# Cracking Down, Pricing Up: Housing Supply in the Wake of Mass Deportation*

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**Abstract**

This paper explores how immigration policy affects residential construction and home prices. Using a staggered-rollout design, we show that increased immigration enforcement reduces homebuilding and drives up new construction prices. Existing stock prices reflect some downward pressure from reduced demand, however upward supply-side pressure dominates in most neighborhoods—except those with the highest baseline share of undocumented residents. Exploring mechanisms, we find a significant loss of undocumented workers in construction, and that domestic workers do not provide one-to-one replacement. Instead, undocumented labor appears to complement domestic labor resulting in net job losses for US-born workers, especially in higher-skilled occupations.

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## 1. Introduction

General consensus holds that historical declines in housing affordability over the past decade are rooted in a secular decline in housing supply (Glaeser and Gyourko, 2018). Since 2011 the US has added an average of 1.1 million new housing units every year (see Figure 1). This is 30% lower than the long-run equilibrium before the Great Recession and 34% lower than the annual new construction demand (Khater et al., 2018). An extensive academic literature exploring supply-side drivers of low homebuilding has tended to focus on the distortionary effects of stringent zoning codes and local building covenants (Molloy, 2020). However, little attention has been paid to the most important production factor in construction—labor supply. Ironically, a major source of construction labor – immigrants – has recently been central to policy debates, with prominent politicians attributing soaring housing prices to an increase in (illegal) immigration.[1] Therefore, critical questions remain: how does immigration policy affect home building and housing prices, and to what extent is output in the construction sector sensitive to labor supplied by undocumented workers?

This paper leverages an increase in immigration enforcement arising from a federal program called Secure Communities (SC), which began in 2008 and eventually rolled out to all counties nationwide by 2013.[2] According to US Immigration and Customs Enforcement (ICE) records, this program was associated with the deportation of more than 300,000 undocumented immigrants during this time period. The residential construction sector is well known to draw heavily upon undocumented labor – Passel and Cohn (2015) among others suggest 15–20% of workers are undocumented. Therefore, Secure Communities provides a laboratory for exploring the sensitivity of output in the residential construction sector to immigration policy and labor supply.

Our core empirical specification is a staggered-rollout Differences-in-Differences design. Recognizing that the Secure Communities intervention occurs in the years following the housing boom of the 2000s, we adopt a high-dimensional set of fixed effects to restrict identifying comparisons to counties: (i) in the same Census Region, (ii) of similar size, and (iii) that experienced similar growth in home prices over the 2000–2007 period. We also show that lagged measures of population growth, new construction volume, and house prices do not predict which counties adopt Secure Communities. As our setting includes several features that are known to bias two-way fixed-effect

[1] E.g., in the vice-presidential debate of the 2024 election, the Republican candidate said "...you have got housing that is totally unaffordable because we brought in millions of illegal immigrants to compete with Americans for scarce homes."

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

estimates, including eventual treatment for all units and the potential for cohort-level treatment heterogeneity, we use the bias-robust DiD estimator proposed by Borusyak et al. (2024) in all specifications.

Focusing first on new construction, we use administrative deed transaction records compiled by CoreLogic to show that counties adopting increased immigration enforcement see a 5.7% reduction in residential construction; for the average size county this corresponds to 80 fewer homes per year. We also show evidence linking the intensity of immigration enforcement to the magnitude of impact: counties which deport larger numbers of individuals under Secure Communities (relative to baseline population) see monotonically larger declines in homebuilding.

Next, we examine the impact of immigration enforcement on home prices. This relationship is complex and ex-ante ambiguous because, in addition to constraining supply, immigration enforcement also mechanically affects demand: increased deportations will reduce the number of residents demanding housing services. In order to at least partially distinguish between supply-side and demand-side channels, we conduct analysis separately between new construction and existing housing stock. As new construction homes are typically built at the higher end of the market, price impacts in this segment are more likely to reflect supply shifts than demand shifts. By contrast, price impact within existing housing stock is better suited to study the net effects of increased immigration enforcement on home prices—including the extent to which deportation-reduced demand shocks place downward pressure on home prices.[3]

We find that Secure Communities increases new construction prices by 4.5%. There is some evidence of builders making endogenous shifts in home attributes, however these have minimal impact on overall price. Including a rich set of home characteristics as hedonic controls, we find that quality-adjusted prices increase by 4.4%. Price impact in existing stock homes is more nuanced. SC has zero effect on overall average existing-stock prices; however, we show that this obscures strong variation between neighborhood demographics and price impact. Using a standard proxy for undocumented status in the American Community Survey – foreign-born individuals with low formal education (LEFB) – we segment tracts into quartiles by undocumented population share. Tracts with very few LEFB residents see a 3.4% increase in existing stock home prices. This effect declines monotonically across the remaining quartiles: a 2.0% increase in the 2nd quartile,

[3]Long-standing theories of filtering in housing markets suggest that partitioning between new construction and existing stock cannot entirely separate supply-side price drivers from demand-side, as shocks within any segment will eventually transmit to some extent across the quality spectrum (Ohls, 1975; Rosenthal, 2014). Nonetheless, we argue that the separation helps to disentangle the two channels, especially over the near-term horizon which our setting enables.

statistically insignificant change in the 3rd quartile, and in the quartile of tracts with the highest share of LEFB residents, we find a 4.1% decrease in prices. This is consistent with immigration enforcement placing downward pressure on homes prices through a demand-side channel, but also suggests that the net effect of enforcement is higher prices in most locations. Finally, we also show real effects of reduced housing supply and higher home prices by examining homeownership rates, a common focus for policymakers: Secure Communities reduces the population share of homeowners by 93 basis points.

The most obvious potential mechanism for reduced building is a decrease in labor supply. The construction sector is a major sector of employment for undocumented residents (Svajlenka, 2021), and therefore increasing immigration enforcement likely means deporting meaningful numbers of construction workers. Using American Community Survey data, we show that not only does SC lead to reductions in construction labor supply, but also that SC appears to land more heavily on construction than on other sectors, including agriculture and food service which both also rely on undocumented labor.

Does domestic labor flow into the construction industry to offset these employment losses? We observe some inflow of US-born workers into lower-skilled construction occupations, with a replacement rate of approximately 40%. However, in higher-skilled occupations, we find declines not only among undocumented workers but also among domestic workers. In aggregate, US-born labor supplied to the construction industry does not increase. This pattern is consistent with a shortage in one segment of the labor supply chain inducing a slowdown in output, which in turn leads to employment losses across complementary occupations.

Examining patterns of wage adjustment, we find evidence that supports this shortage-and-complementarity interpretation. Using reported wages from ACS, we find increases for undocumented workers, ranging from 2–8%. These increases are concentrated in lower-skilled occupations. However, we find no evidence of increases in wages for US-born workers, even in lower-skilled occupations. This equilibrium outcome suggests some labor market friction in attracting additional domestic workers into lower-skilled construction occupations. This may suggest that US-born workers supply labor inelastically to the construction sector, or that builders hold the belief that increasing wage sufficiently to attract full-replacement domestic workers would be profit-reducing.

Finally, we explore the timing of treatment effects. In this setting there is ex-post variation in the intensity of deportations between cohorts. While it is an econometric reality that dynamics cannot be separately identified from heterogeneity in cohort-level treatment effects, our analysis

points towards increasing effects over a near-term horizon, especially for counties where increased enforcement delivers a larger 'dosage' of deportations. For existing-stock prices in particular, the evidence suggests that price-declines in neighborhoods with a larger share of undocumented residents may be transitory: even in these tracts, the supply-side effect appears to dominate, pushing prices higher on average within 3 years of SC implementation.

**Contributions and Related Literature** Our paper's contributions should be viewed along two margins.

First, this paper adds to the literature exploring how policy shapes equilibrium outcomes in housing markets, most specifically with respect to home prices and homeownership. An extensive body of work documents the interplay between credit provision and home prices during the housing boom of the early 2000s (Foote et al., 2021; Adelino et al., 2016; Mian and Sufi, 2009; Greenwald and Guren, 2025; Justiniano et al., 2019). While construction collapsed, predictably, with the onset of the resulting subprime crisis, the literature is still working to understand why this pattern of historically low homebuilding proved so persistent in the years following the Great Recession, leading to dramatic secular increases in the housing cost share of US household budgets (Molloy, 2020; Albouy et al., 2016). Summarizing a leading set of explanations, Glaeser and Gyourko (2018) attribute the shortage of housing 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 supply and lead home prices to exceed the marginal cost of construction.

In contrast to zoning policies, which are typically local, this paper shows that national immigration policy can also have a first-order impact on housing supply and home prices. We empirically resolve a theoretical ambiguity: an increase in deportations pushes home prices upwards on average, despite mechanically reducing aggregate demand for housing services. We also provide novel evidence suggesting that US homebuilding is meaningfully sensitive to labor supply. Negative shocks to the construction workforce appear to be highly persistent and reduce real economic output in the housing sector. In equilibrium, builders do not fully offset workforce shocks by raising wages to attract replacement workers. This has implications for the literature focused on zoning reform and regulatory rigidities in housing markets: policies designed to address housing affordability may be less effective unless they also help increase labor supplied to the construction industry.

Second, this paper contributes to the extensive literature that aims to understand the effects of immigration on local economies. Pioneered by the seminal work of 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 US during the Mariel boatlift (Card, 1990; Borjas, 2017), Mexican immigration to the US during the Mexican Peso Crisis (Monras, 2020), and Jewish immigration into Israel after the collapse of the Soviet Union (Friedberg, 2001; Cohen-Goldner and Paserman, 2011). Lach (2007) and Cortes (2008) show immigration flows are associated with lower prices for nontradable goods and services.

Our paper is most directly connected to a subset of papers that study the impacts of immigration on 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, 2007) show more immigrants are associated with inflated housing rents. Our setting allows us to focus on the housing market impact of outflows rather than inflows, which is especially critical to understand during a period when proposals for mass-deportations have become a part of mainstream political discourse. We are also able to leverage microdata on output in housing markets to understand how immigration flows affect housing supply as well as house prices.

A crucial issue faced by many papers that relate immigration to real economic outcomes is that immigration decisions are almost certainly correlated with the economic trend of local economies. Many 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 US counties introduced by the gradual rollout of the SC program. Several other papers have used this same setting. East et al. (2018) show that SC 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) show that SC has no meaningful impact on local crime rates. Alsan and Yang (2022) show that SC leads to reduced uptake of federal social service programs for Hispanic residents, even among those not eligible for deportation. Grittner and Johnson (2021) find that SC decreases safety in workplaces while also making workers less likely to submit reports to regulators, especially in industries with relatively high Hispanic share.

The rest of this paper proceeds as follows. Section 2 describes the institutional details of the SC immigration shock. Section 3 describes our empirical approach, and Section 4 outlines the key sources of data. Section 5 presents our results. Section 6 concludes.

## 2. SC Background

Secure Communities (SC) was a US Immigration and Customs Enforcement (ICE) program that launched at the end of 2008. The central pillar of SC was enhanced information sharing between local law enforcement and federal immigration databases. Prior to SC, 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 SC, fingerprint information (already collected by local law enforcement pursuant to an arrest) began to be automatically shared with the Department of Homeland Security (DHS).[4] DHS would then match those fingerprints against an internal database of foreign-born individuals. A subset of individuals appearing in this database would be potentially eligible for deportation: (i) those who have been previously deported, (ii) noncitizens without any record of entry into the country, (iii) those with expired visas, and (iv) individuals identified as potential national security threats. 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.

Because coordinating information and logistics across more than 31,000 booking locations nationwide was highly resource-intensive (assuming custody, for instance, requires arranging both transportation and bed space), it was clear from the onset that the program could not simultaneously launch at all locations nationwide (Alsan and Yang, 2022). 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 SC at the beginning of 2013. While we have an exact date for the official start of SC 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 SC for at least half the year. This means, for instance, that the initial set of counties launching SC in October to December of 2008 are coded as a 2009 treatment-cohort. Our results are not sensitive to this choice. Figure 2 maps the expansion of SC by year, depicting the treatment indicator used

[4]Specifically, fingerprints sent to the FBI to check an individual's criminal history (the existing standard), 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.

in all regressions. Internet Appendix Figure A1 maps treatment cohorts using the actual date of SC launch, without consideration for when in the year that initial date falls. In total, more than 300,000 people are deported under SC during our sample period (see Figure 3).

The last set of untreated counties adopted SC in January 2013.[5] The policy remained in place for the next 22 months. Beginning in late 2014, US immigration policy continued to shift on margins of both policy and branding. In November 2014, the Secretary of DHS announced the discontinuation of SC, 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 lead to engagement with DHS: while all encounters with local law enforcement fell under the umbrella of SC, 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 reinstituting SC, and in January 2021 President Biden signed an executive order revoking that reauthorization.[6]

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 January 2013, at which point all counties become treated with the original iteration of SC. We are, therefore, not using any variation from the more difficult-to-interpret period from 2014 onward. We detail this fully in Section 3.

## 3. Empirical Strategy

The phased rollout of SC between 2008 and 2013 allows us to run a county-level staggered difference-in-differences (DiD) design. Our main empirical specification is:

$$y_{it} = \alpha_i + \gamma_{FE(t)} + \beta \mathbb{1}(SC\ Activated_{it}) + \epsilon_{it}. \tag{1}$$

In this equation, $i$ indexes counties and for notational convenience $\gamma_{FE(t)}$ denotes the set of fixed effects absorbing time-variation. A textbook DiD design would use $\gamma_t$: a fixed effect for each time period. Our preferred specification uses a higher-dimensional set of fixed effects that interacts

[5] 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 areas.

[6] 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.

indicators for predetermined county characteristics with year. The purpose is to address any concern that findings may be driven by correlation between rollout patterns and geographic dynamics of the Great Recession boom and bust. We discuss these fixed-effects extensively in section 3.1 below, but first we address the issue of DiD bias.

It is now well known that OLS estimation of a two-way fixed effects (TWFE) specification like equation (1) can generate biased estimates when treatment rollout is staggered in time (De Chaisemartin and D'Haultfoeuille, 2022; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2024). De Chaisemartin and D'Haultfoeuille (2022) survey several papers finding that bias is 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 as the control 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 potential drivers of bias are present in the SC setting. First, essentially every county is treated at some point. From East et al. (2018) we have an activation date for 3,126 counties across 50 states and the District of Columbia. The remaining counties without an activation date are very small areas, either with an atypical governance structure or which are grouped with another statistical reporting unit. Therefore, these counties do not comprise an appropriate counterfactual for the period after full rollout of SC. 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 US 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 have an impact that increases over time: if network effects are important for generating a payoff to migrating to any area, then increased deportation may make future inflows less appealing or less likely.[7]

Several approaches to bias-robust estimation have arisen in the literature. We use the estimator

[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.

proposed by Borusyak et al. (2024), which belongs to a class of imputation-based estimators.[8] At a high level, imputation-based estimators can be thought of as implementing a two-step approach: first estimating the unit-based and time-based component of outcomes from untreated units only, and subsequently estimating a treatment effect in a second stage regression where the dependent variable has been residualized of unit- and time-constant variation from the first step. 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. Imputation estimators also extend tractably to sub-sample analysis, allowing us to explore treatment heterogeneity by factors like ex-ante prevalence of undocumented workers. While the use of a bias-robust estimator does induce meaningful differences from a naive (and incorrect) TWFE estimator, our results are not sensitive to the specific imputation estimator used.

A common feature of all bias-robust estimators is that untreated units are necessary to form valid comparisons. Therefore, once all units have been treated there is no longer any variation to exploit. More concretely: because the "imputation" component of an imputation-based estimator involves projecting time fixed effects from untreated units, once all units have been treated, that projection is no longer possible. In our setting, this means that data from 2013 onward do not inform our estimates as a consequence of the estimator. Therefore, the cessation and/or rebranding of the SC program in 2014 does not affect our inference about the effect of SC activation.

Along with the pooled-DiD estimate, we estimate the event study analog of equation (1) for all main results:

$$y_{it} = \alpha_i + \gamma_{FE(t)} + \sum_{k \neq -1} \beta^k \mathbb{1}(years\ since\ SC\ activated_{it} = k) + \epsilon_{it}. \tag{2}$$

As usual, the event study analysis is helpful for offering a visual assessment of the parallel trends assumption, as well as for illuminating dynamics. That said, our setting limits us to exploring short- and medium-run outcomes. When all units are eventually treated, bias-robust DiD estimators require accepting a limitation on the time-horizon that can be explored: treatment effects are based on variation only during the period of active rollout. For SC, the earliest treatment is coded to 2009, and the last time a control group exists is 2012. This places an upper ceiling on the estimation horizon of four years: contemporaneous plus three. In practice, the $t = 3$ estimate

[8] Initial versions of paper used an early imputation-based estimator proposed by Gardner (2022). The approach of Borusyak et al. (2024) nests this approach, and has become more standard in the literature.

would rely on comparing only the 2009 treatment cohort to the 2012 untreated cohort, and that 2012 cohort is a relatively small number of counties concentrated in Alabama and Illinois. As a result, given the robust set of fixed effects we use, estimation for $t = 3$ is not always feasible, and when it is feasible, the estimate ends up being based on a small number of thinly-populated cells. Therefore, in event studies, we restrict attention to a three-year horizon: contemporaneous plus two. This ensures that no estimate is based on a single cohort, and avoids producing estimates that are generated by a relatively small set of comparisons.

### 3.1 Is the Great Recession a Confounding Factor?

Every outcome in this paper concerns housing markets, and our empirical analysis exploits variation from 2009 to 2013. Because this period falls directly after the subprime mortgage crisis and the Great Recession, concerns about endogeneity are naturally first order. Three aspects of our empirical design and analysis, taken together, should allow the reader to be confident that our results are not driven by some latent recessionary or subprime-related factors:

- For $\gamma_{FE(t)}$ in equation (1), we use a very robust set of fixed effects that restrict identifying comparisons to counties which are similarly exposed to Great Recession dynamics. If the geographic rollout of SC correlates with Great Recession factors in some way, these fixed effects are designed to purge our estimates of that problematic endogenous variation.

- We validate that housing-cycle dynamics and population shifts do not appear to predict SC rollout. Our design, therefore, is not comparing counties which overbuilt during the runup to the subprime crisis to counties with less sensitivity to housing market dynamics; neither is the analysis comparing growing counties with shrinking counties.

- Event study evidence consistently confirms that counties appear to be evolving in parallel before SC treatment, including on margins of homebuilding and house prices. This shows that the identifying comparisons in our analysis are between counties which seem to be reacting similarly to macroeconomic factors – including those related to the subprime crisis – for several years before increased immigration enforcement turns on.

We elaborate on the first two of these aspects below. We discuss parallel trends in the Results section 5.4.

### 3.1.1 High Dimensional Fixed Effects: $\gamma_{FE(t)}$

Our core specification adopts a robust set of fixed effects which are designed to restrict comparisons among counties that have similar ex-ante exposure to subprime and Great Recession-related factors. In equation (1), $\gamma_{FE(t)}$ consists of two sets of indicator variables interacted with year: $\gamma_{FE1(t)}$ and $\gamma_{FE2(t)}$. For the first set, we assign each county to one of 20 quantiles based on house price appreciation (HPA) between 2000 and 2007. Due to the South-to-North rollout pattern, we also interact each quantile indicator with Census Region. This HPA-Region bin is then interacted with year indicators: $\gamma_{FE1(t)} = \{00-07\ \textit{HPA bin}\ \times\ \textit{Region}\ \times\ \textit{year}\}$.

The second set of indicator variables is formed in the same way, starting with 20 quantiles of county size (2005 population): $\gamma_{FE2(t)} = \{2005\ \textit{population bin}\ \times\ \textit{Region}\ \times\ \textit{year}\}$. The interaction with calendar year means that counterfactual outcomes for treatment counties are allowed to evolve differently by county size and lagged house price dynamics. We include both sets of fixed effects in all specifications: $\gamma_{FE(t)} = \gamma_{FE1(t)} + \gamma_{FE2(t)}$.

This approach ensures that our estimates are not driven by correlation between SC treatment and housing cycle dynamics related to the Great Recession. Since SC activation does show a pattern of rollout from the Southern border upward, one potential threat to a causal interpretation would be some version of the following story: many of the initially treated states are so-called "Sand States" which saw the largest runup in construction and home prices during the 2000–2007 period, suggesting the potential for a cyclical collapse in building after the Great Recession that happens to coincide with SC rollout. To address this concern, our empirical design identifies treatment effects by comparing the entire path of outcomes between counties: (i) in the same Census Region, (ii) which have experienced a similar path of home prices prior to the subprime crisis, and (iii) of similar size.

### 3.1.2 What Predicts SC Rollout?

Other scholars have explored determinants of SC rollout. Cox and Miles (2013) consider measures of crime, income, non-Hispanic immigrant share, and political attitudes. Despite clear rhetoric from SC leadership implying a focus on jurisdictions facing high levels of crime, the authors find that only two factors strongly predict county rollout: (i) sharing a border with Mexico and (ii) Hispanic population share.

In Table 2, we explore several additional factors that are directly related to housing demand,

housing markets, and dynamics of the Great Recession cycle. Each column shows the results from regressing an indicator for SC rollout in a given year on county-level characteristics in a stacked dataset, where each stack consists of all counties that either launch SC in a given year or counties that are not yet treated. (So the 2010 stack, for instance, would code counties launching in 2010 as 1, and code counties launching in 2011, 2012, or 2013 as 0, and exclude counties which already launched in 2009.) This regression, therefore, gauges whether a given county-characteristic predicts the specific year of rollout.

Column (1) confirms the existing finding from the literature that *ex-ante* Hispanic population share is a strong predictor of rollout. This county-level feature will be absorbed by the county-fixed effect in our regressions, and so from column (2) onward, we retain Hispanic share as a control variable, and test for the marginal effect of other predictors. Column (2) shows that county size does not meaningfully predict rollout. Columns (3)–(6) test the predictive ability of three-year lagged population growth trends. SC rollout is not associated with a three-year growth trend in US-born population or in Hispanic population. SC rollout is associated with trends in LEFB population, however the effect is very small. The standard deviation of LEFB 3-year growth in the sample is 112%, meaning that a standard deviation shift – more than doubling LEFB population growth – decreases the likelihood of SC launch in a given year by less than 1pp.

Columns (6) and (7) validate the construction of $\gamma_{FE(t)}$ by testing directly for a link between SC and housing boom-bust dynamics. In column (6), we compute the growth in new construction (total units) in the three years prior to launch. We do not find any evidence that prior building predicts SC rollout either economically or statistically.[9] This means that our results are unlikely to be driven by cyclical fluctuations in building around the Great Recession. In column (7), we test whether the total price runup between 2001 and 2007 predicts SC rollout. Again, we find no predictive power, suggesting that any results we find are unlikely to be driven by a correlation between treatment and house price patterns in the years prior to the Great Recession.

In total, the literature already suggests that SC launches earlier in localities with higher Hispanic share, and our findings confirm this. Apart from a weak association with lagged LEFB growth, other relevant factors do not appear to be a strong predictor of rollout. This non-predictability of rollout, along with the stringent restriction of identifying comparisons to counties which are similarly exposed to Great Recessionary factors, should allow the reader to be confident that our

[9]The sample reduction in column (6) is due to a set of small counties that do not show any new construction in the base year, making a growth calculation impossible.

estimates capture the causal impact of increased immigration enforcement. The remaining guardrail of our empirical design is event-study evidence that demonstrates parallel pretrends. In Section 5.4, we show that outcomes are evolving similarly between treatment and control counties for several years prior to SC activation.

## 4. Data

This section introduces datasets used in this study.

### 4.1 Secure Communities

Information on the rollout dates of SC comes from East et al. (2018), who gather the implementation date of SC at county level from ICE.[10] Based on the rollout dates, we construct a county-year-level dummy variable indicating whether SC has been implemented. Due to additional concerns about bias in DiD designs that use continuous treatment variables, we use 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 the year of launch if SC was introduced for at least six months of the year, and untreated otherwise. Once treated a county remains treated throughout the sample.[11] SC was implemented in all counties by 2013.[12] Among 3,126 counties, 2% adopted SC in 2009, 11% in 2010, 31% in 2011, 53% in 2012, and 3% in 2013.[13]

### 4.2 CoreLogic

CoreLogic compiles deed transaction records and property tax roll information from US county assessor and recorder offices. This data spans the near-universe of properties in the US, including variables on property characteristics, geographical locations, ownership changes, transaction date, and sales prices. 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

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

[11] As discussed in Section 2, SC 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.

[12] We exclude the following counties, all of which appear to have atypical governance structures, 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).

[13] If using the calendar year of SC implementation, among 3,126 counties, 0.4% adopted SC in 2008, 3% in 2009, 25% in 2010, 35% in 2011, 33% in 2012, 3% in 2013.

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, as well as 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.

Our final dataset contains 4.59 million observations of newly constructed homes between 2005 and 2012, of which 3.37 million are single-family. For approximately 63% of the single-family observations, we observe arm's-length market prices along with a full set of hedonic characteristics used in the regression analysis. We provide complete details for each step of the CoreLogic data build in Section A of our Internet Appendix.

To examine the effect of SC on the price of existing housing stock, we further draw on 11.51 million arm's-length house resale transactions between 2005 and 2012. The data construction process is described in detail in Section A.3 of our Internet Appendix.

### 4.3 American Community Survey

We use survey data from the 2005–2013 American Community Survey (ACS) to construct measures of employment, wages, and tract-level demographic shares. We construct measures separately for US citizens and for undocumented residents. This latter grouping requires a proxy for documentation status, which is not asked in ACS. Multiple proxies are both feasible and relevant in this setting. Our preferred measure adopts the standard approach in the immigration literature and focuses on individuals who indicate low-education (12 or less years of formal schooling) and foreign-birth. This grouping, denoted LEFB, will include a large share of undocumented individuals. Of course the group will also capture some immigrants who are in the county legally. We also consider "non-citizens", although (a) misreporting or non-response is a particular concern with this question, and (b) non-citizens, like the LEFB designation, will also include a number of immigrants with legal status.

Both of these proxies create a group of individuals who are disproportionately likely to be affected by immigration enforcement. That each grouping also contains people who are not subject to immigration enforcement does not inhibit our ability to use this segmentation to estimate the impact of SC. To the extent that large numbers of undocumented individuals do *not* fall into the LEFB or noncitizenship status, our estimates of workforce impact would be understated.

In examining workforce impact for construction specifically, we also consider respondents who

indicate Hispanic heritage.[14] Although this grouping will certainly include a large number of US citizens, approximately 30% of the US construction workforce is Hispanic[15] as are approximately three-quarters of all undocumented residents (Passel and Cohn, 2018). The grouping of Hispanic respondents, therefore, is likely to include a nontrivial share of those potentially impacted by SC, especially for construction occupations.

Because SC rollout occurs at the county level, we need to align ACS measures to the county-level. ACS microdata is released with geographic granularity at the Public Use Microdata Area (PUMA) level: areas with at least 100,000 people. For approximately half the country (by population), an unambiguous PUMA-to-county mapping is possible: either N-1 or 1-1. In remaining cases, where a PUMA spans multiple counties, we apportion ACS observations between the underlying counties on a population-weighted basis.

Table 1 provides summary statistics for key variables and datasets, including new construction, workforce, house price microdata, and wages.

## 5. Results

### 5.1 New Construction: Quantity

We use administrative deed transaction records from CoreLogic to test whether increased immigration enforcement affects the quantity of residential building in local housing markets. CoreLogic's ownership transfer data contain 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 was built. Therefore, by focusing on the built year, we can also test new construction completed, as long as that property sells at some point before 2022 (the last year reflected in CoreLogic as of writing). This means that our analysis will only miss new construction that fails to sell for more than 10 years. This window is sufficiently long that any omissions are likely limited to a small and highly idiosyncratic number of properties.

Table 3 shows the pooled DiD results following equation (1). Panel A uses units per thousand people as the dependent variable, and Panel B uses total square footage. All results fix the popu-

[14]The ACS data allows us to exclude those who indicate Puerto-Rican heritage, as they are likely to be citizens.

[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.

lation denominator as of 2005. For completeness, the first three columns illustrate how the choice of fixed effects, $\gamma_{FE(t)}$, affects estimates. In addition to county fixed effects, column (1) uses year fixed effects—the standard DiD design. Column (2) uses Region-year fixed effects, in recognition of meaningfully different population growth rates by Census Region. Column (3) is our preferred specification which robustly controls for different exposure to the Great Recession as well as county size, as outlined in Section 3.1.1. While all three columns show declines, the smaller magnitude of our preferred specification shows that restricting comparison to counties that experience similar home price trajectories during the 2000s is meaningful. The reduction of .165 units per 1,000 people in column (3) corresponds to a reduction of 80 homes built each year for the average-sized county (476,000 people in 2005). This is a reduction of 5.7% relative to the pre-policy baseline rate of new construction (2.89 units per 1,000 people) captured by CoreLogic. Because CoreLogic does not have 100% coverage of all geographies, and also because certain classes of new construction may be difficult to observe in the deed transaction records – as in the case of units built but retained by the developer ("develop-to-rent") – these declines are measured within and compared to a subset of total construction activity. However, Appendix Figure A2 plots the measure of new construction from CoreLogic against Census data, and shows that the two series track each other extremely closely with only a level-difference.

Column (4) restricts attention to single-family homes, and shows that this segment accounts for effectively all of the declines that we estimate. This is perhaps unsurprising, as single family homes represent the majority of housing stock in most localities. Column (5) shows that estimated declines outside of single-family are economically and statistically zero. This may suggest that workforces for multi-family development are less reliant on undocumented labor, however this remains an area for future exploration. Column (6) repeats the estimation for new construction, but uses sale-year instead of built-year. There is not a meaningful difference. The slightly larger estimate using the sale-year measure suggests that SC may have a slight impact on time-to-sale.[16]

Panel B repeats the analysis using total square footage of new construction. The number of units is our preferred measure: total square footage is a slightly nosier measure of new construction because it is not always populated in CoreLogic records.[17] As before, comparison of columns (1)–(3) shows that our high-dimensional fixed effects reduce the magnitude of declines estimated. Our

[16] As part of our analysis of price impact, we test for adjustment in time-to-sale directly; Appendix Table IA1 shows a very small effect.

[17] Coverage of hedonic attributes in the tax roll data is variable by county, but not typically by county-year, meaning that there is no reason to believe that the presence of square footage information covaries with SC treatment.

preferred specification in column (3) suggests a decline of 363 sq. ft. per thousand people. This lines up extremely close to the evidence in Panel A: the median newly constructed home (before SC) is 2,232 sq. ft, and so the estimated decline corresponds to 78 fewer units for the average size county. Columns (4) and (5) again show that declines appear in the single-family segment, rather than in multi-family. Column (6) indicates that any wedge between built-year and sale-year does not appear to be consequential.

### 5.2 Effect on House Prices

The Econ 101 prediction associated with reduced housing supply is higher home prices. However, the general equilibrium impact of immigration enforcement on home prices is not so straightforward, as deporting individuals mechanically reduces demand for housing services. Recent political rhetoric has emphasized this potential outcome of increasing immigration enforcement.[18] In this section, we test SC's impact on home prices, using observed transaction values for arm's-length transactions.

We segment our analysis of prices into two components. The goal is to distinguish – to the extent possible – between supply- and demand-side drivers. We first focus on new construction in an attempt to isolate supply-side impacts of SC. 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 highest-end housing stock within a given area, the demand-side impact of SC is less likely to affect price estimates within this segment.

Our second analysis considers the price impact on existing housing stock. There is no reason to believe that SC represents any direct supply shock in this segment.[19] Filtering theories in housing markets suggest that we should expect price spillovers from new construction to existing housing stock, allowing supply-side drivers to affect existing stock prices; however this link is both indirect and potentially realized on a longer timescale. As a result, shifts in existing stock prices seem more likely to arise from changes in demand associated with immigration enforcement. While not a perfectly clean separation of supply-side and demand-side channels, our preferred interpretation is that price impact in new construction is most likely generated through supply-side channels; and that price impacts in existing housing stock reflect both supply- and demand-side channels, but

[18] See, for instance, Stephen Miran's September 2025 speech at the Economic Club of New York after his appointment to the Federal Reserve Board of Governors.

[19] That said, spillover effects are possible: SC's impact on labor supply could slow down residential redevelopment, reducing the share of existing stock that is converted to freshly-refurbished listings.

are likely most responsive to demand-side channels in the near term. In both analyses, we focus on single-family homes: the segment in which large quantity responses are evident.

#### 5.2.1 Price Impact in New Construction

We use log-transaction prices as the dependent variable in equation (1). We test for impact both in raw prices and in specifications that include a rich set of hedonic controls. The effect on raw prices incorporates any impact arising from endogenous changes in home characteristics. Specifications that include hedonic controls represent quality-adjusted prices, measuring how reduced homebuilding affects prices, imagining that we can compare identical homes. Another standard approach to provide this analysis would 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. Instead we control for quality with home characteristics reported in the CoreLogic data. Three major attributes capture size: square footage, number of bathrooms, and number of bedrooms. For square footage, we convert the continuous variable into small discrete bins and then employ a fixed effect for each bin. We include fixed effects for the integer number of bedrooms and bathrooms, along with fixed effects for age (a proxy for time-on-market) and census tract.

Table 4 shows the pooled-DiD estimates. SC increases raw transaction prices for new construction by 4.5%. The inclusion of hedonic controls changes little: in column (2), adding tract, size, and time-to-sale FEs results in an estimated increase of 4.8%; and in column (3), the further addition of FE indicators for bedrooms and bathrooms drives the estimate to 4.4%. These small differences between columns suggest that the effect of endogenous changes in location, sale-timing, or build-attributes is marginal at most. In Appendix Table IA1, we test directly for changes in size and for time-to-sale. Consistent with reduced supply, we find that homes sell a little bit more quickly; however the point estimate suggests a reduction of less than a month. We estimate a statistically insignificant change in home size, number of bedrooms, and number of bathrooms.

#### 5.2.2 Price Impact in Existing Housing Stock

As discussed, a theoretical prediction for SC's impact on existing housing stock prices is more ambiguous. Through a supply-side lens, reduced new construction will place upwards pressure on prices: facing additional bidding competition, some who might have preferred to purchase a newly constructed home will substitute to existing homes instead. However, since immigration

enforcement entails removing individuals from local markets, the reduction in total demand for housing services could place downwards pressure on home prices. Which effect dominates is an empirical question.

Panel B of Table 4 shows the effect of estimating equation (1) within the (much larger) set of resale transactions for existing homes. Columns (1)–(3) replicate the analysis of new construction, with an increasing set of hedonic attributes. As before, the estimates are not sensitive to these controls. All three columns suggest that SC has little effect on existing stock prices. However, this full-sample estimate obscures important and intuitive underlying heterogeneity.

Column (4) explores price impact by tract-level share of undocumented residents. Neighborhoods with fewer undocumented residents would be expected to see relatively less price impact from demand-side factors, and conversely one would anticipate the largest demand-side effects in neighborhoods with the most undocumented residents. We use ACS 5-year estimates from the 2006–2010 vintage to assign tracts to quartiles of undocumented population share, using the LEFB proxy for documentation status (see Section 4 or discussion in Section 5.3). In lowest-quartile LEFB neighborhoods (LEFB share less than 0.1%) we find that home prices increase by average of 3.4%. This price impact appears to monotonically decline with LEFB share: in the second quartile, we see increases of 2.0%; in the 3rd quartile, insignificant effect on price; and in neighborhoods with the largest share of LEFB residents, we find that home prices decline by 4.1%.

These results in column (4) suggest that in areas with little or moderate baseline demand from undocumented residents, the supply-side impact seems to dominate, driving prices up. In areas with the highest baseline demand from undocumented residents, the demand-side impact from immigration enforcement appears more consequential, driving prices down on net. These results are pooled across the entire post-period, which represents 4 years worth of variation. Given that deportations reduce demand immediately, while supply-side tightening may affect prices more slowly – especially for segments of the housing stock which may be less comparable to new construction – it is potentially interesting to explore how price impacts vary over time, and whether this differs with neighborhood demographics. In Section 5.4, we return to treatment effect dynamics both in quantity and in price.

**Effect on Homeownership** Housing construction and house prices are, of course, interesting in their own right. However, for policymakers, the statistic of interest is often homeownership rates: homeownership (especially in the US) is often viewed as a desirable normative outcome for

both individuals and society at large. As with home prices, the net effect of immigration policy on homeownership is ambiguous. As other scholars have shown, immigration enforcement has meaningful impacts on labor market outcomes (East et al., 2018; East and Velásquez, 2022; Saiz and Wachter, 2011) and on rents (Saiz, 2003, 2007). The relationship is therefore fundamentally an empirical question. The ACS contains a question that asks if respondents are homeowners or renters. We use homeownership as the dependent variable in equation (1) and show the results in Table 5. Aligning with prior methodology, column (1) estimates the effect on homeownership while fixing 2005 population. SC reduces homeownership rate by 12.8 individuals per thousand (1.28%). Some of this effect may be mechanical if individuals deported under Secure Communities are homeowners. Column (2) estimates the homeownership rate directly, allowing the population denominator to fluctuate by year. We find that SC reduces the overall US homeownership rate by .93 percentage points.

### 5.3 Evidence on Construction Workforce as Mechanism

Although a range of factors could link immigration enforcement with reduced homebuilding in general equilibrium, the most obvious direct mechanism to explore is workforce. It is well known that undocumented individuals are over-represented in construction occupations, along with agriculture and hospitality. Exact measurement is a challenge, but typical estimates suggest that 15–20% of the construction workforce is undocumented (Passel and Cohn, 2015; Appleby, 2024; Svajlenka, 2021). A sharp increase in immigration enforcement, therefore, potentially represents a sizable negative shock to labor supplied to the construction industry. In turn, this could reduce output if builders are unable to replace deported workers with domestic labor, or if workers attracted into the construction sector are less productive (perhaps due to inexperience) than those exiting the sector.

Figure 3 uses administrative records maintained by ICE and aggregated to the county-year level by the Transactional Records Access Clearinghouse at Syracuse University, to plot the number of removals under SC by calendar year (top), and relative to the launch year (bottom). Mirroring the time-window used in our empirical approach, these figures include deportations from SC inception through the end of federal fiscal year 2013 (September 30th, 2013), but do not include any removals attributed by ICE to SC from fiscal year 2014 onward. The data reflect a total of 306,000 individuals deported. It is important to note that the number of deported individuals in the administrative records reflects a lower-bound on the population impact of SC. There are multiple indirect pathways

to removal short of official deportation: facing increased immigration enforcement, some individuals may elect to voluntarily depart the country; some who would otherwise have immigrated may elect not to; and finally when any individual is officially deported under SC, family members or close associates may also voluntarily leave the country alongside the deported individual. As a result, the true population impact of SC is likely a multiple of ICE-recorded deportations. One salient feature of SC deportations, evident in the bottom panel of Figure 3, is that the pattern after SC activation is increasing and concave. Treated counties do not experience a single spike of removals, but rather see individuals removed over multiple years. This suggests that treatment effects may build over time.

Administrative data on deportations is not sufficient to understand how SC impacts construction workforces. The administrative data contains no information on occupation for those deported under SC.[20] As a result, an industry-level analysis requires estimation using ACS microdata. ACS contains reported occupation as well as information on time worked over the prior year. Our measure of workforce includes those of working age who report construction occupations, regardless of employment status. Informally, we are counting those who report themselves to be construction workers, even if they didn't work over the prior year. This is the best measure of labor supply that we can extract from the ACS data, but it is important to recognize that this is an equilibrium outcome. Physical deportation under SC is one way that construction workers in a given county can be reduced. However, in the ACS data, switching to another occupation will also reflect a reduction of labor supplied to the construction sector. Therefore, our estimates of SC impact will include direct deportation plus other spillover impacts of immigration enforcement.

As described in greater detail in Section 4, we need a proxy for documentation status, because no question specifically addresses this in the ACS data. Our preferred measure is standard in the immigration literature: foreign-born individuals with 12 or less years of formal schooling (LEFB). This measure identifies a group that is more likely to be undocumented, but will also include legal residents. Other proxies are possible—the ACS, for instance, also includes a citizenship question. Like the LEFB measure, non-citizens will encompass both undocumented residents as well as those with legal standing. We show below that estimates of SC's impact on construction workforces are quite similar across proxies.

We first look at the impact on all undocumented working-age individuals using the LEFB proxy. The first row of Table 6's Panel A shows that SC reduces working-age LEFB population by 1.32

[20] To the best of our knowledge, ICE does not solicit or record such information at any point.

workers per 1,000 people in the US in 2005: this corresponds to a reduction of just over 390,000 people. As noted, it is reasonable to expect that this figure would be somewhat larger that the number of direct individuals deported according to ICE. Overall, the alignment of this evidence with administrative records suggests that estimation using ACS microdata is reasonably effective at picking up the impact of SC.

The remainder of Panel A disaggregates the impact of SC across major occupational sectors. Each row is a separate regression with the number of working-age LEFB individuals per thousand people as the dependent variable; note that regression algebra makes this a mechanical disaggregation of the total workforce impact ("All"). The major finding illustrated by Table 6 is that immigration enforcement seems to have a larger impact on construction workers than on other sectors: a decline of .77 individuals per thousand. Construction has long been one of the primary occupational destinations for undocumented workers; however, the magnitude of the observed decline is disproportionately large relative to the sector's share of undocumented employment. This pattern suggests additional cross-sector reallocation away from construction. Consistent with this interpretation, we observe net inflows into sectors such as agriculture. One plausible explanation is spatial: construction activity is concentrated in denser urban areas, where law enforcement presence is stronger, whereas agricultural employment is largely rural, where enforcement intensity tends to be lower (Weisner et al., 2020). Anecdotally, stereotypes linking construction day laborers with undocumented migration may shape enforcement priorities, prompting targeted actions at construction sites and home-improvement store parking lots, thereby amplifying the construction sector's exposure.[21] We leave this for future research. The estimated declines in LEFB construction workforce translate to a reduction of 228,000 workers, or 2.1% of the 11 million workers indicating construction occupations in ACS.

Panel B zooms in on the construction sector. Column (1)–(3) show that declines are measured similarly regardless of proxy grouping for undocumented workers: estimated declines in the non-citizen and Hispanic groupings are very close to the LEFB estimate. Column (4) shows a decline for US-born workers as well, suggesting that domestic labor inflows do not appear to offset SC-driven declines. Column (5) estimates the total impact on construction workforces: a decline of just over 1 worker per thousand population. Aggregated across the population, this corresponds to an estimated reduction of 299,000 construction workers—a 2.7% decline in the workforce.

[21] See, for instance: "Hanging out in the parking lot of a Home Depot, hoping to get hired on a day job, is soliciting." from https://www.vox.com/2015/11/2/9657806/criminal-alien-program

**Different Impacts by Occupational Skill** The finding that SC leads to the decline of US-born construction workers is potentially surprising. If increased immigration enforcement leads to a shortage of construction labor supplied, then the natural prediction would be for US citizens (or legal residents) to fill the vacant positions, increasing employment share for that population. However, the analysis in Panel B of Table 6 shows a statistical zero and a negative point estimate for US workers.

One potential explanation is that construction labor markets are segmented and that undocumented labor supply acts as a complement 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.[22] 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 sort workers by skill.

We follow the labor literature by using the US Department of Labor's Occupational Information Network (O*NET) classification of skills required by four-digit occupational category. O*NET assigns each occupation into one of five Job Zones based on the education, related experience, and job training required to perform each occupation. All construction occupations fall into Job Zones 1–3. We designate those falling into Job Zone 3 as higher-skilled; and those falling into Zones 1–2 as lower-skilled. A complete list of occupations appears in Appendix Table IA2.[23]

Table 7 estimates workforce impact within each skill grouping for LEFB and US-born workers. Within lower-skilled occupations, we see some evidence of domestic labor substituting for undocumented labor. In the likely-undocumented population, SC results in a reduction of .49 workers per thousand; this estimate is precise and highly significant. For US-born lower-skilled workers, the estimated impact is a statistical zero. However, even taking the positive point estimate at face value, this suggests an approximately 40% replacement rate: for every five lower-skilled LEFB workers lost, two US-born workers join the construction sector. This suggests a net loss, which is what column (6) finds, although this point estimate is also statistically insignificant.

An opposite pattern holds within higher-skilled occupations. SC also causes reductions in higher-skilled LEFB employment; however, the impact is approximately half as large for higher-

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

[23] The authors would also like to acknowledge some level of discomfort with describing any of these jobs as lower-skilled; we suspect we would find all of them quite difficult.

skilled workers as for lower-skilled workers: a decline of .28 workers per thousand. Rather than experiencing any offsetting increase, higher-skilled US-born workers also see employment declines in roughly equivalent magnitude: a decrease of .37 workers per thousand. In aggregate, higher-skilled construction occupations experience a total loss of .69 workers per thousand, approximately 70% of the total reduction in construction workforce. This large decline for higher-skilled workers is consistent with overall complementarity between labor supplied by undocumented workers and domestic labor. SC appears to reduce the overall quantity of lower-skilled labor supplied to the construction industry, because domestic labor only partially offsets the shock to immigrant labor supply. And this reduction in lower-skilled labor supply appears to be associated with an overall shrinkage in higher-skilled labor supplied by both domestic and undocumented workers.

**Evidence on Wage Adjustment** If labor shortages are a meaningful bottleneck leading to reduced output, a natural prediction might be that builders, facing increased housing prices, would increase wages to attract additional workers into the construction sector. We test the response of wages using individual-level survey responses from ACS to construct hourly wages. Column (1) of Table 8 estimates the total impact of SC and finds a statistically insignificant increase of 1.3%. Column (2) shows that any increases are entirely driven by LEFB workers: the estimate for US-born workers is a fairly precise zero, while hourly wages for LEFB workers increase by 4.2%. Wage increases for LEFB workers may represent the straightforward impact of reduced supply. It is also entirely possible that immigration enforcement raises the costs of working, and that this 4.2% increase represents a compensating differential. The lack of any increase for US workers indicates some segmentation or friction: the results suggest that builders are not raising wages to attract additional domestic workers to the construction sector.

Is it possible that immigration enforcement leads to compositional shifts in workforce—when some workers are removed, builders may need to accept shifts on margins including age, skill, or experience? Columns (4) and (5) repeat the analysis while including the full set of standard controls: age, age squared, gender, education, and race/ethnicity. This has no impact on the estimate for US workers: there is still a zero effect on hourly wage. About half of the estimated impact for LEFB workers appears to arise from composition: with a full set of controls, hourly wages for LEFB workers still increase by 2.2%.

Columns (3) and (6) break out the impact by higher- and lower-skilled LEFB workers with and without individual-level controls. Wage increases appear to be driven by lower-skilled LEFB

workers. Column (3) suggests that wages for higher-skilled LEFB workers increase by 3.7%, however these increases are lower and statistically insignificant once individual-level controls are added in column (6). Wages for lower-skilled LEFB workers increase by 8.2% without individual controls. Once individual controls are added, column (6) suggests that wages for lower-skilled LEFB workers increase by 6.4%, and further that no other group sees a meaningful and statistically significant increase in wages. This is strikingly consistent with the complementary interpretation of the prior labor supply analysis. Wage increases for LEFB workers would be expected if builders are competing to hire lower-skilled LEFB workers. The lack of a wage increase for lower-skilled domestic labor intuitively matches our finding that domestic labor supply does not provide a full offset. In addition, the lack of wage increases for higher-skilled workers is consistent with the idea that declines in employment in these groups result from an overall slowdown in building rather than shortages in labor supplied by these groups.

In our Internet Appendix, we test for wage responses using several alternate measures. One particularly detailed measure comes from a third party construction analytics firm, RSMeans, which directly surveys local markets to produce an aggregate measure of labor costs. This measure is restricted to a smaller geographic subset of 691 counties which correspond to surveyed labor markets, and also cannot be disaggregated by sub-population (*i.e.* by any proxy for immigration status). We also consider industry-level wages by county-year produced by the Bureau of Labor Statistics' Quarterly Census of Employment and Wages. As with overall construction wages measured in the ACS, we find little evidence that SC shifts average prevailing wage in the construction sector. Across all measures, the point estimates indicate small, statistically insignificant declines of 1% or less (Internet Appendix Table IA3).

In total, this wage analysis tends to support the interpretation that SC generates shortages and meaningful wage adjustment in one particular sector: labor supplied to lower-skilled construction occupations by LEFB workers. Neither wage increases for LEFB workers nor inflows from domestic workers are sufficient to offset these labor shortages. This generates a net reduction in what appears to be a critical input for the single-family home production function. In turn, this appears to lead to a reduction in output, along with reduced labor being supplied from other portions of the labor supply chain by both domestic and undocumented workers alike. While builders do appear to adjust wage for the critical input, this is done narrowly: there is no evidence of increased wage competition for lower-skilled US-born workers. This pattern would be consistent with builders believing that in order to attract large numbers of additional lower-skilled construction workers

into the sector, they would have to raise wages by such a significant amount that it would not be worth it. This remains an open question for future research.

### 5.4 Event Study Evidence: Validation of Parallel Trends

We show event studies to facilitate the visual assessment of parallel pretrends which is relevant in any DiD design. As Section 3 highlights, this is an important pillar of our overall identification strategy given the timing of Secure Communities and the Great Recession.

Figures 4, 5, and 6 show the event study analysis for new construction quantity, new construction prices, and existing stock prices respectively, following equation (2). In each figure, Panels A and B show that outcomes are evolving similarly in treatment and control counties before SC activation. This evidence of parallel trends during the pre-period supports the embedded DiD assumption that treated and control units would evolve similarly in the non-treatment counterfactual.

Event studies are also often helpful for illustrating the temporal path of treatment effects, however the econometric details of our setting complicate a dynamic analysis. The use of an estimator robust to staggered-DiD bias generates a critical corollary in an event study analysis: the number of cohorts that contribute to a given estimate decreases in time horizon. This is especially stark in a setting where all units are eventually treated. Because bias-robust estimators do not make use of treated-units as controls, longer-horizon estimates necessarily rely on the earliest-treated cohorts. We estimate event studies that show dynamic effects over three years: the contemporaneous year, and the following two. While the contemporaneous estimate ($t = 0$) is informed by all four treatment cohorts, the $t = 1$ estimate is informed by three treatment cohorts (2009, 2010, 2011) and the $t = 2$ estimate is informed only by two cohorts (2009, 2010).

If treatment effects were homogeneous by cohort, the event study would unambiguously recover the dynamics of treatment. However, in this setting it is likely that cohort heterogeneity exists. The most natural notion of 'dosage' or 'intensity' of the SC policy varies by cohort: earlier cohorts deport a larger share of the overall population than later cohorts. This is very likely because earlier cohorts simply have a larger share of deportation-eligible individuals than later-treated cohorts. Illustrating this point, Panel C of Figure 4 plots the event study of removals per thousand residents using administrative data from ICE. The removal rate after implementation declines nearly monotonically with each cohort.

Panel D shows the corresponding impact on new construction for each cohort. Here the pattern is also monotonic: cohorts which have largest removal rates also see largest declines in new con-

struction. The 2011 cohort, with the lowest overall removal rate, as well as the shortest post-period, shows no statistically significant reduction. The 2009 cohort, with the largest removal rate, shows not only large declines but also declines that appear to be increasing over time. The 2010 cohort shows a decline that appears to be fairly constant in the post-period. This clear relationship between dosage and the magnitude of response strongly supports the causal link between immigration enforcement and residential construction.

We emphasize that the presence of cohort heterogeneity does not invalidate our empirical design or call into question our identification strategy. The *purpose* of a bias-robust estimator is to ensure that DiD estimates are a convex combination of cohort-specific (heterogeneous) treatment effects. While this section uses event studies to get a sense of both treatment dynamics and of cohort heterogeneity, a critical recognition and caveat for the reader is that these cannot be separately identified.

Cohort variation at least partially explains why event studies show increasing impacts at longer horizons, as well as peak estimates that are substantially larger than the pooled DiD results in Panels A and B of Figure 4, Figures 5, and 6. Later estimates – especially $t = 2$ – are chiefly driven by cohorts that had the greatest intensity of deportations, and correspondingly realized the largest impacts on the outcome variable.

It is also possible that the general pattern of Figures 4, 5, and 6 reflects treatment effects that intensify over time. There are several ex-ante reasons to consider increasing impact likely. Annual aggregation means that the contemporaneous estimate is based on less than a full year's worth of treatment (unless the county launches SC on January 1st). The mechanism of SC depends on some random arrival process between residents and local law enforcement, which also takes time. To the extent that labor constraints inhibit building, these shortages may continue to tighten year-over-year as more undocumented residents are removed and also as natural attrition shrinks the domestic workforce. All these factors would tend to suggest increasing declines in output, which would naturally translate to increasing upward pressure on prices—consistent with the evidence shown.

The event study of price impact in existing stock is interesting to consider through this lens of intensifying treatment effects. In Figure 6, Panels C and D split the sample into neighborhoods with bottom-quartile share of LEFB residents (below .13%; left) and top-quartile share (above 3.5%; right). In tracts with very few LEFB residents, prices increase from the year of SC activation and appear to continue increasing over the two subsequent years as well. By contrast, in tracts

with the most LEFB residents, SC leads to price declines of 5% in the activation year and the first full year of implementation. This is consistent with price impacts arising through both supply- and demand-side channels. In areas that face very little demand from LEFB residents, SC appears to unambiguously raise prices. In areas where LEFB demand is concentrated, SC appears to push prices down in the near-term. By $t = 2$, however, the event study suggests that the net impact on prices even in highest-LEFB share neighborhoods is positive. The dynamic interpretation is that the supply-side impact dominates within three years, pushing prices up on average. Alternatively, an interpretation that focuses on cohort-heterogeneity would be that even in tracts where deportations place the largest downward pressure on home prices via a demand channel, the supply-side channel will still dominate in counties where the dosage of immigration enforcement is large.

Finally, for completeness, Figure 7 shows the event study for labor supply by all proxies for undocumented status, and for domestic labor. Figure A uses our focal LEFB measure, and Figures B and C use non-citizenship and Hispanic ethnicity respectively. In each, the evidence strongly supports a parallel trend assumption, and in each we see declines that increase in time (however, once again, these also reflect cohort heterogeneity). Panel D shows the results for domestic construction labor: here we see little response except at the $t = 2$ horizon. One interpretation is that frictions lead to slow adjustments in domestic labor supplied to the construction industry; the second interpretation is that large domestic inflows are only evident in counties which receive a relatively large dosage of enforcement. The lack of any estimated increases in either the contemporaneous year or at $t = 1$ – times at which the most treated cohorts have already seen a large number of removals – suggests that both explanations are likely at play. If there were not some temporal friction with respect to domestic workers moving into the construction sector, one would expect to see some evidence of inflows in the earlier years, even if they were entirely driven by the most-treated cohorts.

## 6. Conclusion

We document that immigration policy has important first-order supply-side impacts on home-building and home prices. Increasing immigration enforcement leads to: (i) reductions in residential construction, (ii) increases in the price of new construction, (iii) little overall impact on existing stock house prices, but meaningful underlying variation by a given tract's share of undocumented residents.

A county implementing additional enforcement under the Secure Communities program sees a

5.7% reduction in new construction, corresponding to 80 fewer homes per year for the average-sized county. Quality-adjusted prices for new homes increase by 4.4%. In tracts with very few LEFB residents – proxying for demand from undocumented residents – we see a similar price impact on existing stock homes: an increase of 3.4%. These increases are moderated in tracts with larger numbers of LEFB residents. Top-quartile LEFB share tracts see price declines of 4.1% pursuant to SC activation, however some evidence suggests that these declines may be transitory.

We explore the natural mechanism: because a large share of the construction workforce is undocumented, a sharp increase in immigration enforcement is likely to result in deported construction workers. We document this first stage impact in the ACS data: increasing immigration enforcement does lead to an overall reduction of workers in construction occupations, with declines appearing across multiple groupings that proxy for undocumented status. Our analysis suggests that labor supplied by LEFB residents to lower-skilled construction occupations is a critical input for homebuilding. As immigration enforcement reduces undocumented labor supply, we see wage premia for these lower-skilled LEFB workers. Interestingly, we do not see wage increases for US-born workers in these lower-skilled occupations. While there are domestic inflows to lower-skilled occupations, replacement is less than one-for-one. The net effect is a reduction in labor supplied to this set of occupations. We also see reduced labor supplied by workers in higher-skilled occupations. However, these reductions occur for *both* LEFB and US-born workers, and are unaccompanied by wage increases. As a result, this pattern seems most consistent with complimentary between different segments of the construction workforce. Net declines in lower-skilled occupations appear to represent a bottleneck in the production process that leads to reduced homebuilding, along with job losses for higher-skilled workers elsewhere in the labor supply chain.

Housing supply in the United States has been starkly lower than average for most of the past two decades. This paper provides novel evidence that immigration policy has consequential impacts on housing supply: removing construction workers from local markets results in reduced building and subsequently higher home prices. While increased immigration enforcement may also place downward pressure on prices through reduced demand, this paper suggests that the supply-side effects have a more consequential impact on prices in most areas.

One immediate implication is that policymakers interested in overall home affordability must carefully consider how proposed interventions will affect construction labor supply. In the Secure Communities setting, labor market frictions appear to inhibit immediate and full replacement of lost undocumented workers by domestic workers. If this holds generally, a corollary implication for

increasing housing supply is that zoning reform or streamlining, often touted as a necessary step, may not be sufficient: even if localities stand ready to build more homes, our findings suggest that important frictions may remain in securing the necessary construction labor.

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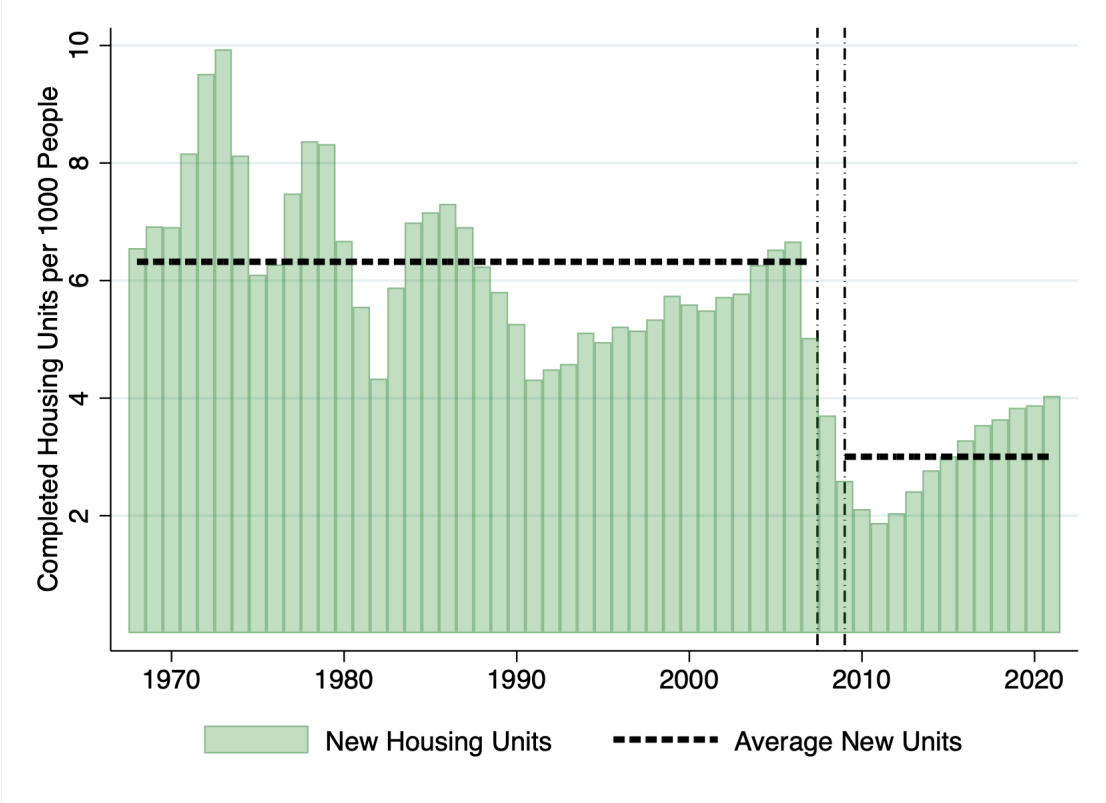
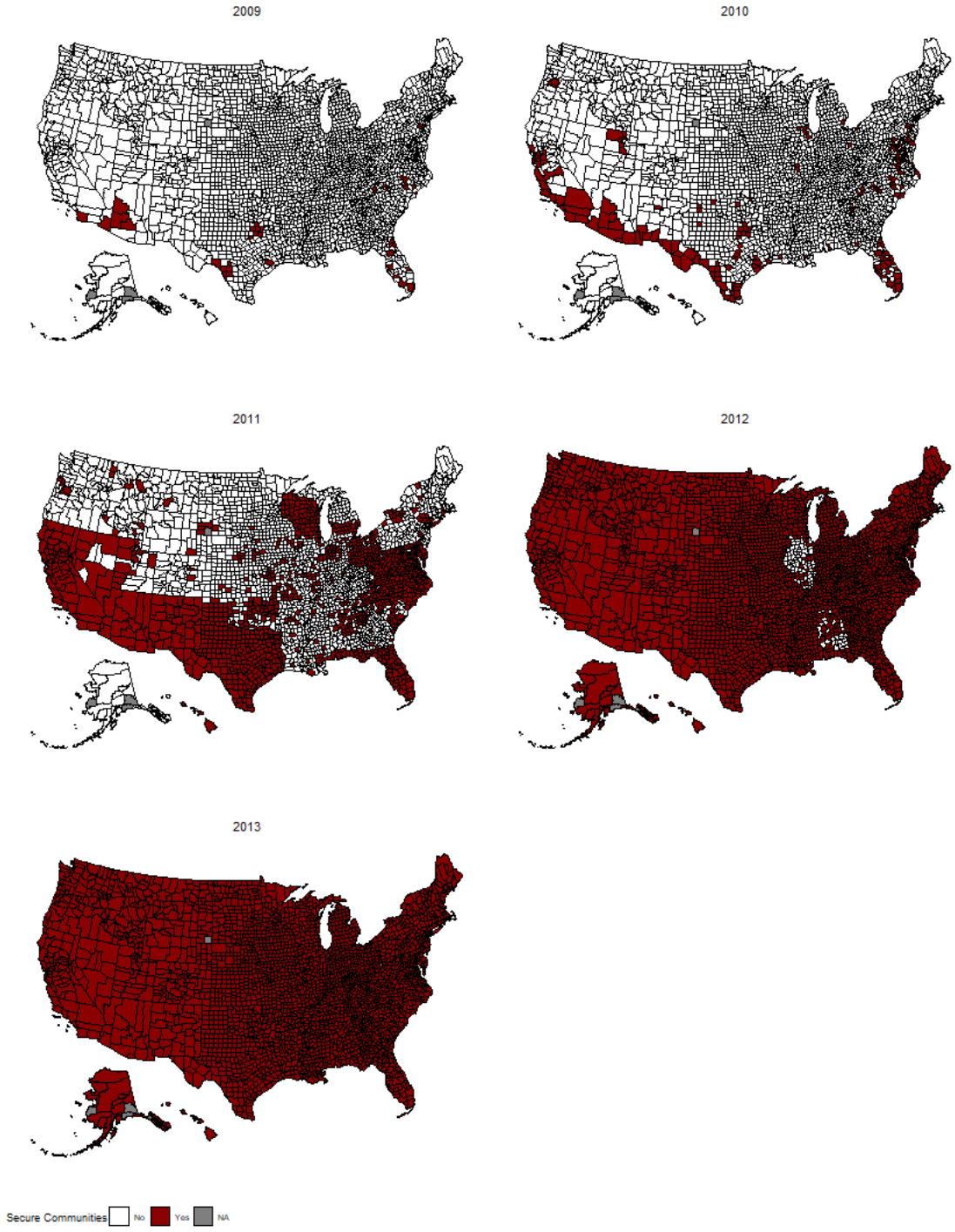
**Figure 1:** New Construction in the US (population-adjusted)



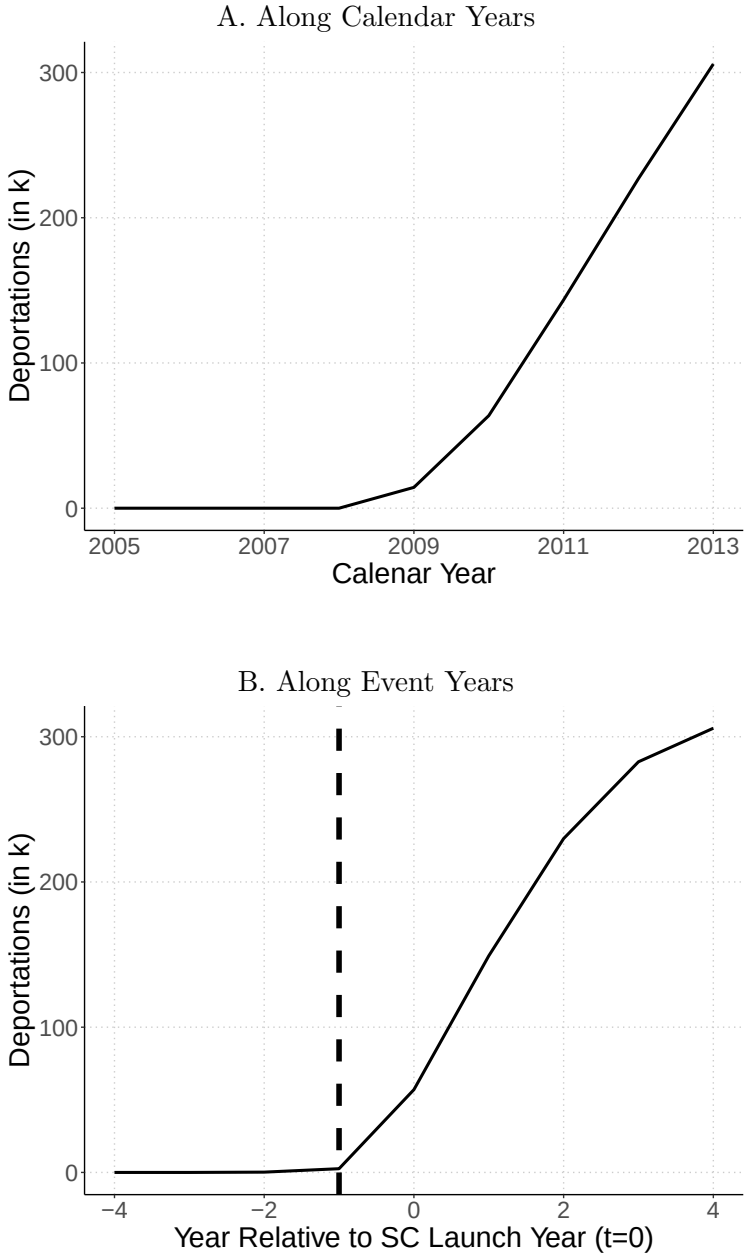

Note: This figure plots the time trends of constructed new housing units. The green bars are the annual new housing units per 1000 population (left axis) in the US from Census Bureau and HUD. The two dashed lines indicates the average levels of new housing units per 1000 population pre-GFC (1968–2007) and post-GFC (2009–2021).

**Figure 2:** Staggered Rollout of Secure Communities



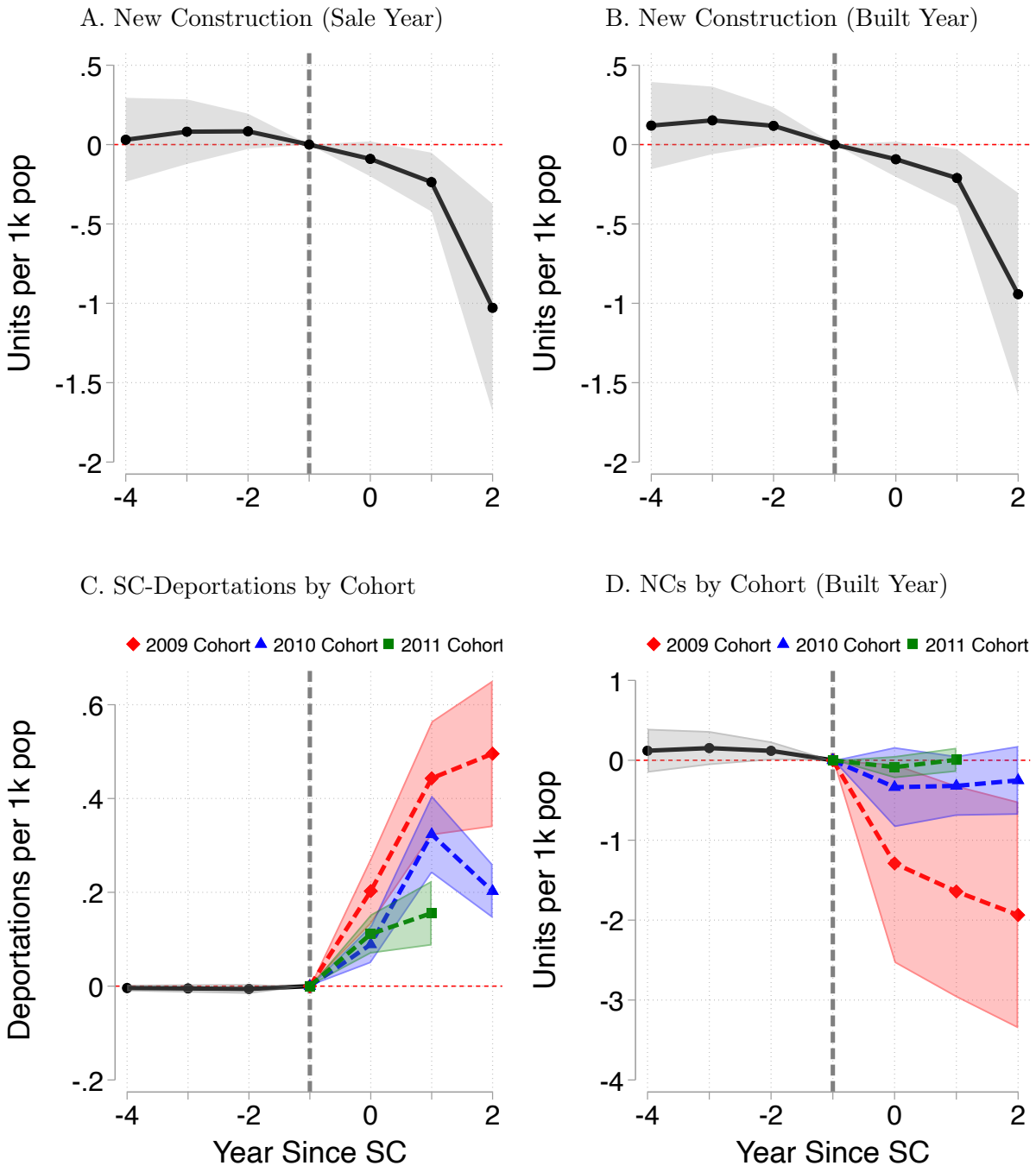

Note: Each panel of this figure shows the counties that implement SC within each year. This map reflects the treatment indicator used in our regressions, which assigns binary treatment status to any county operationalizing SC for at least half the year. Counties launching SC in, for instance, December of year $t$ would therefore be coded as untreated in year $t$ and treated in year $t+1$. Internet 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:** Cumulative Deportations under Secure Communities through 2013



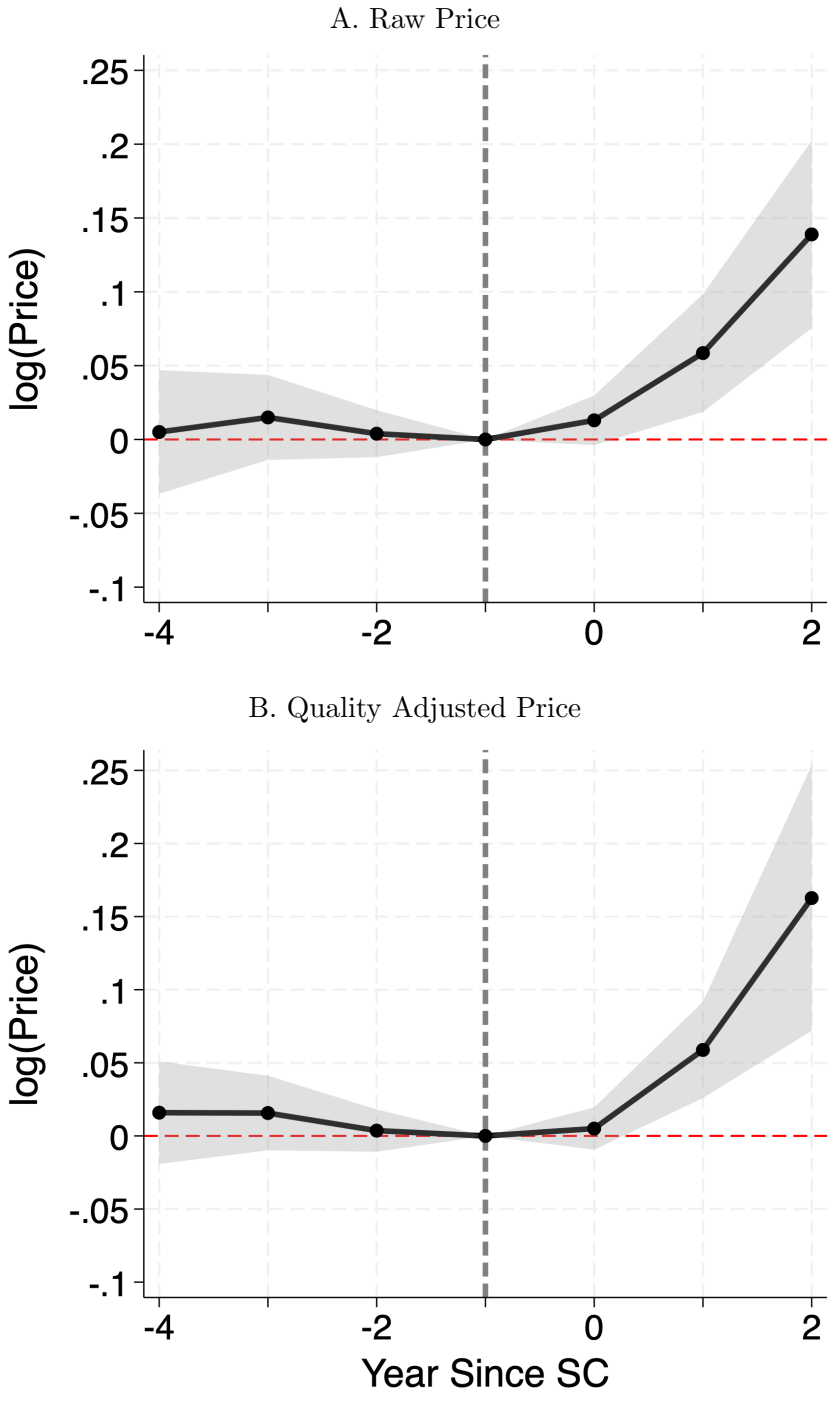

Note: This figure shows the cumulative number of individuals removed under Secure Communities. Data includes all removals through the end of fiscal year 2013 (Sept 30th, 2013), but does not include removals attributed to Secure Communities from FY2014 onward. Deportation counts by county-year are publicly available from TRAC at Syracuse University, and the figures are compiled from individual-level Department of Homeland Security administrative records. The top panel shows cumulative deportations under SC by calendar year, and the bottom panel shows cumulative deportations relative to the launch year.

**Figure 4:** Event Study of New Construction Entering Market



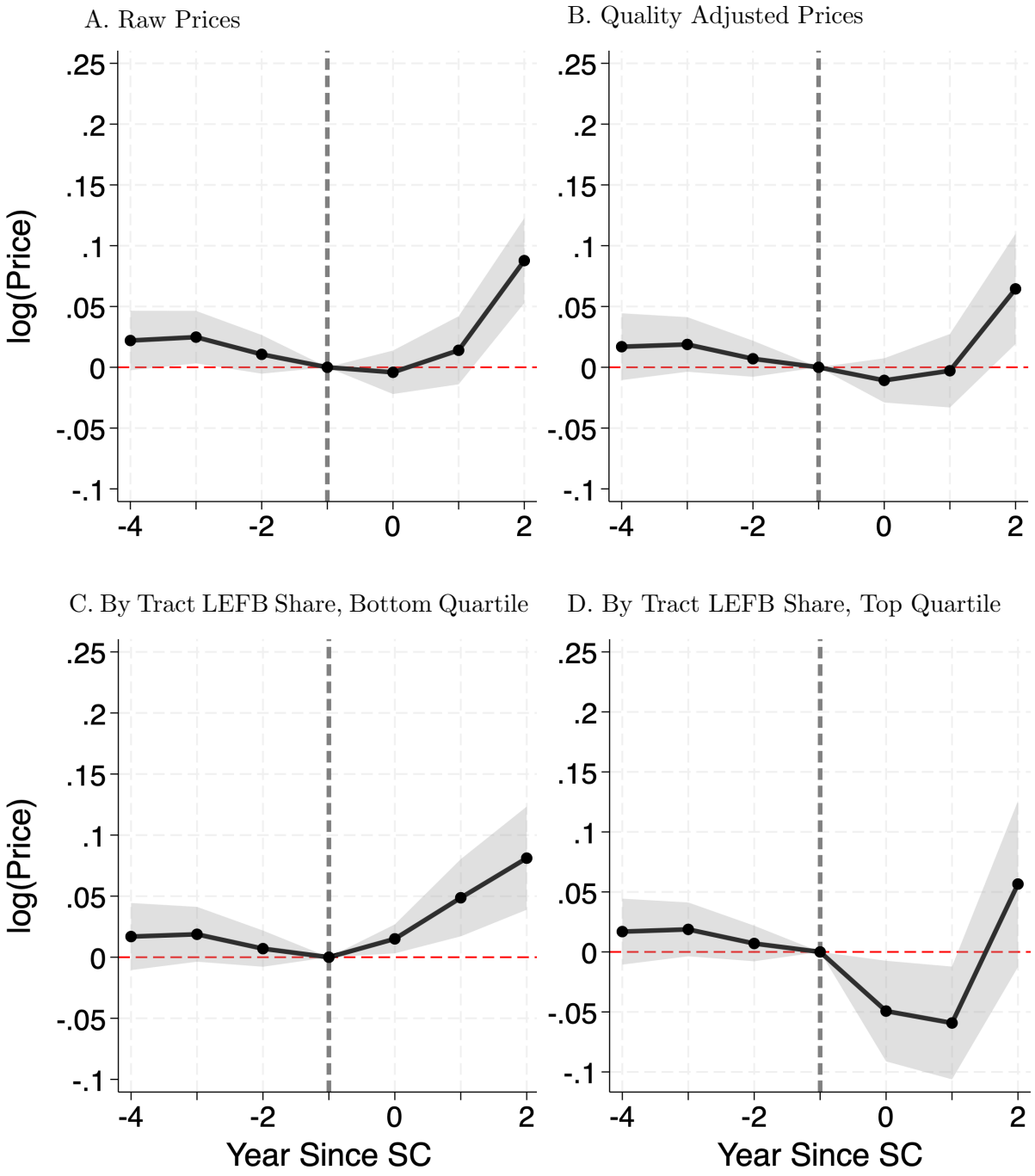

Note: Panels A and B show the event study for new construction units by sale-year and built-year respectively. Panel C and D show SC impacts by cohort. Panel C shows deportations using administrative data from ICE. Panel D estimates the impact on new construction. Cohorts implementing SC in 2009, 2010, and 2011 are depicted in red, blue, and green respectively. All outcome variables are normalized by the 2005 baseline population (per 1,000 residents) and estimated using the Borusyak et al. (2024) DiD estimator with the specification in equation (2). The regressions control for county, *HPA bin* × *Census Region* × *year* and *Population bin* × *Census Region* × *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors are clustered at the county level.

**Figure 5:** Event Study of Price Response, Single Family New Constructions



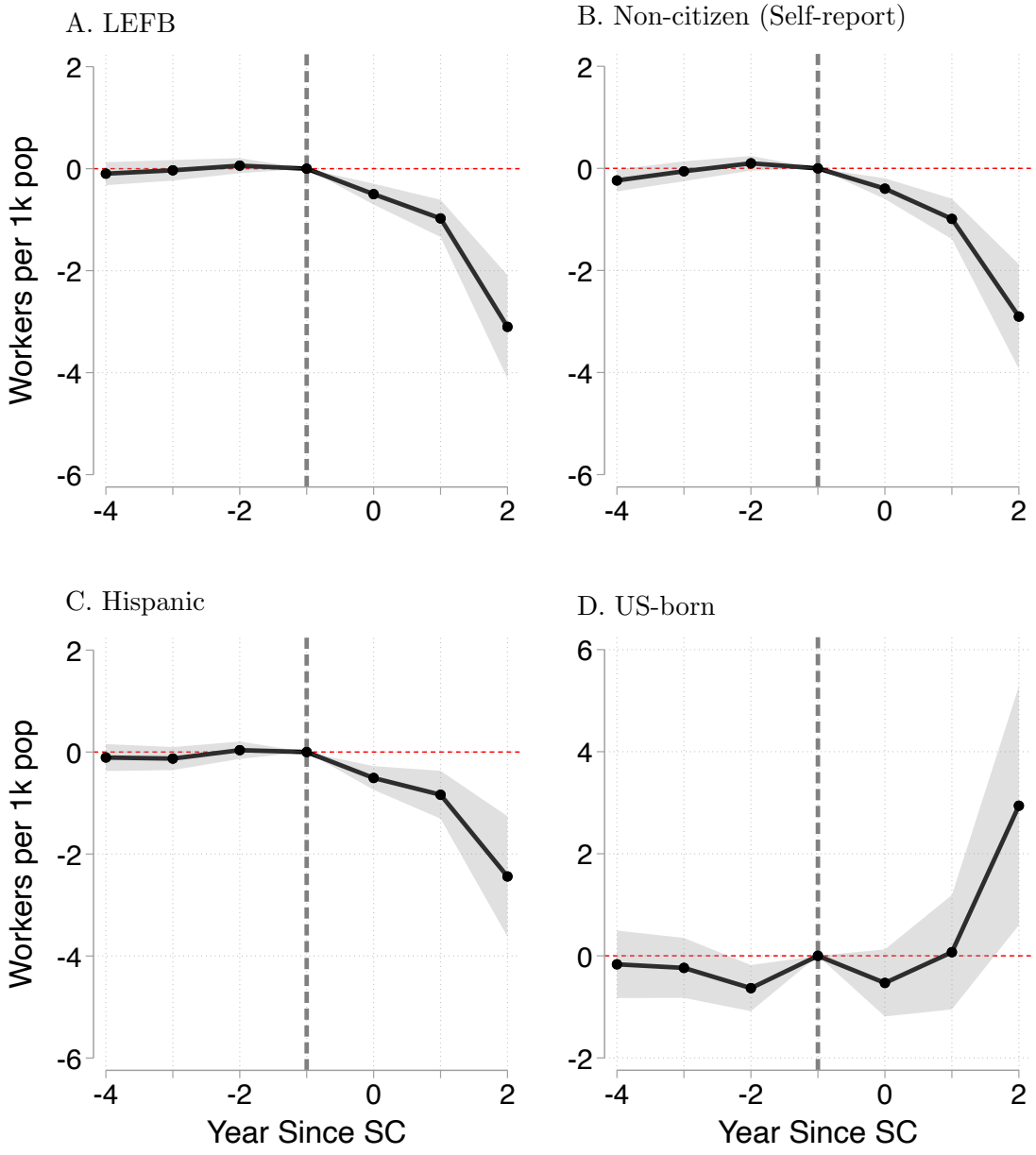

Note: This figure shows the effect of SC on new construction prices, estimated using the Borusyak et al. (2024) DiD estimator. Panel A uses the (log) transaction price without additional hedonic controls, and Panel B additionally controls tracts, square feet bins, age bins, number of bedrooms and number of bathrooms. Both estimations control for county, *HPA bin* × *Census Region* × *year* and *Population bin* × *Census Region* × *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors are clustered at the county level.

**Figure 6:** Event Study of Resale Price, Single-Family Homes




Note: This figure show event studies for the impact of SC on existing stock prices. Panel A uses raw transaction prices, and Panels B, C, and D additionally control tracts, square feet bins, age bins, number of bedrooms and number of bathrooms. Panels C and D explore heterogeneity by tract-level LEFB share: Panel C shows the event study for tracts with fewest LEFB residents, and Panel D shows the event study for tracts with the most LEFB residents. All estimations are based on the Borusyak et al. (2024) DiD estimator, and control for county, *HPA bin* $\times$ *Census Region* $\times$ *year* and *Population bin* $\times$ *Census Region* $\times$ *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors are clustered at the county level.

**Figure 7:** Construction Workforce Impact of Secure Communities




Note: This figure shows event studies for construction workforce impact of SC, estimated using the Borusyak et al. (2024) DiD estimator with the specification in equation (2). Panels A, B, and C focus on proxy groupings designed to capture a large share of undocumented construction workers. Panel A uses our preferred LEFB proxy, Panel B focuses on those reporting non-citizenship in ACS, and Panel C focuses on those reporting Hispanic ethnicity. Panel D focuses on US-born workers. All estimations control for county, *HPA bin* × *Census Region* × *year* and *Population bin* × *Census Region* × *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors are clustered at the county level.

**Table 1:** Summary Statistics

| | Mean | SD | Median | Obs. |
|---|---|---|---|---|
| **A: County-Year Panel** | | | | |
| New Construction, By Built Year, per 1k 2005 population | | | | |
| All Residential Units | 0.91 | 2.45 | 0.08 | 17,890 |
| Single Family Units | 0.72 | 2.04 | 0.05 | 17,890 |
| Non Single Family Units | 0.18 | 0.69 | 0.00 | 17,890 |
| All Residential, Square Feet | 1816.93 | 5136.64 | 142.82 | 17,890 |
| Single Family, Square Feet | 1494.36 | 4425.55 | 94.15 | 17,890 |
| Non Single Family, Square Feet | 322.57 | 1118.35 | 0.00 | 17,890 |
| Construction Workforce, per 1k 2005 population | | | | |
| All | 37.07 | 10.86 | 35.83 | 17,890 |
| Low-education, Foreign-born (LEFB) | 2.86 | 4.32 | 1.16 | 17,890 |
| Non-citizen | 2.56 | 4.12 | 0.87 | 17,890 |
| Hispanic | 3.95 | 6.21 | 1.44 | 17,890 |
| US-born | 33.77 | 10.51 | 33.07 | 17,890 |
| Homeowners, per 1k 2005 population | 731.24 | 84.71 | 732.68 | 17,890 |
| Homeownership Rate (p.p.) | 71.37 | 7.02 | 72.03 | 17,890 |
| **B: New Construction Transactions – Micro Data** | | | | |
| Market Price ($k) | 304.61 | 197.49 | 251.70 | 2,348,967 |
| Age at Sale | 0.41 | 0.92 | 0.00 | 2,348,967 |
| Square Feet | 2356.87 | 886.57 | 2180.00 | 2,348,967 |
| # Bedrooms | 3.52 | 0.81 | 3.00 | 1,769,701 |
| # Bathrooms | 2.88 | 0.85 | 3.00 | 2,087,617 |
| **C: House Resale Transactions – Micro Data** | | | | |
| Market Price ($k) | 305.80 | 291.16 | 223.50 | 11,511,937 |
| Age at Sale | 31.39 | 27.32 | 24.00 | 11,511,937 |
| Square Feet | 1942.07 | 870.75 | 1739.00 | 11,511,937 |
| # Bedrooms | 3.27 | 0.83 | 3.00 | 9,712,566 |
| # Bathrooms | 2.37 | 0.95 | 2.00 | 10,705,314 |
| **D: Construction Labor ACS Micro Data** | | | | |
| Hourly Wage | 15.79 | 8.06 | 14.42 | 916,093 |
| Hourly Wage, LEFB | 13.29 | 7.04 | 11.54 | 106,114 |
| Hourly Wage, US-born | 16.62 | 8.21 | 15.62 | 792,993 |

Note: This table shows summary statistics for county level construction outcomes (Panel A), new construction sales (Panel B), existing stock sales (Panel C), and wage data (Panel D). Housing market outcomes are drawn from CoreLogic. Section A of the Internet Appendix has additional detail on the data construction process. Workforce, homeownership, and wage data come from the American Community Survey (ACS) public use microdata.

**Table 2:** Is Secure Communities (SC) Rollout Predictable?

| | SC Activated | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Hispanic Share | 0.640*** | 0.635*** | 0.635*** | 0.637*** | 0.636*** | 0.772*** | 0.641*** |
| | (0.056) | (0.056) | (0.056) | (0.056) | (0.056) | (0.087) | (0.056) |
| Population (M) | | 0.023 | | | | | |
| | | (0.029) | | | | | |
| 3Yr Pop Growth | | | 0.111 | | | | |
| | | | (0.102) | | | | |
| 3Yr Pop Growth, Hispanic | | | | -0.002 | | | |
| | | | | (0.002) | | | |
| 3Yr Pop Growth, LEFB | | | | | -0.008** | | |
| | | | | | (0.004) | | |
| 3Yr New Construction Growth | | | | | | -0.002 | |
| | | | | | | (0.001) | |
| 01-07 Housing Price Growth | | | | | | | -0.058 |
| | | | | | | | (0.135) |
| HPA 00-07 bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Population bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| R-squared | 0.549 | 0.549 | 0.549 | 0.549 | 0.549 | 0.572 | 0.549 |
| Obs. | 7,393 | 7,393 | 7,391 | 7,388 | 7,389 | 4,654 | 7,393 |

Note: This table explores predictors of Secure Communities (SC) rollout. The dependent variable is a binary indicator for whether SC is activated in a given county. The dataset is stacked over rollout years 2009–2012. Each stack codes counties launching SC in that year as 1 and counties that have not yet launched SC as 0. Counties that have already launched are excluded from a given stack. The regressions control for *HPA bin* $\times$ *Census Region* $\times$ *stack year* and *Population bin* $\times$ *Census Region* $\times$ *stack year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors, reported in parentheses, are clustered at the county level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 3:** New Construction Entering Market

Panel A: Total Units

| | New Construction Units (per 1k 2005 population) | | | | | |
|---|---|---|---|---|---|---|
| | By Built Year | | | | | By Sale Year |
| | | | | Single Family | Non Single Family | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SC Activated | -0.540*** | -0.519*** | -0.165** | -0.192*** | 0.027 | -0.175** |
| | (0.067) | (0.066) | (0.068) | (0.059) | (0.025) | (0.069) |
| County | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year | ✓ | | | | | |
| Region*Year | | ✓ | | | | |
| HPA 00-07 bin*Region*Year | | | ✓ | ✓ | ✓ | ✓ |
| Population bin*Region*Year | | | ✓ | ✓ | ✓ | ✓ |
| Obs | 17,890 | 17,890 | 17,890 | 17,890 | 17,890 | 17,890 |

Panel B: Total Square Feet

| | New Construction Square Feet (per 1k 2005 population) | | | | | |
|---|---|---|---|---|---|---|
| | By Built Year | | | | | By Sale Year |
| | | | | Single Family | Non Single Family | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SC Activated | -1,095*** | -1,042*** | -363*** | -374*** | 11 | -382*** |
| | (135) | (140) | (137) | (122) | (38) | (138) |
| County | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year | ✓ | | | | | |
| Region*Year | | ✓ | | | | |
| HPA 00-07 bin*Region*Year | | | ✓ | ✓ | ✓ | ✓ |
| Population bin*Region*Year | | | ✓ | ✓ | ✓ | ✓ |
| Obs. | 17,890 | 17,890 | 17,890 | 17,890 | 17,890 | 17,890 |

Note: This table shows the impact of SC on new construction quantity in units (Panel A) and total square footage (Panel B), estimated using the Borusyak et al. (2024) DiD estimator with the specification in equation (1). All quantity measures are normalized by the 2005 baseline population (per 1,000 residents). Columns (1)–(5) measure new construction by year built, whereas Column (6) uses the year of sale. Columns (4) and (5) split new construction into single-family and non–single-family segments; the remaining columns consider all residential new construction. The preferred specifications in Columns (3)–(6) control for county, *HPA bin* $\times$ *Census Region* $\times$ *year* and *Population bin* $\times$ *Census Region* $\times$ *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors, reported in parentheses, are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 4:** House Price Response After SC Activation

Panel A: Price Response, Single-Family New Construction

| | log(Price) | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| SC Activated | 0.045*** | 0.048*** | 0.044*** |
| | (0.011) | (0.011) | (0.010) |
| County | ✓ | ✓ | ✓ |
| HPA 00-07 bin*Region*Year | ✓ | ✓ | ✓ |
| Population bin*Region*Year | ✓ | ✓ | ✓ |
| Tract | | ✓ | ✓ |
| Square Feet bin | | ✓ | ✓ |
| Age bin | | ✓ | ✓ |
| Bedroom # bin | | | ✓ |
| Bathroom # bin | | | ✓ |
| Obs | 2,348,967 | 2,348,967 | 2,348,967 |

Panel B: Resale Price Response, Single-Family Homes

| | log(Price) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| SC Activated | 0.013 | 0.004 | 0.001 | |
| | (0.010) | (0.010) | (0.011) | |
| (By Tract-level LEFB Share Quartiles) | | | | |
| SC Activated*Bottom Quartile | | | | 0.034*** |
| | | | | (0.009) |
| SC Activated*2nd Quartile | | | | 0.020** |
| | | | | (0.009) |
| SC Activated*3rd Quartile | | | | -0.010 |
| | | | | (0.014) |
| SC Activated*Top Quartile | | | | -0.041** |
| | | | | (0.020) |
| County | ✓ | ✓ | ✓ | ✓ |
| HPA 00-07 bin*Region*Year | ✓ | ✓ | ✓ | ✓ |
| Population bin*Region*Year | ✓ | ✓ | ✓ | ✓ |
| Tract | | ✓ | ✓ | ✓ |
| Square Feet bin | | ✓ | ✓ | ✓ |
| Age bin | | ✓ | ✓ | ✓ |
| Bedroom # bin | | | ✓ | ✓ |
| Bathroom # bin | | | ✓ | ✓ |
| Obs | 11,511,937 | 11,511,937 | 11,511,937 | 11,511,937 |

Note: This table shows the impact of SC on home prices, estimated using the Borusyak et al. (2024) DiD estimator. Panel A focuses on new construction, and Panel B focuses on existing stock. In Panel B, census tracts are apportioned to quartiles by share of residents indicating LEFB status in the ACS 5-year survey (2006–2010 vintage). All regressions control for county, *HPA bin* × *Census Region* × *year* and *Population bin* × *Census Region* × *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. The hedonic controls include census tract, square footage, house age, and the number of bedrooms and bathrooms, with additional description provided in Section A.4 of the Intenet Appendix. Standard errors, reported in parentheses, are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 5:** SC Effect on Homeownership

| | Homeowners | |
|---|---|---|
| | per 1k 2005 population | Share (in p.p.) |
| | (1) | (2) |
| SC Activated | -12.786*** | -0.931*** |
| | (2.959) | (0.153) |
| County | ✓ | ✓ |
| HPA 00-07 bin*Region*Year | ✓ | ✓ |
| Population bin*Region*Year | ✓ | ✓ |
| Obs. | 17,890 | 17,890 |

Note: This table shows how SC affects homeownership, estimated using the Borusyak et al. (2024) DiD estimator with the specification in equation (1). Column (1) presents estimates for the number of homeowners, normalized by the 2005 baseline population (per 1,000 residents). Column (2) presents estimates for the homeownership rate. Both regressions control for county, *HPA bin* $\times$ *Census Region* $\times$ *year* and *Population bin* $\times$ *Census Region* $\times$ *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors, reported in parentheses, are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 6:** Workforce Impact of SC Activation

Panel A: Overall Effect on Undocumented Workforce Across All Major Sectors

| Occupations | SC Activated | FEs: County | HPA 00-07 bin*Region*Year; | Population bin*Region*Year | Obs. | Share of All LEFB Labor (2005) |
|---|---|---|---|---|---|---|
| All | -1.323*** | ✓ | ✓ | ✓ | 17,890 | - |
| | (0.344) | | | | | |
| Construction | -0.772*** | ✓ | ✓ | ✓ | 17,890 | 12.2% |
| | (0.109) | | | | | |
| Repair | -0.310*** | ✓ | ✓ | ✓ | 17,890 | 2.9% |
| | (0.047) | | | | | |
| White Collar | -0.227** | ✓ | ✓ | ✓ | 17,890 | 15.7% |
| | (0.090) | | | | | |
| Cleaning/Maintenance | -0.134* | ✓ | ✓ | ✓ | 17,890 | 10.1% |
| | (0.076) | | | | | |
| Production | -0.128 | ✓ | ✓ | ✓ | 17,890 | 12.8% |
| | (0.136) | | | | | |
| Personal Care | -0.091* | ✓ | ✓ | ✓ | 17,890 | 3.8% |
| | (0.047) | | | | | |
| Food Service | -0.041 | ✓ | ✓ | ✓ | 17,890 | 8.3% |
| | (0.092) | | | | | |
| Transportation | -0.035 | ✓ | ✓ | ✓ | 17,890 | 8.0% |
| | (0.071) | | | | | |
| Extraction | 0.017 | ✓ | ✓ | ✓ | 17,890 | 0.1% |
| | (0.012) | | | | | |
| Miscellaneous | 0.056** | ✓ | ✓ | ✓ | 17,890 | 0.9% |
| | (0.023) | | | | | |
| Sale | 0.090 | ✓ | ✓ | ✓ | 17,890 | 6.6% |
| | (0.060) | | | | | |
| Agriculture | 0.281*** | ✓ | ✓ | ✓ | 17,890 | 2.8% |
| | (0.070) | | | | | |
| Missing Occupation | -0.031 | ✓ | ✓ | ✓ | 17,890 | 16.0% |
| | (0.154) | | | | | |

Panel B: Effect Within Construction

| | Construction Workforce (per 1k 2005 Population) | | | | |
|---|---|---|---|---|---|
| | LEFB | Alternative Proxies | | US-born | All |
| | | Non-citizen | Hispanic | | |
| | (1) | (2) | (3) | (4) | (5) |
| SC Activated | -0.772*** | -0.694*** | -0.702*** | -0.163 | -1.014*** |
| | (0.109) | (0.118) | (0.135) | (0.337) | (0.371) |
| County | ✓ | ✓ | ✓ | ✓ | ✓ |
| HPA 00-07 bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| Population bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| Obs | 17,890 | 17,890 | 17,890 | 17,890 | 17,890 |

Note: This table reports the impact of SC on the working age population, estimated using the Borusyak et al. (2024) DiD estimator with the specification in equation (1). Panel A presents the effects on undocumented workers, proxied by low-education, foreign-born (LEFB) working age individuals, across all major sectors. Panel B focuses on the construction sector and reports effects separately for undocumented workers in columns (1)–(3), US-born workers in column (4), and all workers in column (5). All workforce outcomes are normalized by the 2005 baseline population (per 1,000 residents). All regressions control for county, *HPA bin* $\times$ *Census Region* $\times$ *year* and *Population bin* $\times$ *Census Region* $\times$ *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors, reported in parentheses, are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 7:** Workforce Impacts Across Skill Groups in Construction

Panel A: Characteristics

| | Skill group characteristics, ACS 2005 | | |
|---|---|---|---|
| Group | US-born share | LEFB share | Average hourly wage ($) |
| Higher-skilled, Construction | 86.0% | 10.5% | 17.63 |
| Lower-skilled, Construction | 72.0% | 24.8% | 13.98 |
| All, Construction | 76.1% | 20.6% | 15.06 |

Panel B: Impact of SC Activation

| | Construction Workforce (per 1k 2005 population) | | | | | |
|---|---|---|---|---|---|---|
| | Higher-skilled | | | Lower-skilled | | |
| | LEFB | US-born | All | LEFB | US-born | All |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SC Activated | -0.283*** | -0.368*** | -0.692*** | -0.488*** | 0.205 | -0.322 |
| | (0.039) | (0.129) | (0.138) | (0.096) | (0.300) | (0.329) |
| County | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| HPA 00-07 bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Population bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Obs | 17,890 | 17,890 | 17,890 | 17,890 | 17,890 | 17,890 |

Note: Panel A contains workforce summary statistics by occupational skill. Panel B shows the effect of SC by skill for both undocumented and US-born workers, estimated using the Borusyak et al. (2024) DiD estimator with the specification in equation (1). Both panels restrict attention to working age individuals. The undocumented status is proxied by an low-education, foreign-born (LEFB) indicator. The skill groups are based on the US Department of Labor's Occupational Information Network (O*NET) classification. Occupations requiring higher levels of education, related experience, and job training are classified as higher-skilled—for example, construction trade supervisors, electricians, plumbers, inspectors, elevator installers, and hazardous-materials removers. The remaining occupations are classified as lower-skilled. A comprehensive list is provide in Table IA2 of the Internet Appendix. All workforce outcomes are normalized by the 2005 baseline population (per 1,000 residents). All regressions control for county, *HPA bin* $\times$ *Census Region* $\times$ *year* and *Population bin* $\times$ *Census Region* $\times$ *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors, reported in parentheses, are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 8:** Construction Wage Response After SC Activation

| | log(Hourly Wage) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SC Activated | 0.013 | | | 0.004 | | |
| | (0.010) | | | (0.009) | | |
| SC Activated*US-born | | -0.003 | | | -0.006 | |
| | | (0.011) | | | (0.010) | |
| SC Activated*US-born*Higher-skilled | | | -0.003 | | | -0.007 |
| | | | (0.011) | | | (0.011) |
| SC Activated*US-born*Lower-skilled | | | -0.001 | | | -0.001 |
| | | | (0.011) | | | (0.010) |
| SC Activated*LEFB | | 0.042*** | | | 0.022** | |
| | | (0.013) | | | (0.011) | |
| SC Activated*LEFB*Higher-skilled | | | 0.037*** | | | 0.016 |
| | | | (0.013) | | | (0.011) |
| SC Activated*LEFB*Lower-skilled | | | 0.082*** | | | 0.064*** |
| | | | (0.019) | | | (0.017) |
| County | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| HPA 00-07 bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Population bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Occupation*Labor Segment | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Age, $Age^2$ | | | | ✓ | ✓ | ✓ |
| Gender | | | | ✓ | ✓ | ✓ |
| Education | | | | ✓ | ✓ | ✓ |
| Race | | | | ✓ | ✓ | ✓ |
| Ethnicity | | | | ✓ | ✓ | ✓ |
| Obs | 916,093 | 916,093 | 916,093 | 916,093 | 916,093 | 916,093 |

Note: This table shows the effect of SC on hourly wage using ACS micro data, estimated with the Borusyak et al. (2024) DiD estimator. To mitigate potential misreporting, we winsorize wages at the 5% level. The low-education, foreign-born (LEFB) indicator is used as a proxy for undocumented status. The skill groups are based on the US Department of Labor's Occupational Information Network (O*NET) classification, with details provided in Table IA2 of the Internet Appendix. All regressions control for county, *HPA bin* × *Census Region* × *year*, *Population bin* × *Census Region* × *year* and Occupation × Labor segment (US-born, LEFB, other) fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors, reported in parentheses, are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

# Internet Appendix

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A.4 Construction of Hedonic Controls . . . . . IA – 4

**B Alternate Wage Measures for Construction** **IA – 4**

## A. Sample Construction: CoreLogic Microdata

All quantity results for new construction are based on 4.59 million records of new construction transactions, representing nearly the entire universe of residential constructions between 2005 and 2012. Price results for new construction are based on a subset of 2.12 million records that contain fully populated attribute data, allowing us to adequately adjust for observable quality. Resale house price results are based on 11.13 million records of resales with a full set of house characteristics. This section details each step of the data construction process.

### A.1 County-Level New Construction Quantity Data

We assemble data on all new residential construction completed in local markets from CoreLogic deeds data. We start with all transaction records flagged as new construction and with a built year between 2005 and 2012. Notably, this includes records with a sales year through 2022. We also permit the recorded sales year to be one year before the built year, to include preconstruction sales. We remove approximately 100k records that despite being flagged as new construction have a prior transaction associated with the same property PIN. Geographically, we keep only properties which we can match to a county launch date for Secure Communities. Practically, this means excluding US Territories and nine small counties with irregular governance structures. Finally, we drop properties with abnormal square footage.[1] In total, this yields 4.59 million records. This is the core dataset for all quantity results.

### A.2 New Construction Price Data

For tests of new construction prices, we impose five further filters. Figure A3 demonstrates the overview of the data construction process. First, we focus on single family construction (3.37 million). Second, to ensure an accurate signal about market value, we restrict attention to arm's-length transactions using general deeds, which removes 81k observations. We also remove 37k observations with a second transaction occurring within 60 days of the focal transaction, to avoid inadvertently including partial-interest transactions.

Our third filter concerns the housing attributes. As usual in real estate microdata, there is only partial coverage of structural characteristics. We focus on standard hedonic attributes in the real estate literature: square footage, age, bedrooms, bathrooms, and census tract. Property attributes

[1] We drop properties with square footage above 99,999 or below 0.

are contained in CoreLogic tax assessor files; each observation in the deeds data contains a unique PIN that permits straightforward linking to the assessor data. Because it is quite common for assessors to receive building information at a meaningful lag, we allow a look-ahead period of five years. That is: for a property transacting in year $t$, we take the first set of fully populated hedonic controls in any year between $t$ and $t + 5$. After this, for records missing attribute data, we match to MLS records and augment them with listing characteristics. This allows us to recover attributes for additional new construction observations. We drop transactions with abnormal values for sales price, square footage, or the number of bedrooms/bathrooms to ensure sample representativeness and avoid potential data errors. In addition, we retain only properties that sell within ten years of their year built: (i) beyond this horizon, transactions may no longer reflect "new construction" pricing, and (ii) as such delays are unusual, we are concerned about large property-specific unobservables. In total, these steps remove approximately 479k transactions.

Forth, because we include two sets of high-dimensional fixed effects, some cells are thinly populated, which creates technical challenges for the imputation-based DiD estimator. To ensure adequate support, we retain only population-bin×Region and HPA-bin×Region cells that have at least 5 transactions in each pre-SC year.[2] Applying this restriction removes approximately 234k transactions. Finally, another 190k transactions are automatically dropped by the DiD estimator in the most restrictive regression specification. For consistency and ease of comparison, we exclude these observations from all other specifications as well.

### A.3 Resale House Price Data

For tests of resale house prices, we collect 57.85 million residential resale transactions from 2005 to 2012, and apply the following five filters to get the core price regression data. Figure A4 demonstrates the overview of the data construction process. First, we focus on single family constructions (49.73 million), to be comparable with new construction price results. Second, to ensure an accurate signal about market value, we restrict attention to arms-length transactions using general deeds, which removes 21.65 million observations. We also remove 3.27 million observations with a second transaction occurring 60 days before or after the focal transaction, to avoid inadvertently including partial-interest transactions. Third, we remove "flip" sales, where property owners aggressively change house characteristics over a short period to increase home value. These

[2]This threshold is the minimum level required for the DiD estimator to run. For the regression on the number of bedrooms in Table IA1, column (4), we further increase the threshold to 10 transactions to ensure that the DiD estimator converges.

rapid changes can lead to less accurate house characteristic information in the assessment records. We designate a transaction as likely to be a flip sale if the seller purchased the property less than two years prior to the sale, resulting in the removal of 1.69 million observations. Fourth, we keep transactions with sales price greater than \$5M or less than \$100,000. We also remove 10.11 million transactions with abnormal characteristics: square footage less than 500 or greater than 10,000; or number of bedrooms greater than 10; or number of bathrooms greater than 10; or more than 200 years old. These attributes are gathered as described above in Section A.2.

The last two filters follow the same approach as for new-construction transactions. We retain only population-bin×Region and HPA-bin×Region cells that have at least 5 transactions in each pre-SC year, resulting in a reduction of 0.26 million observations. An additional 1.23 million transactions are automatically dropped in the most restrictive specification and are excluded from all others for consistency.

In total, we have 11.51 million transactions that form our core dataset for existing-stock price regressions.

### A.4 Construction of Hedonic Controls

We use property age to measure structural vintage. Properties aged between 0 and 10 years are grouped by their integer age. Those aged between 11 and 50 years are grouped into 5-year bins, those aged between 51 and 100 years into 10-year bins, and properties aged between 101 and 200 years are placed in a single category. We discretize square footage into the following bins: (500, 1000], (1000, 1500], (1500, 2000], (2000, 2500], (2500, 3000], (3000, 4000], (4000, 5000], (5000, 5500], and (7500,10000].

Regressions focused on price are restricted to transactions with non-missing information on property age, census tract, and square footage. We retain observations with missing values for the number of bedrooms and/or bathrooms. To avoid changing sample, in specifications that include these controls we include a fixed effect for missing-beds and/or missing-baths.

## B. Alternate Wage Measures for Construction

The wage analysis in Section 5.3 draws on ACS survey microdata. While survey data is appealing in some ways, there are also potential drawbacks. We compute hourly wage in ACS as follows:

$$hourly\ wage = \frac{total\ income}{weeks\ worked \times average\ hours\ worked\ per\ week} \tag{3}$$

Each of the three quantities in the equation above are reported in ACS. However, wages earned are reported as a continuous variable, while weeks worked are reported in fairly broad bins after 2008. We assign a number to each bin by taking the empirical median from the set of respondents for whom we observe *both* the continuous and the categorical value. These numbers are close to the midpoint for each bin, and our results are not sensitive to this choice. The consequence to observing a binned measure of weeks worked is 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. 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.

Potentially the best measure of wages in the construction sector comes from RSMeans, which provides a cost-estimation platform to the homebuilding industry. The RSMeans data is obtained via survey: for each locality and in each year, RSMeans contacts local unions to directly solicit journeyman wage rates for each of 21 different trades. That wage information is aggregated by the weight of that trade's usage within the industry and normalized to be a region-state-year index of total construction wage costs relative to the national average. The regions surveyed tend to be the largest 10–15 urban areas in a given state. We match RSMeans cities to counties to produce an index that covers a total 690 counties. When multiple cities match one county, we take a simple average of the RSMeans index across all matching cities.

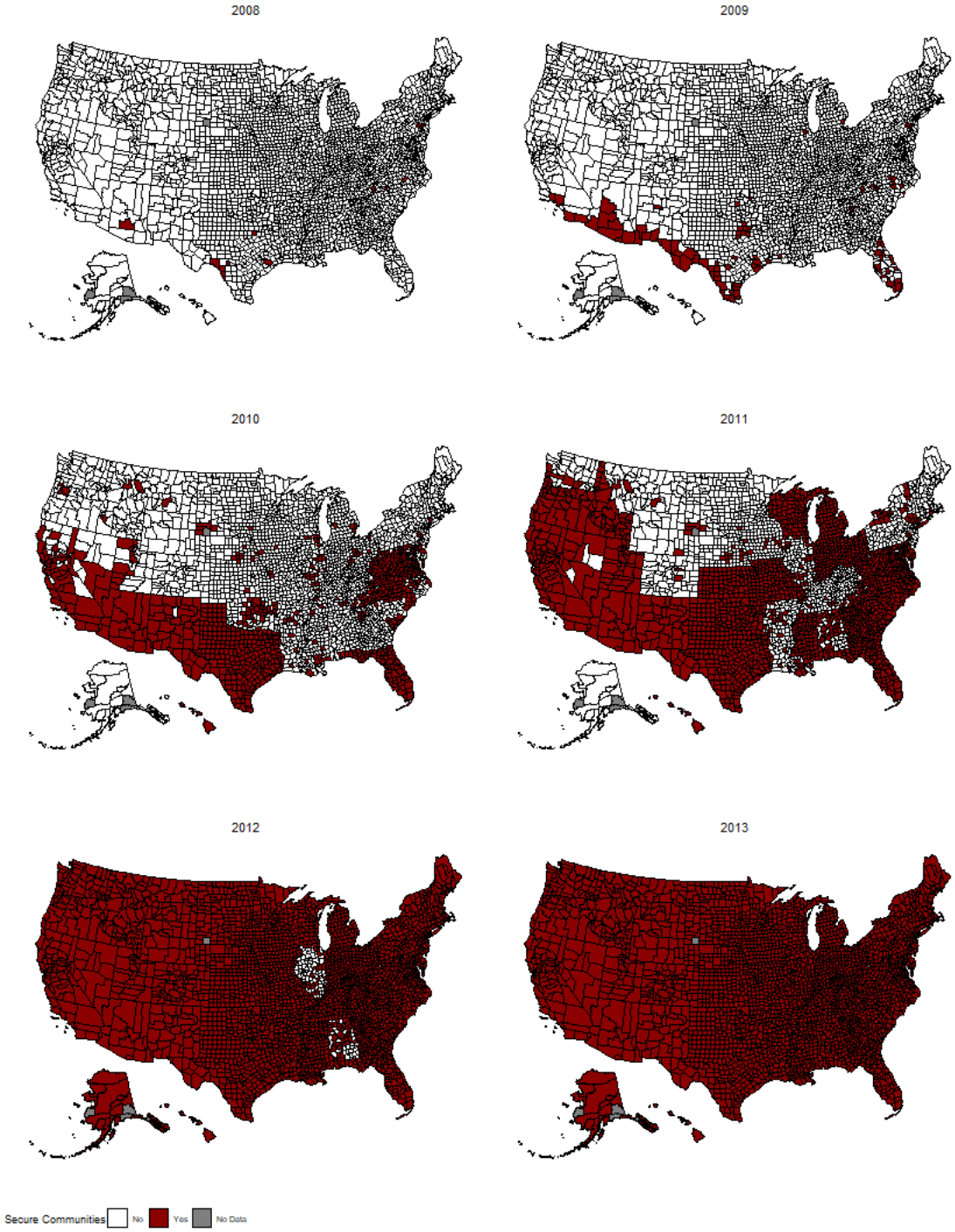
This data does have meaningful limitations. One is that the series begins in 2007. In our estimations, we assign the 2007 figures to 2005 and 2006. Another limitation is geographic: RSMeans-surveyed cites correspond to a geographic footprint of just under 700 counties, representing 70% of the country by population. A third limitation is that RSMeans primarily surveys union rates, and non-union labor can often be more important for SFH building.

We also use industry-level series from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages. These series are produced primarily from employer disclosures made to state systems for purposes of tracking workers covered by unemployment insurance systems. As a result, these series may also be flawed if employers misreport compensation for any reason, which is

not impossible in a setting with a large share of undocumented workers, and norms that sometimes lead to informal cash payments.

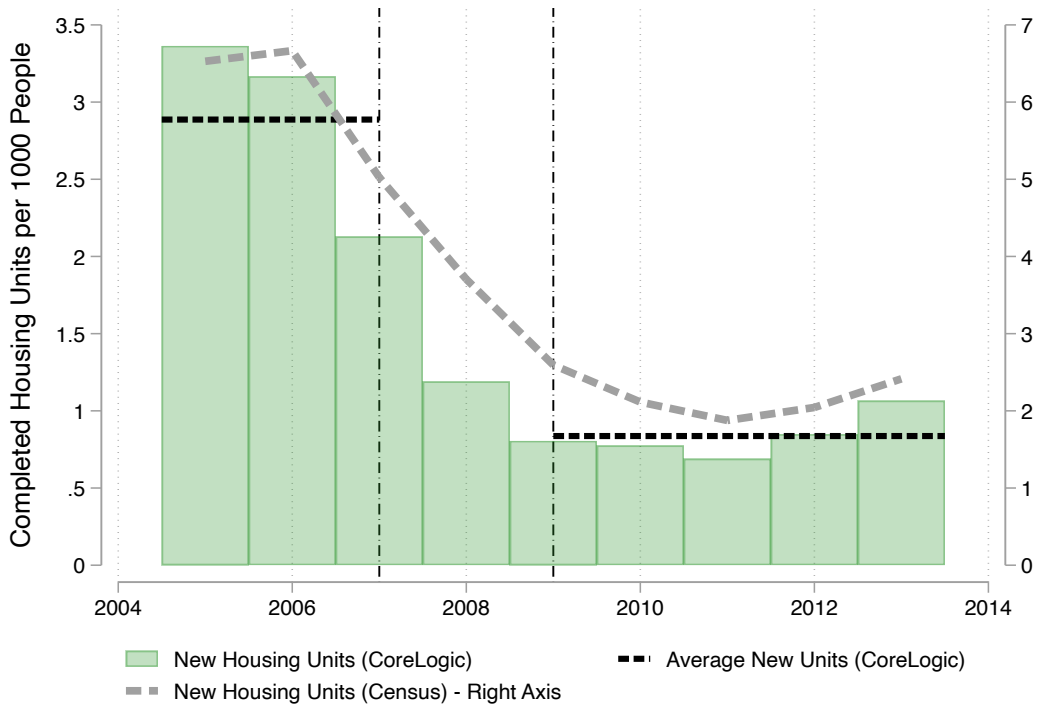
Table IA3 shows the results for the set of counties covered by RSMeans. Using all measures, we find statistically insignificant effects on average wages. In QCEW and ACS, the point estimate suggests a 1% reduction in construction wages. The RSMeans measure also suggests a decline of .64 in the cost index; relative to the pre-policy mean of 83, this is a decline of 77bps. Together with Table 8 in our paper's body, this evidence suggests that SC has little observable impact on county-level average wages, however this obscures shifts that are evident in one sub-population of interest: lower-skilled LEFB workers.

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



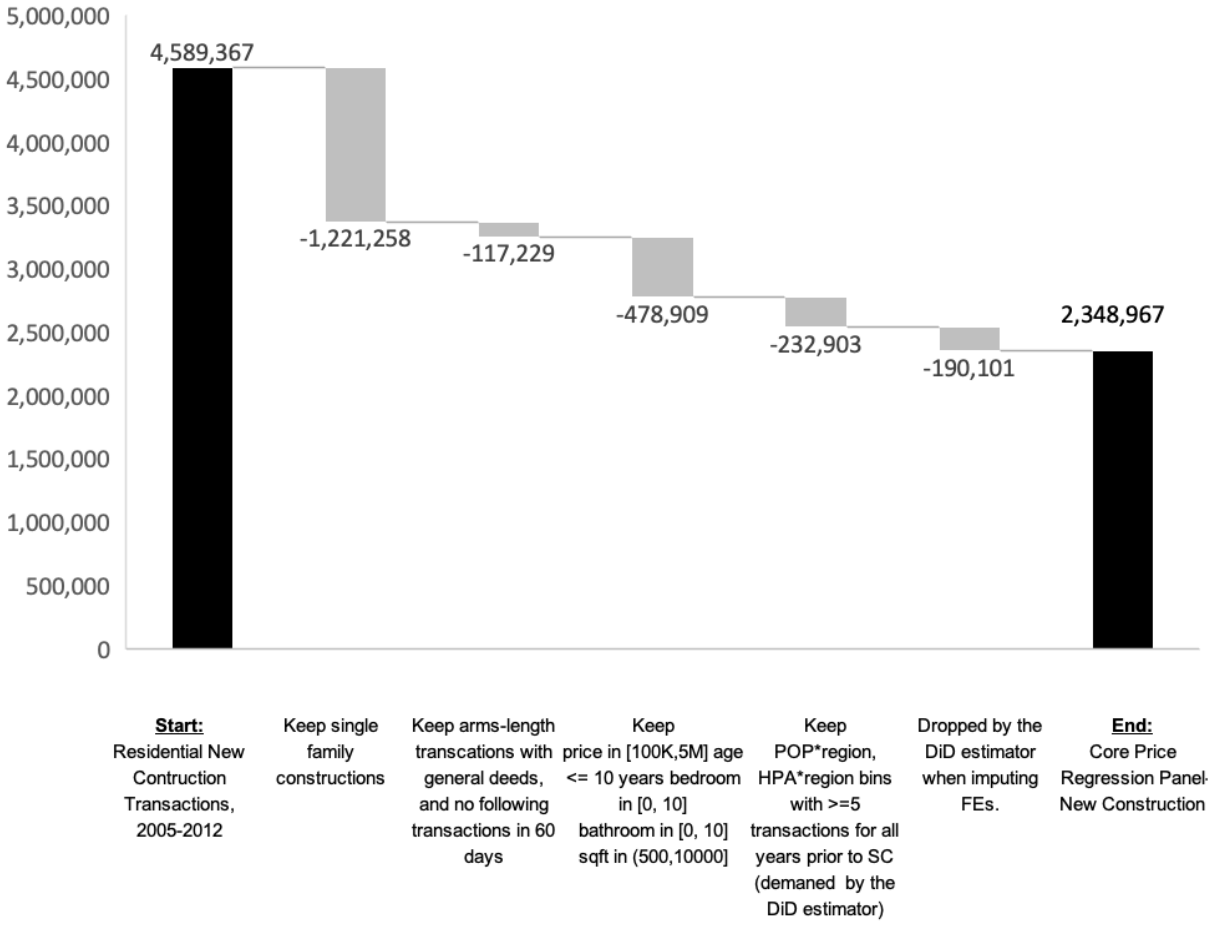

Note: Each panel of this figure shows the counties that implement SC within each year. This map reflects treatment based on exact date of implementation: a county is coded as treated in year $t$ if the launch date falls at any point within year $t$. All regressions in this paper assign annual treatment 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 the paper's Figure 2.

**Figure A2:** New Construction Series: CoreLogic vs Census



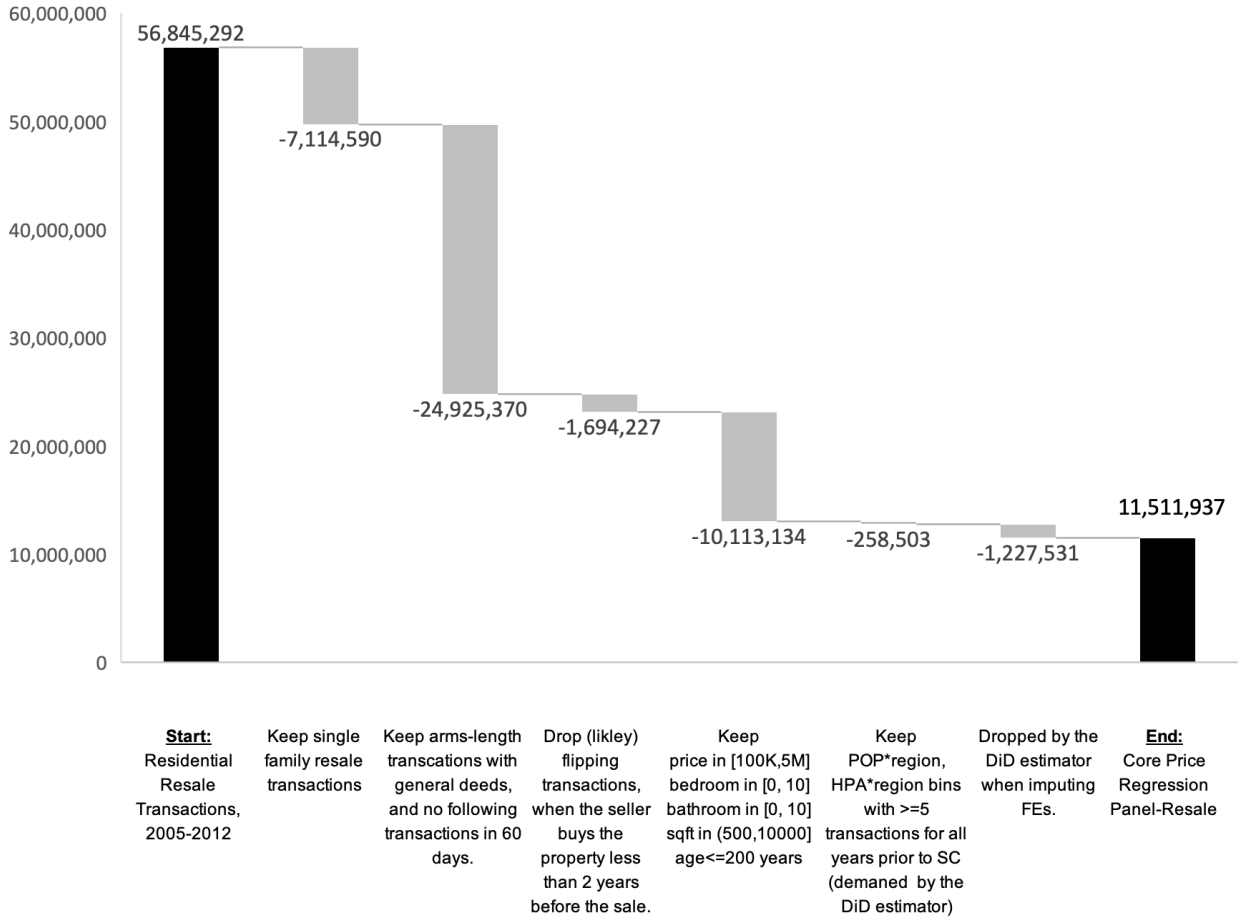

Note: This figure compares Census data on new housing units with CoreLogic data on new housing, and shows that the dynamic path of the two series aligns well. There is a level difference: CoreLogic appears to capture a constant share of the Census total. The green bars reflect data from CoreLogic, and correspond to the left axis. The Census series is shown in the dashed grey line; this corresponds to the right axis.

**Figure A3:** Overview of Price Panel Construction (New Construction)




Note: This figure provides an overview of each step in extracting price data for new construction transactions. Additional detail on each step is available in Section A.2.

**Figure A4:** Overview of Price Panel Construction (Existing Stock)




Note: This figure provides an overview of each step in extracting price data for resale transactions. Additional detail on each step is available in Section A.3.

**Table IA1:** Change of New Construction Characteristics After SC Activation

| | Age | Sold after Built Year (Age≥1) | log(Square Feet) | Bedrooms | Bathrooms |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| SC Activated | -0.073*** | -0.040*** | -0.014 | 0.013 | 0.024 |
| | (0.025) | (0.013) | (0.010) | (0.027) | (0.024) |
| County | ✓ | ✓ | ✓ | ✓ | ✓ |
| POP bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| HPA 00-07 bin*Region*Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| Tract | ✓ | ✓ | ✓ | ✓ | ✓ |
| Obs | 2,348,967 | 2,348,967 | 2,348,967 | 1,714,659 | 2,087,001 |

Note: This table estimates how SC impacts time-to-sale and property attributes of newly constructed properties, estimated using the Borusyak et al. (2024) DiD estimator. Column (1) uses the (integer) difference between sale-year and built-year as the dependent variable. Column (2) considers the share of homes which are sold at least 1 year after they are built. Columns (3)–(5) consider square footage, number of bedrooms, and number of bathrooms respectively. Columns (4)–(5) exclude observations missing reported figures for bedrooms or bathrooms. All regressions control for county, census tract, *HPA bin* $\times$ *Census Region* $\times$ *year* and *Population bin* $\times$ *Census Region* $\times$ *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors, reported in parentheses, are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table IA2:** Skill groups

| Skill group | Code | ACS Occupation | O*NET job zone | US-born share | LEFB share |
|---|---|---|---|---|---|
| High | 6200 | First-Line Supervisors of Construction Trades and Extraction Workers | 3 | 87.6% | 8.9% |
| | 6350 | Electricians | 3 | 88.3% | 7.3% |
| | 6440 | Pipelayers, Plumbers, Pipefitters, and Steamfitters | 3 | 83.7% | 13.7% |
| | 6660 | Construction and Building Inspectors | 3 | 92.1% | 2.2% |
| | 6700 | Elevator Installers and Repairers | 3 | 93.7% | 2.3% |
| | 6720 | Hazardous Materials Removal Workers | 3 | 77.3% | 16.6% |
| Low | 6210 | Boilermakers | 2 | 93.6% | 4.3% |
| | 6230 | Carpenters | 2 | 75.6% | 20.8% |
| | 6310 | Pile-Driver Operators | 2 | - | - |
| | 6320 | Construction Equipment Operators (general) | 2 | 90.9% | 7.9% |
| | 6500 | Reinforcing Iron and Rebar Workers | 2 | 73.8% | 25.6% |
| | 6520 | Sheet Metal Workers | 2 | 88.2% | 9.5% |
| | 6530 | Structural Iron and Steel Workers | 2 | 89.1% | 8.7% |
| | 6740 | Rail-Track Laying and Maintenance Equipment Operators | 2 | 89.5% | 7.3% |
| | 6220 | Brickmasons, Blockmasons, and Stonemasons | 2 | 68.0% | 29.3% |
| | 6240 | Carpet, Floor, and Tile Installers and Finishers | 2 | 65.8% | 29.4% |
| | 6250 | Cement Masons, Concrete Finishers, and Terrazzo Workers | 2 | 67.6% | 31.5% |
| | 6260 | Construction Laborers | 2 | 67.1% | 29.8% |
| | 6300 | Paving, Surfacing, and Tamping Equipment Operators | 2 | 81.1% | 18.6% |
| | 6330 | Drywall Installers, Ceiling Tile Installers, and Tapers | 2 | 54.9% | 42.8% |
| | 6360 | Glaziers | 2 | 83.8% | 14.1% |
| | 6400 | Insulation Workers | 2 | 70.4% | 24.5% |
| | 6420 | Painters, Construction and Maintenance | 2 | 64.7% | 30.6% |
| | 6430 | Paperhangers | 2 | 93.1% | 3.0% |
| | 6460 | Plasterers and Stucco Masons | 2 | 49.9% | 47.8% |
| | 6510 | Roofers | 2 | 65.1% | 33.0% |
| | 6600 | Helpers, Construction Trades | 2 | 63.3% | 33.4% |
| | 6710 | Fence Erectors | 1 | 77.1% | 21.3% |
| | 6730 | Highway Maintenance Workers | 1 | 93.0% | 5.7% |
| | 6750 | Septic Tank Servicers and Sewer Pipe Cleaners | 1 | - | - |
| | 6760 | Miscellaneous Construction and Related Workers | - | 81.4% | 16.1% |

Note: This table shows how construction occupations are grouped into skill categories, using the US Department of Labor's Occupational Information Network (O*NET) classification. For each occupation, we also report the LEFB share and the US-born share (these are not a full partition, and do not add to 100%).

**Table IA3:** Construction Wage Response After SC Activation: Alterative Wage Measures

| | Cost Index (Mean: 81) | log(Hourly Wage) | |
|---|---|---|---|
| | RS Means | ACS | QCEW |
| | (1) | (2) | (3) |
| SC Activated | -0.64 | -0.01 | -0.01 |
| | (0.44) | (0.01) | (0.01) |
| County | ✓ | ✓ | ✓ |
| POP bin*Region*Year | ✓ | ✓ | ✓ |
| HPA 00-07 bin*Region*Year | ✓ | ✓ | ✓ |
| Obs | 4,550 | 4,550 | 4,285 |

Note: This table shows the effect of SC on overall construction wages at the county-year level using several different measures. The estimations use the Borusyak et al. (2024) DiD estimator with the specification in equation (1). Column (1) uses a regional cost index from RSMeans. Column (2) uses ACS data aggregated to county-year. Column (3) uses a series from the Bureau of Labor Statistics' QCEW. All regressions control for county, *HPA bin* $\times$ *Census Region* $\times$ *year* and *Population bin* $\times$ *Census Region* $\times$ *year* fixed effects. *HPA* bins are defined as 20 quantiles based on county house price appreciation between 2000 and 2007, and *Population* bins are defined as 20 quantiles based on county population in the base year 2005. Standard errors, reported in parentheses, are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.