Covering Undocumented Immigrants: The Effects of A Large-Scale Prenatal Care Intervention

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Abstract

We examine the short- and long-term effects of expanded Medicaid pregnancy coverage to undocumented immigrants using a novel dataset linking California birth records to Census surveys and administrative records. We implement a mothers' fixed effects design that compares changes in sibling outcomes across mothers with different likelihood of having undocumented status. We find the policy increased insurance and prenatal care among likely undocumented immigrant women and improved pregnancy outcomes. Later in life, our results suggest that their children may be more likely to enroll in post-secondary education and receive less public support.

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There are nearly 11 million immigrants without legal status in the U.S., who are ineligible for virtually all public programs (Krogstad et al., 2018; Broder and Lessard, 2023). Those who advocate in favor of excluding undocumented immigrants from public benefits highlight the direct fiscal cost of extending benefits to this group, as well as the potential to incentivize unlawful immigration or create dependence on government support. However, expanding public program eligibility to undocumented immigrants may come with benefits as well, if the programs improve the productivity of recipients and enhance the well-being of their families.[1]

Expanding public program eligibility to include undocumented immigrants may be especially beneficial when considering public health insurance for pregnant women. It is estimated that one out of every 16 births in the country is to an undocumented immigrant mother (Passel et al., 2018), but undocumented immigrants do not qualify for routine prenatal care through the Medicaid program in most states. Available evidence indicates that undocumented immigrants are less likely to use adequate prenatal care and experience more complications of labor and delivery than other women (Reed et al., 2005; Korinek and Smith, 2011). Expanding public prenatal coverage to low-income women in this category has the potential to increase access to health services and improve the health of both mothers and their infants, who are U.S. citizens by birthright. Given existing research showing that early life and *in utero* interventions can have long-run and even multigenerational effects, expanding eligibility for this program to pregnant undocumented immigrants could generate substantial benefits for the next generation of US citizens.

In this paper, we examine one of the first expansions of prenatal coverage to undocumented immigrants: a landmark policy change in California, the state with the largest population of undocumented immigrants in the U.S. (Pew Research Center, 2019). In 1988, California used state funding to expand eligibility for its Medicaid program, Medi-Cal, to undocumented pregnant immigrants who previously did not qualify for coverage due to their immigration status. Three years after this policy change, approximately 45 percent of Medicaid-funded births in California were to undocumented immigrant women, accounting for about one-sixth of all births in the state (Norton et al., 1996).[2]

This policy change provides a unique opportunity to investigate both the short- and long-term effects of Medicaid coverage for these pregnancies since California is one of the few states with a

[1] In general, existing evidence indicates that the net fiscal impact of immigration on the federal budget is positive, while the effects on state and local budgets tend to be negative (National Academies of Sciences, Engineering, and Medicine, 2017).

[2] We are including immigrants who gained legalization under the Immigration Reform and Control Act (IRCA) in the "undocumented" category in our discussion here since this group also gained Medi-Cal eligibility under the October 1988 expansion. See further discussion in Section 1.

long history of covering pregnant undocumented immigrants under its Medicaid program (Green et al., 2016). Taking advantage of new opportunities for innovative data linkages facilitated by the U.S. Census Bureau, we build a novel dataset that links confidential state birth certificate records to a variety of federal survey and administrative data sources including the Decennial Census and American Community Survey, Internal Revenue Service records on earnings and Earned Income Tax Credit eligibility, childbearing information from the Census Household Composition Key, information on Medicaid enrollment from the Centers for Medicare & Medicaid Services, administrative data on mortality from the Social Security Administration, and records on post-secondary education from the National Student Clearinghouse. Together, these data sources allow us to examine the effects of this policy intervention on insurance coverage and health care use during pregnancy, health at birth, and later life health and human capital for the cohorts who gained access while *in utero*.

Another advantage of these data linkages is the ability to identify siblings born to the same mother. This information allows us to implement a mothers' fixed effect design to more credibly estimate the effects of the Medi-Cal expansion. This approach controls for time-invariant differences in outcomes across families and estimates policy effects by comparing siblings with different exposure to the policy based on their timing of birth. This sibling comparison allows us to avoid confounding our estimates with large changes in the composition of immigrant women in the state during this time period. We present several analyses that demonstrate the importance of this empirical approach in our context.

In our main analyses, we compare outcomes for siblings born to likely undocumented immigrant mothers before and after the policy change, and compare these differences to those observed among siblings to other immigrant mothers. We estimate likely undocumented status using maternal characteristics available in the linked Census survey data, including detailed information on the mother's country of birth and year of entry to the U.S. We interact the mother's likely undocumented status with a post-policy indicator to provide an estimate of the policy effect for an undocumented woman and her child. Sibling comparisons that occur entirely in the pre- or post-periods are also included in the analysis, which allows us to control for general birth order effects on outcomes and secular changes over time.

Using this approach, we find evidence of an immediate and large increase in Medi-Cal coverage for prenatal care after the policy went into effect.[3] Over the entire post period, we estimate that

[3]Note that throughout the text we use "coverage" to indicate actual participation or enrollment in Medi-Cal.

approximately 25 percent of mothers with likely undocumented status gained Medi-Cal prenatal coverage as a result of the policy change. This increase in Medi-Cal coverage was accompanied by a similarly-sized decrease in uninsurance. We also find an increase in prenatal care utilization among likely undocumented immigrant mothers. We estimate a 1.4 percentage point increase in any prenatal care use, with an average increase of approximately one prenatal visit during the pregnancy. When scaled by the estimated coverage change, our estimates imply that newly enrolled immigrant women were 5.5 percentage points more likely to use any prenatal care and increased their total number of prenatal visits by almost 4 visits, on average. We also find evidence of earlier prenatal care initiation during the pregnancy, as well as a decrease in deliveries in public hospitals and an increase in doctor-delivered births. We find no change in the method of delivery.

These changes in coverage and health care utilization translate into higher birth weights for the infants born to likely undocumented immigrant mothers. When scaled by our first stage estimate, our estimates imply an average 237 gram increase in birthweight (a 5.7 percent increase over baseline). We do not find significant changes in gestational length or the fraction of infants who are small for their gestational age, although the point estimates are consistent with small improvements in these measures.

We conduct additional analyses to explore potential mechanisms behind the infant health improvements. We first examine whether these changes might be explained by fertility responses to the policy. If any policy-induced fertility changes lead to changes in the composition of births, this could help explain the patterns observed in infant health outcomes. In our analysis of fertility response, we find that likely undocumented immigrant mothers are more likely to have additional births following the policy change. This could reflect changes in health during pregnancy, which may in turn reduce miscarriages and result in more live births conditional on pregnancy, or changes in desired fertility due to lower costs associated with pregnancy, which may increase conception rates or reduce abortions. A bounding exercise using conservative assumptions suggests that some, but not all, of the improvements in infant health we observe could be due to favorable selection. Next, we take advantage of additional information available on the birth certificate records to look at changes in pregnancy complications following the expansion. These analyses indicate that likely undocumented immigrant women experience a smaller number of pregnancy complications once the policy goes into effect, suggesting that access to medical care and improved health during pregnancy could also be important mechanisms.

After exploring mechanisms, we next investigate whether the improvement in infant health translates into better health and human capital outcomes later in life. We find evidence suggesting that the infants exposed to the policy change continue to see benefits through young adulthood. We find no evidence that this expansion of prenatal coverage led to higher persistent dependence upon the program: treated children were, if anything, less likely to use Medicaid during their childhoods (possibly due to having better childhood health). We also find higher enrollment in post-secondary school, and some evidence of a reduction in EITC benefits received as an adult. However, we do not find effects on college graduation, rates of childbearing, or annual earnings as observed through age 28, although it is possible that these cohorts will experience earnings growth at older ages. We also find no evidence of an effect on cumulative mortality, but the observed rate of death is low at these ages.

We show that our results are very similar when we restrict the sample to Hispanic immigrants. We also show that our results are robust to a variety of specification checks, including additional control variables for later Medi-Cal income-based expansions, using an alternative method for identifying likely undocumented immigrants, and weighting to account for non-random participation in Census survey data. We also examine the sensitivity of our results when we introduce controls for concurrent immigration reforms under the Immigration Reform and Control Act of 1988 (IRCA), which created a path to legalization for certain undocumented immigrants and has been linked to improved birth outcomes among Mexican immigrants (Cascio et al., 2024). Our results are largely robust to this analysis, as well as a second sensitivity analysis that drops immigrants whose families likely benefited from IRCA, which we identify using newly linked data on Social Security Card applications for mothers in our sample and their spouses. Finally, we conduct a placebo test that re-estimates the model using later cohorts, who were not affected by the policy change, and find no evidence of effects. These additional analyses further increase confidence in our results.

Together, this evidence indicates that expanded prenatal coverage to undocumented immigrants has important consequences for the health and economic well-being of their children. Back-of-the-envelope calculations indicate that the cost-effectiveness of this policy is higher than other interventions that target health at birth. To the extent that longer-term benefits of the policy are present, the government may even fully recoup its initial investment.

Our analysis of California's Medicaid expansion to undocumented immigrants provides a unique opportunity to evaluate a policy change that has been implemented in a growing number of states in recent years. Currently 25 states offer prenatal coverage for this population through the Children's

Health Insurance Program (Brooks et al., 2025), while pregnant undocumented immigrants remain ineligible for public coverage in the remainder of states. In general, U.S. public opinion and policymakers remain divided on whether government health programs should cover undocumented immigrants (e.g. Luhby, 2019). Our findings demonstrate that expansions of public prenatal coverage generate benefits that should be accounted for by policymakers engaged in these debates.

1 Background

1.1 Medicaid Pregnancy Coverage and Infant Health and Well-Being

There are a number of ways that Medicaid eligibility during pregnancy may affect the health and development of an infant. First, improved access to medical care may directly affect pregnancy outcomes. Medicaid pregnancy coverage includes all pregnancy-related medical care, delivery-related care, and 60 days of postpartum care following childbirth. During the period of study, the American College of Obstetricians & Gynecologists recommended between 13 and 15 visits for an uncomplicated pregnancy (The American College of Obstetricians and Gynecologists, 1985), similar to their current guidelines (American Academy of Pediatrics and American College of Obstetricians and Gynecologists, 2017). Initial prenatal care visits include comprehensive physical exams and a review of the patient's medical history with physician assessment of any risks that may require special management. Follow up visits continue to monitor the health of the pregnancy through physical examination and laboratory tests. In the case of any medical problems or pregnancy complications, more frequent visits are necessary to monitor these conditions and speciality care may be required, which would also be covered under Medicaid pregnancy coverage. The physician also develops plans for hospital admission, labor, and delivery with the patient.

In addition to better access to medical care, other types of prenatal care may be important for the long-term health and development of the child. Additional components of prenatal care include nutrition counseling, health and childbirth education, and services to address psychological and social stresses (The American College of Obstetricians and Gynecologists, 1985). In a national survey administered during this time period, most women with Medicaid-funded prenatal care reported receiving guidance related to nutrition and weight gain during their pregnancies, as well as instructions to limit or discontinue use of alcohol, tobacco, and illegal drugs (Miller and Wherry, 2019). In addition, 40 percent of women reported learning about the Women, Infants, and Children (WIC) program from their

health provider and 76 percent received WIC benefits during their pregnancy.[4] In California, women enrolled in Medi-Cal were also eligible for enhanced services including personalized risk assessments for nutrition, health education, and psychosocial needs, and additional support services (Korenbrot et al., 1995).

Prior to the Medi-Cal expansion, more than 30 percent of immigrant mothers in California did not receive prenatal care in the first trimester.[5] Focus group sessions held with providers delivering care to pregnant Latina women in San Francisco, and their patients, during this time period indicate that lower incomes, lack of health insurance, and the absence of legal documentation were among the largest barriers to early initiation of prenatal care (Guendelman and Witt, 1991).[6] Nearly all immigrant women, however, did receive some form of prenatal care, although the average number of prenatal visits was much lower compared to non-immigrant women. In 1989, the average number of prenatal visits was 9 versus 11 for immigrant and non-immigrant women giving birth.[7]

In addition to prenatal care utilization, there are additional channels through which Medicaid eligibility may positively affect the well-being of the mother and her child. Existing evidence indicates that gaining access to public insurance coverage can lead to substantial improvements in mental health (Finkelstein et al., 2012). Other work has demonstrated that the mental health of the mother could influence birth outcomes (e.g. Persson and Rossin-Slater, 2018). In addition, among families who would have otherwise paid for care out of pocket, Medicaid coverage may free up household resources for other types of investment.

Prior research has linked expanded pregnancy Medicaid eligibility for US citizens and permanent legal residents to improved infant health outcomes, including a lower incidence of low birthweight (Currie and Gruber, 1996b; East et al., 2023). And, as these children grow, this early investment appears to lead to better health and economic trajectories. Miller and Wherry (2019) find that expanded Medicaid coverage for pregnancy results in lower rates of chronic illness and fewer related hospitalizations among infants in their early adult years. They also find that infants exposed to expanded

[4] Immigrant women are eligible for benefits under the WIC program regardless of their legal status, which was also true during this period of study (Bosco, 1994).

[5] Authors' calculation based on information reported on the 1983-1988 California birth records.

[6] Uninsured women in the state, including undocumented immigrants, could receive free prenatal care from Title V funded maternal and child health clinics provided that they had family incomes below 200% FPL and lacked public or private health insurance coverage (Guendelman et al., 1994). However, only 0.04 percent of 1989 birth records for infants born to immigrant women indicate that Title V was the principal source of payment for prenatal care (the first year this information was collected on the birth certificate record), as compared to 25 percent of records indicating care was paid for out-of-pocket.

[7] Authors' calculation from 1989 California birth records. This was the first year this information was collected on the birth certificate record.

Medicaid later experience higher high school graduation rates. As we discuss later in this section, this is the first paper to examine whether similar long-term benefits may be present for the children of undocumented immigrants.

1.2 Changes in Medi-Cal Eligibility for Undocumented Immigrants

Prior to the policy change being studied, California's Medi-Cal program limited eligibility for immigrant women to those with permanent legal U.S. residency status (Norton et al., 1996). The state's expansion of Medi-Cal to pregnant women regardless of their immigration or legal status occurred in response to the national creation of the Emergency Medicaid program. The Omnibus Budget Reconciliation Act (OBRA) of 1986 established the Emergency Medicaid program by specifying that Medicaid cover "emergency medical conditions," including emergency labor and delivery services, for immigrants who did not meet the legal residency requirements for the program (Perkins, 2004).[8] It also allowed states to newly draw down federal funds to help cover the medical expenses for these emergency medical conditions (Martucci, 1987).

While OBRA only required Medicaid coverage of labor and delivery services, California decided to go further than the federal requirement and include coverage for other pregnancy-related care as part of its OBRA expansion. The additional non-emergency pregnancy-related services were fully funded by the state (Martucci, 1987). All changes were effective starting in October 1988 under a new state law that extended Medi-Cal eligibility for pregnancy-related services, including prenatal, delivery, and postpartum care, to undocumented immigrants, as well as those with inadequate or expired documentation, or a temporary visa.[9] The state also expanded coverage to undocumented immigrants recently eligible for legal residence under the Immigration Reform and Control Act (IRCA), but who were temporarily ineligible for public benefits under the IRCA law (Martucci, 1988). IRCA created a pathway to amnesty for certain groups of undocumented immigrants, but these individuals were otherwise ineligible for certain public benefits (including Medicaid) for a 5-year period after applying for legalization. In later sensitivity analyses, we include additional control variables for IRCA's legalization programs and exclude immigrants whose families likely benefited from IRCA to investigate whether this concurrent policy may have also affected the outcomes we study.

Over the next 15 months, the state further expanded its Medi-Cal program to pregnant women

[8]The Emergency Medical Treatment and Active Labor Act (EMTALA), also passed in 1986, required Medicare participating hospitals to provide emergency care, including labor and delivery services, regardless of ability to pay or immigration status.

[9]While the effective date of the federal OBRA requirement was January 1, 1987, California was given until January 1, 1989 to implement this change (Martucci, 1987).

with incomes above the current income eligibility threshold, which was around 85% of the federal poverty line (Ellwood and Kenney, 1995). Effective in July 1989, the state expanded Medi-Cal coverage to all pregnant women and infants with family incomes up to 185% FPL. An additional eligibility expansion to 200% FPL was implemented in January 1990 (Mitchell, 2005). Both of the income expansions included immigrants regardless of documentation status.

To address potential physician shortages under Medi-Cal eligibility expansions, the state gradually increased Medi-Cal reimbursement rates for obstetrical care by approximately 85 percent between 1985 and 1989. They also implemented a global fee structure, which further increased reimbursement levels for physicians and reduced administrative paperwork (Prestowitz et al., 2000). These changes were meant to incentivize providers to meet increased demand for prenatal services.

In Figure 1, we estimate the impact of these policy changes on the fraction of immigrant women who would have been eligible for Medi-Cal if they became pregnant, based on their documentation status. To do this, we construct measures of eligibility among female California residents ages 15-44 using detailed information on federal and state eligibility rules over the time period.[10] We apply these rules to a fixed sample of immigrant women from the 1990 Census in each month and year, after adjusting for inflation. The use of a fixed sample to estimate eligibility in each month-year produces information on eligibility changes over the period that are due to state law changes, rather than changes in state demographic or economic characteristics. This type of "simulated eligibility" measure has been used as a policy instrument in a large body of work pioneered by Currie and Gruber (1996a,b) and Cutler and Gruber (1996). In order to examine differences in eligibility by documentation status, we impute documentation status using information on individual characteristics available in the 1990 Census and an algorithm developed by Borjas (2017) that we adapt for use in this context. Under this methodology, an individual is considered to be a legal immigrant if they are a citizen or arrived in the US before 1980 or were born in Cuba, if they receive certain public benefits that are only available to legal immigrants, if they are a veteran or currently in the armed forces or are in an occupation that requires licensing, or if their spouse is a legal immigrant or citizen. Individuals who do not meet any of these criteria are considered "undocumented."[11]

The first of the three vertical lines in Figure 1 depicts the Medi-Cal expansion for undocumented immigrants. As seen in the graph, this policy resulted in a dramatic increase in eligibility for undocumented immigrants from 0 to 56 percent, surpassing estimated eligibility among documented

[10] See Appendix Section A for additional information on the eligibility calculation and rules used.
[11] See Appendix Section B for further information on this imputation.

immigrants (approximately 37 percent). The next two vertical lines depict the later income-based expansions to Medi-Cal. The two groups of immigrant women experience similar sized increases in eligibility as a result of these later expansions (approximately 27 to 28 percentage points, cumulative).

Using administrative Medi-Cal data that included the documentation status of enrollees from this time period, Norton et al. (1996) find a strong response to the Medi-Cal expansion among pregnant undocumented women.[12] The overall number of Medi-Cal enrollees nearly doubled between 1987 and 1991 (from 116,000 to 228,000) and the authors estimate that about 78 percent of the growth was due to the expansion of coverage to undocumented and IRCA immigrant women. In addition, Medi-Cal funded births to these groups represented 45 percent of all Medi-Cal funded births in 1991, and about one-sixth of the total number of births in the state. While some of these newly eligible women enrolled only during the last month of pregnancy, 88 to 93 percent initiated coverage earlier; the average period of enrollment during pregnancy was just over 5 months. The vast majority of new immigrant enrollees after this policy change (84 percent) were undocumented immigrants rather than IRCA immigrants. Therefore, for brevity, we refer to the newly eligible as "undocumented immigrants" throughout the text.

1.3 Evaluation of Expanded Prenatal Coverage for Undocumented Immigrants

Despite the large magnitude of the undocumented coverage expansion in California, we are unaware of any existing study of this policy change. There has also been relatively little work examining publicly-funded prenatal coverage expansions to undocumented immigrants in other states. This may be partly due to the low prevalence of state efforts to cover this population until relatively recently. The Children's Health Insurance Program (CHIP) From-Conception-to-End-of-Pregnancy (FCEP) option (previously called the "unborn child" option) was introduced in 2002 and made federal funding available to states to cover a woman's pregnancy-related care regardless of her immigration status. As of January 2025, 25 states have adopted this option (Brooks et al., 2025). Pregnant undocumented immigrants remain uncovered in other states (other than for emergency medical care), including some states with large immigrant populations such as Florida and Georgia.

A handful of studies have examined the impact of state adoption of the CHIP FCEP option on prenatal care and infant health.[13] Two papers use a difference-in-differences design to compare changes

[12] With the help of two of the authors in the series of papers that used these data (Howell and Brown, 1989; Ellwood and Kenney, 1995; Norton et al., 1996), we tried to track down the original data tapes for our own analyses but found they were either no longer in existence or unavailable.

[13] Predating these studies is work examining the effects of Medicaid coverage for recent legal immigrants following the

in prenatal care and infant health for immigrant women in states with and without these policy options. The authors find evidence of improved prenatal care use for immigrant women associated with state adoption of these policy options, but no detectable changes in birth outcomes (Drewry et al., 2015; Wherry et al., 2017).[14] A third paper uses staggered county adoption of a policy in Oregon to examine expanded coverage for prenatal care for undocumented immigrants enrolled in the state's Emergency Medicaid program. The authors find improved prenatal care use, as well as reductions in extremely low birthweight and infant mortality (Swartz et al., 2017). Of particular interest, they also find evidence of increased use of medical care during the infant's first year of life, including the number of well child visits and higher receipt of recommended care. The analyses are limited to Medicaid claims, so this may overstate increases in medical care utilization if care would have been received elsewhere (Daw and Sommers, 2017), or be problematic if there were changes in the types of women/infants participating in Medicaid after expansion. More broadly of note, none of these papers test for differential trends in outcomes prior to policy adoption, nor do they consider changes over time in the immigrant populations in their study settings.[15] In follow-up work that examines the Oregon policy change using Medicaid claims linked to birth certificate records, the authors are unable to detect changes in newborn health (Rodriguez et al., 2021, 2022).

More recently, Hwang (2023) uses an event study design comparing children of noncitizens with low levels of education in states with and without the CHIP FCEP option to estimate effects on their birth outcomes and later childhood health, as reported in the National Health Interview Survey. While the paper finds no change in average birthweight, it documents improved parent-reported health at

contraction of their eligibility for public benefits under federal welfare reform in 1996 (Joyce et al., 2001; Royer, 2005). These papers reach different conclusions regarding the effects of contracted Medicaid coverage on prenatal care utilization but agree that birth outcomes were mostly unaffected. Since recent legal immigrants tend to be more advantaged than immigrants without legal status (e.g. Marshall et al., 2005), these findings do not necessarily shed light on the effects of expanded coverage to women with undocumented status. See Wherry et al. (2025) for a more thorough review of the evidence on public health insurance coverage for immigrants.

[14] Drewry et al. (2015) focuses on immigrants from Mexico and Central/South American and finds enhanced prenatal care use among the subset who are single and have lower levels of education. Wherry et al. (2017) finds evidence of increased prenatal care use among all immigrant women and use U.S.-born women as an additional comparison group.

[15] In addition to these studies, two papers have examined the contraction of public health insurance benefits for undocumented immigrants in Nebraska in 2010. The authors find evidence of decreased prenatal care use following the termination of Medicaid benefits (Atkins et al., 2018). Also, a comparison between undocumented immigrants giving birth with and without access to Medicaid revealed higher maternal weight gain and increased abnormal conditions among newborns of undocumented immigrants with access to Medicaid (Atkins et al., 2017). However, the authors document that the characteristics of undocumented immigrants giving birth in Nebraska differ significantly between the pre- and post- periods, making it difficult to discern whether these trends are, in fact, due to changes in the state's coverage policy. Finally, another relevant study examines the effects of California's passage of Proposition 187 in November 1994, which restricted eligibility for public benefits to legal residents. Immediately challenged and never enforced, Spetz et al. (2000) document "chilling effects" following its passage in the form of a small reduction in prenatal care visits among low-education immigrant women, but no observed changes in birth outcomes. This reduction in prenatal care use is estimated as a deviation in trend for this population and, similar to the papers described above, is limited in its ability to account for compositional changes in the immigrant population over the study period.

ages 4 to 6. These findings indicate that there may be longer-reaching effects of early coverage for children of immigrants, even if there are no health changes detected at birth.

1.4 Concurrent Changes in Immigration and the Characteristics of Immigrants

An important limitation of these existing studies is that they are unable to fully account for any concurrent changes in the characteristics of pregnant immigrant women that may affect the outcomes studied. Changes in the composition of immigrants, which may vary on both observed and unobserved dimensions, could generate spurious correlations between the timing of a Medicaid expansion and outcome changes. This is not a trivial concern when studying policies targeting undocumented immigrants, a group that has seen tremendous change in their numbers and composition over time in response to changes in national immigration policy (Massey and Pren, 2012; Krogstad et al., 2018).

This may be especially true in California over the period we study, which saw a large increase in low-income immigrants (Sun-Hee Park et al., 2000). Net undocumented immigration started at relatively low levels in the early 1980s, but surged during the mid- to late-1980s, followed by a sharp decrease in the early 1990s (Johnson, 1996). We document relevant changes in the number and composition of immigrant women of reproductive age using data on their year of entry to the U.S. from the 2000 Census. Panel I of Figure A1 traces out the number of immigrant women residing in California by their year of entry, place of birth, and education level. As may be seen in this figure, there is a large increase in the number of immigrants who entered the U.S. from Central America and the Caribbean, followed by a sharp decrease, during the period we study. In addition, there is a noticeable increase in immigrant women with lower educational attainment who entered the U.S. during the middle of the period, also followed by a decrease.

As one might expect, these demographic changes also change the characteristics of women giving birth over the study period. Panel II of Figure A1 uses the same Census data to examine the characteristics of immigrant mothers for children born in the state during our study period. Notably, there is an increase in the number of births to immigrants from Central America and the Caribbean over time. The number of births to immigrant women with lower levels of education also increases over time.[16] These types of compositional changes are difficult to address using standard birth certificate data, which contain very limited data on maternal characteristics. Not adequately accounting for these types of compositional changes may lead to biased estimates of any program effects.

In addition to the challenge of a changing immigrant population, researchers also encounter diffi-

[16] Note that educational attainment is measured at the time of the survey, not the time of birth.

culties in identifying the targeted population of coverage expansions. There is no information on the citizenship or legal residency status of the mother on birth certificate records, nor information on her income or socioeconomic status to determine eligibility for expanded coverage.[17] Without this information, studies relying on birth certificate data have been limited to examining changes in outcomes for all immigrant women, or some subset of these women such as those with low education levels, which could also make it difficult to detect program effects. Meanwhile, studies that use survey data (e.g. Hwang, 2023) may be limited by a smaller sample size.

This paper builds on this small existing literature by offering new evidence on the effects of public prenatal coverage for undocumented immigrant women. Given its historic size and the number of women covered, the Medi-Cal expansion provides a promising setting to detect program impacts. Our study also takes advantage of new data linkages to overcome these existing empirical challenges: we are able to observe changes in outcomes among births to the same mother before and after the expansion, thereby holding fixed the composition of immigrant mothers. We also use newly linked survey data to identify immigrant mothers who were likely to have undocumented status and therefore benefit from the expansion. We estimate program impacts among births to likely undocumented women using births to other immigrant women as a comparison group. We examine the relative trajectories of the outcome variables by estimated undocumented status for several years before and after the expansions occurred, allowing us to both assess the validity of our approach and to document dynamic effects of the policy over time. Finally, we take advantage of new linkages to federal administrative data to explore the potential role of concurrent changes in immigration policy and to trace out the long-run effects of the policy change, providing the first look at the adult health and human capital outcomes of the children who gained Medi-Cal eligibility.

2 Data and Outcomes

Our analysis of the effects of the Medi-Cal expansion uses a novel data linkage between California birth records for children born between January 1984 to October 1994 and several different sources of survey and administrative data. To conduct this linkage, we received permission from the California Department of Public Health to send confidential birth records to the U.S. Census Bureau to assign a Protected Identification Key (PIK) to each individual birth record. This anonymized individual identifier allows for linkages to other Census-held data that have undergone a similar process without the

[17] California birth records only started including educational attainment of the mother in 1989.

retention of personally identifying information. In our case, the full name of the infant, exact date of birth, and county of birth are used to assign a PIK to each infant by comparing individual-level information on the birth certificate input file to the characteristics of records in the Person Identification Validation System (PVS) reference files held by Census (Wagner and Layne, 2014). The PIK rate for the California birth records during the years of our study is 97.2 percent overall and 96.2 percent for births to immigrant mothers,[18] which are comparable to the PIK rates of federal data sources (Mulrow et al., 2011).

Following PIK assignment, we next link these infants to the long-form survey of the 2000 Census and survey waves of the American Community Survey (ACS) from 2001 to 2011, the last year in which any cohort in our sample is below age 18.[19] Participation in these surveys allows us to identify siblings born to the same mother and to observe additional information on the mother not available on the birth certificate record, including detailed information on her country of birth and her year of entry in the US. We identify siblings as those individuals of 17 years of age or younger who are residing at home with the same mother in the Census surveys. Without this linkage to the Census data, it is not possible to otherwise link siblings in the birth records who were born before and after the policy change, since unlike their children, mothers' full identifying information was only recorded on the birth record starting in 1989, or the "post" period. Therefore, researchers are unable to identify children with the same birth mother born before and after the policy directly from the birth records. Importantly for our research design, full identifying information for the infant is available in all birth years, enabling this Census linkage. Using the post-1989 years of data, we validate the accuracy of our survey-based sibling identification procedure by comparing it against siblings identified using the birth mother's identity reported on the birth records starting in 1989. We find that the birth mother is misidentified using the survey-based method in only a very small number of cases (see Appendix Section C for additional details on this process).

Once siblings are identified, we restrict the sample to siblings born to immigrant mothers during the January 1984 to October 1989 period.[20] We use the foreign-born status of the mother as identified in the birth certificate data to identify births to immigrant mothers. We also require that the mother reside in the state of California at the time of each birth, which is reported on the birth record, and that

[18] These statistics were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY19-532.

[19] Note that the 1990 Census has not yet undergone PIK assignment by Census, so this survey is unavailable for our use.

[20] We start the pre-period in January 1984 since this is the oldest cohort observed below the age of 18 in the Census survey data. We define the post-period through October 1994 in order to limit the analysis to the period prior to Proposition 187, mentioned in footnote 15, which may have had chilling effects for immigrants in California.

the record have non-missing information for the child's sex, plurality, and mother's total live birth number. Finally, we exclude a very small number of births to women whose age and place of birth do not match across birth records, as well as those to women whose reported country of birth differs on the birth certificate record and Census survey data (representing less than 5 percent of records).[21] This leaves us with a final sample of approximately 132,000 siblings born to 58,500 unique mothers.[22]

Our analysis is necessarily limited to children who lived in the U.S. at some point between 2000 and 2011 in order to be surveyed in the Census or ACS. Children are observed in the survey data approximately 11 years after birth, on average.[23] Therefore, our estimates of the program's impact will not include effects for mothers or their children who leave the U.S. following birth or during early childhood, nor will they include families whose child dies prior to their inclusion in the Census or ACS. In addition, our estimates are based on individuals whose families are sampled and respond to the Census surveys, which may also miss some types of immigrant families.[24] Finally, our sample criteria requires that children reside with their mothers at the time of interview. Table A1 compares the characteristics of births to all immigrant mothers during our study period to those that receive PIKs and are included in the Census/ACS sample. The children that appear in the survey are more likely to have Asian mothers and less likely to have Hispanic mothers. Notably, the mothers of the children in the survey sample use more medical care during pregnancy and have better birth outcomes.

Depending on how the mothers and children who were excluded from our sample were affected by the Medi-Cal expansion, we may be either under- or over-stating the overall impact of the policy based on our sample alone. Later, we construct representative weights based on the universe of births observed in the birth certificate data that we apply to our Census/ACS sample. To the extent that treatment effects vary only in the observable characteristics used in the construction of these weights, this procedure will generate program effects that are representative of the population of births in the state. However, if there are unobservable characteristics generating selection into the sample (for instance, if there is policy induced return migration), our procedure is unable to account for that.

Table A1 also compares the characteristics of births in the siblings sample to the broader sample of births that received PIKs and were observed in the Census/ACS. The maternal and birth charac-

[21] 4.62 percent; statistic was approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

[22] All numbers have been rounded to comply with Census disclosure avoidance rules.

[23] Average child's age at the time of the survey is 11.27; statistic was approved for release by the U.S. Census Bureau, authorization number CBDRB-FY20-183.

[24] While the exact undercount rate for undocumented immigrants in the Census and ACS surveys is unknown, it is assumed to be 10 percent in national counts prepared by the Department of Homeland Security (e.g. U.S. Department of Homeland Security, 2003; Baker and Rytina, 2013).

teristics look largely similar, although births in the sibling sample are more likely to be higher order births. We further discuss the implications of the siblings sample and our research design in Section 3.

In our main analyses, we use additional characteristics of the mother drawn from the Census surveys to examine the effects of the expansions for infants born to mothers most likely affected by the policy change. Specifically, we estimate the probability that each immigrant mother was an undocumented immigrant around the time of the policy change using a procedure later described in Section 3. Table A2 provides descriptive statistics for immigrant mothers in the sample by the mother's likely undocumented status.[25] While we do not have information on the educational attainment of the mothers or their family structure at the time of birth, we observe this information later when the families are interviewed in the 2000 Census or ACS surveys. Mothers with a higher estimated probability of undocumented status are noticeably more like to have Hispanic ethnicity. They also are younger at the time of birth, have lower education levels, and are slightly more likely to be married at the time of the Census/ACS interview, on average.

2.1 Medi-Cal Eligibility and Insurance Coverage

We do not have information on mother's income at the time of pregnancy to estimate individual eligibility for Medi-Cal prenatal coverage (we only observe her income in 2000 or later in the Census survey data). However, we are able to use her county of residence at the time of birth to estimate average changes in eligibility using county-level measures of eligibility constructed with income information on county residents. To do so, we use a fixed sample of immigrant women drawn from the 1990 Census and impute the undocumented status of these women using the adapted Borjas (2017) algorithm, as described earlier. We then construct measures of the fraction of immigrant women of reproductive age in each county who are eligible by their documentation status during each month and year in our sample period (see Appendix Section A for additional details on the eligibility calculation), which we merge onto the California birth records by county, birth month, and birth year.[26] We next construct an eligibility estimate for each birth in our sample using the formula: $\hat{p} * \text{Elig(undocumented)}_{ct} + (1 - \hat{p}) * \text{Elig(documented)}_{ct}$, where $\hat{p}$ is the mother's estimated prob-

[25] Note that some estimates in this table are not reported because the underlying cell size did not meet Census disclosure rules. These entries are denoted by an "S."

[26] Note that only 34 counties are identified in the 1990 Census; however, these counties represent over 98 percent of the births in the state during our study period. For the 24 non-identified counties, we use an estimate of eligibility among respondents with non-identified counties in the 1990 Census.

ability of undocumented status, c indicates county, and t indicates the month and year of birth.[27] Because we calculate county-level eligibility using a fixed sample of women, any changes in eligibility observed in this analysis will reflect changes in Medi-Cal eligibility rules only, rather than other demographic or socioeconomic changes.

For the analyses that examine changes in individual insurance coverage, we use information from the birth certificate record on the principal source of payment for prenatal care, which was collected starting in 1989. While these data are limited to the "post" period of the Medi-Cal expansion, they allow us to trace out changes over time in Medi-Cal funded prenatal care relative to the months January to September 1989, which occurred during the first year of policy exposure. We might expect this to potentially attenuate our estimated effects of the policy change given that births starting in July 1989 would have a full 9-months of pregnancy exposure to the expansion, which was implemented in October 1988.

2.2 Health Care Utilization and Infant Health

We examine changes in the use of any prenatal care during pregnancy, the use of prenatal care in the first trimester, and the total number of prenatal visits during pregnancy, as well as the location of delivery (hospital vs. non-hospital, public vs. private hospital), method of delivery (cesarean section vs. vaginal birth), and type of attendant (doctor vs. midwife), using information from the birth certificate record. While changes in delivery care are unlikely to affect the birth outcomes we study, they might reflect more general changes in interactions with the health care system that were likely initiated earlier during pregnancy (such as establishing care with a physician), which could matter for pregnancy outcomes. Information on the total number of prenatal visits during pregnancy was only collected on the birth certificate record starting in 1989. Similar to our analyses of prenatal insurance coverage, we therefore examine changes over time in this outcome relative to the months January to September 1989.

To evaluate infant health, we examine average birthweight and gestational length, as well as whether the infant is small for gestational age (birthweight is below the 10th percentile for a given gestational age).[28] In additional analyses, we examine changes in the distributions of birthweight (by 500g bins) and gestational length (early preterm: $<$ 34 weeks, late preterm: 34-36 weeks, early term:

[27] Subgroup analyses for Hispanic immigrants use eligibility estimates constructed specifically for this subgroup using the characteristics of Hispanic immigrants in the 1990 Census.

[28] We exclude observations with reported birthweights of less than 400 grams or more than 6000 grams from any analyses of birthweight. We also exclude observations with reported gestation lengths of less than 18 weeks or over 50 weeks from the analyses of gestational length. Cutoffs for the small-for-gestational age measure are calculated for each birth year.

37-38 weeks, full term: 39+ weeks).

2.3 Long-Term Health and Human Capital

We rely on several sources of administrative data to examine later life outcomes for the cohorts who gained *in utero* eligibility. First, we examine mortality as recorded in the most recent vintage of the Census Numident file (quarter 3 of 2022) available at the time of analysis. This file contains cumulative death data for individuals with a Social Security Number as collected by the Social Security Administration. Mortality measured in the Numident closely tracks with mortality as reported by the Centers for Disease Control and Prevention (CDC) during our period of study; Finlay and Genadek (2021) document that the Census Numident captures around 95 percent of annual CDC death counts in the early-1980s and coverage increases through more recent years. We construct a measure of cumulative mortality as measured through age 27, which is observed for all cohorts in the data. The Census Numident file also contains information on the date on which an individual applied for a Social Security Number, which we use in a robustness check to identify immigrant families who likely benefited from IRCA.

Second, we examine post-secondary school enrollment and college degree attainment information through July 2022, when our youngest cohort is nearly 28 years old, as provided by the National Student Clearinghouse (NSC). Post-secondary school enrollment includes programs that confer associate's, bachelor's, and other certificates, as well as industry certifications and professional licensures. The NSC data cover between 87 to 97 percent of national enrollment in post-secondary, Title IV institutions, depending on the year (National Student Clearinghouse, 2021). NSC provided these data elements to the research team by linking the California birth certificate records to their administrative records using information on student name and exact date of birth. They then returned a data extract to us with an anonymized record identifier that enabled us to merge the de-identified NSC records with our birth certificate data in the Census integrated research environment.

Third, we examine fertility using information from an annual administrative dataset called the Census Household Composition Key (CHCK) available from 2016 to 2022. This dataset uses information from a variety of federal sources, including Social Security Number applications, the Internal Revenue Service (IRS) Form 1040, and the decennial census, to identify the parents of children ages 0 to 19 (U.S. Census Bureau, 2020). Together, these files capture near complete information on births occurring in the U.S. from 1997 through 2021 linked to parent information (Genadek et al., 2021). We

link the CHCK files to our datafiles using individuals' PIKs to observe fertility for all of our study cohorts between the ages of 13 to 26. We examine whether individuals give birth before age 20 (teen fertility), as well as if they ever give birth by the age of 26 (cumulative fertility).

Fourth, we examine adult earnings as measured using W2 forms from the IRS for the 2007 to 2022 tax years that have undergone PIK assignment by the U.S. Census Bureau. This enables us to examine annual earnings for each of our cohorts during the years they turn the ages of 23 to 28. Importantly, the use of W2 reported earnings does not require that individuals file income taxes for us to be able to observe their earnings.

And, finally, we examine the use of public assistance in the forms of an Earned Income Tax Credit (EITC) benefit and Medicaid enrollment. We examine the annual EITC amount received at ages 25 to 27 using information reported on the IRS 1040 form to calculate the EITC benefit amount.[29] We inflation-adjust earnings and EITC benefit amounts to 2021 dollars. Using administrative data from the Centers for Medicare & Medicaid services available from 2000 to 2016, we are able to examine annual Medicaid enrollment. Given the substantial changes in adult Medicaid eligibility rules that occurred during this period as a result of the Affordable Care Act, we focus on childhood (age 16-18) enrollment observed for all cohorts.

Figure A2 provides a summary of these different data sources and elements used in the analyses.

3 Empirical Strategy

As described in Section 1.4, we aim to overcome existing empirical challenges in identifying the effects of expanded prenatal coverage for immigrant women by taking advantage of additional information available in the linked Census/ACS data. Specifically, information on family relationships allows us to examine differential exposure to the undocumented expansion across siblings for the same mother, based on their time of birth. We also use detailed maternal characteristics available in the survey data to estimate which immigrant mothers were most likely affected by the undocumented expansion. Our empirical analyses then estimate relative changes in outcomes among siblings born to these women, as compared to other immigrant women, before and after the policy change. Our analysis also includes sibling groups born either entirely in the pre- or post-policy period, allowing us to net out birth order effects and secular changes over time.

[29] We only have access to IRS 1040 forms through the 2021 tax year, so are only able to calculate EITC amounts for all cohorts through the year they turn 27. Also, for nearly all data years, age 25 was the minimum age for EITC receipt for filers without children.

3.1 Variation in Likely Undocumented Status at Time of Policy

In order to implement our analyses, we first take advantage of the rich information available in the long-form decennial Census and ACS surveys to identify immigrant women (and their children) who we think were most likely to gain eligibility under the policy. While these surveys do not collect information on the legal status of non-citizens, they do have detailed information on the mother's country of birth with over 100 country codes (compared to just eight codes on the birth certificate record), as well as her year of entry in the U.S. Combining this information with her age and county of residence, we estimate the individual likelihood that each immigrant mother had undocumented status at the time of the policy change.

To do this, we use a prediction model estimated with publicly available 1990 Census data and imputed individual documentation status using the Borjas algorithm mentioned earlier (also see further details in Appendix Section B). We first estimate the probability of undocumented status among immigrant women ages 15-44 with young children (under age 6) in California in these data as a function of time-invariant maternal characteristics. We use fixed maternal characteristics because we observe the mothers in our analytic sample in the 2000-2011 Census/ACS data between five to seventeen years after birth and we expect that time-varying characteristics (e.g. occupation) may have changed noticeably during this period. Using the 1990 Census data, we regress imputed documentation status on the mother's country of birth, year of entry in the U.S, year of age in 1990, and county of residence indicator variables using a probit regression.[30] We include the mother's county of residence since this potentially provides some information regarding her socioeconomic status, in addition to accounting for any patterns in residence that result from undocumented immigrants living in areas with more economic opportunities or with established immigrant communities. The coefficient estimates from this model are then applied to the same characteristics observed for the mothers in our main analysis sample (using her county of residence and age from the birth certificate record) to predict the probability that they were undocumented in 1990.[31]

Figure A3 shows the distribution of predicted probabilities among the births to immigrant moth-

[30] While more granular information on year of entry is available in later surveys, the information in the 1990 Census is reported in the following ranges: 1950-1959, 1960-1964, 1965-1969, 1970-1974, 1975-1979, 1980-1981, 1982-1984, 1985-1986, and 1987-1990, which we use to construct indicator variables.

[31] See Table A3 for the coefficient estimates from the probit regression. Some regressors were dropped by the statistical software (Stata) based on data that perfectly predicted the outcome. We instruct Stata to use these rules when constructing predicted values. In addition, for any mothers who entered the U.S. after 1990, we use the coefficient estimate for 1987-1990 entry. For all mothers, we use maternal characteristics (i.e. county of residence) as observed at the time of the first birth in the sample period.

ers in our sample. A large share of these births (36 percent) are to women with probability estimates of less than a 5 percent likelihood of being undocumented. The remaining births are primarily to women with probability estimates spanning from 5 to 80 percent likelihood of being undocumented. The median predicted probability is 34.3 percent for births to immigrant mothers.

Figures A4-A9 show trends in outcomes for births to immigrant mothers with high estimated probabilities of undocumented status (≥ 0.5) compared to those with low probabilities (< 0.25) relative to the policy change. These figures present both the raw trends in outcomes (Figures A4-A6), as well as trends in outcomes once controlling for mother's fixed effects, county fixed effects, county per capita income and poverty rate, birth order, sex, whether the birth is a singleton (Figures A7-A9). Relative to other immigrants, Figure A4 indicates that births to likely undocumented immigrants (those with high estimated probabilities of undocumented status) have higher Medi-Cal eligibility and Medi-Cal coverage after the policy change. There is also evidence of an increase in prenatal care use for this group after the policy, reaching levels of utilization closer to those of other immigrants. Figure A5 provides evidence of a decrease in births delivered in public hospitals and an increase in doctor deliveries among likely undocumented mothers, as well as an increase in birthweight and gestational age relative to births to other immigrants. Finally, Figure A6 provides some evidence of a slight convergence in certain long-term outcomes following the policy (e.g. post-secondary enrollment, EITC benefit amount), with the children of likely undocumented immigrants starting to move toward levels achieved by children of other immigrants. Overall, these plots provide visual evidence of similar trends in outcomes for these two groups of immigrants that tend to deviate starting at the time of the expansion.

3.2 Empirical Specification and Assumptions

Once we estimate the likely undocumented status of immigrant mothers, we proceed to estimate our main empirical model. We compare changes in outcomes among siblings after the policy by the mother's likely undocumented status, while controlling for fixed family characteristics using a mother's fixed effect design. Importantly, we apply this mothers' fixed effect design in the context of a policy change that is unrelated to changes in family characteristics. This is distinct from a mother's fixed effect design that relies on within family variation in program take-up to estimate program effects. Similar approaches combining policy-driven variation with a family fixed effect design have previously been implemented to study access to WIC in Texas (Rossin-Slater, 2013), expansion in

pre-primary education in Uruguay (Berlinski et al., 2008), and a public prenatal intervention in Chile (Clarke et al., 2020). Most directly related to this paper, Aizer et al. (2007) uses a similar research design to examine the impact of a change in the mid-1990s in Medi-Cal pregnancy coverage from fee-for-service to managed care.

Our research design relies on an assumption that in the absence of the Medi-Cal expansion to undocumented immigrant women, outcomes for children of immigrants with different types of documentation status would have evolved similarly, after accounting for fixed differences in the characteristics of their families via a mother fixed effect. While this assumption is not directly testable, we examine whether the pre-treatment trends are similar for the children of immigrants with mothers who are more and less likely to have undocumented status using an event study design. If trends are similar prior to the intervention and diverge only after the policy change, this pattern lends credence to the assumption that the children of mothers with likely legal status are an appropriate counterfactual for the children of likely undocumented immigrant mothers. This test for pre-policy differential trends represents an innovation over existing mother fixed effects analyses, which implement static models.

In addition to investigating the plausibility of our identifying assumptions, the event study design offers another advantage in that it allows us to examine time-varying treatment effects. In our specification below, we consider the policies to "turn on" for children born during the implementation year. However, the effects of the policy may not be observed immediately for several reasons. First, some of the outcomes studied are unable to or unlikely to have immediate effects. For example, none of the births occurring during the six months following the policy change were able to benefit from increased prenatal care access during the first trimester, since this period had already passed when the policy went into place. And, prenatal interventions received later in the gestational period may be less likely to affect certain birth outcomes. Second, it took time for the newly eligible to learn about the policy change, enroll in the Medi-Cal program, and initiate care. Third, the state made several changes after the policy to make it easier for women to enroll in the Medi-Cal program.[32] These were

[32]California adopted several improvements to their Medicaid enrollment systems in an effort to increase coverage among eligible pregnant women. See Figure A10 for a timeline of these changes relative to the undocumented expansion. They include expediting eligibility processes (January 1989), outstationing eligibility workers at high-volume clinics (May 1990), implementing continuous eligibility during pregnancy and the postpartum periods despite changes in income (January 1991), shortening application forms (November 1991), and allowing presumptive eligibility for pregnant women such that they were able to receive services while their application was pending (November 1993). In addition, the state launched a media campaign called Baby-Cal designed to disseminate information about Medi-Cal and the importance of prenatal care (July 1991). General descriptions of these state efforts are available in Hill (1992), Dubay et al. (1995), and California Department of Health Care Services (2016).

state-wide changes but may have differentially affected take-up among undocumented immigrants and contributed to a potential ramp up in the expansion's impact over time. For example, Aizer (2003, 2007) document that state outreach efforts to increase Medi-Cal take-up in the late 1990s had larger effects on Hispanic and Asian families, who faced greater barriers related to language or immigration concerns. Along with these policy changes, evolving immigration policy nationwide may have affected immigrant mothers' willingness to participate in public programs.

We implement the event study analysis using the following specification:

$$y_{imct} = \sum_{\substack{y=-5 \\ y\neq -1}}^{6} \beta_y I(t - \text{Oct. } 1988 = y) \times ProbUndoc_m + \delta_t + \delta_m + \delta_c + \gamma X_{imt} + \theta Z_{ct} + \epsilon_{imct}. \quad (1)$$

We regress outcomes for births (i) to immigrant mothers (m) in month-year (t) on county of residence (δ_c), month-year (δ_t) and mother fixed effects (δ_m). We control for the following characteristics of birth (X_{imct}): sex, plurality, and the sibling birth order (first birth, second birth, third birth, fourth birth or higher). We also control for time-varying county characteristics (Z_{ct}): annual per capita income from the Bureau of Economic Analysis and the annual poverty rate from the U.S. Census Bureau.

The estimated coefficients β_y trace out the evolution of outcomes for births to likely undocumented immigrant mothers compared to other immigrant mothers, conditional on the mother fixed effect and other control variables. These are estimated by interacting an indicator for each 12-month period relative to the implementation date of October 1988 (y) with the mother's estimated undocumented status ($ProbUndoc_m$).[33] The 12 months just prior to implementation ($y = -1$) is the reference period.

We cluster the standard errors by mother. In order to account for the additional estimation required in predicting the mother's likely undocumented status in these specifications, we estimate the standard errors using a bootstrapping procedure that first resamples the 1990 Census to estimate the probability of undocumented status and then resamples the birth records by cluster to estimate the regression models described here.

The estimated coefficients from this specification provide estimates of changes in outcomes for likely undocumented mothers compared to other immigrant mothers relative to the year just prior to the policy change. Estimates for β_y for years prior to $y = 0$ should be close to zero if there are no

[33] Note that β_{-5} signifies 5 years before the undocumented expansion and includes births occurring between January and September in 1984. Meanwhile, β_6 captures births that occurred in October 1994 only. Since these represent only partial years, we do not report them in the event study figures.

differential pre-expansion trends in outcomes for births to immigrant women by likely undocumented status. We would expect relative outcomes to change starting with β_0 if there are effects of the policy.

We can interpret β_y as the effect of moving from 0 to 100 percent exposure, i.e., from 0 to 1 on $I(t - \text{Oct. } 1988 = y) \times ProbUndoc_m$). Equivalently, this can be interpreted as the person-level impact of gaining eligibility for the Medi-Cal program for a likely undocumented immigrant. This is how we prefer to discuss the magnitude of our estimates in the discussion that follows. Alternatively, one could multiply estimates of β_y by the average value of $ProbUndoc_m$ to determine the average effects of the policy among immigrants overall. Both interpretations rely on the assumption that legal immigrants were not affected by the policy change. For instance, if the policy were to have negative effects for legal immigrants who were already eligible (e.g. due to new supply-side constraints), this might serve to overstate the benefits accrued to likely undocumented immigrants. Meanwhile, if there are any positive spillovers to already eligible immigrants (such as through informational channels), this would work to attenuate our estimates of program impact.

Finally, to the extent that our prediction model does not successfully identify likely undocumented immigrants, we expect that this will also work to attenuate our estimates of program impact. While we are unable to directly validate this model, we later provide evidence that predicted undocumented status appears to align well with administrative records on whether mothers have Social Security Numbers. We also provide evidence that this prediction model performs better at identifying mothers gaining Medi-Cal coverage under the policy change than an alternative method of imputing documentation status that has been used in the literature (more discussion in Section 5). However, our inability to directly observe documentation status is a limitation of this study and may lead us to understate true program effects.

A corresponding difference-in-differences specification replaces the summation term in equation (1) by a single interaction between $ProbUndoc_m$ and a dummy for the post-period ($PostOct1988_t$). The estimate β from this model (equation 2) provides the average effect of the policy during the post-period for likely undocumented mothers and their infants. Different from the event study model, the reference period is the entire pre-policy period.

$$y_{imct} = \beta PostOct1988_t \times ProbUndoc_m + \delta_t + \delta_m + \delta_c + \gamma X_{imt} + \theta Z_{ct} + \epsilon_{imct}. \quad (2)$$

There are subtle differences in the source of identification used in this model as compared to the

event study model described in equation (1). The difference-in-differences model above identifies β only using siblings with at least one pre-period and one post-period birth (representing 45 percent of our sample), while the event study model incorporates information from siblings born entirely in the post-period (another 45 percent of our sample) into the estimates of β_y.[34] In a robustness check, we estimate the event study coefficients after dropping those who do not have at least one birth in the pre-period to assess the impact of this identification difference. We find that the results are similar, although somewhat less precise, after applying this sample selection criteria.

3.3 Subgroup Analysis with Hispanic Mothers and their Children

In addition to reporting results for equations (1) and (2) for the full sample, we also present results for the subgroup of Hispanic mothers and their children. As seen in Table A2, the vast majority (90 percent) of mothers in our sample with a high estimated probability of undocumented status are Hispanic, consistent with estimates from this time period on the top source countries for unauthorized immigrants to the U.S. (Warren, 1994). This analysis restricts both the treated and comparison groups to Hispanic women, who arguably might have more similar characteristics than immigrants more broadly. One potential disadvantage, however, could be the greater possibility of attenuated findings, if there are more likely to be positive spillovers for the comparison group in this analysis (i.e. Hispanic immigrants with permanent residency status) or if the algorithm used to estimate documentation status performs worse for Hispanic immigrants. Another thing to note is that the estimates for this subgroup will be specific to Hispanic immigrants and may not necessarily be representative of the experiences of other types of immigrants.

4 Results

4.1 Medi-Cal Eligibility, Insurance Coverage, and Prenatal Care Utilization

We begin by examining the changes in Medi-Cal eligibility and prenatal coverage resulting from the undocumented expansion. Each plot in Figure 2 presents the estimated event study coefficients from equation (1) for all immigrants in blue and the Hispanic subgroup in red. Graph (a) reports the estimated coefficients for Medi-Cal eligibility, which represent relative changes in county-level eligibility for likely undocumented immigrants as compared to other immigrants (since we are unable to cal-

[34] Hagemann (2025) shows that estimates of event study coefficients, where (1) treatment effects vary across treated cohorts and (2) some cohorts are observed only in the post-treatment era, are a weighted average of the treatment effects of each cohort in each post-treatment period, with some treatment effects being assigned negative weights. This applies to our setting, where some mothers have no pre-policy births.

culate individual-level eligibility in our data). We see a large jump in the first year of the policy, estimating approximately a 54 percentage point increase in Medi-Cal eligibility for likely undocumented immigrants, when compared to other immigrants. The eligibility gains then remain flat over the remainder of the study period. Column (1) of Table 1 reports the difference-in-differences coefficient indicating an average increase in eligibility of 55.3 percentage points for likely undocumented immigrants over the entire post-period.

Graph (b) of Figure 2 shows the event study estimates for Medi-Cal prenatal coverage. Due to the unavailability of this information on the birth certificate record before 1989, we are only able to examine changes relative to the first year of the policy. The change in Medi-Cal coverage is immediate but continues to climb during the next year, likely reflecting the increased awareness and efforts by the state to help women enroll. The effect appears to stabilize over the next three years at between 28 and 32 percentage points higher than the reference year. The difference-in-differences estimate (column (2) of Table 1) indicates a 25 percentage point increase, on average, among likely undocumented women during the post-period. Because we measure the increase in prenatal Medi-Cal coverage relative to a partially treated year, the true increase in Medi-Cal prenatal coverage due to the policy is likely even larger.

Figure 2 and Table 1 also show results for uninsurance, private insurance, and other sources of coverage for prenatal care. The drop in uninsurance for likely undocumented women following the first year of the policy (Figure 2(c)) is slightly larger than the increase observed in Medi-Cal prenatal coverage (14 vs. 10 percentage-points). The difference-in-differences estimate indicates an average decrease of 30 percentage points for these women during the post period (column (3) of Table 1). We also observe a much smaller increase in private coverage and decrease in other sources of coverage; see columns (4) and (5) in Table 1. In general, other sources of coverage do not tend to be very relevant, reported for only 2.6 percent of births to likely undocumented immigrants at baseline.

Overall, the estimates show a substantial decrease in uninsurance among likely undocumented women following the Medi-Cal expansion. In contrast to studies of income-based Medicaid expansions during this period (e.g. Cutler and Gruber, 1996; Dave et al., 2011), we do not find meaningful evidence of crowd-out of private coverage. This may be unsurprising given that private coverage is predominately employer-sponsored (Cohen et al., 2009), and undocumented immigrants are overrepresented in low-skilled occupations that tend not to offer these benefits (Fortuny et al., 2007). We also do not find much crowd out of other sources of public coverage.

Next, we examine changes in prenatal care utilization after the policy change. In Figure 2(f), we see evidence of an increase in prenatal care use starting the year after the policy goes into effect, although the estimate is not statistically significant until the following year. The difference-in-differences estimate (column 6 of Table 1) indicates an average increase of 1.4 percentage points for likely undocumented immigrants over the post period. This represents approximately a 36 percent reduction in the share of likely undocumented immigrants without prenatal care during the baseline period (3.9 percent). In terms of the number of prenatal visits, Figure 2(g) indicates a persistent increase over the post-period with a difference-in-differences estimate (column 7 in Table 1) indicating an average of one additional visit during pregnancy, over a baseline of 8.5 visits. This gradual increase in prenatal visits mirrors the pattern observed for take-up in panel (b). Recall that changes over time in this outcome are compared to the first year that the policy was in place due to limited data availability, similar to prenatal insurance coverage. Finally, there is some evidence of earlier initiation of prenatal care that does not emerge until the fourth year of the policy (Figure 2(h)), with an average increase of 4 percentage points (almost 8 percent) over the entire post period (column 7 of Table 1); however, it should be noted that we detect one significant pre-policy event study coefficient. We might expect there to be a delay for this particular outcome, since it requires women to know about and enroll in the program at the start of their pregnancy. This extended delay may indicate that undocumented immigrants still faced barriers to enrollment during their first trimester and could be related to other state efforts to increase awareness of the importance of prenatal care and Medi-Cal eligibility starting in 1991.[35]

Finally, our estimates are similar for the subgroup of Hispanic mothers (Panel B in Table 1, although show slightly smaller gains in Medi-Cal, overall insurance coverage, and prenatal care utilization. The exception is the estimate for early prenatal care utilization, which is slightly larger but may also reflect greater differential pre-trends on this outcome for this subgroup (Figure 2(h) in the red triangles). In general, however, the event study estimates depicted in Figure 2 for this subgroup show very similar patterns to those estimated for likely undocumented immigrants overall.

[35]The BabyCal informational campaign was initially launched in July 1991 and expanded beginning in November 1992 (see Figure A10.) It featured TV and radio ads and billboard advertisements with the messages "Get prenatal care. The State of California can help you," or "Take care of yourself while pregnant, your baby is counting on you" along with a state hotline number. Ads were targeted to Spanish language outlets in addition to English language outlets (Department of Health Services, 1992).

4.2 Delivery Care and Infant Health

Next we examine estimated changes in delivery care and infant health as reported in Figure 3 and Table 2. Starting with delivery care, we find no evidence of a change in hospital deliveries (Figure 3(a)) for likely undocumented immigrants after the policy change and the baseline rate was already at 99.4 percent. However, we do find a large decrease in deliveries in public hospitals with a difference-in-differences estimate (column 2 of Table 2) indicating an average 17.1 percentage point decrease over the post period. We expect that this may slightly overstate the effects of the Medi-Cal expansion since the pre-period coefficients suggest an existing decline in this outcome variable. This pattern may be related to the Emergency Medical Treatment and Labor Act (EMTALA), which required hospitals to admit women in labor regardless of ability to pay for services and may have differentially affected undocumented immigrants given their high rate of uninsurance. The average of the post-period event study coefficients, which estimate changes relative to the year just prior to the policy change, is closer to 15 percentage points and may be a more accurate estimate of the policy's effect.

We also looked at whether the birth was delivered by a doctor (as opposed to a midwife or other type of attendant) and the rate of c-section delivery. We find some evidence of an increase in doctors delivering the births of likely undocumented immigrants starting in the second year of the policy (Figure 3(c)). The difference-in-differences estimate indicates an average 5.1 percentage point increase over the post-period (column (3) of Table 2). This represents a 45 percent decrease in the share of likely undocumented immigrant women with midwives or other attendants, as measured during the baseline period (11.4 percent). We note that there is one significant event study coefficient in the pre-policy period in year -4, although other pre-period coefficients are not statistically significant and close to zero. We find no evidence of a change in the likelihood of a c-section associated with the Medi-Cal expansion (column (4) of Table 2).

Next, we examine the effects of the Medi-Cal expansion on birth outcomes, also reported in Figure 3 and Table 2. We do not find that the policy significantly affected gestation length or an indicator that the infant was small for their gestational age, although the coefficient estimates point in the direction of health improvements. We do find a significant increase in the average birthweight of infants of likely undocumented mothers of approximately 60 grams. The event study estimates in Figure 3(e) indicate that there was a gradual increase in birthweight over the post period, similar to the pattern of estimates seen for both coverage and prenatal care use. However, this plot also reveals some evidence of a differential pre-trend in birthweight, with one significant pre-policy event study coefficient appearing

at event time -3. So, this result should be interpreted with caution, as it may overstate the effect of the policy.

In additional analyses, we examine distributional changes in birthweight and gestation length under the policy. Figure 4(A) presents estimates for birthweight that examine changes by 500 gram bins. Two bars are shown for each birthweight bin, where the first represents the baseline distribution for likely undocumented immigrant mothers and the second shows the estimated change after the policy.[36] As may be seen here, the birthweight distribution appears to have shifted to the right after the policy change, decreasing the number of births with birthweights between 2500 and 3499 grams and increasing the number of births with birthweights of 3500 grams and greater, although not all of these estimates are statistically significant. Figure 4(B) reports estimates for changes in the distribution of gestational length, but we find less evidence of a change here.

Overall, we observe very similar estimates for this set of outcomes among the subgroup of births to Hispanic women.

4.3 Assessing the Mothers' Fixed Effects Design

Are Mothers' Fixed Effects Really Necessary? Our research design relies on a mother's fixed effect to account for non-time varying factors common to siblings. To better understand the importance of this approach for detecting the policy's health benefits, we undertake two additional analyses. First, we show the results of a "naïve" difference-in-differences analysis that estimates changes in outcomes for likely undocumented immigrant women and their infants before and after the policy change as compared to other immigrant women. This analysis estimates equation (2) but replaces the mother's fixed effect with control variables for mother's race and ethnicity, mother's country of birth, and mother's age, which are available on the birth certificate record, as well as county by month-year fixed effects. We use the broad sample of birth certificate records linked to ACS/Census survey data, which has the maternal characteristics needed from those surveys to estimate likely undocumented status as in our main analysis. We do not limit the sample to siblings.

As shown in Figure A11, these estimates indicate lower average birthweights and higher incidence of small-for-gestational age for the infants of likely undocumented immigrant women after the policy change. This perverse "effect" is not surprising given the large changes in the composition of immigrant women over this period that we discuss in Section 1.4. Once we incorporate controls for additional observable characteristics available in the survey data (detailed indicators for mother's coun-

[36] This is estimated by adding the difference-in-differences estimate (and its confidence interval) to the baseline mean.

try of birth and her year of entry to the US), the estimates for birthweight and small-for-gestational age reverse direction, indicating improvements in infant health after the policy change. The pattern of these estimates is similar when we restrict the sample to siblings observed in the linked data. Once we introduce the mothers' fixed effects into the analysis (i.e. our main empirical specification), we see evidence of greater improvements in infant health with larger coefficient estimates for both gestational length and birthweight. This suggests the importance of the mothers' fixed effect in detecting the health benefits of the policy, or that likely undocumented immigrants giving birth during the post-period may still vary in unobservable ways from those who gave birth during the pre-period.

Second, we examine the estimated mothers' fixed effects directly. If these mothers' fixed effects are capturing time-invariant family characteristics that are important to account for in our analysis of the prenatal expansion, they are likely correlated with other determinants of socioeconomic status that we know to be related to birth outcomes. Using the estimates from the difference-in-differences specification for birthweight, we regress the mothers' fixed effects on indicators for the mother's race and ethnicity, county of birth, year of entry to the US, marital status and educational attainment at the time of the Census survey, and county-level income per capita at the time of birth. The results, presented in Table A4, show that the fixed effects estimates are negatively correlated with non-white racial and ethnic groups, later years of entry to the US as compared to before 1982, lower educational attainment, and county-level per capita income. The fixed effects estimates are positively correlated with married status at the time of the survey and certain countries of origin, as compared to the rest of the world. This analysis demonstrates that mothers' fixed effects are related to observable, non-time-varying maternal characteristics that are known determinants of infant health. It is therefore reasonable to expect that they also capture other, *unobservable* characteristics that drive variation in health outcomes across infants, further bolstering the rationale for their inclusion in regression equations (1) and (2).

Does the Within Family Design Generate Spurious Effects? We also show that the use of a within family design does not mechanically generate the findings. Such spurious effects might occur if, for example, there were differential effects of birth order or birth spacing among likely undocumented immigrants and other immigrants. To explore this further, we conduct an additional check where we run a parallel analysis using only birth cohorts born after the policy change.[37] We use the 2010-

[37] We run this exercise using post-period rather than pre-period cohorts since we are only able to identify siblings born in 1983 and later using the Census data. This provides just less than six pre-period cohorts, while we are able to use a full eleven post-period cohorts to mirror the set-up of our main analysis.

2021 waves of the ACS to identify siblings of immigrants born between 1994 and 2004 using the same method for sample construction as used in our main analysis. We estimate the effects of a placebo policy change in October 1998, which allows us to estimate the same number of pre- and post- event terms as in our main model. If our research design generates health at birth "effects" mechanically, we would expect to find significant coefficients in this model. The results may be found in Figure A12. We do not find any evidence of "policy effects" under this exercise, which provides reassurance that neither the mothers' fixed effect approach, nor the method of sample construction, may be responsible for the patterns we observe in our main analysis.

Other Considerations of the Within Family Design? We ultimately conclude that the mother's fixed effects design is necessary to obtain an unbiased estimate of the Medicaid expansion and that this approach is likely to be successful in capturing the impact of the policy we study. However, we also acknowledge that this design has some inherent limitations. First, since we rely on comparisons between siblings born before and after the policy change, we cannot estimate the impact of the policy change on first births. To the extent that prenatal interventions are potentially more beneficial for first births, when mothers have less experience with pregnancy and childbearing, it may be the case that we are understating the benefits of the program on this outcome. Second, differential exposure to the Medicaid program within a family could still have spillover effects that affect the unexposed sibling(s). This could lead us to misrepresent the direct effect of the policy on the targeted sibling(s). Third, our sample is necessarily limited to births in families with two or more children. There are reasons to expect that the characteristics and circumstances of these families may differ from families with only one child. For example, families may only choose to have a second child if the first birth went well. Therefore, our findings will not necessarily represent the experiences of one child families. Future research with an alternative research design is needed to explore this further.

Are The Event Studies Picking Up Compositional Changes? A separate concern about the research design is that the event studies necessarily rely on an "unbalanced" panel since mothers do not give birth in every year. To assess whether this change in the sample of mothers giving birth from period to period might contribute to the patterns we observe in our event study estimates, we follow the approach used in Chyn and Shenhav (2022) and restructure event time as birth order relative to the

timing of the expansion. The specification is as follows:

$$y_{imcbt} = \sum_{\substack{y=-3 \\ y\neq -1}}^{2} \beta_y I(t - \text{Oct. } 1988 = y) \times ProbUndoc_m + \delta_b + \delta_t + \delta_m + \delta_c + \gamma X_{imt} + \theta Z_{ct} + \epsilon_{imcbt}. \quad (3)$$

where b denotes birth order relative to the policy and the event study term (β_y) is omitted for the birth just prior to the policy change.[38] All other control variables are the same as in equation (1).

We present estimates using our main sample in addition to those under alternative balanced sample restrictions in Figure A13. As may be seen here, the specification using our main, unbalanced sample (denoted in blue) presents estimates very similar to our main event studies estimated in calendar time. The remaining estimates on Figure A13 present results from analyses that use three different balanced samples, defined as (1) mothers that have two births during the sample period with one occurring before and one after the expansion (red triangle), (2) mothers that have three births with two births occurring before the expansion and one after (purple square), and (3) mothers that have three births with one birth before and two births after the expansion (green diamond). The estimates for birthweight are very similar to those from the unbalanced sample, providing reassurance that the results are not being driven by the unbalanced nature of our panel. Consistent with our main specification, we continue to find no statistically significant effects in gestation length or indicators that the infant is small for their gestational age.

4.4 Additional Analyses: Exploring Mechanisms

Fertility Changes: We conduct additional analysis to explore whether there were any changes in fertility associated with the policy change. This policy could plausibly affect fertility in two ways. First, to the extent that the additional prenatal care improved maternal and fetal health, it could prevent miscarriages or stillbirths, resulting in more live births for a fixed number of pregnancies. As we show in the next section, these expansions decreased the likelihood of severe complications during pregnancy that could lead to miscarriage. Second, it could affect births through the number of pregnancies carried to term, increasing births even if rates of pregnancy loss did not change. This could occur if Medi-Cal prenatal coverage reduced the costs (both monetary and otherwise) associated with pregnancy and childbirth, making pregnancy more appealing and resulting in more conceptions and/or fewer abortions. Changes on either of these margins could lead to changes in average birth outcomes,

[38] Births that are three or more before the policy are grouped together in the event study terms, while those that are two or more after the policy are grouped together.

depending on the average health of the "marginal" child.

To investigate this, ideally, we would have data on women's conception decisions, abortions, and pregnancy loss over the study period. Given that these type of data are unavailable, we explore fertility responses by evaluating changes in birth rates. In the context of our analysis, the sample is comprised of births with at least one sibling born during the study period. Therefore, any changes in fertility associated with the policy would most directly affect sample composition, and therefore our policy estimates, by changing a mother's decision to have (or the timing) of a subsequent birth.

For this reason, we examine whether, conditional on having an initial birth during the study period, there are changes in subsequent birth rates following the policy change. We construct a panel dataset for every immigrant in the linked birth-ACS/Census records who gave birth during the sample period (not limited to mothers with more than one birth) with information on whether they had a subsequent birth during each month and year following their first observed birth. Note that because we rely on the birth certificate data to measure births, we do not observe births that occur outside of California; if the policy affected cross-state migration, this data limitation may bias our estimates either up or down depending on the direction of such a hypothesized effect. For each of the women in the sample, we estimate the likely undocumented status following the procedure described in Section 3. We then estimate a version of equation (2) that includes the mother fixed effect, month-year fixed effects, and county by year characteristics.[39] The estimated coefficients from this specification provide estimates of changes in monthly birth rates for likely undocumented mothers compared to other immigrant mothers relative to the year just prior to the policy change. For the reasons described above, changes in observed birth rates will capture the net effect of both changes in health during pregnancy and changes in fertility.

As seen in the first column of Table 3, we find a statistically significant increase in subsequent births among immigrant mothers associated with the Medi-Cal expansion (event study estimates may be found in Figure A14). Our estimates indicate a 0.4 percentage point increase in the monthly likelihood of a subsequent birth among likely undocumented immigrant mothers with at least one child. Given the 119,000 immigrant women in the sample, this suggests that the policy led to approximately 9,700 additional siblings during the post-period if we apply the median value of $ProbUndoc_m$ of 0.34 and total the increased number of births per month over the 5 years following the policy (i.e., $119,000 \times 0.34 \times 12 \times 0.004 \times 5$). These policy-induced births would then represent roughly 7 percent

[39] Note that we do not control for the characteristics of the initial birth or county fixed effects since the equation includes the mother's fixed effect.

of our 132,000 siblings sample and 16 percent of the subset of 59,300 siblings born before and after the policy change (i.e. the identifying variation in the difference-in-differences model).[40]

If these births are more likely to be healthier than the average birth among this population, then this increase in childbearing is one potential mechanism behind the improvements in birth outcomes documented under the policy change. To examine how much of our policy effect might be attributable to this mechanism, we conduct a bounding exercise that assumes that all marginal births receive prenatal care and have higher than average birth weight (3500 grams).[41] This procedure implies that selection due to changes in childbearing could explain between 41 to 45 percent of the policy effects observed for prenatal care usage and birthweight. It is important to note that these are upper bounds that assume all marginal births are maximally healthy and receiving prenatal care; in reality, marginal births may in fact be negatively selected (e.g. if they result from averted miscarriages that lead to less healthy infants). Therefore, changes in fertility could be an important mechanism, but do not appear to be the sole mechanism underlying the effect of Medicaid expansion for some of the key outcomes we consider.

Maternal Health During Pregnancy: We next examine the presence of any pregnancy complications as reported on the birth record. Our measure of complications includes pregnancy-specific complications (placenta previa, pre-eclampsia or pregnancy induced hypertension, hemoglobinopathy, kidney infection/pyelonephritis, anemia, and transport of mother from another facility prior to delivery) and pregnancy complications related to chronic diseases (chronic hypertension, cardiac disease, diabetes, lung disease, rubella, Rh sensitization, uterine bleeding before labor, and renal disease).[42] Changes in these types of pregnancy complications might indicate either changes in the diagnosis of or the prevention of conditions that might impact health during pregnancy and infant development.

We also find evidence of a decrease in pregnancy complications for likely undocumented women following the policy change (Table 3 and Figure A14). These findings suggest that improved access to prenatal care translated into better health during pregnancy. Unfortunately, the birth certificate record

[40] 44.93 percent of the 132,000 observations in our siblings sample are sibling sets that include siblings born before and after the policy change.

[41] We assume there were no policy effects other than the change in selection. We use the fact that marginal births represent 16 percent of births among sibling sets with siblings born before and after the policy change. We calculate the pre-post policy change among likely undocumented immigrants as $0.16 \times (\bar{V} - \bar{Y})$, where $\bar{Y}$ is the pre-policy average for likely undocumented immigrants and $\bar{V}$ is our imposed "healthy" outcome (receiving prenatal care, or birth weight of 3500 grams). We then compare this "selection effect" to the policy effects estimated in Tables 1 and 2.

[42] This measure was constructed in consultation with Dr. Priya Batra, an obstetrician-gynecologist, who helped to review medical data worksheets available on the birth records and identify pregnancy complications that were consistently captured over the study period.

did not collect any additional information during this time period that might provide further insights into changes in maternal behaviors during pregnancy, such as nutrition, smoking, or drinking, nor does it provide information on participation in other public programs.

4.5 Later Life Health and Human Capital

The first cohorts who gained *in utero* eligibility under this policy change are now in their early thirties. We are, therefore, able to examine the longer-term effects of this early health intervention. The event study estimates are reported in Figure 5 and the difference-in-differences estimates in Table 4.

The event study estimates, for many of the outcomes, do not show compelling evidence of a change for the children of likely undocumented immigrants born after the policy change. However, Figure 5(b) shows some evidence of a significant increase in post-secondary school enrollment. In addition, Figure 5(h) shows how the expansions affected Medicaid receipt in childhood. We might expect these expansions would increase enrollment in Medicaid if they improved parents' familiarity with the program. There would need to be different parental enrollment decisions across siblings, however, in order to be detected with the mother's fixed effect design. Alternatively, if the improvements in infant health translated to improved health in childhood, parents may be less likely to enroll their children.[43] We find that the latter mechanism appears to outweigh the former, as Figure 5(h) shows a significant decrease in childhood Medicaid receipt. There is also some evidence of a decrease in any fertility experienced by age 26 for cohorts born later in the study period, accompanied by a decrease in the EITC benefit amount. We do not find evidence of a changes in cumulative mortality, teen fertility, or adult wages.

The difference-in-differences estimates in Table 4 mirror the patterns shown in the event study figures. We find a significant increase in post-secondary enrollment and a significant decrease in childhood Medicaid averaged over the entire post-period. We find a 4.2 percentage-point increase in post-secondary school enrollment among children of likely undocumented immigrants (approximately a 7 percent increase over baseline enrollment of 63 percent). We also find that childhood Medicaid participation declines by 3.1 percentage points, an 8 percent increase over baseline participation at about 40 percent. The estimates for any fertility and EITC amount are negative in sign but only the EITC estimate is marginally significant. It suggests a decrease in the annual benefit amount of $64, about a 7 percent decline over the baseline benefit amount.

[43] For example, many Medicaid beneficiaries are enrolled at the point they receive care (e.g., at an emergency department); if treated cohorts' need for care is less acute, they may be less likely to connect to this program.

We observe a similar estimate for post-secondary enrollment among the subgroup of births to Hispanic women, and a slightly smaller reduction in childhood Medicaid benefit receipt (Panel B). We do not find as compelling of evidence of a change in fertility or EITC benefit amount, with smaller estimates on these outcomes for this subgroup.

Overall, these results provide evidence that children of likely undocumented immigrants experienced meaningful changes in medical care use while *in utero* and better pregnancy outcomes as a result of the prenatal eligibility expansion. We find evidence suggesting that these effects translate into better outcomes later in life. These children appear to be less reliant on Medicaid during childhood, perhaps indicating they may be in better health, and are more likely to pursue post-secondary education. We also find some evidence of a reduction in EITC receipt later in adulthood. Together, this indicates that expanding access to health care *in utero* may have put these cohorts on a trajectory of improved economic and health outcomes that not only extends beyond the period of initial investment, but that may continue to yield benefits in the future.

5 Sensitivity Analyses

5.1 Alternative Specifications and Samples

To assess the sensitivity of our results to alternative specification and sample criteria, we present the results of several additional analyses in the difference-in-differences format in Figures 6-8. The first coefficient and confidence interval in each figure (in red) presents the difference-in-difference estimate from our main specification (equation (2)). Event study results for each of the analyses described below may be found in Figures A15-A17.

We first examine the sensitivity of our results when we cluster the standard errors by county, also estimated using a bootstrapping procedure, rather than at the individual mother level. Nearly all results remain statistically significant with this change, with the exception of the decrease in other sources of insurance coverage and the increase in any prenatal care use.

In the next check, we assess whether our results are driven by the expansion of prenatal coverage to undocumented mothers, or whether, alternatively, the changes we observe could be explained by the subsequent Medicaid expansions based on income that occurred shortly after the policy we focus on. Ideally, we could control for an individual's eligibility for Medicaid under income-based rules and thus isolate the variation in eligibility due only to changes in eligibility based on undocumented status. However, this is not possible because we do not have measures of individual income. Instead, we

assess the robustness of our findings to controlling for the change in eligibility following the income-based expansions at the level of the mother's county of residence. To account for the fact that the undocumented group may be more likely to benefit from these income-based expansions (due to their lower income), we calculate these county-level measures separately based on undocumented status, then use these aggregated eligibility measures to control for changes in eligibility for Medicaid under the income-based expansions. See Appendix Section D for more details. As shown in the figures, the inclusion of these measures of exposure to the income-based expansions does not noticeably change the point estimates.

We next examine estimates when we include controls for the length of time between births in the regression specification. If the undocumented expansions changed the timing of births and led to differential birth spacing between likely undocumented and other immigrants, this could be one mechanism driving the changes in birth outcomes observed under the policy. To investigate this, we include controls for time since the prior birth (less than 18 months, 18-23 months, 24-35 months, 36-47 months, 48-59 months, and 60+ months) and interact them with the birth order dummies in equation (2). In this specification, first births are the omitted category and all subsequent birth order dummies are interacted with the birth spacing categories. We find very similar results under this specification.

We next re-estimate our main analyses applying weights to account for differential sampling rates in the Census survey data. As described in Section 2, our analysis sample is necessarily limited to children who were born in California during the study period and who also lived in the U.S. at some point between 2000-2011 in order to be surveyed by the Census Bureau. In addition, to be included in our analysis, it also had to be the case that their family was sampled and responded to either the 2000 Census or 2001-2011 ACS. The analysis of our linked data, therefore, does not provide any information on program effects for children who left the U.S. during early childhood, or for other types of families not captured in the survey data.

While the impacts for these missed groups are unknowable without additional sources of data, we are able to take advantage of the fact that we observe the population of California births–not just survey respondents–in our birth certificate data. Using the data on all California births, we are able to estimate what the overall program effects would be if the effects are similar for all children in the state with a given set of observable characteristics, which we select based on their availability for all birth records. The weights are equal to the inverse ratio of the number of individuals in our linked sample to the population of births in the state in each cell defined using birth year, birth month, birth

order, mother's ethnicity and race, mother's country of birth, sex, plurality, mother's age, and county of residence. As seen in the figures, the estimates are similar when we apply these weights, although we do estimate slightly larger increases in birthweight and gestational length, with the latter estimate now significant at the 5 percent level, as well as post-secondary enrollment.

Next, we explore the potential role of concurrent immigration reforms during our study period (see timeline in Figure A10). Importantly, the Immigration Reform and Control Act (IRCA) became law in November 1986 and created an amnesty program for some undocumented immigrants to become legal U.S. residents.[44] Individuals who had been living continuously in the U.S. since January 1, 1982, as well as special agricultural workers who had been employed in the U.S. for 90 or more days between May 1985 and May 1986, were eligible to apply for legalization (Norton et al., 1996). Later in 1990, the Immigration Act (IMMACT) extended protections to the family members of certain legal immigrants, including these newly legalized IRCA immigrants.[45] If these reforms changed a mother's legal status and/or those of other family members (such as her spouse), this may have had effects on her well-being, as well as the well-being of her off-spring, as recently documented in (Cascio et al., 2024).[46]

We conduct two separate sensitivity analyses to examine whether our estimates confound the effects of legalization under these reforms. First, we incorporate a strategy used in prior work in order to control for the effects of IRCA. Following Cascio and Lewis (2019) who studied the effects of IRCA on income tax filing and EITC receipt, we construct local IRCA application rates using data from the U.S. Immigration and Naturalization Service Legalizations Applications Processing System (LAPS), available from the National Archives, and population estimates for working age adults in 1986 from the U.S. Census. We construct these measures by county and merge this information onto our analytic dataset using county of birth and exclude observations for which county is suppressed in the LAPS data.[47] We then re-run the main difference-in-differences specification but include the

[44] In addition to IRCA, there were a number of changes to social policies throughout the 1990s. These policies could generate bias if they differentially affected changes in outcomes across siblings born before and after October 1988 in households with different predicted undocumented status. However, most policies we identified likely affected siblings in the same family and families with different documentation status similarly. We investigate IRCA in particular as an example of a policy that may have had such confounding effects.

[45] The 1990 Immigration Act provided protection from deportation and work authorization for spouses and unmarried children who were related to legal immigrants, including newly legalized IRCA immigrants, and who entered the United States without legal status prior to May 5, 1988. The law also provided immigrant visas for the spouses and children of legalized immigrants who had attained permanent resident status (Guendelsberger, 1992).

[46] Cascio et al. (2024) find higher birthweights for infants born to foreign-born Mexican mothers associated with higher local IRCA application rates. Their analysis excludes California to avoid confounding the effects of the Medi-Cal expansion to undocumented immigrants.

[47] County is suppressed in the LAPS data for applicants in counties with under 100,000 or fewer than 25 applications.

county IRCA application rate interacted with a post-IRCA indicator for births following 1987. We also run a second version of this specification that interacts this IRCA control variable with the mother's predicted undocumented status, similar to how we interact the Medicaid expansion indicator with her predicted undocumented status. The results from both versions of this sensitivity check are labeled "IRCA application controls" and "IRCA controls x Prob. undoc" in the figures. Overall, our key results do not change with the inclusion of these controls. We still estimate significant increases in Medi-Cal coverage, prenatal care, and birthweight. For long-term outcomes, we continue to see a marginally significant increase in post-secondary enrollment and a significant decrease in childhood Medicaid participation. As in our main results, there is also some evidence of a decrease in EITC benefit amounts.

In the second analysis, we use newly linked data on Social Security Number (SSN) applications for mothers in our sample and their spouses to drop immigrants whose families directly benefited from IRCA and later family reunification policies. Using individual-level information on the date of the first application for an SSN from the Census Numident, Figure A18 provides descriptive information on annual applications and cumulative applications for the immigrant mothers in our sample, as compared to U.S.-born mothers who meet the same sample criteria. Second versions of each plot also depict this information for immigrant mothers in our sample by their predicted undocumented status.

As may be seen in these plots, there is a large spike in annual social security applications among immigrant mothers between 1987 and 1989, but no similar change for US-born mothers. This timing aligns with the requirement that qualified applications apply for IRCA in 1987 and 1988 (Library of Congress, 2024; 8 U.S.C. §1160, 1986), as well as documentation from the Social Security Administration indicating that the agency worked alongside the Immigration and Naturalization Service to assign social security numbers to the large number of people who were applying for legalization (Long, 1993).[48] Of interest, the spike in annual social security applications occurs among immigrant mothers with both higher and lower predicted probabilities of undocumented status. However, as seen from cumulative applications, the baseline prevalence of applications is much higher among those mothers we estimate to have low probabilities of undocumented status (58 versus 13 percent in 1986). This serves as additional validation that our estimate of likely undocumented status is doing a reasonable job of identifying mothers who are more and less likely to have this status.

[48]IRCA also required employers to fill out I-9 forms and submit certifications of citizenship or work authorization after 1986. This also led to an increase in applications among individuals who were legally in the U.S. and wanted work-authorized SSNs. The analysis that follows, by excluding individuals with new applications during 1987 to 1989, will also exclude these individuals.

In this analysis, we exclude mothers who initiated a new SSN application during the 1987-1989 years, as well as those whose spouse identified in the linked ACS/Census data initiated a new SSN application during those years. Our results are very similar with this sample exclusion, although less precise due to a smaller sample size. Of note, the estimates for birthweight and childhood Medicaid remain statistically significant. The estimate for post-secondary enrollment is smaller in size and not significant, but remains positive in sign suggesting increased enrollment. Overall, these analyses indicate to us that the concurrent immigration reforms during this period are unlikely to explain the effects we are attributing to the Medi-Cal expansion.

The last column in Figures 6-8 (difference-in-differences) depicts the results when we drop multiple births from the analysis sample. This change does not affect our results.

The last column in Figures A15-A17 (event studies) explores the sensitivity of our results to excluding mothers whose births occur entirely in the post-period. When treatment effects are heterogeneous, event study coefficients capture a weighted average of treatment effects of all post-treatment cohorts. In a scenario where some cohorts only appear in the post-period (such as mothers in our sample whose births occur entirely after the prenatal expansion policy), some of these weights can be negative (Hagemann, 2025). When we drop births to mothers who only appear in the post-policy era, we find similar (although somewhat less precise) estimates as in our main sample, suggesting that treatment effect heterogeneity is likely mild in our setting. Note that this negative weights issue does not arise in the difference-in-differences specification.

Finally, we re-run the analyses using an alternative method to impute likely undocumented status. A number of studies examining the undocumented population have focused on Hispanic individuals with low educational levels, such as Amuedo-Dorantes et al. (2018, 2022) and Bellerose et al. (2025), given that these are common characteristics of the undocumented population (Millet and Pavilon, 2022). Specifically, we follow Amuedo-Dorantes et al. (2022) and consider a mother undocumented if she was born in Mexico or Central America, is Hispanic, and has at most 12 years of education.[49] In this specification, we substitute this binary measure of likely undocumented status for the predicted probability of undocumented status in equations (1) and (2). We also merge on new measures of Medi-Cal eligibility by month and year and likely undocumented status estimated using this new definition. The results may be found in Table A5 and Figures A19-A21. The first stage analysis indi-

[49]Note we use information on mother's educational status at the time of the ACS/Census survey since the mother's education is not consistently available on the birth certificate record during our study period. Information from the ACS/Census, however, may not exactly align with her educational attainment at the time of the birth.

cates that this is likely a worse proxy for undocumented status than our primary measure. In Table A5, we find a 21 percentage point change in Medi-Cal eligibility associated with the policy for mothers meeting this definition, as compared to other immigrant women, which is much smaller than the 55 percentage point change estimated in our main specification. We also see smaller estimates of changes in prenatal insurance coverage (a 15.5pp increase vs. a 29.8pp increase). For the most part, however, the findings are generally consistent with our main results, although the size of the estimates may differ. They show improvements in birth outcomes, increased post-secondary enrollment (marginally significant), a reduction in fertility, and a reduction in childhood Medicaid receipt and average EITC benefit amounts. There is also evidence, under this specification, of a significant decrease in later wages. Seeing similar signals in the data using this approach increases our confidence in our findings.

Taken together, these analyses show that our results are broadly robust under a large number of alternative specifications and sample definitions. Furthermore, it seems unlikely that other concurrent policies are driving the changes in outcomes we observe, supporting our interpretation of these estimates as capturing the causal effects of the expansion of prenatal Medicaid coverage to undocumented pregnant women.

6 Discussion

Our evidence shows that expanded Medi-Cal coverage to pregnant undocumented immigrants led to increased insurance coverage, prenatal care use, and better pregnancy outcomes. We also find suggestive evidence of longer-term improvements in well-being for the cohorts who were *in utero* with higher educational achievement and less reliance on public support programs.

To better understand the magnitude of our reduced form estimates, we can scale the changes in prenatal care and infant health by the corresponding change in prenatal coverage under the policy. We use our estimate of a 25.2 percentage point increase in Medi-Cal prenatal coverage to conduct this scaling.[50] Applying this scaling implies that newly enrolled immigrant women were 5.6 percentage points more likely to use any prenatal care and increased their number of prenatal visits by 3.8 visits, on average. These changes translated into higher average birthweights of 237 grams (a 7 percent increase over baseline). In terms of longer-term outcomes, we estimate that the children of newly enrolled immigrant women were 16.7 percentage points more likely to enroll in post-secondary school (a 26.5 percent increase over baseline). They were also 12.3 percentage points less likely to receive

[50]We note, however, that this may understate the effect of the policy on coverage (and thereby overstate the effect of coverage on outcomes) given that it is measured relative to a partially-treated year.

childhood Medicaid at ages 16-18, a 32.5 percent decrease over baseline.

6.1 Are Effects of Prenatal Eligibility Different Among Undocumented Mothers?

Nearly all prior work on expansions of prenatal Medicaid focused on income-based expansions that excluded undocumented immigrants. Given the different economic, social, and cultural position of undocumented immigrant mothers, the effects of prenatal coverage expansions could differ substantially for this group. For instance, the benefits of expanded coverage for undocumented immigrant families, who have lower family incomes and higher rates of uninsurance than U.S.-born families (Fortuny et al., 2007), could be larger than for non-immigrants. However, limited English language proficiency, confusion around program eligibility rules, or fears about repercussions for using public benefits are potential barriers to enrollment (Feld and Power, 2000). In addition, language-, cultural-, and discrimination-related challenges to navigating the health care system and receiving quality care (Flavin et al., 2018) are additional reasons that expanded access may not lead to better outcomes.

Most directly related to our results are two papers that examine the impact of income-based prenatal Medicaid expansions on average birthweight. Dave et al. (2008) examine national prenatal expansions between 1985 and 1999 and find a 35.9 gram increase in average birthweight for the children of women gaining coverage. Meanwhile, East et al. (2023) look at slightly earlier expansions in prenatal Medicaid (1980 to 1985) and their estimates imply an average increase of 13 grams, although these estimates are imprecise and not statistically significant.[51] In contrast, we find that birthweight increases by 237 grams for each newly covered mother, many times larger than these previous estimates.[52] This difference is notable given that the prior literature also suggests that these income-based expansions had large effects on prenatal care utilization. The estimates in Dave et al. (2011) indicate that newly covered mothers had 10.5 additional prenatal visits, while analyses in Currie and Gruber (1996b) find that newly eligible mothers were half as likely to delay prenatal care initiation beyond the first trimester. This suggests that access to prenatal care may have a larger impact among undocumented immigrant mothers, or that other benefits of Medicaid enrollment are particularly important to this population - such as the financial benefit or potential knowledge gained of social supports such as WIC. Notably, California's Medicaid program offered enhanced psychosocial services and supports to pregnant enrollees not available in many states, possibly leading to greater benefits of expanded eligibility in this state relative to what has been documented at the national level.

[51] As these authors present event study estimates only, we calculated this by taking the average of the post-expansion effects for birthweight and scaling them by the corresponding average of post-expansion effects for Medicaid coverage.

[52] Note that this may be an overestimate given the limitations of our first stage data; see section 2.1.

At the same time, many previous studies of income-based prenatal coverage expansions focus their analyses on the incidence of low birthweight. These papers find mixed evidence regarding impacts on low birthweight, with any effects concentrated among the most disadvantaged subgroups (e.g. Currie and Gruber, 1996b; Epstein and Newhouse, 1998; Dubay et al., 2001; Levine and Schanzenbach, 2009; Dave et al., 2008; East et al., 2023). In contrast, we find most of the gain in birthweight occurs towards the middle of the distribution, with the probability of having an infant in the lowest birthweight category essentially unaffected by the policy (see Figure 4). One notable difference between our study and these existing analyses is our use of a mothers' fixed effect design that relies on comparisons between siblings born before and after the policy change. Notably, our birthweight results are consistent with the patterns observed in studies estimating changes in access to WIC (Rossin-Slater, 2013) and prenatal care in Chile (Clarke et al., 2020), also estimated using sibling comparisons.

Finally, the one study of the long-term effects of income-based prenatal Medicaid eligibility expansions found improvements in high school graduation but did not detect changes in college enrollment or attendance (Miller and Wherry, 2019). While we do not observe high school graduation in this study, we do find a significant increase in post-secondary school enrollment. This does not appear to translate into higher college graduation rates but may lead to higher attainment of two-year degrees or other coursework that further improves the human capital of these young adults.

6.2 Benefits and Costs

To quantify the short-run costs relative to benefits observed under the Medi-Cal expansion, we calculate the cost per gram of birthweight gained. Norton et al. (1996) report that the cost of Medi-Cal prenatal services was approximately $480 per pregnancy in 1991,[53] which translates into roughly $1,000 today. Combined with the estimated change in birthweight among new enrollees of 237 grams, this indicates that the cost per gram of birthweight for the Medi-Cal expansions was just over $4. This estimate is less than the amounts for two different early health interventions presented in Clarke et al. (2020). The authors calculate $11 per gram under a Chilean prenatal program and $14-15 per gram for the WIC program in the US, based on estimates from Rossin-Slater (2013); Hoynes et al. (2011).

We next consider the long-run benefits of the Medi-Cal expansion, both in terms of government savings and the value to the recipients. For each individual who gained Medi-Cal coverage while *in utero*, we estimate government savings of $566 in 2022 dollars from reduced participation in Medicaid

[53] This estimate excludes the cost of labor and delivery.

at ages 16 to 18, when we discount to the time of birth (i.e. the initial investment).[54] We calculate additional discounted savings from reduced EITC receipt at ages 25 to 27 of $384, although we note that the estimate of this effect was only statistically significant at the 10% level. Combined, these later savings ($950) nearly offset the cost of the initial Medi-Cal benefit ($1,011). This calculation does not include potential savings at other ages not included in our analyses.

The higher rate of post-secondary school enrollment is expected to lead to higher lifetime earnings for recipients, also leading to higher tax revenue for the government. We do not yet observe this change at the ages for which we observe earnings (ages 23-28). However, estimates of lifetime earnings by educational attainment indicate that some college results in $1.9 million in cumulative earnings over the ages of 25 to 64, as compared to $1.6 million for a high school graduate (in 2009 dollars, Carnevale et al., 2021). Combined with the 16.7 percentage point increase in post-secondary enrollment among Medi-Cal recipients, we might, therefore, expect $13,827 in additional after-tax earnings and $3,222 in additional tax revenue for the government (2022 $) for each Medi-Cal recipient, if we follow Hendren and Sprung-Keyser (2020) and assume an effective tax rate of 18.9 percent, and discount back to the time of birth.[55] We can then net out the additional costs associated with the estimated 16.7 percentage point increase in post-secondary school enrollment among recipients, both in terms of their private costs and costs to the government.[56] Once we take into account these increases in educational expenses, the net increase in earnings is $9,514 and the net increase in government revenue is $178 for each Medi-Cal recipient.

These calculations suggest that the ratio of the value of the program to beneficiaries over their lifetimes to the net cost of the program to the government (i.e. the "marginal value of public funds" (MVPF), see Hendren, 2016; Hendren and Sprung-Keyser, 2020) will be infinite. That is, net government spending will be negative over the lifetimes of recipients (i.e. the program is cost-saving).[57] This conclusion is consistent with previous calculations of the MVPF for Medicaid prenatal coverage under

[54] This calculation uses the average annual Medicaid payment for a non-disabled child of $1,627 in 2004 (when the 1988 cohort is 16 years of age) from Centers for Medicare & Medicaid Services (2007), applies the implied 12.3 percentage point decrease in Medicaid enrollment among recipients, and discounts to the time of birth using a 3 percent interest rate.

[55] Note, this is a conservative estimate since it assumes that individuals would have graduated high school or received a GED otherwise and ignores the possibility of acquiring an associate's degree or other degrees after enrolling in post-secondary education, for which lifetime earnings are even higher. The estimates of lifetime earnings from Carnevale et al. (2021) do not apply a discount rate. We, therefore, discount the difference in lifetime earnings between the two degrees back to the time of birth calculated as of age 45, which is halfway in the age period used by the authors to compute lifetime earnings.

[56] We use the OECD estimate of total annual expenditure per student for tertiary education in the US in 2004 ($24,074), assume 2 years of attendance, and apply the shares of public vs. private spending from education in the US in 2004 from Organisation for Economic Co-Operation and Development (2006). We discount back to the time of the initial investment.

[57] Our calculations here do not differentiate between state or federal spending.

income-based expansions in Hendren and Sprung-Keyser (2020). We have not considered here, however, the potential expense to the government of any additional births that were induced by the policy change, as documented in Section 4.4. It is difficult to know the net fiscal costs associated with these births, but existing evidence indicates that second generation immigrants use similar levels of government benefits but pay more in tax revenues than the US-born population overall (National Research Council, 1997; National Academies of Sciences, Engineering, and Medicine, 2017).

Finally, we note that the value of Medi-Cal coverage to recipient families is likely much larger than what is calculated here, and observed in our data, and include other meaningful improvements in quality of life and well-being over their lifetimes. These benefits may even extend to future generations, similar to the pattern observed under prenatal Medicaid expansions that excluded undocumented immigrants (East et al., 2023).

7 Conclusion

One out of every thirteen births in the United States is to an undocumented immigrant. But despite their large presence, in most states, pregnant undocumented immigrants do not qualify for Medicaid coverage of routine prenatal care during pregnancy. In this paper, we demonstrate the impact of the removal of this exclusion from publicly-funded health services on insurance coverage, the use of care during pregnancy, and the health and well-being of the resulting infants, who are themselves U.S. citizens by birthright.

To investigate this question, we take advantage of a dramatic expansion of eligibility for prenatal Medicaid coverage that occurred in California in 1988. After the policy went into effect, nearly half of all births paid for by the state's Medicaid program were to undocumented immigrant mothers (Norton et al., 1996). And, as one of the first states to expand eligibility to this population, the study of California's expansion offers a unique opportunity to examine the long-term effects of Medicaid coverage for these families and their children.

We use a novel dataset that links the universe of California birth records to Census survey and national administrative data, allowing us to overcome several empirical challenges in our setting. Using this newly linked data, we are able to identify family linkages for children and their families observed in the Census survey data. This enables us to take advantage of variation in exposure to the policy of children born to the same mother before and after the expansion occurred, as compared to births occurring entirely before or after the expansion was implemented. This within-mother approach is

crucial in our setting, as we observe large changes in the composition and number of immigrants over our sample period. We also take advantage of detailed maternal characteristics from the linked survey data to identify likely undocumented mothers, who were targeted by the expansion, and compare them to other immigrant mothers in order to net out secular changes in outcomes over time.

We find that the expansion of prenatal coverage to undocumented immigrant mothers significantly increased use of health care by the mother in the prenatal period, and significantly increased the birthweight of their children. We also find some evidence suggesting that the cohorts who benefited *in utero* go on to achieve a higher level of education and are less reliant on government support programs, although our estimates of long-term effects are somewhat noisy. Calculations based on our estimates suggests that, over the long-run, the government likely recoups its initial investment. Our results demonstrate that expanding prenatal Medicaid eligibility to undocumented immigrants has a significant impact on the health and economic outcomes of the next generation of Americans.

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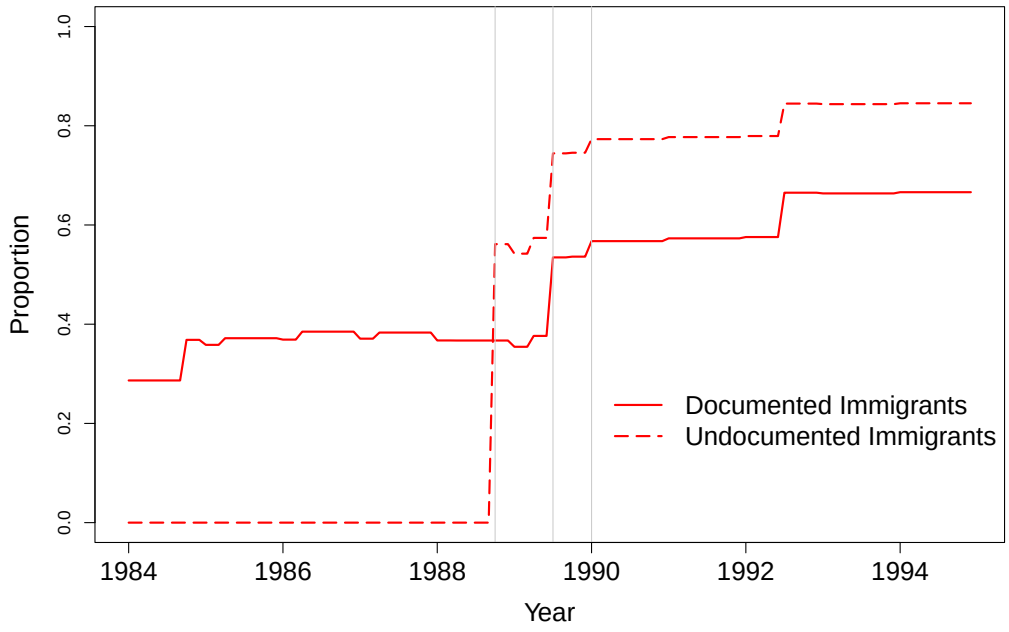
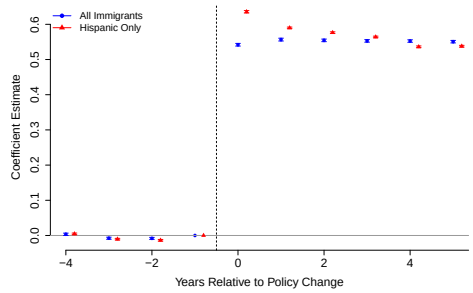
Figure 1: Pregnancy Medi-Cal Eligibility Among Immigrant Women of Reproductive Age by Documentation Status, 1984-1994

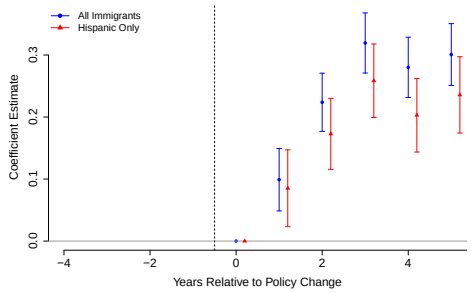


Note: Vertical lines at October 1988, July 1989, and January 1990 depict the dates of the undocumented expansion and two income-based expansions in Medi-Cal eligibility, respectively. Eligibility for immigrant women who are state residents and ages 15-44 in the event of a pregnancy is estimated using federal and state eligibility rules and inflation-adjusted income information from the 1990 Census for each month and year. Immigrant documentation status is imputed using individual characteristics reported in the Census and an adaptation of the Borjas (2017) algorithm. Details on eligibility rules and the documentation status imputation may be found in Appendix Sections A and B.

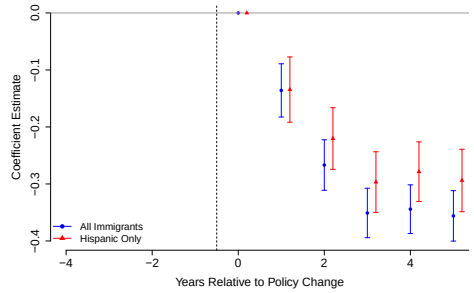
Figure 2: Effects of the Undocumented Expansion on Medi-Cal Eligibility, Insurance Coverage, and Prenatal Care



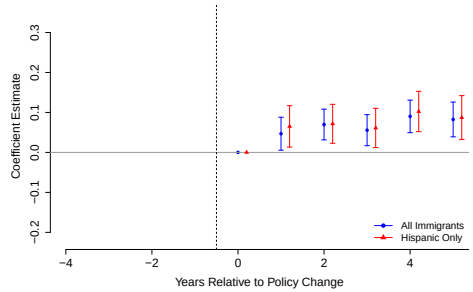
(a) Medi-Cal eligibility



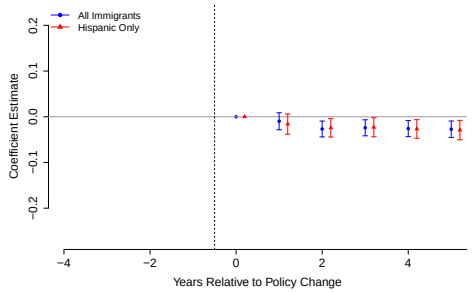
(b) Medi-Cal



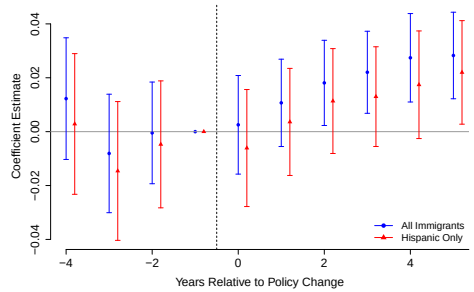
(c) No insurance



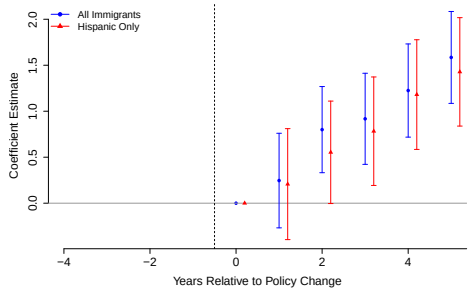
(d) Private coverage



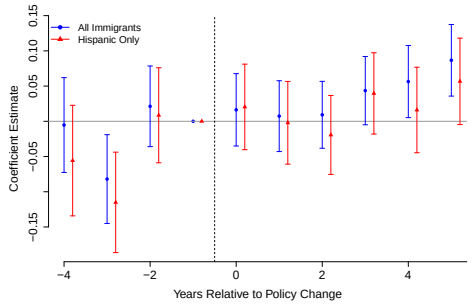
(e) Other coverage



(f) Any prenatal care



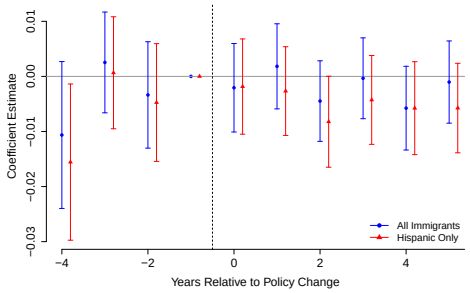
(g) Number of prenatal visits



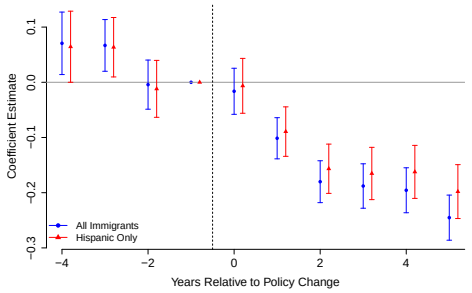
(h) Prenatal care in first trimester

Note: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Medi-Cal eligibility is estimated using a monthly county-level simulated eligibility measure constructed using the 1990 Census and merged on using mother's county of residence, month-year of birth, and predicted documentation status; see text for more details. Coefficients and confidence intervals are estimated using the event study specification (equation 1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Regression models for prenatal insurance coverage and number of prenatal visits estimate changes in outcomes relative to the first year the policy was in place, due to the limited period of data available. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

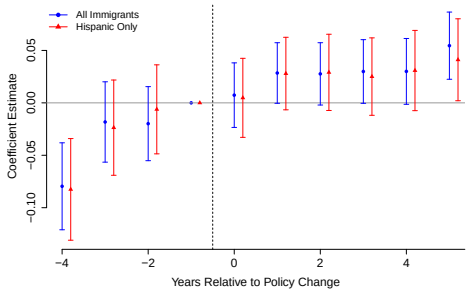
Figure 3: Effects of the Undocumented Expansion on Delivery Care and Infant Health



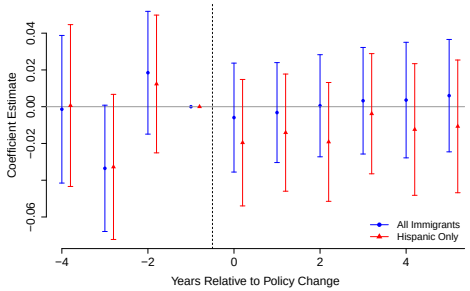
(a) Hospital delivery



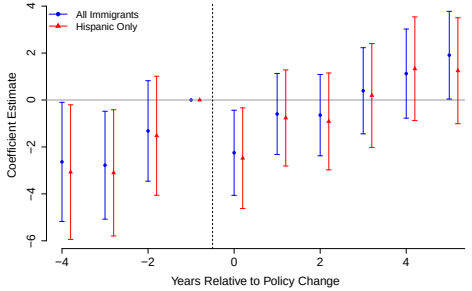
(b) Public hospital



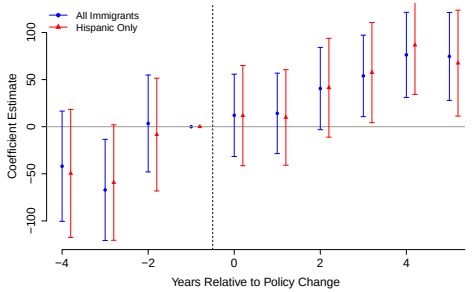
(c) Doctor delivery



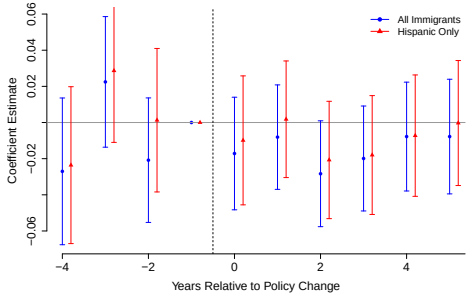
(d) C-section



(e) Gestation age (in days)



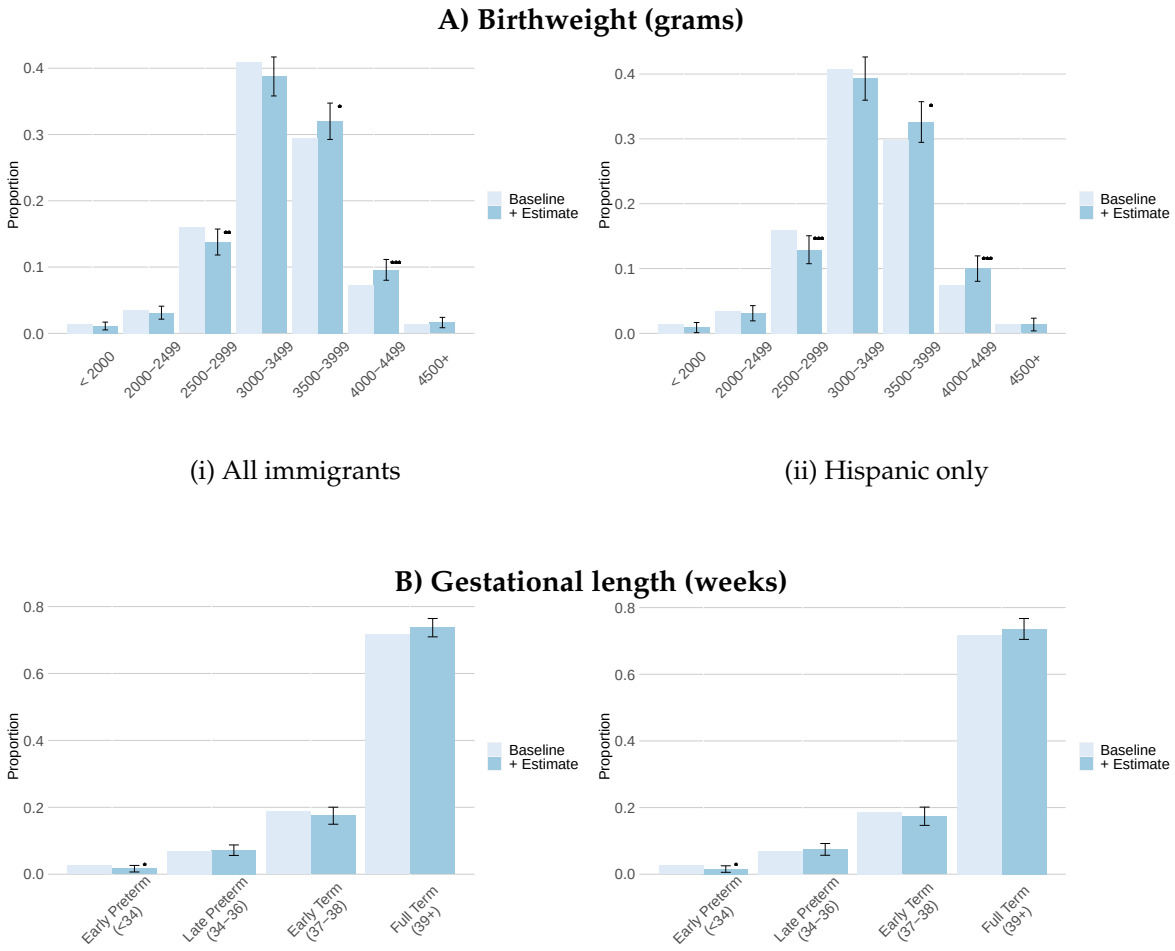
(f) Birthweight (in grams)



(g) Small for gestational age

Note: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Coefficients and confidence intervals are estimated using the event study specification (equation 1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

Figure 4: Effects of the Undocumented Expansion on Distribution of Birth Outcomes

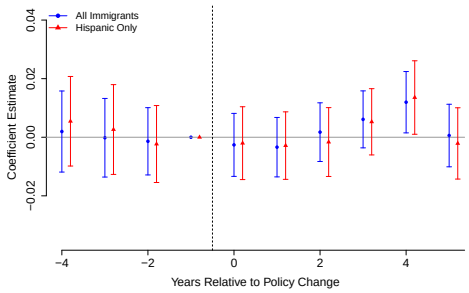


(i) All immigrants (ii) Hispanic only

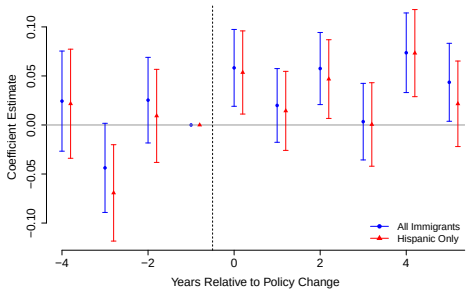
(i) All immigrants (ii) Hispanic only

Note: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Estimates for the post-period calculated by using the baseline mean for likely undocumented immigrants and adding the difference-in-differences estimate (estimated using equation (2) and reported in Table 2) and its confidence interval. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-183, CBDRB-FY22-CES018-007, and CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

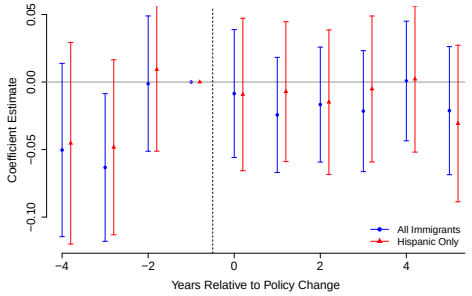
Figure 5: Effects of the Undocumented Expansion on Long-Term Health and Human Capital



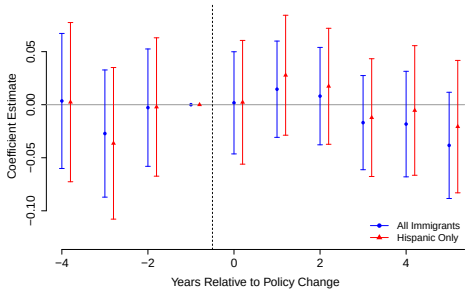
(a) Cumulative mortality



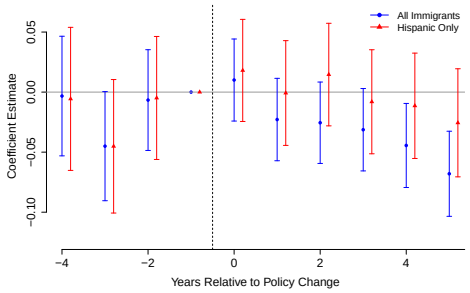
(b) Post-secondary enrollment



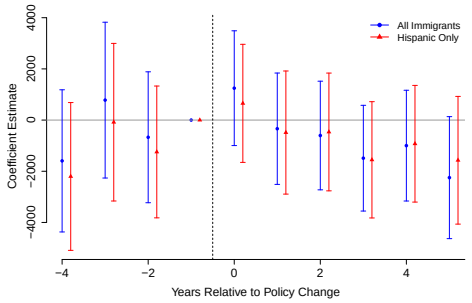
(c) College completion



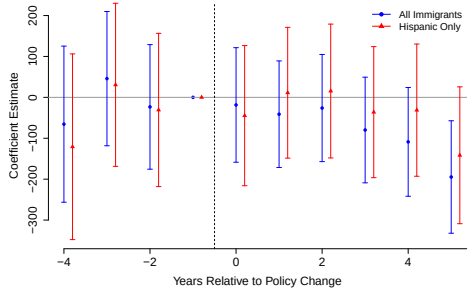
(d) Teen fertility



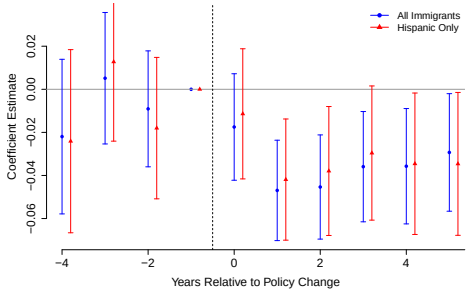
(e) Any fertility



(f) Annual wages



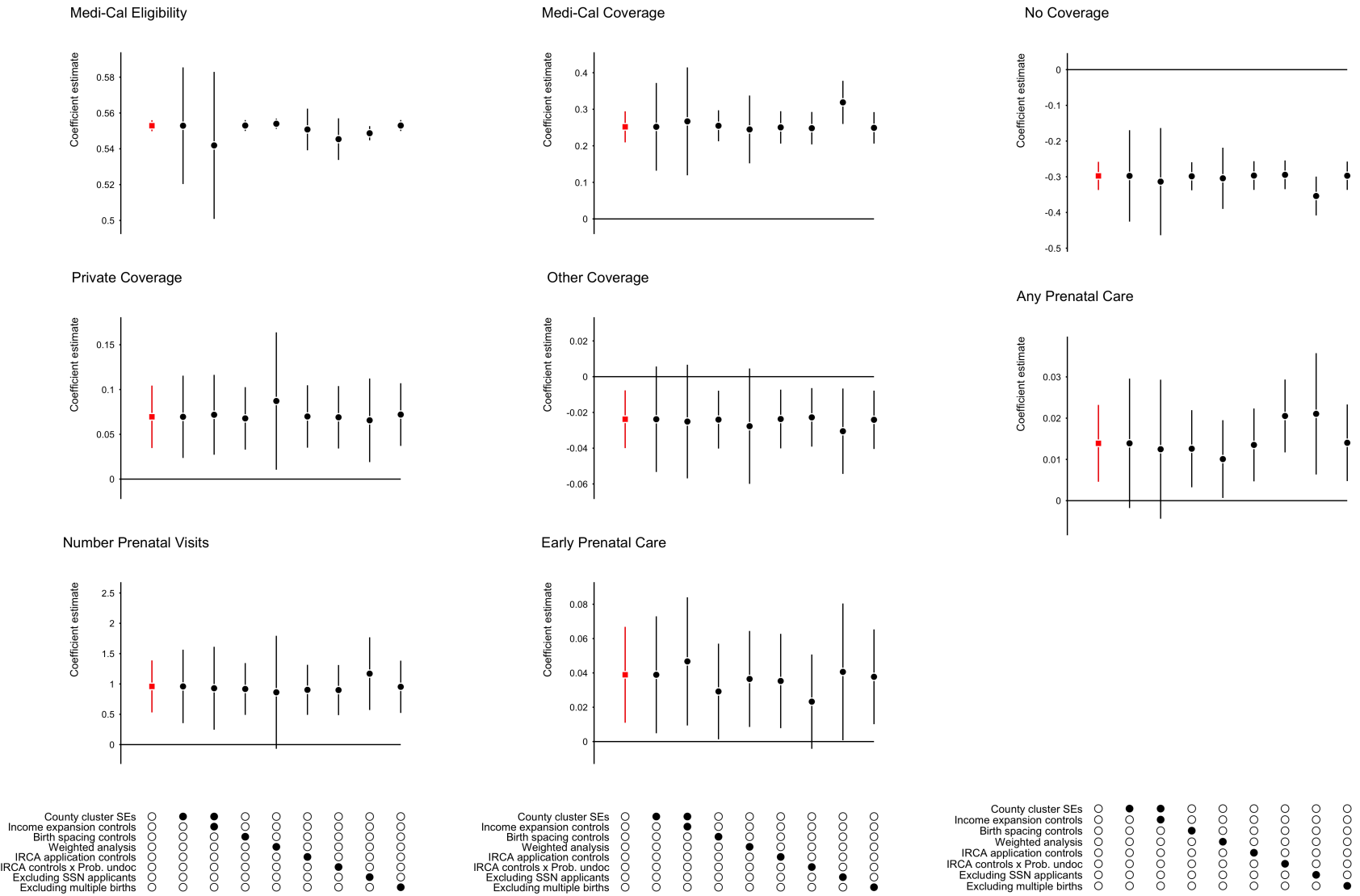
(g) EITC amount



(h) Childhood Medicaid

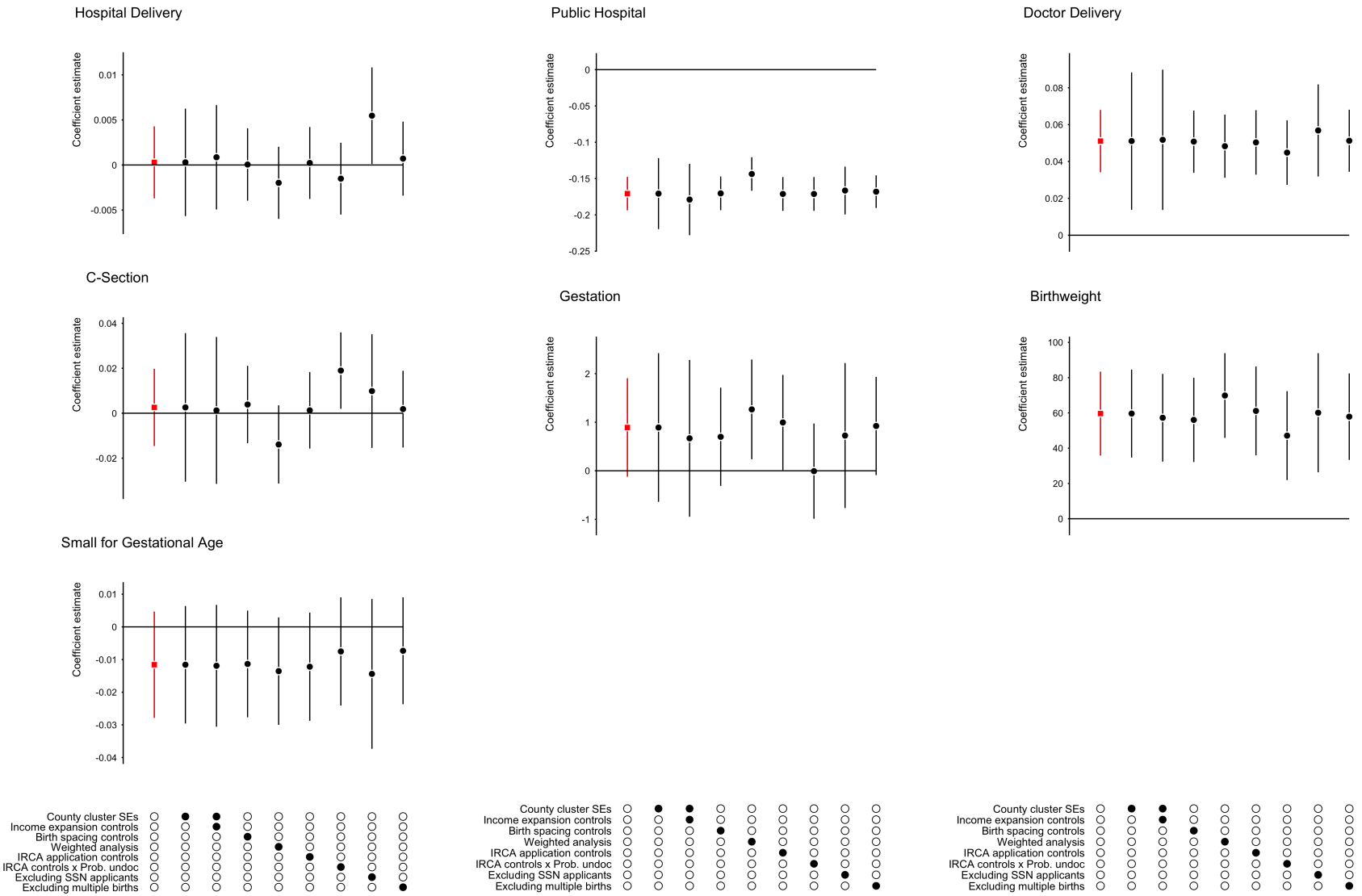
Note: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey, mortality data from the 2022Q3 Census Numident, college attainment as of July 2022 from the National Student Clearinghouse, the 2016-2022 Census Household Composition Key, earnings information from 2007-2022 IRS W-2 forms, EITC amounts calculated from 2007-2021 IRS 1040 forms, and 2000-2016 Medicaid enrollment data from the Centers for Medicare & Medicaid Services; see text for more specific sample information. Coefficients and confidence intervals are estimated using the event study specification (equation 1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Calendar year fixed effects are included in analyses of annual outcomes (wages, EITC, Medicaid). Robust standard errors are clustered by mother and estimated using a bootstrap procedure. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

Figure 6: Alternative Specifications for Medi-Cal Eligibility, Insurance Coverage, and Prenatal Care



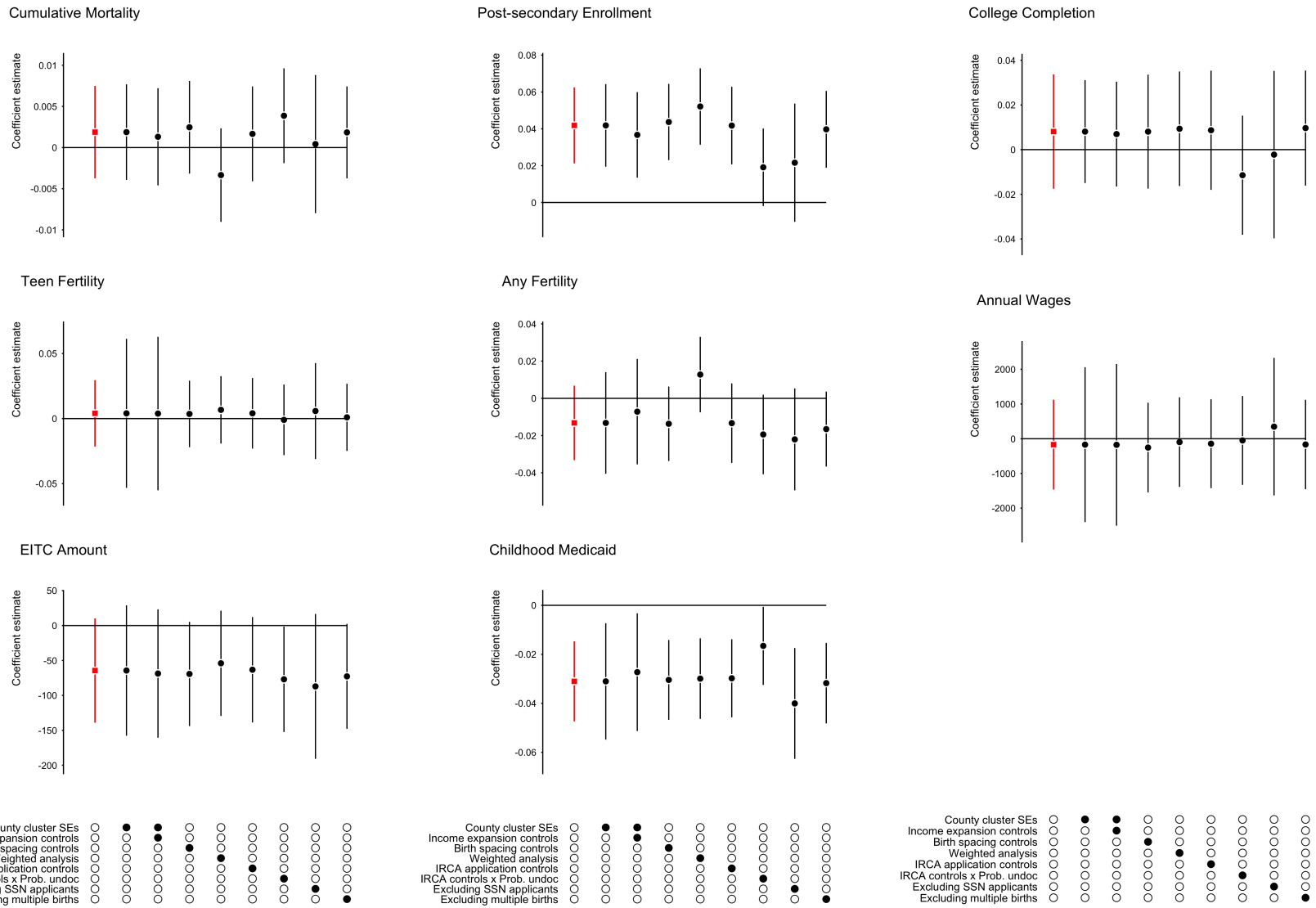
Notes: This figure reports sensitivity analyses for the difference-in-differences estimates reported in Table 1. Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Each estimate and 95% CI are for a different specification or sample, as described in the text. All regressions include birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income,and mother fixed effects. Standard errors are estimated using a bootstrap procedure and clustered by mother unless otherwise specified. Significance levels: *=10%, **=5%, ***=1%. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Figure 7: Alternative Specifications for Hospital and Delivery Care and Infant Health



Notes: This figure reports sensitivity analyses for the difference-in-differences estimates reported in Table 2. Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Each estimate and 95% CI are for a different specification or sample, as described in the text. All regressions include birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income,and mother fixed effects. Standard errors are estimated using a bootstrap procedure and clustered by mother unless otherwise specified. Significance levels: *=10%, **=5%, ***=1%. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Figure 8: Alternative Specifications for Long-Term Health and Human Capital



Note: This figure reports sensitivity analyses for the difference-in-differences estimates reported in Table 4. Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey, mortality data from the 2022Q3 Census Numident, college attainment as of July 2022 from the National Student Clearinghouse, the 2016-2022 Census Household Composition Key, earnings information from 2007-2022 IRS W-2 forms, EITC amounts calculated from 2007-2021 IRS 1040 forms, and 2000-2016 Medicaid enrollment data from the Centers for Medicare & Medicaid Services; see text for more specific sample information. Each estimate and 95% CI are for a different specification or sample, as described in the text. All regressions include birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income,and mother fixed effects. Standard errors are estimated using a bootstrap procedure and clustered by mother unless otherwise specified. Significance levels: *=10%, **=5%, ***=1%. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Table 1: Effect of Undocumented Expansion on Medi-Cal Eligibility, Insurance Coverage, and Prenatal Care

	Medi-Cal Eligibility	Primary Payer for Prenatal Care				Prenatal Care Utilization		
		Medi-Cal	No Insurance	Private	Other	Any	Number of Visits	Early Initiation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. All Immigrants								
Undocumented expansion x	0.553***	0.252***	-0.298***	0.070***	-0.024***	0.014***	0.960***	0.040***
probability undocumented	(0.002)	(0.022)	(0.020)	(0.018)	(0.008)	(0.004)	(0.218)	(0.014)
Baseline mean	0.126	0.293	0.388	0.293	0.026	0.961	8.49	0.516
N	132,000		65,500			129,000	63,500	129,000
B. Hispanic Only Sample								
Undocumented expansion x	0.577***	0.195***	-0.248***	0.077***	-0.024**	0.010*	0.828***	0.044***
probability undocumented	(0.001)	(0.026)	(0.025)	(0.022)	(0.010)	(0.006)	(0.256)	(0.017)
Baseline mean	0.174	0.309	0.398	0.267	0.026	0.976	9.51	0.689
N	90,000		45,500			88,500	44,500	88,500

Notes: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Panel B restricts the sample to births to Hispanic mothers. Medi-Cal eligibility is estimated using a monthly county-level simulated eligibility measure constructed using the 1990 Census and merged on using mother's county of residence, month-year of birth, and predicted documentation status; see text for more details. Coefficient and standard errors are estimated using the difference-in-differences model in equation (2) with birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Regression models for prenatal insurance coverage and the number of prenatal visits estimate changes in outcomes relative to the first year the policy was in place, due to the limited period of data available. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. Significance levels: *=10%, **=5%, ***=1%. Baseline means calculated for cohorts born before October 1988 for Medi-Cal eligibility and for cohorts born before October 1989 for primary payer for prenatal care and the number of prenatal visits among births with a mother whose predicted probability of undocumented status is greater or equal to 0.5. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY22-CES018-007, CBDRB-FY22-CES018-015, CBDRB-FY24-0182, and CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Table 2: Effect of Undocumented Expansion on Delivery Care and Infant Health

	Hospital delivery	Public hospital	Doctor delivery	C-section	Gestation days	Birthweight (gram)	Small for gestational age
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. All Immigrants							
Undocumented expansion x	0.000	-0.171***	0.051***	0.003	0.891*	59.62***	-0.012
probability undocumented	(0.012)	(0.002)	(0.009)	(0.009)	(0.517)	(12.12)	(0.008)
Baseline mean	0.994	0.590	0.886	0.150	278.4	3347	0.106
N	131,000	131,000	132,000	132,000	124,000	132,000	124,000
B. Hispanic Only Sample							
Undocumented expansion x	0.001	-0.148***	0.048***	-0.009	0.881	64.56***	-0.014
immigrant mother	(0.002)	(0.014)	(0.010)	(0.010)	(0.610)	(14.21)	(0.009)
Baseline mean	0.993	0.542	0.923	0.186	279	3404	0.089
N	90,000	90,000	90,000	90,000	85,000	90,000	85,000

Notes: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Panel B restricts the sample to births to Hispanic mothers. Coefficient and standard errors are estimated using the difference-in-differences model in equation (2) with birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. Significance levels: *=10%, **=5%, ***=1%. Baseline means are calculated for cohorts born before October 1988 among births with a mother whose predicted probability of undocumented status is greater or equal to 0.5. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-183, CBDRB-FY22-CES018-007, CBDRB-FY24-0182, and CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Table 3: Effect of Undocumented Expansion on Fertility and Access to Care

	Birth Rate	Pregnancy Complications
	(1)	(2)
A. All Immigrants		
Undocumented expansion x	0.004***	-0.025***
probability undocumented	(0.000)	(0.006)
Baseline mean	0.007	0.028
N	10,730,000	132,000
N (unique individuals)	185,000	132,000
B. Hispanic Only Sample		
Undocumented expansion x	0.004***	-0.021***
immigrant mother	(0.000)	(0.007)
Baseline mean	0.007	0.029
N	6,808,000	90,000
N (unique individuals)	119,000	90,000

Notes: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. The sample for (1) consists of immigrant mothers with at least one birth over the study period. The sample for (2) is our main analytic sample of births to immigrant mothers with at least two births over the study period. Panel B subsets the samples to Hispanic mothers and their children. Estimates for (2) use the difference-in-differences model in equation (2) with birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Estimates for (1) use a variant of this difference-in-differences model that include the poverty rate and per capita income in each county and year, calendar month and year fixed effects, and mother fixed effects. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. Significance levels: *=10%, **=5%, ***=1%. Baseline means calculated for cohorts born before October 1988 among mothers (1) or births with a mother (2) whose predicted probability of undocumented status is greater or equal to 0.5. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY22-CES018-015, CBDRB-FY24-018, and CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Table 4: Effect of Undocumented Expansion on Long-Term Health and Human Capital

	Cumulative mortality Ages 0-27 (1)	Ever enrolled post-secondary (2)	Graduated college (3)	Teen fertility Ages 14-19 (4)	Any fertility Ages 14-26 (5)	Annual wages Ages 23-28 (6)	EITC amount Ages 25-27 (7)	Childhood Medicaid Ages 16-18 (8)
A. All Immigrants								
Undocumented expansion x	0.002	0.042***	0.008	0.004	-0.013	-170.7	-64.4*	-0.031***
probability undocumented	(0.003)	(0.011)	(0.013)	(0.013)	(0.010)	(659.0)	(38.04)	(0.008)
Baseline mean	0.0076	0.630	0.173	0.178	0.457	22,990	870.8	0.378
N	132,000	132,000	130,000	132,000	132,000	791,000	395,000	395,000
N (unique individuals)	132,000	132,000	130,000	132,000	132,000	132,000	132,000	132,000
B. Hispanic Only Sample								
Undocumented expansion x	0.000	0.048***	0.009	0.015	0.014	167.9	-16.63	-0.026***
immigrant mother	(0.003)	(0.011)	(0.015)	(0.016)	(0.012)	(638.3)	(44.84)	(0.010)
Baseline mean	0.0073	0.619	0.141	0.189	0.480	22,550	915.6	0.398
N	90,000	90,000	89,000	90,000	90,000	546,000	273,000	273,000
N (unique individuals)	90,000	90,000	89,000	90,000	90,000	90,000	90,000	90,000

Notes: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey, mortality data from the 2022Q3 Census Numident, college attainment as of July 2022 from the National Student Clearinghouse, the 2016-2022 Census Household Composition Key, earnings information from 2007-2022 IRS W-2 forms, EITC amounts calculated from 2007-2021 IRS 1040 forms, and 2000-2016 Medicaid enrollment data from the Centers for Medicare & Medicaid Services; see text for more specific sample information. Panel B restricts the sample to births to Hispanic mothers. Coefficient and standard errors are estimated using the difference-in-differences model in equation (2) with birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Calendar year fixed effects are included in analyses of annual outcomes (wages, EITC, Medicaid). Robust standard errors are clustered by mother and estimated using a bootstrap procedure. Significance levels: *=10%, **=5%, ***=1%. Baseline means are calculated for cohorts born before October 1988 among births with a mother whose predicted probability of undocumented status is greater or equal to 0.5. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY23-CES-021-001, CBDRB-FY24-0182, and CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Covering the Undocumented: The Effects of A Large-Scale Prenatal Care Intervention

Appendix

Sarah Miller Laura R. Wherry Gloria Aldana

A Estimating Medi-Cal Eligibility

We estimate eligibility for female California and US residents ages 15-44 in the event of a pregnancy using detailed information on state eligibility rules over the time period. Our eligibility calculation applies federal and state eligibility rules for Medicaid under AFDC, Medically Needy, and state-specific optional eligibility groups.[2]

Note, we are unable to use the Current Population Survey for this exercise since the survey only began collecting citizenship information in 1994. We, therefore, use the 1990 Census and inflate or deflate the dollar amounts of household income and earnings to estimate eligibility in each year. The use of a fixed sample to estimate eligibility in each year produces information on eligibility changes over the period that are due to state law changes, rather than changes in state demographic or economic characteristics. This type of measure is often referred to as "simulated eligibility" and has been used as a policy instrument in a large body of work pioneered with Currie and Gruber (1996a,b) and Cutler and Gruber (1996).

Information on California's optional Aid to Families with Dependent Children (AFDC)-related coverage groups prior to the Medi-Cal expansion were drawn from Hill (1987). Information on Ribicoff child programs was drawn from the 1983 Health Care Financing Administration (HCFA)'s *Analysis of State Medicaid Program Characteristics*, the Urban Institute's Transfer Income Model, version 3 (TRIM 3) Database, and from materials provided by Bruce Meyer and used in Meyer and Rosenbaum (2001). State Medically Needy thresholds were taken from TRIM3 and the 1984 and 1986 *Medicare and Medicaid Data Books* issued by the HCFA. Details on state changes under the Medi-Cal undocumented expansion and later income expansions were drawn from guidelines issued by the state in Martucci (1988) and Mitchell (2005).

The period of study also included some later changes in California eligibility rules regarding the consideration of family assets, which we do not consider here since information on family assets is not available in the Census. Beginning on January 1, 1992, assets were disregarded for eligibility for women and infants with incomes between 185-200% FPL; assets were disregarded for all effective February 1, 1994 (Mitchell, 2005).

Also not studied here, California adopted a statewide program in 1992 to subsidize private health insurance coverage for pregnant women and infants with incomes between 200-300% FPL called the Access for Infants and Mothers Program. This was a small program with about 300 women participating each year (Zuckerman et al., 1998).

[2]Please refer to the appendix of East et al. (2023) for a more detailed description of these eligibility rules; source information is described below.

B Imputing Undocumented Immigrants in the Census

We adapt an algorithm created by Borjas (2017) based on a methodology developed by Passel and Cohn (2014) to identify immigrant individuals with undocumented status. This methodology builds on earlier work by Warren and Passel (1987) to estimate the number of undocumented immigrants in the U.S., which is the methodology applied by the Department of Homeland Security in their annual estimates of the size of the undocumented population (see Borjas (2017) for more detailed background information). While not publicly available, the methodology was reverse engineered by Borjas (2017) based on individual Current Population Survey (CPS) data with the undocumented status identifier provided to him by Jeffrey Passel. We use the program previously made available by Borjas on his website in order to apply it in our context.

Under this methodology, an individual is considered to be a legal immigrant if they: are a citizen or arrived in the U.S. before 1980; receives Social Security benefits, SSI, Medicare, or military insurance; are a veteran or are currently in the armed forces; works in the government sector; receives housing assistance (public housing or rental subsidies), or are the spouse of someone who receives housing assistance; was born in Cuba; are in an occupation that requires licensing; or their spouse is a legal immigrant or citizen. The Borjas (2017) algorithm also considers an individual legal if they receive Medicaid; however, we drop this rule given that undocumented immigrants were able to benefit under the Medi-Cal expansion studied here. We substitute the rule that immigrants are of legal status if they receive cash welfare under AFDC. Individuals who do not meet any of these criteria to be determined legal are considered "undocumented."

While Borjas was working with CPS data, we apply this algorithm to the 1990 Census since the CPS only started collecting information on citizenship status in 1994. The two surveys collect nearly identical information under the variables needed to assign undocumented status. There are two exceptions: the 1990 Census does not include information on receipt of health insurance through Medicare or the military, nor the receipt of housing assistance. We, therefore, are unable to consider these criteria when determining undocumented status. However, we examine how the absence of these survey items might affect undocumented assignment by applying the algorithm with and without their inclusion in the 1994-1997 CPS files. We find that the two versions perform nearly identically.

C Identifying Siblings Using the 2000 Census and 2001-2011 ACS

Among children in the 2000 Census and 2001-2011 ACS, we identify siblings as individuals 17 years of age or younger who are residing at home with the same mother. We use the following household and subfamily relationships reported in the survey to identify mothers:

- Rule 1: If female is wife of householder and child is identified as natural-born child, step child, or adopted child of householder, we consider her to be the mother of the child.
- Rule 2: If female is the householder and child is identified as natural-born son or daughter of householder, we consider her to be the mother of the child.
- Rule 3: If the child is identified as a child in a married couple subfamily or a mother-child subfamily and the female is a member in the same subfamily and identified as either a hus-

band/wife subfamily with child or single parent subfamily, we consider her to be the mother of the child.

Note that, unlike for children of the head of a household, information is not available that distinguishes between natural, step, or adopted child relationships between the child in a household and the wife of the household head. Therefore, under Rule 1, we will capture all mother-child relationships including those that are not biological in nature.[3] In addition, the household relationship variable used in Rule 2 to identify the natural-born child of the householder, did not have this level of detail in the 2001-2007 ACS years. In these years, the survey did not distinguish between biological and other types of children. Thus, for Rule 2 in these years, we only require that the child is the son or daughter of the householder, rather than the biological child. Finally, subfamily relationship information is not available in the short-form 2000 Census as needed to implement Rule 3 when using this data source. These data limitations will introduce some noise into the identification of mother-child relationships, but we do not expect this measurement error to differ in any systematic way across children depending on their exposure to the Medicaid expansions while *in utero*.

As additional checks on this assignment procedure, we exclude siblings to mothers who have disconcordant information on her place of birth or age across the birth certificate records. We consider her age at time of birth to be discordant if her reported age for a sibling does not fall within a 2-year window of the age expected based on her reported age and the year of birth of the prior birth observed during our study period. We also exclude siblings to mothers who have disconcordant information on her place of birth on the birth certificate record and Census/ACS report.

Because the 1989 and later birth records do have identifying information available for the mother, we are able to check the performance of this assignment procedure during the later years of our sample. We find that 96.2 percent of the birth records in our analysis sample had the same mother PIK assigned based on the birth record fields as that assigned using our ACS/Census assignment procedure.

D Robustness to Controls for Income-Based Expansions

In Section 5, we examine the sensitivity of our findings to controls for the income-based Medi-Cal expansions that took effect during the 15-month period following the expansion to undocumented immigrants, as described in Section 1. To conduct this analysis, we use county-level measures of the change in eligibility for immigrant women by documentation status that resulted from each of the two different income expansions as measures of exposure to the policy change. These measures are constructed by estimating the corresponding change in eligibility for women of reproductive age in the event of a pregnancy using the 1990 Census. Specifically, the eligibility gains are calculated as (1) the change in county-level eligibility using eligibility rules from July 1989 compared to June 1989, the month just prior to the first expansion that occurred in July 1989; and, (2) the change in county-level eligibility using eligibility rules from January 1990 compared to December 1989 for the second expansion that occurred in January 1990.[4] We merge these county-level measures onto the California

[3] Note, in the handful of cases where both the household head and spouse are female, we consider the household head to be the mother.

[4] Additional details on this eligibility calculation may be found in Appendix Section A. As mentioned earlier, not all counties are identifiable in the 1990 Census. For those counties that are unable to be identified, we use the estimate of the

birth records by county and construct an estimate for each birth in the sample using the following formula for both the July 1989 and January 1990 expansions: $\hat{p} * \text{EligGain(undocumented)}_c + (1 - \hat{p}) * \text{EligGain(documented)}_c$, where $\hat{p}$ is the mother's estimated probability of undocumented status. We interact each of the resulting measures with an indicator for births that occur after the corresponding income expansion date and include these in equation (2). This results in the following regression formula:

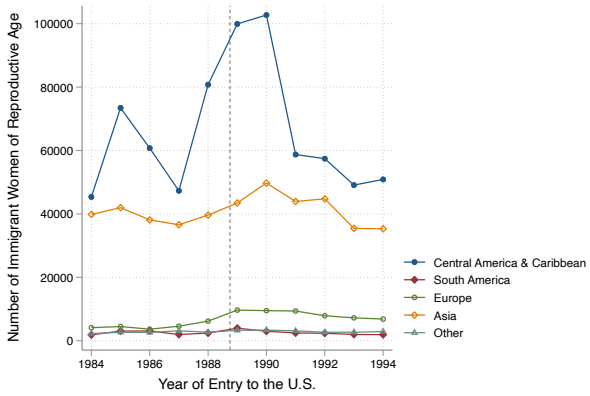
$$\begin{aligned} y_{imt} = \beta PostOct1988_t \times ProbUndoc_m + \psi PostJuly1989_t \times EligGainJuly1989_m \\ + \omega PostJan1990_t \times EligGainJan1990_m + \delta_t + \delta_m + \delta_c + \gamma X_{imt} + \theta Z_{ct} + \epsilon_{imt} \end{aligned} \tag{4}$$

When estimating these regressions, we cluster the standard errors by county using a bootstrap procedure since we are using variation in county-level changes in eligibility to control for the concurrent effects of the income-based Medi-Cal expansions. As shown in Figures 6-8 (difference-in-differences) and Figures A15-A17 (event studies), the inclusion of these controls do not noticeably change the point estimates on any of the outcomes we study. It is the case, however, that we lose statistical significance on the estimates for other sources of insurance coverage and the increase in any prenatal care use under this specification, similar to the other specification check that clusters the standard errors by county.

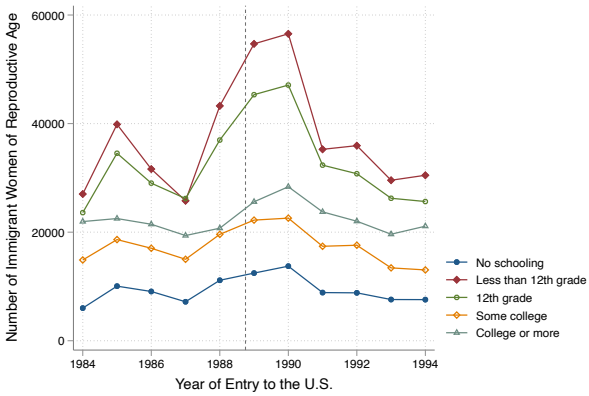
eligibility change for respondents with non-identified counties.

Figure A1: Number of Immigrant Women in California by Year of Entry to the U.S. and Number of California-Born Children of Immigrant Women by Year of Birth

I) Immigrant Women of Reproductive Age

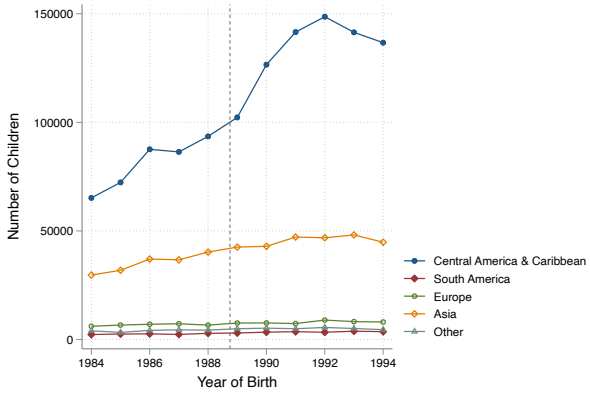


(a) By Place of Birth

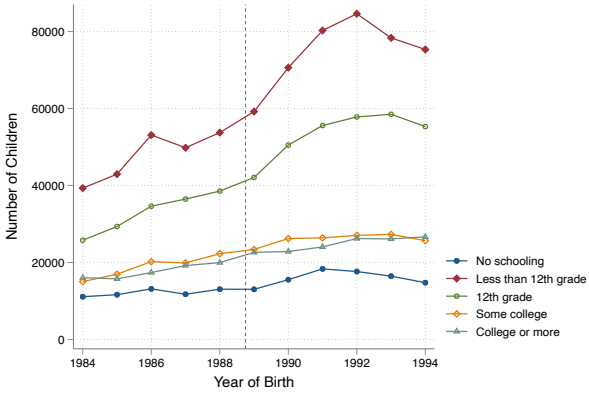


(b) By Educational Attainment

II) California-Born Children of Immigrant Women



(c) By Mother's Place of Birth



(d) By Mother's Educational Attainment

Notes: Numbers of immigrant women of reproductive age in California by year of entry to the U.S. and number of children in California by year of birth are estimated using the 2000 Census. Information on place of birth and educational attainment are from the 2000 survey.

Figure A2: Summary Information on Data Sources and Elements Used in Analyses

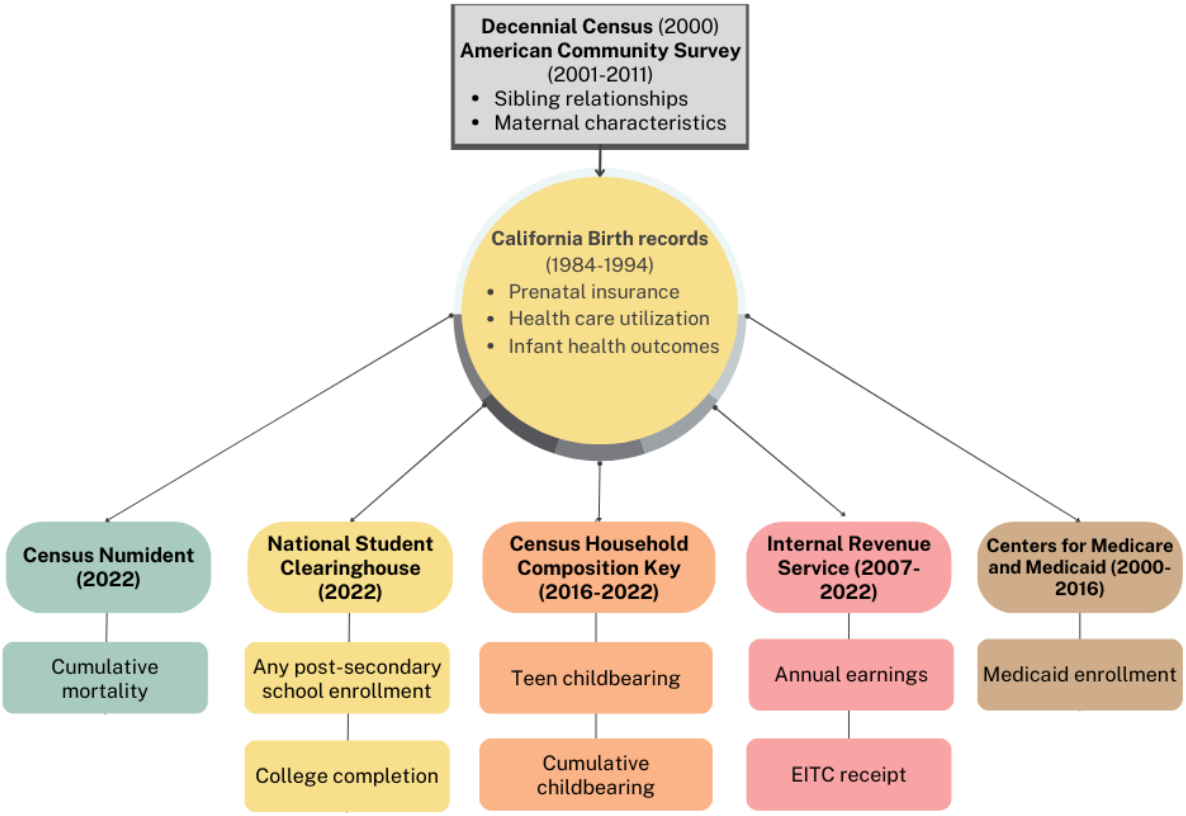
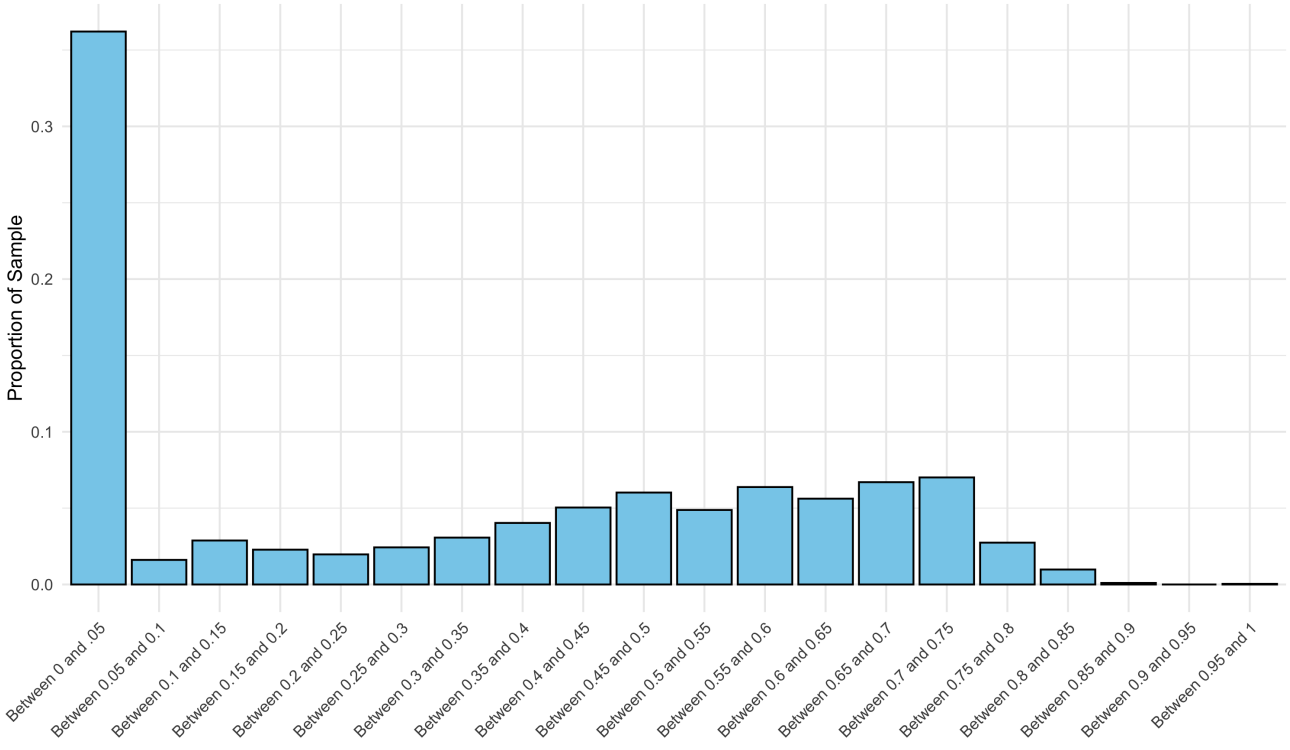
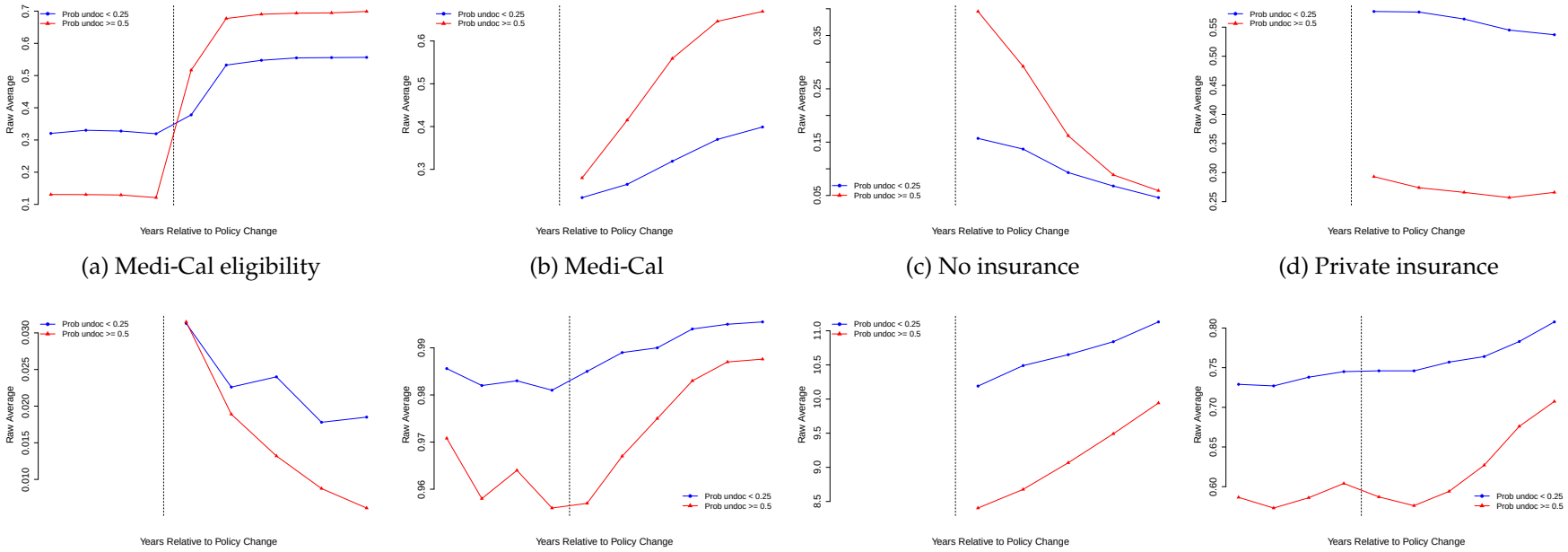


Figure A3: Distribution of Predicted Undocumented Status



Note: Analyses use 1984-1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. Sample defined as CA births during Jan. 1984 - Oct. 1994 to moms with at least two births during this period, after excluding births to women residing outside of the state of California or with missing information for county of residence, nativity, child's sex, plurality, or mother's total live birth number. Mother-child links are established from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. Figure shows distribution of predicted undocumented status of mothers for births in the sample. This is calculated using estimated coefficients from a probit regression of imputed documentation status on age, county of residence, year of entry, and country of birth dummies for the sample of immigrant mothers of children under age 6 residing in California in the 1990 Census. See text for additional details on the undocumented status imputation in the 1990 Census and the prediction model. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

Figure A4: Eligibility, Prenatal Coverage, and Prenatal Care Utilization Over Time by Likely Undocumented Status (raw)

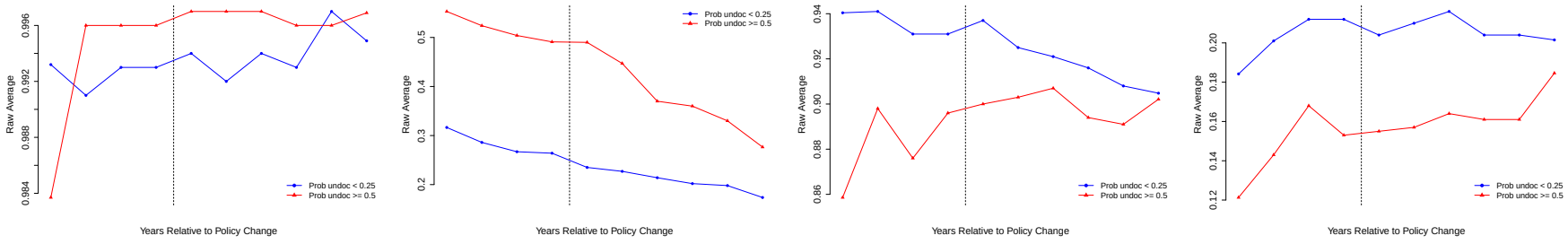


(a) Medi-Cal eligibility (b) Medi-Cal (c) No insurance (d) Private insurance

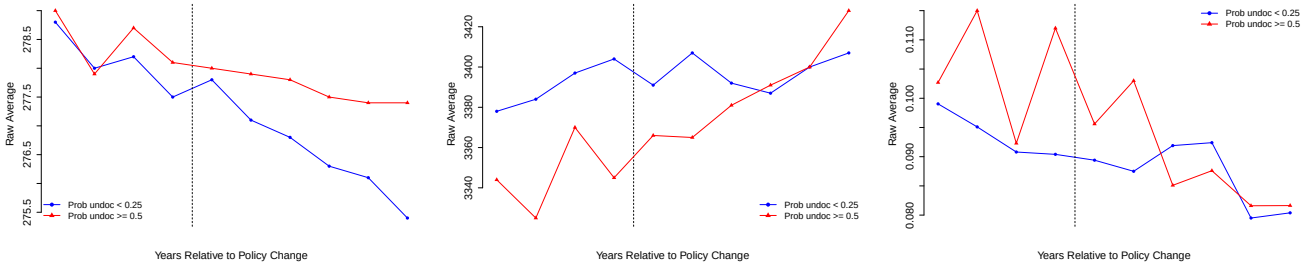
(e) Other insurance (f) Any prenatal care (g) Number of prenatal visits (h) Prenatal care in first trimester

Note: Analyses use 1984-1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. Sample defined as CA births during Jan. 1984 - Oct. 1994 to moms with at least two births during this period, after excluding births to women residing outside of the state of California or with missing information for county of residence, nativity, child's sex, plurality, or mother's total live birth number. Mother-child links are established from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. Plots depict average values of raw outcomes by year for births to high probability and low probability undocumented immigrants. High probability undocumented immigrants are defined as those with an estimated value of greater than or equal to 0.5 for their likely undocumented status. Low probability undocumented immigrants are defined as those with an estimated value of less than 0.25. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-012 and CBDRB-FY26-0165.

Figure A5: Delivery Care and Infant Health Over Time by Likely Undocumented Status (raw)



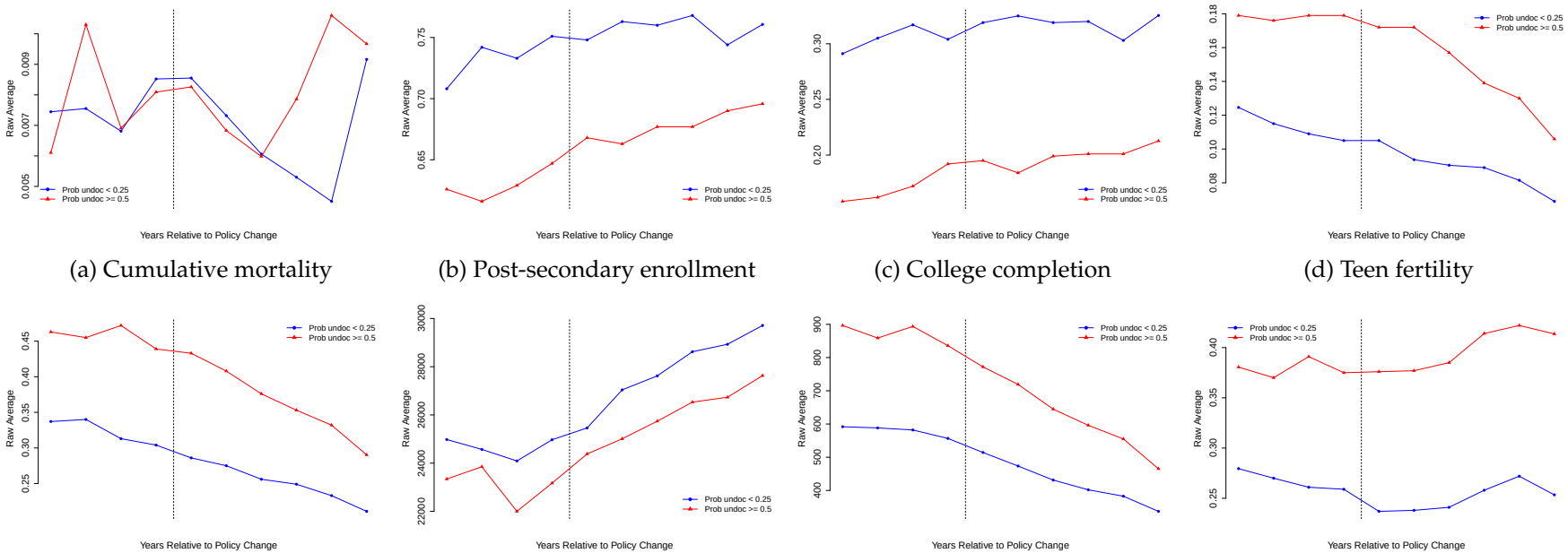
(a) Hospital delivery (b) Public hospital (c) Doctor delivery (d) C-section



(e) Gestation age (f) Birthweight (g) Small for gestational age

Note: Analyses use 1984-1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. Sample defined as CA births during Jan. 1984 - Oct. 1994 to moms with at least two births during this period, after excluding births to women residing outside of the state of California or with missing information for county of residence, nativity, child's sex, plurality, or mother's total live birth number. Mother-child links are established from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. Plots depict average values of raw outcomes by year for births to high probability and low probability undocumented immigrants. High probability undocumented immigrants are defined as those with an estimated value of greater than or equal to 0.5 for their likely undocumented status. Low probability undocumented immigrants are defined as those with an estimated value of less than 0.25. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-012 and CBDRB-FY26-0165.

Figure A6: Long-Term Health and Human Capital Over Time by Likely Undocumented Status (raw)

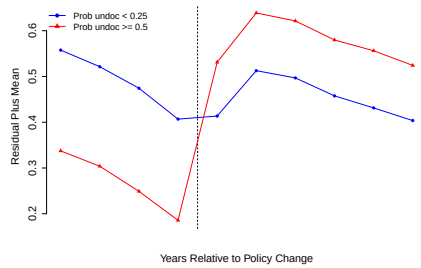


(a) Cumulative mortality (b) Post-secondary enrollment (c) College completion (d) Teen fertility

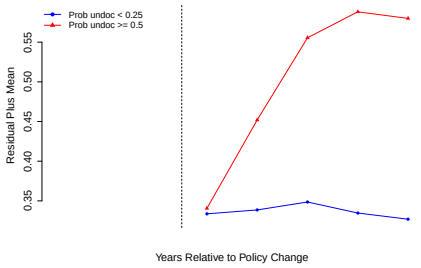
(e) Any fertility (f) Annual wages (g) EITC amount (h) Childhood Medicaid

Note Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey, mortality data from the 2022Q3 Census Numident, college attainment as of July 2022 from the National Student Clearinghouse, the 2016-2022 Census Household Composition Key, earnings information from 2007-2022 IRS W-2 forms, EITC amounts calculated from 2007-2021 IRS 1040 forms, and 2000-2016 Medicaid enrollment data from the Centers for Medicare & Medicaid Services. Sample defined as CA births during Jan. 1984 - Oct. 1994 to moms with at least two births during this period, after excluding births to women residing outside of the state of California or with missing information for county of residence, nativity, child's sex, plurality, or mother's total live birth number. Mother-child links are established from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. Plots depict average values of raw outcomes by year for births to high probability and low probability undocumented immigrants. High probability undocumented immigrants are defined as those with an estimated value of greater than or equal to 0.5 for their likely undocumented status. Low probability undocumented immigrants are defined as those with an estimated value of less than 0.25. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-012 and CBDRB-FY26-0165.

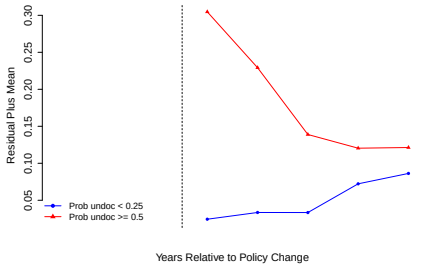
Figure A7: Eligibility, Prenatal Coverage, and Prenatal Care Utilization Over Time by Likely Undocumented Status (residualized)



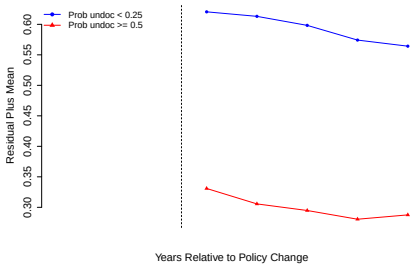
(a) Medi-Cal eligibility (residualized)



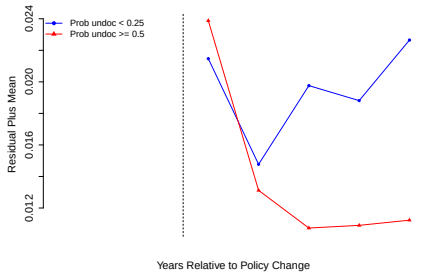
(b) Medi-Cal (residualized)



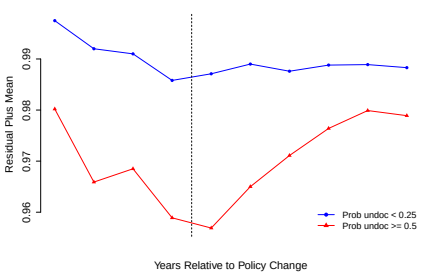
(c) No insurance (residualized)



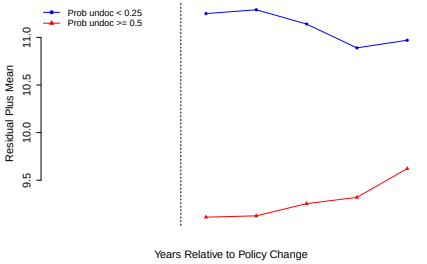
(d) Private insurance (residualized)



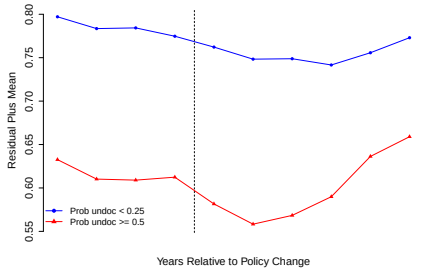
(e) Other insurance (residualized)



(f) Any prenatal care (residualized)



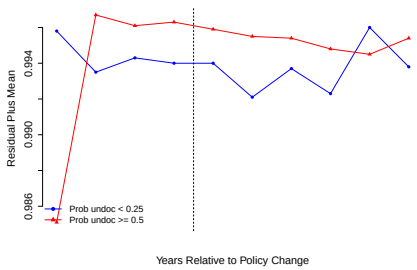
(g) Number of prenatal visits (residualized)



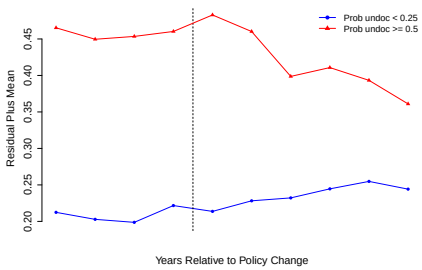
(h) Prenatal care in first trimester (residualized)

Note: Analyses use 1984-1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. Sample defined as CA births during Jan. 1984 - Oct. 1994 to moms with at least two births during this period, after excluding births to women residing outside of the state of California or with missing information for county of residence, nativity, child's sex, plurality, or mother's total live birth number. Mother-child links are established from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. Plots depict average values of residualized outcomes by year for births to high probability and low probability undocumented immigrants. High probability undocumented immigrants are defined as those with an estimated value of greater than or equal to 0.5 for their likely undocumented status. Low probability undocumented immigrants are defined as those with an estimated value of less than 0.25. Residualized outcomes are estimated using a regression of the outcome variable on mothers' fixed effects, birth order dummies, and indicators for female and singleton births for the full sample of immigrant births. Residuals from this regression are then added to the sample mean. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-012 and CBDRB-FY26-0165.

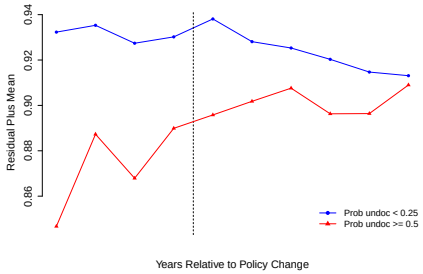
Figure A8: Delivery Care and Infant Health Over Time by Likely Undocumented Status (residualized)



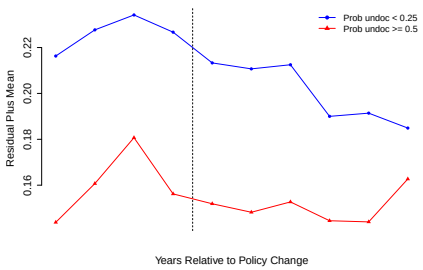
(a) Hospital delivery (residualized)



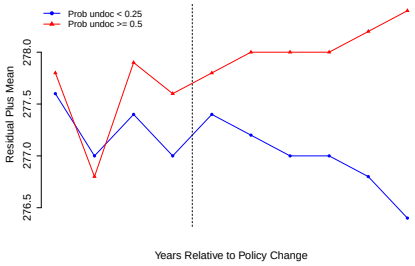
(b) Public hospital (residualized)



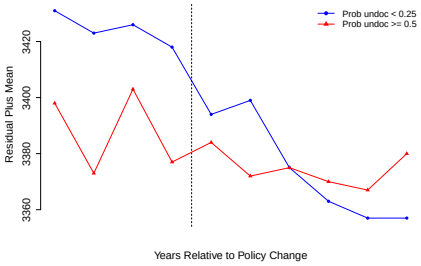
(c) Doctor delivery (residualized)



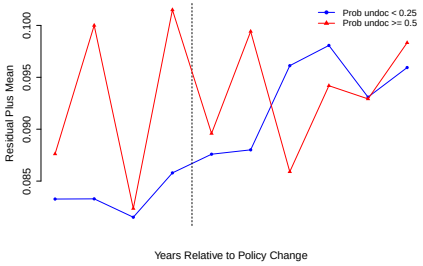
(d) C-section (residualized)



(e) Gestation age (residualized)



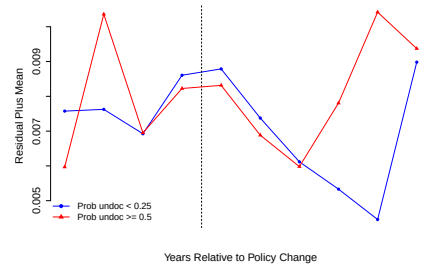
(f) Birthweight (residualized)



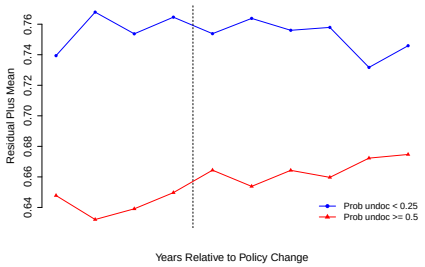
(g) Small for gestational age (residualized)

Analyses use 1984-1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. Sample defined as CA births during Jan. 1984 - Oct. 1994 to moms with at least two births during this period, after excluding births to women residing outside of the state of California or with missing information for county of residence, nativity, child's sex, plurality, or mother's total live birth number. Mother-child links are established from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. Plots depict average values of residualized outcomes by year for births to high probability and low probability undocumented immigrants. High probability undocumented immigrants are defined as those with an estimated value of greater than or equal to 0.5 for their likely undocumented status. Low probability undocumented immigrants are defined as those with an estimated value of less than 0.25. Residualized outcomes are estimated using a regression of the outcome variable on mothers' fixed effects, birth order dummies, and indicators for female and singleton births for the full sample of immigrant births. Residuals from this regression are then added to the sample mean. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

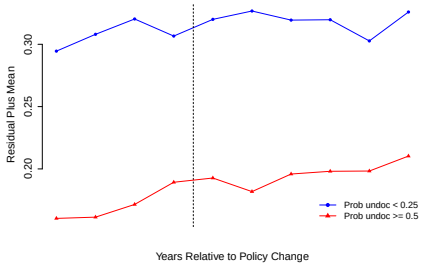
Figure A9: Long-Term Health and Human Capital Over Time by Likely Undocumented Status (residualized)



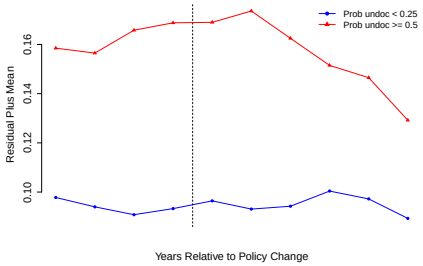
(a) Cumulative mortality (residualized)



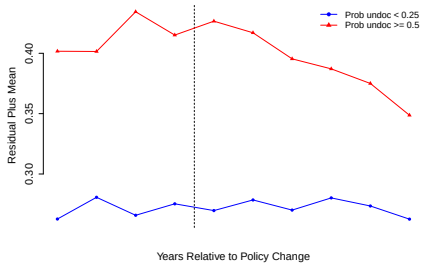
(b) Post-secondary enrollment (residualized)



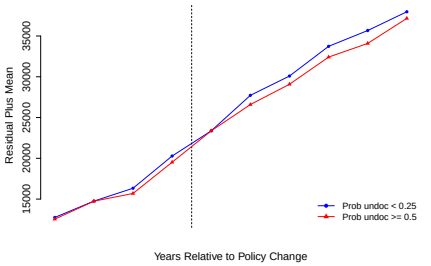
(c) College completion (residualized)



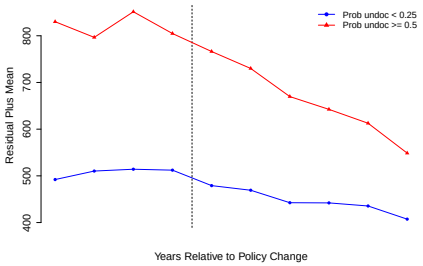
(d) Teen fertility (residualized)



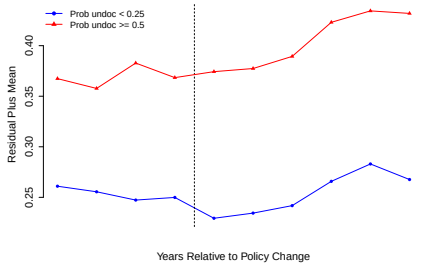
(e) Any fertility (residualized)



(f) Annual wages (residualized)



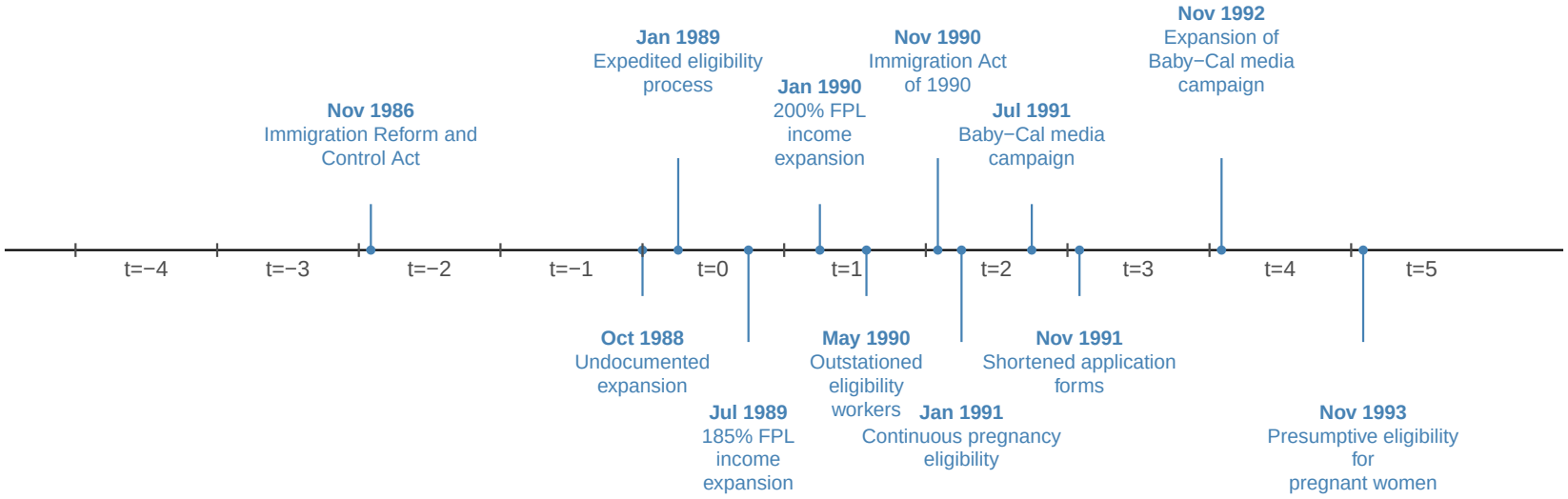
(g) EITC amount (residualized)



(h) Childhood Medicaid (residualized)

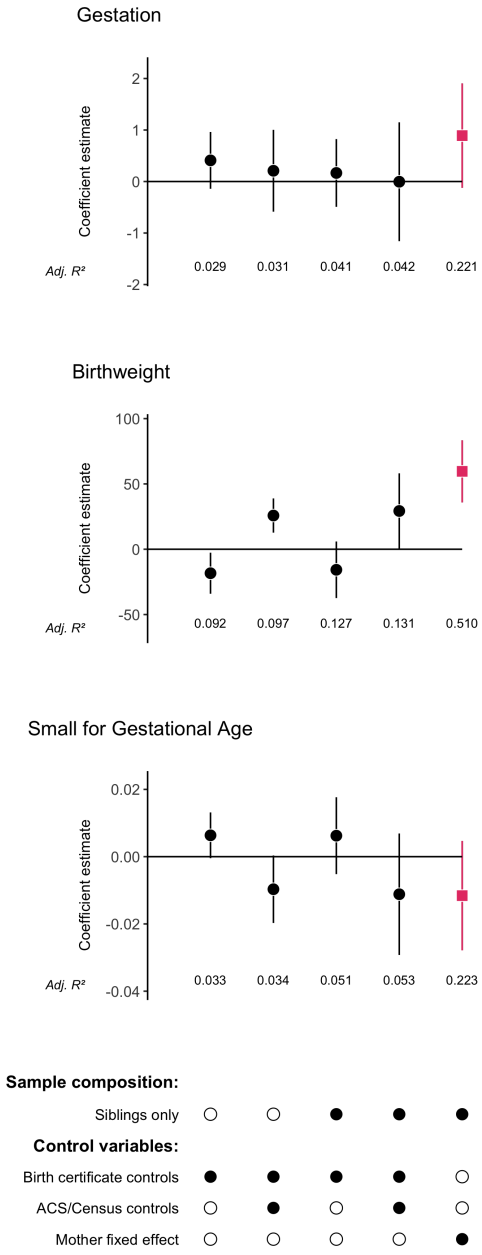
Note: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey, mortality data from the 2022Q3 Census Numident, college attainment as of July 2022 from the National Student Clearinghouse, the 2016-2022 Census Household Composition Key, earnings information from 2007-2022 IRS W-2 forms, EITC amounts calculated from 2007-2021 IRS 1040 forms, and 2000-2016 Medicaid enrollment data from the Centers for Medicare & Medicaid Services. Sample defined as CA births during Jan. 1984 - Oct. 1994 to moms with at least two births during this period, after excluding births to women residing outside of the state of California or with missing information for county of residence, nativity, child's sex, plurality, or mother's total live birth number. Mother-child links are established from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. Plots depict average values of residualized outcomes by year for births to high probability and low probability undocumented immigrants. High probability undocumented immigrants are defined as those with an estimated value of greater than or equal to 0.5 for their likely undocumented status. Low probability undocumented immigrants are defined as those with an estimated value of less than 0.25. Residualized outcomes are estimated using a regression of the outcome variable on mothers' fixed effects, birth order dummies, and indicators for female and singleton births for the full sample of immigrant births, as well as calendar year for any annually-measured outcome variables. Residuals from this regression are then added to the sample mean. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

Figure A10: Timeline of Key Policy Changes Relative to the Undocumented Expansion



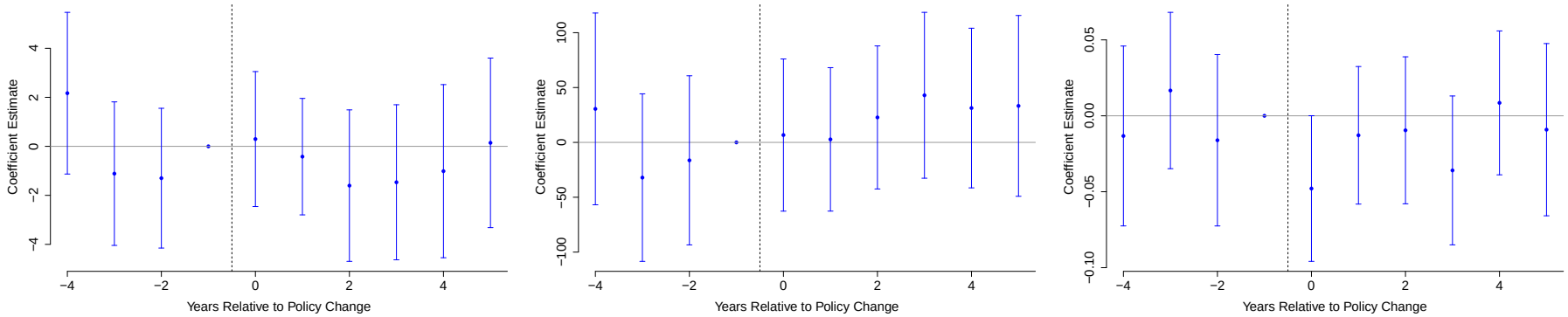
Note: Figure shows dates of major immigration legislation and Medi-Cal policies over the study period relative to the undocumented expansion in October 1988. Sources are Hill (1992), Martucci (1993), Dubay et al. (1995), and California Department of Health Care Services (2016) for the Medi-Cal policies, Library of Congress (2024), and 101st Congress (1990).

Figure A11: Difference-in-Differences Estimates by Sample and Specification



Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. All regression models without mothers' fixed effect include birth order dummies, indicator for singleton births, and indicator for female births, as well as birth year x birth month x county indicators. Birth certificate controls as noted in table are: mother's age, race, ethnicity, and country of birth. ACS/Census controls as noted in table are: detailed county of birth indicators and indicators for year of entry in the US. Coefficient and standard errors in red are estimated using the difference-in-differences model in equation (2) with birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Robust standard errors are clustered by county in the four specifications without mothers' fixed effects, and by mother in the specification that includes mothers' fixed effects. All standard errors are estimated with a bootstrap procedure. Significance levels: *=10%, **=5%, ***=1%. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY26-012 and CBDRB-FY26-0165.

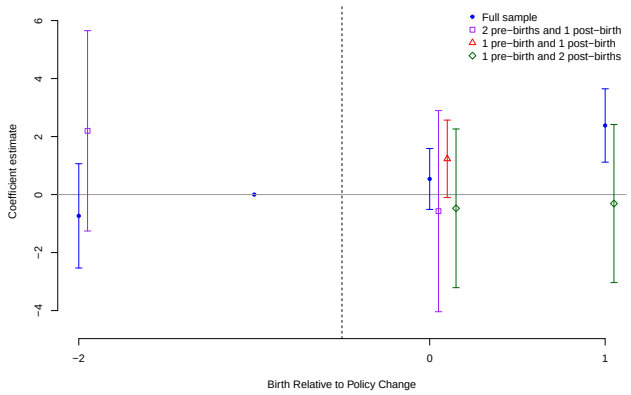
Figure A12: Event Study Estimates for Placebo Policy in October 1998 for Cohorts Born in 1994-2004



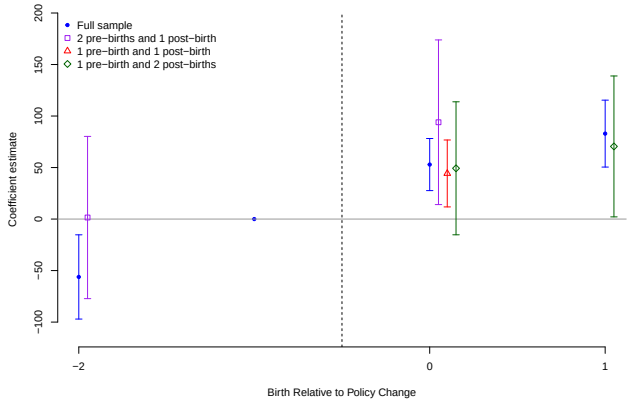
(a) Gestation age (in days) (b) Birthweight (in grams) (c) Small for gestational age

Note: Analyses use January 1994-October 2004 births to mothers residing in the state of California and with non-missing information on mother's county of residence, parity, birth order, sex, and mother's country of birth. Siblings sample is constructed using mother-child links from the 2010-2021 American Community Survey when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and ACS record. Coefficients and confidence intervals are estimated using the event study specification (equation 1) described in the text but using the placebo policy date of October 1998 instead of the true policy date of October 1988. The specification includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty, and mother fixed effects. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

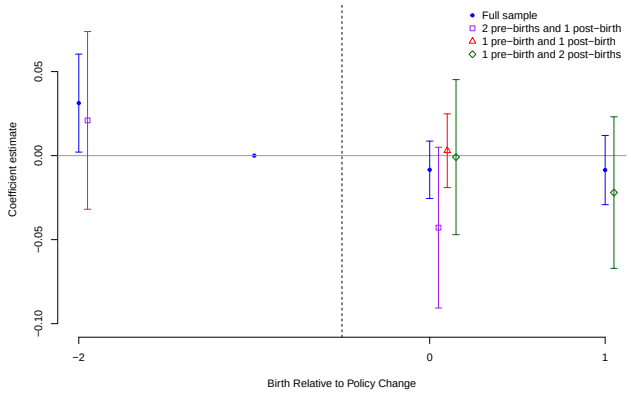
Figure A13: Balanced Sample Event Study Checks Using Birth Order Relative to Time of Expansion



(a) Gestation age (in days)



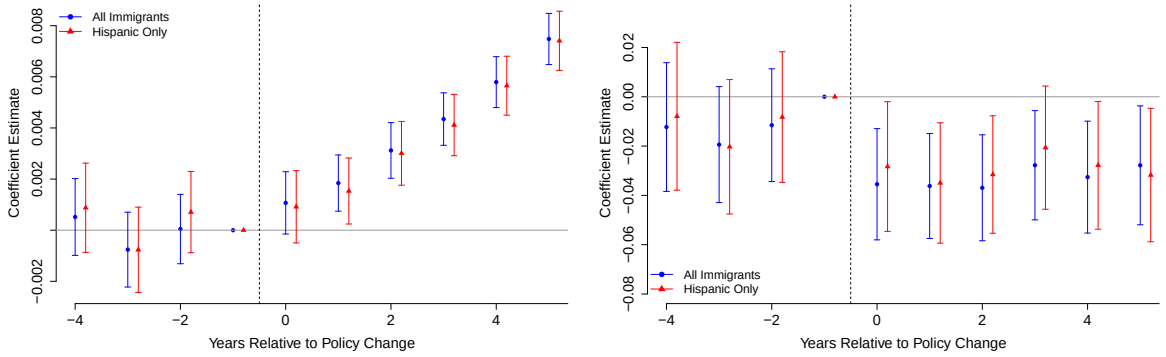
(b) Birthweight (in grams)



(c) Small for gestational age

Note: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. Estimated coefficients and confidence intervals from the event study specification where event time is calculated as birth order relative to the timing of expansion, rather than calendar time, as described in the text and depicted in equation (3). Legend indicates what births are included in the estimation. Event study specification also includes birth order dummies, indicators for female and singleton births, county of residence fixed effects, birth month by birth year fixed effects, and mother fixed effects. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Figure A14: Effects of Undocumented Expansion on Fertility and Maternal Health

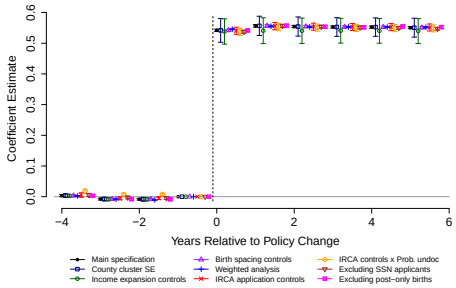


(a) Birth rate

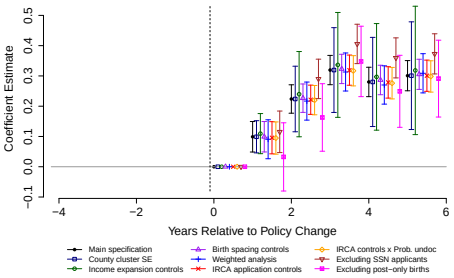
(b) Pregnancy complications

Note: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. The sample for (a) consists of immigrant mothers with at least one birth over the study period. The sample for (b) is our main analytic sample of births to immigrant mothers with at least two births over the study period. Panel B subsets the samples to Hispanic mothers and their children. Estimates for (b) use the event study model in equation (1) with birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Estimates for (a) use a variant of this event study model that include the poverty rate and per capita income in each county and year, calendar month and year fixed effects, and mother fixed effects. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. Significance levels: *=10%, **=5%, ***=1%. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

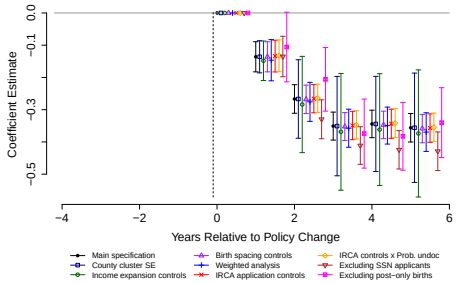
Figure A15: Alternative Specifications for Medi-Cal Eligibility, Insurance Coverage, and Prenatal Care



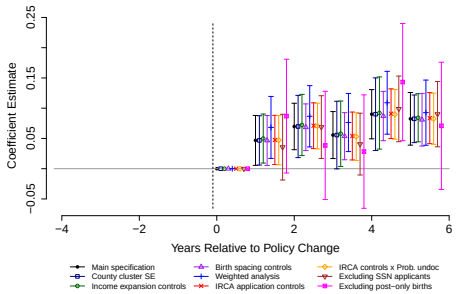
(a) Medi-Cal eligibility



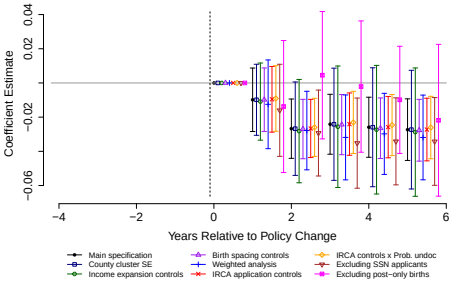
(b) Medi-Cal



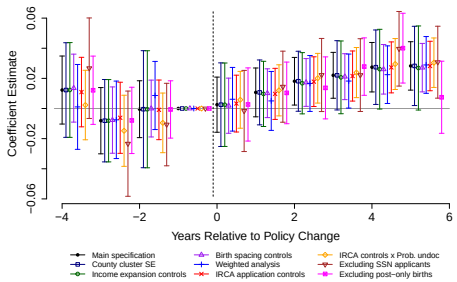
(c) No insurance



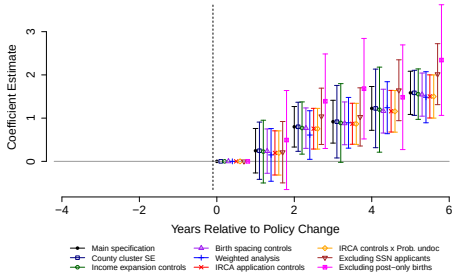
(d) Private coverage



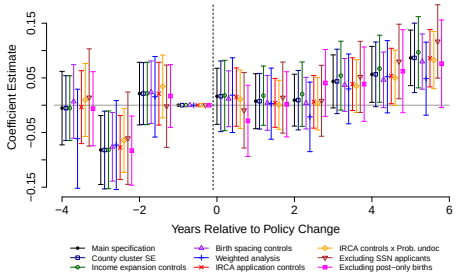
(e) Other coverage



(f) Any prenatal care



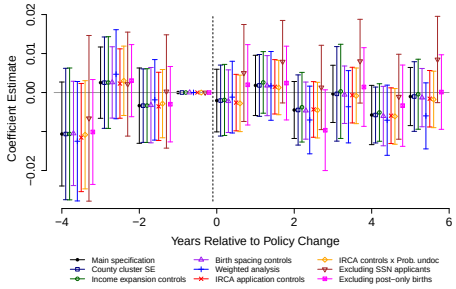
(g) Number of prenatal visits



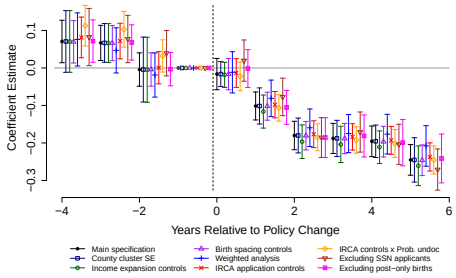
(h) Prenatal care in first trimester

Note: This figure reports sensitivity analyses for the event study estimates reported in Figure 2. Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Medi-Cal eligibility is estimated using a monthly county-level simulated eligibility measure constructed using the 1990 Census and merged on using mother's county of residence, immigrant, and predicted documentation status; see text for more details. Coefficients and confidence intervals are estimated using the event study specification (1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Regression models for prenatal insurance coverage and number of prenatal visits estimate changes in outcomes relative to the first year the policy was in place, due to the limited period of data available. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

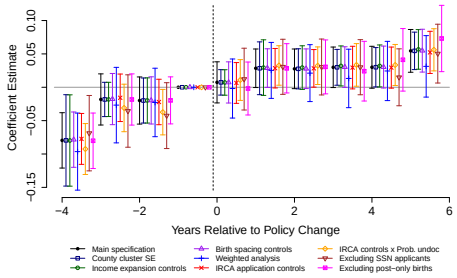
Figure A16: Alternative Specifications for Delivery Care and Infant Health



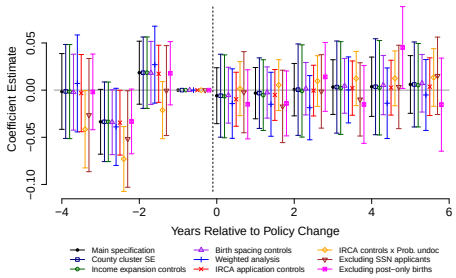
(a) Hospital delivery



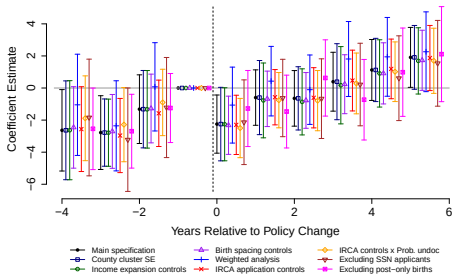
(b) Public hospital



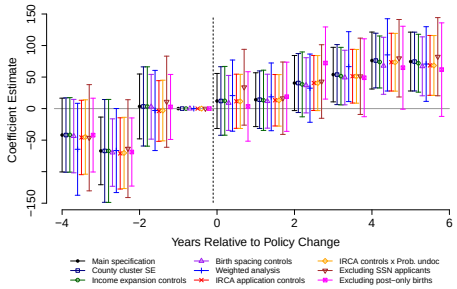
(c) Doctor delivery



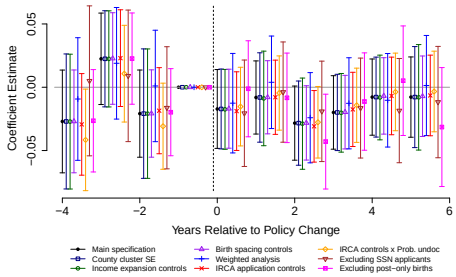
(d) C-section



(e) Gestation age (in days)



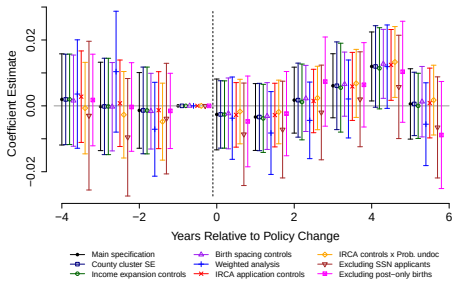
(f) Birthweight (in grams)



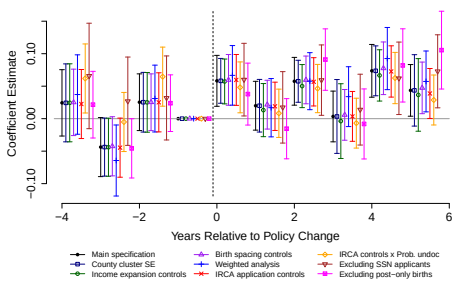
(g) Small for gestational age

Note: This figure reports sensitivity analyses for the event study estimates reported in Figure 3. Analyses use January 1984-October 1994 California birth records linked to the 2000 Censusand 2001-2011 American Community Survey; see text for more specific sample information. Coefficients and confidence intervals are estimated using the event study specification (1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Robust standard errors are clustered by mother and estimated using a bootstrap procedure. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

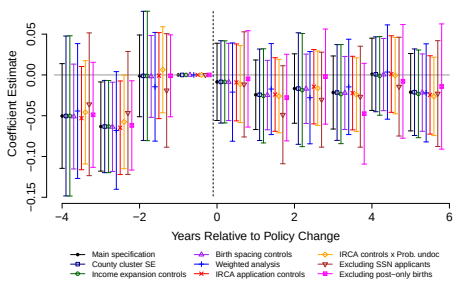
Figure A17: Alternative Specifications for Long-Term Health and Human Capital



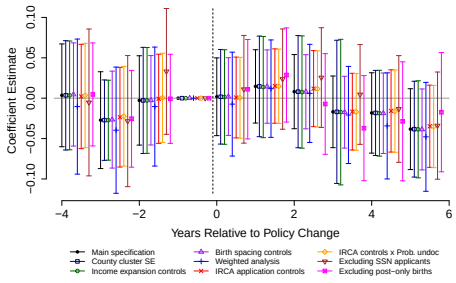
(a) Cumulative mortality



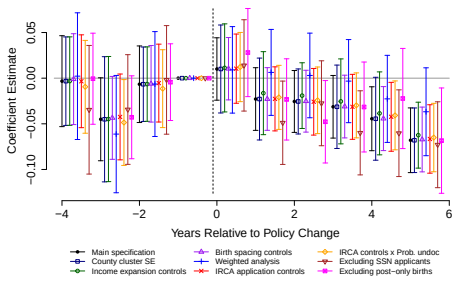
(b) Post-secondary enrollment



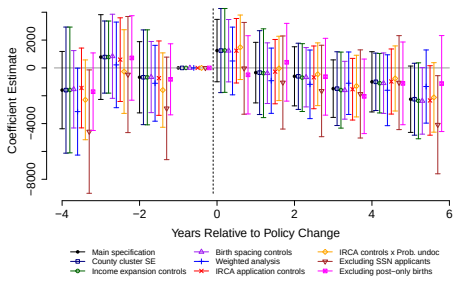
(c) College completion



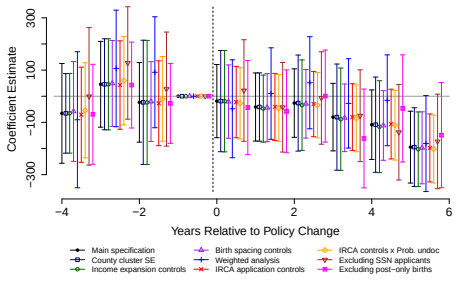
(d) Teen fertility



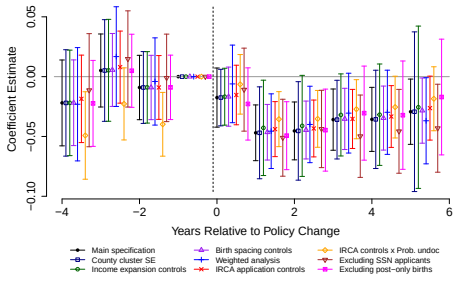
(e) Any fertility



(f) Annual wages



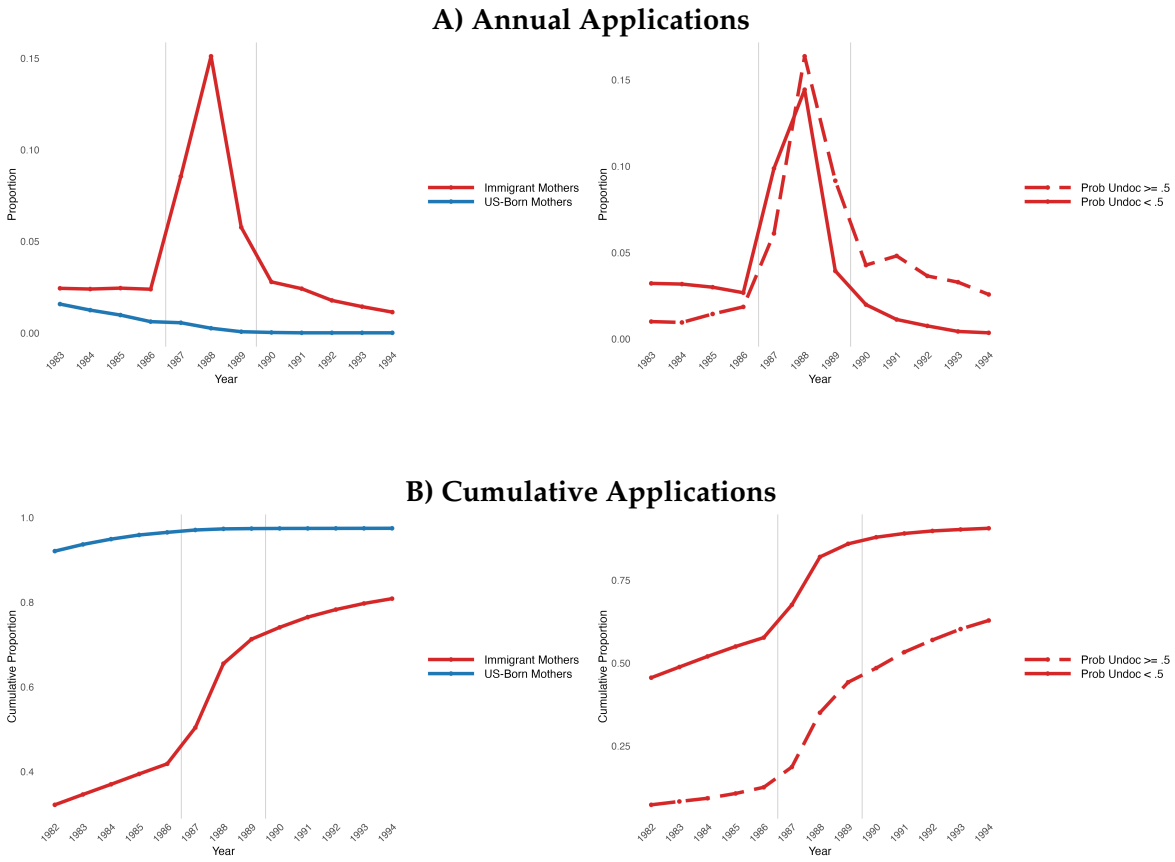
(g) EITC amount



(h) Childhood Medicaid

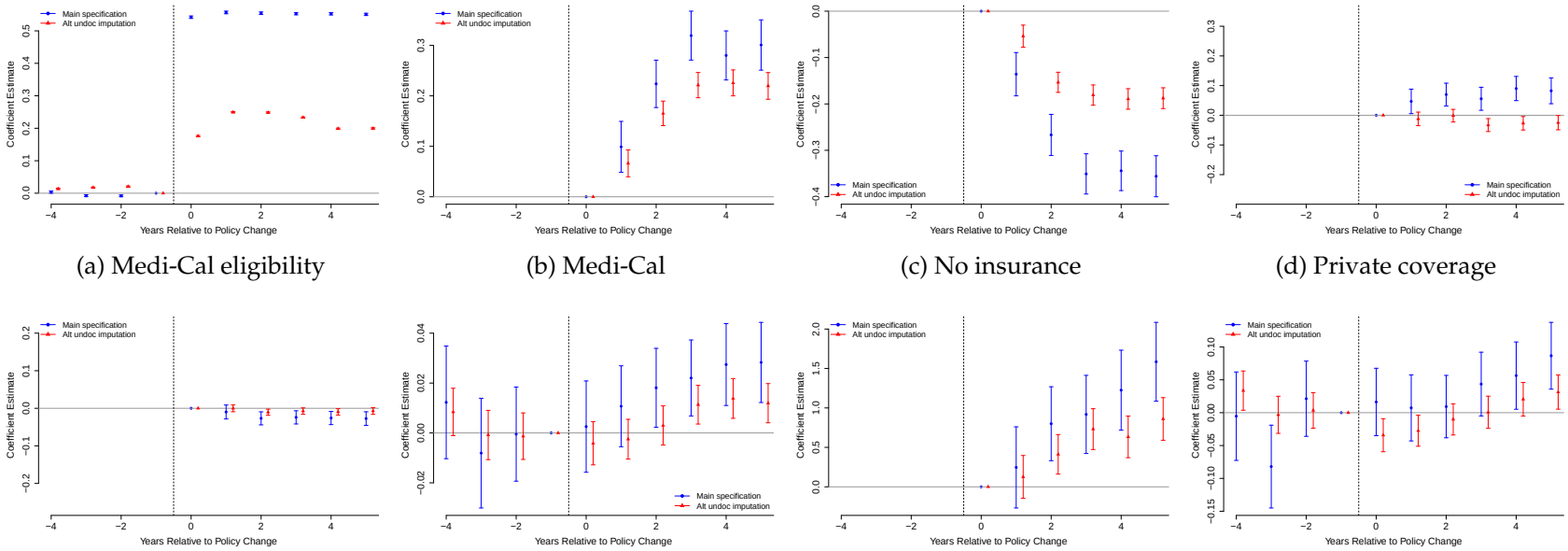
Note: This figure reports sensitivity analyses for the event study estimates reported in Figure 5. Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey, mortality data from the 2022Q3 Census Numident, college attainment as of July 2022 from the National Student Clearinghouse, the 2016-2022 Census Household Composition Key, earnings information from 2007-2022 IRS W-2 forms, EITC amounts calculated from 2007-2021 IRS 1040 forms, and 2000-2016 Medicaid enrollment data from the Centers for Medicare & Medicaid Services; see text for more specific sample information. Coefficients and confidence intervals are estimated using event study specification (1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Calendar year fixed effects are included in analyses of annual outcomes (wages, EITC, Medicaid). Robust standard errors are clustered by mother and estimated using a bootstrap procedure. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

Figure A18: New Applications for Social Security Numbers by Immigration Status



Notes: Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey and data on social security applications from the 2022Q3 Census Numident; see text for more specific sample information. Plots depict the proportion and cumulative proportions of immigrant mothers in the sample, as well as U.S. born mothers otherwise meeting the sample criteria, with new applications for a Social Security card. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Figure A19: Alternative Imputation for Likely Undocumented: Eligibility, Insurance Coverage, and Prenatal Care

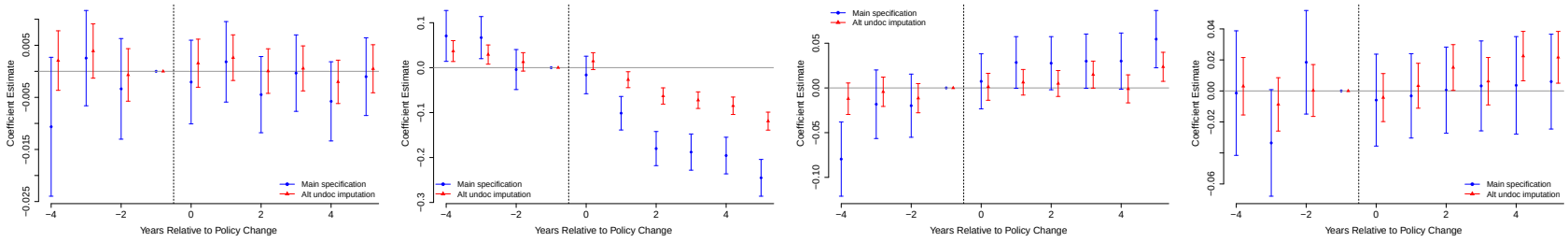


(a) Medi-Cal eligibility (b) Medi-Cal (c) No insurance (d) Private coverage

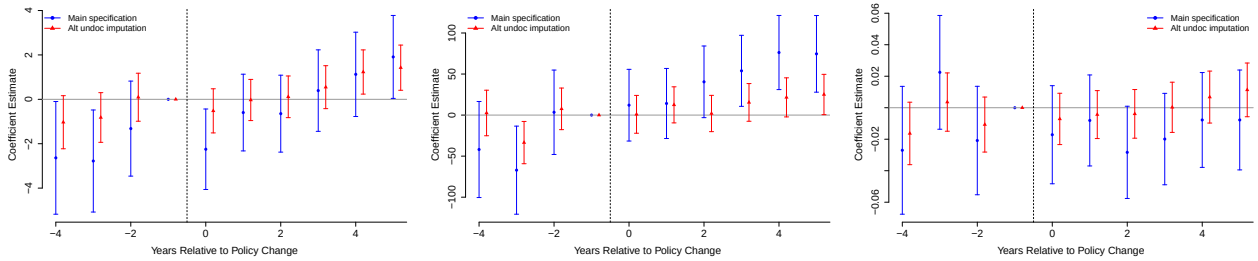
(e) Other coverage (f) Any prenatal care (g) Number of prenatal visits (h) Prenatal care in first trimester

Note: This figure reports an alternative version of the event study estimates reported in Figure 2. Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Medi-Cal eligibility is estimated using a monthly county-level simulated eligibility measure constructed using the 1990 Census and merged on using mother's county of residence, immigrant, and predicted documentation status; see text for more details. Coefficients and confidence intervals are estimated using the event study specification (1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Regression models for prenatal insurance coverage and number of prenatal visits estimate changes in outcomes relative to the first year the policy was in place, due to the limited period of data available. Robust standard errors are clustered by mother, and estimated using a bootstrap procedure for the main specification. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

Figure A20: Alternative Imputation for Likely Undocumented: Delivery Care and Infant Health



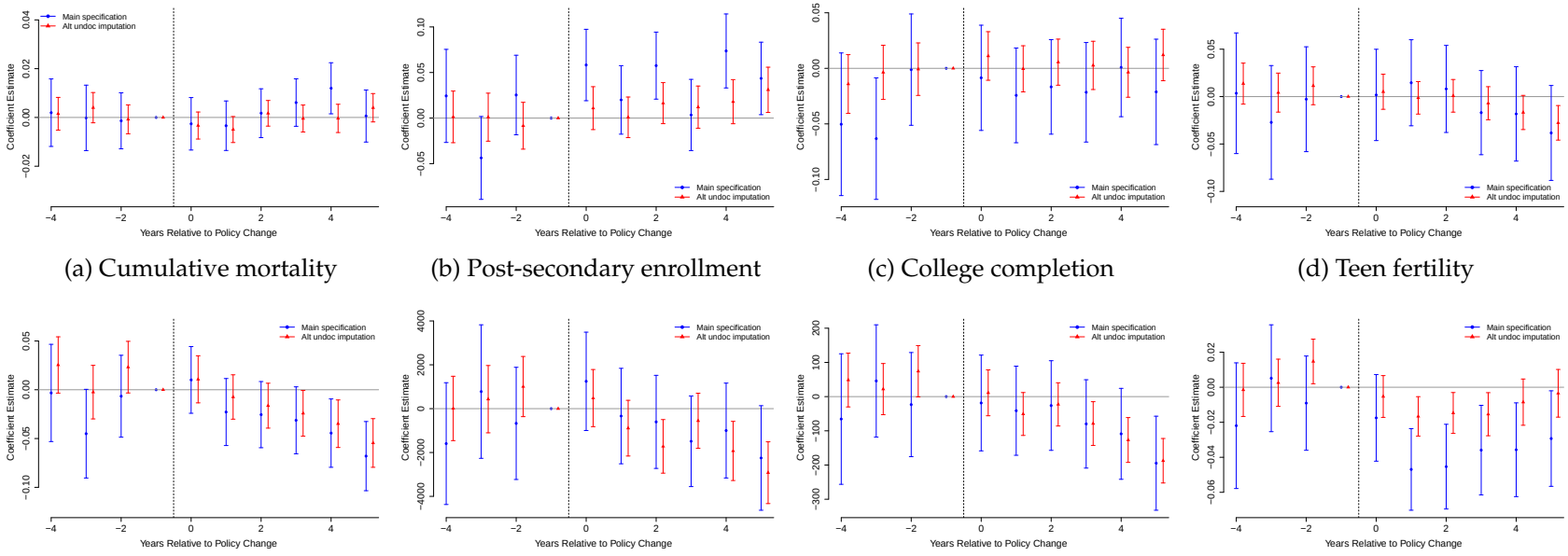
(a) Hospital delivery (b) Public hospital (c) Doctor delivery (d) C-section



(e) Gestation age (in days) (f) Birthweight (in grams) (g) Small for gestational age

Note: This figure reports an alternative version of the event study estimates reported in Figure 3. Analyses use January 1984-October 1994 California birth records linked to the 2000 Censusand 2001-2011 American Community Survey; see text for more specific sample information. Coefficients and confidence intervals are estimated using the event study specification (1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Robust standard errors are clustered by mother, and estimated using a bootstrap procedure for the main specification. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

Figure A21: Alternative Imputation for Likely Undocumented: Long-Term Health and Human Capital



(a) Cumulative mortality (b) Post-secondary enrollment (c) College completion (d) Teen fertility

(e) Any fertility (f) Annual wages (g) EITC amount (h) Childhood Medicaid

Note: This figure reports an alternative version of the event study estimates reported in Figure 5. Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey, mortality data from the 2022Q3 Census Numident, college attainment as of July 2022 from the National Student Clearinghouse, the 2016-2022 Census Household Composition Key, earnings information from 2007-2022 IRS W-2 forms, EITC amounts calculated from 2007-2021 IRS 1040 forms, and 2000-2016 Medicaid enrollment data from the Centers for Medicare & Medicaid Services; see text for more specific sample information. Coefficients and confidence intervals are estimated using event study specification (1) described in the text, which includes birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Calendar year fixed effects are included in analyses of annual outcomes (wages, EITC, Medicaid). Robust standard errors are clustered by mother, and estimated using a bootstrap procedure for the main specification. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012.

Table A1: Characteristics of Births to Immigrant Mothers By PIK and Survey Sample Status

	Total	With PIK	With PIK and in 2000 Census/ACS	Siblings sample
Number of records meeting study criteria	2208000	2123000	270000	132000
Singleton	0.9822	0.9824	0.9837	0.9719
Female	0.4884	0.4890	0.4883	0.4928
Birth number	2.308	2.307	2.274	2.425
First birth	0.3657	0.3655	0.3593	0.2811
Second birth	0.2946	0.2954	0.3061	0.3488
Third birth	0.1736	0.1736	0.1784	0.1952
Fourth birth or higher	0.1661	0.1655	0.1562	0.1749
Mother's age	26.93	26.95	27.44	26.98
Mother's race				
White	0.7705	0.7679	0.7513	0.7812
Black	0.0119	0.0120	0.0105	0.0090
Asian	0.2069	0.2092	0.2293	0.2015
Other	0.0107	0.0108	0.0090	0.0083
Mother's Hispanic ethnicity	0.6855	0.6817	0.6575	0.6936
Mother's country of birth				
China	0.0214	0.0218	0.0269	0.0163
Canada	0.0088	0.0089	0.0104	0.0107
Cuba	0.0034	0.0035	0.0036	0.0034
Japan	0.0096	0.0096	0.0097	0.0087
Mexico	0.5669	0.562	0.5403	0.5813
Philippines	0.0539	0.0541	0.0592	0.0463
Vietnam	0.0342	0.0347	0.0401	0.0397
Rest of the world	0.3018	0.3053	0.3097	0.2936
Health care utilization				
Any prenatal care use	0.9766	0.9773	0.9837	0.9814
Prenatal care during first trimester	0.6763	0.6792	0.7145	0.6942
Delivery in a hospital	0.9944	0.9944	0.9951	0.9943
Delivery in a public hospital	0.3377	0.3366	0.3066	0.3169
Delivery by a doctor	0.9168	0.9172	0.9950	0.9151
C-section	0.1956	0.1958	0.2024	0.1865
Birth outcomes				
Birth weight	3364	3369	3380	3388
Gestational length	277.2	277.4	277.5	277.5
Small for gestational age	0.0982	0.0940	0.0935	0.0907

Note: Table presents average characteristics from the birth certificate records. Study criteria defined births during Jan. 1984 - Oct. 1994 to mothers residing in the state of California and with non-missing information on mother's county of residence, parity, birth order, sex, and mother's country of birth. Siblings sample is constructed using mother-child links from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-183, CBDRB-FY23-CES-021-001, and CBDRB-FY23-CES-021-001. All numbers have been rounded to comply with disclosure avoidance guidelines.

Table A2: Descriptive Statistics for Immigrant Mothers, By Undocumented Status

	Pr(Undocumented)≥.5	Pr(Undocumented)<.5
Age at first birth in sample	23.34	26.34
Number of kids in sample	2.209	2.268
Total kids at time of last birth in sample	2.791	3.135
Mother's race and ethnicity		
Non-Hispanic white	0.0360	0.1255
Non-Hispanic Black	0.0019	0.0117
Hispanic	0.9001	0.5738
Non-Hispanic Asian	0.0568	0.2802
Non-Hispanic other race	0.0049	0.0084
Country of birth (from birth record)		
China	0.0149	0.0185
Canada	S	0.0167
Cuba	S	0.0054
Japan	0.0059	0.0106
Mexico	0.7078	0.5051
Philippines	S	0.0747
Vietnam	S	0.0625
Rest of the world	0.2713	0.3066
Other common countries of birth (from ACS/Census)		
Cambodia	S	0.0189
Germany	S	0.0143
India	0.0080	0.0125
El Salvador	0.1044	0.0266
Guatemala	0.0576	0.0090
Nicaragua	0.0151	0.0038
Korea	0.0142	0.0206
Taiwan	0.0067	0.0139
Probability undocumented	0.6503	0.1452
Year of entry into the U.S. (from ACS/Census)		
Before 1982	0.0522	0.7473
1982-1984	0.1480	0.1303
1985-1986	0.2417	0.0553
1987-1990	0.4484	0.0502
1991 or later	0.1097	0.0169
Age at time of survey	35.65	39.75
Education at time of survey		
Less than high school degree	0.6603	0.4504
High school degree or GED	0.1669	0.1656
Some college	0.1137	0.2302
College or more	0.0591	0.1538
Married parent family at time of survey	0.7665	0.7397
Number of observations	21000	38000

Notes: Analyses use 1984-1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey. Sample defined as CA births during Jan. 1984 - Oct. 1994 to moms with at least two births during this period, after excluding births to women residing outside of the state of California or with missing information for county of residence, nativity, child's sex, plurality, or mother's total live birth number. Mother-child links are established from the 2000 Census and 2001-2011 ACS when the child is age 17 or younger and residing in a household with their mother, as identified using family relationship variables. We exclude siblings with mothers who have discordant place of birth and age information on the birth certificate records, as well as those with mothers who have discordant place of birth information on the birth certificate record and Census/ACS record. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY20-183 and CBDRB-FY23-CES-021-001. All numbers have been rounded to comply with disclosure avoidance guidelines. "S" denotes that estimate has been suppressed due to small cell sizes.

Table A3: Coefficient Estimates from Probit Regression for Undocumented Status in 1990 Census Sample

Age			Ecuador	0.141	(0.217)	Syria	0.155	(0.222)
Age 16	0.387	(0.421)	Guyana	-0.536	(0.604)	Yemen Arab Republic (North)	-0.492	(0.672)
Age 17	0.841**	(0.382)	Paraguay	0.868	(0.743)	Egypt	0.0974	(0.240)
Age 18	0.803**	(0.368)	Peru	0.0458	(0.120)	Morocco	-0.576	(0.631)
Age 19	0.932***	(0.361)	Uruguay	0.761*	(0.462)	Ghana	0.0168	(0.902)
Age 20	0.797**	(0.357)	Venezuela	0.157	(0.354)	Nigeria	-1.090***	(0.308)
Age 21	0.802**	(0.356)	Finland	0.107	(0.395)	Ethiopia	-0.0564	(0.263)
Age 22	0.930***	(0.355)	Iceland	-0.234	(0.883)	Kenya	0.0886	(0.394)
Age 23	0.812**	(0.354)	Norway	-0.740	(0.572)	Tanzania	-0.0582	(0.696)
Age 24	0.797**	(0.354)	Sweden	-0.317	(0.400)	Uganda	0.376	(0.657)
Age 25	0.771**	(0.353)	England	0.0373	(0.125)	Zimbabwe	-0.0349	(0.568)
Age 26	0.716**	(0.353)	Scotland	0.470	(0.364)	South Africa	0.138	(0.241)
Age 27	0.616*	(0.353)	UK	0.529**	(0.244)	Africa (residual)	-0.518	(0.392)
Age 28	0.667*	(0.353)	Ireland	-0.0976	(0.292)	Australia	-0.419	(0.257)
Age 29	0.621*	(0.353)	Belgium	0.226	(0.421)	New Zealand	-0.306	(0.353)
Age 30	0.738**	(0.353)	France	0.172	(0.219)	Fiji	0.228	(0.204)
Age 31	0.566	(0.354)	Netherlands	-0.372	(0.333)	Tonga	0.104	(0.271)
Age 32	0.593*	(0.354)	Switzerland	-0.373	(0.365)	County of Residence		
Age 33	0.596*	(0.354)	Greece	-0.824*	(0.446)	Alameda	0.150	(0.103)
Age 34	0.592*	(0.355)	Italy	-0.478*	(0.286)	Butte	-0.221	(0.265)
Age 35	0.543	(0.355)	Portugal	0.0556	(0.274)	Contra Costa	0.103	(0.121)
Age 36	0.597*	(0.356)	Azores	0.724	(0.570)	El Dorado	0.409	(0.265)
Age 37	0.585	(0.357)	Spain	-0.389	(0.291)	Fresno	-0.0404	(0.112)
Age 38	0.561	(0.357)	Austria	-0.666	(0.668)	Humboldt	-0.413	(0.457)
Age 39	0.665*	(0.359)	Bulgaria	0.442	(0.928)	Imperial	-0.144	(0.168)
Age 40	0.645*	(0.360)	Germany	-0.741***	(0.240)	Kern	0.0262	(0.125)
Age 41	0.468	(0.367)	Hungary	-0.0775	(0.358)	Kings	-0.361	(0.241)
Age 42	0.513	(0.368)	Romania	-0.554**	(0.246)	Los Angeles	0.320***	(0.0893)
Age 43	0.522	(0.373)	Yugoslavia	-0.724*	(0.372)	Marin	0.0319	(0.200)
Age 44	0.453	(0.381)	Other USSR/"Russia"	-0.476***	(0.114)	Merced	0.299**	(0.138)
Year of Entry to the US			China	0.0355	(0.0822)	Monterey	0.0369	(0.127)
1980-1981 entry	4.716	(147.3)	Hong Kong	-0.370**	(0.145)	Napa	0.159	(0.252)
1982-1984 entry	4.946	(147.3)	Taiwan	0.129*	(0.0756)	Orange	0.364***	(0.0934)
1986-1986 entry	5.169	(147.3)	Japan	0.212**	(0.0862)	Placer	0.0769	(0.457)
1987-1990 entry	5.393	(147.3)	Korea	0.0939	(0.0669)	Riverside	0.175*	(0.105)
Country of Birth			Cambodia (Kampuchea)	-0.686***	(0.0945)	Sacramento	0.0663	(0.126)
Canada	-0.455***	(0.140)	Indonesia	0.0497	(0.189)	San Bernardino	0.158	(0.105)
Mexico	0.323***	(0.0412)	Laos	-0.457***	(0.0839)	San Diego	0.0893	(0.0958)
Belize/British Honduras	-0.422*	(0.233)	Malaysia	0.00456	(0.273)	San Francisco	0.316***	(0.113)
Costa Rica	0.0923	(0.299)	Philippines	-0.759***	(0.0579)	San Joaquin	-0.0389	(0.124)
El Salvador	0.553***	(0.0549)	Singapore	-0.0125	(0.323)	San Luis Obispo	0.135	(0.239)
Guatemala	0.635***	(0.0673)	Thailand	-0.333*	(0.196)	San Mateo	0.241**	(0.116)
Honduras	0.352***	(0.118)	Vietnam	-0.631***	(0.0653)	Santa Barbara	0.206	(0.134)
Nicaragua	0.668***	(0.103)	India	0.00712	(0.0839)	Santa Clara	0.412***	(0.0984)
Panama	-0.498*	(0.278)	Bangladesh	0.381	(0.373)	Santa Cruz	0.133	(0.192)
Dominican Republic	-1.202**	(0.532)	Burma (Myanmar)	-1.172**	(0.550)	Shasta	0.376	(0.350)
Jamaica	-0.695	(0.435)	Pakistan	-0.262	(0.168)	Solano	-0.299*	(0.164)
Barbados	-0.0854	(0.885)	Sri Lanka (Ceylon)	-0.234	(0.382)	Sonoma	-0.125	(0.183)
Trinidad	-0.680*	(0.381)	Iraq	-0.362	(0.295)	Stanislaus	0.0232	(0.142)
Argentina	0.467**	(0.182)	Israel/Palestine	-0.0671	(0.143)	Tulare	-0.00498	(0.126)
Brazil	0.152	(0.242)	Jordan	-0.541*	(0.276)	Ventura	-0.0346	(0.119)
Chile	0.000303	(0.204)	Lebanon	-0.479**	(0.186)	Yolo	-0.341	(0.221)
Colombia	0.248*	(0.138)	Saudi Arabia	0.0563	(0.523)	Constant	-6.215	(147.3)

Notes: N=18,195; sample composed of immigrant mothers ages 15-44 with children under age 6 residing in California in the 1990 Census. Coefficient estimates and standard errors from a probit regression of imputed documentation status on age, county of residence, year range of U.S. entry, and country of birth dummies. Stata dropped observations when certain predictors perfectly predicted the outcome variable, which were the following: U.S. entry during years between 1950 to 1979 and birth in Cuba, Haiti, Dominican Republic, Grenada, West Indies, South America (residual), Kuwait, Algeria, Liberia, and Zambia. See text for additional details on undocumented imputation and prediction model.

Table A4: Correlates of Mother's Fixed Effects Estimated for Birthweight

	(1)	(2)
Hispanic ethnicity	-35.46***	-62.36***
	(6.40)	(8.28)
Non-Hispanic, Black race	-54.47***	-44.17**
	(17.78)	(17.95)
Non-Hispanic, other race	-17.22	-6.47
	(21.82)	(21.91)
Non-Hispanic, Asian race	-199.00***	-185.10***
	(6.30)	(7.44)
China		35.46**
		(14.18)
Canada		83.45***
		(13.77)
Cuba		21.19
		(25.70)
Japan		33.35**
		(15.60)
Mexico		48.64***
		(5.57)
Philippines		2.40
		(8.62)
Vietnam		-32.90***
		(8.24)
Entered US between 1982-1987	-46.08***	-44.47***
	(4.91)	(4.90)
Entered US in 1988 or later	-90.87***	-92.22***
	(7.55)	(7.54)
Married at time of survey	24.05***	22.92***
	(3.22)	(3.22)
Less than high school at time of survey	-8.00**	-11.10***
	(3.60)	(3.63)
County-level income per capita	-4.14***	-3.92***
(in 1000s)	(1.05)	(1.04)
N	58,500	58,500

Notes: Analyses use 1984-1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey; see text for more specific sample information. Robust standard errors are estimated with a bootstrap procedure. Significance levels: *=10%, **=5%, ***=1%. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.

Table A5: Alternative Imputation of Likely Undocumented Status Using Hispanic Ethnicity and Low Education

	Medi-Cal Eligibility	Primary Payer for Prenatal Care				Prenatal Care Utilization		
		Medi-Cal	No Insurance	Private	Other	Any	Number of Visits	Early Initiation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Undocumented expansion x likely undocumented	0.211***	0.181***	-0.155***	-0.007*	-0.018*	0.002	0.542	-0.019***
	(0.001)	(0.011)	(0.010)	(0.004)	(0.010)	(0.002)	(0.112)	(0.007)
Baseline mean	0.357	0.294	0.350	0.330	0.027	0.966	8.41	0.608
N	130,000	65,500				129,000	63,500	129,000
	Hospital delivery	Public hospital	Doctor delivery	C-section	Gestation days	Birthweight (gram)	Small for gestational age	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	
Undocumented expansion x likely undocumented	0.000	-0.063***	0.013***	0.008*	0.583**	15.15**	0.004	
	(0.001)	(0.005)	(0.004)	(0.005)	(0.274)	(6.61)	(0.004)	
Baseline mean	0.994	0.489	0.901	0.166	278.7	3411	0.088	
N	131,000	131,000	132,000	132,000	124,000	132,000	124,000	
	Cumulative mortality Ages 0-27	Ever enrolled post-secondary	Graduated college	Teen fertility Ages 14-19	Any fertility Ages 14-26	Annual wages Ages 23-28	EITC amount Ages 25-27	Childhood Medicaid Ages 16-18
	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
Undocumented expansion x likely undocumented	-0.002	0.012*	0.005	-0.010*	-0.025***	-1227***	-91.02***	-0.014***
	(0.002)	(0.007)	(0.006)	(0.005)	(0.007)	(375.2)	(18.1)	(0.004)
Baseline mean	0.009	0.601	0.137	0.190	0.488	22,350	914.9	0.388
N	132,000	132,000	130,000	132,000	132,000	791,000	395,000	395,000
N (unique individuals)	132,000	132,000	130,000	132,000	132,000	132,000	132,000	132,000

Notes: This figure reports an alternative version of the difference-in-differences estimates reported in Tables 1, 2, and 4. Analyses use January 1984-October 1994 California birth records linked to the 2000 Census and 2001-2011 American Community Survey, mortality data from the 2022Q3 Census Numident, college attainment as of July 2022 from the National Student Clearinghouse, the 2016-2022 Census Household Composition Key, earnings information from 2007-2022 IRS W-2 forms, EITC amounts calculated from 2007-2021 IRS 1040 forms, and 2000-2016 Medicaid enrollment data from the Centers for Medicare & Medicaid Services; see text for more specific sample information. Coefficient and standard errors are estimated using a difference-in-differences model with birth order dummies, indicators for female and singleton births, birth year x birth month fixed effects, county of residence fixed effects, annual county controls for poverty and per capita income, and mother fixed effects. Calendar year fixed effects are included in analyses of annual outcomes (wages, EITC, Medicaid). Robust standard errors are clustered by mother. Significance levels: *=10%, **=5%, ***=1%. Alternative definition of likely undocumented is Hispanic ethnicity and high school or less educational attainment. Baseline means are calculated for cohorts born before October 1988 among births with a mother with this status. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY26-012. Numbers have been rounded to comply with disclosure avoidance guidelines.