# Complaint-driven Regulation and Working Conditions: Evidence from Immigration Enforcement *

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**Abstract**

Regulatory agencies overseeing the labor market often rely on worker complaints to direct their inspections. However, if workers face differential barriers to complain, this system could result in ineffective targeting and disparate working conditions. We find that counties' participation in Secure Communities– a localized immigration enforcement program—substantially reduced complaints to government safety regulators, but increased injuries, at workplaces with Hispanic workers. Secure Communities also increased minimum wage violations among Hispanic and—to a lesser extent—non-Hispanic workers, consistent with the public good nature of complaints. Despite worsened working conditions, we find no effect of Secure Communities on worker turnover.
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# 1 Introduction

Many labor market regulations give workers the right to complain to the government agencies that enforce them. These agencies, that tend to face exceedingly limited resources, often rely on these complaints to direct a large share of their enforcement efforts.[1] Worker complaints triggered 20 percent of inspections conducted by the Occupational Safety and Health Administration (OSHA) between 2006 and 2016, over 75 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 (EEOC).[2]

Despite this prevalence, it is not clear if such reliance on complaints is an effective approach to monitor and improve working conditions. On one hand, workers have better information about where violations occur than government agencies do: worker complaints could in theory enable regulators to direct inspections to where noncompliance is most severe. On the other hand, fear of employer retaliation, reluctance to interact with government, and other barriers might make it costly for workers to complain (Weil and Pyles, 2005; Dyck et al., 2010). If these barriers are higher for workers who are more at risk of violations, complaints will not direct inspections where working conditions are most problematic (Chassang and Miquel, 2019). The implications could extend beyond the effectiveness of regulatory targeting. Employers face lower expected enforcement costs when their workers are less likely to complain, which reduces their incentives to comply and to invest in better working conditions. At the same time, because labor market competition and other public policies exert their own discipline on employers, this effect of workers' willingness to complain on working conditions may not in practice be economically meaningful.

To inform these questions regarding the effects of complaint-based regulation, we examine the impact of an exogenous increase in the cost to complaining for some workers on workers' actual willingness to complain and on the working conditions they face. Our context is Secure Communities—a local immigration enforcement program that likely raised Hispanic workers' perceived risk of complaining to government agencies.[3] We focus primarily on Secure Communities' effect on occupational injuries, and also on rates of

[1] One illustrative example of how scarce regulators' enforcement is that Occupational Safety and Health Administration (OSHA) inspects less than 1 percent of the workplaces it oversees each year. See: https://www.osha.gov/data/commonstats.

[2] See the EEOC website: https://www.eeoc.gov/youth/about-eeoc-2.

[3] An 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

minimum wage violations.

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); they are also relatively more likely to experience minimum wage violations (Fine et al., 2021). Despite this higher risk of violations, language barriers, lack of knowledge about labor laws, and other factors plausibly 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 Unites States' annual spending on immigration enforcement has exceeded $20 billion in recent years (Muzaffar and 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 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. We also, as secondary measures, use data on all worker complaints submitted to OSHA that we obtained through a Freedom of Information Act (FOIA) request. Since there are no publicly-available 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 Sur-

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.

vey (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 non-immigration matters with Immigration and Customs Enforcement (ICE). We use the staggered activation of Secure Communities across counties as the basis of a difference-in-difference design and estimate effects on complaints and injuries for workplaces with high versus low shares of Hispanic workers. As our primary speciication, we use the Borusyak et al. (2021) difference-in-difference approach that overcomes the now well-known potential biases that can arise from the more traditional two-way fixed effects estimators (Goodman-Bacon, 2018).

We find that counties' participation in Secure Communities substantially reduced worker complaints in workplaces with a high share of Hispanic workers. In county-industries with a Hispanic workforce share above the 80th percentile of the distribution (among which the average Hispanic share is 38%), the activation of Secure Communities led to 16% fewer complaints. If the effects are linear in the Hispanic share, this would imply that Secure Communities led to $(0.16/0.38 =)$ 42% fewer complaints by 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 in magnitude over time. Among workplaces with a Hispanic share below the 80th percentile (among which the average share Hispanic is very small, just 6%), Secure Communities had no effect on complaints.

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 "high Hispanic" workplaces (those with a Hispanic share above the 80th percentile) increased by 7 percent. Again, we find no effect for "low Hispanic" workplaces.

We obtain very similar estimates using the Callaway and Sant'Anna (2021) framework, an alternative difference-in-difference estimator that is robust to heterogeneous treatment effects. Our estimates are also robust to accounting for early selection into Secure Communities, controlling for potential confounding factors including the severity of the Great Recession, and alternative transformations of the dependent variable.

To complement our analysis on workplace injuries, we use the Current Population Survey to examine

[4] More 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.

whether Secure Communities affected rates of minimum wage violations. This test corroborates and generalizes our analysis on workplace injuries in multiple ways. The Wage and Hour Division (WHD) relies on worker complaints to target its enforcement of minimum wage laws to an even greater extent than does OSHA. We are also able to directly test different effects for Hispanic and non-Hispanic workers (rather than relying on county-industry demographic shares). We find that Secure Communities led Hispanic workers to face 127% higher rates of minimum wage violations, relative to the sample mean. Interestingly, we also find that Secure Communities increased minimum wage violations among non-Hispanic workers, but to a lesser extent: the effect size (52% relative to the sample mean) is 40% the magnitude as for Hispanic workers. This spillover effect is consistent with the public goods nature of complaints (Weil and Pyles, 2005): that is, a lower willingness to complain among one group of workers could affect not just their own working conditions, but also those of their co-workers (or those in the same labor market). Furthermore, we conduct a sharp test of whether it is the reduction in complaining to regulators, as opposed to a general reduction in bargaining power, that determines the deterioration in working conditions: we find no effect of Secure Communities on wages further up in the wage distribution.

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 0.45. 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 Quaterly Workforce Indicators (QWI), we show that Secure Communities had a negligible effect on Hispanic workers' job separation rate and overall job turnover. This unresponsiveness 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. Our narrative has parallels with ways that immigration enforcement has affected crime and crime reporting. Dhingra et al. (2021) find that ICE cooperation with local law enforcement decreased crime reporting by Hispanic communities without reducing crime commission or victimization. Gonçalves et al. (2024) find that Secure Communities reduced crime reporting, but increase crime victimization, among Hispanic individuals, rationalizing prior work finding no effect

on overall reported crime rates (Miles and Cox, 2014). 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 (Alsan and Yang, 2022) and led to a decline in prenatal health care visits, school attendance, and progression and completion, and increased food insecurity among Hispanic families (Rhodes et al., 2015; Dee and Murphy, 2018; Amuedo-Dorantes and Lopez, 2015, 2017; Potochnick et al., 2017). East et al. (2022) find that Secure Communities reduced employment rates of both likely undocumented and U.S. citizens. Our results directly complement largely qualitative prior work showing that immigrant and undocumented workers are less willing to complain to federal agencies, despite facing higher rates of labor violations (Mehta et al., 2002; Bernhardt et al., 2009; Rathod, 2010; Fussell, 2011; Flynn et al., 2015; Rathod, 2015; Liebman et al., 2016); we provide quantitative, causal evidence that corroborates findings from this literature, and we show immigration enforcement has created economically meaningful disparities in working conditions across population groups.

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 enforcement system. Our results suggest that regulatory agencies' reliance on worker complaints undermines the agencies' mission and exacerbates inequality in the labor market. In a similar vein, we complement work that illustrates how the economic and legal environment affects worker safety (Johnson et al., 2022), reports of sexual harassment (Dahl and Knepper, 2021), and the flow of negative information about employers (Sockin et al., 2021). Our study also complements Boudreau et al. (2023), who show theoretically and experimentally that "garbling" workers' responses to sensitive questions substantially increases workers' willingness to speak up about workplace hazards and abuses. Finally, we complement studies that investigate the determinants and effects of complaining in other domains, including citizen complaints about police misconduct (Ba, 2018) and whistle-blowing about Medicare fraud (Leder-Luis, 2021).

Third, our study offers an indirect but novel 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. (2022) and Harju et al. (2021) test whether greater opportunity for voice reduces worker turnover.

## 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 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. For example, the 1938 Fair Labor Standards Act gives workers the right to complain to WHD when their employer is not complying with minimum wage, overtime, or other laws enforced by the agency. 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 (Figure C1 shows which states are and are not under federal OSHA's jurisdiction).

OSHA immediately follows up a complaint with an inspection of the employer if certain conditions are met. These conditions include that the complaint is signed by a current employee, alleges an imminent

danger or serious hazard, and contains reasonable grounds that an OSHA violation exists (U.S. Occupational Safety and Health Administration, 2020).[5] 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;[6] no workplace-specific factors (such as recent injuries) influence the occurrence of a programmed inspection.[7] Even with all these potential triggers, OSHA inspections are rare. OSHA inspects less than 1 percent of workplaces under its jurisdiction each year (U.S. Occupational Safety and Health Administration, 2021).

Complaining might produce an array of benefits for workers.[8] Complaints can trigger inspections, which tend to improve working conditions (Levine et al., 2012; Haviland et al., 2012; Li and Singleton, 2019; Johnson et al., 2023). 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. For one, many workers do not know their rights to complain (Alexander and Prasad, 2014). But even for those who know their rights, 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).[9] Furthermore, an individual worker might not complain because they do not internalize

[5] For 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.

[6] A 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.

[7] The exception to this statement is OSHA's Site-Specific Targeting program, active from 1998–2011, which focused a subset of programmed inspections on establishments that had recently experienced high injury rates.

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

[9] Technically, 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 take advantage of these protections, OSHA is limited in the damages it can actually pursue on a claimant's behalf,

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 barriers to complaining are likely particularly acute 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. Employer retaliation is more costly, as undocumented immigrants have fewer employment options and lack access to the social safety net;[10] furthermore, these individuals might fear that any government interaction—including complaining to regulatory agencies—raises the risk of deportation.[11] 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, 2022; Dee and Murphy, 2018; Comino et al., 2016; Dhingra et al., 2021; Grittner, 2019; Gonçalves et al., 2024).

For employers, their workers' threat of complaining directly affects their expected costs of workplace

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). It is thus no surprise that evidence is widespread of employer retaliation against workers who exercise their rights to complain (Stansbury, 2021).

[10] Institutional 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 under the National Labor Relations Act.

[11] Deportation 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.

injuries. All else equal, a lower threat of worker complaint reduces the probability that OSHA or other regulators will inspect the employer, which reduces the employer's expected value of future 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 and other labor 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). 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.[12]

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, immigration 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 Worker Complaints and Workplace Injuries

Our primary measure of worker complaints is the occurrence of inspections conducted by OSHA that are responses to a worker complaint. We call these "complaint-driven inspections." As described above, complaints that meet certain conditions (such as being signed by a worker and having reasonable grounds that

[12]Employers 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).

workers are exposed to a serious hazard or imminent danger) trigger an OSHA inspection within a few weeks. An advantage of this measure is that it captures the subset of complaints that definitely are from workers at the referenced workplace, reference an issue within OSHA's jurisdiction, and allege issues serious enough for OSHA to merit an inspection. Another advantage is that this complaint measure mirrors our measure of workplace safety, described later in this section.

As a secondary measure of complaints, we use data on complaints received by OSHA (and its state-run counterparts) from 2001 through 2019, which we obtained through a Freedom of Information Act (FOIA) request with the U.S. Department of Labor. For each complaint, we observe employer characteristics (name, address, industry) as well as various classifications, such as whether the complaint alleges a serious or imminent safety or heath hazard. This measure has the advantage that it captures all complaints to OSHA, rather than just the subset that result in inspections. However, a disadvantage is that by nature it also includes complaints that do not fall under OSHA's jurisdiction, come from individuals not employed in the referenced workplace, or do not reference any underlying safety issues.

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 injury is an injury that results in a worker fatality or the hospitalization of three or more workers. The law requires that the employer reports any serious injury to OSHA and that OSHA subsequently inspect the workplace immediately. A limitation of this measure is that it captures only the most serious injuries, and thus we do not measure the (more numerous) less serious injuries that do not result in death or multiple hospitalization. That said, a benefit of this measure is that very serious injuries tend to be reported accurately, whereas less-serious injuries are often subject to concerns of mis-reporting both by workers and employers (Biddle and Roberts, 2003; Boden et al., 2010). Furthermore, prior research shows that OSHA's injury-driven inspections are a reliable measure of the occurrence of fatal and serious nonfatal work-related injuries (Mendeloff and Kagey, 1990; Johnson et al., 2022).

Data on complaint- and injury-driven inspections come from OSHA's Integrated Management Information System (IMIS) (U.S. Department of Labor, 2021). We drop the small share of inspections that are of

public-sector workplaces.[13] We collapsed the IMIS and FOIA data to create a dataset with the annual number of complaint-driven inspections, various categories of complaints received, and injury-driven inspections for each county-industry. We operationalize "industry" roughly as two-digit NAICS codes.

### 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 Alsan and Yang (2022) 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.

[13] We do not observe public/private sector status of complaints in the FOIA data, so we cannot make a similar restriction there.

### 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 demographics of the worker that filed each complaint to OSHA, or observe the share of workers who are Hispanic at each workplace that receives an OSHA inspection. 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. We operationalize "industry" roughly as two-digit NAICS codes. We use county-industries' Hispanic workforce share averaged over the years 2005 through 2007. We choose these years because they precede the start of Secure Communities, which might directly affect Hispanic workforce shares.[14]

Hispanic workforce shares vary widely across both counties and industries. As an example, 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 Summary Statistics

Table 1 includes basic descriptive statistics for our primary variables. During our sample period of 2003–2016, the average number of complaint-driven OSHA inspections in a county-industry was 3. The average number of total complaints received was 8.5; just under two-thirds of these alleged any serious hazard (5.4 relative to 8.5), and less than half (3.4) alleged a serious safety hazard. Notably, injury-driven inspections are quite rare: the average number of injury-driven inspections was 0.9, and over 75% of county-industry–years experienced zero of them. The average county-industry has a Hispanic workforce share of 0.12,[15] with a standard deviation of 0.16.

[14] Our 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.

[15] As comparison, the share of the U.S. population that was of Hispanic or Latino descent in 2006 was 14.8 percent (**?**).

### 3.5 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. Figure C2 illustrates the correlation between the rate of complaint-driven inspections (per 100,000 workers) and the average of the rate of injury-driven inspections over the prior three years in a county-industry. The figure is a binned scatter plot that is weighted by employment and controls for the rate of programmed OSHA inspections, whether the county lies in a metropolitan statistical area, the county's population and median household income, as well as fixed effects for industry, state, OSHA-region–year (to capture time-varying shocks correlated within regions) and state-run-plan–year (to capture any time-varying changes to OSHA or state-plan leadership). We restrict the sample for these correlations to the years 2003–2007 before Secure Communities was in place. There is a clear positive relationship: one more injury-driven inspection per worker over the prior three years is associated with 0.22 more complaints per 100,000 workers in the following year ($p < 0.01$). Generally, workers are more likely to complain to OSHA where they face higher injury risks.

We then examine whether the propensity to complain and risk of workplace injury differs for Hispanic workers. We first correlate the number of complaint-driven inspections per 100,000 workers with the county-inustry's Hispanic workforce share, residualizing on the same controls and fixed effects. The binned scatterplot in Figure C3a shows a negative correlation: a 10 percentage point increase in the Hispanic workforce share is correlated with 0.087 fewer complaints per 100,000 workers ($p < 0.01$). However, Figure C3b 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 0.082 more injuries per 100,000 worker ($p < 0.01$).

These results reveal 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. This correlational anal-

ysis is consistent with the implications discussed in Section 2 but 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, workplace injuries, and violations of labor regulations.

# 4 The Effects of Immigration Enforcement on Worker Complaints and Injuries

Using variation in the timing of Secure Communities activation across counties, and variation in the Hispanic workforce shares across county–industries, we find robust evidence that Secure Communities reduced complaints to OSHA, but increased injury rates, among workplaces with Hispanic workers.

## 4.1 Empirical Strategy to Estimate the Causal Effect of Immigration Enforcement

To estimate the effect of Secure Communities on worker complaints and injuries, our identifying variation comes from the staggered adoption of Secure Communities across counties over time. In such a setting, it is now well known that a two-way fixed effects estimator can yield biased estimates of the causal effect of Secure Communities (Goodman-Bacon, 2018), an issue particularly magnified by the fact that all counties eventually adopted Secure Communities (so there are no "never treated" counties). In light of these concerns, we use the imputation-based difference-in-difference framework of Borusyak et al. (2021), which overcomes these sources of bias. We also use the framework of Callaway and Sant'Anna (2021) as a robustness check and obtain similar results.

In our main specifications, our dependent variable $Y$ is the inverse hyperbolic sine of the number of complaint-driven or injury-driven OSHA inspections for county–industry $c, i$ in year $t$. 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 using other transformations, such as

taking the log or taking the rate per worker.

Our key explanatory variable is an indicator equal to 1 starting in the year the county began participating in Secure Communities, $SC_{c,t}$. To separately analyze the effects of Secure Communities for workplaces with and without a presence of Hispanic workers, we estimate difference-in-difference regressions for two groups: 1) county-industries with a Hispanic workforce share above the 80th percentile of the distribution ("High-Hispanic") and 2) county-industries with a Hispanic share below the 80th percentile ("Low-Hispanic"). In all regressions, we control for the inverse hyperbolic sine of county-industry employment, $asinh(employment_{c,i,t})$. to account for any changes in employment levels, and we cluster standard errors by county. 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.[16] We restrict our sample to the 48 contiguous U.S. states and the District of Columbia between the years 2003–2012; we cannot include years after 2012 with the Borusyak et al. (2021) estimator since, after that year, Secure Communities was universally adopted (so there were no remaining "not-yet-treated" counties). With these conditions, we estimate both the overall effect of Secure Communities across all post-period years, as well as its dynamic effects over time in an event study design.

Our strategy relies on the identification assumption that, conditional on county-industry employment 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, which we describe below.

## 4.2 Baseline Estimates

Figure 1 plots event-study estimates of the dynamic effects of Secure Communities on complaints (panel A) and on injuries (panel B), separately for *Low-Hispanic* (left column) and *High-Hispanic* workplaces. Table 2 presents regression estimates of the average effects for the same outcomes and groups.

[16] This weighting serves two purposes. First, the weighted regressions estimate average effects for the population that are policy-relevant. Second, weighting minimizes the influence of measurement error in the Hispanic workforce share on our estimates. Smaller county-industries, in which measurement error in the Hispanic workforce share is particularly severe, get a lower weight and thus contribute less to our estimates.

Beginning with complaints, Figure 1 shows no differential pre-trends in the years before Secure Communities activation either for *Low-* or *High-Hispanic* workplaces, implying that the onset of Secure Communities was not correlated with underlying trends affecting workplace conditions. The coefficients remain flat for *Low-Hispanic* workplaces in the post-period, implying no change in complaints for this group. For *High-Hispanic* workplaces, the coefficient becomes negative in the year Secure Communities is activated—indicating fewer complaints—and grows in magnitude over time. Columns 1 and 2 of Table 2 provide corresponding regression estimates of the average causal effects on complaints, indicating that Secure Communities had no effect for *Low-Hispanic* workplaces ($\hat{\beta} = -0.003$, $p = 0.92$) but caused a significant decrease in complaints among *High-Hispanic* workplaces ($\hat{\beta} = -0.159$, $p < .01$). To interpret these magnitudes, consider that 1) since the outcome variable is an inverse hyperbolic sine, we can interpret the coefficients roughly as percentage changes,[17] and 2) as shown in the table footer, the average Hispanic workforce share in the *High-Hispanic* group is 38%. This implies that Secure Communities would have caused $(0.159/0.38 =)$ 42% fewer complaints in a hypothetical workplace with a 100% Hispanic workforce share.

Moving to injuries, panel (b) of Figure 1 also shows no pre-trends for either group, and no change following Secure Communities activation for *Low-Hispanic* workplaces. However, for the *High-Hispanic* group, the coefficient becomes *positive* the year Secure Communities is activated and grows in magnitude over time, indicating an *increase* in injuries in this group. Columns 3 and 4 of Table 2 show corresponding regression estimates that Secure Communities had no effect on injuries in *Low-Hispanic* workplaces ($\hat{\beta} = -0.013$, $p = 0.30$) but caused a significant increase in injuries in *High-Hispanic* workplaces ($\hat{\beta} = 0.072$, $p < .01$).

In Panel (b) of Table 2, we report estimates of Secure Communities on the same outcomes and groups but using the approach from Callaway and Sant'Anna (2021), an alternative to Borusyak et al. (2021) that also overcomes the potential bias from TWFE.[18] Each coefficient is remarkably similar to those with obtain with Borusyak et al. (2021), indicating our results are not driven by our choice of estimator.

[17] Chen and Roth (2023) show that the inverse hyperbolic sine transformation of a dependent variable may not lead a regression coefficient to approximate a percent change in some cases. However, we show later that we get similar magnitudes using other transformations, suggesting this concern is not of practical relevance here.

[18] Callaway and Sant'Anna (2021) first considers "group-time average treatment effects:" the average treatment effect for group $g$ and time $t$, where groups $g$ are defined by the time period when units are first treated. The authors then provide steps to aggregate these many group-time effects into a single summary measure, such as the average treatment effect on the treated (ATT). Inference is obtained via bootstrapping. We refer readers to Callaway and Sant'Anna (2021) for more details.

In Table C1, we report effects of Secure Communities (again using the Borusyak et al. (2021) estimator) on various alternative measures of complaints, based on our FOIA data. As described in Section 3.1, these measures have the advantage that they reflect actual complaints received by OSHA, but they have the disadvantages that they are less comparable to our injury measure, may reflect concerns outside of OSHA's jurisdiction, and may not come from a worker at the associated workplace. Given these concerns, we consider: complaints alleging a serious safety hazard, complaints alleging a serious safety *or* health hazard, and all complaints. For all three measures, the coefficient is negative for *High-Hispanic* workplaces, and for those alleging a safety hazard the coefficient ($-0.149$) is quite similar in magnitude to our baseline measure of complaint inspections and highly statistically significant ($p < .01$). For the other two categories, the magnitude is smaller and just barely not statistically significant at conventional levels ($p = 0.102$ and $p = 0.11$, respectively).

### 4.3 Robustness to Confounding Factors and Specification Choices

In Appendix B, we describe in detail how we assess the sensitivity and robustness of our results to potential confounding factors and specification choices that could bias our estimate of the effects of Secure Communities. We first test if selection into early adoption of Secure Communities confounds our estimates. We re-estimate our regression equation on various sub-samples that exclude a) all counties at the Mexico–U.S. border, b) any county that adopted Secure Communities in its first year (2008), and c) the state of Arizona, which in 2010 passed measure SB 1070, one of the most sweeping and stringent immigration enforcement programs in the country. We also test the sensitivity of our results to confounding factors like the Great Recession, contemporaneous adoptions of other immigration enforcement programs such as the 287(g) program, overall OSHA inspection activity, and accounting for time-varying industry-specific shocks. We also estimate our regressions using alternative modelling choices like using a transformations other than inverse hyperbolic sine of the count of complaints and injuries, such as using log instead of inverse hyperbolic sine, the rate per worker, and the log of the rate per worker.

We summarize the results of these checks in Figure 2. Appendix B provides further details of each of these checks and shows the regression tables. Our estimates are remarkably stable across this range of

robustness checks.[19]

# 5 Corroborating Evidence: Immigration Enforcement and Minimum Wage Violations

Secure Communities also led to higher rates of minimum wage violations. This analysis serves three purposes: it generalizes our results on injuries, it enables us to more directly examine effects on Hispanic versus non-Hispanic workers, and it offers a sharp test of whether it is the change in complaining as opposed to other factors that affected working conditions.

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 minimum wage laws, the Wage and Hour Division (WHD) of the Department of Labor, relies on worker complaints to target enforcement to an even greater extent than OSHA: 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, then—following our framework in Section 2, employers would be less motivated to comply with minimum wage laws.

Following prior work, we use the Current Population Survey (CPS) Outgoing Rotation Group (ORG) to measure 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

[19] The one exception is that our estimated effect of Secure Communities on injuries in High-Hispanic workplaces attenuates when we use the rate of injuries per worker as our dependent variable, instead of the inverse hyperbolic sine or the log of the count of injuries. However, as we describe in Appendix B, we put less weight on this result given the huge outliers that characterize this transformation of this variable.

participation.

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. One challenge to this measure is that there is very likely measurement error in reported wages. 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, in our primary regressions, we restrict the sample to hourly workers, who directly report their wage to the CPS.

We again use the Borusyak et al. (2021) difference-in-difference approach to estimate the causal effect of Secure Communities on minimum wage violations. We control for the state's effective minimum wage ($MW_{c(s)t}$), and a vector of individual controls $X_{it}$ that includes age, age squared, an indicator if an individual identifies as White, and an indicator if the individual identifies as female. The sample is working age individuals aged 20–64. [20] We cluster standard errors by county. We estimate separate regressions for individuals that do and do not identify as Hispanic.

Panel (a) of Figure 3 reports event study estimates for the effect of Secure Communities on minimum wage violations among Hispanic workers. The coefficients in the pre-period are flat and centered around zero, then become positive and statistically significant beginning two years after Secure Communities is activated, indicating that Secure Communities led to higher rates of minimum wage violations among Hispanic workers. The magnitude is sizable: the corresponding regression estimate (reported in Column 1 of Table 3) reveals that on average, Secure Communities increased the likelihood that a Hispanic worker was paid below the minimum wage by 0.036 percentage points ($p < .01$), a 126% increase over the sample mean. (Column 4 of Table Table 3 shows we get very similar estimates when we expand the sample to include workers not paid by the hour.)

Panel (b) of Figure 3 reports analogous event study estimates for non-Hispanic workers. The coefficients in the pre-period again indicate no evidence of pre-trends. The post-period coefficients become positive and statistically significant in year two, mirroring the pattern for Hispanic workers. The effect size is large but about 40% as large as that for Hispanic workers: the corresponding regression estimate (reported in Column 2 of Table 3 is that Secure Communities increased the likelihood that a non-Hispanic worker is paid below

[20] We obtain essentially identical estimates if we restrict the sample to workers in the bottom quintile of the wage distribution in their state-year, to focus on workers most at risk of a minimum wage violation, as has been done in prior work (e.g., Galvin (2016)).

the minimum wage by 0.014 percentage points ($p < .01$), representing a 54% increase over the sample mean. (Column 5 of Table Table 3 shows we get very similar estimates when we expand the sample to include workers not paid by the hour.)

The evidence that Secure Communities increased minimum wage violations among not just Hispanic workers, but also (to a lesser extent) non-Hispanic workers, is consistent with the public goods nature of complaints. As described in Section 2, a complaint by one worker might accrue positive benefits to other workers at the same employer (or even the same local labor market) who are also facing labor violations (Weil and Pyles, 2005). That is, in workplaces (or local labor markets) with a diverse group of workers, a reduced willingness to complain among one subset of workers would affect not only that group's subsequent risk of facing labor violations, but also have spillover effects on risks faced by co-workers or those in the same labor market.[21] Following this logic, any spillover effect should be attenuated among groups of workers less likely to work with Hispanic or immigrant workers. Given evidence that immigrant workers are more likely to work with other immigrant workers (Andersson et al., 2014), in Column 3 of Table 3 we show the regression estimate of the effect on minimum wage violations of White, non-Hispanic, non-foreign born workers. The estimate is still positive and highly significant ($p < .01$), but the magnitude (representing a 44% increase over the sample mean) is smaller than the effect for non-Hispanic workers overall. (Column 6 of Table Table 3 shows we get very similar estimates when we expand the sample to include workers not paid by the hour.)

These results corroborate, generalize, and enrich 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). Additionally, whereas workplace safety is a function of both employer and worker behavior,[22] noncompliance with the minimum wage is arguably less influenced by worker behavior, bolstering the interpretation that the change in working conditions was driven by employers responding to workers' threat of complaining.

Still, a reasonable question is whether Secure Communities increased injuries and minimum wage vi-

[21] We note that this evidence of spillovers is not inconsistent with our estimates in Section 4.2 that showed no effect of Secure Communities on injuries in county-industries with low share of Hispanic workers; in such workplaces, there is little to no scope for spillovers since there are almost no Hispanic workers present.

[22] For example, in theory Secure Communities could have affected injuries by increasing stress among Hispanic workers.

olations because of Hispanic workers' reduced willingness to complain or because of other factors. For example, immigration enforcement could have led to a reduction in local aggregate demand, and thus labor demand (East et al., 2022). It could also have generally weakened Hispanic workers' bargaining power independent of their propensity to complain.

One way to disentangle these channels is to test whether Secure Communities also affected wages higher up in the wage distribution. If the effects shown in Table 3 were due to an overall decrease in Hispanic workers' bargaining power or to an overall decrease in labor demand, then Secure Communities should also increase the probability that Hispanic workers were paid less than arbitrary thresholds higher up in the wage distribution—for example by causing a leftward shift in the entire wage distribution. If, in contrast, minimum wage violations increased because Hispanic workers' willingness to complain decreased, we would not expect any effect on wages above the minimum wage.

In the spirit of this test, we re-estimate regression underlying Column 1 of Table 3, but we replace the dependent variable with indicators that a worker's wage is less than a specific threshold that is above the minimum wage. We restrict the sample to workers that are paid hourly. We include all the same controls as describe above.

We report results of this test in Figure C4. Each dot represents the estimate of the change in the probability that Hispanic workers are paid less than the threshold marked on the x-axis, following enactment of Secure Communities.[23] The leftmost dot corresponds to the estimate in Column 1 of Table 3: As already shown, Secure Communities significantly increased the probability that Hispanic workers were paid less than the minimum wage. Moving to the right, each dot represents the change in the probability that Hispanic workers were paid less than $1 above the minimum wage, $2 above, and up to $8 above the minimum wage. The estimate is positive and nearly significant for $1 above the minimum wage, and they are all very close to zero starting at $2 above the minimum wage.[24] This null effect at higher thresholds bolsters the interpretation that Secure Communities led to worse working conditions due to employers responding to their workers' reduced threat of complaining to labor enforcement agencies, as opposed to other factors

[23] To obtain this probability, we scale the coefficient we obtain in the regression by the mean of the dependent variable for the estimation sample.

[24] The positive coefficient for the threshold of $1 above the minimum wage could arise if some of the increase in minimum wage violations came from workers initially making more than $1 above the minimum wage. More broadly, these could represent spillover effects of the minimum wage (Cengiz et al., 2019).

such as shifts in labor demand or a general decline in bargaining power.

# 6 Effects of Secure Communities on Worker Turnover: "Voice" versus "Exit"

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. These results support our prediction that a negative shock to workers' propensity to complain dampens employers' incentives to invest in workplace safety and regulatory compliance. 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. Using the estimates in Columns 2 and 4 of Table 2, Panel A, taking the ratio of these two coefficients implies that the elasticity of Hispanic workers' injury risk with respect to their propensity to complain is $(0.072/0.159 = 0.45$.[25]

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 context 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, 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.

[25] We cannot estimate a similar elasticity for minimum wage violations because we lack data on complaints to WHD. However, given the magnitudes of our estimated effects on minimum wage violations, this implied elasticity would presumably be large.

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 strength of the threat of leaving one's job (the "exit" option) increases the value of complaining (the "voice" option) as a safeguard against unfavorable working conditions.

We provide suggestive quantitative evidence of this (ir)relevance of the "exit" option for Hispanic workers in Table 4. We use data from the QWI 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 at the quarterly level for Hispanic workers: the (log) number of worker separations, the rate of worker separations scaled by employment in the prior quarter, and the "stable" worker turnover rate. The stable turnover rate is the rate at which stable jobs begin and end, where "stable" hires and separations are those that pertain to a job that a worker had for at least one full quarter.[26] We again use the Borusyak et al. (2021) difference-in-difference framework to estimate the effect of Secure Communities on these outcomes

Column 1 of Table 4 reveals that Secure Communities had no detectable effect on the number of separations by Hispanic workers: the point estimate suggests a small ($\hat{\beta} = -0.018$) but insignificant ($p = 0.14$) *negative* effect on the count of separations by Hispanic workers. The point estimate is even smaller, essentially zero, in Column 2 when the dependent variable is separations by Hispanic workers, scaled by total Hispanic employment in the prior quarter. We get a similarly tiny and insignificant estimate in Column 3 when the dependent variable is the stable turnover rate for Hispanic workers. If the "exit" option were readily available to Hispanic workers, then we would expect to see a large share of Hispanic workers quit their

[26]The QWI calculates 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.

job in response to the increase in injury risk that we documented in Section 4.2. Overall, these results collectively suggest that "exit" was not a readily accessible option for Hispanic workers who faced deteriorating working conditions.

This small and negative effect on turnover also illustrates that the effects of Secure Communities on complaints, injuries, and minimum wage violations 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, could have such a big effect on their risk of both injury and minimum wage violations (Hirschman, 1970).

# 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 seems to reverse among Hispanic workers: workplaces with large shares of Hispanic are relatively less likely to complain to government safety regulators, but are more likely to face serious occupational injuries.

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 corroborating evidence that Secure Communities increased minimum wage violations, both for Hispanic workers but also—consistent with the public goods nature of complaints—for non-Hispanic workers. 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.

For worker complaints to be an effective mechanism to target enforcement, oversight agencies could take steps to reduce the risks of complaining for vulnerable groups. For example, agencies could provide formal guarantees for immigrant workers that filing a complaint will not heighten deportation risk for themselves, their co-workers, or families. In fact, OSHA issued a policy change in 2003 with this intent, whereby the agency could issue temporary work visas to immigrant workers who bring forth allegations of OSHA violations.[27] Our results indicate that a policy like this could have meaningful effects on immigrant workers' working conditions.

[27] See "READOUT: US Department of Labor expands OSHA's ability to protect all workers by certifying special visa applications to ensure effective enforcement." https://www.osha.gov/news/newsreleases/readout/02132023, accessed March 2024.

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# 8 Tables and Figures

Table 1: Summary Statistics

| | Mean | SD | percentile: | | | | Max |
|---|---|---|---|---|---|---|---|
| | | | 25th | 50th | 75th | 95th | |
| Complaint-driven inspections | 3 | 7.3 | 0 | 1 | 3 | 13 | 219 |
| Total complaints | 8.5 | 17.7 | 1 | 3 | 10 | 32 | 416 |
| Complaints alleging serious safety or health hazard | 5.4 | 10 | 0 | 2 | 6 | 22 | 226 |
| Complaints alleging serious safety hazard | 3.4 | 7.3 | 0 | 1 | 4 | 15 | 176 |
| Injury-driven inspections | .9 | 5.3 | 0 | 0 | 0 | 3 | 235 |
| Employment | 23536.6 | 42353.2 | 3696 | 11501 | 27036 | 81082 | 935589 |
| Share of the year Secure Communities is active | .4 | .5 | 0 | 0 | 1 | 1 | 1 |
| Hispanic workforce share | .12 | .16 | .02 | .05 | .16 | .46 | 1 |

Observations are at the county–industry year, spanning 2003–2016, and are weighted by each county-industry's average employment over the years 2006–2008.

Table 2: Effects of Secure Communities on Complaints and Injuries, By Share of Workforce that is Hispanic

| Dependent variable: | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Inverse hyperbolic sine of | | | |
| | Complaint-driven inspections | | Injury-driven inspections | |
| Share Hispanic: | Low | High | Low | High |
| Panel A: Borusyak et al. (2021) Estimator | | | | |
| SC | -0.003 | -0.159*** | -0.013 | 0.072*** |
| | (0.030) | (0.043) | (0.013) | (0.027) |
| Panel B: Callaway and Sant'Anna (2021) Estimator | | | | |
| SC | 0.031 | -0.172*** | 0.002 | 0.112*** |
| | (0.0347) | (0.039) | (0.019) | (0.030) |
| Mean Dep Var (in levels) | 2.19 | 5.92 | 0.26 | 3.32 |
| Mean share Hispanic | 0.06 | 0.38 | 0.06 | 0.38 |
| # Observations | 300,590 | 36,670 | 300,590 | 36,670 |

*Note:* * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Each column shows results of a separate regression in which the dependent variable is the inverse hyperbolic sine of the variable noted in the column header. The key explanatory variable, $SC$ is an indicator equal to 1 beginning the year a county participated in Secure Communities. "High Hispanic share" is equal to 1 if the county-industry's Hispanic share is above the 80th percentile. All regressions control for the inverse hypoerbolic sine of county-industry's total employment.

Table 3: The Effect of Secure Communities on Minimum Wage Violations

| Dependent variable: | Wage < (Min Wage - $0.25) | | | | | |
|---|---|---|---|---|---|---|
| Sample: | Hourly workers | | | All workers | | |
| Demographic Group | Hispanic | Non-Hispanic | Non-Hispanic, White, non-foreign born | Hispanic | Non-Hispanic | Non-Hispanic, White, non-foreign born |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SC | 0.036*** | 0.014*** | 0.012*** | 0.037*** | 0.013*** | 0.011*** |
| | (0.009) | (0.003) | (0.003) | (0.008) | (0.002) | (0.002) |
| Mean Dep Var | 0.028 | 0.026 | 0.027 | 0.041 | 0.027 | 0.025 |
| # Observations | 73,675 | 252,593 | 179,450 | 102,486 | 471,688 | 344,893 |

*Note:* * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results from estimating the effect of Secure Communities activation on whether a worker is paid below the minimum wage, using data from the CPS ORG, using the Borusyak et al. (2021) difference-in-difference estimator. Each regression controls for the state minimum wage, the respondent's age and age-squared, and indicators if a respondent identifies as female, as Hispanic, and as White. In all regressions, the sample is workers aged 20–64. In Columns 1–3, the sample is restricted to workers who report being paid by the hour.

Table 4: Secure Communities Had Negligible Effects on Hispanic Workers' Employment and Job Turnover

| Dependent variable: | Log of separations by Hispanic workers[a] (1) | Rate of separations by Hispanic workers, scaled by employment[b] (2) | Hispanic worker turnover rate[c] (3) |
|---|---|---|---|
| SC | -0.019 | 0.000 | -0.002 |
| | (0.013) | (0.005) | (0.001) |
| Mean Dep Var (in levels) | 901.77 | 0.28 | 0.13 |
| # Observations | 304,265 | 234,680 | 201,855 |

*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 estimating the effect o Secure Communities on the the dependent shown in the column header, using the Borusyak et al. (2021) difference-in-difference framework.

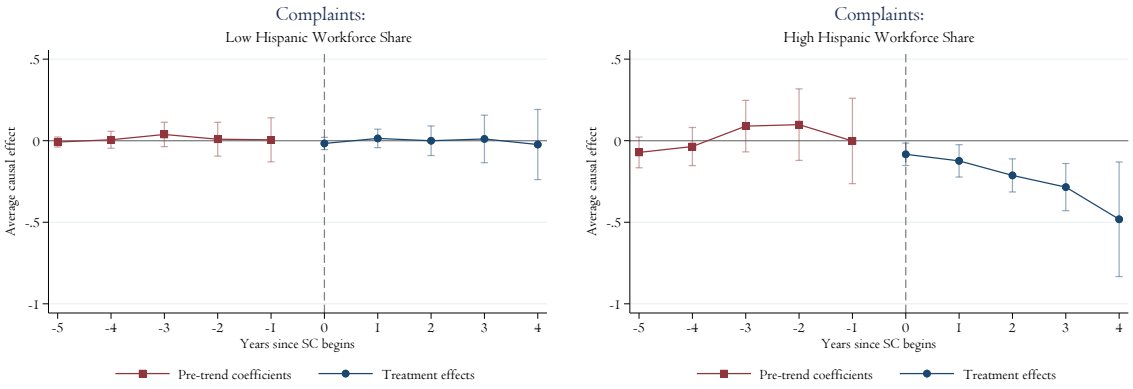
[a] Log of one plus the number of Hispanic workers whose job ended in the relevant quarter

[b] Number 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.

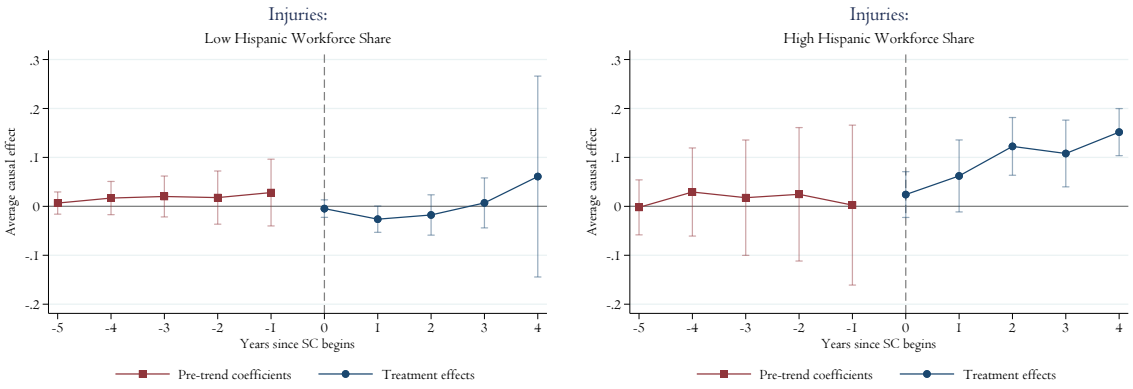
[c] The sum of the number of stable hires of Hispanic workers in the reference quarter and stable separations of Hispanic workers in the next quarter, divided by the average full-quarter employment of Hispanic workers.

Figure 1: Dynamic Effects of Secure Communities on Complaints and Injuries: Event Study Estimates

(a) Complaints



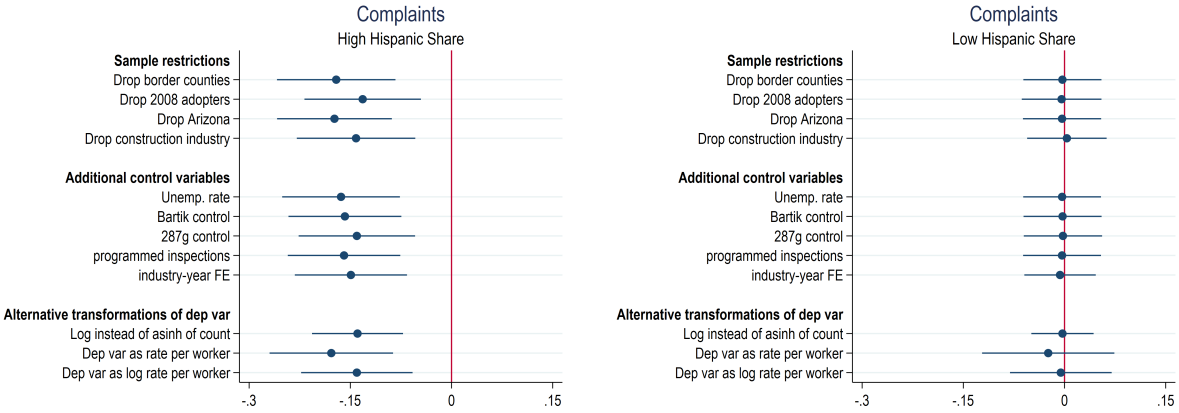

(b) Injuries



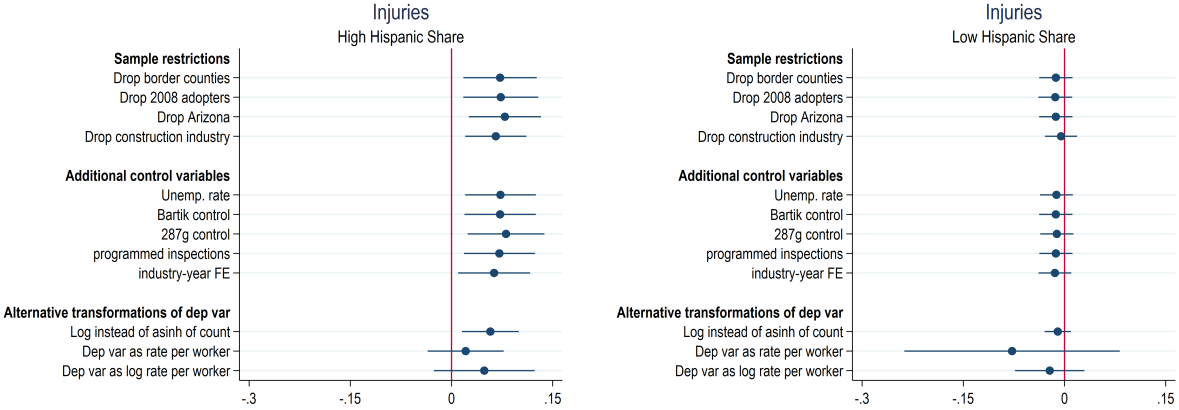

*Note:* Figures show coefficient estimates and 95 percent confidence intervals of the dynamic effects of Secure Communities on worker complaints and workplace injuries, using the Borusyak et al. (2021) difference-in-difference estimator. County–industries with a "High Hispanic Share" are those with a Hispanic workforce share above the 80th percentile of the sample distribution. Each regression controls for the inverse hyperbolic sine of county-industry employment. Standard errors are clustered at the county.

Figure 2: Estimates of Secure Communities on Complaints and Injuries Are Stable Across a Range of Robustness Checks

(a) Effect on Complaints



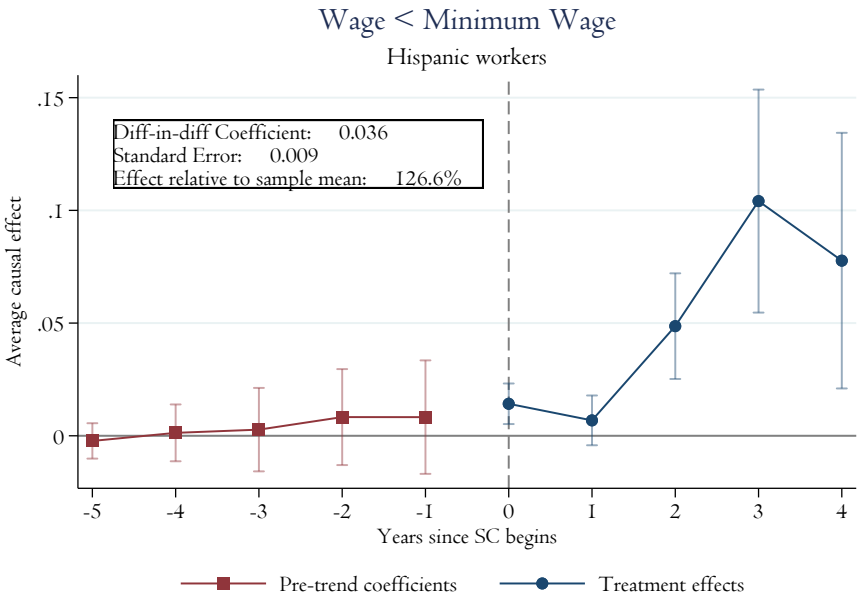

(b) Effect on Injuries



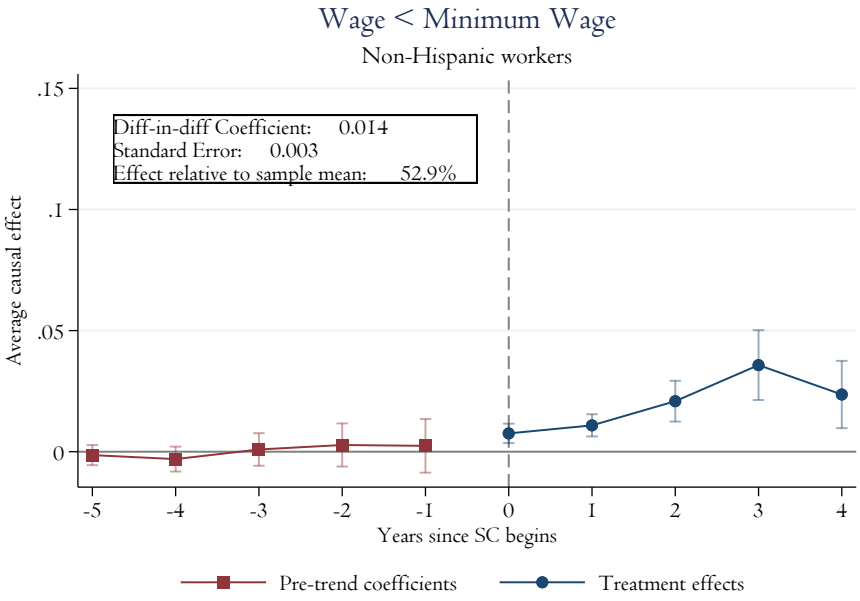

*Note:* Figures shows coefficient estimates and 95 percent confidence intervals estimating the effect of Secure Communities activation on the dependent variable specified in the Panel title, either for county–industries with a Hispanic workforce share above the 80th percentile ("High Hispanic") or below it ("Low Hispanic"). Unless otherwise specified, regressions include controls for the inverse hyperbolic sine of county-industry employment, All regressions estimated using the Borusyak et al. (2021) difference-in-difference framework. Standard errors are clustered by county. See Appendix B for details about each specification.

Figure 3: Dynamic Effects of Secure Communities on Minimum Wage Violations

(a) Hispanic Workers




(b) Non-Hispanic Workers




*Note:* Figures show coefficient estimates and 95 percent confidence intervals of the dynamic effects of Secure Communities on minimum wage violations, using the Borusyak et al. (2021) difference-in-difference estimator. The dataset is drawn from the CPS ORG, and the dependent variable is an indicator equal to one if the respondent reports receiving an hourly wage that is less than 25 cents below the state minimum wage. Regressions are restricted to hourly workers aged 20–64 and include controls for the state minimum wage, the respondent's age and age-squared, and indicators if a respondent identifies as female, as Hispanic, and as White. Standard errors are clustered at the county.

# Appendix

## A Further Details on Data Sources and Construction

### 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.

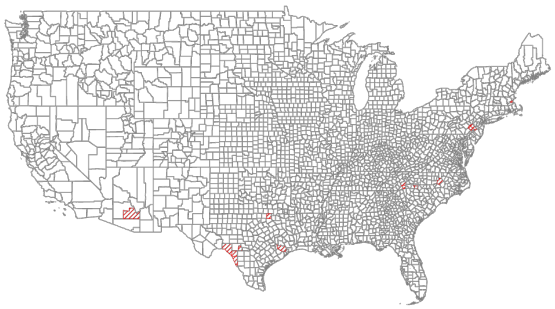
Figure A1 makes clear that Secure Communities started in counties at the Mexico-U.S. border and counties at the Gulf of Mexico. These counties also tend to have a higher Hispanic workforce share, as Figure A3 shows. We formally investigate the correlates of early program adoption in Table A1, where we 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.[28] 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.

[28] 287(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.

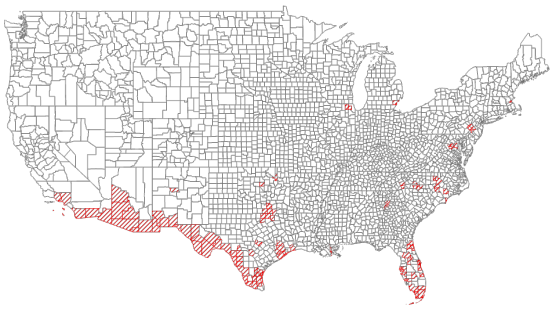
Figure A1: Roll-out of Secure Communities

Red hatches indicate that county participates in Secure Communities

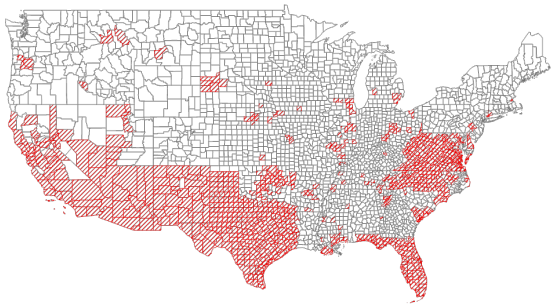
2008



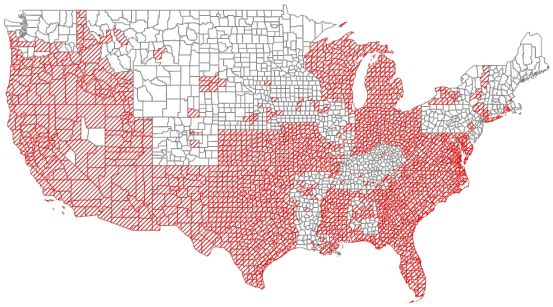
2009



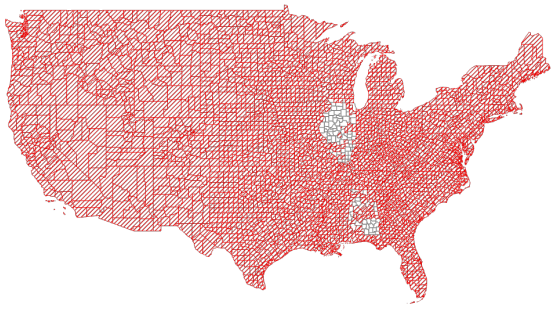
2010



2011



2012



2013

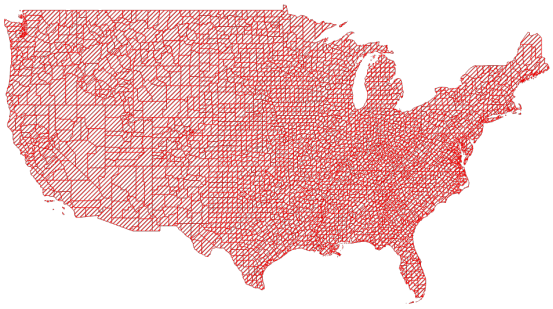


Table A1: Correlates of Early Secure Communities Roll-out

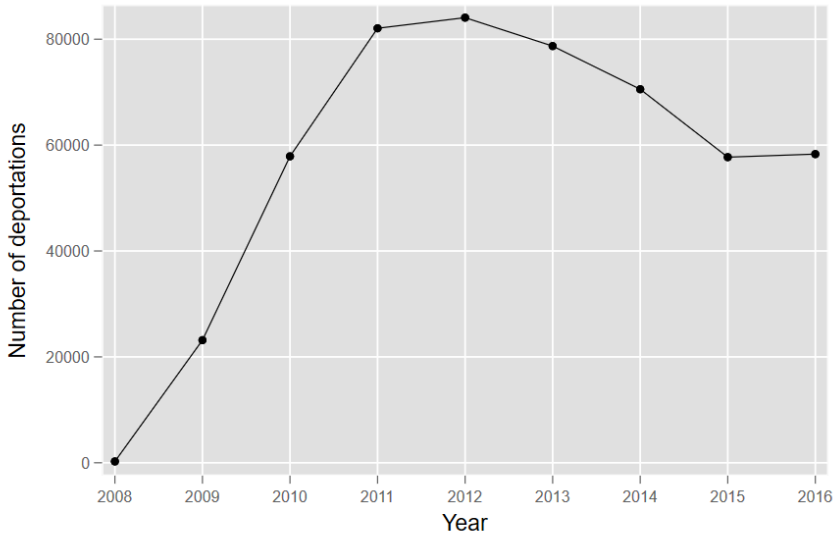
| Dependent variable: County participates in SC in first year | (1) | (2) |
|---|---|---|
| Log(Population 2000) | 0.006** | 0.003 |
| | (0.002) | (0.002) |
| Percent population Hispanic 2000 | 0.001*** | |
| | (0.000) | |
| Percent population non-citizens 2000 | | 0.002*** |
| | | (0.001) |
| Percent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

## 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 opera-

Figure A2: Removals under Secure Communities



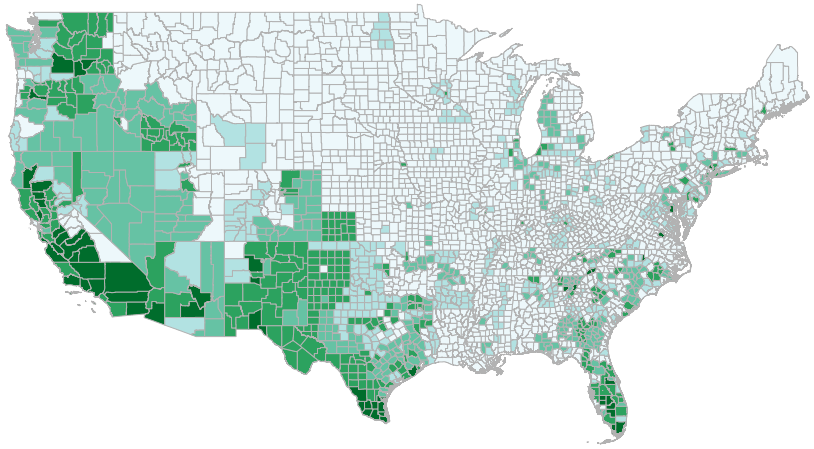

tionalize "industry" roughly as two-digit NAICS codes. We connect counties to PUMAs using the 2000–2010 PUMA crosswalk from the Integrated Public Use Microdata Series.[29] For each year, we then take the average of the Hispanic workforce share over all PUMAs that belong to or comprise a county, weighted by the county's population in each PUMA.[30] Finally, we average shares over 2005–2007. This process 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 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.

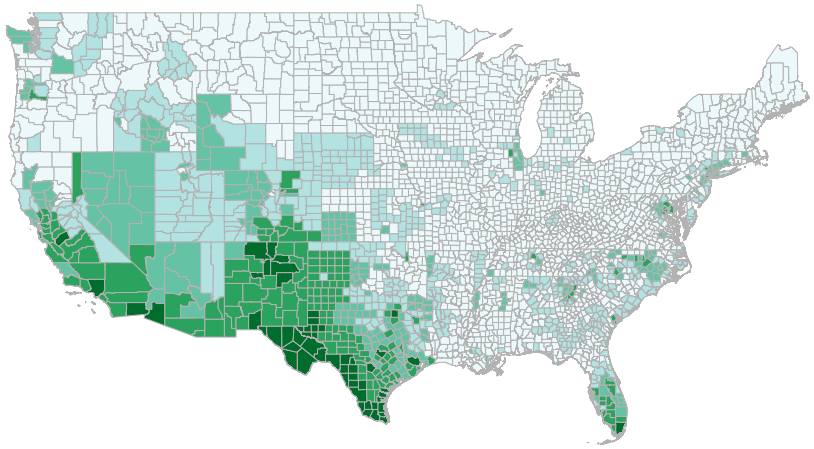
[29] Available at https://usa.ipums.org/usa/volii/pumas10.shtml#crosswalk.
[30] Some PUMAs are larger than counties while some are smaller. PUMA and county borders can intersect.

Figure A3: Spatial Distribution of Hispanic Workforce Share

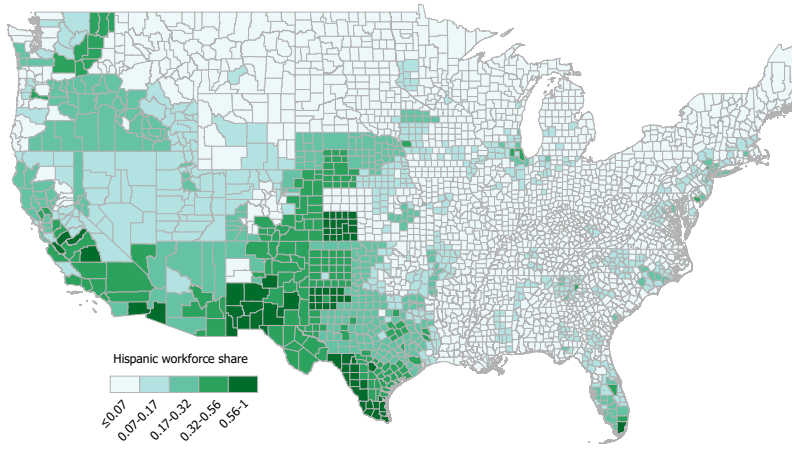
(a) Agriculture



(b) Construction



(c) Manufacturing



# B Robustness Checks

In this section, we describe in detail our various robustness checks that were displayed visually in Figure 2.

**Selection into Secure Communities and Other Confounding Factors:** Table B1 displays regression results for various robustness checks concerning the sample and control variables. The table contains four panels, showing results for complaints separately for "high" and "low" Hispanic share workplaces, and for injuries for "high" and "low" Hispanic workplaces.

We first test if selection into early adoption confounds our estimates by reestimating our regression equation on various subsamples. We drop: all counties at the Mexico–U.S. border (Column 1), any county that adopted Secure Communities in its first year, 2008 (Column 2), and the state of Arizona, which in 2010 passed measure SB 1070, one of the most sweeping and stringent immigration enforcement programs in the country (Column 3). Our results are robust to these restrictions.

Second, we assess the sensitivity of our results to potential confounding effects of the 2007–2009 Great Recession, which in the aggregate coincided with the onset of Secure Communities. Even though the timing of Secure Communities activation was unrelated to geographic variation in severity of the Great Recession (East et al., 2022), it is informative nonetheless to account for it. In Column 4, we estimate our model dropping the construction sector, which was was hit particularly hard by the Great Recession. In Column 5, we next estimate our baseline regression controlling for the county unemployment rate. In Column 6, we follow East et al. (2022) and include a "Bartik"-style measure of labor demand in our regressions (Bartik, 1992). The county Bartik measure is the population-weighted average of the PUMA Bartik measure. The PUMA Bartik measure is a weighted average of nationwide change in industry employment each year, with the weights equal to the industry's share of the PUMA's total employment in 2005. The measure captures overall changes in labor demand in each county, based on its initial industry mix. Our estimates are essentially unchanged in all of these cases.

We next control for other factors that could in theory affect our estimates. In Column 7, we include an indicator equal to one for counties participating in the 287(g) program, a separate voluntary immigration enforcement program that had begun several years earlier, which could be important if the timing of the Secure Communities rollout was correlated with the latent policy environment towards immigration. In

Table B1: Robustness Checks: Selection into Secure Communities and Other Confounding Factors

| | (1) Drop border counties | (2) Drop 2008 adopters | (3) Drop Arizona | (4) Drop construction | (5) Control for unemp. rate | (6) Control for Bartik | (7) Control for 287(g) | (8) Control for programmed | (9) industry-year FE |
|---|---|---|---|---|---|---|---|---|---|
| Panel A: Complaints: "High-Hispanic" workplaces | | | | | | | | | |
| SC | -0.171*** | -0.132*** | -0.174*** | -0.142*** | -0.164*** | -0.158*** | -0.140*** | -0.159*** | -0.149*** |
| | (0.045) | (0.044) | (0.043) | (0.045) | (0.045) | (0.043) | (0.044) | (0.043) | (0.042) |
| Panel B: Complaints: "Low-Hispanic" workplaces | | | | | | | | | |
| SC | -0.003 | -0.004 | -0.004 | 0.004 | -0.004 | -0.003 | -0.002 | -0.004 | -0.007 |
| | (0.030) | (0.030) | (0.030) | (0.030) | (0.029) | (0.030) | (0.030) | (0.029) | (0.027) |
| Panel C: Injuries: "High-Hispanic" workplaces | | | | | | | | | |
| SC | 0.072*** | 0.073*** | 0.079*** | 0.066*** | 0.073*** | 0.072*** | 0.081*** | 0.071*** | 0.063** |
| | (0.028) | (0.028) | (0.027) | (0.023) | (0.027) | (0.027) | (0.029) | (0.027) | (0.027) |
| Panel D: Injuries: "Low-Hispanic" workplaces | | | | | | | | | |
| SC | -0.013 | -0.014 | -0.013 | -0.005 | -0.012 | -0.013 | -0.011 | -0.013 | -0.014 |
| | (0.013) | (0.013) | (0.013) | (0.012) | (0.012) | (0.013) | (0.013) | (0.013) | (0.012) |

* 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 variable denoted in the Panel title, on the sample of either "high" or "low" Hispanic workplaces. "High Hispanic" workplaces are those in a county-industry with a Hispanic workforce share above the 80th percentile of the sample distribution. Each cell is the estimate from a separate regression using the Borusyak et al. (2021) difference-in-difference estimator to estimate the causal effect of Secure Communities activation on the outcome of interest. Each regression controls for the asinh of county-industry employment.

Column 8, we control for the overall level of OSHA's inspection activity by including the inverse hyperbolic sine of programmed inspection counts. Programmed inspections are regularly scheduled OSHA inspections that are neither driven by worker complaints nor by injuries. In Column 9, we include industry-year (as opposed to simply year) fixed effects. In each case, our estimates are unchanged.

**Alternative Dependent Variable Transformations:** Recall that our baseline dependent variable is the inverse hyperbolic sine of complaint- or injury-driven inspections. Since the number of complaints or number of inspections are both count variables bounded at zero, alternative choices could be more appropriate. We check the robustness of our results in three alternative specifications that we report in Table B2.

In Column 1, we use the log of the number of complaints or injuries plus one as the dependent variable and reestimate our difference-in-difference model. We add one to the number of inspections before taking the log to retain observations with zero inspections. Mindful of research showing potential biases that can arise when using log or asinh transformations of variables that contains zeroes (Chen and Roth, 2023), in Column 2 we change the dependent variable to be the rate (per 10,000 employees) of complaints or injuries.[31] For complaints, the coefficient ($\hat{\beta} = -0.375, p < .01$), represents an 18% decrease in complaints

[31] In Figure 2, we rescale this coefficient by the sample mean, to get a magnitude that reflects a percent change, since the coefficients in this specification represent *level* changes in the rate of complaints and injuries due to Secure Communities, whereas each of the coefficients in Figure 2 represent approximate *percent* changes.

relative to the sample mean of 2.1; this proportional effect is essentially identical to what we obtain with our baseline specification using the asinh of the count of complaints. For injuries, the magnitude is attenuated; the coefficient ($\hat{\beta} = 0.020$) is not statistically significant, and represents a 2.4% increase relative to the sample mean of 0.94—quite a bit smaller than what we obtain using the asinh or the log of the count. We hesitate to draw much inference from this attenuation under this specification, for two reasons. First, the distribution of the rate of injuries is massively skewed, such that extreme outliers might create instability when using it as a dependent variable. Second, when we change the dependent variable to be an indicator if there are *any* injuries in the county-industry–year, we obtain a coefficient of $0.058$ that is highly significant ($p < .01$); this implies a substantial extensive margin response of injuries to Secure Communities, which is not surprising given that in our sample over 75% of county-industry–year observations have zero injuries. In Column 3, we use the log of the rate of complaints or injuries per 10,000 employees as the dependent variable. For complaints, the estimate is (proportionally) essentially identical to using either the count or the level of the rate. For injuries, the point estimate ($0.05$) is between what we obtain using the count and using the level of the rate, though it is not statistically signiicant ($p = 0.2$).

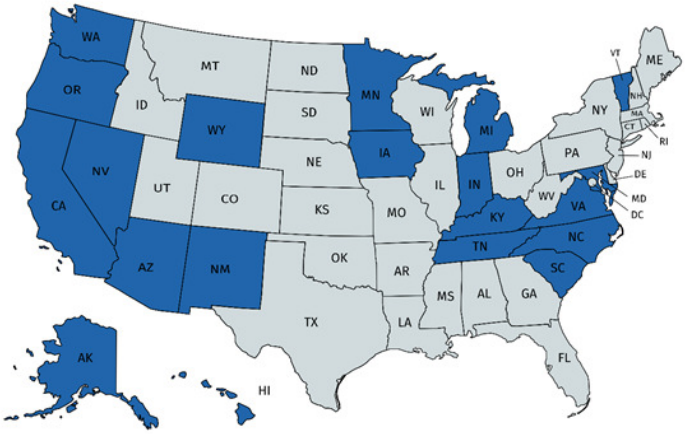
Table B2: Results Are Robust to Alternative Modelling Choices

| Dependent variable: | (1) Log of count | (2) Count per 10,000 workers | (3) Log of count per 10,000 workers |
|---|---|---|---|
| Panel A: Complaints: "High-Hispanic" workplaces | | | |
| SC | -0.139*** | -0.375*** | -0.140*** |
| | (0.034) | (0.098) | (0.042) |
| Panel B: Complaints: "Low-Hispanic" workplaces | | | |
| SC | -0.003 | -0.038 | -0.005 |
| | (0.024) | (0.079) | (0.038) |
| Panel C: Injuries: "High-Hispanic" workplaces | | | |
| SC | 0.058*** | 0.020 | 0.049 |
| | (0.021) | (0.027) | (0.038) |
| Panel D: Injuries: "Low-Hispanic" workplaces | | | |
| SC | -0.010 | -0.016 | -0.022 |
| | (0.010) | (0.017) | (0.026) |

*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 variable denoted in the Panel title, on the sample of either "high" or "low" Hispanic workplaces. "High Hispanic" workplaces are those in a county-industry with a Hispanic workforce share above the 80th percentile of the sample distribution. Each cell is the estimate from a separate regression using the Borusyak et al. (2021) difference-in-difference estimator to estimate the causal effect of Secure Communities activation on the outcome of interest. Column 1 controls for the log of employment, Columns 2 and 3 do not include any control variables.

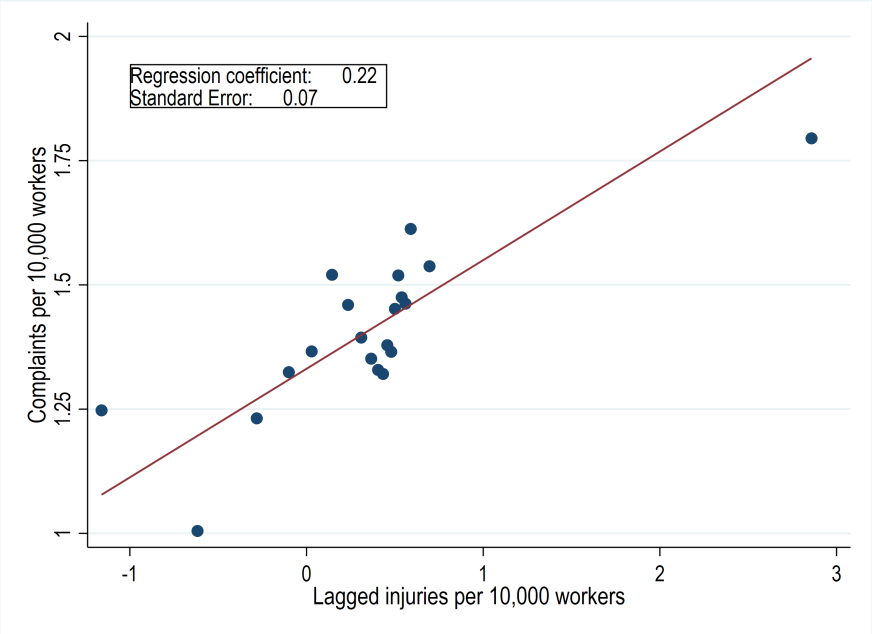
# C Additional Figures and Tables

Figure C1: OSHA directly regulates 28 states (in Gray); the remaining 22 states (in blue) have their own state-run Occupational Safety and Health Plan




*Note:* Private-sector establishments in the 29 states in gray are under federal OSHA jurisdiction. Source: www.osha.gov/dcsp/osp/. Map created with https://mapchart.net/usa.html.

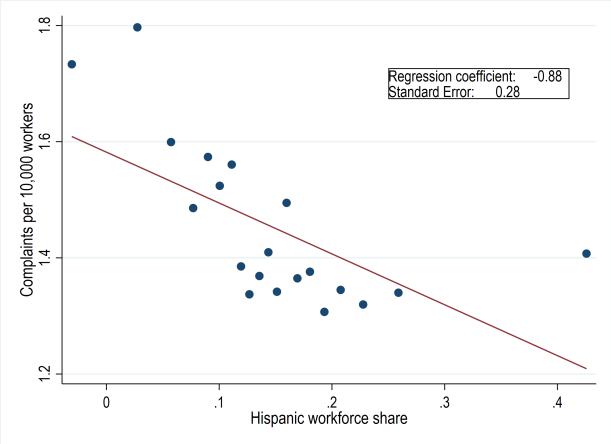
Figure C2: Relationship between Complaint-Driven Inspections and Injury-Driven Inspections



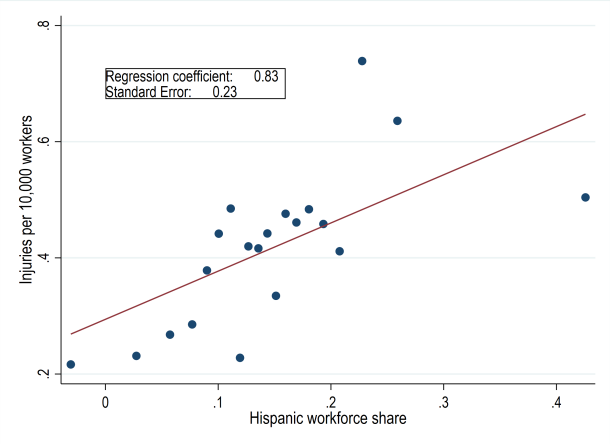

*Note:* The figure shows a binned scatterplot in which observations are weighted by employment. *Lagged Injuries per 10,000 workers* is the rate of injuries averaged over the prior three years. The scatterplot is residualized on the county-industry's annual rate of programmed OSHA inspections, the county's log population and median household income, and fixed effects for industry, the county's metro status, state, and year–OSHA region and year–state-OSHA-plan status.

Figure C3: Correlation between Worker Complaints, Workplace Injuries, and Hispanic Workforce Share

(a) Complaint-driven inspections per 10,000 workers




(b) Injury-driven inspections per 10,000 workers



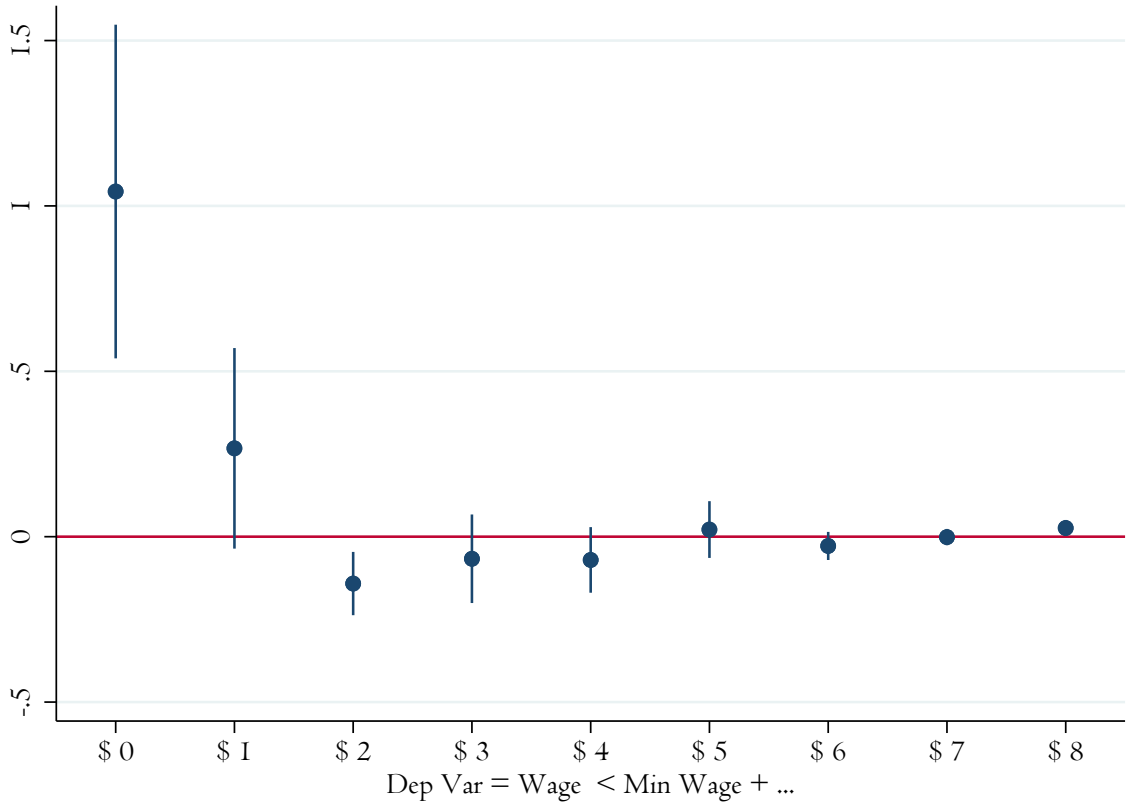

*Note:* The figures show binned scatterplots in which observations are weighted by employment. The *Hispanic workforce share* is the share of workers in a county–industry that identify as Hispanic, averaged over the years 2005–2007, in the ACS. The scatterplot is residualized on the county-industry's annual rate of programmed OSHA inspections, the county's log population and median household income, and fixed effects for industry, the county's metro status, state, and year–OSHA region and year–state-OSHA-plan status.

Table C1: Effects of Secure Communities on Various Categories of Complaints Received by OSHA

| Dependent variable: | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Inverse hyperbolic sine of complaints received by OSHA... | | | | | |
| | ...alleging a serious safety hazard | | ...alleging a serious safety or health hazard | | ...overall | |
| Share Hispanic: | Low | High | Low | High | Low | High |
| SC | -0.007 | -0.149*** | -0.011 | -0.092 | -0.018 | -0.066 |
| | (0.025) | (0.055) | (0.029) | (0.056) | (0.028) | (0.041) |
| Mean Dep Var (in levels) | 2.79 | 5.22 | 4.56 | 7.85 | 6.04 | 16.96 |
| Mean share Hispanic | 0.06 | 0.38 | 0.06 | 0.38 | 0.06 | 0.38 |
| # Observations | 300,590 | 36,670 | 300,590 | 36,670 | 300,590 | 36,670 |

*Note:* * p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Each column shows results of a separate regression in which the dependent variable is the inverse hyperbolic sine of the variable noted in the column header. The key explanatory variable, $SC$ is an indicator equal to 1 beginning the year a county participated in Secure Communities. "High Hispanic share" is equal to 1 if the county-industry's Hispanic share is above the 80th percentile. All regressions are estimated with the Borusyak et al. (2021) approach and control for the inverse hypoerbolic sine of county-industry's total employment.

Figure C4: Secure Communities Increased the Probability that Hispanic Workers were Paid Below the Minimum Wage, but not Below Higher Wage Thresholds




*Note:* Each dot represents an estimate from a separate regression, which estimates the effect of Secure Communities activation on the probability that a worker reports an hourly wage that is below the threshold denoted on the x-axis. Each regression uses the Borusyak et al. (2021) difference-in-difference estimator, and the dependent variable is an indicator equal to 1 if the reported hourly wage is below the corresponding threshold. To facilitate interpretation across models, each dot is the coefficient from the relevant regression divided by the sample mean of the dependent variable. The sample is Hispanic individuals aged 20–64, and each model controls for the state's effective minimum wage, the worker's age and age-squared, and indicators if the worker identifies as White and as female. The band represents the 95 percent confidence intervals. See Section 5 for details.