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Marriage as Insurance: Household Responses to Immigration Policy Uncertainty

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Abstract

This paper examines whether marriage serves as a strategic response to immigration policy uncertainty. We study transitions into marriage among cohabiting binational couples—defined as unions between a U.S. citizen and a noncitizen partner—following the shift in immigration policy expectations during the 2016 U.S. presidential campaign and the subsequent tightening of enforcement in 2017. Using ACS data from 2008 to 2019 and a difference-in-differences design, we compare marriage transitions among binational couples to those of homogeneous citizen couples. Immigration policy uncertainty increased marriage rates among binational couples by approximately 1.5–1.8 percentage points, or about 8-10 percent relative to pre-treatment levels. Event-study estimates show no differential pre-trends and indicate that the response began in 2016, prior to the formal reinstatement of Secure Communities. The effect is concentrated among likely unauthorized immigrants and individuals from targeted nationalities. The findings suggest that marriage functioned as a form of legal and economic insurance in response to heightened deportation risk.

Keywords: Marriage, Immigration Policy, Household Formation

JEL Codes: J12, J15, J61

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1. Introduction

Marriage is typically viewed as a social and economic institution shaped by preferences, norms, matching considerations and life-cycle factors (Becker, 1973; Stevenson and Wolfers, 2007). Yet in environments of legal uncertainty, it may also serve as a form of insurance. This paper studies whether binational couples use marriage strategically to mitigate the heightened immigration policy risk during the 2016 U.S. presidential campaign and its aftermath.

The 2016 presidential campaign marked a sharp shift in immigration policy expectations in the United States. For couples with a noncitizen member, the prospect of intensified deportations and stricter enforcement increases the perceived risk of family separation (Amuedo-Dorantes and Arenas-Arroyo, 2019). Those expectations were likely reinforced once the new administration took office in January 2017 and reinstated Secure Communities nationwide. This shift in the policy environment may have changed the value of formalizing their union. Deportation or forced migration can imply the loss and distortion of investments in work, educational progress, household formation, and social networks (Alsan and Yang, 2024; East and Velásquez, 2024; Amuedo-Dorantes and Antman, 2022). We examine whether binational couples responded to this change by transitioning into marriage at higher rates.

By 2015, immigration enforcement was not new. Secure Communities had expanded nationwide between 2008 and 2014 (Cox and Miles, 2013) and was subsequently scaled back. What changed in 2015 was the salience and tone of the public debate. Immigration became central to the presidential campaign, with repeated promises of mass deportations, stricter border enforcement, and restrictions targeting specific nationalities. As Trump secured the Republican nomination in mid-2016 and won the general election in November 2016, these campaign promises increasingly shaped expectations about future enforcement (Lopez et al., 2018). Upon taking office in January 2017, the administration reinstated Secure Communities nationwide and introduced additional measures aimed at tightening immigration control.

Together, these developments generated a period of heightened uncertainty for immigrant households.

We exploit the shift in immigration policy expectations emerged in mid-2015, together with the subsequent tightening of enforcement in 2017, as a source of variation in perceived deportation risk. We implement a difference-in-differences design comparing cohabiting binational couples—defined as unions between a U.S. citizen and a noncitizen partner—to homogeneous citizen couples, whose legal status is not directly affected by immigration enforcement. This comparison captures differential exposure to immigration-related uncertainty while accounting for common time trends in marriage behavior.

To examine the timing of responses and assess the plausibility of parallel trends, we complement the baseline specification with an event-study framework. In additional analyses, we explore heterogeneity by state-level enforcement intensity and sanctuary status to assess the relative importance of realized enforcement versus changes in expectations.

The analysis uses data from the American Community Survey (ACS) from 2008 to 2019. We identify cohabiting couples within households and construct an annual indicator for transitions into marriage among previously unmarried unions. The unit of analysis is the couple-year. Because binational couples represent a relatively small share of the sample, we complement the cross-sectional analysis with a short retrospective pseudo-panel. Using information on year of marriage and year of naturalization, we reconstruct marriage and citizenship status up to two years prior to the survey year, under the assumption that the couple was already cohabiting during that period.

We merge the ACS with state-level data on immigration enforcement activity, including detentions and removals, as well as information on sanctuary jurisdictions. These data allow us to examine both aggregate shifts in policy expectations and cross-state variation in realized enforcement.

We find that immigration policy uncertainty increased marriage rates among binational couples by approximately 1.84 percentage points in the baseline specification, corresponding to a 10 percent increase relative to the pre-treatment mean. The effect is robust to extensive individual and household controls, state-level macroeconomic conditions, and controls for DACA eligibility. Event-study estimates show no evidence of differential pre-trends and indicate that the response emerges in 2016, prior to the formal reinstatement of Secure Communities, consistent with a role for changes in expectations.

Moreover, in areas where the Trump electoral signal was stronger and more credible in 2016, measured by primary election outcomes in that year, couples responded more intensively, and in particular couples with at least one child under 16 in the household. In contrast, the estimated effect does not vary systematically with cross-state differences in post-2017 enforcement intensity or sanctuary status, suggesting that realized enforcement alone cannot account for the increase in marriage.

The response is concentrated among unauthorized immigrants, individuals from nationalities targeted in the political debate, and couples with lower or middle levels of education. These patterns are consistent with marriage functioning as a protective response among households facing greater exposure to deportation risk.

The findings are robust to alternative definitions of the post-treatment period and to using noncitizen–noncitizen couples as a control group, reinforcing the interpretation that the results capture a differential response among binational unions rather than a general post-2016 shift in marriage trends.

This paper contributes to that literature by documenting a novel margin of adjustment: the legal formalization of unions. There is a large body of literature examining the effects of immigration enforcement in the United States on economic and social outcomes, including labor market performance (East et al., 2023), mobility (Watson, 2013), participation in social programs (Alsan and Yang, 2024), local crime (Miles and Cox, 2014), and mental health (Wang and Kaushal, 2019). We show that immigration policy uncertainty reshapes household formation decisions, with marriage operating as a strategic response to mitigate deportation risk.

A smaller but growing strand of research examines how enforcement policies affect marriage patterns. Bansak and Pearlman (2022) find that increased deportations altered marriage composition for immigrant women in US, raising endogamous marriage while reducing certain forms of exogamy, with no impact on marriage to native-born men. Unlike studies focused on partner choice or intermarriage formation, we analyze transitions into marriage among already cohabiting couple. This allows us to isolate the formalization decision within existing unions, rather than changes in partner selection. Moreover, the evidence presented highlights the role of expectations and perceived policy risk, rather than solely realized enforcement.

Conversely, other work shows that policies granting legal status can reduce incentives for intermarriage. Redpath (2024) show that spousal visa access increases mixed-citizenship same-sex marriages, while EU enlargement reduced intermarriage and increased separation among mixed couples in Italy (Adda et al., 2025). Relatedly, previous work has also documented effect of specific programs granting temporal protection and increasing formalization. Gihleb et al. (2023) show that DACA eligibility altered household arrangements but did not detect effects on marriage.

More recently, Amuedo-Dorantes and Wang (2025) demonstrate that the threat of DACA's termination increased intermarriage among eligible undocumented immigrants. While that study focuses on uncertainty surrounding a particular policy program and the eligibility criteria within the undocumented population, our analysis considers a broader political shock that altered immigration enforcement expectations nationwide. By examining marriage transitions at the couple level and comparing binational to homogeneous unions, we capture marriage as legal formalization and protection in response to heightened enforcement risk, extending beyond the scope of a single program and encompassing a wider population of immigrant households.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background and the evolution of immigration enforcement during the 2016 election cycle.

Section 3 introduces the data and sample construction. Section 4 presents the empirical strategy. Section 5 reports the main results and event-study evidence. Section 6 examines enforcement intensity, sanctuary status, and heterogeneous effects. Section 7 concludes.

2. Political Background

On June 16, 2015, Donald Trump announced his candidacy for President of the United States. In a 47-minute announcement speech, immigration was introduced within the first minutes and remained a central theme throughout. The speech framed immigration as associated with crime and economic competition, and singled out particular immigrant groups: *" (..) They're sending people that have lots of problems, and they're bringing those problems with us. They're bringing drugs. They're bringing crime. (...) It's coming from more than Mexico. It's coming from all over South and Latin America, and it's coming probably— probably— from the Middle East"* (Time, 2015).

During the inaugural speech and in following public presentations Trump emphasized proposals such as mass deportations and stricter border enforcement. Immigration policy subsequently became one of the defining issues of the 2016 electoral cycle, increasing the salience of enforcement and deportation in public debate

As the campaign progressed and Trump secured the Republican nomination in July 2016, the likelihood that these proposals could translate into policy increased. His victory in the general election in November 2016 further reinforced expectations of a shift toward stricter immigration enforcement. For immigrant households this period marked a substantial increase in perceived deportation risk and uncertainty regarding future legal status.

These expectations were rapidly followed by concrete policy actions. On January 25, 2017, shortly after taking office, President Trump signed Executive Order 13768 ("Enhancing Public Safety in the Interior of the United States"), which reinstated the Secure Communities (SC) program. Unlike its initial rollout between 2008 and 2013, which expanded gradually across jurisdictions (Cox and Miles, 2013), the 2017 reinstatement was implemented nationwide and immediately reactivated cooperation between local law enforcement and federal immigration authorities. This shift substantially expanded the scope of interior enforcement.

Two days later, on 27 January 2017, the administration issued Executive Order 13769, commonly referred to as the "travel ban," which suspended entry into the United States for nationals of several predominantly Muslim countries (initially Iran, Iraq, Libya, Somalia, Sudan, Syria, and Yemen) and temporarily halted the refugee admission program. Although the order was subsequently revised and subject to litigation, later versions maintained restrictions on multiple countries and expanded the list of affected nationalities. These measures reinforced the perception that specific immigrant groups were being directly targeted by federal immigration policy.

Then in September 2017, the administration announced its intention to terminate the Deferred Action for Childhood Arrivals (DACA) program, which had provided temporary protection from deportation and work authorization to eligible undocumented immigrants. Although federal court rulings in 2018 required the program to continue accepting renewals, the attempted rescission introduced significant uncertainty for beneficiaries. DACA remained active—at least for renewals—throughout the period covered in our analysis (through 2019), but its future legal status remained contested.

For the purposes of this paper, this period is characterized by two related developments: a sharp shift in immigration policy expectations beginning in mid-2015, and the subsequent implementation of stricter enforcement measures in early 2017. Both elements are central to understanding the changes in perceived deportation risk faced by binational couples.

3. Data and Sample Construction

3.1. Data

This paper uses data from the American Community Survey (ACS), a nationally representative annual survey conducted by the U.S. Census Bureau. We rely on ACS waves from 2008 to 2019. The large sample size—approximately 1 percent of the U.S. population each year—allows us to identify a sufficient number of binational couples and observe marriage transitions at annual frequency.

We use harmonized microdata from IPUMS-USA (Steven Ruggles and Sobek, 2017), which ensures consistent variable definitions across survey waves. The ACS is particularly well suited for this study because it provides detailed information on household relationships, marital status, citizenship status, birthplace, and the retrospective year of marriage and naturalization. These variables allow us to identify cohabiting couples, classify their citizenship composition, and reconstruct marriage transitions over time.

In addition to the ACS, we incorporate information from several external data sources to characterize the local policy environment. Data on Primary Election in 2016 are obtained from Bucknell University Election Data Center (Bucknell University, 2017) and the 2016 U.S. Election dataset on GitHub (Hamner, 2017). Data on Sanctuary City policies are obtained from the replication package of Alsan and Yang (2024). Information on the rollout of the Secure Communities program and other local enforcement immigration policies is drawn from the replication materials accompanying East and Velásquez (2024). Finally, data on ICE detentions are obtained from The Deportation Data Project and data on removals under SC were obtained directly from tables in the Transactional Records Access Clearinghouse (TRAC) database website. These external datasets are merged with the ACS at the appropriate geographic and temporal levels and are used to complement the individual- and household-level information contained in the survey.

3.2. Sample Construction

Our unit of analysis is the couple. We identify couples in the ACS as two individuals within the same household where one is listed as the household head and the other reports being either the spouse or an unmarried partner. The sample is therefore restricted to cohabiting couples observed at the time of the survey. We limit attention to individuals between ages 16 and 60.

We classify couples as married if both partners report being married and the spouse is present in the household. Couples are classified as unmarried if the relationship is reported as "unmarried partner" and the household head reports a non-married status (never married, divorced, separated, or widowed). Our focus is on the legal status of the current union, rather than the individual's marital history.

The outcome of interest is an indicator for whether an unmarried couple transitions into marriage in a given year. Couples already married in a given year are excluded from the risk set. Because the ACS reports the year—but not the month—of marriage and naturalization, we assign marital and citizenship status at the annual level. Individuals reporting being unmarried in year t are treated as unmarried throughout that year, and transitions are recorded at the annual frequency.

We define a couple as binational if one partner is a U.S. citizen and the other is a noncitizen. Couples are defined as homogeneous if both partners share the same citizenship status. From this group, we exclude couples in which citizenship was acquired through naturalization, as these unions may have previously been binational and therefore exposed to immigration-related risk prior to naturalization.

We construct the sample using two complementary approaches: a cross-sectional design and a short retrospective pseudo-panel. In the cross-sectional sample, marital and citizenship status are defined as reported in the survey year, and each couple enters the sample only once. This approach provides a clean snapshot of couple composition but yields a relatively small number of marriage transitions among binational couples in any given year.

To increase statistical power, we exploit the retrospective information in the ACS on year of marriage and year of naturalization. These variables allow us to reconstruct marital and citizenship status up to two years prior to the survey year. The pseudo-panel therefore expands the effective number of couple-year observations. The key identifying assumption is that couples observed in year t were already formed and cohabiting in year $t - n$, with $n \in 1, 2$ [1]

Table 1 compares observable characteristics across the cross-sectional sample and the pseudo-panel constructed using one- and two-year retrospective windows. As expected, the pseudo-panel differs systematically from the cross section. Couples identified through longer retrospective windows exhibit higher levels of education, income, and asset ownership, and are more likely to be in second or higher-order unions.

By contrast, we do not observe substantial differences in age or employment status. The differences across samples therefore reflect selection into longer cohabitation spells rather than simple aging.

These differences arise mechanically from the panel construction. The retrospective approach requires couples to have been cohabiting prior to the observed marriage transition, effectively selecting unions that survive longer before formalization. Couples who remain together for longer periods prior to marriage are, on average, more educated and economically stable, and display more complex marital trajectories. In contrast, the cross-sectional sample captures a larger share of younger unions and relationships that transition into marriage more quickly or without extended cohabitation.

These compositional differences are important for interpreting the magnitude of the estimated effects across samples. In all specifications, we control for the full set of observable characteristics and include indicators for the source of the observation (cross section,

[1] For example, consider a couple observed in the 2018 ACS. Using information on year of marriage and year of naturalization, we can reconstruct their marital and citizenship status in 2016 and 2017. If the couple married in 2016 and the foreign partner naturalized that same year, we classify them as a binational unmarried couple at the beginning of 2016 and record a transition into marriage in that year.

Table 1: Summary Statistics by Sample

Variable	Cross-Section	Panel 1	Panel 2
Observation	682655	780590	852578
Binational Couples	41265	55440	68197
Binational Couples (%)	6.04	7.10	8.00
Educ (Head): Low	0.066	0.060	0.055
Educ (Head): Mid	0.637	0.619	0.606
Educ (Head): High	0.297	0.320	0.339
Educ (Partner): Low	0.087	0.080	0.074
Educ (Partner): Mid	0.668	0.651	0.638
Educ (Partner): High	0.245	0.269	0.288
Age grp: 18–24	0.141	0.140	0.133
Age grp: 25–34	0.379	0.400	0.417
Age grp: 35–44	0.220	0.216	0.217
Age grp: 45–54	0.187	0.174	0.167
Age grp: 55+	0.074	0.069	0.066
Age diff: 0–2	0.450	0.461	0.469
Age diff: 3–5	0.272	0.270	0.269
Age diff: 6–10	0.136	0.132	0.130
Age diff: 11–15	0.070	0.067	0.065
Age diff: 15+	0.073	0.070	0.067
Mar order (Head): 1st	0.576	0.468	0.395
Mar order (Head): 2nd	0.310	0.385	0.436
Mar order (Head): 3+	0.114	0.147	0.168
Mar order (Partner): 1st	0.592	0.482	0.407
Mar order (Partner): 2nd	0.300	0.380	0.434
Mar order (Partner): 3+	0.108	0.139	0.159
Employed (Head)	0.836	0.842	0.848
Not emp. (Head)	0.164	0.158	0.152
Employed (Partner)	0.768	0.774	0.780
Not emp. (Partner)	0.232	0.226	0.220
Fertility (last yr)	0.061	0.070	0.084
Num. children	0.774	0.771	0.788
Rooms	5.390	5.426	5.480
Mortgage	1.232	1.269	1.317
Any health ins.	0.828	0.843	0.857
HH income	77319.6	79912.2	82837.5

Source: Own author calculation based on ACS data.

one-year panel, or two-year panel), ensuring that differences in couple composition do not mechanically drive the results.

4. Empirical Strategy

We propose an identification strategy following a diff-in-diff setting, in which we exploit the temporal variation in the uncertainty of the enforcement inmigration policies before and after Trump announcing running for election at the end of 2015, and the variation in the degree of exposure regarding immigration status for different groups. More precisely, we compare the evolution of marriage rates among individuals potentially affected by immigration policy with those not affected, before and after the 2016.

Our treatment group are binational cohabiting unmarried couples, defined as union between a U.S.-born individual and a foreign-born,non-citizen partner. The control group are homogeneous cohabiting unmarried couples, defined as unions between two U.S. citizens or, more restrictively, between two U.S.-born individuals.

Binational couples are differentially exposed to immigration-related uncertainty because the legal status of one member of the household is directly affected by expectations regarding future changes in immigration policy and enforcement. By contrast, cohabiting couples composed exclusively of U.S. citizens are not directly exposed to such uncertainty and therefore provide a natural comparison group to capture common time trends in marriage behavior unrelated to immigration status.

Heightened immigration-related uncertainty may affect marriage decisions by increasing the perceived costs of remaining in an informal or non-institutionalized union. For binational couples, formalizing the relationship through marriage can reduce uncertainty regarding household stability, future separation risk, and interactions with public institutions. As a result, increases in uncertainty may accelerate or increase transitions into marriage among couples already close to formalization.

Under the assumption that, absent the increase in immigration-related uncertainty, marriage trends among these groups would have evolved similarly, differences post Trump's presidential campaign announcement in marriage behavior can be attributed to differential exposure to immigration policy uncertainty. We assess the plausibility of this assumption by examining pre-treatment trends in levels and through an event-study specification. Additional robustness checks are discussed below. Descriptive trends in levels are reported in Appendix Figure A.1 and A.2, for the cross section sample and pseudo-panel sample respectively.[2] No substantial differences in the slopes are observed previous to 2015 and a subsequent increase in marriage is registered only for binational couple in the three different alternatives of the graph.

The causal effect of the uncertainty regarding migration on marriage rate is estimated with the following model:

$$Y_{ist} = \alpha + \beta(\text{Binational}_i \times \text{Post}_t) + \gamma X_{ist} + \theta Z_{st} + \delta_t + \mu_s + \varepsilon_{ist}, \tag{1}$$

In equation (1), Y_{ist} denotes the marriage outcome for couple i residing in state s in year t. Binational_i is an indicator equal to one for binational cohabiting unmarried couples, and Post_t captures the post-treatment period. The coefficient of interest, β, measures the differential change in marriage rates among binational couples relative to the control group following the increase in immigration-related uncertainty.

The vector X_{ist} includes a rich set of couple-level controls, capturing educational attainment of both partners, age and age differences within the couple, marital history, fertility in last year and number of children in the household, housing characteristics, employment status of each partner, total household income and access to health insurance, indicators for the retrospective window used in the pseudo-panel construction and unauthorized immigrant status fixed effects.

The vector Z_{st} includes time-varying state-level characteristics, such as labor market

[2]For the case of cross section data, a two-year average is presented given the relative small size of observation for the treated group in each year

conditions measured by unemployment and employment rate, median wage growth, housing market indicators measured by HPI index and median gross rent, and measures of the local immigrant environment, capturing the share of immigrant in construction and services. All specifications include year fixed effects, δ_t, and state fixed effects, μ_s, which absorb common shocks over time and time-invariant differences across states, respectively. Standard errors are clustered at the state level.

Couples of same sex are excluded from the main analysis to avoid confounding our estimates with differential changes at the state level regarding legalization of same sex marriage, which took place during the same period. In the most complete specification we include to the main equation an interaction for DACA eligibility with year indicators (2012–2019). The terms capture potential eligibility to the DACA program, a specific immigration policy that was launched in 2012, that may affect differentially to the binational couple as an option to temporarily differ the deportation threat. Importantly, in our samples the share of elegible for the program among binational couples is stable and relatively low, around 13.4% of this group.

To further test the validity of our identification strategy, we estimate an event study specification of the following form:

$$Y_{ist} = \alpha + \sum_{\tau \neq \tau_0} \beta_\tau \left(\text{Binational}_i \times \mathbb{1}\{t = \tau\}\right) + \gamma X_{ist} + \theta Z_{st} + \delta_t + \mu_s + \varepsilon_{ist}, \qquad (2)$$

where $\mathbb{1}\{t = \tau\}$ is an indicator for each calendar year relative to the event, and τ_0 denotes the reference (baseline) year. We present estimations considering two alternatives baseline years (2014 and 2015). The coefficients β_τ trace the dynamic evolution of marriage rates among binational couples relative to the control group before and after Trump announcement for presidential the election. To properly account for the pre-treatment changes in immigration enforcement policies executed via Secure Communities roll-out and 278g agreements, here we include and enforcement term interacted with year indicators. This includes the period up to 2014, when the SC roll-out was completed, affecting only pre-preriods coefficients.

The event study specification allows us to visually and statistically assess the presence of pre-treatment trends, examining the timing of the response following the treatment event.

5. Results

Table 2 reports the estimates for the coefficient of interest capturing the effect on marriage transitions following the increase in immigration policy uncertainty after 2015. To present our main results, we first report estimates based on the cross-sectional specification, as the sample size is large enough for a precise estimation of the aggregated effect and avoids any potential compositional changes mechanically introduced by the construction of the pseudo-panel. Across all specifications presented in this table, we find a positive and statistically significant effect.

Column (1) reports the baseline specification without controls, yielding an estimated increase of 1.94 percentage points (standard error = 0.005) in the marriage rate of binational couples relative to couples in which both members are U.S. citizens. The coefficient of interest decreases to 1.39 percentage point and it is more precisely estimated (standard error = 0.0028) when incorporating the full set of controls, including individual characteristics of both partners as well as couple-level and household controls. The size of the point estimate corresponds to a 7.28 percent increase relative to the pre-treatment mean of the control group (0.184).

Column (3) adds state-level macroeconomic controls to account for potential effects of regional economic conditions on marriage decisions in a given year. These controls include labor market indicators, housing market conditions, and the share of immigrants employed in construction and services, thereby accounting for industry-specific dynamics particularly relevant for the treated population. The estimated effect corresponds to a 1.39 percentage point increase in the probability of marriage and remains statistically significant under this specification.

Column (4) controls for couples potentially eligible for the DACA program, which was in effect between 2012 and 2019. Previous research has documented an increase in marriage rates among illegible couples around 2014. The estimated post-period effect increases when adding this control to 1.73 p.p (standard error = 0.0035). The pattern is consistent with DACA having previously raised marriage rates among eligible individuals, thereby attenuating the additional marriage response to subsequent immigration enforcement shocks. Accounting for DACA suggests that the measured effect reflects a broader response among binational couples rather than behavior driven by targeted immigration programs.

Finally, Column (5) evaluates whether the effect is driven by cases in which the immigrant member of the binational couple is likely undocumented, following the methodology proposed by Borjas (2017). We find that the estimated effect persists and increases to 1.84 percentage points (standard error = 0.0034). Under our most comprehensive specification, the increase in immigration-related uncertainty among binational couples is associated with a 1.84 percentage point increase in the probability of marriage, corresponding to a sizeable 10 percent increase relative to the pre-treatment average.

Table 2: Difference-in-Differences Estimates of Marriage Transitions. Cross section sample.

	Get Married				
Binational × Post	0.0194***	0.0139***	0.0134***	0.0173***	0.0184***
	(0.0054)	(0.0028)	(0.0029)	(0.0035)	(0.0034)
Observations (000s)	69390.7	69390.7	69390.7	69390.7	69390.7
R-squared	0.011	0.383	0.383	0.383	0.383
Treated mean (Pre)	0.184	0.184	0.184	0.184	0.184
State FE	YES	YES	YES	YES	YES
Controls	NO	YES	YES	YES	YES
Macro controls	NO	NO	YES	YES	YES
DACA controls	NO	NO	NO	YES	YES
Undocument controls	NO	NO	NO	NO	YES

Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions. The dependent variable is an indicator equal to one if a cohabiting unmarried couple transitions into marriage in a given calendar year. All specifications include individual and couple-level controls, state-level time-varying controls, year fixed effects, and state fixed effects. The pre-treatment mean corresponds to the average marriage transition rate among binational couples before the treatment period. Standard errors are clustered at the state level. *Source:* Own author calculation based on ACS data.

Tables A.8 and A.9 in the Appendix replicate the estimates from Table 2 using the pseudo-panel specification with one- and two-year retrospective windows. The cross-sectional and one-year pseudo-panel estimates are similar in magnitude (1.55 and 1.96 percentage points, respectively), with the panel estimate being more precisely estimated. The relative magnitude is smaller in the one-year panel sample (0.0155/0.25), reflecting differences in the pre-treatment mean. In the two-year panel sample, the point estimate is closer in magnitude (1.96 p,p and standard error = 0.0022), and the relative effect size is also closer to that obtained in the cross-section (7.9 percent, 0.0196/0.246).

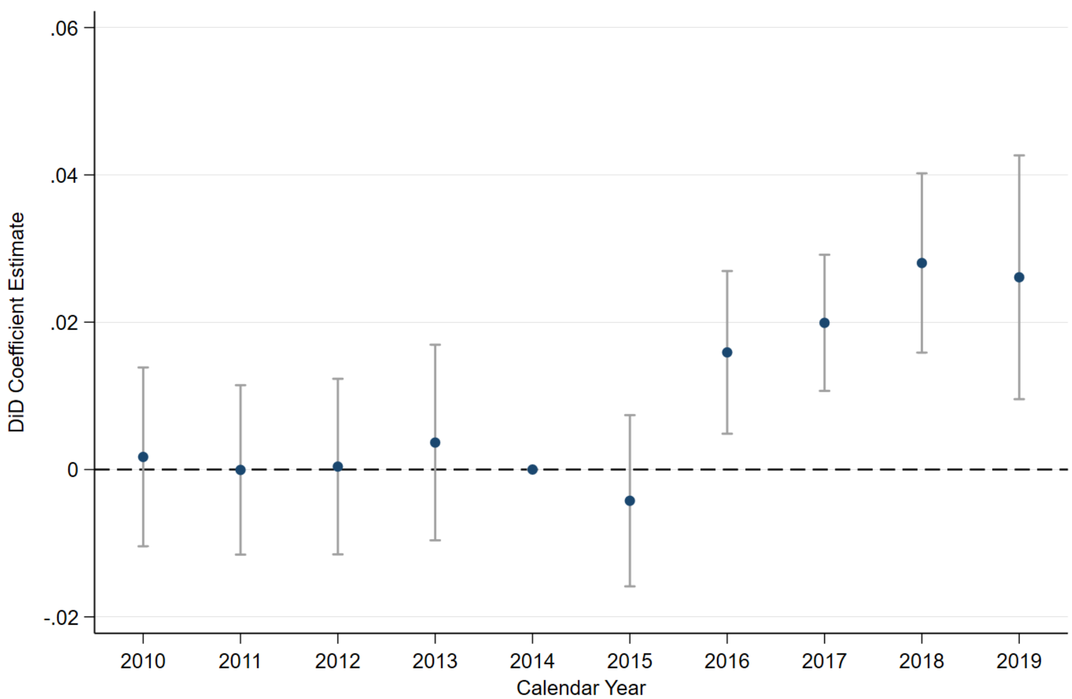
Taken together, we consistently find positive coefficients and statistically different from zero across all specifications and samples, reinforcing the robustness of the main findings. The cross-sectional specification provides a transparent benchmark to validate the pseudo-panel estimations, which allow us to exploit additional variation and obtain more precise dynamic estimates.

The difference in pre-treatment means between the cross-sectional and pseudo-panel samples is expected, given the compositional changes introduced mechanically by reconstructing couple trajectories, as discussed previously. These differences in levels are also visible in the pre-trend figures. The pseudo-panel captures more stable couples who have cohabited for longer periods, and therefore exhibit higher baseline marriage rates. Consistent with this selection pattern, increases in immigration-related uncertainty may have a larger marginal impact among such unions, which are closer to formalization.

The panel estimation is particularly useful to assess the validity of our identification strategy through an event study. Figure 1 plots the point estimates and standard errors for each year relative to a base year. We use 2014 as our baseline, as it represents the last full pre-treatment year prior to the rise in the anti-immigrant rhetoric and the shift in expectations surrounding immigration policy, associated with the launch of Donald Trump's presidential in 2015. Although the 2015 point estimate is not statistically different from zero, we avoid relying on a period potentially affected by increasing uncertainty, as may already capture anticipatory responses, temporary postponement or adjustment behavior. As a robustness,in the Appendix, we present the same specification using as an alternative reference year 2015 and a pooled 2014–2015 baseline. We also offer the estimations based on the cross-sectional sample, pooling relative coefficients within 2 years, giving the small sample falling into each year.

Across all the possible specifications, pre-treatment coefficients are small in magnitude and statistically indistinguishable from zero, providing no evidence of differential pre-trends between treatment and control groups. In contrast,post-2015 estimates are positive and statistically significant, consistent with an increase in marriage transitions among binational couples after the shift in immigration policy expectations.

Figure 1: Effect of increase in immigration-related uncertainty on marriages. Event Study. Pseudo-panel sample



Notes: The figure reports event-study coefficients from equation (2), estimating the differential change in marriage transitions for binational couples relative to homogeneous citizen couples. The omitted category is 2014. The specification includes the full set of controls, state, year fixed effects and enforcement terms interacted with year indicators for the pre-period. Standard errors are clustered at the state level. Pseudo-panel data are reconstructed from retrospective ACS information. Source: Own author calculation based on ACS data.

6. Potential Mechanism: Anti-migration rhetoric and Migration Enforcement

The estimated effects are, a priori, consistent with two distinct mechanisms that may be operating simultaneously. The first refers to a generalized climate of uncertainty and expectations of deteriorating conditions for immigrants, which may have pushed couples to formalize their unions as an institutional strategy to hedge against future shocks. Under this channel, individuals respond to the fear of potential future interventions by anticipating decisions in an environment characterized by mistrust and noisy signals.

A second mechanism links the increase in marriage rates to concrete and observable changes in couples' local environments. As promises of heightened immigration control and deportation began to materialize, couples may have responded by formalizing their unions. In the post-treatment period analyzed here, the main enforcement policy was the reactivation of Secure Communities (SC) in January 2017, which reinstated cooperation with local authorities in jurisdictions where it had previously operated until 2013.

The fact that we observe effects already in 2016 in the event study specification—prior to President Trump taking office and therefore before the formal resurgence of enforcement—is, a priori, consistent with a response to changes in immigration policy expectations and rhetoric. This timing is particularly suggestive of individuals updating their beliefs and behavior following the presidential primaries and the consolidation of the candidacy, when the likelihood of policy change became more salient.

To shed further light on these potential mechanisms, we begin by examining whether subnational variation in exposure to anti-immigrant political signals predicts differential marriage responses, before turning to variation in realized enforcement intensity.

6.1. Expectation channel (electoral signal)

To further examine the expectations channel, we construct a measure of local exposure to anti-immigrant rhetoric using Donald Trump's support in the 2016 presidential primaries at the PUMA level. The use of primary election results has a key advantage: these elections took place between February and June 2016, prior to any formal enforcement measures implemented after the inauguration in January 2017. As such, they capture variation in political signaling and expectations rather than realized enforcement.

Districts in which Trump won the primaries by larger margins likely experienced a stronger local shock in terms of anti-immigrant rhetoric and perceived deportation risk. If the increase in marriage rates is driven by changes in expectations rather than by enforcement actions per se, the effect should be stronger in areas where Trump's electoral performance—and therefore the credibility of his immigration proposals—was higher.

Electoral outcomes provide subnational variation in the intensity of political support prior to the general election. To account for systematic differences across more Trump-supportive districts, we control for initial immigrant composition at the PUMA level, in addition to the full set of individual, household, and macroeconomic controls used in the baseline specification.

Electoral data were compiled and harmonized using county-level results provided by the Bucknell University Election Data Center (Bucknell University, 2017). In states where county-level data were incomplete or unavailable, we supplemented the information using the 2016 U.S. Election dataset on GitHub (Hamner, 2017).[3] Data at the PUMA level was constructed taking PUMA-County equivalence employing geographical crosswalks. Due to Caucus election format or reporting at different geographical level than County, we have valid election data for 40 States. [4]

[3]When both sources were available, we relied on the Bucknell data, as it is based on finalized (certified) results from state election boards.

[4]By using the second source of data we were able to recover elections results for Hawaii, Iowa, Louisiana, Utah and Virginia. Data for is missing for Alaska, Colorado, District of Columbia, Kansas, Maine, Mas-

We consider multiple measures of electoral performance, each capturing a distinct dimension of the expectation channel. First, we use the share of votes obtained by Trump in the district. Second, we use his vote margin over the runner-up candidate (when he won) or the margin relative to the leading candidate (when he did not). For both measures, we distinguish between performance relative to all candidates (total) and performance within the Republican primary field.

Trump's total vote share captures the overall pro-Trump climate in the local environment, reflecting exposure to anti-immigrant rhetoric in the immediate context where couples reside. This measure is primarily indicative of the intensity of local political support and the salience of immigration discourse. The vote share within the Republican primary field instead captures ideological alignment among Republican voters in the district. While informative about intra-party consolidation behind Trump's proposals, this measure does not necessarily signal the broader electoral viability of the candidate.

The vote margin, in contrast, captures electoral competitiveness and thus the credibility of Trump's candidacy. A larger margin relative to all candidates provides a clearer signal that Trump's policy proposals were not merely rhetorical but increasingly likely to translate into federal policy. In this sense, the vote margin may more directly proxy for the perceived probability of policy implementation rather than for local ideological climate alone.

To isolate the expectations channel from realized enforcement effects, we define 2016 as the sole post-treatment year in this specification. Including subsequent years would risk conflating changes in expectations with differences in actual enforcement intensity across states after 2017. In addition, we restrict the sample to couples in the retrospective pseudo-panel who report residing in the same PUMA as one year prior, mitigating potential bias from endogenous migration responses.

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sachusetts, Minnesota, North Dakota, Rhode Island, Vermont, Wyoming. Data is not available for North Dakota, and Wyoming due to the inability to find complete results for these states. Kansas reports election outcomes at Congressional District level. Massachusetts, Rhode Island, Vermont reports at Town level. Alaska reports data at the State House District and Minnesota at Congressional District. Colorado and Maine report at the County level but both are Caucus election and are not available in any of the sources.

The results from the triple interaction between Binational, Post, and Trump's electoral performance are reported in Table 3, using the two-year pseudo-panel sample. Because the post-treatment period is limited to 2016, the one-year panel and cross-sectional samples suffer from limited statistical power for binational couples estimation. All electoral measures are standardized.

The interaction with Trump's total vote share displays the expected positive sign (0.6 p.p.), although it does not reach conventional levels of statistical significance. In contrast, the interaction with the vote margin relative to all candidates yields a positive coefficient of 0.9 p.p and statistically significant at 5% level (sd=0.0044). This is interpreted as an increase of one standard deviation in Trump's primary vote margin increasing in 2016 the probability of marriage among binational couples in 0.9 p.p., additional to the 2.3 p.p. of the baseline effect.

Appendix Table A.14 reports results using performance within the Republican primary field. While coefficients are generally positive, none of them are statistically distinguishable from zero at conventional levels, in this case. This is suggestive that immigrants seem to place greater weight on Trump's relative competitiveness in the overall electoral field than on his level of support within his own party.

Taken together, these findings suggest that the local anti-immigrant climate per se is not sufficient to trigger marriage formalization. Living in a more Trump-supportive area, independent of the broader electoral viability of the candidate, does not systematically increase marriage transitions. Instead, what appears to matter is the perceived probability that Trump would prevail over all the opponents and implement his immigration agenda at the federal level. Thus, binational couples residing in areas where the electoral signal was stronger and more credible responded more intensively, formalizing their unions in anticipation of heightened deportation risk.

Finally, we explore heterogeneity by the presence of children in the household. If the expectations channel operates through perceived deportation risk and its associated family

Table 3: Triple Differences. Share and Margin of votes considering Republican candidates in 2016 primary elections. Pseudo-Panel 2 year sample. Post = 2016

		Child Under 16	
	All	Yes	No
Panel A: Share Votes			
Binational × Post	0.022***	0.032***	0.011
	(0.0053)	(0.0084)	(0.0073)
Binational × Post × Trump Prim. Election	0.006	0.010	0.002
	(0.0047)	(0.0069)	(0.0065)
Panel B: Margin of Votes			
Binational × Post	0.023***	0.034***	0.012
	(0.0054)	(0.0085)	(0.0075)
Binational × Post × Trump Prim. Election	0.009**	0.014**	0.004
	(0.0044)	(0.0064)	(0.0061)
Obs (000s)	91,802	38,407	53,394
State FE	YES	YES	YES
Controls	YES	YES	YES
Macro controls	YES	YES	YES
DACA controls	YES	YES	YES
Undoc controls	YES	YES	YES
PUMA imm share	YES	YES	YES

Notes: Electoral data was sourced from Bucknell University Election Data Center and supplemented with the 2016 U.S. Election dataset on GitHub, for states where county-level data were incomplete or unavailable. Data at the PUMA level was constructed taking PUMA-County equivalence employing geographical crosswalks. Share of votes measures the percentage of votes obtained by Donald Trump in competition with the Republicans candidates in the district. Margin of votes measures the difference in votes share over the runner-up Republican candidate (when he won) or the margin relative to the leading candidate (when he did not). No election data available for Alaska, Colorado, District of Columbia, Kansas, Maine, Massachusetts, Minnesota, North Dakota, Rhode Island, Vermont, Wyoming. Both measures are standardized. Post period treatment was restricted to 2016 to isolate the expectations channel prior to SC enforcement revival (2017). Standard errors clustered at PUMA level in parentheses. *Source:* Own author calculation based on Bucknell University Data Center and Hamner (2017).

costs, we should observe stronger effects among couples with children. For binational couples without children, separation—while costly—may be partially reversible through relocation or delayed reunification. In contrast, for couples with children, forced separation entails substantially higher emotional and economic costs, with long-term implications for child development and household stability.

Consistent with this interpretation, the estimated effect of electoral competitiveness is larger (1.4 p.p) and statistically significant among couples with children under age 16, while it becomes small (0.04 p.p) and statistically indistinguishable from zero among couples without children. This is consistent with the channel of expectation, where couples potentially facing the higher cost internalize the signals and respond by protecting through union formalization at the highest rates.

6.2. Secure Communities

Next, we turn to testing the materialize enforcement as a relevant channel, examining whether the estimated increase in marriage is concentrated in states that experienced a stronger resurgence in Secure Communities enforcement after 2017 or in areas that were declared as Sanctuary Cities before the treatment period.

The exercise is informative but not causal, as cross-state differences in SC enforcement intensity may be correlated with unobserved factors. We obtained data on detentions from The Deportation Data Project and removals under SC from the TRAC database. We computed the percentage change at the state level between 2017–2018 and 2015–2016, the latter corresponding to a period when ICE activity was comparatively subdued following the temporary discontinuation of SC. Unlike the original phased rollout of SC between 2008 and 2013, its 2017 reinstatement occurred through immediate nationwide activation rather than gradual deployment.

Beginning in January 2017, a sharp increase in detentions and removals is observed in a substantial number of states, which in many cases continues through 2018. The evolution of detentions and removals by state is shown in Figures A.5 and A.6 in the Appendix, where a U-shaped pattern is visible for most states, albeit with considerable heterogeneity in magnitude, as some states experienced only moderate or limited increases.

Under a triple difference-in-differences framework, we exploit cross-state variation in the percentage change in Secure Communities enforcement intensity by interacting this measure with our coefficient of interest. Specifically, we estimate the interaction between Binational × Post and a continuous measure of the post-2017 resurgence in enforcement activity. This specification allows us to assess whether the estimated increase in marriage is amplified in states experiencing stronger enforcement resurgence.

Table 4: Difference-in-Differences Estimates of Marriage Transitions and Secure Communities Enforcement Intensity (2017–2018). Pseudo-Panel two-year sample.

	(1) Detentions	(2) Removals
Binational × Post	0.02075***	0.01893***
	(0.0025)	(0.0034)
Binational × Post × SC intensity	-0.00033	0.00007
	(0.00039)	(0.00061)
Observations (000s)	231,657.2	233,433.8
R-squared	0.294	0.294
State FE	YES	YES
Controls	YES	YES
DACA controls	YES	YES
Macro controls	YES	YES
Undocumented controls	YES	YES

Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions and the interaction with the intensity of SC reinstatement. Intensity of enforcement is calculated based on the percentage change in the number of detentions and removals between the 2017–2018 and 2015–2016 periods, for each variable separately. Coefficients on the triple interaction are scaled to reflect a 10 percentage-point increase in enforcement intensity. Pseudo-Panel data is reconstructed using retrospective information from the ACS (see Data and Sample Construction Section). Standard errors are clustered at the state level. *Source:* Own author calculation based on ACS data,TRAC and Deportation Data Project.

The results, reported in Table 4, indicate that the baseline Binational × Post effect remains positive and statistically significant across both measures of enforcement intensity. In contrast, the triple interaction term between Binational × Post and Secure Communities intensity is economically small and statistically indistinguishable from zero in both cases. These estimates provide no evidence that the marriage response is systematically stronger in states with larger enforcement increases. The results hold in the same direction for any of the three samples constructed.

Table A.10 in the Appendix further divides states into three groups based on the intensity of the resurgence in SC activity for detentions and removals (see Figure A.7 in the Appendix). The results show that the estimated marriage response is statistically significant and positive across all groups and remains present even in states with low Secure Communities resurgence, reinforcing the view that the effect is not solely driven by realized enforcement actions.

6.3. Sanctuary Cities

Sanctuary cities are local jurisdictions that limit cooperation with federal immigration enforcement by restricting ICE detainers or limiting information sharing with federal authorities. Sanctuary policies expanded gradually following the initial rollout of Secure Communities in 2008, with a more pronounced wave of adoptions around 2014 and again in 2017, often as an explicit institutional response to federal enforcement efforts.

Although the jurisdiction can not prevent federal enforcement directly, the sanctuary status implies a reduction in detention and deportation risk for undocumented immigrants (Transactional Records Access Clearinghouse, 2019), by weakening the channels through which local law enforcement collaborates with federal immigration authorities. If the enforcement channel were the primary mechanism driving our results, we would expect a smaller increase—or potentially no increase—in marriage rates among binational couples residing in sanctuary jurisdictions, where effective immigration enforcement is less intense. Of course, similar marriage response across sanctuary and non-sanctuary jurisdictions not necessarily invalidate the enforcement channel, but would be suggestive that the enforcement alone cannot fully account for the observed effects.

We gather data on sanctuary jurisdictions from the replication files of Alsan and Yang

(2024), who document sanctuary status through 2017 using ICE administrative records. We complement this dataset with information on jurisdictions adopting sanctuary policies between 2016 and 2019, using data from the Immigrant Legal Resource Center (ILRC). The map for the distribution of Sanctuary areas and their timing is offering in Figure A.9.

In our empirical analysis, we focus on jurisdictions that had declared sanctuary status prior to the treatment period, excluding areas that adopted sanctuary policies after 2016. This restriction ensures that sanctuary designation is predetermined with respect to the post-treatment period, thereby avoiding contamination from jurisdictions that changed their status during the period under study. By holding sanctuary status fixed, we obtain a cleaner and more conservative comparison.[5]

Table 5 presents the results from interacting the coefficient of interest with a binary indicator equal to one for jurisdictions that had adopted sanctuary status prior to 2016. The baseline Binational $\times$ Post effect remains positive and statistically significant across specifications. By contrast, the triple interaction term is not statistically different from zero, indicating no detectable differential in marriage rates between jurisdictions where immigration enforcement becomes operative again in 2017 and those where cooperation with federal authorities is limited or restricted. These findings are consistent across both the cross-sectional sample and the one-year pseudo-panel specification.

Taken together, these findings are consistent with a generalized response to changes in immigration policy expectations, while not ruling out a complementary role for realized enforcement. Although cross-state variation in Secure Communities resurgence is not interpreted as exogenous, the effect is not confined to states experiencing stronger enforcement increases. The fact that marriage responses emerge already in 2016, prior to the formal reactivation of Secure Communities, suggests that campaign-induced shifts in immigration policy expectations were an important driver, with enforcement dynamics potentially reinforcing

[5]It is reasonable to expect that in jurisdictions with longer-standing sanctuary policies, the credibility and perceived effectiveness of such measures may be greater among the immigrant population.

these effects thereafter.

Table 5: Difference-in-Differences Estimates of Marriage Transitions and Secure Communities Enforcement Intensity (2017–2018). Pseudo-Panel two-year sample.

	Get Married
Binational × Post	0.021***
	(0.0037)
Binational × Sanctuary	-0.003
	(0.0040)
Post × Sanctuary	-0.003*
	(0.0015)
Binational × Post × Sanctuary	0.005
	(0.0059)
Observations (000s)	158159.5
R-squared	0.295
State FE	YES
Controls	YES
DACA controls	YES
Macro controls	YES
Undocument controls	YES

Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions and its interaction with a binary indicator for sanctuary jurisdictions adopted prior to 2016. The dependent variable is an indicator equal to one if a cohabiting unmarried couple transitions into marriage in a given calendar year. The specification includes the full set of individual and state-level controls, year and state fixed effects. Standard errors are clustered at the state level. *Source:* Own author calculation based on ACS data and sanctuary classification from Alsan and Yang (2024) and ILRC.

7. Heterogeneity

In this section we explore heterogeneous effect for particular sub-population that allow us to understand and characterize better the results from previous section.

7.1. Unauthorized immigration

Following the methodology of Borjas (2017), we indirectly identify potential unauthorized immigrants in the ACS by classifying as legally authorized those foreign-born individuals who are naturalized citizens, arrived before 1980, are veterans, work in the government sector, receive SSI, Medicaid or Medicare benefits, are employed in licensed occupations, or are students likely to hold temporary legal visas. The remaining foreign-born noncitizens are classified as likely unauthorized. We then define binational couples based on the legal status of the noncitizen spouse. [6]

If likely undocumented immigrants face a higher level of uncertainty and a higher risk of being arrested and deported, we would expect that the effect on marriages for this subgroup will be higher, as the marital union acts as the most direct legal channel toward regularization. [7] Even when adjustment is not immediately feasible, marriage may improve access to legal relief and reduce deportation risk in expected terms.

Table 6 shows that the post-2015 increase in marriage rates is concentrated among binational couples with an unauthorized immigrant member. In the cross-sectional data, only the coefficient for this group is positive and statistically significant, showing a marginally increase in marriages of 2.18 p.p (standard error = 0.0042). In the one-year pseudo-panel sample, the estimated effect for unauthorized remains significant (1.77 p.p) and more precisely estimated (standard error = 0.0036), and in this case the coefficient is also positive for authorized couples, but smaller in magnitude (1.14 p.p) and statistically significant only at the 10 percent level (standard error = 0.0057.)

We exclude results from the two-year pseudo-panel sample because several criteria used in the classification, such as occupational category, employment in the government sector, or participation in public benefit programs, cannot be reliably reconstructed retrospectively and may also change with marital status. For this reason, the cross-sectional specification is the preferred and more conservative approach for this exercise.

[6] It is important to note that this is an approximation of the undocumented population, and not a exact definition of the status.

[7] As immediate relatives of citizens, undocumented spouses may become eligible for adjustment of status (conditional on lawful entry), apply for permanent residency or obtain temporary work authorization during the process.

Table 6: Difference-in-Differences Estimates by unauthorized and authorized immigrants. Cross section and 1-year pseudo panel.

	Unauthorized	Authorized
Panel A: Cross-Section Sample		
Binational × Post	0.0218***	0.0116
	(0.0042)	(0.0075)
Observations (000s)	68071.0	66082.4
R-squared	0.382	0.373
Panel B: Pseudo-Panel (1-Year)		
Binational × Post	0.0177***	0.0114*
	(0.0036)	(0.0057)
Observations (000s)	145622.0	141022.7
R-squared	0.373	0.367
State FE	YES	YES
Individual Controls	YES	YES
Macro Controls	YES	YES
DACA Eligible	YES	YES

Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions, estimated separately for binational couples with an unauthorized and an authorized immigrant member. Unauthorized status is imputed following the methodology of Borjas (2017). All specifications include the full set of individual and state-level controls, year and state fixed effects. Standard errors are clustered at the state level. *Source:* Own author calculation based on ACS data.

7.2. Targeted Nationalities

In a similar spirit, we identify binational couples where the immigrant member has a targeted nationality. The anti-immigrant rhetoric and the local enforcement activities where notoriously concentrated in specific sub-groups of immigrants, specially those coming from Mexico and Latin America, Africa, Russia and some Middle East banned countries[8]. Based on the country of origin, we classify as targeted immigrant if the person was born in any of these countries. As the country of origin is a time invariant characteristic, we are more

[8]This category includes individuals born in Saudi Arabia, Yemen, Oman, Lebanon, Syria, Iraq, Iran, Jordan, Kuwait, United Arab Emirates, Qatar, Turkey, Israel/Palestine, Afghanistan, Armenia, Georgia, Azerbaijan, Pakistan, Bangladesh, Nepal, and Sri Lanka.

reliable in both pseudo-panel sample in this exercise.

Consistent with the finding for the case of unauthorized, in Table 7, in columns 1 and 2, we present evidence that the increase in marriages rates is concentrated on binational couple with one of the member has a targeted nationality. Across the three samples we observe a marked positive effect statistically different from zero (ranging from 2.39 p.p. to 2.44 p.p.), whereas for the group of non-targeted countries the point estimate is low and not distinguishable from zero with the only exception in the two-years sample where the coefficients is around 0.08 p.p and significant at 10% level.

7.3. Education

Lastly, we explore whether there are heterogeneous effects by the educational level of the household head or first member in the couple (as declared in ACS). We expect lower-educated immigrants being more likely to work in informal or low-skilled occupations with greater exposure to local law enforcement and fewer institutional protections, which may increase both their perceived and actual risk of detection.

Also, they typically have fewer financial resources to afford legal representation and may face greater informational constraints when navigating complex immigration procedures. In addition, compared to higher-educated immigrants, they are less likely to have access to alternative legal channels of regularization, such as employment-based visas or employer sponsorship. In contrast, higher-educated immigrants may possess greater economic resources, stronger networks, and alternative adjustment options, reducing the marginal incentive to respond to enforcement shocks through marriage.

Table 7, in columns 3 and 4, supports the hypothesis of a stronger response among low and mid educated immigrants. For the cross section and a 1 year pseudo-panel sample we obtained a marginally positive increase in the marriage rates only for this group, being close in magnitude (2.00 p.p and 1.97 p.p) and statistically different from zero in both cases (standard error of 0.0044 and 0.0033 respectively). For this sample we can not instead detect

Table 7: Difference-in-Differences Estimates by Targeted Status and Education Level. Cross section, 1-year and 2-year pseudo panel.

	Targeted Status		Education Level	
	Targeted	Non-Targeted	Low-Mid	High
Panel A: Cross-Section Sample				
Binational × Post	0.0239***	0.0041	0.0200***	0.0050
	(0.0047)	(0.0064)	(0.0044)	(0.0058)
Observations (000s)	68210.0	65943.4	51139.4	18251.3
R-squared	0.379	0.377	0.319	0.549
Panel B: Pseudo-Panel (1-Year)				
Binational × Post	0.0220***	-0.0007	0.0197***	0.0008
	(0.0039)	(0.0052)	(0.0033)	(0.0050)
Observations (000s)	145896.3	140748.4	107479.0	41278.1
R-squared	0.371	0.369	0.328	0.468
Panel C: Pseudo-Panel (2-Year)				
Binational × Post	0.0244***	0.0080*	0.0209***	0.0123***
	(0.0031)	(0.0041)	(0.0031)	(0.0037)
Observations (000s)	230061.8	221576.5	166959.4	68119.4
R-squared	0.293	0.292	0.266	0.346
State FE	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES
Macro Controls	YES	YES	YES	YES
DACA Eligible	YES	YES	YES	YES
Undocument Controls	YES	YES	YES	YES

Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions, estimated separately by targeted nationality status and by education level of the household head. Targeted immigrants are defined based on country of birth, including Mexico, Latin America, Africa, Russia, and selected Middle Eastern countries, as described in the text. Low–mid education includes primary and secondary schooling, whereas high education refers to post-secondary attainment. All specifications include the full set of individual and state-level controls, year and state fixed effects. Standard errors are clustered at the state level. *Source:* Own author calculation based on ACS data.

a sizeable effect statistically different from zero for the highly educated group.

In panel C we estimate positive and significant effect for both groups, 2.09 p.p for low-mid and 1.23 p.p. for high educ. Although different from the previous two panels, the results are consistent with this sample, which captures more educated, high socioeconomic status, and long-standing couples. Indeed, by comparing the observations between panels B and C, the increase in the sample size is coming only from an increase in the highly educated column. Couples of these characteristics who have been together for longer are more prone to marginally increase the formalization of the union. This is consistent with an increase for both groups.

Overall, our findings for the effect in three groups analyzed, are consistent with responses in marriages being concentrated and higher for sub-groups facing a higher degree of uncertainty regarding the migration policy and a higher probability of being targeted by the immigration enforcement.

8. Robustness

8.1. Treatment timing

As a first robustness exercise, we assess whether our results are sensitive to the definition of the post-treatment period. One potential concern is that Donald Trump's candidacy may not have been perceived as electorally viable at the beginning of his campaign, implying that proposed changes in immigration enforcement were not immediately considered credible by affected populations. Under this view, the relevant policy uncertainty shock may have materialized only after the Republican primaries, or more plausibly, after the 2016 presidential election when enforcement policy changes became imminent.

To address this concern, we replicate Table 2 redefining the post-treatment period to begin in 2017, once the Trump administration took office and immigration enforcement priorities were formally implemented. Table A.11 summarizes the results. In the cross-sectional sample, the estimated effects remain positive and statistically significant across specifications. In our most comprehensive specification, we estimate an average increase of 1.5 percentage points in marriage rates among binational couples (standard error = 0.0045), corresponding to approximately a 8 percent increase relative to the pre-treatment baseline (18.6 percent). A similar pattern is verified for the pseudo-panel samples.

Our main findings are robust to an alternative specification of the treatment timing, aligning with the timing of policy implementation. The exercise rules out that the effect is driven by spurious changes in marriage unrelated to the immigration enforcement policy of the Trump administration.

8.2. Both non-citizens couple as control group

The next robustness exercise evaluates to what extent our results holds if we adopt a different control group. Instead of using the evolution of marriages among couples where both are citizens, we employ as control couples where both members are foreign non-citizens.

The exercises address the potential concerns of treatment and control being structurally different populations, as US-citizen couples may differ from a binational couple potentially in cultural differences, preferences, beliefs or attitudes. If this difference correlates with the marginally decision regarding marriage, part of the effect could be counfunding with unobserved traits.

Controlling with couples where both are foreign non-citizen may allow a comparison between more similar groups, at least in cultural dimensions. On the other hand, this is not an entirely clean control group, as couples composed of two noncitizen members may also be exposed to the migration-related uncertainty shock and be target of immigration enforcement activities.

This exposure could increase the expected value of marriage, as formal union signals family stability in interactions with authorities, facilitates household decisions such as child-bearing, relocation, or contingency planning, and grants access to private or institutional benefits unrelated to migration status—such as health insurance, housing, or childcare. For this reason, both non-citizens as control group, could be thought as a group with a ”partial” treatment, given by a more indirect effect.

However, marriage to this group does not give access to the rights that marriage a US citizen implies in terms of status adjustment, residency or work permit. Controlling for this group tests more directly the channel of citizenship as the way to legalize and protect the migratory status. If the effect remains this would be suggestive of our effect is not driven by a general post-2017 shock to marriage, and instead, we are capturing a differential response among couples for whom marriage expands immigration-related options.

When analyzing the estimates shown in Appendix in Table A.12 and A.13, an interesting patterns emerge. In the full sample (Panel A), we are unable to detect statistically significant differences in marriage responses between binational couples and couples composed of two noncitizens. However, once we restrict the analysis to Latino couples (Panel B), who represent approximately 87 percent of the noncitizen–noncitizen control group, the effects become positive and statistically significant, particularly when the treatment period is defined from 2017 onward.

Our interpretation is that, because the control group also experiences an increase in marriage rates following the treatment period and because the sample size is smaller in this specification, we are only able to detect an additional effect of being in a binational couple among groups that are more directly affected and whose behavioral response is stronger, such as Latinos. The results suggest that this is precisely the group for whom marriage expands immigration-related options, as becoming an immediate relative of a U.S. citizen provides access to legal protection and a more stable migratory status. This pattern is consistent with the findings for targeted countries.

We view these robustness results as strengthening the credibility of our main findings. By detecting an effect even when comparing binational couples to a control group that is partially exposed to enforcement-related uncertainty, we can rule out that our estimates simply capture an aggregate post-2016 (or post-2017) shift in marriage trends. Instead, the results point to an increase in marriage as a strategic, marginal response to heightened immigration uncertainty, particularly among groups most directly exposed to enforcement risk.

9. Conclusions

This paper examines whether couples respond to heightened immigration-related uncertainty by transitioning into marriage. We exploit variation in couple composition to identify differential exposure to the shift in immigration policy expectations during the 2016 U.S. presidential campaign and the subsequent tightening of enforcement in 2017.

Using American Community Survey data from 2008 to 2019, we identify cohabiting binational couples—defined as unions between a U.S. citizen and a noncitizen partner—and compare their marriage transitions to those of citizen–citizen couples before and after 2016 in a difference-in-differences and event-study framework. To increase statistical power, we complement the cross-sectional analysis with a short retrospective pseudo-panel constructed from reported year of marriage and naturalization.

We find that binational couples respond to heightened uncertainty by transitioning into marriage at higher rates. The estimated effect ranges from 1.5 to 1.8 percentage points, corresponding to an 8–10 percent increase relative to pre-shock marriage rates. Our results are consistent across the different panels, robust to a wide range of controls, and are not explained by the potential eligibility of binational couples into the DACA program. Event-study estimates show no evidence of differential pre-trends and indicate that the response begins in 2016, prior to the formal reinstatement of Secure Communities.

We do not find larger responses in states experiencing greater post-2017 enforcement intensity, nor do we observe systematic differences between sanctuary and non-sanctuary jurisdictions. Instead, we find evidence of higher marriage rates in districts where Trump had better performance in the 2016 primary election. In particular, increasing the margin of votes over the runner-up and decreasing it with respect to the leading candidate, has a positive effect on couples responses. This pattern is reinforced for couples with at least one child under 16 years of age in the household.

These patterns suggest that shifts in policy expectations during the campaign period played a central role, with subsequent enforcement dynamics potentially reinforcing the initial response. This is also supported by heterogeneity analysis, revealing that the effect is concentrated among likely unauthorized immigrants, lower- and middle-educated households, and individuals of nationalities prominently targeted in political rhetoric.

This paper contributes to the literature by documenting the legal formalization of unions as a strategic reaction to shift in policy uncertainty and migration enforcement. The results highlight the importance of strategic and behavioral responses not only to policy targeted programs but also to the shift in expectations and perceived risk. When state institutions are perceived as sources of uncertainty rather than protection, individuals may respond by seeking alternative forms of institutional security. In this sense, marriage becomes a means of safeguarding long-term investments in joint life projects: family formation, employment trajectories, and shared residency.

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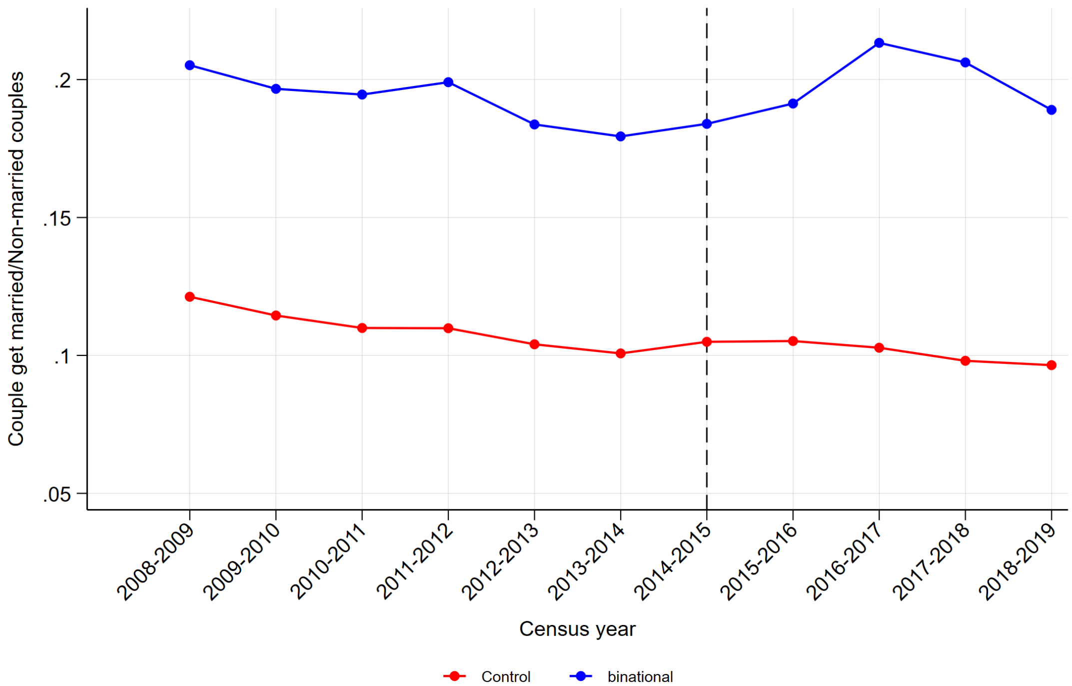
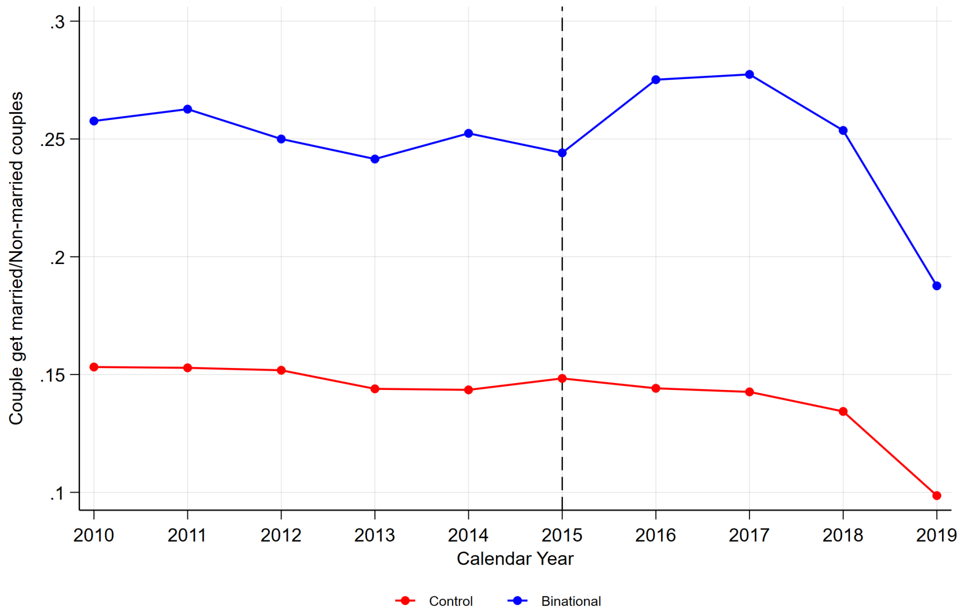
A. Appendix

Figure A.1: Evolution of Marriage Rates by Couple Type. Sample based on cross section data.

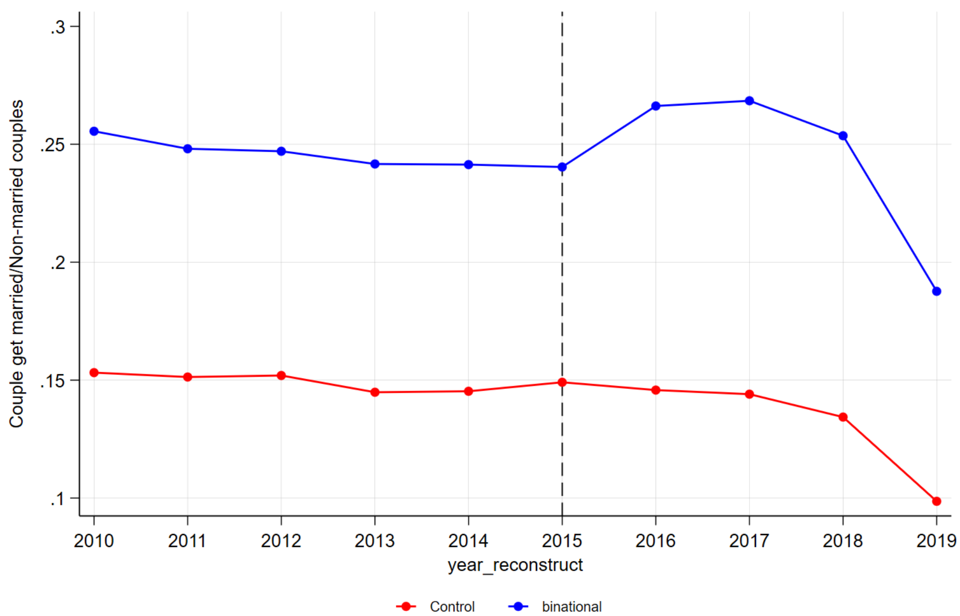


Notes: The figure displays average marriage transition rates by couple type using cross-sectional data. Two-year averages are reported to smooth year-to-year variation given small sample size for binational couple in a given year. Source: Own author calculation based on ACS data

Figure A.2: Evolution of Marriage Rates by Couple Type. Pseudo-panel samples.



(a) Panel window: 1 year



(b) Panel window: 2 years

Notes: The figure displays average marriage transition rates by couple type using the pseudo-panel sample constructed from retrospective ACS information. Source: Own author calculation based on ACS data.

Table A.8: Difference-in-Differences Estimates of Marriage Transitions. One-year Pseudo-panel sample.

	Get Married				
Binational × Post	0.0193***	0.0112***	0.0106***	0.0143***	0.0155***
	(0.0028)	(0.0026)	(0.0025)	(0.0027)	(0.0028)
Observations (000s)	148757.1	148757.1	148757.1	148757.1	148757.1
R-squared	0.012	0.373	0.373	0.373	0.373
Treated mean (Pre)	0.250	0.250	0.250	0.250	0.250
State FE	YES	YES	YES	YES	YES
Controls	NO	YES	YES	YES	YES
Macro controls	NO	NO	YES	YES	YES
DACA controls	NO	NO	NO	YES	YES
Undocument controls	NO	NO	NO	NO	YES

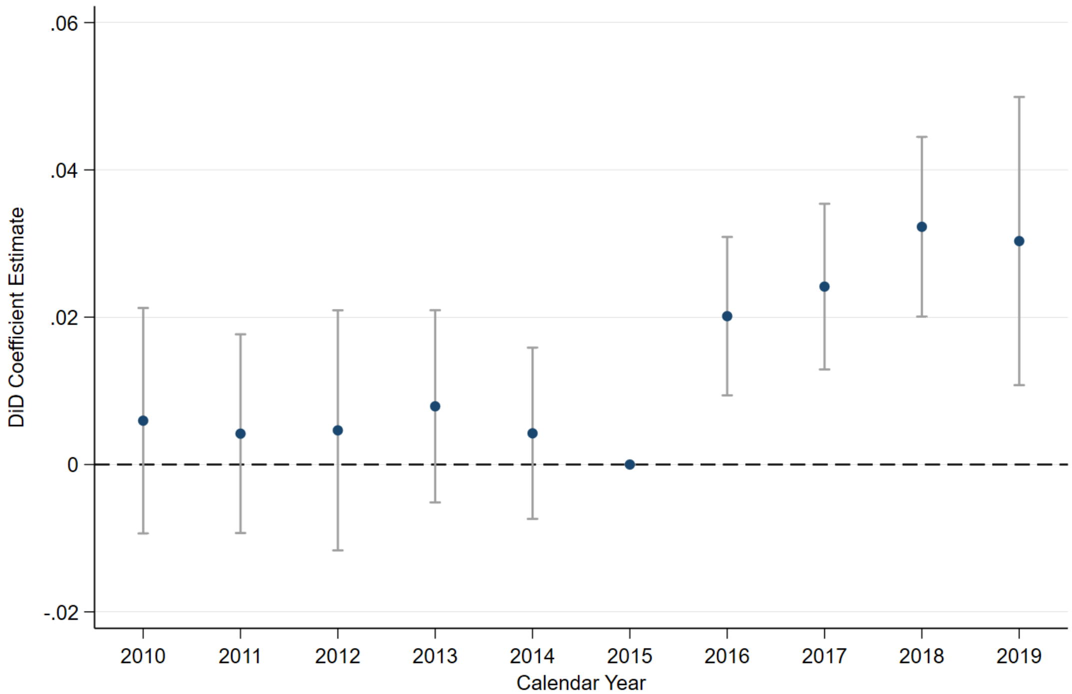
Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions. The dependent variable is an indicator equal to one if a cohabiting unmarried couple transitions into marriage in a given calendar year. Calendar year outcomes are reconstructed using retrospective information from the ACS (see Data and Sample Construction Section). All specifications include individual and couple-level controls, state-level time-varying controls, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. *Source:* Own author calculation based on ACS data.

Table A.9: Difference-in-Differences Estimates of Marriage Transitions. Two-years Pseudo-panel sample.

	Get Married				
Binational × Post	0.0228***	0.0162***	0.0157***	0.0185***	0.0196***
	(0.0022)	(0.0020)	(0.0019)	(0.0022)	(0.0022)
Observations (000s)	235078.8	235078.8	235078.8	235078.8	235078.8
R-squared	0.010	0.294	0.294	0.294	0.294
Treated mean (Pre)	0.246	0.246	0.246	0.246	0.246
State FE	YES	YES	YES	YES	YES
Controls	NO	YES	YES	YES	YES
Macro controls	NO	NO	YES	YES	YES
DACA controls	NO	NO	NO	YES	YES
Undocument controls	NO	NO	NO	NO	YES

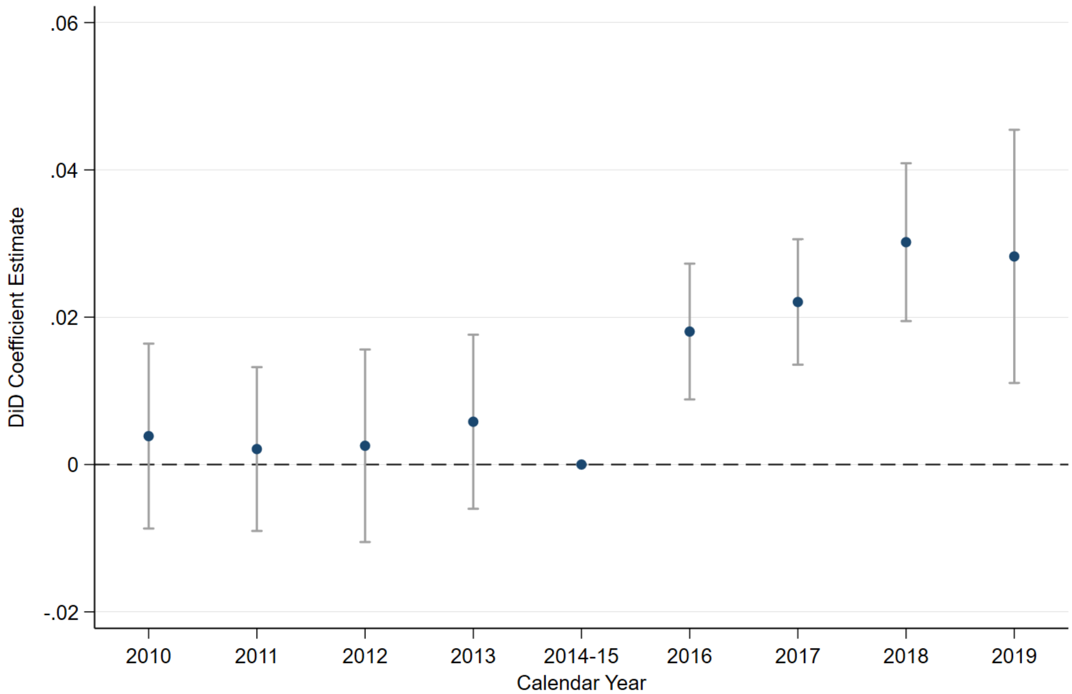
Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions. The dependent variable is an indicator equal to one if a cohabiting unmarried couple transitions into marriage in a given calendar year. Calendar year outcomes are reconstructed using retrospective information from the ACS (see Data and Sample Construction Section). All specifications include individual and couple-level controls, state-level time-varying controls, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. *Source:* Own author calculation based on ACS data.

Figure A.3: Effect of increase in immigration-related uncertainty on marriages. Event Study. Pseudo-panel sample



Notes: The figure reports event-study coefficients from equation (2), estimating the differential change in marriage transitions for binational couples relative to homogeneous citizen couples. The omitted category is 2015. The specification includes the full set of controls, state, year fixed effects and enforcement terms interacted with year indicators for the pre-period. Standard errors are clustered at the state level. Pseudo-panel data are reconstructed from retrospective ACS information. Source: Own author calculation based on ACS data.

Figure A.4: Effect of increase in immigration-related uncertainty on marriages. Event Study. Pseudo-panel sample



Notes: The figure reports event-study coefficients from equation (2), estimating the differential change in marriage transitions for binational couples relative to homogeneous citizen couples. The omitted category is the average for 2014-2015. The specification includes the full set of controls, state, year fixed effects and enforcement terms interacted with year indicators for the pre-period. Standard errors are clustered at the state level. Pseudo-panel data are reconstructed from retrospective ACS information. Source: Own author calculation based on ACS data.

Figure A.5: Evolution Removals by State under Secure Communities 2012-2020

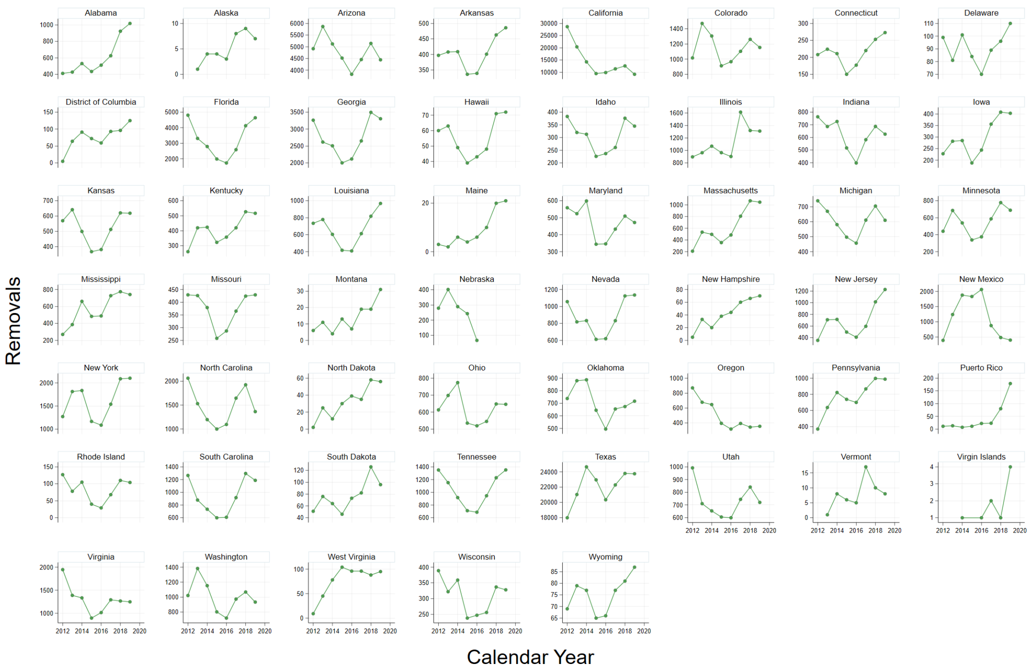
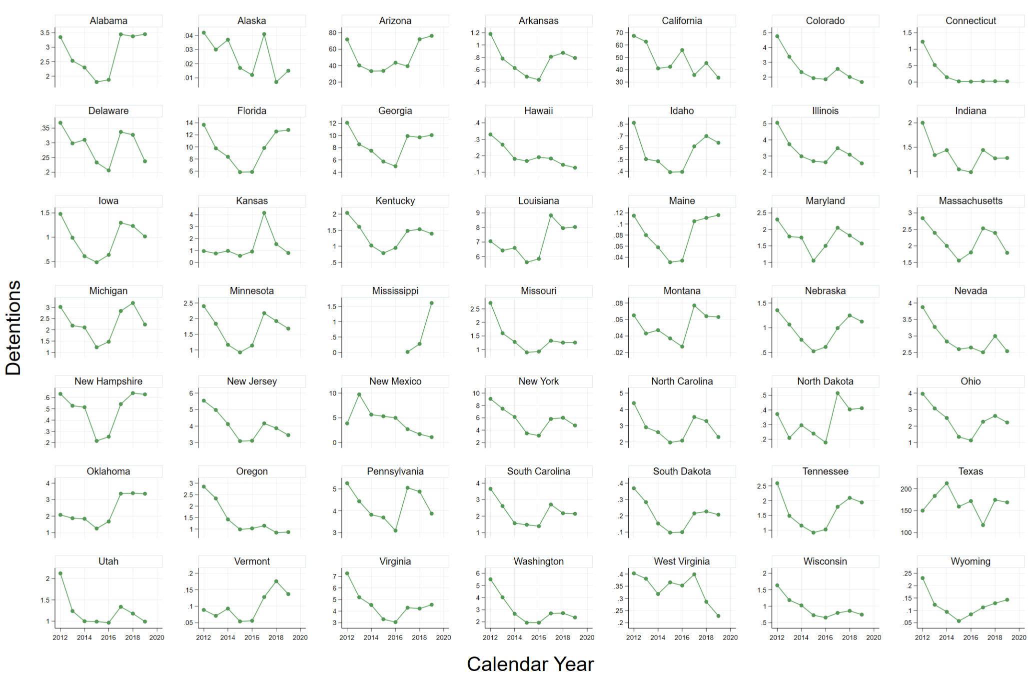


Figure A.6: Evolution of Detentions by State 2012-2020



Souce: Own author calculation based on TRAC and Deportation Data Project data.

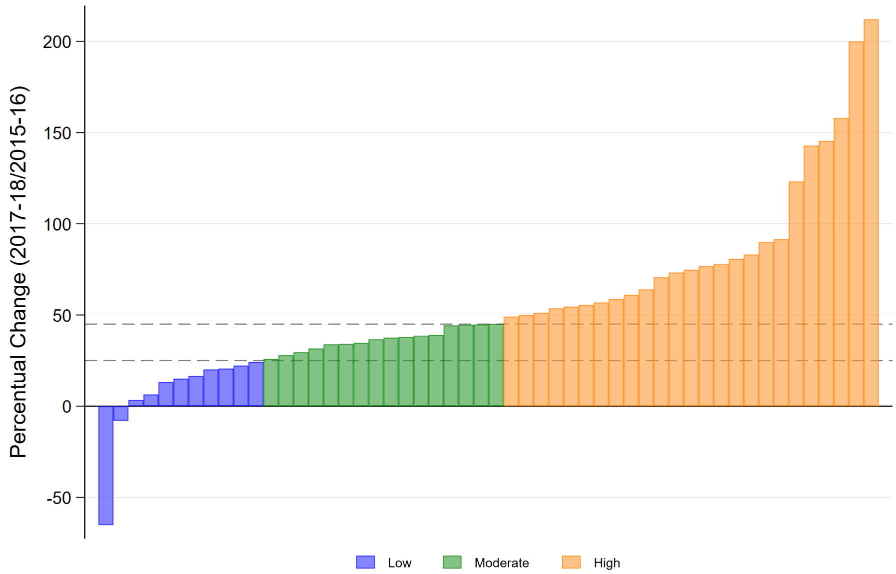
Table A.10: Difference-in-Differences Estimates of Marriage Transitions by intensity of SC enforcement 2017-18 at State Level. Psuedo Panel sample.

	SC enforcement (2017-2018)		
	Low	Moderate	Surge
Panel A: Detentions			
Binational × Post	0.0135***	0.0278***	0.0144**
	(0.0036)	(0.0059)	(0.0065)
Observations (000s)	14675.8	24017.2	29696.8
R-squared	0.407	0.387	0.369
Panel B: Removals			
Binational × Post	0.0153***	0.0205***	0.0196***
	(0.0036)	(0.0067)	(0.0062)
Observations (000s)	21551.9	19085.1	28273.0
R-squared	0.390	0.383	0.378
State FE	YES	YES	YES
Controls	YES	YES	YES
Macro controls	YES	YES	YES
DACA elegible	YES	YES	YES
Undocument controls	YES	YES	YES

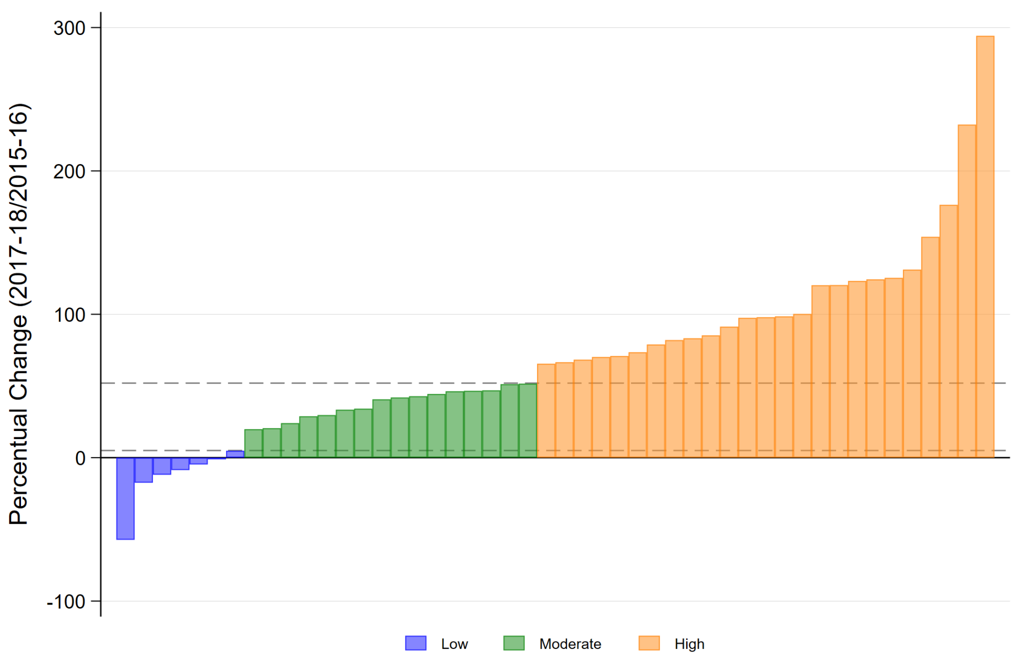
Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions. The dependent variable is an indicator equal to one if a cohabiting unmarried couple transitions into marriage in a given calendar year. Pseudo-Panel data is reconstructed using retrospective information from the ACS (see Data and Sample Construction Section). Levels of enforcement are calculated based on the percentage change in the number of detentions and removals between the 2017–2018 and 2015–2016 periods. The specific thresholds were set according to the distribution of this variable in each case, as shown in the Figure A.7. The specification includes the full set of controls. Standard errors are clustered at the state level. *Souce:* Own author calculation based on ACS, TRAC and Deportation Data Project data.

Figure A.7: Change in SC enforcement 2017-2018 vs 2015-2016

(each bar represents one State)



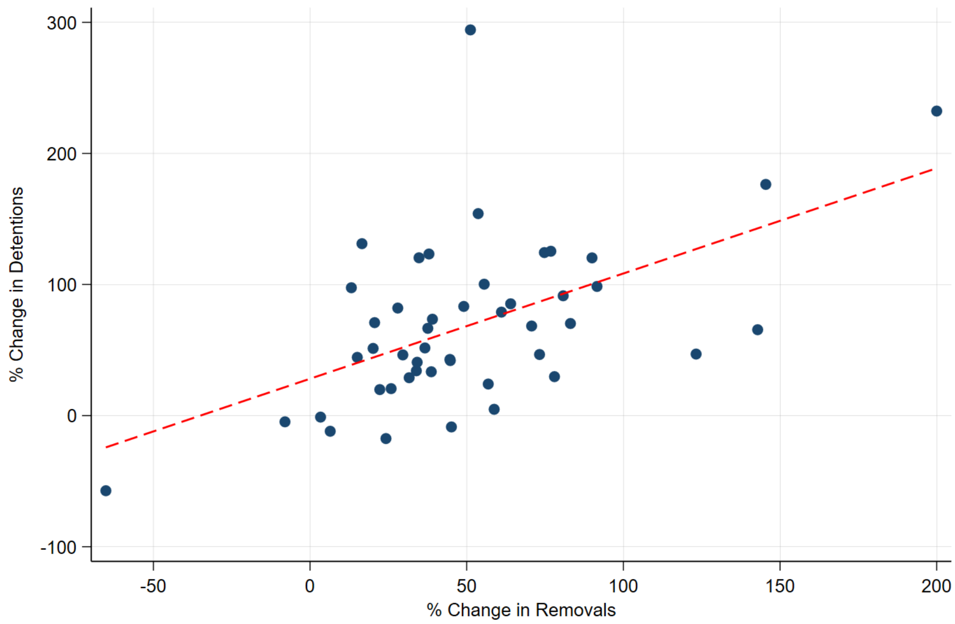
(a) Removals



(b) Detentions

Notes: Levels of enforcement are calculated based on the percentage change in the number of detentions and removals between the 2017–2018 and 2015–2016 periods. The specific thresholds were set according to the distribution of each variable, as shown in the Figure. During the baseline period, the Secure Communities (SC) program was suspended. *Souce:* Own author calculation based on TRAC and Deportation Data Project data.

Figure A.8: Correlation between changes in Detentions and Removals. Percentage change 2017-18 vs 2015-16



Notes: Calculated based on the percentage change in the number of detentions and removals between the 2017–2018 and 2015–2016 periods. Each dot represent one state. Complete information on detentions or removals for the hole period is not available for Mississippi, Nebraska and Rhode Island. *Souce:* Own author calculation based on TRAC and Deportation Data Project data.

Figure A.9: Sanctuary Cities roll out. 2008-2019

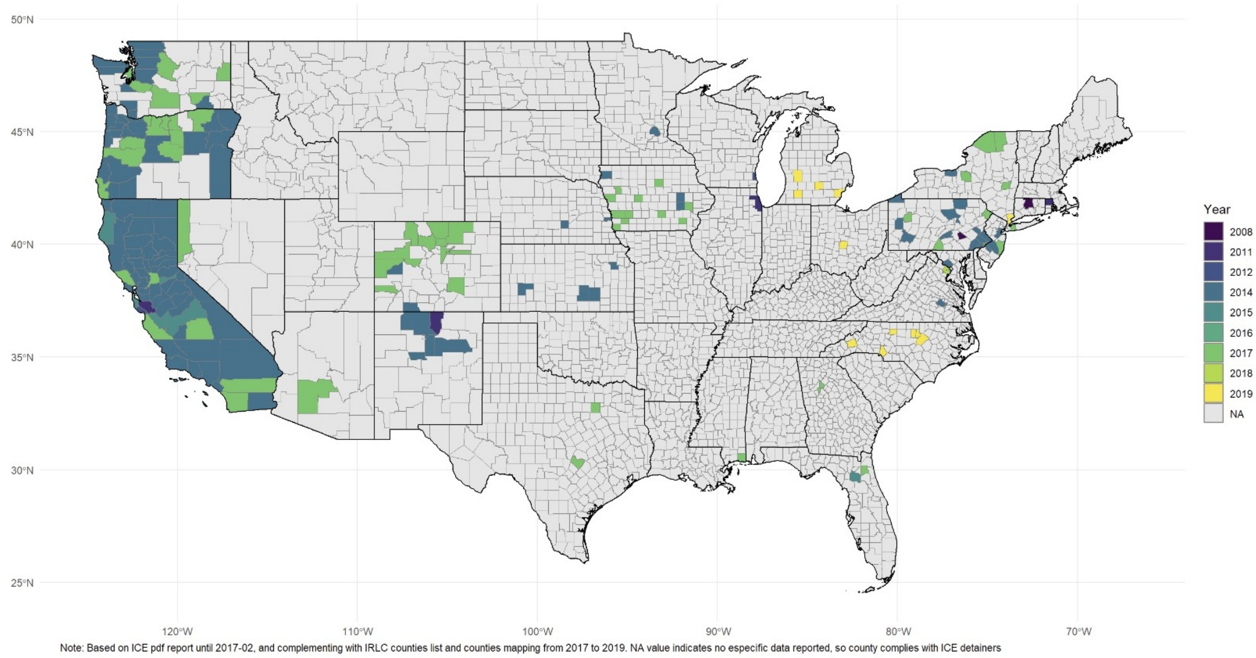


Table A.11: Difference-in-Differences Estimates of Marriage Transitions. Cross section sample. Post period starting 2017.

	Get Married				
Binational × Post	0.0171***	0.0122***	0.0115***	0.0140***	0.0150***
	(0.0046)	(0.0034)	(0.0035)	(0.0044)	(0.0045)
Observations (000s)	69390.7	69390.7	69390.7	69390.7	69390.7
R-squared	0.011	0.383	0.383	0.383	0.383
Treated mean (Pre)	0.186	0.186	0.186	0.186	0.186
State FE	YES	YES	YES	YES	YES
Controls	NO	YES	YES	YES	YES
Macro controls	NO	NO	YES	YES	YES
DACA controls	NO	NO	NO	YES	YES
Undocument controls	NO	NO	NO	NO	YES

Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions. The dependent variable is an indicator equal to one if a cohabiting unmarried couple transitions into marriage in a given calendar year. All specifications include individual and couple-level controls, state-level time-varying controls, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. The pre-treatment mean corresponds to the average marriage transition rate among binational couples before the treatment period. Standard errors are clustered at the state level. *Source:* Own author calculation based on ACS data.

Table A.12: Difference-in-Differences Estimates of Marriage Transitions. Cross Section Sample. Post Period 2016-2019.

	Get Married				
	(1)	(2)	(3)	(4)	(5)
Panel A: All Sample					
Binational × Post	-0.0020	0.0018	0.0018	0.0017	0.0013
	(0.0071)	(0.0040)	(0.0039)	(0.0039)	(0.0038)
Observations (000s)	10296.4	10296.4	10296.4	10296.4	10296.4
R-squared	0.026	0.490	0.490	0.490	0.491
Panel B: Latinos					
Binational × Post	0.0034	0.0075*	0.0076*	0.0073*	0.0072*
	(0.0072)	(0.0044)	(0.0044)	(0.0043)	(0.0042)
Observations (000s)	7986.6	7986.6	7986.6	7986.6	7986.6
R-squared	0.028	0.429	0.430	0.430	0.431
State FE	YES	YES	YES	YES	YES
Controls	NO	YES	YES	YES	YES
Macro controls	NO	NO	YES	YES	YES
DACA controls	NO	NO	NO	YES	YES
Undocument controls	NO	NO	NO	NO	YES

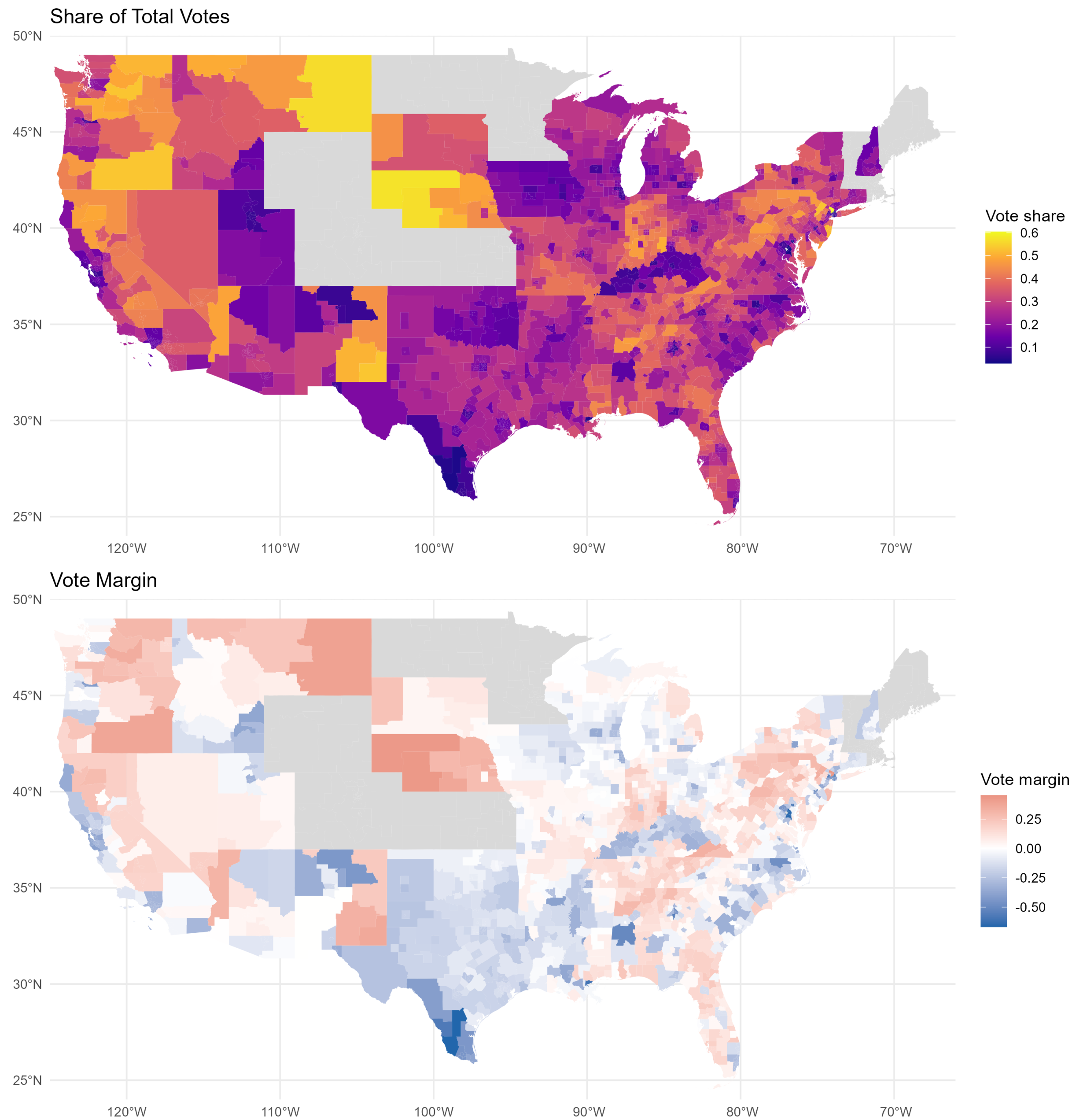
Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions. The dependent variable is an indicator equal to one if a cohabiting unmarried couple transitions into marriage in a given calendar year. All specifications include individual and couple-level controls, state-level time-varying controls, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Panel A reports estimates for the full sample; Panel B restricts the sample to Latino individuals. *Source:* Own author calculation based on ACS data.

Table A.13: Difference-in-Differences Estimates of Marriage Transitions. Cross Section Sample. Post Period 2017-2019.

	Get Married				
	(1)	(2)	(3)	(4)	(5)
Panel A: All Sample					
Binational × Post	-0.0013	0.0046	0.0049	0.0046	0.0043
	(0.0067)	(0.0046)	(0.0046)	(0.0047)	(0.0046)
Observations (000s)	10296.4	10296.4	10296.4	10296.4	10296.4
R-squared	0.026	0.489	0.490	0.490	0.491
Panel B: Latinos					
Binational × Post	0.0085	0.0135***	0.0138***	0.0132***	0.0133***
	(0.0063)	(0.0041)	(0.0042)	(0.0042)	(0.0042)
Observations (000s)	7986.6	7986.6	7986.6	7986.6	7986.6
R-squared	0.028	0.429	0.429	0.430	0.431
State FE	YES	YES	YES	YES	YES
Controls	NO	YES	YES	YES	YES
Macro controls	NO	NO	YES	YES	YES
DACA controls	NO	NO	NO	YES	YES
Undocument controls	NO	NO	NO	NO	YES

Notes: The table reports difference-in-differences estimates of the effect of immigration-related uncertainty on marriage transitions. The dependent variable is an indicator equal to one if a cohabiting unmarried couple transitions into marriage in a given calendar year. All specifications include individual and couple-level controls, state-level time-varying controls, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Panel A reports estimates for the full sample; Panel B restricts the sample to Latino individuals. *Source:* Own author calculation based on ACS data.

Figure A.10: Trump's electoral outcome in 2016 Primary Election by PUMA among all candidates.



Notes: Electoral data was sourced from Bucknell University Election Data Center and supplemented with the 2016 U.S. Election dataset on GitHub, for states where county-level data were incomplete or unavailable. Data at the PUMA level was constructed taking PUMA-County equivalence employing geographical crosswalks. Share of votes measures the percentage of votes obtained by Donald Trump in competition with the Republican candidates only. Margin of votes measures the difference in vote share over the runner-up Republican candidate (when he won) or the margin relative to the leading candidate (when he did not), computed within the Republican primary only. No election data available for Alaska, Colorado, District of Columbia, Kansas, Maine, Massachusetts, Minnesota, North Dakota, Rhode Island, Vermont, Wyoming.
Source: Own author calculation based on Bucknell University Data Center and Hamner (2017).

Table A.14: Triple Differences. Share and Margin of votes considering Republican candidates in 2016 primary elections. Pseudo-Panel 2 year sample. Post = 2016

		Child Under 16	
	All	Yes	No
Panel A: Share Votes			
Binational × Post	0.020***	0.030***	0.010
	(0.0051)	(0.0080)	(0.0071)
Binational × Post × Trump Prim. Election	0.001	0.004	-0.002
	(0.0041)	(0.0060)	(0.0054)
Panel B: Margin of Votes			
Binational × Post	0.020***	0.030***	0.010
	(0.0051)	(0.0080)	(0.0070)
Binational × Post × Trump Prim. Election	0.001	0.003	0.000
	(0.0040)	(0.0060)	(0.0052)
Obs (000s)	91,802	38,407	53,394
State FE	YES	YES	YES
Controls	YES	YES	YES
Macro controls	YES	YES	YES
DACA controls	YES	YES	YES
Undoc controls	YES	YES	YES
PUMA imm share	YES	YES	YES

Notes: Electoral data was sourced from Bucknell University Election Data Center and supplemented with the 2016 U.S. Election dataset on GitHub, for states where county-level data were incomplete or unavailable. Data at the PUMA level was constructed taking PUMA-County equivalence employing geographical crosswalks. Share of votes measures the percentage of votes obtained by Donald Trump in competition with the Republican candidates only. Margin of votes measures the difference in vote share over the runner-up Republican candidate (when he won) or the margin relative to the leading candidate (when he did not), computed within the Republican primary only. No election data available for Alaska, Colorado, District of Columbia, Kansas, Maine, Massachusetts, Minnesota, North Dakota, Rhode Island, Vermont, Wyoming. Both measures are standardized. Post period treatment was restricted to 2016 to isolate the expectations channel prior to SC enforcement revival (2017). Standard errors clustered at PUMA level in parentheses. *Source:* Own author calculation based on Bucknell University Data Center and Hamner (2017).