Labor Mobility and Corporate Environmental Performance*

Viet A. Dang, Ning Gao, and Tiancheng Yu†

First version: September 2022

This version: December 2022

Abstract

Using U.S. plant-level data, we find that corporate emissions decline significantly when labor mobility increases due to weaker enforcement of covenants not to compete (CNCs) in the states of residency. The effect is more pronounced for firms relying more on highly skilled labor and intangible capital, having lower degrees of financial constraints, or facing greater product market competition. We further document that, as labor mobility restrictions relax, treated firms increase their green innovation and green investment. Our results suggest that greater labor mobility improves corporate environmental performance through boosting emission abatement activities, highlighting an environmental benefit of labor mobility.

JEL Classifications: J62, K13, Q53, Q54, Q55

Keywords: labor mobility, covenants not to compete (CNC), air pollutant emissions, green innovation, green investment

---

* We are grateful to Maria Marchica for her helpful comments and suggestions. The usual disclaimer applies.

† Viet Dang and Ning Gao are affiliated with the Alliance Manchester Business School, University of Manchester. Tiancheng Yu is affiliated with the University of Exeter Business School, University of Exeter. Email: Viet Dang, <Vietanh.Dang@manchester.ac.uk>; Ning Gao, <Ning.Gao@manchester.ac.uk>; and Tiancheng Yu <t.yu@exeter.ac.uk>.

1. Introduction

Firms' pollutant emissions, as a byproduct of their production and operations, constitute a major driver of many environmental issues, such as global warming, extreme weather, and the rise of sea levels (e.g., Stern, 2008; Currie et al., 2014). Existing research has confirmed the costly negative externalities of air pollutant emissions on labor markets, including human health and labor productivity, as well as the economy (e.g., Graff Zivin and Neidell, 2013; Dell et al., 2014). Meanwhile, labor as a direct input to corporate production profoundly impacts energy efficiency, productivity, and technology (e.g., Wozniak, 1987; Black and Lynch, 1996), which, in turn, influences corporate emissions (Chen et al., 2021). However, to the best of our knowledge, few studies have been conducted to examine the effects of labor market frictions on corporate environmental decisions and outcomes. In this paper, we fill this research gap by studying how labor mobility, one of the most important aspects of labor markets, affects firm emissions.

Companies often restrict labor mobility to preempt competition from rival firms (e.g., Kaplan and Strömberg, 2003; Conti, 2014).[1] There has been an ongoing debate on the economic impact of labor mobility restrictions in academia and the business world (Saxenian, 1996; Gilson, 1999; Marx et al., 2009; Barnett and Sichelman, 2016), which has prompted governments and authorities to regulate such restrictions. For example, on July 9, 2021,

[1] A notable example is that of Amazon, who required its employees, including even temporary warehouse workers, to sign 18-month non-compete agreements, preventing these employees from working for its competitors. Facing public criticism, in March 2015, Amazon decided to remove such agreements from its employment contracts (see https://www.theguardian.com/technology/2015/mar/27/amazon-remove-noncompete-clause-contracts-hourly-workers).

President Joe Biden signed an executive order that encourages the Federal Trade Commission (FTC) to ease labor mobility restrictions.[2]

A priori, how labor mobility impacts corporate environmental performance is ambiguous. On the one hand, free labor movement enhances knowledge spillovers across companies, which generates investment opportunities and improves technologies (Almeida and Kogut, 1999; Rosenkopf and Almeida, 2003; Agrawal et al., 2006; Parrotta and Pozzoli, 2012). The prospects of knowledge spillovers incentivize firms to hire employees with valuable know-how and promote them to invest in human capital and innovation (Gilson, 1999; Marx et al., 2009; Garmaise, 2011; Samila and Sorenson, 2011). Importantly, several studies document that knowledge spillovers improve green technologies and investments (Bosetti et al., 2008; Dechezleprêtre et al., 2017; Dechezleprêtre et al., 2020), which help to reduce corporate emissions (Alam et al., 2019; Chen and Lee, 2020; Gao and Li, 2021). Therefore, mobile labor markets could increase firms' ability and incentive to mitigate emissions through knowledge spillovers.

On the other hand, greater labor mobility reduces employers' incentive to invest in human capital and research and development (R&D) (e.g., Conti, 2014; Barnett and Sichelman, 2016) because they are concerned about increased risk of key employees with valuable knowledge and skills being poached by competitors (Rubin and Shedd, 1981; Grossman and Hart, 1986; Jeffers, 2019). Such firms may find it difficult to reduce emissions, because pollution abatement relies significantly on investments in human capital and technologies (Xu and Kim, 2022). This line of reasoning, thus, suggests that firms facing a more mobile labor force may experience higher emissions.

[2] For more details, see https://www.cnbc.com/2021/07/22/biden-administration-aims-to-rein-in-abusive-non-compete-agreements.html.

To test the above competing hypotheses and establish the causal impact of labor mobility on corporate emissions, we exploit the state-level changes in the enforcement strength of CNCs as exogenous shocks to the degree of labor mobility. CNCs, also known as non-compete agreements (NCAs), are specific employment contracts signed between corporate employers and employees to prevent departing employees from joining or establishing competing firms within a certain period and geographic area. CNCs are prevalent across various occupations, especially in high-skill and high-paying positions, such as engineering and computer and mathematical jobs (Starr et al., 2021). Bai et al. (2022) show that the number of lawsuits against employees on CNC breaches has increased in recent years, and such litigation risk can force new employers to stay away from hiring workers subject to CNCs. Meanwhile, to avoid potential lawsuits, highly-skilled professionals (e.g., engineers) who have signed CNCs often involuntarily switch to different industries (Marx, 2011), leading to a brain drain in industries with labor mobility restricted by these agreements (Marx et al., 2010).

In the U.S., CNCs are governed by the relevant jurisdictions of the states where employees work, and so the enforceability of CNCs varies across states and overtime. For example, North Dakota forbids CNC enforcement, and California does not even recognize CNCs. In contrast, Florida has the strongest CNC enforceability, which was strengthened through a state legislature in 1996. Recent research documents that state-level CNC enforcement significantly affects labor mobility, especially in knowledge-intensive occupations (e.g., Garmaise, 2011; Jeffers, 2019; Starr et al., 2021). To the extent that changes in CNC enforceability are driven by either state court rulings or legislations, which are arguably exogenous to company decisions and outcomes (Ewens and Marx, 2018; Jeffers, 2019),[3] they provide an ideal setting in which to examine the effect of labor mobility on corporate emissions.

[3] Section 4.2.1 discusses in detail the endogeneity issues regarding the state-level CNC enforcement changes.

We adopt a difference-in-differences (DID) approach to test whether and to what extent weakened CNC enforcement affects corporate emissions of air pollutants. Using plant-level data obtained from the Environmental Protection Agency's (EPA) Air Markets Program Data (AMPD), we find that carbon dioxide (CO_2), sulfur dioxide (SO_2), and nitrogen oxides (NO_x) emissions drop significantly by 18.1%, 12.6%, and 3.4%, respectively, following the weakening of CNC enforceability. This effect is economically large and consistent with the knowledge spillover hypothesis that greater labor mobility improves corporate environmental performance through increased green innovation and investment.

Our main finding persists in several robustness tests. First, we investigate the dynamic effects of weakened CNC enforcement on plant emissions and find that the documented decline in emissions only appears after the weakening of CNC enforceability. This analysis provides evidence in support of the key assumption underlying our DID analysis, namely, parallel pre-shock trends, and alleviates endogeneity concerns about potential reverse causality between firm emissions and weakened CNC enforcement. Second, we find that our baseline results remain unchanged when using a propensity score matched sample, suggesting that our inference is unlikely to be driven by differences in the characteristics between the treated and control groups. Additionally, our baseline results are robust to using a control sample consisting of plants in neighboring states, using firm headquarters locations to define the treated and control groups (instead of the plant locations), using a stacked DID regression approach, and a placebo test.

To gain further insights into the effect of labor mobility on firm emissions, we explore the cross-sectional variations in key industry and firm characteristics using difference-in-difference-in-differences (DDD) regression models. Because CNCs are more common and relevant for skilled professionals (e.g., Garmaise, 2011) and for firms with greater intangible capital (Kini et al., 2021), we expect the impact of weakened CNC enforcement on corporate emissions to be more pronounced for firms with greater reliance on skilled labor or intangible capital. Our DDD results are consistent with this conjecture. Furthermore, to the extent that firms require ample financial resources to invest in abatement activities that mitigate emissions (Xu and Kim, 2022), we expect firms with lower degrees of financial constraints to be in a better position to take advantage of the benefits from a more mobile labor force (e.g., knowledge spillover effects) to reduce emissions. Our empirical finding is in line with this argument. Finally, we expect firms facing greater product market competition to mitigate their emissions more significantly in response to weakened CNCs enforcement, because such firms arguably are able to exploit greater levels of knowledge spillovers from their competitors (Henderson and Cockburn, 1996). Again, our empirical result is in line with this prediction.

In the final test, we seek to better understand the economic mechanisms driving our findings, that is, how increased labor mobility facilitates technological solutions and abatement activities and that drive down corporate emissions. To this end, we study the effect of weakened CNC enforceability on green innovation and green investment. Using both firm- and plant-level data, we find that firms improve the quantity and quality of green innovation and increase green investment when CNCs become less enforceable. These results suggest that greater labor mobility enhances green innovation and reinforces green investment, thereby mitigating air pollutant emissions through technology spillovers.

Our study contributes to two strands of literature. First, we add to an emerging literature exploring the various effects of labor mobility on company decisions and outcomes. Recent research in this area has studied the impact of labor mobility on firm value (He and Wintoki, 2020; Shen, 2021), investment (Jeffers, 2019; Bai et al., 2022), innovation (Samila and Sorenson, 2011; Xiao, 2022), entrepreneurship (Samila and Sorenson, 2011; Starr et al., 2018), corporate financial decisions (Aobdia, 2018; Chen et al., 2018; He, 2018), and corporate governance (Garmaise, 2011; Cici et al., 2021; Kini et al., 2021). Compared to the extant studies, we provide new evidence of the social and environmental consequences of labor mobility restrictions for the corporate sector. Our results suggest that attempts to restrict labor mobility result in a negative impact on corporate environmental profiles, which, in turn, impose negative externalities on society and the public.

In particular, our findings speak to the ongoing debate over labor mobility restrictions and their economic impact. Proponents of such restrictions argue that limiting employee mobility helps to protect company intellectual property and investments, promoting overall innovation (e.g., Barnett and Sichelman, 2016). Opponents, however, believe that labor-mobility constraints reduce employees' bargaining power and wages (Johnson et al., 2021) and diminish innovation via restricted knowledge spillovers (Gilson, 1999; Marx et al., 2009), thereby having negative consequences on regional growth (Saxenian, 1996).

Second, we add to the growing literature on the determinants of corporate environmental performance, particularly corporate emissions of air pollutants. Recent studies have investigated how firms' emissions are driven by environmental policies (e.g., Shapiro and Walker, 2018), corporate finance (Andersen, 2017; Levine et al., 2018; Goetz, 2019; Akey and Appel, 2021; Gao and Li, 2021; Iovino et al., 2021; Bartram et al., 2022; Lyu et al., 2022; Xu and Kim, 2022), shareholders (Akey and Appel, 2019; Chu and Zhao, 2019; Kim et al., 2019; Shive and Forster, 2020; Azar et al., 2021; Choi et al., 2021; Dasgupta et al., 2021; Heath et al., 2021; Naaraayanan et al., 2021), non-shareholder stakeholders (Bellon, 2021, 2022; Chen, 2022; Choy et al., 2022; Dai et al., 2022), corporate governance (Wang and Yu, 2019; Li et al., 2021b; Altunbas et al., 2022), external governance (Duflo et al., 2013; Grinstein and Larkin, 2021), political factors (Chu et al., 2021; Heitz et al., 2021; Bisetti et al., 2022), and human capital investment (Chen et al., 2021). To the best of our knowledge, the current paper is the first to demonstrate how a major labor market friction, namely, labor mobility restrictions, reduces corporate environmental performance, thus showing how relaxing such restrictions could lower corporate emissions.

This study provides relevant policy implications. Our empirical evidence suggests that greater labor mobility improves corporate pollution abatement and ultimately contributes to environmental protection. To mitigate air pollution and climate change, as well as their negative consequences on the public and society, regulators should consider the benefits of policy reforms aimed at relaxing labor mobility restrictions, which facilitate the propagation of green innovation and green investment throughout the corporate sector. Our paper thus highlights the potential environmental and social benefits of mobile labor markets.

The rest of this paper proceeds as follows. Section 2 develops the testable hypotheses. Section 3 describes our data and empirical methods. Section 4 presents the empirical findings. Section 5 concludes.

2. Hypothesis Development

Theory predicts an ambiguous relation between the degree of labor mobility and the extent of corporate emissions. On the one hand, greater labor mobility facilitates knowledge spillovers among firms since skilled workers moving across firms port their knowledge, skills, and know-how with them, which will benefit their new employers (Almeida and Kogut, 1999; Rosenkopf and Almeida, 2003; Agrawal et al., 2006; Parrotta and Pozzoli, 2012). Through such knowledge spillovers, firms may learn about new investment opportunities and technologies, which, in turn, facilitate their investments in human capital and innovation (Gilson, 1999; Marx et al., 2009; Garmaise, 2011; Samila and Sorenson, 2011). Meanwhile, knowledge spillovers are important in shaping the diffusion of technologies and spreading scientific developments that help to foster corporate innovation, which boost firms' green technologies and increase green investment (Bosetti et al., 2008; Dechezleprêtre et al., 2017; Dechezleprêtre et al., 2020).

Previous literature demonstrates that corporate environmental performance benefits substantially from clean technology advances through decreased energy consumption, improved energy efficiency, and enhanced pollution abatement (Alam et al., 2019; Chen and Lee, 2020; Gao and Li, 2021).[4] Overall, in mobile labor markets, firms may have greater incentive and ability to engage in pollution abatement due to knowledge spillovers. Based on these arguments, we predict that weaker labor mobility restrictions will have a positive impact on corporate emissions of air pollutants. Our hypothesis can be stated as follows.

Hypothesis 1a: Greater labor mobility leads to lower corporate emissions.

On the other hand, when labor markets are mobile, companies could be reluctant to invest in human capital by cutting back on staff training or by reducing investments in R&D, since they are concerned that valuable workers might start their own competing businesses or work for rivals (Barnett and Sichelman, 2016; Jeffers, 2019). Such a concern is highlighted by several previous studies, which show that incumbent firms may forgo certain investment in their workforce due to a "hold-up" problem (Rubin and Shedd, 1981; Grossman and Hart, 1986; Acemoglu and Shimer, 1999), while eschew innovating or adopting risky R&D strategy due to the concern that departing employees could transfer proprietary information to rival firms (Conti, 2014; Barnett and Sichelman, 2016).[5] In addition, because worker mobility facilitates the entry of new firms, incumbent firms may experience higher competitive pressure,

[4] Dechezleprêtre et al. (2017) find that the magnitude of knowledge spillovers from green technologies is large, even comparable in scope to those observed in the IT industry.

[5] A hold-up problem arises when firms must invest before hiring their employees and wages are negotiated ex-post. As a result of this problem, firms cannot make optimal investments in the presence of workers' bargaining power ex-post (Acemoglu and Shimer, 1999).

leading to lower investments (Jeffers, 2019). With lower investments in R&D and human capital, firms may experience a deterioration in their environmental performance, given that pollution abatement is costly and requires substantial inputs of energy, labor, raw materials, and technologies in production processes (Chen et al., 2021; Xu and Kim, 2022). To the extent that greater labor mobility could discourage firms from investing in the human capital and R&D required for emission abatement, we expect labor mobility to be negatively associated with corporate emissions. We thus formulate the following hypothesis.

Hypothesis 1b: Greater labor mobility leads to higher corporate emissions.

3. Data and Empirical Methods

3.1. Data on Corporate Emissions

3.1.1. Plant-level Emissions and Firm-level Financial Information

As in recent research (e.g., Shive and Forster, 2020; Grinstein and Larkin, 2021), the source of emissions data used in this study is the AMPD.[6] Since 1990, following the requirements under 40 Code of Federal Regulations (CFR) Part 75, EPA's Clean Air Markets Division (CAMD) has monitored and gathered emissions data in the power sector to ensure compliance with various emissions control programs administered by the EPA, including the Acid Rain Program, the Cross-State Air Pollution Rule, and Mercury and Air Toxics Standard, among others. The purpose of 40 CFR Part 75 is to establish requirements for continuous monitoring, recordkeeping, and reporting of CO_2, SO_2, and NO_x emissions, along with facility, operation, and quality assurance test data from electricity generating units (EGUs). EGUs regulated by

[6] Since August 2022, the AMPD has been replaced by a new database, Clean Air Markets Program Data (CAMPD), which can be accessed at https://campd.epa.gov.

the emissions control programs must submit the data to the EPA,[7] within 30 days at the end of each calendar quarter.

Among the reported data from EGUs, those covering air pollutant emissions, which we use in our empirical analysis, are considered to be of outstanding quality (Shive and Forster, 2020). The reason is that the EPA has gone to great lengths to ensure data accuracy. For example, the EPA operates quality assurance tests at emission sources, conducts periodic field audits to verify that emission monitors and data handling systems are functioning properly, and performs additional checks, such as detecting anomalous data. The EPA then makes certain data, including CO_2, SO_2, and NO_x emissions, operations, and facility information, publicly accessible through the AMPD, a web-based dataset. These data are provided at the emission source level (i.e., plant level) and are available for the years 1980, 1985, 1990, and annually starting from 1995.

We retrieve firm-level financial data from the Compustat database. Since the AMPD dataset does not disclose any parent company information for emission sources and there is no linking table or common identifier between the AMPD and Compustat, it is challenging to link the two databases. To address this issue, we use the information obtained from the EPA's Greenhouse Gas Reporting Program (GHGRP). The GHGRP collects U.S. parent company information at the highest level for plants and facilities that emit 25,000 metric tons or more of greenhouse gases (GHG) per year and provides a power-plant crosswalk that links the AMPD and GHGRP through a plant identifier called the Office of Regulatory Information Systems Plant Location (ORISPL), starting from 2010.[8] We, therefore, restrict our AMPD sample to the period between 2010 and 2019 and merge the AMPD and GHGRP databases using the ORISPL

[7] In general, EGUs with a nameplate capacity of greater than 25 megawatts are affected by the programs.

[8] We find that the GHGRP power plant crosswalk covers 96% of plant-year observations in the AMPD from 2010 to 2019.

identifier to obtain parental company information. We then conduct a name-matching procedure to link the AMPD and Compustat databases through parent company names (see Online Appendix OA1 for more details on the name-matching process).

3.1.2. Green Innovation and Investment

For our green innovation analysis reported in Section 4.4., we rely on the United States Patent and Trademark Office (USPTO) database. We first use the linking table provided by Kogan et al. (2017) to match the USPTO and Compustat databases. Since the majority (64%) of our sample firms are in the utilities sector (Standard Industrial Classification codes (SIC) 4900–4999), which is not covered in Kogan et al. (2017), we adopt the name-matching method as detailed in Online Appendix OA1 to link the unmatched sample firms with the USPTO data.

We then determine whether a patent is "green" or not based on the Cooperative Patent Classification (CPC).[9] Our first green innovation measure follows Li et al. (2021a), where we classify a patent as "green" if it has at least one CPC code of Y02 or Y04S.[10] We construct the second measure as in Cohen et al. (2022). Specifically, we use a patent search strategy that the Organization for Economic Co-operation and Development (OECD) developed for the identification of environment-related technologies, which include environmental management, water-related adaptation technologies, biodiversity protection and ecosystem health, and various climate change mitigation technologies.[11]

---

[9] https://www.uspto.gov/web/patents/classification/cpc/html/cpc.html.

[10] Code Y02 refers to patents that mitigate anthropogenic emissions of GHG and technologies that facilitate the adaptation to the adverse effects of climate change. Code Y04S represents systems integrating technologies on power network operation, communication, or information technologies for improving the electrical power generation, transmission, distribution, management, or usage (i.e., smart grids).

[11] Refer to Haščič and Migotto (2015) for a detailed explanation.

In terms of green investment data, we retrieve plant-level information from the United States Energy Information Administration (EIA) following Grinstein and Larkin (2021).[12] The EIA conducts surveys to collect information on electricity generation processes from electric power plants, including data on electric power sales and revenue, electric generator capacity, and power plant operating data. To construct measures of (plant-level) green investment, we use the form EIA-923 to obtain data on the use of flue-gas desulfurization (FGD) equipment, quantities of fuels, and electricity generation. Specifically, for coal power plants, FGD technologies may remove around 90% of pollutants in the flue gases (Popp, 2003). In addition, we use the data on three main types of fossil fuels, namely, coal, petroleum, and natural gas. Coal is the most polluting fuel for power generation, followed by petroleum and natural gas (Grinstein and Larkin, 2021). To measure plant-level electricity production, we use net electricity generation measured in Megawatt hours. Linking the EIA and AMPD databases is straightforward through the ORISPL identifier.

3.2. Empirical Methods

To test the competing hypotheses (*H1a* and *H1b*) regarding the impact of labor mobility on air pollutant emissions, we adopt a DID identification strategy that exploits the exogenous changes in state-level CNC enforcement. Table 1 lists the CNC enforcement changes used in this study based on Ewens and Marx (2018).[13]

[Insert Table 1 about here]

[12] https://www.eia.gov/electricity/data/detail-data.php.

[13] As in Ewens and Marx (2018), New York and New Mexico are excluded from our sample. The CNC enforceability changes in New York and New Mexico were specific to the broadcasting industry and physicians, respectively, which are irrelevant to our sample firms.

We estimate a DID model with the CNC enforcement changes as our treatment of interest. The baseline regression model is as follows:

$$Ln\left(1 + Emissions_{j,t}\right) = \alpha + \beta Weaker\ CNC\ Enforcement_{s,t} + \gamma \boldsymbol{X_{i,t-1}}$$
$$+\delta \boldsymbol{Z_{s,t-1}} + \nu_j + \phi_{c,t} + \varepsilon_{j,t}, \tag{1}$$

where j, i, c, s, and t denote plants, firms, industries, states, and years, respectively; $Ln\left(1 + Emissions_{j,t}\right)$ is the natural logarithm of one plus the emissions (*CO_2_R*, *SO_2_R*, and *NO_x_R*) by plant j in year t; $Weaker\ CNC\ Enforcement_{s,t}$ is an indicator that equals one if a plant is in a state that weakened CNC enforceability by the end of year t, minus one if a plant is in a state that strengthened CNC enforceability by the end of year t, and zero otherwise; $\boldsymbol{X_{i,t-1}}$ and $\boldsymbol{Z_{s,t-1}}$ denote a vector of the one-year lagged firm- and state-level control variables, respectively; ν_j and $\phi_{c,t}$ are plant fixed effects and industry-by-year fixed effects defined at the six-digit North American Industry Classification System (NAICS) level, respectively; and $\varepsilon_{j,t}$ is the error term.[14] Standard errors are clustered at the state level, which is the level of treatment, to correct for serial correlation within the same state (Bertrand et al., 2004).

We follow Shive and Forster (2020) to use *CO_2_R*, *SO_2_R*, and *NO_x_R* as our emissions variables. Specifically, we use actual emissions volumes (*CO_2*, *SO_2*, and *NO_x*) scaled by firm revenues obtained from the Compustat database.[15] In terms of firm-level characteristics, we control for *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, and *Firm Age* following the existing climate finance literature (e.g., Shive and Forster, 2020; Bartram et al.,

[14] Table OA1 in our online appendix reports the regression results for models with industry-by-year fixed effects rather than year fixed effects. The coefficients on the treatment indicators are qualitatively the same as our baseline results.

[15] In Table OA2 in our online appendix, we also use the natural log of one plus the actual emissions volumes as the dependent variables. The results remain qualitatively unchanged.

2022; Xu and Kim, 2022). In addition, we include *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes* to control for state-level economic and political characteristics. Appendix A provides detailed variable definitions.

Our final sample includes 5,240 plant-years and 575 firm-years from 2010 to 2019. Table 2 presents summary statistics for the main variables used in our study. All the continuous variables, except for state-level controls, are winsorized at the top and bottom 1% of their sample distributions. Compared with the whole Compustat universe, our sample firms have larger total assets (*Total Assets*), higher leverage (*Leverage*), more tangible assets (*Fixed Assets*), and lower growth prospects (*Market-to-Book*). The reason for these differences is that these firms primarily operate in the utilities sector.

[Insert Table 2 about here]

4. Empirical Results

4.1. Baseline Results

Table 3 reports the baseline results from regressing air pollutant emissions on weakened state-level CNC enforcement. Columns (1) and (2), (3) and (4), and (5) and (6) present the regression results for the three types of air pollutants, namely, CO_2, SO_2, and NO_x emissions, respectively. The regression specification for columns (1), (3), and (5) includes only plant and industry-by-year fixed effects and no control variables to alleviate the problem of “bad controls” (Angrist and Pischke, 2008). In columns (2), (4), and (6), we further add all the firm- and state-level control variables defined above. The coefficients on the variable of interest, *Weaker CNC Enforcement*, are consistent across different specifications, showing a significant and negative impact of lower CNC enforceability on plant emissions.[16] These results suggest that in the presence of weaker CNC enforcement, corporate emissions of CO_2, SO_2, and NO_x decrease by 18.1%, 12.6%, and 3.4%, respectively. These economic magnitudes are practically important. Overall, our baseline results suggest that greater labor mobility leads to lower corporate emissions and better environmental performance overall, which is in line with *Hypothesis 1a* and inconsistent with *Hypothesis 1b*.

[Insert Table 3 about here]

4.2. Robustness Tests

4.2.1. Endogeneity Concerns on the Changes in CNC Enforcement

The identification strategy used in the above analysis relies on a key assumption that the state-level changes in CNC enforcement provide exogenous shocks to labor mobility in the DID model, and thus are orthogonal to corporate emissions. In what follows, we discuss the validity of this assumption and provide empirical evidence to address potential endogeneity concerns.

As discussed, the CNC enforcement changes are driven by either state Supreme Court rulings or legislative decisions. Hence, there is less concern about lobbying or political pressure affecting court rulings, which are judicial decisions based on the merits of specific cases (Ewens and Marx, 2018; Jeffers, 2019). Two states, namely, Georgia and New Hampshire,

[16] Heath et al. (2022) point out a multiple testing problem that reusing natural experiments to examine different outcome variables may increase the probability of false discoveries. To the best of our knowledge, around 10 papers have used the state-level changes in CNC enforcement in a staggered DID setting (e.g., Chen et al., 2018; He, 2018; Huang et al., 2019; Jeffers, 2019; He and Wintoki, 2020; Cici et al., 2021; Çolak and Korkeamäki, 2021; Kini et al., 2021; Heath et al., 2022; Xiao, 2022). According to Heath et al. (2022), the adjusted critical value of the *t*-statistics given 10 prior findings using the same experiment is around 2.8. Most of our baseline results exceed the threshold suggested, thus mitigating the concern about possible false discoveries.

experienced changes to CNC enforcement due to legislations rather than court rulings, which raises the question of whether the CNC enforcement in these two states is plausibly exogenous. However, there were considerable uncertainties before the increase in CNC enforceability in Georgia, since the statutory change was due to a 2010 referendum that was criticized as misleading as it did not directly refer to CNCs (Ewens and Marx, 2018; Jeffers, 2019). Meanwhile, the New Hampshire law, which weakened CNC enforcement, was proposed by a state representative who had personally been influenced by a CNC, implying that this change was not directly due to corporate environmental policies (Ewens and Marx, 2018). In summary, the legislations passed in Georgia and New Hampshire were arguably exogenous. Nonetheless, in untabulated analysis, we find that our baseline results are robust to excluding these two states from the sample.

To further alleviate the concern that state-level macroeconomic, political, and legal conditions may drive the changes in CNC enforcement, we follow previous literature (e.g., Acharya et al., 2014) and estimate a Cox proportional hazard model that predicts these changes. In the model, a "failure event" is defined as a change in CNC enforcement in a state; we remove a state from the sample for the years after it experiences some changes to CNC enforceability. We report the estimation results in Table 4. Column (1) includes plant CO_2, SO_2, and NO_x emissions aggregated at the state level (*State CO_2 Emissions*, *State SO_2 Emissions*, and *State NO_x Emissions*, respectively). Column (2) further controls for two measures of the rigidity of labor markets, namely, an index capturing the number of wrongful discharge laws that a state has recognized (*Wrongful Discharge Laws*) and the percentage of workers that are covered by a collective bargaining agreement in a state (*State Union Membership*). We also include state unemployment rate (*State Unemployment Rate*), GDP per Capita (*Log (State GDP per Capita)*), GDP growth (*State GDP Growth*), and general election votes for the Democratic Party (*Democratic Votes*) in Column (3). Across these models, none of the independent variables is statistically significant, suggesting that neither these state-level factors nor aggregated plant emissions are likely to influence the changes in state CNC enforcement. The latter results also help to allay concerns regarding potential reverse causality.

[Insert Table 4 about here]

4.2.2. Parallel Trends Assumption

Next, we examine the relation between weakened CNC enforcement and the timing of carbon emissions changes to validate another important assumption of the DID approach, namely, parallel trends (Bertrand and Mullainathan, 2003).[17] In Table 5, we replace the *Weaker CNC Enforcement* variable in columns (1) and (2) of Table 3 (i.e., our preferred specifications) with five indicators, namely, *Weaker CNC Enforcement*$^{-2}$, *Weaker CNC Enforcement*$^{-1}$, *Weaker CNC Enforcement*^{0}, *Weaker CNC Enforcement*$^{+1}$, and *Weaker CNC Enforcement*$^{2+}$. Specifically, *Weaker CNC Enforcement*$^{-2}$ and *Weaker CNC Enforcement*$^{-1}$ are dummy variables that equal one (minus one) if a state would weaken (strengthen) its CNC enforceability in two years and one year, respectively, and zero otherwise; *Weaker CNC Enforcement*^{0} is an indicator equal to one (minus one) if the state weakens (strengthens) its CNC enforceability in the current year and zero otherwise; *Weaker CNC Enforcement*$^{+1}$ and *Weaker CNC Enforcement*$^{2+}$ are indicators that equal one (minus one) if the state weakened (strengthened) its CNC enforceability one year before and two or more years before, respectively, and zero otherwise.

The regression results reported in Table 5 show that the coefficients on *Weaker CNC Enforcement*$^{-2}$ and *Weaker CNC Enforcement*$^{-1}$ are statistically insignificant across alternative

[17] In the remainder of the analysis, we follow previous research (e.g., Shive and Forster, 2020) and use CO_2 emissions as the main dependent variable.

specifications with and without the control variables, supporting the parallel trends assumption and further implying that our results are unlikely to be driven by reverse causality. Consistent with our baseline results, the coefficients on *Weaker CNC Enforcement*^{0}, *Weaker CNC Enforcement*$^{+1}$, and *Weaker CNC Enforcement*$^{2+}$ are significantly negative. In addition, the negative and significant coefficients on *Weaker CNC Enforcement*$^{2+}$ suggest that the impact of the lower CNC enforceability on carbon emissions persists in the long run.

[Insert Table 5 about here]

4.2.3. Covariate Balance between the Treated and Control Groups

To alleviate another concern that differences in the characteristics of the control and treated groups might drive our findings, we estimate the effect of weakened CNC enforcement on carbon emissions by using a propensity score matched sample. To create this sample, we retain all the observations for the control and treated groups one year before the CNC enforcement changes. We treat each year's state-level changes as a separate event. Then for each event, we use a probit model to estimate the probability of being a treated firm by regressing a treatment indicator (equal to one if the firm belongs to the treated group, and zero otherwise) on all the firm-level covariates, namely, *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book, ROA*, and *Firm Age*, used in our baseline models. Next, within each three-digit NAICS industry cluster and for each event, we match each treated firm to a control firm based on the closest propensity score with a caliper of 0.1 and without replacement. Our final matched sample consists of 90 treated and control firms. Panel A of Table 6 reports the test for covariate balance of the matched sample. The test results show no significant difference in the mean values of any covariates between the treated and control groups, implying that our propensity score matching (PSM) procedure is successful.

Using the propensity score matched sample, we report the regression results in Panel B of Table 6. We find that these results are in line with our baseline findings, as they continue to show a significant and negative effect of weaker CNC enforcement on carbon emissions, with similar economic magnitudes. In summary, the PSM analysis suggests that our baseline results are robust to accounting for covariate balance.

[Insert Table 6 about here]

4.2.4. Additional Robustness Analysis

We further establish the robustness of our main inference through several additional tests. First, to alleviate another endogeneity concern that local economic, legal, or political factors may drive our main inference, we construct a sample consisting of plants located in the treated states and their neighboring control states that may face similar regional conditions due to their geographical proximity. Columns (1) and (2) of Table 7 show that the regression results for this sample are consistent with our baseline findings.

Since knowledge workers who are likely to be affected by CNCs could work at, or near, a firm's headquarter (Bai et al., 2022), in the next robustness check, we use firm headquarters locations to redefine the variable of interest, *Weaker CNC Enforcement*. In columns (3) and (4) of Table 7, we then re-estimate the DID model and find the results to remain unchanged.

Cengiz et al. (2019) and Baker et al. (2022) demonstrate the potential biases due to treatment effect heterogeneity in staggered DID regressions with two-way fixed effects (TWFE). To address this concern, we follow the suggestions provided by these studies and employ a stacked regression approach. The results, reported in columns (5) and (6) of Table 7, indicate that our baseline findings are robust to using this alternative research design.

In a recent study, Flammer and Kacperczyk (2019) find that firms strategically improve their corporate social responsibility (CSR) performance in response to the threat of knowledge leakage caused by greater labor mobility. Their finding raises the concern that our evidence of lower corporate emissions might be driven by firms' increased engagement in CSR activities. To address this concern, we further add to our main model specification (column (2) of Table 3) four main measures of CSR performance used in Flammer and Kacperczyk (2019), namely, the CSR rating score (*KLD Index*), the net CSR rating score (*Net KLD Index*), the environmental performance score (*KLD Environment Index*), and the net environmental performance score (*Net KLD Environment Index*).[18] Table OA3 in our online appendix shows that our main finding continues to hold.

Recent evidence suggests that when CNCs become less enforceable, firms may raise their investments and, therefore, experience higher sales growth and profits (Bai et al., 2022).[19] To alleviate the possibility that the observed decline in emissions might be due to increased sales (i.e., higher values of the denominators in our dependent variables), we control for firm investment in Table OA4 in our online appendix. Our results remain qualitatively the same.

Despite several tests validating the DID approach presented above, there remains yet another concern that our baseline findings could be driven by unobserved factors that coincided with the changes in CNC enforcement. To rule out this possibility, following Heider and Ljungqvist (2015), we conduct a placebo test in which we randomly assign (pseudo) treated states and enforcement change years and then investigate the effect of the constructed (pseudo) shocks on carbon emissions. We repeat the procedure 5,000 times and report the distribution of estimated *t*-statistics of the coefficient estimates in Figure OA1 in our online appendix. The red vertical line in Figure OA1 represents the *t*-statistic estimated from our baseline regression.

---

[18] We find that the correlations between plant emissions and the four CSR performance measures range from –0.12 to 0.03.

[19] Nevertheless, some studies suggest that firms reduce their investments in response to weakened CNC enforcement (e.g., Conti, 2014; Barnett and Sichelman, 2016).

We find that this *t*-statistic differs significantly from the *t*-statistics estimated from the placebo samples, indicating that unobserved confounding factors are unlikely to drive our baseline findings.

[Insert Table 7 about here]

4.3. Cross-sectional Analysis

In this section, we investigate potential cross-sectional variations in the effect of weakened CNC enforceability on corporate emissions to provide some evidence on the underlying mechanisms. We employ a DDD model by interacting the *Weaker CNC Enforcement* indicator with the moderating variable of interest, which represents industry- or firm-level cross-sectional heterogeneity. In addition, we further add state-by-year fixed effects to our model specification to account for unobserved and time-varying confounding factors at the state level.

4.3.1. Impact of Labor Skills and Intangible Capital

The baseline findings in this study are consistent with the hypothesis that greater labor mobility (i.e., weaker CNC enforcement) leads to lower corporate emissions through knowledge spillover effects in the corporate sector. Since CNCs primarily restrict the mobility of highly-skilled workers (Garmaise, 2011; Marx, 2011; Jeffers, 2019), we predict that the impact of lower CNC enforceability on corporate emissions will be more pronounced for plants with a greater share of skilled workers. We follow Ghaly et al. (2017) and construct the measure of skilled labor at the state-by-industry level by using the Occupational Employment Statistics (OES) data from the Bureau of Labor Statistics and the information on the classification of occupations from the U.S. Department of Labor's Occupational Information Network (O*NET). Next, we estimate the DDD model by interacting the *Weaker CNC Enforcement* indicator with *High Labor Skills*, a dummy variable equal to one (zero) if a plant has an above-median (below-median) value of the measure. Column (1) of Table 8 reports the regression results. In line with our prediction, the coefficient on the interaction term of interest is significantly negative, indicating that, following the weakening of CNC enforceability, plants with greater reliance on skilled workers, which are more exposed to CNCs, experience even lower emissions.

CNCs are more relevant when firms have more intangible assets, including intellectual, human, and organizational capital (Kini et al., 2021). Therefore, we expect the impact of weakened CNC enforcement on corporate emissions to be more pronounced for firms with greater intangible capital. To gauge firms' intangible capital, we employ three firm-level measures. The first measure is knowledge capital (Peters and Taylor (2017), which is an estimate of the stock of knowledge capital, obtained by applying the perpetual-inventory method to a firm's past spending on R&D. The second and third measures are R&D and SG&A expenses, respectively, following previous research (e.g., Eisfeldt and Papanikolaou, 2013; Qiu and Wang, 2018). R&D expenses are an important component of a firm's intangible capital. Meanwhile, a large portion of SG&A expenses includes expenses associated with labor training costs, IT investment, advertising and marketing expenses, and R&D spending, which help to improve company proprietary knowledge. We set three conditioning variables, *High Knowledge Capital*, *High R&D Expense*, and *High SG&A Expense*, to one (zero) if a firm has an above-median (below-median) value of knowledge capital, R&D expenditures, and SG&A expenses, all scaled by the book value of total assets, respectively. The regression results reported in columns (2) to (4) of Table 8 show that the coefficients on the three interaction terms of interest are all significantly negative. Consistent with our expectation, these results suggest that, when CNCs are less enforceable, firms with higher intangible capital face a more mobile labor force and can reduce emissions significantly more than those with lower intangible capital.

[Insert Table 8 about here]

4.3.2. Impact of Financial Constraints and Product Market Competition

Recent research documents that firms' financial resources play an important role in affecting their emission abatement behaviors (e.g., Bartram et al., 2022; Xu and Kim, 2022). With higher knowledge spillovers induced by less enforceable CNCs, financially unconstrained firms should be in a better position to take advantage of this benefit and allocate their ample financial resources to environmental abatement activities. We thus expect financially unconstrained firms to reduce more emissions than their financially constrained counterparts. Following Xu and Kim (2022), we use a text-based measure of financial constraints developed by Bodnaruk et al. (2015).[20] We define less (more) financially constrained firms as those with below- (above-) median values of this measure and estimate the DDD model that includes an interaction between the *Weaker CNC Enforcement* and the conditioning variable (i.e., *Low Financial Constraints*). Column (1) of Table 9 shows that the coefficient on the interaction term is significant and negative. In line with our conjecture, this result suggests that the effect of weakened CNC enforceability on corporate emissions is more pronounced for firms with lower levels of financial constraints, i.e., those firms who could benefit most from a more mobile labor force.

It is arguable that competition motivates companies to adopt rivals' knowledge and technologies where they can (Henderson and Cockburn, 1996), which catalyzes knowledge spillovers. We predict that firms operating in highly competitive markets will stand to benefit more from the knowledge spillover effects induced by greater labor mobility and hence will

[20] Since traditional measures of financial constraints based on accounting data could be highly correlated with production levels, which are directly related to corporate emissions (Xu and Kim, 2022), we use the text-based measure developed by Bodnaruk et al. (2015) to avoid endogeneity issues.

mitigate their emissions more significantly, compared to those firms facing lower competitive pressure. We employ two text-based measures of product market competition, namely, product fluidity (Hoberg et al., 2014) and similarity (Hoberg and Phillips, 2016).[21] To construct the conditioning variables, we set *High Product Fluidity* and *High Product Similarity* to one (zero) if a firm has a value of product fluidity and similarity above (below) the sample median, respectively. We then interact the *Weaker CNC Enforcement* indicator with each conditioning variable and present the DDD regression results in columns (2) and (3) of Table 9. Consistent with our prediction, we find that, in response to weaker CNC enforcement, firms facing higher degrees of product market competition seem to benefit more from greater labor mobility, and be able to reduce their emissions significantly more than those with lower levels of competition.

[Insert Table 9 about here]

4.4. Channel Tests: Green Innovation and Investment

As discussed in Section 2, greater labor mobility is associated with a reduction in corporate emissions because more mobile labor markets foster knowledge spillovers in the corporate sector that ultimately boost corporate abatement activities. To provide direct evidence on these economic channels, in our final test, we analyze the effect of weaker CNC enforcement on green innovation and green investment.

As detailed in Section 3.1.2, we classify a patent as "green" using the approaches in Li et al. (2021a) and Cohen et al. (2022). We replace the dependent variable in Eq. (1) with a green innovation measure and conduct the DID regression at the firm level. First, we use the number of green patents a firm generates (*Log (1+Green Patent)*) to measure the quantity of

[21] The fluidity measure captures rival firms' changes in products and services surrounding the focal firm, and the similarity measure gauges the similarity of rival firms' products and services relative to the focal firm.

green innovation. The regression results reported in columns (1) and (2) of Table 10 show a significant and positive relation between lower CNC enforceability and the number of green patents. In terms of economic magnitude, we find that weaker CNC enforcement leads to an increase of 11.0–13.6% in the number of green patents. Second, we employ the number of forward patent citations (*Log (1+Green Citation)*) to proxy for the quality of green innovation. In columns (3) and (4) of Table 10, we find that the citations received by green patents increase significantly by 19.5–20.2% following decreases in CNC enforceability. Overall, the results in Table 10 support our prediction that firms can take advantage of enhanced knowledge spillovers in more mobile labor markets by increasing both the quantity and quality of their green innovation.

[Insert Table 10 about here]

We next examine the effect of less enforceable CNCs on green investment. To this end, we estimate the DID model as in Eq. (1) and use the following measures of green investment as the dependent variable, namely, *Scrubber Dummy*, *Log (1+Coal Quantities)*, *Log (1+Petroleum Quantities)*, and *Pct. Clean Energy* (Grinstein and Larkin, 2021). Plant-level controls, such as *Scrubber Dummy* and *Net Generation*, are further included in the regression models following Grinstein and Larkin (2021).[22] We first focus on whether a plant invests in at least one FGD unit (*Scrubber Dummy*), an important emission control technology. The regression results presented in column (1) of Table 11 suggest that plants significantly increase their investments in FGD technologies following decreases in CNC enforceability. Next, we investigate whether plants change their fuel mix in response to decreased enforcement of CNCs and report the results in columns (2) to (4) of Table 11. We find that when CNCs are less enforceable, plants rely significantly less on polluting fuels, including coal (*Log (1+Coal*

[22] In column (1) of Table 11, we do not include *Scrubber Dummy* as the plant-level control variable.

Quantities)) and petroleum (*Log (1+Petroleum Quantities)*), while they significantly use cleaner energy sources (*Pct. Clean Energy*), such as natural gas, solar, and other renewables. Taken together, the results in Table 11 indicate that, the knowledge spillover effects of mobile labor markets enable firms to invest significantly more in abatement activities that help to reduce emissions.

[Insert Table 11 about here]

5. Conclusion

This study provides novel evidence of the role of labor mobility in enhancing corporate environmental performance, particularly emissions of air pollutants. To establish causality, we employ a DID approach by exploiting the (staggered) state-level changes in CNC enforceability that capture exogenous variations in labor mobility. Consistent with the hypothesis that knowledge spillovers created by a more mobile labor force boost green technologies and green investment and hence reduce air pollutant emissions, we find that weakened CNC enforcement indeed leads to lower corporate emissions. This effect is more pronounced for firms with greater reliance on skilled labor and intangible capital, as well as those firms experiencing lower levels of financial constraints and greater competitive pressure. Our analysis further shows that, following decreases in CNC enforceability, firms benefit from the knowledge spillover effects of greater labor mobility and experience a marked increase in their green innovation and green investment, thus providing further empirical support for the proposed economic channels.

This paper highlights the need to study how labor market frictions influence corporate environmental decisions and outcomes. Our evidence supports the view that more mobile labor markets foster green innovation and bring about tangible social and environmental benefits. In the face of the current climate crisis, it is thus important for policymakers to consider the benefits of relaxing labor mobility restrictions, which might help to tackle environmental issues and global warming through knowledge spillovers.

References

Acemoglu, D, Shimer, R, 1999. Holdups and efficiency with search frictions. *International Economic Review* 40 (4), 827-849.

Acharya, VV, Baghai, RP, Subramanian, KV, 2014. Wrongful discharge laws and innovation. *Review of Financial Studies* 27 (1), 301-346.

Agrawal, A, Cockburn, I, McHale, J, 2006. Gone but not forgotten: knowledge flows, labor mobility, and enduring social relationships. *Journal of Economic Geography* 6 (5), 571-591.

Akey, P, Appel, I, 2019. Environmental externalities of activism. Working Paper. University of Toronto, Toronto.

Akey, P, Appel, I, 2021. The limits of limited liability: Evidence from industrial pollution. *Journal of Finance* 76 (1), 5-55.

Alam, MS, Atif, M, Chien-Chi, C, Soytaş, U, 2019. Does corporate R&D investment affect firm environmental performance? Evidence from G-6 countries. *Energy Economics* 78, 401-411.

Almeida, P, Kogut, B, 1999. Localization of knowledge and the mobility of engineers in regional networks. *Management Science* 45 (7), 905-917.

Altunbas, Y, Gambacorta, L, Reghezza, A, Velliscig, G, 2022. Does gender diversity in the workplace mitigate climate change? Working Paper. University of Wales, Bangor.

Andersen, DC, 2017. Do credit constraints favor dirty production? Theory and plant-level evidence. *Journal of Environmental Economics and Management* 84, 189-208.

Angrist, JD, Pischke, J-S, 2008. Mostly harmless econometrics: An empiricist's companion. Princeton university press.

Aobdia, D, 2018. Employee mobility, noncompete agreements, product-market competition, and company disclosure. *Review of Accounting Studies* 23 (1), 296-346.

Azar, J, Duro, M, Kadach, I, Ormazabal, G, 2021. The big three and corporate carbon emissions around the world. *Journal of Financial Economics* 142 (2), 674-696.

Bai, JJ, Eldemire-Poindexter, A, Serfling, M, 2022. Growing pains: The effect of labor mobility on corporate investment over the business cycle. Working Paper. Northeastern University, Boston.

Baker, AC, Larcker, DF, Wang, CC, 2022. How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics* 144 (2), 370-395.

Barnett, J, Sichelman, TM, 2016. Revisiting labor mobility in innovation markets. Working Paper. University of Southern California, Los Angeles.

Bartram, SM, Hou, K, Kim, S, 2022. Real effects of climate policy: Financial constraints and spillovers. *Journal of Financial Economics* 143 (2), 668-696.

Bellon, A, 2021. Fresh start or fresh water: The impact of environmental lender liability. Working Paper. University of North Carolina at Chapel Hill, Chapel Hill.

Bellon, A, 2022. Does private equity ownership make firms cleaner? the role of environmental liability risks. Working Paper. University of North Carolina at Chapel Hill, Chapel Hill.

Bertrand, M, Duflo, E, Mullainathan, S, 2004. How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* 119 (1), 249-275.

Bertrand, M, Mullainathan, S, 2003. Enjoying the quiet life? Corporate governance and managerial preferences. *Journal of Political Economy* 111 (5), 1043-1075.

Bisetti, E, Lewellen, S, Sarkar, A, 2022. Smokestacks and the swamp. Working Paper. Hong Kong University of Science and Technology, Hong Kong.

Black, SE, Lynch, LM, 1996. Human-capital investments and productivity. *American Economic Review* 86 (2), 263-267.

Bodnaruk, A, Loughran, T, McDonald, B, 2015. Using 10-K text to gauge financial constraints. *Journal of Financial and Quantitative Analysis* 50 (4), 623-646.

Bosetti, V, Carraro, C, Massetti, E, Tavoni, M, 2008. International energy R&D spillovers and the economics of greenhouse gas atmospheric stabilization. *Energy Economics* 30 (6), 2912-2929.

Cengiz, D, Dube, A, Lindner, A, Zipperer, B, 2019. The effect of minimum wages on low-wage jobs. *Quarterly Journal of Economics* 134 (3), 1405-1454.

Chen, J, 2022. Creditor control of environmental activity: The role of liquidation value. Working Paper. Drexel University, Philadelphia.

Chen, S, Song, H, Wu, C, 2021. Human capital investment and firms' industrial emissions: Evidence and mechanism. *Journal of Economic Behavior & Organization* 182, 162-184.

Chen, T-Y, Zhang, G, Zhou, Y, 2018. Enforceability of non-compete covenants, discretionary investments, and financial reporting practices: Evidence from a natural experiment. *Journal of Accounting and Economics* 65 (1), 41-60.

Chen, Y, Lee, C-C, 2020. Does technological innovation reduce CO2 emissions? Cross-country evidence. *Journal of Cleaner Production* 263, 121550.

Choi, D, Gao, Z, Jiang, W, Zhang, H, 2021. Global carbon divestment and firms' actions. Working Paper. The Chinese University of Hong Kong, Hong Kong.

Choy, S, Jiang, S, Liao, S, Wang, E, 2022. Public environmental enforcement and private lender monitoring: Evidence from environmental covenants. Working Paper. University of Toronto, Toronto.

Chu, Y, Guo, SY, Zhao, D, Zheng, M, 2021. Political risk and toxic releases. Working Paper. University of North Carolina at Charlotte, Charlotte.

Chu, Y, Zhao, D, 2019. Green hedge fund activists. Working Paper. University of North Carolina at Charlotte, Charlotte.

Cici, G, Hendriock, M, Kempf, A, 2021. The impact of labor mobility restrictions on managerial actions: Evidence from the mutual fund industry. *Journal of Banking & Finance* 122, 105994.

Cohen, L, Gurun, UG, Nguyen, Q, 2022. The ESG-innovation disconnect: Evidence from green patenting. Working Paper. Harvard University, Cambridge.

Çolak, G, Korkeamäki, T, 2021. CEO mobility and corporate policy risk. *Journal of Corporate Finance* 69, 102037.

Conti, R, 2014. Do non-competition agreements lead firms to pursue risky R&D projects? *Strategic Management Journal* 35 (8), 1230-1248.

Currie, J, Zivin, JG, Mullins, J, Neidell, M, 2014. What do we know about short-and long-term effects of early-life exposure to pollution? *Annual Review of Resource Economics* 6 (1), 217-247.

Dai, R, Duan, R, Liang, H, Ng, L, 2022. Outsourcing climate change. Working Paper. University of Pennsylvania, Philadelphia.

Dang, VA, Gao, N, Yu, T, 2022. Climate policy risk and corporate financial decisions: Evidence from the NOx budget trading program. *Management Science* Forthcoming.

Dasgupta, S, Huynh, T, Xia, Y, 2021. Joining forces: The spillover effects of EPA enforcement actions and the role of socially responsible investors. Working Paper. The Chinese University of Hong Kong, Hong Kong.

Dechezleprêtre, A, Glachant, M, Haščič, I, Johnstone, N, Ménière, Y, 2020. Invention and transfer of climate change–mitigation technologies: A global analysis. *Review of Environmental Economics and Policy*.

Dechezleprêtre, A, Martin, R, Mohnen, M, 2017. Knowledge spillovers from clean and dirty technologies. Working Paper. London School of Economics, London.

Dell, M, Jones, BF, Olken, BA, 2014. What do we learn from the weather? The new climate-economy literature. *Journal of Economic Literature* 52 (3), 740-798.

Duflo, E, Greenstone, M, Pande, R, Ryan, N, 2013. Truth-telling by third-party auditors and the response of polluting firms: Experimental evidence from India. *Quarterly Journal of Economics* 128 (4), 1499-1545.

Eisfeldt, AL, Papanikolaou, D, 2013. Organization capital and the cross-section of expected returns. *Journal of Finance* 68 (4), 1365-1406.

Ewens, M, Marx, M, 2018. Founder replacement and startup performance. *Review of Financial Studies* 31 (4), 1532-1565.

Flammer, C, Kacperczyk, A, 2019. Corporate social responsibility as a defense against knowledge spillovers: Evidence from the inevitable disclosure doctrine. *Strategic Management Journal* 40 (8), 1243-1267.

Gao, M, Li, X, 2021. The environmental impact of green innovation. Working Paper. Peking University, Beijing.

Garmaise, MJ, 2011. Ties that truly bind: Noncompetition agreements, executive compensation, and firm investment. *Journal of Law, Economics, and Organization* 27 (2), 376-425.

Ghaly, M, Anh Dang, V, Stathopoulos, K, 2017. Cash holdings and labor heterogeneity: The role of skilled labor. *Review of Financial Studies* 30 (10), 3636-3668.

Gilson, RJ, 1999. The legal infrastructure of high technology industrial districts: Silicon Valley, Route 128, and covenants not to compete. *NYUl Rev.* 74, 575.

Goetz, MR, 2019. Financing conditions and toxic emissions. Working Paper. Deutsche Bundesbank.

Graff Zivin, J, Neidell, M, 2013. Environment, health, and human capital. *Journal of Economic Literature* 51 (3), 689-730.

Grinstein, Y, Larkin, Y, 2021. Minimizing costs, maximizing sustainability. Working Paper. IDC Herzliya, Herzliya.

Grossman, SJ, Hart, OD, 1986. The costs and benefits of ownership: A theory of vertical and lateral integration. *Journal of Political Economy* 94 (4), 691-719.

Haščič, I, Migotto, M, 2015. Measuring environmental innovation using patent data. Working Paper. OECD Environment Directorate, Paris.

He, Z, 2018. Money held for moving stars: Talent competition and corporate cash holdings. *Journal of Corporate Finance* 51, 210-234.

He, Z, Wintoki, MB, 2020. Who benefits from removing mobility restrictions? Working Paper. University of New Hampshire, Durham.

Heath, D, Macciocchi, D, Michaely, R, Ringgenberg, MC, 2021. Does socially responsible investing change firm behavior? Working Paper. University of Utah, Salt Lake City.

Heath, D, Ringgenberg, MC, Samadi, M, Werner, IM, 2022. Reusing natural experiments. *Journal of Finance* Forthcoming.

Heider, F, Ljungqvist, A, 2015. As certain as debt and taxes: Estimating the tax sensitivity of leverage from state tax changes. *Journal of Financial Economics* 118 (3), 684-712.

Heitz, A, Wang, Y, Wang, Z, 2021. Corporate political connections and favorable environmental regulatory enforcement. *Management Science* Forthcoming.

Henderson, R, Cockburn, I, 1996. Scale, scope, and spillovers: The determinants of research productivity in drug discovery. *RAND Journal of Economics* 27 (1), 32-59.

Hoberg, G, Phillips, G, 2016. Text-based network industries and endogenous product differentiation. *Journal of Political Economy* 124 (5), 1423-1465.

Hoberg, G, Phillips, G, Prabhala, N, 2014. Product market threats, payouts, and financial flexibility. *Journal of Finance* 69 (1), 293-324.

Hsu, P-H, Li, K, Tsou, C-Y, 2021. The pollution premium. *Journal of Finance* Forthcoming.

Huang, J, Jain, BA, Kini, O, 2019. Industry tournament incentives and the product-market benefits of corporate liquidity. *Journal of Financial and Quantitative Analysis* 54 (2), 829-876.

Iovino, L, Martin, T, Sauvagnat, J, 2021. Corporate taxation and carbon emissions. Working Paper. Bocconi University, Milan.

Jeffers, J, 2019. The impact of restricting labor mobility on corporate investment and entrepreneurship. Working Paper. University of Chicago, Chicago.

Johnson, MS, Lavetti, K, Lipsitz, M, 2021. The labor market effects of legal restrictions on worker mobility. Working Paper. Duke University, Durham.

Kaplan, SN, Strömberg, P, 2003. Financial contracting theory meets the real world: An empirical analysis of venture capital contracts. *Review of Economic Studies* 70 (2), 281-315.

Kim, I, Wan, H, Wang, B, Yang, T, 2019. Institutional investors and corporate environmental, social, and governance policies: Evidence from toxics release data. *Management Science* 65 (10), 4901-4926.

Kini, O, Williams, R, Yin, S, 2021. CEO noncompete agreements, job risk, and compensation. *Review of Financial Studies* 34 (10), 4701-4744.

Kogan, L, Papanikolaou, D, Seru, A, Stoffman, N, 2017. Technological innovation, resource allocation, and growth. *Quarterly Journal of Economics* 132 (2), 665-712.

Levine, R, Lin, C, Wang, Z, Xie, W, 2018. Bank liquidity, credit supply, and the environment. Working Paper. University of California at Berkeley, Berkeley

Li, F, Lin, C, Lin, T-C, 2021a. A one-two punch to the economy: Climate vulnerability and corporate innovation. Working Paper. Deakin University, Victoria.

Li, W, Xu, Q, Zhu, Q, 2021b. CEO hometown favoritism in corporate environmental policies. Working Paper. Shanghai University of Finance and Economics, Shanghai.

Lyu, X, Shan, C, Tang, DY, 2022. Corporate finance and firm pollution. Working Paper. Shanghai University of Finance and Economics, Shanghai.

Marx, M, 2011. The firm strikes back: Non-compete agreements and the mobility of technical professionals. *American Sociological Review* 76 (5), 695-712.

Marx, M, Singh, J, Fleming, L, 2010. Regional disadvantage? Non-compete agreements and brain drain. Working Paper. Cornell University, Ithaca.

Marx, M, Strumsky, D, Fleming, L, 2009. Mobility, skills, and the Michigan non-compete experiment. *Management Science* 55 (6), 875-889.

Naaraayanan, SL, Sachdeva, K, Sharma, V, 2021. The real effects of environmental activist investing. Working Paper. London Business School, London.

Parrotta, P, Pozzoli, D, 2012. The effect of learning by hiring on productivity. *RAND Journal of Economics* 43 (1), 167-185.

Peters, RH, Taylor, LA, 2017. Intangible capital and the investment-q relation. *Journal of Financial Economics* 123 (2), 251-272.

Popp, D, 2003. Pollution control innovations and the Clean Air Act of 1990. *Journal of Policy Analysis and Management* 22 (4), 641-660.

Qiu, B, Wang, T, 2018. Does knowledge protection benefit shareholders? Evidence from stock market reaction and firm investment in knowledge assets. *Journal of Financial and Quantitative Analysis* 53 (3), 1341-1370.

Rosenkopf, L, Almeida, P, 2003. Overcoming local search through alliances and mobility. *Management Science* 49 (6), 751-766.

Rubin, PH, Shedd, P, 1981. Human capital and covenants not to compete. *The Journal of Legal Studies* 10 (1), 93-110.

Samila, S, Sorenson, O, 2011. Noncompete covenants: Incentives to innovate or impediments to growth. *Management Science* 57 (3), 425-438.

Saxenian, A, 1996. Regional advantage: Culture and competition in silicon valley and route 128. Harvard University Press.

Shapiro, JS, Walker, R, 2018. Why is pollution from US manufacturing declining? The roles of environmental regulation, productivity, and trade. *American Economic Review* 108 (12), 3814-3854.

Shen, M, 2021. Skilled labor mobility and firm value: Evidence from green card allocations. *Review of Financial Studies* 34 (10), 4663-4700.

Shive, SA, Forster, MM, 2020. Corporate governance and pollution externalities of public and private firms. *Review of Financial Studies* 33 (3), 1296-1330.

Starr, E, Balasubramanian, N, Sakakibara, M, 2018. Screening spinouts? How noncompete enforceability affects the creation, growth, and survival of new firms. *Management Science* 64 (2), 552-572.

Starr, E, Prescott, JJ, Bishara, N, 2021. Noncompetes in the US labor force. *Journal of Law and Economics*.

Stern, N, 2008. The economics of climate change. *American Economic Review* 98 (2), 1-37.

Wang, Z, Yu, L, 2019. Are firms with female CEOs more environmentally friendly? Working Paper. The University of Hong Kong, Hong Kong.

Wasi, N, Flaaen, A, 2015. Record linkage using Stata: Preprocessing, linking, and reviewing utilities. *The Stata Journal* 15 (3), 672-697.

Wozniak, GD, 1987. Human capital, information, and the early adoption of new technology. *Journal of Human Resources*, 101-112.

Xiao, F, 2022. Non-competes and innovation: Evidence from medical devices. *Research Policy* 51 (6), 104527.

Xu, Q, Kim, T, 2022. Financial constraints and corporate environmental policies. *Review of Financial Studies* 35 (2), 576-635.

Table 1. Changes in CNC Enforcement

This table reports the CNC enforcement changes during our sample period from 2010 to 2019 obtained from Ewens and Marx (2018).

State	Year	Case	CNC Enforcement
Georgia	2010	Restrictive Covenants Act	Strengthened
South Carolina	2010	Invs, Inc. v. Century Builders of Piedmont, Inc.	Weakened
Colorado	2011	Lucht's Concrete Pumping, Inc. v. Horner	Strengthened
Illinois	2011	Reliable Fire Equipment Co. v. Arredondo	Strengthened
Texas	2011	Marsh v. Cook	Strengthened
New Hampshire	2012	N.H. Rev. Stat. Ann. Sec. 275-70	Weakened
Kentucky	2014	Creech v. Brown	Weakened

Table 2. Summary Statistics of the Main Variables

This table reports the summary statistics of the main variables used in our empirical analysis. The sample consists of AMPD plants and Compustat firms from 2010 to 2019. The sample used for the baseline results includes 5,240 plant-years and 575 firm-years. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars.

	N	Mean	P25	Median	P75	Std. Dev.
Emissions Variables (Plant-level)						
CO_2_R (short tons/$m)	5064	313.585	10.402	79.631	295.323	744.786
SO_2_R (tons/$m)	5147	0.408	0.000	0.001	0.122	1.422
NO_x_R (tons/$m)	5240	0.205	0.004	0.022	0.147	0.626
CO_2 (thousands of short tons)	5064	2152.169	115.430	894.519	2599.871	3218.835
SO_2 (thousands of tons)	5147	2.569	0.001	0.008	1.441	6.303
NO_x (thousands of tons)	5240	1.378	0.049	0.190	1.369	2.530
Control Variables (Firm- and Plant-level)						
Total Assets ($m)	575	37908.061	4502.248	15540.666	34104.500	75182.794
Leverage	575	0.408	0.315	0.409	0.490	0.160
Fixed Assets	575	0.615	0.559	0.666	0.744	0.180
Market-to-Book	575	1.247	1.101	1.197	1.338	0.230
ROA	575	0.091	0.076	0.086	0.104	0.031
Firm Age	575	52.972	54.000	60.000	63.000	17.684
State Unemployment Rate	5240	0.067	0.048	0.065	0.084	0.023
Log (State GDP per Capita)	5240	10.829	10.713	10.839	10.946	0.177
State GDP Growth	5240	0.030	0.020	0.036	0.047	0.029
Democratic Votes	5240	0.454	0.385	0.460	0.523	0.105

Table 3. Weaker CNC Enforcement and Emissions

This table reports the results from the OLS regressions relating plant emissions to weaker CNC enforcement during 2010–2019. The dependent variables are *Log (1+CO_2_R)*, *Log (1+SO_2_R)*, and *Log (1+NO_x_R)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that weakened CNC enforceability in a year, minus one if a plant is in a state that strengthened CNC enforceability in a year, and zero otherwise. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2_R)		Log (1+SO_2_R)		Log (1+NO_x_R)	
	(1)	(2)	(3)	(4)	(5)	(6)
Weaker CNC Enforcement	–0.254***	–0.181***	–0.144***	–0.126***	–0.040***	–0.034***
	(–4.85)	(–2.80)	(–5.71)	(–4.49)	(–5.25)	(–4.30)
Log Total Assets		–0.855***		–0.110***		–0.101***
		(–14.73)		(–4.38)		(–5.43)
Leverage		0.017		–0.079		–0.053
		(0.06)		(–0.66)		(–0.63)
Fixed Assets		0.302		–0.120		–0.069
		(0.84)		(–0.69)		(–0.68)
Market-to-Book		–0.745***		–0.210**		–0.119**
		(–2.70)		(–2.09)		(–2.36)
ROA		–0.695		–0.602		–0.517*
		(–0.76)		(–1.52)		(–1.74)
Firm Age		0.014***		0.003***		0.002***
		(3.42)		(4.12)		(2.78)
State Unemployment Rate		–3.766		0.623		–0.859
		(–1.31)		(0.65)		(–1.20)
Log (State GDP per Capita)		–1.155*		–0.460**		–0.301**
		(–1.82)		(–2.12)		(–2.46)
State GDP Growth		1.712***		0.476**		0.271***
		(3.28)		(2.06)		(3.13)
Democratic Votes		–0.460		0.231*		0.053
		(–1.41)		(1.80)		(1.02)
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,064	5,064	5,147	5,147	5,240	5,240
R-squared	0.924	0.938	0.836	0.844	0.877	0.890

Table 4. Hazard Model of Changes in CNC Enforcement

This table reports the results from a Cox proportional hazard model, where a "failure event" is a change in CNC enforcement in a state. The sample period is from 2010 to 2019. The observations of a state are excluded from the sample once the state changes its CNC enforceability. Independent variables are measured as of year *t*–1. Appendix A provides variable definitions. All the independent variables, except indicator variables, are normalized to have a mean of zero and a standard deviation of one. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*z*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
State CO_2 Emissions	0.905	0.818	0.699
	(1.64)	(1.55)	(1.20)
State SO_2 Emissions	0.069	0.322	0.307
	(0.18)	(0.94)	(0.74)
State NO_x Emissions	–0.571	–0.745	–0.645
	(–0.74)	(–0.93)	(–0.69)
Wrongful Discharge Laws		0.069	0.115
		(0.24)	(0.36)
State Union Membership		–0.477	–0.573
		(–1.38)	(–1.09)
State Unemployment Rate			0.506
			(0.56)
Log (State GDP per Capita)			0.614
			(0.79)
State GDP Growth			0.037
			(0.07)
Democratic Votes			–0.151
			(–0.28)
Year FE	Yes	Yes	Yes
Observations	389	386	386

Table 5. Weaker CNC Enforcement and

the Timing of Carbon Emissions Changes

This table reports the results from the OLS regressions relating plant carbon emissions to weaker CNC enforcement during 2010–2019. The dependent variable is *Log (1+*CO_2*_R)*. The variable *Weaker CNC Enforcement*$^{-2}$ is an indicator variable equal to one (minus one) if a plant is in a state that would weaken (strengthen) its CNC enforceability in two years and zero otherwise; *Weaker CNC Enforcement*$^{-1}$ is an indicator variable equal to one (minus one) if a plant is in a state that would weaken (strengthen) its CNC enforceability in one year and zero otherwise; *Weaker CNC Enforcement*^{0} is an indicator variable equal to one (minus one) if a plant is in a state that weakens (strengthens) its CNC enforceability in the current year, and zero otherwise; *Weaker CNC Enforcement*$^{+1}$ is an indicator variable equal to one (minus one) if a plant is in a state that weakened (strengthened) its CNC enforceability the year before, and zero otherwise; *Weaker CNC Enforcement*$^{2+}$ is an indicator variable equal to one (minus one) if a plant is in a state that weakened (strengthened) its CNC enforceability two or more years before and, zero otherwise. *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2_R)	
	(1)	(2)
Weaker CNC Enforcement^{-2}	0.257	0.005
	(1.00)	(0.03)
Weaker CNC Enforcement^{-1}	–0.011	–0.165
	(–0.11)	(–1.44)
Weaker CNC Enforcement^{0}	–0.235**	–0.300***
	(–2.38)	(–2.88)
Weaker CNC Enforcement^{+1}	–0.209***	–0.221**
	(–2.73)	(–2.13)
Weaker CNC Enforcement^{2+}	–0.292**	–0.289**
	(–2.53)	(–2.29)
Controls	No	Yes
Plant FE	Yes	Yes
Industry × Year FE	Yes	Yes
Observations	5,064	5,064
R-squared	0.924	0.938

Table 6. Robustness: Propensity Score Matched Sample

Panel A of this table tabulates the means of the propensity scores and the firm-level control variables across the treated and control groups for the propensity score matched sample. Panel B of reports the results from the OLS regressions relating plant carbon emissions to weaker CNC enforcement during 2010–2019. The dependent variable is *Log (1+CO_2_R)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that has weakened CNC enforceability, minus one if a plant is in a state that has strengthened CNC enforceability, and zero otherwise. For the matching procedure, we treat each year's CNC enforcement changes as a separate event and retain all firm-year observations one year before the event. We then estimate the propensity scores using a probit model for this sample, including the full set of firm-level controls (i.e., *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, and *Firm Age*). In addition, for each event we match each treated firm to a control firm selected within the same three-digit NAICS cluster, without replacement, and based on the closest propensity score (within 0.1). *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Covariate Balance of the Matched Sample

	Control Group (Obs.=90)	Treated Group (Obs.=90)	Mean Difference	*p*-value
Propensity Score	0.239	0.241	–0.002	0.849
Log Total Assets	10.033	9.925	0.108	0.304
Leverage	0.536	0.542	–0.006	0.775
Fixed Assets	0.627	0.649	–0.022	0.259
Market-to-Book	1.035	1.044	–0.009	0.668
ROA	0.085	0.088	–0.003	0.445
Firm Age	43.278	43.733	–0.456	0.885

Panel B: DID Regression Using Matched Sample

	Log (1+CO_2_R)	
	(1)	(2)
Weaker CNC Enforcement	–0.208***	–0.144**
	(–3.72)	(–2.36)
Controls	No	Yes
Plant FE	Yes	Yes
Industry × Year FE	Yes	Yes
Observations	1,437	1,437
R-squared	0.949	0.958

Table 7. Robustness: Using Samples Based on Neighboring States or Firm Headquarters Locations, and Using Stacked Regressions

This table reports the results from the OLS regressions relating plant carbon emissions to weaker CNC enforcement during 2010–2019. The dependent variable is *Log (1+*CO_2*_R)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that weakened CNC enforceability, minus one if a plant is in a state that strengthened CNC enforceability, and zero otherwise. In columns (1) to (2), we include plants located both in the states with CNC enforcement changes and their neighboring states. In columns (3) to (4), we redefine *Weaker CNC Enforcement*, using firm headquarters, as an indicator variable equal to one if a firm is headquartered in a state that weakened CNC enforceability, minus one if a firm is headquartered in a state that strengthened CNC enforceability, and zero otherwise. In columns (5) to (6), we use the stacked regression approach to estimate the staggered DID model (Baker et al., 2022). *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2_R)					
	Neighboring States		Headquarters Locations		Stacked Regressions	
	(1)	(2)	(3)	(4)	(5)	(6)
Weaker CNC Enforcement	–0.238***	–0.167**	–0.221*	–0.161**	–0.257***	–0.204***
	(–4.30)	(–2.62)	(–1.89)	(–2.25)	(–4.64)	(–3.39)
Controls	No	Yes	No	Yes	No	Yes
Plant FE	Yes	Yes	Yes	Yes	No	No
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Plant × Cohort FE	No	No	No	No	Yes	Yes
Year × Cohort FE	No	No	No	No	Yes	Yes
Observations	4,458	4,458	5,026	5,026	29,904	29,904
R-squared	0.923	0.939	0.937	0.941	0.925	0.938

Table 8. Moderating Effects of Labor Skills and Intangible Capital

This table reports the results from the OLS regressions relating plant carbon emissions to weaker CNC enforcement during 2010–2019. The dependent variable is *Log (1+CO_2_R)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that weakened CNC enforceability, minus one if a plant is in a state that strengthened CNC enforceability, and zero otherwise; *High Labor Skills* is an indicator variable that is one if a plant has an above-median value of the labor skills measure computed following Ghaly et al. (2017), and zero otherwise; *High Knowledge Capital* is an indicator variable that is one if a firm has an above-median value of knowledge capital scaled by the book value of total assets (Peters and Taylor, 2017), and zero otherwise; *High R&D Expense* is an indicator variable equal to one if a firm has an above-median value of R&D expense scaled by the book value of total assets, and zero otherwise; *High SG&A Expense* is an indicator variable equal to one if a firm has an above-median value of SG&A expense scaled by the book value of total assets, and zero otherwise. Each specification includes all interaction terms, though some interaction terms are not tabulated for brevity. *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2_R)			
	(1)	(2)	(3)	(4)
Weaker CNC Enforcement × High Labor Skills	–0.086**			
	(–2.32)			
Weaker CNC Enforcement × High Knowledge Capital		–0.373*		
		(–1.96)		
Weaker CNC Enforcement × High R&D Expense			–0.574***	
			(–4.29)	
Weaker CNC Enforcement × High SG&A Expense				–0.281*
				(–1.73)
Controls	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	3,899	4,810	4,779	4,779
R-squared	0.955	0.939	0.949	0.950

Table 9. Moderating Effects of Financial Constraints and Product Market Competition

This table reports the results from the OLS regressions relating plant carbon emissions to weaker CNC enforcement during 2010–2019. The dependent variable is *Log (1+CO_2_R)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that weakened CNC enforceability, minus one if a plant is in a state that strengthened CNC enforceability, and zero otherwise; *Low Financial Constraints* is an indicator variable equal to one if a firm has a below-median value of the financial constraints measure (Bodnaruk et al., 2015), and zero otherwise; *High Product Fluidity* is an indicator variable equal to one if a firm has an above-median value of product fluidity (Hoberg et al., 2014), and zero otherwise; *High Product Similarity* is an indicator variable equal to one if a firm has an above-median value of product similarity (Hoberg and Phillips, 2016), and zero otherwise. Each specification includes all interaction terms, though some interaction terms are not tabulated for brevity. *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2_R)		
	(1)	(2)	(3)
Weaker CNC Enforcement × Low Financial Constraints	–0.188*		
	(–1.88)		
Weaker CNC Enforcement × High Product Fluidity		–0.262*	
		(–1.80)	
Weaker CNC Enforcement × High Product Similarity			–0.210**
			(–2.06)
Controls	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Observations	4,681	4,550	4,556
R-squared	0.950	0.949	0.949

Table 10. Weaker CNC Enforcement and Green Innovation

This table reports the results from the OLS regressions relating corporate green innovation to weaker CNC enforcement during 2010–2019. The dependent variables are *Log (1+Green Patent)* and *Log (1+Green Citation)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a firm is headquartered in a state that has decreased CNC enforceability, minus one if a firm is headquartered in a state that has increased CNC enforceability, and zero otherwise. In columns (1) and (3) (columns (2) and (4)), we identify green patents following Li et al. (2021a) (Cohen et al. (2022)). *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+Green Patent)		Log (1+Green Citation)	
	(1)	(2)	(3)	(4)
Weaker CNC Enforcement	0.110**	0.136**	0.202***	0.195**
	(2.12)	(2.29)	(2.80)	(2.64)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Observations	458	458	458	458
R-squared	0.954	0.956	0.882	0.864

Table 11. Weaker CNC Enforcement and Green Investment

This table reports the results from the OLS regressions relating plants' green investment to weaker CNC enforcement during 2010–2019. The dependent variables are *Scrubber Dummy*, *Log (1+Coal Quantities)*, *Log (1+Petroleum Quantities)*, and *Pct. Clean Energy*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that weakened CNC enforceability, minus one if a plant is in a state that strengthened CNC enforceability, and zero otherwise. *Firm Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. In Column (1), *Plant Controls* include *Net Generation*, whereas in Columns (2) to (4), *Plant Controls* include *Scrubber Dummy* and *Net Generation*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Scrubber Dummy	Log (1+Coal Quantities)	Log (1+Petroleum Quantities)	Pct. Clean Energy
Weaker CNC Enforcement	0.042***	–0.895***	–0.481**	0.039**
	(4.55)	(–6.08)	(–2.14)	(2.38)
Firm Controls	Yes	Yes	Yes	Yes
Plant Controls	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Observations	5,187	5,200	5,200	5,199
R-squared	0.914	0.923	0.909	0.953

Appendix A. Variable Definitions

Variable	Description (For definitions of the data items in parentheses, refer to Compustat designations)
Emissions	
CO_2	Carbon dioxide emissions in thousands of short tons, obtained from the Air Markets Program Data (AMPD).
CO_2_R	CO_2 emissions defined above scaled by firm total revenue (revt) in millions of dollars.
NO_x	Nitrogen oxide emissions in thousands of metric tons, obtained from the AMPD.
NO_x_R	NO_x emissions defined above scaled by firm total revenue (revt) in millions of dollars.
SO_2	Sulfur dioxide emissions in thousands of metric tons, obtained from the AMPD.
SO_2_R	SO_2 emissions defined above scaled by firm total revenue (revt) in millions of dollars.
Control, Conditioning, and Other Dependent Variables	
Acquisition Expenditures	The ratio of acquisition expenditures (aqc) to total assets (at).
Capital Expenditures	The ratio of capital expenditures (capx) to total assets (at).
Coal Quantities	The total amount (in short tons) of coal a plant uses in a year. The data are obtained from the Energy Information Administration (EIA) form EIA-923.
Firm Age	The number of years since a firm first appeared in the Compustat database.
Fixed Assets	The ratio of property, plant, and equipment (ppent) to book value of total assets (at).
Green Citation	The number of forward citations of a firm's green patents. We identify green patents following the methods of Li et al. (2021a) and Cohen et al. (2022), respectively.
Green Patent	The number of successful green patent applications filed by a firm. We identify green patents following the methods of Li et al. (2021a) and Cohen et al. (2022), respectively.
High Knowledge Capital	An indicator variable equal to one if a firm has an above-median value of knowledge capital scaled by the book value of total assets (Peters and Taylor, 2017), and zero otherwise.
High Labor Skills	An indicator variable equal to one if a plant has an above-median value of labor skills measured following Ghaly et al. (2017), and zero otherwise.
High Product Fluidity	An indicator variable equal to one if a firm has an above-median value of the text-based measure of product market fluidity (Hoberg et al., 2014), and zero otherwise. The product market fluidity measure is obtained from the Hoberg-Phillips Data Library.
High Product Similarity	An indicator variable equal to one if a firm has an above-median value of the text-based measure of product similarity (Hoberg and Phillips, 2016), and zero otherwise. The product similarity measure is obtained from the Hoberg-Phillips Data Library.
High R&D Expense	An indicator variable equal to one if a firm has an above-median value of R&D expense scaled by the book value of total assets, and zero otherwise.
High SG&A Expense	An indicator variable equal to one if a firm has an above-median value for SG&A expense scaled by the book value of total assets, and zero otherwise.
KLD Environment Index	The total number of strengths regarding the natural environment obtained from the MSCI ESG KLD STATS (formerly KLD) database.

KLD Index	The total number of strengths regarding employees, customers, the natural environment, and communities obtained from the MSCI ESG KLD STATS (formerly KLD) database.
Leverage	The book value of long-term debt (dltt) plus debt in current liabilities (dlc) divided by market value of debt and equity (i.e., long-term debt (dltt) plus debt in current liabilities (dlc) plus market value of equity (prcc_f * csho)).
Log Total Assets	The natural logarithm of the book value of total assets (at) in millions of 1999 dollars.
Low Financial Constraints	An indicator variable equal to one if a firm has a below-median value of a text-based measure of financial constraints (Bodnaruk et al., 2015), and zero otherwise.
Market-to-Book	Book value of liabilities (dlc + dltt + pstkl) plus market value of equity (prcc_f * csho) divided by total assets (at).
Net Generation	The total amount (in Megawatt hour) of electricity a plant generated. The data are obtained from the Energy Information Administration (EIA) form EIA-923.
Net KLD Environment Index	An index calculated by subtracting the total number of concerns from the total number of strengths regarding the natural environment obtained from MSCI ESG KLD STATS (formerly KLD) database.
Net KLD Index	An index calculated by subtracting the total number of concerns from the total number of strengths regarding employees, customers, the natural environment, and communities obtained from the MSCI ESG KLD STATS (formerly KLD) database.
Pct. Clean Energy	The fraction of the amount of clean energy (e.g., natural gas solar, and other renewables) to total energy a plant uses in a year. The data are obtained from the Energy Information Administration (EIA) form EIA-923.
Democratic Votes	The fraction of a state's general-election votes for the Democratic Party in a year.
Petroleum Quantities	The total amount (in barrels) of petroleum a plant uses in a year. The data are obtained from the Energy Information Administration (EIA) form EIA-923.
R&D Expenses	Research and development expenses (xrd) as a proportion of total assets (at).
R&D Dummy	An indicator variable set to one if a firm did not report research and development expenses (xrd), else zero.
ROA	Operating income before depreciation (oibdp) divided by the book value of total assets (at).
Scrubber Dummy	An indicator variable set to one if a plant has at least one flue-gas desulfurization (FGD) unit in operation in a year, else zero. The data are obtained from the Energy Information Administration (EIA) form EIA-923.
State GDP Growth	The annual GDP growth rate of a state over a year.
State CO_2 Emissions	Total carbon emissions of a state in a year (in thousands of short tons) obtained from the Air Markets Program Data (AMPD).
State GDP Per Capita	A state's GDP (in million dollars) divided by its total population in a year.
State NO_x Emissions	Total NO_x emissions of a state in a year (in thousands of metric tons) obtained from the Air Markets Program Data (AMPD).
State SO_2 Emissions	Total SO_2 emissions of a state in a year (in thousands of metric tons) obtained from the Air Markets Program Data (AMPD).
State Unemployment Rate	The unemployment rate of a state in a year.
State Union Membership	The percentage of workers covered by a collective bargaining agreement in a state in a year.
Wrongful Discharge Laws	The number of wrongful discharge laws (i.e., the good faith exception, the implied contract exception, and the public policy exception) that a state has recognized in a year.

Online Appendix for

"Labor Mobility and Corporate Environmental Performance"

Online Appendix OA1. Matching Data from AMPD and Compustat

To merge the data retrieved from AMPD and Compustat, we apply a fuzzy-matching procedure using company names. First, to improve matching accuracy, we retrieve historical company names from the Center for Research in Security Prices (CRSP) database and the COMPHIST file provided by the CRSP-Compustat merged database (Dang et al., 2022; Xu and Kim, 2022). Second, we follow recent literature (e.g., Akey and Appel, 2021; Hsu et al., 2021; Xu and Kim, 2022) to standardize the company names for AMPD and Compustat firms. Specifically, we convert company names into upper case and remove all punctuation, special characters, and corporate designators (e.g., “LLC”, “INC”, “CORP”, “COMPANY”, “CORPORATION”). Third, based on the standardized company names, we use a Stata record-linking algorithm (*reclink2)*, developed by Wasi and Flaaen (2015) to produce matching scores for all possible company-name pairs between the AMPD and Compustat. For each match, we retain the five pairs having the highest matching scores and, at the same time, require these scores to be equal to or above 0.95. Finally, we manually check all the retained pairs to determine the most appropriate matches.

Figure OA1. Distribution of the *T*-statistics of the Coefficient in Placebo Tests

This figure plots the distribution of the *t*-statistics of the coefficient on the variable *Weaker CNC Enforcement* generated from the placebo tests that randomize the assignment of CNC enforcement changes to states (sampled without replacement) and years. We estimate the OLS regressions relating plant carbon emissions (*Log (1+CO_2_R)*) to the pseudo CNC-enforcement-change events during 2010–2019. We repeat this procedure for 5,000 times, store the *t*-statistics of coefficient estimates, and plot their distribution below. The red vertical line represents the *t*-statistic generated from our baseline regression.

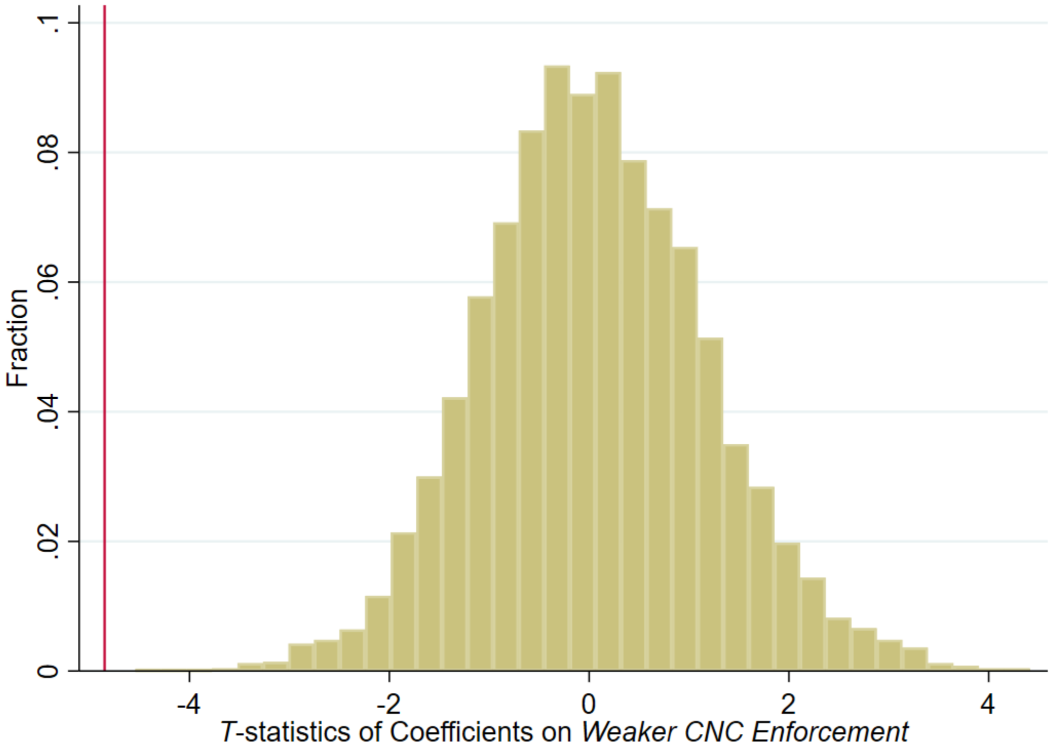

Table OA1. Robustness: Baseline Results with Different Fixed Effects

This table reports the results from the OLS regressions relating plant emissions to weaker CNC enforcement during 2010–2019. The dependent variables are *Log (1+CO_2_R)*, *Log (1+SO_2_R)*, and *Log (1+NO_x_R)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that has decreased CNC enforceability, minus one if a plant is located in a state that has increased CNC enforceability, and zero otherwise. *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2_R)		Log (1+SO_2_R)		Log (1+NO_x_R)	
	(1)	(2)	(3)	(4)	(5)	(6)
Weaker CNC Enforcement	–0.264***	–0.197***	–0.145***	–0.129***	–0.037***	–0.034***
	(–5.16)	(–3.17)	(–5.84)	(–4.60)	(–4.44)	(–3.98)
Controls	No	Yes	No	Yes	No	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,118	5,118	5,196	5,196	5,311	5,311
R-squared	0.924	0.938	0.836	0.844	0.876	0.889

Table OA2. Weaker CNC Enforcement and Emission Levels

This table reports the results from the OLS regressions relating plant emission levels to weaker CNC enforcement for during 2010–2019. The dependent variables are *Log (1+*CO_2*)*, *Log (1+*SO_2*)*, and *Log (1+*NO_x*)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that has weakened CNC enforceability, minus one if a plant is in a state that has increased CNC enforceability, and zero otherwise. *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2)		Log (1+SO_2)		Log (1+NO_x)	
	(1)	(2)	(3)	(4)	(5)	(6)
Weaker CNC Enforcement	–0.231***	–0.192***	–0.478***	–0.426**	–0.218***	–0.212***
	(–3.46)	(–3.60)	(–3.61)	(–2.27)	(–5.59)	(–3.52)
Controls	No	Yes	No	Yes	No	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,064	5,064	5,147	5,147	5,240	5,240
R-squared	0.923	0.924	0.932	0.932	0.918	0.919

Table OA3. Robustness: Controlling for CSR Scores

This table reports the results from the OLS regressions relating plant carbon emissions to weaker CNC enforcement during 2010–2019. The dependent variable is *Log (1+CO_2_R)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that has decreased CNC enforceability, minus one if a plant is in a state that has increased CNC enforceability, and zero otherwise. *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1^{st} and 99^{th} percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2_R)			
	(1)	(2)	(3)	(4)
Weaker CNC Enforcement	–0.163***	–0.180***	–0.165***	–0.167**
	(–2.73)	(–2.87)	(–2.71)	(–2.52)
KLD Index	–0.028**			
	(–2.45)			
Net KLD Index		–0.024**		
		(–2.11)		
KLD Environment Index			–0.061**	
			(–2.57)	
Net KLD Environment Index				–0.027**
				(–2.06)
Controls	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Observations	4,433	4,433	4,433	4,433
R-squared	0.941	0.941	0.941	0.941

Table OA4. Robustness: Controlling for Additional Variables

This table reports the results from the OLS regressions relating plant carbon emissions to weaker CNC enforcement 2010–2019. The dependent variable is *Log (1+CO_2_R)*. The variable *Weaker CNC Enforcement* is an indicator variable equal to one if a plant is in a state that has decreased CNC enforceability, minus one if a plant is located in a state that has increased CNC enforceability, and zero otherwise. *Controls* include *Log Total Assets*, *Leverage*, *Fixed Assets*, *Market-to-Book*, *ROA*, *Firm Age*, *State Unemployment Rate*, *Log (State GDP per Capita)*, *State GDP Growth*, and *Democratic Votes*. The coefficients on all the control variables are omitted for brevity. Appendix A provides variable definitions. The continuous variables, except macroeconomic ones, are winsorized at their 1st and 99th percentiles. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log (1+CO_2_R)			
	(1)	(2)	(3)	(4)
Weaker CNC Enforcement	–0.181***	–0.174**	–0.196***	–0.191***
	(–2.83)	(–2.59)	(–3.15)	(–3.00)
R&D Expenses	5.007			19.978
	(0.12)			(0.37)
R&D Dummy	–0.078			–0.005
	(–0.11)			(–0.01)
Capital Expenditures		1.080		1.147
		(1.01)		(1.08)
Acquisition Expenditures			–0.487	–0.398
			(–1.49)	(–1.25)
Controls	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Observations	5,064	5,064	4,885	4,885
R-squared	0.938	0.938	0.938	0.938