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 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, Yemane, and Adams, 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, Yemane, and Adams, 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 the State Children's Health Insurance Program (SCHIP, see Dubay Haley, and Kenney, 2002). 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).[1] Enrolling eligible children in the Medicaid program has the potential to reduce un-insurance rates and improve child health.

There is also widespread interest in the determinants of program participation more generally. In the wake of the 1996 welfare reform known as PRWORA (the Personal Responsibility and Work Opportunity Reconciliation Act) and the

[1] 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.

associated decline in safety net participation, 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.[2] 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 alone.

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 reductions in immigrant safety net use. 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 between enforcement of immigration law and participation in Medicaid by children of non-citizens, even when the children themselves are eligible 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

[2] The term more generally is used to describe a "discouraging or deterring effect, especially one resulting from a restrictive law or regulation." (Definition downloaded March 6, 2013 from dictionary.com). 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. (See "The Chilling Effect in Constitutional Law," *Columbia Law Review*, Vol. 69(5), 1969.)

immigration law. The results also highlight the importance of seemingly unrelated policy choices in determining take-up of safety net programs.

The next section discusses background on Medicaid take-up and enforcement. Section II describes the enforcement data and trends in enforcement. In Section III, the Medicaid data are discussed. Section IV presents the methodology and results, and Section V concludes.

I. Background

Economists interested in understanding take-up of public programs have emphasized the roles of stigma, information, and program design.[3] 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. Immigrant social networks may matter due to stigma or information flows (see Bertrand, Luttmer, and Mullainathan, 2000, and Gee and Giuntella, 2011). 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).

[3] 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 (Borjas and Hilton, 1996, Bertrand, Luttmer, and Mullainathan, 2000, and Aizer and Currie, 2004). Barriers to participation may be exacerbated if individuals believe they are likely to exceed income limits in the near future. 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.

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.[4] 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.[5] Welfare

[4] 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).

[5] Welfare reform also restricted immigrant eligibility for other safety net programs in ways that differed across states. Exceptions to immigrant restrictions were made for some groups.

Previous literature exploits state variation in implementation of the welfare reform bill. 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. However, they also interpret their results as evidence of "chilling effects," in this case arising from the icy national policy

reform also reduced participation in other programs such as TANF (Temporary Assistance for Needy Families) and SSI (Supplemental Security Income) which could have indirectly affected Medicaid rates disproportionately for immigrants if their initial participation was higher.

Despite the anti-immigrant language of the welfare reform bill, the actual number of immigrants made ineligible for Medicaid by its passage was quite small. Some observers hypothesize that indirect "chilling effects" may have discouraged immigrant participation in public programs for which they remained eligible. Though the existence of "chilling" due to an icy policy climate is plausible, fear and informal dissuasion are difficult to observe. Empirical analyses typically assume that otherwise unexplained declines in participation or take-up of non-citizens are due to chilling effects.[6]

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. Undocumented immigrants are ineligible for non-emergency Medicaid throughout the period, but it is possible that enforcement deters those who would otherwise fraudulently seek benefits. A more likely impact, however, is on the children of immigrants, a majority of

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

[6] 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 Medicaid participation fell for pre-enactment immigrants following welfare reform even though they maintained eligibility. Lurie (2008) finds that insurance coverage for citizen children of non-permanent residents fell more than insurance coverage of citizen children of permanent residents following welfare reform and attributes the effect to chilling.

whom are citizens and therefore eligible if they meet income limits. Enforcement could impact the willingness of undocumented parents to interact with public agencies even though their children are eligible for benefits.

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 (Berk *et al*., 2000). Ethnographic research suggests that government checkpoints and patrols reduce the willingness of undocumented migrants to travel and visit health care providers (Nunez and Heyman, 2007, and Heyman, Nunez, and Talavera, 2009). Undocumented immigrants are also hesitant to access emergency relief following worksite raids (Capps *et al.*, 2007).

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.[7] Of six welfare sites studied in a 2003 report for the Department of

[7]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).

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, applications at two of the six 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 (Holcomb *et al.*, 2003).

While enforcement is particularly likely to affect decisions of undocumented immigrants, it is also possible that legal immigrants could be dissuaded from applying for benefits in a high-enforcement regime. Legal residents might interpret enforcement levels as predictive of "public charge" deportation or predictive of their likely reception at the welfare office.[8] The dataset used in the analysis below reports citizenship status but does not identify legal status for non-citizens.

There has been little previous work examining the link between enforcement and enrollment in public programs.[9] Nevertheless, heightened enforcement of immigration law is known to affect immigrant labor market decisions, suggesting that immigrants are aware of and respond to enforcement activity.[10]

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[8] A long-standing but seldom used doctrine suggests that immigrants deemed a "public charge" could be deported or denied future citizenship (see Schlosberg and Wiley, 1998, and National Immigration Law Center, 2009). This doctrine was highlighted in the welfare reform legislation of 1996.

[9] One exception is unpublished work by Vargas (2010) who explores the effect of fear of deportation on WIC (the Woman, Infants, and Children nutrition program) and SCHIP participation for immigrants in mixed status families.

[10] For example, see Davila and Pagan (1997), Bansak (2005), and Orrenius and Zavodny (2009).

To investigate the interactions between program participation and enforcement of immigration law, I exploit spatial and temporal variation in enforcement action between 1992 and 2002. The increase in immigration enforcement in the 1990s varied substantially across the 33 INS administrative districts. In the next section, I discuss the patterns of enforcement and factors driving variation across areas and over time.

II. Enforcement and Enforcement Data

Immigration enforcement data were obtained from INS Yearbooks of Immigration Statistics and 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.[11] "Deportable aliens located" is the INS term for apprehensions.

Figure 1 shows trends in enforcement over time, using the measure of enforcement activity described below. There is a sharp increase in enforcement in the mid-1990s, presumably due to the sharply increasing INS budget and manpower.[12] The Illegal Immigration Reform and Immigrant Responsibility Act

[11] Data on deportable aliens located by district and year are publicly available in the Yearbooks for the years included in the study. Country-of-origin detail was obtained via Freedom of Information Act request to allow an investigation of whether immigrants are more responsive to local enforcement within their own country-of-origin or language group. I did not find evidence that this was the case (See Watson, 2010) and those results are not included here. Border enforcement activities are not considered because they are less likely to affect resident immigrants and because the geographic distribution of the impact is unclear.

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

of 1996 increased enforcement expenditures and gave the INS expanded authority to locate and remove undocumented immigrants. The number of internal "deportable aliens located" – apprehensions - 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.[13] To construct an indicator of enforcement activity, I start with the number of deportable aliens located in a given fiscal year in a cluster divided by the estimated number of non-citizens in the cluster in 1995.[14] I then average this number over two years (the year prior to and the year of the Medicaid decision) and take the log as the indicator of enforcement activity.[15] Figure 2 reports the

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budget increased 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.)

[13] 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.

[14] I estimate the number of non-citizens using IPUMS Census data (Ruggles *et al.*, 2010) for 1990 and 2000, in which citizenship status (though not legal status) is reported for all respondents. These numbers are aggregated by INS cluster and year, and the average of these two numbers is the estimated non-citizen population for 1995. As a robustness check, below I try using a time-varying number of non-citizens in the denominator based on linear interpolation between years. This does not substantively affect the results.

[15] The analysis using the log form of the enforcement variable assumes the effect of a doubling of enforcement is the same regardless of the initial level of enforcement. There are no cluster-years with zero deportable aliens located, so the choice of functional form does not affect the number of observations. Given the relatively few papers that investigate enforcement theoretically or empirically,

raw data - the number of deportable aliens located per non-citizen by fiscal year - for 7 of the 25 INS clusters in the data. Some areas, such as Arizona-Nevada, experienced sharp increases in arrests while others, such as California, saw more modest changes. Figure 3 plots enforcement and Medicaid participation rates among children of non-citizens for these two areas.

It is important to understand what drives variation in enforcement within a district over time. 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. New immigrants are also less likely to apply for safety net programs, so it is important to account for this potential source of bias. Second, though enforcement is implemented by Federal authorities, local attitudes toward immigration could indirectly influence the actions of the district managers. This factor could bias the estimated effect of Federal enforcement if local attitudes are related to local Medicaid policy or immigrant characteristics. As discussed below,

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the appropriate choice of functional form is not obvious *a priori*. However, one would expect that each unit increase in enforcement will have a diminished effect after a certain point once all undocumented residents have been deterred in enrolling.

Results using levels rather than logs are reported in Table 10, column II. The magnitude reported there suggests that raising enforcement from zero to its empirically observed highest level of 0.054 would reduce Medicaid participation by about 25 percentage points among children of non-citizens on a base of 45 percent. Given that a significant fraction of non-citizens are documented and therefore presumably less sensitive to enforcement, further reductions would be unlikely with increased enforcement.

Investigation using a quadratic specification (not shown) suggests a significant negative coefficient on the linear term and a significant positive coefficient on the squared term. The inflection point is at the 98th percentile of the enforcement distribution. In sum, it appears that the relationship between enforcement and Medicaid participation is approximately linear over much of the data range but diminishes at the highest levels of enforcement.

I use a number of controls and robustness checks to reduce the threat of bias from new immigration and local attitudes.

Resources available for enforcement activity also have an important impact on the number of apprehensions. Substantial 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 (see Center for Equal Opportunity, 1995, General Accounting Office, 1999, and Siskin *et al.*, 2006). Davila, Pagan, and Grau (1999) suggest that the agency seeks to maximize total apprehensions rather than minimize the number of undocumented immigrants.

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

Idiosyncratic preferences of district managers combined with aggregate budget fluctuations are likely important determinants in the degree of immigration enforcement within districts over time. The key identifying assumption of the empirical strategy described below is that, after controlling for a wide range of potential confounding factors, variation in enforcement stems from sources that are not directly related to differential Medicaid participation among children of non-citizens.

III. Medicaid 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.[16] 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.[17] In the years following the introduction of the State Children's

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

[17] Some individuals may respond with their current coverage status which would generate error in the outcome variable.

Health Insurance Program (SCHIP), children participating in the SCHIP program are coded as participating in Medicaid. Citizenship status and country of origin of each household member are available starting in the 1994 survey.

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.[18] In addition to reducing contamination from PWRORA, an advantage to excluding this group is that it mitigates bias coming from new immigrant inflows; such inflows are likely to be associated with both increased enforcement and lower participation rates. The primary analysis is based on a low-socioeconomic-status (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, a below-poverty sample, and additional alternative samples.

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.[19] Under-reporting of program participation

[18] Low-SES newly arrived children are more likely to be poor than the main sample (poverty rates of 58 rather than 47 percent) and less likely to participate in Medicaid (34 percent versus 47 percent). As shown in Table 4, the inclusion of these 2,772 children does not substantively affect the results.

[19] Using mother's status allows one to pool mother-only and two-parent families. Alternative methods of assigning child's status are discussed 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 reported results are

is an important limitation of these data.[20] Of particular concern is the potential that under-reporting behavior is responsive to enforcement; I discuss the implications of endogenous under-reporting below.

As is common in the literature, I use the data available in the CPS to impute each child's Medicaid eligibility for analyses of take-up.[21] 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.[22] One of these exploits Medicaid income

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conservative. Children who are themselves non-citizens appear to be more responsive to enforcement than other children of non-citizens.

[20] 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, Mok, and Sullivan 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. In addition, respondents may report their current Medicaid status rather than their status last year as requested in the survey. In this case the lag structure of the effect of enforcement is slightly misstated.

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.

To further evaluate under-reporting, one can compare published administrative total Medicaid enrollment to that implied by CPS reporting across the March samples (including adults) for 1994-2003. This exercise suggests that the CPS does capture state variation and within-state variation over time in Medicaid participation (results not shown). The reporting rate implied by this exercise is 71 percent.

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

[22] Because recipients of AFDC/TANF (cash welfare) are typically enrolled in Medicaid, the first eligibility measure incorporates imputed AFDC/TANF

eligibility thresholds and the other additionally incorporates children who may be eligible because they are eligible for cash welfare. I also show results for a sample of children under the poverty line; almost all such children are income-eligible for Medicaid.

Table 1 shows 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. Table 1 also reports summary statistics of variables on state welfare policy and local attitudes about immigration; these data are described below.

<TABLE 1 ABOUT HERE>

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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.

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

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 patterns in the data, I start by considering a sample of children of non-citizens only with a limited set of controls. The preliminary linear probability model is:

$$(1) \qquad Medicaid_{ict} = \beta_0 + \beta_1 \log(enforce_{ct}) + \theta_c * year + \Phi_c + \delta_t + \mu_{ict}$$

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 to capture smoothly evolving changes in the area that could impact Medicaid participation, Φ_c represent cluster fixed effects to account for permanent differences in participation across areas, and time fixed effects λ_t control for annual shocks that affect all non-citizens nationally. Though this is a relatively simple specification, it does account for many potential sources of unobserved heterogeneity; these include differences in levels or trends in stigma, outreach, bureaucratic barriers to enrolling in programs, or other factors that could impact participation.

Standard errors are clustered by INS cluster to account for common shocks in a given local area, and statistical significance is evaluated using a T distribution with 25 degrees of freedom, as suggested by Cameron, Gelbach, and Miller (2008) to account for the fact that there are fewer than 30 clusters.[23]

[23] Cameron, Gelbach, and Miller (2008) also suggest a modification to the standard errors. In this case the adjustment is very small (a 2% increase) and has not been performed. Bootstrapped standard errors are reported in the final column of Table 10.

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. A log-point increase is roughly the equivalent of moving from the 30th percentile to the 70th percentile of the observed enforcement distribution, or roughly the equivalent of moving from the average enforcement in 1994 to the average enforcement in 2000.

<TABLE 2 ABOUT HERE>

It is also evident from Table 2 that there is no comparable effect on children of 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:

$$Medicaid_{icsgt} = \beta_0 + \beta_1 \log(enforce_{ct}) * noncit_i + \beta_2 \log(enforce_{ct}) + \Omega_{csg} + \Omega_{csg} * noncit_i + \lambda_t + \lambda_t * noncit_i + \theta_c * year + X_i B_3 + \mu_{icsgt} \quad (2)$$

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 fixed effects Ω_{csg} alone and interacted with non-citizen status to capture permanent state differences facing children of non-citizens of a particular country-of-origin group,[24] and year dummies λ_t alone and interacted with $noncit_i$ to account for annual changes in non-citizen participation nationally. Cluster-specific time trends are included to account for smoothly evolving changes in population characteristics or policy parameters. Overall, this large set of fixed effects is intended to capture unobserved heterogeneity across time and place that could affect Medicaid participation.

Demographic controls X_i include child age*year fixed effects, mother's education, mother's marital status, indicators for whether the family lies below 100 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.[25] 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.

Standard errors are clustered on INS cluster to account for common shocks within a cluster and serially correlated shocks over time. As described above, a T-distribution with 25 degrees of freedom is used for hypothesis testing because there are 25 clusters in the data.

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[24] 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.

[25] Time of arrival controls are included because connections to social services rise with time in the United States.

Table 3 shows the main results for the low-SES sample with different sets of controls and sub-samples. The preferred baseline specification (second column of Table III) shows that one log point increase in enforcement efforts differentially reduces Medicaid participation by children of non-citizens by 10.1 percentage points. As noted above, a log-point increase is a relevant benchmark; it is roughly equivalent to the increase in enforcement between 1994 (46 per 10,000 non-citizens arrested) and 2000 (98 per 10,000). As shown in the second row of column II, there is no significant effect of enforcement on the omitted group – children of citizens.

<TABLE 3 ABOUT HERE>

One can also restrict to citizen children, children whose mother's arrived more than five years ago, or both. 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.

Table 4 examines the effect of enforcement for alternative samples. If the sample is restricted to children living below the poverty line, the point estimate is larger at -0.134. That is, a log point increase in enforcement reduces Medicaid participation by 13.4 percentage points for poor children. On the other hand, the effect across the full income distribution is smaller at -0.052, but the estimated impact is statistically significant even including higher-income families.[26]

[26] Though children living above 200 percent of the poverty line are much less likely to enroll in Medicaid, the full sample results account for the possibility of an endogenous response of income to enforcement. There are also a number of

<TABLE 4 ABOUT HERE>

I also try restricting the sample to those imputed to be eligible for Medicaid. Eligibility is imputed in two ways. The first incorporates the AFDC/TANF (cash welfare) pathway and eligibility arising due to Medicaid expansions. The second imputation ignores the AFDC/TANF eligibility pathway.[27] The standard errors are larger for these estimates, perhaps because of measurement error in the imputation algorithm. Nevertheless, the estimated coefficients are of similar magnitude to the baseline and retain statistical significance.

Finally, I evaluate robustness by considering two additional samples. Column VI includes newly-arrived non-citizen children in the estimation sample. The results are not affected very much. Column VII restricts the sample to mothers living in the United States since before the sample start date of 1992. The fact that the estimates are similar suggests that recent migration patterns are not driving the results; it appears that long-standing immigrant families are affected by enforcement.

In Table 5, 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, mother's time since arrival, mother's Mexican origin, the rate of undocumented

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states with SCHIP income eligibility limits above 200 percent of the poverty line in the later years of the sample.

[27] Aid to Families with Dependent Children (AFDC) is the cash welfare program in place before welfare reform in 1996. The new program is known as Temporary Assistance to Needy Families (TANF). Because Medicaid eligibility expansions are not the central focus of the paper, I do not attempt to simulate exogenous eligibility rates or fully model the complex eligibility rules. See Watson (2010) for details.

status in mother's country-of-origin group, and migration. Of the eleven variables considered, there are two associations that are significant at the 10 percent level. Enforcement is correlated with maternal marital status and work behavior in the low-SES sample. Marital status is included throughout the analysis as a baseline control. Maternal work status is not included in the baseline due to endogeneity concerns, but column IX of Table 10 demonstrates that controlling for it does not substantively affect the results. Variables that are not related to enforcement include poverty status, citizenship of mother's spouse, maternal education, age of child, arrival time of mother, Mexican origin of mother, undocumented rate in country of origin group, and whether the mother moved states last year. There is no correlation of any of these variables with enforcement in the full sample. The absence of correlation between most observable variables and enforcement allays concerns about endogenous internal migration and spurious correlation between immigrant characteristics and enforcement policies.

<TABLE 5 ABOUT HERE>

C. Who Responds to Enforcement?

In Table 6, I use a triple interaction approach to explore the responsiveness of different sub-groups to enforcement policy. For example, one might suspect that country-of-origin groups with many undocumented migrants are likely to respond more dramatically to enforcement efforts. The share of undocumented residents differs substantially across country-of-origin groups (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 origin immigrants have the highest proportion undocumented at roughly 52 percent. Children of mothers born in Mexico do appear to respond 5.9 percentage points more than other children to enforcement

efforts, as shown in the first column of Table 6. I also examine mothers from countries with at least 25 percent residents estimated to be undocumented.[28] The main 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 II. Column III of Table 6 indicates that non-citizen children (who may or may not be undocumented) are more responsive to enforcement than other children of non-citizens.

<TABLE 6 ABOUT HERE.>

The final columns of Table 6 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.

Further analysis (not shown) suggests that there is a greater impact of enforcement in metropolitan areas with many non-citizens. This may arise because enforcement per non-citizen is disproportionately located in these areas, because 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 areas with many non-citizens.

D. Insurance Status, Health, and Program Participation

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

Table 7 presents the relationship between enforcement and overall insurance status. The effect of enforcement on public health insurance is almost as large as the effect on Medicaid. This suggests that most immigrants deterred from Medicaid/SCHIP due to enforcement do not enroll in alternative public health insurance programs.[29] There is a 7.9 percentage point reduction in participation in public health insurance only, and an additional reduction of 1.3 percentage points in those covered by both public and private insurance for some part of the year. The reduction in un-insurance of 5 percentage points is only half of the reduction in Medicaid participation.

<TABLE 7 ABOUT HERE.>

Private health insurance increases by a statistically insignificant 4.2 percentage points in response to enforcement for the low-SES sample. The point estimates suggest that a 10 percentage point increase in public insurance participation (due to absence of enforcement) crowds out 4.6-5.9 percentage points of private insurance for the low-SES sample.[30] Though estimates of crowd-out in the previous literature are wide-ranging, Gruber and Simon (2008) review the literature and find crowd-out rates averaging around 0.6. Nevertheless, the estimated effect of enforcement on private insurance is not statistically

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

[30] The exact crowd-out ratio depends on how one treats children reporting both private and public coverage. Following Gruber and Simon (2008), I calculate on measure of crowd-out which ignores the overlap population and divides the change in private only by the change in public only. The implied crowd out ratio is 0.53. Alternatively, treating the overlap population as moving from "private only" to "public only" in the absence of enforcement yields a crowd-out ratio of 0.59. Using a formula of one minus the change in uninsured divided by the change in publicly insured as in Shore-Sheppard (2008) yields a ratio of 0.46.

distinguishable from zero. The analysis presented here has standard errors too large to generate a meaningful crowd-out estimate.

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. Though estimates are imprecise, the analysis shown in Table 8 suggests that higher enforcement is associated with children moving out of the "very good" health category and into the "poor" health category on a five point scale.[31] 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 8 ABOUT HERE.>

One can also analyze the effect of enforcement on other poverty programs (results not shown). The impact on receipt of cash assistance (AFDC/TANF) is small and statistically insignificant. On the other hand, there is evidence that Food Stamp participation does respond to enforcement for children below the poverty line. 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

[31] 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. See Watson (2010).

explanation. 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 limit the ability to observe a response. The differences across programs suggest that enforcement can interact with program design to influence participation.[32]

E. State Policy Climate and Local Attitudes

The results presented above could be biased if enforcement activity is correlated with state level policy affecting program generosity towards immigrants following welfare reform. I use three definitions of generosity to investigate this possibility. 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.[33] Kaushal and Kaestner (2005) offer a simpler definition, describing a state as "generous" if it offered TANF or Medicaid to post-enactment immigrants. As a third alternative, I describe states as generous if Zimmerman and Tumlin (1999) categorize immigrant safety net programs in the state as most

[32] See Watson (2010) for details. 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 for a very small effect on DI in the under 100% of poverty sample.

[33] 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. The Borjas definition includes the six largest immigrant states; 89 percent of children of non-citizens in the sample live in a generous state according to the Borjas definition. The six states with the highest numbers of immigrants are California, Florida, Illinois, New York, New Jersey, and Texas.

available or somewhat available.[34] 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.[35]

Table 9 shows the effect of state policy climate.[36] Both the Borjas and the Kaushal and Kaestner definitions of generosity show a negative (wrong-signed) and insignificant effect of state generosity on participation for children of non-citizens. 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 -0.069 when the Zimmerman and Tumlin measure of generosity is included, but the coefficient maintains statistical significance at the 10 percent level.

<TABLE 9 ABOUT HERE.>

Another potential confounder is local attitudes toward immigration, which could indirectly influence enforcement activity and directly influence the Medicaid participation decision. Such a response might reflect "chilling" but would not be a direct impact of Federal enforcement activity (Seghetti, Vina, and Ester, 2004).

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[34] Under the Kaushal and Kaestner 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. For Zimmerman and Tumlin, 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.

[35] State-specific welfare reform dates are taken from Bitler, Gelbach, and Hoynes (2005). Results are similar if one uses 1997 as the reform year in all states.

[36] 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.

Though local attitudes are difficult to measure, I collect information on media coverage of immigration enforcement, survey responses to immigration questions for state residents, and immigration stances of Congressional representatives. Inclusion of these variables does not impact the main results.

F. Robustness

Table 10 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 reported effect is of the same order of magnitude as that implied by the log specification evaluated at the sample mean. The estimated marginal effect using the probit model (column III) is also similar. In the fourth column of Table 10, the denominator in the enforcement variable 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.

<TABLE 10 ABOUT HERE.>

The fifth column drops the cluster specific linear time trend, which again has little impact on the result. The sixth column incorporates state-by-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 sixth and seventh columns, the enforcement results are weakened *only* when the new variables and a full set of demographic

controls are also included, suggesting that the analysis may be limited by statistical power issues (the analyses without controls are not shown).

The eighth column of Table 10 controls for the effect of new legal immigration. This variable serves as a proxy for new immigration of undocumented immigrants which could correlate with Medicaid participation; inclusion of this variable does not alter the results very much. Similarly, controlling for maternal employment status (column IX) or Medicaid managed care penetration (column X) does not affect the results.

In the final column of Table 10, I try bootstrapping the standard errors. Clustered standard errors can lead to over-rejection when there are fewer than 30 clusters Cameron, Gelbach, and Miller (2008). As noted above, hypothesis testing throughout has been performed using a distribution with 25 degrees of freedom. Column XI performs the additional step of constructing standard errors using a cluster bootstrap with 1000 replications. The standard error does increase slightly from 0.038 to 0.043, but the estimated coefficient is still significant at the five percent level. Using a bootstrap-t procedure for hypothesis testing (not shown) also confirms statistical significance at the 5% level.

I perform an additional set of robustness checks not shown in the table. For example, I 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.[37]

The baseline analysis uses the mother's citizenship status to predict Medicaid participation. Results (not shown) 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. 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.

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.[38] To investigate the possibility of endogenous citizenship, I 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.[39] To further investigate the issue

[37] Similar results occur (coefficient of -0.05 with a p-value of 0.13) when removing the entire cluster of Texas-Oklahoma-New Mexico. Results are significant at the 5% level and in the -0.084 to -0.122 range when each of the other clusters is removed one at a time from the analysis.

[38] Van Hook (2003) notes that the number of naturalizations was 240,000 in 1992 and peaked in 1996 at over one million. She argues that the changing composition of citizenship may explain up to half of the decline in non-citizen welfare participation following welfare reform.

[39] Results not shown; see Watson (2010). 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

of endogenous citizenship, I instrument for mother's citizenship using her country of origin. The results (not shown) 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 percent 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 percent and unresponsive to enforcement. I find that the results are robust to reporting rates of 70, 80, or 90 percent under the highest enforcement. As an additional test, I compare Medicaid reporting in full sample in the CPS and published administrative Medicaid participation totals for each state and year and find no evidence that reporting rates are lower in periods of higher enforcement (analysis not shown).

It is also reassuring that the 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.

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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.

G. 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. That is, I use the estimated coefficients to identify predicted Medicaid participation when replacing the observed level of enforcement with the initial level of enforcement.

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 the remaining 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.[40]

V. Conclusion

Previous analyses have found that program participation decisions respond to policy changes in ways that extend beyond what would be expected based on the

[40] 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 SCHIP program, which most states adopted in 1998, may help explain rising participation rates after the 1999 survey year. Buchmueller, LoSasso, and Wong (2008) document that 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.

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)

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 previously unrecognized 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.

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.

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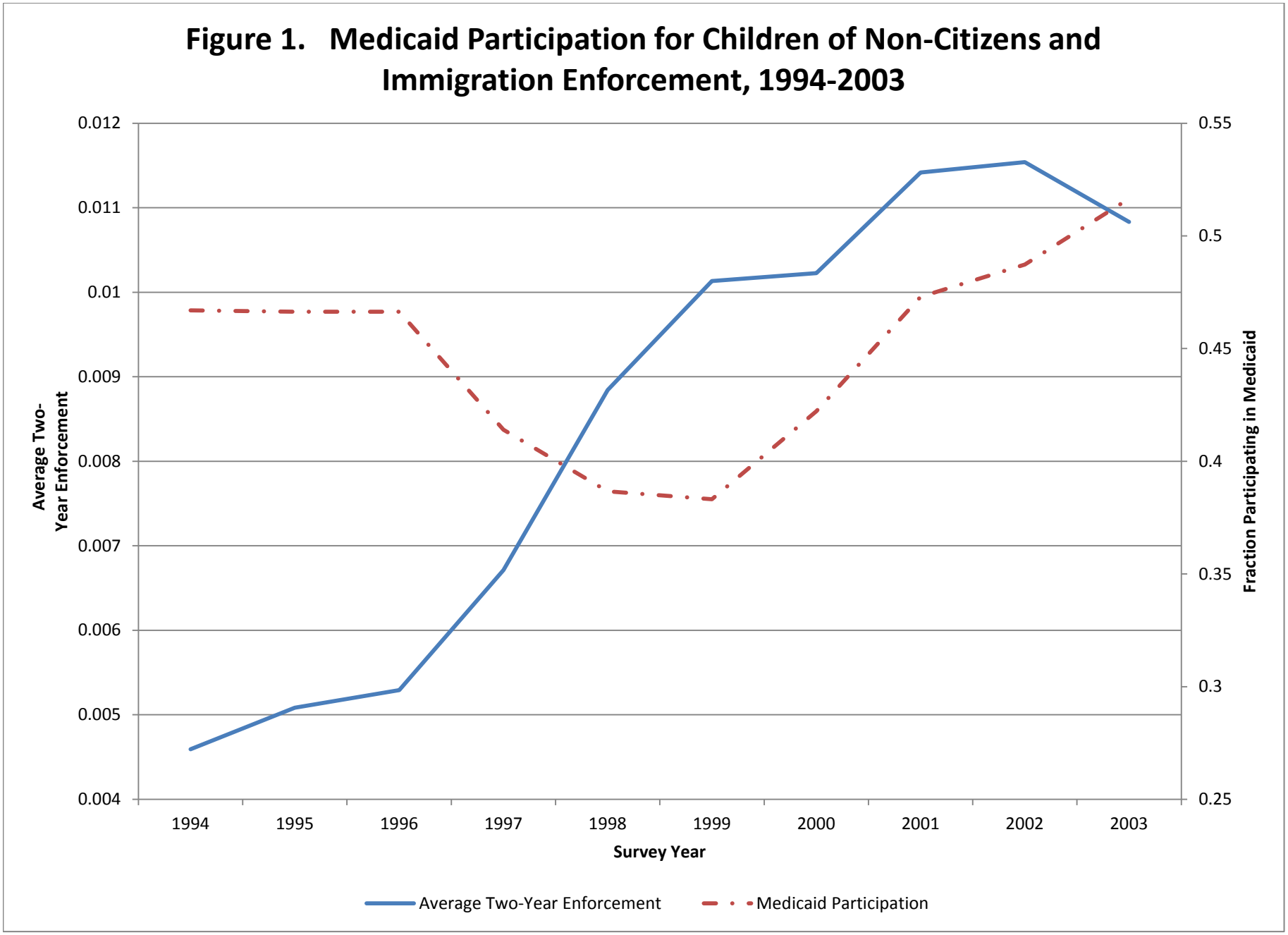
Figure 1. Medicaid Participation for Children of Non-Citizens and Immigration Enforcement, 1994-2003
0.012
0.011
0.01
0.009
0.008
0.007
0.006
0.005
0.004
Average Two-Year Enforcement
0.55
0.5
0.45
0.4
0.35
0.3
0.25
Fraction Participating in Medicaid
1994
1995
1996
1997
1998
1999
2000
2001
2002
2003
Survey Year
Average Two-Year Enforcement
Medicaid Participation

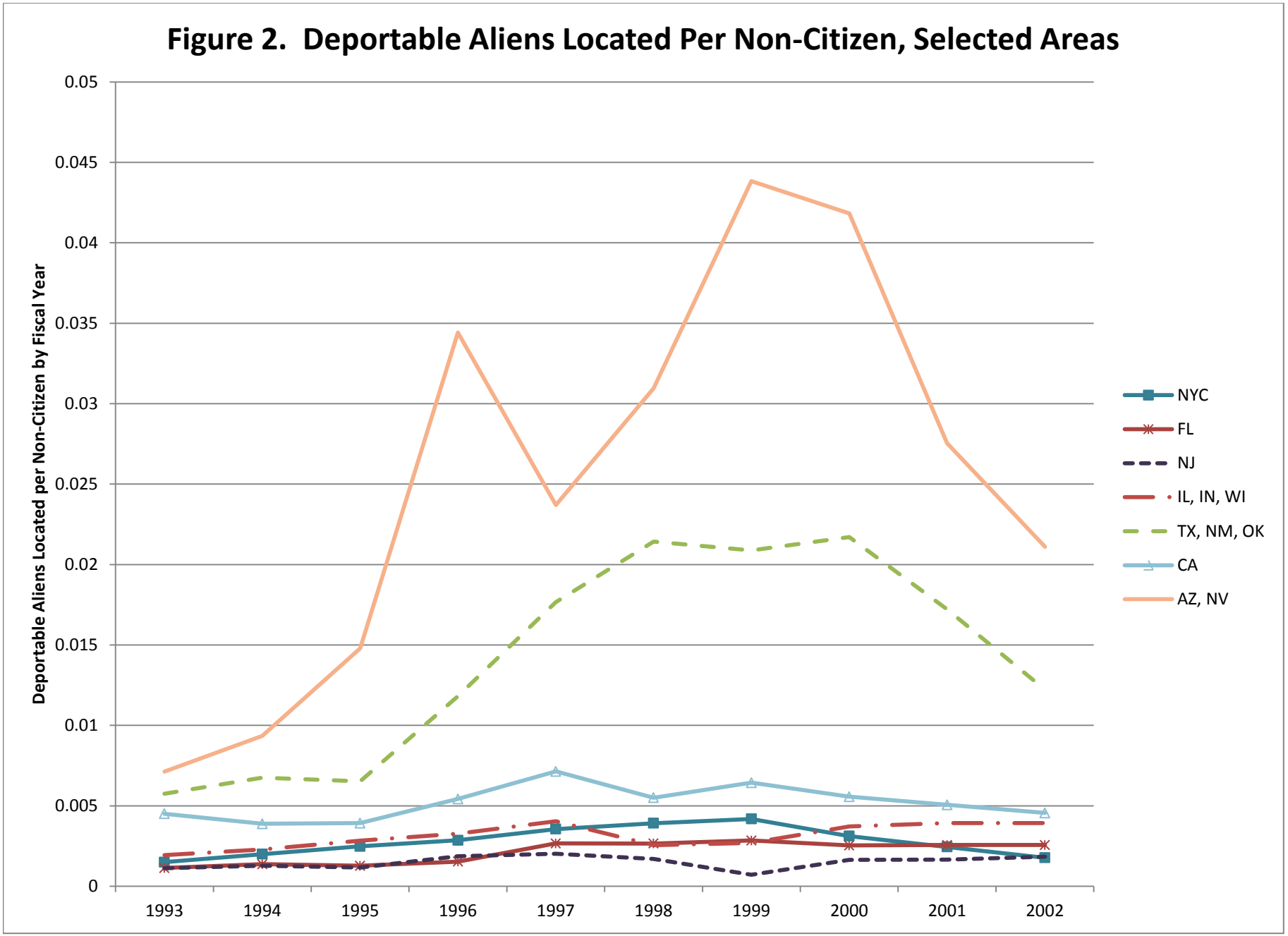
Figure 2. Deportable Aliens Located Per Non-Citizen, Selected Areas
Deportable Aliens Located per Non-Citizen by Fiscal Year
0.05
0.045
0.04
0.035
0.03
0.025
0.02
0.015
0.01
0.005
0
1993
1994
1995
1996
1997
1998
1999
2000
2001
2002
NYC
FL
NJ
IL, IN, WI
TX, NM, OK
CA
AZ, NV

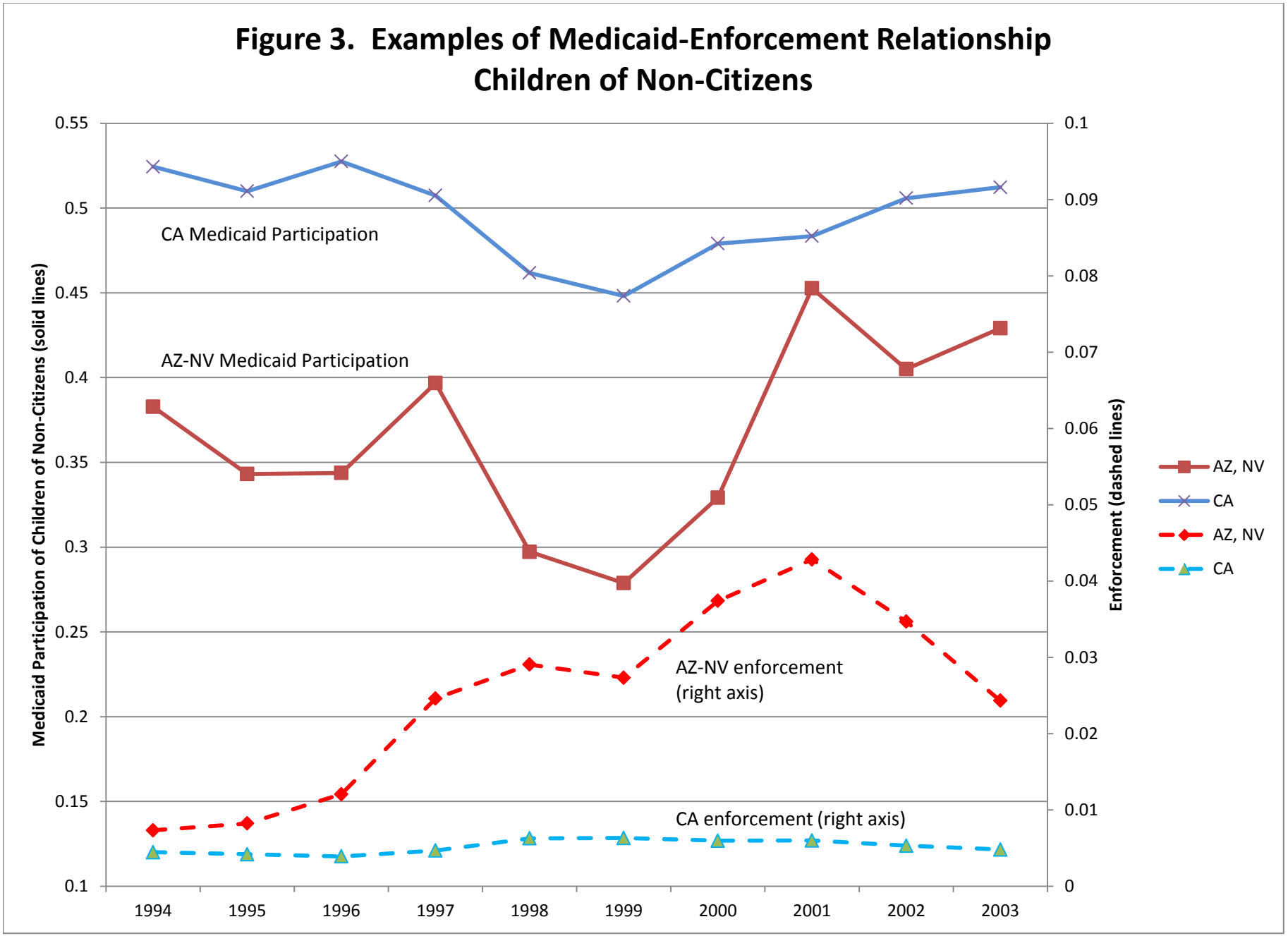
Figure 3. Examples of Medicaid-Enforcement Relationship
Children of Non-Citizens
Medicaid Participation of Children of Non-Citizens (solid lines)
Enforcement (dashed lines)
0.55
0.5
0.45
0.4
0.35
0.3
0.25
0.2
0.15
0.1
0.1
0.09
0.08
0.07
0.06
0.05
0.04
0.03
0.02
0.01
0
1994
1995
1996
1997
1998
1999
2000
2001
2002
2003
CA Medicaid Participation
AZ-NV Medicaid Participation
AZ-NV enforcement
(right axis)
CA enforcement (right axis)
AZ, NV
CA
AZ, NV
CA

Table 1. Means of 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	0.76	0.73	0.54	0.31	0.29
Medicaid Eligibile (Definition 2)	0.69	0.68	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

Dependent Variable: Medicaid Participation	I	II	III	IV
Sample	**Mother Non-Citizen**	**Mother Non-Citizen**	**Mother Citizen**	**Mother Citizen**
	(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)
Cluster f.e.	yes	yes	yes	yes
Year f.e.	yes	yes	yes	yes
Cluster-specific time trends	yes	yes	yes	yes
Number of Observations	26,942	23,528	119,264	102,790
R-squared	0.042	0.041	0.016	0.016

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. Linear probability model. Standard errors in parentheses are clustered by INS cluster. Signficance evaluated using a T distribution with 25 d.f. 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 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. The sample differs across columns because leads cannot be calculated for 2002 and 2003.

Table 3. Effect of Immigration Enforcement on Medicaid Participation, Low SES sample

Dependent Variable: Medicaid Participation	I	II	III	IV	V	VI
Sub-Sample	**All**	**All**	**Kid Citizen**	**Mom Arrived > 5 Years**	**Kid Citizen and Mom Arrived> 5 years**	**Mom Foreign Born**
	(mean=0.47)	*(mean=0.47)*	*(mean=0.47)*	*(mean=0.47)*	*(mean=0.47)*	*(mean=0.44)*
Mom Non-Cit*Log(Enforcement)	**-0.128*** **	**-0.101** **	**-0.110** **	**-0.104** **	**-0.115** **	**-0.114** **
	(0.035)	(0.038)	(0.046)	(0.037)	(0.045)	(0.051)
Log(Enforcement)	**0.016**	**0.012**	**0.011**	**0.011**	**0.010**	**0.086**
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.064)
Mom Non-Cit*State f.e.	yes					
Mom Non-Cit*Year f.e.	yes	yes	yes	yes	yes	yes
Cluster-Specific Time Trends	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
Number of Observations	146,206	146,206	140,587	143,599	137,980	33,920
R-squared	0.026	0.227	0.228	0.226	0.228	0.260

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. Linear probability model. Standard errors in parentheses are clustered by INS cluster. Signficance evaluated using a T distribution with 25 d.f. 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, an indicator for being below 100 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, an indicator for the mother being currently married, and the state unemployment rate.

Table 4. Effect of Immigration Enforcement on Medicaid Participation, Alternative Samples

Dependent Variable: Medicaid Participation	I	II	III	IV	V	VI	VII
Sample	**Low SES (Baseline)**	**Under Poverty Line**	**All Income Levels**	**Medicaid Eligible (Definition 1)**	**Medicaid Eligible (Definition 2)**	**Low SES including newly arrived non-citizen children**	**Mothers Arriving Before 1992**
	(mean=0.47)	*(mean=0.65)*	*(mean=0.21)*	*(mean=0.52)*	*(mean=0.51)*	*(mean=0.46)*	*(mean=0.47)*
Mom Non-Cit*Log(Enforcement)	**-0.101****	**-0.134*****	**-0.052****	**-0.095****	**-0.090****	**-0.091****	**-0.099****
	(0.038)	(0.037)	(0.023)	(0.046)	(0.042)	(0.035)	(0.040)
Log(Enforcement)	**0.012**	**0.028**	**0.002**	**0.014**	**0.009**	**0.015**	**0.010**
	(0.020)	(0.031)	(0.011)	(0.022)	(0.025)	(0.020)	(0.020)
Mom Non-Cit*Year f.e.	yes	yes	yes	yes	yes	yes	yes
Cluster-Specific Time Trends	yes	yes	yes	yes	yes	yes	yes
State*Group*Mom Non-Cit f.e.	yes	yes	yes	yes	yes	yes	yes
Demographic Controls	yes	yes	yes	yes	yes	yes	yes
Number of Observations	146,206	69,570	392,939	126,893	112,452	148,978	138,897
R-squared	0.227	0.158	0.359	0.209	0.212	0.227	0.229

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. Linear probability model. Standard errors in parentheses are clustered by INS cluster. Signficance evaluated using a T distribution with 25 d.f. All samples exclude non-citizen children whose mothers arrived within five years except for the final column. 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 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, an indicator for the mother being currently married, and the state unemployment rate.

Table 5. Does Enforcement Predict Other Characteristics? Low SES Sample

Panel A.

	I	II	III	IV	V	VI
Dependent Variable	Poverty Under 100% FPL *(mean=0.47)*	Mom Married *(mean=0.55)*	Mom Spouse Citizen (if Married) *(mean=0.77)*	Mom Some College *(mean=0.26)*	Mom High School Grad *(mean=0.41)*	Mom Worked Last Year *(mean=0.59)*
Mom Non-Cit*Log(Enforcement)	**-0.021**	**0.061****	**-0.003**	**-0.009**	**0.019**	**0.043***
	(0.030)	(0.027)	(0.020)	(0.016)	(0.022)	(0.025)
Number of Observations	146,206	146,206	80,038	146,206	146,206	146,206
R-squared	0.046	0.094	0.635	0.081	0.078	0.082

Panel B.

	VII	VIII	IX	X	XI
Dependent Variable	Age of Child *(mean=7.9)*	Mom Arrived Within Five Years *(mean=0.02)*	Mom Mexican *(mean=0.12)*	Fraction Undocu-mented in Group *(mean=0.08)*	Moved State Last Year *(mean=0.04)*
Mom Non-Cit*Log(Enforcement)	**-0.041**	**-0.007**	**-0.028**	**0.007**	**-0.009**
	(0.211)	(0.012)	(0.037)	(0.013)	(0.020)
Number of Observations	146,206	146,206	146,206	142,739	146,206
R-squared	0.028	0.200	0.595	0.731	0.029

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. Linear probability model. Standard errors in parentheses are clustered by INS cluster. Signficance evaluated using a T distribution with 25 d.f. Regression models include mother citizen*year effects, cluster-specific time trends, and state*group*mother non-citizen effects, but no other demographic controls.

Table 6. Differential Responses to Enforcement, Low-SES Sample

Dependent Variable: Medicaid (mean=0.47)	I	II	III	IV	V
Characteristic	Mother Mexican	Mother From High Undocumented Group	Child Non-Citizen	Child in Very Good/Excellent Health	Child in Fair/Poor Health
Mother Non-Cit*Log(Enforcement)*Characteristic	**-0.059****	**-0.122****	**-0.057*****	**-0.017*****	**0.014****
	(0.029)	(0.048)	(0.013)	(0.003)	(0.005)
Mother Non-Cit*Log(Enforcement)	**-0.088****	**-0.067***	**-0.089****	**-0.113*****	**-0.127*****
	(0.032)	(0.035)	(0.037)	(0.035)	(0.034)
Number of Observations	146,206	142,739	146,206	114,904	114,904
R-squared	0.227	0.220	0.222	0.221	0.220

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. Linear probability model. Standard errors in parentheses are clustered by INS cluster. Signficance evaluated using a T distribution with 25 d.f. 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(Enforcement)*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 Brazil, Colombia, Dominica, Ecuador, Guatemala, Honduras, Kenya, Mexico, and Venezuala. Rates of documentation are unavailable for a small number of country-of-origin groups; individuals born in those countries are excluded. Health status analyses use survey years 1996 and later.

Table 7. Effect of Immigration Enforcement on Health Insurance Status, Low SES Sample

	I	II	III	IV	V	VI
Dependent Variable	**Medicaid**	**Any Public Health Insurance**	**Public Health Insurance Only**	**Private Health Insurance Only**	**Both Private and Public Insurance**	**Any Health Insurance**
	(mean=0.47)	*(mean=0.49)*	*(mean=0.41)*	*(mean=0.31)*	*(mean=0.08)*	*(mean=0.81)*
Mom Non-Cit*Log(Enforcement)	**-0.101****	**-0.092****	**-0.079****	**0.042**	**-0.013**	**-0.050****
	(0.038)	(0.034)	(0.030)	(0.028)	(0.014)	(0.024)
Number of Observations	146,206	146,206	146,206	146,206	146,206	146,206
R-squared	0.227	0.205	0.218	0.203	0.044	0.085

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. Linear probability model. Standard errors in parentheses are clustered by INS cluster. Signficance evaluated using a T distribution with 25 d.f. 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.

Table 8. Effect of Immigration Enforcement on Parent-Reported Child Health, Low SES Sample

	I	II	III	IV	V
Dependent Variable	**Excellent Health**	**Very Good Health**	**Good Health**	**Fair Health**	**Poor Health**
	(mean=0.398)	*(mean=0.319)*	*(mean=0.240)*	*(mean=0.036)*	*(mean=0.007)*
Mom Non-Cit*Log(Enforcement)	**0.028**	**-0.083***	**0.054**	**-0.008**	**0.009*** **
	(0.022)	(0.043)	(0.032)	(0.013)	(0.003)
Number of Observations	114,904	114,904	114,904	114,904	114,904
R-squared	0.052	0.034	0.048	0.024	0.021

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. See notes from previous tables. Regressions similar to Table 3 column II except that the dependent variable is parent-reported health status on a five point scale. Health status is recorded starting in 1996.

Table 9. Local Determinants of Participation, Low-SES sample

Dependent Variable: 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)	**-0.101****	**-0.109*****	**-0.118****	**-0.069***	**-0.087****	**-0.101****	**-0.100*****
	(0.038)	(0.036)	(0.043)	(0.033)	(0.033)	(0.039)	(0.033)
Mom Non-Cit*Local Climate		**-0.032**	**-0.031**	**0.051**	**multiple variables**	**-0.041**	**0.205**
		(0.037)	(0.032)	(0.031)		(0.162)	(0.227)
Number of Observations	146,206	146,206	146,206	146,206	144,940	143,244	144,870
R-squared	0.227	0.227	0.227	0.227	0.227	0.227	0.226

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. See notes from previous tables. All regressions include full set of fixed effects and demographic controls as in Table 3 column II. Definitions of state policy generosity, media coverage, anti-immigrant sentiment, and anti-immigrant congressional representation are described in the text and in more detail in Watson (2010).

Table 10. Robustness Checks, Low-SES Sample

Panel A.

	I	II	III	IV	V	VI
Dependent Variable: Medicaid (mean=0.47)						
Change from Preferred Specification	None	Linear Enforcement Measure	Probit	Estimated Number of Non-Citizens Rather Than Point-in-Time Measure	Drop Cluster-Specific Linear Time trend	Add State-Specific Linear Time Trends*Non-Cit
Mom Non-Cit*Log(Enforcement)	**-0.101****		**-0.085***	**-0.094***	**-0.090****	**-0.060**
	(0.038)		(0.049)	(0.047)	(0.037)	(0.042)
Mom Non-Cit*Enforcement		**-4.882***				
		(2.755)				
Number of Observations	146,206	146,206	143,465	146,206	146,206	146,206
R-squared	0.227	0.227	0.169	0.227	0.226	0.229

Panel B.

	VII	VIII	IX	X	XI
Dependent Variable: Medicaid (mean=0.47)					
Change from Preferred Specification	Add Control for State Unemployment Rate*Non-Cit	Add Control for State New Legal Immigration Rate *Non-Cit	Add Control for Maternal Employment	Add Control for Medicaid Managed Care Penetration	Bootstrap Standard Errors
Mom Non-Cit*Log(Enforcement)	**-0.076***	**-0.096****	**-0.096****	**-0.101****	**-0.101****
	(0.043)	(0.037)	(0.036)	(0.038)	(0.043)
Number of Observations	146,206	146,206	146,206	146,206	146,206
R-squared	0.227	0.227	0.234	0.227	0.227

Notes: *** Significant at the 1 percent level. **Significant at the 5 percent level. *Signficant at the 10 percent level. See notes from previous tables. All regressions include full set of fixed effects and demographic controls as in Table 3 column II except as noted. For Probit in column III, marginal effect for children of non-cititzens is shown. Some observations were dropped due to collinearity, standard error is generated using the delta method, Pseudo-R2 reported. Comparable probit effect for children of citizens is 0.011 with a standard error of 0.020. Bootstrap standard errors are cluster bootstrapped with 1000 replications.