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When labor enforcement and immigration enforcement collide: Deterring worker complaints worsens workplace safety

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When Labor Enforcement and Immigration Enforcement Collide: Deterring Worker Complaints Worsens Workplace Safety

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ABSTRACT

Regulatory agencies overseeing the labor market often rely on worker complaints to direct their enforcement. However, if workers face differential barriers to complain, this system could result in ineffective targeting and create disparities in working conditions. To investigate these implications, we examine how the onset of Secure Communities—a localized immigration enforcement program—affected occupational safety and health. Counties’ participation in Secure Communities substantially reduced complaints to government safety regulators, but increased injuries, at workplaces with Hispanic workers. We show that these effects are most consistent with employers reducing safety inputs in response to workers’ decreased willingness to complain.

JEL Classification Codes: J28, J81, I18

Key Words: labor regulations; workplace safety; immigration enforcement

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1 Introduction

Regulatory agencies overseeing the labor market rely heavily on worker complaints to target their enforcement resources. Worker complaints triggered 20 percent of inspections conducted by the Occupational Safety and Health Administration (OSHA) between 2006 and 2016, over 75 percent percent of inspections conducted by the Wage and Hour Division of the Department of Labor in recent years (Weil and Pyles, 2005), and 100 percent of enforcement of workplace discrimination laws conducted by the Equal Employment Opportunity Commission.¹ If workers have better information about violations than government agencies, worker complaints can enable regulators to direct their scarce enforcement to workplaces where noncompliance is most severe.²

A concern with this approach to regulation is that fear of employer retaliation, reluctance to interact with government, and other barriers might make it costly for workers to complain. If these barriers are higher for workers who are more at risk of violations, worker complaints will not direct inspections where working conditions are most problematic. Furthermore, if employers recognize that they face lower expected enforcement costs when their workers are less likely to complain, barriers to complaining might directly affect employers' inputs into compliance and thus the conditions that workers face. At the same time, because labor market competition and other public policies exert their own discipline on employers, it is not clear whether this effect of workers' willingness to complain on working conditions would be economically meaningful.

We examine how one potentially salient barrier to complain—immigration enforcement—affects the provision of occupational safety and health. We motivate our analysis with descriptive evidence that Hispanic workers³ generally face higher barriers to complain about hazardous working conditions. We then examine the causal effect of Secure Communities—a local immigration enforcement policy that likely raised Hispanic workers' cost of complaining—on worker complaints and the occurrence of work-related injuries.

Our analysis offers a compelling case study to examine the extent to which barriers affect both workers' likelihood to complain to the government and employers' provision of working conditions. Occupational injuries and illnesses are pervasive and costly. There were over 3 million work-related injuries and illnesses in 2019 (U.S. Bureau of Labor Statistics, 2020). Occupational injuries and illnesses cause direct costs, like medical care, and indirect costs, like lost productivity, of over \$250 billion each year (Leigh, 2011). Hispanic workers are more likely than white non-Hispanic workers to suffer work-related injuries (Dong et al., 2010); however, language barriers, lack of knowledge about labor laws, and other factors presum-

¹See the EEOC website: <https://www.eeoc.gov/youth/about-eeoc-2>.

²One illustrative example of how scarce regulators' enforcement is that OSHA inspects less than 1 percent of the workplaces it oversees each year. See: <https://www.osha.gov/data/commonstats>.

³An American Hispanic/Latina/Latino/LatinX individual must contend with their identity through a variety of lenses. We acknowledge that a single term cannot capture this diversity. We use the term “Hispanic” for consistency with our data sources throughout the paper. It corresponds to the Hispanic origin variable in the American Community Survey (ACS), our main data source for worker demographics. In the ACS, “Hispanic/Spanish/Latino origin” refers to individuals of Cuban, Mexican, Puerto Rican, South or Central American, or other Spanish culture or origin, including Spaniards, regardless of race.

ably make Hispanic workers relatively more reluctant to complain (Rathod, 2010). Stronger immigration enforcement increases the cost of complaining for undocumented workers because any interaction with a government agency may trigger their deportation. Because Hispanic individuals are more likely to live and work with undocumented immigrants than other groups, immigration enforcement might also decrease Hispanic workers' willingness to complain, regardless of their immigration status (Hall et al., 2019; Andersson et al., 2014). Given the massive scale of resources spent on immigration enforcement, the resulting effects on workplace safety could be economically meaningful. The United States' annual spending on immigration enforcement has exceeded \$20 billion in recent years (Muzaffar Chishti and Jessica Bolter, 2020) and has led to the forced removal of over 200,000 individuals each year since 2006 (Transactional Records Access Clearinghouse, 2016).

We combine data on worker complaints to OSHA, OSHA inspections, workplace demographics, and immigration enforcement. We use data on all worker complaints submitted to OSHA, obtained through a Freedom of Information Act (FOIA) request, as well as data on OSHA inspections triggered by a worker complaint and inspections triggered by a serious workplace injury, to create annual series of worker complaints and workplace injuries at the county-industry level. Since there are no establishment-level data on workplace demographics, we leverage variation in the share of workers who are Hispanic across counties in a given industry, which we obtain from the American Community Survey (ACS).⁴ We measure the strength of local immigration enforcement using counties' participation in Secure Communities, a federal program in which local agencies automatically shared fingerprints of individuals arrested for nonimmigration matters with Immigration and Customs Enforcement (ICE). We use triple-difference regressions that compare the rates of worker complaints and workplace injuries between workplaces with different shares of Hispanic workers and across counties, industries, and years. Our identifying variation comes from differences in the timing of counties' participation in Secure Communities and from differences in the Hispanic workforce share across industries and counties.

We find that counties' participation in Secure Communities substantially reduced worker complaints in workplaces with a high share of Hispanic workers. One way to convey the magnitude of the effect is to consider how the activation of Secure Communities affected the difference in complaint rates between county-industries that employ 0 percent vs. 100 percent Hispanic workers. Our baseline estimates imply that Secure Communities reduced complaints by 40–50 percent in county-industries with an entirely Hispanic workforce, relative to county-industries with no Hispanic workers. Event study estimates reveal that this effect on complaints showed up immediately in the year Secure Communities began, persisted for several years, and increased over time. In contrast, we find that Secure Communities led to a substantial *increase* in injuries among workplaces with a high share of Hispanic workers. We estimate that injuries at workplaces with an entirely Hispanic workforce increased by roughly 24 percent relative to workplaces with no Hispanic workers. Our estimates are robust to accounting for measurement error, using alternative measures of exposure to Secure Communities, addressing the selection into Secure Communities, using alternative re-

⁴More precisely, we obtain these shares for each public use microdata area (PUMA) and map these to counties using a crosswalk. See Section 3.3 for details.

gression specifications, controlling for potential confounding factors, and the potential for bias arising from time-varying treatment effects.

We provide evidence that the effects we estimate are consistent with immigration enforcement a) deterring Hispanic workers from complaining to government agencies and b) leading employers to spend fewer resources reducing workplace hazards. First, we show that Secure Communities led to more violations of workplace safety and health regulations among workplaces with more Hispanic workers. Second, we show that immigration enforcement does not significantly affect complaints and injuries at workplaces where workers are represented by a labor union. Unions help workers to anonymously file complaints and often include contractual provisions that employers protect their workers from immigration agents. Unionized Hispanic workers might thus be less fearful of complaining in the face of immigration enforcement. Third, we use the Current Population Survey to show that Secure Communities led to higher rates of minimum wage violations among Hispanic workers. Because the Wage and Hour Division (WHD) also heavily relies on worker complaints to target its enforcement of minimum wage laws, these results corroborate and generalize our findings on the effect of immigration enforcement on workplace safety.

Assuming that Secure Communities only increased injury rates by reducing worker complaints, our estimates imply an elasticity of Hispanic workers' injury risk with respect to their probability of complaining of roughly 0.47. We explore why workers' risk of injury responds so strongly to changes in their propensity to complain through the lens of the "exit" versus "voice" paradigm of [Hirschman \(1970\)](#). Using data from the Quarterly Workforce Indicators (QWI), we show that Secure Communities had a negligible effect on Hispanic workers' job separation rate and overall job turnover. This impotence of Hispanic workers' "exit" option might explain why a change in their willingness to complain (the "voice" option) had a particularly large effect on the quality of their working conditions, in line with predictions from [Hirschman \(1970\)](#).

Our paper advances the literature in several ways. First, we contribute to a growing literature on the economic effects of immigration enforcement. Using similar measures of immigration enforcement, [Dhingra et al. \(2021\)](#) find that ICE cooperation with local law enforcement decreased crime reporting by Hispanic communities without reducing crime commission or victimization. [Grittner \(2019\)](#) shows that Secure Communities reduced help-seeking of Hispanic domestic violence victims. Other studies show that stronger immigration enforcement reduced the use of social safety net programs like food stamps and subsidized health care, led to a decline in prenatal health care visits, school attendance, and progression and completion, and increased food insecurity among Hispanic families ([Alsan and Yang, 2018](#); [Rhodes et al., 2015](#); [Dee and Murphy, 2018](#); [Amuedo-Dorantes and Lopez, 2015, 2017](#); [Potochnick et al., 2017](#)). [East et al. \(2012\)](#) find that Secure Communities reduced employment rates of both likely undocumented and U.S. citizens.

Second, we complement the literature on the effects of labor market regulations and inspections on working conditions ([Viscusi, 1986](#); [Weil, 1996](#); [Levine et al., 2012](#); [Li and Singleton, 2019](#); [Galvin, 2016](#); [Johnson, 2020](#)). Most notably, we build on [Weil and Pyles \(2005\)](#), who provide a framework for why workers might be reluctant to exercise their statutory rights to complain. We demonstrate the importance of regulatory enforcement for working conditions and highlight an important shortcoming of the current en-

forcement system. Our results suggest that regulatory agencies' reliance on worker complaints undermines the agencies' mission and exacerbates inequality in the labor market. [Weil \(2014\)](#) shows that the rise of domestic outsourcing has similar implications.

Third, our study offers an indirect but potentially important test of the exit and voice paradigm of [Hirschman \(1970\)](#). Hirschman's work illustrated the conditions under which voice is a potent tool for workers to improve their working conditions. Empirical tests of this thesis have proved to be challenging. Our finding that workers' risk of injury is highly responsive to their propensity to complain confirms [Hirschman's \(1970\)](#) prediction that voice drives working conditions when the exit option is less available. This result complements recent work that empirically tests other aspects of the exit and voice paradigm. For example, [Adhvaryu et al. \(2019\)](#) and [Harju et al. \(2021\)](#) test whether greater opportunity for voice reduces worker turnover. Finally, we complement studies that investigate the determinants and effects of complaining in other domains, including worker complaints about sexual harassment ([Dahl and Knepper, 2021](#)), citizen complaints about police misconduct ([Ba, 2018](#)) and whistle-blowing about Medicare fraud ([Leder-Luis, 2021](#)).

2 Institutional Background and Conceptual Framework

Workers have a right to complain about a broad range of working conditions to government agencies, but some workers might face barriers workers in exercising these rights. These barriers might be particularly high for Hispanic workers, especially in an era of enhanced immigration enforcement. Finally, the barriers that workers face to complain might directly affect employers' compliance with labor regulations and workplace safety.

Workers have a legal right to complain to agencies in several regulatory domains of the labor market. Most relevant to this paper, the 1970 Occupational Safety and Health Act gives workers the right to complain about occupational safety and health issues to the U.S. Occupational Safety and Health Administration (OSHA), the federal regulatory agency charged with ensuring "safe and healthful working conditions" in the United States ([OSHA, 2020](#)). Most private-sector employers must comply with hundreds of OSHA standards that range from the maintenance of specific capital equipment to more general restrictions on exposing workers to hazards. Workers can complain to OSHA electronically, by mail, by phone, or in person if they feel they are being exposed to hazards or believe their employer is violating OSHA standards. Twenty-eight states and Washington, D.C., are under OSHA's jurisdiction. The remaining 22 states have their own OSHA-approved state-run occupational safety and health plan that operates with similar standards and rights to complain. Workers also have the right to complain in domains outside of safety and health, including wage and hour standards according to the 1938 Fair Labor Standards Act.

A worker complaint to OSHA, with some conditions that will be described in [Section 3.1](#), automatically triggers an OSHA inspection at the workplace with the alleged hazard. During inspections, inspectors review

paperwork and tour a facility’s operations to assess its hazards and compliance with standards. Inspectors issue citations for each violation of a standard that they observe. Inspections can also occur for reasons other than a worker complaint. Certain types of injuries (described in detail in [Section 3.1](#)) automatically trigger an OSHA inspection. Other inspections are “programmed,” in which OSHA targets facilities in a particular industry or that possess a specific hazard, pursuant to national or regional priorities.⁵ Even with all these potential triggers, OSHA inspections are relatively rare. OSHA inspects less than 1 percent of workplaces each year ([U.S. Occupational Safety and Health Administration, 2021](#)).

Complaining might produce an array of benefits for workers.⁶ Complaints can trigger inspections, which tend to improve working conditions ([Levine et al., 2012](#); [Haviland et al., 2012](#); [Li and Singleton, 2019](#)). As a result, complaining can meaningfully improve one’s working conditions in expectation, especially given how unlikely an inspection is otherwise. The benefit of complaining is thus higher the more job hazards that a worker faces. More generally, complaining is a form of worker “voice,” which can enable workers to elicit changes in working conditions when the “exit” option of leaving their job is not available or not preferred ([Hirschman, 1970](#)).

Despite these benefits, workers might not exercise their right to complain, even when they face hazardous or illegal conditions. Complaining can entail explicit costs. One particularly salient cost is employer retaliation: while technically illegal, OSHA receives thousands of reports each year alleging retaliation (e.g., firing, reducing pay) against workers who complain or serve as whistle-blowers ([Weatherford, 2013](#)).⁷ Apart from strict benefits and costs, other barriers might attenuate workers’ likelihood to complain. Many workers do not know their rights to complain ([Alexander and Prasad, 2014](#)). Furthermore, an individual worker might not complain because they do not internalize the full benefit of doing so. If a complaint by one worker leads to improved working conditions for all workers at the establishment, then the “public good” of complaints will be underprovided ([Weil and Pyles, 2005](#)).

These costs and other barriers to complaining are particularly pronounced for Hispanic workers, especially those who are immigrants. Immigrant workers may not be aware of their rights, may not know how to formally complain, and may face language barriers ([Mehrotra et al., 2018](#); [Rathod, 2010](#)). Complaining is especially costly for undocumented immigrant workers. The risk of employer retaliation is more costly, as undocumented immigrants have fewer employment options and lack access to the social safety net;⁸ furthermore, these individuals might fear that any government interaction—including complaining to

⁵A small share of OSHA inspections are triggered by other reasons, including a “referral”—an allegation of hazards made by an inspector, government agency or media—or as a “follow-up” to a prior inspection.

⁶This framework of the benefits and costs of complaining draws inspiration from [Weil and Pyles \(2005\)](#).

⁷Technically, the 1970 Occupational Safety and Health Act protects workers against employer retaliation for complaining to OSHA and charges OSHA with investigating and enforcing cases of alleged retaliation. However, workers face exceedingly high barriers to taking advantage of these protections, OSHA is limited in the damages it can actually pursue on a claimant’s behalf, and OSHA only files lawsuits for a tiny fraction of thousands of reports of retaliation it receives each year ([Weatherford, 2013](#)). OSHA’s own inspectors do not believe that the legal protections offered by the 1970 act are effective ([Government Accountability Office, 1990](#)).

⁸Institutional factors further compound the differential benefits to complaining for undocumented immigrants. The U.S. Supreme Court’s 2002 decision *Hoffman Plastic Compounds v NLRB* established that undocumented workers do not have the same remedies (such as backpay or reemployment) as documented workers if their employers engage in unfair labor practices

regulatory agencies—raises the risk of deportation.⁹ These barriers and costs could be present for Hispanic individuals, regardless of their own immigrant status. Hispanic individuals are more likely to live with an undocumented immigrant than U.S.-born white or Black/African American individuals (Hall et al., 2019), and they are more likely to work with other Hispanic immigrants than are other demographic groups (Andersson et al., 2014). As a result, Hispanic individuals tend to worry more that any government interaction will increase the chance that they or someone in their community may be deported (Maslin and Sonenshein, 2017).

Because increased immigration enforcement raises the risk of deportation, it further compounds the costs of complaining for undocumented immigrants and Hispanic workers more broadly. Indeed, immigration enforcement has been shown to have a similar “chilling effect” on other behaviors, such as reducing the take-up of social insurance, school attendance, and crime reporting of Hispanic individuals, regardless of their own immigrant status (Alsan and Yang, 2018; Dee and Murphy, 2018; Comino et al., 2016; Rugh and Hall, 2016; Dhingra et al., 2021; Grittner, 2019).

For employers, their workers’ threat of complaining directly affects their expected costs of workplace injuries. All else equal, a lower threat of worker complaint reduces the probability that OSHA will inspect the employer, which reduces the employer’s expected value of future OSHA fines, costs of remediation, and any associated negative publicity (Johnson, 2020). Because stronger immigration enforcement reduces Hispanic workers’ willingness to complain, it effectively lowers the cost of violating safety regulations for employers of Hispanic workers. In consequence, stronger immigration enforcement would make employers less motivated to make costly safety investments, leading to more hazardous working conditions in workplaces with Hispanic workers (Becker, 1968).¹⁰ Stronger immigration enforcement could further reduce employers’ expected costs of injuries for other reasons. For example, enforcement might dissuade Hispanic workers from filing for workers’ compensation if they get injured at work, especially if they fear employer retaliation.¹¹

Given this institutional background and brief conceptual framework, we expect the following: Hispanic workers face higher barriers to complain to the government than their non-Hispanic counterparts, immigra-

under the National Labor Relations Act.

⁹Deportation risks are substantially higher for Hispanic immigrants than for any other immigrant group. Among all individuals deported between 2005 and 2014, 94 percent were Hispanic, even though only 76 percent of the undocumented population were (Migration Policy Institute, 2019; Rugh and Hall, 2016). This fact further motivates our focus on Hispanic workers.

¹⁰Research demonstrates a similar mechanism for the effect of immigration enforcement on crime. The legalization of almost 3 million undocumented immigrants through the 1986 U.S. Immigration Reform and Control Act increased the probability that Hispanic victims reported a crime and reduced the victimization of Hispanic individuals (Comino et al., 2016). Conversely, ICE cooperation with local law enforcement decreased crime reporting by Hispanic communities without reducing crime commission or victimization (Dhingra et al., 2021). In contrast, research finds that increased local immigration enforcement through 287(g) and Secure Communities had little effects on overall crime rates (Cox and Miles, 2013; Forrester and Nowrasteh, 2018; Miles and Cox, 2014; Treyger et al., 2014).

¹¹Employers pay premiums into the workers’ compensation system, and workers are guaranteed a portion of their earnings as compensation if they file for workers’ compensation in the event of a work-related injury. Because employers’ premiums are a function of their workers’ claim history, it is costly for employers when their workers file a claim. Several studies have documented that a substantial share of workers do not apply for workers’ compensation for work-related injuries because they fear employer retaliation (Biddle and Roberts, 2003; Fan et al., 2006; Bernhardt et al., 2009).

tion enforcement further reduces Hispanic workers' willingness to complain, and immigration enforcement in turn increases Hispanic workers' risk of facing workplace hazards and violations of labor regulations.

3 Data and Descriptive Evidence

We want to estimate the causal impact of local immigration enforcement on Hispanic workers' complaints and safety and health outcomes. To do so, we need data on worker complaints and injuries, workplace demographics, and immigration enforcement.

3.1 Measuring the Incidence of Worker Complaints and Workplace Injuries

We use three measures of worker complaints; each has its own advantage. First, we use all complaints received by OSHA from 2001 through 2019. We call these "total complaints." They include "formal" and "nonformal" complaints. A formal complaint is one that a) asserts that an imminent danger or violation of an OSHA standard is exposing workers to potential harm, and b) at least one employee or employee representative has signed. OSHA follows up the majority of formal complaints with an inspection, unless it determines that there is no reasonable ground for a violation. Nonformal complaints are those that do not meet the criteria for a formal complaint (that is, do not assert a potential harm and/or are anonymous). OSHA inspects only a small share of nonformal complaints that meet certain criteria, such as if imminent danger is likely ([U.S. Occupational Safety and Health Administration, 2020](#)).¹² The total complaints measure has the advantage that it captures all complaints to OSHA. However, it paints with a broad brush: a worker's cost of filing a (signed) formal complaint could be quite different than the cost of filing an (anonymous) nonformal complaint.

Our second measure of worker complaints is formal complaints. This measure has the advantage that it focuses on the subset of complaints that likely trigger OSHA inspections. An advantage of this measure is that formal complaints might be more correlated with underlying health and safety issues than nonformal complaints, since one of the criteria for a complaint to be deemed formal is that it asserts that workers are being exposed to a potential physical or health harm.

Our third measure of worker complaints is the occurrence of inspections conducted by OSHA that are responses to a complaint. We call these "complaint-driven inspections." As described above, nearly all formal complaints, and a subset of nonformal complaints, trigger an OSHA inspection within a few weeks. An advantage of this measure is that it captures the subset of complaints that OSHA deems serious enough to merit an inspection. Another advantage is that this complaint measure mirrors our measure of workplace

¹²For complaints that do not result in an inspection, OSHA contacts the employer and mentions the complaint and the referenced concerns. OSHA gives the employer five days to respond with a plan to address the complaint. If the employer does not respond within five days, OSHA has the right to schedule an inspection of the employer.

safety, described next.

Because most employers do not directly report injuries to OSHA, we cannot measure workplace injuries directly for the majority of workplaces. Instead, we measure worker injuries using OSHA inspections that are triggered by a serious accident. We call these “injury-driven inspections.” A serious accident is an accident that results in a worker fatality or the hospitalization of three or more workers. The law requires that the employer reports any serious accident to OSHA and that OSHA subsequently inspect the workplace immediately. Injury-driven inspections are a reliable measure of the occurrence of fatal and serious nonfatal work-related injuries ([Mendeloff and Kagey, 1990](#)).

We obtained data on total and formal complaints from 2001 through 2019 through a Freedom of Information Act (FOIA) request with the U.S. Department of Labor. Data on complaint- and injury-driven inspections come from OSHA’s Integrated Management Information System (IMIS) ([U.S. Department of Labor, 2021](#)). For our inspection measures, we drop the small share of inspections that are of public-sector workplaces (we do not observe public/private sector status of complaints in the FOIA data so cannot make a similar restriction there). We collapsed the FOIA and IMIS data to create a dataset with the annual number of total complaints, formal complaints, complaint-driven inspections, and injury-driven inspections for each county-industry. We operationalize “industry” roughly as two-digit NAICS codes. We exclude industries in the service sector, since many of them (such as finance and insurance) rarely experience work-related injuries.

3.2 Measuring Local Immigration Enforcement

We use counties’ participation in Secure Communities as a proxy for local immigration enforcement. Secure Communities was enacted as an information-sharing program between local law enforcement and ICE. In participating counties, local law enforcement agencies automatically share fingerprints of a person they arrested with ICE, which determines if it can deport the arrested person for violating immigration law. If ICE finds reason for deportation, it requests the local law enforcement agency to hold the person for up to 48 hours. Within that time, ICE tries to take the person into custody and start removal proceedings. Secure Communities had far-reaching consequences for immigrants in the United States, leading to over 512,700 removals between 2008 and 2016 ([Transactional Records Access Clearinghouse, 2019](#)). [Figure A2](#) shows that the number of Secure Communities–related removals increased quickly as Secure Communities was activated across the country. [Cox and Miles \(2013\)](#) and [Capps et al. \(2011\)](#) describe Secure Communities in detail.

We use the staggered roll-out of Secure Communities as our identifying variation. Participation in Secure Communities was mandatory for all counties. Because of logistical constraints, such as a lack of fingerprint scanners and insufficient ICE personnel, ICE rolled out Secure Communities gradually. [Section A1](#) describes the roll-out in detail. Like [Cox and Miles \(2013\)](#), we find that Secure Communities started earlier in counties closer to Mexico and counties with a larger Hispanic population. We account for this selection

in our empirical strategy by including county-industry fixed effects and by conducting several robustness checks. We combined data on the Secure Communities program from [U.S. Immigration and Customs Enforcement \(2013\)](#) and [U.S. Immigration and Customs Enforcement \(2014\)](#) to obtain the start date of Secure Communities for each county in the United States.

3.3 Measuring Hispanic Workforce Shares

[Section 2](#) argued immigration enforcement likely increased Hispanic workers' barriers to complain about workplace safety violations. Ideally, we could observe the share of workers who are Hispanic at each workplace and connect it to the workplace-level worker complaints and injuries data. However, we are unaware of any dataset that includes establishment-level worker demographics and covers all establishments in the United States.

We therefore use the share of Hispanic workers at the county-industry level from the ACS 1 percent representative sample as a proxy. [Section A2](#) describes in detail how we obtain this measure.¹³ In robustness checks we instead use a workplace's share of foreign-born noncitizen workers and Hispanic foreign-born noncitizen workers. We operationalize "industry" roughly as two-digit NAICS codes. We use county-industries' average Hispanic workforce share in the years 2005 through 2007. We choose these years because they precede the start of Secure Communities, and Hispanic workforce shares might subsequently become endogenous.¹⁴

Hispanic workforce shares vary widely across both counties and industries. [Figure A3](#) shows the spatial distribution of the Hispanic workforce share in agriculture, construction and manufacturing. There are clear differences in the Hispanic workforce share within industries, across counties.

3.4 Descriptive Evidence on Worker Complaints, Workplace Hazards, and Workforce Demographics

Descriptive evidence suggests that workers are more likely to complain to OSHA when they face higher risk of workplace injury, but that this relationship appears to reverse among workplaces with a large Hispanic workforce share.

We first investigate the relationship between workers' propensity to complain and the workplace hazard they face. We estimate the correlation between the rate of complaint-driven inspections (per 100,000

¹³We consider an alternative data source, the Quarterly Workforce Indicators, in [Appendix B](#).

¹⁴Our estimate of each county-industry's annual Hispanic workforce share is measured with error, because we use a 1 percent representative sample from the ACS to infer a population statistic. This measurement error could lead to attenuation bias of estimates from regressions that include the Hispanic workforce share as an explanatory variable. In practice, this attenuation bias does not materially affect our estimates. Our regressions weight observations by county-industries' initial employment, such that small county-industries—for which measurement error is particularly severe—have less influence on our estimates.

workers) and the lagged rate of injury-driven inspections in a county-industry. [Figure C1](#) illustrates this relationship in a binned scatter plot, controlling for the rate of programmed OSHA inspections, whether the county lies in a metropolitan statistical area, the county’s poverty rate, as well as OSHA region, year, and industry fixed effects. We find clear evidence for a positive relationship. One more injury-driven inspection per worker is associated with 0.27 more complaints per worker in the following year ($p < 0.01$). This suggests that worker complaints generally help direct inspections to workplaces where injury risks are higher.

We then examine how the propensity to complain and risk of workplace injury correlates with workers’ demographics. We first correlate the number of complaint-driven inspections per 100,000 workers with the Hispanic workforce share, residualizing on the same controls. The binned scatterplot in [Figure C2a](#) shows a strong negative correlation: a 10 percentage point increase in the Hispanic workforce share is correlated with a 0.09 *fewer* complaints per worker ($p < 0.01$). However, [Figure C2b](#) shows that injuries are *more* frequent when a higher share of the workforce is Hispanic: a 10 percentage point increase in the Hispanic workforce share is associated with 0.25 *more* injuries per worker ($p < 0.01$).

These results indicate that workplaces with more Hispanic workers are more hazardous, but that workers at these workplaces are less likely to complain about safety issues to the government. However, this correlational analysis leaves important questions unanswered. Does the lower rate of complaints among workplaces with a higher share of Hispanic workers reflect a barrier to complaining, such as a reluctance to interact with government for fear of deportation? Does Hispanic workers’ reduced willingness to complain have a causal effect on employers’ behavior and the hazards workers face? The next section addresses these questions by examining the effect of immigration enforcement on worker complaints and workplace injuries.

4 The Effects of Immigration Enforcement on Worker Complaints and Injuries

This section presents our estimates of the effects of Secure Communities on worker complaints and workplace injuries, estimates of the program’s dynamic effects over time, and checks of the robustness of our estimates.

4.1 Empirical Strategy to Estimate the Causal Effect of Immigration Enforcement

To estimate the effect of Secure Communities on worker complaints and injuries, we use a triple-difference design. Identification comes from the variation in complaints and injuries in county-industries with different levels of Hispanic workforce shares across years with and without Secure Communities.

We estimate the following equation for county c , industry i and year t :

$$\begin{aligned}
\text{asinh}(Y_{cit}) = & \beta_1 SC_{ct} + \beta_2 SC_{ct} \times \text{exposure}_{ci} \\
& + \gamma_1 \text{asinh}(\text{employment}_{cit}) + \gamma_2 \text{asinh}(\text{programmed_inspect}_{cit}) \\
& + \text{exposure}_{ci} \times \delta_t + \zeta_{ci} + \eta_{it} + \theta_{rt} + \iota_{jt} + \epsilon_{cit}.
\end{aligned} \tag{1}$$

Our dependent variable Y is the inverse hyperbolic sine of the number of worker complaints, complaint-driven OSHA inspections, or injury-driven inspections. We take the inverse hyperbolic sine to reduce the influence of extreme outliers while preserving observations with zero inspections. In robustness checks we show that our results are very similar when we instead use a log specification or the rate of complaints or injuries per worker.

We regress this dependent variable on the fraction of the year during which the county participated in Secure Communities, SC_{ct} , and the interaction of this fraction with a county-industry level exposure measure, $SC_{ct} \times \text{exposure}_{ci}$. The exposure measure is either the county-industry’s Hispanic workforce share (ranging from 0 to 1) or an indicator for the Hispanic workforce share being above the 80th percentile of the distribution. To adjust for overall changes in employment levels, we control for the inverse hyperbolic sine of county-industry employment, $\text{asinh}(\text{employment}_{cit})$. We control for the overall level of OSHA’s inspection activity by including the inverse hyperbolic sine of programmed inspection counts, $\text{asinh}(\text{programmed_inspect}_{cit})$. Programmed inspections are regularly scheduled OSHA inspections that are neither driven by worker complaints nor by injuries.¹⁵

$\text{exposure}_{ci} \times \delta_t$ is an interaction between the exposure measure and year fixed effects. This allows us to control for unobservables that differentially affect workplaces with a higher or lower share of Hispanic (or noncitizen, or Hispanic noncitizen) workers over time. ζ_{ci} are county-industry fixed effects, and η_{it} are industry-year fixed effects. We include OSHA region-year fixed effects θ_{rt} to control for unobservable time-varying factors that differentially affect particular geographic areas.¹⁶ Finally, we include OSHA jurisdiction-year fixed effects ι_{jt} , which indicate if the county’s state is one of the 28 states under OSHA’s jurisdiction or one of the 22 states with its own state-run plan. This term accounts for differences in how OSHA’s regional offices handle worker complaints, combined with time-varying shocks that differentially affect states under OSHA’s jurisdiction, such as a change in OSHA leadership.¹⁷ We cluster standard errors at the county, since this is the unit of our identifying variation (Bertrand et al., 2004). We weight observations by the county-industry’s average share of total employment across the years 2005–2007, prior to the onset of Secure Communities.

Our coefficient of interest is β_2 . It captures the differential impact of Secure Communities on complaints

¹⁵All of our results are the same if we omit the control for programmed inspections.

¹⁶We do not include the more granular county-year fixed effects in our main specification. If we did, our only identifying variation would come from variation in Hispanic workforce shares across industries *within* counties. In Section B, we show results from this model. The point estimates are similar but have substantially larger standard errors.

¹⁷As we show below, our results are insensitive to either broader or more narrow time-varying geographic fixed effects.

or injuries in a workplace that has a 100 percent Hispanic workforce. We restrict our sample to the 48 contiguous U.S. states and the District of Columbia between the years 2003–2016. Since Secure Communities started in 2008, this restriction yields at least five pre–Secure Communities years for every county.

Our strategy relies on the identification assumption that, conditional on county-industry employment, programmed inspections, and the fixed effects, no unobserved factors are correlated with the timing of the Secure Communities roll-out that also affect complaints or injuries differently for county-industries with high versus low Hispanic workforce shares. We take several steps to assess the validity of this identification assumption in Section 4.4.

4.2 Baseline Estimates

Table 1 presents our estimates of the effect of local immigration enforcement on worker complaints and workplace injuries. In columns 1–3, the dependent variable is one of our three measures of worker complaints: the total number of complaints received by OSHA, the total number of formal complaints (those signed by at least one employee), and the number of OSHA inspections triggered by a worker complaint. Results in all three columns show that Secure Communities significantly reduced complaints in county-industries with a high share of Hispanic workers. Since the outcome variable is an inverse hyperbolic sine, we can interpret the coefficients roughly as percentage changes. In column 1, the coefficient on $SC \times \text{Hispanic share}$ implies that Secure Communities reduced the total number of complaints filed to OSHA by 60 percent among county-industries with a 100 percent Hispanic workforce share, relative to a county-industry with a 0 percent Hispanic share ($p = 0.03$). Columns 2 and 3 show a slightly smaller effect for formal complaints signed by a worker ($\hat{\beta} = -0.46, p = 0.02$) and complaint-driven OSHA inspections ($\hat{\beta} = -0.49, p < 0.01$).

Two opposing mechanisms could explain this reduction in complaints. On the one hand, stronger immigration enforcement could have led to an improvement in workplace safety—for example by reducing employment in risky jobs—and hence a reduction in worker complaints. On the other hand, stronger enforcement could have dissuaded workers from filing a complaint independent of workplace hazards. As our framework in Section 2 outlined, this could lead forward-looking employers to increase hazards. To test if immigration enforcement affected workplace hazards, we use the inverse hyperbolic sine of injury-driven inspections as our outcome variable and reestimate Equation (1). Column 4 shows that Secure Communities led to a 31 percent increase in injuries for county-industries with 100 percent Hispanic workers, relative to county-industries with no Hispanic workers ($p = 0.01$).

One potential issue in interpreting the coefficients is that the specifications in Panel A assume that the effect of immigration enforcement on complaints and injuries is linear in the Hispanic workforce share. This assumption could be problematic, for example if Hispanic workers are more or less responsive to immigration enforcement depending on what share of their coworkers are also Hispanic. To evaluate this concern, we instead use an indicator equal to one if the Hispanic workforce share is above the 80th percentile

Table 1: Secure Communities Reduced Worker Complaints but Increased Workplace Injuries

Dependent variable:	Inverse hyperbolic sine of			
	Total complaints (1)	Formal complaints (2)	Complaint-driven inspections (3)	Injury-driven inspections (4)
Panel A: Linear exposure measure				
SC	0.070** (0.033)	0.016 (0.037)	0.097*** (0.033)	-0.060*** (0.021)
SC×Hispanic share	-0.60** (0.28)	-0.46** (0.20)	-0.49*** (0.18)	0.31** (0.13)
Panel B: Indicator for “high” exposure measure				
SC	0.032 (0.022)	-0.011 (0.033)	0.088*** (0.030)	-0.051** (0.020)
SC×high Hispanic share	-0.21** (0.09)	-0.18* (0.09)	-0.23*** (0.07)	0.11** (0.06)
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓
Exposure measure × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
OSHA region × year FE	✓	✓	✓	✓
OSHA jurisdiction × year FE	✓	✓	✓	✓
Mean Dep Var (in levels)	11.08	2.75	4.64	1.42
Mean share Hispanic if high-Hispanic share=1	0.40	0.40	0.40	0.40
# Observations	216,411	216,411	216,411	216,411

Note: * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of worker complaints to OSHA (columns 1–3) or the number of OSHA inspections triggered by a serious worker injury (column 4) on an indicator for a county participating in Secure Communities, the interaction of this indicator with the exposure measure (Hispanic workforce share in Panel A, or indicator for high Hispanic workforce share in Panel B), and the controls and fixed effects indicated in the bottom panel. “High Hispanic share” is equal to 1 if the county-industry’s Hispanic share is above the 80th percentile. Asinh stands for inverse hyperbolic sine.

of the distribution (“high Hispanic share”). Panel B of [Table 1](#) shows that the results are robust to replacing the Hispanic workforce share with the indicator for a high Hispanic workforce share.¹⁸

If we assume that the effect of Secure Communities is zero for all non-Hispanic workers, we can derive an estimate of the effects on an individual worker’s propensity to complain. Secure Communities reduced the probability of a Hispanic worker complaining to OSHA by roughly 53 percent (using the sum of the main and interaction effects in column 1 for all complaints), but increased that worker’s chance of being injured by 25 percent.

We note that columns 1 and 3 in [Table 1](#) show a positive and statistically significant estimate for the coefficient of *SC*, implying that Secure Communities led to a small *increase* in total complaints and complaint-driven inspections in county-industries with zero Hispanic workers. This effect is perhaps surprising but not entirely implausible. Prior research has found that Secure Communities decreased employment rates among all workers, including U.S. citizens ([East et al., 2012](#)). If this decrease in employment was concentrated in riskier jobs, it could have reduced total workplace hazards. That said, this main effect is not robust to alternative specifications: for example, as shown in [Table B3](#), including state-year (rather than region-year) fixed effects attenuates the main effect toward zero and renders it statistically insignificant, while leaving the interaction term unaffected.

4.3 Event Study Estimates

Our triple-difference regression design in [Equation \(1\)](#) comes with two limitations. First, our estimates might be confounded by pre-trends—the trends of complaints and injuries could differ across county-industries in the years preceding a county’s participation in Secure Communities. Second, these estimates do not provide any insights on the dynamics of the effects—that is, when the effects show up and how long they last.

To address these two limitations, we estimate the following event study regression:

$$\begin{aligned}
 asinh(Y_{cit}) = & \sum_{k \neq -1} \beta_1^k (I_{c,t=k}) + \sum_{k \neq -1} \beta_2^k (I_{c,t=k} \times exposure_{ci}) \\
 & + \gamma_1 asinh(employment_{cit}) + \gamma_2 asinh(programmed_inspect_{cit}) \\
 & + exposure_{ci} \times \delta_t + \zeta_{ci} + \eta_{it} + \theta_{rt} + \iota_{jt} + \epsilon_{cit}.
 \end{aligned} \tag{2}$$

In [Equation \(2\)](#), $I_{c,t=k}$ is an indicator for each year k relative to the the activation of Secure Communities in county c . We treat the year right before activation as the reference year and drop its indicator

¹⁸To compare the magnitudes of the estimates in Panels A and B, consider that the mean *Hispanic Share*, conditional on being above the 80th percentile of the distribution, is 0.4. Thus, the coefficient on *SC* × *high Hispanic share* in Column 1 of Panel B implies that Secure Communities reduced complaints in county-industries with 100 percent Hispanic share by 53 percent (0.21/0.4=0.525), which is quite close to the estimate of 60 percent in Panel A.

from the equation. We include the same four sets of fixed effects as in our baseline regression specification [Equation \(1\)](#). To avoid spurious estimates from very long leads or lags, we group all observations six years or earlier before and five years or later after Secure Communities activation into one period. The coefficients of interest are the β_2^k s. They are the difference in complaints or injuries between county-industries with Hispanic workers and county-industries without any Hispanic workers in each year relative to Secure Communities' activation.

[Figure 1](#) shows the results. Three patterns emerge. First, complaints and injuries did not have differential trends between workplaces with and without Hispanic workers in the years before Secure Communities activation. If anything, worker complaints were on a slightly positive relative trend in county-industries with Hispanic workers. Second, the decrease in complaints in county industries with Hispanic workers was immediate, persistent, and grew over time. Third, injuries increased one year after Secure Communities activation in county-industries with Hispanic workers and remained high.

The event study results corroborate our previous results. In county-industries with a high share of Hispanic workers, local immigration enforcement discouraged workers from complaining, but led those workers to experience higher injury rates. We assess the robustness of our estimates in the next section.

4.4 Robustness Checks

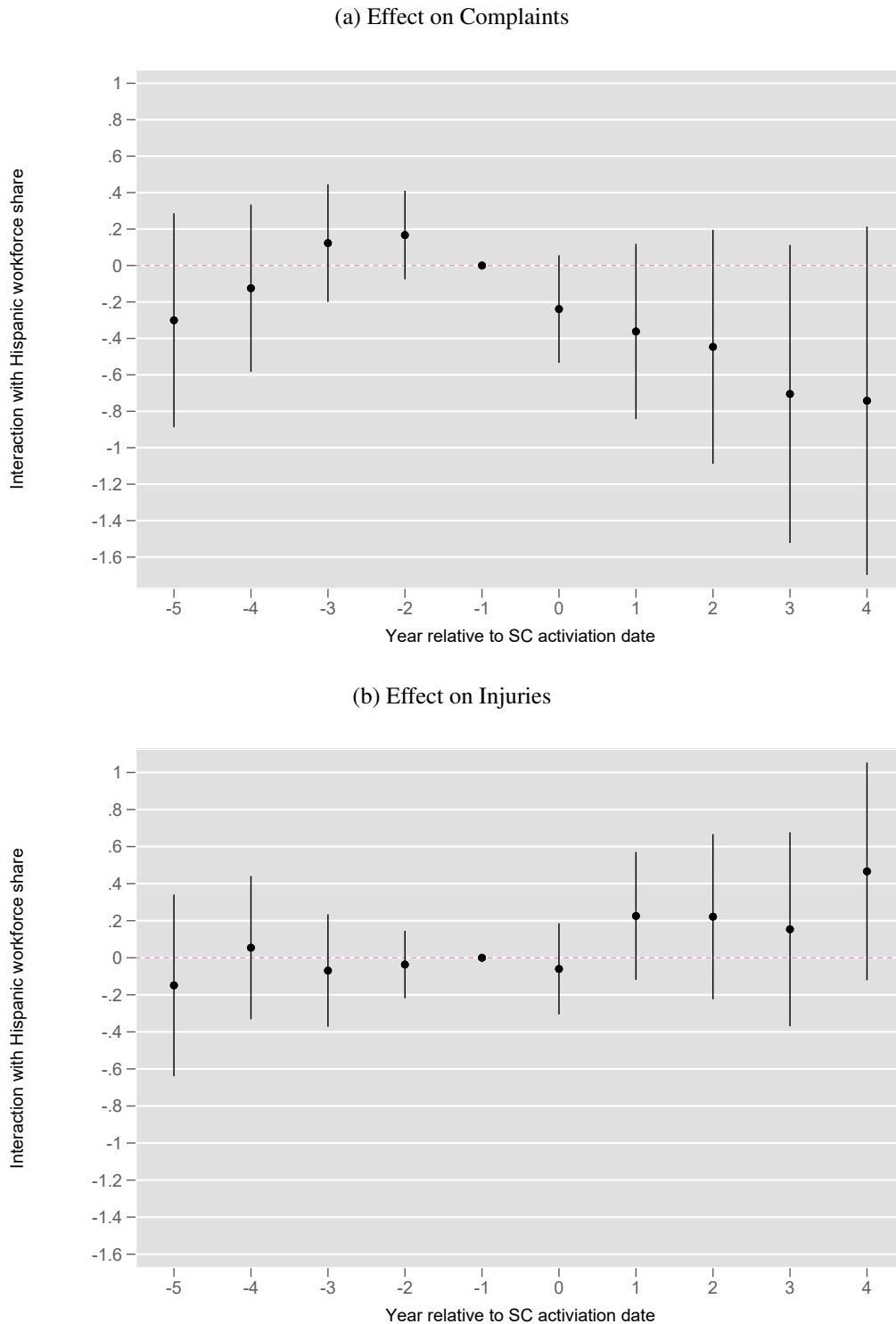
In this section, we check the robustness of our results along several dimensions. First, we consider alternative measures for county-industries' exposure to immigration enforcement. Second, we ensure that measurement error in our exposure variables does not substantially bias our estimates. Third, we ensure our results are not driven by endogenous selection into Secure Communities, outlier counties, or confounding factors like the Great Recession, or particular specification choices. Finally, we assess the sensitivity of our estimates to potential bias from heterogeneous treatment effects.

4.4.1 Considering Alternative Exposure Measures

In our main specification, we use the share of a county-industry's workforce that is Hispanic to measure a county-industry's exposure to immigration enforcement. We now instead use the share of noncitizens and the share of Hispanic noncitizens in a county-industry's workforce to capture exposure to Secure Communities. Because of the restriction to noncitizens, both groups likely include a higher proportion of undocumented immigrants than Hispanic workers overall. The effect of immigration enforcement on worker complaints and injuries thus might be larger for these groups than in our baseline specification.

Panel A of [Table 2](#) shows estimates for the effect of Secure Communities on the number of complaint-driven inspections. It shows the baseline results (column 1) together with the results for the noncitizen (column 2) and the Hispanic noncitizen (column 3) exposure measures. Irrespective of the exposure measure

Figure 1: Effects of Secure Communities on Complaints and Injuries Are Immediate and Persistent



Note: Figures show coefficient estimates and 95 percent confidence intervals of β_2^k in Equation (2) where $exposure_{ci}$ is the county-industry's Hispanic workforce share. These are the dynamic effects of Secure Communities on worker complaints and workplace injuries in workplaces with a Hispanic workforce share of 100 percent, relative to one with a share of 0 percent. Regressions include controls for the inverse hyperbolic sine of county-industry employment, the inverse hyperbolic sine of programmed inspections, the interaction of the Hispanic workforce share with year fixed effects, county-industry fixed effect, industry-year fixed effect and OSHA region-jurisdiction-year fixed effects. Standard errors are clustered at the county.

Table 2: Results Are Robust to Alternative Measures of Exposure to Secure Communities

Exposure measure:	(1) Hispanic workforce share	(2) Noncitizen workforce share	(3) Hispanic noncitizen workforce share
Panel A: Dep Var = Complaint-driven inspections			
SC	0.097*** (0.033)	0.101*** (0.033)	0.091*** (0.031)
SC×Hispanic share	-0.49*** (0.18)		
SC×noncitizen share		-0.78*** (0.25)	
SC×Hispanic noncitizen share			-0.95*** (0.28)
Panel B: Dep Var = Injury-driven inspections			
SC	-0.06*** (0.02)	-0.05** (0.02)	-0.05** (0.02)
SC×Hispanic share	0.31** (0.13)		
SC×noncitizen share		0.37* (0.19)	
SC×Hispanic noncitizen share			0.43* (0.23)
Asinh(programmed inspections)	✓	✓	✓
Asinh(employment)	✓	✓	✓
Exposure measure × year	✓	✓	✓
County × industry FE	✓	✓	✓
Industry × year FE	✓	✓	✓
OSHA region × year FE	✓	✓	✓
OSHA jurisdiction × year FE	✓	✓	✓
# Observations	216,411	216,411	216,411

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of complaint-driven inspections (Panel A) or injury-driven inspections (Panel B) on an indicator for a county participating in Secure Communities, the interaction of this indicator with the exposure measure, and the controls and fixed effects indicated in the bottom panel. Asinh stands for inverse hyperbolic sine.

we use, we find a significant negative effect of Secure Communities on worker complaints in high-exposure county-industries. Compared to the baseline results, the effect is 61 percent larger when we use the noncitizen workforce share and nearly twice as large when we use the Hispanic noncitizen workforce share. Panel B of Table 2 shows that the effect of Secure Communities on workplace injuries also increase in magnitude by 20 and 39 percent using the noncitizen and Hispanic noncitizen exposure measures ($p = 0.053$ and $p = 0.060$, respectively).

4.4.2 Using a Different Data Source to Measure Workforce Demographics

As discussed in [Section 3.3](#), the Hispanic workforce share in our analysis is measured with error. This is because we use a statistic drawn from a 1 percent nationwide representative sample—the percent of respondents to the ACS who report working in a particular industry who are Hispanic—to infer a statistic relevant to the population—the percent of workers in a particular county-industry who are Hispanic. In [Section B1](#), we conduct analyses in which we use the QWI, rather than the ACS, to measure county-industries’ Hispanic workforce share. In that Appendix, we report results in which we directly swap out the QWI-based *Hispanic Share* for the ACS-based one, as well as an instrumental variables approach to eliminate the measurement error. In both cases we get very similar estimates to our baseline results, suggesting that neither our particular data source nor measurement error are influencing our estimates.

4.4.3 Robustness to Confounding Factors and Specification Choices

In [Section B2](#), we assess the robustness of our estimates to endogenous selection of counties into Secure Communities, large influence from outliers, confounding factors like the Great Recession, and alternative modelling choices. We summarize the results of these checks in [Figure 2](#). [Section B2](#) explains each check and shows the regression tables. Our estimates are remarkably stable across this range of robustness checks.

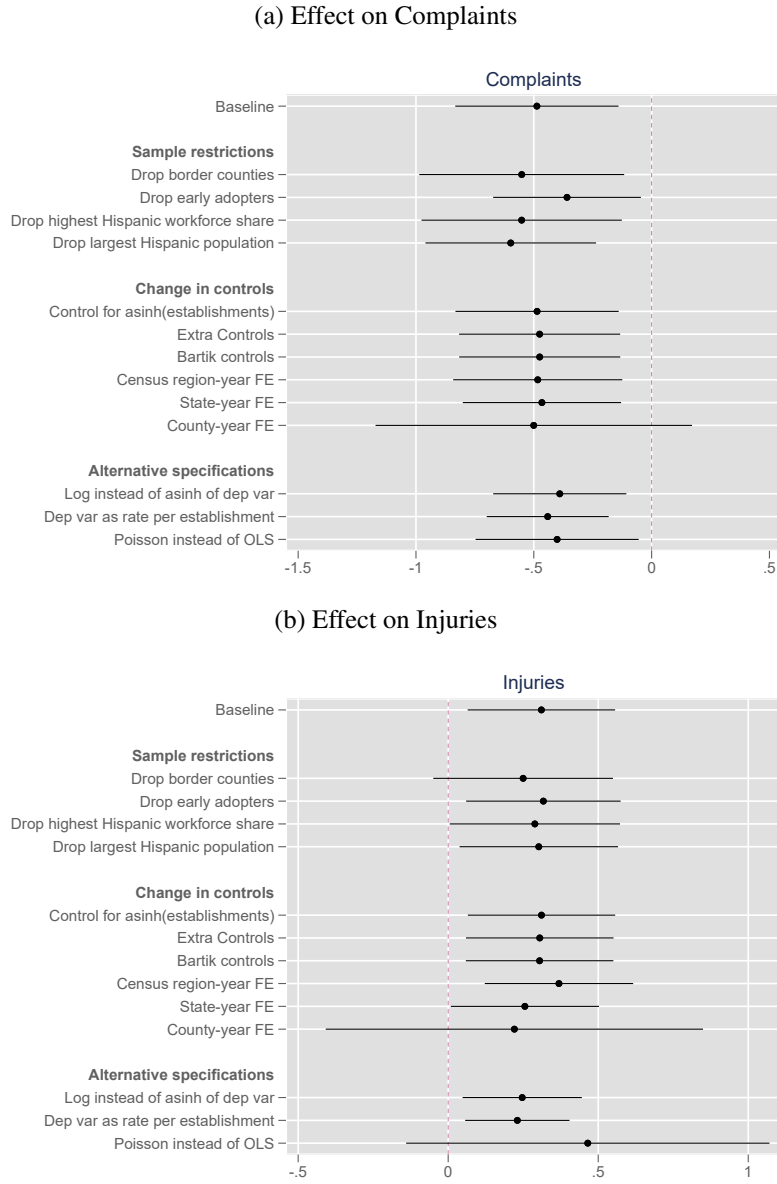
4.5 Sensitivity to Potential Bias from Heterogeneous Treatment Effects

Recent studies have advanced our understanding of the performance of difference-in-difference estimators in settings where a treatment is adopted in a staggered fashion and effects of the treatment might be heterogeneous across time. In particular, [Goodman-Bacon \(2018\)](#) shows that a two-way fixed effects estimator (i.e., in our case, one with county-industry and year fixed effects) can be biased because it uses early treatment groups as control units for later treatment groups. We assess the sensitivity of our results to this concern in Appendix [Section B3](#). We find no evidence that our estimates are materially influenced by this concern.

5 Assessing the Mechanism Through Which Immigration Enforcement Affects Workplace Safety

Our results reveal that Secure Communities reduced complaints to OSHA and increased injury rates at workplaces with high shares of Hispanic, noncitizen, and Hispanic noncitizen workers. These results are consistent with our framework in [Section 2](#), which describes why employers would adjust their safety inputs to workers’ willingness to complain. However, other factors might lead to the increases in injuries following Secure Communities activation.

Figure 2: Results Are Stable Across a Range of Robustness Checks



Note: Figures shows coefficient estimates and 95 percent confidence intervals of β_2^k in Equation (1) where $exposure_{ci}$ is the county-industry's Hispanic workforce share. Unless otherwise specified, regressions include controls for the inverse hyperbolic sine of county-industry employment, the inverse hyperbolic sine of programmed inspections, the interaction of the Hispanic workforce share with year fixed effects, county-industry fixed effect, industry-year fixed effect and OSHA region-jurisdiction-year fixed effects. Standard errors are clustered by county. See Section B2 for details about each specification.

Three pieces of evidence support the interpretation that employers’ response to workers’ willingness to complain at least partially caused the increase in injuries following Secure Communities. First, we show that Secure Communities reduced compliance with safety and health regulations. Second, we demonstrate that Secure Communities had no effect on worker complaints or injury rates in unionized workplaces. Third, we show that Secure Communities also decreased employers’ compliance with minimum wage laws which, like OSHA regulations, are enforced heavily based on worker complaints.

5.1 Immigration Enforcement Reduced Compliance with Safety Regulations

One salient example of employers’ inputs into safety is their compliance with government safety and health regulations. This section examines if violations of OSHA regulations increased following the introduction of Secure Communities. To estimate the effect of Secure Communities on OSHA compliance, we estimate a modified version of the regression model in [Equation \(1\)](#):

$$\begin{aligned}
 Viol_{pcit} = & \beta_1 SC_{ct} + \beta_2 SC_{ct} \times exposure_{ci} \\
 & + \gamma X_{pcit} + exposure_{ci} \times \delta_t + \zeta_{ci} + \eta_{it} + \theta_{rt} + \iota_{jt} + \epsilon_{pcit}.
 \end{aligned}
 \tag{3}$$

Here, our dependent variable is the number of violations found at inspection of workplace p in county c , in industry i , conducted in year t .¹⁹ SC , $exposure$, and the four sets of fixed effects are the same as in [Equation \(1\)](#). We control for a vector of workplace-specific characteristics in X , including the number of employees reported present during the inspection, whether workers in the inspected facility were represented by a labor union, and whether the inspection was limited in scope to a subset of the facility’s area of operations.²⁰

We consider three categories of violations: 1) the total number of violations detected, 2) the number of “serious” violations, and 3) the number of “repeat” or “willful” violations. A violation is “serious” if OSHA determines that it results in a high probability of death or serious harm. Serious violations are likely highly correlated with the occurrence of injuries. A violation is “repeat” if OSHA has previously cited the employer for it. A violation is “willful” if the employer has demonstrated an intentional disregard for OSHA standards or a plain indifference to employee safety and health.

Because violations are only observed conditional on an inspection occurring, we restrict the estimation of [Equation \(3\)](#) to programmed inspections. As described above, the occurrence of programmed inspections

¹⁹We do not transform our dependent variable using the inverse hyperbolic sine—like we did in [Equation \(1\)](#)—because the number of violations detected in an inspection is more discrete and more compressed toward zero than the number of worker complaints and workplace injuries. Estimates are similar if we transform the dependent variable with the inverse hyperbolic sine, but the magnitudes are slightly smaller.

²⁰Because this regression is at the inspection level, we do not control for county-industry employment as we did in [Equation \(1\)](#), though doing so has no effect on our estimates.

is exogenous to events at a particular workplace, conditional on industry and location. OSHA’s National Emphasis Programs, which focus on nationwide priorities, and Local Emphasis Programs, which focus on regional priorities, determine where programmed inspections take place.

Table 3: Secure Communities Reduced Compliance with Safety and Health Regulations in Workplaces with High Hispanic Workforce Shares

Dependent variable:	Number of		
	Overall violations (1)	Serious violations (2)	Repeat or willful violations (3)
Panel A: All states			
SC	-0.16*** (0.05)	-0.15*** (0.05)	-0.01* (0.01)
SC×Hispanic share	0.46** (0.23)	0.52*** (0.19)	0.05* (0.03)
Mean Dep Var	1.75	1.39	0.07
# Observations	578,866	578,866	578,866
Panel B: States under federal OSHA jurisdiction			
SC	-0.08 (0.07)	-0.09 (0.06)	-0.02* (0.01)
SC×Hispanic share	0.56* (0.31)	0.64** (0.28)	0.06* (0.04)
Mean Dep Var	2.13	1.82	0.09
# Observations	248,903	248,903	248,903
Exposure measure × year	✓	✓	✓
County × industry FE	✓	✓	✓
Industry × year FE	✓	✓	✓
OSHA region × year FE	✓	✓	✓
OSHA jurisdiction × year FE	✓	✓	✓

Note: * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the number of the of violations indicated in the column title that were detected in an OSHA inspection on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share, and the controls and fixed effects indicated in the bottom panel.

Panel A of Table 3 shows results for regressions estimated using all U.S. states. Panel B shows results from regressions on the subest of 28 states that are under federal OSHA jurisdiction. We restrict the sample to these states because states outside of OSHA’s jurisdiction have very different rules for designating violations as serious, repeat, or willful. For both samples and all measures of violations, the results indicate

that Secure Communities led to worse compliance with OSHA regulations among workplaces with Hispanic workers. Column 1 of Panel A reveals that Secure Communities led to 0.46 more violations among workplaces with 100 percent Hispanic workers ($p = 0.05$), relative to workplaces without any Hispanic workers, a 26 percent increase relative to the sample mean ($0.46/1.75$). For the sample of states under federal OSHA jurisdiction, the estimated effect increases to 0.56 ($p = 0.07$). Column 2 shows that at workplaces with 100 percent Hispanic workers, serious violations increased by 0.52 ($p < 0.01$) in all states and by 0.64 ($p = 0.02$) in states under federal OSHA jurisdiction. Secure Communities also increased repeat or willful violations (column 3), but the estimate is only marginally statistically significant.

Across all specifications, the main effect of Secure Communities is negative. However, this effect attenuates in size (and loses statistical significance in two of three columns) when restricting the sample to states under federal OSHA jurisdiction in Panel B. Furthermore, as with our results on complaints and injuries, this main effect is not robust to other sensible specifications: including state-year (rather than region-year) fixed effects attenuates the magnitude and erases the significance of this main effect, while leaving the interaction effect unchanged (results not shown). Additionally, as we describe in [Section 4](#), a reduction in the number of violations at workplaces with few Hispanic workers might reflect an overall negative effect of Secure Communities on employment in risky jobs ([East et al., 2012](#)).

Compliance with government safety regulations is a salient measure of employers' inputs into workplace safety. This section showed that immigration enforcement led employers in workplaces with a large share of Hispanic workers to reduce their compliance with workplace safety regulations. This evidence supports the hypothesis that Secure Communities, by decreasing workers' likelihood of complaining, reduced incentives for employers to provide workplace safety and thus increased workplace injuries.

5.2 Labor Unions Mitigated the Effect of Immigration Enforcement on Complaints and Injuries

Labor unions likely mitigate the extent to which immigration enforcement affects both workers' willingness to complain and any subsequent change in safety. First, labor unions remove barriers that inhibit worker complaints. Unions inform workers about their rights to complain, solve the public good problem, and file complaints on workers' behalf ([Weil and Pyles, 2005](#)). Consistent with these actions, unions make workers more likely to complain to OSHA ([Sojourner and Yang, 2020](#)). Second, unions limit employers' ability to respond to a change in workers' willingness to complain. Unions include specific safety and health provisions in contract agreements ([Morantz, 2018](#)), limiting employers' ability to adjust their inputs into safety. Third, unions ensure that employers have adequate ongoing incentives to limit work-related injuries, for example by providing resources for injured workers to file for workers' compensation ([Hirsch et al., 1997](#)).

For these reasons, if immigration enforcement truly affected worker safety through employers' response to workers' willingness to complain, we would expect these effects to be less pronounced in unionized

workplaces.

To investigate if unions influence the effect of Secure Communities on worker complaints and injuries, we separately calculate the number of complaint-driven and injury-driven inspections in each county-industry-year for unionized or nonunionized workplaces. During our sample period, roughly 16 percent of both complaint-driven and injury-driven OSHA inspections occurred at unionized workplaces.²¹

Table 4 shows the effect of Secure Communities on worker complaints and inspections separately for nonunionized and unionized workplaces. Column 1 shows that Secure Communities reduced complaints at nonunionized workplaces with 100 percent Hispanic workers by 53 percent ($p < 0.01$). Column 2 shows that the effect is small and statistically insignificant (-0.05 , $p = 0.72$) for unionized workplaces. The two coefficients are statistically significantly different from each other ($p = 0.02$).²² Column 3 shows that Secure Communities significantly increased injuries at nonunionized workplaces with high Hispanic workforce shares by 29 percent ($p = 0.02$). The effect at unionized workplaces is also positive, but smaller and statistically insignificant (0.1 , $p = 0.25$). The difference between the two coefficients is statistically insignificant ($p = 0.2$). We get qualitatively similar results when we measure the dependent variables as rates per 10,000 workers instead as the inverse hyperbolic sine of counts, similar to the specification used in Table B5. Table C1 shows that for this specification, the difference in the effect on injuries for unionized and nonunionized workplaces is statistically significant ($p = 0.013$).

5.3 Immigration Enforcement Decreased Compliance with the Minimum Wage

Our results revealed that Secure Communities led to an increase in injury rates for workplaces with high shares of Hispanic workers. We interpret this as a result of employers reducing inputs into safety in response to a lower probability of worker complaints. Prior work shows that workplace injuries are a function of employers' inputs and resources (Cohn and Wardlaw, 2016; Charles et al., 2019); however, they are also a function of worker behavior. In theory, Secure Communities could have affected these worker-related inputs by increasing stress among workers or by creating a change in the composition of workers. Such effects challenge our interpretation of the results.

To address this concern, we examine if Secure Communities affected another dimension of working conditions: compliance with minimum wage laws. Employer noncompliance with minimum wage laws is surprisingly common (Ashenfelter and Smith, 1979) and is influenced by the policy environment (Galvin, 2016; Clemens and Strain, 2020). The government agency that monitors and enforces compliance with

²¹A limitation of this test is that we do not observe the Hispanic workforce share separately for unionized and nonunionized workplaces. However, the unionization rates of Hispanic and non-Hispanic workers are quite similar. During our observation period, the average unionization rate was around 9.9 percent among Hispanic workers and 11.6 percent among non-Hispanic white workers (U.S. Bureau of Labor Statistics, 2019).

²²We obtain this p-value from a fully interacted model that estimates the effect for unionized and nonunionized workplaces in a single regression that yields identical point estimates and standard errors as two split sample models we report in the table. We report the split sample estimates in the table for ease of exposition.

Table 4: Unions Prevented Effects of Secure Communities on Worker Complaints and Workplace Injuries

Dependent variable:	Inverse hyperbolic sine of			
	Complaint-driven inspections		Injury-driven Inspections	
Sample:	Nonunionized	Unionized	Nonunionized	Unionized
	(1)	(2)	(3)	(4)
SC	0.088*** (0.033)	0.032 (0.023)	-0.055*** (0.021)	-0.007 (0.011)
SC×Hispanic share	-0.53*** (0.18)	-0.05 (0.15)	0.29** (0.12)	0.10 (0.09)
p-value on difference:	0.02		0.2	
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓
Hispanic share × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
OSHA region × year FE	✓	✓	✓	✓
OSHA jurisdiction × year FE	✓	✓	✓	✓
Mean Dep Var (in levels)	3.80	0.84	1.17	0.25
# Observations	216,411	216,411	216,411	216,411

Note: * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of inspections in nonunionized (columns 1 and 3) and unionized (columns 2 and 4) workplaces on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share, and the controls and fixed effects indicated in the bottom panel.

minimum wage laws, the WHD of the Department of Labor, relies on worker complaints to target its enforcement even more than OSHA does: worker complaints trigger over 75 percent of WHD inspections (Weil and Pyles, 2005). If Secure Communities also reduced Hispanic workers’ willingness to complain about minimum wage violations, it could have increased the risk that these workers face minimum wage violations.

Following prior work, we use the Current Population Survey (CPS) Outgoing Rotation Group (ORG) to measure workers’ wages and compliance with minimum wage laws. For workers who report being paid hourly, the CPS directly asks the worker for their hourly wage. For other workers, we estimate a worker’s hourly wage by dividing their weekly earnings by their reported usual weekly hours. Each respondent also reports demographics like gender, age, race, and ethnicity, and other variables such as their education. Only roughly 40 percent of CPS ORG observations include the respondent’s county. For the remaining respondents, the lowest level of geography reported is at a broader level such as Core-Based Statistical Area or state. For those respondents for which we observe the county, we merge in the variable capturing a county’s yearly Secure Communities participation with the ORG dataset.

We use each state’s effective minimum wage to create a variable equal to 1 if a worker’s reported hourly wage is below the state’s minimum wage (*Below*). There is very likely measurement error in reported wages, and thus in our variable *Below*. We take two steps to address this error. First, we use an indicator if a worker’s reported wage is at least \$0.25 below the minimum wage as our dependent variable. Second, we use *Below* but restrict the sample to hourly workers, who directly report their wage to the CPS.

We estimate the following linear probability model for worker i in county c , census region r , year t , and quarter q :

$$Below_{ict} = \beta_1 SC_{ct} + \beta_2 SC_{ct} \times Hispanic_i + \beta_3 MW_{c(s)t} + \Gamma \mathbf{X}_{it} + \zeta_c + \theta_{rtq} + \epsilon_{ict}. \quad (4)$$

Our coefficient of interest is β_2 . It reflects the effect of Secure Communities on minimum wage violations among Hispanic workers relative to their non-Hispanic counterparts. We control for the state’s effective minimum wage ($MW_{c(s)t}$), and a vector of individual controls X_{it} that includes if an individual identifies as Hispanic or LatinX, their reported race (white, Black, with not white or Black as the omitted group), female, age, age squared, and if they are a member of a labor union. We include fixed effects for a respondent’s county (ζ_c) and for census region-year-quarter (θ_{rtq}). Following Galvin (2016), we restrict the sample to workers in the bottom quintile of the wage distribution in their state-year in order to focus on workers most at risk of a minimum wage violation. We cluster standard errors by county.

We report our estimates in Table 5. Columns 1 and 2 show results for regression on all workers; columns 3 and 4 show results for regressions on hourly workers only. Column 1 indicates that Secure Communities increased the probability that a Hispanic worker was paid below the minimum wage by 1.2 percentage points ($p = 0.05$), relative to the effect on a non-Hispanic worker. This represents a 6 percent increase relative to

Table 5: Secure Communities Led to Minimum Wage Violations for Hispanic Workers

Dependent variable:	Indicator for wage below			
	Min Wage	(Min Wage - 0.25)	Min Wage	(Min Wage - 0.25)
Sample:	All workers		Hourly workers	
	(1)	(2)	(3)	(4)
SC	0.005 (0.011)	0.014 (0.011)	-0.001 (0.010)	0.005 (0.009)
Hispanic=1 × SC	0.012** (0.006)	0.008 (0.005)	0.017*** (0.006)	0.012*** (0.005)
County FE	✓	✓	✓	✓
Census region × year × quarterFE	✓	✓	✓	✓
Mean Dep Var	0.20	0.17	0.14	0.11
# Observations	155,752	155,752	125,451	125,451

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results from the CPS ORG of regressing one of the various measures of a worker being paid less than the effective state minimum wage, on an indicator for a county participating in Secure Communities, the interaction of this indicator with an indicator that a worker identifies as Hispanic, the state minimum wage, worker demographics (indicators if a respondent identifies as female, as Hispanic, as Black, and as white), the respondent's age and age-squared, and if the worker is in a labor union, as well as the fixed effects indicated in the bottom panel. The sample is workers whose reported wage is in the bottom quintile of the wage distribution in their state-year.

the sample mean. In column 2, the dependent variable is equal to 1 if the respondent's wage is at least 0.25 below the minimum wage. The point estimate attenuates slightly in magnitude and barely loses statistical significance ($p = 0.12$), but as a percent of the sample mean it is quite similar to column 1 ($0.008/0.17 = 4.7$ percent). Restricting the sample to hourly workers (those workers for which measurement error is less of a concern) in columns 3 and 4 slightly increases the magnitude of the estimates. The estimate in column 3 implies that Secure Communities led to a 1.7 percentage point increase in the probability a Hispanic worker was paid below the minimum wage ($p < 0.01$), representing a 12 percent increase relative to the sample mean. The result changes little in column 4.

Across each of these four specifications, the estimated coefficient of SC —representing the effect of Secure Communities on non-Hispanic workers—is close to zero and not statistically significant.

These results corroborate and generalize our main findings on the effect of immigration enforcement on workplace safety. We find that Secure Communities led to worse working conditions for Hispanic workers using a different dataset, a different outcome, and a different source of variation (based on individual workers' reported ethnicity rather than county-industry average worker demographics). By reducing workers' willingness to complain to government agencies, Secure Communities led to employer responses that deteriorated working conditions.

6 Why Are Injuries So Responsive to Workers' Propensity to Complain?

Our results thus far have demonstrated that Secure Communities led to a large decrease in complaints to OSHA and a large increase in injuries (and minimum wage violations) among workplaces with Hispanic workers. We provided evidence that this combination of effects is best explained by employers adjusting their inputs into safety in response to workers' reduced propensity to complain. If no other factors influenced these changes, we can use the relative magnitude of the effects of Secure Communities on complaints and injuries to roughly infer how workers' risk of injury relates to their propensity to complain. Focusing on the estimates in columns 1 and 4 of [Table 1](#), taking the ratio of these two magnitudes implies that the elasticity of Hispanic workers' injury risk with respect to their propensity to complain is 0.47.²³

This is a big effect. While it may be straightforward that workers' propensity to complain directly affects their risk of injury, it might seem surprising that the effect is so large. After all, employers have other incentives to limit workplace injuries. Employers' workers' compensation premiums increase when their employees experience injuries ([Moore and Viscusi, 1989](#)). Labor market competition should also ensure that employers have to pay higher wages for workers to accept higher injury risk ([Rosen, 1986](#)). Given these factors, is it plausible that Hispanic workers' risk of injury would be so responsive to their threat of complaining? In this section, we provide some qualitative and quantitative evidence to answer this question.

Evidence suggests that the disciplinary forces of workers' compensation and labor market competition might be more muted for employers of Hispanic workers and especially undocumented workers. For example, a nontrivial share of workers injured on the job do not file for workers' compensation even when they are eligible ([Fan et al., 2006](#)). Conditional on getting injured, Hispanic workers are less likely to file for workers' compensation than their non-Hispanic counterparts ([Dong et al., 2007](#); [Friedman et al., 2012](#)). All else equal, injuries are less expensive to employers when their injured workers do not file for workers' compensation, since their premiums do not rise to reflect the true number of injuries.

Furthermore, the disciplinary force of labor market competition on workers' injury risk might be more muted among Hispanic workers, undocumented or otherwise. Undocumented workers have a particularly weak outside option. They face higher search frictions on the labor market because fewer employers are allowed and willing to hire them, and they are ineligible for social insurance like unemployment insurance if they lose their job. Citizen and documented Hispanic workers might also be more reluctant to leave their jobs given a deterioration in working conditions: conditional on job loss, Hispanic workers are less likely to take-up unemployment insurance than non-Hispanic workers ([Nichols and Simms, 2012](#)). This lower outside option value plausibly mitigates the extent to which labor market competition compels employers of Hispanic workers to ensure safe and healthful working conditions. Through the lens of ([Hirschman, 1970](#)), a reduced potency of the threat of leaving one's job (the "exit" option) increases the value of complaining

²³We obtain this elasticity by dividing the sum of the coefficients of *SC* and *SC* × *Share Hispanic* for injury-driven inspections in column 4 of [Table 1](#) by the sum of the coefficients of *SC* and *SC* × *Share Hispanic* for total complaints in column 1: $(-0.060+0.31)/(0.070-0.60)=0.471$). Because the dependent variable in these regressions is an inverse hyperbolic sine, these coefficients approximately represent percent changes.

(the “voice” option) as a means for a worker to respond to a deterioration in working conditions.

We provide suggestive quantitative evidence of this reduced potency of the “exit” option for Hispanic workers in [Table 6](#). We use data from the QWI (described in [Section B1](#)), which contains quarterly data on various aspects of employment dynamics at the county-industry level. The QWI disaggregates these data by worker ethnicity. We measure the following three variables, separately for Hispanic workers and non-Hispanic workers: beginning-of-quarter employment, the number of worker separations (scaled by initial employment), and the “stable” worker turnover rate. The stable turnover rate is the rate at which stable jobs begin and end.²⁴ To estimate the effect of Secure Communities on these outcomes, we run a similar regression to [Equation \(1\)](#) except that we remove the variable $SC_{ct} \times Exposure_i$. Because we observe the above outcomes directly for Hispanic workers, we can estimate a standard difference in difference model. Specifically, we estimate the following model:

$$asinh(Y_{cit}) = \beta_1 SC_{ct} + \zeta_{ci} + \eta_{it} + \theta_{rt} + \epsilon_{cit}, \quad (5)$$

where all fixed effects and symbols are the same as in [Equation \(1\)](#).²⁵

Column 1 of [Table 6](#) shows results for a regression equation where the dependent variable is the inverse hyperbolic sine of the start-of-quarter number of Hispanic employees. We estimate that Secure Communities reduced the Hispanic workforce by 2.1 percent ($p < 0.01$). This estimate corroborates the research by [East et al. \(2012\)](#), who find that Secure Communities led to lower employment rates for both undocumented workers and citizen workers. In column 2, the dependent variable is the number of job separations (voluntary or involuntary) of Hispanic workers, scaled by the start-of-quarter total employment of Hispanic workers. If the “exit” option is readily available to Hispanic workers, Secure Communities should have a large effect on this variable. We would expect to see a large share of Hispanic workers quit their job in response to the increase in injury risk that we documented in [Section 4.2](#). The coefficient estimate in column 2 is positive and statistically significant, but very small (0.004, or 1.6 percent of the sample mean). In column 3, the dependent variable is the stable turnover rate for Hispanic workers.²⁶ The coefficient on SC is *negative* ($-0.002, p < 0.01$), but very small (1.7 percent of the sample mean). This result implies that Secure Communities led to a small *decrease* in job churn among Hispanic workers. Overall, these results collectively indicate that “exit” was not a viable option for Hispanic workers who faced deteriorating working conditions.

²⁴We calculate this variable by summing the number of stable hires in the reference quarter and stable separations in the next quarter, and dividing by the average full-quarter employment. “Stable” hires and separations are those that pertain to a job that a worker had for at least one full quarter.

²⁵We omit the OSHA jurisdiction-year fixed effects, but our results do not change if we include it.

²⁶Note that the “stable separations” used in this measure differs from the “separations” variable in column 2, which is equal to the “number of workers whose job with a given employer ended in the specified quarter” and thus includes separations QWI does not consider “stable.”

Table 6: Secure Communities Had Negligible Effects on Hispanic Workers Employment and Job Turnover

Dependent variable:	Inverse hyperbolic sine of Hispanic employment (1)	Hispanic separation rate ^a (2)	Hispanic stable turnover rate ^b (3)
SC	−0.021*** (0.005)	0.004*** (0.001)	−0.002*** (0.001)
County × industry FE	✓	✓	✓
Industry × year FE	✓	✓	✓
OSHA region × year FE	✓	✓	✓
Mean Dep Var (in levels)	3,470.71	0.25	0.12
# Observations	208,713	186,622	148,232

*Note:** p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results from regressions where of the dependent shown in the column header on an indicator for a county participating in Secure Communities and the fixed effects indicated in the bottom panel.

^aNumber of Hispanic workers whose job ended in the relevant quarter, divided by the total number of Hispanic workers at the start of the relevant quarter.

^b The sum of the number of stable hires of Hispanic workers in the reference quarter and stable separations of Hispanic workers in the next quarter, divided by the average full-quarter employment of Hispanic workers.

This small and negative effect on turnover also illustrates that the effects of Secure Communities on complaints and injuries cannot simply be explained by changes in worker composition. Inexperienced workers are more likely to get injured on the job (Breslin and Smith, 2006). A large positive effect of Secure Communities on turnover could imply that changes in worker composition, not changes in employer inputs, explain why Secure Communities led to more workplace injuries. The estimates in this table do not support this explanation.

In sum, Secure Communities substantially deteriorated working conditions for Hispanic workers, but this deterioration did not usher in a wave of worker separations or job churn. This nonresponse of “exit” illustrates why changes to workers’ ability to exercise “voice,” via complaints, had such a big effect on their risk of injury and their risk of minimum wage violations.

7 Conclusion

A labor market regulatory regime that relies on worker complaints to target its enforcement resources has appealing characteristics. Because workers are more likely than government bureaucrats to know where working conditions are unsafe or illegal, complaints can serve as an effective form of decentralized targeting. However, if the workers at higher risk also face higher barriers to complain, worker complaints might not be an effective signal for illegal or unsafe working conditions. In this case, complaint-based enforcement can

even exacerbate existing inequalities in the labor market. We presented descriptive evidence consistent with this trade-off. While worker complaints direct occupational safety and health inspections to more dangerous workplaces on average, this relationship reverses when workers face high barriers to complain. Workplaces with large shares of Hispanic, noncitizen, or Hispanic noncitizen workers experience more injuries, but their workers are less likely to complain.

We showed that an exogenous change in the barriers to complain—an increase in local immigration enforcement caused by the onset of Secure Communities—substantially reduced complaints to OSHA among workplaces with Hispanic workers but increased workplace injuries. We provided evidence that employers’ response to a decrease in worker complaints drives this deterioration in workplace safety. Finally, we presented suggestive evidence for why workplace injuries are so responsive to changes in workers’ willingness to complain: Secure Communities had a negligible impact on Hispanic workers’ job separation or turnover rates.

Our study builds on prior work that documents the barriers that workers face in exercising their rights to complain. We also reveal that these barriers lead to economically meaningful disparities in working conditions—in both rates of serious workplace injuries and noncompliance with the minimum wage. Our finding that unionized workers appeared immune to the increase in barriers to complain provides new evidence of how unions facilitate the enforcement of labor regulations ([Morantz, 2018](#)).

Our findings imply that local immigration enforcement reduces the effectiveness of worker complaints as signals for hazardous working conditions. If oversight agencies want to use worker complaints to target their enforcement, they must take into account workforce demographics and factors that change workers’ willingness to complain. For example, agencies could seek to develop ways to enable workers to issue complaints in a way that is truly anonymous and hidden from their employers. Additionally, agencies could provide formal guarantees for immigrant workers that filing a complaint will not trigger an investigation into their or their family members’ immigration status. Given the widespread fear of immigration enforcement, however, it may be challenging to elicit worker trust in such mechanisms.

We focused our analysis on the context of immigration enforcement. It is plausible that the implications of our work extend to other sources of barriers to complain, such as workers’ union status or the health of the local labor market. We encourage future work to examine the implications of complaint-based enforcement in these settings.

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Appendix

A Data

A1 Secure Communities Roll-out

Figure A1 illustrates the roll-out of Secure Communities. The program started in Harris county (Texas) on October 27, 2008. Thirteen other counties followed in 2008. In 2009, 91 counties were added, mostly at the U.S.-Mexican border. The biggest expansion took place in 2011 and 2012, when 1,098 and 1,016 counties joined the program, respectively. By January 2013, all U.S. counties were participating in the program.

Figure A1 makes clear that Secure Communities started in counties at the Mexico-U.S. border and counties at the Gulf of Mexico. These counties also tend to have a higher Hispanic workforce share, as Figure A3 shows. We formally investigate the correlates of early program adoption in Table A1, where we assess the factors that predict county participation in Secure Communities within the first year of the program. Counties with a larger population, counties with a larger relative Hispanic or foreign-born noncitizen population and counties close to Mexico were more likely to adopt Secure Communities early. Previous participation in a 287(g) partnership is also significantly positively correlated with an early Secure Communities start.²⁷ These results corroborate observations by Cox and Miles (2013), who find that the federal government chose early adopter counties based on their proximity to Mexico and the size of their Hispanic population. We account for the selection into early adoption of Secure Communities in our empirical strategy by including county-industry fixed effects in all regressions and by conducting several robustness checks.

A2 Hispanic Workforce Shares

We obtain the share of workers who are Hispanic for each industry and Public Use Microdata Area (PUMA) in 2005, 2006, and 2007 from the 1 percent sample of the ACS (U.S. Census Bureau, 2020). We operationalize “industry” roughly as two-digit NAICS codes. We connect counties to PUMAs using the 2000–2010 PUMA crosswalk from the Integrated Public Use Microdata Series.²⁸ For each year, we then take the average of the Hispanic workforce share over all PUMAs that belong to or comprise a county, weighted by the county’s population in each PUMA.²⁹ Finally, we average shares over 2005–2007. This process

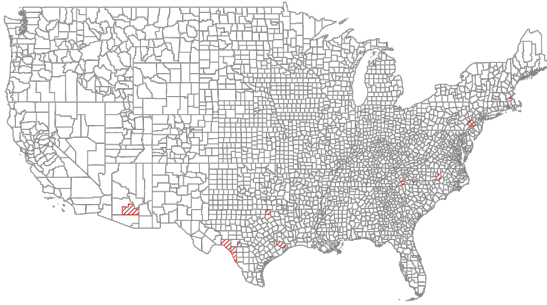
²⁷287(g) partnerships, which began in 2003, are another local immigration enforcement program that authorize officers of participating local law enforcement agencies to enforce federal immigration law. We do not use participation in 287(g) partnerships as a measure of local immigration enforcement for two reasons. First, unlike Secure Communities, participation in the 287(g) program is voluntary. Law enforcement agencies in counties apply for a partnership, and ICE then decides whether to approve the application. As a result, there is substantial selection into 287(g) participation, which makes it challenging to find a suitable comparison group to identify the program’s causal effect. Second, 287(g) operated on a relatively small scale. Only 182 counties applied for a 287(g) partnership between 2005 and 2011, and of these ICE approved 82 for a partnership.

²⁸Available at <https://usa.ipums.org/usa/volii/pumas10.shtml#crosswalk>.

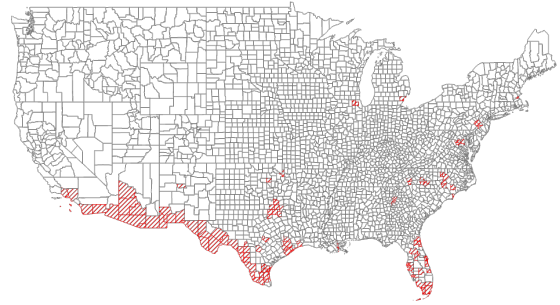
²⁹Some PUMAs are larger than counties while some are smaller. PUMA and county borders can intersect.

Figure A1: Roll-out of Secure Communities
Red hatches indicate that county participates in Secure Communities

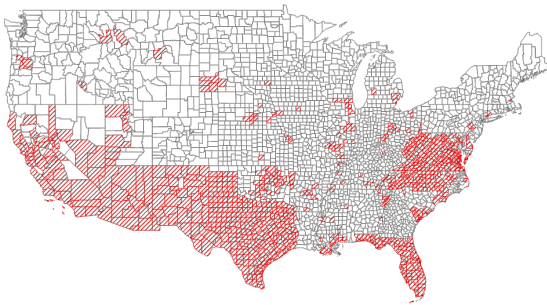
2008



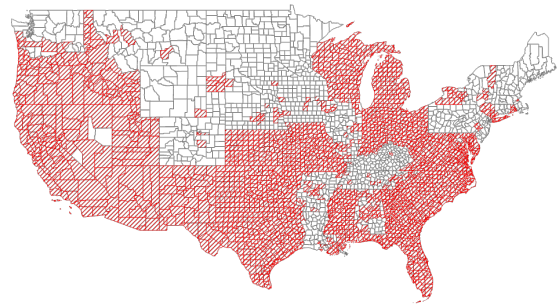
2009



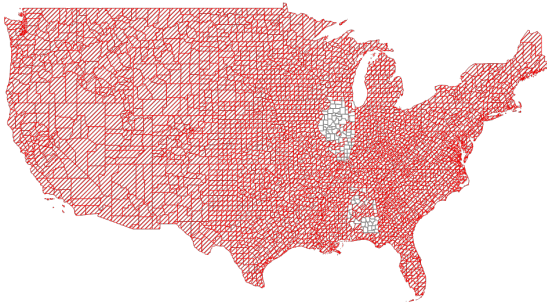
2010



2011



2012



2013

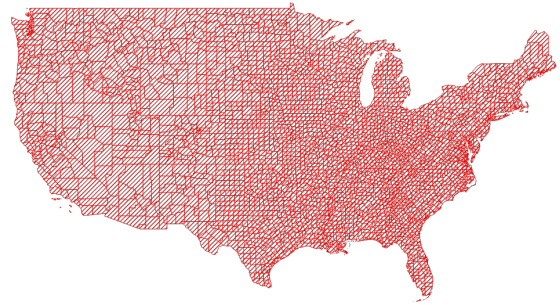


Table A1: Correlates of Early Secure Communities Roll-out

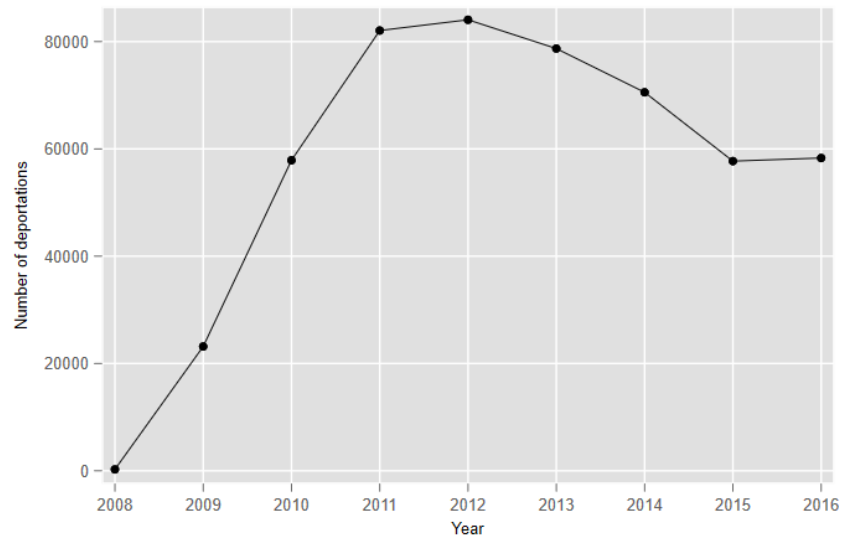
Dependent variable: County participates in SC in first year		
	(1)	(2)
Log(Population 2000)	0.006** (0.002)	0.003 (0.002)
Percent population Hispanic 2000	0.001*** (0.000)	
Percent population noncitizens 2000		0.002*** (0.001)
Percent population Black 2000	0.000** (0.000)	-0.000 (0.000)
County is at Mexican border	0.053*** (0.013)	0.059*** (0.014)
County is at Gulf of Mexico	0.030*** (0.009)	0.034*** (0.009)
Distance from county centroid to Mexican border in 100km	0.000 (0.000)	-0.002*** (0.000)
Metro area	0.013* (0.008)	0.013* (0.007)
Had 287(g) at SC start	0.046*** (0.008)	0.053*** (0.009)
Index crimes per 100 in 2000	0.001 (0.001)	0.003** (0.001)
Republican vote share in 2000 Presidential election	0.000 (0.000)	-0.000** (0.000)
Log(Median hh income 2000)	0.017 (0.015)	-0.001 (0.015)
Unemployment rate 2000	-0.002 (0.002)	-0.000 (0.002)
Pseudo R2	0.48	0.43
N	2,955	2,955

Note: * p<0.10, ** p<0.05, *** p<0.01. Table shows average marginal effects from probit regressions. The outcome variable is an indicator for a county participating in Secure Communities within the first year of program start. Three counties are excluded since they have missing values on index crimes.

yields a dataset containing the pre-Secure Communities Hispanic workforce share for each county-industry combination.

Hispanic workforce shares vary widely across both counties and industries. [Figure A3](#) shows the spatial distribution of the Hispanic workforce share in agriculture, construction and manufacturing. There are clear differences in the Hispanic workforce share within industries, across counties. For example, 0 percent of

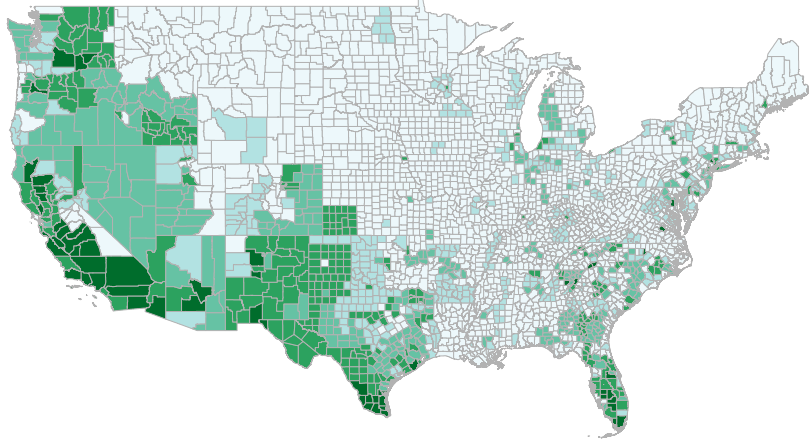
Figure A2: Removals under Secure Communities



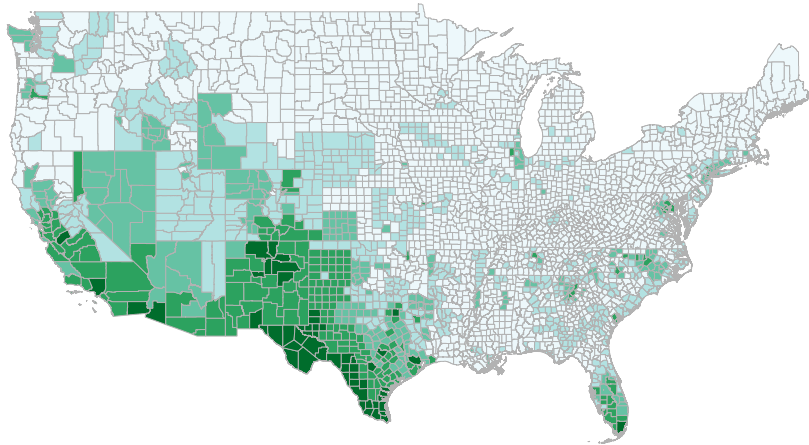
workers in the construction sector are Hispanic in Decatur county, Indiana, whereas 86 percent of workers in construction are Hispanic in El Paso County, Texas. The variation within counties across industries is smaller but still pronounced. In Kern County, California, 85 percent of workers in agriculture are Hispanic, compared to 50 percent of workers in construction.

Figure A3: Spatial Distribution of Hispanic Workforce Share

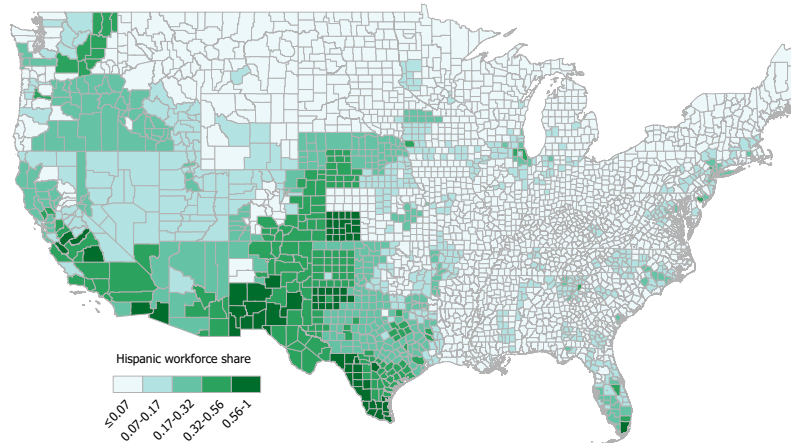
(a) Agriculture



(b) Construction



(c) Manufacturing



B Robustness Checks

B1 Addressing Measurement Error in Workforce Demographics with Instrumental Variables

One concern with our approach to measuring the Hispanic workforce share of county-industries is that we are using a sample statistic, the share of individuals in a 1 percent representative sample of the U.S. (the 2005–2007 ACS) that work in a PUMA-industry and are Hispanic, to infer a population statistic, the share of total workers in that PUMA-industry that are Hispanic. In theory, this measurement error leads to attenuation bias in our regression estimates. In practice, we expect this concern to be minimal because we weight our regressions by each county-industry’s initial employment. In this section, we use an instrumental variables approach to check if the measurement error does not affect our estimates.

Let $Hispanic_Share^*$ denote the true Hispanic workforce share for a county-industry in 2008 (the year before Secure Communities began). Let $Hispanic_Share^{ACS}$ denote our measurement of $Hispanic_Share^*$ that we obtain from the ACS and that is measured with error.

We can express the measured value $Hispanic_Share^{ACS}$ as the sum of the true value and random measurement error η :

$$Hispanic_Share^{ACS} = Hispanic_Share^* + \eta \quad (6)$$

Suppose we have a second measurement of $Hispanic_Share^*$, denoted by w , with its own independent error ν , *i.e.*

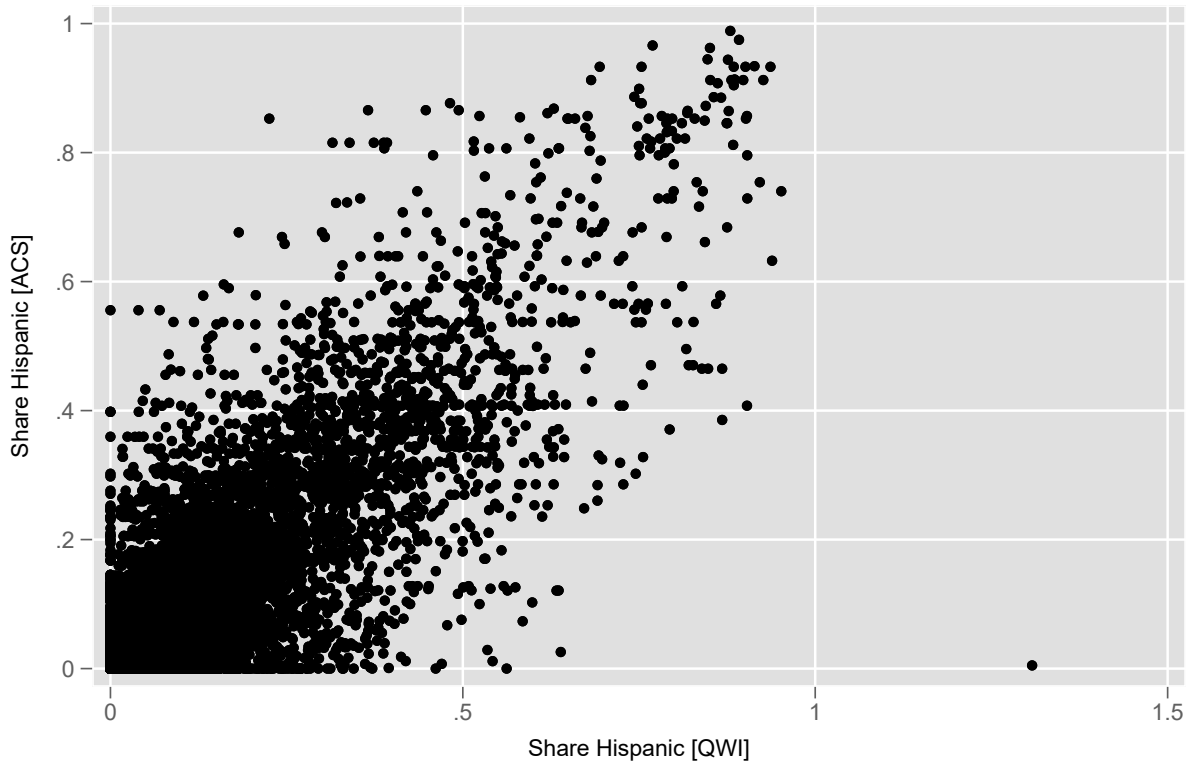
$$w = Hispanic_Share^* + \nu \quad (7)$$

Then, as long as w is mean-independent of both the error in the estimating equation (ϵ) and the error in the first measure (η), we can eliminate the attenuation bias by using w as an instrument for $Hispanic_Share^{ACS}$ (Wooldridge, 2016).

We use the annual share of the workforce that is Hispanic or Latino by county-industry from the QWI as the instrument w and denote it with $Hispanic_Share^{QWI}$. The QWI, released by the U.S. Census Bureau and based on the Longitudinal Employer-Household Dynamics dataset, provides detailed labor market statistics by geography, industry, worker demographics and other characteristics. The QWI’s measure of the Hispanic workforce per county-industry comes from the Social Security Administration’s Numident file and other sources that are all unrelated to the ACS (Abowd et al., 2009).

Figure B1 illustrates the relationship between $Hispanic_Share^{ACS}$ and $Hispanic_Share^{QWI}$. The two measures are highly correlated, but they also exhibit substantial differences. This relationship provides

Figure B1: The Relationship between the Measure of the Share of the Workforce That Is Hispanic by County-Industry from the ACS versus the Same Measure from the QWI



Note: The figure shows a scatterplot comparing the measure of a county-industry's workforce that is Hispanic in 2008 obtained from the ACS (y-axis) and from the QWI (x-axis). The sample is all county-industries in our main analysis.

supporting evidence that $Hispanic_Share^{QWI}$ is a valid instrument for $Hispanic_Share^{ACS}$.

We estimate a variation of Equation (1), instrumenting $exposure_{ci}$ — the Hispanic workforce share from the ACS — with $Hispanic_Share^{QWI}$ and report the results in Table B1. Columns 1 and 2 show the results for worker complaints (we use complaint-driven inspections as our measure here), and columns 3 and 4 show the results for workplace injuries. In column 1, we simply replace the Hispanic workforce share from the ACS with the measure from the QWI and reestimate Equation (1), analogous to the “reduced form” in an IV regression.³⁰ The point estimate on the coefficient of interest, $SC \times Hispanic\ share$, -0.51 ($p < 0.01$) is slightly larger but close to the estimate we obtain when we use the Hispanic workforce share from ACS ($-0.39, p < 0.01$). In column 2, we report the second stage from an IV regression where we instrument the ACS Hispanic workforce share with the QWI Hispanic workforce share. The estimate is the same as in the reduced form regression in column 1.

Columns 3 and 4 show results of reduced form and IV regressions where the dependent variable is

³⁰The sample size in these regressions (208,240) is slightly smaller than that in our baseline analysis (216,411) because a few county-industries do not have data in the QWI file.

Table B1: Instrumental Variable Regressions Show That Measurement Error Does Not Substantially Affect Our Estimates

Dependent variable: Model:	Inverse hyperbolic sine of:			
	Complaint-driven inspections		Injury-driven inspections	
	OLS (1)	IV (2)	OLS (3)	IV (4)
SC	0.101*** (0.034)	0.100*** (0.034)	-0.062*** (0.022)	-0.062*** (0.022)
SC×Hispanic share [QWI]	-0.56*** (0.19)		0.29** (0.13)	
SC × Share Hispanic workers		-0.55*** (0.18)		0.30** (0.13)
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓
Hispanic workforce share × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
OSHA region × year FE				
OSHA jurisdiction × year FE	✓	✓	✓	✓
# Observations	208,240	208,240	208,240	208,240

Note: * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of complaint-driven inspections (columns 1 and 2) or the number of injury-driven inspections (columns 3 and 4) on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share, and the controls and fixed effects indicated in the bottom panel. In columns 1 and 3, the Hispanic workforce share is from the QWI. In columns 2 and 4, the Hispanic workforce share from the ACS is instrumented with the Hispanic workforce share from the QWI. Asinh stands for inverse hyperbolic sine.

the inverse hyperbolic sine of the number of injury inspections. Column 3 shows that replacing the ACS Hispanic workforce share with the QWI share hardly changes the effect estimate (0.17, $p = 0.09$ with QWI, compared to 0.18, $p = 0.028$ with the ACS). Column 4 shows that the IV estimate (0.18, $p = 0.06$) is the same as the main estimate from Table 1. These results suggest that measurement error does not affect the estimates in our setting.

B2 Robustness to Confounding Factors and Specification Choices

B2.1 Addressing Counties' Selection into Secure Communities and Other Sample Choices

We first test if selection into early adoption confounds our estimates by reestimating our regression equation on two subsamples. First, we drop all counties at the Mexico-U.S. border or the Gulf of Mexico. Second,

Table B2: Results Are Robust to Alternative Sample Choices

	(1) Baseline	(2) Drop border & Gulf counties	(3) Drop pre-2010 adopters	(4) Drop high Hispanic workforce share counties	(5) Drop high Hispanic population counties
Panel A: Complaint-driven inspections					
SC	0.097*** (0.033)	0.099*** (0.036)	0.092*** (0.034)	0.102*** (0.036)	0.102*** (0.034)
SC×Hispanic share	-0.49*** (0.18)	-0.55** (0.22)	-0.36** (0.16)	-0.55** (0.22)	-0.60*** (0.18)
Panel B: Injury-driven inspections					
SC	-0.060*** (0.021)	-0.057** (0.022)	-0.070*** (0.022)	-0.064*** (0.022)	-0.059*** (0.021)
SC×Hispanic share	0.31** (0.13)	0.25 (0.15)	0.32** (0.13)	0.29** (0.14)	0.30** (0.13)
Asinh(programmed inspections)	✓	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓	✓
Hispanic share × year	✓	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓	✓
OSHA region × year FE	✓	✓	✓		
OSHA jurisdiction × year FE	✓	✓	✓		
# Observations	216,411	211,063	215,375	246,367	215,753

* p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of complaint-driven inspections (Panel A) or injury-driven inspections (Panel B) on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share, and the controls and fixed effects indicated in the bottom panel. Asinh stands for inverse hyperbolic sine.

we drop any county that adopted Secure Communities in its first year, 2008. Columns 2 and 3 of [Table B2](#) show that our results are robust. [Figure 2](#), under the header “Sample restrictions,” illustrates it.

Second, we check if our results are driven by a few county-industries with a large Hispanic workforce share. Column 4 of [Table B2](#) shows results for a sample where we dropped the ten counties with the highest Hispanic workforce share within each industry. Column 5 shows results for a sample where we dropped any county that lies in or contains 1 of the 10 PUMAs with the largest Hispanic population in 2005, 2006, or 2007.

B2.2 Assessing Sensitivity to Confounding Factors

We next check if our results are robust when we control for potentially confounding factors. [Table B3](#) and [Figure 2](#) (under header “Change in controls”) show the results. First, we include the inverse hyperbolic sine of the number of establishments as a control (column 2). Second, we include the following additional controls: the log county population, the county’s Hispanic population share, the county unemployment rate, the county poverty rate, the log median household income, the Republican vote share in the preceding presidential election, and a measure of county participation in the 287(g) program (column 2).

Third, we follow [East et al. \(2012\)](#) and include a “Bartik”-style measure of labor supply in our re-

Table B3: The Estimated Effects of Secure Communities Are Robust to Controlling for Confounding Factors

	(1) Baseline	(2) Control for establishments	(3) Extra controls	(4) Bartik + controls	(5) Census region-year FE	(6) State-year FE	(7) County-year FE
Panel A: Complaint-driven inspections							
SC	0.097*** (0.033)	0.097*** (0.033)	0.098*** (0.033)	0.098*** (0.033)	0.113*** (0.036)	0.045 (0.035)	
SC×Hispanic share	-0.49*** (0.18)	-0.49*** (0.18)	-0.48*** (0.17)	-0.47*** (0.17)	-0.48*** (0.18)	-0.47*** (0.17)	-0.50 (0.34)
Panel B: Injury-driven inspections							
SC	-0.060*** (0.021)	-0.060*** (0.021)	-0.055*** (0.021)	-0.055** (0.021)	-0.048** (0.022)	-0.033 (0.021)	
SC×Hispanic share	0.31** (0.13)	0.31** (0.13)	0.31** (0.13)	0.30** (0.13)	0.37*** (0.13)	0.26** (0.13)	0.22 (0.32)
Asinh(programmed inspections)	✓	✓	✓	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓	✓	✓	✓
Asinh(establishments)		✓					
Controls			✓	✓			
Bartik measure				✓			
Hispanic share × year	✓	✓	✓	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓	✓	✓	✓
OSHA region × year FE	✓	✓	✓	✓			
OSHA jurisdiction × year FE	✓	✓	✓	✓			
Census region × year FE					✓		
State × year FE						✓	
County × year FE							✓
# Observations	216,411	216,411	216,331	216,179	216,411	216,411	215,458

Note: * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of complaint-driven inspections (Panel A) or injury-driven inspections (Panel B) on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share and the controls and fixed effects indicated in the bottom panel. Asinh stands for inverse hyperbolic sine. Controls in columns 3 and 4 include the log county population, the Hispanic population share, the county unemployment rate, the county poverty rate (in percent), the log median household income, the Republican vote share in the last presidential election and an indicator that the county participated in the 287(g) program. The county Bartik measure is the population-weighted average of the Public Use Microdata Area (PUMA) Bartik measure. The PUMA Bartik measure is the 2005 share of the PUMA's industry employment in total PUMA employment, multiplied by the nationwide change in industry employment each year.

gressions to address potentially confounding effects of the Great Recession (Bartik, 1992) (column 4). The county Bartik measure is the population-weighted average of the PUMA Bartik measure. The PUMA Bartik measure is the 2005 share of the PUMA's industry employment in total PUMA employment, multiplied by the nationwide change in industry employment each year. The measure captures the effect of labor demand-driven, industry-specific nationwide changes that affected county-industries differently depending on the industry's share in county employment before the recession.

Fourth, we use different sets of fixed effects. Recall that our main specification accounts for time-varying unobserved factors by including OSHA region-year fixed effects and OSHA jurisdiction-year fixed effects. As a robustness check, we instead use fixed effects at the level of (a) the census region-year (column 5); (b) the state-year (column 6); and (c) the county-year (column 7). Specification (c) is a very demanding test since all remaining identifying variation now comes from differences in the Hispanic workforce share across industries in the same county.

B2.3 Alternative Regression Specifications

Recall that our baseline regression is an OLS regression where the dependent variable is the inverse hyperbolic sine of complaint- or injury-driven inspections. Since the number of inspections is a count variable

bounded at zero, alternative choices could be more appropriate. We check the robustness of our results in three alternative specifications.

First, we use the log of the number of inspections plus one as the dependent variable and reestimate [Equation \(1\)](#). We add one to the number of inspections before taking the log to retain observations with zero inspections ([Table B4](#) column 1). Second, we use the inverse hyperbolic sine of the *rate* of inspections per 10,000 employees as the dependent variable and reestimate [Equation \(1\)](#) ([Table B4](#) column 2). Third, we estimate a Poisson regression instead of the OLS regression with the number of inspections as the dependent variable ([Table B4](#) column 3). [Figure 2](#) illustrates the point estimates and 95 percent confidence intervals of the coefficient on $SC \times Share\ Hispanic$ for the three specifications under the heading “Alternative specifications.”

Finally, we replicate [Table 1](#), except that we change the dependent variable to be the rate (per 10,000 employees) of complaints or injuries. We display the results in [Table B5](#). We do not include these coefficients in [Figure 2](#), since the coefficients in this specification represent *level* changes in complaints and injuries due to Secure Communities, whereas each of the coefficients in [Figure 2](#) represent approximate *percent* changes. We obtain qualitatively similar estimates as the baseline results using the inverse hyperbolic sine.

Table B4: Results Are Robust to Alternative Modelling Choices

Dependent variable:	Log of number of inspections	Inverse hyperbolic sine of inspections per worker	Number of inspections
Specification:	OLS (1)	OLS (2)	Poisson (3)
Panel A: Complaint-driven inspections			
SC	0.076*** (0.027)	0.117*** (0.028)	0.195*** (0.053)
SC×Hispanic share	-0.39*** (0.14)	-0.44*** (0.13)	-0.40** (0.18)
Panel B: Injury-driven inspections			
SC	-0.046*** (0.016)	-0.046*** (0.015)	0.015 (0.126)
SC×Hispanic share	0.25** (0.10)	0.23*** (0.09)	0.47 (0.31)
Programmed inspections	✓	✓	✓
Employment	✓	✓	✓
Hispanic share × year	✓	✓	✓
County × industry FE	✓	✓	✓
Industry × year FE	✓	✓	✓
OSHA region × year FE			
OSHA jurisdiction × year FE	✓	✓	✓
Mean Dep Var (in levels)	1.42	1.42	1.42
# Observations	216,411	216,411	81,928

Note: * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the dependent variable indicated in the top panel on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share, and the controls and fixed effects indicated in the bottom panel. In the log regressions (column 1), the controls for programmed inspections and employment are in logs. In the inverse hyperbolic sine regressions (column 2), the controls for programmed inspections and employment are in inverse hyperbolic sine. In the Poisson regression (column 3), the controls are the number of programmed inspections and employment in number of workers.

Table B5: Secure Communities Reduced Worker Complaints but Increased Workplace Injuries: Dep Var Measured as Rate per 10,000 workers

Dependent variable:	Rate (per 10,000 workers) of:			
	Total complaints (1)	Formal complaints (2)	Complaint-driven inspections (3)	Injury-driven inspections (4)
Panel A: Linear exposure measure				
SC	0.432*** (0.155)	0.127* (0.068)	0.459*** (0.111)	-0.087*** (0.030)
SC×Hispanic share	-2.44*** (0.78)	-1.22*** (0.37)	-1.87*** (0.58)	0.47** (0.21)
Panel B: Indicator for “high” exposure measure				
SC	0.310** (0.142)	0.065 (0.061)	0.387*** (0.106)	-0.061** (0.028)
SC×high Hispanic share	-0.88*** (0.30)	-0.37** (0.18)	-0.62*** (0.21)	0.13* (0.08)
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)				
Exposure measure × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
OSHA region × year FE	✓	✓	✓	✓
OSHA jurisdiction × year FE	✓	✓	✓	✓
Mean Dep Var (in levels)	6.33	1.56	2.85	0.64
Mean share Hispanic if high-Hispanic share=1	0.40	0.40	0.40	0.40
# Observations	216,411	216,411	216,411	216,411

Note: * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the rate (per 10,000 employees) of the number of worker complaints to OSHA (columns 1–3) or the number of OSHA inspections triggered by a serious worker injury (column 4) on an indicator for a county participating in Secure Communities, the interaction of this indicator with the exposure measure (Hispanic workforce share in Panel A, or indicator for high Hispanic workforce share in Panel B), and the controls and fixed effects indicated in the bottom panel. “High Hispanic share” is equal to 1 if the county-industry’s Hispanic share is above the 80th percentile..

B3 Assessing Potential Bias from Heterogeneous Treatment Effects

Goodman-Bacon (2018) revealed a potential bias in difference-in-difference (DD) estimates of the effect of a treatment that is adopted in a staggered fashion over time across space. A standard two-way fixed effects estimator yields a coefficient that represents a weighted average of all possible 2x2 DD estimators that compare timing groups to each other. Some of these groups include earlier-treated groups acting as a control for later-treated groups, which can cause bias if treatment effects change over time. We apply the decomposition from Goodman-Bacon (2018) to assess whether this potential bias meaningfully affects our results reported in Section 4.

We must make some adjustments to our main estimating equation in Equation (1) to apply the Goodman-Bacon decomposition. First, while we use a triple-difference design, the Goodman-Bacon decomposition applies only to a difference-in-difference (a double difference). Hence, we estimate the effect of Secure Communities separately for workplaces with a high Hispanic workforce share and workplaces with a low Hispanic workforce share, as described in Panel B of Table 1. Second, while our main results weight observations by county-industries' initial employment, we cannot include such weights using the Goodman-Bacon decomposition. Third, while we include a broad set of controls (for total employment and for OSHA programmed inspections) and fixed effects (e.g., for time-varying industry and regional shocks), we cannot include such controls in the Goodman Bacon decomposition. Fourth, while our *SC* variable used in the main analysis is equal to the fraction of the year that a county participated in Secure Communities, the Goodman-Bacon decomposition requires a 0/1 treatment variable. We thus recode *SC* to equal 1 if the county participated in Secure Communities for any part of the year.

We report results of this exercise in Table B6. To aid comparison, Panel A reports estimates of the effects of Secure Communities on our four main outcomes, separately for workplaces with high and low Hispanic workforce shares, using the same sets of fixed effects, controls, and weights as our main specification. The first row of Panel B reports similar estimates, but from a specification amenable to the Goodman-Bacon decomposition: with no additional fixed effects or controls, no weights, and the 0/1 *SC* measure. We do not interpret the magnitudes of the coefficients in Panel B, because the omitted weights and controls and the continuous measure of Secure Communities are important elements of our empirical strategy.³¹ Rather, the purpose is to see the extent to which these magnitudes in Panel B are driven by earlier-versus-

³¹Even though we do not care about the magnitudes of the coefficients in Panel B, it is instructive to understand the reasons the magnitudes change from those in Panel A. In general, the coefficients in Panel B are smaller in magnitude than those in Panel A. This attenuation of the coefficients is largely due to the removal of the weights; the exclusion of the controls and extra fixed effects barely affects the estimates. The removal of weights almost certainly attenuates the coefficient due to measurement error in the *Hispanic Share* variable, which is more pronounced in smaller county-industries. The differences between the *SC* coefficient for high- and low-Hispanic share workplaces are qualitatively the same in Panel B as in Panel A for the three measures of complaints: in all cases, the coefficient on *SC* is clearly more negative for workplaces with a high Hispanic workforce share than for workplaces with a low Hispanic workforce share. However, for injuries, the coefficients for the two groups are very similar and both are very close to zero. This is due to using the 0/1 *SC* measure in Panel B, as opposed to the continuous *SC* measure in Panel A. Because Secure Communities only affected injuries with a delay (as shown in Figure 1b), it is expected that using a treatment variable equal to 1 in the activation year (rather than equal to the fraction of the year in which Secure Communities was activated) attenuates the coefficient on injuries for High-Hispanic workplaces.

later treated groups (the cleaner comparison) and later-versus-earlier treated groups (the more problematic comparison).³²

The remaining rows of Panel B present the Goodman-Bacon decomposition for our four outcomes.³³ Earlier-to later treated groups make up 53.6 percent of the weight in the estimates in panel B; later-to-earlier treated groups make up the remaining 46.4 percent. Across all four outcomes, the average estimates among the earlier-later treated groups is quite similar to the average estimates among the later-earlier treated groups. For example, in column 1, where the outcome is *Total Complaints*, these estimates are -0.0386 and -0.0505 , respectively. It would be concerning if the average estimate among the later-earlier treated groups was substantially different. This is not the case.

Figure B2 provides further evidence that potential bias from later- versus earlier-treated comparison groups does not bias our estimates in a large way. It shows a scatterplot of each 2x2 difference-in-differences estimate against its weight, corresponding to the estimate in column 1 of Panel B. The difference-in-differences terms corresponding to earlier-vs.-later treated groups (the cleaner comparisons) are represented by circles; those corresponding to later-vs.-earlier groups (the problematic comparisons) are represented by triangles. A few takeaways emerge. First, 26 out of the 30 estimates are negative; among the four that are positive, three are in the later-vs.-earlier treated group. While these could represent a source of bias in theory, all three of these terms add extremely little weight to the overall estimate. Second, some of the terms with the largest weights are the (problematic) later versus earlier treated comparison, but the DD estimates among these comparisons are very similar to the other comparisons with high weights that are based on earlier-versus-later treated comparisons. This similarity suggests that our estimates are not driven by a few (potentially problematic) later-earlier treated groups.

Overall, these results indicate that our main estimates are not meaningfully biased by issues arriving from time-varying treatment effects.

³²In settings in which there is a group of units that is never treated, the 2x2 estimators also includes a group in which untreated groups serve as the control group for treated units. However, since Secure Communities was eventually implemented in all counties, there is no “never treated” group. Thus, this group does not apply in our setting.

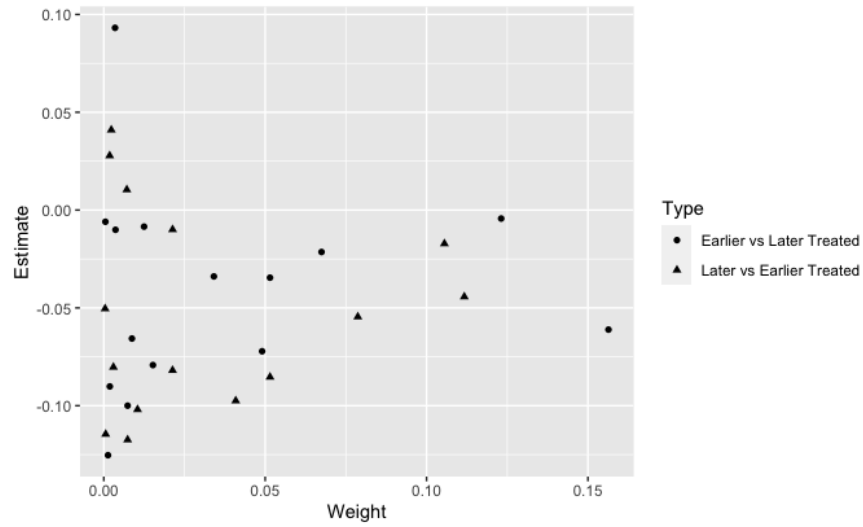
³³We only present the decomposition for the high Hispanic workforce share groups: the reason for this is that our computing power did not enable us to obtain the decomposition for the (much larger) Low-Hispanic group.

Table B6: [Goodman-Bacon \(2018\)](#) Decomposition of the Two-Way Fixed Effects Estimate of Secure Communities on Complaints and Injuries

Dependent Variable:	Total Complaints		Formal Complaints		Complaint-driven Inspections		Injury-driven Inspections	
	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low	(7) High	(8) Low
Hispanic Share:								
Panel A: Main Specification (with weights and controls)								
<i>SC</i>	-0.125 (0.0761)	0.0220 (0.0230)	-0.164** (0.0809)	-0.0139 (0.0359)	-0.0801 (0.0577)	0.0710** (0.0326)	0.0493 (0.0580)	-0.0545** (0.0217)
Panel B: Goodman-Bacon Decomposition (no controls or weights)								
TWFE coefficient on <i>SC</i>	-0.0441** (0.018)	0.006 (0.007)	-0.040** (0.017)	-0.012** (0.005)	-0.0496*** (0.016)	0.000 (0.006)	-0.0036 (0.012)	-0.001 (0.003)
<u>Earlier vs Later Treated groups</u> weight=53.61%								
beta	-0.0386		-0.0431		-0.0620		-0.0113	
<u>Later vs Earlier Treated groups</u> weight=46.39%								
beta	-0.0505		-0.0344		-0.0354		0.0054	
Observations	33,278	183,133	33,278	183,133	33,278	183,133	33,278	183,133

Note: * p<0.10, ** p<0.05, *** p<0.01. Both Panel A and the first row of Panel B display the coefficient on *SC* from a difference-in-difference regression, in which the dependent variable is the inverse hyperbolic sine of the variable indicated, and the sample is either restricted to county-industries with a “high” Hispanic workforce share (above the 80th percentile), or a “low” Hispanic share. The regressions reported in both panels include county-industry and year fixed effects; that in Panel A also includes industry-year, OSHA region-year, and OSHA jurisdiction fixed effects, and also controls for inverse hyperbolic sine of employment and the number of programmed inspections. The rest of Panel B reports the [Goodman-Bacon \(2018\)](#) decomposition of the coefficients reported in the first row of Panel B. See text for details.

Figure B2: Goodman-Bacon (2018) Difference-in-Difference Decomposition for the Effect of Secure Communities on Total Complaints



Note: The figure plots each 2x2 DD components from the Goodman-Bacon (2018) decomposition theorem against their weight for estimated effect of Secure Communities on *Total Complaints*, which is shown in Panel B of column 1 of Table B6. The circles are terms in which earlier treated units act as the treatment group, and later treated units as the control group. The triangles are terms in which later treated units act at the treatment group, and earlier treated units as the control group.

C Additional Figures and Tables

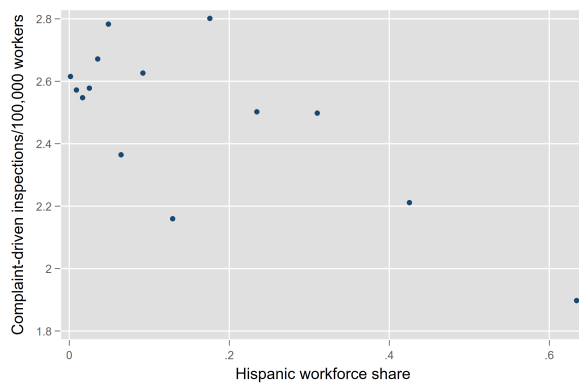
Figure C1: Relationship between Complaint-Driven Inspections and Injury-Driven Inspections



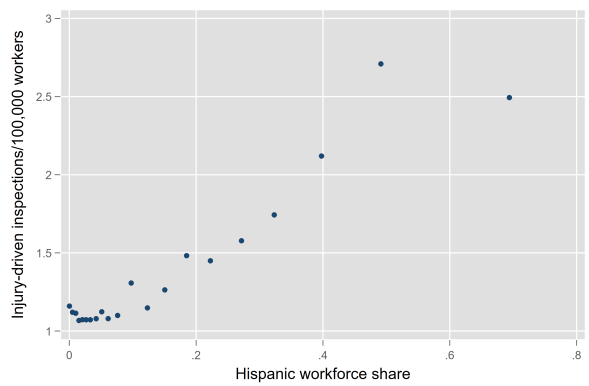
Note: Sample includes agriculture, construction, manufacturing, wholesale trade, retail trade, and transportation/warehousing. Lagged inspections are inspections from previous year. Inspections are winsorized at the 99th percentile. All correlations control for year fixed effects, industry fixed effects, employment, the number or rate of programmed inspections, metro status of the county and the log county population. Plot produced with binsreg package by [Cattaneo et al. \(2019\)](#) and plotting package by [Bischof \(2017\)](#).

Figure C2: Correlation between Worker Complaints, Workplace Injuries, and Hispanic Workforce Share

(a) Complaint-driven inspections per 100,000 workers



(b) Injury-driven inspections per 100,000 workers



Note: Sample includes agriculture, construction, manufacturing, wholesale trade, retail trade, and transportation/warehousing. Inspection rates are winsorized at the 99th percentile. All correlations are weighted by employment and control for year fixed effects, industry fixed effects, the rate of programmed inspections, metro status of the county, the log county population, and the county's poverty rate. Plots produced with binsreg package by Cattaneo et al. (2019) and plottig package by Bischof (2017).

Table C1: Unions Prevented Effects of Secure Communities on Worker Complaints and Workplace Injuries: Dep Var Measured in Rates per 10,000 Workers

Dependent variable:	Rate (per 10,000 employees) of:			
	Complaint-driven inspections		Injury-driven inspections	
Sample:	Nonunionized	Unionized	Nonunionized	Unionized
	(1)	(2)	(3)	(4)
SC	0.386*** (0.098)	0.090*** (0.032)	-0.082*** (0.027)	-0.002 (0.007)
SC×Hispanic share	-1.54*** (0.54)	-0.22 (0.20)	0.48** (0.21)	0.02 (0.04)
p-value on difference:	0.019		0.013	
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓
Hispanic share × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
OSHA region × year FE	✓	✓	✓	✓
OSHA jurisdiction × year FE	✓	✓	✓	✓
Mean Dep Var (in levels)	3.83	0.84	1.17	0.25
# Observations	175,265	175,265	175,265	175,265

Note: * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of inspections in nonunionized (columns 1 and 3) and unionized (columns 2 and 4) workplaces on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share, and the controls and fixed effects indicated in the bottom panel.