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# Take a ride on the (not so) green side: How do CDM projects affect Indian manufacturing firms' environmental performance?

Jūratė Jaraitė,<sup>\*†</sup> Oliwia Kurtyka,<sup>‡</sup> and H el ene Ollivier<sup> </sup>

# Take a ride on the (not so) green side: How do CDM projects affect Indian manufacturing firms' environmental performance?

## Abstract

This study examines the causal impacts of the Clean Development Mechanism (CDM) on the environmental performance of Indian manufacturing firms, as measured by their energy use, carbon dioxide (CO<sub>2</sub>) emissions, and intensities of CO<sub>2</sub> emissions per sales and per energy use. The impacts of CDM projects are estimated using either two-way fixed-effect regressions or an estimator built for an event study with staggered treatment (Sun & Abraham, 2021) combined with a sample of ever-treated firms only or a sample comparing treated to never-treated control firms using semi-parametric matching. We found that CDM projects significantly increased firms' CO<sub>2</sub> emissions and energy use after treatment, but had no effect on CO<sub>2</sub> emission intensity per sales and only a small negative effect on the CO<sub>2</sub> content of energy use (only for the matched sample). These results reveal that CDM projects led to a positive scale effect (increased sales) after investments were made, and that these investments triggered a limited emission-reducing technique effect (decreased CO<sub>2</sub> intensity).

*Keywords:* carbon offsets, CDM projects, CO<sub>2</sub> emissions, firms' environmental performance, India, micro level data

*JEL codes:* D22, Q53, Q54, Q58

# 1 Introduction

It is widely expected that future growth in greenhouse gas (GHG) emissions over the next century will be dominated by developing countries.<sup>1</sup> Thus, finding effective means of curbing GHG emissions in these countries is crucial for combating climate change. One of the principal mechanisms in place today for reducing emissions in developing countries is the Clean Development Mechanism (CDM), which enables firms in developing countries to sell certified emission reduction (CER) credits to buyers from developed countries, thereby reducing the effective cost of green investments for these firms.<sup>2</sup> All else equal, lower cost of green investment should increase green-technology adoption and lower emission intensity. This corresponds to the “technique effect” described in the literature on trade/growth and the environment (Copeland & Taylor, 2003; Forslid et al., 2018; Barrows & Ollivier, 2021). However, green technology adoption may also lower marginal costs of production, thereby raising output. This “scale effect”, as it is known in the literature, would tend to increase emissions. Hence, the causal effect of the CDM on emissions levels from developing-country firms is *a priori* ambiguous.

In this paper, we conduct the first *ex-post* analysis of the CDM’s contribution to emission reductions at the firm level. To do this, we link the registry of CDM projects in a single country – India – to detailed manufacturing information that allows us to track CDM-participating and CDM-non-participating firms over a long time horizon. India is an excellent case study for two reasons. First, India has hosted roughly a quarter of CDM projects world-wide, encompassing a wide variety of investments in renewable energy (mainly biomass, wind, and solar) or in energy efficiency technologies. Second, due to an unusual reporting requirement from the Indian Companies Act, firms must report their energy consumption in quantity, which allows us to compute CO<sub>2</sub> emissions by multiplying self-reported energy usage statistics by CO<sub>2</sub> content emission factors of different energy types and summing over energy types (Barrows & Ollivier, 2018; Forslid et al., 2018; Marin & Vona, 2021). From the CDM registry, we merge 294 firms to annual input and output data from the Prowess Indian manufacturing dataset over the period 1992-2015.<sup>3</sup>

We face two main challenges in identifying the causal effect of the CDM. First, participation is voluntary, and hence, non-random. It is possible that unobserved determinants of CO<sub>2</sub> emissions correlates with CDM participation. Second, firms enter the CDM at different times during the sample. Given technological growth, an investment made in

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<sup>1</sup>To illustrate, CO<sub>2</sub> emissions from fuel combustion in India and China increased by 335% and 351%, respectively, from 1990 to 2018 (authors’ calculations based on IEA data from [iea.org](http://iea.org)).

<sup>2</sup>To obtain CER credits, the investing firm had to prove that a CDM project will reduce GHG emissions relative to a counterfactual that would occur in the absence of investment. In other words, the firm had to demonstrate the additionality of the project based on ex-ante calculations. See the UNFCCC website for details about the CDM: <https://unfccc.int/process-and-meetings/the-kyoto-protocol/mechanisms-under-the-kyoto-protocol/the-clean-development-mechanism>.

<sup>3</sup>The Prowess dataset has been used in many economic and environmental studies of the Indian manufacturing sector (Goldberg et al., 2010; De Loecker et al., 2016; Barrows & Ollivier, 2021). The other manufacturing database in India, the Annual Survey of Industries, run by the Indian government each year does not contain the names of firms, which impedes the merging of datasets using company names.

2000 may exploit different technological opportunities relatively to an investment made in 2013 for instance. We thus worry that the standard two-way fixed effect regression that includes leads and lags of the treatment may suffer from a contamination issue that arises from the fact that investing at different times may result in heterogeneous effects on firms' outcomes (Borusyak & Jaravel, 2018; Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfoeuille, 2020; Sun & Abraham, 2021; Goodman-Bacon, 2021). To address non-random selection, we adopt two strategies. First, we restrict the sample to ever-treated firms – that is, firms that ever participate in the CDM during the period 1992-2015 – and evaluate firm-level outcomes in an event study setting. In this exercise, we compare firms that have already received treatment to not-yet-treated firms. Second, we match CDM-participating firms to non-CDM-participating firms based on pre-treatment characteristics. This alternative allows us to use a larger pool of control firms, given the small number of treated firms present in our sample. In both cases, we also implement the estimation strategy from Sun & Abraham (2021), which corrects two-way fixed effect regression for contamination issues stemming from cohort-specific dynamic treatment effects. Essentially, our event-study estimates are constructed as a weighted sum of cohort-specific dynamic treatment effects, where cohorts are defined by the years of CDM registration date and where firms belonging to the last cohort serve as controls.

Our analysis indicates that CDM projects did *not* reduce the CO<sub>2</sub> emission levels of participating firms, as emissions increased after the registration of a CDM project. However, the effect on other firm-level outcomes is not uniform across the different empirical approaches. In the sample of ever-treated firms and in the preferred econometric specification that is free of contamination and that controls for firm fixed effects and sector-period fixed effects, firms hosting CDM projects increased their CO<sub>2</sub> emissions and energy use by 15% and 25%, respectively, in the 4-year period after registration. They also engaged in some investments, since their capital stock increased by 14% during that period, and since they produced more electricity on site, especially from renewable energy, rather than buying it from the grid. Sales increased by 16% in the 4-year period after registration, revealing a strong scale effect. By contrast, firms' emission intensity, that is the amount of CO<sub>2</sub> per unit of sales, and the CO<sub>2</sub> content of energy consumption – two measures of the technique effect – were left unaffected by CDM projects. In the matched sample, using similar econometric specifications, we find that participating firms increased their CO<sub>2</sub> emissions and energy use by 32% and 6%, respectively, in the 4-year period after registration. We also validate that capital stock, the share of self-generated electricity, and sales increased after registering a CDM project. But in this setting, we find that the CO<sub>2</sub> content in energy consumption significantly decreased after the project is implemented, whereas the emission intensity in sales remained unaffected.

A mix of evidence obtained with a standard two-way fixed effect regression and with the estimator proposed by Sun & Abraham (2021) to accommodate for heterogeneous dynamic effects depending on the years of registration of CDM projects allows us to cautiously conclude that the scale effect that pushes CO<sub>2</sub> emissions upward after firms made a CDM investment seems to dominate the technique effect that should have lowered the emission intensity of firms' production and energy use. Overall, our results tend to support the

pessimistic view that CDM projects did not help Indian firms mitigate their CO<sub>2</sub> emissions.

This paper contributes to the literature on the economic and environmental evaluation of CDM projects.<sup>4</sup> Early studies based on descriptive analyses of Project Design Documents (PDD) estimated that over 40% of registered CDM projects and 20% of CER credits generated lacked additionality (Michaelowa & Purohit, 2007; Schneider, 2007). Here, the debate over additionality was framed as follows: would firms have invested in emission abatement without the CDM program? If the answer was yes, the project was deemed not to be additional. Investments in large-scale wind and hydropower projects are at the center of this debate because other regulations (e.g., feed-in tariffs, fiscal incentives) played an important role in fostering these investments (He & Morse, 2013; Erickson et al., 2014; Sawhney & Rahul, 2014; Trotter et al., 2015). Several studies assessed the environmental impacts of CDM participation at the macro or regional levels.<sup>5</sup> For instance, Huang & Barker (2012) found that CDM participation at the country level significantly reduced total CO<sub>2</sub> emissions per capita (by 1.31%) and especially those from the manufacturing and energy sectors (by 3-8%). By contrast, Zhang et al. (2018) showed that CDM projects did not improve the overall energy efficiency of sixteen countries hosting CDM projects during the 1990-2015 period. Finally, Zhang & Wang (2011) showed that sulphur dioxide (SO<sub>2</sub>) emissions did not decline in Chinese prefectures that undertook CDM projects, and assuming complementarity between CO<sub>2</sub> and SO<sub>2</sub> emissions due to co-generation, they inferred that CO<sub>2</sub> emissions were unlikely to decline in the same prefectures.

To the best of our knowledge, our paper is the first to evaluate ex post the effects of CDM projects using firm-level data. The closest work to ours is a study by Dechezleprêtre et al. (2014) investigating the additionality of wind power CDM projects in India at the project level. The authors compared the profitability of investing in wind energy production in a CDM project or without the CDM endorsement by matching projects with similar characteristics, since some wind power projects failed to receive financial support from the CDM program. Dechezleprêtre et al. (2014) found that the majority of wind power CDM projects were not additional. Compared to this study, our paper evaluates all types of CDM projects within the manufacturing sector, focuses on the company level instead of the project level, and characterizes firms' overall environmental performance instead of the profitability of a project.

Finally, this paper is related to the literature on the effects of policy and environmental shocks on technological investments, innovation, and productivity at the firm level. In particular, Bustos (2011) showed that technological upgrading is made profitable by the increase in revenues induced by a larger market access. In the context of Indian manufacturing firms, Allcott et al. (2016) estimated that, given the loss in revenues and producer surplus of 5 to 10% suffered by the average plant due to average reported level of electricity shortages, investing in a power generator allowed firms to reduce the costs from shortages and improve their productivities. In the same way we find limited evidence of

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<sup>4</sup>For an overview of the literature on CDM projects, we refer the reader to Spalding-Fecher et al. (2012).

<sup>5</sup>Many papers study the impact of the CDM on sustainable development goals in hosting countries (see, e.g., Mori-Clement 2019; Du & Takeuchi 2019), but we will not present this literature as it lies outside of the scope of our paper.

technique effect, Calel (2020) found that British firms did not respond to the European carbon market by decreasing carbon intensity of output, but rather by innovating more as shown in greater low-carbon patenting and R&D spending. In the recent U.S. context, Acemoglu et al. (2019) revealed that the shale gas boom had counteracting effects on CO<sub>2</sub> emissions: it allowed countries to substitute away from coal, which reduced emissions, but it also increased the scale of aggregate production, which may increase emissions. This generates the same ambiguous net effect on CO<sub>2</sub> emissions as in the case of CDM projects, but the shock is technological progress in shale gas extraction, not an offset program.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting of the CDM and the distribution of CDM projects in India over time and across sectors and states, and presents the event study research design. Section 3 describes the data and the samples used for the analysis. Section 4 presents the main estimation results and robustness tests, while Section 5 discusses the conclusions drawn.

## 2 Evaluating the Clean Development Mechanism in India

Identifying the causal effects of CDM projects at the firm level faces three challenges. First, the nature of technological investments imply that firms undertake a transformation of their production process that depends on the technologies available at the time of the investment. For example, a firm investing in 2000 may face different technological opportunities than a firm investing in 2010 or 2015. Technological progress is specific to each industry, and it may not be linear. As a result, comparing the effects of CDM projects undertaken at different times needs to account for this technological progress.

Second, the induced changes in emission intensity, emission levels, and revenues may not materialize immediately when technological investments are made. These dynamic treatment effects need to be measured relative to the investment timing, which is specific to each firm. This is often done in a two-way fixed-effect regression that includes leads and lags of the treatment. Yet, this standard method may not be valid when there are heterogeneous dynamic effects. In our context, heterogeneity would arise from the different timing of treatment across firms, since the set of available technologies is not the same at different times. As a result, we need to adopt an empirical strategy that is robust to dynamic effects heterogeneity.

Third, participation is voluntary. If a firm participates into an emission-reduction project under the CDM, it can obtain saleable CER credits after going through several stages of validation (CDM Executive Board, 2008). These stages can be summarized into three steps. First, the firm should receive a letter of approval from a Designated National Authority, stating that the project helps the host country achieve its sustainable development goals. Second, the firm should have its Project Design Document validated by a Designated Operational Entity, which acknowledges that the requirements of the CDM program are met. Third, the project should be registered by the CDM Executive Board.

The project may be withdrawn or rejected at any stage of the process.<sup>6</sup> Revisions may be required to comply with the requirements of the CDM. On average, the registration process takes 18 months starting from the publication of the PDD. Due to this voluntary participation, identifying a group of “control” firms whose outcomes can be compared to the ones of the “treated” firms is challenging.

Furthermore, understanding the driving forces behind participation is difficult. There are many forces that can play an important role, including generating revenues from the sale of CER credits, generating revenues from additional sales of electricity generated through CDM projects, or gains in cost reductions from CDM-induced technological upgrading (Lütken, 2012). Firms that undertake R&D activities and employ engineers who focus on energy efficiency improvements are especially able to identify opportunities for CDM projects (Schneider et al., 2009; Phillips et al., 2013). Similarly, large firms that tend to export and/or have multinational affiliates may also have more information about suitable CDM investments (Dechezleprêtre et al., 2008, 2009; Phillips et al., 2013). Also, large companies that have better access to capital are more likely to undertake massive CDM projects (Schneider et al., 2009; Pulver et al., 2010; Arimura et al., 2012). Additionally, participating firms may hire consultants to help them navigate through the rather complex validation process. These consultants play an important role in providing potential investors with trusted information from business networks and past experience (Pulver et al., 2010), and in targeting large firms with technologies that are far from the frontier, which provides major opportunities for emission reductions (Koo, 2017; Zhang et al., 2018). As they often request payment for their services in the form of a percentage of CERs, they are more likely to partner with financially stable companies that have the capacity to implement large CDM projects. In our empirical strategy, we address this selection issue in two ways: first, by focusing on a sample of ever-treated firms, exploiting the fact that they are not treated at the same time, and second, by matching firms undertaking CDM projects to otherwise similar firms.

## 2.1 CDM projects in India

A large proportion of CDM projects undertaken worldwide are hosted in India. This is an interesting context because, during the period from 1990 to 2018, CO<sub>2</sub> emissions from fuel combustion increased by 335% in India. A large part of this growth was due to rapid expansion in manufacturing output, following trade liberalization and other market reforms that occurred in the early 1990s. Also, its rapid increase in electricity use was mostly fueled by coal, which is highly polluting. For most of the period, CO<sub>2</sub> emissions were not regulated in India. The central government has however adopted recently some schemes and mechanisms that put an implicit price on carbon, by assigning specific energy reductions targets to energy intensive units from industrial sectors with high emissions

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<sup>6</sup>For example, Xie et al. (2014) compare successful and unsuccessful CDM projects and find that larger projects have a lower probability of being rejected, whereas projects with high internal rate of return have a higher probability of being rejected.



since 2017, and by introducing a tax on coal since 2010.<sup>7</sup>

Without explicitly aiming to reduce CO<sub>2</sub> emissions, both the central and state governments implemented public policies that fostered the use of renewable energy in electricity generation. Table A.1 provides details on the instruments used and on the recent policy targets set by the central and state governments. The growth in wind and solar energy was primarily driven by state-level feed-in-tariffs (FIT) introduced in 2000. Furthermore, the central government implemented in 2011 a new system of Renewable Energy Certificates, which allows for inter-state trading and serves as an alternative to FITs. The four main renewable energy producing states – Tamil Nadu, Maharashtra, Gujarat, and Rajasthan – have set ambitious targets for wind and solar power from 2012 onward, and have used preferential tariffs to help electricity providers and other investors to meet these targets. Of course, an alternative instrument that fosters renewable energy is the Clean Development Mechanism.

To get detailed information about CDM projects in India, we exploit a dataset from the UNEP CDM Pipeline Database.<sup>8</sup> Among the 12,474 CDM projects reported in this database, 3,337 are hosted in India. Among the Indian projects, 1,669 obtained registered status, and 247 projects were in the process of registration (validation status requested), whereas the rest of the projects had received unsuccessful status for various reasons (i.e., projects were withdrawn, rejected, negatively evaluated or replaced). We collected information on the names of firms investing in CDM projects, project registration date, and the type of CDM investment from the CDM Pipeline dataset and PDD documents.<sup>9</sup>

Table 1 shows the distribution of Indian CDM projects across investment types and CDM project registration status. We can see that most CDM projects aim to produce energy from various renewable energy sources (e.g., wind, biomass, hydro, and solar energies). Other CDM projects seek to improve firms' energy efficiency either through self-generation of electricity or through investments in industrial production processes.

To illustrate the diversity of CDM projects, let us consider the iron and steel industry in India, which is highly polluting – representing 6.2% of national GHG emissions, 28% of industrial emissions, and 25% of industrial total energy consumption (Murali Ramakrishnan et al., 2013) – for a relatively small contribution to India's GDP (3%). The details of CDM projects in this industry reveal that firms adopted several strategies to reduce their CO<sub>2</sub> emissions. For instance, some firms improved their energy efficiency by capturing and

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<sup>7</sup>The government adopted environmental policies throughout the 1980s and 1990s, with the objective to reduce damages from criteria air pollutants and water pollution, though with very limited effect (Greenstone & Hanna, 2014). The coal tax started at a low rate of 50 rupees per tonne, and subsequently increased to 100 rupees in 2014, 200 rupees in 2015, and 400 rupees in 2016. Revenues collected through the tax were sought to finance clean-energy initiatives. But this setup failed to achieve the desired outcomes as a significant portion of the revenues were unutilized, and in the end, in 2017, the tax was abolished and replaced by a new tax on coal production at the same rate of 400 rupees per tonne (source: <https://www.timesnownews.com/business-economy/industry/article/clean-energy-cess-tax-on-coal/683809>).

<sup>8</sup>The authors obtained the version of this database from [www.cdmpipeline.org](http://www.cdmpipeline.org) in May 2019.

<sup>9</sup>Collecting the name of each participating firm required a thorough examination of PDDs available at the website: <https://cdm.unfccc.int/Projects/projsearch.html>.

Table 1: Distribution of Indian CDM projects across project types and registration status

Type of project	# Registered Projects	# Projects under Evaluation	# Unsuccessful Projects
Wind energy	717	68	273
Biomass energy	263	37	282
Hydro energy	167	26	123
Solar energy	133	35	25
EE through own generation	75	19	133
EE in industrial process	59	12	120
Fossil fuel switch	30	9	44
Others	225	41	207

*Notes:* “EE” stands for Energy Efficiency projects, and “#” for number of. Numbers in the table exclude 214 projects "replaced at validation" to avoid double-counting. *Source:* Authors’ compilations from the CDM Pipeline database.

reusing waste gases from blast furnace for energy generation. Some firms switched from coal or heavy fuel oil to less carbon-intensive fuels in their boilers. Finally, some firms invested in biomass or wind power electricity generation.

One specificity of India is that a very low proportion of firms hosting CDM projects had signed an emission reduction purchase agreement with a CER buyer at the time of registration (Warnecke et al., 2015). It implies that most CDM projects were started as unilateral activities, without having an investing country or buyer on hand. This could be explained by the fact that these projects were able to generate enough revenues – from electricity generation for instance – or by the fact that Indian firms had better access to credit than manufacturing firms in other developing countries.

## 2.2 Distribution of CDM projects over time and across sectors and states

We combine the CDM Pipeline dataset with information on Indian manufacturing firms from a dataset called Prowess, which is collected by the Center for Monitoring the Indian Economy (CMIE). Prowess is based on annual reports filed publicly by large Indian manufacturers and contains information primarily from income statements and balance sheets. Even though the data spans the period from 1988 to 2016, there are few observations before 1992. Variables include revenues, the value of fixed capital stock, total costs of labor and materials, and total energy consumption. In addition, Prowess contains detailed descriptions of the energy inputs used for production. In particular, firms report annual expenditures and consumption (with units) of different energy sources – including coal, electricity from the grid, natural gas, and biomass. Industries are grouped using India’s National Industrial Classification codes.

Both the CDM Pipeline and Prowess datasets identify firms by their name. Most CDM

projects have more than one participating firm. On average, there are 12 participants for each CDM project. We thus create lists of firms participating in each CDM project. We merge the two datasets by hand, based on name recognition. Since Prowess focuses on the manufacturing sector, CDM projects hosted by the power sector are excluded from our analysis. From an initial list of 734 firms that were successful in implementing at least one CDM project, we found 339 of them in Prowess. A firm that figures in Prowess can participate in several CDM projects (up to 14). On average, participating firms hosted 1.55 CDM projects over the period of 2000-2016. By definition, if a firm that applies to host several CDM projects obtains at least one successfully registered CDM project over the entire period, then it belongs to the group of “CDM firms.”

The first CDM project registered in India appeared in 2000. Focusing on the first registered project for each CDM firm, we consider 339 projects that were registered in the period between 2000 and 2016, with an uneven distribution over time. Figure 1 reveals how registration dates are spread across these 17 years, with spikes in 2003, 2007, and 2012. Due to technological progress, firms that registered their CDM projects in 2001-2002, for instance, were likely to face different opportunities than firms that registered their CDM projects in 2012-2013. To explore this time dimension in our analysis, we created cohorts by grouping firms that registered their projects within a 4-year period. We thus assume that, during a 4-year period, technologies available in each sector are relatively similar. As a result, we obtain 4 cohorts: the first one corresponds to firms registering in 2000-2003, the second one to firms registering in 2004-2007, the third one to firms registering in 2008-2011, and the fourth one to firms registering in 2012-2015. We thus exclude the one firm that registered its CDM project in 2016 from our sample.

Figure 1: Distribution of CDM registration dates over 2000-2016

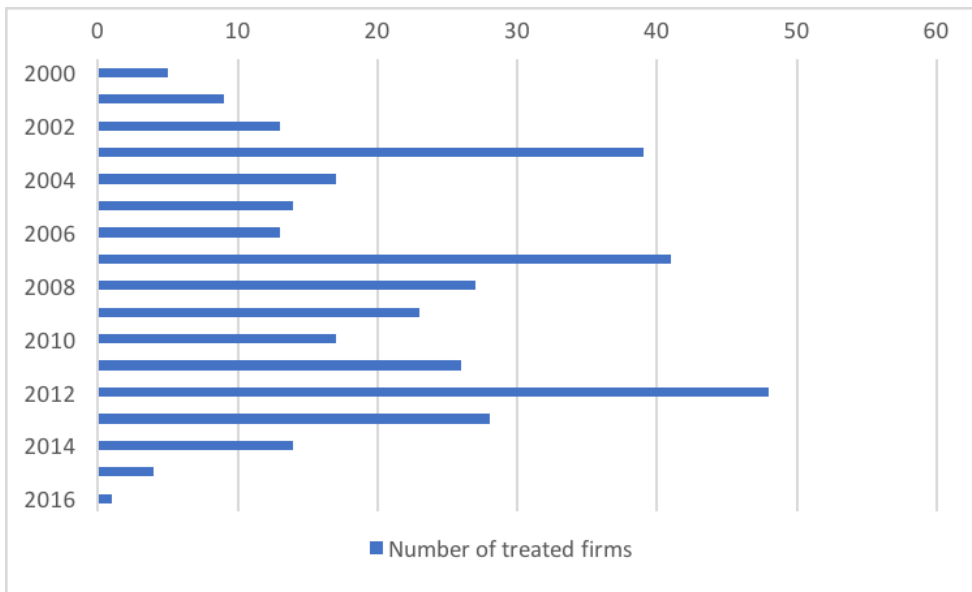


Table 2 describes the distribution of firms belonging to each of these four cohorts across

sectors. We further restricted the sample to firms that have information for at least one period before and one period after their registration date. We thus obtain 294 CDM firms. Cohort 1 consists of 41 firms, cohort 2 of 75 firms, cohort 3 of 84 firms, and cohort 4 of 94 firms. The sectors with good coverage across all four cohorts are food industries, textiles, chemicals, nonmetallic mineral products, and basic and fabricated metals.<sup>10</sup> Some sectors do not cover all cohorts (including leather industries, wood and furniture industries, and other manufacturing industries). Part of the analysis below uses an empirical strategy that relies on an across-cohort comparison. As a result, for the analysis that compares across cohorts, we use a restricted sample that only covers these sectors with good coverage across cohorts, leaving us with a sample of 217 CDM firms (74% of the 294 CDM firms).<sup>11</sup>

Table 2: Size of cohorts, by sector

Sectors	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Total
Food, beverages & tobacco	4	21	13	14	52
Textiles	11	12	8	14	45
Leather	0	0	1	0	1
Wood & furniture	1	1	0	1	3
Pulp & paper	4	5	5	0	14
Coke & petroleum prod.	0	3	5	1	9
Chemicals	5	11	10	8	34
Pharmaceuticals	0	1	2	4	7
Plastics & rubbers	0	2	5	6	13
Nonmetallic mineral prod.	7	7	6	8	28
Basic & fabricated metals	3	11	23	21	58
Computers & electronics	0	1	1	8	10
Machinery & transport eq.	6	0	3	7	16
Other manufacturing	0	0	2	2	4
All sectors	41	75	84	94	294

*Notes:* This table reports the number of CDM firms in each cohort by sector.

Table A.3 shows that the distribution of CDM projects across project type in our samples of analysis follows the distribution of all CDM projects in India, with a few exceptions. For instance, wind energy investments represent 47% of CDM projects in all our samples of analysis, whereas they represent 43% of all Indian CDM projects. Biomass energy investments represent 20% of the projects, instead of 16%. Furthermore, our samples under-represent investments in hydro and solar energy (with a negligible number of projects), and slightly over-represent energy-efficiency investments through own electricity generation (10-11% instead of 4.5%) or in industrial processes (7% instead of 3.5%).

<sup>10</sup>The definition of good sectoral coverage corresponds to having at least one firm in each cohort.

<sup>11</sup>Table A.2 summarizes the distribution of CDM firms by cohort and sector for the restricted sample of 217 firms.

Figure A.1 reveals that CDM projects are located all over India, based on their registering firms' headquarters' address, with important clusters in the states of Gujarat, Maharashtra, Tamil Nadu, the National Capital Territory (NCT) of Delhi and West Bengal, and then in smaller proportion in the states of Karnataka, Rajasthan, Madhya Pradesh, Punjab, Uttar Pradesh, Telangana, and Chhattisgarh. This distribution across states mirrors the distribution of factories in India, with Tamil Nadu, Maharashtra, Gujarat, Uttar Pradesh, and Andhra Pradesh being the most industrialized states. Furthermore, Table A.4 shows that the geographic distribution of CDM projects across states is similar across the four cohorts. Indeed, in each cohort, the highest number of CDM projects are located in the same five states: Tamil Nadu, Maharashtra, Gujarat, NCT of Delhi, and West Bengal.

Creating cohorts imposes to restructure the data. We generate 4-year periods starting from 1992 and ending in 2015, and compute mean values for each outcome variable over these 4-year periods. We thus obtain six calendar time periods, denoted by  $t$ . Period  $t = 1$  corresponds to 1992-1995, period  $t = 2$  to 1996-1999, period  $t = 3$  to 2000-2003, period  $t = 4$  to 2004-2007, period  $t = 5$  to 2008-2011, and period  $t = 6$  to 2012-2015.

### 2.3 Event studies design

Our first empirical approach follows the event study design. To avoid the selection issue associated with voluntary participation in the CDM, we restrict our sample to ever-treated firms. We thus consider a staggered adoption design where units are treated at different times. Hence, all firms participate in a CDM project, but they do so at different times. The treatment status of each firm  $i$  in sector  $s$  over time is a non-decreasing sequence of zeros and then ones, that is  $D_{is,m} \leq D_{is,t}$  for  $m < t$ .<sup>12</sup> The reason is that we consider only the first CDM project registered for each firm. We can thus uniquely characterize a treatment path by the time period of the initial treatment, denoted by  $E_i = \min\{t : D_{is,t} = 1\}$ . Firms that belong to the same cohort share the same  $E_i = e$ . Precisely, we have  $E_i = 3$  for cohort 1,  $E_i = 4$  for cohort 2,  $E_i = 5$  for cohort 3, and  $E_i = 6$  for cohort 4. Figure A.2 allows us to visualize the different cohorts by the period (or wave) at which they start to be treated (in red) in contrast to the periods before registration of their first CDM project (in blue).

We define  $Y_{is,t}^e$  to be the potential outcome in period  $t$  when firm  $i$  in sector  $s$  is first treated in time period  $e$ . We define  $Y_{is,t}^\infty$  to be the potential outcome if firm  $i$  never receives the treatment, which corresponds to the baseline outcome. Since the timing of the initial treatment uniquely characterizes one's treatment path, we can represent the observed outcome for firm  $i$  as

$$Y_{is,t} = Y_{is,t}^\infty + \sum_{3 \leq e \leq 6} (Y_{is,t}^e - Y_{is,t}^\infty) \cdot 1\{E_i = e\}, \quad (1)$$

where  $1\{E_i = e\}$  is a dummy variable that equals 1 if firm  $i$  is first treated in period  $e$ ,

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<sup>12</sup>This is defined in the empirical literature as an absorbing treatment (Sun & Abraham, 2021).

0 otherwise. The firm-level treatment effect can thus be defined as the difference between the observed outcome relative to the never-treated counterfactual outcome:  $Y_{is,t} - Y_{is,t}^\infty$ .

Given the nature of technological investments, we must take account of dynamic treatment effects. In fact, investments may take some time to bear fruit in reducing the average marginal cost of production or in modifying the emission intensity of production. As a result, we want to identify the specific effect from being  $\ell$  periods away from the treatment. We also anticipate heterogeneous effects across cohorts. As a result, we follow Sun & Abraham (2021) in defining a ‘‘cohort-specific average treatment on the treated’’ (CATT) as the average treatment effect on cohort  $e$   $\ell$  periods from initial treatment:

$$\text{CATT}_{e,\ell} = E [Y_{is,e+\ell} - Y_{is,e+\ell}^\infty | E_i = e]. \quad (2)$$

Firms that belong to the same cohort are treated at the same time  $e$ , and experience a treatment path reflected by  $\text{CATT}_{e,\ell}$  that varies over periods  $\ell$ . Hence, we shift from calendar time index  $t$  to relative period index  $\ell$ , which is relative to the period when firms are first treated (denoted by  $\ell = 0$ ). Specifically, for cohort 1,  $\ell$  ranges from -2 to 3; for cohort 2, it ranges from -3 to 2; for cohort 3, it ranges from -4 to 1; and for cohort 4, it ranges from -5 to 0. Overall,  $\ell \in [-5, 3]$ .

Ideally, we want to estimate a weighted average of  $\text{CATT}_{e,\ell}$  for period  $\ell$  that reflects the average treatment effect of CDM projects  $\ell$  periods away from treatment, with reasonable weights. These weights must sum to one and be non-negative. In particular, we focus on the following weighted average of  $\text{CATT}_{e,\ell}$ , where the weights reflect the share of cohorts that are present  $\ell$  periods relative to treatment:

$$v_\ell = \sum_e \text{CATT}_{e,\ell} Pr\{E_i = e | E_i \in [-\ell, 6 - \ell]\}. \quad (3)$$

To proceed, we consider two estimation methods. Defining  $D_{is,t}^\ell = 1\{t - E_i = \ell\}$  as an indicator for firm  $i$  being  $\ell$  periods away from initial treatment at period  $t$ , we start with a two-way fixed effects (FE) regression of the following form:

$$Y_{is,t} = \alpha_i + \lambda_{s,t} + \sum_{\ell=-4}^{-2} \mu_\ell D_{is,t}^\ell + \sum_{\ell=0}^3 \mu_\ell D_{is,t}^\ell + \nu_{is,t}, \quad (4)$$

where  $\alpha_i$  controls for firm time-invariant characteristics, whereas sector-by-period fixed effects  $\lambda_{s,t}$  control for sector-specific time-varying unobservables, such as sector-wide changes in technology, policy or demand. In particular, these sector-by-period fixed effects capture technology-vintage effects that derive from the fact that firms making technological investments at different times may choose from different types of technologies depending on the time of their investment. Additionally, these sector-by-period fixed effects capture any public policy implemented by the central government.<sup>13</sup> The relative times  $\ell = t - E_i$  cover

<sup>13</sup>These sector-by-period fixed effects, however, do not capture state-level policies (such as FITs for renewable energy). We thus rely on the distribution of CDM firms being similar across states across cohorts, as shown in Table A.4, to avoid these potential confounding factors influencing CDM firms’ outcomes.

most possible relative periods, except  $\{-5, -1\}$ . Excluding some relative periods from this dynamic specification is necessary to avoid multicollinearity, either among the relative period indicators  $D_{is,t}^\ell$ , or with the firm and sector-by-period fixed effects.<sup>14</sup> It is standard to exclude the period right before treatment ( $\ell = -1$ ). With a sample defined between two calendar time periods, we need to exclude at least two relative period indicators, thus we also exclude  $\ell = -5$ .

This two-way fixed effects regression approach, although standard, may suffer from a contamination issue, in the sense that estimates  $\mu_\ell$  may be contaminated by treatment effects from other periods even under the assumptions of parallel trends and no anticipation. The parallel trend assumption states that baseline outcomes should follow the same trend across cohorts. The lack of anticipation assumption supposes that there is no treatment effect in pre-treatment periods. The usual test for these two assumptions rests on verifying that  $\mu_\ell$  are not statistically significant from zero in the pre-period ( $\ell < 0$ ). As demonstrated by Sun & Abraham (2021) and Goodman-Bacon (2021), testing for pretrends using pre-period coefficients is not valid when there is contamination, which can occur either due to heterogeneous effects across cohorts or due to non-zero effects associated with the excluded periods.

In our context, we anticipate that treatment effects may be heterogeneous across cohorts. As a result, we also consider an estimation strategy that is robust to this contamination issue. Specifically, we follow the three-step procedure described in Sun & Abraham (2021) to obtain an “interaction-weighted” (IW) estimator. First, we estimate  $\text{CATT}_{e,\ell}$  using a linear two-way fixed effects specification that interacts relative period indicators with cohort indicators, excluding the indicator for cohort 4 ( $e = 6$ ):

$$Y_{is,t} = \alpha_i + \lambda_{s,t} + \sum_{e=3}^5 \sum_{\ell \neq -1} \delta_{e,\ell} 1\{E_i = e\} \cdot D_{is,t}^\ell + \epsilon_{is,t}. \quad (5)$$

Since our sample contains only firms that will be treated by CDM projects at different times, we must consider the latest treated cohort – cohort 4 – as the control group, and estimate regression (5) on observations from period  $t=1$  to  $t=5$ , thereby excluding the last period when cohort 4 received treatment.<sup>15</sup> Second, we estimate the weights  $Pr\{E_i = e | E_i \in [-\ell, 6 - \ell]\}$  by sample shares of each cohort in the relevant periods  $\ell$ . Third, we take a weighted average of estimates for  $\text{CATT}_{e,\ell}$  from the first step with weight estimates from the second step. More formally, the IW estimator is

$$\widehat{v}_\ell = \sum_e \widehat{\delta}_{e,\ell} \widehat{Pr}\{E_i = e | E_i \in [-\ell, 6 - \ell]\}. \quad (6)$$

This approach relies on the parallel trends and no anticipation assumptions, which implies that the estimated  $\widehat{\delta}_{e,\ell}$  from regression (5) is a consistent estimator for  $\text{CATT}_{e,\ell}$ . The sample

<sup>14</sup>See the discussion in Borusyak and Jaravel (2018), and in Sun and Abraham (2021).

<sup>15</sup>Excluding this last period from 2012 onward also allows us to avoid attributing to CDM projects the effects of confounding factors such as the ambitious state-level renewable energy policies documented in Table A.1, which started in 2012.

shares of each cohort are also consistent estimators for the population shares. Thus, the IW estimator is consistent for  $v_\ell$ .

In our context, the timing of investments in CDM projects is probably endogenous to some unobservable characteristics of firms. For instance, a firm that has been operating for many years is more likely to invest in new technologies earlier than a newly created firm since the latter has more recent vintage capital. Thus, it is not plausible that the baseline outcome  $Y_{is,t}^\infty$  is mean independent of the timing of treatment. The parallel trends assumption is more plausible as it allows this timing to depend on unobserved time-invariant characteristics. We may also expect never-treated firms to be on a different trend than firms that will participate into the CDM. Given our sample, we thus restrict the parallel trends assumption to firms that ever participated into a CDM project. Additionally, it is plausible that there is no anticipatory response from firms, since building new capital takes time, and since the existence of credit constraints might discourage firms to make such investments prior to getting their CDM project validated.<sup>16</sup>

The identification of causal effects of CDM projects necessitates to exclude general equilibrium effects: hence, the participation of one firm in a CDM project cannot influence the outcomes of other control firms. According to Imbens & Wooldridge (2009), the validity of this stable unit treatment value assumption (SUTVA) depends on the scope of the program, as well as on resulting effects. In the case of CDM projects in India, we argue that SUTVA is plausibly satisfied because these projects are scattered geographically and over time within a sector. Indeed, Table 2 reveals that very few firms register a CDM project within a sector-period. Figure A.1 also shows that participating firms are located all over India, but mostly in the most industrialized states, and that they represent a small proportion of the total number of manufacturing firms present in the Prowess dataset in each state (less than 8.5%).

## 2.4 Event studies design with semi-parametric matching

Due to the small size of our sample of ever-treated CDM firms and due to the fact that firms belonging to different cohorts may differ in specific ways, we also consider two-way fixed effects and interaction-weighted estimators with semi-parametric matching. The rationale behind matching is to identify control firms with common support and good covariate balance for all treated firms. By considering never-treated firms as the control group in our matched sample, we might be able to capture better sector-specific trends when assessing the effects of CDM projects on participating firms.<sup>17</sup> Therefore, restricting our sample to sectors for which we have a well-balanced distribution of CDM firms across cohorts is no longer necessary. However, due to the selection issue, we must find a sample

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<sup>16</sup>Until December 2005, firms could request that the crediting period for CERs could start before the date of registration of the CDM project. However, we do not observe any firm that has a crediting period starting before registration in our sample. Thus, firms likely did not anticipate the validation of their project.

<sup>17</sup>We exclude from the sample of never-treated firms those that applied to host one or several CDM projects and obtained no success in registering any of them.



of control firms for which the assumption of parallel trends with treated firms is satisfied for the comparison to be valid.

In our application, we implement one-to-one nearest-neighbor matching (as in Colmer et al. 2020). The FE regression equation is, instead of equation 4, as follows:

$$Y_{is,t} - Y_{ks,t} = \alpha_{ik} + \lambda_{s,t} + \sum_{\ell=-4}^{-2} \mu_{\ell} D_{is,t}^{\ell} + \sum_{\ell=0}^3 \mu_{\ell} D_{is,t}^{\ell} + \nu_{iks,t}. \quad (7)$$

The left-hand side of equation (7) denotes the difference in outcome between treated firm  $i$  and its matched control firm  $k$  in period  $t$ . We constrain the set of potential control firms to belong to the same sector  $s$  as each treated firm  $i$ . The treatment dynamic path remains defined only on firm  $i$ 's treatment status since firm  $k$ , by definition, never implements a CDM project. Fixed effects  $\alpha_{ik}$  control for time-invariant characteristics of the pair of treated firm  $i$  and matched control firm  $k$ , whereas  $\lambda_{s,t}$  absorb the average difference in outcomes between ever-treated firms and their matched control firms within a sector  $s$  at period  $t$ .

To obtain the IW estimator, we proceed in three steps, as described above. We estimate  $\text{CATT}_{e,\ell}$  from the following regression, instead of equation 5,

$$Y_{is,t} - Y_{ks,t} = \alpha_{ik} + \lambda_{s,t} + \sum_{e=3}^5 \sum_{\ell \neq -1} \delta_{e,\ell} 1\{E_i = e\} \cdot D_{is,t}^{\ell} + \epsilon_{is,t}. \quad (8)$$

Then, we estimate the weights  $Pr\{E_i = e | E_i \in [-\ell, 6 - \ell]\}$  by sample shares of each cohort in period  $\ell$  for the matched sample, and combine these weights with estimated  $\text{CATT}_{e,\ell}$  to form the IW estimator (eq. 6).

We match treated firms to control firms based on (i) the sector of the firm, (ii) the time path of observed firm-level information in our sample, (iii) the logarithm of their CO<sub>2</sub> emission levels in the relative period  $\ell = -1$ , that is, one period before treatment. Recall that each period corresponds to a 4-year interval. Thus, we do not explicitly match on pre-treatment trends, but on a 4-year period average. In the following, we exploit leads to test for the validity of the parallel trends assumption. Matching exactly on the sector is required by the fact that technologies differ widely across sectors. Matching on the time path of observations is necessary given the small size of treated firms and the fact that the panel is unbalanced. Consider a treated firm for which we have observations at periods  $t = 2, 4, 5$ . Suppose that this firm registered a CDM project at  $t = 5$ , which implies that we must find a matched control firm with similar characteristics at  $t = 4$ . Suppose further that two potential control firms exist, one for which we have observations at periods  $t = 2, 3, 4, 5$  and another for which observations exist only at periods  $t = 1, 3, 4$ . If we match the treated firm with the latter, this pair of firms will not be included in the regressions (7) and (8) because the difference in outcomes only exist for the excluded period  $t = -1$ . Therefore, matching with the former control firm is a better strategy. To implement this strategy, we created an indicator for the sequence of periods for which firms have observations. We then created a mapping between these indicators and a set of other combinations for which

observations exist at the same periods at least.<sup>18</sup> After defining this mapping, we find a matched control firm for each treated firm based on the similarity in the logarithm of the CO<sub>2</sub> emissions. We perform matching with replacement, and we cluster standard errors at the level of the matched pair.<sup>19</sup>

Our matching strategy allows us to verify that covariates are balanced between treated and control firms in the relative period  $\ell = -1$  for both the variable used for matching (log of CO<sub>2</sub> emissions) and other outcome variables. Appendix C provides further details on the matching quality and balancing tests.

### 3 Data Description

This section details the different variables used in our analysis and provides descriptive statistics. As described in Section 2.2., the two datasets that we combine for this study are the UNEP CDM Pipeline dataset and the Prowess dataset.

#### 3.1 Variables

The Prowess dataset provides general information about the firm (industry, head office address, export status, etc), as well as balance sheet information (containing variables such as total sales, total cost of production, total labor costs, various measures of capital, etc.). In this study, we use total sales, capital stock, and total labor costs to reflect the potential effects of CDM projects on firms’ revenues and input use. We expect the stock of capital to increase due to investments, and total labor costs to be impacted either upward or downward depending on whether technological change induced by CDM projects is biased away from labor or not. These variables are expressed in millions of current year Rupees. The dataset also contains detailed energy use data (including electricity from the grid, fossil fuels, and renewable energy sources) in quantity at the firm-year level. With this data, we compute both total energy use (in Metric Million British Thermal Unit – MMBtu) using energy-specific conversion factors and total CO<sub>2</sub> emissions (in kiloton – kt).

To compute CO<sub>2</sub> emissions at the firm level, we followed previous work on multiplying energy consumption by fuel-specific CO<sub>2</sub> emission factors (Barrows & Ollivier, 2018; Forslid et al., 2018; Marin & Vona, 2021). This strategy relies on the assumptions that a given source of energy has a fixed carbon content, and that burning the energy input releases that carbon, which forms CO<sub>2</sub> in combination with oxygen, regardless of the technology used. These assumptions seem reasonable in the context of CO<sub>2</sub> emissions in India – where carbon capture technologies are rarely used – and are applied to primary energy sources

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<sup>18</sup>To provide an example, consider the sequence “NYNNYY” where “N” denotes no observation and “Y” denotes an observation. This sequence could be matched with a firm having one of the following sequences: “YYYYYY”, “YYNNYY”, “YYNYYY”, “YYNYYY”, “NYNNYY”, “NYNYYY”.

<sup>19</sup>In the main matched sample, we impose the following distance restriction: the ratio of CO<sub>2</sub> emissions of treated firms relative to control firms in  $\ell = -1$  must lie between [0.5, 5]. We perform robustness tests by dropping all distance restrictions, as shown in Appendix Table D.18.

(e.g., coal, gas, petroleum). For electricity production, however, technologies play a role in determining the amount of CO<sub>2</sub> emissions associated with a kWh of electricity. When firms generate their own electricity, they report the energy inputs used for that self-generation. Hence, we can compute CO<sub>2</sub> emissions from self-generated electricity using the quantities of energy sources and multiplying them by our CO<sub>2</sub> emission factors. When firms purchase electricity from the grid, we use a CO<sub>2</sub> emission factor that reflects the average energy mix of electricity in India.<sup>20</sup> We provide details on the construction of these variables in Appendix B.

Whereas the levels of energy use and CO<sub>2</sub> emissions reflect both the scale of production and the technology used, we also provide two measures of technology. First, we compute CO<sub>2</sub> emission intensity measured in proportion to total sales. Second, we compute the CO<sub>2</sub> content in energy use (ton of CO<sub>2</sub> emissions per MMBtu). While the former reflects how much CO<sub>2</sub> emissions are embodied in output production, the latter reflects how CO<sub>2</sub> intensive energy is. These two measures offer different perspectives on technology. To illustrate, suppose that a firm adopts a fuel-switching strategy that lowers the CO<sub>2</sub> content in energy use but raises the unit cost of production, and thus lowers production and sales. In this context, CO<sub>2</sub> emission intensity may remain unchanged (since both CO<sub>2</sub> levels and sales declined), while the CO<sub>2</sub> content in energy use declined.

Given the importance of self-generation of electricity in CDM projects and its potential in reducing CO<sub>2</sub> emission intensity in sales or in energy use when it relies on renewable energies, we further consider expenditures for electricity purchased from the grid, expenditures from self generation, and expenditures related to renewable energy relative to total energy cost. By doing so, we identify whether the channel through which CDM projects may reduce CO<sub>2</sub> emissions is related to a shift from grid electricity to self-generated power production, and in particular to renewable energy power production.

One caveat that arises from our data description is that we only measure CO<sub>2</sub> emissions related to energy consumption. If some manufacturing sectors emit CO<sub>2</sub> emissions or other greenhouse gases due to their production process alone (e.g., due to chemical reactions during the production of chemicals, iron and steel, and cement), and independent of their energy use, we cannot capture these emissions with our data.

## 3.2 Descriptive statistics

In this section, we provide descriptive statistics for our two main samples of analysis. For the analysis that relies on across-cohort comparison, we use the restricted sample of 217 ever-treated firms in sectors for which we have a well-balanced distribution of treated firms across cohorts. For the analysis that relies on semi-parametric matching, we consider all sectors: specifically, from the 294 CDM firms available in both Prowess and UNEP CDM Pipeline dataset for which we observe outcomes prior and post treatment, we were able to match 197 CDM firms to 167 non-CDM firms.

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<sup>20</sup>Prowess reports only the headquarters' location, not the exact location of the producing establishment. Many Indian firms have multiple establishments, which can potentially be located in different districts or states. Hence, we cannot attribute regional CO<sub>2</sub> emissions factors for electricity to firms.

Table 3 provides descriptive statistics separately for each cohort of the 217 CDM firms for the period 1992-2015. We observe some dissimilarities across cohorts, especially in size. Firms in cohorts 1, 2, and 3 are, on average, bigger in terms of total sales, capital stock, and labor cost than firms in cohort 4. For instance, firms in cohort 4 generate on average 124.3 millions of rupees per year, whereas the other cohorts generate more than 300 millions of rupees per year. Had we included all 294 ever-treated firms, we would have had an even starker contrast across cohorts, as indicated in Table B.6 where firms in cohorts 1, 2, and 3 generate respectively more than 3, 4, and 9 times the amount of annual sales generated by cohort 4. Because CO<sub>2</sub> gases are emitted in proportion to production, it is natural to observe the same contrast for CO<sub>2</sub> emissions and energy use. Firms in cohort 4 emit roughly half the amount of CO<sub>2</sub> produced by other cohorts (or even a third in the case of cohort 2) and use less than half the amount of energy.<sup>21</sup>

For other variables, the contrast between cohort 4 and the other cohorts is attenuated. For CO<sub>2</sub> intensity, the earlier the participation in the CDM, the dirtier the technology. This reveals that effort made in targeting firms with technologies that were far from the frontier was efficient. The CO<sub>2</sub> content in energy use varies across cohorts, with similar mean values between cohorts 1 and 3, and between 2 and 4. Finally, the shares of expenditures from self-generated electricity, grid electricity, and renewable energy in total energy cost are quite similar across cohorts, with more than 40% for grid electricity, roughly 20% for self generation, and 4% for renewable energy.

In Table A.4, we also verify that the distribution of CDM projects by Indian state is similar across cohorts. In particular, we show that the proportion of CDM projects located in the four major renewable energy producing states (Tamil Nadu, Maharashtra, Gujarat, Rajasthan) is exactly the same (50%) in each cohort. Indeed, some state-level policies (e.g., policies fostering renewable energy power generation) could influence firms' CO<sub>2</sub> emissions and economic outcomes. It is thus important that cohort 4, which will serve as a control group in Sun & Abraham (2021)'s approach, shares the same characteristics as the other cohorts.

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<sup>21</sup>Appendix Section C provides an in-depth comparison of the distributions of the outcome variables across cohorts.

Table 3: Descriptive statistics for ever-treated firms in the restricted sample, by cohort

	Cohort 1	Cohort 2	Cohort 3	Cohort 4
Total sales (M.R.)				
mean (s.d.)	310.8 (1044.5)	403.8 (1035.1)	378.3 (912.5)	124.3 (344.8)
# observations	162	343	284	328
Capital stock (M.R.)				
mean (s.d.)	226.7 (774)	370.0 (978.5)	297.6 (776.9)	105.9 (401.2)
# observations	162	343	284	328
Labor cost (M.R.)				
mean (s.d.)	11.3 (32.0)	32.9 (145.3)	17.4 (61.5)	6.3 (24.9)
# observations	162	343	284	328
CO <sub>2</sub> emissions (kt)				
mean (s.d.)	1477 (6115)	2670 (7787)	1578 (5017)	855 (3903)
# observations	143	291	231	245
Energy use (10 <sup>6</sup> MMBtu)				
mean (s.d.)	35.32 (204.5)	25.52 (77.95)	15.48 (46.82)	7.14 (29.63)
# observations	143	291	231	245
CO <sub>2</sub> intensity (kt/M.R.)				
mean (s.d.)	3028.8 (36075)	37.29 (533.6)	7.40 (29.03)	3.57 (7.34)
# observations	143	291	231	245
CO <sub>2</sub> content in energy use (t/MMBtu)				
mean (s.d.)	1.55 (14.65)	0.17 (0.16)	2.24 (28.76)	0.44 (2.79)
# observations	143	291	231	245
Share of self-generated electricity in energy cost				
mean (s.d.)	.27 (.27)	.22 (.21)	.19 (.21)	.19 (.24)
# observations	146	296	244	271
Share of grid electricity in energy cost				
mean (s.d.)	.43 (.30)	.42 (.28)	.43 (.29)	.54 (.30)
# observations	146	296	244	271
Share of renewable energy in energy cost				
mean (s.d.)	.04 (.11)	.04 (.15)	.04 (.14)	.025 (.09)
# observations	146	296	244	271

*Notes:* Data are available for the period of 1992-2015 for 217 CDM firms. All monetary variables are reported in millions of current year rupees, denoted by M.R. CO<sub>2</sub> emissions are reported in kilotons (kt) while energy use is reported in 10<sup>6</sup> MMBtu. The shares of self-generated electricity, grid electricity, and renewable energy correspond to the share of these expenditures in total energy cost.

Given that ever-treated firms from different cohorts are not exactly comparable in size and in CO<sub>2</sub> emission levels, we further investigate the effect of registering a CDM project using a sample that matches participating firms with similar never-treated firms. We

were able to match 197 CDM firms to 167 non-CDM firms. Table 4 provides descriptive statistics separately for CDM firms and their matched control firms for the period of 1992-2015. By including periods after treatment, we expect that outcomes may differ between CDM firms and non-CDM firms. We refer the reader to Appendix C for a comparison between CDM firms and their matched control firms at pre-period  $\ell = -1$ . Table 4 shows that CDM firms are, on average, bigger in terms of total sales, capital stock, and labor cost than non-CDM firms. Yet, in terms of CO<sub>2</sub> emissions and energy use, treated and control firms are comparable. On average, CDM firms are more CO<sub>2</sub> emission intensive in sales and in energy than non-CDM firms. This is in line with our expectations: more energy intensive firms have stronger incentives to reduce their energy use and improve their emission intensity. Treated and control firms rely on electricity from the grid in similar proportions, whereas CDM firms tend to generate more electricity on site and use slightly more renewable energy.

Table A.5 compares the distributions across states of the four cohorts of CDM firms and their control counterparts. Overall, the proportion of firms located in the major states for CDM in the matched sample (Tamil Nadu, Maharashtra, Gujarat, and West Bengal) amounts to 56% of CDM firms and 49% of control firms, which is very similar. Since we do not match on firms' headquarters' location, we observe some discrepancies in the geographic distribution of CDM and control firms within each cohort. Taking the example of Tamil Nadu, we observe more CDM firms than control firms for cohorts 1 and 2 (14 vs 3 and 10 vs 2, respectively) whereas the numbers are similar for cohorts 3 and 4 (7 vs 8 and 9 vs 6, respectively). For the major states, the distribution of CDM firms and their control firms by state is comparable within each cohort.

Table 4: Descriptive statistics for the matched sample

Variable	CDM firms			Control firms		
	#obs.	mean	s.d.	#obs.	mean	s.d.
Total sales (M.R.)	1045	231	706	1045	120	316
Capital stock (M.R.)	1045	184	593	1042	101	278
Labor cost (M.R.)	1045	15.4	86	1045	6.8	17
CO <sub>2</sub> emissions (kt)	989	990	4524	959	803	9387
Energy use (10 <sup>6</sup> MMBtu)	989	12	86	959	12	111
CO <sub>2</sub> intensity (kt/M.R.)	989	14.9	291	959	6.1	23.9
CO <sub>2</sub> content in energy (t/MMBtu)	989	1.16	16	959	0.34	4.68
Self-generated electricity share	993	.20	.23	976	.15	.22
Grid electricity share	993	.48	.30	976	.51	.29
Renewable energy share	993	.05	.14	976	.03	.10

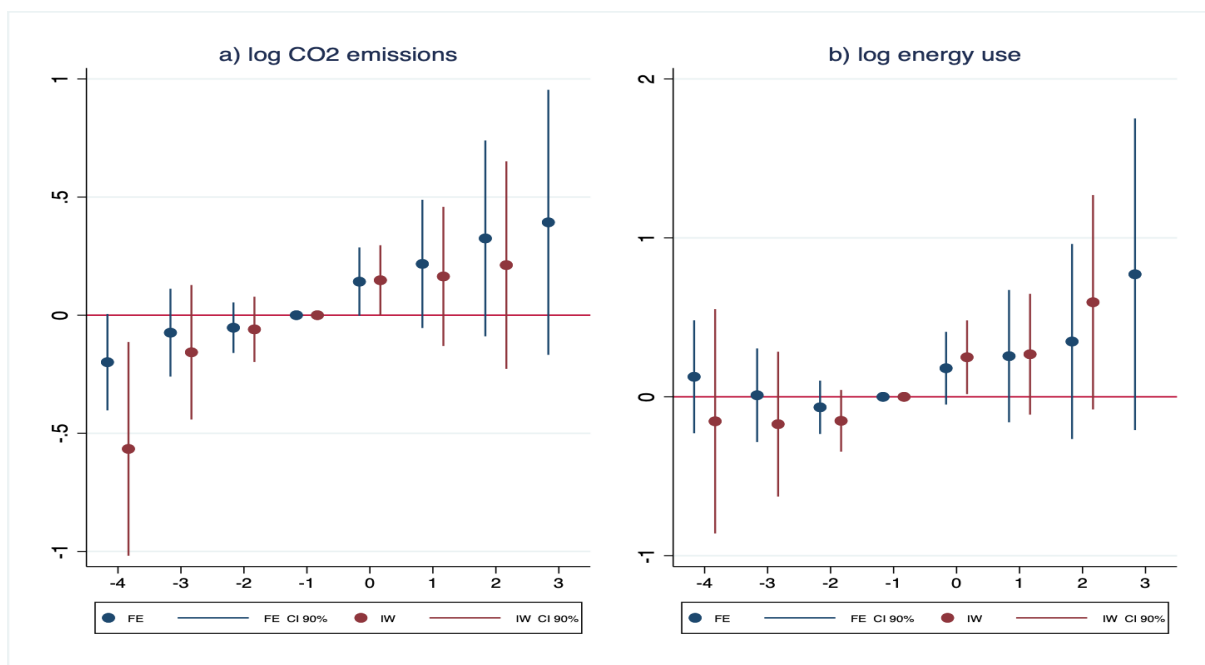
*Notes:* Data are available for the period of 1992-2015 for 197 CDM firms and 167 matched control firms. All monetary variables are reported in millions of current year rupees, denoted by M.R. CO<sub>2</sub> emissions are reported in kilotons (kt) while energy use is reported in 10<sup>6</sup> MMBtu. The shares of self-generated electricity, grid electricity, and renewable energy correspond to the share of these expenditures in total energy cost.

## 4 Results

### 4.1 Main results from the sample of ever-treated firms

Figure 2 presents graphically the estimation results for the fixed effect regression (4) and for the IW estimator (6) for CO<sub>2</sub> emissions and energy use outcomes using the restricted sample of 217 ever-treated firms. We report the point estimates as dots and the 90% confidence intervals as bars, using blue for the FE estimator and red for the IW estimator. First, we estimate with a 10% level of statistical significance, that firms registering a CDM project experienced a 14% increase in CO<sub>2</sub> emissions in the 4-year period after registration ( $\ell = 0$ ), using either the FE estimator or the IW estimator. This increase may not be long-lasting as none of the estimates in  $\ell > 0$  are statistically significant at the 10% level. Second, we estimate that energy use (measured in MMBtu) increased by 25% in the period after registration ( $\ell = 0$ ) with a 10% statistical significance, using the IW estimator. None of the estimates obtained with the FE estimator are statistically significant at the 10% level for energy use. To illustrate how the IW estimates  $\hat{v}_\ell$  relate to the underlying CATT <sub>$e,\ell$</sub>  estimates  $\hat{\delta}_{e,\ell}$ , we report in Table D.10 all the coefficients.

Figure 2: Results for CO<sub>2</sub> emissions and energy use, with only ever-treated firms



*Notes:* Estimates of coefficients  $\mu_\ell$  for  $\ell = -4, \dots, 3$  from the FE estimator (4) and  $\hat{v}_\ell$  for  $\ell = -4, \dots, 2$  from the IW estimator (6), using the restricted sample of ever-treated firms, are reported graphically. Panel a) reports these coefficients for CO<sub>2</sub> emissions and panel b) for energy use. The x-axis represents the value of  $\ell$ , the dots the point estimates, the bar the 90% confidence intervals. Standard errors are clustered at the firm level. Number of firm-year observations is 904 for the FE estimator and 742 for the IW estimator, and number of firms is 186 and 179, respectively.

For both outcomes in Figure 2, we validate the absence of pre-trends, except for one case. Indeed, all estimates for  $\ell < 0$  are not statistically significant in both the FE and IW regression designs, except the estimate in  $\ell = -4$  using the IW estimator. It suggests that firms reduced their CO<sub>2</sub> emissions by 56% (relatively to  $\ell = -1$ ) during the period that lies 13 to 16 years before registering a CDM project. Given the staggered adoption design, it is thus only associated with firms in cohort 3, which would have reduced their emissions in the period 1992-1995. It is unlikely that this effect reflects anticipatory behaviors from Indian firms since the Clean Development Mechanism emerged from the Kyoto protocol which was signed in 1997, and started to have projects being registered in India in 2000. It may suggest, however, that firms in cohort 3 were on an increasing pre-trend for CO<sub>2</sub> emissions in comparison to firms in cohort 4, starting 13 to 16 years before registering a CDM project.

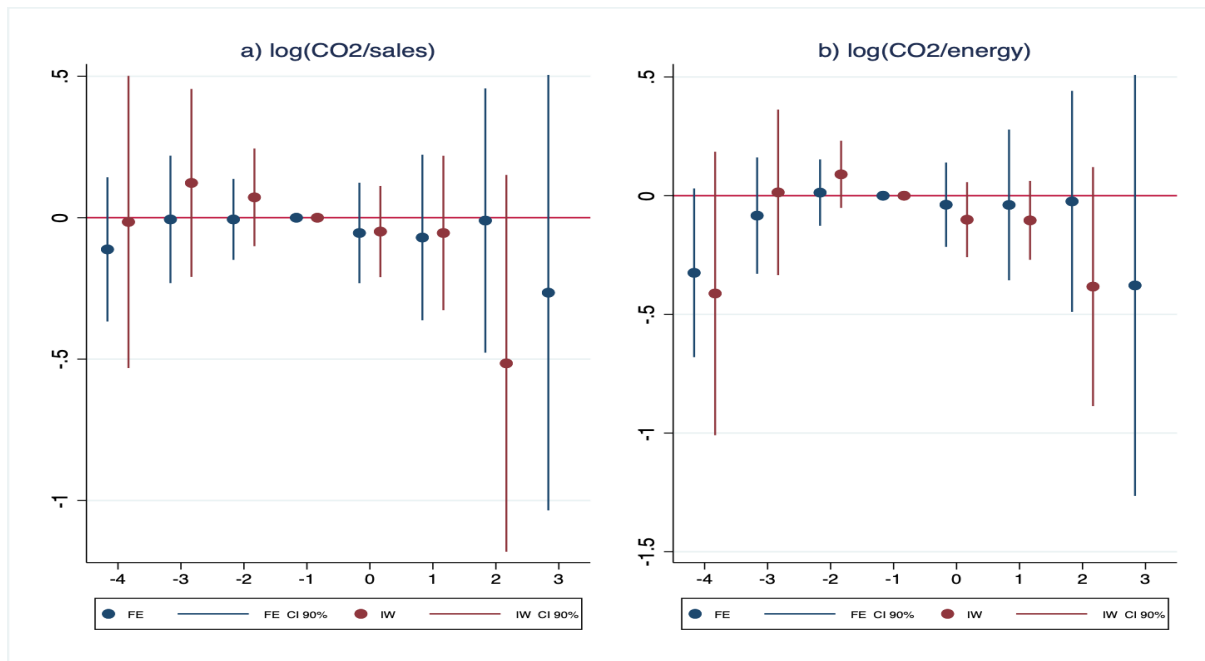
Figure 3 presents graphically the estimation results for the dynamic effects of CDM projects on firm's CO<sub>2</sub> emission intensity in total sales and in energy use. We fail to reject the null hypothesis of no effect on CO<sub>2</sub> emissions intensity relative to these two dimensions. Yet, we notice that the sign of post-treatment estimates is negative for both estimators (all estimates being not statistically significant at the 10% level) in both cases. We validate the absence of pre-trends in both the FE and IW regression designs.

Figure 4 presents the estimation results for the fixed effect regression ( $\widehat{\mu}_\ell$ ) and for the IW estimator ( $\widehat{v}_\ell$  only) for total sales, capital stock, labor cost, the shares of self-generated and grid electricity, and the share of renewable energy expenditure in total energy cost. We estimate, with a 10% level of statistical significance, that firms registering a CDM project experienced a 16% increase in sales in  $\ell = 0$ , as well as a 14% increase in  $\ell = 0$  and a 36% increase in  $\ell = 2$  in capital stock, using the IW estimator. For sales, the FE estimate in  $\ell = 0$  reveals an increase of the same magnitude (also significant at the 10% level). For capital, the FE estimator indicates slightly larger effects (a 19.7% increase in  $\ell = 0$  and a 32% increase in  $\ell = 1$ , both with p-value<0.05), but we have less confidence in these estimates as they might suffer from contamination due to dynamic effects heterogeneity. The scale of investment associated with CDM projects is thus large, even considering just the 14% increase in the first 4-year period after treatment. We fail to reject the null hypothesis of no effect on firms' labor cost.

However, firms selecting into different timings of treatment do not follow parallel trends for both sales and capital, even after controlling for unobserved time-invariant characteristics, in the very early periods ( $\ell = \{-4, -3\}$ ). This is especially clear when we use the IW estimates which reveal that participating firms from the third cohort experienced a decline in sales (50% with p-value<0.05) and capital stock (43% with p-value<0.1) in the period 1992-1995, and firms from the third and second cohorts experienced a decline in capital (30% with p-value<0.05) in 1996-1999, relative to period  $\ell = -1$  and to firms in cohort 4. These pre-trends imply that the effects of CDM projects on firms' sales and capital stock may be over-estimated, in the sense that cohorts 2 and 3 were already on a growing trend 9 to 16 years before registration. The FE estimator is less able to capture these patterns due to a contamination issue; yet, we also estimate pre-trends for capital in  $\ell = -3$  (significant at the 10% level).



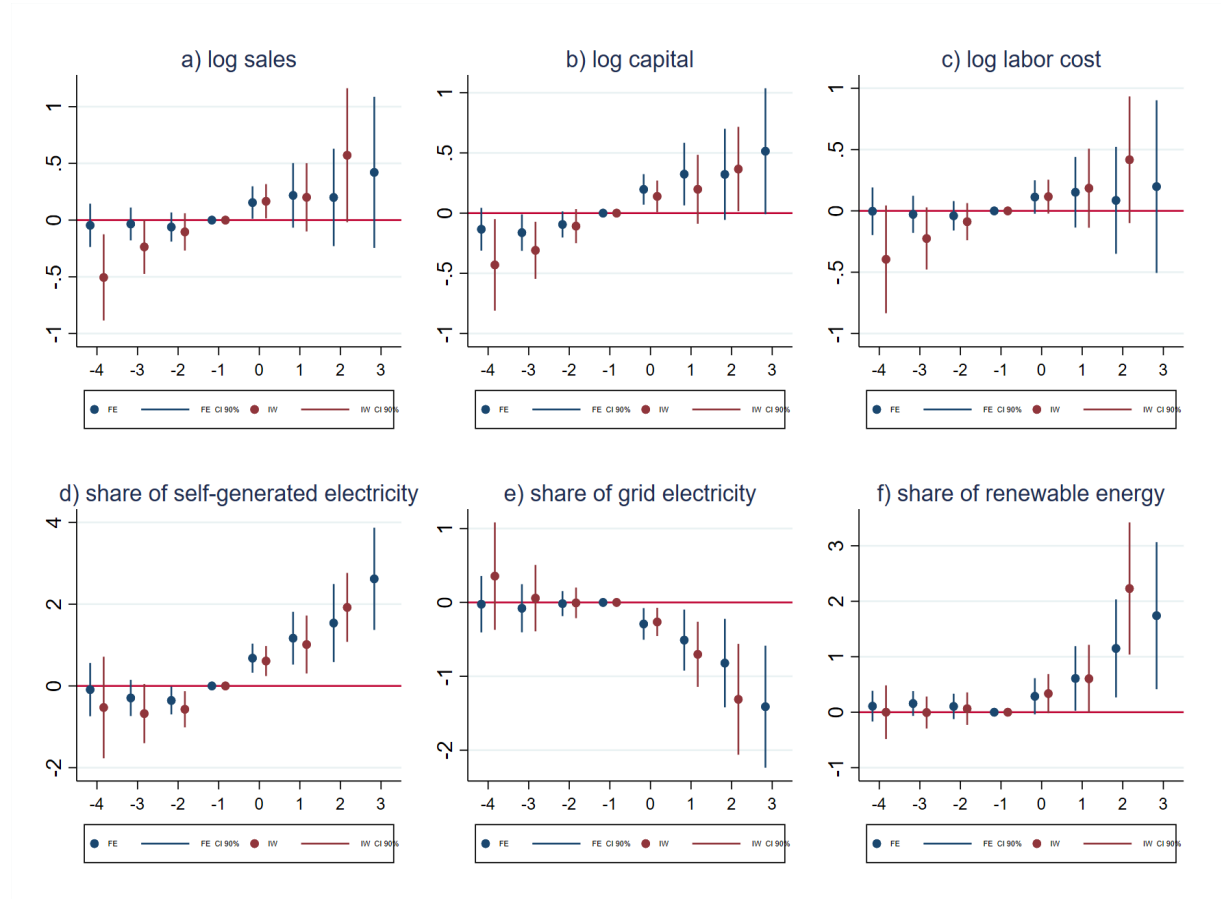
Figure 3: Results for CO<sub>2</sub> emissions intensity in sales and in energy, with only ever-treated firms



Notes: Estimates of coefficients  $\mu_\ell$  for  $\ell = -4, \dots, 3$  from the FE estimator (4) and  $\hat{v}_\ell$  for  $\ell = -4, \dots, 2$  from the IW estimator (6), using the restricted sample of ever-treated firms, are reported graphically. Panel a) reports these coefficients for CO<sub>2</sub> emission intensity and panel b) for CO<sub>2</sub> content in energy use. The x-axis represents the value of  $\ell$ , the dots the point estimates, the bar the 90% confidence intervals. Standard errors are clustered at the firm level. Number of firm-year observations is 904 for the FE estimator and 742 for the IW estimator, and number of firms is 186 and 179, respectively.

Figure 4 reveals that, after registering a CDM project, firms increased their shares of self-generated electricity and renewable energy in total energy expenditures and reduced their share of electricity purchased from the grid in total energy expenditures. These changes indicate that CDM projects impacted firms' technology, in particular their energy sourcing. We estimate that participating firms increased the share of self-generated electricity in energy expenditures by 60% in  $\ell = 0$ , by 101% in  $\ell = 1$ , and by 192% in  $\ell = 2$ , statistically significant at the 1%, 5%, and 1% levels respectively, using the IW estimator. The magnitudes of the FE estimates are very similar (all with  $p$ -value  $< 0.01$ ), which suggests that there might not be a contamination issue for this outcome. Starting from an average of 20% of energy cost in the 4-year period before registration ( $\ell = -1$ ), self generation saw its cost share increase to 32% in  $\ell = 0$  and 40% in  $\ell = 1$ . This effect is thus strong in magnitude and long-lasting. However, as time passes, the number of firms on which the estimates are based declines. For instance, only firms that belong to cohort 1 can be observed 9 to 12 years ( $\ell = 2$ ) after registration using the IW estimator. Furthermore, we notice that the estimates in  $\ell = -2$  are negative and statistically significant at the 10% and 5% levels, respectively using the FE and IW estimators. These

Figure 4: Results for total sales, capital, labor cost, the shares of self-generated and grid electricity and renewable energy in total energy expenditures, with only ever-treated firms



*Notes:* Estimates of coefficients  $\mu_\ell$  for  $\ell = -4, \dots, 3$  from the FE estimator (4) and  $\hat{v}_\ell$  for  $\ell = -4, \dots, 2$  from the IW estimator (6), using the restricted sample of ever-treated firms, are reported graphically. Panel a) reports these coefficients for sales, panel b) for capital, panel c) for labor cost, and panels d), e), f) for the shares of self-generated electricity, grid electricity and renewable energy in energy expenditures, respectively. The x-axis represents the value of  $\ell$ , the dots the point estimates, the bar the 90% confidence intervals. Standard errors are clustered at the firm level. Number of firm-year observations range from 713 for labor cost (IW) to 1117 for sales (FE), and number of firms range from 173 to 217.

pre-trends suggest that will-be-treated firms were already on a growing trend in their use of self-generated electricity 5 to 8 years before registering a CDM project, but the patterns are not as evident as in the case of sales and capital.

Figure 4 also reports that the share of electricity purchased from the grid decreased by 26% in  $\ell = 0$ , by 70% in  $\ell = 1$ , and by 131% in  $\ell = 2$  (with p-value at least inferior to 0.05), using the IW estimator. Again, the magnitudes using the FE estimator are similar, and we can reject pre-trends for this outcome. Combining the two previous results indicates that, after registering a CDM project, firms shifted their energy sourcing from grid electricity to

self generation. Additionally, we estimate that the share of renewable energy expenditure increased by 60% in  $\ell = 1$  and by 223% in  $\ell = 2$  (significant at the 10% and 1% levels, respectively), using the IW estimator. The magnitudes using the FE estimator are similar, and we can also reject pre-trends. This result indicates that CDM projects helped firms rely more on renewable energy. However, these shifts in energy sourcing – which reflects a technique effect – were not sufficient to lower CO<sub>2</sub> emissions at the firm level.

*Robustness.* In Appendix Table D.11, we show that our results are partly robust to using the full sample of ever-treated firms. On the one hand, the estimates are no longer statistically significant at the 10% level for the effect on CO<sub>2</sub> emissions and energy use. Yet, the sign and magnitude of the estimates are preserved. For instance, with a p-value of 0.135, we estimate a 12% increase in CO<sub>2</sub> emissions in  $\ell = 0$ , using the IW estimator. On the other hand, the lack of statistically significant effect on CO<sub>2</sub> emission intensity in sales and in energy use is robust to the larger sample of ever-treated firms. Furthermore, Table D.12 shows that the increases in total sales, capital, and self generation, and the decline in expenditure share from electricity from the grid are robust to the full sample of ever-treated firms. We also validate the absence of pre-trends for all outcomes when using the full sample.

We further compare our results to the ones obtained when we control for period fixed effects, instead of sector-period fixed effects. Table D.13 shows that signs and magnitudes are similar, but we lose statistical significance for some outcomes (e.g., CO<sub>2</sub> emissions and sales). Moreover, the issue of pre-trends is more prevalent in this case.

*Discussion.* Using a sample of ever-treated firms with an empirical strategy that is robust to dynamic effects heterogeneity, and assigning the role of control firms to the ones that participated in the CDM after 2012, we found that participating firms increased their CO<sub>2</sub> emission levels, their energy use, their sales, and capital stock, that they changed their energy sourcing, while leaving their emission intensities unaffected. Firms registering a CDM project thus made an investment that raised both their use of energy and their CO<sub>2</sub> emissions, which rules out a purely carbon-free renewable energy investment (exploiting wind or solar energies, for instance) even though this type of investments is frequent.

Since the increase in sales, which occurred immediately after the investment was made, is simultaneous and of similar magnitude to the increase in CO<sub>2</sub> emissions, we can rule out that participating firms were able to find a new source of revenues (e.g., from CER credits) that had no impact on their emissions. This is in line with the observation that most Indian firms that engaged in CDM projects had not signed an emission reduction purchase agreement with a CER buyer at the time of registration (Warnecke et al., 2015). As firms made investments in capital and change their energy sourcing for electricity, we can speculate that firms adopted better technologies through the CDM project, which reduced their marginal cost and eventually made their demand increase, thus resulting in an increase in both production and emissions. This corresponds to a scale effect.<sup>22</sup> Clearly,

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<sup>22</sup>A thorough reading of PDDs reveals that some participating firms anticipated this scale effect in the form of plant expansion or green field projects, as a result of better access to maintenance and quality control skills from technology suppliers, or as a result of access to more reliable electricity supply.

the increase in CO<sub>2</sub> emissions goes against the stated goal of the Clean Development Mechanism. Our results reveal that CDM projects helped firms to invest and potentially to grow, but not to reduce their emissions.

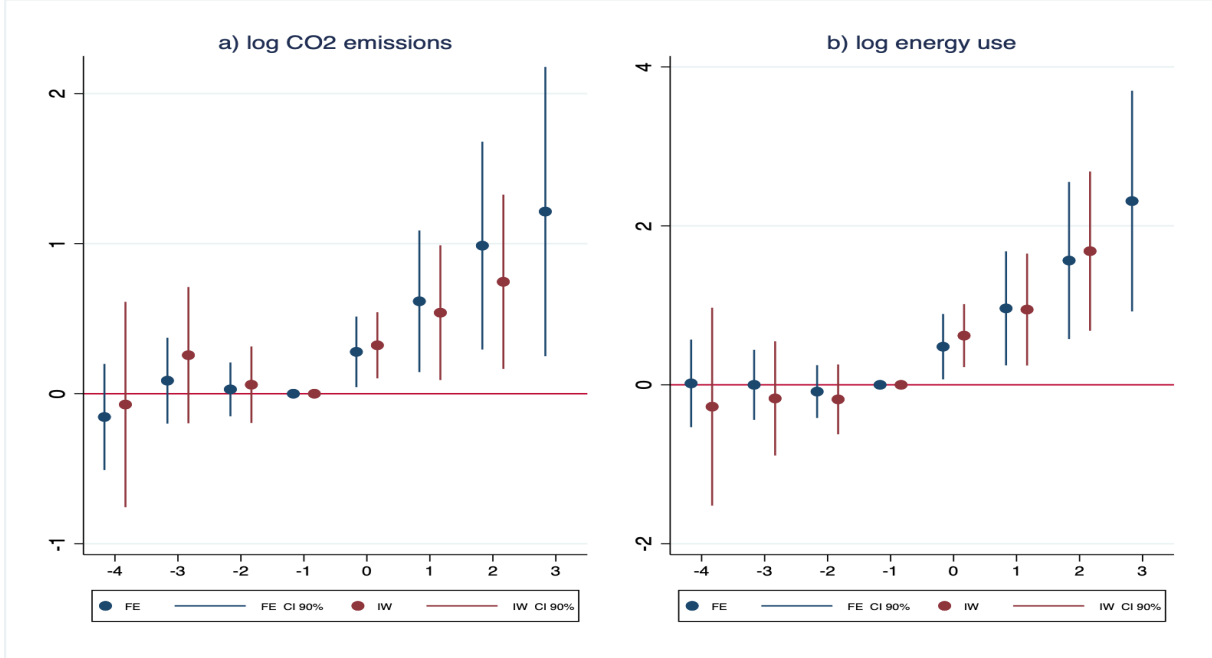
Turning to the estimated zero effect on CO<sub>2</sub> emission intensity in sales and in energy, it suggests that emission levels, energy use, and sales evolved in similar fashion. Thus, evidence of a technique effect is almost absent in our context. Yet, we are cautious not to over-interpret our results given the small number of treated firms in our sample. Also, we noticed that firms belonging to different cohorts were not following parallel trends for some outcomes, even after controlling for unobserved time-invariant characteristics. Therefore, we further explore this question using a matched sample where control firms never participated into CDM projects, but were selected to be paired with a treated firm with similar characteristics.

## 4.2 Main results from the matched sample

Figure 5 presents the estimation results for the FE regression (7) and for the IW estimator (8) for CO<sub>2</sub> emissions and energy use outcomes using the matched sample. The estimates  $\hat{\mu}_\ell$  and  $\hat{v}_\ell$  are all statistically significant at least at the 10% and 5% levels, respectively, after the registration of the project ( $\ell \geq 0$ ). We validate the absence of pre-trends in both the FE and IW regression designs. The magnitudes are large since, using the IW estimator, CO<sub>2</sub> emissions in treated firms increased by 32% in  $\ell = 0$ , by 54% in  $\ell = 1$ , and by 74% in  $\ell = 2$  (all with a p-value  $< .05$ ) more than in their matched control firms, relatively to the initial within-pair difference in the period before registration. Similarly, using the IW estimator, we estimate that energy use in treated firms increased by 62% in  $\ell = 0$  (p-value  $< .05$ ), by 95% in  $\ell = 1$  (p-value  $< .05$ ), and by 168% in  $\ell = 2$  (p-value  $< .01$ ) more than in their matched control firms. For the IW estimator, these increases are relative to the difference in outcomes between treated firms from the fourth cohort and their matched control firms. The FE estimator indicates similar magnitudes than the IW estimator, which suggests that heterogeneity in dynamic treatment effects does not bias the results of the two-way fixed-effect estimator for these outcomes in the matched sample.

Figure 6 presents the estimation results for the dynamic effects of CDM projects on firm's CO<sub>2</sub> emission intensity in total sales and in energy use. We find that participating in a CDM project did not significantly impact CO<sub>2</sub> emission intensity in total sales in treated and control firms differently, but it reduced significantly the relative CO<sub>2</sub> content in energy use of treated firms. Using the IW estimator, the CO<sub>2</sub> content in energy use in treated firms decreased by 30% in  $\ell = 0$  (p-value  $< .1$ ), by 41% in  $\ell = 1$  (p-value = .108), and by 94% in  $\ell = 2$  (p-value  $< .05$ ) more than in their matched control firms, relatively to the initial within-pair difference in the period before registration and to the fourth cohort matched pairs. The FE estimator indicates similar magnitudes than the IW estimator, at least for the first two 4-year periods, but the estimates are not statistically significant except the one in period  $\ell = 3$ . For this outcome, for which there is a discrepancy between the FE and IW estimates, we note that the FE estimates may suffer from a contamination issue across periods, which justifies our preference for the IW estimator. We also validate

Figure 5: Results for CO<sub>2</sub> emissions and energy use, with semi-parametric matching

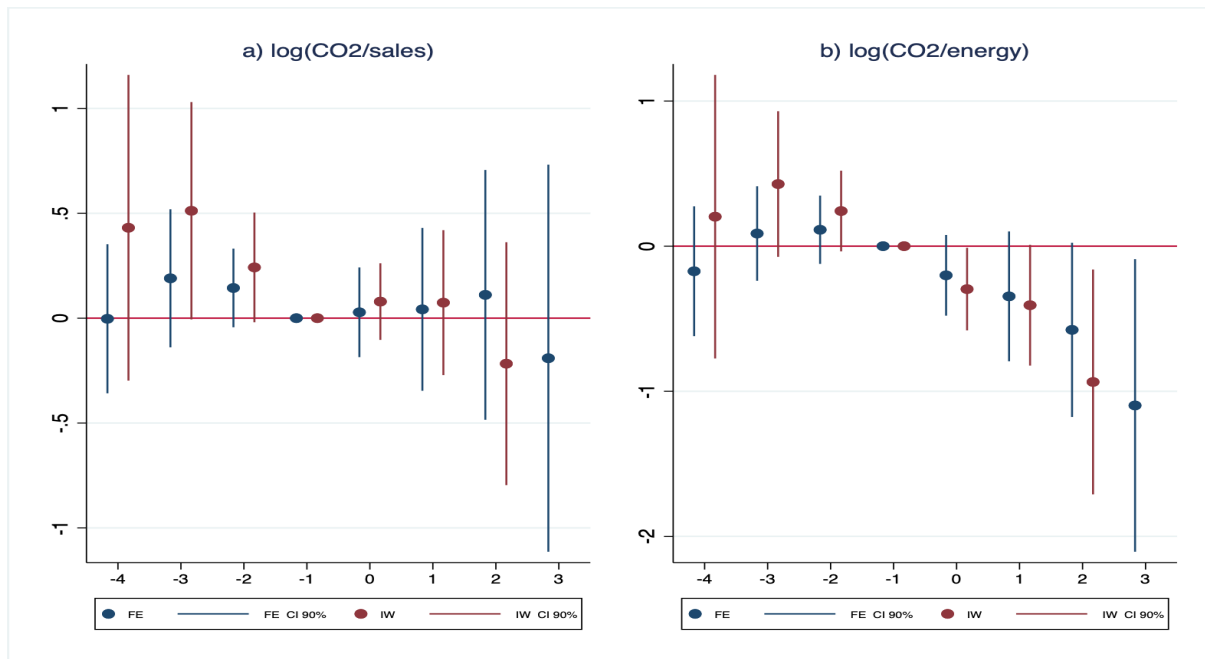


*Notes:* Estimates of coefficients  $\mu_\ell$  for  $\ell = -4, \dots, 3$  from the FE estimator (4) and  $\hat{v}_\ell$  for  $\ell = -4, \dots, 2$  from the IW estimator (6) are reported graphically. Panel a) reports these coefficients for CO<sub>2</sub> emissions and panel b) for energy use. The x-axis represents the value of  $\ell$ , the dots the point estimates, the bar the 90% confidence intervals. Standard errors are clustered at the matched-pair level. Number of observations is 901 for the FE estimator and 745 for the IW estimator, and number of matched pairs is 188 and 182, respectively.

the absence of pre-trends.

Finally, Figure 7 presents the estimation results for the FE regression ( $\hat{\mu}_\ell$ ) and for the IW estimator ( $\hat{v}_\ell$  only) for total sales, capital stock, labor cost, and the shares of self-generated electricity, renewable energy, and electricity from the grid in total energy cost. We validate the absence of pre-trends for all outcomes. Using the IW estimator, we estimate, with a 5% level of statistical significance, that firms registering a CDM project experienced 24% higher sales in  $\ell = 0$ , 52% in  $\ell = 1$ , and 92% in  $\ell = 2$ , relative to their matched counterparts. Only the FE estimates for periods  $\ell = 2$  and  $\ell = 3$  are statistically significantly different from zero (at the 10% level) for sales, but the magnitudes are similar across regression designs. Similarly, using the IW estimator, we find that capital stock in treated firms increased by 27% in  $\ell = 0$ , by 58% in  $\ell = 1$ , and by 97% in  $\ell = 2$  (all with a p-value  $< .01$ ) more than in their matched control firms, relatively to the initial within-pair difference in the period before registration and to the fourth cohort matched pairs. The FE estimator for capital indicates similar effects in magnitude, with a 27% higher level in  $\ell = 0$  (p-value  $< 0.01$ ), 55% in  $\ell = 1$  (p-value  $< 0.05$ ), 77% in  $\ell = 2$  (p-value  $< .05$ ), and 128% in  $\ell = 3$  (p-value  $< .01$ ). The effect of CDM registration on capital is thus large and long-lasting. The large magnitude of the latest periods must be considered with caution

Figure 6: Results for CO<sub>2</sub> emissions intensity in sales and in energy, with semi-parametric matching



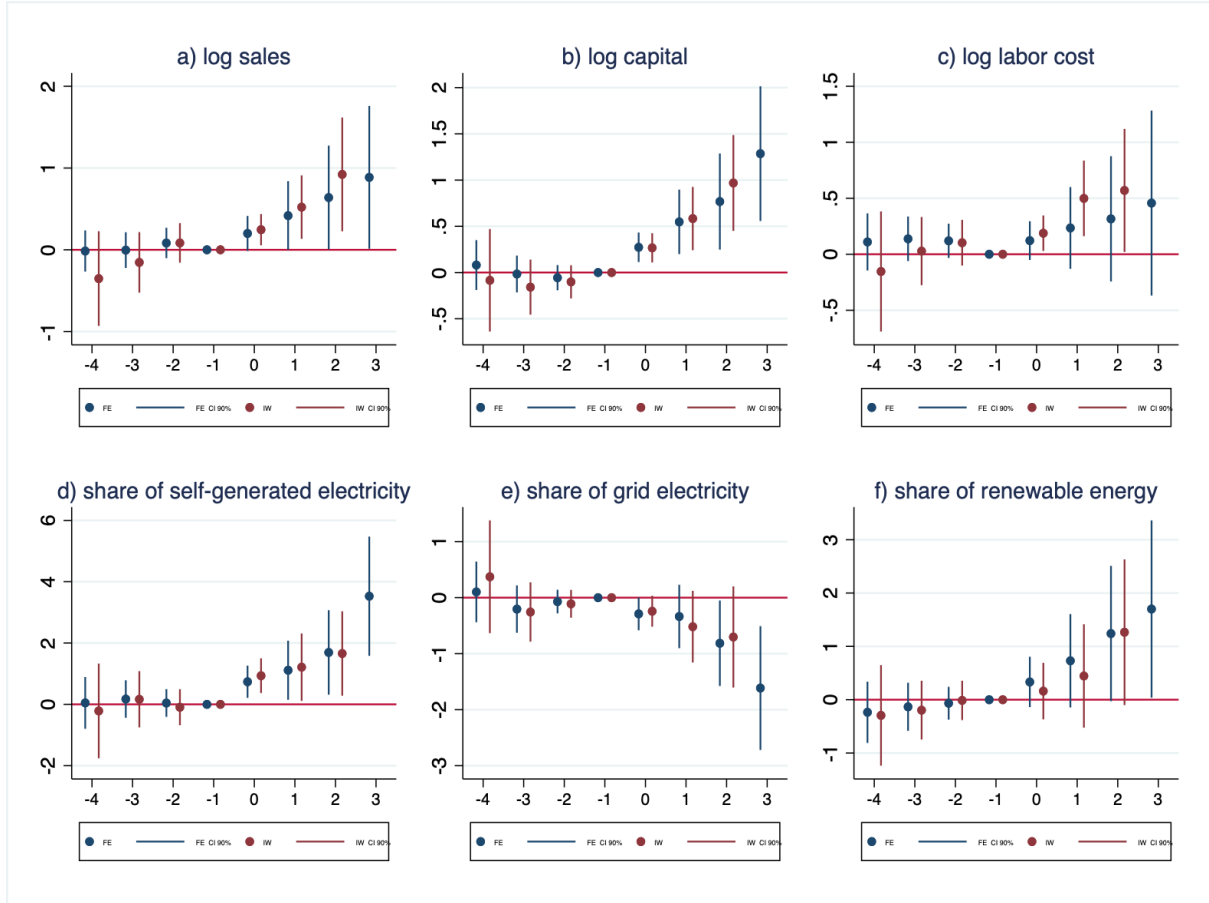
*Notes:* Estimates of coefficients  $\mu_\ell$  for  $\ell = -4, \dots, 3$  from the FE estimator (4) and  $\hat{v}_\ell$  for  $\ell = -4, \dots, 2$  from the IW estimator (6) are reported graphically. Panel a) reports these coefficients for log of CO<sub>2</sub> emission intensity and panel b) for log of CO<sub>2</sub> content in energy use. The x-axis represents the value of  $\ell$ , the dots the point estimates, the bar the 90% confidence intervals. Standard errors are clustered at the matched-pair level. Number of observations is 901 for the FE estimator and 745 for the IW estimator, and number of matched pairs is 188 and 182, respectively.

since, as time passes, the number of firms on which the estimates are based declines and only the first cohort experienced up to 13-16 years and 9-12 years of treatment in the FE and IW regression design, respectively.

Although we failed to reject the null hypothesis of no effect on labor cost when we used the sample of ever-treated firms, Figure 7 reveals, using the IW estimator, that labor cost increased more in treated firms by 19% in  $\ell = 0$ , by 50% in  $\ell = 1$ , and by 57% in  $\ell = 2$  (with p-value  $< .05$ ,  $.05$ , and  $.1$ , respectively) than in their matched counterparts. This suggests that when participating firms made capital investment, technological change was not factor-biased since all inputs (labor, capital, and energy) increased in similar ways.

As previously found for the sample of ever-treated firms, Figure 7 reveals that, after registering a CDM project, firms depended more on self generation for electricity. Indeed, we estimate that participating firms increased the share of self-generated electricity in energy expenditures by 93% in  $\ell = 0$ , by 121% in  $\ell = 1$ , and by 166% in  $\ell = 2$  (with p-value  $< .01$ ,  $.1$ , and  $.1$ , respectively) more than their paired counterparts relative to the difference between the fourth cohort matched pairs, using the IW estimator. The magnitudes of the FE estimates are similar, with one exception in  $\ell = 3$  for which the

Figure 7: Results for total sales, capital, labor cost, the shares of self-generated and grid electricity, and the share of renewable energy expenditure, with semi-parametric matching



Notes: Estimates of coefficients  $\mu_\ell$  for  $\ell = -4, \dots, 3$  from the FE estimator (4) and  $\hat{v}_\ell$  for  $\ell = -4, \dots, 2$  from the IW estimator (6) are reported graphically. Panel a) reports these coefficients for log of sales, panel b) for capital, panel c) for labor cost, and panels d),e),f) for the shares of self-generated electricity, grid electricity and renewable energy in total energy cost, respectively. The x-axis represents the value of  $\ell$ , the dots the point estimates, the bar the 90% confidence intervals. Standard errors are clustered at the matched-pair level. Number of observations range from 761 for renewable energy share (IW) to 1045 for sales (FE), and number of matched pairs range from 186 to 197.

magnitude is larger (353% increase, with p-value  $< .01$ ) while the effect is estimated on firms belonging to cohort 1 only. The decrease in the share of electricity purchased from the grid and the increase in renewable energy are only statistically significant for the latest periods using the FE estimator, and are not robust to the IW regression design. Yet, despite the noise of the estimates, Figure 7 reveals similar patterns to the ones observed in Figure 4 for these outcomes.

*Robustness.* In Appendix Tables D.14 and D.15, we show that our results are robust to using the sample of CDM firms from sectors with well-balanced distribution across cohorts,

as in Section 4.1, with their matched counterparts. Overall, we validate the absence of pre-trends using this restricted sample, except in two occurrences (for CO<sub>2</sub> intensity in sales in  $\ell = -2$  using the IW estimator and for CO<sub>2</sub> content in energy in  $\ell = -4$  using the FE estimator). In Table D.16, we show that the results on CO<sub>2</sub> emissions, CO<sub>2</sub> intensity, total sales, capital, and self generation from registering a CDM project are similar when we control for period fixed effects, instead of sector-period fixed effects. Additionally, we obtain robust results for these outcomes when we implement 2:1 nearest neighbor matching, as noted in Table D.17, in particular in the increase in CO<sub>2</sub> emissions and the decline in CO<sub>2</sub> content in energy. We note that the result on self generation sees its statistical significance decline, especially with the IW estimator. Table D.18 further validates the robustness of our results for a wider set of matched firms (218 pairs) obtained by dropping the distance restriction imposed in the main sample.

*Discussion.* Using semi-parametric matching with an empirical strategy that is robust to dynamic effects heterogeneity, we verify that participating firms increased their CO<sub>2</sub> emission levels, their energy use, their sales and capital stock, and that they changed their energy sourcing, while leaving their CO<sub>2</sub> emission intensity in value unaffected, as with the sample of ever-treated firms only. Using the matched sample, we find in addition that firms increased their labor cost (revealing that technology investments were not biased away from labor) and decreased their CO<sub>2</sub> content in energy.

The magnitudes of the effects observed using the semi-parametric matching research design are almost doubled relative to the magnitudes observed using the event-study design with ever-treated firms only. This discrepancy can be explained in two ways. First, in the sample of ever-treated firms, firms belonging to cohorts 1, 2, and 3 were not entirely comparable to firms in cohort 4. Using an empirical strategy that rests on across-cohort comparison could therefore bias the effect of CDM projects and result in a downward bias if firms belonging to cohort 4 were already on a trend that was closer to post-treatment trend than pre-treatment trend for cohorts 1, 2, and 3. Second, in the matched sample, we control for the time-invariant log difference in any outcome between treated firms and their matched pairs. By construction, outcomes in the pre-period  $\ell = -1$  are similar between treated firms and their matched pairs, especially for CO<sub>2</sub> emission levels. Starting from low difference values, the magnitudes in terms of proportional change can then be large.

Combining the evidence from our different empirical designs, we can tentatively conclude that firms registering a CDM project were affected by a strong scale effect. It is revealed by the increase in sales, energy use, and CO<sub>2</sub> emissions in post-treatment periods. By contrast, the evidence of a technique effect is much weaker, since it is only revealed by the decrease in the CO<sub>2</sub> content in energy in participating firms in the matched sample. Lowering the carbon content of energy was probably achieved through the shift toward more self-generated electricity, especially if this electricity was produced with renewable energies. Yet, we have weak evidence of the increase in renewable energy use in the matched sample. We thus conclude that this technique effect was dominated by the scale effect since CO<sub>2</sub> emissions were not reduced, but increased, after the registration of a CDM project.



## 5 Conclusion

Using firm-level data for the manufacturing sector in India, we assessed whether participating in CDM projects had an impact on firms' environmental performance. Our assessment of the efficiency of the Clean Development Mechanism is pessimistic, as we observed that CO<sub>2</sub> emissions were not reduced, but increased, after registering a CDM project. This evidence relies on manufacturing firms only, and on the emissions that are related to fuel consumption, thereby leaving some potential extra channels through which firms could reduce their emissions unaccounted for.

With our data, we were able to identify the changes associated with CDM projects at the firm level. Participating firms made investments and changed the sourcing of electricity by relying more on self generation and renewable energy than on the grid, which allowed them to grow more in terms of total sales, energy consumption, and CO<sub>2</sub> emissions. This reveals the importance of a scale effect that pushed CO<sub>2</sub> emissions up as firms produced more. The evidence of the scale effect can be found using our two empirical strategies – two-way fixed effect regressions and interaction-weighted estimator – using both our sample of ever-treated firms only and our matched sample, but the evidence is stronger for the matched sample. The scale effect was counterbalanced by a small technique effect, revealed by the decline in the CO<sub>2</sub> content of energy, but this effect was only identified in the sample with semi-parametric matching. Furthermore, the CO<sub>2</sub> emission intensity in sales remained unaffected by CDM projects, in both samples. From this evidence, we tentatively conclude that the scale effect dominated the technique effect, leaving treated firms with higher CO<sub>2</sub> emission levels after the registration of their first CDM project.

This large scale effect following firm-level investments may find a parallel in the “rebound effect” observed when households invest in less emission intensive appliances, buy more energy-efficient cars, or weatherize their home (Steren et al., 2016; Fowlie et al., 2018; Gillingham et al., 2020). The rebound effect is usually triggered by behavioral changes: households raise the indoor temperature during the winter after weatherization, they drive more their energy-efficient cars, etc. Similarly, the scale effect may be driven by a more intensive use of cheaper and more reliable energy. Yet, market forces are more likely to drive the scale effect than behavioral changes, as firms' productivity usually improves after investments. We leave for future research the exploration of investment-induced and innovation-induced scale effects in other empirical contexts.

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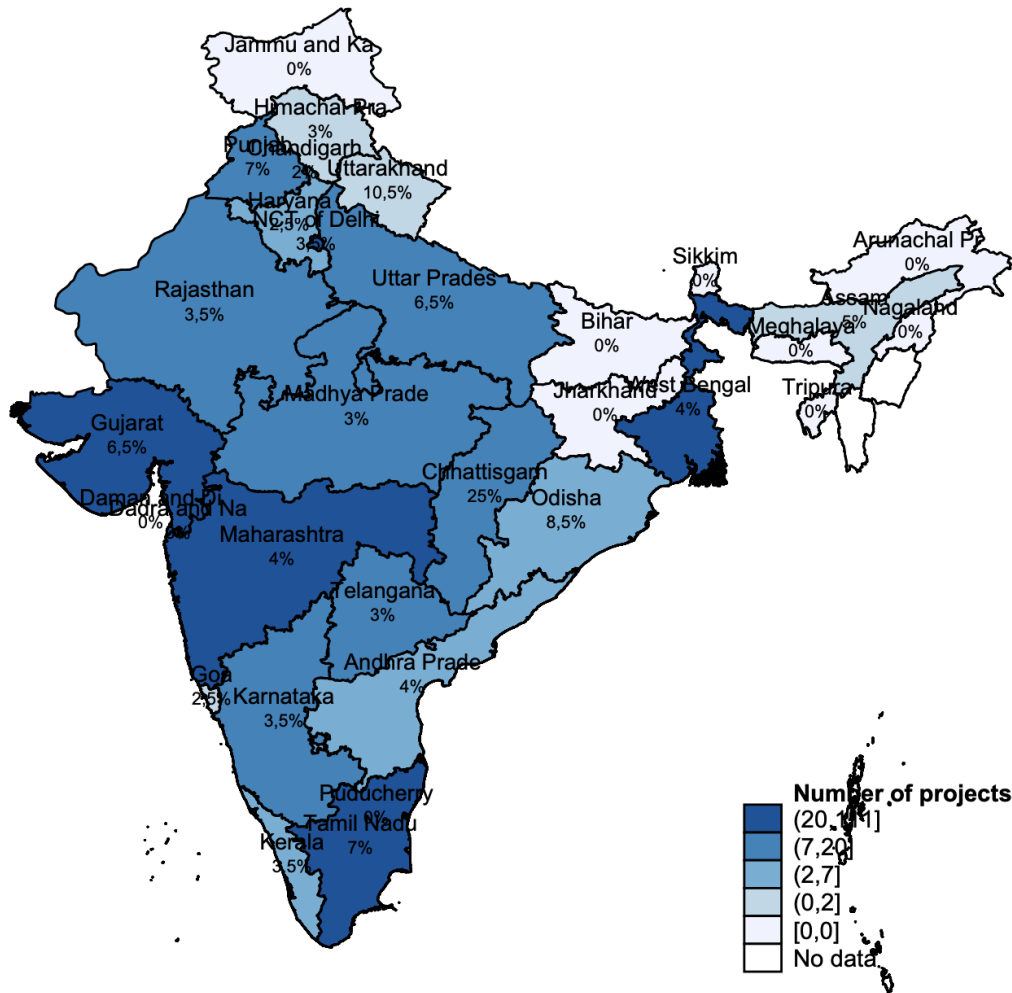
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# Appendix

## A Describing CDM projects

Figure A.1: Distribution of CDM projects by Indian states



*Notes:* All CDM projects are located in an Indian state depending on the headquarter address of the participating firm. If a CDM project is common across several firms, we assume that each participating firm has one CDM project. We use the restricted sample of 217 firms hosting 338 CDM projects. We report the total number of CDM projects by state using shades of blue. We also report the percentage of manufacturing firms that are associated with a CDM project relative to all manufacturing firms that are present in ProWess for each state (the figure is indicated below the name of each state).

Figure A.2: Distribution of CDM treatment for ever-treated firms by periods

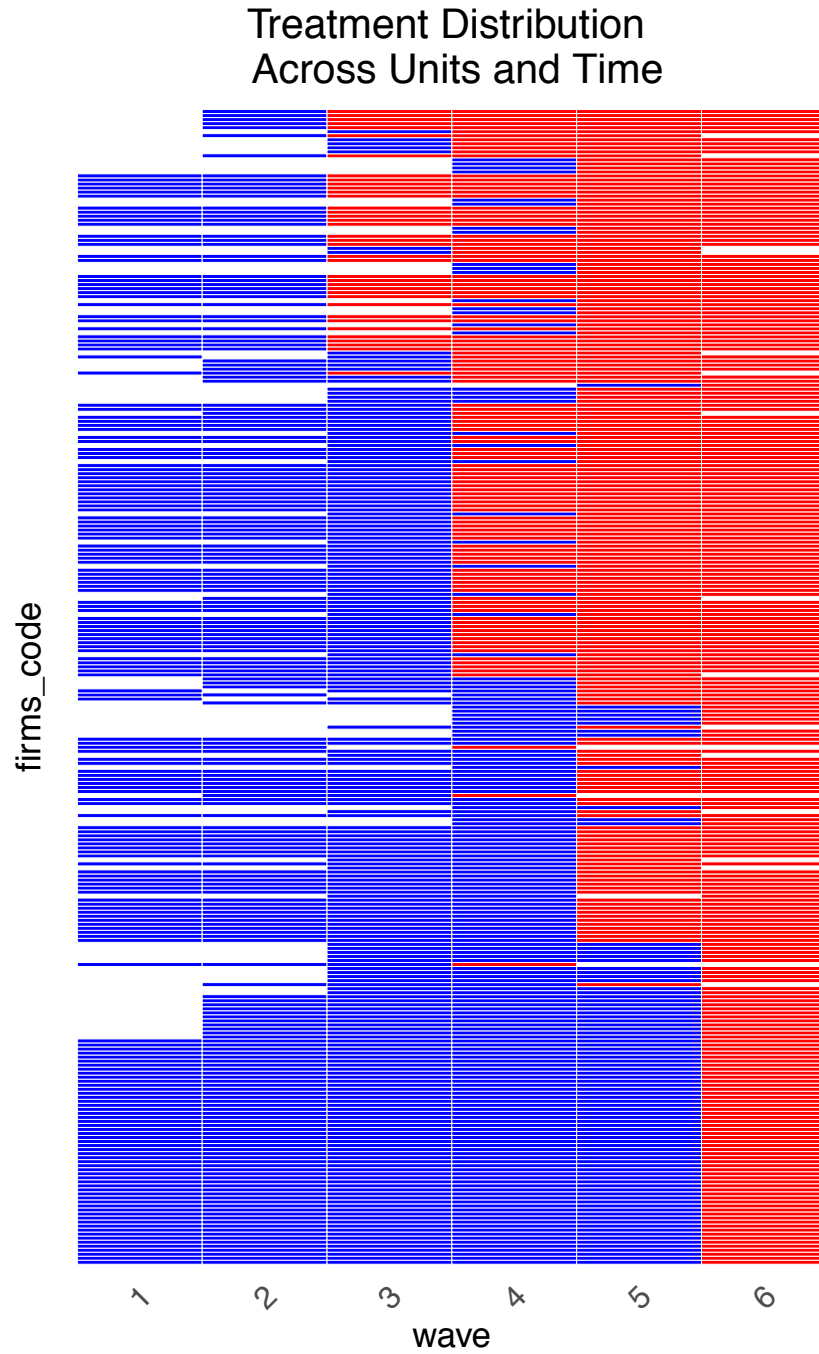


Table A.1: Policy framework for renewable energy in India

Policy framework	Details	Dates	Institutional level
<b>Instruments</b>			
Accelerated depreciation	Renewable projects are allowed to depreciate 80% in the first year.	From mid-90s to 2012	Federal
Generation based incentive	A subsidy of US 0.01 per kWh generated.	From 2009	Federal
Feed-in tariffs	Preferential tariffs set by state Electricity Regulatory Commissions.	From 2000	State
Renewable energy certificates	Market-based instrument to address the mismatch between availability and requirement of utilities to meet their state-level renewable purchase obligation.	From 2011	Federal
Income tax exemption	A 100% tax waiver on profits for any 10-year period during the first 15 years of the operational life of a power generation project.	From 2002 to 2013	Federal
Other benefits (excise, wheeling charges)	Concessional rates for excise and customs duty.	From 2002	Federal
<b>Recent Policies</b>			
National Action Plan on Climate Change	National target of renewable energy set at 5% in 2010, with 1% increase annually till 2020.	From 2010	Federal
Tamil Nadu Solar Energy Policy	Target of 3000 MW of solar power by 2015.	From 2012	Tamil Nadu State
Tamil Nadu Wind Energy Policy	Target of 5500 MW of wind power.	From 2015	Tamil Nadu State
Rajasthan Solar Energy Policy	Target of 25000 MW of installed capacity.	From 2014	Rajasthan State
Rajasthan Wind Energy Policy	Wind power plants can go for direct sale to distribution utilities, with the objective of 300 MW in 2013-14, 400 MW in 2014-15, 500 MW in 2015-16.	From 2012	Rajasthan State
Gujarat Solar Power Policy	Foster large scale solar power generation plants using differentiated tariffs.	From 2015	Gujarat State
Gujarat Wind Power Policy	Coordination by the Gujarat Energy Development agency.	From 2016	Gujarat State
Maharashtra Renewable Energy Policy	Target of 7500 MW for solar capacity and 5000 MW for wind power capacity.	From 2015	Maharashtra State

Sources: Shrimali et al. (2013) and Dasgupta & Sankhyayan (2018).



Table A.2: Size of cohorts for the restricted sample, by sector

Sectors	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Total
Food, beverages & tobacco	4	21	13	14	52
Textiles	11	12	8	14	45
Chemicals	5	11	10	8	34
Nonmetallic mineral prod.	7	7	6	8	28
Basic & fabricated metals	3	11	23	21	58
All sectors	30	62	60	65	217

*Notes:* This Table is an extract from Table 2 where we impose the restriction that sectors must have a well-balanced distribution across cohorts to form the restricted sample of 217 CDM firms.

Table A.3: Distribution of CDM projects across the different samples of analysis

Type of project	All CDM projects	Samples of analysis		
		Prowess 294 firms	Restricted 217 firms	Matched 197 firms
Wind energy	717	219	159	149
Biomass energy	263	94	69	71
Hydro energy	167	2	1	1
Solar energy	133	9	0	0
EE through own generation	75	47	45	36
EE in industrial process	59	34	25	23
Fossil fuel switch	30	10	9	3
Others	225	48	30	28
Total	1669	463	338	311

*Notes:* “EE” stands for Energy Efficiency projects.

Table A.4: Distribution of CDM projects across states for the restricted sample of ever-treated firms

States	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Total
Tamil Nadu	17	16	14	12	59
Maharashtra	2	20	17	20	59
Gujarat	9	6	19	7	41
NCT of Delhi	6	12	9	11	38
West Bengal	15	6	5	6	32
Rajasthan	3	11	4	1	19
Uttar Pradesh	0	10	4	0	14
Punjab	2	8	0	3	13
Madhya Pradesh	0	5	5	2	12
Chhattisgam	0	4	5	2	11
Telangana	1	0	3	5	9
Odisha	4	1	0	1	6
Andhra Pradesh	0	0	3	3	6
Karnataka	0	2	2	1	5
Haryana	0	2	2	0	4
Kerala	0	0	2	1	3
Himachal Pradesh	0	2	0	0	2
Dadra and Nagar Haveli	0	0	0	2	2
Chandigarh	1	0	0	0	1
Uttarakhand	0	0	1	0	1
Goa	0	1	0	0	1
Total	60	106	95	77	338

*Notes:* All CDM projects are located in an Indian state depending on the headquarter address of the participating firm. States are ranked in decreasing order of the number of CDM projects located on their territory. We focus on the restricted sample of 217 firms hosting 338 CDM projects.

Table A.5: Distribution of firms across states for the matched sample

States	Cohort 1		Cohort 2		Cohort 3		Cohort 4		All cohorts	
	CDM	C	CDM	C	CDM	C	CDM	C	CDM	C
Tamil Nadu	14	3	10	2	7	8	9	6	40	19
Maharashtra	1	8	8	6	6	10	13	11	28	35
Gujarat	3	6	4	5	9	1	10	6	26	18
West Bengal	4	3	5	8	3	6	5	8	17	25
Uttar Pradesh	1	0	6	5	5	3	0	2	12	10
NCT of Delhi	1	3	3	9	2	5	5	2	11	19
Punjab	1	2	7	1	0	2	3	4	11	9
Chhattisgarh	0	0	3	0	4	0	2	2	9	2
Telangana	1	1	0	4	3	2	4	6	8	13
Karnataka	1	1	3	2	2	0	0	3	6	6
Rajasthan	1	1	2	3	2	2	1	1	6	7
Andhra Pradesh	1	0	0	1	3	1	1	0	5	2
Kerala	0	3	1	0	2	0	1	1	4	4
Madhya Pradesh	0	0	1	3	1	1	2	0	4	4
Odisha	2	1	1	4	0	3	1	0	4	8
Uttarakhand	0	0	0	0	2	0	0	0	2	0
Chandigarh	1	0	0	1	0	1	0	1	1	3
Dadra & Nagar Haveli	0	0	0	0	0	5	1	0	1	5
Haryana	0	0	0	0	1	0	0	1	1	1
Himachal Pradesh	0	0	1	0	0	0	0	1	1	1
Goa	0	0	0	0	0	0	0	2	0	2
Jharkhand	0	0	0	1	0	2	0	1	0	4
Total	32	32	55	55	52	52	58	58	197	197

*Notes:* This table describes the geographic distribution of CDM firms (denoted by ‘CDM’) and their matched control counterparts (denoted by ‘C’) by state and by cohort. States are ranked in decreasing order of the number of CDM firms located on their territory. We focus on the sample of 197 CDM firms and 197 control firms (some firms being considered several times because we perform matching with replacement).

## B Data Appendix

We compiled a firm-level panel dataset that spans the period from 1988 to 2016 based on the Prowess database, collected by the Centre for Monitoring the Indian Economy. Like most firm surveys, the Prowess database contains information primarily from income statements and balance sheets of large publicly listed companies.

Unlike firm-level data from the Annual Survey of Industries, data from Prowess are not anonymized. As a result, we used the name of firms to merge information about sales, production, and inputs from their annual reports to the list of Indian firms participating in CDM projects collected from the UNEP database CDM Pipeline. The names of firms participating in CDM projects were either extracted manually from firms' PDD files obtained by project name on the UNFCCC website (<https://cdm.unfccc.int/Projects/projsearch.html>) or from IGES CDM Project Database (Institute for Global Environmental Strategies, 2020). We were able to find 339 of our list of 734 firms that were successful in implementing at least one CDM project in the Prowess dataset.

We only exploited firm-level information from the Prowess database, even though many studies have focused on product-level information also contained in Prowess (Barrows & Ollivier, 2018; De Loecker et al., 2016; Goldberg et al., 2010). In their annual reports, firms give detailed accounts of both inputs – especially energy – and outputs. In particular, firms report total values of their sales, assets, and fixed assets (capital). On the input side, firms report total compensation to their workers (wages), the value of materials, and total fuel and energy expenditures. They also report in detail the value and quantity (with units) of each energy source – coal, electricity from the grid, natural gas, etc. – used in the production process. From this detailed information, we compute the shares of self-generated and grid electricity in total energy expenditures, and the share of renewable energy in total energy cost.

We used data on energy inputs to compute CO<sub>2</sub> emissions. We followed previous work in multiplying energy consumption by energy-specific CO<sub>2</sub> emission factors. In particular, we followed the same methodology as in Barrows & Ollivier (2018). We translated physical quantities of energy consumed into physical quantities of CO<sub>2</sub> emissions and sum over energy sources to compute firm-level CO<sub>2</sub> emissions. Source specific emission factors come from the US EPA 2012 Climate Registry Default Emissions Factors (<http://theclimateregistry.org/wp-content/uploads/2015/01/2012-Climate-Registry-Default-Emissions-Factors.pdf>). For electricity generation, we used the Indian average CO<sub>2</sub> emission intensity of grid electricity that equals 951 g of CO<sub>2</sub> per kWh. Once units have been standardized, if we could not match units of energy from Prowess with the EPA report, we dropped these observations. Overall, we were able to assign an emission factor to 83% of energy source unit pairs (e.g., kWh of electricity). Of the 17% of such pairs to which we could not assign an emission factor, in many cases it appeared that units were misspecified: for instance, we could not assign an emission factor to observations denominated in “liters” of electricity.

We also addressed an issue related to self-generated electricity. When firms produce their own electricity, they report energy inputs used for that production. Hence, to avoid

double counting, we computed CO<sub>2</sub> emissions for self-generated electricity based on reported energy inputs, and we did not use the national average CO<sub>2</sub> emission factor for grid electricity.

Finally, we treated outlier observations in the input dataset in two ways. First, we identified firm emission intensities which look like entry errors and assign to those values instead the average emission intensity of the firm over the period. This procedure affects less than 1% of the data. Next, we dropped entire firm profile if the emission intensity of the dirtiest year of the firm was at least 600 times greater than the cleanest year of the firm. We also dropped a few outlier observations with implausibly large implied CO<sub>2</sub> emissions.

Table B.6: Descriptive statistics for ever-treated firms in the full sample, by cohort

	Cohort 1	Cohort 2	Cohort 3	Cohort 4
Total sales (M.R.)				
mean (s.d.)	406 (1233)	695 (3945 )	1362 (6661)	146 (384)
# observations	227	415	399	478
Capital stock (M.R.)				
mean (s.d.)	264 (778)	580 (2912)	464 (1595)	103 (365)
# observations	227	415	399	478
Labor cost (M.R.)				
mean (s.d.)	19.8 (56.8)	34.3 (138.2)	26.6 (104.2)	6.6 (22.2)
# observations	227	415	399	478
CO <sub>2</sub> emissions (kt)				
mean (s.d.)	1230 (5354)	2457 (7285)	1594 (4794)	704 (3540)
# observations	188	355	301	300
Energy use (10 <sup>6</sup> MMBtu)				
mean (s.d.)	27 (178)	23 (73)	17 (48)	6 (27)
# observations	188	355	301	300
CO <sub>2</sub> intensity (kt/M.R.)				
mean (s.d.)	2304 (31463)	31.1 (483)	6.2 (25.5)	3 (6.7)
# observations	188	355	301	300
CO <sub>2</sub> content in energy use (t/MMBtu)				
mean (s.d.)	2.4 (20.7)	0.17 (0.15)	1.9 (25.4)	0.48 (2.7)
# observations	188	355	301	300
Share of self-generated electricity in energy cost				
mean (s.d.)	.25 (.25)	.21 (.21)	.18 (.21)	.17 (.23)
# observations	190	360	322	324
Share of grid electricity in energy cost				
mean (s.d.)	.43 (.30)	.41 (.28)	.41 (.30)	.57 (.30)
# observations	190	360	322	324
Share of renewable energy in energy cost				
mean (s.d.)	.04 (.11)	.06 (.18)	.05 (.15)	.02 (.09)
# observations	190	360	322	324

*Notes:* Data are available for the period of 1992-2015 for 41 CDM firms that belong to the cohort 1, 75 firms that belong to cohort 2, 84 firms that belong to cohort 3, and 94 firms that belong to cohort 4. All monetary variables are reported in millions of current year rupees, denoted by M.R. CO<sub>2</sub> emissions are reported in kilotons (kt) while energy use is reported in 10<sup>6</sup> MMBtu. The shares of self-generated and grid electricity, and the share of renewable energy correspond to the shares of these expenditures in total energy cost.

## C Balance and Common Support

### C.1 Restricted sample of ever-treated firms

Table C.7 compares the distributions of the outcome variables across the four cohorts, in the restricted sample of ever-treated firms. Using the non-parametric two-sample Wilcoxon rank-sum (Mann-Whitney) test, we were able to reject at the 1% significance level the hypothesis that two independent samples (e.g., samples of ever-treated firms in cohort 1 and in cohort 2, denoted 1 vs 2) are from populations with the same distribution, for most outcome variables. There are a few exceptions. In particular, the distributions for (log of) total sales and CO<sub>2</sub> content in energy use for firms in cohorts 1 and 4 are not distinguishable, the distributions of the share of renewable energy in total energy cost for cohorts 2 and 4, as well as for cohorts 3 and 4, are not distinguishable; and the distributions of the share of self-generation in energy cost for cohorts 3 and 4 are also not distinguishable.

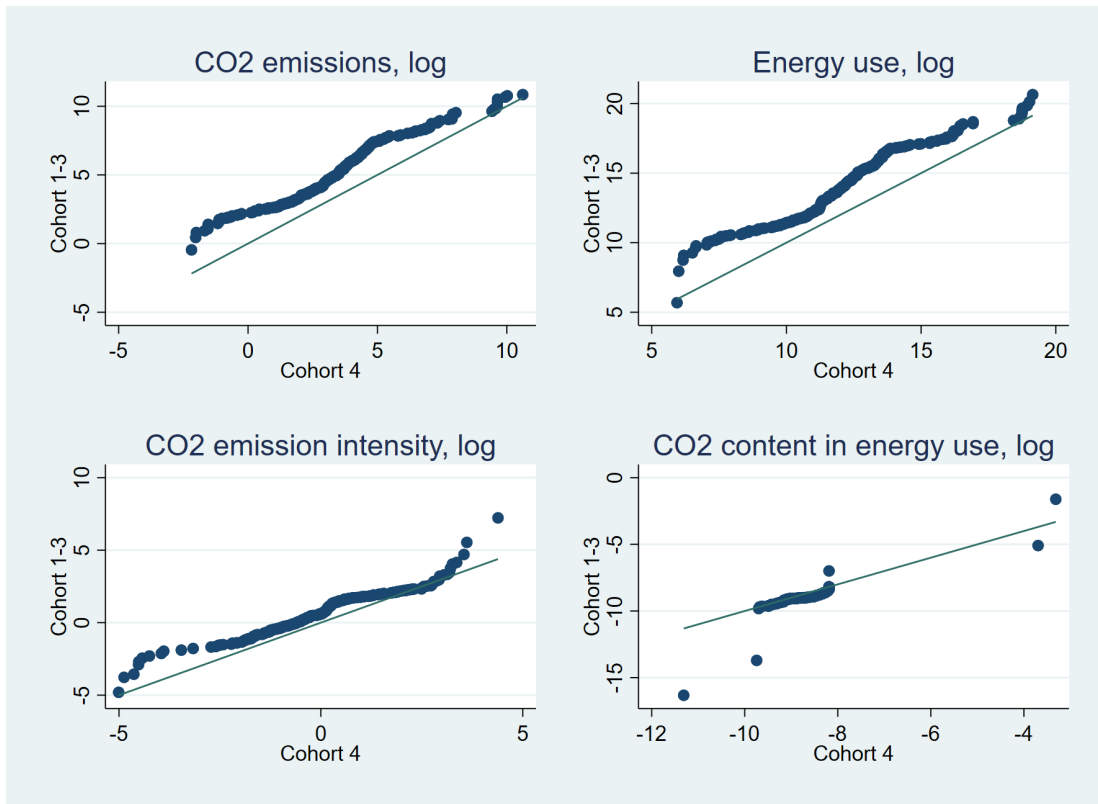
Using the sample of ever-treated firms, our research design rests on a comparison between treated cohorts 1,2,3 and cohort 4. In the following, we thus group all first three cohorts' outcomes together and draw a parallel with the outcomes of cohort 4. Figures C.3 and C.4 present the quantile-quantile (QQ) plots of the distributions of outcome variables for cohorts 1,2,3 and for cohort 4. The 45°-line corresponds to identical distributions. We observe deviations from the 45°-line for most outcome variables. In particular, firms in cohorts 1,2,3 tend to have higher CO<sub>2</sub> emissions and energy use than firms in cohort 4, for all quantiles.

Table C.7: Results from the non-parametric two-sample Wilcoxon rank-sum (Mann-Whitney) test, using the restricted sample of ever-treated firms

	Cohort 1	Cohort 2	Cohort 3	Cohort 4	1 vs 2	1 vs 3	1 vs 4	2 vs 3	2 vs 4	3 vs 4
	# obs.	# obs.	# obs.	# obs.	p-value	p-value	p-value	p-value	p-value	p-value
Total sales, log	162	343	284	328	0.000	0.000	0.175	0.148	0.000	0.000
Capital stock, log	162	343	284	328	0.005	0.252	0.000	0.061	0.000	0.000
Labor cost, log	162	342	284	328	0.000	0.347	0.001	0.000	0.000	0.000
CO <sub>2</sub> emissions, log	143	291	231	245	0.000	0.317	0.000	0.001	0.000	0.000
Energy use, log	143	291	231	245	0.000	0.092	0.001	0.008	0.000	0.000
CO <sub>2</sub> intensity, log	143	291	231	245	0.559	0.002	0.000	0.002	0.000	0.019
CO <sub>2</sub> content in energy, log	143	291	231	245	0.002	0.030	0.920	0.578	0.000	0.000
Self-generated electricity share	146	296	244	271	0.105	0.000	0.000	0.016	0.000	0.247
Grid electricity share	146	296	244	271	0.664	0.882	0.000	0.773	0.000	0.000
Renewable energy share	146	296	244	271	0.046	0.054	0.012	0.938	0.525	0.608

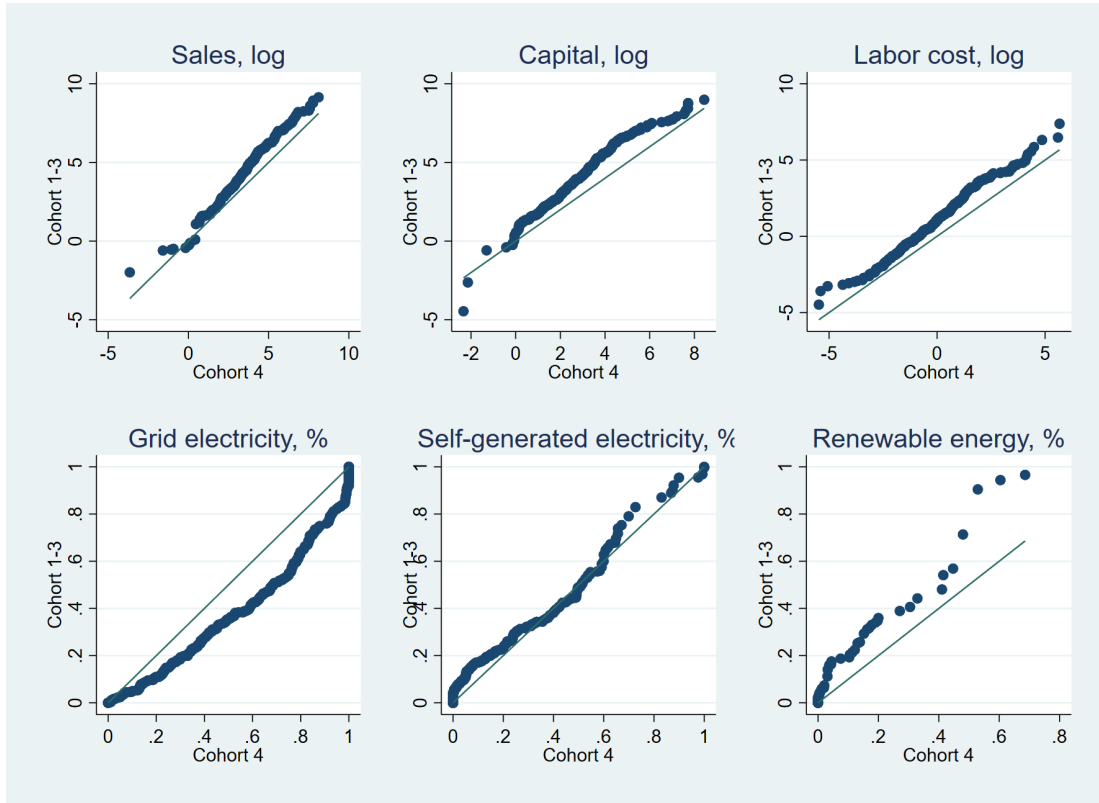


Figure C.3: QQ plots for the environmental outcome variables, cohorts 1,2,3 vs cohort 4



*Notes:* These QQ plots use the restricted sample of ever-treated firms for the period of 1992-2015 and compares the distributions of outcomes in (log) CO<sub>2</sub> emissions, energy use, CO<sub>2</sub> emission intensities in sales and in energy across quantiles for firms in cohorts 1,2,3 and firms in cohort 4.

Figure C.4: QQ plots for the economic outcome variables, cohorts 1,2,3 vs cohort 4



*Notes:* These QQ plots use the restricted sample of ever-treated firms for the period of 1992-2015 and compares the distributions of outcomes in (log) sales, capital, labor cost, and the shares of grid electricity, self-generated electricity, and renewable energy in energy cost across quantiles for firms in cohorts 1,2,3 and firms in cohort 4.

## C.2 Semi-parametric matching design

To assess the quality of matching and comparability of the matched treated and control firms, we carried out different tests. First, we compare the initial sample of treated firms to all never-treated firms. Then we compare treated firms and never-treated firms in the matched sample. See section 2.4 for more details about our matching strategy.

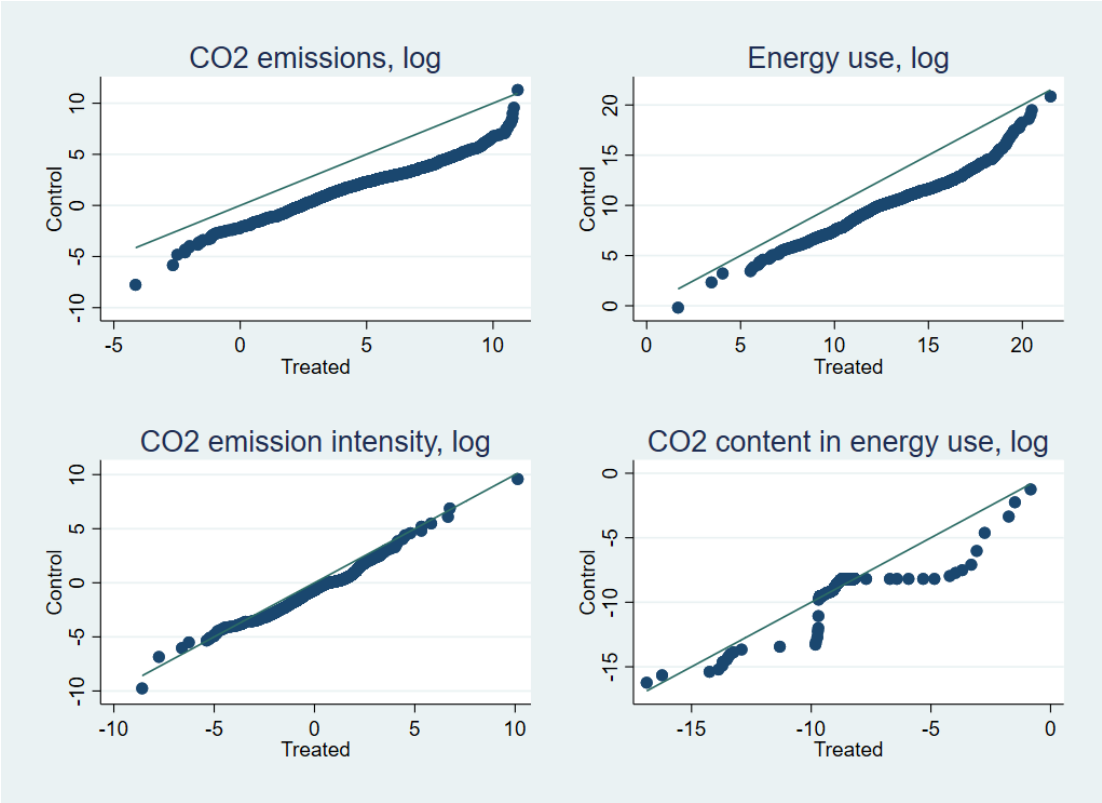
Table C.8 compares the distributions of the outcome variables between CDM firms and non-CDM firms in the initial sample, before matching. Using the non-parametric two-sample Wilcoxon rank-sum (Mann-Whitney) test, we were able to reject the hypothesis that the sample of CDM firms and the sample of all non-CDM firms are from populations with the same distribution. Standard  $t$ -tests for differences in means reject the hypotheses of no substantive difference for all outcome variables (not reported). Figures C.5 and C.6 compare the distributions of outcomes for CDM firms and all non-CDM firms by means of the plotted quantiles. We observe deviations from the 45<sup>o</sup>-line for most outcome variables. This supports our decision to pursue matching when comparing CDM firms and non-CDM

firms.

Table C.8: Results from the non-parametric two-sample Wilcoxon rank-sum (Mann-Whitney) test, using the original sample of CDM firms and all non-CDM firms

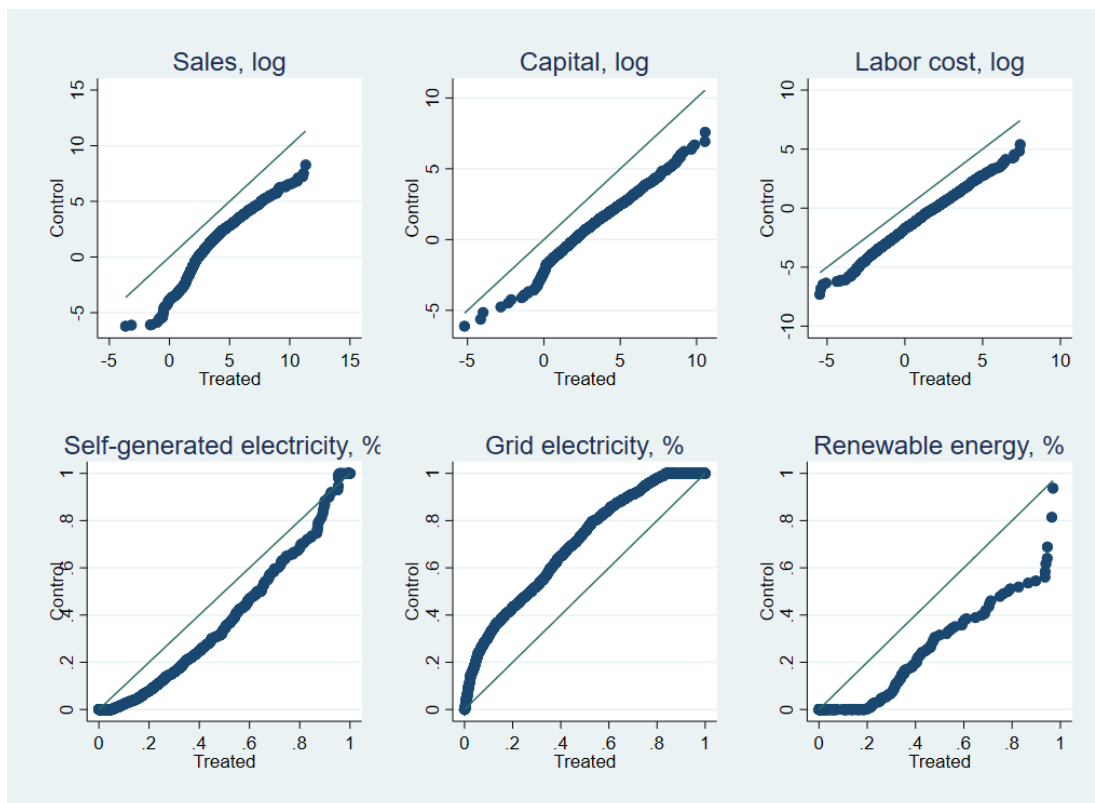
	CDM firms	Non-CDM firms	Test
	# obs.	# obs.	p-value
Total sales, log	1503	26026	0.000
Capital stock, log	1503	25896	0.000
Labor cost, log	1502	25770	0.000
CO <sub>2</sub> emissions, log	1137	12321	0.000
Energy use, log	1137	12321	0.000
CO <sub>2</sub> intensity, log	1137	12321	0.000
CO <sub>2</sub> content in energy use, log	1137	12321	0.000
Self-generated electricity share	1187	12755	0.000
Grid electricity share	1187	12755	0.000
Renewable energy share	1187	12755	0.000

Figure C.5: QQ plots for the environmental outcome variables, CDM firms vs all non-CDM firms



*Notes:* These QQ plots use the initial sample of CDM firms and all non-CDM firms for the period of 1992-2015 and compares the distributions of outcomes in (log) CO<sub>2</sub> emissions, energy use, CO<sub>2</sub> emission intensities in sales and in energy across quantiles.

Figure C.6: QQ plots for the economic outcome variables, CDM firms vs all non-CDM firms



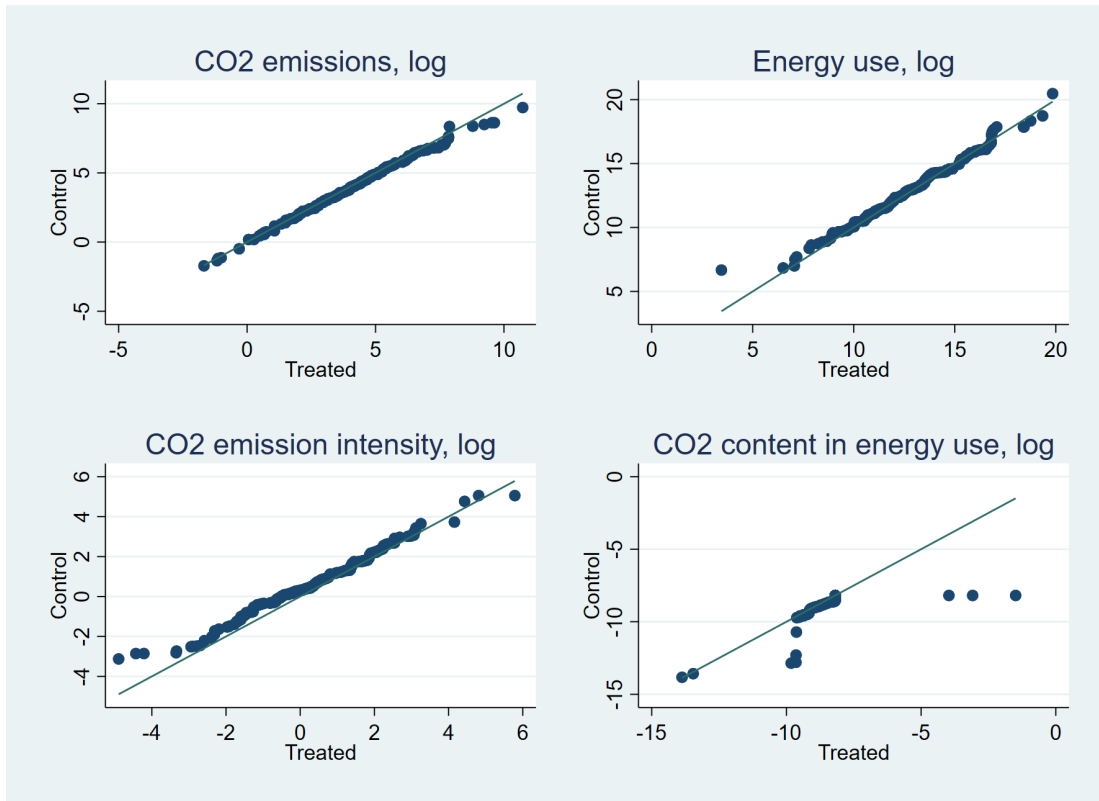
*Notes:* These QQ plots use the initial sample of CDM firms and all non-CDM firms for the period of 1992-2015 and compares the distributions of outcomes in (log) sales, capital, labor cost, and the shares of grid electricity, self-generated electricity, and renewable energy in energy cost across quantiles.

Table C.9 compares the distributions of the outcome variables between CDM firms and non-CDM firms, using the matched sample. Tests were performed at matching time, which corresponds to the relative period  $\ell = -1$ , that is, one period before treatment. Using the non-parametric two-sample Wilcoxon rank-sum (Mann-Whitney) test, we were unable to reject at the 1% significance level the hypothesis that the samples of 197 CDM firms and of their paired non-CDM firms are from populations with the same distribution for all outcome variable, except for log of total sales. Standard  $t$ -tests for differences in means did not reject the hypotheses of no substantive difference for all outcome variables, except for log total sales (not reported). Figures C.7 and C.8 compare the distributions of CDM firms and non-CDM firms in the matched sample by means of the plotted quantiles. We observe very small deviations from the 45°-line for the main matching variable (log of CO<sub>2</sub> emissions), as well as for most other outcome variables. This ensures even further that the quality of matching is very good.

Table C.9: Results from the non-parametric two-sample Wilcoxon rank-sum (Mann-Whitney) test, using the matched sample of CDM and non-CDM firms

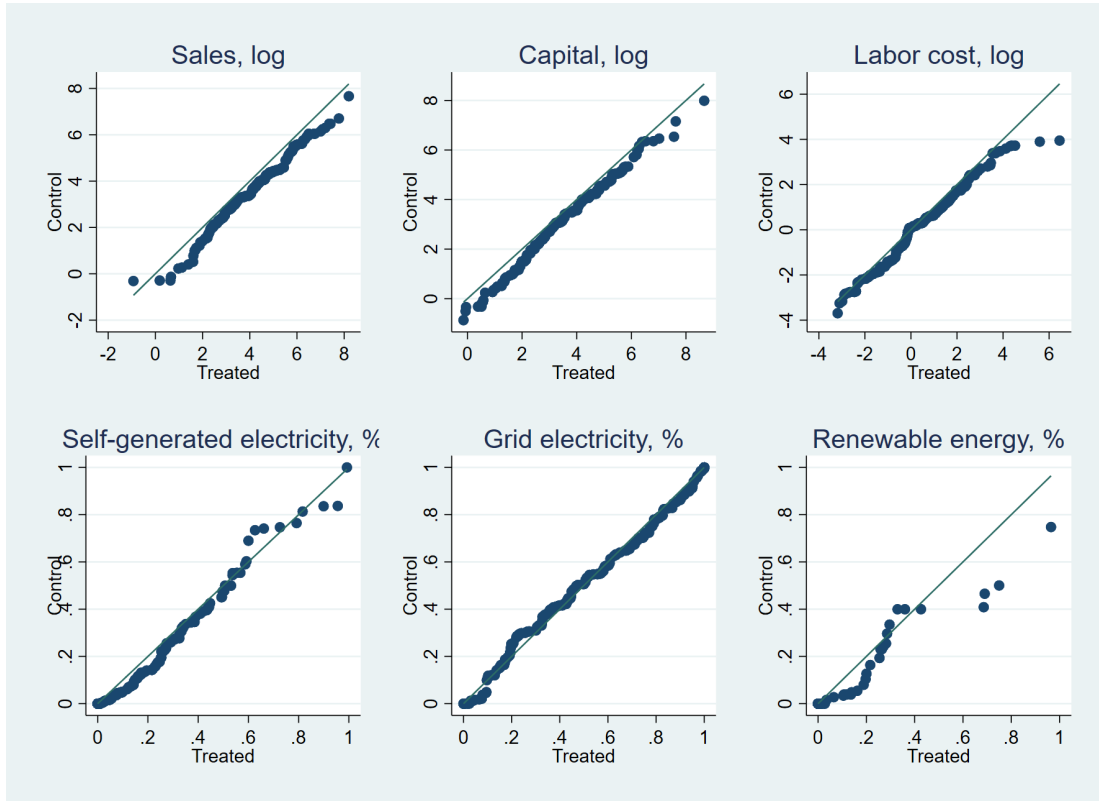
	CDM firms # obs.	Non-CDM firms # obs.	Test p-value
Total sales, log	197	197	0.002
Capital stock, log	197	197	0.032
Labor cost, log	197	197	0.166
CO <sub>2</sub> emissions, log	197	197	0.587
Energy use, log	197	197	0.036
CO <sub>2</sub> intensity, log	197	197	0.036
CO <sub>2</sub> content in energy use, log	197	197	0.019
Self-generated electricity share	195	195	0.081
Grid electricity share	195	195	0.973
Renewable energy share	195	195	0.295

Figure C.7: QQ plots for the environmental outcome variables, matched CDM and non-CDM firms



*Notes:* These QQ plots use the sample of 197 CDM firms and their matched non-CDM firms for the relative period  $\ell = -1$  and compares the distributions of outcomes in (log) CO<sub>2</sub> emissions, energy use, CO<sub>2</sub> emission intensities in sales and in energy across quantiles.

Figure C.8: QQ plots for the economic outcome variables, matched CDM and non-CDM firms



*Notes:* These QQ plots use the sample of 197 CDM firms and their matched non-CDM firms for the relative period  $\ell = -1$  and compares the distributions of outcomes in (log) sales, capital, labor cost, and the shares of grid electricity, self-generated electricity, and renewable energy in energy cost across quantiles.

## D Additional Results

Table D.10: Results for CO<sub>2</sub> emissions and energy use, with only ever-treated firms

$\ell$ period relative to treatment	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	CATT <sub><math>e,\ell</math></sub> $\hat{\delta}_{1,\ell}$ $\hat{\delta}_{2,\ell}$ $\hat{\delta}_{3,\ell}$		
(a) CO <sub>2</sub> emissions (kt)					
-4	-.199 (.124)	-.566** (.275)			-.566 (.275)
-3	-.074 (.113)	-.157 (.173)		-.096 (.246)	-.221 (.204)
-2	-.053 (.065)	-.060 (.084)	.103 (.171)	-.107 (.171)	-.095 (.152)
-1	0	0	0	0	0
0	.142* (.088)	.148* (.090)	.156 (.191)	.085 (.170)	.211 (.149)
1	.217 (.165)	.164 (.179)	.191 (.228)	.151 (.233)	
2	.325 (.252)	.212 (.267)	.212 (.267)		
3	.393 (.341)				
(b) Energy use (MMBtu)					
-4	.126 (.216)	-.154 (.429)			-.154 (.429)
-3	.010 (.179)	-.172 (.277)		.207 (.369)	-.563 (.363)
-2	-.066 (.102)	-.151 (.118)	.025 (.288)	-.112 (.227)	-.279 (.226)
-1	0	0	0	0	0
0	.180 (.139)	.249* (.141)	.384 (.316)	.148 (.215)	.287 (.280)
1	.256 (.253)	.268 (.231)	.496 (.350)	.158 (.289)	
2	.348 (.373)	.595 (.410)	.595 (.410)		
3	.771 (.596)				
# Observations	904	742	742	742	742
# Firms	186	179	179	179	179

*Notes:* This table reports three sets of estimates for the dynamic effects of CDM projects on log of CO<sub>2</sub> in panel (a) and log of energy use in panel (b). The first column reports estimates from the FE estimator  $\hat{\mu}_\ell$ . Columns 3-5 report estimates for CATT <sub>$e,\ell$</sub>  from  $\hat{\delta}_{e,\ell}$ . Column 2 reports the IW estimates which are constructed as the weighted average of  $\hat{\delta}_{e,\ell}$ 's across cohorts. Standard errors (clustered on firm) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, and \*\*\* 0.01 only in the first two columns.



Table D.11: Results for pollution and energy outcomes, using the full sample of ever-treated firms

$\ell$ period relative	CO <sub>2</sub> emissions		Energy use		CO <sub>2</sub> intensity in sales		CO <sub>2</sub> content in energy	
	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$
to treatment								
-4	-.101 (.126)	-.348 (.253)	.046 (.215)	-.199 (.386)	-.036 (.266)	.006 (.279)	-.147 (.214)	-.150 (.345)
-3	-.031 (.108)	-.109 (.159)	-.004 (.182)	-.173 (.260)	-.0001 (.185)	.137 (.186)	-.027 (.156)	.064 (.215)
-2	-.036 (.060)	-.046 (.075)	-.128 (.113)	-.204 (.134)	.007 (.096)	.039 (.095)	.092 (.096)	.158 (.111)
-1	0	0	0	0	0	0	0	0
0	.101 (.081)	.120 (.080)	.127 (.140)	.145 (.136)	-.042 (.084)	-.057 (.087)	-.026 (.111)	-.025 (.099)
1	.137 (.157)	.132 (.160)	.225 (.252)	.237 (.230)	-.052 (.124)	-.046 (.150)	-.088 (.195)	-.104 (.133)
2	.153 (.241)	.099 (.239)	.241 (.378)	.395 (.383)	-.045 (.194)	-.469 (.362)	-.088 (.287)	-.295 (.294)
3	.129 (.318)		.575 (.574)		-.276 (.317)		-.446 (.504)	
# Obs.	1125	925	1125	925	1125	925	1125	925
# Firms	231	224	231	224	231	224	231	224

*Notes:* This table reports two sets of estimates for the dynamic effects of CDM projects on (log of) CO<sub>2</sub> emissions, energy use, CO<sub>2</sub> intensity in sales and in energy use. Even columns report estimates from the FE estimator  $\hat{\mu}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5, 6$ . Uneven columns report the IW estimates  $\hat{v}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5$ . Standard errors (clustered on firm) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table D.12: Results for economic outcomes, using the full sample of ever-treated firms

$\ell$ period relative to treatment	Total sales		Capital		Labor cost		Self-generated electricity		Grid electricity	
	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$
-4	.010 (.096)	-.292 (.191)	-.059 (.095)	-.247 (.195)	.029 (.099)	-.198 (.215)	.025 (.383)	-.472 (.692)	-.044 (.205)	.251 (.391)
-3	.0004 (.076)	-.157 (.121)	-.088 (.081)	-.199 (.124)	.004 (.079)	-.122 (.129)	-.219 (.268)	-.609 (.414)	-.034 (.178)	.135 (.242)
-2	-.038 (.064)	-.067 (.083)	-.056 (.056)	-.066 (.073)	-.002 (.061)	-.016 (.079)	-.233 (.195)	-.393 (.249)	.008 (.093)	.033 (.116)
-1	0	0	0	0	0	0	0	0	0	0
0	.106 (.073)	.129* (.077)	.156** (.068)	.121* (.069)	.079 (.072)	.081 (.072)	.680*** (.206)	.665*** (.206)	-.306** (.126)	-.282*** (.118)
1	.136 (.146)	.147 (.158)	.236* (.142)	.169 (.153)	.087 (.151)	.128 (.171)	1.14*** (.370)	1.10*** (.390)	-.554** (.236)	-.690*** (.255)
2	.062 (.221)	.347 (.301)	.203 (.204)	.295 (.209)	-.010 (.228)	.252 (.278)	1.51*** (.549)	1.86*** (.486)	-.768** (.327)	-1.14*** (.429)
3	.107 (.336)		.324 (.283)		-.006 (.358)		2.50*** (.735)		-1.09** (.469)	
# Obs.	1512	1245	1512	1236	1512	1236	1182	973	1182	973
# Firms	294	291	294	289	294	289	242	235	242	235

*Notes:* This table reports two sets of estimates for the dynamic effects of CDM projects on (log of) total sales, capital, labor cost, and the share of self-generated and grid electricity expenditures in total energy cost. Even columns report estimates from the FE estimator  $\hat{\mu}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5, 6$ . Uneven columns report the IW estimates  $\hat{v}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5$ . Standard errors (clustered on firm) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table D.13: Results obtained controlling for period fixed effects, instead of sector-period FE, using the restricted sample of ever-treated firms

$\ell$ period relative to treatment	CO <sub>2</sub> emissions		CO <sub>2</sub> intensity		Total sales		Capital		Self-generated electricity	
	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$
-4	-.195 (.125)	-.510* (.265)	-.042 (.151)	.171 (.295)	-.108 (.118)	-.627*** (.238)	-.175 (.118)	-.515** (.251)	-.200 (.396)	-.787 (.760)
-3	-.081 (.111)	-.172 (.169)	.030 (.130)	.187 (.196)	-.055 (.089)	-.283* (.148)	-.161* (.093)	-.314** (.146)	-.333 (.292)	-.748 (.470)
-2	-.070 (.063)	-.083 (.081)	-.004 (.088)	.077 (.107)	-.073 (.080)	-.133 (.104)	-.094 (.066)	-.111 (.086)	-.369* (.212)	-.599** (.274)
-1	0	0	0	0	0	0	0	0	0	0
0	.127 (.088)	.135 (.094)	-.045 (.106)	-.045 (.096)	.117 (.091)	.138 (.092)	.183** (.078)	.129* (.079)	.655*** (.224)	.612*** (.220)
1	.173 (.167)	.097 (.182)	-.057 (.179)	-.048 (.162)	.127 (.180)	.087 (.187)	.279* (.161)	.140 (.170)	1.08*** (.400)	.913** (.422)
2	.224 (.250)	.123 (.251)	-.004 (.278)	-.444 (.369)	.059 (.270)	.396 (.362)	.222 (.237)	.267 (.221)	1.33** (.588)	1.63*** (.514)
3	.288 (.328)		-.253 (.442)		.209 (.416)		.372 (.326)		2.55*** (.767)	
# Obs.	904	713	904	742	1117	915	1117	915	953	782
# Firms	186	173	186	179	217	216	217	216	195	188
Period FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

*Notes:* This table reports two sets of estimates for the dynamic effects of CDM projects on (log of) CO<sub>2</sub> emissions, CO<sub>2</sub> intensity, total sales, capital, and the share of self-generated electricity expenditures in total energy cost. Even columns report estimates from the FE estimator  $\hat{\mu}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5, 6$ . Uneven columns report the IW estimates  $\hat{v}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5$ . Standard errors (clustered on firm) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table D.14: Results for pollution and energy outcomes using the restricted sample of CDM firms, with semi-parametric matching

$\ell$ period relative	CO <sub>2</sub> emissions		Energy use		CO <sub>2</sub> intensity in sales		CO <sub>2</sub> content in energy	
	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$
to treatment								
-4	-.204 (.203)	-.188 (.430)	.249 (.323)	.229 (.818)	.025 (.227)	.447 (.494)	-.453* (.273)	-.417 (.611)
-3	.092 (.175)	.260 (.294)	.195 (.258)	.171 (.444)	.183 (.218)	.481 (.348)	-.102 (.180)	.089 (.284)
-2	.088 (.111)	.143 (.162)	.119 (.194)	.054 (.260)	.197 (.121)	.335** (.167)	-.031 (.134)	.089 (.156)
-1	0	0	0	0	0	0	0	0
0	.266* (.151)	.305** (.148)	.432* (.256)	.637** (.263)	-.009 (.141)	.062 (.121)	-.167 (.172)	-.332* (.183)
1	.600** (.301)	.541* (.299)	.855* (.436)	.956** (.432)	.003 (.252)	.093 (.221)	-.255 (.270)	-.414* (.234)
2	1.01** (.440)	.763** (.387)	1.43** (.592)	1.72** (.674)	.077 (.387)	-.285 (.395)	-.415 (.361)	-.955* (.530)
3	1.13* (.606)		2.07** (.827)		-.320 (.621)		-.939 (.660)	
# Obs.	754	621	754	621	754	621	754	621
# Pairs	158	152	158	152	158	152	158	152

*Notes:* This table reports two sets of estimates for the dynamic effects of CDM projects on (log of) CO<sub>2</sub> emissions, energy use, and CO<sub>2</sub> intensity in sales and in energy. Even columns report estimates from the FE estimator  $\hat{\mu}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5, 6$ . Uneven columns report the IW estimates  $\hat{v}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5$ . Standard errors (clustered on matched pair) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table D.15: Results for economic outcomes using the restricted sample of CDM firms, with semi-parametric matching

$\ell$ period relative to treatment	Total sales		Capital		Labor cost		Self-generated electricity		Grid electricity	
	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$
-4	-.065 (.171)	-.455 (.392)	.048 (.175)	-.057 (.371)	.081 (.171)	-.201 (.357)	.046 (.545)	.338 (.968)	.097 (.371)	.429 (.697)
-3	.004 (.146)	-.137 (.247)	-.059 (.131)	-.147 (.198)	.171 (.131)	.116 (.202)	.083 (.389)	.201 (.591)	-.192 (.283)	-.239 (.364)
-2	.101 (.126)	.095 (.161)	-.041 (.092)	-.069 (.118)	.155 (.101)	.156 (.132)	-.054 (.296)	-.155 (.389)	-.070 (.142)	-.112 (.162)
-1	0	0	0	0	0	0	0	0	0	0
0	.207 (.145)	.247* (.132)	.260** (.103)	.241** (.101)	.089 (.113)	.153 (.106)	.870** (.339)	.941** (.378)	-.314* (.188)	-.281 (.181)
1	.442 (.285)	.547** (.263)	.544** (.226)	.537** (.219)	.140 (.236)	.436** (.221)	1.45** (.624)	1.36* (.722)	-.289 (.368)	-.505 (.415)
2	.722* (.430)	1.01** (.486)	.764** (.339)	.788*** (.301)	.251 (.362)	.477 (.349)	2.02** (.904)	1.78** (.896)	-.902* (.508)	-.815 (.604)
3	.972* (.588)		1.22** (.475)		.342 (.527)		4.22*** (1.29)		-1.93*** (.735)	
# Obs.	871	716	868	713	869	714	775	637	775	637
# Pairs	165	165	165	165	165	165	160	156	160	156

*Notes:* This table reports two sets of estimates for the dynamic effects of CDM projects on (log of) total sales, capital, labor cost, and the share of self-generated and grid electricity expenditures in total energy cost. Even columns report estimates from the FE estimator  $\hat{\mu}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5, 6$ . Uneven columns report the IW estimates  $\hat{v}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5$ . Standard errors (clustered on matched pair) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table D.16: Results obtained controlling for period fixed effects, instead of sector-period FE, with semi-parametric matching

$\ell$ period relative to treatment	CO <sub>2</sub> emissions		CO <sub>2</sub> intensity		Total sales		Capital		Self-generated electricity	
	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$
-4	-.122 (.217)	.080 (.422)	.022 (.196)	.573 (.411)	-.018 (.158)	-.345 (.367)	.051 (.167)	-.131 (.354)	.138 (.553)	.136 (.981)
-3	.063 (.187)	.269 (.287)	.145 (.185)	.493* (.295)	-.006 (.136)	-.145 (.232)	-.056 (.117)	-.197 (.181)	.157 (.383)	.202 (.551)
-2	.011 (.111)	.044 (.156)	.106 (.113)	.204 (.159)	.098 (.112)	.091 (.145)	-.052 (.080)	-.095 (.108)	.096 (.252)	-.062 (.323)
-1	0	0	0	0	0	0	0	0	0	0
0	.256* (.140)	.301** (.132)	.036 (.130)	.081 (.114)	.175 (.126)	.232** (.111)	.274*** (.092)	.270*** (.091)	.725** (.314)	.940*** (.341)
1	.591** (.276)	.493* (.259)	.089 (.230)	.053 (.205)	.388 (.249)	.514** (.221)	.553*** (.200)	.596*** (.195)	1.03* (.571)	1.10* (.647)
2	.898** (.408)	.536 (.338)	.135 (.345)	-.236 (.315)	.564 (.376)	.753* (.405)	.752** (.301)	.860 *** (.312)	1.52* (.808)	1.56* (.836)
3	1.03* (.569)		-.155 (.531)		.661 (.525)		1.15*** (.438)		3.37*** (1.13)	
# Obs.	906	750	906	745	1045	859	1042	856	928	761
# Pairs	190	184	190	182	197	197	197	197	190	186
Period FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

*Notes:* This table reports two sets of estimates for the dynamic effects of CDM projects on (log of) CO<sub>2</sub> emissions, CO<sub>2</sub> intensity, total sales, capital, and the share of self-generated electricity expenditures in total energy cost. Even columns report estimates from the FE estimator  $\hat{\mu}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5, 6$ . Uneven columns report the IW estimates  $\hat{v}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5$ . Standard errors (clustered on matched pair) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table D.17: Results obtained using 2:1 nearest-neighbor matching

$\ell$ period relative to treatment	CO <sub>2</sub> emissions		CO <sub>2</sub> intensity		CO <sub>2</sub> content in energy		Total sales		Self-generated electricity	
	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$
-4	.029 (.182)	.087 (.374)	.205 (.187)	.562 (.386)	-.125 (.222)	.275 (.432)	.001 (.137)	-.3227 .314	.275 (.296)	1.02 (.736)
-3	.180 (.148)	.293 (.229)	.235 (.169)	.435* (.260)	.055 (.178)	.343 (.260)	-.032 (.113)	-.213 .195	.329 (.241)	0.648* (.388)
-2	.115 (.085)	.121 (.115)	.167* (.097)	.218* (.123)	.139 (.118)	.250* (.133)	.031 (.094)	.003 .125	.127 (.160)	.291 (.237)
-1	0	0	0	0	0	0	0	0	0	0
0	.219* (.116)	.334*** (.108)	-.011 (.114)	.084 (.094)	-.179 (.144)	-.217 (.137)	.214** (.103)	.275*** (.095)	.300 (.221)	.206 (.245)
1	.529** (.233)	.635*** (.222)	.038 (.210)	.278 (.193)	-.510** (.239)	-.492** (.198)	.486** (.211)	.520*** (.199)	.691* (.409)	.627 (.435)
2	.847** (.341)	.711** (.281)	.210 (.315)	.005 (.320)	-.642* (.351)	-.982** (.427)	.662** (.321)	.910** (.396)	1.03* (.597)	1.08* (.645)
3	.842* (.460)		-.087 (.478)		-1.19** (.588)		.785* (.447)		2.14** (.868)	
# Obs.	946	783	946	783	946	783	1045	859	713	587
# Pairs	190	186	190	186	190	186	197	197	148	142

*Notes:* This table reports two sets of estimates for the dynamic effects of CDM projects on (log of) CO<sub>2</sub> emissions, CO<sub>2</sub> intensity in sales and in energy, total sales, and the share of self-generated electricity expenditures in total energy cost. Even columns report estimates from the FE estimator  $\hat{\mu}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5, 6$ . Uneven columns report the IW estimates  $\hat{v}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5$ . Standard errors (clustered on matched pair) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table D.18: Results obtained using 1:1 nearest-neighbor matching with no distance restriction

$\ell$ period relative to treatment	CO <sub>2</sub> emissions		CO <sub>2</sub> intensity		CO <sub>2</sub> content in energy		Total sales		Self-generated electricity	
	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$	FE $\hat{\mu}_\ell$	IW $\hat{v}_\ell$
-4	-0.250 (.217)	-0.183 (.409)	-0.105 (.214)	.348 (.420)	-0.153 (.257)	.366 (.563)	-0.059 (.157)	-0.448 (.354)	-0.108 (.509)	-0.554 (.925)
-3	-0.006 (.173)	.169 (.267)	.089 (.202)	.444 (.305)	.112 (.189)	.511* (.300)	-0.004 (.129)	-0.163 (.216)	-0.038 (.371)	-0.203 (.588)
-2	.022 (.108)	.088 (.150)	.112 (.118)	.216 (.162)	.189 (.142)	.323* (.169)	.097 (.109)	.115 (.146)	-0.027 (.278)	-0.198 (.360)
-1	0	0	0	0	0	0	0	0	0	0
0	.333** (.140)	.339*** (.131)	-0.012 (.149)	.004 (.138)	-0.209 (.159)	-0.275* (.164)	.225* (.129)	.274** (.115)	.804** (.319)	1.01*** (.336)
1	.698** (.278)	.546** (.262)	.012 (.253)	-0.062 (.245)	-0.312 (.257)	-0.449* (.247)	.459* (.253)	.551** (.235)	1.29** (.588)	1.26* (.655)
2	1.07*** (.412)	.641* (.364)	.015 (.398)	-0.675 (.560)	-0.519 (.347)	-0.898** (.450)	.686* (.376)	1.07** (.445)	1.67** (.846)	1.66* (.879)
3	1.27** (.580)		-0.476 (.642)		-1.00* (.585)		1.01* (.525)		3.42*** (1.22)	
# Obs	992	818	992	818	992	818	1162	955	1023	842
# Pairs	207	200	207	200	207	200	218	218	211	206

*Notes:* This table reports two sets of estimates for the dynamic effects of CDM projects on (log of) CO<sub>2</sub> emissions, CO<sub>2</sub> intensity in sales and in energy, total sales, and the share of self-generated electricity expenditures in total energy cost. Even columns report estimates from the FE estimator  $\hat{\mu}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5, 6$ . Uneven columns report the IW estimates  $\hat{v}_\ell$ , including observations from  $t = 1, 2, 3, 4, 5$ . Standard errors (clustered on matched pair) are shown in parentheses. Statistical significance levels are indicated as \* 0.10, \*\* 0.05, \*\*\* 0.01.