THE
ECONOMIC
JOURNAL

The Economic Journal, **134** (*October*), 2803–2834 https://doi.org/10.1093/ej/ueae048
Advance Access Publication Date: 20 June 2024

ENERGY TAX EXEMPTIONS AND INDUSTRIAL PRODUCTION*

Andreas Gerster and Stefan Lamp

This paper investigates the impact of a large electricity tax exemption on production levels, employment and input choices in the German manufacturing industry. For two policy designs, we show that exempted plants increase their electricity use. This effect is larger under a notched exemption policy, where passing an eligibility threshold yields infra-marginal benefits, compared to a policy without such benefits. We detect no significant impact of the exemptions on production levels, export shares and employment. Using counterfactual simulations, we document that notched policies substantially distort firms' production input choices when financial stakes are high and compliance costs are low.

Tax exemptions are a widely used policy tool to influence firm behaviour. For example, policy makers reduce corporate income taxes for innovative firms and create incentives for the construction of new production sites through temporary tax cuts (see, e.g., Bond, 1981; Mata and Guimarães, 2019; Mast, 2020; Chen *et al.*, 2021). Moreover, governments in many industrialised countries exempt manufacturing firms from environmental regulations such as energy and carbon taxes.[1] While tax exemptions are heavily used in practice, there is a recurring concern

* Corresponding author: Andreas Gerster, Department of Economics, University of Mannheim, L7 3-5, 68161 Mannheim, Germany. Email: gerster@uni-mannheim.de

This paper was received on 6 July 2020 and accepted on 28 May 2024. The Editor was Frederic Vermeulen.

The data and codes for this paper are available on the Journal repository. They were checked for their ability to reproduce the results presented in the paper. The authors were granted an exemption to publish parts of their data because access to these data is restricted. However, the authors provided a simulated or synthetic dataset that allowed the Journal to run their codes. The synthetic/simulated data and the codes for the parts subject to exemption are also available on the Journal repository. They were checked for their ability to generate all tables and figures in the paper; however, the synthetic/simulated data are not designed to reproduce the same results. The replication package for this paper is available at the following address: https://doi.org/10.5281/zenodo.11348256.

We are grateful for comments and suggestions by Mark A. Andor, Antonio Bento, Sylvain Chabé-Ferret, Massimo Filippini, Manuel Frondel, Kathrine von Graevenitz, Beat Hintermann, Sébastien Houde, Elisabeth Isaksen, Jacob LaRiviere, Marita Laukkanen, Sebastian Pieper, Michael Themann, Ulrich J. Wagner and Bert Willems. We also thank participants of the 12th Conference on the Economics of Energy and Climate Change Toulouse, 7th IZA Workshop on the Environment and Labor Markets, 6th WCERE, 32nd EEA Conference, TSE Workshop on Environmental Regulation and Industrial Performance, 6th Mannheim Energy Conference, ZEW Workshop on Environmental Analysis, 8th EMEE Workshop, 6th Atlantic Workshop on Energy and Environmental Economics, 8th RGS Doctoral Conference, 14th European Conference of the IAEE, as well as the seminar audiences at Kiel University, NHH Bergen, PUC Chile and TSE. We also thank Kathrin Stief and Kerstin Stockmayer from the Research Data Centre Baden-Württemberg for their assistance in working with the confidential AFiD data. All errors are solely the responsibility of the authors. An earlier version of this paper has been circulated under the title 'Do Electricity Prices Matter? Plant-Level Evidence from German Manufacturing'.

A. Gerster and S. Lamp acknowledge financial support by the Federal Ministry of Education and Research (BMBF) through TRACE (Grant 01LA1815A), the German Research Foundation (DFG) through CRC TR 224 (Project B07), the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation program (Grant Agreement No. 772331), and support from the ANR under Grant ANR-17-EURE-0010 (Investissements d'Avenir program), respectively.

[1] The OECD Database on Policy Instruments for the Environment lists roughly 2,400 exemptions from environmentally related taxes worldwide, from which some 1,900 exemptions apply to the private sector (OECD, 2020). In particular, energy tax exemptions for energy-intensive and trade-exposed (EITE) industries are employed in the UK, Belgium, Finland, France, Germany, the Netherlands and Italy, for instance.

that they may not achieve their political objectives, but rather distort firm behaviour and reduce tax revenues.

The rationale of energy tax exemptions is to protect energy-intensive and trade-exposed industries from adverse competitiveness effects of climate policies. A major problem is that environmental regulations typically apply only in some jurisdictions, but not in others. Such incomplete regulation can cause 'leakage' of industrial activity and emissions from regulated to unregulated jurisdictions (e.g., Fischer and Fox, 2012; Fowlie and Reguant, 2018). Proponents of tax exemptions therefore argue that they are necessary to sustain domestic production levels. Critics, on the other hand, worry that exemptions might induce rent-seeking behaviours, distort firm production decisions and lead to higher energy uses (OECD, 2001; 2015).

This paper studies the impact of energy tax exemptions in the context of a large levy on electricity, the German *renewable energy levy* (REL). The REL was introduced to finance renewable energies and accounted for roughly one-third of the average industrial electricity price in 2013. We use rich administrative data covering the universe of German manufacturing plants to examine how production levels, employment and the use of energy inputs were affected by an exemption from the REL under two different policy designs. In the years 2003 to 2012, exemptions were granted based on a notched policy design, where passing an eligibility threshold reduced marginal prices and involved infra-marginal benefits two years later. A policy reform in 2012 largely removed these infra-marginal benefits and expanded the eligibility criteria to a larger group of plants. We contrast the effects of REL exemptions under both policies to explore how differences in policy design influence production choices.

Our empirical approach consists of two quasi-experimental methods and counterfactual simulations based on a stylised structural production model. Both reduced-form identification strategies exploit a distinct source of exogenous variation. First, to estimate the causal effects under the notched policy design, we take advantage of the fact that eligibility for an exemption was only granted to plants that used more than 10 gigawatt-hour (GWh) of electricity two years earlier. We provide evidence that the severe financial crisis of 2008 and 2009 prevented plants from potentially manipulating their electricity use in those years despite the notched exemption schedule. This allows us to identify the effect of the exemptions in the years 2010 and 2011 based on a fuzzy regression discontinuity (RD) design for plants around the eligibility threshold. This approach compares virtually identical plants that barely met or failed to meet the eligibility threshold of 10 GWh of electricity consumption during the years of the financial crisis to investigate how REL exemptions change plant-level production two years later, when the short-lived financial and economic crisis had already ended in Germany.

Second, to identify the effects of an exemption after the 2012 policy change, we exploit the fact that the eligibility threshold was reduced from 10 to 1 GWh of annual electricity consumption. This reduction more than doubled the number of exempted plants in manufacturing from roughly 700 to 1,700. We focus on the group of newly eligible plants and estimate the average treatment effect for plants exempted in 2013 using a matching difference-in-differences (DiD) estimator. This estimator exploits the longitudinal structure of our dataset and the rich information it provides about plant characteristics. It compares how changes in outcomes for newly exempted plants differ from changes in outcomes for a matched control group of non-exempt plants that are very similar in terms of their observed characteristics.

We set up a model of production to put our empirical estimates in context. The model is built to incorporate two stylised facts about the exemption and bunching behaviour of firms. First, under the notched exemption design, only few firms bunch above the eligibility threshold.

To rationalise such behaviour, we allow for the presence of a bunching cost. Second, we find that on average only three out of four eligible plants decide to claim an exemption. Our model thus considers compliance costs that may arise when claiming an exemption. Compliance costs may occur because firms must hire independent accountants to verify their eligibility status and hand in certified documentation about their energy-saving practices, for example. We show that the parameters of the compliance cost distribution can be identified from the exemption rates of eligible plants with different electricity use levels. Furthermore, we show that the bunching cost parameters are identified from the percentage of 'bunchers' below the threshold and the electricity demand of the marginal bunching plant. Both statistics are not directly observable, but can be estimated using methods from the bunching literature (e.g., Kleven, 2016; Almunia and Lopez-Rodriguez, 2018).

Our main reduced-form estimates show that the REL exemptions lead to significant increases in electricity consumption under both policy designs. We find that exempted plants in the reformed schedule increased their electricity consumption on average by approximately 3% in 2013. Yet, the effect sizes in 2010 and 2011 under the original (notched) schedule were about one order of magnitude larger. By contrast, we do not find statistically significant impacts of the REL exemption on competitiveness indicators such as sales, export share or employment.

The results from the counterfactual simulations provide an explanation for the difference in effect sizes. In particular, we find that infra-marginal effects on electricity use from plants that bunch above the eligibility threshold can amount to 27% in 2010. Beyond that, our results highlight the importance of the compliance cost and the stakes involved for understanding market behaviour under a notched design. While bunching was only of limited relevance in the years 2008 to 2011, we show that it would have led to an increase in electricity use of about 1,144 GWh had the REL levels increased to 2017 levels and compliance cost been absent. Furthermore, we find that the presence of compliance cost reduces incentives for bunching, but also constitutes a major cost component for firms, amounting to about 290–340 million euros in 2012 and 2013.

We conduct extensive robustness tests for our main findings and present supporting evidence for the identifying assumptions of the reduced-form estimates. For the fuzzy RD design, we test for selection around the eligibility threshold based on density tests to ensure that the financial crisis prevented plants from manipulating their electricity consumption in the years 2008 and 2009. This finding is also supported by placebo treatment effect regressions that show no sign of a discontinuity in baseline variables around the eligibility threshold prior to the exemption year. We further test for different bandwidths and limit the sample to single-plant firms to exclude the possibility of intra-firm spillovers that might arise if firms are partially exempted. For our matching DiD approach, we provide evidence of common trends for several important plant-level characteristics. We also test whether our results are robust to different propensity score specifications and matching strategies. To investigate whether potential anticipation of the policy reform or spillovers may matter, we condition on characteristics in the year prior to its announcement and restrict our sample to single-plant firms. We also estimate a DiD model that only exploits variation in eligibility in response to the 2012 policy reform for identification, thereby testing the robustness of our findings to a potential selection on trends.

This paper makes three main contributions. First, we contribute to the literature on incomplete environmental regulation. A growing strand of this literature has focused on the analysis of policy instruments against leakage, including free allocation of pollution permits, output-based rebates and border tax adjustments (see, for instance, Bernard *et al.*, 2007; Fowlie, 2009; Martin *et al.*, 2014b; Fowlie *et al.*, 2016). A result from this literature is that exemptions of EITE industrial plants are inferior to border tax adjustments or output-based rebates (e.g., Böhringer *et al.*, 2012; Fowlie *et al.*, 2016). Yet, despite their shortcomings, exemptions from environmental regulation for EITE plants are still used in practice and evidence on their performance has remained scarce.

We add to this literature by evaluating a large exemption policy for EITE plants in the German manufacturing sector. Our results confirm that exemptions for EITE plants are an imperfect anti-leakage policy. In particular, we find no evidence that they increase the international competitiveness of exempted plants. Rather, they significantly influence fuel input choices and lead to higher electricity uses. These results are robust across our two identification strategies, which adds to their credibility. We thus provide evidence for an ongoing political discussion on the effective design of exemptions, which has gained momentum after the recent initiative of the European Union (EU) to introduce carbon tariffs at the EU border.[2]

Second, our paper contributes to the literature on the evaluation of environmental regulations for industrial firms. One focus of this literature has been to investigate how emission markets, carbon taxes and the introduction of air pollution standards affect production in manufacturing (see, e.g., Greenstone, 2002; Fowlie *et al.*, 2012; Greenstone *et al.*, 2012; Martin *et al.*, 2014a; Hintermann *et al.*, 2020, as well as Martin *et al.*, 2016 and Dechezleprêtre and Sato, 2017 for reviews). Furthermore, Martin *et al.* (2014a) estimated the effect of the climate change levy on production of manufacturing plants in the UK, using plants that were exempted from the levy as a control group.

Our evaluation goes beyond the previous studies by investigating how design features of the exemption regime affect the behaviour of market participants. We document that a substantial share of eligible plants do not claim an exemption to avoid the compliance cost from having to comply with organisational requirements. We also find evidence of a significant bunching cost that prevented plants from strategically manipulating their electricity use under the notched policy. Furthermore, we show that the overall response to an exemption is strongly affected by the exemption design. The increase in electricity use for exempted plants is larger under a notched tax design, compared to a policy design where notches have been largely removed. In addition, we find that notched exemption designs may cause substantial bunching when exemptions are high and compliance costs are low.

Third, we contribute to a literature on the effects of regulatory thresholds on firm behaviour. The influence of notched tax designs has been investigated in the context of corporate profit taxes (e.g., Almunia and Lopez-Rodriguez, 2018), R&D investments (Chen *et al.*, 2021) and labour regulations (e.g., Garicano *et al.*, 2016). As a common result, these studies find that notches distort firms' tax reporting, investment and employment decisions, with negative welfare consequences. Despite the fact that notched exemption designs for EITE industries are a common policy instrument, it has remained an open question how they affect energy input use decisions for industrial production. Furthermore, studies that explore the role of compliance cost under notched tax schemes have remained scarce. One exception is Harju *et al.* (2019), who found that a substantial compliance cost prevents small businesses from increasing their gross value added beyond a threshold for inclusion into the value added tax system.

We provide novel evidence how a notched tax exemption design regarding production input taxes affect firm behaviour in the industry. In particular, we combine reduced-form policy evaluations with an analysis of bunching behaviour to estimate bunching and compliance costs, which we show to be non-negligible. Our estimates imply that manipulating electricity

[2] See https://www.consilium.europa.eu/en/press/press-releases/2022/03/15/carbon-border-adjustment-mechanism-cbam-council-agrees-its-negotiating-mandate/.

use above eligibility thresholds only becomes profitable when the stakes of an exemption are particularly high. Furthermore, we document a nuanced interplay between exemption notches and compliance cost. On the one hand, organisational requirements that cause compliance cost mitigate welfare-reducing rent-seeking behaviour from bunching. On the other hand, they impose non-negligible cost on firms, with adverse welfare effects.

Beyond these three main contributions, we also relate to a literature that has investigated the role of energy prices for industrial production. This literature has gained attention by policy makers after the recent surge in energy prices. Previous studies have shown that higher prices reduce energy use and procurement in manufacturing (Marin and Vona, 2021; von Graevenitz and Rottner, 2022), but also modestly decrease employment (e.g., Commins *et al.*, 2011, Deschenes, 2012), and co-determine the locations of firms (Kahn and Mansur, 2013). A novelty of our setting is that we can exploit large policy-induced price variation to identify price elasticities.

The remainder of this paper is structured as follows. In Section 1, we describe the institutional details of the REL exemptions and discuss how differences in policy design influence input choices. Section 2 introduces our data and describes the assignment mechanism. The empirical analysis is divided into three parts. In Section 3, we investigate the impact of REL exemptions under the original policy design, while we evaluate their impact after the 2012 reform in Section 4. Section 5 describes how we estimate the production model and conduct counterfactual analyses to highlight the efficiency and distributional implications of exemption policy designs. Finally, Section 6 concludes.

1. Institutional Background

1.1. *REL Exemptions and Electricity Prices*

In 2000, the German *Renewable Energy Act* introduced one of the world's most ambitious renewable energy support schemes. Its core element is the provision of generous feed-in tariffs (FiTs) to producers of electricity from renewable sources. FiTs guarantee long-term investment security by providing a fixed price per kilowatt-hour (kWh) of generated electricity above the wholesale price of electricity.[3] The introduction of FiTs triggered a rapid increase in the share of renewable energy production from approximately 6% in 2000 to almost 30% in 2014. Consequently, the policy has also led to rapidly rising annual subsidy costs, reaching 22 billion euros (EUR) in 2014 alone.

In Germany, FiT payments are financed by the REL, a per kilowatt-hour surcharge on electricity prices that has to be paid by all households and businesses alike. Figure 1 displays the evolution of the REL together with the average industry electricity prices in Germany between 2000 and 2017. In this period, average electricity prices for the industry have risen substantially, from about 6 cents per kWh in 2000 to 17 cents per kWh in 2017. An important role in this increase is played by the REL, which increased from 0.2 cents per kWh in 2000 to 6.88 cents per kWh in 2017, accounting for more than a third of the average industry electricity price in that year.

Rising electricity prices have spurred concerns about potential adverse effects to the international competitiveness of the German manufacturing industry. To limit such concerns, the government has introduced exemptions from the REL for energy-intensive plants from 2003

[3] We provide evidence on the evolution of FiT rates for the example of solar photovoltaic installations in Germany together with the average electricity prices in Online Appendix Figure A.1. FiT policies are a key policy instrument to support renewable energy deployment in most European countries and many other jurisdictions such as Australia, California and Ontario.

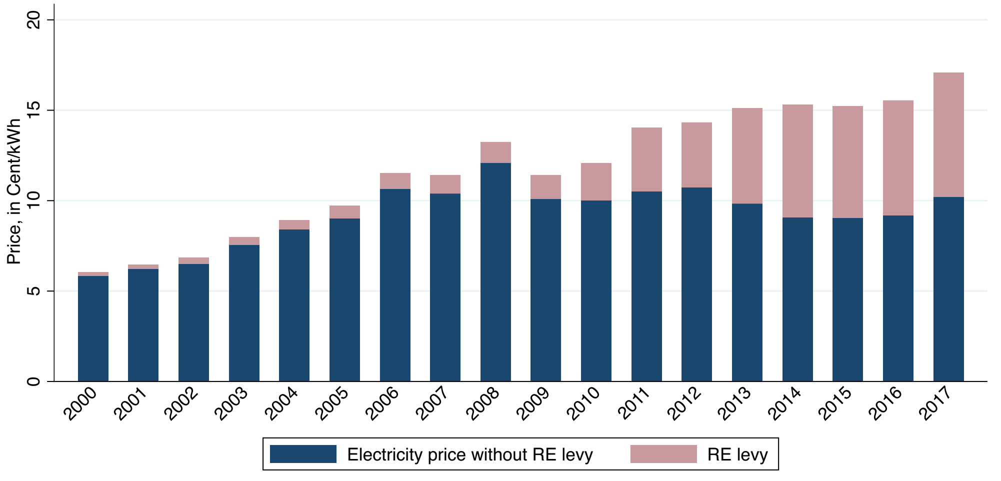

Fig. 1. *Average Industry Electricity Prices in Germany.*
Notes: Average industry electricity prices (nominal, including taxes) in Germany for plants with an annual electricity consumption between 0.16 and 20 GWh.
Source: BDEW (2022).

onwards. Eligibility for an exemption is based on two threshold values: first, the total annual electricity consumption of a plant and, second, the electricity intensity of the respective firm, defined as the ratio of electricity cost to gross value added (GVA).

To be exempted, plants need to apply at the Federal Office for Economic Affairs and Export Control (Bundesamt für Wirtschaft und Ausfuhrkontrolle, BAFA). In any given year; plants apply by submitting verified information on their electricity use, electricity cost and GVA in the previous year. Since 2008, plants also have to prove that accredited external experts have surveyed and assessed the energy consumption and energy-saving potentials at the plant level. Based on this information, BAFA grants eligible plants an exemption for the following year. Therefore, this procedure introduces a time gap of two years between the baseline period, i.e., the year that determines eligibility, and the year for which the exemption is granted. The large majority of exemptions are granted to plants in the manufacturing sector, on which we focus in our analysis.

Under the original exemption scheme, medium-sized and large plants in the manufacturing sector were eligible for REL exemptions if they used more than 10 GWh of electricity and if the ratio of electricity cost to GVA at the firm level exceeded 15%. Exempted plants paid a drastically reduced REL of 0.05 cents per kWh for all electricity consumption exceeding 10% of their baseline use in the year determining eligibility. Very electricity-intensive plants with an electricity consumption above 100 GWh and a ratio of electricity cost to GVA of more than 20% were fully exempted.

These exemption rules were revised as part of a large policy reform to modernise the German FiT scheme, effective from 2013 onwards. This revision extended the eligibility criteria for exemptions of manufacturing plants considerably by reducing the consumption threshold from 10 to 1 GWh of annual electricity use. It also marginally lowered the second eligibility criterion concerning the ratio of electricity expenditure to GVA from 15% to 14%. As a

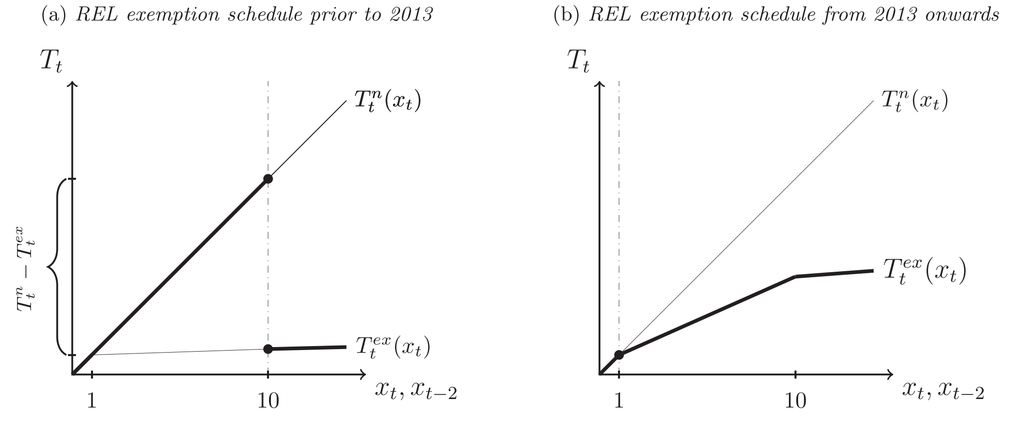

Fig. 2. *REL Payment Schedules for Exempted Plants.*
Notes: The lines $T_t^n(x_t)$ and $T_t^{ex}(x_t)$ denote the REL payments for electricity in period t if *not exempted* and *exempted*, respectively. The vertical dashed lines denote eligibility thresholds of 10 and 1 GWh in the two policy designs prior to 2013 (Figure 2a) and from 2013 onwards (Figure 2b). The thick lines plot the REL payment in period t as a function of the input use in period $t-2$ (assuming, for simplicity, that $x_t = x_{t-2}$ and that passing the eligibility threshold leads to an exemption).

consequence, the number of exempted plants increased from 683 in 2012 to 1,663 in 2013 (see Online Appendix Table A.2). While the number of eligible plants in *manufacturing* increased significantly, the total amount of electricity exempted from the REL remained virtually unchanged by the policy reform. This is mainly due to the fact that large firms in the water supply, recycling, construction and public transportation sectors were no longer eligible for an exemption after 2012.[4] Newly eligible plants applied broadly in the first year of their implementation, indicating that they have been aware of the reformed REL exemption rules. This is also supported by a sharp increase in application and rejection rates.[5]

In addition to lowering the eligibility thresholds, the reform affected the REL payment schedule for exempted plants as follows. While all plants pay the full REL for the first gigawatt-hour of electricity use, exempted plants pay a reduced rate of 10% of the levy for any additional electricity consumption between 1 and 10 GWh, and 1% for the consumption above 10 GWh. In the next subsection, we give details on how the financial incentives for plants changed in response to the policy reform.

1.2. *Incentives under Both REL Exemption Designs*

Figure 2 plots the original exemption schedule (panel (a)) and the revised schedule after the policy reform (panel (b)), where T_t^n and T_t^{ex} denote the total REL payment for non-exempted and exempted plants, respectively. Under the original policy design (panel (a)), plants can be exempted in period t if they consumed more than 10 GWh of electricity in the baseline period $t-2$, indicated by the vertical dashed line, where x_t denotes electricity consumption in period t.

4 The reform expanded the total amount of exempted electricity by only 3.5% (3.4 terawatt-hours (TWh) in 2013). This contributed to a negligible increase of 0.04 euro-cent/kWh to the REL in 2013. *Source:* BAFA (2010–2013).

5 While the rejection rate reported by BAFA typically ranged between 4% and 10% prior to 2013, it increased to 19% in 2013 (BAFA, 2010–2013). Data on plant applications and rejections are only available at the aggregate level.

For simplicity, we consider a plant that also passes the second eligibility criterion on electricity intensity at the firm level.

An exemption under the original policy design has two main implications. First, it reduces marginal electricity prices, as indicated by the change in the slope of the REL payment function, which is flatter for T_t^{ex}. Second, it implies infra-marginal benefits as an exemption applies for all electricity consumed in excess of 10% of the baseline consumption. To illustrate this, consider a plant that consumes exactly 10 GWh of electricity in period $t-2$. If the plant consumed slightly less in $t-2$, it would not benefit from an exemption and would face REL payments of T_t^n in period t. With an electricity use of at least 10 GWh in period $t-2$, it passes the eligibility threshold and can get exempted in period t. An exemption reduces the total REL payment in period t by the amount $T_t^n - T_t^{ex}$. This infra-marginal benefit generates incentives for plants to locate above the exemption eligibility threshold. Exemption schedules that offer such infra-marginal benefits are typically referred to as notched tax designs (see, for instance, Sallee and Slemrod, 2012; Kleven, 2016). We use this terminology when we refer to the original REL exemption design.

As shown in panel (b) of Figure 2, the reform of the REL exemption rules largely eliminated the tax notch for plants close to the new eligibility threshold of 1 GWh. Only the marginal REL payments change at this point, providing little incentives for plants to expand electricity use in order to reach eligibility.

1.3. *Production Input Choices and Policy Design*

To understand the potential impact of REL exemptions on electricity use under both policy designs, we develop a stylised model of production in the spirit of Lucas (1978) and Almunia and Lopez-Rodriguez (2018). Let the profit of a (single-plant) firm be given by

$$\pi = \psi y(x, z) - qz - px - T(x),$$

where x represents the main production input, electricity, z is a composite input good and $y(\cdot)$ is a production function that is strictly continuous, increasing and quasi-concave. Firms have heterogeneous productivity, denoted by parameter $\psi \in [\underline{\psi}, \bar{\psi}]$, which is assumed to be distributed in the population of firms with a (continuous) density function $g(\cdot)$ and cumulative density function $G(\cdot)$. Firms purchase the inputs x and z on competitive factor markets at prices p and q, respectively, and sell their output on a competitive output market at a price normalised to one.

While the composite input z is untaxed, the government implements a notched tax schedule $T(x)$ for the input x, defined as

$$T(x) = \begin{cases} tx - V(\psi, C) & \text{if } x \geq \hat{x}, \\ tx & \text{if } x < \hat{x}, \end{cases}$$

where t denotes a per-unit tax rate of x and $V(\psi, C)$ denotes the net value of a tax exemption that a firm with productivity ψ and compliance cost C obtains when its input use exceeds a predefined threshold value $\hat{x}$ in the current period.

The net value of an exemption can be written as $V(\psi, C) = A(\psi) - C$, where $A(\psi)$ denotes the financial value from an exemption and C denotes the compliance cost from obtaining it, which we assume to be distributed in the population of firms with a density function $f(\cdot)$ and cumulative density function $F(\cdot)$. In our setting, $A(\psi)$ corresponds to the present value of being exempted from the tax two years later in response to passing the electricity use eligibility threshold today. This value increases in ψ as more productive firms use more electricity and hence profit more from an exemption. Furthermore, compliance costs C arise because firms have to hand in certification from accountants that they meet the eligibility criteria and documentation about their energy management practices, for example.

When firms deviate from their optimal production path in order to become eligible for an exemption two years later, they face bunching costs. Bunching costs represent the profit loss from deviating from the optimal production path. Hence, they are non-negative and an increasing function of the distance between the threshold value $\hat{x}$ and the firms' counterfactual input choice in the absence of the notch, x^c.

In Online Appendix B, we derive three main outcomes of the model. First, a firm with an electricity use below the eligibility threshold, $x^c(\psi) < \hat{x}$, manipulates its electricity use to become eligible if and only if

$$A(\psi) - C \geq \kappa(\psi), \tag{1}$$

where $\kappa(\psi) = \kappa(\hat{x} - x^c(\psi))$ is the bunching cost for a firm with productivity ψ and counterfactual electricity demand $x^c(\psi)$ in the absence of a notch. Second, an eligible firm with $x^c(\psi) > \hat{x}$ applies for an exemption if and only if the present value of an exemption exceeds the compliance cost:

$$A(\psi) \geq C.$$

Third, the impact of a tax exemption under the notched design can be decomposed as

$$\frac{\partial x^*}{\partial t^{ex}} = \mathrm{MPR} + \mathrm{BR}, \tag{2}$$

where MPR denotes the marginal price response by all exempted plants from a reduction of electricity prices and BR denotes a net bunching response. The net bunching response corresponds to the incremental increase in electricity use by plants below the eligibility threshold that choose to bunch only after electricity prices decrease.[6]

Hence, the model yields three theoretical predictions on firm's electricity input use under the notched policy design. First, bunching above the eligibility threshold occurs only if the value of an exemption $A(\psi)$ exceeds the cost of manipulating the input variable. As REL exemptions have increased over time, we expect to see less bunching in years when the REL has been modest. Furthermore, bunching may not occur at all when the sum of the compliance and bunching costs is prohibitively high. Second, eligible firms may choose not to apply for an exemption if it involves compliance costs that exceed the value of an exemption. As the value of an exemption increases in plants' electricity use, we thus expect that the exemption rate among eligible firms increases in their electricity use. Third, our model predicts that an exemption increases the input use more under a notched exemption design than under a policy design where the notch is not present. This prediction follows from observing that eliminating the tax notch also eliminates the net bunching response, which enters additively into (2). We test these predictions in the following empirical sections of this paper.

[6] Firms below the threshold in period t can nonetheless obtain an exemption in that period if their electricity use two years earlier had exceeded the threshold.

2. Data

Our empirical analyses are based on a rich administrative dataset on the German manufacturing industry for the period 2007 to 2017 (*AFiD, Amtliche Firmendaten in Deutschland*). The dataset is administered by the research data centres of the Statistical Offices of the Federal States and covers the universe of plants from the manufacturing sector with more than twenty employees. It contains around 40,000 observations per year and includes a variety of plant-level characteristics, such as sales, exports, number of employees, as well as average wage levels. It also comprises detailed plant-level information on fourteen different energy inputs, including electricity, gas, coal and oil. Based on this information, we calculate CO_2 emissions using annual average emission coefficients of the respective fuel types from the German environmental agency (UBA, 2018a).[7] In addition, AFiD provides information on total energy cost and gross value added at the firm level for a representative sample of firms. We complement these data with information on electricity cost at the firm level, which are available for the same representative sample, but only at four-year intervals (2006, 2010, 2014). To calculate the ratio of electricity cost to GVA for all firms and years, we interpolate the data based on firm-level electricity prices and the quantity of electricity purchases, which we observe annually (see Online Appendix D).[8]

We link our data with the full list of plants that are exempted from paying the REL. These data are available for the years 2010 to 2013 from the *Federal Office for Economic Affairs and Export Control (BAFA)*. To match this dataset to AFiD, we rely on Bureau van Dijk identifiers, tax identification numbers,and official municipality identifiers. This procedure allows us to match about 95% of exempted plants to the AFiD company register. From these, we only keep plants in manufacturing. We also ensure that we can uniquely identify exemptions at the plant level and that exempted plants do not violate eligibility criteria according to our data. These criteria are fulfilled by 91% to 95% of the matched plants in the years 2010 to 2013, which we then use for our analyses.

Table 1 presents summary statistics for three main groups of plants for the year 2013. The first group (columns (1) to (3)) comprises plants that were not exempted from paying the REL. On average, plants in that group have 137 employees and sales of about 31 million EUR. The second group (columns (4) to (6)) focuses on the group of small and medium-sized energy-intensive plants that consumed between 1 and 10 GWh of electricity and were newly eligible for the REL exemption in 2013. While the number of employees and sales are slightly smaller than for the non-exempted plants (78 and 30 million EUR, respectively), these plants use considerably more electricity on average (5.3 versus 3.6 GWh). The third group (columns (7) to (9)) captures all plants that were exempted in 2013, including those that had been exempted prior to the policy change. This group comprises medium and large manufacturing plants with 180 employees and 85 million EUR of sales on average. The average electricity consumption in that group exceeds 46 GWh, which reflects the presence of some heavy electricity users. The table further highlights that the fuel energy mix used in the German manufacturing industry is dominated by electricity and natural gas and roughly similar for the three groups of plants.

[7] For electricity, we rely on the average carbon factor of the German electricity fuel mix in each year. Using data from ENTSO-E (available from 2015), we confirm that the average and marginal emission factors in Germany are comparable. We find an average marginal emission factor of 555 grams CO_2/kWh of electricity production in 2015, while the German environmental agency (UBA) lists an average of 575 grams CO_2/kWh (not considering imports and exports in both cases). UBA lists comparable values of 550 grams CO_2/kWh for the average emission factor in 2010–1. The high carbon emission intensity of electricity generation in Germany is mainly due to the large share of coal and lignite plants that can be both infra-marginal and marginal (the price-setting technology).

[8] Electricity prices for non-household consumers are from Eurostat (2023).

Table 1. *Summary Statistics, 2013.*

Variable	Not exempt			REL exempt: 1–10 GWh			REL exempt: all		
	Mean (1)	SD (2)	Obs. (3)	Mean (4)	SD (5)	Obs. (6)	Mean (7)	SD (8)	Obs. (9)
Panel A: plant-level data									
Economic covariates:									
Sales (million €)	31.06	131.99	41,026	29.53	116.54	655	84.76	233.51	1,815
Export share (of sales)	0.21	0.26	41,052	0.21	0.25	659	0.27	0.29	1,820
Number of employees	137	617	40,471	78	99	664	180	288	1,817
Investments (million €)	1.22	15.05	41,020	0.76	4.01	652	2.32	7.49	1,890
Avg. wage per employee (thsd. €)	34.01	13.65	40,471	33.95	10.39	664	38.7	15.23	1,817
Energy-related covariates:									
Electricity use (GWh)	3.56	47.51	40,224	5.34	3.53	660	46.03	151.45	1,805
Electricity use (2011) (GWh)	3.64	45.21	38,251	5.24	2.75	673	55.57	186.99	1,574
Other energy use (GWh)	15.23	618.82	41,269	10.42	22.15	660	124.53	741.48	1,850
Own electricity generation (%)	0.09	0.28	42,578	0.09	0.29	673	0.11	0.32	1,952
Electricity share in total energy	0.5	0.26	40,223	0.59	0.31	660	0.55	0.31	1,805
Gas share in total energy	0.31	0.3	40,728	0.29	0.31	660	0.29	0.31	1,822
Oil share in total energy	0.13	0.24	40,728	0.05	0.14	660	0.05	0.14	1,822
Coal share in total energy	0	0.06	40,728	0.01	0.08	660	0.02	0.12	1,822
Renewable share in total energy	0.05	0.17	40,728	0.06	0.19	660	0.09	0.22	1,822
Total CO_2 emissions (thsd. tonnes)	5,377	180,836	41,272	4,896	4,960	660	50,185	228,659	1,850
Direct CO_2 emissions (thsd. tonnes)	3,713	175,362	41,272	1,828	4,287	660	25,507	194,130	1,850
Panel B: firm-level data									
Number of plants per firm	1.17	1.57	36,826	1.22	0.96	530	1.43	1.24	1,376
Gross value added (million €)	25.68	264.62	14,755	44.25	610.88	255	43.77	356.69	853
Total energy cost (million €)	1.67	14.91	14,754	1.52	2.78	255	10.46	24.25	853
Total electricity cost (million €)	0.42	4.55	36,560	1.07	6.19	530	5.77	23.37	1,374
Electricity cost intensity (%)	0.04	0.08	36,177	0.25	0.2	524	0.28	0.24	1,363

Notes: Descriptive statistics for the group of exempted and non-exempted plants for the year 2013. Columns (1)–(3) refer to all non-exempted plants, while columns (4)–(6) refer to the group of newly exempted plants in 2013 (1–10 GWh annual electricity consumption). Columns (7)–(9) relate to all REL exempted plants in 2013, independent of their size. Electricity cost intensity is defined as the total electricity cost over gross value added at the firm level.

Source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States of Germany, AFiD-Panel Industriebetriebe 2004–2017, AFiD-Panel Industrieunternehmen 2004–2017, AFiD-Modul Energieverwendung 2004–2017, AFiD-Panel Unternehmensstrukturstatistiken 2008–2017, Material- und Wareneingangserhebung, 2006, 2010, 2014 (henceforth AFiD 2007–17), own calculations. (henceforth AFiD 2007–17), own calculations.

When comparing figures for electricity use in 2013 to their counterparts in 2011, we find an increase for the group of newly REL exempted plants from 5.2 GWh in 2011 to 5.3 GWh in 2013 (column (4)). On the other hand, we see a decrease for non-exempted plants (column (1)) and the group of all REL exempted plants (column (7)). This observation provides the first suggestive evidence that the REL exemption might lead to higher electricity consumption. For completeness, we present the summary statistics for our pooled sample 2007–17 in the Online Appendix (Table A.1).

2.1. *Stylised Facts about Bunching and Exemption Behaviour*

We continue by evaluating firms' bunching behaviour, i.e., the extent to which plants strategically manipulated their electricity consumption to become eligible for the REL exemptions two years later. If the cost to manipulate electricity uses were prohibitively high, we would expect to see a distribution of baseline electricity consumption that is continuous around the eligibility threshold. Otherwise, we would anticipate bunching with a higher density of plants above the threshold.

Table 2. *Bunching Behaviour and RE Levy Exemptions over Time.*

Year	(1) 2007	(2) 2008	(3) 2009	(4) 2010	(5) 2011	(6) 2012	(7) 2013
McCrary test statistic	0.016	0.010	0.004	0.324***	−0.029	0.120	−0.016
	(0.133)	(0.111)	(0.108)	(0.139)	(0.119)	(0.122)	(0.130)
No. of exempted plants	–	–	–	539	579	697	1,574
Exempted eligible plants	–	–	–	72%	76%	75%	65%
REL (cent/kWh)	1.03	1.16	1.32	2.05	3.53	3.59	5.28
Notch present in $t+2$	Yes	Yes	Yes	Yes	No	No	No

Notes: Test statistics from McCrary's test of continuity (McCrary, 2008) for electricity use at the 10 GWh threshold, using default bandwidth calculations (approximately 4 GWh). ∗∗∗ denotes statistical significance at the 1% level. As the heavy right skew in the electricity consumption distribution challenges convergence, plants with an electricity consumption of more than 20 or less than 1 GWh are excluded. SEs are reported in parentheses. Eligibility is determined based on electricity use and (imputed) electricity cost to GVA. Exemption shares are available only after 2009.
Source: AFiD 2007–17, own calculations.

To test for a discontinuity in the density function, we use a test proposed by McCrary (2008) for the years 2007 to 2013.[9] The test statistics from Table 2 demonstrate that bunching was rare despite the economic incentives created by the tax notch. We detect a statistically significant discontinuity only for 2010, when the notched exemption design was still in place and the REL had risen considerably to 2.05 cents per kWh.

For the years prior to 2010, we do not find any evidence of bunching, which can be explained by two factors. First, the REL was relatively low at 1–1.3 cents per kWh so that there was less incentive for bunching than in 2010, when the REL doubled to 2 cents per kWh. Second, the years 2008 and 2009 coincided with the financial crisis that had an unparalleled impact on German manufacturing. During such times of extreme economic uncertainty, it may have been much more difficult to manipulate electricity consumption in order to reach the threshold level of electricity use, compared to times with more predictable economic activity. In 2009, for example, GVA in the manufacturing sector plummeted by 19% and many firms resorted to short-term working arrangements for their employees.

For the years after 2010, we again do not detect any sign of strategic manipulations of electricity use. This finding is in line with the change in exemption rules that was announced in the summer of 2011 and effectively eliminated the incentive to bunch above the 10 GWh eligibility threshold. The evolution of bunching behaviour thus supports the prediction by our model that bunching to reach eligibility under a notched schedule occurs only when benefits of an exemption are sufficiently large (see (1)).

Table 2 also shows that not all eligible plants apply for an exemption. In 2010, the first year covered by our exemption data, only about three out of four (72%) eligible plants claimed an exemption. This percentage increases to about 75% in the two following years. In 2013, the total number of exempted plants in our sample increases to more than 1,500 in response to the reduction in eligibility criteria and the exemption rates declined slightly.

To test whether plants are more likely to claim an exemption when the value of an exemption is higher, Figure 3 plots the exemption rates among eligible plants in 2012 against their baseline consumption two years earlier. Plants with baseline electricity use of less than 10 GWh are not

[9] The McCrary (2008) test statistic for the years 2014–17 does not show any signs of bunching behaviour under the reformed schedule with a statistic of (SEs in parentheses) −0.066 (0.121), −0.108 (0.114), 0.159 (0.137) and −0.052 (0.110), respectively. For visual inspection, we plot the distribution of plants around the 10 GWh threshold for individual years in Online Appendix Figure A.3.

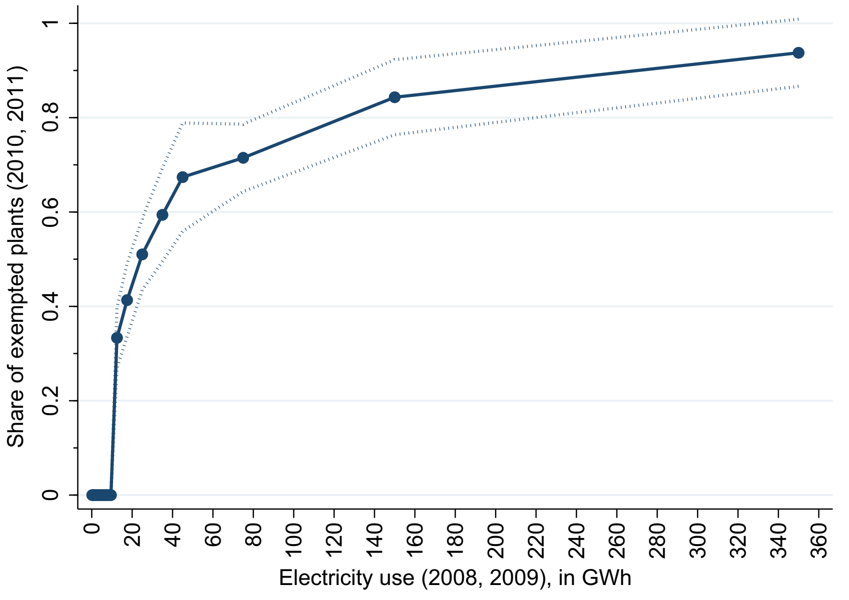

Fig. 3. *Exemption Shares among the Eligible by Baseline Electricity Consumption.*
Notes: Exemption shares are estimated as the sample average in bins and plotted at the bin midpoints (upper bound of the highest bin is 500 GWh). Dotted lines denote 95% confidence intervals (SEs clustered at the plant level).
Source: AFiD 2007–17, own calculations.

eligible and thus have exemption rates of zero. Among the eligible plants, only about 35% with an electricity use just above the 10 GWh threshold claim an exemption. Yet, the percentage increases almost to 100% for plants with an annual electricity use of about 360 GWh. This finding supports the conjecture that firms make a trade-off between the financial benefits of an exemption and the compliance cost associated with its use.

The idea that firm-level barriers such as compliance cost influence exemption decisions is further supported by results from a linear probability model that we estimate for plants that became newly eligible in 2013. Regressing plants' exemption status on plant-level characteristics, we show that the probability of an exemption for eligible plants increases by 40 percentage points when at least one plant of the same firm had been exempted previously, holding plant-level characteristics such as electricity use and cost intensity constant (see Online Appendix Table A.5, column 3). Consistent with the eligibility rules, we also find that higher baseline electricity consumption and higher electricity cost intensity are statistically significant predictors for an exemption.

3. REL Exemptions under the Notched Policy Design

Our first program evaluation focuses on the impact of REL exemptions under the original, notched tax design. Our goal is to estimate the effect of the REL exemption on energy input choices and competitiveness indicators for German manufacturing plants. Throughout our analysis, we follow the potential outcome framework (Rubin, 1974; Splawa-Neyman *et al.*, 1990) and define D_{it} as a treatment indicator that equals one if plant i in year t is exempted and zero otherwise. The potential outcome of plant i in case of treatment is denoted by $Y_{it}(1)$, while $Y_{it}(0)$ denotes the potential outcome in case the plant is not treated, i.e., continues to pay the full REL. We are interested in estimating the average treatment effect on the treated (ATT), given by $ATT = E[Y_{it}(1) - Y_{it}(0) \mid D_{it} = 1]$, where $E[\cdot]$ denotes the expectation operator.

3.1. *Econometric Strategy*

To overcome the fundamental problem of a missing counterfactual, we conduct an RD analysis. The central idea of an RD design is to take advantage of institutional rules that determine the treatment eligibility based on whether a so-called running variable R_i exceeds a cutoff value c. In our example, R_i corresponds to the baseline electricity use and c represents the cutoff value of 10 GWh. As REL exemptions are only granted to plants above the 10 GWh threshold that have applied for the exemption and pass the second eligibility criterion, the design of this policy qualifies for a fuzzy RD, in which the probability of treatment jumps at the threshold (Imbens and Lemieux, 2008).

If plants only imprecisely control the running variable R_i, observations on either side of the cutoff are similar in both observable and unobservable characteristics. This local randomisation can then be exploited to estimate a local average treatment effect for 'compliers' at the cutoff (Lee and Lemieux, 2010), i.e., for plants that are exempted in response to barely passing the 10 GWh threshold. As RD designs closely mimic a randomised experiment, they allow us to estimate treatment effects with a particularly high degree of internal validity. For example, RD designs are robust to business cycle and factor price developments, since they would equally affect the plants marginally above and below the threshold.

The fuzzy RD approach builds on three main identifying assumptions. First, the treatment probability needs to jump at the cutoff value c, an assumption that can be easily verified in the data. Second, passing the threshold is assumed to affect treatment probabilities for all plants in the same direction, so that no plant would be more likely to receive treatment if it lost eligibility, which is very plausible in our empirical setting. Third, the conditional expectations of the potential outcomes, $E(Y_i(j) \mid R_i)$ for $j \in \{0, 1\}$, are assumed to be continuous at the cutoff. This assumption reflects the idea that plants have only imprecise control over the running variable. If manipulation was possible, plants that benefit the most from the exemption would select above the threshold and the conditional expectations of potential outcomes would be discontinuous at the cutoff. To circumvent such concerns, we focus on the baseline years 2008 and 2009 during which the financial crisis led to unprecedented cuts in production levels, which made manipulation of the running variable very costly for firms.

Under these identifying assumptions, the ATT for compliers at the cutoff, which we denote as ATT^{RD}, is defined as (see Imbens and Lemieux, 2008)

$$ATT^{RD} = \frac{\lim_{\epsilon \downarrow 0} E(Y_i \mid R_i = c + \epsilon) - \lim_{\epsilon \uparrow 0} E(Y_i \mid R_i = c + \epsilon)}{\lim_{\epsilon \downarrow 0} E(T_i \mid R_i = c + \epsilon) - \lim_{\epsilon \uparrow 0} E(T_i \mid R_i = c + \epsilon)}, \tag{3}$$

which represents the discontinuity in the outcome variable at the threshold, divided by the discontinuity in the treatment probability. In a setting where the group of treated plants consists exclusively of compliers, as in our case, the estimated treatment effect corresponds to the ATT at the cutoff (Battistin and Rettore, 2008).

The ATT^{RD} can be estimated by replacing the conditional expectations from (3) by sample counterparts, using either parametric or non-parametric techniques. As proposed by Hahn *et al.* (2001), we estimate conditional expectations of the outcome variable by local linear regressions. This method fits linear regressions separately at each side of the threshold, using only observations within a certain bandwidth and weighting them by a kernel function. To decrease sampling variability, extensions of RD designs allow for the inclusion of explanatory variables that are predetermined relative to the running variable R_i (Lee and Lemieux, 2010; Calonico *et al.*, 2019). Given the limited number of plants at the threshold and to improve statistical power, we pool the observations for both outcome years 2010 and 2011 and cluster SEs at the firm level to account for potential serial correlation in the error terms. In addition, we further control for lagged outcome variables (in period $t-3$) in our fuzzy RD regressions. Following Calonico *et al.* (2014; 2019), we determine bandwidths using a fully data-driven selection procedure that minimises the mean squared error (MSE) of the estimator. In the main specification, we employ a triangular kernel. As conventional non-parametric local polynomial estimators tend to over-reject the null hypothesis of no treatment effect, we conduct inference based on robust bias-adjusted confidence intervals that have better coverage rates in finite samples (Calonico *et al.*, 2014).

3.2. *Discussion of Identifying Assumptions*

In line with the discussion in Section 1.3, a key concern for the validity of the fuzzy RD design is the fact that plants may increase their electricity consumption in the baseline years above the eligibility threshold of 10 GWh to benefit from the exemption two years later. Such selection could violate the core identifying assumption, continuity of conditional expectations at the threshold.

As shown by our model, plants will select above the threshold only when it is economically beneficial to do so. In particular, a sufficient condition for plants not to select above the threshold is that the bunching and compliance costs for the exemption exceed its benefits (see (1)). In our context, the profitability of bunching hinges on the magnitude of the bunching cost. As electricity use is highly output dependent in manufacturing, manipulating it to reach eligibility was much more costly in the years of the financial crisis, 2008 and 2009, compared to times with predictable economic activity. The notion that bunching costs were prohibitively high in the years 2008 and 2009 is supported by the absence of any evidence for bunching in those years (see Table 2 and Online Appendix Figure A.4).

Our identification strategy to use times of extraordinary economic circumstances during baseline years may ensure continuity of conditional expectations at the threshold, but could introduce other challenges. First, if the crisis persisted until the outcome years, the external validity of our estimates for non-crisis years might be limited. We argue that this is likely not a problem in our context as the financial crisis was short lived in Germany and led to a quick rebound of economic activity by 2010. Second, if sectors that experienced a fast recovery after the crisis were over-represented on one side of the threshold, our estimates might be biased. Such changes in the sectoral composition may only have little influence on the total number of firms above and below the threshold. Hence, they could be difficult to detect by testing for a discontinuity in the aggregate electricity use distribution. In Online Appendix Table E.2, we show that the sectoral composition in the baseline years is indistinguishable above and below the 10 GWh threshold, which alleviates such concerns.

Another identification challenge could arise if other regulations were based on the same eligibility criteria as those used for REL exemptions. In Germany, for example, firms in the manufacturing sector can obtain an exemption from electricity network charges when they have atypical usage or when they consume more than 10 GWh of electricity in more than 7,000 hours. Despite using the same 10 GWh cutoff, we consider confounding effects from that policy to be negligible for two reasons. First, the predominant reason for granting network charge exemptions is atypical usage (von Graevenitz and Rottner, 2022), which is unlikely to differ across the 10 GWh threshold. Second, exemptions from network charges are granted for the year in which a plant is eligible, while there is a two-year lag for REL exemptions. Hence, both exemption schemes will only take effect in different years, which should eliminate confounding effects. We test this conjecture empirically via placebo regressions in Section 3.4 below.

3.3. *Main Results*

We turn to the estimation of treatment effects for all outcome variables next. To improve the precision of the fuzzy RD estimates, our preferred specification excludes all firms with an *energy cost to GVA* ratio below 15%. We also present results for a second specification where we additionally exclude all firms with a low (imputed) *electricity cost to GVA ratio*. To keep the majority of all treated plants despite the measurement error in electricity cost, we drop firms with an electricity cost to GVA ratio below 10% rather than 15%. This specification excludes further firms that cannot be eligible and thus yields a larger jump in the treatment probability at the threshold (from 0% to about 28% rather than 18%; see the first-stage results reported in Table 3 as well as Online Appendix Figure A.5).[10] For both specifications, we drop as outliers the 1% of observations with the highest or lowest relative changes in electricity consumption between the baseline period (2008 and 2009) and the outcome years (2010 and 2011). We also drop plants with own electricity generation capacities because electricity from own-generation facilities is not subject to the REL.

Figure 4 presents the first graphical evidence on the effect of the REL exemption on electricity use for our main sample, firms with an energy cost to GVA of at least 15%. It plots the electricity consumption in the years of an exemption against the electricity consumption in the baseline period that determines eligibility, superimposing fitted lines from third-order polynomials. The figure shows that plants that slightly exceed the eligibility threshold in the baseline period consume more electricity than those slightly below that threshold two years later. As plants above and below the threshold have virtually identical characteristics, and only differ in their probability of receiving the exemption, this finding indicates that REL exemptions increase plants' electricity use.

The fuzzy RD estimates in Table 3 show that REL exemptions increased electricity consumption on average by approximately 3.1 GWh for exempted plants, an effect that is statistically significant at the 5% level. More specifically, and given the local nature of the RD design, this effect implies that compliers at the cutoff, i.e., exempted plants that consumed around 10 GWh during 2008 and 2009, increase their electricity consumption in 2010 and 2011 by about one-third of their baseline consumption. The results for logged electricity use show that the average relative increase is even larger, yet imprecisely estimated, and amounts to 78%.[11]

[10] The results when excluding all firms with an electricity cost ratio to GVA of less than 15% produce the same qualitative findings, but smaller point estimates (columns 5 and 6 of Online Appendix Table E.1). However, about a hundred treated plants are lost, which makes it difficult to compare these estimates.

[11] Because log differences are large, we convert them to relative treatment effects by calculating $\%\Delta y = 100 \times (exp^{\beta} - 1)$. A larger relative increase arises when plants with low counterfactual electricity use respond more strongly

Table 3. *Fuzzy RD Estimates (at the Cutoff).*

Main sample	Energy cost/GVA > 0.15		Elect. cost/GVA > 0.10	
	ATT^{RD} (1)	SE (2)	ATT^{RD} (3)	SE (4)
Panel A: electricity and fuel usage				
Electricity consumption (GWh)	3.156**	1.402	1.885	1.279
Log electricity consumption	0.578*	0.307	0.32	0.195
Log electricity purchase	0.617*	0.372	0.313*	0.185
Log fossil fuel consumption	−0.119	0.507	0.137	0.429
Share of total energy mix				
Electricity (%)	0.123	0.12	−0.024	0.073
Fossil fuel (%)	−0.186*	0.101	−0.041	0.059
Panel B: CO2 emissions				
Log CO_2, direct	−0.082	0.492	0.18	0.443
Log CO_2, total	0.614*	0.355	0.259	0.242
Panel C: competitiveness indicators				
Log employment	0.152	0.173	0.076	0.119
Log sales	0.374	0.288	0.212	0.191
Export share	−0.118	0.074	−0.028	0.056
Log investment	0.774	1.239	0.142	0.949
$\mathbb{1}$(investment > 0)	−0.166	0.186	−0.100	0.170
$\mathbb{1}$(investment machinery > 0)	−0.113	0.164	−0.17	0.132
No. of observations	39,202		5,608	
No. of exempted plants	497		481	
First stage	0.176		0.284	

Notes: Columns (1) and (2) limit the sample to all energy-intensive firms with an energy cost to GVA ratio above 15% in 2008 and 2009. Columns (3) and (4) further limit the sample to firms with an electricity cost to GVA ratio above 10%. Own electricity producers are omitted from the sample. Number of observations and exempted plants refer to the total number of observations (plants) in the sample, independent of the bandwidth. Each cell represents a separate estimation, based on the MSE-optimal bandwidth selector (Calonico *et al.*, 2019). SEs are clustered at the firm level. * $p < .1$, ** $p < .05$.
Source: AFiD 2007–17, own calculations.

To investigate the channels that underlie the large increase in electricity use, we test whether plants reduced their consumption of other fuels, which could explain part of the large observed increase in electricity consumption. These results are shown in panel A of Table 3. We do not find direct evidence of fuel switching, as shown by the negative, yet statistically insignificant point estimate on (the log of) fossil fuel consumption. Yet, when analysing the shares of different fuels in total energy use, we detect that the REL exemption significantly decreased the share of fossil fuels, while increasing the electricity share by a similar magnitude. These findings show that the positive effect on electricity use cannot be explained by a mere scale effect, i.e., an increase in production levels based on the current input mix, which should leave fuel shares largely unaffected. Rather, it supports the fact that REL exemptions increase the use of electricity.

To investigate how the increase in energy consumption translates into carbon emissions, we report two measures of CO_2 emissions in panel B of Table 3. The first measure corresponds to direct CO_2 emissions that stem from on-site fuel consumption (log CO_2, direct). The second measure also takes into account the indirect emissions embodied in the use of electricity purchased

than plants with high use. This pattern is consistent with large infra-marginal bunching effects of plants that would otherwise not have reached eligibility for an exemption in $t + 2$.

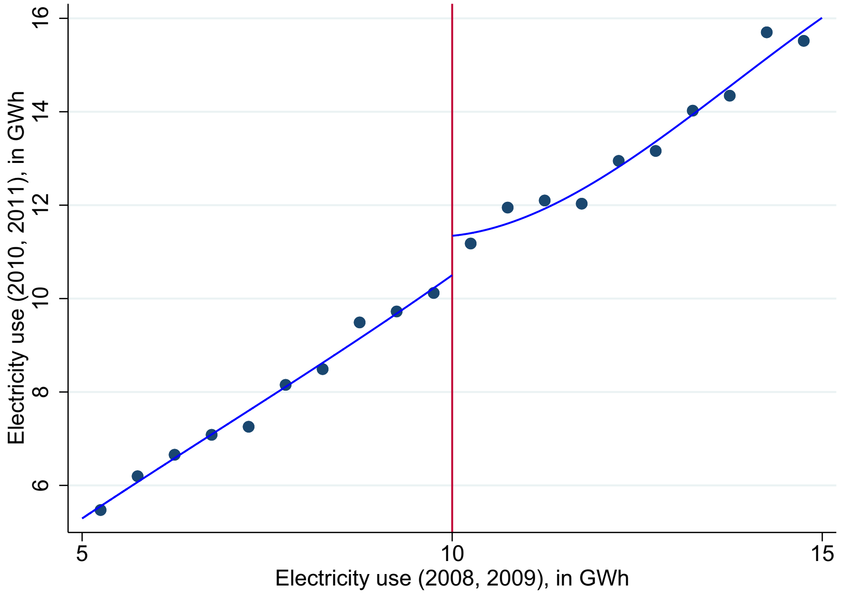

Fig. 4. *Electricity use in 2010 and 2011 versus the Base Period.*
Notes: Electricity consumption in the years 2010 and 2011 corresponds to averages within 0.5 GWh bins of electricity consumption two years prior. The lines represent fitted values from third-order polynomials, estimated separately for both sides of the threshold.
Source: AFiD 2007–17, own calculations.

from utilities (log CO_2, total). Our results show that the increase in electricity consumption led to a surge in total CO_2 emissions by almost 85% (evaluating the point estimate of 0.614 as relative treatment effects), which is statistically significant at the 10% level. By contrast, we do not find any evidence that direct emissions changed. These findings closely mirror our result of a strong increase in the use of electricity, which is associated with a high average carbon emission factor of about 550 g $C0_2$ per kWh in the years 2010–1 in Germany (UBA, 2018b).[12]

Furthermore, plants may be able to expand their competitive position and expand their production, leading to larger electricity use. In this case, we would expect to see an increase in sales and employment, which we investigate in panel C of Table 3. Yet, we do not find any statistically significant impacts of the exemption on any of the variables, which does not allow us to draw strong conclusions about the extent to which higher electricity consumption has been used for productive purposes. In addition, we show that the REL exemptions did not trigger additional investment in machinery or otherwise, which speaks against an expansion of production capacities in response to the exemption that might lead to long-run effects.

[12] As electricity generation in Germany is covered by the European Union Emissions Trading Scheme, an increase in total emissions by the manufacturing plants does not necessarily imply that emissions at the economy level have increased as well. Yet, in response to low permit prices during the end of phase 2 of the EU ETS (2010–12) and the beginning of phase 3 (2013–20), the European Union has decided to introduce a market stability mechanism and to withdraw excessive permits from the market from 2024 onwards (e.g., Perino, 2018). An increase in the demand for emission permits prior to that year reduces the amount of excessive permits that are withdrawn. Hence, total carbon emission may have actually increased in response to the exemption policy.

We then estimate the treatment effects for the sample of plants that have an (imputed) electricity cost to GVA of at least 10%. As shown in column (3) of Table 3, our main estimates are smaller than those presented in column (1), yet remain large in absolute terms. We estimate an average increase in electricity use by about 1.9 GWh at the threshold and a log difference of 0.32, which translates into an average relative effect of approximately 38%. Yet, both effects are not statistically significant at conventional levels (p-values of .14 and .10, respectively).

One reason for our large point estimates under a notched design is that REL exemptions reduce electricity and thus the bunching cost. Hence, they may lead to additional infra-marginal responses by plants that manipulate their electricity use in order to reach eligibility for an exemption two years later, as discussed in Section 1.2. As a result of the sizeable SEs, our fuzzy RD approach does not allow us to determine effect sizes with precision. Rather, we use our structural model to test the plausibility of the magnitude of the bunching response in Section 5 below.

3.4. *Robustness*

To investigate the validity of our fuzzy RD approach, we first provide supportive evidence for two important identifying assumptions: the stable unit treatment value assumption (SUTVA) and the assumption of local randomisation around the eligibility threshold. SUTVA requires the absence of treatment spillovers to non-exempted plants. In our context, SUTVA might be violated for two reasons. First, as plants interact on product and factor markets, exemptions might trigger general equilibrium effects that also influence non-exempted plants. However, general equilibrium effects are unlikely to be substantial in our context, as the only variation in exemptions stems from a limited number of plants that change eligibility status during the study period. In addition, we do not find any significant effects on competitiveness indicators for treated plants, which further reduces concerns about such spillovers. Second, multi-plant firms might shift production from non-exempted plants to exempted plants. We test for the presence of such intra-firm spillovers by restricting our analysis to single-plant firms. As the first column of Online Appendix Table E.1 shows, the point estimates for electricity and fuel variables remain comparable to the main results. However, the estimates lose some of their statistical significance, which is likely due to the smaller sample size.

The identifying assumption of local randomisation implies that all variables measured in the base period are balanced around the cutoff. As a consequence, placebo fuzzy RD regressions on baseline variables should not indicate any discontinuity at the cutoff. This provides us with a powerful test to check whether plants were able to select above the eligibility threshold during the financial crisis. Column 3 of Online Appendix Table E.1 shows that we do not detect any statistically significant effect for variables determined prior to the exemption. This evidence supports local randomisation and also speaks against the concern that the financial crisis affected plants above the threshold differently than plants below the threshold. In that case, we would expect to observe a discontinuity at the threshold for covariates related to these shocks (e.g., sales or employment). This placebo test also reduces concerns about confounding policies that use the same 10 GWh threshold, but become effective with a time lag other than two years.

Furthermore, we show that our findings are similar when we include own electricity producers, yet estimated with less precision (Online Appendix Table E.3). We also find that our results are robust to the choice of the bandwidth used in the estimation, as documented in Online Appendix E.1.

4. REL Exemptions under the Revised Policy Design

In a next step, we investigate the impact of REL exemptions after the 2012 reform that eliminated the tax notch and considerably expanded the group of plants eligible for exemptions. We focus on the impact of the REL exemption under the revised policy in the first year after its implementation in 2013 based on a matching DiD approach that allows us to compare newly exempted plants to highly similar control plants that share a common economic history. In addition, we exploit the availability of outcome data for the years 2014 to 2017 and estimate the intention-to-treat effects in those years.

4.1. *Econometric Strategy*

The matching DiD approach allows us to exploit both the longitudinal structure of our dataset and to use the rich information on plant-level characteristics. In this setting the ATT can be expressed as

$$ATT^{DiD} = \frac{1}{N_1} \sum_{i \epsilon I_1} \left\{ (Y_{it}(1) - Y_{i0}(0)) - \sum_{k \epsilon I_0} W_{N_0,N_1}(i,k)(Y_{kt}(0) - Y_{k0}(0)) \right\}, \tag{4}$$

where Y_{it} refers to the outcome of plant i in the outcome year, $t = 2013$ and Y_{i0} represents the outcome variable in the base year (2011), determining treatment status; I_1 denotes the set of N_1 exempted plants, while I_0 and N_0 refer to the control group. Furthermore, the term W_{N_0,N_1} with $\sum_{k \epsilon I_0} W_{N_0,N_1}(i,k) = 1$ determines the weighting of counterfactual observation k.

The validity of the matching DiD estimator depends on three main identifying assumptions: conditional independence, overlapping support and SUTVA (Heckman *et al.*, 1997). First, conditional independence requires that the (counterfactual) change in the outcome variable in the absence of treatment, $Y_{it}(0) - Y_{i0}(0)$, is independent of the treatment status, conditional on a set of covariates X_{it}. This identifying assumption is weaker than the common trend assumption from standard DiD models as it only has to hold for a subset of control plants that are similar to treated plants in terms of observable plant characteristics. Second, overlapping support requires that the support of the distribution of the conditioning covariates in the control group overlaps with the respective support for the treatment group. This ensures that, for every treated plant, we can find a similar control plant that serves as a counterfactual. This assumption can easily be verified graphically and is met in our setting (see Online Appendix Figure E.2). Third, SUTVA requires that potential outcomes at one plant are independent of the treatment status of other plants. We provide indirect evidence in the next subsection that both SUTVA and conditional independence are credible assumptions in our empirical setting.

For the matching DiD estimation, we restrict our sample to manufacturing plants with an annual electricity consumption in the base year 2011 between 1 and 10 GWh. These are the plants that pass the electricity use threshold after the 2012 reform, but not before. We also drop as outliers the 1% of observations with the highest or lowest relative changes in the electricity consumption to sales ratio between the baseline period and the outcome year. Furthermore, we winsorise the following main balancing variables: electricity use, gas use, electricity share in total energy, sales, export share, and employees at the 1st and 99th percentiles.

We then employ propensity score matching to construct a control group of non-exempted plants that closely match treated plants in terms of pre-treatment covariates for the year 2011. This procedure ensures that control plants have a similar size and electricity intensity as treated plants. To do so, we perform strict matching within the two-digit economic sector (ISIC Rev. 4) based on the following predetermined variables that directly influence the treatment status and plants' potential outcomes in 2013: electricity cost to GVA (and lags thereof), log of sales and log of employment. Including lagged values for the electricity cost to GVA share for up to three years prior to 2011 helps us to match treated and control plants that share a similar economic history. Including further covariates ensures that matching takes into account factors related to firm size that are independent of electricity intensity. As a robustness check, we also employ a minimum specification in which we match within economic subsectors and condition only on energy (electricity) cost to GVA in the base period 2011. Our results are robust to the choice of the variables included in the propensity score, yet, balancing improves through the inclusion of additional covariates.[13]

For matching, we use different algorithms based on nearest-neighbour (NN) matching, NN matching with caliper and replacement, and one-to-many matching with caliper and replacement. Using caliper matching ensures that the characteristics of all nearest neighbours are close to those of the treated plants. Following Rosenbaum and Rubin (1985), we set the caliper to 25% of the SD of the estimated propensity score. To obtain consistent estimates for the SEs, we conduct post-matching inference as suggested by Abadie and Spiess (2022).

4.1.1. *Discussion of identifying assumptions*

Conditional independence requires that changes in outcome variables are independent of the treatment status, conditional on covariates. This assumption is equivalent to the common trend assumption of the standard DiD model and is particularly plausible when conditioning on a set of covariates that affect both treatment assignment and potential outcomes. While untestable in principle, the assumption is more plausible if outcome trends are parallel prior to the policy intervention. For the years 2007 to 2017, Figure 5 plots the evolution of key outcome variables, which we demean with respect to the year 2011. These graphs provide visual evidence that the trends of treated and matched control plants are parallel in the years leading up to the REL exemption. We also observe parallel pre-trends for variables that we did not specifically include in our propensity score specification, such as export share or natural gas consumption. These findings imply that our specification balances treated and control plants in terms of other covariates that might otherwise confound our estimates, as well as potentially unobserved ones. The common trend assumption is also supported by t-tests, which do not show any statistically significant differences in trends for the treatment and control groups prior to 2011, with the exemption of small differences in the trends from 2010 and 2009 to 2011 for the electricity share in total energy (for details, see Online Appendix Table E.5).

A violation of conditional independence could also arise from coinciding policies that differently affected treated and control plants in 2013. For example, the expansion of the eligibility criteria for an REL exemption in 2013 coincides with the start of the third trading phase under the European Union Emissions Trading Scheme (EU ETS). If plants that become newly exempt from the REL also changed their regulation status under the EU ETS, our estimates might partly capture the effect of being regulated under the EU ETS. In Online Appendix Table A.4, we show

[13] If selection into treatment is affected by both transitory and permanent shocks, simulations by Chabé-Ferret (2017) show the possibility of bias and advise to match on covariates from several years and to implement a symmetric difference-in-differences design. By conditioning on several pre-treatment years and analysing first differences, we implement both recommendations in our preferred specification.

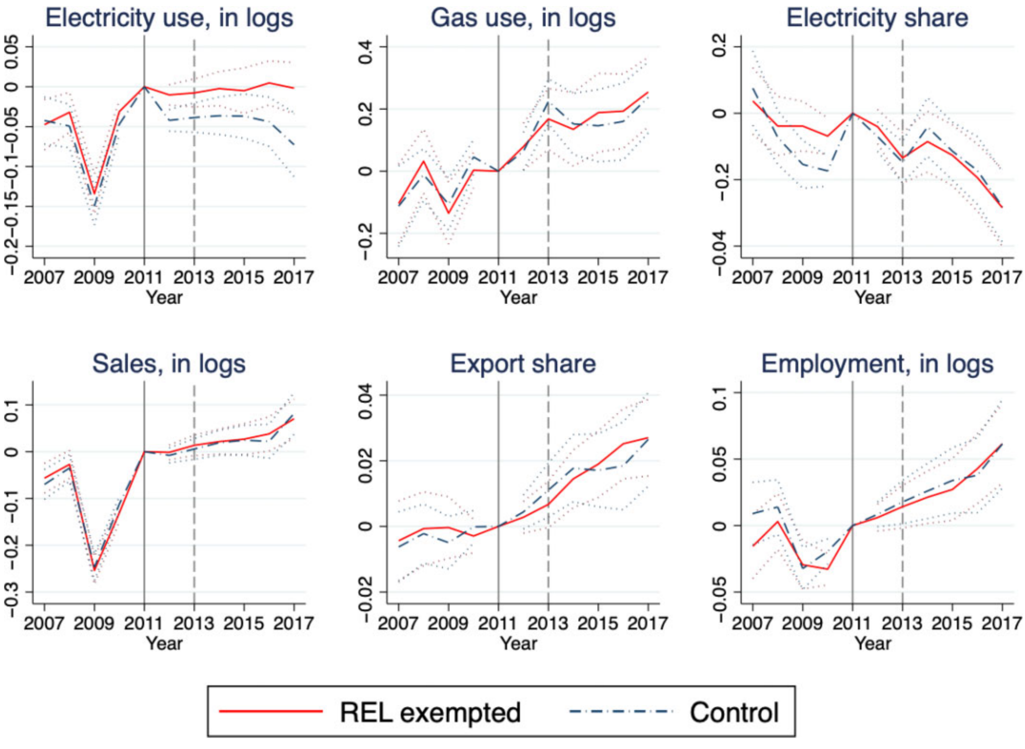

Fig. 5. *Common Trends: Main Matching Specification.*
Notes: Analysis of parallel pre-treatment trends for treated plants (REL exempted in 2013) and matched control plants based on nearest-neighbour matching. The figure plots the growth rate of the respective variables with respect to 2011, the year determining treatment status together with 95% confidence intervals. The vertical lines indicate the baseline year 2011 and the main outcome year 2013.
Source: AFiD 2007–17, own calculations.

that the regulation status under the EU ETS changed only very little for the plants in our sample.[14] Furthermore, the changes are statistically indistinguishable for our treatment and control plants, which reduces concerns about confounding effects.

Under SUTVA, only treated plants may be affected by the treatment. To exclude the possibility of intra-firm spillovers, we estimate our main treatment effect using only the subset of single-plant firms. Another concern might be that the exemption of additional plants can lead to a higher levy for the remaining contributors as the REL is constructed to raise a predetermined level of public funds. However, while the 2012 reform increased the number of exempted plants in manufacturing, it removed exemptions for some energy-intensive sectors outside of manufacturing, such as water supply, recycling and public transportation, which nearly offset the total amount of newly exempted electricity. In addition, spillovers through competition in factor and product markets may be relevant in case exempted firms could strongly improve their competitiveness, which is ultimately an empirical question. We test for these effects formally in the next subsection. As for the RD design, we do not find any short-term competitiveness impacts of the exemptions, which mitigates such concerns.

[14] EU ETS status is drawn from European Commission (2023).

Table 4. *Matching DiD Estimates.*

Main sample	All plants		5–10 GWh	
	ATT^{DiD}	SE	ATT^{DiD}	SE
Δ 2013–2011	(1)	(2)	(3)	(4)
Panel A: electricity and fuel usage				
Electricity consumption (GWh)	0.092*	0.055	0.334**	0.145
Log electricity consumption	0.028**	0.012	0.062**	0.024
Log electricity purchase	0.037***	0.012	0.061***	0.023
Log fossil fuel consumption	−0.055	0.04	−0.041	0.044
Share of total energy mix				
Electricity (%)	0.004	0.005	0.007	0.007
Fossil fuel (%)	−0.008	0.005	−0.016**	0.007
Panel B: CO2 emissions				
Log CO_2, direct	−0.036	0.039	−0.016	0.043
Log CO_2, total	0.017	0.013	0.042*	0.022
Panel C: competitiveness indicators				
Log employment	0.007	0.012	0.021	0.017
Log sales	0.008	0.015	0.016	0.025
Export share	−0.002	0.005	0.015	0.011
Log investment	0.031	0.139	−0.287	0.196
$\mathbb{1}$(investment > 0)	−0.031	0.022	−0.022	0.032
$\mathbb{1}$(investment machinery > 0)	0.026	0.02	0.015	0.032
No. of observations	702		270	
No. of treated plants	351		135	

Notes: Outcome variables defined in differences 2013–2011. The table presents the ATT^{DiD} and SEs from NN matching without replacement following specification (4). The sample is limited to plants that report in both the treatment year and the base year. Inference follows Abadie and Spiess (2022). * $p < .1$, ** $p < .05$, *** $p < .01$.
Source: AFiD 2007–17, own calculations.

4.2. *Main Results*

Table 4 presents the results for ATT^{DiD}, using the main propensity score specification and one-to-one NN matching. Column (1) reports ATT^{DiD} for the group of plants consuming between 1–10 GWh of electricity in the base period and column (3) limits the sample to plants that consume between 5–10 GWh in the baseline period. These plants are more comparable to the plants around the 10 GWh threshold for which we estimated treatment effects under the notched policy design. We calculate SEs based on post-matching inference (Abadie and Spiess, 2022) for NN matching without replacement. All outcomes are expressed as differences between the treatment year (2013) and the year that determines treatment eligibility (2011).

Panel A shows that the REL exemption under the reformed policy schedule led to an increase in electricity consumption by about 3% for all plants (column (1)) and 6% for the plants with an electricity consumption with 5–10 GWh. Both estimates are considerably smaller than the effect sizes found under the notched exemption design. When taking into account that an exemption reduces the marginal electricity price by 31.4%, our estimates imply a short-run price elasticity for electricity in the range between −0.09 and −0.20.[15] In addition, we again find some evidence that plants reduced their share of fossil fuels in total energy use. Our point estimates are negative, and statistically significant for the sample of plants with 5–10 GWh electricity use.

[15] An exemption in 2013 reduces the REL by 90% (REL, 5.28 cent/kWh; average electricity price, 15.11 cent/kWh).

In panel B, we investigate changes in CO_2 emissions. Our estimates for direct CO_2 emissions are negative, yet not statistically significant. Their sign is consistent with our finding that plants reduce fossil fuel consumption. The estimates for total emissions are positive, although only significant for the sample of 5–10 GWh plants that show a stronger electricity use response to the exemption.

In panel C, we investigate how the REL exemptions influence competitiveness indicators in the short run. We find that the point estimates of these variables are all close to zero and not statistically significant at any conventional level. The higher degree of precision compared to the RD design allows us to reject the null hypotheses that employment, sales and the export share have responded strongly to the REL exemptions. Accordingly, our results cast doubt on the effectiveness of REL exemptions to foster the competitiveness of the industry.

4.2.1. *Robustness*

We conduct robustness checks and additional tests of our identifying assumptions in the matching DiD setting. First, we provide an indirect test for *SUTVA* by restricting the analysis to single-plant firms (column 1 of Online Appendix Table E.7). As the REL reform benefited mostly small and medium-sized manufacturing plants from the levy payment, the majority of our sample are single-plant firms, so the concerns for direct spillovers are limited (see Table 1 and Online Appendix Table A.3). The point estimates are aligned with our main results, indicating that intra-firm spillovers are of limited concerns in this setting. Similarly, as we do not find any significant effects of the REL exemption on sales or other competitiveness measures in the short run, we expect no indirect equilibrium effects invalidating our DiD strategy.

Second, we deal with concerns regarding possible *anticipation* from the reform announcement in 2011 by matching on variables from the previous year (column 3 of Online Appendix Table E.7). Plants that knew about the policy change in 2011 may have anticipated future exemptions and adjusted their production in that year already. To test for this possibility, we match the treatment and control groups based on the pre-announcement year 2010, when plants were not yet informed about the reform. Finally, column 3 of the same table excludes own electricity producers from the sample. Both robustness checks confirm our main findings.

In Online Appendix Table E.7, we further show that the main point estimates are unaffected by the matching algorithm, employing NN matching with caliper and replacement and similarly one-to-many matching with caliper and replacement. Similarly, we provide evidence that our main results are robust to the choice of specification for the propensity score specification (Online Appendix E.2.1). In these specifications, we estimate the propensity score only on electricity cost to GVA (energy cost to GVA) within economic subsectors, without using lags or further covariates. In Online Appendix Table E.10, we also estimate two specifications that match on energy use to sales rather than electricity cost to GVA and on the electricity share in total energy use. The results remain almost unchanged, which reduces concerns that differences between treated and controlled plants in electricity prices or in the relative importance of electricity as a factor of production may confound our estimates.

As an additional robustness check, we test whether our findings are robust to alternative estimation approaches that exploit merely the change in eligibility induced by the policy reform. In particular, we estimate the intention-to-treat (ITT) effect in a DiD setting where we exploit only the change in eligibility status due to the 2013 policy reform as treatment. To ensure that differences in electricity intensity between newly eligible and non-eligible plants do not confound our estimates, we restrict the sample to firms with an electricity cost to GVA ratio around the

14% threshold, between 10% and 18% (see Online Appendix E.3 for details). Again, we find a statistically significant increase in electricity use, which supports the findings from our main specification.

Another concern might be that we explore the impact of REL exemptions on sales rather than physical quantities. Previous research by Hintermann *et al.* (2020) estimates a positive energy cost pass-through of 40%–60% for German manufacturing plants. If prices decreased in response to an exemption, a null effect on sales could be consistent with an increase in physical production. Based on the estimates by Hintermann *et al.* (2020), however, we find that the maximum decrease in revenues due to cost pass-through is 0.4%, so that any effect on physical output should be largely similar to what we estimate for sales.[16]

4.2.2. *Long-run effects*

To gauge the long-run impacts of the exemptions, we estimate the ITT effects of an exemption for the years 2014–7. The empirical specification is identical to (4), except that the dependent variable takes as value the difference between the outcome year and the base year 2011. Treatment is determined by the REL exemption status in 2013. Because the number of exempted plants has slightly increased from some 1,700 in 2013 to 2,000 in the subsequent years, the ITT can be interpreted as a lower bound for the average treatment effect in those years.

The estimates, presented in Online Appendix Table A.6, confirm our previous findings. We show that the effect size for log electricity use increases from 3% in 2014 to about 7.7% in 2017. This finding mirrors the slight increase in the REL over time from 5.28 cents per kWh in 2013 to 6.88 cents per kWh in 2017, but also suggests that the responsiveness to REL exemptions increases over time. We obtain negative and statistically significant estimates for the fossil fuel share, which support the findings from our main specification that firms substitute electricity for fossil fuels. For the years 2014 to 2016, we also detect a statistically significant positive effect on investments. Other than that, we again do not find any significant impact on plant-level competitiveness variables.

5. Model Estimation and Counterfactual Simulations

To identify the parameters of our model, we make four structural assumptions. First, we assume that compliance costs C are constant over time and independently distributed according to a lognormal distribution, $C \sim \log N(\mu, \sigma)$, where μ and σ are the mean and SD of the exponentiated normal distribution. Second, we allow for the presence of fixed bunching cost β and variable bunching cost γ, which we assume to increase linearly in the distance to the threshold: $\kappa(x^c) = \beta + \gamma(\hat{x} - x^c)$. Third, we assume that the input demand for electricity in the absence of a notch is isoelastic with an elasticity of η. Fourth, we suppose that firms form expectations about the value of an exemption based on the magnitude of the REL and the electricity use in the respective baseline period.

[16] By definition, $dx/x = ds/s - dp/p$, where dx/x denotes the relative effect of REL exemptions on quantities, whereas ds/s and dp/p denote the effects on sales and prices (in response to cost pass-through). REL exemptions reduce electricity cost for the average newly exempt plant in 2013 with an electricity use of 5.3 GWh (Table 1) by approximately 0.2 million EUR (5.3 − 1 GWh) × 0.9 × 0.0528 EUR/kWh. This amount translates into an equivalent price change of $dp/p = 0.6 \times (-0.2/29.5) = -0.004$ for the average plant with total sales of 29.5 million EUR (Table 1). Hence, the maximum quantity response under pass-through of energy cost will maximally be 0.4 percentage points larger than that which we identify based on revenue data.

The identification of the structural parameters proceeds in three steps (see Online Appendix C for details). First, the input demand elasticity η is identified by our evaluation of the exemption under the reformed design. Second, we identify the parameters of the compliance cost distribution μ and σ by the exemption behaviour of eligible plants. Note that the value of an exemption, $A(x^c(\psi))$, is a function of the electricity demand in the absence of a notch, x^c, which in turn depends on the productivity ψ. For plants outside the bunching range, the counterfactual electricity use x^c equals the observable use x. Hence, we can express the probability of an exemption as

$$Pr_{exempt}(x) = F_c(A(x)) \quad \text{if } x \geq x^u, \tag{5}$$

where x^u is the upper bound of the bunching range (see Online Appendix C for a derivation). Equation (5) links the parameters of the compliance cost distribution to observable firm behaviour and thus enables us to estimate them via maximum likelihood. Intuitively, we exploit the fact that the decision of an eligible firm to not claim an exemption implies that the unobserved compliance costs exceed the exemption value.

Third, we identify the bunching cost parameters β and γ from the following two conditions that characterise firms' bunching behaviour:

$$\lim_{\epsilon \to 0} Pr_{bunch}(\hat{x} - \epsilon) = F_c(A(\hat{x}) - \beta),$$

$$A(x^m(0)) = \beta + \gamma(\hat{x} - x^m(0)).$$

The first condition states that the probability to bunch just below the threshold equals the probability that compliance costs are smaller than the value of an exemption, less the fixed bunching cost. This condition follows from (1) and exploits the fact that variable bunching costs are zero just below the threshold. The second condition states that a marginal buncher with the lowest possible compliance cost $C = \underline{c} = 0$ is indifferent between bunching and not bunching. As we can estimate both statistics using methods from the bunching literature, we obtain a system of two equations with two unknowns, which we solve to identify the bunching cost parameters.

For estimation, we use exemption behaviour among eligible plants in 2012 and the bunching behaviour in the corresponding base period 2010 (see Online Appendix C for details).[17] This allows us to test the plausibility of our model by comparing simulated outcomes with the actual outcomes in all other years. We find that the fitted values from the lognormal distribution closely align with actual exemption behaviour (see Online Appendix Figure C.1). Our estimates for the bunching cost imply a fixed bunching cost of 0.055 million EUR. This value equals roughly one-third of the 2010 exemption value for a plant with an electricity use of 10 GWh, and about one-half of that value in 2008 and 2009, respectively.[18] We estimate a variable bunching cost γ of around 8.2 cents per kWh, which is lower than the average 2010 electricity price of 12 cents per kWh. Hence, the marginal product from using more electricity is positive for bunching firms, for instance because they reduce costly electricity conservation measures.

To assess the efficiency and distributional implications of exemption design features, we simulate market outcomes under two sets of scenarios. In a first set, we test the plausibility of

[17] As a robustness check, we estimate the compliance cost based on a sample of firms with an electricity cost to GVA of at least 25% (see Online Appendix E.4). The results remain virtually unchanged.

[18] An exemption reduces the REL from 2.05 (1.32, 1.16) cents per kWh by 0.05 cents for 90% of baseline use, which yields a value of 0.180 (0.114, 0.999) million EUR for a marginal plant with a baseline use of 10 GWh in 2010 (2009, 2008).

Table 5. *Simulations of Efficiency and Distributional Implications of Policy Designs.*

Panel A: bunching behaviour (in t)

	(1) No. of bunchers	(2) Bunching (GWh)	(3) Max. bunching (%)	(4) Bunching cost (million EUR)	(5) Externality cost (million EUR)
Simulations for bunching in 2008 to 2011 under the respective exemption designs					
(1) 2011 (reformed)	0	–	–	–	–
(2) 2010 (notched)	34	36.8	27.4	4.9	0.6
(3) 2009 (notched)	10	4.2	9.3	1	0.1
(4) 2008 (notched)	2	0.5	2.6	0.2	0
Counterfactual simulations for 2013 under a notched exemption design					
(5) 2011 (notched)	56	55.5	27.4	7.9	1.4
(6) REL 2017	149	275.5	64.1	30.4	6.9
(7) Costless compliance	183	225.5	29.7	29.1	5.6
(8) No fixed bunching cost	83	107.1	39.9	7.8	2.7
(9) No frictions, REL 2017	445	1,144.2	82.3	84.1	28.5

Panel B: exemption behaviour (in $t + 2$)

	(6) No. of exemptions (actual no.)	(7) Electricity use change (GWh)	(8) Exemption value (million EUR; actual value)	(9) Compliance cost (million EUR)	(10) Externality cost (million EUR)
Simulations for exemptions in 2010 to 2013 under the respective exemption designs					
(1) 2013 (reformed)	1,239 (1,574)	2,172.90	3,874 (3,804)	335.7	73
(2) 2012 (notched)	764 (697)	1,514.30	2,531 (2,394)	289.8	38.2
(3) 2011 (notched)	558 (579)	1,306.90	2,146 (2,250)	165.1	32.5
(4) 2010 (notched)	480 (539)	811.9	1,136 (1,220)	122.8	14.2
Counterfactual simulations for 2013 under a notched exemption design					
(5) 2013 (notched)	832	2,081.10	3,681	303	69.9
(6) REL 2017	1,025	2,888.90	5,109	486.3	97.1
(7) Costless compliance	1,319	2,423.60	4,257	0.0	81.4
(8) No fixed bunching cost	859	2,087.10	3,691	305.2	70.1
(9) No frictions, REL 2017	1,581	3,237	5,692	0.0	108.8

Notes: For every scenario, we determine the profit-maximising market behaviour in the baseline period (panel A) and exemption period (panel B). Values represent averages over 200 compliance cost draws. The scenarios in rows (1)–(4) simulate market behaviours under the actual exemption designs that were in place from 2010 to 2013. The scenarios in rows (5)–(9) assume that a notched exemption regime was in place in 2013. In rows (6)–(8), we additionally set the REL to 2017 levels (6.88 cents per kWh), eliminate compliance cost and set the fixed bunching cost to zero, respectively. Scenario (9) simultaneously implements all these three changes. The results shown in the columns are aggregate sums, with the exception of the maximum bunching response from column (6). The exemption value is calculated by taking the magnitude of the REL and the respective exemption rules into account. Externality costs are calculated as explained in Online Appendix F.
Source: AFiD 2007–17, own calculations.

our model by comparing simulated with actual bunching and exemption behaviour (rows (1) to (4) of Table 5). In a second set, we conduct counterfactual simulations of market behaviour assuming that a notched regime had continued to exist in 2013, that the 2013 REL had been at 2017 levels, that compliance was costless and that fixed bunching costs were absent (rows (5) to (9) of Table 5).

For every scenario, we draw 200 realisations of the compliance cost and then determine the profit-maximising bunching, exemption and input use behaviour. The values presented in Table 5 are averages across all simulations. The simulations provide us with a quantification of the number of bunchers and exempted plants, as well as the total increase in electricity use due to the bunching behaviour and the exemption, respectively. We also assess the efficiency implications by calculating the total bunching and compliance costs that plants incur. To assess the externality cost from changes in electricity use, we first determine the average wedge between the social cost of electricity and the input prices paid by firms for the years 2008 to 2013 following Borenstein and Bushnell (2022) (see Online Appendix F for details). We find that the social cost of electricity exceeded the cost paid by firms by 1.28 to 3.36 cents per kWh. We then multiply these wedges with the electricity use change in a given year to obtain a measure for the externality cost.

The results from the simulations in rows (1) to (4) confirm that our model captures key features of actual exemption behaviour. As shown in columns (6) and (8), the number of exempted plants and the value of an exemption predicted by our model closely mimic the actual numbers, which we display in parentheses. We simulate that only few plants would bunch over the eligibility threshold in the years 2008 and 2009 (column (1)), while bunching considerably increases in 2010. This finding reflects the fact that the value of an exemption was relatively small in 2008 and 2009, compared to 2010. Hence, only small increases in the bunching cost due to the financial crisis suffice to reduce bunching to zero in 2008 and 2009.

Column (3) clarifies that infra-marginal bunching effects can be substantial. We find that the maximal increase in electricity use because of bunching amounts to 26.9% in 2010. This finding supports the hypothesis that average treatment effects under a notched regime may be particularly large. In our example, the net bunching response for the plant with the largest bunching response is $26.9\% - 2.8\% = 24.1\%$, and thus exceeds the marginal price response by one order of magnitude. Yet, our simulations (2)–(4) also demonstrate that the overall bunching cost (column (4)) and externality cost from bunching (column (5)) were minor from an aggregate perspective, reaching 4.7 and 0.6 million EUR in 2010, respectively. By contrast, we find that the total compliance cost and externality cost from an exemption two years later amounted to 289.9 and 38.2 million EUR in 2012, respectively (row (2), columns (9) and (10)).

Our second set of counterfactual simulations explore how market behaviour would have evolved in 2013 if the notched design had still been in place (row (5)). In that case, we find that bunching would have substantially increased to fifty-six bunching plants and a total bunching effect of 56 GWh. As row (6) shows, this increase would have been even more drastic if the REL levy was at 2017 levels (6.88 cents per kWh). In this scenario 149 plants would bunch and increase their electricity by 276 GWh to reach eligibility. The exemptions would have also led to a far greater redistributional burden (5,109 million EUR of exemption value) and an externality cost of about 97 million EUR. Furthermore, the compliance cost would have increased to 486 million EUR as more plants would have claimed an exemption. Hence, one reason why the notched design had only limited distortive effects in the years prior to 2013 is that the REL was sufficiently low.

Another reason for this finding is the presence of compliance cost. Had compliance cost been zero, the increase in the number of bunching and exempted plants would have reached 183 and 1,319, respectively (row (7)). This result suggests that policy makers face a trade-off when designing notched exemption schemes with more or less stringent organisational requirements: higher requirements and thus compliance costs reduce rent-seeking behaviour through bunching and limit the number of exemption claims, but impose substantial costs on firms (e.g., row (5), column (9)). By contrast, we find that the absence of fixed bunching costs would change market outcomes only little (row (8)).

When we set the REL to 2017 levels and eliminate compliance and fixed bunching costs, we find that 445 plants would bunch and increase their electricity use by about 1.1 TWh of electricity merely to reach eligibility for an exemption two years later (row (9)). This results clarifies that the distortive effects from notches have significant adverse aggregate impacts when the stakes are high and frictions through bunching and compliance costs are absent. In this scenario, the exemptions would have caused a redistributive burden of about 5,700 million EUR annually, and an increase in electricity use by 3.2 TWh, which translates into an externality cost of 108.8 million EUR.

6. Conclusion

This paper analyses how a large electricity tax exemption scheme, the exemption from the German REL, affects the use of energy inputs and production outcomes of manufacturing plants. Our findings show that REL exemptions lead to significant increases in electricity consumption under two exemption designs. We find that exempted plants increased their electricity consumption on average by approximately 3% in 2013, when a reformed design without notches was in place. By contrast, the effect sizes under the original (notched) schedule were about one order of magnitude larger. Our analysis also highlights the importance of compliance cost and the stakes involved for understanding market behaviour under notched policy designs. While bunching was only of limited relevance in the years 2008 to 2011, we show that it would have led to an increase in electricity use of about 1,144 GWh had the REL levels increased to 2017 levels and compliance cost been absent.

By contrast, we do not find statistically significant impacts of the REL exemption on competitiveness indicators such as sales, export share or employment. This evidence contrasts with the goal of exemption policies to sustain competitiveness and domestic production of manufacturing plants. It casts doubt on the effectiveness of a costly exemption policy that puts an additional burden on all electricity consumers (for distributional implications of other renewable energy policies, see, e.g., Reguant, 2019). Our results thus suggest the use of other policy instruments against leakage, such as carbon-border adjustments or output-based subsidies (e.g., Fowlie *et al.*, 2016).

Regarding external validity, we identify the exemption effects for a group of energy-intensive plants with about 1–10 GWh of electricity use. It would be interesting to know whether these estimates can be extrapolated to larger plants. Yet, as exogenous variation in exemptions is absent for these plants, empirical designs to evaluate the causal effect of these exemptions face fundamental identification problems. Similarly, price shocks that exceed the price variation we use for identification may produce different firm-level responses. It may thus be difficult to conclude from our study that the current drastic increase in energy input prices does not affect firms' competitiveness.

Taken together, our findings caution against defining the eligibility for an exemption based on production inputs. Furthermore, they show that exemptions for EITE plants may not be justified on the grounds of competitiveness concerns, at least for medium-sized plants. Both insights allow policy makers to optimise the design of exemption policies in order to sustain domestic production levels, while minimising cost and production input distortions. More generally, our findings are also useful to improve support policies in other contexts. For example, policy makers worldwide have decided to support businesses against demand reductions induced by a pandemic and soaring energy input costs. The design features of such policies are likely to interact with market outcomes, and our findings may prove useful in avoiding welfare losses due to unintended consequences of design choices.

University of Mannheim & RWI - Leibniz Institute for Economic Research, Germany
Toulouse School of Economics, University of Toulouse Capitole, France

Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

References

Abadie, A. and Spiess, J. (2022). 'Robust post-matching inference', *Journal of the American Statistical Association*, vol. 117(538), pp. 983–95.
Almunia, M. and Lopez-Rodriguez, D. (2018). 'Under the radar: The effects of monitoring firms on tax compliance', *American Economic Journal: Economic Policy*, vol. 10(1), pp. 1–38.
BAFA. (2010–2013). 'Hintergrundinformationen zur Besonderen Ausgleichsregelung', Bundesamt für Wirtschaft und Ausfuhrkontrolle, https://www.bafa.de/SharedDocs/Downloads/DE/Energie/bar_statistik.html (last accessed: 16 June 2015).
Battistin, E. and Rettore, E. (2008). 'Ineligibles and eligible non-participants as a double comparison group in regression-discontinuity designs', *Journal of Econometrics*, vol. 142(2), pp. 715–30.
BDEW. (2022). 'BDEW-strompreisanalyse Juli 2022', https://www.bdew.de/media/documents/220727_BDEW-Strompreisanalyse_Juli_2022.pdf (last accessed: November 2023).
Bernard, A.L., Fischer, C. and Fox, A.K. (2007). 'Is there a rationale for output-based rebating of environmental levies?', *Resource and Energy Economics*, vol. 29(2), pp. 83–101.
Böhringer, C., Carbone, J.C. and Rutherford, T.F. (2012). 'Unilateral climate policy design: Efficiency and equity implications of alternative instruments to reduce carbon leakage', *Energy Economics*, vol. 34(S2), pp. S208–17.
Bond, E. (1981). 'Tax holidays and industry behavior', *The Review of Economics and Statistics*, vol. 63(1), pp. 88–95.
Borenstein, S. and Bushnell, J.B. (2022). 'Do two electricity pricing wrongs make a right? Cost recovery, externalities, and efficiency', *American Economic Journal: Economic Policy*, vol. 14(4), pp. 80–110.
Calonico, S., Cattaneo, M.D., Farrell, M.H. and Titiunik, R. (2019). 'Regression discontinuity designs using covariates', *The Review of Economics and Statistics*, vol. 101(3), pp. 442–51.
Calonico, S., Cattaneo, M.D. and Titiunik, R. (2014). 'Robust non-parametric confidence intervals for regression-discontinuity designs', *Econometrica*, vol. 82(6), pp. 2295–326.
Chabé-Ferret, S. (2017). 'Should we combine difference in differences with conditioning on pre-treatment outcomes?', Working Paper 17-824, Toulouse School of Economics.
Chen, Z., Liu, Z., Suárez Serrato, J.C. and Xu, D.Y. (2021). 'Notching R&D investment with corporate income tax cuts in China', *American Economic Review*, vol. 111(7), pp. 2065–100.
Commins, N., Lyons, S., Schiffbauer, M. and Tol, R.S. (2011). 'Climate policy & corporate behavior', *The Energy Journal*, vol. 32(4), pp. 51–68.
Dechezleprêtre, A. and Sato, M. (2017). 'The impacts of environmental regulations on competitiveness', *Review of Environmental Economics and Policy*, vol. 11(2), pp. 183–206.
Deschenes, O. (2012). 'Climate policy and labor markets', in (D. Fullerton and C. Wolfram, eds.), *The Design and Implementation of US Climate Policy*, pp. 37–52, Chicago: University of Chicago Press.
European Commission. (2023). 'European Union transaction log', https://ec.europa.eu/clima/ets/oha.do?form=oha&languageCode=en&accountHolder=&installationIdentifier=&installationName=&permitIdentifier=&search=Search&searchType=oha&currentSortSettings= (last accessed: November 2023).
Eurostat. (2023). 'Electricity prices for non-household consumers—bi-annual data 2007-2017 (nrg_pc_205)', https://ec.europa.eu/eurostat/databrowser/product/view/nrg_pc_205?lang=en (last accessed: November 2023).
Fischer, C. and Fox, A.K. (2012). 'Comparing policies to combat emissions leakage: Border carbon adjustments versus rebates', *Journal of Environmental Economics and Management*, vol. 64(2), pp. 199–216.
Fowlie, M., Holland, S.P. and Mansur, E.T. (2012). 'What do emissions markets deliver and to whom? Evidence from Southern California's NO x trading program', *American Economic Review*, vol. 102(2), pp. 965–93.
Fowlie, M. and Reguant, M. (2018). 'Challenges in the measurement of leakage risk', *AEA Papers and Proceedings*, vol. 108(May), pp. 124–9.
Fowlie, M., Reguant, M. and Ryan, S.P. (2016). 'Market-based emissions regulation and industry dynamics', *Journal of Political Economy*, vol. 124(1), pp. 249–302.
Fowlie, M.L. (2009). 'Incomplete environmental regulation, imperfect competition, and emissions leakage', *American Economic Journal: Economic Policy*, vol. 1(2), pp. 72–112.
Garicano, L., Lelarge, C. and Van Reenen, J. (2016). 'Firm size distortions and the productivity distribution: Evidence from France', *American Economic Review*, vol. 106(11), pp. 3439–79.

Greenstone, M. (2002). 'The impacts of environmental regulations on industrial activity: Evidence from the 1970 and 1977 Clean Air Act amendments and the census of manufactures', *Journal of Political Economy*, vol. 110(6), pp. 1175–219.

Greenstone, M., List, J.A. and Syverson, C. (2012). 'The effects of environmental regulation on the competitiveness of US manufacturing', Working Paper 18392, National Bureau of Economic Research.

Hahn, J., Todd, P. and van der Klaauw, W. (2001). 'Identification and estimation of treatment effects with a regression-discontinuity design', *Econometrica*, vol. 69(1), pp. 201–9.

Harju, J., Matikka, T. and Rauhanen, T. (2019). 'Compliance costs vs. tax incentives: Why do entrepreneurs respond to size-based regulations?', *Journal of Public Economics*, vol. 173(May), pp. 139–64.

Heckman, J.J., Ichimura, H. and Todd, P.E. (1997). 'Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme', *The Review of Economic Studies*, vol. 64(4), pp. 605–54.

Hintermann, B., Žarković, M., Di Maria, C. and Wagner, U.J. (2020). 'The effect of climate policy on productivity and cost pass-through in the German manufacturing sector', Working Paper 2020/11, Wirtschaftswissenschaftliches Zentrum.

Imbens, G.W. and Lemieux, T. (2008). 'Regression discontinuity designs: A guide to practice', *Journal of Econometrics*, vol. 142(2), pp. 615–35.

Kahn, M.E. and Mansur, E.T. (2013). 'Do local energy prices and regulation affect the geographic concentration of employment?', *Journal of Public Economics*, vol. 101(May), pp. 105–14.

Kleven, H.J. (2016). 'Bunching', *Annual Review of Economics*, vol. 8(October), pp. 435–64. https://doi.org/10.1146/annurev-economics-080315-015234.

Lee, D.S. and Lemieux, T. (2010). 'Regression discontinuity designs in economics', *Journal of Economic Literature*, vol. 48(2), pp. 281–355.

Lucas, R.E. (1978). 'On the size distribution of business firms', *The Bell Journal of Economics*, vol. 9(2), pp. 508–23.

Marin, G. and Vona, F. (2021). 'The impact of energy prices on socioeconomic and environmental performance: Evidence from French manufacturing establishments, 1997–2015', *European Economic Review*, vol. 135(June), p. 103739.

Martin, R., Muûls, M. and Wagner, U.J. (2016). 'The impact of the European Union emissions trading scheme on regulated firms: What is the evidence after ten years?', *Review of Environmental Economics and Policy*, vol. 10(1), pp. 129–48.

Martin, R., de Preux, L.B. and Wagner, U.J. (2014a). 'The impact of a carbon tax on manufacturing: Evidence from microdata', *Journal of Public Economics*, vol. 117(September), pp. 1–14.

Martin, R., Muûls, M., de Preux, L.B. and Wagner, U.J. (2014b). 'Industry compensation under relocation risk: A firm-level analysis of the EU emissions trading scheme', *American Economic Review*, vol. 104(8), pp. 2482–508.

Mast, E. (2020). 'Race to the bottom? Local tax break competition and business location', *American Economic Journal: Applied Economics*, vol. 12(1), pp. 288–317.

Mata, J. and Guimarães, P. (2019). 'Temporary investment incentives and divestment by foreign firms', *Oxford Economic Papers*, vol. 71(1), pp. 166–86.

McCrary, J. (2008). 'Manipulation of the running variable in the regression discontinuity design: A density test', *Journal of Econometrics*, vol. 142(2), pp. 698–714.

OECD. (2001). *Environmentally Related Taxes in OECD Countries*, Paris: OECD Publishing.

OECD. (2015). *Taxing Energy Use: OECD and Selected Partner Economies*, Paris: OECD Publishing.

OECD. (2020). 'Policy instruments for the environment database', https://www.oecd.org/environment/indicators-modelling-outlooks/policy-instruments-for-environment-database/ (last accessed: 26 October 2022).

Perino, G. (2018). 'New EU ETS phase 4 rules temporarily puncture waterbed', *Nature Climate Change*, vol. 8(4), pp. 262–4.

RDC of the Federal Statistical Office and Statistical Offices of the Federal States of Germany, AFiD-Modul Energieverwendung. (2004–2017). doi: 10.21242/43531.2021.00.03.1.1.0.

RDC of the Federal Statistical Office and Statistical Offices of the Federal States of Germany, AFiD-Panel Industriebetriebe. (2004–2017). doi: 10.21242/42111.2021.00.01.1.1.0.

RDC of the Federal Statistical Office and Statistical Offices of the Federal States of Germany, AFiD-Panel Industrieunternehmen. (2004–2017). doi: 10.21242/42221.2021.00.01.1.1.0.

RDC of the Federal Statistical Office and Statistical Offices of the Federal States of Germany, AFiD-Panel Unternehmensstrukturstatistiken. (2008–2017). doi: 10.21242/42231.2019.00.05.1.1.1.

RDC of the Federal Statistical Office and Statistical Offices of the Federal States of Germany, Material- und Wareneingangserhebung. (2006, 2010, 2014). https://www.destatis.de/DE/Themen/Branchen-Unternehmen/Industrie-Verarbeitendes-Gewerbe/Publikationen/Downloads-Struktur/material-und-wareneingangserhebung-2040424149005.html (last accessed: November 2023).

Reguant, M. (2019). 'The efficiency and sectoral distributional impacts of large-scale renewable energy policies', *Journal of the Association of Environmental and Resource Economists*, vol. 6(S1), pp. S129–68.

Rosenbaum, P.R. and Rubin, D.B. (1985). 'Constructing a control group using multivariate matched sampling methods that incorporate the propensity score', *The American Statistician*, vol. 39(1), pp. 33–8.

Rubin, D.B. (1974). 'Estimating causal effects of treatments in randomized and nonrandomized studies', *Journal of Educational Psychology*, vol. 66(5), pp. 688–701.

Sallee, J.M. and Slemrod, J. (2012). ‘Car notches: Strategic automaker responses to fuel economy policy’, *Journal of Public Economics*, vol. 96(11–12), pp. 981–99.

Splawa-Neyman, J., Dabrowska, D.M. and Speed, T.P. (1990). ‘On the application of probability theory to agricultural experiments. Essay on principles. Section 9’, *Statistical Science*, vol. 5(4), pp. 465–72.

UBA. (2018a). *Berichterstattung unter der Klimarahmenkonvention der Vereinten Nationen und dem Kyoto-Protokoll 2012: Nationaler Inventarbericht zum Deutschen Treibhausgasinventar 1990-2010*, Dessau: Umweltbundesamt.

UBA. (2018b). *Entwicklung der spezifischen Kohlendioxid-Emissionen des deutschen Strommix in den Jahren 1990-2017*, Dessau: Umweltbundesamt.

von Graevenitz, K. and Rottner, E. (2022). ‘Do manufacturing plants respond to exogenous changes in electricity prices? Evidence from administrative micro-data’, Discussion Paper 22-038, ZEW—Centre for European Economic Research.

The Economic Journal, **134** (*October*), 2803–2834 https://doi.org/10.1093/ej/ueae048
Advance Access Publication Date: 20 June 2024