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

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Abstract

In this paper we study the relationship between financial constraints and direct emissions at the firm level. We employ data on French manufacturing companies containing detailed information on carbon emissions, environmental policy exposure, and financial variables. We exploit the European Central Bank hit of the zero lower bound to identify an exogenous shock to financial constraints. Our main finding is that, after relaxing financial constraints, the more constrained firms reduced their CO_2 emissions the most. We also investigate on heterogeneity and transmission channels of the main effect. Our findings reveal that the main result is driven by small and medium enterprises, with the emissions of larger corporations being less elastic to the credit ease. Our results highlight that small and medium enterprises reduced their emissions throughout investment in self-generation and cogeneration of electricity, and improvements in both energy and economic efficiency. At last, we investigate on the interaction between the shock to financing constraints and the EU-ETS environmental policy. We use a nearest neighbour matching algorithm to construct a subsample of firms containing ETS treated companies and a control of non-ETS matched firms. Our estimates suggest that the interactions between financing constraints and environmental policies are small. We argue that the large size of treated companies, allows them to escape from issues related to financing constraints.

JEL Codes: Q52, Q48, D22.

Keywords: Financial constraints, environmental performance of firms, climate policies.

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1 Introduction

The "lending view" of monetary policy posit that the central banks can exert influence over the economy by means of interest rate adjustment which influence the cost of capital and the credit supply (Bernanke and Gertler, 1995; Kashyap and Stein, 2000). However, the effects of an untargeted monetary policy on firms' behaviour could be highly heterogeneous because the response to the same monetary policy signal depend upon firms' size (Gertler and Gilchrist, 1994), initial indebtedness (Auer et al., 2021a), reliance of external finance (Ferrando et al., 2019; Durante et al., 2022), age (Cloyne et al., 2023), collateral availability (Cole et al., 2024), and distance to default (Ottonello and Winberry, 2020).

More recently Ferrando et al. (2023), De Haas and Popov (2023) and De Haas et al. (2024) highlighted that adverse credit conditions might hold back corporations from investing in specific technologies, such as green and energy-efficient ones, which are instrumental for reducing carbon emissions and meeting the ambitious decarbonization targets sealed in the Paris Agreement. These investments are in fact more capital-intensive than traditional ones and, as such, they rely more on external financing (Almeida and Campello, 2007; Eyraud et al., 2013; Best, 2017; Accetturo et al., 2024) and they are more likely to be affected by problems of asymmetric information and credit constraints (Hubbard, 1998; Kaplan and Zingales, 1997). Furthermore, in absence of ambitious climate policies that alter relative prices of carbon-intensive inputs, such as green subsidies or carbon taxes, green investments are often characterized by lower private returns than traditional ones (Jaffe et al., 2005; Dechezleprêtre et al., 2019). This evidence allows for the possibility that the central banks can play a crucial role in decarbonizing an economic system by lowering borrowing costs and loosening credit supply, as also highlighted by Schmidt et al. (2019). Such a role for the central banks, has often been advocated from a theoretical or policy perspective. (Aglietta et al., 2015; Cœuré, 2018; Campiglio et al., 2018; Rudebusch 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). However, the effects and the channels through which monetary policy can affect firm-level carbon emissions remains an under-investigated topic from an empirical viewpoint, with few notable exceptions (Andersen, 2017; Levine et al., 2018; Goetz, 2018).

In this paper, we contribute to fill this gap by investigating (i) the impact of a monetary policy that eases the credit supply on firm-level carbon emissions, (ii) the heterogeneity of such an impact, and (iii) the transmission mechanisms through which this impact unfolds. To carry out this study, we exploit rich administrative and survey data containing information on carbon emissions, energy use, financial

conditions, green investments, value added and productivity for a large sample of French manufacturing companies. A first novelty of our study is that we are able to identify the mechanisms through which looser financing constraints operate by conducting a Kaya-type decomposition of firm-level emission dynamics over a long time horizon ($2000-2019$) also looking at investments in photovoltaic panels, auto-generation of energy, changes in the energy mix and general productivity improvements. The second novelty is the availability of such information for firms of very different size, allowing us to overcome a key limitation of previous studies that rely on large listed corporations which are on average less exposed to financial constraints (Goetz, 2018; Levine et al., 2018). Third, our study is the unique investigation on the possible existence of an interaction between credit policies and carbon pricing ones by exploiting a subsample of corporations subject to the EU Emission Trading Scheme (EU-ETS). Because regulated firms are larger and more capital intensive (Marin et al., 2018), they are less exposed to credit constraints and their emissions might be inelastic with respect to the credit conditions.

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 was cut to zero in July 2012, leading to a period of long-lasting and credible monetary and credit supply expansion. Following the standard Difference-in-Differences designs and the common practice used in the literature on the effect of financial development on economic performance (Rajan and Zingales, 1998; Manova, 2013), we allow the effect of the monetary policy variation to be heterogeneous across firms using an interaction with a firm-specific measure of financial exposures that predates the policy intervention (i.e., the firm debt-to-asset ratio before 2012). Furthermore, our data allow us to refine and disambiguate the heterogeneity in the treatment effect by differentiating the effects of monetary policy by firm size.

Our findings highlight the key role of credit supply policies in the transition toward a low-carbon economy. Our favourite estimate indicates that easing credit constraint reduces CO_2 emissions by almost 10% in most financially exposed firms (75^{th} percentile of initial DTA) relative to less financially exposed ones (25^{th} percentile of initial DTA).[1] This is the net effect resulting from two contrasting forces: a modest emission increase of $+2.1\%$ associated with a *size effect* and a pronounced emission decrease associated with a *technique effect*. Besides lending support to the parallel trend assumption, event-study estimates suggest that the technique effect materializes in the medium-to-long term, while the size effect in the short-term. Firms become more environmentally efficient by increasing capital intensity, becoming more productive, and investing into auto-generation of energy. Interestingly, small firms disproportionately

[1]The initial DTA is measured by the average before the ZLB hit.

benefit from the relaxation of credit constraints, improving their environmental performances through multiple channels. Our estimates also indicate that financial constraints do not concern EU-ETS regulated firms, with the coefficients being insignificant. Since the EU-ETS policy has been established in 2005, we argue that this last result might be also due to the fact that these companies started their campaign toward the reduction of CO_2 emissions already before 2011, when the expansionary monetary policy shock loosened financing constraints. Overall, our results imply that CO_2 emissions fell by 2.1 million tonnes on average between 2011 and 2019. This effect is about half of the effect estimated by Colmer et al. (2024) for the EU-ETS policy, and accounts for about 10% of the aggregate reduction in industrial emissions during this period. The magnitude of the effect suggested by our estimates is instead larger with respect to the effect estimated by Goetz (2018) on US data, suggesting that the US the Maturity Extension Programme reduced CO_2 emissions by 2.2% a year. This might be explained by the fact that the sample by Goetz (2018) only comprises listed companies, which are large and, according to our results, less sensitive to financing constraints.

Contribution to the literature

In a general equilibrium setting, Andersen (2016) and Lessmann and Kalkuhl (2024) highlighted that monetary policy easing bears two contrasting effects on CO_2 emissions. On the one side, lower interest rates boost short-run economic activity, leading to higher energy demand and, in turn, to carbon emissions (*scale effect*). On the other side, an expansion of the monetary base contributes to the decline in emissions by reducing the cost of capital and making capital-intensive and carbon-free energy/technologies relatively cheaper (*technique effect*).

Aghion et al. (2024), and Campiglio et al. (2024) have recently shown how financing constraints can threaten the green transition in endogenous growth settings. Aghion et al. (2024), in particular, highlighted that firms get better at innovating in technologies that they have previously succeeded in. Therefore big and old firms specialize in brown innovation, while the smaller and younger firms are responsible for a large share of the transition to green innovation. This view is consistent with a long-standing empirical literature (see Schiantarelli, 1996; Cetorelli and Strahan, 2006; Savignac, 2008; Bottazzi et al., 2014; Hall et al., 2016, among the others) demonstrated that small and young firms are often subject to financial constraints. Oppositely, Lanteri and Rampini (2025) highlight that cleaner and newer capital requires a larger down payment, leading financially constrained, smaller firms to optimally invest in dirtier and older capital vis-à-vis unconstrained, larger firms.

From an empirical viewpoint credit availability have been shown to impact on employment (Caggese and Cuñat, 2008), productivity (Caggese, 2019), export (Manova et al., 2015; Altomonte et al., 2016) and investment composition in terms of duration (Campello et al., 2010) and risk (Aghion et al., 2010; Lin and Paravisini, 2013). Recently, financial constraints have been shown to also affect outcomes related to green investment (Costa et al., 2024) and green innovation (Ghisetti et al., 2017; Kalantzis et al., 2022; De Haas et al., 2024). We contribute to this stream of literature by investigating the effects of financing constraints on a set of new dependent variables associated to firm-level emissions of CO_2. In so doing, we connect the two streams of literature that associate financial constraints with the economic and environmental performance of firms.

To our knowledge, there are only few empirical contributions focused on the causal effects of financial constraints on emissions of CO_2 at the firm (or plant) level. Andersen (2017) employs US data of publicly listed companies and their plants to correlate the plant-level creditworthiness score (based on past payment punctuality) with emissions stemming from about 650 different toxic chemicals. The findings suggest that credit-constrained firms generate more emissions, but the data are not experimental and the author does not clearly identify exogeneity in the creditworthiness of plants, acknowledging that the results do not support strong conclusions about causality but solely associations between the two variables of interest. Differently, Goetz (2018) exploits an exogenous shock to the long-run cost of debt, due to the unconventional monetary policy carried out by the Federal Reserve (the Maturity Extension Program) starting from September 2011. By interacting this event with the long-term debt of companies before the shock, he finds that credit easing allows the most financially constrained companies to reduce their levels of toxic emissions. However, the data is a sample of US listed companies, thus the focus is limited to large corporations only and covers the 2006-2016 period. An alternative identification strategy is exploited by Levine et al. (2018), that analyse shale discoveries that created exogenous liquidity windfalls for local bank branches over the $2000 - 2014$ period. With this exogenous positive credit supply shock, they assess the effect on different environmental performance measures at the firm level. They find that when a firm's relationship lender receives positive liquidity shocks, the firm reduces its emissions, gets higher environmental performance ratings, and increases the frequency with which it mentions environmental concerns in its SEC 10-K filings.

In terms of identification strategy, our contribution builds and extends Goetz (2018). However, we complement the previous findings reported above and we go beyond them in four different dimensions. First, we employ a sample of French companies which comprises both large corporations and small en-

terprises. This is a significant contribution because the latter are more vulnerable to credit constraints. Second, we provide evidence for a large European country rather than for the United States. Hence, the institutional arrangements and the financial systems is substantially different to previous studies. Third, our measure of carbon emissions is drawn from data on energy consumption by different sources, and it is therefore the most precise and reliable measure of direct emissions at the firm level, allowing us to minimize measurement error. Fourth, by interacting our main independent variable with the firm size, we take into account more heterogeneity than previous studies can incorporate. The fact that our results hold true also in this case, and that the effect is larger for smaller firms, also allows to solve all possible ambiguities related to the choice of the exposure to the monetary policy shock.

Finally, we contribute to the burgeoning literature examining the economic and environmental performance at the micro-level (see the recent survey by Dechezleprêtre et al., 2019). Most papers focus on evaluating the effects of environmental policies (Martin et al., 2014; Ahmadi et al., 2022; Dechezleprêtre et al., 2023; Colmer et al., 2024; Martinsson et al., 2024; Kruse et al., 2024), energy price shocks (Marin and Vona, 2021; Fontagné et al., 2024) or globalization trends (Li and Zhou, 2017; Cherniwchan, 2017; Dussaux et al., 2023).

We contribute to this literature in two ways. First, we allow the effectiveness of the main EU climate policy, the EU-ETS, to depend on the firm exposure to financial constraints. Because the EU-ETS predate the monetary policy shock used for identification here, we do not aim at evaluating the effect of the EU-ETS but the differential effectiveness of relaxing the financial constraint for firm under the EU-ETS relative to a credible counterfactual. Second, we are able to compare and benchmark the size of our effects to those found by Colmer et al. (2024) for the EU-ETS using the same dataset. This is crucial to provide insights for the design of green policy packages, including policies that can help decarbonize our economies.

The remainder of the paper is organized as follows. Section 2 presents a general theoretical framework that helps us to think about the direct and indirect relationship between financial constraints, production and carbon emissions. Section 3 discusses the main idea behind the identification strategy employed in our empirical exercise. Section 4 presents the data sources, the cleaning process and the construction of the main variables, together with some preliminary summary statistics. The results from the regression analysis with the event-study plots are presented and extensively discussed in Section 5. In Section 6 we present some counterfactual analysis, to estimate the possible aggregate effect stemming from the estimates of our baseline estimates. Section 7 concludes, also discussing the policy relevance of our results.f

2 General framework

In this section we build a general setting to better reason about the relationship between financing constraints, production, and carbon emissions of business firms. Let us consider a generic firm (i) operating at a given year (t). The firm uses an Hicks-neutral technology that combines capital $K_{i,t}$, labour $L_{i,t}$, and energy $E_{i,t}$ with the aim of producing output $Y_{i,t}$:[2]

$$Y_{i,t} = A_{i,t} F\left(K_{i,t}, L_{i,t}, E_{i,t}\right). \tag{1}$$

For sake of simplicity, let us assume that all firms in a sector face the same prices, so that we can express inputs in terms of their expenditures. And let us assume that all firms have the possibility to employ different energy sources (e.g., fossil fuels, natural gas, electricity, etc.) so that a firm's energy efficiency $A^E_{i,t}$ can be measured as the weighted average of the energy efficiency from each different source (j) in the firm's energy mix:

$$A^E_{i,t} = \sum_{j=1}^{J} A^E_{i,t,j} \frac{E_{i,t,j}}{E_{i,t}} \tag{2}$$

where $A^E_{i,t,j}$ and $\frac{E_{i,t,j}}{E_{i,t}}$ measure respectively the energy efficiency and the share of utilization of the j-th energy source at the i-th firm, in period t.

Let us further assume that the production process generates an amount of carbon emissions that directly depends on total production and energy efficiency:

$$CO2_{i,t} = G\left(Y_{i,t}, A^E_{i,t}\right) \tag{3}$$

where $G'_Y > 0$ and $G'_{A^E} < 0$.

To model financial constraints, we follow a strand of literature proposed by Moll (2014); Midrigan and Xu (2014); Manaresi and Pierri (2024) which hinges upon a setting early proposed by Evans and Jovanovic (1989) in a model of entrepreneurial choice under liquidity constraints. Hence, we assume that the firm's choices are subject to two dynamic constraints:

$$\begin{aligned} K_{i,t} &= K_{i,t-1}(1-\delta_i) + I_{i,t} \\ X_{i,t} &\leq X_{i,t-1}\Lambda_{i,t} \quad \text{with} \quad X \in \{K, L, E\} \end{aligned} \tag{4}$$

The first row of equation 4 represents the standard law of motion of capital, with heterogeneous depreci-

[2]The function F could be of any kind such as a Cobb-Douglas, a CES, or a Leontief. For example, one might employ the nested CES case also specified in Hassler et al. (2012) which is sufficiently general to accommodate several standard production functions, depending on the values of few parameters measuring the elasticity of substitution between inputs.

ation (δ_i) and investment $I_{i,t}$. The second row characterizes the financial constraints, by means of a factor $\Lambda_{i,t} \geq 1$ that could limit the optimal firm choices for the three inputs. Let us consider capital to provide two extreme cases. If $\Lambda_{i,t} = 1$ financial constraints are so tight that a firm can only replace the amount of depreciated capital (hence $K_t = K_{t-1}$. At the opposite extreme, if $\Lambda_{i,t} = +\infty$, the firm is free from financial constraints and can pick the optimal level of capital. As already discussed at length in the introduction, the across-section and across-time heterogeneity in $\Lambda_{i,t}$, may reflect a number of factors (e.g., individual firms size, leverage, internal liquidity, as well as aggregate monetary, regulatory, or fiscal policies).

Although stylized, this setting is sufficient to allow us investigating how an exogenous shock to credit constraints affects a firm's environmental performance, as measured by her emissions of CO_2, throughout different channels. In particular, by applying the chain rule on Equation 3, it is straightforward to observe that the effects of a shock to credit constraints on emissions depends upon the responses of both output and energy efficiency:[3]

$$\frac{\partial CO2}{\partial \Lambda} = \frac{\partial G}{\partial Y}\left[A\underbrace{\left(\frac{\partial F}{\partial K}\frac{\partial K}{\partial \Lambda} + \frac{\partial F}{\partial L}\frac{\partial L}{\partial \Lambda} + \frac{\partial F}{\partial E}\frac{\partial E}{\partial \Lambda}\right)}_{\text{scale}} + \underbrace{\left(\frac{\partial A}{\partial \Lambda}\right)}_{\text{economic eff.}} F(K,L,E)\right] + \frac{\partial G}{\partial A^E}\left[\sum_j \left(\underbrace{\frac{\partial A_j^E}{\partial \Lambda}}_{\text{source eff.}} \frac{E_j}{E} + A_j^E \underbrace{\frac{\partial \frac{E_j}{E}}{\partial \Lambda}}_{\text{energy mix}}\right)\right] \quad (5)$$

From equation 5 we can disentangle different effects. First, there is the *scale effect*, originating from the response of the inputs of production ($\partial X/\partial \Lambda$, for $X \in \{K, L, E\}$). A number of articles have estimated how financial constraints affect inputs of production and, in particular, Cingano et al. (2016) and Amiti and Weinstein (2018), provided evidence that financing constraints exert a negative effect on capital. Similarly Chodorow-Reich (2014) and Bentolila et al. (2018) suggest the presence of a negative effect on labour. Investigating the net effect is also interesting, to assess how financial constraints affect the capital intensity. Second, there is an *economic efficiency* effect ($\partial A/\partial \Lambda$), depending on the reaction of the productivity to the financial constraints shock. As already highlighted by Caggese (2019) and Altomonte et al. (2016), this effect emerges from the fact that tighter financial constraints impair investment into new and more productive capital. Third, there is the *technique effect*. This is visible in equation 5 as the combination of the two effects on the variation in the *energy sources efficiencies* ($\partial A^e/\partial \Lambda$) and in the *energy mix* ($\partial(E_j/E)/\partial \Lambda$). In this respect the previous evidence is scant, but such an effect is likely to be mediated by investment in green technologies, as highlighted by Aghion et al. (2024), Lanteri and Rampini (2025), and Ghisetti et al.

[3]For sake of clarity in what follows we omit indices. All equations refer to a single firm i at period t.

(2017).[4] Finally, an interesting question arises about the possibility that the economic efficiency effect is stronger/weaker that the energy efficiency one ($\partial A^E/\partial \Lambda$).

In what follows, we will focus on the empirical counterparts of these forces, providing a joint overview about how financial constraints affect the environmental performances of firms throughout the different channels.

3 The effect of credit easing on carbon emissions

As highlighted in the previous sections, an exogenous variation to financing constraints may have ambiguous impacts on a firm's CO_2 emissions. On the one hand, unleashing credit tightness allows firms to expand production, to increase investments in physical and human capital, leading in turn to a higher quantity of emissions. On the other hand, it allows them to increase green investments that could abate their emissions. In what follows we explain our identification strategy allowing us to estimate the net effect and to disentangle the effects on different channels.

3.1 The channels through which credit easing affects emissions

First and foremost, we conduct a firm-level, Kaya-like decomposition of the total effect of credit constraint on emissions. More specifically, by denoting with $CO2_{it}$ the emissions of carbon dioxide stemming from firm i in period t, and by E_{it} and VA_{it} its energy consumption and value added, we decompose the level of emissions for each firm in our estimation sample using the following identity:

$$CO2_{it} \equiv \frac{CO2_{it}}{E_{it}} \times \frac{E_{it}}{VA_{it}} \times VA_{it} \tag{6}$$

where the first two components capture the technique effect (respectively measured by the carbon content of the energy used, and the energy intensity), while the third one captures the overall scale effect.In the next section, we will illustrate how we estimate the causal effect of easing credit condition on each of these components.

Second, the mechanisms through which the technology effect materializes are often unexplored by the literature. Suppose we estimate that by relaxing financial constraints, firms' reduce the carbon content of energy or they improve energy efficiency. How does it happen? Several competing forces could be at work with very different policy implications: an increase in general productivity, a change in the energy mix, increased investment in newer/greener capital equipments, or investment in self-production of energy

[4]Notice that Lanteri and Rampini (2025) employ a richer framework to derive implications that are also in line with ours.

using low-carbon sources (e.g., photovoltaic energy, heat pumps or co-generation). In our paper, we are able to pin down the contribution of most of these mechanisms.

Finally, we are able to explore the interaction between a climate policy (notably the EU-ETS) and a monetary policy that relax the financing constraints (the ZLB). In doing so, we shed light on the role of credit tightness as a mediating factor for the effectiveness of a cap-and-trade scheme. A caveat is needed here. Because the beginning of the EU-ETS predates the plausibly exogenous variation to financial constraints identified in this paper, we do not evaluate the causal effect of the EU-ETS, but only the differential effect generated by looser credit constraints between ETS-regulated companies and a suitable control group.

3.2 Identifying the causal effect of financial constraints

Estimating the causal impact of the propagation of an aggregate variation to the credit supply at the firm level is a non-trivial task. The main hurdle reside in the fact that financial constraints at the firm level might be simultaneously correlated with a firm economic and environmental outcomes. Indeed, firms might fix a "debt ratio target" which could be based on the information about the cost of interest payments (see Graham et al., 2015). As such, the profit margin might depend on the cost of external financing that also affects the level of investments, including investment in greener capital equipments. Furthermore, in a context of expected increases in carbon pricing, banks can incorporate the risk of carbon-intensive assets to become stranded, thus increasing the interest rates for carbon intensive firms.

A classic solution to such an endogeneity problem dates back to the seminal paper of Rajan and Zingales (1998) and combines a systemic change in the credit supply that is common to all firms, and a time-invariant firm-specific measure of financial constraint predating the aggregate change.[5]

Following this well-established approach, we choose the debt-to-asset (DtA) ratio as the initial local measure of exposure to financial constraint. The DtA captures the leverage of a company and is one of the favourite indirect measure extensively employed by the extant literature (see Whited, 1992; Greenaway et al., 2007; Cao and Leung, 2020; Alnamlah et al., 2022), especially so when analysing large European economies (Bellone et al., 2010; Bottazzi et al., 2014; de Guevara et al., 2021). A first argument that motivates our choice is that more leveraged companies are on average more financially constrained and have more difficulty to raise additional external financial resources, as highlighted by Ferrando et al. (2019). A second argument is that an expansion of the monetary base via interest rate cuts, reduces the cost of

[5]Notice that this identification strategy is also similar to a shift-share design that combines a global (EU level in our case) shift with a local (firm-level in our case) measure of exposure.

debt refinancing for more leveraged companies (Auer et al., 2021b).

We acknowledge that the DtA can only be an imperfect measure of financial constraint, because it could also signal the willingness of investors and banks to provide financial resources to the firm – especially when a firm possesses sufficient amount of collateral assets to be pledged against the new debt. We tackle this measurement error problem in three ways. First, by employing two alternative measures of financial constraints – i.e. the debt-to-equity (DtE) and the gross operating profits (GOP) ratio, measured by the ratio between profits and sales revenues.[6] Second, we consider the pre-shock size as another dimension of heterogeneity in the firm's exposure to easing credit conditions. The intuition is that smaller firms are more likely to be financially constraints, making the debt-to-asset ratio a more reliable measure of their credit exposure to credit constraints. Instead, a large firm can have a high debt-to-asset without being financially constrained. Third, we also run robustness regressions controlling for the capital-labour ratio, which proxies the capital intensity of a firm and its chances to provide collateral in exchange for funds.

As for the global change in the credit supply, we exploit an aggregate, long-lasting and credible variation of the ECB choices from July 2011, when the nominal interest rate has been set to zero and the monetary policy hit the Zero Lower Bound. Although the ZLB loosened the credit constraints for all French firms at the same time, the benefits that companies received have been highly heterogeneous depending on whether the company was financially constrained or not (see Alder, 2023). A non-indebted company did not benefit directly from a variation in the cost of refinancing its outstanding debt. On the opposite side, a heavily indebted company enjoyed large benefits from the easing, because the cost of refinancing the outstanding debt has suddenly decreased. Finally, a crucial advantage of the ZLB is its exogeneity with respect to the firms' environmental performance. First, because the unique objective at the ECB is inflation targeting, plausibly making the monetary policy decisions uncorrelated with the levels of industrial emissions of CO_2.[7]

A key issue concerns the discontinuity created by the ZLB with respect to a standard fine tuning of the interest rate made by the ECB. In this respect, the change in the monetary policy stance underlined by the ZLB has been highly persistent and reflected the announced entrance into a "new-normal" for the

[6]The DtE has been chosen because it allows one to better capture the capital at risk of a firm's liabilities. The GOP has been chosen because it proxies the ability of the firms to generate internal liquid resources and captures a firm's ability to carry out investment without the need of external finance.

[7]Only in the recent years the ECB has also announced some generic intention to commit toward a monetary policy stance that is also fit to contrast climate change and to support the green transition (see Lagarde, 2022). However, during the period under analysis in this paper (i.e., 2000 – 2019) there has been no signal from any ECB governor about using monetary policy instruments to interact with environmental concerns.

monetary policy within the Euro Area. This justifies the use of the moment in which the ECB announced to set the nominal rate on the marginal refinancing operations at the ZLB (i.e., July 2012) as a turning point in our Difference-in-Differences (DID) design.[8] A second issue might be related to the fact that the expansionary monetary policy has begun before 2012, in fact the interest rates have been lowered by the ECB also in response to the failure of Lehman Brothers, in the last quarter of 2008. However, such an expansionary policy was not persistent, as the ECB raised again its key interest rates between May 2009 and November 2011. In contrast, since 2012 the expansion was long-lasting, with the ECB that did not raise the main refinancing rate for a decade, until July 2022. Finally, to tackle residual concerns on the timing of the change in the ECB monetary policy stance, we assess the robustness of our results by employing the ECB adoption of unconventional monetary policy starting from 2015 with the Asset Purchase Programme (APP), as well as by controlling for the reference interbank rate for European economies. Finally, although it seems plausible to treat the monetary policy shock as good as randomly assigned, it is still possible that most financially constrained companies were on a different emission pattern before the announcement of the ZLB, thus violating the key "parallel trend assumption". That is, credit constrained and non-credit constrained firms would have followed different emission patterns anyway, even in absence of the monetary policy change. We exploit the length of our longitudinal data to conduct a second-best test of the parallel trend assumption, by assessing the presence of pre-trends in an event study design.[9]

All in all, by interacting our time-varying indicator of the monetary policy stance (the ZLB) with the firm-specific measure of the initial exposure to the shock (the DtA), we reveal the exposure to the financing constraints at the firm level.

3.3 Empirical implementation

We here present the equations used to estimate the effects of easing the financing constraints.

Baseline specification. Our primary results are obtained using a standard two-way fixed-effects DID specification:

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

[8]The credibility of the change in the monetary policy stance also arises from the declaration by the ECB president. As a matter of fact Mario Draghi's notable "*whatever it takes*" speech is also dated July 2012.

[9]Furthermore, since the credit easing shock might be correlated with other macroeconomic variations, we also carry out a robustness check in which we interact the exposure (i.e., DtA) with the EONIA interest rate and with the growth rate of GDP for the Euro Area. This ensures that the effect that we are estimating stems from variations in the credit constraints rather than other macroeconomic factors.

where Y_{it} is either CO_2 emissions, CO_2 emissions per value added, and any of the terms of the Kaya-type decomposition presented in equation 6 (namely carbon content of energy, energy efficiency, and value added).

The variable $DtA_{i,pre}$ is the time-invariant debt-to-asset ratio predating the ZLB. This is computed as the average DtA over $2000-2011$, in order to maximize the sample of firms used in the main analysis, and to smooth out possible outlying years.[10] The main coefficient of interest is β, the marginal effect of a variation in the credit constraints after the ECB rate was set to zero. The effect of the standalone variable $DtA_{i,pre}$ is absorbed by the firm fixed effects (μ_i), which control in the most flexible way for the time-invariant unobserved heterogeneity across firms. We also include sector-by-year fixed effects (λ_{ts}) to account for sector-specific trends in a flexible manner. We also account for possible time-trends that are specific to the initial energy mix of companies, by including time-by-decile fixed effects – where the deciles are computed on the average share of electricity usage by each company over $2000-2011$. Finally, to wipe-out the possible confounding effects generated by the EU-ETS policy (for the regulated companies) we include time-by-phase of the ETS fixed effects (θ_{ets}), where the phases relate to the different changes in the EU-ETS setup.[11]. Finally, in a set of robustness tests we also control for firm-specific variables that could be correlated with our measure of exposure to financial constraints, such as the capital-labour ratio, firm sales and firm productivity.

The same regression setting is also employed to analyse the mechanisms trough which these effects are generated, and in that case we will change our dependent variables to measure different types of firm-level variations related to the share of electricity use, investment in the auto-production of energy via co-generation technologies or solar panels, the ratio between capital stock and labour input, and total factor and labour productivity. The last channel is also particularly important, because it capture the economic efficiency effect discussed in Equation 5.

Because electricity and steam do not generate any direct CO_2 emissions, firm-year pairs for which all energy consisted of electricity and/or steam are automatically set to release zero energy-related CO_2 emissions. As a consequence, taking logarithms of CO_2 would lead to the non-random exclusion of many interesting observations. In particular it will wipe out some relevant observations regarding all firms that had strictly positive emissions before 2011, but then reached zero direct energy-related emissions later on.

[10]Formally, our measure of exposure reads: $DtA_{i,pre} = (\tau_1 - \tau_0)^{-1} \sum_{t=\tau_0}^{\tau_1} \left(\frac{Debt}{Assets}\right)_{it}$, where $\tau_0 = 2000$ and $\tau_1 = 2011$. Since not all firms are surveyed every year, such a measure allows us to maximize the sample size providing us with a sufficient amount of statistical power.

[11]Similarly to Colmer et al. (2024), the EU-ETS phases in our sample are respectively the announcement period ($2001-2004$), the trading phase I ($2005-2007$), the trading phase II ($2008-2012$), and the trading phase III ($2013-2019$).

For this reason, we use the inverse hyperbolic sine transformation (ihs) rather than the usual logarithmic transformation (Aihounton and Henningsen, 2021).[12]

Event-study design. The DID setting outlined in equation 7 can easily be generalized by means of an event-study design that allows to test for pre-trend and that qualitatively assess the unfolding of the effect under investigation. More specifically, we estimate:

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

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. Such an event-study setting allows us to test the empirical validity of our identifying assumption. In the regression of equation 8, the time-varying coefficients β_τ refer to the differential (flexible) trends in the 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.[13]

Heterogeneity by firm size. Financial constraints can only be imperfectly represented by the debt-to-asset, hence we also estimate a set of augmented regressions, in which we account for the heterogeneity of financial constraints for different size, which is known to be highly (inversely) correlated with the degree of credit constraints (see Schiantarelli, 1996; Beck et al., 2005; Bottazzi et al., 2014). More formally, we interact the main independent variable with five dummy variables, denoting the size class to which a firm belongs before the shock.[14] The augmented equation takes the following form:

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

where $k \in \{1, \ldots, 5\}$ indicates the membership to each specific size class in the first observation year and τ is time trend which allows one to capture the behaviour of the dependent variable for each group of

[12]Note that $ihs(Y_{it}) = ln(Y_{it} + \sqrt{Y_{it}^2 + 1})$, which approximates to $log(2Y_{it})$ except for very small values of Y. This transformation is widely employed because (i) estimated parameters can be roughly interpreted in the same way as a logarithmic transformation; (ii) differently from the log, the inverse hyperbolic sine is well defined at zero. However, since we take the inverse hyperbolic sine transformation, the decomposition of equation 6 only approximately holds. Furthermore, as recent contributions have highlighted that the ihs is scale-dependent (see Chen and Roth, 2024), we also estimate robustness exercises using the logarithmic transformation plus a small constant and the simple logarithm that wipes out all zeros.

[13]Notice that also the case in which all coefficients before the events are significant but of opposite sign vis-à-vis the coefficients after the event is interesting because it indicates that the event had a strong impact by reverting the trend predating the shock.

[14]In particular, we compute the firms' average sales before the shock and we then build five classes using the quintiles of the sales' in-sample distribution.

firms trough time.[15] This model is sufficiently flexible to allow us to have firms belonging to the different size classes to display heterogeneous trends before the shock to financial constraints (the α_k parameters) but also to measure the effect that the shock has on the dependent variables for each size class (the β_k parameters).

Policy interaction. The last model consists in an augmented specification of the baseline equation 7, but also accounting for the interaction of financial constraints with the EU-ETS policy. Following Colmer et al. (2024), we employ a 1-to-1 nearest neighbour matching algorithm for this exercise, in order to have a balanced sample constructed to estimate the following regression equation:

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

where we have included in the model a triple interaction ($DtA_{i,pre} \times ETS_i \times ZLB_t$) such that the coefficient β_3 can be interpreted as the marginal effect of relaxing financing constraint only for the ETS-treated companies. The pairwise interactions saturate the model, but are also important to account for the effect of relaxing financial constraints on untreated companies (the variable $DtA_{i,pre} \times ZLB_t$), and for the effect of the ZLB on the ETS companies independently from the debt-to-asset ratio (captured by the variable $ETS_t \times ZLB_t$).[16]

4 Data and descriptives

Our work relies on a set of detailed data covering the $2000 - 2019$ period.[17] The main dependent variable of our empirical exercise is the amount of combustion-related carbon dioxide (CO_2) emissions of companies. Similarly to Marin and Vona (2021), the variable is retrieved from the *EACEI* survey ("*Enquête sur les consommations d'énergie dans l'industrie*"). The EACEI survey is conducted yearly by the French national statistical office (INSEE) and provides detailed information on energy purchase and use with a detailed breakdown by source (13 different sources). The sampling scheme of EACEI includes all French manufacturing establishments with a response rate close to 90%. Even if the sampling strategy has changed over time, the industrial coverage remained constant, including all manufacturing except the sectors of energy production, agrifood and sawmills. Around 12,000 establishments are drawn for the sample each year

[15] Notice that the fifth group, comprising largest firms, has been excluded from the first summation term in the regression to avoid perfect multicollinearity. The trend of the largest companies before the ZLB represents the reference category.

[16] The interaction $DTA_{i,pre} \times ETS_i$ is left out because it is absorbed by the firm fixed effects μ_i.

[17] We decided to ignore observations for $2020 - 2022$ (even if already available) because the Covid-19 pandemics and the subsequent fiscal policies implemented by the EU and the French government have strongly affected both the economic and environmental outcomes of French companies (see Gourinchas et al., 2021; Guerini et al., 2022, among the others) that would require specific and detailed analyses.

and it includes (i) all industrial establishments with 20 employees or more in the most energy consuming sectors; (ii) all establishments with more than ten employees in the manufacturing of industrial gases sector; (iii) all establishments with more than 250 employees on the 31st of December of that year; (iv) a random sample of establishments with employment between 20 and 249 employees in sectors that are not energy intensive. The fourth point is particularly relevant with respect to small firms, indicating that the sample is rotating and the panel is unbalanced. Emissions of CO_2 for each sampled establishment are calculated by multiplying the technical conversion coefficient with the energy consumption from different energy sources. Emissions are then aggregated across all the surveyed establishments belonging to the same enterprise.[18]

Financial data for French firms were retrieved from administrative balance sheet and income statement data available from the FARE-FICUS database ("*Annual structural statistics of companies from the ESANE scheme*").[19] The database covers the universe of French non-financial corporations that are subject to corporate taxation legislation. However, we exclude from the analysis companies with incomplete information; firms in Agriculture, Forest and Fishing (AZ), Finance and Insurance (KZ) and Public Administration, Education, Human Health and Social Work (OQ) sectors; legal persons and organizations subject to administrative law, as well as self-employees and craftsmen.[20]

Finally, we also identify firms subject to the EU Emission Trading Scheme (ETS), that is the most important climate mitigation policy tool for the EU.[21] The unique firm identifier (SIREN) of Operators Holding Accounts of regulated entities retrieved from the European Union Transaction Log were used to identify firms with at least one establishment subject to the policy. Table 1 summarizes the basic information of the employed data sources.[22]

The unit of reference for our analysis is the firm. However, the *EACEI* survey is collected for single

[18]We limit our analysis to firm-year pairs for which at least 90% of employment (establishment-level employment is retrieved from *DADS-Etablissements*, that is administrative data on the universe of French establishments) was recorded in establishments that took part of the EACEI survey in that year.

[19]Despite the fact that the energy survey is collected at the establishment level, our analysis is performed at the firm level because financial data are only measured for firms and not for establishments.

[20]Notice that in 2008, data on firm debt are missing from the original source for all companies. Although this is a drawback for the database if one aims at studying the year-on-year debt variation, it is a limit that does not strongly affect our measure of exposure to financial constrains because we average the debt-to-assets ratio over $2000 - 2011$.

[21]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 thousands 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 correspond to their annual emissions of greenhouse gases. Allowances can be freely traded among participants. The policy experienced substantial changes over the period of reference, with a first pilot phase (2005-2007) where allowances were mostly allocated for free to installations, followed by a second phase (2008-2012) characterized by an highest penalty for non-compliance (from 40 to 100 euro per tonne of emissions beyond compliance), a third phase (2013-2020) with the default allocation rule being the auctioning of permits.

[22]More information on the data sources are available at `https://www.casd.eu/` and `https://www.euets.info/`.

Table 1: Information about the data sources.

Database	Period	Coverage	Variables of interest
FICUS-FARE	2000-2019	Universe of firms	Sales, debt, assets, …
EACEI	2000-2019	Survey of ≈10k establishments per year	Energy use by source
DADS	2000-2019	Universe of establishments	Employment
EUTL	2005-2019	Universe of ETS-regulated establishments	Dummy for ETS firms

establishments. The different unit of analysis of the two sources is irrelevant for single-establishment firms. For multi-establishments companies, however, it could be the case that in each year the EACEI survey just selects a fraction of all the establishments to be part of the survey. This would imply that, on the one hand, we consider the financial situation of the whole company, while on the other hand we would just consider part of CO_2 emissions and energy use. To limit measurement errors, we just retain in our sample those firm-year pairs for which establishments account for at least 90% of employment were included in the EACEI sample in that year.[23] For those multi-establishment firm-year pairs that are retained in the sample, we then rescale balance sheet variables proportionally with the share of employment in EACEI-covered establishments. Finally, we also exclude firms located in the *Départments d'Outre-Mer* (Guadeloupe, Guyane, Martinique, Mayotte and La Réunion) and all firms with missing or incomplete information.

The cleaning process leads to 72130 observations consisting of an unbalanced panel of 13401 unique firms over the $2000-2019$ period. Table 2 provides descriptive evidence about the dependent variables and the main regressors, aggregated by size class, constructed from sales distribution quintiles.[24]

The relationship between our indicators of financial constraints and firm size is negative but at best weak, with companies in the top quintile of the size distribution having a DtA (DtE) that is 10% (26%) smaller than the one in the first quintile of the size distribution. Also the gross operating profit ratio is positively and monotonically related with firm's size, suggesting that the ability to generate profits grows with size and contributes to reducing financial constraints. As for emissions, while the level of emissions is clearly growing with size, the intensity of CO_2 emissions per value added is the largest for the top quintile, mostly driven by higher energy intensity of value added rather than by higher carbon content

[23]Data on employment for all establishments in France was retrieved from the DADS-Etablissement ("*Panel des Déclarations Annuelles de Données Sociales*") database. By considering the most stringent requirement (i.e. firms with all establishments surveyed in EACEI) we would loose about 21.4% of observations and 7.3% of unique firms. Still, results based on the subsample of firm-year pairs for which all establishments were covered by EACEI are very similar to the ones reported in the paper and remain available upon request.

[24]Appendix A contains similar descriptive statistics, but conditional upon the firms' ETS status as well as by their DtA distribution quintiles.

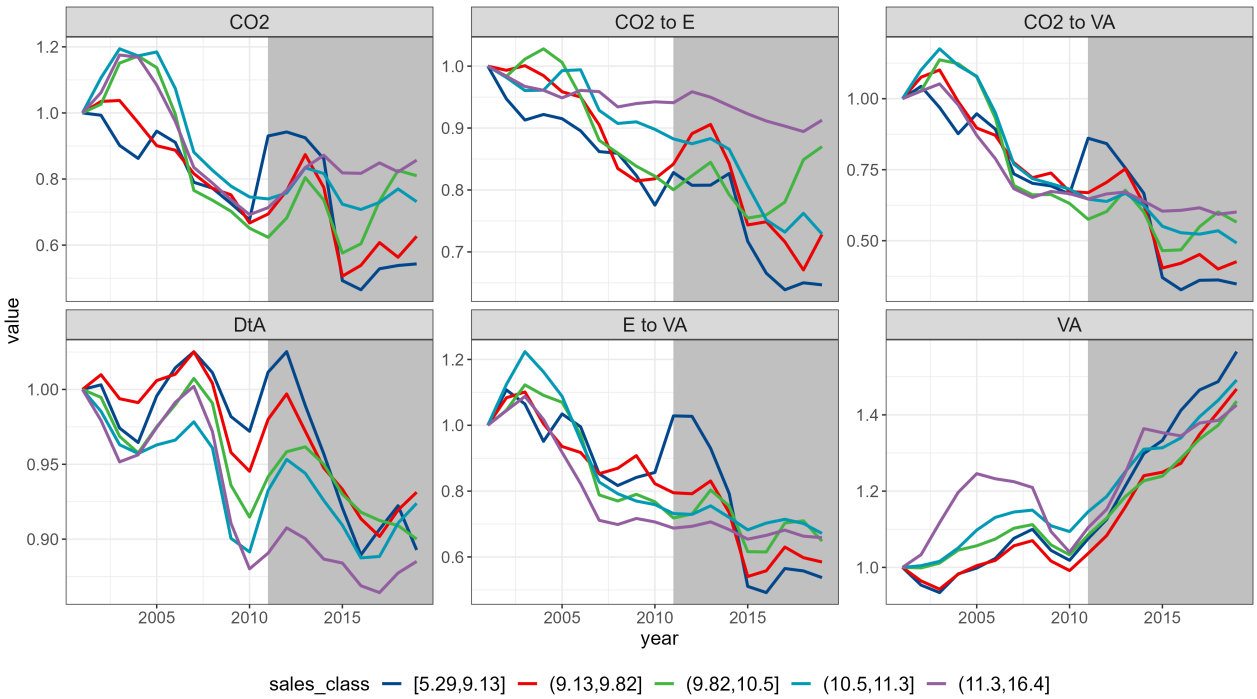
Table 2: Summary statistics of key variables, conditional on size class.

Size class	DtA	DtE	GOP	CO_2	CO_2-to-VA	CO_2-to-E	E-to-VA	VA	%ELE
$Size_1$	0.5681	1.2100	0.0368	1129	1.2284	86.2240	0.0106	1352	0.5790
	(0.2379)	(1.3817)	(0.3468)	(5684)	(11.9027)	(58.9153)	(0.1048)	(808)	(0.2817)
$Size_2$	0.5450	1.0412	0.0530	2140	1.0435	79.6716	0.0096	2565	0.6014
	(0.2246)	(1.1700)	(0.0930)	(6526)	(3.8472)	(56.4092)	(0.0308)	(1412)	(0.2806)
$Size_3$	0.5306	0.9892	0.0562	4241	1.2478	81.1260	0.0118	4679	0.5844
	(0.2246)	(1.1179)	(0.0945)	(12211)	(9.6565)	(52.7187)	(0.0790)	(2704)	(0.2662)
$Size_4$	0.5214	0.9434	0.0623	8956	1.5295	81.9698	0.0149	9063	0.5707
	(0.2175)	(1.0218)	(0.0914)	(27817)	(14.2070)	(52.0904)	(0.1197)	(5268)	(0.2608)
$Size_5$	0.5088	0.8991	0.0703	77930	4.1723	81.7005	0.0298	44914	0.5506
	(0.2176)	(0.9753)	(0.1020)	(499799)	(161.5556)	(50.0291)	(0.8359)	(114483)	(0.2499)
All	0.5342	1.0132	0.0570	18879	1.8439	82.0736	0.0154	12558	0.5771
	(0.2251)	(1.1423)	(0.0874)	(225200)	(72.6528)	(54.0475)	(0.3813)	(53653)	(0.2682)

Note: For each size class, the first row reports the mean while the second row (in parentheses) reports the standard deviation. Size classes (in logs) are defined as $[5.29, 9.13]$, $(9.13, 9.82]$, $(9.82, 10.5]$, $(10.5, 11.3]$, $(11.3, 16.4]$. CO_2 is measured in kilotonnes; $CO2/E$ in kilotonnes to kilowatts; E/VA in in kilowatts to Mln €; VA in Mln €.

of energy. This is due to the fact that the most emission intensive sectors (e.g. basic metals, refineries) are characterized by substantial economies of scale and, consequently, larger firms. Interestingly, the dispersion of CO_2 emissions per value added within each size quintile is very large, especially so at the top and at the bottom of the firm's size distribution. Finally, the reliance on electricity as a source of energy is not clearly related to firm's size.

Figure 1: Time series of main variables by size class.



Note: Variables have been normalized to one at 2000 (by size class). The grey area highlights the post-ZLB period.

5 Results

5.1 The average effect of credit easing on emissions

Table 3 presents the first set of results. The negative estimate in the first column ($ihs(CO_2)$) suggests that the ZLB effect has been stronger for firms with tighter financial constraints before the shock. More precisely, for an additional percentage point of the DtA ratio, firms' emissions declined by 0.45% more.[25] Moreover, the intensity of emissions ($ihs(CO_2/VA)$) has also declined signalling that the reduction in emissions is not due to the fact that more leveraged companies have been growing less. Looking at the last column ($ihs(VA)$), we find that looser financial constraints significantly support firms which are more financially fragile (by 0.076%). This implies that the scale effect is positive, as expected, and coherently with the negative effect observed for the first column, we find that the technology effect is negative and larger (in absolute value). In turn, columns ($ihs(CO_2/E)$) and ($ihs(E/VA)$) suggest that after the credit easing, more constrained firms limit their emissions thanks to reduction of the carbon content of energy and of energy intensity.

To better quantify the magnitude of all the effects, Table 3 reports the average of the dependent variable before the shock ($2000-2011$), the marginal effect for one interquartile range difference in DtA. Hence, our results suggest that after the ZLB shock a firm located at the 3^{rd} quartile of the DtA distribution reduces CO_2 emissions about 9.52% more than a firm located at the 1^{st} quartile, notwithstanding it grows 2.06% more.

Table 3: Baseline results on the effects of monetary policy on emissions.

	Dependent Variable:				
	$ihs(CO_2)$	$ihs(CO_2/VA)$	$ihs(CO_2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtA_{pre} \times ZLB$	-0.450^{***}	-0.525^{***}	-0.297^{**}	-0.228^{***}	0.076^{**}
	(0.147)	(0.148)	(0.125)	(0.052)	(0.037)
Mean pre-ZLB	1748.22 kt	0.19 kt/Mln	39.30 kt/kWh	0.005 kWh/Mln	9217.82 Mln
ΔIQ effect	-9.52%	-10.74%	-6.75%	-5.40%	2.06%
N. obs.	69,248	69,248	69,248	69,248	69,248
N. firms	10,536	10,536	10,536	10,536	10,536

Note: DtA_{pre} is the debt-to-asset ratio before the policy change. Fixed effects by firm, time-by-sector, time-by-ETS phase, and time-by-decile of electricity share pre-ZLB are included. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

[25] The regression is log-linear and our estimates must be interpreted as semi-elasticities.

5.1.1 Heterogeneity: the role of timing

Although the average effect is often the parameter of most interest, we acknowledge that it could hide differences over the time domain. An event study allows us to uncover the timing of the effect as well as to spot potential violation of the parallel trend assumption. Results are reported in Figure 2.

We find that for years before 2012 the coefficients are not statistically significant for CO_2 emissions (top-left), carbon content of energy (top-right) and energy intensity (bottom-left).[26] We conclude that the net effect and the technology effect do not display evidence for pre-trends before the ZLB hit. For value added (bottom-right) differences between more or less financially constrained companies appear to be present well before the ZLB instead. However, the direction of the pre-trend is opposite in comparison to the effect after the shock signalling that the ZLB had a strong enough effect, able to invert a pre-existing trend.

The event study also reveals interesting features about the dynamics of the effects. More precisely, we find that the scale effect appears almost immediately, with more financially fragile companies that can rapidly benefit from the new availability of financial resources. Differently, the technology effect takes effect slowly with carbon content and energy intensity becoming significant after 2 years. As a consequence, also the reduction of CO_2 takes about 2-3 years to become significant. We argue that such results are reasonable because in order to abate emissions or to reduce the amount of energy input firms need to carry out some physical investment that might ask for construction time (see Amendola and Gaffard, 2014) while firms can scale up production thanks to capacity underutilization in normal times.

5.1.2 Heterogeneity: the role of firm size

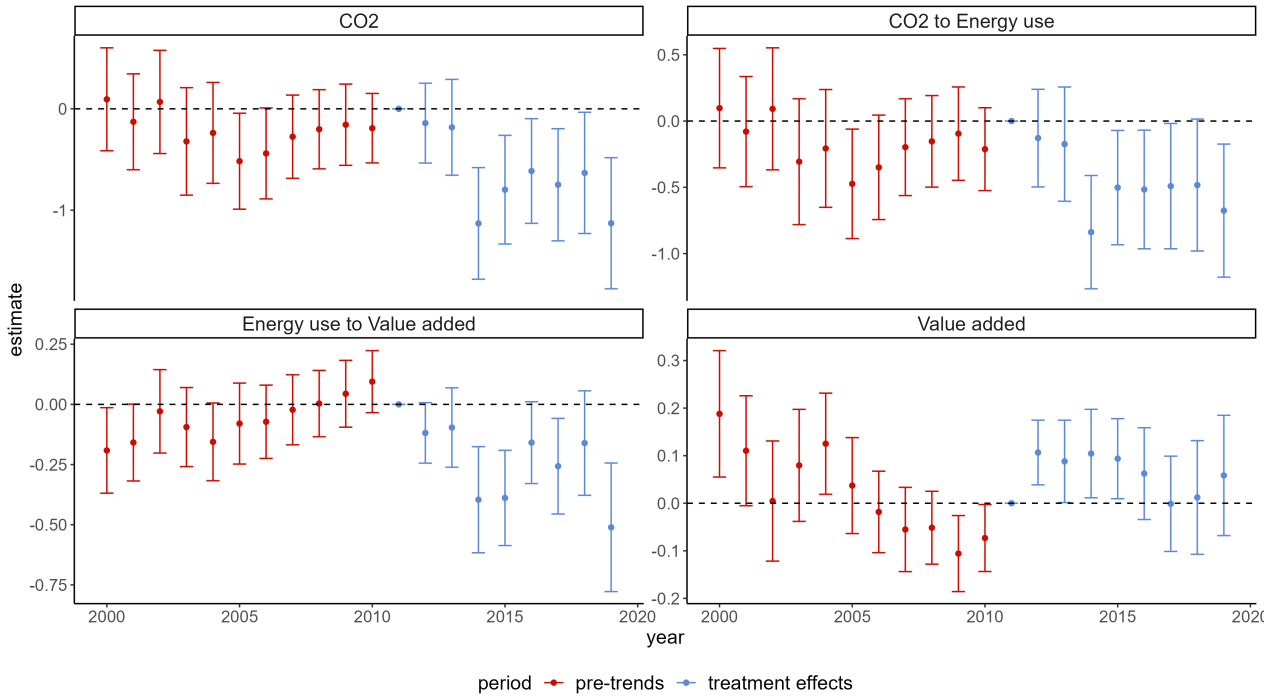
We carry out a second exercise to better understand the heterogeneity around the average effect, in particular with the aim of understanding whether firms' size mediate the impact of financial constraints. Firm's size could be a relevant source of heterogeneity because for a given DtA ratio larger companies could increase their creditworthiness by pledging an higher amount of collateral.

Table 4 collects the results of a model that regresses our set of dependent variables on a triple interaction between ZLB, DtA and the class size, as measured by the quintile of the average sales before 2012, also controlling for specific pre-trend in each size-class.[27] We find no relevant differences among sales

[26] Only 2005 appears to be slightly significant for the first two variables. Similarly, only 2000 is slightly significant for energy intensity.

[27] The $year \times Size_5$ regressor is omitted and the pre-trend of the largest companies serves as reference point.

Figure 2: Event study estimates for the main outcome variables.



Note: Estimates are normalized to zero at 2011, the last year before the ZLB.

classes before the hit of the ZLB.[28] After the credit easing, instead, we find that the three groups encompassing micro, small and medium-sized enterprises have benefited from the loosening of the financing constraints. The net effect ($ihs(CO_2)$ column) is negative for these groups and larger than the average effect (cfr. Table 3). Once again, the improvement in the environmental performance derives from a negative technique effect, evident from the decline in carbon content and emission intensity for smaller and more financially constrained firms (respectively the $ihs(CO_2/E)$ and $ihs(E/VA)$ columns). As expected, we also find a positive size effect ($ihs(VA)$ column), even if small in magnitude.

Finally, let us point toward the fact that for all variables the magnitude of the effect declines with firm size, being null for the group of largest corporations. We interpret these findings as a confirmation for fact that for big firms financial constraints are looser and the DtA has limited explicative power. As a matter of fact, if we compute the marginal effect for one interquartile range difference in DtA also conditioning on firm size, we find that the ZLB shock led to a decline of CO_2 emissions of about 18%, 12% and 8% respectively for small ($Size_1$), medium-small ($Size_2$) and medium seized firms ($Size_3$) while to an insignificant effect for the two largest size classes.

[28] At the 5% level, only smallest firms were growing at a slightly lower pace in the pre-ZLB period.

Table 4: Estimates for the main outcome variables, conditional on size class.

	Dependent Variable:				
	$ihs(CO_2)$	$ihs(CO_2/VA)$	$ihs(CO_2/E)$	$ihs(E/VA)$	$ihs(VA)$
$year \times Size_1$	−0.004	0.003	−0.001	0.004	−0.007**
	(0.011)	(0.011)	(0.010)	(0.004)	(0.003)
$year \times Size_2$	−0.018*	−0.015	−0.014	−0.001	−0.003
	(0.010)	(0.010)	(0.009)	(0.003)	(0.003)
$year \times Size_3$	0.000	0.004	0.004	0.000	−0.004
	(0.010)	(0.010)	(0.009)	(0.003)	(0.003)
$year \times Size_4$	0.000	0.000	0.000	0.000	0.000
	(0.009)	(0.009)	(0.008)	(0.003)	(0.003)
$DtA_{pre} \times ZLB \times Size_1$	−1.039***	−1.241***	−0.856***	−0.386***	0.202***
	(0.189)	(0.190)	(0.159)	(0.071)	(0.041)
$DtA_{pre} \times ZLB \times Size_2$	−0.588***	−0.727***	−0.344**	−0.383***	0.140***
	(0.200)	(0.201)	(0.172)	(0.067)	(0.039)
$DtA_{pre} \times ZLB \times Size_3$	−0.414**	−0.471**	−0.294*	−0.177***	0.057
	(0.200)	(0.202)	(0.174)	(0.067)	(0.045)
$DtA_{pre} \times ZLB \times Size_4$	−0.099	−0.164	−0.013	−0.150**	0.065
	(0.179)	(0.181)	(0.156)	(0.062)	(0.045)
$DtA_{pre} \times ZLB \times Size_5$	0.103	0.212	0.202	0.010	−0.109**
	(0.179)	(0.182)	(0.155)	(0.066)	(0.051)
N. obs.	69,248	69,248	69,248	69,248	69,248
N. firms	10,536	10,536	10,536	10,536	10,536

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

5.2 Mechanisms: energy mix, green investment and productivity

We now proceed to investigate the possible mechanisms through which the more financially constrained firms take advantage of the easier credit supply and reduce their carbon emissions.

We focus on a set of transmission mechanisms that we can measure from our database and that correspond to alternative abatement strategies. These correspond to changes in the energy mix toward cleaner energy, investment in green technologies, capital deepening, and productivity improvements. In particular, we measure the first channel by means of the share of electricity use; the second channel by a dummy variable capturing the installation of self-generation of clean energy (i.e., installation of co-generators or photovoltaic panels);[29] capital deepening is measured by the capital-labour ratio; finally, productivity is proxied by both apparent labour productivity (ALP) and total factor productivity (TFP).[30]

[29]The *EACEI* survey provides this kind of information and we thus exploit a dummy variables to investigate if financially constrained firms become more likely to self-produce electricity after the shock. For simplicity and coherence with all the other regressions, we estimate a simple Linear Probability Model.

[30]TFP has been estimated with the Wooldridge (2009) method using the `prodest` R library (see Rovigatti, 2017).

Table 5: Estimates for the transmission mechanisms.

	Dependent Variable:				
	$\%Electricity$	$SelfGeneration$	$ihs(K/L)$	$ihs(ALP)$	$ihs(TFP)$
$DtA_{pre} \times ZLB$	−0.014	0.045**	0.372***	0.188***	−0.002
	(0.012)	(0.018)	(0.069)	(0.030)	(0.004)
Mean pre-ZLB	0.57%	0.04%	12.09	42.22	10.69
ΔIQ effect	-0.36%	1.19%	9.79%	4.94%	-0.07%
N. obs.	68,203	69,248	69,248	69,248	69,248
N. firms	10,469	10,536	10,536	10,536	10,536

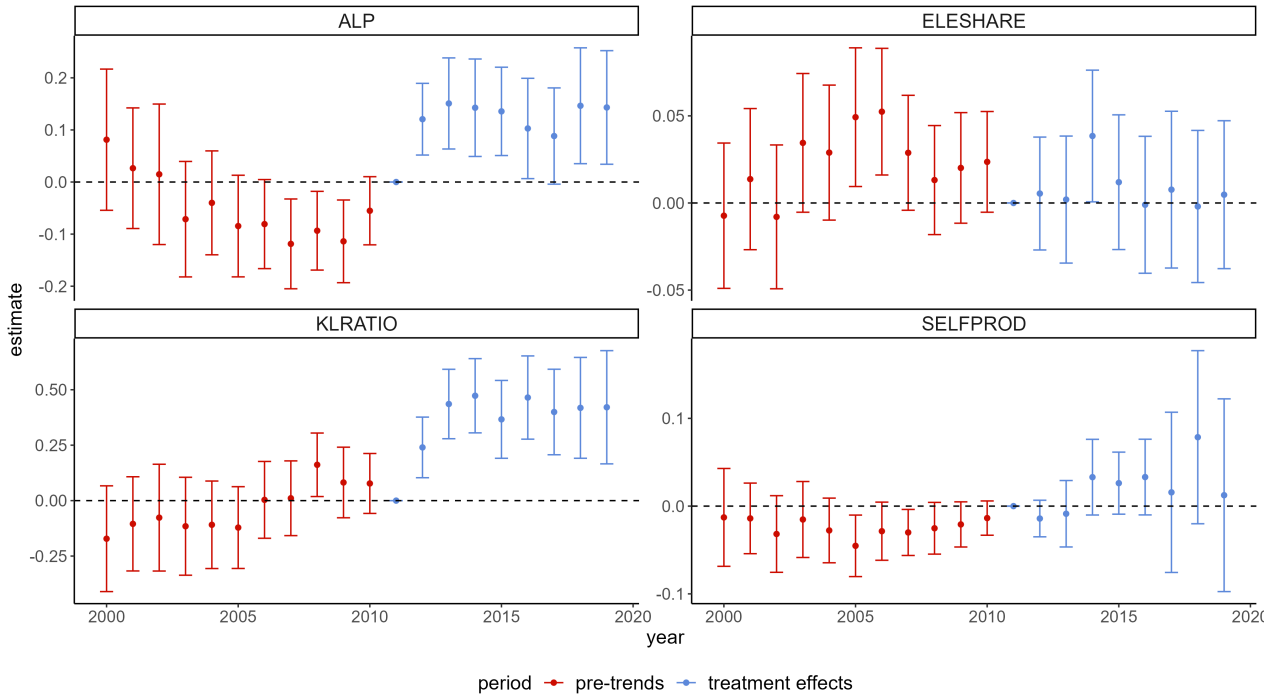
Note: DtA_{pre} is the debt-to-asset ratio before the policy change. Fixed effects by firm, time-by-sector, time-by-ETS phase, and time-by-decile of electricity share pre-ZLB are included. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 5 presents the results. We find no significant impact on the share of electricity ($\%Ele.$ column), implying that the effect on the carbon content of production previously estimated (cfr. Table 3) is possibly given by a shift in the energy mix from carbon to gas. The estimates for investment in self-generation technologies and capital deepening suggest that the ZLB led more financially constrained firms to invest more in these dimensions. Such a result is key to explain the source from which our baseline results have been derived. Loosening the credit supply led more financially constrained companies to adopt co-generation and solar-panels techniques and to update and renew their stock of capital.Finally, also the productivity channel seems to provide some (although weak) evidence suggesting that more financially fragile companies have become more productive – when considering labour productivity, but not in terms of TFP – with respect to more financially sound ones.

5.2.1 Heterogeneity: the role of timing

We proceed by estimating the transmission mechanisms equations also taking into account the heterogeneity of firm size. Results are presented in Table 6 and, in accordance with the main decomposition, suggest that small companies are those that benefit the most after the hit of the zero lower bound. The estimates suggests that the easier credit conditions lead to higher investment into self production of energy for more financially fragile firms only if they are relatively small (i.e., into Q1 and Q2). Interestingly, we find instead that the credit easing helped financially constrained firms to increase their capital-labour ratio and their labour productivity more than the unconstrained peers, whatever the size of the firm.

Figure 3: Event study estimates for the mechanism outcome variables.



Note: Estimates are normalized to zero at 2011, the last year before the ZLB.

5.2.2 Heterogeneity: the role of firm size

5.3 Policy: financing constraints under the EU-ETS framework

The last set of results already provided some information which could be useful for policy makers to target credit policies toward specific groups of firms. However, in this section we carry out a more detailed policy evaluation exercise. In particular, we test the existence of a possible interaction between the credit easing policies with the EU-ETS environmental policy. We thus estimate equation 10.[31]

Results, presented in Table 7, point toward the presence of a very mild difference between ETS treated and non-ETS matched companies, as most of the parameters associated to the triple interactions are non significant. More precisely, this difference seems to be relevant only in terms of emission intensity (as measured by the CO_2 to value added ratio). In particular, all the signs of the estimated coefficient would have indicated that, given the same level of DtA, a credit easing shock leads the ETS treated firms to react more (by decreasing the emissions levels, the emission intensity and the energy intensity) rather than their non-ETS matched twins. However, all coefficients are not significant at the 5% level, indicating that the interaction between credit easing and the EU-ETS environmental policy is weak.[32]

[31]Details about the quality of the matching protocol and on the balancing of the subsample are detailed in Appendix C.
[32]One shall also consider that these regressions are estimated on a small sample of 103 treated and 103 matched companies,

Table 6: Estimates for the mechanisms outcome variables, conditional on size class

	Dependent Variable:				
	$\%Electricity$	$SelfGeneration$	$ihs(K/L)$	$ihs(ALP)$	$ihs(TFP)$
$year \times Size_1$	−0.001	0.009***	−0.014***	−0.003	0.000
	(0.001)	(0.001)	(0.005)	(0.002)	(0.000)
$year \times Size_2$	−0.002**	0.010***	−0.007*	−0.003	0.000
	(0.001)	(0.001)	(0.004)	(0.002)	(0.000)
$year \times Size_3$	−0.002***	0.007***	−0.006	−0.001	0.000
	(0.001)	(0.001)	(0.004)	(0.002)	(0.000)
$year \times Size_4$	−0.002***	0.003**	−0.005	0.000	0.000
	(0.001)	(0.001)	(0.004)	(0.002)	(0.000)
$DtA_{pre} \times ZLB \times Size_1$	0.013	0.041**	0.529***	0.233***	0.011**
	(0.017)	(0.017)	(0.090)	(0.036)	(0.006)
$DtA_{pre} \times ZLB \times Size_2$	−0.016	0.040**	0.378***	0.250***	0.006
	(0.015)	(0.017)	(0.079)	(0.032)	(0.005)
$DtA_{pre} \times ZLB \times Size_3$	−0.012	0.023	0.367***	0.134***	−0.007
	(0.015)	(0.019)	(0.079)	(0.036)	(0.005)
$DtA_{pre} \times ZLB \times Size_4$	−0.011	0.054**	0.407***	0.197***	−0.005
	(0.014)	(0.022)	(0.085)	(0.037)	(0.006)
$DtA_{pre} \times ZLB \times Size_5$	−0.049***	0.031	0.185**	0.113**	−0.019***
	(0.014)	(0.027)	(0.093)	(0.045)	(0.006)
N. obs.	68,203	69,248	69,248	69,248	69,248
N. firms	10,469	10,536	10,536	10,536	10,536

Note: DtA_{pre} is the debt-to-asset ratio before the policy change. The $year \times Size_5$ regressor is omitted because of perfect Fixed effects by firm, time-by-sector, time-by-ETS phase, and time-by-decile of electricity share pre-ZLB are included. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 7: Estimates of the policy regression model when financial fragility is measured by DtA ratio.

	Dep. Variable				
	$ihs(CO_2)$	$ihs(CO_2/VA)$	$ihs(CO_2/E)$	$ihs(E/VA)$	$ihs(VA)$
$ETS \times ZLB$	0.353	0.069	−0.017	0.086	0.284
	(0.524)	(0.520)	(0.442)	(0.221)	(0.228)
$DtA \times ZLB$	0.001	−0.471	−0.303	−0.168	0.471
	(0.707)	(0.798)	(0.639)	(0.339)	(0.321)
$DtA \times ZLB \times ETS$	−0.507	−0.380	−0.073	−0.307	−0.127
	(0.940)	(0.984)	(0.828)	(0.415)	(0.375)
N. obs.	2,375	2,375	2,375	2,375	2,375
N. firms	199	199	199	199	199

Note: DtA_{pre} is the debt-to-asset ratio before the policy change. Fixed effects by firm, time-by-sector, and time-by-decile of electricity share pre-ZLB are included. Standard errors are clustered at the firm level. * p < 0.1, ** p < 0.05, *** p < 0.01.

To better highlight the differences between subgroups of firms, we also re-estimate the baseline regression (see equation 7) using the three splitted samples comprising respectively ETS firms, non-ETS

causing the standard errors to be large.

matched and non-ETS unmatched firms. Results for the three groups are reported in Table 8.

Table 8: Estimates of the baseline regression model for the different subsamples defined by the ETS.

Panel A: *ETS treated firms*					
			Dep. Variable		
	$ihs(CO_2)$	$ihs(CO_2/VA)$	$ihs(CO_2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtA \times ZLB$	−0.593	−1.041**	−0.559	−0.482	0.447*
	(0.472)	(0.495)	(0.389)	(0.297)	(0.227)
Mean pre-ZLB	212654.42 kt	6.34 kt/Mln	101.99 kt/KWh	0.062 KWh/Mln	33559.28 Mln
ΔIQ effect	-10.86%	-15.69%	-10.38%	-9.28%	13.68%
R^2 Adj.	0.657	0.662	0.531	0.877	0.911
Panel B: *non-ETS matched firms*					
			Dep. Variable		
	$ihs(CO_2)$	$ihs(CO_2/VA)$	$ihs(CO_2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtA \times ZLB$	−0.113	−0.593	−0.432	−0.162	0.480
	(0.801)	(1.039)	(0.755)	(0.354)	(0.411)
Mean pre-ZLB	118846.36 kt	4.42 kt/Mln	131.58 kt/KW	0.034 KW/Mln	26911.02 Mln
ΔIQ effect	-3.11%	-13.03%	-10.21%	-4.35%	17.95%
R^2 Adj.	0.860	0.881	0.668	0.933	0.911
Panel C: *non-ETS unmatched firms*					
			Dep. Variable		
	$ihs(CO_2)$	$ihs(CO_2/VA)$	$ihs(CO_2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtA \times ZLB$	−0.431***	−0.491***	−0.278**	−0.213***	0.060
	(0.151)	(0.154)	(0.130)	(0.054)	(0.038)
Mean pre-ZLB	938.23 kt	0.17 kt/Mln	42.90 kt/KW	0.004 KW/Mln	5382.29 Mln
ΔIQ effect	-9.17%	-10.16%	-6.36%	-5.02%	1.62%
R^2 Adj.	0.658	0.594	0.561	0.764	0.916

We find some differences between the whole sample estimates and the three sub-groups. First, for matched companies (middle panel), none of the coefficient is significant, indicating that the financial constraints loosening did not affect their average emissions, their carbon content of production, their energy efficiency, nor their scale of activity. Second, we find instead that after the ZLB shock, most indebted treated firms (top panel) did reduce their carbon intensity more (i.e., they reduced the $\frac{CO_2}{VA}$ ratio) and they did so by improving their energy efficiency ($\frac{E}{VA}$). These last results are significant at the 10% but the statistical power in this sample is reduced due to the lower number of observations. The fact that the results are however significant for treated companies but not for the matched ones might highlight the fact that these firms have more incentives to reduce their emission intensity since they bear higher costs per unit of emissions. Furthermore, we also argue that the small significance of these results can

be also due to the very large size of treated and matched companies with respect to all other firms in the sample, which might partially offset their financial fragility when looking out for financial resources. In particular, in light of the results presented in Section 5.1.2, we argue that for a given level of DtA the financial constraints of these firms are substantially less stringent vis-à-vis the constraints experienced by the small firms. The third set of results (bottom panel) on the unmatched companies, also provide evidence for such an interpretation. The subsample of firms which are neither treated, nor matched are the closest to the estimates carried out on the full sample.

5.4 Robustness checks

In this section we discuss several alternative models that we have estimated with the aim of testing the robustness of our main estimates with respect to the different modelling choices taken. In particular, we divide the modelling choices into five categories concerning (i) the measures of the exposure to the monetary policy shock, that provide a proxy of credit constraints tightness; (ii) the most appropriate measure that captures a credible variation in the ECB monetary policy stance; (iii) the transformation to be applied to the dependent variables, since there are many zeros that might affect the results; (iv) the inclusion of potentially relevant omitted variables; (v) the matching algorithm used to create a balanced subsample of ETS treated and non-ETS matched companies.

In what follows we briefly discuss about the alternatives and the results obtained. All the regressions tables are outlined in Appendix D.

Alternative measures of the exposure to the monetary policy shock. As alternative to the debt-to-assets ratio, we also employ the debt-to-equity ratio or the gross operating profits ratio (gross operating profits over sales). The rationale behind the usage of average DtE in the years before the arrival of the policy shock, is close to choice behind the use of the DtA. Both these measures account for the outstanding debt that a firm already had before entering into the period of easy credit. A firm with a large amount of debt is expected to have less "credit space" and it is more likely to be financially constrained. The unique difference is related to the denominator, which in the main analysis (DtA) captures the total size of a firm's balance sheet, while in the DtE robustness, it only accounts for private capital. In a different fashion, the rationale for using the GOP is that a firm with a large amount of operating profits is able to raise an higher amount of internal liquidity. In line with the pecking order theory, such a firm can use the internal liquid resources to finance new investment plans without resorting to external debt – the more costly financing alternative. Accordingly, a firm with a higher value of pre-shock GOP is less financially

constrained.

Appendix D.1 presents the estimates of the baseline regressions (Tables 13, and 14), of the heterogeneity regressions (Tables 15, and 16), of the mechanisms regressions (Tables 17, and 18), and of the mechanisms with heterogeneity regressions (Tables 19, and 20). Although the magnitude of the coefficients could change from one exposure measure to the other, as well as the marginal interquartile effects, the coefficients signs and significances are consistent with the main results outlined earlier in the paper according to which, financially constrained firms have benefited the most from the ease of credit conditions both from the economic and environmental viewpoints.

Alternative monetary policy shocks. One potential limitation to our measure of the credit easing shock (the hit of the ZLB) could be the fact that the theoretical equilibrium of the nominal interest rate after 2012 would have been negative. In this case, setting an interest rate equal to zero would imply a positive gap between the nominal interest rate and the equilibrium rate, indicating a policy which was indeed expansionary, but not sufficient to lead to the equilibrium amount of credit. Although we discussed at length the reasons behind this choice from a theoretical point of view, to overcome this possible limitation we also estimate the model with two alternative shocks. First an unconventional monetary policy (namely the Asset Purchase Programme by the ECB) that was implemented under the governance of Mario Draghi starting from 2015. In this alternative setting, we vary the arrival time of the shock to 2015 – while it was 2012 under the ZLB. Secondly, we also try to model a continuous variation in the nominal interest rate to overcome the potential criti que toward a dummy variable measures of the monetary policy. We thus use the EONIA interest rate, which is the interbank rate and form the basis for the formation of many bank lending costs.

Appendix D.2 presents the baseline estimates when using either the APP (Table 21) or the EONIA Table 22 for the EONIA) as well as the heterogeneity estimates for APP and EONIA (respectively in Tables 23 and 24). All the results for the APP monetary policy shock are comparable to the baseline ones in terms of magnitude and statistical significance, whereas the results on the continuous measure are similar in terms of environmental performance, while different in terms of the effect on value-added. This difference, however, could be rationalized by two considerations. First, because the interest rate is a continuous measure of the monetary policy, we had to accommodate the regression setting and we cannot use the pre-shock average. For this reason the DtA variable is measured as the contemporaneous debt-to-asset ratio and therefore varies both across firms, and over time (that's why also is standalone coefficient appears in the results in Table 22). Second, differently from hitting the ZLB in a year in which

also the "whatever it takes" speech has been addressed, a single year-to-year variation in the interest rate does not represent a credible long-term commitment for easier credit conditions, we thus believe that every single change in the interest rate is less of a signal for the shock that we would like to capture in this work. All in all, we consider the results to be satisfactory.

Logarithmic transformation. Recent contributions by Aihounton and Henningsen (2021) and Cole et al. (2024) cast doubts on the safe use of the ihs transformation. For this reason, we also re-estimate all our baseline models using different transformations. We first use the $log(y + \epsilon) * k$, with $\epsilon = 0.01$ and $k = 1000$ in order to (i) preserve the zero-valued observations and (ii) move to the right on the logarithmic function. Secondly, we also use the simple $log(y)$ which, however, reduces the sample size by purging all the zero-valued observations. From a theoretical viewpoint, the choice among the three alternative transformations (i.e., ihs, log^+ and log) depends on the problem to be analyzed. As a matter of fact the ihs might be problematic whenever the zero-valued observations indicate that an individual did not enter into some activity (i.e. an extensive margin discontinuity). In our situation this is not the case because a zero-valued CO_2 emissions derive from the energy mix strategies employed by firms and the discontinuity does not denote a particular firm choice to engage or do not engage in some peculiar activities.

Appendix D.3 presents the baseline estimates when using either the log^+ (Table 25) or the log (Table 26 transformations. While the log^+ transformation delivers results which are equivalent to the preferred specification in terms of sign and significance of the parameters, the results are less robust to the simple log transformation because even if all the signs are preserved, the statistical significance remains valid for value added and for energy efficiency (where the zero-valued observations are fewer) but is lost for regressions in which CO_2 emissions is on the dependent variable, also because approximately 10% of the original sample is dropped.

Alternative specifications. The final robustness checks contain a series of test to study whether the results are driven by specific years (i.e., the early 2000's), specific sectors (i.e., the energy intensive ones) or whether there is some form of omitted variable bias (due to differences in capital intensity or productivity).

Appendix D.4 presents the Kaya-type decomposition results for these settings. First, Table 27 excludes from the sample the first four years (i.e., the time span covers only $2004 - -2019$). Results are robust to such a specification. Second, Table 28 is carried out considering only the 2 digits industries for which

energy intensity is stronger. All signs are preserved and statistical significance holds for energy intensity and value added. Statistical significance is lost instead for the regressions for which CO_2 emissions is a dependent variable. Third, Table 29 presents the results when including also three continuous variables measuring (i) total sales, (ii) the capital-labor ratio, and (iii) the apparent labor productivity.[33] We first highlight that the main effect of interest is barely affected by these inclusions, indicating that the OVB concerns are minor. Finally, we also record that the three controls, present the expected signs.

Alternative matching strategy: the Coarsened Exact Matching (CEM). The results presented in Section 5.3, related to the interaction between credit easing and environmental policies might depend on the choice of the chosen matching algorithm. To test our model, we employ as a robustness check the Coarsened Exact Matching (CEM) algorithm by Iacus et al. (2012), which is an exact matching protocol and applies multidimensional binning to find companies similar to the treated ones. Also in this case, we use a 1-to-1 matching strategy.

Appendix D.5 provides the results of the ETS policy augmented regression estimated over the sample constructed using the CEM algorithm. Resuls are summarized in Table 30 and we simply record that the implications drawn are consistent with the ones highlighted by the main policy exercise presented in Section 5.3.

6 Counterfactual Analysis

By way of conclusions, we finalize our work with an estimation of the aggregate amount of CO_2 that has been saved, thanks to the credit easing dictated by the zero nominal interest rates. It is worth mentioning that such an exercise only represents a partial equilibrium result, because we do not model the possible behavioral responses and the adaptation of the agents due to the new policy stance adopted by the ECB. As such, it shall be taken as a simple counterfactual scenario of "comparative statics". Nonetheless, it could be informative for policymakers to gauge some preliminary indication on the impact of monetary policy on carbon emissions.

Starting from the estimates obtained in our event-study setting, we compute the emissions savings

[33] We here include sales as a continuous variable rather than transforming it into a discrete variable, as determined by the quintiles, as done in all the heterogeneity analysis throughout the paper. Results are aligned.

($\Delta\widehat{CO}_{2i}$) for the generic firm i as:

$$\Delta\widehat{CO}_{2i} = CO_{2i}(ZLB_t = 1) - \widehat{CO}_{2i}(ZLB_t = 0) = \sum_{\tau=2011}^{2019} \hat{\beta}_\tau DTA_i \ , \tag{11}$$

where β_τ are the coefficients estimated from Equation 8 and presented in Figure 2, while DTA_i is the pre-shock average debt-to-assets ratio of the company.[34]

We then aggregate by simply summing over the firm dimension:

$$\Delta\widehat{CO}_2 = \sum_{i=1}^{N} \sum_{\tau=2011}^{2019} \hat{\beta}_\tau DTA_i \ . \tag{12}$$

We estimate that about $20Mt$ of CO_2 has not been emitted thanks to the ZLB.

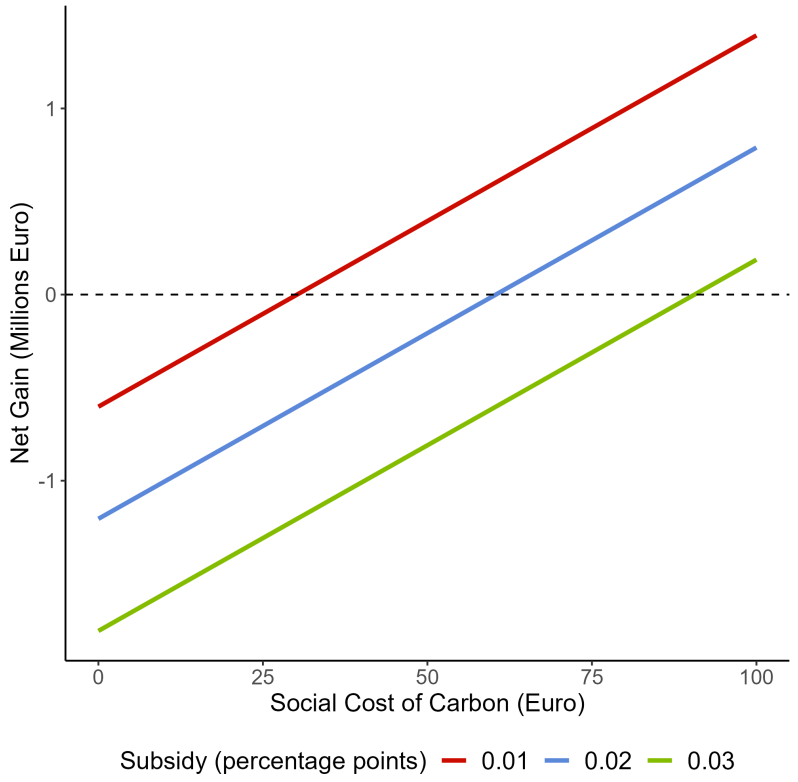
At last, we carry out a simple cost-benefit analysis building upon this counterfactual. Let us make three assumptions. First, that the benefit is measured by the amount of CO_2 saved, priced at the value of the social cost of carbon. Second, that the cost is measured by the interests paid on debt. Third, that a certain amount of the interests costs are borne by a government under the form of a subsidy. With these assumptions at hand – and using the aggregate debt stock before the ZLB by averaging for each firm the outstanding debt over the 2000-2011 period and them summing the firm-specific average debt across firms – we can carry out our simple cost-benefit analysis. The net benefit writes:

$$\text{Net Benefit}(P_{scc}, r_s) = P_{scc}\Delta\widehat{CO}_2 - r_s Debt \tag{13}$$

where $\Delta\widehat{CO}_2$ is the total amount of carbon saved, as previously estimated, P_{scc} is the social cost of carbon and r_s is the part of the interest rate covered by a government subsidy. This is equivalent to a guarantee on the firm's debt equal to a share r_s of the total debt. Since there is uncertainty concerning the social cost of carbon, we leave this number as a free parameter and we compute the net benefit as a function of P_{scc}. Similarly, r can be considered as a policy variable, to be chosen by a policymaker. Figure 4 provides therefore the net benefit as a function of P_{scc}, conditional upon three different values of r_s.

[34]Following Colmer et al. (2024), we set to zero all the non-significant parameters of the event study.

Figure 4: Aggregate carbon savings as a function of P_{scc} and for different values of r_s.



7 Conclusions

Climate policies increase company's energy cost in the short-term. In turn, the long-term effect depends on the firm's capacity to successfully adapt by changing the input mix and investing in carbon-saving technologies. However, the incidence of higher costs is not homogeneous across firms. Financially constrained firms will find it more difficult to invest in new equipment and skills that improve the carbon content of production. This paper investigates on the role that financial constraints play in the green transition. We rely on a unique database on French manufacturing companies containing detailed information on carbon emission, exposure to policies (such as the EU-ETS) and balance sheet data. We build reliable indicators of the extent to which firms are financially constrained (such as the debt-to-asset ratio and the debt-to-equity ratios) and we exploit either exogenous monetary policy shocks or energy price shocks in order to study how firm specific carbon emissions react, as a function of the degree of financial constraints.

Our findings suggest that firms in the sample are heterogeneously hit by these shocks. In particular, financially fragile and credit constrained companies are likely to have benefited the most from the relaxation of financial constraints that happened in 2011 due to the zero-lower bound monetary shock.

Financially fragile companies have reacted by reducing CO_2 emissions more, vis-à-vis their more robust peers. They have done so, notwithstanding the presence of a scale effect, which allowed them to increase their value added more then the unconstrained competitors. Our estimates suggest that the reduction in CO_2 emissions is due both to a reduction in the carbon content of energy use and to improved energy efficiency. Similar results are found also in presence of exogenous energy price shock. In particular, we find the elasticity of CO_2 emissions to energy price to be about -1.5.

Our estimates also suggest that the relaxation of financing constraints was stronger for smaller firms, not subject to the ETS policy. In fact, our estimates for larger corporations (treated or not to the EU-ETS) suggest that financial constraints matter only to a less extent, and the coefficients are only occasionally significant. Since the EU-ETS policy has been established in 2005, we argue that this last result might also derive from the fact that these companies have started their campaign to reduce CO_2 emissions already before 2011, when the expansionary monetary policy shock loosened financing constraints.

Our results have one important policy implications for the green transition. In particular, they might imply that a policy which tightens credit conditions and increases financial constraints for firms – e.g., abrupt interest rate hikes – might reduce the ability of the manufacturing companies to adapt their production processes, possibly slowing down the green transition.

Acknowledgments. Data access was provided by CASD and authorized by the French comité du secret statistique, project GreenFiT. We thank Natalia Fabra, Marzio Galeotti and Federico Frattini 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, a *FEEM Internal seminar*, a *LSE Grantham seminar*, the 2024 *EEA-ESEM congress* in Rotterdam, and the Bank of Italy *"Workshop on the economic implications of climate change and the transition to a low-carbon economy"*. The usual disclaimer applies.

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Appendix A Additional descriptive statistics

We here report the descriptive statistics for the main variables of interest as a function of financial fragility, as measured by the debt-to-assets ratio (Table 9); and as a function of the ETS status (Table 10), as specified by the matching strategy detailed in Section 3.3 and Appendix C.

Let's start with Table 9. These descriptive statistics highlight several features of the data. First, the three alternative measures of financial fragility satisfy the expected correlations, with the two measures of indebtedness (DtA and DtE) being positively correlated. Furthermore, both measures negatively correlate with the ability of a firm to generate internal liquidity, as measured by the GOP. Second, financially fragile companies are on average smaller and pollute less, perhaps due to a greener energy mix which can be inferred from the ratio between CO_2 emissions and energy use.

Also moving to Table 10 we find that ETS firms are very different from all the other firms in terms of CO_2 emissions. This is largely expected, since the ETS policy targets the high-polluting firms. However, we find that the matching allows us to minimize the differences between matched companies and ETS treated firms as their indebtedness and size are quite similar. Unmatched companies display a smaller size (VA column), a lower level of emissions (CO2 column), a greener energy mix (CO2-to-E and %ELE columns), and lower energy intensity (E-to-VA column).

Table 9: Summary statistics of main variables, conditional on the DtA class. For each size class, the first row reports the mean, the second row (in parentheses) reports the standard deviation.

DtA class	DtA	DtE	GOP	CO_2	CO_2-to-VA	CO_2-to-E	E-to-VA	VA	%ELE
DtA_1	0.2951	0.3924	0.0971	22085	2.8476	89.5734	0.0192	20402	0.5386
	(0.1232)	(0.3695)	(0.1309)	(202764)	(160.5958)	(53.2102)	(0.8289)	(95210)	(0.2605)
DtA_2	0.4339	0.6398	0.0686	19400	1.3766	83.8967	0.0120	12019	0.5669
	(0.1240)	(0.5445)	(0.0897)	(225830)	(7.1348)	(53.8791)	(0.0970)	(27465)	(0.2675)
DtA_3	0.5296	0.8674	0.0573	11636	1.3803	80.7848	0.0122	11211	0.5845
	(0.1331)	(0.6746)	(0.0857)	(87605)	(8.5226)	(54.1983)	(0.0535)	(23572)	(0.2706)
DtA_4	0.6249	1.1391	0.0405	32111	1.9345	79.7766	0.0167	11693	0.5926
	(0.1426)	(0.8553)	(0.3217)	(386514)	(20.3957)	(54.9652)	(0.1565)	(59607)	(0.2704)
DtA_5	0.7984	2.0720	0.0155	8930	1.6885	76.1523	0.0169	7346	0.6036
	(0.2063)	(1.8323)	(0.0970)	(54367)	(8.5688)	(53.0093)	(0.0592)	(19045)	(0.2673)
$Overall$	0.5342	1.0132	0.0570	18879	1.8439	82.0736	0.0154	12558	0.5771
	(0.2251)	(1.1423)	(0.0874)	(225200)	(72.6528)	(54.0475)	(0.3813)	(53653)	(0.2682)

Table 10: Summary statistics of main variables, conditional on the treatment type. For each size class, the first row reports the mean, the second row (in parentheses) reports the standard deviation.

Treatment	DtA	DtE	GOP	CO_2	CO_2-to-VA	CO_2-to-E	E-to-VA	VA	%ELE
ETS	0.5017	1.0110	0.0965	469750	17.7887	120.9603	0.1191	78040	0.2935
	(0.2166)	(1.1818)	(0.1139)	(1326739)	(50.9567)	(62.2008)	(0.3011)	(265352)	(0.2149)
matched	0.5041	0.9276	0.0657	175036	8.3656	116.7171	0.0626	44278	0.3680
	(0.2202)	(0.9697)	(0.1018)	(688596)	(19.2930)	(48.8523)	(0.1411)	(163427)	(0.2314)
unmatched	0.5353	1.0143	0.0550	6932	1.4085	80.7513	0.0125	10705	0.5861
	(0.2254)	(1.1434)	(0.1745)	(51950)	(73.4489)	(53.4402)	(0.3847)	(31209)	(0.2652)
Overall	0.5342	1.0132	0.0570	18879	1.8439	82.0736	0.0154	12558	0.5771
	(0.2251)	(1.1423)	(0.0874)	(225200)	(72.6528)	(54.0475)	(0.3813)	(53653)	(0.2682)

Appendix B Marginal effects on KLE inputs and on debt

Our general theoretical framework (see Section 2) suggests that the size effect, embeds the marginal effects of financial constraints on the inputs of the production function, namely capital, labor and energy (KLE). We here provide the estimates of these effects.

Coherently with the previous literature (Cingano et al., 2016; Amiti and Weinstein, 2018) our estimates suggest that relaxing the credit constraints induces a capital deepening which is larger for firms with tighter constraints. This could be due to the fact that when credit availability increases, previously constrained firms invest into new capital machines. These machines also appear to be more energy efficient, given the negative sign recorded on the total amount of energy employed by the firm. However, in stark contrast with the previous literature (Chodorow-Reich, 2014), we find that the credit easing brings about a negative effect on the amount of hours worked.

Table 11: Effects on capital, labor and energy use.

	Dep. Variable		
	$ihs(K)$	$ihs(L)$	$ihs(E)$
$DtA \times ZLB$	0.260***	−0.112***	−0.153***
	(0.072)	(0.026)	(0.050)
Mean pre-ZLB	3596.13	198.71	41.50
ΔIQ effect	7.81	-2.79	-3.73
R2 Adj.	0.888	0.928	0.894

Table 12: Effect on debt and indebtedness

	Dep. Variable					
	Debt	Debt	DtA	DtA	DtE	DtE
$DTA \times ZLB$	−0.439***		−0.250***		−0.497***	
	(0.049)		(0.014)		(0.033)	
$DTA \times ZLB \times Size_1$		−0.328***		−0.257***		−0.489***
		(0.054)		(0.016)		(0.040)
$DTA \times ZLB \times Size_2$		−0.363***		−0.245***		−0.441***
		(0.054)		(0.016)		(0.051)
$DTA \times ZLB \times Size_3$		−0.408***		−0.226***		−0.435***
		(0.058)		(0.019)		(0.055)
$DTA \times ZLB \times Size_4$		−0.442***		−0.253***		−0.551***
		(0.057)		(0.017)		(0.051)
$DTA \times ZLB \times Size_5$		−0.713***		−0.265***		−0.592***
		(0.063)		(0.018)		(0.049)
R2 Adj.	0.723	0.724	0.684	0.684	0.469	0.470

Note 1. Fixed effects by firm, by time-sector, by ETS phase, and by time-electricity decile pre-ZLB are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations: 72130. N. of firms: 13401.
Note 4. * p < 0.1, ** p < 0.05, *** p < 0.01.

Appendix C Policy interaction: the EU Emission Trading Scheme

C.1 Institutional background on the EU-ETS

Starting from 2005 (the 1^{st} trading phase of the EU-ETS) all the treated companies have been assigned, by the regulatory authority, a certain amount of emission allowances. Those firms whose CO_2 emissions overcome the initial endowment must buy the additional allowances on the ETS market or, alternatively, pay a fee. As the aver market price for carbon emissions have averaged at around \$21 in the second phase of the ETS, regulated companies have since then experienced an increase in production costs. This mechanism provides the regulated companies with more incentives to abate their emissions vis-à-vis unregulated firms. A-priori, we thus expect that the financially constrained companies which are also regulated could enjoy major benefits from the credit easing. On the other side, we also acknowledge that treated firms are on average very large companies and could be less financially constrained due to the large value of the collateral they could use to borrow even when they are highly leveraged.

C.2 Matching

The main challenge for the identification of the effect of financial constraints easing under ETS regulation – on top of the general ones already discussed in Section 3.2 – resides in the fact that the regulated companies are not a random subsample. The subsamples of treated and untreated firms are ex-ante different. In fact, the EU-ETS regulatory framework targets the most polluting firm, which are on average larger, less financially fragile, more productive, and more capital intensive (see descriptive statistics in Table 10).

To overcome this issue, we follow Rosenbaum and Rubin (1983) by employing a matching algorithm based on the propensity score in order to build a subsample of untreated companies that closely match the treated ones in all those aspects. In particular, the matching algorithm selects a sample of untreated firms such that the distance between the distributions of all the regulated and the selected set of unregulated companies is minimized. We employ a 1-to-1 matching, such that a treated company could be matched by a unique untreated firm. We also add constraint the algorithm with an exact matching for the industry in which the matched and treated firms operate – at the NACE 2-digits level. The characteristics that a matched company shall resemble are: total sales, the DtA ratio, labor productivity and total emissions of CO_2. All of them are calculated as the averages in the years before the introduction of the ETS (i.e., over the $2000 - 2004$ time frame). At the end of the matching process we will have three different groups of companies: ETS treated firms, non-ETS matched firms, and non-ETS unmatched firms.

Figure 5: Standardized mean differences between treated companies and all the untreated ones (in red) and between treated companies and matched ones (in blue) for the different variables. Left Panel: nearest neighbor matching. Right panel: coarsened exact matching. DtA: debt-to-assets; $CO2$: inverse hyperbolic sine of CO_2 emissions; $SALES$: firm size as measured by the inverse hyperbolic sine of sales; ALP: inverse hyperbolic sine of the apparent labor productivity. Remaining variables represent sector specific dummies and they are forced to match exactly.

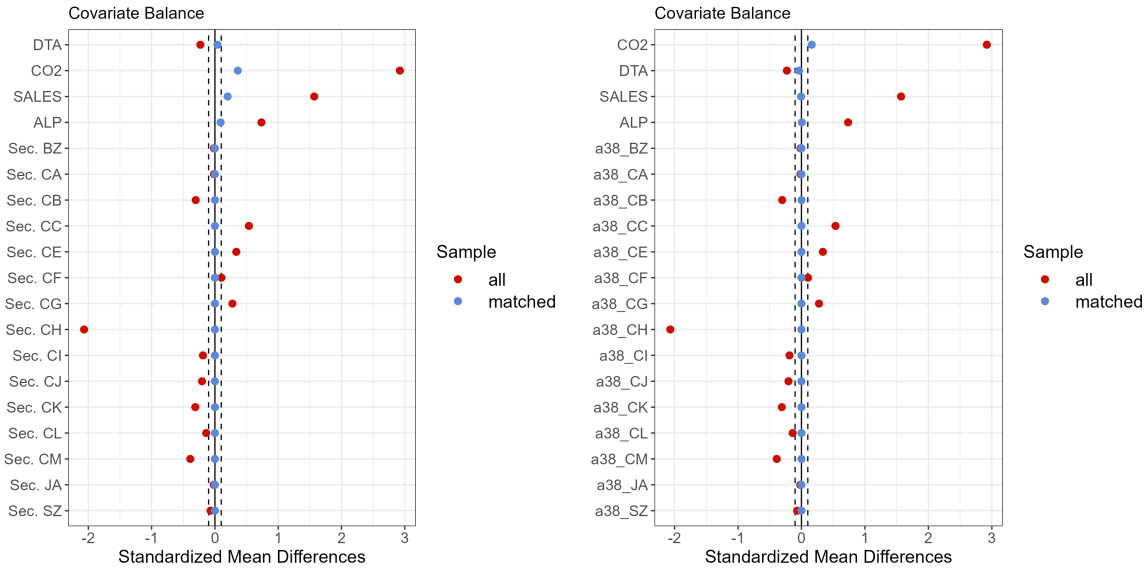
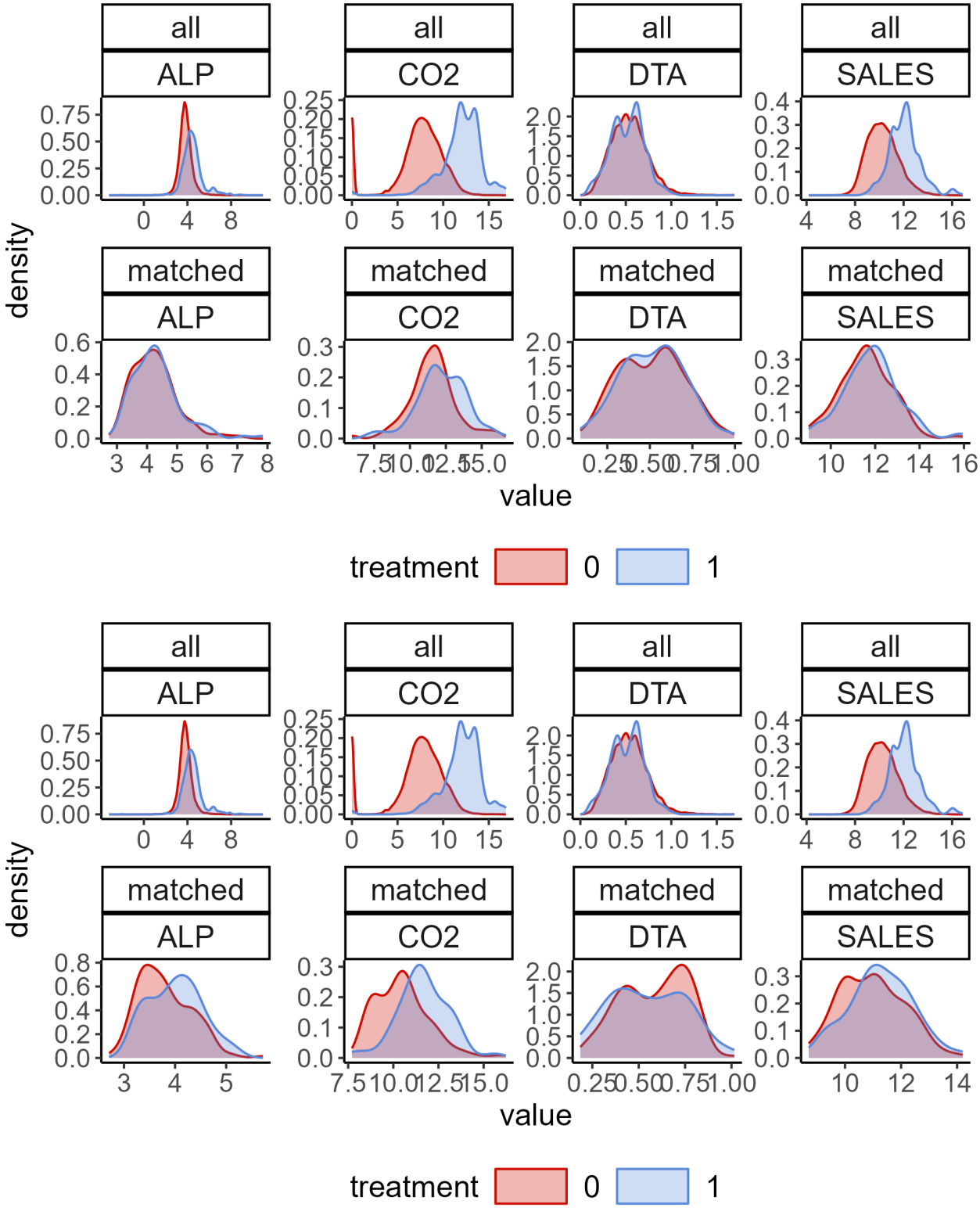


Figure 6: Distribution of the control variables for the matching process. Top-panels: all non-ETS firms (red) vs. ETS treated firms (blue). Bottom-panel: non-ETS matched firms (red) vs. ETS treated firms (blue). DtA: debt-to-assets; $CO2$: inverse hyperbolic sine of CO_2 emissions; $SALES$: firm size as measured by sales; ALP: apparent labor productivity. Top figure: nearest neighbor matching. Bottom figure: coarsened exact matching.



Appendix D Robustness checks

D.1 Alternative exposure to the monetary policy shock

In this sub-section we present the main results when the exposure to the shock is measured either by the Debt-to-Equity ratio or by the Gross-Operating-Profit to sales ratio. We collect the results in the same order as in the main text, for both variables,: (i) baseline regressions (see Tables 13 and 14), (ii) baseline regressions with size class heterogeneity (see Tables 15 and 16), (iii) mechanisms regressions (see Tables 17 and 18), and (iv) mechanisms regressions with size class heterogeneity (see Tables 19 and 20).

These regressions are not discussed further, because the interpretation of the coefficients and the qualitative economic implications are substantially the same of the regressions extensively described within the main text.

Table 13: Estimates of the baseline regression model when financial fragility is measured by the debt-to-equity ratio. All the dependent variables have been transformed with the ihs function.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtE \times ZLB$	-0.092^{***}	-0.107^{***}	-0.073^{**}	-0.034^{***}	0.015^{*}
	(0.034)	(0.035)	(0.029)	(0.013)	(0.009)
Mean pre-ZLB	1748.22 kt	0.19 kt/Mln	39.30 kt/kWh	0.005 kWh/Mln	9217.82 Mln
ΔIQ effect	-5.50	-6.34	-4.37	-2.11	0.93
R^2 Adj.	0.667	0.593	0.537	0.790	0.920

Table 14: Estimates of the baseline regression model when financial fragility is measured by the gross operating profits to sales ratio.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$GOP \times ZLB$	1.158^{***}	0.983^{***}	0.070	0.913^{***}	0.175
	(0.411)	(0.366)	(0.332)	(0.139)	(0.136)
Mean pre-ZLB	1748.22 kt	0.19 kt/Mln	39.30 kt/kWh	0.005 kWh/Mln	9217.82 Mln
ΔIQ effect	23.38%	15.54%	1.54%	11.65%	2.58%
R^2 Adj.	0.667	0.593	0.537	0.791	0.920

Table 15: Estimates of the baseline regression model when accounting for firm heterogeneity as measured by the quintiles of the size distributions. The coefficient to $year \times Size_5$ have been omitted because of multicollinearity, making the largest size class the reference group.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtE \times ZLB \times Size_1$	−0.173***	−0.216***	−0.154***	−0.062***	0.043***
	(0.048)	(0.049)	(0.037)	(0.020)	(0.014)
$DtE \times ZLB \times Size_2$	−0.116	−0.146**	−0.078	−0.068**	0.030**
	(0.072)	(0.073)	(0.057)	(0.027)	(0.013)
$DtE \times ZLB \times Size_3$	−0.120	−0.134*	−0.108	−0.026	0.014
	(0.076)	(0.078)	(0.069)	(0.023)	(0.016)
$DtE \times ZLB \times Size_4$	0.007	−0.006	0.013	−0.019	0.013
	(0.050)	(0.053)	(0.045)	(0.021)	(0.015)
$DtE \times ZLB \times Size_5$	0.075	0.131**	0.085*	0.046**	−0.057***
	(0.054)	(0.056)	(0.048)	(0.021)	(0.017)
R^2 Adj.	0.668	0.594	0.538	0.791	0.920

Note 1. Fixed effects by firm, by time-sector, by ETS phase, and by time-electricity decile pre-ZLB are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations: 69248. N. of firms: 10536.
Note 4. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 16: Estimates of the baseline regression model when accounting for firm heterogeneity as measured by the quintiles of the size distributions. The coefficient to $year \times Size_5$ have been omitted because of multicollinearity, making the largest size class the reference group.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$GOP \times ZLB \times Size_1$	−0.871	−1.108	−1.781**	0.673**	0.237
	(0.984)	(0.910)	(0.820)	(0.315)	(0.257)
$GOP \times ZLB \times Size_2$	1.287	0.871	0.120	0.751***	0.416**
	(0.790)	(0.786)	(0.666)	(0.256)	(0.163)
$GOP \times ZLB \times Size_3$	2.638***	2.149***	1.146**	1.003***	0.489***
	(0.669)	(0.670)	(0.530)	(0.261)	(0.173)
$GOP \times ZLB \times Size_4$	1.447**	1.222**	0.552	0.670***	0.225
	(0.576)	(0.563)	(0.482)	(0.225)	(0.187)
$GOP \times ZLB \times Size_5$	0.387	0.545	−0.542	1.087***	−0.158
	(0.788)	(0.643)	(0.644)	(0.219)	(0.285)
R^2 Adj.	0.668	0.594	0.538	0.791	0.920

Note 1. Fixed effects by firm, by time-sector, by ETS phase, and by time-electricity decile pre-ZLB are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations: 69248. N. of firms: 10536.
Note 4. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 17: Estimates of the regression models for the transmission mechanisms. $\%Ele.$: share of electricity; $Self.Prod.$: dummy variable if the company uses some kind of energy self-generation techniques (solar panels and co-generation); K/L: capital-to-labor ratio; alp: apparent labor productivity; tfp: total factor productivity.

	$\%Ele.$	$Self.Prod.$	$ihs(K/L)$	$ihs(alp)$	$ihs(tfp)$
$DtE \times ZLB$	0.000	0.006	0.059***	0.038***	0.001
	(0.003)	(0.004)	(0.016)	(0.007)	(0.001)
Mean pre-ZLB	0.57	0.04	12.09	42.22	10.69
ΔIQ effect	-0.02%	0.38%	3.69%	2.38%	0.05%
R2 Adj.	0.745	0.813	0.799	0.707	0.849

Table 18: Estimates of the regression models for the transmission mechanisms. $\%Ele.$: share of electricity; $Self.Prod.$: dummy variable if the company uses some kind of energy self-generation techniques (solar panels and co-generation); K/L: capital-to-labor ratio; alp: apparent labor productivity; tfp: total factor productivity.

	$\%Ele.$	$Self.Prod.$	$ihs(K/L)$	$ihs(alp)$	$ihs(tfp)$
$GOP \times ZLB$	0.050*	−0.094*	−0.784***	−0.734***	0.026**
	(0.028)	(0.049)	(0.177)	(0.112)	(0.012)
Mean pre-ZLB	0.57	0.04	12.09	42.22	10.69
ΔIQ effect	0.50%	-0.96%	-7.07%	-5.94%	0.26%
R2 Adj.	0.745	0.813	0.799	0.707	0.849

Table 19: Estimates of the regression models for the transmission mechanisms when accounting for firm heterogeneity as measured by the quintiles of the size distributions. $\%Ele.$: share of electricity; $Self.Prod.$: dummy variable if the company uses some kind of energy self-generation techniques (solar panels and co-generation); K/L: capital-to-labor ratio; alp: apparent labor productivity; tfp: total factor productivity.

	Dep. Variable				
	$\%Ele.$	$Self.Prod.$	$ihs(K/L)$	$ihs(alp)$	$ihs(tfp)$
$DtE \times ZLB \times Size_1$	0.005	0.005	0.086***	0.043***	0.004**
	(0.005)	(0.003)	(0.027)	(0.011)	(0.002)
$DtE \times ZLB \times Size_2$	−0.001	0.004	0.070***	0.056***	0.003
	(0.005)	(0.004)	(0.022)	(0.012)	(0.002)
$DtE \times ZLB \times Size_3$	0.003	0.000	0.083***	0.032**	−0.001
	(0.005)	(0.005)	(0.028)	(0.012)	(0.002)
$DtE \times ZLB \times Size_4$	0.001	0.012	0.065**	0.041***	0.000
	(0.004)	(0.007)	(0.029)	(0.012)	(0.002)
$DtE \times ZLB \times Size_5$	−0.014***	0.000	−0.029	0.010	−0.005**
	(0.005)	(0.012)	(0.037)	(0.016)	(0.002)
R^2 Adj.	0.746	0.815	0.800	0.707	0.849

Note 1. Fixed effects by firm, by time-sector, and by ETS phase are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations (%Ele.): 68203. N. of firms (%Ele.): 10469.
Note 4. N. of observations (other): 69248. N. of firms (other): 10536.
Note 5. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 20: Estimates of the regression models for the transmission mechanisms when accounting for firm heterogeneity as measured by the quintiles of the size distributions. $\%Ele.$: share of electricity; $Self.Prod.$: dummy variable if the company uses some kind of energy self-generation techniques (solar panels and co-generation); K/L: capital-to-labor ratio; alp: apparent labor productivity; tfp: total factor productivity.

	Dep. Variable				
	$\%Ele.$	$Self.Prod.$	$ihs(K/L)$	$ihs(alp)$	$ihs(tfp)$
$GOP \times ZLB \times Size_1$	0.236**	−0.039	−0.675*	−0.486	0.055*
	(0.094)	(0.053)	(0.401)	(0.304)	(0.028)
$GOP \times ZLB \times Size_2$	0.099	0.032	−0.641**	−0.726***	0.034*
	(0.061)	(0.053)	(0.260)	(0.131)	(0.017)
$GOP \times ZLB \times Size_3$	−0.027	−0.102	−0.874***	−0.650***	0.025
	(0.051)	(0.089)	(0.330)	(0.162)	(0.020)
$GOP \times ZLB \times Size_4$	0.024	−0.056	−1.041***	−0.676***	0.021
	(0.044)	(0.086)	(0.289)	(0.143)	(0.022)
$GOP \times ZLB \times Size_5$	0.027	−0.094	−0.716**	−0.954***	0.020
	(0.036)	(0.106)	(0.296)	(0.198)	(0.021)
R^2 Adj.	0.746	0.815	0.800	0.707	0.849

Note 1. Fixed effects by firm, by time-sector, and by ETS phase are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations (%Ele.): 68203. N. of firms (%Ele.): 10469.
Note 4. N. of observations (other): 69248. N. of firms (other): 10536.
Note 5. * p < 0.1, ** p < 0.05, *** p < 0.01.

D.2 Alternative monetary policy shocks

In this sub-section we present the main results when the shock is measured either by another dummy variable that captures the European version of the Quantitative Easing (i.e., the Asset Purchase Programme), that has started in 2015, or by a continuous variable capturing the transmission of monetary policy to the banking sector, as measured by the EONIA interbank rate.[35] We here report for both shocks: (i) the baseline regressions (see Tables 21 and 22) and (ii) the baseline regressions with size class heterogeneity (see Tables 23 and 24).

Table 21: Estimates of the baseline regression model when financial fragility is measured by the debt-to-assets ratio and the monetary policy shock by the Asset Purchase Programme.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtA \times APP$	−0.567***	−0.590***	−0.351**	−0.240***	0.023
	(0.179)	(0.179)	(0.148)	(0.064)	(0.040)
Mean pre-ZLB	1748.22 t	0.19 t/Mln	39.30 t/KW	0.005 KW/Mln	9217.82 Mln
ΔIQ effect	-11.36%	-11.71%	-7.76%	-5.60%	0.62%
R^2 Adj.	0.667	0.593	0.537	0.790	0.920

Table 22: Estimates of the baseline regression model when financial fragility is measured by the debt-to-assets ratio and the monetary policy shock by the EONIA interest rate.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
DtA	−0.289***	0.322***	−0.209**	0.531***	−0.611***
	(0.097)	(0.099)	(0.082)	(0.039)	(0.027)
$DtA \times (-RATE)$	−0.098***	−0.079**	−0.061**	−0.017	−0.019**
	(0.036)	(0.036)	(0.031)	(0.012)	(0.009)
R^2 Adj.	0.667	0.593	0.537	0.791	0.920

Results for the APP shock are perfectly aligned with the ones obtained with the ZLB shock. They suggest that constrained companies have benefited the most (decreasing emissions and increasing value added) also when the shock is encoded as the arrival of the unconventional monetary policy paradigm. The results from the continuous measure, as modeled by the European (EONIA) interbank interest rate, are also in line (at large) with previous findings. The interaction term ($DtA \times (-RATE)$) points toward a negative effect of a monetary expansion on CO_2 emissions, which is stronger for financially constrained firms. The unique difference relates to the value added, which declines more for financially fragile firms when the monetary policy is more expansionary. We argue that this difference could be due to the fact that a year-to-year change in the interbank rate does not necessarily reflect a strong signal on markets and is therefore a signal with less credibility that the changes that happened in 2011 (zero nominal interest rate and the "whatever it takes") and 2015 (unconventional monetary policy).

Without additional comments, we here highlight that also the results on the heterogeneity of the effects, as depending from the size classes, confirm the previous findings described in detail in the main text.

[35] To evaluate the shock as expansionary, we use the negative of the EONIA interest rate. In this setting, we also keep the DtA as time-varying rather than a pre-shock average. The reason is that such a regression does not require the identification of a specific shock, as measured by the ZLB dummy in the main regressions.

Table 23: 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. The coefficient to $year \times Size_5$ have been omitted because of multicollinearity, making the largest size class the reference group.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtA \times APP \times Size_1$	−1.083***	−1.217***	−0.782***	−0.435***	0.134***
	(0.241)	(0.239)	(0.203)	(0.083)	(0.045)
$DtA \times APP \times Size_2$	−0.739***	−0.812***	−0.461**	−0.350***	0.073*
	(0.248)	(0.247)	(0.204)	(0.083)	(0.044)
$DtA \times APP \times Size_3$	−0.514**	−0.498**	−0.274	−0.224***	−0.016
	(0.238)	(0.240)	(0.197)	(0.083)	(0.050)
$DtA \times APP \times Size_4$	−0.208	−0.196	−0.104	−0.093	−0.012
	(0.211)	(0.212)	(0.178)	(0.079)	(0.048)
$DtA \times APP \times Size_5$	0.009	0.101	0.126	−0.025	−0.092*
	(0.206)	(0.207)	(0.175)	(0.078)	(0.055)
R^2 Adj.	0.668	0.594	0.538	0.791	0.920

Note 1. Fixed effects by firm, by time-sector, by ETS phase, and by time-electricity decile pre-ZLB are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations: 69248. N. of firms: 10536.
Note 4. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 24: 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 negative of the EONIA interest rate. The coefficient to $year \times Size_5$ have been omitted because of multicollinearity, making the largest size class the reference group.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$(-RATE) \times Size_2$	−0.177***	−0.155**	−0.123**	−0.032	−0.022
	(0.062)	(0.062)	(0.053)	(0.022)	(0.016)
$(-RATE) \times Size_3$	−0.065	−0.056	−0.045	−0.010	−0.009
	(0.056)	(0.057)	(0.049)	(0.019)	(0.016)
$(-RATE) \times Size_4$	0.030	0.040	0.045	−0.005	−0.010
	(0.057)	(0.056)	(0.048)	(0.021)	(0.016)
$(-RATE) \times Size_5$	−0.014	−0.021	−0.012	−0.009	0.007
	(0.051)	(0.051)	(0.045)	(0.018)	(0.016)
$DtA \times (-RATE) \times Size_2$	0.064	−0.008	−0.011	0.003	0.072**
	(0.107)	(0.107)	(0.092)	(0.039)	(0.028)
$DtA \times (-RATE) \times Size_3$	−0.115	−0.162	−0.113	−0.049	0.047*
	(0.100)	(0.100)	(0.088)	(0.035)	(0.027)
$DtA \times (-RATE) \times Size_4$	−0.141	−0.173*	−0.153*	−0.020	0.032
	(0.099)	(0.099)	(0.085)	(0.038)	(0.027)
$DtA \times (-RATE) \times Size_5$	−0.012	−0.030	−0.020	−0.010	0.018
	(0.090)	(0.091)	(0.080)	(0.033)	(0.028)
R^2 Adj.	0.665	0.590	0.533	0.789	0.923

Note 1. Fixed effects by firm, by time-sector, by ETS phase, and by time-electricity decile pre-ZLB are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations: 64491. N. of firms: 10334.
Note 4. * p < 0.1, ** p < 0.05, *** p < 0.01.

D.3 Alternative measures of the dependent variables

Table 25: Estimates of the baseline regression model when financial fragility is measured by the debt-to-assets ratio. All the dependent variables have been transformed with the $log(k \times (Y + \epsilon))$ transformation, with $k = 1000$ and $\epsilon = 0.01$.

	Dep. Variable				
	$log^+(CO2)$	$log^+(CO2/VA)$	$log^+(CO2/E)$	$log^+(E/VA)$	$log^+(VA)$
$DtA \times ZLB$	−0.707***	−0.782***	−0.393**	−0.389***	0.076**
	(0.218)	(0.219)	(0.185)	(0.084)	(0.037)
Mean pre-ZLB	1012.88 kt	0.13 kt/Mln	31.96 kt/KW	0.004 KW/Mln	1014.94 Mln
ΔIQ effect	-13.33%	-14.26%	-8.54%	-8.48%	2.06%
R^2 Adj.	0.582	0.523	0.489	0.656	0.920

Note 1. Fixed effects by firm, by time-sector, by ETS phase, and by time-electricity decile pre-ZLB are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations: 69248. N. of firms: 10536.
Note 4. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 26: Estimates of the baseline regression model when financial fragility is measured by the debt-to-assets ratio. All the dependent variables have been transformed with the log function, leading to the omissions of all zero-valued observations.

	Dep. Variable				
	$log(CO2)$	$log(CO2/VA)$	$log(CO2/E)$	$log(E/VA)$	$log(VA)$
$DtA \times ZLB$	−0.019	−0.089	0.040	−0.101**	0.076**
	(0.063)	(0.066)	(0.049)	(0.043)	(0.037)
Mean pre-ZLB	1013.26 kt	0.15 kt/Mln	35.26 kt/KW	0.004 KW/Mln	1015.16 Mln
ΔIQ effect	-0.49%	-2.23%	1.08%	-2.52%	2.06%
R^2 Adj.	0.885	0.818	0.762	0.839	0.920
N. Obs.	61 355	61 355	61 355	68 203	69 248
N. Firms.	9728	9728	9728	10 469	10 536

D.4 Alternative specifications

Table 27: Estimates of the baseline regression model when financial fragility is measured by the debt-to-assets ratio, for the period 2004-2019.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtA \times ZLB$	−0.383**	−0.438***	−0.224*	−0.214***	0.055
	(0.149)	(0.151)	(0.128)	(0.053)	(0.035)
Mean pre-ZLB	902.58 kt	0.13 kt/Mln	32.54 t/KW	0.004 KW/Mln	6850.25 Mln
ΔIQ effect	-8.31%	-9.26%	-5.24%	-5.03%	1.48%
R^2 Adj.	0.709	0.647	0.600	0.802	0.917

Table 28: Estimates of the baseline regression model when financial fragility is measured by the debt-to-assets ratio and focusing on energy intensive sectors.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$DtA \times ZLB$	−0.213	−0.327	−0.110	−0.217***	0.114**
	(0.223)	(0.226)	(0.191)	(0.081)	(0.057)
Mean pre-ZLB	1736.83 kt	0.26 kt/Mln	34.23 kt/KW	0.008 KW/Mln	6704.56 Mln
ΔIQ effect	-5.15%	-7.48%	-2.80%	-5.22%	3.23%
R^2 Adj.	0.709	0.647	0.600	0.802	0.917

Table 29: Estimates of the baseline regression model when financial fragility is measured by the debt-to-assets ratio and focusing on energy intensive sectors..

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$ihs(SALES)$	0.426***	−0.250***	−0.032	−0.218***	0.676***
	(0.047)	(0.046)	(0.039)	(0.017)	(0.011)
$ihs(ALP)$	−0.109***	−0.628***	0.029	−0.657***	0.519***
	(0.037)	(0.038)	(0.029)	(0.019)	(0.015)
$ihs(K/L)$	−0.034**	0.029	−0.033**	0.062***	−0.062***
	(0.017)	(0.018)	(0.014)	(0.008)	(0.005)
$DtA \times ZLB$	−0.393***	−0.432***	−0.292**	−0.140***	0.039**
	(0.147)	(0.148)	(0.126)	(0.048)	(0.018)
R^2 Adj.	0.669	0.601	0.537	0.825	0.981

D.5 Alternative matching strategy for the ETS policy regressions

Standard nearest neighbor matching estimators based on propensity scores has been recently exposed to critiques (Iacus et al., 2012, see). Although the balancing between EST treated and non-ETS matched samples seemed appropriate, the regressions including the ETS dummy are conditional upon the sub-sample creations. As a check for robustness we thus employ the coarsened exact matching (CEM) algorithm proposed by Iacus et al. (2012). Results are reported in Table 30. Even if the samples are slightly changed, we find that the conclusions outlined in the main text are substantially unvaried.

Table 30: Estimates of the augmented regression model when financial fragility is measured by debt-to-assets. The employed matching algorithm is the CEM.

	Dep. Variable				
	$ihs(CO2)$	$ihs(CO2/VA)$	$ihs(CO2/E)$	$ihs(E/VA)$	$ihs(VA)$
$ZLB \times ETS$	1.284*	0.635	0.694	−0.059	0.650***
	(0.707)	(0.751)	(0.479)	(0.385)	(0.239)
$DTA \times ZLB$	1.007	0.073	0.503	−0.429	0.934***
	(0.889)	(0.899)	(0.574)	(0.451)	(0.287)
$DTA \times ZLB \times ETS$	−1.809*	−1.118	−1.458*	0.341	−0.691*
	(1.042)	(1.103)	(0.761)	(0.564)	(0.376)
R^2 Adj.	0.624	0.580	0.397	0.864	0.885

Note 1. Std. Errors are clustered at the firm level. Fixed effects by firm and by time-sector are included.
Note 2. Std. Errors are clustered at the firm level.
Note 3. N. of observations: 1973. N. of firms: 181.
Note 4. * p < 0.1, ** p < 0.05, *** p < 0.01.