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Essays in Applied Microeconomics

Laxman Timilsina
The Graduate Center, City University of New York

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by

Laxman Timilsina

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Professor Prabal K. De

Date | Chair of Examining Committee

Professor Wim P.M. Vijverberg

Date | Chair of Examining Committee

Professor Christos Giannikos

Date | Executive Officer

Professor Jonathan Conning
Professor Prabal K. De
Professor Wim P.M. Vijverberg

Supervisory Committee

THE CITY UNIVERSITY OF NEW YORK

Abstract

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Advisers: Prabal K. De and Wim P.M. Vijverberg

This dissertation consists of three chapters.

Chapter 1 Immigration Policy Shocks and Infant Health This paper evaluates the effect of positive and negative immigration policy shocks on infant health outcomes in the U.S. I examine changes in mean birth weight and the incidence of low birth weight (LBW) at the metropolitan statistical area (MSA) level around two major institutional shocks: The 1986 Immigration Reform Act (IRCA), which favored immigrants, and the increase in Immigration and Customs Enforcement (ICE) agency arrests at the start of 2017 which might have put immigrants at greater risk of apprehension. It uses a triple difference approach, comparing birth outcomes of foreign-born mothers with U.S.-born mothers (relative to mothers living in MSAs with a higher and lower concentration of IRCA applications and an increase in ICE arrests) before and after the two immigration policies. I find that in MSAs that had higher IRCA applications, mean birth weight increased, and the incidence of LBW decreased by 3−6 percent for babies born to foreign-born mothers. By contrast, in MSAs that had higher ICE arrests starting in 2017, mean birth weight decreased, and the incidence of LBW increased by 3−7 percent for babies born to foreign-born mothers. The effect of the increase in ICE arrests was more pronounced among mothers who were born in Latin and Central American countries. Sub-sample analysis shows that the incidence of LBW increased by as much as 12 percent for babies born to foreign-born mothers of Hispanic origin.

Chapter 2 Cash-based Maternal Health Interventions Can Improve Childhood Vaccination - Evidence from India Childhood vaccination has lagged in countries like India, despite its demonstrated positive effects on health outcomes like infant mortality. At the same time, many Conditional Cash Transfer programs have been effective in improving health outcomes. We estimate the effect of the world's largest maternal health program, Janani Surakhsa Yojana (JSY, Maternal Protection Scheme), on childhood vaccination in India. We exploit exogenous variations in the expansion of the policy around the country and the fact that some key vaccines are given at or near birth to identify the effects of cash-based maternal health policy on infant immunization. We find that JSY increased the probability of BCG and DPT vaccination among newborns and infants. However, we find almost no significant effects on the measles vaccine, which is administered several months

after birth.

Chapter 3 Political Connections and Household Welfare: Evidence from India
Using nationally represented survey data from India, this paper estimates the effect of knowing a politician on household welfare by first looking at the household's ability to capture public welfare programs, followed by jobs prospect of the household members. To rule out the potential endogeneity, household's acquaintance to politicians is instrumented with percentage of household within each village that are member of Self-Help Groups (SHGs). I propose that higher prevalence of SHGs in the village, a traditional micro money lending scheme in India, increases the probability of knowing a politician. Results show that knowing a politician at a local level vastly improves the chances of a household to obtain welfare programs like ration cards and insurance cards. Similarly, members of households that know a politician at the state and federal level are more likely to have a government job and less likely to work as daily wage laborers.

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Chapter 1

Immigration Policy Shocks and Infant Health

1.1 Introduction

The political debate on immigration is at a historic peak on both sides of the Atlantic.[1] While the majority of the literature looks at the impact of immigration policies on the labor market and crime[2], this paper evaluates one of the lesser studied consequences of immigration policies — impact on infant health, specifically, mean birth weight and incidence of low birth weight (LBW), which is one of the key indicators of lifetime health and economic well-being. I analyze differences in birth outcomes around two major institutional shocks: the 1986 Immigration Reform Act (IRCA) and the increase in Immigration and Customs Enforcement

[1] For example, the response to North African immigrants crossing the Mediterranean, the influx of refugees from Syria's civil war into Europe, and the rise of political rhetoric against undocumented immigrants in the United States and the increasing Immigration and Customs Enforcement (ICE) arrests of immigrants during and after the 2016 presidential election.

[2] Considerable literature on economics has been devoted to determining the cause and effect of immigration. A primary concern of immigration is its impact on the labor market, especially on the natives' wages (Card, 1990; Borjas, 2003; Dustmann et al., 2016).Other research focuses on its effect on legal status and crime (Butcher and Piehl, 1998; Bell et al., 2010; Baker, 2015). Few but growing studies have looked at the impact of immigration on child health (Amuedo-Dorantes and Mundra, 2005; Salmasi and Pieroni, 2015).

(ICE) agency arrests at the start of 2017.

The IRCA was an institutional shock that favored immigrants by legalizing over three million people, almost 1.1 percent of the total population, implying a substantial shift in the lives of both immigrants and the U.S.-born population (Baker, 2015). This law, dubbed one of the most substantial amnesty provisions to date, undoubtedly favored immigrants.[3] On the contrary, the intense debate surrounding the issue of immigration during the 2016 election and the subsequent Executive Order 13,768 issued at the start of the new administration was an institutional shock that put immigrants at risk, as it led to a spike in Immigration and Customs Enforcement (ICE) agency arrests. For example, comparison of ICE arrests from January 20, 2016 through the end of the fiscal year with the same period in the fiscal year 2017 shows a 42 percent increase, from 77,806 to 110,568 in 2017 (ICE, 2017). This increase coupled with an increasing frequency of hate crimes — a rise of 17 percent from 2016 to 2017 (FBI, 2017)— put immigrant communities at a heightened sense of fear and/or concern of being deported (Perreira and Pedroza, 2019).

A study of the impact of these two immigration policies on child health is important for a number of reasons. First, early childhood shocks, especially *in utero*, can have long-term health and economic effects that extend to adulthood (Almond and Currie, 2011; Almond et al., 2017). Second, low birth weight can result in high costs, directly as medical expenses[4] and indirectly as long-term health and other cognitive development consequences.[5] Third, the impact of immigration laws can be felt for generations, as those born to immigrants are

[3] After the law was enacted, the twin Legalized Population Surveys (LPS1 and LPS2) were conducted in 1989 and 1992. Of the respondents, 75 percent said that finding work was "somewhat or much easier" after the IRCA. Similarly, reported wages increased by 30 percent to 40 percent (Baker, 2015). Although no surveys or research have been done to look at the law's impact on child health, it is likely that due to increase in wage and employment, the latter of which typically means an increase in health insurance coverage, as well as the lack of fear of deportation among previously undocumented immigrants, the law might have improved health outcomes for infants.

[4] Studies estimate that a representative newborn weighing 2000g or less can result in direct hospital costs of $15,000 (Almond et al., 2005; Abrevaya and Dahl, 2008).

[5] The effects of early health measured through LBW on adult outcomes are set early in life and tend to remain constant over time (Figlio et al., 2014).

automatically U.S. citizens and most stay in the country (Camarota et al., 2018).[6] Given that approximately one in five births in the U..S are to immigrant mothers, and annually around 300,000 births are to undocumented mothers (Camarota et al., 2018), understanding the health impact on these infants should be a significant public policy interest. It is well documented in the literature that immigrants (especially undocumented) report high level of stress, depression and anxiety disorders (Giuntella et al., 2020; Perreira and Pedroza, 2019). Because of the lack of work authorization, health insurance and limited or no access to welfare programs immigrants live with unmet healthcare needs and often have worse self-reported health outcomes (Giuntella et al., 2020).These problems are further magnified by anti-immigrant rhetoric, hate crimes, negative media coverage, language barriers, and the lack of knowledge of often complex healthcare services in the U.S. (Martinez et al., 2015; Suárez-Orozco and Hirokazu, 2013). Thus, adding the fear of deportation on top of already existing precarious socio-economic conditions may increase the stress level of pregnant mothers resulting in adverse birth outcomes.

Using Natality microdata from the National Vital Statistics System, which covers the full census of births in the U.S., I construct birth outcome variables and mothers' nativity. Since it is difficult to identify who is an immigrant mother (either documented or undocumented) in the data, I first compare yearly birth outcomes by mothers' nativity (U.S.-born versus foreign-born) before and after the two institutional shocks, 1983–1987 and 1988–1992 for the IRCA law and 2013–2016 and 2017–2018 for increase in ICE arrests. Additionally, I compare mothers born in Latin American, Central American countries and Mexico with the U.S.-born mothers. Interpreting the double-difference as causal estimates of the policies would require that the trends in birth outcomes between U.S. and foreign-born mothers be parallel. However, the birth outcomes of U.S.-born mothers might be characteristically

[6] The 14th Amendment: "All persons born or naturalized in the United States, and subject to the jurisdiction thereof, are citizens of the United States and of the State wherein they reside..."

different than foreign-born mothers, and the healthcare intake between them could also be different. Further, not all foreign-born mothers are immigrants; they could have become a naturalized U.S citizens and as such might not be affected by the policies.

To overcome these concerns, I employ a triple difference strategy. Exploiting the variations in IRCA applications and an increase in ICE arrests by Metropolitan Areas (MSAs), I compare the double-difference with mothers living in different types of MSAs. I use the data from the 1990 Legalization Summary Tapes created by the Immigration and Naturalization Service (now US Citizenship and Immigration Service), which lists total application and approved applications by counties to proxy MSAs that had relatively more undocumented immigrants. I classify MSAs that had higher per capita[7] applications as "treated" MSAs and those who had fewer (less than the mean and median value) as "control" MSAs. Similarly, I use the data from Transactional Records Access Clearinghouse (TRAC) at Syracuse University, which has detailed arrests by ICE by counties from 2014 to 2018 (TRAC, 2018) to proxy the "fear" felt by the immigrants following the 2016 election. I classify MSAs that had a higher per capita increase in arrests (total from February 2017 to May 2018 minus total from October 2015 to January 2017) as above and create treated and control MSAs. Foreign-born mothers living in MSAs that had lesser or no IRCA applications are more likely to be U.S. citizens. Similarly, foreign-born mothers living in MSAs who saw no changes in ICE arrests might not have felt a heightened sense of stress and fear of deportation and arrests and may therefore serve as control groups.

Using the DD and DDD method, this paper measures the impact of the two immigration policies on mean birth weight and LBW, defined as less than 2,500 grams. These outcomes are standard in measuring infant health and are highly correlated with the long-term health and economic wellbeing of an individual (Almond and Currie, 2011; Hoynes et al., 2015a). I

[7] I divide the applications by MSA's foreign-born population. I use per-capita applications/arrests and per-capita foreign-born population interchangeably.

further look at possible mechanisms that might drive these birth outcomes such as maternal health behaviors and prenatal care (PNC) utilizations. Moreover, contrary to popular belief, the effect of anti-immigration rhetoric and fear of detention and deportations might not be only limited to unauthorized immigrants. The stress and anxiety surrounding deportation and anti-immigrant sentiment are also prevalent among documented immigrants and U.S. citizens whose legal presence is questioned because of race/ethnicity (Wang and Kaushal, 2018; Perreira and Pedroza, 2019). Thus, I also conduct sub-sample analyses by mothers' race and ethnicity for increase in ICE arrests.

I find that infants born to foreign-born mothers in MSAs that had higher number of per-capita IRCA applications weighed, on average, 14 grams more. More importantly, there is a significant decrease in the incidence of LBW for foreign-born mothers living in MSAs that had per-capita application greater than mean and median - a decrease of 6 and 3 percent, respectively. The effect is more pronounced among mothers born in Mexico (a decrease in the incidence of LBW by 10 and 12 percent). Similarly, there is a significant improvement in the utilization of prenatal care visits. The number of PNC visits increased by 2 visits and the percentage of mothers with no PNC care decreased. The impact of increased ICE arrests had the opposite effect. Infants born to foreign-born mothers in MSAs that had a higher number of increases in ICE arrests weighed on average 12 grams less. More importantly, there is a significant increase in the incidence of LBW among foreign-born mothers living in MSAs that had an increase in ICE arrests of higher than mean and median – an increase of 6 and 3 percent, respectively.

The effect is more pronounced among mothers born in Latin American countries (an increase in the incidence of LBW by 7 and 6 percent). Similarly, for infants born to mothers from Central American countries, the incidence of LBW increased by 8 and 7 percent. The uses of prenatal care decreased slightly, but the coefficients are not significant. Furthermore, the mean birth weight decreased by more than 20 grams, and the incidence of LBW increased by 11 and 12 percent for foreign-born Hispanic mothers. The birth outcomes are not significant for non-Hispanic White and Black mothers. The results are consistent across various models and robustness checks.

Although the debate on immigration is mostly focused on wages and crime, these results indicate that policymakers should also focus on the health consequences of immigration policies, especially for newborn babies. Low birth weight is one of the leading indicators of infant health and a key risk factor in infant mortality. In fact, the mortality rate is 24 times higher for babies with low birth weights than those with normal birth weights (Matthews and MacDorman, 2013). Similarly, prenatal care visits are crucial in detecting high-risk pregnancies and other complications. The American Academy of Pediatrics and the American College of Obstetricians and Gynecologists recommends between 9 and 15 PNC visits, starting in the first trimester (AAP, 2012). Given the importance of LBW and PNC utilization for pregnant mothers, this paper is one of the first to comprehensively examine the impact of negative and positive immigration laws and policies in the U.S. especially for infants born to immigrants (documented and undocumented) mothers.

1.2 Background

The number of people living in a country they were not born in is increasing across the globe. In 1980, approximately 100 million people lived in a country other than that of their birth (Friedberg and Hunt, 1995). By 2017, the number of immigrants had increased to 258 million (UN, 2017). The highest numbers of immigrants reside in the Euro Zone (78 million) and the U.S. (50 million). In Europe and in the U.S, this large presence of immigrants, either documented and undocumented, has stimulated debate among politicians and policymakers. In political discourse, the current rightward shift of western democracies is often blamed on immigration.

While the primary concerns of a large immigrant population are employment, wages, crime, and the uses of social safety nets, one important, yet less discussed, issue is child health. With unprecedented worldwide migration, countries increasingly have used denial of access to healthcare to dissuade immigration (Hacker et al., 2015). Thus, immigrants, especially undocumented, have little access to medical care, and even when they have access, the fear of being reported to immigration authorities stops them from utilizing it. Immigrants, documented or not, underutilize healthcare and have less health insurance coverage than their native counterparts either by governmental policy design or because of their inability to navigate the complex healthcare system, bureaucratic issues and discrimination (Hacker et al., 2015; Khanlou et al., 2017). Martinez et al. (2015) conducted a systematic review of literature that examined the impact of immigration policies on health status among undocumented immigrants throughout the world. They report that the "majority of studies established a clear association between harsh immigration policies and mental health outcomes such as depression, anxiety, and post-traumatic stress disorder."[8]

While the direct result of immigrant-averse health policies is a lack of access to health care, intangible aspects, like psychological trauma, are often more troublesome. In particular, a mother's trauma while pregnant affects the newborn's immediate and long-term health and cognitive abilities. Ample medical and psychological literature has linked stress during pregnancy with birth outcomes. Psychosocial stressors can affect pregnant mothers by shifting stress hormone balances to affect a developing fetus, triggering premature birth, and leading to growth restriction and low birth weight (Novak et al., 2017).[9] Severe stress on the parent can affect the offspring's health, including psychopathology, or so-called inter-

[8]Health outcomes like, PTSD, anxiety and depression were found not only in adult undocumented immigrants but also among young children. For more see (Martinez et al., 2015).

[9]The hypothesis is that, such stress disrupts the balance of maternal glucocorticoid levels and 11 beta-hydroxysteroid dehydrogenase type 2 (HSD2), an enzyme that metabolizes cortisol into inactive cortisone. Glucocorticoid concentrations and lower placental HSD2 are linked to LBW (O'Donnell et al., 2011; Novak et al., 2017).

generational transmission of stress (Bowers and Yehuda, 2016). The psychological trauma can be exacerbated when the fear increases.

In the next subsections, I discuss in detail two institutional shocks (Immigration Reform and Control Act (IRCA) of 1986, and immigration rhetoric during and the increase in ICE arrests after the 2016 election) that affected immigrant communities, along with other policies (Medicaid Expansion in 1986 and 2010) which may have impacted the birth outcomes of infants.

1.2.1 Immigration Reform and Control Act of 1986

Broadly, immigration can be divided into two kinds: legal (documented) and illegal (undocumented). In the U.S., documented immigrants can legally work and obtain certain welfare benefits; undocumented immigrants often remain underground and work for lower wages with no benefits (Fasani, 2014). The number of undocumented immigrants was estimated to be 1.5 million in 1980, increasing to 11 million by 2018 (Fazel-Zarandi et al., 2018; Passel, 1986). Because these people often live in an informal, 'shadow' economy, there is an ongoing debate about their future. One policy initiative that dealt favorably with undocumented immigrants in the United States is the 1986 Immigration Reform and Control Act (IRCA). In the U.S., immigration laws have been passed and amended since 1929, and from 1986 to 2009, approximately four million immigrants have been legalized (Kerwin, 2010). Historically the living conditions of undocumented immigrants have been consistently poor.[10]

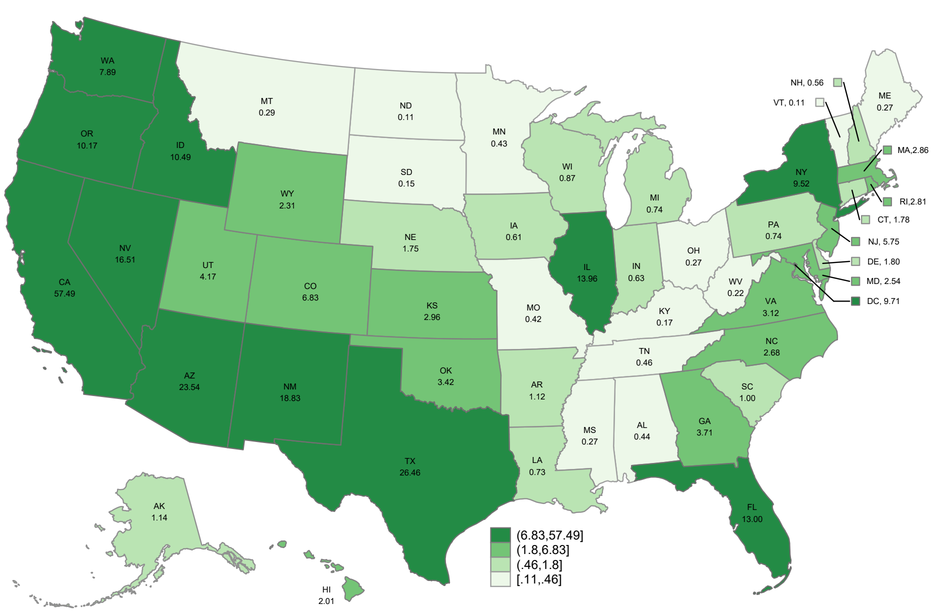
Following such reports, the US Congress with bipartisan support passed IRCA in October 1986. This is one of the most significant immigration policy changes that positively

[10]For example: "In 1951 the President's Commission on Migratory Labor condemned the abysmal living conditions of illegal immigrants employed as migrant farm workers in the United States. At the time, workers were found living in orchards and irrigation ditches. They lived in constant fear of apprehension, like fugitives, and were routinely exploited by their employers, who could maintain unsafe working conditions, cut wages, or abruptly dismiss them with little fear of reprisal. In many cases the life of these migrants was, according to the commission, 'virtually peonage'" (Schlosser, 1995).

affected undocumented immigrants. In exchange for legalizing approximately three million undocumented immigrants, the law sought to curtail the future flow of undocumented immigration by imposing sanctions on employers who knowingly hired undocumented workers and by increasing border enforcement (Linder, 2011). Of the approximately 3.2 million undocumented immigrants in 1986, 3.04 million applied for the program (Woodrow and Passel, 1990).

Between 1987 and 1990, over 2.8 million were given legal status (Baker, 2015). The applications were predominantly in coastal states, larger counties, and metropolitan areas. I aggregate the county-level population by MSAs and State and divide it by the foreign-born population in 1990. Applications vary by states (Figure 1.1a) and by MSAs (Figure 1.1b). California, Texas, and Arizona have the highest per capita applications. MSAs with applications higher than the median (dark colored in Figure 1.1b) are also found across the Midwest and in the Northeast.[11]

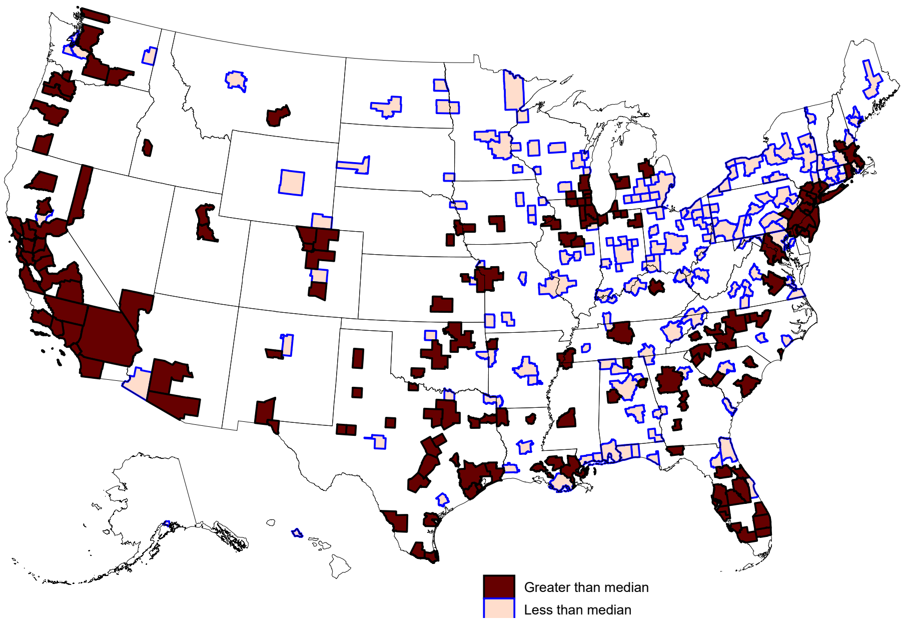
Figure 1.1a: IRCA Applications per 1000 by State



Notes: Author's calculation using 1990 Legalization Summary Tapes data.

[11] Four states, North Dakota, South Dakota, Wyoming and Vermont, had no counties with more than 25 applications; hence, the MSAs in these states have no data.

Figure 1.1b: IRCA Applications by MSA



Notes: Author's calculation using 1990 Legalization Summary Tapes data. Dark MSAs have greater than median IRCA applications and light MSAs have less than median.

All in all, the legalized immigrants represented approximately 1.1 percent of the then U.S population. This kind of large scale of legalization can produce a behavior shift in societies: in the labor market, family life, health, and the interaction between government and community (Baker, 2015). Given the massive changes in immigrants' legal status, there are many comprehensive studies on the impact of this law. For example, Linder (2011) examined the impact of the law on the future flow of undocumented immigration, while Baker (2015) studied its impact on crime. Cascio and Lewis (2016) analyzed the impact of IRCA on social safety nets, like the earned income tax credit (EITC) and food stamps, and Cobb-Clark et al. (1995) examined the law's impact on wages.

Furthermore, along with the IRCA, the federal government passed the Federal Omnibus Budget Reconciliation Act, which provided some social and welfare protection to these applicants.[12] Although one aim of the IRCA law was to curtail the flow of undocumented

[12] I discuss the issue in detail in section 1.2.3.

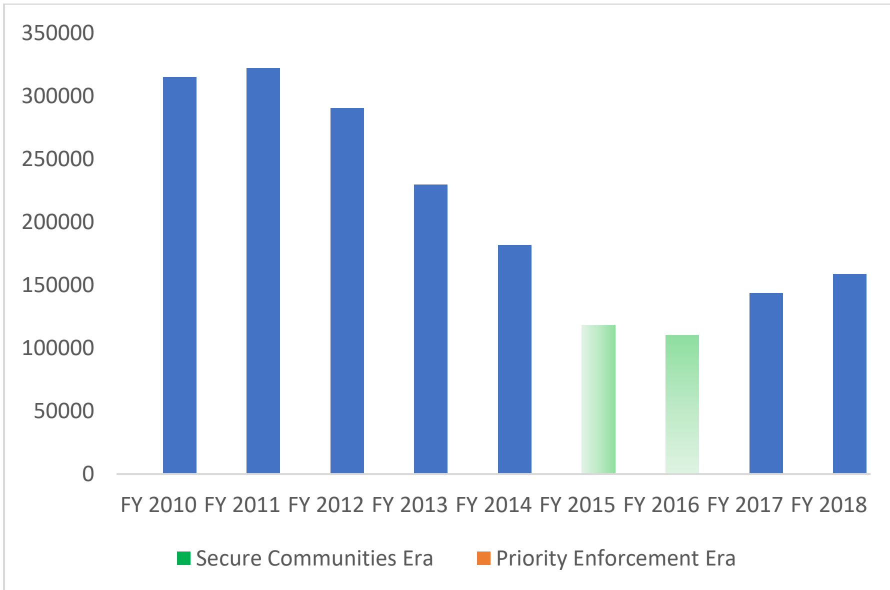
immigrants into the United States, by 2000, the number of immigrants had increased to 9 million and, by 2016, approximately 11 million (Fazel-Zarandi et al., 2018).

1.2.2 Immigration Rhetoric and the 2016 Election

The landscape of rhetoric hostile to undocumented immigrants has changed significantly in the last few decades. Following the passage of the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 and the subsequent creation of Immigration and Customs Enforcement (ICE), a new division within the Department of Homeland Security, the number of deportations of illegal immigration accelerated (Hacker et al., 2015). The launch of the Secure Communities program in 2008, which required local police to match fingerprints of all arrested individuals with the ICE database, amplified the ICE detention and arrests. Figure 1.2a shows ICE apprehensions by year. They peaked in 2011 and gradually decreased afterward, significantly so in 2015 and 2016. This is largely due to the memorandum issued by the Department of Homeland Security (DHS), which discontinued the Secure Communities program and started the Priority Enforcement Program (PEP) on November 20, 2014. The PEP prioritizes arrests of convicted criminals and other undocumented immigrants who pose a danger to public safety. However, Executive Order 13,768 issued on January 25, 2017, reversed the program back to Secure Communities Era (ICE, 2017).

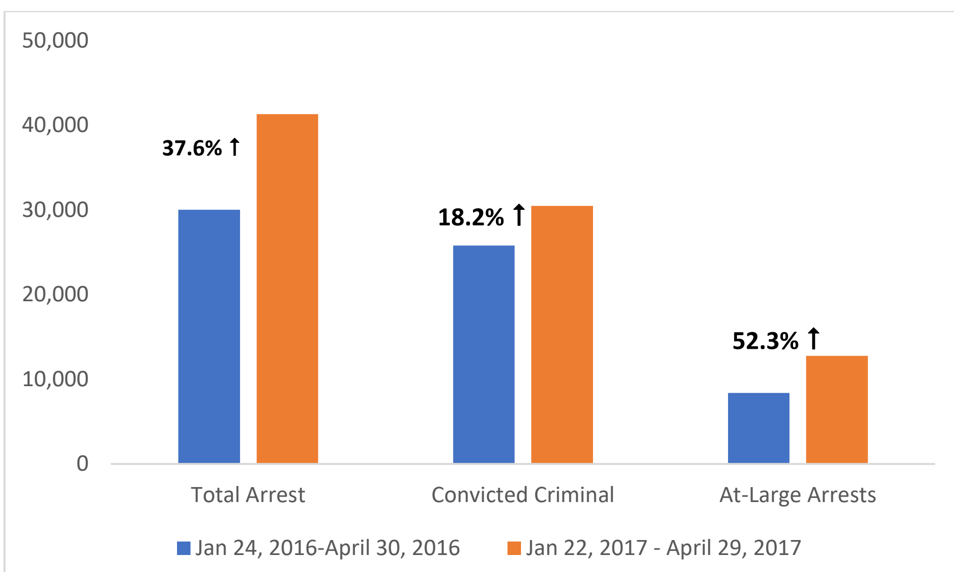
This decision increased the number of arrests by ICE dramatically. Figure 1.2b compares the ICE arrests for the first 100 days of the current administration to the same period of previous administration. There is an almost 40 percent increase in ICE arrests. Overall, between the fiscal year 2016 and 2017, total requests to detain immigrants increased by 81 percent, and complete removals by ICE increased by 37 percent (ICE, 2017).

Figure 1.2a: ICE Administrative (Interior) Arrests, FY 2010 - FY 2018



Source: U.S. Immigration and Customs Enforcement (ICE)

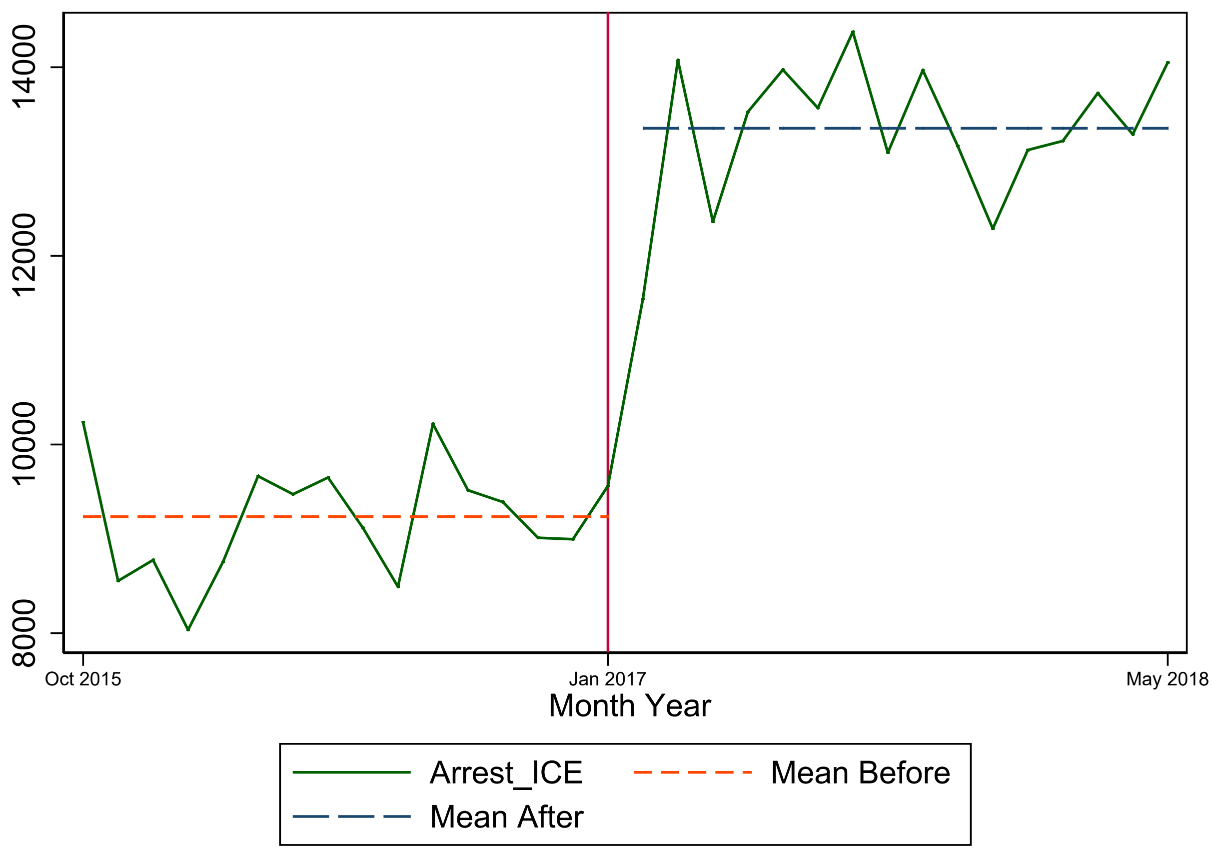
Figure 1.2b: First 100 days comparison between two administration



Source: U.S. Immigration and Customs Enforcement (ICE)

Transactional Records Access Clearinghouse at Syracuse University, records data about immigration and has detailed arrest information on all individuals from 2014 to May 2018 by ICE (TRAC, 2018). Using the data, I calculate total monthly arrests from October 2015 to May 2018 and plot it in Figure 2.3. The small dotted line shows the average arrests from October 2015 to January 2017, and the more prominent dotted line shows the average arrests from February 2017 to May 2018. The number of average monthly arrests jumped by 45 percent, closely aligning with the administrative data from ICE.

Figure 1.3: Monthly ICE Arrest

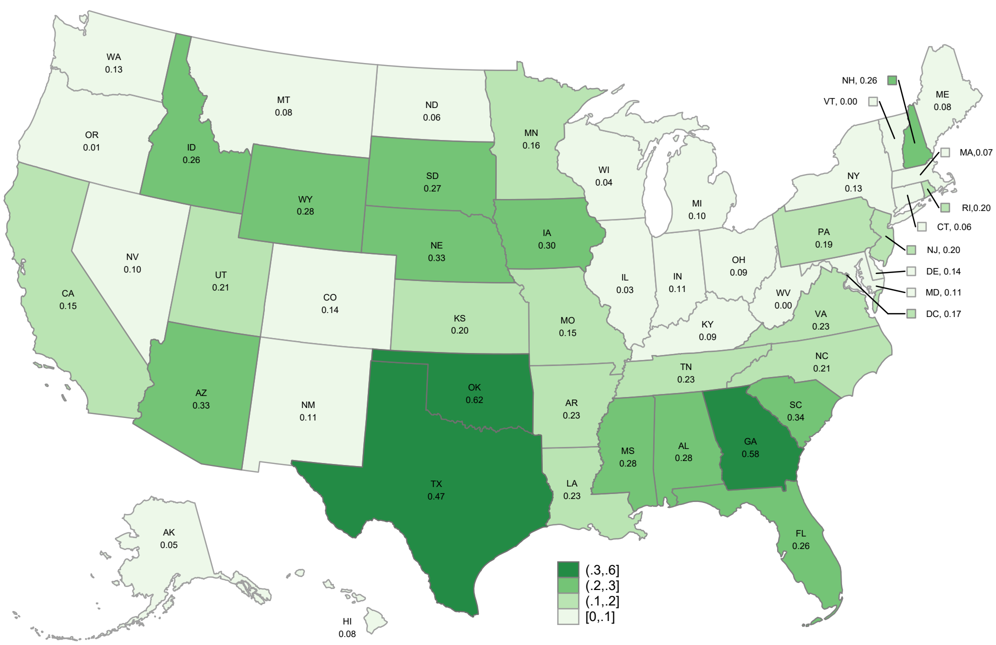


Notes: Author's calculation using TRAC data

For each state and MSA, I calculate the difference in a total change in ICE arrests nine months before (October 2015 to January 2017) and after (February 2017 to May 2018)

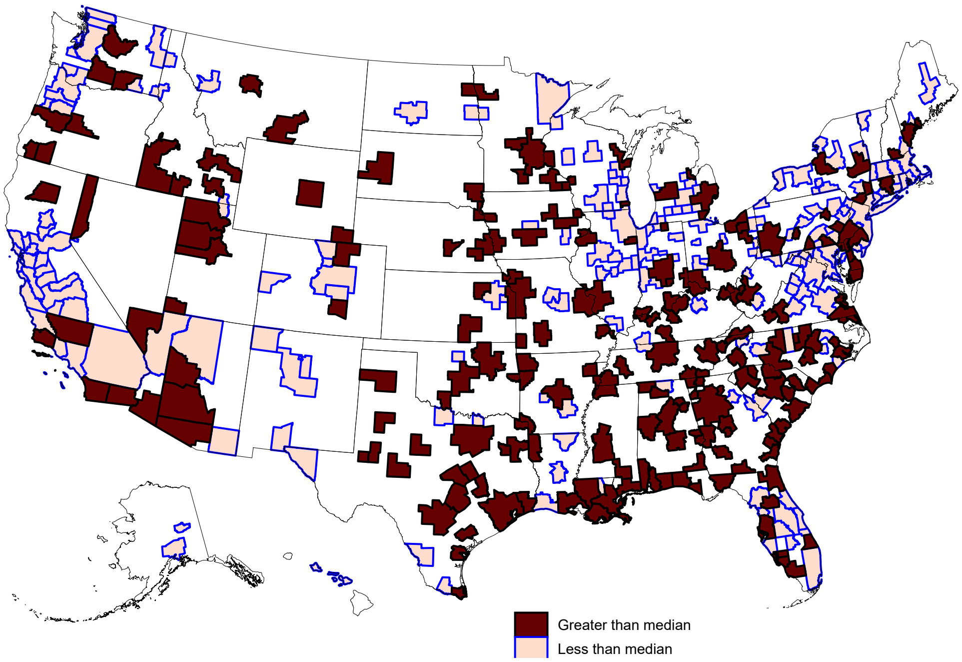
January 2017. I divide the change by the number of the foreign-born population in 2017. Figure 1.4a below presents the change in ICE arrests by states per 1000 population. The increase in arrests varies across states and is more prevalent in Texas, Georgia, and Arizona. Figure 1.4b provides data by MSAs. The dark-bordered MSAs had arrests higher than the median.

Figure 1.4a: Change in ICE Arrests per 1000 by State



Notes: Author's calculation using TRAC data.

Figure 1.4b: Change in ICE Arrests by MSA



Notes: Author's calculation using TRAC data. Dark MSAs have greater than median ICE arrests and light MSAs have less than median.

The 2016 presidential election, especially during the Republican party's primary race, was heavily focused on the issue of immigration (Edwards and Rushin, 2018). Coupled with the increasing ICE arrests following the 2016 election, evidence suggests that hate crimes against immigrants, the non-white population in the U.S., and in general are on the rise (Edwards and Rushin, 2018). In one recently reported incident, ICE agents arrested a patient in surgery and a mother who came to the hospital to pick up her newborn son. In addition, many workers who were injured on the job were arrested while on their way to the hospital (Saadi et al., 2017). This led the American Academy of Pediatrics to issue a statement highlighting an unusual ripple effect of immigration crackdowns: *"They harm children's health, potentially for life"* (Stein, 2017).

A small but growing number of empirical studies have documented the impact of increasing ICE arrests and hateful rhetoric on the immigration population (Novak et al., 2017;

Lopez et al., 2016). These studies have found that a raid in a small community in Iowa in 2016 resulted in both American-born and immigrant Latina mothers in Iowa being 24 percent more likely to have low birthweight babies (Novak et al., 2017). Similarly, the raid was associated with increase in stress and lower self-rated health scores (Lopez et al., 2016). Likewise, Krieger et al. (2018) found an increase in severe stressors among pregnant women in NYC after the 2016 election, which might have contributed to the increase in preterm births. The results were more pronounced among Hispanic women. A considerable literature has found a negative impact on immigrant communities' mental health following detention by immigration authorities (Werthern et al., 2018). Finally, the literature suggests a negative relationship between restrictive immigration laws and the birth outcomes of immigrant women (Torche and Sirois, 2018; Villalonga-Olives et al., 2016).

1.2.3 Health Insurance and Medicaid

While these two institutional shocks could potentially impact infant health, especially among immigrant mothers, it is necessary to discuss the uses of healthcare and various healthcare laws that impact the wellbeing of newborns directly. In this section, I discuss the U.S. healthcare system and laws that impact health outcomes.

The U.S. healthcare system is expensive, complex, and fragmented. It is a mixture of private sector, government-sponsored, and government-owned. While the majority of the population receives employment-based private insurance, and the elderly receive Medicare, the poor depend on government-financed Medicaid. Launched in 1965, Medicaid has significant impact. As of 2017, it covered approximately 75 million Americans, almost 19 percent of the total population.[13] Medicaid is jointly administered by the federal and state governments. The Medicaid program underwent two major reforms in 1986 and 2010. In 1986, along with the passage of IRCA, Medicaid was expanded significantly as part of an omnibus

[13]https://www.nber.org/reporter/2017number1/duggan.html

bill, especially in regard to pregnant mothers.

1986 Medicaid Expansion

During the early 1980s, the U.S. lagged behind other developed nations in child health indicators, especially infant mortality rates (Howell, 2001). In 1980, the United States was ranked 19th among developed countries in its rate of infant deaths. While other developed nations had programs that helped pregnant mothers and young infants, ranging from universal health coverage to paid maternity leave, many poor pregnant women in the U.S. were left uninsured (Howell, 2001). The Omnibus Budget Reconciliation Act (OBRA) of 1981 had severely reduced access to healthcare, especially for the poor. Studies have estimated that as many as 500,000 families lost healthcare due to OBRA (Yelowitz, 1995). A study conducted by the Institute of Medicine investigated the relationship between access to prenatal care and infant health. This study found every dollar spent on prenatal care reduced health expenditure during childbirth arising from low birth weight by \$3.38 (IOM, 1985). Based on the IOM study and other public and political pressure, a bipartisan coalition in Congress passed various Medicaid related bills, starting in 1986, which significantly expanded Medicaid: a major expansion occurred between April 1987 and July 1989 (Hill, 1990).

The Consolidated Omnibus Budget Reconciliation Act (COBRA) passed in 1985 started the expansion of Medicaid in subsequent years.[14] Given the mandate from the federal government, states rapidly expanded eligibility, but few states went beyond the Federal mandate. In the first two years of the law, half the states expanded eligibility up to 100 percent of the Federal Poverty Level (FPL) (Hill, 1990). By 1991, the average income threshold for

[14] Hill (1990) summarizes the legislation as follows: COBRA-1986 gave states the option to extend Medicaid income eligibility to pregnant women and children 0 to 5 up to 100 percent of Federal Poverty Level (FPL) and allowed states to simplify enrollment processes; COBRA-1987 gave states the option to extend Medicaid income eligibility to pregnant women and infants to 185 percent FPL; COBRA-1989 mandated coverage for pregnant women up to 133 percent FPL; COBRA-1990 mandated continuous eligibility for pregnant women through 60 days postpartum and for newborns living in their mothers' households up to age one.

Medicaid eligibility for pregnant women rose from just 48 percent to 159 percent of FPL (NGA, 1992).

Consequently, the law has helped poor pregnant mothers as well. Between 1979 and 1992, the percentage of pregnant women receiving public insurance rose from 12.4 percent to 43.3 percent, an increase of 250 percent (Currie and Gruber, 1996). The majority of these women had low socioeconomic status (Dubay et al., 2001). However, some studies have questioned this huge jump in coverage (Dave et al., 2008). While some research has found a positive impact of Medicaid expansion on health outcomes (Currie and Gruber, 1996), others have yielded negative results (Epstein and Newhouse, 1998). Howell (2001) reviewed all the literature on the impact of Medicaid expansion on pregnant women and summarized, *"these studies show evidence that new groups of pregnant women received health insurance coverage through Medicaid, and that some women received improved prenatal care services. The evidence is much weaker that the expansions led to improved birth outcomes."* Thus, although some of the results are less conclusive, the expansion did help many pregnant and poor women and families.

2010 Medicaid Expansion

Although the U.S spends about 20 percent of its GDP on healthcare, there are disparities in access and care (Gruber, 2011). The healthcare system is still fragmented, and, as of 2010, approximately 50 million people remained uninsured (Gruber, 2011). The U.S is the only industrialized country without universal coverage. To improve on coverage among other things, on March 23, 2010, President Barack Obama signed the Patient Protection and Affordable Care Act (ACA). This act has many insurance-related components; one of the most important is its expansion of the Medicaid program. Although the law mandated all states to expand Medicaid to everyone below 138 percent of Federal Poverty Level (FPL), a 2012 Supreme Court decision allowed states to opt-out of the program (Simon et al., 2017).

As a result, only 37 states and the District of Columbia had expanded Medicaid by the end of 2015: two in 2011, four in 2012, twenty-two in 2014, three in 2015, and six more recently (Simon et al., 2017).[15] In essence, the Medicaid expansion made public health insurance available to low-income, non-elderly, non-disabled childless adults (Simon et al., 2017). As of 2019, some 12 million Americans gained Medicaid coverage through the ACA Medicaid expansions (Soni, 2020).

The early impact of the law has generally been positive. First, the uninsured rate declined sharply following the law, decreasing to 9.1 percent in 2015 from 16 percent in 2010, a change of 43 percent (Obama, 2016). This growth was mostly due to the expansion of Medicaid eligibility. States that expanded Medicaid contributed more to the improvement in uninsured rates. Moreover, the law's passage had many other positive health benefits, such as access to treatment and overall lower healthcare costs (Fry and Sommers, 2018). Despite this success, the American healthcare system is still flawed in important ways. Critically to the scope of this study, undocumented/unauthorized immigrants' families and children cannot purchase insurance through ACA, nor can they access public welfare programs like Medicaid (Perreira and Pedroza, 2019). Below I discuss this issue further.

1.2.4 Immigrants and Access to Health Care

In 2014, approximately one in five births in the U.S. was to a foreign-born (either documented or undocumented) mother (Camarota et al., 2018). Out of that twenty percent, around seven percent of the births were to undocumented parents (Passel and Cohn, 2018).[16] In the United States, undocumented immigrants are normally barred from receiving any government benefits (Camarota et al., 2018; Perreira and Pedroza, 2019). Given this, understanding the health impact on these infants should be of significant public policy interest.

[15] States are continuingly expanding the Medicaid.
[16] That is approximately 300,000 births per year.

Before the Emergency Medical Treatment and Active Labor Act (EMTALA) in 1986, uninsured patients were denied any form of medical treatment or were transferred to public hospitals, a process called dumping (Zibulewsky, 2001). After the law, all patients had to be admitted to hospitals for emergencies and are treated regardless of immigrant or insurance status. As such, all pregnant mothers are provided for during delivery, and the child has guaranteed access to medical assistance for 60 days after birth (Camarota et al., 2018). Although helped during the delivery, these women cannot seek other necessary treatments, like prenatal care and other regular checkups. The IRCA law, which also expanded Medicaid, restricted the newly legalized immigrants from certain benefits for five years (OIG, 1998). Similarly, the ACA law passed in 2010 prevents undocumented immigrants from using its services (Fernández and Rodriguez, 2017).

As a result of these limitations, undocumented immigrants often seek healthcare only in life-threatening conditions. Overall, they use less healthcare than native or legal immigrants (Sabin, 2009; Perreira and Pedroza, 2019). In addition to their legal inability to access private and public health insurance, factors such as language barriers, fear of deportation, failure to take time off work, mistrust of the healthcare system, and lack of access to transportation bar them from seeking proper medical treatment. As a result, many undocumented immigrants live with unmet healthcare needs (Derose KP, 2007; Gusmano, 2012), faring worse on self-reported health outcomes than their native and immigrant counterparts (Derose et al., 2009; Wolff et al., 2005).

Access to healthcare improves maternal and child health in many ways (Currie and Gruber, 1996). The primary source is prenatal care visits. The link between prenatal care and reproductive and infant health is well established (Cohen, 2009; Hill, 1990; Korinek and Smith, 2011). Mothers who lack prenatal care are twice as likely to have low birth weight babies and one and half times more likely to have babies born prematurely (Gold et al., 1987). Birth weight is one of the leading indicators of fetus health. Children born with low birth weights are at risk of mortality and post-neonatal mortality (OTA, 1987a). In 1996 the average cost of caring for a surviving low-birth weight baby was $9,712 compared to $678 for an infant weighing more than 2,500 grams (Currie and Gruber, 1996).

While some states and cities have expanded their inclusion to undocumented mothers during pregnancy, these mothers could also seek prenatal care and other routine services via Federally Qualified Health Centers, which offer health services for uninsured patients. These services are available for free or are scaled according to the patient's ability to pay (Fabi, 2019). Although the children born to undocumented parents are automatically American citizens and thus qualify for federal health benefits, parents are often hesitant to enroll, fearing deportation (Perreira and Pedroza, 2019).[17]

In politics and other public discourse, immigrants are often blamed for increasing medical costs in the United States (Bell et al., 2010). However, Goldman (2006) have found that the foreign-born, especially undocumented immigrants, use few medical services and contribute significantly less than U.S. born natives to the nation's medical costs. Since public welfare programs, like Medicaid, are not readily available to them (Camarota et al., 2018; Perreira and Pedroza, 2019), the health status of pregnant mothers and the unborn child might be at risk. These problems are further magnified by anti-immigrant rhetoric, hate crimes, negative media coverage, language barriers faced by the mothers, and the lack of knowledge of often complex healthcare services in the United States (Martinez et al., 2015; Suárez-Orozco and Hirokazu, 2013).

Whether because of lack of access to health care, fear of deportation, or other barriers, both documented and undocumented immigrants face challenges when seeking proper health care in the U.S. Given the short-term (direct medical bills) and long-term (cognitive development) costs to infants born in poor health, this subject needs to be carefully examined.

[17] For anecdotal evidence covered in press see: https://www.theguardian.com/us-news/2018/dec/21/us-immigrant-undocumented-families-benefit-programs-chip-snap-deportation-fears

Focusing on two major immigration institutional shocks, this study estimates the effect on birth outcomes such as low birth weights and prenatal care behaviors of pregnant mothers.

1.3 Data and Variables

1.3.1 Data

To analyze the impact of immigration laws on birth outcomes, this paper uses data from various sources. First, to calculate the birth outcomes, I use Vital Statistics Natality Files for- the years 1983 to 1992, and 2012 to 2018. I obtained the restricted dataset from the Center for Disease Control and Prevention (CDC), which identifies the mothers' county of residence. The files contain information regarding almost all births taking place in the United States within the relevant time frames.

I use the MSA definition defined in the 1992 Natality files.[18] All mothers' MSA and County Federal Information Processing Standards (FIPS) of residence are identified. There is a total of 320 MSAs in 1992 Natality files. Using this definition, I construct 320 unique MSAs for the years 1983 to 1992. These MSAs represent 80 percent of total births in the U.S. Similarly, for the second part of the analysis, I use the 5-digit MSA and Division code defined by the Office of Management and Budget in September 2018. At that time, there was a total of 384 MSAs. I construct 384 unique MSAs from the Natality Files for the years 2013 to 2018.[19] These MSAs represent approximately 87 percent of total births in the U.S.

Second, to calculate the concentration of immigrants who applied for legalization after the IRCA, I use the data from the 1990 Legalization Summary Tapes created by the Immigration and Naturalization Service (now U.S. Citizenship and Immigration Service), which includes

[18] According to National Center for Health Statistics (1994), "MSA: Metropolitan statistical areas are those established by the U.S. Office of Management and Budget (OMB) using 1990 Census population counts. For New England, the New England County Metropolitan Areas (NECMA) are used."

[19] The MSAs FIPS recorded in Natality files keep changing boundaries over time. This is a common way of aggregating MSA level data when there are county identifiers (Dettling and Kearney, 2011).

the number of total applications and approved applications by counties.[20] Only counties that had applications over 25 are reported, and there are 382 counties in total. About 3.2 million people filed and application, and about 5 percent of the applications were rejected. Appendix Table 1.A1 shows the types of applicants who applied as well as those who were rejected. About 5 percent of the applicants were rejected. Similarly, Appendix Table 1.A2 shows the characteristics of the population that was legalized under IRCA. About 70 percent of the applicants were from Mexico. Appendix Table 1.A3 shows the timing of the application. Approximately 38 percent of the applicants applied in 1987, and 62 percent applied in 1988.

To calculate the change in ICE arrests following the election, I use the data from TRAC at Syracuse University, which collects data on individual arrests by ICE and the border patrol agency and has detailed ICE arrests by county from 2014 to 2018. The data includes only the *"interior"* arrests by ICE.[21] The majority of deportation and apprehensions by ICE include the capture by U.S. Customs and Border Protection (CBP) at the borders. For example, 30 percent of removals by ICE in 2015 were from interior arrests, and other removals are apprehended at or near the border or ports of entry (ICE, 2015). Because of this, I exclude the 23 border counties along the Mexico-US border in my main analysis. Due to this, 10 MSAs along the border are dropped, which represents around 3 percent of total MSA births.

I get county-level foreign-born population from the Census Bureau for 1990 and American Community Survey 2017. Furthermore, to control for MSA specific characteristics, I use data from the Bureau of Economic Analysis. I get the data by county and collapse them by MSA. Specifically, I use percentage change in total employment, per capita total transfers by the

[20] The data are available in Scott Baker's website.

[21] The specific data I use, according to TRAC, is "what ICE refers to as "interior arrests." According to ICE, "these data do not include arrests made by Customs and Border Protection (CBP) when the individual is transferred to ICE for detention and eventual removal." See more at: $https://trac.syr.edu/phptools/immigration/arrest/about_data.html$

government,[22] and income per capita per MSA.

1.3.2 Variables

First, using the Natality files, I create birth outcome variables. I keep only singleton births. I drop observations that had missing birth weights.[23] Next, I compute a binary variable equal to 1 if birth weight is less than 2500 grams. Similarly, to analyze the prenatal characteristics, I create a binary variable for whether mothers had any prenatal care and a variable for the total number of prenatal care visits. I multiply the fraction variables by 100.

I aggregate the above data by year, MSAs, and demographics groups. Following Hoynes et al. (2015a), I define the demographic groups as follows: mother's age (<25, 25-34 and >34), education (<12, 12, >12 and missing), parity of birth (1st, 2nd, and 3rd or greater birth to a mother), race (white, black and others), marital status and the sex of the child. The groups are similar for IRCA and ICE arrests except for race/ethnicity, where complete ethnicity in Natality files is not available during IRCA years. For ICE, I create 4 mutually exclusive race/ethnicity variables, non-Hispanic white, non-Hispanic black, Hispanic, and others.

1.3.3 Treatment Groups

Using mothers' countries of birth first, I define treatment and control groups of mothers. Given that the distinction is available for the U.S., Mexico, Canada, and the rest of the world for IRCA years, I make three groups, U.S.-born, Outside of the U.S (foreign-born)

[22]This is defined as the sum of: "Personal current transfer receipts- Receipts of persons from government and business for which no current services are performed. Current transfer receipts from government include Social Security benefits, medical benefits, veterans' benefits, and unemployment insurance benefits. Current transfer receipts from business include liability payments for personal injury and corporate gifts to non-profit institutions." ($https://apps.bea.gov/iTable/iTable.cfm?reqid = 70\&step = 1\&isuri = 1\&acrdn = 7\#reqid = 70\&step = 1\&isuri = 1\&acrdn = 7$)

[23]This is a common way of calculating births weights in the literature (Hoynes et al., 2015a).

and Mexico born. Thus, the main treatment group is coded 1 if a mother is born outside of the U.S. and 0 if born in the U.S. Further, I only look at mothers born in Mexico vs. born in the U.S. For ICE years, mothers' birth country is identified for all countries and as such I create four groups of mothers: the U.S.-born, rest of the world (foreign-born), born in Latin American countries and Central American countries.[24] Thus, the main treatment group is coded 1 for mothers born outside of the U.S. and 0 if born in the U.S. Furthermore, I compare mothers born in Latin American counties vs. the U.S. and mothers born in Central American countries vs. the U.S. Table 1.1a, below shows the percentages of mothers born in respective countries. Column 1 shows the mothers' birthplace characteristics for IRCA application years. On average, 16 percent of mothers were born outside of the U.S. of whom 6 percent were born in Mexico. Column 2 shows the mothers' birthplace characteristics for ICE arrests. On average, 24 percent of mothers were born outside of the U.S. Of these 12 percent were born in Latin American countries, and 9 percent were born in Central American countries.

Table 1.1a: Percentages of Births by Mothers' Country of Birth

	IRCA Applications	ICE Arrests
U.S. %	84.49	75.53
Foreign %	15.51	24.47
Mexico %	6.13	
Latin American Countries %		12.45
Central American Countries %		9.34

Notes: The sample period is 1983-1992 for IRCA applications and 2013-2018 for ICE arrests. Percentages of births from mothers born in the U.S., Outside of the U.S (Foreign), Mexico, Latin American Countries and Central American Countries are reported. The list of countries for Latin and Central American countries are in Appendix Table A4.

Next, I define the treatment and control periods as follows. I define the pre-intervention years before IRCA from 1983 to 1987. Although IRCA was passed in October 1986, the law

[24] I show the list of countries for each group in the Appendix Table 1.A4.

took time to take effect.[25] Since I examine yearly data, I make 1988 births the starting point as when the law had an effect on pregnant mothers.[26] Thus, I code the post-intervention years from 1988 to 1992. Similarly, for ICE arrests, I define the pre-intervention period as births in 2012 to 2016 and post-intervention period for the years 2017 and 2018. Table 1.1b provides basic summary statistics for all MSAs by mother's country of birth before and after the intervention for IRCA applications.

[25]There are three types of delay, which may overlap. One is the administrative implementation of the law. The second is a response lag: people need to realize their opportunity to file an application and need time to compete their application. In fact, 38 percent applied for the program in 1987 and the other 62 percent in 1988 (see Table 1.A3 in the appendix). The third is the length of time (up to nine months) of pregnancy.

[26]Making 1987 the start does not change the results.

Table 1.1b: Summary Statistics by Mother's Country of Birth (IRCA)

	Before			After		
Main Variables	U.S.	Foreign	Mexico	U.S.	Foreign	Mexico
Birth Weight (grams)	3366	3359	3413	3364	3355	3392
LBW <2500g %	6.05	4.94	4.22	6.19	4.94	4.27
No Prenatal Care %	1.68	3.41	5.46	1.81	3.00	5.00
No. of PNC Visit	9.88	6.39	3.32	10.98	9.19	7.90
Other Variables (Share)						
Age <25	0.42	0.34	0.46	0.38	0.34	0.47
Age >24 & <36	0.51	0.55	0.46	0.53	0.55	0.45
Age >35	0.06	0.11	0.08	0.09	0.12	0.08
Education 1 (Less than HS)	0.15	0.13	0.12	0.17	0.37	0.62
Education 2 (HS)	0.32	0.17	0.03	0.35	0.24	0.15
Education 3 (more than HS)	0.30	0.19	0.01	0.38	0.27	0.07
Education 4 (missing)	0.23	0.51	0.84	0.10	0.13	0.15
Parity 1	0.43	0.39	0.32	0.42	0.39	0.36
Parity 2	0.33	0.31	0.28	0.33	0.30	0.27
Parity 3 or more	0.23	0.30	0.40	0.25	0.30	0.37
Race 1 (White)	0.81	0.68	0.99	0.79	0.71	1.00
Race 2 (Black)	0.18	0.10	0.00	0.05	0.18	0.00
Race 3 (Others)	0.01	0.22	0.01	0.16	0.11	0.00
Married	0.76	0.81	0.75	0.71	0.74	0.66
Female Child	0.49	0.49	0.49	0.49	0.49	0.49

Notes: Sample period for before is 1983-1987 and after is 1988-1982.

The average birth weight of children born to mothers born in the U.S. and the rest of the world is similar before the IRCA; however, it is higher for mothers born in Mexico. Furthermore, the incidence of LBW among U.S born mothers is much lower compared to mothers born outside of the U.S and Mexico. Although surprising, this is consistent with the literature of "healthy immigrant effect" or "Hispanic health paradox" (Giuntella and Mazzonna, 2015; Giuntella, 2017). The epidemiological paradox reflects that despite a vast difference in socioeconomic profile, the incidence of LBW is lower or similar for mothers of

Hispanic origin. The incidence of LBW remains constant among mothers born outside of the U.S and Mexico; it seems to have increased slightly among U.S born mothers. Similarly, the percentage of mothers with no prenatal care decreased among mothers born outside of the U.S and Mexico. Other points to note are that mothers born in the U.S. have a better education than that of mothers born outside, and that foreign-born mothers are older than native mothers.

Table 1.1c below shows the summary statistics by different groups of mothers for ICE arrests. Similar to IRCA applications, the incidence of LBW is higher among the U.S born mothers. Similarly, the percentage of mothers with no prenatal care decreased among mothers born outside of the U.S and Mexico. Other point to note is that mothers born in the U.S. have a better education than that of mothers born outside. Similarly, foreign-born mothers are older than native mothers.

Table 1.1c: Summary Statistics by Mother's Country of Birth (ICE)

	Before				After			
Main Variables	U.S.	Foreign	Latin America	Central America	U.S	Foreign	Latin America	Central America
Birth Weight (grams)	3308	3294	3313	3323	3299	3281	3295	3304
LBW <2500g %	6.48	5.71	5.61	5.32	6.76	6.02	6.01	5.75
No Prenatal Care %	1.48	1.61	1.94	2.22	1.71	1.85	2.27	2.60
No. of PNC Visit	11.36	10.91	10.80	10.73	11.43	10.91	10.74	10.69
Other Variables (Share)								
Age <25	0.30	0.17	0.23	0.25	0.26	0.15	0.21	0.23
Age >24 & <36	0.56	0.59	0.55	0.55	0.58	0.59	0.55	0.54
Age >35	0.14	0.24	0.22	0.20	0.16	0.26	0.25	0.23
Education 1 (Less than HS)	0.11	0.27	0.43	0.52	0.09	0.23	0.37	0.47
Education 2 (HS)	0.24	0.23	0.29	0.29	0.25	0.23	0.30	0.31
Education 3 (more than HS)	0.64	0.48	0.27	0.17	0.65	0.52	0.31	0.20
Education 4 (missing)	0.01	0.02	0.02	0.02	0.01	0.02	0.02	0.02
Parity 1	0.41	0.35	0.28	0.24	0.40	0.35	0.29	0.24
Parity 2	0.32	0.32	0.30	0.28	0.32	0.32	0.30	0.28
Parity 3 or more	0.27	0.33	0.42	0.47	0.28	0.32	0.41	0.47
Race 1 (Non-Hispanic White)	0.63	0.16	0.02	0.01	0.61	0.16	0.02	0.01
Race 2 (Non-Hispanic Black)	0.18	0.11	0.07	0.00	0.18	0.12	0.08	0.00
Race 3 (Hispanic)	0.16	0.48	0.90	0.98	0.17	0.46	0.89	0.98
Race (Others)	0.04	0.25	0.01	0.00	0.04	0.26	0.01	0.01
Female Child	1.51	1.51	1.51	1.51	1.51	1.51	1.51	1.51
Married	0.58	0.67	0.51	0.49	0.58	0.69	0.53	0.49

Notes: Sample period for before is 2013-2016 and after is 2017-2018.

Next, I use the variation in IRCA applications and ICE arrests to create treated and control MSAs. Using the 1990 Legalization Summary Tapes, I calculate the total IRCA applications by MSA and divide it by the MSA's foreign-born population in 1990. Then, using the data from TRAC, I calculate total monthly ICE arrests by MSA form October 2015 to January 2017 and from February 2017 to May 2018. I subtract the latter from the former and divide it by the MSA's foreign-born population in 2017. Henceforth for simplicity, I will refer to the IRCA per foreign-born population applicants as IRCA applications and change in ICE arrests per foreign-born population as ICE arrests. Table 1.2a, panel A below, shows the distribution of IRCA applications and ICE arrests. The data is right skewed in both cases.

Table 1.2a: Distribution of Treatment MSAs

Panel A: Summary Statistics	IRCA Applications per 1000 foreign population	ICE Arrests per 1000 foreign population
Mean	79.88	3.27
Median	25.07	1.19
Standard Deviation	122.27	7.26
Min	0.00	0.00
Max	661.13	77.13
Interquartile Range	94.85	3.43
Panel B: Number of Unique MSAs		
Total MSAs	320	374
MSAs with zero Applications/Arrests	76	84
MSAs with non-zero Applications/Arrests	244	290
Treat MSAs (1 if greater than Mean)		
Treated MSAs	91	99
Control MSAs	229	275
Treat MSAs (1 if greater than Median)		
Treated MSAs	160	187
Control MSAs	160	187

Notes: When treatment is defined by above mean, 28 and 27 percent of MSAs are in treatment group for IRCA and ICE respectively. One MSA for ICE Arrests per 1000 is outlier and is top coded. It is top coded to 77.13 from 271.94 arrests. 76 MSAs has no IRCA applications (only counties with greater than 25 applications are reported in the data) and 84 counties have no change in increase in ICE arrests.

I use the above distribution to proxy Metropolitan Statistical Areas that had relatively more undocumented immigrant mothers. Specifically I define two types of MSAs: (i) a variable *treat_msa_mean* as 1 if the IRCA applications/ICE arrests in an MSA are greater than the mean and as 0 if less than the mean and ii) a variable *treat_msa_median* as 1 if the IRCA applications/ICE arrests in an MSA are greater than the median and as 0 if less than the median. Mothers living in the "treated" MSAs should be impacted by the laws more severely compared to mothers living in the "control" MSAs. I call these treatment groups, 'mean' and 'median' treatment. Table 2a, panel B shows a unique number of MSAs by treatment and control group. The percentage of treated MSAs for mean treatment by above and below mean is 28 percent for IRCA applications and 27 percent for ICE arrests. There were 76 MSAs that had zero applications; this is because the IRCA data provides for counties that had 25 or more applications. Counties in these MSAs had no reported applications. Similarly, 84 MSAs have zero arrests as there were no reported arrests in TRAC data. Table 1.2b and 1.2c below show the difference in the estimation sample by treated and control MSAs. Table 1.2b below shows summary statistics for mothers living in MSAs, which has greater than mean and median applications for IRCA. The first thing to note is that the percentage of U.S born mothers is above 90 percent in control MSAs. Further the percentage of mothers born in Mexico is less than one percent in control MSAs compared to over 10 percent in treated MSAs. The birth outcomes in treated MSAs are better after the IRCA. The incidence of LBW decreased significantly. This is likely because these MSAs had more undocumented immigrants who lacked health insurance, among many other factors that affect birth outcomes.

Table 1.2b: Summary Statistics by Treatment MSAs (IRCA)

	Treat MSA Mean				Treat MSA Median			
	Before		After		Before		After	
Mother's Cntry of Birth	Treat	Control	Treat	Control	Treat	Control	Treat	Control
U.S. %	79.77	91.05	73.58	89.11	82.54	95.42	77.59	94.81
Foreign %	20.23	8.95	26.42	10.89	17.46	4.58	22.41	5.19
Mexico %	11.44	0.29	17.01	0.65	6.91	0.20	10.63	0.36
Main Variables								
Birth Weight (grams)	3366	3364	3365	3360	3359	3379	3358	3372
LBW <2500g %	5.71	6.02	5.63	6.22	5.97	5.74	5.98	5.96
No Prenatal Care %	2.15	1.76	2.4	1.74	2.31	1.04	2.31	1.32
No. of PNC Visit	7.07	10.88	10.05	11.1	8.61	11.15	10.4	11.32
Other Variables (Share)								
Age <25	0.42	0.41	0.39	0.36	0.41	0.43	0.37	0.39
Age >24 & <36	0.51	0.53	0.51	0.54	0.52	0.52	0.53	0.53
Age >35	0.07	0.07	0.09	0.09	0.07	0.06	0.1	0.08
Education 1 (Less than HS)	0.09	0.18	0.25	0.17	0.13	0.18	0.22	0.17
Education 2 (HS)	0.15	0.40	0.29	0.36	0.25	0.41	0.31	0.37
Education 3 (more than HS)	0.16	0.37	0.33	0.39	0.25	0.37	0.35	0.38
Education 4 (missing)	0.61	0.05	0.13	0.08	0.37	0.04	0.11	0.08
Parity 1	0.42	0.44	0.41	0.42	0.43	0.43	0.42	0.42
Parity 2	0.32	0.33	0.31	0.33	0.32	0.34	0.32	0.33
Parity 3 or more	0.26	0.23	0.28	0.25	0.25	0.23	0.27	0.25
Race 1 (White)	0.81	0.78	0.81	0.76	0.78	0.82	0.77	0.8
Race 2 (Black)	0.13	0.19	0.08	0.07	0.17	0.15	0.08	0.06
Race 3 (Others)	0.06	0.03	0.12	0.17	0.05	0.03	0.15	0.15
Female Child	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49
Married	0.77	0.77	0.72	0.71	0.76	0.79	0.71	0.73

Notes: Sample period for before is 1983-1987 and after is 1988-1982. Treat MSA Mean implies MSAs that had application greater than mean are coded 1and 0 otherwise and Treat MSA Median implies that MSAs that had application greater than the median is coded 1 and 0 otherwise.

Table 1.2c shows similar statistics for change in ICE arrests by treated and control MSAs before and after the 2016 election. The percentage of foreign-born mothers is slightly lower in treated MSAs than control MSAs. The change in birth weights is similar for both groups before and after. However, the percent of mothers with no PNC changed more in treated

MSAs post-2016, a difference of 0.11 percent.

Table 1.2c: Summary Statistics by Treatment MSAs (ICE)

	Treat MSA Mean				Treat MSA Median			
	Before		After		Before		After	
Mother's Cntry of Birth	Treat	Control	Treat	Control	Treat	Control	Treat	Control
U.S. %	85.37	73.15	84.64	73.04	81.63	69.67	81.08	69.69
Foreign %	14.63	26.85	15.36	26.96	18.37	30.33	18.92	30.31
Latin America %	8.34	15.70	8.68	15.66	10.81	17.67	10.84	17.74
Central America %	7.12	12.32	7.14	11.45	9.54	13.03	9.16	12.04
Main Variables								
Birth Weight (grams)	3288	3309	3278	3298	3297	3313	3286	3302
LBW <2500g %	6.91	6.14	7.20	6.42	6.63	5.97	6.90	6.25
No Prenatal Care %	1.93	1.41	2.23	1.62	2.08	0.95	2.36	1.13
No. of PNC Visit	11.21	11.26	11.34	11.29	11.10	11.41	11.19	11.41
Other Variables (Share)								
Age <25	0.31	0.26	0.27	0.22	0.30	0.24	0.26	0.20
Age >24 & <36	0.56	0.57	0.58	0.58	0.56	0.57	0.58	0.58
Age >35	0.13	0.17	0.15	0.20	0.14	0.19	0.16	0.22
Education 1 (Less than HS)	0.14	0.15	0.12	0.12	0.15	0.14	0.13	0.12
Education 2 (HS)	0.25	0.24	0.27	0.24	0.25	0.23	0.26	0.23
Education 3 (more than HS)	0.59	0.60	0.60	0.62	0.59	0.61	0.61	0.63
Education 4 (missing)	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.02
Parity 1	0.39	0.40	0.38	0.39	0.39	0.41	0.38	0.40
Parity 2	0.31	0.32	0.32	0.32	0.31	0.32	0.32	0.32
Parity 3 or more	0.30	0.28	0.30	0.29	0.30	0.27	0.31	0.28
Race 1 (Non-Hispanic White)	0.60	0.49	0.58	0.48	0.56	0.47	0.54	0.46
Race 2 (Non-Hispanic Black)	0.22	0.15	0.22	0.15	0.19	0.14	0.19	0.14
Race 3 (Hispanic)	0.13	0.26	0.14	0.27	0.19	0.28	0.20	0.28
Race 4 (Others)	0.05	0.10	0.05	0.10	0.06	0.12	0.06	0.13
Female Child	1.51	1.51	1.51	1.51	1.51	1.51	1.51	1.51
Married	0.58	0.61	0.58	0.61	0.59	0.61	0.59	0.62

Notes: Sample period for before is 2013-2016 and after is 2017-2018. Treat MSA Mean implies MSAs that had application greater than mean are coded 1and 0 otherwise and Treat MSA Median implies that MSAs that had application greater than the median is coded 1 and 0 otherwise.

1.4 Empirical Strategy

1.4.1 Difference in Difference (DD) Estimate

The main outcome of interest is mean birth weight and the incidence of low birth weight (LBW) of a newborn child. The first difference compares the birth outcomes of infants before and after the IRCA applications and ICE arrests. Given that I cannot identify the mother's immigration and nativity status in the Natality files, first I proxy immigrant mothers by using their country of birth. The U.S. Census Bureau estimated that in 2015, approximately 60 percent of the foreign-born population were not a U.S citizen.[27] Using the definition of post and pre years as defined above, I estimate the following standard difference-in-differences regression model.

$$y_{mgit} = \beta_0 + \beta_1 Post_t + \beta_2 Treat\_Mother_m + \beta_3 (Post_t * Treat\_Mother_m) + \pi X_{it} + \delta_i + \lambda_t + \gamma_g + \epsilon_{mgit} \tag{1}$$

where y_{mgit} is the outcome variable indicating birth outcomes for the cell defined as treated mother (mothers' nativity/country of birth) m, demographic group g, living in MSA i and in year t. X_{it} is a set of time varying MSA characteristics, which are percent change in total employment, per capita total transfers by the government and income per capita; δ_i is a set of MSA dummies, which controls for MSA fixed effects; λ_t is a vector of year dummies; γ_g is a vector of demographic group dummies and β_3 is the coefficient of interest, which measures the impact of two laws on birth outcomes. Finally, ϵ_{mgit} is the error term and standard errors are clustered at the MSA-level. I estimate the above equation for both laws.

[27] https://www.migrationpolicy.org/article/frequently-requested-statistics-immigrants-and-immigration-united-states

1.4.2 Parallel Trends and Placebo Tests

The identification of the model above relies on the assumption that in the absence of the IRCA applications and ICE arrests, the difference in birth outcomes would have been similar among treated and control group of mothers. As discussed above, the birth outcomes among U.S.-born mothers are very different from foreign-born mothers. The incidence of LBW among U.S.-born mother is much higher, due to the so-called "Hispanic paradox". Next the healthcare intake for native mothers is different from those of immigrant mothers. Similarly, as discussed above, the expansion of Medicaid, along with the IRCA act and Medicaid expansion following ACA, might differentially affect these different groups of mothers, especially since both laws explicitly barred the use of Medicaid for undocumented migrants. Further, over time, overall birth outcomes across the country might have been improving.

One way of testing whether the trends are similar before the law is to plot the trends before and after the intervention and visually inspect if the pre-trends are similar. Given that I have a long enough pre-treatment period, I can perform a formal test, a so-called placebo, or falsification test. Specifically, I limit the data to the pre-intervention period, assign a different pre and post period and estimate the equation (1) above. I code the post-period as 1985 and 1986 and pre-period as 1983 and 1984 for IRCA, and the post-period as 2014 and 2015 and pre-period as 2012 and 2013 for ICE arrests. If the coefficient β_3 is close to zero and/or statistically insignificant, we may be relatively sure that the trends in birth outcomes are parallel. However, if the coefficient β_3 is nonzero and statistically significant, then it fails the parallel trend assumption (which I show it fails for ICE arrests); in that case, interpreting the coefficient β_3 in equation (1) above as the causal impact of IRCA applications and ICE arrests could be a problem.

1.4.3 Difference in Difference in Difference (DDD) Estimate

As discussed above, the trends in birth outcomes among native and foreign-born mothers might be different. Not only that, but the estimate from DD might also be confounded by omitted variables such as immigrants choosing to live in the same places. To mitigate this issue, I exploit the variation in IRCA applications and ICE arrests in MSAs and estimate a triple difference (DDD) model. Essentially, I compare the double difference in equation (1) above with birth outcomes in treated and control MSAs. Comparing the double-difference above for mothers living in MSAs with lower and higher applications/arrests is credible for several reasons.

First, if the expansion of Medicaid in 1986 led to an improvement in birth outcomes for all groups of mothers across the country, then the DDD model will net out such effects. Second, given the difficulty in identifying a mother's migration status, it could be plausible to assume that MSAs that had lesser or no IRCA applications are likely to be the MSAs where fewer undocumented mothers were present and may not have been affected by the law. Similarly, immigrant mothers living in MSAs that saw an increase in ICE arrests are more likely to be at risk and might be more in fear of deportation than mothers living in MSAs that saw no change or lower increase in arrests. The sudden increase of ICE arrests (a change of 45 percent – see Figure 2.3 above) starting in 2017 might have put pregnant mothers at a heightened sense of fear relative to those living in communities that saw less or no increase in arrests. Thus, comparing the double-difference above with control and treatment MSAs will net out any omitted variables and unobservable characteristics that affect birth outcomes among native and foreign-born mothers.

The triple difference estimates of the impact of IRCA and ICE arrests are estimated by the following equation.

$$
\begin{aligned}
y_{mgzt} = {} & \beta_0 + \beta_1 Post_t + \beta_2 Treat\_Mother_m + \beta_3 Treat\_MSA\_Type_z + \beta_4 (Post_t * Treat\_Mother_m) \\
& + \beta_5 (Post_t * Treat\_MSA\_Type_z) + \beta_6 (Treat\_Mother_m * Treat\_MSA\_Type_z) \\
& + \beta_7 (Post_t * Treat\_Mother_m * Treat\_MSA\_Type_z) + \pi X_{zt} + \delta_z + \lambda_t + \gamma_g + \epsilon_{mgzt}
\end{aligned}
\tag{2}
$$

where y_{mgzt} is a measure of infant health for the cell defined by treated mother (mother's nativity/country of birth) m, demographic group g, in MSA treatment type z and year t.

The main coefficient of interest is β_7 (the DDD estimator), which measures the impact of the IRCA and ICE laws on foreign-born mothers, living in MSAs that had higher application/arrests in post-intervention years (1988-1992) and (2017 and 2018). Coefficients β_1 to β_6 are slopes on linear terms and double interactions. Standard levels are clustered at MSA levels. All variables are the same as defined in equation (1) except $Treat\_MSA\_Type_z$ which indicates observation in treatment (highly affected) or control (less affected) MSAs. $Treat\_MSA\_Type_z$ is classified in the following models:

Model 1: $Treat\_MSA_z\_Mean$ equal to 1 if applications/arrests[28] in MSA z exceed the mean and 0 otherwise

Model 2: $Treat\_MSA_z\_Median$ equal to 1 if applications/arrests in MSA z exceed the median and 0 otherwise

Dividing the variables, applications per MSAs and change in ICE arrests per MSAs, into mean and median might fail to capture the distributional impact of the laws. Particularly, there could be no difference between MSAs that are just above the median and MSAs that are just below; however, they will be classified in treated and control MSAs. Thus, as an alternative identification strategy, I modify the above setting and estimate a DDD model

[28] Arrests refers as change in ICE arrests as defined earlier.

with continuous treatment. Instead of two treated and control MSA groups, I now have a continuous treatment, which is applications/arrests per foreign population. Therefore, the third model is:

Model 3: $Treat\_MSA_z\_Cont$ as the actual number of applications/arrests for MSA i.[29]

As mentioned above, I perform placebo tests for the DDD analysis as well. I only take the data for the pre-intervention year and re-define the post and pre years as similar as above and run equation (2).

1.5 Results

First I begin with presenting results for IRCA applications. Section 1.5.1 presents the main results, section 1.5.1 tests parallel trend assumption and in section 1.5.1 I show event study analysis. Then I present the results for ICE arrests. Section 1.5.2 presents the main results of ICE and section 1.5.2 tests parallel trend assumption and finally section 1.5.2 shows event study analysis.

1.5.1 Main results IRCA

Table 1.3 presents the impact of IRCA on mothers' birth outcomes. Panel A presents the results of DD regression. Only the coefficient of the interaction term β_3 is reported in the table. Column 1 shows that babies born after the IRCA law to mothers who were born outside of the U.S. on average weighed 8 grams more. Similarly, the incidence of LBW (column 2) decreased by 0.276 percentage points (relative to the mean of 5.94 percent). Column 3-4 shows that the babies born after the IRCA law to mothers born in Mexico on average weighed 24 grams more, and the incidence of LBW decreased by 0.846 percentage points (relative to the mean of 6.01) compared to babies born to U.S. born mothers after

[29] For ICE arrests I divide the per-foreign-born population change in arrests by 100 in all regression analysis.

IRCA.

Panel B presents results for DDD estimations. Model 1 compares the double-difference results above between mothers living in MSAs that had IRCA applications of greater than mean. It shows that babies born to foreign-born mothers living in MSAs that had higher IRCA applications on average weighed 14 grams more, and the incidence of LBW decreased by 0.340 percentage points (relative to the mean of 5.94) after the IRCA. Model 2 shows similar results, but when comparing MSAs that had greater than median applications. The coefficients are smaller, but the signs are in the same direction. Model 3 shows that a 10 percentage points increase in IRCA applications corresponds to an increase of 6 grams in birth weights and decrease in the incidence of LBW by 0.168 percentage points for babies born to foreign-born mothers after IRCA. Similarly, when comparing U.S born mothers to mothers born in Mexico, the effect size increases. One thing to note is that the magnitude of the DDD estimates is larger than that of the DD estimates. This is expected, as foreign-born mothers living in MSAs that had more IRCA applications are more likely to be undocumented mothers and as such more affected by the law.

Table 1.3: Impact of IRCA Applications

Mother's Cntry of Birth	Foreign vs U.S.		Mexico vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression				
Treat_Mother*Post	7.548**	-0.276***	23.510***	-0.846***
	(3.259)	(0.077)	(3.902)	(0.097)
Panel B: DDD Regression				
Model 1:	14.067***	-0.340***	22.705***	-0.602**
Treat_Mother*Post*Treat_MSA_Mean	(3.295)	(0.084)	(6.018)	(0.245)
Model 2:	5.746	-0.189*	9.327	-0.717**
Treat_Mother*Post*Treat_MSA_Median	(3.877)	(0.100)	(8.060)	(0.290)
Model 3:	62.236***	-1.680***	72.909**	-2.100**
Treat_Mother*Post*Treat_MSA_Cont	(13.276)	(0.332)	(35.706)	(0.838)
N	886089	886089	651303	651303
Dep Var Mean	3363.34	5.94	3366.73	6.01

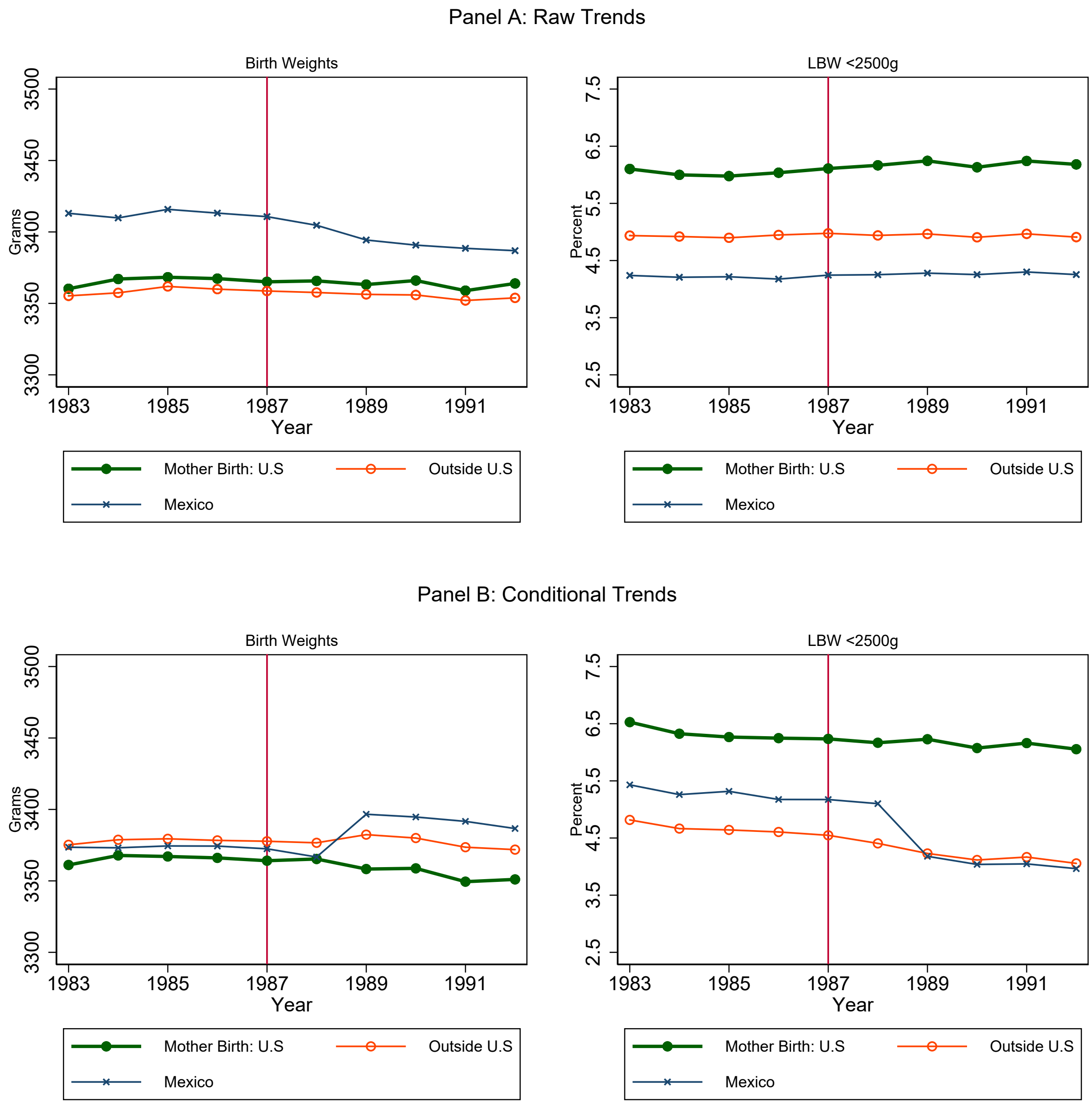
Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for the year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

However, as discussed above, the aforementioned results are formulated based on the assumption that in the absence of treatment (IRCA), the outcome would have been similar between treated and untreated mothers and MSAs across the pre and post-intervention periods. This is commonly known as "parallel trend assumption" in the difference-in-difference model. In the next subsection, I examine this assumption.

Parallel trends and Placebo Test

One way to check if the pre-trends in birth outcomes among different groups of mothers are similar, is to visually inspect the trends. Figure 1.5a below plots the trends in birth weights and the incidence of LBW for various groups of mothers before and after IRCA. Panel A plots raw trends. As discussed above, the incidence of LBW among U.S. born mothers is much higher than among foreign-born mothers. Although we do not see much difference in raw trends, panel B plots conditional birth outcomes for the same groups of mothers. I plot the marginal coefficients of birth outcomes by year after running the equation (1). It shows that the average outcomes for all three types of mothers are similar before IRCA and seem to increase for foreign-born and Mexico born mothers. Similarly, the incidence of LBW remains constant for U.S. born mothers and seem to have decreased from among mothers born outside of the U.S.

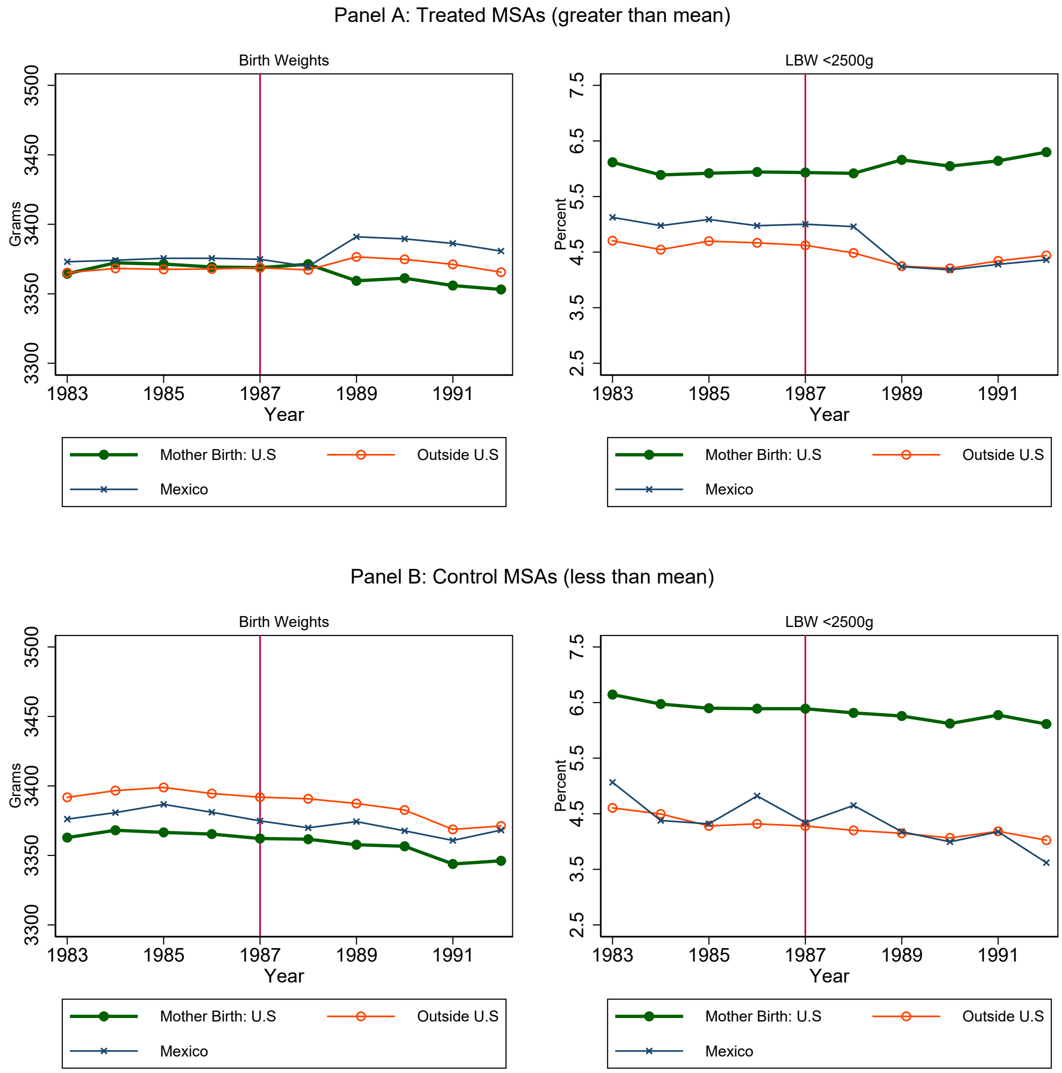
Figure 1.5a: IRCA - Trends by mother birth Country



Notes: Panel A plots mean birth weights and LBW by mothers' birth country. Panel B presents the marginal coefficients of mothers' birth country after running equation 1 above before and after IRCA applications.

Next in Figure 1.5b I plot the conditional trends graphs by mothers living in MSAs that greater and less than mean IRCA applications. Panel A shows the birth outcomes for mothers living in MSAs that had IRCA applications of greater than mean. Birth weights for all three groups are similar before the law and start to increase among foreign-born and Mexican-born mothers. However, in panel B, the trends are similar for all groups of mothers before and after the program likely due to the mothers here not being affected by the program.

Figure 1.5b: IRCA - Conditional Trends by MSA Type



Notes: Panel A plots the marginal coefficients of mothers' birth country after running equation for treated MSAs (MSAs that is greater than mean) and panel B plots the marginal coefficients of mothers' birth country for control MSAs (MSAs that is less than mean) before and after IRCA applications.

One way to check whether there were differential trends in birth outcomes among treated and control groups of mothers is to run a placebo test. Specifically, I code the post-period as 1985 and 1986 and pre-period as 1983 and 1984 and run equations (1) and (2) above. Table 1.4 below presents the results. None of the results, both in DD and DDD regression, are significant. Further, the coefficients are much smaller and close to zero. To some extent this validates the parallel trend assumption.

Table 1.4: Placebo Test, IRCA Applications

Mother's Cntry of Birth	Foreign vs U.S.		Mexico vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression				
Treat_Mother*Post	0.029	0.028	2.312	-0.019
	(1.572)	(0.057)	(3.182)	(0.061)
Panel B: DDD Regression				
Model 1:	-1.747	0.153	4.135	0.027
Treat_Mother*Post*Treat_MSA_Mean	(3.214)	(0.116)	(13.325)	(0.367)
Model 2:	1.957	0.013	10.046	0.358
Treat_Mother*Post*Treat_MSA_Median	(3.631)	(0.130)	(26.932)	(0.677)
Model 3:	1.666	0.107	-13.597	1.578*
Treat_Mother*Post*Treat_MSA_Cont	(13.330)	(0.416)	(25.633)	(0.875)
N	317900	317900	232470	232470
Dep Var Mean	3364.87	5.88	3367.98	5.94

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. Post is coded as 1 for year 1985 and 1986 and 0 for years 1983 and 1984. All regressions control for the year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

We see that the IRCA had a significant impact, especially among foreign-born mothers, in reducing the incidence of LBW. Overall, the program had an impact on the increase in average birth weights and reduction of incidence of LBW by 5 to 15 percent.

Event Study

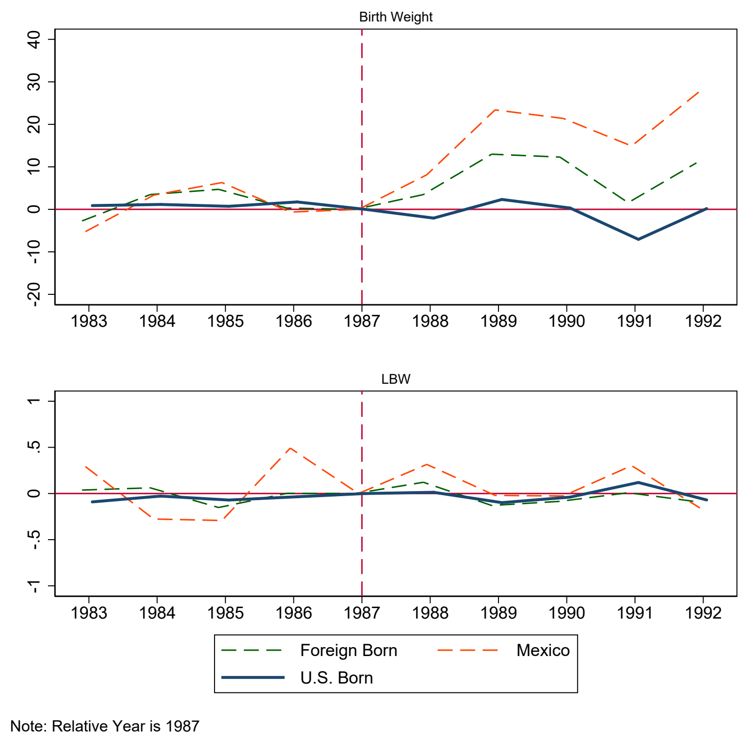
Next I perform an event study analysis for different groups of mothers living in treated and control MSAs. Specifically, this means estimating equation (2) above with year dummies interacted with MSA type dummies. This will also allow to detect if there are differential trends between different groups of mothers before IRCA. For each group of mothers (U.S.-born, foreign-born and born in Mexico) I estimate the following equation.

$$y_{gzt} = \alpha + \sum_{j \neq k} \beta_j (MSA\_Treat\_Type_z * Year(t = j)) + \pi X_{gzt} + \delta_z + \lambda_t + \gamma_g + \epsilon_{gzt} \qquad (3)$$

where t = 1983, 1985. . . 1992 and k = 1987. Variables $(MSA\_Treat\_Type_z * Year(t = j))$ are the interaction of treatment MSAs (mean treatment)[30] with year dummies except for the year 1987 which is comparison year. I plot the coefficients of β_j from equation (3) for three different types of mothers group in Figure 1.6.

[30]Using median treatment yields similar results.

Figure 1.6: Event Study - IRCA



Notes: Panel A plots mean birth weights and Panel B plots LBW by mothers' birth country.

Figure 1.6, panel A shows the event study analysis for birth weights. The coefficients from 1983 to 1986 are smaller and around zero and starts to increase after 1988. One point to note is that, for babies born to U.S. born mothers, average birth weights remain the same after the IRCA. This is not surprising since they are not affected by the law. Panel B shows the event study analysis for LBW. The coefficients after 1987 are decreasing but the trends are not as clear. Appendix Table 1.A5 shows the results of equation (3).[31] Similarly, appendix Figure 2.A1, shows the Figure 1.6 with 95 percent confidence interval lines.

[31] I also perform a F-Test for coefficients of year 1983-1986 being jointly 0. All p-values are insignificant except for birth weight for foreign-born mothers.

1.5.2 Main results ICE

Next, I look at the results from the increase in ICE arrests. Table 1.5a displays the impact of the increase in ICE arrests after 2016 on birth outcomes. Panel A shows the results of DD results. Column 1-2 compares foreign-born mothers to U.S-born mothers; column 3-4 compares the U.S.-born mothers to mothers born in Latin American and Caribbean countries; and column 5-6 compares U.S. born mothers to mothers born in Central American countries. The table is structured similarly to Table 1.3 above. Average birth weight for babies born to foreign-born mothers living in MSAs that saw an increase in ICE arrests after 2016 decreased, and the incidence of LBW increased, but the coefficients are not significant.

Panel B shows DDD results. Model 1 compares the double-difference results between mothers living in MSAs that saw an increase in ICE arrests of greater than mean. It shows that babies born to foreign-born mothers living in MSAs that had higher ICE arrests on average weighed 13 grams less, and the incidence of LBW increased by 0.403 percentage points (relative to the mean of 6.42) after 2016. Model 2 shows similar results, but when comparing MSAs that had greater than median applications. The coefficients are similar in magnitude. Model 3 shows that a 10-percentage point increase in ICE arrests per 100 foreign-born population corresponds to a decrease of 2 grams in birth weights and an increase in the incidence of LBW by 0.04 percentage points for babies born to foreign-born mothers after 2016. Similarly, when comparing the U.S born mothers to mothers born in Latin American countries (columns 3 and 4), the effect size increases slightly, and the signs are similar. Further, when comparing mothers born in Central American countries, the magnitude of coefficients increases more. Overall, the increase in ICE arrests after 2016 increased the incidence of LBW by 3-8 percent. The magnitude of coefficient increases in DDD estimates compared to DD estimates. This is expected, as foreign-born mothers living in MSAs that had higher ICE arrests are likely to be undocumented mothers and as such affected by the law.

Table 1.5a: Impact of ICE Arrests

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	-0.704	0.011	-5.891*	0.067	-5.787	0.036
	(2.450)	(0.065)	(3.086)	(0.083)	(4.003)	(0.114)
Panel B: DDD Regression						
Model 1:	-12.976***	0.403***	-15.656***	0.454***	-18.985***	0.513***
Treat_Mother*Post*Treat_MSA_Mean	(3.716)	(0.115)	(4.604)	(0.117)	(5.441)	(0.147)
Model 2:	-11.608***	0.214*	-14.106***	0.371***	-16.211***	0.478***
Treat_Mother*Post*Treat_MSA_Median	(3.626)	(0.114)	(4.136)	(0.122)	(5.498)	(0.161)
Model 3:	-12.616***	0.293**	-14.750***	0.403**	-16.742**	0.425**
Treat_Mother*Post*Treat_MSA_Cont	(4.827)	(0.136)	(5.627)	(0.156)	(7.022)	(0.201)
N	1062820	1062820	805166	805166	805166	805166
Dep Var Mean	3301.24	6.42	3305.99	6.48	3305.99	6.48

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Given the intense debate about immigration, race, and ethnicity during the 2016 elections, especially during the Republican primaries, coupled with increasing hate crimes in 2017, I next look at the impact by the race of mothers. Table 1.5b below shows the impact of ICE arrests for mothers by race and ethnicity. I run equations (1) and (2) above on sub-samples of mothers. Column 1-2 shows results for non-Hispanic White mothers; column 3-4 shows result for non-Hispanic Black mothers, and column 5-6 shows results for Hispanic mothers. There is a slight decrease in average birth weight for non-Hispanic white and Black mothers, but the coefficients are not significant. However, for Hispanic mothers, the effect is larger. The incidence of LBW increased by 10-12 percent for foreign-born Hispanic mothers living in MSAs that saw an increase in ICE arrest after 2016.

Table 1.5b: Impact of ICE Arrests by Race of Mother

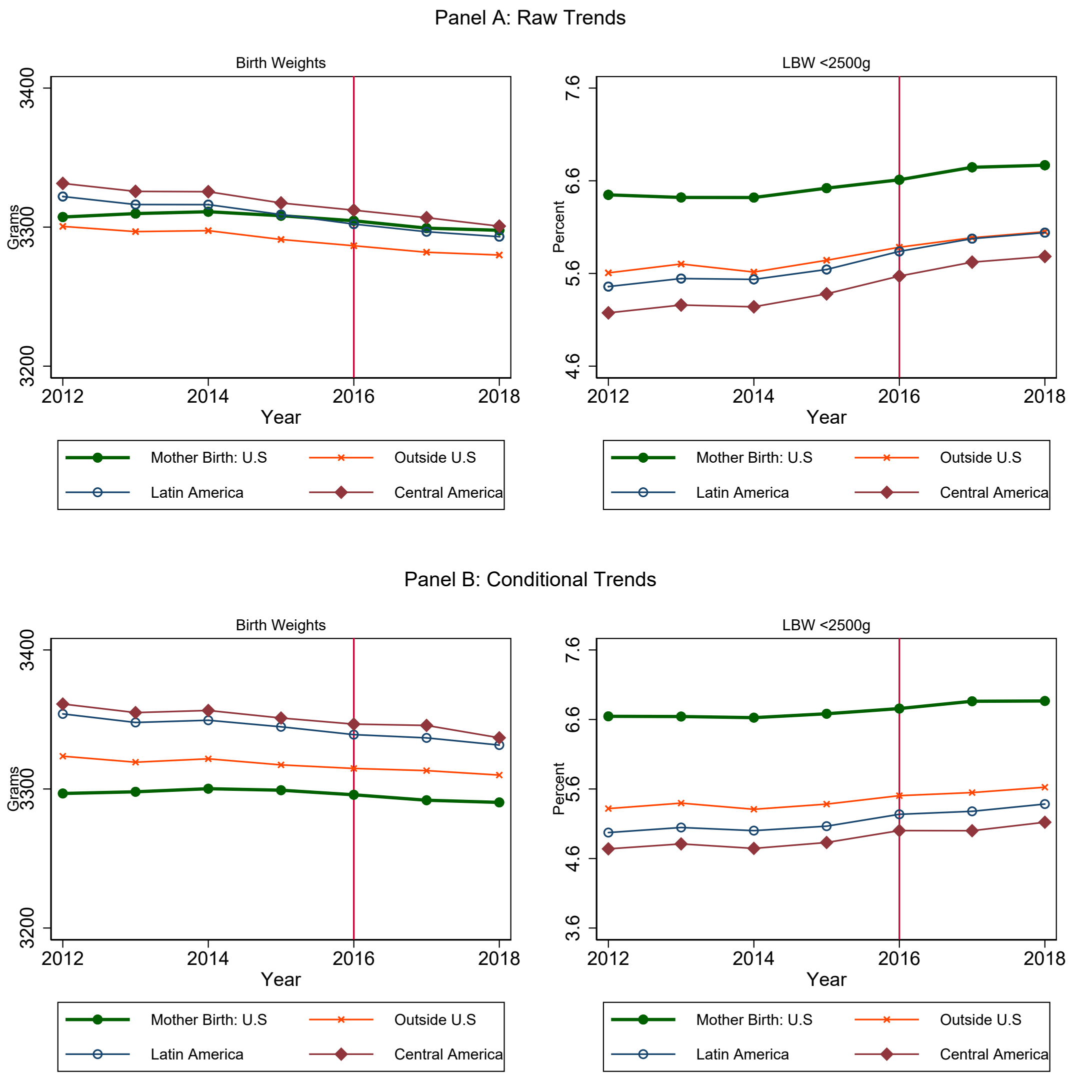
Mother's Cntry of Birth (Foreign vs U.S.)	White		Black		Hispanic	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	2.216	-0.121	9.066***	-0.360***	1.004	-0.111
	(3.014)	(0.073)	(2.673)	(0.095)	(4.803)	(0.120)
Panel B: DDD Regression						
Model 1:	-16.208***	0.334**	-8.393	0.441	-19.692***	0.661***
Treat_Mother*Post*Treat_MSA_Mean	(5.827)	(0.168)	(7.813)	(0.280)	(6.888)	(0.210)
Model 2:	-14.946***	0.162	2.659	0.048	-26.202***	0.727***
Treat_Mother*Post*Treat_MSA_Median	(4.988)	(0.140)	(6.364)	(0.222)	(7.335)	(0.194)
Model 3:	-16.834**	0.219	-3.575	0.381	-17.530**	0.552***
Treat_Mother*Post*Treat_MSA_Cont	(7.471)	(0.214)	(8.294)	(0.352)	(7.201)	(0.192)
N	344138	344138	225562	225562	305338	305338
Dep Var Mean	3374.91	5.04	3120.02	11.17	3295.37	5.96

Notes: Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All results are for foreign-born vs the U.S born mothers. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Parallel trends and Placebo Test

Similar to IRCA above, Figure 1.7a plots the birth outcome trends among different groups of mothers before and after 2016. Panel A plots raw trends. The incidence of LBW among U.S. born mothers is much higher compared to foreign-born mothers. The incidence of LBW starts to increase, starting in 2015. Likely because the impact of rhetoric about immigration during the 2015 republican primary election was felt among foreign-born mothers. Panel B plots conditional birth outcomes for the same groups of mothers, and the trends are similar.

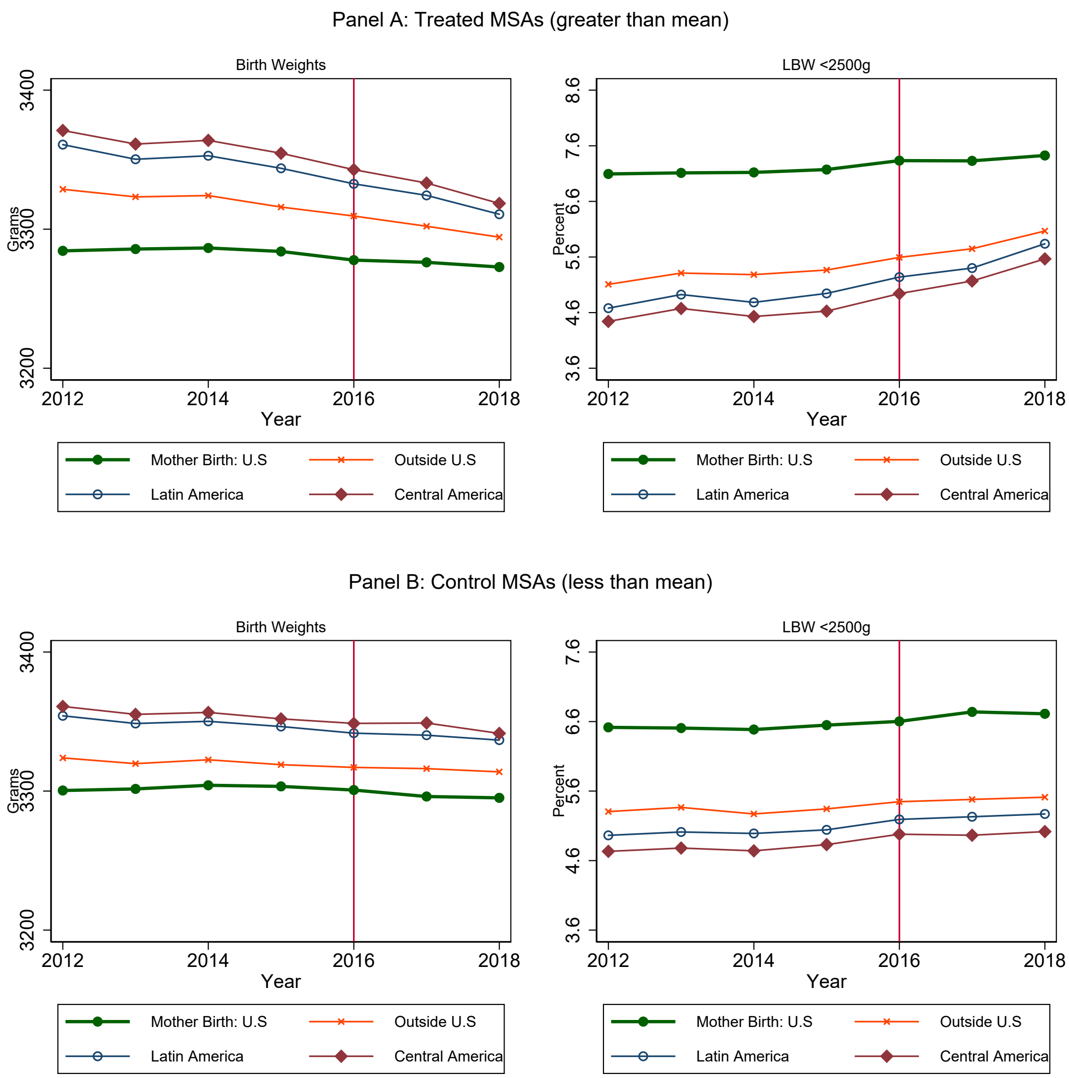
Figure 1.7a: ICE - Trends by mother birth Country



Notes: Panel A plots mean birth weights and LBW by mothers' birth country. Panel B presents the marginal coefficients of mothers' birth country after running equation 1 above before and after ICE arrests.

Figure 1.7b plots the trends among different groups of mothers in MSAs that saw an increase in ICE arrests greater than mean (panel A) and less than mean (panel B). The decrease in average birth outcomes and an increase in the incidence of LBW among foreign-born mothers are more pronounced in MSAs that saw an increase in ICE arrests of greater than mean.

Figure 1.7b: ICE - Conditional Trends by MSA Type



Notes: Panel A plots the marginal coefficients of mothers' birth country after running equation for treated MSAs (MSAs that is greater than mean) and panel B plots the marginal coefficients of mothers' birth country for control MSAs (MSAs that is less than mean) before and after ICE arrests.

A more formal way of checking whether the pre-trends were similar is to run a placebo test. I code year 2014 and 2015 as 1 (post-period) and 2012 and 2013 as 0 (pre-period). Table 1.6a panel A shows DD results. Average birth weight decreased among foreign-born mothers. However, the DDD results (panel B) are not significant, and the coefficients are close to zero except for average birth outcomes for model 2 among Latin American and Central American mothers.

Table 1.6a: Placebo Test, ICE Arrests

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	-4.010***	-0.021	-5.466***	-0.002	-5.742***	-0.013
	(1.191)	(0.045)	(1.210)	(0.040)	(1.375)	(0.048)
Panel B: DDD Regression						
Model 1:	-1.605	0.098	-0.744	-0.037	-0.037	-0.046
Treat_Mother*Post*Treat_MSA_Mean	(2.215)	(0.112)	(2.834)	(3.215)	(3.215)	(0.126)
Model 2:	-1.945	0.038	-4.656**	0.043	-5.633**	0.097
Treat_Mother*Post*Treat_MSA_Median	(2.433)	(0.084)	(2.327)	(0.085)	(2.585)	(0.094)
Model 3:	-2.959	0.057	-6.723	0.140	-7.079	0.138
Treat_Mother*Post*Treat_MSA_Cont	(3.882)	(0.140)	(4.117)	(0.134)	(5.104)	(0.160)
N	604634	604634	512278	512278	458683	458683
Dep Var Mean	3306.16	6.26	3310.04	6.33	3310.94	6.32

Notes: Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. Post is coded 1 for years 2014 and 2015 and 0 for years 2012 and 2013. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.6b shows the placebo test results by race and ethnicity of mothers. The results are similar.

Table 1.6b: Placebo Test, ICE Arrests by Race of Mother

Mother's Cntry of Birth (Foreign vs U.S.)	White		Black		Hispanic	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	-5.037***	-0.071	6.279**	-0.206*	-5.031***	0.018
	(1.927)	(0.056)	(3.108)	(0.118)	(1.272)	(0.059)
Panel B: DDD Regression						
Model 1:	-11.234***	0.244	-4.224	0.062	-0.080	-0.023
Treat_Mother*Post*Treat_MSA_Mean	(4.271)	(0.195)	(7.020)	(0.325)	(3.678)	(0.156)
Model 2:	-8.883**	0.139	7.347	-0.144	-3.559	0.026
Treat_Mother*Post*Treat_MSA_Median	(3.464)	(0.120)	(7.477)	(0.261)	(2.564)	(0.119)
Model 3:	-3.677	-0.019	-17.339**	0.092	-3.904	0.079
Treat_Mother*Post*Treat_MSA_Cont	(7.611)	(0.294)	(7.119)	(0.304)	(3.058)	(0.153)
N	196877	196877	125796	125796	173662	173662
Dep Var Mean	3377.99	4.97	3124.66	10.95	3302.38	5.79

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. Post is coded 1 for years 2014 and 2015 and 0 for years 2012 and 2013. All results are for foreign-born vs the U.S born mothers. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

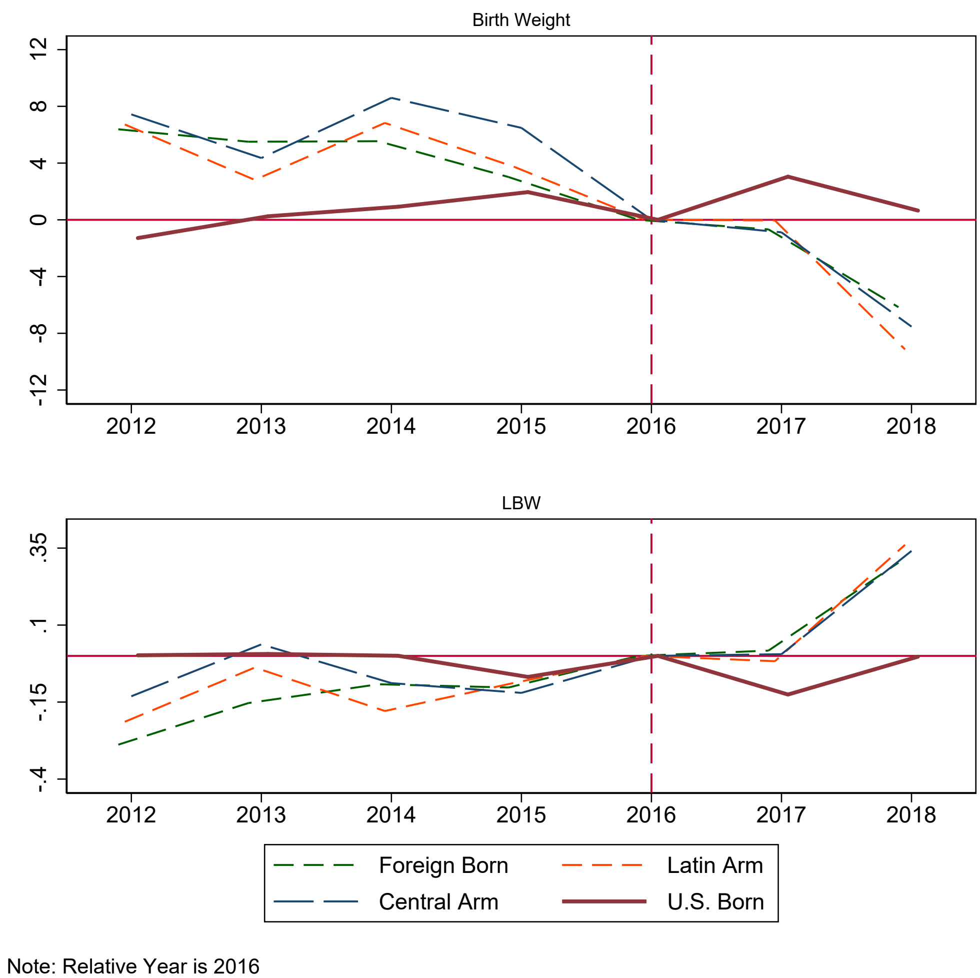
Event Study

Similarly, to section 1.5.1 above, next I perform an event study analysis for different groups of mothers living in treated and control MSAs for ICE years. Specifically, for each group of mothers (U.S. born, foreign-born, Latin American Countries born and born in Central American Countries) I estimate the following equation.

$$y_{gzt} = \alpha + \sum_{j \neq k} \beta_j (MSA\_Treat\_Type_z * Year(t = j)) + \pi X_{gzt} + \delta_z + \lambda_t + \gamma_g + \epsilon_{gzt} \qquad (4)$$

where t = 2013, 2014...2018 and k = 2016. Variables (($MSA\_Treat\_Type_z$*Year(t=j)) are the interaction of treatment MSAs (mean treatment) with year dummies except for the year 2016, which is the comparison year. I plot the coefficients of β_j for 4 different types of mothers group in Figure 1.8a.

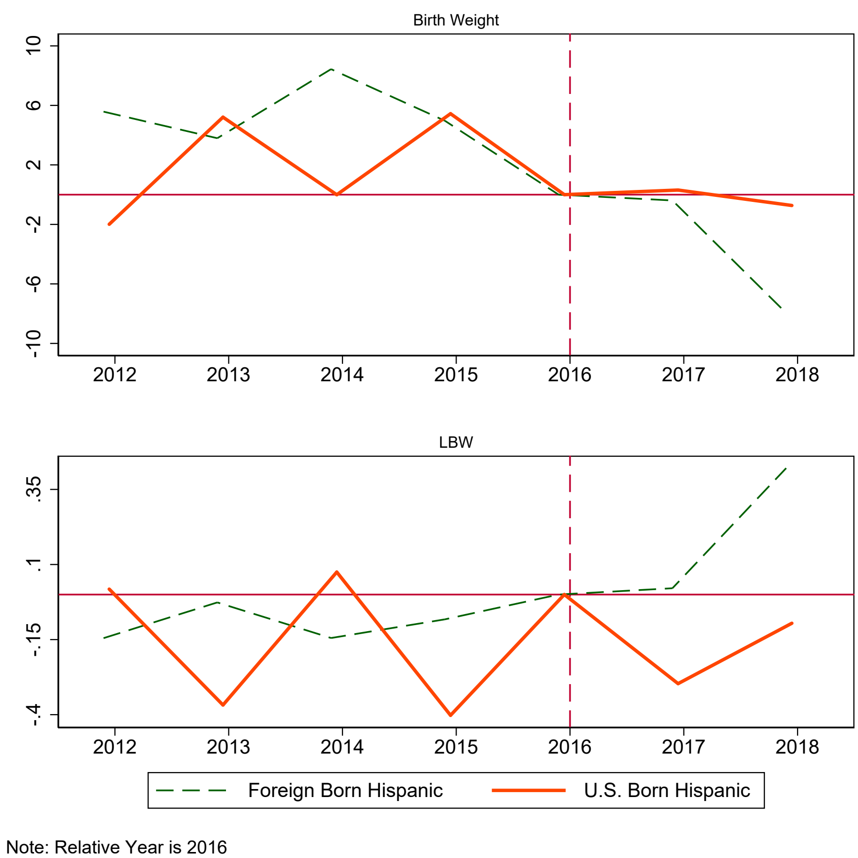
Figure 1.8a: Event Study - ICE



Notes: Panel A plots mean birth weights and Panel B plots LBW by mothers' birth country.

Figure 1.8a, panel A shows the event study analysis for birth weights. The coefficients from 2012 to 2014 for U.S. born mothers are around zero and remain the same after 2016. However, birth weight for mothers born outside of the U.S. are higher and decreases significantly in 2018. This is also not surprising as U.S. born mothers are not affected by the increase in ICE arrests. LBW follows similar pattern. Next in Figure 1.8b, I plot the same figures for only Hispanic mothers. The birth weights increase for foreign-born Hispanic mothers after 2016 but not for U.S. born Hispanic mothers. Appendix Table 1.A6 shows the results of equation (4).[32] Similarly, appendix Figure 1.A2 and 1.A3, shows the Figure 1.8a and 1.8b with 95 percent confidence interval lines.

Figure 1.8b: Event Study - ICE (Hispanic mothers)



Notes: Panel A plots mean birth weights and Panel B plots LBW for Hispanic mothers (U.S. born vs. Foreign-Born)

[32] I also perform a F-Test for coefficients of year 2012-2016 being jointly 0. None of the p-values are significant.

1.6 Mechanisms

What mechanisms lead to decreasing/increasing birth outcomes? Inputs that impacts birth outcomes are access to health care, nutritional intake, smoking and drinking, education, neighborhood, wealth, and uses of prenatal care among others (Chung et al., 2016). All these issues, while important, are beyond the scope of this study. However, the data allows to look at the prenatal care utilization of mothers. One of the leading indicators of low and premature birth prevention is the use of prenatal care. Given the stigma, and fear of being reported to ICE, immigrant mothers may delay prenatal care (PNC) visits. Perceived fear of deportation and harassment may also delay immigrant mothers' visits (Hacker et al., 2015). The utilization of PNC among immigrant mothers is inadequate. Studies estimate that uninsured women are up to five times more likely to delay prenatal care until late in their pregnancies and consequently have adverse health outcomes, including low baby birth weight and prematurity (Haas et al., 1993).

I examine the PNC utilization of pregnant mothers before and after the interventions. Specifically, I analyze whether mothers had any prenatal care visits and the total number of prenatal care visits. The structure and methodology are similar to above. Table 1.7 presents the impact of IRCA law on PNC characteristics. The number of PNC visits went up by about 2 times in all specifications. The incidence of mothers with no PNC is not significant, although the signs are negative, indicating that the percentage of the mothers with no PNC went down after the IRCA.

Table 1.7: Mechanisms IRCA Applications

Mother's Cntry of Birth	Outside vs U.S.		Mexico vs U.S.	
	No PNC	Num. of Visits	No PNC	Num. of Visits
Panel A: DD Regression				
Treat_Mother*Post	-0.440	0.680***	-0.868*	1.410***
	(0.275)	(0.229)	(0.451)	(0.345)
Panel B: DDD Regression				
Model 1:	0.324	1.491***	1.359	1.191***
Treat_Mother*Post*Treat_MSA_Mean	(0.432)	(0.328)	(1.764)	(0.390)
Model 2:	-0.692***	1.143***	-1.668	1.647***
Treat_Mother*Post*Treat_MSA_Median	(0.246)	(0.265)	(1.459)	(0.410)
Model 3:	-0.853	5.220***	-2.017	2.302
Treat_Mother*Post*Treat_MSA_Cont	(1.551)	(1.551)	(3.218)	(2.721)
N	880723	858760	647730	631438
Dep Var Mean	1.97	10.09	1.96	10.20

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are mother's prenatal care utilization. All regressions control for the year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.8a shows results for PNC utilization for ICE arrests. Panel A shows DD regression results. The incidence of mothers with no PNC increased for all groups of mothers, and the number of PNC visits decreased. For DDD specifications, however, the results are opposite.

Table 1.8a: Mechanisms ICE Arrests

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	No PNC	Num. of Visits	No PNC	Num. of Visits	No PNC	Num. of Visits
Panel A: DD Regression						
Treat_Mother*Post	0.234**	-0.139***	0.457**	-0.259***	0.551**	-0.240***
	(0.115)	(0.033)	(0.181)	(0.052)	(0.219)	(0.064)
Panel B: DDD Regression						
Model 1:	0.003	0.181***	-0.119	0.311***	-0.182	0.279**
Treat_Mother*Post*Treat_MSA_Mean	(0.267)	(0.069)	(0.273)	(0.118)	(0.333)	(0.136)
Model 2:	0.064	0.177***	0.053	0.315***	-0.001	0.248**
Treat_Mother*Post*Treat_MSA_Median	(0.196)	(0.065)	(0.313)	(0.098)	(0.369)	(0.126)
Model 3:	0.062	0.231**	-0.020	0.392***	-0.063	0.359**
Treat_Mother*Post*Treat_MSA_Cont	(0.212)	(0.097)	(0.261)	(0.146)	(0.327)	(0.157)
N	901595	901595	891124	891124	794671	794671
Dep Var Mean	1.60	11.25	1.66	11.27	1.68	11.28

Notes: Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependents variables are the mother's prenatal care utilization. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.8b shows the same results by the mother's race and ethnicity. The incidence of mothers with no PNC increased among Hispanic mothers, and the number of PNC visits decreased. However, for DDD specifications, the results are not significant.

Table 1.8b: Mechanisms ICE Arrests, by Mother Race

Mother's Cntry of Birth (Foreign vs U.S.)	White		Black		Hispanic	
	No PNC	Num. of Visits	No PNC	Num. of Visits	No PNC	Num. of Visits
Panel A: DD Regression						
Treat_Mother*Post	0.062	-0.079**	0.019	-0.059	0.448**	-0.233***
	(0.076)	(0.032)	(0.133)	(0.049)	(0.177)	(0.067)
Panel B: DDD Regression						
Model 1:	-0.147	0.145**	0.215	0.223***	0.082	0.185
Treat_Mother*Post*Treat_MSA_Mean	(0.202)	(0.060)	(0.470)	(0.085)	(0.277)	(0.115)
Model 2:	-0.067	0.138**	0.233	0.089	0.171	0.221*
Treat_Mother*Post*Treat_MSA_Median	(0.164)	(0.059)	(0.279)	(0.085)	(0.296)	(0.118)
Model 3:	-0.094	0.104	0.199	0.398***	-0.063	0.298**
Treat_Mother*Post*Treat_MSA_Cont	(0.131)	(0.080)	(0.507)	(0.124)	(0.207)	(0.122)
N	340530	340530	221747	221747	301435	301435
Dep Var Mean	1.07	11.70	3.07	10.37	2.00	10.86

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependents variables are the mother's prenatal care utilization. All results are for foreign-born vs the U.S born mothers. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

The results presented above strongly suggest that the reduced form estimates measuring the impact of IRCA applications and ICE arrests are causal. However, the results might be biased if other laws that might impact birth outcomes were passed along with IRCA applications and ICE arrests. To address such concerns, I next conduct various robustness checks.

1.7 Robustness Checks

1.7.1 Endogeneity - Medicaid Expansion

As noted above, concurrent with the IRCA law, the federal government expanded Medicaid, thereby giving states some flexibility to implement the expansion. Although the law mandated all states to cover the benefits by 1990, if some states expanded earlier or more than others, the estimate could be biased. States had a two-year window to meet the federal standard for minimum coverage for Medicaid. By 1989, all States were mandated to cover citizens up to 133 percent of the Federal Poverty Level. I use the data from Hill (1990), that lists the year which states decided to expand the FPL coverage and the percentage of coverage.[33] I control for these two variables and re-run Table 1.3 above. Appendix Table 1.A7 shows the results, and the result does not change.

Similarly, following the ACA in 2010, and the Supreme Court decision of 2012,[34] states could opt-in/out of Medicaid expansion. Using the data from the Kaiser Family Foundation (KFF, 2020), I create a start year of Medicaid expansion and the percentage of population covered by the state. I control for these variables and re-run Table 1.5a above, and the results are robust. Appendix Table 1.A8 shows the results. Given that some MSAs falls in multiple states controlling with the state dummy variable becomes tricky. For MSAs that are in multiple states, I took the mean of the states.[35]

Although I control for yearly per-capita government transfers by MSAs and start year of Medicaid expansion in the specification above, if the expansion affected birth outcomes differentially across treated and control groups, then the estimates above might be biased. Using Current Population Survey (CPS) data, I plot the percentage of mothers who have

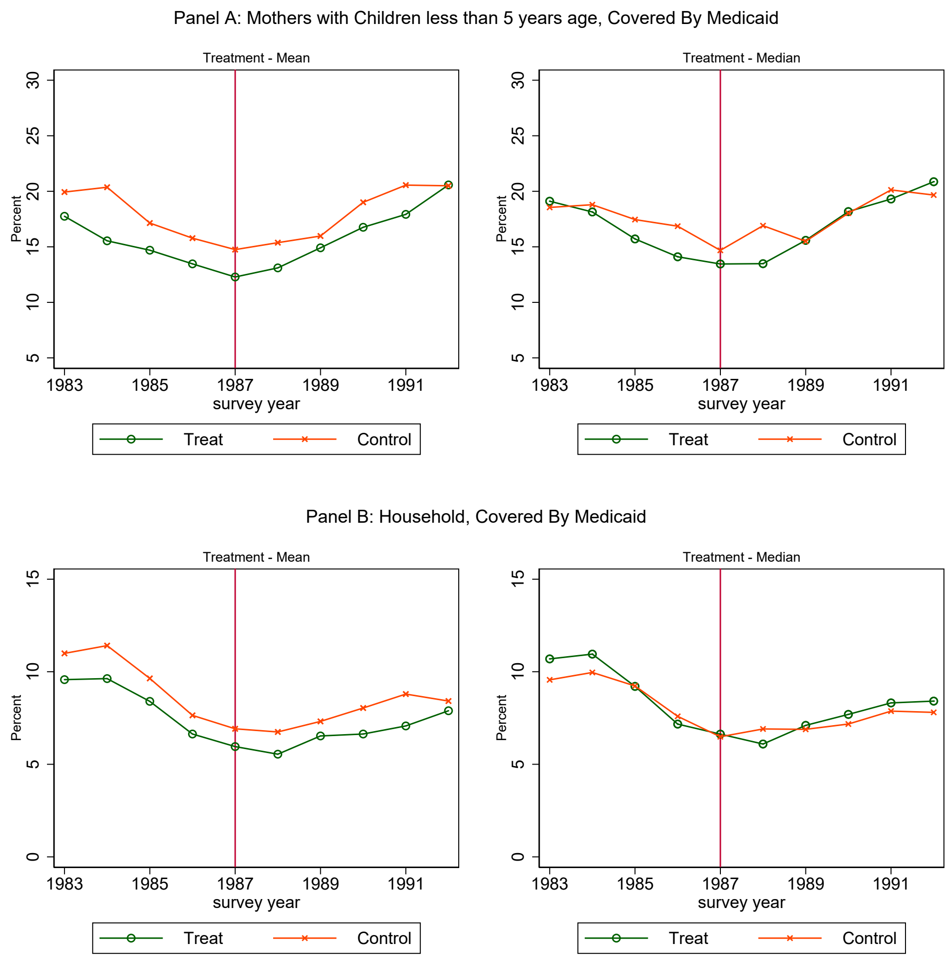
[33] Percentage of coverage is for 1989 and 1991. I replace 75 percent for year 1988 and earlier. I replace coverage of 1991 for 1990-1992.

[34] *In National Federation of Independent Business v. Sebelius.*

[35] If for example, one MSA falls in 3 state, and only 2 state expanded Medicaid in 1989 then, that MSA has a dummy variable of 0.66.

children less than 5 years old who were covered by Medicaid before and after IRCA. The data do not identify the mother's nativity or place of birth. Figure 1.9a below plots the yearly coverage. The top two figures plot Medicaid coverage of mothers living in treatment and control MSAs by mean and median. Post-1987, the coverage starts to increase, but the rates are similar in both types of MSAs. The bottom figures plot the percentage of households that had at least one person covered by Medicaid, and it shows a similar pattern.

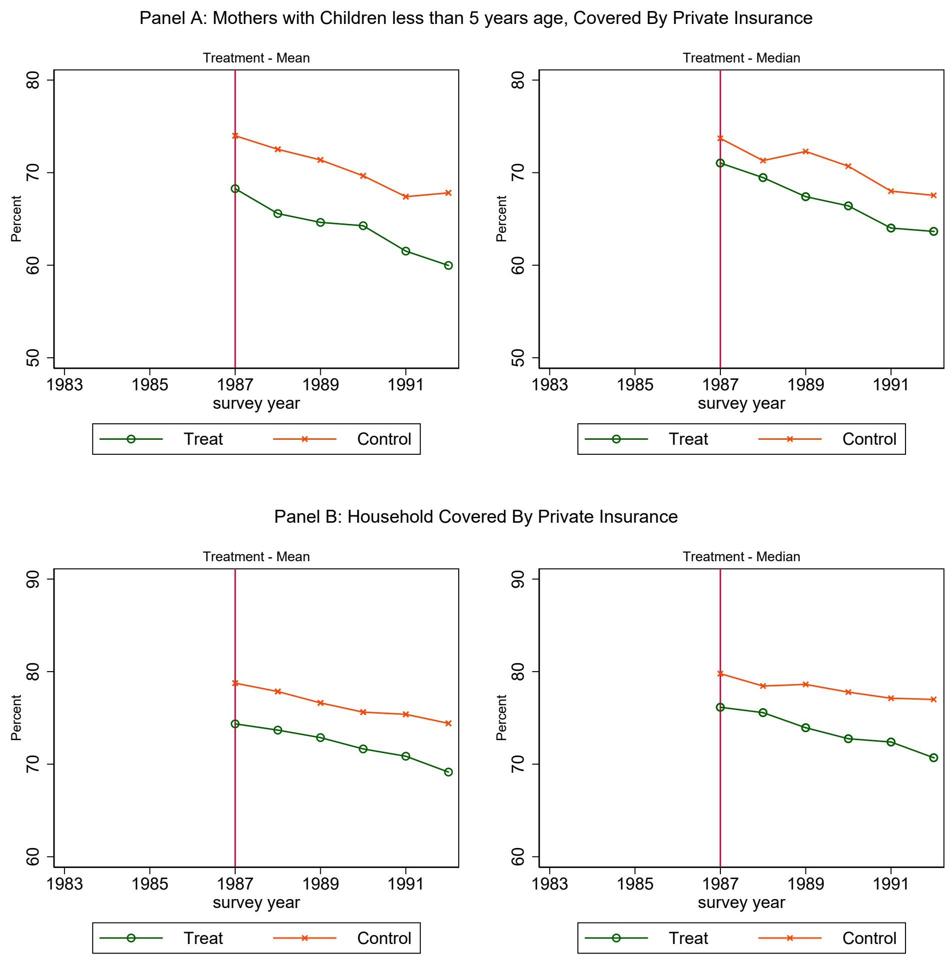
Figure 1.9a: Medicaid Coverage 1983-1992



Notes: Panel A plots yearly percentage of mothers who have children less than 5 years old who are covered by Medicaid before and after IRCA applications. Figure on the left plots MSA treatment defined by mean and the right is for median. Panel B plots the same graphs but for percentages of households covered by Medicaid.

Figure 1.9b plots mothers and households covered by private insurance. The data pre-1987 were not available. It shows that private insurance coverage is decreasing, but the trends are parallel. It is consistent with the literature that following Medicaid expansion, there is some evidence of crowding out of private insurance coverage.

Figure 1.9b: Insurance Coverage 1983-1992



Notes: Panel A plots yearly percentage of mothers who have children less than 5 years old who are covered by private insurance before and after IRCA applications. Figure on the left plots MSA treatment defined by mean and the right is for median. Panel B plots the same graphs but for percentages of households covered by private insurance. Data is not available from 1983-1986.

Although the trends look parallel, I run a DD regression for Medicaid and Insurance coverage as dependent variable for mothers and households before and after the IRCA. The dependent variable is coded 1 if mothers and households are covered by Medicaid. Demographic group control includes age, marital status, education, and race.[36] Table 1.9 below shows the results. None of the coefficients is significant likely suggesting that the Medicaid expansion affected mothers living in treated and control MSAs similarly.

Table 1.9: Medicaid Expansion of 1986

MSA Treat Type:	Mean		Median		Continuous	
	Mother	HH	Mother	HH	Mother	HH
DD	0.709	-0.091	0.907	0.190	6.632	0.449
	(1.121)	(0.408)	(0.932)	(0.362)	(5.994)	(2.522)
N	55593	360532	55593	360532	55593	360532
Dep Var Mean	16.96	7.68	16.96	7.68	16.96	7.68

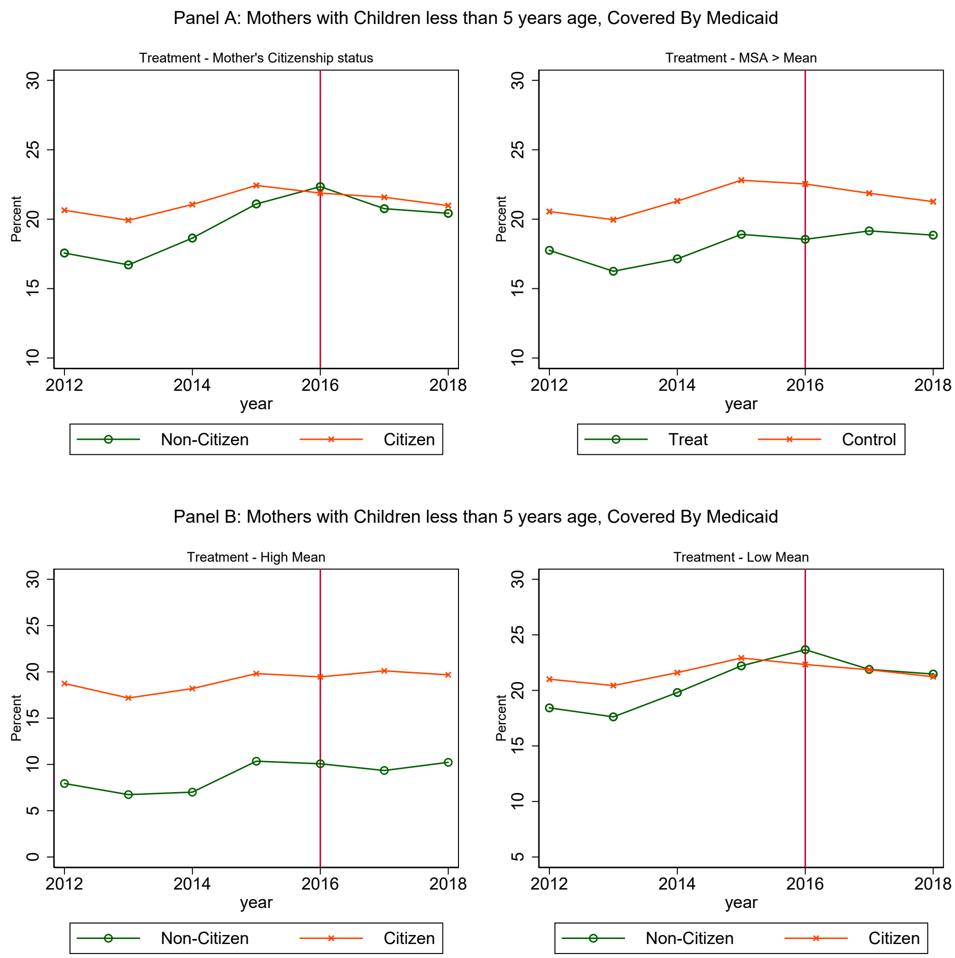
Notes: Only the DD coefficient is reported. Dependent variable Mother is a binary variable indicating if mothers with children less than five years of age are covered by the Medicaid. HH is a binary variable indicating if a household is covered by the Medicaid expansion of 1986. Observations are at the mother and HH level respectively. Columns 1-2 present results for treatment greater than mean per capita IRCA application, columns 3-4 present results for treatment greater median and column 5-6 presents results for continuous treatment. All regression controls for the year and MSA fixed effects. Columns 1, 3 and 5 controls for mothers' age, education, marital status, and race/ethnicity. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Next, I perform a similar exercise for ICE arrests. Using American Community Survey (ACS) data, I plot Medicaid coverage for non-citizen mothers. Figure 1.10a top left plots, Medicaid coverage for mothers with children less than 5 years old. The coverage increased after 2013 for such mothers and converged by 2016, probably because of the expansion of Medicaid coverage post-2014. Top right plots the coverage by MSA type (defined greater than mean). MSAs that saw an increase in ICE arrests of greater than mean has lesser coverage, but the trends are similar. The bottom left figure plots the coverage by non-

[36]Race is defined as, White, Black and Others.

citizen and citizen mothers in MSAs with greater than mean ICE arrests. The difference in coverage is higher, but the trends are similar. Bottom right plots by mother's type in MSAs that saw an increase in ICE arrests less than mean, and the coverage is very similar.

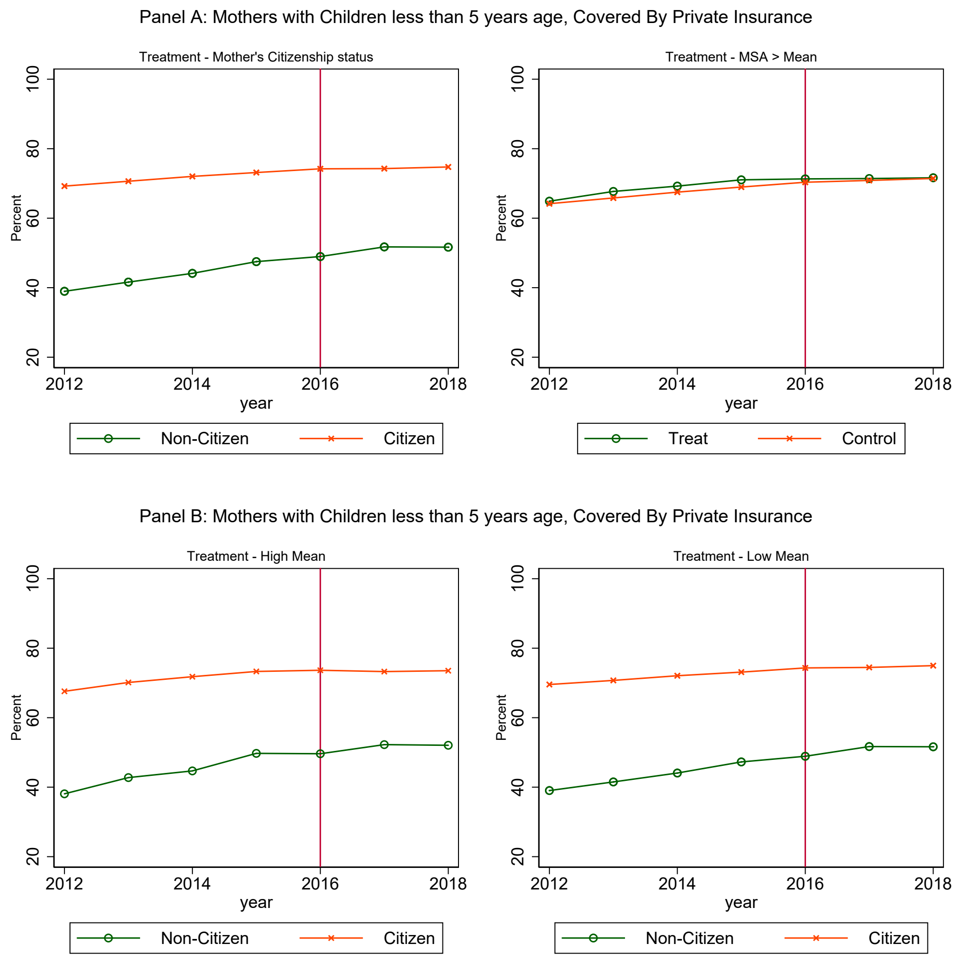
Figure 1.10a: Health Insurance Coverage



Notes: Panel A left figure plots yearly percentage of mothers who have children less than 5 years old who are covered by Medicaid during before and after ICE arrests by mother's nativity. Figure on the left plots the same coverage by MSA treatment defined by mean. Panel B plots the same graphs by mothers' nativity for treated MSA (left) and control MSA (left). MSA treatment definition is mean treatment.

Next, in Figure 1.10b below, I plot similar figures for private insurance coverage. Non-citizen mothers are covered by private insurance at a much lower rate, but the trends are similar.

Figure 1.10b: Health Insurance Coverage



Notes: Panel A left figure plots yearly percentage of mothers who have children less than 5 years old who are covered by private insurance during before and after ICE arrests by mother's nativity. Figure on the left plots the same coverage by MSA treatment defined by mean. Panel B plots the same graphs by mothers' nativity for treated MSA (left) and control MSA (left). MSA treatment definition is mean treatment.

Next, I run regression as Medicaid coverage and private insurance coverage for mothers with children less than 5 years of age dependent variable. Table 1.10 Panel A presents DD regression results, and panel B presents DDD regression. Column 1-2 present results for all mothers. It shows that non-citizen mothers' Medicaid coverage and private insurance coverage went up after 2016; however, that is not the case for DDD regression. There is significant negative coverage for Medicaid and positive coverage for private insurance for model 2 but not significant for other models. Columns 3-8 present result by mothers' race, and there is no consistent difference in Medicaid as well as private insurance coverage. This further assures that the expansion of Medicaid after 2010 did not affect native and foreign-born mothers living in treated and control MSAs differentially.

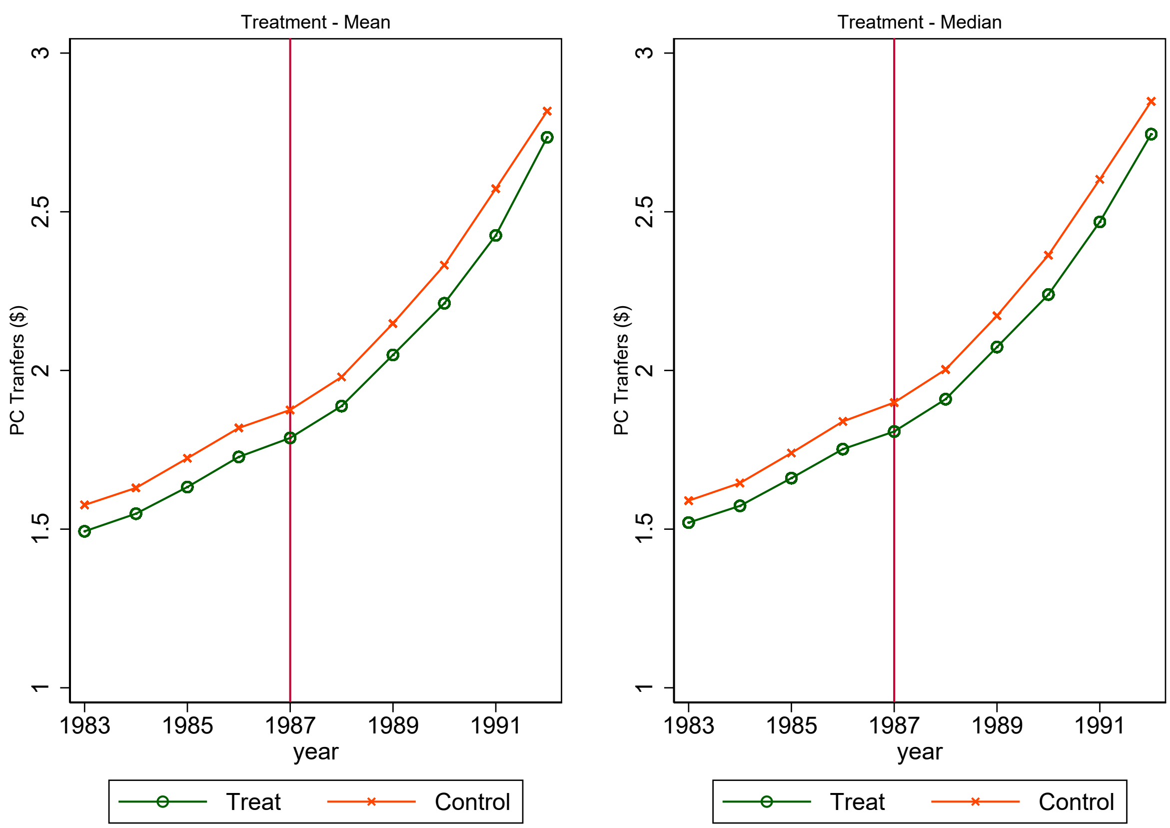
Table 1.10: Medicaid Expansion 2014

	All Mothers		White		Black		Hispanic	
Panel A: DD Regression	Medicaid	Insurance	Medicaid	Insurance	Medicaid	Insurance	Medicaid	Insurance
Non-Citizen*Post	2.695***	0.938**	3.455*	-2.408	0.494	-1.593	1.947*	1.384**
	(0.767)	(0.433)	(1.760)	(2.030)	(1.907)	(1.762)	(1.003)	(0.678)
Panel B: DDD Regression								
Model 1:	-1.118	2.072	-2.782	-4.091	-1.232	1.892	1.308	2.804
Non-Citizen*Post*Treat_MSA_Mean	(1.407)	(1.544)	(3.597)	(5.484)	(5.464)	(5.618)	(2.369)	(2.743)
Model 2:	-3.432***	1.930**	-0.082	1.676	0.943	2.002	-3.292**	0.153
Non-Citizen*Post*Treat_MSA_Median	(1.169)	(0.939)	(3.410)	(3.895)	(3.815)	(3.498)	(1.590)	(1.411)
Model 3:	0.771	-2.563	4.386*	-10.173**	5.595	-5.100	0.570	-2.028
Non-Citizen*Post*Treat_MSA_Cont	(2.029)	(2.344)	(2.455)	(4.296)	(6.627)	(6.980)	(1.939)	(2.080)
N	390168	390168	201105	201105	48284	48284	101616	101616
Dep Var Mean	26.69	58.64	19.19	73.55	44.29	44.40	30.32	38.91

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Non-Citizen is coded 0 if a mother is either a U.S. citizen (including naturalized citizen) and 0 otherwise. Observations are the individual (mother) level. Dependent variable Medicaid is coded 1 if mothers with children less than five years of age are covered by the Medicaid and Insurance is coded 1 if mothers with children less than five years of age are covered by private insurance. Columns 1-2 presents results for all mothers, columns 3-4 presents result for non-Hispanic White mothers, columns 5-6 presents result for non-Hispanic Black mothers and columns 7-8 presents results for Hispanic mothers. All regression controls for the year and MSA fixed effects and mothers' age, education, marital status and race/ethnicity. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Furthermore, along with Medicaid, the Earned Income Tax Credit (EITC) was expanded in 1986 and again in 1990. States had some flexibility in implementing EITC. Hoynes et al. (2015a) find that "a $1,000 treatment-on-the-treated leads to a 2 to 3 percent decline in low birth weight". One way to check if these laws might have differentially impact treated and control MSAs would be to look at the growth of government funding by year. I graph per capita government transfer before and after each law in treated and control MSAs. Figure 1.11 shows the transfers before and after the IRCA law. Overall, the transfers are increasing; however, the increment is parallel between treated and control MSAs both before and after the IRCA.

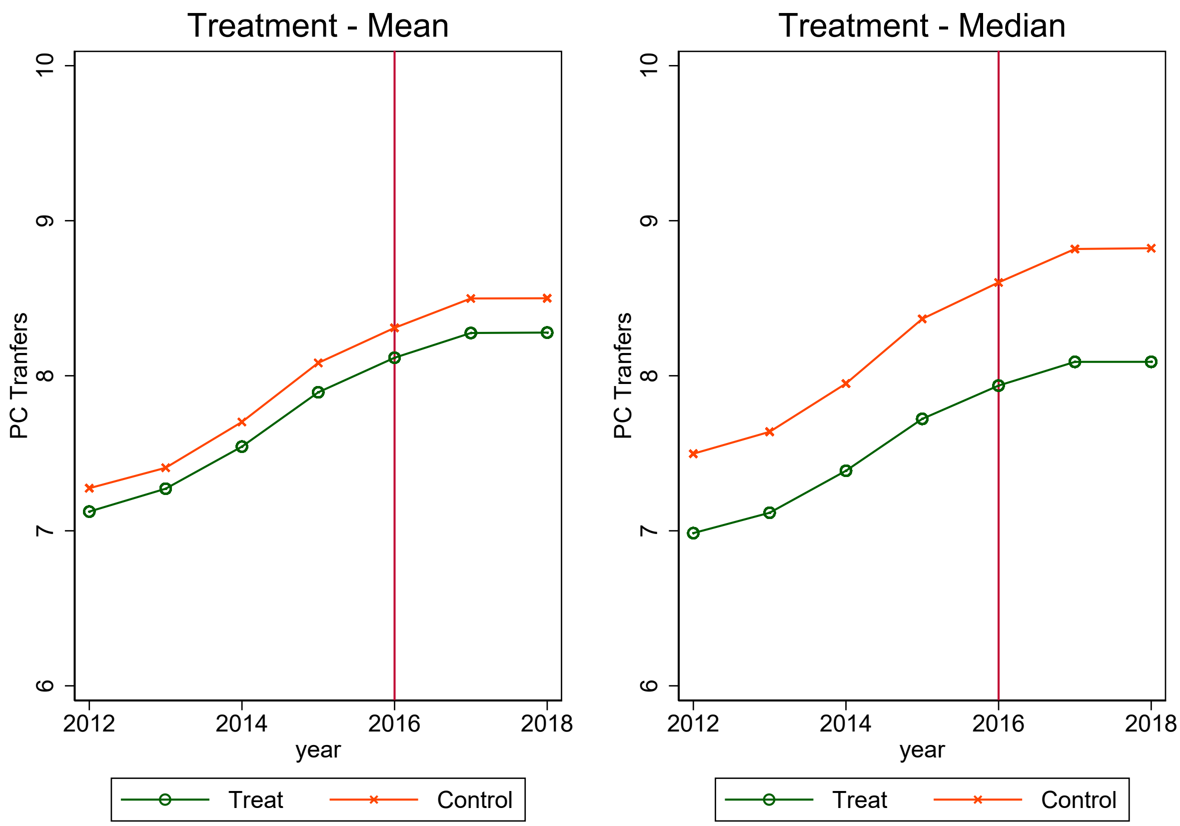
Figure 1.11: Per-Capita Transfers 1983-1992



Notes: The figure plots yearly per capita government transfers for treated and control MSAs before and after IRCA applications which is defined as the sum of: "Personal current transfer receipts- Receipts of persons from government and business for which no current services are performed. Current transfer receipts from government include Social Security benefits, medical benefits, veterans' benefits, and unemployment insurance benefits. Current transfer receipts from business include liability payments for personal injury and corporate gifts to nonprofit institutions."

Figure 1.12 shows the transfer before and after 2016, and the trends are parallel indicating that although there is a slight difference in government transfers between treated and control MSAs, the trends are similar.

Figure 1.12: Per-Capita Transfers 2012-2018



Notes: The figure plots yearly per capita government transfers for treated and control MSAs before and after ICE arrests which is defined as the sum of: "Personal current transfer receipts- Receipts of persons from government and business for which no current services are performed. Current transfer receipts from government include Social Security benefits, medical benefits, veterans' benefits, and unemployment insurance benefits. Current transfer receipts from business include liability payments for personal injury and corporate gifts to nonprofit institutions."

1.7.2 Endogeneity – Sanctuary Cities

Among many other things, one of the main roles of Sanctuary cities is to limit the role of federal and local officials in emigration enforcement by refusing to honor ICE detainers' request (Martínez-Schuldt and Martínez, 2017). Citing that these jurisdictions encourage more undocumented immigrants which deter more crime, the executive order entitled "Enhancing Public Safety in the Interior of the United States" (Executive Order No. 13768, 2017) was signed in 2017. This law primarily threatened to withhold funding for law enforcement initiatives, from such jurisdictions until they fully cooperate with the federal government (Martínez-Schuldt and Martínez, 2017).

Sanctuary cities started as a faith-based response to the mass migration of Central American refugees to the United States in the 1980s, commonly known as the Central American Refugee Sanctuary Movement (CSM). By 1987, twenty-two city councils had declared some form of CSM (Aboii, 2014). According to the Center for Immigration Studies (CIS), as of 2020, ten states and 69 counties from 16 other states have some form of sanctuary laws passed.[37] If there is a differential impact on birth outcomes among mothers living in sanctuary states and cities, then the DDD estimates above will be biased. Using the data from CIS, I create a dummy variable for sanctuary MSAs if an MSA has at least one county that is a sanctuary county. I run a quadruple difference estimate as follows.

$$\begin{aligned} y_{mgzts} = {} & \beta_0 + \beta_1(Post_t * Treat\_Mother_m * Treat\_MSA\_Type_z * Sanctuary\_MSA_s) \\ & + \sum_{k=2}^{5} \beta_k * (4\ Tripple\ Interactions) + \sum_{k=6}^{11} \beta_k * (6\ Double\ Interactions) \\ & + \sum_{k=12}^{15} \beta_k * (4\ Linear\ Terms) + \pi X_{zt} + \delta_z + \lambda_t + \gamma_g + \epsilon_{mgzt} \end{aligned} \tag{5}$$

where β_1 is the quadruple difference estimate and coefficient of interest and indicates if the triple difference estimates in equation (2) are differentially affected sanctuary MSAs. $Sanctuary\_MSA_s$ indicates if an MSA is a sanctuary MSA. All estimation samples and

[37] https://cis.org/Map-Sanctuary-Cities-Counties-and-States

methodology are the same as equation (2) above. I run this equation across the 3 models described above.

Table 1.11 below shows the results. If sanctuary MSAs were affected differently, then we should see a positive effect on birth outcomes among foreign-born mothers living in sanctuary MSAs before and after 2016. None of the coefficients are significant for model 1. For model 2, the incidence of LBW decreased. Although not all results show positive birth outcomes among mothers living in sanctuary cities and states, I control for sanctuary MSAs and re-run estimates in Table 1.5a, and the results are robust. The results are in Appendix Table 1.A9.

Table 1.11: Impact of ICE Arrests - Quadruple Difference

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Model 1:						
Mother_Birth*Post*	1.488	0.053	-1.816	0.020	-0.766	0.122
Treat_MSA_Mean*Sanctuary_MSA	(7.171)	(0.200)	(8.765)	(0.231)	(9.949)	(0.222)
Model 2:						
Mother_Birth*Post*	1.542	-0.411**	5.858	-0.566***	12.432	-0.904***
Treat_MSA_Median*Sanctuary_MSA	(9.225)	(0.201)	(7.241)	(0.210)	(10.169)	(0.292)
Model 3:						
Mother_Birth*Post*	14.940*	-0.639*	-0.556	-0.449	0.912	-0.269
Treat_MSA_Cont*Sanctuary_MSA	(8.954)	(0.330)	(11.697)	(0.390)	(13.311)	(0.363)
N	1062820	1062820	904000	904000	805166	805166
Dep Var Mean	3301.24	6.42	3304.96	6.48	3305.99	6.48

Notes: Only the coefficient of Quadruple (DDDD) is reported. Observations are at the year, MSA, demographic group, mothers' nativity and MSA cell level. Sanctuary_MSA is coded 1 if at least 1 county in the MSA has a sanctuary county. Dependent variables are infant birth outcomes. All regression controls for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

1.7.3 Endogeneity – Fertility Selection

The IRCA law legalized around 3 million people and it could lead to changes in fertility behaviors among the newly legalized population. It could be that foreign-born mothers in treated MSAs might select to have more births due to perhaps increasing income through new and better jobs. Following Hoynes et al. (2015a), in Table 1.12, I regress the log of births on treatment types similar to the Tables 1.3 and 1.5a. Column 1-2 shows results for IRCA applications, and columns 3-5 shows results for ICE arrests. The results show a small but inconsistent impact on overall births. Panel A shows DD results. Births increase among foreign-born mothers but decrease for Mexican-born mothers. However, the results are not significant in the DDD model. Similarly, there is a decrease in births among foreign-born mothers after 2016; however, the results are opposite in the DDD model.

Table 1.12: Composition of Births

	IRCA		ICE		
Mother's Cntry of Birth	Foreign v U.S.	Mexico v U.S.	Foreign v U.S.	LA v U.S.	CA v U.S.
Panel A: DD Regression					
Treat_Mother	0.056***	-0.111***	-0.009	-0.039*	-0.108***
	(0.013)	(0.036)	(0.012)	(0.021)	(0.024)
Panel B: DDD Regression					
Model 1:	-0.006	-0.166**	0.052***	0.046	0.119***
Treat_Mother*Post*Treat_MSA_Mean	(0.037)	(0.066)	(0.019)	(0.030)	(0.036)
Model 2:	0.056**	-0.027	0.041*	0.056	0.161***
Treat_Mother*Post*Treat_MSA_Median	(0.022)	(0.068)	(0.024)	(0.043)	(0.050)
Model 3:	-0.316	-0.478	0.025*	0.020	0.058**
Treat_Mother*Post*Treat_MSA_Cont	(0.197)	(0.308)	(0.014)	(0.020)	(0.028)
N	886089	651303	1062820	904000	805166
Dep Var Mean	1.78	2.00	1.46	1.49	1.58

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are log of total births by cell. Columns 1-2 presents results for IRCA. Columns 3-5 presents results for ICE arrests. All regression controls for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Further, if the composition of birth among different groups is affected differently, then the estimates might be biased. Next, I examine the impact of IRCA applications and ICE arrests on the compositions of births among various demographic groups. Specifically, I create dependent variables equal to the share of births for each demographic's group of mothers. Appendix Table 1.A10 shows the results for IRCA applications. Appendix Table 1.A11 shows results for ICE arrests. The results show small but significant increases/decreases in the share of births; however, the pattern is inconsistent across groups.

Since the regressions in all of the models above are weighted by total births per cell, the increase in births and share of births should not be a threat to the regression design itself. However, if there are differences in the number of births, there might be concern about the endogeneity of unobservable characteristics. I control for linear year times demographic groups and re-estimate the main results. Appendix Table 1.A12 and 1.A13 for IRCA applications and ICE arrests show the results respectively, and the results are robust.

Another concern of endogeneity could be the issue of non-random migration of mothers and families into immigrant friendly MSAs. Since I look at mothers' county of residence and not the occurrence of birth, this issue should not be a problem. Although the results are robust, it might be sensitive to alternative specifications. In the following section, I carry out various sensitivity analyses.

1.8 Sensitivity Analysis

1.8.1 Alternative specifications for IRCA

First, in the primary analysis, I define my treatment MSAs using the total applications for IRCA. However, given that I am looking at birth outcomes, it may be more representative to look at applications by women age 15-49 and divide it by the MSA's foreign population. Thus, I re-define the treatment types of above mean, median, and continuous treatment and re-estimate the main analysis for IRCA. All other specifications are the same as the main results. Tables 1.13 below shows the results.

Table 1.13: Impact of IRCA Applications - Alternative Specification

Mother's Cntry of Birth	Foreign vs U.S.		Mexico vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression				
Treat_Mother*Post	7.548**	-0.276***	23.510***	-0.846***
	(3.259)	(0.077)	(3.902)	(0.097)
Panel B: DDD Regression				
Model 1:	3.561	-0.098	21.716***	-0.802***
Treat_Mother*Post*Treat_MSA_Mean	(4.231)	(0.105)	(7.288)	(0.238)
Model 2:	7.018*	-0.212**	12.187	-0.527*
Treat_Mother*Post*Treat_MSA_Median	(3.805)	(0.101)	(7.662)	(0.283)
Model 3:	226.596***	-6.205***	277.395**	-8.349***
Treat_Mother*Post*Treat_MSA_Cont	(39.474)	(0.950)	(115.738)	(2.599)
N	886089	886089	651303	651303
Dep Var Mean	3363.34	5.94	3366.73	6.01

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for the year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

The results are very similar to the main results. The coefficients are a little smaller in magnitude compared to Table 1.3. Second, in the main analysis I use all MSAs, however, only 244 MSAs have positive applications. I drop the missing MSAs and re-estimate the main analysis for IRCA. All other specifications are the same as the main results. Tables 1.14 below shows the results. The results are similar to the Table 1.3.

Table 1.14: Impact of IRCA Applications (Non-Missing MSAs)

Mother's Cntry of Birth	Foreign vs U.S.		Mexico vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression				
Treat_Mother*Post	7.335**	-0.278***	23.121***	-0.847***
	(3.278)	(0.078)	(3.885)	(0.097)
Panel B: DDD Regression				
Model 1:	15.337***	-0.411***	20.367***	-0.534**
Treat_Mother*Post*Treat_MSA_Mean	(3.068)	(0.072)	(6.536)	(0.237)
Model 2:	3.905	-0.077	35.270***	-1.327***
Treat_Mother*Post*Treat_MSA_Median	(4.305)	(0.113)	(9.310)	(0.318)
Model 3:	62.785***	-1.675***	76.121**	-2.147**
Treat_Mother*Post*Treat_MSA_Cont	(13.515)	(0.337)	(38.159)	(0.896)
N	754463	754463	543210	543210
Dep Var Mean	3361.92	5.97	3365.32	6.04

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. Only non-missing MSAs are in the sample, a total of 244 MSAs. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

1.8.2 Alternative specifications for ICE

In the main specification for ICE arrests (Table 1.5a) above, I exclude the border counties. Here I include all counties and MSAs and re-estimate Table 1.5a. The number of MSAs increase to 384 compared to 374 in the main specification. Table 1.15 below shows the results. The magnitudes of coefficients are a little smaller, and the coefficients in model 1 in DDD specifications are not significant; however, the signs are similar. Given these border counties might skew the data and therefore it is not surprising that the mean treatment specification is not significant.

Table 1.15: Impact of ICE Arrests- Alternative Specification

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	-0.155	-0.001	-5.134*	0.048	-4.847	0.014
	(2.474)	(0.065)	(2.940)	(0.080)	(3.718)	(0.106)
Panel B: DDD Regression						
Model 1:	-6.056	0.244	-8.277	0.262	-9.835	0.272
Treat_Mother*Post*Treat_MSA_Mean	(7.061)	(0.177)	(7.642)	(0.191)	(8.956)	(0.231)
Model 2:	-8.605**	0.144	-10.351**	0.277**	-11.211*	0.350**
Treat_Mother*Post*Treat_MSA_Median	(4.105)	(0.118)	(4.601)	(0.130)	(5.865)	(0.166)
Model 3:	-10.536**	0.249*	-12.508**	0.356**	-13.960**	0.363*
Treat_Mother*Post*Treat_MSA_Cont	(4.923)	(0.139)	(5.599)	(0.158)	(6.860)	(0.198)
N	1089695	1089695	927981	927981	827481	827481
Dep Var Mean	3300.63	6.40	3304.25	6.46	3305.22	6.46

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All 384 MSAs are included in the analysis. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Next, in the main analysis I use all MSAs, however, only 290 MSAs have increasing arrests. I drop the missing MSAs and re-estimate the main analysis for ICE. All other specifications are the same as the main results. Tables 1.16 below shows the results. The results are similar to the Table 1.5a.

Table 1.16: Impact of ICE Arrests (Non-Missing MSAs)

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	-1.108	0.025	-5.390***	-0.014	-5.789***	-0.021
	(2.640)	(0.068)	(1.279)	(0.042)	(1.483)	(0.051)
Panel B: DDD Regression						
Model 1:	-13.818***	0.480***	-16.831***	0.506***	-19.102***	0.528***
Treat_Mother*Post*Treat_MSA_Mean	(4.057)	(0.114)	(4.860)	(0.125)	(5.884)	(0.161)
Model 2:	-10.731***	0.191*	-10.962**	0.289**	-12.630**	0.366**
Treat_Mother*Post*Treat_MSA_Median	(3.813)	(0.115)	(4.524)	(0.121)	(5.832)	(0.162)
					665065	665065
Model 3:	-12.016**	0.274*	-13.946**	0.388**	-16.225**	0.415*
Treat_Mother*Post*Treat_MSA_Cont	(5.014)	(0.139)	(5.870)	(0.161)	(7.487)	(0.213)
N	882539	882539	753636	753636	665065	665065
Dep Var Mean	3297.72	6.46	3301.32	6.53	3302.31	6.53

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. Only non-missing MSAs are in the sample, a total of 290 MSAs All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

The raw IRCA applications and ICE arrests data in the study are at the county level, and the Natality files also have geographic details at the county level. Thus, analyzing the data at the county level might give more precision to the estimates. Next, I estimate the main results at the county level. Given that only 382 counties had more than 25 IRCA applications, I limit my analysis to these counties only.[38] Table 1.17 shows the for IRCA applications. The estimated coefficients are very similar to Table 1.3 and the standard errors are smaller.

[38]This is mainly because the number of counties with IRCA applications greater than median will be 0 and counties greater than mean will be considerably fewer in treated groups.

Table 1.17: Impact of IRCA - County Level

Mother's Cntry of Birth	Foreign vs U.S.		Mexico vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression				
Treat_Mother*Post	5.653**	-0.208***	21.462***	-0.809***
	(2.468)	(0.074)	(4.318)	(0.103)
Panel B: DDD Regression				
Model 1:	14.703***	-0.424***	23.546***	-0.609**
Treat_Mother*Post*Treat_County_Mean	(2.361)	(0.069)	(8.669)	(0.283)
Model 2:	5.657*	-0.117	36.668***	-1.688***
Treat_Mother*Post*Treat_County_Median	(3.174)	(0.092)	(10.864)	(0.397)
Model 3:	53.069***	-1.517***	69.082*	-2.055**
Treat_Mother*Post*Treat_County_Cont	(10.267)	(0.311)	(39.643)	(1.006)
N	1088958	1088958	526091	526091
Dep Var Mean	3358.57	6.03	3362.53	6.10

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, county, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for the year, county and demographic group fixed effects. Additional controls for time varying county characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per counties-year-demographics groups. Standard errors are clustered at the county level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.18 shows the results for ICE arrests. All counties are used in the estimation. The estimated coefficients are very similar to Table 1.5a and the standard error are smaller. Using the county level analysis does not change the magnitude of the coefficients but it decreases the standard errors giving the model more precision.

Table 1.18: Impact of ICE - County Level

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	-1.561	0.015	-6.873***	0.063	-6.598**	0.038
	(1.732)	(0.040)	(2.346)	(0.064)	(3.041)	(0.087)
Panel B: DDD Regression						
Model 1:	-9.548***	0.294***	-9.849***	0.305***	-11.761***	0.360***
Treat_Mother*Post*Treat_MSA_Mean	(2.553)	(0.072)	(3.206)	(0.095)	(4.012)	(0.122)
Model 2:	-6.103**	0.164**	-8.332**	0.294***	-11.536**	0.467***
Treat_Mother*Post*Treat_MSA_Median	(2.619)	(0.071)	(3.707)	(0.105)	(4.934)	(0.146)
Model 3:	-2.681**	0.076**	-2.898**	0.085**	-2.688	0.076
Treat_Mother*Post*Treat_MSA_Cont	(1.241)	(0.038)	(1.421)	(0.041)	(1.637)	(0.052)
N	3204760	3204760	2857244	2857244	2684512	2684512
Dep Var Mean	3301.74	6.44	3305.02	6.49	3305.89	6.49

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, county, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for year, county and demographic group fixed effects. Additional controls for time varying county characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per counties-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

1.9 Discussion

In this paper, I find that immigration policy favoring immigrants has had a positive impact on infant health outcomes, while policy that puts immigrants at risk has had a negative one. The results are robust across various specifications. One of the key limitations of this research is the difficulty in identifying the mother's immigrant status. The Natality files do not identify the immigration status of a mother. In addition, it is hard to identify the undocumented population in the U.S. To estimate the number of children born to documented/undocumented mothers, researchers use Natality files as well as other surveys, such as the American Community Survey and Current Population Survey. They use various methodologies to estimate the total number of immigrants (especially undocumented immigrants) in the U.S. Often they use criteria such as foreign-born mothers, mothers' origin, nonmarried mothers, and those with less than a college education to estimate the number of births to undocumented mothers (Camarota et al., 2018).

One way to check whether the immigration policies affected mothers whose characteristics closely resembles immigrants, especially undocumented mothers, is to carry out a subsample analysis for those who are likely to be documented and undocumented. If foreign-born mothers who are older and married and have more education are likely to be documented immigrants, then there should be a differential impact for these two different groups. In Appendix Table 1.A14 , I regress mothers with two different sets of demographic characteristics. One group consists of likely documented mothers who are married, older than 35, and have more than a high school education, while the other group consists of likely undocumented mothers, those who are unmarried, younger than 25, and have at most a high school education.

Columns 1–4 shows results for IRCA. Columns 1–2 shows results for "likely documented" mothers. The coefficients are similar in sign and magnitude compared to the main estimates (Table 1.3), but not all of them are significant. However, for "likely undocumented" mothers (columns 3–4), the coefficients are significant and much larger (almost double). Columns 5–8 show a similar analysis for ICE arrests. Once again, the coefficients are similar in magnitude and signs, but not all are significant for likely documented samples of mothers and are larger and significant for likely undocumented groups of mothers compared to the main estimates in Table 1.5a above. While the classification of mothers' immigration type is a rough measure, the coefficients for likely undocumented immigrant mothers are much higher for both IRCA applications and ICE arrests. This finding suggests that the main results above are therefore lower-bound estimates.

While the IRCA law affected solely immigrants or undocumented immigrants who entered the U.S after 1980, the ICE arrests might have affected all migrant mothers and, in some cases, even non-White U.S. citizen mothers. The heightened sense of fear and stress of being deported is prevalent not only among undocumented citizens but also among their friends, family, and sometimes the whole community (Perreira and Pedroza, 2019). Thus, the impact of an increase in ICE arrests may not be solely among the undocumented immigrants but even among U.S citizens and lawfully present immigrants whose legal presence in the U.S is questioned on the basis of race and ethnicity (Perreira and Pedroza, 2019). Overall, the magnitude of estimated coefficients in this paper is similar to the prevalent literature. For example, Salmasi and Pieroni (2015) finds that the legalization of 1.2 million immigrants in Italy in 2002 led to a reduction in the incidence of low birth weight by 2-3 percent. On the other hand, Novak et al. (2017) find that following a large immigration raid in a small community, babies born to Latino mothers had a 24 percent greater risk of LBW. The estimated impact of IRCA law and ICE arrests falls between these two estimates.

1.10 Conclusion

Adverse birth outcomes have been found to result in high economic costs in the form of direct medical costs and long-term developmental consequences. While the direct cost is obvious, indirect consequences could be more harmful to society. Infants born with low birth weight are likely to delay schooling, attend special education, and face worse labor outcomes and even earn less (Corman and Chaikind, 1998; Currie and Hyson, 1999). Since one in five births in the U.S. is to an immigrant mother, and approximately 300,000 births per year are to undocumented mothers understanding these infants' health impacts should be of public policy interest. Since undocumented immigrants are generally poor and are barred from receiving any government benefits, and any child born in the U.S. is automatically a citizen, understanding the health of such infants is crucial.

The 1986 IRCA policy legalized approximately three million undocumented immigrants. This paper finds that immigration laws favoring immigrants have positive impacts on infant health, while policies that put immigrants at risk have negative impacts on the birth outcomes. I find that the IRCA law reduced the incidence of LBW by 3–6 percent, and infants in general weighed 14 grams more. This effect is more pronounced among mothers born in Mexico (a decrease in the incidence of LBW by 10 and 12 percent). On the other hand, the rhetoric against undocumented immigration during the 2016 election and subsequent increase of arrests by ICE of undocumented immigration at the start of the new administration had a negative impact on birth outcomes.

I find that infants born to foreign-born mothers in MSAs that had a higher number of increases in ICE arrests weighed on average 12 grams less. More importantly, there is a significant increase in the incidence of LBW by 3–6 percent. The effect is more pronounced among mothers born in Latin American countries (an increase in the incidence of LBW by 6–7 percent). Similarly, for infants born to Central American mothers, the incidence of LBW increased by 7–8 percent. Finally, the subsample analysis shows that the incidence of LBW increased by 11–12 percent for foreign-born Hispanic mothers. Similarly, there is some improvement in the usage of PNC visits after the IRCA law. The results are robust to various sensitivity analysis and robustness checks.

Legalization policies such as the IRCA have positive effects on pregnant mothers, giving access to healthcare, better working conditions, and credit as well as increasing prenatal care visits. On the other hand, ICE arrests might induce stress, which can cause LBW and affect the infant beyond birth. Fear of deportation by ICE is one of the leading worries of immigrants (Hacker et al., 2011). While it can be assumed it to be true for undocumented immigrants, often documented immigrants are also fearful of their own "status" being questioned and being harassed (Hacker et al., 2011). Thus, in this context, the findings in this study should be of huge concern to various stakeholders of society.

These results are striking for several reasons. First, often immigration debates focus on the cost of health care, reduced wages, culture, and politics, but rarely address infant health. Second, because caring for infants regardless of their parents' status, given that all children are US citizens, is ethical, public policy should focus on improving infant health, especially reducing the incidence of Low Birth Weight.

1.11 Appendix

1.11.1 Tables

Table 1.A1: Summary Statistics of Applicants

Number of Application	Applied	Rejected
SAW Applicants	1,276,743	49,128
Non-SAW Applicants	1,762,495	96,842
Total	3,039,238	145,970

Source: Taken from Baker (2015). SAW refers to the IRCA's Special Agricultural Worker program which provided legal status to certain types of agricultural workers present in the United States.

Table 1.A2: Characteristics of IRCA Legalized Population

	IRCA Applicants
Percent from Mexico	69
Percent from Mexico, Central America	82
Percent with no high school diploma	72
Percent with college education	12
Mean age	34
Percent male	58
Percent married	62
Median weekly earnings in 2012 prices (ages 18 to 64)	$455
Ratio of earnings to all US full-time workers	60%

Source: Enchautegui, 2013 - using LPS Survey

Table 1.A3: IRCA Applicants by year

Year	Percentage of Applications
1987	38.17%
1988	61.99%
1989	0.26%

Source: Enchautegui, 2013 - using LPS Survey

Table 1.A4: Mother Birth Country Classification

Latin America and the Caribbean (LA)		Central America (CA)
Antigua and Barbuda	Guyana	Mexico
Argentina	Haiti	Guatemala
Bahamas	Honduras	Honduras
Barbados	Jamaica	Nicaragua
Belize	Mexico	El Salvador
Bolivia	Nicaragua	Costa Rica
Brazil	Panama	Panama
Chile	Paraguay	Belize
Colombia	Peru	
Costa Rica	Saint Kitts & Nevis	
Cuba	Saint Lucia	
Dominica	St. Vincent & Grenadines	
Dominican Republic	Suriname	
Ecuador	Trinidad and Tobago	
El Salvador	Uruguay	
Grenada	Venezuela	
Guatemala		

Table 1.A5: Event Study - IRCA

	U.S. Born		Foreign-Born		Mexico Born	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Year 1983	0.883	-0.091	-2.725	0.037	-5.238	0.292
	(2.425)	(0.079)	(2.771)	(0.099)	(17.648)	(0.472)
Year 1984	1.152	-0.028	3.482	0.062	3.345	-0.276
	(2.069)	(0.074)	(3.117)	(0.109)	(13.485)	(0.388)
Year 1985	0.721	-0.070	4.708	-0.152	6.297	-0.292
	(1.839)	(0.072)	(3.173)	(0.116)	(8.165)	(0.344)
Year 1986	1.742	-0.035	0.303	0.002	-0.652	0.492
	(1.385)	(0.050)	(3.000)	(0.110)	(10.004)	(0.388)
Year 1988	-2.058*	0.013	3.475	0.122	8.129	0.315
	(1.178)	(0.043)	(3.078)	(0.113)	(9.756)	(0.345)
Year 1989	2.325	-0.099	12.976***	-0.129	23.407***	-0.020
	(1.819)	(0.068)	(4.554)	(0.135)	(8.404)	(0.357)
Year 1990	0.312	-0.039	12.304***	-0.085	21.391**	-0.027
	(2.219)	(0.077)	(3.836)	(0.132)	(9.791)	(0.305)
Year 1991	-7.061***	0.119	1.570	0.009	14.907*	0.302
	(2.509)	(0.087)	(3.738)	(0.127)	(8.383)	(0.316)
Year 1992	0.182	-0.070	10.932**	-0.089	27.805***	-0.156
	(2.838)	(0.097)	(4.366)	(0.140)	(9.119)	(0.269)
N	579450	579450	306639	306639	71853	71853
R-squared	0.705	0.375	0.374	0.064	0.464	0.064
Dep Var Mean	3364.59	6.12	3356.54	4.94	3399.55	4.25
F-Test	0.748	0.678	0.063	0.451	0.882	0.327

Notes: Only the β_j coefficients from equation 3 are reported. Relative year is 1987. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for the year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.A6: Event Study - ICE

	U.S. Born		Foreign-Born		LA Born		CA Born	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Year 2012	-1.285	0.002	6.384**	-0.288**	6.723	-0.214	7.433	-0.131
	(2.259)	(0.077)	(2.802)	(0.116)	(4.783)	(0.167)	(5.579)	(0.178)
Year 2013	0.248	0.006	5.506*	-0.153	2.814	-0.038	4.356	0.037
	(2.615)	(0.081)	(3.026)	(0.124)	(4.256)	(0.187)	(4.851)	(0.212)
Year 2014	0.920	0.000	5.543**	-0.092	6.831	-0.179	8.600*	-0.088
	(1.800)	(0.064)	(2.817)	(0.114)	(4.389)	(0.163)	(4.674)	(0.168)
Year 2015	1.954	-0.068	3.020	-0.103	3.717	-0.087	6.483	-0.120
	(1.522)	(0.056)	(3.244)	(0.113)	(3.691)	(0.138)	(4.146)	(0.163)
Year 2017	3.042**	-0.126**	-0.674	0.017	-0.046	-0.017	-0.881	0.005
	(1.525)	(0.064)	(3.298)	(0.112)	(4.024)	(0.158)	(4.796)	(0.182)
Year 2018	0.652	-0.003	-6.160	0.303**	-9.133**	0.361**	-7.529	0.341*
	(1.684)	(0.064)	(3.817)	(0.145)	(4.385)	(0.163)	(4.820)	(0.199)
N	669429	669429	440145	440145	270757	270757	170093	170093
R-squared	0.580	0.245	0.261	0.056	0.194	0.051	0.199	0.038
Dep Var Mean	3304.69	6.60	3290.28	5.83	3306.59	5.76	3316.76	5.46
F-Test	0.489	0.606	0.184	0.160	0.368	0.645	0.297	0.736

Notes: Only the β_j coefficients from equation 4 are reported. Relative year is 2016. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.A7: Impact of IRCA Applications (Medicaid Expansion Control)

Mother's Cntry of Birth	Foreign vs U.S.		Mexico vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression				
Treat_Mother*Post	7.456**	23.495***	-0.853***	-0.846***
	(3.246)	(3.841)	(0.097)	(0.097)
Panel B: DDD Regression				
Model 1:	14.189***	-0.346***	22.754***	-0.610**
Treat_Mother*Post*Treat_MSA_Mean	(3.271)	(0.083)	(5.975)	(0.245)
Model 2:	5.694	-0.190*	9.330	-0.728**
Treat_Mother*Post*Treat_MSA_Median	(3.855)	(0.099)	(8.025)	(0.292)
Model 3:	62.682***	-1.703***	73.113**	-2.122**
Treat_Mother*Post*Treat_MSA_Cont	(13.186)	(0.330)	(35.551)	(0.836)
N	886089	886089	651303	651303
Dep Var Mean	3363.34	5.94	3366.73	6.01

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for the year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. It also controls for the year MSAs expanded the Medicaid up to 133% of Federal Poverty Level (FPL) and yearly percentage of FPL covered. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.A8: Impact of ICE Arrests (Medicaid Expansion Control)

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	-0.762	0.007	-6.050**	0.063	-5.863	0.035
	(2.424)	(0.064)	(3.039)	(0.083)	(3.977)	(0.113)
Panel B: DDD Regression						
Model 1:	-12.955***	0.407***	-15.603***	0.458***	-19.030***	0.516***
Treat_Mother*Post*Treat_MSA_Mean	(3.718)	(0.115)	(4.586)	(0.117)	(5.414)	(0.147)
Model 2:	-11.508***	0.217*	-13.931***	0.373***	-16.187***	0.477***
Treat_Mother*Post*Treat_MSA_Median	(3.625)	(0.114)	(4.119)	(0.122)	(5.494)	(0.161)
Model 3:	-12.599***	0.298**	-14.694***	0.407***	-16.814**	0.428**
Treat_Mother*Post*Treat_MSA_Cont	(4.814)	(0.136)	(5.584)	(0.156)	(6.992)	(0.201)
N	1062820	1062820	805166	805166	805166	805166
Dep Var Mean	3301.24	6.42	3305.99	6.48	3305.99	6.48

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. It also controls for the year MSAs expanded the Medicaid after the 2012 Supreme court ruling under ACA up and yearly percentage of population covered by Medicaid. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.A9: Impact of ICE Arrests - Control for Sanctuary MSAs

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Mother_Birth*Post	-0.704	0.011	-5.891*	0.067	-5.787	0.036
	(2.450)	(0.065)	(3.086)	(0.083)	(4.003)	(0.114)
Panel B: DDD Regression						
Model 1:	-12.976***	0.403***	-15.656***	0.454***	-18.985***	0.513***
Treat_Mother*Post*Treat_MSA_Mean	(3.716)	(0.115)	(4.604)	(0.117)	(5.441)	(0.147)
Model 2:	-11.608***	0.214*	-14.106***	0.371***	-16.211***	0.478***
Treat_Mother*Post*Treat_MSA_Median	(3.626)	(0.114)	(4.136)	(0.122)	(5.498)	(0.161)
Model 3:	-12.616***	0.293**	-14.750***	0.403**	-16.742**	0.425**
Treat_Mother*Post*Treat_MSA_Cont	(4.827)	(0.136)	(5.627)	(0.156)	(7.022)	(0.201)
N	1062820	1062820	904000	904000	805166	805166
Dep Var Mean	3301.24	6.42	3304.96	6.48	3305.99	6.48

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita and if a MSA is a sanctuary MSA. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.A10: Fraction of Birth, IRCA

	DD		DDD	
	Foreign vs U.S.	Mexico vs U.S.	Foreign vs U.S.	Mexico vs U.S.
Age <25	0.011***	0.074***	0.024***	-0.050***
	(0.003)	(0.010)	(0.006)	(0.016)
Age >24 & <36	-0.005*	-0.046***	-0.015***	0.050***
	(0.003)	(0.010)	(0.005)	(0.016)
Age >35	-0.007***	-0.028***	-0.010***	-0.001
	(0.001)	(0.005)	(0.002)	(0.008)
Education 1 (Less than HS)	0.048***	0.106***	0.141***	0.126***
	(0.006)	(0.013)	(0.014)	(0.027)
Education 2 (HS)	-0.014***	0.000	-0.075***	-0.083***
	(0.004)	(0.009)	(0.009)	(0.016)
Education 3 (more than HS)	-0.034***	-0.090***	-0.071***	-0.050***
	(0.004)	(0.009)	(0.009)	(0.018)
Education 4 (missing)	0.000	-0.016***	0.005**	0.007
	(0.001)	(0.004)	(0.002)	(0.006)
Parity 1	0.016***	0.041***	0.002	0.013
	(0.002)	(0.009)	(0.004)	(0.014)
Parity 2	-0.004**	-0.001	0.006*	0.003
	(0.002)	(0.008)	(0.003)	(0.012)
Parity 3	-0.012***	-0.039***	-0.008*	-0.016
	(0.002)	(0.008)	(0.004)	(0.013)
Race 1 (White)	0.005*	0.015***	0.028***	-0.003
	(0.003)	(0.003)	(0.007)	(0.005)
Race 2 (Black)	0.266***	0.096***	-0.110***	-0.026**
	(0.008)	(0.006)	(0.017)	(0.012)
Race 3 (Others)	-0.271***	-0.111***	0.082***	0.029**
	(0.008)	(0.007)	(0.018)	(0.013)
Female Child	0.001	-0.024***	-0.004	0.032**
	(0.002)	(0.009)	(0.003)	(0.014)
Married	0.013***	-0.007	-0.027***	-0.010
	(0.002)	(0.006)	(0.004)	(0.010)

Notes: For each variable, the coefficients of DD and DDD are reported. Each coefficient is for regressing fraction of births by demographic groups on treatment and controls for year and MSA fixed effect. DD results is comparing mothers' birth countries and DDD results compare the DD results with MSA treatment define as greater and less than mean IRCA applications. Column 1 shows DD coefficients for each demographic group for mother born outside vs born in the U.S. Column 2 shows for mother born in Mexico vs born in the U.S. Column 3 shows DDD coefficients for each demographic group for mother born outside vs born in the U.S comparing with treated and control MSAs. Column 4 shows for mothers born in Mexico vs born in the U.S. with treated and control MSAs. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.A11: Fraction of Birth, ICE

	DD			DDD		
	Foreign vs U.S.	LA vs U.S	CA v U.S	Foreign vs U.S.	Mexico vs U.S.	CA v U.S
Age <25	-0.127***	-0.080***	-0.061***	0.004	-0.001	-0.004
	(0.003)	(0.004)	(0.004)	(0.004)	(0.006)	(0.007)
Age >24 & <36	0.041***	-0.000	-0.005	-0.007	-0.000	0.001
	(0.002)	(0.003)	(0.004)	(0.005)	(0.007)	(0.009)
Age >35	0.087***	0.080***	0.066***	0.003	0.001	0.004
	(0.002)	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)
Education 1 (Less than HS)	0.159***	0.296***	0.376***	0.002	0.006	0.007
	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)	(0.009)
Education 2 (HS)	-0.039***	0.005	0.016***	0.007	0.005	-0.007
	(0.003)	(0.004)	(0.004)	(0.005)	(0.007)	(0.008)
Education 3 (more than HS)	-0.129***	-0.309***	-0.402***	-0.006	-0.009	-0.003
	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
Education 4 (missing)	0.010***	0.008***	0.010***	-0.002	-0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)
Parity 1	-0.067***	-0.123***	-0.154***	0.001	-0.002	0.003
	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)	(0.007)
Parity 2	-0.008***	-0.027***	-0.037***	-0.001	0.005	-0.003
	(0.002)	(0.002)	(0.002)	(0.003)	(0.006)	(0.007)
Parity 3	0.075***	0.150***	0.191***	-0.001	-0.003	0.001
	(0.004)	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)
Race 1 (Non-Hispanic White)	-0.505***	-0.675***	-0.692***	-0.001	-0.002	-0.004
	(0.008)	(0.008)	(0.009)	(0.004)	(0.004)	(0.004)
Race 2 (Non-Hispanic Black)	-0.043***	-0.081***	-0.135***	0.003	-0.003	0.002
	(0.008)	(0.008)	(0.007)	(0.004)	(0.005)	(0.002)
Race 3 (Hispanic)	0.336***	0.778***	0.852***	-0.001	0.000	-0.002
	(0.010)	(0.008)	(0.007)	(0.005)	(0.005)	(0.005)
Race 4 (Others)	0.212***	-0.021***	-0.026***	-0.001	0.005***	0.004***
	(0.007)	(0.003)	(0.003)	(0.004)	(0.002)	(0.001)
Female Child	0.001*	0.007***	0.008***	0.005	-0.001	0.004
	(0.001)	(0.002)	(0.002)	(0.003)	(0.006)	(0.008)
Married	0.127***	0.002	-0.035***	0.001	-0.014	-0.008
	(0.006)	(0.006)	(0.007)	(0.008)	(0.009)	(0.011)

Notes: For each variable the coefficients of DD and DDD are reported. Each coefficient is for regressing fraction of births by groups on treatment and controls for year and MSA fixed effect. DD results are comparing mothers' birth countries and DDD results compare the DD results with MSA treatment define greater and less than mean IRCA applications. Column 1 shows DD coefficients for each demographic group for mother born outside vs born in the U.S. Column 2 shows for mother born in Latin American countries vs born in the U.S. Column 3 shows for mother born in Central American countries vs born in the U.S. Column 4 shows DDD coefficients for each demographic group for mother born outside vs born in the U.S comparing with treated and control MSAs. Column 5 shows for mothers born in Latin American countries vs born in the U.S with treated and control MSAs. Column 6 shows for mothers born in Central American countries vs born in the U.S with treated and control MSAs. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.A12: Impact of IRCA Applications (Linear Trends Controls)

Mother's Cntry of Birth	Foreign vs U.S.		Mexico vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression				
Treat_Mother*Post	12.401***	-0.538***	18.943***	-0.696***
	(3.258)	(0.072)	(3.721)	(0.087)
Panel B: DDD Regression				
Model 1:	11.360***	-0.266**	25.714***	-0.683***
Treat_Mother*Post*Treat_MSA_Mean	(3.791)	(0.107)	(6.131)	(0.236)
Model 2:	-0.311	0.068	9.273	-0.713**
Treat_Mother*Post*Treat_MSA_Median	(4.037)	(0.126)	(8.309)	(0.291)
Model 3:	43.846***	-0.970**	87.654**	-2.508***
Treat_Mother*Post*Treat_MSA_Cont	(14.958)	(0.411)	(35.955)	(0.807)
N	886089	886089	651303	651303
Dep Var Mean	3363.34	5.94	3366.73	6.01

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for the year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All regression controls for demographic-group times linear year dummies. All estimates are weighted by the total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 1.A13: Impact of ICE Arrests (Linear Trends Controls)

Mother's Cntry of Birth	Foreign vs U.S.		LA vs U.S.		CA vs U.S.	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression						
Treat_Mother*Post	4.113	-0.102	1.486	-0.068	2.447	-0.123
	(2.585)	(0.066)	(3.673)	(0.099)	(5.181)	(0.149)
Panel B: DDD Regression						
Model 1:						
Treat_Mother*Post*Treat_MSA_Mean	-11.924***	0.394***	-15.266***	0.461***	-14.538***	0.408***
	(3.703)	(0.116)	(4.613)	(0.116)	(5.484)	(0.154)
Model 2:						
Treat_Mother*Post*Treat_MSA_Median	-10.610***	0.206*	-13.725***	0.383***	-16.419***	0.501***
	(3.647)	(0.115)	(4.068)	(0.120)	(5.548)	(0.163)
Model 3:						
Treat_Mother*Post*Treat_MSA_Cont	-11.530**	0.280**	-14.538***	0.408***	-16.933**	0.436**
	(4.585)	(0.134)	(5.484)	(0.154)	(7.006)	(0.201)
N	1062820	1062820	805166	805166	805166	805166
Dep Var Mean	3301.24	6.42	3305.99	6.48	3305.99	6.48

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. All regressions control for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. It also controls for the year MSAs expanded the Medicaid after the 2012 Supreme court ruling under ACA up and yearly percentage of population covered by Medicaid. All estimates are weighted by the total number of births per MSAs-year-demographics groups. All regression controls for demographic-group times linear year dummies. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

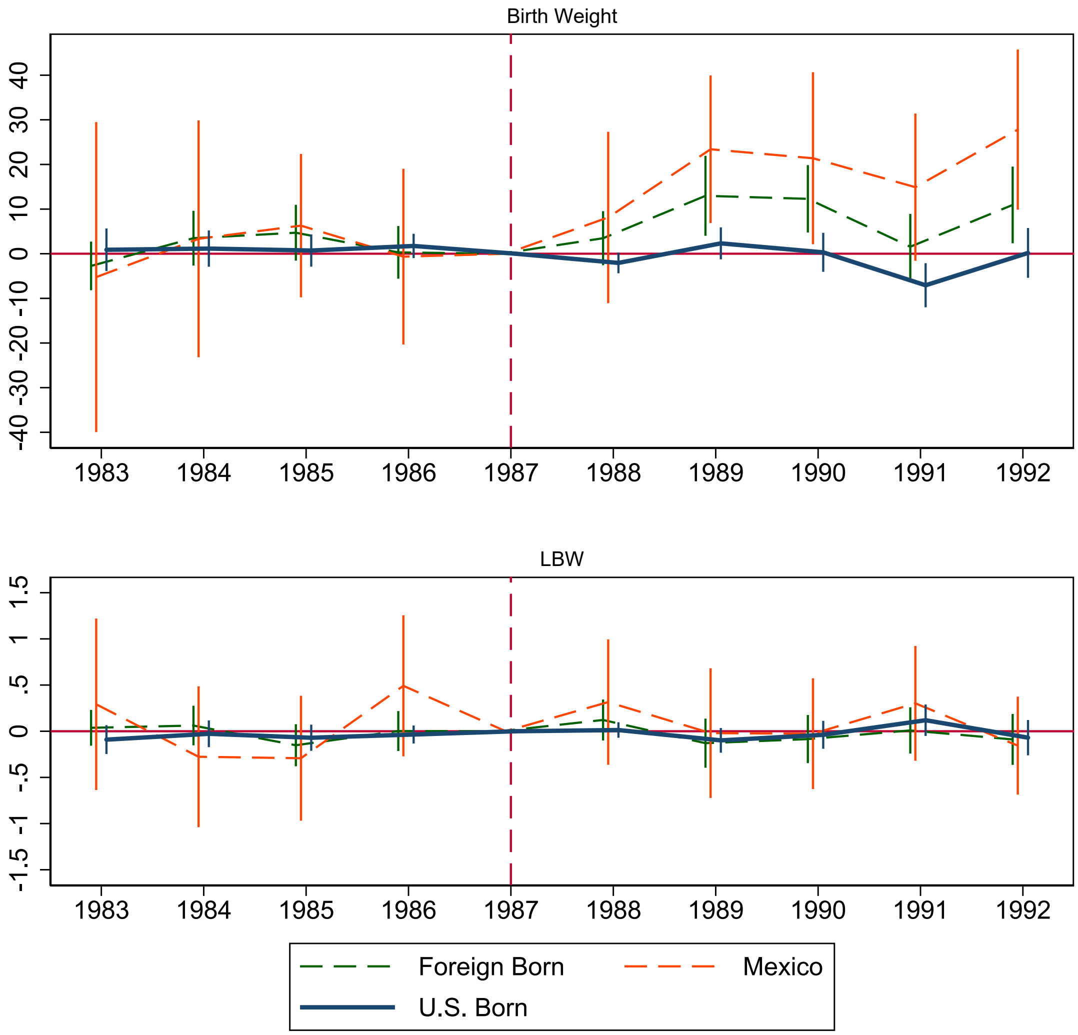
Table 1.A14 : Heterogeneous Impact of IRCA and ICE

Mother's Cntry of Birth (Foreign vs U.S.)	IRCA (Likely Documented)		IRCA (Likely Undocumented)		ICE (Likely Documented)		ICE (Likely Undocumented)	
	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW	Birth Weight	LBW
Panel A: DD Regression								
Treat_Mother*Post	2.082	0.190	-42.181***	0.958***	-2.543	0.253***	-2.006	-0.205
	(4.942)	(0.141)	(10.752)	(0.300)	(1.926)	(0.097)	(3.883)	(0.179)
Panel B: DDD Regression								
Model 1:	30.995***	-0.152	-24.396**	0.791**	-13.932**	0.493*	-23.614***	0.772*
Treat_Mother*Post*Treat_MSA_Mean	(10.501)	(0.257)	(11.400)	(0.386)	(6.874)	(0.262)	(7.271)	(0.400)
Model 2:	6.435	-0.305	-51.609***	2.071***	-5.272	-0.114	-21.192***	0.742**
Treat_Mother*Post*Treat_MSA_Median	(10.524)	(0.312)	(16.541)	(0.732)	(4.045)	(0.197)	(6.754)	(0.361)
Model 3:	158.293	-1.838	-115.629*	4.246**	-13.505*	-0.029	-15.631*	0.060
Treat_Mother*Post*Treat_MSA_Cont	(103.721)	(1.602)	(68.457)	(2.094)	(7.486)	(0.369)	(8.346)	(0.348)
N	48382	48382	50847	50847	74752	74752	60473	60473
Dep Var Mean	3466.61	4.56	3159.10	0.10	3364.99	5.47	3158.10	9.08

Notes: Panel A reports the coefficients of DD regressions and Panel B reports the DDD regression coefficients. Observations are at the year, MSA, demographic group and mothers' nativity cell level. Dependent variables are infant birth outcomes. Columns 1-4 presents results for IRCA and 5-8 for ICE arrests. Likely Undocumented implies sub sample of demographic group: married mothers, greater than high school education and greater than 35 years of age and Likely undocumented implies sub sample of demographic group: unmarried mothers, less than high school or less education and less than 25 years of age. All results compare foreign-born mothers with the U.S. born mothers. All regression controls for year, MSA and demographic group fixed effects. Additional controls for time varying MSA characteristics include percent change in total employment, per capita total transfers by the government and income per capita. All estimates are weighted by total number of births per MSAs-year-demographics groups. Standard errors are clustered at the MSA level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

1.11.2 Figures

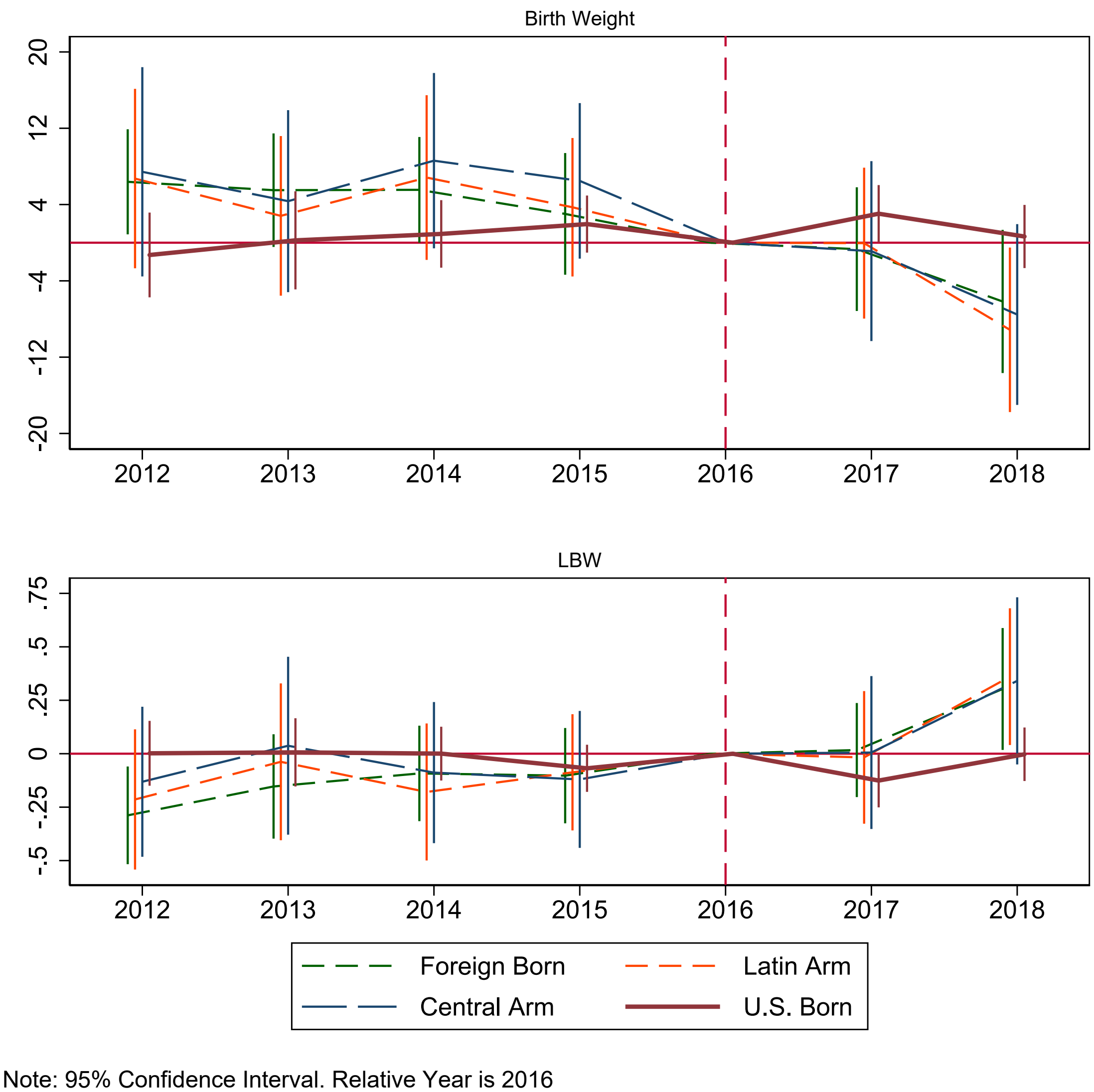
Figure 1.A1: Event Study - IRCA



Note: 95% Confidence Interval. Relative Year is 1987

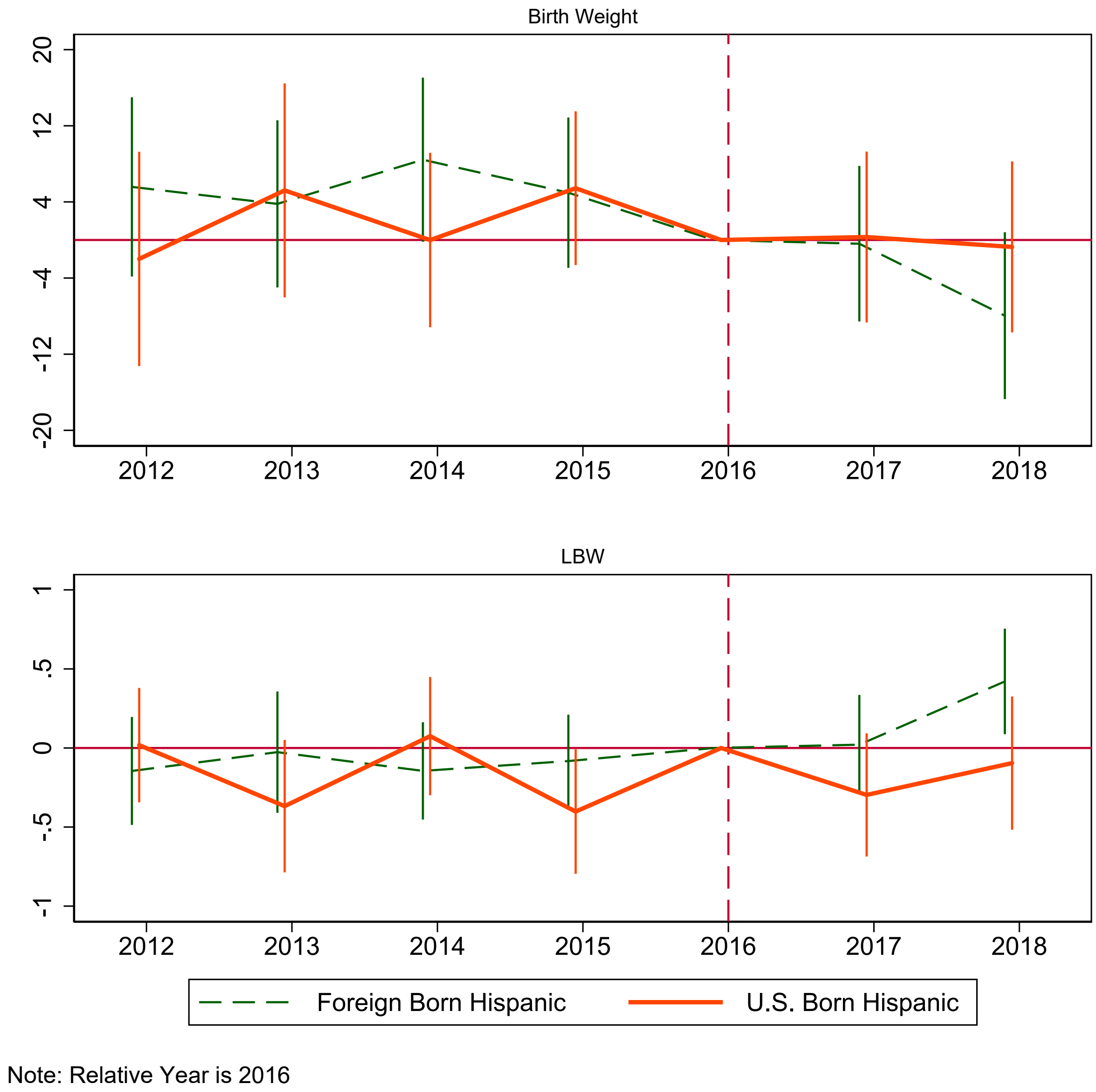
Notes: Panel A plots mean birth weights and Panel B plots LBW by mothers' birth country.

Figure 1.A2: Event Study - ICE



Notes: Panel A plots mean birth weights and Panel B plots LBW by mothers' birth country.

Figure 1.A3: Event Study - ICE (Hispanic mothers)



Notes: Panel A plots mean birth weights and Panel B plots LBW for Hispanic mothers (U.S. born vs. Foreign-Born). 95% Confidence Interval lines are included.

Chapter 2

Cash-based Maternal Health Interventions Can Improve Childhood Vaccination - Evidence from India

2.1 Introduction

More than four million infants died around the world within the first year of life in 2017, which was 75% of all under-five deaths (WHO, 2017). More than 90% of these deaths occurred in developing countries mainly due to limited access to basic but effective health interventions. Vaccination is one such intervention that plays a vital role in preventing communicable diseases and reducing infant mortality and morbidity in one of the most cost-effective ways (Anand and Barnighausen, 1994; Ekwueme et al., 2000). Specifically, vaccines prevent an estimated 2 to 3 million deaths every year from diphtheria, tetanus, pertussis (whooping cough), and measles; yet, 18.7 million infant children worldwide still lack basic vaccines (WHO, 2015). Though the lack of immunization remains a bigger problem in developing countries, vaccine-preventable diseases like measles have threatened parts of the developed world in the recent past (Nelson, 2019). The overarching policy question is to find effective policy interventions to increase vaccination. Though lack of vaccine uptake can be a result of a multitude of factors, limited available evidence suggests that parents generally want to vaccinate their children, but sometimes fail to do so, particularly in poorer countries, because of lack of resources and incentives (Banerjee et al., 2010; Das, 2010).

Despite making progress in child immunization in recent years, India has one of the biggest populations of unvaccinated children (WHO, 2020). Successive governments have focused both on maternal health services and childhood vaccination against major infectious diseases in the early 2000s. The most prominent maternal health program undertaken was Janani Surakhsa Yojana (JSY) in 2005, a conditional cash transfer program where mothers received monetary incentives varying by rural location and poverty status, for institutional or health care worker-supervised childbirth (GOI, 2006). The rationale was two-fold. First, it was based on the presumption that high out-of-pocket-expenditure deters families from seeking skilled/institutional care. Second, the actual cash benefit would provide additional incentives to access safe delivery.

Though the program was not directly focused on vaccination, there are two ways it could potentially impact the uptake. First, the program relied on health workers called Accredited Social Health Activists (ASHA) for various logistical purposes, as we discuss in detail later in the paper. One of the responsibilities of these workers was to help the mother vaccinate their child on schedule (Sidney et al., 2012; JSI, 2016; Debnath, 2018). Second, there were also potential spillover effects. The incentive to give birth at health facilities was also likely to increase the chance of being vaccinated compared to giving birth at home. Vaccines are inexpensive in India, and the actual cost to families for vaccination is small or zero meaning that institutional and informational barriers are more likely to prevent vaccination than cost barriers. According to one estimate, as of 2017, the cost of each dose of BCG cost around Rs. 3.25 ($0.05), Rs 2.6 ($0.04), and Rs. 32.5 ($0.5), respectively, while the central government allocates approximately US$25 (Rs 1625) per child for vaccines and operational costs (Chatterjee et al., 2018). Together, we expect a priori that exposure to the JSY will increase vaccination.

We examine the effect of the JSY program on child vaccination using a nationally representative longitudinal survey that collected data on a panel of women between the ages of 15 and 49, before and after the implementation of the program, allowing us to identify the effect of the program cleanly. Although there have been several studies on the impact of the JSY on maternal health, this paper offers several new contributions. We present one of the first comprehensive evidences on the effect of the JSY on child vaccination, as all the previous studies were mostly focused only on maternal health behaviors and outcomes. Second, longitudinal data allow us to track the same mothers before and after the policy, yielding clearer estimates of these effects. Third, the majority of the studies discussed below were conducted before 2012 and captured only the early effects of the program. Our data allow us to examine the program effect up to six years after the program's introduction. Finally, our results offer insights for health policymakers trying to increase vaccination uptakes in countries that have lagged.

The primary analysis uses the India Human Development Survey (IHDS), a 2-wave nationally representative longitudinal dataset. The survey collected data in 2004 and 2012, respectively, on a large set of demographic, reproductive health, and economic variables in India. The dataset has allowed us to create both a pooled and a longitudinal sample of women with recent pregnancy and delivery. The state-level rollout of the JSY across India started in 2005-06. Therefore, the first round of IHDS interviews occurred before the rollout and the second round occurred when the rollout was well underway. Our vaccination variables and control variables come from this dataset. Our treatment variable comes from the District Level Health Survey (DLHS), conducted by the Government of India. Combining two datasets allows us to link individual vaccination with district-wide deployment of the

JSY. While DLHS does not allow us to build a panel dataset, it contains specific questions on delivery-related cash transfer and is conducted several times between 2004 and 2012, allowing us to map the JSY expansion more precisely. Additionally, the majority of the previous studies have used DLHS data making our results comparable to the existing literature. On the other hand, the IHDS allows us to create a panel of mothers who gave birth before and after the introduction of JSY, a novelty compared to the pre-existing literature on the JSY.

The main empirical strategy is based on a difference in differences estimation both within a pooled cross-section and a longitudinal framework. Our sample consists of women who gave birth within 4 years of the interview in wave 1 and 6-7 years of the interview in wave 2 of IHDS. There was no JSY during the first-round interviews ending in 2004, and children born before then were not exposed to the JSY, as the program was rolled out after the fiscal year 2005-2006 and for all JSY treatment calculations, we maintain fiscal years. For the second round, depending on the year of birth of the child (which we can identify from the birth history module), we can assign the rate of dissemination of the JSY within that district. Additionally, to test robustness, we estimate similar models (subject to variable availability) using the DLHS data.

Using these data and empirical strategy, we find that JSY has had large effects on vaccination, though such effects are strongest for vaccines such as BCG and DPT that are administered at birth and less so for measles that is administered at least nine months after birth. Specifically, children born in districts where at least 25% of the women reported getting JSY incentives that year are between 4% to 6% more likely to receive BCG and DPT. Such effects are obtained after controlling for numerous time-varying and time-invariant confounding variables, including district and time fixed effects, and are robust to several model specifications.

The results come with some empirical concerns, the main being the fact that the JSY was not rolled out randomly across India. States that had low institutional delivery and higher maternal mortality rates were prioritized. However, there are several reasons why this is not a major threat to our strategy. First, though decisions were made at the state level; the actual rollout was not smooth and not according to the laid-out plans. Additionally, our policy variations come from district-level implementations, which are nested within states administratively in India. Even if a state was designated as a priority state, variations existed between districts, meaning that the program was not necessarily implemented in all districts in a high priority state before a low-priority one. A related empirical concern is the possibility that variations in other variables (for example, better district-level health awareness) over time may affect both JSY and women's health outcomes. There are two safeguards against this threat in our strategy. First, we control for district fixed effects in the relevant specification to control for time-invariant factors. Next, for the time-varying district-level factors, in addition to controlling for time-varying district-level factors in some specifications, we test for the presence of such forces by mapping district-level trends in vaccination before the program started rolling out. By showing visually that the pre-existing differential trends in vaccinations are parallel to each other for districts that do and do not add JSY, we argue that there does not seem to be the presence of such forces. The timing of birth and JSY introduction also ameliorates concerns of self-selection, as we associate JSY dissemination at the time of birth, not conception. One has to anticipate the introduction of JSY several months ahead to select herself into the program, making it an unlikely source of bias. Nevertheless, we perform additional tests to examine if such behavior is found in our sample.

Policymakers and academics often argue that a significant investment in mothers leads to improvement in children's health by making pregnancy safer, preventing mother-to-child transmission of disease, and expanding programs of immunization. Our results contribute to such a policy domain. If maternal health programs are also instrumental in improving some of the child outcomes, then such benefits should be factored into the overall cost-effectiveness and welfare analysis of these programs. At the same time, policymakers need to be careful that such programs may not completely substitute more targeted programs like specific immunization drives, such as for polio vaccination in India that has helped eliminate the disease.

2.2 Background

2.2.1 Vaccinations in India

Vaccination rates in India lag behind global rates. For example, the rate of complete vaccinations for infants aged 12-23 months in terms of BCG (Bacillus Calmette Guerin), DPT (Diphtheria, Pertussis and Tetanus Toxoid) and the measles was 64% in India in 2010, compared to the global rate of 81%. One notable exception is the polio vaccine, which has achieved universal coverage.[1] Lack of vaccination has also impacted overall infant health, mortality, and gender equity. India ranks 163 among 223 countries in the world in terms of life expectancy at birth (Arsenault et al., 2017). There was inequity in uptake also, as Oster (2009) found that about 30% of the gender imbalance in mortality could be explained by differential access to vaccination. Several factors contributed to this lack of uptake, including supply bottlenecks, infrastructure problems such as lack of cold storage and absentee health workers (Paul et al., 2011). There is also considerable spatial heterogeneity in vaccination (Khan et al., 2018). Finally, while vaccines like BCG that are administered at birth have a higher rate, such rates for vaccines like DPT that require follow-up visits with multiple doses and MMR that requires taking a child to vaccination several months after birth were lower.

[1] We do not include polio vaccination in our analysis, because (i) As of February 25, 2012, the World Health Organization (WHO) removed India from the list of 'polio-endemic' countries (PAHO, 2012), and (ii) the campaign for polio vaccination started in in India in 1979 by forming the Expanded Programme on Immunization (EPI) (John and Vashishtha, 2013), a long time before the JSY came into existence,. A similar approach has also been adopted in previous literature as citetvikram2012a. We find no significant impact of the JSY on polio vaccine uptake, as shown in Appendix Table 2.A2.

The current vaccination schedule is still governed by the Universal Vaccination Program (UIP) adopted in 1985.[2]

2.2.2 JSY in India - Timing and Expansion

The Janani Suraksha Yojana (JSY), the Maternal Protection Scheme, was started by the Government of India as part of the National Rural Health Mission (NRHM) in India. The main objective was to lower maternal, and infant mortality rates by increasing institutional and professionally supervised birth delivery particularly in rural areas and for families below the poverty line. The program was set up as a conditional cash transfer scheme to overcome financial constraints and set incentives for extra income. Under the scheme, all pregnant women irrespective of age and socioeconomic status were eligible for a cash incentive after delivery in a government or accredited private health facility. The actual amount of money offered differed between “high focus” and “low focus” states. The former referred to states that lagged in maternal health indicators before the program, while the latter constitutes all other states. In 10 high-focus states, the cash incentive was 1,000 rupees (roughly \$20 at the time) for women from urban areas and 1,400 rupees (\$28) for women from rural areas. In low-focus states, the cash incentive is 700 rupees and was limited to women below the poverty line, as well as scheduled caste/tribe women (GOI, 2014). The logistics were often carried out by community-level health workers who were formally called ASHA, a word that means ‘hope’ in many Indian languages and is an acronym for Accredited Social Health Activists. These social health workers were hired mainly on a part-time basis and tasked

[2]Currently the vaccination schedule under the UIP includes BCG (Bacillus Calmette Guerin) (1 dose at Birth, up to 1 year if not given earlier), DPT (Diphtheria, Pertussis and Tetanus Toxoid) (5 doses; three primary doses at 6,10,14 weeks and two booster doses at 16-24 months and 5 Years of age), OPV (Oral Polio Vaccine) (5 doses; dose at birth, three primary doses at 6,10 and 14 weeks and one booster dose at 16-24 months of age); Hepatitis B vaccine (4 doses; 0 dose within 24 hours of birth and three doses at 6, 10 and 14 weeks of age); and Measles (2 doses; first dose at 9-12 months and second dose at 16-24 months of age) (GOI, 2015). We also abstain from analyzing Hepatitis vaccines due to overall lack of uptake, particularly in the early 2000’s.

with identifying pregnant women and motivating them to receive antenatal care, institutional deliveries, and postnatal care.[3] Even though JSY was a federal (central government in India) scheme, its implementation differed across the states and union territories (Lim et al., 2010). However, the overall program expanded over the years with the number of beneficiaries increasing from 0.74 million in 2005-06 to 10 million in 2009-10, thus covering around 40 percent of total deliveries in the country (Paul, 2010).

2.2.3 Previous Research on the impacts of JSY

A considerable body of research exists on the impact of JSY, but the focus has mostly been on antenatal and postnatal care utilization and on in-facility deliveries. By contrast, with some exceptions, vaccination has not been rigorously analyzed to the same extent in the context of this program despite being a first order policy concern. Some of the preliminary studies were confined to specific villages or hospitals in India (Deolalikar et al., 2008; Satapathy et al., 2009). Given the principal focus of the program on reproductive health, more research focuses on maternal health outcomes. Khan et al. (2010) reported on the early expansion of JSY in the state of Uttar Pradesh and one of the few ones to document some of the logistics of the program, such as targeting, awareness, and whether eligible families received the cash incentives after delivery. They found that women were aware of the program, and the majority of them did receive full financial incentives. They also found that institutional deliveries increased. This is similar to Guin et al. (2012)) and Gupta et al. (2012), who evaluated (before and after design) the impact of JSY in a Tertiary Referral Hospital in the state of Madhya Pradesh in India. However, they did not find any change in maternal mortality. Other studies confirm local findings at the national level. Randive et al. (2013) found an increase in institutional delivery combining two national surveys, but no changes

[3]Appendix Table A4 reports our results on the impact of JSY on institutional deliveries to update the existing literature some of which dealt with only early impacts.

in maternal mortality rates. In one of the first rigorous statistical impact evaluations of the program at the national level, Lim et al. (2010) found that the program increased institutional deliveries and reproductive health outcomes significantly. In a qualitative study, Vellakkal et al. (2017) identify some enabling factors like involvement of ASHA workers and some impeding factors such as trust in traditional child-birth processes. Carvalho and Rokicki (2018) replicated those findings and confirmed that the original results to be replicable and robust to various specifications. However, they failed to make a convincing argument for causality. Powell-Jackson et al. (2015) use a linear difference-in-difference model to estimate the impacts of the JSY on maternal and neonatal health. One of the first studies on the impacts of JSY on immunization was Carvalho et al. (2014), who analyzed the early effect of JSY on immunization, among other variables using DLHS data. Our study is different in several respects. First, the paper uses data from 2007-2008 which captured only early impacts of the program. Second, it did not focus on obtaining causal estimates. Finally, in a recent paper, Andrew and Vera-Hernández (2020) discuss the role of supply constraints played in mediating the spill-over effects of the JSY on vaccination. Our study extends this approach to focus on additional positive spillover effects. We also extend the methodology by using longitudinal data, accounting for individual unobserved heterogeneity. Moreover these studies report the early effects of the program, while our analysis uses more recent data.[4] Finally, a recent work by Chatterjee et al. (2018) found a positive impact of JSY on immunization in India, though the data used and methodology are different from this paper.

[4]Previous research has also focused on different outcomes of interest. For example Modugu et al. (2012) focus on the effects of the JSY on out of pocket expenditures, while Jain et al. (2015) focus on the role of the JSY in reducing socioeconomic inequalities and conclude that the program has been equitable in nature by weakening the link between mother's wealth and education and desirable health behaviors like health professional-assisted deliveries and postnatal checkups. Surprisingly, none of the studies above emphasized the program's potential impact on infant mortality or other health outcomes including vaccination.

2.3 Data

2.3.1 Data Sources

The data come from multiple sources. To measure children and mothers' characteristics, including vaccination rates, we use two waves of the India Human Development Survey (IHDS), a quasi- nationally representative multi-topic survey. Interviews for the two waves were conducted in 2003-04 and 2011-12, respectively. In the first wave, 41,554 households located in 384 districts (out of 593 districts identified in the 2001 Indian Census) in 33 states and union territories of India were interviewed. The primary sampling units were households living in the same residence. The household module contains information on family backgrounds such as religion and economic status. The individual module in the first wave has data on birth history, including information on the last birth of eligible women aged 15-49 and constitutes the main source of our outcome and control variables. Finally, the village module contains information on community-level infrastructure questions like the availability of roads, health centers, and hospitals.

The IHDS lacks information on JSY transfers. To construct JSY coverage and test parallel trends for vaccination rates between JSY and non-JSY districts, we use the District Level Household and Facility Survey (DLHS) for its eligible women section of round III and IV data. This is a repeated cross-section survey carried out by the International Institute for Population Sciences in Mumbai, India, and designed to provide estimates on maternal and child health and service utilization at the district level in India (NFHS-4, 2016). In particular, we use DLHS III and IV that were carried out in 2007-2008 and 2011-2012, respectively. However, for DLHS IV, the states of Assam, Bihar, Chhattisgarh, Jharkhand, Madhya Pradesh, Odisha, Rajasthan, Uttar Pradesh, and Uttarakhand were not covered in a different survey called the Annual Health Survey (AHS). Accordingly, we supplement the above data with the AHS data for the year 2010-2011 and 2012-2013, respectively. Finally, since the publicly available dataset for DLHS IV is missing for the three states of Gujarat, Jammu and Kashmir, and Delhi, we use the National Family Health Survey (NFHS-4, 2016) data to get JSY data for these states. To summarize, IHDS provides data to identify infants and their mothers in the two waves, providing a pre- and post- JSY comparison. To analyze which districts are covered by the program, DLHS, AHS, and NFHS were used.

2.3.2 Sample Selection

IHDS is a longitudinal survey of households. However, since we are concerned with neonatal health, the same women might not appear twice in the sample unless they gave birth in both rounds of interview (even though the survey could otherwise track and interview them). Additionally, there is a new group of women who gave live births within that period who were not included in the mothers' sample in the first round. To capture these changes, we have constructed two different samples. First, we have created a sample that includes all relevant mothers (and their children) in both rounds resulting in a repeated cross-section sample akin to many non-longitudinal surveys that take place over several years (for example, the District Level Health Survey in India that we have used to construct the JSY expansion variable). Second, we have created a (balanced) panel sample where every member has given birth within the relevant time window within each round. Results corresponding to this sample can be found in the appendix.

The wave I selection in both cases is identical. We gather information from the eligible women aged 15-49 about their last birth(s). As mentioned earlier, the interviews took place in 2003-2004, and the cut-off date for these women to report their last birth was January 01, 2000. Their birth history file has detailed information on childhood vaccination. For the pooled sample, we repeat this procedure in the second round where the interviews were carried out between 2011-2013, and the cut-off date to report last birth is January 01, 2005. Then we append these two samples to create our pooled estimation sample. For the longitudinal sample, we retain only those mothers who gave birth again within that relevant period in wave II. Having identified the mothers, we go into the birth history file to establish the link between children's characteristics and mothers' characteristics. Once this link is established, we merge several individual and household level characteristics from the individual and household files, respectively. Therefore, each row of data consists of a child-mother combination with their own and household information.

2.3.3 Treatment and Control Groups

While creating the JSY coverage variable, we follow the same methodology as Powell-Jackson et al. (2015). We select all women who gave birth (live birth or stillbirth) in the last 12 months by the date of the last birth. Among them, we select those who gave birth in government facilities only. Then for those who reported giving birth in government facilities, we check if they received any financial aid, especially if they answered 'yes' to the following question: *"Did you receive any government financial assistance for delivery care under the Janani Suraksha Yojana or state-specific scheme."*[5] Then we calculate the JSY coverage rates by collapsing the above variable by State and Districts, which is merged to birth-year of last births from IHDS dataset. For the three States missing in DLHS-IV, we use NFHS data to proxy different years, as discussed above. Finally, to create the JSY treatment variable, we follow the literature such as Powell-Jackson et al. (2015) and create JSY coverage thresholds to create our binary treatment variable.[6] Unlike the previous studies, we choose 25% as our threshold to create a "JSY district," instead of 10%, because we have more recent data, and the program coverage increased over time. We perform a robustness analysis with a 50%

[5] We treat the birth year as a fiscal year to align with the JSY policy and use, April 2004 -March 2005 as the birth year 2004-2005 and so forth.

[6] "Specifically, we use the term JSY coverage to refer to the number of women who gave birth in a public facility and received the cash as a proportion of women who gave birth in a public facility" ((Powell-Jackson et al., 2015), p 158).

threshold.[7]

2.3.4 Migration and Attrition

Since our identification strategy depends on exogenous geographic variations in the policy rollout, domestic migration can bias our estimates. Fortunately, domestic migration is small in India in general citetchandrasekhar2015a that is also reflected in our sample. In the balanced panel sample, 3807 mothers gave birth in both rounds. Among them, 2773 stayed in the same house, 544 split off from their initial household but remained in same building/compound, 488 split and remained in the same village/urban locality, and only two migrated elsewhere. Another potential problem is selective attrition. Note that due to the specific way our longitudinal sample is constructed, which is a balanced panel by design, the usual meaning of attrition is less relevant here. In a typical longitudinal household survey, the main attrition issue is lack of trackability. However, in our case, that is less of a problem, because we drop women in the second round who were trackable but did not give birth within the relevant time frame.

For the pooled cross-section sample, 11968 mothers reported having given births after January 01, 2000. To match the fiscal year settings, we only take births after April 2000. There were 632 births with a birthdate before April 2000. There were 432 births with no birthdate and 14 births with the birth ID missing leaving us with 10980 births in wave I. Among these women, 3807 gave birth in the second round also, which is the size of our balanced panel sample. In wave II, 14254 women who gave birth after January 01, 2005. Out of these, 456 births have a missing ID, 61 were born before January 01, 2005, and 15 had the dates of birth missing, leaving us with a sample of 13722 livebirths.

[7] We do not use a continuous measure because we think that impact of a “marginal” increase in coverage on the outcomes would fail to capture the nature of the program. However, our results are qualitatively similar when a continuous measure of JSY (percentage of women reporting receiving JSY benefit) is used.

2.3.5 Outcome and Control Variables

We examine three types of vaccination, namely, BCG, DPT (first dose), and Measles. Each mother was asked about their last birth's vaccination information. Variables are coded one if vaccine dates are written on their vaccination card or if the mother responded that her child received the corresponding vaccines. We have used the broader definition of vaccination because it closely aligns with the countrywide statistics available for India from international agencies like the World Health Organization (WHO, 2020). For the first dose vaccine of DPT, we drop the observation for children who are less than two months old during the time of the interview and were reported to have taken the vaccine. Similarly, for the measles vaccine, we exclude children under nine months old.[8] For control variables, we use the child's gender, child's age, mother's age, mother's education, mother's employment, birth order[9], if the mother gave birth in both of the rounds, father's education, interview recall period (month's since last birth and interview date), household poverty status (or asset holding) and urban location according to the 2000 census and religion dummies. In some specifications, we also control for community and district characteristics like the number of health centers and access to roads.

2.3.6 Descriptive statistics

Tables 2.1 reports the summary statistics for the study variables for the pooled sample. In Table 2.1, the average rates of BCG vaccination were 89% for districts that received JSY according to our definition and 84% for districts that did not, respectively. These uptake rates are similar to the estimated numbers reported by the WHO (World Health Organization, 2012).

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[8] Since these vaccines were administered before relevant ages, the data likely suffers from measurement errors from recall bias. The main results remain the same if these observations are included.

[9] Birth order is defined as follows: first child 1, second child 2, third child 3 and 4 for 4 or more than 4th child

Table 2.1: Summary Statistics Pooled Sample - IHDS wave I and II

	JSY District	Non-JSY District
Outcome Variables		
= 1 if BCG vaccine (at birth)	0.8898	0.8434
	[.3132]	[.3634]
= 1 if DPT vaccine (4-6 weeks from birth)	0.8750	0.8343
	[.3307]	[.3719]
= 1 if Measles vaccine (9 months from birth)	0.7396	0.7114
	[.4389]	[.4531]
Observations	8020	13941
Selected Control Variables		
= 1 if Boy	0.5502	0.5544
	[.4975]	[.497]
Child's Father's education	7.0298	7.1555
	[4.5692]	[4.7807]
Child's Mother's education	5.6507	5.3937
	[4.8314]	[4.9667]
=1 if child's Mother has a job	0.2431	0.2054
	[.429]	[.404]
=1 if Urban	0.2978	0.349
	[.4573]	[.4767]
= 1 if below poverty line	0.2899	0.2669
	[.4537]	[.4424]
Observations	8020	13941

Note: Standard deviations are in brackets. Parental education is measured in years of schooling.

The rates for DPT vaccine were slightly lower (about 1%). The rates for Measles are lower by more than fifteen percentage points in both types of the district. This is understandable because, as the table shows, the measles vaccine is administered at a later age requiring caregivers to bring the child to a clinic, a more arduous undertaking, particularly in rural areas. The tables also show that there are very few differences between the two groups.

2.4 Empirical Strategy

In this section, we describe each of the model specifications estimated. Our basic empirical strategy is to compare changes in our measures of vaccination in districts where JSY was introduced between the two waves at various time points, relative to those where it was not. For the pooled sample, we supplement our standard difference-in-differences models with an event study type model to test for dynamic policy effects.

2.4.1 Estimation

First, using our pooled cross-section sample from the two waves of IHDS, we run individual-level difference-in-differences regressions of each outcome on JSY availability (measured at the district-level). The primary regression estimated is

$$Y_{idt} = \alpha + \omega JSY_{dt} + X_{idt}\theta + Z_{idt}\beta + \delta_d + \lambda_t + \epsilon_{idt} \tag{1}$$

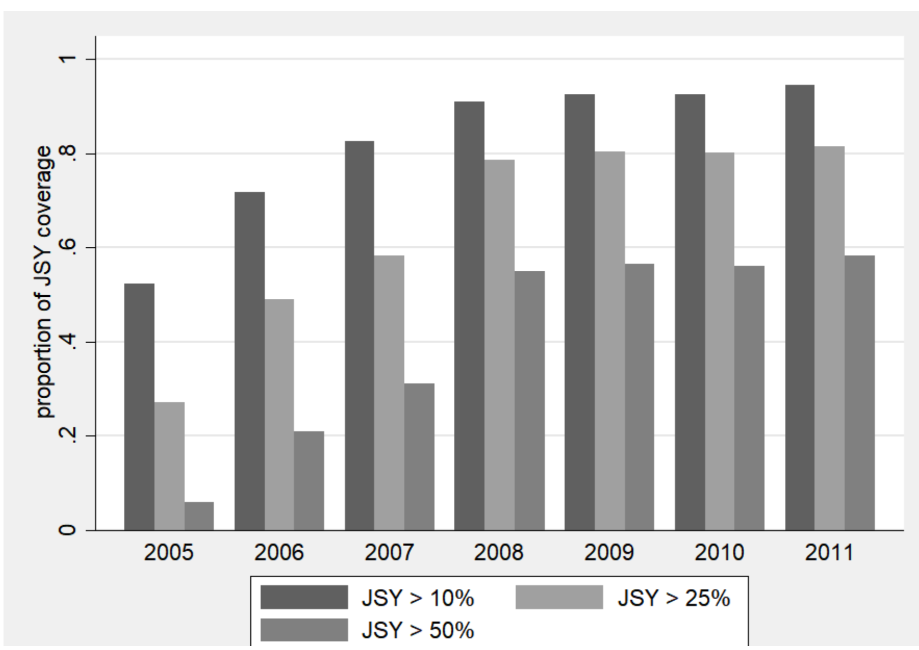
where Y is a binary outcome indicating vaccine outcome measures (BCG, DPT, and Measles) for individual i living in district d born in year t; JSY_{dt} is a binary variable indicating 1 if a district has JSY coverage (25% or more) in year t, and 0 otherwise. δ_d is a set of district dummies, which controls for district fixed effects, λ_t is a vector of year dummies. Our coefficient of interest is ω, which measures the effect of expanding JSY in a district on the outcome variable. Z is the matrix of additional (time-varying characteristics at communities (district level and village level (or sub-district)) in some specifications (such as health infrastructure). The matrix X includes all the individual- and household- level socio-economic variables discussed in the previous section. Finally, ϵ is the error term clustered at the district-level to address the non-independence of observations from the same district over time.

2.4.2 Common trend assumption

These equations are identified under the assumption that in the absence of treatment, the change in outcome between pre- and post-intervention periods for the treated is similar to the untreated districts. This is the well-known ”parallel trends” assumption of difference-in-differences (DID) models. The inability to control for systematic trends in a DID framework may bias the parameters measuring the program effects (Heckman and Vytlacil, 2007). The assumption is open to questions for several reasons in this context.

Officially, the program targeted some states as ‘high focus’ states that had high maternal mortality rates, a concern that has already been documented in the literature such as (Powell-Jackson et al., 2015). However, we can be confident in our strategy if these two groups of districts have similar trends in outcomes prior to the start of the program, making our estimates more credible. The argument proceeds in several steps. First, it is helpful to look at the timeline of the rate of JSY coverage expansion in India, illustrated in Figure 2.1.

Figure 2.1: Expansion of Janani Suraksha Yojana over time – District Level Health Survey



Note: Each bar represents proportion of districts in which at least x% of women reported receiving JSY benefits, x = 10, 25, 50.

The figure shows that JSY coverage varies by district and year after its introduction in 2005-06. Therefore, one has to choose a reference year to determine which districts received the 25% (or 50%) threshold of becoming a treated district. We choose 2008-09 as the the reference year because, as the graph illustrates, the expansion became stable after this period. Similar results are obtained if we use 2009-10 as the reference year.[10] Next, we can check if these two groups of districts have similar trends in outcomes before the start of the program, ameliorating the concerns that forces other than JSY were already in place to influence the outcome variables differently.

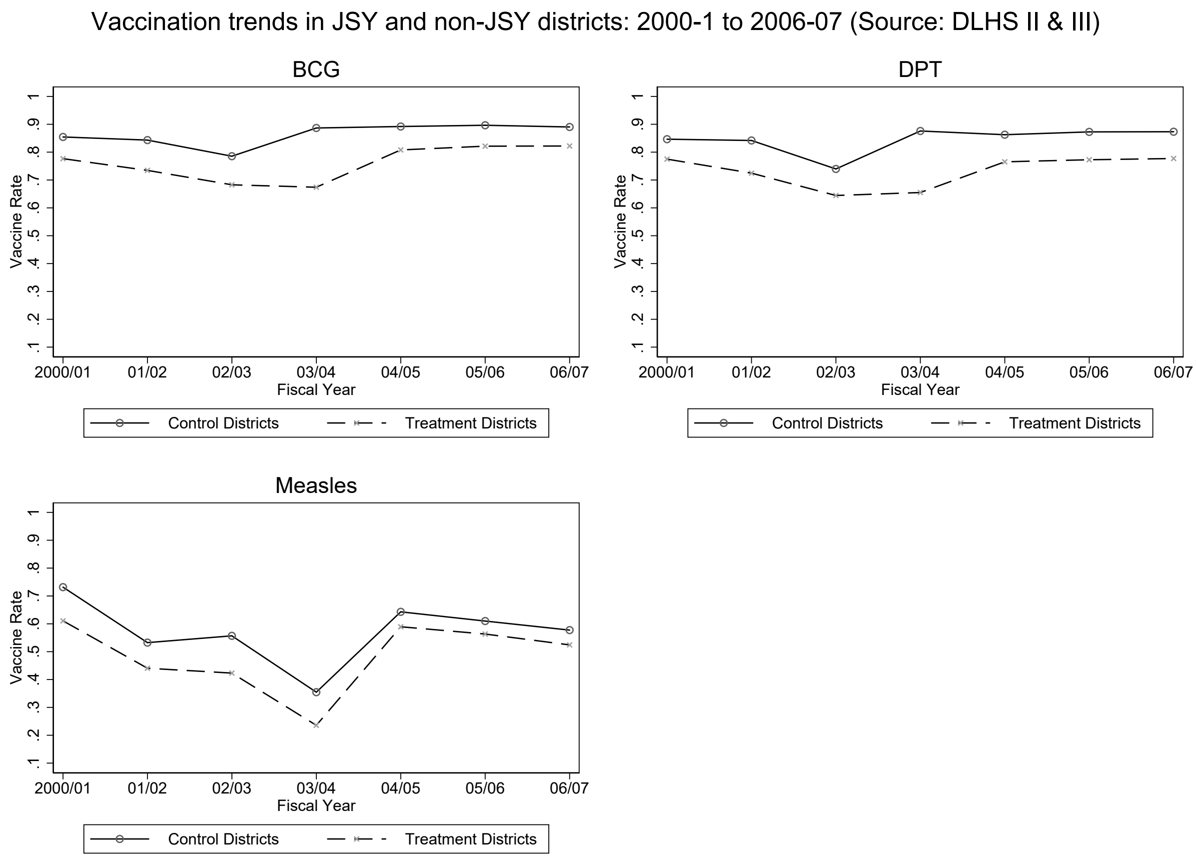
Figure 2.2 illustrates this point, which presents a visual representation of the incidence of vaccination in JSY and non-JSY districts before the implementation of JSY to see how such trends have unfolded over time. Since these graphs are critical to our identification strategy, it is important to explain their construction in detail. We have calculated these proportions in the following way. We start with DLHS2, which was conducted in 2002-2004. There, all mothers were asked about their previous two births, and we use the information on their last birth. The responses help us pin down the year of birth and corresponding vaccination information. Using this data, we create the vaccination rates by districts starting from 2000-2001 to 2003-04. Each vaccine was coded one if they answered yes to the question "Received BCG/DPT/Measles."

Similarly, we use DLHS3, which was conducted in 2007-2008 to create vaccine rates for the years 2004-05 to 2008-09. Each vaccine was coded one if there was a vaccination card with the date written plus if they answered yes to the question "received vaccinations against tuberculosis/DPT/Measles." Then we average over annual birth cohorts to calculate, say, the percentage of newborns who were administered BCG in 2005 in a particular district. We have combined the graphs for all the years from 2000-01 to 2006-07.[11]

[10] The figure is available upon request.

[11] Though the question for DLHS II and III were somewhat different with respect to the vaccine uptakes, the results from such differences, if any, would likely affect both the treatment and control group similarly.

Figure 2.2: Trends in immunization in treated and non-treated districts prior to the program implementation.



Notes: (Definition 1: Treat = 1 if 25% of the mothers reported cash receipt under the program in District Level Health Survey as of year 2008-09)

The figure shows that (i) the proportion of children who received various vaccines differed across JSY and non-JSY districts before the program, but (ii) such differences persisted over time until the roll-out of JSY. Specifically, the trends in all vaccines were similar in both treatment and control districts, with control districts consistently showing higher vaccination rates. This is expected and consistent with the policy aspect that JSY targeted low-achieving (high focus) states. A few more additional facts further strengthen our strategy. First, the JSY was a maternal health intervention program, and it was not meant to affect vaccination;

to our knowledge, no pre- or post-policy document focused on this angle. Therefore, from the immunization policy point of view, it was an exogenous shock with unintended consequences. Second, most Indian states are big and diverse. Even if a state is deemed high focus, it does not mean that all districts within that state would also be prioritized at the same time. Finally, we also use propensity score matching methods to create comparable treatment and control districts using data from the pre-intervention year as an additional robustness check. Finally, since it is difficult to pin down treated and control districts with respect to a single time point, we further do event study analysis to see if there is any difference between treated and control district in pre-treatment years.

2.4.3 Endogenous Births

One concern is that our method of sample selection can introduce selection bias through potentially endogenous fertility. We argue that there are several reasons why this may not be a concern. First, such endogeneity is a threat to our identification strategy only if some women choose to become pregnant, anticipating the rollout of the JSY. This is highly unlikely. Benefits from social programs in India are uncertain. Even a highly credible promise of a cash incentive several months in the future is likely to play a minor, if any, role in fertility decisions. We are also mindful of this problem while merging the JSY information. The district-level JSY concentration is merged with the appropriate date of birth.[12]

Nevertheless, we try to test this conjecture in our data. We cannot create a test of differential fertility with conventional measures like total fertility rate; such data are not readily available and often difficult to calculate in India (Guilmoto and Rajan, 2013; Haque et al., 2019). Therefore, we develop our test following the intuition of Hoynes et al. (2015b)by adopting the same difference-in-differences strategy to see if the treatment districts had

[12] It is true that this does not eliminate the possibility that some women became pregnant after the JSY was introduced to their districts. However, the timeline, where JSY rollout started in right earnest in 2009 and interviews for IHDS taking place in 2010-11 making number of such cases extremely small.

significantly higher birth than the control ones. If JSY incentivized fertility, this would be the case. Specifically, we calculate the total number of births per district-year, we estimate a model similar to (1) above at district and year level, where our dependent variable is the log of births.

Table 2.2: Difference-in-differences estimates of JSY on number or births and proportion of birth among poor families at the district level - wave I and wave II of Indian Human Development Survey

	Log number of births in district-year cell	Fraction of Birth among Poor families in district-year cell
JSY coverage 25%	0.039	-0.0274*
	(0.0308)	(0.014)
Observations	4,191	4,191

Notes: : Each column is a separate difference in differences regression using IHDS data on JSY treatment. All regressions are in district-year cell. Column 1 regresses log of total births by district (fiscal) year and Column 2 regresses fraction of births in households below poverty line by district year respectively. Regression includes year and district fixed effects. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.2 presents these estimates, where in the first column the dependent variable is district-year (log) birth rate. To check this potential effect is prevalent only among poorer households as the financial incentive per se is a small amount, we estimate the same model with the proportion of birth in each district-year for households below poverty line. The estimates show that there is no evidence of any positive and significant effect of the JSY on birth rates. In particular, the negative estimate in column (2) gives us confidence that empirical evidence supports our conjecture that JSY incentives may be too small to increase birth rates independently.

2.5 Results

2.5.1 Main Results

Table 2.3 presents our main results - the difference-in-differences estimates of the implementation of JSY on measures of vaccination for individual vaccines subject to the appropriate age. The first column refers to BCG, which is administered at birth, the second column refers to DPT, which is administered within two months of birth, and the third column refers to the measles vaccine, which is administered when a child is at least nine months old. The table shows two key results. First, JSY increased the likelihood of vaccination for all three vaccines positively and significantly. In terms of magnitude, belonging to a JSY district increased the probability of getting the BCG vaccine by almost 4.4 percentage points, of initiating the DPT vaccine by nearly 4.8 percentage points, and of being vaccinated against measles by roughly 2.5 percentage points. These results correspond to the pooled sample of children between 0-48 months of age from the India Human Development Survey.

Table 2.3: Effects of JSY on Child Immunization – Pooled sample from wave I and wave II of Indian Human Development Survey

	(1)	(2)	(3
	BCG	DPT	Measles
JSY coverage 25%	0.0444***	0.0477***	0.0247*
	(0.0102)	(0.0109)	(0.0148)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	21,961	21,150	18,465

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy, household below poverty dummy and dummy for mother who gave birth in both rounds of the survey. For DPT only children older than 2 months, during the time if interview is included. Similarly, for Measles, children older than 9 months are included.

Second, the magnitudes of point estimates are higher for BCG and DPT than those of the measles vaccine. These differences are anticipated and consistent with the mechanism through which maternal health intervention programs may affect infant health. We hypothesized earlier that the JSY program, by offering cash incentives to mothers to deliver in more formal settings may indirectly improve related but unintended behaviors like vaccination. If this is the case, and indeed our main results show that there is supporting empirical evidence, then the effect should be stronger for vaccines that are given at or very close to birth, which is tied to financial incentives. Though relationships with a doctor, health worker, or hospital may still increase the likelihood of bringing the child back for measles vaccine months later, the JSY-impact is likely to get weaker by then. The results confirm such a phenomenon.

These results are obtained after controlling for several potential confounding factors such as child gender and age, parental education, birth order dummies, if mother gave birth in both rounds, interview recall period, religion, household poverty status, urban location, as well as district and year fixed effects. The inclusion of district fixed effects allows us to control for time-invariant unobservable district characteristics, which may affect outcomes, while year fixed effects absorb the effects of unobservable yearly shocks that are common across districts. The estimated effects of other explanatory variables are as expected (please see Appendix Table 2.A3 for the complete version of Table 2.3). Male children are generally more likely to receive vaccines in general, while being poor reduces it. Both father and mother's education are strongly and positively associated with the probability of vaccination.[13]

2.5.2 Robustness Checks

The main result so far is that JSY significantly and positively increased the probability of infant vaccination for all three vaccines under consideration, both for our pooled and longitudinal sample. This section reports the results of several robustness checks of our main specifications.

Analysis incorporating village infrastructure variables

Apart from being longitudinal, the IHDS also has the useful feature of containing rich village-level infrastructure data. Even though we control for district fixed effects, time-varying village level controls are important to partial out additional confounding factors that may affect health outcomes like vaccination. While this approach adds additional controls, we have to exclude any villages for which such infrastructure data was not available. The trade-off is losing a significant sample size. Point estimates from equation (1) that includes

[13] Similar results are obtained from our balanced panel sample. The details can be found in Appendix section 2.73 along with the results reported in appendix tables 2.A8 and 2.A9 respectively.

village-level controls are presented in Table 2.4.

Table 2.4: Robustness tests I – Village health infrastructure controls - wave I and wave II of Indian Human Development Survey

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 25%	0.0447***	0.0429***	0.0129
	(0.014)	(0.0145)	(0.0182)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes
Observations	14,499	13,927	12,164

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy, household below poverty dummy and dummy for mother who gave birth in both rounds of the survey. For DPT only children older than 2 months, during the time if interview is included. Similarly, for Measles, children older than 9 months are included. Village controls include indicator variables for Roads, Public Health Center, Government Maternity Center, and Anganwadi Center.

The village controls include if the village has (i) a road, (ii) a government maternity center, (iii) a health center, and (iv) an Anganwadi (public health worker) center. Even with the inclusion of the village controls, the coefficients on JSY are positive, with similar magnitude, and significant for BCG and DPT indicating that the addition of such local infrastructure variables does not take away the contribution of the program. Section 2.72 in the appendix shows additional robustness checks, such as using alternative treatment status threshold and time varying district controls.

Analysis incorporating ASHA Workers

The JSY was largely a demand-side financing of health scheme that encouraged women to adopt certain health behavior, such as giving birth at a health facility. However, there was a second, less researched aspect of the program that involved some supply-side incentives. The National Rural Health Mission (NRHM) also introduced trained Accredited Social Health Activist (ASHA) workers in India from 2005-2006.[14] These workers were typically female, between 25 and 45 years of age and have schooling level of eighth grade and above (Gupta et al., 2012; Shrivastava and Shrivastava, 2012; Dongre and Kapur, 2013) . Their general mandate was to promote health awareness among women. They also received performance-based incentives for referrals and accompaniments to health care facilities for pregnant women. Therefore, the presence of these workers can potentially influence our outcome variables (Debnath, 2018).

[14] ASHA means hope in several Indian languages.

Table 2.5: Robustness tests II – Controlling for ASHA worker coverage - wave I and wave II of Indian Human Development Survey

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 25%	0.0417*** (0.0106)	0.0449*** (0.0114)	0.0272* (0.0162)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
ASHA Control	Yes	Yes	Yes
Observations	20,415	19,651	17,058

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy and household below poverty dummy. For DPT only children older than 2 months, during the time if interview is included. Similarly, for Measles, children older than 9 months are included. ASHA control refers to percentage of deliveries urged in health facilities by ASHA workers by district.

Table 2.5 reports coefficient estimates from our model from equation (1) controlling for ASHA worker coverage. This variable has been constructed the same way as the JSY variable is created. For each district, from the IHDS, we calculate the percentage of women who received services of an ASHA worker. As the coefficients show, the results remain qualitatively similar. However, when we compare the magnitudes with our main results, we see that they are slightly smaller in this case. This is unsurprising, as, given the ASHA workers' mandate, variation in their availability should explain variation in the vaccination uptake. Nevertheless, the demand-side cash-based incentives continue to remain a significant explanatory factor.

Analysis using DLHS Data

We provide additional evidence on the impact of JSY on vaccination using the second dataset that we have used for constructing the JSY treatment variable and pre-trend analysis. As discussed above, this data, the DLHS, contains both vaccination and JSY coverage information. The data has one key advantage, but several disadvantages. The advantage comes from its sample size, which is many times larger than the IHDS. Unfortunately, the data lacks several key control variables like poverty status, which makes the fitted models less complete. The data also does not allow us to construct any longitudinal sample. Nevertheless, it provides a natural robustness check to our main results, because we would be more confident in our estimates if the coefficient estimates are at least qualitatively similar.

Using this data, we follow a similar specification to that above, regressing vaccination uptake on JSY treatment. The controls include the infant's gender, mother's age, and urban location indicator. The results are presented in Table 2.6.

Table 2.6: Robustness tests III – Effect of JSY on Childhood Vaccination in DLHS Data

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 25%	0.0477***	0.0472***	0.0176**
	(0.00583)	(0.00802)	(0.00846)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	1,711,351	1,711,278	1,711,133

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Individual characteristics include child gender, mother's age, and urban location indicator.

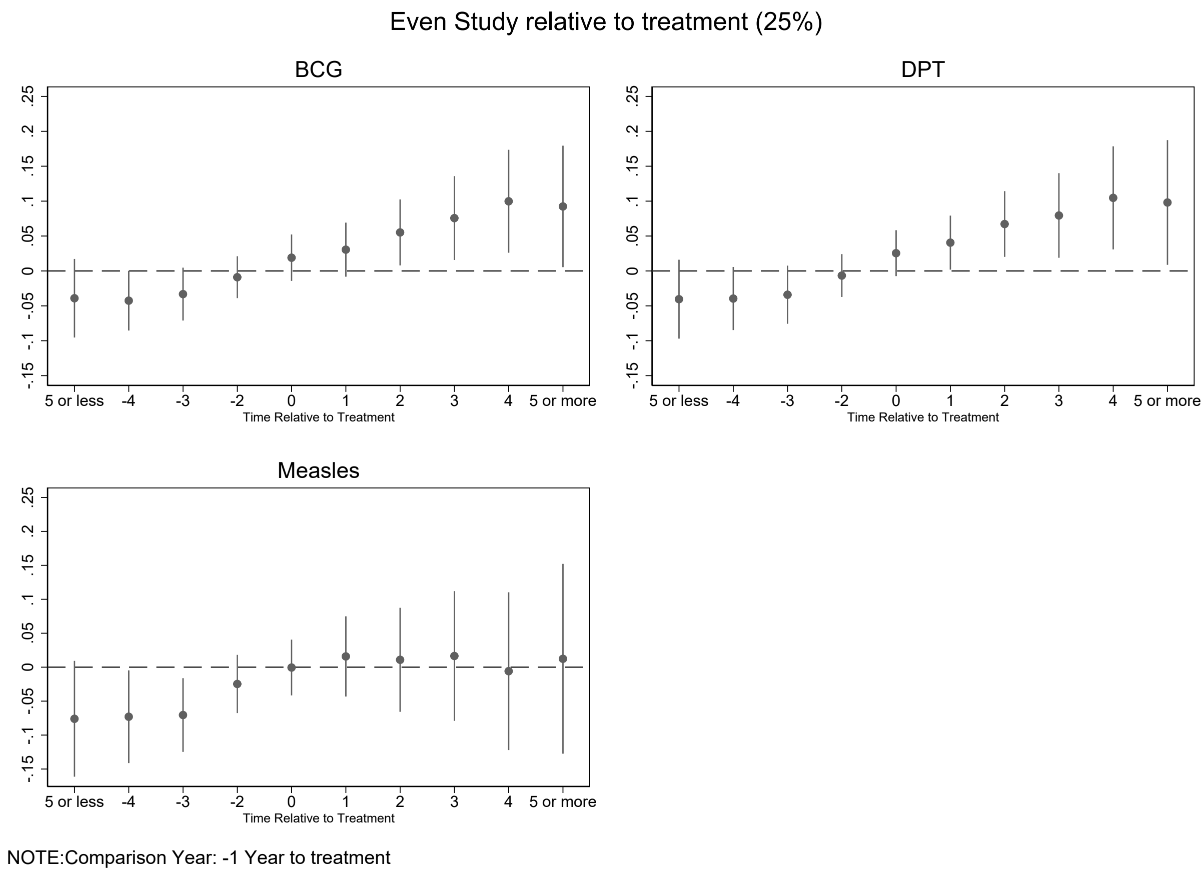
The coefficient estimates are both qualitatively and quantitatively, similar to those of Table 2.4. For BCG, living in a JSY district increases the likelihood of vaccination by roughly 5%, which is similar to DPT vaccination also. Finally, consistent with the IHDS data, we see a smaller effect on the measles vaccine.

2.5.3 Event study analysis

Our estimates so far have relied on the assumption that districts received the program exogenously, and the JSY rollout was not correlated with omitted variables. We have already tried to demonstrate that the trends in vaccination between treatment and control districts are parallel. Additionally, since the pooled sample spanned more than ten years, it allows us to perform an event study style analysis to test our assumption of exogeneity of the treatment. In this model, we create up to five years of lag and lead concerning the year in which the JSY had been implemented in a particular district. We study the effect of JSY five years before it was introduced in a district, during the first year of the implementation, and then five years after the program is in effect. To implement it, we replace the JSY coverage variable in the model by these lags and leads representing the number of periods relative to policy implementation. Next, we regress our vaccine variables on the year coefficient and other control variables used in equation 1 above. The coefficients are plotted in Fig. 2.3.[15]

[15] The estimates and standard errors are available upon request.

Figure 2.3: Initiation of three vaccines, event study coefficients.



Notes: (Definition 1: Treat = 1 if 25% of the mothers reported cash receipt under the program in District Level Health Survey as of year 2008-09. Each figure presents the estimated coefficients from the main regression model from the pooled using data from the IHDS and DLHS, where the outcome variable is receipt of BCG, first dose of DPT and Measles respectively. Spikes are 95% confidence intervals of the estimated coefficients.)

The point estimates for the JSY effects indicate that (i) the introduction of JSY led to increased rates of uptake for both BCG and DPT vaccines, but (ii) such effects were weaker, or of mixed nature for the measles vaccine. Therefore, the event study results also confirm the main findings that the JSY was more effective in inducing at-birth vaccination and less so for follow up ones.[16]

[16] One thing to note is that the apparent presence of pre-effects in pre-JSY years for all vaccine types. This could be due to fact that districts with 10% coverage could still be meaningful, and it is reflected in those

2.6 Discussion and Conclusion

Health policy has long focused on infants and children, who are at a high risk of diseases. Some of these diseases are preventable by vaccines in a cost-effective way. Vaccines not only boost individual immunity but also herd immunity – a process through which a population develops immunity from a disease in such a way that even people who cannot be vaccinated are also protected (Anderson and May, 1990; Fine, 1993). Unfortunately, vaccination rates have lagged in many societies across the world, and policymakers are trying to find ways to increase vaccination. The range of policies has varied from encouraging medical referrals to immunization campaigns to creating legal mandates. Since reproductive health policies focus on pregnant women, a relevant question is whether delivery in a safer environment may improve not only maternal health but also health outcomes like vaccination, which has long term positive health consequences.

In this paper, we have addressed that question by examining the effects of India's largest maternal health program, JSY, on childhood vaccination which were not directly incentivized under the program. Our empirical strategy uses the heterogeneity in the implementation of the program across districts and produces several clear results. First, the program increased vaccination rates for BCG and DPT, which are administered at or very close to birth. Second, the effect wanes or disappears for the measles vaccine, which is administered several months after birth. Third, the program effect for those two vaccines is robust. We have used two samples, one pooled and one longitudinal, and in both cases, the program effect remains positive and significant. The effects also remain so in various sub-samples and across groups.

Nevertheless, the study is not devoid of limitations. First, though IHDS is a village-level survey, it doesn't provide village-level identifiers so the analysis using district-level aggrega-

effects. We perform an event study analysis for 10% coverage rate. Figure 2.A1 in appendix shows that for 10% coverage the pre-implementation coefficients are zero. This is supported by the regression analysis as shown in Appendix Table 2.A1.

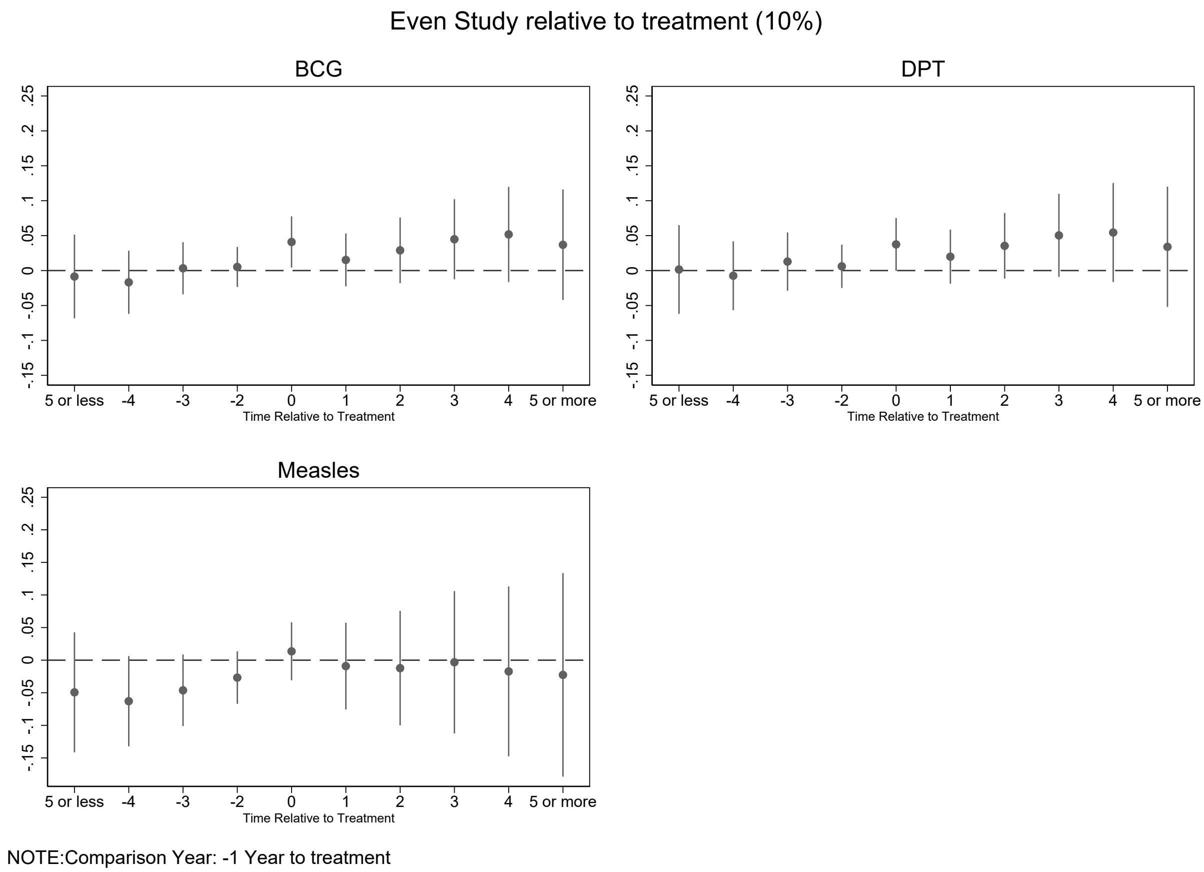
tion cannot control for the village-level variation precisely. Second, vaccination also depends on various technological factors like storage facilities that we have not controlled for (though there is no evidence that such factors interacted with the policy). It also depends on a variety of supply-side variables such as availability of vaccines. As Andrew and Vera-Hernández (2020) show, in supply-constrained areas, the JSY was not as effective. Future research has to explore these channels in greater details. Vaccination uptakes are also notoriously prone to measurement errors. Though the problem is less severe here for the reliability of the IHDS survey, it nevertheless limits certain analyses such as completion of all doses of DPT.

Despite these limitations, we believe our results are credible and contribute towards the health policy literature on vaccination, particularly in developing countries such as India (Banerjee et al., 2010; Das, 2010; De, 2017). Since the policy we are evaluating was mainly focused on specific maternal health behavior and outcomes like institutional delivery, the driving forces behind the policy were likely to be exogenous to the forces driving immunization policy, significantly ameliorating endogeneity concerns. Indeed, previous literature has demonstrated the exogeneity of the JSY rollout itself. A combination of two data sources also lowers the potential bias coming in from systematic measurement and survey errors. Together, the main result, that maternal health interventions can be successful in improving the uptake of some vaccines is robust and useful for global health policy.

2.7 Appendix

2.7.1 Figures and Tables

Figure 2.A1: Initiation of three vaccines, event study coefficients



Notes: ((Treat = 1 if 10% of the mothers reported cash receipt under the program in IHDS Survey. Each figure presents the estimated coefficients from the main regression model from the pooled using data from the IHDS and DLHS, where the outcome variable is receipt of BCG, first dose of DPT and Measles respectively. Spikes are 95% confidence intervals of the estimated coefficients.)

Table 2.A1: Alternative treatment status threshold – 10% JSY Coverage - wave I and wave II of Indian Human Development Survey

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 10%	0.0171	0.0196	0.0311*
	(0.0114)	(0.0120)	(0.0163)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	21,961	21,150	18,465

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy, household below poverty dummy and dummy for mother who gave birth in both rounds of the survey. For DPT only kids older than 2 months, during the time if interview is included. Similarly, for Measles, kids older than 9 months are included.

Table 2.A2: Effects of JSY on Polio Immunization – Pooled and Panel sample from wave I and wave II of Indian Human Development Survey

	(1) Polio Pooled Sample	(2) Polio Panel Sample
JSY coverage 25%	0.00856	0.0398
	(0.0129)	(0.0262)
District FE	Yes	Yes
Year FE	Yes	No
Controls	Yes	Yes
Observations	21,961	6,230

Notes: Polio immunization outcomes are binary variables set to one if the mother has answered YES to Polio immunization question, and zero otherwise. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. JSY treatment status is defined at the district level. Column 1 shows for pooled sample and controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy, household below poverty dummy and dummy for mother who gave birth in both rounds of the survey. Column 2 shows results for the panel sample. In this model, the controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, parity dummy [parity is defined as, 1 if number of children is 1, 2 if 2 and 3 if 3 and 4 if greater than 4], urban dummy, religion dummy and household below poverty dummy.

Table 2.A3: Complete version of Table 2.3

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 25%	0.0444***	0.0477***	0.0247*
	(0.0102)	(0.0109)	(0.0148)
Child AGE	0.00121	0.00728*	0.0355***
	(0.00382)	(0.00415)	(0.00515)
Interview recall period	-0.00124	-0.00284***	-0.00312***
	(0.000823)	(0.000892)	(0.00112)
If child is a boy =1	0.0109**	0.00655	0.0200***
	(0.00440)	(0.00448)	(0.00584)
Poor == 1	-0.0190**	-0.0148*	-0.0222**
	(0.00740)	(0.00779)	(0.00861)
Mother Age	-0.000481	-0.000159	0.000331
	(0.000616)	(0.000710)	(0.000793)
Child's Mother's education	0.00502***	0.00530***	0.0112***
	(0.000757)	(0.000883)	(0.00102)
Child's Father's education	0.00520***	0.00597***	0.00645***
	(0.000784)	(0.000871)	(0.00100)
Urban residence from census 2011	0.0120	0.0198	0.0309**
	(0.0110)	(0.0127)	(0.0153)
Mother in both round = 1	-0.0115**	-0.0142**	-0.00516
	(0.00572)	(0.00607)	(0.00754)
Mother has a job = 1	-0.00811	-0.00847	-0.00361
	(0.00646)	(0.00690)	(0.00981)
Parity 2 =1	-0.00204	-0.00652	-0.00537
	(0.00563)	(0.00600)	(0.00821)
Parity 3 =1	0.00707	0.00496	0.00491
	(0.00710)	(0.00742)	(0.00999)
Parity 4 =1	-0.0388***	-0.0527***	-0.0369***
	(0.00910)	(0.0104)	(0.0118)
Religion			
Hindu (reference)			
Muslim	-0.0432***	-0.0368***	-0.0664***
	(0.0125)	(0.0136)	(0.0155)
Other	-0.0159	-0.00868	0.0120
	(0.0139)	(0.0135)	(0.0179)
Observations	21,961	21,150	18,465
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level.

2.7.2 Additional Robustness Checks

We consider additional robustness checks of our main results with tables and figures below. First we show that, the JSY increased the incidence of institutional delivery. In Table 2.A4 we show that the percentage of mothers giving births in government facilities increased by 7-10%, which is expected. Next, we examine whether our results are sensitive to our choice of 25% threshold by re-classifying districts to be a JSY- district if 50% of the qualifying women have reported having received the aforementioned financial incentive. Previous literature such as Powell-Jackson et al. (2015) has also chosen these thresholds. Table 2.A5 reports these results. The most noteworthy aspect of the point estimates is that the magnitudes of the effects of JSY are higher for all three vaccines. Additionally, the effect is significant for vaccination against measles. This is unsurprising and bolsters the claim that JSY was instrumental in increasing vaccination uptakes – children in districts with a stronger JSY presence were more likely to be vaccinated.

Although we control for time-varying village controls in a previous specification (section 5.3.1,) it came at the drawbacks of losing sample size. As an additional check, we include time-varying district-level control variables such as road and health infrastructures to our baseline specification and examine if the results change. Specifically, we use the DLHS II and III data to calculate distance to nearest bus stop, distance to district headquarters, percentage of villages connected by roads, percentage of villages with colleges, percentage of villages with government hospitals, private clinics, private hospitals, sub-centers, primary health centers (PHC), community health centers (CHC) and government dispensaries centers. These variables are calculated from the DLHS and then merged with the individual-level sample from the IHDS data. Appendix Table 2.A6 shows the results, and estimates are similar when we control for time-varying district characteristics.

To construct our JSY treatment variable beyond 2007-08 (DLHS III data), we had to combine the Annual Health Survey with the District Level Health Survey because they were split administratively, as subsections 3.1 and 3.2 explain. One concern may be that our results are influenced by such construction. Table 2.A7 restricts our sample to DLHS II and III making it directly comparable to the previous literature. The results are similar qualitatively but are smaller in magnitude, perhaps explaining that in time JSY was influential in driving vaccination uptake. Finally, for our panel sample, we compute propensity score-adjusted estimates as an additional robustness check. The results are similar. In all the appendix tables, further details are provided in table notes.

Table 2.A4: Increase in institutional delivery as a result of JSY - Probability of giving births in government facilities - wave I and wave II of Indian Human Development Survey

	(1) Births at Government Facilities	(2)
	Pooled Sample	Panel Sample
JSY coverage 25%	0.061***	0.097***
	(0.015)	(0.025)
District FE	Yes	Yes
Year FE	Yes	No
Mother FE	No	Yes
Controls	Yes	Yes
Observations	22045	6280

Notes: The outcome is a binary variable set to one if the mother gave birth in government facilities. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. JSY treatment status is defined at the district level. . Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy, household below poverty dummy and dummy for mother who gave birth in both rounds of the survey. For DPT only kids older than 2 months, during the time if interview is included. Similarly, for Measles, kids older than 9 months are included.

Table 2.A5: Alternative treatment status threshold – 50% JSY Coverage - wave I and wave II of Indian Human Development Survey

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 50%	0.0746***	0.0727***	0.0495***
	(0.0132)	(0.0148)	(0.0185)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	21,961	21,150	18,465

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy, household below poverty dummy and dummy for mother who gave birth in both rounds of the survey. For DPT only kids older than 2 months, during the time if interview is included. Similarly, for Measles, kids older than 9 months are included.

Table 2.A6: Time-varying District controls - wave I and wave II of Indian Human Development Survey and three rounds of District Level Health Survey Data

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 25%	0.0417***	0.0454***	0.0167
	(0.0109)	(0.0116)	(0.0150)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	21,175	20,388	17,787

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy and household below poverty dummy. District-level control variables were obtained from various rounds of the District Level Health Survey data. Specifically, for each village in respective district, we calculate the following variables and take mean by the district: Distance to nearest bus stand (in km), distance to district headquarter (in km), if roads connect village, if village has a college, a government hospital, private clinic, private hospitals, sub-center, public health center, community health center, government dispensary, and mobile health child clinic.

2.7.3 Balanced Panel

Effects of JSY on childhood vaccination – Panel Sample

Additionally, taking advantage of the longitudinal nature of the IHDS data we estimate the effects of JSY on vaccinations in our balanced panel sample. The equation (1) above differs slightly while estimating for the panel sample and takes the following form:

$$Y_{idt} = \alpha + \omega JSY_{dt} + \pi POST + \beta(POST * JSY_{dt}) + X_{idt}\theta + M_i + \epsilon_{idt} \tag{2}$$

where POST is an indicator variable for wave II and Mi captures the mother fixed effect.

Table 2.A7: Re-estimation of Table 4 when the treatment variable has been constructed by using only DLHS II and III. All other variables are from wave I and wave II of Indian Human Development Survey

	(1) BCG	(2) DPT	(3) Measles
DID	0.0404**	0.0461**	0.0154
	(0.0172)	(0.0181)	(0.0229)
Observations	13,944	13,525	12,201
R-squared	0.249	0.263	0.286

Notes: This table follows the same model as Table 3. Please refer to those notes also. As mentioned in the text, DLHS IV survey is carried out only for low focus States. Another survey called the Annual Health Survey substituted it in the high focus states. Given that these surveys might have different structure and timing, our JSY variable could be noisy. Thus, we only use DLHS II and III to construct the JSY coverage variable as in Powel-Jackson et al. (2015). The data stops at fiscal year 2007-08. The significant pattern is similar to our main results, though the magnitudes of coefficients are smaller. This is to be expected because the impact of JSY increased with along with its coverage captured by additional years of data.

The key parameter of interest in Equations (2) is β.

Table 2.A8 shows the summary statistics for the panel sample (similar to Table 2.1 above). Since this is a balanced panel, they offer additional insights. For all three vaccines, the rates were much lower for non-JSY districts in pre-JSY period. Additionally, these districts had lower levels of parental education and lower incidence of poverty. This is not surprising, as the program targeted, poorer, rural states – so called high focus states. However, the vaccination gap closed considerably for all three vaccines following the program expansion, even though the poverty gap persisted, or even widened slightly. This is a prima facie evidence that the program was effective in increasing vaccination in backward areas.

Table 2.A8: Summary Statistics at baseline in 2004 (wave I) – Panel Sample - IHDS wave I and II

	Before JSY		After JSY	
	JSY District	Non-JSY District	JSY District	Non-JSY District
Outcome Variables				
= 1 if BCG vaccine (at birth)	0.7433	0.8253	0.8608	0.849
	[.4369]	[.3799]	[.3463]	[.3582]
= 1 if DPT vaccine (2 months from birth)	0.7324	0.818	0.8353	0.8357
	[.4428]	[.386]	[.371]	[.3707]
= 1 if Measles vaccine (9 months from birth)	0.5488	0.6688	0.7138	0.783
	[.4978]	[.4709]	[.4521]	[.4124]
Observations	1839	1265	1839	1265
Selected Control Variables				
= 1 if Boy	0.4356	0.4838	0.5802	0.6032
	[.496]	[.4999]	[.4937]	[.4894]
Child's Father's education	5.82	7.1391	5.8624	7.0474
	[4.743]	[4.6299]	[4.6431]	[4.6393]
Child's Mother's education	3.6308	5.3138	3.8668	5.4593
	[4.592]	[4.878]	[4.5516]	[4.6751]
=1 if child's Mother has a job	0.2344	0.1423	0.3502	0.2854
	[.4237]	[.3495]	[.4772]	[.4518]
=1 if Urban	0.2561	0.3099	0.267	0.3257
	[.4366]	[.4626]	[.4425]	[.4688]
= 1 if below poverty line	0.3622	0.2482	0.3893	0.2474
	[.4808]	[.4322]	[.4877]	[.4317]
Observations	1839	1265	1839	1265

Note: Standard deviations are in brackets. Parental education is measured in years of schooling.

The estimated effects of the JSY program for the balanced panel sample of mothers are given in Table 2.A9. This table is organized in the same way as Table 2.3.

Table 2.A9: Effects of JSY on Child Immunization –Panel sample from wave I and wave II of Indian Human Development Survey

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 25%	0.0635***	0.0455**	0.0205
	(0.0205)	(0.0211)	(0.0295)
District FE	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	6,208	5,714	4,430

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy and household below poverty dummy. For DPT only kids older than 2 months, during the time if interview is included. Similarly, for Measles, kids older than 9 months are included.

Though the pattern of sign and significance is the same as those corresponding to the pooled sample, the magnitudes are somewhat different. For example, for the same mother who gave birth in a JSY district during wave II, the probability of her child getting the BCG vaccine increases by almost six (compared to four for our pooled sample) percentage points. The magnitudes of effects for the DPT are similar, and Measles is not significant. We also present results in the appendix from the specification where the treatment and control groups are matched by propensity scores to deal with differences in observable characteristics (Table 2.A10).

Table 2.A10: Propensity Score Adjusted estimates for the Panel Sample − wave I and wave II of Indian Human Development Survey

	(1) BCG	(2) DPT	(3) Measles
JSY coverage 25%	0.0586***	0.0659***	0.0455*
	(0.0224)	(0.0255)	(0.0267)
District FE	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	7,180	6,818	5,762

Notes: All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy and household below poverty dummy. For DPT only kids older than 2 months, during the time if interview is included. Similarly, for Measles, kids older than 9 months are included.

Taken together, our main results (Table 2.3), results using DLHS data (Table 2.6) and results from panel sample (Table 2.A9) show that, JSY has an positive effect on increasing immunization rate (especially, BCG and DPT) across India.

2.7.4 Heterogeneity

Finally, we explore wheher there is heterogeneity in the effect of JSY for different sub-populations. Specifically, we examine if the effects differed differed across the lines of gender and urbanity respectively. Tables 2.A11 and 2.A12 show the results by gender and by urban level respectively. India has a long, unfortunate tradition of discriminating against girls in the fields of health and education. Therefore, it is worth inquiring if the positive effects of the health programs differed by gender. The gender sub-sample results are reported in Table A11.

Table 2.A11: Heterogeneity in childhood vaccination by gender − wave I and wave II of Indian Human Development Survey

	(1) Boy	(2) Girl	(3) Boy	(4) Girl	(5) Boy	(6) Girl
	BCG		DPT		Measles	
JSY coverage 25%	0.0415***	0.0465***	0.0395***	0.0563***	0.00499	0.0523**
	(0.0121)	(0.0140)	(0.0129)	(0.0146)	(0.0170)	(0.0207)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,142	9,819	11,727	9,423	10,354	8,111

All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy and household below poverty dummy. For DPT only kids older than 2 months, during the time if interview is included. Similarly, for Measles, kids older than 9 months are included.

The results show that the effect of JSY has been stronger for girls. This may mean that in the absence of the program, more care would have been taken to vaccinate the boys anyway; however, the program had an impact on the mothers to vaccinate the girls. These results are also remarkable against the backdrop of the vast literature on child preference in India. The rate and problem of preference of a male child are well documented in the literature and show that girls with two or more elder sisters in the family are more likely to be stunted and less likely to be fully immunized (Pande et al., 2006). Additionally, girls are not only less likely to be immunized; they also tend to drop out at a faster rate than boys for doses of DPT and Polio. Similarly, girls are likely to be immunized at a later age than boys (Pande and Yazbeck, 2003). Given this problem, our results suggest that JSY had significantly improved the vaccination rate, specifically among girls.

Next, we examine the differences across rural and urban regions. These regions are defined in the IHDS, which in turn used categorization from the Indian census of 2000. One distinctive feature of India's growth is that the rural-urban dichotomy is persistent, as agriculture's shares in both GDP and total employment have not declined rapidly (Kotwal et al., 2011). Table A12 disaggregates the impact of JSY on vaccination by urban location. The urban level sub samples show that the positive effect of the JSY on vacination wa not limited to a particular rural or urban area. The coefficient estimates are statistically significant for the rural sample only, showing that the program might have been more effective in rural areas.

Table 2.A12: Heterogeneity in childhood by rural-urban divide − wave I and wave II of Indian Human Development Survey

	(1) Urban	(2) Rural	(3) Urban	(4) Rural	(5) Urban	(6) Rural
	BCG		DPT		Measles	
JSY coverage 25%	0.0444***	0.0433***	0.0431***	0.0518***	0.0117	0.0438**
	(0.0137)	(0.0137)	(0.0144)	(0.0142)	(0.0182)	(0.0199)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,708	7,253	14,129	7,021	12,345	6,120

All three immunization outcomes are binary variables set to one if the mother has answered YES to that immunization question. All regressions are estimated with ordinary least squares model. Standard errors in parenthesis are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1. JSY treatment status is defined at the district level. Controls include, child age in years, mother age, mother education, mother employment dummy, father education, interview recall period in months, survey wave dummy, birth order dummies, urban dummy, religion dummy and household below poverty dummy. For DPT only kids older than 2 months, during the time if interview is included. Similarly, for Measles, kids older than 9 months are included. Urban location information comes from IHDS, which in turn uses the Indian census classification.

Chapter 3

Political Connections and Household Welfare: Evidence from India

"Raul's mother, Asha, a kindergarden teacher with mysterious connection to local politicians and the police, had managed to secure him several nights of temp work at the Intercontinental hotel, across the sewage lake, Rahul [a boy from the slum] . . . had seen the overcity opulence firsthand."
Katherine Boo: Behind the beautiful forevers[1]

3.1 Introduction

A large and growing body of literature has shown the role played by community and political based networks in supporting and gaining private and public economic activity.[2] When social capital and political connections play important roles in economic gains, being in a powerful political position or knowing a politician matters. Such political capital can be converted into economic capital by increasing networks, which would add a lot of information related to job opportunities and other economic opportunities. A household with political connection would be less vulnerable and more competitive (Chen, 1999). Political connection provides

[1] Boo (2011).

[2] e.g., Townsend (1994), Grimard (1997), Fafchamps and Lund (2003), Mazzocco and Saini (2012), Angelucci et al. (2017).

easier access to profitable economic opportunities (eg. government jobs, off farm employment and private sector employment). Political elites especially in rural places in developing countries still exert control over valuable economic resources. For example, family members can be assigned to high-salaried posts and their connections can benefit the family's private business (Walder, 2002; Morduch and Sicular, 2000).

When it comes to political rent seeking, India is no different. Patronage is deeply rooted in India. Xu (2017) finds that in 1854 governors connected to the Secretary of State enjoyed higher salaries through the promotion to higher paid positions and larger colonies. In addition to patronage, one common feature of low-income democracies such as India is political corruption. Various studies have shown the prevalence of elite capture of public goods (Duflo et al., 2008; Pande, 2008; Panda, 2015) and bribery (Bertrand et al., 2007) in India. When there is little oversight and benefits come at very little cost for the recipients, welfare programs and jobs (especially government jobs) are often found to be captured by wealthy and politically well-connected households (Gaiha, 2000; Kaushik, 1991).

Using the India Human Development Survey (IHDS) data (collected over two waves 2005 and 2011),[3] I investigate whether politically connected households are better off in terms of access to welfare programs and jobs opportunities for their members. While I use a rich dataset to control for various factors, knowing a politician may depend on unobservable characteristics of a household such as household head's ability. To rule out potential endogeneity, I instrument political connection with percentage of household within each village that are member of Self-Help Groups (SHGs). I propose that being a member of SHGs, a traditional micro money lending scheme in India increases the probability of "knowing" a politician. I construct the probability of SHGs being available in the village/town by calculating the percentages of households in a village/town that say they are member of SHGs. Results

[3]The data come from the two waves the India Human Development Survey (IHDS), which is a quasi-nationally representative multi-topic survey (Desai and Vanneman, 2012).

show that knowing a politician in local level vastly improves the chances of a household to obtain welfare programs like ration cards and insurance cards. Similarly, household members are more likely to have government jobs, and are less likely to work as daily wage laborers.

I further look to see if knowing the different types and levels of politicians differs in gaining vital welfare schemes made for the poor. India is a federal republic with three layers of government: local, state, and federal levels. The IHDS questionnaire asks a couple of questions on a household's acquaintance about politicians. First, it asks if a household knows a politician at the local level (village panchayat) and second if they know someone who is elected member of state and federal parliament. Finally, it asks if they know someone who is a political party official. Using these variables, I create three levels of political connection: If a household knows someone who is an official/member of village/town, if a household knows someone who is an elected member of the state or federal parliament, and finally if the household knows know someone who is a political party official. In 1989, the Indian central government took steps to add power to local government via direct funding to strengthen local development. Given this mixed federal, state, and local level power structure, knowing different types of politicians may yield different benefits.

I find that knowing a local level politician (panchayat officials) increases the probability of obtaining any ration cards by 12%, below poverty level (BPL) card by 9% Antyodaya card (card for poorest of the poor) (17%), MGNEGRA card (guaranteed part-time job) (20%) and RSBY (insurance) card (12%). Knowing elected politicians does not have any effect on obtaining welfare benefits. This is likely due to the fact that designation of such welfare program (to qualify for such programs) is administrated at local levels. Similarly, knowing a politician (elected in state and federal parliament) decreases the probability of at least one household member working in agriculture and daily labor jobs. Finally knowing elected politicians and party officials increases the chance of at least one member of the household having a government and permanent jobs, especially at least one female member. Knowing a local level politicians has no effect.

This paper contributes to a growing body of literature in political elite capture. Panda (2015) finds that a household that knows a politician at the local level (somebody close or a household member who is an official at village panchayat) is 15% more likely to receive a BPL card. That paper uses the first wave of IHDS data. However, this essay differs from Panda (2015) in few different ways. First, it uses the second wave of the IHDS data collected 6 years after the first, and second, it uses a different and novel instrumental variable to correct for the endogeneity of political connection. Finally, it looks at all kinds of welfare programs (not just likelyhood of receiving BPL cards) and also looks at a household's job prospects. Given that India spends billions of dollars on targeted welfare programs like ration cards for its poorest citizens, if such programs are misused for political gain then the objectives of welfare policy are not achieved.

The rest of the paper is structured as follows. Section 2 briefs about background, Section 3 describes the data and variables, Section 4 goes over methodology and identification strategy, Section 5 presents results and Section 6 performs robustness check. Finally Section 7 discusses and Section 8 concludes.

3.2 Background

The power of political connection and its impact on firms, household and individual welfare is still common. Using data from Italy, Gagliarducci and Manacorda (2016) find that "the monetary return to having a politician in the family is around 3.5 percent worth of private sector earnings and that each politician is able to extract rents for his family worth between one fourth and one full private sector job per year." Fafchamps and Labonne (2017) find that in the Philippines families connected to politician with current office holders are likely to be employed in better paying occupation. Individuals who are connected to such politicians are 22 percent more likely to get the job relative to the control mean. Similarly, Zhang et al. (2016) find that family member with a political background is more likely to engage in entrepreneurship. Finally, Folke et al. (2017) find that earnings for children of newly appointed mayors in Sweden rise by about 15%.

Throughout the known human history, either in tribal villages or in royal courts, one of the dominant roles in appointment of bureaucratic position has been patronage. Kings and Chiefs dominated most things and appointed whoever they pleased and often they would be their family members or friends (Sundell, 2014; Xu, 2017). However, starting with the imperial China and 19^{th} century Prussia, the way of recruitment has changed (Weber, 1922). We have education and skills qualification often measured through exams and interviews, which determines whether one gets the job or not (Sundell, 2014). While the level of bureaucratic and private sector patronage is on the decline especially in well-advanced economies, political rent seeking still exists.

How much advantage one inherits from their families' wealth and status directly impacts the opportunity that society provides to them (Emran and Su, 2015). Extreme level of inequality may pass across generations and limit opportunity for individuals which could undermine fairer competition and upward mobility. While some researchers claim that "almost all the earnings advantages or disadvantages of ancestors are wiped out in three generations" (Becker and Tomes, 1986), looking at the data from Italy, Barone and Mocetti (2016), claim that persistence of socioeconomic status across generations lasts for more than six centuries.

Panda (2015) looks at the chances of obtaining an important ration card, the so-called Below Poverty Level (BPL) card, if a household knows a politician. The paper finds that knowing a politician increases the chance of obtaining BPL card by as much as 15%. Mitra et al. (2017) observe the spike in household consumption (red meat and clothes) during election times in India to compare vote buying and an increase in spending during election times.

3.2.1 India's Political Structure

India is a federal republic with 29 states and 6 union territories. There are three distinct types of governing bodies, federal, state and local. There are a total 257,000 local government bodies of which 252,249 are rural and 4,751 urbans. Of the rural local government, 608 are zila parishad at the district level, 6,614 are panchayat samaiti at the block level, and 245,027 gram panchayat at the village level (CLGF, 2019). The definition and governing principle for local government are different from state to state. While federal government provides oversight for these governing bodies, states have considerable power. Each state has its own MPs, and Chief Ministers. And at the federal level is a parliamentary democracy.

"In 1989 the government of India took steps to enhance the role of rural local governments through direct funding. The aim was to create units responsible for economic development at the local level, and to create jobs directly" (CLGF, 2019). Given this mixed federal, state and local level power structure, knowing different types of politician may yield different benefits. Exploiting the rich survey dataset that asks question about household's acquaintance with various level of politicians, I investigate if knowing different kinds of politician improves household's well-being.

3.2.2 Public Welfare Programs in India

A popular welfare program run by the Indian government, known as the public distribution system (PDS), attempts to provide basic food supplies like rice, wheat, etc. at a subsidized rate. The system has provided food security for millions of poor across urban and rural areas. There have been three types of PDS cards or ration cards distributed to households across the country: the BPL card, the Antyodaya card (poorest of the poor), and APL (above-poverty-line) card (Panda, 2015). Under these schemes, households are allocated a certain number of grains per month. For example, BPL households could get a quota of 35kg of food grains per month. The central government allocates the number of PDS cards per state and the state government uses these limits to come up with cut-off rates for households to qualify for the PDS. The local government is responsible for identifying the poor households by assigning poverty scorecards for each household which will be used to assign PDS cards for households. Households that fall below their respective state- and district-specific cut-offs are classified as being below the poverty line and are issued BPL cards (Panda, 2015). Given the complex layers of identifying the household that qualifies for BPL cards, mismanagement and corruption is often rampant in India (Saxena, 2009).

The government of India has also expanded the targeted welfare programs into health and guaranteed jobs programs. The National Rural Employment Guarantee Act, 2005 created a program called Mahatma Gandhi National Rural Employment Guarantee Scheme (MGN-REGA), or sometimes called Mahatma Gandhi Employment Guarantee Act (NREGA) which guarantees up to 100 days of unskilled manual labor per year for qualifying households. Once again local political officials are given a central role in planning and implementation (Ravallion et al., 2013). Next, in 2008, India's Labor Ministry launched a hospital insurance scheme called Rashtriya Swasthya Bima Yojana (RSBY), which covered households that are below the poverty level. RSBY provides affordable and accessible healthcare services along with insurance coverage for secondary care (Malhi et al., 2020). Finally, in 2005, the government of India, with an aim to reduce maternal and child health, introduce a maternal health program that gave money to incentivize women to give birth in a health facility. In this study, I evaluate if being connected to a politician increases the probability of obtaining these vital services aimed at the poor in India.

3.3 Data and Variables

The data comes from the India Human Development Survey (IHDS), which is a quasi-nationally representative multi-topic survey (Desai and Vanneman, 2012). The survey is carried out in two rounds. Wave I interviews were conducted from late 2004 to late 2005 and wave II was from January 2011 to May 2013. Wave II interviewed 42,152 households across India.[4] I use the second-round survey for the primary analysis in this study. The data is publicly available to download online.

3.3.1 Variables

Using the rich IHDS data, I create three levels of political connection variables. The first level of connection is at the village/municipality level. The question in the survey is: *Is anyone in the household a member/official of the village panchayat / nagarpalika / ward committee? IF NO: Is there someone close to the household, who is a member?* This question is mostly about knowing the village and municipal level leaders. These leaders mostly are responsible for distributing welfare funds like ration cards. I create three variables, someone in the household, someone outside of the household, and the final one either of the two.

The second question is about higher-up politicians at the state and federal level, which is divided into two parts, elected officials and political party officials. *Do you or any members of your household have personal acquaintance with someone who works in any of the following occupations) i) Among your relatives/caste/community and ii) outside the community/caste? Such as MP/MLA, Zilla parishad member (excluding village panchayat).*[5]

I use the above questions to create three variables: Village Panchayat member/Official, a binary variable equal to 1 if a household has members in their household or outside and zero otherwise; Acquaintance with an elected politician, a binary variable equal to 1 if a household

[4] For more see: https://www.icpsr.umich.edu/icpsrweb/content/DSDR/idhs-II-data-guide.html

[5] These are verbatim questions from the questionnaire.

knows anyone either among their caste/relatives or outside and finally Acquaintance with a political party officials, a binary variable equal to 1 if a household knows any political party officials either among their caste/relatives or outside and 0 otherwise. Table 3.1a below shows the descriptive statistics for political connection variables.

Table 3.1a: Summary Statistics - Political Connection

	N	Mean
Village Panchayat Member/Official		
In Household	41496	4.09%
Someone Close	41494	23.59%
Both	41496	27.68%
Acquaintance with Elected Politician		
Among Relatives/community /Caste	41523	6.60%
Outside community /Caste	41523	13.25%
Both	41523	16.15%
Acquaintance with Political Party Officials		
Among Relatives/Caste	41523	7.62%
Outside community /Caste	41539	12.73%
Both	41549	16.25%

Notes: Some households have missing values

In the table we can see that around 4% of households have a village panchayat member in the household and 24% knows someone outside of household and overall, 28% of households know someone in the village panchayat. Similarly, 7% of households have acquaintance with elected politician either among relatives within their community and caste, 13% outside of community and caste and 16% of households have acquaintance with either. Finally, 8% of households have acquaintance with political party officials either among relatives within their community and caste, 13% have acquaintance with someone outside of community and caste and 15% of households have acquaintance with either.

To see the effect of elite capture among households, I look at various welfare measure that a household can claim: mainly ration cards (if household has any types of ration card), BPL card or below poverty line card and Antyodaya card (a ration card for poorest of the Similarly, I look if a household has a Mahatma Gandhi National Rural Employment Guarantee Act or MGNREGA card, Rashtriya Swasthya Bima Yojana or RSBY insurance card, and finally, if the household receives any government provided maternity benefit via Janani Suraksha Yojana (a maternal health program). All variables are binary.

Next, I examine whether politically connected households have better job opportunities. I create a variable Agriculture work if at least one person in the household is engaged in agriculture cultivation or agricultural labor work. Next, I create a variable Labor work if at least one person in the household works as a daily wage laborer. I create a variable Government work if at least one person in the household has a government job and create a variable Permanent Job if at least one person in the household has a permanent job. Finally, I create two variables, if a female member of the household has a government or permanent jobs.

Further I create control variables as follows. Poor equals one (defined in the dataset[6]), log of family income, household residence type pakka equals 1 if building has walls and flats, and equals 0 if it is chawl, slums and others. Household head's education, age, has a government job, permanent job and a dummy if household head is a male. Similarly, I look at household head's father's or husband's education level, household size, if household has electricity, and own a bicycle. We also look at seven caste types of dummy variables, namely: Brahmin, Forward Caste, OBC, Dalit, Adivasi, Muslim and Sikh/Jain/Christian. Finally, using the

[6] "Poverty IHDS calculated household poverty based on the monthly consumption per capita (copc, see Consumption) and the official Planning Commission poverty line as of 2005. poor is a dichotomous (0/1) variable indicating whether the household is below this poverty line or not. Users can calculate a poverty line ratio by dividing the monthly consumption per capita (copc) by the official poverty line (pcpl). The poverty line (pcpl) varies by state and urban/rural residence. It is based on 1970s calculations of income needed to support minimal calorie consumption and has been adjusted by price indexes since then." For more see: https://ihds.umd.edu/poverty

census 2011 data I have district level data on literacy rates and total population. Table 2.1b below provides summary of household level characteristics.

In Table 2.1b we see that households that know a politician at the local level (Village Panchayat Member/Official) are considerably more likely to have all forms of elite capture (have a ration and insurance cards). However, if one knows politicians higher up, they are less likely to obtain welfare benefits. Similarly, a household that knows politicians seem to be richer, have more income, have more electricity, and own a bicycle.

3.4 Methodology and Identification

3.4.1 Methodology

I begin by estimating the following probit model:

$$y_h^* = \beta_0 + \beta_1 POL_h + \beta_2 H_h + \beta_3 X_h + \epsilon_h \tag{1}$$

such that,

$$\begin{aligned} y_h &= 1 \;\; if \;\; y_h^* \geq 0 \\ &= 0 \;\; if \;\; y_h^* < 0 \end{aligned}$$

where, y_h is an outcome variable, POL_h is 1 if the household knows a politician, which is of three kinds: Village panchayat member/office, Acquaintance with elected politician and political party official. H_h is a vector of household head's characteristics such as: age, gender, education level, holds a government job, has a permanent job and HH head's father/husband's education. X_h is a vector describing the household structure such as: household poverty, household size and type, has electricity and owns a bicycle and ϵ_h is error term

Table 2.1b: Summary Statistics: All Variables

	Village Panchayat Member/Official			Acquaintance with Elected Politician			Acquaintance with Political Party Officials		
	Yes	No	N	Yes	No	N	Yes	No	N
Elite Capture									
Has Ration card	0.87	0.86	41472	0.89	0.86	41499	0.90	0.86	41521
Has BPL card	0.36	0.33	41473	0.29	0.35	41500	0.28	0.35	41522
Has Antyodaya card	0.08	0.05	41473	0.03	0.06	41500	0.03	0.06	41522
Has MGNREGA card	0.35	0.26	41426	0.20	0.31	41453	0.22	0.30	41475
Has RSBY	0.15	0.14	41242	0.14	0.15	41268	0.17	0.14	41289
Maternity Benefit	0.03	0.03	41496	0.02	0.03	41523	0.02	0.03	41549
Job Opportunities									
Agriculture Work	0.37	0.23	41496	0.31	0.26	41523	0.26	0.27	41549
Labor Work	0.39	0.39	41496	0.27	0.41	41523	0.28	0.41	41549
Government Work	0.10	0.10	41496	0.16	0.09	41523	0.17	0.09	41549
Permanent Job	0.17	0.21	41496	0.27	0.18	41523	0.29	0.18	41549
Government Work (Female Only)	0.03	0.03	41496	0.05	0.02	41523	0.05	0.02	41549
Permanent Job (Female Only)	0.05	0.05	41496	0.07	0.05	41523	0.08	0.05	41549
HH Characteristics									
Poor	0.17	0.17	41481	0.11	0.18	41508	0.10	0.18	41526
Household Type Pakka	0.97	0.94	41496	0.93	0.95	41523	0.94	0.95	41549
HH Head Male	0.87	0.86	41496	0.90	0.85	41523	0.90	0.85	41549
HH Head Education	49.94	49.67	41496	51.29	49.44	41523	50.91	49.51	41549
HH Head Age	49.94	49.67	41496	51.29	49.44	41523	50.91	49.51	41549
HH Head Father Edu	2.28	2.62	41162	3.38	2.36	41189	3.56	2.32	41213
Rural	0.80	0.61	41496	0.63	0.67	41523	0.57	0.68	41549
Household Size	5.05	4.77	41496	5.07	4.81	41523	4.98	4.83	41549
Has Electricity	0.87	0.87	41339	0.94	0.86	41366	0.94	0.86	41386
Has a Bicycle	0.56	0.54	41483	0.55	0.54	41510	0.57	0.54	41524
District popn (log)	14.86	15.02	41496	14.99	14.97	41523	15.04	14.96	41549
District Education (years)	5.97	6.39	41496	6.80	6.17	41523	7.01	6.13	41549
Member of SHGs	0.20	0.19	41496	0.20	0.19	41523	0.22	0.18	41549
Caste:									
Brahmin	0.05	0.05	41473	0.07	0.05	41500	0.06	0.05	41526
Forward Caste	0.16	0.17	41473	0.23	0.15	41500	0.24	0.15	41526
OBC	0.36	0.34	41473	0.31	0.35	41500	0.29	0.35	41526
Dalit	0.20	0.21	41473	0.15	0.22	41500	0.16	0.22	41526
Adivasi	0.11	0.08	41473	0.09	0.09	41500	0.09	0.09	41526
Muslim	0.09	0.12	41473	0.10	0.12	41500	0.12	0.11	41526
Sikh/Jain/Christian	0.03	0.03	41473	0.04	0.03	41500	0.04	0.03	41526

Notes: Some households have missing values clustered at the village level.

β_1 is our coefficient of interest.

Estimating the above equations using probit model faces multiple challenges. The coefficient β_1 is consistent only if ϵ_h are independently- and identically-distributed, with zero mean and unit variance. However, this assumption could be violated for few reasons. The first and foremost is that, while we have a rich dataset and multiple set of controls, we may still face the issue of omitted variable such as ability. It could be likely that, a more capable household is outgoing and knows politician etc. If that is the case, the estimates from probit are biased due to a correlation between political connection and the unobserved ability.

To correct for these concerns, I estimate a bivariate probit (BP) regression model (Greene, 2001) which uses latent linear index models and assumes joint normal errors. Using a maximum-likelihood (MLE) method, I estimate the following equations:

$$y_h^* = \beta_0 + \beta_1 POL_h + \beta_2 H_h + \beta_3 X_h + \nu_h \tag{2}$$

$$POL_h^* = \beta_0 + \beta_1 (Village\_SHG\_Pctg)_h + \beta_2 H_h + \beta_3 X_h + \epsilon_h \tag{3}$$

such that,

$$\begin{aligned} POL_h &= 1 \;\; if \;\; POL_h^* \geq 0 \\ &= 0 \;\; if \;\; POL_h^* < 0 \\ y_h &= 1 \;\; if \;\; y_h^* \geq 0 \\ &= 0 \;\; if \;\; y_h^* < 0 \end{aligned}$$

In order to improve the identification for the bivariate probit model, I use an instrument called Village_SHG_Pctg which measures the degree of prevalence of Self-Help Groups (SHGs) in a village. Specifically, the instrumental variable is the percentage of household in a village that are member of SHGs. An instrument is proper if it is correlated with political connection ($POL*$) but does not directly determine the outcome variable $y*$. I propose that higher prevalence of SHGs in a villages/municipalities increases the probability of knowing a politician significantly. I argue that availability of SHGs at the village level on population and education is "as good as random". I further discuss the validity of the instrument below.

3.4.2 Self Help Groups, Political Connection and Instrumental Variable

In 1974 when Bangladesh was hit by a devastating flood, Professor Muhammad Yunus lent 27 dollars to group of women in the city of Jobra which they used to make bamboo baskets and sold them to generate income for their families. The concept of microcredit – providing small loans to poor people - thus, is mostly associated with Muhammad Yunus, a 2006 Nobel Prize winner (Yunus, 2003). Since then the idea of microcredit[7] has been lauded[8] as the cure to World's poverty to being a medium to profit off of poor (Polgreen and Bajaj, 2010).

India, home of over a billion people where around 30 percent of the people live below poverty line,[9] has been a hotbed for microcredit. Historically, the typical form of microcredit has been a Self Help Groups model. SHGs typically composes of 15-20 members, mostly women who are involved in credit and savings activities. National Bank of Agriculture and Rural Development (NABARD) plays an important role in financing these groups (Nair, 2005). As of 2003, over 700,000 groups had obtained over Rs.20 billion (US$425 million) in loans such groups (Nair, 2005). By 2007, some 40 million households were organized in more than 2.8 million SHGs that borrowed more than US$ 1 billion of credit (Deininger and Liu, 2009).

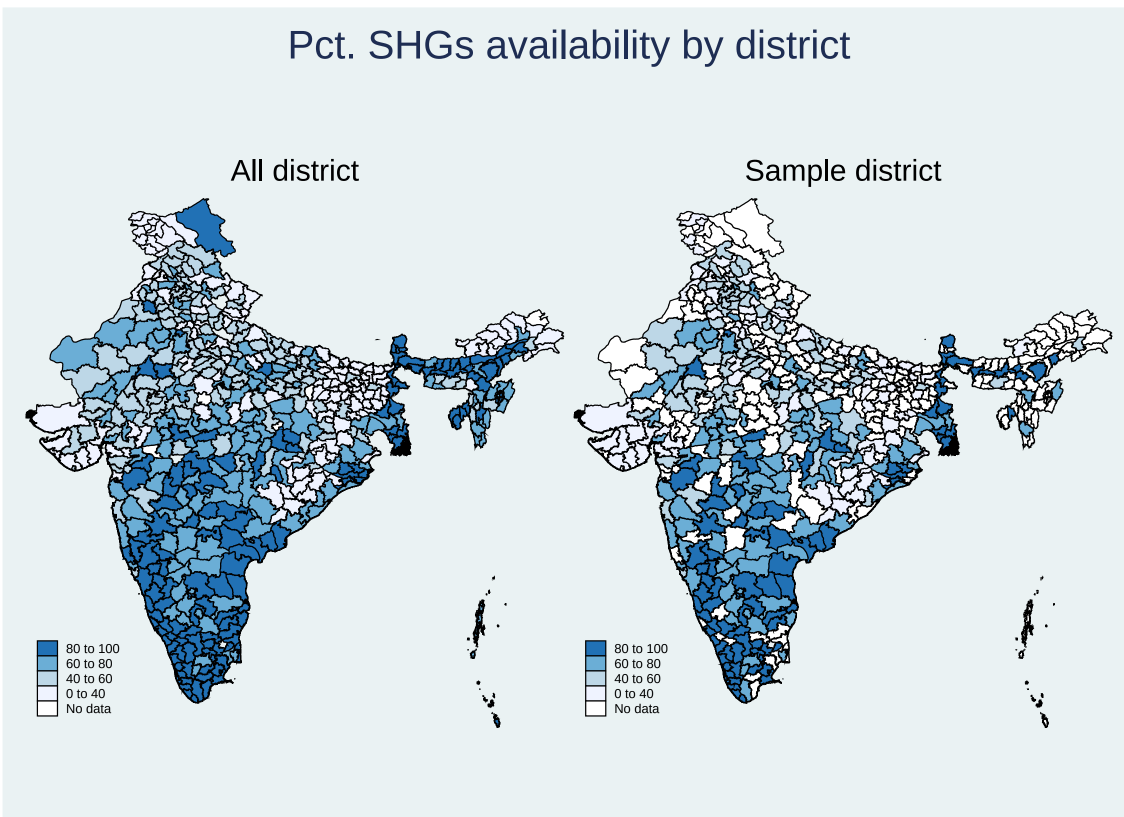
[7]Microcredit, microfinance and self-help groups essentially means providing small loans to poor people which they can use to start business or be in the first ladder of development.

[8]See Morduch and Haley (2002) for summary of various literature that shows the impact of microcredit.

[9]In 2011, World Bank estimated 22% lived below poverty line (World Bank, 2020).

Using the data from District Level Household Survey (DLHS III)[10], I plot the percentage of villages/municipalities in each district that have a SHG in the figure below. For each village/municipality the survey asks whether any SHGs was available. Using this I calculate the availability of SHGs at district level. The following map shows the penetration of SHGs across Indian district. Map on the left is for all the districts in India and on the right are the sample districts from IHDS II data. We see that SHGs are concentrated in mostly southern districts.[11]

Figure 3.1: Percentages of SHG by districts



Source: DLHS III

The impact of microcredit loan on poverty alleviation has been highly touted. In the

[10]The survey was carried out between 2008-2009 (DLHS-3, 2020).

[11]My analysis is at the village level and uses IHDS data. I use DLHS data for the figure because the IHDS data does not disclose the village names and the village infrastructure survey is carried out only for rural areas.

decade of 2000, many studies studied the impact of microfinance on poverty alleviation as well as household wellbeing at various level: income, health, children education women empowerment among others[12] and reported positive findings. Given this literature and its conclusion, SHGs as instrument might be correlated with the outcome variables - job prospects of a household. However, as of late, studies show that, microcredit credit has little to no effect in any spectrum of household wellbeing. In January 2015 volume of American Economic Journal: Applied Economics, six highly influential, RCT evaluations showed mixed results (Banerjee et al., 2015). These researches find that, microcredit increased credit of a household and there was uptick in small business activities but found no effect on household's income, consumption and social effects.

The other aspect of using SHGs as an instrument is, that the prevalence of higher SHGs in a village should increases the probability of knowing a politician. Although SHGs primarily are used to provide small loans, given its structure it may induce other benefits. Among others it includes emphasis on social empowerment, outreach, and capacity building. Borrowers are encouraged to attend regular meetings. Deininger and Liu (2009) write "...federation of SHGs is a central element not only with respect to peer monitoring and diversification of risks on the financial side but federations at village and higher levels are also used to assist in implementation of government programs, help SHGs provide other services -from technical assistance to marketing- and allow members' participation in local government."

Given the nature of organization of SHGs, it would be reasonable to argue that membership of a SHG increases one's social capital. Social capital is often generated through repeated interaction and interdependence within a group of population. Increasing level of politically relevant social capital can enhance the probability of an individual being engaged in politics (Lake and Huckfeldt, 1998). Mosley et al. (2004) look at the contribution of microfinance on community-building and political participation. Looking at the data from

[12]For more see Morduch and Haley (2002) which summaries the literature.

Russia, Slovakia and Romania they find that being a member of microfinance compared to control group led to higher membership of church associations and formation of informal political organization. Howard (2013) shows that, being a microfinance borrower is positively and significantly associated with political participation in rural Senegal.

From IHDS data, using the question “Is your household a member of Self Help Group”, I calculate the percentages of households that are a member of SHG in a village. In Appendix Table 3.A1 I regress three types of political connections on the percentage of villages and I find the higher prevalence of SHGs at the village/town increases the probability of knowing a politician at the local level by 7%. However, the higher prevalence of SHGs at the village/town has no effect on knowing elected politicians and party officials.

3.5 Results

I start by presenting probit regression. Table 3.2a presents results for the household’s ability to obtain various welfare programs (access to welfare benefits). We see that knowing any kind of politician increases the probability of obtaining any ration cards by almost two percentage points. A household’s political connection in obtaining BPL and Antyodaya card does not matter, if anything it has negative effect.

Table 3.2b shows the results for the household’s job prospects. Knowing a politician increases the probability of at least one household member working in agriculture and decreases the probability of doing manual labor jobs. Similarly, it increases the probability of having a government jobs and permanent jobs.

However, as discussed above, these results might be biased due to the likely endogeneity of political connections. It could be that the probability of obtaining welfare benefits and political connection could be jointly determined. Next I present results from the bivariate probit regression models which corrects for the endogeneity.

Table 3.2a: Probit Regression (Elite Capture)

	Ration Card Any	BPL Card	Antyodaya Card	MGNREGA Card	RSBY (Insurance)	Maternity Benefit
Village Panchayat	0.011**	-0.004	0.007	0.038***	0.024***	-0.000
	(0.005)	(0.007)	(0.007)	(0.006)	(0.005)	(0.002)
N	40841	40918	40918	40733	40286	40287
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029
Acqt Elected Official	0.015***	-0.024***	-0.033***	-0.020***	0.006	-0.004
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.003)
N	40868	40945	40945	40760	40312	40314
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029
Acqt Party Official	0.021***	-0.028***	-0.032***	-0.016**	0.030***	-0.006*
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.003)
N	40881	40958	40958	40773	40325	40327
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029

Notes: Only the marginal effect of main variable (types of political connection) is reported. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. All regressions include State level fixed effects. Robust standard errors are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 3.2b: Probit Regression (Job Opportunities)

	Agriculture Work	Labor Work	Government Work	Permanent Job	Government Work (Female Only)	Permanent Job (Female Only)
Village Panchayat	0.032***	-0.010	0.009**	0.001	0.005**	0.004
	(0.006)	(0.007)	(0.004)	(0.005)	(0.002)	(0.003)
N	40940	40940	40940	40940	40940	40940
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051
Acqt Elected Official	0.022***	-0.059***	0.020***	0.026***	0.007***	0.010***
	(0.007)	(0.008)	(0.004)	(0.006)	(0.002)	(0.003)
N	40967	40967	40967	40967	40967	40967
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051
Acqt Party Official	0.014*	-0.058***	0.016***	0.026***	0.007***	0.010***
	(0.007)	(0.008)	(0.004)	(0.006)	(0.002)	(0.003)
N	40980	40980	40980	40980	40980	40980
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051

Notes: Only the marginal effect of main variable (types of political connection) is reported. All regressions are instrumented by the percentage of households in each village/town who are members of Self-Help Groups. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. All regressions include State level fixed effects. Robust standard errors are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 3.3a shows results from the bivariate probit regression models where the political connection is instrumented by the prevalence of SHGs in a village. It presents results for the household's ability to obtain various welfare programs (access to welfare benefits). Results show that knowing a politician at the local level, increases the probability of obtaining, any rations cards by (12%), BPL card (9%), Antyodaya card (17%), MGNEGRA card (20%) and RSBY card (12%). Knowing a politician at higher level (elected politicians and political party officials) does not seem to have much effect in obtaining welfare benefits; if anything has negative effect. This is likely due to the fact that designation of such welfare program (to qualify for such programs) are administrated at local levels.

Table 3.3a: IV Probit Regression (Elite Capture)

	Ration Card Any	BPL Card	Antyodaya Card	NEGRA Card	RSBY (Insurance)	Maternity Benefit
Village Panchayat	0.118***	0.090*	0.166***	0.195***	0.123**	0.006
	(0.043)	(0.054)	(0.059)	(0.043)	(0.049)	(0.014)
N	40917	40918	40918	40872	40696	40940
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029
Acqt Elected Official	-0.076**	-0.364***	-0.345***	-0.112***	-0.074**	-0.074*
	(0.033)	(0.024)	(0.026)	(0.035)	(0.036)	(0.040)
N	40944	40945	40945	40899	40722	40967
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029
Acqt Party Official	0.053	-0.315***	-0.240***	-0.031	0.033	-0.108
	(0.036)	(0.033)	(0.047)	(0.040)	(0.032)	(0.069)
N	40957	40958	40958	40912	40735	40980
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029

Notes: Only the marginal effect of main variable (types of political connection) is reported. All regressions are instrumented by the percentage of households in each village/town who are members of Self-Help Groups. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. Additional controls include district level log of population and literacy rates. All regressions include State level fixed effects. Standard errors clustered at village/town level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 3.3b shows the results from the bivariate probit regression models for the household's job prospects. Knowing any kinds of politicians at the local level has no effect. However, knowing elected politicians and political party officials decreases the probability of at least one person in the household working in agriculture and daily wage labor. Knowing a politician does not have any effect on the probability of at least one member in the household having a government and permanent jobs. However, knowing elected official increases the probability of at least one female member of a household having a government and permanent jobs.

Table 3.3b: IV Probit Regression (Job Opportunities)

	Agriculture Work	Labor Work	Government Work	Permanent Job	Government Work (Female Only)	Permanent Job (Female Only)
Village Panchayat	-0.079	0.059	0.005	-0.009	-0.005	0.005
	(0.199)	(0.071)	(0.030)	(0.053)	(0.013)	(0.025)
N	40940	40940	40940	40940	40940	40940
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051
Acqt Elected Official	-0.133***	-0.387***	0.025	-0.040	0.043*	0.086***
	(0.051)	(0.027)	(0.112)	(0.059)	(0.025)	(0.024)
N	40967	40967	40967	40967	40967	40967
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051
Acqt Party Official	-0.085*	-0.299***	-0.199***	0.031	0.030	0.026
	(0.046)	(0.055)	(0.030)	(0.043)	(0.020)	(0.024)
N	40980	40980	40980	40980	40980	40980
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051

Notes: Only the marginal effect of main variable (types of political connection) is reported. All regressions are instrumented by the percentage of households in each village/town who are members of Self-Help Groups. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. Additional controls include district level log of population and literacy rates. All regressions include State level fixed effects. Standard errors clustered at village/town level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

3.6 Robustness Check

Panda (2015) uses being a member of a social group[13] to instrument political connection to estimate the elite capture of obtaining BPL card in India. Member of social group is defined as 1 if anybody in the household is a member of a social group association and 0 otherwise. In order to gauge the effectiveness of the instrument (prevalence of SHGs in village/town), I use being a member of a social group and run the analysis above. Results are reported the results in Tables 3.4a and 3.4b. Results show that knowing a local level politician increases the probability of obtaining any ration cards increases by almost 9%. The probability of obtaining a BPL card and an Antyodaya card is positive but not significant. Similarly, it increases the probability of obtaining RSBY (insurance) by 10%.

Table 4b shows the results for the household's job prospects. Knowing a politician decreases the probability of at least one household member in agriculture and labor work. Finally, knowing elected politicians increases the probability of at least one female member of the household in having permanent jobs. The magnitudes are smaller, but the results are similar to my main results in Table 3a and b.

[13]for more see Panda (2015).

Table 3.4a: IV Probit Regression (Elite Capture) - Social Group Instrument

	Ration Card Any	BPL Card	Antyodaya Card	NEGRA Card	RSBY (Insurance)	Maternity Benefit
Village Panchayat	0.088**	0.041	0.097	0.065	0.098*	-0.002
	(0.038)	(0.059)	(0.082)	(0.078)	(0.055)	(0.015)
N	40909	40910	40910	40864	40688	40932
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029
Acqt Elected Official	-0.018	-0.325***	-0.313***	-0.077***	0.023	-0.020**
	(0.036)	(0.030)	(0.031)	(0.025)	(0.025)	(0.009)
N	40909	40910	40910	40864	40688	40932
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029
Acqt Party Official	0.066**	-0.305***	-0.244***	-0.061**	0.063**	-0.040**
	(0.029)	(0.032)	(0.039)	(0.026)	(0.026)	(0.019)
N	40909	40910	40910	40864	40688	40932
Dep Var Mean	0.865	0.342	0.397	0.288	0.144	0.029

Notes: Only the marginal effect of main variable (types of political connection) is reported. All regressions are instrumented by if a family member is member of a social group (caste) association. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. Additional controls include district level log of population and literacy rates. All regressions include State level fixed effects. Standard errors clustered at village/town level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 3.4b: IV Probit Regression (Job Opportunities) - Social Group Instrument

	Agriculture Work	Labor Work	Government Work	Permanent Job	Government Work (Female Only)	Permanent Job (Female Only)
Village Panchayat	-0.191***	0.008	0.022	-0.017	-0.003	0.011
	(0.039)	(0.085)	(0.026)	(0.049)	(0.013)	(0.022)
N	40932	40932	40932	40932	40932	40932
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051
Acqt Elected Official	-0.095**	-0.347***	0.053	0.012	0.032	0.076***
	(0.042)	(0.040)	(0.040)	(0.037)	(0.026)	(0.022)
N	40932	40932	40932	40932	40932	40932
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051
Acqt Party Official	-0.101***	-0.280***	0.049	0.046	0.030	0.034*
	(0.037)	(0.058)	(0.035)	(0.035)	(0.019)	(0.020)
N	40932	40932	40932	40932	40932	40932
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051

Notes: Only the marginal effect of main variable (types of political connection) is reported. All regressions are instrumented by if a family member is member of a social group (caste) association. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. Additional controls include district level log of population and literacy rates. All regressions include State level fixed effects. Standard errors clustered at village/town level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Further, I use the prevalence of SHGs in village/town in 2005 (the first wave of the IHDS survey) to instrument for wave II data. Using the wave I data to instrument reduces the threat to the validity of the instrument. Given that the prevalence of SHG in a village/down becomes predetermined in a temporal sense, it may add extra validity to the instrument. Results are reported the results in Tables 3.5a and 3.5b below. Results show that knowing a local level politician increases the probability of obtaining Antyodaya card increases by almost 12%. The probability of obtaining a BPL card and any ration card is positive but not significant.

Table 3.5a: IV Probit Regression (Elite Capture) - 2005 SHG Instrument

	Ration Card Any	BPL Card	Antyodaya Card	NEGRA Card	RSBY (Insurance)	Maternity Benefit
Village Panchayat	0.051	0.054	0.122*	0.049	-0.157**	-0.024
	(0.037)	(0.059)	(0.074)	(0.097)	(0.072)	(0.048)
N	40900	40901	40901	40855	40679	40923
Dep Var Mean	0.865	0.342	0.397	0.288	0.145	0.029
Acqt Elected Official	-0.069**	-0.359***	-0.339***	-0.093***	-0.069**	-0.062**
	(0.032)	(0.024)	(0.027)	(0.028)	(0.031)	(0.025)
N	40927	40928	40928	40882	40705	40950
Dep Var Mean	0.865	0.342	0.397	0.288	0.145	0.029
Acqt Party Official	0.035	-0.319***	-0.249***	-0.056*	0.013	-0.129***
	(0.031)	(0.030)	(0.040)	(0.029)	(0.032)	(0.039)
N	40940	40941	40941	40895	40718	40963
Dep Var Mean	0.865	0.342	0.397	0.288	0.145	0.029

Notes: Only the marginal effect of main variable (types of political connection) is reported. All regressions are instrumented by the percentage of households in each village/town who are members of Self-Help Groups. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. Additional controls include district level log of population and literacy rates. All regressions include State level fixed effects. Standard errors clustered at village/town level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

Table 5b shows the results for the household's job prospects. Knowing a politician decreases the probability of at least one household member in agriculture and labor work. Finally, knowing elected politicians increases the probability of at least one female member of the household in having permanent jobs. The magnitudes are smaller but the results are similar to my main results in Table 3a and b.

Table 3.5b: IV Probit Regression (Job Opportunities) - 2005 SHG Instrument

	Agriculture Work	Labor Work	Government Work	Permanent Job	Government Work (Female Only)	Permanent Job (Female Only)
Village Panchayat	-0.192***	0.008	0.022	-0.017	-0.003	0.011
	(0.039)	(0.085)	(0.026)	(0.049)	(0.013)	(0.022)
N	40915	40915	40915	40915	40915	40915
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051
Acqt Elected Official	-0.094**	-0.347***	0.053	0.012	0.032	0.076***
	(0.042)	(0.040)	(0.039)	(0.037)	(0.026)	(0.022)
N	40915	40915	40915	40915	40915	40915
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051
Acqt Party Official	-0.100***	-0.280***	0.048	0.044	0.030	0.034*
	(0.037)	(0.058)	(0.035)	(0.035)	(0.019)	(0.020)
N	40915	40915	40915	40915	40915	40915
Dep Var Mean	0.272	0.389	0.101	0.198	0.028	0.051

Notes: Only the marginal effect of main variable (types of political connection) is reported. All regressions are instrumented by if a family member is member of a social group (caste) association. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. Additional controls include district level log of population and literacy rates. All regressions include State level fixed effects. Standard errors clustered at village/town level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

3.7 Discussion

Given the federal structure and more emphasis in local level of India, the operation and supervising of Ration Cards programs and other subsidies program largely is the responsibility of local and state government. State Government uses local governing bodies for identification of eligible families and other matters (Panda, 2015). When federal oversight is poor and programs are distributed at the local level, the chances of elite capture increases. The results in this paper could provide some important policy lesson in terms of curbing corruption at the local level.

India spends large sum of money for its various targeted welfare programs to alleviate poverty and hardships to its vulnerable citizens. In 2014–15, Rs 1,226.76 billion (US$18.9 billion) was spent on food subsidies (Shrayana Bhattacharya and Puri, 2017). Similarly, for MGNREGA, in last 10 years, it spent $51 billion in wages (Mathur and Bolia, 2016). The Indian Ministry of Labor and Employment (MoLE) launched the RSBY in April 2008, to provide insurance coverage for inpatient care to poor families. It spent about $1 billion in 2016 (Raghavan, 2018). Given these huge expenditures, especially targeted for the poor, misuse and misallocation of such funds is concerning. When welfare program does not reach the intended beneficiaries, it adds more pressure to Government to increase the funding. These benefits in turn could be passed to the children in the household giving them unfair advantage.

3.8 Conclusion

Patronage, corruption, and elite capture are not new issues in any society but are especially prevalent in developing countries where institutions and rule of law are weak. Providing welfare to protect its vulnerable citizen has become a hallmark of democratic countries. When political corruption runs high, welfare is often assigned to relatives and used for vote buying. In this paper, I look at a household's likelihood to obtain welfare benefits and their member's job prospects if they know politicians. I categorize politicians into three types: members at the local level (village/municipalities), elected politicians at the state and federal level, and finally political party officials.

To rule out of endogeneity I instrument political connection with prevalence of SHGs at village/town level. First stage results show that being a member of SHGs increases the portability of knowing a politician by 7 percent. I find that households that know politicians at local level are almost 20 percent more likely to obtain Rations cards. If such large number of critical benefits targeted for the poor is captured by elites and political clients, it should be matter of high concern for the government, policymakers and stakeholders of our societies.

Further I look at the job prospects of a household. Households that know a politician (elected and party officials) are less likely to have at least one-member working as agriculture or labor work and more likely to have government or permanent jobs.

3.9 Appendix

Table 3.A1: Political Connection and prevalence of SHGs

	Village Panchayat	Acqt Elected Official	Acqt Party Official
Village_SHG_Pctg	0.073**	-0.017	0.027
	(0.030)	(0.020)	(0.020)
N	40940	40967	40980
R-squared	0.165	0.136	0.158
Dep Var Mean	0.277	0.161	0.161

Notes: Only the marginal effect of main variable is reported. Controls include: log of income, family size, male HH head (dummy), HH head education, HH head father/husband education, HH head permanent job, HH head government job, HH has electricity, owns a bicycle, rural (dummy) and if household is pakka and 7 caste groups. Additional controls include district level log of population and literacy rates. All regressions include State level fixed effects. Standard errors clustered at village/town level and are in parenthesis. The symbols *, ** and *** represent significance at the 10, 5 and 1 percent, respectively.

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