Inside the Refrigerator:

Immigration Enforcement and Chilling Effects in Medicaid Participation

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Abstract:

"Chilling effects" are a popular explanation for low program take-up rates among immigrants, but the effects of an icy policy climate are inherently hard to measure. This paper finds robust evidence that heightened Federal immigration enforcement reduces Medicaid participation among children of non-citizens, even when children are themselves citizens. The decline in immigrant Medicaid participation around the time of welfare reform is largely explained by a contemporaneous spike in enforcement activity. The results imply that safety net participation is influenced not only by program design, but also by a broader set of seemingly unrelated policy choices.

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Given the widespread concern about moral hazard and crowd-out arising from social safety net programs, it is perhaps surprising that a high fraction of low-income individuals fail to participate in programs for which they are eligible. A Kaiser Family Foundation report estimates that 52 percent of eligible adults without private insurance took up Medicaid in 2002, for example (Davidoff *et al.*, 2005). Take-up rates are particularly low for immigrants; just 30 percent of eligible non-citizen adults were enrolled in Medicaid in 2002, compared with 57 percent of citizens. (Davidoff *et al.*, 2005).

The factors that promote or inhibit Medicaid enrollment are of particular interest to policy-makers. Estimates suggest that a majority of the nation's uninsured children are eligible for Medicaid and other public programs. For example, a 2002 Urban Institute report estimates that up to 57 percent of uninsured children are eligible for Medicaid and another 26 percent are eligible for SCHIP (Dubay *et al.*, 2002).[1] Although Medicaid enrollment can occur after a negative health shock, *ex ante* enrollment may facilitate access to and utilization of preventative care, and may reduce avoidable hospitalizations (Buchmueller *et al.*, 2005).[2] Enrolling eligible children in the Medicaid program has the potential to reduce un-insurance rates and improve child health.

[1] These estimates may include some children who are in fact ineligible due to immigration status.

[2] The review piece by Buchmueller *et al.* (2005) concludes that extending insurance coverage to the currently uninsured would increase child physician visits by 30 to 50 percent, and that these visits would increase preventative care.

There is also widespread interest in the determinants of program participation more generally. In the wake of the 1996 welfare reform and the associated decline in participation in public programs, some researchers have posited that the general policy environment can affect program participation even for those who are eligible. Such indirect effects are termed "chilling effects" because they arise from an icy policy climate rather than from eligibility rules. The term more generally is used to describe a situation in which "speech or conduct is suppressed by fear of penalization at the interests of an individual or group."[3] In the context of welfare reform, "chilling" has been cited as a potential explanation for declines in program participation beyond what would be predicted due to eligibility changes.

The "chilling" literature has emphasized the disproportionate decline in program participation among immigrants following welfare reform. Empirically, "chilling" has been treated as a residual that explains otherwise puzzling responses to changes in safety net programs. This paper investigates a previously unexplored and quantifiable determinant of chilling for immigrants, Federal immigration enforcement, to assess the extent to which the overall policy environment influences participation decisions in Medicaid. The results suggest an economically and statistically significant relationship

[3] http://en.wikipedia.org/wiki/Chilling_effect_(term). Supreme Court Justice William J. Brennan used the term to describe a situation in which there was a policy deterring freedom of expression but no law explicitly prohibiting the expression.

between the level of enforcement and participation in Medicaid by children of non-citizens, even when the children themselves are citizens.

The findings in this paper suggest that the policy goal of reducing un-insurance among American children may be at odds with the policy goal of enforcing immigration law. The results also highlight the importance of seemingly unrelated policy choices in determining take-up of safety net programs.

I. Background

Economists interested in understanding take-up of public programs have emphasized the roles of stigma, information, and program design.[4] Though a full discussion of the take-up literature is beyond the scope of this paper, Remler and Glied (2003) and Currie (2004) offer reviews. Both conclude that the most consistent determinant of take-up is

[4] For example, Daponte, Sanders, and Taylor (1999) find that providing information about Food Stamp eligibility to low-income households substantially increases participation rates, particularly for households with the most to gain from participation. Other studies explore how culture propagated through social networks could influence participation, perhaps due to stigma or information (Bertrand *et al*., 2000, Borjas and Hilton, 1996, and Aizer and Currie, 2004.)

program design, including information provision, transactions costs, and the generosity of benefits.[5]

Take-up tends to be especially low among immigrants. Immigrants may have particular difficulty obtaining information about programs, completing English application forms, and navigating the complex administrative system. Stigmatization of participation may be high for some immigrant groups (Bertrand *et al.,* 2000). A sizable literature suggests that immigrant groups have higher eligibility for and lower take-up rates of public programs, and that assimilation facilitates take-up (Currie, 2004).

Until recently, the role of the broader policy climate in influencing program participation has received less attention. After welfare reform, however, there was a decline in program participation beyond what would have been expected due to strict eligibility changes, especially for immigrants.[6] Some observers hypothesize that "chilling effects" arising from the anti-immigrant language of the welfare reform bill may have

[5] Despite its popularity as an explanation, there has been little empirical work successfully isolating the effect of stigma on program take-up.

[6] A sizable literature explores the effect of welfare reform on health insurance more broadly. See Bitler, Gelbach, and Hoynes (2005) and DeLeire, Levine, and Levy (2006) for examples. More recent work focuses on the 2005 Deficit Reduction Act which increased citizenship documentation requirements (Sommers, 2010).

discouraged immigrant participation in public programs for which they remained eligible.[7]

Though the existence of "chilling" due to an icy policy climate is plausible, fear and informal dissuasion are difficult to observe. Analysts typically assume that otherwise unexplained declines in participation or take-up of non-citizens are due to chilling effects. Mazzolari (2004), for example, accounts for a wide range of economic and demographic factors and finds that non-citizen immigrants have an unexplained decline in take-up of several safety net programs of 3-4 percentage points following welfare reform. She attributes this excess decline to chilling. Similarly, Kandula *et al.* (2004) report that

[7] The 1996 PRWORA welfare reform bill included a number of provisions that were targeted towards immigrants. Immigrant eligibility for public means-tested programs was restricted for legal non-citizens. For Medicaid, the law banned the use of federal funds for most post-enactment immigrants (those arriving after August 1996) for the first five years after arrival. States had the option to use their own funds to provide Medicaid to this group and about half of them chose to do so. The law also allowed states to ban legal pre-enactment non-citizen immigrants from participating in Medicaid, though almost all continued offering Medicaid to pre-enactment immigrants. In addition, the reform made it harder for states to use their own funds to provide benefits to undocumented immigrants. Welfare reform also restricted immigrant eligibility for food stamps, Supplemental Security Income, and cash welfare in ways that differed across states. Exceptions to immigrant restrictions were made for recently arrived refugees, Cuban/Haitian entrants, and some other groups.

Medicaid participation fell for pre-enactment immigrants following welfare reform even though they maintained eligibility.

Other literature exploits variation in state generosity towards immigrants following reform.[8] The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) welfare reform bill removed Federal support for post-enactment immigrants (those arriving after August 1996) for the first five years of residence; states have the option to use their own funds to support this group. Royer (2005) finds that non-citizen Medicaid take-up declined for those states that denied benefits to new immigrants following reform. Borjas (2003) reports that non-citizen Medicaid participation fell more in less generous states. Noting that most non-citizens in the sample had arrived before 1996 and therefore maintained eligibility for Medicaid, Borjas surmises that declines in participation stemmed from the "chilling effects" of welfare reform. In contrast, Kaushal and Kaestner (2005) do not find differences in new immigrant Medicaid participation in more and less generous states.[9] However, they also interpret their results as evidence of "chilling effects," in this case arising from the icy national policy environment.

[8] Hungerman (2005) uses the differential impact of welfare reform on non-citizens to study charitable giving.

[9] Kaestner and Kaushal (2005) report no evidence of "chilling" in TANF (Temporary Assistance to Needy Families, the program providing cash welfare following welfare reform) participation for new immigrants.

In sum, previous analyses have found that program participation decisions respond to policy changes in ways that extend beyond what would be expected based on the strict eligibility changes. These unexplained changes in participation decisions are commonly attributed to chilling. An Urban Institute report on the subject concludes:

> "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." (Fix and Passel, 1999)

This paper takes a different approach by considering chilling induced by Federal enforcement of immigration laws. Enforcement sharply increased in the mid-1990s, around the same time as welfare reform. There are good reasons to believe that Immigration and Naturalization Service (INS) actions could affect program participation. For example, following Proposition 187's passage in 1994 in California, the Department of Health Services developed a program with the Immigration and Naturalization Service to request repayment of Medicaid benefits for non-citizen immigrants upon re-entry into the United States after a trip abroad. Other anecdotes suggest that applicants for citizenship were occasionally asked to reimburse the government for previously used benefits, though this was not official policy.

For undocumented immigrants seeking health insurance for their children, fear of government authority is a natural concern. Loue, Cooper, and Lloyd (2005) interview 157 women in San Diego in 1999-2001 and find that roughly a quarter of immigrants arriving after 1996 and a quarter of undocumented immigrants had heard that they could not obtain medical care due to immigration status. Similar proportions said they were somewhat or very afraid to obtain medical care for themselves or a family member. A Kaiser Family Foundation study found that 33 to 50 percent of undocumented immigrants said they were afraid they would not receive health care because of their immigration status. Those who reported fear were also less likely to access medical care they believed they needed (Berk *et al*., 2000).

Program design and the general policy climate have the potential to exacerbate or ameliorate the fears of undocumented immigrants. For instance, application forms for means-tested programs typically require or request Social Security numbers for every member of the household, even if only children are applying for benefits.[10] Of six welfare sites studied in a 2003 report for the Department of Health and Human Services, only one uses an application that explicitly states that applicant information will not be shared with the Immigration and Naturalization Service (INS). On the other hand,

[10] Recently, some states have removed requests for household social security numbers on application forms in an effort to increase Medicaid and State Children's Health Insurance Program participation among children of undocumented immigrants (Holcomb *et al.*, 2003).

applications at two sites explicitly state that information will be shared with the INS and that the INS response could affect benefit levels or lead to an investigation.[11]

Heightened enforcement of immigration law is known to affect immigrant behavior. Ethnographic research (Nunez and Heyman, 2007) suggests that government checkpoints and patrols reduce the willingness of undocumented migrants to travel. Eleven of twenty unauthorized immigrants in a Texas border region indicated that visible enforcement discouraged visits to health care providers (Heyman, Nunez, and Talavera, 2009).

Labor market outcomes are also linked to enforcement. Davila and Pagan (1997) report that immigrant industry of employment is responsive to worksite enforcement levels, for example. Bansak (2005) finds a negative effect of employer sanctions on wages of likely illegal immigrants in the 1980s. Orrenius and Zavodny (2009) report adverse labor market consequences for Latin American immigrants in the post-2001 period which they attribute to increased enforcement.

Enforcement may particularly discourage interaction with government bureaucracy. An Urban Institute Report detailing three worksite raids indicates that fewer than ten percent of immigrant families relied on public sources of emergency relief following a raid. Perceived ineligibility for assistance and fear of interacting with public agencies were cited as the two main reasons that public assistance was not sought (Capps *et al.,* 2007). Fear that the use of benefits would lead to deportation was cited as a primary reason for

[11]Holcomb *et al. (*2003).

generally low take-up rates of public programs in raid communities. In sum, immigrants appear to be aware of enforcement and to respond to it in a variety of ways.

INS policy could influence the program participation decisions even for legal permanent residents. For example, the welfare reform bill reiterated a long-standing doctrine that immigrants deemed a "public charge" could be deported or denied future citizenship. Though "public charge" deportations have rarely been implemented in the post-war period, the term was not defined in the legislation. It was not until late 1997 that a clarification was made indicating that occasional use of safety net services would not be grounds for deportation or denial of citizenship. Nevertheless, even after that date there were reports of immigrants being told that participation in public programs could jeopardize their immigration status (Schlosberg and Wiley, 1998). A 1999 INS document specifies that permission to (re-)enter the United States or obtain a green card could be linked to an immigrant's use of cash benefits, an immigrant's child's use of cash benefits, or an immigrant's use of medical benefits for long term care (National Immigration Law Center, 2009). Heightened enforcement could intensify fears about public charge consequences of benefit use.

To investigate the interactions between program participation and enforcement of immigration law, I exploit spatial and temporal variation in enforcement action between 1993 and 2002. The increase in immigration enforcement in the 1990s varied substantially across the 33 INS administrative districts and across country-of-origin groups. In the next section, I discuss the patterns of enforcement and factors driving

variation across areas and over time. There has been little previous work examining the link between enforcement and program participation.[12]

II. Enforcement and Enforcement Data

Immigration enforcement data were obtained from the Department of Homeland Security via a 2009 Freedom of Information Act request. The dataset covers fiscal years 1992 to 2003 and consists of counts of Immigration and Naturalization Services "deportable aliens located" as the result of internal investigations, by INS internal district, country of origin, and fiscal year.[13] "Deportable aliens located" is the INS term for apprehensions. Because some cells are suppressed due to confidentiality concerns, these data are supplemented with published reports in the INS Statistical Yearbooks listing deportable aliens located by INS district and fiscal year.

Figure 1 shows trends in enforcement over time. There is a sharp increase in enforcement in the mid-1990s, presumably due to the sharply increasing INS budget and manpower.[14] The Illegal Immigration Reform and Immigrant Responsibility Act of 1996

[12] One exception is unpublished work by Vargas (2010) who explores the effect of fear of deportation on WIC and SCHIP participation for immigrants in mixed status families.

[13] Border enforcement activities are excluded because they are less likely to affect resident immigrants and because the geographic distribution of the impact is unclear.

[14] Full-time equivalent staffing for internal immigration enforcement jumped from 1746 in fiscal year 1995 to 2513 in fiscal year 1998. The overall enforcement budget increased

increased enforcement expenditures and gave the INS expanded authority to locate and remove undocumented immigrants. The number of internal deportable aliens located went from 70,000 in 1995 to 123,000 in 1997, for example. These trends mirror Medicaid participation rates for children of non-citizens.

I aggregate the 33 INS districts into 25 "clusters" of states which map into Current Population Survey geography for use in the analysis described below.[15] The level of enforcement in a fiscal year is summarized by the number of deportable aliens located divided by the estimated number of non-citizens in 1995.[16] The log average enforcement over a two year period including the year prior to and year of the Medicaid decision is the indicator of enforcement activity; results using levels of enforcement rather than logs are reported in the appendix. Figure 2 reports the level of enforcement activity by fiscal year for 7 of the 25 INS clusters in the data. Some areas, such as Texas, experienced sharp

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from 2.1 billion to 3.4 billion over the same time period, and the share of those funds spent on border control declined from 64 to 56 percent, leaving additional resources for internal enforcement and investigations. (Source: "Immigration Enforcement Spending Since IRCA," Migration Policy Institute Fact Sheet, November 2005.)

[15] Clusters are usually a single state or a group of states. The one exception is that the New York metropolitan area within New York state is an independent INS district and its own cluster. INS districts follow county lines and are often states or groups of states.

[16] I estimate the number of non-citizens using IPUMS Census data for 1990 and 2000. The average of these two numbers is the estimated population for 1995.

increases in enforcement activity while others, such as California, saw more modest changes.[17]

To distinguish the impact of enforcement from potential confounding variables, it is helpful to understand what drives variation in enforcement within a district over time. There are several potentially important factors. First, new illegal immigration is likely to affect both the perceived need for enforcement as well as the number of apprehensions conditional on the level of effort. Second, though enforcement is implemented by Federal authorities, local attitudes toward immigration could influence the actions of the district manager. Third, the budget and staff available to district offices have a direct impact on the level of enforcement activity. Finally, district managers have a large amount of discretion as to the level and type of enforcement they pursue. I discuss each issue in turn.

New immigration could be a potential confounding factor if it affects enforcement and has a direct effect on Medicaid participation decisions.[18] I use a number of approaches to

[17] The sensitivity of the results to the omission of individual states is discussed below.

[18] New immigrants are generally less likely to participate in safety net programs, and undocumented immigrants are excluded from non-emergency Medicaid both before and after welfare reform. It is difficult to estimate the number of new undocumented immigrants in a given state. The government produces estimates of inflows of undocumented immigrants for large states based on the Current Population Survey, but these tend to be quite noisy and are not available nationwide. More reliable estimates are

address the potential bias stemming from the correlation between new immigration and enforcement. First, regressions account for the main effect of enforcement on citizens, so any effect of new immigration that burdens non-citizens and citizens equally is controlled. Second, I remove non-citizen children arriving within five years of the survey date from the sample. In some specifications, I further limit the sample to mothers who arrived more than five years ago or mothers who arrived prior to 1992. These results indicate that enforcement affects the Medicaid participation of the long-standing non-citizen population, and that differential rates of new immigration are not driving the results. In addition, I allow new *legal* immigration to a state to differentially affect non-citizen Medicaid participation decisions, but I find no evidence that it does so. Furthermore, I document below that enforcement is not correlated with observable characteristics of non-citizen families in the sample.

A second potential cause of enforcement variation is local attitudes. District managers work for the Federal government and have no obligation to tailor enforcement to local preferences. Local law enforcement officers are not legally permitted to enforce Federal immigration law during the sample period.[19] However, local efforts could facilitate

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produced using decennial Census data but these lack annual detail. *Legal* immigration inflows are reliably reported at the state-year level, however, and are likely to be correlated with inflows of undocumented migrants.

[19] See Seghetti *et al.* (2004). Starting in 2003, the Federal government has trained selected local law enforcement agencies to play a more active role in enforcing immigration law.

Federal apprehensions if undocumented immigrants are apprehended for other crimes and then transferred to Federal authorities.

In the analysis below, I consider three imperfect proxies for local attitudes – media coverage of enforcement activity, survey data on attitudes toward immigration, and immigration issue "report card" scores for Congressional representatives. None of these proxies offer much predictive power and controlling for them does not alter the results. Nevertheless, it is possible that local attitudes are important but not captured by the available variables; if so, the "chilling" that appears to be induced by enforcement may stem in part from general anti-immigrant sentiment at the local level.

Resources available for enforcement activity have an important impact on the number of apprehensions. Although changes in aggregate enforcement spending stemmed from the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 and related Congressional policy changes, it is less clear how resources were allocated across districts. Reports typically describe the INS as a dysfunctional agency without the cultural will or the information infrastructure to make optimal resource allocation decisions.[20] Davila, Pagan, and Grau (1999) suggest that the agency seeks to maximize total apprehensions rather than minimize the number of undocumented immigrants.

[20] See, for example, Center for Equal Opportunity (1995), Siskin *et al.* (2006), and General Accounting Office (1999).

Furthermore, the bureaucracy of the INS is generally perceived to leave a large amount of discretion to district managers. Many observers lament the lack of centralized decision making and the absence of communication between districts. Martin (2000), for example, notes:

> "Consistency of approach among district offices has been a longstanding issue for INS….[T]he position of INS district director has traditionally carried considerable power and wide enforcement discretion. District directors proudly place their own distinctive personal stamp on the actions of the district office, and sometimes this custom has led to broad disparities in actual practices, with regard to both enforcement and services (adjudications). Even within district offices, particular units sometimes follow their own priorities. (p.2)"

Similarly, a GAO report concludes that the "INS leadership had allowed INS' organizational structure to become decentralized without adequate controls. Specifically, its regional structure had created geographical separation among INS programs and hampered resource allocation and consistent program implementation."[21] Idiosyncratic preferences of district managers combined with aggregate budget fluctuations are likely important determinants in the degree of immigration enforcement within districts over time.

[21] General Accounting Office (1999), page 3, summarizing a January 1991 GAO/GGD report.

In sum, variation in immigration enforcement may stem from several sources. Because the determinants of enforcement cannot be easily characterized, the empirical strategy controls for a wide range of potential factors that could be correlated with enforcement. The key identifying assumption is that, after controlling for these factors, variation in enforcement stems from sources that are uncorrelated with differential Medicaid participation for children of non-citizens.

III. Medicaid Data and Other Data

Information on Medicaid participation comes from the March Annual Demographic Supplements to the Current Population Survey (CPS), a survey implemented by the U.S. Census Bureau which aims to be nationally representative of households in the United States.[22] The CPS asks whether each individual in the household was covered by Medicaid in the previous calendar year and is among the most commonly used data sets in studies of Medicaid participation. In the years following the introduction of the State Children's Health Insurance Program (SCHIP), children participating in the SCHIP program are coded as participating in Medicaid.[23] Citizenship status and country of

[22] Undocumented immigrants are likely to be undercounted in the Current Population Survey; legal status of non-citizens is not reported.

[23] States vary as to whether SCHIP programs are administratively distinct from the Medicaid program.

origin of each household member are available starting in the 1994 survey. The survey contains a number of other demographic and economic indicators as well.

I pool the March surveys for the years 1994-2003 to generate the sample, which covers the reference years 1993-2002. My sample is limited to children under 18 years of age who can be matched to a mother within the household. I also exclude children directly targeted by the provisions of the 1996 PWRORA bill: non-citizen children whose mothers arrived less than five years prior to the survey. Another advantage to excluding this group is that it mitigates bias coming from new immigrant inflows; such inflows are likely to be associated with increased enforcement. The primary analysis is based on a low-SES sample, which is limited to children below 200 percent of the poverty line whose mothers lack a college degree. However, because it is possible that income is endogenous to enforcement, I also show results for the full sample.

I assign children's status based on their mother's country of origin and citizenship status, under the assumption that mothers are likely to make decisions about Medicaid enrollment for the family.[24] The immigration status of non-citizens (i.e. whether they are

[24] Using mother's status allows one to pool mother-only and two-parent families. Alternative methods of assigning child's status are also explored below; the citizenship status of the mother's spouse (typically the child's father) appears to be at least as important as that of the child's mother, so the baseline results are conservative. Children who are themselves non-citizens appear to be more responsive to enforcement than other children of non-citizens, as shown in Appendix Table 2.

documented or undocumented immigrants) is not observable in the CPS. Below I show that children whose mothers are from counties with a high number of undocumented migrants are more sensitive to enforcement. However, I cannot rule out the possibility that legal non-citizens are being "chilled" by enforcement efforts.[25]

Under-reporting of program participation is an important limitation of these data. Meyer, Mok, and Sullivan (2009) find substantial under-reporting of public benefit receipt compared to administrative records in five major surveys, including the Current Population Survey. The Meyer *et al.* study does not examine Medicaid participation, but finds reporting rates of only 50-70 percent for AFDC/TANF (cash welfare) in the CPS. Medicaid misreporting may be a particular problem because state Medicaid programs have multiple names and Medicaid may lack the salience of cash welfare for participants. However, Klerman, Ringel, and Roth (2005) find a Medicaid reporting rate of 70 percent for adults and 75 percent for children in the CPS using California data, with much lower rates for welfare reporting in the same sample. Of particular concern is the potential that under-reporting behavior is responsive to enforcement; I explore the implications of endogenous under-reporting below.

[25] Legal non-citizens could fear that benefit use would jeopardize their transition to citizenship, and this fear might be heightened in a period of high enforcement.

As is common in the literature, I use the data available in the CPS to impute each child's Medicaid eligibility.[26] This imputation includes measurement error. For example, individuals with high levels of medical expenses may qualify for Medicaid but appear ineligible, whereas individuals with high levels of assets may be disqualified but appear eligible. I use two alternative measures of eligibility. Because recipients of AFDC/TANF (cash welfare) are typically enrolled in Medicaid, the first eligibility measure incorporates imputed AFDC/TANF eligibility. A child is imputed to be eligible for Medicaid if her family appears to qualify for AFDC/TANF or if her family appears to qualify for Medicaid via "expansion eligibility." "Expansion eligibility" includes children with family income low enough to qualify for Medicaid regardless of AFDC/TANF status.

Because AFDC/TANF eligibility is difficult to measure, a second definition of Medicaid eligibility relies on expansion eligibility only.[27] Over 88 percent of children deemed eligible through the first definition are imputed to be eligible using the Medicaid expansion eligibility rules only. Both measures of eligibility are imperfect, and analyses

[26] Many thanks to Lara Shore-Sheppard for sharing the imputation algorithm and eligibility rules.

[27] To impute eligibility for TANF after 1996, I use AFDC rules in place in 1996. For subsequent years, states were required to offer Medicaid to those children who would have been eligible under AFDC rules. States also have work requirements and other policies that shape eligibility for TANF; these are not fully captured by my imputation algorithm.

that examine take-up (rather than overall participation) should be interpreted with some caution.

Table 1 shows the key summary statistics for the children in the low-SES sample and the full sample. Children of non-citizens are also more likely to be income-eligible for Medicaid, to lack health insurance, and to have inferior health status. Non-citizen children have less educated mothers but are less likely to live in single parent families. Medicaid participation is highest for children of non-citizens, mainly because they tend to be poor.

The analysis also requires information on state welfare policy. I rely on detailed information provided by Zimmerman and Tumlin (1999) on state welfare policies related to immigrants following welfare reform. I use three definitions of generosity. First, I follow Borjas (2003) and consider a state "generous" if it offered food assistance or SSI to pre-enactment immigrants or offered any of four major programs (TANF, Medicaid, food assistance, or SSI) to post-enactment immigrants.[28] This definition includes the six largest immigrant states; 89 percent of children of non-citizens in my sample live in a

[28] Post-enactment immigrants are those arriving after welfare reform in August 1996. TANF refers to Temporary Assistance to Needy Families, the cash welfare which replaced Aid to Families with Dependent Children (AFDC) program after welfare reform. SSI refers to Supplemental Security Income, which provides cash to low-income disabled individuals.

generous state according to the Borjas definition.[29] Kaushal and Kaestner (2005) offer a simpler definition, describing a state as "generous" if it offered TANF or Medicaid to post-enactment immigrants. Under this definition 56-57 percent of children of non-citizens live in generous states. Among the six largest immigrant states, only California and Illinois are considered generous. As a third alternative, I describe states as generous if Zimmerman and Tumlin (1999) categorize immigrant safety net programs in the state as most available or somewhat available. All of the major immigrant states except Texas are included as generous; 72 percent of children of non-citizens live in generous states according to the Zimmerman and Tumlin definition. For all three measures of generosity, the state is labeled as generous or not generous after welfare reform and the generosity variable equals zero for all states prior to welfare reform.

I measure perception of enforcement using newspaper coverage of immigration enforcement events; a typical event is a raid on an employment or housing site. The sample of newspaper articles comes from a balanced panel of newspapers available in Lexis-Nexis (English) and Proquest Ethnic NewsWatch (English and Spanish); articles are included if they cover a non-criminal internal immigration enforcement event involving five or more migrants.[30] I construct three measures of coverage: the number of articles in national news media relating to an event within the cluster, the circulation-

[29] The six states with the highest numbers of immigrants are California, Florida, Illinois, New York, New Jersey, and Texas.

[30] The Spanish-language article sample from ProQuest Ethnic Newswatch is too small to generate meaningful separate analysis.

weighted number of local articles relating to an event within the cluster, and the circulation-weighted number of articles in local newspapers regarding any event. All three measures are adjusted for the cluster population size. Due to incomplete coverage in the databases, these variables are noisy proxies for actual media attention to enforcement.

I also use the American National Election Study (ANES) to calculate state-level measures of attitudes towards immigration.[31] The ANES asks each respondent whether he or she would like to see immigration increased, unchanged, or decreased in the years 1992, 1994, 1996, 1998, 2000, and 2004. The answers to this question are collapsed and aggregated to the state level to generate the fraction of state residents who would like to see immigration decreased. Interpolation is used for non-response years. For states without responses, the average of the Census region is used. Unfortunately, small sample sizes in the ANES mean that this variable does not offer much predictive power.

Finally, I use Congressional representation in each state as a proxy for local attitudes towards immigration. Immigration report cards for each member of Congress are obtained from an advocacy group which aims to curb immigration, NumbersUSA.[32] Report card scores range from 0 to 100 and are based on the members' votes on immigration related legislation from 1989-2010; high scores indicate that the

[31] The National Election Studies (www.electionstudies.org). THE 2004 NATIONAL ELECTION STUDY [dataset]. Ann Arbor, MI: University of Michigan, Center for Political Studies [producer and distributor].

[32]See http://www.numbersusa.com/content/my/tools/grades.

representative typically votes to reduce immigration. State scores are averages of Congressional members' career scores for representatives in office during the two years prior to the CPS survey year.[33]

IV. Methodology and Results

A. Enforcement and Non-Citizen Medicaid Participation

The analysis examines the effect of immigration enforcement on Medicaid participation by children of non-citizens. For an overview of the data, I start by considering a sample of children of non-citizens only. The preliminary linear probability model is:

$$Medicaid_{ict} = \beta_0 + \beta_1 enforce_{ct} + \theta_c * year + \delta_t + \mu_{ict}$$

where *enforce* refers to INS enforcement activity in cluster *c* relevant for participation year *t*, θ_c interacted with *year* controls for a cluster-specific linear time trend, and time fixed effects λ_t control for shocks that affect all non-citizens nationally. Standard errors are clustered by INS cluster to account for common shocks in a given local area.

Table 2 shows the results for the low-SES sample and the overall sample of children of non-citizens. One log-point increase in enforcement activity in one's local area reduces Medicaid participation by 8.7 percentage points for low-SES children and 4.9 percentage

[33] I use the NOMINATE data set (http://www.voteview.com/dwnomin.htm) and Wikipedia to identify members of Congress in office at the end of each Congressional session.

points for all children. It is also evident from Table 2 that there is no comparable effect on children of non-citizens, suggesting that the results for the non-citizen sample are not generated by factors discouraging Medicaid participation more generally. Furthermore, there are no comparable effects if one considers the *lead* in enforcement, where the lead is defined as the average of the survey year (the year following the reference year) and the subsequent year. These results suggest that enforcement reduces Medicaid participation for children of non-citizens.

B. Full Analysis of Participation

To improve statistical power and to more fully account for local shocks, the bulk of the analysis combines non-citizens and citizens and looks for a *differential* response to enforcement activity. The preferred specification is a linear probability model:

$$Medicaid_{icsgt} = \beta_0 + \beta_1 enforce_{ct} * noncit_i + \beta_2 enforce_{ct} + \Omega_{csg} * noncit_i + \lambda_t * noncit_i + X_i B_3 + \mu_{icsgt}$$

where *enforce* refers to INS enforcement activity in cluster *c* relevant for participation year *t*, $noncit_i$ indicates that the mother of child *i* is a non-citizen. Controls account for cluster-state-group-citizen fixed effects $\Omega_{csg} * noncit_i$ to capture permanent state differences facing children of non-citizens of a particular country-of-origin group,[34] and year dummies λ_t interacted with $noncit_i$ to account for annual changes in non-citizen participation nationally. Demographic controls X_i include child age*year fixed effects,

[34] The New York City metropolitan area and the remainder of New York are treated as separate "cluster-states" because they are located within separate INS clusters.

mother's education, mother's marital status, indicators for whether the family lies below 100 or 200 percent of the poverty line, an indicator for whether the mother has been in the U.S. at least five years, an indicator for whether the mother arrived in the U.S. during the 1980s, and an indicator for whether the mother arrived prior to 1980. Standard errors are clustered on INS cluster to account for common shocks. In this specification, the key coefficient β_1 represents the effect of enforcement on children of non-citizens over and above the effect of enforcement on other children.

Panel A of Table 3 shows the main results for the low-SES sample with different sets of controls. The preferred baseline specification (second column) shows that one log point increase in enforcement efforts differentially reduces Medicaid participation by children of non-citizens by 9.2 percentage points. One can also restrict to citizen children, children whose mother's arrived more than five years ago, or both.[35] Results are largely comparable for these groups. That is, even for children born in the U.S. to long-standing non-citizen residents, enforcement influences the Medicaid participation decision. Similar effects are estimated if the comparison group is restricted to children of foreign-born citizens.

[35] Recall that non-citizen children whose mothers arrived less than five years ago are potentially directly affected by welfare reform and are therefore excluded from all analyses. The results are not substantively changed if this group is included.

Panel B of Table 3 shows analogous results for the full sample.[36] Point estimates are roughly half the size for this group because few high-SES children participate in the Medicaid program. Nevertheless, even in the full sample there is a statistically significant reduction in Medicaid participation for children of non-citizens of at least 4.7 percentage points.

In Appendix Table 1, I explore whether enforcement is predictive of other observable factors that might influence participation. These include family poverty status, mother's marital status, mother's education, mother's labor supply, child's age, and mother's time since arrival. There is no statistically significant relationship between enforcement and any of these factors. This fact suggests that Medicaid participation is influenced by enforcement rather than by contemporaneous economic or demographic changes across areas that disproportionately affect non-citizens.

C. Eligibility and Take-Up

Table 4 explores the implications of enforcement for Medicaid eligibility. The baseline results (i.e., those reported in column II of Table 3) could be biased if enforcement changes coincided with state eligibility expansions that disproportionately benefited non-citizens, or if economic conditions changed such that fewer non-citizens were eligible. I

[36] Though children living above 200 percent of the poverty line are unlikely to enroll in Medicaid, the full sample results account for the possibility of an endogenous response of income to enforcement.

impute eligibility in two ways, as described in the Section III. The first incorporates the AFDC/TANF pathway and eligibility arising due to Medicaid expansions and the second ignores the AFDC/TANF eligibility pathway. Columns II and III of Table 4 show that immigration enforcement is not predictive of Medicaid *eligibility* for the low-SES sample or the overall sample.

Table 4 also examines the effect of immigration on take-up of Medicaid – that is, participation conditional on eligibility. Comparing column I to columns IV and V for the low-SES sample, it is clear that the effects of enforcement on take-up are of similar magnitude to the effects on participation. This is not surprising given that at least two-thirds of the low-SES sample is Medicaid eligible. For the high-SES sample, the effect on take-up is larger than the effect of participation. In sum, it appears that enforcement is not correlated with income eligibility. Rather, enforcement discourages take-up *conditional on eligibility* for children of non-citizens.

D. Is Chilling National or Local?

In Table 5, I explore alternative dimensions of enforcement for the foreign born low-SES sample. Column I repeats the preferred analysis for the foreign-born sample using enforcement at the INS cluster level. Column II instead considers enforcement targeted at one's country-of-origin group at the *national* level, and finds that it is not predictive of

Medicaid participation.[37] Similarly, group-specific enforcement in one's cluster has a smaller effect than overall enforcement in one's local area and has a statistically insignificant effect on participation.[38] It appears that aggregate local enforcement is the most important determinant of participation, though some caution is warranted due to measurement error in the local group-specific variable. When all three measures of enforcement are included simultaneously, the standard errors are large and one cannot say anything definitive.

Measures of enforcement are unavailable at geography smaller than the INS district. However, it is possible that metropolitan areas with many non-citizens experience a disproportionate share of district enforcement per non-citizen. Furthermore, even if enforcement is proportional to the number of non-citizens across cities within a district, residents of areas with many non-citizens might be more connected to immigrant social networks and thus more aware of enforcement policy.

To explore effects in high- and low-exposure areas, I find the fraction non-citizen of the total population for each of the 201 metropolitan areas in the sample. The median level

[37] Because the regressions control for non-citizen*year effects, this is the effect of enforcement targeted towards one's group over and above aggregate national changes in enforcement.

[38] The relatively weak results for group-cluster enforcement may stem from measurement error. Local group-specific enforcement is suppressed for small cells in the enforcement data.

of fraction non-citizen is computed for each country-of-origin group in the sample, and for each group the sample is split into those above and below the median.[39] As is evident in column V of Table 5, there is an insignificant effect of cluster-level enforcement for non-citizens residing in areas with few other non-citizens. Columns VI through VIII also suggest no statistically detectible pattern relating enforcement and Medicaid participation for those living in "low exposure" areas.

In areas with many non-citizens, on the other hand, the effect of aggregate enforcement at the cluster level is quite pronounced (see column IX of Table 5). There is also a marginally significant effect of local group-specific enforcement in column XI. In the horse race in column XII, aggregate cluster-level enforcement appears to be more important than group-specific local enforcement, but this may be due to measurement error in the latter variable.

The more substantial impact of enforcement in non-citizen enclaves may arise because enforcement per non-citizen is disproportionately located in these areas, because

[39] I calculate exposure to non-citizens by averaging the fraction non-citizen in the metropolitan area in the 1990 Census and fraction non-citizen in the 2000 Census. The median is constructed separately for each country of origin group because groups that cluster in non-citizen areas may also respond differently to enforcement for other reasons. I combine those at and above the median into a single group; the results are not sensitive to this choice. Results are also quite similar if exposure to non-citizens is replaced with exposure to same-group members.

immigrants have more access to information about enforcement actions, because immigrant social networks are more likely to include someone affected, or some combination of these factors. Baseline participation rates are also higher in high-exposure areas.

E. Who Responds to Enforcement?

In Appendix Table 2, I use a triple interaction approach to explore the responsiveness of different sub-groups to enforcement policy.[40] For example, the first two columns indicate that children under 2 and children under 7 are slightly more affected by enforcement than other children, though the differences are small. Similarly, married mothers are slightly more likely to respond to enforcement. The presence of siblings does not significantly change the impact of enforcement.

According to INS, the share of undocumented residents differs substantially across country-of-origin groups.[41] One might suspect that groups with many undocumented migrants are likely to respond more dramatically to enforcement efforts. Mexicans have the highest proportion undocumented of any group in the U.S.; roughly 52 percent of the Mexican-born population living in the U.S. is estimated to be undocumented. Children of

[40] All two-way interactions are accounted for in these regressions.

[41] U.S. Immigration and Naturalization Service, "Estimates of the Unauthorized Immigrant Population Residing in the United States: 1990 to 2000," Office of Policy and Planning, Report 1211.

Mexican mothers do appear to respond more than other children to enforcement efforts, as shown in the fifth column of Appendix Table 2.

I also examine mothers from countries with at least 25 percent residents estimated to be undocumented.[42] The effect of enforcement is marginally significant for groups in which most immigrants are documented, but is nearly triple in size for groups with a high fraction of undocumented migrants, as shown in column VI. Column VII of Appendix Table 2 indicates that non-citizen children are more responsive to enforcement than other children of non-citizens.

The final columns of Appendix Table 2 investigate whether responsiveness to enforcement varies by child health status. Medicaid participation is most responsive for the healthiest children, perhaps because parents view participation for these children as less essential.[43]

F. Insurance Status, Health, and Program Participation

Table 6 presents the relationship between enforcement and overall insurance status. The effect of enforcement on public health insurance is almost identical to the effect on Medicaid. This suggests that immigrants deterred from Medicaid due to enforcement are

[42] High-undocumented groups include those with mothers born in Guatemala, Honduras, Mexico, Dominica, Brazil, Colombia, Ecuador, Venezuela, and Kenya.

[43] As discussed below, health status may respond directly to enforcement.

not enrolling in alternative public health insurance programs.[44] Private health insurance increases slightly (and statistically insignificantly) in response to enforcement for the low-SES sample. The point estimates from Panel A imply that a 10 percentage point increase in Medicaid participation (due to absence of enforcement) crowds out 1.4 percentage points of private insurance for the low-SES sample. However, the crowd-out "ratio" (the change in private insurance divided by the change in Medicaid) would have standard errors too large to generate a precise crowd-out estimate.

Table 6 also suggests that the reduction in a child having any health insurance (5.8 percentage points due to a log-point increase in enforcement) is only 63 percent of the reduction in Medicaid, implying 3.7 percentage points of "crowd-out" associated with a 10 percentage point change in Medicaid. The discrepancy between the change in private insurance and the change in any insurance stems from the fact that about 8 percent of low-SES children participate both in Medicaid and private insurance over the course of a year.[45] Thus, the point estimates suggest that of every 100 children discouraged from Medicaid participation due to enforcement, 14 enroll in private insurance that they would

[44] Alternative public programs could include idiosyncratic state programs, Indian Health Service programs, military insurance programs, etc.

[45] About 21 percent of low-SES children with private health insurance during some point of the year also have public health insurance during some point in the year. About 17 percent of low-SES children with public health insurance also have private insurance. The CPS does not offer information about whether these sources of insurance are concurrent or sequential.

not have otherwise had, and another 23 rely exclusively on private insurance that they would have had for part or all of the year.[46] These are not precisely estimated numbers, however, and the confidence intervals are also consistent with no crowd-out. Panel B presents results for the full sample; here the point estimates suggest little or no crowd-out.

Reductions in Medicaid participation could lead to inferior child health. Aizer (2003) shows that exogenous increases in Medicaid participation reduce hospitalizations for conditions that benefit from preventative care. Enforcement could also directly impact reported health status by affecting the level of stress in the household or the willingness of parents to seek health care conditional on insurance status. The Current Population Survey asks the respondent to rate the child's health status on a five-point scale from Excellent to Poor; such data are readily available starting in survey year 1996.[47]

The impact of enforcement on reported health status is shown in Table 6. Column V of Table 6 replicates the Medicaid analysis for the survey years 1996 and onward; the point estimates are slightly larger than the baseline. The final columns of Table 6 demonstrate that the higher levels of enforcement are associated with inferior reported health

[46] The different implied crowd-rates highlights the importance of considering within-year insurance transitions. See Buchmueller and Shore-Sheppard (2010).

[47] Similar self-reported health scales are widely used and shown to predict mortality across race/ethnicity groups (McGee *et al.*, 1999). It is nevertheless possible that enforcement could affect reporting biases.

outcomes both in the low-SES and the full samples. Further analysis (not shown) reveals that higher enforcement is primarily associated with children moving from the "very good" health category to the "good" health category. The limited health data in the CPS allow for the examination of the reduced form relationship between enforcement and reported health, but do not allow one to distinguish among mechanisms.

Table 7 presents the estimated effect of enforcement on other poverty programs. The impact on receipt of public assistance (AFDC/TANF) is small and statistically insignificant in the low-SES sample and full samples. In Panel B, a very low SES sample includes only children below the poverty line, who are much more likely to be eligible for cash welfare. The coefficient for welfare participation is larger in the very low-SES sample but still statistically insignificant at conventional levels.

On the other hand, there is evidence that Food Stamp participation does respond to enforcement. The magnitude of the estimated response is comparable to that of Medicaid in all three samples, though less precisely estimated. The differing response of food stamps and cash welfare to enforcement is puzzling. Some states have integrated a Food Stamp screen into the Medicaid/SCHIP determination process and some states have stand-alone food stamp application locations (Holcomb *et al*., 2003), which may

contribute to the explanation.[48] The differences suggest that enforcement can interact with program design to influence participation.[49]

The results also suggest important interactions across programs. The last two columns of Table 7 split the sample into those children receiving either cash welfare or food stamps and those receiving neither. In all three panels, enforcement is associated with reduced Medicaid participation *only* among those not participating in other safety net programs. These results are consistent with the notion that enforcement discourages interaction with all government bureaucracy for many non-citizen mothers; those who are less affected by enforcement frequently participate in multiple programs.

[48] It is also possible that higher marriage rates among immigrants or low welfare participation rates (17 percent for children of non-citizen mothers in the low-SES sample) may influence responsiveness.

[49] A full discussion of differences in responsiveness across programs is beyond the scope of this paper. I also explore two other programs with very low participation rates – Supplemental security Income and Disability Insurance (results not shown). I am unable to detect any statistically significant effects except a very small effect on DI in the very Low-SES sample.

G. State Policy Climate and Local Attitudes

The chilling literature has emphasized state policy generosity towards immigrants. It is important to account for state policy changes around the time of welfare reform in the analysis of the effect of enforcement on Medicaid participation. Researchers have used various criteria to categorize as a state as generous. As described in Section III, I consider three alternative definitions of state generosity. For all three measures of generosity, the state is labeled as generous or not generous after welfare reform and the generosity variable equals zero for all states prior to welfare reform.

Table 8 shows the effect of state policy climate.[50] Both the Borjas and the Kaushal and Kaestner definitions of generosity show a negative (wrong-signed) and insignificant effect of state generosity on non-citizen participation. Inclusion of these variables slightly *increases* the magnitude of the estimated coefficient on enforcement. The Zimmerman and Tumlin definition of generosity is positively (though insignificantly) associated with Medicaid participation. The coefficient on enforcement is reduced to -0.065 when the Zimmerman and Tumlin measure of generosity is included, but the coefficient maintains statistical significance.

As described in section III, I also incorporate several measures of local immigration attitudes – media coverage, local attitudes, and congressional representation. The three

[50] State Medicaid policies vary on a range of other dimensions that could differentially affect non-citizens. A full exploration is beyond the scope of this paper.

media variables interacted with non-citizen status are included in the fourth column of Table 8. Individual coefficients are not shown; only one (the number of national news stories about local events) is statistically significant with the sign as expected and another is wrong-signed and significant. The inclusion of the media coverage variables slightly reduces the magnitude of the coefficient on enforcement but does not affect statistical significance. As shown in the final two columns of Table 8, anti-immigrant sentiment of the population and anti-immigration Congressional representation have no detectible effect on Medicaid participation and do not affect the coefficient on enforcement. These variables, like the media coverage variables, are imperfect proxies for local attitudes, so one cannot rule out the possibility that local attitudes matter to Medicaid participation decisions.

H. Robustness

Appendix Table 3 presents the results of sensitivity analysis. The preferred specification is replicated in the first column. The second column shows the results using a linear rather than logged measure of enforcement. The results suggest that a one percentage point increase in enforcement (e.g. increasing from one arrest per 100 non-citizens to two arrests per 100 non-citizens) reduces Medicaid participation by 4.9 percentage points. This effect is of the same order of magnitude as that implied by the log specification evaluated at the sample mean.

The baseline measure of enforcement divides deportable aliens located by the estimated number of non-citizens in the cluster in 1995. In the third column of Appendix Table 3, the denominator instead incorporates a time-varying measure of the number of non-citizens based on linear interpolation between Census years. This alternative method of defining enforcement has little impact on the results.

The fourth column of Appendix 3 restricts the sample to mothers arriving in the U.S. prior to 1992. The robustness of the results to this sample restriction mitigates concerns about the correlation of enforcement activity with unobservable characteristics of new migrants.

The fifth column incorporates state-citizen-specific linear time trends. This variable reduces the size of the enforcement coefficient by about a quarter and raises the standard error, rendering the coefficient insignificant. The result indicates that some of the identifying variation is caused by differential time trends for non-citizens and citizens across states, which could be caused by enforcement or other factors. Similarly, allowing the effect of the state unemployment rate to vary by citizenship status somewhat weakens the enforcement coefficient. In both the fifth and sixth columns, the enforcement results are weakened only when the new variables and a full set of demographic controls are included, suggesting that the analysis may be limited by statistical power issues (the analyses without controls are not shown).

The final column of Appendix Table 3 controls for the effect of new legal immigration. The results are not substantively changed. I also try dropping each of state one at a time (results not shown). The magnitude and statistical significance of the key coefficient are robust to exclusion of all individual states other than Texas. Dropping Texas reduces the key coefficient to -0.054 and raises the p-value to 0.11. This sensitivity is not surprising given the important changes in enforcement in Texas over the time period and given the fact that about 11 percent of the non-citizen low-SES sample resides in Texas.

The baseline analysis uses the mother's citizenship status to predict Medicaid participation. Appendix Table 4 explores alternative definitions of citizenship. Results are similar if the mother's spouse is a non-citizen, if either parent is a non-citizen, or if both parents are non-citizens. The final two columns of Appendix Table 4 show that having a non-citizen spouse makes a citizen mother much more responsive to enforcement but has a relatively minor effect on a non-citizen mother. In sum, families are responsive to enforcement when either or both parents are non-citizens.

I. Endogenous Citizenship and Endogenous Under-Reporting

One potential threat to identification is that individuals have some ability to decide whether to become citizens, and they may pursue citizenship if the policy climate is less favorable towards non-citizens. Rates of citizenship increased substantially over the

sample period.[51] Van Hook (2003) argues that the changing composition of citizenship may explain up to half of the decline in non-citizen welfare participation following welfare reform. To investigate the possibility of endogenous citizenship, I first examine whether the probability that a child's mother is a citizen appears to respond to enforcement. I do not find evidence that this is the case, perhaps because it usually takes five years of legal residence plus a year or more of processing time to become a citizen.[52]

To further investigate the issue of endogenous citizenship, I instrument for mother's citizenship using her country of origin. In this framework one can control for state-group fixed effects but not state-group-citizen fixed effects. The OLS analysis using the revised specification yields a smaller but statistically significant differential effect of enforcement on children of non-citizens. The instrumented coefficients shown in Appendix Table 5 are larger than the OLS estimates and similar to the baseline effects reported in Table 3, suggesting that endogenous citizenship is not driving the results. This test does not rule out the possibility of selective return migration or survey non-response by those fearing

[51] Van Hook (2003) notes that the number of naturalizations was 240,000 in 1992 and peaked in 1996 at over one million.

[52] Results not shown. In a regression with mother non-citizen on the left hand side and including state-group fixed effects and education controls, the coefficient on enforcement is 0.012 with a standard error of 0.009; in other words, enforcement has an insignificant and wrong-signed coefficient. High application fees, English language requirements, and legal barriers may further deter would-be citizens. Immigrants married to citizens and those serving in the military have shorter residency requirements.

enforcement, but does suggest that the relationship between enforcement and Medicaid participation does not arise because of selective maternal entry into citizenship.

It is also important to consider the effect of bias arising from under-reporting of Medicaid. Of particular concern is the possibility that enforcement reduces the reporting rate differentially for children of non-citizens. To assess the degree to which endogenous under-reporting could be driving the results, I simulate data assuming that the reporting rate for children of non-citizens varies linearly up to 100% with the percentile of the enforcement distribution. Children who report "no Medicaid" are randomly assigned to "Medicaid" accordingly. In the simulation, reporting rates for children of citizens are assumed to be 100% and unresponsive to enforcement.

The results of this exercise are presented in Appendix Table 6. Reporting rates of 80 to 90 percent under the highest enforcement only slightly attenuate the results, and the coefficient on enforcement remains marginally significant even if reporting rates range from 70 percent under the highest enforcement to 100 percent under the lowest enforcement. Thus, moderate degrees of enforcement-induced under-reporting are unlikely to be responsible for the baseline results.[53] It is also reassuring that the

[53] If enforcement does not affect participation but drives severe under-reporting for non-citizens, the baseline results are misleading. It is also possible that enforcement affects the overall survey response rate for non-citizens. If undocumented immigrants are less likely to participate in the survey under high enforcement, the baseline results are likely to be biased towards zero.

estimated response to enforcement is similar in states with stand-alone SCHIP programs and other states (results not shown), suggesting that confusion about whether the program is public insurance is unlikely to be driving the results.

J. Magnitude of the Effects

To gauge the magnitude of the effects, I use the estimated model to predict what would have happened to Medicaid participation among children of non-citizens if enforcement levels had maintained their initial levels – specifically, the average of 1993 and 1994 levels. The results suggest that participation would have fallen from 46.5 percent in survey year 1995 to 45.5 percent in survey year 2000, a drop of 1 percentage point, had enforcement stayed constant at the 1993-1994 levels. The rise in immigration enforcement can therefore explain three-quarters of the actual 4.4 percentage point decline during this time. Using the 1995 to 1999 time frame, the simulation indicates enforcement can explain almost half of the actual 8.3 percentage point decline. A large fraction of the decline in immigrant Medicaid participation around 1996, which has previously been attributed to welfare reform, is due to the contemporaneous rise in immigration enforcement.[54]

[54] Aggregate enforcement explains only a small fraction of the rebound in non-citizen participation rates in the latter years of the sample. Other factors such as the adoption of the SCHIP program, which most states adopted in 1998, may help explain rising participation rates after the 1999 survey year. Buchmueller *et al.* (2008) document that

V. Conclusion

The results presented here cast new light on the chilling of immigrant Medicaid participation around the time of welfare reform. Previous literature documents an unexplained decline in immigrant program participation and hypothesizes that low take-up stems from fear and confusion stemming from changes in welfare policy. The current paper suggests a new potential culprit - Federal immigration enforcement – which contributes to immigrant reluctance to participate in Medicaid. Immigration enforcement "chills" would-be Medicaid applicants even when they remain eligible. The results imply that much of the decline in immigrant Medicaid participation around the time of welfare reform can in fact be attributed to increased enforcement of immigration law. Enforcement is also linked to the health of children of immigrants.

The results suggest a tension between health policy goals and enforcement of immigration law. The findings also highlight the fact that seemingly unrelated policies can have important consequences for program take-up. Economists interested in take-up have mainly focused on program design and interactions across safety net programs. However, interactions across broad policy areas may be important determinants of program participation.

SCHIP take-up among children of immigrants was at least as high as take-up for children of natives, thereby causing convergence in public health insurance rates.

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Figure 1. Medicaid Participation for Children of Non-Citizens and Immigration Enforcement, 1994-2003

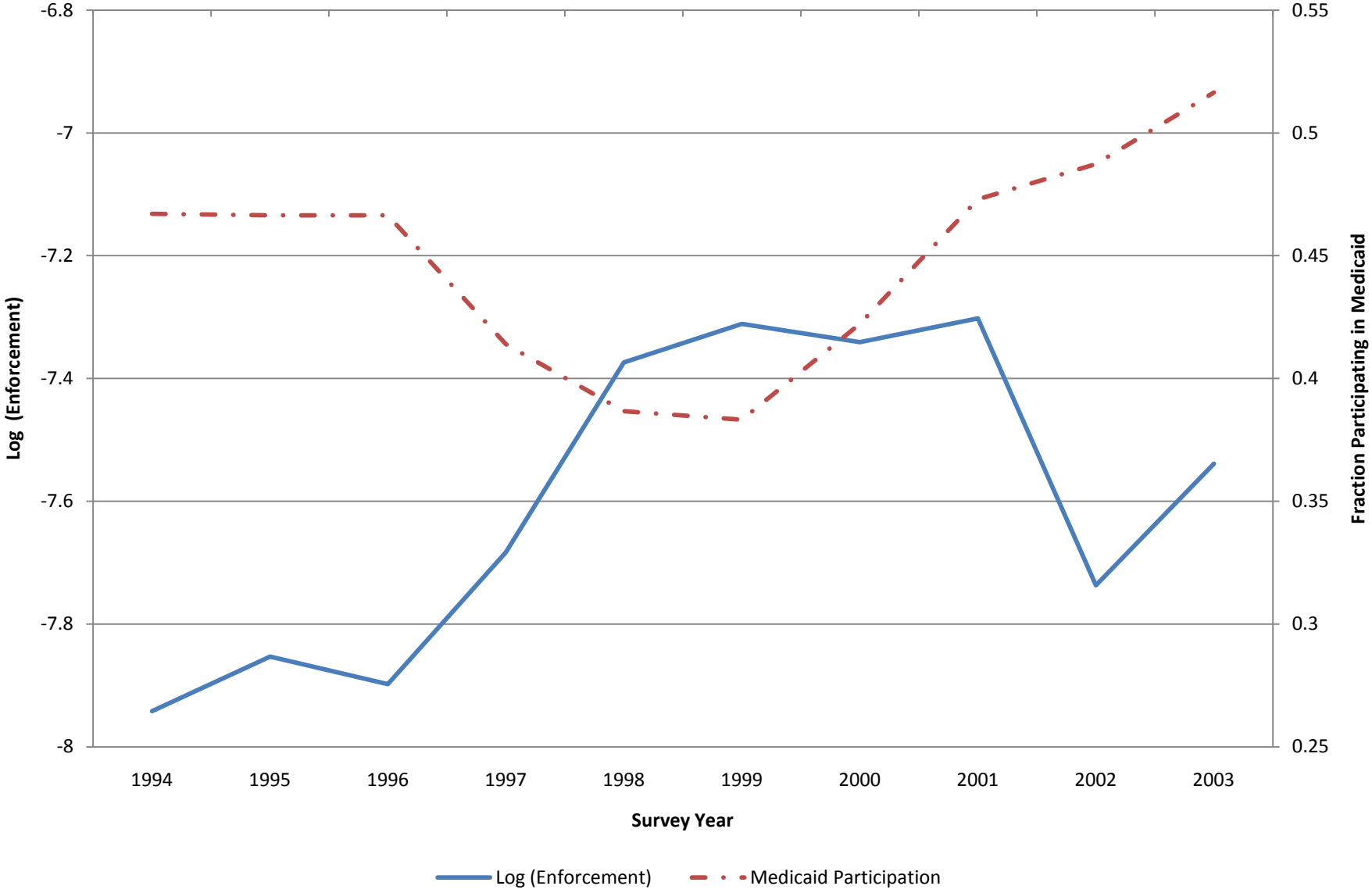

Figure 2. Deportable Aliens Located Per Non-Citizen, Selected Areas

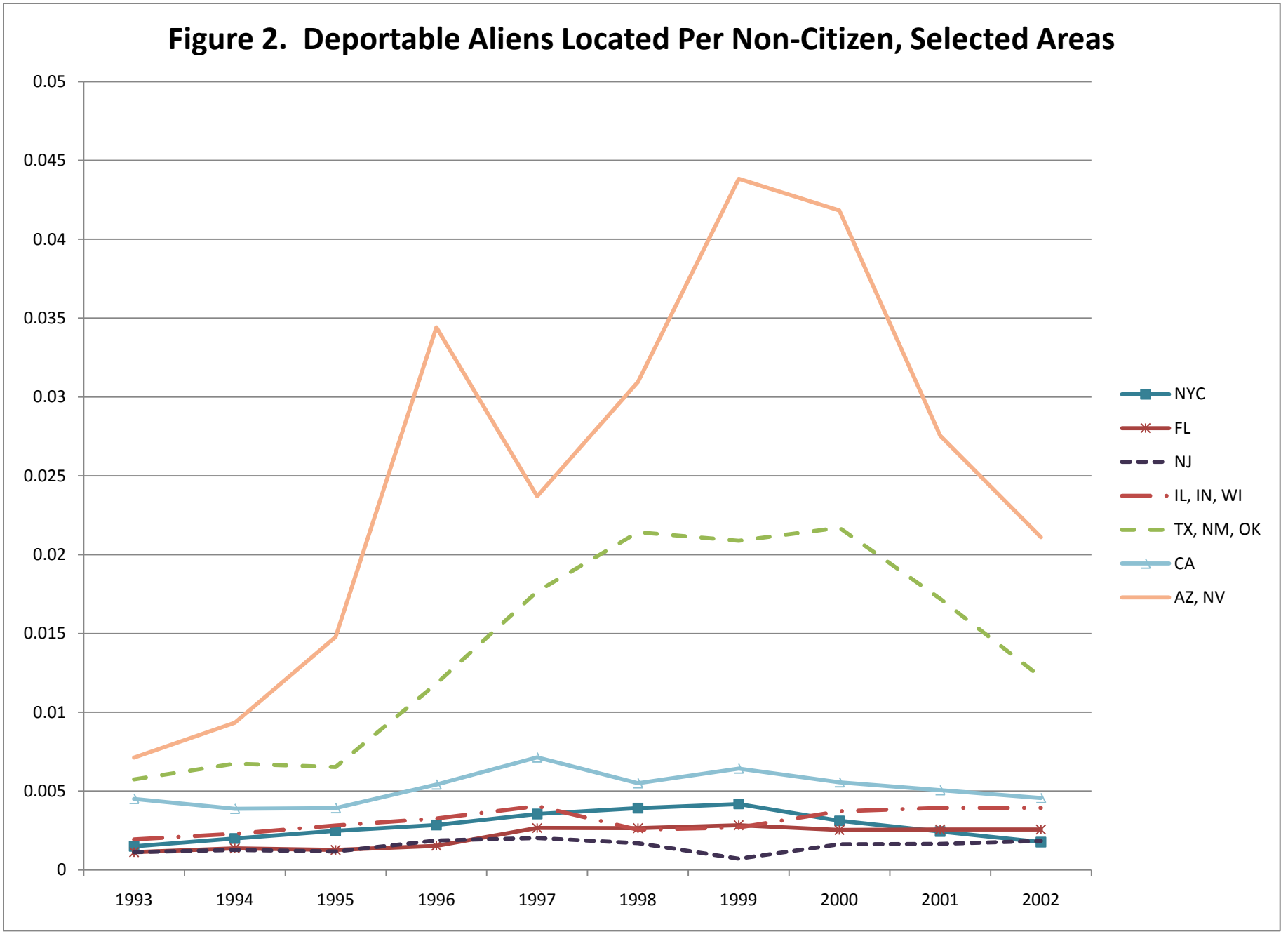

Table 1. Means of Key Variables

	Low-SES Sample			Full Sample		
	Mom Non-Citizen (N=26,942)	Mom Foreign Born Citizen (N=6,978)	Mom Native Born (N=112,286)	Mom Non-Citizen (N=42,012)	Mom Foreign Born Citizen (N=19,371)	Mom Native Born (N=331,558)
Medicaid	0.45	0.39	0.47	0.32	0.17	0.20
Medicaid Eligible (Definition 1)	0.80	0.75	0.73	0.54	0.31	0.29
Medicaid Eligibile (Definition 2)	0.69	0.67	0.64	0.47	0.28	0.26
Any Health Insurance	0.68	0.74	0.84	0.74	0.85	0.90
Any Food Stamps	0.27	0.23	0.35	0.17	0.09	0.13
Any Public Assistance/Welfare	0.17	0.14	0.22	0.11	0.05	0.08
Any SSI	0.04	0.07	0.07	0.03	0.04	0.03
Any DI	0.01	0.01	0.01	0.01	0.01	0.01
Child is Citizen	0.81	0.96	1.00	0.82	0.97	1.00
Mom is High School Grad Exactly	0.22	0.33	0.45	0.22	0.27	0.34
Mom is Some College Exactly	0.09	0.22	0.29	0.13	0.24	0.31
Mom is College Grad or More	0.00	0.00	0.00	0.13	0.27	0.23
Family Under 200% FPL	1.00	1.00	1.00	0.65	0.37	0.36
Child Age	7.47	9.08	7.91	7.51	9.23	8.45
Mom Worked Last Year	0.42	0.57	0.62	0.52	0.73	0.75
Mom Married	0.76	0.70	0.49	0.82	0.84	0.75
Mom Spouse Citizen (if married)	0.22	0.70	0.96	0.31	0.81	0.98
Lives in Generous State (Borjas Definition)	0.89	0.88	0.68	0.89	0.89	0.71
Lives in Generous State (KK Definition)	0.57	0.49	0.41	0.56	0.53	0.45
Lives in Generous State (ZT Definition)	0.72	0.70	0.44	0.72	0.75	0.48
Anti-Immigrant Sentiment in State	0.53	0.52	0.53	0.53	0.52	0.53
National Coverage of Local Events Index	2.24	2.35	2.18	2.27	2.34	2.22
Local Coverage Index	0.04	0.03	0.03	0.03	0.03	0.03
Local Coverage of Local Events Index	0.04	0.03	0.03	0.03	0.03	0.03
Enforcement Level in Cluster*1000	7.79	7.80	8.44	7.53	6.92	8.39
Enforcement Level for Group*1000	1.99	0.88	n/a	1.61	0.49	n/a
Enforcement Level in Cluster-Group*1000	2.15	1.11	n/a	1.73	0.60	n/a
Child in Excellent/Very Good Health	0.69	0.70	0.72	0.73	0.77	0.83
Child in Good Health	0.27	0.26	0.23	0.24	0.20	0.15
Child in Poor Health	0.01	0.01	0.01	0.00	0.00	0.00

Notes: All samples exclude non-citizen children whose mothers arrived within the past five years. The Low-SES sample includes children of mothers lacking a college degree and under 200 percent of the poverty line. Medicaid eligibility definition 1 imputes the AFDC/TANF eligibility pathway; Medicaid eligibility definition 2 does not. Measures of state generosity and anti-immigrant sentiment described in text. Enforcement level is the average number of deportable aliens located in the reference year and previous year per non-citizen in the cluster, group, or cluster-group.

Table 2. Preliminary Analysis

Dep.Var: Medicaid Participation	I	II	III	IV
Sample	**Mother Non-Citizen**	**Mother Non-Citizen**	**Mother Citizen**	**Mother Citizen**
Cluster f.e.	yes	yes	yes	yes
Year f.e.	yes	yes	yes	yes
Cluster-specific time trends	yes	yes	yes	yes
Panel A. Low-SES Sample	*(mean=0.45)*	*(mean=0.44)*	*(mean=0.47)*	*(mean=0.47)*
Log(Enforcement)	**-0.087+**		**0.019**	
	(0.043)		(0.022)	
Log(Lead of Enforcement)		**0.005**		**-0.002**
		(0.032)		(0.023)
Number of Observations	26,942	23,528	119,264	102,790
R-squared	0.042	0.041	0.016	0.016
Panel B. Full Sample	*(mean=0.32)*	*(mean=0.32)*	*(mean=0.20)*	*(mean=0.20)*
Log(Enforcement)	**-0.049+**		**0.014**	
	(0.027)		(0.014)	
Log(Lead of Enforcement)		**0.013**		**0.002**
		(0.018)		(0.013)
Number of Observations	42,012	36,103	350,929	296,775
R-squared	0.033	0.033	0.010	0.010

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster. The lead of enforcement is measured as the average of the number of deportable aliens located per non-citizen in the two years following the reference year in the INS cluster.

Table 3. Effect of Immigration Enforcement on Medicaid Participation

Dep.Var: Medicaid Participation	I	II	III	IV	V	VI
Sample	**All**	**All**	**Kid Citizen**	**Mom Arrived > 5 Years**	**Kid Citizen and Mom Arrived> 5 years**	**Mom Foreign Born**
Mom Non-Cit*State f.e.	yes					
Mom Non-Cit*Year f.e.	yes	yes	yes	yes	yes	yes
Log(Enforcement)	yes	yes	yes	yes	yes	yes
State*Group*Mom Non-Cit f.e.		yes	yes	yes	yes	yes
Demographic Controls		yes	yes	yes	yes	yes
Panel A. Low-SES Sample	*(mean=0.47)*	*(mean=0.47)*	*(mean=0.47)*	*(mean=0.47)*	*(mean=0.47)*	*(mean=0.44)*
Mom Non-Cit*Log(Enforcement)	**-0.106****	**-0.092***	**-0.099***	**-0.095***	**-0.103***	**-0.113***
	(0.031)	(0.035)	(0.041)	(0.035)	(0.040)	(0.048)
Number of Observations	146,206	146,206	140,587	143,599	137,980	33,920
R-squared	0.025	0.226	0.227	0.225	0.227	0.258
Panel B. Full Sample	*(mean=0.21)*	*(mean=0.21)*	*(mean=0.21)*	*(mean=0.21)*	*(mean=0.21)*	*(mean=0.27)*
Mom Non-Cit*Log(Enforcement)	**-0.049***	**-0.047***	**-0.049+**	**-0.048***	**-0.052***	**-0.079***
	(0.023)	(0.022)	(0.024)	(0.023)	(0.024)	(0.028)
Number of Observations	392,941	392,939	384,288	388,856	380,205	61,383
R-squared	0.023	0.359	0.364	0.358	0.364	0.327

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. New York City and the remainder of New York are treated as distinct states because they lie in different INS clusters. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster. Demographic controls include dummies for mother's educational attainment, age*year fixed effects, indicators for being below 100 percent and 200 percent of the poverty line, an indicator for the mother arriving in the U.S. within the previous five years, an indicator for the mother arriving in the U.S. after birth and prior to 1980, an indicator for the mother arriving in the U.S. during the 1980s, and an indicator for the mother being currently married.

Table 4. Effect of Immigration Enforcement on Medicaid Eligibility and Take-Up

	I	II	III	IV	V
Dependent Variable	**Medicaid Participation**	**Eligibility Def 1**	**Eligibility Def 2**	**Medicaid (if Eligibile by Def 1)**	**Medicaid (if Eligibile by Def 2)**
Mom Non-Cit*Year f.e.	yes	yes	yes	yes	yes
Log(Enforcement)	yes	yes	yes	yes	yes
State*Group*Mom Non-Cit f.e.	yes	yes	yes	yes	yes
Demographic Controls	yes	yes	yes	yes	yes
Panel A. Low-SES Sample	*(mean=0.47)*	*(mean=0.74)*	*(mean=0.65)*	*(mean=0.56)*	*(mean=0.55)*
Mom Non-Cit*Log(Enforcement)	**-0.092***	**0.017**	**0.032**	**-0.115***	**-0.112***
	(0.035)	(0.025)	(0.029)	(0.047)	(0.042)
Number of Observations	146,206	146,206	146,206	109,433	96,444
R-squared	0.226	0.453	0.555	0.181	0.183
Panel B. Full Sample	*(mean=0.21)*	*(mean=0.32)*	*(mean=0.28)*	*(mean=0.51)*	*(mean=0.51)*
Mom Non-Cit*Log(Enforcement)	**-0.047***	**0.019**	**0.024**	**-0.099***	**-0.093***
	(0.022)	(0.025)	(0.024)	(0.046)	(0.042)
Number of Observations	392,939	392,939	392,939	126,839	112,367
R-squared	0.359	0.667	0.603	0.207	0.211

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. New York City and the remainder of New York are treated as distinct states because they lie in different INS clusters. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster. Demographic controls as described in Table 3. Eligibility Definition 1 imputes eligibility incorporating the AFDC/TANF eligibility pathway and Medicaid expansions. Eligibility Definition 2 ignores the AFDC/TANF eligibility pathway.

Table 5. Measurement of Enforcement, Foreign-Born Low-SES Sample

Dep.Var.: Medicaid Participation	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
Sample	All	All	All	All	Low Exposure	Low Exposure	Low Exposure	Low Exposure	High Exposure	High Exposure	High Exposure	High Exposure
		(mean=0.44)				*(mean=0.41)*				*(mean=0.50)*		
Mom Non-Cit*Log(Enforcement of All Non-Citizens in Cluster)	**-0.113***			**-0.123**	**0.013**			**-0.021**	**-0.412****			**-0.417****
	(0.048)			(0.074)	(0.060)			(0.121)	(0.069)			(0.096)
Mom Non-Cit*Log(Enforcement of Group Nationally)		**0.057**		**0.076**		**-0.029**		**-0.066**		**0.027**		**0.043**
		(0.091)		(0.076)		(0.161)		(0.170)		(0.122)		(0.078)
Mom Non-Cit*Log(Enforcement of Group Within Cluster)			**-0.047**	**-0.003**			**0.005**	**0.050**			**-0.152+**	**-0.017**
			(0.036)	(0.048)			(0.049)	(0.096)			(0.075)	(0.046)
Number of Observations	33,920	33,920	33,809	33,809	12,089	12,089	12,054	12,054	16,475	16,475	16,457	16,457
R-squared	0.192	0.192	0.190	0.190	0.263	0.262	0.259	0.261	0.172	0.171	0.170	0.172

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. All regressions include full set of fixed effects and demographic controls as in the previous tables. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster. High exposure indicates that the child resides in an area with greater than or equal to the fraction of non-citizens of a typical child from her (mother's) country of origin group.

Table 6. Effect of Immigration Enforcement on Health Insurance Status and Health

	I	II	III	IV	V	VI	VII	VIII
Dependent Variable	**Medicaid**	**Public Health Insuance**	**Private Health Insurance**	**Any Health Insurance**	**Medicaid (Survey Year>=1996)**	**Excellent or Very Good Health (Survey Year>=1996)**	**Good Health (Survey Year >=1996)**	**Poor Health (Survey Year>=1996)**
Mom Non-Cit*Year f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Log(Enforcement)	yes	yes	yes	yes	yes	yes	yes	yes
State*Group*Mom Non-Cit f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Demographic Controls	yes	yes	yes	yes	yes	yes	yes	yes
Panel A. Low-SES Sample	*(mean=0.47)*	*(mean=0.49)*	*(mean=0.39)*	*(mean=0.81)*	*(mean=0.46)*	*(mean=0.72)*	*(mean=0.24)*	*(mean=0.01)*
Mom Non-Cit*Log(Enforcement)	**-0.092***	**-0.092***	**0.013**	**-0.058***	**-0.134****	**-0.078+**	**0.070***	**0.010****
	(0.035)	(0.033)	(0.023)	(0.022)	(0.033)	(0.038)	(0.032)	(0.004)
Number of Observations	146,206	146,206	146,206	146,206	114,904	114,904	114,904	114,904
R-squared	0.226	0.203	0.210	0.085	0.213	0.054	0.047	0.020
Panel B. Full Sample	*(mean=0.21)*	*(mean=0.24)*	*(mean=0.70)*	*(mean=0.89)*	*(mean=0.21)*	*(mean=0.81)*	*(mean=0.16)*	*(mean=0.00)*
Mom Non-Cit*Log(Enforcement)	**-0.047***	**-0.048***	**-0.016**	**-0.044***	**-0.073****	**-0.051***	**0.045***	**0.007****
	(0.022)	(0.023)	(0.017)	(0.017)	(0.021)	(0.022)	(0.018)	(0.002)
Number of Observations	392,939	392,939	392,939	392,939	316,467	316,467	316,467	316,467
R-squared	0.359	0.313	0.393	0.095	0.346	0.054	0.047	0.014

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. New York City and the remainder of New York are treated as distinct states because they lie in different INS clusters. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster. Demographic controls as in Table 3. Columns V through VIII based on years 1996 onwards.

Table 7. Effect of Immigration Enforcement on Program Participation

	I	II	III	IV	V
Dependent Variable	**Medicaid**	**Public Assistance/ Welfare**	**Food Stamps**	**Medicaid, if Receiving Food Stamps or Public Assistance**	**Medicaid, if Not Receiving Food Stamps or Public Assistance**
Mom Non-Cit*Year f.e.	yes	yes	yes	yes	yes
Log(Enforcement)	yes	yes	yes	yes	yes
State*Group*Mom Non-Cit f.e.	yes	yes	yes	yes	yes
Demographic Controls	yes	yes	yes	yes	yes
Panel A. Low-SES Sample	*(mean=0.49)*	*(mean=0.21)*	*(mean=0.33)*	*(mean=0.86)*	*(mean=0.24)*
Mom Non-Cit*Log(Enforcement)	**-0.092***	**-0.015**	**-0.078+**	**0.046**	**-0.111****
	(0.035)	(0.020)	(0.041)	(0.030)	(0.032)
Number of Observations	146,206	146,206	146,206	51,479	94,727
R-squared	0.226	0.258	0.258	0.126	0.121
Panel B. Very Low-SES Sample	*(mean=0.66)*	*(mean=0.36)*	*(mean=0.55)*	*(mean=0.89)*	*(mean=0.33)*
Mom Non-Cit*Log(Enforcement)	**-0.152****	**-0.049**	**-0.152***	**0.073+**	**-0.202****
	(0.036)	(0.031)	(0.059)	(0.038)	(0.042)
Number of Observations	67,184	67,184	67,184	38,707	28,477
R-squared	0.146	0.228	0.131	0.116	0.143
Panel C. Full Sample	*(mean=0.21)*	*(mean=0.08)*	*(mean=0.13)*	*(mean=0.85)*	*(mean=0.10)*
Mom Non-Cit*Log(Enforcement)	**-0.047***	**0.007**	**-0.051+**	**0.061+**	**-0.055****
	(0.022)	(0.011)	(0.027)	(0.034)	(0.016)
Number of Observations	392,939	392,939	392,939	55,894	337,045
R-squared	0.359	0.287	0.383	0.134	0.148

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. The very low-SES sample is further restricted to those below the poverty line. New York City and the remainder of New York are treated as distinct states because they lie in different INS clusters. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster. Demographic controls as in Table 3.

Table 8. Local Determinants of Participation, Low-SES sample

Dep.Var: Medicaid Participation *(mean=0.47)*	I	II	III	IV	V	VI	VII
Measure of Local Climate	Baseline	Generous*Post-Reform: Borjas definition	Generous*Post-Reform: Kaushal and Kaestner definition	Generous*Post-Reform: Zimmerman and Tumlin definition	Cluster Media Coverage of Enforcement	State Anti-Immigrant Sentiment	State Anti-Immigrant Congressional Representation
Mom Non-Cit*Log(Enforcement)	**-0.092***	**-0.113****	**-0.114****	**-0.065***	**-0.081***	**-0.096***	**-0.093****
	(0.035)	(0.026)	(0.038)	(0.030)	(0.032)	(0.036)	(0.030)
Mom Non-Cit*Local Climate		**-0.096**	**-0.045**	**0.041**	**not shown**	**0.010**	**0.222**
		(0.056)	(0.038)	(0.039)		(0.167)	(0.207)
Number of Observations	146,206	146,206	146,206	146,206	146,206	146,206	146,206
R-squared	0.101	0.101	0.101	0.100	0.100	0.100	0.100

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. All regressions include full set of fixed effects and demographic controls as in the previous tables. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster. Definitions of state policy generosity, media coverage, anti-immigrant sentiment, and anti-immigrant congressional representation are described in the text.

Appendix Table 1. Does Enforcement Predict Other Characteristics?

	I	II	III	IV	V	VI	VII	VIII	IX	X
Dependent Variable	Poverty Under 200% FPL	Poverty Under 100% FPL	Mom Married	Mom Spouse Citizen (if Married)	Mom College Grad	Mom Some College	Mom High School Grad	Mom Worked Last Year	Age of Child	Mom Arrived Within Five Years
Panel A. Low-SES Sample										
Mom Non-Cit*Log(Enforcement)	**n/a**	**-0.004**	**0.031**	**-0.006**	**n/a**	**-0.007**	**0.012**	**0.005**	**-0.174**	**-0.003**
		(0.029)	(0.022)	(0.018)		(0.017)	(0.023)	(0.022)	(0.238)	(0.009)
Number of Observations		146,206	146,206	80,038		146,206	146,206	146,206	146,206	146,206
R-squared		0.045	0.093	0.634		0.081	0.078	0.081	0.028	0.200
Panel B. Full Sample										
Mom Non-Cit*Log(Enforcement)	**0.004**	**-0.014**	**-0.025**	**0.019**	**-0.015**	**0.005**	**0.016**	**0.015**	**-0.101**	**0.004**
	(0.023)	(0.024)	(0.018)	(0.015)	(0.015)	(0.015)	(0.015)	(0.027)	(0.176)	(0.009)
Number of Observations	392,941	392,941	392,941	297,360	392,939	392,939	392,939	392,939	392,941	392,941
R-squared	0.095	0.066	0.037	0.563	0.068	0.046	0.037	0.063	0.021	0.192

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. Regressions include citizen*year fixed effects and state*group*citizen fixed effects but not demographic controls. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster.

Appendix Table 2. Differential Responses to Enforcement, Low-SES Sample

Dependent Variable: Medicaid	I	II	III	IV	V	VI	VII	VIII	IX
Characteristic	Child Under 2	Child Under 7	Siblings At Home	Mother Married	Mother Mexican	Mother From High Undocumented Group	Child Non-Citizen	Child in Very Good/Excellent Health	Child in Poor Health
Mother Non-Cit*Log(Enforcement)*Characteristic	**-0.007***	**-0.006***	**0.001**	**-0.009****	**-0.055+**	**-0.114***	**-0.058****	**-0.017****	**0.019**
	(0.003)	(0.002)	(0.008)	(0.002)	(0.027)	(0.044)	(0.013)	(0.003)	(0.013)
Mother Non-Cit*Log(Enforcement)	**-0.091***	**-0.090***	**-0.090***	**-0.093***	**-0.079***	**-0.058+**	**-0.080***	**-0.123****	**-0.135****
	(0.036)	(0.036)	(0.036)	(0.037)	(0.029)	(0.032)	(0.034)	(0.034)	(0.034)
Number of Observations	146,206	146,206	146,206	146,206	146,206	142,739	146,206	114,904	114,904
R-squared	0.226	0.226	0.229	0.226	0.226	0.219	0.227	0.219	0.215

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. All regressions include full set of fixed effects and demographic controls as in the previous tables and the two-way interaction Log(Emforcement)*Characteristic. (One exception is that the coefficient on Kidcit*enforcement is reported rather than the triple interaction.) Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster. Mothers from "High Undocumented Group" are those from countries estimated to have at least 25 percent residing illegaly in the U.S. The countries include Guatemala, Honduras, Mexico, Dominica, Brazil, Colombia, Ecuador, Venezuala, and Kenya. Rates of documentation are unavailable for a small number of country-of-origin groups. Health status analyses use survey years 1996 and later.

Appendix Table 3. Robustness Checks, Low-SES Sample

	I	II	III	IV	V	VI	VII
Dependent Variable: Medicaid							
Change from Preferred Specification	Preferred Specification	Linear Enforcement Measure	Estimated Number of Non-Citizens Rather Than Point-in-Time Measure	Restrict to Moms Arriving Before 1992	Add State-Specific Linear Time Trends*Non-Cit	Add Control for State Unemployment Rate*Non-Cit	Add Control for State New Legal Immigration Rate *Non-Cit
Sample	All	All	All	Mothers Arriving Before 1992	All	All	All
Mom Non-Cit*Log(Enforcement)	**-0.092***		**-0.089***	**-0.089***	**-0.067**	**-0.072+**	**-0.089***
	(0.035)		(0.042)	(0.037)	(0.041)	(0.042)	(0.035)
Mom Non-Cit*Enforcement		**-4.945+**					
		(2.700)					
Number of Observations	146,206	146,206	146,206	138,897	146,206	146,206	146,206
R-squared	0.226	0.226	0.226	0.228	0.227	0.226	0.226

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. All regressions include full set of fixed effects and demographic controls as in the previous table. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster.

Appendix Table 4. Alternative Definitions of Citizenship, Low-SES Sample

Dependent Variable: Medicaid									
Definition of Citizenship	Mom Non-Citizen	Mom's Spouse Non-Citizen	Mom and Spouse Non-Citizen	Mom and Spouse Mixed Citizenship	Any Parent Non-Citizen	Any Parent Non-Citizen	Household Head Non-Citizen	Mom's Spouse Non-Citizen	Mom's Spouse Non-Citizen
Sample	All	Married	Married	Married	Married	All	All	Married and Non-Citizen Mom	Married and Citizen Mom
Definition of Non-Cit*Log(Enforcement)	**-0.099***	**-0.123****	**-0.120****	**-0.085+**	**-0.112****	**-0.105****	**-0.113****	**-0.055**	**-0.156****
	(0.036)	(0.032)	(0.037)	(0.044)	(0.038)	(0.031)	(0.027)	(0.034)	(0.051)
Number of Observations	146,206	80,038	80,038	80,038	80,038	146,206	146,206	19,857	60,181
R-squared	0.101	0.109	0.106	0.108	0.108	0.102	0.102	0.201	0.092

Notes: Linear probability model. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. All samples exclude non-citizen children whose mothers arrived within five years. The Low-SES sample is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. All regressions include full set of fixed effects and demographic controls as in the previous tables, except that citizenship categories for fixed effects are defined as indicated. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster.

Appendix Table 5. Instrumenting for Citizenship (Low-SES Sample)

Dep.Var: Medicaid Participation	**OLS**	**IV**
Mom Non-Cit*Year f.e.	yes	Instrumented
Log(Enforcement)	yes	yes
State*Group f.e.	yes	yes
Mom Non-Cit*Log(Enforcement)	**-0.045****	**-0.101****
	(0.009)	(0.037)
Number of Observations	146206	146206
R-squared	0.071	

Notes: OLS model differs from baseline model in that state-group fixed effects are included rather than state-group-citizen fixed effects. IV model instruments for citizen*year fixed effects and citizen*enforcement using group*year and group*enforcement. Standard errors in parantheses are clustered by INS cluster. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. Sample excludes non-citizen children whose mothers arrived within five years and is restricted to children living below 200 percent of the poverty line and whose mothers have less than a college degree. All regressions include full set of fixed effects but exclude demographic controls. Enforcement is measured as the average of the number of deportable aliens located per non-citizen in the reference year and the year prior to the reference year in the INS cluster.

Appendix Table 6. Simulated Under-Reporting, Low-SES Sample

	I	II	III	IV	V
Dependent Variable: Medicaid					
Simulated Non-Citizen Reporting Rate Under Highest Enforcement	**50%**	**60%**	**70%**	**80%**	**90%**
Definition of Non-Cit*Log(Enforcement)	**-0.035**	**-0.050**	**-0.063+**	**-0.079***	**-0.087***
	(0.032)	(0.032)	(0.031)	(0.033)	(0.035)
Number of Observations	146,206	146,206	146,206	146,206	146,206
R-squared	0.219	0.220	0.221	0.223	0.224

Notes: Baseline specification using simulated data. **, *, and + represent statistical significance at the 1, 5, and 10 percent levels. Reporting rates are assumed to vary linearly up to 100% based on the percentile of the enforcement distribution. Reporting rates for children of citizens are assumed to be 100% and unresponsive to enforcement. See text for more details.