all children gaining eligibility for

samples of all children ages 0-17 pooled for the 1981-1988 years of the March CPS. See the appendix for additional discussion.

⁷ Although many of the expansions were first introduced as state options, Wherry and Meyer (forthcoming) estimate that, when holding socioeconomic characteristics fixed, the majority of the variation in eligibility at the September 30, 1983 cutoff across states resulted from the federal requirement for all states to cover poor children born after this date rather than from optional state expansions. See the appendix of their paper for additional information.

⁸ Some prior work has excluded Arizona from analyses of Medicaid expansions for children due to its late implementation of Medicaid. We include Arizona in this analysis because, although slow to implement Medicaid, the state provided government-supported health care for families on AFDC both prior to and following the introduction of its Medicaid program in 1982 (Freeman and Kirkman-Liff 1985). In addition, the federal mandate to expand eligibility for children born after September 30, 1983 with family incomes up to the poverty line in 1991 applied to all states including Arizona (Congressional Research Service 1988).

⁹ California represents a large population and experienced the smallest policy discontinuity in our sample. For that reason, we also estimate models that exclude California from our analysis (see Appendix Tables 4-8). As expected, specifications that exclude California tend to uncover larger but somewhat less precise effects.

Medicaid had “conditional coverage” in that their expenses were covered in the event of hospitalization or the need for costly medical care (Cutler and Gruber 1996). Not only could eligibility be granted retroactively for a period of up to 3 months prior to the date of application, but many states were giving children the opportunity to sign up for Medicaid at the sites where they received health care (Congressional Research Service 1993). Since the value of Medicaid is highest when children are sick, parents are likely to wait until medical care is needed to sign up for coverage (Marton and Yelowitz 2014).

Card and Shore-Sheppard (2004) first examined changes in Medicaid enrollment for children born after September 30, 1983 following the expansions in Medicaid eligibility. The authors found Medicaid take up rates of between 8 and 11 percent among the newly eligible, with little evidence of substitution of public for private coverage (i.e., crowd out). In an analysis of similar spirit, we explore differences in the discontinuity in coverage by child race. We use the pooled 1992-1996 National Health Insurance Survey (NHIS) Health Insurance Supplements to examine changes in reported Medicaid coverage during the last month for cohorts born after September 30, 1983 at ages 8-13.¹⁰ We estimate a simple regression discontinuity model and regress Medicaid coverage on an indicator for birth cohorts October 1983 and later, a quadratic function in birth month cohort interacted with this indicator, and a set of calendar month fixed effects. We estimate this specification using 4-, 3-, and 2-year observation windows of birth month cohorts on either side of the birthdate cutoff. Standard errors are heteroskedasticity-robust and are allowed to be non-independent within birth month cohort cluster. We also use local linear regression to estimate the discontinuity in Medicaid coverage at the September 30, 1983 cutoff relying on two different data-driven optimal bandwidth selectors. More details on these estimation methods are described later in Section V.

Figure 3 plots reported levels of Medicaid coverage for each birth month cohort with a 4-year window on each side of the birthdate cutoff (centered at October 1983). The lines are fitted values from a quadratic regression function in birth month cohort interacted with a dummy variable for cohorts born after September 30, 1983. The graphs for blacks and, to a lesser extent, for all races, show evidence of an increase in Medicaid coverage at the cutoff. When we look

¹⁰ As recommended by the National Center for Health Statistics, we exclude over-sampled Hispanic respondents in the 1992 NHIS in this analysis; however, results are very similar when these respondents are included.

separately at children in households with incomes below the poverty line, we see additional visual evidence of a discontinuity in coverage.

Table 3 presents the corresponding regression estimates. For all races, we see some evidence of an increase in Medicaid coverage with point estimates ranging between 1 and 2 percentage points. However, for the most part, the estimates are not statistically significant. We do, however, see strong evidence of an increase in Medicaid coverage for blacks of between 5 and 8 percentage points depending on the specification. The estimates are significantly different from zero at the .05 level in four of five specifications and at the .10 level in the remaining case. Given our previous estimate that 17 percent of black children gained eligibility at the cutoff, these estimates represent a take-up rate on the order of approximately 29 to 47 percent. For non-blacks, we do not find a significant increase in Medicaid coverage and the point estimates are much smaller, indicating less than a one-percentage point change in coverage and implying a take-up rate of at most six percent. Examining children with families below the poverty line only, we find an increase in Medicaid coverage of between 6 and 9 percentage points. The estimates are significant at the .01 level in three of five specifications and at the .10 level in the remaining two cases. We find no change among children in families with incomes above the poverty line.

We also examined changes in reports of any insurance coverage during the last month (also in Table 3). The change in overall insurance coverage for black children is not statistically significant, but the coefficients suggest between a 2 and 5-percentage point increase among those born just after the cutoff. The fact that the change in Medicaid coverage is larger than the change in overall insurance coverage suggests that these expansions were associated with some crowd-out (i.e., that some children enrolled in Medicaid who would otherwise have enrolled in private insurance). In additional analyses, we examined changes in the probability of a doctor visit in the last 12 months, as well as any short-stay hospital visits (not related to delivery) (see Appendix Table 1). We find some evidence of increases in doctor visits among black children, although it is not consistently significant across bandwidth choice. All figures associated with this analysis may be found in the appendix (Appendix Figures 1-3).

Summary

Changes in Medicaid eligibility and coverage documented in Figures 1 through 3 and Tables 1 through 3 lead to important empirical implications. There is clear variation in treatment by race, poverty, and state, and the differences range from zero to substantial. Accordingly, if Medicaid coverage has long-term effects on health and use of medical care such as hospitalization, then it is plausible to expect that effects will vary in a way consistent with the variation in treatment.

IV. Data

To conduct our analysis, we combine discharge-level hospital data from three sources. First, we use hospitalization data from the Healthcare Cost and Utilization Project (HCUP) State Inpatient Databases. We purchased all state databases available from HCUP for the 1999 and 2009 years that contained information on the patient's date of birth. These data provide discharge-level information on all inpatient hospitalizations that occurred in acute care hospitals 1999 in Arizona, Hawaii, Iowa, New Jersey, New York, Oregon, and Wisconsin, and in 2009, on Arizona, Arkansas, Colorado, Hawaii, Iowa, Kentucky, Maryland, Michigan, Nebraska, New Jersey, New York, North Carolina, Oregon, South Dakota, Utah, Vermont, and Wisconsin.¹¹ We supplement these data with the census of hospital discharges that occurred in Texas and California in 1999 and 2009, obtained from the Texas Department of State Health Services and the California Health and Human Services Agency, resulting in the complete census of hospital discharges for 9 states in 1999 and for 19 states in 2009.¹² To our knowledge, this represents all of the available hospital discharge data containing information on the patient's date of birth for these years.

In addition to hospital discharge data, we use data from all State Emergency Discharge Databases available from HCUP in 2009 that include information on the patient's date of birth. These databases provide information on all outpatient emergency department visits that occurred

¹¹ The inpatient data from Nebraska and North Carolina in 2009 do not have information on patient race and are therefore excluded from all models run by race. Similarly, the inpatient data from Oregon in 1999 do not include information on race and is excluded from all models run by race for this year.

¹² Psychiatric hospitals are included in the discharge data from California, Kentucky, Michigan, Oregon, Texas, Utah, and Wisconsin. Other states include visits to psychiatric or other specialty units within general acute care hospitals but not visits to specialty hospitals themselves.

in Arizona, Hawaii, Iowa, Kentucky, New Jersey, New York, Utah and Wisconsin (obtained from HCUP) and California (obtained from the California Health and Human Services Agency) in 2009. These data cover all visits for which a patient was treated in an emergency department and released the same day, rather than being admitted to the hospital. The data are only more recently available and not available for 1999.

Both the hospital discharge and emergency department data contain information on the diagnoses associated with each visit, total charges, and patient demographics including race and birth month and year. In 2009 but not in 1999, we observe whether the patient is from a low-income zip code (defined as a zip code with median income below \$39,999).¹³ We classify primary diagnoses as relating to “chronic” or “non-chronic” conditions using the Chronic Condition Indicator software distributed by HCUP.¹⁴ We exclude hospitalizations and ED visits for diagnoses related to pregnancy and delivery.

Combined, our hospitalization data include 689,546 discharge-level observations for diagnoses not related to pregnancy and delivery and 3.9 million emergency department visits in 2009 for patients born between 1979 and 1987. Our hospitalization sample covers approximately 36 percent of the national population in 1999 and 50 percent of the national population in 2009, and our emergency department visit sample covers about 29 percent of the US population.¹⁵ These large sample sizes are critical for our analysis because they allow us to detect changes in hospitalizations and emergency department visits even among young populations with low utilization rates and for conditions that are relatively rare.

¹³ Data obtained from HCUP contain a variable indicating that the median income of the patient’s zip code is below \$39,999. For data from Texas and California, we use the American Community Survey and individual patient zip codes to create this variable following the same criteria.

¹⁴ Downloaded from <http://www.hcup-us.ahrq.gov/toolssoftware/chronic/chronic.jsp> on 11/11/2014. The HCUP Chronic Conditions Indicator categorizes all diagnosis codes as chronic or not chronic. The definition of a chronic condition requires that it lasts 12 months or longer and that it either (1) places limitations on self-care, independent living, and social interactions; or (2) needs ongoing intervention with medical products, services, and special equipment. The classification was developed based on an existing body of work on the chronicity of conditions and in consultation with a physician panel.

¹⁵ Calculated using state population estimates from the U.S. Census Bureau.

Table 4 presents descriptive statistics on hospital and emergency department utilization rates for our sample. The first three columns display hospitalization rates (per 10,000 individuals) for 15 year-olds in 1999, the first year for which we have data. In 1999, there were approximately 260 hospitalizations (not including hospital visits related to pregnancy and delivery) per 10,000 population for all races. Hospitalization rates at this age were higher for blacks, who experienced approximately 323 hospitalizations per 10,000 individuals, and lower for non-blacks, who experienced approximately 253 hospitalizations per 10,000 individuals. About 53 percent of these hospitalizations were for chronic illnesses overall; for blacks, chronic illnesses represented about 60 percent of total hospitalizations. For 15-year-olds, the most common of these chronic illnesses are mental disorders, followed by asthma and diabetes.

The next three columns display hospitalization rates for 25 year-olds in 2009. Hospitalization rates are more common for this age group: among all races, there were 326 hospital visits per 10,000 population; among blacks, there were 517 visits per 10,000 population; among non-blacks, there were 304 visits per 10,000 population. We observe a striking difference in hospital utilization rates across race groups, particularly for chronic illnesses: blacks at age 25 have a chronic illness hospitalization rate more than twice that of non-blacks. About 57 percent of hospitalizations of black patients were for chronic conditions and about 45 percent of hospitalizations of non-black patients were for chronic conditions. The most common chronic condition for this age group is also mental disorders. The second most common is diabetes and the third most common is asthma.

Emergency department visits are more common than hospitalizations and tend to treat less severe conditions. ED use is described in columns 7 through 9. On average, there are 3,152 emergency department visits per 10,000 individuals in 2009, almost ten times the hospitalization rate. Among blacks, this rate is 5,715 per 10,000 individuals; among non-blacks, it is 2,891 per 10,000 individuals. ED visits tend to be for acute conditions; only 12 percent of ED visits are for chronic illnesses, relative to 47 percent of hospitalizations for this age group. We again observe dramatic differences across races related to chronic illnesses, as blacks experience more than twice the rate of ED utilization for chronic illnesses as non-blacks.

These descriptive statistics highlight the importance of using a large sample to investigate utilization in these age groups. With a per capita hospitalization rate of under 0.026 in 1999, and

0.033 in 2009, it would be very difficult to detect changes in utilization rates among the relevant cohorts using, for example, survey data. By employing large administrative datasets, we are able to credibly investigate changes in hospitalizations and ED visits even though overall usage rates in these age groups are low.

V. Empirical Strategy

To estimate the impact of childhood Medicaid eligibility on later life hospitalization and ED visits, we use a regression discontinuity approach and compare outcomes for cohorts born just before and after the September 30, 1983 birthdate cutoff. We rely on both a parametric specification (e.g., polynomial) and local linear regression to estimate the discontinuity in outcomes at the birth date cutoff point. These complementary methods offer tradeoffs in terms of bias and variance and are presented together to assess the stability of results (Lee and Lemieux 2010). We use the log number of hospitalizations or ED visits as the dependent variable, which assumes that population trends smoothly across birth month cohorts.¹⁶ Estimates of the RD are interpreted as the proportionate change in the rate of hospitalizations or ED visits.

We first estimate a second-order polynomial regression model that uses observations from monthly cohorts born within a specified window of the cutoff date. Each cohort born between October 1979 and September 1987 is denoted using the integer values $c \in [-48, 47]$, where $c=0$ for the first cohort born after the cutoff (October 1983). We present results for our main specification that relies on a 4-year ($c \in [-48, 47]$) window of birth month cohorts on either side of the cutoff. Additionally, we show alternative analyses that use 3-year ($c \in [-36, 35]$) and 2-year ($c \in [-24, 23]$) windows of birth month cohorts.

The regression specification is given by

¹⁶ Wherry and Meyer (forthcoming) find evidence of a decrease in mortality at ages 15-18 resulting from the Medicaid expansions for black children born after the cutoff. Without adjusting for the corresponding change in the underlying population count at age 25, this biases us against detecting a decrease in later life hospitalizations or ED visits.

$$\log(y_c) = \alpha + \beta D_c + \gamma_0 c + \gamma_1 c^2 + \gamma_2 D_c \cdot c + \gamma_3 D_c \cdot c^2 + \delta_m M_c + \varepsilon_c \quad (1)$$

where y_c represents the number of hospitalizations or ED visits for a given birth cohort and D_c is an indicator for cohorts born after September 30, 1983 ($c \geq 0$). We include a quadratic function in birth month cohort c that is allowed to differ on both sides of the cutoff point by including an interaction term for those cohorts born after the cutoff. In addition, we include calendar month dummies M_c to control for variation in outcomes related to the link between timing of birth and family characteristics (Buckles and Hungerman 2012). The inclusion of these dummies will also net out the effects of policies that may differentially affect cohorts born in certain months (for example, school entry dates). For each outcome, we present visual evidence by plotting in two-month bins the residuals for birth month cohorts in the 4-year observation window from a regression on the set of calendar month dummies. The fitted line is the result of regressing the residuals on a quadratic function in birth month cohort interacted with the post-September 30, 1983 dummy.

We also use local linear regression to estimate the discontinuity in outcomes at the cutoff point. The estimation is conducted with a triangular kernel and we present results that employ two different optimal bandwidth selector procedures proposed by Imbens and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014).

These methods estimate the effect of the eligibility expansions averaged across the full sample of children at the cutoff. This is an example of a “fuzzy” RD design because factors other than date of birth determine eligibility for and take-up of public health insurance. Although we do not have information in our data on whether individuals were eligible for or took up public health insurance, we are able to examine outcomes for certain subsamples that were more likely to be affected by the change in Medicaid policy. In particular, we examine outcomes separately by race and income in accord with the variation in treatment documented previously.

We also investigate differences in outcomes by state of residence. As described earlier, states’ socioeconomic characteristics and eligibility criteria in place prior to the expansions led to variation in the size of the gain in Medicaid eligibility for children born after September 30,

1983. We exploit this variation and estimate changes in outcomes associated with the relative size of the discontinuity in childhood eligibility in each state.

We estimate the following specification for the 4-year window of observations around the cutoff:

$$\log(y_{cs}) = \alpha + \beta_0 D_c \cdot G_s + \beta_1 D_c + \gamma_{0s} c_s + \gamma_{1s} c_s^2 + \gamma_{2s} D_c \cdot c_s + \gamma_{3s} D_c \cdot c_s^2 + \delta_s + \delta_m M_c + \varepsilon_{cs} \quad (2)$$

where we regress the log of a given state-cohort outcome y_{cs} on an indicator for cohorts born after the cutoff D_c and its interaction with a measure of the size of the discontinuity in each state in eligibility-years G_s . In addition to including state and calendar month of birth fixed effects, we also include second order polynomial trends in birth month cohort that differ on either side of the discontinuity. In more flexible models, we allow these trends to vary by state. This regression is weighted using the state population of individuals in the range of ages used in these regressions, i.e., ages 22 to 30. These estimates are from the 2007 to 2011 5-year estimates of the American Community Survey. Due to the small number of states in our sample, we use the studentized wild bootstrap method with 999 bootstrap repetitions clustered by state for hypothesis testing and constructing confidence intervals. This method has been shown to perform well even when there are relatively few clusters (see Cameron, Gelbach and Miller 2008).

Some state by birth month cohort cells have zero hospitalizations for blacks; if this is the case for any birth month cohort in a state, we drop that entire state when conducting the analysis for the black and non-black subsamples. This leads us to drop two states in our analysis.

VI. Results

Figure 4 presents the profile of log hospitalizations by birth month cohort in 1999, when the cohorts born just on either side of the cutoff are approximately 15 years of age. As seen in the figure, hospitalizations are correlated with age (i.e. birth month), which is the running variable in the RD estimation. As noted earlier, we address the possibility of different trends on either side of the September 30, 1983 cutoff by allowing the polynomial in birth month cohort to have different coefficients on either side of the cutoff. Visually, the figure reveals little evidence of a discontinuity in outcomes at the September 30, 1983 threshold. Estimates of the discontinuity

from the regression analysis reported in Table 5 support this conclusion. Overall, we do not find a consistent pattern suggesting changes in hospital utilization in this year. We observe statistically significant decreases in non-pregnancy hospitalizations and non-chronic illness hospitalizations for all races, but only in the local linear regression models. We find some evidence of significant increases in non-pregnancy hospitalizations and chronic-illness hospitalizations for non-black patients, but the coefficient estimates are only significant in some of the specifications that use a polynomial in birth cohort. We also note that the statistical power of the analysis is modest due to both the smaller number of states available for this year and the low rates of utilization for this age group.

Figure 5 displays hospitalization outcomes in 2009 when cohorts born around the cutoff were approximately 25 years old. Table 6 presents the corresponding discontinuity estimates. Among all races, we find no evidence of a significant reduction in hospitalizations for those born after the birthdate cutoff. However, for blacks, there is a notable drop in hospitalizations visible at the cutoff. The regression analysis indicates a reduction in hospitalizations of between 7 and 15 percent for those cohorts born just after the September 30, 1983 date, depending on the specification. All estimates are statistically significant with the exception of the estimate under the global polynomial specification with the largest window of observations, which is significant at the 10 percent level. Furthermore, there is clear evidence of a significant decline in hospitalizations related to chronic illness. Our estimates indicate declines in hospitalizations for chronic illnesses on the order of 11 to 18 percent across specifications. For hospitalizations related to non-chronic illness, the estimated decline is smaller—3 to 11 percent—and only significant when using local linear regression methods. We do not find any evidence of a similar improvement for non-blacks. We find some evidence of an increase in hospitalizations for this group, but the estimates are not significant in most specifications and are not supported by the visual evidence presented in Figure 5.

Figure 6 and Table 7 present similar results for emergency department visits. We find a reduction in rates of ED visits of between 2 and 5 percent among black cohorts born immediately after the birth date cutoff, although the estimates are not significant across all bandwidth choices. When we examine ED visits by their relation to chronic illness, we find evidence of a sizeable decline

in visits related to chronic illness (of between 10 and 15 percent). For all races and non-blacks, we find no evidence of a similar reduction in ED visits.

Assuming similar effect sizes and hospitalization rates across other states, our point estimates imply that, nationally, there were approximately 2,200 to 4,900 fewer inpatient hospitalizations among black cohorts born during the first year after the cutoff at age 25.¹⁷ The change in the probability of gaining eligibility across the birth date cutoff was about 17 percentage points for blacks. If we assume that the reduction in hospitalizations observed in 2009 is entirely a result of the eligibility expansion, the point estimates from our specifications imply that there were between 2.1 and 4.6 fewer hospitalizations at age 25 for every 100 black children who were made eligible for (on average) 4.8 additional years of Medicaid eligibility as a result of the expansions. This reduction for eligible children is large relative to the average rate of hospitalization among all 25-year-old blacks, representing 41 to 88 percent fewer hospitalizations relative to that average. However, because the children that were affected by these expansions were in poor households, and because the poor tend to be in worse health than the general population, it is likely that their baseline hospitalization rates would be higher than that of a typical black 25 year old.¹⁸ Overall, while the point estimates are somewhat large, they are plausible. In addition, the 95% confidence intervals on our estimates allow for the possibility of smaller effect sizes.

We can further scale these estimates by take-up rates to arrive at the effect of Medicaid coverage, rather than Medicaid eligibility, on hospitalizations later in life. However, because parents tend to enroll children when they become ill or injured (Marton and Yelowitz 2014), this calculation would describe the local effect on (most likely) the sickest children with the highest rates of hospitalization. This may present an overly optimistic view of what Medicaid coverage expansions can accomplish for the average Medicaid-eligible child. Nonetheless, we perform such a calculation. Based on the estimates presented in Tables 2 and 3, we estimated take-up

¹⁷ Using the Census Estimate that in 2009 there were 617,000 blacks age 25, and that the average hospitalization rate at age 25 for blacks was 517.1 per 10,000 (Table 4).

¹⁸ Case, Lubotsky and Paxson (2002) find that children from low-income families have worse health in childhood, and that the differences between children raised in low- and high-income families become more pronounced as the children grow older and enter adulthood.

rates on the order of 29 to 47 percent.¹⁹ A take-up rate of 29 percent implies that for every 100 black children who enrolled in Medicaid there were between 7.3 and 15.7 fewer hospitalizations at age 25. Meanwhile, a take-up rate of 47 percent implies between 4.5 and 9.7 fewer hospitalizations for every 100 black children who enrolled in Medicaid.

Similarly, our point estimates imply that, nationally, there were approximately 7,100 and 17,600 fewer emergency department visits experienced by blacks born the first year after the cutoff at age 25. Again assuming this reduction is driven entirely by the eligibility expansion, this estimate implies that there were between 6.7 and 16.8 fewer emergency department visits at this age for every 100 black children made eligible as a result of the expansions. Comparing this to average ED use in the population of blacks suggests that gaining an average of 4.8 additional years of Medicaid eligibility in childhood lowers emergency department use by 12 to 29 percent at age 25. However, as we noted previously, baseline ED use among adults who grew up in low-income families is likely higher than average ED use in the population. When scaled by our estimate of 29 percent take-up, this implies that there were between 23.2 and 58.0 fewer ED visits at age 25 for every 100 black children newly enrolled in Medicaid. Using our higher take-up estimate (47 percent) implies between 14.3 and 35.8 fewer ED visits for every 100 new enrollees.

Low-income Zipcodes

Next we examine changes in hospitalizations and ED visits in 2009 for patients from low-income zipcodes (Tables 8-9 and Figures 7-8).²⁰ If children who grew up in poor families still reside in low-income zipcodes, we might expect to see larger changes for patients from these zipcodes. We find a reduction in total hospitalizations of between 10 and 23 percent among black cohorts in low-income zipcodes born just after September 30, 1983, and the coefficient estimate is statistically significant at the .05 level in four of our five specifications. This range of estimates indicates that the effects are larger in low-income zipcodes than in the full sample of zipcodes (7

¹⁹ These takeup rates as well as others in the literature should be interpreted cautiously since they are subject to substantial biases due to measurement error both in eligibility and reported coverage (Klerman et al. 2009). Note that measurement error in a binary dependent variable generally leads to bias (Hausman, Abrevaya and Scott-Morton 1998).

²⁰ The HCUP hospitalization data includes information on median income of the patient's zip code in 2009 only, so we are unable to conduct this analysis with the 1999 data.

to 15 percent). In addition, the decline appears to be concentrated among hospitalizations related to chronic illness, where we see reductions of between 15 and 28 percent that are statistically significant in all specifications. There is no significant reduction in hospitalizations for non-chronic illnesses.

Similarly, we find evidence of a larger decrease in ED visits for blacks in low-income zipcodes than for all blacks with estimates ranging between 4 and 6 percent, although the estimates are only significant in certain specifications. There is some evidence for reductions in chronic and non-chronic illness related ED visits as well, but the estimates are not consistently significant.

Finally, as in our previous analysis, we find no evidence of a significant reduction in hospitalizations or ED visits from persons from low-income zipcodes of all races or non-blacks.

In both the full sample and the low-income sample we estimate different effects across race groups, estimating large reductions for blacks, but not for non-blacks. There are two likely explanations for this heterogeneity. First, as described in Section III, the policy change had a larger impact for blacks, who were more than twice as likely to experience a gain in eligibility. We also find less evidence of take-up of Medicaid coverage among non-blacks as compared to blacks. Second, as noted in Section IV, blacks experience dramatically higher hospitalization rates on average than non-blacks. This difference is particularly pronounced for hospitalizations related to chronic illnesses: for these types of hospitalizations, the utilization rate is more than double among blacks than among non-blacks (as compared to acute conditions, for which the utilization rate is only 34 percent higher among blacks). As a result, it may be the case that the intervention itself was more effective for this race group because this group has a much higher baseline risk.

Heterogeneity by State

In this section, we use the substantial differences across states in the average size of the discontinuity in eligibility-years for cohorts born at the cutoff (see Table 2) as an additional source of variation in our analysis. If our observed changes in utilization across the threshold are indeed driven by differences in Medicaid eligibility in childhood, we might expect the discontinuity in utilization at the cutoff birth date to be larger in states where the change in eligibility across that threshold is greater.

To explore this heterogeneity, we estimate the model described by equation (2). The variable G_s is the estimated size of the discontinuity in eligibility that occurs at the cutoff in state s (as reported in Table 2). Our coefficient of interest is the interaction between D_c and the size of the discontinuity in the number of Medicaid-eligible years in state s , which captures the marginal effect of an additional year of eligibility on the outcome variable. We estimate two versions of this model: a “flexible” version that allows trends by birth month cohort to vary by state and a “restrictive” version that requires the time patterns to be the same in all states. The flexible version may be preferred because it allows birth month trends to be different across states; however, because our number of observations is small relative to the number of parameters we estimate, the less demanding restrictive version is also appealing. We therefore have elected to report both versions, but note that there is little qualitative difference in results.

Table 10 presents the results using hospitalizations in 2009. We find evidence suggesting that the decrease in hospitalizations is most pronounced for individuals living in states with large discontinuities.²¹ For patients of all races, we find that a one year increase in the size of the discontinuity at the birth date cutoff reduces hospitalizations by between 6 and 8 percent, although the effect is only significant at the 10 percent level in the flexible model. We also find some evidence of reductions in chronic and non-chronic illness hospitalizations, although in the flexible model the effects are not statistically significant.

The second panel presents results for black patients. Although we observe negative point estimates associated with increases in the discontinuity size across states, none of the effects are statistically significant, and in this case, we have limited statistical power to detect modest effect sizes. The third panel presents results for non-black patients. Here, we find that a one-year increase in the size of the discontinuity is associated with significant reductions in overall hospitalizations and hospitalizations related to both chronic and non-chronic illnesses. Our results suggest that a one year increase in eligibility is associated with a reduction in non-pregnancy hospitalizations of 8 or 10 percent, depending on the specification. We also find a

²¹ The results using hospitalizations in 1999, when birth cohorts born around the cutoff birth date are 15 years old, are reported in Appendix Table 3. As in our original specification, we do not find systematic evidence that those born immediately after the cutoff had fewer hospitalizations at this age.

reduction in chronic illness hospitalizations of 7 or 8 percent and a reduction in non-chronic illness hospitalizations of 10 and 12 percent.

In Table 11 we perform a similar analysis using emergency department data. In the top panel we report the results for all races. We find evidence of a decline in ED visits for cohorts born after the cutoff although the estimate is only statistically significant under the flexible model. The coefficient indicates that an additional year of Medicaid eligibility is associated with an 8 percent reduction in ED use at age 25. We also find significant declines for chronic and non-chronic illnesses under the flexible model, where an additional year of Medicaid eligibility is associated with 11 and 7 percent reductions respectively.

Conducting this analysis for blacks, we again find negative point estimates for all model specifications and visit types although none are statistically significant. Among non-blacks, we see that a one year increase in Medicaid eligibility is associated with an 8 percent reduction in overall ED visits, a 15 percent reduction in chronic illness ED visits, and a 7 percent reduction in non-chronic illness ED visits, although as with the results for all races, these effects are only statistically significant in the flexible model.

Because we uncover significant effects for non-blacks and not for blacks in the state-level models, at first glance these results might appear contradictory to those reported in the previous section. However, this is not necessarily the case. First, because our measures of the discontinuity at the state level are race-specific, they account for the fact that the effect of the policy was much smaller on non-blacks than on blacks. The results in the previous section looked at the overall change by race without scaling for the differences in the amount of exposure to treatment. Second, although the effects are not significant, we do observe negative point estimates for all models and visit types among the black sample, suggesting that the effects are indeed larger in states with a larger discontinuity. However, because many states in our sample have small black populations, it is the case that both the estimate of the size of the discontinuity in each state and the birth-month level utilization rates are imprecisely measured. We attempt to account for this by weighting the regressions by the state population; however, we are still forced to drop a large number of birth month observations for blacks due to zeros in the dependent variable.

VII. Sensitivity Analyses

We conduct several sensitivity analyses. First, we estimate the effect of discontinuous Medicaid eligibility on two types of hospitalizations that are unlikely to be affected by medical intervention in childhood: hospitalizations for appendicitis and injury. Second, we use birth month cohorts born between January 1965 and September 1983 to estimate placebo effects at cutoffs where the cohorts did not actually experience a discontinuity in Medicaid eligibility. Third, we examine the sensitivity of the estimates to the exclusion of California, which both comprises a large proportion of our sample and has the smallest discontinuity size of any state in our sample. Finally, we explore the sensitivity of our results to the inclusion of cohort-specific characteristics including measures of health at birth.

Hospitalizations for Acute Conditions

We first present estimates using hospitalizations for appendicitis and injuries for all patients and the low-income sample in 2009 by race group. Both appendicitis and injury are acute conditions that are likely not sensitive to medical care received in the past. For that reason, we believe it is unlikely that coverage in childhood could plausibly influence hospitalizations for these conditions. If we find effects on these types of hospitalizations, it may indicate that the assumptions of our RD model are incorrect.

The results of these analyses are reported in Appendix Table 2. The first panel shows the results for hospitalizations in 2009 for patients from all zip codes stratified by race. The second panel shows similar results for patients from low-income zip codes. In both panels, we find point estimates that are close to zero, none of which are statistically significant for any race group. Although the confidence intervals are large, we note that the point estimates are smaller in magnitude than those reported for all hospitalizations and chronic illness related hospitalizations and that the direction of the estimates is not consistent, with roughly half of the specifications reporting small, statistically insignificant positive effects and half reporting similarly sized negative effects. Overall, this suggests there was little impact of the policy on these types of visits. This result is consistent with our expectation that these types of visits should not be affected by access to medical care in childhood.

Placebo Estimates at Non-Discontinuity Points

We conduct a second type of placebo test using data on cohorts born prior to the actual eligibility cutoff. We place an artificial “cut off” date in the center of each four year window (eight year period) beginning with cohorts born in January 1965. We then estimate models that mimic our main “global” polynomial specification that uses a 4 year window and the “local” specification that chooses an optimal bandwidth using the Imbens and Kalyanaraman (2012) procedure. Our final placebo “cut off” is placed at September 1979, so that the last month used in the estimation of these placebo effects is September 1983, immediately before the actual change in Medicaid eligibility occurs. This results in 129 “placebo estimates” estimated at birth dates where no policy discontinuity existed. We perform this analysis using the sample of black patients and outcomes where we previously uncovered significant results using traditional inference.

Using these placebo estimates, we construct histograms, which we report in Figures 4 and 5. The effect estimated at the “true” cutoff is shown on the figure as a black vertical line. The two graphs in Appendix Figure 4 show the distribution of placebo statistics for the global polynomial and local linear specifications of the total number of hospitalizations in 2009 for black patients. We find that the true estimate among black patients is large relative to the placebo estimates. Using the global polynomial specification, the true effect exceeds all but 14 (10 percent) of the placebo estimates in absolute value. Using the local linear specification, the true effect exceeds all but 1 (less than 1 percent) of the placebo estimates.

The next two graphs in Appendix Figure 4 show the distribution of placebo estimates for chronic illness related hospitalizations. The effect for blacks estimated at the September 30, 1983 cutoff is larger in absolute value than all but 7 (5.4 percent) of the placebo estimates in the global model and all but 4 (3.1 percent) of the placebo estimates in the local model.

The next four panels of Appendix Figure 4 report the distribution of placebo estimates for hospitalizations of low-income black patients in 2009. Using data on all non-pregnancy hospitalizations, we find that the true effect is larger in magnitude than all but 5 (3.8 percent) of placebo estimates in the global model and it exceeds all placebo effects in the local model. When we conduct this analysis for chronic illness related hospitalizations, we find that the true effect exceeds all placebo estimates in both the local and the global model.

In Appendix Figure 5 we present similar distributions for placebo estimates using emergency department data. Here, our results conform less to the original inference conducted in Tables 7 and 9. We find that the true effect exceeds only about 64 and 45 percent of the placebo effects in magnitude for total hospitalizations in the global and local models, respectively. We find somewhat more promising results for chronic illness related ED visits: using the global model, we find that the true effect exceeds all but 14 percent of the placebo effects and, using the local model, we find that the true effect exceeds all but 6 percent of placebo effects. In the low income sample, we find that the true effect exceeds all but 22 percent of the placebo effects for all non-pregnancy hospitalizations in the global model and a little over half of the placebo effects in the local model.

Overall, the placebo simulations conducted in this section strongly suggest that the estimated effects of the Medicaid policy that we observe on hospitalizations among black cohorts is larger than estimates that we might observe due to chance. In addition, the simulations provide particularly convincing evidence supporting our results for the low-income subsample of blacks. The placebo tests are less convincing when applied to the emergency department results; although the effect of Medicaid on chronic illness emergency department visits estimated at the true discontinuity exceeds the majority of placebo estimates, many of placebo estimates are larger in absolute value than the “true” effect in other models.

Exclusion of California

Throughout our analysis, we include all available states. However, as illustrated in Table 2, some states have relatively small discontinuities in eligibility at the birth date cutoff. In particular, California has the smallest discontinuity in eligibility (about 0.05 years) and represents a large share of the total population in our state sample (about 25 percent). We therefore explore how sensitive our results are to excluding California from our analysis by re-estimating Tables 5 through 9 without California.

We present the results in Appendix Tables 4-8. As expected, when California is excluded, our results tend to be larger and are more likely to be statistically significant. We find that the reform reduces hospitalizations among black patients in 2009 by between 9.5 and 17.0 percent (compared to between 7.1 and 15.4 percent in our original analysis) and all effects are

statistically significant; it reduced chronic illness hospitalizations by between 13.2 and 20.1 percent (compared to 10.6 and 18.4 percent). In the low income sample, when California is excluded, we also detect statistically significant reductions in ED visits for all races. These results suggest that ED visits were between 3 and 5 percent lower among patients of all races for those born after the birth date cutoff.

Inclusion of Cohort-Specific Characteristics

Finally, we examine the sensitivity of our estimates to the inclusion of several cohort-specific characteristics drawn from the National Vital Statistics System Birth Data files for 1979 to 1987. We include controls for the following birth outcomes: the fraction of mothers with at least a high school education, fraction of mothers married, and fraction of mothers receiving any prenatal care; the incidence of low birth weight and very low birth weight births; and the number of births. Reported in Appendix Tables 9-11, the results are robust to the inclusion of these covariates.

VIII. Was the Upfront Cost of the Medicaid Expansions Offset by Lower Utilization Later in Life?

The results presented in this paper provide evidence that expanding Medicaid coverage to children lowers future health care costs by improving health and reducing later life hospital and emergency department use among those who gain eligibility. In this section, we provide “back of the envelope” calculations on the magnitude of these cost savings relative to the upfront cost of expanding Medicaid.

To conduct this analysis, we estimate models similar to those reported in Tables 6 and 7 but using log of total costs by birth month cohort as the dependent variable. We calculate total hospital costs by applying HCUP “cost to charge” ratios to the discharge-level data on total charges.²² These “cost to charge” ratios are designed to estimate the resource cost of a hospital visit using data from accounting reports collected by the Centers for Medicare and Medicaid

²² We do not have total charges for California or Texas, so we impute these values at the three digit diagnosis code level using the charge data from the relevant age group in other states.

Services. We then sum total costs at the birth month cohort level and estimate models as described in equation (2).

The results of this analysis are reported in Table 12 and Figure 9. We find that hospital costs among black cohorts fell by between 8 and 14 percent for those born immediately after the birth date cutoff. These reductions in costs are highly significant in the local linear models, but only marginally significant or not significant in the global polynomial specifications. Using the point estimates, and assuming that the results apply to all states, and using the total hospitalization costs of those born the year before the cutoff as a baseline, our analysis implies that the Medicaid expansions reduced total hospital costs at age 25 by between \$18.5 and \$32.4 million for black cohorts born the year following the cutoff.²³

The second panel of Table 11 reports the results for ED costs. We find that the Medicaid expansions reduced ED costs by 5 to 8 percent in 2009. Performing similar calculations as described above, we estimate that the expansions reduced emergency department costs at age 25 among black cohorts born during the following year of between \$6.7 and \$10.6 million.²⁴

To arrive at an estimate of the original cost of expanding Medicaid, we rely on the average spending per child enrolled in Medicaid in 1991, which was \$902 per child (in 1991 dollars) (Congressional Research Services (1993)).²⁵ Multiplying this amount by the average gain in years enrolled in Medicaid using information from Tables 1 and 3 and assuming a 3 percent discount rate, this implies that the total cost of the eligibility expansions for all children born during the year following the September 30, 1983 cutoff was approximately \$910 million dollars

²³ The total cost of 2009 hospitalizations for blacks born between October 1982 and September 1983 in our sample states was \$88 million. With approximately 38 percent of all 25-year-old blacks in the U.S. represented in our sample, we estimate total costs for these cohorts at the national level at \$231.6 million.

²⁴ Similar to the last calculation, we estimate total emergency department costs for the black cohorts born the year before the cutoff at the national level at \$133.3 million based on the total costs of \$28 million observed for these cohorts in our sample states. States in the emergency department sample represent about 21 percent of all 25-year-old blacks in the U.S.

²⁵ Because most (78 percent) of our variation in the discontinuity is a result of the Omnibus Reconciliation Act of 1990 that was implemented in 1991, we use 1991 as our base year in these calculations.

in 2009.²⁶ The cost offsets from childhood Medicaid expansions, totaling between \$25.2 and \$43 million at age 25, therefore represent between 3 and 5 percent of the total cost of the expansions. If the reduction in utilization we observe at age 25 persists for several years, the cost offsets associated with these expansions will be even larger.

Finally, we examine changes in total costs by the source of payment for visits in order to evaluate the incidence of these cost savings. These results are reported in Appendix Table 12. We find that total costs associated with publicly-insured hospital visits fell significantly for black patients, by about 14 percent, although the effects are not statistically significant for all bandwidths. We do not find significant changes in total costs associated with visits for other payers. Total hospital costs excluding pregnancies to public payers for 25 year-old black patients in 2009 were about \$125 million. Our results therefore imply that costs to public payers were about \$17.5 million lower for individuals born just after the cutoff. Cost savings accruing to the government therefore represent between 54 and 95 percent of the \$18.5 to \$32.4 million in estimated hospitalization-related cost savings and between 41 and 69 percent of the \$25.2 to \$43 million in estimated total cost savings.

Among emergency department visits, we find significant reductions of between 7 and 11 percent in costs associated with visits paid for by private insurance. Performing a similar calculation as above, this would imply that ED costs to private insurers were between \$3 and \$4.8 million lower for individuals born immediately after the cutoff birth date. Our results also indicate that there may have been meaningful reductions in costs for self-pay ED visits, although these effects are not statistically significant in all models. In addition, we find consistently negative, although not statistically significant, effects on costs to public payers.

These calculations indicate that the long-run cost savings from the Medicaid expansions may be quite substantial. Considering the other research on the long run effects of these expansions on other outcomes, the true cost offsets of the Medicaid expansions might be larger still.

Specifically, our estimates do not incorporate other benefits to government (such as higher

²⁶ The Census estimate for the total number of 25-year-olds in 2009 is 4,264,000. We multiply this estimate by the 0.48 year average gain in childhood eligibility, a take-up rate of 29 percent (which is the median of the takeup rates calculated based on the estimates in Tables 1 and 3), the \$902 cost per year of enrollment per child in 1991, and a 3% discount rate to arrive at our estimate.

income tax receipt and lower earned income tax payments, as found in Brown, Kowalski, and Lurie (2015), or to beneficiaries, such as better education outcomes (Cohodes et al. forthcoming) or lower mortality (Wherry and Meyer forthcoming; Brown, Kowalski, and Lurie 2015).

IX. Conclusion

Policies that expand public health insurance coverage tend to increase utilization and, thus, the total resources devoted to health care spending in the economy in the short term. However, there may be longer-term costs savings that do not materialize until later in life because of improved health. While these long-term cost savings are often cited in policy discussions and debates, very little credible evidence exists on the magnitude of these effects, or even if they are present at all. This is a crucial gap in our understanding of the role of public health insurance coverage as these cost offsets potentially represent a substantial, but previously unaccounted-for, benefit of such programs. There is also limited evidence on the technology of how investments in health in the pre-teen and early teen years affect health among adults. Understanding this process of health production is important in designing policies to ameliorate disadvantage.

In this paper, we provide evidence of such effects by exploiting a discontinuity in the number of years a child is eligible for Medicaid based on his or her date of birth. Because several of the early Medicaid coverage expansions to poor children applied only to children born after September 30, 1983, children born immediately after this cutoff received more years of Medicaid eligibility throughout childhood. Among blacks, who were most likely to be affected by these expansions, we find that those born immediately after the cutoff had a significant reduction in hospitalizations and emergency department visits at age 25 compared to those born immediately before the cutoff. The effect is particularly pronounced for chronic illness related hospitalizations and ED visits, among patients in low-income neighborhoods, and in states where the size of the eligibility discontinuity was large. A back of the envelope calculation based on our point estimates suggests that these reductions in utilization for the cohorts born one year after the birth date cutoff offset between 3 and 5 percent of the total cost of the expansions we study, and that a large fraction of this cost savings accrued to the government in the form of lower public insurance payments. Our evidence also suggests that health interventions in the pre-teen and early teen years for disadvantaged populations can provide long-term health benefits.

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Table 1. Childhood Medicaid Eligibility Gain for Children Born in October vs. September 1983 by Race Group

	Percent Gaining Eligibility	Average Gain (in Years) for Children Gaining Eligibility	Average Gain (in Years) for Total Child Population
All Races	10.00	4.54	0.48
Blacks	17.25	4.91	0.87
Non-Blacks	8.71	4.41	0.41

Table 2. Average Childhood Medicaid Eligibility Gain for Children Born in October vs. September 1983 by State

	All Races	Blacks	Non-Blacks
Arkansas	1.33	2.31	1.04
Arizona	0.81	1.23	0.79
California	0.05	0.09	0.05
Colorado	0.72	0.88	0.71
Hawaii	0.26	0.35	0.26
Iowa	0.40	0.60	0.39
Kentucky	0.60	0.78	0.59
Maryland	1.10	1.68	0.88
Michigan	0.21	0.30	0.19
Nebraska	0.55	1.16	0.52
New Jersey	0.26	0.56	0.21
New York	0.14	0.19	0.13
North Carolina	0.18	0.27	0.15
Oregon	0.38	0.13	0.39
South Dakota	1.16	2.67	1.15
Texas	1.08	1.29	1.04
Utah	0.16	0.03	0.16
Vermont	0.06	0.02	0.06
Wisconsin	0.23	0.63	0.20

Notes: Weighted averages calculated from simulation of lifetime eligibility if born in September vs. October 1983 for a children ages 0-17 in the pooled 1981-1988 March CPS. See appendix for additional discussion of these estimates and how they differ from Wherry and Meyer (forthcoming).

Table 3. Estimates of Effect of Childhood Medicaid Eligibility on Health Insurance Coverage at Ages 8-13

	All Races		Blacks		Non-Blacks		Households in Poverty		Households Not in Poverty	
	(1) Medicaid	(2) Any Insurance	(3) Medicaid	(4) Any Insurance	(5) Medicaid	(6) Any Insurance	(7) Medicaid	(8) Any Insurance	(9) Medicaid	(10) Any Insurance
<i>Global polynomial model</i>										
4-Year window	0.010 (-0.008, 0.027)	0.017 (-0.005, 0.039)	0.054** (0.002, 0.105)	0.032 (-0.023, 0.086)	-0.003 (-0.022, 0.016)	0.015 (-0.008, 0.037)	0.089*** (0.044, 0.133)	0.022 (-0.017, 0.061)	-0.001 (-0.013, 0.011)	0.009 (-0.012, 0.030)
3-Year window	0.017 (-0.004, 0.038)	0.021* (-0.004, 0.045)	0.071** (0.012, 0.129)	0.047 (-0.017, 0.111)	-0.001 (-0.024, 0.022)	0.017 (-0.007, 0.040)	0.091*** (0.038, 0.145)	0.046** (0.001, 0.092)	0.000 (-0.013, 0.013)	0.008 (-0.017, 0.033)
2-Year window	0.014 (-0.010, 0.038)	0.006 (-0.021, 0.032)	0.045* (-0.004, 0.095)	0.014 (-0.056, 0.084)	0.001 (-0.026, 0.029)	0.005 (-0.023, 0.033)	0.057* (-0.005, 0.118)	0.023 (-0.029, 0.074)	-0.000 (-0.015, 0.014)	-0.001 (-0.032, 0.030)
<i>Local linear regression</i>										
IK Bandwidth Selector	0.014* (-0.001, 0.028)	0.019** (0.000, 0.038)	0.051** (0.003, 0.099)	0.030 (-0.021, 0.081)	0.004 (-0.012, 0.019)	0.017* (-0.000, 0.034)	0.086*** (0.032, 0.141)	0.032 (-0.008, 0.072)	0.001 (-0.010, 0.013)	0.009 (-0.011, 0.029)
CCT Bandwidth Selector	0.023** (0.001, 0.045)	0.015 (-0.008, 0.039)	0.077** (0.009, 0.145)	0.032 (-0.036, 0.100)	0.006 (-0.014, 0.027)	0.014 (-0.010, 0.039)	0.073* (-0.004, 0.150)	0.035 (-0.031, 0.102)	0.003 (-0.011, 0.017)	0.004 (-0.021, 0.030)
<i>Baseline mean</i>	0.145	0.815	0.319	0.799	0.111	0.818	0.503	0.678	0.042	0.860
<i>N</i>	54,410	58,771	9,000	10,027	45,410	48,744	10,609	11,202	40,258	42,808

Notes: Data from 1992-1996 National Health Interview Survey Health Insurance Supplements. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. 95% confidence intervals reported in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Rates of Hospital and ED Utilization (per 10,000) for 15 and 25-Year-Olds

	Rate for 15-Year-Olds in 1999			Rates for 25-Year-Olds in 2009					
	Hospitalizations			Hospitalizations			Emergency Department Visits		
	All Races	Black	Non-Black	All Races	Black	Non-Black	All Races	Black	Non-Black
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total visits (excluding pregnancy)	260.44	323.14	253.35	326.44	517.14	303.71	3,152.10	5,714.95	2,890.76
By Relation to Chronic Illness									
visits related to chronic illness	137.90	193.63	131.50	153.66	293.15	137.03	378.23	795.63	335.67
visits not related to chronic illness	122.54	129.52	121.85	172.78	223.99	166.67	2,773.87	4,919.33	2,555.09

Notes: Data for inpatient hospitalizations from states: AZ, CA, HI, IA, NY, OR, TX, and WI (1999 and 2009), as well as AR, CO, KY, MD, MI, NJ, SD, UT, and VT (2009 only). Data for emergency department visits from states: AZ, CA, HI, IA, KY, NJ, NY, UT, and WI. Rates were calculated using age-specific population estimates by race for these states from the 2009 American Community Survey and the 2000 Census 1% sample downloaded from IPUMS. Hospitalizations and ED visits exclude those related to pregnancy and delivery.

*** p<0.01, ** p<0.05, * p<0.1

Table 5. Estimates of Effect of Childhood Medicaid Eligibility on Log Hospitalizations at Age 15 (1999)

	All Races			Blacks			Non-Blacks		
	(1) All	(2) Chronic	(3) Non-Chronic	(4) All	(5) Chronic	(6) Non-Chronic	(7) All	(8) Chronic	(9) Non-Chronic
<i>Global polynomial model</i>									
4-Year window ($N = 96$)	0.001 (-0.037, 0.040)	-0.026 (-0.089, 0.037)	0.020 (-0.023, 0.063)	-0.025 (-0.142, 0.092)	-0.071 (-0.246, 0.105)	0.039 (-0.077, 0.156)	0.042** (0.009, 0.074)	0.042 (-0.013, 0.096)	0.041* (-0.002, 0.084)
3-Year window ($N = 72$)	-0.001 (-0.041, 0.039)	-0.001 (-0.071, 0.069)	-0.003 (-0.050, 0.044)	0.020 (-0.125, 0.166)	0.028 (-0.174, 0.230)	0.022 (-0.127, 0.170)	0.026 (-0.011, 0.064)	0.028 (-0.039, 0.095)	0.025 (-0.025, 0.074)
2-Year window ($N = 48$)	0.017 (-0.032, 0.065)	0.033 (-0.043, 0.110)	-0.001 (-0.068, 0.066)	0.034 (-0.150, 0.217)	-0.031 (-0.319, 0.257)	0.135 (-0.043, 0.314)	0.060*** (0.020, 0.100)	0.087** (0.017, 0.157)	0.038 (-0.030, 0.107)
<i>Local linear regression</i>									
IK Bandwidth Selector	-0.028** (-0.054, -0.002)	-0.021 (-0.069, 0.027)	-0.041** (-0.076, -0.005)	-0.013 (-0.121, 0.094)	-0.005 (-0.165, 0.155)	0.014 (-0.104, 0.133)	0.018 (-0.012, 0.049)	0.027 (-0.016, 0.069)	0.004 (-0.040, 0.048)
CCT Bandwidth Selector	-0.055** (-0.103, -0.008)	-0.029 (-0.090, 0.033)	-0.067*** (-0.112, -0.022)	-0.021 (-0.163, 0.122)	-0.027 (-0.281, 0.227)	0.009 (-0.114, 0.132)	0.032 (-0.014, 0.078)	0.020 (-0.032, 0.072)	0.038 (-0.034, 0.110)

Notes: Sample includes birth-month observations from pooled AZ, CA, CO, HI, IA, MD, MI, NJ, NY, TX, VT, and WI data. Models with all races also include OR. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. 95% confidence intervals reported in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6. Estimates of Effect of Childhood Medicaid Eligibility on Log Hospitalizations at Age 25 (2009)

	All Races			Blacks			Non-Blacks		
	(1) All	(2) Chronic	(3) Non-Chronic	(4) All	(5) Chronic	(6) Non-Chronic	(7) All	(8) Chronic	(9) Non-Chronic
<i>Global polynomial model</i>									
4-Year window ($N=96$)	0.002 (-0.025, 0.029)	0.010 (-0.029, 0.050)	-0.006 (-0.036, 0.024)	-0.071* (-0.144, 0.003)	-0.106** (-0.198, -0.014)	-0.025 (-0.108, 0.058)	0.016 (-0.012, 0.045)	0.038* (-0.004, 0.080)	-0.001 (-0.037, 0.035)
3-Year window ($N=72$)	0.003 (-0.027, 0.033)	0.014 (-0.032, 0.061)	-0.007 (-0.038, 0.023)	-0.095** (-0.176, -0.013)	-0.120** (-0.223, -0.017)	-0.061 (-0.158, 0.036)	0.022 (-0.011, 0.054)	0.046* (-0.005, 0.097)	0.001 (-0.036, 0.038)
2-Year window ($N=48$)	0.023 (-0.014, 0.061)	0.037 (-0.025, 0.100)	0.010 (-0.029, 0.049)	-0.144*** (-0.247, -0.040)	-0.167** (-0.301, -0.034)	-0.113* (-0.232, 0.006)	0.054*** (0.020, 0.089)	0.081*** (0.031, 0.132)	0.031 (-0.013, 0.075)
<i>Local linear regression</i>									
IK Bandwidth Selector	-0.008 (-0.032, 0.016)	-0.004 (-0.040, 0.032)	-0.014 (-0.035, 0.007)	-0.126*** (-0.183, -0.068)	-0.145*** (-0.216, -0.073)	-0.081** (-0.151, -0.012)	0.016 (-0.012, 0.043)	0.031 (-0.011, 0.072)	0.002 (-0.027, 0.031)
CCT Bandwidth Selector	-0.016 (-0.048, 0.016)	-0.003 (-0.047, 0.041)	-0.007 (-0.043, 0.029)	-0.154*** (-0.228, -0.081)	-0.184*** (-0.278, -0.091)	-0.114*** (-0.179, -0.049)	0.013 (-0.026, 0.051)	0.032 (-0.013, 0.077)	0.037 (-0.017, 0.091)

Notes: Sample includes birth-month observations from pooled AR, AZ, CO, CA, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, VT and WI hospital data. Models with all races also include NC and NE. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. 95% confidence intervals reported in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7. Estimates of Effect of Childhood Medicaid Eligibility on Log Emergency Department Visits at Age 25 (2009)

	All Races			Blacks			Non-Blacks		
	(1) All	(2) Chronic	(3) Non-Chronic	(4) All	(5) Chronic	(6) Non-Chronic	(7) All	(8) Chronic	(9) Non-Chronic
<i>Global polynomial model</i>									
4-Year window ($N=96$)	-0.008 (-0.029, 0.012)	-0.018 (-0.049, 0.013)	-0.007 (-0.029, 0.015)	-0.037** (-0.071, -0.003)	-0.101* (-0.210, 0.007)	-0.027* (-0.057, 0.004)	-0.003 (-0.024, 0.019)	0.002 (-0.026, 0.029)	-0.003 (-0.026, 0.020)
3-Year window ($N=72$)	-0.009 (-0.032, 0.014)	-0.025 (-0.063, 0.013)	-0.006 (-0.030, 0.018)	-0.049** (-0.088, -0.010)	-0.153** (-0.279, -0.027)	-0.031* (-0.065, 0.004)	-0.001 (-0.024, 0.022)	0.005 (-0.029, 0.039)	-0.002 (-0.026, 0.023)
2-Year window ($N=48$)	0.015 (-0.010, 0.041)	0.011 (-0.030, 0.051)	0.016 (-0.011, 0.043)	-0.028 (-0.076, 0.020)	-0.148* (-0.306, 0.010)	-0.007 (-0.051, 0.037)	0.024* (-0.003, 0.050)	0.049** (0.009, 0.088)	0.020 (-0.009, 0.049)
<i>Local linear regression</i>									
IK Bandwidth Selector	0.010 (-0.011, 0.032)	-0.014 (-0.039, 0.010)	0.014 (-0.010, 0.037)	-0.022* (-0.045, 0.002)	-0.123*** (-0.210, -0.036)	-0.009 (-0.031, 0.012)	0.019 (-0.006, 0.044)	0.014 (-0.008, 0.037)	0.018 (-0.008, 0.045)
CCT Bandwidth Selector	0.017 (-0.011, 0.045)	-0.014 (-0.040, 0.011)	0.025 (-0.007, 0.056)	-0.022* (-0.045, 0.002)	-0.142*** (-0.237, -0.048)	-0.001 (-0.024, 0.022)	0.025* (-0.004, 0.054)	0.012 (-0.011, 0.035)	0.027 (-0.007, 0.060)

Notes: Sample includes birth-month observations from pooled AZ, CA, HI, IA, KY, NJ, NY, UT, and WI ED data. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. 95% confidence intervals reported in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8. Estimates of the Effect of Childhood Medicaid Eligibility on Log Hospitalizations in Low-Income Zipcodes at Age 25 (2009)

	All Races			Blacks			Non-Blacks		
	(1) All	(2) Chronic	(3) Non-Chronic	(4) All	(5) Chronic	(6) Non-Chronic	(7) All	(8) Chronic	(9) Non-Chronic
<i>Global polynomial model</i>									
4-Year window ($N=96$)	0.012 (-0.033, 0.058)	0.007 (-0.056, 0.070)	0.017 (-0.030, 0.065)	-0.100 (-0.225, 0.024)	-0.148** (-0.293, -0.003)	-0.032 (-0.181, 0.118)	0.052* (-0.001, 0.106)	0.070* (-0.011, 0.151)	0.038 (-0.021, 0.096)
3-Year window ($N=72$)	-0.004 (-0.058, 0.050)	-0.011 (-0.086, 0.065)	0.002 (-0.052, 0.056)	-0.150** (-0.294, -0.005)	-0.188** (-0.353, -0.023)	-0.092 (-0.266, 0.082)	0.046 (-0.023, 0.114)	0.062 (-0.038, 0.162)	0.032 (-0.039, 0.103)
2-Year window ($N=48$)	0.004 (-0.060, 0.068)	-0.019 (-0.101, 0.064)	0.025 (-0.043, 0.092)	-0.227** (-0.412, -0.043)	-0.280*** (-0.467, -0.094)	-0.150 (-0.376, 0.075)	0.078* (-0.005, 0.161)	0.080* (-0.015, 0.174)	0.075 (-0.020, 0.171)
<i>Local linear regression</i>									
IK Bandwidth Selector	-0.007 (-0.045, 0.031)	-0.025 (-0.080, 0.030)	0.001 (-0.039, 0.040)	-0.169** (-0.306, -0.032)	-0.235*** (-0.386, -0.084)	-0.081 (-0.235, 0.073)	0.036 (-0.017, 0.089)	0.037 (-0.034, 0.108)	0.035 (-0.019, 0.090)
CCT Bandwidth Selector	-0.012 (-0.058, 0.034)	-0.047 (-0.113, 0.020)	0.019 (-0.036, 0.074)	-0.170** (-0.308, -0.031)	-0.257*** (-0.433, -0.081)	-0.080 (-0.245, 0.085)	0.041 (-0.023, 0.106)	0.038 (-0.049, 0.125)	0.068* (-0.010, 0.146)

Notes: Sample includes birth-month observations from pooled AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, VT, and WI hospital data. Models with all races also include NC and NE. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. 95% confidence intervals reported in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9. Estimates of Effect of Childhood Medicaid Eligibility on Log Emergency Department Visits in Low-Income Zipcodes at Age 25 (2009)

	All Races			Blacks			Non-Blacks		
	(1) All	(2) Chronic	(3) Non-Chronic	(4) All	(5) Chronic	(6) Non-Chronic	(7) All	(8) Chronic	(9) Non-Chronic
<i>Global polynomial model</i>									
4-Year window ($N=96$)	-0.026 (-0.075, -0.002)	-0.045 (-0.095, 0.025)	-0.024 (-0.064, 0.013)	-0.057** (-0.106, -0.008)	-0.129* (-0.283, 0.025)	-0.045** (-0.086, -0.004)	-0.017 (-0.056, 0.022)	-0.012 (-0.068, 0.044)	-0.017 (-0.059, 0.024)
3-Year window ($N=72$)	-0.040* (-0.081, 0.000)	-0.062* (-0.130, 0.006)	-0.037* (-0.079, 0.004)	-0.064** (-0.123, -0.005)	-0.134 (-0.308, 0.039)	-0.052** (-0.102, -0.003)	-0.033 (-0.080, 0.014)	-0.033 (-0.100, 0.034)	-0.032 (-0.081, 0.016)
2-Year window ($N=48$)	-0.013 (-0.058, 0.032)	-0.039 (-0.129, 0.051)	-0.009 (-0.053, 0.035)	-0.036 (-0.112, 0.041)	-0.169 (-0.380, 0.042)	-0.012 (-0.076, 0.052)	-0.005 (-0.054, 0.044)	0.012 (-0.063, 0.087)	-0.007 (-0.059, 0.045)
<i>Local linear regression</i>									
IK Bandwidth Selector	-0.019 (-0.045, 0.007)	-0.044* (-0.093, 0.006)	-0.018 (-0.046, 0.011)	-0.035* (-0.073, 0.002)	-0.114 (-0.258, 0.031)	-0.026* (-0.053, 0.002)	-0.016 (-0.049, 0.017)	-0.018 (-0.061, 0.026)	-0.015 (-0.050, 0.019)
CCT Bandwidth Selector	-0.013 (-0.054, 0.027)	-0.052* (-0.108, 0.005)	-0.007 (-0.050, 0.037)	-0.034 (-0.076, 0.008)	-0.136* (-0.290, 0.018)	-0.010 (-0.040, 0.020)	-0.008 (-0.056, 0.041)	-0.018 (-0.071, 0.035)	-0.006 (-0.058, 0.045)

Notes: Sample includes birth-month observations from pooled AZ, CA, HI, IA, KY, NJ, NY, UT, and WI ED data. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. 95% confidence intervals reported in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Table 10. Estimates of Effect of State Childhood Medicaid Eligibility Gain on Hospitalizations At Age 25
By Race**

	Post x Size of Discontinuity Restricted Model	Post x Size of Discontinuity Flexible Model	N
All Races			
Log Total Hospitalizations (excluding pregnancy)	-0.080*** (-0.112, -0.050)	-0.064* (-0.139, 0.007)	1824
By Relation to Chronic Illness			
log hospitalizations related to chronic illness	-0.064* (-0.138, 0.005)	-0.069 (-0.198, 0.042)	1824
log hospitalizations related to non-chronic illness	-0.099*** (-0.145, -0.053)	-0.067 (-0.149, 0.016)	1824
Blacks			
Log Total Hospitalizations (excluding pregnancy)	-0.047 (-0.161, 0.066)	-0.017 (-0.389, 0.357)	960
By Relation to Chronic Illness			
log hospitalizations related to chronic illness	-0.036 (-0.179, 0.108)	-0.023 (-0.608, 0.554)	959
log hospitalizations related to non-chronic illness	-0.077 (-0.201, 0.044)	-0.010 (-0.173, 0.154)	960
Non Blacks			
Log Total Hospitalizations (excluding pregnancy)	-0.098*** (-0.114, -0.082)	-0.081*** (-0.108, -0.053)	960
By Relation to Chronic Illness			
log hospitalizations related to chronic illness	-0.084*** (-0.153, -0.015)	-0.069*** (-0.120, -0.018)	960
log hospitalizations related to non-chronic illness	-0.116*** (-0.134, -0.097)	-0.102*** (-0.140, -0.064)	960

Notes: 2009 hospitalization data are from AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, VT, and WI. Models using all races also include data from NC and NE. In addition to the indicator for cohorts born after the cutoff and its interaction with the size of the discontinuity and state fixed effects, the flexible regression specification also includes state-specific quadratic functions in birth month cohort that are interacted with the indicator for cohorts born after the cutoff. Clustered wild bootstrap 95% confidence intervals are reported in brackets and were used for hypothesis

*** p<0.01, ** p<0.05, * p<0.1

Table 11. Effect of State Childhood Medicaid Eligibility Gain on ED Visits At Age 25 By Race

	Post x Size of Discontinuity Restricted Model	Post x Size of Discontinuity Flexible Model	N
All Races			
Log Total ED Visits in 2009	-0.038 (-0.129, 0.052)	-0.077*** (-0.123, -0.030)	864
By Relation to Chronic Illness			
log ED visits related to chronic illness	-0.020 (-0.157, 0.117)	-0.113*** (-0.196, -0.029)	864
log ED visits related to non-chronic illness	-0.043 (-0.129, 0.043)	-0.073*** (-0.112, -0.034)	864
Blacks			
Log Total ED Visits in 2009	-0.042 (-0.115, 0.058)	-0.037 (-0.169, 0.071)	862
By Relation to Chronic Illness			
log ED visits related to chronic illness	-0.025 (-0.159, 0.101)	-0.030 (-0.497, 0.428)	787
log ED visits related to non-chronic illness	-0.049 (-0.129, 0.032)	-0.042 (-0.172, 0.089)	862
Non Blacks			
Log Total ED Visits in 2009	-0.027 (-0.117, 0.063)	-0.079*** (-0.109, -0.048)	864
By Relation to Chronic Illness			
log ED visits related to chronic illness	-0.013 (-0.182, 0.153)	-0.143** (-0.296, -0.000)	864
log ED visits related to non-chronic illness	-0.031 (-0.113, 0.052)	-0.071*** (-0.083, -0.060)	864

Notes: 2009 ED data are from AZ, CA, HI, IA, KY, NJ, NY, UT, VT and WI. In addition to the indicator for cohorts born after the cutoff and its interaction with the size of the discontinuity and state fixed effects, the flexible regression specification also includes state-specific quadratic functions in birth month cohort that are interacted with the indicator for cohorts born after the cutoff. Clustered wild bootstrap 95% confidence intervals are reported in brackets and were

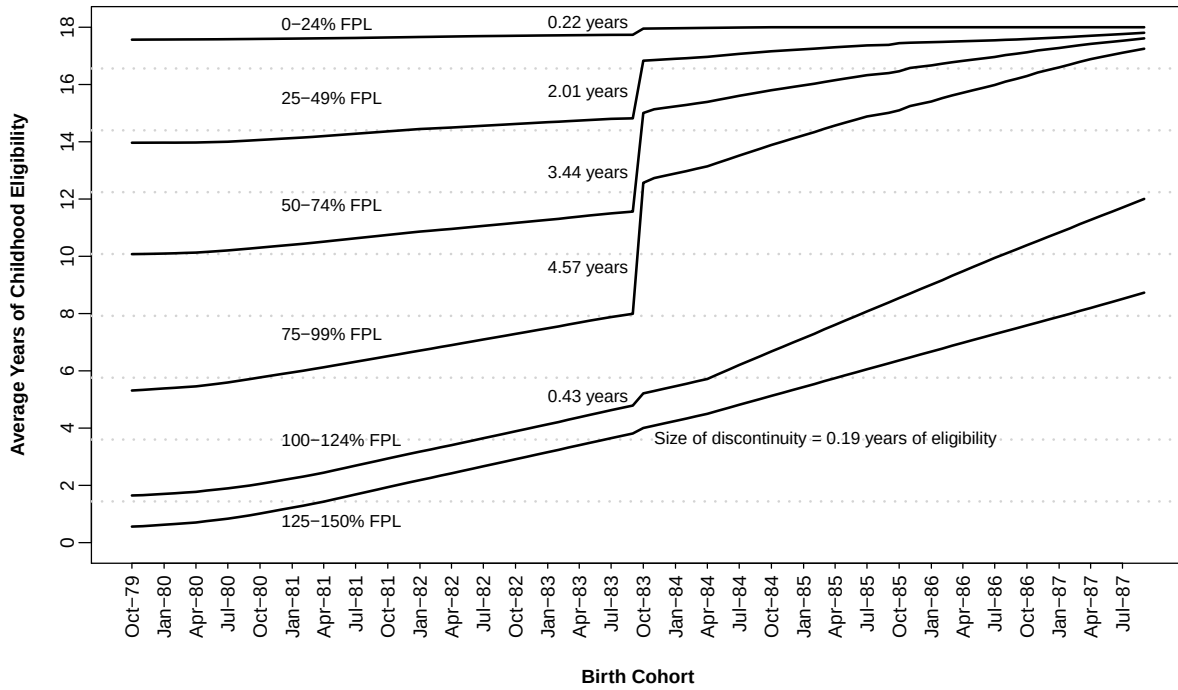
*** p<0.01, ** p<0.05, * p<0.1

Table 12. Effect of Medicaid on 2009 Logged Total Hospital and ED Costs By Payer for Each Birth Cohort, By Race

	All Races		Blacks		Non-Blacks	
	(1)	(2)	(3)	(4)	(5)	(6)
	Hospital Costs	ED Costs	Hospital Costs	ED Costs	Hospital Costs	ED Costs
<i>Global polynomial model</i>						
4-Year window ($N=96$)	-0.005 (-0.033, 0.023)	-0.007 (-0.028, 0.014)	-0.078* (-0.164, 0.007)	-0.045* (-0.093, 0.004)	0.007 (-0.029, 0.043)	-0.001 (-0.022, 0.021)
3-Year window ($N=72$)	0.003 (-0.031, 0.037)	-0.007 (-0.032, 0.017)	-0.077 (-0.180, 0.027)	-0.079*** (-0.133, -0.024)	0.019 (-0.021, 0.058)	0.004 (-0.020, 0.028)
2-Year window ($N=48$)	0.020 (-0.021, 0.061)	0.018 (-0.011, 0.047)	-0.130* (-0.270, 0.011)	-0.073** (-0.139, -0.007)	0.054** (0.008, 0.101)	0.032** (0.002, 0.062)
<i>Local linear regression</i>						
IK Bandwidth Selector	-0.009 (-0.030, 0.013)	0.006 (-0.012, 0.023)	-0.099*** (-0.161, -0.036)	-0.051** (-0.093, -0.009)	0.017 (-0.015, 0.048)	0.021* (-0.001, 0.042)
CCT Bandwidth Selector	-0.013 (-0.048, 0.023)	0.010 (-0.013, 0.032)	-0.137*** (-0.200, -0.073)	-0.062*** (-0.104, -0.020)	0.019 (-0.020, 0.058)	0.025* (-0.001, 0.051)

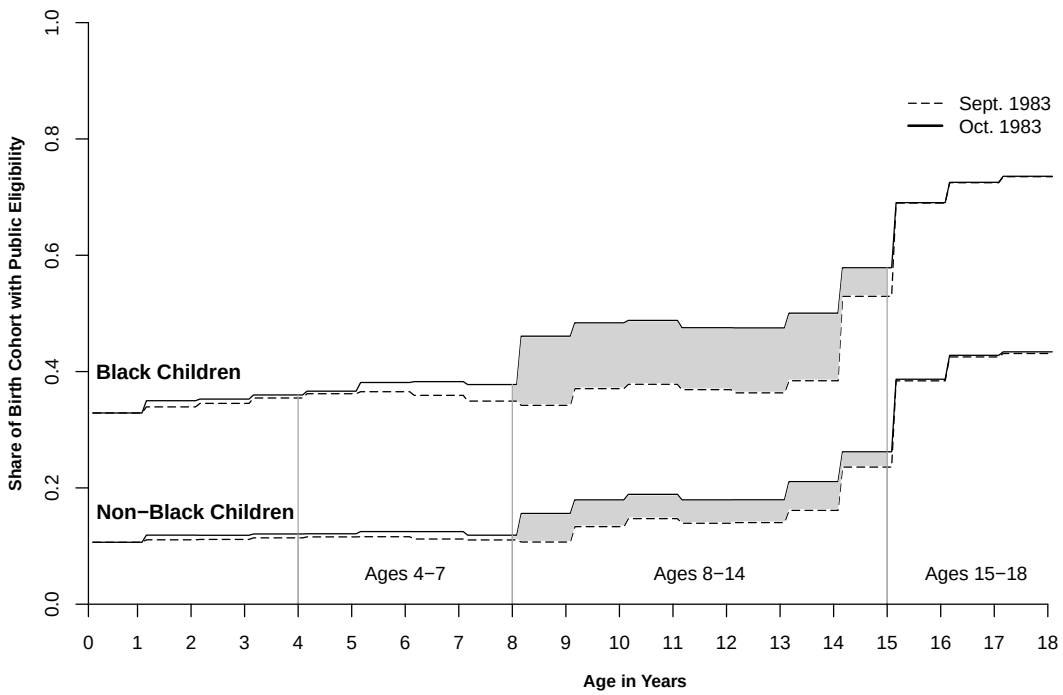
Notes: Sample includes AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, VT and WI. Results for all races also include NC and NE. All global regression models include birth month fixed effects and a quadratic function in birth month cohort interacted with an indicator that the birth month cohort is October 1983 or later. 95% confidence intervals reported in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Figure 1. Average Years of Childhood Eligibility for Medicaid/SCHIP by Birth Cohort and Family Income (%FPL)



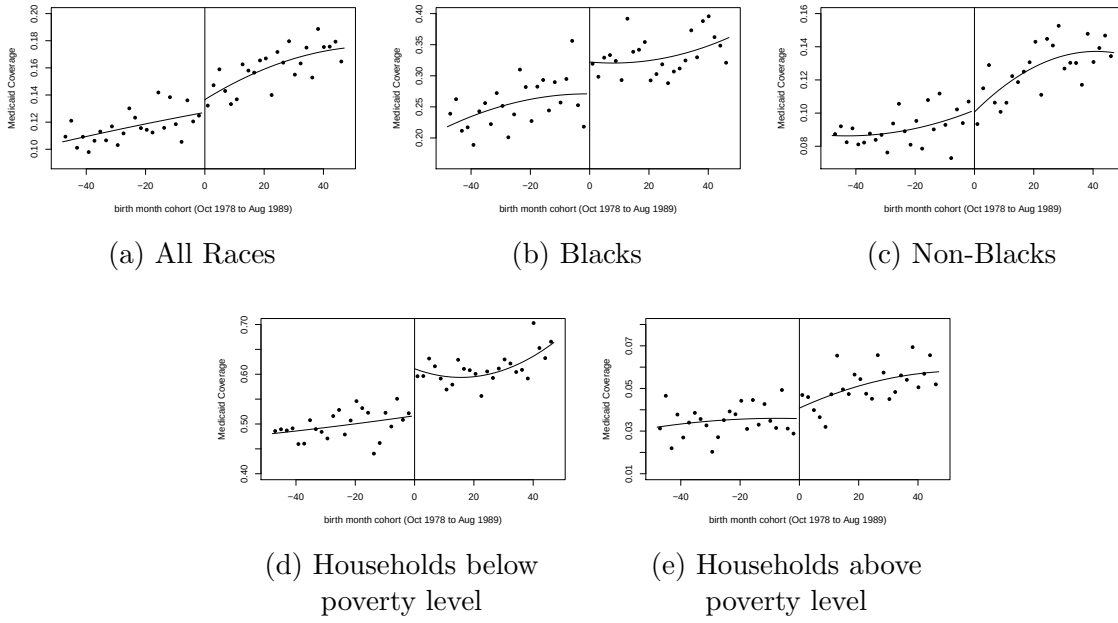
Source: Wherry and Meyer (2014).

Figure 2. Average Public Eligibility at Each Age of Childhood by Birth Month Cohort and Child Race



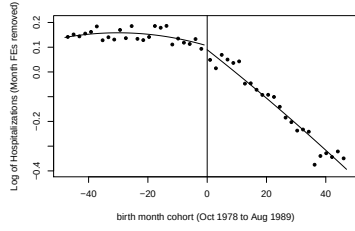
Notes: Weighted average calculated using the characteristics and state of residence of a sample of black or non-black children of ages 0-17 in the 1981-1988 March CPS. See Wherry and Meyer (2014) for additional information.

Figure 3: Medicaid Coverage in Childhood, Ages 8 to 13, NHIS

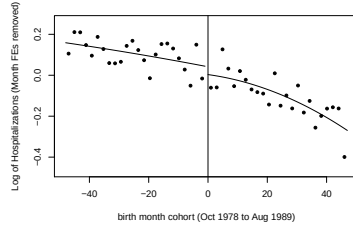


Source: Authors' calculations from the National Health Interview Survey, 1992-1996. Cohorts born in 1983 are between the ages of 8 and 13 in these figures. The trend is estimated using children between the ages of 4 and 17.

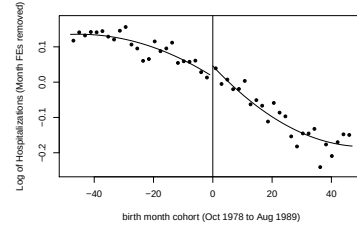
Figure 4: 1999 Hospitalizations, Calendar Month of Birth Fixed Effects Removed



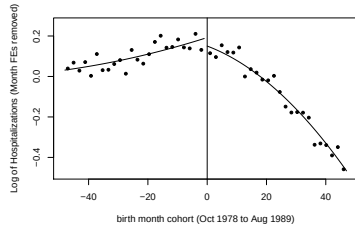
(a) All Hospitalizations, All Races



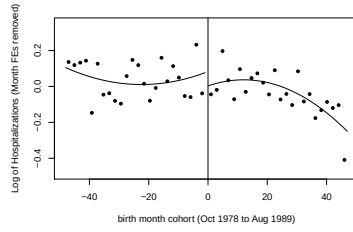
(b) All Hospitalizations, Blacks



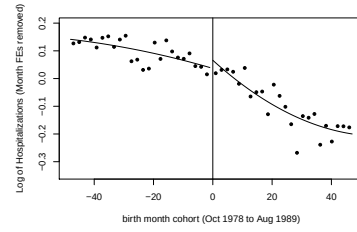
(c) All Hospitalizations, Non-Blacks



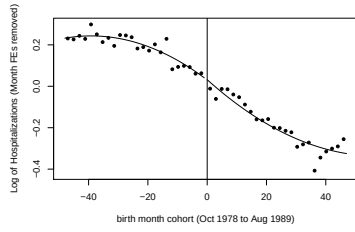
(d) Chronic Illness Hospitalizations, All Races



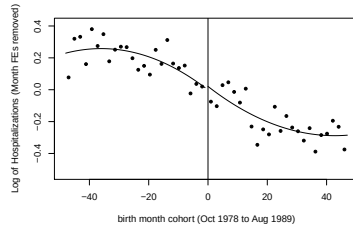
(e) Chronic Illness Hospitalizations, Blacks



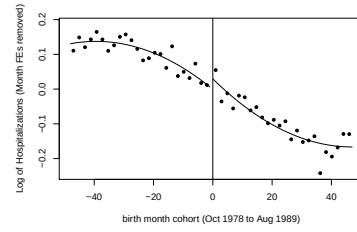
(f) Chronic Illness Hospitalizations, Non-Blacks



(g) Non-chronic Illness Hospitalizations, All Races



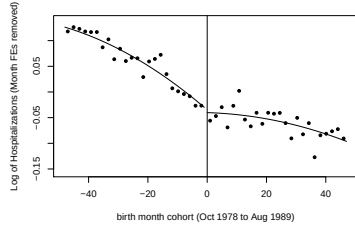
(h) Non-chronic Illness Hospitalizations, Blacks



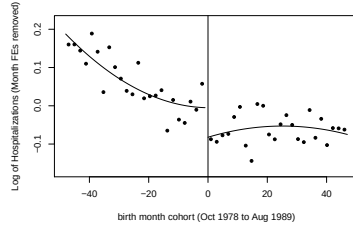
(i) Non-chronic Illness Hospitalizations, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all hospitalizations that occurred in 1999 in AZ, CA, IA, NY, OR, TX, HI, UT, and WI.

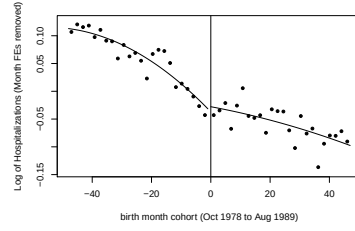
Figure 5: 2009 Hospitalizations, Calendar Month of Birth Fixed Effects Removed



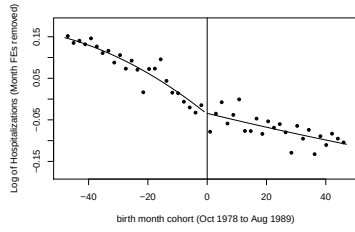
(a) All Hospitalizations, All Races



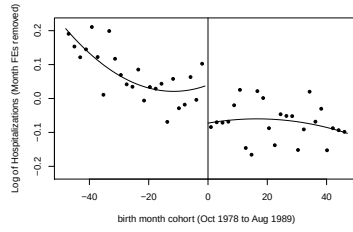
(b) All Hospitalizations, Blacks



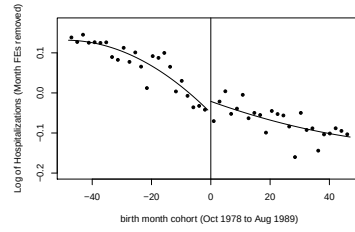
(c) All Hospitalizations, Non-Blacks



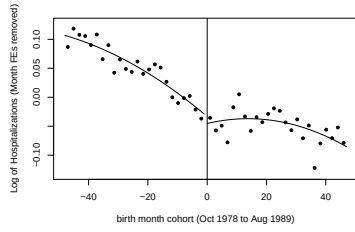
(d) Chronic Illness Hospitalizations, All Races



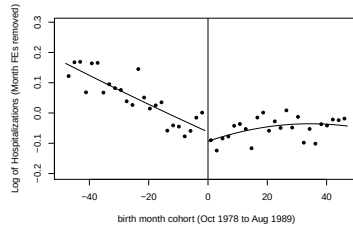
(e) Chronic Illness Hospitalizations, Blacks



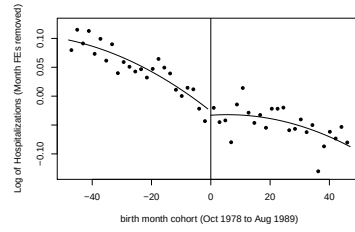
(f) Chronic Illness Hospitalizations, Non-Blacks



(g) Non-chronic Illness Hospitalizations, All Races



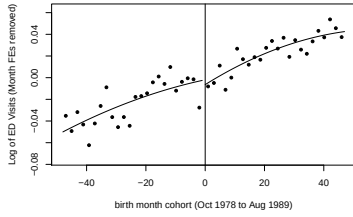
(h) Non-chronic Illness Hospitalizations, Blacks



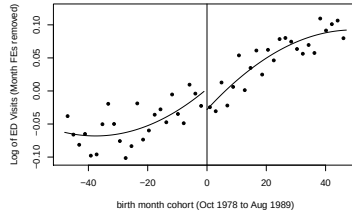
(i) Non-chronic Illness Hospitalizations, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all hospitalizations that occurred in 2009 in AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, and WI. Figures for all races also include NE and NC.

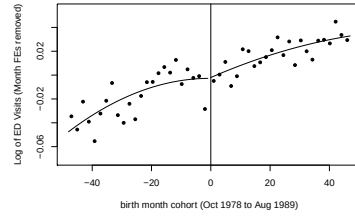
Figure 6: 2009 Emergency Department Visits, Calendar Month of Birth Fixed Effects Removed



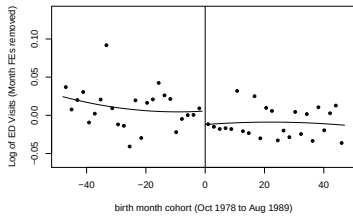
(a) All ED Visits, All Races



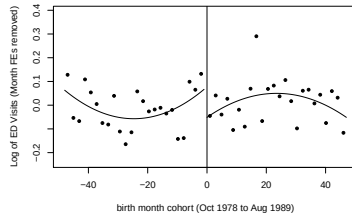
(b) All ED Visits, Blacks



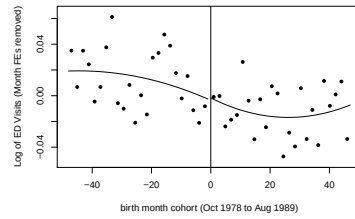
(c) All ED Visits,
Non-Blacks



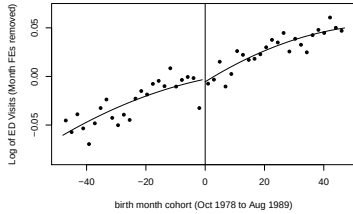
(d) Chronic Illness ED
Visits, All Races



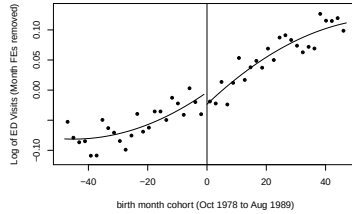
(e) Chronic Illness ED Visits,
Blacks



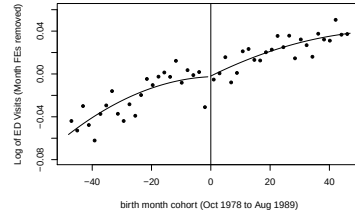
(f) Chronic Illness ED Visits,
Non-Blacks



(g) Non-Chronic Illness ED
Visits, All Races



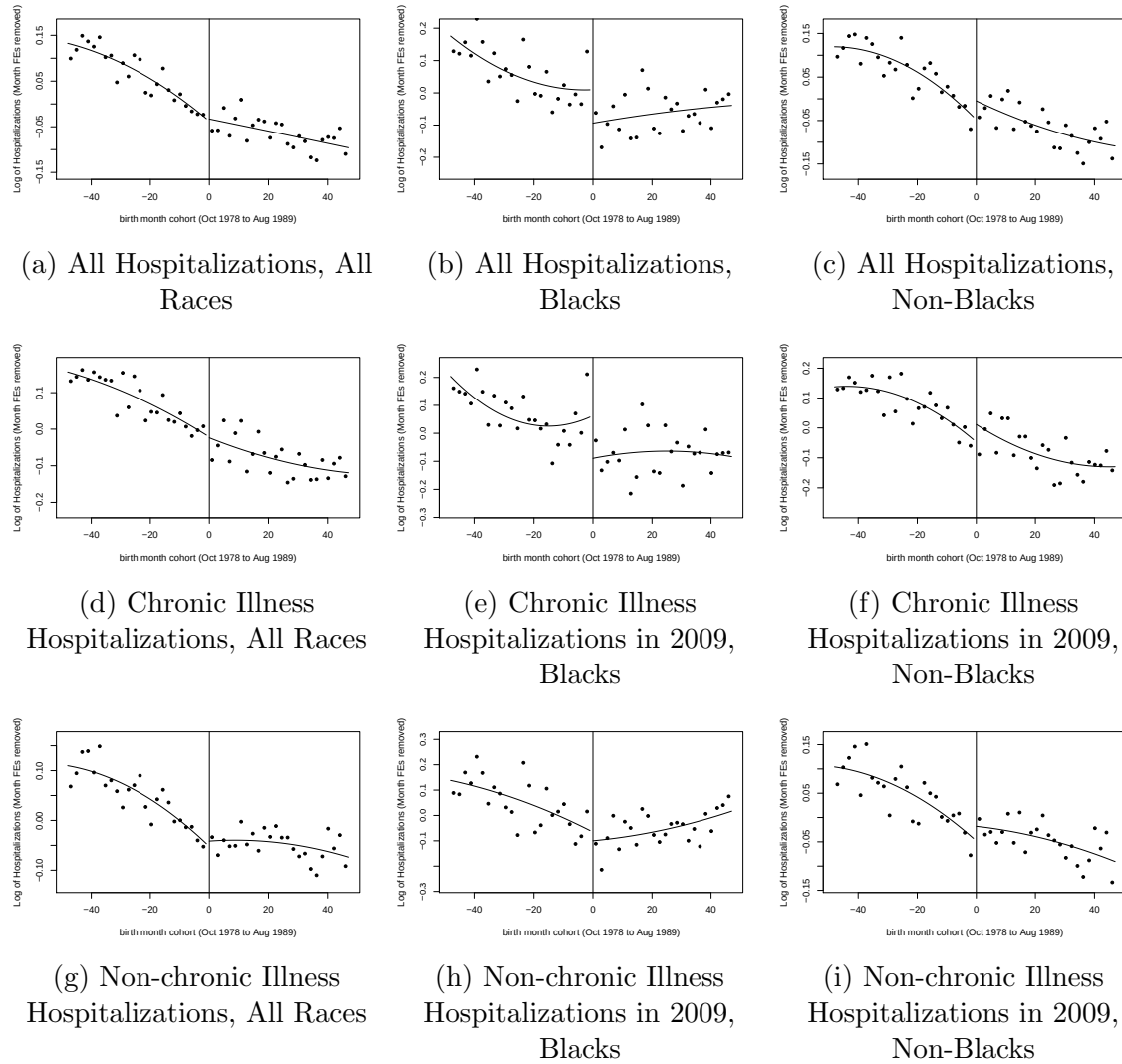
(h) Non-Chronic Illness ED
Visits, Blacks



(i) Non-Chronic Illness ED
Visits, Non-Blacks

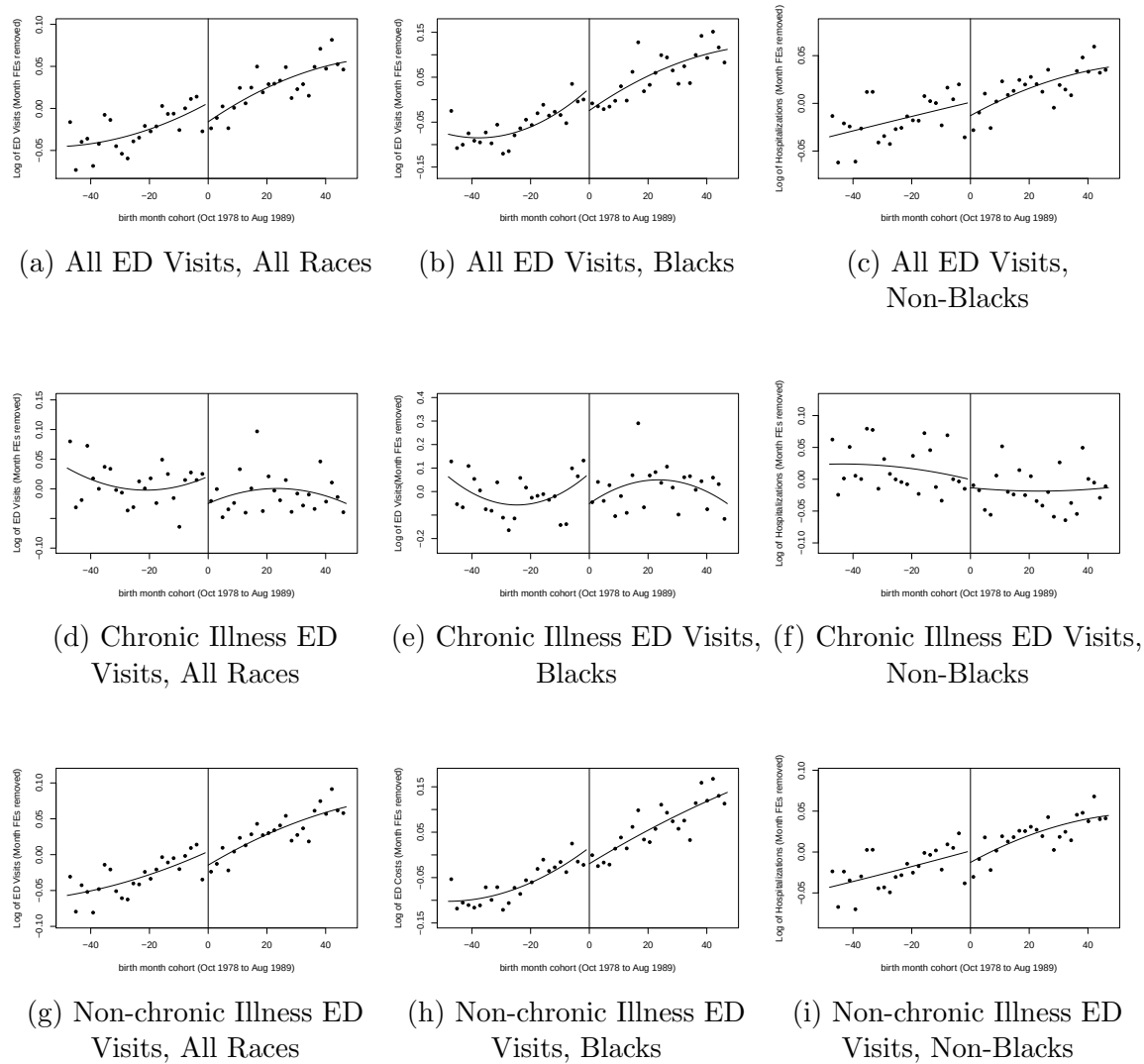
Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all emergency department visits that occurred in 2009 in AZ, CA, HI, IA, KY, NJ, NY, UT, and WI.

Figure 7: 2009 Hospitalizations, Patients from Low-Income Zipcodes, Calendar Month of Birth Fixed Effects Removed



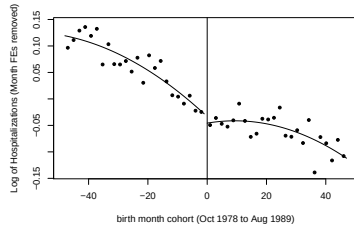
Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all hospitalizations of patients from low-income zipcodes (zipcodes with median income lower than \$39,999) that occurred in 2009 in AR, AZ, CA, CO, HI, IA, KY, MD, MI, NJ, NY, OR, SD, TX, UT, and WI. Figures for all races also include NE and NC.

Figure 8: 2009 Emergency Department Visits by Patients from Low-Income Zipcodes, Calendar Month of Birth Fixed Effects Removed

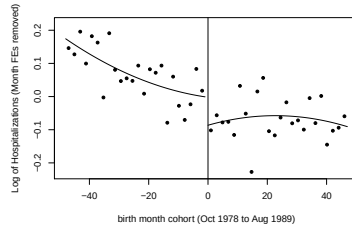


Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all emergency department visits by patients from low-income zipcodes (zipcodes with median income below \$39,999) that occurred in 2009 in AZ, HI, IA, KY, NJ, NY, UT, and WI.

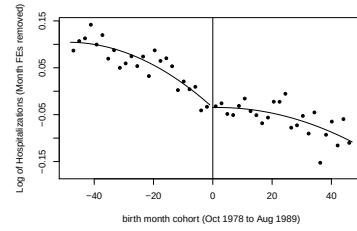
Figure 9: 2009 Hospital Costs, Calendar Month of Birth Fixed Effects Removed



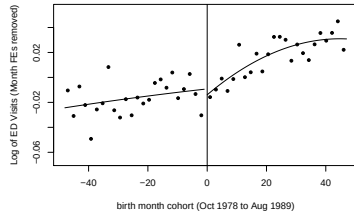
(a) Total Hospital Costs, All Races



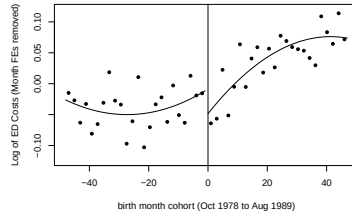
(b) Total Hospital Costs, Blacks



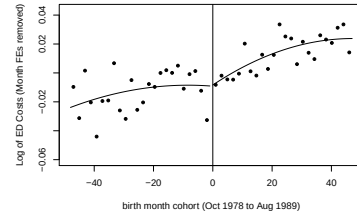
(c) Total ED Costs, Non-Blacks



(d) Total ED Costs, All Races



(e) Total ED Costs, Blacks



(f) Total ED Costs, Non-Blacks

Figures plot the residuals from a regression of the log of hospitalizations by birth month on calendar month of birth fixed effects. Results presented using two-month bins. These models use data on all hospitalizations that occurred in 2009 in AR, AZ, CA, CO, HI, IA, MD, MI, NJ, NY, OR, SD, TX, UT, and WI and ED visits from AZ, CA, HI, IA, KY, NJ, NY, UT, and WI. Hospital cost figures for all races also include NE and NC.

Exhibit 30

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

Early-life Medicaid Coverage and Intergenerational Economic Mobility

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Rourke L. O'Brien¹ and Cassandra L. Robertson²

Abstract

New data reveal significant variation in economic mobility outcomes across U.S. localities. This suggests that social structures, institutions, and public policies—particularly those that influence critical early-life environments—play an important role in shaping mobility processes. Using new county-level estimates of intergenerational economic mobility for children born between 1980 and 1986, we exploit the uneven expansions of Medicaid eligibility across states to isolate the causal effect of this specific policy change on mobility outcomes. Instrumental-variable regression models reveal that increasing the proportion of low-income pregnant women eligible for Medicaid improved the mobility outcomes of their children in adulthood. We find no evidence that Medicaid coverage in later childhood years influences mobility outcomes. This study has implications for the normative evaluation of this policy intervention as well as our understanding of mobility processes in an era of rising inequality.

Keywords

economic mobility, health insurance, inequality, public policy

New estimates of intergenerational economic mobility reveal substantial subnational variation in the degree of economic mobility across U.S. localities (Chetty et al. 2014). These estimates, based on linked administrative tax records for virtually every child born between 1980 and 1993 and their parents, demonstrate that the likelihood a child in the poorest income quintile moves to the richest income quintile in adulthood is strongly conditioned by where he or she grows up. Evidence from families that moved across localities further demonstrates that the local context a child is exposed to early in life has a direct, measureable effect on later-life outcomes, including earnings, college attendance, fertility, and marriage (Chetty and Hendren 2016).

What explains this variation in observed rates of intergenerational economic mobility? In their groundbreaking study, Chetty et al. (2014; hereafter “CHKS”) examine a range of demographic, institutional, and policy correlates of economic mobility across U.S. localities, including government spending, tax policy, and school quality. Notably, variation

in population health and health policy were omitted from this analysis, which is surprising given the large and growing literature detailing the central role of health status in shaping economic outcomes both within and between generations (Haas 2006; Haas, Glymour, and Berkman 2011; Kane 2015).

In this paper we argue that health is a key pathway for the transmission of economic status from parents to children and, consequently, that variation in health policy context can help us understand why the economic-mobility outcomes of children vary across localities and between adjacent birth cohorts. We do this by testing whether variation in one targeted health policy intervention—Medicaid

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public health insurance coverage for low-income pregnant women and their children—has a measurable impact on the economic-mobility outcomes of those children when measured in adulthood. Put another way, we seek to test whether variation in Medicaid eligibility policy across states and over time explains some of the observed variation in the place-based, cohort-specific economic-mobility estimates generated by CHKS.

Previous studies employing rigorous identification strategies and individual-level data consistently find that early-life Medicaid coverage leads to absolute long-run improvements in health, educational attainment, and wages (Black, Devereux, and Salvanes 2007; Cohodes et al. 2014; Levine and Schanzenbach 2009; Miller and Wherry 2017; Smith 2009). Yet demonstrating that a policy intervention improves outcomes for affected individuals does not directly imply that it has an effect on economic-mobility parameters estimated at the population level. For one, the effect of the policy intervention—particularly one targeted to a subset of the population—may be too small to influence the overall level of mobility; expanding early-life Medicaid coverage may improve education and labor market outcomes for kids on the margin without notably impacting the population-level correlation between the income percentile ranks of parents and their children in adulthood. Moreover, the effect of expanding Medicaid coverage to low-income children may be offset by simultaneous shifts in policies or institutional arrangements that benefit children from middle- or upper-income families, resulting in an absolute improvement for all but no net change in relative ranking. At the same time, targeted policy interventions, like Medicaid, can have positive “spillover” effects for individuals not directly impacted by the program—this, too, motivates analyzing the effect of public policies on mobility parameters estimated on the population as whole.

Therefore, in this paper we ask, does state-year variation in early-life Medicaid coverage predict county-by-birth-cohort variation in intergenerational economic mobility? The county-level economic-mobility estimates generated by CHKS are based on cohorts born in the 1980s, a period during which Medicaid eligibility expanded dramatically and unevenly across states as a result of policy changes at the federal and state levels. We exploit this variation to examine whether state-year variation in Medicaid eligibility policy predicts county-by-birth-cohort estimates of intergenerational economic mobility. To identify the causal effect of Medicaid policy on economic mobility, we employ a

simulated measure of Medicaid coverage developed by Currie and Gruber (1996) and Cutler and Gruber (1996) (see also Cohodes et al. 2014; Miller and Wherry 2017) that isolates variation in Medicaid coverage driven by real changes in policy and not due to changes in underlying state demographics.

We find that early-life Medicaid coverage across states and over time has a direct, measureable effect on the observed economic-mobility levels of these birth cohorts as measured in adulthood. Our results indicate that county-birth cohorts exposed to higher levels of Medicaid eligibility in utero and during the first year of life experienced greater levels of economic mobility, operationalized as a reduction in the correlation between the income percentile ranks of parents and their children in adulthood. Notably, we find no effect of Medicaid coverage after infancy on economic-mobility levels, suggesting that coverage extensions to children after infancy had no discernable effect on mobility outcomes. Robustness checks demonstrate that the observed relationship between early-life Medicaid coverage and intergenerational economic mobility cannot be accounted for by simultaneous policy shifts at the state or local level.

Economic mobility, the definition of the “American dream,” is a salient and potent construct in American political discourse; identifying which policies successfully increase the level of economic mobility in American society has important implications for our normative evaluation of these interventions. In demonstrating a causal link between Medicaid and intergenerational economic mobility, this study simultaneously highlights the role of health policy context in explaining spatial and temporal variation in mobility levels and underscores how targeted early-life policy interventions can serve to reduce the determinative power of parents’ economic position on their child’s future economic position. In so doing, this article offers one of the first causal explanations for the observed variation in rates of economic mobility for these cohorts across U.S. localities and has direct implications for ongoing policy debates regarding the size and reach of the Medicaid program and other interventions targeted at improving early-life environments.

BACKGROUND

Health as a Determinant of Intergenerational Economic Mobility

Health, at the level of both individuals and populations, is a key pathway for the transmission

of economic status across generations. Research consistently demonstrates that health status early in life—from the fetal period to infancy to early childhood—has an independent effect on later-life health, educational attainment, and labor market outcomes, which are key determinants of intergenerational economic mobility (Haas 2006; Haas et al. 2011; Smith 2009).

Birth weight—an important measure of health endowment at birth—offers perhaps the clearest illustration of how health can serve to reproduce social inequalities across generations (Kane 2015; Palloni 2006). The incidence of low-weight births, typically defined as less than 2,500 grams, is socially patterned; babies born to low-income and minority mothers are significantly more likely to be underweight than babies born to higher-income parents (Aizer and Currie 2014). Maternal health, exposure to pollution, lack of medical care, poor health behaviors, and greater exposure to sources of stress, such as domestic violence, have all been linked to higher incidence of low-weight births (Aizer 2011; Aizer and Currie 2014; Currie 2009; see also Scharber 2014). Yet beyond individual determinants, birth weight is also strongly conditioned by social and policy context. For example, Walton (2009) found that higher levels of residential segregation are associated with an increased risk of low birth weight for babies born to African American mothers, and Strully, Rehkopf, and Xuan (2010) demonstrated that targeted cash transfers in the form of state-level earned income tax credits (EITCs) reduce the incidence of low-weight births.

In addition to these temporally proximate determinants, recent work by Kane (2015) demonstrated that birth weight is shaped by forces at work long before conception. In a multigenerational analysis using data from the National Longitudinal Survey of Youth 1979 cohort and their children, Kane found that a mother's own early-life conditions impact the likelihood her child will be born underweight. Kane's theoretical model emphasizes health as a key pathway for the transmission of economic status across generations and underscores how disparities in health endowment at birth can serve to perpetuate social and economic inequalities within and between groups.

Infant birth weight has been shown to influence cognitive functioning and school achievement on a variety of measures, including educational attainment and child test scores, even among siblings raised in the same family and social context (Bharadwaj, Eberhard, and Neilson 2018; Black et al. 2007; Conley, Strully, and Bennett 2003;

Currie 2009; Figlio et al. 2013; Oreopoulos et al. 2008). In one recent study using merged birth and school administrative records for Florida children born in the 1990s, Figlio et al. (2013) found that low birth weight is associated with lower test scores, a gap that persists over time and is largely unaffected by school quality. Studies using sibling and twin pairs to isolate the unique effect of infant birth weight on life outcomes have found that the impact of low birth weight extends past school age to the labor market, resulting in lower wages, greater reliance on disability programs, and an increased probability of living in a low-income area in adulthood (Black et al. 2007; Oreopoulos et al. 2008).

Beyond infant health, parental socioeconomic status (SES) impacts health status throughout childhood and adolescence on a wide range of measures, from growth stunting to asthma to obesity (Brooks-Gunn and Duncan 1997; Currie 2009; Currie and Lin 2007). Children who grow up in poverty are at greater risk of a range of negative health-related outcomes, from behavioral disorders to chronic disease to premature death (Currie 2009). And, as Case, Lubotsky, and Paxson (2002) demonstrated, the negative health effects of growing up in a poor family accumulate over time. Poor children have less access to healthcare and lower vaccination rates (Klevens and Luman 2001), indicating that expanding access to health insurance could serve to reduce the degree to which economic and health inequalities are mutually reinforcing.

Poor childhood health is associated with lower cognitive and verbal ability in school and the gap in language and cognitive ability between children of high- and low-SES parents (Salm and Schunk 2008). This disadvantage compounds over time, with childhood health having direct implications for later-life educational achievement and labor market outcomes (see, e.g., Smith 2007, 2009). In one recent analysis using data from the Health and Retirement Study, Haas and colleagues (2011) found that adults who report poor health in childhood earn substantially less during prime working years. The authors demonstrate that poor childhood health reduces lifetime earnings through multiple pathways, including lower educational attainment, reduced labor force participation, and higher disease burden in adulthood.

These studies document the long-term consequences of SES on infant and child health and the subsequent effects of child health on educational and labor market outcomes, illustrating the central role of health in mobility processes. They suggest

that health policy interventions targeted at improving the health status of low-income children early in life may serve to increase economic mobility by mitigating a key pathway for the transmission of economic disadvantage across generations.

Impact of Early-life Medicaid Coverage: Existing Research

Existing research documents the short- and long-run effects of Medicaid coverage on individual health, educational attainment, and labor market outcomes. The seminal work of Currie and Gruber (1996) demonstrated that expanding Medicaid to low-income pregnant women increased health insurance coverage and reduced infant mortality. Currie (1995) further found that expanding public health insurance coverage for low-income children reduced inequalities in healthcare utilization and health outcomes for children. Lurie (2009) also found that Medicaid coverage increased doctor visits, and Joyce and Racine (2005) found a positive effect of coverage on vaccination rates. More recent work by Miller and Wherry (2017) found that early-life exposure to Medicaid had lasting consequences for health in adulthood, specifically in reducing obesity, diabetes, and some types of hospitalizations.

Beyond health and healthcare utilization, there is a large and growing body of evidence linking early-life Medicaid coverage to improved educational outcomes. Levine and Schanzenbach (2009) found that Medicaid expansions increased children's reading test scores. Cohodes et al. (2014) found that early-life Medicaid coverage improved the high school completion, college attendance, and college completion rates of low-income children. Using Internal Revenue Service administrative data, Brown, Kowalski, and Lurie (2015) found significant impacts of early-life Medicaid exposure on individual labor market outcomes. Specifically, they found that children affected by expanding Medicaid coverage in the 1980s and 1990s were more likely to attend college, collected less in EITC payments, and paid more in cumulative taxes by age 28. They further found a positive effect of early-life Medicaid coverage on wages but only for women (see, however, Boudreaux, Golberstein, and McAlpine 2016).

Given this large and growing body of work documenting the long-run positive effects of expanding public health insurance coverage on individual human capital attainment and labor market performance, we might expect state-year variation in Medicaid eligibility policy to have a measureable

impact on population-based estimates of economic mobility. In the next sections, we describe the data used to test this hypothesis.

DATA AND METHODS

Place-based Estimates of Intergenerational Economic Mobility

CHKS generated estimates of intergenerational economic mobility using federal tax records for nearly the total population of 40 million children born between 1980 and 1993 and their parents. CHKS used these data to estimate economic mobility through a multistage process. First, they ranked children within each birth year cohort based on income in adulthood at age 26. They next ranked the parents of these children relative to each other, based on mean family income when the children were young adolescents. The authors then generated an estimate of mobility by comparing the birth cohort-specific income percentile rank of children in adulthood to that of their parents' years earlier; the slope of this rank-rank relationship is the correlation between the child's and parents' positions in the income distribution. CHKS referred to this measure as relative economic mobility. CHKS found that the relationship between mean child income ranks and mean parent income ranks is linear across U.S. localities. Therefore, the overall level of economic mobility for a given county-birth cohort is captured in two key parameters: (1) the slope, which again is the correlation between the income ranks of parents and children, and (2) the intercept, which is the expected mean rank of children at the bottom of the income distribution (i.e., born to families at the zero percentile of income).

In the analyses that follow, we estimated the effect of early-life Medicaid coverage on these two economic mobility parameters. Following CHKS, we also used the slope and intercept to calculate a measure of absolute upward mobility, which is the conditional expectation of a child's rank in adulthood given his or her parents' income rank. Absolute upward mobility provides a more intuitive measure of the prospects of children from each percentile in adulthood and is an important complement to our understanding of relative mobility. We used the slope and intercept to generate measures of absolute upward mobility at given points in the income distribution. Multiplying the rank-rank slope by the specific income percentile in the distribution we wished to examine (10th percentile, 50th percentile, 90th percentile) and adding

the county- and birth cohort-specific intercept yielded the county-birth cohort-specific mean income rank of children born to parents at that point in the distribution.

Given the availability of near population-level data, CHKS were able to generate mobility parameters for children in birth year cohorts across U.S. localities. The analyses presented below used mobility estimates generated by Chetty et al. (2014) from a modified sample of “stayers”—parents and children who remained in the same county until the child was 18. These mobility estimates were generated for every birth cohort from 1980 to 1986 for more than 1,400 counties. An alternative measure of intergenerational mobility for all residents (stayers and movers) was estimated for each birth cohort at the commuting-zone level—clusters of counties oriented around a principal city. As commuting zones cross state lines and Medicaid policy is set at the state level, using commuting zones would add noise to our estimates. We therefore used the county-level estimates generated by CHKS from their modified stayers sample in the analyses that follow; we note that our results are substantively robust to the use of commuting zones as well as to generating state-level mobility rates from these county-level estimates.

Medicaid Expansions: 1980s and 1990s

Medicaid eligibility for low-income pregnant women and children expanded dramatically in the 1980s and 1990s (for an overview, see Currie and Gruber 1996; Miller and Wherry 2017). Prior to 1980, Medicaid eligibility was tied closely to eligibility rules governing Aid to Families with Dependent Children (AFDC) cash welfare; as states set eligibility thresholds for AFDC, there was significant cross-state variation in who qualified for Medicaid. Notably, however, prior to 1980, virtually every state had an income limit for AFDC eligibility that was below the federal poverty line—often below 50% of the federal poverty line. Throughout the 1980s, Congress expanded Medicaid coverage for low-income pregnant women and their newborn children in an effort to reduce infant mortality and improve infant health. Initial expansions of the program came from federal mandates that states provide coverage to any pregnant woman meeting eligibility requirements for AFDC, regardless of their participation in the program, as well as their infant children. Later expansions mandated that

states provide coverage to pregnant women and infants in households with incomes up to 133% of the federal poverty line and gave states the option to expand coverage further still, up to 185% of the federal poverty line.

These policy changes—at both the federal and state levels—yielded a substantial and significant expansion in Medicaid eligibility for pregnant women and infants over the 1980s and 1990s. However, the degree of variation in the percentage of pregnant women eligible for coverage across states and over time can be attributed to changes in eligibility rules, shifts in underlying state demographics, and the interaction between the two. For example, states with relatively low income cutoffs for AFDC—and thereby Medicaid coverage—before the expansions saw larger increases in the percentage of women eligible than states that covered a greater percentage of women from the start. Therefore, variation within states over time is a function of not only state and federal policy shifts but also changes in the socioeconomic composition of the state. Even absent a change in eligibility policy, increasing the percentage of women in poverty will increase the percentage of pregnant women eligible for Medicaid.

In order to isolate the unique effect of Medicaid policy shifts on eligibility net of shifts in state sociodemographics, the analyses presented below employed a simulated measure of state Medicaid eligibility constructed by Miller and Wherry (2017). First employed by Currie and Gruber (1996) and Cutler and Gruber (1996), this measure was generated by applying state-year-specific Medicaid eligibility rules to a nationally representative sample of women ages 15 to 44 drawn from the Current Population Survey. Using a national sample to generate state-year-specific estimates of the fraction of women who would be eligible for coverage if they became pregnant produces an estimate of “simulated eligibility” that is unaffected by any changes in state demographic or economic characteristics that may influence coverage level over and above changes in actual Medicaid policy (see Miller and Wherry 2017). Following Currie and Gruber (1996) and later studies (e.g., Cohodes et al. 2014), we instrumented for state-level changes in Medicaid eligibility with this measure of simulated eligibility.

Tables A1 and A2 in the appendix display the real and simulated percentages of women ages 15 to 44 who would be eligible for Medicaid coverage if they became pregnant, by state for selected years, as well as the percentage change over this time

period. Note that in states with relatively low poverty rates during this time period (e.g., Massachusetts), simulated eligibility is higher than real eligibility, whereas in states with relatively high poverty rates during this time period (e.g., Texas), simulated eligibility is lower than real eligibility. This illustrates that the simulated measure is capturing only variation in eligibility policy and not differences in state sociodemographics.

To examine the effect of increased Medicaid coverage across an individual's childhood, we also employed a state-year measure of Medicaid eligibility coverage for each cohort from ages 1 to 18. This measure was generated by calculating the fraction of children eligible for Medicaid coverage at each age, given a specific birth year and state. The fraction of children eligible was then summed at each age across state and birth cohort, which yielded a measure representing the cumulative average years of public eligibility during childhood. As shifting demographic factors and economic conditions may also influence eligibility across childhood, Miller and Wherry (2017) also constructed a simulated measure of childhood eligibility. The simulated measure of childhood Medicaid eligibility was constructed using a similar method to simulated prenatal eligibility: a nationally representative draw of 1,000 children of a specific age was taken from the American Community Survey, and then the state-year-specific eligibility criteria were applied to this simulated cohort. Then, the fraction of children eligible at each year was summed to generate a simulated measure of cumulative eligibility across childhood.

In the analyses presented below, we used both actual and simulated Medicaid eligibility measures in models estimating the relationship between early-life Medicaid coverage and county-birth cohort observed levels of intergenerational economic mobility.

Analytic Strategy

To estimate the effect of Medicaid coverage on intergenerational economic mobility, we matched the birth cohort-specific county-level mobility estimates to the state-level estimates of Medicaid coverage (both real and simulated) in the same year. Given a gestational period of approximately 40 weeks, matching on birth year is an imperfect measure of coverage: for children born later in the calendar year, this measure captured the entire prenatal period, whereas for children born earlier in

the calendar year, this measure captured coverage levels for more of the first year of life. However, given that Medicaid coverage was typically simultaneously extended to pregnant women and their infant children until age 1, in utero coverage and coverage in infancy were highly correlated. We explore the sensitivity of our findings to alternative lag structures in our Robustness Checks section.

To analyze the effect of Medicaid coverage on intergenerational economic mobility, we estimated

$$Y_{cst} = \beta(\text{MedElig}_{st}) + \beta X_{st} + \gamma_t + \alpha_c + \varepsilon_{scst}$$

where Y_{cst} is the measure of economic mobility at age 26 of children born in year t and county c within state s . X_{st} is a vector of time-varying state-level covariates: percentage of the population with at least a high school degree, percentage black, percentage Hispanic, percentage single-parent households, percentage below the poverty line, percentage unemployed, and log-transformed total income in the state. These covariates were taken from the Current Population Survey (IPUMS). We additionally included a measure of total state and local non-health spending to net out potential confounding due to simultaneous policy shifts at the state or local level. All models included county fixed effects (α_c) to net out all time-invariant characteristics of the county (and state) as well as year fixed effects (γ_t) to net out trends that affected all counties equally. Standard errors were clustered at the state level to account for correlations between counties within states.

For each outcome we present estimates from three different model specifications: ordinary least squares (OLS) estimates using actual Medicaid eligibility coverage, reduced form estimates using our simulated measure of Medicaid coverage, and two-stage least squares (2SLS) instrumental variable (IV) estimates using simulated coverage to instrument for actual coverage. For the 2SLS model specification, the first stage takes the form

$$\text{MedEligReal}_{cst} = \beta(\text{MedEligSimulated}_{st}) + \beta X_{st} + \gamma_t + \alpha_c + \varepsilon_{scst}$$

where the simulated eligibility coverage at the state level predicts real eligibility coverage.

We first examined the effect of Medicaid coverage on the two population economic-mobility parameters estimated by CHKS—the rank–rank slope and intercept—and then analyzed absolute upward-mobility outcomes at different points in the income distribution.

Table 1. Medicaid Coverage in Birth Year and Intergenerational Economic Mobility; County–Birth Cohort Mobility Rank–Rank Slope and Intercept.

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Reduced Form	2SLS	OLS	Reduced Form	2SLS
Variable	Rank–Rank Slope	Rank–Rank Slope	Rank–Rank Slope	Intercept	Intercept	Intercept
Medicaid coverage (real)	–.033 (.037)			.047* (.023)		
Medicaid coverage (simulated)		–.101* (.046)	–.100* (.045)		.081* (.038)	.081* (.038)
First-stage <i>F</i> statistic <i>F</i> (1, 49)			211.98***			211.98***
County–year observations	9,429	9,429	9,429	9,429	9,429	9,429
<i>R</i> ²	.014	.015	.012	.025	.026	.025

Note: OLS = ordinary least squares; 2SLS = two-stage least squares. Robust standard errors in parentheses; standard errors clustered at the state level. All models include state fixed effects, year fixed effects, and a vector of state-level, time-varying covariates. Medicaid coverage refers to percentage of women ages 15 to 44 who would be eligible for Medicaid coverage if pregnant.

†*p* < .10, **p* < .05, ***p* < .01, ****p* < .001 (two-tailed tests).

RESULTS

Table 1 presents estimates of the effect of Medicaid coverage—operationalized as the fraction of women ages 15 to 44 who would be eligible for Medicaid in the event of a pregnancy—on the county-level rank–rank slope (Models 1 to 3) and intercept (Models 4 to 6), the two mobility parameters generated by CHKS. The estimated coefficient in Model 1—using actual Medicaid coverage levels to predict the county–birth cohort’s estimated rank–rank slope—is negative but is not statistically significant. Model 2 substitutes our simulated measure of Medicaid eligibility, which isolates the direct effect of changes in Medicaid eligibility policy on coverage levels, netting out variation in coverage due to shifts in underlying demographics. The estimated coefficient on the simulated measure of Medicaid eligibility is both larger and statistically significant.

Model 3 presents results from the 2SLS where we use the simulated measure of Medicaid coverage to instrument for actual Medicaid coverage. The first-stage *F* statistic on excluded instruments is over 200, indicating that the instrument is robust and subject to minimal bias. The point estimate indicates that a 10–percentage point increase in Medicaid coverage is associated with a 1-point decrease in the correlation between the income ranks of parents and children; this indicates that state variation in Medicaid coverage levels for cohorts in utero and during the first year of life had

a direct, demonstrable impact on the economic-mobility outcomes of those cohorts as measured in adulthood.

Models 4 through 6 present estimates from models analyzing the impact of Medicaid coverage on the county-level intercept. Across all three model specifications we see a positive relationship between Medicaid coverage and the county-level intercept estimated by CHKS. To more readily interpret the effect of Medicaid coverage on mobility outcomes, Table 2 presents estimates from models predicting absolute economic mobility, operationalized as the expected mean income rank in adulthood of children born to parents at a given point in the income distribution. These mobility estimates were calculated using the county-level slope and intercept analyzed above. Here again we present estimated coefficients from three model specifications to examine the effect of Medicaid coverage on the absolute mobility outcomes of children born to parents at the 10th percentile of the national income distribution (Models 1 to 3), the 50th percentile (Models 4 to 6), and 90th percentile (Models 7 to 9).

Turning first to the estimated effect of Medicaid coverage on the absolute mobility outcomes of children from low-income families, we see a positive and statistically significant coefficient across all three model specifications. Estimates from the preferred IV model specification indicates a

Table 2. Medicaid Coverage in Birth Year and Intergenerational Economic Mobility: Expected Income Rank of Children Born at 10th, 50th, and 90th Income Percentiles.

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	Reduced Form	2SLS	OLS	Reduced Form	2SLS	OLS	Reduced Form	2SLS
	10th Percentile	10th Percentile	10th Percentile	50th Percentile	50th Percentile	50th Percentile	90th Percentile	90th Percentile	90th Percentile
Medicaid coverage (real)	.044* (.020)			.030 [†] (.016)			.017 (.023)		
Medicaid coverage (simulated)		.071* (.035)	.071* (.035)		.031 (.030)	.031 (.030)		-.009 (.035)	-.009 (.034)
First-stage <i>F</i> statistic <i>F</i> (1, 49)			211.98***			211.98***			211.98***
County-year observations	9,429	9,429	9,429	9,429	9,429	9,429	9,429	9,429	9,429
<i>R</i> ²	.030	.031	.029	.058	.057	.058	.029	.029	.029

Note: OLS = ordinary least squares; 2SLS = two-stage least squares. Robust standard errors in parentheses; standard errors clustered at the state level. All models include state fixed effects, year fixed effects, and a vector of state-level, time-varying covariates. Medicaid coverage refers to percentage of women ages 15 to 44 who would be eligible for Medicaid coverage if pregnant.

[†]*p* < .10, **p* < .05, ****p* < .001 (two-tailed tests).

Table 3. Medicaid Coverage in Birth Year and Exposure in Childhood on County–Birth Cohort Mobility Rank–Rank Slope and Intercept.

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Reduced Form	2SLS	OLS	Reduced Form	2SLS
Variable	Rank–Rank Slope	Rank–Rank Slope	Rank–Rank Slope	Intercept	Intercept	Intercept
Medicaid coverage: pregnant women (real)	–.031 (.037)			.043 [†] (.022)		
Medicaid coverage: childhood (real)	–.142 (.218)			.259 [†] (.155)		
Medicaid coverage: pregnant women (simulated)		–.102* (.043)	–.099* (.044)		.082* (.034)	.080* (.035)
Medicaid coverage: childhood (simulated)		–.518 (.456)	–.573 (.536)		.525 (.412)	.579 (.477)
First-stage <i>F</i> statistic <i>F</i> (2, 49)			107.17***			107.17***
County–year observations	9,429	9,429	9,429	9,429	9,429	9,429
<i>R</i> ²	.014	.015	.011	.027	.028	.023

Note: OLS = ordinary least squares; 2SLS = two-stage least squares. Robust standard errors in parentheses; standard errors clustered at the state level. All models include state fixed effects, year fixed effects, and a vector of state-level, time-varying covariates.

[†]*p* < .10, **p* < .05, ***p* < .01, ****p* < .001 (two-tailed tests).

10–percentage point increase in Medicaid Coverage is associated with .7-point higher expected mean income rank in adulthood. A .7-point increase over the expected mean rank of children born to parents at the 10th percentile translates to approximately \$500 higher yearly income in adulthood. Notably, we see no effect of Medicaid coverage on the absolute mobility outcomes of children born to parents at the 50th percentile of the national income distribution (Models 4 to 6) or to children born to parents at the 90th percentile.

Taken together, these estimates reveal that expanding Medicaid coverage to low-income pregnant women and their infant children improved the mobility outcomes of those children as measured in adulthood. This indicates that health policy context very early in life—in utero and during the first year—has direct implications for the mobility outcomes of those children measured decades later. But what about Medicaid coverage levels after infancy? Does variation in Medicaid coverage during childhood years (ages 1 to 18) have a measurable impact on a county–birth cohort’s observed economic mobility outcomes?

Table 3 reproduces the main models presented in Table 1, now including a measure of Medicaid

eligibility during childhood. Across all three model specifications, we see no effect of increased Medicaid eligibility in childhood on either of the county–birth cohort mobility parameters. These models suggest that while state Medicaid coverage levels in utero and in infancy influence county–cohort economic-mobility outcomes, expansions of coverage in later childhood have no measurable impact on observed mobility levels. This finding runs in line with Miller and Wherry (2017), who also find strong evidence for early-life coverage and no evidence of positive effects for coverage extended in childhood, and runs counter to Cohodes et al. (2014) and Brown et al. (2015), who find significant long-run positive effects of coverage gained in later childhood.

Robustness Checks

Employing an IV strategy allowed us to isolate the direct effect of changing Medicaid eligibility policy on economic mobility, net of shifts in state economic conditions and sociodemographics. Yet, it could be argued that simultaneous trends or policy shifts occurring during this same time period may be confounding our analyses. We attempted to

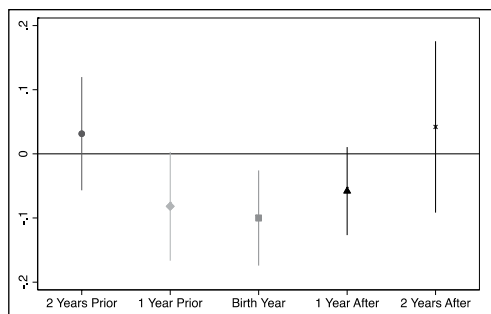


Figure 1. Estimated Effect of Medicaid Eligibility before and after Focal Cohort's Birth Year on County Rank–Rank Slope.

Note: Coefficients and 90% confidence intervals from fully adjusted instrumental variable models.

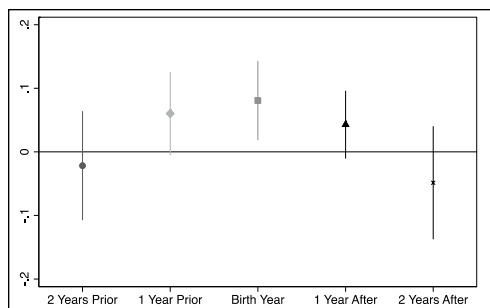


Figure 2. Estimated Effect of Medicaid Eligibility before and after Focal Cohort's Birth Year on County Mobility Intercept.

Note: Coefficients and 90% confidence intervals from fully adjusted instrumental variable models.

account for this in our main models by including a measure of total state and local nongovernment spending; however, this covariate may have been too coarse to pick up policy shifts that are not captured in aggregate spending data. To account for potential co-occurring policy changes at the state and local level, we conducted several additional robustness checks. We first tested the sensitivity of our estimates to different measures of state and local government spending, including total direct spending and total intergovernmental spending; these alternative operationalizations had no effect on the size or significance of our estimates. We then reestimated our models with the inclusion of state-specific linear time trends; here again the effect of Medicaid coverage on mobility outcomes remains large and statistically significant. This provides additional evidence that the observed association between Medicaid policy and mobility outcomes is not being driven by correlated trends at the state level.

To further test that the observed association between Medicaid eligibility and mobility outcomes is not being driven by contemporaneous policy shifts, we conducted a “placebo test” by estimating the effect of Medicaid coverage on mobility outcomes using simulated Medicaid eligibility one and two years before and one and two years after the focal birth cohort (see Chetty and Hendren 2016). If Medicaid expansions were correlated with other state-level trends or policy interventions that might also affect economic mobility (e.g., investing in education or labor market reforms), we would expect our measures of Medicaid coverage to predict mobility outcomes for adjacent cohorts. Figures 1 through 3 present estimated coefficients

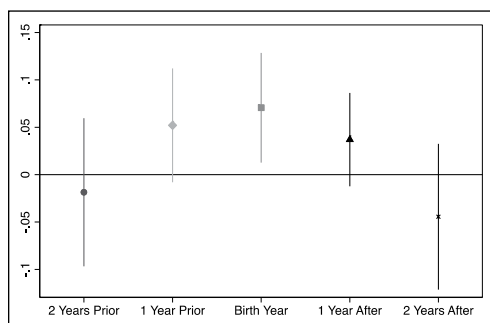


Figure 3. Estimated Effect of Medicaid Eligibility before and after Focal Cohort's Birth Year on Expected Mean Income Rank of Children Born at 10th Percentile.

Note: Coefficients and 90% confidence intervals from fully adjusted instrumental variable models.

from IV models for three different mobility outcomes: rank–rank slope, intercept, and absolute mobility for children born to parents at the 10th percentile. Across all three outcomes, the coefficient on Medicaid coverage two years before and two years after the birth year are not statistically significant. Notably, the coefficient on Medicaid eligibility for one year before and one year after the focal birth cohort approaches statistical significance and is slightly smaller in magnitude relative to the estimate from the focal birth cohort. This is expected given that Medicaid coverage was typically extended simultaneously to low-income pregnant women and infants up to age 1, resulting in an overlapping window of coverage in utero and during the first year of life. This falsification test provides further evidence that the observed association between

Medicaid eligibility and economic mobility outcomes is not being driven by other state-level trends or co-occurring policy shifts.

DISCUSSION

Analyzing new place-based estimates of intergenerational economic mobility, we find that the Medicaid expansions of the 1980s increased the level of economic mobility experienced by affected cohorts. Increasing the percentage of low-income pregnant women and infant children eligible for this public health insurance program effectively reduced the correlation between the economic position of parents and their children. This finding not only offers additional evidence of the positive, long-run effects of the Medicaid program but also serves to illustrate how subnational policy variation can help account for observed variation in economic-mobility outcomes across localities and between birth cohorts.

Health is a critical pathway for the transmission of economic status across generations; our findings add to a growing body of work demonstrating how targeted early-life interventions that improve infant and maternal health can have lasting positive consequences. Improving maternal and infant health, however, is just one pathway through which expanding health insurance coverage may impact overall economic-mobility levels. Studies have linked insurance coverage, and the Medicaid expansions specifically, to improved outcomes on other measures, including a reduction in bankruptcies from medical debt (Gross and Notowidigdo 2011). In reducing out-of-pocket costs and shielding families from potentially devastating debt (see Kalousova and Burgard 2013), increasing insurance coverage may serve to improve mobility outcomes by enabling families to invest more income in mobility-enhancing pathways, such as education. Although our analytic strategy—which isolates exposure to public health insurance in the birth year—suggests Medicaid affects mobility outcomes primarily by improving infant and maternal health and access to healthcare, we cannot rule out the myriad of additional pathways through which expanding insurance coverage leads to improvements in economic mobility.

It is also important to acknowledge the data used in this analysis present several clear limitations. First, the mobility estimates generated by CHKS measure child income only at age 26, whereas previous mobility measures capture child income later in life, during their prime working

years. However, CHKS find that mobility estimates stabilize when children reach their late 20s, as opposed to the later ages favored by much of the literature, and demonstrate that using several years of data to measure a child's income has a negligible effect on mobility estimates. A second limitation of our study is that the mobility parameters generated by CHKS are built on the strong assumption that the relationship between the income percentile ranks of children and their parents is linear. As we use the publicly available data from CHKS, we do not have the individual-level data, and we thus cannot confirm the linearity of the relationship independently. However, CHKS provide substantial evidence to support their claim that this operationalization of the mobility relationship is accurate (see Data Appendix in Chetty et al. 2014). As such, we feel confident that, taken together, these parameters offer a useful summary metric of the local economic-mobility contexts.

At the same time, we must caution readers about the generalizability of our findings. This analysis demonstrates that expanding Medicaid coverage to low-income pregnant women in the 1980s and 1990s improved the economic-mobility outcomes of their children in adulthood. In so doing, this study adds to the growing body of evidence documenting the lasting positive consequences of this program expansion for low-income families. Yet these findings do not suggest that further expanding Medicaid eligibility will necessarily result in higher levels of economic mobility. This is because the Medicaid expansions of the 1980s, 1990s, and later extended coverage to very-low-income individuals; further expansions would therefore extend coverage to relatively less disadvantaged persons, who may not benefit from the program in the same way or to the same degree (see, however, Baicker and Finkelstein 2011). Moreover, it should be emphasized that the findings of this particular study—and of any study of economic mobility or other outcomes that span generations—are specific to the cohorts and time period analyzed. Studies of past social processes cannot be readily generalized to predictions of the future.

Nevertheless, this article provides further evidence that targeted interventions early in life can effectively mitigate the deterministic power of parents' social position on child outcomes. Indeed, policies and programs that focus on improving maternal and infant health—for example, through nutrition programs—or improving a child's social and cognitive development—for example, through Head Start and other early-schooling interventions—are also

likely to play an important role in explaining variation in economic-mobility outcomes across place and over time. Future work should examine potential complementarities across programs to better understand how variation in the generosity of social safety-net programs and general public spending—on health, education, income assistance, and so on—serve to structure mobility outcomes.

Understanding the effect of public policies and programs on the structure of opportunity is important for our normative evaluation of these interventions. In an era of rising inequality, even public policies and programs that have been found to improve the

absolute physical or material well-being of families may not necessarily influence patterns of economic mobility. Fully capturing the effect of policy interventions on mobility processes requires us to consider all of the pathways through which economic status is transmitted across generations. Health—across generations, throughout the life course, and between populations—must be integrated into analyses of economic mobility at both individual and population levels. Doing so will not only improve our understanding of how social inequalities are reproduced across generations but also reveal how policy interventions that improve health serve to affirm the American dream.

APPENDIX

Table A1. Percentage of Women Ages 15 to 44 Eligible for Medicaid Coverage by State.

State	Percentage of Women Eligible			Percentage Change in Eligibility	
	1980	1986	1993	1980–1986	1986–1993
Alabama	10.16	17.00	40.86	67.30	140.29
Alaska	6.17	23.04	32.77	273.70	42.24
Arizona	1.83	12.32	38.82	572.26	215.02
Arkansas	7.65	16.81	43.71	119.93	159.95
California	25.64	29.98	61.25	16.94	104.28
Colorado	6.55	19.20	28.12	193.27	46.49
Connecticut	11.60	17.68	34.45	52.42	94.77
Delaware	5.64	14.71	42.25	160.91	187.23
District of Columbia	20.65	21.37	58.06	3.51	171.62
Florida	10.56	12.06	50.88	14.13	322.03
Georgia	3.62	13.81	34.69	282.11	151.15
Hawaii	16.78	22.75	67.84	35.55	198.21
Idaho	13.70	17.74	31.36	29.51	76.82
Illinois	14.96	18.05	32.15	20.67	78.08
Indiana	4.40	14.34	38.92	226.06	171.49
Iowa	6.39	17.14	39.42	168.08	130.02
Kansas	12.51	16.28	37.32	30.12	129.27
Kentucky	9.79	14.88	52.13	51.96	250.44
Louisiana	39.23	51.94	61.34	32.41	18.10
Maine	15.22	23.51	45.71	54.49	94.46
Maryland	14.92	15.46	32.55	3.63	110.52
Massachusetts	16.44	19.15	39.26	16.48	104.97
Michigan	18.18	24.26	42.89	33.43	76.79
Minnesota	10.44	26.81	59.51	156.83	122.00
Mississippi	6.05	28.42	59.49	369.94	109.33
Missouri	5.25	17.20	38.32	227.32	122.82
Montana	13.67	19.91	32.85	45.62	64.96

(continued)

Table A1. (continued)

State	Percentage of Women Eligible			Percentage Change in Eligibility	
	1980	1986	1993	1980–1986	1986–1993
Nebraska	15.21	16.96	24.42	11.53	43.96
Nevada	9.71	9.80	26.45	0.90	169.95
New Hampshire	6.02	8.96	25.33	48.84	182.77
New Jersey	11.66	16.88	55.32	44.76	227.67
New Mexico	9.85	18.27	52.24	85.41	185.94
New York	21.69	26.49	46.68	22.12	76.22
North Carolina	10.16	12.60	43.99	24.07	249.09
North Dakota	13.91	16.78	31.54	20.65	87.97
Ohio	10.47	16.94	31.31	61.83	84.81
Oklahoma	8.00	15.42	46.61	92.88	202.28
Oregon	12.47	16.23	30.76	30.16	89.47
Pennsylvania	13.59	16.48	39.46	21.26	139.44
Rhode Island	21.11	17.03	47.35	–19.35	178.11
South Carolina	10.04	22.98	53.91	128.92	134.62
South Dakota	7.32	21.74	32.20	196.95	48.10
Tennessee	8.99	13.72	44.19	52.68	221.99
Texas	3.48	12.24	49.27	251.59	302.56
Utah	9.99	29.52	26.31	195.39	–10.87
Vermont	20.35	23.39	42.95	14.95	83.62
Virginia	12.53	13.46	25.40	7.37	88.71
Washington	18.34	23.87	35.51	30.15	48.76
West Virginia	8.54	21.45	45.98	151.17	114.42
Wisconsin	15.41	25.41	34.23	64.90	34.68
Wyoming	3.62	18.57	30.17	412.78	62.47

Table A2. Simulated Percentage of Women Ages 15 to 44 Eligible for Medicaid Coverage by State.

State	Simulated Percentage of Women Eligible			Percentage Change in Eligibility	
	1980	1986	1993	1980–1986	1986–1993
Alabama	7.44	10.65	34.35	43.19	222.62
Alaska	9.54	33.94	41.05	255.58	20.97
Arizona	5.07	16.60	35.86	227.50	116.05
Arkansas	8.37	12.44	34.35	48.54	176.20
California	26.39	30.01	58.61	13.70	95.29
Colorado	10.80	21.87	34.35	102.44	57.11
Connecticut	17.41	25.23	44.82	44.96	77.65
Delaware	7.05	16.75	43.65	137.78	160.55
District of Columbia	14.04	19.19	44.90	36.62	134.06
Florida	8.75	14.41	44.90	64.64	211.66
Georgia	4.61	15.00	37.11	225.58	147.33
Hawaii	18.92	23.68	73.84	25.15	211.88
Idaho	12.93	16.40	34.46	26.79	110.13

(continued)

Table A2. (continued)

State	Simulated Percentage of Women Eligible			Percentage Change in Eligibility	
	1980	1986	1993	1980–1986	1986–1993
Illinois	12.63	17.48	34.35	38.44	96.50
Indiana	6.76	15.89	37.38	135.03	135.27
Iowa	8.94	20.44	44.90	128.48	119.72
Kansas	15.71	19.58	37.38	24.67	90.87
Kentucky	10.43	12.51	44.82	19.97	258.18
Louisiana	34.17	46.37	48.25	35.69	4.07
Maine	13.49	27.11	42.93	100.96	58.35
Maryland	14.99	18.38	44.82	22.63	143.92
Massachusetts	17.50	24.57	48.17	40.42	96.03
Michigan	16.25	22.89	44.78	40.87	95.58
Minnesota	15.75	25.74	63.47	63.40	146.56
Mississippi	5.33	19.40	44.80	264.13	130.93
Missouri	6.68	15.89	34.35	137.96	116.23
Montana	14.79	18.69	34.35	26.34	83.82
Nebraska	14.40	18.85	34.35	30.87	82.24
Nevada	13.07	16.26	34.35	24.41	111.28
New Hampshire	13.04	20.77	37.54	59.34	80.72
New Jersey	13.04	20.03	67.88	53.52	238.97
New Mexico	9.38	14.63	44.80	56.06	206.15
New York	17.82	25.32	44.82	42.05	77.06
North Carolina	9.99	14.63	44.80	46.48	206.31
North Dakota	12.80	19.47	34.35	52.10	76.46
Ohio	11.22	17.07	34.35	52.20	101.22
Oklahoma	12.00	17.31	37.38	44.25	115.94
Oregon	12.13	21.42	34.35	76.62	60.38
Pennsylvania	14.40	19.44	44.82	34.97	130.56
Rhode Island	18.63	22.35	48.17	19.99	115.51
South Carolina	9.90	19.71	44.80	99.09	127.35
South Dakota	9.08	19.56	34.35	115.46	75.66
Tennessee	8.84	11.74	44.80	32.75	281.68
Texas	4.08	12.68	44.82	210.61	253.52
Utah	15.75	32.35	34.35	105.34	6.19
Vermont	21.46	26.33	48.71	22.72	84.98
Virginia	11.17	16.45	34.35	47.29	108.81
Washington	18.42	24.89	43.46	35.09	74.60
West Virginia	10.51	14.76	37.38	40.33	153.31
Wisconsin	17.83	26.85	38.59	50.61	43.75
Wyoming	6.45	19.62	34.35	203.88	75.13

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Exhibit 31

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

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THE LONG-RUN EFFECTS OF CHILDHOOD INSURANCE COVERAGE:
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The Long-Run Effects of Childhood Insurance Coverage: Medicaid Implementation, Adult Health, and Labor Market Outcomes

Andrew Goodman-Bacon

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ABSTRACT

This paper exploits the original introduction of Medicaid (1966-1970) and the federal mandate that states cover all cash welfare recipients to estimate the effect of childhood Medicaid eligibility on adult health, labor supply, program participation, and income. Cohorts born closer to Medicaid implementation and in states with higher pre-existing welfare-based eligibility accumulated more Medicaid eligibility in childhood but did not differ on a range of other health, socioeconomic, and policy characteristics. Early childhood Medicaid eligibility reduces mortality and disability and, for whites, increases extensive margin labor supply, and reduces receipt of disability transfer programs and public health insurance up to 50 years later. Total income does not change because earnings replace disability benefits. The government earns a discounted annual return of between 2 and 7 percent on the original cost of childhood coverage for these cohorts, most of which comes from lower cash transfer payments.

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In 2014, the joint federal and state public health insurance programs, Medicaid and the State Children's Health Insurance Program, covered 40 percent of children and cost \$475 billion. Costs have been central to recent arguments about the size of the Medicaid program (Sommers and Epstein 2013). Current federal budget proposals would convert Medicaid into a block grant program, several states have implemented major cuts to eligibility and services, and six states have recently considered opting out of the program (Adamy and King Jr. 2010).

Short-run empirical evaluations show that Medicaid improves health, but this has not resolved debates about the program's future. For example, while Medicaid saves lives (Currie and Gruber 1996a, b, Goodman-Bacon forthcoming, Sommers, Baicker, and Epstein 2012), the health effects are small for middle-income groups, and costs per life saved can be high. Experimental estimates from the Oregon Health Insurance Experiment show improvements in self-reported health measures but not in clinical measures, providing support for both Medicaid's advocates (Kishore 2014) and critics (Antos and Capretta 2014, Roy 2014). Therefore, short-run health effects may not justify the size of the program (Finkelstein, Hendren, and Luttmer 2015).

Accounting for Medicaid's effects over the course of its recipients' lives may drastically change this cost-benefit calculation. Because it primarily covers children during critical periods, Medicaid may have its largest effects later in life (Cunha, Heckman, and Schennach 2010). Improvements in adult health and economic outcomes could also *lower* public costs by reducing transfers or health care spending in programs linked to poor health, or by increasing tax revenue.

New research based on eligibility expansions from the 1980s shows that Medicaid can have positive long-run effects on health, human capital, earnings, and tax payments (Brown, Kowalski, and Lurie 2014, Cohodes et al. 2014, Miller and Wherry 2014, Wherry and Meyer 2013, Wherry et al. 2015). Yet these studies observe cohorts in their 20s, so longer-run effects, especially those tied to health conditions that emerge at older ages, may be significantly larger or smaller than existing estimates. Direct estimates of longer-run effects are central to conclusions about the return to Medicaid spending, though. When calculating Medicaid's return on investment, for example, Brown, Kowalski, and Lurie (2014) assume that effects at age 28 will persist for 32 years, and this assumption accounts for three quarters of the estimated return (42 of 56 cents per dollar, p. 21).

This paper provides new evidence on Medicaid's longer-run effects by exploiting the program's introduction between 1966 and 1970 and the federal mandate that Medicaid cover all

cash welfare recipients (“categorical eligibility”). These two program features led to a sudden increase in public insurance eligibility that was larger in areas with higher welfare participation. From a long-run perspective, cohorts born closer to Medicaid spent more years potentially eligible for it, and those from higher-welfare states had a higher eligibility rate in each year. Thus, *cumulative* childhood Medicaid eligibility phased in gradually across cohorts but more quickly for those from higher-welfare states.

To estimate the effect of cumulative Medicaid eligibility on adult outcomes, I use a difference-in-differences model that compares cohorts born at different times relative to Medicaid implementation in states with different categorical eligibility rates in the year *of* implementation. This initial welfare rate provides a fixed way to compare states with different levels of pre-Medicaid categorical eligibility. Variation in initial welfare rates came from long-standing institutional features of states, and was uncorrelated with levels or trends in economic, demographic, health, and policy characteristics; nevertheless, this variation strongly predicts cumulative Medicaid eligibility and contemporaneous Medicaid participation (Goodman-Bacon forthcoming). Comparing adult outcomes across cohorts born in different years relative to Medicaid implementation and in states with different initial welfare rates is therefore unlikely to confound the program’s effects with other health, socioeconomic, or policy changes.

Event-study specifications support the validity of the design by showing the relationship between initial eligibility and adult outcomes for *each* cohort born up to 30 years before and five years after Medicaid implementation. Outcome variables are measured by state and year of birth and include cumulative mortality rates from 1980-1999 (using Vital Statistics Mortality Files), self-reported disability rates, labor market status, transfer program participation, and the distribution of earnings and transfer income (from the 2000-2014 Census and American Community Survey). These outcomes track patterns of eligibility closely: they are uncorrelated with initial Medicaid eligibility for respondents who are too old to have qualified as children; diverge gradually in higher- versus lower-eligibility states for cohorts with increasing years of exposure; and flatten out for post-Medicaid cohorts with the same predicted childhood eligibility.

The results show that cohorts with early-life Medicaid eligibility experience lower adult mortality and disability rates. Among white adults, these health improvements reduce the use of disability benefits and public insurance, and increase employment. New earnings largely offset lower transfers, leaving individual income unchanged, a similar form of program substitution as

that identified by Kline and Walters (2016). The government, on the other hand, saves on benefit payments *and* earns a small amount of new income tax revenue: \$21.2 billion per year in total relative to a total cost of childhood coverage of about \$132 billion in 2012 dollars. At a standard 3 percent discount rate, these changes imply about a 7 percent return *every year* on the initial investment. Using observed treasury rates to discount the costs and benefits implies about a 2 percent return and suggests that, between 2000 and 2014, the government recouped about 28 percent of the (true) original cost. Almost 60 percent of this return comes from reductions in cash transfer payments, and the remainder comes from increased income tax revenue (28 percent) and lower public insurance spending (13 percent).

I. EXPECTED EFFECTS OF MEDICAID IMPLEMENTATION ON LATER-LIFE OUTCOMES

Medicaid's original introduction provides an especially clean context in which to study the program's long-run effects. Before Medicaid, private insurance was rare among the poor, public medical programs were small, and free sources of medical care were uncommon and often of low quality (Goodman-Bacon 2015).¹ As a result, poor children frequently went without medical care. Figure 1 shows that fewer than half of poor children in the early 1960s had seen a doctor in the previous year relative to three quarters of middle-income children.

Poor children were also strikingly unhealthy in ways that extended into adulthood. Their mortality rates were twice as high as those of non-poor children (National Center for Health Statistics 1965), and they suffered more often from a range of specific symptoms.² In terms of adult health, one highly publicized 1964 report showed that over one-quarter of Army inductees were rejected on medical grounds, most commonly for diseases and defects of the "bones and organs of movement" (President's Task Force on Manpower Conservation 1964). The report's

¹ Only about eight percent of adults received any free care in 1960 (Morgan et al. 1962), and only 2.8 and 13.4 percent of low-income children in non-Medicaid states had doctor or clinic visits (respectively) without charges in 1969 (Loewenstein 1971, p. 2.11 table 2.31). 9 percent of respondents (with children) in the 1968 PSID reported that they could get "free care." Anecdotal evidence suggests that free care was low-quality and hard to obtain. A 1964 Children's Bureau report describes a hospital outpatient department in Dallas, Texas as "deplorable." In Birmingham, Alabama "many [are] turned away from outpatient clinic (40 or more a day) due to lack of funds...a mother returned with her dead baby in a sack" (Lesser et al. 1964). One hospital administrator in New York City bemoaned the passage of Medicaid, asking "How do you expect [continuing medical research] to be carried out if patients come to the hospital only for medical care and are not interested in taking part in new and as yet unaccepted methods of treatment?" (Stevens and Stevens 1974, pp. 99).

² Parental reports of specific disease incidence appear not to provide reliable measures of disease burden. In the 1963-1965 National Health Examination Survey (USDHHS/NCHS 1991), for example, higher-income children are more likely to report having mumps, bronchitis, scarlet fever, polio, allergies, or a heart murmur. However, poor children are more likely to have symptoms that are observable without a diagnosis such as a sore throat, colds, a "heart problem," or identifiable conditions such as whooping cough.

“most significant finding” was that these differences were correlated with socioeconomic status and that “75 percent of all persons rejected for failure to meet the medical and physical standards would probably benefit from treatment” (italics in original, pp 25).

A. Medicaid Implementation, Children’s Insurance Coverage, and Aggregate Utilization

Medicaid’s passage as title XIX of the 1965 Social Security Act Amendments represented a major expansion in the availability and generosity of (publicly funded) medical care for poor children relative to the small existing federal/state medical financing system for welfare recipients. Medicaid removed federal reimbursement caps, increased federal matching rates, defined a set of required medical services (inpatient, outpatient, physician, lab/x-ray, and nursing home) and mandated coverage for recipients of cash transfer programs (the “categorical eligibility” requirement). Almost all categorically eligible children (89 percent) qualified through the Aid to Families with Dependent Children (AFDC) program (DHEW 1976). All states except Alaska (1972) and Arizona (1982) implemented Medicaid between 1966 and 1970.

During Medicaid’s first decade, states also expanded their efforts to identify and screen children for debilitating but treatable conditions. The Early and Periodic Screening, Diagnosis and Treatment (EPSDT) program required states to locate eligible children and “ascertain their physical or mental defects, and [provide] such health care, treatment, and other measures to correct or ameliorate defects and chronic conditions discovered thereby” (PL 90-248 quoted in Stevens and Stevens 1974).³ President Johnson stressed EPSDT’s potential later-life effects when he advocated for the program: “Ignorance, ill health, personality disorder—these are disabilities often contracted in childhood: afflictions which linger to cripple the man and damage the next generation” (Johnson 1967).

Immediately following Medicaid implementation, public insurance coverage among children increased sharply while uninsurance rates fell. Less than one percent of children had public coverage in 1963, but about 15 percent did by the mid-1970s, and almost all of this increase reflected reductions in uninsurance (Goodman-Bacon 2015, figure 1).

³ Stevens and Stevens (1974) discuss lags in the promulgation of EPSDT regulations in the late 1960’s but emphasize that the program was a major new proposal, requiring screened children to receive a “full health history, an analysis of physical growth, developmental assessment, unclothed physical inspection, ear, nose, mouth, and throat inspection, vision testing, hearing testing, anemia testing, sickle cell, TB, urine and lead-poisoning testing, as well as nutritional and immunization status reports” (pp. 257, note 50). They also cite an early experience in Mississippi in which “1300 abnormalities were discerned in the first 1200 children screened” (quoting Howard Newman, pp. 257 note 51).

The large increase in coverage meant that poor children received substantially more medical care. Appendix table 1.1 presents cross-sectional differences in utilization by Medicaid eligibility across 10 surveys from before and after Medicaid implementation showing that children eligible for Medicaid used much more medical care than ineligible poor children in the same state, or similar children in non-Medicaid states.⁴ Figure 1 shows the net result of these utilization increases: the steep income gradient in children's doctor visits in the early 1960s almost completely disappeared by 1975.

B. State Differences in Contemporaneous and Cumulative AFDC-Based Medicaid Eligibility

Because Medicaid—through its categorical eligibility requirement—built on existing cash welfare systems, it inherited large, long-standing state-level differences in the size of these programs. Therefore, while all states experienced a sudden increase in public insurance eligibility when Medicaid began, this jump was much larger in states with higher welfare participation. Patterns of AFDC participation also differed strongly by race. For example, 1.3 percent of white children in Nevada were eligible through AFDC when the state's program began in July 1967, but 5 percent were eligible when New Mexico's program started in December 1966. For nonwhites, differences in initial eligibility are reversed: 22 percent in Nevada versus 10 percent in New Mexico. I therefore stratify the analysis by race to capture the distinct variation in white and nonwhite initial eligibility.

The sharp change in eligibility after Medicaid translates to a phasing in of cumulative eligibility across cohorts born in the years leading up to implementation. A white child born (and raised) in Nevada in 1950 had two childhood years under Medicaid and a 1.3 percent eligibility rate at age 17, and a 1.6 percent eligibility rate at age 18. Her expected number of years of *full* eligibility is 0.029.⁵ A similar child from New Mexico, however, had higher eligibility and three years of exposure, for 0.17 years of cumulative eligibility. For the 1960 cohort, born 10 years closer to the start of Medicaid, cumulative childhood eligibility is 0.47 for Nevada and 0.85 for New Mexico.

⁴ Appendices are available here: http://www-personal.umich.edu/~ajgb/medicaid_longrun_appendices_ajgb.pdf

⁵ All eligibility measures refer to the expected number of *full* years of Medicaid eligibility. Treating 1967 as a full year of implementation and assuming that the monthly AFDC participation rate in Nevada of 1.3 percent is constant, then the expected number of *months* of eligibility in 1967 (the interval at which AFDC eligibility is actually determined) is $12 \times 0.013 = 0.156$, which is the same as 0.013 full years of eligibility. Because of churning in AFDC caseloads, the expected number of years with *any* Medicaid eligibility is higher.

To construct a cohort-level measure of cumulative childhood Medicaid eligibility, I use state-by-year-by-race data on AFDC rates, statutory Medicaid implementation dates, and migration information in the Census. The number of expected years of childhood Medicaid coverage for a cohort born in state s in year c (of race r) is a weighted sum across the years of childhood *and* the states of residence (ℓ) of that cohort:

$$m_{rsc} = \sum_{y=c}^{y=c+18} \sum_{\ell} \sigma_{rsc}^y(\ell) \cdot 1\{y \geq t_{\ell}^*\} \cdot AFDC_{ry\ell} \quad (1)$$

$\sigma_{rsc}^y(\ell)$ is the share of cohort c (race r) born in state s living in state ℓ in year y .⁶ The Medicaid implementation dummy, $1\{y \geq t_{\ell}^*\}$, equals one if year y is after state ℓ 's Medicaid implementation date ($1966 \leq t_{\ell}^* \leq 1970$). $AFDC_{ry\ell}$ is the observed AFDC rate for children of race r in state ℓ in year y .⁷ Since 89 percent of Medicaid children qualified through AFDC, $1\{y \geq t_{\ell}^*\} \cdot AFDC_{ry\ell}$ is a close proxy for annual child Medicaid eligibility.

Differences in contemporaneous AFDC rates led to wide variation in cumulative childhood Medicaid eligibility. Figure 2 plots m_{rsc} for cohorts defined by year of birth relative to Medicaid implementation in their *birth* state. After Medicaid, white children gained about a year of eligibility on average and nonwhite children gained about five years (solid line), but differences across states in cumulative Medicaid eligibility are about as large as the average increases. Cross-state variation in AFDC also strongly predicts Medicaid *participation*: a one percentage point difference in AFDC rates when Medicaid began led to a 1.9 point increase (s.e. = 0.4) in annual child Medicaid utilization for at least six years after (Goodman-Bacon forthcoming, appendix figure 2B.4).

C. Expected Longer-Run Effects

A large body of evidence suggests that this rapid growth in insurance coverage and medical care use should affect health and economic outcomes later in life. Infant and child health are strongly correlated with test scores, education, labor supply, earnings, and welfare receipt in adulthood (Currie, Decker, and Lin 2008, Smith 2009). Early life exposure to specific infectious diseases negatively affects adult health, education, and earnings (influenza: Almond 2006,

⁶ Using the state of residence and 5-year migration variables in the 1970-2000 Censuses, I can calculate $\sigma_{rsc}^y(\ell)$ every five years starting in 1965. I linearly interpolate between these observations to obtain birth-state-by-birth-year-by-calendar-year estimates of the state of residence distribution.

⁷ Age-specific AFDC rates are not available at this time, but the 1970 Census shows that welfare participation rates are essentially constant during childhood. Details on the calculation of race-specific AFDC rates are in appendix 1.

malaria: Barreca 2010, pneumonia: Bhalotra and Venkataramani 2015, hookworm: Bleakley 2007, gastrointestinal disease: Chay, Guryan, and Mazumder 2009, 2014, meningitis: Roed et al. 2013, typhoid fever: Beach et al. 2014). A number of mechanisms could link child and adult health, including an inflammatory immune response (Crimmins and Finch 2006), a diversion of nutritional resources (Fogel 1997), reduced energy and school performance (Adhvaryu et al. 2015, Bleakley 2007), direct organ damage, or changes in parental investments (Becker and Tomes 1976). Evidence on Medicaid implementation, and the categorical eligibility requirement in particular, shows substantial short-run improvements in child health (Goodman-Bacon forthcoming). Medicaid reduced infant deaths through improved hospital care with no discernible effect on health at birth, and reduced deaths, mainly from treatable infectious diseases, among young children.⁸

Recent work uses both state-by-year variation and a birth date discontinuity in the 1980s eligibility expansions to estimate Medicaid's effects across the life course and finds striking improvements in health and economic outcomes. Childhood eligibility is associated with improvements in both teenage health (obesity, BMI: Cohodes et al. 2014, self-reported health: Currie, Decker, and Lin 2008, mortality: Wherry and Meyer 2013) and adult health (mortality: Brown, Kowalski, and Lurie 2014, obesity, BMI, chronic illness: Miller and Wherry 2014), and with reductions in adult hospitalizations for chronic disorders (Wherry and Meyer 2013, Wherry et al. 2015). Medicaid's long-run benefits extend beyond health to academic achievement (Levine and Schanzenbach 2009), educational attainment (Cohodes et al. 2014), and earnings (Brown, Kowalski, and Lurie 2014).

These results, however, may not provide a good guide to Medicaid's longer-run effects because the cohorts affected by the 1980s expansions are mainly observed during their 20s. The longer-run effects could grow if Medicaid reduces the lifetime incidence of chronic conditions, or could fade if Medicaid simply delays the age of onset.⁹ The only direct evidence on effects at older ages is mixed. Using the differential timing of Medicaid adoption across states, Boudreaux, Golberstein, and McAlpine (2016) find that, among adults who were poor in 1968, childhood

⁸ The lack of contemporaneous effects on health at birth rules out a fetal programming explanation for any long run effects. However, acute care at birth can, itself, improve later-life outcomes (Bharadwaj, Løken, and Neilson 2013).

⁹ The age profile of chronic illness suggests that Medicaid's effects could change drastically after age 30. National Health Interview Survey (NHIS) data show that chronic conditions such as hypertension, diabetes, cancer, and arthritis strike adults aged 30-64 more than five times as often as adults aged 19-30, the typical age range used in existing long-run studies (Minnesota Population Center and State Health Access Data Assistance Center 2012).

Medicaid exposure leads to higher scores on an index of adult health outcomes but not on an index of economic outcomes.¹⁰

In summary, the contemporaneous effects of Medicaid's introduction appear to have triggered life-course health processes that matter for adult outcomes in other contexts, including more recent Medicaid expansions. Medicaid's origin thus provides an opportunity to understand the program's longer-run effects in a way that has not previously been possible.

II. DATA: MEASURING ADULT OUTCOMES BY STATE AND YEAR OF BIRTH

I first examine an extreme but objective health outcome: cumulative mortality rates. I construct 20-year mortality rates conditional on living to 1980, by dividing the count of deaths from 1980 to 1999 from the Multiple Cause of Death Files (United States Department of Health and Human Services 2009) for a given race-by-birth-state-by-birth-year cell by the corresponding population estimate from the 1980 Census. Summing deaths over 20 years increases power relative to shorter time periods. Furthermore, race reporting among Hispanic Census respondents changed as the number of race categories grew, but this did not always occur on death certificates, which are filled out by medical examiners or funeral directors. Matching "white" and "nonwhite" deaths to similarly coarse denominators in 1980 avoids time-varying misclassification (Arias et al. 2008). For cohorts born close to Medicaid implementation, these mortality rates cover a range of ages between adolescence and mid-40s.

The primary mortality outcome is the log of 20-year mortality rates from non-AIDS causes. During the 1980s and 1990s, more adults aged 25-49 died of AIDS than any other cause, and the incidence of AIDS mortality across cohorts and states is correlated to some extent with childhood Medicaid exposure: it was highest for those born in the 1950s, fell strongly for those born in the 1960s (who survived to benefit from ARVs), and was concentrated among nonwhite men in New York and New Jersey, two relatively high-AFDC states.¹¹ I use cause-elimination

¹⁰ Furthermore, the structure of the 1980s expansions often makes it difficult to know why Medicaid affects shorter- and longer-run health and economic outcomes. While medical care use increased for pregnant mothers and children who gained new coverage (Currie and Gruber 1996a, b), it may have fallen among those who switched from private insurance to Medicaid (Currie and Gruber 2001). Crowd-out families also gained disposable income (Leininger, Levy, and Schanzenbach 2012) but faced incentives to draw down savings (Gruber and Yelowitz 1999). New Medicaid recipients were also adjunctively eligible for food benefits (Bitler and Currie 2004) and, in some cases, gained Medicaid coverage as a consequence of expansions in cash welfare eligibility. Both of these programs have been shown to have longer-run effects (Aizer et al. 2014, Hoynes, Schanzenbach, and Almond 2012).

¹¹ Empirically, the inclusion of New York and New Jersey is the primary reason why AIDS affects the estimates. Dropping these two states yields overall mortality results similar to those reported below and also yields null results on AIDS-related deaths.

life table methods (Manton and Stallard 1984) to construct cumulative mortality rates that reflect the force of non-AIDS mortality and not the effect of AIDS on the size of the population at-risk.¹² I construct other cause-specific mortality rates in the same way.

My second group of health outcomes is the set of disability variables in the 2000-2014 Census and American Community Survey (ACS). These are commonly used to measure health (Bound et al. 2003), and are extremely relevant to labor market outcomes. The types of disability are hearing or vision problems; difficulty with activities such as walking or carrying (ambulatory difficulty); difficulty going outside the home (mobility difficulty); difficulty getting around inside the home (self-care difficulty); difficulty learning, remembering, or concentrating (cognitive difficulty); and difficulty working at a job or business (work limitation). Because of changes in the question text, this analysis uses native-born respondents ages 25 to 64 born before 1976 in the 2000-2007 Censuses and ACS (Ruggles et al. 2010) collapsed to averages by race (r ; white and nonwhite), birth state (s), and birth cohort (c).

I use the 2000-2014 Census and ACS to calculate rates of labor force participation, employment, cash transfer or public insurance receipt, average income by source, and the distribution of such income by source.

III. RESEARCH DESIGN: MEDICAID IMPLEMENTATION, CATEGORICAL ELIGIBILITY, AND CUMULATIVE ELIGIBILITY ACROSS STATES AND COHORTS

Cumulative Medicaid eligibility, m_{rsc} , may be spuriously correlated with adult outcomes if, for example, migration sorts healthier children to higher- or lower-eligibility states, or if changes in AFDC rates reflect demographic, economic, or policy conditions. To address these potential biases, I adapt the difference-in-differences strategy in Goodman-Bacon (forthcoming) and compare changes in outcomes across cohorts born in states with different child AFDC rates in the year of Medicaid implementation. This initial categorical eligibility rate, denoted $AFDC_{rs}^*$, provides a fixed ranking of states by which to compare adult outcomes and avoids comparisons between earlier and later Medicaid-adopting states, which differed on a range of characteristics.

¹² Let the 1980 population of cohort c from state s be $POP_{cs,1980}$, and denote annual AIDS-related deaths by $d_{cs,y}^{AIDS}$, and non-AIDS-related deaths by $d_{cs,y}^{OTH}$. It is straightforward to calculate cause-specific mortality rates in 1980 as $mr_{cs,1980}^{AIDS} = \frac{d_{cs,1980}^{AIDS}}{POP_{cs,1980}}$, and $mr_{cs,1980}^{OTH} = \frac{d_{cs,1980}^{OTH}}{POP_{cs,1980}}$. Subsequent mortality rates can be calculated similarly using annual deaths in the numerators and the surviving cohort population, $POP_{cs,1980} - \sum_{j=1980}^y (d_{cs,j}^{AIDS} + d_{cs,j}^{OTH})$, in the denominator. If period mortality rates would have been the same in the absence of other causes (ie. independent risks), then an estimate of the cause-elimination mortality rate from cause k is $1 - \prod_{j=1980}^{1999} (1 - mr_{cs,j}^k)$.

A. Evidence on the Validity of the Initial-AFDC Research Design

For comparisons based on initial AFDC rates to generate consistent estimates of Medicaid's effect, $AFDC_{rs}^*$ must predict cumulative eligibility (relevance) and be uncorrelated with other determinants of cross-cohort changes in adult outcomes (excludability). Figure 2 provides crude evidence on the strength of $AFDC_{rs}^*$ for predicting cumulative eligibility. The dashed lines plot average cumulative eligibility in states with above- and below-median values of $AFDC_{rs}^*$. The average difference across the state groups for fully treated cohorts is 0.47 years for white children (s.e. = 0.03) and 1.12 years for nonwhite children (s.e. = 0.13).

As to the second assumption, initial AFDC rates are plausibly excludable instruments because cross-state variation in welfare participation arose largely because of historical institutional factors unrelated to the circumstances facing cohorts first treated by Medicaid (Alston and Ferrie 1985, Bell 1965, Moehling 2007). To construct a high-powered test of the validity of the research design, I follow Kling, Liebman, and Katz (2007) and examine two indices of potential confounders. One combines child health measures using annual vital statistics data from 1950-1965, and the other combines childhood socioeconomic (SES) measures using data from the 1950-1970 Censuses. The components are coded to represent "good" outcomes, standardized using the mean and standard deviation from 1950, and averaged together with equal weight. I estimate the "shape" of any pre-Medicaid trends non-parametrically by regressing each index on year dummies interacted with $AFDC_{rs}^*$, and then regress the index on a linear trend interacted with $AFDC_{rs}^*$ to increase the power of the test to reject the null of no differential trends (as long as such trends are roughly linear). I also pool all years and test for cross-sectional balance in the level of each index. This avoids a series of potentially noisy balance tests that may produce a small number of significant correlations due to multiple testing.

Figure 3 plots the results and shows little evidence that changing circumstances faced by infants and children in the 1950s and 1960s are correlated with initial AFDC rates. Neither trends nor levels in the health or SES indices differ significantly for whites. The white health index in states that were 5 percentage points apart in $AFDC_{rs}^*$ would only have diverged by 0.17 standard deviations over 15 years ($0.0022 \times 5 \times 15 = 0.165$). The differential pre-trends and level differences in the nonwhite health index are smaller by an order of magnitude. $AFDC_{rs}^*$ has a small correlation with levels and trends in nonwhite SES. Still, the magnitudes are small: the nonwhite SES index in states that were 8 percentage points apart in $AFDC_{rs}^*$ (the standard deviation of

nonwhite AFDC rates) would have diverged by just 0.25 standard deviations over 15 years ($0.0021 \times 8 \times 15 = 0.25$).

Appendix table A3.3 presents the results from three additional index-based balance tests. The first uses newly collected data on the incidence and eradication of polio (from the March of Dimes Archives) to show that one of the signature public health achievements of the 20th century, the dissemination of the Salk polio vaccine between 1954 and 1957, was uncorrelated with initial AFDC rates. The second test uses the 1963 Survey of Health Services Utilization and Expenditures to show that parents' self-reported willingness to seek care and views on the value of medical care are also uncorrelated with initial AFDC rates. Finally, I use data on the quality of dwellings and appliance ownership first available in the 1960 Census to show that home quality and durable goods consumption are also orthogonal to initial AFDC rates.

B. Event-Study Specification

A more direct test of the research design comes from reduced-form event-study models that trace out the relationship between adult outcomes and $AFDC_{rs}^*$ for cohorts born in different years relative to Medicaid ("event-cohorts"). The estimating equation for outcome Y is:

$$Y_{rsc} = \mathbf{X}'_{rsc} \boldsymbol{\beta} + AFDC_{rs}^* \left[\sum_{j=-(a+1)}^{-20} \pi_j 1\{c - t_s^* = j\} + \sum_{j=-18}^{b+1} \phi_j 1\{c - t_s^* = j\} \right] + \varepsilon_{rsc} \quad (2)$$

My preferred specification of $\mathbf{X}'_{rsc}$ includes fixed effects for state, cohort, and age; region-by-cohort fixed effects, to account for convergence in outcomes across U.S. regions unrelated to Medicaid (Chay, Guryan, and Mazumder 2009, Stephens and Yang 2013); and Medicaid-year-by-cohort fixed effects, to eliminate comparisons between earlier and later Medicaid-adopting states, which were on different trajectories both in terms of socioeconomic and health outcomes before Medicaid.¹³ I also include the general fertility and infant mortality rates, per-capita income in each cohort's birth year, and the average number of hospital beds per-capita in each cohort's first 12 years. Identification in equation (2) comes from comparisons of respondents born in the same region in the same event-time across values of $AFDC_{rs}^*$. I cluster standard errors by birth state (48 clusters), and for key coefficients, I present p -values from a wild-cluster percentile- t bootstrap.

¹³ Between 1950 and 1970, for example, white child poverty fell by about 21 percent in states that implemented Medicaid before 1969, but by 33 percent in states that implemented in 1969 or 1970 (s.e. of the difference is 2.3).

The coefficients of interest, π_j and ϕ_j , trace out changes in the relationship between $AFDC_{rs}^*$ and y_{sct} across event-cohorts relative to the omitted group, $j = -19$.¹⁴ The π_j are falsification tests. Cohorts born more than 18 years before the introduction of Medicaid had no childhood coverage, so changes in their outcomes should not be related to initial Medicaid eligibility.¹⁵ The ϕ_j are intention-to-treat (ITT) effects that measure the relationship between an additional percentage point of initial eligibility and changes in outcomes for cohorts first exposed $-j$ years after birth, i.e., at age $\max\{0, t_s^* - c\}$. Because exposed cohorts are treated from age $\max\{0, t_s^* - c\}$ to 18, each ϕ_j is analogous to a distinct experiment in which the Medicaid dose differs by $AFDC_{rs}^* \cdot (19 - \max\{0, t_s^* - c\})$ and coverage begins at age $\max\{0, t_s^* - c\}$.

These results can be used to test several features of both the treatment effects and the design. First, the ϕ_j will be zero if Medicaid has no effect when received at age $\max\{0, t_s^* - c\}$ and older. The semiparametric event-study specification makes no assumptions about *when* coverage begins to matter, a salient issue in the literature on long-run effects. The ϕ_j do not separately identify heterogeneous effects by age at exposure versus amount of exposure, though, because cohorts who were young when Medicaid was passed also had more coverage. Conclusions about age versus amount of coverage require additional assumptions. Second, the pattern of the ϕ_j around $t_s^* = c$ (i.e., $j = 0$) provides an additional test of the design because all cohorts born after Medicaid have the same age (0) and amount of exposure (19 years). The ϕ_j should be similar for $j \geq 0$ if the effects are due to Medicaid because the “experiment” is the same for these cohorts.

C. Instrumental Variables Specification

To express the effects in terms of years of childhood eligibility, I estimate instrumental variables models that use the predicted “dose” described above as an instrument for actual cumulative eligibility m_{rsc} , defined in equation (1). The instrument, z_{rsc} , eliminates variation from annual changes in AFDC rates and migration and generates a predicted cumulative eligibility based only on initial AFDC rates in each cohort’s birth state:

$$z_{rsc} = \sum_{y=c}^{c+18} 1\{y \geq t_s^*\} \cdot AFDC_{rs}^* = AFDC_{rs}^* \cdot (19 - \max\{0, t_s^* - c\}) \quad (3)$$

¹⁴ I report coefficients for event-cohorts born between a years prior to and b years after Medicaid implementation. Cohorts born outside the event window $[-a, b]$ are grouped into (unreported) terms for $-(a + 1)$ and $(b + 1)$.

¹⁵ Members of these cohorts could still have qualified for Medicaid as public assistance recipients or through Medically Needy provisions, but survey data show a sharp drop in Medicaid use and eligibility after age 18.

IV estimates that use z_{rsc} as instruments for m_{rsc} estimate the average ITT effect of an additional year of cohort-level cumulative eligibility across ages of exposure. Splitting the eligibility variables by sub-periods of childhood allows a test of whether the average effect per year of eligibility differs by age at exposure.

IV. INTENTION-TO-TREAT EFFECTS OF MEDICAID ON ADULT HEALTH

Figure 4 plots first-stage event-study estimates of equation (2) that measure the relationship between $AFDC_{rs}^*$ and cross-cohort changes in migration-adjusted cumulative Medicaid eligibility, m_{rsc} . The coefficients for event-times -30 through -20 are small by construction: there is no relationship between $AFDC_{rs}^*$ and cumulative eligibility for cohorts with *no* childhood Medicaid exposure. The positive and increasing coefficients for event times -18 through 0 show that even after incorporating childhood migration, cohorts born in states with higher initial eligibility accumulate more childhood eligibility per year of exposure to any Medicaid program. The slope is twice as steep for whites as for nonwhites (0.007 versus 0.003), and column 1 of table 1 presents first-stage coefficients on z_{rsc} that confirm this difference. Initial eligibility strongly predicts white cumulative eligibility (0.72, s.e. = 0.16), but is only weakly related to nonwhite cumulative eligibility (0.20, s.e. = 0.18). The coefficients for event times 1 through 5 flatten out (whites) or erode (nonwhites), which underscores the earlier claim that the “dose” of childhood Medicaid exposure is the same for cohorts born after implementation. If Medicaid has long-run effects, then event-study estimates for other outcomes should have this pattern as well.

A. Cumulative Mortality, 1980-2000

The event-study estimates in figure 5 show that Medicaid reduces later-life mortality.¹⁶ The white point estimates are small for cohorts with no childhood Medicaid exposure—a key test of the validity of the design. They are also small for cohorts that were only eligible in their teenage years, suggesting that later childhood eligibility has no effect on subsequent mortality. The coefficient on the interaction between $AFDC_{rs}^*$ and a linear event-time variable between -30 and -11 is small and insignificant (-0.02, s.e. = 0.02), implying that mortality would only differ by about 0.4 percent between cohorts born 20 years apart in states with a one percentage point difference in initial AFDC rates ($1 \times -0.02 \times 20$). The estimates are negative and growing for white

¹⁶ The dataset includes cohorts born as early as 1936—aged 64 in 2000 and born 30 years before the earliest Medicaid implementation date—and as late as 1975—aged 25 in 2000 and born five years after the latest Medicaid implementation year. This fixes the event-times at which I observe cohorts from all states: [-30,5].

cohorts with increasing exposure under age 10 (the linear trend break is -0.15, s.e. = 0.07), and they flatten out for those with full childhood exposure (0.23, s.e. = 0.11). Both features match the shape of the first stage and support the AFDC-based research design. The nonwhite trend-break estimates are similar. Results from the same specification used for whites show that these breaks are relative to an upward trend in mortality for the oldest cohorts (0.07, s.e. = 0.03). To address this, the nonwhite results plotted in figure 5 come from a two-step procedure in which I estimate the linear pre-trend on data through event-time -9 (the best-fitting trend break point), remove it from the full dataset, and estimate equation (2) on the adjusted data.¹⁷

To highlight the magnitude of these results, table 2 presents IV estimates of the effect of childhood eligibility separately for ages 0-10 and 11-18.¹⁸ Column 1 shows the effects for all-cause mortality rates, and column 2 shows results for non-AIDS mortality that correspond to figure 5. The effects of early childhood eligibility are qualitatively similar for both outcomes but are about two-thirds as large for non-AIDS mortality and, for white adults, significantly more precise. Each year of childhood Medicaid eligibility is associated with a reduction in adult mortality of between 15 and 20 log points.

What size effects among treated adults are required to rationalize these ITT effects? In terms of a proportional reduction per year of coverage, this will be larger than the ITT since this population segment must account for *all* the averted deaths but may be smaller to the extent that adults with childhood Medicaid eligibility have more AFDC exposure (by definition) and higher baseline mortality. Appendix 8 shows how to use post-treatment data on mortality differences by poverty status (available in the National Longitudinal Mortality Study) and on childhood AFDC receipt to infer the proportional treatment effect on the treated that is consistent with the ITT in table 2. A year of childhood coverage reduces cumulative non-AIDS-related mortality rates by 8 percent among treated white adults and 10 percent among treated nonwhite adults.

¹⁷ A common strategy to deal with trends in a difference-in-differences design is to include unit-specific linear time trends. The event-study figures clearly show time-varying treatment effects, however, in which case unit-specific trends cannot distinguish between treatment effects and pre-existing trends (Lee and Solon 2011). My two-step procedure has no effect on the estimated trend *breaks*. It only alters the orientation of the event-study coefficients and the resulting IV estimates. I apply a degrees-of-freedom correction to the second-step standard errors to account for the (one) regressor estimated in step one.

¹⁸ First-stage estimates for age-specific eligibility measures are in table 1, and age-group-specific first-stage event-study results are in appendix figure 2.1. The nonwhite age-specific first-stage estimates are stronger than the overall first stage but still reflect the previously mentioned changes in nonwhite AFDC rates. For instance, predicted eligibility under age 10 is *negatively* associated with actual eligibility between ages 11 and 18. Being born in a low-AFDC state in the 1960s means that nonwhites have low early childhood eligibility but relatively higher later childhood eligibility, which induces the negative correlation.

These reductions are larger than the other estimates of childhood Medicaid coverage on adult mortality. Wherry and Meyer (2013) find an ITT effect per year of eligibility of about -0.375 internal-cause deaths per 10,000 for black teens 15-18, which translates to a 6 percent decline in annual teenage internal-cause mortality per year of eligibility among the treated. The comparable ATET in Brown, Kowalski, and Lurie (2014) is about a 1 percent reduction in mortality between ages 18 and 27. These differences make sense. Both recent studies focus on eligibility later in childhood, and the treated subgroups, particularly in Brown, Kowalski, and Lurie's (2014) setting, which includes eligibility for non-poor children, whose families have higher incomes than those of categorically eligible children and who may benefit less. Finally, the 1980s expansions led to higher levels of childhood eligibility than those under initial implementation. A typical Roy pattern of selection implies the highest marginal treatment effects at lower levels of participation (Heckman and Vytlacil 2005).

The rest of table 2 presents estimates by cause of death. Columns 3 through 7 show that Medicaid's effects are strongest among the most plausibly affected conditions—internal causes—but that Medicaid worked through a range of conditions within this group. The internal cause estimates (column 3) are noticeably larger than the estimates for either non-AIDS-related causes or external-causes (column 7). The leading internal causes of death among adults other than AIDS were cardiovascular disease and cancer. Cardiovascular disease plays a more important role for nonwhite mortality than white mortality, consistent with its higher incidence among nonwhite adults. Both groups have similar cancer effects—a rare finding in the literature on life-course health. Behavioral changes could underlie this link, as could changes in the infectious origin of certain cancers. For instance, the bacterium *h. pylori* is the primary cause of stomach cancer, and human papillomavirus causes cervical cancer. Medicaid coverage may have altered the incidence of such conditions.

Strikingly, column 6 shows large reductions in suicide (-39.7, s.e. = 8.4 for whites, -39.3, s.e. = 15.9 for nonwhites), although suicides account for a small share of the full effect. This result is consistent both with reductions in the burden of chronic illness as discussed in Case and Deaton (2015) and with Medicaid's positive effects on contemporaneous and later-life mental health (Finkelstein et al. 2012, Miller and Wherry 2014), both of which may reduce suicides.

These results suggest that about 345,000 lives were saved between 1980 and 1999—54,000 among whites and 291,000 among nonwhites. Even assuming a relatively low value of statistical

life of \$845,000 (the lower end of the confidence interval of Ashenfelter and Greenstone's (2004) estimates [table 2, converted to 2012 dollars]), this suggests that Medicaid's longer-run mortality reductions are worth at least \$291 billion.

B. Self-Reported Disability

Results using an independent measure of health, self-reported disability, affirm that childhood Medicaid eligibility improves adult health. Figure 6 plots event-study estimates for the most common self-reported disability in the Census: difficulties with activities like walking, climbing stairs, reaching, lifting, or carrying (ambulatory difficulty).¹⁹ The point estimates (multiplied by 100 so that the y-axis measures fractions of a percentage point) match the first-stage and mortality results closely. They are small for cohorts with no childhood Medicaid exposure, as well as for whites in cohorts first exposed after age 10 (the linear trend from -23 to -11 is -0.007, s.e. = 0.008) and nonwhites in cohorts exposed after age 4 (pre-trend = -0.002, s.e. = 0.003).²⁰ There are clear negative trend breaks after these ages (white: -0.021, s.e. = 0.01; nonwhite: -0.014, s.e. = 0.01). Crucially, the trends reverse for cohorts with full childhood exposure (i.e., born after Medicaid implementation). A test that these coefficients are equal, a direct prediction of the design, yields *p*-values of 0.99 (whites) and 0.27 (nonwhites).

Table 3 presents IV estimates for ambulatory difficulty across specifications, using the trend breaks in figure 6 to break up eligibility ages at age 10 for whites and age 4 for nonwhites. The model in column 1 includes only event-time dummies and $AFDC_{rs}^*$, and the effects of early childhood eligibility are negative and imprecise. My preferred specification (column 2) shows that each full year of early-life cumulative eligibility reduces white ambulatory difficulty rates by 3.84 percentage points (s.e. = 1.15) and nonwhite rates by 2.92 percentage points (s.e. = 1.81). Column 3 shows that the white result is not sensitive to population weighting (Solon, Haider, and

¹⁹ Because of changes in the text of the disability questions, these results use data from 2000-2007 only. This determines the event-time window because 64 year olds in 2007 were born 23 years before the earliest Medicaid implementation year (1966), and 25 year olds in 2000 were born five years after the latest Medicaid implementation year in the sample (1970). The results are not sensitive to including all survey years or to collapsing across survey years to the state-by-cohort level.

²⁰ The trend break estimates listed in the figure come from fitting linear trends with three sections. The pre-trend goes through zero at time -19 (like the event-study estimates), the phase-in trend begins somewhere between time -19 and -1, and the post-trend begins at zero, when all cohorts have full childhood exposure. I present estimates that maximize the *F*-statistic on the trend terms, thus remaining agnostic about the age at which exposure begins to have longer-run effects. *F*-statistics are plotted in appendix figure 2.4. As in figure 5, the nonwhite results are from a two-step procedure that adjusts the data by removing a positive pre-trend in disability rates between event-cohorts -23 and -4. This does not affect the estimated trend breaks, just the orientation of the coefficients.

Wooldridge 2015). The magnitudes fall only slightly with the inclusion of state-specific linear cohort trends (column 4), although the pattern of effects suggests that trends are not an appropriate control (Lee and Solon 2011).

The last two columns address the concern that characteristics of respondent's *current* state confound comparisons based on state of birth. Column 5 expands the dataset by state of residence and includes cohort-by-state-of-residence fixed effects. This controls non-parametrically for factors that vary across cohorts and states of *residence* (strongly correlated with birth state) such as age-varying effects of state policies, trends in chronic pain and opioid abuse (Case and Deaton 2015), AIDS incidence, and adult migration. Identification comes from changes across cohorts in the relationship between disability and $AFDC_{rs}^*$ for respondents who *live* in the same state. Column 6 further expands the dataset by survey year and controls for interactions between cohort and state-by-year unemployment rates to test whether self-reported disability is a response to labor demand conditions (Charles and Decicca 2008). Neither specification alters the conclusion that early Medicaid exposure reduces disability.

The effects of early Medicaid eligibility for white adults extend to all disability measures (panel A of table 4), pointing to broad improvements in functional capacity. Perhaps as a consequence of lower incidence of the relatively severe limitations, appendix figure 3.4 also shows a suggestive negative effect of early Medicaid coverage on the probability of living in group quarters (-0.38, s.e. = 0.22). The effects across types of disabilities are consistent with general improvements in health due to Medicaid but also underscore the difficulty in uncovering Medicaid's specific physiological channels.

Nonwhite results only appear for ambulatory difficulty, despite the fact that Medicaid's contemporaneous effects on child health were strongest and most precise for nonwhite children. The differences in short- and long-run effects may relate to aspects of Medicaid utilization other than crude eligibility rates. Even among those eligible for Medicaid in the 1960s, white children were 17 percentage points more likely to use medical care in a year than nonwhite children (65 versus 48 percent; Loewenstein 1971 table 2.1), and they saw private providers twice as often as nonwhite children (80 versus to 43 percent for most recent site of care; Loewenstein 1971 tables 2.45, 2.46, and 5.15).²¹ In the short run, it may have been easy for simple medical care to save

²¹ This matches direct reports about provider availability/access. When asked "Do you think that people who are eligible to get free medical care through their local welfare departments must go to certain places or can they go

nonwhite children's lives. Differences in *types* of care received by nonwhite kids may limit their long-run health benefits. Alternatively, because nonwhite adults experience higher disability and mortality rates than white adults, even if Medicaid's effects are similar by race, competing risks may work against observing this effect in the population (Freedman and Spillman 2016).

How big are these effects relative to counterfactual disability rates among treated children? Unlike mortality, disability rates for adults with child welfare receipt are observable in the PSID (University of Michigan Survey Research Center 2016). Forty-one percent of white children whose families received AFDC income in 1968 reported a work limitation as adults in 2001, compared to 15 percent on average. This implies that the actual rate of ambulatory difficulty among the treated subset is $(5.7 \times 41/15)$ 15.6 percent.²² This number *includes* Medicaid's treatment effect, however. Analyses of PSID data (Smith and Yeung 1998) and administrative data on welfare spells (Berger and Black 1998) suggest that the average white child with any childhood AFDC spent about two full years on the program by age 10 (likely spread out over multiple calendar years). Therefore, the results in table 3 suggest that their adult disability rates are lower by (2×3.84) 7.7 percentage points. Adding this effect implies a counterfactual rate of ambulatory difficulty among the treated of 23 percent $(5.7 \times 41/15 + 3.84 \times 2)$ and a proportional reduction in ambulatory difficulty per year of Medicaid eligibility among treated children of 16.5 percent. Similar calculations for nonwhite adults imply a reduction in adult disability of 13 percent per year of childhood coverage.

These results suggest large improvements in quality of life in response to Medicaid. Activity limitations are fundamental to many concepts of well-being itself (Sen 1993) and are closely related to self-reported happiness and satisfaction.²³ Furthermore, the disability and mortality results reinforce each other, since deaths from potentially disabling conditions fall and since reductions in disability may feed back to a reduction in suicide (Giannini et al. 2010).

anywhere?" 61 percent of white categorically eligible household heads reported that they could go "anywhere," compared to only 46 percent of nonwhite household heads. White categorically eligible families were also twice as likely as nonwhite families to have switched providers in the previous two years (Loewenstein 1971). (There was no racial difference in recent provider switching among poor families in states that had not implemented Medicaid by 1968.)

²² This calculation uses the ambulatory difficulty rate among those born from 1955 to 1975: 5.7 percent. The implied rate among the treated (15.6 percent) is actually lower than the rate among poor adults in the Census, 17.7 percent.

²³ The 2001 National Health Interview Survey (MPC and SHADAC 2012) shows that 21 percent of poor non-elderly adults with an activity limitation report being happy "a little" or "none" of the time. The figure for poor adults with no limitations is 8.6 percent and for non-poor adults with limitations is 12.7 percent.

C. Selective Survival

The preceding evidence suggests that Medicaid's effect on disability could be biased by changes in the composition of survivors. If Medicaid saved the lives of those who ultimately became disabled, then the estimates in tables 3 and 4 will understate Medicaid's effect on disability. The observed disability rate (dr_{rsc}) is the average of the rates among those who were saved by Medicaid (dr_{rsc}^1) and those who would always have survived (dr_{rsc}^0):

$$dr_{rsc} = (1 - p_{rsc}) \cdot dr_{rsc}^0 + p_{rsc} \cdot dr_{rsc}^1 \quad (4)$$

p_{rsc} is the share of each cohort that survived to appear in the Census/ACS data because of Medicaid. I use the contemporaneous infant and child mortality estimates in Goodman-Bacon (forthcoming) and the cumulative adult mortality estimates from table 2 to construct true and counterfactual probabilities of surviving to the year 2000 and calculate p_{rsc} as their difference. Then, since dr_{rsc}^1 lies between 0 and 1, I bound the treatment effect on dr_{rsc}^0 using different assumptions about dr_{rsc}^1 , similar to Bharadwaj, Løken, and Neilson (2013, table 4).

Row 1 of table 5 shows that selective survival plays almost no role in the treatment effects for white cohorts. Estimates differ by less than 5 percent across almost the full range of assumptions about dr_{rsc}^1 . Rows 2-4 show that selective survival could explain much of the difference by race in the average effects under age 10. For low values of dr_{rsc}^1 , the nonwhite effects are about half as large as the white effects (although not statistically distinguishable), and this difference falls to three quarters for the highest values of dr_{rsc}^1 (50 or 80 percent). Rows 3 and 4, however, show selective survival does not explain why the nonwhite effects are concentrated under age 4 instead of age 10. The survival adjustments magnify the ages 0-4 effect (it exceeds the white estimates for values of dr_{rsc}^1 of 30 percent or more) but never lead to economically or statistically significant effects for ages 5-10.

V. INTENTION-TO-TREAT EFFECTS OF MEDICAID ON ADULT LABOR MARKET OUTCOMES

The results in section IV imply that childhood Medicaid eligibility induces substantial improvements in adults' physical health. This section examines how these health improvements affect labor supply and income as well as public costs and revenues.

A. Labor Supply and Transfer Program Participation

Both event-study and IV estimates show that the positive health effects of childhood Medicaid eligibility translate into increased extensive margin labor supply and reduced transfer

program participation for whites. The series with closed triangles in figure 7 plots event-study estimates of Medicaid’s effect on annual employment. In line with all the other results, the trends for cohorts exposed to Medicaid after age 10 are flat (-0.002 , $s.e. = 0.011$), and there is a clear trend break for the same cohorts that experienced health improvements (0.048 , $s.e. = 0.015$), which is largely eliminated for groups with full childhood exposure (-0.035 , $s.e. = 0.016$). IV estimates (table 6) show that each year of childhood Medicaid eligibility reduces the probability of being out the labor force by 6.59 percentage points ($s.e. = 1.49$) and increases employment (current: 5.82 , $s.e.=1.18$; annual: 6.33 , $s.e. = 1.37$), most of which is full-time/full-year (4.72 , $s.e. = 0.75$). The 2001 PSID shows relatively small differences in employment between white adults with and without childhood AFDC receipt, which suggests that the implied 13-point increase in employment among treated children represents an 18 percent increase over their counterfactual employment rate of 72 percent (based on the employment rate among whites born from 1955-1975, 85 percent).²⁴

Medicaid’s effects on disability benefit receipt (Social Security Disability Insurance [SSDI] or Supplemental Security Income [SSI]; the series with open squares) are almost the mirror image of the labor supply effects and track the disability results closely. The pre-trend is small and insignificant (-0.012 , $s.e. = 0.012$), there is a negative trend break for cohorts exposed at age 10 or younger (-0.022 , $s.e. = 0.015$) and a positive one for cohorts with full exposure (0.036 , $s.e. = 0.007$). The corresponding IV estimate in table 7 shows a reduction in disability transfer participation of -5.88 percentage points ($s.e. = 1.54$). Other welfare receipt (mostly Temporary Assistance for Needy Families, TANF) actually rises slightly (0.83 , $s.e. = 0.14$). Disability benefits are higher than TANF benefits, meaning that, abstracting from non-negligible application costs, people who qualify for both prefer SSI/SSDI. Health improvements that disqualify households for disability benefits may simply lead some to take up TANF.²⁵

²⁴ Effects for nonwhite labor supply have similar signs, but, consistent with their smaller disability results, the magnitudes are much smaller and none are statistically significant. The rest of this section presents and discusses results for white adults.

²⁵ The appendices provide additional evidence on the validity of the design using employment and public assistance data from 1970, 1980, and 1990. These Censuses contain labor supply and program participation measures for much older cohorts during prime working years. I use these data to conduct two related falsification tests. Figure 4.8 uses the 1980 and 1990 Censuses to extend figure 7’s pre-period to 45 years. There is no relationship between $AFDC_{rs}^*$ and employment or public assistance trends even for cohorts born in the 1920s. Figure 2.4 shifts event-time back for cohorts observed in the 1970 and 1980 Censuses and estimates “false” event-studies across the same *ages* used in the main analysis but in much earlier survey years. There is no evidence that age/employment or age/public-assistance patterns were correlated with $AFDC_{rs}^*$ for untreated cohorts.

The connection between cash and in-kind benefits means that Medicaid also has important intertemporal effects within the public insurance system. Column 4 of table 7 shows that cohorts with higher Medicaid eligibility in early childhood use public insurance *less* often as adults (-4.19, s.e. = 0.98).²⁶ The reduction in public coverage is largely offset by private insurance because these adults work more, but since about three quarters of these jobs are full time, total insurance coverage falls slightly (and insignificantly: -1.16, s.e. = 0.70).

These results speak directly to empirical analyses of disability insurance and benefit programs. Recent research uses random assignment of disability applications across evaluators with different award rates to show that, *holding health constant*, disability benefits reduce labor supply (French and Song 2014, Maestas, Mullen, and Strand 2013). Another approach decomposes time-series changes in disability receipt into its components *holding nothing constant*, and concludes that health improvements have had little impact on the rolls (Autor and Duggan 2006, Duggan and Imberman 2009). Reform proposals therefore emphasize ways to improve medical reviews, tighten eligibility criteria, smooth out the benefit structure (Autor and Duggan 2006), or increase administrative capacity (Liebman 2015). The results in tables 6 and 7, on the other hand, show that *holding program incentives constant* (through the cross-state comparisons), improvements in health greatly reduce disability benefit receipt and increase labor supply.²⁷ Multiplying the effect per year of eligibility (-5.88) by the population in affected cohorts (56 million whites with early childhood eligibility) and the average eligibility under age 10 (0.37 years) suggests that there are 1.2 million fewer SSI/SSDI recipients because of Medicaid implementation—about 15 percent of the average number of white, non-elderly recipients between 2000 and 2014.²⁸

²⁶ This effect comes both from Medicaid, for which almost all SSI recipients are categorically eligible, and Medicare, which SSDI recipients can receive after a two-year waiting period. ACS data show that among non-elderly SSI recipients, 94% have Medicaid and 33% have Medicare, while among SSDI recipients, 33% have Medicaid and 44% have Medicare.

²⁷ That the effect of early Medicaid coverage on employment (6.33) is larger than its effect on disability assistance (-5.88) supports the claim that improved adult health is the main causal channel because even rejected disability applicants are quite unhealthy and work at low levels (Bound 1989). Underlying improvements in activity limitations would, therefore, tend to increase labor supply among *both* recipients and non-recipients of SSI/SSDI.

²⁸ These effects appear to get slightly stronger at older ages. Appendix figure 4.9 plots separate IV estimates for annual employment and disability benefit receipt for each of the 15 survey years. The design is unchanged, but the results describe effects for cohorts with early childhood coverage between their mid-30s and late-40s. For both outcomes, the estimate effects are larger when the relevant cohorts are older.

B. Sources of Income

Increases in labor supply and reductions in transfer program receipt offset each other in terms of income. Figure 8 plots a series of IV coefficients for eligibility between ages 0 and 10 for dependent variables defined as the probability of reporting earnings, transfer income, or total income greater than or equal to x . When $x = 0$, for example, the earnings coefficient measures the probability of any earnings—i.e., annual employment—and the transfer coefficient measures the probability of any transfer income—i.e., public assistance participation. As x moves up, the results trace out Medicaid’s effect on the distribution of income by source.

The distributional analysis provides two important pieces of information about Medicaid’s effect. First, the earnings effect is concentrated in the lower part of the distribution. Because income mobility for these cohorts is low (Chetty et al. 2014, Lee and Solon 2009), this lends further support to the claim that the effects are due to Medicaid’s treatment of poor children. Second, the figure shows that increased earnings offset reduced transfer income. The positive earnings coefficients are larger than the negative transfer income coefficients, but the estimates for total income are insignificant. The effects for earnings between \$20,000 (the 99th percentile of transfer income in 2014) and \$55,000 (close to the mean for employed whites in 2014) remain positive and marginally significant, suggesting some effect on middle-class jobs.

Table 8 quantifies Medicaid’s effect on average income by source.²⁹ Columns 1 and 2 show that average earned income increases, but the full-sample estimate is quite noisy. Trimming earnings above \$100,000 (as in Chetty et al. 2011) changes the point estimate by less than 3 percent but cuts the standard error by a factor of almost four, leading to the expected significant increase in earnings: \$1,810 (s.e. = 617). Transfer income falls by \$590 (s.e. = 162), and total income is higher (\$609), but not by a significant amount (s.e. = 654). Medicaid does not significantly reduce adult poverty rates (column 5) because it largely affects the *composition* of income rather than the amount.

C. Human Capital

Long-run research based on the 1980s expansions finds increases in educational attainment (Brown, Kowalski, and Lurie 2014, Cohodes et al. 2014), but table 9 shows no evidence that

²⁹ Because the effects in figure 9 are essentially differences in (one minus) the CDFs of non-negative random variables, their integral approximates Medicaid’s effect on the mean of each income source. Summing each point times \$2,000 (the bin width) yields estimates very close to those in table 7: \$2,359 increase in earnings, \$1,092 increase in total income, and \$600 reduction in transfer income.

Medicaid implementation affected high school completion, college attendance, or college graduation probabilities. Multiplying the upper end of these confidence intervals by average early childhood eligibility (0.37) rules out cohort-level effects larger than 1.3 percent for high school graduation, 3.2 percent for college attendance, and 1.6 percent for college graduation.

While the null result on education differs from related research, human capital is not the most likely mechanism for the extensive margin labor supply effects documented above. Long-run health studies that link earnings to improved education tend to work through higher wages rather than extensive margin labor supply. Bhalotra and Venkataramani (2015), for example, find that the introduction of sulfa drugs increased income and education among white men but failed to affect employment rates (table 6, column 3). Similarly Southern black cohorts born around the time of the 1964 Civil Rights Acts experienced large improvements in test scores, educational attainment, and earnings, but not because of increases in employment rates (Chay, Guryan, and Mazumder 2009, 2014). Long-run evaluations of educational interventions come to similar conclusions. Chetty, Friedman, and Rockoff (2014), for example, find that extensive margin labor supply explains at *most* 23 percent of the long-run earnings effects of teacher quality.³⁰

VI. DISCUSSION: THE RETURN TO MEDICAID SPENDING

The results above show large effects of early childhood Medicaid coverage on adult health, labor supply, and program participation, mainly for whites. Some of these benefits accrue to individuals. The longevity effects alone, for example, are very large even with a conservative estimate of the value of lives saved (at least \$291 billion). Incorporating the value of improved physical and cognitive functioning would add significantly to this number. This is in stark contrast to recent welfare estimates from the Oregon Health Insurance Experiment showing that Medicaid coverage is worth *less* than its cost (Finkelstein, Hendren, and Luttmer 2015).

With respect to total personal income, these changes largely cancel out. Medicaid alters the composition but not the amount of income, and recipients are not materially better off. This is similar to the program substitution identified by (Kline and Walters 2016). It also implies that

³⁰ This fits well with simple employment and wage comparisons by disability status and by education. The wage gap by high school graduation status (\$6.50) is much larger than the wage gap by disability status (\$2.20), but the employment gap is much larger by disability (45 points) than by education (25 points). Thus, the reasons why exposure to Medicaid had no effect on educational attainment remain interesting, but the effects on health, employment, transfer participation, and income are all much more consistent with improved health and reduced activity limitations than with higher human capital attainment.

disability programs successfully insure against poor health and by doing so limit the extent to which early health investments can ameliorate adult poverty.

In contrast to the null effects on personal income, the government gains tax revenue from the new earnings *and* saves on transfer payments. How big, then, are the aggregate changes in revenue relative to the cost of childhood coverage for these cohorts? I estimate Medicaid's effect on measures of total income and payroll taxes, as well as specific tax items such as the EITC using NBER's Taxsim 9.0 (Feenberg and Coutts 1993). I follow Agrawal and Hoyt (2016) in preparing the data and allocate family-level tax liability across people according to their share of income. Each year of early childhood eligibility increases average annual federal tax liability by \$294 (s.e. = 153). The distribution of tax liability, however, spreads out because of the increase in extensive margin labor supply. Households with a counterfactual tax bill of zero either owe positive taxes or receive large EITC refunds. Recent work finds reductions in EITC payments (Brown, Kowalski, and Lurie 2014), which can occur if Medicaid moves earnings above the plateau range of about \$18,000 or reduces fertility in a way that shifts down the EITC schedule. My results on extensive margin labor supply and the low level of new earnings, however, are consistent with increases in EITC. Multiplying the revenue results by the total years of early childhood eligibility (0.37 years*56 million whites with any eligibility) gives an annual increase in tax revenue of \$6.1 billion.

The newly employed adults appear to have left transfer programs, however, and the government saves *all* of these foregone benefits. Each year of early childhood eligibility reduces transfer income by \$590 (table 7, column 3), which implies an annual savings of \$12.2 billion (56 million*0.37*-590)—twice as much as the new tax revenue. Other research on Medicaid has neglected its impact on public assistance, and the large public return from reducing transfers shows that this omission matters greatly for the future savings of child Medicaid coverage.

Reductions in public insurance participation also represent an important source of savings. Each year of early Medicaid eligibility reduces public insurance coverage by 4.19 percentage points (table 6, column 4), and the results for self-reported disability and disability transfer receipt suggest that most of those who leave public insurance would have qualified through disability provisions. Per-enrollee expenditures are very high for disabled recipients of Medicaid (\$16,643; Kaiser Family Foundation 2012) and Medicare (\$10,495; CMS 2013 table 3.6), but they are also strongly influenced by the right tail of spending. The *median* SSDI recipient on

Medicare, for example, spends between \$2,000 and \$5,000 (and is probably on the lower end of this range since more than 47% of recipients spend under \$2,000), and the average spending within that category is \$3,326. Using this as a benchmark for public insurance spending suggests that lower public medical costs save \$2.9 billion per year ($56 \text{ million} * 0.37 * \$3,326 * -0.0419$).³¹

Relative to the cost of covering cohorts born between 1956 and 1975, the implied 21.2 billion in annual savings as a result of this coverage is 16 percent of the original cost of coverage. To calculate these costs, I first use data on total expenditures from 1966-1975 (Goodman-Bacon forthcoming) since all of this spending applies to the cohorts studied here. I use CPS data (Flood et al. 2015) to calculate the share of child Medicaid recipients born before 1976 for each calendar year between 1976 and 1993 (when the 1975 cohort was 18), and multiply this by total child Medicaid spending in each year (CMS 2013 table 13.10).³² This implies that it cost \$132 billion (in 2012 dollars) to cover the relevant cohorts that contribute to the effects documented above.

Because the costs and long-run benefits are separated by several decades, discounting strongly affects the ultimate return calculations. The standard approach discounts annual benefits (2000-2014) and annual costs (1966-1993) to 2014 dollars, which yields an annual return of between 5.6 and 8.5 percent (average of 6.9 percent). If these effects approximate what similar policy changes would achieve today, then the government can expect to earn this return over a similar time frame at interest rates of 3 percent. This discounting assumption suggests that we have saved 104 percent of the original *discounted* cost just in the 15 sample years.

This exercise is less suited to examining the savings that the government has actually enjoyed because real interest rates that determined the cost of borrowing when these expenditures were made were often much higher than 3 percent. Nominal 10-year treasury bond rates, for example, were over 10 percent for the first half of the 1980s, when about one third of the nominal expenditures on these cohorts occurred. Following the method used by OMB to conduct cost-effectiveness analysis, I also discount the (nominal) costs and benefits using observed (nominal) 10-year treasury bond rates. This yields similar benefits but much higher costs, suggesting that

³¹ These public savings accrue mainly to the federal government, meaning that Medicaid's long-run effects represent a substantial intergovernmental transfer. States paid roughly half the cost of Medicaid in the 1960s and 1970s, but the federal government recoups most of the savings through SSDI and Medicare.

³² I interpolate the share from 1 in 1975 to the observed 1980 value, the first year the CPS asks about Medicaid.

the annual return is between 1.6 and 2.6 percent (average of 1.9 percent), and the government has saved 28 percent of the *true* cost of covering the original Medicaid cohorts.³³

VII. CONCLUSION

This paper uses the original introduction of Medicaid combined with historical variation in welfare-based Medicaid eligibility across states to provide evidence on the effect of childhood insurance coverage on adult outcomes. Despite large contemporaneous effects and high participation, nonwhite children covered by Medicaid do not appear to experience significant changes in adult outcomes. White children, on the other hand, are healthier adults by a number of measures—cumulative mortality and self-reported disability—work more, and are less likely to receive public transfer benefits, particularly those tied to disability. These cohorts were not, however, differentially well off in childhood nor did they experience different underlying trends in early-life health or exposure to related public programs from the 1960s (Goodman-Bacon forthcoming). The results consistently show that coverage at younger ages, typically below age 10, matters the most. Since Medicaid coverage provided a broad range of medical services, the adult health effects, across causes of death and types of disability, are similarly widely distributed.

The health improvements themselves are certainly quite valuable to individuals, but the labor supply and program participation effects offset each other so that material well-being—poverty and total income—are unaffected. These changes, however, doubly benefit the government. I calculate that the government saves between 1.9 and 7 percent of the original cost of covering these cohorts every year, depending on the method of discounting used. Two-thirds of these savings come from reductions in cash and in-kind transfers, which have not previously been studied in this context. Early-life health programs can improve later-life health directly, have little effect on poverty because they crowd out public benefits, and yet generate significant public savings.

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³³ These calculations ignore distortions introduced by other methods of financing, such as increased taxes (Tax Foundation 1968).

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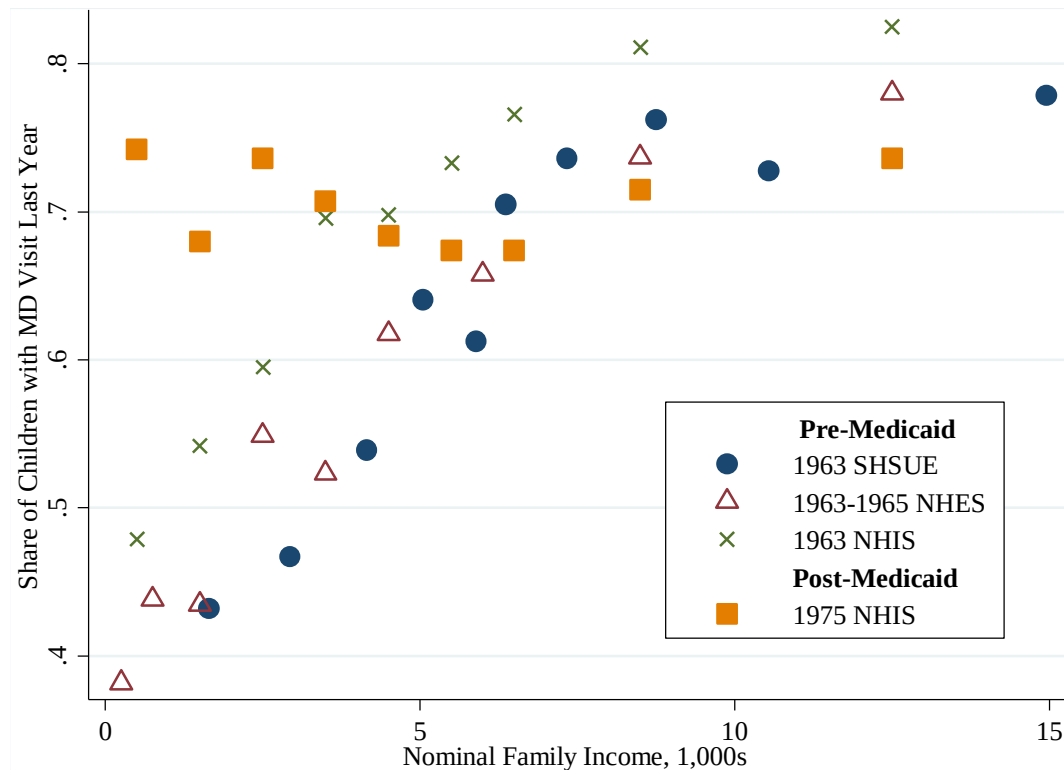
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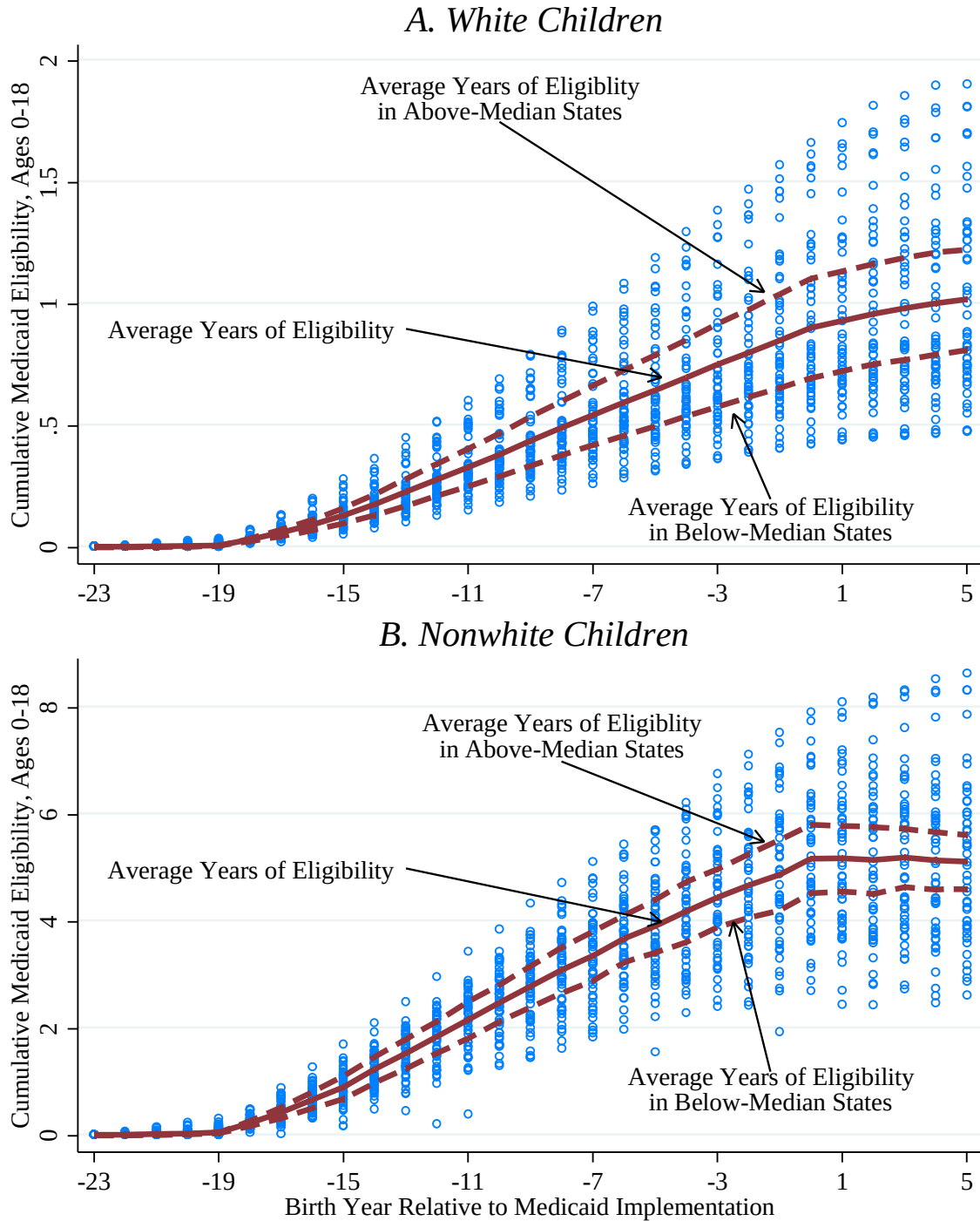
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Figure 1. Family Income and the Probability that Children Saw a Doctor in the Previous Year, Before and After Medicaid



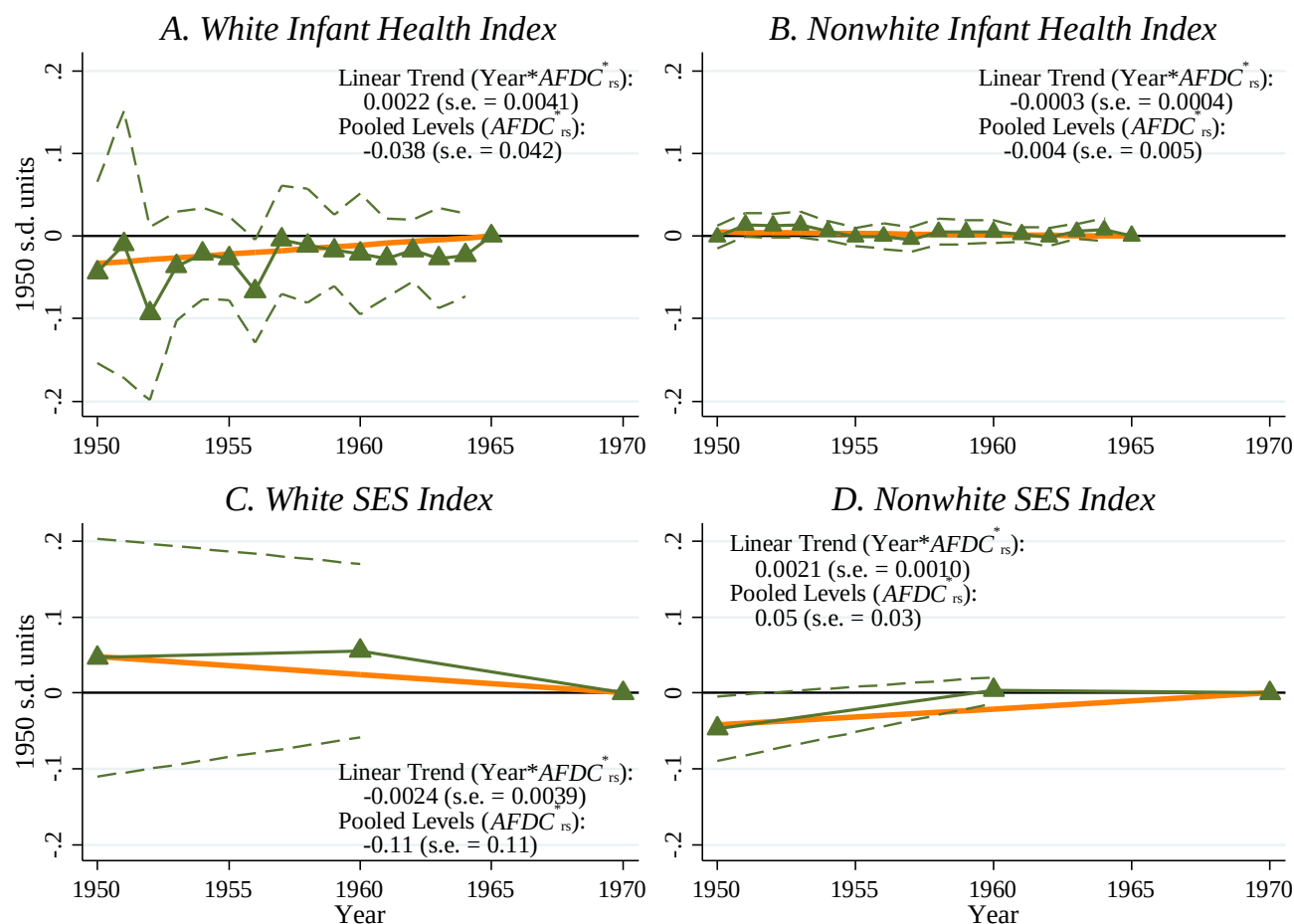
Notes: The figure plots the share of children who report having seen a doctor in the previous year in four survey data sources: the 1963 Survey of Health Services Utilization and Expenditure (CHAS 1988), the 1963-1965 National Health Examination Survey (ICPSR); and the 1963 and 1975 National Health Interview Surveys (NHIS). In all but the SHSUE, family income is reported as the median value of each bracket in which total family income is reported. In the SHSUE, it is the mean value within each decile. For scale, only bins less than or equal to \$15,000 are plotted (income is measured in nominal dollars; the poverty line for a family of four is between \$3,000 and \$5,000). By this measure, income ceases to be a significant predictor of any annual doctor visit after Medicaid was implemented. The univariate regression slopes associated with these cell means are 0.027 (s.e. = 0.006) in the SHSUE, 0.027 (s.e. = 0.003) in the NHES, 0.029 (s.e. = 0.005) in the 1963 NHIS, and 0.0029 (s.e. = 0.002) in the 1975 NHIS. Given the clear nonlinearity in the pre-Medicaid years, the slopes on the observations of family income under \$10,000 have the same pattern but are about twice as large (except for the 1975 slope: -0.004, s.e. = 0.004).

Figure 2. Cumulative Childhood Medicaid Eligibility by Race and Event Time



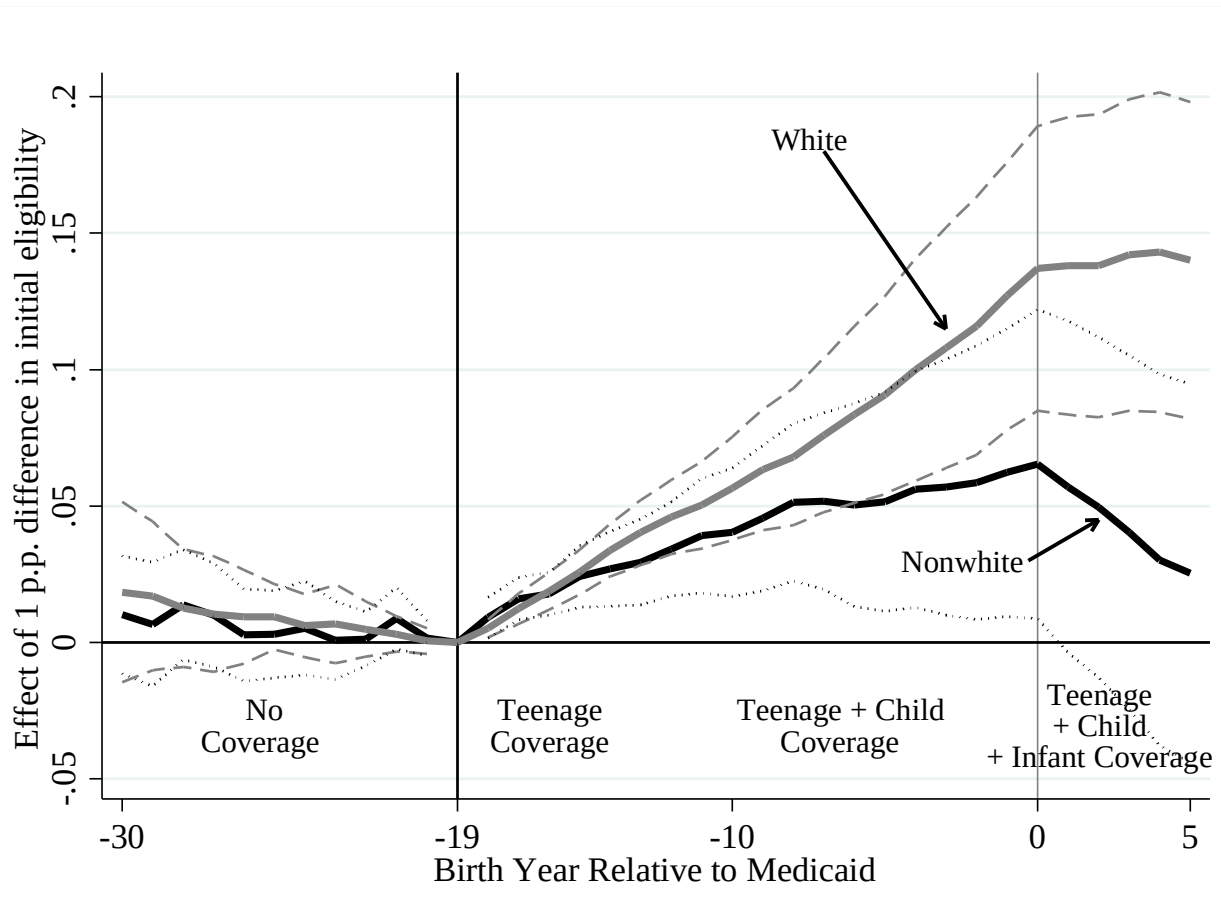
Notes: The figure plots cumulative childhood categorical Medicaid eligibility for birth cohorts from each state born in the 23 years before (1943-1947) and five years after Medicaid implementation (1971-1975). Note that the y-axes are different in the two panels. Cumulative eligibility is calculated according to equation (1) in the text. For each cohort, each year of childhood contributes a state-of-residence weighted mean of child AFDC rates interacted with a post-Medicaid dummy. These are summed over ages 0-18 to get an expected number of years of childhood eligibility. The solid line shows average eligibility across cohorts. The dashed lines show average eligibility in states with above- or below-median AFDC rates in the year of Medicaid implementation. For cohorts with full childhood coverage, eligibility differs across the two groups by 0.47 years for whites and by about 1.1 years for nonwhites.

Figure 3. Test of Differential Trends: Pre-Medicaid Changes in Indices of Infant Health and Socioeconomic Measures



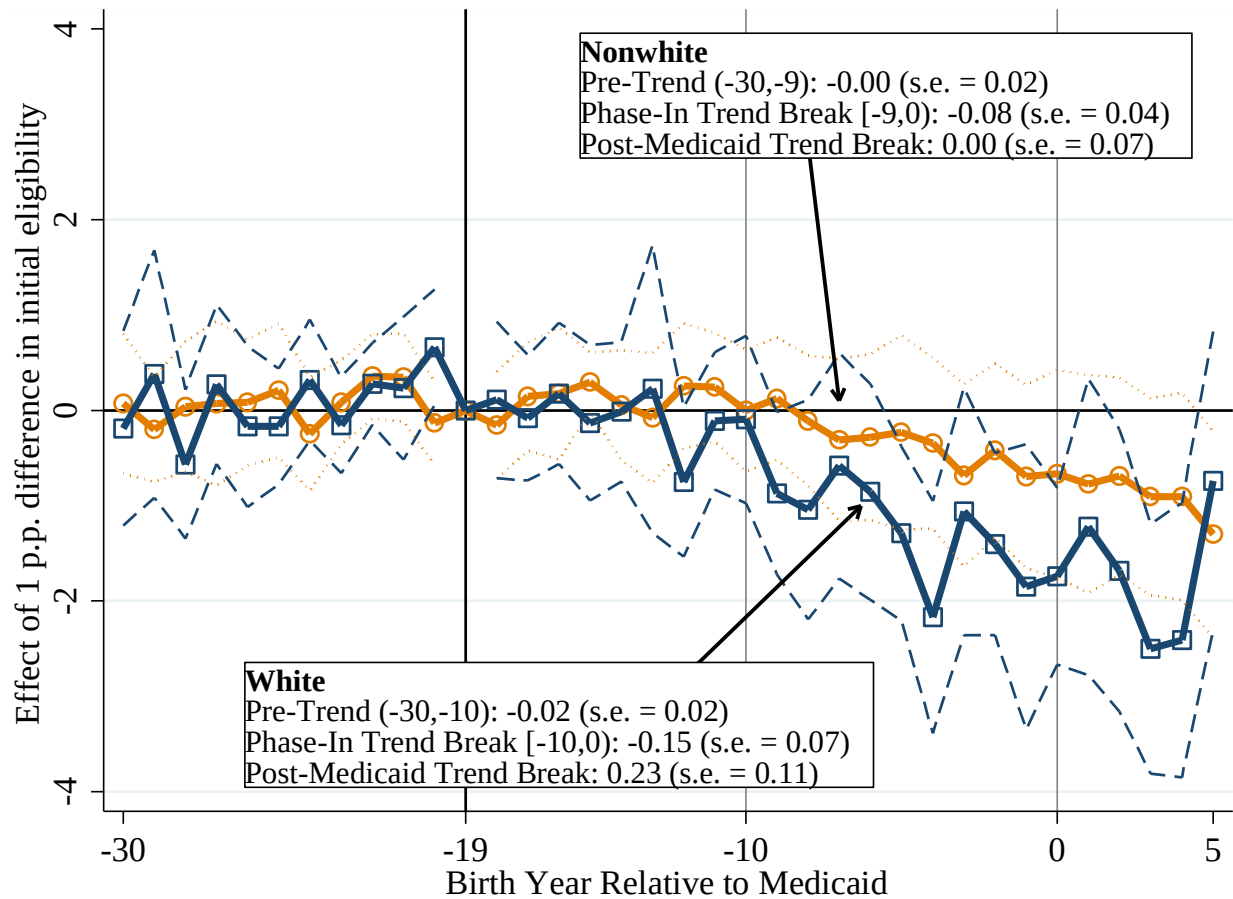
Notes: The infant health index is an equally weighted mean of the following variables standardized by their 1950 mean and standard deviation: low and very low birth weight rates, neonatal and postneonatal infant mortality rates, the sex ratio at birth, and the share of births in a hospital. The SES index is constructed similarly (for children under age 10) and includes the 25th, 50th, and 75th percentiles of children's household incomes; the child poverty rate; the share of children in households whose head has a high school degree or more, is in the labor force, and is employed; the share of children who live with no parents or both parents; household size; and the share of children ages 4-6 enrolled in school. The closed triangles are coefficients on the interaction between year dummies and $AFDC^*_{rs}$, and the straight lines are the estimated coefficient on an interaction between continuous year and $AFDC^*_{rs}$. The estimated slope and standard error are noted in the figure. The coefficient for "pooled levels" comes from a bivariate regression of the index on $AFDC^*_{rs}$. Regressions are weighted by births or the sum of Census weights, and standard errors (and the dashed 95-percent pointwise confidence intervals) are clustered by state.

Figure 4. First-Stage Relationship Between Cumulative and Initial Medicaid Eligibility Before and After Medicaid Implementation



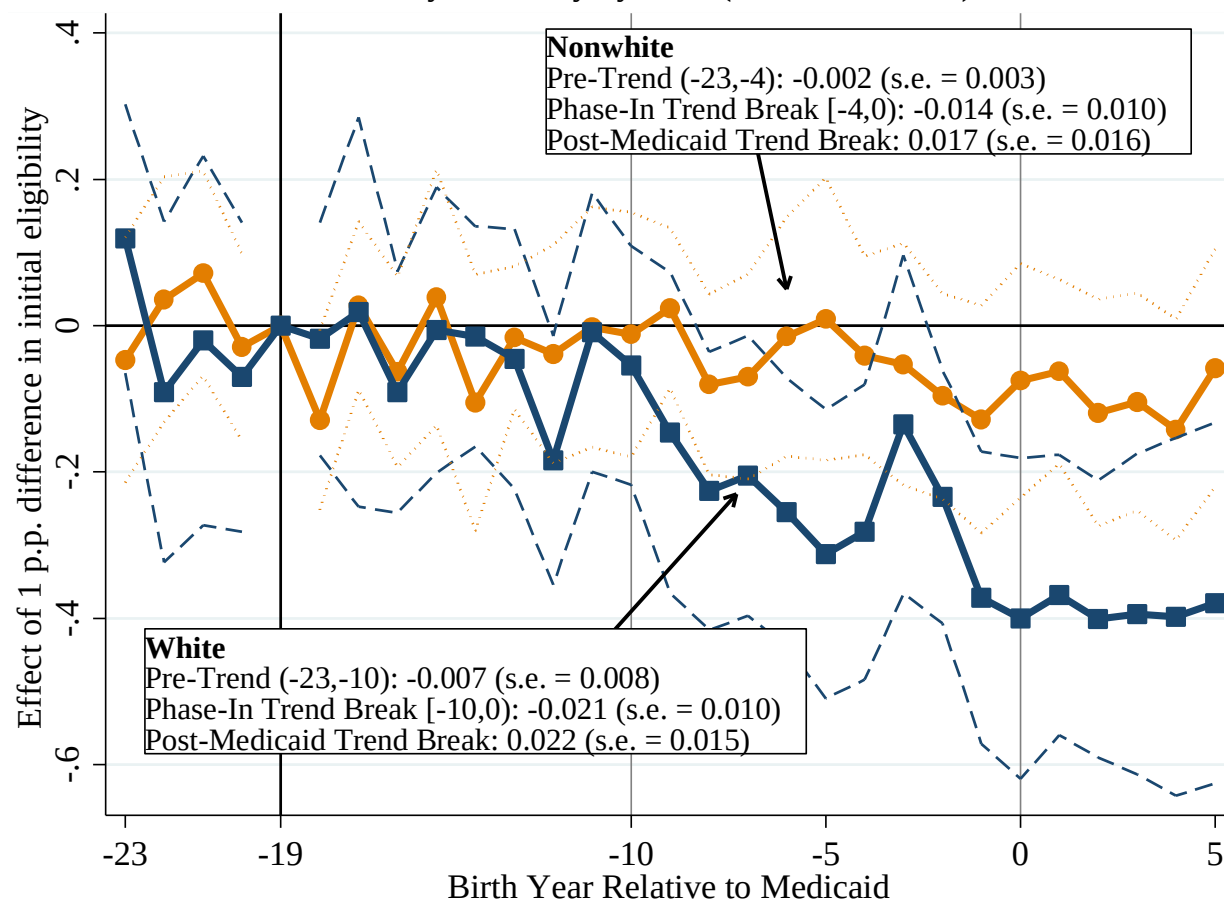
Notes: The dependent variable is each cohort's cumulative, migration-adjusted Medicaid eligibility for ages 0-18. The figure plots the estimated coefficients on interactions between $AFDC_{rs}^*$ and event-time dummies for each of 30 years before and five years after Medicaid. Time -19 is omitted. The dataset includes one observation per state/year cohort because childhood eligibility is determined by age 18. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; state per-capita income and hospital beds averaged over childhood; and each cohort's general fertility rate and infant mortality rate. The dashed lines are 95-percent confidence intervals based on standard errors clustered by birth state. While the above/below median differences in eligibility in figure 2 are larger for nonwhites than whites, the effect per point of the AFDC rate is smaller both because of the model's controls and because the underlying AFDC differences across high- and low-AFDC states are much larger for nonwhite than for white AFDC rates.

Figure 5. Reduced-Form Event-Study Estimates of Medicaid's Effect on log 20-Year Non-AIDS Mortality Rates (coefficients $\times 100$), 1980-1999



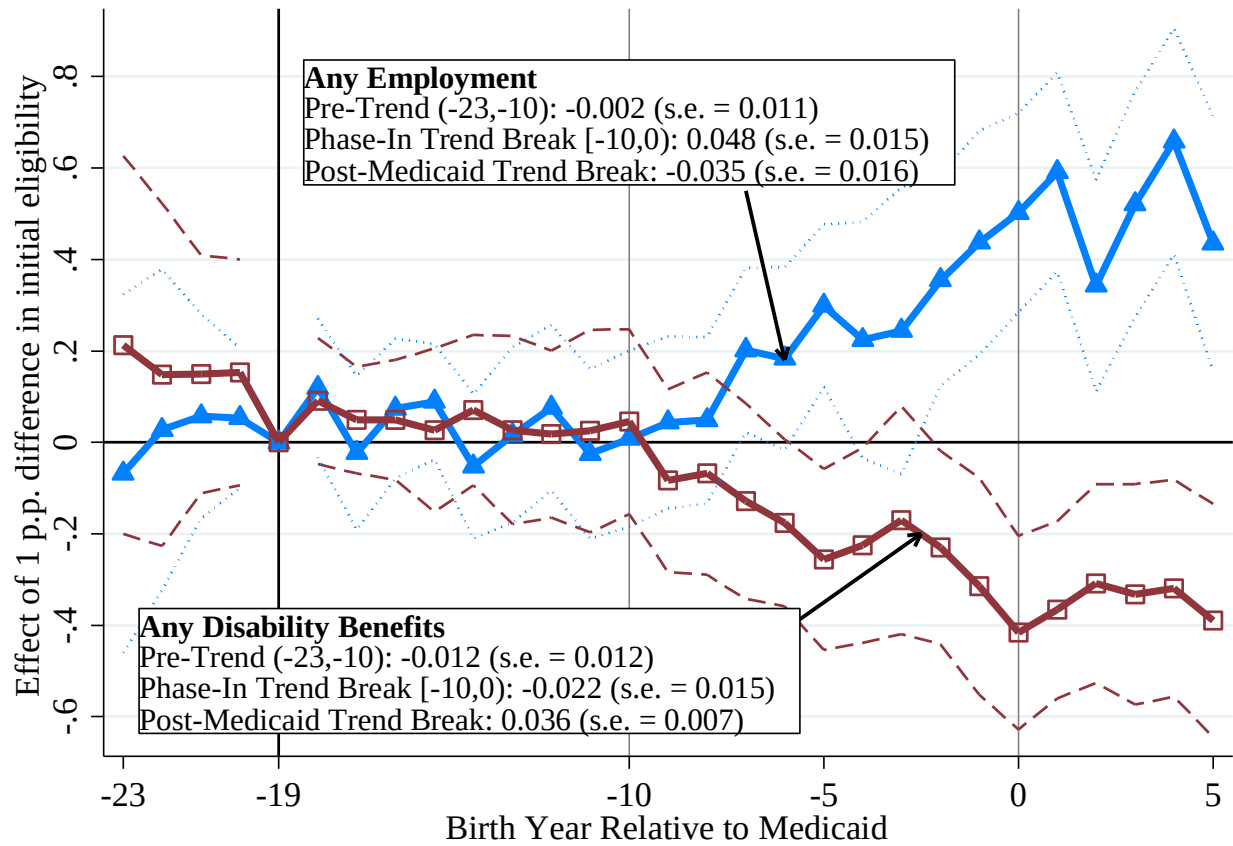
Notes: The figure plots the estimated coefficients on interactions between $AFDC_{rs}^*$ and event-time dummies for each of 30 years before and five years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; state per-capita income and hospital beds averaged over childhood; and each cohort's general fertility rate. The nonwhite estimates also adjust for a linear trend interacted with $AFDC_{rs}^*$ for event-times prior to -10. Estimates are weighted by the 1980 population. The dashed lines are 95-percent confidence intervals based on standard errors clustered by birth state. The break at zero is fixed because all subsequent cohorts are exposed to Medicaid for their entire childhoods. Source: Ruggles et al. (2010), United States Department of Health and Human Services (2009).

Figure 6. Reduced-Form Event-Study Estimates of Medicaid’s Effect on Adult Rates of Ambulatory Difficulty by Race (coefficients×100)



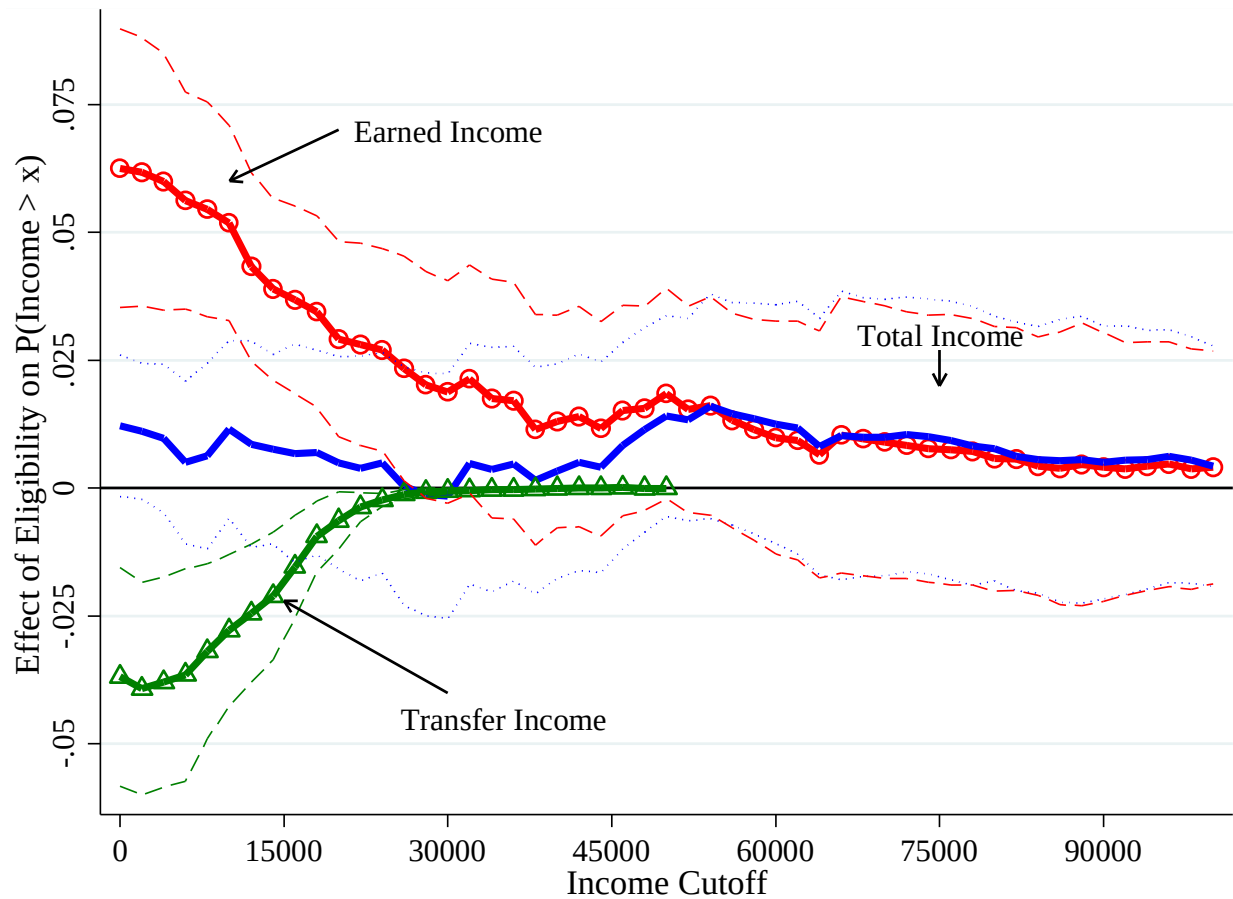
Notes: The dependent variable is the share of respondents in each state-of-birth-by-cohort cell who report having a “long-lasting condition that substantially limits one or more basic physical activities such as walking, climbing stairs, reaching, lifting, or carrying” (ambulatory difficulty). The estimation sample includes Census/ACS years 2000-2007, when the question text was comparable (see appendix figure 1.3). The figure plots the estimated coefficients on interactions between $AFDC_{rs}^*$ and event-time dummies for each of 23 years before and five years after Medicaid. Time -19 is omitted. The model includes birth-state, region-by-birth-year, and Medicaid-year-by-birth-year fixed effects; state per-capita income and hospital beds averaged over childhood; and each cohort’s general fertility rate and infant mortality rate. Estimates are weighted by the sum of the Census weights in each cell (unweighted estimates are similar and plotted in appendix figure 3.1). The dashed lines are based on standard errors clustered by birth state. The trend break coefficients come from a model that fits straight lines between event times [-23, -11], [-10, -1], and [0, 5]. The break at zero is fixed because all subsequent cohorts are exposed to Medicaid for their entire childhoods. The break at -10 comes from maximizing the F -statistic on the three trend terms that use different break points from -22 through -2. A plot of these F -statistics is in appendix figure 2.4.

Figure 7. Reduced-Form Event-Study Estimates of Medicaid's Effect on Employment Rates and Disability Benefit Receipt, White Respondents (coefficients×100)



Notes: The dependent variable is the share of white respondents in each state-of-birth-by-cohort cell who report having any annual employment (closed triangles) or receiving income from a disability-related transfer program such as SSI or SSDI (open squares). The estimation sample includes Census/ACS years 2000-2014. Because these questions are comparable over time, appendix figure 4.8 presents similar results using the 1980 and 1990 Census, which allows for a 45-year pre-trend (not all covariates are available for these cohorts). The estimates are nearly identical, and neither employment nor disability benefit receipt exhibit trends correlated with initial AFDC for cohorts born as early as 1920. For details on the specification, see text and notes to figure 6.

Figure 8. Instrumental Variables Estimates of the Effect of Medicaid Eligibility Before age 10 on the Distribution of Income By Source, White Respondents



Notes: The figure plots instrumental variables estimates of the effect of cumulative Medicaid eligibility under age 10 on the probability of earnings, transfer income, or total income greater than the amount on the x-axis (measured in \$2,000 bins in 2012 dollars). The sample includes Census/ACS years from 2000 to 2014. \$50,000 is the maximum of the transfer income variable.

Table 1. First-Stage Relationship between Predicted Eligibility and Migration-Adjusted Cumulative Medicaid Eligibility

	(1)	(2)	(3)
	<i>Cumulative Eligibility, Ages 0 -18</i>	<i>Cumulative Eligibility, Ages 0 -10</i>	<i>Cumulative Eligibility, Ages 11 -18</i>
<i>A. White Adults</i>			
Predicted Eligibility at:			
Ages 0-18	0.72 [0.16]		
Ages 0-10		0.77 [0.22]	0.08 [0.07]
Ages 11-18		-0.04 [0.12]	0.57 [0.14]
<i>F</i> -statistic	21.7		
Angrist/Pischke <i>F</i> -statistic		33.0	13.1
<i>B. Nonwhite Adults</i>			
Ages 0-18	0.20 [0.18]		
Ages 0-10		0.40 [0.20]	-0.42 [0.08]
Ages 11-18		-0.02 [0.08]	0.57 [0.14]
<i>F</i> -statistic	1.2		
Angrist/Pischke <i>F</i> -statistic		19.7	13.1

Notes: Column 1 presents first-stage estimates for the effect of predicted childhood Medicaid eligibility, z_{rsc} , on actual, migration-adjusted cumulative childhood Medicaid eligibility, m_{rsc} . Columns 2 and 3 present similar first-stage estimates that split eligibility into two sub-periods: ages 0-10 and ages 11-18. *F*-statistics that measure the strength of the age-specific instruments for each eligibility variable are presented for these regressions (Angrist and Pischke 2009).

**Table 2. Instrumental Variables Estimates of Medicaid's Effect on log 20-Year Mortality Rates by Race and Cause
(coefficients×100)**

Cause of Death:	(1) All-Cause	(2)	(3)	(4)	(5)	(6)	(7)
		Non-AIDS-Related Causes					
		Total	Internal (incl. Suicide)	Cardio- vascular	Cancer	Suicide	External (Homicide + Accidents)
Childhood Medicaid Eligibility		<i>A. White Adults, 1980-1999</i>					
Ages 0-10	-23.9 [9.8] (0.058)	-15.5 [5.4] (0.006)	-31.7 [6.9] (0.002)	-15.0 [9.6] (0.168)	-19.0 [9.0] (0.120)	-39.7 [8.4] (0.002)	-1.9 [6.7] (0.776)
Ages 11-18	7.2 [14.3]	-11.0 [7.2]	-8.6 [7.2]	31.1 [10.8]	1.5 [7.8]	0.9 [11.2]	-10.4 [14.3]
H ₀ : 0-10 = 11-18 (<i>p</i> -val)	0.19	0.70	0.08	0.01	0.13	0.03	0.65
Mean Dependent Variable (deaths per 100,000)	3,690	3,090	2,280	800	937	293	826
Childhood Medicaid Eligibility		<i>B. Nonwhite Adults, 1980-1999</i>					
Ages 0-10	-30.7 [13.3] (0.020)	-19.6 [9.4] (0.040)	-25.2 [7.6] (0.002)	-27.3 [11.1] (0.022)	-17.3 [8.3] (0.052)	-39.3 [15.9] (0.002)	-18.2 [10.1] (0.054)
Ages 11-18	10.0 [10.7]	4.8 [7.0]	7.8 [6.9]	6.7 [8.3]	2.2 [7.2]	-5.0 [9.7]	4.0 [7.2]
H ₀ : 0-10 = 11-18 (<i>p</i> -val)	0.05	0.06	0.01	0.04	0.11	0.06	0.11
Mean Dependent Variable (deaths per 100,000)	7,980	5,600	3,910	1,880	1,360	203	1,830

Notes: The table presents instrumental variables estimates of Medicaid's effect on log cumulative mortality rates (1980-1999) by cause. Standard errors are clustered by state of birth, and for effects between ages 0 and 10, *p*-values from 500 draws of a percentile-*t* wild cluster bootstrap are in parentheses. Mortality rates by cause do not add to the total because they are calculated using cause-elimination life table methods to account for the confounding influence of competing risks from the other causes.

Table 3. Instrumental Variables Estimates of Medicaid's Effect on Adult Rates of Ambulatory Difficulty by Race and Across Specifications (coefficients×100)

	(1)	(2)	(3)	(4)	(5)	(6)
Childhood Medicaid Eligibility	<i>A. White Adults, 2000-2007</i>					
Ages 0-10	-1.56	-3.84	-3.15	-2.34	-2.75	-3.80
	[1.69]	[1.15]	[1.49]	[1.16]	[0.76]	[1.15]
	(0.480)	(0.004)	(0.134)	(0.226)	(0.010)	(0.010)
Ages 11-18	1.63	-1.13	-0.99	-0.42	0.45	-0.96
	[2.62]	[1.43]	[1.46]	[1.26]	[1.07]	[1.41]
H ₀ : 0-10 = 11-18 (<i>p</i> -val)	0.02	0.21	0.40	0.23	0.04	0.18
Observations		1,962			94,434	455,057
Childhood Medicaid Eligibility	<i>B. Nonwhite Adults, 2000-2007</i>					
Ages 0-4	-1.33	-2.92	0.11	-2.10	-2.08	-3.51
	[0.93]	[1.81]	[1.74]	[0.96]	[1.31]	[1.97]
	(0.168)	(0.048)	(0.966)	(0.058)	(0.150)	(0.070)
Ages 5-18	0.04	-0.16	-0.37	0.32	-0.24	-0.30
	[0.44]	[0.71]	[1.23]	[0.56]	[0.86]	[0.7]
H ₀ : 0-4 = 5-18 (<i>p</i> -val)	0.11	0.05	0.82	0.01	0.03	0.04
Observations		1,962			60,362	176,543
Covariates	Initial eligibility + Time-to-Medicaid FE	(1) + State FE+ Year FE + Medicaid-Year-by-Cohort FE + Region-by-Cohort FE + Xst		(2) + State-specific cohort trends	(2) + Cohort-by-state-of-residence FE	(2) + Cohort-by-Year-by-Unemployment-rate interactions
Weighted?	Y	Y	N	Y	Y	Y
Level of Observation		State-of-birth/cohort			State-of-residence/state-of-birth/cohort	State-of-residence/state-of-birth/cohort/year

Notes: The table presents instrumental variables estimates of Medicaid's effect on ambulatory difficulty across specifications. Standard errors are clustered by state of birth. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the early childhood effects are in parentheses. The age ranges are determined by the best-fitting trend breaks from figure 6. Nonwhite results come from a two-step procedure in which a linear pre-trend from event-time -23 to -4 is removed and IV estimates are based on these adjusted data.

Table 4. Instrumental Variables Estimates of Medicaid's Effect on Adult Disability Measures by Race (coefficients×100)

	(1) Ambulatory Difficulty	(2) Hearing/Vision Difficulty	(3) Mobility Difficulty	(4) Self-Care Difficulty	(5) Cognitive Difficulty	(6) Work Limitation
Childhood Medicaid Eligibility	<i>A. White Adults, 2000-2007</i>					
Ages 0-10	-3.84 [1.15] (0.012)	-1.17 [0.29] (0.004)	-1.36 [0.36] (0.006)	-1.24 [0.29] (0.002)	-1.70 [0.39] (0.008)	-2.99 [0.74] (0.004)
Ages 11-18	-1.13 [1.43]	0.28 [0.72]	-0.69 [0.57]	0.35 [0.49]	0.31 [0.63]	-2.14 [1.16]
H ₀ : 0-10 = 11-18 (<i>p</i> -val)	0.21	0.07	0.35	0.02	0.03	0.53
Mean Dependent Variable	8.61	3.15	3.75	2.27	4.41	8.50
Childhood Medicaid Eligibility	<i>B. Nonwhite Adults, 2000-2007</i>					
Ages 0-4	-2.92 [1.81] (0.054)	0.28 [0.8] (0.748)	-0.40 [0.69] (0.606)	0.03 [0.53] (0.974)	0.01 [0.84] (0.994)	0.602 [1.08] (0.600)
Ages 5-18	-0.16 [0.71]	-0.20 [0.47]	0.27 [0.36]	0.01 [0.31]	-0.33 [0.46]	0.35 [0.59]
H ₀ : 0-4 = 5-18 (<i>p</i> -val)	0.05	0.34	0.19	0.98	0.59	0.75
Mean Dependent Variable	12.70	4.03	6.53	3.93	6.87	13.00
Question Text	Does this person have any of the following long-lasting conditions: ...substantially limits ≥1 basic physical activities such as walking, climbing stairs, reaching, lifting, or carrying?		Because of a physical, mental, or emotional condition lasting ≥ 6 months, does this person have any difficulty:			
		Blindness, deafness, or a severe vision or hearing impairment?	Going outside the home alone to shop or visit a doctor's office?	Dressing, bathing, or getting around inside the home?	Learning, remembering, or concentrating?	Working at a job or business?

Notes: The table presents instrumental variables estimates of Medicaid's effect on all disability measures available in the Census. The specification is the same as in figure 6 and column 2 of table 3. There are 1,962 observations. The age ranges are determined by the best-fitting trend breaks from figure 6. Nonwhite results use a procedure in which a linear pre-trend from event-time -23 to -4 is removed and IV estimates are based on these adjusted data. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the early childhood effects are in parentheses.

Table 5. Instrumental Variables Estimates of Medicaid's Effect on Disability, Adjusting for Selective Survival

	(1)	(2)	(3)	(4)	(5)
<i>Disability Rate Among Those Induced to Survive</i>	<i>0%</i>	<i>15%</i>	<i>30%</i>	<i>50%</i>	<i>80%</i>
Childhood Medicaid Eligibility					
White, Ages 0-10	-3.83	-3.86	-3.90	-3.94	-4.01
	[1.15]	[1.15]	[1.15]	[1.15]	[1.16]
Nonwhite, Ages 0-10	-1.77	-2.08	-2.40	-2.82	-3.45
	[1.3]	[1.29]	[1.29]	[1.29]	[1.3]
Nonwhite, Ages 0-4	-3.09	-3.61	-4.13	-4.82	-5.85
	[2.04]	[2.02]	[2.02]	[2.02]	[2.05]
Nonwhite, Ages 5-10	-0.26	-0.35	-0.44	-0.55	-0.73
	[0.87]	[0.86]	[0.86]	[0.85]	[0.85]
H ₀ : Row 1 = Row 2 (<i>p</i> -val)	0.23	0.30	0.38	0.51	0.74
H ₀ : Row 1 = Row 3 (<i>p</i> -val)	0.75	0.91	0.92	0.70	0.42

Notes: The table presents IV estimates from two regressions that pool white and nonwhite observations and use outcome variables adjusted for differential survival (see figure 5) under different assumptions about the disability rate of induced survivors. All covariates, instruments, and exogenous variables are fully interacted with white and nonwhite dummies so that the coefficients are comparable to those in table 3. In model 1 (rows 1 and 2), both white and nonwhite eligibility are measured from ages 0-10 and 11-18. In model 2, white eligibility is the same (row 1) and nonwhite eligibility is measured from ages 0-4, 5-10 (rows 3 and 4), and 11-18. The *p*-values come from *t*-tests of the null hypothesis that the early childhood effects are equal across race. Moving across rows shows the role of selective survival in the white/nonwhite differences in the results. Survival has no effect on the white results. Under the assumption that Medicaid saved the lives of nonwhite adults who would have been very disabled, the nonwhite treatment effects are almost twice as large, but as the event-study results in figure 6 show, they are still present only for exposure under age 4.

Table 6. Instrumental Variables Estimates of Medicaid's Effect on Extensive Margin Labor Supply for White Adults (coefficients×100)

	(1)	(2)	(3)	(4)
	Out of the Labor Force	Currently Employed	Any Employment Last Year	Full- Time/Full- Year Employment
Childhood Medicaid Eligibility				
Ages 0-10	-6.59	5.82	6.33	4.72
	[1.47]	[1.18]	[1.37]	[0.75]
	(0.002)	(0.002)	(0.002)	(0.002)
Ages 11-18	1.46	-2.10	-0.94	-2.44
	[2.00]	[1.95]	[1.97]	[1.93]
H ₀ : 0-10 = 11-18 (<i>p</i> -val)	<0.01	<0.01	<0.01	<0.01
Mean Dependent Variable	22.0	74.2	80.9	51.4

Notes: The sample includes the 2000-2014 Census/ACS and has 1,914 observations. For details on the specification, see notes to figure 6. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the effects of exposure at age 10 and younger are in parentheses.

Table 7. Instrumental Variables Estimates of Medicaid's Effect on Public Assistance Receipt for White Adults (coefficients×100)

	(1)	(2)	(3)	(4)	(5)
	Any Public Assistance	Disability Benefits	TANF or General Assistance	Public Insurance	Any Insurance
Childhood Medicaid Eligibility					
Ages 0-10	-5.09	-5.88	0.83	-4.19	-1.16
	[1.50]	[1.54]	[0.14]	[0.98]	[0.70]
	(0.020)	(0.014)	(0.002)	(0.002)	(0.138)
Ages 11-18	1.95	2.23	-0.19	0.77	4.04
	[3.48]	[3.40]	[0.29]	[1.62]	[1.33]
H ₀ : 0-10 = 11-18 (<i>p</i> -val)	0.09	0.05	0.00	0.00	0.00
Mean Dependent Variable	15.8	15.1	1.0	13.5	7.7

Notes: The sample includes the 2000-2014 Census/ACS and has 1,914 observations. For details on the specification, see notes to figure 6. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the effects of exposure at age 10 and younger are in parentheses.

Table 8. Instrumental Variables Estimates of Medicaid's Effect on Average Income by Source for White Adults (coefficients×100)

	(1)	(2)	(3)	(4)	(5)
	Earned Income	Earned Income (Trimmed: \$100k)	Transfer Income	Total Income (Trimmed: \$100k)	Poverty Rate
Childhood Medicaid Eligibility					
Ages 0-10	1,870	1,810	-590	609	1.16
	[2,134]	[617]	[162]	[654]	[1.01]
	(0.592)	(0.008)	(0.002)	(0.366)	(0.612)
Ages 11-18	-3,756	-2,348	-512	-3,975	2.23
	[3,144]	[964]	[416]	[801]	[0.70]
H ₀ : 0-10 = 11-18 (<i>p</i> -val)	0.26	0.00	0.84	0.00	0.34
Mean Dependent Variable	45,088	31,945	1,026	35,441	7.5

Notes: The sample includes the 2000-2014 Census/ACS and has 1,914 observations. For details on the specification, see notes to figure 6. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the effects of exposure at age 10 and younger are in parentheses.

Table 9. Instrumental Variables Estimates of Medicaid's Effect on Educational Attainment for White Adults (coefficients×100)

	(1) High School Grad	(2) Any College	(3) Bachelor's Degree
Childhood Medicaid Eligibility			
Ages 0-10	1.27 [1.1] (0.496)	1.55 [2.91] (0.676)	0.57 [1.91] (0.828)
Ages 11-18	0.20 [1.81]	-1.47 [3.21]	-2.07 [1.56]
H ₀ : 0-10 = 11-18 (<i>p</i> -val)	0.66	0.57	0.35
Mean Dependent Variable	91.7	62.6	31.4

Notes: The sample includes the 2000-2014 Census/ACS and has 1,914 observations. For details on the specification, see notes to figure 6. *p*-values from 500 draws of a wild cluster percentile-*t* bootstrap for the significance of the effects of exposure at age 10 and younger are in parentheses.

Exhibit 32

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

January 17, 2018

SNAP Is Linked with Improved Nutritional Outcomes and Lower Health Care Costs

By Steven Carlson and Brynne Keith-Jennings

New and emerging research links the Supplemental Nutrition Assistance Program (SNAP, formerly food stamps), the nation's most important anti-hunger program, with improved health outcomes and lower health care costs. This research adds to previous work showing SNAP's powerful capacity to help families buy adequate food, reduce poverty, and help stabilize the economy during recessions.

SNAP is the primary source of nutrition assistance for many low-income people. In a typical month of 2017, SNAP helped about 42 million low-income Americans afford a nutritious diet. It provides important nutritional support for low-wage working families, low-income seniors, and people with disabilities living on fixed incomes: close to 70 percent of SNAP participants are in families with children, and more than one-quarter are in households with seniors or people with disabilities. While SNAP provides only a modest benefit — just \$1.40 on average per person per meal in 2017 — it forms a critical foundation for the health and well-being of low-income Americans, lifting millions out of poverty and improving food security.

Although evaluating SNAP's impact presents a daunting challenge in part because it is so widely available and because participants tend to be substantially different from non-participants, research emerging in the last ten years suggests that SNAP may affect household well-being in ways that extend beyond its intended aim to improve food security and nutrition. The available evidence suggests that SNAP is at least associated with and may promote better health and lower health care costs. And, to the extent this connection exists, it is plausible that policies that limit program eligibility and cut SNAP benefits would harm health and raise health care costs. Substantial research on SNAP and related areas has shown:

- **Food insecurity increases the risk of adverse health outcomes, complicates the ability to manage illness, and is linked to higher health care costs.** On average, after controlling for a range of socioeconomic and demographic characteristics expected to affect food security and health care costs, people in food-insecure households — those lacking consistent access to adequate food at some point during the year due to limited resources — spend roughly 45 percent more on medical care in a year (\$6,100) than people in food-secure households (\$4,200). And extensive research has shown a strong correlation between food insecurity and

chronic health conditions among various age groups: children, working-age adults, and seniors.

- **SNAP improves food security, offers benefits that enable families to purchase healthier diets, and frees up resources that can be used for health-promoting activities and needed medical care.** SNAP reduces the overall prevalence of food insecurity by as much as 30 percent, and is even more effective among the most vulnerable, such as children and those with “very low food security,” that is, when one or more household members have to skip meals or otherwise eat less during the year due to lack of money. The largest and most rigorous examination of the relationship between SNAP participation and food security found that food insecurity among children fell by roughly a third after their families received SNAP benefits for six months.
- **SNAP is associated with improved current and long-term health.** SNAP participants are more likely to report excellent or very good health than low-income non-participants. Early access to SNAP among pregnant mothers and in early childhood improved birth outcomes and long-term health as adults. Elderly SNAP participants are less likely than similar non-participants to forgo their full prescribed dosage of medicine due to cost. SNAP may also help low-income seniors live independently in their communities and avoid hospitalization.
- **SNAP is linked with reduced health care costs.** On average, again after controlling for factors expected to affect spending on medical care, low-income adults participating in SNAP incur about \$1,400, or nearly 25 percent, less in medical care costs in a year than low-income non-participants. The difference is even greater for those with hypertension (nearly \$2,700 less) and coronary heart disease (over \$4,100 less).

A Brief Introduction to the Supplemental Nutrition Assistance Program

SNAP is broadly available to almost all households with low incomes and few resources, unlike most means-tested benefit programs which are restricted to certain categories of low-income individuals.^a SNAP eligibility rules and benefit levels are, for the most part, uniform across the nation. Under federal rules, a household must meet three criteria to qualify for SNAP benefits (although states have flexibility to adjust these limits):

- Its gross monthly income generally must be at or below 130 percent of the poverty line, or \$2,213 (about \$26,600 a year) for a three-person family in fiscal year 2018. Households with elderly or disabled members and households that are “categorically eligible” for SNAP because they receive public assistance — such as Temporary Assistance for Needy Families or Supplemental Security Income — are not subject to the gross income test. The limit on gross income is higher for bigger families and lower for smaller families.
- The household’s net monthly income, or income after deductions are applied for items such as high housing costs and child care, must be less than or equal to the poverty line (\$1,702 a month or about \$20,400 a year for a three-person family in fiscal year 2018).
- Its assets must fall below \$2,250 for households without an elderly or disabled member and \$3,250 for those with an elderly or disabled member in fiscal year 2018.

SNAP targets its benefits according to need: households with less income receive larger benefits than households with more income since they need more help to afford an adequate diet. The benefit formula assumes that families will spend 30 percent of their net income for food; SNAP makes up the difference between that 30 percent contribution and the cost of a low-cost but nutritionally adequate diet.^b

^a Some categories of people are not eligible for SNAP regardless of their income or assets, such as individuals who are on strike, all unauthorized immigrants, and certain lawfully present immigrants. For more information, see “A Quick Guide to SNAP Eligibility and Benefits,” Center on Budget and Policy Priorities, updated September 14, 2017, <https://www.cbpp.org/research/a-quick-guide-to-snap-eligibility-and-benefits>.

^b For more information on the structure and effectiveness of SNAP, see “Policy Basics: Introduction to the Supplemental Nutrition Assistance Program (SNAP),” Center on Budget and Policy Priorities, updated October 3, 2017, <http://www.cbpp.org/research/policy-basics-introduction-to-the-supplemental-nutrition-assistance-program-snap>.

The Importance of Social Determinants of Health

Research accumulated over the last 25 years has firmly established the powerful effects of the circumstances in which people grow, live, work, and age on health, development, and longevity.¹ People disadvantaged by poor social and economic circumstances — including low income, poor education, insecure employment, poor housing, and inadequate retirement pensions — have worse health from the moment of birth throughout life. An average 40-year-old in the poorest 1 percent of American men, for example, will die 15 years sooner than a man in the richest 1 percent; the gap for American women is ten years.²

¹ Paula Braveman, Susan Egerter, and David R. Williams, “The Social Determinants of Health: Coming of Age,” *Annual Review of Public Health*, 32:381-98, 2011, <http://www.annualreviews.org/doi/pdf/10.1146/annurev-publhealth-031210-101218>; Nancy E. Adler *et al.*, “Addressing Social Determinants of Health and Health Disparities,” Vital Directions for Health and Health Care Series, National Academy of Medicine, September 19, 2016, <https://nam.edu/wp-content/uploads/2016/09/Addressing-Social-Determinants-of-Health-and-Health-Disparities.pdf>.

² Raj Chetty *et al.*, “The Association Between Income and Life Expectancy in the United States, 2001-2014,” *Journal of the American Medical Association*, 315(16):1750-1766, 2016, <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4866586/>.

While the ways that social and economic circumstances affect health are complicated and not fully understood, some research implicates chronic stress — including that stemming from economic hardships — in the more rapid onset or progression of chronic illnesses. Coping with the challenges of daily life can be particularly stressful when money is tight. The accumulated strain from trying to meet basic needs for food, shelter, and clothing with too few resources may lead to more damage than a single dramatically stressful event.³

Food insecurity may influence health and development through its effects on nutrition and as a component of overall family stress. Families that struggle with limited resources to put enough food on the table may buy more affordable but less nutritious foods. And the anxiety associated with unpredictable or intermittent meals may be a source of chronic stress that, if left unchecked, can contribute to an increased risk of chronic conditions, including high blood pressure, heart disease, obesity, and diabetes.

Food assistance programs like SNAP can help prevent or moderate the effects of food insecurity. Though its benefits are modest, SNAP helps low-income individuals and families who struggle to afford basic needs have enough to eat, enables them to afford healthier diets, and frees up resources that can be used on health-promoting activities and preventive health care. The benefits it provides may also offset some of the stress associated with food insecurity, and make it somewhat easier to manage chronic illness. Thus, SNAP may offer a pathway to improved health and lower public expenditures on health care.

Food Insecurity Is Associated with Poor Health Outcomes

An extensive body of research reveals a consistent and strong correlation between food insecurity and poor health outcomes across the life cycle. Food insecurity among children is linked to increased risks of poor diets, the development of chronic health conditions including asthma and anemia, cognitive and behavioral problems, anxiety and depression, and poorer general health. Food insecurity among working-age adults is associated with poorer diet quality; multiple chronic conditions, including hypertension, coronary heart disease, diabetes, and kidney disease; and poorer general and mental health. And among seniors, food insecurity is linked to poorer diets, chronic conditions such as diabetes and anemia, worse general health, depression, more limitations in daily activities, and decreased quality of life.⁴ Though to a lesser degree than with food insecurity, research suggests that marginal food security, defined by at least one reported indication of stress related to having insufficient food (but not as many indications as those considered food insecure), is also linked with adverse health outcomes among young children and caregivers.⁵

³ Richard Wilkinson and Michael Marmot, “Social Determinants of Health: The Solid Facts,” World Health Organization, 2003 (2nd edition), http://www.euro.who.int/_data/assets/pdf_file/0005/98438/e81384.pdf.

⁴ For summaries of the extensive research on the health consequences of food insecurity, see Barbara A. Laraia, “Food Insecurity and Chronic Disease,” *Advances in Nutrition: An International Review Journal*, 4(2):203-212, 2013, <http://advances.nutrition.org/content/4/2/203.full>; Alisha Coleman-Jensen, William McFall, and Mark Nord, “Food Insecurity in Households with Children,” EIB-113. U.S. Department of Agriculture, Economic Research Service, May 2013, https://www.ers.usda.gov/webdocs/publications/43763/37672_eib-113.pdf?v=41424; and Craig Gundersen and James P. Ziliak, “Food Insecurity and Health Outcomes,” *Health Affairs*, 34(11):1830-1839, 2015, [http://gatttonweb.uky.edu/Faculty/Ziliak/GZ_HealthAffairs_34\(11\)_2015.pdf](http://gatttonweb.uky.edu/Faculty/Ziliak/GZ_HealthAffairs_34(11)_2015.pdf).

⁵ John T. Cook *et al.*, “Are Food Insecurity’s Health Impacts Underestimated in the U.S. Population? Marginal Food Security Also Predicts Adverse Health Outcomes in Young U.S. Children and Mothers,” *Advances in Nutrition: An International Review Journal*, 4(1):51-61, 2013, <http://advances.nutrition.org/content/4/1/51.long>.

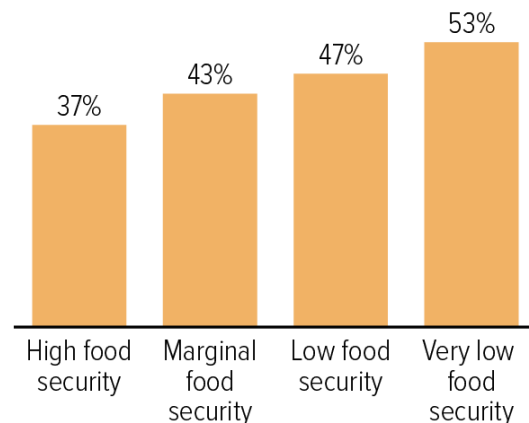
Recent work by researchers at the U.S. Department of Agriculture’s (USDA) Economic Research Service documents the strong correlation between food insecurity and chronic health conditions among low-income working-age adults. Their work shows that the risk of each of ten chronic conditions — hypertension, coronary heart disease, hepatitis, stroke, cancer, asthma, diabetes, arthritis, chronic obstructive pulmonary disease, and kidney disease — increases substantially as the severity of food insecurity increases (see Figure 1). Adults in households with very low food security are at least 40 percent more likely to be diagnosed with at least one of the ten chronic diseases than adults in food-secure households.⁶

Research has established a strong association between food insecurity and physical and mental health. Some of the research is based on study designs that make it difficult to determine whether food insecurity causes poor health, poor health contributes to food insecurity, or both are caused by other unobserved factors. Poor physical or mental health may make it harder to work, for example, leading to lower income and increasing the risk of food insecurity. Conversely, limited budgets may force families to purchase cheaper but less healthy food, or make a choice between buying food or medications, with adverse consequences for their health. But regardless of the precise mechanism that links the two, food insecurity is a strong predictor of poorer physical and mental health.

FIGURE 1

Adults in Households with Less Food Security Are Likelier to Have a Chronic Illness

Probability of any chronic illness



Note: “High food security” = household has access to nutritious food at all times during the year. “Marginal food security” = household is concerned about adequacy, but does not compromise quality or quantity, of their diet. “Low food security” = household reduces quality or quantity of their diet at some point during the year because of limited resources. “Very low food security” = one or more household members skip meals or otherwise eat less at some point during the year because they lack money.

Source: Christian A. Gregory and Alisha Coleman-Jensen, “Food Insecurity, Chronic Disease, and Health Among Working-Age Adults,” U.S. Department of Agriculture, July 2017. Adjusted for differences in demographic, socioeconomic and other characteristics. Sample includes working-age adults in households at or below 200% of the federal poverty level.

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Food Insecurity Is Associated with Cost-Related Medication Underuse

People facing food insecurity may put off attending to their health when faced with other pressing needs, choosing food over medication, postponing needed medical care, or forgoing special medical diets, for example. Taking less medication than prescribed, or not taking it at all, due to cost is a significant public health problem that affects the health and well-being of as many as 1 in 4 working-age adults.⁷

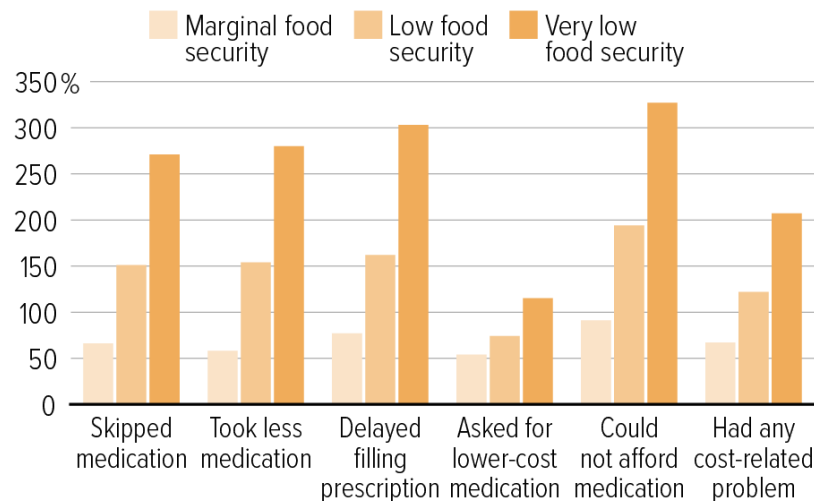
⁶ Christian A. Gregory and Alisha Coleman-Jensen, “Food Insecurity, Chronic Disease, and Health Among Working-Age Adults,” ERR-235, U.S. Department of Agriculture, Economic Research Service, July 2017, <https://www.ers.usda.gov/webdocs/publications/84467/err-235.pdf?v=42942>.

⁷ Dena Herman *et al.*, “Food Insecurity and Cost-Related Medication Underuse Among Nonelderly Adults in a Nationally Representative Sample,” *American Journal of Public Health*, 105(10):e48-e59, October 2015, <http://ajph.aphapublications.org/doi/full/10.2105/AJPH.2015.302712>

FIGURE 2

Adults in Households with Less Food Security Are Likelier to Skip Needed Medications

Percent more likely relative to food-secure households



Note: "Food secure" = household has access to nutritious food at all times during the year. "Marginal food security" = household is concerned about adequacy, but does not compromise quality or quantity, of their diet. "Low food security" = household reduces quality or quantity of their diet at some point during the year because of limited resources. "Very low food security" = one or more household members skip meals or otherwise eat less at some point during the year because they lack money.

Source: Dena Herman et al., "Food Insecurity and Cost-Related Medication Underuse Among Nonelderly Adults in a Nationally Representative Sample," *American Journal of Public Health*, October 2015. Adjusted for differences in demographic, socioeconomic and other characteristics. Sample includes adults age 18 to 64.

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Several recent studies based on large national surveys find that food insecurity is associated with the underuse of medication due to cost among working-age adults, including those with chronic disease and diabetes, and among the elderly. Among working-age adults, for example, those in food-insecure households are about four times more likely to have a problem with medication underuse because of cost than those in food-secure households, even after adjusting for differences in the characteristics of food-secure and food-insecure households. There is also evidence that cost-related medication underuse increases with the severity of food insecurity: compared to people in food-secure households, for example, people in households with marginal food security are about 50 percent more likely to have at least one cost-related medication problem, people in households with low food security are more than twice as likely, and those in households with very low food security, the most severe form, are more than three times as likely (see Figure 2).⁸

⁸ Seth A. Berkowitz, Hilary K. Seligman, and Niteesh K. Choudhry, "Treat or Eat: Food Insecurity, Cost-Related Medication Underuse, and Unmet Needs," *The American Journal of Medicine*, 127(4):303-310, 2014, [http://www.amjmed.com/article/S0002-9343\(14\)00030-8/fulltext](http://www.amjmed.com/article/S0002-9343(14)00030-8/fulltext); Herman et al. (2015); Chadwick K. Knight et al., "Household food insecurity and medication 'scrimping' among US adults with diabetes," *Preventive Medicine*, 83:41-45, February 2016, <http://www.sciencedirect.com/science/article/pii/S0091743515003679>; Minal R. Patel et al., "Social determinants of health, cost-related nonadherence, and cost-reducing behaviors among adults with diabetes: findings from the National Health Interview Survey," *Medical Care*, 54(8):796-803, August 2016, <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4945373/>; and

Food Insecurity Is Associated with Higher Health Care Use and Costs

Food insecurity imposes a substantial burden on society, including lost productivity and avoidable health care costs. While it may not be possible to create a precise estimate of food insecurity's economic burden, there is reason to think it could be substantial.⁹

Canadian researchers, for example, using linked survey and administrative data on 67,000 working-age adults in Ontario province, show that public health care expenditures are substantially higher for food-insecure people, even after adjusting for other socioeconomic and demographic characteristics that might affect either food security or costs. The findings are particularly compelling because the study occurred in the context of Canada's universal health care system, alleviating concerns that the observed differences are due to differences in access to health insurance. The researchers found that individuals in households with moderate food insecurity are a third more likely to use health care services — and expenses among these health care users are a third higher — than those in food-secure households. And as food insecurity increases, so do health care costs. Individuals in households with the most severe food insecurity are 71 percent more likely to use health care services, and the expenses of these health care users are 76 percent higher, than those in food-secure households (see Figure 3).¹⁰

Researchers in the United States, using national survey data to capture out-of-pocket expenses and insurance payments for two years after a household experiences food insecurity, found similar results. On average, again after adjusting for a range of socioeconomic and demographic characteristics expected to affect food security and spending on health care (both out-of-pocket and paid by insurance, including Medicare and Medicaid), people in food-insecure households spend roughly 45 percent more on medical costs in a year (\$6,100) than people in food-secure households (\$4,200). Annual health care costs are \$4,400 higher among those with diabetes, \$2,200 higher among those with hypertension, and \$5,100 higher among those with heart disease.¹¹

Patience Afulani *et al.*, “Food Insecurity and Health Outcomes Among Older Adults: The Role of Cost-Related Medication Underuse,” *Journal of Nutrition in Gerontology and Geriatrics*, 34(3):319-342, August 12, 2015, <http://www.tandfonline.com/doi/abs/10.1080/21551197.2015.1054575>.

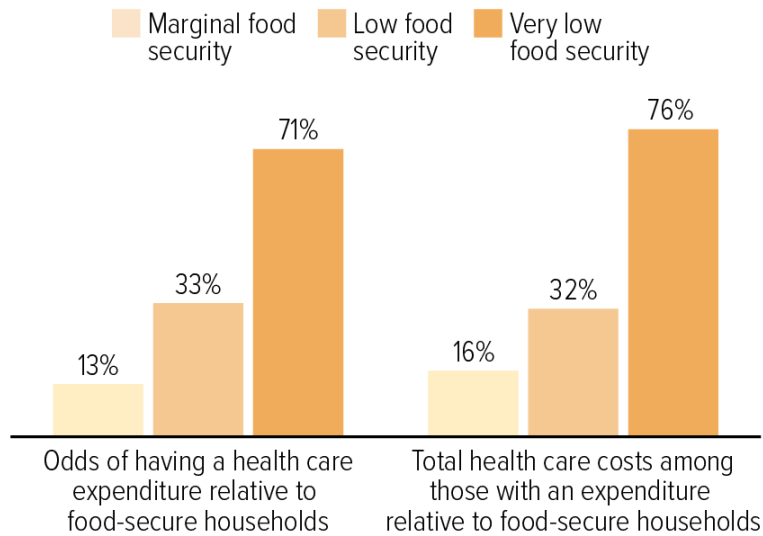
⁹ Though fraught with uncertainty, some estimates suggest the annual cost of poor health and illness associated with food insecurity in the United States is as much as \$160 billion. Donald S. Shepard, Elizabeth Setren, and Donna Cooper, “Hunger in America: Suffering We All Pay For,” Center for American Progress, October 2011, https://www.americanprogress.org/wp-content/uploads/issues/2011/10/pdf/hunger_paper.pdf; and John T. Cook and Ana Paula Poblacion, “Estimating the Health-Related Costs of Food Insecurity and Hunger,” in *The Nourishing Effect: Ending Hunger, Improving Health, Reducing Inequality*, Bread for the World, 2016, http://childrenshealthwatch.org/wp-content/uploads/JohnCook_cost_of_hunger_study.pdf.

¹⁰ Valerie Tarasuk *et al.*, “Association Between Household Food Insecurity and Annual Health Care Costs,” *Canadian Medical Association Journal*, 187(14):E429-E436, October 6, 2015, <http://www.cmaj.ca/content/187/14/E429>. Note that the Canadian classifications (marginal, moderate, and severe) differ from those used to monitor food security in the United States (food insecure and very low food security), though both measures are based on the same set of questions. The authors also report results that include prescription drug benefits. Because these benefits are available only to low-income households in Ontario, and having low income is a strong predictor of food insecurity, their inclusion is likely to confound (and probably overstate) the association between food insecurity and health care costs. As expected, the inclusion of prescription drug costs results in even greater differences in total health care expenditures: 49 percent higher for households with moderate food insecurity and 121 percent higher for those with severe food insecurity.

¹¹ Seth A. Berkowitz, Sanjay Basu, James B. Meigs, and Hillary K. Seligman, “Food Insecurity and Health Care Expenditures in the United States, 2011-2013,” *Health Services Research*, June 13, 2017, <http://onlinelibrary.wiley.com/doi/10.1111/1475-6773.12730/full>.

FIGURE 3

Adults in Households with More Food Insecurity Are Likelier to Use Health Care, Have Higher Costs



Note: "Marginal food security" = household reports at least one indication of difficulty with food access due to limited resources. "Low food security" = household reduces quality or quantity of their diet at some point during the year because of limited resources. "Very low food security" = one or more household members skip meals or otherwise eat less at some point during the year because they lack money. "Health care expenditures (costs)" = public expenditures on direct medical costs, excluding out-of-pocket and private insurance payments.

Source: Adapted from Valerie Tarasuk et al., "Association Between Household Food Insecurity and Annual Health Care Costs," Canadian Medical Association Journal, October 2015. Based on linked survey and administrative health care records for adults age 18 to 64, Ontario, Canada.

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While these results do not on their own fully establish a causal relationship, the study's longitudinal design provides evidence that exposure to food insecurity is likely to be associated with higher subsequent health care expenditures. But it is also worth noting that this study determined food security status only once based on a household's food-related problems in the 30 days before the survey interview took place. Food security, however, can change from month to month as a family's circumstances improve or worsen. As a result, the question of whether, and how much, the duration of exposure to food insecurity matters for subsequent health care expenditures remains unanswered.

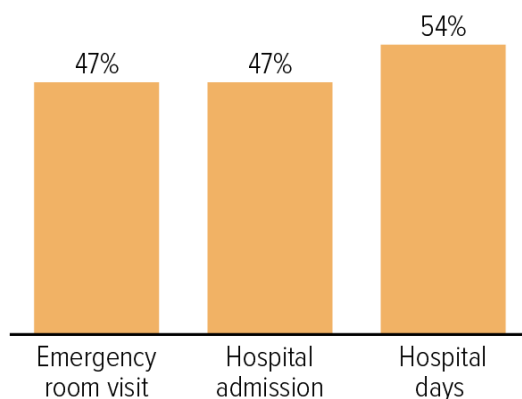
Food insecurity is also associated with greater use of health care services. Adults in food-insecure households are about 50 percent more likely to visit an emergency room and to be admitted to a hospital, and they stay hospitalized about 50 percent longer, than adults in food-secure households

(see Figure 4). Food-insecure seniors are more likely to make use of health care services, including office visits, overnight stays in a hospital, and emergency rooms, than food-secure seniors.¹²

FIGURE 4

Adults in Food-Insecure Households Have More Emergency Room Visits and Hospital Admissions

Percent more likely relative to food-secure households



Note: "Food-insecure" = lacking consistent access to nutritious food during the year due to limited resources.

Source: Seth Berkowitz, Hilary K. Seligman, and Sanjay Basu, "Impact of Food Insecurity and SNAP Participation on Healthcare Utilization and Expenditures," University of Kentucky Center for Poverty Research, 2017.

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SNAP Helps Put Food on the Table and Improves Food Security

SNAP serves a vital role in improving the health and well-being of low-income people by making adequate, nutritious food more affordable. Because SNAP enables low-income households to spend more on food than their limited budgets would otherwise allow, it makes it easier to put enough food on the table.

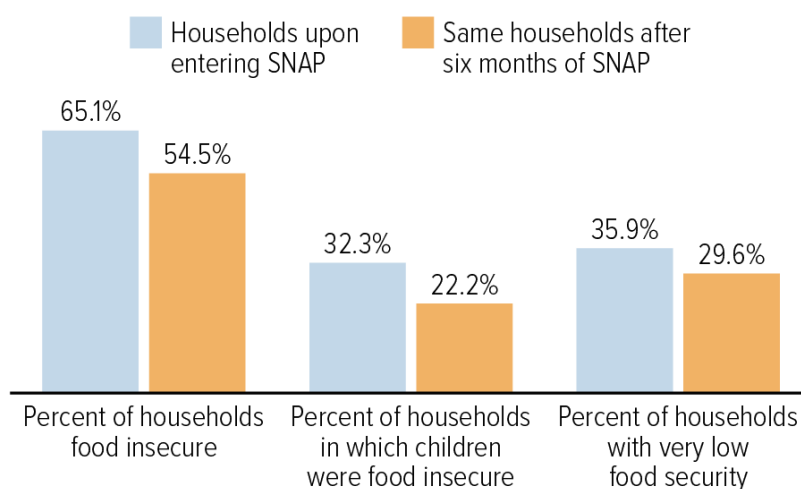
Estimating SNAP's impact on food security is challenging, however, because households that participate in SNAP differ in systematic ways from households that do not. Notably, one of the main tools researchers use to separate a program's impact from such differences — a randomized control trial — is generally not feasible as a way to measure the impact of SNAP because withholding nutrition benefits from a random sample of low-income individuals raises serious

¹² Seth Berkowitz, Hilary K. Seligman, and Sanjay Basu, "Impact of Food Insecurity and SNAP Participation on Healthcare Utilization and Expenditures," University of Kentucky Center for Poverty Research Discussion Paper Series, DP2017-02, 2017, http://uknowledge.uky.edu/cgi/viewcontent.cgi?article=1105&context=ukcpr_papers; and Vibha Bhargava and JungSun Lee, "Food Insecurity and Health Care Utilization Among Older Adults in the United States," *Journal of Nutrition in Gerontology and Geriatrics*, 35(3):177-192, 2016, <http://www.tandfonline.com/doi/abs/10.1080/21551197.2016.1200334>.

ethical questions (see Appendix). Failing to account for those differences can lead to misleading results. It is hard to imagine how, for example, giving a family more resources to buy food could reduce their food security as some early studies suggested. Recent evidence, based on studies with stronger research designs that support causal inferences, shows that SNAP participation does substantially improve food security. SNAP reduces the overall prevalence of food insecurity by as much as 30 percent, and is even more effective among the most vulnerable, such as children and those with very low food security.

FIGURE 5

SNAP Helps Families Afford Adequate Food



Note: "Food insecure" = household lacks consistent access to nutritious food at some point during the year because of limited resources. "Households in which children were food insecure" = households in which both children and adults experience food insecurity during the year. "Very low food security" = one or more household members have to skip meals or otherwise eat less at some point during the year because they lack money.

Source: Agriculture Department, "Measuring the Effect of Supplemental Nutrition Assistance Program (SNAP) Participation on Food Security," August 2013. This chart shows the results of a study that looked at longitudinal data comparing SNAP households upon beginning to receive SNAP, and six months after SNAP receipt.

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The largest and most rigorous examination of the relationship between SNAP participation and food security found that overall food insecurity fell by as much as one-fifth and that food insecurity among children fell by roughly a third after their families received SNAP benefits for six months (see Figure 5).¹³ Researchers at the Urban Institute used national survey data from the late 1990s and

¹³ James Mabli *et al.*, "Measuring the Effect of Supplemental Nutrition Assistance Program (SNAP) Participation on Food Security," Food and Nutrition Service, USDA, 2013, <https://www.fns.usda.gov/measuring-effect-snap-participation-food-security-0>. By following families as they enter SNAP, this study avoids two major pitfalls that would otherwise make it difficult to assert that participation is responsible for the improvements in food security. First, it avoids the problems associated with self-selection by using these families as their own controls, comparing their food security status just before entering SNAP to their status six months later (rather than to non-participants). Second, it avoids the problems associated with underreporting of SNAP participation in most surveys by drawing the sample of families directly from the records of local SNAP offices.

early to mid-2000s to show that SNAP reduces the likelihood of being food insecure by roughly 30 percent and the likelihood of being very food insecure by roughly 20 percent.¹⁴

There is also direct evidence that a more generous SNAP benefit would help many more families afford adequate food. USDA's large-scale randomized experiment to test the effects of providing additional benefits during summer months when children do not have access to free or reduced-price school meals produced striking results: the additional benefits reduced very low food security among children by about one-third.¹⁵ And food insecurity fell after the American Recovery and Reinvestment Act of 2009 temporarily increased SNAP benefits to help those affected most by the Great Recession.¹⁶

SNAP Is Associated with Improved Current and Long-Term Health Outcomes

Maintaining good health requires ongoing investment of both time and money. It can be difficult for low-income households to make those investments when faced with multiple demands on limited budgets. The food assistance offered by SNAP helps with a modest benefit that may nevertheless make it easier for individuals and families to afford healthier food. SNAP benefits also free up resources that can be used on health-promoting activities and preventive health care by reducing what families must spend out of pocket on food. And the addition of SNAP benefits to a household's budget may reduce the stress associated with not having enough money to feed a family. Some health impacts may be possible in the short term, but other potential health benefits may take longer to emerge and depend on how long families participate and the amount of benefits they receive.

While not conclusive, existing research suggests that SNAP may, in fact, offer a pathway to better health.

- **SNAP is associated with improvements in current health.** After adjusting for differences in demographic, socioeconomic, and other characteristics, adults who participate in SNAP are more likely to assess their own health as excellent or very good (see Figure 6), as are parents who assess their child's health. Adults have fewer sick days, make fewer visits to a doctor, are less likely to forgo needed care because they cannot afford it, and are less likely to exhibit psychological distress.¹⁷

¹⁴ Caroline Ratcliffe, Signe-Mary McKernan, and Sisi Zhang, "How Much Does the Supplemental Nutrition Assistance Program Reduce Food Insecurity?" *American Journal of Agricultural Economics*, 93(4):1082-1098, July 12, 2011, <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4154696/>.

¹⁵ The demonstration initially provided a monthly benefit of \$60 in the summer of 2012 and then compared the relative effectiveness of a smaller benefit (\$30) in the summers of 2013 and 2014. The results suggested that both increases produced similar improvements in the most severe form of food insecurity among children. See Ann Collins *et al.*, "Summer Electronic Benefits Transfer for Children (SEBTC) Demonstration: Summary Report," prepared for the Food and Nutrition Service, USDA, May 2016, <https://fns-prod.azuresedge.net/sites/default/files/ops/sebtfinalreport.pdf>.

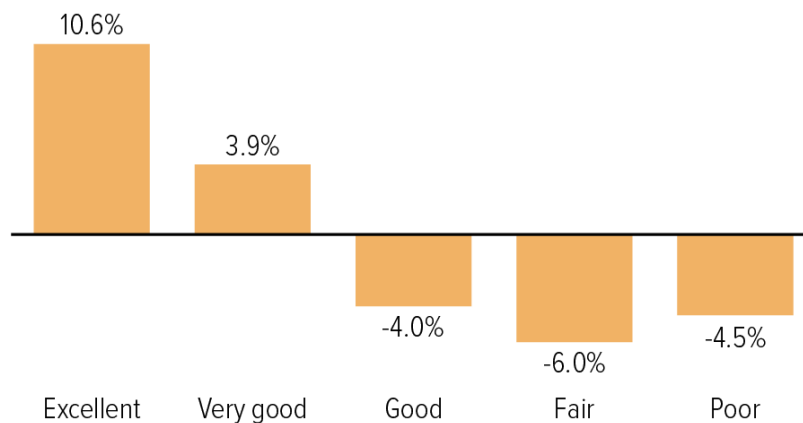
¹⁶ Mark Nord and Mark Prell, "Food Security Improved Following the 2009 ARRA Increase in SNAP Benefits," ERR-116, U.S. Department of Agriculture, Economic Research Service, April 2011, https://www.ers.usda.gov/webdocs/publications/44837/7469_err116.pdf?v=41056.

¹⁷ Christian A. Gregory and Partha Deb, "Does SNAP Improve Your Health?" *Food Policy*, 50:11-19, 2015, <http://www.sciencedirect.com/science/article/pii/S0306919214001419>; Daniel P. Miller and Taryn Morrissey, "Using Natural Experiments to Identify the Effects of SNAP on Child and Adult Health," University of Kentucky Center for Poverty Research, Discussion Paper Series, DP2017-04, January 2017,

FIGURE 6

SNAP Participants Report Better Health Than Eligible Non-Participants

Percent more or less likely to describe health as:



Source: Christian A. Gregory and Partha Deb, "Does SNAP Improve Your Health?" Food Policy, 2015. Adjusted for differences in demographic, socioeconomic and other characteristics. Sample includes adults aged 20 to 64 in households with income at or below 130% of the federal poverty level.

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Other researchers have shown that children receiving SNAP are less likely than low-income non-participants to be in fair or poor health or underweight, and their families are less likely to make tradeoffs between paying for health care and paying for other basic needs, like food, housing, heating, and electricity.¹⁸ Children who lose some or all of their SNAP benefits are more likely to have poor health and be food insecure compared to children in families that maintain benefits, and families that lose benefits are more likely to forgo medical care or make health care tradeoffs than families who consistently receive SNAP benefits.¹⁹

https://uknowledge.uky.edu/cgi/viewcontent.cgi?referer=&httpsredir=1&article=1103&context=ukqpr_papers; and Vanessa M. Oddo and James Mabli, "Participation in the Supplemental Nutrition Assistance Program and Psychological Distress," *American Journal of Public Health*, 6:e30-e35, June 2015,

<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4431109/pdf/AJPH.2014.302480.pdf>.

¹⁸ Katherine M. Joyce *et al.*, "Household Hardships, Public Programs, and Their Associations with the Health and Development of Very Young Children: Insights from Children's HealthWatch," *Journal of Applied Research on Children: Informing Policy for Children at Risk*, 3(1), 2012, http://www.childrenshealthwatch.org/wp-content/uploads/KJ_IARC_2012.pdf; and Stephanie Ettinger de Cuba *et al.*, "The SNAP Vaccine: Boosting Children's Health," Children's HealthWatch, February 1, 2012, <http://childrenshealthwatch.org/the-snap-vaccine-boosting-childrens-health/>.

¹⁹ Allison Bovell *et al.*, "Making SNAP Work for Families Leaving Poverty," Children's HealthWatch, November 2014, <http://www.childrenshealthwatch.org/wp-content/uploads/FINALPhilly-Food-to-web3.pdf>; and Stephanie Ettinger de Cuba *et al.*, "Punishing Hard Work: The Unintended Consequences of Cutting SNAP Benefits," Children's HealthWatch, December 12, 2013, <http://childrenshealthwatch.org/punishing-hard-work-the-unintended-consequences-of-cutting-snap-benefits/>.

- **Early access to SNAP can improve birth outcomes and long-term health.** Poor nutrition during childhood may harm health and earnings decades later by altering physical development and affecting the ability to learn. Researchers comparing the long-term outcomes of individuals in different areas of the country when SNAP expanded nationwide in the 1960s and early 1970s found that mothers exposed to SNAP during pregnancy gave birth to fewer low-birth-weight babies. Adults with access to food stamps in early childhood had lower risks of obesity and other conditions related to heart disease and diabetes (see Figure 7).²⁰

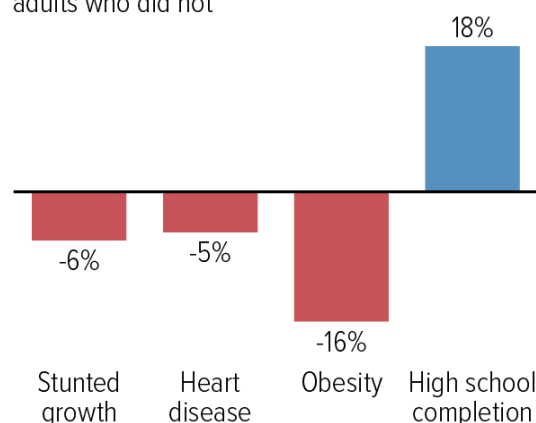
Similarly, the experience of children of immigrants during a period when eligibility rules changed for their parents offers evidence that an additional year of SNAP eligibility in early life (from in utero to age 4) is associated with improvements in health outcomes — parental reports of health status, overnight hospitalizations, and number of doctor’s visits — between ages 6 and 16, providing evidence that participating in the program as a young child can impact later-life health as soon as school age.²¹ While immigrants are a select and relatively more disadvantaged group of SNAP participants, children of immigrants account for about a quarter of all children and one-third of children in poverty. Welfare reform eliminated SNAP eligibility for most immigrant families in 1996, but subsequent federal legislation gradually restored eligibility between 1998 and 2003. These changes in eligibility allow for a relatively strong test of the impact of SNAP on children’s health, as the researchers compared health outcomes among children whose parents lost eligibility for SNAP when they were in early childhood to similar children whose parents remained eligible.

Families who live in areas with lower food prices — and who, therefore, have a somewhat easier time stretching their SNAP benefits to meet their food needs — may be more likely to take advantage of needed medical care. One study suggests, for example, that children who live in lower-cost areas (with higher SNAP purchasing power) are more likely to have had an

FIGURE 7

Children With Access to SNAP Fare Better Years Later

Percentage-point change in outcomes for adults who received SNAP as children, compared to adults who did not



Note: The study compared individuals who had access to SNAP (then food stamps) in early childhood after its introduction in the 1960s and early 1970s to similar children who did not (because they were born before its introduction) in each county.

Source: Hoynes, Schanzenbach, and Almond, “Long-Run Impacts of Childhood Access to the Safety Net,” *American Economic Review*, April 2016.

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²⁰ Douglas Almond, Hillary Hoynes, and Diane Schanzenbach, “Inside the War on Poverty: The Impact of Food Stamps on Birth Outcomes,” *The Review of Economics and Statistics*, 93(2), May 2011, https://www.mitpressjournals.org/doi/pdfplus/10.1162/REST_a_00089; and Hilary Hoynes, Diane Whitmore Schanzenbach, and Douglas Almond, “Long-Run Impacts of Childhood Access to the Safety Net,” *American Economic Review*, 106(4):903–934, April 2016, <https://pdfs.semanticscholar.org/c94b/26c57bb565b566913d2af161e555edeb7f21.pdf>. It is important to note that much has changed since the initial introduction of the Food Stamp Program, including the introduction and growth of the Special Supplemental Nutrition Program for Women, Infants, and Children, and Medicaid.

²¹ Chloe East, “The Effect of Food Stamps on Children’s Health: Evidence from Immigrants’ Changing Eligibility,” September 2, 2016, http://ceast.weebly.com/uploads/8/9/9/7/8997263/east_jmp.pdf.

annual check-up and may be less likely to delay or go without medical care due to its cost. Children who live in higher-cost areas (with lower SNAP purchasing power) miss more days of school due to illness.²²

SNAP Is Linked with Improved Medication Adherence

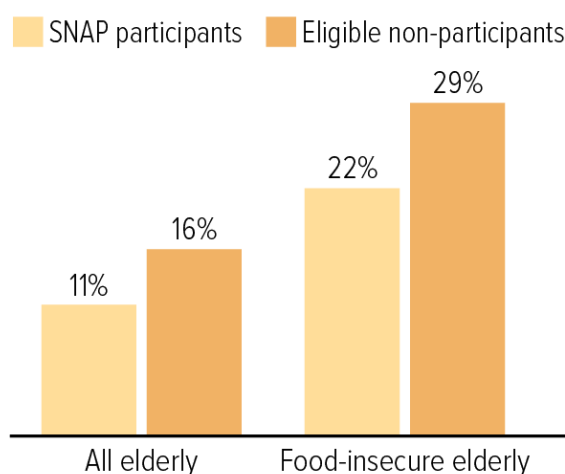
As noted above, people experiencing food insecurity are more likely to put off prescription refills, skip doses, or take less medication than prescribed to save money. SNAP may help participants adhere to medication regimens by improving their food security and reducing their out-of-pocket costs for food, freeing up resources that might be used for medication.

Few studies have considered — and only one has focused on — SNAP’s impact on cost-related medication underuse. An analysis of elderly SNAP participants concludes that they are 5 percentage points less likely to cut back on their medications because of cost than eligible non-participants, a 30 percent reduction. This difference persists even after controlling for differences in food security between the two groups: even among only those in food-insecure households, elderly SNAP participants are still 7 percentage points less likely to have cost-related medication problems than eligible non-participants, a reduction of 26 percent (see Figure 8).²³ These results suggest that increasing access to SNAP might make it easier for low-income seniors to afford their prescription medications.

FIGURE 8

Elderly SNAP Participants Less Likely to Skip Needed Medications

Percent who skip or stop medications, take smaller doses, or delay a prescription due to cost



Note: “Food insecure” = lacking consistent access to nutritious food during the year due to limited resources. Adjusted for differences in demographic, socioeconomic, health, and other characteristics.

Source: Mithuna Srinivasan and Jennifer A. Pooler, “Cost-Related Medication Nonadherence for Older Adults Participating in SNAP, 2013–2015,” *American Journal of Public Health*, December 2017

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SNAP Is Associated with Reduced Health Care Costs

Recognition of the importance of social determinants of health calls attention to programs, policies, and practices that shape the physical and social environments in which individuals live and work. Some of the most important opportunities for improving health and reducing health care spending may be found in economic and social resources that promote healthy living and working conditions and healthy choices. By increasing access to healthier food choices and reducing the

²² See Hilary Hoynes, Erin Brondhetti, and Garret Christensen, “The Real Value of SNAP Benefits and Health Outcomes,” University of Kentucky Center for Poverty Research, Discussion Paper Series, DP2017-03, January 2017, https://uknowledge.uky.edu/cgi/viewcontent.cgi?referer=&httpsredir=1&article=1104&context=ukcpr_papers.

²³ Mithuna Srinivasan and Jennifer A. Pooler, “Cost-Related Medication Nonadherence for Older Adults Participating in SNAP, 2013–2015,” *American Journal of Public Health*, published online December 21, 2017, <http://ajph.aphapublications.org/doi/10.2105/AJPH.2017.304176>.

stress of food insecurity, as well as by freeing up resources that participants can spend on their health, SNAP may be one such path to lower health care costs, the available evidence, while limited, suggests.

- **SNAP is associated with a reduced likelihood of nursing home and hospital admissions among low-income elderly participants, compared to low-income non-participating counterparts.** Researchers looking at over 60,000 low-income seniors in Maryland report that SNAP participants are 23 percent less likely to enter a nursing home and 4 percent less likely to be hospitalized in the year after receiving SNAP than non-participants. The amount of SNAP benefits received also matters: larger monthly benefits were associated with a further reduction in the odds of nursing home admissions, the length of stays among those admitted, and the likelihood of hospitalization (see Figure 9).²⁴

This research controls for several socioeconomic and demographic characteristics that could otherwise bias the results. Similarly, it also controls for chronic conditions and Medicaid participation, which would affect health and health care utilization outcomes. Despite this design, it may not fully account for unobserved differences between participants and non-participants. It is noteworthy, however, that analyses using the amount of SNAP benefits received rather than simply participation in the previous year reveals a similar relationship with the likelihood of nursing home and hospital admissions. Because that analysis is limited to those who participate in SNAP and looks only at differences in the amount received, it is not subject to the same concern about self-selection into the program.

More generally, research using longitudinal data that followed older adults for ten years suggests that food-insecure seniors who participate in SNAP are 46 percent less likely to be hospitalized than non-participating, low-income seniors. Even among food-secure seniors, participation is associated with an 18 percent reduction in the likelihood of hospitalization.²⁵

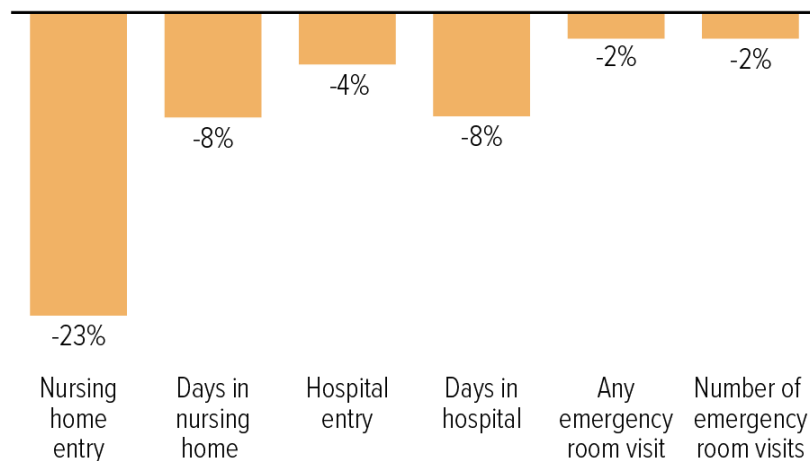
²⁴ See Sarah L. Szanton *et al.*, “Food assistance is associated with decreased nursing home admissions for Maryland’s dually eligible older adults,” *BMC Geriatrics*, 17(1), 162, 2017, <https://bmgeriatr.biomedcentral.com/articles/10.1186/s12877-017-0553-x>; and Laura J. Samuel *et al.*, “Does the Supplemental Nutrition Assistance Program Affect Hospital Utilization Among Older Adults? The Case of Maryland,” *Population Health Management*, 2017, http://www.bdtrust.org/wp-content/uploads/2017/07/Pop-Health-Mgmt_Hospitalizations_linked.pdf. In addition to controlling for demographic and socioeconomic factors that may contribute to differences between SNAP participants and non-participants, hospitalization findings include a control for proportion of time spent on Medicaid. As the authors note, SNAP participants are more likely to participate in Medicaid, and Medicaid participation is linked with hospital utilization. Models that do not include this control (but do control for demographic and socioeconomic factors) find that SNAP participants had a 14 percent lower probability of hospitalization compared to non-participants.

²⁵ J. Kim, “Are Older Adults Who Participate in the Supplemental Nutrition Assistance Program Healthier Than Eligible Nonparticipants? Evidence from the Health and Retirement Study,” *The Gerontologist*, 55 (Supplement Issue 2):672, November 1, 2015, https://academic.oup.com/gerontologist/article/55/Suppl_2/672/2489236.

FIGURE 9

Elderly SNAP Participants Are Less Likely to Use Health Care Services

Percent relative to low-income elderly non-participants



Source: Sarah L. Szanton et al., "Food assistance is associated with decreased nursing home admissions for Maryland's dually eligible older adults," *BMC Geriatrics*, July 2017; and Laura J. Samuel et al., "Does the Supplemental Nutrition Assistance Program Affect Hospital Utilization Among Older Adults? The Case of Maryland," *Population Health Management*, 2017. Adjusted for differences in demographic, socioeconomic and other characteristics. Results for hospital and emergency room visits adjust for proportion of the year on Medicaid. Sample includes adults age 65 and older eligible for both Medicare and Medicaid in Maryland.

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- SNAP participation is also linked with lower overall health care expenditures and Medicaid/Medicare costs.** An analysis of national data on overall health care expenditures links SNAP participation to lower health care costs. On average, after controlling for factors expected to affect spending on medical care, low-income adults participating in SNAP incur about \$1,400, or nearly 25 percent, less in medical care costs in a year, including those paid by private or public insurance, than non-participants (see Figure 10). The differences are even greater among those with hypertension (nearly \$2,700 less) and coronary heart disease (about \$4,100 less).²⁶ While the approach used to generate these results may not fully account for

²⁶ Seth A. Berkowitz et al., "Supplemental Nutrition Assistance Program (SNAP) Participation and Health Care Expenditures Among Low-Income Adults," *JAMA Internal Medicine*, November 2017, https://jamanetwork.com/journals/jamainternalmedicine/article-abstract/2653910?amp%3butm_source=JAMA+Intern+MedPublishAheadofPrint&utm_campaign=25-09-2017; and Seth Berkowitz, Hilary K. Seligman, and Sanjay Basu, "Impact of Food Insecurity and SNAP Participation on Healthcare Utilization and Expenditures," University of Kentucky Center for Poverty Research Discussion Paper Series, DP2017-02, 2017, http://uknowledge.uky.edu/cgi/viewcontent.cgi?article=1105&context=ukcpr_papers. An older study that did not fully account for unobserved differences did not find a significant difference in Medicare spending among low-income elderly with diabetes between those who received food stamps (as SNAP was known then) and those who did not. See Lauren H. Nicholas, "Can Food Stamps Help to Reduce Medicare Spending on Diabetes?" *Economics and Human Biology*, 9(1):1-13, January 2011, <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC3032985/>. Still older work using a different data set and analytic approach concluded that SNAP increased medical expenses of nonelderly women by \$300 to \$700 per year, mainly because program benefits freed up additional discretionary income that could be spent on medical care. See Chad D. Meyerhoefer and Yuri Pylypchuk, "Does Participation in the Food Stamp Program Increase the Prevalence of Obesity and Health Care Spending?" *American Journal of Agricultural Economics*, 90(2):287-305, May 1, 2008, <https://academic.oup.com/ajae/article-abstract/90/2/287/99451>.

unobserved differences between SNAP participants and non-participants, two alternative analyses that mitigate potential for bias due to self-selection into SNAP also demonstrate reduced health care expenditures — of as much as \$5,000 — associated with SNAP participation. None of these approaches, however, accounts for either the duration or amount of benefits received.

Conclusion

The research reviewed here suggests that policies to improve the food security of low-income individuals and families and increase their access to SNAP might reverberate across a variety of health outcomes. Food insecurity is strongly associated with increases in the risk of adverse health outcomes, may complicate the ability to manage illness, and is linked to higher health care costs. SNAP improves food security, offers benefits that enable families to purchase healthier diets, and frees up resources that can be used for health-promoting activities and needed medical care. Policies that address food insecurity, SNAP participation, and benefits could improve health, reduce health disparities, and lower costs.

States should be especially interested in the possibility that enrolling low-income eligible individuals and families in SNAP might help reduce health care costs. States and the federal government share the cost of providing health care coverage for low-income individuals and families through Medicaid. The state share of Medicaid costs ranges from about 25 percent to 50 percent, with poorer states paying less. Overall, Medicaid accounts for roughly a quarter of total state expenditures. In contrast, the federal government is responsible for the entire cost of SNAP benefits.²⁷ As a result, changes in policies that improve access to SNAP's benefits could provide short- and long-term relief to state Medicaid budgets.

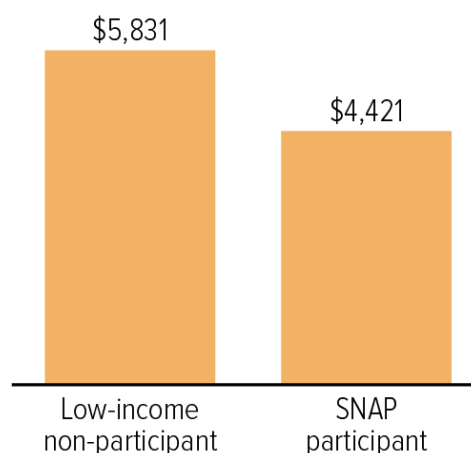
While the existing evidence has advanced understanding of the relationship between SNAP, food security, and health, important gaps remain. High-quality studies with rigorous designs that adequately address issues of self-selection, underreporting of program participation, and the direction of causation are relatively scarce. Additional investment in future research efforts is needed to:

²⁷ While states are responsible for half of the administrative cost of eligibility determinations, employment and training, nutrition education, and anti-fraud activities, these costs represent a small part of SNAP's overall costs. In fiscal year 2016, the federal government spent about \$73 billion on SNAP. Ninety-three percent went directly to benefits that households used to purchase food, and 6.5 percent went to state administrative costs.

FIGURE 10

A SNAP Participant Incurs \$1,400 Less for Health Care

Estimated annual per-person health care spending



Note: Health care spending includes out-of-pocket expenses and costs paid by private and public insurance, including Medicare and Medicaid.

Source: Seth Berkowitz, Hilary K., Seligman, and Sanjay Basu, "Impact of Food Insecurity and SNAP Participation on Healthcare Utilization and Expenditures," University of Kentucky Center for Poverty Research, 2017.

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- Confirm the direction and determine the strength of the causal relationship among: 1) food insecurity, SNAP participation, and health status; 2) use of health care services; and 3) health care costs;
- Identify and understand the ways that food insecurity and SNAP participation could lead to changes in health status, use of services, and costs; and
- Determine the extent to which factors such as the duration of exposure to food insecurity or the amount of SNAP benefits received affects health status, use of services, and costs.

Appendix

The modern form of SNAP (formerly, the Food Stamp Program) has been a widely available and deeply entrenched part of the nation's safety net for low-income Americans for 40 years. Key features of the program — including a funding structure that makes benefits available to anyone who qualifies under the program's rules without restrictions to certain categories of low-income individuals — are critical to its success. These same features make the task of evaluating its impacts more difficult.

The gold standard for evaluating the effectiveness of a program or intervention is the randomized control trial, which is designed to minimize the risk that factors unrelated to the program influence the results. If people are randomly assigned to a program, researchers can accurately estimate its impact as the difference in outcomes between those who participate and those who do not because they were randomly selected from the same population, lived through the same shifting programmatic, economic, and social conditions, and differ only in their program experience. However, randomized control trials are generally not feasible to evaluate SNAP's effectiveness, in part because the program is so widely available, and because withholding nutrition benefits from a random sample of low-income individuals would raise serious ethical questions.

As a result, estimating the effect of SNAP on health, health care utilization, and costs presents a daunting challenge. In addition to disentangling and isolating SNAP participation and benefits from the complex interactions of other social and economic factors, the physical environment, and individual behaviors that determine health, researchers must wrestle with three thorny issues: selection bias, measurement error, and causation.

Selection Bias. In the absence of a randomized control trial, research results may be biased by the self-selection of low-income individuals and families into SNAP. Households that participate in SNAP may differ in systematic ways from households that do not. If those who choose to enroll in SNAP are more able, more motivated, healthier, or have access to better health care than those who do not, then selection bias may lead researchers to mistakenly conclude that SNAP is more effective than it really is. But if those who enroll in SNAP are more disadvantaged, more food insecure, less healthy, or exhibit more risky behaviors than those who do not, then selection bias may lead to conclusions that SNAP is *less* effective than it really is. Most analyses routinely control for some potential differences, but others — local attitudes toward health and program participation, for example — are usually unobserved and not measured.

The research studies reviewed here have used a variety of data sources and empirical methods, some more rigorous than others, to isolate SNAP's impact on food insecurity and health from the compositional differences between participants and non-participants. There is little reason to put too

much confidence in the results of any single study. The consistency of results across studies, however, adds considerable weight to the validity of their collective conclusion that SNAP improves food security.

In addition, most of the available evidence suggests that selection bias is more likely to lead to *underestimates* of SNAP's beneficial impacts because participants are more disadvantaged and prone to adverse health outcomes than non-participants. National survey data show that SNAP participants are much less likely to be in excellent or very good health; more likely to be disabled or have a family member with functional limitations; more likely to be diagnosed with attention deficit disorder, asthma, diabetes, back problems, stroke, heart attack, and ulcer; and much more likely to have contact with the medical system but less likely to be able to afford care. When compared to low-income non-participants, SNAP participants are more likely to report worse health, more sick days, and greater use of health care *before* entering the program than non-participants. SNAP participants are also about a third more likely to die from any cause than low-income non-participants.²⁸

Measurement Error. A second issue arises because SNAP participation is underreported in most household surveys. As many as half of SNAP recipients fail to report their participation in the program in large-scale national surveys, while virtually none mistakenly report participating when they do not. An example illustrates why this matters. Imagine that we can correctly identify a group of 1,000 people eligible for SNAP. Among this group, we know that 800 receive SNAP benefits and 200 do not, consistent with current estimates of SNAP participation rates. We then survey each of them and ask whether they receive SNAP. If half of those who truly participate mistakenly report that they do not, we would count only 400 participants (assuming none of those who did not participate mistakenly report that they did). The other 400 would appear in the survey to be non-participants. Thus, there would be twice as many *participants* as non-participants in the comparison group of “non-participants,” effectively masking any impact SNAP participation might have on outcomes of interest. Few research studies, and none included in this review, have attempted to account for the measurement error introduced by underreporting of program participation.²⁹

Causation. An extensive body of research reveals a strong *correlation* between food insecurity, and to a lesser extent SNAP, and health. With few exceptions, however, much of this research is based on study designs that make it difficult to determine *causation*. Ideally, we would like to know whether food insecurity causes poor health, whether poor health leads to food insecurity, or whether there are unobserved factors that jointly influence both food insecurity and health. It is hard to draw definitive conclusions about SNAP's impact on health through its positive impact on food security absent a strongly established causal link from food insecurity to health.

²⁸ Marianne P. Bitler, “The Health and Nutrition Effects of SNAP: Selection into the Program and a Review of the Literature on Its Effects,” in *SNAP Matters: How Food Stamps Affect Health and Well-Being*, 2016, http://www.ukqpr.org/sites/www.ukqpr.org/files/documents/DP2014-02_0.pdf; Gregory and Deb (2015); and Zach Conrad, Colin D. Rehm, Parke Wilde, and Dariush Mozaffarian, “Cardiometabolic Mortality by Supplemental Nutrition Assistance Program Participation and Eligibility in the United States,” *American Journal of Public Health*, 107(3):466-474, March 2017, <https://www.ncbi.nlm.nih.gov/pubmed/28103061>.

²⁹ An exception in the literature is Brent Kreider *et al.*, “Identifying the Effects of SNAP (Food Stamps) on Child Health Outcomes When Participation Is Endogenous and Misreported,” *Journal of the American Statistical Association*, 107(499):958-975, September 2012, [http://batten.virginia.edu/sites/default/files/research/attachments/JASA_KPGJ_online\(1\).pdf](http://batten.virginia.edu/sites/default/files/research/attachments/JASA_KPGJ_online(1).pdf). Using relatively strong but plausible assumptions to address self-selection and classification errors simultaneously, the authors estimate that SNAP reduces the prevalence of child food insecurity by at least 8 percentage points.

Exhibit 33

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

Proposed Changes to “Public Charge” Policies for Immigrants: Implications for Health Coverage

Published: Sep 24, 2018



Key Takeaways

On September 22, 2018, the Trump Administration announced a proposed rule that would make changes to “public charge” policies. Under longstanding policy, the federal government can deny an individual entry into the U.S. or adjustment to legal permanent resident (LPR) status (i.e., a green card) if he or she is determined likely to become a public charge.

- Under the proposed rule, officials would newly consider use of certain previously excluded programs, including Medicaid, the Supplemental Nutrition Assistance Program, the Medicare Part D Low-Income Subsidy Program, and several housing programs, in public charge determinations.
- The changes would likely lead to broad decreases in participation in Medicaid and other programs among legal immigrant families and their primarily U.S.-born children beyond those directly affected by the changes. Nationwide, over 19 million or one in four (25%) children live in a family with an immigrant parent, and nearly nine in ten (86%) of these children are citizens.
- Decreased participation in these programs would contribute to more uninsured individuals and negatively affect the health and financial stability of families and the growth and healthy development of their children.

Introduction

On September 22, 2018, the Trump Administration announced a [proposed rule](https://www.dhs.gov/news/2018/09/22/dhs-announces-new-proposed-immigration-rule-enforce-long-standing-law-promotes-self) (<https://www.dhs.gov/news/2018/09/22/dhs-announces-new-proposed-immigration-rule-enforce-long-standing-law-promotes-self>) to make changes to “public charge” policies that govern how the use of public benefits may affect individuals’ ability to enter the U.S. or adjust to legal permanent resident (LPR) status (i.e., obtain a “green card”). The preamble to the proposed rule indicates that its primary goal is to ensure that individuals who apply for admission to the U.S. or for adjustment of status are self-sufficient. The preamble also identifies a range of consequences on the health and financial stability of families as well direct and indirect costs associated with

the rule. This fact sheet provides an overview of the proposed rule and its implications for health and health coverage of legal immigrant families and their predominantly U.S.-born children.

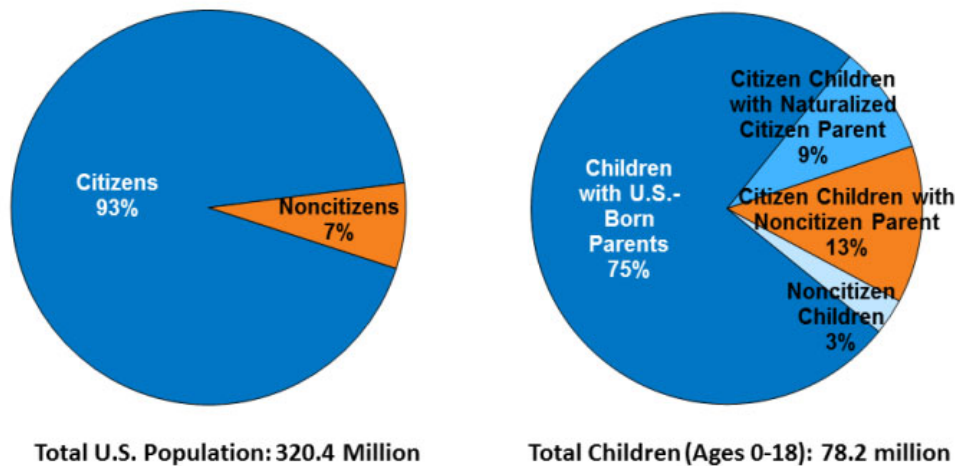
Whom Would the Proposed Changes Affect?

The proposed changes to public charge policies would primarily affect immigrants who are applying for a green card through a family-based petition. The proposed rule would affect lawfully present immigrants seeking to become LPRs or “green card” holders and individuals seeking to immigrate to the U.S.¹ Most individuals seeking to adjust to LPR status or to immigrate to the U.S. are immediate relatives of U.S. citizens or have a family-based sponsor. In 2016, 1.2 million individuals obtained LPR status, including over half a million who were already present in the U.S.² Some immigrants, including refugees and asylees, remain exempt from public charge determinations under law. Public charge policies do not apply to LPRs seeking to obtain citizenship. However, obtaining LPR status is a key step toward citizenship for immigrants seeking naturalization.

The proposed rule would likely increase confusion and fear among all legal immigrant families about using public programs for themselves and their children, regardless of whether they are directly affected by the policy changes. In 2016, there were 23 million noncitizens residing in the U.S. About six in ten noncitizens were lawfully present immigrants, who include LPRs, refugees, asylees, and other individuals who are authorized to live in the U.S.³ In addition, over 19 million or one in four (25%) children live with an immigrant parent, and nearly nine in ten (86%) of these children are citizens (Figure 1).⁴

Figure 1

Total Population and Children by Citizenship and Parents' Citizenship Status, 2016



SOURCE: Kaiser Family Foundation analysis of March 2017 Current Population Survey, Annual Social and Economic Supplement



Figure 1: Total Population and Children by Citizenship and Parents' Citizenship Status, 2016

What are the Key Changes in the Proposed Rule?

Under longstanding policy, if authorities determine that an individual is “likely to become a public charge,” they may deny that person’s application for lawful permanent residence or their entry into the U.S.⁵ Certain immigrants, including refugees and asylees, are exempt from public charge determinations under law. In making a public charge determination, officials must consider the totality of the person’s circumstances, including, at minimum, the individual’s age; health; family status; assets, resources, and financial status; and education and skills.

Under previous policy clarified in 1999, the federal government specified that it would not consider use of Medicaid, the Children’s Health Insurance Program (CHIP), or other non-cash benefits in public charge determinations. Historically, there has been confusion about whether use of Medicaid, CHIP, or other non-cash programs applies in public charge determinations.⁶ In 1999, the Immigration and Naturalization Service (now part of the Department of Homeland Security (DHS)) issued guidance that defined a public charge as someone who has become or who is likely to become “primarily dependent on the government for subsistence, as demonstrated by either the receipt of public cash assistance for income maintenance or institutionalization for long-term care at government expense.”⁷ The guidance specified that the federal government would not consider use of Medicaid, CHIP or other supportive programs in public charge determinations, with the exception of use of

Medicaid for long-term institutional care.⁸ The guidance noted that this clarification was necessary because ongoing confusion about public charge policies “deterred eligible aliens and their families, including U.S. citizen children, from seeking important health and nutrition benefits that they are legally entitled to receive. This reluctance to access benefits has an adverse impact not just on the potential recipients, but on public health and the general welfare.”⁹

The proposed rule would broaden the programs that the federal government would consider in public charge determinations to include previously excluded health, nutrition, and housing programs. The proposed rule would redefine public charge as an “alien who receives one or more public benefits” and would define public benefits to include cash assistance for income maintenance, government-funded institutionalized long-term care and certain health, nutrition, and housing programs that were previously excluded from public charge determinations. These programs would include non-emergency Medicaid, the Medicare Part D Low-Income Subsidy Program, the Supplemental Nutrition Assistance Program (SNAP), and several housing programs (see Appendix Table 1). The proposed rule does not include CHIP or subsidies for Affordable Care Act Marketplace coverage as public benefits. DHS specifically requests public comment on whether to include CHIP as a public benefit.

The proposed rule would establish thresholds for use of public benefits to determine an individual to be a public charge. These thresholds would be tied to the value of the benefits received and/or total months a benefit was received. Specifically, for benefits that have a cash value or that can be translated into a cash value (e.g., cash assistance, SNAP, housing vouchers, or rental assistance), the threshold would be 15% of the federal poverty level (FPL) for a single person in a 12-month period (\$1,821 as of 2018). For benefits that cannot be translated into a cash value (e.g., Medicaid and public housing), the threshold would be receipt of the benefit for 12 months within a 36-month period or 9 months if an individual receives both types of benefits (i.e., those with a cash value and those without a cash value). Public charge determinations would only consider the value of the benefit going toward the individual and would not take into account benefits received by family members. Public charge determinations would not consider receipt of benefits by active duty or reserve service members or their spouses or children.

The federal government would consider current or recent receipt of public benefits as a heavily weighted negative factor in an individual's public charge determination. As noted, public charge determinations consider the likelihood of an individual becoming a public charge in the future, based on a totality of circumstances. The proposed rule specifies certain factors that immigration officials would consider as positive factors that would decrease the likelihood of an individual becoming a public charge as well as negative factors that would increase the likelihood of someone becoming a public charge. Under the proposed rule, officials would consider current receipt or approval for receipt of a public benefit or receipt of a public benefit within the previous 36 months as a heavily weighted negative factor. Other heavily weighted negative factors related to health and health coverage include having a

medical condition that is likely to require extensive treatment or institutionalization and being uninsured and lacking the financial resources to pay for the medical costs associated with the condition.

The proposed rule also outlines policies related to consideration of income as part of public charge determinations. Specifically, DHS would consider whether the individual has annual gross household income of at least 125% of the FPL (\$25,975 for a family of three in 2018) and would require specified levels of household assets and resources if the individual has income below that level. Conversely, having income above 250% of the FPL (\$51,950 for a family of three in 2018) would be considered as a heavily weighted positive factor in public charge determinations.

The proposed rule also makes changes related to use of public charge bonds. It outlines policies related to use of public charge bonds that would allow an individual to adjust status if found inadmissible based on a public charge determination and sets the minimum amount of these bonds at \$10,000.

What are the Implications for Health & Health Coverage?

Today, Medicaid fills gaps in private coverage for lawfully present immigrants, providing them access to needed care and financial protections that support their ability to work and care for their children. Medicaid provides families access to preventive and primary care, including prenatal care, as well as care for chronic conditions. In addition, the coverage provides families financial protection from high medical costs. By enabling families to meet their health care needs, Medicaid supports families' ability to work and care for their children. The majority of lawfully present immigrants live in a family with at least one full-time worker (83%), a rate equal to that of citizens.¹⁰ However, lawfully present immigrants are more likely than citizens to live in low-income families and often work in jobs and industries that do not offer health coverage. Reflecting their lower incomes and limited access to private coverage, one in four nonelderly lawfully present immigrants has Medicaid or CHIP coverage.¹¹ However among the low-income nonelderly population, lawfully present immigrants are less likely than citizens to have Medicaid or CHIP. These lower coverage rates reflect eligibility restrictions for immigrants (<https://www.kff.org/disparities-policy/fact-sheet/health-coverage-of-immigrants/>) that require many otherwise eligible lawfully present immigrants to wait five years after obtaining lawful status before they may enroll as well as barriers to enrollment for eligible immigrants, including fear.¹²

The proposed changes would likely lead to broad declines in participation in Medicaid and other programs among immigrant families, including their primarily U.S.-born children. Despite efforts to assure families that Medicaid and CHIP could not be used in public charge determinations under previous policy, many eligible immigrants did not enroll themselves or their children because they feared that it could negatively affect their status. The proposed rule would amplify these fears, which would likely lead to broad falloffs in participation in Medicaid and other programs among a broader group of individuals than those directly impacted by the policy change. Previous research shows that welfare reform

had this chilling effect on immigrant families' participation in public programs and suggests that the proposed rule would likely lead to declines in Medicaid/CHIP enrollment among citizen children with a noncitizen parent.¹³ Analysis (https://www.kff.org/report-section/potential-effects-of-public-charge-changes-on-health-coverage-for-citizen-children-issue-brief/#endnote_link_257512-5) shows that, if Medicaid/CHIP disenrollment rates range from 15% to 35%, an estimated 875,000 to 2 million citizen children with a noncitizen parent could drop Medicaid/CHIP coverage despite remaining eligible. Other recent analysis (<https://www.kff.org/disparities-policy/issue-brief/family-consequences-of-detention-deportation-effects-on-finances-health-and-well-being/>) shows that, prior to announcement of the proposed rule, families were already experiencing growing fears of participation in health, nutrition, and other programs that led them to disenroll or avoid enrolling themselves and their children. The preamble to the proposed rule recognizes that the rule may lead to disenrollment or foregone enrollment in public benefit programs among foreign-born noncitizens as well as U.S. citizens who are members of mixed status households.

Decreased participation in Medicaid/CHIP would increase the uninsured rate among immigrant families, negatively affecting their health and financial stability. Coverage losses would reduce access to care for families, contributing to worse health outcomes. Reduced participation in nutrition and other programs would likely compound these effects. Overall, reduced participation in Medicaid and other programs would negatively affect their health and financial stability and the growth and healthy development of their children, who are predominantly U.S.-born. The preamble to the proposed rule recognizes these and other anticipated impacts. It notes that disenrollment or foregone enrollment in public benefit programs could lead to worse health outcomes, especially for pregnant or breastfeeding women, infants, or children; reduced prescription adherence; increased emergency room use and emergent care due to delayed treatment; increased prevalence of diseases; increased uncompensated care; increased rates of poverty and housing instability; and reduced productivity and educational attainment. Moreover, the preamble to the proposed rule indicates that DHS has determined that the rule may decrease disposable income and increase poverty of certain families and children, including U.S. citizen children. The preamble to the proposed rule also identifies potential impacts on communities, including decreased revenues to health care providers, pharmacies, grocery retailers, agricultural producers and landlords, as well as new direct and indirect costs for individuals and organizations serving immigrant families.

Next Steps

After the proposed rule is officially published in the Federal Register, there will be a 60-day public comment period. Following that comment period and DHS's consideration of the comments, it would issue final regulations and an effective date for the rule. The rule would not be retroactive, meaning that DHS would not consider an individual's use of the previously excluded health, nutrition, and housing programs prior to the effective date established in the final rule in public charge determinations. Substantial outreach efforts would be needed to educate families and stakeholders about the policy changes, which DHS notes will require time and costs for agencies, health care providers, and other individuals and organizations.

Appendix Table 1: Key Differences between Previous and Proposed “Public Charge” Policies

	Policy Based on 1999 Guidance	<u>Proposed Rule</u> (https://www.dhs.gov/news/2018/09/22/dhs-announces-new-proposed-immigration-rule-enforce-long-standing-law-promotes-self) Announced September 22, 2018
Definition of Public Charge	An alien who has become or who is likely to become “primarily dependent on the government for subsistence, as demonstrated by either the receipt of public cash assistance for income maintenance or institutionalization for long-term care at government expense.”	Public charge means an alien who receives one or more public benefits.
Public Benefits that May Be Considered for Public Charge Purposes	• SSI • TANF • State/local cash assistance programs • Public assistance for long-term care in an institution (including Medicaid)	• SSI • TANF • Federal, state, or local cash assistance programs • SNAP • Section 8 Housing Voucher Program • Section 8 Rental Assistance • Medicaid (except for emergency Medicaid, certain disability services related to education, and benefits received by foreign-born children of U.S. citizen parents who will be automatically eligible to become citizens) • Medicare Part D Low-Income Subsidy Program • Institutionalized long-term care at government expense • Subsidized public housing DHS requests public comment on whether to include CHIP as a public benefit.
Consideration of Use of Public Benefits in in	• May take into consideration past and current receipt of cash public assistance for income maintenance or	• For cash benefits and benefits that can be translated to a cash value, the benefit exceeds 15% of

a Public Charge Determination	institutionalized long-term care. • No weight should be placed on receipt of non-cash benefits or receipt of cash benefits for purposes other than income maintenance. • Cash benefits received by children or other family members should not be attributed to the individual, unless the family member's benefits are the family's sole source of support.	the FPL for a household of one within a 12-month period. • For benefits that cannot be translated into a cash value, receipt of any benefit for more than 12 months within a 36-month period • When an individual receives both types of benefits, benefits that cannot be translated into a cash value are received for more than 9 months within a 36-month period • Would not consider benefits received by active duty or reserve service members and their families. • Would only consider benefits going to the individual, not those received by family members
Heavily Weighted Negative Factors	Not Specified	• Not a full-time student and is authorized to work, but is unable to demonstrate employment. • Currently receiving or approved to receive one or more public benefits. • Has received one or more public benefits within the prior 36 months. • Has a medical condition that requires extensive treatment or institutionalization and is uninsured and does not have sufficient resources to pay for medical costs related to the condition. • Previously found inadmissible or deportable on public charge grounds.
Heavily Weighted Positive Factors	Not Specified	• Household has financial assets/resources of at least 250% of the FPL

-
	• Authorized to work or employed with an income of at least 250% of the FPL
-

Endnotes

1. The proposed changes would also affect certain people seeking to extend or adjust their non-immigrant status while in the U.S. The preamble also notes that the proposed rule interprets public charge as it relates to inadmissibility, but not public charge deportability grounds, which will continue to be governed by Department of Justice precedent decisions.
[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-1\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-1)
2. "Table 6. Persons Obtaining Lawful Permanent Resident Status by Type and Major Class of Admission: Fiscal Years 2014 to 2016," *2016 Yearbook of Immigration Statistics*, Department of Homeland Security, <https://www.dhs.gov/immigration-statistics/yearbook/2016/table6> (<https://www.dhs.gov/immigration-statistics/yearbook/2016/table6>), accessed February 12, 2018.
[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-2\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-2)
3. Kaiser Family Foundation analysis of the March 2017 Current Population Survey, Annual Social and Economic Supplement.
[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-3\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-3)
4. Kaiser Family Foundation analysis of the March 2017 Current Population Survey, Annual Social and Economic Supplement.
[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-4\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-4)
5. Becoming a public charge may also be a basis for deportation in extremely limited circumstances. "Public Charge Fact Sheet," U.S. Citizenship and Immigration Services, <https://www.uscis.gov/news/fact-sheets/public-charge-fact-sheet> (<https://www.uscis.gov/news/fact-sheets/public-charge-fact-sheet>), accessed February 12, 2018.
[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-5\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-5)

6. This confusion increased after new Medicaid and CHIP eligibility restrictions were imposed on immigrants in 1996. Those restrictions required many lawfully present immigrants to wait five years after obtaining lawful status before they could enroll in Medicaid or CHIP and made some lawfully present immigrants ineligible for coverage. However, they did not change public charge policy.

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7. “Field Guidance on Deportability and Inadmissibility on Public Charge Grounds,” Immigration and Naturalization Service, Justice, 64 Fed. Reg. 28689-28693 (March 26, 1999), <https://www.gpo.gov/fdsys/pkg/FR-1999-05-26/pdf/99-13202.pdf> (<https://www.gpo.gov/fdsys/pkg/FR-1999-05-26/pdf/99-13202.pdf>).

[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-7\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-7)

8. Ibid.

[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-8\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-8)

9. Ibid.

[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-9\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-9)

10. Kaiser Family Foundation analysis of the March 2017 Current Population Survey, Annual Social and Economic Supplement.

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11. Ibid.

[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-11\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-11)

12. Ibid.

[← Return to text \(https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-12\)](https://www.kff.org/disparities-policy/fact-sheet/proposed-changes-to-public-charge-policies-for-immigrants-implications-for-health-coverage/#endnote_link_274689-12)

13. Neeraj Kaushal and Robert Kaestner, “Welfare Reform and Health Insurance of Immigrants,” Health Services Research, 40(3), (June 2005), <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1361164/> (<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1361164/>); Michael Fix and Jeffrey Passel, “Trends in Noncitizens’ and Citizens’ Use of Public Benefits Following Welfare Reform 1994-97” (Washington, DC: The Urban Institute, March 1, 1999) <https://www.urban.org/sites/default/files/publication/69781/408086-Trends-in-Noncitizens-and-Citizens-Use-of-Public-Benefits-Following-Welfare-Reform.pdf> (<https://www.urban.org/sites/default/files/publication/69781/408086-Trends-in-Noncitizens-and-Citizens-Use-of-Public-Benefits-Following-Welfare-Reform.pdf>); Namratha R. Kandula, et. al,

“The Unintended Impact of Welfare Reform on the Medicaid Enrollment of Eligible Immigrants,” Health Services Research, 39(5), (October 2004), <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1361081/> (<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1361081/>); Rachel Benson Gold, “Immigrants and Medicaid After Welfare Reform,” (Washington, DC: The Guttmacher Institute, May 1, 2003), <https://www.guttmacher.org/gpr/2003/05/immigrants-and-medicaid-after-welfare-reform> (<https://www.guttmacher.org/gpr/2003/05/immigrants-and-medicaid-after-welfare-reform>).
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Filling the need for trusted information on national health issues, the Kaiser Family Foundation is a nonprofit organization based in Menlo Park, California.

Exhibit 34

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds



Uninsurance among Parents, 1997-2014

Long-Term Trends and Recent Patterns

Michael Karpman, Jason A. Gates, Genevieve M. Kenney, and Stacey McMorrow

June 2016

The key health insurance coverage provisions of the Affordable Care Act (ACA) were designed to reduce uninsurance primarily among nonelderly adults, including parents living with dependent children. In the years leading up to the ACA's passage in 2010, the share of adults without coverage had been rising steadily as uninsurance among children had been falling (Rosenbaum and Kenney 2014). Early estimates from the Urban Institute's Health Reform Monitoring Survey (HRMS) show a break in this trend following implementation of the ACA's key coverage provisions, a period when the uninsurance rate for adults fell steeply (Karpman, Long, and Zuckerman 2016). HRMS data indicate that the gap in uninsurance between parents and children narrowed between September 2013 and March 2015 (Karpman et al. 2015). In addition, related work shows that mothers living with dependent children had substantial declines in their uninsurance rates between 2013 and 2014 (Karpman, Gates, Kenney et al. 2016).

The health insurance provisions of the ACA, including broader eligibility for Medicaid, tax credits to purchase health plans through the new health insurance Marketplaces, insurance market reforms that prohibit denials of coverage or premium rates based on pre-existing conditions, and a tax penalty for not having coverage, have the potential to improve not only parents' access to health insurance coverage and care and their financial stability but also their children's coverage and well-being. Several studies have found that previous coverage expansions for parents were associated with increased coverage for children (Aizer and Grogger 2003; Dubay and Kenney 2003; Ku and Broaddus 2000) and suggest that children have greater access to care when their parents are insured (Davidoff et al. 2003; Guendelman and Pearl 2004; Gifford, Weech-Maldonado, and Farley Short 2005).

In this brief, we use data from the National Health Interview Survey (NHIS) to examine trends in uninsurance for custodial parents between 1997 and 2014, the most recent full year for which NHIS data on parents' coverage status are available. We focus on custodial parents living in a household with

their dependent children age 18 and under because noncustodial parents are not identifiable on the NHIS. We provide estimates of uninsurance among custodial parents (subsequently referred to as parents) overall and among those with family incomes above and below 138 percent of the federal poverty level (FPL), which is the new Medicaid eligibility level established for adults under the ACA. As of June 2016, 31 states and the District of Columbia had expanded Medicaid to 138 percent of FPL for parents and childless adults. We also explore differences in health care access, service use, and health care affordability between insured parents and the remaining uninsured as of 2014, changes in the characteristics of uninsured parents between 2013 and 2014, and uninsurance rates for various subgroups of parents in 2013 and 2014. This analysis provides context for recent changes in coverage among parents and offers new information on the access and affordability challenges facing both insured and uninsured parents. Related briefs examine outcomes under the ACA separately for mothers, fathers, and parents of young children (Gates, McMorro, Kenney et al. 2016; Karpman, Gates, Kenney et al. 2016; Karpman, Gates, McMorro et al. 2016).

Data and Methods

This analysis uses nationally representative data for the civilian noninstitutionalized population from the NHIS, which is conducted annually by the National Center for Health Statistics. The NHIS consists of three main sections: the family core, sample adult core, and sample child core. Questions on the family core are answered for each member of the family by a knowledgeable adult and include basic demographic information, educational attainment, employment status, general health status, and detailed information on health insurance coverage. Questions on the sample child and sample adult core are asked of one random child age 17 or under (if present; excluding emancipated minors) and one random adult over age 17 in each family; the questions solicit extensive information on health status and details on health care access, use, and affordability. The NHIS is fielded continuously throughout the year and can be used to provide nationally representative annual or quarterly estimates. We use public use data from the 1997–2014 Integrated Health Interview Series, which provides harmonized versions of NHIS variables across years.¹

We use data from 1997 to 2014 to track uninsurance counts and rates for parents ages 19 to 64. For our analysis, we construct health insurance units (HIUs) to better reflect the units used to determine health insurance eligibility relative to the broader family definition available on the NHIS. Hereafter, we use the terms “family” and “HIU” interchangeably, but all analysis of changes in uninsurance by income uses the HIU as the relevant unit for determining income as a percentage of FPL. Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. Based on this definition, in 2014 the NHIS included data on approximately 25,000 parents from the family core file and 10,000 parents from the sample adult file. Parents are considered uninsured if they do not have coverage through private health insurance, Medicare, Medicaid, military health insurance, or other public insurance at the time of the survey. We examine changes between 2013 and 2014 in the composition of uninsured parents and rates of uninsurance among parents by age (19 to 25, 26 to 34, 35 to 49, or 50 to 64); race/ethnicity (non-

Hispanic white, non-Hispanic black, non-Hispanic other race, or Hispanic); sex; health status (excellent or very good, good, or fair or poor); disability status (has any activity limitation or has no activity limitation); citizenship status; HIU income (138 percent of FPL or less, above 138 percent and equal to or below 400 percent of FPL, or above 400 percent of FPL); work status (full-time worker, part-time worker, or not working); education level (less than high school, high school, some college, or graduated college); HIU citizenship (any noncitizen in HIU or no noncitizen in HIU); census region (Northeast, Midwest, South, or West); and state Medicaid expansion status as of May 2014, indicating whether expanded eligibility was available for the majority of the year.

We also compare health care access, service use, and affordability experiences for uninsured parents in 2014 to those for insured parents. For these analyses, we define uninsured parents as those who lacked health insurance coverage for all of the previous 12 months and insured parents as those who had health insurance coverage for all of the previous 12 months. We construct four measures of access and service use including having a usual source of care other than the emergency department, having seen a general doctor in the past 12 months, and indicators for having seen a specialist and a dentist in the past 12 months. We also generate four measures of affordability. Using information reported by a family respondent, we identify parents whose families had trouble paying medical bills in the past 12 months as well as a subset whose families were unable to pay their medical bills. We also measure the share of parents who had any unmet need because of affordability in the past 12 months (including unmet needs for medical care, dental care, prescription drugs, eyeglasses, mental health care, specialist care and follow-up care), and the share of parents for whom medical care was delayed because of the cost in the past 12 months. When assessing differences in these measures between uninsured and insured parents, we adjust for individual and HIU characteristics including age, sex, race/ethnicity, education level, work status, HIU income, citizenship, region, reported health status, and activity limitations.

Results

Changes in Uninsurance among Parents

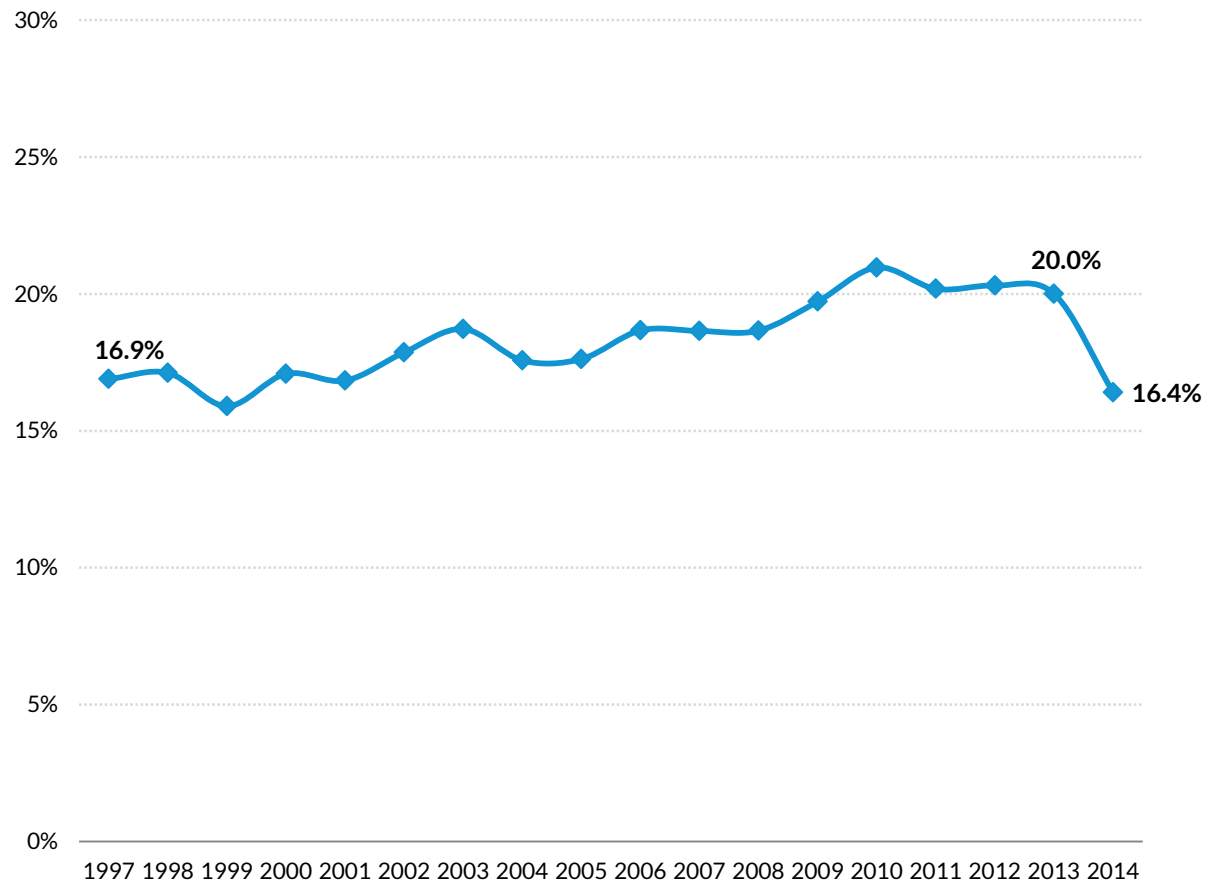
Although uninsurance among parents rose and fell with the fluctuations of the economy between 1997 and 2013, on balance their uninsurance rate drifted upward 3.1 percentage points, increasing from 16.9 to 20.0 percent (figure 1). In contrast, between 2013 and 2014, the uninsurance rate for parents fell 3.6 percentage points, a decline that was more than three times larger than any other year-to-year change since 1997. Though some of the decline between 2013 and 2014 may be caused by the economic recovery, the uninsurance rate for parents had fallen only 1 percentage point between its peak in 2010 (21.0 percent) and 2013 (20.0 percent), just before implementation of the ACA's major coverage provisions.

Figure 2 shows that uninsurance fell among parents with family incomes above and below 138 percent of FPL between 1997 and 2014. There was a larger percentage-point decline in uninsurance for lower-income parents, from 43.4 percent to 35.7 percent, compared with the decline from 10.3 percent

to 9.8 percent for parents with incomes above 138 percent of FPL. In 2014, however, lower-income parents remained nearly four times as likely to be uninsured as moderate- and higher-income parents, a ratio that has decreased only slightly since 1997.

FIGURE 1

Uninsurance among Parents, 1997–2014

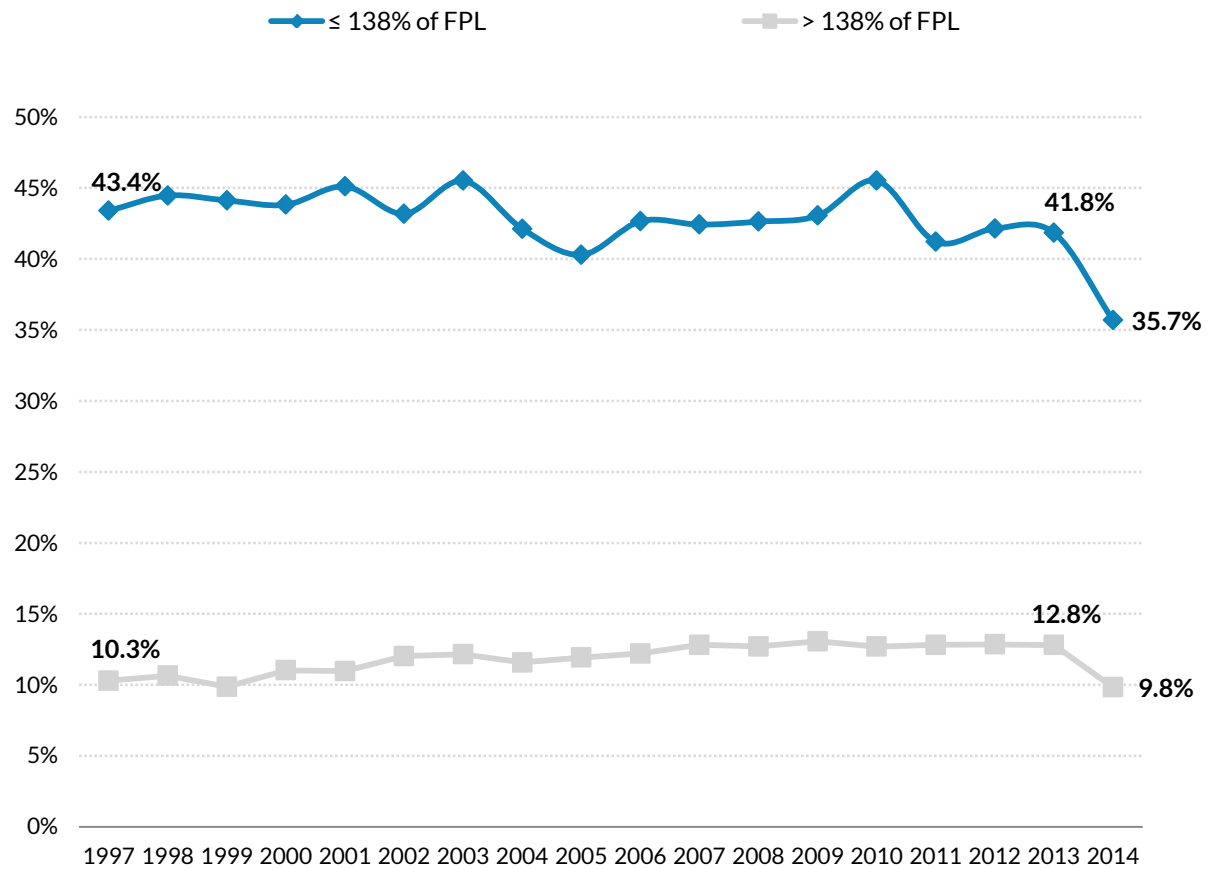


Source: Urban Institute tabulations of 1997–2014 National Health Interview Survey data.

Notes: Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. Uninsured is at time of survey.

FIGURE 2

Uninsurance among Parents by Income, 1997–2014



Source: Urban Institute tabulations of 1997–2014 National Health Interview Survey data.

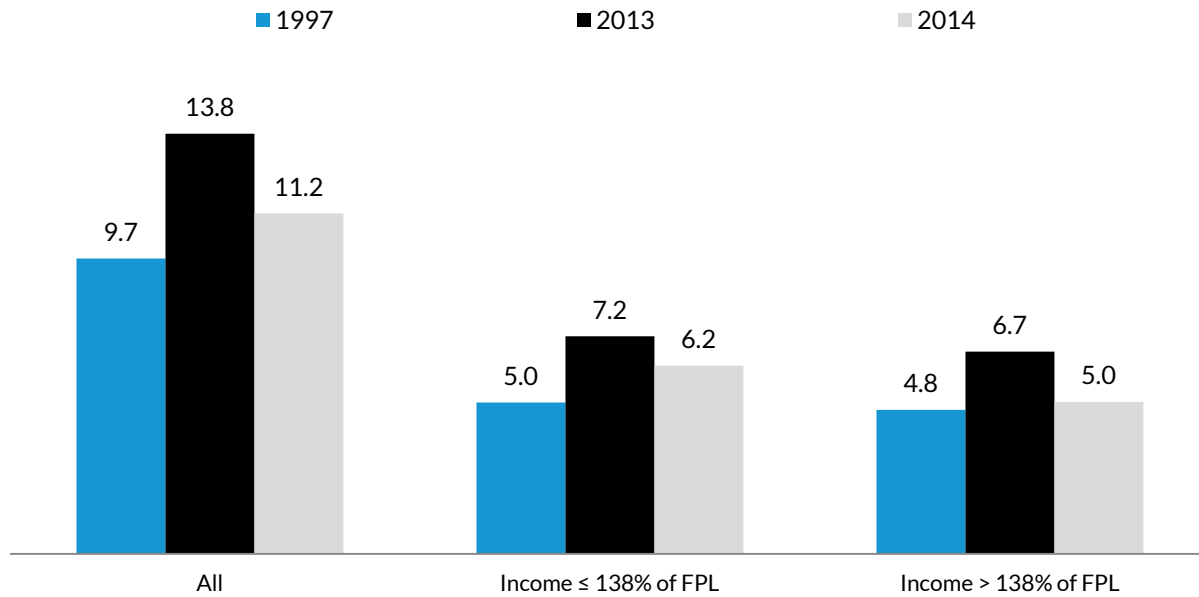
Notes: FPL = the federal poverty level. Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. Uninsured is at time of survey. Income is for the health insurance unit.

Approximately 2.6 million parents gained coverage between 2013 and 2014, including nearly 1 million parents with incomes at or below 138 percent of FPL and nearly 1.7 million parents with incomes above that level (figure 3). More than 11 million parents remained uninsured in 2014, and parents with incomes below 138 percent of FPL constituted more than 55 percent of remaining uninsured parents, a slightly higher share than in 1997 or 2013.

FIGURE 3

Number of Uninsured Parents, 1997, 2013, and 2014

Millions



Source: Urban Institute tabulations of 1997, 2013, and 2014 National Health Interview Survey data.

Notes: FPL = the federal poverty level. Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. Uninsured is at time of survey.

Composition of Parents Remaining Uninsured in 2014

The characteristics of uninsured parents shifted between 2013 and 2014. A greater proportion of the remaining uninsured were noncitizens in 2014 relative to 2013 (40.2 percent versus 35.7 percent), potentially reflecting the exclusion of undocumented noncitizens from the ACA's new coverage options or a lack of awareness of new coverage options among lawfully present noncitizens (table 1). Nearly half (48.9 percent) of parents who were uninsured in 2014 were Hispanic; this is not statistically different from the share who were Hispanic in 2013. More than 40 percent of uninsured parents had at least one noncitizen in the family. Between 2013 and 2014, the remaining uninsured parents became increasingly concentrated in states that chose not to expand Medicaid, with 59.4 percent residing in those states in 2014.

TABLE 1

Characteristics of Uninsured Parents in 2013 and 2014

	2013 (%)	2014 (%)	
Sex			
Female	54.1	52.9	*
Male	45.9	47.1	*
Age			
19 to 25	10.9	10.6	
26 to 34	34.0	34.4	
35 to 49	47.0	46.5	
50 to 64	8.1	8.6	
Race/ethnicity			
White, non-Hispanic	36.1	34.7	
Black, non-Hispanic	11.3	10.6	
Hispanic	46.5	48.9	
Other race, non-Hispanic	6.1	5.9	
Citizenship status			
Citizen	64.3	59.8	**
Noncitizen	35.7	40.2	**
Self-reported health status			
Excellent or very good	57.2	58.4	
Good	32.2	32.7	
Fair or poor	10.6	8.9	**
Limitations			
Has any activity limitation	4.6	3.7	*
Has no activity limitation	95.4	96.3	*
Medicaid expansion status			
Expansion state	47.0	40.6	***
Nonexpansion state	53.0	59.4	***
Region			
Northeast	9.5	9.8	
South	46.6	50.0	*
Midwest	16.3	16.4	
West	27.5	23.8	**
Education level			
Less than high school	32.9	35.5	*
High school	30.5	32.5	
Some college	27.3	23.5	***
College	9.3	8.5	
Work status			
Full time	53.4	52.6	
Part time	14.3	14.6	
Not working	32.3	32.8	
HIU citizenship status			
Any noncitizen in HIU	39.9	43.3	*
No noncitizens in HIU	60.1	56.7	*
Income by HIU			
≤ 138% of FPL	51.8	55.3	**
138–400% of FPL	42.6	39.1	**
> 400% of FPL	5.6	5.6	

Source: Urban Institute tabulations of 2013 and 2014 National Health Interview Survey data

Notes: FPL = the federal poverty level; HIU = health insurance unit. Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 0 to 18 or under living in their household. All measures are at time of survey. State Medicaid expansion status is as of May 2014.

*/**/** Estimate for 2014 differs significantly from the 2013 estimate at $p < 0.10/0.05/0.01$, respectively.

Parents who were uninsured in 2014 were also slightly less likely to be in fair or poor health (8.9 versus 10.6 percent) or to have a limitation on their activities (3.7 versus 4.6 percent) than those who were uninsured in 2013, suggesting that uninsured parents with health problems or disabilities may have been more likely to seek coverage under the ACA than parents in better health. The regional composition of uninsured parents also changed, with more uninsured parents in the South (50.0 percent versus 46.6 percent) and fewer in the West (23.8 percent versus 27.5 percent) in 2014 than in 2013. In addition, a somewhat smaller share of uninsured parents had some college education short of a bachelor's degree in 2014 compared with 2013. Although the share of uninsured parents with incomes at or below 138 percent of FPL increased, there was a moderate decrease in the share of uninsured parents with incomes between 138 and 400 percent of FPL, the income range targeted by the ACA's premium tax credits for Marketplace coverage. In 2014 as in 2013, nearly 95 percent of uninsured parents had incomes below 400 percent of FPL.

Other characteristics of uninsured parents remained stable between 2013 and 2014. For instance, the age and racial/ethnic distributions and patterns in employment status of uninsured parents have not changed. Over two-thirds (67.2 percent) of parents who were uninsured in 2014 worked either full-time or part-time.

Variation in Uninsured Rates among Parents

Between 2013 and 2014, the uninsurance rate fell for every demographic, socioeconomic, and geographic subgroup of parents examined except for those in the Northeast, where parents already had a lower uninsurance rate than their peers in other regions (table 2). The largest declines in uninsurance were found among parents with low or moderate incomes: a 6.2 percentage-point decrease for parents with incomes at or below 138 percent of FPL and a 5.0 percentage-point decrease for parents with incomes between 138 and 400 percent of FPL. Parents with incomes above 400 percent of FPL continued to have the lowest uninsurance rate of any income group at 2.6 percent compared with 16.2 percent for moderate-income parents and 35.7 percent for lower-income parents. Uninsurance rates also fell most sharply among parents with other characteristics associated with low or moderate incomes, including those who were ages 19 to 25 (5.6 percentage points), were non-Hispanic black (5.0 percentage points), were Hispanic (6.9 percentage points), were in fair or poor health (6.7 percentage points), or had less than a high school education (6.0 percentage points).

TABLE 2

Rates of Uninsurance among Parents in 2013 and 2014

	2013 (%)	2014 (%)		
All parents	20.0	16.4	***	
Sex				
Female ^a	19.5	15.7	***	
Male	20.6	17.3	***	+++
Age				
19 to 25 ^a	31.8	26.2	**	
26 to 34	25.5	21.5	***	+++
35 to 49	17.5	14.1	***	+++
50 to 64	12.6	10.9	*	+++
Race/ethnicity				
White, non-Hispanic	12.2	9.8	***	+++
Black, non-Hispanic	20.3	15.3	***	+++
Hispanic ^a	43.4	36.5	***	
Other race, non-Hispanic	14.7	11.3	**	+++
Citizenship status				
Citizen ^a	14.8	11.4	***	
Noncitizen	51.9	46.4	***	+++
Self-reported health status				
Excellent or very good ^a	17.0	14.0	***	
Good	26.2	22.0	***	+++
Fair or poor	26.9	20.2	***	+++
Limitations				
Has any activity limitation ^a	15.5	11.2	***	
Has no activity limitation	20.3	16.7	***	+++
Medicaid expansion status				
Expansion state ^a	17.6	12.7	***	
Nonexpansion state	22.7	20.4	**	+++
Region				
Northeast ^a	11.6	10.2		
South	25.0	21.6	***	+++
Midwest	14.5	12.1	**	+
West	23.1	16.3	***	+++
Educational attainment				
Less than high school ^a	47.8	41.8	***	
High school	25.7	22.2	***	+++
Some college	18.2	13.1	***	+++
College	5.6	4.1	***	+++
Work status				
Full time ^a	16.2	13.1	***	
Part time	26.5	22.0	***	+++
Not working	27.7	23.2	***	+++
HIU citizenship				
Any noncitizen in HIU ^a	47.7	41.3	***	
No noncitizens in HIU	14.4	11.2	***	+++
Income by HIU				
≤ 138% of FPL	41.8	35.7	***	+++
138–400% of FPL	21.2	16.2	***	+++
> 400% of FPL ^a	3.2	2.6	***	

Source: Urban Institute tabulations of 2013 and 2014 National Health Interview Survey data.

Notes: FPL = the federal poverty level; HIU = health insurance unit. Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. All measures are at time of survey. State Medicaid expansion status is as of May 2014.

*/**/** Estimate for 2014 differs significantly from the 2013 estimate at $p < 0.10/0.05/0.01$, respectively. +/++/+++ Estimate differs significantly from that of the reference category (marked with a) at $p < 0.10/0.05/0.01$, respectively.

Uninsurance rates in 2014 varied significantly by age, race/ethnicity, gender, and citizenship status. Parents were more than twice as likely to be uninsured if they were ages 19 to 25 (26.2 percent) than if they were ages 50 to 64 (10.9 percent). Hispanic parents continued to have the highest uninsurance rate of any racial/ethnic group (36.5 percent), over twice as high as the uninsurance rate for non-Hispanic black parents (15.3 percent) and over three times as high as the rate for non-Hispanic white parents (9.8 percent). Fathers had higher uninsurance rates than mothers (17.3 percent versus 15.7 percent), and nearly half of noncitizen parents were uninsured (46.4 percent) compared with 11.4 percent of parents who were citizens.

Parents who reported being in excellent or very good health were less likely to be uninsured (14.0 percent) than those in worse health. However, parents with at least one activity limitation were less likely to be uninsured than parents with no activity limitations (11.2 percent versus 16.7 percent), perhaps because of Medicaid's historic role in providing coverage for low-income individuals with disabilities.

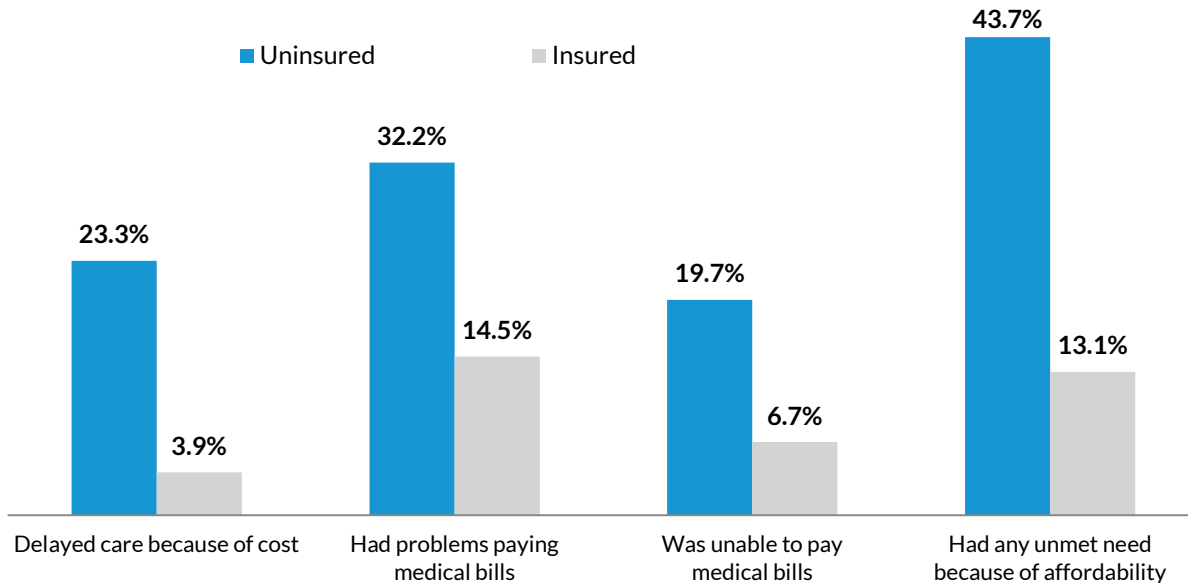
Despite coverage gains in the South and West, these two regions continued to have higher uninsurance rates for parents than the Northeast (21.6 percent and 16.3 percent versus 10.2 percent, respectively). Not surprisingly, parents remain more likely to be uninsured if they do not work full time, have lower educational attainment, or live in families with at least one noncitizen.

Access, Service Use, and Affordability for Uninsured Parents

Parents who were uninsured for all 12 months before the 2014 survey were more likely to report problems affording health care than parents who were insured for all 12 months (figure 4). They were six times as likely to report delaying care because of the cost, over twice as likely to report problems paying family medical bills or being unable to pay bills at all, and more than three times as likely to say they had an unmet need for health care because it was not affordable.

FIGURE 4

Health Care Affordability among Uninsured and Insured Parents, 2014



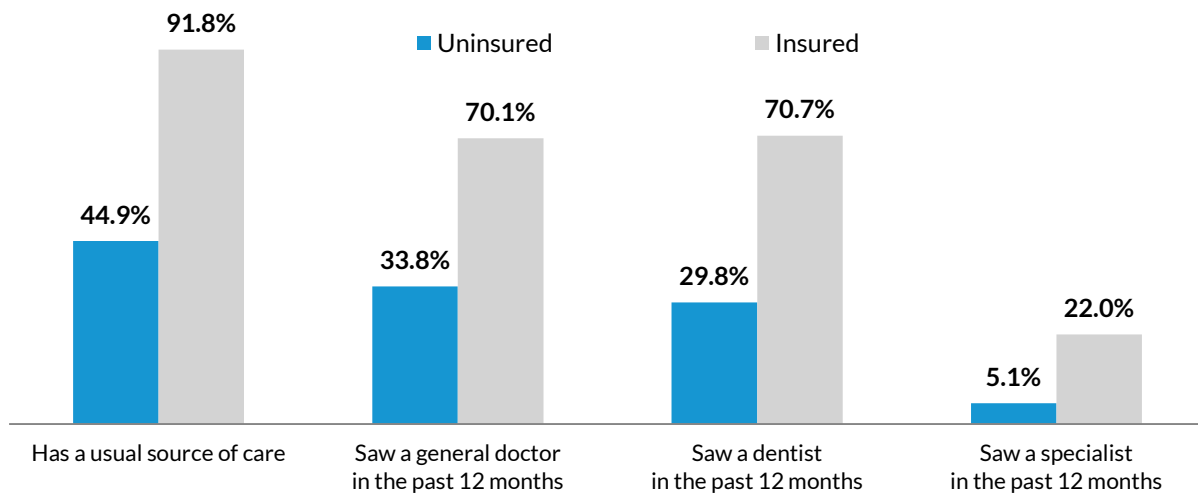
Source: Urban Institute tabulations of 2014 National Health Interview Survey data.

Notes: Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. Uninsured parents are defined as those who lacked health insurance for all of the previous 12 months. Insured parents are defined as those with health insurance for all of the previous 12 months. All measures are for experience in the 12 months before survey. "Any unmet need" includes unmet need for medical care, dental care, prescription drugs, eyeglasses, mental health care, specialist care and follow-up care. All estimates for the uninsured differ significantly from estimates for the insured ($p < 0.01$).

Parents without coverage were also less likely to report having a usual source of care than insured parents (44.9 percent versus 91.8 percent) and had lower use of health care services (figure 5). Insured parents were more likely than their uninsured peers to have seen a general doctor, dentist, or specialist in the previous 12 months. These differences in health care affordability, access, and service use persist even when we account for differences in the characteristics of insured and uninsured parents. Figure 6 shows that, controlling for observable characteristics of the parents and their families, uninsured parents were 19.8 percentage points more likely to delay care because of the cost, 28.3 percentage points more likely to go without care for affordability reasons, and 12.4 percentage points more likely to have problems paying family medical bills. Similarly, figure 7 shows that uninsured parents were 41.9 percentage points less likely to have a usual source of care and continued to be less likely than insured parents to have seen a general doctor, a dentist, or a specialist in the previous year, although the differences between insured and uninsured parents are somewhat smaller with the regression adjustment.

FIGURE 5

Health Care Access and Service Use Among Uninsured and Insured Parents, 2014

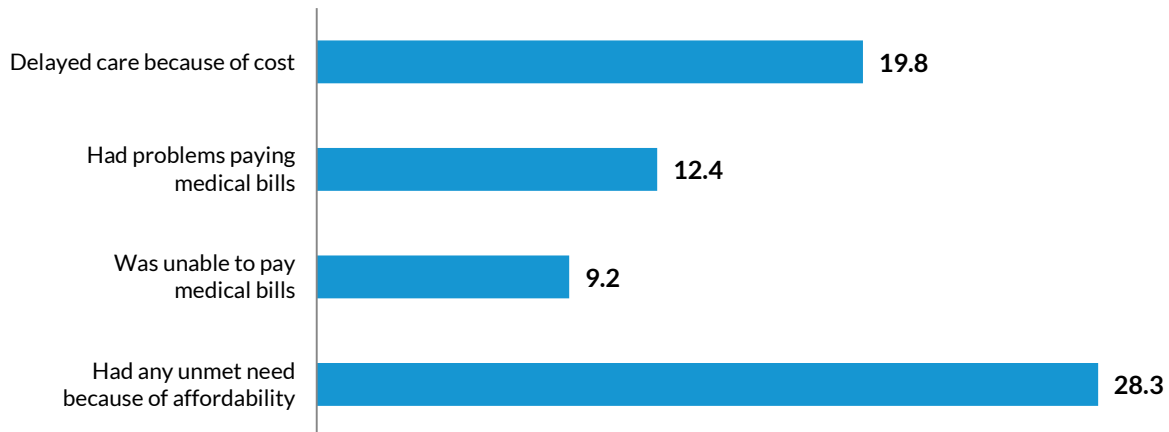


Source: Urban Institute tabulations of 2014 National Health Interview Survey data.

Notes: Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. Uninsured parents are defined as those who lacked health insurance for all of the previous 12 months. Insured parents are defined as those with health insurance for all of the previous 12 months. Usual source of care is at time of survey. All estimates for the uninsured differ significantly from estimates for the insured ($p < 0.01$).

FIGURE 6

Percentage-Point Differences in Affordability Measures between Uninsured and Insured Parents, 2014

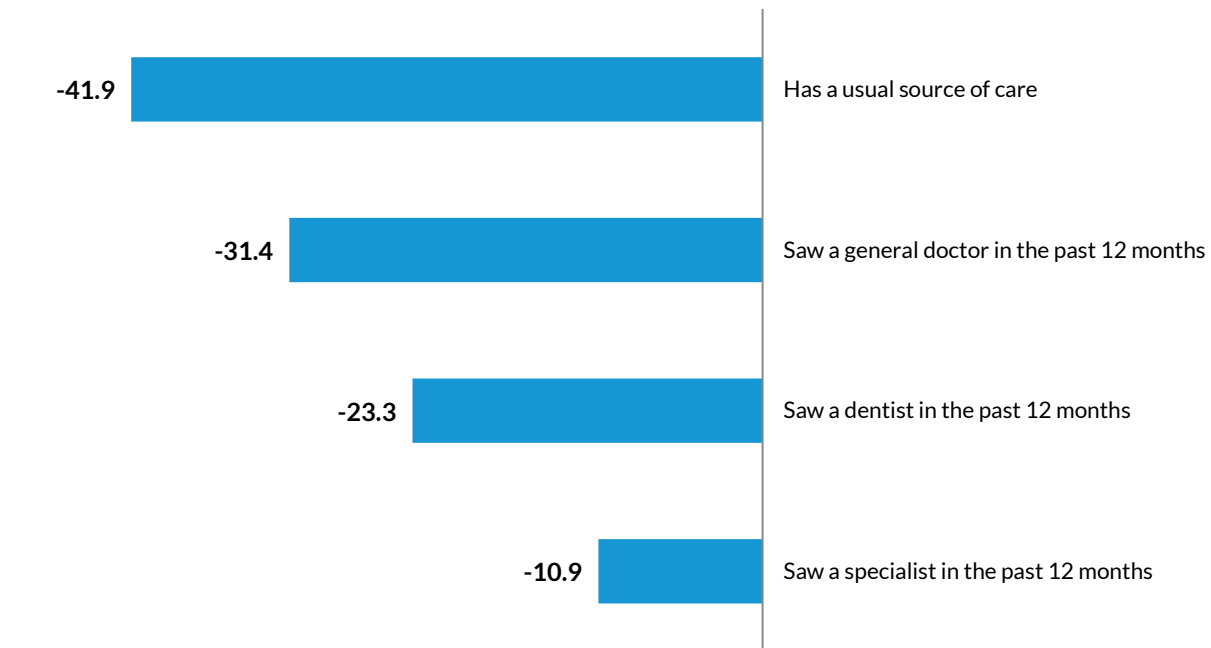


Source: Urban Institute tabulations of 2014 National Health Interview Survey data.

Notes: Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. Uninsured parents are defined as those who lacked health insurance for all of the previous 12 months. Insured parents are defined as those with health insurance for all of the previous 12 months. All measures are for reported experience in the 12 months before the survey. "Any unmet need" includes unmet need for medical care, dental care, prescription drugs, eyeglasses, mental health care, specialist care, and follow-up care. Differences are regression adjusted for sex, race/ethnicity, education level, work status, health insurance unit income, citizenship, region, self-reported health status, and activity limitations. All differences are statistically significant ($p < 0.01$).

FIGURE 7

Percentage-Point Differences in Access and Service Use between Uninsured and Insured Parents, 2014



Source: Urban Institute tabulations of 2014 National Health Interview Survey data.

Notes: Parents are defined as adults ages 19 to 64 who were identified as the mother or father of a child age 18 or under living in their household. Uninsured parents are defined as those who lacked health insurance for all of the previous 12 months. Insured parents are defined as those with health insurance for all of the previous 12 months. Usual source of care is at time of survey. Differences are regression adjusted for sex, race/ethnicity, education level, work status, health insurance unit income, citizenship, region, self-reported health status, and activity limitations. All differences are statistically significant ($p < 0.01$).

Conclusion

The share of nonelderly parents without health insurance increased from 16.9 percent in 1997 to 20.0 percent in 2013, just before the implementation of the ACA's Medicaid expansion, Marketplaces, and several of the law's major insurance market reforms. Between 2013 and 2014, the uninsurance rate for parents fell 3.6 percentage points, representing a decline in the uninsured population of 2.6 million. A separate analysis of NHIS data shows that children have also experienced declines in uninsurance under the ACA (Gates, Karpman, Kenney et al. 2016). Although these coverage improvements for parents and children coincide with the implementation of the ACA's major coverage provisions, some of these gains may also be attributed to other changes, such as an improving economy.

Uninsurance fell between 2013 and 2014 for almost every subgroup of parents we examined with particularly sharp declines for low- and moderate-income parents, those in poor or fair health, those living in families with noncitizens, and Hispanic parents; simultaneously, those same groups still had high rates of uninsurance in 2014 compared with their peers. Over one-third of parents with incomes at

or below 138 percent of FPL and over one-third of Hispanic parents did not have coverage, and uninsurance rates were relatively high among parents who were young, less educated, not working, or in less than very good health. Some parents, such as undocumented immigrants and parents with incomes below 100 percent of FPL in states that have not expanded Medicaid, continue to have few viable pathways to coverage. In many of the largest nonexpansion states, parents must have very low incomes to qualify for Medicaid. For instance, parents seeking Medicaid are not eligible unless their incomes are less than 39 percent of FPL in Virginia, 37 percent of FPL in Georgia, 34 percent of FPL in Florida, and 18 percent of FPL in Texas.² Moreover, even among parents who are likely to have access to financial assistance, many do not think they can afford coverage and often face difficulties paying for such basic needs as food and housing (Karpman, Gates, and Kenney 2016).

Parents who do not have coverage are less likely than insured parents to have a usual source of care and to see doctors and dentists and are more likely to forgo care for affordability reasons and have problems paying family medical bills. These differences in access, service use, and affordability remain after controlling for the demographic and socioeconomic characteristics of parents. Uninsured parents' disconnection from the health care system could have adverse spillover effects for children, who may be less likely to get needed health care and more likely to experience hardships if their parents' health or finances deteriorate.

Given that 55 percent of uninsured parents had incomes below 138 percent of FPL and were disproportionately likely to live in the South and West in 2014, further reductions in uninsurance among parents may depend on additional state expansions of Medicaid, increased take up of Medicaid and Marketplace coverage among those who are eligible, and on tackling affordability issues facing noncitizen parents. Targeted outreach to young parents and Hispanic parents may also be needed to further raise awareness of options for obtaining coverage.

Moreover, even as coverage continues to expand, 14.5 percent of parents who had coverage for a full year before the 2014 survey reported problems paying medical bills, and 13.1 percent reported going without care because it was not affordable in the previous year. Underinsurance in the employer and nongroup markets, driven by increasing deductibles and other forms of cost sharing, continues to pose challenges, particularly for those with low incomes or high health needs (Collins et al. 2015). Further, provider capacity may not be sufficient to support the influx of newly insured in all areas. Thus, additional monitoring of access and affordability is critical to ensure that parents with health insurance coverage are able to access needed care and are receiving adequate protection from high medical bills.

Notes

1. "Integrated Health Interview Series: Version 6.12" 2016, Minnesota Population Center, University of Minnesota and State Health Access Data Assistance Center, accessed April 7, 2016, <http://www.ihis.us>.
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Exhibit 35

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

NBER WORKING PAPER SERIES

THE EFFECT OF CHILD HEALTH INSURANCE ACCESS ON SCHOOLING:
EVIDENCE FROM PUBLIC INSURANCE EXPANSIONS

Sarah Cohodes
Daniel Grossman
Samuel Kleiner
Michael F. Lovenheim

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The Effect of Child Health Insurance Access on Schooling: Evidence from Public Insurance Expansions

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ABSTRACT

Public health insurance programs comprise a large share of federal and state government expenditures. Although a sizable literature analyzes the effects of these programs on health care utilization and health outcomes, little prior work has examined the long-term effects and resultant health improvements on important outcomes, such as educational attainment. We contribute to filling this gap in the literature by examining the effects of the public insurance expansions among children in the 1980s and 1990s on their future educational attainment. Our findings indicate that expanding health insurance coverage for low-income children increases the rate of high school completion and college completion. These estimates are robust to only using federal Medicaid expansions, and mostly are due to expansions that occur when the children are older (i.e., not newborns). We present suggestive evidence that better health is one of the mechanisms driving our results by showing that Medicaid eligibility when young translates into better teen health. Overall, our results indicate that the long-run benefits of public health insurance are substantial.

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1. Introduction

Whether and how to provide access to affordable healthcare for low-income Americans has become a central policy issue in the US, driven in part by the large and persistent health disparities that exist across the socioeconomic spectrum. The importance of this issue is underscored by the intense debate surrounding the passage and implementation of the 2010 Affordable Care Act (ACA), one of the largest expansions of public health insurance in US history. A core component of the ACA was to further expand eligibility for Medicaid, which is the primary method through which the government provides affordable health insurance to low-income families. Since its inception in 1965, Medicaid has gone through repeated expansions that have greatly expanded the scope of the program as well as the public sector's role in health insurance provision. As a result, over 50% of children in the United States currently are eligible for publicly-provided health insurance through this program,² and health insurance coverage is high amongst this population (DeNavas-Walt et al., 2013).

The expansions that generated this high level of coverage were expensive. In 2012, total state and federal spending on Medicaid was \$415.2 billion (Henry J. Kaiser Family Foundation, 2014), which makes it the largest government program that targets low-income Americans.³ The substantial public funds devoted to providing health insurance to low-income children, as well as recent debates over the value of such insurance that surrounded the passage of the ACA, highlight the importance of understanding what benefits, if any, accrue to individuals due to health insurance access when they are young.

² Throughout this paper, we refer to “public health insurance” and Medicaid synonymously. Publicly-provided health insurance also includes State Children's Health Insurance Plans (SCHIP). Medicare, however, is not included in our definition of public health insurance for purposes of this paper.

³ As a point of reference, total expenditures on food stamps (SNAP) in 2012 were \$78.4 billion, and spending on Temporary Aid for Needy Families (TANF) was \$31.4 billion. Total Medicare expenditures were \$536 billion, which highlights that the Medicare and Medicaid/SCHIP programs are of roughly similar size.

The effect of Medicaid expansions on access to healthcare and on subsequent child health has been studied extensively, (e.g., Currie and Gruber, 1996a, 1996b; Moss and Carver, 1998; Baldwin et al., 1998; Cutler and Gruber, 1996, LoSasso and Buchmueller, 2004; Gruber and Simon, 2008), typically showing that Medicaid expansions increase access to healthcare, decrease infant mortality, and improve childhood health. Furthermore, these expansions and Medicaid access more generally have been linked to a lower likelihood of bankruptcy and to less medical debt (Gross and Notowidigdo, 2011; Finkelstein et al., 2012). If Medicaid leads to better health outcomes among children and to more stable finances among low-income households, as is suggested by prior research, Medicaid expansions could lead to long-run benefits for affected children. Given the persistently high returns to human capital investments (e.g., Autor, Katz and Kearney, 2008) as well as human capital models that suggest childhood health and family resources should both positively influence educational attainment, examining the effects of Medicaid expansions on long-run educational attainment is of considerable policy interest.

In this paper, we provide the first US-based evidence in the literature on how expanding health insurance for children influences their eventual educational attainment.⁴ Similar to prior work on Medicaid, we exploit the expansions of Medicaid and the State Children's Health Insurance Program (SCHIP) that took place in the 1980s and 1990s to examine how the educational attainment of these children was affected by access to these programs. We use data on 22-29 year olds born between 1980 and 1990 from the 2005-2012 American Community Survey (ACS) that allow us to match each respondent to his or her state of birth. We then use

⁴ Two prior studies have examined the role of public health insurance on educational attainment in the developing world (Alcaraz et al., 2013; Chen and Jin, 2012), however it is doubtful that these results can be generalized to the United States or to other industrialized nations.

data from the March Current Population Survey (CPS) to calculate Medicaid eligibility by age, state, year and race that we link to our ACS sample.

With these data, we follow the method of simulated instrumental variables pioneered by Currie and Gruber (1996a, 1996b) and Cutler and Gruber (1996), in which we use Medicaid eligibility of a fixed population in each age, state, year and race as an instrument for actual eligibility. This IV approach accounts for the fact that the composition of a state may be endogenous to Medicaid eligibility rules. By using a fixed sample to calculate eligibility, the model is identified using eligibility rule changes only. The underlying identification assumption for our purposes is that Medicaid rules are not changing due to unobserved cross-cohort trends in educational attainment. A large body of prior work has established the credibility of this assumption in terms of health, fertility and family bankruptcy (Currie and Gruber, 1996a; DeLeire et al., 2011; Gross and Notowidigdo, 2011). We extend this literature by implementing a series of robustness checks, including using only federal Medicaid variation that cannot be affected by state-level choices, to further support the validity of this methodology.

The main contribution of this paper to the literature is to demonstrate the effect of health insurance access among both young and school-age children on their long-run educational attainment. While there is a sizable body of research demonstrating a link between fetal health as well as the provision of fetal healthcare services on future educational outcomes (e.g., Figlio et al., 2013; Levine and Schanzenbach, 2009; Currie and Gruber, 1996b), the effect of children's access to healthcare services on their educational attainment has not been studied previously.⁵

⁵ Currie, Decker and Lin (2008) present suggestive evidence that exposure to Medicaid expansions when young lead to better health in adolescence, which suggests there could be an effect on educational attainment as well. In a related study, Brown, Kowalski and Lurie (2014) use IRS tax data to show that the eligibility expansions in the 1980s led to higher earnings by the time individuals reached the age of 31. Their work does not examine educational attainment, but their results and ours strongly complement one another.

From a policy perspective, this is an important group to consider because of the large amount spent on providing health insurance to non-newborn children. Furthermore, socioeconomic disparities in educational outcomes begin at young ages and largely persist throughout the lifecycle (Carneiro and Heckman, 2002; Todd and Wolpin, 2007). Thus, it is critical to understand whether reducing health insurance disparities across the socioeconomic distribution among children can be useful as a means to close these persistent educational gaps that are present in later years.

We find consistent evidence that Medicaid exposure when young increases later educational attainment. A 10 percentage point increase in average Medicaid eligibility between the ages of 0-17 decreases the high school dropout rate by 0.4-0.5 of a percentage point, has a small and typically insignificant effect on college enrollment, and increases the four-year college attainment rate (i.e., BA receipt) by 0.6-1.0 percentage point. These estimates translate into declines in high school non-completion of about 4.0-5.9% and increases in BA attainment of about 2.3%-3.0% relative to the sample means. In separate estimates by race, we find that the high school completion effects are localized to nonwhites, while the college completion rate impacts are largest among white children.

Another important contribution of this analysis is to examine whether it is sufficient for a child to be treated at birth or whether there are returns to expanding eligibility amongst older, largely school-age children. Prior work in this area has focused more on eligibility at birth than on the effects of eligibility at older ages (Levine and Schanzenbach, 2009; Currie and Gruber, 1996b).⁶ We provide some of the first estimates in this literature on heterogeneity by age at the time of expansion. While we do not find strong age patterns in the data, we see evidence that

⁶ Currie and Gruber (1996a) examine effects of Medicaid expansions on child mortality for children 1-14, but they do not break out the effects by child age at expansion.

Medicaid expansions at older ages have effects on educational attainment. That is, Medicaid expansions to slightly older, mostly school-age children increase educational attainment, not just eligibility expansions at birth.⁷

As a means to understand a central mechanism through which these effects operate, we examine the impact of Medicaid eligibility when young on teen health. Using data from the Youth Risk Behavior Surveillance System (YRBSS), we show suggestive evidence that Medicaid eligibility between age 0 and one's age at the time of the survey has sizable positive effects on a range of health outcomes. For example, an increase in Medicaid eligibility throughout one's youth reduces risky sexual activity, body mass index (BMI), drinking, and smoking cigarettes or marijuana. Furthermore, Medicaid eligibility decreases the number of reported mental health issues, and students are less likely to report having an eating disorder. While these estimates typically are not statistically significantly different from zero at conventional levels, they provide support for the idea that better health is an important mechanism that drives at least part of the increased educational attainment we document.

Overall, our results point to large effects of Medicaid expansions for children on their eventual educational attainment. These effects are particularly important because lower-income families are most affected by Medicaid and SCHIP expansions, and it is children from these families that have exhibited the most sluggish growth in educational attainment over the past 30 years (Bailey and Dynarski, 2011). Our estimates suggest that the long-run returns to providing health insurance access to children are larger than just the short-run gains in health status, and

⁷ The effect of Medicaid eligibility at birth could be biased downward due to the fact that Medicaid reduces infant mortality (Currie and Gruber, 1996b). Any resulting compositional changes in birth cohorts likely would lead to a reduction in long-run outcomes, all else equal. To the extent Medicaid changes the composition of births and/or of older children through reduced mortality, this should attenuate our estimates. However, infant and child mortality rates are sufficiently low in the US that any such attenuation is probably very small.

that part of the return to these expansions is a potential reduction in inequality and higher economic growth that stems from the creation of a more skilled workforce.

The rest of this paper is organized as follows: Section 2 describes the public health expansions we use in our analysis, and Section 3 reviews the literature on the effects of health insurance on health and family finances as well as the literature examining the links between health, family resources and educational outcomes. Section 4 provides a description of the data. We outline our empirical strategy and detail our results in Sections 5 and 6, respectively, before concluding in Section 7.

2. Medicaid and Public Health Care Expansions for Children

The Medicaid program was introduced in 1965 and phased in mostly over the late 1960s as a health insurance component for state-based cash welfare programs that targeted low-income, single-parent families. Beginning in the mid-1980s, the Medicaid program was slowly separated from cash welfare, first by extending benefits to low-income children in two-parent families and then by raising the income eligibility thresholds for two groups: children and pregnant women (Gruber, 2003; Gruber and Simon, 2008).⁸ Thus, since the 1980s, Medicaid has been expanded to many low-income families who did not previously qualify due to their income levels, family composition and/or labor force participation. As a result of these expansions, by the mid-1990s, most children in America below the poverty line, and all young children below 133% of the poverty line, were eligible for Medicaid. In certain states, their parents were as well.

Importantly, for most of these expansions, states could choose to implement the expansion based on their own eligibility preferences. By the early 1990's, states were required to cover all children below 100% of the poverty line, and children under age 6 below 133% of the

⁸ For more details on Medicaid expansions, see Currie and Gruber (1996a), Gruber (2003), and Gruber and Simon (2008).

poverty line. Many states opted to provide more generous coverage, however, for which the federal government would provide matching funds up to a certain threshold. In 1997, Congress passed the State Children's Health Insurance Plan (SCHIP), which was one of the largest expansions of public health insurance to date. SCHIP provided matching funds to states to expand coverage to children from households with incomes below 200% of the poverty line. Prior to SCHIP, states were permitted to cover children up to 200% of the poverty line, but, without federal matching funds, very few states did so.

In this paper, we exploit these expansions in Medicaid generosity in the 1980s and 1990s that were phased in at different times, and with different generosity levels across states, to identify the effect of Medicaid eligibility on long-run educational attainment. Thus, we use both state-level variation, which assumes the timing of state eligibility changes is exogenous with respect to underlying trends in educational attainment of residents, and federal variation to explicitly test the robustness of our estimates to the assumption that the state Medicaid variation is exogenous.

3. Previous Literature

The effect of Medicaid eligibility on education will flow through two main potential channels: better health due to Medicaid take-up, as well as higher household resources stemming from the insurance protection provided by Medicaid. This paper thus relates to the large literature examining the effect of Medicaid on health care utilization, health outcomes and household finances, as well as the literature linking health and family resource changes to educational outcomes. Below, we discuss both sets of research in turn.

3.1 Effects of Medicaid Expansions on Utilization, Health Outcomes and Family Finances

Much prior research has documented the effects of Medicaid expansions on both the use of medical care and health status. In their examination of the effects of health insurance on utilization, Buchmueller et al. (2005) provide a detailed survey of this literature, noting that economic theory predicts that health insurance coverage induces greater medical care utilization by reducing the cost of care to patients (Phelps, 1997). Consistent with this prediction, studies of state-based Medicaid and federal expansions of coverage show that these programs lead to increases in health care use on both extensive and intensive margins. These effects are observed both among children and adults (e.g. Currie and Gruber, 1996a, 1996b; Currie, 2000; Kaestner et al., 2000; Kaestner et al., 2001; Almeida, Dubay, and Ko, 2001; Banthin and Selden, 2003; Dafny and Gruber, 2005). As noted by Levy and Meltzer (2008), while health insurance increases the quantity of care consumed, the effects of coverage will vary based on the availability of providers and efficacy of the medical care consumed as a result of increased coverage. While their review suggests that health insurance improves the health of infants and children (with little conclusive evidence shown for non-elderly adults), Finkelstein et al. (2012) present evidence that the expansion of public insurance improves both physical and mental health for adults as well.

Recent work also has suggested that public health insurance successfully shelters low-income families from financial risk associated with negative health shocks. Gross and Notowidigdo (2011) show that families exposed to Medicaid expansions are less likely to declare bankruptcy, while the estimates of Dave et al. (2013) indicate that Medicaid eligibility was associated with a decrease in the employment probability of women who recently gave birth. In their study of the randomized Medicaid experiment in Oregon, Finkelstein et al. (2012) find that

obtaining access to public health insurance reduces the amount of out-of-pocket medical expenditures as well as medical debt.

3.2 Effects of Health and Family Resources on Educational Attainment

How are such changes in child health and family finances from Medicaid expansions predicted to affect educational attainment? A sizable literature dating back to the seminal contribution of Grossman (1972) examines the effect of education on future health,⁹ but much less work has been done estimating the effect of health in childhood on educational achievement and attainment. Existing research has documented that better fetal health translates into increased educational outcomes. These studies testing the “fetal origins” hypothesis overwhelmingly show that health interventions and shocks among pregnant women, as well as differences in measurable health at birth, have long-run consequences for cognitive ability, educational outcomes of children, and adult health (e.g., Miller and Wherry, 2014; Figlio et al., 2013; Almond and Mazumder, 2011; Almond, Edlund and Palme, 2009; Almond, 2006; Black, Devereaux and Salvanes, 2007; Oreopoulos et al., 2008; Royer, 2009).

Despite the evidence linking fetal health to long-run outcomes, little research exists examining how childhood health after birth impacts such outcomes. Currie et al. (2010) find that children with health problems in early childhood have poorer long-run health, a higher likelihood of being on social assistance, and lower educational outcomes. Case, Fertig and Paxson (2005) and Case, Lubotsky and Paxson (2002) both show that worse health in childhood is negatively associated with long-run outcomes, such as health, educational attainment, and labor market outcomes. Historical evidence also suggests such a link exists: hookworm eradication led to

⁹ There currently is very mixed evidence on whether education affects long-run health outcomes (e.g., Adams et al., 2003; Cutler and Lleras Muney, 2006; Grossman, 2004; Clark and Royer, 2013), with much heterogeneity in terms of the credibility of the identification strategies used, the time periods and countries studied, and the education levels examined.

more school attendance and literacy gains in the US south in the early 1900's (Bleakley 2007), and malaria eradication efforts resulted in small gains in income for cohorts whose regions were treated before other cohorts (Bleakley 2010).¹⁰

Cox and Reback (2013) as well as Lovenheim, Reback and Wedenoja (2013) examine the effect of health care *access* on educational attainment using the rollout of school-based health centers in the US. The former study finds that center openings lead to high attendance rates, while the latter shows they cause lower teen birth rates but do not affect high school dropout rates. The students treated by these centers are typically in high school, so the differences between these estimates and the large effects of health found by researchers examining younger children may potentially be due to heterogeneity in the effects of health at different times during childhood.

Another main channel through which Medicaid can influence educational attainment is through its effect on family resources. Several recent studies attempt to isolate the causal impact of additional funds on educational achievement. Dahl and Lochner (2012) use the Earned Income Tax Credit (EITC) as an instrument for unexpected income changes and find that an additional \$1000 of income for a family in poverty results in children's test score gains of about 0.06 standard deviations. Duncan, Morris, and Rodrigues (2011) find test score effects of a similar magnitude to those in Dahl and Lochner (2012) when they examine 11 random assignment experiments of welfare and anti-poverty programs from the 1990's. Micheltore (2013) shows that income changes from the EITC also lead to higher college enrollment and completion. Together, these studies suggest that changes in family finances and child health generated by Medicaid expansions could lead to impacts on long-run educational attainment.

¹⁰ See Almond and Currie (2011) for a comprehensive overview of the fetal origins hypothesis and Eide and Showalter (2011) for evidence on the effect of health on human capital outcomes throughout the life cycle.

While we provide the first analysis in the literature of the long-run effects of Medicaid on educational attainment, there are two papers in the literature that are closely associated with ours. The first paper is Levine and Schanzenbach (2009), which analyzes the effect of Medicaid and SCHIP expansions *at birth* on future educational achievement as measured by state-level NAEP scores. They examine differences in Medicaid expansion by state and the differences between age cohorts in a triple difference framework. Their results suggest that a 50 percentage point increase in Medicaid eligibility corresponds to a 0.09 standard deviation increase in reading test scores. They find no effect on math test scores, however.

Our analysis is distinguished from theirs along several dimensions. First, we focus on the effects of expanding health insurance to children of all ages. This question has been studied much less but is particularly important given the expected increase in the number of insured children due to the implementation of the ACA (Kenney et al., 2011) and the amount of money spent in the US on providing health care to children through Medicaid.¹¹ Indeed, our results indicate that expanding eligibility to non-newborn children is an important driver of the long-run effects of Medicaid, which further highlights the relative contribution of our analysis. Second, we examine effects on long-run educational attainment rather than on test scores at younger ages. A growing body of evidence points to the effect of given educational interventions on test scores being a poor predictor of the effects on the longer-run outcomes that are of greater interest, such as educational attainment and earnings (e.g., Ludwig and Miller, 2007; Chetty et al., 2011; Deming et al., 2013).¹²

¹¹ If health insurance among school-age children did not positively affect these children, ostensibly the government could only offer Medicaid to pregnant women and households with very young children. Thus, it is important to understand what value there is to offering school-age children Medicaid.

¹² Much of this evidence suggests that it is particularly problematic to use effects on contemporaneous test scores to predict long-run outcomes. Levine and Schanzenbach (2009) examine effects on the NAEP scores of 4th and 8th

The second related work is a working paper by Brown, Kowalski and Lurie (2014). They use IRS tax data to examine the effect of Medicaid expansions throughout a child’s early life on earnings. Because their data begin in 1996, they only observe their youngest cohorts at age 15, which means there could be potential problems associated with endogenous mobility.¹³ Nonetheless, they find results that are highly complementary to our own: Medicaid eligibility increases from 0-18 are associated with higher earnings, lower EITC receipt, and higher labor force participation. That they obtain these estimates on a different dataset using somewhat different cohorts is notable. Together, our results point to large effects of Medicaid expansions on the long-run outcomes of affected children.

4. Data

We use three sources of data in our analysis of the effects of insurance expansions on educational attainment. Below, we describe these sources of data, as well as the construction of the variables that we use in our investigation.

4.1 Medicaid Eligibility Data

Our Medicaid eligibility data are constructed for the years during which our 1980-1990 birth cohort are between the ages of 0-17 using the March Current Population Survey (CPS). We construct two eligibility measures, using state and year information on eligibility rules similar to those used in Gross and Notowidigdo (2011) and Gruber and Simon (2008).¹⁴ Eligibility

graders, which themselves are longer-run test score outcomes. Furthermore, instructors are unlikely to manipulate NAEP scores endogenously with respect to Medicaid eligibility rates, which would not necessarily be the case for contemporaneous test scores used to evaluate a given educational intervention. Nevertheless, it is not at all clear that effects on NAEP scores would translate into higher educational attainment, which underscores the importance of our analysis.

¹³ Endogenous mobility is less of an issue in our analysis because we observe state of birth rather than one’s state of residence at age 15 or older.

¹⁴ We are extremely grateful to Tal Gross and Kosali Simon for providing us with the computer code that forms the basis for our eligibility calculations.

calculations are based on the household's income, the age and number of children in the household, and the gender and unemployment status of the head of household.

The first Medicaid eligibility measure we construct is the proportion of households of a given race (white, nonwhite) with children of age a in state s and year t who are eligible for Medicaid, where $a \in (0,1,...17)$. Thus, for example, we calculate the proportion of households with 5-year-olds in New York who are eligible for Medicaid in each year between 1980 and 2007. We calculate eligibility separately by child's race due to the strong correlation between race and Medicaid eligibility: a given change in eligibility rules is likely to impact nonwhites differently than whites even though the Medicaid rules themselves are race-neutral.

These calculations allow us to measure the proportion of children of each age and race group that are Medicaid-eligible in each state and in each year between 1980 and 2007. As described below, our outcome data span the years 2005-2012. We focus on the 1980-1990 birth cohorts who are between the ages of 22 and 29 in 2005-2012, which is why our CPS sample ends in 2007 (when the 1990 birth cohort is 17).¹⁵ Due to small sample sizes in the CPS, particularly within each age-race-state cell, we use three-year moving averages of calculated eligibility instead of yearly eligibility.¹⁶ Aside from making the estimates more precise, our use of these moving averages has little effect on the results. We refer to this measure of Medicaid eligibility as “actual eligibility.”

Actual eligibility varies within states over time due to changes in eligibility rules, changes in demographic composition, and changes in the economic circumstances of households.

¹⁵ We have conducted extensive sensitivity analyses using different birth cohort ranges and ACS age ranges. Our results are not very sensitive to the age range or birth cohorts used. These sensitivity analyses are available from the authors upon request.

¹⁶ This method necessitates the use of CPS data through 2009 (which contains 2008 income information), to enable the construction of our 3-year moving average.

In order to isolate the variation in Medicaid eligibility due to eligibility rule changes, we follow the method first used in Currie and Gruber (1996a, 1996b) and Cutler and Gruber (1996) and calculate the proportion of each state, age and race in each year that would be eligible for Medicaid using a fixed national sample that does not vary across states or over time. We use a 20% national sample from the 1986 CPS and calculate the share of this fixed population with a child of age a in year t and race r that would be eligible for Medicaid in each state using that state's Medicaid eligibility rules in that year. The 20% sample is comprised of 31,223 individuals, and these respondents are assigned to each state (including DC) so that the total sample size is 1.6 million ($31,223 \times 51$). Critically, this sample does not vary by demographic characteristics across states or over time. Thus, this measure is unaffected by state-specific trends in populations or economic conditions that relate to both eligibility and coverage, (e.g. a state-level recession).

For each year, we use the same 1.6 million person sample and adjust family income for inflation using the Consumer Price Index for All Urban Consumers. We then calculate our measure of “simulated fixed eligibility” for individuals of a given state, age and race based on the federal- and state-specific Medicaid eligibility rules in effect in a given year.¹⁷ Finally, we collapse these estimates into unique state-year-age-race cells that yield the proportion of the fixed sample eligible for Medicaid in each cell. Since the fixed sample includes the same sample in every year, we calculate yearly eligibility rather than the three-year moving average that we use for actual eligibility.

Our baseline estimates include Medicaid eligibility variation coming from federal Medicaid expansions, state decisions about whether they will provide more generous benefits

¹⁷ Of the initial 31,223 individuals in the fixed sample, 23,870 are white and 7,353 are nonwhite.

than required by federal law, as well as the timing of state expansions and their generosity levels. Among these sources of variation, the one that is most worrisome is the timing of state expansions: state expansion decisions may be endogenous with respect to underlying trends in educational attainment. Thus, we also construct measures of Medicaid eligibility that only are a function of federal rules. Federal Medicaid rules have different impacts on states due to pre-existing state-level AFDC policies. Hence, we fix AFDC rules in each state as of 1980 and then calculate 3-year moving average actual eligibility as well as yearly fixed simulated eligibility for each age, race and state that would occur *only* due to changes in federal regulations governing Medicaid eligibility thresholds. Put differently, our federal eligibility measures yield state-year-age-race eligibility that would occur if no states provided more generous Medicaid access than required under federal law. The reason this is not simply a cohort-based analysis, then, is that the effect of federal rules varies by state according to (fixed) welfare policies. By design, this source of Medicaid eligibility variation cannot be correlated with any decisions states can make regarding Medicaid policies.

Trends in our Medicaid eligibility measures, both overall and by race, are shown in Figure 1. For each birth cohort, we show the average eligibility between the ages of 0-17 to which the cohort was exposed. The panels of the figure show, for the 1980-1990 birth cohorts, actual eligibility that is a function of both state and federal rules as well as eligibility that uses only federal rules. As demonstrated in Figure 1, there was a dramatic rise in Medicaid eligibility that took place across the birth cohorts we study. Overall, average eligibility rates over the course of childhood increased 172% between the 1980 and 1990 birth cohorts. Much of this was the non-linear increase in eligibility that came from the 1990 federal Medicaid expansion that extended eligibility to all children born after September 30, 1983 in families up to 100% of the

poverty line. In the second and third panel of Figure 1, we show that the proportional increases experienced between whites and nonwhites were similar, but the higher baseline eligibility rates among nonwhites in 1980 led to much higher eligibility among the 1990 cohort than among the 1980 cohort. In our data, over 50% of nonwhites born in 1990 were eligible for Medicaid over the course of their childhood, while less than 30% of whites were eligible among this birth cohort.

Figure 1 also shows that the trends in overall eligibility track the trends in federal eligibility closely, especially after the 1984 birth cohort, which highlights the importance of federal Medicaid policies for identification. The simulated eligibility trends are very close to the actual trends as well. Thus, most of the aggregate pattern in Medicaid eligibility is due to policy changes rather than demographic shifts in the US population.

4.2 Educational Attainment

The main outcome data we use come from the 2005-2012 American Community Survey (ACS). The ACS was designed to replace the Census, and thus the variables and design across the two surveys are almost identical. The sample for our analysis consists of birth cohorts from 1980-1990 who are between 22 and 29 in 2005-2012. Thus, for each individual in our sample, we observe eligibility in his or her birth state at each age between 0 and 17. Table 1 shows the birth cohorts included in our analysis sample at each age and year. The top row shows the ACS (i.e., calendar) year, and the column shows the age of the respondent. For example, in the 2008 ACS, observations of 25 year olds come from the 1983 cohort. This table illustrates that we do not observe each birth cohort in each ACS survey due to our constructed age cutoffs. For example, 29 year olds are observed in 2009-2012 and come from the 1980-1983 birth cohorts only, whereas 25 year olds come from the 1980-1987 birth cohorts and are included in each of

the ACS years in this analysis. Our use of 1980 as the earliest birth cohort is driven by our lack of information about state-specific Medicaid eligibility pre-1980, and thus it is not feasible to use earlier birth cohorts.¹⁸ Furthermore, we examine individuals only up to age 29 as by age 29 most education has been completed (Bound, Lovenheim and Turner, 2010). Including older individuals would reduce the number of calendar years in which we can identify eligibility for such respondents.

The central benefit of using the ACS data for this analysis is our ability to link each respondent to the state of his or her birth. Using the current state of residence is problematic because students may endogenously sort across states, especially if Medicaid indeed impacts education outcomes. One's state of birth is unlikely to be related to Medicaid rules, however, especially since prior work has found no link between Medicaid rules and fertility patterns (Zavodny and Bitler, 2010; DeLeire, Lopoo and Simon, 2011). We calculate, for each respondent, indicators for whether the person did not complete high school, whether she attended any college and whether she obtained a Bachelors Degree (BA). Our measure of high school completion includes GEDs, which is potentially problematic if Medicaid eligibility shifts students from obtaining a traditional high school diploma to a GED.¹⁹ In 2008 and after, however, the ACS asks directly about GED completion. Using data from 2008-2012, we find little evidence that our main high school completion results are being driven by GEDs, as shown in Table 4. We thus conclude that our use of pre-2008 data is not biasing the main conclusions

¹⁸ We also note that Medicaid eligibility was very low pre-1980 and there were few expansions. Thus, our focus on birth cohorts between 1980 and 1990 captures most of the policy-driven variation in Medicaid exposure that has occurred since the program's inception.

¹⁹ Heckman and LaFontaine (2006) present evidence that the returns to a GED are lower than the returns to a high school diploma. Thus, examining patterns of substitution across these degrees is of interest.

one might draw from our results about the relationship between Medicaid and high school completion.

We collapse the data to birth cohort, state of birth, survey year, race (white/nonwhite) means for all variables, using the individual census weights. We then link each birth cohort, state-of-birth, race, survey year cell to the Medicaid eligibility means discussed in Section 4.1. In particular, we calculate average eligibility for each birth cohort in each survey year (t), state of birth (s) and race (r) over their childhood ages ($a \in [0,17]$):

$$eligibility_{sart} = \frac{1}{18} \sum_{i=0}^{17} \overline{elig}_{sirt}, \quad (1)$$

where $\overline{elig}_{sirt}$ is the average Medicaid eligibility in birth state s and survey year t of race r when the individual was age i .

We construct an identical measure using fixed simulated eligibility:

$$fs\_eligibility_{sart} = \frac{1}{18} \sum_{i=0}^{17} \overline{fs\_elig}_{sirt}, \quad (2)$$

where $fs\_eligibility$ is fixed simulated Medicaid eligibility of those in a given birth cohort and race (r) in state of birth (s) and survey year (t) when the individual was age (a) that is calculated using a constant sample from the 1986 CPS, as described above.

Descriptive tabulations of the analysis data for the full sample and by race group are shown in Table 2. In the full sample, the average respondent is 25, and about 68% of the respondents are white. The gender and age composition of the sample varies little across race groups. Furthermore, the educational attainment of nonwhites is much lower than whites, while average Medicaid eligibility is much higher for nonwhites. Both of these patterns reflect the strong correlation between socioeconomic status and race, which highlights the potential

importance of any effect of Medicaid eligibility on educational attainment to help address gaps in educational outcomes between whites and nonwhites.

5. Empirical Methodology

To analyze the effect of Medicaid eligibility expansions on educational attainment, we use difference-in-difference methods, in which our identification strategy makes use of differences across states over time in both the eligibility criteria (which varies by income, age and family composition) and the timing of the expansions. Specifically, we estimate models of the following form:

$$Y_{sart} = \beta_0 + \beta_1 \text{eligibility}_{sart} + \beta_2 X_{sart} + \gamma_{rs} + \delta_{rt} + \theta_{ra} + \varepsilon_{sart} \quad (3)$$

where Y_{sart} is the educational outcome (high school non-completion rate, college attendance rate or college graduation rate) in state-of-birth s for age a , of race r in survey year t . The variable $\text{eligibility}_{sart}$ comes from equation (1) above and denotes the mean fraction of individuals between the ages of 0 and 17 who were eligible for Medicaid in state s when respondents in year t were aged 0-17.

In the baseline specification, the vector X_{sart} includes percent male and an indicator for whether the observation is for the nonwhite sample or not. As we discuss below, we then include in X_{sart} some measures of potential confounding policies. The model includes as well a set of race-by-age fixed effects (θ_{ra}), race-by-state-of-birth fixed effects (γ_{rs}) and race-by-calendar year fixed effects (δ_{rt}).²⁰ The race-by-age fixed effects in particular are important because they account for the fact that older individuals have more time to complete their education and that this age pattern might be different across whites and nonwhites. The race-by-state fixed effects control for fixed differences across states that are correlated with both Medicaid eligibility and

²⁰ Henceforth, we will refer to “state fixed effects” and “state-of-birth fixed effects” synonymously.

educational attainment, such as the higher education structure and the industrial mix in the state, which we allow to vary by race as well. The race-by-year fixed effects account for any economy-wide shocks that could be correlated with prior Medicaid expansions and that might be different across racial groups.

The coefficient of interest in equation (3) is β_1 ; conditional on the set of controls and fixed effects in the model, the variation used to identify this coefficient comes from increases in eligibility within state across birth cohorts over time. This is basically a difference-in-difference specification, where the treatment dose varies across different cohorts depending on the state and year of birth as well as on one's race. As discussed in Section 4, this variation comes from two sources: the first is rule changes that expand Medicaid eligibility to different populations within each state, and the second is demographic shifts that expand the proportion of individuals who meet pre-existing eligibility criteria.

For our analysis, the second source of variation is potentially problematic. If there are demographic changes that affect the proportion of people eligible for Medicaid, these changes are likely to be correlated with educational attainment. Our limited set of demographic controls cannot fully account for such changes, although demographic changes that expand Medicaid eligibility most likely generate a negative bias in estimating the effect of Medicaid on educational attainment. We therefore use an instrumental variables strategy that is robust to demographic shifts. This IV strategy amounts to using $fs\_eligibility$ from equation (2) as an instrument for $eligibility$. Because $fs\_eligibility$ is based on eligibility rules in each year using a fixed sample of individuals from the 1986 CPS, it is only affected by eligibility rule changes over time within states.

Similar to any difference-in-difference analysis, there are two main assumptions we invoke. The first is that Medicaid expansions are not correlated with underlying trends in educational attainment across cohorts at the state level. A particular concern for our identification strategy would be if Medicaid expansions are occurring in states that are becoming more affluent. Then, even simulated fixed eligibility changes would be positively correlated with underlying and unobserved trends in educational attainment. We do not believe such a situation is likely, however, since states probably would be more compelled to expand Medicaid eligibility due to increased, not decreased demand for public insurance. This is a common identification assumption that has been invoked repeatedly in the Medicaid literature (e.g., Currie and Gruber, 1996a, 1996b; Cutler and Gruber, 1996; Gross and Notowidigdo, 2011; Gruber and Simon, 2008). The second assumption underlying our identification strategy is that there are no other state-level policies that are correlated with Medicaid expansions that themselves might affect educational attainment.

We provide an extensive set of robustness checks to provide additional confidence that our results are not being driven by endogenous state Medicaid eligibility expansions or by other policies. First, in some specifications we control for average state EITC amounts between the ages of 0-17 for each cohort. Prior work linking EITC policies to educational outcomes suggests EITC generosity could be a confounding factor if it is correlated with Medicaid generosity.²¹ We also control for average school spending per pupil in the years in which each cohort was 5-17, separately by urban, rural and suburban districts. Although there is a tenuous link between school expenditures and education outcomes (see Hanushek, 2003 for an overview of this literature), recent work has linked school spending increases from school finance reform to higher long-run

²¹ See Micheltmore (2013) for an overview of state-level EITC laws. We thank Kathy Micheltmore for providing us with these data.

educational outcomes (Jackson, Johnson and Persico, 2014). If such spending changes are correlated with Medicaid expansions, it could generate a bias in our results. We view these alternative policies as the two that are most likely to produce confounding effects, and our estimates that control for these policies provide evidence on whether this is so. In some specifications, we also include race-by-state-of-birth-by-year fixed effects as well as race-by-age-by-year fixed effects.²² These fixed effects allow us to control flexibly for any contemporaneous age- or state-specific shocks, separately by race, which are correlated with prior Medicaid expansions.

We provide more direct evidence that endogenous state Medicaid expansions are not biasing our estimates by using only federal Medicaid eligibility rules as discussed in Section 4.1. The race-by-state-of-birth fixed effects control for the fixed differences in AFDC rules across states, and the identifying variation in the federal model comes solely through the fact that federal rule changes have differential impacts on states due to pre-existing AFDC policies. Thus, there is no scope in these models for endogenous state decisions regarding Medicaid, and to the extent we obtain similar results using this variation, it will provide confidence in the validity of the results that use state Medicaid variation as well. To the best of our knowledge, this is the first paper to provide estimates using only federal eligibility variation, so these results are of interest in their own right insofar as they help validate the widely-employed assumption that state Medicaid expansions are exogenous.

²² Note that we do not control for race-by-state-by-age fixed effects. Thus, some of the identifying variation could be coming from fixed differences across ages within a state. However, this would require the existence of shocks to specific ages (but not birth cohorts) in a state that happen to be correlated with Medicaid eligibility differences. Furthermore, the estimates that use only federal variation would be unaffected by any such shocks. We have estimated models using these fixed effects, and the results are qualitatively similar (if somewhat less precise). We do not include them in the analysis because there is little economic justification for these controls.

We also conduct robustness tests that include race and state of birth specific linear trends across birth cohorts. These models are identified off of the non-linear increases in Medicaid eligibility that followed from state and federal law changes, and they help guard against any upward bias from correlated secular trends in educational attainment and Medicaid eligibility. We further provide a robustness check in which we randomly assign observed eligibility levels across age-state-year cells. Overall, our estimates are robust to using variation in Medicaid eligibility from different sources and to the series of robustness checks we conduct. These findings support the validity of our identification strategy.

Because errors are unlikely to be independent within states of birth over time, we cluster all standard errors at the state-of-birth level. All estimates also are weighted using sample weights provided in the ACS.

6. Results

6.1 Main Results

Table 3 presents the main results from our estimation of equation (3). Each cell in the table comes from a separate regression, with Panel A showing results that use all Medicaid eligibility and Panel B showing results using only federal eligibility. The first column in the table presents the first stage, which shows how a change in fixed simulated eligibility translates into actual eligibility. The table also shows the effect of actual Medicaid eligibility (“OLS”) and fixed simulated eligibility (“RF,” for reduced form) on high school non-completion, college enrollment and four-year college completion as well as the associated IV estimates.

Across outcomes and the specifications shown in different rows, we find consistent evidence that Medicaid eligibility when young increases educational attainment. Focusing on the baseline IV results in row (1), a 10 percentage point increase in fixed simulated eligibility

reduces high school non-completion by 0.38 of a percentage point, increases college enrollment by 0.30 of a percentage point, and increases BA attainment by 0.61 of a percentage point. The high school and college completion estimates are statistically significantly different from zero at the 5% level and 10% levels, respectively. Relative to the mean attainment rates shown in Table 2, these estimates translate into a 4.0% decline in high school dropouts and a 2.3% increase in BA receipt. As shown in Figure 1, there was a 24 percentage point increase in average eligibility during childhood between the 1980 and 1990 birth cohorts. Our estimates suggest this change would have reduced high school non-completion by 9.7% and increased college completion by 5.5%.²³

To put these effects in perspective, it is helpful to compare them to educational attainment trends over this period. Murnane (2013) shows that high school graduation rates increased by about 6 percentage points between the 1980 and 1990 birth cohorts. Since a 24 percentage point increase in Medicaid would increase high school completion by 0.9 percentage points, our results indicate that 15% of this increase can be attributed to Medicaid expansions. Our tabulations from the Current Population Survey indicate that college completion rates among 23-year olds between the 1980 and 1990 birth cohorts increased by 4.8 percentage points. A 24 percentage point Medicaid eligibility increase would increase BA attainment by 1.5 percentage

²³ It is likely that these gains in educational attainment are even more pronounced among those that take up Medicaid. We estimate take-up of Medicaid in our cohorts as well and find results similar to those in Gruber and Simon (2008). These estimates are available from the authors upon request. However, we note that neither these estimates nor those in the literature elsewhere (Cutler and Gruber 1996) are the appropriate “first stages” in our context. These estimates provide the contemporaneous effects on take-up, where we would need an estimate of the effect on take-up over one’s entire childhood to match our reduced form results. Unfortunately, no comprehensive longitudinal datasets exist with sufficient sample size to allow us to estimate this relationship. The CPS, which we use for the contemporaneous take-up results, is not sufficient, for while it has many waves of data, it does not follow the same families over many years. Following the majority of the Medicaid literature, we therefore focus on eligibility rather than on take-up.

points using the baseline results, which implies that Medicaid expansions can explain 30.5% of the overall BA attainment increases over this period.

How do these effect sizes compare to effects from other education interventions? Such comparisons are complicated by the fact that we are examining attainment at relatively older ages, which is rare in the literature examining early lifetime interventions. One point of comparison is the class size literature. Dynarski, Hyman and Schanzenbach (2013) show the effects of the Tennessee STAR class size experiment on BA attainment. Their estimates point to a 1.6 percentage point increase in the likelihood of BA receipt due to being randomized into a smaller class in primary school. Deming et al. (2014) show lottery-based results from an open enrollment system in the Charlotte-Mecklenburg schools that winning a lottery and thus attending a higher-quality high school increases the likelihood of college completion by 4.7 percentage points. In their analysis of school accountability, Deming et al. (2013) estimates that a 1 percentage point increase in the likelihood of being subject to accountability sanctions for low-performing students increases their high school graduation rate by 1 percentage point and increases their BA attainment rate by 0.6 of a percentage point. Finally, Garces, Thomas and Currie (2002) show that Head Start participants are 4 percentage points more likely to graduate from high school in estimates that control for selection using mother fixed effects. Overall, our estimates indicate that a 10 percentage point Medicaid expansion produces educational attainment increases that are either of equal size or somewhat smaller than these other notable educational interventions.

Rows (2)-(4) of Table 3 show our estimates are largely robust to adding additional controls for EITC and school spending (row 2) as well as race-state-year and race-age-year fixed effects (rows 3 and 4) into the models. The high school non-completion estimates become

slightly smaller in absolute value, but they also become less precise. Qualitatively, they do not change by much. The other estimates change little in terms of magnitude, but all are less precisely estimated due to the demanding nature of these models.

Table 3 also demonstrates that the OLS and reduced form/IV results are quite different from each other. The OLS estimates in Panel A show Medicaid eligibility increases are associated with smaller high school dropout declines and with smaller college completion increases. These results are suggestive that the bias from failing to account for the correlation between demographics and Medicaid eligibility would cause one to find a smaller effect of Medicaid on educational attainment. Once this confounding factor is controlled for, however, Table 3 indicates Medicaid expansions have a positive and sizable impact on long-run educational attainment.

Panel B of Table 3 shows estimates that use only federal Medicaid eligibility. Focusing on the baseline estimates in row (5), we show that federal eligibility expansions reduce high school dropout and increase college enrollment and completion. Comparing the estimates in row (5) to the baseline results in row (1), the point estimates for the reduced form are smaller in absolute value when only the federal variation is used. As the IV estimates show, this difference mostly reflects the smaller first stage. In Panel A, the first-stage estimates are around 0.9, suggesting that a 10 percentage point change in fixed simulated eligibility is associated with a 9 percentage point change in actual eligibility.²⁴ As expected, the link between federal Medicaid rules and actual eligibility is much weaker because we are ignoring state responses to the federal

²⁴ Our first-stage estimates are similar to what has been found in prior work. Cutler and Gruber (1996) report a first-stage of 0.84 for children and 0.95 for women, while Gross and Notowidigdo (2011) have an implied first-stage estimate of 0.61.

regulation changes. However, the first stage for the federal variation still is sizable in magnitude and is statistically significant from zero at the 1% level.

Comparing the IV estimates from similar models across panels shows that using the federal variation only produces results that are quantitatively and qualitatively similar to the estimates that use state variation as well. For high school non-completion in the baseline specification (row 1), the estimates indicate a 10 percentage point eligibility increase during childhood reduces dropout by 0.38 of a percentage point using all Medicaid variation, and it reduces dropout by 0.55 of a percentage point using only federal variation (row 5). For college enrollment, the estimates in row (5) are smaller than those in row (1), and they are inconsistent with all but a small increase in college attendance. Finally, for college completion, the IV coefficients across panels of Table 3 show very similar effects of Medicaid eligibility expansions. Comparisons of rows (2) and (6) show that our estimates using federal variation are robust to the inclusion of EITC and school spending controls as well.²⁵ That these two models yield similar estimates of the effect of changes in Medicaid eligibility among children on long-run educational attainment supports our use of all Medicaid variation, as it suggests state Medicaid eligibility variation is not endogenous with respect to long-run educational outcomes.

A final potential concern with the results in Table 3 is that the high school completion variable groups GED and high school diploma recipients together. Starting in 2008, the ACS began asking separately about high school diploma and GED receipt, and in Table 4 we present estimates using 2008-2012 data where we separate high school diploma non-receipt from diploma and GED non-receipt. Henceforth, we will only present IV and OLS estimates due to

²⁵ We do not present federal variation results that include race-state-year and race-age-year fixed effects. Due to the limited amount of variation in federal Medicaid eligibility, including these fixed effects yields large standard errors that make the resulting estimates uninformative.

space considerations; reduced form estimates are available upon request. As the table demonstrates, the effects are extremely similar across the two measures of high school completion, suggesting that our baseline estimates do not obscure potential shifts between traditional diplomas and GEDs. In addition, the some college and college plus estimates are similar in the 2008-2012 sample, if somewhat larger among all outcomes. These results suggest our estimates are not driven by the particular sample period we chose.

6.2 Educational Attainment Results by Race and Age at Expansion

Thus far, we have estimated models that pool effects across racial groups. But, given persistent racial disparities in educational attainment, heterogeneous effects by race are of considerable interest. In Online Appendix Tables A-1 and A-2, we estimate our models separately for whites and nonwhites, respectively. For whites, the effects on high school non-completion are negative, but they are smaller in absolute value than in the pooled model and they are not statistically significant at conventional levels. Particularly when we include the full set of fixed effects and when we use only federal variation, there is a positive effect of Medicaid on college enrollment for whites. The effect is on the order of 1.0 to 1.3 percentage points for each 10 percentage point increase in Medicaid eligibility. There also is a sizable, positive effect on college completion for whites, especially using the federal variation. While these point estimates are large – suggesting a 2.5 percentage point increase from a 10 percentage point Medicaid eligibility increase – they are consistent with observed increases in white college completion across these cohorts.²⁶

²⁶ CPS tabulations indicate that college completion rates among white 23 year olds increased by 6.4 percentage points between the 1980 and 1990 birth cohorts. White Medicaid eligibility expanded by 19 percentage points across cohorts, which would increase BA attainment rates by 4.75 ($=0.25 \times 0.19 \times 100$) percentage points. This is 74% of the total BA attainment increase over this period.

Among nonwhites, the effects on high school non-completion are particularly large. We find that high school non-completion is reduced by about 0.45-0.59 percentage points for each 10 percentage point increase in Medicaid eligibility. There is little evidence of a college enrollment effect, and although we find an increase in college completion, it is smaller than for the white sample. Taken together, these estimates are consistent with a larger effect of Medicaid eligibility for whites on higher education attendance and completion and a larger effect for nonwhites on high school completion.

As discussed in Section 3.2., one of the contributions of this paper is to identify whether there are effects of health insurance access after birth. Since prior work in this area has examined effects of Medicaid eligibility among pregnant women, our estimates are informative about any impacts of public health insurance among older children. In Table 5, we present IV estimates of equation (3) that control separately for Medicaid eligibility when a respondent was 0-3, 4-8, 9-13 and 14-17. In Panel A, we show results from the baseline specification, while in Panel B we include our full set of fixed effects (akin to row 4 in Table 3).

The results are not precise, due to the demanding nature of this specification, and there are no strong age patterns that emerge. What is striking about these results is that they suggest that Medicaid expansions at all ages are important for long-run outcomes such as education. Thus, our results indicate that expanding Medicaid eligibility to children after birth (and infancy) can have a substantial effect on long-run educational attainment. This finding sheds some light on why our estimates are somewhat larger than those in Levine and Schanzenbach (2009), who examine test score effects of Medicaid eligibility at birth. Our results show that educational outcomes, at least in the longer-run, are sensitive to Medicaid expansions that target school-age children as well.

6.3 Robustness Checks

In this section, we present two robustness checks that yield additional insight into the validity of our central identifying assumption, namely that there are not differential underlying trends in educational attainment correlated with public health insurance eligibility expansions. First, in Table 6, we present results from the models presented in Table 3 that also include state-specific linear birth cohort trends, separately by race. If there are differential trends in educational attainment correlated with Medicaid expansions, these results should yield substantively different results from our baseline model. However, the results are very similar to those in Table 3, suggesting that linear differences in trends across states are not biasing our baseline estimates.

Second, in Table 7, we show the mean and the 2.5th and 97.5th percentiles from 500 simulations that randomly assign Medicaid eligibility and fixed simulated eligibility across age-state-year cells. That is, we take combinations of actual and fixed simulated eligibility, and as a pair randomly assign them to different age-state-year cells, separately by race. This assignment is done with replacement. Both for the baseline model and for the model including race-state-year and race-age-year fixed effects, the average estimates are very close to zero. Furthermore, the non-parametric confidence intervals suggests these null estimates are precisely estimated. This robustness check suggests the results presented in Table 3 are due to the specific way the Medicaid eligibility expansions were rolled out over time within states. When we randomly assign eligibility levels, they are no longer meaningfully related to educational attainment.

6.4 The Effect of Childhood Medicaid Eligibility on Teen Health

As discussed in Section 4, one of the main mechanisms through which public health insurance can affect long-run educational attainment is through promoting better health amongst

children. In order to examine the potential importance of this mechanism, we estimate the effect of Medicaid eligibility during childhood on health outcomes in the teenage years. To do this analysis, we use the Youth Risk Behavior Surveillance System (YRBSS), which is a nationally-representative survey of 9th to 12th grade students that is conducted by the Centers for Disease Control (CDC).²⁷ The data are available from 1991-2011²⁸ and contain state identifiers, so we can estimate equation (3) using these data with health outcomes as the dependent variable. A central drawback of these data, however, is that we do not observe state of birth, only state of current residence. These estimates therefore are potentially biased by endogenous mobility.

Table 8 contains health outcome estimates from the YRBSS data. Here, we calculate Medicaid eligibility from age 0 up until each respondent's age. Thus, for a 15-year-old, the eligibility measure is average eligibility in the respondent's state of residence he/she would have experienced between the ages of 0-14. These estimates also include race-state-year and race-age-year fixed effects. Across virtually all measures of health outcomes, Table 8 shows that Medicaid eligibility during childhood translates into better health and better health behaviors by the time one is a teenager. However, due to low power, the standard errors are large, rendering these estimates more suggestive than conclusive. Still, they are informative as to a key mechanism likely driving our results.

The first row of Table 8 includes as a dependent variable a risky sex index that we construct based on several questions regarding sexual activity,²⁹ and our IV results indicate a 10 percentage point increase in Medicaid eligibility during youth reduces risky sexual behavior by

²⁷ These data can be accessed at: http://www.cdc.gov/HealthyYouth/yrbs/index.htm?s_cid=tw_cdc16.

²⁸ We limit our sample to years 1995-2007 because in these years sample respondents between the ages of 14 and 18 are most similar to the 1980 to 1990 birth cohorts we analyze using ACS data.

²⁹ See Online Appendix Table A-3 for a list of the variables used to construct this index as well as individual estimates for each measure. The variables that constitute this index are indented directly below the Risky Sex Index in the table. We also report the individual estimates for the variables that make up our measures of whether a respondent has a mental health issue or an eating disorder in Online Appendix Table A-3.

3.5% relative to the sample mean.³⁰ There is, however, an increase in the likelihood of being sexually active due to public health insurance eligibility. Thus, while teens are having sex more, they are practicing safer sex on average.

Our results suggest that those exposed to higher Medicaid eligibility at young ages have lower weight as well. There is a negative effect of Medicaid eligibility on BMI, on the order of a 3.9% reduction relative to the sample mean from a 10 percentage point increase in eligibility. We also find, as indicated in rows 4 and 5, that the likelihood of being overweight or obese declines considerably. In addition, there are sizable, though imprecisely estimated, declines in the likelihood of smoking marijuana, smoking cigarettes, and in alcohol consumption due to Medicaid eligibility. Medicaid expansions are associated with mentally healthier teens as well, as measured with a mental health index as well as by the prevalence of eating disorders.

That teens with higher Medicaid eligibility throughout their childhoods are healthier and have fewer harmful health behaviors suggests that they are indeed utilizing the medical services that are provided to them, are healthier as a result, and that increased health is one mechanism that is driving the educational attainment effects we find. To the extent that such increases in health enter into the education production function, they are likely to be one of the mechanisms driving the higher educational attainment that stems from the same Medicaid eligibility increases. While more work is necessary to clearly understand the role of health in producing educational outcomes, these results provide suggestive evidence that such a link is present and that health may be an important input into the education production function.

7. Conclusion

³⁰ The first stage estimate for these regressions is 0.882, with a standard error of 0.123.

In this paper, we provide the first evidence on the effects of public health insurance expansions on long-run educational attainment in the US. Overall, our results suggest large effects of childhood Medicaid expansions on eventual educational outcomes. Our baseline estimates indicate that a 10 percentage point increase in Medicaid eligibility between the ages of 0 and 17 decreases the likelihood of not completing high school by approximately 4.0-5.9% and increases the 4-year college completion rate by 2.3%-3.0%. The effects on high school completion are largest among nonwhites, while the effects on college attendance and completion are largest for whites. We also present evidence that public health insurance expansions when children are of school age are closely linked with long-run educational attainment; eligibility expansions beyond birth lead to higher educational attainment. To the best of our knowledge, these are the first estimates to demonstrate the importance of health insurance eligibility amongst older children, particularly as it relates to educational outcomes. Our analysis concludes by showing that the health insurance expansions we examine also translate into better health amongst teenagers, which we posit is an important mechanism through which health insurance access for children impacts their educational attainment.

Although the public health insurance expansions we study occurred in the past several decades, our results have several implications that are important for current public policy. First, they suggest that the long-run benefits of providing health insurance to low-income children may be much larger than the short-run gains. Evidence pointing to the large and growing returns to educational attainment (e.g., Autor, Katz and Kearney, 2008) as well as the importance of education in increasing intergenerational economic mobility (Black and Devereaux, 2011; Chetty et al., 2014) suggests that the returns on the public investments in health insurance in the 1980s and 1990s will be realized for some time.

Second, our results relate to current policy discussions over the future of the SCHIP program, which have accompanied the larger debate over the ACA. More specifically, the ACA prohibits states from imposing eligibility and enrollment standards for Medicaid and SCHIP that were more restrictive than those in place in March 2010 (when the ACA was passed) until 2019. However, there have been attempts in Congress to repeal these provisions, which would essentially allow states to cut SCHIP benefits and eligibility. In addition, SCHIP funding is up for re-authorization in 2015, and its passage is far from assured. A back-of-the-envelope calculation indicates that eliminating the SCHIP program would reduce eligibility for public health insurance by 15.4 percentage points. Our baseline estimates suggest such a decline would increase the high school dropout rate by six-tenths of a percentage point and would decrease the college enrollment rate by five-tenths of a percentage point and the college completion rate by 0.9 of a percentage point. The results from this study highlight the need to account for the types of long-run effects of public health insurance provision when considering changes to the publicly provided health care system that is targeted at low-income children.

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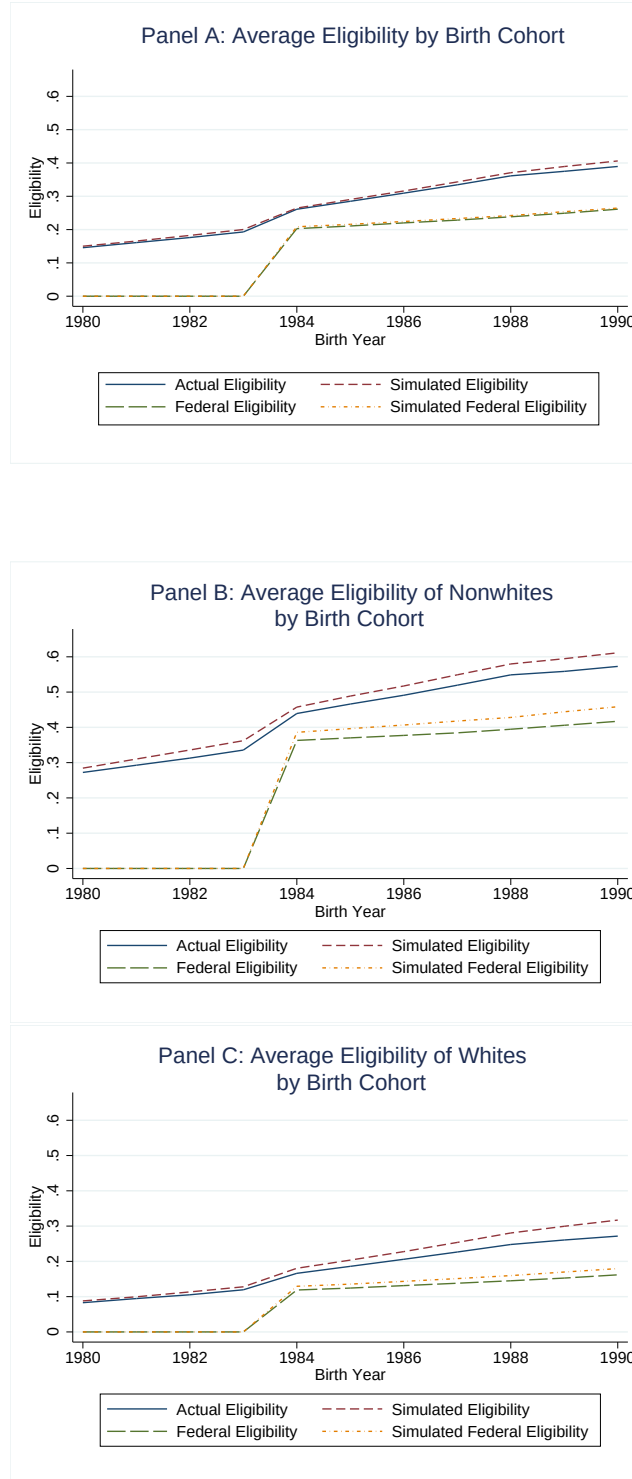
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Figure 1: Medicaid Eligibility by Birth Cohort and Race



The figure shows average eligibility of 0-17 year olds by birth cohort calculated using 1980-2004 CPS data combined with state by year Medicaid eligibility rules. Eligibility is calculated separately for whites and non-whites. Simulated fixed eligibility is calculated by applying state-by-year rules to 1986 CPS data. Federal eligibility uses only federal Medicaid rules, applied to each state using fixed 1980 AFDC rules.

Table 1: Birth Cohorts by Age in Each ACS Year

Age	2005	2006	2007	2008	2009	2010	2011	2012
22	1983	1984	1985	1986	1987	1988	1989	1990
23	1982	1983	1984	1985	1986	1987	1988	1989
24	1981	1982	1983	1984	1985	1986	1987	1988
25	1980	1981	1982	1983	1984	1985	1986	1987
26		1980	1981	1982	1983	1984	1985	1986
27			1980	1981	1982	1983	1984	1985
28				1980	1981	1982	1983	1984
29					1980	1981	1982	1983

Table 2: Summary Statistics for Analysis Samples

Variable Name	All	White	Nonwhite
No High School	0.094 (0.048)	0.071 (0.029)	0.143 (0.045)
No High School or GED	0.126 (0.054)	0.102 (0.038)	0.176 (0.050)
At Least Some College	0.656 (0.086)	0.694 (0.062)	0.572 (0.071)
College Graduate	0.265 (0.108)	0.309 (0.096)	0.172 (0.065)
Age	25.001 (2.156)	25.031 (2.155)	24.936 (2.157)
Male	0.504 (0.039)	0.508 (0.032)	0.497 (0.049)
White	0.683 (0.466)	1.000 (0.000)	0.000 (0.000)
Black	0.143 (0.266)	0.000 (0.000)	0.451 (0.290)
Hispanic	0.123 (0.230)	0.000 (0.000)	0.386 (0.255)
Other Race	0.052 (0.108)	0.000 (0.000)	0.163 (0.135)
Married	0.616 (0.089)	0.671 (0.027)	0.496 (0.053)
Age 0-17 3-year Average Medicaid Eligibility	0.237 (0.152)	0.156 (0.077)	0.410 (0.127)
Age 0-17 Average Fixed Simulated Medicaid Eligibility	0.254 (0.155)	0.171 (0.083)	0.434 (0.116)
Age 0-17 3-year Federal Average Simulated Medicaid Eligibility	0.113 (0.140)	0.068 (0.070)	0.208 (0.195)
Age 0-17 Average Federal Fixed Simulated Medicaid Eligibility	0.122 (0.148)	0.074 (0.073)	0.225 (0.205)
Observations	5494	2754	2740

Source: Author's tabulations from the 2005-2012 ACS. The samples consist of 1980-1990 birth cohorts aged 22-29, for whom we observe Medicaid eligibility in every year in their birth state from age 0 through 17. All tabulations were done using ACS sample weights. Standard deviations are shown in parentheses. Average eligibility is calculated using 3-year moving averages. The GED tabulations only include ACS years 2008-2012. Federal Medicaid eligibility is calculated using federal rules only, interacted with 1980 state AFDC rules as described in the text.

Table 3: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment

		No HS			Some College			College Plus			
Specification		1 st Stage	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
<u>Panel A: All Eligibility</u>											
(1)	Baseline	0.910*** (0.109)	-0.030** (0.014)	-0.035** (0.015)	-0.038** (0.016)	0.022 (0.018)	0.027 (0.022)	0.030 (0.024)	0.019 (0.016)	0.055* (0.028)	0.061* (0.033)
(2)	EITC & School Spending	0.951*** (0.075)	-0.023 (0.015)	-0.035** (0.015)	-0.037** (0.015)	0.024 (0.019)	0.028 (0.020)	0.029 (0.021)	0.017 (0.020)	0.062** (0.030)	0.065** (0.032)
(3)	EITC, School Spending, R-S-Y & R-A-Y FE	0.894*** (0.105)	-0.000 (0.021)	-0.020 (0.024)	-0.022 (0.024)	0.044 (0.031)	0.064 (0.041)	0.073* (0.042)	0.041 (0.028)	0.095 (0.060)	0.108* (0.065)
(4)	Baseline + R-S-Y & R-A-Y FE	0.846*** (0.159)	-0.001 (0.020)	-0.021 (0.025)	-0.025 (0.027)	0.009 (0.033)	0.074 (0.055)	0.088 (0.067)	0.036 (0.026)	0.079 (0.062)	0.095 (0.069)
<u>Panel B: Federal Eligibility</u>											
(5)	Baseline	0.211*** (0.030)	-0.030** (0.014)	-0.012*** (0.004)	-0.055*** (0.021)	0.022 (0.018)	0.002 (0.007)	0.012 (0.032)	0.019 (0.016)	0.017*** (0.006)	0.079*** (0.027)
(6)	EITC & School Spending	0.209*** (0.031)	-0.023 (0.015)	-0.011*** (0.004)	-0.054** (0.021)	0.024 (0.019)	0.002 (0.007)	0.012 (0.032)	0.017 (0.020)	0.017*** (0.006)	0.079*** (0.027)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2005-2012 ACS. Each cell in the table comes from a separate regression (N=5480). The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the dependent variable, and the "RF" columns refer to models that use fixed simulated eligibility as the independent variable. All estimates include an indicator for the cell being nonwhite or not as well as race-by age, race-by-calendar year and race-by-state of birth fixed effects. Rows 3 and 4 include race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 4: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment, Separating GED and HS Diplomas, 2008-2012

			No HS Diploma		No GED or HS		Some College		College Plus	
Specification		1 st Stage	OLS	IV	OLS	IV	OLS	IV	OLS	IV
<u>Panel A: All Eligibility</u>										
(1)	Baseline	0.910*** (0.113)	-0.020 (0.019)	-0.047** (0.019)	-0.023 (0.020)	-0.041** (0.020)	0.009 (0.020)	0.043 (0.036)	0.025 (0.019)	0.081* (0.043)
(2)	EITC & School Spending	0.945*** (0.076)	-0.013 (0.019)	-0.047*** (0.018)	-0.015 (0.020)	-0.040** (0.019)	0.021 (0.022)	0.035 (0.026)	0.021 (0.022)	0.083** (0.041)
(3)	EITC, School Spending R-S-Y & R-A-Y FE	0.892*** (0.105)	0.011 (0.021)	-0.025 (0.024)	0.004 (0.023)	-0.015 (0.027)	0.032 (0.028)	0.071* (0.038)	0.024 (0.031)	0.102 (0.072)
(4)	Baseline + R-S-Y & R-A-Y FE	0.842*** (0.160)	0.009 (0.020)	-0.028 (0.026)	0.003 (0.023)	-0.021 (0.028)	-0.003 (0.030)	0.090 (0.067)	0.019 (0.026)	0.092 (0.076)
<u>Panel B: Federal Eligibility</u>										
(5)	Baseline	0.210*** (0.029)	-0.020 (0.019)	-0.074*** (0.025)	-0.023 (0.020)	-0.074*** (0.025)	0.009 (0.020)	-0.003 (0.038)	0.025 (0.019)	0.087*** (0.029)
(6)	EITC & School Spending	0.205*** (0.030)	-0.013 (0.019)	-0.072*** (0.026)	-0.015 (0.020)	-0.072*** (0.026)	0.021 (0.022)	0.001 (0.041)	0.021 (0.022)	0.088*** (0.031)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2008-2012 ACS. Each cell in the table comes from a separate regression (N=3957). The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the dependent variable, and the "RF" columns refer to models that use fixed simulated eligibility as the independent variable. All estimates include an indicator for the cell being nonwhite or not as well as race-by-age fixed effects, race-by-calendar year fixed effects and race-by-state of birth fixed effects. Rows 3 and 4 include race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 5: IV Estimats of the Effect of Average Medicaid Eligibility During School Years on Educational Attainment, by Age at Eligibility

Age Range	No HS Diploma	Any College	College Plus
<u>Panel A: Baseline Model</u>			
0-3	-0.011 (0.014)	0.017 (0.014)	-0.006 (0.007)
4-8	-0.031** (0.010)	0.012 (0.014)	0.038** (0.014)
9-13	0.008 (0.009)	-0.026* (0.015)	-0.027 (0.017)
14-17	-0.012 (0.009)	0.058** (0.011)	0.068** (0.015)
<u>Panel B: Baseline + R-S-Y and R-A-Y FE</u>			
0-3	0.000 (0.021)	-0.007 (0.031)	0.052 (0.038)
4-8	-0.027 (0.020)	0.024 (0.025)	0.052 (0.058)
9-13	0.004 (0.011)	0.013 (0.028)	-0.002 (0.022)
14-17	-0.014 (0.015)	0.070** (0.027)	0.023 (0.024)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2005-2012 ACS. Each cell in the table comes from a separate regression (N=5480). All estimates include an indicator for the cell being nonwhite or not, race-by-age-fixed effects, race-by-calendar year fixed effects and race-by-state of birth fixed effects. Estimates in Panel B also includes race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 6: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment, Including State of Birth Linear Time Trends Separately by Race

			No HS			Some College			College Plus		
Specification		1 st Stage	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
<u>Panel A: All Eligibility</u>											
(1)	Baseline	0.856*** (0.097)	-0.027* (0.014)	-0.025* (0.015)	-0.029* (0.017)	-0.002 (0.018)	0.001 (0.021)	0.001 (0.024)	0.011 (0.023)	0.042 (0.033)	0.049 (0.039)
(2)	EITC & School Spending	0.915*** (0.063)	-0.020 (0.014)	-0.032** (0.015)	-0.035** (0.016)	-0.003 (0.019)	-0.000 (0.020)	-0.001 (0.021)	0.008 (0.025)	0.048 (0.035)	0.053 (0.038)
(3)	EITC, School Spending, R-S-Y & R-A-Y FE	0.812*** (0.099)	0.013 (0.020)	-0.021 (0.026)	-0.025 (0.029)	0.031 (0.035)	0.059* (0.035)	0.072* (0.040)	0.049 (0.036)	0.070 (0.072)	0.087 (0.082)
(4)	Baseline + R-S-Y & R-A-Y FE	0.735*** (0.157)	0.011 (0.020)	-0.019 (0.025)	-0.025 (0.032)	0.006 (0.031)	0.085* (0.048)	0.115 (0.074)	0.044 (0.034)	0.057 (0.080)	0.078 (0.098)
<u>Panel B: Federal Eligibility</u>											
(5)	Baseline	0.215*** (0.031)	-0.027* (0.014)	-0.012*** (0.004)	-0.057*** (0.020)	-0.002 (0.018)	0.003 (0.007)	0.016 (0.031)	0.011 (0.023)	0.017*** (0.006)	0.081*** (0.027)
(6)	EITC & School Spending	0.214*** (0.031)	-0.020 (0.014)	-0.011*** (0.004)	-0.053*** (0.020)	-0.003 (0.019)	0.003 (0.007)	0.016 (0.031)	0.008 (0.025)	0.017*** (0.006)	0.080*** (0.026)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2005-2012 ACS. Each cell in the table comes from a separate regression (N=5480). The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the dependent variable, and the "IV" columns refer to models that use fixed simulated eligibility as the independent variable. All estimates include an indicator for the cell being nonwhite or not as well as race-by-age fixed effects, race-by-calendar year fixed effects and race-by-state of birth fixed effects. Rows 3 and 4 include race by state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects. Estimates also include race by state of birth linear time trends. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table 7: Placebo Test with Randomly Assigned Medicaid Eligibility

	No HS Graduation		Some College		BA	
	RF	IV	RF	IV	RF	IV
Baseline	$1.95e^{-5}$ (-0.007, 0.007)	$2.28e^{-5}$ (-0.008, 0.008)	$-3.42e^{-5}$ (-0.010, 0.011)	$-4.54e^{-5}$ (-0.011, 0.012)	-0.0001 (-0.009, 0.009)	-0.0002 (-0.010, 0.010)
Baseline + R-S-Y & R-A-Y FE	0.0002 (-0.007, 0.007)	0.0002 (-0.008, 0.008)	$-5.09e^{-5}$ (-0.010, 0.011)	$-8.32e^{-5}$ (-0.012, 0.013)	-0.0003 (-0.010, 0.011)	-0.0004 (-0.011, 0.012)

Source: Authors' estimation of equation (3) in the text using 22-29 year old respondents from the 2005-2012 ACS. We randomly assign age-state-year eligibility and fixed simulated eligibility, as a pair, across different age-state-year cells. This is done separately by race. We conduct 500 separate simulations for each outcome, both including and excluding state of birth by calendar year (R-S-Y) fixed effects and race by age by calendar year (R-A-Y) fixed effects. All estimates include an indicator for the cell being non-white or not as well as race-by-age fixed effects, race-by-calendar year fixed effects and race-by-state of birth fixed effects. The table shows the mean estimate across all 500 runs, as well as the 2.5th and 97.5th percentiles in parentheses. The range in parentheses thus show the non-parametric 95% confidence interval. IV estimates are constructed by dividing the reduced form (RF) by the first stage, which also is estimated using this method. First stage estimates are available upon request from the authors.

Table 8: The Effect of Average Medicaid Eligibility During School Years on Teen Health Outcomes and Behaviors, Observed Between Ages 14-18

Dependent Variable	Mean	OLS	IV
Risky Sex Index (0-4) (n=1441)	0.962	-0.082 (0.209)	-0.034 (0.302)
Ever Had Sex (n=1400)	0.481	0.125 (0.102)	0.172 (0.181)
Body Mass Index (n=1122)	23.210	-0.107 (1.006)	-0.897 (1.214)
Overweight (n=1122)	0.258	-0.040 (0.061)	-0.163 (0.107)
Obese (n=1122)	0.086	-0.052 (0.071)	-0.076 (0.070)
Ever Use Marijuana (n=1441)	0.432	-0.054 (0.162)	-0.174 (0.209)
Number of Days Drank Past Month (n=1439)	2.690	-0.605 (1.113)	-2.832* (1.708)
Ever Smoke Regularly (n=1436)	0.197	-0.069 (0.113)	-0.233 (0.157)
Any Mental Health Issue (n=1441)	0.223	0.002 (0.124)	-0.112 (0.105)
Eating Disorder (n=1440)	0.101	-0.180*** (0.059)	-0.327*** (0.077)

Source: Authors' estimation of equation (3) in the text using 14-18 year old respondents from the 1995-2007 YRBS. Each cell in the table comes from a separate regression. Risky sex index is a count variable that includes ever had sex, no birth control last sexual encounter (a combination of no birth control and condom use variables), ever pregnant, and used alcohol or drugs last sexual encounter. See Appendix Table A-3 for estimates of individual components of the Risky Sex Index as well as the variables from which we calculate whether an individual has a mental health issue or an eating disorder. Medicaid eligibility is calculated as average from age 0 to current age, so for age 14 it is 0-14. The "OLS" column refers to models that use a three-year moving average of actual eligibility as the independent variable, and the "IV" column refers to models that instrument actual eligibility with fixed simulated eligibility. The first stage coefficient is 0.882 (se=0.123). The estimates include indicators for nonwhite as well as race by age, race by calendar year, and race by state of residence fixed effects. Standard errors clustered at the state-of-residence level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table A-1: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment using the White Sample

			No HS			Some College			College Plus		
Specification		1 st Stage	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
<u>Panel A: All Eligibility</u>											
(1)	Baseline	0.706*** (0.134)	-0.022 (0.018)	-0.018 (0.023)	-0.025 (0.032)	-0.005 (0.032)	0.014 (0.029)	0.019 (0.042)	0.020 (0.042)	0.051 (0.049)	0.073 (0.074)
(2)	EITC & School Spending	0.801*** (0.084)	-0.012 (0.019)	-0.030 (0.024)	-0.037 (0.031)	-0.003 (0.033)	0.013 (0.028)	0.016 (0.035)	0.020 (0.046)	0.061 (0.052)	0.076 (0.067)
(3)	EITC, School Spending, S-Y and A-Y FE	0.745*** (0.125)	0.017 (0.022)	-0.028 (0.033)	-0.038 (0.043)	0.029 (0.050)	0.075 (0.046)	0.101* (0.057)	0.102 (0.064)	0.098 (0.091)	0.132 (0.118)
(4)	Baseline + S-Y and A-Y FE	0.656*** (0.190)	0.016 (0.019)	-0.026 (0.031)	-0.039 (0.048)	-0.009 (0.041)	0.105* (0.058)	0.159 (0.109)	0.088 (0.059)	0.079 (0.100)	0.121 (0.141)
<u>Panel B: Federal Eligibility</u>											
(5)	Baseline	0.234*** (0.027)	-0.022 (0.018)	-0.012 (0.010)	-0.052 (0.040)	-0.005 (0.032)	0.030** (0.015)	0.127** (0.061)	0.020 (0.042)	0.059*** (0.015)	0.250*** (0.066)
(6)	EITC & School Spending	0.236*** (0.028)	-0.012 (0.019)	-0.012 (0.010)	-0.052 (0.040)	-0.003 (0.033)	0.031** (0.015)	0.129** (0.062)	0.020 (0.046)	0.060*** (0.015)	0.253*** (0.069)

Source: Authors' estimation of equation (3) in the text using 22-29 year old white respondents from the 2005-2012 ACS. Each cell in the table comes from a separate regression (N=2754). The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the dependent variable, and the "RF" columns refer to models that use fixed simulated eligibility as the independent variable. All estimates include age fixed effects, calendar year fixed effects and state of birth fixed effects. Rows 3 and 4 include state of birth by calendar year (S-Y) fixed effects and age by calendar year (A-Y) fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table A-2: The Effect of Average Medicaid Eligibility During School Years on Educational Attainment using the Nonwhite Sample

			No HS			Some College			College Plus		
Specification		1 st Stage	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
<u>Panel A: All Eligibility</u>											
(1)	Baseline	1.128*** (0.137)	-0.035* (0.019)	-0.053** (0.023)	-0.047** (0.020)	0.039 (0.025)	0.042 (0.031)	0.036 (0.027)	0.019 (0.014)	0.059*** (0.019)	0.053*** (0.019)
(2)	EITC & School Spending	1.095*** (0.100)	-0.035* (0.019)	-0.050** (0.022)	-0.045** (0.019)	0.041 (0.027)	0.040 (0.034)	0.036 (0.030)	0.012 (0.016)	0.057*** (0.020)	0.052*** (0.019)
(3)	EITC, School Spending, S-Y & A-Y FE	1.059*** (0.182)	-0.012 (0.031)	-0.011 (0.046)	-0.011 (0.039)	0.058 (0.045)	0.051 (0.083)	0.050 (0.069)	0.001 (0.032)	0.090* (0.048)	0.087* (0.050)
(4)	Baseline + S-Y & A-Y FE	1.101*** (0.256)	-0.013 (0.030)	-0.015 (0.049)	-0.014 (0.039)	0.022 (0.051)	0.033 (0.087)	0.032 (0.071)	-0.000 (0.029)	0.079 (0.053)	0.074 (0.056)
<u>Panel B: Federal Eligibility</u>											
(5)	Baseline	0.206*** (0.032)	-0.035* (0.019)	-0.012** (0.005)	-0.056** (0.027)	0.039 (0.025)	-0.004 (0.008)	-0.018 (0.036)	0.019 (0.014)	0.007 (0.007)	0.034 (0.031)
(6)	EITC & School Spending	0.200*** (0.033)	-0.035* (0.019)	-0.012** (0.005)	-0.059** (0.029)	0.041 (0.027)	-0.004 (0.008)	-0.021 (0.038)	0.012 (0.016)	0.006 (0.007)	0.031 (0.033)

Source: Authors' estimation of equation (3) in the text using 22-29 year old non-white respondents from the 2005-2012 ACS. Each cell in the table comes from a separate regression (N=2726). The "OLS" columns refer to models that use a three-year moving average of actual eligibility as the dependent variable, and the "RF" columns refer to models that use fixed simulated eligibility as the independent variable. All estimates include age fixed effects, calendar year fixed effects and state of birth fixed effects. Rows 3 and 4 include state of birth by calendar year (S-Y) fixed effects and age by calendar year (A-Y) fixed effects. Standard errors clustered at the state-of-birth level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Table A-3: The Effect of Average Medicaid Eligibility During School Years on Teen Health Outcomes and Behaviors, Observed Between Ages 14-18

Dependent Variable	Mean	OLS	IV
Risky Sex Index (0-4) (n=1441)	0.962	-0.082 (0.209)	-0.034 (0.302)
Ever Had Sex (n=1400)	0.481	0.125 (0.102)	0.172 (0.181)
No Birth Control (n=1338)	0.230	-0.080 (0.166)	-0.214 (0.210)
Used Condom Last Encounter (n=1338)	0.633	0.222 (0.180)	0.217 (0.188)
Ever Pregnant or Impregnate Partner (n=1057)	0.051	0.063 (0.051)	0.072 (0.073)
Used Drugs or Alcohol Last Sexual Encounter (n=1339)	0.243	-0.091 (0.146)	-0.114 (0.211)
Any Mental Health Issue (n=1441)	0.223	0.002 (0.124)	-0.112 (0.105)
Considered Suicide (n=1441)	0.186	-0.008 (0.120)	-0.049 (0.105)
Planned Suicide (n=1441)	0.149	0.021 (0.120)	-0.102 (0.127)
Attempted Suicide (n=1438)	0.084	-0.033 (0.067)	-0.154* (0.083)
Hurt Self Attempting Suicide (n=1438)	0.026	-0.047 (0.033)	-0.131*** (0.048)
Number of Suicide Attempts (n=1438)	0.180	-0.103 (0.160)	-0.449** (0.228)
Eating Disorder (n=1440)	0.101	-0.180*** (0.059)	-0.327*** (0.077)
Bulimic (n=1440)	0.050	-0.091** (0.044)	-0.163** (0.064)
Used Diet Pills (n=1440)	0.074	-0.129*** (0.044)	-0.205*** (0.052)

Source: Authors' estimation of equation (3) in the text using 14-18 year old respondents from the 1995-2007 YRBS. Each cell in the table comes from a separate regression, and the indented variables make up the index listed above them. Any mental health issue is coded as 1 if an individual reports any of the suicide ideation variables included in the table. Any eating disorder includes both bulimia and diet pill use. Medicaid eligibility is calculated as average from age 0 to current age, so for age 14 it is 0-14. The "OLS" column refers to models that use a three-year moving average of actual eligibility as the independent variable, and the "IV" column refers to models that instrument actual eligibility with fixed simulated eligibility. The first stage coefficient is 0.882 (se=0.123). The estimates include indicators for nonwhite as well as race by age, race by calendar year, and race by state of residence fixed effects. Standard errors clustered at the state-of-residence level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

Exhibit 36

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

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The Effect of Welfare Reform on Immigrant Children

by Gillian Dutton

These changes, enacted as part of welfare reform, emerged from highly contentious debates about the role of working-age immigrants in today's society—their impact on labor markets, dependence on public assistance, and contributions to the changing ethnic composition of our citizenry. Immigrant children, who will be profoundly affected, were essentially invisible in those debates.¹

The enactment of new welfare reform and immigration laws in the summer of 1996 occurred at the apogee of a period characterized by intense hostility toward immigrants, including both those who were legally residing in the United States as well as those who were here without documents. During the two and a half years since then, federal legislation has restored some of the public assistance safety net for immigrants residing within the United States as of August 22, 1996. Most immigrants

arriving after that date, however, face serious obstacles to eligibility. State programs created to bridge some of the gaps in the federal safety net remain underutilized.

Access for those still eligible for federal and state assistance has been severely hampered by a number of indirect consequences of the new laws. The complexity of eligibility determinations and ongoing barriers to assistance caused by limited English skills make it likely that agencies will incorrectly deny benefits to immigrants who do apply. Increased fear that receipt of benefits may preclude obtaining legal permanent residence status or citizenship and the chilling effect of new verification and reporting procedures have further reduced the likelihood that immigrants will even seek assistance. Research reflects a startling decline in immigrant access to assistance and supports the growing fear that vulnerable groups such as children may bear lasting impact of these years even as antiimmigrant sentiment begins to wane.² By familiarizing themselves with

¹ FROM GENERATION TO GENERATION: THE HEALTH AND WELL-BEING OF CHILDREN IN IMMIGRANT FAMILIES xvi (Donald J. Hernandez & Evan Charney eds., 1998). The views expressed in this article are mine and do not necessarily reflect those of the Northwest Justice Project or the University of Washington. I would like to thank the National Immigration Law Center, and in particular Tanya Broder and Josh Bernstein, for materials used in this article.

² Recent data from Los Angeles County show a 70-percent decline in the number of monthly approved CalWORKS (California's Temporary Assistance for Needy Families program) applications for legal-immigrant-headed households between January 1996 and January 1998. This decline occurred even though there was no change in eligibility, as state programs filled the gaps left by changes in federal law. See WENDY ZIMMERMAN & MICHAEL FIX, URBAN INST., DECLINING IMMIGRANT APPLICATIONS FOR MEDI-CAL AND WELFARE BENEFITS IN LOS ANGELES COUNTY (July 1998) <www.urban.org/immig/lacounty/html>.

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the most common barriers to assistance and ways to overcome them, advocates can do much to help immigrant children access the benefits they need to lead better lives.

I. Introduction

The decline in access to benefits often falls hardest upon children in immigrant families.³ For children, access to good nutrition and preventive health care is crucial to both physical and intellectual development.⁴ Studies have shown that access to health care is greatly dependent upon both health insurance coverage and contact with a usual source of care; immigrant children are three times less likely as children born in the United States to have these important protections.⁵ Then, too, children in immigrant families have higher poverty rates than children in native families, and the rates are much higher for certain immigrant groups from Southeast Asia and Central America.⁶

Children in immigrant families also have higher rates of other factors associated with negative impact on health. They are more likely to live in overcrowded housing, in families with many siblings where the language spoken at home is not English, and with parents who have less than an eighth-grade education.⁷ Children in refugee families who fled their native countries because of war and other catastrophic events are at increased risk of mental health problems due to these traumatic experiences.⁸ Studies have shown that children in immigrant families are at

particular risk of certain infectious diseases, such as drug-resistant tuberculosis and hepatitis B.⁹ These children are also at higher risk of environmental factors, such as pesticides and lead exposure, which are associated with a number of health problems, including negative effects on brain development.¹⁰ They have higher rates of poor vision, poor dental health, and unintentional injury.¹¹

State and federal legislators, even those who have been eager to cut benefits for immigrant adults, are aware of the special vulnerability of children.¹² This concern for children has led to the decision by a number of states to provide some limited coverage for nonqualified children particularly in the area of health care.¹³ Moreover, research has shown that providing for regular preventive health care is more cost-effective than paying for emergency intervention, which is still provided to nonqualified low-income children eligible for emergency Medicaid under federal law. Providing prenatal care is also more cost-effective. These are many states' considerations in deciding to supplement the federally funded Women, Infant, and Child Nutrition program, Head-Start, school lunches, and emergency Medicaid programs with state-funded medical assistance for pregnant women and children regardless of immigration status.¹⁴

Ironically, while some states have continued programs that benefit children regardless of their immigration status, access to benefits for legal permanent res-

³ One in every five children living in the United States—14 million children total—is an immigrant or has immigrant parents. Over three-fourths of these children were born in the United States. FROM GENERATION TO GENERATION, *supra* note 1, at 17.

⁴ *Id.* at 10.

⁵ *Id.*

⁶ *Id.* at 42–44.

⁷ *Id.* at 51–55.

⁸ *Id.* at 57–58.

⁹ *Id.* at 70–72.

¹⁰ *Id.* at 72–74.

¹¹ *Id.* at 78.

¹² The Urban Institute estimates that over one million children under 18 in the United States today are undocumented. See JEFFREY S. PASSEL & JOAN R. KAHN, URBAN INST., IMMIGRATION, FERTILITY, AND THE FUTURE AMERICAN WORK FORCE (1998) (report to the Social Security Administration).

¹³ Fifteen states, including California, Massachusetts, New York, and Washington, provide some medical assistance for those not covered by federal Medicaid programs.

¹⁴ ZIMMERMAN & FIX, *supra* note 2.

ident and citizen children in immigrant families has declined due to new verification and reporting requirements and stricter requirements for obtaining legal permanent residence. Many children eligible to become legal permanent residents remain without permanent legal residence status because of their parents' inability to pay to file papers or to meet the new financial support requirements.¹⁵ Other children are in fact already legal permanent residents or citizens but live with families with a parent or a sibling whose undocumented status is behind a fear of accessing care.

Although the 1996 welfare law significantly reduced immigrant eligibility for benefits, advocates—including those working within organizations restricted by Legal Services Corporation regulations limiting representation to certain immigrants and prohibiting challenges to welfare reform—can take many steps to give immigrant children a better chance.¹⁶ In this article I focus on specific areas where children's access to care can be improved, even within the current restrictions. I describe concrete approaches and identify resources for advocates who would maximize the chance for immigrant children to receive the federal and state benefits for which they are eligible without jeopardizing their or family members' immigration status and safety.¹⁷

II. Immigrant Eligibility Determinations and Services

Determining public assistance eligibility for immigrants has always been complicated by most agency staff's very little knowledge of immigration status; too often agency staff deny assistance as a result. Recent laws have added to the confusion in several ways. First, the federal definition of eligibility for certain programs has changed several times due to both subsequent legislation and detailed regulations, making it difficult for staff to apply eligibility determinations correctly unless their manuals are both up-to-date and thorough. Second, press coverage about the harsh measures, while serving to alert both voters and their representatives to the injustice of some of the cuts, has also had the effect of making immigrants and agency staff alike believe that few programs are available to them.

One initial step advocates should take is to review their state's implementing legislation and regulations to ensure that eligibility rules include recent changes and sufficient explanation of immigration categories for making correct determinations. This is especially true in states with a recent immigrant population but no long history of immigrant settlement.¹⁸ A subsequent

¹⁵ The Immigration and Naturalization Service (INS) estimates that 60 percent of the family visas approved in 1996 would not have been approved if the new affidavit-of-support form published in December 1997 had been required.

¹⁶ The general prohibition on the provision of legal assistance to ineligible aliens by advocates funded by the Legal Services Corporation (LSC) is set out in 45 C.F.R. pt. 1626 (1997). Eligible aliens include the following: an alien lawfully admitted for permanent residence; an alien who is the spouse, parent, or unmarried child under 21 of a U.S. citizen and who has filed an application for adjustment of status to lawful permanent residence; a refugee; an asylee; a conditional entrant under Section 203(a)(7) of the Immigration and Nationality Act; a lawful temporary resident under the "SAW" program; an H-2A temporary worker; and an alien granted withholding of deportation. The alien restriction does not apply to certain representation of domestic violence victims if non-LSC funding is used. LSC regulations prohibit recipients of LSC funds from participating in litigation that challenges laws or regulations enacted as part of a federal or state welfare system (defined as Aid to Families with Dependent Children and programs designed to replace or modify it). 42 U.S.C. § 1639.

¹⁷ For additional information on the effect of welfare reform on immigrants see Tanya Broder, National Immigration Law Ctr., *State and Local Policies on Immigrants and Public Benefits: Responding to the 1996 Welfare Law*, 31 CLEARINGHOUSE REV. 503 (Jan.-Feb. 1998), and my forthcoming article in the same publication.

¹⁸ In the 1980s 76 percent of immigrants arriving in the United States resided in six states—California, Florida, Illinois, New Jersey, New York, and Texas. FROM GENERATION TO GENERATION, *supra* note 1, at 20. Between 1990 and 1996 the number of immigrants settling in the 40 states with no history of immigrant settlement rose by 50 percent—from 3.7 million to 5.6 million. Wendy Zimmerman & Michael Fix, *The Legacies of Welfare Reform's Immigrant Restrictions*, 75 INTERPRETER RELEASES 1577, 1580 (1998).

step is to ensure that once the laws are implemented, immigrant access is not hampered by lack of interpreter services or by discriminatory treatment by staff.

A. Immigration Status and Eligibility

The failure of agency staff to understand the variety of immigration documents and the information the documents convey is a most common reason for incorrect denials.

1. Understanding Immigration Documents

While immigrants who have adjusted to permanent legal resident status usually have a document called an I-551 ("green card"), many others who are legally in the United States, even in "qualified" status, may not. Other documents commonly used by immigrants are an I-94, or Arrival/Departure Record, and an Employment Authorization Document I-688B. All three use a different set of codes to indicate the holder's immigration status.¹⁹

Further, some states have enacted state-funded programs which are not limited to "qualified" immigrants but instead assist "legal" residents or a group referred

to as "permanently residing under color of law."²⁰ Advocates should check to see what definitions are given of these terms and should ensure that state welfare department manuals contain lists of these distinct immigration categories and explanations of what documents may be supplied. Every manual should contain guidelines explaining that immigration documents vary widely.

Although most states adopt federally prescribed verification procedures, such procedures should not ignore individual program requirements that direct agency staff to assist the individual in obtaining necessary documentation. This is especially true for documents necessary to prove citizenship. Persons who were born abroad but who derived citizenship from one or both of their parents being a citizen are examples of citizens who often have no documents to prove citizenship.²¹ Children whose parents both "naturalize" or become citizens before the child's 18th birthday, and thereby cause the child to be naturalized as well, are other examples (increasingly discovered as many legal permanent residents rush to become citizens). Such naturalized children are often completely unaware of their having become citizens.²²

¹⁹ Thus, for instance, a refugee who first enters the United States typically has an I-94 marked "admitted under Section 207." Once that same refugee adjusts to legal permanent resident status, he or she is issued a legal permanent resident card with a code that is usually RE6, RE7, RE8, or R86 to indicate refugee status. The code on that same person's employment authorization card is listed as 274a.12(a)(4). For a list of typical codes see NATIONAL IMMIGRATION LAW CTR., GUIDE TO ALIEN ELIGIBILITY FOR FEDERAL PROGRAMS (3d ed. 1994). For a list of documents that may be used see Interim Guidance on Verification of Citizenship, Qualified Alien Status, and Eligibility Under Title XVI of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, 62 Fed. Reg. 61344 (1997).

²⁰ "Permanently residing under color of law" (PRUCOL) is a public assistance term referring to a group of immigration classifications. It is most often found as a list of immigration statuses, including precise terms such as "registry applicant" as well as broad terms such as "person known to the INS whom the INS does not intend to deport." While PRUCOL has been replaced by the narrower term "qualified" for federal programs, many states are still covering a broader group of immigrants for state programs. For a discussion of the evolution of the term see Robert Rubin, *Walking a Gray Line: The "Color of Law" Test Governing Noncitizen Eligibility for Public Benefits*, 24 SAN DIEGO L. REV. 411 (1987).

²¹ See Interim Guidance, *supra* note 19, for some examples. The Interim Guidance lists only a few of the more common examples and refers the welfare agency to INS for more information. Advocates should check to see whether guidelines specify the provision of assistance pending verification and help, including payment, to individuals who may need to pay to obtain documentation. For a detailed explanation of derivative naturalization see DANIEL LEVY, NATIONAL IMMIGRATION LAW CTR., U.S. CITIZENSHIP & NATURALIZATION HANDBOOK (1999).

²² Advocates can often assist immigrants who have lost immigration documents by filing a Freedom of Information Act request with INS. However, advocates must be aware that even such a request is not undertaken without some risk to the client since a review of the file may trigger adverse action from INS. If in doubt, advocates should consult an immigration specialist first.

2. Determining Date of Arrival

When determining immigrant eligibility for assistance, knowing the immigrant's date of arrival is most important after ascertaining what kind of status the immigrant has. The Personal Responsibility and Work Opportunity Reconciliation Act provided that most immigrants arriving on or after August 22, 1996, would be barred from receiving federal means-tested benefits for a period of five years.²³ In Verification Guidelines issued on November 19, 1997, the Immigration and Naturalization Service (INS) clarified that "arriving" meant physically arriving and stated that the bar did not apply to those who "entered the United States prior to August 22, 1996, and were continuously present in the United States until attaining qualified alien status."²⁴ Many currently undocumented children, some of whom have not yet adjusted to legal permanent resident status because of long waiting lists for visas, others because their parents have still not applied for them, will eventually receive a legal permanent resident card.²⁵ In most cases the date on the card is the date of adjustment—not the date of physical entry. Both immigrants and state welfare staff have to be

alerted to the children's exemption from the five-year bar.

3. Understanding Exemptions

Like their parents, immigrant children now face one new hurdle that is as problematic as it is mundane. The incredibly complex determination of immigrant eligibility is now one that varies among federal and state programs and that can result in denials unless workers are well trained, careful, and experienced. For federal programs, in addition to determining the date of arrival and whether the immigrant is "qualified," eligibility workers must take into account whether an immigrant applicant falls within a special group of "humanitarian" immigrant categories. These include refugees, asylees, Amerasians, Cuban Haitian entrants, persons granted withholding of deportation/removal, and persons granted conditional entry.²⁶ In most cases workers must also consider two other factors to determine eligibility—whether the children and their parents have a work history totaling 40 quarters and whether an applicant or applicant's parent or spouse is either a veteran or active-duty service member of the U.S. armed forces.²⁷ For Supplemental

²³ Personal Responsibility and Work Opportunity Reconciliation Act, Pub. L. No. 104-193, 110 Stat. 2105 (1996). Federal means-tested benefits have so far been defined as Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI), food stamps, and Medicaid (including Children's Health Insurance Program).

²⁴ Interim Guidance, *supra* note 19, at 61350.

²⁵ Although U.S. citizens may petition for a visa for spouses, parents, and children without a long wait, legal permanent residents who may petition for their spouses and unmarried children under 21 often face long waits for visas, especially if they come from countries with a large number of pending applications, such as Mexico and the Philippines.

²⁶ These immigrants, even if they arrived after August 22, 1996, are exempt from the five-year bar. If they meet program eligibility requirements, they must be provided with SSI, food stamps, and SSI-related Medicaid during the first seven years after arrival.

²⁷ Special protection is provided to immigrants in the "qualified" category who can show that they have 40 quarters (or ten years) of work history established by Social Security Administration records. Although the amounts vary according to the year, a worker in 1997 could earn four work quarters by earning approximately \$2,680 during the year. Work quarters can be "credited" to a spouse and to children under 18. The spouse loses the credited quarters upon divorce. The children do not ever lose the credited quarters and even get credit for quarters earned before they were born. For purposes of this exemption, no "credit" may be given for quarters earned after January 1, 1997, if a federal means-tested benefit is received in that quarter. The 40-quarter credit ensures that these immigrants must be provided TANF, SSI, food stamps, and Medicaid as long as otherwise eligible. This exemption does not protect immigrants arriving after August 22, 1996, from the five-year bar. The armed forces or "veteran" exemption ensures eligibility for federal public benefits and protects against the five-year bar.

Security Income (SSI), public assistance agencies must determine whether the individual was receiving benefits or at least present in the United States on August 22, 1996.²⁸ For food stamps, the worker needs to know if the person was present in the United States on August 22, 1996, and the person's age at that time.²⁹

Frequent problems in determining immigrant eligibility include failure to ask

proper work authorization. Advocates should not forget to advise a client anxious to obtain credit for 40 quarters that as of December 1, 1997, a quarter may be counted only if the client received no federal means-tested benefit in that quarter.

4. Ensuring That State Programs Are Utilized

States have responded to the needs of immigrants by enacting a whole series of programs, from SSI replacements to cash assistance for families, food stamp programs, and medical assistance programs.³⁰ Unfortunately these programs continue to be underused, in part because of fear of verification and reporting and in part because state programs have not done sufficient outreach to counteract the impression of ineligibility left by terminations from the federal programs.³¹ This problem is particularly severe in programs sending out conflicting information in successive letters and notices. To ensure that immigrants are familiar with the programs for which they are eligible, advocates should encourage states to target community-based organizations and ethnic media with information about the availability of new programs. Advocates also may find that participating with state and local offices that develop these campaigns can be useful in ensuring that the information sent out in such campaigns is culturally appropriate.

B. Access to Interpreter Services

Research has long shown that, for immigrants who speak limited English,

To ensure that immigrants are familiar with the programs for which they are eligible, advocates should encourage states to target community-based organizations and ethnic media with information about the availability of new programs.

about a parent's work quarters and confusion about applying the veteran exception. Advocates should review welfare agency manuals to make sure they clearly explain how to apply these exemptions. They should also assist immigrants in checking that work quarters are correctly credited since the 40 quarters of work history is used not only to determine eligibility for a program but also increasingly to determine when the obligations under the new affidavit of support are no longer in effect. Immigrants who wish to obtain credit for unreported work should consult with an advocate if some of the work was performed without

²⁸ The Noncitizen Benefit Clarification and Other Technical Amendments Act of 1998 ensures that all immigrants receiving SSI on August 22, 1996, may continue to be eligible. SSI is provided for any immigrant who was not on assistance but who was in the United States on August 22, 1996, as long as the immigrant is "qualified" and "disabled" (whether or not he or she is 65). For a discussion of immigrant eligibility for SSI and social security see Ethel Zelenske, *Representing Noncitizens in Social Security and Supplemental Security Income Claims*, 32 CLEARINGHOUSE REV. 100 (Jul.-Aug.1998).

²⁹ The Agricultural Research, Extension, and Education Reform Act of 1998 added to the food-stamp-eligible list qualified immigrants who were in the United States as of August 22, 1996, and (1) are under 18, (2) were 65 or older as of that date, or (3) are receiving disability-related assistance. This legislation also extended protection for refugee groups from five to seven years and introduced a new group, Hmong and Highland Laotians, to the category of immigrants eligible for federal food stamps.

³⁰ For more information see KELLY CARMODY, CENTER ON BUDGET & POLICY PRIORITIES, STATE OPTIONS TO ASSIST LEGAL IMMIGRANTS INELIGIBLE FOR FEDERAL BENEFITS (1998); Zimmerman & Fix, *supra* note 18.

³¹ See ZIMMERMAN & FIX, *supra* note 2.

adequate access to social and health services can be assured only when the agencies provide interpreter and translation services.³² Advocates, prompted in part by this research, have for years sought to ensure that programs receiving federal financial assistance comply with the Civil Rights Act's Title VI requirements prohibiting discrimination on the basis of national origin.³³

1. Access to Social Services and Health Services

While many state welfare departments use bilingual staff or interpreters for oral communication and give recipients translated notices, advocates should check that their state has implemented an effective program to provide such services.³⁴ At a minimum the state should have a system for identifying the applicant's language, should have qualified bilingual staff or interpreters, and should have a system for translating documents which require a response from the applicant/recipient or give information about the reduction or termination of benefits.³⁵

Particular care should be taken to monitor the provision of interpreter services in the context of managed care. Implementation of managed care in medical assistance programs for low-income residents requires translated materials

explaining patient rights to emergency services. Telephonic interpreter services are also increasingly needed for emergency contacts since, under managed care, screening for urgent health needs no longer takes place in hospitals but by phone with on-call nurses, health insurance company representatives, or individual doctor's offices. Advocates should be sure to work with their regional U.S. Department of Health and Human Services Office for Civil Rights, which routinely investigates compliance with regulations.

2. English as a Second Language and Training under Welfare-to-Work Programs

With the implementation of welfare-to-work programs, parents of immigrant children in low-income families face the same work requirements and time limits as their citizen counterparts. Immigrant children and their families are even more vulnerable to the potential loss of cash assistance for two reasons. First, although immigrant families, with the exception of refugees, receive proportionally no more assistance than nonimmigrant families, their incomes are lower, that is, children in these families have less disposable income to meet needs for health care, nutrition, and clothing.³⁶

³² For a brief history on the evolution of interpreting services in health care see Robert W. Putsch, *Cross-cultural Communication: The Special Case of Interpreters in Health Care*, 254 JAMA 3344 (1985).

³³ Title VI of the Civil Rights Act of 1964, 42 U.S.C. § 2000d. For a discussion of legal bases for challenging the lack of multilingual AFDC/TANF services see CENTER ON SOC. WELFARE POLICY & LAW, PUB. NO. 646, SECURING ACCESS TO MULTILINGUAL AFDC PROCEDURES, PERSONNEL, AND NOTICES (1995). For a very helpful and thorough manual on multilingual services for medical assistance see JANE PERKINS ET AL., NATIONAL HEALTH LAW PROGRAM, ENSURING LINGUISTIC ACCESS IN HEALTH CARE SETTINGS: LEGAL RIGHTS AND RESPONSIBILITIES (1998). According to the Department of Health and Human Services Office for Civil Rights, "a common cause of discrimination on the basis of national origin is the use of ineffective methods of communication between English-speaking staff and persons who, because of their national origin, are limited English proficient." Office for Civil Rights, U.S. Dep't of Health & Human Servs., Guidance Memorandum, Title VI Prohibition Against National Origin Discrimination—Persons with Limited-English Proficiency (1998) (available by searching for "Guidance Memorandum" in the U.S. Department of Health & Human Services Web site, www.hhs.gov).

³⁴ The National Health Law Program cites the large number of different languages spoken, inadequate funding for interpreter services, lack of familiarity of legal requirements, and failure to enforce applicable laws as reasons why so many Medicaid providers lack necessary services. See *Ensuring Linguistic Access in Health Care Settings*, HEALTH ADVOCATE, Winter 1998, at 1.

³⁵ Office for Civil Rights, Guidance Memorandum, *supra* note 33.

³⁶ On nonimmigrant families see Zimmerman & Fix, *supra* note 18, at 1586.

Second, limited-English speakers are not always given the interpreter assistance necessary for successful completion of job search and other welfare participation requirements and are then sanctioned for failing to contact employers or follow through on orientation programs. Because they often receive only English-language notices and lack knowledge about the programs, many are not familiar with the due process rights accorded them in eligibility determinations and often resign themselves to accepting a reduced grant. For some, lack of appropriate interpreters means that they are never screened for family violence, physical or mental impairment, or the presence of a disabled family member needing care. For those who are lucky enough to find jobs despite being unable to speak English, wage and skill improvement is particularly difficult, as they usually work full-time and must learn English before gaining access to postemployment training programs.

Children in refugee families are particularly at risk since they are members of a group that historically has had more trouble earning enough to work their way off assistance.³⁷ While this is obviously not true of all refugees, a number of factors may explain this phenomenon. For some, the inability to work is due to the psychological and physical impairments that they suffered as a result of the strife and persecution from which they fled in their home countries. For others, the inability to work stems from their limited English and educational background or the lack of recent job history due to months and years spent waiting for resettlement in refugee camps.

The same laws that ensure the availability of interpreters and translated notices from the welfare department can be used to ensure such services in work search and job orientation programs, whether provided by welfare department staff or by contracted agencies.³⁸ Advocates can play a vital role in working with commu-

nity colleges, vocational programs, and other private and public funders in developing programs to meet immigrant needs.

C. Discrimination

One of the rarely mentioned effects of the antiimmigrant legislation is the increase, both real and perceived, of discrimination against adult and child immigrants. Immigrants regularly report being scorned for their inability to speak English or being told to go back where they came from.³⁹ While advocates may often feel that such complaints are best dealt with on an individual basis, a number of complaints can indicate a need for cultural sensitivity training. Several such complaints may also indicate problems in assisting immigrants. In such cases the written documentation of a problem is extremely useful in determining whether the agency response to the problem is adequate. The Office for Civil Rights investigates complaints to determine any violation patterns and practices.

III. Immigration Consequences of Using Benefits

While the 1996 federal welfare law restricts immigrant access to benefits in several important ways, a number of states have created programs both for those who are not "qualified" and for those who are "qualified" but arriving after August 22, 1996, and not exempt from the five-year bar. Advocates have been surprised by how few immigrants are using these state-funded programs. For an explanation of the alarming decrease in access to services, especially access to health care, one must understand the immigration consequences, both real and perceived, of the new welfare and immigration laws. Advocates can do much to improve access to services by monitoring their state welfare office's and local INS district office's implementation of these immigration-related laws and by instructing social service workers and

³⁷ For more information on the lower income levels of refugees see *id.* at 1582.

³⁸ The Office for Civil Rights points out that all subcontractors who receive federal funds are also affected. See Office for Civil Rights, Guidance Memorandum, *supra* note 33.

³⁹ This treatment is particularly painful for refugees and asylees who are unable to return to their country of origin and are often dealing with mental and physical problems in addition to the trauma of resettlement.

clients themselves. Because immigrants usually discuss the immigration consequences of receiving public assistance with an immigration attorney or an immigration advocacy agency, what is crucial is that advocates knowledgeable about immigrant eligibility for public benefits share their knowledge and materials with the immigration bar. Failure to share this information can result in clients feeling further confused as immigration attorneys advise them to do one thing and legal services attorneys and social service and health workers advise them to do another.

A. Refusal to Access Benefits Due to Public-Charge Policies

A little over a year and a half ago the medical community and advocacy community began to hear reports of immigrant clients, mainly children and pregnant women, who were refusing to accept medical assistance.⁴⁰ In some states prenatal care and children's health programs are available to all low-income residents, regardless of immigration status. One reason for this availability is that states have determined that providing preventive care

is more cost-effective to waiting for the health problem to become an emergency, even though the immigrant would then be eligible for federally funded Medicaid.⁴¹ Another reason for such preventive-care programs—controlling communicable disease and avoiding the poor birth outcomes and lifetime effects on child health and development of undiagnosed and untreated child illnesses—was underscored by reports of outbreaks of contagious disease, untreated high-risk pregnancies, and childhood diseases around the country.⁴²

Investigation of the problem revealed that immigrants' decisions not to access care were the result of a combination of factors. One factor was a new, unauthorized INS policy of detaining legal permanent residents seeking to reenter the United States after brief visits abroad. These legal permanent residents were questioned about their receipt of benefits (including medical assistance) while living in the United States.⁴³ Many immigrants were being asked to repay their medical assistance before they could be let back into the country.⁴⁴ Although INS subsequently clarified that this practice was illegal, many immigrants remained afraid.⁴⁵

⁴⁰ Claudia Schlosberg & Dinah Wiley, *The Impact of INS Public Charge Determinations on Immigrant Access to Health Care* (May 22, 1998) <www.healthlaw.org/publiccharge.html>.

⁴¹ See, e.g., Letter from Howard Koh, Massachusetts Department of Public Health, to Donna Shalala, Secretary, U.S. Department of Health and Human Services (June 8, 1998) (complaining that failure to use primary care, such as prenatal care, which saves \$3 for every dollar spent, resulted in a high cost to society from emergency room services).

⁴² Examples of these problems include outbreaks of rubella in New York City because of decreased vaccination rates among Hispanic residents, untreated ear infections in a citizen child in Washington, and an untreated pregnancy-related diabetes in a California woman married to a U.S. citizen. See Schlosberg & Wiley, *supra* note 40.

⁴³ The State Department instituted a Public Charge Lookout System, which connected consular offices with state welfare departments in order to relay information about the immigrant's use of state and federal benefits in the United States.

⁴⁴ Abuses of the Public Charge Lookout System are documented in Schlosberg & Wiley, *supra* note 40.

⁴⁵ INS explained that no authority required legal permanent residents (LPRs) to repay benefits correctly received and that, with very limited exceptions, LPRs absent from the United States for 180 days or less were not to be denied readmission due to public charge. Immigration & Naturalization Serv., Public Charge: INA Section 212(a)(4) and 237(a)(5)—Duration of Departure of LPRs and Repayment of Public Benefits (Dec. 16, 1997) (memorandum in my file). The Health Care Financing Administration also sent to State Medicaid Directors a letter explaining that States were not to disclose information about an individual's receipt of Medicaid benefits to INS for public-charge determinations since such a disclosure was not directly connected to the administration of the State plan. The letter also explained that States generally were not to accept repayments of Medicaid benefits unless the benefits were fraudulently received or an overpayment had occurred. Letter from Sally Richardson, Health Care Financing Administration, to State Medicaid Directors (Dec. 17, 1997).

National Resources for Immigration-Related Materials and Information

These organizations can be contacted for advice, community education material, and publications covering specific legal areas. They also may provide technical support, counseling, brief banks, and training materials.

American Immigration Lawyers Association

1400 Eye St. NW, Suite 1200
Washington, DC 20005
AILA Infonet: 202.216.2400; fax 202.371.9449
For the *AILA Monthly* contact 202.371.9377.

Asian Pacific American Legal Center

1010 S. Flower St., Suite 302
Los Angeles, CA 90015
213.748.2022; fax 213.748.0679
apalc@earthlink.net
For *The Annual Report* newsletter contact Citizenship Project Department.

Ayuda

1736 Columbia Rd. NW
Washington, DC 20009
202.387.4848; fax 202.387.0324
immayuda@erols.com
www.incacorp.com/ayuda
For *Ayuda Newsletter* contact the office manager.

Center on Budget and Policy Priorities

820 First St. NE, Suite 510
Washington, DC 20002
202.408.1080; fax 202.408.1056
www.cbpp.org

Immigrant Legal Resource Center

1663 Mission St., Suite 602
San Francisco, CA 94103
415.255.9499; fax 415.255.9792
ilrc@ilrc.org
For *Citizenship Alert!* contact alert@ilrc.org.

"ILRC Attorney of the Day" is available for free consultation (phone, fax, or E-mail) to people working with seniors, people with disabilities, advocates, and teachers who are assisting citizenship applicants (not individual applicants).

National Asian Pacific American Legal Consortium

1140 Connecticut Ave. NW, Suite 1200
Washington, DC 20036
202.296.2300; fax 202.296.2318
For *The NAPALC Review* contact 202.296.2300.

National Asian Pacific Center on Aging

1511 3d Ave., Suite 914
Seattle, WA 98101
206.624.1221; fax 206.624.1023
For *The Fax* contact 206.624.1221.

Continued on next page

National Resources for Immigration-Related Materials and Information (continued)

National Citizenship Network/Immigration & Refugee Services of America

1717 Massachusetts Ave. NW, Suite 701

Washington, DC 20036

202.797.2105; fax 202.797.0837

www.irsa_uscr.org

For fax and e-mail list materials on naturalization contact the program director.

National Immigration Law Center

1101 14th St. NW, Suite 410

Washington, DC 20005

202.216.0261; fax 202.216.0266

1102 S. Crenshaw Blvd., Suite 101

Los Angeles, CA 90019

213.938.6452; fax 213.964.7940

For *Immigrants' Rights Update and Public Benefits Alert* contact 213.938.6452.

National Immigration Project of the National Lawyers' Guild

14 Beacon St., Suite 602

Boston, MA 02018

617.227.9727; fax 612.227.5495

nip@nlg.org

www.nlg.org

For *Immigration Newsletter* contact the office manager.

National Immigration Forum

220 I St. NE, Suite 220

Washington, DC 20002-4362

202.544.0004; fax 202.544.1905

www.immigrationforum.org

For *The Golden Door* newsletter contact the office manager.

National Network for Immigrant and Refugee Rights

310 8th St., Suite 307

Oakland, CA 94607

510.465.1984; fax 510.465.1885

nnirr@nnirr.org

www.nnirr.org

For *Network News* newsletter contact 510.465.1984.

A second factor was that immigrants applying for adjustment to legal permanent residence were being denied for being a "public charge" if they were receiving any benefits, including medical assistance, at the time of adjustment.⁴⁶ Here, too, some INS district offices and consular offices were conditioning the

for reasons of public health policy and lack of private or employer-provided insurance.⁴⁸ Advocates can distribute relevant INS documents and community education materials to health care providers who are often desperate for information when talking to immigrants unable to get necessary medical treatment without medical assistance to pay for it. Advocates can educate immigrants themselves about the risks of forgoing health care and about the need to get individual advice from an immigration attorney before deciding whether to accept assistance.

Advocates should inform both immigration officers and attorneys about the expansion of health care programs, particularly those for children, for reasons of public health policy and lack of private or employer-provided insurance.

grant of legal permanent residence upon repayment of benefits received. A third factor was that immigrants (even those who were legal permanent residents or naturalized citizens) feared that if they received benefits, they would then not be able to petition for family members to join them as immigrants.⁴⁷

Advocates can work with immigrants and immigration advocates to monitor local INS district offices for misapplication of current law. Advocates should inform both INS officers and immigration attorneys about the expansion of health care programs, particularly those for children,

B. Affidavits of Support

Many undocumented immigrants are declining benefits because they fear they will not qualify for legal permanent residence. Many citizens and legal permanent residents are worried about the effect of their past receipt of benefits on their new binding affidavits of support, required in almost all family visa petitions. The new income requirements, rather than past receipt of benefits, often pose the greater barrier to immigrating family members.⁴⁹ New regulations require that the sponsor filling out the affidavit of support show income at 125 percent of poverty guidelines for the sponsor and the sponsor's dependents in addition to the immigrants being sponsored.⁵⁰

⁴⁶ See Schlosberg & Wiley, *supra* note 40.

⁴⁷ INS's concern about the use of benefits by immigrants can be traced back to a long existing policy commonly known as "public charge"—a term INS uses to describe an immigrant who has become or is likely to become dependent on government benefits. See YOLANDA VERA, NATIONAL IMMIGRATION LAW CTR., PUBLIC CHARGE (1997); Broder, *supra* note 17, at 519–20. While the decline in access to benefits is in part related to the fear of public charge, it is also due to concern about increased verification and reporting procedures. See Schlosberg & Wiley, *supra* note 40. For a broader discussion of public charge and verification and reporting procedures see my forthcoming article in CLEARINGHOUSE REVIEW.

⁴⁸ This expansion has recently increased due to the Child Health Insurance Program. Some states now cover children in families with incomes as high as 300 percent of the federal poverty level. Studies show that children make up half of the estimated 37 million people who receive Medicaid. The majority of these children live in households with parents who work. Schlosberg & Wiley, *supra* note 40, at 6.

⁴⁹ An INS survey found that one-third of immigrants petitioning family members in 1994 would not have met the new requirement. A survey by the Urban Institute put the number at 40 percent for immigrants and 25 percent for U.S.-born citizens. Both studies pointed to how the requirements would fall hardest on immigrants from countries in Central America and Asia, particularly Southeast Asia. Celia W. Dugger, *Immigration Law's Fine Print Emerges, Setting off a Debate About Welfare Provisions*, N.Y. TIMES, Oct. 20, 1997.

⁵⁰ 8 U.S.C. § 1183(a)(f)(6)(A)(ii). Income from spouses, dependents, and other relatives living in the sponsor's household for at least six months may be combined to meet the 125-percent requirement, but an additional side contract is required. 62 Fed. Reg. 54346, 54353–54 (1997).

The new Section 213A affidavit of support (Form I-864) replaces the old nonbinding form (I-134) and is required for family-based visas and adjustment-of-status applications filed on or after December 19, 1997.⁵¹ The new forms are required in almost all new family visa petitions, with an exception for widows or widowers filing petitions based on prior marriage to a citizen and for battered spouses and children filing self-petitions based on a relationship to a citizen or legal permanent resident spouse responsible for the battery.⁵²

Although the new forms add the requirement that the sponsor be a U.S. resident and the petitioning relative, the biggest change is that these forms are now legally enforceable against the sponsor by the sponsored immigrant or any government agency that provides a means-tested benefit to the immigrant while the affidavit is in force.⁵³ The sponsor's obligation ends only when the sponsored immigrant becomes a citizen, is credited with 40 quarters of employment in the United States, dies, or leaves the country and abandons permanent resident status, or when the sponsor himself or herself dies.⁵⁴ Not all benefits are subject to recovery from the sponsor, and advocates should educate immigrants about those available.

Many of these family members are spouses and children who are currently living in the United States and are ineligible for most benefits and fearful of being detected by INS since they do not yet have legal residence. Very often what has prevented the citizen or legal permanent resident parents of these children from applying for legal residence is money.⁵⁵ Many of those legal permanent residents and U.S. citizens who wish to petition for spouses and children but are currently unable to

meet the 125-percent standard have to wait to increase their incomes before they can file. Although the new regulations allow them to meet the income requirements by finding a joint sponsor to sign the affidavit of support, finding joint sponsors is difficult because of the potentially open-ended liability, especially concerning health costs.⁵⁶ For these immigrants, careful advice about the complexities of the new immigration law must be combined with help in increasing their incomes through access to English as a Second Language classes, training programs, and community economic development.

C. Sponsor Deeming

The new affidavits of support (Form I-864) differ from the old ones (Form I-134) in that they are binding on the sponsor. While this no doubt will assist some sponsored immigrants, including children, in suing for support from a sponsor, the new affidavits are adding a new layer of complexity to a process called "sponsor deeming." Like many other confusing immigration rules, the complexity caused by the new affidavits has often led to blanket denials of assistance by confused welfare workers, even where assistance should have been provided. This has been especially true for women and children who are applying for assistance after fleeing an abusive spouse who, if the wife and children do have legal permanent residence status, is usually the sponsor. Advocates will now have to be vigilant in ensuring that state welfare departments take into account the differences between the old and new affidavits.

Because of sponsor deeming, affidavits of support have always affected the sponsored immigrant's eligibility for certain kinds of public assistance. Prior to

⁵¹ 8 U.S.C. § 1183.

⁵² *Id.*

⁵³ 62 Fed. Reg. 54346–56 (1997)

⁵⁴ 8 U.S.C. § 1631.

⁵⁵ Including the fees for the application, the necessary medical certification, the adjustment fee, and the fees needed to hire an attorney, the total amount can easily surpass \$2,000 per person.

⁵⁶ Immigration attorneys and advocates report that the unpredictability of the new requirements has made people worried about becoming a sponsor. *Immigrant Sponsors Get New Burden: Financial Support Must Be Assured*, ATLANTA J. & CONST., Dec. 19, 1997.

the 1996 welfare law, immigrants who had an affidavit of support filled out on their behalf were required to supply information on the sponsor's income and resources when applying for three federal programs—SSI, food stamps, and Aid to Families with Dependent Children.⁵⁷ A certain portion of the income and resources was then “allocated” for use by the sponsor and the sponsor's dependents (including other persons for whom the sponsor filled out an affidavit of support), while the remainder was “deemed,” or considered available, to the sponsored immigrant, whether or not the money was actually provided. If the income and resources were above the program eligibility guidelines, the applicant was considered ineligible for assistance. Since many states are still using these rules for immigrants with old affidavits of support, and since the rules differ among programs, advocates must check welfare department regulations to see if these rules are correctly implemented.

The new affidavits of support differ from the old ones in some important ways. They may last indefinitely, or at least until the immigrant becomes a citizen or can be credited with 40 work quarters. They allow the sponsored immigrant or agency providing benefits to sue for reimbursement, and they can be applied to a broad range of benefits, including medical assistance.⁵⁸ Currently they are applied to all federal means-tested benefits, but, because most immigrants arriving on or after August 22, 1996, are barred from such benefits for the first five years

after they obtain “qualified” status, sponsor deeming in these programs will not take effect for several years.⁵⁹ Nonetheless, advocates must familiarize themselves with the potential ramifications of the new affidavits if they are to advise immigrant families of assistance available to them. Predicting the effects of the new affidavits is particularly difficult because most states have not formally determined which, if any, state and local benefits will be subject to the sponsor-deeming rules. Because no federal regulations have been promulgated, advocates should be sure that state programs correctly include at least the exemptions provided for in the statute. These include immigrants who have worked 40 qualifying quarters, domestic violence victims, immigrants facing hunger or homelessness, and immigrants whose sponsors have since died.⁶⁰

In addition to reviewing state regulations, advocates can work to educate social service and health care providers about the many kinds of benefits available to sponsored immigrants even while the affidavit of support is in force. Sponsor deeming is applied to the following federal programs: SSI, Temporary Assistance for Needy Families, food stamps, non-emergency Medicaid, and Children's Health Insurance Program.

Advocates should share this information with immigration lawyers and advocacy groups so that they do not incorrectly advise potential sponsors and sponsored immigrants that no benefits are available. Until sponsor-deeming policies become clearer, families may want to take

⁵⁷ For SSI see 42 U.S.C. § 1382j, *as amended by* Pub. L. No. 103-153; 20 C.F.R. §§ 416.1160 *et seq.* (1998). For food stamps see 7 U.S.C. § 2014(i); 7 C.F.R. § 273.11(j) (1998). For AFDC (TANF) see 42 U.S.C. § 608(j); 45 C.F.R. § 233.51 (1997).

⁵⁸ Immigration and Nationality Act of 1996 § 213(a)(1)(B), 8 U.S.C. §§ 1183(a)(1)(b), 1624, 1631–32 (1996).

⁵⁹ Immigrants who are on active duty, veterans, and their family members are exempt from the five-year bar but not exempt from sponsor deeming. These groups are allowed to execute a new affidavit of support if they meet 100 percent of federal poverty guidelines, not the 125 percent required of all other immigrants. However, because no federal regulations have been issued regarding the implementation of sponsor deeming, states are for the most part waiting to enforce it.

⁶⁰ Domestic violence victims must meet two additional requirements. They must no longer live with the batterer, and the domestic violence must have a substantial connection to the need for benefits. With a court order documenting the abuse, the exemption may be extended. 8 U.S.C. § 1631(f), *as amended by* Pub. L. No. 105-33, § 5571, 111 Stat. 251. If the assistance is necessary to prevent hunger or homelessness, it can be provided for a period of up to 12 months. 8 U.S.C. § 1631(e).

advantage of regulations allowing them to count some resources toward the 125-percent income requirement rather than looking for additional sponsors outside the family. Advocates should help in proving work history (the 40-quarter exemption), especially where the spouse and children of a citizen or legal permanent resident may be able to share the credits. Similarly naturalization assistance should also be continued as an important avenue for putting an end to sponsor deeming.⁶¹ Advocates should inform state administrators about the need to explain clearly whether a particular benefit utilizes sponsor deeming or requires reimbursement, as programs providing such benefits are likely be underutilized.

IV. Conclusion

The issues discussed above underscore the complex questions faced by all immigrants seeking access to benefits for which they are eligible. Public debate about immigration policy has moved slightly from intense antiimmigrant sentiment toward some appreciation for immigrants' contributions to the country's economy and culture. This swing has had little effect on most immigrants themselves. Confused and fearful, they are reluctant to seek assistance, even if thereby their children go hungry and without medical care.

The new laws have brought changes that many still hope to reverse, particularly where children are affected. In the meantime, advocates can play an important role in ensuring that state agencies



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do not go beyond those laws in denying benefits and that immigrants and those assisting them are appropriately advised about eligibility for benefits and any negative effects of such benefits. Advocates must see that the consequences—whether intentional or not—of these laws for immigrants and their children do not go unnoticed and undocumented.

⁶¹ For an overview of how advocates can get involved in helping immigrants obtain citizenship see Evelyn H. Cruz & Eric Cohen, *Naturalization: Helping Immigrant Clients Maintain Access to Social Services*, 32 CLEARINGHOUSE REV. 50 (May–June 1998).

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Exhibit 37

County of Santa Clara
Comment to DHS Docket No. USCIS–2010–0012
Inadmissibility on Public Charge Grounds

2-24-2012

Household Hardships, Public Programs, and Their Associations with the Health and Development of Very Young Children: Insights from Children's HealthWatch

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Evaluating Food Insecurity and Household Hardships

Food insecurity, a household-level economic and social condition of limited or uncertain access to adequate food for all household members to live an active and healthy life, is a serious public health problem in the US. This problem waxes and wanes with fluctuations in public policies and economic climate. While the prevalence of household food insecurity has remained fairly stable over the past 2 years, the increase in 2008, the first full year of the recession, was the largest since the national food security survey began in 1995¹ and has not returned to pre-2008 levels.

The most recent US Department of Agriculture (USDA) assessment of food security in the US reported that overall food insecurity numbers (measured by the 18-item US Food Security Scale [FSS]²) were relatively unchanged from 2009 to 2010, with 14.5% of households (17.2 million) considered food insecure; most of those who were food insecure lived inside metropolitan areas.³ That same year, 21.6% of all US children (16.2 million) lived in food-insecure households. Pediatric clinicians have observed that not only food insecurity but also other material hardships, such as energy and housing insecurity (respectively, inadequate home energy availability due to economic constraints and overcrowded living situations or frequent moves), exert a cumulative negative effect on child health.⁴

Many research and advocacy groups use econometrics and/or biostatistics to explore the health and educational effects of food insecurity.^{5,6} Children's HealthWatch (formerly known as the Children's Sentinel Nutrition Assessment Program or C-SNAP) is one such group. Children's HealthWatch was founded in 1998 to bring evidence and analysis from the front lines of pediatric care to policy makers and the public. The goal of Children's HealthWatch was to develop a continuous dataset that could monitor child health in real time as economic conditions and government assistance programs changed. At the time, other researchers collected young children's anthropometric data primarily from datasets of children already receiving benefits from the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) or Headstart and reflected conditions from 1 to several years before publication.⁷

Some national data collection efforts obtained health information but not data on family hardship or safety-net program participation, while still others that collected program participation data lacked information on children's health, growth, and development. At present, several important longitudinal studies include the FSS but mostly focus on older children. To our knowledge, the Children's HealthWatch dataset is the oldest and

largest of children ages 0 to 4 year olds that includes other material hardships, such as energy insecurity.⁸⁻¹¹

From its inception, the study has focused on a sentinel populationⁱ of very young children. The rationale for this focus is both scientific and sociological. From a scientific perspective, during the developmental window from birth to preschool, the rapidly increasing size and function of the developing brain demands consistently high levels of nutritional substrate. Deficits in the growth of brain and body following nutritional deprivation or other hardships during this sensitive period, when the foundations of future health and cognitive development are largely determined, are difficult to remediate later in life. Paradoxically, this sensitive period is also the developmental epoch during which children in the US are most likely to live in poverty¹² and least likely to participate in formal child care or educational settings.¹³ Thus, children of this age are typically “visible” only to their family members and health care providers.

As a result, the needs of very young children are often not addressed in important policy debates. Consistent with the medical model, Children’s HealthWatch focuses on two broad phenomena: 1) the association of one or more exposures (material hardships) to each other and to health outcomes; and 2) the effect of population-level interventions (such as food assistance programs) on reducing the severity or prevalence of the exposure or reducing the negative health impact of the exposure. Monitoring and reporting on the association of these factors with the well-being of the youngest children lends this constituency a voice in matters of policy that will eventually impact their potential to be healthy and productive adults.

Children’s HealthWatch monitors the impact of food insecurity alone and in conjunction with other hardships common to low-income families; these hardships include energy insecurity, housing insecurity, and constrained access to health care. Such research evaluates systematically the common-sense notion that ready access to sufficient healthful food, safe and stable housing, and adequate household energy resources can position young children on a trajectory for health and success in school and, later, in the workforce.

ⁱ Sentinel samples are subpopulations in which occurrence of a disease indicates or predicts rates in the general population or subpopulations that may be especially vulnerable to a disease and experience higher disease rates before the general population is affected (the “canaries in the coal mine”). They are also subpopulations in which occurrence of or exposure to disease at one age or life-cycle phase may reliably predict disease at later ages or life-cycle phases.

This paper explores Children's HealthWatch's research methods, selected findings, and examples of diverse approaches to dissemination of these findings in professional settings, national and local reports and briefs, and legislative testimony. An exhaustive list of this work can be found at www.childrenshealthwatch.org.

Children's HealthWatch—Analytic Focus

Though the 18 items that comprise the FSS are essentially unchanged, the category in which an individual fits according to how he or she scores on the scale has varied over time; these variations have resulted in several possible classifications describing a household's or individual's experience with access to food.¹⁴ The FSS also includes 8 child-focused items to measure food security specifically among children.¹⁵ The terms "food insecure" and "food secure" are the broad categories used to describe experience with food as determined by the FSS and are described in Table 1.

Table 1. Changes in Description of Food Security¹⁶

USDA Pre-2006 Label	USDA Current Label	Description of Conditions in the Household, per Food Security Survey
Food security	High food security	No reported indications of food-access problems or limitations
	Marginal food security	One or two reported indications—typically of anxiety over food sufficiency or shortage of food in the house; little or no indication of changes in diets or food intake
Food insecurity without hunger	Low food security	Reports of reduced quality, variety, or desirability of diet; little or no indication of reduced food intake
Food insecurity with hunger	Very low food security	Reports of multiple indications of disrupted eating patterns and reduced food intake

Most frontline providers continue to use the common-sense pre-2006 terms rather than low and very low food security. Children's HealthWatch analyses use the conservative approach of 3 endorsed items on the FSS to refer to a family as "food insecure" while some groups

interpret food insecurity at the marginal food secure level or use alternate scoring systems.¹⁷

Children's HealthWatch uses the scoring system of the USDA's Economic Research Service (ERS) to divide households into 3 mutually exclusive categories: 1) food secure on the 18-item FSS; 2) food insecure on the household scale but food secure on the child scale; and 3) food insecure on the household scale *and* the child scale. (See Table 2)

Table 2. Levels of Food Security and Insecurity¹⁸

Level of Food Insecurity	Assessing Status Using the FSS
Food secure on the household and child scale	Household reports 0 to 2 indications of food insecurity to the entire set of 18 questions
Food insecure on the household scale but food secure on the child scale	Household reports 3 or more indications of food insecurity in response to the entire set of 18-questions
Food insecure on the household scale <i>and</i> the child scale	Household reports 2 or more of the child-referenced questions (11-18) of the FSS

Children's HealthWatch Study Sample Composition

Children's HealthWatch currently collects data through one-on-one surveys between trained interviewers and caregivers of patients under the age of 4 in emergency departments or primary care clinics in 5 US cities. Since 1998, over 42,000 households have completed the survey at one of the 7 research sites listed below. Each of the sites is located in a predominantly low-income area and has an affiliated multidisciplinary clinic to provide care to underweight young children, including those identified in the course of research procedures. Each site has approval for human subjects research from its Institutional Review Board (IRB), and all studies have been conducted with IRB approval in place.

Data collection sites are:

- University of Maryland School of Medicine, *Baltimore, Maryland*
- Boston Medical Center, *Boston, Massachusetts*
- Arkansas Children's Hospital, *Little Rock, Arkansas*
- Hennepin County Medical Center, *Minneapolis, Minnesota*
- St. Christopher's Hospital for Children, *Philadelphia, Pennsylvania*

Inactive sites (due to insufficient funding) are:

- Harbor UCLA Medical Center Torrance, *Los Angeles, California* (inactive as of 2001)
- Mary's Center for Maternal and Child Care, *Washington, DC* (inactive as of 2000)

Caregivers interviewed by Children's HealthWatch comprise a sentinel cross-sectional convenience sample. Interviewers survey caregiver-child dyads consenting to participation in the survey and meeting the following inclusion criteria: 1) patient is <48 months of age and seeking care at acute or primary care clinics or hospital emergency departments; 2) caregiver lives in the state where the interview is being conducted; 3) the interview can be conducted verbally in English, Spanish, or (Minneapolis only) Somali; 4) the household has not participated in the Children's HealthWatch survey in the previous 6 months; and 5) the caregiver lives with the child and has full knowledge of the child's life. Caregivers of critically ill or injured children are not approached.

In early 2011, the eligible child age range was broadened from 0 to 36 months to 0 to 48 months in an effort to expand the sample to an age group where the risk of obesity increases and developmental findings better predict school readiness.

The Children's HealthWatch Survey

Each caregiver completes one full survey during his or her child's visit to the emergency department or primary care center. While the survey has been modified slightly over time to accommodate changes in policy and research questions, the areas of focus generally include the following:

Data acquired through medical record review and/or anthropometric measurement:

- Child's dehydration and hospital admission status on the day of interview
- Child's current height and weight (at some sites, measurements are taken by clinical staff; at others, the interviewers use standardized protocols to measure children)

Data acquired through caregiver report:

- Developmental assessment (Parents' Evaluation of Development Status¹⁹) and use of Early Intervention services
- Demographic background
- Caregiver and household employment status, income, and use of child care

- Child's health history, including lifetime hospitalizations and health insurance coverage
- Household access to medical care and healthcare trade-offs
- Caregiver health questions, including the Kemper Depression Screener²⁰ (female caregivers only), and maternal/paternal anthropometric measurements
- Evaluation of housing security
- Evaluation of energy security
- The USDA FSS¹⁵
- Participation in public assistance programs including Supplemental Nutrition Assistance Program (SNAP, formerly The Food Stamp Program), WIC, Temporary Assistance for Needy Families (TANF), energy assistance such as Low-Income Home Energy Assistance Program (LIHEAP), and housing subsidies, such as Section 8 and public housing.

At the end of every interview, the caregiver is offered outreach services that differ by site, ranging from a list of community resources and WIC offices to follow-up from an outreach worker. Caregivers are also offered the chance to be contacted for media or legislative testimony opportunities. Small incentives—for example, supermarket gift cards or a toy for the child—are provided.

Survey Analysis

All data from the Children's HealthWatch survey are maintained and analyzed at the Boston University School of Public Health's Data Coordinating Center. Every 6 months, a new batch of clean data is added to the dataset. There is no "typical" data analysis in Children's HealthWatch. Each analysis includes (at a minimum) descriptive statistics, bivariate associational measures, and multivariate analyses adjusted for potential confounding factors. Propensity score matching may also be used. Because the study is ongoing, with some measures being added or omitted over time and sites joining and leaving the project, different publications from different periods described below have varying sample sizes depending on how many participants had been interviewed at the time of the analysis. In addition, some papers focus only on subsamples selected from the overall sample to answer specific questions.

Monitoring Children's Health on the Front Lines of Care

Federal and state programs exist to support households struggling with the costs of basic needs, but there can be logistical and bureaucratic barriers to program participation. Moreover, some discretionary programs

lack funding to support all those who are eligible.ⁱⁱ Children's HealthWatch examines the health effects of nutrition assistance programs, including SNAP, WIC, and programs that indirectly fight food insecurity, such as energy assistance programs and subsidized housing, described in Table 3. Of these programs, only SNAP is an entitlement program, designed to serve all eligible people who apply. Discretionary programs, on the other hand, are vulnerable to budget cuts year to year. When funding for discretionary programs is cut at the federal level, individual states create criteria to determine which families will receive assistance. Some states use their own budgets to supplement the needs of programs so that every eligible family receives assistance. In other cases, assistance may be provided on a first-come, first-served basis.

Table 3. Selection of Federal Assistance Programs

Federal Program	Description
Supplemental Nutrition Assistance Program (SNAP)	Formerly called the Food Stamp Program, SNAP is an entitlement program; households are provided a monthly allotment for food purchases on an Electronic Benefits Transfer card.
Temporary Assistance for Needy Families (TANF)	Program that provides monthly cash benefits to very low-income families based on eligibility standards established by the state within federal guidelines. Recipient families must fulfill ongoing work and other requirements, and there is a time limit on benefits.
Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)	Discretionary program that provides nutrient-dense foods to income-eligible pregnant, postpartum, and lactating women and children up to the age of 5.
Low Income Home Energy Assistance Program (LIHEAP)	Energy assistance program that provides funds for heating costs, often on a first-come, first-served basis.
Section 8 and Public Housing	Two of the types of housing subsidies available to individuals at low-income levels; eligible families sometimes spend multiple years on waiting lists before receiving the subsidy.

ⁱⁱ Discretionary programs are funded year to year and are not obligated to serve all those who are eligible; TANF, WIC, LIHEAP, and housing subsidies fall into this category.

Overview of Findings

Children's HealthWatch and others have found that, while food insecurity at both the household and child levels has a negative impact on child health outcomes, other hardships also come into play. These hardships, such as food insecurity, may be modified by participation in public assistance programs. All families, but particularly those who have limited incomes and young children, are constantly juggling the costs of paying for basic needs like food, shelter, household utilities, and medical care. A change in one affects the others; parents, despite the best of intentions, have to make difficult decisions whether to pay for a child's prescription, buy nutrient-dense food, or allocate scarce financial resources to rent or utility bills. Supports—including housing subsidies, WIC, and energy assistance—can offset some costs and free resources for other needs, in turn allowing parents to do more to promote their children's food security and health.

Examination of Food Insecurity in Children's HealthWatch Populations

Children's HealthWatch and others have concluded that household and child food insecurity have negative impacts on child health.^{21,22} Analysis by other groups suggests that the threshold for adverse health or developmental effects may actually be lower than previously thought (i.e., even a marginal score on the FSS may correlate with poor outcomes).^{23,24} As outlined in more detail in the following sections, Children's HealthWatch analyses have found that food insecurity puts children at a higher risk for iron-deficiency and iron-deficiency anemia, lifetime hospitalizations, fair or poor health, and developmental concerns. It should be noted that Gundersen and Kreider²⁵ observed that in samples like those of Children's HealthWatch, it is more likely that the negative health impacts of food insecurity have been underreported rather than overreported.

History of Hospitalizations and Report of Fair/Poor Health Related to Food Insecurity

The survey includes questions about the child's hospitalizations since birth and the caregiver's rating of his or her child's health status.ⁱⁱⁱ In a variety of analyses using the Children's HealthWatch dataset, children living in food insecure households are more likely to have been hospitalized and to have their health reported as fair or poor (versus excellent or good) than

ⁱⁱⁱ This question is asked in the National Health Assessment Needs Education Survey III with 5 response alternatives instead of 4. In that version, "very good" is also an option.

their counterparts living in food secure households. A 2004 study aimed to determine whether household food insecurity is associated with adverse health outcomes in the Children's HealthWatch sample.²¹ In the sample of 11,539 children under 36 months old, interviewed between 1998 and 2002, 21.4% were food insecure. Although household SNAP benefit receipt attenuated associations between food insecurity and the child's fair or poor health status, the odds of fair or poor health in children living in food insecure households remained almost twice that of children in food secure households [AOR=1.90, 95% CI=1.66-2.18]^{iv} even after controlling for SNAP participation. The odds of hospitalization since birth among the children in food insecure homes were more than 30% higher than those of the children in food secure homes [AOR =1.31 95% CI=1.16-1.48] and not modified by SNAP participation.

In 2006, further analyses addressed child as well as household food insecurity. In a sample of 17,158 child-caregiver dyads, 10% reported only household-level food insecurity, and 12% reported child food insecurity in addition to household-level food insecurity. Adjusted analyses revealed child-level food insecurity increased the adjusted odds of negative child health outcomes compared to household food insecurity with children food secure [AOR 1.51 v AOR 1.99-2.00].¹⁸

While data specific to the reason for prior hospitalizations are not collected, these findings suggest that an increase in food insecurity is correlated with an increased likelihood of history of hospitalization for young children. The average charge^v in 2009 of a single hospitalization for a child under 1 for any reason was \$13,300. The average charge for a single hospitalization for a child aged 1-4 was \$21,045.²⁶ Hospital fees are paid through a number of streams, including the individual, insurance (i.e., public and private), or at times, the hospital that provided the care. None of these figures, however, include rehabilitation, follow-up care, or home care costs.²⁷

Iron-Deficiency Anemia and Food Insecurity

Iron-deficiency anemia has been correlated with impaired cognitive, mental and psychomotor development and diminished immune response in children.^{29,30} Children's HealthWatch wanted to determine if a relationship exists between anemia and food insecurity in the study population. Two independent analyses at Children's HealthWatch sites

^{iv} We have only included Adjusted Odds Ratios for peer-reviewed publications.

^v Hospital charges reflect the amount the hospital billed for the entire hospital stay and do not include professional (physician) fees. Costs tend to reflect the actual costs to produce hospital services, while charges represent what the hospital billed for the care.²⁸

combined survey results with hematological data collected in retrospective chart reviews to explore iron-deficiency, iron-deficiency anemia, and food insecurity in the study population. Children eligible for both analyses received primary care at the respective medical centers and had appropriate lab values available in their medical records.

Researchers examined the association between child-level food insecurity and iron status among the Boston sample. Though interviews at Boston Medical Center were conducted exclusively in the Emergency Department, many of these patients used Boston Medical Center as their medical home for primary care. As a result, researchers were able to collect retrospectively appropriate lab measures for 626 children over the age of 6 months at the time of their interview between June 1996 and May 2001.

Key Findings: Iron-deficiency Anemia and Food Insecurity

- Food insecure young children were at 2.4 times greater risk of having iron-deficiency anemia compared to their food secure peers

In the final sample (n=626), 7% had iron deficiency without anemia, and 11% had iron-deficiency anemia. Adjusting for confounding variables, food insecure children had 2.4 greater odds of having iron-deficiency anemia [AOR, 2.4; 95% CI, 1.1-5.2, p=0.02] compared to food secure children.³¹ In other words, food insecure children were at 2.4 times greater risk for irreversible pathophysiologic effects of iron deficiency that might leave them more developmentally at-risk than their food secure peers—all by 3 years of age. The threshold for this effect was child food insecurity, not household food insecurity alone, suggesting a dose response of nutritional deprivation.

The second study examined a sample of 2,853 children in Minneapolis and focused on the relationship between anemia and household (as opposed to child) food security. In this study, levels of food insecurity were divided into 3 renamed categories (as explained above): high/marginal food security, low food security, and very low food security (which encompasses child food insecurity). Children younger than 36 months old living in households with very low food security were almost twice as likely to have iron-deficiency anemia compared with those in food secure households, independent of age, gender, WIC participation, race/ethnicity, US-born status, breast feeding, health insurance type, and plasma lead concentration [AOR=1.98, 95% CI 1.11, 3.53].³²

Child Development and Food Insecurity

Children's HealthWatch research found that young children from food insecure households are more likely to be at developmental risk in early childhood. Such risk is an early warning sign for lack of readiness for school. The Parents' Evaluation of Developmental Status (PEDS)¹⁹ used to evaluate children from birth to 8 years of age was introduced to the survey in 2004 and is administered to all caregivers with children who are at least 4 months old, the age at which the screen becomes more readily interpretable. In addition, caregivers are asked about their children's current or past enrollment in Early Intervention programs, regardless of age. To assist detection of developmental disabilities, the PEDS consists of 10 questions that assess cognition, expressive and receptive language, fine and gross motor behavior, socioemotional development, self-help, and learning.³³ Even after excluding families reporting food insecurity with hunger and controlling for multiple potential confounders, analysis showed children in food insecure families were significantly more likely than those in food secure households to screen positive on the PEDS [AOR 1.77, 95% CI 1.23-2.56].³⁴ The effects of food insecurity on early childhood development may be explained through both nutritive and non-nutritive pathways. Families experiencing food insecurity and constrained by finances often choose less expensive, calorie-dense but nutrient-poor foods to maximize satiety for all household members. This compromise can result in a deficiency of micronutrients (as suggested by the association of food insecurity and anemia described above), a deficiency which may alter children's developing neurotransmitters.³⁵

Maternal depression is prevalent in our data set and is among the important non-nutritive pathways by which food insecurity impacts child development. Among 5,306 mothers in Children's HealthWatch data who completed the maternal depression screen from 2000 to 2001, 35% had positive reports of depressive symptoms.³⁶ Mothers with depressive symptoms were more likely to report household food insecurity [AOR: 2.69; 95% CI: 2.33–3.11] than mothers without depressive symptoms. Mothers with depressive symptoms were also more likely to have had their TANF benefits reduced [AOR: 1.52; 95% CI: 1.03–2.25] and to have lost SNAP benefits [AOR: 1.56; 95% CI: 1.06 –2.30] but not WIC than mothers who did not report depressive symptoms. Lastly, mothers with depressive symptoms were more likely to report that their children were at developmental risk on the PEDS, but the association of food insecurity with the child's developmental risk was robust after controlling for the mother's depressive symptoms.³⁴ Other literature reports strong links between maternal depression and adverse child development outcomes.

For example, both maternal depression and paternal depression have been shown to have negative impacts on child development.³⁷ However, whether family hardships such as food insecurity caused depressive symptoms or depressive symptoms caused hardships cannot be answered by cross-sectional datasets like Children's HealthWatch.

SNAP and WIC Decrease Food Insecurity and Attenuate Its Impact on Children's Outcomes

Multiple research groups conclude that governmental assistance programs reduce food insecurity and support children's health.³⁸⁻⁴⁰ Using data from the longitudinal Three-City Study where at least one child had to be 0 to 4 years old or 10 to 14 years old, DePolt et al⁴¹ concluded that participation in SNAP is likely associated with fewer food hardships. Though governmental assistance programs do not entirely eliminate food insecurity, they may attenuate the negative impacts. Knowing that food insecurity leads to fair/poor health outcomes, Children's HealthWatch researchers investigated whether or not SNAP receipt—a logical remedy for food insecurity—modified effects of food insecurity among a sample of 11,539 caregivers, 21.4% of whom reported household food insecurity.²¹ A dose-response relation appeared between fair/poor health status and severity of food insecurity. Looking at food insecurity as a dichotomous predictor (food-secure v food-insecure), children living in food-insecure households had nearly twice as great odds of having their health status reported as "fair/poor" as those for similar children in food-secure households [AOR 1.90, 95% CI: 1.66–2.18]. When the sample was analyzed by severity of food insecurity, children in households categorized as food insecure without hunger had odds of health being reported fair/poor greater than those in food-secure households [AOR 1.73, 95% CI: 1.48-2.02] and children in households that were food insecure with hunger were even more likely to be reported as being in fair/poor health compared to children in food-secure households [AOR 2.31, 95% CI 1.89-2.82]. Sub-analysis showed that being in food insecure household increased the odds of fair/poor health by 2.11 times among children in households eligible for but not receiving SNAP benefit. In comparison, those children living in food insecure families who received SNAP benefits had increased odds of fair/poor health 1.52 times greater than those in food secure families—a much lower level of risk.²¹ In the Children's HealthWatch sample, receipt of SNAP has an especially powerful positive effect for children of immigrants—a group whose level of poverty and food insecurity is consistently higher than the general population. Children of immigrant parents who receive SNAP benefits are 32% less likely to be in

poor health than children of immigrant parents whose families do not receive them.⁴²

Barriers to Accessing Assistance Programs Associated with Poor Household and Child Outcomes

Although SNAP is an entitlement program, there are families who may be eligible for SNAP but do not receive the benefit due to access barriers. Barriers to SNAP participation reported by Children's HealthWatch families included not having information about the program, being too young to be head of household for SNAP benefits, having concerns about the bureaucratic hassle of applying for SNAP, administrative issues like missing deadlines, and immigration concerns. Those families who were eligible for but did not receive SNAP because of one or more of these access barriers were found to have a greater likelihood of experiencing food insecurity at the household and child levels, experiencing housing insecurity, and needing to make trade-offs between medical care and basic needs like paying for rent, utilities, or food.⁴³ Children were not the only ones who felt the ill effects of barriers to the program. Female caregivers in families who experienced access barriers to SNAP were more likely to report having depressive symptoms.

Even when a family receives SNAP, the maximum level of benefits is rarely enough to purchase sufficient healthful food each month. The maximum SNAP benefit is calculated on the basis of the cost of the Thrifty Food Plan, established by USDA scientists as a nutritionally adequate diet at the lowest possible cost.⁴⁴ The 2008 report, *Coming Up Short: High Food Costs Outstrip Food Stamp Benefits*⁴⁵ (based on a pilot study in Boston⁴⁶), examined the accessibility and affordability of food items on the Thrifty Food Plan shopping list and more healthy food items in corner stores, medium-sized stores, and supermarkets in Boston and Philadelphia. In November 2011, the study was updated with data from Philadelphia in a report entitled *The Real Cost of a Healthy Diet: 2011*.⁴⁷ In all versions of this study and at both sites, we found that the maximum SNAP allotment for a family of 4 was not sufficient to purchase the items on the Thrifty Food Plan^{vi} market basket shopping list in any size store.

Unlike SNAP, WIC is not an entitlement program. WIC serves 53% of all babies born in the US. Despite the program's wide reach among infants and health and development benefits for young children, nationally only 57% of all eligible children and women received WIC from 1994 to 2003.⁴⁸ A number of studies⁴⁹⁻⁵¹ have demonstrated the positive effect of

^{vi} Details of the USDA "Thrifty Food Plan" and other food plans are available at www.cnpp.usda.gov/usdafoodplanscostoffood.htm.

WIC participation during pregnancy on infant outcomes. Less is known about postnatal participation. Children's HealthWatch found that receipt of WIC for children under 3 is associated with a greater likelihood of the children being in good health, being food secure, and having a healthy weight and height for their age compared to peers who were eligible for but did not receive WIC due to access barriers. In addition, receipt of WIC was associated with decreased risk of developmental delay among children under the age of 3 when compared to children whose mothers reported access barriers to WIC. Mothers in the Children's HealthWatch sample reported that the most common barriers were limited WIC office hours, problems with transportation, difficulty getting to the WIC office to pick up vouchers, and the lack of a permanent address.⁵²

Housing Insecurity:

Child Health Outcomes and Housing Subsidy Programs

The work of Children's HealthWatch is often guided by the experience of our clinicians. Our pediatricians heard over and over from patient families in clinic that housing stability was having a direct impact on their young children. Thus, evaluation of "housing security" was introduced into the Children's HealthWatch survey. There is no simple, widely used federal definition of "housing instability" or "housing insecurity." The US Department of Health and Human Services defines "housing insecurity" as "high housing costs in proportion to income, poor housing quality, unstable neighborhoods, overcrowding, or homelessness." Children's HealthWatch, however, operationalizes the concept as 3 conditions short of outright homelessness: crowded, doubled up, and multiple moves in the past year.⁵³ A relatively small proportion of the Children's HealthWatch sample (approximately 5%) report that they are homeless or do not have a steady place to sleep at night. However, almost half fit the more inclusive term of "housing insecurity." Of an overall sample of non-homeless eligible caregiver-child dyads (n=22,069), 46% experienced "housing insecurity." Within the definition of housing insecurity, we define 2 groups: crowding (more than 2 people per bedroom) or doubling up (living temporarily with other families because of economic difficulties), referred to collectively as "crowding," and multiple moves (2 or more moves in past year). In this cohort, 9% of families with secure housing reported household food insecurity, compared to 12% of overcrowded families and 16% of those with multiple moves. After considering multiple potential confounding characteristics, children living in overcrowded housing were more likely than housing secure children to experience household and child food insecurity; those in households that had multiple moves had increased

risks of household and child food insecurity as well as fair/poor child health and developmental delays. While neither household nor child food insecurity alone predicted poor anthropometric outcomes in previous studies, in these analyses multiple moves were associated with lower weight for age z scores [AOR= -0.082 vs. -0.013, $p=.02$].⁵³ Children living with both housing and food insecurity face a dual threat to their health and development.

Children's HealthWatch has begun to explore, but not yet published in peer-reviewed journals, an alternate indicator of housing stress not covered by the housing insecurity definition delineated above. Compared to families who have not reported problems paying the rent or mortgage on time in the last year, families who were behind on rent or mortgage more frequently experienced food and/or energy insecurity and made more trade-offs between housing, utilities, food, or other expenses to pay medical bills.

Children's HealthWatch twice evaluated the impact of subsidized housing as a treatment for the adverse health effects associated with housing insecurity. Housing subsidies limit the amount paid by families for rent to 30% of their income with the remainder made up by the subsidy. *Subsidized Housing and Children's Nutritional Status*,⁵⁴ published in June 2005, examined the relationship between food security and child health among children living in subsidized housing compared to children in families renting apartments at market rates, but whose eligibility for subsidized housing was indicated by their acceptance onto a waiting list for such housing. Children living in families on the wait list for subsidized housing had lower weight-for-age scores than children living in subsidized housing. More recently, *Rx for Hunger: Affordable Housing*⁵⁵ focused on solutions to the findings that children living in subsidized housing were more likely to be food secure and less likely to be seriously underweight (<5th percentile weight-for-age according to Centers for Disease Control/National Center for Health Statistics growth criteria) than children whose families were on the wait list for subsidized housing.

Energy Insecurity:

Child Health Outcomes and Energy Assistance Programs

Proper heating or cooling in a child's environment is particularly important to the youngest children whose high surface area to mass ratio creates poor thermoregulation abilities and thus makes them extraordinarily vulnerable to extreme heat and cold. Such young children may not be able to express verbally when they are hot or cold. A Boston study, which was a precursor to Children's HealthWatch, reported a lower weight-for-age in

children seeking emergency hospital care within 3 months following the coldest month of the year than all children seeking care the rest of the year.⁵⁶ Contributing to these physiological processes is the “heat or eat” dilemma—a quandary well known to families and their physicians in resource-poor areas. Low-income families must often decide between paying to heat their home during the winter or buying food for their family. Even if heat is not the particular issue, other utilities may be—affecting refrigeration for food, electricity to plug in a nebulizer for an asthmatic child, or a phone line to receive calls about job interviews. If a family must choose between keeping a warm house and lights on or eating a nutritious meal, sometimes nutrition suffers.

To measure this phenomenon, the Children’s HealthWatch research team empirically developed and published a research indicator for household energy security defined as a household experiencing at least 1 of the following conditions within the previous year: moderate energy insecurity (a threatened utility shut-off or refusal to deliver heating fuel) and severe energy insecurity (an actual utility shut-off or refused delivery of heating fuel, an unheated or uncooled day because of the inability to pay utility bills, or the use of a cooking stove as a source of heat). Findings showed that household energy insecurity strongly correlated with household and child food insecurity, with the relationship intensifying with the severity of the energy insecurity. Those in households with severe energy insecurity had odds of household food insecurity 3 times as great as those in energy secure households [AOR=3.06 95% CI: 2.46–3.81]. Even moderate energy insecurity (threatened but not actually shut-off) was associated with greater odds of household food insecurity, child food insecurity, child fair/poor health, and hospitalization since birth.⁵⁷

Children’s HealthWatch also assessed the potential policy “treatment” for energy insecurity—the federal program, LIHEAP—which assists households with home energy expenses. In the 2006 article, “Heat or Eat: The Low Income Home Energy Assistance Program and Nutritional and Health Risks Among Children Less Than 3 Years of Age,”⁵⁸ researchers looked at the growth measurements and health of children living in families receiving and not receiving LIHEAP. Covariate adjusted analyses indicated fewer children living in households receiving energy assistance were underweight (p-value=.01) or were admitted from the emergency room visit compared to those living in similar households not receiving energy assistance [AOR=1.32; 95% CI=1.00–1.74]. However, there was no increase in overweight among children in households receiving versus not receiving energy assistance. More recent findings

have shown that families who received energy assistance were also 14% more likely to be housing secure than those without assistance from LIHEAP.⁵⁹

Receipt or Loss of Benefits and Impact on Food Insecurity and Children's Outcomes

SNAP and WIC are not the only programs that modulate the odds of experiencing food insecurity or the effects of food insecurity. A report prepared by Children's HealthWatch for the Joint Center on Political and Economic Studies in 2006, *The Impact of Food Insecurity on the Development of Young Low-Income Black and Latino Children*⁶⁰, indicated that TANF, WIC, subsidized housing, SNAP, and energy assistance mitigated the effect of food insecurity on the health and growth of low-income black children. Similarly, low-income Latino children whose families received TANF, WIC, subsidized housing, or SNAP were more likely to be food secure than their low-income peers who did not receive these benefits. WIC receipt, in particular, was linked to healthy weight-and height-for-age in both black and Latino children.⁶⁰ Conversely, for black children, reductions in TANF left children 56% more likely to be food insecure while sanctions made them 78% more likely to be food insecure, when compared to black children in families that had not had benefits reduced or sanctioned. Similar results in benefit reduction or sanctions were seen in SNAP; for black children in families with a reduction in benefits, infants and toddlers were 33% more likely to be food insecure, while those children in families with sanctioned benefits were 84% more likely to be food insecure. For Latino children, the effects were more severe. Latino children whose caregivers experienced sanctions in SNAP or reductions in TANF were twice as likely to be food insecure than Latino children who did not experience such changes to household benefits. Latino children whose family TANF benefit was sanctioned were 63% more likely to be food insecure.

The loss of governmental program benefits, whether due to sanctions or to losing eligibility because of higher incomes, can also be associated with increased levels of child food insecurity and, in turn, poor child health outcomes. Young children whose families lost SNAP or TANF benefits because their incomes increased above the maximum level of income eligibility more frequently experienced food insecurity than those who remained on SNAP or TANF. The phenomenon of being worse off after marginally gaining income but losing benefits is called the "cliff effect." In the September 2010 Policy Action Brief *Earning More, Receiving Less: Loss of Benefits and Child Hunger*,⁶¹ Children's

HealthWatch reported that, within the Children's HealthWatch sample, the rate of child food insecurity among those currently receiving SNAP was 6.9%, while among families that had lost SNAP benefits due to an increase in income, the rate was 8.9%. Similarly, those who lost TANF benefits due to an increase in income had higher rates of child food insecurity than those currently receiving benefits.

Cumulative Material Hardships

Growing out of the understanding that material hardships are interrelated, the Cumulative Hardship Index (see graphic) was developed by the Children's HealthWatch research group, along with an analogous composite indicator of child wellness. The 3 included hardships are food, housing, and energy insecurity. The index was developed by examining a subset of our larger sample (7,141 participants at the 5 active study sites between July 2004 and December 2007).⁴ The composite indicator of child wellness synthesizes measures of growth, health, and developmental risk. Findings were replicated in a yet unpublished analysis of a Children's Healthwatch Cohort recruited after 2007.

Calculating a Family Hardship Score

Step 1: Families are assigned a score of 0, 1 or 2 for each component (food, energy and housing insecurity) depending on how they respond to a set of questions. They receive a score of 0 if they do not experience insecurity for that component, a score of 1 if they experience moderate insecurity for that component and a score of 2 if they experience it at a severe level.

Step 2: Their scores are summed across the three components to arrive at a total hardship score. A family's level of hardship is then characterized by their total score.

A total score of 0 No Hardship

A total score of 1-3 Moderate Hardship

A total score of 4-6 Severe Hardship

Example: A family could be moderately food insecure (score = 1), severely housing insecure (score = 2) and moderately energy insecure (score = 1) for a total score of 4. This score would place them in the Severe Hardship category.

A "well child" was defined by caregiver report and medical record review as:

- Good to excellent health
- No hospitalizations since birth
- Not identified as developmentally "at risk" on the PEDS
- Weight-for-age > 5th percentile < 95th percentile
- Weight-for-height > 10th percentile < 95th percentile
- BMI < 85th percentile for children > 24 months of age

Children's HealthWatch concluded, "increasing levels of a composite measure of remediable adverse material conditions correlated with decreasing adjusted odds of wellness among young US children."⁴ In other words, children who experience more than 1 of these hardships suffer greater health and developmental risks than children who

experience only 1 hardship. After this work was published,⁴ findings were presented in the report *Healthy Families in Hard Times: Solutions for Multiple Family Hardships*⁶² to make these results accessible to and useful for policy and advocacy audiences.

We turn now from summarizing various print methods of dissemination of Children's HealthWatch findings (peer-reviewed journal articles for other scientists and reports and briefs for advocates, policy makers, and the general public) to an example of how the work can be synthesized with clinical anecdote and used in the setting of policy formation from legislative hearings to assessment of the effects of a policy change.

Evidence-Based Congressional Testimony from Children's HealthWatch, Example July 2008

The scientific evidence produced by Children's HealthWatch is disseminated in a wide variety of settings, including legislative hearings. In at least one instance, once a policy suggested by this research was adopted, subsequent research efforts permitted rapid evaluation of its effectiveness.

On July 23, 2008,⁶³ Congressman Joe Baca of California called to order the hearing of the House of Representatives Subcommittee on Department Operations, Oversight, Nutrition, and Forestry to review the short- and long-term costs of hunger in America. He stated:

Good morning to all of you. And thank you for being here with the Subcommittee to examine the short and long term costs of hunger and that is a very important subject now as we look at what is going on in our country. I am especially grateful to our outstanding witnesses for making the effort to be here today. I appreciate your willingness to educate us. And I state to "educate us" on the result of various studies you have conducted. And the more education we receive, the better, more knowledgeable we are in dealing with the problem.⁶⁴

Included on the panel of policy and public health experts was Dr Diana B. Cutts, a pediatrician at Hennepin County Medical Center in Minneapolis, MN, and Co-Principal Investigator for Children's HealthWatch. By representing our work, she spoke on behalf of the thousands of families who participated in this survey and informed Congress—in accessible but scientific terms—about the direct, positive impact of the nutrition assistance programs under legislative consideration on the bodies and brains of babies and toddlers in the US. Other witnesses included representatives from the Food Research and Action Center, the USDA, California Food Policy Advocates, the Harvard

University School of Public Health, and the Schneider Institutes for Health Policy at Brandeis University.

Synthesizing Children's HealthWatch findings and her personal experience as a pediatrician, Dr Cutts elucidated the relationship between food insecurity, its harmful health effects, and the protective buffer that nutrition assistance programs can provide. Dr Cutts also described the clinical manifestations of hunger. By describing the increased vulnerability to chronic illness and infection in one particular patient and citing Children's HealthWatch findings on thousands of food insecure households with young children, she illuminated for the committee how science was very real in the lives of her patients. Children's HealthWatch research findings were used to make clear this connection was not only the anecdotal experience of a single pediatrician. She stated:

Children from food insecure households are 30% more likely to be hospitalized because of their diminished reserve and vulnerability in the face of typical childhood illness. . . .these kids can't just bounce back because their immune systems are so depressed from inadequate nutrition that they often begin a cycle of weight loss and recurrent infections that then perpetuate each other.

Dr Cutts also described the personal stories of several children living in food insecure households, some of whom have conditions illustrated in Children's HealthWatch publications, including anemia and developmental delays. Dr Cutts described how household hardships compound medical problems as she told the story of the family of a recently hospitalized 5-year-old asthmatic child who could not afford the child's medications because they needed to pay for food, utilities, and rent. After connecting the dots between policy and child health for the committee, Dr Cutts suggested a practical application of Children's HealthWatch's findings through federal policy. She stated:

Do all of my patients' ills stem from food insecurity? Of course not. But, my reality is that for more than a third of them, food insecurity is a constant companion to their health, directly and indirectly influencing it in both immediate and distant ways. . . . But my reach as their doctor is typically one child, one family at a time. Your reach spans the country and I urge you to think of our time together in clinic and boldly work to create programs and policies that promote healthy and bright futures for all children. For example, I know that Congress is considering another economic stimulus package; I encourage you to make a temporarily increased food stamp benefit part of the package, as it would do so much to directly help the children I've just told you about. . . . Other programs that assist low-income families with basic needs that compete with the food budget, such as housing, energy, and childcare assistance, are equally vital, particularly in our current economic climate of rising food and energy prices.

The increased SNAP benefit, advocated by many stakeholders, was ultimately supported by the committee and issued to SNAP recipients in April 2009 as part of the American Recovery and Reinvestment Act. Dr Cutts's testimony and Children's HealthWatch data played a small role in making that happen.

The question then arose: did the increase actually make any impact? Children's HealthWatch researchers were recently able to address this question directly in real-time with data collected before, during, and after the SNAP increase. The policy action brief, *Boost to SNAP Benefits Protected Young Children's Health*,⁶⁵ (image below) was released in October 2011 and showed that young children in families receiving SNAP after the increase were more likely to be classified as "well" (as defined above) compared to young children in families who were likely eligible but not receiving the benefit. The release was timed to allow consideration by legislators convening for the (now dissolved) Joint Select Committee on Deficit Reduction, charged with reducing the national deficit.

The brief *Boost to SNAP Benefits Protected Young Children's Health* was disseminated to educate all Capitol Hill staff focused on agriculture issues (under which nutrition assistance falls) and policy and legislative contacts nationally and in each Children's HealthWatch city.

CHILDREN'S HEALTHWATCH POLICY ACTION BRIEF

CHILDREN'S HEALTHWATCH POLICY ACTION BRIEF October 2011

Boost to SNAP Benefits Protected Young Children's Health

The Great Recession has taken a significant toll on American children. In 2010, 25 percent of children under age five were living in poverty (up from 21 percent in 2007).¹ Not surprisingly, the increase in the poverty rate that gave hand in hand with the widespread increase in poverty for families with children under age five, which increased from 17.2 to 22 percent over the same period.² (20 percent increase).³

Fortunately, the Supplemental Nutrition Assistance Program (SNAP)—formerly the Food Stamp Program—has demonstrated rapid response to economic increases and the number of families receiving its benefits. Since the beginning of the recession SNAP participation increased by 17.2 million people, or 63 percent, from 44.5 million in May 2011.⁴ SNAP recipients received increased assistance in 2009 when the American Recovery and Reinvestment Act (ARRA) raised SNAP benefits across the board by a maximum of 13.5 percent to stimulate the economy. SNAP has been down since January between \$1.72 and \$1.79 in its economic activity for only 11 months.⁵ Private and state jobs, and improve the food security of low-income households. A recent study by the USDA Economic Research Service (ERS) showed that the ongoing ARRA SNAP improvements reduced food insecurity among low-income households during a time of tremendous economic hardship. ERS found that food insecurity rates among SNAP-eligible families with incomes less than 130 percent of the federal poverty line declined by 22 percentage points. Food insecurity rates were unchanged among households with incomes sustained at above the SNAP eligibility level.⁶

What makes SNAP benefits valuable for those more directly making the cost of food. ERS identified are also some of the most difficult choices about the health of young children. Recent research by Children's HealthWatch compared the health of children in families receiving SNAP with those in families that are not eligible for the program, but not receiving SNAP before and after the benefit increase. Families eligible are based on their participation in other means tested programs.

Summary of Findings

1. This April 2008 increase in SNAP benefits protected the health and well-being of young children, low-income children during a period of growing financial hardship for many families in America.
2. Prior to the increase in SNAP benefits, children in families receiving SNAP were no more likely to be food insecure than those in similar families who were eligible but not receiving SNAP.
3. In the two years after the increase, children in families receiving SNAP were significantly more likely to be food insecure than those in similar families who were eligible but did not receive SNAP.

Children's HealthWatch and others have already shown that SNAP food security protects children's health by reducing food insecurity.⁷ This new research demonstrates that increased SNAP benefits would also have a positive impact on young children's health. We have known for some time that the pre-recession benefits were cut as a result that was too low. A central element of SNAP benefit calculations is the cost of the Thrifty Food Plan, the national standard for a nutritious diet. Annual SNAP benefits are calculated based on a household's size. SNAP recipients received the maximum benefit in 2008 (rather the ARRA increase would have been an additional 10 percentage points in Boston and \$124 per month in Philadelphia to purchase the Thrifty Food Plan). This ARRA increase, which applied benefits to 180 million families, a household of four, would have helped to bridge the gap between the cost of a nutritious diet and the SNAP benefit and thus improved children's health.

Figure 1: Young children in families who received SNAP benefits more likely to be well

Year	eligible but not receiving SNAP (%)	receiving SNAP (%)
2006	~75	~85
2007	~75	~85
2008	~75	~85
2009	~75	~85
2010	~75	~85

Conclusion

This scientific evidence shows that access to the foundation for children's health and academic success is established that first three years of life. As such, we ensure that all our children arrive at school healthy and ready to learn. As a nation we must make sure that families have the resources to nourish their children and to keep them in their early years. Children's HealthWatch research indicates that increased SNAP benefits can have a positive health impact. Sustaining these improved benefits would promote the health and well-being of America's youngest and most vulnerable children. SNAP can help us to be ready to learn, and, ready to learn.

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2. U.S. Census Bureau, Current Population Reports, 2010, 2008, annual social and economic supplements.
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Limitations of Children's HealthWatch Findings

Although the Children's HealthWatch dataset has many strengths, there are certainly limitations. Interviewers are trained to be as professional as possible, properly describe consent procedures, assure the participant of confidentiality, and administer the survey to each participant equally and without bias. However, it is possible that a participant may respond to the survey differently because he or she is in a clinical setting. As with any study, there are also eligible individuals who choose not to participate for their own reasons and other individuals who are not eligible for the study as previously described. Accurate income data other than that based on broad categories of eligibility for public health insurance and other public benefits is difficult to elicit; as a result, the child's insurance status is typically used as a proxy for low-income in Children's HealthWatch analyses (private insurance is used as an exclusion criteria for the analytic dataset).

As with any cross-sectional study design, one cannot determine causal relationships from the findings in any of the studies mentioned. At this time, the Children's HealthWatch sample is not representative of rural populations and no longer collects data from institutions in the western parts of the country. As in many sentinel systems designed for timeliness and early identification of trends, the survey assesses families in emergency departments and hospital-based clinics serving predominantly lower income populations.⁶⁶ Therefore, the children represent a group at elevated risk for negative health outcomes and/or developmental risk rather than a random sample of all children in the US. Because the research group is primarily comprised of health professionals rather than economists, the statistical techniques used are derived from public health practice rather than those more commonly used by economists who study hardship and public assistance programs.⁵

Key Points and Children's HealthWatch Recommendations for Policy Action

The body of work by Children's HealthWatch underscores the importance of food security in ensuring healthy growth and development for young children. Funding of SNAP and discretionary programs, like WIC and housing subsidies, targeted to poor families is currently politically contentious. Children's HealthWatch often tries to reframe the discussion by pointing out that the programs should not be viewed as ideological footballs but as effective methods of health promotion; whether or not a family receives assistance from these programs can be the determining factor in a child's overall health and development. While there are other

findings not presented here due to space considerations, the research summarized in this paper provides evidence-based support for:

- Maximized and sustained funding for programs like SNAP, WIC, TANF, LIHEAP, and housing subsidy programs.
- Maintenance of program structure for SNAP and other key entitlement programs. Changes to the fundamental structure of these programs would remove their ability to expand in tough economic times and would leave many eligible people without benefits when they need them most.
- Re-evaluation of the use of the Thrifty Food Plan as the basis for SNAP benefit calculations in order to better gauge the true cost of purchasing foods that form a healthy diet; the Low Cost Food Plan is a more realistic reflection of costs in urban communities. This will likely result in an increase in SNAP benefits.
- Reduction or elimination of barriers to key public assistance programs for those who are eligible and want to apply, including legal permanent residents.
- Careful consideration of the impacts of legislation dealing with greenhouse gas emissions and global climate disruption to ensure energy price increases do not fall disproportionately on low-income families.

Discussion

Children's HealthWatch provides one model of how the clinical and research expertise of pediatric health professionals can be utilized, as the American Academy of Pediatrics urges, to "stand up, speak up, and step up for children."⁶⁷ Other leading researchers focus on school-age children, adolescents, and children with special needs and employ a variety of methodologies to assess hardship.⁸⁻¹¹ The pediatric surveillance model is different from those of equally important efforts from other disciplines. Although open to criticism from an econometric perspective, such a model leverages the credibility of pediatricians to gain the attention of policy makers and the public for the youngest, poorest, and most invisible Americans—our young children.

For such children, early deprivation of basic needs may result in poor health and developmental delays that may or may not be remediated later in life. These children struggle in the classroom and, as a result, may incur long-term costs for school and health care systems and later difficulties participating in a competitive global workforce. As we illustrated with the example of Dr Cutts's 2008 testimony before Congress, 2009 increase in SNAP benefit, and our recent 2011 Policy Action Brief linking

the increase to positive child health outcomes, good science makes good policy when shared effectively. Scientists can generate credible evidence for other scientists. The challenge is to translate the findings into useful formats for advocates, funders, and policy makers so that accurate information is disseminated, effective policy is proposed, resources are focused, and decision makers understand the impact of their choices based on data rather than ideology. With empirically sound evidence, advocates can bring their concerns to policy makers and educate them about programs that have been shown to decrease America's families' material hardships and suggest budget priorities that might best allocate critical resources to key assistance programs. Working alone, no group will solve children's poverty and multiple hardships. However, working collaboratively each group has a role to play in protecting the health and well-being of young children and their families.

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