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Immigration policy and fertility: Evidence from undocumented migrants in the U.S. ☆

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ABSTRACT

Using the 2005–2014 waves of the American Community Survey –a period characterized by the rapid expansion of interior immigration enforcement initiatives across the United States, we evaluate the impact of a tougher policy environment on undocumented immigrants' fertility. We find that a one standard deviation increases in enforcement lowers childbearing among likely undocumented women by 5%. The effect stems from police-based measures linked to increased deportations, which may raise uncertainty about the future of the family unit and its resources.

1. Introduction

Almost 20% of the undocumented immigrants were deported between 2009 and 2013 following the expansion of immigration enforcement after 9/11. Since then, immigration enforcement has been playing an increasingly important role in American politics. In light of Congress' inability to enact a comprehensive immigration reform, a number of states and localities across the United States have taken immigration matters into their own hands. A variety of immigration enforcement policies were adopted at the local and state levels during President Obama's Administration, and reactivated by President Trump in February 2017. All these initiatives intended to curb the number of undocumented immigrants by discouraging their entry and, more importantly, by facilitating their identification, apprehension and, ultimately, deportation. As a result, more than 1.8 million of the estimated 11 to 12 million undocumented immigrants were deported under President Obama's

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Administration alone (Vaughan, 2013),[1] with deportations from the interior of the United States rising at the fastest pace. Specifically, over the 2003 through 2011 period, interior removals rose by roughly 520%,[2] while border removals increased by 76% (see Fig. A in the Appendix). Deportations have continued to grow over the last years.[3,4]

At the same time, the Hispanic fertility rate has been declining. A report from the Pew Hispanic Center in 2012 noted the strong reduction in fertility rates among immigrants, especially Hispanics –a group that encompasses most of the undocumented population in the United States. Between 2007 and 2010, the birth rate for foreign-born women dropped by 14%, compared to 6% in the case of U.S.-born women. Mexican immigrant women experienced the largest decline –about 23% (Livingston and Cohn, 2012).[5] In fact, this drop reached 26% among likely undocumented women between 2005 and 2014.[6,7]

This paper provides the first empirical evidence of the effect of intensified immigration enforcement on immigrant women's childbearing –a behavioral response largely grounded on fear of apprehension, deportation, and separation, as well as on economic constraints. Knowledge of how immigrant fertility responds to intensified enforcement is important for various reasons. Hispanic immigrants have had significant demographic and economic contributions to the United States. For instance, in terms of demographics, recent reports have indicated how the United States' fertility rate is now below replacement rate (Mathews and Hamilton, 2019), after being one of the few developed countries with fertility rates close to replacement rates thanks to immigrants' offspring (Kotkin and Ozuna, 2012). Hispanic women, especially immigrants (Livingston, 2017), continue to be among the groups displaying higher fertility rates (Mathews and Hamilton, 2019). Higher fertility rates are helpful in building a younger workforce. In 2015, Hispanics were, on average, 28.7 years old, relative to 43.3 years old in the case of white non-Hispanics. Between 2010 and 2015, the non-Hispanic workforce shrunk, whereas the Hispanic workforce grew by nearly 2.5 million (Schink and Hayes-Bautista, 2017). Furthermore, because Hispanics display higher labor force participation rates than non-Hispanics,[8] the so-called Hispanic contribution to U.S. GDP growth has been estimated to grow 70% faster that the non-Hispanic contribution (Schink and Hayes-Bautista, 2017). Hispanics not only contribute more than one out of every 10 dollars in total tax revenues in six U.S. states,[9] but also increase the ratio of workers to retirees and the viability of Social Security (Griswold, 2012). In sum, gaining greater awareness of the implications of intensified immigration enforcement on migrant fertility is both socially and economically important.

Intensified immigration enforcement could impact immigrant fertility through various mechanisms. On one hand, the mere separation from a partner and the fragmentation of the family through deportation can either end or place fertility on hold. But, even among intact households, a tougher climate might negatively impact family income (*e.g.* Bansak, 2005; Orrenius and Zavodny, 2009; Bohn et al., 2015; Amuedo-Dorantes et al., 2018), its access to important health care services and benefits (*e.g.* Watson, 2014) and, overall, increase uncertainty about the future of the family unit and its ability to raise children. Under the neoclassical approach to fertility (Becker, 1960), the uncertain environment, as well as constrained access to jobs and other resources, can make fertility a risky and costly choice. On the other hand, because of birth right citizenship, undocumented women might want to have their children while still in the country to provide their children with better life opportunities. In a related vein, a popular claim in the press has been that undocumented women might be interested in having their children in the United States because these children could sponsor their parents for citizenship (the so-called "anchor baby" hypothesis). Note, however, that the mere childbearing of a child in the United States does not allow an undocumented parent to legalize. The latter can only happen once the U.S. citizen child reaches 21 years of age and a host of other circumstances are met.[10]

Our focus is on assessing the effect that the piecemeal approach to immigration enforcement might be having on the fertility of immigrant women most likely impacted by such policies –namely: undocumented women. To that end, we use a unique data set that combines data from the 2005––2014 waves of the ACS –a period of rapid growth in interior immigration enforcement, and detailed information on the intensity of immigration enforcement at the local and state levels over that period. To identify the effects of tougher enforcement on the fertility of these women, we exploit the temporal and geographic variation across metropolitan areas (MSAs) in the adoption of these measures. Our findings show that the average

[1] Ninety-one percent of removals were men during 2003-2013, even as women represent 47% of undocumented population in the United States (Bergeron *et al.* 2014).

[2] Kandel (2016) documents the increase in number of arrests associated to various interior immigration enforcement priorities over that time span. See: https://fas.org/sgp/crs/homesec/R44627.pdf

[3] See, for example: https://www.washingtonpost.com/local/immigration-arrests-up-during-trump/2017/05/17/74399a04-3b12-11e7-9e48-c4f199710b69_story.html

[4] Between January 22 and April 29, ICE conducted around 10,800 "non-criminal arrests," compared to just 4,200 in 2016—an increase of more than 150% (U.S. Immigration and Customs Enforcement (ICE), 2017a).

[5] This figure is regardless the citizenship of the women.

[6] In the data section, we explain in detail how we proxy for the likely undocumented status of women.

[7] Authors' tabulations using data from the American Community Survey (ACS) for the 2005 through 2014 period.

[8] In 2016, labor force participation rates among Hispanics averaged 66%, compared to 62% among whites non-Hispanics according to the Bureau of Labor Statistics (see: https://www.bls.gov/opub/reports/race-and-ethnicity/2016/home.htm)

[9] Report available: http://research.newamericaneconomy.org/wp-content/uploads/sites/2/2017/12/Hispanic_V5.pdf

[10] Specifically, if the parents never entered the United States legally, they will most likely need to depart the United States and file with the U.S. embassy in their home country. However, due to their unlawful status while in the United States, they will be banned from entering the country for, at least, 10 years. See: https://www.nolo.com/legal-encyclopedia/how-soon-can-the-us-born-child-undocumented-immigrant-petition-the-parent.html

increase in interior immigration enforcement between the mid-2000s to the mid-2010s lowered the likelihood of childbearing among likely undocumented immigrant women by 5%; thereby accounting for approximately one fifth of the drop in fertility experienced by these women over the period under consideration. These results prove robust to several identification and robustness tests that show how our findings can be interpreted as lower-bound estimates. We also explore the policy channels to better understand which policies are responsible for the observed impacts. We find that the effects can be attributed to police-based measures (as opposed to employment restricting measures, like employment verification mandates), hinting on the relevance of apprehension and deportation fear instilled in migrants by these policies –both elements less prevalent in employment-based enforcement measures. Lastly, to understand better the mechanisms through which the observed impacts are taking place, we perform several heterogeneity analyses that reveal how the negative impact of intensified enforcement on the fertility of likely unauthorized women is even present in the case of intact households. Therefore, household compositional effects (*e.g.* deportation of a partner) are not the exclusive mechanism through which fertility might end or be placed on hold. Furthermore, we show how the impact of intensified immigration enforcement appears to be concentrated among women in the lowest family income quartile, as well as among families where the couple is likely unauthorized. These findings suggest that, both, current and future economic resources –possibly more uncertain when both partners are likely unauthorized– play an important role.

The contribution of this research is twofold. *First*, it adds to a rapidly growing literature concerned with the consequences of a fragmented and intensified approach to immigration enforcement. To our knowledge, this is the first study examining the impact of interior immigration enforcement on the fertility patterns of undocumented immigrant women. In so doing, it complements a number of studies exploring the effects of intensified enforcement on undocumented immigrants' residential choices, employment, earnings, remitting and on their children's access to healthcare and schooling outcomes (*e.g.* Amuedo-Dorantes et al., 2013, 2018; Amuedo-Dorantes and Lopez, 2015; Bohn et al., 2014; Kostandini et al., 2013; Watson, 2014).

Second, it adds to a vast literature on fertility and its policy determinants.[11] In our case, the focus is on how immigrant fertility is impacted by changes in immigration policy.[12] To our knowledge, there are two studies that focus, in particular, on the fertility of Hispanic immigrant women –more likely to be deemed undocumented. Falasco and Heer (1984) explore how legal status might affect fertility through its effects on male and female wages. Amuedo-Dorantes et al. (2016) study how welfare reform (the 1996 passage of PRWORA) lowered the fertility of foreign-born non-citizen women. Yet, to date, there is a lack of understanding of how the currently fragmented approach to immigration policy and enforcement is affecting undocumented immigrant women's fertility.

The paper is organized as follows. Section 2 delineates the institutional background with regards to immigration enforcement and discusses its link to immigrant fertility. Section 3 describes the data, and Section 4 the empirical methodology. Section 5 presents the main findings, whereas Section 6 assesses the policy channels and mechanisms through which the found impacts are likely taking place. Finally, Section 7 concludes the study.

2. Background on immigration enforcement

Since 9/11, the United States has witnessed an escalation of immigration enforcement aimed at identifying undocumented immigrants for removal. The policies have ranged from worksite enforcement and work eligibility verification to the engagement of local and state law enforcement personnel in the enforcement of immigration policy. As a result, more than 4.5 million undocumented immigrants have been removed following the passage of the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 (IIRIRA) (Bergeron and Hipsman 2014). While the deportation of criminal aliens has always taken place under the U.S. immigration law, it was with the enactment of the 1996 IIRIRA and its implementation that removals increased from an average of 3% in the 1970–1996 period to 19% during 2003–2006, and to a record high of 65% in 2012 (Bergeron and Hipsman 2014).

In what follows, we describe the various local and state immigration enforcement policies we take into consideration in this analysis.[13]

2.1. The 287(g) agreements

The 287(*g*) agreements evolved from the 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA), which allowed state and local agencies to enforce immigration law. State and local law enforcement entities would sign an agreement with Immigration and Customs Enforcement (ICE) that detailed the extension and limitations of the authority to be delegated. The 287(*g*) agreements enabled state and local officers to interrogate immigrants, arrest them without warrant and begin the process of their removal when appropriate. This was the only program that allowed state and local law enforcement officials to enforce federal immigration law directly. There were three types of 287(*g*) agreements: "task force", "jail enforcement" and a "hybrid". The "task force" allowed local and state officers to interrogate and arrest non-citizens during their regular duties of law enforcement operations. The number of agreements in place by 2012 was significantly cut

[11] While not necessarily focusing on immigrant fertility, the literature has examined the impact of other policy changes, including parental leaves (*e.g.* Lalive and Zweimüller, 2009), cash transfers (*e.g.* Milligan, 2005), family planning programs (*e.g.* Bailey, 2012), among many others.

[12] For instance, focusing on Germany, Avitabile et al. (2014) examines how changes to the German citizenship law impacted immigrants' fertility choices.

[13] Table A in the Appendix provides a summary of the various policies and key traits.

down following the reduction in funding for the 287(*g*) program, and the almost universal adoption of the Secure Communities program. The "jail enforcement" model permitted local officers to interrogate immigrants who had been arrested on state and local charges about their immigration status. Under this program, 402,079 potentially removable aliens have been identified, mostly at local jails, between 2006 and 2015. Additionally, more than 1675 state and local officers have been trained and certified by ICE to enforce immigration law (U.S. Immigration and Customs Enforcement (ICE), 2016).

2.2. Secure communities

The Secure Communities (SC) program was designed to empower ICE to prioritize the use of enforcement resources to target non-citizens who have committed serious crimes. This was to be achieved by checking their fingerprints against the Federal Bureau of Investigation (FBI) dataset for criminal arrest and convictions, and the Department of Homeland Security (DHS) dataset that tracks their immigration history. The program expanded quickly since its initial implementation in 7 jurisdictions in 2008 to 3181 jurisdictions in 2013. The Priority Enforcement Program (PEP) replaced SC in July 2015 (U.S. Immigration and Customs Enforcement (ICE), 2016), but Secure Communities was reinstated by President Trump early 2017.

2.3. Omnibus immigration laws

In addition to the aforementioned initiatives sponsored through ICE, some states intensified immigration enforcement by legislating the state's ability to enforce immigration law in a number of daily life scenarios. For example, Arizona and Alabama enacted laws with provisions that allowed state and local enforcement officers to check an individual's immigration status during a "lawful stop, detention or arrest" if there was suspicion of the person being an undocumented immigrant –the "show me your papers" clause. In fact, Alabama even required schools to record students' immigration status. Arizona was the first state to enact this kind of law in 2010 (SB1070), but it was quickly followed by six other states in 2011, namely: Alabama (HB56), Georgia (HB87), Indiana (SB590), South Carolina (S20) and Utah (H116, H466, H469, and H497).

2.4. Employment verification systems

Lastly, several states mandated the use of electronic programs to check the work eligibility of new employees –also known as *E*-Verify mandates. *E*-verify allows employers to screen newly hired workers for work eligibility and, therefore, could limit the employment and earning opportunities of undocumented migrants. The employer introduces the biographic information (name, social security number, date of birth, citizenship and alien registration number) of the new worker into an online computer system. The system examines the information in the dataset from the Social Security Administration (SSA) and from the Department of Homeland Security (DHS) and, subsequently, determines whether the worker is authorized to work in the United States. In the cases for which work eligibility is not confirmed, the employer receives a "tentative non-confirmation" and the worker needs to correct the problem within eight business days. *E*-Verify expanded rapidly after it was first adopted. Specifically, the number of employers participating in the program grew by more than 400% between 2001 and 2014 alone from 1064 to 482,692 (Department of Homeland, Security, 2014).

3. Conceptual framework: immigrant fertility and immigration enforcement

In recent years, researchers have documented a reduction in fertility rates in the United States, especially among the foreign born population (Livingston and Cohn, 2012). As noted in the Introduction, within immigrants, Hispanic women and, in particular, Mexican foreign-born women have exhibited the largest decline. While some of this decline might have been associated to the slowdown of the economy during the Great Recession, our focus is on the role that the intensification of immigration enforcement might have played on women's childbearing, which we isolate through various robustness and identification checks.[14]

Tougher immigration enforcement might directly inhibit fertility through various, often overlapping, policy channels and mechanisms. The *deportation* of the household's head or her/his partner (with deportations being attributed to police-based enforcement) is likely to either end or place fertility on hold. Yet, even among intact households, fertility might decline in response to a *reduction in family income* and/or *uncertainty* about the future of the family unit and the ability to raise children. Both can occur as a result of more restrictive hiring practices (as in the case of employment-based policies like *E*-Verify mandates), or from apprehension and deportation fears accompanying intensified police-based enforcement. In this vein, prior work has shown how employment verification mandates curtail the job opportunities and, in turn, the earnings of likely undocumented immigrants (*e.g.* Amuedo-Dorantes et al., 2018; Amuedo-Dorantes and Bansak, 2012; Bohn and Lofstrom, 2013). Likewise, some authors have pointed out how tougher immigration laws can increase fear of apprehension

[14] An important distinction between reductions in childbearing during economic slowdowns and those associated to tougher immigration enforcement is that, while women foreseeing harsher economic times common to all women might plan the former, the latter are exclusive to undocumented women due to their legal status. Our concern is on these fertility reductions by likely undocumented women who, to the extent that they have always lacked access to public health care, are also not likely to be associated to greater access to contraceptive services.

and deportation, leading families to adopt a style of life that restricts their access to employment opportunties, as well as services –including food stamps or Medicaid, even if they qualify for the assistance (Amuedo-Dorantes et al., 2013, 2016; Watson 2014).

However, stepped-up enforcement could also motivate undocumented immigrants to have their children while still in the United States to ensure they will get U.S. citizenship and gain access to a wide range of better life opportunities they would lack elsewhere. This view is related to the so-called "anchor baby" hypothesis, according to which undocumented migrant parents might choose to have their children while in the United States with the hope they might be able to sponsor them in the future.[15]

We can formalize the aforementioned hypotheses using a standard model of consumer demand in which fertility choices are made under a set of constraints. In particular, the demand for children can be modeled as a function of household income, the cost of children and parents' taste for children. More formally, we can assume that parents seek to maximize a utility function given by:

$$U = U(n, s) \tag{1}$$

Utility depends on the number of children n and all other consumption items –labeled s. In this simple model, parents maximize Eq. (1) subject to the following budget constraint:

$$I = \pi_s \, s + p_n n \tag{2}$$

where I is household income, p_n is the per unit price of children, and π_s is the per unit price of the composite commodity. Therefore, the demand function for children is directly related to household income and inversely related to the price of children and household income. Taking the price of the composite good as numeraire, we can express the demand of children as:

$$n = N\left(\frac{I}{p_n}\right) \tag{3}$$

Using this simple framework, we can foresee how fertility might respond to changes in immigration enforcement. For example, intensified enforcement may raise the per unit price of children if parents now evade health care and public benefits for fear they might be identified (Watson 2014), or by negatively impacting the mental and physical health of family members.[16] Assuming that children are normal goods, an increase in the cost or price of children will lead to a reduced demand for children.

The effect of intensified immigration enforcement operating through employment restrictions, as those directly placed by employment verification mandates or indirectly by intensified police-based enforcement that induces migrants to live in the shadows is, however, vaguer. Fewer employment opportunities resulting from E-Verify mandates or from fear to expose oneself to the police (*e.g.* driving to work, working illegally, etcetera), might lower household income, resulting in income and substitution effects working in opposite directions. On one hand, under the assumption that children are normal goods, a lower purchasing power might lead to an income effect that reduces the demand for children. On the other hand, fewer employment opportunities lower the opportunity cost of not working and, in turn, the cost of staying home to raise children. This substitution effect would result in an increased demand for children. In sum, the final impact of restricted employment opportunities on the demand for children remains an empirical question.

Similarly, stepped-up enforcement could raise fertility among undocumented immigrants while they are in the United States if they wish to ensure their children will get U.S. citizenship and, therefore, access to a wide range of better life opportunities they would lack elsewhere. Yet, following Becker and Lewis (1973) and Becker and Tomes (1976) "quality-quantity" trade-off hypothesis, Avitabile et al. (2014) find that the granting of birth right citizenship in Germany lowered the cost or price of child "quality", leading parents to lower their demand for children and increase, instead, their investments in child "quality." In their case, parents maximized a utility function, which not only depended on the number of children and the consumption of other goods, but also on child quality. In our case, having the children in the United States guarantees them citizenship, possibly lowering the cost of child quality. Following Becker and Lewis (1973), a decrease in the price of child quality has a positive direct effect on child quality, but a negative indirect effect on the number of children due to the increase in the shadow price of quantity. As a result, it might reduce fertility.

Overall, then, whether fertility declines or rises in the midst of intensified immigration enforcement remains an empirical question with potential distinct answers due to large heterogeneity in migrant women. In what follows, we examine that relationship.

[15] Ignatow and Williams (2011) note how the main source of this term is partisan news websites. In reality, however, this is not an immediate possibility. Children cannot sponsor parents until they turn 21 years old. In addition, the parents need to have entered the country legally. Otherwise, they will need to return to their home countries and initiate the application process, while enduring a 10-year re-entry ban and additional penalties.

[16] While the ACS is not ideal to gauge changes in mental or physical health, the public health, psychology and sociology literatures have documented how intensified enforcement has increased fear of deportation and, in turn, immigrants' health care access and health (*e.g.* Cholera et al., 2021; Hacker et al., 2011; Martinez et al., 2015; Ornelas et al., 2019; Stutz et al. 2019). These damaging impacts have also been underscored by the economics literature and the press (e.g. Alsan and Yang, 2019; Sanchez, 2019).

4. Data

We use two different datasets in our analysis. The first one consists of data from the 2005 through 2014 waves of the American Community Survey (ACS) provided by the Integrated Public Use Microdata Series (Ruggles et al., 2017). It focuses on a period of rapid growth and expansion of in interior immigration enforcement for which detailed information on the Metropolitan Statistical Area (MSA) where respondents reside is available. The second dataset consists of data gathered on the adoption of the interior immigration enforcement measures discussed earlier –namely: local and state level 287(g) agreements, Secure Communities, employment verification mandates and omnibus immigration laws.

4.1. ACS data

Our main source of data is the ACS, which provides rich demographic, social, economic, and housing information about individuals and the households to which they belong. Approximately 3.5 million randomly sampled households are interviewed on a yearly basis. In addition to its size, the ACS allows us to exploit the temporal and local variation in immigration policies over the period under consideration by consistently identifying the metropolitan area (MSA) where women live.[17] It also gathers information about ethnicity and citizenship status –key traits, along with educational attainment and time in the United States, in predicting the likely undocumented immigration status of respondents.

To understand how undocumented immigrants are included in the data, it is important to note a few facts about the ACS sampling. First, the ACS relies on the Master Address File (MAF) –namely, the Census Bureau's official inventory of known housing units in the United States, to generate a sample frame. Next, addresses are randomly drawn from that sample frame. The data is then collected via internet, mail, telephone, and personal visits. The household receives a mailed request to respond via internet, with an option to complete a paper questionnaire and return by mail.[18] If there is no response after one month, the Census Bureau follows up with computer-assisted telephone interviews. If there is still non-response, the address is selected for computer-assisted personal interviewing. According to the Census Bureau, the response rate is above 95%.

A logical concern when studying likely undocumented immigrants is being able to proxy for their unauthorized status, since this information is not collected by any representative official dataset. Specifically, while the ACS asks the individual whether she/he is an U.S.-citizen, no information regarding their specific immigrant visa or legal status is gathered. Researchers have used a variety of methods to proxy for the likely undocumented status of immigrants. While we experiment with three different methodologies, all of which derive consistent results as we show later on, we proxy for immigrants' likely undocumented status using a series of demographic traits shown to be good predictors of immigrants' undocumented status over the time span covered in this study –namely, being non-citizen, low-skill (with less than a high school diploma), long-term resident (with 5 years or more of U.S. residency) and Hispanic. Why are these traits good predictors of their undocumented immigration status during this period? The Census Bureau and the Department of Homeland Security estimate that nearly 40% of non-citizens are authorized immigrants (Acosta et al., 2014; Baker and Rytin, 2013). In addition, as previous research has pointed out (see for example, Bohn and Pugatch, 2013; Orrenius and Zavodny, 2016), most unauthorized immigrants have relatively low educational levels.[19] Furthermore, due to the closeness and the presence of a large migrant network, more than sixty-seven percent of undocumented immigrants in the United States are from Central America. Hence, we follow the convention in the literature of adopting Hispanic non-citizens as a proxy for individuals who are likely to be undocumented (Passel and Cohn, 2009; Pope, 2016; Orrenius et al., 2016; Bohn and Pugatch, 2013). Nevertheless, to address any concerns regarding the possibility that the sample might include low-skilled immigrants or college students with non-immigrant visas, we restrict our analysis to Hispanic non-citizen individuals who have not completed high school and who have lived in the United States for at least 5 years. This last restriction further ensures that the low-skilled migrants are not legally in the United States with a non-immigrant visa –typically granted to low-skilled migrants for a much shorter duration. Using these traits, along with the weights of the ACS, we obtain an estimated unauthorized immigrant population of 12,791,033 immigrants –a figure that is very close to the estimated population of 11 to 12 million undocumented immigrants in the United States over the sample period under consideration using the residual method. According to the more elaborate aggregate estimates from the Center for Migration Studies (CMS), that number was 11,010,000 immigrants over the time span we focus on –a fairly close estimate considering the CMS advertence that: "Estimates are shown for unauthorized population sizes of 1000 or more. All the estimates are rounded to 1000 s. The sum of the numbers for the countries is not likely to agree with the U.S. totals because estimates of fewer than 1000 are not included in the table."

As noted earlier, as a robustness check, we also experiment with using two alternative methodologies to proxy for the likely undocumented status of women in our sample: (1) a residual approach, initially proposed by Passel et al. (2014) and

[17] We focus on the period 2005–2014 because: (1) it captures one of intensified immigration enforcement, and (2) it is a period over which we can consistently identify MSAs. MSAs are integrated by a large urban core and surrounding communities that have a high degree of economic and social integration with the urban core.

[18] The households are contacted and interviewed in English and Spanish.

[19] About three-quarters of adult unauthorized immigrants have no more education than a high school diploma (Passel and Cohn, 2009).

Table 1
Summary statistics.

Descriptive Statistic:	Mean	S.D	Min	Max	Observations
Panel A: Dependent Variable					
Probability of Childbearing	0.09	0.28	0	1	106,033
Panel B: Individual Characteristics					
Age	32.39	7.73	15	45	106,033
Married	0.57	0.49	0	1	106,033
Number of Own Children Under 5 in the Household	0.4	0.66	0	7	106,033
Years in the United States	13.41	6.31	5	45	106,033
Years of Education	7.28	3.27	0	12	106,033
Panel C: Area Characteristics					
TANF	0.56	0.5	0	1	106,033
CHIP	0.84	0.37	0	1	106,033
Food Stamp	0.44	0.5	0	1	106,033
Unemployment Rate	0.08	0.02	0.02	0.14	106,033
Panel D: Enforcement Index					
Enforcement Index	0.82	0.93	0	5	106,033
Enforcement Index using Historical Location	0.07	0.13	0	1.38	106,033
Police-based/Deportation Policies	0.74	0.79	0	4	106,033
Employment Restrictive Policies	0.078	0.26	0	1	106,033
State Level Policies	0.18	0.53	0	3	106,033
Local Level Policies	0.65	0.67	0	2	106,033

Notes: Sample: Hispanic, non-citizen, low-skilled women with 5+ years of residency in the United States. Data from ACS 2005–2014.

subsequently applied by others (*e.g.* Borjas 2017);[20] and (2) a statistically imputed unauthorized status (*e.g.* Van Hook et al., 2015, among others). As we will show, our findings prove robust to the use of these alternative ways of predicting immigrants' legal status.

In addition, to address concerns of differential response rates among noncitizen women (Johnson and Dye 2005; Hook and Bachmeier, 2013), we perform various checks. *First*, we examine the population weights for our group of likely undocumented immigrants to assess the extent of survey non-response among our population of interest over the period under examination. If non-response rose, the weights should have risen, other things equal. Yet, an initial inspection reveals that the weights remained stable over the period under study –a result in line with the findings from a series of studies using the ACS over the 2000 through 2014 period in order to assess non-response rates or the loss of representativeness of the ACS following the intensification of enforcement (see, for example, Bohn et al., 2014; Pope, 2016; Orrenius and Zavodny, 2016). *Secondly*, we check whether samples being collected show different characteristics in MSAs with higher enforcement. If likely undocumented women are significantly less likely to respond in MSAs with stricter enforcement, we should observe a higher citizen to non-citizen ratio in those MSAs, when compared to MSAs with less enforcement. Yet, as we show in Table 5, those shares do not differ with the level of immigration enforcement, suggesting that the response rate of noncitizen women does not significantly differ based on the intensity of immigration enforcement.[21]

Finally, because our interest is on fertility, we focus on women ages 16 to 45 years old, and use the following ACS question: "*Had you given birth to any children in the past 12 months?*" to create a dummy variable that takes the value of 1 if the answer is 'yes' and 0 otherwise. While the ACS is rich in information regarding other individual traits needed for this analysis, such as citizenship, an important limitation when measuring fertility is the lack of precise information on when the birth occurred and its gestation period, *i.e.* if it was premature or not. We only know if the person indicated having a child in the past 12 months. Since most births in any given year are likely conceived about one year prior due to the 40-week average gestation period, we use the level of interior immigration enforcement from one year prior and adjust the rest of the regressors in our analysis accordingly.[22]

Table 1 provides summary statistics for the key traits of women in our sample. We work with a sample that contains 106,033 likely undocumented women (namely, Hispanic non-citizen women who have less than a high school diploma and have lived in the United States 5 years or more) living in the United States between 2005 and 2014. Approximately 9% of

[20] According to this approach, a person is deemed to be legally in the United States in s/he satisfy any of the following criteria: arrived before 1980, has U.S. citizenship, receives public benefits, works in the government sector, was born in Cuba, has an occupation that requires licensing, or has a spouse who is a legal immigrant or U.S. citizen. Everyone else is likely undocumented.

[21] In addition, in separate analyses available from the authors, we look at whether basic demographic traits of women within a given MSA, such as their age, marital status, or educational attainment, appear to have changed on account to intensified immigration enforcement. We find no evidence of such a pattern.

[22] The regressors used in the analysis are detailed in Table B in the Appendix. They include age, marital status, number of children less than 5 years of age, years in the United States, educational attainment, as well as some MSA and state level controls. We assume that educational attainment, marital status and the MSA of residence did not change and adjust the remaining regressors accordingly. Because the share of movers is not large, and the vast majority of moves occur locally within the same MSA (*e.g.* https://www.valuepenguin.com/2019/11/metro-migration-patterns), the results prove robust to excluding movers from the sample (see: Panel A of Table 6).

them reported giving birth in the past 12 months. They were, on average, 32 years old and 57% were married. On average, these women had been in the United States for more than one decade and had approximately 7 years of schooling (education ranges from 0 years of schooling to 12 years of school, but no high school diploma). About 40% had children less than 5 years of age. In addition, Table 1 includes some time-varying state level characteristics, such as the share of likely undocumented women in the sample residing in states that expanded Temporary Assistance for Needy Families (TANF), Children's Health Insurance Program (CHIP) or Supplemental Nutrition Assistance Program (SNAP) to 'unqualified immigrants' based on the federal law.[23,24,25]

4.2. Interior immigration enforcement data

To gauge the impact of intensified immigration enforcement on the fertility of likely undocumented women, we rely on the geographic and temporal variation in the adoption of the interior enforcement initiatives detailed in Section 2. Specifically, we gather historical and current data on the implementation of 287(*g*) agreements at the state level from the ICEs 287(*g*) Fact Sheet website, from Amuedo-Dorantes and Bansak (2014), and from Kostandini et al. (2013).[26] Data on the rolling of the Secure Communities program at the county level is compiled from ICE's releases on activated jurisdictions.[27] Finally, data on state level omnibus immigration laws and employment verification mandates is gathered from the National Conference of State Legislatures.[28]

Our purpose is to gauge the impact of intensified immigration enforcement on the childbearing likelihood of likely undocumented women.[29] Since some of the aforementioned enforcement initiatives are adopted at the county level, it could be the case that a particular county in the MSA activates a 287(*g*) agreement, whereas other counties in the MSA do not. In those instances, some of the women in the MSA might be affected by the measure, whereas others might not. In addition, some of the measures might have been in place for only a few months each year if they were activated midyear. To address these issues, we construct a population-weighted index that provides a few advantages. *First*, the index provides a tractable way of gauging the impact of the tougher climate created by the diversity of interior immigration enforcement initiatives we consider herein. Still, in subsequent heterogeneity analyses, we distinguish between police-based and employer-based enforcement measures given the distinct resources they rely upon (police *vs*. employers), as well as their different consequences.

Second, as we shall describe in what follows, the index not only addresses the varying geographic coverage of the enforcement measures (some at the county level, others at the state level) through the construction of a population weighted measure of immigration enforcement but, in addition, accounts for the number of months each measure was in place in a particular year. In that manner, it allows us to capture the depth and intensity of immigration enforcement in each MSA to which fertility is more likely to respond to, as opposed to only whether a particular interior immigration enforcement was in place.

Third, immigration enforcement is an interconnected system administered by various federal, state, and local authorities and agencies with similar missions. The measures are often correlated. The index allows us to better gauge the impact of a toughened immigration enforcement climate, while accounting for the interconnectedness of the various measures.

Hence, we first calculate the following index for each initiative *k*:

$$EI_{mt}^{k} = \frac{1}{N_{m,2000}} \sum_{c \varepsilon m}^{m} \frac{1}{12} \sum_{j=1}^{12} 1\left(E_{j,\ c}\right) P_{c,2000} \tag{4}$$

[23] Undocumented immigrants have never qualified for federally funded assistance. The 1996 Personal Responsibility and Work Reconciliation Act (PRWORA) restricted access for lawfully present immigrants based on their immigration status, date of arrival and length of U.S. residency. PRWORA also set parameters for how states might administer public benefit programs. Some states extended coverage to immigrant groups that were not covered by federal law. Although coverage was never extended to undocumented immigrants, more than half of undocumented migrants reside in mixed-status households with legal migrants or U.S. citizens (*e.g.* Bolter et al., 2021) who, in turn, might enjoy access to such benefits. Because of the complex welfare eligibility dynamics and ways in which undocumented immigrants might partake in some of those benefits, we use simpler indicators reflective of whether the state expanded its coverage above and beyond the one offered federally. Similarly, we include an indicator for whether the state offers CHIP to lawfully residing youth less than 21 years of age (a group that would include U.S. born children of undocumented mothers), and to pregnant women.

[24] This program was formerly known as the Food Stamp program.

[25] In alternative model specifications, we also include MSA unemployment rates, the share of Hispanics in the MSA and information on the share of the electorate voting for Republican candidates for the U.S. House of Representatives –all measured prior to the rollout of interior immigration enforcement and interacted with a time trend.

[26] Since the ICE website contains only a list of the current active agreements, we review old websites and prior research using these agreements to ensemble a complete dataset spanning over the period under consideration. Once we have the start date of each 287(g) agreement, we calculate the period of time during which these agreements have been in place.

[27] See: https://www.ice.gov/doclib/secure-communities/pdf/sc-activated.pdf

[28] See: http://www.ncsl.org/documents/statefed/omnibus_laws.pdf

[29] It is worth noting that the index is a proxy of the intensity of immigration enforcement to which respondents in a particular MSA might be exposed to. At the end of the day, the true intensity of any enforcement measure will inevitably vary across jurisdictions as each one is different and might implement alike measures more or less strictly depending on who is in charge of its implementation or other unobserved local traits. To address that limitation, we include area fixed-effects as well as area-specific time trends intended to capture such idiosyncrasies.

where $1(E_{j,\ c})$ is an indicator function that informs about the implementation of measure k in county c in month j during the year in question, $P_{c,2000}$ is the population of county c according to the 2000 Census –that is, prior to the rolling of the enforcement initiatives being considered; and $N_{m,2000}$ is the total population in the MSA.[30] Subsequently, we compute an index of the overall enforcement level to which a woman living in MSA m and time (year) t is exposed as the sum of the indices for each enforcement initiative at the (MSA, year) level in Eq. (4). That is:[31]

$$\text{Total Enforcement}_{m,t} = \sum_{k\varepsilon K}^{K} EI_{m,t}^{k} \tag{5}$$

As can be seen from Table 1, our proxy for the intensity of interior immigration enforcement averages 0.82 and fluctuated significantly between 0 (*i.e.* no enforcement) and 5 (all local and state level initiatives) over the years under consideration. Fig. B in the Appendix exemplifies its *geographic* variation with a growing number of MSAs joining in and adopting interior immigration initiatives during that period. In addition, Fig. C in the Appendix illustrates the *temporal* variation in the intensity of the constructed immigration enforcement index as MSAs adopted multiple enforcement measures. Finally, the intensification of immigration enforcement captured by Figs. B and C overlaps with the increase in interior removals displayed in Fig. A, confirming prior report findings on the role of the examined immigration enforcement initiatives on removals (Kandel, 2016).[32]

5. Empirical strategy

Our main aim is to evaluate how fertility decisions of likely undocumented women might be affected by the intensification of immigration enforcement. To that end, we exploit the temporal and geographic variation in the immigration enforcement index described above using the following benchmark model:

$$y_{i,m,t} = \alpha + \beta_1\ Total\ Enforcement_{m,t} + X'_{i,m,t}\ \beta_2 + Z'_{m(s),t}\beta_3\ +\ \gamma_m +\ \theta_t +\ \gamma_m t + \varepsilon_{i,m,t} \tag{6}$$

where $y_{i,m,t}$ is a dummy variable that is equal to 1 if woman i, living in MSA m had a child in year t. $Total\ Enforcement_{m,t}$ is the index serving as a proxy for the intensity of enforcement climate to which a woman living in the MSA m and year t might be exposed.

The vector $X'_{i,m,t}$ includes a range of individual characteristics known to influence the fertility decisions, such as age,[33] marital status, number of children, years in the United States and years of schooling. The literature has suggested that Mexican immigrants' higher fertility rates are attributable to some degree to the fact that many female Mexican immigrants would have entered the United States to reunite with their migrant husbands in response to favored family reunification (Parrado, 2011, Raley and Sweeney, 2009). Hence, we control for marital status. In addition, we include the number of children less than 5 living with the mother (Falasco and Heer, 1984), as well as the number of years living in the United States, since those who have been in the country longer might exhibit fertility patterns that look more like those of natives (see, for example, Goldstein and Goldstein, 1981). Finally, we control for educational attainment given the inverse relationship between years of education and fertility among Hispanic women (Parrado and Morgan, 2008).[34]

The vector $Z'_{m(s),t}$ contains specific MSA- and state-time varying characteristics that might affect the decision of having a child, such as local unemployment rates and the generosity of welfare benefits in the state. Specifically, since non-citizen women's childbearing could prove responsive to the generosity of welfare benefits (Amuedo-Dorantes et al., 2016), we include information on whether the state offered extended access to certain public assistance programs, such as Temporary Assistance for Needy Families (TANF), Children's Health Insurance Program and Supplemental Nutrition Assistance Program (SNAP).[35]

To conclude, Eq. (6) also includes MSA fixed effects (γ_m) and year fixed effects (θ_t) to control, respectively, for unobserved time invariant MSA characteristics (*e.g.* residing in areas less welcoming to immigrants) and for aggregate level shocks potentially impacting immigrant fertility (*e.g.* economic downturns). Additionally, we examine the sensitivity of our results to the inclusion of MSA-specific time trends. The latter account for differences in fertility trends rates across MSAs driven by other unaccounted policies or macroeconomic factors not captured by the vector $Z'_{m(s),t}$. Standard errors are clustered at the MSA level.[36]

[30] For example, if an MSA is comprised of 2 counties, the index for each of the k enforcement measures being considered (for instance, Secure Communities) in MSA m in year t would be given by: $Secure\,Communities\,index_{mt} = \frac{Months\_Coverage}{12} * \frac{Cnty\,1Pop}{MSAPop} + \frac{Months\_Coverage}{12} * \frac{Cnty\,2Pop}{MSAPop}$

[31] Where k refers to each policy, *i.e.*: 287(g) local, 287(g) state, Secure Communities (SC), omnibus immigration laws and E-Verify.

[32] In subsequent analysis, we also experiment with alternative indices that group the various enforcement initiatives according to the entity involved in their application (*i.e.* police-based or employer-based measures). The grouping makes sense since many of the policies, as was the case with the 287(g) and the Secure Communities program, were designed to progressively replace each other and their goals and involved police actions are alike.

[33] Our results prove robust to the inclusion of age as a continuous variable vs. as group dummies.

[34] Income is not included due to its endogenous nature. Instead, we control for educational attainment, which is correlated with income.

[35] A detailed description of these regressors, along with all other variables, can be found in Table B in the Appendix.

[36] Results prove robust to clustering at the state level and to the use of state-specific linear time trends.

Table 2
Childbearing among likely unauthorized women.

Regressors	Model Specification		
	(1)	(2)	(3)
Enforcement Index	−0.005***	−0.005***	−0.005**
	(0.002)	(0.002)	(0.002)
Age	−0.004***	−0.004***	−0.004***
	(0.000)	(0.000)	(0.000)
Married	0.018***	0.018***	0.018***
	(0.002)	(0.002)	(0.003)
Number of Own Children under 5 in the Household	0.248***	0.248***	0.248***
	(0.005)	(0.005)	(0.005)
Years in the U.S.	−0.001***	−0.001***	−0.001***
	(0.000)	(0.000)	(0.000)
Years of Education	−0.0003	−0.0003	−0.0003
	(0.0003)	(0.0003)	(0.0003)
TANF	−0.007	−0.005	−0.006
	(0.005)	(0.004)	(0.004)
CHIP	0.037***	0.034***	0.036***
	(0.009)	(0.007)	(0.008)
Food Stamp	−0.015	−0.013	−0.015
	(0.017)	(0.015)	(0.015)
Unemployment Rate		0.325***	0.166
		(0.121)	(0.292)
Year FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
MSA-trends	No	No	Yes
D.V. Mean	0.09		
Observations	106,033	106,033	106,033
R-squared	0.125	0.125	0.129

Notes: Sample: Hispanic, non-citizen, low-skilled women with 5+ years of residency in the United States. All model specifications include a constant term. Standard errors are shown in parentheses and are clustered at the MSA level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The coefficient of interest is β_1, which captures the relationship between the intensity of local and state-level immigration enforcement and the childbearing likelihood of likely undocumented women. A negative coefficient would be consistent with the prediction that tougher enforcement curtails fertility among likely undocumented women.

6. The response of immigrant fertility to immigration enforcement

6.1. Main findings

Table 2 displays the results from estimating Eq. (6) for the sample of likely undocumented women, as captured by Hispanic non-citizen women with less than a high school education and at least 5 years of residency in the United States, using ordinary least squares (OLS). We estimate several specifications that progressively add controls to assess the robustness of our findings to the inclusion of potentially endogenous controls, such as MSA unemployment rates in specification (2). We also display the results with and without MSA-specific time trends to address any concerns regarding their inclusion in the model (*i.e.* specifications (2) and (3)).[37] Regardless of the model specification used, the intensification of immigration enforcement appears to have had a negative impact on the childbearing likelihood of likely undocumented women. Focusing on the most complete model specification, we find that a one standard deviation increase in the enforcement index (approximately equal to the average intensity of interior immigration enforcement during the period under consideration) lowers the childbearing likelihood of likely undocumented women by 5%.[38] As such, tougher interior immigration enforcement could be responsible for one fifth of the fertility reduction experienced by these women over the period under consideration.

To put the abovementioned policy impact in perspective, it is helpful to discuss the remaining coefficient estimates in Table 2, which exhibit the expected signs. For example, there is an inverse relationship between the age of the mother and the likelihood of childbearing –women one year older are 4% less likely to have a child. The opposite is true with regards to married women –who are 20% to have a child than unmarried women, and with women with more children under the age of 5. We also find that women who have been living longer in the United States are less likely to have had a child during

[37] In alternative model specification, we experiment with adding country-of-origin fixed effects. Estimates, available from the authors, prove robust to the addition of such controls.

[38] The standard deviation of the enforcement index is 0.93 and, on average, approximately 9% of likely undocumented women gave birth in the past year. Therefore: {[(−0.005)*0.93]/0.09}=0.05 or 5%.

Table 3
Probability of childbearing of naturalized and native women – placebo checks.

Regressors	Model Specification		
	(1)	(2)	(3)
Panel A: Probability of Childbearing of Naturalized Women			
Enforcement Index	−0.004	−0.004	0.001
	(0.004)	(0.005)	(0.005)
Individual Characteristics	Yes	Yes	Yes
Welfare Programs	Yes	Yes	Yes
Area Characteristics	No	Yes	Yes
Year FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
MSA-trends	No	No	Yes
D.V. Mean		0.06	
Observations	19,556	19,556	19,556
R-squared	0.164	0.164	0.178
Panel B: Probability of Childbearing of Native Women			
Enforcement Index	−0.002	−0.002	−0.001
	(0.002)	(0.002)	(0.003)
Individual Characteristics	Yes	Yes	Yes
Welfare Programs	Yes	Yes	Yes
Area Characteristics	No	Yes	Yes
Year FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
MSA-trends	No	No	Yes
D.V. Mean		0.07	
Observations	98,467	98,467	98,467
R-squared	0.140	0.145	0.145

Notes: **Panel A:** *Sample*: Hispanic naturalized low-skilled women with 5+ years of residency in the United States. **Panel B:** *Sample:* Hispanic native low-skilled women. **Both Panels***:* All regressions include a constant term and the controls in the most complete specification in Table 2. Standard errors are shown in parentheses and are clustered at the MSA level. Standard errors are shown in parentheses and are clustered at the MSA level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

the past year, possibly as their childbearing patterns assimilate to those of natives.[39] Finally, other state level policies, such as CHIP, appear to be highly linked to likely undocumented women's fertility, which is 40% higher if they reside in states extending the Children's Health Insurance Program to lawfully residing children and to all pregnant women.

Because interior immigration enforcement took off during the Great Recession years, one might be concerned that much of the impact attributed to the intensification of immigration enforcement was truly due to the recessionary economy. After all, poverty and unemployment grew more rapidly among Latinos (Livingston and Cohn, 2012). Although some of the model specifications include year fixed-effects, MSA unemployment rates and/or MSA-specific time trends addressing such a concern, we also experiment with re-estimating Eq. (6) using two other samples of also Hispanic low-skilled women as placebos checks, namely: naturalized and U.S.-born women who do not have likely undocumented partners. Given their citizenship status, they should not have been affected by the intensification of immigration enforcement to the same extent of their likely undocumented counterparts.

Results from those estimations are displayed in Table 3 under Panels A and B. If, indeed, the impacts found in Table 2 were the by-product of tougher economic times, we should be able to find a statistically significant impact of intensified enforcement on the childbearing likelihood of these two other samples of women. Yet, the estimates in Panels A and B of Table 3 clearly reveal the lack of a statistically significant impact of intensified immigration enforcement on these women's childbearing likelihood. The estimates are also statistically different from those in Table 2. [40] In sum, the impacts identified in Table 2 are unique to likely undocumented women, even if similarly skilled Hispanic and, in some instances, also foreign-born women were also impacted by the Great Recession.

In Table 4, we conduct further checks to address any remaining concerns regarding the confounding role of the economic downturn during part of the period being examined. First, as other studies including the Great Recession in their data

[39] Table C in the Appendix sheds more light on these relationships. Because the typical age-at-migration is in the late teens-early twenties, and most childbearing takes place when women are in their twenties/thirties, it is not surprising to find the curtailing effect of immigration enforcement concentrated in women 25–34 years of age, who have typically been in the United States between 5 and 10 years and who are less likely to be married than their older counterparts.

[40] With: Prob > Chi2=0.0084 when comparing the estimates from Table 2 and Panel A in Table 3, and with a Prob > Chi2=0.0716 when comparing the estimates in Table 2 and Panel B in Table 3.

Table 4
Robustness checks addressing confounding economic factors.

Column:	(1)	(2)	(3)	(4)
Robustness Check:	Including House Price Trend	Excluding 2008	Excluding 2008 and 2009	Removals as D.V.
Enforcement Index	−0.006*	−0.007**	−0.006*	
	(0.003)	(0.003)	(0.004)	
Unemployment $Rate_{t-1}$				−0.002
				(0.073)
Individual Characteristics	Yes	Yes	Yes	
Welfare Programs	Yes	Yes	Yes	
Area Characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
MSA Trends	Yes	Yes	Yes	Yes
Mean D.V.	0.09	0.09	0.09	0.25
Observations	59,100	95,739	84,417	15,702
R-squared	0.127	0.120	0.123	0.448

Notes: All regressions include a constant term and the controls in the most complete specification in Table 2. Column (1) controls for trends in housing prices, *i.e.* average prices from 2000 to 2004 (prior to our period of study)*time trend. House price data, which are collected from https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx#atvol, are not available for all MSAs, explaining the reduced number of observations in that model. Columns (2) and (3) exclude the year of the Great Recession, as well as the year after. Standard errors are shown in parentheses and are clustered at the MSA level. Column (5) uses removals per 1000 individuals as the dependent variable.

analysis (*e.g.* Charles et al., 2018), we experiment with controlling for housing prices in our model specification. Doing so helps address the housing bust associated to declines in employment in the construction sector –a sector that thrived in states with larger shares of undocumented migrants (*e.g.* Arizona) and one that particularly suffered during the Great Recession. Even though the sample drops due to the lack of information on housing prices for all MSAs in the original sample, the estimate in column (1) of Table 4 is largely similar to the on in Table 2, which is not surprising given that the model already accounts for MSA-specific time trends capturing any location specific economic circumstances.

Next, we exploit the fact that the period of analysis, just as the adoption of interior immigration enforcement, both expand beyond the Great Recession. As such, we are able to experiment with excluding the year of the Great Recession (for babies conceived and delivered during that year), as well as the year after (for babies conceived during that year and delivered during the next year). As shown in columns 2 and 3 of Table 4, our findings remain qualitatively unchanged. If we eliminate the year of the Great Recession, the estimated impact is identical in magnitude and statistical significance. If we further eliminate 2009 (*i.e.* exclude both 2008 and 2009), we lose some precision in our estimate likely due to the smaller sample size, but the estimated impact is largely unchanged and still statistically different from zero at the 10% level.

Finally, we assess if interior removals –the embodiment of intensified immigration enforcement– are correlated to business cycles. To that end, we gather data on county level deportation from TRAC and examine their correlation to local business cycles as captured by local unemployment rates. As shown in column 4 of Table 4, we find no evidence of deportations being higher in localities with a worse economy, for example. This is not surprising, as we shall further discuss in identification checks that follow. Many states adopted tougher measures either before or after the Great Recession, not necessarily during the downturn. For instance, Florida was the first state to adopt a state-wide 287(*g*) agreement with ICE and did so in 2002, well before the Great Recession. In contrast, Arizona passed the infamous SB1070 in 2010 –two years after the Great Recession. Furthermore, the rollout of Secure Communities –the most significant contributor to the immigration enforcement index since it was the one to reach nationwide coverage and the sole enforcement initiative starting in 2008– was conducted federally as opposed to locally. The federal rollout largely eliminated the possibility for localities with a worse economy to manipulate the program adoption –an aspect that has been, in fact, exploited for identification purposes by various studies (*e.g.* Alsan and Yang, 2019).

In sum, while economic conditions are likely to impact fertility, the inclusion of controls for time varying economic conditions at the local and state levels, as well as MSA-specific time trends to capture location specific risks in our most preferred specification in Table 2, along with: (1) The lack of an alike enforcement impact for Hispanic, foreign-born, and low-skilled women in Table 3; (2) The qualitatively similar results in Table 4 when we include further controls reflective of local economic conditions or exclude the Great Recession altogether; and (3) The lack of a significant level of correlation between removals and the business cycle at the heart of the expressed concern, are all suggestive of the estimated impact of intensified immigration enforcement is not driven by the Great Recession.[41]

[41] Identification checks in Tables D and E in the Appendix (to be discussed in the next section) will provide further reassurance of the robustness of our findings.

Table 5
Identification tests #1.

Panel A: Assessing the Existence of Parallel Pre-trends	
Dependent Variable:	Probability of Childbearing
Enforcement Index in Prior Years	
4 Years Prior to EI>0	0.003
	(0.006)
3 Years Prior to EI>0	0.001
	(0.005)
2 Years Prior to EI>0	−0.005
	(0.004)
1 Year Prior to the EI>0	0.001
	(0.003)
Enforcement Index	−0.005***
	(0.002)
D.V. Mean	0.09
Observations	106,033
R-squared	0.125

Panel B: The Role of Fertility in Explaining the Adoption of Tougher Immigration Enforcement

Dependent Variable:	Year when IE>0	Immigration Enforcement when IE>0
Average Fertility in MSA	91.550	0.187
	(128.237)	(0.208)
Collapsed Individual Characteristics	Yes	Yes
Area Characteristics	Yes	Yes
State FE	Yes	Yes
Observations	240	240
R-squared	0.321	0.476

Notes: **Panel A:** It shows the results from estimating Eq. (7) using a sample of Hispanic, non-citizen, low-skilled women who have lived in the United States 5+ years. The regression includes a constant term and the controls in the most complete specification in Table 2. **Panel B:** It shows the results from estimating Eq. (8) using 2005 ACS data collapsed at the MSA level, and including state fixed-effects. The Collapsed Individual Characteristics are the average age of likely undocumented women, share of married likely undocumented women, the average number of children of likely undocumented women, the average education level of women, and the average number of years in the United States of likely undocumented women. Area Characteristics include the share voting Republican, average unemployment rate, the share of undocumented immigrants, the share of individuals receiving TANF, CHIP and Food Stamp. **Both Panels:** Standard errors are shown in parentheses and are clustered at the MSA level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

6.2. Identification tests

The main assumption underlying our empirical strategy is that differences in the outcome being examined across treated and control units did not predate treatment itself. To address this concern, we estimate Eq. (6) including a full set of dummies spanning from four years prior to the adoption of any initiative in the MSA in question. We can then gauge if reductions in fertility preceded the adoption of tougher enforcement measures in the MSA as follows:

$$y_{i,m,t} = \alpha + \sum_{b=-4}^{-1} \delta_b D_{m,b} + \ \beta_1 \ Total\ Enforcement_{m,t} + X'_{i,m,t}\ \beta_2 + Z'_{m(s),t}\beta_3 + \gamma_m + + \theta_t + \gamma_m t + \varepsilon_{i,m,t} \tag{7}$$

where D_b is a dummy for b years prior to the enforcement index turning positive. Note that, because the adoption of these initiatives occurred at different points in time across MSAs, D_1 might be equal to 2006 for some MSAs, 2007 for others, and so on. Table 5, Panel A, shows the results from estimating Eq. (7) via OLS. It is evident that reductions in fertility did not take place prior to the adoption of tougher immigration enforcement measures in the MSA, as none of the coefficients for the preceding years are statistically different from zero. Furthermore, the point estimate on our key regressor continues to be statistically different from zero and of similar magnitude to the one in the last column of Table 2.

Another related threat to identification is whether the adoption of stricter immigration enforcement by the MSA was endogenous due the fact that many localities opted for tougher enforcement and, even when they did not (as with Secure Communities), the rollout was not accidental. Even though no policy is ever random, our concern for inference-making purposes is if the policies are correlated to fertility rates in the MSA prior to the beginning of our sample period in 2005.[42] To assess if that was the case, we take the first year in our sample and aggregate the data at the MSA level to estimate the following equation:

$$Y_m = \alpha + X'^0_m \alpha + Z'^0_m \mu + \varepsilon_m \tag{8}$$

[42] We cannot identify consistently MSAs due changes in MSA delineations before 2005.

Table 6
Identification tests #2 - assessing the non-random location of immigrants.

Panel A: Excluding Recent Movers from the Sample	
Dependent Variable:	Probability of Childbearing
Enforcement Index	−0.005**
	(0.002)
Observations	91,519
R-squared	0.121
Panel B: Assessing the Role of Immigration Enforcement in the MSA's Population Composition	
Dependent Variable:	Share of (Citizens/Non-Citizens) in MSA
Enforcement Index$_{t-1}$	0.0337
	(0.048)
Enforcement Index$_{t-2}$	−0.0206
	(0.052)
Enforcement Index$_{t-3}$	0.0004
	(0.061)
Enforcement Index$_{t-4}$	−0.0129
	(0.064)
Observations	1,237
R-squared	0.46

Notes: All regressions include a constant term and the controls in the most complete specification in Table 2. **Panel A:** Hispanic, non-citizen, low-skilled women who have lived in the United States 5+ years and reporting living in the same location over the past year. **Panel B:** Citizen/Non-Citizen Ratio by MSA and year. **Panel C:** Hispanic, non-citizen, low-skilled women who have lived in the United States 5+ years. **All Panels:** Standard errors are shown in parentheses and are clustered at the MSA level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

where Y_m is EI_m -namely, the enforcement level when the enforcement index turned positive in MSA m, or $EI\ Year_m$ -that is, the year in which the immigration enforcement turned positive in MSA m. The vector $X_m^{'0}$ is the average share of likely unauthorized women between 16 and 45 years of age giving birth in MSA m one year earlier in the 2005 ACS (that is, in 2004), whereas the vector $Z_m^{'0}$ contains collapsed individual level traits, as well as the area level traits included in Eq. (6) for each MSA in the base year.[43] We estimate Eq. (8) including state fixed effects, and we cluster standard errors at the state level. The results from this exercise are displayed in Panel B of Table 5. Fertility rates at the MSA level prior to the adoption of stricter enforcement measures do not seem to play a significant role in the timing of tougher immigration enforcement or on its level when first adopted by the MSA.[44]

Finally, we might be concerned about the self-selection of undocumented migrants into localities with lesser enforcement. After all, we would expect undocumented women to be sensitive to immigration enforcement due to the inherent risk of deportation in areas with tougher enforcement. Since migrants, especially undocumented ones, are a relatively mobile population, they might move in response to the adopted enforcement measures.[45] In those instances, exposure to tougher immigration enforcement is likely endogenous, and may result in a downward biased estimate of the impact of intensified immigration enforcement on fertility.[46] To assess the degree to which our estimates might be biased due to the non-random residential choices made by undocumented immigrants, we conduct a couple of checks given the limitations inherent in each approach.

First, we re-estimate the model in Eq. (6) using, exclusively, data on women who report not moving over the past year. The location choices made by this group of women should be less likely to be contaminated by migrants' responsiveness to the toughening of immigration enforcement. However, as can be seen in Panel A of Table 6, the estimated impact of intensified immigration enforcement on fertility is in line with the one reported in Table 2; suggesting a tempered response of migrants' residential location choices to intensified immigration enforcement policies.

Second, we evaluate the possibility that immigrants may selectively decide to migrate out of MSAs that adopt tougher enforcement, thereby changing the MSA's population composition and immigrant fertility in the MSA. To assess if that appears to have been the case, we construct the share of citizen to non-citizens in each MSA and regress it on the level of

[43] We exclude from this analysis the MSAs in the state of Florida, which were the only ones that had already implemented tougher immigration enforcement measures (namely the state level 287(g) signed by Florida in 2002). Results do not seem to significantly differ, however, when Florida is included.

[44] Table D in the Appendix displays the results for the remaining coefficients. None of the variable included in the model appears to function as a strong predictor of the adoption timing of immigration enforcement or of its intensity when first adopted.

[45] Note that our concern is with regards the endogenous choice of MSA of residence. While mobility within a MSA is likely to be even more frequent, all localities in a MSA have the same level of immigration enforcement in our analysis.

[46] Another source of downward bias could be the fact that some of the women whose partners have been deported might have returned to Mexico.

interior immigration enforcement of each locality when the index first turned positive. As seen in Panel B of Table 6, there is no evidence of enforcement changing the population composition of the MSA.[47,48]

In sum, the estimate in Table 2 proves robust to various identification checks addressing the possibility of differential fertility pre-trends prior to the implementation of tougher immigration enforcement, as well as biases stemming from the non-random adoption of tougher policies or the selective residential choices made by likely undocumented immigrants.

6.3. Robustness checks

We next perform additional checks assessing the robustness of our findings to two additional concerns when examining the responses by likely undocumented immigrants to tougher immigration enforcement: (1) how unauthorized immigrants are identified in the sample, and (2) the possibility that the findings may be driven by a few MSAs.

First, we experiment with two alternative methodologies to proxy for immigrants' unauthoriszed status: (a) a residual method approach, and (b) statistical imputation methods. Residual methodologies rely on the method initially proposed by Passel et al. (2014) and subsequently applied by others (*e.g.* Borjas, 2017). According to that latter, a person is deemed to be legally in the United States if s/he meets any of the following criteria: arrived before 1980, has U.S. citizenship, receives public benefits, works in the government sector, was born in Cuba, has an occupation that requires licensing, or has a spouse who is a legal immigrant or U.S. citizen. Everyone else is likely undocumented.

Statistical imputation methods use "donor samples" containing information on immigrants' legal status, to derive out-of-sample predictions of migrants' legal status. Unfortunately, most datasets are not representative of the immigrant population. One exception is the Survey of Income and Program Participation (SIPP), which has been deemed to be representative of the immigrant population and used as a donor dataset to infer the legal status of immigrants in other datasets (*i.e.* target datasets). However, the most recent module containing information on immigrants' legal status refers to 2008 –that is, prior to the rollout of Secure Communities and tougher immigration enforcement. Furthermore, the SIPP is not valid for doing inferences of policy impacts at the MSA level (Hook et al., 2015), as it is the intent of the present study. Nevertheless, as a robustness check, we experiment with using these two alternative methods to identify the sample of likely undocumented migrants.[49]

Panel A in Table 7 displays the findings from these robustness checks. As can be seen therein, a one standard deviation increase in immigration enforcement continues to lower the average share of likely undocumented women having a child by 5% regardless of the methodology employed to identify the likely unauthorized.[50]

Next, we experiment with excluding from our sample a few county outliers –such as Maricopa County in Arizona (Panel B, Table 7), followed by all border counties (Panel C, Table 7). Our purpose is to gauge if our findings are exclusively driven by these counties. However, as can be seen in Panels B and C of Table 7, we continue to find impacts similar to the one in Table 2.

In sum, our results prove robust to the use of alternative proxies of immigrants' likely undocumented status, as well as to the exclusion of potential county outliers from the sample.

7. Policy channels and enforcement impact mechanisms

Thus far, we have documented how the adoption of tougher immigration enforcement at the local and state levels has contributed to the lower childbearing likelihood among likely undocumented women. In this section, we further investigate

[47] We obtain similar results if, instead, we model non-citizen growth rates. Overall, there is no consensus in the literature on how tougher immigration enforcement affects population composition. Results vary largely depending on the enforcement measure being examined (*e.g. E*-Verify mandates, 287(*g*), Secure Communities), the scope of the study (*e.g.* national, state, or local), the time period of interest, and the mobility examined (*i.e.* inter-state, intra-state, or international), among other things. For instance, Watson (2013) finds some evidence of non-citizens with a college education responding to the 287(*g*) task force program. However, the program does not induce non-citizens to exit the U.S. or deter them from re-entering from abroad once Maricopa county is excluded from the analysis, signaling the sensitivity of the results to the sample choice. Similarly, in response to the out-of-state mobility results reported by Bohn et al. (2014) in Arizona after the adoption of an *E*-Verify mandate with VAWA, Amuedo-Dorantes and Lozano (2017)) explore the destinations of Mexican non-citizens leaving the state. They find that the results vary largely depending on the states entering the synthetic control group.

[48] We also experiment with using an instrumental variable approach that proxies for what migrants' probable locations would have been in the absence of tougher enforcement. We rely on a shift-share instrument constructed using information on the distribution of migrants from prior to the rollout of interior immigration enforcement across the United States, along with information on the current level of immigration enforcement in each MSA. Because of the limitations of most instruments, we use these results to gather some insight about potential biases in the OLS estimates. The methodology and results in Table E in the Appendix suggest that the same one standard deviation increase in the enforcement index lowers the childbearing propensity of likely undocumented women by close to 8%. Therefore, the OLS estimates might constitute a lower bound of the true impact of tougher immigration enforcement on the fertility of likely undocumented women.

[49] We are grateful to Manuel Pastor and Justin Scoggins for sharing their proxies for having a likely undocumented status in the ACS using this methodology.

[50] Descriptive statistics for the samples of undocumented women constructed using the residual and the imputation methods available from the authors show that women in the two samples have alike childbearing propensities, age, marital status, no. of children, and reside in states that are similar in terms of welfare generosity. The main differences between the two samples are with regards to the longer U.S. residency, lower educational attainment, and also lower level of interior immigration enforcement to which women in the sample constructed using the residual method are exposed when compared to women in the sample constructed using the imputation method.

Table 7
Robustness checks.

Regressors	Panel A: Using an Alternative Definition of Likely Undocumented Women		Panel B	Panel C
	Residual Method	Statistical Imputation Method	Excluding Maricopa County	Excluding Border Counties
Enforcement Index	−0.005**	−0.005***	−0.006**	−0.006**
	(0.002)	(0.002)	(0.003)	(0.003)
Individual Characteristics	Yes	Yes	Yes	Yes
Welfare Programs	Yes	Yes	Yes	Yes
Area Characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
MSA Trends	Yes	Yes	Yes	Yes
Mean D.V.	0.08	0.08	0.09	0.09
Observations	126,382	194,981	103,235	99,197
R-squared	0.119	0.113	0.118	0.118

Notes: All regressions include a constant term and the controls in the most complete specification in Table 2. Standard errors are shown in parentheses and are clustered at the MSA level.

the type of policies likely responsible for the found impacts, as well as into the mechanisms through which fertility cutbacks are likely taking place.

7.1. Policy channels

Tougher immigration enforcement has had a negative impact on undocumented women's fertility. But, have all immigration enforcement measures contributed similarly to such an outcome? If not, can we identify which are the more unsettling immigration enforcement policies when it comes to their disturbance of regular fertility patterns?

To address these questions, we group alike policies, such as the ones that involve the local and state police in the implementation of immigration policy and differentiate between policies that are clearly linked to apprehensions and deportations –what we refer to as *police-based* enforcement, and policies that are not –as in the case of employment verification mandates. Both policies are likely to, for example, curtail employment opportunities –one by directly restricting the hiring of likely undocumented workers, and the other one by probably inducing them to live in the shadows to evade apprehension. Yet, they also differ with regards to their link to deportations. Unlike employment-based enforcement, police-based enforcement is clearly linked to removals and, consequently, to intensified apprehension and deportation fears. As such, the distinction between police-based and employment-based policies underscores the importance that fear of deportation –associated to police-based enforcement and its ensuing deportations. Panel A in Table 8 displays the results from this exercise. According to the estimates therein, a one standard deviation increase in police-based immigration enforcement lowers the probability of childbearing likely unauthorized women by 6%. The fact that police-based measures more directly associated to removals are the ones driving the impacts hints on the importance of deportation fears on migrants' behavior, especially since immigration enforcement does not seem to have significantly altered MSAs' population composition (Panel B, Table 6), as we would expect if the impact were driven by deportations of non-citizens.

We further differentiate the policies according to their geographic scope –namely, among local *versus* state level measures. Consistent with our findings in Panel A, the estimates in Panel B of Table 8 reveal that local policies –all of which are police-based measures– are the ones reducing the fertility of likely undocumented women. Specifically, a one standard deviation increase in local immigration enforcement lowers the probability of childbearing among likely unauthorized women by 8%.

Overall, the estimates in Table 8 underscore the importance of local, police-based interior immigration enforcement responsible for most interior removals and, therefore, feared by undocumented immigrants, in shaping their fertility.

7.2. Mechanisms

Identifying the policy channel is critical for isolating the importance that deportation fear, in addition to other factors, has in explaining the fertility of likely undocumented women amid intensified immigration enforcement. However, what can we say about the potentially overlapping mechanisms through which fertility cutbacks are taking place? What role are compositional effects (*e.g.* deportations of partners), drops in family income and overall uncertainty about the family's ability to raise its offspring have in explaining the observed reduction in fertility?

To assess which of the abovementioned triggering mechanisms might be driving the results, we address the following questions: (1) Do fertility cutbacks solely occur when a partner is no longer present, or are they also observed among intact households? (2) Are they observed across all families in the income distribution, or are they restricted to poorer

Table 8
The impact of various types of enforcement on the probability of childbearing of likely unauthorized women.

Regressors	Coefficient (S.E.)
Panel A: By Type of Enforcement Measure	
Police-based/Deportation Policies	−0.007***
	(0.003)
Employment Restrictive Policies	−0.002
	(0.006)
Panel B: By Geographic Scope of the Enforcement Measure	
Local-Level Enforcement	−0.011**
	(0.006)
State-Level Enforcement	−0.001
	(0.006)
Individual Characteristics	Yes
Welfare Programs	Yes
Area Characteristics	Yes
Year FE	Yes
MSA FE	Yes
MSA-trends	Yes
D. V. Mean	0.09
Observations	106,033
R-squared	0.125

Notes: Sample: Hispanic, non-citizen, low-skilled women who have lived in the United States 5+ years. All regressions include a constant term and the controls in the most complete specification in Table 2. Standard errors are shown in parentheses and are clustered at the MSA level.

Table 9
Probability of childbearing of likely unauthorized women by household characteristics.

Regressors	Panel A: By Spousal Presence	Panel B: By Partner Citizenship Status			Panel C: By Family Income Quartile			
	IntactCouples	Likely unauthorizedPartner	Naturalized Partner	Native Partner	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Enforcement Index	−0.007**	−0.008**	−0.005	−0.006	−0.011*	−0.003	−0.006	−0.006
	(0.003)	(0.004)	(0.009)	(0.014)	(0.006)	(0.006)	(0.005)	(0.005)
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Welfare Programs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA-trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
D.V. Mean	0.08	0.10	0.07	0.11	0.11	0.09	0.08	0.07
Observations	56,511	53,787	9628	4326	25,442	25,442	25,442	25,442
R-squared	0.180	0.160	0.194	0.228	0.125	0.159	0.140	0.127

Notes: Sample: **Panel A**: Hispanic, non-citizen, low-skilled women living with a partner and with 5+ years of residency in the United States. **Panel B:** Hispanic, non-citizen, low-skilled, married women with 5+ years of residency in the United States. **Panel C:** Hispanic, non-citizen, low-skilled women with 5+ years of residency in the United States. **All Panels:** All regressions include a constant term and the controls in the most complete specification in Table 2. Standard errors are shown in parentheses and are clustered at the MSA level.

families? and (3) Do fertility reductions take place across all types of couples, or are they more likely among couples of likely undocumented immigrants subject to greater uncertainty?

The estimates in Table 9 address these questions. Starting with Panel A, we can see that the negative impact of intensified immigration enforcement on the fertility of likely undocumented women is present among intact households.[51] Therefore,

[51] We are somewhat limited in the ability to perform this analysis for families in which the partner is missing using the ACS, which does not allow us to identify if the partner is absent unless the couple is married. The resulting sample size of married women with absent spouses is too small to make reliable inferences.

the fertility impacts of intensified immigration enforcement cannot be solely attributed to household compositional effects as captured by the deportation of a partner, which can obviously place fertility on hold. Rather, other circumstances possibly linked to living in the shadows to evade apprehension, such as limited income sources or increased uncertainty about the future, might be at play.

To further substantiate this conclusion, we distinguish among different types of couples: (a) those in which both partners are likely undocumented, (b) those of likely undocumented women whose partners are naturalized, and (c) the ones composed of likely undocumented women whose partners are natives. As can be seen from the estimates in Panel B, the impact of intensified immigration enforcement is concentrated among women whose partners are also likely undocumented, suggesting that they probably face greater income constraints and uncertainty. Note that, to the extent that mothers in the second and third columns of Panel B are also likely undocumented, they are not free of such ailments. Yet, the fact that intensified enforcement is not significantly altering their childbearing is suggestive of greater economic hardships and uncertainty endured when both partners are likely undocumented as key factors in explaining fertility choices.

To conclude, we try to distinguish the role played by current income restrictions, as opposed to increased uncertainty, which should be endured by all likely undocumented women to some degree. Because of the endogenous nature of women's fertility with respect to family income, we take a descriptive approach and investigate how the intensification of immigration enforcement appears to have impacted likely unauthorized women's childbearing depending on whether their family income falls in the bottom, middle or top quartiles of the distribution of family income.[52] The results from this exercise are displayed in Panel C. According to the estimates in columns (1) through (4), the intensification of immigration enforcement primarily impacted the fertility patterns of likely unauthorized women in the bottom family income quartile. The fact that the impact is only recognizable among women in this group suggests that, in addition to increased uncertainty about the future of the family unit and its resources, lower family income plays an important role on likely unauthorized women's fertility.

8. Summary and policy implications

We examine the effect that the progressive intensification of immigration enforcement in the United States over the past two decades has had on the childbearing patterns of likely undocumented women. The analysis exploits the temporal and geographical variation on the implementation of the interior immigration policies to identify the impact of tougher immigration enforcement on these women's fertility. Using 2005 through 2014 ACS data, we find that the average increase in interior immigration enforcement during that time span lowered the likelihood of childbearing among likely undocumented immigrant women by 5%. Since likely undocumented women's fertility dropped by approximately 26% over that period, intensified immigration enforcement could be responsible for about one fifth of the decline.

We also show how the impact of intensified interior immigration enforcement on fertility is driven by police-based measures and appears to be stronger among intact families, families headed by a likely undocumented couple, as well as among the poorest families in the bottom income quartile. These findings underscore the importance of limited income resources, along with increased uncertainty emanating from an intensified fear of deportation, on likely undocumented women's fertility. Finally, to the extent that intensified immigration enforcement affects the childbearing decisions of women in intact households, the implications of this type of policy can be significantly broader, not only impacting households shattered by deportations.

As it is often the case with studies focused on migrant populations, our analysis is stemmed by several data shortcomings. Two main ones are worth summarizing here. First, the ACS lacks information on immigrants' legal status, which we proxy using three different methodologies. Second, we do not have information on undocumented migrant women who returned home, possibly since most deportees have been men (Bergeron et al., 2014). Finally, our estimates capture short-run impacts of immigration policy. Given the relatively recent nature of these policies, we cannot gauge its full long-run impacts on immigrant women's fertility. Further research assessing these long-term impacts is warranted.

Overall, however, the findings prove robust to several identification and robustness checks and have important policy implication for the United States. Aside from the damage inflicted on the families of mostly U.S. citizen children affected by these tougher immigration policies, it is worth emphasizing that: (a) these are reductions in fertility that stem from immigration enforcement and, consequently, less likely to be deemed voluntary; and (b) the importance of immigrant fertility –significantly higher than that of natives, in many developed nations. Because immigrants tend to be considerably younger than natives and have higher fertility rates, immigration increases the ratio of workers to retirees and the viability of Social Security (Griswold, 2012). Currently, the United States is one of the few countries with fertility rates close to replacement rates, thanks to immigrants and their offspring (Kotkin and Ozuna, 2012).

Declaration of Competing Interest

None.

[52] The income quartiles are defined within the sample.

Appendix

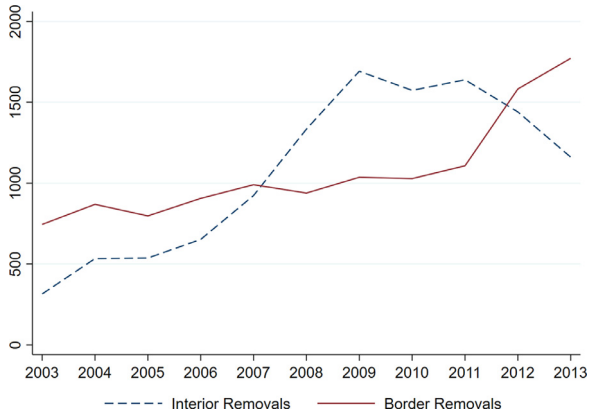

Fig. A. Interior and Border Removals per 100,000 Estimated unauthorized Resident Aliens
Source: Removal Data: DHS OIS, Yearbook of Immigration Statistics, FY 2010–2013. Unauthorized Resident Aliens, ProCon.org: http://immigration.procon.org/view.resource.php?resourceID=000844.

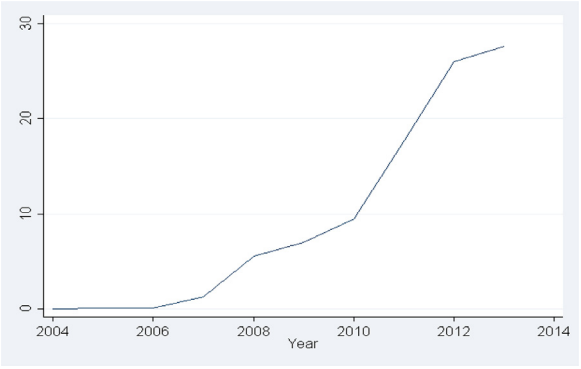

Fig. B. Growth Rate in the Number of MSAs Activating Interior Immigration Enforcement Measures.

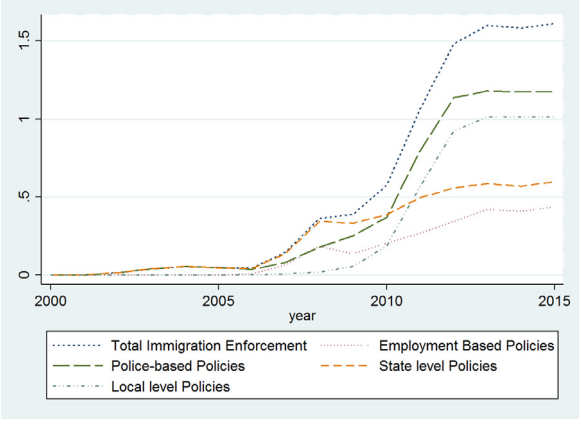

Fig. C. Average Enforcement Index by Nature of the Policy.
Notes: Average enforcement index per year.

Table A
Immigration enforcement programs.

Nature of the Law	Law	Years	Where?	Objective	Who implements it?	Scope	Signed by	What it Consists of:
Police-Based Measures	**287(g)**	2002–2012	Street/Jail	Make communities safer by the identification and removal of serious criminals	State and local law enforcement entities	State and Local (County, City or Town)	State and local enforcement entities signed a contract (Memorandum of Agreement -MOA) with the U.S. Immigration and Customs Enforcement (ICE)	There are various functions: **Task Force**: allows local and state officers interrogate and arrest noncitizens during their regular duties on law enforcement operations. **Jail enforcement** permits local officers to question immigrants arrested on state and local charges about their immigration status. **Hybrid model:** which allow participate in both types of programs.
	Secure Communities	2009–2014 2017-	Nation's jail and prisons	Identify noncitizens who have committed serious crime using biometric information	Police	Local (County)	Jurisdictions	The program allows for the submission of biometric information on detainees checked against records in FBI and DHS databases.
	Omnibus Immigration Laws	2010-	Street/Jail	Identification noncitizen	State and local law enforcement entities	State	State governor	Comprehensive laws that may include: • A "show me your papers" clause, enabling the police to request proper identification documentation during a lawful stop. • Require that schools report students' legal status.
Employment Based Measures	**E-Verify**	2001-	Firms	Screen newly hired workers	Firms	State	State governor	Electronic program that allows employers to screen newly hired workers for work eligibility.

Table B
Definition of key variables.

Childbearing	Dummy variable 1-Woman reports to have a child during the last 12 months 0-Otherwise
Married	Dummy variable 1-Married woman 0-Otherwise
Number of Own Children Under 5	Number of own children less than 5 years of age excluding new-borns.
Years in the U.S.	Number of years of U.S. residency
Age	Woman's Age
Years of Education	Number of Years of Education
Unemployment Rate in MSA in 2000	Unemployment rate by MSA in 2000
Share of Hispanics Immigrants in MSA in 2000	Share of Hispanics Immigrants by MSA in 2000
Share Voting Republican in the State in 2000	Share of votes going to Republican candidates for the U.S. House of Representatives by state and year. Source: Office of the Clerk, US House of Representatives, http://clerk.house.gov/member_info/electionInfo/index.aspx.
TANF	Dummy variable: 1- State offered TANF for unqualified immigrants 0-Otherwise
CHIP	Dummy Variable: 1- State offered CHIP benefits to lawfully present immigrant children and pregnant women 0-Otherwise
Food Stamp	Dummy Variable: 1- State offered food stamps to unqualified immigrants 0-Otherwise

Table C
Probability of childbearing among likely unauthorized women – heterogeneous impacts by age and years in the U.S.

Regressors	Years in the U.S.			Age Range		
	5–10 Years	11–15 Years	More 15	15–24	25–34	35–45
Enforcement Index	−0.015**	0.001	−0.005	−0.006	−0.016**	−0.001
	(0.006)	(0.005)	(0.004)	(0.010)	(0.007)	(0.003)
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Welfare Programs	Yes	Yes	Yes	Yes	Yes	Yes
Area Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,672	36,670	36,531	16,750	35,369	48,741
R-squared	0.132	0.132	0.14	0.148	0.119	0.128

Notes: Sample: Hispanic non-citizen low skilled women living in the United States in excess of 4 years. All regressions include a constant term and the controls in the most complete specification in Table 2. Standard errors are shown in parentheses and are clustered at the MSA level.

Table D
The determinants of the adoption timing and intensity of immigration enforcement.

Dependent Variable:	Year when IE>0	Immigration Enforcement when IE>0
Average Fertility in MSA	91.550	0.187
	(128.237)	(0.208)
Average Age	−2.329	−0.001
	(6.034)	(0.008)
Share Married	−84.844	−0.212*
	(88.349)	(0.117)
Share of LU with children less than 5 years old	9.113	0.126
	(58.505)	(0.081)
Average Years in the US	−20.917	0.011
	(18.823)	(0.011)
Average Years of Education	−24.386	0.001
	(30.151)	(0.055)
Unemployment Rate	−104.728	−0.104
	(99.319)	(0.192)
Share of LU	165.603	−0.213
	(141.748)	(0.216)
Observations	240	240
R-squared	0.321	0.476

Notes: The regressions show the results from estimating Eq. (8) using 2005 ACS data collapsed at the MSA level and including state fixed-effects. The Collapsed Individual Characteristics are the average age of likely undocumented women, share of married likely undocumented women, the average number of children of likely undocumented women, the average education level of women, and the average number of years in the United States of likely undocumented women. Area Characteristics include average unemployment rate, the share of undocumented immigrants. Standard errors are shown in parentheses and are clustered at the MSA level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table E
Instrumenting the location of immigrants.

Dependent Variable:	Probability of Childbearing
Enforcement Index	−0.008**
	(0.004)
First Stage Results	
IV	0.9204**
	(0.016)
Observations	106,033
R-squared	0.125

Notes: The regression above includes a constant term and the controls in the most complete specification in Table 2. We use data on the historical location of likely undocumented women from the same country of origin (Bartel 1989; Card 2001; Cortes and Tessada 2011, among many others) to proxy migrants' probable location in the absence of tougher enforcement. Specifically, we rely on the 2000 Census to construct the following share informing of the concentration of undocumented immigrants from the same country of origin in each MSA prior to the implementation of any of the enforcement initiatives under study: $Share\ of\ Undocumented\ Immigrants_{m,o,2000} = \frac{undocumented\ \ immigrants_{m,o,2000}}{undocumented\ \ immigrants_{o,2000}}$. We then interact this share with the enforcement index in each respective MSA and year to proxy for the likely exposure to tougher enforcement. As shown in the first-stage results, the *shift-share* instrument is highly correlated to the exposure to tougher enforcement of likely undocumented women in our sample –a correlation based on immigrants' entrenched tendency to reside in areas with established networks of their countrymen (Bartel 1989; Massey et al. 1993; Munshi 2003; Card, 2001; Cortes and Tessada 2011, among many others).
Sample: Hispanic, non-citizen, low-skilled women who have lived in the United States 5+ years. Standard errors are shown in parentheses and are clustered at the MSA level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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