Complaint-Driven Enforcement of Labor Regulations*

Amanda M. Grittner Matthew S. Johnson

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Abstract

Regulatory agencies overseeing the labor market rely on complaints by workers to target their enforcement resources. Such a system is effective if workers are more likely than centralized agencies to know where illegal working conditions are; moreover, workers' threat of complaining might incentivize employers to improve working conditions. However, if workers face differential barriers to complaining, then complaint-driven enforcement could exacerbate existing inequalities in the labor market. We examine this trade-off in the context of the US Occupational Safety and Health Administration (OSHA). On average, worker complaints to OSHA arise more frequently at more hazardous workplaces. However, this relationship reverses for workplaces with larger shares of Hispanic workers, a group that likely faces high barriers to complain. Furthermore, immigration enforcement reduces complaints to OSHA among workplaces with Hispanic workers and leads these workplaces to experience *more* injuries, indicating that workers' propensity to complain affects employers' safety inputs.

*Grittner: Duke University, amanda.grittner@duke.edu; Johnson: Duke University, matthew.johnson@duke.edu. We thank Jesse Bruhn, Rob Garlick, Kevin Lang, Arnaud Maurel, Marjorie McElroy, and Seth Sanders for guidance and comments, as well as seminar participants at Boston University and Duke University.

1 Introduction

Regulatory agencies overseeing the labor market use inspections and investigations as their primarily tools to monitor and enforce compliance with their regulatory standards. These agencies heavily rely on complaints of unsafe or illegal working conditions by workers to target their enforcement. Worker complaints initiated 78 percent of inspections conducted by the Wage and Hour Division of the Department of Labor in 2004 (Weil and Pyles, 2005) and 19 percent of inspections conducted by the Occupational Safety and Health Administration (OSHA) between 2006 and 2016. The Equal Employment Opportunity Commission (EEOC) relies exclusively on employees' complaints to enforce civil rights laws against workplace discrimination.[1]

A regulatory system that relies on complaints can be effective if complaints arise at the most dangerous or most exploitative workplaces. Workers are more likely than centralized agencies to know where hazardous working conditions, sub-minimum wage payments, and other illegal practices occur. Directing enforcement to locations of reported misconduct can, thus, ensure that these resources are allocated where problems are greatest. Furthermore, such a system will improve overall working conditions if the *threat* of workers' ability to complain to federal agencies deters employers from providing illegal or hazardous working conditions in the first place. At the same time, this strategy hinges on workers' ability and willingness to complain to a federal agency. Fear of retaliation, lack of information, and lower bargaining power vis-à-vis her employer can all make a worker less likely to complain (Weil and Pyles, 2005). If poor working conditions and the factors that inhibit workers' propensity to complain are positively correlated, then complaint-driven enforcement is unlikely to direct regulatory resources where they are most needed and may exacerbate existing inequalities in working conditions across the labor market.

Understanding this trade-off is essential if regulatory agencies are to play an effective role in promoting beneficial outcomes for workers. Effective targeting of inspection resources is crucial given regulatory agencies' limited resources: OSHA, for example, was able to inspect less than 1 percent of the eight million workplaces it regulated in 2016.[2]

We examine this trade-off in regulatory design in the setting of occupational safety and health regulation. We first assess whether workers are *on average* more likely to complain to the government when they face higher workplace hazards. We then investigate whether this relationship changes for a particular group of workers likely to face high barriers to complain—namely, Hispanic workers.[3] Finally, we analyze if immigration enforcement affects Hispanic workers' willingness to complain about hazardous workplace conditions as well as the actual job hazard that Hispanic workers face.

Focusing on Hispanic workers enables us to examine the trade-offs inherent in relying on worker com-

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

[2] OSHA Commonly Used Statistics, available at https://www.osha.gov/oshstats/commonstats.html

[3] We use the term "Hispanic" and "Latino" interchangeably to refer to individuals of Cuban, Mexican, Puerto Rican, South or Central American, or other Spanish culture or origin, including Spaniards, regardless of race. Our definition is based on the American Community Survey.

plaints to target enforcement. Hispanic workers are more likely than their white, non-Hispanic counterparts to work in hazardous workplaces and to suffer work-related injuries (Dong et al., 2010). At the same time, Hispanic workers likely face particularly high barriers to complaining to government agencies. One salient barrier is that Hispanic individuals are more likely to live with an undocumented immigrant, or to be undocumented immigrants themselves than other ethnic groups in the United States (Hall et al., 2019) .[4] As a result, Hispanic individuals—regardless of their own immigrant status—might worry more that any government interaction will increase the chance that they, a friend, or a family member could be deported (Hall et al., 2019; Maslin and Sonenshein, 2017). Hispanic workers may thus seek to limit their interaction with government agencies, including by complaining about working conditions.

Heightened immigration enforcement could exacerbate Hispanic workers' reluctance to report illegal conditions to the government. One example is illustrative. In July 2005, Immigration and Customs Enforcement (ICE) agents, pretending to be OSHA officials, informed the group of largely Hispanic construction workers at an Air Force Base in Goldsboro, North Carolina that there would be a mandatory workplace safety and health meeting the following day. When workers showed up, ICE performed a "sting" immigration raid and arrested undocumented workers on the spot.[5]

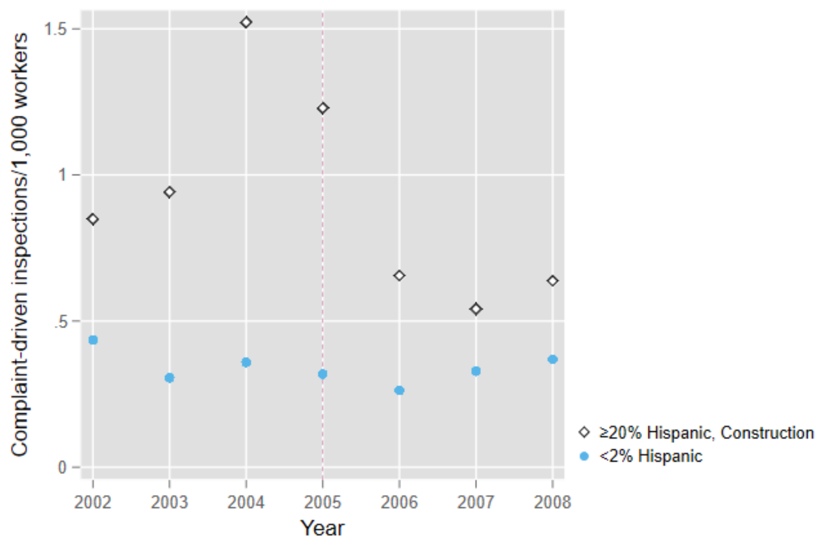
Figure 1 provides evidence that this event had a chilling effect on worker complaints to OSHA. The figure shows the annual number of complaints to OSHA per 1,000 workers in North Carolina before and after the 2005 Goldsboro raid. Complaints among workplaces with a high share of Hispanic workers dropped substantially after 2005 (we describe in Section 3.2 how we define "high share of Hispanic workers"), whereas complaints at workplaces with few Hispanic workers stayed essentially flat. More recent anecdotal evidence suggests that the risks to undocumented workers of engaging with government safety officials was not confined to the Goldsboro immigration raid. In 2019, after the deadly collapse of a construction site for a Hard Rock Hotel in New Orleans, U.S. Customs and Border Patrol agents arrested and deported a construction worker who had filed a lawsuit seeking damages for injuries against the project owners (Hassan, 2019).

Beyond affecting workers' willingness to complain, immigration enforcement may also change the hazards workers face on the job. If forward-looking employers know their workers are unlikely to alert government agencies about unsafe or illegal conditions, they expect less enforcement of workplace rules and regulations. This makes it less costly for employers to violate workplace safety standards, minimum wage laws and other labor market regulations. Recent news articles illustrate this potential behavioral response. In an era of enhanced immigration crackdowns, reports indicate that Latino immigrant workers have been en-

[4]There are no direct measures of the percentage of the Hispanic population that is undocumented, but several indirect measures suggest that Hispanic individuals are more likely to be undocumented than non-Hispanic individuals. According to the ACS, 29% of Hispanic or Latino individuals were foreign-born non-citizens in 2005, compared to 4% of non-Hispanic Black or African American individuals and 2% of non-Hispanic white individuals (U.S. Census Bureau, 2005). An estimated 9,850,000 undocumented immigrants in the U.S. were from Latin America in 2007, accounting for 22% of the total Hispanic/Latino population (Passel and Cohn, 2019; Bureau, 2007).

[5]Greenhouse, Steve (2005). Immigration Sting Puts 2 U.S. Agencies at Odds *The New York Times*, July 16. url:https://www.nytimes.com/2005/07/16/politics/immigration-sting-puts-2-us-agencies-at-odds.html.

Figure 1: After the 2005 Goldsboro Raid, Complaints to OSHA Dropped Among North Carolina Workplaces with Hispanic Workers



during unpaid wages, untreated injuries, and various abuses at work while being unwilling to alert authorities (Mehrotra et al., 2018).

To analyze the trade-offs in complaint-based targeting formally, we must overcome several data hurdles. The most salient of these is that we lack establishment-level data on workplace demographics. Instead, we leverage variation in the share of workers that are Hispanic across counties in a given industry. We operationalize "industry" roughly as 2-digit codes based on the North American Industry Classification System (NAICS). We obtain the share of workers that are Hispanic for each industry and county in 2005, 2006 and 2007 from the American Community Survey (ACS).[6] We then 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 to OSHA and serious workplace injuries at the county-industry level. We focus our main analysis on agriculture, construction and manufacturing since these sectors have both the highest average Hispanic workforce shares and relatively high rates of worker complaints and workplace injuries.[7]

We find correlational evidence that – in general – complaint-driven enforcement targets inspections to locations with hazardous workplace conditions. A county-industry's complaint rate (the rate of complaint-driven inspections per worker) is positively associated with its injury rate (rate of injury-driven inspections per worker) in the previous year. This implies that worker complaints *generally* lead the regulator to workplaces with relatively high hazardous conditions.

[6]More precisely, we obtain these shares for each public use microdata area (PUMA) and map these to counties using a crosswalk. See Section 3.2 for details.

[7]Agriculture, construction and manufacturing, together with hospitality, were also the most common industries of employment for unauthorized immigrants between 2012 and 2016 (Gelatt and Zong, 2018).

However, this relationship changes for workplaces with a high share of Hispanic workers. We find striking evidence of an *increasing*, roughly linear relationship between a county-industry's injury rate and the share of its workers that are Hispanic—conditioning on industry, employment, and several other characteristics. In contrast, there is a *decreasing* relationship between a county-industry's complaint rate and the share of its workers that are Hispanic. This inverse relationship is even stronger when we consider alternative measures of worker demographics, such as the share of a county-industry's workers that are non-citizens or Hispanic non-citizens, rather than the broader "Hispanic" category. These results provide suggestive evidence that worker complaints do not direct regulators to the most hazardous workplaces in settings where a large share of workers is likely reluctant to file a complaint.

To examine if this correlational evidence reflects a causal relationship, we then investigate if immigration enforcement exacerbates Hispanic workers' reluctance to complain and the hazards they face. We use counties' participation in Secure Communities (SC) to measure the strength of local immigration enforcement. Under this program, which was rolled out across the United States between 2008 and 2013, local agencies were required to share fingerprints of individuals arrested for non-immigration matters with ICE, which then determined whether to start deportation proceedings.[8] Because Secure Communities substantially raised the risk of deportation, it might have dissuaded both undocumented immigrant workers, and Hispanic workers more broadly, from reporting unsafe or illegal workplace conditions to OSHA – even though OSHA is independent from ICE.

To identify the causal effect of immigration enforcement, we use triple-difference regressions that compare the rates of inspections initiated by worker complaints across counties, industries and years. Our identifying variation comes from differences in the timing of counties' participation in Secure Communities and from differences in the Hispanic workforce share across industries and counties.

We find that counties' participation in Secure Communities significantly reduced complaint rates in workplaces with a high share of Hispanic workers. One way to convey the magnitude of the effect is to consider how the activation of Secure Communities affected the difference in complaint rates between county-industries that employ 0 percent vs. 100 percent Hispanic workers. Our estimates imply that Secure Communities reduced complaints by at least 18 percent among county-industries with a 100 percent Hispanic workforce, relative to county-industries with 0 percent Hispanic workforce share. This reduction in complaints is even larger when we account for measurement error in our *Hispanic Share* variable. Event study estimates reveal that this chilling effect on complaints showed up immediately in the year Secure Communities began, persisted for several years, and increased over time.

Forward-looking employers would recognize that a reduction in their workers' willingness to complain to OSHA reduces their effective cost of violating workplace safety standards. Because complying with workplace safety regulations is costly, employers may increase job hazards in the face of immigration enforcement. We find evidence consistent with this employer response. We estimate that Secure Communities

[8] See Section 3.3 for details.

led to a substantial increase in injury rates among workplaces with Hispanic workers. Workplace injury rates at workplaces with a 100 percent Hispanic workforce share increased by at least 16 percent, and potentially as high as 30 percent, relative to workplaces with a 0 percent Hispanic workforce share.

Our estimates are robust to accounting for measurement error in our exposure variables, alternative measures of exposure to Secure Communities, addressing the selection of counties into early adoption of Secure Communities, and controlling for potential confounding factors such as the intensity of the Great Recession.

Finally, we conduct three analyses to provide evidence that the effects we estimate are consistent with immigration enforcement a) deterring Hispanic workers from complaining to government agencies, and b) subsequently leading employers to increase workplace hazards. First, we show that our results are not explained by industries replacing Hispanic with non-Hispanic workers, which could increase workplace injuries simply by increasing turnover and the rates of inexperienced workers. Second, we show that Secure Communities led to more violations of workplace safety and health regulations among workplaces with more Hispanic workers, implying that employers reduced inputs into safety. Third, we show that the effects of immigration enforcement on complaints and injuries are entirely eliminated at workplaces where workers are represented by a labor union. Because unions provide protection for workers to anonymously file complaints to OSHA, immigration enforcement does not have any chilling effect on unionized workers' willingness to complain.

Our findings imply that relying on worker complaints to target inspection efforts is inadequate where external factors – such as immigration enforcement – deter workers from reporting hazardous or otherwise illegal working conditions. In these settings, a system that uses worker complaints to target inspections is not only an ineffective use of enforcement resources, but it also exacerbates existing inequalities in working conditions.

2 Literature Review and Conceptual Framework

In this section, we provide a brief discussion of the rights that workers have to complain about working conditions to the government and the barriers workers might face in exercising these rights. We explain why these barriers would be particularly high for Hispanic workers and especially immigrant Hispanic workers, especially in an era of enhanced immigration enforcement. Finally we illustrate how these barriers that workers face would affect employers' inputs into compliance with labor regulations and worker safety. We simultaneously describe our contribution to various literatures.

Workers have a legal right to complain to agencies in several regulatory domains of the labor market, including wage and hour standards (according to the 1938 Fair Labor Standards Act) and occupational safety and health standards (according to the 1970 Occupational Safety and Health Act). Complaining might produce an array of benefits. For example, complaining to OSHA nearly always triggers an OSHA inspection, and several studies have shown that OSHA inspections improve working conditions (Levine et al., 2012; Haviland et al., 2012; Li and Singleton, 2019). The benefits to complaining would thus be increasing in the 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 – i.e. leaving the job – is not available or preferred (Hirschman, 1970).

There are various reasons why workers might be unlikely to exercise this right to complain—even when they face hazardous or illegal conditions. Workers might be unlikely to complain if they do not know their rights under the complaint process, fear employer retaliation, or do not believe that complaining will be effective (Alexander and Prasad, 2014).[9] An individual worker might not complain because she does not internalize the full benefit of doing so; if a complaint by one worker triggers a change in working conditions that affects all workers at the establishment, then the "public good" of complaints will be under-provided (Weil and Pyles, 2005). Indeed, Weil and Pyles (2005) find that violations of labor standards predict very litle of the overall variation in rates of workers' complaints about workplace rights.

These barriers are likely to be particularly pronounced for immigrant workers, especially those of Hispanic or Latino descent. Prior descriptive and theoretical work suggests that immigrant workers are less likely to report unsafe workplace conditions or workplace injuries because they do not know their rights, do not know how to formally complain, face language barriers, have fewer employment options and fear employers' reprisal and deportation (Mehrotra et al., 2018; Rathod, 2010).[10] These barriers to complain 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 Whites or African Americans (Hall et al., 2019). As a result, they tend to worry more that any government interaction will increase the chance that they, a friend, or a family member may be deported (Maslin and Sonenshein, 2017). Indeed, prior studies examining other aspects of immigration enforcement either assume or directly show that immigration enforcement affects the behavior of Hispanic individuals, regardless of their own immigrant status (Alsan and Yang, 2018; Dee and Murphy, 2018; Comino et al., 2016; Rugh and Hall, 2016).

Further institutional factors make it particularly costly for undocumented immigrants, as well as their family members, friends or co-workers, to complain. The U.S. Supreme Court's decision *Hoffman Plastic Compounds v NLRB* established that undocumented workers do not have the same protection from unfair labor practices as citizens and legal immigrants do.[11] This decision reduced the benefits to complaining for

[9]Technically, the 1970 Occupational Safety and Health Act protects workers against employer retaliation for complaining to OSHA. However, enforcement of this protection is infamously non-existent, and workers face exceedingly high barriers to take advantage of these protections (Weatherford, 2013). Tellingly, OSHA's own inspectors do not believe that the legal protections offered by the 1970 Act are effective (Government Accountability Office, 1990).

[10]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 individuals.

[11]In the Hoffman Plastic Compounds, Inc v. NLRB case, Hoffman laid off an undocumented worker who had participated in union organizing activities (535 U.S. 137 (2002)). Attorneys for the worker and three other laid off employees brought charges for unfair labor practice before the National Labor Relations Board (NLRB). The NLRB found that Hoffman had unlawfully targeted

undocumented workers. It also highlighted the cost of complaining; the potential detection and deportation of undocumented workers.

Heightened immigration enforcement—by raising the risk of deportation—could create an even larger chilling effect on Hispanic workers' willingness to complain to the government. Prior work has documented that stronger local immigration enforcement reduced the use of social safety net programs like food stamps and subsidized health care among Hispanic individuals – even those who are citizens (Alsan and Yang, 2018). Closely related to our paper, Grittner (2019) shows that stronger local immigration enforcement reduced help-seeking of Hispanic domestic violence victims. Prior work has also shown that immigration enforcement reduced employment rates of individuals most likely to be undocumented (East et al., 2018); this reduction in job prospects could further increase the perceived costs of complaining among Hispanic workers.[12]

Stronger immigration enforcement could also affect employers' behavior. By making workers less likely to exercise their rights to complain, immigration enforcement reduces the probability that the employer will receive an OSHA enforcement inspection. As a result, immigration enforcement reduces the expected value of future OSHA fines for noncompliance and lowers the cost of employers' non-compliance with workplace safety regulations. Stronger immigration enforcement could reduce employers' expected costs of injuries for other reasons. Enforcement might dissuade immigrant workers from filing for workers' compensation if they get injured at work, especially if they fear employer retaliation.[13] In consequence, stronger immigration enforcement could lower employers' effective costs of maintaining a hazardous workplace and lead to more hazardous working conditions in workplaces with many immigrant workers (Becker, 1968).

We expect the factors just described—that Hispanic workers face barriers to complain about working conditions, that immigration enforcement would exacerbate these barriers, and that employers would in turn increase hazards— to be less pronounced in unionized workplaces. Unions can solve the "public good" issue of filing complaints; they also inform workers about their rights to complain, explain how to file a complaint, and protect workers from employer retaliation (Weil and Pyles, 2005). Moreover, unions independently induce employers to address safety and health issues (Morantz, 2018): they include specific safety and health provisions in contract agreements, enable injured workers to file for workers' compensation (Hirsch et al., 1997) and strengthen the frequency, duration, and intensity of regulatory inspections (Morantz, 2011). Labor unions could thus mitigate any chilling effect of immigration enforcement on Hispanic workers'

the workers because of their union involvement and ordered Hoffman to reemploy the workers with back pay - irregardless of documentation status. Hoffman appealed and the U.S. Supreme Court ultimately decided that undocumented workers are not entitled to the remedy of back pay for violations of the National Labor Relations Act (Rathod, 2010).

[12]East and Velasquez (2018) also find that local immigration enforcement had spillover effects, reducing employment rates of U.S. citizens. Other studies have investigated effects of immigration enforcement on outcomes less related to our study, such as school attendance and performance and health care visits (Dee and Murphy, 2018; Amuedo-Dorantes and Lopez, 2015, 2017; Rhodes et al., 2015).

[13]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 file work-related injuries to workers' compensation, often because they fear employer retaliation (Biddle and Roberts, 2003; Fan et al., 2006; Bernhardt et al., 2009).

willingness to complain and speak up about safety issues.

3 Data

To undertake our analysis, we need measures of workplace complaints and injuries, a measure of workplace demographics, and a measure of localized immigration enforcement. We discuss the data we use for each of these measures in turn.

3.1 Measuring the Incidence of Workplace Complaints and Injuries with OSHA Inspections

We measure counts of worker injuries and complaints using the occurrence of inspections conducted by OSHA that are triggered by these respective events. OSHA, created in 1970, is the federal regulatory agency charged with ensuring "safe and healthful working conditions" in the U.S. by setting and enforcing standards.[14] Most private-sector employers are required to comply with hundreds of OSHA standards, which range from the maintenance of specific capital equipment to more general restrictions on exposing workers to particular hazards. Twenty-eight states (and Washington DC) are under OSHA's jurisdiction; the remaining 22 states have their own OSHA-approved state-run occupational safety and health plan. However, the description below pertains to both the federal OSHA and its various state-run counterparts.

Inspections are OSHA's primary tool for monitoring compliance with its health and safety standards. During inspections, inspectors review paperwork and tour a facility's operations to assess its hazards and compliance with standards. When an inspector finds a facility to be out of compliance with any standards, she issues citations for each violation she observes.

OSHA inspections can be initiated for multiple reasons. The two reasons most relevant to this paper are serious accidents and worker complaints.

A serious accident – also called a "catastrophe" by OSHA – is an accident that results in a worker fatality or the hospitalization of three or more workers. In the event of a serious accident, the employer is required by law to report it to OSHA and OSHA is required to inspect the workplace. Prior research has concluded that the occurrence of an OSHA inspection triggered by an accident is a reliable measure of the occurrence of fatal and serious non-fatal work-related injuries (Mendeloff and Kagey, 1990).

Additionally, the Occupational Safety and Health Act of 1970 endows workers with the right to complain to OSHA if they are experiencing hazardous or illegal working conditions. Workers can file these complaints electronically, by mail, by phone, or in person. OSHA takes explicit measures to attempt to hide the worker's

[14] OSHA, *About OSHA*, available at https://www.osha.gov/aboutosha, accessed July 28, 2020.

identity from the employer. Most worker complaints result in an inspection of the employer.[15] Thus, the occurrence of OSHA inspections that are triggered by a worker injury or complaint are indications that a serious injury or a complaint has taken place, respectively.

OSHA inspections can occur for other reasons. "Programmed" inspections, which make up roughly 60% of OSHA's overall inspections, focus on particular industries or hazards. These inspections are pursuant to OSHA's National Emphasis Programs, which focus on nationwide priorities, or Local Emphasis Programs, which focus on regional priorities. Because programmed inspections target facilities only based on them being in a particular industry or possessing a specific hazard, they are exogenous to any facility-specific factors such as recent injuries.[16]

We identify the occurrence of OSHA inspections using OSHA's Integrated Management Information System (IMIS), which is a database that contains detailed information on every OSHA inspection conducted since the late 1970s.[17] Key variables in the database are the date the inspection is opened, the reason the inspection was initiated (accident, complaint, referral, programmed, other), facility characteristics (name, address, industry, etc.), and whether or not the workers at the inspected establishment were represented by a labor union. To obtain the county of each inspection, we used geocoding to obtain the latitude and longitude of each address and matched the coordinates to its associated county.

We collapse the IMIS dataset to create a dataset with the annual number of complaint- and injury-driven inspections for each county-industry. We calculate the number of inspections per 100,000 workers using annual data on county-industry employment from the Bureau of Labor Statistics Quarterly Census for Employment and Wages .[18]

The IMIS dataset also includes a detailed report of each violation found (if any) at each inspection, including the OSHA standard that was violated. For each violation that the inspector finds, the inspector and the associated Area Office director categorize the severity of the violation into one of four categories: serious, repeat, willful, and other-than-serious. A violation is "serious" if OSHA determines that it results in a high probability of death or serious harm; it is "repeat" if OSHA has cited the employer for the same condition or hazard in the past; it is "willful" if the employer has demonstrated either an intentional disregard for OSHA standards or a plain indifference to employee safety and health; it is "other-than-serious" if it would probably not result in death or serious harm. For one analysis, we preserve the IMIS dataset at the inspection level and calculate the total number of violations detected for each inspection, as well as the total number of violations under each of the four categories.

[15] In some cases, when the complaint is not considered "formal", e.g. there is no individual signature accompanying the complaint, OSHA deals with the complaint by corresponding via phone or mail with the employer, rather than an inspection. In rare cases, OSHA decides that the complaint is not warranted and dismisses it.

[16] 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.

[17] IMIS can be downloaded here: https://enforcedata.dol.gov/views/data_summary.php.

[18] Available at https://www.bls.gov/cew/downloadable-data-files.htm

3.2 Measuring Workforce Demographics

In Section 2, we discussed why some workers would face relatively high barriers to complaining to the government for a given level of job hazard. We focus on three particular groups: Hispanic or Latino workers (our primary measure), foreign-born non-citizen workers, and Hispanic foreign-born non-citizen workers.

Ideally, we could observe these demographic measures at each establishment and connect it to the establishment-level complaint- and injury-driven inspections. However, we are unaware of any dataset that includes establishment-level worker demographics and covers all establishments in the United States. We overcome this hurdle by leveraging variation in the share of workers that are Hispanic (or foreign-born non-citizens or Hispanic foreign-born non-citizens) across counties in a given industry.

More specifically, we obtain the share of workers that are Hispanic for each industry and Public Use Microdata Area (PUMA) in 2005, 2006 and 2007 from the 1% sample of the ACS.[19] We operationalize "industry" roughly as 2-digit NAICS codes. We use the years 2005–2007 because they precede the start of Secure Communities, the program we use to measure the intensity of local immigration enforcement (see Section 3.3).

We connect counties to PUMAs using the 2000-2010 PUMA crosswalk from the Integrated Public Use Microdata Series.[20] 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.[21] 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.

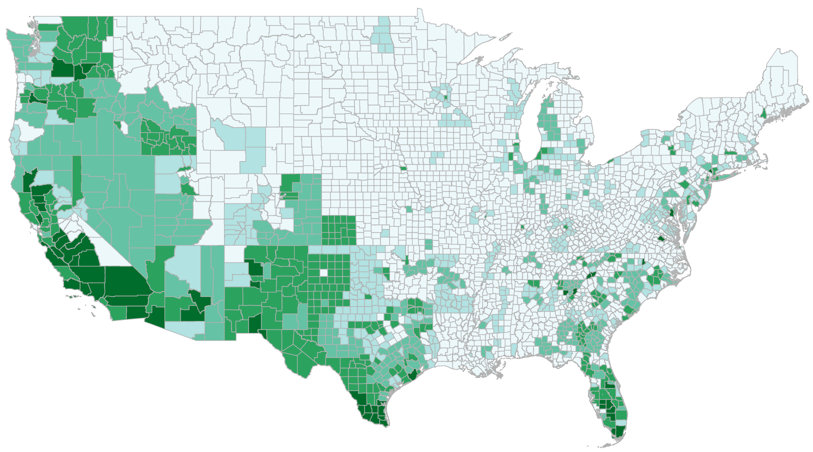
Hispanic workforce shares vary widely across both counties and industries. Figure 2 shows the spatial distribution of the Hispanic workforce share in agriculture, construction and manufacturing. There are clear differences in the Hispanic workforce share within industries, across counties. For example, zero 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.

One concern with our approach to measuring the Hispanic workforce share of county-industries is that we are using a sample statistic – the share of individuals in a 1% representative sample of the U.S. (the 2005–2007 ACS) that work in a PUMA-industry and are Hispanic – to infer a population statistic (the share of total workers in that PUMA-industry that are Hispanic). This approach inevitably introduces classical measurement error, which will bias our regression estimates towards zero. This measurement error is likely more pronounced for county-industries with relatively few workers. For such county-industries, sampling

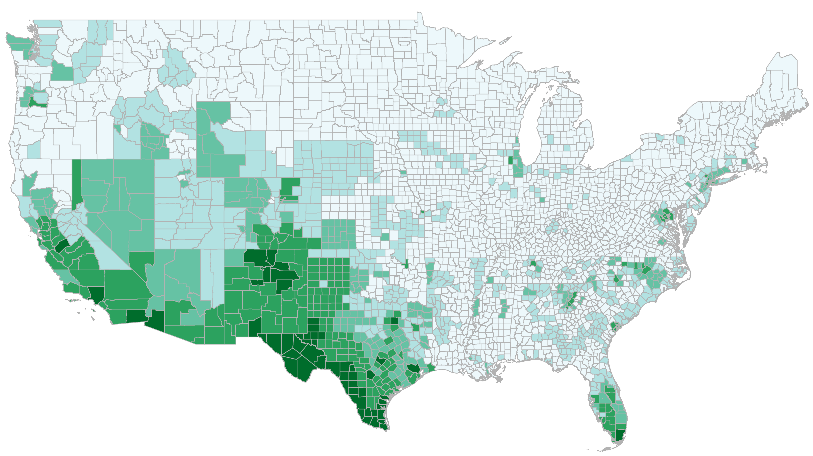
[19] Available at https://usa.ipums.org/usa/
[20] Available at https://usa.ipums.org/usa/volii/pumas10.shtml#crosswalk
[21] Some PUMAs are larger than counties while some are smaller. PUMA and county borders can intersect.

Figure 2: Spatial distribution of Hispanic workforce share

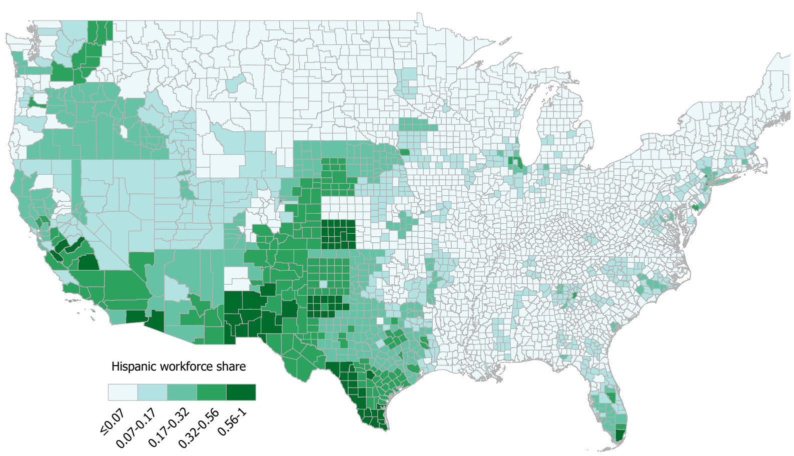
(a) Agriculture



(b) Construction



(c) Manufacturing



variation could introduce large swings in the Hispanic workforce that we measure. In Appendix A, we investigate the degree of this measurement error and its variation across county-industries' employment. Based on this analysis, we use two sample restrictions in our main analysis. First, we restrict our sample to county-industries that average at least 96 workers over the years 2001 through 2008, which is roughly the the 25th percentile. Second, we restrict our sample to county-industries with at least 500 workers, which is roughly the point where the measurement error in our measured Hispanic workforce share levels off (as described in Appendix A).

3.3 Measuring Local Immigration Enforcement with Secure Communities

We use counties' participation in Secure Communities to measure the intensity of local immigration enforcement. Secure Communities was enacted as an information-sharing program between local law enforcement and ICE. Normally, when local law enforcement officials arrest a person and book them into a local jail, they share the arrestee's fingerprints with the Federal Bureau of Investigation (FBI) and the FBI checks its database for warrants and past violations. Under the Secure Communities program, the FBI automatically forwards the fingerprints to the Department of Homeland Security (DHS). The DHS compares the fingerprints with IDENT, a large database that contains fingerprints of all non-citizen individuals that the DHS or ICE have ever fingerprinted.[22] If IDENT returns a match, ICE uses additional information to assess the person's status and determine if the person can be deported for violating immigration law. If ICE determines that there is reason for deportation, the agency requests the local law enforcement agency to hold the person for up to 48 hours. Within that time, ICE tries to take the arrestee into custody and start removal proceedings.[23]

Participation in Secure Communities was mandatory for all counties. Because of logistical constraints, such as a lack of fingerprint scanners and insufficient ICE personnel to process IDENT matches, ICE rolled out Secure Communities gradually. Figure 3 illustrates the roll-out. The program started in Harris county (Texas) on October 27th, 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 counties were participating in the program.

Because we use the staggered roll-out of Secure Communities as our identifying variation, it is critical to understand why some counties entered the program before others. Figure 3 makes clear that Secure Communities started in counties at the Mexico-U.S. border and counties at the Gulf of Mexico. These counties

[22]DHS and ICE collect fingerprints from non-citizens in very different situations. Lawful immigrants must provide fingerprints to DHS when they enter the U.S. or when they apply for immigration benefits such as a visa or a green card. ICE takes finger prints when they arrest individuals for violating immigration law, start removal proceedings or deport an individual. IDENT thus includes individuals who have violated immigration law, immigrants with lawful status, and even naturalized citizens (Cox and Miles, 2013). However, it does not include undocumented immigrants who have never been in contact with ICE.

[23]Law enforcement agencies do not receive any funding from ICE or the DHS through Secure Communities. ICE only pays for maintaining the IDENT database and for database access (Capps et al., 2011).

also tend to have a higher Hispanic workforce share, as Figure 2 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 non-citizen 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.[24] 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.

Secure Communities had far-reaching consequences for immigrants in the U.S. It led to over 512,700 removals, including deportations and voluntary departures, between 2008 and 2016 (Transactional Records Access Clearinghouse, 2019). Removals are classified as Secure Communities-related if the Secure Communities fingerprint submission triggered the removal proceedings. Figure A2 shows that the number of Secure Communities-related removals increased quickly as Secure Communities was activated across the country. It reached a peak in 2012 when 97% of counties had joined the program.

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 U.S.

4 Worker Complaints, Workplace Hazards, and Workforce Demographics: Correlational Evidence

This section explores the relationship between worker complaints, workplace injuries and the demographics of the workforce. We first investigate the relationship between workers' propensity to complain and the actual workplace hazard across industries and counties. We then examine whether this relationship depends on the share of workers that are Hispanic, non-citizens, or Hispanic non-citizens.

We use data from the 48 contiguous U.S. states and the District of Columbia from 2001 through 2016. Our analysis focuses on agriculture (including forestry and fishing), construction, and manufacturing. These sectors have both the highest Hispanic and non-citizen workforce shares and a high rate of worker complaints

[24] 287(g) partnerships, begun 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 its 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 counties for a partnership.

Figure 3: Roll-out of Secure Communities
Red hatches indicate that county participates in Secure Communities

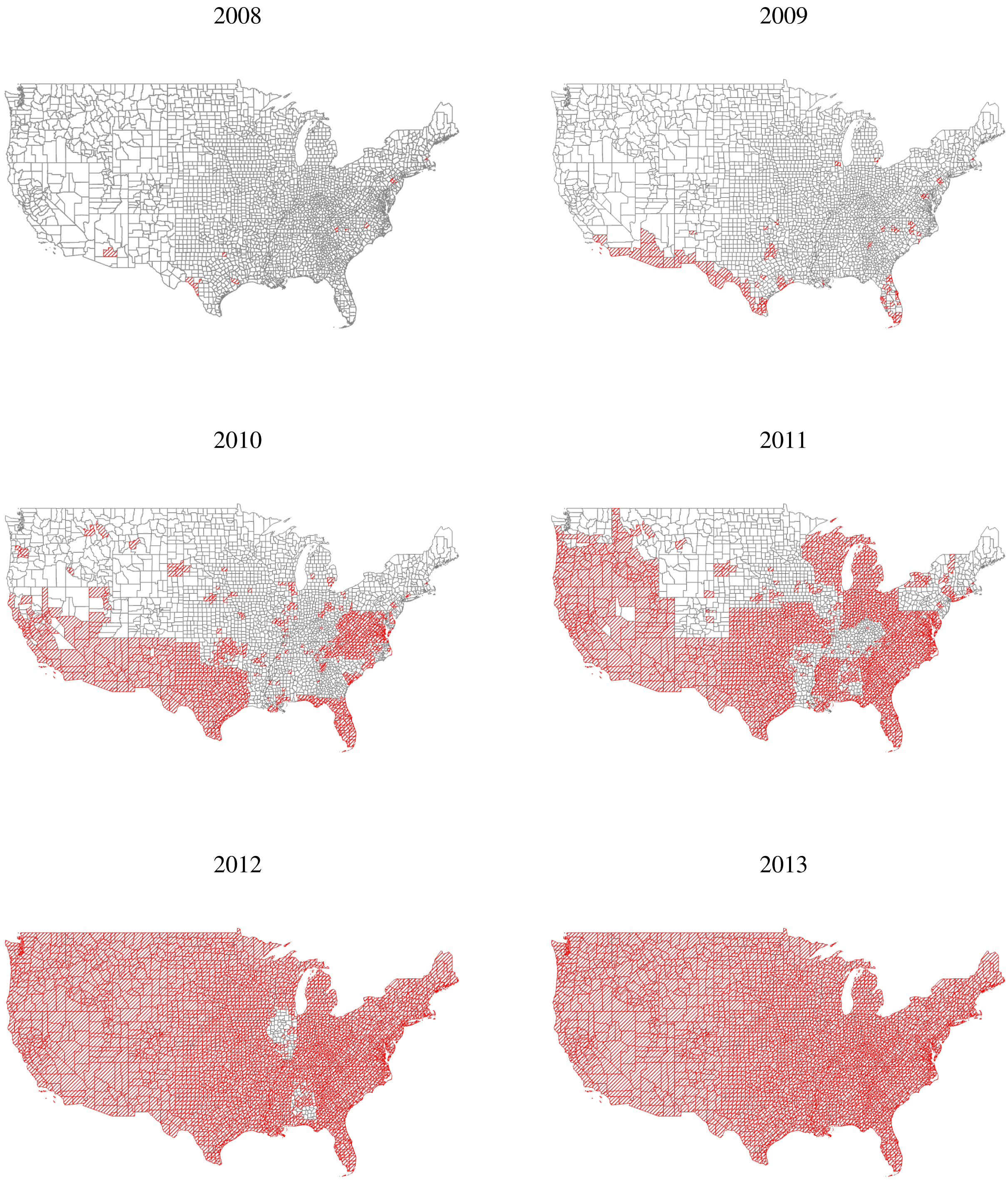
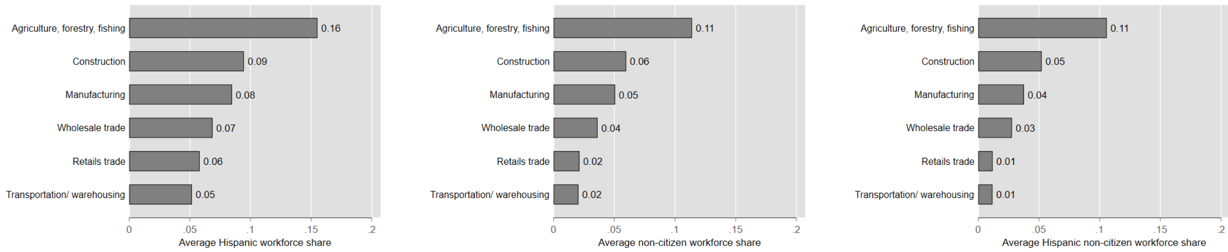


Figure 4: Workforce demographics by industry

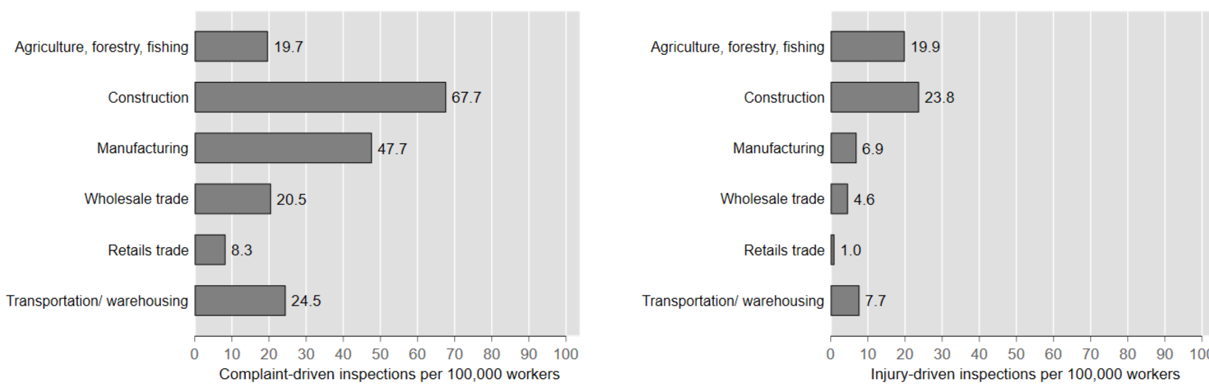
(a) Hispanic workforce (b) Non-citizen workforce (c) Hispanic non-citizen workforce



Average workforce demographics across county-industry-years. Plots produced with plottig package by Bischof (2017).

Figure 5: Worker complaint and workplace injuries rates by industry

(a) Worker complaints (b) Workplace injuries



Average inspection rates across county-industry-years. Plots produced with plottig package by Bischof (2017).

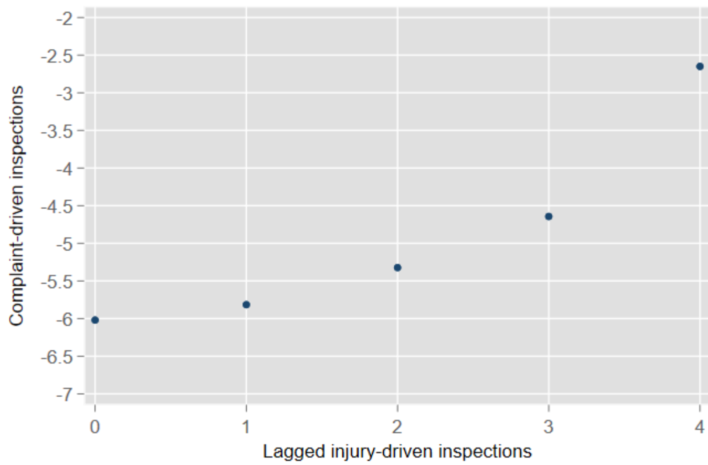
and injuries, as Figure 4 and Figure 5 illustrate. To reduce the impact of measurement error in workforce demographics, we restrict our analysis to county-industries that have an average yearly employment of at least 96 between 2001 through 2008 (see Section 3.2 and Appendix A for details).

4.1 Are Workers More Likely to Complain When They Face Higher Workplace Hazards?

To study the relationship between workers' willingness to complain and the workplace hazards they face, we calculate the correlation between the number of complaint-driven inspections and the lagged number of injury-driven inspections. We control for any factors that may confound the correlation, including log county-industry employment, the log county population, whether the county lies in a metropolitan statistical area, the county poverty rate and year and industry fixed effects.

Figure 6 shows shows a binned scatter plot of a county-industry's complaint-driven inspections against

Figure 6: Relationship between complaint-driven inspections and injury-driven inspections



Sample includes agriculture, construction and manufacturing. Lagged inspections are inspections from the previous year. Inspections are winsorized at the 99th percentile. All correlations control for year fixed effects, industry fixed effects, the log county employment, the number of programmed inspections, metro status of the county, the log county population and the county's poverty rate. Numbers on the y-axis are negative because we show the residualized correlation after controlling for fixed effects and covariates. Plot produced with binsreg package by Cattaneo et al. (2019) and plottig package by Bischof (2017).

its injury-driven inspections in the previous year. The relationship is clearly positive.[25] Since we control for employment levels, this relationship is not simply driven by the size of industries. Ceteris paribus, workers are more likely to complain about workplace conditions if workplace hazards are high. The positive relationship also exists when we consider a larger set of industries in Figure A3. These findings suggest that worker complaints generally help direct inspections to workplaces where injury risks tend to be higher.

4.2 Workforce Demographics, Worker Complaints, and Injury Rates

Are *all* workers more likely to complain when they face higher job hazards? As discussed above in Section 2, if some workers face especially high barriers to complain, then these workers might be less likely to complain for any level of job hazard. Moreover, since employers' inputs into safety might be a function of their workers' willingness to complain, these workers might face higher job hazards *because* of their barriers to complain.

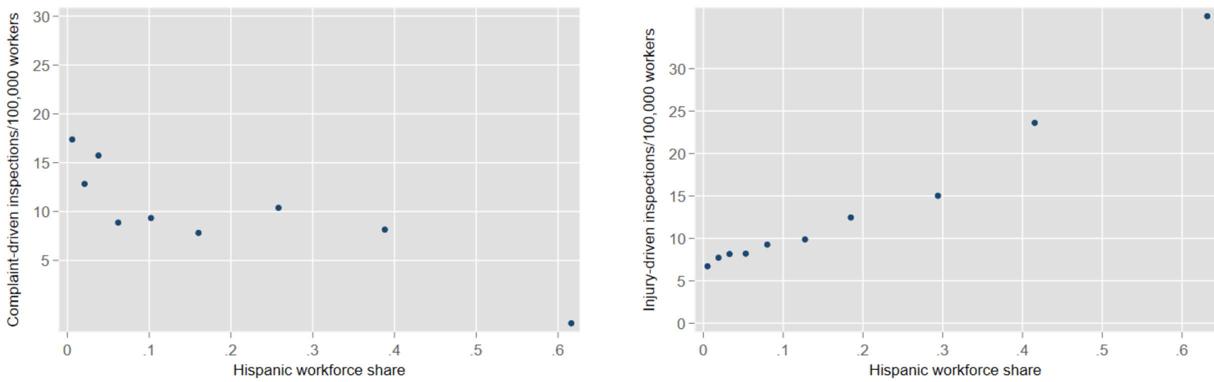
As explained in Section 3.2, we first consider a county-industry's workforce share that is Hispanic as a measure of such barriers. We then use the share of the foreign-born non-citizen workforce and the share of the Hispanic foreign-born non-citizen workforce as alternative measures.

We calculate the correlation between complaint-driven inspections per 100,000 workers and the Hispanic workforce share. As before, we control for confounding factors, including the number of programmed

[25] Numbers on the y-axis can be negative because we show the residualized correlation, i.e. the correlation between injury- and complaint-driven inspections after controlling for fixed effects and covariates.

Figure 7: Correlation between worker complaints, workplace injuries and Hispanic workforce share

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



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

inspections per 100,000 workers, whether a county is a metropolitan statistical area, the log county population, the county's poverty rate and industry and year fixed effects. Since the outcome variable is a rate with employment in the denominator, we weight observations by employment (Solon et al., 2015).

Figure 7a shows a binned scatter plot relating a county-industry's complaint-driven inspection rate with its Hispanic workforce share, after residualizing on the controls described above. The figure reveals a clear negative correlation. The negative relationship is stronger when we consider the non-citizen workforce share or the Hispanic non-citizen workforce share, as Figure A4a and Figure A5a illustrate. Worker complaints are less frequent when a higher share of the workforce are Hispanic workers, non-citizens, or Hispanic non-citizens.

Does this mean that Hispanic workers face lower job hazards? Figure 7b reveals that the opposite is true. There is a clear *positive* correlation between injury-driven inspections and the Hispanic workforce share. Results are similar when we consider the non-citizen workforce share or the Hispanic non-citizen workforce share in Figure A4b and Figure A5b. Workplaces that have a higher share of Hispanic and immigrant workers have a *higher* injury-driven inspection rate, indicating that they are more hazardous.

These results indicate that workplaces with more Hispanic and/or immigrant workers are more hazardous, but workers at these workplaces are less likely to complain about safety issues to the government. An immediate question, though, is whether this relationship reflects the unique barriers to raising complaints to the government outlined in Section 2, or rather reflects other differences between Hispanic and non-Hispanic workers that have nothing to do with barriers to complaining. Figure A6 illustrates that this relationship between complaint rates and injury rates does not exhibit the same pattern based on the share of the workforce that is African American. County-industries with a higher share of African American/Black workers have slightly lower complaint rates, but also slightly lower injury rates than workplaces with a low share of African American/Black workers (conditional on factors such as the local poverty rate).

Taken together, our analysis shows that Hispanic, non-citizen immigrant and Hispanic non-citizen immigrant workers tend to work in more hazardous working conditions, but are less likely to complain about them. These findings suggest that it is problematic if oversight agencies rely on worker complaints to detect unsafe working conditions in settings where the share of Hispanic, immigrant and likely undocumented immigrant workers is high.

However, it is difficult to draw many conclusions from this correlational analysis alone. Does the lower rate of complaints among workplaces with a higher share of Hispanic workers truly reflect a barrier to complaining, such as a reluctance to interact with government for fear of deportation? Does the reduced willingness to complain among Hispanic workers have a causal effect on employers' behavior, particularly the safety inputs employers provide in the workplace and the hazards workers face? To shed light on these questions, we next assess how immigration enforcement affects the dynamics discussed in this section.

5 Empirical Strategy to Estimate the Causal Effect of Immigration Enforcement

To estimate the effect of local immigration enforcement on worker complaints, we compare counts of complaint-driven inspections across county-industries with high and low Hispanic workforce shares and across counties that participated and did not participate in Secure Communities in a given year. We estimate the following equation for county c, 2-digit NAICS industry i and year t:

$$\begin{aligned} asinh(inspect_{cit}) = {} & \beta_1 SC_{ct} + \beta_2 SC_{ct} \times exposure_{ci} \\ & + \gamma_1 asinh(employment_{cit}) + \gamma_2 asinh(programmed\_inspect_{cit}) \\ & + exposure_{ci} \times \delta_t + \zeta_{ci} + \eta_{it} + \theta_{rt} + \epsilon_{cit} \end{aligned} \tag{1}$$

Our dependent variable is the inverse hyperbolic sine of the number of either complaint-driven inspections or injury-driven inspections. We take the inverse hyperbolic sine to reduce the influence of extreme outliers while preserving observations with zero inspections. We regress this dependent variable on the fraction of the year during which the county participated in Secure Communities, SC_{ct}, and the interaction of this fraction with a county-industry level exposure measure, $SC_{ct} \times exposure_{ci}$. The exposure measure is either the county-industry's Hispanic workforce share (ranging from 0 to 1) or an indicator for the Hispanic workforce share being above the 80^{th} percentile of the distribution. To adjust for overall changes in employment levels, we control for the inverse hyperbolic sine of county-industry employment,

$asinh(employment_{cit})$. We control for the overall level of OSHA's inspection activity by including the inverse hyperbolic sine of programmed inspection counts, $asinh(programmed\_inspect_{cit})$. As discussed in Section 3.1, programmed inspections are regularly scheduled OSHA inspections that are neither driven by worker complaints nor by injuries.[26]

Our regression includes four types of fixed effects. $exposure_{ci} \times \delta_t$ is an interaction between the exposure measure and year fixed effects; this allows us to control for unobservables that differentially affect workplaces with a higher or lower share of Hispanic (or non-citizen, or Hispanic non-citizen) workers over time. ζ_{ci} is a county-industry fixed effect, η_{it} is an industry-year fixed effect, and θ_{rt} is a census region-year fixed effect. We cluster standard errors at the county, since this is the unit of our identifying variation (Bertrand et al., 2004).

Our coefficient of interest is β_2: this coefficient captures the differential impact of Secure Communities on complaints or injuries in workplaces that have a high exposure to immigration enforcement.

We restrict our main sample to county-industries in agriculture, construction, and manufacturing in the 48 contiguous U.S. states and the District of Columbia between the years 2003 through 2016. Since Secure Communities started in 2008, this yields at least five pre-Secure Communities years for every county. We use two main samples based on our analysis of the measurement error in the Hispanic workforce share (see Appendix A for details). The first sample includes all county-industries that average at least 96 workers over the years 2001 through 2008, which is roughly the the 25$^{\text{th}}$ percentile. The second sample includes all county-industries with at least 500 workers, which is roughly the point where the measurement error in our measured Hispanic workforce share levels off. We consider variations on this sample choice in robustness checks.

Our strategy relies on the identification assumption that, conditional on county-industry employment, programmed inspections, and our four sets of fixed effects, there are no unobserved factors that vary across counties, industries and years, that are correlated with the timing of the Secure Communities roll-out and affect complaint- or injury-driven inspections differently for county-industries with high versus low exposure levels. We take several steps to assess the validity of this identification assumption in Section 6.4.

6 Results

6.1 Effect of Local Immigration Enforcement on Worker Complaints

This section presents estimates of the effect of local immigration enforcement on worker complaints. We show results for our two samples of county-industries. As described in Section 3.2 and Appendix A, the first sample includes county-industries that had an average of at least 96 workers – the 25$^{\text{th}}$ percentile – between

[26] All of our results are essentially identical if we omit the control for programmed inspections.

Table 1: Effect of Secure Communities Participation on Worker Complaints

Dependent variable:	Inverse hyperbolic sine of complaint-driven inspections			
Sample	Mean employment > 96		Mean employment >500	
	(1)	(2)	(3)	(4)
SC	0.003	0.004	0.023	0.018
	(0.017)	(0.015)	(0.026)	(0.024)
SC×Hispanic share	-0.18**		-0.32**	
	(0.08)		(0.13)	
SC×high Hispanic share		-0.09***		-0.14***
		(0.03)		(0.04)
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓
Exposure measure × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
Census region × year FE	✓	✓	✓	✓
Mean # complaint-driven inspections	1.39	1.39	2.34	2.34
# Observations	90,664	90,664	52,206	52,206

* p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of complaint-driven inspections on an indicator for a county participating in Secure Communities, the interaction of this indicator with the exposure measure (Hispanic workforce share or indicator for high Hispanic workforce share), and the controls and fixed effects indicated in the bottom panel. "High Hispanic share" is equal to 1 if the county-industry's Hispanic share is above the 80th percentile. Columns 1 and 2 show results for regressions that use the sample of county-industries with an average employment of 96 before 2008. Columns 3 and 4 use the sample of county-industries with an average employment of 500 before 2008.

2001 and 2008. The second sample includes only county-industries with an average of at least 500 workers to reduce the influence of measurement error in the Hispanic workforce share.

Table 1 shows that Secure Communities significantly reduced complaints in county-industries with a high share of Hispanic workers. Since the outcome variable is an inverse hyperbolic sine, we can interpret the coefficients as percentage changes. In column 1, the point estimate of the main effect on SC is 0.003 (p=0.858) and statistically insignificant, implying that participation in Secure Communities had no effect on complaints in county-industries with zero Hispanic workers. On the other hand, the coefficient on *SC×Hispanic share* ($-0.18, p = 0.08$) implies that Secure Communities reduced complaints significantly more among county-industries with a large Hispanic workforce share. Combining the main and interaction terms implies that Secure Communities led to 17.7 percent fewer complaints in workplaces with 100 percent Hispanic workers ($0.003 + -0.180 = -0.177, p = 0.017$)).

The specification shown in column 1 assumes that the effect of immigration enforcement on complaints is linear in the Hispanic workforce share. This assumption could be problematic, since no county-industry has a 100 percent Hispanic workforce share. To address this concern, we use an indicator for a high Hispanic workforce share as an alternative exposure measure. The indicator is equal to one if the Hispanic workforce share is above the 80$^{\text{th}}$ percentile of the distribution. Column 2 of Table 1 shows that Secure Communities reduced worker complaints by 9 percent ($p < 0.01$) in county-industries with a Hispanic workforce share above the 80$^{\text{th}}$ percentile. To compare this estimate to the estimate reported in column 1, consider that among county-industries with a Hispanic workforce share above the 80$^{\text{th}}$ percentile, the average Hispanic workforce share is 34.5 percent. The estimate in column 1, then, implies that Secure Communities reduced complaints in this group by 6.2 percent ($-0.18 * 0.345 = -0.062$. This implied estimate is slightly smaller than the estimate that we directly obtain in column 2, but not qualitatively different, suggesting that the assumption of linearity is not problematic.

Columns 3 and 4 show results for the sample of county-industries with an average pre-Secure Communities employment of at least 500. The point estimates on *SC×Hispanic share* and on *SC×high Hispanic share* increase substantially and almost double in size. Column 3 implies that in workplaces with only Hispanic workers (with a 100% Hispanic share), Secure Communities decreased complaints by 30 percent ($0.023 - 0.325 = -0.301, p = .008$). The specification in column 4 implies that Secure Communities reduced complaints by 14 percent in workplaces with a Hispanic workforce share above the 80$^{\text{th}}$ percentile, ($p < .01$).

The increase in magnitude of the point estimates from columns 1 and 2 to columns 3 and 4 indicates that our estimates in columns 1 and 2 likely suffer from attenuation bias. Measurement error in the Hispanic workforce share in smaller county-industries biases our results in columns 1 and 2 towards zero. However, these differences could also reflect heterogeneous treatment effects of immigration enforcement. In Figure A7, we provide suggestive evidence that the higher magnitude in the more restrictive sample in columns 3 and 4 is, in fact, due to correcting for measurement error. In the figure, we show how our coefficient estimate on *SC×Hispanic share* changes when we estimate Equation (1) using progressively larger employment cutoffs. The magnitude of the negative coefficient becomes progressively larger from -0.18 for an employment cutoff of 96, to -0.4 at a cutoff of 700, after which the estimate levels off. Our analysis in Appendix A reveals that the measurement error in the Hispanic workforce share appears to level off for county-industries with roughly 500 to 700 employees. The point estimates on *SC×Hispanic share* becoming stable right around where we observe the measurement error in *Hispanic share* levelling off provides suggestive evidence that the estimates in columns 3 and 4 are closer to the "true" effect of Secure Communities on worker complaints than those in columns 1 and 2.

What do these effect sizes mean for an individual worker's propensity to complain? To answer this question, we make two simplifying, but plausible, assumptions. First, assume that Secure Communities has zero effects on complaints of all non-Hispanic workers. Second, assume that effects are homogeneous for all Hispanic workers, regardless of the share of their co-workers that are also Hispanic. Under these assumptions, the effect of Secure Communities on an individual Hispanic worker's probability of complaining equals the coefficient estimates from columns 1 and 3. Using the estimate from column 3, which we expect is closer to the "true" effect as just argued, Secure Communities reduced the probability of a Hispanic worker complaining to OSHA by 32 percent.

This reduction in complaints could be explained by two potential mechanisms. On the one hand, stronger immigration enforcement may have led to an improvement in workplace safety – for example by leading to less employment in risky jobs – and hence a reduction in worker complaints. On the other hand, stronger enforcement may have dissuaded workers from filing a complaint conditional on risk. As our framework in Section 2 outlined, this could lead forward-looking employers to increase hazards. We investigate this question empirically in the next section.

6.2 Effect of Local Immigration Enforcement on Workplace Injuries

Having shown that local immigration enforcement reduced the rates at which Hispanic workers complained to OSHA about hazardous workplace conditions, we now examine whether immigration enforcement affected the hazards that workers faced. We use the inverse hyperbolic sine of injury-driven inspections as our outcome variable and re-estimate Equation (1).

Table 2 shows the results for both samples and exposure measures. In column 1, the coefficient on the interaction term *SC*×*Hispanic share* implies that a workplace with 100 percent Hispanic workers experienced a 16 percent higher injury rate relative to a workplace with no Hispanic workers ($p < 0.01$). Combining the coefficient estimate for *SC* with that for the interaction term *SC*×*Hispanic share* implies that a county-industry with only Hispanic workers (a Hispanic workforce share of 100 percent) experienced an increase in injury rates of 11 percent due to participation in Secure Communities ($-0.05 + 0.16 = 0.11, p = 0.07$). As with worker complaints, the magnitude of the estimated effect size increases if we use the sample of larger county-industries in column 3. We find a differential increase in injury rates of 30 percent in workplaces where all workers are Hispanic ($p < 0.01$). Figure A8 shows how the coefficient on the interaction term *SC*×*Hispanic share* changes when we restrict the sample to progressively higher employment cutoffs. The coefficient estimate increases from 0.16 for county-industries with on average at least 96 workers to around 0.33 for any sample including county-industries with on average at least 600 workers or more.

When we use the indicator of a high Hispanic workforce share as an exposure measure in columns 2 and 4, the implied effects on injuries are very similar. In the full sample, in column 2 we estimate that Secure Communities led to 4 percent more injuries among workplaces with a Hispanic share above the 80$^{\text{th}}$ percentile relative to workplaces with fewer Hispanic workers ($p = 0.09$); in the more restricted sample in column 4 that accounts for measurement error, we estimate those above the 80$^{\text{th}}$ percentile went on to experience 7 percent more injuries relative to those below ($p = 0.049$).

We note that, in all specifications, the coefficient estimate on the main effect of SC in Table 2 implies a negative and statistically significant effect of Secure Communities on workplace injuries in county-industries with zero Hispanic workers. This is surprising at first, but not entirely implausible. Prior research has found

Table 2: Effect of Secure Communities Participation on Workplace Injuries

Dependent variable:	Inverse hyperbolic sine of injury-driven inspections			
Sample	Mean employment > 96		Mean employment >500	
	(1)	(2)	(3)	(4)
SC	-0.05***	-0.05***	-0.08***	-0.07***
	(0.01)	(0.01)	(0.02)	(0.01)
SC×Hispanic share	0.16***		0.30***	
	(0.06)		(0.10)	
SC×high Hispanic share		0.04*		0.07**
		(0.02)		(0.03)
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓
Exposure measure × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
Census region × year FE	✓	✓	✓	✓
Mean # injury-driven inspections	0.40	0.40	0.66	0.66
# Observations	90,664	90,664	52,206	52,206

* p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of injury-driven inspections on an indicator for a county participating in Secure Communities, the interaction of this indicator with the exposure measure (Hispanic workforce share or indicator for high Hispanic workforce share), and the controls and fixed effects indicated in the bottom panel. "High Hispanic share" is equal to 1 if the county-industry's Hispanic share is above the 80th percentile. Columns 1 and 2 show results for regressions that use the sample of county-industries with an average employment of 96 before 2008. Columns 3 and 4 use the sample of county-industries with an average employment of 500 before 2008.

that Secure Communities led to decreases in employment rates, not only among likely undocumented immigrants but also among native and citizen workers (East and Velasquez, 2018). Even though we condition on employment in all of our regressions, this reduction in employment could lead to fewer injuries at workplaces without Hispanic workers if the decrease in employment was concentrated in riskier jobs, We explore this hypothesis in Table A2, where we estimate Equation (1) with the inverse hyperbolic sine of employment as the dependent variable (and omitting this variable as a control). In column 1, we find that Secure Communities reduced employment by 3 percent ($p < .01$) for county-industries without Hispanic workers. The coefficient on the interaction term *SC×Hispanic share* is very close to zero and statistically insignificant, implying that the effect on employment was orthogonal to county-industries' Hispanic workforce share. In other words, Secure Communities led to a reduction in employment even for non-Hispanic workers, consistent with the findings by East and Velasquez (2018). Columns 2 through 4 show that this relationship holds when we use the indicator for a high Hispanic workforce share and a higher employment cutoff. Thus, these results suggest that Secure Communities might have led to a small decrease in overall injuries by leading to lower employment. However, this overall decline was countered by a much larger increase in injuries among workplaces with more Hispanic workers.

Overall, the results in this section provide strong evidence that local immigration enforcement led to higher job hazards at workplaces with a large share of Hispanic workers. Combined with the estimates reported in Section 6.1, our findings suggest that employers responded to stronger local immigration enforcement, and the reduction in workers' willingness to complain that it induced, by reducing inputs into safety and increasing job hazards.

6.3 Event Study Estimates

Our triple-difference regression design in Equation (1) comes with two limitations. First, our estimates might be confounded by "pre-trends," *i.e.* if complaints and injuries are on differential trends in the years preceding a county's participation in Secure Communities. Second, we cannot speak to any potential dynamics, *i.e.* if the effects show up immediately and decay over time, or if they only show up after a few years.

To address these concerns, we conduct an event study analysis to complement our triple-difference estimates. We estimate the following specification:

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

In Equation (2) , $I_{c,t=k}$ is an indicator for each year k relative to the the activation of Secure Communities in county c. We treat the year right before activation as the comparison year and drop its indicator from the equation. In our main event study specification, $exposure_{ci}$ is the county-industry's Hispanic workforce share. We also show an alternative specification where $exposure_{ci}$ is an indicator for the county-industry's Hispanic workforce share being above the 80$^{\text{th}}$ percentile. As before, we control for employment levels, overall inspection activity, the interaction between the exposure measure and year fixed effects, county-industry fixed effects, industry-year fixed effects and census region year fixed effects. To avoid spurious estimates from very long leads or lags, we bin all years six years or earlier before and five years or later after Secure Communities activation into one period. The coefficients of interest are the β_2^ks, which estimate the differential rates of complaints or injuries in county-industries with a only Hispanic workers relative to county-industries without any Hispanic workers in each year relative to Secure Communities activation.

Figure 8a shows the estimates of β_2^k for complaint-driven inspections. Two patterns emerge. First, county-industries with 100 percent Hispanic workers do not exhibit statistically significantly different rates of worker complaints in the years leading up to Secure Communities activation than those with fewer Hispanic workers; if anything, the trend is slightly upward. Second, relative to county-industries with no Hispanic workers, worker complaints in county-industries with 100 percent Hispanic workers decreased by 12 percent the year of Secure Communities activation and decreased even further in the following years. Figure A9a shows the results of the event study when we use the indicator of a a high Hispanic workforce share as the exposure measure. This paints an even clearer picture. Worker complaints are almost identical between workplaces with high and low Hispanic workforce shares before Secure Communities activation, and complaints decline continuously as soon as Secure Communities begins in county-industries with high Hispanic workforce shares.

Figure 8b shows the results for workplace injuries. In the years before Secure Communities activation, injury rates were almost identical in workplaces with 100 percent Hispanic workers compared to workplaces without any Hispanic workers. Starting the year after Secure Communities activation, injuries increased by 16 percent in workplaces with 100 percent Hispanic workers and remain at around 20 percent until 5 years after the activation. Using the high Hispanic workforce share indicator as an exposure measure in Figure A9a presents a similar pattern, though the increase in injuries in the years following Secure Communities activation is slightly more muted.

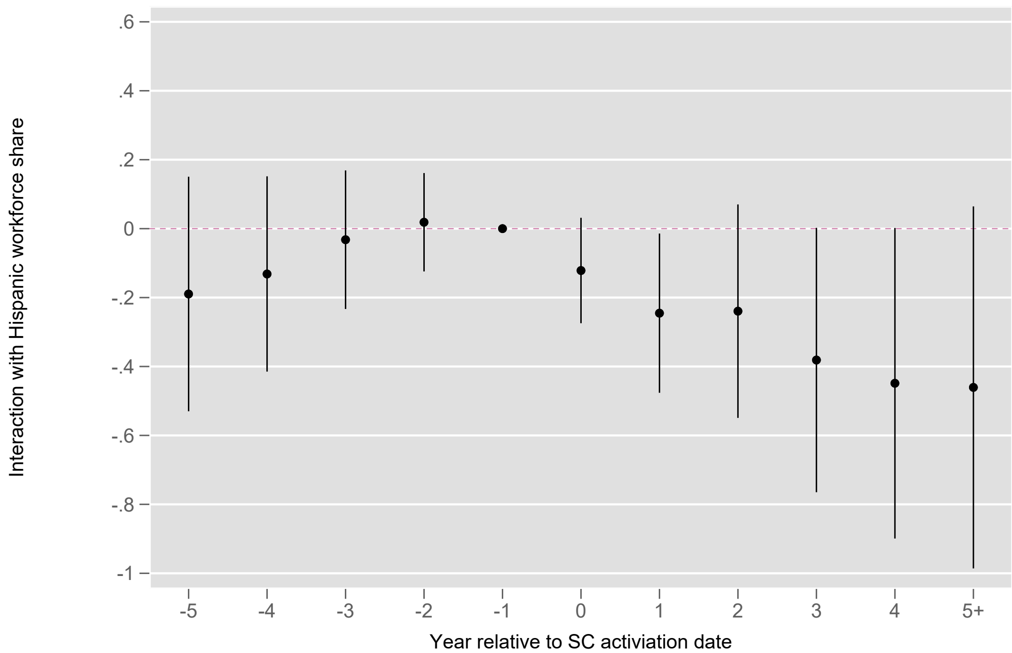
Overall, the event study results corroborate our previous results. Local immigration enforcement discouraged workers from complaining in county-industries with a high share of Hispanic workers. However, injury rates in such workplaces increased. This suggests that employers responded to lower complaint rates and less resulting regulatory enforcement by reducing their efforts to maintain safe workplaces.

6.4 Robustness Checks

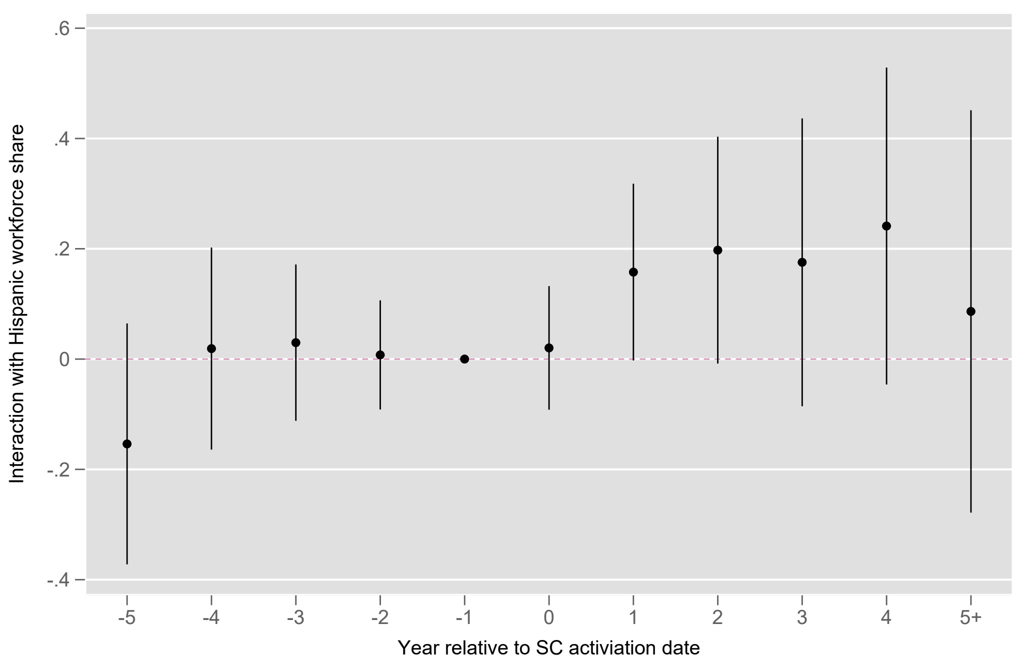
In this section, we check the robustness of our results along several dimensions. First, we address the measurement error present in our exposure variables using an empirical Bayes shrinkage estimator. Second,

Figure 8: Dynamic effects of Secure Communities on Complaints and Injuries

(a) Effect on Complaints



(b) Effect on Injuries



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

we consider alternative measures to capture county-industries' exposure to immigration enforcement, and we conduct a placebo check on a group of workers that should not be affected by immigration enforcement. Third, we address counties' selection into early activation of Secure Communities. Fourth, we show that our results are not driven by small subsets of counties or by particular sample choices. Fifth, we check the robustness of our estimation to the inclusion of additional controls, the potentially confounding impact of the Great Recession, and other specification choices. We discuss these tests in turn.

6.4.1 Addressing Measurement Error in Workforce Demographics with Empirical Bayes

As discussed in Section 3.2, the Hispanic workforce share in our analysis is measured with error. This is because we use a statistic drawn from a one-percent nationwide representative sample – the percent of respondents to the ACS who report working in a particular industry that are Hispanic – to infer a statistic relevant to the population – the percent of workers in a particular county-industry that are Hispanic. In our main analysis, we addressed this issue by restricting our sample to county-industries with relatively high employment.

In this section, we use an empirical Bayes shrinkage estimator as an alternative approach to address this measurement error. This approach is similar in spirit to Bayesian learning. The parameter that we want to measure is $Hispanic\, share_{pit}$, the percent of workers working in PUMA p and industry i in year t that are Hispanic or Latino. (We use PUMA instead of county since it maps onto the geographic level available in the ACS.) The empirical Bayes estimate of this parameter is a weighted average of two components. The first components is a data-driven prior based on features of the nationwide Hispanic workforce share distribution. The second component is $\widehat{Hispanic\, share}_{pit}$, the percent of ACS respondents working in PUMA p and industry i in year t that self-reported as Hispanic. The weights depend on the relative precision with which each component is estimated.

We use our data to select the first component, the prior.[27] The simplest prior to use is $\widehat{Hispanic\, share}_{t}$: the nationwide average Hispanic workforce share each year. However, this ignores the wide differences in the Hispanic workforce shares across industries and geography. We incorporate this variation to create a more informative prior. To form our prior for any given county-industry, we regress the Hispanic workforce share observed in the ACS ($\widehat{Hispanic\, share}_{pit}$) on an indicator for the county being located in a state that borders Mexico, the distance of the centroid of the county's state to the Mexican border, and industry fixed effects. Our prior for each county-industry's Hispanic workforce share is the fitted value from this regression, and its standard error is the standard error of this fitted value. This prior is a very biased estimate of our parameter of interest, $Hispanic\, share_{pit}$. However, it is also a very precise estimate since we use all the data we have.

The second component of the empirical Bayes estimate is the observed rate from the ACS, $\widehat{Hispanic\, share}_{pit}$.

[27]This approach is different from standard Bayesian inference, where the researcher decides the prior before any data are observed.

More concretely, we obtain this component by regressing a county-industry's annual observed Hispanic workforce share from the ACS on PUMA-industry fixed effects, using the years 2005–2007, and obtaining the fitted value from this regression. This is an unbiased measure of our parameter of interest; however, it is likely imprecisely estimated, particularly for county-industry-years with few ACS respondents. Our standard error of this component is the standard error of the fitted value.

The weights used to calculate the empirical Bayes estimate depend on the relative standard errors of the first and second component. Put another way, the empirical Bayes estimate "shrinks" the observed Hispanic workforce share ($\widehat{Hispanic\,share}_{pit}$) towards the prior; the degree of shrinkage is inversely related to the precision with which the observed share is measured. Because the observed shares are measured more precisely in larger county-industries (i.e. those with more ACS respondents), the degree of shrinkage is roughly inversely related to a county-industry's total employment. We describe this approach formally in in Appendix B.

Applying this process, we obtain an adjusted measure of each county-industry's annual Hispanic workforce share. In Table A3, we present estimates of the effects of Secure Communities on complaints (Panel A) and injuries (Panel B), using this adjusted measure. Panel A, column 1 indicates that Secure Communities led to a differential increase in complaint rates of 37 percent ($p < 0.01$). This coefficient is twice as large as the corresponding coefficient in column 1 of Table 1, which used the unadjusted Hispanic workforce share from the ACS. The increase in the coefficient is consistent with the hypothesis that the empirical Bayes estimator reduces measurement error and thus attenuation bias.

Panel B of Table A3 shows results for the effect of Secure Communities on workplace injuries. The estimates have the same magnitude, but larger standard errors than those obtained using the unadjusted Hispanic workforce share measure.

Overall, these findings suggests that our main results underestimate the effect of Secure Communities on complaints at workplaces with a high share of Hispanic workers, and that formally accounting for measurement error in our key explanatory variable does not change the qualitative conclusions from our results.

6.4.2 Considering Alternative Exposure Measures

In our main specification, we use the share of a county-industry's workforce that is Hispanic to measure a county-industry's exposure to immigration enforcement. We now re-estimate Equation (1), but use the share of non-citizens or the share of Hispanic non-citizen in a county-industry's workforce to capture exposure to Secure Communities. Because of the restriction to non-citizens, both groups likely include a higher proportion of undocumented immigrants than Hispanic workers overall. It is therefore plausible that the effect of immigration enforcement on worker complaints and injuries is larger than in our baseline specification.

Panel A of Table 3 shows the baseline results (column 1) together with the results for the non-citizen

Table 3: Results are robust to alternative exposure measures

Exposure measure:	(1) Hispanic workforce share	(2) Non-citizen workforce share	(3) Hispanic non-citizen workforce share
Panel A: Complaint-driven inspections			
SC	0.003	0.005	0.006
	(0.017)	(0.016)	(0.016)
SC×Hispanic share	-0.18**		
	(0.08)		
SC×non-citizen share		-0.23*	
		(0.13)	
SC×Hispanic non-citizen share			-0.30**
			(0.13)
Panel B: Injury-driven inspections			
SC	-0.05***	-0.04***	-0.05***
	(0.01)	(0.01)	(0.01)
SC×Hispanic share	0.16***		
	(0.06)		
SC×non-citizen share		0.18*	
		(0.10)	
SC×Hispanic non-citizen share			0.20*
			(0.10)
Asinh(programmed inspections)	✓	✓	✓
Asinh(employment)	✓	✓	✓
Exposure measure × year	✓	✓	✓
County × industry FE	✓	✓	✓
Industry × year FE	✓	✓	✓
Census region × year FE	✓	✓	✓
# Observations	90,664	90,664	90,664

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

(column 2) and the Hispanic non-citizen exposure measure (column 3). Irrespective of the exposure measure we use, we find a significant negative effect of Secure Communities on worker complaints in high-exposure county-industries. Compared to the baseline results, the effect is 5 percentage points larger when we use the non-citizen workforce share and 12 percentage points larger when we use the Hispanic non-citizen workforce share, but these estimates are not statistically significantly different from each other. Panel B of Table 3 shows that the results for workplace injuries are also robust to using the alternative exposure measures. The effect of Secure Communities on injuries is positive and statistically significant in all three specifications. The magnitude of the estimated effect again increases slightly when we use the non-citizen or non-citizen Hispanic workforce share.

We next run a "placebo check" where we use the Black/African American workforce share instead of the Hispanic workforce share as our exposure measure. Less than 4 percent of non-Hispanic Blacks/African Americans are foreign-born non-citizens, compared to 29 percent of Hispanics/Latinos (U.S. Census Bureau, 2005). As described in Section 2, Hispanic individuals are also more likely to live with an undocumented immigrant (Hall et al., 2019; Maslin and Sonenshein, 2017). Immigration enforcement should thus not affect complaints filed or injuries experienced by Black or African American workers. Table A4 confirms this intuition. Secure Communities did not affect worker complaints or workplace injuries in county-industries with a higher Black/African American workforce share.

6.4.3 Addressing Counties' Selection into Secure Communities

We next address counties' selection into early adoption of Secure Communities. Figure 3 indicates that the roll-out of 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 4 shows. We test if this selection into early adoption confounds our estimates by re-estimating our regression equation on two sub-samples. First, we drop all counties at the Mexico-U.S. border or the Gulf of Mexico. Second, we drop any county that adopted Secure Communities before 2010. Columns 2 and 3 of Table 4 show that our results are robust to either of these restriction; If anything, the results are slightly larger than those from our baseline specification in Column 1(with the exception of the estimate on injuries that drops border of Gulf counties in Column 2, which drops slightly in magnitude and significance.

6.4.4 Ensuring that Sample Choices Do Not Drive Estimates

Our results may also be driven by a few county-industries with a large Hispanic workforce share. We conduct two robustness checks to address this concern. First, for each industry, we drop the ten counties with the highest Hispanic workforce share. Second, we drop any county that lies in or contains one of the ten PUMAs with the largest Hispanic population in 2005, 2006 or 2007. Columns 4 and 5 of Table 4 show that our results are robust to these sample changes.

Recall that our analysis so far is restricted to the agriculture, construction and manufacturing sectors. In column 6 of Table 4 we show that estimates are very similar when we include the wholesale trade, retail trade and transportation/warehousing sectors. We also restricted our dataset to county-industries that have at least 96 or 500 employees on average over all years. Column 7 of Table 4 shows that reducing this cutoff to 32 does not change our results.

Table 4: Results are robust to alternative samples

	(1) Baseline	(2) Drop border & Gulf counties	(3) Drop pre-2010 adopters	(4) Drop high Hispanic workforce share counties	(5) Drop high Hispanic population counties	(6) All industries	(7) Employment ≥ 32
Panel A: Complaint-driven inspections							
SC	0.003	0.006	0.007	0.011	0.006	0.015	-0.002
	(0.017)	(0.017)	(0.018)	(0.011)	(0.017)	(0.010)	(0.014)
SC×Hispanic share	-0.18**	-0.25**	-0.24**	-0.23***	-0.20**	-0.18***	-0.15**
	(0.08)	(0.10)	(0.10)	(0.07)	(0.08)	(0.06)	(0.07)
Panel B: Injury-driven inspections							
SC	-0.05***	-0.05***	-0.05***	-0.03***	-0.05***	-0.03***	-0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
SC×Hispanic share	0.16***	0.11	0.17**	0.08*	0.13**	0.08**	0.13**
	(0.06)	(0.08)	(0.08)	(0.05)	(0.06)	(0.04)	(0.05)
Asinh(programmed inspections)	✓	✓	✓	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓	✓	✓	✓
Hispanic share × year	✓	✓	✓	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓	✓	✓	✓
Census region × year FE	✓	✓	✓	✓	✓	✓	✓
# Observations	90,664	88,228	86,954	214,191	90,342	187,837	108,094

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

6.4.5 Assessing Sensitivity to Confounding Factors and Specification Choices

We next check if our results are robust when we control for potentially confounding factors. Column 2 of Table 5 shows results of regressions that include a vector of controls for time-varying demographic, economic, and political factors at the county level. We include the log county population, the county's Hispanic population share, the Black/African American workforce share, the county unemployment rate, the county poverty rate, the log median household income and the Republican vote share in the last presidential election. We also control for county participation in the 287(g) program since 287(g) participation is correlated with Secure Communities roll-out. Including these controls changes neither the coefficient estimates nor the standard errors.

Since our sample period spans the Great Recessions, a potential concern is that industry-specific demand factors that were correlated with Secure Communities roll-out affected worker complaints and injuries. We follow East and Velasquez (2018) and include a "Bartik"-style measure of labor supply in our regressions to address this concern (Bartik, 1992). The county Bartik measure is the population-weighted average of the PUMA Bartik measure. The PUMA Bartik measure is the 2005 share of the PUMA's industry employment in total PUMA employment, multiplied by the nationwide change in industry employment each year. The measure captures the effect of labor demand-driven, industry-specific nationwide changes that affected county-industries differently depending on the industry's share in county employment before the recession. Column 3 of Table 5 shows that including this measure does not have any effect on our results.

In columns 4 and 5 of Table 5 we consider two versions of more detailed fixed effects. In column 4, we show results from regressions that include state-year instead of Census region-year fixed effects. The estimates hardly change. In column 5, we show results from regressions that include a full set of county-

Table 5: The estimated effects of Secure Communities are robust to controlling for confounding factors

	(1) Baseline	(2) Controls	(3) Bartik	(4) State-year FE	(5) Full FE
Panel A: Complaint-driven inspections					
SC	0.003	0.003	0.004	-0.003	
	(0.017)	(0.017)	(0.017)	(0.024)	
SC×Hispanic share	-0.18**	-0.18**	-0.18**	-0.20**	-0.34*
	(0.08)	(0.08)	(0.08)	(0.09)	(0.17)
Panel B: Injury-driven inspections					
SC	-0.05***	-0.05***	-0.05***	-0.06***	
	(0.01)	(0.01)	(0.01)	(0.02)	
SC×Hispanic share	0.16***	0.16***	0.16***	0.14**	0.03
	(0.06)	(0.06)	(0.06)	(0.07)	(0.15)
Asinh(programmed inspections)	✓	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓	✓
Controls		✓	✓		
Bartik measure			✓		
Hispanic share × year	✓	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓	✓
Census region × year FE	✓	✓	✓		
State × year FE				✓	
County × year FE					✓
# Observations	90,664	90,634	90,564	90,664	85,470

* p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of complaint-driven inspections (Panel A) or injury-driven inspections (Panel B) on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share and the variables, and the controls and fixed effects indicated in the bottom panel.Controls in regressions 2 and 3 include the log county population, the Hispanic population share, the Black/African American workforce share, the county unemployment rate, the county poverty rate (in %), the log median household income, the Republican vote share in the last presidential election and an indicator that the county participated in the 287(g) program. The county Bartik measure is the population-weighted average of the Public Use Microdata Area (PUMA) Bartik measure. The PUMA Bartik measure is the 2005 share of the PUMA's industry employment in total PUMA employment, multiplied by the nationwide change in industry employment each year.

industry, county-year and industry-year fixed effects. Including all three interactive fixed effects is a very demanding test, since our identifying variation is now limited to differences in the Hispanic workforce share across industries in the same county. Nevertheless, the estimated effect of Secure Communities on worker complaints remains negative and significant. The effect on injuries remains negative, but it decreases and is not statistically distinguishable from zero.

For our final robustness check, recall that our dependent variable is the inverse hyperbolic sine of inspections. To check if our results are sensitive to this specification, we use the natural logarithm of the number of inspections plus one as our dependent variable. Table A5 shows that the results are robust to this alternative transformation.

7 Assessing the Mechanism through Which Immigration Enforcement Affects Safety

The previous section provided evidence that local immigration enforcement reduced complaints to OSHA among workplaces with high shares of Hispanic (and/or non-citizen) workers and increased injuries rates. These results are consistent with our framework in Section 2, which described why employers would adjust their inputs into safety based on workers' willingness to complain. However, other explanations are possible.

In this section, we first consider a plausible alternative explanation—that Secure Communities led to differential turnover in workplaces with many Hispanic workers—for which we find no evidence. We then present two pieces of evidence to support that the mechanisms we outline increased workplace injuries following Secure Communities. First, we show that Secure Communities led to worse compliance with safety and health regulations. Second, we demonstrate that Secure Communities had no effect on safety (or complaints) in unionized workplaces.

7.1 Considering Alternative Explanations

Secure Communities led to a dramatic increase in deportations of undocumented immigrants, the vast majority being Hispanic. This could have led to relatively high turnover at workplaces that had initially employed many Hispanic workers. Since inexperienced workers are more likely to get injured on the job (Breslin and Smith, 2006), this turnover could explain why Secure Communities led to higher workplace injury rates.

Results that we have already discussed suggest that this mechanism is unlikely to explain our results. As we described in Section 6.2, and showed in Table A2, Secure Communities did not lead to differential changes in employment levels in county-industries with more or fewer Hispanic workers.

However, even if Secure Communities did not lead to differential changes in employment *levels* at workplaces with initially high Hispanic workforce shares, it could have led to changes in the *composition* of the workforce, as establishments might have replaced Hispanic workers (undocumented or not) with workers of other ethnicities. To consider this possibility, we calculate the annual observed Hispanic (as well as non-citizen and Hispanic non-citizen) workforce share from the ACS for each county-industry each year 2005–2016, and we regress this annual workforce share on SC_{ct} (the share of the year that Secure Communities was activated in county c) as well as county-industry, industry-year, and region-year fixed effects. We report results in Table A6. There is no evidence that Secure Communities led to a change in workforce demographics; the coefficients in all three columns (estimating the effect of Secure Communities on the share of the workforce that is Hispanic, non-citizen, and Hispanic non-citizen, respectively) are all tiny in magnitude and statistically insignificant.

7.2 Immigration Enforcement Leads to Worse Compliance with Safety Regulations

Our framework indicates that immigration enforcement, by reducing Hispanic workers' willingness to complain about safety hazards, will reduce employers' inputs into safety. One salient example of employers' inputs into safety is their compliance with government safety and health regulations. Did Secure Communities reduce this input into safety?

To answer this question, we examine whether violations of OSHA regulations increased following the introduction of Secure Communities. To estimate the effect of Secure Communities on OSHA compliance, we estimate a slightly modified version of the regression model Equation (1):

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

Here, our dependent variable is the number of violations found at inspection of workplace j in county c, in industry i, conducted in year t.[28] *SC*, *exposure*, and the four sets of fixed effects are as described above. We control for a vector of workplace-specific controls in X, including the number of employees reported present during the inspection. Because this regression is at the inspection level, we do not control for county-industry employment as we did in Equation (1) (though doing so has no effect on our estimates).

We consider three categories of violations: the total number of violations detected, the number of violations categorized as "serious", and the number of violations categorized as either "repeat" or "willful". As described in Section 3.1, "serious" violations are those that OSHA deems most likely to lead to serious harm, and thus are presumably more highly correlated with the occurrence of injuries. "Repeat" violations are violations that OSHA has previously cited the employer for, and "willful" violations demonstrate employer negligence. Repeat and willful violations thus likely represent *employers'* inputs into safety, rather than worker behavior.

Because violations are only observed conditional on an inspection occurring, we restrict the estimation of Equation (3) to programmed inspections. As described in Section 3.1, the occurrence of programmed inspections is exogenous to events at a particular workplace, conditional on industry and location.

We report our estimates in Table 6. In Panel A, we use the share of the Hispanic workforce as our exposure measure. Focusing on the interaction term *SC*×*Hispanic share*, column 1 reveals that the participation

[28] Whereas we transformed our dependent variables using the inverse hyperbolic sine when the dependent variable was the number of annual complaint or injury inspections in a county-industry, here we keep our dependent variable in levels. The reason is that the number of violations detected in a regression is much more discrete, and more compressed toward zero, so this transformation appears unwarranted. All of the estimates that we report in this section are similar if we transform the dependent variable with the inverse hyperbolic sine, but the magnitudes are slightly smaller.

in Secure Communities led to 0.48 more violations among workplaces with 100 percent Hispanic workers, relative to workplaces without any Hispanic workers ($p = 0.029$). This is a 28 percent increase relative to the sample mean ($0.48/1.72$). Column 2 shows an even starker increase in serious violations in workplaces with Hispanic workers. The coefficient on *SC×Hispanic share* is 0.52 ($p < 0.01$), or 38 percent of the sample mean. Column 3 reveals that Secure Communities also led to a large increase in repeat or willful violations. The coefficient on *SC×Hispanic share* is 0.05 ($p = 0.059$), which represents a 62.5 percent increase relative to the sample mean.

We note that, across all specifications the main effect of Secure Communities is negative and statistically significant. For example, the estimate in column 1 implies that Secure Communities led to 0.17 fewer violations among workplaces without any Hispanic workers ($p < 0.01$), which is roughly one-third of the interaction term and 10 percent of the sample mean. While this result may seem puzzling at first, it mirrors our findings for the overall effect of Secure Communities on workplace injuries. As we described in Section 6.2, this reduction in injuries and violations at workplaces with few Hispanic workers likely reflects the negative effect of Secure Communities on employment, which we document in Table A2 and which corroborates evidence in East and Velasquez (2018).

In Panel B, we use the indicator for the Hispanic workforce share being above the 80$^{\text{th}}$ percentile to measure exposure to Secure Communities. [29] The estimates are very similar to those in Panel A.

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

7.3 Labor Unions Mitigate the Chilling Effect of Immigration Enforcement

In Section 2, we discussed how labor unions may mitigate the effect of immigration enforcement on worker complaints and, thus, employers' response. Because unions provide protections that make it safer and easier for individual workers to file complaints, we expect to see rates of complaints and injuries to be less affected by Secure Communities in unionized workplaces than in non-unionized workplaces. If we observe such heterogeneity based on union presence, it bolsters the evidence that the relationship between immigration enforcement and worker safety that we observe is in fact due to employers' response to workers' willingness to complain.

We separate the number of complaint-driven and injury-driven inspections in each county-industry-year by whether they originated at unionized or non-unionized workplaces. During our sample period, roughly

[29] Note that the 80$^{\text{th}}$ percentile in this sample of OSHA inspections is 0.31, which is higher than the 80$^{\text{th}}$ percentile in collapsed sample used in prior regressions, which is 0.17.

Table 6: The Effect of Secure Communities on Compliance with Safety and Health Regulations

Dependent variable: number of	Overall violations (1)	Serious violations (2)	Repeat or willful violations (3)
Panel A: Exposure measure: Hispanic workforce share			
SC	-0.16***	-0.15***	-0.01
	(0.05)	(0.05)	(0.01)
SC×Hispanic share	0.46**	0.51***	0.05*
	(0.22)	(0.19)	(0.03)
Panel B: Exposure measure: High Hispanic workforce share			
SC	-0.10**	-0.08**	-0.01
	(0.04)	(0.04)	(0.01)
SC×high Hispanic share	0.06	0.07	0.00
	(0.08)	(0.06)	(0.01)
Exposure measure × year	✓	✓	✓
County × industry FE	✓	✓	✓
Industry × year FE	✓	✓	✓
Census region × year FE	✓	✓	✓
Mean Dep Var	1.72	1.38	0.08
# Observations	539,254	539,254	539,254

* p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the number of the of violations indicated in the column title that were detected in an OSHA inspection on an indicator for a county participating in Secure Communities, the interaction of this indicator with the exposure measure (Hispanic workforce share or indicator for high Hispanic workforce share), and the controls and fixed effects indicated in the bottom panel. "High Hispanic share" is equal to 1 if the county-industry's Hispanic share is above the 80th percentile.

16 percent of both complaint-driven and injury-driven OSHA inspections occurred at unionized workplaces. If unions mitigate the chilling effect of immigration enforcement, the magnitude of the coefficient estimate on the interaction term *SC×Hispanic share* will be smaller at unionized workplaces than at non-unionized workplaces.[30]

In Table 7, we report our main specification, corresponding to columns 1 and 3 of Table 1 and Table 2, separately for non-unionized and unionized workplaces. As in our main results, we show estimates derived from our two samples including county-industries with an average employment above 96 (columns 1 and 2) and 500 (Columns 3 and 4), respectively. Column 1 of Panel A shows that Secure Communities substantially reduced complaints at non-unionized workplaces with a high Hispanic workforce share. The coefficient on *SC×Hispanic share* is -0.16 (p=0.05). Column 2 shows that the effect is substantially smaller for unionized workplaces. The coefficient on *SC×Hispanic share* is close to zero (-0.05) and statistically insignificant. However, the two coefficients across the two samples are not statistically significantly different from each other (p=0.19).[31] The difference in the impact of Secure Communities becomes even clearer when we restrict our sample to larger county-industries in columns 3 and 4. The estimated effect in non-unionized workplaces almost doubles while the effect in unionized workplaces remains small and statistically insignificant. The two coefficients are almost statistically different from each other (p=0.106).

Turning to injuries, we find that Secure Communities only increased injuries at non-unionized workplaces with high Hispanic workforce shares, but not at unionized workplaces. In Panel B of Table 7, the coefficient on *SC×Hispanic share* is positive (0.13) and statistically significant for non-unionized workplaces, but close to zero and insignificant for unionized workplaces. Restricting our sample to large county-industries magnifies the differences. The coefficient on the interaction term increases in magnitude to 0.25 for non-unionized workplaces ($p < .01$), but remains small (0.04) and statistically insignificant for unionized workplaces. The two estimates are statistically significantly different from each other ($p = .05$).

Labor unions provide resources, information, and protection to enable individual workers to file complaints anonymously to government agencies. As a result, we hypothesized in Section 2 that their presence would mitigate the extent to which immigration enforcement would reduce Hispanic workers' willingness to complain to OSHA and, thus, would lead employers to increase job hazards. The results in this section imply that, in the context of Secure Communities, unions did indeed play this role.

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

[31] We obtain this p-value from a fully interacted model that estimates the effect for unionized and non-unionized workplaces in a single regression and obtains identical estimates to the split sample models we report in the table. We report the split sample estimates in the table for ease of exposition.

Table 7: Heterogeneous Effect of Secure Communities on Complaints and Injuries, by Union Presence

Sample	Mean employment > 96		Mean employment >500	
	Non-unionized	Unionized	Non-Unionized	Unionized
	(1)	(2)	(3)	(4)
Panel A: Dependent variable: inverse hyperbolic sine of complaint-driven inspections				
SC	-0.001	0.008	0.013	0.022
	(0.016)	(0.009)	(0.025)	(0.015)
SC×Hispanic share	-0.16**	-0.05	-0.30**	-0.08
	(0.08)	(0.05)	(0.13)	(0.08)
Mean # complaint-driven inspections	1.17	0.22	1.97	0.38
p-value on union/non-union difference:	0.190		0.106	
Panel B: Dependent variable: inverse hyperbolic sine of injury-driven inspections				
SC	-0.05***	-0.01**	-0.07***	-0.02**
	(0.01)	(0.00)	(0.01)	(0.01)
SC×Hispanic share	0.13**	0.02	0.25***	0.04
	(0.06)	(0.03)	(0.09)	(0.05)
Mean # injury-driven inspections	0.34	0.06	0.55	0.11
p-value on union/non-union difference:	0.11		0.05	
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓
Exposure measure × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
Census region × year FE	✓	✓	✓	✓
# Observations	90,664	90,664	52,206	52,206

* p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of inspections in non-unionized (columns 1 and 3) and unionized (columns 2 and 4) workplaces, respectively, on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Hispanic workforce share, and the controls and fixed effects indicated in the bottom panel. Columns 1 and 2 show results for regressions that use the sample of county-industries with an average employment of 96 before 2008. Columns 3 and 4 use the sample of county-industries with an average employment of 500 before 2008.

8 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 problematic and illegal working conditions are, complaints serve as a form of decentralized targeting. Additionally, endowing workers with the right to complain incentivizes employers to preemptively improve their working conditions to avoid costly government scrutiny. At the same time, many workers face substantial barriers to complain, and these barriers are higher for some groups of workers than others. As a result, complaint-based enforcement might exacerbate existing inequalities in the labor market.

We presented correlational evidence that worker complaints on average direct occupational safety and health inspections to relatively dangerous workplaces. However, this relationship reverses when workers face high barriers to complain: although workers in workplaces with large shares of Hispanic, non-citizen or Hispanic non-citizen experience more injuries, they are less likely to complain. We then investigated the effects of an exogenous change in the barriers to complain using the onset of heightened immigration enforcement spurred by Secure Communities. We found that the introduction of Secure Communities led to substantially fewer complaints to OSHA among workplaces with a large share of Hispanic workers but led these workplaces to experience *more* injuries.

We provided several pieces of evidence that the deterioration in workplace safety caused by immigration enforcement is consistent with a reduced threat of worker complaints lowering employers' cost of maintaining a hazardous workplace. With this finding, we build on prior work that documents the various barriers that workers face in exercising their rights to complain, and we also reveal that these barriers lead to meaningful disparities in rates of fatal and serious non-fatal workplace injuries.

Though we focused our analysis of the implications of complaint-based regulatory enforcement in the context of Hispanic workers and immigration enforcement, it is plausible that the implications of our work extend to both other regulatory contexts (e.g. minimum wage and overtime violations) and to other sources of barriers to complain (e.g. workers' union status or the health of the local labor market). We leave such directions for future work.

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Appendix

A Assessing the Degree of Measurement Error in Hispanic Workforce Shares

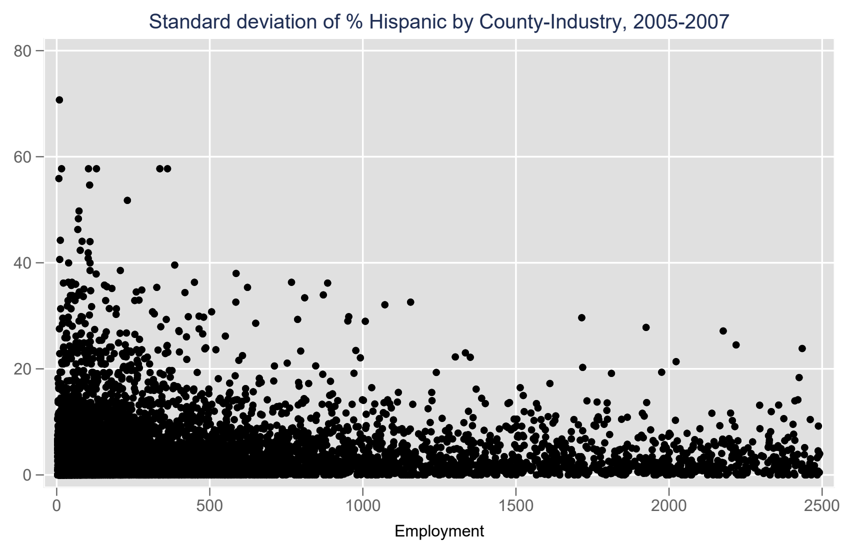
As described in Section 3.2, one concern with our approach to measuring the Hispanic workforce share of county-industries is that we are using a sample statistic – the share of workers in an industry that are Hispanic in the 2005–2007 ACS, which is a 1 percent sample of the U.S. population – to infer a population statistic. Here, we investigate the extent to which this approach results in measurement error and how the magnitude of the measurement error relates to county-industry employment.

We assume that that the *Hispanic share* variable that we calculate from the ACS is a better proxy for the true Hispanic share in the population when there is less variation in the ACS-derived *Hispanic share* across years. Large swings in the *Hispanic share* across consecutive years suggests a lack of reliability in our sample statistic. If this measurement error decreases in the size of a county-industry, the temporal correlation of the *Hispanic share* in the ACS should be higher for larger county-industries. Put differently, the *Hispanic share* in the ACS should vary less across years in larger county-industries.

Figure A1 displays a scatter plot relating the standard deviation of the *Hispanic share* between 2005, 2006, and 2007 to county-industry employment in 2006. The figure illustrates that this statistic varies substantially in very small county-industries. This variability declines sharply with size, levelling out at around 500 employees.

This analysis implies that including very small county-industries will introduce classical measurement error in regressions in which the *Hispanic share* is an independent variable. Therefore, we apply minimum employment restrictions to create two samples for our main analysis. Our first sample only includes county-industries that average at least 96 workers over the year 2001 through 2008, which is roughly the the 25th percentile. Second, we use a sample of county-industries with on average of at least 500 workers, the level at which the measurement error in *Hispanic Share* flattens out.

Figure A1: Assessing Measurement Error in *Hispanic Share* by County-Industry Employment



Each observation is a county-industry. The variable on the y-axis is the standard deviation in the calculated *Hispanic Share* across the years 2005–2007. The x-axis is the county-industry's total employment in 2006.

B Using Empirical Bayes Shrinkage Estimator to Fix Measurement Error in Hispanic Workforce Shares

C Additional Figures

Figure A2: Removals under Secure Communities

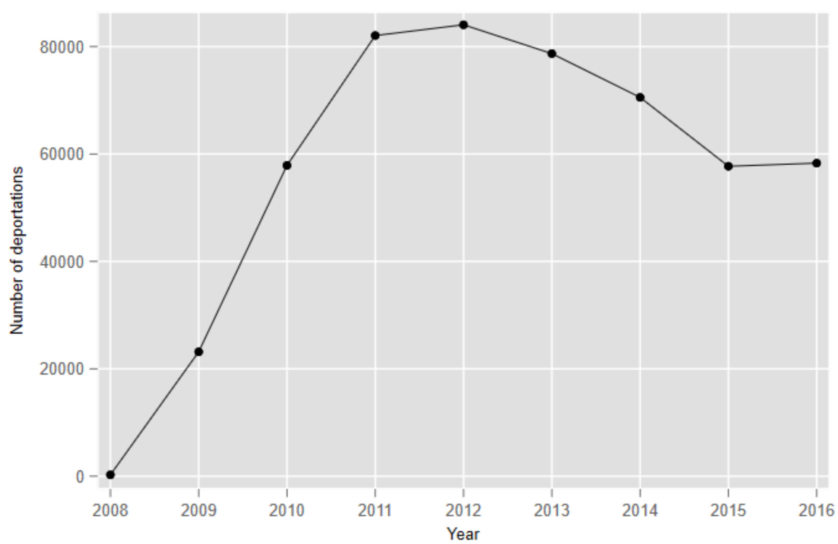
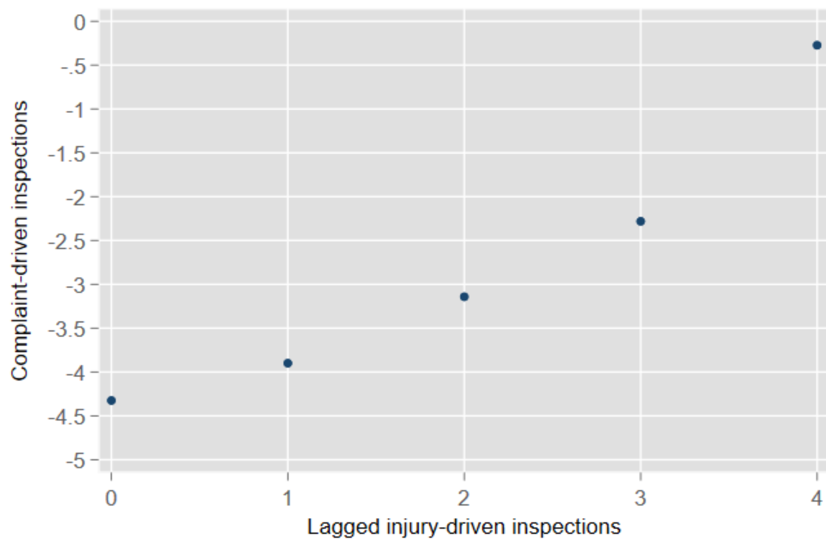


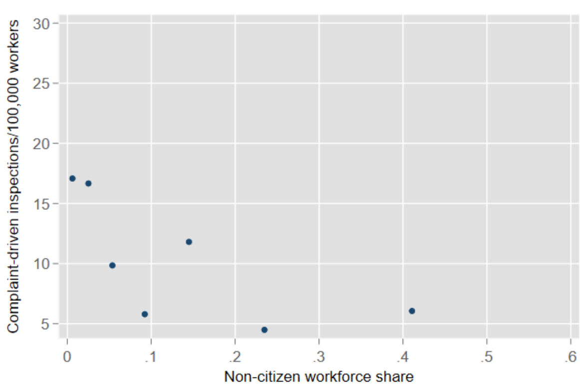
Figure A3: Relationship between complaint-driven inspections and injury-driven inspections in larger set of industries



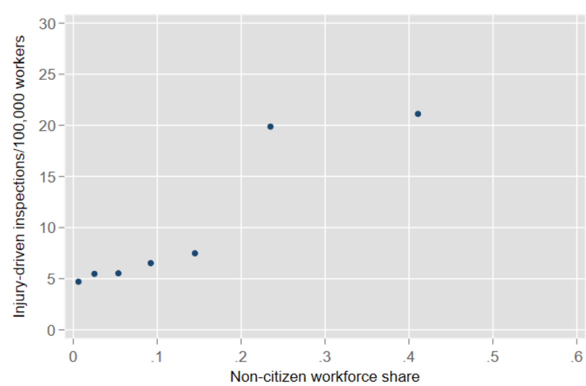
Sample includes agriculture, construction, manufacturing, wholesale trade, retail trade and transportation/warehousing. Lagged inspections are inspections from previous year. Inspections are winsorized at the 99th percentile. All correlations control for year fixed effects, industry fixed effects, employment,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

Figure A4: Correlation between worker complaints and injuries and non-citizen workforce share

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



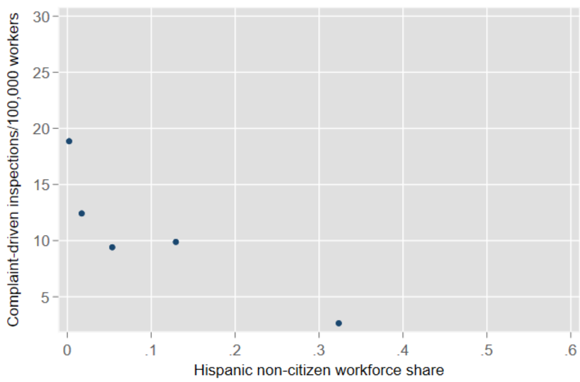
(b) Injury-driven inspections per 100,000 workers



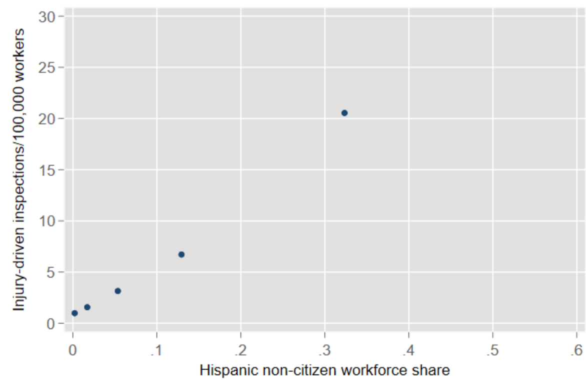
Sample includes agriculture, construction and manufacturing. Inspection rates are winsorized at the 99th percentile. All correlations are weighted by employment and control for year fixed effects, industry fixed effects, the rate of programmed inspections, metro status of the county and the log county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

Figure A5: Correlation between worker complaints and injuries and Hispanic non-citizen workforce share

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



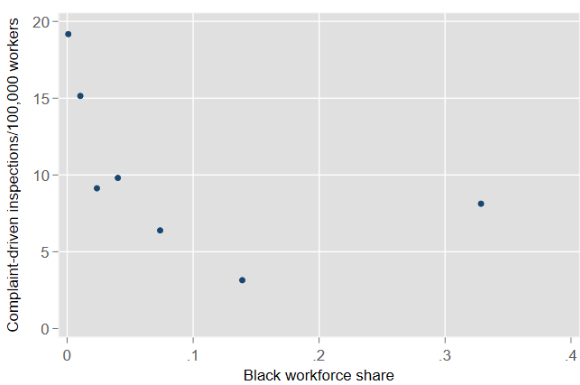
(b) Injury-driven inspections per 100,000 workers



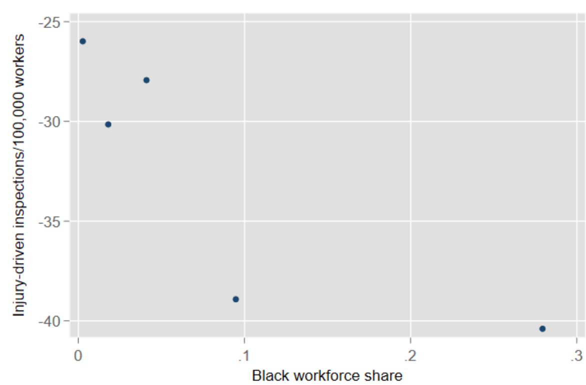
Sample includes agriculture, construction and manufacturing. Inspection rates are winsorized at the 99th percentile. All correlations are weighted by employment and control for year fixed effects, industry fixed effects, the rate of programmed inspections, metro status of the county and the log county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

Figure A6: Correlation between worker complaints, workplace injuries and African American/Black workforce share

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



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



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

Figure A7: Coefficient estimates of effect of Secure Communities on worker complaints for different employment cutoffs

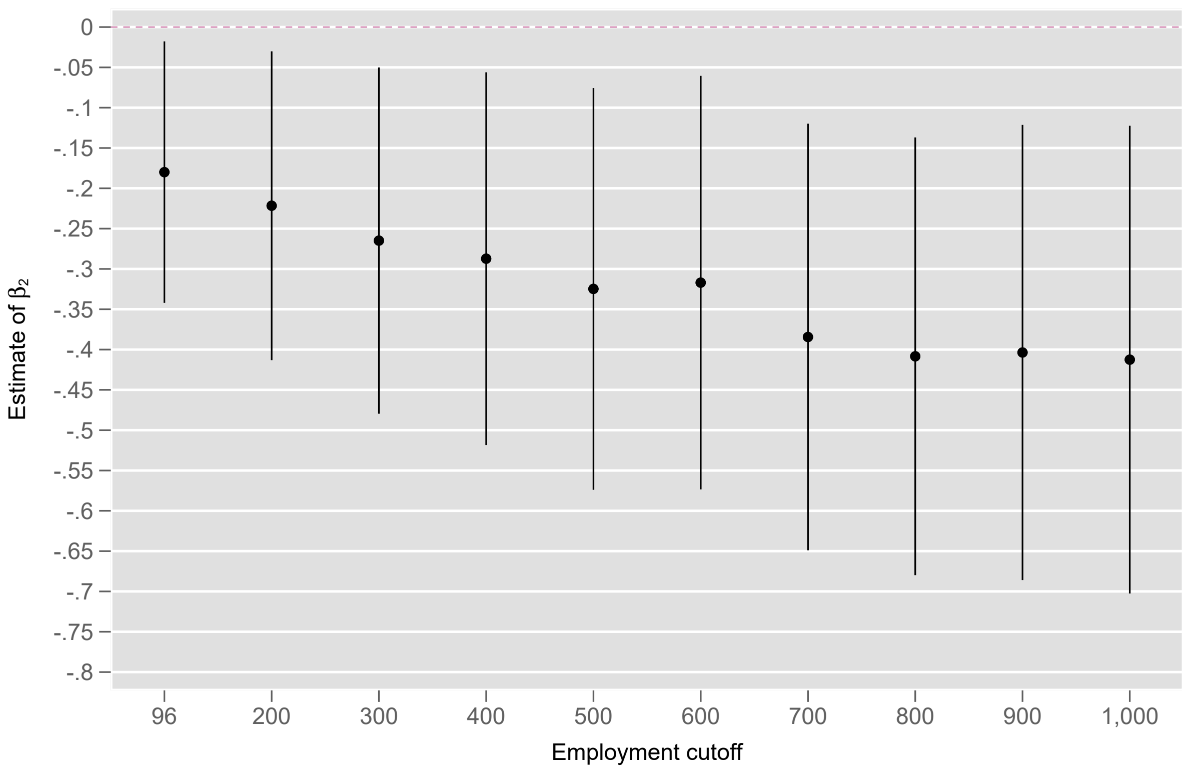


Figure shows coefficient estimates and 95% confidence intervals of β_2^k in Equation (1) for samples defined by different minimum cutoffs for a county-industry's average employment over 2001–2008. Each estimate stems for a separate regression on the specified sample. The exposure measure is the Hispanic workforce share. Regressions include controls for the inverse hyperbolic sine of county-industry employment, the inverse hyperbolic sine of programmed inspections, the interaction of Hispanic workforce share with year fixed effects, county-industry fixed effect, industry-year fixed effect and census region-year fixed effects. Standard errors used for confidence interval calculation are clustered at the county.

Figure A8: Coefficient estimates of effect of Secure Communities on workplace injuries for different employment cutoffs

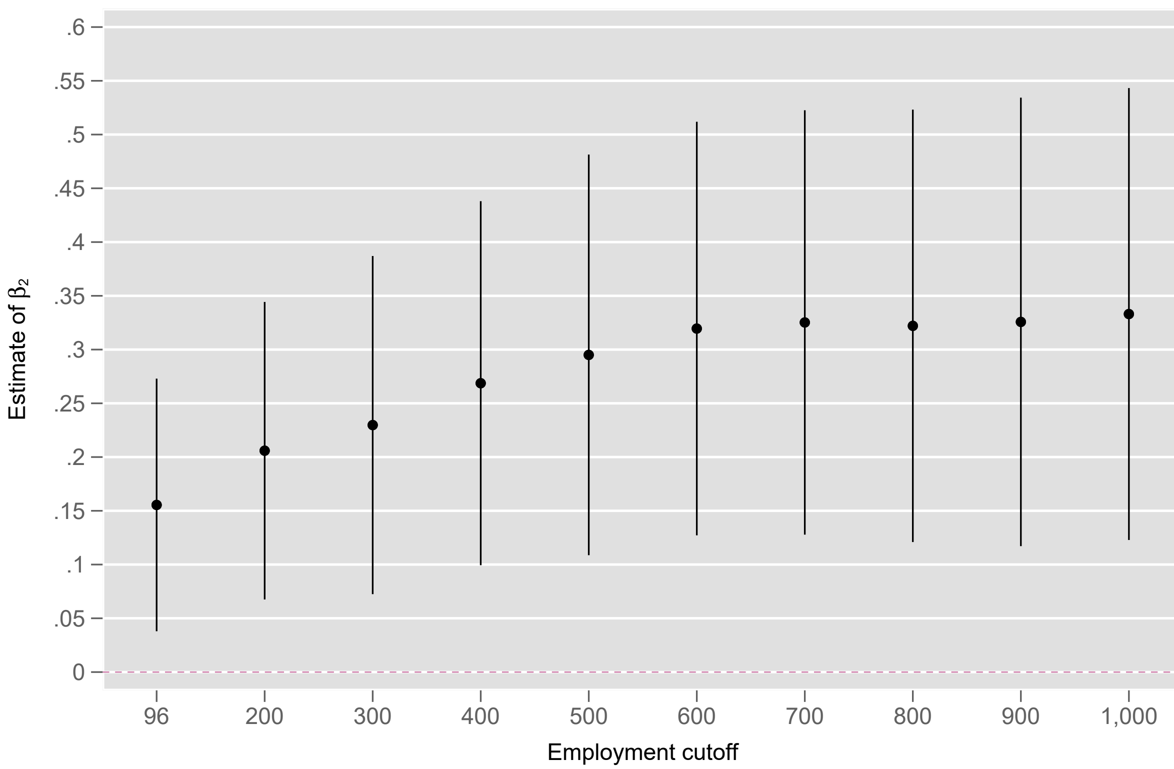
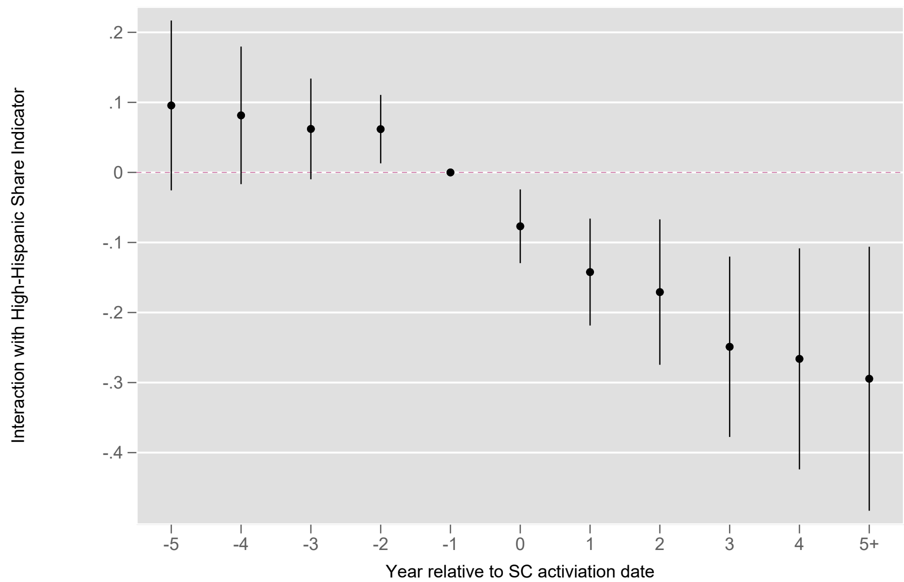


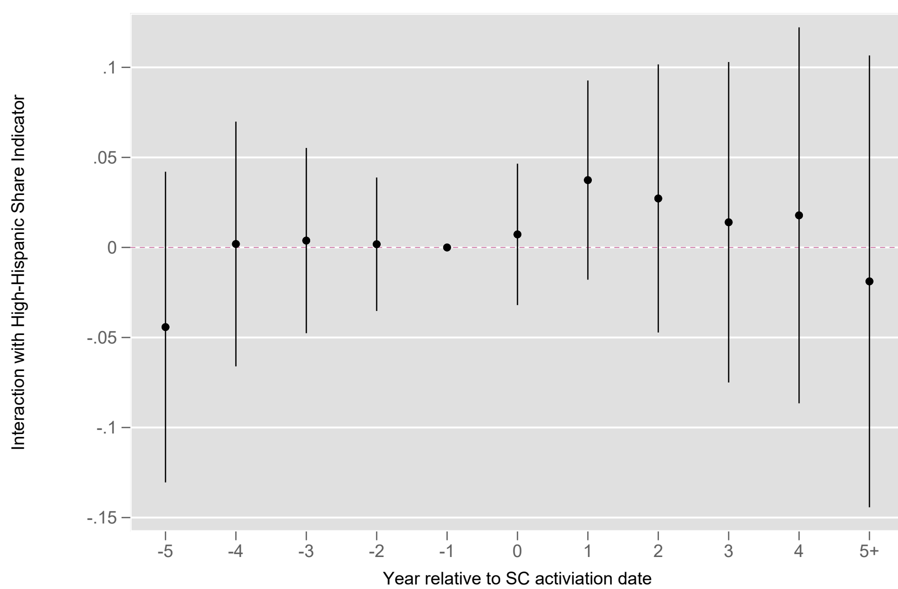
Figure shows coefficient estimates and 95% confidence intervals of β_2^k in Equation (1) for samples defined by different minimum cutoffs for a county-industry's average employment over 2001–2008. Each estimate stems for a separate regression on the specified sample. The exposure measure is the Hispanic workforce share. Regressions include controls for the inverse hyperbolic sine of county-industry employment, the inverse hyperbolic sine of programmed inspections, the interaction of Hispanic workforce share with year fixed effects, county-industry fixed effect, industry-year fixed effect and census region-year fixed effects. Standard errors used for confidence interval calculation are clustered at the county.

Figure A9: Dynamic effects of Secure Communities on Complaints and Injuries using the indicator for a high Hispanic workforce share as the Exposure Variable

(a) Effect on Complaints



(b) Effect on Injuries



Figures shows coefficient estimates and 95% confidence intervals of β_2^k in Equation (2) where $exposure_{ci}$ is an indicator for the county-industry's Hispanic workforce share being above the 80$^{\text{th}}$ percentile. Regressions include controls for the inverse hyperbolic sine of county-industry employment, the inverse hyperbolic sine of programmed inspections, the interaction of the indicator for a high Hispanic workforce share with year fixed effects, county-industry fixed effect, industry-year fixed effect and census region-year fixed effects. Standard errors used for confidence interval calculation are clustered at the county.

D Additional Tables

Table A1: Correlates of early Secure Communities Rollout

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

* 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.

Table A2: Effect of Secure Communities Participation on Employment

Dependent variable:	Inverse hyperbolic sine of annual county-industry employment			
Sample	Mean employment > 96		Mean employment >500	
	(1)	(2)	(3)	(4)
SC	-0.03***	-0.03***	-0.02*	-0.01**
	(0.01)	(0.01)	(0.01)	(0.01)
SC×Hispanic share	-0.004		-0.023	
	(0.030)		(0.033)	
SC×high Hispanic share		-0.002		-0.005
		(0.011)		(0.012)
Asinh(programmed inspections)	✓	✓	✓	✓
Exposure measure × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
Census region × year FE	✓	✓	✓	✓
Mean empoloyment	3,088	3,088	5,190	5,190
# Observations	90,664	90,664	52,206	52,206

* 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 annual county-industry employment on an indicator for a county participating in Secure Communities, the interaction of this indicator with the exposure measure (Hispanic workforce share or indicator for high Hispanic workforce share), and the controls and fixed effects indicated in the bottom panel. Columns 1 and 2 show results for regressions that use the sample of county-industries with an average employment of 96 before 2008. Columns 3 and 4 use the sample of county-industries with an average employment of 500 before 2008.

Table A3: Results are robust using an empirical Bayes shrinkage estimator to address measurement error in workplace demographics

Exposure measure:	(1) Hispanic workforce share	(2) High-Hispanic workforce share
Panel A: Complaint-driven inspections		
SC	0.039	0.004
	(0.026)	(0.015)
SC×Hispanic share [Empirical Bayes]	-0.37***	
	(0.14)	
SC×high Hispanic share [Empirical Bayes]		-0.09***
		(0.03)
Panel B: Injury-driven inspections		
SC	-0.07***	-0.05***
	(0.02)	(0.01)
SC×Hispanic share [Empirical Bayes]	0.15	
	(0.10)	
SC×high Hispanic share [Empirical Bayes]		0.04*
		(0.02)
Asinh(programmed inspections)	✓	✓
Asinh(employment)	✓	✓
Exposure measure × year	✓	✓
County × industry FE	✓	✓
Industry × year FE	✓	✓
Census region × year FE	✓	✓
# Observations	90,384	90,664

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of complaint-driven inspections (Panel A) or injury-driven inspections (Panel B) on an indicator for a county participating in Secure Communities, and the interaction of this indicator with the exposure measure, where the *Hispanic Share* has been adjusted using an empirical Bayes shrinkage estimator to account for measurement error. See Section 6.4 for details. Regressions also include the controls and fixed effects indicated in the bottom panel.

Table A4: Placebo check: effect of Secure Communities on county-industries with higher Black/African American workforce share

Dependent variable: Inverse hyperbolic sine of	complaint-driven inspections		injury-driven inspections	
	(1)	(2)	(3)	(4)
SC	-0.004	-0.003	-0.043***	-0.043***
	(0.015)	(0.015)	(0.010)	(0.010)
SC×Black share	-0.06		-0.01	
	(0.12)		(0.09)	
SC×high Black share		-0.028		0.000
		(0.030)		(0.024)
Asinh(programmed inspections)	✓	✓	✓	✓
Asinh(employment)	✓	✓	✓	✓
Exposure measure × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
Census region × year FE	✓	✓	✓	✓
# Observations	90,664	90,664	90,664	90,664

* p<0.10, ** p<0.05, *** p<0.01. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the inverse hyperbolic sine of the number of complaint-driven inspections on an indicator for a county participating in Secure Communities, the interaction of this indicator with the Black/African American workforce share or an indicator for a high Black/African American workforce share and the controls and fixed effects indicated in the bottom panel.

Table A5: Estimating the Effect of Secure Communities Using Log Regressions

Sample	Mean employment > 96		Mean employment >500	
	(1)	(2)	(3)	(4)
Panel A: Dependent variable: natural logarithm of complaint-driven inspections + 1				
SC	0.002	0.003	0.018	0.014
	(0.013)	(0.012)	(0.020)	(0.019)
SC×Hispanic share	-0.15**		-0.26**	
	(0.07)		(0.10)	
SC×high Hispanic share		-0.07***		-0.11***
		(0.02)		(0.03)
Panel A: Dependent variable: natural logarithm of complaint-driven inspections + 1				
SC	-0.04***	-0.04***	-0.06***	-0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
SC×Hispanic share	0.12**		0.23***	
	(0.05)		(0.08)	
SC×high Hispanic share		0.03		0.05*
		(0.02)		(0.03)
Ln(programmed inspections)	✓	✓	✓	✓
Ln(employment)	✓	✓	✓	✓
Exposure measure × year	✓	✓	✓	✓
County × industry FE	✓	✓	✓	✓
Industry × year FE	✓	✓	✓	✓
Census region × year FE	✓	✓	✓	✓
# Observations	90,664	90,664	52,206	52,206

* 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 natural logarithm of the number of complaint-driven inspections (Panel A) or injury-driven inspections (Panel B) + 1 on an indicator for a county participating in Secure Communities, the interaction of this indicator with the exposure measure (Hispanic workforce share in columns 1 and 3, an indicator for high Hispanic workforce share in column 2 and 4), and the controls and fixed effects indicated in the bottom panel. Columns 1 and 2 show results for regressions that use the sample of county-industries with an average employment of 96 before 2008. Columns 3 and 4 use the sample of county-industries with an average employment of 500 before 2008.

Table A6: Effect of Secure Communities on Hispanic, Non-Citizen and Hispanic Non-citizen Workforce Shares

Percent of workforce that are	(1) Hispanic	(2) Non-citizens	(3) Hispanic non-citizens
SC	-0.08	0.12	0.06
	(0.20)	(0.18)	(0.17)
County × industry FE	✓	✓	✓
Industry × year FE	✓	✓	✓
Census region × year FE	✓	✓	✓
Mean dep. variable	11.36	7.14	6.06
# Observations	77,632	77,632	77,632

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Heteroskedasticity-robust standard errors clustered at the county in parentheses. Table shows results of regressing the percent of the county-industry workforce that are Hispanic (column 1), non-citizens (column 2) or Hispanic non-citizens (column3) on and indicator for the county participating in Secure Communities and the fixed effects indicated in the bottom panel.