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December 2025

Grantham Research Institute on Climate Change and the Environment
Working Paper No. 437

ISSN 2515-5717 (Online)

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Suggested citation:
Guerini M, Marin M and Vona F (2025) *Can easing financial constraints reduce carbon emissions? Evidence from a large sample of French companies*. Grantham Research Institute on Climate Change and the Environment Working Paper 437. London: London School of Economics and Political Science

Can Easing Financial Constraints Reduce Carbon Emissions? Evidence from a Large Sample of French Companies

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Abstract

We study how monetary policy shapes firm-level carbon emissions by exploiting the ECB's 2012 entry into the zero lower bound as a plausibly exogenous credit easing. Using administrative and survey data on French manufacturing firms from 2000–2019 and a difference-in-differences design with debt-to-asset ratios as exposure, we show that financially constrained firms cut emissions 9.4% more than unconstrained ones, primarily through lower energy intensity and capital-deepening productivity gains. Small and medium enterprises drive the results. Aggregating our estimates implies average annual reductions of 3.3%, amounting to 5.3 million tonnes of CO_2 saved

JEL Codes: Q52, Q48, D22.

Keywords: Financial constraints, credit supply, firm-level carbon emissions, climate policies.

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1 Introduction

Economists have long treated money neutrality as a benchmark (Friedman, 1968; Lucas, 1996), yet growing evidence suggests that monetary policy can generate persistent real effects also over the medium and long run. Firm level studies show that monetary policy shocks affect investment (Ottonello and Winberry, 2020) and innovation as well as productivity-enhancing activities (Aghion et al., 2012). These effects are heterogeneous, varying with firm size (Crouzet and Mehrotra, 2020; Greenwald et al., 2022), age (Cloyne et al., 2023), indebtedness (Auer et al., 2021), reliance on external finance (Ferrando et al., 2019; Durante et al., 2022), collateral availability (Cole et al., 2024), and financial fragility (Ottonello and Winberry, 2020). Challenging the strict neutrality view, such evidence also underscores the real impact that monetary policy – mediated by credit market constraints – exerts on investments in new technologies and innovation that are the drivers of long-term economic growth (Rajan and Zingales, 1998; Ma and Zimmermann, 2023).

In a world where the opposition to carbon pricing is widespread (Bosetti et al., 2025), academics and policymakers have increasingly focused on how credit conditions shape investments in the energy transition, including green and energy-efficient technologies (Aglietta et al., 2015; Cœuré, 2018; Campiglio et al., 2018; Rudebusch et al., 2019; Schmidt et al., 2019; Powell, 2020; Polzin and Sanders, 2020; Schoenmaker, 2021; Lagarde, 2022; Aghion et al., 2022; Egli et al., 2022; Faria et al., 2023). Green technologies tend to be more capital-intensive and reliant on external finance than conventional technologies (Almeida and Campello, 2007; Eyraud et al., 2013; Best, 2017; Accetturo et al., 2024), face greater uncertainty and informational frictions (Bettarelli et al., 2024), and often yield lower private returns in the absence of ambitious climate policies (Jaffe et al., 2005; Dechezleprêtre et al., 2019). It follows that credit constraints seem to bite more for firms investing in low-carbon technologies, or more broadly, seeking to improve their environmental performance (Noailly and Smeets, 2022; Ferrando et al., 2023; De Haas and Popov, 2023; De Haas et al., 2024; European Investment Bank, 2025). Yet, the empirical channels through which monetary policy shocks affect firm level green investments and environmental performance remain largely unexplored.

This paper contributes to fill this gap by examining (i) how monetary easing affects firm level carbon emissions and energy efficiency, (ii) the heterogeneity of these effects by firm size, (iii) the underlying transmission channels and, (iv) the interaction with climate policies. Using rich administrative and survey data on French manufacturing firms from 2000 to 2019 – including emissions, energy use, financial conditions, environmental protection investments, and productivity – we first decompose emission changes into improvements in energy efficiency, carbon intensity, and firm scale via a Kaya-type decomposition. Second, we capture heterogeneity across firm sizes, overcoming limitations of prior work that focused on large, listed corporations facing weaker financial constraints (Andersen, 2016; Goetz, 2018; Levine et al., 2018). Third, our data allow us to investigate several transmission mechanisms, including the effects of relaxing credit constraints on green investments, electrification, energy self-generation, productivity, capital intensity, and input mix. Fourth, leveraging a subsample of firms under the EU Emission Trading Scheme (EU ETS), we investigate how monetary policy interacts with carbon pricing. Although financial constraints may tighten due to the increase in costs induced by the EU ETS, regulated firms are also larger (Dechezleprêtre et al., 2023), more capital-intensive (Marin et al., 2018) and thus less vulnerable to cycles in the credit supply (Zaklan, 2023).

To identify the effect of a credit easing on firm level emissions, we exploit a plausibly exogenous variation in the European Central Bank (ECB) interest rate policy – with deposit facility rate cut to zero in July 2012 – leading to a period of long-lasting and credible credit supply expansion. To do so, we use a Difference-in-Differences (DiD) design inspired by the literature on the effect of financial development on economic performance (Rajan and Zingales, 1998; Manova, 2013), where the effect of the ZLB varies across firms depending on a measure of financial exposures that predates the policy intervention. In our main analyses, we use the firm debt-to-asset (DtA) ratio predating the ZLB as measure of financial exposure. Our rich data further allow us to exploit firm size variation in a triple-difference design. This is critical because high DtA ratios bind more tightly for smaller firms, which face greater constraints due to limited collateral and internal funds, sharpening the identification of the policy's effect as well as the policy implications of our work.

Our findings underscore the critical role of credit supply policies in advancing the low-carbon transition. First, easing credit constraints reduces firm level CO_2 emissions by roughly 9.41% more for financially exposed firms (75th percentile of initial DtA) compared to less exposed firms (25th percentile). This net effect arises from two offsetting forces identified by the theoretical literature (Andersen, 2016; Lessmann and Kalkuhl, 2024): a modest increase of 2% in value added (the size effect) is more than offset by a 6.31% decline in the carbon content of energy and a 5.65% reduction in energy intensity (the technique effect). An event-study extension of our DiD design confirms that parallel trends hold, with the technique effect unfolding over the medium to long term, while the size effect emerging immediately. The technique effect primarily reflects shifts in input mix – capital deepening combined with reductions in labor and energy use – and efficiency gains (e.g., in TFP), rather than increased investment in environmental protection or energy self-generation. Consistent with the untargeted nature of the monetary policy, the ZLB does not induce a larger number of firms to begin investing in environmental protection (extensive margin); however, it slightly increases the fraction of investments targeting low-carbon technologies for those firms that already did invest in these technologies (intensive margin). Second, and more remarkable, only small and medium-sized firms benefit from relaxed credit constraints, achieving significantly larger improvements in both environmental and economic performance than the average firm – for which we do not observe any induced decarbonization. For these firms, an interquartile increase in DtA corresponds to an 18% reduction in CO_2 emissions. While as for the average firm in our sample, this effect is driven by stronger capital deepening and general efficiency gains, we also observe an increased electricity usage that is accompanied by stable employment levels. Third, easing financial constraints does not significantly affect firms regulated under the EU Emission Trading Scheme (EU ETS) relative to a matched control group. This aligns with the larger size and capital intensity of ETS firms, which renders them less sensitive to credit supply shocks.

Quantitatively, our estimates imply that, thanks to the easing of the credit constraints associated with the ZLB, firm level CO_2 emissions declined by an average of 3.2% per year between 2012 and 2019. In relative terms, this effect is one order of magnitude smaller with respect to the impact that Colmer et al. (2024) attribute to the introduction of the EU ETS. Yet in aggregate terms, the total carbon savings are strikingly similar. We estimate about 5.3 million tonnes of CO_2 saved in the manufacturing sector, compared with Colmer et al. (2024) 5.4 million tonnes for the EU ETS. Such similar aggregated effect reflects the different reach of the two policies. While monetary policy is untargeted and yields a smaller marginal effect at the firm level, it nonetheless operates on a much broader set of firms than the EU ETS.

Contribution to the literature

Theory suggests that monetary policy influences CO_2 emissions through two opposing channels: a *technique effect*, whereby lower capital costs make carbon-free, capital-intensive technologies more attractive, and a *scale effect*, whereby stronger economic activity raises energy demand (Andersen, 2016; Lessmann and Kalkuhl, 2024). The net impact depends critically on both the extent to which the policy is able to reduce the cost differential between financing green and brown technologies (Campiglio et al., 2024) and the degree of financial constraints faced by firms (Lanteri and Rampini, 2025). Path-dependent innovation implies that large and high-polluting incumbents, often less constrained, continue investing in "brown" technologies, while smaller and younger firms—key drivers of green innovation—remain more sensitive to credit conditions (Aghion et al., 2024). Consequently, easing financial constraints can lower CO_2 emissions by enabling cleaner technology adoption and innovation. Empirical evidence supports this link, showing that access to finance shapes firms' environmental performance through green investments and innovation (Ghisetti et al., 2017; Kalantzis et al., 2022; De Haas et al., 2024; Costa et al., 2024), especially among small companies (Howell, 2017; Noailly and Smeets, 2022). Our contribution to this literature is to provide a direct estimate of credit easing effects on CO_2 emissions and to decompose these effects into size and technique channels, holding a tight link with extant theoretical frameworks.

From an empirical viewpoint, credit availability has been shown to impact employment (Caggese and Cuñat, 2008), productivity (Caggese, 2019), export (Manova et al., 2015; Altomonte et al., 2016) and investment composition (Campello et al., 2010; Aghion et al., 2010; Lin and Paravisini, 2013). However, empirical research on the causal impact of financial constraints on firm-level environmental performance remains scarce. Andersen (2016, 2017) documents that credit-constrained U.S. plants emit more toxic pollutants, though without the aim of establishing a clear causal effect. Closely related to our paper, Goetz (2018) exploits the Federal Reserve's 2011 Maturity Extension Program as a quasi-experiment to identify a causal effect, showing that credit easing reduces toxic emissions among financially constrained listed firms. Levine et al. (2018) leverages arguably exogenous liquidity shocks to local banks driven by the windfall of shale gas revenues, showing that firms in a pre-existing relationship with such banks improved their environmental performance ratings. Yet, these studies focus on large U.S. listed corporations, use different measures of environmental performance (ratings or toxic release inventories), and largely ignore firm size heterogeneity. Our study advances this literature by providing one of the first causal estimates of how financial frictions influence firm-level direct CO_2 emissions. We do so using a representative sample of French manufacturing firms spanning both large corporations and small enterprises, in a European institutional context, with high-quality energy-based measures of direct CO_2 emissions. Remarkably, we show that responses are markedly stronger among smaller, more financially constrained firms.

Finally, our study intersects with a growing literature on how firms adjust their economic and environmental performance in response to policies and shocks (see Dechezleprêtre et al., 2019). While most existing work focuses on the effects of environmental regulation (Martin et al., 2014; Ahmadi et al., 2022; Dechezleprêtre et al., 2023; Colmer et al., 2024; Martinsson et al., 2024; Kruse et al., 2024; Zaklan, 2023), energy price changes (Marin and Vona, 2021; Fontagné et al., 2024), or trade liberalization (Li and Zhou, 2017; Cherniwchan, 2017; Dussaux et al., 2023), less attention has been devoted to how financial frictions shape the effectiveness of climate policies. This interaction is particularly salient for the EU Emissions Trading System (EU ETS), where firms' ability to adjust to carbon pricing may depend on their access to credit. We extend this literature in two ways. First, we assess how relaxing financial constraints affects CO_2 emissions for both EU ETS and non-ETS firms, providing evidence on whether credit easing enhances the effectiveness of the EU ETS. Second, using the same data as Colmer et al. (2024), we benchmark our estimates against those for the EU ETS, offering insights into the relative magnitudes of monetary and regulatory interventions. These findings speak to the design of coordinated policy packages in a second-best world of multiple externalities, where aligning monetary and climate policies may be crucial for achieving decarbonization goals (Stern et al., 2022).

The remainder of the paper is organized as follows. Section 2 presents a simple theoretical framework that helps us to think about the direct and indirect relationships between financial constraints, production and carbon emissions. Section 3 discusses the main idea behind the identification strategy employed in our empirical analysis. Section 4 presents the data, the main variables and a brief descriptive analysis to give some context. Section 5 contains all the main results, while Section 6 quantifies the main results and present a simple counterfactual policy analysis. Section 7 concludes. Five Appendices complement the paper by providing details on (i) the sample construction, (ii) the descriptive statistics, (iii) complementary results, (iv) the matching strategy for the joint analysis of credit easing with the EU ETS environmental policy, and (v) a large number of robustness checks.

2 Conceptual framework

A growing theoretical literature shows that financial constraints can limit the adoption of clean technologies (Lanteri and Rampini, 2025; Campiglio et al., 2024), also highlighting the contrasting roles of the scale and technique effects (Andersen, 2016). Building on these contributions, we develop a simple conceptual framework of firms' choices under financial constraints that embeds the Kaya identity decomposition of emissions. Our aim is not to recover structural parameters, but to clarify the channels through which easing credit constraints affects carbon emissions and to guide the interpretation of our empirical results. In a nutshell, this simple framework provides the link between theory and the empirical analysis that follows.

To fix ideas, we recall the Kaya-like decomposition that can be applied to our data at the firm level. Denote $CO2_{it}$ as carbon emissions of firm i in period t, with E_{it} and VA_{it} representing energy consumption and value added, respectively. Firm level emissions can be expressed as:

$$CO2_{it} \equiv \frac{CO2_{it}}{E_{it}} \times \frac{E_{it}}{VA_{it}} \times VA_{it} \tag{1}$$

The first two components capture the technique effect – the carbon content of energy use and energy intensity, respectively – while the third is a standard scale effect. Three examples illustrate the logic of the decomposition at the firm level. A shift in the energy mix from high-carbon fuels (coal, oil) to lower-carbon alternatives (gas, electricity) lowers the first component ($CO2/E$). Gains in total factor productivity reduce the energy required per unit of value added, lowering the second component (E/VA). By contrast, stronger demand raises the third component (VA). We next show how this decomposition can be derived in a very simple theoretical framework, which allows us to further decompose the size and technique effects.

To model financial constraints in a simplified way, we build on the entrepreneurial-choice framework of Evans and Jovanovic (1989), further developed in Moll (2014), Midrigan and Xu (2014), and Manaresi and Pierri (2024). Output $Y_{i,t}$ is produced with capital $K_{i,t}$, labor $L_{i,t}$, and energy $E_{i,t}$ through a Hicks-neutral technology:

$$Y_{i,t} = A_{i,t} F\left(K_{i,t}, L_{i,t}, E_{i,t}\right), \tag{2}$$

where $A_{i,t}$ denotes total factor productivity and $F(\cdot)$ may take any functional form (e.g., Cobb–Douglas, CES, Leontief). Firms can draw on different energy sources $j = 1, \ldots, J$, so that firm level energy efficiency $A^E_{i,t}$ is the weighted average across sources:

$$A^E_{i,t} = \sum_{j=1}^{J} A^E_{i,t,j} \frac{E_{i,t,j}}{E_{i,t}} \tag{3}$$

where shifts toward cleaner sources raise the aggregate $A^E_{i,t}$.[1] Emissions are modeled by a simple reduced form function G:

$$CO_{2,i,t} = G\left(Y_{i,t}, A^E_{i,t}\right), \quad \text{with} \quad G'_Y > 0, \quad G'_{A^E} < 0. \tag{4}$$

Following Manaresi and Pierri (2024), input choices are subject to two constraints:

$$K_{i,t} = K_{i,t-1}(1 - \delta_i) + I_{i,t} \tag{5}$$

$$X_{i,t} \leq X_{i,t-1} \Lambda_{i,t} \quad \text{with} \quad X \in \{K, L, E\}$$

[1]Within a directed technical change framework (Acemoglu et al., 2012), (Hassler et al., 2021) separately identify both $A^E_{i,t}$ and $A_{i,t}$, showing that the trends of the former grew faster than the latter following the oil crises of the 1970s.

where the first row of Equation 5 presents the standard law of motion of capital accumulation, and the second captures financial constraints. The factor $\Lambda_{i,t} \geq 1$ governs the tightness of the constraint. If $\Lambda_{i,t} = 1$, the firm can only replace depreciated capital (tight constraint); if $\Lambda_{i,t} = +\infty$, the firm is unconstrained and chooses $X_{i,t} = X^{\star}_{i,t}$. Heterogeneity in $\Lambda_{i,t}$ across firms and time reflects micro differences in size, leverage, and liquidity, as well as heterogeneous responses to aggregate drivers such as monetary, regulatory, or fiscal policies.

Although stylized, this setting is sufficient to clarify the channels through which an exogenous relaxation of credit constraints (i.e., an increase in $\Lambda_{i,t}$) affects firm level CO_2 emissions. It also extends the insights of the Kaya identity which allows us to distinguish between general productivity gains and directed green technological change (Hassler et al., 2021), and to further decompose the size effect. Applying the chain rule to Equation 4, the effect of relaxing credit constraints on emissions can be broken down into a number of sub-components that go beyond the three terms of the Kaya identity but can be mapped onto changes in input composition, total factor productivity, energy efficiency and carbon intensity:[2]

$$\frac{\partial CO2}{\partial \Lambda} = \frac{\partial G}{\partial Y}\left[A\underbrace{\left(\frac{\partial F}{\partial K}\frac{\partial K}{\partial \Lambda} + \frac{\partial F}{\partial L}\frac{\partial L}{\partial \Lambda} + \frac{\partial F}{\partial E}\frac{\partial E}{\partial \Lambda}\right)}_{\text{VA}} + \underbrace{\left(\frac{\partial A}{\partial \Lambda}\right)}_{\text{TFP}} F(K, L, E)\right] + \frac{\partial G}{\partial A^E}\left[\sum_j \left(\underbrace{\frac{\partial A_j^E}{\partial \Lambda}}_{E/VA}\frac{E_j}{E} + A_j^E \underbrace{\frac{\partial \frac{E_j}{E}}{\partial \Lambda}}_{CO_2/E}\right)\right] \tag{6}$$

From Equation 6, three main effects emerge. First, the scale effect, arising from changes in inputs K, L, and E, which maps directly into the VA component of the Kaya identity. As anticipated, the scale effect is further decomposed by input, as financial constraints may differentially limit adjustments in capital, labor, and energy. Second, the economic efficiency effect that captures the response of total factor productivity (TFP). This effect does not explicitly appear into the Kaya decomposition (cf. Equation 1), but it allows us to better qualify the origin of the broader "technique effect", because a rise in the TFP could potentially reduce the energy intensity of a company. Third, the "green technique effect", represented here by shifts either in the efficiency of energy sources ($\partial A^E/\partial\Lambda$) or in the energy mix ($\partial(E_j/E)/\partial\Lambda$), which also impact the E/VA (energy intensity) and $CO2/E$ (carbon content of energy) components of the Kaya identity.

In our empirical analysis, we exploit this theoretical structure and interpret the ECB's 2012 zero-lower-bound (ZLB) deposit rate cut as a plausibly exogenous relaxation of credit constraints, which disproportionately eased financing conditions for more constrained firms. Heterogeneity in $\Lambda_{i,t}$ across firms

[2]For clarity, we omit indices in what follows; all terms refer to a single firm i at time t.

– driven by pre-existing indebtedness and size – allows us to identify differential responses in carbon emissions, decomposed into scale, economic efficiency, and green technique effects. Overall, this mapping provides a clear link between the conceptual framework and the empirical strategy, guiding our interpretation of observed emission changes as the consequence of the easing of credit supply.

3 Estimating the effect of credit easing on carbon emissions

As discussed in the previous sections, an exogenous relaxation of financing constraints can have ambiguous effects on a firm's carbon emissions. On the one hand, easier access to credit may enable firms to expand production and invest more in physical and human capital, potentially increasing emissions. On the other hand, it can facilitate environmental protection investments that reduce emissions. In this section we first outline our identification strategy, and then detail the empirical implementation, disentangling the impact of credit easing across different transmission channels.

3.1 Identifying the causal effect of easing financial constraints

Estimating the causal impact of easing credit constraints at the firm level is challenging because typical measures of financial exposure are correlated with actual and expected firm performance, which are only imperfectly observed. These concerns are relevant also for environmental outcomes. Forward-looking expectations matter greatly for financing investments that are new and more uncertain, such as green technologies (Campiglio et al., 2024). Furthermore, banks are increasingly incorporating climate risks in their credit decisions (Huang et al., 2022).[3]

To address these endogeneity concerns, we adopt the well-established approach of Rajan and Zingales (1998) and Manova (2013), recently applied by Goetz (2018) to environmental outcomes. The idea is to combine, in a standard difference-in-differences setting, a macroeconomic change in credit availability with a time-invariant, pre-determined firm-level measure of financial constraint. This identification strategy is also akin to a reduced-form shift-share design, where a global (EU-level) policy shock interacts with local (firm-level) exposure (see Borusyak et al., 2022).

As for the aggregate shift in credit supply, we exploit a major, long-lasting, and credible change in ECB monetary policy that occurred in July 2012, when the deposit facility rate was set to zero as the Euro Area entered the Zero Lower Bound (ZLB) for the first time. While the ZLB simultaneously loosened

[3] For instance, if carbon pricing (or climate damages) is expected to rise, banks may anticipate stranded-asset risks for carbon-intensive (or climate-exposed) firms, increasing their financing costs.

credit constraints for all French firms, its impact varied with firms' initial financial conditions (see Alder et al., 2023). Non-indebted firms had little to gain from lower refinancing costs, whereas highly leveraged firms benefited substantially from the drop in the cost of rolling over their debt. Besides its credibility, a further advantage of using the ZLB is its plausibly exogenous nature with respect to firms' environmental performance, since the ECB's mandate is strictly focused on inflation targeting, making monetary policy decisions orthogonal to carbon emissions.[4]

Two potential challenges arise when using the ZLB as a shock. The first concerns the discontinuity it represents compared to standard ECB rate adjustments. The ZLB marked a highly persistent shift in monetary policy, widely interpreted as the start of a "new normal" in the Euro Area.[5] This twist in the monetary policy stance justifies taking July 2012 – the moment the deposit facility rate was set to zero – as the turning point in our DiD design. A second concern is that monetary easing began earlier, as large ECB rate cuts followed the Lehman Brothers collapse in late 2008. However, that expansion was very much short-lived since already between May 2009 and July 2011 the ECB raised its interest rates twice. By contrast, from mid-2012 onward the easing was both deeper and long-lasting, with the main refinancing rate and the deposit facility rate kept unchanged for a full decade. Finally, to address residual concerns about timing, we conduct two robustness checks that exploit the ECB's adoption of unconventional monetary policy from 2015 with the Asset Purchase Programme (APP), or that control for continuous yearly movements in the average benchmark interbank rate across European economies.

We also acknowledge that concerns might arise because the debt-to-asset (DtA) ratio is only an imperfect proxy for financial constraints, as it may partly capture the willingness of investors and banks to extend credit – particularly when firms hold sufficient assets that can be pledged as collateral (e.g., Kiyotaki and Moore, 1997). We address this potential measurement issue in three ways. First, we test robustness to an alternative indicator of financial constraints (the debt-to-equity, DtE) ratio, which better captures the capital at risk in a firm's liabilities. Second, we exploit heterogeneity by firm size in a triple-difference design, recognizing that smaller (larger) firms with high DtA are more (less) likely to face binding credit constraints, making the ratio a more (less) reliable exposure measure for them. Third, we estimate specifications that directly control for collateralizable assets – such as capital intensity – even though this could be a "bad control", it helps us rule out the measurement error in our exposure variable.

This identification strategy, by isolating exogenous variation in credit supply, allows us to estimate

[4]Only in recent years has the ECB announced a more explicit intention to align monetary policy with climate objectives (see Lagarde, 2022). In the period covered by this study (i.e., 2000–2019), no such link was present.

[5]The credibility of this turning point was reinforced by Mario Draghi's "whatever it takes" speech, delivered in the same month.

the scale (VA), green technique ($CO2/E$ and E/VA), and economic efficiency (TFP) effects described in Section 2.

3.2 Empirical implementation

Baseline setting. To implement our identification strategy, we estimate a standard two-way fixed-effects Difference-in-Differences (DiD) specification:

$$ihs(Y_{it}) = \beta(DtA_{i,pre} \times ZLB_t) + \mu_i + \lambda_{ts} + \eta_{tp} + \theta_{ets} + \varepsilon_{it} \tag{7}$$

where Y_{it} denotes either carbon emissions, carbon emissions per unit of value added, or one of the components of the Kaya-type decomposition in Equation 1. $DtA_{i,pre}$ is the time-invariant debt-to-asset ratio measured before the ZLB, computed as the individual firm average between 2000–2011 to smooth out potential outliers.[6] ZLB_t is a dummy equal to 1 from 2012 onward, capturing the ECB deposit rate hitting the zero lower bound. The coefficient of interest, β, measures how the ZLB shock differentially affected firms depending on their pre-existing financial exposure.

Firm fixed effects (μ_i) absorb time-invariant heterogeneity across firms, while sector-by-year fixed effects (λ_{ts}) account for sector-specific trends. We also include year-by-electricity-decile fixed effects (η_{tp}), where the deciles are defined by each firm's average share of electricity in total energy use over 2000–2011, to flexibly capture trends related to the initial energy mix. To control for EU Emissions Trading System (EU ETS) policies, we include phase-by-year fixed effects (θ_{ets}) for companies subject to the EU ETS. Following Colmer et al. (2024) we use announcement (2001–2004), trading phase I (2005–2007), trading phase II (2008–2012), and trading phase III (2013–2019) to incorporate potentially trend changes during the different ETS phases. Standard errors are clustered at the firm level, to account for the possibility that the errors are correlated across the different years within each firm.

The same framework is applied to study the effects of relaxing the financial constraints on the various components of Equation 6 in Section 2. First, our rich data allows us to examine the scale effect separately for the three production inputs (K, L, E). Second, we are able to identify the general economic efficiency effects (i.e. TFP). Third, we can dissect the mechanisms behind the green technique effects by estimating the impact of financial constraints on environmental protection investments, investments in electricity self-generation (e.g., cogeneration or solar panels) and the share of electricity on total energy use.

[6] Formally, $DtA_{i,pre} = (\tau_1 - \tau_0)^{-1} \sum_{t=\tau_0}^{\tau_1} \frac{\text{Debt}_{it}}{\text{Assets}_{it}}$, with $\tau_0 = 2000$ and $\tau_1 = 2011$. It should be noted, however, that information on debt was not reported in year 2008.

Because electricity does not generate direct carbon emissions, firm-year pairs that rely exclusively on this energy source are coded as zero emissions. A log transformation would mechanically drop many of these non-random zero-valued observations. To avoid this, we use the inverse hyperbolic sine (ihs) transformation, which retains zero values while approximating the log transformation for larger values.[7] The ihs transformation is attractive because it allows us to keep the full sample, including zero-emission observations, while maintaining a functional form that is widely used and easily comparable across studies. But the ihs is not without limitations, since estimated coefficients depend on the unit of measurement of the transformed variable – potentially complicating causal interpretation (see Aihounton and Henningsen, 2021; Chen and Roth, 2024). For this reason, we complement our baseline ihs results with robustness checks using alternative estimators, including the Negative Binomial Quasi Maximum Likelihood Estimator (NB-QMLE), as also recommended by Chen and Roth (2024).

Event-study design. The DiD framework in Equation 7 can be extended to an event-study specification to test for pre-trends and investigate the dynamic evolution of the treatment effect:

$$ihs(Y_{it}) = \sum_{\tau=2000}^{2019} \beta_\tau (DtA_{i,pre} \times \tau) + \mu_i + \lambda_{ts} + \eta_{tp} + \theta_{ets} + \varepsilon_{it} \tag{8}$$

The coefficients β_τ capture in a flexible manner the differential trends in the dependent variable across firms with different pre-existing debt-to-asset ratios. Although all firms faced the ZLB, differences in leverage allow us to examine whether more leveraged firms were already on distinct trajectories. If pre-event coefficients are statistically indistinguishable from zero, this supports the parallel trends assumption. In cases where pre-event coefficients are significant but opposite in sign to post-event effects, this may indicate reversal of prior trends, although such patterns should be interpreted more cautiously. This event-study complements the main DiD by providing a dynamic and visual assessment of treatment effects.[8]

Although our regression provides a two-way fixed effects estimate, where all firms are treated but the exposure to the shock is a continuous variable, one might argue that the results cannot be interpreted as causal because more indebted companies could have already had a different trend before the arrival of the ZLB. The event-study setting allows us to test the empirical validity of such an identifying assumption. In the regression of Equation 8, the time-varying coefficients β_τ refer to the differential (flexible) trends in the

[7] Formally, $ihs(Y_{it}) = \ln(Y_{it} + \sqrt{Y_{it}^2 + 1})$ and for large Y_{it} we have that $ihs(Y_{it}) \approx \ln(2Y_{it})$.

[8] Alternatively, we also control for pre-trends by including the growth rate of the dependent variable before the ZLB in the baseline setting of Equation 7.

dependent variable for firms with heterogeneous levels of debt-to-asset ratio, without the identification of a particular shock to financial constraints. In particular, if all the coefficients β_τ before the event of interest do not significantly deviate from zero, the parallel trend assumption is satisfied. To test for this hypothesis, we carry out a Wald test to assess the joint significance of all the β_τ coefficients pre-dating the ZLB.

Heterogeneity by firm size. Since financial constraints are only imperfectly captured by the debt-to-asset ratio, we estimate an augmented specification that accounts for heterogeneity across firm sizes, which is known to be strongly inversely correlated with the degree of credit constraints. Specifically, we interact the main independent variable with five size-class dummies, based on firms' average sales pre-shock.[9] The size-augmented regression reads:

$$ihs(Y_{it}) = \sum_{k=1}^{4} \alpha_k(Size_{k,pre} \times t) + \sum_{k=1}^{5} \beta_k(Size_{k,pre} \times DtA_{i,pre} \times ZLB_t) + \mu_i + \lambda_{ts} + \eta_{tp} + \theta_{ets} + \varepsilon_{it} \tag{9}$$

where $k \in \{1, \ldots, 5\}$ indexes size classes computed before the ZLB, and the α_k terms capture group-specific time trends.[10] This specification allows firms in different size classes to exhibit heterogeneous pre-shock trends (α_k) while also estimating the differential impact of the ZLB-induced credit easing on the dependent variables for each size class (β_k).

Policy interaction. The final specification augments the baseline setting by accounting for the interaction between financial constraints with the EU ETS environmental regulation. The goal of this regression is not to evaluate the effect of the ETS, but rather to estimate the effect of easing credit constraints for firms regulated by the EU ETS. As the EU ETS policy only regulates companies and installations that emit large amounts of greenhouse gases, these firms are not a random sub-sample of the population. Therefore, the regressions that follow require constructing a credible counterfactual.

We construct a balanced sub-sample using a N-to-one coarsened exact matching algorithm (Iacus et al., 2012) to find a set of firms similar to the EU ETS companies in terms of size, productivity, capital-labor ratio and industrial class.[11] We then employ the sub-sample comprising treated and matched companies to estimate an augmented baseline regression of the following form:

$$ihs(Y_{it}) = \beta_1(ETS_i \times ZLB_t) + \beta_2(DtA_{i,pre} \times ZLB_t) + \beta_3(DtA_{i,pre} \times ETS_i \times ZLB_t) + \mu_i + \lambda_{ts} + \eta_{tp} + \theta_{ets} + \varepsilon_{it} \tag{10}$$

[9] We first compute the average sales for each firm in the years before the ZLB and we then construct the five size-classes based on the quintiles of the in-sample distribution.

[10] The fifth, largest size group is omitted from the first summation to avoid perfect multicollinearity, serving as the reference category.

[11] Details about the matching strategy are reported in Appendix D.

where ETS_i represents a dummy with value one for EU ETS regulated companies, and the coefficient β_3 associated with the triple interaction ($DtA_{i,pre} \times ETS_i \times ZLB_t$) can be interpreted as the marginal effect of easing credit constraints for ETS-regulated firms. The pairwise interactions among variables are also included to capture the effect of the ZLB on Non-ETS firms ($DtA_{i,pre} \times ZLB_t$) and the effect of the ZLB on ETS firms independently of their leverage ($ETS_i \times ZLB_t$).[12]

4 Data and descriptive statistics

Our work relies on a set of various rich data sources covering the 2000-2019 period.[13] The main dependent variable of our empirical analysis is the amount of combustion-related carbon dioxide (CO_2) emissions of companies. This is computed by multiplying each energy input by its technical CO_2 emission factor retrieved from the US Energy Information Administration. Information on energy consumption is obtained from the *EACEI* survey (*Enquête sur les consommations d'énergie dans l'industrie*). The survey collects detailed information on energy purchase, consumption and expenditure broken down by 12 energy inputs for a representative sample of manufacturing establishments. All establishments with more than 250 employees are included, while establishments between 20 and 250 are sampled according to a stratified design (by sector NACE, size and region, see **?**, for further details)and Appendix A for details on the sectors used in our analysis]Marin2021. The response rate of the survey is around 90%. Emissions and energy-related information (energy consumption and energy mix) is then aggregated across all the surveyed establishments belonging to the same enterprise, as our core independent variable measuring financing constraints which can only be measured at the firm level. This is a critical step, as for many enterprises just a subset of their establishments is covered by EACEI, leading to underestimated emissions. To have a better and consistent picture of enterprise-level energy use and emissions, we limit our analysis to firm-year pairs for which establishments representing at least 90% of the enterprise's employment were included in EACEI in that particular year.[14]

Financial data for French firms are retrieved from administrative data on balance sheet and income statement available from the FARE-FICUS databases ("*Annual structural statistics of companies from the*

[12]The interaction $DtA_{i,pre} \times ETS_i$ is omitted because it is absorbed by the firm fixed effects μ_i.

[13]We decided to ignore observations for 2020-2023 (even if already available) because the Covid-19 pandemic and the subsequent fiscal policies implemented by the European Union (EU) and the French government have strongly affected both the economic and environmental outcomes of French companies (see, among others, Gourinchas et al., 2025; Guerini et al., 2024) that would require specific and detailed analyses.

[14]Establishment-level employment is retrieved from *DADS-Etablissements*, that is administrative data on employment for the universe of French establishments in the private sector. If we considered the most stringent requirement (i.e., firms with all establishments surveyed in *EACEI*), we would lose about 21.4% of observations and 7.3% of unique firms.

Table 1: Information about the data sources.

Database	Period	Coverage	Main variables of interest
FICUS-FARE	2000-2019	Universe of firms	Debt-to-Asset, Value Added, Capital, Sales, Productivity
EACEI	2000-2019	Survey of ≈12k establishments/year	Carbon Emissions, Energy Demand, Self Production, Electricity Share
ANTIPOL	2000-2019	Survey of ≈12k establishments/year	Green Specific Investment, Green Integrated Investment, Air Investment
DADS	2000-2019	Universe of establishments	Labor
EUTL	2005-2019	Universe of ETS-regulated establishments	ETS Dummy

ESANE scheme"). The database covers the universe of French non-financial corporations subject to corporate taxation.[15] For multi-establishment firm-year pairs that are retained in the sample, we re-scale balance sheet variables proportionally with the share of employment in *EACEI*-covered establishments.

In addition, we also employ the *ANTIPOL* database ("*Enquête sur les investissements dans l'industrie pour protéger l'environnement*"), an annual survey on approximately 12 000 establishments concerning their environmental protection investments and studies. In particular, this survey focuses on tangible and intangible investments destined for environmental protection. We employ the same rule used for *EACEI* to aggregate information from *ANTIPOL* from the establishment to the enterprise-level for multi-establishment enterprises. The samples of establishments surveyed by *ANTIPOL* and *EACEI* are not the same. The overlap between the two samples is limited to very large enterprises (1534 enterprises, resulting in 5532 observations). For this reason, when considering dependent variables from *ANTIPOL*, we employ a different sample than the one used for the *EACEI*-based analysis.

We also identify firms subject to the EU Emission Trading System (ETS), that is the main climate policy at the EU level.[16] We consider as EU ETS companies those that own at least one regulated installation. We use the unique firm identifier (SIREN) of Operators Holding Accounts of regulated entities retrieved from the European Union Transaction Log to identify firms with at least one establishment subject to the policy.[17] Table 1 summarizes the basic information of the employed data sources. Our main sample

[15]We exclude from the analysis companies with incomplete information as well as firms located in the *Départments d'Outre-Mer* (Guadeloupe, Guyane, Martinique, Mayotte and La Réunion). Variables are deflated using INSEE industry-specific deflators.

[16]The EU ETS is the largest cap-and-trade scheme for greenhouse gases currently in place in the world. It was firstly introduced in 2005 and currently covers about 11 000 industrial installations, accounting for about 40% of EU greenhouse gas emissions. It consists in a system of tradable emissions allowance, where industrial installations are required to return each year a number of emission allowances that corresponds to their annual emissions of greenhouse gases. Allowances can be freely traded among participants. The policy experienced substantial changes over the period of reference, a first pilot phase (2005–2007) in which allowances were mostly allocated for free, followed by a second phase (2008–2012) characterized by a higher penalty for non-compliance (from €40 to €100 per tonne of emissions beyond compliance), a third phase (2013-2020) with the default allocation rule being the auctioning of permits. See (European Commission, 2015) for further information.

[17]For those few Operators Holding Accounts for which the SIREN number was not reported, we identified the corresponding

consists of an unbalanced panel of 13 432 unique firms (12 303 for *ANTIPOL*), corresponding to 71 853 observations (81 867 for *ANTIPOL*) over the 2000-2019 period. Additional information about the sample construction is detailed in Appendix A.

Table 2 provides descriptive evidence about the main variables of interest, aggregated by size class as constructed from quintiles of the sales distribution. We employ sales rather than labor as revenues directly measures a firm's market activity and are more correlated with the market power of a company, also highlighting its possible ability to overcome credit constraints, even if financially fragile.[18] The relationship between our indicators of financial constraints and firm size is negative, with companies in the top quintile of the size distribution being characterized by DtA and DtE that are respectively about 10% and 25% smaller than firms in the bottom quintile. The level of carbon emissions is positively associated to size. However, the intensity of emissions per unit of value added ($CO2/E$) is also larger for the top quintile firms, driven by higher energy intensity (E/VA). This might be due to the fact that the most emission-intensive sectors – e.g., basic metals and refineries – are also characterized by substantial economies of scale and, consequently, they are more likely to belong to the top quintile group. Notably, the dispersion of carbon emissions per value added within each size quintile is very large, especially at the top and at the bottom of the firm size distribution.

Table 2: Summary statistics of key variables, depending on size class.

Size class	DtA	DtE	$CO2$	$CO2/VA$	$CO2/E$	E/VA	VA	N
All	0.5342	1.0132	18879	1.8439	82.0736	0.0154	12558	71853
	(0.2251)	(1.1423)	(225200)	(72.6528)	(54.0475)	(0.3813)	(53653)	
Small	0.5681	1.2100	1129	1.2284	86.2240	0.0106	1352	13336
	(0.2379)	(1.3817)	(5684)	(11.9027)	(58.9153)	(0.1048)	(808)	
Medium-Small	0.5450	1.0412	2140	1.0435	79.6716	0.0096	2565	14928
	(0.2246)	(1.1700)	(6526)	(3.8472)	(56.4092)	(0.0308)	(1412)	
Medium	0.5306	0.9892	4241	1.2478	81.1260	0.0118	4679	14756
	(0.2246)	(1.1179)	(12211)	(9.6565)	(52.7187)	(0.0790)	(2704)	
Medium-Large	0.5214	0.9434	8956	1.5295	81.9698	0.0149	9063	15143
	(0.2175)	(1.0218)	(27817)	(14.2070)	(52.0904)	(0.1197)	(5268)	
Large	0.5088	0.8991	77930	4.1723	81.7005	0.0298	44914	14320
	(0.2176)	(0.9753)	(499799)	(161.5556)	(50.0291)	(0.8359)	(114483)	

Notes: For each size class, the first row reports the mean while the second row the standard deviation, in parentheses. Size classes (in ihs) are defined as: Small $[5.29, 9.13]$, Medium-Small $(9.13, 9.82]$, Medium $(9.82, 10.5]$, Medium-Large $(10.5, 11.3]$, Large $(11.3, 16.4]$. $CO2$ is measured in kilotonnes; $CO2/E$ in kilotonnes to kilowatts; E/VA in in kilowatts to Mln €; VA in Mln €.

To test that the change in monetary policy led to an increase in debt, in Table 3 we group the firms by their debt-to-asset ratio before the change in monetary policy stance and we present their average DtA

enterprise in our dataset through matching on name and address.

[18] We computed similar descriptive statistics for secondary variables, and also conditioning upon the quintiles of the firms' DtA distribution and on the EU ETS status. Since the main implications are similar, we report these tables in Appendix B.

Table 3: Comparison of DtA and Debt averages before and after the Zero-Lower Bound.

DtA class	DtA before	DtA after	Δ DtA	Debt before	Debt after	Δ Debt	*N*
All	0.5458	0.5109	-0.0349***	14504.72	20231.51	5726.79***	71853
	(0.2242)	(0.2246)	(-18.3580)	(66239.28)	(101218.10)	(7.3786)	
Low	0.2788	0.3268	.0480***	14994.39	28383.73	13389.34***	14387
	(0.0913)	(0.1665)	(17.6954)	(73655.17)	(172833.74)	(4.8904)	
Medium-Low	0.4275	0.4497	.0222***	12632.14	15925.51	3293.37***	14564
	(0.0897)	(0.1749)	(7.8643)	(56450.51)	(50566.92)	(3.3768)	
Medium	0.5344	0.5253	-0.0091***	13206.35	18792.99	5586.64***	14533
	(0.1032)	(0.1805)	(-3.0234)	(35216.17)	(49307.15)	(6.5720)	
Medium-High	0.6405	0.5985	-0.0420***	17632.50	21882.41	4249.91**	14408
	(0.1087)	(0.1928)	(-13.0400)	(102735.77)	(114612.25)	(2.0303)	
High	0.8344	0.7015	-0.1329***	14085.10	15037.24	952.14	13961
	(0.1849)	(0.2211)	(-31.1017)	(41019.38)	(29076.06)	(1.4453)	

Notes: For each DtA class, the first row reports the mean while the second row the standard deviation, in parentheses. DtA classes are defined as: Low $[0, 37\%]$, Medium-Low $(37\%, 48\%]$, Medium $(48\%, 58\%]$, Medium-High $(58\%, 70\%]$, and High $(70\%, 169\%]$. The null hypothesis for the Δ columns is the equality of the cross-sectional average before and after the ZLB, within a DtA class.

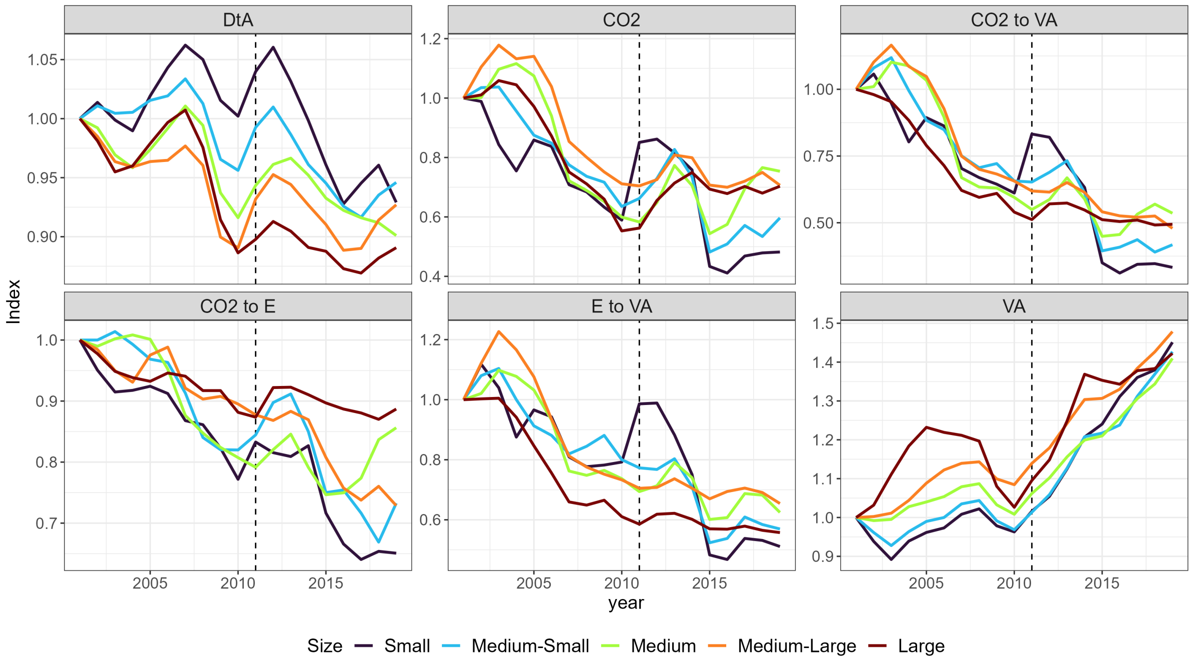
and debt levels, both before and after the ZLB. These descriptive statistics underline two facts. Firstly, we observe that the debt level has increased for all groups. This suggests that the main mechanisms underlying our identifying assumption is validated as the overall debt has grown after the ZLB.[19]. Secondly, we find that the three groups of firms that entered the ZLB phase with high leverage, have all undergone a deleveraging process thereafter. Hence, highly indebted companies exploited the sharp decline in debt costs to increase their assets and renegotiate their outstanding debts, overall lowering their exposure.

Further insights into the dynamics of key variables are gathered in Figure 1, conditional on size class. Overall, most indicators display monotonic trends between 2000 and 2019. Carbon emissions, emissions per unit of energy, and emissions per unit of value added all declined, largely mirroring the fall in energy intensity. Before the ZLB, differences across size classes are limited (except for $CO2/E$), but after 2012 some heterogeneity emerges: smaller firms show greater variability in both energy use and emissions.[20] By contrast, value added rose steadily for all size groups, although firms in the bottom quintile recorded the fastest average growth between 2012 and 2019.

[19] The t-stat indicates, however, that this increase is insignificant for the DtA_5 group, representing the most leveraged firms before the ZLB

[20] Part of this variability can also be imputed to the sampling scheme of the *EACEI* database, as the smaller firms are less likely to be observed in multiple subsequent periods.

Figure 1: Time series of main variables conditional on size class.



Notes: Variables have been normalized to one at 2000 within size class. Size classes are defined by the quintiles of average sales before the ZLB. Size classes (in ihs) are defined as: Small $[5.29, 9.13]$, Medium-Small $(9.13, 9.82]$, Medium $(9.82, 10.5]$, Medium-Large $(10.5, 11.3]$, Large $(11.3, 16.4]$. The vertical dashed line points to the hit of the ZLB.

5 Results

In this section, we turn to the empirical results. We begin with the baseline estimates of how credit easing affected firms' emissions, moving from the main DiD specification to an event-study analysis, and then to heterogeneity across firm size. We proceed with the underlying mechanisms following the same sequence, which allows us to better qualify the role of scale, technique, and productivity effects. Finally, we explore how the main results interact with environmental policies, focusing on firms regulated by the EU ETS.

5.1 The average effect of credit easing on emissions

Table 4 presents the main results of the paper associated with the estimation of the baseline model of Equation 7. In column 1, the negative coefficient indicates that CO_2 emissions reductions are larger for firms facing tighter financial constraints before the shock. Specifically, a percentage point increase in the Debt-to-Asset (DtA_{pre}) is associated with a 0.444% decline in CO_2 emissions. Using the interquartile range (IQR) of financial fragility pre-dating the shock, our estimate would imply a reduction in CO_2 emissions of 9.41%.

The result of column 1 is remarkable as easing the financial constraints can either increase or decrease total emissions, depending on the relative size of the technique and the scale effects. We find that, while both effects play a role and are statistically significant, the negative technique effect (-0.517, column 2) more than offsets the positive size effect (0.073, column 5). Quantitatively, an IQR increase in financial fragility translates into a 2% increase in firms' value added and a 10.6% reduction in emission intensity (CO_2/VA).

The technical effect of the Kaya identity can be further decomposed into a change in the carbon content of energy use (column 3) and a change in the energy intensity of value added (column 4). Our results indicate that both margins of the technique effect are relevant and of approximately the same size. To illustrate, an IQR increase in financial fragility is associated with a 6.31% reduction in the carbon content of energy and a 5.65% improvement in energy efficiency. Section 5.4 shows that these results are robust to different modeling choices, such as using alternative measures of exposure and different transformations of the dependent variables.

Table 4: Baseline results on the effect of credit easing on the main outcome variables.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	-0.444***	-0.517***	-0.275**	-0.242***	0.073**
	(0.147)	(0.148)	(0.125)	(0.052)	(0.037)
ΔIQR effect	-9.41%	-10.60%	-6.31%	-5.65%	2.00%
N. obs.	68 964	68 964	68 964	68 964	68 964
N. firms	10 533	10 533	10 533	10 533	10 533

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. ΔIQR effect is the marginal effect over the interquartile range of the DtA distribution. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

The coefficients estimated in Table 4 could be interpreted as causal provided that the standard parallel trend assumption is not violated.[21] In Figure 2, we estimate the event study model of Equation 8 to lend support to the credibility of this assumption and to precisely pinpoint the timing of the effect. Across the board, the figure suggests that, prior to 2012, the coefficients on carbon emissions (top-left), carbon content of energy (top-right), and energy intensity (bottom-left) are not statistically significant. There are only few exceptions where we observe a statistically significant effect going in the same direction as the main effect, particularly for $CO2$ (2005 coefficient), $CO2/E$ (2005), E/VA (2000) and VA (2000 and 2004). Nonetheless, in all these cases the significant effects are followed by a prolonged period (i.e., at least 5

[21] That is: our measure of financial fragility should not have had any impact on the dependent variables of interest before the arrival of the ZLB.

years) of statistically insignificant effects. The absence of pre-trends is also detected using a Wald test for the joint significance of the pre-ZLB coefficients, which fails to reject the null of no pre-trends, except for VA.[22] In this latter case, however, the pre-trend of VA goes in the opposite direction to our post-ZLB results. This points toward the interpretation that the ZLB had an impact that was sufficiently strong to reverse a pre-existing trend. In Table C.1 of Appendix C, we further explore the credibility of the parallel trend assumption by explicitly controlling for the trends of the dependent variable between 2000 and 2011, before the arrival of the ZLB.[23] We find that qualitatively our results are confirmed, although the statistical power is somewhat limited relative to the preferred regressions, also due to a lower number of observations.[24]

The event study also sheds light on the timing of the ZLB effects. The scale effect emerges almost on impact, as financially constrained firms quickly benefit from lower credit costs. By contrast, the technique effect unfolds more gradually. Both the carbon content of energy and energy intensity become statistically significant only two years after the ZLB, also delaying the observed reduction in CO_2 emissions from 2014 onward. This pattern is intuitive since expanding production can be achieved rapidly by mobilizing idle capacity, while lowering emissions or improving energy efficiency requires capital investments and structural adjustments that take longer to materialize (Hicks, 1987; Del Boca et al., 2008).

5.1.1 Heterogeneity: the role of firm size

Firm size is an important source of heterogeneity, as larger companies generally have higher creditworthiness than smaller ones, even for a similar level of Debt-to-Asset. Table 5 presents the results of the baseline model, modified to include the triple interaction between the ZLB dummy, pre-ZLB DtA, and five size class dummies (see Equation 9). This augmented regression also controls for linear trends specific to each size class, which purge pre-existing differences in trends between firms of different sizes.[25]

Our central finding is that the impact of relaxing financial constraint on emissions reductions is concentrated among small, medium-small and medium size enterprises, while absent for medium-large and

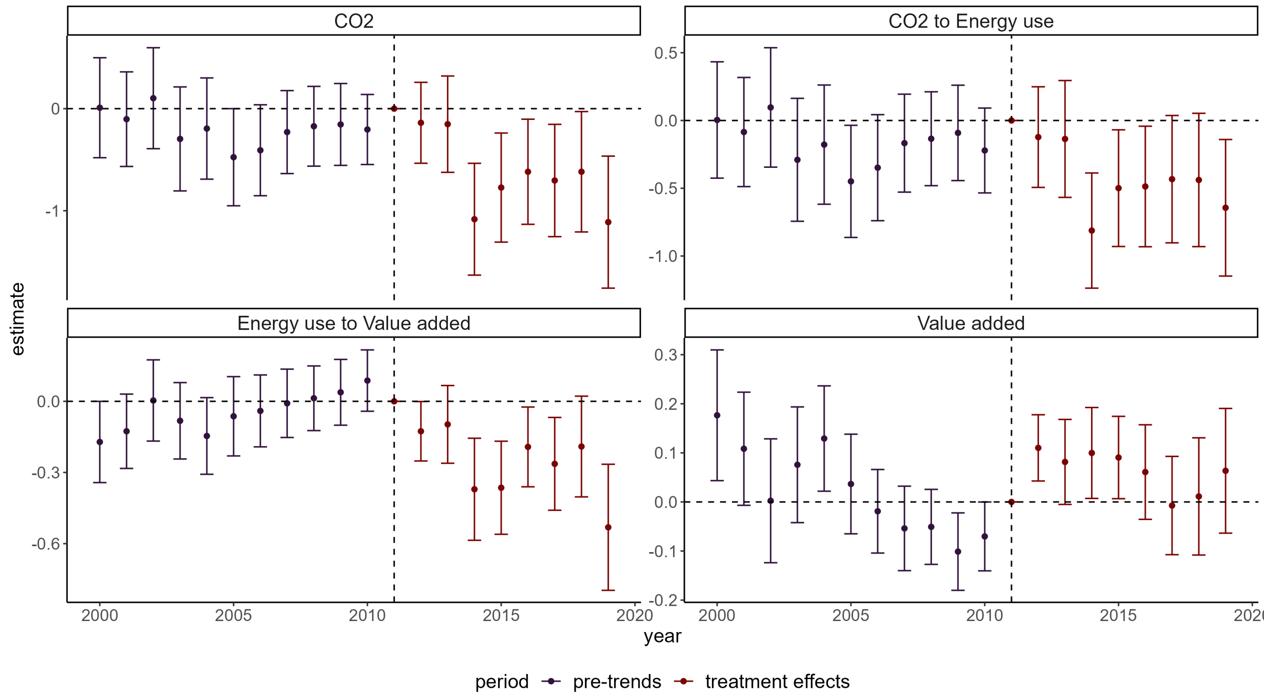
[22]The parallel trend Wald test p-values are $CO2 = 0.42$; $CO2/E = 0.29$; $E/VA = 0.13$; $VA < 0.01$ indicating the absence of pre-trends for all variables except for VA.

[23]In particular, we first compute the firm-level averages of Y between 2000-2005 (early phase before the ZLB, so $\bar{Y}_i^{early}$) and between 2006-2011 (late phase before the ZLB, hence $\bar{Y}_i^{late}$). Then, we include the difference between the two averages (i.e., $\bar{Y}_i^{late} - \bar{Y}_i^{early}$) interacted with year-specific dummies as an additional regressor in Equation 7.

[24]Because not all firms are surveyed in all years or because some firms were founded after 2006, the new regressor displays a larger number of missing values with respect to the other variables. Moreover, this process wipes out many small firms that are found to be highly relevant in the next sections.

[25]The coefficient corresponding to $year \times Size_5$ is omitted due to collinearity. This implies that the trend displayed by the largest companies before the ZLB serves as the reference category in each regression.

Figure 2: Event study analysis of the effect of credit easing on main outcome variables.



Notes: Coefficients are normalized to zero in 2011, the last year before the ZLB.

large firms. As highlighted by the statistically insignificant coefficients associated with the size classes trends, these effects are not driven by differences across firms pre-dating the ZLB shock.[26] Quantitatively, the impact of an IQR increase in DtA_{pre} on CO_2 is larger than the average effect for small (-18.08%) and medium-small (-11.73%) enterprises.

Moving across the columns of the decomposition, it appears clear that the interplay between the negative technique effect and the positive size effect is much more pronounced for firms in the two smaller size classes. Of the two components of the green technique effect, the decarbonization of the energy mix matters more for small firms, while energy efficiency improvements gain prominence as firm size increases (in relative terms). This aligns with previous empirical findings that the expected gains of adopting more "radical" green technologies are potentially larger for smaller firms and may be hampered by the presence of credit constraints (Howell, 2017; Noailly and Smeets, 2022). Finally, we observe that for all variables of the decomposition, the magnitude of the effect decreases with firm size, becoming negligible for the largest corporations. This supports the claim that financial constraints are less binding for large firms, giving the DtA a weaker explanatory power within this size class.

[26] To be sure, these results are qualitatively unchanged in Table C.2 of Appendix C, where we include the pre-trend of the dependent variable to Equation 9.

Table 5: The effect of credit easing on the main outcome variables depending on firm size

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$year \times Size_1$	0.001	0.008	0.003	0.005	−0.006**
	(0.011)	(0.011)	(0.009)	(0.004)	(0.003)
$year \times Size_2$	−0.013	−0.011	−0.011	0.001	−0.002
	(0.010)	(0.010)	(0.009)	(0.003)	(0.003)
$year \times Size_3$	0.004	0.008	0.007	0.001	−0.004
	(0.009)	(0.009)	(0.008)	(0.003)	(0.003)
$year \times Size_4$	0.001	0.001	0.000	0.001	0.000
	(0.008)	(0.008)	(0.007)	(0.003)	(0.003)
$DtA_{pre} \times ZLB \times Size_1$	−1.024***	−1.222***	−0.830***	−0.392***	0.198***
	(0.190)	(0.190)	(0.160)	(0.070)	(0.041)
$DtA_{pre} \times ZLB \times Size_2$	−0.580***	−0.714***	−0.326*	−0.388***	0.133***
	(0.200)	(0.201)	(0.172)	(0.066)	(0.039)
$DtA_{pre} \times ZLB \times Size_3$	−0.392**	−0.447**	−0.267	−0.180***	0.055
	(0.200)	(0.201)	(0.173)	(0.067)	(0.045)
$DtA_{pre} \times ZLB \times Size_4$	−0.051	−0.114	0.036	−0.150**	0.063
	(0.179)	(0.178)	(0.152)	(0.061)	(0.045)
$DtA_{pre} \times ZLB \times Size_5$	0.010	0.116	0.175	−0.059	−0.105**
	(0.176)	(0.177)	(0.152)	(0.063)	(0.051)
ΔIQR effect ($Size_1$)	-18.08%	-19.90%	-15.91%	-9.14%	6.17%
ΔIQR effect ($Size_2$)	-11.73%	-13.58%	-7.40%	-8.56%	3.79%
ΔIQR effect ($Size_3$)	-8.09%	-8.98%	-5.84%	-4.11%	1.40%
ΔIQR effect ($Size_4$)	-1.27%	-2.77%	0.95%	-3.59%	1.68%
ΔIQR effect ($Size_5$)	0.25%	3.02%	4.71%	-1.42%	-2.46%
N. obs.	68 964	68 964	68 964	68 964	68 964
N. firms	10 533	10 533	10 533	10 533	10 533

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. The $year \times Size_5$ regressor is omitted because of perfect multicollinearity and is the reference category. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Size classes (in ihs) are defined as: $Size_1 \in [5.29, 9.13]$, $Size_2 \in (9.13, 9.82]$, $Size_3 \in (9.82, 10.5]$, $Size_4 \in (10.5, 11.3]$, $Size_5 \in (11.3, 16.4]$. ΔIQR effect is the marginal effect over the interquartile range of the DtA distribution. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

5.2 Mechanisms

In this section, we examine the potential mechanisms through which financially constrained firms exploit the easing access to credit to reduce their carbon emissions. Our rich data allows us to construct an empirical counterpart for each of the components of the theoretical decomposition presented in Section 2: (i) total factor productivity (TFP) estimates or apparent labor productivity (ALP, as measured by the ratio between value added and employment), to capture general improvements in economic efficiency.[27]; (ii) capital, labor and energy to further decompose the size effect; (iii) the share of electricity in total energy consumption to proxy changes in the energy mix towards cleaner sources, while the effect on energy efficiency were already covered in the Kaya-type decomposition of Table 4. In addition, we try

[27]TFP is estimated with the Levinsohn and Petrin (2003) method, in which we employ investment as a proxy for unobserved productivity shocks. We rely upon the `prodest` R library (see Rovigatti, 2017) for the estimation procedure.

to further understand the green technique effect by assessing the impact of easing financial constraints on cogeneration investments – measured by a dummy variable indicating whether a firm has installed self-generation systems such as co-generators or photovoltaic panels – and investments in environmental protection. However, this last channel can be estimated only on a small subset of firms, after merging our sample with the *ANTIPOL* database (as described in the data construction Appendix A) and hence is discussed separately in Section 5.2.2.

Table 6 presents the main results, divided in *Panel A* for green and general technique effect and *Panel B* for the decomposition of the size effect into the three different inputs of production. Overall, the main finding is that easing credit constraints leads to reduction in CO_2 emissions through improvements in general productivity and capital deepening, while both electrification and investments in cogeneration play no significant role in aggregate. This is not surprising, since the monetary policy easing at the origin of the financial constraints relaxation was not targeted towards green sectors nor to green technologies. In a closely related paper, Goetz (2018) finds a positive and significant effect for large US corporate pollution abatement investment. The difference might be due either to the fact that the US corporations are larger or that the monetary policy shock in Goetz (2018) has been identified throughout the maturity extension programme and targeted only the long-term debt, creating a different type of discontinuity relative to ours. However, the absence of a positive effect on electricity usage suggests that most of the observed reduction in the carbon content of energy (see Table 4) is due to changes in the energy mix from more to less carbon-intensive fossil fuel – such as a transition from coal to gas. This is also consistent with the impacts of a French tax on the consumption of electricity (the so-called CSPE - *Contribution au service public de l'électricité*), which increased steadily since 2000 and discouraged the electrification of manufacturing production (Marin and Vona, 2021).

Following the same structure of the main results, we expand the analysis of the transmission mechanisms using an event study framework, presented in Figure C.1 in Appendix C. The graphical results as well as the Wald tests point toward the presence of pre-existing trends for ALP and TFP (Panel A), although such trends go in opposite direction to the estimated effect after the credit easing.[28] With respect to the effect on the input mix (i.e., Panel B), we find a significant pre-trend only for employment and mild pre-trends for capital-labor ratio and capital stock.[29] While this suggests that financially constrained firms started substituting labor with capital before the ZLB, there is a detectable acceleration in the size of the coefficient after the ZLB. In contrast, we do not find any significant pre-trend for energy use, which

[28] For Panel A, the p-values of the Wald test for parallel trends are: $\%Elect. = 0.03$; $Selfgen. = 0.93$; $ALP = 0.04$; $TFP = 0.02$.

[29] For Panel B, the p-values of the Wald test for parallel trends are: $K/L = 0.06$; $K = 0.08$; $L = 0.00$; $E = 0.90$.

Table 6: Mechanisms: the effects of credit easing on productivity, self-generation investments, and the input mix.

Panel A.	**Dep. Variable** (last two ihs-transformed):			
	(1)	(2)	(3)	(4)
	$\%Elect.$	$Selfgen.$	ALP	TFP
$DtA_{pre} \times ZLB$	0.008	-0.004	0.193***	0.036***
	(0.011)	(0.015)	(0.028)	(0.008)
ΔIQR effect	0.22%	-0.12%	5.06%	0.94%
N. obs.	67 925	67 929	68 964	68 964
N. firms	10 465	10 463	10 533	10 533
Panel B.	**Dep. Variable** (all ihs-transformed):			
	(1)	(2)	(3)	(4)
	K/L	K	L	E
$DtA_{pre} \times ZLB$	0.393***	0.274***	-0.119***	-0.169***
	(0.066)	(0.069)	(0.027)	(0.050)
ΔIQR effect	12.65%	8.27%	-2.95%	-4.08%
N. obs.	68 964	68 964	68 964	68 964
N. firms	10 533	10 533	10 533	10 533

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. $\%Elect.$ and $Selfgen.$ are respectively the share of electricity usage and a dummy variable equal one for firms installing cogeneration or solar panels technologies. They have fewer observations because of missing answers about these specific questions. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.
The p-values of the Wald test (H_0: absence of parallel trend) are: $\%Elect. = 0.03$; $Selfgen. = 0.93$; $ALP = 0.04$; $TFP = 0.02$; $K/L = 0.06$; $K = 0.08$; $L = 0.00$; $E = 0.90$.

is the most relevant input for our main results.

5.2.1 Heterogeneity: the role of firm size

We then explore the heterogeneity in the incidence of the different mechanisms by firm size classes. The results are presented in Table 7. In line with the main results, we observe that productivity improvements (both TFP and ALP) induced by the relaxation of credit constraints are stronger for smaller firms than for larger ones (Columns 2-3, Panel A), while we do not observe any effect on cogeneration investments (Column 4). However, productivity improvements are also detectable for large and medium-sized firms. As a remarkable divergence in the firm response to the ZLB, we find that the ZLB induces small firms to electrify their production processes, while very large firms do the opposite (Column 1, Panel A). In Panel B, we find that smaller firms are more elastic – vis-à-vis the larger ones – in adjusting their input mix in response to the credit easing. As expected, Columns 1 and 2 suggest that the induced acceleration in capital deepening is larger for small firms, but evident for all firms. What is different across firms is adjustment to the ZLB for the other two inputs. On the one hand, capital deepening is associated with a detectable reduction in energy use, especially for small (class size 1-2) and large (class size 5) firms.

Table 7: Mechanisms: the effects of credit easing depending on firm size.

Panel A.	Dep. Variable (last two ihs-transformed):			
	(1)	(2)	(3)	(4)
	$\%Elect.$	$Selfgen.$	ALP	TFP
$DtA_{pre} \times ZLB \times Size_1$	0.039**	−0.008	0.235***	0.049***
	(0.016)	(0.014)	(0.035)	(0.011)
$DtA_{pre} \times ZLB \times Size_2$	0.011	−0.012	0.249***	0.055***
	(0.014)	(0.013)	(0.031)	(0.009)
$DtA_{pre} \times ZLB \times Size_3$	0.006	−0.014	0.132***	0.020**
	(0.014)	(0.016)	(0.034)	(0.010)
$DtA_{pre} \times ZLB \times Size_4$	0.007	0.007	0.190***	0.032***
	(0.013)	(0.018)	(0.035)	(0.011)
$DtA_{pre} \times ZLB \times Size_5$	−0.031**	−0.001	0.154***	0.021**
	(0.014)	(0.022)	(0.040)	(0.011)
ΔIQR effect ($Size_1$)	1.14%	-0.24%	7.46%	1.41%
ΔIQR effect ($Size_2$)	0.31%	-0.31%	7.52%	1.49%
ΔIQR effect ($Size_3$)	0.15%	-0.34%	3.52%	0.51%
ΔIQR effect ($Size_4$)	0.18%	0.19%	5.41%	0.85%
ΔIQR effect ($Size_5$)	-0.74%	-0.03%	4.11%	0.53%
N. obs.	67 925	67 929	68 964	68 964
N. firms	10 465	10 463	10 533	10 533

Panel B.	Dep. Variable (all ihs-transformed):			
	(1)	(2)	(3)	(4)
	K/L	K	L	E
$DtA_{pre} \times ZLB \times Size_1$	0.541***	0.504***	−0.037	−0.194***
	(0.086)	(0.089)	(0.029)	(0.070)
$DtA_{pre} \times ZLB \times Size_2$	0.423***	0.307***	−0.116***	−0.255***
	(0.073)	(0.076)	(0.029)	(0.065)
$DtA_{pre} \times ZLB \times Size_3$	0.399***	0.322***	−0.077**	−0.125**
	(0.076)	(0.081)	(0.033)	(0.064)
$DtA_{pre} \times ZLB \times Size_4$	0.385***	0.258***	−0.127***	−0.087
	(0.078)	(0.080)	(0.033)	(0.061)
$DtA_{pre} \times ZLB \times Size_5$	0.234**	−0.026	−0.260***	−0.165***
	(0.091)	(0.094)	(0.037)	(0.059)
ΔIQR effect ($Size_1$)	20.24%	18.49%	-1.02%	-4.97%
ΔIQR effect ($Size_2$)	14.01%	9.57%	-2.91%	-5.99%
ΔIQR effect ($Size_3$)	12.24%	9.47%	-1.86%	-2.94%
ΔIQR effect ($Size_4$)	12.11%	7.58%	-3.08%	-2.14%
ΔIQR effect ($Size_5$)	6.47%	-0.63%	-5.63%	-3.74%
Num. obs.	68 964	68 964	68 964	68 964
N. firms	10 533	10 533	10 533	10 533

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. All the $year \times Size$ regressors have been omitted from the table for brevity, but are included in the regression. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Size classes (in ihs) are defined as: Small $[5.29, 9.13]$, Medium-Small $(9.13, 9.82]$, Medium $(9.82, 10.5]$, Medium-Large $(10.5, 11.3]$, Large $(11.3, 16.4]$. ΔIQR effect is the marginal effect over the interquartile range of the DtA distribution. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

On the other hand, labor demand is absent for the smallest firms and significantly larger for the biggest ones. Overall, this heterogeneity analysis suggests that two main mechanisms we detected in aggregate (capital deepening and productivity improvements) are stronger for smaller firms, but it also reveals that electrification plays a role in accounting for firm heterogeneity.

5.2.2 The role of environmental protection investment

Theoretical considerations suggest that one of the key mechanisms linking financing constraints to emissions is investment in green technologies (Aghion et al., 2024), often embodied in new capital equipment aimed at reducing a firm's environmental footprint. To test the relevance of this channel, we draw on data from the *ANTIPOL* survey, which provides detailed information on firms' environmental protection investments, also focusing on tangible assets. As discussed in Section 4, the main limitation of combining *EACEI* and *ANTIPOL* data is that smaller firms are under-represented: they are rarely surveyed in both datasets in the same year, exacerbating the well-known sample biased toward large corporations in *EACEI* (Marin and Vona, 2021). To address this sample selection issue, we pursue two complementary strategies. First, we merge our balance sheet data only with *ANTIPOL* to estimate the effect of credit easing on green investment using a sample that only partially overlaps with our main dataset, but remains more balanced across firms of different sizes and thus allows us to test this channel for small and medium size firms. Second, we merge all three datasets – balance sheet, *EACEI*, and *ANTIPOL* – to focus on a smaller subsample of firms included in our main regressions. While this latter sample is biased toward large firms, it allows for a more direct comparison with our baseline results for larger corporations, which, we show, are not responsive to credit easing shocks.

ANTIPOL provides data on different types of green investments (see Appendix A). The most relevant for decarbonization are the so-called "integrated" investments, which introduce cleaner production processes or less carbon- and pollution-intensive machinery. We also examine the effect of the ZLB on "air-related" investments, which target specific abatement activities, such as filters or other end-of-pipe equipment to prevent air emissions. These investments target more local pollutants, but can bring substantial co-benefits in terms of decarbonization.

Because the *ANTIPOL* survey show that 88% firms do not undertake any environmental protection investment, we distinguish between the extensive- and the intensive-margin effect of the ZLB on green investment. For the extensive margin, we estimate a linear probability model to assess how credit easing affects the likelihood that a firm undertakes either an integrated or an air-related investment. For the intensive margin, we restrict the sample to firms reporting positive green investment, using as outcomes the share of integrated or air-related investment over total investment in tangible capital. In both case, we use the baseline model of Equation 7.

Panel A of Table 8 shows that credit easing did not significantly change the firm's propensity to invest in green technologies. We find no significant effects on the green extensive margin, except for a small

Table 8: The effect of credit easing on green investments.

Panel A. *Extensive margin*				
	ANTIPOL		*ANTIPOL + EACEI*	
	$Pr(I^{int} = 1)$	$Pr(I^{air} = 1)$	$Pr(I^{int} = 1)$	$Pr(I^{air} = 1)$
$DtA_{pre} \times ZLB$	−0.029	−0.042**	0.017	−0.033
	(0.018)	(0.018)	(0.032)	(0.033)
N. Obs.	81 867	81 867	38 474	38 474
N. Firms	12 303	12 303	6155	6155
Panel B. *Intensive margin*				
	ANTIPOL		*ANTIPOL + EACEI*	
	I^{int}/I^{tan}	I^{air}/I^{tan}	I^{int}/I^{tan}	I^{air}/I^{tan}
$DtA_{pre} \times ZLB$	0.044**	−0.005	0.025	0.018
	(0.021)	(0.013)	(0.029)	(0.020)
N. Obs.	9757	9757	5532	5532
N. Firms	2780	2780	1534	1534

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. $Pr(I^{int} = 1)$ and $Pr(I^{air} = 1)$ define the probability that a firm undertakes integrated green capital and air-specific investments. I^{int}/I^{tan}, I^{air}/I^{tan}, measure the ratio of investment in integrated green capital to tangible capital and investment in air-specific capital to tangible capital. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

negative effect on the probability of undertaking air-related investments in the full *ANTIPOL* sample (column 2). In line with the main results, column 3 and 4 suggest that large firms not previously engaged in environmental investments did not change their behaviour following the monetary easing. By contrast, Panel B indicates a significant positive effect on the share of integrated investments within the larger *ANTIPOL* sample (column 1), whereas the effect on air-related investments is statistically insignificant. These results imply that, among firms already investing in environmental protection, the ZLB induced a shift toward integrated machinery that are more directly related to reducing CO_2 emissions. Air-related investments, instead, target more local pollutants, explaining their weaker association with decarbonization induced by credit easing. Finally, when turning to the smaller *ANTIPOL + EACEI* merged sample (columns 3–4), coefficients remain positive but lose statistical significance, consistent with the bias toward larger firms for which, as shown earlier, the effects of credit easing are often null.

To give context to the analysis of the effect of the ZLB on green investment, we replicate the baseline decomposition analysis of Table 4 for the restricted sample of *ANTIPOL + EACEI* firms. Findings are highlighted in Appendix C (see Table C.3) and confirm our intuition on the effect brought about by the sample bias toward large firms. Although all coefficients present the same signs with respect to the main baseline regression (see Table 4), we underline that almost all of the coefficients become here statistically insignificant (except for the effect on E/VA). Once again, this result points toward the paramount importance of accounting for smaller companies when aiming at the identification of the firm-level effects of financial constraints.

5.3 Policy: financing constraints under the EU ETS framework

In this section, we examine how monetary policy interacts with carbon pricing. While financial constraints may tighten for regulated firms due to the higher compliance costs induced by the EU ETS, these firms are typically larger, more capital-intensive, and therefore less sensitive to fluctuations in credit supply. To test this hypothesis, we assess whether relaxing financial constraints affects carbon emissions differently for ETS and Non-ETS firms, thereby providing evidence on whether credit easing amplifies or dampens the effectiveness of the EU ETS.

As discussed in Section 2, this analysis requires constructing a balanced subsample, as the EU ETS policy targets firms and installations with high greenhouse gas emissions. We implement an N-to-one coarsened exact matching procedure (Iacus et al., 2012) to identify a control group of firms similar to ETS participants in terms of size, productivity, capital–labor ratio, industry classification, and baseline carbon emissions.[30]

Table 9: Estimates of the interaction between credit easing and environmental EU ETS policy using the subsample of treated and matched firms.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$ETS \times ZLB$	0.721	0.391	0.428	−0.037	0.330
	(0.905)	(0.899)	(0.619)	(0.432)	(0.248)
$DtA_{pre} \times ZLB$	−0.368	−0.817	−0.562	−0.255	0.450
	(1.195)	(1.140)	(0.930)	(0.469)	(0.290)
$DtA \times ZLB \times ETS$	0.033	0.172	−0.345	0.517	−0.139
	(1.443)	(1.417)	(1.112)	(0.641)	(0.421)
ΔIQR effect (matched)	−12.06%	−26.80%	−18.42%	−8.36%	14.73%
ΔIQR effect (treated)	−8.41%	−17.19%	−24.29%	−7.10%	8.78%
N. obs.	2323	2323	2323	2323	2323
N. firms	226	226	226	226	226

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 9 presents the results of this extension, as specified by Equation 10. The key finding is that the ZLB did not differentially affect emissions between ETS and matched Non-ETS firms. Although the coefficient on the triple interaction term for CO_2 is positive, it is small and statistically insignificant, indicating that easing financial constraints did not alter the behavior of regulated firms relative to compara-

[30] Details on the matching strategy are provided in Appendix D.

ble unregulated firms. The same pattern holds across all other components of the Kaya decomposition. While the limited size of the matched sample (about 2000 observations) may partly account for the lack of precision, the evidence is consistent with the view that both ETS and matched firms—being large, capital-intensive corporations—face relatively loose financing constraints. For such firms, the debt-to-asset ratio is a weaker measure of credit exposure, and the ZLB shock likely played a limited role in shaping their emissions trajectory.

Table 10: Estimates of the interaction between credit easing and environmental EU ETS policy using the separated samples of treated, matched, and unmatched firms.

	(1)	(2)	(3)	(4)	(5)
Panel A. *ETS treated firms*					
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−1.250	−1.620	−1.300	−0.320	0.371
	(1.478)	(1.546)	(1.152)	(0.613)	(0.411)
ΔIQR effect	−19.43%	−21.85%	−19.82%	−7.45%	12.22%
N. obs.	655	655	655	655	655
N. firms	51	51	51	51	51
Panel B. *Non-ETS matched firms*					
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.669	−0.863	−0.829	−0.035	0.194
	(1.352)	(1.314)	(1.058)	(0.554)	(0.295)
ΔIQR effect	−15.99%	−18.96%	−18.47%	−1.12%	7.02%
N. obs.	1562	1562	1562	1562	1562
N. firms	171	171	171	171	171
Panel C. *Non-ETS unmatched firms*					
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.443***	−0.501***	−0.262**	−0.239***	0.058
	(0.150)	(0.151)	(0.127)	(0.053)	(0.037)
ΔIQR effect	−9.31%	−10.25%	−5.99%	−5.53%	1.55%
N. obs.	66 600	66 600	66 600	66 600	66 600
N. firms	10 307	10 307	10 307	10 307	10 307

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To better highlight differences across firm subgroups, we re-estimate the baseline regression using three separate samples: ETS firms, Non-ETS matched firms, and Non-ETS unmatched firms. Results are presented in Table 10. Estimates for Non-ETS unmatched firms (Panel C) are fully consistent with the baseline regression. In contrast, the estimates are insignificant in Panel A and Panel B, corresponding to ETS firms and Non-ETS matched firms, respectively. Overall, these findings suggest that credit supply policies primarily affect firms outside existing carbon-pricing regimes, highlighting the potential complementarity between monetary easing and environmental regulation.

5.4 Robustness checks

In this section we aim at testing the robustness of our main results with respect to different modeling choices. In particular, we separate the modeling choices into five categories concerning (i) the firm-specific measure of exposure to the ZLB shock – because alternative balance sheet indicators might capture different dimensions masked by the DtA, such as the presence of private capital at risk; (ii) the measure that identifies a credible variation in the ECB monetary policy stance – because unconventional monetary policies might have had higher effects on the recapitalization of the banking sector and in turn on the credit supply; (iii) the estimation method – because estimates under the inverse hyperbolic sine transformation cannot be safely interpreted as marginal effects; (iv) the inclusion of potentially relevant omitted variables; (v) the appropriate exclusion of the fact that the main results are driven by trends that pre-date the exogenous variation in the monetary policy stance.

In what follows we briefly discuss the solutions to all these issues and the robustness of the results. All the regression tables for these exercises are outlined in Appendix E.

Alternative measures of the exposure to the monetary policy shock. As an alternative to the debt-to-asset ratio, we employ the debt-to-equity (DtE) ratio, which better accounts for the private capital at risk and allows us to test the robustness toward some measurement error in the exposure measure.

Appendix E.1 presents the estimates of the baseline regressions for the Kaya decomposition of CO_2 (Table E.1), for the decomposition with heterogeneity by size (Table E.2), as well as the regressions for the transmission mechanisms (Table E.3), and for the transmission mechanisms with size heterogeneity (Table E.4).

Although the magnitude of the coefficients and of the marginal effects varies from one specification to the other, we find that the direction and the significance of the coefficients are broadly consistent with the main results. Estimates using the DtE confirm that financially constrained firms have benefited most from the easier credit conditions brought about by the ZLB and have reduced CO_2 emissions.

Alternative monetary policy shocks and confounding macro factors. A potential limitation for our study is related to the measure of the credit easing event, as measured by the ZLB. As a matter of fact one argument could be that the equilibrium for the nominal interest rate between 2012 and 2019 was negative. In this case, setting an interest rate equal to zero would imply a positive gap between the actual and the equilibrium interest rate. This would imply that a zero-rate policy was not sufficiently expansionary to lead to the supply of credit expected in equilibrium. In the main text we have discussed at length the motivation behind our choice from a theoretical perspective. However, to overcome this possible limitation we have re-estimated the model with an alternative timing that captures the arrival of the unconventional monetary policy – namely the Asset Purchase Programme (APP) by the ECB. The APP was implemented under the governance of Mario Draghi starting from 2015.

Appendix E.2 presents the estimates for the baseline decomposition when using the APP in place of the ZLB (Table E.5). For the sake of completeness, we also report the decomposition conditional on firm size (Table E.6). All the main results are broadly confirmed.

Furthermore, to disambiguate the effect of the ZLB monetary policy shock from the one possibly arising from other macroeconomic factors, we also estimate two alternative models that control for continuous variation in two main macroeconomic variables. First, we account for yearly variation in the nominal interbank interest rate, which also overcomes a potential critique toward the use of a dummy variable for measuring the new monetary policy stance. Second, we account for yearly variation in GDP that could also affect the credit supply according to the business cycle conditions.

These two exercises on the baseline Kaya decomposition are presented in Tables E.7 and E.8 to disambiguate the monetary policy shock. Results show that the channel under scrutiny is well identified, as the size and significance of the coefficients are robust to the two alternative macroeconomic factors, which only play marginal roles.

Alternative estimator. In the main text we have discussed the recent contributions by Aihounton and Henningsen (2021), Cole et al. (2024) and Chen and Roth (2024) that cast doubts on the safe usage of the *ihs* transformation, in particular when interpreting the estimated coefficients as marginal effects. Chen and Roth (2024) suggest that one possible solution to understand whether the estimates are affected is to use Poisson or Negative Binomial regressions. We thus re-estimate our models using the Negative Binomial Quasi Maximum Likelihood Estimator (NB-QMLE) – or the Poisson estimator in few cases in which over-dispersion is not present – to preserve the zero-valued observations and to overcome the *ihs* limitations. In this case the emissions and the other variables have to be interpreted as truncated.

Appendix E.3 presents the estimates. Results are summarized in Tables E.9 and E.10 for the baseline Kaya decomposition without and with heterogeneity; and in Tables E.11 and E.12 for the mechanisms regressions without and with heterogeneity.

Also in this case, the results and the main conclusions are in line with the regression presented in the main text.

Alternative model specifications. An additional set of robustness tests carry out a battery of regressions to study whether the results are driven by specific years (i.e., the early 2000s), specific sectors (i.e., the energy-intensive ones) or by some omitted variables (i.e., initial differences in size, capital intensity or productivity).

Appendix E.4 presents the Kaya-type decomposition controlling for these concerns. First, Table E.13 excludes from the sample the first four years (i.e., the sample now covers the 2004-2019 period) which can be characterized by lower quality in the data collection. Results are robust to such a specification. Second, Table E.14 estimates the model considering only the 2-digit industries for which energy intensity exceeds the median. The sign of all coefficients is preserved, although the statistical significance holds only for the energy intensity, and value added. Third, Table E.15 presents the results when including also three continuous variables measuring (i) total sales, (ii) the capital-labor ratio, and (iii) the apparent labor productivity, as their absence might give rise to some form of omitted variable bias if they are correlated with the financial fragility of a company.[31] We highlight that the main effect of interest is barely affected by these inclusions, indicating that concerns about omitted-variable bias (OVB) are minor.

6 Quantification of aggregate carbon savings

We use our estimates to compute the aggregate amount of carbon emissions that has been saved thanks to the credit easing induced by the ZLB. In doing so, we follow a partial equilibrium approach, focusing on the specification that allows for heterogeneous effects by firm size and therefore relying on the estimates reported in Table 5. This choice is motivated by the fact that the coefficients for large firms are statistically insignificant, and computing the aggregate policy effect using the coefficient for the baseline model would lead to an overestimation of the effect of the credit easing.

On the one side, one might argue that the credit easing might increase aggregate demand, raising the equilibrium price of capital goods, intermediate inputs, and energy. This potentially attenuates the firm-level expansionary effects, indicating that we are overestimating the effect. On the other side, credit easing may benefit constrained firms relative to unconstrained ones altering relative wages and return to capital. If the expanding firms are also more environmentally friendly, their expansion may crowd out dirtier ones, implying that our estimates might represent lower bounds of the true effect. Our exercise does not aim at modeling all the general equilibrium feedbacks, but can be interpreted as a simple back-

[31] We here include sales as a continuous variable rather than transforming it into a discrete variable, as defined by quintiles, because this was already done in all the heterogeneity analysis throughout the paper.

of-the-envelope calculation of the aggregate savings, all else equal.

We compute the aggregate emissions savings (ΔCO_2) as:

$$\Delta CO_2 = -\sum_{i \in k} \hat{\beta}_k DtA_{i,pre} CO_{2i,pre} \tag{11}$$

where $DtA_{i,pre}$ and $CO_{2i,pre}$ represent the average debt-to-asset ratio and carbon emissions of firm i before the ZLB monetary policy shock, β_k is the coefficient associated with the effect of the ZLB for class size k (as reported in Table 5). The summation across all firms allows us to estimate the annual average of the carbon emissions saved thanks to the credit easing policy. Following Colmer et al. (2024), we set $\beta_4 = \beta_5 = 0$ as they are non-significant in the regression accounting for size quintiles. This is tantamount to considering large firms as completely unaffected by the credit easing. Furthermore, we cap the maximum emission reduction to 100% of the pre-ZLB emissions, since the estimated effect can be larger for particularly exposed firms.[32]

Our estimates suggest that, absent the ZLB, aggregate carbon emissions would have been about 5.3 million tonnes higher per year. In absolute terms, this figure is strikingly close to the 5.4 million tonnes of annual abatement attributed to the EU ETS by Colmer et al. (2024). The comparison, however, highlights important differences. First, the effect of the EU ETS is an order of magnitude larger than that of the ZLB (47% versus 3.3%). Second, the EU ETS targets a relatively small group of large industrial emitters, whereas the ZLB-induced credit expansion was untargeted and affected the entire manufacturing sector, albeit our estimates suggest that SMEs were the most responsive to the shock.

Counterfactual policy exercise. Using the estimated emissions savings from the ZLB, we then conduct a simple cost–benefit exercise. Also in this case, our approach is deliberately simple, as we value the avoided emissions using plausible values of the social cost of carbon (SCC) – the monetary value of the damage of one tonne of CO_2. To make the policy exercise more compelling and put ourselves in the worst case scenario for public finances, we also assume that the interest rate cut is tantamount to a subsidy or a tax credit on the firms' interest payments.[33]

Formally, we compute the net benefit of such a policy as a function of the social cost of carbon (SCC)

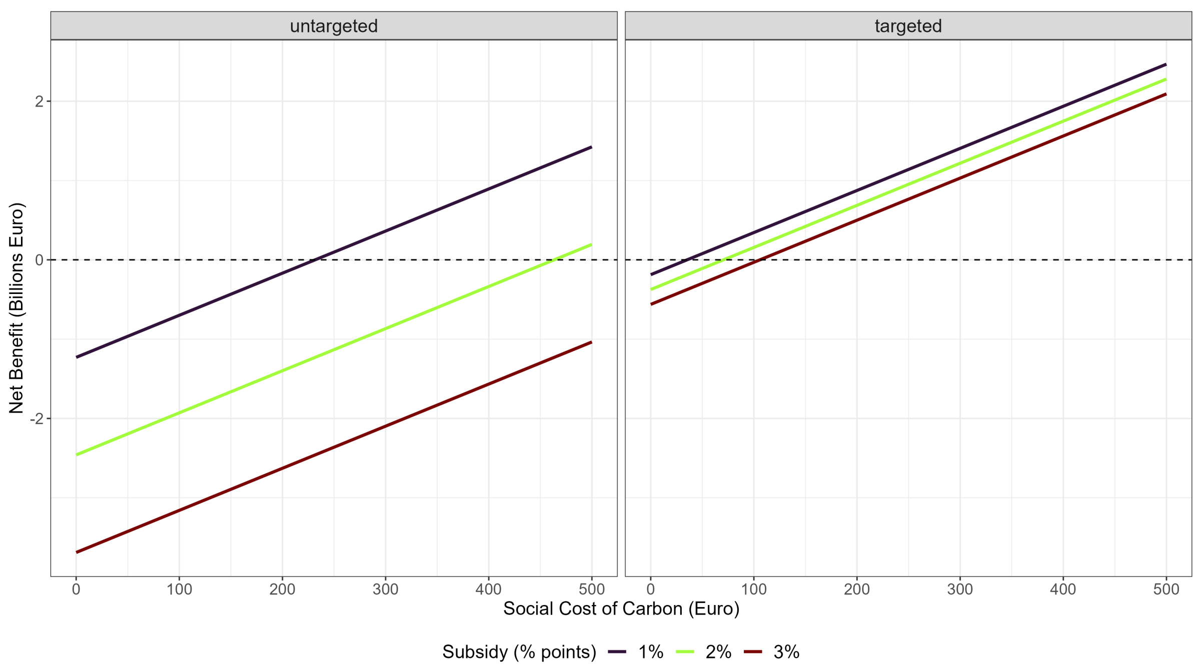
[32] Only less than 100 companies display this behaviour.
[33] This is equivalent to assume back that monetary policy has no effect on the real economy.

and the interest rate subsidized by the government (r) as follows:

$$\text{Net Benefit}(SCC, r) = \sum_i SCC \times \Delta CO_{2i} - r \times Debt_{i,pre} \tag{12}$$

where ΔCO_{2i} measures the firm level amount of carbon saved as estimated in Equation 11, while $Debt_{i,pre}$ is the average debt level of a firm before the ZLB. Because the social cost of carbon has a wide range of plausible values (Anthoff and Tol, 2013; Lemoine, 2021; Tol, 2023), we compute net benefits as a function of $SCC \in [0, 500]$, as well as of the free policy variable $r \in \{0.01, 0.02, 0.03\}$ under two alternative scenarios.[34] In the *untargeted* scenario, all firms receive a subsidy proportional to their pre-ZLB debt, while in the *targeted* case, the subsidy is restricted to the three smallest firm groups – i.e. those with statistically significant β_j coefficients in Table 5. As expected, the targeted design delivers substantially higher efficiency gains.

Figure 3: Net benefit in the counterfactual policy scenario.



Notes: Computations are based on $SCC \in [0, 500]$ and $r \in \{0.01, 0.02, 0.03\}$ for both the untargeted (left) and targeted (right) scenarios.

Under the untargeted scenario (Figure 3, left), debt subsidies yield a positive net benefit only if the intervention is modest ($r \leq 2\%$) and the social cost of carbon is high ($SCC \geq 200$). In practice, this implies that such a policy would be viable only when paired with strong carbon pricing. By contrast, targeting the policy (Figure 3, right) to smaller firms substantially cuts the policy costs and delivers positive net

[34] Plausible values are above €50/t.

benefits even with lower carbon prices (e.g. $SCC \geq 50$ at $r = 1\%$). The gain arises from avoiding transfers to large firms that, according to our estimates, would not reduce emissions in response to debt relief.

Although stylized, this exercise suggests that the coordination of green policies with credit supply measures could enhance effectiveness toward the green transition.

7 Conclusions

This paper studies the channels through which monetary policy shocks can affect firm-level environmental performance and green investment. As a source of plausibly exogenous variation we exploit the the ECB's entry at the Zero Lower Bound (ZLB) in July 2012, in combination with rich administrative and survey data on French manufacturing firms from 2000 to 2019.

Our findings underscore the critical role of the monetary policy in advancing the low-carbon transition. First, easing credit constraints reduces firm-level CO_2 emissions by roughly 9.41% more for financially constrained firms (75th percentile of initial DtA) compared to less constrained firms (25th percentile). This net effect arises from two contrasting forces: a modest increase of 2% in value added (the size effect) is more than offset by 6.31% and 5.65% declines in the carbon content of energy and energy intensity respectively (the technique effect). The technique effect primarily reflects shifts in input mix – capital deepening combined with reductions in labor and energy use – and efficiency gains (e.g., in TFP), with only modest increases in investment in environmental protection or electrification. Second, and more remarkable, small and medium-sized firms benefit the most from relaxed credit constraints, achieving significantly larger improvements in both environmental and economic performance than the average firm – for which we do not observe any induced decarbonization. For these firms, an interquartile increase in DtA corresponds to an 18.08% reduction in CO_2 emissions. This effect is driven by stronger capital deepening and general efficiency gains. We also observe an increased electricity usage that is accompanied by stable employment levels. This suggests that two main mechanisms we detect in aggregate (capital deepening and productivity improvements) are stronger for smaller firms, but it also reveals that electrification plays a role in accounting for firm heterogeneity. Third, easing financial constraints does not significantly affect firms regulated under the EU Emission Trading Scheme (EU ETS) relative to a matched control group. This aligns with the larger size and capital intensity of ETS firms, which renders them less sensitive to credit supply shocks. Finally, a back-of-the-envelope calculation based on the aggregation of our estimates, implies that about 5.3 million tonnes of CO_2 were saved in the manufacturing sector thanks to the relaxation of the credit constraints associated with the ZLB. This corresponds to an average decline in emissions of 3.2% per year between 2012 and 2019.

Overall, our findings underscore the importance of credit conditions for the low-carbon transition. Monetary policy, while not specifically designed for climate goals, can have significant environmental consequences on the green transition of small and more vulnerable firms. More broadly, our results point to the presence of complementarities between climate and financial policies. If carbon pricing shapes the firms' incentives, monetary policy – by affecting the availability of credit – determines the ability of firms to respond to the incentives. Coordinating the two policies may be critical for accelerating the decarbonization of the manufacturing sector.

Acknowledgments. Data access was provided by CASD and authorized by the French comité du secret statistique, project GreenFiT. We thank Natalia Fabra, Federico Frattini, Marzio Galeotti and Francesco Lamperti for useful comments and suggestions. We thank participants to the 2023 *IAERE* in Naples, the 2024 *IWEE* in Bolzano, the *XII International Academic Symposium for Accelerating the Net-Zero Economy Transformation* in Barcelona, the 2024 *EEA-ESEM congress* in Rotterdam, the 2024 Bank of Italy *"Workshop on the economic implications of climate change and the transition to a low-carbon economy"*, a *LSE Grantham seminar*, a *FEEM internal seminar*, and a *Sant'Anna School seminar*. The usual disclaimers apply.

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Appendix A Further details on the sample construction

The main source of data is the *EACEI* survey. *EACEI* (Enquête sur les consommations d'énergie dans l'industrie) is a survey of manufacturing establishments that provides information on energy consumption (quantity and value) broken down by energy type: electricity (consumed and autoproduced), steam, natural gas, other types of gas, coal, lignite, coke, propane, butane, heavy fuel oil, heating oil and other petroleum products. In the first part of our period (2000-2010), sectors 10-12 (Manufacture of food products, beverages and tobacco products, NACE Rev 2) were not included in the survey design, while the sector 19 (Manufacture of coke and refined petroleum products) is only included from year 2002 onwards. From 2007 onwards, other nonmanufacturing industrial sectors were included (e.g., 38.3 'Material recovery').[35] All establishments with more than 250 employees are requested to participate to the survey, while only a sample of establishments with 20 or more employees (stratified by nomenclature of activities (NTE) dedicated to energy consumption, workforce bands, and region) are interviewed.[36] The response rate is always very high, near to 90%.

EACEI is available for a sample manufacturing establishments. The unit of reference is here the SIRET, a 11 digits unique identifier of the establishment where the first 9 digits represent the firm SIREN code. Because we focus on the firm, rather than on the establishment, the initial cleaning process over the period 2000-2019 is aimed at just considering enterprises that are well represented in *EACEI* and involves combining at the establishment level *EACEI* with the DADS-Etablissements database, from which we retrieve data about all the employees at each establishment and each firm. This is a crucial step (also see Marin and Vona, 2021) as it allows us to understand the extent to which establishments surveyed in *EACEI* are exhaustive in describing energy use of the enterprise as a whole. As a matter of fact there are multi-establishment firms for which only a subset of all establishments have been surveyed in *EACEI*. We wipe out from our sample all the firms for which the employment jointly surveyed in *EACEI* does not reach at least 90% of the total firm level employment. Otherwise, we would associate to the firm an amount of emissions which is only a small fraction of its total emissions. After cleaning and aggregating the database at the firm level, we are left with a sample of 106 113 observations for 23 964 different establishments, belonging to 20 470 distinct firms. The *EACEI* survey provides this (on cogeneration) information, allowing us to use a dummy variable to assess whether financially constrained firms became more likely to self-produce electricity after the shock.

Balance sheet information for French firms was retrieved from the *FICUS* (Statistique structurelle annuelle d'entreprises issue du dispositif SUSE, for the years between 2000-2007) and *FARE* (Statistique structurelle annuelle d'entreprises issue du dispositif ESANE, for the years between 2008-2019) database, which contain information on balance sheets and income statements for the universe of non-agricultural and non-financial companies in France. For this data source, we restrict our sample to manufacturing firms in territorial France (excluding the overseas

[35]Other relatively minor sectors were not included in the survey design of *EACEI* (classification NAF Rev. 2): 16.10A, 16.10B, 20.13A, 24.46Z.

[36]All establishments with 20 employees or more are surveyed for sectors 23.32Z, 23.51Z and 23.52Z (NAF Rev. 2 classification), while all establishments with 10 or more employees are interviewed for sector 20.11Z (NAF Rev. 2 classification).

departments and territories) for which there is at least 1 employee and for which the sectoral code is non missing. In this database, the unit of reference is the SIREN code, a 9 digits unique identifier of the firm, defined as the legal entity that is subject to the corporate tax. This sample consists in 2 519 946 observations for 339 600 distinct firms over the period 2000-2019.

The *FICUS/FARE* and the *EACEI* database are then merged together based on the unique identifiers of French firms (SIREN), with the latter database imposing the sample limit. After collapsing the establishment-year observations at the firm-year level – by summing environmental as well as economic variables over the establishments controlled by the same parent company – we end up with 95 870 firm-year observations for the same 20 470 unique firms.

We then proceed with the usual cleaning steps. We deflate all nominal balance sheet variables using sector specific deflators provided from the INSEE. We drop companies with missing data on key variables or negative and infinite debt-to-asset (DtA), debt-to-equity (DtE), interest coverage (IC), and gross operating profit (GOP) ratios. We also clean negative value added observations and we then remove outliers at the top 1% of the DtA, DtE ratios, and at the top and bottom 1% of the IC and GOP ratios. To estimate productivity, we also remove firms for which data on sales, employment, capital, and investment are missing. The cleaning process leads us to the estimation sample comprising 73 295 firm-year observations for the same 13 563 unique firms.

We then carry out the matching strategy with the *EUTL* data and we drop from our sample the companies that have been covered by the EU ETS regulation after the very first wave, before 2005. This step allows us to purge from our sample a small number of companies that could have been hit by two different shocks at the same time (i.e., ZLB credit easing and EU ETS policy), allowing us to better capture the effect of the ZLB without mixing it with other disturbances.[37]

We are left with an estimation sample of 71 853 observations related to 13 432 firms, and approximately 99% of these companies belong to the C3 (electrical, electronic, IT equipment and machinery), C4 (transport equipment), C5 (other industrial products).[38]

For the regressions related to environmental protection investments we also leverage upon the *ANTIPOL* (Enquête sur les études et les investissements pour protéger l'environnement) database, collected by INSEE. In particular, we use integrated investments, defined as investments in *"production tools to make them better performing in environmental terms than other equipment with similar functions and characteristics. This category includes equipment that generates less pollution compared to other tools available on the market."* as well as air investment, which is a specific subcateogiry of these more general integrated investments (see also Colmer et al. (2024)). The main issue of merging together *EACEI* with *FICUS/FARE* and then also with *ANTIPOL* is related to the fact that the samples of establishments surveyed by *ANTIPOL* and *EACEI* are not the same. Once the two surveys are merged, we are therefore left with a much smaller sample of about 30 thousands observations, with a significant bias toward larger companies.

[37] This step removes 131 unique firms, for a total of 1442 firm-year observations.

[38] Labels are drawn from the A17 sectors under the NAF Rev.2 industrial classification.

For sake of comparison, in the original sample the frequencies by size class are $\{13336; 14298; 14756; 15143; 14320\}$ respectively for $\{Small, Medium - Small, Medium, Medium - Large, Large\}$, naturally reflecting the fact that size classes have been constructed from the quintiles of the sales distribution before some cleaning steps, and that smaller firms are more likely to present some missing observation. However, after merging with *ANTIPOL*, the sub-sample displays the following frequencies: $\{3289; 5438; 8406; 11162; 12656\}$ with a clear sample-size bias toward large firms. Some of the observations are singletons and are wiped out by fixed-effects in the regressions that follow.

For this reason, to avoid such a data limitation and to retrieve the lost statistical power, the regressions related to environmental protection investments (See Section 5.2.2) leverage on a slightly different sample, constructed from the merge of *FICUS/FARE* with *ANTIPOL* only.

Appendix B Additional descriptive statistics

In this section we report some additional descriptive statistics for the main variables of interest, conditional upon the debt-to-asset ratio and upon their ETS status, that is defined according to the matching strategy detailed in Section 3.2 and Appendix D.

Table B.2 highlights the fact the three alternative measures of financial fragility satisfy the expected correlations, with the two measures of indebtedness (DtA and DtE) being positively correlated. On top of that, we record that financially fragile companies are on average smaller and pollute less, apparently because of a greener energy mix which can be inferred from the $CO2/E$ ratio.

Moving to Table B.3 we discover that ETS firms are different from all the other firms (also from the matched ones) in terms of carbon emissions. This is somehow expected, because the ETS policy precisely targets the high-polluting firms. However, we find that the matching allows us to minimize the differences between matched companies and treated ones as their indebtedness, size and productivity are very similar – more attention to the matching quality is given in Appendix D. Unmatched companies are smaller, generate less emissions, are endowed with a greener energy mix, and display lower energy intensity.

Table B.1: Summary statistics of mechanism variables, depending on size class.

Size class	$\%Ele.$	$Selfprod.$	ALP	TFP	K/L	K	L	E
All	0.5342	1.0132	18879	1.8439	82.0736	0.0154	12558	71853
	(0.2251)	(1.1423)	(225200)	(72.6528)	(54.0475)	(0.3813)	(53653)	
Small	0.5792	0.0153	35.9637	3.6192	18.8012	670.7546	41.6303	9.5339
	(0.2817)	(0.1229)	(34.3082)	(0.4511)	(35.1827)	(894.0806)	(23.8090)	(32)
Medium-Small	0.6018	0.0155	43.6576	3.7555	26.8677	1501.7830	66.6353	19.7583
	(0.2806)	(0.1237)	(50.7933)	(0.4386)	(66.6548)	(1652.8289)	(36.6054)	(41)
Medium	0.5851	0.0272	50.7829	3.8382	34.5151	2988.4226	109.9957	39.6195
	(0.2662)	(0.1626)	(104.3339)	(0.4599)	(112.6533)	(3192.0616)	(71.1195)	(76)
Medium-Large	0.5717	0.0485	58.5735	3.9598	43.9766	6305.9217	182.7140	91.9424
	(0.2614)	(0.2147)	(132.3369)	(0.4829)	(164.7392)	(6509.9058)	(105.9547)	(195)
Large	0.5514	0.1205	81.1464	4.1531	64.5491	32381.3285	623.8079	711.7234
	(0.2507)	(0.3256)	(370.6859)	(0.5440)	(245.5598)	(81403.4957)	(1237.7563)	(2960)

Notes: For each size class, the first row reports the mean while the second row the standard deviation, in parentheses. Size classes (in ihs) are defined as: Small $[5.29, 9.13]$, Medium-Small $(9.13, 9.82]$, Medium $(9.82, 10.5]$, Medium-Large $(10.5, 11.3]$, Large $(11.3, 16.4]$. $CO2$ is measured in kilotonnes; $CO2/E$ in kilotonnes to kilowatts; E/VA in in kilowatts to Mln €; VA in Mln €.

Table B.2: Summary statistics of key variables, depending on DtA class.

DtA class	DtA	DtE	$CO2$	$CO2/VA$	$CO2/E$	E/VA	VA
All	0.5342	1.0132	18879	1.8439	82.0736	0.0154	12558
	(0.2251)	(1.1423)	(225200)	(72.6528)	(54.0475)	(0.3813)	(53653)
Low	0.2949	0.3919	28462.8034	2.9107	89.6059	0.0197	20492.8221
	(0.1230)	(0.3690)	(272164)	(160.7808)	(52.9335)	(0.8299)	(95350)
Medium-Low	0.4337	0.6397	21652.5918	1.4230	83.8763	0.0125	12053.2349
	(0.1240)	(0.5453)	(229206)	(7.2105)	(53.5636)	(0.0975)	(27513)
Medium	0.5296	0.8663	13917.5040	1.4468	80.7424	0.0128	11224.0413
	(0.1326)	(0.6694)	(108961)	(9.2304)	(54.0330)	(0.0569)	(23597)
Medium-High	0.6249	1.1380	34461.4929	2.0014	79.5588	0.0176	11436.8987
	(0.1423)	(0.8505)	(385408)	(20.4831)	(54.8922)	(0.1643)	(58957)
High	0.7979	2.0656	10216.8819	1.7488	76.1832	0.0174	7370.9743
	(0.2059)	(1.8225)	(58844)	(8.6905)	(52.7987)	(0.0600)	(19059)

Notes: For each size class, the first row reports the mean while the second row the standard deviation, in parentheses. Size classes (in ihs) are defined as: Small $[5.29, 9.13]$, Medium-Small $(9.13, 9.82]$, Medium $(9.82, 10.5]$, Medium-Large $(10.5, 11.3]$, Large $(11.3, 16.4]$. $CO2$ is measured in kilotonnes; $CO2/E$ in kilotonnes to kilowatts; E/VA in in kilowatts to Mln €; VA in Mln €.

Table B.3: Summary statistics of key variables, depending on treatment class.

Treatment	DtA	DtE	CO_2	CO_2/VA	CO_2/E	E/VA	VA
All	0.5342	1.0132	18879	1.8439	82.0736	0.0154	12558
	(0.2251)	(1.1423)	(225200)	(72.6528)	(54.0475)	(0.3813)	(53653)
ETS	0.5017	1.0110	469750	17.7887	120.9603	0.1191	78040
	(0.2166)	(1.1818)	(1326739)	(50.9567)	(62.2008)	(0.3011)	(265352)
matched	0.5041	0.9276	175036	8.3656	116.7171	0.0626	44278
	(0.2202)	(0.9697)	(08596)	(19.2930)	(48.8523)	(0.1411)	(163427)
unmatched	0.5353	1.0143	6932	1.4085	80.7513	0.0125	10705
	(0.2254)	(1.1434)	(0.1	(73.4489)	(53.4402)	(0.3847)	(31209)

Notes: For each treatment class, the first row reports the mean while the second row the standard deviation, in parentheses. $CO2$ is measured in kilotonnes; $CO2/E$ in kilotonnes to kilowatts; E/VA in in kilowatts to Mln €; VA in Mln €.

Appendix C Complementary Results

C.1 Additional controls for pre-trends

Table C.1: Estimates of the baseline regression model including pre-trend controls.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.254	−0.290	−0.082	−0.211***	0.009
	(0.180)	(0.183)	(0.153)	(0.063)	(0.043)
ΔIQR effect	−5.89%	−6.62%	−2.05%	−5.00%	0.25%
N. obs.	50 808	50 808	50 808	50 808	50 808
N. firms	5884	5884	5884	5884	5884

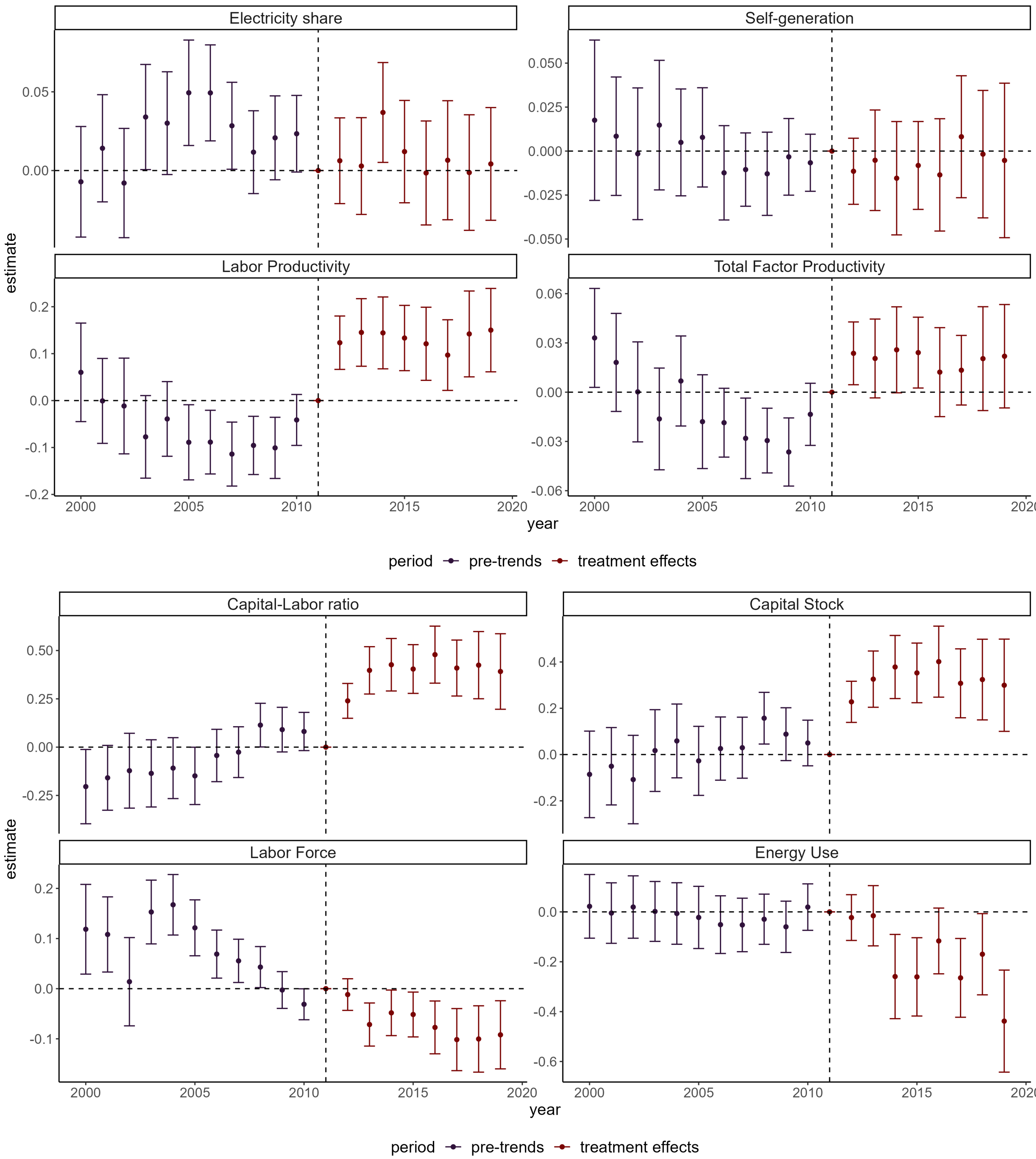
Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table C.2: Estimates of the baseline regression model depending on size class and including pre-trend controls.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB \times Size_1$	−0.734***	−0.834***	−0.504**	−0.340***	0.079*
	(0.266)	(0.270)	(0.222)	(0.097)	(0.048)
$DtA_{pre} \times ZLB \times Size_2$	−0.431*	−0.556**	−0.177	−0.386***	0.099**
	(0.250)	(0.251)	(0.211)	(0.083)	(0.044)
$DtA_{pre} \times ZLB \times Size_3$	−0.205	−0.222	−0.079	−0.141*	−0.013
	(0.232)	(0.236)	(0.202)	(0.078)	(0.051)
$DtA_{pre} \times ZLB \times Size_4$	−0.031	−0.107	0.024	−0.134*	0.033
	(0.203)	(0.202)	(0.173)	(0.069)	(0.049)
$DtA_{pre} \times ZLB \times Size_5$	−0.030	0.066	0.181	−0.113*	−0.111**
	(0.182)	(0.185)	(0.156)	(0.068)	(0.052)
ΔIQR effect ($Size_1$)	−14.66%	−15.96%	−11.16%	−8.13%	2.33%
ΔIQR effect ($Size_2$)	−9.32%	−11.36%	−4.32%	−8.52%	2.77%
ΔIQR effect ($Size_3$)	−4.62%	−4.96%	−1.90%	−3.28%	−0.32%
ΔIQR effect ($Size_4$)	−0.80%	−2.63%	0.63%	−3.23%	0.87%
ΔIQR effect ($Size_5$)	−0.72%	1.69%	4.88%	−2.62%	−2.50%
N. obs.	50 808	50 808	50 808	50 808	50 808
N. firms	5884	5884	5884	5884	5884

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. The $year \times Size_5$ regressor is omitted because of perfect multi-collinearity and is the reference category. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Figure C.1: Mechanisms results of the effects of credit easing on main outcome variables when using an event study approach.



Notes: Coefficients are normalized to zero in 2011, the last year before the ZLB.

Table C.3: Baseline results on the ANTIPOL subsample.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.241	−0.273	−0.113	−0.160**	0.032
	(0.188)	(0.188)	(0.160)	(0.065)	(0.051)
ΔIQR effect	−5.52%	−6.16%	−2.75%	−3.81%	0.83%
N. obs.	39 100	39 100	39 100	39 100	39 100
N. firms	6262	6262	6262	6262	6262

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Appendix D Policy interaction: the EU Emission Trading Scheme

D.1 Institutional background on the EU ETS

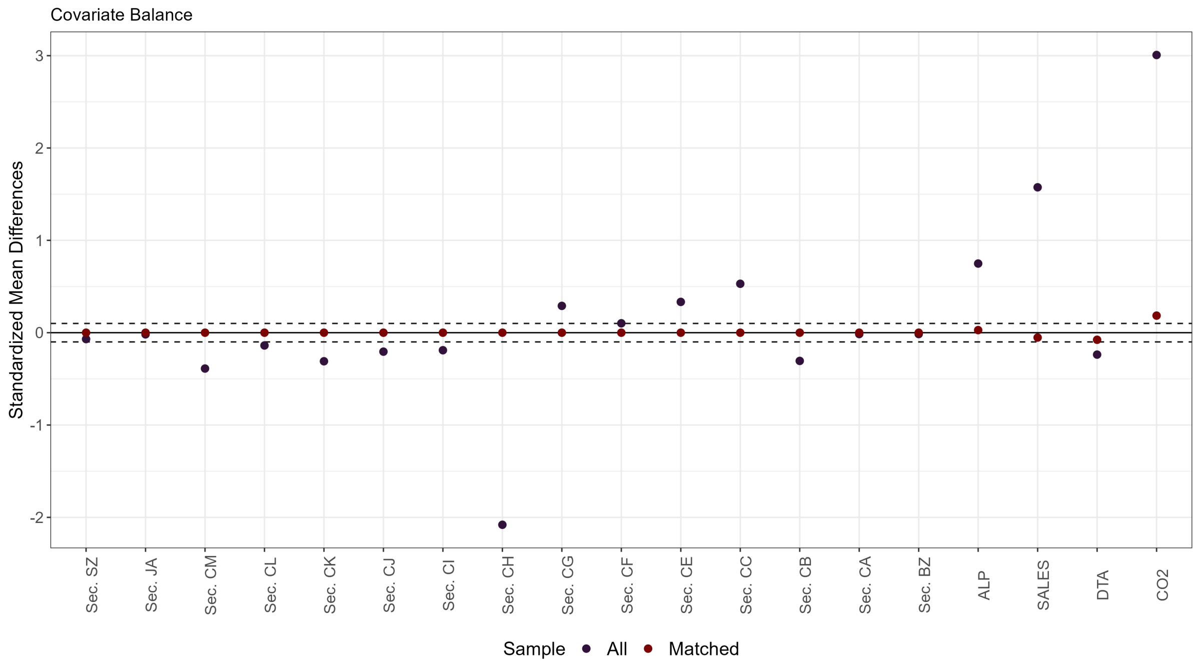
Starting from 2005 (the 1^{st} trading phase of the EU ETS) all the ETS treated companies have been assigned a certain amount of emission allowances by the regulatory authority. All firms whose CO_2 emissions overcome the initial endowment must buy the additional allowances on the ETS market or, alternatively, pay a fee. The market price for carbon emissions have averaged at around \$21 in the second phase of the ETS. This corresponds to an increase in production costs for ETS companies. Therefore, the EU ETS provides the regulated companies with the incentives to abate their emissions vis-à-vis unregulated firms. A-priori, we expect that companies which are regulated by the ETS and that are more financially constrained could enjoy more benefits from the credit easing. On the other side, we also acknowledge that treated firms are typically very large companies and financial constraints might be a minor issue since they have many physical asset that they can pledge as collateral and borrow even when their debt-to-asset ratio is already large.

D.2 Matching

On top to the general identification problems that we have already discussed in Section 3.1, the main challenge for the identification of the effect of financial constraints easing under ETS resides in the fact that the ETS companies are not a random subsample of the population. The EU ETS framework targets the most polluting firm, which are on average larger, less financially fragile, more productive, and more capital intensive (see the descriptive statistics in Table B.3).

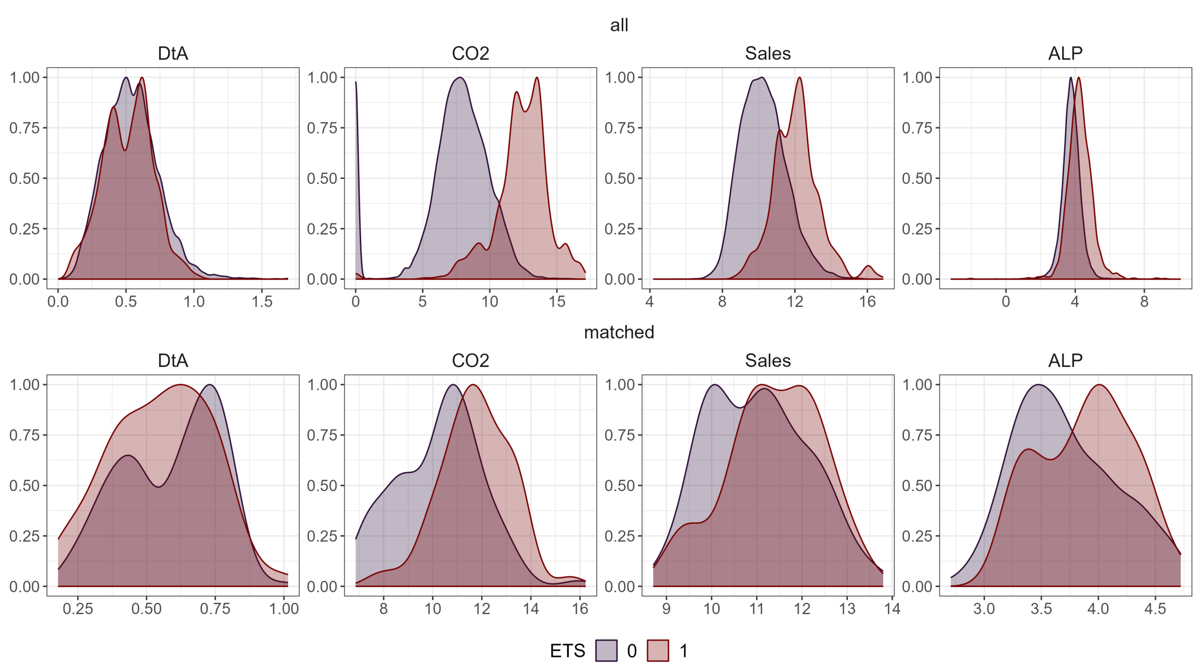
Coarsened Exact Matching Standard matching estimators based on the propensity scores (such as the nearest neighbour) have been recently exposed to critiques (Iacus et al., 2012, see). Hence, we employ the coarsened exact matching (CEM) algorithm proposed by Iacus et al. (2012). This is an N-to-one non-parametric and model-free matching strategy, simply based on a multidimensional binning of the observations. Figure D.1 shows that the CEM matching provides a good balance among the covariates of the treated and matched companies.

Figure D.1: Matching quality with the coarsened exact matching.



Notes: Blue dots: standardized mean differences between treated and all the untreated companies. Red dots: standardized mean differences between treated and matched companies. DtA: debt-to-asset; $CO2$: carbon emissions; $Sales$: firm sales; ALP: apparent labor productivity. All the other variables represent sector specific dummies and they are exactly matched.

Figure D.2: Distributions before and after matching with the coarsened exact matching.



Notes: Top panels: distributions of treated (red) and all untreated (blue) companies. Bottom panels: distributions of treated (red) and matched (blue) companies DtA: debt-to-asset; $CO2$: carbon emissions; $Sales$: firm sales; ALP: apparent labor productivity. All the other variables represent sector specific dummies and they are exactly matched.

Appendix E Robustness checks

E.1 Alternative exposure to the monetary policy shock

Table E.1: Estimates of the baseline regression model when financial fragility is measured by the debt-to-equity ratio.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtE_{pre} \times ZLB$	−0.094***	−0.108***	−0.070**	−0.038***	0.014
	(0.035)	(0.035)	(0.029)	(0.013)	(0.009)
ΔIQR effect	−5.58%	−6.39%	−4.21%	−2.34%	0.90%

Notes: DtE_{pre} is the debt-to-equity ratio before the policy change. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.2: Estimates of the baseline regression model when financial fragility is measured by the debt-to-equity ratio and when accounting for firm heterogeneity as measured by the quintiles of the size distributions.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtE_{pre} \times ZLB \times Size_1$	−0.171***	−0.213***	−0.151***	−0.062***	0.042***
	(0.048)	(0.049)	(0.037)	(0.020)	(0.014)
$DtE_{pre} \times ZLB \times Size_2$	−0.119*	−0.148**	−0.077	−0.071***	0.029**
	(0.072)	(0.073)	(0.057)	(0.027)	(0.012)
$DtE_{pre} \times ZLB \times Size_3$	−0.117	−0.130*	−0.105	−0.026	0.013
	(0.077)	(0.077)	(0.069)	(0.023)	(0.016)
$DtE_{pre} \times ZLB \times Size_4$	0.026	0.015	0.036	−0.021	0.012
	(0.050)	(0.053)	(0.044)	(0.020)	(0.015)
$DtE_{pre} \times ZLB \times Size_5$	0.036	0.091	0.065	0.026	−0.055***
	(0.059)	(0.060)	(0.052)	(0.020)	(0.017)
ΔIQR effect ($Size_1$)	−11.85%	−14.45%	−10.58%	−4.50%	3.21%
ΔIQR effect ($Size_2$)	−6.98%	−8.54%	−4.61%	−4.25%	1.81%
ΔIQR effect ($Size_3$)	−6.12%	−6.77%	−5.51%	−1.41%	0.75%
ΔIQR effect ($Size_4$)	1.54%	0.84%	2.08%	−1.20%	0.69%
ΔIQR effect ($Size_5$)	2.24%	5.81%	4.09%	1.61%	−3.26%

Notes: DtE_{pre} is the debt-to-equity ratio before the policy change. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.3: Mechanisms results of the effects of credit easing on main outcome variables when the exposure to ZLB is measured by the debt-to-equity ratio.

Panel A.	**Dep. Variable** (last two ihs-transformed):			
	(1)	(2)	(3)	(4)
	$\%Elect.$	$Selfgen.$	ALP	TFP
$DtE_{pre} \times ZLB$	0.003	0.000	0.041***	0.008***
	(0.003)	(0.003)	(0.007)	(0.002)
ΔIQR effect	0.17%	−0.02%	2.55%	0.49%
N. obs.	67 925	67 929	68 964	68 964
N. firms	10 465	10 463	10 533	10 533
Panel B.	**Dep. Variable** (all ihs-transformed):			
	(1)	(2)	(3)	(4)
	K/L	K	L	E
$DtE_{pre} \times ZLB$	0.067***	0.040**	−0.027***	−0.024**
	(0.016)	(0.017)	(0.006)	(0.012)
ΔIQR effect	4.32%	2.57%	−1.64%	−1.47%
N. obs.	68 964	68 964	68 964	68 964
N. firms	10 533	10 533	10 533	10 533

Notes: DtE_{pre} is the debt-to-equity ratio before the ZLB. $\%Elect.$ and $Selfgen.$ are respectively the share of electricity usage and a dummy variable equal to one for firms installing cogeneration or solar panels technologies. They have fewer observations because of missing answers about these specific questions. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.4: Mechanisms results of the effects of credit easing on main outcome variables conditional on size class when the exposure to ZLB is measured by the debt-to-equity ratio.

Panel A.	**Dep. Variable** (last two ihs-transformed):			
	(1)	(2)	(3)	(4)
	$\%Elect.$	$Selfgen.$	ALP	TFP
$DtE_{pre} \times ZLB \times Size_1$	0.008*	0.000	0.043***	0.009**
	(0.005)	(0.002)	(0.011)	(0.004)
$DtE_{pre} \times ZLB \times Size_2$	0.004	−0.002	0.056***	0.013***
	(0.004)	(0.002)	(0.012)	(0.003)
$DtE_{pre} \times ZLB \times Size_3$	0.004	−0.003	0.030**	0.004
	(0.005)	(0.003)	(0.012)	(0.004)
$DtE_{pre} \times ZLB \times Size_4$	0.003	0.003	0.042***	0.007*
	(0.004)	(0.006)	(0.011)	(0.004)
$DtE_{pre} \times ZLB \times Size_5$	−0.010**	−0.001	0.026*	0.003
	(0.005)	(0.007)	(0.016)	(0.004)
ΔIQR effect ($Size_1$)	0.21%	0.00%	1.24%	0.27%
ΔIQR effect ($Size_2$)	0.10%	−0.06%	1.53%	0.34%
ΔIQR effect ($Size_3$)	0.09%	−0.09%	0.77%	0.10%
ΔIQR effect ($Size_4$)	0.07%	0.09%	1.12%	0.18%
ΔIQR effect ($Size_5$)	−0.25%	−0.02%	0.65%	0.08%
Panel B.	**Dep. Variable** (all ihs-transformed):			
	(1)	(2)	(3)	(4)
	K/L	K	L	E
$DtE_{pre} \times ZLB \times Size_1$	0.089***	0.088***	−0.001	−0.020
	(0.026)	(0.027)	(0.007)	(0.020)
$DtE_{pre} \times ZLB \times Size_2$	0.082***	0.055**	−0.027***	−0.042*
	(0.021)	(0.022)	(0.008)	(0.024)
$DtE_{pre} \times ZLB \times Size_3$	0.090***	0.073**	−0.017*	−0.012
	(0.028)	(0.030)	(0.010)	(0.020)
$DtE_{pre} \times ZLB \times Size_4$	0.059**	0.029	−0.030***	−0.009
	(0.027)	(0.027)	(0.011)	(0.018)
$DtE_{pre} \times ZLB \times Size_5$	−0.003	−0.084**	−0.081***	−0.029
	(0.036)	(0.037)	(0.014)	(0.018)
ΔIQR effect ($Size_1$)	7.02%	6.91%	−0.11%	−1.48%
ΔIQR effect ($Size_2$)	5.34%	3.52%	−1.68%	−2.56%
ΔIQR effect ($Size_3$)	5.23%	4.19%	−0.94%	−0.68%
ΔIQR effect ($Size_4$)	3.51%	1.68%	−1.73%	−0.52%
ΔIQR effect ($Size_5$)	−0.19%	−4.93%	−4.75%	−1.73%

Notes: DtE_{pre} is the debt-to-equity ratio before the ZLB. All the $year \times Size$ regressors have been omitted from the table for brevity, but are included in the regression. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

E.2 Alternative monetary policy shocks and confounding macro factors

Table E.5: Estimates of the baseline regression model when financial fragility is measured by the debt-to-asset ratio and the monetary policy shock by the Asset Purchase Programme.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times APP$	−0.566***	−0.588***	−0.334**	−0.255***	0.022
	(0.180)	(0.179)	(0.147)	(0.063)	(0.040)
ΔIQR effect	−11.36%	−11.69%	−7.46%	−5.90%	0.58%

Notes: DtA_{pre} is the debt-to-asset ratio before the APP. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.6: Estimates of the baseline regression model when accounting for firm heterogeneity as measured by the quintiles of the size distributions. Expansionary monetary policy is measured by the APP.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times APP \times Size_1$	−1.080***	−1.209***	−0.772***	−0.437***	0.129***
	(0.241)	(0.239)	(0.202)	(0.082)	(0.045)
$DtA_{pre} \times APP \times Size_2$	−0.734***	−0.800***	−0.443**	−0.357***	0.066
	(0.249)	(0.247)	(0.204)	(0.081)	(0.044)
$DtA_{pre} \times APP \times Size_3$	−0.537**	−0.522**	−0.286	−0.236***	−0.014
	(0.238)	(0.239)	(0.196)	(0.082)	(0.050)
$DtA_{pre} \times APP \times Size_4$	−0.179	−0.168	−0.050	−0.118	−0.010
	(0.212)	(0.211)	(0.174)	(0.076)	(0.048)
$DtA_{pre} \times APP \times Size_5$	−0.042	0.048	0.115	−0.066	−0.090
	(0.206)	(0.205)	(0.174)	(0.072)	(0.055)
ΔIQR effect ($Size_1$)	−18.58%	−19.74%	−15.14%	−9.96%	3.89%
ΔIQR effect ($Size_2$)	−13.45%	−14.24%	−9.26%	−7.76%	1.76%
ΔIQR effect ($Size_3$)	−10.63%	−10.41%	−6.37%	−5.38%	−0.36%
ΔIQR effect ($Size_4$)	−4.21%	−3.99%	−1.26%	−2.87%	−0.26%
ΔIQR effect ($Size_5$)	−1.03%	1.24%	3.04%	−1.60%	−2.16%

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. The coefficient to $year \times Size_5$ has been omitted because of multicollinearity, making the largest size class the reference group. The coefficients of the pre-trend in each group have been omitted for the sake of brevity. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.7: Estimates of the baseline regression model with the additional control of a continuous variable capturing cost of debt, as measured by the interbank interest rate.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.392***	−0.511***	−0.224*	−0.287***	0.119***
	(0.158)	(0.161)	(0.136)	(0.057)	(0.036)
$DtA_{pre} \times -RATE$	−0.022	−0.003	−0.021	0.019	−0.019*
	(0.039)	(0.039)	(0.034)	(0.014)	(0.010)
ΔIQR effect	−8.52%	−10.51%	−5.27%	−6.55%	3.31%

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.8: Estimates of the baseline regression model with the additional control of a continuous variable capturing the business cycle, as measured by the GDP.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.286*	−0.462***	−0.189	−0.274***	0.176***
	(0.162)	(0.164)	(0.141)	(0.055)	(0.036)
$DtA_{pre} \times GDP$	−0.017	−0.006	−0.009	0.003	−0.011***
	(0.013)	(0.013)	(0.011)	(0.004)	(0.003)
ΔIQR effect	−6.54%	−9.73%	−4.52%	−6.29%	5.06%

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

E.3 Alternative estimator

Table E.9: NB-QMLE results on main outcome variables.

	Dep. Variable (all in level):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.235***	−0.378***	−0.041	−0.964***	0.054
	(0.077)	(0.122)	(0.050)	(0.247)	(0.033)

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.10: NB-QMLE results on main outcome variables conditional on size class.

	Dep. Variable (all in level):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB \times Size_1$	−0.429***	−0.730***	−0.205***	−1.319***	0.163***
	(0.125)	(0.228)	(0.076)	(0.426)	(0.036)
$DtA_{pre} \times ZLB \times Size_2$	−0.402***	−0.622***	−0.087	−1.221***	0.102***
	(0.118)	(0.135)	(0.074)	(0.349)	(0.035)
$DtA_{pre} \times ZLB \times Size_3$	−0.110	−0.134	−0.010	−0.658*	0.042
	(0.103)	(0.151)	(0.066)	(0.371)	(0.040)
$DtA_{pre} \times ZLB \times Size_4$	−0.083	−0.374**	0.026	−1.201***	0.042
	(0.093)	(0.163)	(0.060)	(0.363)	(0.039)
$DtA_{pre} \times ZLB \times Size_5$	−0.139	−0.047	0.078	−0.557***	−0.104**
	(0.095)	(0.144)	(0.061)	(0.197)	(0.045)

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. The $year \times Size_5$ regressor is omitted because of perfect multicollinearity and is the reference category. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.11: NB-QMLE results on mechanisms outcome variables.

Panel A.	Dep. Variable (all in level):			
	(1)	(2)	(3)	(4)
	$\%Elect.$	$Selfgen.$	ALP	TFP
$DtA_{pre} \times ZLB$	0.010	−0.016	0.173***	0.039***
	(0.017)	(0.332)	(0.028)	(0.007)
Panel B.	**Dep. Variable (all in level):**			
	(1)	(2)	(3)	(4)
	K/L	K	L	E
$DtA_{pre} \times ZLB$	0.108**	0.078	−0.136***	−0.163***
	(0.052)	(0.054)	(0.027)	(0.044)

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. $\%Elect.$ and $Selfgen.$ are respectively the share of electricity usage and a dummy variable equal one for firms installing cogeneration or solar panels technologies. Fewer observations are available due to missing answers to these specific questions. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.12: NB-QMLE results on mechanisms outcome variables conditional on size class.

Panel A.	Dep. Variable (all in level):			
	(1)	(2)	(3)	(4)
	$\%Elect.$	$Selfgen.$	ALP	TFP
$DtA_{pre} \times ZLB \times Size_1$	0.055**	−0.013	0.204***	0.050***
	(0.025)	(0.833)	(0.033)	(0.009)
$DtA_{pre} \times ZLB \times Size_2$	0.014	0.054	0.212***	0.052***
	(0.022)	(0.499)	(0.030)	(0.007)
$DtA_{pre} \times ZLB \times Size_3$	0.008	−0.615	0.116***	0.025***
	(0.022)	(0.596)	(0.034)	(0.008)
$DtA_{pre} \times ZLB \times Size_4$	0.008	0.193	0.184***	0.040***
	(0.021)	(0.412)	(0.039)	(0.008)
$DtA_{pre} \times ZLB \times Size_5$	−0.053**	−0.040	0.152***	0.029***
	(0.023)	(0.350)	(0.046)	(0.009)
Panel B.	**Dep. Variable (all in level):**			
	(1)	(2)	(3)	(4)
	K/L	K	L	E
$DtA_{pre} \times ZLB \times Size_1$	0.132**	0.219***	−0.045	−0.221**
	(0.060)	(0.062)	(0.029)	(0.090)
$DtA_{pre} \times ZLB \times Size_2$	0.148***	0.122**	−0.121***	−0.223***
	(0.056)	(0.058)	(0.029)	(0.061)
$DtA_{pre} \times ZLB \times Size_3$	0.151**	0.155**	−0.088***	−0.088
	(0.063)	(0.067)	(0.032)	(0.054)
$DtA_{pre} \times ZLB \times Size_4$	0.101	0.044	−0.137***	−0.119**
	(0.066)	(0.064)	(0.032)	(0.052)
$DtA_{pre} \times ZLB \times Size_5$	0.042	−0.135*	−0.268***	−0.194***
	(0.071)	(0.071)	(0.036)	(0.053)

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. All the $year \times Size$ regressors have been omitted from the table for brevity, but are included in the regression. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

E.4 Alternative model specifications

Table E.13: Estimates of the baseline regression model for the period 2004-2019.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.373**	−0.426***	−0.203	−0.222***	0.053
	(0.150)	(0.151)	(0.128)	(0.052)	(0.035)
ΔIQR effect	−8.13%	−9.05%	−4.80%	−5.21%	1.42%

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.14: Estimates of the baseline regression model for the energy intensive sectors.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.219	−0.325	−0.075	−0.250***	0.107*
	(0.227)	(0.227)	(0.192)	(0.080)	(0.057)
ΔIQR effect	−5.27%	−7.45%	−1.94%	−5.93%	3.02%

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table E.15: Estimates of the baseline regression model including other controls.

	Dep. Variable (all ihs-transformed):				
	(1)	(2)	(3)	(4)	(5)
	CO_2	CO_2/VA	CO_2/E	E/VA	VA
$DtA_{pre} \times ZLB$	−0.389***	−0.423***	−0.266**	−0.157***	0.035**
	(0.147)	(0.148)	(0.125)	(0.048)	(0.017)
Q	0.515***	−0.170***	0.014	−0.184***	0.685***
	(0.046)	(0.045)	(0.038)	(0.017)	(0.011)
ALP	−0.063*	−0.619***	0.041	−0.660***	0.556***
	(0.032)	(0.036)	(0.027)	(0.019)	(0.015)
K/L	−0.036*	0.040**	−0.040***	0.080***	−0.076***
	(0.018)	(0.018)	(0.015)	(0.007)	(0.004)
ΔIQR effect	−8.46%	−9.06%	−6.14%	−3.82%	0.92%

Notes: DtA_{pre} is the debt-to-asset ratio before the ZLB. The three control variables are all ihs-transformed. Additional controls are firm fixed effects, sector-by-year dummies, ETS phase dummies, and year-by-decile of electricity share pre-ZLB. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.