# Substitution between CSR Activities: Evidence from Hiring and Mistreating Unauthorized Workers and Pollution


## Abstract

We argue substitution exists among CSR investments and exogenously increasing one CSR investment could lead to a decrease in another CSR investment. We provide evidence using the U.S. states' staggered adoptions of E-Verify mandates, which curtails a labor-related social bad by reducing the hiring of unauthorized workers and related workplace abuses. We find the mandate leads to an increase in plant-level pollution, an environmental social bad, and the effect is stronger when the mandate applies to more employers, for plants in states with more unauthorized workers in the labor force, and for plants with jobs that are inherently more hazardous.



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## 1. Introduction

Corporate social responsibility (CSR), or environmental, social, and governance (ESG) activities, have increasingly attracted interest from practitioner communities and academics in recent decades. CSR spans efforts in multiple dimensions, primarily environmental and social issues, to address a firm's externalities that are not internalized by shareholders (Magill, Quinzii, and Rochet 2015; Christenen, Hail, and Leuz 2021).[1] The literature of strategic CSR argues that investments in CSR can be part of an optimal firm strategy due to the demand for them from socially responsible stakeholders, such as consumers, employees, activists, and regulators (Baron 2001; Kitzmueller and Shimshack 2012).

One implication of the strategic view is that CSR activities on various dimensions are likely evaluated as an aggregated package by the relevant stakeholders, and that the equilibrium levels of different dimensions of CSR, rather than being independently determined, can substitute for each other. For instance, socially responsible stakeholders could view different combinations of efforts on environmental issues and social issues as being similarly socially responsible and thus the equilibrium levels of the two different CSR activities are determined by their relative costs. As firms are constrained by limited resources, conflicts of interests among stakeholder groups competing for financial resources and managerial attention may arise (Wang et al. 2016). Thus, managers may prioritize and balance different aspects of CSR, leading to a substitution between different CSR activities.

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[1] Out of the three dimensions of ESG—environmental, social, and governance, the environmental and social dimensions are major ones, because the governance dimension overlaps with traditional corporate governance issues (Liang and Renneboog 2017).

Based on these insights, we argue that exogenously increasing one CSR activity could lead to a reduction in another CSR activity in equilibrium. For instance, forcing firms to be more employee-friendly may induce them to be less environment-friendly. We provide evidence on the substitution proposition by examining the impact of a regulation-induced reduction in the hiring of unauthorized workers and the related workplace abuses (a labor-related social bad[2]) on pollution (an environmental social bad).[3] We take a broad view that CSR activities include firms' engagement in and compliance with environmental and social issues, and view the hiring of unauthorized workers and the related workplace abuses as a social bad.[4] Firms' engagement in not hiring or abusing unauthorized workers is thus a CSR activity in the dimension of labor relations, which is an important social aspect of CSR (Edmans 2012; Fick 2014; Wang et al. 2016).

Hiring unauthorized workers has adverse social impacts and is socially irresponsible because it not only reduces the employment opportunities and compensation of authorized workers but also encourages unauthorized workers to enter the United States illegally (Amuedo-Dorantes and Bansak 2014).[5] Moreover, as unauthorized workers tend to be underpaid and to work under poorer and more dangerous conditions, their employers generally perform worse in the labor dimension of CSR (Joseph 2011; Green 2014; Lee 2018; Garcia Quijano 2020). For instance, Mehta et al. (2002) and Orrenius and Zavodny (2009) find that immigrant workers are more likely to

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[2] A "good" is something consumers are willing to pay for, and a "bad" is something that consumers are willing to pay to have removed or must be compensated to accept (Becker and Murphy 1993).

[3] We focus on social bads because stakeholders and regulators tend to care more about the curtailment of social bads than the provision of social goods. Pollution and hiring and mistreating unauthorized workers are both negative CSR behaviors (i.e., social bads). One is more likely to find a substitution effect for two social bads if stakeholders view reduction of a social bad as more comparable to reduction of another social bad than to provision of a social good (e.g., corporate philanthropy).

[4] Engagement refers to a firm's voluntary investment in CSR projects, while compliance refers to behavior that a firm is required or encouraged to follow (Liang and Renneboog 2017).

[5] According to Orrenius and Zavodny (2012), almost all unauthorized immigrant men are in the labor force, either working or actively searching for a job. Their labor force participation rate is extraordinarily high—94% among working-age men in 2008. The high participation rate is due to the fact that almost all of them enter the U.S. to work.

experience workplace injuries or fatalities than native-born workers and that, among immigrant workers, the unauthorized ones are even more likely to experience unsafe conditions.

Specifically, we examine the impact of the E-Verify mandate, which requires employers to verify the work authorization of new hires and thus reduces the hiring of unauthorized workers (and their related mistreatment).[6] E-Verify is a free, internet-based program run by the federal government. It allows employers that enroll in the program to confirm the eligibility of employees to work in U.S. While the federal government requires its own agencies and contractors to use E-Verify, its use by other employers is voluntary unless it is required by the individual state. Currently, 22 states mandate the use of E-Verify for at least some employers (see Table 1). Nine states require E-Verify for all employers ("universal E-Verify" hereafter), and 13 require it for certain public employers and/or public contractors ("public E-Verify" hereafter). Firms that are not subject to any government rule may still choose to use the system.

The purpose of an employment verification mandate is to reduce the hiring of unauthorized workers. Prior studies show that E-Verify mandates, particularly universal E-Verify, are effective at this, in that they reduce the likelihood of employment for unauthorized workers (e.g., Amuedo-Dorantes and Bansak 2012). The use of the system has increased steadily, and the share of newly hired workers that are run through the system reached 50% nationwide in 2015 (Orrenius and Zavodny 2017). There is also suggestive evidence that the mandates deter illegal immigration to the U.S. in general (Amuedo-Dorantes and Bansak 2014; Orrenius and Zavodny 2016, 2017).

[6] As the use of unauthorized workers is inherently positively correlated with workplace abuses, the setting does not allow us to examine how the reduced use of unauthorized workers substitutes for other labor-related social bads. Indeed, we assume that the adoption of E-Verify mandates will reduce workplace abuses, which we confirm empirically in Section 4.4.4.

The E-Verify setting has several advantages for investigating the substitution between firms' equilibrium CSR activities. First, in order to establish substitution, one CSR activity must change exogenously. The E-Verify mandates introduce an exogenous reduction in the hiring of unauthorized workers and the related workplace abuses at the state level. By investigating how other CSR activities respond to the mandate, we can examine whether the mandated change in one dimension of CSR (not hiring and mistreating unauthorized workers) substitutes other CSR activities in equilibrium.[7] Second, the staggered adoption of E-Verify mandates across states (see Table 1) allows us to implement a generalized difference-in-differences (DiD) design and mitigate the concern of confounding factors (e.g., Bertrand and Mullainathan 2003). Third, unauthorized workers often do manual, low-paying jobs and tend to be employed by facilities in *polluting* industries such as manufacturing, metal mining, electric power generation, and hazardous waste treatment. Thus, the E-Verify setting allows for a powerful test of our substitution argument.

We measure facility-level pollution using the toxic release data from the Toxic Release Inventory (TRI) database provided by the U.S. Environmental Protection Agency (EPA). We follow Akey and Appel (2019, 2021) and conduct the analysis at the plant-chemical-year level using a generalized DiD design, in which we control for time-invariant heterogeneity at the plant-chemical level and time-varying heterogeneity at the chemical level and at the industry level.[8] We find that the adoption of E-Verify mandates significantly increases toxic releases, by 6.0%–6.6%, and that the effect is stronger for universal E-Verify (about 11.0%) than for public E-Verify (about 5.0%). Given the median chemical release of 107 pounds at the plant-chemical level in our sample,

[7] While we expect our substitution argument to apply to all CSR activities, the argument does not imply that substitution exists in any pair of CSR activities. Thus, our findings should be viewed as an example of such substitution, not a proof.
[8] Our main findings are robust to further controlling for time-varying heterogeneity at the parent firm level and state-level macroeconomic conditions, including GDP growth, population growth, and unemployment rate.

the estimated effect of universal E-Verify mandates implies a 12-pound increase in toxic releases for each plant-chemical. The effect of universal mandates becomes stronger over time, consistent with their being enforced more strictly in later years after their passage.

We test the parallel trends assumption by examining the pre-adoption trend of toxic releases for plants in the treatment states relative to control states (Roberts and Whited 2012). We find consistent evidence that the pre-adoption trends are similar for plants in states that adopt universal E-Verify (public E-Verify) mandates and plants in other states, supporting the parallel trends assumption. Barrios (2021) and Baker et al. (2021) argue that because already-treated units are used as comparison units for later treated units, the standard staggered DiD design is subject to a potential bias. Thus, we conduct robustness tests using the stacked-regression approach they suggest, and find consistent results.

We further conduct a cross-sectional analysis based on the proportion of unauthorized workers in the state's labor force. We find that the effect of E-Verify mandates, both universal and public, is stronger for states that have a larger proportion of unauthorized workers in the labor force in the pre-adoption year. In these states, the adoption of universal (public) E-Verify mandates leads to an increase in toxic releases of 14.0%–17.5% (13.6%–14.5%). In contrast, for states with lower proportion of unauthorized workers, the adoption does not lead to a significant increase in toxic releases.

In another cross-sectional analysis, we find that the effect of E-Verify mandates, both universal and public, on pollution is stronger for plants with jobs that are inherently more hazardous. When the job is inherently of high hazard and requires more of a safety investment by the employer, such as in iron and steel foundries, it is easier for the employer to exploit unauthorized workers by reducing the safety investment. Thus, the labor-related CSR improvement due to the E-Verify mandate is larger for plants with high-hazard jobs, and the effect of E-Verify mandates on pollution is stronger accordingly.

One presumption of our empirical predictions is that a reduction in unauthorized workers is accompanied by reduced workplace abuses. This presumption is supported by the finding in prior literature that unauthorized workers tend to be underpaid and to work under poorer and more dangerous conditions (Joseph 2011; Green 2014; Lee 2018; Garcia Quijano 2020). Nevertheless, to provide direct evidence that E-Verify mandates reduce workplace abuses of unauthorized workers and thus improve labor-related CSR, we further examine their impact on employers' labor violations using the data on facility-level workplace safety inspections and violations. We find that employers' labor-related violations decrease after the adoption of universal E-Verify.

We further investigate the possible channels for the increased toxic releases due to the adoption of E-Verify mandates. We explore three possible channels: reducing pollution abatement actions, increasing production, and reducing toxic emission efficiency (releasing more toxics given the same production level). We find no evidence that the mandates' adoption leads to changes in pollution abatement actions. Instead, our evidence suggests that the effect is mainly driven by reduced emission efficiency, and may also be driven by increased production in the post-adoption period.

Finally, we address the alternative explanation that our main finding could be due to changes in the financial conditions of a plant's parent firm following the E-Verify mandates. In particular, financial constraints could increase firms' toxic emissions (Xu and Kim 2022), and firms could increase pollution in order to meet or beat earnings benchmarks (Thomas et al. 2022, Liu et al. 2021). We find that the E-Verify mandates does not lead to a significant increase in either financial constraints or incentives to meet or beat earnings benchmarks for a plant's parent firm, minimizing the possibility of such alternative explanations.

Our study contributes to the CSR literature (e.g., Liang and Renneboog 2017; Cronqvist and Yu 2017; Dai et al. 2021). Our innovation is to propose and empirically show a substitution between the equilibrium levels of different CSR activities. To our knowledge, we are the first to explicitly show a substitution effect by examining how one form of CSR responds to an exogenous change in another form of CSR. Our finding has two implications. First, policymakers that are considering the use of regulation to reduce a social bad or increase a social good should weigh the possibility of a hidden cost: a reduction in another CSR activity. This is especially important given the increasing trend, in many countries and regions, of mandating or specifying certain aspects of CSR (Wang et al. 2016). Second, due to the substitution effect, researchers should measure CSR activities as a package when the construct of interest is a firm's overall investment in CSR. Focusing on a single aspect of CSR, such as being employee-friendly or reducing pollution, could lead to biased findings.

We also contribute to the literature on the determinants of firms' environmental behaviors (e.g., Akey and Appel 2019; Xu and Kim 2022). Extant studies have shown that firms' environmental behaviors are influenced by ownership structure (e.g., Akey and Appel 2019; Shive and Forster 2020), financial and resource constraints (e.g., Cohn and Deryugina 2018; Goetz 2019; Xu and Kim 2022), and legal liability protections (Akey and Appel 2021). We take a unique perspective and show that regulating other CSR activities could impact firms' environmental behaviors through a substitution effect. While prior studies on the relation between the labor market and pollution focus on the effect of pollution on the labor market (e.g., Graff Zivin and

Neidell 2012; Hanna and Oliva 2015), our study suggests a channel through which the labor market could influence pollution.

Finally, our study adds to the understanding of the economic impacts of the E-Verify mandates. Prior studies have documented or discussed the intended or unintended consequences of the E-Verify mandates, such as reducing the likelihood of hiring unauthorized workers (e.g., Amuedo-Dorantes and Bansak 2012), differential impacts on unauthorized workers of different genders (Amuedo-Dorantes and Bansak 2014), and employers' turning to alternative ways of using unauthorized workers (Ayón et al. 2011). We show that the mandates also decrease employers' labor violations. And, most importantly, while prior studies focus on the mandates' consequences in the labor market, we show that they have an unintended impact on pollution.

## 2. Institutional Background

Unauthorized immigrants account for a significant portion of the U.S. population. In 2017, there were around 10.5 million unauthorized immigrants, representing 3.2% of the population; among them are 7.6 million unauthorized workers, who collectively account for 4.6% of the workforce (Krogstad et al. 2019). Most unauthorized workers work in low-wage jobs (Garcia Quijano 2020). Due to the fear of deportation, unauthorized workers experience more workplace abuses, such as being underpaid, going unpaid, or working in unsafe conditions, without seeking legal protections (Mehta et al. 2002; Orrenius and Zavodny 2009; Green 2014; Lee 2018). Specifically, Orrenius and Zavodny (2009) document that immigrant workers are more likely to experience workplace injuries or fatalities than native-born workers. Mehta et al. (2002) find that among immigrant workers, the unauthorized ones are more likely to experience unsafe conditions.

Survey evidence suggests that 76% of unauthorized workers experience wage theft, while 37% receive less than the minimum wage (Green 2014).

Immigration policy has long targeted illegal immigration. In recent decades, this has included policy efforts to eliminate employment opportunities for unauthorized immigrants. The Immigration Reform and Control Act (IRCA) of 1986 established employer sanctions for hiring unauthorized workers for the first time in U.S. history. However, IRCA failed to curb the number of unauthorized workers due to its lack of a reliable system for verifying work authorization of job seekers. Under IRCA, employers were only required to check worker documents for identification and employment authorization. This spawned a market for fraudulent documents that "proved" identity and work eligibility. Despite the passage of the act, the population of illegal immigrants increased from 5 million in 1986 to 12 million in 2007 (Plumer 2013a,b).

Congress responded to the document fraud under the IRCA by establishing an electronic verification system in 1996—a system that evolved into the current E-Verify system.[9] Under E-Verify, within three days after hiring a worker, employers are required to submit the worker's biographic information into the system. The data is then checked against Social Security Administration (SSA) and/or Department of Homeland Security (DHS) records. If a worker's identity data cannot be verified, the worker has eight days to contact federal agencies to resolve the issue, after which the worker's employment may be terminated.

E-Verify, which has been available for employers in all states since 2003, began as a voluntary program. Subsequently, many states enacted legislation requiring the use of E-Verify

[9] The congress creates three pilot programs: Basic Pilot, Citizenship Attestation Verification Pilot, and Machine-Readable Document Pilot. The latter two were dropped in favor of the more effective Basic Pilot. The Basic Pilot was first implemented in several states in 1997 and became available to employers in all states in 2003.

for at least some public and/or private employers. In 2006, Colorado became the first state to require E-Verify for public employers. A year later, Arizona became the first to require E-Verify for all employers. [10] Currently, 22 states mandate the use of E-Verify for at least some employers (see Table 1). Nine states require it for all employers (universal E-Verify),[11] and 13 mandate it for public employers or public contractors (public E-Verify). [12] Firms that are not subject to a government requirement may still voluntarily use the system. In addition to state governments, the federal government also requires employers with federal contracts or subcontracts to use E-Verify if the contracts contain the Federal Acquisition Regulation (FAR) E-Verify clause (Kidd 2011).

Evidence from empirical research on the policy implications of E-Verify is generally consistent with the expected employment and population effects. Amuedo-Dorantes et al. (2012, 2014) find that E-Verify mandates, particularly universal E-Verify, significantly decrease the likelihood of employment for likely unauthorized workers. Bohn et al. (2014) document a reduction in the fraction of the Hispanic noncitizen population in Arizona after the state adopted a universal E-Verify system in 2007. Raphael et al. (2009) also find significant declines in the employment and population of Hispanics in states that mandate E-Verify. Orrenius and Zavodny (2016) further find that some unauthorized workers either move to other states or leave the country, resulting in population reductions in states that mandate E-Verify.

## 3. Theoretical Framework and Hypothesis

[10] From its inception, the Legal Arizona Workers Act, which mandates the statewide usage of E-Verify in Arizona, was highly controversial and was viewed as the roughest immigration enforcement (Chishti and Bergeron 2011). As soon as it was signed, a coalition of immigrant-rights and business groups filed a lawsuit to enjoin the act. On June 15, 2011, the U.S. Supreme Court upheld the Act.

[11] These states are Alabama, Arizona, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Utah. Some of these states have exemptions for small businesses.

[12] These states are Colorado, Florida, Idaho, Indiana, Michigan, Minnesota, Missouri, Nebraska, Oklahoma, Pennsylvania, Texas, Virginia, and West Virginia.

### 3.1 Theoretical Views of CSR

The literature has proposed two broad frameworks to explain the existence of CSR activities: not-for-profit or strategic (Kitzmuller and Shimshack 2012). The not-for-profit view of CSR posits that CSR investment is not part of firms' value maximization strategies but instead results when shareholders' or managers' concerns about social or environmental performance generate incentives to invest in CSR at the expense of monetary profits (Wartick and Cochran 1985; Wood 1991; Reinhardt et al. 2008). When managers have not-for-profit objectives to invest in CSR but shareholders do not, CSR investment can still arise due to managers' moral hazard problem (Friedman 1970; Kitzmuller and Shimshack 2012).

In contrast, the strategic view of CSR—the more popular of the two explanations—assumes that shareholders' objectives are to maximize firm value. Under this view, when stakeholders such as customers, employees, social activists, and regulators have social or environmental preferences, investment in CSR arises as part of the value maximization strategy (Baron 2001; McWilliams and Siegel 2001; Kitzmuller and Shimshack 2012). In other words, because of stakeholders' demand for CSR, investing in it can enhance profitability and firm value, which is often referred to as "doing well by doing good" (Dowell et al. 2000; Dimson, Karakas, and Li 2015; Liang and Renneboog 2017). Thus, firms trade off between the economic benefits and costs of CSR activities to determine the optimal levels, just as they do in other investment and operational decisions.

Kitzmuller and Shimshack (2012) review empirical support for the two views (not-for-profit vs. strategic) and conclude that the empirical evidence is not consistent with the non-for-profit motivations of CSR; the observational evidence for strategic CSR, they find, is more favorable. Under the strategic view of CSR, different CSR activities can be viewed as part of a package of "good behavior"; the equilibrium composition of the package depends on the market demand for

CSR and what the firm can offer cost-effectively. In practice, as firms are constrained by limited resources, different stakeholder groups may compete for financial resources and managerial attention, and managers may therefore need to prioritize and balance aspects of CSR (Wang et al. 2016).

### 3.2 Hypothesis and Empirical Predictions

Building upon the theory of strategic CSR, we hypothesize that exogenously increasing one CSR activity could lead to a reduction of another CSR activity in equilibrium. This prediction is premised on the assumption that different CSR activities aggregately satisfy the demand for CSR from various stakeholders and that there could be substitutions among the different activities. For instance, stakeholders could view different combinations of environment friendliness and employee friendliness as being equally socially responsible in the aggregate. Firms could thus maintain an optimal level of CSR reputation while minimizing the costs of CSR through cost-effective combinations of different CSR activities. If this is the case, then an exogenous increase in one CSR activity—which a regulation might cause—could lead to a decrease in another CSR activity. This hypothesis is consistent with Wang et al.'s (2016) argument that managers may prioritize aspects of CSR based on stakeholders' competition for financial resources and managerial attention. When a regulation forces one aspect of CSR to improve, managers will prioritize this activity and reduce investment in other CSR activities.

The hypothesis is not obvious ex ante for three reasons. First, it is possible that stakeholders do not view different CSR activities as a package. For instance, environmental activists could focus solely on environmental performance when they evaluate a firm's CSR performance. If they do, then they could impose costs on a polluting firm even if it invests heavily in other CSR activities (such as donating to charity). Second, when a regulation increases the cost of using one social bad (e.g., workplace abuses), the equilibrium production level could decrease, which could reduce the use of another social bad (e.g., pollution). Third, to the extent that CSR investments are driven by shareholders' or managers' not-for-profit incentives (i.e., their social and environmental objectives), the level of a CSR activity could be primarily determined by their specific social or environmental objective—for example, how much pollution is tolerable based on an environmental objective—and thus not be a function of another CSR activity.

To test the hypothesis, it is critical to identify an exogenous shock to one aspect of a firm's CSR and then examine the change in other CSR activities. We provide evidence by examining the impact of the E-Verify mandate—which, by requiring employers to verify the work authorization of new hires, reduces the hiring of unauthorized workers and related workplace abuses (a social bad)—on pollution (another social bad). CSR broadly includes social and environmental performance (Kitzmuller and Shimshack 2012). Hiring unauthorized workers has adverse social impacts and is socially irresponsible. It not only reduces the employment opportunities and compensation of authorized workers but also encourages unauthorized workers to enter the United States illegally (Amuedo-Dorantes and Bansak 2014). Moreover, as we discuss in Section 2, unauthorized workers experience more workplace abuses, such as being underpaid or denied pay or working in unsafe conditions, without seeking legal protections (Mehta et al. 2002; Orrenius and Zavodny 2009; Green 2014; Lee 2018). Thus, the use of unauthorized workers also increases labor mistreatment.

As we discuss in Section 2, the E-Verify mandates force employers to reduce their hiring of unauthorized workers, which exogenously increases their investment in the CSR activities of hiring authorized workers and being employee-friendly. Based on the substitution hypothesis above, we predict that the mandates reduce plants' investment in another important aspect of CSR—pollution reduction. Thus, we expect pollution to increase after the mandates. This leads to our first empirical prediction as follows.

*P1: Plant-level pollution increases after a plant's location state adopts the E-Verify mandate.*

If the effect of E-Verify mandates on plant-level pollution occurs because plants substitute the reduction in unauthorized workers with more pollution, we expect the effect to be stronger for universal E-Verify mandates than for public E-Verify mandates, because the former leads to greater reduction in the use of unauthorized workers at the state level. Consistent with the stronger impact of universal E-Verify, Amuedo-Dorantes and Bansak (2012) document that the effect of universal E-Verify mandates on the likelihood employment for likely unauthorized workers is stronger than that of public E-Verify mandates.

*P2: The effect of E-Verify mandates on plant-level pollution is stronger for universal E-Verify than for public E-Verify.*

The impact of the E-Verify mandates on the use of unauthorized workers depends on the extent to which local plants hired unauthorized workers prior to the mandate, which is a function of the fraction of unauthorized workers in the total labor force in a state. For geographical and historical reasons, the population of unauthorized immigrants varies much across states. According to Passel and Cohn (2018), a majority of the unauthorized immigrants in the United States live in six states: California (2.2 million in 2016), Texas (1.6 million), Florida (0.78 million), New York (0.73 million), New Jersey (0.48 million), and Illinois (0.4 million). In contrast, there are eight states with 5,000 or fewer unauthorized immigrants in 2016.[13] The fraction of unauthorized workers in the total labor force ranges between 0.2% (Vermont) and 10.6% (Nevada), with the national average being 4.8%. We expect E-Verify mandates to have a stronger impact on plant-level pollution in states with a higher fraction of unauthorized workers in the total labor force prior to the mandate.

*P3*: *The effect of E-Verify mandates on plant-level pollution is stronger in states with a higher fraction of unauthorized workers in the total labor force prior to the mandate.*

One important reason why an E-Verify mandate can improve the labor component of CSR is that it reduces workplace abuses for unauthorized workers. One such abuse is to require unauthorized workers to work in conditions that are less safe than the normal conditions for authorized workers (Mehta et al. 2002; Lee 2018). Such mistreatment is more likely if the job is inherently more hazardous, such as in iron and steel foundries. When the job is inherently of low hazard and requires less of a safety investment by the employer, the employer will find it more difficult to exploit unauthorized workers by reducing the safety investment. Thus, the labor-related CRS improvement due to the E-Verify mandate is likely to be smaller for plants with low-hazard jobs. Consequently, we predict that E-Verify mandates have a stronger impact on plant-level pollution for plants with inherently more hazardous jobs.

*P4*: *The effect of E-Verify mandates on plant-level pollution is stronger for plants with jobs that are inherently more hazardous.*

Even if our CSR-substitution hypothesis is valid, our predictions above are not obvious, because firms engage in many CSR activities and it is unclear, ex ante, which other CSR activities

[13] These states are Alaska, Maine, Montana, North Dakota, South Dakota, Vermont, West Virginia, and Wyoming.

will be affected. For instance, plants may respond to the E-Verify mandates by reducing donations to the community instead of by increasing pollution. Thus, whether the E-Verify mandates have a significant impact on plant-level pollution is ultimately an empirical question.

## 4. Empirical Analyses

### 4.1 Data

We use the toxic release data from the Toxic Release Inventory (TRI) program to measure firm pollution (e.g., Akey and Appel 2019, 2021; Xu and Kim 2022; Zhou 2020). The TRI program was established by the Emergency Planning and Community Right-to-Know Act (EPCRA) in 1986. It requires facilities in the manufacturing and utility industries that have at least 10 full-time workers to report the release of toxic chemicals on the TRI chemical lists if they use, process, or produce the chemicals above certain amounts. Section 313 of EPCRA specifies that chemicals covered by the TRI program cause at least one of the following: 1) cancer or other chronic human health effects; 2) significant adverse acute human health effects; or 3) significant adverse environmental effects (Xu and Kim 2022). Currently, 675 toxic chemicals released into underground, land, water, or air are included in the list.

A facility must report its toxic release information for a calendar year to the EPA before July of the next calendar year. The EPA monitors the self-reported data for potential errors through a quality check. The agency queries the "red-flagged" facilities, including those that report large decreases in releases or zero releases, that discontinue reporting, or that report the same release quantities as in the previous year (Zhou 2020). Upon identifying noncompliance, the EPA can initiate civil enforcement actions.[14] According to Kerth and Vinyard (2012), the TRI program is the most comprehensive information source for toxic releases in U.S. The program provides not only the amounts of toxic releases for each chemical but also information on the production related to, and pollution abatement actions for, each chemical. The reported abatement actions fall into several categories, including process modifications, better operating practices, actions to prevent spills and leaks, product redesigns and so on (Akey and Appel 2019).

### 4.2 Variable Measurement and Summary Statistics

We examine the impact of E-Verify mandates on pollution at the chemical-plant-year level (e.g., Akey and Appel 2019, 2021). This approach allows us to avoid the issue of aggregating pollutants with differential environmental impacts (Chatterji et a. 2009). Our main measure of pollution is the amount of a chemical (in pounds) released by a plant in a year (*Toxic*). In our channel analysis, we measure pollution abatement by the number of abatement activities (*Abatement*) (Akey and Appel 2019). To separately examine operation-related (engaging in a better operating process) and process-related (making process modifications) abatement actions, we also explore the number of operation-related abatement activities (*Abatement_Op*) and the number of process-related abatement activities (*Abatement_Pro*).

We follow Akey and Appel (2019) and use normalized production (*Normalized_Prod*) to measure production at the plant-chemical level. The TRI dataset reports the production ratio for each chemical (*Production_Ratio*). Specifically, if a chemical is used to produce product A, *Production_Ratio* is calculated as the amount of product A produced in year $t$ divided by the

[14] For instance, Lucas-Milhaupt Warwick LLC, a metal producer, was required by the EPA to pay $69,265 in 2016 for failing to file the proper toxic release inventory reporting forms for copper and silver for 2012, 2013, and 2014, violating the EPCRA (Zhou 2020).

amount produced in year *t-1*. *Normalized_Prod* is defined as the production amount in year *t* scaled by the amount in the first year that a chemical is reported in the TRI database. It is calculated as the product of all historical *Production_Ratio*.[15]

To examine the impact of E-Verify mandates, we create an indicator variable *EVerify*, which equals 1 if a plant's location state has passed a universal or public E-Verify mandate in year *t*, and 0 otherwise. To separately investigate the impacts of universal and public E-Verify mandates, we define another two indicator variables, *EVerify_All* and *EVerify_Pub*. *EVerify_All* (*EVerify_Pub*) equals 1 if a plant's location state has passed a universal (public) E-Verify mandate in year *t*, and 0 otherwise. To create a sharper comparison of the years before and after the passage of E-Verify mandates, we remove the year in which a plant's state passes the mandate.

Our sample period is from 2000 to 2018. The sample consists of 1,375,415 plan-chemical observations for 39,679 plants. Table 2, Panel A reports the industry distribution of the sample. The majority of plants are from the manufacturing industry (89.7%). At the plant-chemical level, 81.1% observations are from manufacturing. While the utilities industry accounts for only 2.2% of plants, its average plant-level toxic release is the highest, 7.3 million pounds per year. Plants in the mining, quarrying, and oil and gas extraction industry produce the second-highest per-plant toxic release: 4.0 million pounds per year.

Table 3 reports summary statistics of the sample. The average toxic release (*Toxic*) at the plant-chemical level is 17,603 pounds, and the median is 107 pounds. The distribution of *Toxic* is highly skewed, which justifies the use of log transformation (*Ln(1+Toxic)*) in the regression analysis. The mean and median of *Ln(1+Toxic)* are close, 4.70 vs. 4.68, suggesting that the log

[15] We set *Production_Ratio* to one if it is missing from the database (Akey and Appel 2019).

transformation substantially reduces skewness. The mean of *EVerify* is 0.22, indicating that 22% of plant-chemical observations are in states that have passed universal or public E-Verify mandates. The means of *EVerify_All* and *EVerify_Pub* are 0.08 and 0.14, respectively, indicating that 8% (14%) of plant-chemical observations are in states that have passed universal (public) E-Verify mandates.

**4.3 Research Design**

We estimate an OLS model similar to the one used by Akey and Appel (2019, 2021):

$$Ln(1+Toxic_{c,p,s,t}) = \beta_1 EVerify_{s,t}\ (\gamma_1 EVerify\_All_{s,t} + \gamma_2 EVerify\_Pub_{s,t}) + \alpha_{p,c} + \alpha_{c,t} + \varepsilon_{c,p,s,t}, \quad (1)$$

where $c$, $p$, $s$, and $t$ denote chemical, plant, location state, and year, respectively. *Ln(1+Toxic)*, *EVerify*, *EVerify_All*, and *EVerify_Pub* are as defined in Section 4.2. $\alpha_{p,c}$ denotes plant-chemical fixed effects, which control for time-invariant heterogeneity for a chemical at the plant level. $\alpha_{c,t}$ is chemical-year fixed effects, which control for time-varying heterogeneity at the chemical level. The estimate of $\beta_1$ ($\gamma_1$, $\gamma_2$) captures the effect of (universal, public) E-Verify mandates on within-plant change in the release of a chemical relative to plants that use the same chemicals but are located in states that do not experience a change in (universal, public) E-Verify mandates. In an augmented model, we further include industry-year fixed effects. Because the E-Verify mandates are adopted at the state level, we cluster robust standard errors at the state level (e.g., Klasa et al. 2018). Our first empirical prediction (P1) is that $\beta_1$, $\gamma_1$, and $\gamma_2$ are significantly positive; the second empirical prediction (P2) is that $\gamma_1$ is significantly more positive than $\gamma_2$.

To test our third empirical prediction, P3, we separately examine whether the effect of universal (public) E-Verify mandates is stronger for plants located in states with a larger fraction of unauthorized workers in the labor force (*Unauthorized Workers*). Following Amuedo-Dorantes and Bansak (2012), we define unauthorized workers as Hispanic noncitizens who are 16–45 years old and have a high school education or less. We use the data from American Community Survey to estimate the percentage of unauthorized workers in the labor force for each state. A natural research design is to partition the sample based on *Unauthorized Workers* in each state and estimate equation (1) separately. However, as the fraction of unauthorized workers in the labor force could change after the E-Verify mandates, partitioning the sample based on the fraction in each state-year could lead to an inconsistent estimate of the treatment effect (Gormley and Matsa 2011). To address this issue, we employ a cohort-based approach (e.g., Gormley and Matsa 2011; Bourveau et al. 2018).

Specifically, to conduct the test for universal E-Verify, we create, for each year in which a universal E-Verify mandate was adopted, a cohort consisting of all plants located in states that adopted the mandate that year (treatment plants of the cohort) and all plants headquartered in states without universal E-Verify mandate (control plants of the cohort). We keep the observations for the five years before and after the adoption year. We also ensure that control plants are from states that do not have the mandate throughout the 11-year window. Next, we partition the sample for this cohort based on high vs. low *Unauthorized Workers* measured in the year before the adoption year. For example, the cohort for the 2011 adoption consists of all plants located in the states that passed the universal E-Verify mandate that year, namely, Alabama, Georgia, Louisiana, North Carolina, South Carolina, and Tennessee (treatment plants), and all plants located in states that do not have the universal E-Verify mandate in the 2006–2016 period (control plants). We keep all plant-years for the treatment and control plants for the period 2006–2016 as a cohort and partition the observations in each cohort into subsamples with high vs. low values of the variable *Unauthorized Workers* measured in 2010, based on the sample median.

We create subsamples for each adoption year using a similar approach and pool the subsamples of high (low) *Unauthorized Workers* in all cohorts to form the corresponding subsample for all adoption years. We estimate equation (1) for each subsample and expect $\gamma_1$ to be more positive for the subsample of high *Unauthorized Workers. Unauthorized Workers* is measured at the state level, so if a cohort only has one treatment state, one of the two subsamples for that cohort will not have a treatment plant. Such is the case for the 2007 (no treatment plants in the high group), 2008 (no treatment plants in the low group), and 2010 (no treatment plants in the low group) cohorts. We remove the three subsamples without treatment plants.[16] We utilize a similar approach for the cross-sectional test of public E-Verify mandates and expect $\gamma_2$ to be more positive for the subsample of high *Unauthorized Workers.*

We employ a similar approach to test the empirical prediction P4. We measure whether a plant's job is inherently hazardous using the workplace injury data from the Occupational Safety and Health Administration (OSHA) (e.g., Caskey and Ozel 2017). During the period 1996–2011, OSHA surveyed private-sector establishments to collect data on work-related acute illnesses and injuries that occurred in the previous year. As the OSHA data does not cover all facilities, we construct an industry-year level workplace-safety measure by taking the average of the total case rate (*TCR*)—the measure used by Caskey and Ozel (2017)—for all facilities in each 3-digit SIC industry for a given year. *TCR* is calculated as the number of cases of injuries and illnesses in an establishment in a given year divided by the number of hours worked by all employees in the establishment, multiplied by 200,000 (Caskey and Ozel 2017). A higher value of industry average *TCR* indicates lower workplace safety. Using the industry average *TCR* as a partition variable, we

[16] Our results are similar when these three subsamples are included.

form two subsamples based on the median of each cohort following the procedure described above. We expect $\gamma_1$ ($\gamma_2$) to be more positive for the subsample of plants with high industry average *TCR* when the cohorts are formed based on universal (public) E-Verify.

### 4.4 Empirical Results

#### *4.4.1 Baseline Results*

Table 4 reports the results of estimating equation (1). We report the combined effect of universal and public E-Verify mandates in columns 1 and 2 using different fixed effects structures. Column 1 presents the results of the basic model with only plant-chemical and chemical-year fixed effects. The coefficient of *EVerify* is positive and significant (0.059, *t*-statistic = 2.92). In column 2, we add industry-year fixed effects to the basic model in column 1. The coefficient of *EVerify* remains positive and significant (0.063, *t*-statistic = 3.22). As the dependent variable is *Ln(1+Toxic)*, these coefficient estimates suggest that the E-Verify mandate leads to a relative increase of toxic release of around 6.0%–6.6%.[17] These effects are economically meaningful and are consistent with our first empirical prediction (P1).

Columns 3 and 4 report the results of separately estimating the impacts of universal and public E-Verify mandates. In both models with different fixed effects structures, the coefficients of *EVerify_All* and *EVerify_Pub* are both positive and significant, consistent with P1. Moreover, the coefficient of *EVerify_All* is significantly larger than that of *EVerify_Pub* in both models, consistent with P2. The coefficient of *EVerify_All* ranges from 0.101 to 0.104, suggesting that the universal E-Verify mandate leads to an increase of toxic release of around 10.7%–11.1%. The

[17] We denote the toxic releases before and after the mandate with $y_1$ and $y_0$, respectively. Equation (1) implies that $\ln(1+ y_1)=\ln(1+ y_0)+\beta_1$. Thus, the relative increase of toxic release is $y_1/y_0-1= (1+1/y_0)\, e^{\beta_1} - 1/y_0 -1$. If $y_0$ is set to the sample median of 107, the relative increase is 6.0% when $\beta_1 = 0.058$ and 6.6% when $\beta_1 = 0.063$. We use a similar approach to calculate the economic effects for the subsequent tests.

coefficients of *EVerify_Pub*, 0.034 and 0.053, suggest that the public E-Verify mandate leads to an increase of toxic release of around 4.5%–5.4%. Overall, the results in Table 4 are consistent with our prediction that the passage of E-Verify mandates increases pollution and that the effect is stronger for universal E-Verify than for public E-Verify.

#### *4.4.2 Timing of the Effect*

We next explore the timing of the effects of universal and public E-Verify mandates to provide evidence on how the effect changes over time as well as on the validity of the parallel trends assumption. We conduct the analysis first for universal E-Verify. Specifically, we modify equation (1) by replacing *EVerify_All* with a series of indicator variables *EVerify_All(t)*, where *t* equals -4, -3, …, 3, 4, and 4+ (e.g., Autor 2003; Klasa et al. 2018; Huang et al. 2020). *EVerify_All(-4)* is an indicator variable equal to 1 if a plant's location state passes the universal E-Verify mandate in four years, and 0 otherwise. The other indicator variables are defined similarly, except that *EVerify_All(4+)* is an indicator variable equal to 1 if a plant is located in a state that adopted a universal E-Verify mandate more than four years ago.

Table 5, Panel A reports the results of this analysis. Column 1 presents the results for the base model with only plant-chemical and chemical-year fixed effects. The coefficients of the pre-adoption indicator variables *EVerify_All(-4)*, *EVerify_All(-3)*, *EVerify_All(-2)*, and *EVerify_All(-1)* are all insignificant. This finding suggests that, compared to the default years, toxic releases do not change significantly in years -4 to -1. These results support the parallel trends assumption. The coefficients of the post-adoption indicator variables are all positive, and they are significant for *EVerify_All(3)*, *EVerify_All(4)*, and *EVerify_All(4+)*. The coefficients monotonically increase over time, indicating that the effect becomes stronger over time. In column 2, we remove the pre-adoption indicator variables so that the coefficients of the post-adoption indicators can be interpreted as the treatment effect by years. We find that the coefficients exhibit a similar pattern as in column 1. Columns 3 and 4 report the results for models with industry-year fixed effects. The results are consistent with those in columns 1 and 2. Collectively, these results support the parallel trends assumption and indicate that the impact of universal E-Verify mandates is stronger in later years. The stronger effect over time is presumably due to the stricter enforcement of the mandates in later years.[18]

We perform a similar test for public E-Verify and report the results in Panel B of Table 5. For both models, the coefficients of the pre-adoption indicator variables are all insignificant, supporting the parallel trends assumption (columns 1 and 3). The results in columns 2 and 4 indicate that the effect takes place in the first year after the adoption. Unlike in the case of universal E-Verify, the effect of public E-Verify does not monotonically strengthen over time.

#### *4.4.3 Cross-Sectional Tests Based on the Fraction of Unauthorized Workers in the Labor Force*

We further examine how the effect of E-Verify mandates on toxic releases varies with the proportion of unauthorized workers in a state, using the cohort-based approach described in Section 4.3. Table 6 reports the results. Panel A reports the results for universal E-Verify. The subsample of high *Unauthorized Workers* includes the adoptions in 2007 by Arizona, in 2010 by Utah, and in 2011 by Georgia and North Carolina. The low *Unauthorized Workers* subsample includes the adoptions in 2008 by Mississippi and in 2011 by Alabama, Louisiana, South Carolina,

[18] According to everify.gov, the number of employers participating E-Verify monotonically increases from 2001 to 2017.

and Tennessee. The sample sizes of the high and low *Unauthorized Workers* groups are not balanced. This is primarily because (as we discuss in Section 4.3) a cohort that only has one treatment state will not have treatment plants for one of its two subsamples and thus that subsample must be removed.

Columns 1 and 2 present the results for the base model with plant-chemical and chemical-year fixed effects. In the subsample of high *Unauthorized Workers*, the coefficient of *EVerify_All* is positive and significant (0.130, *t*-statistic = 2.57, column 1). In contrast, the coefficient is insignificant in the subsample of low *Unauthorized Workers* (0.034, *t*-statistic = 0.94, column 2), and the difference in the two coefficients is significant (*p*-value = 0.061). Columns 3 and 4 report the results based on models with industry-year fixed effects in addition to the plant-chemical and chemical-year fixed effects. The results are consistent with those in columns 1 and 2. The coefficient of *EVerify_All* is 0.160 and significant (*t*-statistic = 2.49) in the high *Unauthorized Workers* subsample, which is significantly larger than in the low *Unauthorized Workers* subsample (0.016, *t*-statistic = 0.46). In states with a high fraction of unauthorized workers, chemical releases increase by 14.0%–17.5% after the passage of universal E-Verify mandate. These results support our prediction that the impact of universal E-Verify mandates is stronger in states with a higher fraction of unauthorized workers in the labor force (P3).[19]

We report the results for public E-Verify in Panel B of Table 6. The subsample of high *Unauthorized Workers* includes the adoptions in 2006 by Colorado, in 2007 by Oklahoma, in 2009 by Idaho and Nebraska, in 2010 by Virginia, in 2011 by Florida, and in 2014 by Texas. The low

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[19] We note that the coefficients of *EVerify_Pub* are also significantly more positive in the high *Unauthorized Workers* subsample in both models, suggesting that the impact of public E-Verify mandates is also stronger in states with a higher fraction of unauthorized workers in the labor force. However, in this analysis, the cohorts are formed based on the adoption of universal E-Verify, and *EVerify_Pub* is used as a control variable.

*Unauthorized Workers* subsample includes the adoptions in 2008 by Missouri, in 2011 by Indiana and Minnesota, and in 2012 by Michigan, Pennsylvania, and West Virginia. For the same reason we discuss for Panel A, the sample sizes of the high and low *Unauthorized Workers* groups are not balanced. In both models, the coefficient of *EVerify_Pub* is positive and significant in the high *Unauthorized Workers* group (columns 1 and 3) and insignificant in the low *Unauthorized Workers* group (columns 2 and 4). Moreover, the coefficient in the high group is significantly larger than the coefficient in the corresponding low group. In states with high fraction of unauthorized workers, chemical releases increase by 13.6%–14.5% after the adoption of public E-Verify mandates.[20] Taken together, the results in Table 6 support our third empirical prediction that the impact of E-Verify mandates is stronger in states with a higher fraction of unauthorized workers in the labor force.[21]

#### *4.4.4 Cross-Sectional Tests Based on Workplace Safety*

We next examine how the effect of E-Verify mandates on toxic releases varies with workplace safety (P4). Table 7 reports the results. Panel A reports the results for universal E-Verify. Columns 1 and 2 present the results for the model with plant-chemical and chemical-year fixed effects. In the subsample of plants with high workplace injuries and illness, the coefficient of *EVerify_All* is positive and significant (0.139, *t*-statistic = 3.36, column 1). In contrast, the coefficient is insignificant in the subsample of plants with low injuries and illnesses (-0.014, *t*-

[20] As in Panel A of Table 6, *EVerify_All* is a control variable in all regressions in Panel B, not a treatment variable, because in this analysis the cohorts are formed based on the adoption of public E-Verify.
[21] In an untabulated analysis, we conduct the same analysis for universal and public E-Verify combined (i.e., the variable *EVerify*). The results are qualitatively similar. For both models, the coefficients of *EVerify* are positive and significant in the high *Unauthorized Workers* group and insignificant in the low *Unauthorized Workers* group, and the differences are significant.

statistic = -0.30, column 2). The difference in the two coefficients is significant (*p*-value = 0.002). Columns 3 and 4 report the results based on models with industry-year fixed effects in addition to plant-chemical and chemical-year fixed effects. The results are consistent with those in columns 1 and 2. The coefficient of *EVerify_All* is 0.124 and significant (*t*-statistic = 3.43) in the high injuries and illnesses subsample, and is significantly larger than in the low injuries and illnesses subsample (-0.019, *t*-statistic = -0.42). These results support our prediction that the impact of universal E-Verify mandates is stronger in plants with inherently more hazardous jobs.

We report the results for public E-Verify in Panel B of Table 7. For both models, we find that the coefficient of *EVerify_Pub* is positive and significant in the high injuries and illnesses group (columns 1 and 3) and insignificant in the low injuries and illnesses group (columns 2 and 4). Moreover, the coefficient in the high group is significantly larger than the coefficient in the low group for the model with plant-chemical and chemical-year fixed effects (columns 1 and 2). Taken together, the results in Table 7 support our fourth empirical prediction that the impact of the E-Verify mandates on pollution is stronger in plants with inherently more hazardous job.[22]

### *4.4.5 Impact of E-Verify Mandates on Labor Violations*

Our empirical predictions are based on the premise that after the adoption of E-Verify mandates, employers reduce not only their use of unauthorized workers but also workplace abuses. Our cross-sectional analysis in Section 4.4.4 is consistent with this premise. To provide further direct evidence that E-Verify mandates reduce workplace abuses, we examine their impact on employers' labor violations. We use the data on facility-level workplace safety inspections and

[22] In an untabulated analysis, we conduct the same analysis for universal and public E-Verify combined (i.e., the variable *EVerify*). The results are qualitatively similar. For both models, the coefficients of *EVerify* are positive and significant in the high injuries and illnesses group and insignificant in the low injuries and illnesses group, and the differences are significant.

violations from the OSHA for this analysis.[23] OSHA inspects facilities regularly to ensure their compliance with OSHA requirements, prevent injuries, illness, and death in the workplace, and help workers and employers reduce on-the-job hazards.[24] After an inspection, if a violation is confirmed, OSHA issues a citation and notification of penalty, which provides detailed information about the nature of the violation as well as associated penalties. We use inspections conducted by OSHA from 2000 to 2018 to investigate the impact of E-Verify mandates on the likelihood of labor violations due to hazardous chemicals in the workplace (*HazardViol_Dummy*) and the likelihood of labor violations that lead to monetary fines (*Fines_Dummy*).

We regress *HazardViol_Dummy* and *Fines_Dummy* on *EVerify_All* and *EVerify_Pub*, controlling for establishment and industry-year fixed effects. The results are reported in columns 1 and 3 of Table 8. The coefficients of *EVerify_All* are negative and significant in both columns, indicating that labor violations decrease after the adoption of universal E-Verify mandates. The adoption of a universal E-Verify mandate reduces the likelihood of violations due to hazardous chemicals by 1.0 percentage points (column 1) and the likelihood of being fined for labor violations by 5.9 percentage points (column 3). These effects are economically meaningful, compared to the average likelihood of such violations, which is 2% and 59%, respectively. The coefficients of *EVerify_Pub* are also negative, but they are insignificant and are significantly smaller in magnitude than those of *EVerify_All.* To provide evidence for the parallel trends assumption for universal E-Verify, we replace *EVerify_All* with a set of time indicators, as in Section 4.4.2. The results, which

[23] We do not use the workplace injuries and illness data (as in Section 4.4.4) for this analysis because the OSHA survey stopped in 2011, resulting in very limited number of observations for the post-adoption period. Despite this data limitation, we still find evidence that cases of injuries and illnesses decrease after the adoption of both universal and public E-verify (untabulated).

[24] OSHA prioritizes inspections related to immediate danger situations, worker complaints, severe injuries, referrals, follow-up inspections, and targeted inspections.

are reported in columns 2 and 4, indicate that the assumption holds for the models in both columns 1 and 2. Overall, the results of Table 8 confirm that employers reduce workplace abuses after the adoption of E-Verify mandates.

### *4.4.6 Channel Analyses*

After establishing that E-Verify mandates increase toxic releases, we explore three possible channels of the effect: i) reducing pollution abatement actions; ii) increasing production; and iii) reducing toxic emission efficiency (i.e., releasing more toxics given the same production level) (Akey and Appel 2019). To explore the three channels, we use the following approach (e.g., Chen et al. 2019). Because pollution abatement actions and production are observable and measurable using the TRI data, we first examine whether they change after the adoption of E-Verify mandates. We do so by using the natural logarithm of one plus the number of abatement actions (*Abatement*, *Abatement_Op*, and *Abatement_Pro*) or normalized production (*Ln(Normalized_Prod)*) as the dependent variable and estimating equation (1). If the coefficient of *EVerify_All* (*EVerify_Pub*) is significant, we conclude that reducing abatement actions or increasing production is a channel. Next, as emission efficiency is unobservable, we examine this potential channel indirectly. Specifically, if neither reduced abatement nor increased production is a channel, we conclude that reduced emission efficiency is the major channel. If reduced abatement, increased production, or both are channels, we estimate equation (1) after including the abatement measure, normalized production, or both as additional control variables. A significantly positive coefficient of *EVerify_All* (*EVerify_Pub*) would suggest that reduced emission efficiency is a channel.

Table 9, Panel A reports the results of estimating the effect of E-Verify mandates on the number of pollution abatement actions. Columns 1 and 2 present the results for *Abatement* as the dependent variable. The coefficients of *EVerify_All* and *EVerify_Pub* are insignificant in both models, indicating that pollution abatement activities do not change after the passage of universal or public E-Verify mandates. When we separately investigate operation-related (columns 3 and 4) and process-related (columns 5 and 6) pollution abatement actions, we find similar results. Thus, there is no significant evidence that pollution abatement actions change after the adoption of E-Verify mandates. This suggests that reducing abatement actions is not a channel.

Columns 1 and 2 of Table 9, Panel B report the results of exploring increased production as a possible channel using *Ln(Normalized_Prod)* as the dependent variable. We find that the coefficients of *EVerify_All* and *EVerify_Pub* are positive in both models, and they are significant in the model with industry-year fixed effects (column 2). The coefficients of *EVerify_All* are slightly larger than those of *EVerify_Pub*. Thus, there is modest evidence that production increases after the passage of universal or public E-Verify mandates. The increase could occur because plants adopt lower environmental standards following the mandate, which would decrease their production costs related to pollution reduction.

Given that we find a possible production increase after the E-Verify mandates, we next explore whether reduced emission efficiency is also a possible channel. We do this by estimating equation (1) after controlling for *Ln(Normalized_Prod)*. The results are reported in columns 3 and 4 of Table 9, Panel B. We find that the coefficients of *EVerify_All* and *EVerify_Pub* remain positive and significant and are slightly smaller in magnitude than the corresponding ones in Table 4. We interpret these results as suggestive evidence that reduced emission efficiency is also a channel of the increased pollution we document.[25] We view the evidence as suggestive only because we

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[25] As expected, the coefficients of *Ln(Normalized_Prod)* are strongly positive, suggesting that toxic releases increase with the production level.

cannot fully control for the effect of production, due to possible model misspecification. We address this concern by including higher order (N) polynomials of *Ln(Normalized_Prod)* to account for nonlinearity. We find similar results when setting N to 2 and 3. These robustness results increase our confidence that reduced emission efficiency is a possible channel.

### *4.5 Addressing Alternative Explanations*

Recent studies show that corporate environmental policies could be influenced by firms' financial conditions. In particular, Xu and Kim (2022) documents that financial constraints increase firms' toxic emissions; Thomas et al. (2022) and Liu et al. (2021) find that firms could increase pollution in order to meet or beat earnings benchmarks. In this subsection, we address the concern that our findings could be due to E-Verify mandates increasing the possibility of financial constraints or the need to meet or beat earnings benchmarks. We do so by exploring whether the mandates lead to greater financial constraints or higher incidences of benchmark meeting/beating for a plant's parent firm.

We construct comprehensive financial constraint measures based on prior studies by Whited and Wu (2006) (*WW Index*), Kaplan and Zingales (1997) (*KZ Index*), and Hadlock and Pierce (2010) (*SA Index*). We also follow the prior literature and consider managers' incentives to meet or beat three earnings benchmarks: consensus analyst forecast, zero threshold, and the prior year's earnings (e.g., Cohen et al. 2008; Doyle et al. 2013; Thomas et al. 2022). We conduct the analyses at the plant-year level, but measure firm variables at a plant's parent firm level.[26] The results are reported in Table 10. Each regression includes plant-parent and industry-year fixed effects. The

[26] As the purpose of this analysis is to rule out the alternative explanations for the plant-chemical-year level analysis in Table 4, we conduct this analysis at the plant-year level, measuring *EVerify_All* and *EVerify_Pub* using a plant's location state. A parent firm could appear multiple times in the sample if it is the parent of multiple plants.

control variables for the analysis of financial constraints (columns 1-3) follow Klasa et al (2018); those for meeting/beating incentives follow Doyle et al. (2013). All control variables are defined in Appendix A. The coefficients of *EVerify_All* and *EVerify_Pub* are insignificant in all columns.[27] These results suggest that the E-Verify mandates does not lead to a significant increase in either financial constraints or incentives to meet or beat earnings benchmarks for a plant's parent firm. This finding helps rule out the alternative explanation that our main results might be driven by changes of financial conditions following the mandates.

### *4.6 Robustness Tests*

Barrios (2021) and Baker et al. (2021) argue that a regular generalized DiD design for staggered events can lead to biased estimates of the treatment effect because already-treated units act as comparison units for later-treated units. Because changes in the outcomes of earlier-treated firms are subtracted from the changes of later-treated units, the design is likely to be biased in the presence of treatment-effect heterogeneity. One solution they propose is to use the stacked-regression approach in Cengiz et al. (2019). The basic idea is to create event-specific cohorts using "clean controls"—control firms that are not affected by previous treatment—and then stack these cohorts together to estimate an average treatment effect across all cohorts, allowing the unit and time fixed effects to change with the cohort. This method is similar to the cohort-based approach we use in the cross-sectional analysis, except that we do not allow the fixed effects to differ across cohorts. Thus, we conduct a set of robustness analyses using the stacked regression approach (e.g., Dey et al. 2021).

[27] The results are similar when the control variables are removed, except that the coefficients become significantly negative for meeting/beating the zero benchmark.

We first form cohorts based on the adoption of universal E-Verify following the procedure described in Section 4.3, then pool all cohorts together to form the cohort-based sample. We then estimate equation (1), interacting all fixed effects with the cohort indicators. The results are reported in columns 1 and 2 of Table 10, Panel A. As the cohorts are formed based on universal E-Verify adoptions, we focus on the coefficient of *Everify_All*. In both models, the coefficient is positive and significant and slightly smaller in magnitude than the corresponding one in Table 4. We conduct similar analyses using cohorts formed based on the adoption of public E-Verify and report the results in columns 3 and 4. We find that the coefficient of *Everify_Pub* is positive and significant in both models and slightly larger in magnitude than the corresponding one in Table 4. Thus, our main findings are robust to using the stacked-regression approach.

We also explore whether our results in Table 4 are sensitive to including additional fixed effects and state level macroeconomic variables. Specifically, we add parent-year fixed effects to columns 2 and 4 of Table 4 to further control for time-varying heterogeneity at the parent firm level. The results in columns 1 and 3 of Table 11, Panel B indicate that the coefficients of *EVerify*, *Everify_All*, and *Everify_Pub* remain similar as in Table 4. On top of parent-year fixed effects, we further control for state unemployment rate (*Unemployment*), population growth rate (*Population Growth*), GDP growth rate (*GDP Growth*), political balance (*Political Balance*), and a measure of state level politicians' environmental preference (*LCV Score*).[28] *Political Balance* is the fraction of a state's Congress members representing the state in the U.S. House of Representatives that belong to the Democratic Party (Klasa et al. 2018). Columns 2 and 4 of Table 11, Panel B report that the results remain very similar. Thus, our main findings are robust to these additional controls.

[28] *LCV Score* is the average score of a state's Senate and House members voting for important environmental legislation. The data is available at League of Conservation Voters (LCV)'s website (https://scorecard.lcv.org/).

## 5. Conclusion

Based on the strategic view of corporate social responsibility (CSR), we argue that substitution exists in firms' production of social bads (and goods). Substitution may occur because socially responsible stakeholders view different socially responsible behaviors as substitutes and because firms' provision of CSR is subject to resource constraint. The substitution effect implies that exogenously reducing production of one social bad could lead to an increase in the production of another social bad. To provide evidence on this proposition, we use the U.S. states' staggered adoptions of E-Verify mandates, which require all or some employers to verify the work authorization of new hires and thus reduce the hiring of unauthorized workers and related workplace abuses. Although these mandates target and curb the hiring of unauthorized workers and related workplace abuses (one social bad), we use facility-level data to assess their impact on pollution (another social bad).

We find that the adoption of E-Verify mandates significantly increases toxic releases, by 6%–7%. The effect is stronger for universal E-Verify mandates, which require all employers to use E-Verify, than for public E-Verify mandates, which apply only to certain public employers or public contractors. The effect of E-Verify mandates, both universal and public, is stronger for plants located in states with a higher proportion of unauthorized workers in the labor force in the year before adoption and for plants with inherently more hazardous jobs. We also find that the mandates reduce employers' labor violations, confirming the presumption that a reduction in unauthorized workers is accompanied by reduced workplace abuses. Further analyses suggest that the increase in pollution is mainly due to reduced toxic emission efficiency, and may be due to increased production.

Our study contributes to the literature of CSR by showing a substitution between the equilibrium levels of different CSR activities. Our finding highlights a hidden cost to increasing CSR through regulation: a mandated increase in one CSR activity may lead to a reduction in another. We also contribute to research on the determinants of firms' environmental behaviors by showing that mandated employment verification through E-Verify can increase pollution. One limitation of our study is that we show only how reducing the use of unauthorized workers and the related workplace abuses substitutes for pollution reduction. Future studies can explore substitutions among other CSR activities.

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## Appendix A: Variable Definitions

| Variable | Definition |
|---|---|
| *Abatement* | The number of pollution abatement activities in a year. |
| *Abatement_Op* | The number of operation-related pollution abatement activities in a year. |
| *Abatement_Pro* | The number of process-related pollution abatement activities in a year. |
| *Dividend* | An indicator variable equal to one if a firm pays common dividends, and zero otherwise. |
| *Earn Vol* | Standard deviation of a firm's *ROA* over the previous five years. |
| *EVerify* | An indicator variable that equals 1 if a plant's location state has passed a universal or public E-Verify mandate in the year, and 0 otherwise. |
| *EVerify_All* | An indicator variable that equals 1 if a plant's location state has passed a universal E-Verify mandate in the year, and 0 otherwise. |
| *EVerify_Pub* | An indicator variable that equals 1 if a plant's location state has passed a public E-Verify mandate in the year, and 0 otherwise. |
| *Fines_Dummy* | An indicator variable equal to 1 if an OSHA inspection leads to monetary fines, and 0 otherwise. |
| *Fixed Assets* | Net value of property, plant and equipment divided by total assets. |
| *GDP Growth* | GDP growth rate in percent for a state during a year. |
| *HazardViol_Dummy* | An indicator variable equal to 1 if an OSHA inspection leads to labor violations related to hazardous chemicals in the workplace, and 0 otherwise. |
| *LCV Score* | The average score of a state's Congress members' voting for environmental legislation during a year. |
| *Ln(1+Abatement)* | The natural logarithm of one plus *Abatement*. |
| *Ln(1+Abatement_Op)* | The natural logarithm of one plus *Abatement_Op*. |
| *Ln(1+Abatement_Pro)* | The natural logarithm of one plus *Abatement_Pro* |
| *Ln(1+Toxic)* | The natural logarithm of one plus *Toxic*. |
| *Ln(Assets)* | The natural logarithm of total assets. |
| *Ln(Normalized_Prod)* | The natural logarithm of *Normalized_Prod*. |
| *Loss* | An indicator variable equal to one for negative income before extraordinary items, and zero otherwise. |
| *MTB* | Market capitalization divided by book value of equity. |
| *Normalized_Prod* | The measure of normalized production from Akey and Appel (2019), which is defined as the production amount in year *t* scaled by the amount in the first year that a chemical is reported in the TRI database. Specifically, it is calculated as the product of all historical *Production_Ratio. Production_Ratio* is set to one if it is missing from the TRI database. |
| *Political Balance* | The fraction of a state's congress members representing their state in the U.S. House of Representatives that belong to the Democratic Party. |

| | |
|---|---|
| *Population Growth* | Change of population of year *t* relative to year *t-1* in percentage for a state. |
| *Production_Ratio* | The production ratio for each chemical provided in the TRI dataset. Specifically, if a chemical is used to produce product A, *Production_Ratio* is calculated as the amount of product A produced in year *t* divided by the amount in year *t-1*. |
| *ROA* | Income before extraordinary items divided by total assets. |
| *Sales Growth* | The change of total revenue of year *t* relative to that of year *t-1*. |
| *Toxic* | The amount of toxic release (in pounds) in a year for a plant-chemical. |
| *Unemployment* | The average monthly unemployment rate in percent for a state during a year. |

**Table 1 E-Verify Mandate Adoption by States**

| State | Passage Date | Effective Date | Employers Affected |
|---|---|---|---|
| Alabama | 2011-06-09 | 2011-09-01 | All employers |
| Arizona | 2007-07-02 | 2008-01-01 | All employers |
| Colorado | 2006-06-06 | 2006-08-07 | State agency, political subdivisions, and state contractors |
| Florida | 2011-01-04 | 2011-05-27 | State agencies and state contractors and subcontractors |
| Georgia | 2011-04-14 | 2011-07-01 | All employers |
| Idaho | 2009-05-29 | 2009-07-01 | State/local agencies, political subdivisions, and public contractors and subcontractors |
| Indiana | 2011-05-10 | 2011-07-01 | State/local agencies, public contractors, and private employers deducting employee wages from their state income taxes |
| Louisiana | 2011-07-01 | 2011-08-15 | All employers |
| Michigan | 2012-05-31 | 2012-06-26 | Contractors and subcontractors of Department of Human Services and Department of Transportation |
| Minnesota | 2011-07-21 | 2011-07-22 | State contractors and subcontractors with contracts in excess of $50,000 |
| Missouri | 2008-07-07 | 2009-01-01 | Public employers, public contractors and subcontractors, and any business with a state contract or grant |
| Mississippi | 2008-03-18 | 2008-07-01 | All employers |
| North Carolina | 2011-06-23 | 2011-10-01 | All employers |
| Nebraska | 2009-04-08 | 2009-10-01 | Public employers, public contractors and subcontractor, and businesses qualifying for state tax incentive |
| Oklahoma | 2007-05-08 | 2007-11-01 | Public employers, public contractors and subcontractors, and private employers with government contracts |
| Pennsylvania | 2012-07-05 | 2013-01-01 | Public works contractors and subcontractors |
| South Carolina | 2011-06-27 | 2012-01-01 | All employers |
| Tennessee | 2011-06-07 | 2012-01-01 | All employers |
| Texas | 2014-12-03 | 2015-02-19 | State agencies |
| Utah | 2010-03-31 | 2010-07-01 | All employers |
| Virginia | 2010-04-11 | 2010-12-01 | State agencies and public contractors and subcontractors |
| West Virginia | 2012-03-10 | 2012-06-10 | Public employers and contractors |

This table reports states that have passed E-Verify mandates and the corresponding passage and effective dates. The information is mainly sourced from https://www.ncsl.org/research/immigration/state-e-verify-action.aspx.

**Table 2 Industry Distribution**

| Industry | Plant-Chemicals | | Plants | | Average Toxic Release (pounds) | |
|---|---|---|---|---|---|---|
| | N | % | N | % | Plant-Chemical | Plant |
| Agriculture, Forestry, Fishing and Hunting (11) | 792 | 0.06 | 103 | 0.26 | 49,100 | 377,545 |
| Mining, Quarrying, and Oil and Gas Extraction (21) | 17,367 | 1.26 | 495 | 1.25 | 114,097 | 4,003,073 |
| Utilities (22) | 100,455 | 7.30 | 884 | 2.23 | 64,650 | 7,346,643 |
| Construction (23) | 265 | 0.02 | 37 | 0.09 | 6,794 | 48,657 |
| Manufacturing (31-33) | 1,114,923 | 81.06 | 35,591 | 89.70 | 12,967 | 406,216 |
| Wholesale Trade (42) | 91,652 | 6.66 | 1,859 | 4.69 | 827 | 40,752 |
| Retail Trade (44-45) | 244 | 0.02 | 36 | 0.09 | 1,212 | 8,217 |
| Transportation and Warehousing (48-49) | 1,411 | 0.10 | 106 | 0.27 | 3,293 | 43,837 |
| Information (51) | 158 | 0.01 | 13 | 0.03 | 2,367 | 28,766 |
| Real Estate and Rental and Leasing (53) | 11 | 0.00 | 3 | 0.01 | 880 | 3,225 |
| Professional, Scientific, and Technical Services (54) | 820 | 0.06 | 51 | 0.13 | 894 | 14,375 |
| Management of Companies and Enterprises (55) | 35 | 0.00 | 4 | 0.01 | 89 | 775 |
| Administrative and Support and Waste Management and Remediation Services (56) | 46,896 | 3.41 | 422 | 1.06 | 24,577 | 2,731,160 |
| Educational Services (61) | 24 | 0.00 | 5 | 0.01 | 110 | 526 |
| Arts, Entertainment, and Recreation (71) | 7 | 0.00 | 3 | 0.01 | 16,425 | 38,325 |
| Other services (81) | 355 | 0.03 | 67 | 0.17 | 7,972 | 42,241 |
| Total | 1,375,415 | 100 | 39,679 | 100 | | |

This table reports the industry (2-digit NAICS) distribution for the sample. The sample period is from 2000 to 2018. The passage years of E-Verify are excluded.

**Table 3 Summary Statistics**

| | N | Mean | Q1 | Median | Q3 | STD |
|---|---|---|---|---|---|---|
| *EVerify* | 1,375,415 | 0.22 | 0.00 | 0.00 | 0.00 | 0.41 |
| *EVerify_All* | 1,375,415 | 0.08 | 0.00 | 0.00 | 0.00 | 0.27 |
| *EVerify_Pub* | 1,375,415 | 0.14 | 0.00 | 0.00 | 0.00 | 0.35 |
| *Toxic* | 1,375,415 | 17,603 | 0.44 | 107 | 2,619 | 72,987 |
| *Ln(1+Toxic)* | 1,375,415 | 4.70 | 0.36 | 4.68 | 7.87 | 3.93 |
| *Abatement* | 1,375,415 | 0.26 | 0.00 | 0.00 | 0.00 | 0.92 |
| *Abatement_Op* | 1,375,415 | 0.17 | 0.00 | 0.00 | 0.00 | 0.62 |
| *Abatement_Pro* | 1,375,415 | 0.03 | 0.00 | 0.00 | 0.00 | 0.17 |
| *Production_Ratio* | 1,354,115 | 0.98 | 0.89 | 1.00 | 1.09 | 0.36 |
| *Normalized_Prod* | 1,349,842 | 117.89 | 0.83 | 1.11 | 1.99 | 975.87 |
| *Ln(Normalized_Prod)* | 1,349,842 | 0.42 | -0.18 | 0.10 | 0.69 | 1.75 |

This table reports summary statistics for the sample of observations at the plant-chemical level. The sample period is from 2000 to 2018. The passage years of E-Verify are excluded. Variable definitions are provided in Appendix A.

**Table 4 The Effect of E-Verify Mandates on Toxic Releases**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *EVerify* | 0.058*** | 0.063*** | | |
| | (2.92) | (3.22) | | |
| *EVerify_All* | | | 0.104*** | 0.101*** |
| | | | (2.67) | (2.74) |
| *EVerify_Pub* | | | 0.044** | 0.052*** |
| | | | (2.54) | (2.91) |
| *p*-Value for *EVerify_All* > *EVerify_Pub* | | | 0.061 | 0.082 |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | Yes | No | Yes |
| Adj R-squared | 0.922 | 0.923 | 0.922 | 0.923 |
| Observations | 1,375,415 | 1,375,415 | 1,375,415 | 1,375,415 |

This table reports the OLS regression results for the effect of E-Verify mandates on the amount of toxic release at the plant-chemical level. The sample period is from 2000 to 2018. The dependent variable *Ln(1+Toxic)* is the natural logarithm of one plus the release amount for a chemical (in pounds). *EVerify* is an indicator variable the equals 1 if a plant's location state has adopted any E-Verify mandate in the year, and 0 otherwise. *EVerify_All* (*EVerify_Pub*) is an indicator variable the equals 1 if a plant's location state has adopted a universal (public) E-Verify mandate in the year, and 0 otherwise. The adoption years of E-Verify are excluded. Robust standard errors are clustered by state. The reported numbers are coefficient estimates and *t*-statistics (in parentheses). Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

**Table 5 The Timing of the Effect of E-Verify Mandates on Toxic Releases**

**Panel A: Universal E-Verify**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *EVerify_All(-4)* | -0.002 | | 0.003 | |
| | (-0.08) | | (0.14) | |
| *EVerify_All(-3)* | 0.012 | | 0.017 | |
| | (0.33) | | (0.55) | |
| *EVerify_All(-2)* | 0.021 | | 0.023 | |
| | (0.40) | | (0.53) | |
| *EVerify_All(-1)* | 0.039 | | 0.045 | |
| | (0.83) | | (1.12) | |
| *EVerify_All(1)* | 0.062 | 0.054 | 0.066 | 0.055 |
| | (1.08) | (1.36) | (1.34) | (1.54) |
| *EVerify_All(2)* | 0.079 | 0.071 | 0.078 | 0.067 |
| | (1.23) | (1.47) | (1.42) | (1.55) |
| *EVerify_All(3)* | 0.105* | 0.098** | 0.110** | 0.100** |
| | (1.83) | (2.24) | (2.22) | (2.47) |
| *EVerify_All(4)* | 0.117** | 0.109*** | 0.119** | 0.109*** |
| | (2.19) | (2.78) | (2.59) | (2.96) |
| *EVerify_All(4+)* | 0.149*** | 0.142*** | 0.144*** | 0.134*** |
| | (3.05) | (3.63) | (3.26) | (3.33) |
| *EVerify_Pub* | 0.045** | 0.047*** | 0.052*** | 0.055*** |
| | (2.42) | (2.69) | (2.85) | (3.08) |
| | | | | |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | No | Yes | Yes |
| | | | | |
| Adj R-squared | 0.922 | 0.922 | 0.923 | 0.923 |
| Observations | 1,375,415 | 1,375,415 | 1,375,415 | 1,375,415 |

**Panel B: Public E-Verify**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *EVerify_Pub(-4)* | -0.002 | | -0.003 | |
| | (-0.12) | | (-0.21) | |
| *EVerify_Pub(-3)* | 0.021 | | 0.031 | |
| | (0.65) | | (1.13) | |
| *EVerify_Pub(-2)* | 0.026 | | 0.032 | |
| | (0.61) | | (0.91) | |
| *EVerify_Pub(-1)* | 0.047 | | 0.057 | |
| | (1.24) | | (1.63) | |
| *EVerify_Pub(1)* | 0.047 | 0.032* | 0.058* | 0.038** |
| | (1.36) | (1.75) | (1.86) | (2.17) |
| *EVerify_Pub(2)* | 0.052 | 0.037 | 0.064* | 0.045** |
| | (1.39) | (1.65) | (1.93) | (2.16) |
| *EVerify_Pub(3)* | 0.061 | 0.046* | 0.073** | 0.055** |
| | (1.49) | (1.68) | (2.03) | (2.19) |
| *EVerify_Pub(4)* | 0.056 | 0.042 | 0.070** | 0.052* |
| | (1.50) | (1.52) | (2.10) | (1.97) |
| *EVerify_Pub(4+)* | 0.053 | 0.038 | 0.072* | 0.053 |
| | (1.36) | (1.18) | (1.89) | (1.64) |
| *EVerify_All* | 0.096** | 0.091** | 0.093** | 0.086** |
| | (2.48) | (2.24) | (2.46) | (2.24) |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | No | Yes | Yes |
| Adj R-squared | 0.922 | 0.922 | 0.923 | 0.923 |
| Observations | 1,375,415 | 1,375,415 | 1,375,415 | 1,375,415 |

This table reports the OLS regression results for the timing of the effect of E-Verify mandates on the amount of toxic release at the plant-chemical level. The sample period is from 2001 to 2018. The dependent variable *Ln(1+Toxic)* is the natural logarithm of one plus the release amount for a chemical (in pounds). Panels A and B present the results for universal and public E-Verify mandates, respectively. *EVerify_All(-4)* is an indicator variable the equals 1 if a plant's location state will pass a universal E-Verify mandate in four years, and 0 otherwise. *EVerify_All(-3)* to *EVerify_All(4)* are defined similarly. *EVerify_All(4+)* is an indicator variable the equals 1 if a plant's location state passed a universal E-Verify mandate more than four years ago, and 0 otherwise. The indicator variables for public E-Verify in Panel B are defined similarly. *EVerify_All* (*EVerify_Pub*) is an indicator variable the equals 1 if a plant's location state has adopted a universal (public) E-Verify mandate in the year, and 0 otherwise. The adoption years of E-Verify are excluded for both universal and public E-Verify. Robust standard errors are clustered by state. The reported numbers are coefficient estimates and *t*-statistics (in parentheses). Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

**Table 6 Cross-Sectional Tests Based on the Fraction of Unauthorized Workers in the Labor Force**

**Panel A: Universal E-Verify**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | Fraction of Unauthorized Workers | | | |
| | High | Low | High | Low |
| | (1) | (2) | (3) | (4) |
| ***EVerify_All*** | **0.130**** | **0.034** | **0.160**** | **0.016** |
| | **(2.57)** | **(0.94)** | **(2.49)** | **(0.46)** |
| *EVerify_Pub* | 0.080*** | -0.010 | 0.088*** | -0.012 |
| | (4.57) | (-0.57) | (4.49) | (-0.73) |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | No | Yes | Yes |
| *p*-value for *EVerify_All*: High > Low | 0.061 | | 0.025 | |
| *p*-value for *EVerify_Pub:* High > Low | 0.001 | | 0.000 | |
| Adj R-squared | 0.937 | 0.944 | 0.938 | 0.945 |
| Observations | 961,785 | 671,452 | 961,665 | 671,245 |

**Panel B: Public E-Verify**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | Fraction of Unauthorized Workers | | | |
| | High | Low | High | Low |
| | (1) | (2) | (3) | (4) |
| ***EVerify_Pub*** | **0.126***** | **-0.006** | **0.134***** | **-0.007** |
| | **(6.11)** | **(-0.25)** | **(5.61)** | **(-0.30)** |
| *EVerify_All* | 0.029 | 0.103** | 0.058 | 0.080* |
| | (1.18) | (2.23) | (1.04) | (1.78) |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | No | Yes | Yes |
| *p*-value for *EVerify_Pub*: High > Low | 0.000 | | 0.000 | |
| *p*-value for *EVerify_All:* High > Low | 0.081 | | 0.240 | |
| Adj R-squared | 0.934 | 0.945 | 0.936 | 0.947 |
| Observations | 1,442,662 | 740,607 | 1,442,573 | 740,432 |

This table reports the OLS regression results for examining how the effect of E-Verify mandates on the amount of toxic release at the plant-chemical level varies with the fraction of unauthorized workers in the state in the year prior to the passage year of E-Verify mandate. The sample period is from 2002 to 2016 for

Panel A and from 2001 to 2018 for Panel B. We use the cohort-based approach described in Section 4.3. Panels A and B present the results for universal and public E-Verify mandates, respectively. The dependent variable *Ln(1+Toxic)* is the natural logarithm of one plus the release amount for a chemical (in pounds). *EVerify_All* (*EVerify_Pub*) is an indicator variable the equals 1 if a plant's location state has adopted a universal (public) E-Verify mandate in the year, and 0 otherwise. The passage years of E-Verify are excluded. Robust standard errors are clustered by state. The reported numbers are coefficient estimates and *t*-statistics (in parentheses). Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

**Table 7 Cross-Sectional Tests Based on Workplace Safety**

**Panel A: Universal E-Verify**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | Workplace Injuries and Illnesses | | | |
| | High | Low | High | Low |
| | (1) | (2) | (3) | (4) |
| ***EVerify_All*** | **0.139*****| **-0.014** | **0.124*****| **-0.019** |
| | **(3.36)** | **(-0.30)** | **(3.43)** | **(-0.42)** |
| *EVerify_Pub* | 0.024 | 0.034* | 0.026 | 0.038** |
| | (1.07) | (1.94) | (1.12) | (2.29) |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | No | Yes | Yes |
| *p*-value for *EVerify_All*: High > Low | 0.002 | | 0.001 | |
| *p*-value for *EVerify_Pub:* High > Low | 0.358 | | 0.294 | |
| Adj R-squared | 0.944 | 0.939 | 0.945 | 0.940 |
| Observations | 1,167,802 | 1,240,643 | 1,167,706 | 1,240,530 |

**Panel B: Public E-Verify**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | Workplace Injuries and Illnesses | | | |
| | High | Low | High | Low |
| | (1) | (2) | (3) | (4) |
| ***EVerify_Pub*** | **0.068**** | **-0.009** | **0.055*** | **0.017** |
| | **(2.30)** | **(-0.31)** | **(1.93)** | **(0.58)** |
| *EVerify_All* | 0.061* | 0.108* | 0.055 | 0.090* |
| | (1.73) | (1.97) | (1.42) | (1.98) |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | No | Yes | Yes |
| *p*-value for *EVerify_Pub*: High > Low | 0.022 | | 0.139 | |
| *p*-value for *EVerify_All:* High > Low | 0.108 | | 0.156 | |
| Adj R-squared | 0.940 | 0.943 | 0.942 | 0.943 |
| Observations | 1,593,956 | 16,864,74 | 1,593,869 | 1,686,375 |

This table reports the OLS regression results for examining how the effect of E-Verify mandates on the amount of toxic release at the plant-chemical level varies with workplace safety. The sample period is from 2002 to 2016 for Panel A and from 2001 to 2018 for Panel B. We use a cohort-based approach described in Section 4.3. Panels A and B present the results for universal and public E-Verify mandates, respectively. The dependent variable *Ln(1+Toxic)* is the natural logarithm of one plus the release amount for a chemical (in pounds). *EVerify_All* (*EVerify_Pub*) is an indicator variable the equals 1 if a plant's location state has adopted a universal (public) E-Verify mandate in the year, and 0 otherwise. The passage years of E-Verify are excluded. Robust standard errors are clustered by state. The reported numbers are coefficient estimates and *t*-statistics (in parentheses). Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

**Table 8 The Effect of E-Verify Mandates on Labor Violations**

| | *HazardViol_Dummy* | | *Fines_Dummy* | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *EVerify_All* | -0.010** | | -0.059*** | |
| | (-2.21) | | (-2.93) | |
| *EVerify_Pub* | -0.004 | -0.004 | -0.027 | -0.022 |
| | (-1.22) | (-1.58) | (-1.32) | (-1.09) |
| *EVerify_All(-4)* | | 0.005 | | -0.011 |
| | | (1.28) | | (-0.50) |
| *EVerify_All(-3)* | | 0.003 | | -0.010 |
| | | (0.67) | | (-0.49) |
| *EVerify_All(-2)* | | 0.001 | | -0.015 |
| | | (0.29) | | (-0.61) |
| *EVerify_All(-1)* | | -0.001 | | -0.025 |
| | | (-0.24) | | (-1.00) |
| *EVerify_All(1)* | | -0.007 | | -0.030 |
| | | (-1.05) | | (-0.95) |
| *EVerify_All(2)* | | -0.010** | | -0.085*** |
| | | (-2.37) | | (-3.09) |
| *EVerify_All(3)* | | -0.010* | | -0.080*** |
| | | (-2.00) | | (-4.08) |
| *EVerify_All(4)* | | -0.011* | | -0.067** |
| | | (-1.79) | | (-2.13) |
| *EVerify_All(4+)* | | -0.011** | | -0.056 |
| | | (-2.37) | | (-1.62) |
| *p*-Value for *EVerify_All* > *EVerify_Pub* | 0.067 | | 0.050 | |
| Establishment FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes |
| Adj R-squared | 0.127 | 0.127 | 0.182 | 0.182 |
| Observations | 704,779 | 704,779 | 704,779 | 704,779 |

This table reports the OLS regression results for the effect of E-Verify mandates on labor violations. The sample period is from 2000 to 2018. *HazardViol_Dummy* is an indicator variable equal to 1 if an inspection leads to violations related to hazardous chemicals in workplace. *Fines_Dummy* is an indicator variable equal to 1 if an inspection leads to monetary fines. *EVerify* is an indicator variable equal to 1 if a plant's location state has adopted any E-Verify mandate in the year, and 0 otherwise. *EVerify_All* (*EVerify_Pub*) is an indicator variable equal to 1 if a plant's location state has adopted a universal (public) E-Verify mandate in the year, and 0 otherwise. The adoption years of E-Verify are excluded. Robust standard errors are clustered by state. The reported numbers are estimated coefficient estimates and *t*-statistics (in parentheses). Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

**Table 9 Channel Analyses**

**Panel A: Pollution Abatement Activities**

| | *Ln(1+Abatement)* | | *Ln(1+Abatement_Op)* | | *Ln(1+Abatement_Pro)* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *EVerify_All* | 0.003 | 0.000 | 0.003 | 0.001 | 0.002 | 0.001 |
| | (0.36) | (0.05) | (0.39) | (0.08) | (0.61) | (0.54) |
| *EVerify_Pub* | 0.002 | 0.002 | 0.002 | 0.002 | -0.002 | -0.002 |
| | (0.27) | (0.28) | (0.31) | (0.34) | (-1.03) | (-1.09) |
| Plant-Chemical FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | Yes | No | Yes | No | Yes |
| Adj R-squared | 0.479 | 0.486 | 0.485 | 0.492 | 0.312 | 0.317 |
| Observations | 1,375,415 | 1,375,415 | 1,375,415 | 1,375,415 | 1,375,415 | 1,375,415 |

**Panel B: Production and Emission Efficiency**

| | *Ln(Normalized_Prod)* | | *Ln(1+Toxic)* | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *EVerify_All* | 0.046 | 0.048* | 0.092*** | 0.089*** |
| | (1.63) | (1.94) | (2.69) | (2.72) |
| *EVerify_Pub* | 0.032 | 0.037* | 0.035** | 0.043*** |
| | (1.49) | (1.85) | (2.35) | (2.76) |
| *Ln(Normalized_Prod)* | | | 0.210*** | 0.205*** |
| | | | (36.71) | (34.12) |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | No | Yes | No | Yes |
| Adj R-squared | 0.867 | 0.870 | 0.924 | 0.925 |
| Observations | 1,349,842 | 1,349,842 | 1,349,842 | 1,349,842 |

This table reports the OLS regression results for exploring the channels of the effect of E-Verify mandates on the amount of toxic release at the plant-chemical level. Panel A reports the effect of the mandates on pollution abatement activities (*Abatement*), operation-related abatement activities (*Abatement_Op*), and process-related abatement activities (*Abatement_Pro*). Columns 1 and 2 of Panel B report the effect of the mandates on production, measured with *Ln(Normalized_Prod)*. Columns 3 and 4 of Panel B report the effect of the mandates on chemical emission efficiency. The sample period is from 2000 to 2018. *EVerify_All* (*EVerify_Pub*) is an indicator variable that equals 1 if a plant's location state has adopted a universal (public) E-Verify mandate in the year, and 0 otherwise. The passage years of E-Verify are excluded. Robust standard errors are clustered by state. The reported numbers are coefficient estimates and *t*-statistics (in parentheses). Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

**Table 10 The Effect of E-Verify Mandates on Financial Constraints and Meet/Beat Incentives**

| | *WW Index* | *KZ Index* | *SA Index* | *Meet/Beat _Forecast* | *Meet/Beat _Zero* | *Meet/Beat _LastYear* |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *EVerify_All* | 0.001 | 0.008 | 0.004 | -0.001 | -0.006 | -0.002 |
| | (0.84) | (0.09) | (0.70) | (-0.16) | (-1.52) | (-0.28) |
| *EVerify_Pub* | 0.000 | -0.021 | -0.006 | 0.007 | -0.002 | 0.001 |
| | (0.28) | (-0.29) | (-1.60) | (1.17) | (-0.67) | (0.17) |
| *Ln(Assets)* | -0.023*** | 0.320*** | -0.016*** | 0.008*** | -0.013*** | 0.020*** |
| | (-47.27) | (7.85) | (-5.18) | (2.89) | (-5.71) | (6.57) |
| *MTB* | 0.001*** | -0.056*** | -0.000 | -0.001*** | 0.001*** | 0.000 |
| | (20.35) | (-7.79) | (-0.42) | (-3.07) | (3.80) | (1.16) |
| *ROA* | 0.048*** | -6.669*** | -0.015 | 0.317*** | -0.498*** | -0.045** |
| | (14.50) | (-15.33) | (-0.44) | (6.41) | (-14.85) | (-2.05) |
| *Fixed Assets* | 0.017*** | 12.758*** | 0.171*** | | | |
| | (5.31) | (20.85) | (7.21) | | | |
| *Earn Vol* | 0.026*** | -5.807*** | 0.128*** | | | |
| | (6.46) | (-6.21) | (4.50) | | | |
| *Dividend* | -0.063*** | -3.133*** | -0.021*** | | | |
| | (-116.97) | (-16.87) | (-10.58) | | | |
| *Unemployment* | 0.000 | -0.009 | 0.001 | -0.002 | 0.000 | 0.001 |
| | (0.58) | (-0.47) | (1.66) | (-1.23) | (0.10) | (0.28) |
| *Population Growth* | -0.000 | -0.051* | -0.001 | -0.005 | -0.000 | -0.006** |
| | (-0.78) | (-1.71) | (-0.95) | (-1.59) | (-0.30) | (-2.04) |
| *GDP Growth* | 0.000* | 0.011* | 0.000 | 0.001 | -0.000 | 0.002*** |
| | (1.79) | (1.68) | (1.38) | (0.72) | (-1.25) | (3.02) |
| *Sales growth* | | | | 0.003 | -0.039*** | -0.053*** |
| | | | | (0.40) | (-7.38) | (-6.15) |
| *Loss* | | | | -0.006 | 0.122*** | 0.051*** |
| | | | | (-1.08) | (20.45) | (8.17) |
| Plant-Parent FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj R-squared | 0.952 | 0.764 | 0.994 | 0.256 | 0.227 | 0.266 |
| Observations | 91,782 | 104,872 | 104,902 | 88,987 | 100,584 | 100,584 |

This table reports the OLS regression results for the effects of E-Verify mandates on a plant's parent firm's financial constraints (columns 1–3) and incentives to meat/beat earnings benchmarks (columns 4-6). The sample is at the plant-year level with firm variables measured for a plant's parent firm. Financial constraints are measured with Whited and Wu's (2006) *WW Index*, Kaplan and Zingales' (1997) *KZ Index*, and Hadlock and Pierce's (2010) *SA Index*. We examine three earnings benchmarks: consensus analyst forecast (*Meet/Beat_Forecast*), zero threshold (*Meet/Beat_Zero*), and last year's earnings (*Meet/Beat_LastYear*). The sample period is from 2000 to 2018. *EVerify_All* (*EVerify_Pub*) is an indicator variable that equals 1 if a plant's location state has adopted a universal (public) E-Verify mandate in the year, and 0 otherwise. The passage years of E-Verify are excluded. All regressions included plant-parent and industry-year fixed effects. Robust standard errors are clustered by state. The reported numbers are coefficient estimates and *t*-

statistics (in parentheses). Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

**Table 11 Robustness Tests**

**Panel A: Stacked-Regression Approach**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | Cohorts on Universal E-Verify | | Cohorts on Public E-Verify | |
| | (1) | (2) | (3) | (4) |
| *EVerify_All* | **0.072**** | **0.065*** | 0.087** | 0.076** |
| | **(2.04)** | **(1.88)** | (2.10) | (2.15) |
| *EVerify_Pub* | 0.021 | 0.027* | **0.051***** | **0.060***** |
| | (1.53) | (1.82) | **(2.90)** | **(3.54)** |
| Plant-Chemical-Cohort FE | Yes | Yes | Yes | Yes |
| Chemical-Year-Cohort FE | Yes | Yes | Yes | Yes |
| Industry-Year-Cohort FE | No | Yes | No | Yes |
| *p*-Value for *EVerify_All* > *EVerify_Pub* | 0.053 | 0.097 | 0.204 | 0.323 |
| Adj R-squared | 0.938 | 0.939 | 0.939 | 0.940 |
| Observations | 2,586,068 | 2,585,119 | 3,950,114 | 3,948,251 |

**Panel B: Additional Fixed Effects and Controls**

| | *Ln(1+Toxic)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *EVerify* | 0.045** | 0.044** | | |
| | (2.47) | (2.42) | | |
| *EVerify_All* | | | 0.103** | 0.099*** |
| | | | (2.63) | (2.76) |
| *EVerify_Pub* | | | 0.042** | 0.051*** |
| | | | (2.46) | (2.92) |
| *Unemployment* | | -0.012** | -0.017*** | -0.013*** |
| | | (-2.65) | (-4.11) | (-3.12) |
| *Population Growth* | | -0.008 | 0.000 | 0.002 |
| | | (-1.04) | (0.02) | (0.36) |
| *GDP Growth* | | -0.001 | -0.000 | -0.000 |
| | | (-0.74) | (-0.01) | (-0.21) |
| *Political Balance* | | | 0.002 | -0.016 |
| | | | (0.06) | (-0.48) |
| *LCV Score* | | | -0.031 | -0.015 |
| | | | (-0.99) | (-0.43) |
| *p*-Value for *EVerify_All* > *EVerify_Pub* | | | 0.067 | 0.086 |
| Plant-Chemical FE | Yes | Yes | Yes | Yes |
| Chemical-Year FE | Yes | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes | Yes |
| Parent-Year FE | Yes | Yes | Yes | Yes |
| Adj R-squared | 0.928 | 0.928 | 0.928 | 0.928 |
| Observations | 1,375,415 | 1,365,789 | 1,375,415 | 1,365,789 |

This table reports robustness tests for the effect of E-Verify mandates on the amount of toxic release at the plant-chemical level. Panel A reports the results of the stack-regression approach proposed by Baker et al. (2021). Panel B reports robustness tests for Table 3 using parent-year fixed effects and state-year level macroeconomic variables. The sample period is 2000 to 2018. The dependent variable *Ln(1+Toxic)* is the natural logarithm of one plus the release amount for a chemical (in pounds). *EVerify* is an indicator variable the equals 1 if a plant's location state has adopted any E-Verify mandate in the year, and 0 otherwise. *EVerify_All* (*EVerify_Pub*) is an indicator variable the equals 1 if a plant's location state has adopted a universal (public) E-Verify mandate in the year, and 0 otherwise. The adoption years of E-Verify are excluded. Robust standard errors are clustered by state. The reported numbers are estimated coefficient estimates and *t*-statistics (in parentheses). Variable definitions are provided in Appendix A. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.