How Do Labor Shortages Affect Residential Construction and Housing Affordability?*

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Abstract

U.S. housing markets have faced a secular shortage of housing supply in the past decade. Most explanations in the literature have tended to focus on the distortionary effect of local housing regulations. This paper provides novel evidence on a less-explored channel affecting housing supply: shortages of construction labor. We exploit the staggered rollout of a national increase in immigration enforcement to identify negative shocks to construction sector employment that are likely exogenous with respect to local housing market conditions. We show that treated counties experience large and persistent reductions in residential construction activity, using measures of both planned and realized construction activity. This reduced housing supply is associated with increases in home prices. We also show that domestic labor supply does not fully offset immigration-related reductions in the construction sector; and that within higher-skilled construction occupations, U.S. workers see net declines as a consequence of increased immigration enforcement.

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1 Introduction

U.S. housing markets have faced a secular shortage of housing supply in the past decade. Since 2011, the U.S. on average added 1.1 million new housing units every year, which is 30% lower than the long run equilibrium annual construction units before the Great Recession, and 34% lower than the annual new construction demand estimated by Freddie Mac (Khater et al. 2018). As home prices have continued to diverge from household incomes over the past 10-15 years, why has housing supply remained so persistently low? Although housing underproduction is widely perceived as a key contributor to fast-growing house prices, the extensive academic literature exploring this question has tended to focus on the distortionary effects of excessive housing regulations (i.e. zoning and building codes). These administrative policies are typically viewed as the central factor in limiting housing supply (Glaeser and Gyourko 2018, Molloy 2020). This paper, however, provides novel evidence documenting that labor supply is also an important channel affecting housing supply. We show that negative shocks to a region's construction workforce are highly persistent, and lead to sharp reductions in overall residential construction activity as well as subsequently higher home prices.

The labor shortage in housing has remained stark over the past decade (Porter 2019, Freddie Mac 2017). Although the association is not causal, Figure 1 shows the U.S. construction sector lost 2.2 million employment (or 29% of its total employment peak at 2006) during the Great Recession, but barely recovered afterwards although the economy has greatly expanded. Even before the pandemic, construction firms ranked work shortage as the biggest hurdle for their businesses, and 78% of them were experiencing difficulty in filling their job positions.[1] We use a national shock to construction labor to quantify the sensitivity of housing market outcomes to labor supply.

Our setting exploits an increase in immigration enforcement arising from a Federal program called Secure Communities, which began in 2008 and eventually rolled out to all counties nationwide by 2013.[2] According to ICE records, this program was associated with the deportation of more than 400,000 undocumented immigrants during this time period. Other scholars have documented large impacts on local population and employment. We use the population shock of Secure Communities to identify a negative labor shock to the residential construction sector, which is well known to be a large source of employment for

[1]This is based on the 2019 Construction Outlook Survey (`https://www.agc.org/sites/default/files/Files/Communications/2019_Outlook_Survey_National.pdf`) by the Associated General Contractors of America.

[2] This program underwent several iterations between 2008 and 2021. We detail full timing in Section 2.

undocumented immigrants (Svajlenka 2021).

We first establish a first-stage by exploiting the staggered spatial rollout of Secure Communities at the county level, along with microdata from the American Community Survey, to show that treated counties experience large reductions in overall construction employment. These reductions last for at least three years after Secure Communities implementation. In addition, we also show that from the standpoint of the overall construction industry, domestic labor and immigrant labor appear to be compliments rather than substitutes. Not only does domestic labor flowing into the construction industry (either from other industries or from outside the labor force) fail to fully offset employment losses, but in fact, immigration enforcement leads to reductions in total construction employment for U.S. born workers also. We show that this effect is heterogeneous by skill. For lower-skilled occupations, reductions in undocumented workers are partially offset by increases in domestic labor supply. However, employment in higher-skilled occupations is reduced for both undocumented immigrants and U.S. citizens.

We then show that this net reduction in the construction labor force leads to a slowdown in residential construction. We use two measures of construction activity. Data on homebuilding permits allows us to measure a decrease in planned or intended residential construction. We also use housing transaction microdata to show a reduction in realized homebuilding by measuring the total quantity of new construction entering local housing markets. These reductions increase over time, and are large relative to baseline construction. Three years after Secure Communities rollout, the median county has foregone nearly an entire year's worth of additional residential construction: 633 fewer buildings are permitted, and 1,178 fewer newly constructed homes enter the market.

In the final section of our paper, we empirically document the anticipated link between reduced quantities and increased prices. Several factors complicate this analysis. First, it is clear that increased immigration enforcement will have a demand-side impact as well as a supply-side effect: at the most mechanical level, an increase in deportations means fewer residents demanding housing services. However, by focusing on new construction, we restrict focus to a market segment that is relatively less likely to include undocumented buyers. Second, we find evidence that Secure Communities is associated with endogenous shifts in housing characteristics. Overall, the reduction in construction workforce appears to lead to smaller houses being built. However, on a quality-adjusted basis we show a large increase in the price of new residential construction. Three years after Secure Communities implementation, the average new construction parcel is $50,000 dollars more expensive, an increase of 16% relative to the baseline.

Overall, our results show that housing supply is highly sensitive to labor supply. Negative shocks to the construction workforce appear to be highly persistent, and appear to have very meaningful effects on real economic output. While there is widespread recognition on behalf of policymakers and academics that a growing housing affordability crisis fundamentally has its roots in restricted housing supply, most explanations have tended to assume policy-based barriers to expanding supply. Our findings suggest that factor constraints may play an important role as well. One implication is that policies designed to address housing affordability may be less effective unless they also help increase labor supplied to the construction industry. Another implication of high sensitivity between labor supplied and housing produced is that non-housing policies with second order impact on labor markets may nonetheless have first order impact on housing prices.

Related literature.—This paper contributes to three important lines of literature. First, we shed new light on the housing unaffordability issue in the U.S. Several papers (Molloy 2020, Albouy et al. 2016) highlight the secular rise of the housing cost in U.S. household budgets in the recent period and associate it with the limited house supply. Glaeser and Gyourko (2018) attributes the shortage of house supply to regulations on land use and building, since it has been well-documented both theoretically (Glaeser and Gyourko 2003, Ortalo-Magné and Prat 2014, Helsley and Strange 1995) and empirically (Malpezzi and Green 1996, Ihlanfeldt 2007, Zabel and Dalton 2011, Jackson 2018) that housing regulations reduce housing supply and make house price go beyond the marginal cost of construction. This paper complements the literature by tying the house supply shortage with a recent development in the U.S. housing market that construction labors were at a historically low level during the past decade. And we show the lack of labor is another critical factor that exacerbates the underproduction of houses.

Second, this paper contributes to the extensive literature that aims to understand the effects of immigration on local economies. Pioneered by the seminal work by Card (1990), many papers study the labor market effect using geographic variations of immigration flows, but come to different conclusions (Altonji and Card 1991, Hunt 1992, Card 2001, Friedberg 2001, Cohen-Goldner and Paserman 2011, Borjas 2017, Borjas and Monras 2017, Monras 2020). All these studies focus on a sudden influx of immigrants, like Cuban immigration into the U.S. during Mariel boatlift (Card 1990, Borjas 2017), Mexican immigration to the U.S. during Mexican Peso Crisis (Molloy 2020), and Jewish immigration into Israel after the collapse of the Soviet Union (Friedberg 2001, Cohen-Goldner and Paserman 2011). The crucial issue is that immigrants are likely to be correlated with the economic trend of the local economies, so the cross-sectional estimation tends to be biased upward. Many of these papers partially solve this endogeneity bias by using previous immigration labor share as instrumental variables, however the concern that immigration share is correlated with persistent economic shocks remains.

Our paper avoids this concern by leveraging quasi-experimental regulatory variations across U.S. counties introduced by the gradual roll-out of the Secure Communities program. Several other papers have used this same setting. East et al. (2018) show that Secure Communities leads to reductions in employment for likely-undocumented residents, but does not lead to local increases in either employment or wages for domestic workers. Miles and Cox (2014) shows that Secure Communities has no meaningful impact on local crime rates. Alsan and Yang (2022) show that Secure Communities leads to reduced uptake of federal social service programs for Hispanic residents, even among those not eligible for deportation.

Compared with the study on labor market outcomes, very few papers have looked at the impact on the product market. Two exceptions are Lach (2007) and Cortes (2008), which show immigration flows are associated with lower prices for non-tradable good and services, using previous immigration labor share as instrumental variables. This paper puts a particular focus on housing, which is the biggest durable consumption for households. We provide a detailed picture of the effects along both quantity and price dimensions, and in both new and resale sectors.

Third, this paper also contributes to the line of literature that studies the role of immigration in the housing market. Previous studies mainly focus on the demand channel. Saiz and Wachter (2011) and Sá (2015) find growing immigration settlement is viewed as a negative amenity and leads to native flight and slower housing value appreciation. But at the MSA level, Saiz (2003) and Saiz (2007) show more immigrants are associated with inflated housing rents. Our paper provides novel evidence that immigration flows have a first order impact on housing supply. We also document a link between this supply shock and home prices. These price estimates are potentially confounded with the demand shocks arising from increased deportations. However, we minimize contamination from this demand channel by focusing on new construction homes, a market segment that is less likely to be directly affected by demand from undocumented residents.

The rest of this paper proceeds as follows. Section 2 describes the institutional details of the immigration shock that we employ as a laboratory for studying how labor impacts housing supply. Section 3 describes our empirical approach, and Section 4 outlines the key sources of data. Section 5 presents our results. Section 6 concludes.

2 Secure Communities Background

Secure Communities was a U.S. Immigration and Customs Enforcement (ICE) program that launched at the end of 2008. The central pillar of Secure Communities was enhanced information sharing between local law enforcement and federal immigration databases. Prior to Secure Communities, local policing authorities would not, in general, investigate a detained individual's immigration status as this required the physical presence of a federal officer (Miles and Cox 2014, Alsan and Yang 2022). Under Secure Communities, fingerprint information (already collected by local law enforcement pursuant to an arrest) began to be automatically shared with the Department of Homeland Security (DHS).[3] DHS would then match those fingerprints against an internal database of foreign-born individuals. A subset of individuals in this database are eligible for deportation: individuals who have been previously deported or who have overstayed their visa; and individual's who would be subject to deportation if convicted of committing the offense for which they were detained. Given a fingerprint match, ICE would validate that the individual is removable under immigration law, and upon validation would coordinate with local law enforcement to take custody and begin deportation.

The initial launch included five counties in the last months of 2008. The program gradually expanded nationally, with the last set of untreated counties adopting at the beginning of 2013. While we have an exact date for the official start of Secure Communities in each county, our empirical analysis necessarily uses annual aggregates. Therefore we code counties as initially treated during the first year in which they have implemented Secure Communities for at least half the year. This means, for instance, that the initial set of counties launching Secure Communities in Oct–Dec of 2008 are coded as a 2009 treatment-cohort. Our results are not sensitive to this choice. Figure 2 maps the expansion of Secure Communities by year, depicting the treatment indicator used in all regressions. Appendix Figure A1 maps treatment cohorts using the actual date of Secure Communities launch, without consideration for when in the year that initial date falls.

In January 2013, all remaining counties adopted Secure Communities.[4] The policy remained in place for the next 22 months. Beginning in late 2014, U.S. immigration policy continued to shift on margins of both policy and branding. In November 2014, the Secretary of DHS announced the discontinuation of Secure Communities, and (on the same day) announced a new policy called the "Priority Enforcement Program" (PEP). The major difference between the two programs was the severity of offense that would occasion engage-

[3] Specifically, fingerprints sent to the FBI to check an individual's criminal history (an existing practice) would then be forwarded by the FBI to DHS. Miles and Cox (2014) and Alsan and Yang (2022) provide extensive detail on the tactical implementation and respective roles of local police, the FBI, and DHS.

[4] There are nine counties for which we do not have an adoption date. Each appears to be a very small county with an atypical governance structure. Our sense is that these counties are each likely folded into the administrative governance of a larger neighboring county, and therefore do not represent non-treatment regions.

ment with DHS: while all encounters with local law enforcement fell under the umbrella of Secure Communities, PEP applied only once an individual had been convicted of a relatively serious crime, or if ICE believed national security interests to be at stake. In 2017, President Trump signed an order re-instituting Secure Communities, and in January 2021 President Biden signed an executive order revoking that reauthorization.[5]

Typically this period between late-2014 and early-2021 would complicate empirical analysis, as it is somewhat unclear whether this should be deemed a 'treatment' period or not. In our setting, however, state of the art techniques in difference-in-differences analysis dictate that we use only variation through 2013, at which point all counties become treated with the original iteration of Secure Communities. We are, therefore, not using any results from the more-difficult-to-interpret period from 2014 onward. We elaborate on this issue at length in section 3.

3 Empirical Strategy

The phased rollout of Secure Communities between 2008 and 2013 allows us to run a county-level staggered Difference-in-Differences design. In it's canonical form, this research design recovers a causal impact of some intervention by comparing the gap in outcomes between treated and untreated units before and after treatment. Allowing i to denote cross-sectional units, and t time periods, the analysis is commonly implemented with two-way fixed-effects OLS:

$$y_{it} = \alpha_i + \gamma_t + \beta \mathbb{1}(treatment_{it}) + \epsilon_{it} \tag{1}$$

In recent years, several papers have shown a potential for bias in the estimated causal treatment effect, β, that arises specifically in staggered-rollout designs (De Chaisemartin and D'Haultfoeuille 2022, De Chaisemartin and d'Haultfoeuille 2020, Goodman-Bacon 2021, Callaway and Sant'Anna 2021, Borusyak et al. 2021). Two potential drivers of this bias are strongly present in our setting. We first outline the concern, and then describe our preferred solution.

In the TWFE regression, the weighted average represented by β arises from pairing untreated units that become treated (denote as $0 \rightarrow 1$) with two types of comparison observations: units that are, and remain, untreated ($0 \rightarrow 0$); and also units that are already

[5] https://trumpwhitehouse.archives.gov/presidential-actions/executive-order-enhancing-public-safety-interior-united-states/ and https://www.whitehouse.gov/briefing-room/presidential-actions/2021/01/20/executive-order-the-revision-of-civil-immigration-enforcement-policies-and-priorities/ respectively.

treated and remain so across the comparison window ($1 \to 1$). The second type of comparison is the potential source of bias.[6] De Chaisemartin and D'Haultfoeuille (2022) survey several papers that find this bias to be more likely in settings where most units are eventually treated. The scarcity of untreated units in later periods means that the TWFE model necessarily places greater weight on potentially problematic pairings that use already-treated units for the comparison observation. A related issue arises if treatment effects increase in treatment duration. Sun and Abraham (2021) show that the resultant time-heterogeneity in treatment effects can lead to spurious violations of the parallel trends assumption that underlies causal interpretations of DiD estimators.

Both signifiers of potential bias are strongly present in the Secure Communities setting. First, essentially every county is treated at some point. From East et al. (2018) we have an activation date for 3,134 counties. The remaining nine counties without an activation date are very small regions either with an atypical governance structure or which are grouped with another statistical reporting unit. Therefore these counties do not comprise an appropriate counterfactual region for the period after full rollout of Secure Communities. Second, the local impact of increased immigration enforcement is likely to be heterogeneous over time. An individual's choice to emigrate is presumably a function of: (i) expected economic payoff to residing in the United States while undocumented, (ii) the available payoff to remaining in the home country, and (iii) expected costs due to immigration enforcement actions. The first two of these factors are time-varying, which suggests that a shock to the expected costs of immigration enforcement will have different effects on immigration flows at different periods in time. In addition, it seems very possible that increasing immigration enforcement could an impact that increases over time: if network effects are important for generating a payoff to migrating to any region, then increased deportation may make future inflows less appealing or less likely.[7]

Several papers have presented estimators that can address these concerns with the standard TWFE model. Our preferred specifications all use the approach of Gardner (2022). This is a two-stage estimation technique that first estimates both sets of fixed effects (cross-sectional and yearly) from untreated units. Practically, this means that increasing treatment effects over time will not erroneously shift cross-sectional averages, nor will time fixed effects late in our sample rest primarily on outcomes in treated regions. Purged of problematic

[6] De Chaisemartin and D'Haultfoeuille (2022) survey the findings of several papers in this literature and provide unified notation that illustrates the econometric sources of bias.

[7] Alternatively, one could tell an opposite story as well: if local demand for immigrant residents is static and capped for any reason, then increased deportations could make immigration more attractive. Either pattern would lead to a causal effect that shifts as a function of treatment time.

variation, these estimated fixed effects are then used to produce fitted values which become the dependent variable in the second stage estimation. This approach is appealing for both its transparency and computational simplicity. In particular, analytic inference is possible, which is advantageous for our analysis of prices which draws upon transaction microdata spanning tens of millions of observations. While the use of bias-robust estimators does induce meaningful differences from a standard (and incorrect) DiD estimator, our results are not sensitive to the specific estimator selected. In our Appendix we show that our findings are consistent across a range of other bias-robust estimation techniques.

4 Data

This section introduces data sets used in this study.

4.1 SC

Information on the roll-out dates of SC comes from East et al. (2018), who gather the implementation date of SC at county level from the U.S. Immigration and Customs Enforcement (ICS).[8] Based on the roll-out dates, we construct a county-year level dummy variable, SC_{jt} which indicates whether SC has been implemented in each county j and each year t. Due to additional concerns about bias in DiD designs that use continuous treatment variables, we create a binary indicator for SC treatment rather than a continuous variable capturing partial treatment in the implementation year. We code a county as treated in year-of-launch if SC was introduced for at least six months of the year, and untreated otherwise. Once treated a county remained treated through out sample.[9] SC was implemented in all counties by 2013.[10] Among 3172 counties, 2% adopted SC in 2009, 10% in 2010, 32% in 2011, 52% in 2012, and 3% in 2013.[11]

[8] We thank Chloe East for generously sharing this data with us.

[9] As discussed in Section 2, Secure Communities underwent periods of suspension and/or rebranding starting in 2014. As a consequence of our empirical design, our estimates come from identifying variation between 2009 and 2013, and so this post-2014 period does not affect our results.

[10] We exclude the following counties in our analysis due to missing SC implementation information: Hoonah-Angoon Census Area, Alaska (FIPS code: 02105), Kalawao County, Hawawi (15005), Shannon County, South Dakota (46113), Emporia City, Virginia (51595), Fairfax City, Virginia (51600), Manassas City, Virginia (51683), (51685), Poquoson City,Virginia (51735), Doddridge County, West Virginia (54017).

[11] If using the Calendar year of SC implementation, among 3134 counties, 0.5% adopted SC in 2008, 3% in 2009, 25% in 2010, 35% in 2011, 33% in 2012, 3% in 2013.

4.2 ACS

We gather county-year level population and employment information from the 2005-2020 American Community Survey and merge them with the SC roll-out data. County-year level variables are aggregated from individual level information with individual weights. Because SC rollout occurs at the county level, we need to create ACS measures also at the county-level. ACS microdata is released with geographic granularity at the Public Use Microdata Area (PUMA) level: regions with at least 100,000 people. This means that PUMA-to-county links are possible only for relatively large counties. We are able to create the relevant measures for 331 counties, which form the sample for our analysis of employment and population. For other results which do not rely on the microdata, we are able to produce both national estimates as well as estimates based only on the ACS-covered sample of counties.

Table 1 summarizes the county-level characteristics based on the roll-out (Calendar) year of SC. Especially at the beginning of the national rollout, SC was initially implemented in counties with larger population and larger share of non-citizen and LEFB. This pattern was more pronounced in 2008-2009 and disappeared after 2010.

4.3 Permits

County-level permits are from Building Permits Survey (BPS) in Census Bureau. The number of permits measure the number of new privately-owned residential construction in each county in each year. The permits data contains information on both the number of permitted buildings, and the total number of units represented by those buildings. Both buildings and total units are reported in bins by building size: buildings with one unit, two units, three or four units, and five and more units. In regressions, we normalize the number of permits by the county-level population in 2005.

4.4 CoreLogic

CoreLogic compiles deed transaction records and property tax roll information from U.S. County assessors and recorder offices. This data spans the near-universe of properties in the United States, including variables on property characteristics, geographical locations, ownership changes, transactions time and values. A sale is flagged as a "new construction" transaction if the property is sold from the builder to the first owner. We construct county-year level measures of new construction sold by aggregating the number of properties sold, as well as the total square footage of properties sold. Importantly, CoreLogic allows us to observe both the year in which newly constructed homes are sold, and also the year that the home was built. This allows us to be sure that any decrease in the amount of new construction sold into a given market is not simply arising from longer delays between completion and sale.

4.5 Wages

We use two different measures of wages: county-year-by-industry wage data produced by BLS' Quarterly Census of Employment and Wages (QCEW), and wage indexes constructed directly from ACS. The QCEW measure draws upon administrative employer reporting to state unemployment insurance programs, while the ACS measure relies on employee survey response. While employing potentially richer data, the QCEW measures are not disaggregated by sub-population. The ACS measure can be produced for each of our populations of interest. However, the ACS data is potentially subject to a meaningful source of measurement error: while wages earned are reported as a continuous variable, weeks worked are reported in fairly broad bins. This means that smallish intensive margin adjustments in time worked will tend to be unobservable. In a setting where reductions in work are prevalent, this may lead to a downward bias in observed measures of average wages.[12]

5 Results

5.1 Population and Employment

We begin by establishing that increased immigration enforcement under Secure Communities decreased overall labor supplied to the construction industry. Because documentation status is not asked in the ACS data, we use three demographic groupings as proxies for undocumented immigrants: non-citizens, low-education and foreign-born (LEFB), and Mexican. All three proxies are imperfect.

Non-citizens will include not only undocumented residents, but also conditional and permanent residents, as well as those holding non-immigrant status.[13] In addition, misreporting may be high if undocumented residents are hesitant to respond truthfully to questions about citizenship (Van Hook and Bachmeier 2013). The LEFB grouping is a standard designation

[12] Following the literature, we compute average wage as total income divided by total hours worked for those between 20 and 64 years old who worked at least half-time in the prior year. The concern is a precisely reported downward shift in the numerator (arising from less working overall) that is not matched by a downward shift in the denominator because reductions are not large enough to move an individual between weeks-worked bins.

[13] Source: https://travel.state.gov/content/travel/en/us-visas/visa-information-resources/all-visa-categories.html

used in the immigration literature. Of course this grouping will also include naturalized immigrants with low education. Finally, we consider respondents who indicate Hispanic heritage.[14]. Although this grouping will certainly include a large number of U.S. citizens, approximately 30 percent of the U.S. construction workforce is Hispanic[15] and an estimated 25 percent of the construction workforce is undocumented (Svajlenka 2021). The grouping of Hispanic respondents, therefore, is likely to include a non-trivial share of those potentially impacted by Secure Communities. For all results, we include a fourth grouping of U.S. born residents as a natural comparison set.

Using the ACS microdata allows us to differentiate between U.S. citizens and likely-undocumented immigrants, but also constrains us to examine relatively larger counties, as smaller counties are grouped together into a single Public Use Microdata Area and we cannot disaggregate to the county level. The population and employment data used in this section spans 331 counties with a total population of 156 million (in 2005). This represents just over half of the total 2005 U.S. population. We demonstrate a first-stage impact on population and employment in this sub-sample. Then, when we move on to focus on second-stage construction outcomes, we will estimate effects both within the ACS-covered subset of counties, as well as the entire country. We find extremely similar patterns in both samples.

Our preferred specifications are event study versions of equation 1:

$$y_{it} = \alpha_i + \gamma_t + \sum_k \beta^k (time\_since\_treatment_{it} = k) + \epsilon_{it}. \tag{2}$$

As described in Section 3, we use the bias-robust estimator of Gardner (2022) for all estimations. Because this approach relies on estimating fixed-effects from pre-treatment data and because every county is eventually treated, we face a mechanical limitation on the number of post-treatment coefficients that can be identified: this cannot exceed the number of periods separating first-treatment from last-treatment. In our setting, Secure Communities is implemented between 2009 and 2013, which means that we can estimate an impact up to four years after treatment.[16]

Figure 3 shows how Secure Communities affected overall population. We use 2005 population to normalize the dependent variable to population share. By using pre-determined

[14] The ACS data permits us to exclude respondents indicating Puerto-Rican heritage

[15] The Construction Industry: Characteristics of the Employed, 2003-2020; Bureau of Labor Statistics, available at https://www.bls.gov/spotlight/2022/the-construction-industry-labor-force-2003-to-2020/home.htm.

[16] The first date of SC implementation is October 27th, 2008. We require at least 6 months of treatment to denote a county as 'treated' for the year, meaning that six initial rollout counties are considered untreated in 2008 despite up to two months of partial treatment. Our results are not sensitive to this choice.

population from the start of our sample period, we ensure that shares are not affected by any total population changes that may arise from SC. The largest negative shock is evident in the LEFB population. As expected, Secure Communities has no impact in the pre-period, and is then associated with declines in likely-undocumented population starting upon treatment, and increasing over time. Four years after SC implementation, the average county realizes an 80bps reduction in LEFB population. For the median county in our data, which has approximately 250,000 residents, this corresponds to just over 2,000 people. While the results for non-citizens are very slightly suggestive of longer-term declines, the estimates are much noisier and all statistically insignificant. The bottom row in Figure 3 suggests that Secure Communities tends to increase Hispanic and U.S. born populations. This is not necessarily surprising. There are fairly natural stories that would suggest such patterns. If a reduction in undocumented immigrants leads to a larger stock of unfilled jobs without affecting overall labor demand, one would expect citizens or documented residents to flow into those jobs. Our central interest is in identifying a shock to construction workforce, so we do not explore these population impacts further. Instead we take the overall evidence of Figure 3 as a top-level confirmation that Secure Communities did affect regional populations. We also note that comparison of the two right-hand graphs in Figure 3 shows that total population does not decrease. The LEFB population is definitionally disjoint from the US-born population (a distinction that does not apply to the population indicating Hispanic heritage), and the peak magnitude of increase in US-born residents slightly outweighs the peak estimated decline for LEFB residents. This means that net population losses are not a potential explanation for any declines in residential construction that we find.

Figure 4 estimates the impact on construction workers within the same sub-populations. Again, we normalize the number of workers by 2005 county-wide population. Here, the evidence is highly consistent across all three groupings that encompass undocumented workers. In all cases, there is a sharp decline beginning in the year of treatment, and increasing during the horizon we can examine. Magnitudes are quite similar. Taking the estimates for the LEFB population, the initial implementation of SC leads to a reduction in LEFB construction employment equivalent to 9bps of the county population. For the median county in our data, this is equivalent to 221 fewer workers. Four years after SC rollout, we estimate a the reduction to be half-a-percent of county population—1,279 workers for the median county.

As described in Section 3, our event studies are all estimated using the two-stage estimation technique of Gardner (2022). This technique has an important empirical consequence: the control group of observations used to identify time fixed-effects shifts at each time period (because only units remaining untreated are used). In Figure 5, we show the impact of SC on LEFB construction workers by treatment-cohort using a balanced (and constant) control group of the last counties to be treated. Cohort 1 compares counties treated in 2009 with only counties untreated through 2012. Cohort 2 compares counties treated in 2010 with only counties untreated through 2012; and likewise for cohorts 3 and 4. We include this evidence to demonstrate that the unusual use of a control group that changes in composition through time does not drive the patterns we find. Each cohort, compared to a fixed control group, shows evidence of a similar decline in LEFB construction workers. Each subsequent cohort loses an estimation period, because all counties are treated in 2013. This prevents us from seeing a full four-years of evidence for each cohort, but based on what can be examined, patterns look very similar across cohorts.

Returning to Figure 4, we also find that Secure Communities leads to the decline of U.S. born construction workers which, especially in light of the population results showing increases in U.S. born residents, is surprising. If, as a first-order effect, increased immigration enforcement does not impact demand for construction services – an issue which we revisit at length in Section 5.4 – then the natural prediction would be for U.S. citizens (or legal residents) to fill the vacant positions, increasing employment share for that population. Yet the event study shows a clear decline. Magnitudes are smaller: the peak effect, two years after treatment, represents a decline of 400 workers for the median county. This is a strongly statistically significant result.[17]

One potential explanation is that construction labor markets are segmented, and that undocumented labor supply acts as a compliment to domestic labor rather than being a substitute. We test this theory according to skills-based segmentation. Studies have shown that undocumented immigrants are more likely to hold lower-skilled jobs than domestic workers.[18] It is also possible that higher rates of unionization in skilled trades create additional barriers to undocumented workers holding these jobs. If a shortage of lower-skilled labor makes it more difficult to find workers to finish framing a house, this will also reduce demand for electricians and plumbers required at the subsequent stage of construction. We test this hypothesis by using ACS occupation codes to split workers by skill.

We assign the following occupations as "lower skill": (i) construction laborers, (ii) helpers in construction trades, (iii) painters and maintenance, (iv) drywall installers, (v) carpenters,

[17] There is possibly some weak evidence to support a trend towards recovery in periods 3 and 4, however this is not statistically significant. The relatively larger standard error for the latest event-study coefficient is common across most of our results, and is likely due to having very few untreated units four years after rollout begins. As a consequence of the Gardner (2022) procedure, this means that late-sample γ_t fixed effects are estimated with less power, leading to larger standard errors in the second stage (event-time) estimate.

[18] https://www.pewresearch.org/fact-tank/2020/02/24/the-share-of-immigrant-workers-in-high-skill-jobs-is-rising-in-the-u-s/

and (vi) roofers. All remaining categories within the construction subcategory are classified as higher skill. In addition to management occupations ("supervisors") and frequently unionized occupations (e.g., "electricians" and "plumbers"), this classification includes several occupations that may have significant skill heterogeneity (e.g., "sheet metal workers" or "hazardous materials removal"). As a result our partitioning should be regarded only as a high-level separation between occupations which are likely to include the lowest-skilled workers, and a set of occupations that are, on average, higher skilled.

Figure 6 shows event-study results by skill classification. Within lower-skill occupations, domestic labor appears to be a substitute for immigrant labor. The direct impact of Secure Communities is a reduction in the likely-undocumented grouping of LEFB construction workers. This is partially, but not totally, offset by increases in U.S. born employment for lower skilled occupations. The rate at which U.S. workers replace lost LEFB labor, measured as the ratio of coefficients, is between 30 and 36% for each of the three years following SC adoption. After three years, the net effect suggests a loss of approximately 379 workers in these lower-skilled occupations for the median county.

An opposite pattern holds within higher-skilled occupations. Secure Communities also causes reductions in high-skill LEFB employment; however the impact is substantially smaller within this population. (The estimates are, in fact, statistical zeros; though a downward trend in the point estimates is still apparent.) Rather than experiencing any offsetting increase, higher-skilled U.S. born workers also see employment declines. Immigration enforcement appears to reduce the overall quantity of low-skill construction labor supplied because domestic labor only partially offsets the shock to immigrant labor supply. And this reduction in low-skilled labor supply appears to be associated with an overall shrinkage in higher-skilled labor supplied by both domestic and undocumented workers.

5.2 Direct Evidence on Homebuilding

The prior section provides indirect evidence of reduced construction activity through reductions in overall construction employment. In this section, we explore the impact on residential homebuilding more directly. We focus on two measures of residential construction activity: intended construction (residential permits), and completed new construction transactions (using administrative tax-roll microdata).

5.2.1 Permits

We begin by examining permitting intensity from the U.S. Census Building Permits Survey. We estimate event studies, as per equation 2, with total permitting per 100,000 residents as the outcome variable. Figure 7 shows the results for permitting buildings (top) and total permitted units (bottom). The two measures are very highly correlated ($\rho = .91$), and so results are quite similar between the two specifications.[19] Secure Communities leads to a sharp reduction in both permitted building and total planned units. This effect is quite large. Focusing on the top panel of Figure 7, Secure Communities leads to 69 fewer buildings per 100,000 residents in the launch year, and approximately 100 buildings per 100k residents in the next two. For the median county of 250,000 residents, this implies a total reduction of 633 buildings over three years. Across all county-years, the median number of buildings permitted is 678, meaning that the three-year effect of Secure Communities corresponds to a year's equivalent reduction in residential construction. The latest-estimated effect (at T=3) is even larger; though as usual in our results, the standard errors on this longest-horizon estimate are quite wide.

Both event studies in Figure 7 show some evidence of a statistically significant, downward pre-trend. This raises the concern that Secure Communities was initially rolled out in regions that were already experiencing declines in construction. Although nothing in the public discourse of Secure Communities implies this, that does not alleviate all concerns, as rollout being associated (deliberately or not) with any correlate of construction activity would be problematic. It is also possible that statistically-significant estimates in the pre-period represent anticipation effects: a public awareness of future immigration enforcement may induce contemporaneous response. Obtaining a permit is a forward-looking action, by definition, and so perhaps builders pull forward permitting activity (and construction plans) in anticipation of future labor shortfalls. The declines pursuant to SC treatment are larger than any pre-existing downward trend would predict, but we acknowledge that the reader should be mindful of the possibility of confounding influences in rollout.

Figure 8 disaggregates the effect by building size. Single-family homes are the chief driver of the decline, which is perhaps unsurprising as single-unit buildings also represent the majority of housing stock in most places. It also becomes clear that a possible downward trend in the pre-period is a characteristic only of single-family homes. The parallel trends assumption seems to hold much clearly for other classes of buildings. While we consider it very unlikely that Secure Communities rollout was explicitly a function of trends in one sub-class of residential housing, the concern that rollout may have been influenced by some factor that correlates with single-family home construction remains.

No large treatment effect is evident for medium-size buildings. If anything, the event

[19] In addition, we use the exact reported number of buildings and units, rather than a model-based imputation of totals which the Census also provides. This choice does not meaningfully affect results.

study suggests that SC is associated with increases in two-unit buildings, though magnitudes are small. This may point towards some margin of endogenous substitution by builders when labor becomes scarce, but we do not have any evidence that speaks more directly to such substitution. No statistically significant response is evident for three or four unit buildings. For the largest buildings, declines are also evident. While the number of buildings is small - two to four fewer buildings per year – this may nonetheless reflect a meaningful impact on housing supply, since these buildings contain large numbers of units. Figure A2 repeats the estimation by building size but focus on total units. For the largest buildings (bottom figure), the peak reduction occurs at T=1, and is approximately 27 units. Also of note is that the long-horizon estimate for total number of units is fairly small reduction of approximately five. (This estimate is also a statistical zero). While the analysis shows a statistically meaningful reduction in the number of large buildings at T=3, the evidence suggests only a moderate reduction in total units across all large buildings being built. Again, this may reflect endogenous changes in the planned number of units per building occasioned by labor scarcity, but we do not empirically explore this channel further.

The preceding set of results is conditioned on the 331 counties separately identified in the ACS microdata. However, the permits data is available for nearly every county (3,046 total). We can repeat the estimation across this larger sample. Figures A3 and A4 show the results. The patterns we find in the ACS sample are extremely similar in the full sample.

5.2.2 Observed New Construction

For several reasons, changes in permitting activity might not correspond to changes in actual construction. It is perhaps most likely that permitting activity overstates construction: builders may obtain a permit and subsequently decide to abandon the project due to downstream frictions like hiring labor or securing financing. In this case, our permit-based estimates would understate the effect of immigration enforcement. However, if there are changes in unpermitted construction, it could be the case that permitting declines overstate the true impact of labor shortages. We use administrative tax roll data from CoreLogic to test directly for new construction supplied to the housing market.

CoreLogic's deeds records contains a flag for new construction sale, which lets us aggregate a measure of new construction by county-year. Using home sales still leaves a possible wedge between total construction activity and our econometric measure: homes may be built, but fail to sell. However, we observe both the date of sale, and the year in which the house is reported built. Therefore, we can test directly for new construction amounts by using sale dates. But, crucially, by focusing on the built date, we can also test new construction completed–as long as that property sells at some point before 2022 (the last year reflected in CoreLogic). This means that our analysis will only miss new construction that fails to sell for more than 10 years, which is sufficiently long that it is likely to include only a small and highly idiosyncratic number of properties.

Figure 9 shows the results from an event study regression following equation 2, where the dependent variable is the aggregate square footage of new construction per-capita that enters the market. The top panel restricts estimation to the subset of counties separately identifiable in the ACS microdata, and the bottom panel uses all counties. In each panel, the left graph uses our preferred specification that focuses on built-year; the right-hand graphs use sale-year instead. The reductions suggested in permitted activity are clearly also mirrored in new construction sold into the housing market. The top-left graph shows a reduction in total new construction square footage of 1.2 sqft per resident in the initial year of Secure Communities. The reduction increases in each of the next three years, and is -6.8 sqft per resident after 3 years. This represents a large reduction, that is also strikingly consistent with our findings from permits. For the median county, the peak reduction is 1.71M fewer square feet. Between 2008 and 2013, the mean square footage of a newly constructed property is 3,001 square feet. Therefore, this suggests 570 fewer buildings. Cumulating over the entire post-treatment horizon, our estimates suggest a total reduction of 1,178 homes. The corresponding figure in the permits analysis is 678 fewer buildings. This suggests that there is some degree of slippage between survey-reported permits and completed construction, which is what one would expect if difficulty in securing labor leads to failure of executing in planned construction. There are not highly meaningful differences between the measure that uses the built-year of new construction and the measure that uses sale-year. This is consistent with Secure Communities having no large impact on the short-term intensity or timing of sales in the new construction segment of the housing market.

5.3 House Prices

So far we have shown a quantity response. An immigration shock reduces the number of workers in an industry which draws significantly upon undocumented labor. This reduction is persistent in time, and leads to construction slowdown which we observe in both permitting patterns and in the supply of new construction entering housing markets. We now connect this quantity response with prices.

Our primary analysis focus on new construction prices. The reasoning is twofold. First, this is the segment where we have shown a direct quantity response. Filtering theories in housing markets would lead us to expect price spillovers to existing housing stock – and we do test for this – however this link is both indirect and potentially realized on a longer timescale.

Second, focusing on new construction allows us to consider a segment where demand-side shocks arising from Secure Communities are less likely to confound estimates. At the very most fundamental level, an increase in deportations within a given area mechanically means fewer residents demanding housing services.[20] In addition, other work has shown that Secure Communities leads to a range of economic spillovers (East et al. 2018, Miles and Cox 2014, Alsan and Yang 2022). However, the housing literature has documented that new construction tends to be added at the upper end of local house price distributions, and so to the extent that undocumented immigrants are unlikely to be purchasing above-median housing stock within a given region, the demand-side impact of Secure Communities is less likely to affect our price estimates.

We estimate event studies using transaction prices as the dependent variable. The inclusion of controls for property attributes will be critical. Controls are essential because reduced labor supply may also lead to endogenous shifts in home characteristics. A standard approach to this concern would be to use a repeat-sales sample along with a property-level fixed effect to control for all (time-invariant) property unobservables. However, due to our focus on new construction, we cannot make use of a repeat-sales technique. Figure 10 shows the results of estimating the event study without controls (left) and with semi-parametric controls (right). The top panel is again the ACS-microdata subset of counties, and the bottom panel is all counties. The sharp contrast between the left- and right-hand side suggests endogenous shifts on home characteristics do occur. Without any property controls, Secure Communities is associated with declines in new construction prices. However, once controls are added, SC appears to lead to higher prices. At a high level, this suggests that treatment leads to a shift in the quality or characteristics of homes being built.

Given a shift towards smaller homes, it is unsurprising that the left-hand graphs in Figure 10 show an unconditional decline in prices. We use home characteristics reported in the CoreLogic data to control for these quality attributes. We use two major attributes to capture size: square footage and number of bathrooms. For each, we convert the continuous variable into small discrete bins, and then employ a fixed effect for each bin. We also include fixed effects for each of three available amenity indicators: presence of a pool, a fireplace, and a patio. Once we control for home attributes, the right-hand side shows that quality-adjusted new-construction prices are higher following the implementation of Secure Communities. This occurs at a delay: there is no meaningful price response until two years following implementation, after which there is a strong trend upwards. Within the ACS-

[20] Though, as discussed above, our empirical results suggest that SC is associated with net increases in population.

sample counties, the average new-construction property has become $60,000 more expensive 3 years after Secure Communities rollout: an increase of 18% relative to the average price of new-construction before SC. Price impact is similar in the national sample: a peak effect of $50,000 after three years, which represents a 16% increase.

5.4 Wages

Authors' Note: This section outlines emerging draft results. Work still in progress as of March 2023. We anticipate substantially extending this analyses in subsequent drafts.

The declines that we document in both construction employment and home-building are extremely persistent, which represents a potential puzzle. If we assume that home-builders optimize over intensity of construction before the SC shock, then what shifts should be expected given an exogenous shock to immigration enforcement? The first stage impact, as we've shown, is a reduction in workers but no reduction in total population. If increased immigration enforcement itself does not meaningfully change the optimal number of homes to build in a given region, then we would assume that builders attempt to attract workers into the construction sector in sufficient quantity to return to the prior level of employment. Absent a pool of unemployed workers easily enticed into the construction sector (which our ex-post results showing net declines in employment would certainly seem to rule out), basic economic theory would suggest that static demand and reduced labor supply would place upwards pressure on wages. In this section, we test for evidence of such wage increases.

There are possible stories that would predict a lack of wage increases. Perhaps builders currently earn zero economic profits, and therefore cannot profitably increase wages. This seems a difficult story to square with patterns of sharply increasing home prices during this time - not to mention evidence in this paper showing that Secure Communities increases new construction transaction prices (section 5.3). Another possibility is that increased immigration enforcement changes the builder's optimization. Although we do not have any direct evidence that speaks to this, we note that other literature has tended to find small effects of Secure Communities on factors like crime (Miles and Cox 2014, Hines and Peri 2019), and moderate effects on labor markets (East et al. 2018, East and Velásquez 2022) that would not seem to motivate a large shift in optimal homebuilding–again given the overall backdrop of scarcity in U.S. housing markets.

We obtain wage measures from both QCEW and ACS. The chief difference, as described in Section 4.5, is that the QCEW measure, while likely more precise, cannot be disaggregated by sub-population. The ACS measure can be computed for each population of interest, but as a consequence of reported granularity, is more prone to measurement error. We begin by estimating the impact that Secure Communities has on overall wages. Our results parallel a central finding from (East et al. 2018): Secure Communities appears to depress wages overall. The top panel of Figure 11 shows the estimated effect using QCEW data on average annual wages. SC is associated with a decline of approximately $1,000-1,500 annually, a reduction of 2-3% from the mean. QCEW average wages are computed as the ratio of total wages to total employment, meaning that this measure also captures intensive margin shifts. The bottom panel repeats the estimation using the ACS-constructed measure of average hourly wage, which will not vary (at least mechanically) with intensive margin shifts. Reductions here are smaller: a peak decline of $.20 or 87bps of the average. Using the same ACS data, East et al. (2018) find similar estimates for overall reductions of 50-60bps. Those estimations are done at the CZ-level rather than the county-level, which likely explains the difference in magnitudes.

Figure 12 uses the ACS measure to estimate impact on workers by sub-group. In the top panel, we fail to reject the null of no impact on LEFB workers. Although the point estimates suggest very large declines (approaching at 30% reduction at peak), the estimates are also extremely imprecise. As Section 4.5 describes, there is also good reason to think that aggregation structure of the underlying ACS data might bias estimates downwards. The bottom panel shows moderate declines in US wages reaching 6% at the farthest horizon. Again, however, these results may be subject to the same downward bias. At a high level, we interpret Figure 12 as a meaningful lack of evidence suggesting any *increase* in wages.

Figure 13 shows that patterns look different in the hospitality industry: one of two sectors that employ a greater number of undocumented workers.[21] Using the industry-wide QCEW measure of average wages, we find a clear pattern of increasing wages in the two years following SC rollout, although the point estimates are only marginally statistically significant. This suggests to us that there is nothing about the overall backdrop of increase immigration enforcement that rules out wage increases in more heavily-affected labor market segments.

In turn, this suggests that there is some friction within the construction industry that leads builders not to increase wages to attract additional workers. Although it is hard to believe that builders are not making any economic profits – and indeed, our results showing increases in new construction sales prices suggest that the SC itself may provide builders some extra room to raise wages – the lack of observed wage increases may reflect an equilibrium

[21] Sources vary in ranking agriculture, hospitality/service, and construction as employers of undocumented workers. Most sources seem to place agriculture or hospitality as the top two, and construction third.

outcome based on builders' beliefs. Builders may perceive that the market-clearing wage required to attain 100% replacement by domestic labor would be too high to be profitable. That belief (regardless of its veracity) could lead builders to forgo any attempt to raise wages and to reduce activity instead.

6 Conclusion

We show that negative shocks to construction labor supply are highly persistent, and have a large effect on the construction of residential housing. We exploit the staggered rollout of additional immigration enforcement under the Secure Communities program to identify shocks to the labor force that are plausibly exogenous to local housing market conditions. We empirically document a first stage: using several proxies for undocumented residents, Secure Communities does lead to reduced population at the county level. Secure Communities also leads to a reduction in the amount of labor supplied to the construction sector. This effect is heterogeneous by occupation: we show that declines in immigrant labor supplied to low-skilled occupations are partially offset by increases in domestic labor supplied. Within higher-skilled occupations, we find that (smallish) declines in immigrant labor supplied are matched with even larger declines for U.S. workers. Our interpretation is that within residential construction, low-skilled labor is a compliment to high-skilled labor. Because domestic labor only partially replaces lost immigrant labor, Secure Communities leads to a net decline of low-skilled labor, and that in turn leads to a reduction in total labor demanded.

We show that negative labor shocks are associated with reductions in homebuilding, using two measures: both planned future construction (permits) and realized construction (observed sales of new homes). In both measures, Secure Communities leads to an economically and statistically large slowdown in construction activity. We also show how this reduction in housing supply affects the prices of new homes. While the average home sold declines in price, our results suggest that this comes from endogenous adjustment on home characteristics. The quality-adjusted price of newly constructed homes increases following Secure Communities.

We also find a striking lack of evidence for builders increasing wages in order to attract replacement workers. Assuming that the immigration shock does not change the optimal amount of construction for firms – which seems broadly reasonable given longstanding housing stock shortages and no total population declines in treated counties – the lack of wage adjustment to attract more labor seems surprising. We show that Secure Communities does appear to induce wage increases in another industry which draws heavily upon undocumented workers, suggesting that industry-specific features may drive the lack of wage increases for construction workers.

Housing supply in the United States has been starkly lower than average for most of the past two decades. This paper provides novel evidence on a new channel that has substantial impact on housing supply: shortages in labor supplied to the residential construction sector. This paper also suggests that immigration policy, along with other interventions that directly affect domestic labor supply, may be important levers for policymakers interested in overall home affordability.

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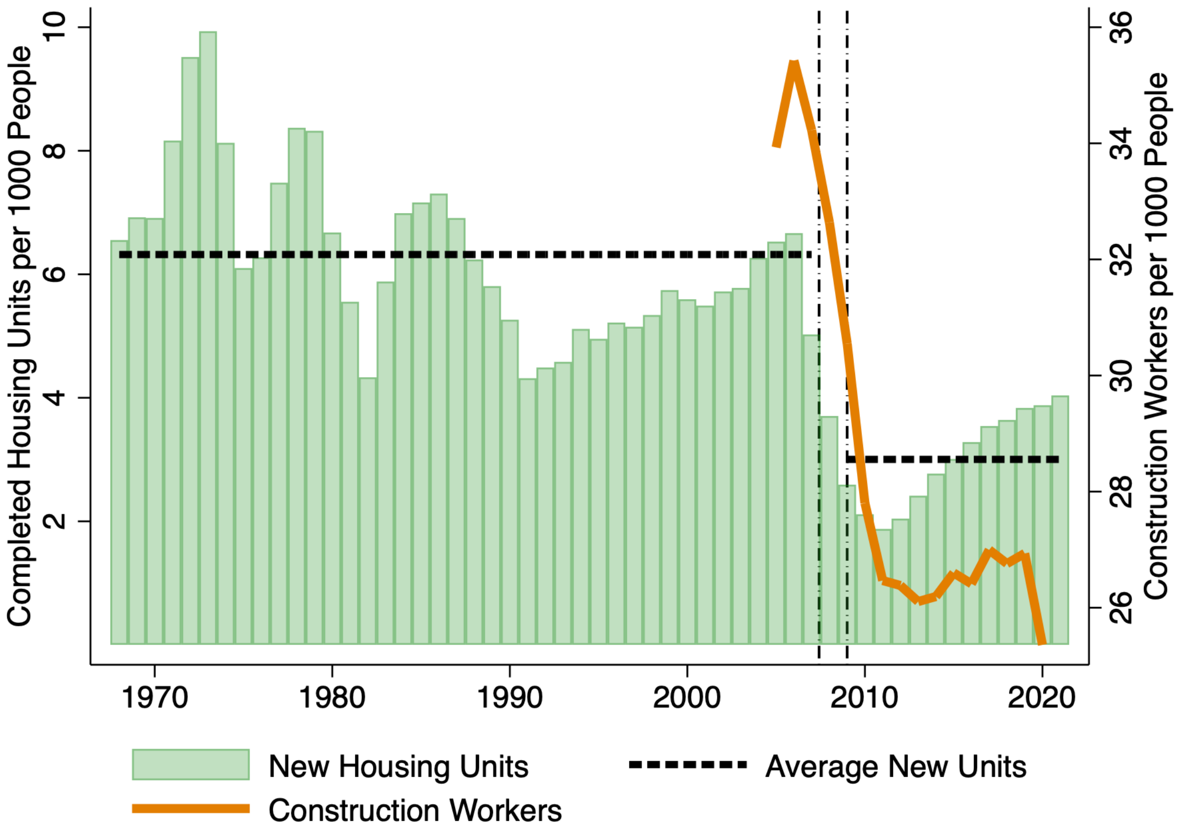
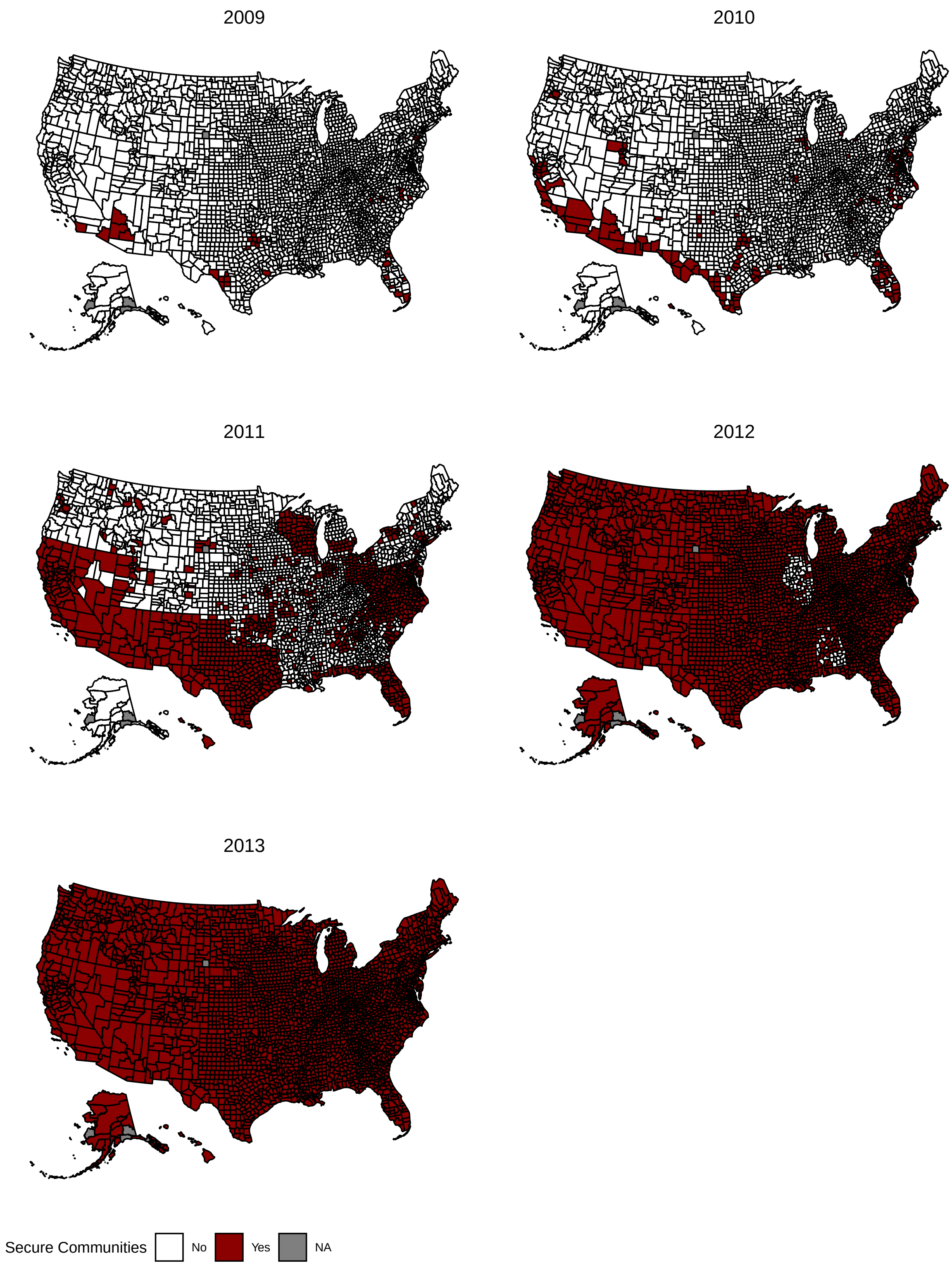
Figure 1: New Constructions and Construction Workers



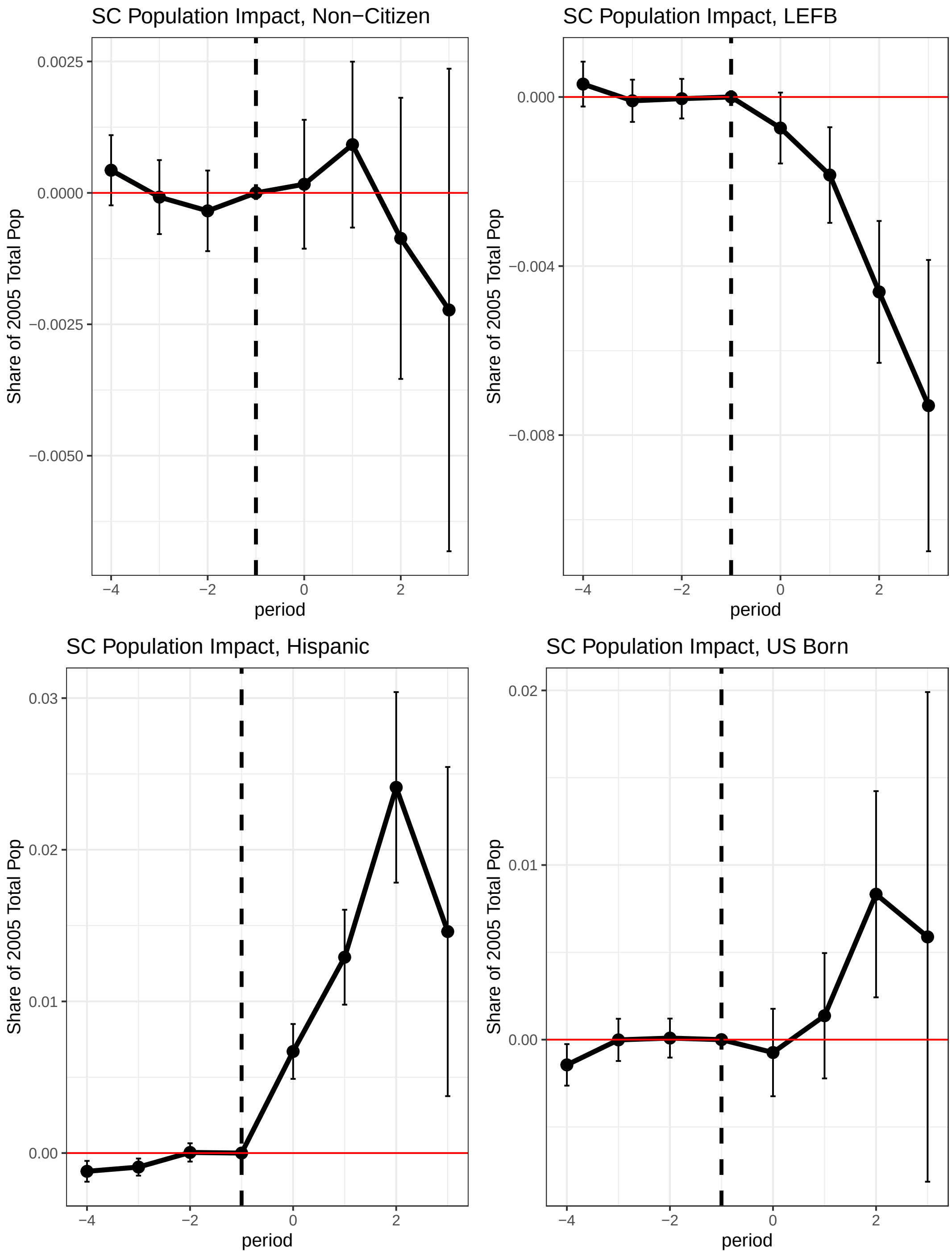
Note: This figure plots the time treads of constructed new housing units and construction workers. The green bars are the annual new housing units per 1000 population (left axis) in the U.S. from Census Bureau and HUD. The two dash lines indicates the average levels of new housing units per 1000 population pre-GFC (1968–2007) and post-GFC (2009–2021). The orange line plots the number of construction workers per 1000 population (right axis) from ACS.

Figure 2: Staggered Rollout of Secure Communities



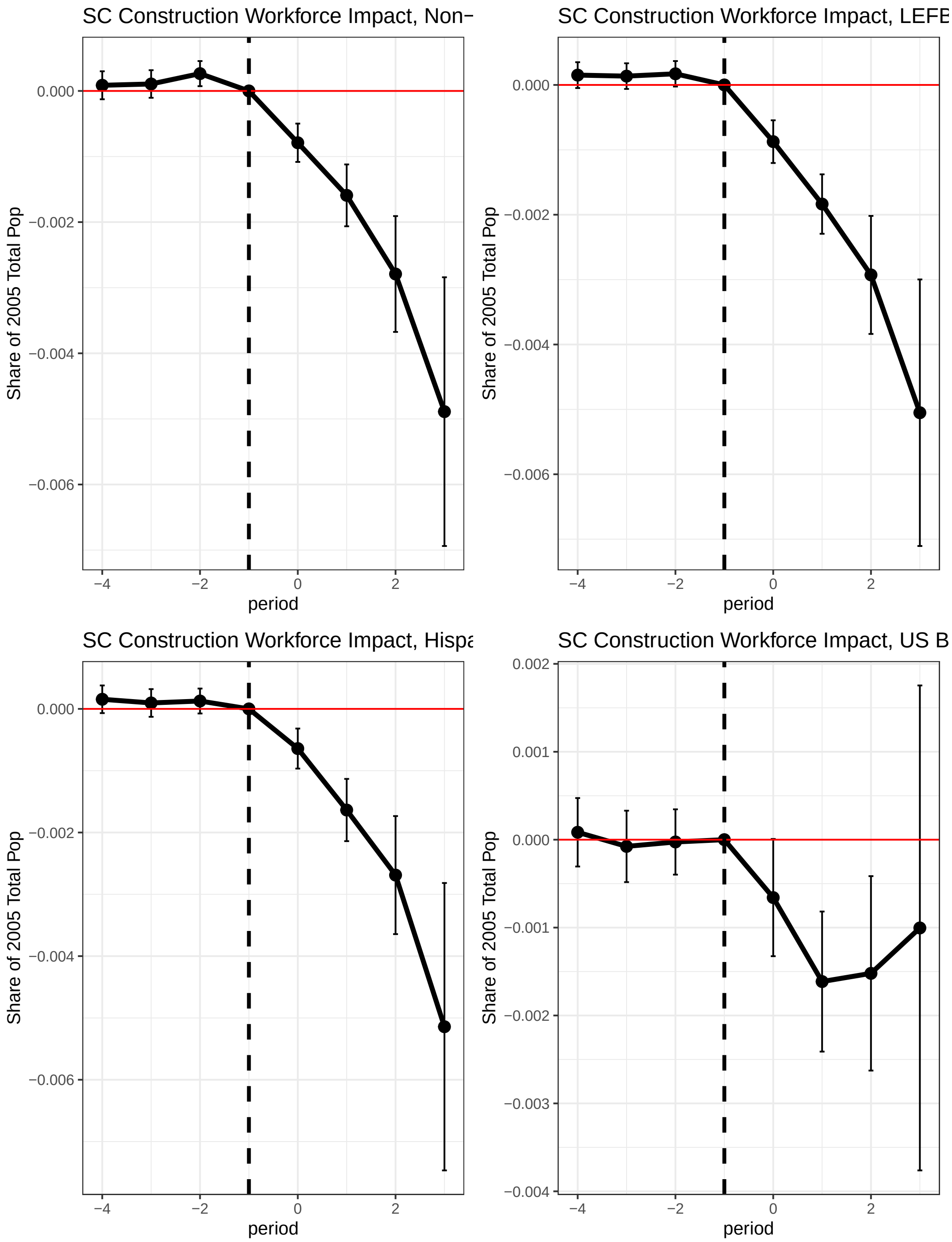
Note: Each panel of this figure shows the counties that implement Secure Communities within each year. This map reflects the treatment indicator used in our regressions, which assigns binary treatment status to any county operationalizing Secure Communities for at least half the year. Counties launching Secure Communities in, for instance, December of year t would therefore be coded as untreated in year t and treated in year $t+1$. Appendix Figure A1 shows treatment status by county-year using only the year of adoption without any consideration of how late in the year implementation started.

Figure 3: Population Impact of Secure Communities



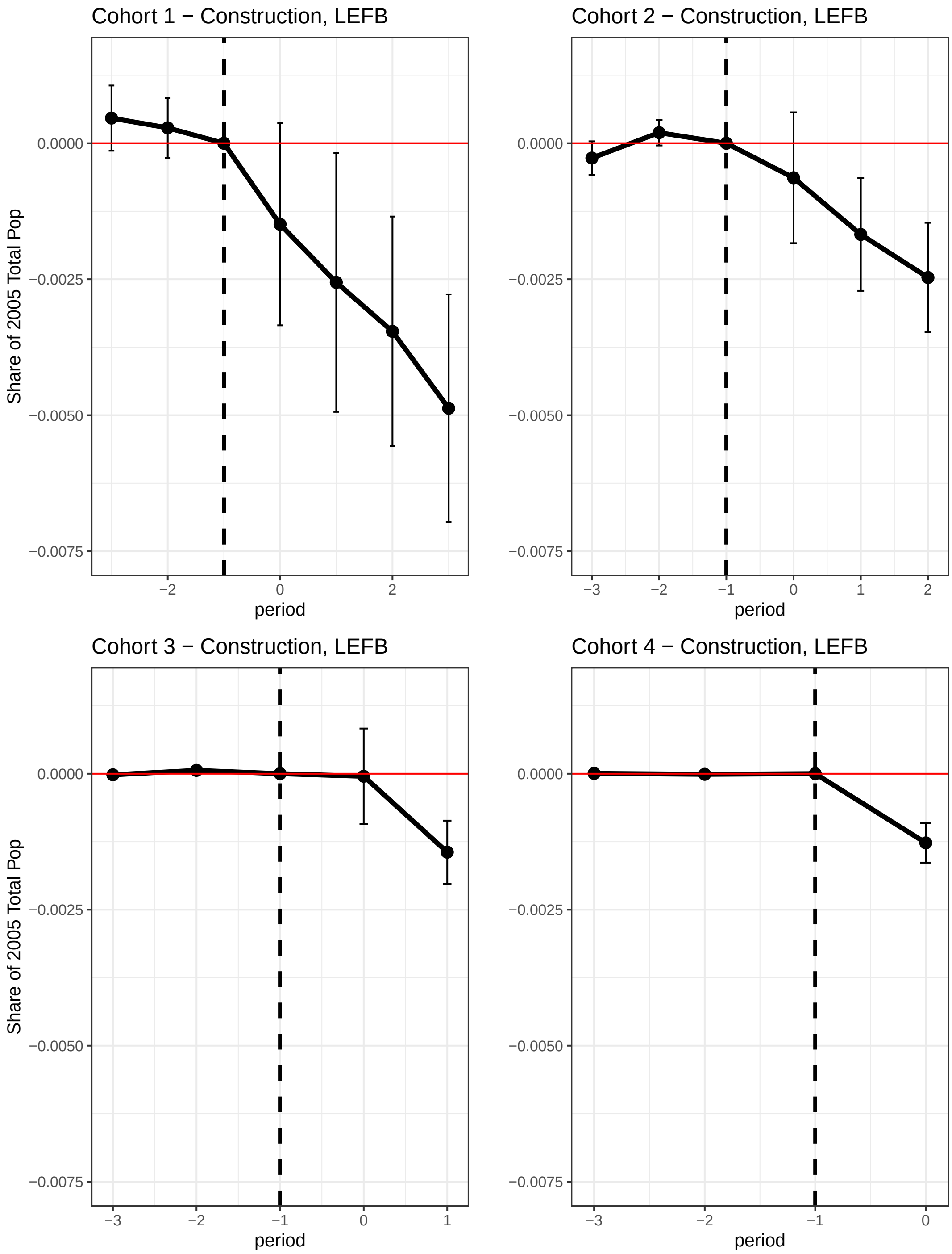
Note: This figure plots the impact of Secure Communities on population changes, with the approach of Gardner (2022) and specification (2). The four panels examine the impact on non-citizen, low-education and foreign-born (LEFB), Hispanic, and U.S. born populations. We use the 2005 population to normalize the dependent variable to population share. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 4: Construction Employment Impact of Secure Communities



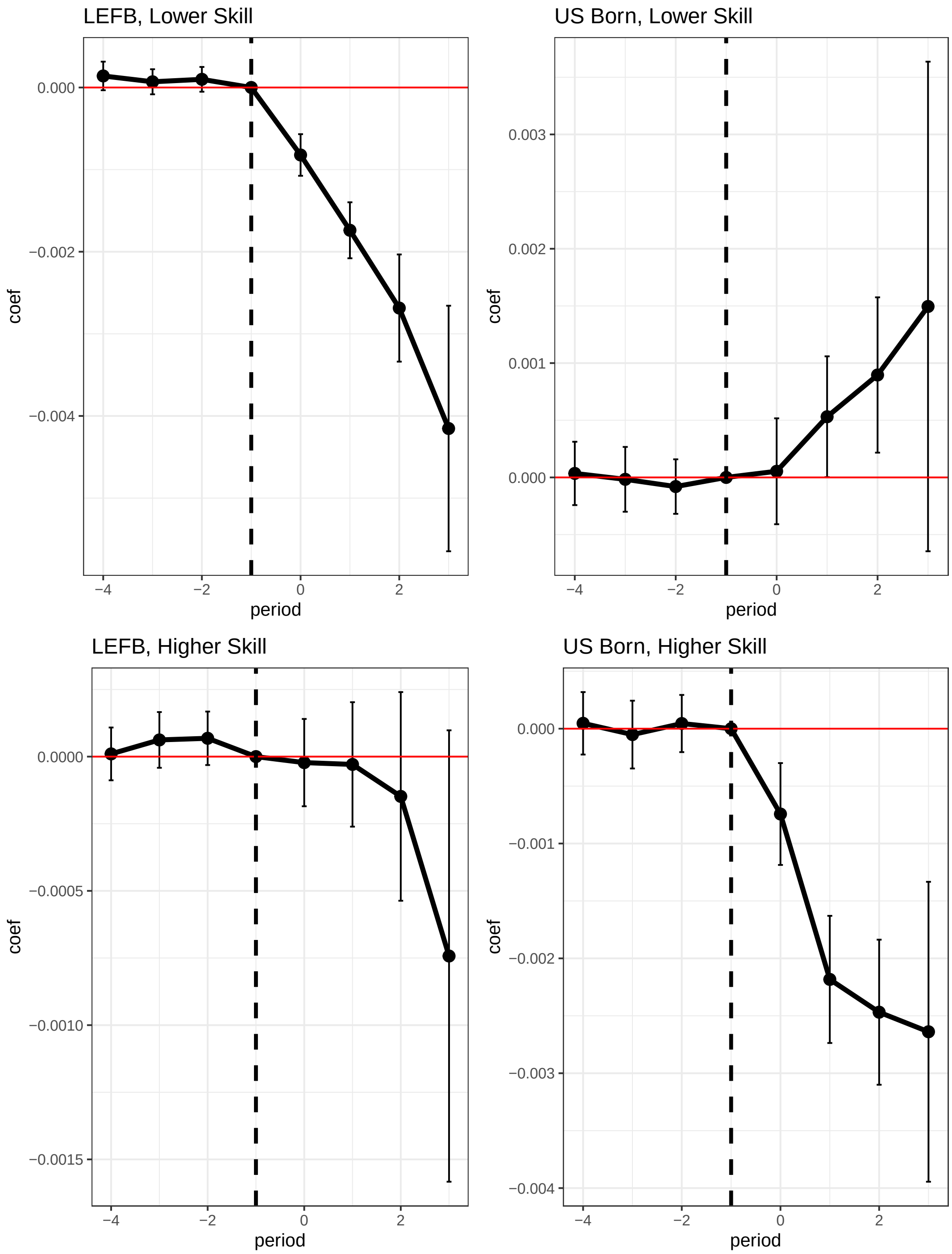
Note: This figure plots the impact of Secure Communities on construction employment, with the approach of Gardner (2022) and specification (2). The four panels examine the impact on non-citizen, low-education and foreign-born (LEFB), Hispanic, and U.S. born workers in the construction sector. We normalize the number of workers by 2005 county-wide population. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 5: Construction-LEFB Impact by Cohort



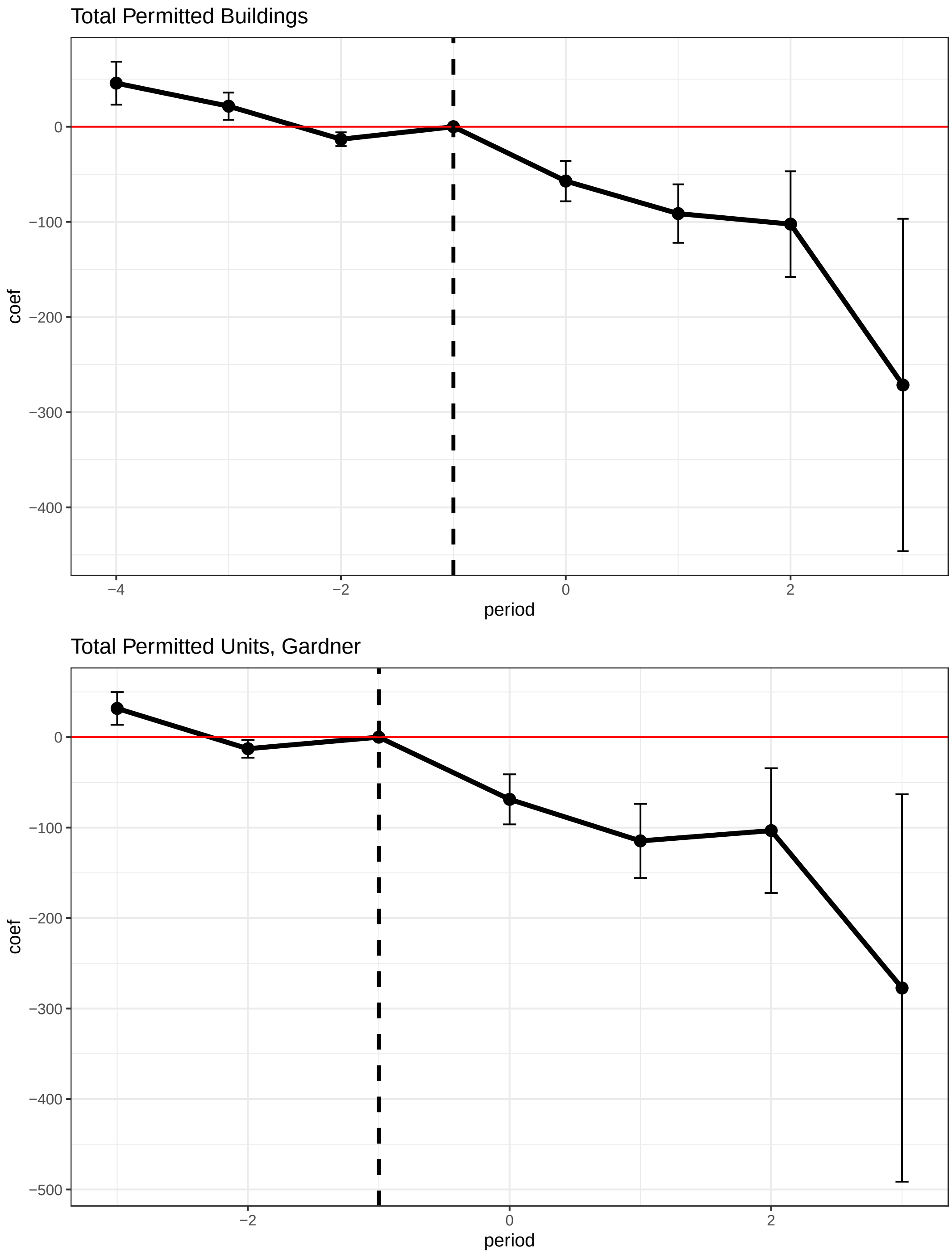
Note: This figure plots the impact of Secure Communities on LEFB construction employment by treatment-cohort, with the approach of Gardner (2022) and specification (2). We use a balanced (and constantt) control group of the last counties to be treated. The four panels compare counties treated in 2009, 2010, 2011, 2012, respectively, with only counties untreated through 2012. We normalize the number of workers by 2005 county-wide population. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 6: Employment Impact By Skill



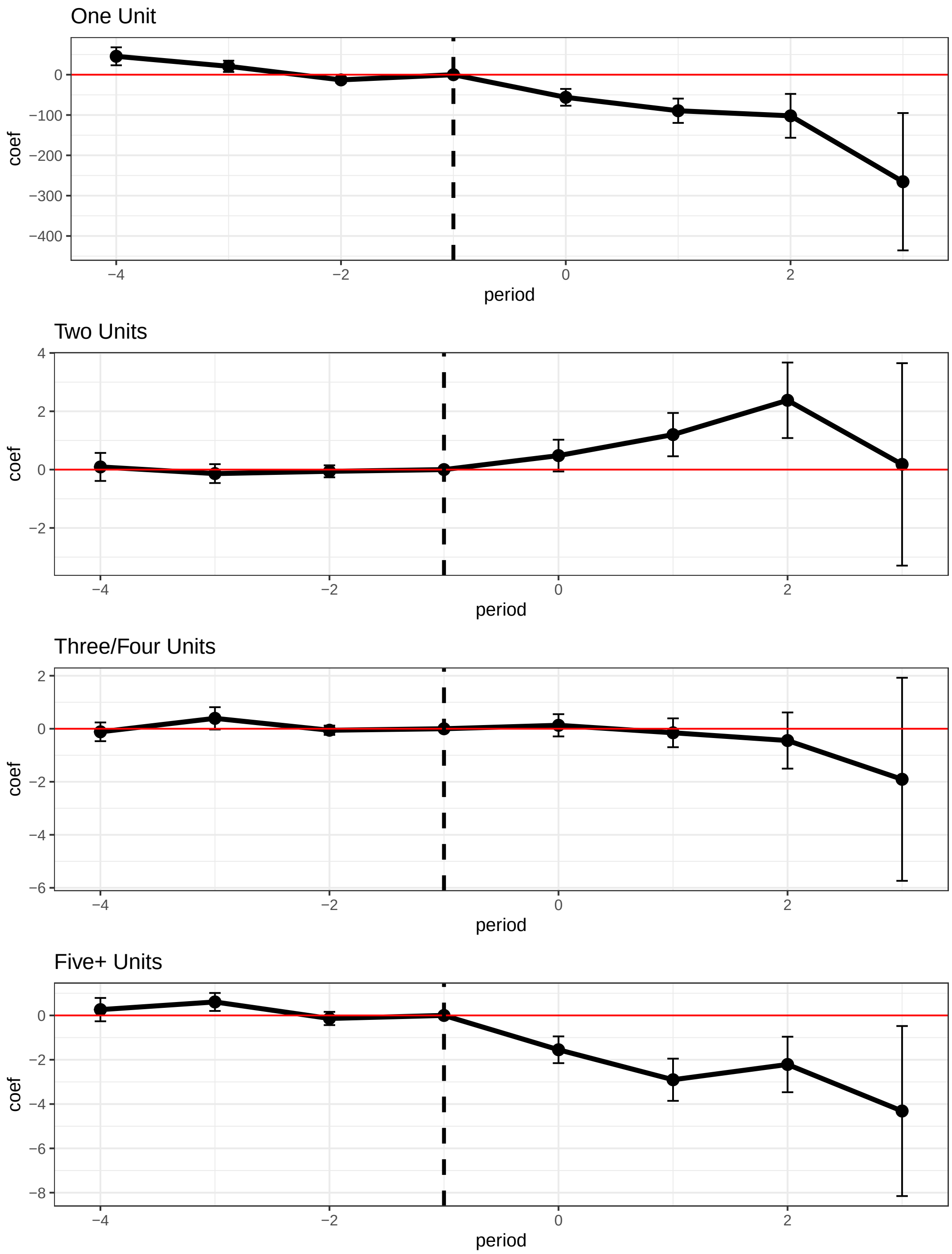
Note: This figure plots the impact of Secure Communities on LEFB and U.S. born construction employment by skill classification, with the approach of Gardner (2022) and specification (2). We define the following occupations as lower skill, construction laborers, helpers in construction trades, painters and maintenance, drywall installers, carpenters, and roofers. All remaining occupations are regarded as higher skill. The four panels plot the estimate impact on LEFB-lower skill, U.S. born-lower skill, LEFB-higher skill, and U.S. born-higher skill workers in the construction sector. We normalize the number of workers by 2005 county-wide population. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 7: Total Permits per 100k residents (ACS Counties)



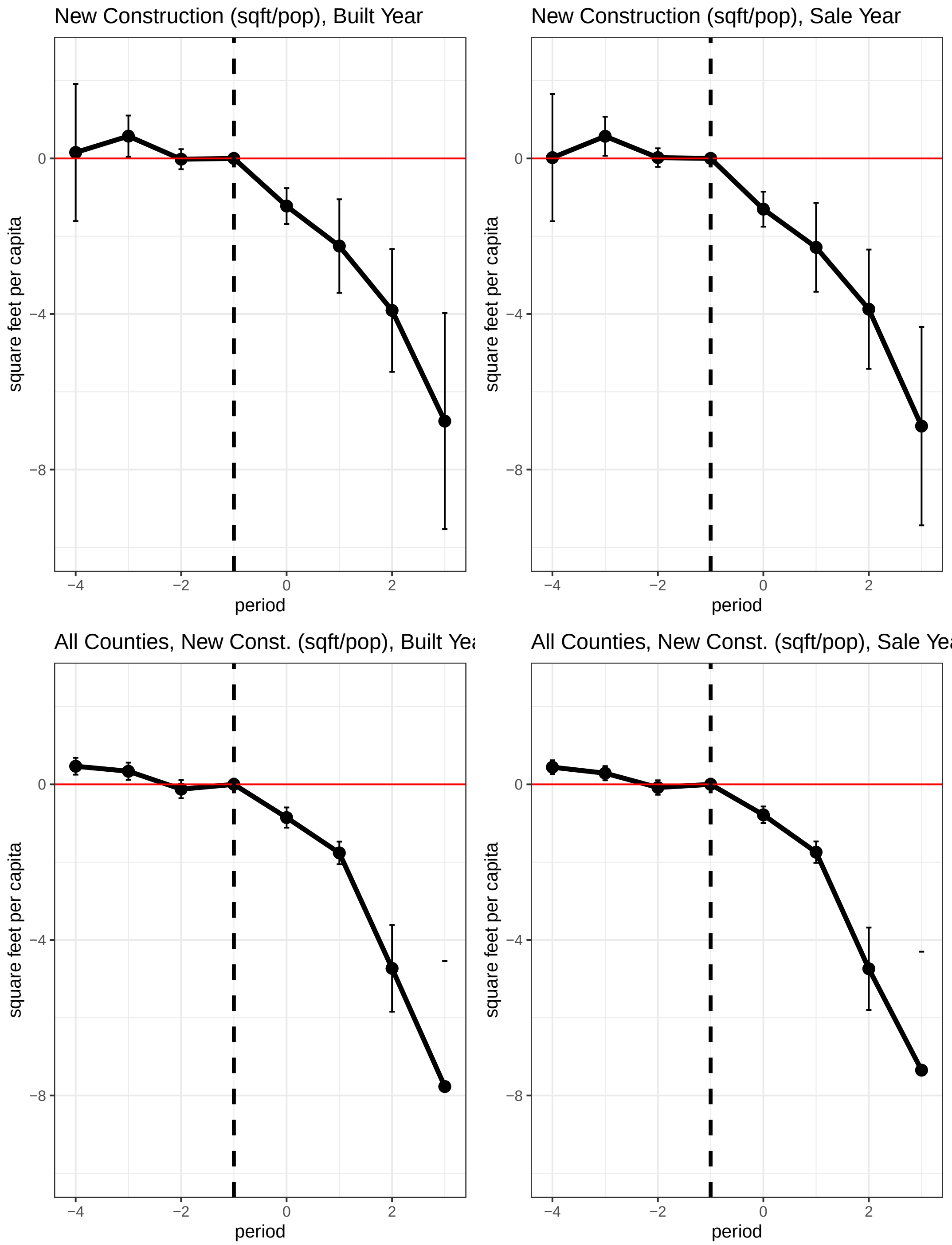
Note: This figure plots the impact of Secure Communities on residential construction activity measured by residential permits (intended construction), with the approach of Gardner (2022) and specification (2). The two panels examine the impact on permitting buildings (top) and total permitted units (bottom). We use total permits per 100,000 residents as the outcome variables. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 8: Permits by Building Class (ACS Counties)



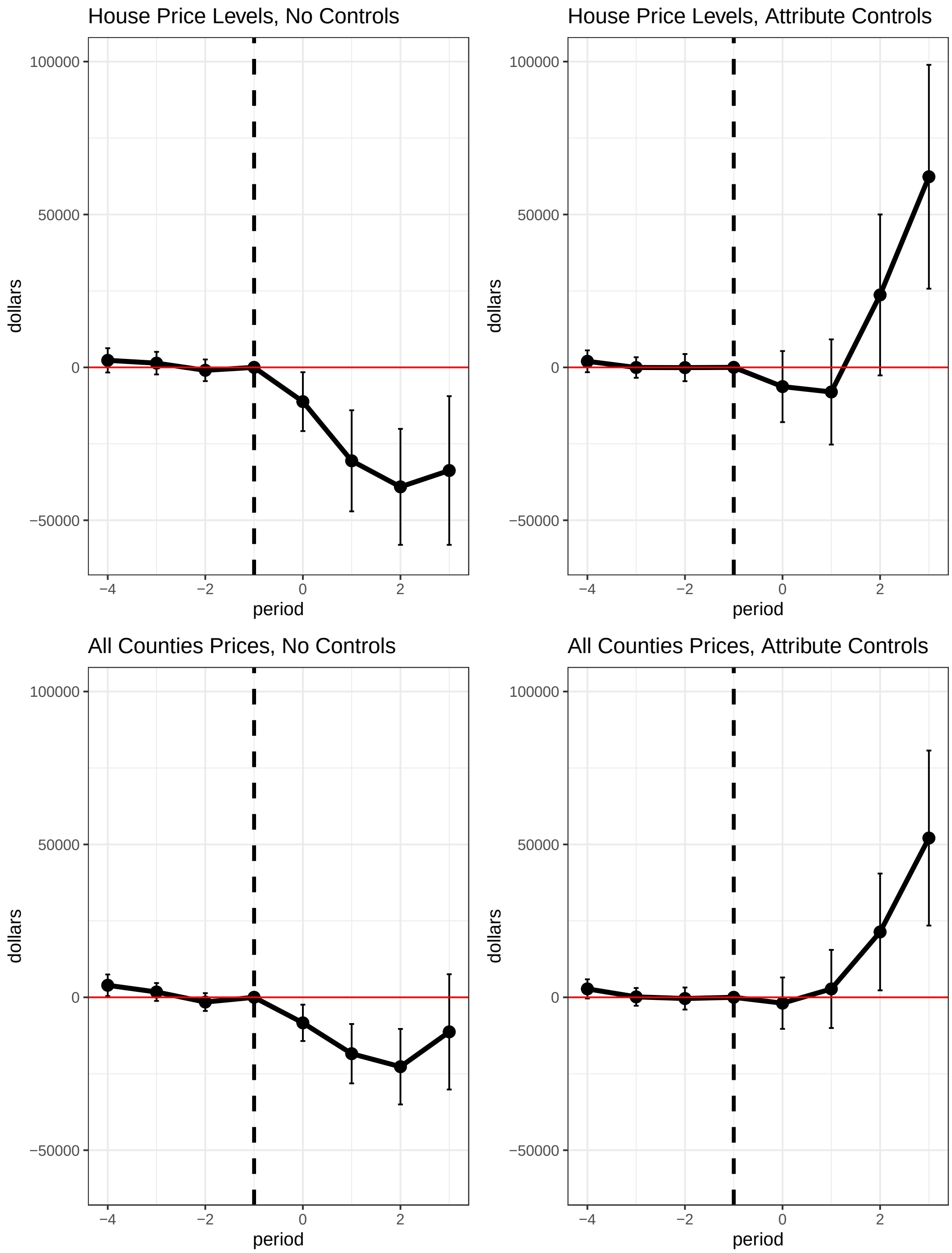
Note: This figure plots the impact of Secure Communities on residential permits (intended construction) by building size, with the approach of Gardner (2022) and specification (2). The four panels examine the impact on permitting buildings of one-unit, two-unit, three/four-unit, and five/more-unit. We use total permitting buildings per 100,000 residents as the outcome variables. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 9: New Construction Entering Market



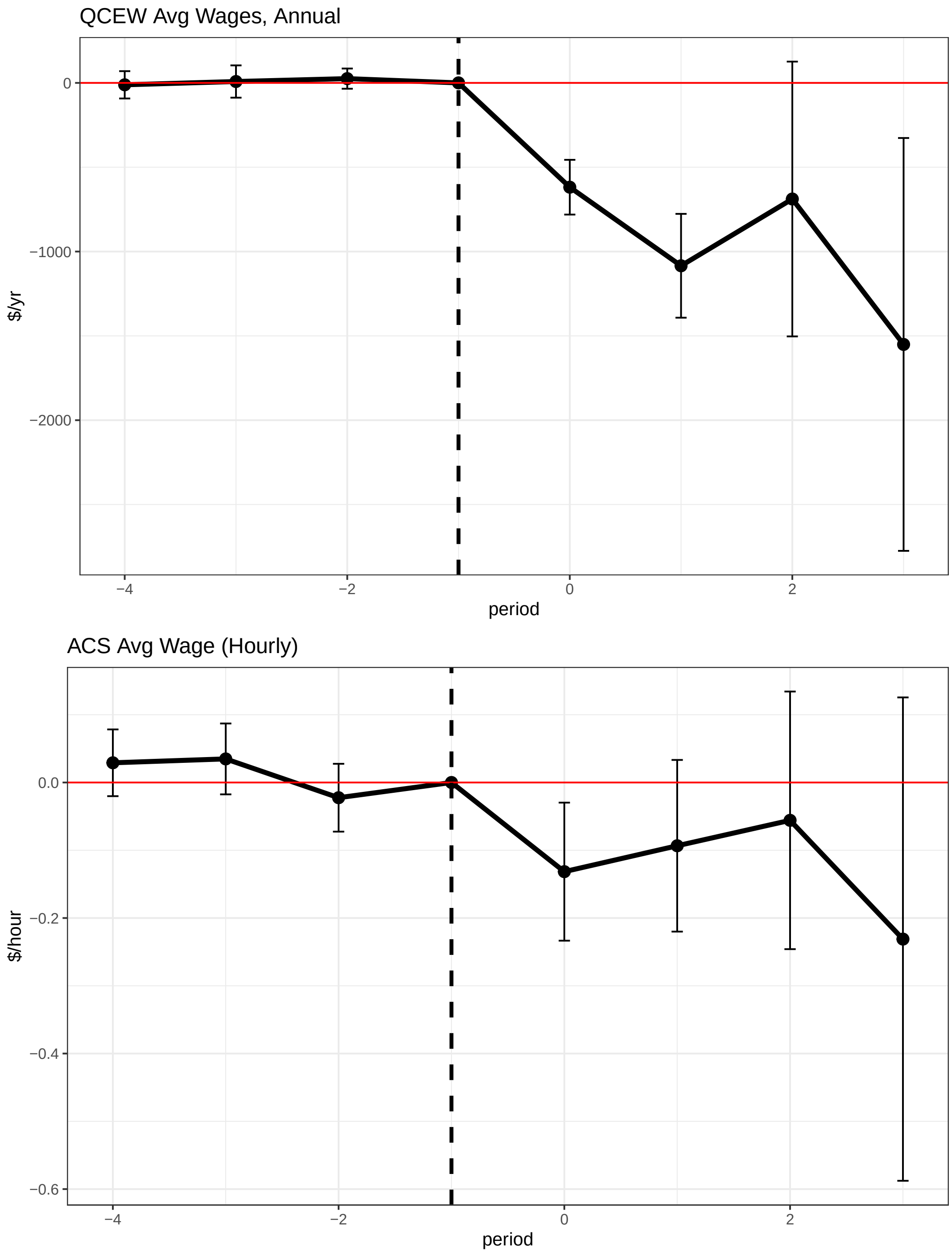
Note: This figure plots the impact of Secure Communities on residential construction activity measured by observed new construction (completed new construction), with the approach of Gardner (2022) and specification (2). The completed new construction is aggregated to county-year level using the administrative tax roll data from CoreLogic. We measure new construction based on either the date of sales or the built date of each property. The top panel examine the impact on new construction measured based on built-year (left) and sale-year (right), using only the subset of counties separately identifiable in the ACS microdata. The bottom panel uses all counties. We use total square footage normalized by 2005 county-wide population as the outcome variables. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 10: Price Effect on Newly Constructed Homes



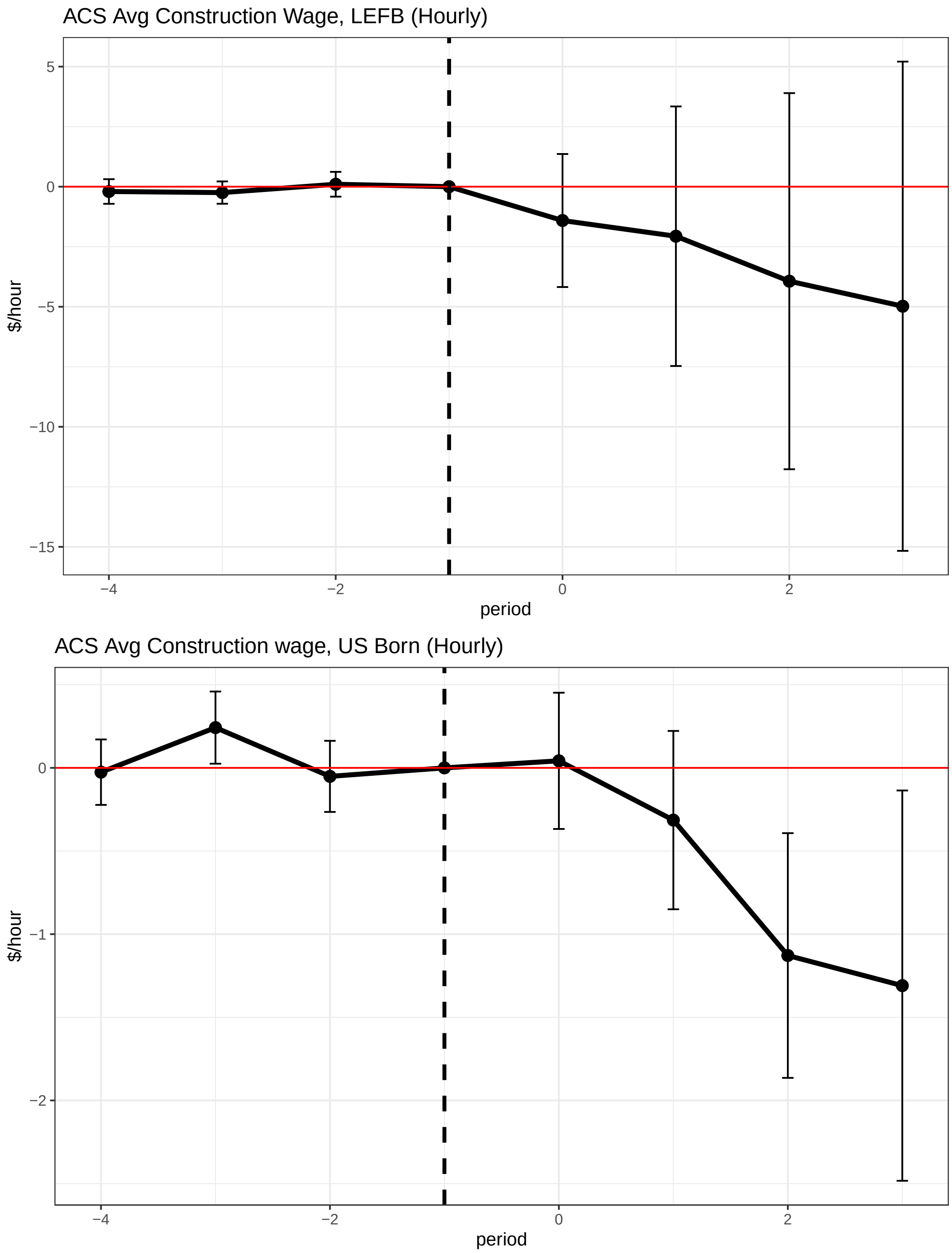
Note: This figure plots the impact of Secure Communities on new construction prices, with the approach of Gardner (2022) and specification (2). We use transaction prices as the dependent variable, from the administrative tax roll data from CoreLogic. The top panel examine the impact on new construction prices without controls (left) and with semi-parametric controls(right), using only the subset of counties separately identifiable in the ACS microdata. The bottom panel uses all counties. Controls in the right plots are home characteristics reported in the CoreLogic data, including square footage, number of bathrooms, presence of a pool, a fireplace, and a patio. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 11: Effect on Overall Average Wages (all occupations)



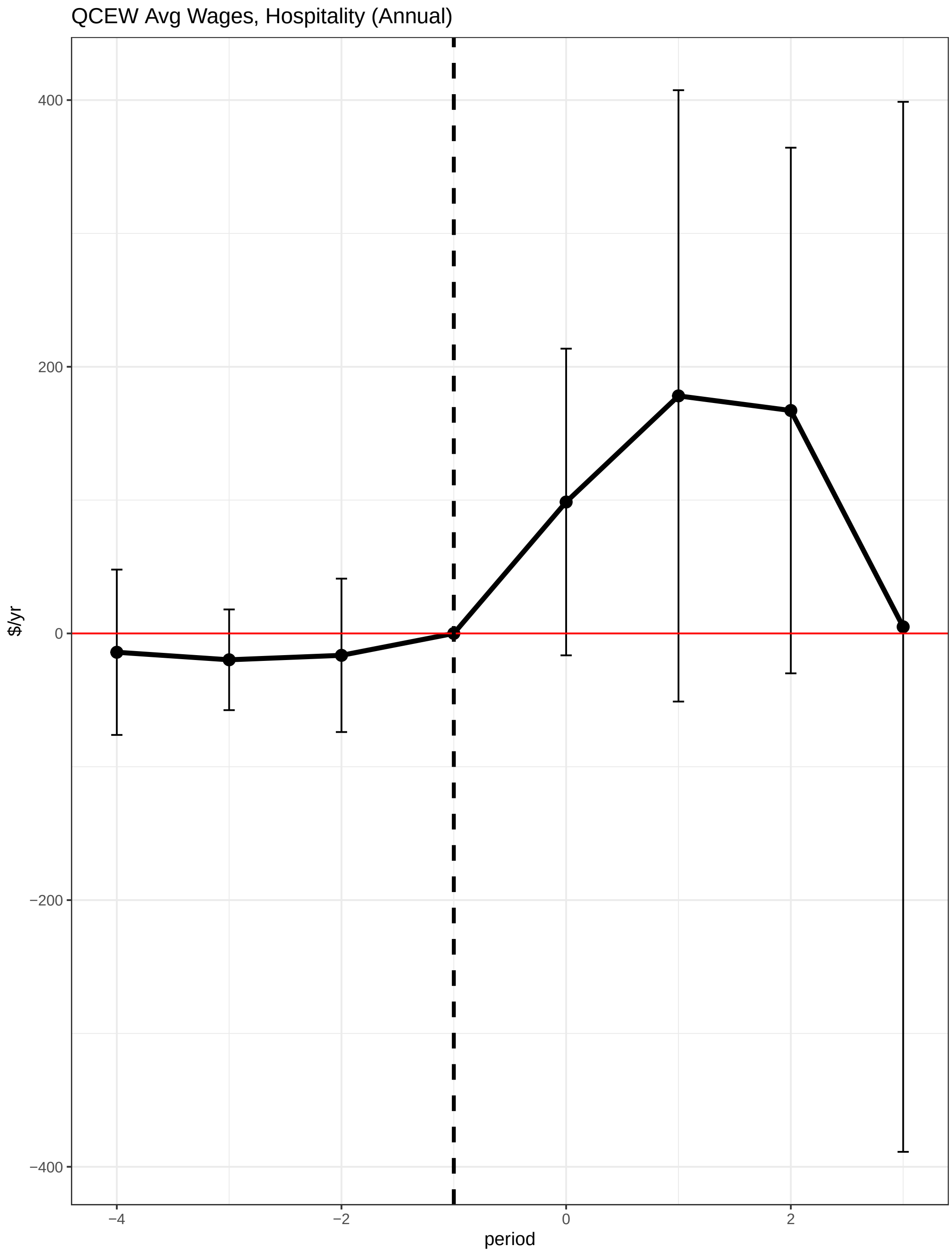
Note: This figure plots the impact of Secure Communities on average county-level wages for all workers across all industries with the approach of Gardner (2022) and specification (2). The top panel uses a measure of average annual wages from QCEW, and the bottom panel uses a measure of hourly average wage constructed from ACS microdata. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 12: Effect on Construction Wages – LEFB and U.S. Born



Note: This figure plots the impact of Secure Communities on average county-level wages for construction workers with the approach of Gardner (2022) and specification (2). Both panels use a measure of hourly average wage constructed from ACS microdata. The top panel shows impact on LEFB workers, and the bottom panel shows impact on U.S. born workers. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure 13: Effect on Hospitality Wages



Note: This figure plots the impact of Secure Communities on average county-level wages for hospitality and service workers with the approach of Gardner (2022) and specification (2). This figure uses QCEW data on average annual wages. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

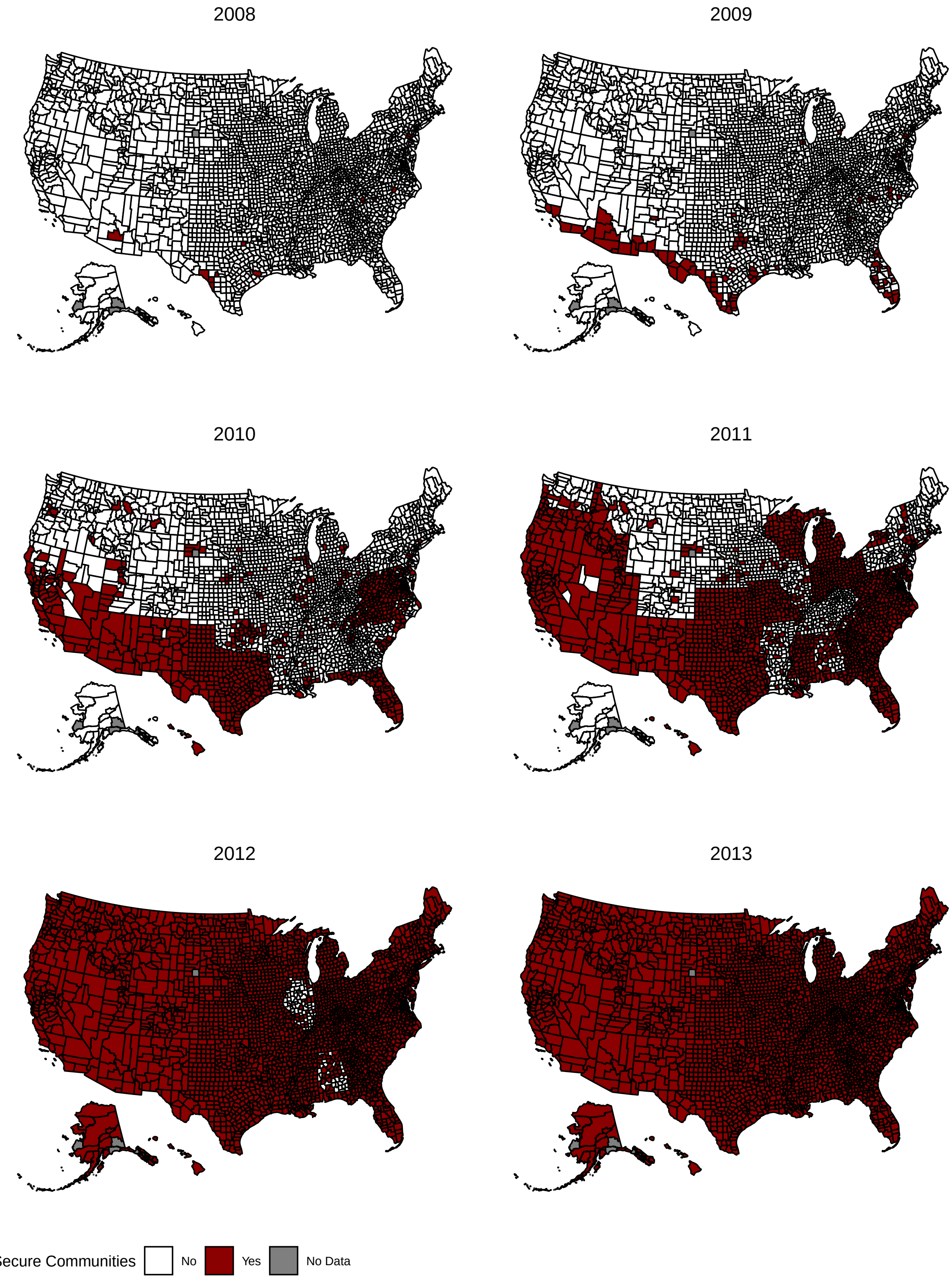
Table 1: County-level characteristics for roll-out

	SC Year					
	2008	2009	2010	2011	2012	2013
Panel A: ACS sample						
# of counties	5	44	142	88	91	6
%	1%	12%	38%	23%	24%	2%
Population	1721549	944314	459630	249572	456294	972964
Normalized Population						
Non Citizen	12%	10%	7%	3%	5%	4%
US born	85%	89%	94%	99%	93%	99%
LEFB	9%	7%	5%	2%	4%	3%
Normalized Employment						
Total	60%	55%	58%	58%	59%	59%
Non Citizen	8%	7%	5%	2%	4%	2%
US born	46%	44%	50%	54%	51%	54%
LEFB	9%	7%	5%	2%	4%	3%
Normalized Construction Employment						
Total	4%	4%	4%	3%	3%	3%
Non Citizen	2%	1%	1%	0.2%	0.3%	0.2%
US born	2%	2%	3%	3%	3%	3%
LEFB	2%	1%	1%	0.2%	0.4%	0.2%
Permit (buildings)						
Total	6913	3015	1607	797	933	976
One-unit	6629	2899	1551	770	843	820
Two-unit	66	24	14	11	39	29
Three-or-four-unit	24	28	13	5	25	59
Five-or-more-unit	193	64	29	12	26	69
Panel B: Whole sample						
# of counties	14	88	783	1099	1044	106
%	0.5%	3%	25%	35%	33%	3%
Population	753264	609746	123625	57685	71700	90748
Permit (buildings)						
Total	4001	2289	494	192	188	172
One-unit	3881	2203	477	186	176	158
Two-unit	26	21	5	3	5	3
Three-or-four-unit	14	18	4	1	3	4
Five-or-more-unit	80	46	8	2	4	7

Note: This table summarizes the county-level characteristics based on the roll-out (Calendar) year of Security Communities. Panel A uses only the subset of counties separately identifiable in the ACS microdata, while Panel B include all counties. For each year of 2008-2013, this table provides the number (and share) of counties that implemented the Security Communities in that year. We report the average levels across counties of 2010 population, normalized population, employment, and construction employment, for total and each group of non-citizen, US born, and LEFB, in addition to the average number of permitting buildings in total and by building size.

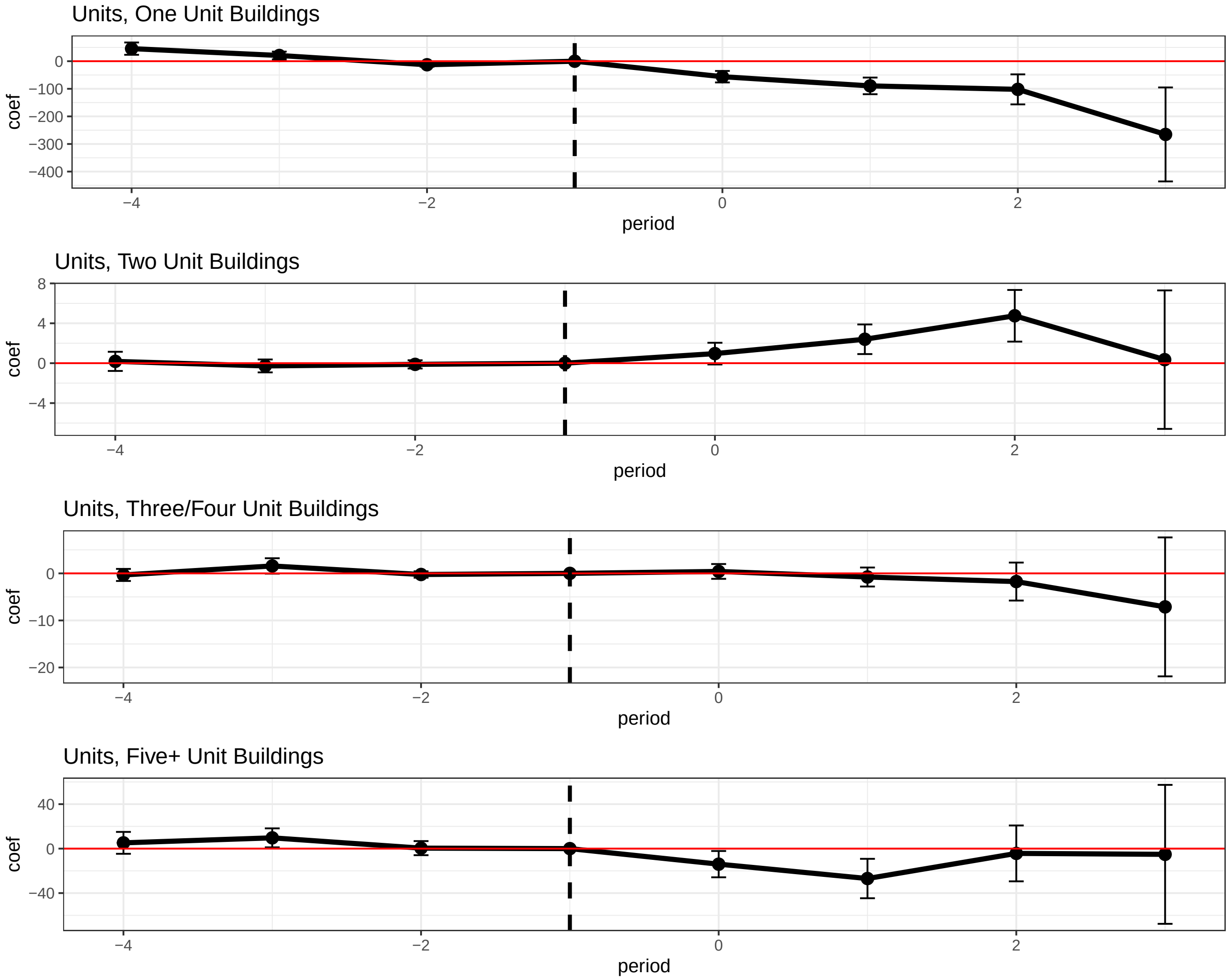
A Appendix

Figure A1: Staggered Rollout of Secure Communities (by exact date)



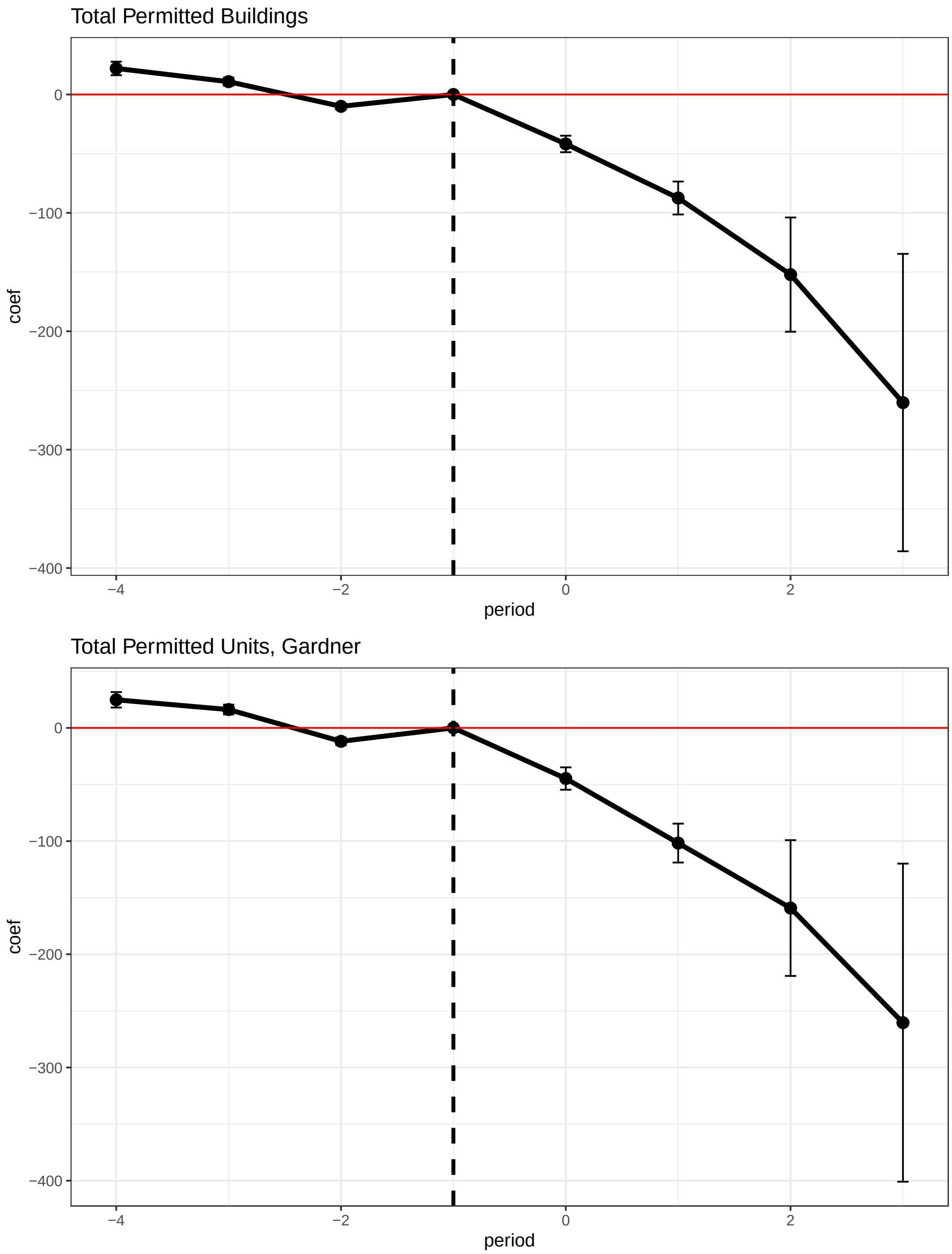
Note: Each panel of this figure shows the counties that implement Secure Communities within each year. This map reflects treatment based on exact date of implementation: a county is coded as treated in year t if the launch date falls at any point within year t. All regressions in this paper assign annual treatment status only to counties which have been treated for at least half a year; a corresponding map of this empirical treatment indicator is shown in Figure 2.

Figure A2: Permitted Units by Building Class



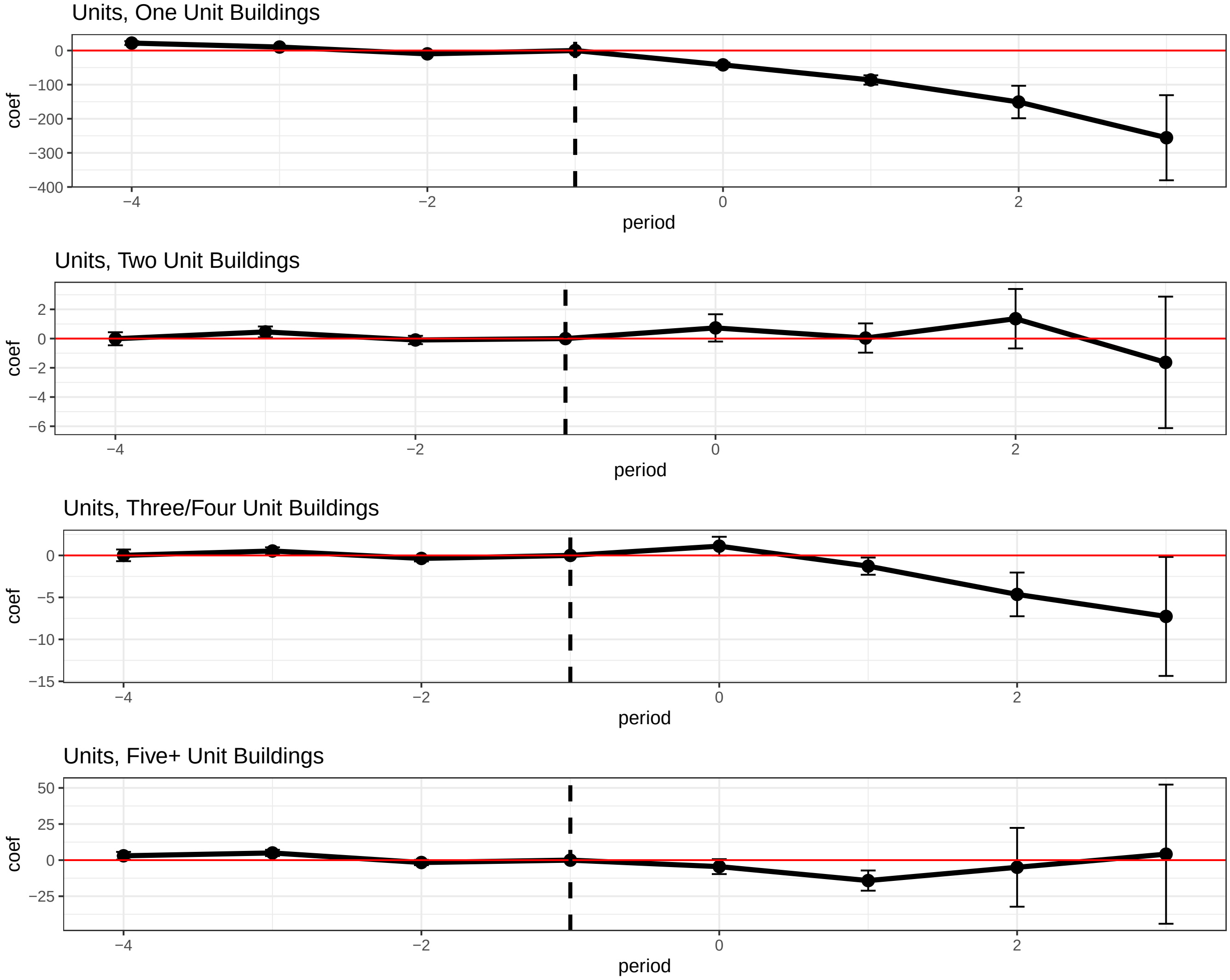
Note: This figure plots the impact of Secure Communities on residential permits (intended construction) by building size, with the approach of Gardner (2022) and specification (2). The four panels examine the impact on permitted units of one-unit, two-unit, three/four-unit, and five/more-unit buildings. We use total permitted units per 100,000 residents as the outcome variables. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure A3: Permits by Building Class (Full Sample)



Note: This figure plots the impact of Secure Communities on residential construction activity measured by residential permits (intended construction), with the approach of Gardner (2022) and specification (2). Estimations in this figure include all counties. The two panels examine the impact on permitting buildings (top) and total permitted units (bottom). We use total permits per 100,000 residents as the outcome variables. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.

Figure A4: Permitted Units by Building Class (Full Sample)



Note: This figure plots the impact of Secure Communities on residential permits (intended construction) by building size, with the approach of Gardner (2022) and specification (2). Estimations in this figure include all counties. The four panels examine the impact on permitting buildings of one-unit, two-unit, three/four-unit, and five/more-unit. We use total permitting buildings per 100,000 residents as the outcome variables. 95% confidence intervals are plotted around the point estimations, and the standard errors are clustered at the county level. The unit of one period is a year.