Discussion Paper Series – CRC TR 224

Discussion Paper No. 388
Project B 07

Energy Tax Exemptions and Industrial Production

Andreas Gerster [1]
Stefan Lamp [2]

December 2022

[1] University of Mannheim, Email: gerster@uni-mannheim.de
[2] Universidad Carlos III Madrid, University of Toulouse Capitole, Email: slamp@eco.uc3m.es

Support by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) through CRC TR 224 is gratefully acknowledged.

Collaborative Research Center Transregio 224 - www.crctr224.de
Rheinische Friedrich-Wilhelms-Universität Bonn - Universität Mannheim

Energy Tax Exemptions and Industrial Production

Andreas Gerster [*] Stefan Lamp [†]

December 22, 2022

Environmental policies are often accompanied by exemptions for energy-intensive and trade-exposed industrial firms to avoid leakage from regulated to unregulated jurisdictions. 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 different policy designs, we show that exempted plants significantly increase their electricity use. This effect is considerably larger under a notched exemption policy, where passing an eligibility threshold yields infra-marginal benefits, compared to a revised policy where these benefits have been largely removed. We detect no significant impact of the exemptions on production levels, export shares, and employment. Using counterfactual simulations, we document substantial distortive effects of notched exemption policies when financial stakes are high and compliance cost for firms are low.

Keywords: Environmental policy, leakage, energy taxes, manufacturing industry.

JEL classification: D22, H23, L60, Q41.

Acknowledgements: 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), and the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation program (grant agreement No 772331), respectively.

[*]University of Mannheim, L7 3-5, 68161 Mannheim, Germany, and RWI - Leibniz Institute for Economic Research, Essen, Germany; phone: +49 621-181-1791, e-mail: gerster@uni-mannheim.de.

[†]Universidad Carlos III Madrid, Spain; phone: +34 91-624-9652, e-mail: slamp@eco.uc3m.es; Toulouse School of Economics, University of Toulouse Capitole, Toulouse, France.

1. Introduction

In the absence of legally binding international agreements, many environmental regulations apply only in some jurisdictions, but not in others. Policy makers are concerned about incomplete regulation as it can cause 'leakage' of industrial activity and emissions from regulated to unregulated jurisdictions, which may undermine the effectiveness of domestic environmental policies (e.g., Fischer and Fox 2012; Fowlie and Reguant 2018). In practice, a widespread policy against leakage is the exemption of the energy-intensive and trade-exposed (EITE) industry from energy or carbon taxes.[1] The introduction of energy tax exemptions in many industrialized countries, such as France, Germany, Italy, and the United Kingdom, has triggered a controversial policy debate. While proponents argue that exemptions are necessary to sustain domestic production levels, critics worry that they might offset incentives for improving energy efficiency 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 stylized 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

[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). Many of the exemptions apply to the transport and agriculture sectors, but energy tax exemptions for EITE industries are no exception. In the United Kingdom, Belgium, and Finland, for example, they are granted to manufacturing firms in EITE sectors. In France, Germany, the Netherlands, and Italy, they can be claimed by EITE firms with an energy use above pre-defined eligibility thresholds.

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 stylized facts about the exemption and bunching behavior of firms. First, under the notched exemption design, only few firms bunch above the eligibility threshold. To rationalize such behavior, we allow for the presence of 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 cost that may arise when claiming an exemption. Compliance cost 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 derive 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. Almunia and Lopez-Rodriguez 2018; Kleven 2016).

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.

Results from the counterfactual simulations provide an explanation for the difference in effect sizes. In particular, we find that inframarginal 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 compliance cost and the stakes involved for understanding market behavior 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 1,000 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 Mio. EUR 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 difference-in-differences (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 Fowlie, 2009; Fowlie et al., 2016; Martin et al., 2014a; Bernard et al., 2007). A result from this literature is that exemptions of EITE industrial plants are inferior to border tax adjustments or output-based rebates (e.g., Fowlie et al. 2016; Böhringer et al. 2012). 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., Fowlie et al. 2012, Greenstone 2002, Greenstone et al. 2012, Martin et al. 2014b, as well as Martin et al. 2016 and Dechezleprêtre and Sato 2017 for reviews.) Furthermore, Martin et al. (2014b) estimate the effect of the climate change levy on production of manufacturing plants in the UK, using plants that were exempted from the levy as control group.

Our evaluation goes beyond the previous studies by investigating how design features of the exemption regime affect the behavior of market participants. We document that a substantial share of eligible plants do not claim an exemption to avoid compliance cost from having to comply with organizational requirements. We also find evidence of 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

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

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 behavior. 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 labor 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 find that substantial compliance cost prevent small businesses from increasing their gross value added beyond a threshold for inclusion into the value added tax system.

We provide novel evidence how notched exemptions of production input taxes affect firm behavior in the industry. In particular, we combine reduced-form policy evaluations with an analysis of bunching behavior to estimate bunching and compliance cost, which we show to be non-negligible. Our estimates imply that manipulating electricity 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, organizational requirements that cause compliance cost mitigate welfare-reducing rent-seeking behavior 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 in manufacturing (Marin and Vona, 2021), but also modestly decrease employment (e.g., Deschenes 2012, Commins et al. 2011), and co-determine the location of firms (Kahn and Mansur, 2013). A novelty of our setting is that we can exploit large price variation to identify price elasticities.

The remainder of this paper is structured as follows. In Section 2, we describe the institutional details of the REL exemptions and discuss how differences in policy design influence input choices. Section 3 introduces our data and describes the assignment mechanism. The empirical analysis is divided into three parts. In Section 4, we investigate the impact of REL exemptions under the original policy design, while we evaluate their impact after the 2012 reform in Section 5. Section 6 describes how we estimate the production model and conduct counterfactual analyses to highlight the efficiency and distributional implications of exemption policy designs. Finally, Section 7 concludes.

2. Institutional background

2.1. REL exemptions and electricity prices

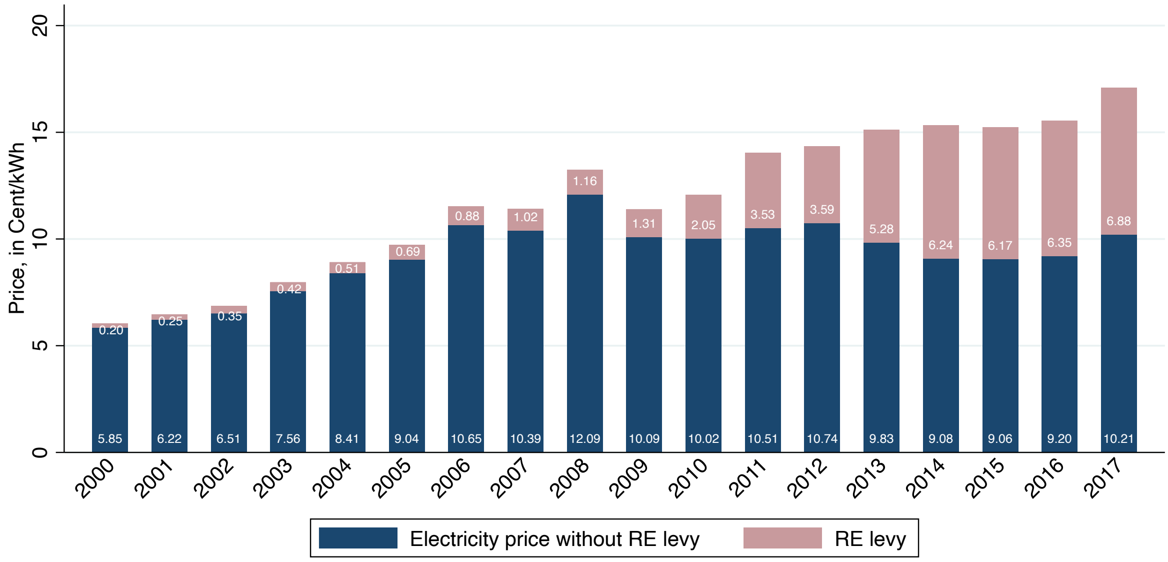
In 2000, the German *Renewable Energy Act* introduced one of the world's most ambitious renewable energy support schemes. Its core element is the provision of generous feed-in tariffs (FiTs) to producers of electricity from renewable sources. FiTs guarantee long-term investment security by providing 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 Renewable Energy Levy (REL), a per kWh 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 cent 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

[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 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.

Figure 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 GWh and 20 GWh (BDEW, 2022).

government has introduced exemptions from the REL for energy-intensive plants from 2003 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 modernize 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 GWh 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 consequence, the number of exempted plants increased from 683 in 2012 to 1,663 in 2013 (see 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 its 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 GWh 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.

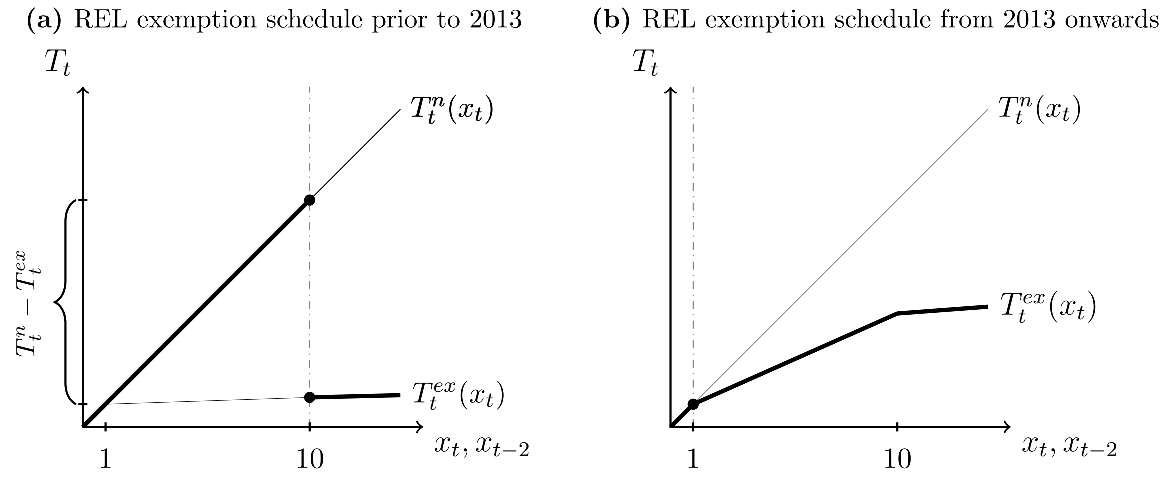
2.2. Incentives under both REL exemption designs

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-cents / kWh to the REL in 2013. Source: BAFA (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, 2013). Data on plant applications and rejections are only available at the aggregate level.

Figure 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 the eligibility threshold of 10 and 1 GWh in the two policy designs. 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).

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 Kleven, 2016; Sallee and Slemrod, 2012). 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.

2.3. Production input choices and policy design

To understand the potential impact of REL exemptions on electricity use under both policy designs, we develop a stylized 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 normalized to one.

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

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

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 cost 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 cost. Bunching cost 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 Appendix Section 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:

$$A(\psi) \geq C. \tag{2}$$

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

$$\frac{\partial x^*}{\partial t^{ex}} = \mathrm{MPR} + \mathrm{BR}, \tag{3}$$

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 compliance and bunching cost is prohibitively high. Second, eligible firms may choose not to apply for an exemption if it involves compliance cost 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

[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.

the net bunching response, which enters additively into Equation (3). We test these predictions in the following empirical sections of this paper.

3. 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 centers of the Statistical Offices of the Federal States and covers the universe of plants from the manufacturing sector with more than 20 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 14 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 this 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 Appendix Section D).

We link our data with the full list of plants that are exempted from paying the REL. These data is 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.

[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 2010-11. 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).

Table 1: Summary statistics, 2013

VARIABLE	Not exempt Mean (1)	Std. dev. (2)	Obs. (3)	REL exempt: 1-10 GWh Mean (4)	Std. dev. (5)	Obs. (6)	REL exempt: all Mean (7)	Std. dev. (8)	Obs. (9)
Plant-level data									
Economic covariates									
Sales, in 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, in million €	1.22	15.05	41,020	0.76	4.01	652	2.32	7.49	1,890
Avg. wage per employee, thd. €	34.01	13.65	40,471	33.95	10.39	664	38.7	15.23	1,817
Energy-related covariates									
Electricity use, in GWh	3.56	47.51	40,224	5.34	3.53	660	46.03	151.45	1,805
Electricity use (2011), in GWh	3.64	45.21	38,251	5.24	2.75	673	55.57	186.99	1,574
Other energy use , in GWh	15.23	618.82	41,269	10.42	22.15	660	124.53	741.48	1,850
Own electricity generation, in %	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, in 1,000 t	5,377	180,836	41,272	4,896	4,960	660	50,185	228,659	1,850
Direct CO_2 emissions, in 1,000 t	3,713	175,362	41,272	1,828	4,287	660	25,507	194,130	1,850
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, in million €	25.68	264.62	14,755	44.25	610.88	255	43.77	356.69	853
Total energy cost, in million €	1.67	14.91	14,754	1.52	2.78	255	10.46	24.25	853
Total electricity cost, in million €	0.42	4.55	36,560	1.07	6.19	530	5.77	23.37	1,374
Electricity cost intensity, in %	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 defined as total electricity cost over gross value added at the firm level. Source: Research Data Centers of the Federal Statistical Offices and the Statistical Offices of the Länder: AFiD Panel Manufacturing Plants, AFiD Module Energy Use, and Cost Structure Survey, 2007-2017, own calculations.

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 Mio. EUR, respectively), these plants use considerably more electricity on average (5.3 GWh vs. 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 Mio. 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.

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 first suggestive evidence that the REL exemption might lead to higher electricity consumption. For completeness, we present the summary statistics for our pooled sample 2007-2017 in the Appendix (Table A.1).

3.1. Stylized facts about bunching and exemption behavior

We continue by evaluating firms' bunching behavior, 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.

To test for a discontinuity in the density function, we use a test proposed by McCrary (2008) for the years 2007 to 2013.[8] 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 ct 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 ct. per kWh so that there was less incentive for bunching than in 2010, when the REL doubled to 2 ct. per kWh. Second, the years 2008 and 2009 coincided with the financial crisis which 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

[8]The McCrary (2008) test statistic for the years 2014-2017 does not show any signs of bunching behavior under the reformed schedule with a statistic of (standard errors 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 Appendix Figure A.3.

Table 2: Bunching behavior and RE levy exemptions over time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year	2007	2008	2009	2010	2011	2012	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)
# of exempted plants	-	-	-	549	579	697	1,574
Exempted eligible plants	-	-	-	72%	76%	75%	65%
REL, in ct/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 bandwidths calculations (approximately 4 GWh). 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. Standard errors in parentheses. Eligibility is determined based on electricity use and (imputed) electricity cost to GVA. Exemption shares are available only after 2009. Source: AFiD Panel, own calculations.

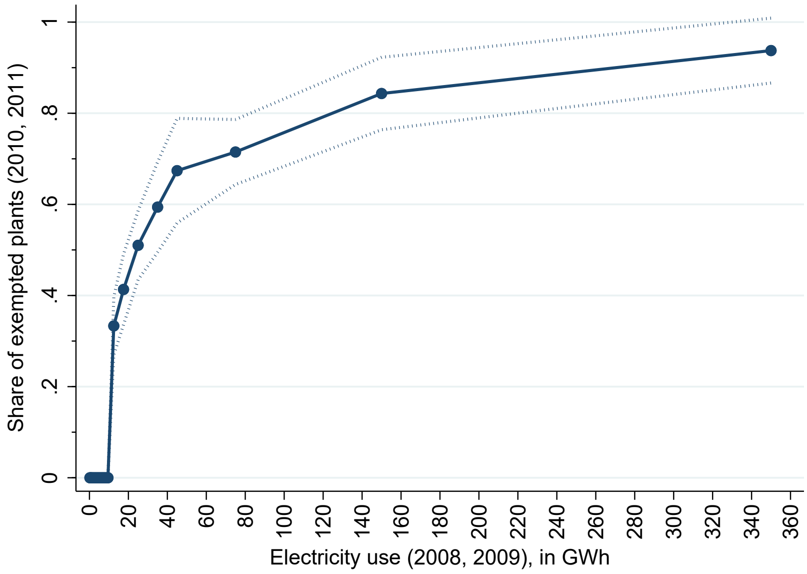
level of electricity use, compared to times with more predictable economic activity. In 2009, for example, GVA in the manufacturing sector plummeted by 19% and many firms resorted to short-term working arrangements for their employees.

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 behavior 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 (Equation 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%) of 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 rate rates declined slightly.

To test whether plants are more likely to claim an exemption when the value of an exemption is higher, Figure 4 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 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

Figure 4: 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: 500 GWh). Dotted lines denote 95% confidence intervals (standard errors clustered at the plant level).

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 Appendix Table A.4, 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.

4. 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 outcomes 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.

4.1. Econometric strategy

To overcome the fundamental problem of a missing counterfactual, we conduct a regression discontinuity (RD) analysis. The central idea of a 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 randomization can then be exploited to estimate a local average treatment effects 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 randomized 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, $E(Y_i(j)|R_i)$ 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 by the following expression (see Imbens and Lemieux, 2008):

$$ATT^{RD} = \frac{\lim_{\epsilon\downarrow 0} E(Y_i|R_i = c+\epsilon) - \lim_{\epsilon\uparrow 0} E(Y_i|R_i = c+\epsilon)}{\lim_{\epsilon\downarrow 0} E(T_i|R_i = c+\epsilon) - \lim_{\epsilon\uparrow 0} E(T_i|R_i = c+\epsilon)}, \tag{4}$$

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 Equation (4) by sample counterparts, using either parametric or nonparametric 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 standard errors 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) and Calonico et al. (2019), we determine bandwidths using a fully data-driven selection procedure that minimizes the mean squared error (MSE) of the estimator. In the main specification, we employ a triangular kernel. As conventional nonparametric 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).

Discussion of identifying assumptions

In line with the discussion in Section 2.3, 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.

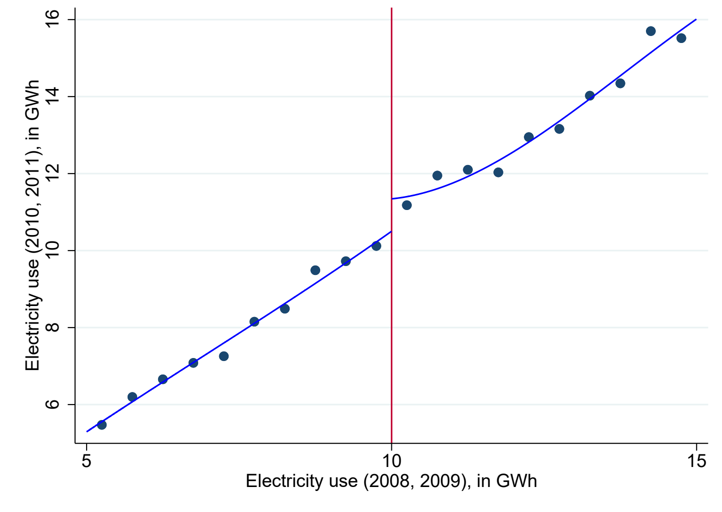
As shown by our model, plants will select above the threshold only when it is economically beneficial to do so. In particular, a sufficient condition for plants not to select above the threshold is that the bunching and compliance costs for the exemption exceed its benefits (Equation 1). In our context, 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 cost 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 Appendix Figure A.5).

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 Appendix Table G.2, we show that the sectoral composition in the baseline years is indistinguishable above and below the 10 GWh threshold, which alleviates such concerns.

4.2. 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 plant 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

Figure 5: Electricity use in 2010 and 2011 versus base period



Notes: Electricity consumption in the years 2010 and 2011 correspond 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 Panel, own calculations.

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 first-stage results reported in Table 3 as well as Appendix Figure A.6).[9] 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 5 presents 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.

[9]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 Appendix Table G.1). However, about 100 treated plants are lost, which makes it difficult to compare these estimates.

Table 3: Results Fuzzy RD Estimates (at the Cutoff)

Main sample	energy cost/GVA >.15		elect. cost/GVA >.10	
	ATT^{RD}	SE	ATT^{RD}	SE
	(1)	(2)	(3)	(4)
***Panel A:** Electricity & 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
# of observations	39,202		5,608	
# 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 further 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). Standard errors clustered at the firm level. * p<.1, ** p<.05, and *** p<.01. Source: AFiD Panel, own calculations.

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%.[10]

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

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

find direct evidence of fuel switching, as shown by the negative, yet statistically insignificant point estimate on (log of) fossil fuel consumption. Yet, when analyzing 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 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 2010-2011 in Germany (UBA, 2018b).[11]

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.

[11] As electricity generation in Germany is covered by the European Union Emissions Trading Scheme (EU ETS), 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 (2010-2012) and the beginning of Phase 3 (2013-2020), 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 an 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: 0.14 and 0.10, respectively).

One reason for our large point estimates under a notched design is that REL exemptions reduce electricity and thus bunching cost. Hence, they may lead to additional inframarginal responses by plants that manipulate their electricity use in order to reach eligibility for an exemption two years later, as discussed in Section 2.2. As a result of the sizeable standard errors, 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 their magnitude of the bunching response in Section 6.

4.3. 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 randomization 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 Appendix Table G.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 randomization 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 Appendix Table G.1 shows that we do not detect any statistically significant effect for variables determined prior to the exemption. This evidence supports local randomization 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).

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

5. 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 difference-in-differences (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.

5.1. 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 follows:

$$ATT^{DiD} = \frac{1}{N_1} \sum_{i \epsilon I_1} \left\{ (Y_{it}(1) - Y_{i0}(0)) - \sum_{k \epsilon I_0} W_{N_0,N_1}(i,k)(Y_{kt}(0) - Y_{k0}(0)) \right\}, \tag{5}$$

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 counterfactual. This assumption can easily be verified graphically and is met in our setting (see Appendix Figure G.3). 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 windsorize the main balancing variables electricity use, gas use, electricity share in total energy, sales, export share, and employees at the 1st and 99th percentile.

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 2-digit economic sector (ISIC Rev. 4) based on the following pre-determined 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.[12]

For matching, we use different algorithms based on nearest neighbor (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 neighbors are close to those of the treated plants. Following Rosenbaum and Rubin (1985), we set the caliper to 25% of the standard deviation of the estimated propensity score. To obtain consistent estimates for the standard errors, we conduct post-matching inference as suggested by Abadie and Spiess (2022).

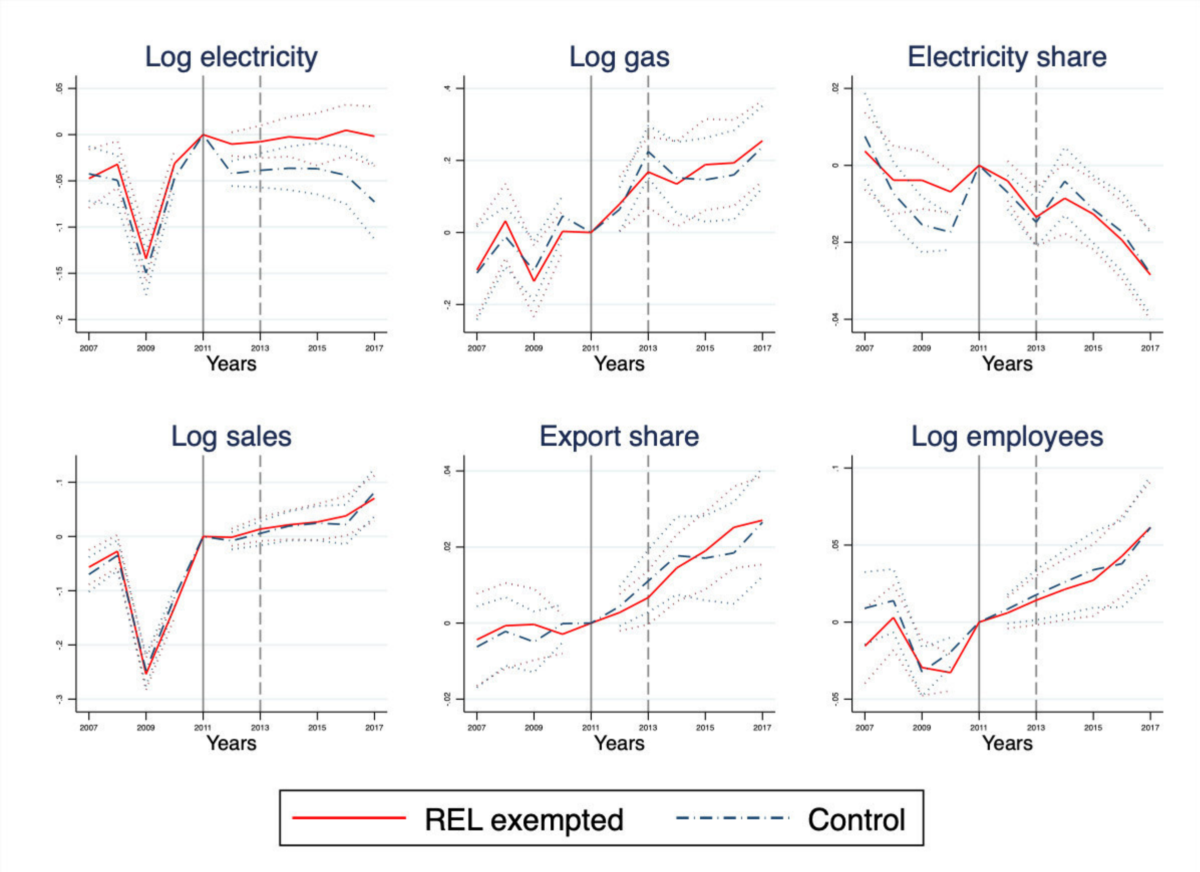
Discussion of identifying assumptions

Conditional independence requires that changes in outcome variables are independent of the treatment status, conditional on covariates. This assumption is equivalent to the common trends 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 7 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 which 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 trends assumption is also supported by t-tests, which do not show any statistically significant differences in trends for the treatment and control group prior to 2011, with the exemption of small differences in the trend from 2010 and 2009 to 2011 for electricity share in total energy (for details, see Appendix Table G.5).

Similar to the fuzzy RD design, SUTVA assumes that only treated plants are affected by the treatment. To exclude the possibility of intra-firm spillovers, we estimate our main treatment

[12] 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 analyzing first differences, we implement both recommendations in our preferred specification.

Figure 7: 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 neighbor matching. The figure plots growth rate of the respective variables with respect to 2011, the year determining treatment status together with 95% confidence intervals. The vertical lines indicate baseline year 2011 and the main outcome year 2013.

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 pre-determined 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.

Table 4: Results 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 & 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
# of observations	702		270	
# of treated plants	351		135	

Notes: Outcome variables defined in differences 2013-2011. The table presents the ATT^{DiD} and standard errors (SE) from nearest neighbor (NN) matching without replacement following Specification (5). 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, and *** p<.01.

5.2. Main results

Table 4 presents the results for the ATT^{DiD}, using the main propensity score specification and one-to-one nearest neighbor (NN) matching. Column 1 reports the ATT^{DiD} for the group of plants consuming between 1-10 GWh electricity in the base period, 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 standard errors 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 effects 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.[13] 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.

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.

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 Appendix Table G.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 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 Table G.7 in the Appendix). 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,

[13] An exemption in 2013 reduces the REL by 90% (REL: 5.28 ct/kWh, average electricity price: 15.11 ct/kWh).

we match the treatment and control group 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 Appendix Table G.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 (Appendix Section 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.

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 Appendix Section E.3 for details). Again, we find a statistically significant increase in electricity use, which supports the findings from our main specification.

Long-run effects

To gauge the long-run impacts of the exemptions, we estimate the ITT effects of an exemption for the years 2014 - 2017. The empirical specification is identical to Equation (5), 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 Appendix Table A.5, 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 ct. per kWh in 2013 to 6.88 ct 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.

6. Model estimation and counterfactual simulations

To identify the parameters of our model, we make four structural assumptions. First, we assume that compliance cost C are constant over time and independently distributed according to a lognormal distribution, $C \sim logN(\mu, \sigma)$, where μ and σ are the mean and standard deviation 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.

The identification of the structural parameters proceeds in three steps (see Appendix Section 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 behavior 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:

$$Pr_{exempt}(x) = F_c(A(x)) \text{ if } x \geq x^u, \tag{6}$$

where x^u is the upper bound of the bunching range (see Appendix C for a derivation). Equation (6) links the parameters of the compliance cost distribution to observable firm behavior and thus enables us to estimate them via Maximum Likelihood. Intuitively, we exploit that the decision of an eligible firm to not claim an exemption implies that the unobserved compliance cost exceed the exemption value.

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

$$\lim_{\epsilon \to 0} Pr_{bunch}(\hat{x} - \epsilon) = F_c(A(\hat{x}) - \beta)$$
$$A(x^m(0)) = \beta + \gamma(\hat{x} - x^m(0)).$$

The first condition states that the probability to bunch just below the threshold equals the probability that compliance cost are smaller than the value of an exemption, less the fixed bunching cost. This condition follows from Equation (1) and exploits that variable bunching cost 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 behavior among eligible plants in 2012 and the bunching behavior in the corresponding base period 2010 (see Appendix Section C for details).[14] 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 behavior (see Appendix Figure D.1). Our estimates for the bunching cost imply a fixed bunching cost of 0.055 Mio. 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.[15] We estimate variable bunching cost γ of around 8.2 ct per kWh, which is lower than the average 2010 electricity price of 12 ct 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 our model by comparing simulated with actual bunching and exemption behavior (Rows 1 to 4 of Table 5). In a second set, we conduct counterfactual simulations of market behavior assuming that a notched regime had continued to exist in 2013, that the 2013 REL had been at 2017

[14] 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 Appendix Section E.4). The results remain virtually unchanged.

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

levels, that compliance was costless, and that fixed bunching cost were absent (Rows 5 to 9 of Table 5).

For every scenario, we draw 200 realizations of the compliance cost and then determine the profit-maximizing bunching, exemption, and input use behavior. 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 behavior and the exemption, respectively. We also assess the efficiency implications by calculating the total bunching and compliance cost that plants incur. To assess 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 Appendix F for details). We find that the social cost of electricity exceeded the cost paid by firms by 1.28 to 3.36 ct. 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 the Rows (1) to (4) confirm that our model captures key features of actual exemption behavior. As shown in Columns (6) and (8), the number of exempted plants and the value of an exemption predicted by our model closely mimics 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 that the value of an exemption was relatively small in 2008 and 2009, compared to 2010. Hence, only small increases in bunching cost due to the financial crisis suffice to reduce bunching to zero in 2008 and 2009.

Column (3) clarifies that inframarginal 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 hypotheses 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 the externality cost from bunching (Column 5) were minor from an aggregate perspective, reaching 4.7 Mio. EUR and 0.6 Mio. 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 Mio. EUR and 38.2 Mio. EUR in 2012, respectively (Row 2, Column 9 and 10).

Table 5: Simulations of Efficiency and Distributional Implications of Policy Designs

(a) Bunching Behavior (in t)

	(1) # of bunchers	(2) Bunching, in GWh	(3) Max. bunching, in %	(4) Bunching cost, in Mio. EUR	(5) Externality cost, in Mio. EUR
Simulations for Bunching in 2008 to 2011 Under the Respective Exemption Designs					
(1) 2011 (reformed)	0	–	–	–	–
(2) 2010 (notched)	34	36.1	26.9	4.7	0.6
(3) 2009 (notched)	11	4.9	10.0	1.0	0.1
(4) 2008 (notched)	3	0.8	3.7	0.3	0.01
Counterfactual Simulations for 2013 under a Notched Exemption Design					
(5) 2011 (notched)	56	55.3	26.8	7.5	1.4
(6) REL 2017	145	258.2	60.3	28.8	6.4
(7) Costless compliance	181	220.9	29.2	27.8	5.5
(8) No fixed bunching cost	75	90.1	36.0	7.3	2.2
(9) No frictions, REL 2017	414	1,008.3	74.2	82.0	25.1

(b) Exemption Behavior (in $t+2$)

	(6) # of exemptions (actual #)	(7) Electricity use change, in GWh	(8) Exemption value, in Mio. EUR (actual value)	(9) Compliance cost, in Mio. EUR	(10) Externality cost, in Mio. EUR
Simulations for Exemptions in 2010 to 2013 Under the Respective Exemption Designs					
(1) 2013 (reformed)	1,239 (1,574)	2,172.9	3,874 (3,804)	335.7	73.0
(2) 2012 (notched)	764 (697)	1,514.2	2,531 (2,394)	289.9	38.2
(3) 2011 (notched)	559 (579)	1,307.1	2,146 (2,250)	165.1	32.5
(4) 2010 (notched)	481 (539)	812.1	1,136 (1,220)	122.8	14.2
Counterfactual Simulations for 2013 under a Notched Exemption Design					
(5) 2013 (notched)	833	2,081.3	3,681	303.2	69.9
(6) REL 2017	1,020	2,887.9	5,108	486.2	97.0
(7) Costless compliance	1,317	2,423.2	4,259	0.0	81.4
(8) No fixed bunching cost	852	2,085.6	3,689	304.8	70.1
(9) No frictions, REL 2017	1,550	3,231.3	5,683	0.0	108.6

Notes: For every scenario, we determine the profit-maximizing market behavior 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 behaviors 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 ct. per kWh), eliminate compliance cost, and set 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 cost are calculated as explained in Appendix Section F.

Our second set of counterfactual simulations explore how market behavior 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 56 bunching plants and a total bunching effect of 55 GWh. As Row (6) shows, this increase would have been even more drastic if the REL levy was at 2017 levels (6.88 ct per kWh). In this scenario 145 plants would bunch and increase their electricity by 258 GWh to reach eligibility. The exemptions would have also led to a far greater redistributional burden (5,108 Mio. EUR of exemption value) and externality cost of about 70 Mio. EUR. Furthermore, the compliance cost would have increased to 486 Mio. 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 181 and 1,371, respectively (Row 7). This result suggests that policy makers face a trade-off when designing notched exemption schemes with more or less stringent organizational requirements: higher requirements and thus compliance cost reduce rent-seeking behavior through bunching and limit the number of exemption claims, but impose substantial cost on firms (e.g. Row 5, Column 9). By contrast, we find that the absence of fixed bunching cost would change market outcomes only little (Row 8).

When we set the REL to 2017 levels and eliminate compliance and fixed bunching cost, we find that 414 plants would bunch and increase their electricity use by about 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 Mio. EUR annually, and an increase in electricity use by 3.2 TWh, which translates into externality cost of 108.6 Mio. EUR.

7. Conclusion

This paper analyses how a large electricity tax exemption scheme, the exemption from the German renewable energy levy (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 behavior 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 1,000 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 optimize the design of exemption policies in order to sustain domestic production levels, while minimizing 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 cost. 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.

References

Abadie, Alberto, and Jann Spiess. 2022. "Robust Post-Matching Inference." *Journal of the American Statistical Association* 117 (538): 983–995.

Almunia, Miguel, and David Lopez-Rodriguez. 2018. "Under the Radar: The Effects of Monitoring Firms on Tax Compliance." *American Economic Journal: Economic Policy* 10 (1): 1–38.

BAFA. 2013. "Hintergrundinformationen zur Besonderen Ausgleichsregelung: Antragsverfahren 2012 auf Begrenzung der EEG-Umlage 2013." Bundesamt für Wirtschaft und Ausfuhrkontrolle.

Battistin, Erich, and Enrico Rettore. 2008. "Ineligibles and Eligible Non-Participants as a Double Comparison Group in Regression-Discontinuity Designs." *Journal of Econometrics* 142 (2): 715–730.

BDEW. 2022. "BDEW-Strompreisanalyse Juli 2022." Bundesverband der Energie- und Wasserwirtschaft.

Bernard, Alain L., Carolyn Fischer, and Alan K. Fox. 2007. "Is There a Rationale for Output-Based Rebating of Environmental Levies?." *Resource and Energy Economics* 29 (2): 83–101.

Böhringer, Christoph, Jared C. Carbone, and Thomas F. Rutherford. 2012. "Unilateral climate policy design: Efficiency and equity implications of alternative instruments to reduce carbon leakage." *Energy Economics* 34 S208–S217.

Borenstein, Severin, and James B Bushnell. 2022. "Do two Electricity Pricing Wrongs make a Right? Cost Recovery, Externalities, and Efficiency." *American Economic Journal: Economic Policy* 14 (4): 80–110.

Calonico, Sebastian, Matias D. Cattaneo, Farrell Max H., and Rocio Titiunik. 2019. "Regression Discontinuity Designs Using Covariates." *The Review of Economics and Statistics* 101 (3): 442–451.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–2326.

Chabé-Ferret, Sylvain. 2017. "Should We Combine Difference In Differences with Conditioning on Pre-Treatment Outcomes?." *TSE Working Paper, n. 17-824.*

Chen, Zhao, Zhikuo Liu, Juan Carlos Suárez Serrato, and Daniel Yi Xu. 2021. "Notching R&D investment with corporate income tax cuts in China." *American Economic Review* 111 (7): 2065–2100.

Commins, Nicola, Seán Lyons, Marc Schiffbauer, and Richard S.J. Tol. 2011. "Climate Policy & Corporate Behavior." *The Energy Journal* 32 (4): 51–68.

Dechezleprêtre, Antoine, and Misato Sato. 2017. "The Impacts of Environmental Regulations on Competitiveness." *Review of Environmental Economics and Policy* 11 (2): 183–206.

Deschenes, Olivier. 2012. "Climate Policy and Labor Markets." In *The Design and Implementation of US Climate Policy*, edited by Fullerton, Don, and Catherine D. Wolfram 37–49, Chicago, Ill. and London: University of Chicago Press.

Fischer, Carolyn, and Alan K. Fox. 2012. "Comparing policies to combat emissions leakage: Border carbon adjustments versus rebates." *Journal of Environmental Economics and Management* 64 (2): 199–216.

Fowlie, Meredith, Stephen P. Holland, and Erin T. Mansur. 2012. "What Do Emissions Markets Deliver and to Whom? Evidence from Southern California's NO x Trading Program." *American Economic Review* 102 (2): 965–993.

Fowlie, Meredith L. 2009. "Incomplete Environmental Regulation, Imperfect Competition, and Emissions Leakage." *American Economic Journal: Economic Policy* 1 (2): 72–112.

Fowlie, Meredith, and Mar Reguant. 2018. "Challenges in the Measurement of Leakage Risk." *AEA Papers and Proceedings* 108 124–129.

Fowlie, Meredith, Mar Reguant, and Stephen P. Ryan. 2016. "Market-Based Emissions Regulation and Industry Dynamics." *Journal of Political Economy* 124 (1): 249–302.

Garicano, Luis, Claire Lelarge, and John Van Reenen. 2016. "Firm size distortions and the productivity distribution: Evidence from France." *American Economic Review* 106 (11): 3439–79.

Greenstone, Michael. 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* 110 (6): 1175–1219.

Greenstone, Michael, John A. List, and Chad Syverson. 2012. "The Effects of Environmental Regulation on the Competitiveness of US Manufacturing." *NBER Working Paper* 18392.

Hahn, Jinyong, Petra Todd, and Wilbert van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69 (1): 201–209.

Harju, Jarkko, Tuomas Matikka, and Timo Rauhanen. 2019. "Compliance costs vs. tax incentives: Why do entrepreneurs respond to size-based regulations?" *Journal of Public Economics* 173 139–164.

Heckman, James J., Hidehiko Ichimura, and Petra E. Todd. 1997. "Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme." *The Review of Economic Studies* 64 (4): 605–654.

Imbens, Guido W., and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics* 142 (2): 615–635.

Kahn, Matthew E., and Erin T. Mansur. 2013. "Do local energy prices and regulation affect the geographic concentration of employment?." *Journal of Public Economics* 101 105–114.

Kleven, Henrik J, and Mazhar Waseem. 2013. "Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan." *The Quarterly Journal of Economics* 128 (2): 669–723.

Kleven, Henrik Jacobsen. 2016. "Bunching." *Annual Review of Economics* 8 435–464.

Küchler, Swantje, and Rupert Wronski. 2015. "Was Strom wirklich kostet." In *Vergleich der staatlichen Förderungen und gesamtgesellschaftlichen Kosten konventioneller und erneuerbarer Energien. Forum Ökologisch-Soziale Marktwirtschaft*, `https://foes.de/pdf/2015-01-Was-Strom-wirklich-kostet-lang.pdf`.

Lee, David S., and Thomas Lemieux. 2010. “Regression Discontinuity Designs in Economics.” *Journal of Economic Literature* 48 (2): 281–355.

Lucas, Robert E. 1978. “On the Size Distribution of Business Firms.” *The Bell Journal of Economics* 9 (2): 508–523.

Marin, Giovanni, and Francesco Vona. 2021. “The impact of energy prices on socioeconomic and environmental performance: Evidence from French manufacturing establishments, 1997–2015.” *European Economic Review* 135 103739.

Martin, Ralf, Mirabelle Muûls, Laure B. de Preux, and Ulrich J. Wagner. 2014a. “Industry Compensation under Relocation Risk: A Firm-Level Analysis of the EU Emissions Trading Scheme.” *American Economic Review* 104 (8): 2482–2508.

Martin, Ralf, Mirabelle Muûls, and Ulrich J. Wagner. 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* 10 (1): 129–148.

Martin, Ralf, Laure B. de Preux, and Ulrich J. Wagner. 2014b. “The impact of a carbon tax on manufacturing: Evidence from microdata.” *Journal of Public Economics* 117 1–14.

McCrary, Justin. 2008. “Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test.” *Journal of Econometrics* 142 (2): 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. “Database on Policy Instruments for the Environment - Exemptions in Environmentally Related Taxes.” `https://pinedatabase.oecd.org/#`.

Perino, Grischa. 2018. “New EU ETS Phase 4 rules temporarily puncture waterbed.” *Nature Climate Change* 8 (4): 262–264.

Reguant, Mar. 2019. “The Efficiency and Sectoral Distributional Impacts of Large-Scale Renewable Energy Policies.” *Journal of the Association of Environmental and Resource Economists* 6 (S1): S129–S168.

Research Data Centres of the Federal Statistical Offices, and the Statistical Offices of the Laender. 2007 - 2017. “AFiD-Panel Manufacturing Plants, AFiD-Module Energy Use, Cost Structure Survey, and Material and Incoming Goods Statistic.”

Rosenbaum, Paul R., and Donald B. Rubin. 1985. “Constructing a control group using multivariate matched sampling methods that incorporate the propensity score.” *The American Statistician* 39 (1): 33–38.

Rubin, Donald B. 1974. “Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies.” *Journal of Educational Psychology* 66 (5): 688.

Sallee, James M, and Joel Slemrod. 2012. “Car notches: Strategic automaker responses to fuel economy policy.” *Journal of Public Economics* 96 (11-12): 981–999.

Splawa-Neyman, Jerzy, D. M. Dabrowska, and T. P. Speed. 1990. “On the Application of Probability Theory to Agricultural Experiments. Essay on Principles. Section 9..” *Statistical Science* 5 (4): 465–472, `http://www.jstor.org/stable/2245382`.

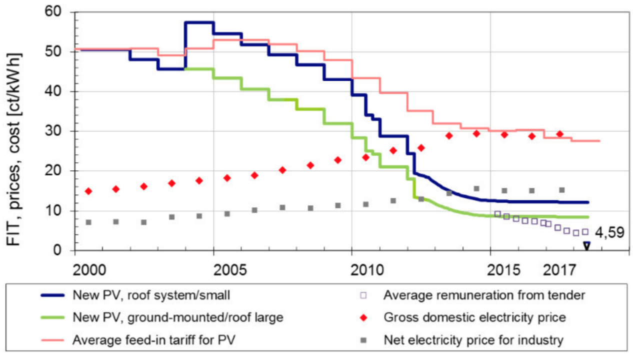
UBA. 2018a. “Berichterstattung unter der Klimarahmenkonvention der Vereinten Nationen und dem Kyoto-Protokoll 2012. Nationaler Inventarbericht zum Deutschen Treibhausgasinventar 1990-2010.” *Climate Change* 12, Umweltbundesamt.

UBA. 2018b. “Entwicklung der spezifischen Kohlendioxid-Emissionen des deutschen Strommix in den Jahren 1990 – 2017.” *Climate Change* 11, Umweltbundesamt.

Appendix (for online publication)

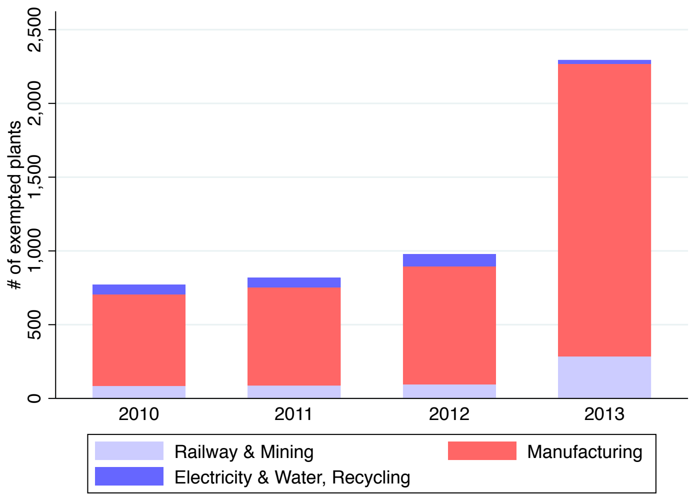
A. Additional Tables and Figures

Figure A.1: Evolution of Feed-in Tariffs (FiTs) for Solar Installations



Notes: Evolution of FiT for new solar photovoltaic installations and average electricity prices in Germany. Source: Fraunhofer ISE (2018).

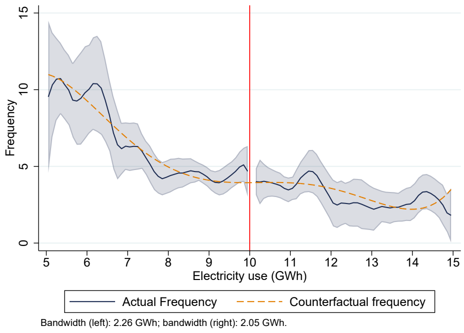
Figure A.2: Renewable Energy Levy (REL) Exempted Plants



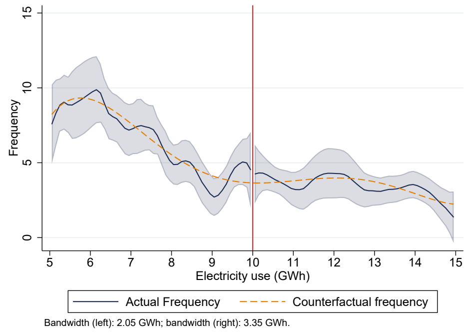
Notes: Total number of REL exempted plants in railway & mining, manufacturing, as well as electricity & water and recycling industry for the years 2010 to 2013. Source: Federal Office of Economics and Export Control (BAFA).

Figure A.3: Bunching Around 10 GWh Threshold

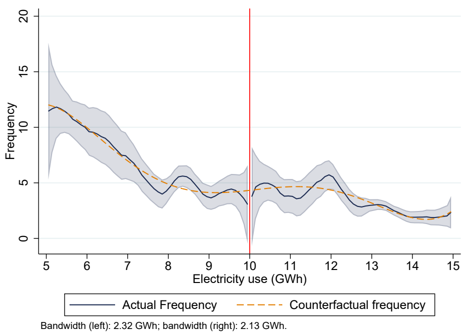
(a) 2007



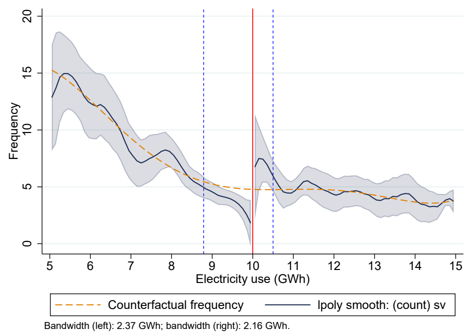
(b) 2008



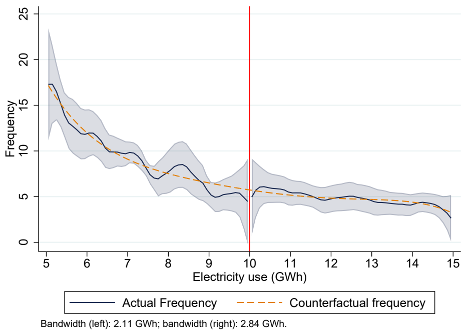
(c) 2009



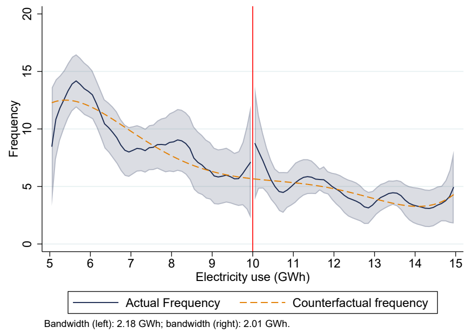
(d) 2010



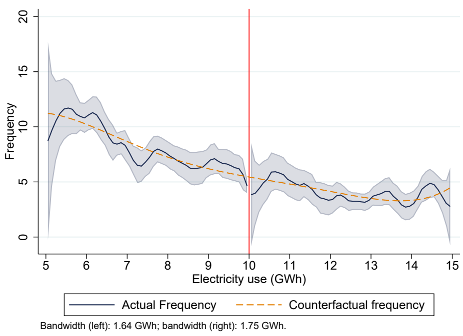
(e) 2011



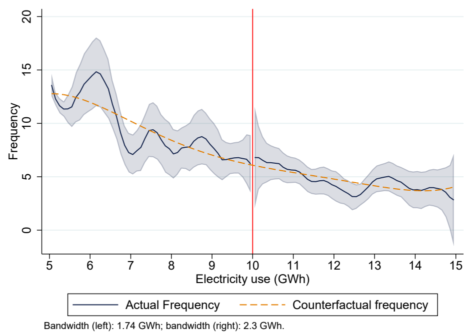
(f) 2012



(g) 2013

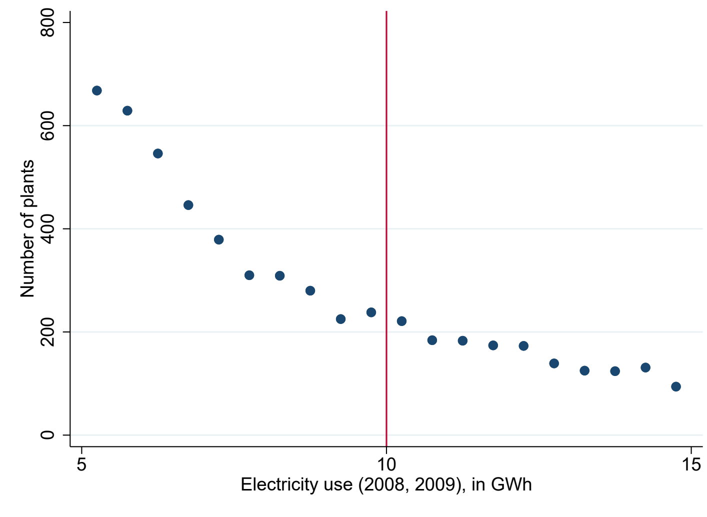


(h) 2014



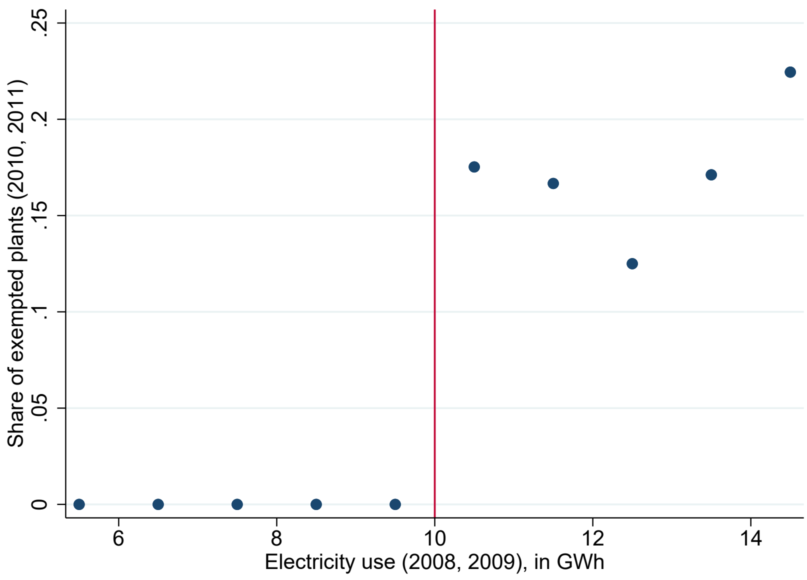
Notes: Distribution of plants around the 10 GWh threshold for the years 2007-2014. We estimate the counterfactual frequency as explained in Appendix Section C. For data confidentiality reasons, we estimate the actual frequency using local linear regressions (rule-of-thumb bandwidths are displayed the figures). Source: AFiD Panel, own calculations.

Figure A.5: Number of Plants in 0.5 GWh Bins of Electricity Consumption in 2008 and 2009



Notes: Absolute frequency of plants within 0.5 GWh bins of electricity use in the years 2008 and 2009. Source: AFiD Panel, own calculations.

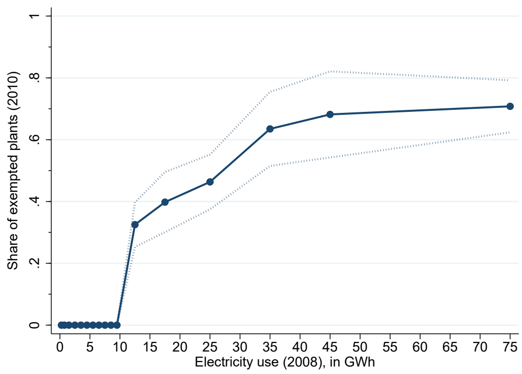
Figure A.6: Exemption Shares in 2010 and 2011 for Energy Intensive Plants



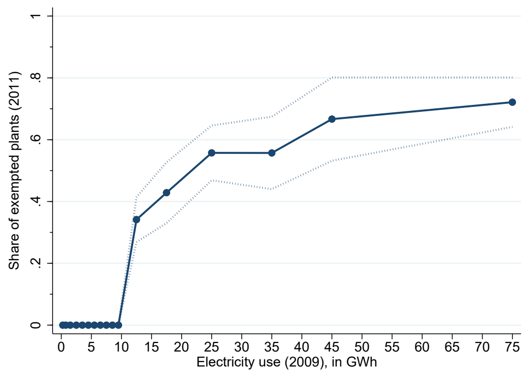
Notes: REL exemption shares (2010 and 2011) correspond to averages within 1 GWh bins of electricity consumption two years prior to the treatment period. For reasons of data confidentiality, the minimum bin width for this plot is 1 GWh. Source: AFiD Panel, own calculations.

Figure A.7: Exempted Plants in All Eligible Plants

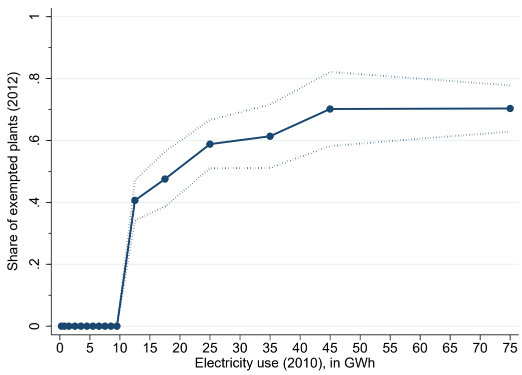
(a) Baseline 2008 for exemption 2010



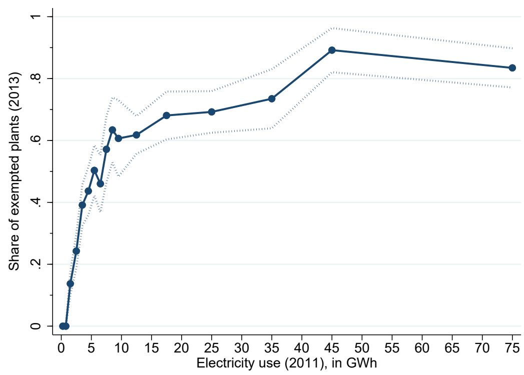
(b) Baseline 2009 for exemption 2011



(c) Baseline 2010 for exemption 2012



(d) Baseline 2011 for exemption 2013



Notes: Share of exempted plants in all plants by baseline electricity use. Dotted lines represent 95% confidence intervals. Source: AFiD Panel, own calculations.

Table A.1: Summary Statistics, 2007-2017, all plants

VARIABLE	Mean (1)	Std. dev. (2)	Obs. (3)
Plant-level data			
Economic covariates			
Sales, in million €	38.71	424.32	473,542
Export share (of sales)	0.21	0.26	473,906
Number of employees	138	597	466,710
Investments, in million €	1.26	14.25	473,730
Avg. wage per employee, thd. €	33.8	13.71	466,710
Energy-related covariates			
Electricity use, in GWh	5.36	57.07	464,444
Other energy use , in GWh	19.26	606.51	476,965
Own electricity generation, in %	0.08	0.27	496,697
Electricity share in total energy	0.52	0.26	464,426
Gas share in total energy	0.29	0.3	471,532
Oil share in total energy	0.13	0.24	471,532
Coal share in total energy	0.01	0.06	471,532
Renewable share in total energy	0.05	0.16	471,532
Total CO_2 emissions, in 1,000 t	7,171	180,150	477,095
Direct CO_2 emissions, in 1,000 t	4,580	172,722	477,095
Firm-level data			
Number of plants per firm	1.19	1.57	421,056
Gross value added (GVA), in million €	27.15	275.26	170,275
Total energy cost, in million €	1.96	15.5	170,221
Total electricity cost, in million €	0.58	6.32	417,979
Electricity cost to GVA, in %	0.05	0.1	413,023

Notes: Descriptive statistics for all plants for the year 2007-2017. Source: AFiD Panel, own calculations.

Table A.2: REL Exempted Plants (by Manufacturing Sub-sector)

		2010		2011		2012		2013	
	ISIC (Rev.4)	#	Share	#	Share	#	Share	#	Share
Manufacturing:									
Food & Beverages	10,11,12	54	7.00%	63	7.70%	78	7.97%	382	16.64%
Textiles & Leather	13,14,15	17	2.20%	15	1.83%	19	1.94%	56	2.44%
Wood, Paper & Print	16, 17, 18	132	17.12%	130	15.89%	152	15.53	238	10.37%
Mineral Oil	19	4	0.52%	4	0.49%	5	0.51%	14	0.61%
Chemicals	20,21	122	15.82%	130	15.89%	144	14.71%	231	10.07%
Rubber & Plastics	22	46	5.97%	55	6.72%	84	8.58%	298	12.98%
Non-metallic minerals	23	100	12.97%	105	12.84%	126	12.87%	244	10.63%
Basic metals	24	111	14.40%	121	14.79%	138	14.10%	222	9.67%
Fabricated Metals	25	20	2.59%	22	2.69%	30	3.06%	222	9.67%
Optics & Electronics	26, 27	10	1.30%	12	1.47%	16	1.63%	38	1.66%
Machinery	28	3	0.39%	3	0.37%	3	0.31%	11	0.48%
Vehicles & Transport	29, 30	2	0.26%	5	0.61%	5	0.51%	24	1.05%
Other manufacturing	31, 32, 33	1	0.13%	1	0.12%	1	0.10%	5	0.22%
Other sectors (excluded from analysis):									
Railway	49	49	6.36%	49	5.99%	51	5.21%	53	2.31%
Mining	0	35	4.54%	38	4.65%	45	4.60%	231	10.07%
Recycling	37, 38	8	1.04%	6	0.73%	13	1.33%	26	1.13%
Electricity & Water	35,36	36	4.67%	38	4.65%	45	4.60%	0	0.00%
Construction	43	21	2.72%	21	2.57%	24	2.45%	0	0.00%
# exempted plants		771		818		979		2295	

Notes: Number of REL exempted plants by economic sub-sector and year. Source: Federal Office of Economics and Export Control (BAFA).

Table A.3: Number of Plants per REL Exempted Firm

Number of plants per firm	2010	2011	2012	2013
1	375	400	498	1238
2-3	50	54	69	182
4-5	10	9	12	28
6-10	3	5	3	6
$\geq$ 10	2	2	2	5
# of exempted firms	440	470	584	1459

Notes: Number of REL exempted single-plant firms as well as multi-plant firms over the years 2010 to 2013. Source: Federal Office of Economics and Export Control (BAFA).

Table A.4: Treatment Eligibility and Uptake, 2013

REL exempted (2013)	(1)	(2)	(3)
Firm level variables:			
Employment (#)	-0.000005^{***}	-0.000004^{*}	-0.000004^{*}
	(0.000001)	(0.000002)	(0.000002)
White collar workers (%)	0.0012	0.0028	0.0032
	(0.0093)	(0.0092)	(0.0092)
Sales (m €)	0.000025^{***}	0.000020^{*}	0.000020^{*}
	(0.000007)	(0.000010)	(0.000010)
Eligible for exemption in 2011	0.360^{***}		
	(0.0130)		
Number of plants:			
2-4	0.0122^{***}	0.00448^{*}	0.00175
	(0.00226)	(0.00230)	(0.00221)
5-9	0.0179^{***}	0.00269	-0.00565
	(0.00555)	(0.00577)	(0.00554)
10-19	0.0274^{***}	0.0126	0.00214
	(0.00781)	(0.00865)	(0.00793)
> 20	-0.0155^{***}	-0.0505^{***}	-0.0513^{***}
	(0.00365)	(0.00547)	(0.00550)
Electricity cost intensity (%) in 2011:			
0.10 - 0.13		0.0370^{***}	0.0366^{***}
		(0.00570)	(0.00564)
0.14 - 0.17		0.170^{***}	0.160^{***}
		(0.0150)	(0.0147)
0.18 - 0.21		0.297^{***}	0.283^{***}
		(0.0262)	(0.0258)
0.22 - 0.25		0.399^{***}	0.380^{***}
		(0.0222)	(0.0223)
> 0.25		0.213^{***}	0.196^{***}
		(0.0209)	(0.0207)
Electricity use (GWh) in 2011:			
1-3		0.00764^{***}	0.00775^{***}
		(0.00146)	(0.00145)
4-6		0.0585^{***}	0.0572^{***}
		(0.00530)	(0.00523)
> 7		0.102^{***}	0.0951^{***}
		(0.00905)	(0.00890)
Previous exemptions (2010-2012):			
One or more plants of firm			0.103^{**}
			(0.0421)
Eligible × One or more plants			0.392^{***}
			(0.0687)
Constant	0.0133^{***}	0.00470^{**}	0.00426^{**}
	(0.00219)	(0.00205)	(0.00200)
N	36,228	36,200	36,200
Adj. R2	0.273	0.266	0.286
Sector FE	Yes	Yes	Yes

Notes: Dependent variable is a dummy that equals 1 if a plant is REL exempted in 2013 and zero otherwise. Eligibility for exemption is based on electricity use and (imputed) electricity cost intensity in 2011. Omitted base categories for number of plants (1), electricity cost intensity (≤ 0.10), and electricity use (≤ 1 GWh) not reported in the table. Sample: 2013, plants with electricity use ≤ 10 GWh in 2011. Standard errors clustered at the plant level. * p<0.1, **p<0.05, and ***p<0.01. Source: AFiD Panel, own calculations.

Table A.5: Results Matching DiD Estimates Long Run

Treatment difference	Δ 2014-2011		Δ 2015-2011		Δ 2016-2011		Δ 2017-2011	
	ATT^{DiD}	SE	ATT^{DiD}	SE	ATT^{DiD}	SE	ATT^{DiD}	SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
***Panel A:** Electricity & fuel usage*								
Electricity consumption [GWh]	0.104	0.073	0.107	0.091	0.2*	0.102	0.25**	0.112
Log electricity consumption	0.03**	0.015	0.034*	0.019	0.05**	0.02	0.077***	0.026
Log electricity purchase	0.046***	0.016	0.054**	0.021	0.075***	0.022	0.105***	0.028
Log fossil fuel consumption	−0.061	0.055	−0.022	0.056	−0.035	0.06	−0.051	0.057
Share of total energy mix:								
Electricity [%]	0	0.006	0.004	0.006	0.003	0.008	0.006	0.008
Fossil fuel [%]	−0.01	0.007	−0.01	0.007	−0.014*	0.008	−0.018**	0.008
***Panel B:** CO2 emissions*								
Log CO_2, direct	−0.024	0.054	0.017	0.057	−0.005	0.061	−0.026	0.058
Log CO_2, total	0.023	0.017	0.035*	0.02	0.038*	0.022	0.06**	0.027
***Panel C:** Competitiveness indicators*								
Log employment	0.007	0.015	0.006	0.017	0.014	0.02	0.01	0.022
Log sales	0	0.018	0.003	0.022	0.014	0.025	−0.009	0.03
Export share	0	0.007	0.004	0.007	0.008	0.008	0.003	0.009
Log investment	0.308**	0.138	0.448***	0.151	0.282*	0.153	0.022	0.15
$\mathbb{1}$(investment > 0)	−0.011	0.022	−0.02	0.024	0.009	0.026	0	0.027
$\mathbb{1}$(investment machinery > 0)	0.023	0.021	0.009	0.022	0.017	0.021	0.026	0.021
# of observations	702		702		702		702	
# of treated plants	351		351		351		351	

Notes: Outcome variables defined in differences with the base year 2011 for outcome years 2014 to 2017 (Columns 1, 3, 5 and 7, respectively). The table presents the ATT^{DiD} and standard errors (SE) from nearest neighbor (NN) matching without replacement following Specification (5). The sample is limited to plants that report in the treatment years and the base year. Inference follows Abadie and Spiess (2022). * p<.1, ** p<.05, and *** p<.01. Source: AFiD Panel, own calculations.

B. Conceptual Model for Input Tax Notch

In this section, we describe how a firm makes production input choices under a notched tax schedule (as described in Section 2.3).

Input use and bunching behavior under a notched design

Let us first consider the impact of the tax notch A on input use. Let x^c and z^c denote the (hypothetical) optimal input choice for x and z in the absence of the notch (i.e., if $A = 0$). The optimal inputs are implicitly defined by the two first order conditions for profit maximization, $\psi y_x = p + t$, and $\psi y_z = q$, where the subscripts denote first derivatives of the production function with respect to these variables, respectively. The comparative statics of the optimal input choices show that $\partial x^c / \partial \psi > 0$, i.e., firms with a larger productivity ψ use more of the input x, irrespective of the substitutability of the inputs x and z. As a consequence, the productivity ψ uniquely pins down the electricity use in the absence of a notch, x^c. Hence, we can use ψ or x_c to describe firm-level heterogeneity in productivity.

We proceed by deriving the conditions under which firms below the eligiblility threshold bunch, i.e., increase their electricity use in order to become eligible for an exemption. Firms bunch if their profit after bunching exceeds the profits they would realize otherwise. Hence, firms bunch if and only if:

$$\begin{aligned}
&\pi(\hat{x}, \bar{z}) \geq \pi(x^c, z^c) \\
\iff &\psi y(\hat{x}, \bar{z}) - q\bar{z} - (p+t)\hat{x} + A(\psi) - c \geq \psi y(x^c, z^c) - qz^c - (p+t)x^c \\
\iff &A(\psi) - c \geq \underbrace{\psi y(x^c, z^c) - qz^c - (p+t)x^c - \psi y(\hat{x}, \bar{z}) - q\bar{z} - (p+t)\hat{x}}_{\kappa(\hat{x}, x^c(\psi)) = \kappa(\psi)} \\
\iff &A(\psi) \geq c + \kappa(\psi), \qquad (7)
\end{aligned}$$

where $\bar{z}$ denotes a firm's profit maximizing choice of the input z, conditional on bunching to the notch threshold $\hat{x}$, and $\kappa(\hat{x}, x^c)$ denotes the bunching cost, i.e., the profit loss from deviating from the optimal production choices x^c and z^c.

To quantify the amount of bunching, we define the "marginal buncher" as a firm with cost $C = c$ and productivity ψ^m that would be indifferent between using the optimal input level in the absence of the notch, x^c, and increasing its electricity consumption to $\hat{x}$ in order to

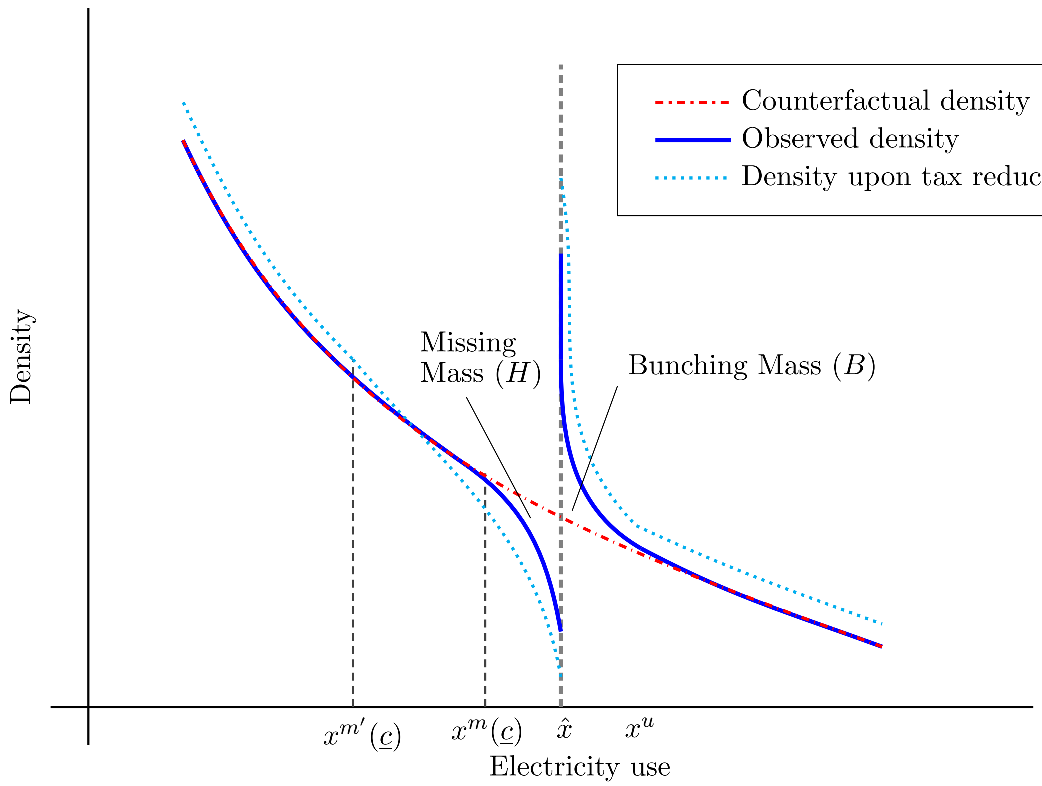


Figure C.1: Bunching with heterogeneity in productivity and compliance cost

become eligible for an exemption. Hence, the marginal buncher $\psi^m(c)$ is implicitly defined by the following equation:

$$A(\psi^m) = c + \kappa(\psi^m).$$

Firms with compliance cost c bunch if and only if their productivity ψ is larger than the marginal productivity $\psi^m(c)$. We can now determine the profit maximizing demand for the taxed input under the notched schedule:

$$x^*(\psi, c) = \begin{cases} x^c & \text{if } \underline{\psi} \leq \psi < \psi^m(c) \text{ or } \psi^{\hat{x}}(c) \leq \psi < \bar{\psi} \\ \hat{x} & \text{if } \psi^m(c) \leq \psi < \psi^{\hat{x}}(c). \end{cases} \tag{8}$$

Figure C.1 illustrates how the presence of the tax notch changes the distribution of input choices x^*. For simplicity, let $x^m(c)$ be the quantity that the marginal buncher with compliance cost c would use as an input, i.e. $x^m(c) = x^*(\psi^m, c)$. For firms with sufficiently low productivity ($\psi < \psi^m(c)$ or, equivalently, firms that choose $x^* < x^m(c)$), the notch does not change input choices, as increasing the input demand by $\Delta x = \hat{x} - x^c$ would result in profit losses that outweigh the gains from obtaining A. This is the case for all firms with electricity use x^c that are lower than the electricity use of the buncher with the lowest compliance cost $\underline{c}$, $x^m(\underline{c})$. For more productive firms, bunching may be profitable if the compliance and bunching cost are sufficiently small. As bunching cost increase in the distance to the threshold, firms close to it are particularly likely to bunch. As a result of bunching, there is missing mass in the interval $x^m \leq x^* < \hat{x}$ and bunching mass in the interval $\hat{x} \leq x^* < x^u$, where x^u denotes the upper bound of the bunching region.

Exemptions under a notched design with compliance cost

Next, we determine the conditions under which eligible firms ($x^c > \hat{x}$) apply for an exemption.[16] Eligible firms do not need to adjust their production input choices in order to become eligible. Hence, they apply for an exemption if and only if:

$$\psi y(x^c, z^c) - qz^c - (p+t)\hat{x}^c + A(\psi) - c \geq \psi y(x^c, z^c) - qz^c - (p+t)x^c$$
$$\iff A(\psi) \geq c. \tag{9}$$

.

Effect of a tax exemption on input use under a notched design

We then investigate how exempting plants from paying the tax t changes the demand for the input x^* under a notched tax schedule. A tax change has two main effects. First, it changes the input demand for all firms. Second, it changes the productivity of the marginal buncher by $d\psi = \psi^{m'}(c) - \psi^m(c)$, where $\psi^{m'}(c)$ denotes the productivity of the marginal buncher after the tax change.

We now determine the first derivative of $x^*(c) = \int_0^\infty x^*(\psi, c) dG(\psi|c)$ with respect to a reduction in the tax rate t by t^{ex}, where $G(\psi|c)$ and $g(\psi|c)$ denote the cumulative density function and density function of ψ, conditional on $C = c$. Using Equation (8), we take the first derivative of $x^*(\psi, c)$ with respect to t^{ex} for every ψ. We then integrate $\partial x^*(\psi, c)/\partial t^{ex}$ over the entire support of $G(\psi|c)$ and rearrange terms, which yields:

$$\frac{\partial x^*(c)}{\partial t^{ex}} = \underbrace{\int_0^\infty \frac{\partial x^c}{\partial t^{ex}} g(\psi|c) d\psi}_{\text{Marginal price response, MPR(c)}} + \underbrace{\int_{\psi^{m'}(c)}^{\psi^m(c)} (\hat{x} - x^c)\, g(\psi|c) d\psi - \int_{\psi^{m'}(c)}^{\psi^{\hat{x}}(c)} \frac{\partial x^c}{\partial t^{ex}} g(\psi|c) d\psi}_{\text{Net bunching response, BR(c)}}.$$

[16] Firms that increase their input use to become eligible (bunchers) will always apply for an exemption because it would never be optimal to incur the bunching cost without getting exempted.

The treatment effect in the population of firms (as shown in Equation 3 in the main text) is then given by:

$$\begin{aligned}\frac{\partial x^*}{\partial t^{ex}} &= \int_c \frac{\partial x^*(c)}{\partial t^{ex}} f(c)dc \\ &= \int_c \text{MPR(c)} f(c)dc + \int_c \text{BR(c)} f(c)dc \\ &= \text{MPR} + \text{BR},\end{aligned}$$

where MPR and BR denote the population-level marginal price and net bunching response, respectively.

This equation clarifies that the effect of a tax reduction in the presence of a notched schedule can be decomposed into two components. The first term equals the change in demand for all firms under the hypothetical scenario that there was no notch, which we denote as the marginal price response in the absence of a notch. This effect corresponds to the rightward shift of the density, as shown by the blue dotted line in Figure C.1 for the interval $x < x^{m'}(\underline{c})$, for example. With a tax reduction, the marginal price response is always positive, which reflects the basic notion that an input is used more when its price decreases.

The second component gives the net bunching response (BR), i.e., the net effect of a tax reduction on input demand for bunching firms. This effect reflects that some firms bunch only after the tax reduction and increase their input use for that reason. In particular, for every c, the electricity use of the marginal buncher decreases from $x^m(c)$ to $x^{m'}(c)$, as shown in Figure C.1. To fix ideas, let us consider a group of firm with compliance cost c. Those firms firms with input demand between $x^m(c)$ and $x^{m'}(c)$ bunch only after the tax reduction and increases their input demand by $\hat{x} - x^c$. Because we are interested in the net effect of a tax increase on bunching, we subtract the (counterfactual) increase in electricity use of all bunchers in the absence of a notch, which is given in the third term. For bunchers, this counterfactual marginal increase in electricity use does not materialize and thus reduces the net bunching response.

C. Identification and Estimation of Structural Parameters

In this subsection, we derive the conditions used to identify the structural parameters and explain how we estimate them.

Identification of compliance cost parameters

We start from Equation (1) and make explicit that a firm with productivity ψ has a counterfactual electricity use $x^c(\psi)$ in the absence of a notch:

$$A(x^c(\psi)) \geq C.$$

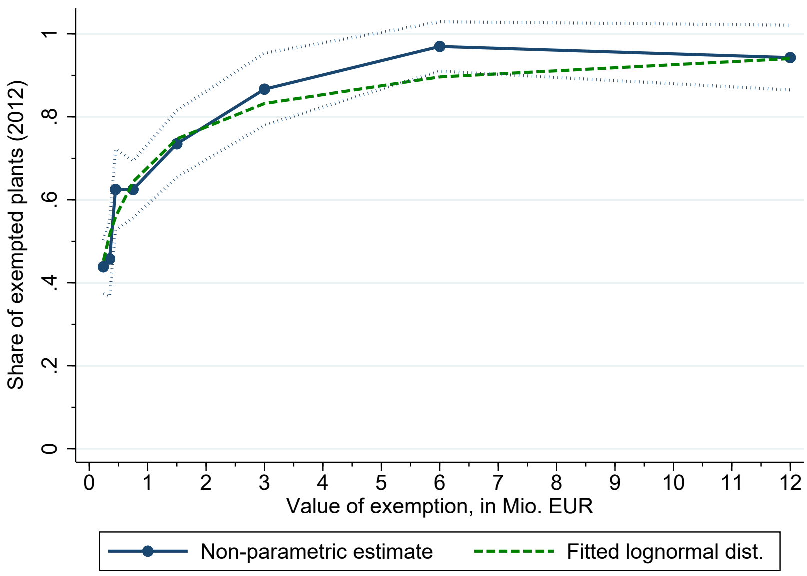
Hence, we can express the probability of an exemption as follows:

$$\begin{aligned} Pr_{exempt}(x^c) &= \int 1(A(x^c) \geq c) f(c) dc \\ &= F_c(A(x^c)). \end{aligned}$$

For firms with an electricity use x that exceeds the upper limit of the bunching range x^u, the observed electricity use equals the counterfactual use in the absence of a notch, which gives:

$$Pr_{exempt}(x) = F_c(A(x)) \text{ if } x > x^u.$$

Figure D.1: Share of Exempted Plants and Value of Exemption



Notes: Blue line represents predicted exemption shares, estimated as the sample average in bins and plotted at the bin midpoints. Dotted lines denote 95% confidence intervals. Green line plots the exemption rates implied by the fitted lognormal distribution.

Estimation of compliance cost parameters

Under the assumption that $C \sim logN(\mu, \sigma)$, we can construct the likelihood function for all eligible plants above the bunching range and maximize it to obtain parameter estimates for μ and σ ($\hat{\mu} = -1.14, \hat{\sigma} = 2.33$).

Figure D.1 plots the observed exemption rates against the exemption rates implied by our estimates for all plants with baseline electricity uses above the bunching range. It shows that the structural assumption of lognormality allows us to reproduce the main features of actual exemption behavior.

Identification of bunching cost parameters

The probability to bunch for a firm that has an counterfactual electricity use just below the threshold ($x^c = \hat{x} - \epsilon$) is given by:

$$Pr_{bunch}(\hat{x} - \epsilon) = Pr(A(\hat{x} - \epsilon) \geq c + \beta + \gamma\epsilon)$$

As the distance to the threshold ϵ converges to zero, we obtain:

$$\lim_{\epsilon \to 0} Pr_{bunch}(\hat{x} - \epsilon) = Pr(A(\hat{x}) \geq c + \beta).$$

Furthermore, a marginal buncher is defined as:

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

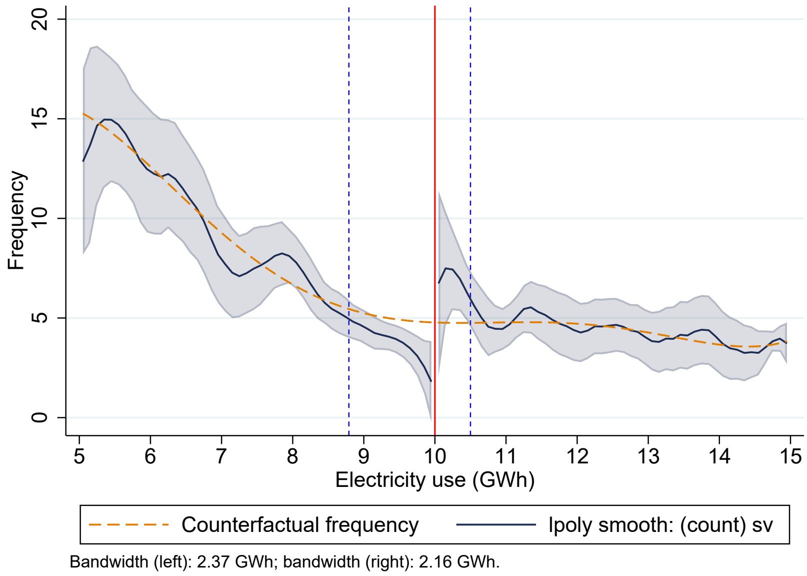
When we consider the marginal buncher with the smallest compliance cost ($\underline{c} = 0$), this equation simplifies to:

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

Estimation of bunching cost parameters

We apply the techniques developed in the taxation literature (e.g., Kleven and Waseem 2013) to construct a counterfactual density in the absence of a notch. We construct narrow bins of electricity use in 2010 with a width of 0.01 GWh and calculate the observed frequency within every bin. We then proceed in three steps (for a detailed description of the methodology, see Almunia and Lopez-Rodriguez (2018)). First, we use visual inspection of the observed frequency to determine the upper bound of the bunching range at 10.5 GWh. Second, we determine the counterfactual density by estimating a polynomial regression of order five, where observations

Figure D.2: Actual and Counterfactual Frequency Around the Threshold



Notes: The dashed line represents the counterfactual frequency. For data confidentiality reasons, we show the predictions from local linear regressions rather than the actual frequencies in 0.1 GWh bins, which underlie this graph. The bunching range and the marginal buncher (shown as blue vertical dashes) are determined based on the actual frequencies.

within the bunching region are omitted. The lower bound of the bunching region is determined via an iterative procedure. In every step of the procedure, the counterfactual density is estimated as well as the bunching mass above and the missing mass below the threshold (as the distance between the observed and the counterfactual density). The procedure continuously decreases the lower bunching bound until the bunching mass equals the missing mass.

Using this method, we estimate the lower bound at 8.79 GWh. In our conceptual model, the lower bound corresponds to the electricity use of the marginal buncher at the lowest compliance cost, $x^m(0)$. To estimate the probability to bunch below the threshold, we calculate the counterfactual frequency of plants just below the threshold (in the interval $[9.5, 10]$) and divide it by observed frequency in the same interval, which yields 34%. Figure D.2 summarizes our results by showing the counterfactual electricity use density that we estimate for 2010 in the absence of the notch, the observed frequency, as well as the bounds of the bunching region.

Using $x^m(0) = 8.79$ GWh and $\lim_{\epsilon \to 0} Pr_{bunch}(\hat{x} - \epsilon) = 34\%$, we estimate the fixed bunching cost β at 0.055 Mio. EUR and the variable bunching cost γ at 0.082 Mio. EUR per GWh.

D. Additional Data on Electricity Cost and Gross Value Added

To calculate the second criteria that determines eligibility of the RE levy exemption, the ratio of electricity cost to gross value added (GVA), we obtain additional data from the Statistical Offices of the Federal States in Germany. Firm-level data on electricity cost is collected in the 'material and incoming goods statistics' (MIGS) in four-year intervals. We have access to the survey waves 2006, 2010, and 2014. This data is not a standard AFiD product, but usage was granted upon special request. The survey scope is aligned with the sample that constitutes the 'cost structure survey' and includes approximately 18,000 firms with at least 20 employees in the German manufacturing sector. The data is representative with regard to economic sub-sectors and firm size. However, while the cost structure survey is collected every year, MIGS is only collected in four-year intervals (both surveys are based on a random sample of the universe of German manufacturing firms). As a consequence, we do not observe neither GVA nor electricity cost for all firms in all years of our sample period. To construct a measure of electricity cost to GVA for all firms and all years, we interpolate both variables based on two auxiliary variables that are observe annually: electricity use and sales.

We start by interpolating GVA for all firm in all periods. To impute missing values, we first define the GVA-to-sales ratio of a firm dividing GVA by total sales. As this measure is rather constant over time, we use it to impute GVA in time periods when only output is observed. In particular, we multiply the firm level GVA-to-sales ratio by firm sales to obtain a measure of the GVA. If a firm has missing values for GVA in all periods, we impute it using the average GVA intensity by three-digit economic sub-sector and size class (<50, 50-250, 250-500, >500 employees) in the same year.

To impute missing electricity cost information, we proceed analogously, yet, have to make additional assumptions about annual price changes. We start by calculating the average firm-level electricity price for each year by dividing its total electricity cost by the total amount of electricity purchased (both at the firm level). We clean this data for outliers by winsorizing the obtained electricity price at the 5th and 95th percentile. This procedure eliminates extreme prices (electricity prices below 4 cent per kWh or above 40 cent per kWh) that are inconsistent with the average electricity prices shown in Figure 1.

Table E.1: Electricity Cost and Energy Cost Intensity

	2006	2007	2008	2009	2010	2011	2012	2013	2014
(1) Electricity cost / GVA	0.054	0.056	0.07	0.077	0.082	0.083	0.084	0.082	0.085
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
(2) Energy cost / GVA	0.112	0.113	0.158	0.167	0.159	0.163	0.17	0.17	0.158
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
(3) Electricity cost / energy cost	0.622	0.613	0.557	0.541	0.643	0.637	0.617	0.585	0.698
	(0.011)	(0.012)	(0.01)	(0.009)	(0.013)	(0.011)	(0.01)	(0.01)	(0.021)
(4) Electricity use / sales	0.178	0.169	0.165	0.181	0.172	0.161	0.16	0.16	0.159
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)

Notes: Sample averages and standard error of the mean. 2006-2014, own calculations.

In case a firm has missing values for one or more of the three survey waves (2006, 2010, or 2014), we employ additional data from Eurostat[17] on average annual electricity prices to fill in missing values. In particular, we calculate the electricity price ratio (growth rate) between any two survey periods, e.g., $price_{2010}/price_{2006}$, and extrapolate electricity prices by multiplying them with that growth rate. The extrapolation also takes firm exemption status (or a potential change in exemption status) into account. As a result, we obtain a balanced dataset for electricity prices in 2006, 2010, and 2014 for all firms that have been included at least once in MIGS.

For years between the survey waves, we interpolate electricity prices linearly.[18] In a final step, we then multiply the annual electricity purchase at the firm level by the interpolated electricity prices to obtain firm-level electricity cost. Dividing it by the GVA yields our measure of electricity cost to GVA.

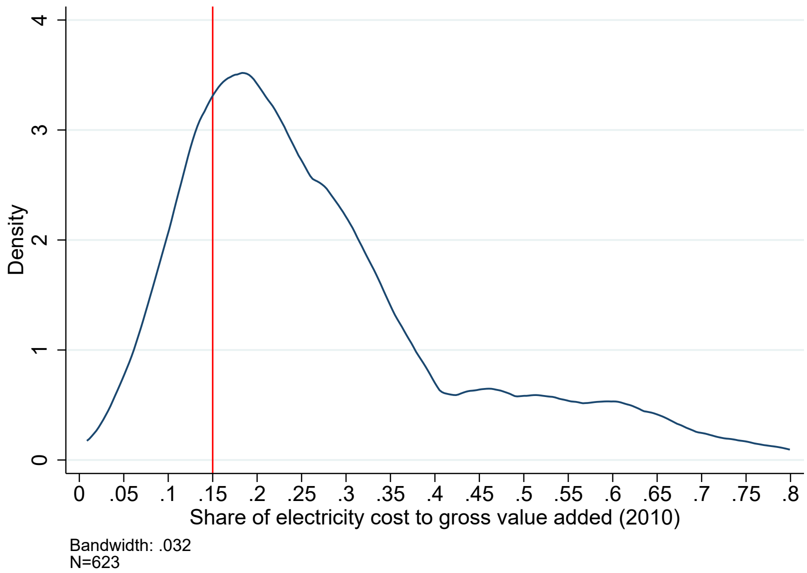
Data quality

We assess its data quality in two ways. First, we test the plausibility of the evolution of electricity cost to GVA by comparing it to two other measures that we observe in the data: energy cost to gross value added (GVA) and the ratio of electricity use to sales for the period 2006 to 2014. In line with the evolution of electricity prices. We find that the electricity cost to GVA increases from 5.4% in 2006 to 8.5% in 2014 (Row (1) of Table E.1), which is consistent with the evolution of electricity prices shown in Figure 1 of the main text. We also find electricity cost to GVA are significantly smaller than the energy cost to GVA, amounting to some 60% of

[17]Electricity prices for non-household consumers, bi-annual data (from 2007 onward), last accessed 8 December 2022, `https://ec.europa.eu/eurostat/databrowser/explore/all/envir?lang=en&subtheme=nrg.nrg_price.nrg_pc&display=list&sort=category&extractionId=NRG_PC_205`. Data is available for different consumption bins and tax levels.

[18]As average prices have been rather flat over this time period with some price increases as well as decreases, we opt for using fixed weights rather than year-on-year changes observed in the aggregate data. This procedure guarantees that all weights are bound between 0 and 1.

Figure E.1: Density of Electricity Intensity, Interpolated Data



Notes: Distribution of electricity cost to GVA for the year 2010, using the interpolated data. Source: AFiD Panel, own calculations.

the latter. Finally, Row (4) verifies that the increase in the electricity cost to GVA over time is driven by price increases rather than higher use: the electricity use to sales ratio declines from about 0.18 in 2006 to 0.16 in 2014.

Second, Figure E.1 plots the distribution of the interpolated electricity cost to GVA in the year 2010 for the sample of exempted plants. By design of the policy, we expect plants to be exempted only when reporting an electricity cost to GVA of at least 0.15. As expected, most of the mass of observations is to the right of the threshold, and only few plants are to the left of it. The fact that we observe some plants with an an electricity cost to GVA below 0.15 might arise from measurement error in the original data as we calculate GVA using its official definition and several variables from the cost structure survey, as well as the interpolation procedure explained above. The graph is informative concerning the size of the potential measurement error, which can be bound at roughly +/- 10 percentage points. We use these bounds in a robustness check of our counterfactual simulation results, by estimating the compliance cost in a sample of electricity cost to GVA of more than 0.25 (see Appendix Section E.4).

E. Robustness Checks

E.1. RD Design

This section provides evidence that the main identifying assumptions SUTVA and no observable differences in the baseline period are met in the RD setting (Table G.1) and that there are no observable differences in the sectoral composition around the 10 GWh threshold in the baseline period 2008 and 2009 (Table G.2). We also provide additional robustness checks regarding the treatment of own-electricity producers and the choice of bandwidth in Tables G.3 and G.4, respectively.

Table G.1: Robustness Fuzzy RD Estimates (at the Cutoff)

Main sample	Single-plant firms		Baseline		elect. cost/GVA >.15	
	ATT^{RD}	SE	ATT^{RD}	SE	ATT^{RD}	SE
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *Electricity & fuel usage*						
Electricity consumption [GWh]	4.184*	2.329	0.000	0.000	0.659	0.943
Log electricity consumption	0.674*	0.383	−0.008	0.011	0.17	0.139
Log electricity purchase	0.777	0.519	0.125	0.124	0.176	0.129
Log fossil fuel consumption	0.8	0.737	0.19	0.51	0.23	0.331
Share of total energy mix:						
Electricity [%]	0.063	0.112	0.013	0.091	−0.035	0.052
Fossil fuel [%]	−0.146	0.099	−0.057	0.084	−0.041	0.053
Panel B: *CO2 emissions*						
Log CO_2, direct	0.396	0.68	0.311	0.502	0.196	0.337
Log CO_2, total	0.718	0.462	0.402	0.306	0.057	0.244
Panel C: *Competitiveness indicators*						
Log employment	0.189	0.247	−0.071	0.112	−0.019	0.117
Log sales	0.342	0.392	−0.325	0.242	0.112	0.174
Export share	−0.042	0.094	−0.069	0.053	−0.062	0.057
Log investment	0.547	1.299	−0.493	0.97	0.053	0.819
$\mathbb{1}$(investment > 0)	−0.245	0.279	−0.099	0.144	−0.106	0.154
$\mathbb{1}$(investment machinery > 0)	−0.105	0.225	−0.158	0.179	−0.155	0.12
# of observations	32,708		40,246		3,234	
# of treated plants	497		497		405	

Notes: Observations from firms with an energy cost share to GVA below 15% in 2008 and 2009 are excluded from the analysis. Number of observations and number of treated 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). Columns 1 and 2 further restrict the sample to single-plant firms. Columns 3 and 4 estimate differences in the baseline period and Columns 5 and 6 limit the sample to plants with an electricity cost to GVA ratio of .15. As we condition on lagged electricity use, the test statistic for electricity use is zero for electricity consumption by construction. Standard errors clustered at the firm level. * p<.1, ** p<.05, and *** p<.01. Source: AFiD Panel, own calculations.

Table G.2: Sector Composition Around the Cutoff

Bandwidth around 10 GWh	0.5 GWh		1 GWh	
2-digit sub-sector	Beta (1)	SE (2)	Beta (3)	SE (4)
Manufacturing (C):				
Food products (10)	−0.001	0.032	−0.02	0.024
Beverages (11)	0.01	0.011	0.003	0.01
Tobacco (12)	0	0	0	0
Textiles (13)	−0.005	0.013	−0.003	0.01
Wearing apparel (14)	0	0	0.002	0.002
Leather and related products (15)	−0.004	0.004	−0.004	0.004
Wood and Cork, except furniture (16)	−0.013	0.01	−0.002	0.008
Paper (17)	0.003	0.013	0.019	0.014
Printing (18)	0.026*	0.013	0.025**	0.012
Coke and refined petroleum (19)	0	0	0	0
Chemical products (20)	−0.028	0.027	−0.017	0.018
Pharmaceutical products (21)	0.002	0.011	−0.005	0.01
Rubber and plastic products (22)	−0.024	0.028	−0.003	0.022
Non-metallic mineral products (23)	0.013	0.024	0.012	0.018
Basic metals (24)	0	0.022	0.006	0.015
Fabricated metals, except machinery (25)	−0.043	0.031	−0.036	0.022
Computer, electronic and optical products (26)	0.014	0.014	0.004	0.011
Electrical equipment (27)	0.012	0.019	0.006	0.012
Machinery and equipment (28)	0.024	0.023	0.011	0.019
Motor vehicles, trailers and semi-trailers (29)	−0.011	0.021	−0.01	0.016
Other transport equipment (30)	0.004	0.004	0.003	0.005
Furniture (31)	0.007	0.014	0.009	0.011
Other manufacturing (32)	0.008	0.008	0.003	0.006
Repair and installation of machinery (33)	0.004	0.004	−0.003	0.004
# of observations	528		1,010	
Chi-square statistic (24 deg. of freedom)	16.120	(0.709)	19.696	(0.541)

Notes: Observations from firms with an energy cost share to GVA below 15% in 2008 and 2009 are excluded from the analysis. The table reports regression coefficients, where we regress a dummy variable equal to one if a plant is in a given sub-sector on an indicator for being above the threshold of 10 GWh, restricting the sample to either plants within 0.5 GWh around the 10 GWh threshold (Column 2) or plants within 1 GWh of the threshold (Column 4). Sample: 2008-2009. 2-digit sector definitions according to ISIC (Rev. 4) (10-33). Standard errors clustered at the plant level. * p<.1, ** p<.05, and *** p<.01. Chi-square statistic tests for the homogeneity of the distribution of plants across all sectors above and below the threshold, p-values in parentheses. Source: AFiD Panel, own calculations.

Table G.3: Robustness Fuzzy RD Estimates (at the Cutoff) - Own-electricity producers

Main sample	energy cost/GVA >.15		elect. cost/GVA >.1	
	ATT^{RD} (1)	SE (2)	ATT^{RD} (3)	SE (4)
Panel A: *Electricity & fuel usage*				
Electricity consumption [GWh]	3.958*	2.181	1.035	1.578
Log electricity consumption	0.753*	0.427	0.369*	0.221
Log electricity purchase	0.796*	0.465	0.364*	0.214
Log fossil fuel consumption	−0.079	0.48	0.192	0.417
Share of total energy mix:				
Electricity [%]	0.147	0.127	−0.025	0.071
Fossil fuel [%]	−0.204*	0.106	−0.036	0.058
Panel B: *CO2 emissions*				
Log CO_2, direct	−0.087	0.476	0.215	0.426
Log CO_2, total	0.675*	0.379	0.221	0.237
Panel C: *Competitiveness indicators*				
Log employment	0.119	0.16	0.071	0.119
Log sales	0.274	0.281	0.167	0.202
Export share	−0.144*	0.08	−0.039	0.057
Log investment	0.543	1.273	−0.342	0.939
$\mathbb{1}$(investment > 0)	−0.318	0.213	−0.146	0.171
$\mathbb{1}$(investment machinery > 0)	−0.17	0.17	−0.116	0.131
# of observations	39,202		6,034	
# of treated plants	592		570	
First-stage	0.164		0.288	

Notes: Sample includes plants that are own-electricity producers. Columns 1 and 2 limit the sample to all energy intensive firms with an energy cost share to GVA above 15% in 2008 and 2009. Columns 3 and 4 further limit the sample to firms with an electricity cost to GVA ratio above .1. Number of observations and number of treated 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). Standard errors clustered at the firm level. * p<.1, ** p<.05, and *** p<.01. Source: AFiD Panel, own calculations.

Table G.4: Robustness: Bandwidth in Fuzzy RD Design

	50% optimal bandwidth		200% optimal bandwidth	
	ATT^{RD}	SE	ATT^{RD}	SE
	(1)	(2)	(3)	(4)
Panel A: *Electricity & fuel usage*				
Electricity consumption [GWh]	4.354**	1.963	2.792**	1.272
Log electricity consumption	1.574*	0.936	0.466**	0.192
Log electricity purchase	2.419	1.553	0.545**	0.226
Log fossil fuel consumption	−0.815	0.769	0.785	0.498
Share of total energy mix:				
Electricity [%]	0.256	0.209	0.059	0.081
Fossil fuel [%]	−0.31*	0.174	−0.111	0.071
Panel B: *CO2 emissions*				
Log CO_2, direct	−0.387	0.691	0.677	0.457
Log CO_2, total	0.435	0.576	0.495*	0.271
Panel C: *Competitiveness indicators*				
Log employment	0.255	0.282	0.165	0.136
Log sales	0.856*	0.514	0.404*	0.232
Export share	−0.149	0.113	−0.061	0.059
Log investment	2.803	2.263	0.455	0.954
$\mathbb{1}$(investment > 0)	−0.278	0.265	−0.013	0.156
$\mathbb{1}$(investment machinery > 0)	−0.428*	0.235	−0.202	0.164
# of observations	39,202		39,202	
# of treated plants	497		497	

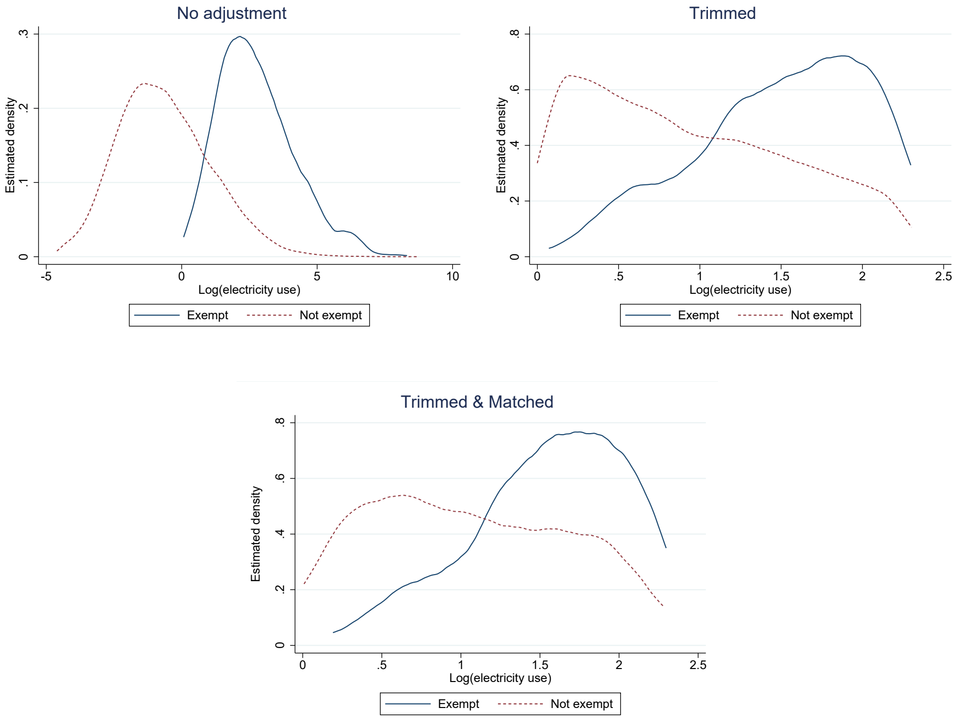
Notes: Observations from firms with an energy cost share to GVA below 15% in 2008 and 2009 are excluded from the analysis. Number of observations and number of treated plants refer to the total number of observations (plants) in the sample, independent of the bandwidth. Each cell represents a separate estimation, based on either 50% or 200% of the MSE-optimal bandwidth selector (Calonico et al., 2019) in Columns 1 and 3, respectively. Standard errors clustered at the firm level. * p<.1, ** p<.05, and *** p<.01. Source: AFiD Panel, own calculations.

E.2. Matching DiD

This section provides additional evidence concerning the identification assumptions as well as robustness for the matching DiD approach. Figure G.1 shows how trimming and matching improves the overlap for the main variable electricity use. Similarly, Figure G.3 shows that there is considerable overlap for the main propensity score, i.e., we are able to find a suitable control plant for each treated plant. Table G.5 provides a formal test for parallel pre-treatment trends and confirms that there are generally no differences between the treated and control group. The only significant variables are the electricity share in 2010 and 2009 and log employment in 2010. Yet, these differences are small in absolute size and not persistent when focusing on prior years (as also indicated by Figure 7 in the main text).

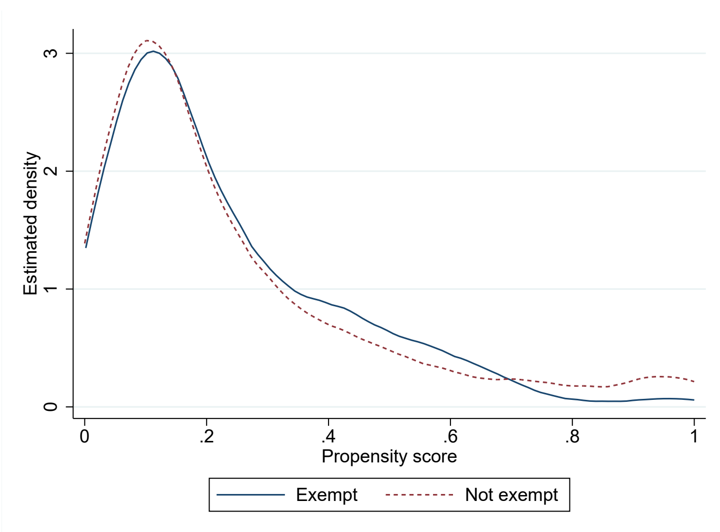
We show that restricting the sample to single-plant firms yields the same qualitative results (Column 1 of Table G.6). Furthermore, we demonstrate that our main effects are unaffected by the choice of the baseline year. As the policy change has been discussed already in 2011, firms might have anticipated the change and adapted their electricity input use. We therefore match on the year prior to the policy discussion, 2010 (Column 2 of Table G.6). Similarly, we provide evidence that the treatment of own-electricity producers does not affect our main findings (Column 3 of the same table). Table G.7 also highlights that the point estimates are unaffected by the choice of the matching algorithm.

Figure G.1: Overlap - Electricity Consumption



Notes: Density distribution of log electricity for exempted plants and non-exempted plants, without adjustment (Panel a), with trimming 1-10 GWh (Panel b), and with trimming and matching (Panel c). Source: AFiD Panel, own calculations.

Figure G.3: Overlap - Propensity Score



Notes: Overlap of the propensity score following our main matching specification and using nearest neighbor matching without replacement. Source: AFiD Panel, own calculations.

Table G.5: Test for Parallel Pre-treatment trends - Main Propensity Score

	Beta	SE	P-value
Δ *2010-2011*			
Log electricity	0.016	0.01	0.102
Electricity cost to GVA	0.01	0.007	0.156
Electricity share	0.01	0.004	0.004***
Log energy use	-0.023	0.033	0.49
Log gas use	-0.043	0.041	0.293
Log sales	-0.019	0.012	0.128
Log employees	-0.013	0.008	0.082*
Export share	-0.003	0.004	0.441
Log wages	0.012	0.008	0.156
Δ *2009-2011*			
Log electricity	0.015	0.018	0.38
Electricity cost to GVA	0.006	0.009	0.54
Electricity share	0.012	0.005	0.028**
Log energy use	-0.025	0.042	0.552
Log gas use	-0.029	0.066	0.66
Log sales	-0.007	0.021	0.753
Log employees	0.003	0.012	0.818
Export share	0.005	0.006	0.465
Log wages	0	0.012	0.976
Δ *2008-2011*			
Log electricity	0.017	0.019	0.36
Electricity cost to GVA	0.01	0.009	0.234
Electricity share	0.003	0.006	0.574
Log energy use	0.024	0.046	0.605
Log gas use	0.043	0.068	0.521
Log sales	0.008	0.021	0.711
Log employees	-0.011	0.015	0.465
Export share	0.002	0.007	0.836
Log wages	0.003	0.012	0.777
# of observations	7,062		

Notes: Test for parallel pre-treatment trends in key outcome variables for matched sample. We pool the sample for all years and regress the demeaned variables on year dummies as well as an interaction term for the year dummy with the treatment dummy and report the main interaction terms. Each outcome variable represents a separate regression to test for differences between the treatment and control group in the individual years. Main coefficient (beta) and standard error (SE) reported together with p-values. Standard errors clustered at the plant level. * p<0.1, **p<0.05, and ***p<0.01. Source: AFiD Panel, own calculations.

Table G.6: Main Robustness Checks for Matching DiD estimates

	Single-plant firms		Anticipation		No own-electricity gen.	
	ATT^{DiD}	SE	ATT^{DiD}	SE	ATT^{DiD}	SE
Δ 2013-2011	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *Electricity & fuel usage*						
Electricity consumption [GWh]	0.096	0.069	0.085	0.056	0.124**	0.062
Log electricity consumption	0.018	0.014	0.026**	0.013	0.035***	0.013
Log electricity purchase	0.026*	0.015	0.031**	0.013	0.045***	0.013
Log fossil fuel consumption	0.004	0.045	−0.02	0.037	−0.031	0.041
Share of total energy mix:						
Electricity [%]	0.005	0.007	0.002	0.005	−0.002	0.006
Fossil fuel [%]	−0.007	0.006	−0.004	0.005	−0.004	0.006
Panel B: *CO2 emissions*						
Log CO_2, direct	0.014	0.047	−0.022	0.037	0.001	0.039
Log CO_2, total	0.014	0.017	0.011	0.014	0.043***	0.015
Panel C: *Competitiveness indicators*						
Log employment	−0.005	0.013	−0.003	0.011	−0.001	0.012
Log sales	−0.017	0.018	0.01	0.014	−0.009	0.014
Export share	0.005	0.006	0	0.005	0.002	0.005
Log investment	0.033	0.166	0.091	0.133	0.066	0.146
$\mathbb{1}$(investment > 0)	−0.012	0.028	−0.026	0.022	−0.047**	0.022
$\mathbb{1}$(investment machinery > 0)	0.012	0.026	0.003	0.02	0.012	0.022
# of observations	508		702		676	
# of treated plants	254		351		338	

Notes: Outcome variables defined in differences 2013-2011. The table presents the ATT^{DiD} and standard errors (SE) from nearest neighbor (NN) matching without replacement. Columns 1 and 2 limit the sample to single plant firms. Columns 3 and 4 condition the propensity score on 2010, the pre-announcement year of the policy reform. Columns 5 and 6 limit the sample to plants that do not have own-electricity generation capacity in the base-year (2011). 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, and *** p<.01. Source: AFiD Panel, own calculations.

Table G.7: Results Matching DiD Estimates: Robustness Matching Algorithm

	1:1 matching w/ caliper and replacement		1:m matching w/ caliper and replacement	
	ATT^{DiD}	SE	ATT^{DiD}	SE
Δ 2013-2011	(1)	(2)	(3)	(4)
***Panel A:** Electricity & fuel usage*				
Electricity consumption [GWh]	0.087	0.055	0.098**	0.049
Log electricity consumption	0.03**	0.013	0.031***	0.012
Log electricity purchase	0.04***	0.014	0.031***	0.012
Log fossil fuel consumption	−0.055	0.043	−0.031	0.032
Share of total energy mix:				
Electricity [%]	0.004	0.005	0.006	0.005
Fossil fuel [%]	−0.01	0.006	−0.009*	0.005
***Panel B:** CO2 emissions*				
Log CO_2, direct	−0.046	0.043	−0.022	0.035
Log CO_2, total	0.009	0.017	0.014	0.014
***Panel C:** Competitiveness indicators*				
Log employment	0.001	0.013	−0.004	0.011
Log sales	−0.007	0.016	−0.004	0.014
Export share	0.001	0.005	0.003	0.004
Log investment	0.178	0.136	0.027	0.112
$\mathbb{1}$(investment > 0)	−0.045*	0.024	−0.04	0.028
$\mathbb{1}$(investment machinery > 0)	0.035	0.024	0.011	0.016
# of observations	748		2,349	
# of treated plants	425		425	

Notes: Outcome variables defined in differences 2013-2011. The table presents the ATT^{DiD} and standard errors (SE) from nearest neighbor (NN) matching with caliper and replacement in Columns 1 and 2, and one-to-many matching with caliper and replacement in Columns 3 and 4 following Specification (5). The sample is limited to plants that report in both the treatment year and the base year. Robust standard errors in Columns 2 and 4. * p<.1, ** p<.05, and *** p<.01. Source: AFiD Panel, own calculations.

E.2.1. Alternative Propensity Score Definitions

For robustness, we estimate two alternative propensity score variants, based on a minimum specification that only conditions on electricity cost intensity or alternatively energy cost intensity in the base period 2011 within economic subsector. These specifications do not include any additional covariates in the base period, nor do they condition on lags of electricity intensity measures. We use energy cost for matching because it is directly observed in the sample and thus not prone to measurement error from imputing electricity cost. Yet, as energy cost are elicited only for a subset of firms, this specification leads to a smaller sample size.

As shown in both Figures G.4 and G.5, the matching variables lead to very similar pre-treatment trends. Similarly, the main effects in Tables G.8 and G.9 are highly aligned with out preferred specification.

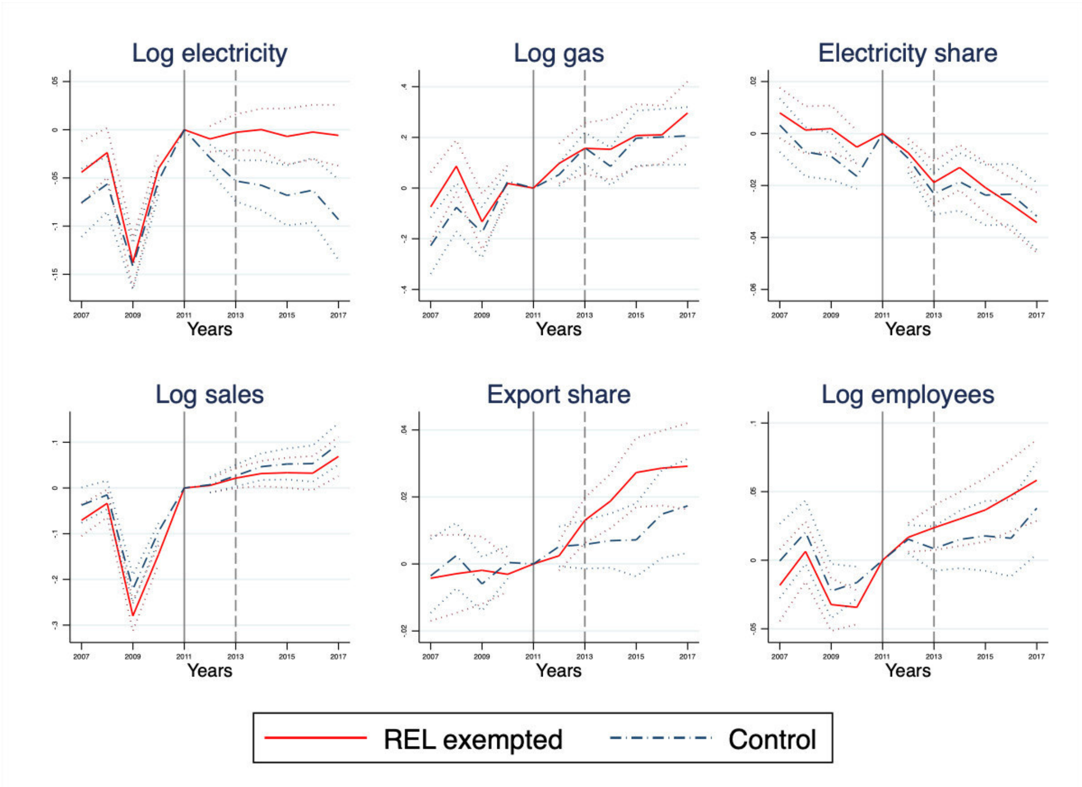
D.3.1.1 Propensity Score 1: Electricity Cost Intensity

Table G.8: Results Matching DiD Estimates

Main sample	all plants		single-plant firms		5-10 GWh	
	ATT^{DiD}	SE	ATT^{DiD}	SE	ATT^{DiD}	SE
Δ 2013-2011	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *Electricity & fuel usage*						
Electricity consumption [GWh]	0.172***	0.054	0.148**	0.065	0.361***	0.125
Log electricity consumption	0.05***	0.013	0.041***	0.016	0.055***	0.019
Log electricity purchase	0.055***	0.014	0.041***	0.016	0.074***	0.021
Log fossil fuel consumption	0.002	0.032	−0.012	0.037	−0.058	0.048
Share of total energy mix:						
Electricity [%]	0.01*	0.006	0.008	0.008	0.001	0.008
Fossil fuel [%]	−0.011**	0.005	−0.01*	0.006	−0.015**	0.007
Panel B: *CO2 emissions*						
Log CO_2, direct	0.002	0.033	−0.021	0.038	−0.04	0.046
Log CO_2, total	0.033**	0.014	0.024	0.016	0.044**	0.02
Panel C: *Competitiveness indicators*						
Log employment	0.03**	0.012	0.01	0.015	0.016	0.015
Log sales	0.002	0.016	−0.029	0.02	−0.027	0.022
Export share	0.003	0.004	0.003	0.006	−0.003	0.009
Log investment	0.023	0.125	0.125	0.162	−0.304*	0.176
$\mathbb{1}$(investment > 0)	−0.014	0.022	0.014	0.028	0	0.025
$\mathbb{1}$(investment machinery > 0)	0.014	0.018	0.024	0.021	0.012	0.033
# of observations	848		572		336	
# of treated plants	424		286		168	

Notes: Outcome variables defined in differences 2013-2011. The table presents the ATT^{DiD} and standard errors (SE) from nearest neighbor (NN) matching without replacement following Specification (5). 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, and *** p<.01. Source: AFiD Panel, own calculations.

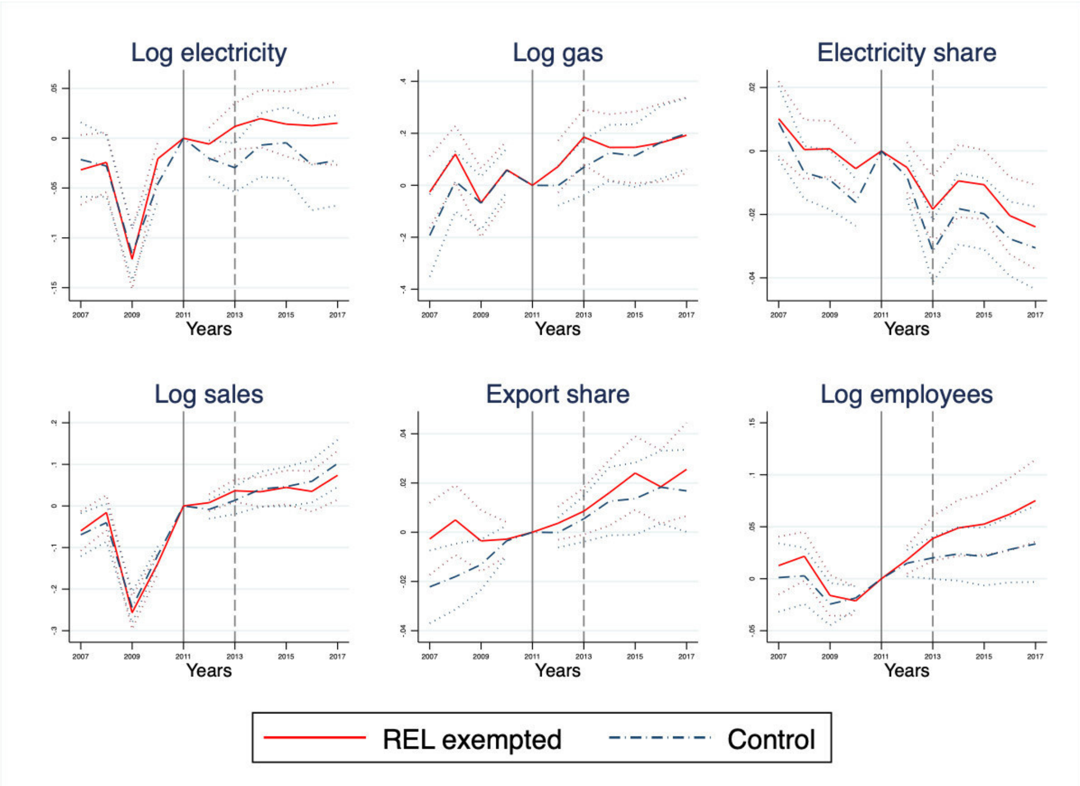
Figure G.4: Common trends: Minimal Specification, Electricity Cost to GVA



Notes: Analysis of parallel pre-treatment trends for treated plants (REL exempted in 2013) and matched control plants based on nearest neighbor matching. Results based on strict sector propensity score matching on electricity cost intensity in the base year 2011. The figure plots growth rate of the respective variables with respect to 2011, the year determining treatment status. The vertical line indicates the base year (2011) as well as the first treatment year (2013). Source: AFiD Panel, own calculations.

D.3.1.2 Propensity Score 2: Energy Cost Intensity

Figure G.5: Common trends: Minimal specification, Energy Cost to GVA



Notes: Analysis of parallel pre-treatment trends for treated plants (REL exempted in 2013) and matched control plants based on nearest neighbor matching. Results based on strict sector propensity score matching on energy cost intensity in the base year 2011. The figure plots growth rate of the respective variables with respect to 2011, the year determining treatment status. The vertical line indicates the base year (2011) as well as the first treatment year (2013). Source: AFiD Panel, own calculations.

Table G.9: Results Matching DiD Estimates

Main sample	all plants		single-plant firms		5-10 GWh	
	ATT^{DiD}	SE	ATT^{DiD}	SE	ATT^{DiD}	SE
Δ 2013-2011	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *Electricity & fuel usage*						
Electricity consumption [GWh]	0.174**	0.07	0.351***	0.106	0.332***	0.123
Log electricity consumption	0.04***	0.015	0.067***	0.023	0.057***	0.02
Log electricity purchase	0.049***	0.015	0.072***	0.022	0.056***	0.02
Log fossil fuel consumption	−0.029	0.051	−0.025	0.068	−0.056	0.06
Share of total energy mix:						
Electricity [%]	0.016**	0.007	0.01	0.01	0.005	0.008
Fossil fuel [%]	−0.016**	0.006	−0.009	0.009	−0.022***	0.008
Panel B: *CO2 emissions*						
Log CO_2, direct	−0.046	0.05	−0.033	0.068	−0.048	0.061
Log CO_2, total	0.015	0.018	0.047*	0.025	0.021	0.024
Panel C: *Competitiveness indicators*						
Log employment	0.025*	0.014	0.043**	0.018	0.021	0.018
Log sales	0.03	0.021	0.049*	0.027	−0.018	0.024
Export share	0.005	0.007	0	0.008	0.012	0.011
Log investment	0.254*	0.146	−0.166	0.205	0.29	0.181
$\mathbb{1}$(investment > 0)	0.023	0.023	0.034	0.032	0.029	0.025
$\mathbb{1}$(investment machinery > 0)	0.012	0.025	0.047	0.031	0.029	0.029
# of observations	520		298		272	
# of treated plants	260		149		136	

Notes: Outcome variables defined in differences 2013-2011. The table presents the ATT^{DiD} and standard errors (SE) from nearest neighbor (NN) matching without replacement following Specification (5). 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, and *** p<.01. Source: AFiD Panel, own calculations.

E.3. Difference-in-Differences Analysis

Our main matching specification controls for unobserved differences at the plant level, which cannot confound our estimates. Yet, a concern could be potential selection into treatment based on unobserved trends. As a robustness check, we implement an alternative estimation approach that only exploits changes in eligibility status for identification.

To implement this approach, we use the subset of plants that are newly eligible for the exemption (1-10 GWh electricity use in the baseline period 2011). We then construct two groups of plants: one with an electricity cost to GVA ratio of at least 0.14, whose eligibility status changes from 2011 to 2013 and a second group with a ratio below 0.14, which remain not eligible in 2013. Furthermore, we ensure that electricity intensity is not a confounding factor by only considering only plants with a similar electricity intensity (between 0.1 and 0.18).

The choice of a maximum deviation of 4 percentage points reflects a trade-off. When we restrict the maximum deviation further, the difference in the exemption rates vanishes (because electricity intensity is measured with some error). When we set it too large, we risk to capture confounding effects that arise because plants with different electricity intensities react differently to the pronounced increase in electricity prices during that time span. The effects we present are robust for intermediate deviations of 0.03-0.05 around the cutoff, yet become unstable if we consider very smaller deviations (0.02 or smaller) or remove the threshold altogether. Additional results are available from the authors upon request.

Similar to our matching DiD approach, we limit the sample to plants that report electricity in all years 2007-2017 to avoid changes in composition. We then estimate the following specification:

$$\Delta y_{ist} = eligible_i \times \sum_{t \neq 2011} year_t + \mu_{t,s} + \varepsilon_{ist}, \tag{10}$$

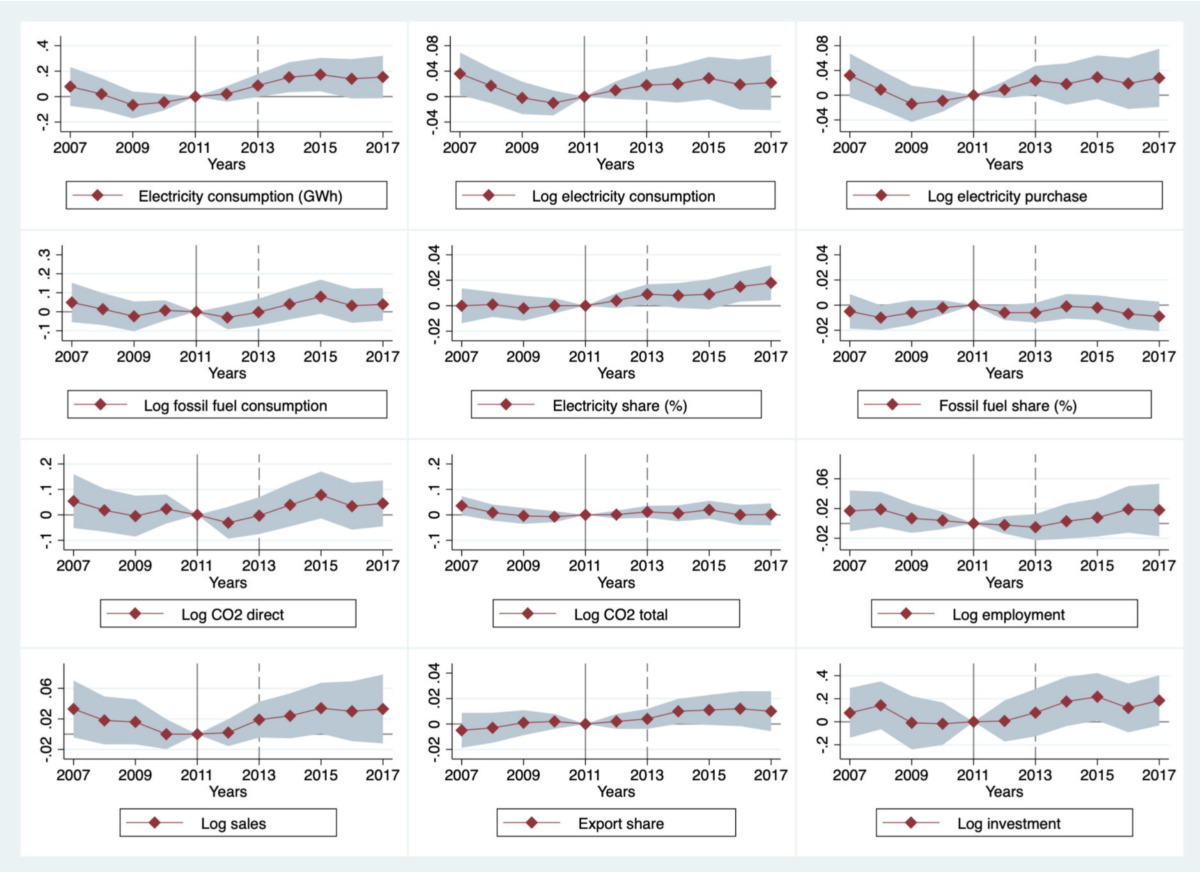
where Δy_{ist} is defined as the demeaned (with respect to 2011) outcome variable for plant i in sector s in time period t. The main variable of interest is the effect of the eligibility dummy, which is equal to one for the group of plants with an electricity cost intensity of more than 0.14, which become newly eligible between 2011 and 2013 and zero otherwise. We interact it with year dummies ($year$) to estimate the average difference in the evolution of mean outcomes for both groups of plants (relative to 2011, for every year). To capture possible unobserved trends at the sectoral level, we control for two-digit sector by year fixed effects $\mu_{t,s}$. Standard errors are clustered at the plant level to allow for correlation over time within the same plant.

The estimation equation can be directly used to test for parallel pre-treatment trends in the period 2007 to 2010. Our main coefficient of interest is the treatment effect in 2013 as well as the impact in subsequent years. All effects can be interpreted with respect to the baseline period 2011, which is omitted from the regression. As treatment is based on the eligibility criteria for the exemption, the results can be interpreted as intention-to-treat (ITT) effects. These estimates thus provide us with a useful lower bound for our main matching DiD results.

Figure G.6 plots the effects for key outcome variables together with the 95% confidence interval. We generally do not find any significant differences between the treated group (plants that fulfill the eligibility criteria in 2011 for the REL exemption in 2013) and the control group in the three years leading up to 2011. On the other hand, we can reject the null hypothesis of a zero effect for electricity use for the treated group starting in 2013. The main effect for electricity consumption is 0.087 GWh, with a p-value of 0.06. The effect size is thus highly comparable to the treatment effect that we estimate with the matching DiD approach in the main text of 0.092 in Table 4. Similarly, log electricity consumption shows a point estimate of 0.018 in 2013, increasing to 0.029 in 2015, and is thus again similar to the matching DiD findings. Finally, we confirm a significant increase in the electricity share of eligible plants after 2015, but do not find any significant impact on the competitiveness indicators. The fact that the main estimates of the DiD approach are highly aligned with the results from the matching DiD setting supports that selection issues are not a major concern in our setting when estimating the main treatment effects.

We use this model also to estimate the first stage of the treatment assignment in this setting, namely we regress the exemption status on eligibility and find a total effect of 0.168 from year 2013 onward, significant at $p < 0.001$. This effect is conditional on sector-by-year fixed effects and is consistent with the share of exempted plants among the eligible, which has on average increased by about that magnitude for all plants with electricity uses between 1 to 10 GWh (see Panel (d) of Figure A.7).

Figure G.6: DID: Parallel Trends and Intention-To-Treat Effects



Notes: Analysis of intention to treat (ITT) effects for plants that are eligible for the REL exemptions in 2013. Sample restricted to plants with an annual electricity use of 1-10 GWh in 2011 and a share of electricity cost to GVA from 10 to 18%. Each plot refers to a separate regression where the main outcome variable is demeaned with respect to the baseline period 2011. Point estimates for the interaction term of treatment eligibility and year following Equation (10) plotted together with 95% confidence intervals. The vertical lines indicates the year determining treatment (2011, omitted category) and the main outcome year 2013. Source: AFiD Panel, own calculations.

E.4. Counterfactual Simulations

In this subsection, we test the robustness of our counterfactual simulations. To rule out that the sample of plants with an (imputed) electricity cost to GVA share of more than 0.15 may contain some non-eligible plants, we also conduct our simulations based on compliance cost estimates that we obtain from a sample of firms with an electricity cost to GVA of at least 0.25. As shown in Section D, it is very rare that measurement error exceeds 10 percentage points. Hence, restricting the sample to firms with an electricity cost to GVA of more than 0.25 almost certainly restricts the estimation to eligible plants. As shown in Table G.10, the results from the counterfactual simulations remain almost unchanged, which alleviates concerns about the impact of measurement error on the estimation of the parameters and the simulations.

Table G.10: Simulations of Bunching and Exemption Effects - Robustness

(a) Bunching Behavior (in t)

	(1) # of bunchers	(2) Bunching, in GWh	(3) Max. bunching, in %	(4) Bunching cost, in Mio. EUR	(5) Externality cost, in Mio. EUR
Simulations for Bunching in 2008 to 2011 Under the Respective Exemption Designs					
(1) 2011 (reformed)	0	–	–	–	–
(2) 2010 (notched)	50	66.0	33,8%	7.8	1.2
(3) 2009 (notched)	11	5.0	10,1%	1.3	0.1
(4) 2008 (notched)	1	0.1	01,2%	0.1	0.0
Counterfactual Simulations for 2013 under a Notched Exemption Design					
(5) 2011 (notched)	76	89.9	35,3%	11.5	2.2
(6) REL 2017	212	484.4	85,6%	43.3	12.1
(7) Costless compliance	219	317.2	37,6%	36.0	7.9
(8) No fixed bunching cost	137	247.2	61,7%	11.8	6.2
(9) No frictions, REL 2017	612	1980.2	25,1%	95.4	49.3

(b) Exemption Behavior (in $t+2$)

	(6) # of exemptions (actual #)	(7) Electricity use change, in GWh	(8) Exemption value, in Mio. EUR (actual value)	(9) Compliance cost, in Mio. EUR	(10) Externality cost, in Mio. EUR
Simulations for Exemptions in 2010 to 2013 Under the Respective Exemption Designs					
(1) 2013 (reformed)	1374 (1574)	2227.4	3965 (3804)	305.0	74.8
(2) 2012 (notched)	824 (697)	1548.0	2585 (2393)	261.3	39.0
(3) 2011 (notched)	601 (579)	1345.5	2206 (2249)	149.3	33.5
(4) 2010 (notched)	523 (539)	839.9	1174 (1219)	112.6	14.7
Counterfactual Simulations for 2013 under a Notched Exemption Design					
(5) 2013 (notched)	899	2129.3	3762 (3765)	273.3	71.5
(6) REL 2017	1124	2941.9	5197 (4948)	436.6	98.8
(7) Costless compliance	1355	2431.0	4269 (3765)	0.0	81.7
(8) No fixed bunching cost	961	2141.9	3783 (3765)	277.2	72.0
(9) No frictions, REL 2017	1748	3265.6	5739 (4948)	0.0	109.7

Notes: For every scenario, we present profit-maximizing market behavior in the baseline period (Panel a) and exemption period (Panel b). Values represent averages over 200 compliance cost draws. To estimate compliance cost conservatively, we use only eligible plants with an electricity cost to GVA ratio of at least 25%. The scenarios in Rows (1)-(4) simulate market behaviors 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 ct. per kWh), eliminate compliance cost, and set 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 cost are calculated as explained in Appendix Section F. Source: AFiD Panel, own calculations.

F. Wedge Between Social Marginal Cost of Electricity and the Average Price Paid by the Industry

To obtain an estimate for the social marginal cost (SMC) of electricity in Germany, we follow Borenstein and Bushnell (2022) and account for three main cost components: the (wholesale) price of electricity, direct state subsidies (and comparable other payments to producers), as well as non-internalizable external costs of electricity generation. The respective values shown in Table F.1 are drawn from Küchler and Wronski (2015), who calculate the total cost by electricity generation technology in Germany in 2014.

Table F.1: Social Marginal Cost of Electricity, 2014

	Conventional				Renewable		
Technology	Nuclear	Coal	Lignite	Gas	Wind	Hydro	Solar PV
Wholesale electr. price or FiT), ct/kWh	3.86	3.86	3.86	3.86	9.7	9.1	31.7
Subsidies (fin. support, tax reductions), ct/kWh	0.6	2.4	1	-0.1	-0.4		-0.4
Non-internalizable external costs, ct/kWh	9.8 - 32.7	8.1	10	4.2	-0.2	-0.2	0.8
SMC by technology, ct/kWh	14.26 - 37.16	14.36	14.86	7.96	9.1	8.9	32.1
Production shares 2014[1]	0.183	0.207	0.295	0.11	0.099	0.039	0.066
(1) Weighted SMC, ct/kWh				14.23			
(2) Avg. ind. elec. price incl. REL (excl. VAT), ct/kWh[2]				13.51			

Wedge between SMC and avg. ind. elec. prices (for exempted and non-exempted plants)

	2008	2009	2010	2011	2012	2013
(3) REL, ct / kWh	1.12	1.33	2.05	3.53	3.59	5.28
Wedge for non-exempted, ct / kWh: (1) − (2)	0.72	0.72	0.72	0.72	0.72	0.72
Wedge for exempted, ct / kWh: (1) − [(2) − (3)]	1.84	2.05	2.77	4.25	4.31	6.00
Average wedge in range $[p_{elec}, p_{elec} - (3)]$	1.28	1.39	1.75	2.49	2.52	3.36

Notes: Data Sources: SMC by technology: Küchler and Wronski (2015), [1]Production shares by technology: Source BDEW, 12/2014 , [2]Average industry electricity prices: Eurostat, Electricity prices for non-household consumers, bi-annual data (see Footnote 17).

While conventional technologies receive the market (wholesale) price of electricity, producers from renewables (wind, hydro, and solar PV) mostly obtain fixed payments (feed-in tariffs, FiTs), that are set above the wholesale electricity price. The first element therefore refers to the price paid for production from each of these technologies. Similarly, as discussed in detail in Küchler and Wronski (2015) there are several direct payments in form of tax-financed subsidies or other financial benefits to producers of conventional electricity that need to be added to the electricity price. The negative effects for solar, wind, and gas result from the fact that these technologies paid a higher energy tax as should be required by the general model of energy taxation (based on energy content and external costs) (see discussion in Küchler and Wronski (2015)).

Finally, the external costs resulting from each of the technologies need to be accounted for. These mainly refer to emission related costs, but also take into account external costs due to the possibility of nuclear accidents or material and energy costs in the production of renewable technologies, such as solar PV panels. While electricity generation is part of the European Emission Trading Scheme, the permit prices have been at an historic low during the period 2013 to 2014. The non-internalizable external costs, aim to account for the excess damages, not account for by carbon prices in case of fossil fuels or other externalities. There exist a wide vary of estimates for the external costs of nuclear energy. For a conservative value, we take the lower bound estimate provided in Küchler and Wronski (2015).

To obtain a single SMC for electricity in 2014, we multiply the price per kWh by the respective production shares of each technology in 2014. Note that this approach likely results in a lower bound for the true SMC as we do not account for production from some technologies, such as biogas or co-generation, which might receive FiTs above the market price of electricity. Similarly, our calculations do not take into account additional costs from grid balancing (ancillary service costs), which are typically small (less than 1% of the total energy price), and other costs from distributional losses. Yet, the simple accounting framework provides us with a useful benchmark to compare the SMC to the average electricity price in the industry.

Following this procedure, we obtain an SMC of electricity of 14.2 ct / kWh. This value is higher than the average electricity price of the non-exempted firms of 13.5 ct / kWh in 2014. Hence, we see that the an exemption from the levy increases the gap between the SMC and the electricity price considerably. As a result, any increase in electricity use leads to welfare losses.

To assess the social damages from higher electricity use, we calculate the wedge between the SMC and the price that firms pay when exempted and not exempted. Because we observe the SMC only for 2014, we assume that the wedge for firms that are non-exempted is time-constant. We consider this assumption as not very restrictive as variation in wholesale prices over time leaves the wedge unaffected. Because we assess inframarginal price changes, we then calculate the average wedge between the average industry price of a non-exempted plant and an exempted plant (last row of Table F.1). We obtain an average wedge that increases from 1.28 ct/kWh in 2008 to 3.36 ct./kWh in 2013.

Under the assumption that demand for electricity is locally linear in the price range affected by an exemption $[p_{elec}, p_{elec} - REL]$, the product of the average wedge and the aggregate electricity use change equals the welfare loss from an increase in electricity use in response to REL exemptions.