Energy Tax Exemptions and Industrial Production

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Unilateral climate 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 inframarginal 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. Our results cast doubt on the necessity of energy tax exemptions to retain domestic production and caution against the use of notched exemption policies.

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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 climate policies (e.g., Fischer and Fox 2012; Fowlie and Reguant 2018). 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 2014. 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 also contrast the effects of REL exemptions under both policies to explore how differences in policy design influence production choices.

Our empirical strategy exploits two sources of exogenous variation. First, to estimate the causal effects under the 'notched' policy design, we exploit the fact that eligibility for an exemption was only granted to plants that used more than 10 gigawatt-hour (GWh) of electricity two years earlier. We provide evidence that the severe financial crisis of 2008 and 2009 prevented plants from potentially manipulating their electricity use in those years despite the notched ex-

[1] The OECD Database on Policy Instruments for the Environment lists roughly 2,400 exemptions from environmentally related taxes in all OECD countries (OECD, 2020). In the United Kingdom, Belgium, and Finland, for example, energy tax exemptions are granted to manufacturing firms in sectors that are defined as EITE. In other countries, such as France, the Netherlands, and Italy, exemptions are granted to energy-intensive manufacturing firms with energy use above pre-defined eligibility thresholds.

emption 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.

Our main results show that the REL exemptions lead to increases in electricity consumption under both policy designs. We find that exempted plants in the original (notched) schedule increased their electricity consumption on average by 40-50% compared to the control group, while the 2012 reform led to an increase of approximately 5-7%, which translates into an own price elasticity for electricity of about -2 and -0.2, respectively. Based on a stylized model of firm production, we show that these differences can be rationalized by the distortionary effects introduced by the notched tax design. By contrast, we do not find statistically significant impacts of the REL exemption on short-term competitiveness indicators such as sales, export share, employment, and investment.

We conduct extensive robustness tests for our main findings and present supporting evidence for the identifying assumptions. 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 may matter, we condition on characteristics in the year prior to its announcement. In addition, we test for the presence of intra-firm spillovers by restricting our sample to single-plant firms.

This paper makes three main contributions. First, we contribute to the growing literature on incomplete environmental regulation. Previous studies have shown that leakage is a concern for environmental regulation in the electricity and cement market (Fowlie, 2009; Fowlie et al., 2016), for example. One channel is that multinational firms may relocate production to subsidiaries in unregulated jurisdictions (Hanna, 2010; Borghesi et al., 2020).[2] Other studies have 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 Martin et al., 2014a; Bernard et al., 2007).[3] We contribute to this literature by evaluating a large exemption policy for EITE plants in the German manufacturing sector. While similar exemptions are employed in many industrialized countries (OECD, 2020), empirical evidence on their causal effects is scarce. One exception is Martin et al. (2014b), who evaluate the impact of the climate change levy on production of manufacturing plants in the UK, using plants that were exempted from the levy as control group.[4] We complement their work by showing that exemption schemes may only have a limited potential to increase international competitiveness in the short-run, while they significantly influence fuel input choices and lead to higher energy uses. Similarly, focusing on a rich empirical setting, we are able to compare two policy settings for exemption rules and contribute to the ongoing policy discussion on the effective design of exemption rules.

Second, our paper contributes more broadly 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 produc-

[2]Further studies on the European emission trading scheme (ETS) did not detect adverse effects on firm holdings of fixed assets (Moore et al., 2019) or changes in net trade flows (Naegele and Zaklan, 2019).

[3]The relative merits of these instruments have been extensively studied based on computable generable equilibrium (CGE) models (e.g. Fischer and Fox 2007, Fischer and Fox 2012, Böhringer et al. 2014a, Carbone and Rivers 2017). In addition, output-based compensation payments for indirect carbon costs in Great-Britain and Finland are analyzed in (unpublished) work by Basaglia et al. (2019) and a recent government report by Laukkanen et al. (2019), respectively. As a recent theoretical contribution, Ahlvik and Liski (2019) propose an optimal compensation mechanism for mobile firms that have private information about both relocation and abatement cost.

[4]Further exceptions include CGE simulations by Böhringer et al. (2012), who find that exemptions are inferior to border tax adjustments and output-based allocation policies in terms of global cost-effectiveness, as well as Böhringer et al. (2014b), who find that reducing environmental taxes for energy intensive and trade-exposed firms to mitigate leakage yields only small efficiency gains compared to uniform pricing.

tion in manufacturing (see, e.g., Fowlie et al. 2012, Greenstone 2002, Greenstone et al. 2012, as well as Martin et al. 2016 and Dechezleprêtre and Sato 2017 for reviews.) We contribute to this strand of the literature by showing that design features of tax exemption policies may interact with firm production choices. Our findings suggest that the increase in electricity use for exempted plants is almost one order of magnitude larger under a 'notched' tax design, compared to a policy design where notches have been largely removed. These results indicate that plants use exemptions in a given year as a 'subsidy for bunching' in order to reach eligibility for an exemption in future years.

Third, we contribute to a literature that has investigated the role of energy prices for industrial production. Previous studies have shown that higher prices reduce energy use in manufacturing (Marin and Vona, 2019), but also modestly decrease employment (e.g., Deschenes 2012, Commins et al. 2011), and co-determine the location of firms (Kahn and Mansur, 2013). We complement this literature by evaluating how large exogenous variations in electricity prices affect production, employment, and the energy input mix of manufacturing plants. An advantage of our setting is that we can exploit two natural experiments that induce large variations in electricity prices for identification. In the absence of such variation, a major empirical challenge for obtaining unbiased estimates is the isolation of the exogenous component of energy prices.

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. The empirical analysis is divided into two 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. We compare our results from both settings in Section 6. Section 7 discusses the policy implications of our findings and concludes.

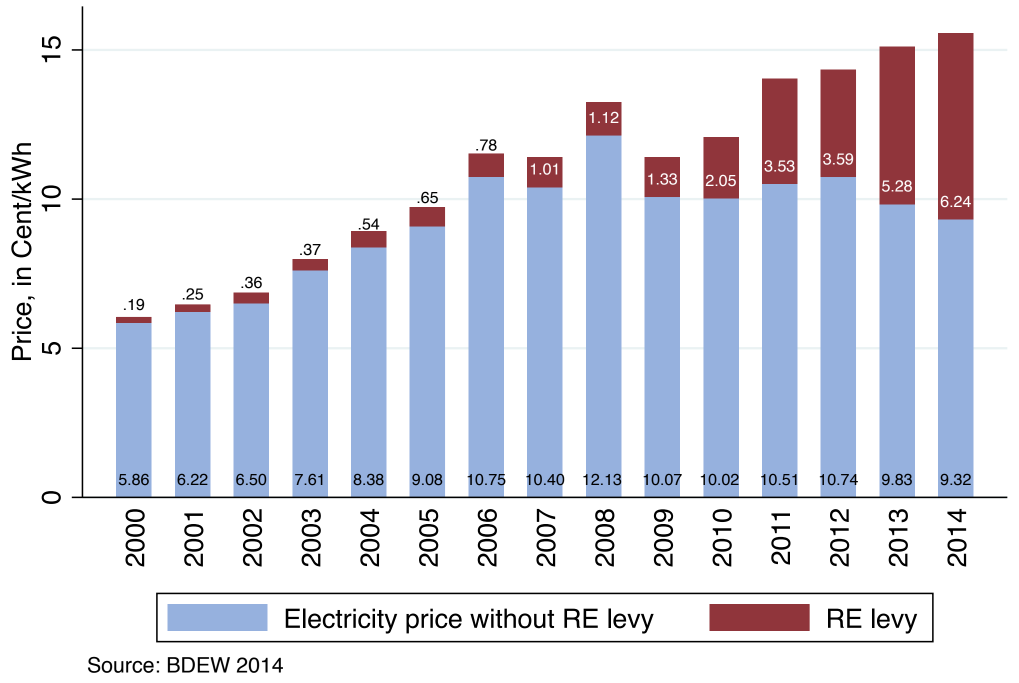
2. Institutional background

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.[5] The introduction of FiTs triggered a rapid increase in the share

[5]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

Figure 1: Average Industry Electricity Prices in Germany



Notes: Average industry electricity prices (including taxes) in Germany for plants with an annual electricity consumption between 160 MWh and 200 GWh (BDEW, 2014).

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 2014. In this period, average electricity prices for the industry have risen substantially, from about 6 cents per kWh in 2000 to 15 cent per kWh in 2014. An important role in this increase is played by the REL, which increased from 0.19 cents per kWh in 2000 to 6.24 cents per kWh in 2014, accounting for more than a third of the average industry electricity price in that year.

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

to support renewable energy deployment in most European countries and many other jurisdictions such as Australia, California, and Ontario.

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.

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 gross value added in the previous year. 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 (about 80%) are granted to plants in the manufacturing sector, on which we focus in our analysis (for details on the sectorial composition see Appendix Section A.1).

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 gross value added 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 gross value added 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 gross value added 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.1).[6] 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

[6]To be eligible for the exemption, plants also have to document that they operate a certified energy management system. However, this is not a strict requirement as plants can resort to a simplified procedure that only requires them to 'judge' possible energy savings for all energy consuming sites. In 2012, 84% of all plants took that option (BAFA, 2014). In 2013, generous transitional provisions prevented plants from losing eligibility for not having a certified energy management system.

2012.[7] 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.[8]

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

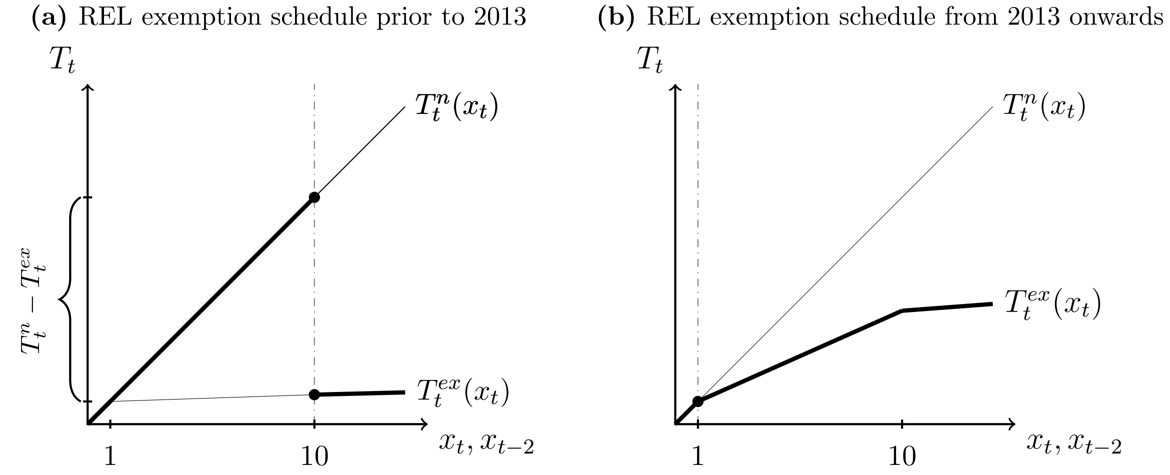
Figure 2 plots the original exemption schedule (Panel a) and the revised schedule after the policy reform (Panel b), where T_t^n and T_t^{ex} denote the total REL payment for non-exempted and exempted plants, respectively. Under the original policy design (Panel a), plants can be exempted in period t 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. 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

[7] 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).

[8] 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 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).

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:

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

where x represents the main production input, electricity, and z is a composite input good. Firms have heterogeneous productivity regarding the input x, 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:

$$T(x) = \begin{cases} tx - A^f \text{ if } x \geq \hat{x} \\ tx \qquad\quad \text{if } x < \hat{x}, \end{cases}$$

where t denotes a per-unit tax rate of x and A denotes a lump-sum payment that the firm obtains when its input use exceeds a predefined threshold value $\hat{x}$ in the current period. In our setting, A corresponds to the present value of being exempted from the tax two years later in response to passing the electricity use eligibility threshold today.

In Appendix Section B, we show that the impact of a tax exemption under the notched design can be decomposed as follows:

$$\frac{\partial x^*}{\partial t^{ex}} = \underbrace{\int_0^\infty \frac{\partial x^c}{\partial t^{ex}} g(\psi) d\psi}_{\text{Marginal price response}} + \underbrace{\int_{\psi^{m'}}^{\psi^m} (\hat{x} - x^c)\, g(\psi) d\psi - \int_{\psi^{m'}}^{\psi^{\hat{x}}} \frac{\partial x^c}{\partial t^{ex}} g(\psi) d\psi}_{\text{Net bunching response}}, \tag{1}$$

where $x^* = \int_0^\infty x^*(\psi) dG(\psi)$ denotes firms' average input use and t^{ex} denotes the reduction in the tax rate t that is granted by an exemption. Furthermore, x^c denotes firms' optimal input choice in the hypothetical case without the tax notch ($A = 0$). The productivity for a firm that is indifferent between increasing its input use to become eligible for an exemption in the time period before experiencing the tax reduction is given by ψ^m, while $\psi^{m'}$ denotes the productivity of the marginal firm in the period after the tax reduction. As a tax exemption reduces the electricity price for the firm, bunching becomes cheaper, so that $\psi^{m'} \leq \psi^m$.

Equation (1) highlights two distinct mechanisms of how a tax exemption in a given time period affects the use of the production input under the notched policy design. First, an exemption reduces marginal tax rates for all exempted firms. This reduction yields a marginal price response (MPR), which is captured by the first term of Equation (1). Second, an exemption in a given period works as a 'subsidy for bunching'. Lower prices make it profitable for more firms to increase their electricity use in order to obtain the infra-marginal benefits from getting exempted in the future. In our model, this is captured by a change in the marginal firm from ψ^m to $\psi^{m'}$. The incremental adjustment of energy inputs for the firms in the interval $[\psi^m, \psi^{m'}]$ is captured by the second term, while the third term gives the hypothetical change in input use for the same firms in absence of bunching. Hence, we denote the second and third term of Equation (1) as the net bunching response (BR).

Our conceptual model provides two theoretical predictions on firm's electricity input use. First, we expect 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 directly from observing that eliminating the tax notch also eliminates the net bunching response, which enters additively into Equation (1). Second, we expect that bunching above the eligibility threshold occurs under the notched policy design when manipulation of the input variable is not too costly. We test for both these predictions in the empirical section of this paper, where we use two distinct sources of exogenous variation to provide causal estimates concerning the impact of the REL exemption on production inputs and plant-level outcomes.

3. Data

Our empirical analysis is based on a rich administrative dataset on the German manufacturing industry for the period 2007 to 2013 (*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).[9] In addition, AFiD provides information on total energy cost and gross value added at the firm level for a representative sample of firms. Information on electricity cost that would allow us to calculate the ratio of electricity cost to gross value added is not available.

We link these 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

[9] 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 €	37.265	453.930	39,045	26.527	69.815	641	79.284	219.216	1,458
Export share (of sales)	0.214	0.263	39,045	0.212	0.262	641	0.281	0.286	1,458
Number of employees	136	620	38,422	73	83	645	177	251	1,454
Investments, in million €	1.229	15.275	39,198	0.775	5.563	639	2.339	7.160	1,444
Avg. wage per employee, thd. €	33.695	13.614	38,421	33.577	9.795	645	38.755	14.848	1,454
Energy-related covariates									
Electricity use, in GWh	3.652	48.653	38,917	5.474	4.360	630	52.280	164.919	1,429
Electricity use (2011), in GWh	3.768	46.610	36,693	5.135	2.350	608	56.096	192.672	1,431
Other energy use , in GWh	15.939	631.243	39,049	9.574	18.800	638	120.379	602.199	1,443
Own electricity generation, in %	0.089	0.285	40,755	0.085	0.279	659	0.129	0.335	1,482
Electricity share in total energy	0.518	0.259	38,917	0.599	0.310	630	0.558	0.316	1,429
Gas share in total energy	0.297	0.292	38,917	0.281	0.307	630	0.281	0.301	1,429
Oil share in total energy	0.134	0.237	38,917	0.050	0.136	630	0.035	0.115	1,429
Coal share in total energy	0.005	0.063	38,917	0.010	0.086	630	0.031	0.134	1,429
Renewable share in total energy	0.047	0.161	38,917	0.061	0.194	630	0.094	0.229	1,429
Total CO_2 emissions, in 1,000 t	5,540	181,444	39,049	4,707	4,983	638	50,328	181,140	1,443
Direct CO_2 emissions, in 1,000 t	3,862	175,915	39,049	1,692	4,064	638	22,876	139,819	1,443
Firm-level data									
Number of plants per firm	3.124	13.079	40,755	2.088	2.908	659	2.304	3.124	1,482
Gross value added, in million €	118.257	918.299	17,807	19.119	53.772	355	43.443	111.323	1,006
Sales, in million €	172.129	2073.110	40,755	87.524	355.118	659	175.171	512.104	1,482
Total energy cost, in million €	6.094	37.237	17,806	7.068	27.229	355	21.392	44.040	1,006

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. 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, 2007-2013, own calculations.

match close to 93% of plants (662 out of 715 plants) under the original tax exemption regime in 2010 and 2011 and close to 80% of the newly exempted plants in 2013.

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 136 employees and sales of about 37 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 (73 and 27 Mio. EUR, respectively), these plants use considerably more electricity on average (5.5 GWh vs. 3.7 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 177 employees and 79 Mio. EUR of sales on average. The average electricity consumption in that group exceeds 56

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 5.1 GWh in 2011 to 5.4 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. In the following two sections, we provide details on how we estimate the causal impact of the REL exemption on electricity use as well as other energy inputs and plant-level outcomes under each of the two policy designs.

4. REL exemptions under the notched policy design

Our empirical analysis first 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) 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 can 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. A concern in our setting might be that plants strategically increase their baseline consumption in period t to become eligible for the exemption in the future period $t + 2$. To circumvent such concerns, we focus on baseline years during the financial crisis, when plants faced unprecedented cuts in production levels that made such manipulations very costly. This is especially true as electricity use in manufacturing is highly output dependent. We present supporting evidence in the next section that plants were in fact unable to precisely control the running variable during the years 2008 and 2009.

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):

$$ATT^{RD} = \frac{\lim_{\epsilon \downarrow 0} E(Y_i|R_i = c + \epsilon) - \lim_{\epsilon \uparrow 0} E(Y_i|R_i = c + \epsilon)}{\lim_{\epsilon \downarrow 0} E(T_i|R_i = c + \epsilon) - \lim_{\epsilon \uparrow 0} E(T_i|R_i = c + \epsilon)}, \tag{2}$$

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 (2) 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 face incentives to 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. Yet, selection is particularly difficult when plants are hit by unanticipated shocks. For that reason, we consider for identification only the baseline years that coincide with the financial crisis in 2008 and 2009, which had an unparalleled impact on German manufacturing. For example, in 2009, gross value added in the manufacturing sector plummeted by 19% and many firms resorted to short-term working arrangements for their employees. During times of extreme economic uncertainty, manipulating electricity consumption to reach eligibility status is much more difficult and costly compared to times with predictable economic activity.

This conjecture is supported by our data. If plants have only imprecise control over the running variable, we expect to see a distribution of baseline electricity consumption that is

Figure 4: Number of Plants in 0.5 GWh Bins of Electricity Consumption in 2008 and 2009

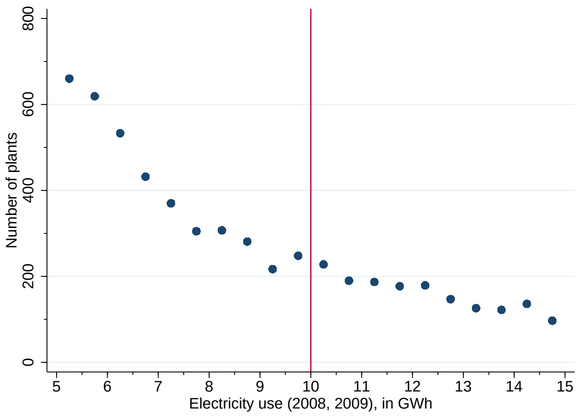


Table 2: McCrary's Test of Continuity

Year	2008	2009	2010	2011	2012
Test statistic	-0.02	-0.04	0.35***	0.01	0.15
	(0.11)	(0.12)	(0.14)	(0.12)	(0.12)
# of obs.	12,161	12,186	12,585	12,849	12,800

Notes for Figure 4: Absolute frequency of plants within 0.5 GWh bins of electricity use in the years 2008 and 2009. Source: AFiD Panel, own calculations. *Notes for Table 2:* 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. Source: AFiD Panel, own calculations.

continuous around the threshold value. Otherwise, we would anticipate bunching with more plants to the right of the threshold. As shown by Figure 4, we do not find any signs of bunching in the years 2008 and 2009 that determine treatment eligibility for the post crisis years 2010 and 2011, respectively. This finding is supported by formal tests for a discontinuity in the density function at the threshold, as proposed by McCrary (2008). The test statistics from Table 2 demonstrate that we cannot reject the null hypothesis of a continuously distributed running variable for these years. Hence, we find no evidence that plants strategically manipulated their electricity consumption in 2008 and 2009 to become eligible for the REL exemptions in 2010 and 2011.

The financial crisis was short-lived in Germany and led to a quick rebound of economic activity by 2010, when gross value added in manufacturing increased by 18%. In line with our prior, in that year we find evidence that some plants might have increased their electricity consumption to become eligible for the exemption in 2012 (Table 2). For later years, 2011 and 2012, we do not detect any signs of strategic manipulations of electricity use. This is in line with the change in exemption rules that was announced in the summer of 2011. Under the reformed schedule, plants did no longer face incentives to select above the 10 GWh eligibility threshold in 2011 and 2012 to become eligible two years later. The fact that we observe bunching in the year 2010 does not speak against the validity of our empirical strategy as it occurs in an outcome year rather than baseline year.

4.2. Main results

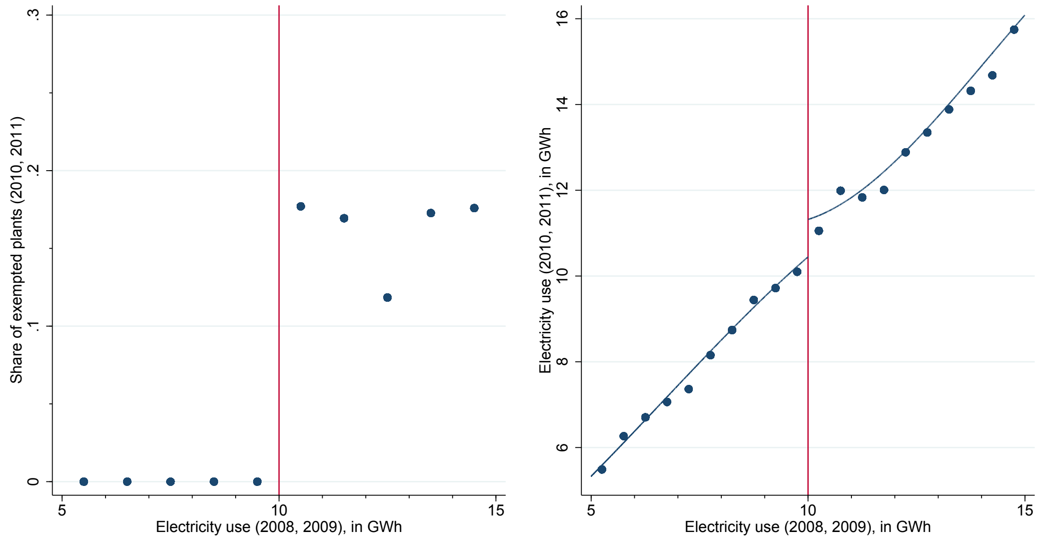
We turn to the estimation of treatment effects for all outcome variables next. To improve the precision of the fuzzy RD estimates, our preferred specification excludes all firms with an *energy* cost to gross value added ratio below 15%. These firms certainly do not meet the second eligibility criterion that the ratio of *electricity* cost to gross value added had to be at least 15% under the original exemption policy. Furthermore, 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. We provide robustness checks concerning the treatment of outliers in Section 4.3.

Figure 5 presents graphical evidence on the effect of the REL exemption on electricity use. The left panel of the figure shows that the share of exempted plants, i.e. the treatment probability, jumps from 0 to about 18% when electricity consumption in the baseline period (2008 and 2009) crosses the eligibility threshold of 10 GWh. All plants above the threshold that are not exempted either did not satisfy the second eligibility criterion in terms of the ratio of electricity cost to gross value added or did not apply for the exemption.

The right panel of Figure 5 plots the electricity consumption in the years of an exemption against the electricity consumption in the baseline period that determines eligibility, superimposing fitted lines from third order polynomials. The figure shows that plants that slightly exceed the eligibility threshold in the baseline period consume more electricity than those slightly below that threshold two years later. As plants above and below the threshold have virtually identical characteristics, and only differ in their probability of receiving the exemption, this finding indicates that REL exemptions increase plants' electricity use.

The fuzzy RD estimates in Table 3 show that REL exemptions increased electricity consumption on average by approximately 4 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 by about 40% in 2010 and 2011.

Figure 5: Exemption Shares in 2010 and 2011 (left panel) and Electricity Consumption in 2010 and 2011 (right panel)



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. 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.

This magnitude is consistent with estimates of relative treatment effect in a log specification, where we find an average treatment effect of about 50% for electricity consumption.

To investigate the channels that underlie the large increase in electricity use, we test whether plants reduced their consumption of other fuels, which could explain part of the large observed increase in electricity consumption. These results are shown in Panel A of Table 3. We do not find direct evidence of fuel switching, as shown by the positive, 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

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

	ATT^{RD} (1)	Standard errors (2)	n (3)
Panel A: *Electricity & fuel usage*			
Electricity consumption [GWh]	4.037**	1.756	33,032
Log electricity consumption	0.526*	0.301	33,407
Log fossil fuel consumption	0.086	0.500	29,945
Share of total energy mix:			
Electricity [%]	0.187	0.122	33,102
Fossil fuel [%]	−0.232**	0.118	33,077
Panel B: *CO2 emissions*			
Log CO_2, direct	0.175	0.506	29,960
Log CO_2, total	0.685*	0.377	33,268
Panel C: *Competitiveness indicators*			
Log employment	0.153	0.181	32,639
Log sales	0.394	0.299	34,119
Export share	−0.137*	0.081	33,957
Indicator variable for investment	−0.166	0.206	35,861
Indicator variable for investment in machinery	−0.130	0.183	35,861
Log investment	0.847	1.178	25,736

Notes: Observations from non-eligible firms with an energy cost share below 15% in 2008 and 2009 are excluded from the analysis. Each line represents a separate estimation, based on the MSE-optimal bandwidth selector (Calonico et al., 2019). The number of effective observations is approximately 1,400 for electricity and fossil fuel consumption. Standard errors clustered at the firm level. * p<.1, ** p<.05, and *** p<.01. Source: AFiD Panel, own calculations.

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 69%, 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).[10]

Our main estimates imply an own price elasticity of electricity of about negative 1.9-2.5, which is large compared to other studies that aim at short-run elasticities in the manufacturing industry.[11] One reason for such a large response is that REL exemptions under a notched

[10] 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.

[11] Much of this literature relies on sectoral data and uses time-series variation in energy prices for identification, finding price elasticities of about −0.15 to −0.2 (see for instance Bernstein and Madlener, 2015; Hyland and

design can work as a 'subsidy for bunching', as discussed in Section 2.2. 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.

For sales and employment, we estimate positive, yet statistically insignificant effects, 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 do not find evidence that the international competitiveness improved for exempted plants. Rather, we find a negative and statistically significant effect on the export share, which, however, can be partially explained by an increase in national sales. 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. We conduct robustness checks of our main findings and the identifying assumptions in the next subsection.

4.3. Robustness

To investigate the validity of our findings, 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 could 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 Table 4 shows, the point estimates for electricity and fuel variables remain comparable to

Haller, 2018; Neenan and Eom, 2008; Paul et al., 2009). On the other hand, (Martin et al., 2014b) estimate elasticities of a similar magnitude compared to our study when focusing on the introduction of the climate change levy in the UK relying on detailed firm level data.

the main results. However, the estimates lose their statistical significance, which is likely due to the smaller sample size.[12]

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. The second panel of Table 4 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).

In Online Appendix C.1, we further show that our findings are robust to the choice of the bandwidth used and potential outliers in the estimation. For a variety of outcome variables, we report estimates for different percent of the MSE-optimal bandwidth and show that they remain largely unchanged, except when bandwidths are very small or large. Moreover, we test for the exclusion of potential outliers (Figure C.1). Compared to our main results, all point estimates show the same sign and similar magnitudes.

5. REL exemptions under the revised policy design

In a subsequent 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 evaluate 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.

[12]The number of effective observations, i.e. of observations within the bandwidth used for the fuzzy RD estimation, drops from 1,411 to 953.

Table 4: Test of Identifying Assumptions: SUTVA and Local Randomization

	SUTVA			Pre-determined outcomes		
	ATT^{RD} (1)	Std. Er. (2)	n (3)	ATT^{RD} (4)	Std. Er. (5)	n (6)
***Panel A:** Electricity & fuel usage*						
Electricity consumption [GWh]	3.851	6.329	27,868	-	-	-
Log electricity consumption	0.468	0.387	27,868	0.0001	0.0001	34,250
Log fossil fuel consumption	−0.123	0.916	25,447	0.204	0.426	31,034
Share of total energy mix:						
Electricity [%]	0.018	0.113	27,868	0.004	0.080	33,945
Fossil fuel [%]	−0.161	0.114	27,868	−0.070	0.083	33,920
***Panel B:** CO2 emissions*						
Log CO_2, direct	0.061	0.998	25,456	0.382	0.426	31,065
Log CO_2, total	0.677	0.508	27,874	0.357	0.269	34,116
***Panel C:** Competitiveness indicators*						
Log employment	−0.114	0.345	28,671	−0.110	0.093	33,152
Log sales	0.199	0.474	28,980	−0.337	0.214	34,525
Export share	−0.076	0.101	28,980	−0.079	0.050	34,363
Indicator variable for investment	−0.192	0.269	29,575	−0.063	0.129	35,861
Indicator for investment in machinery	−0.266	0.243	29,575	−0.162	0.171	35,861
Log investment	1.391	1.539	22,006	0.008	0.817	27,004

Notes: Observations from non-eligible firms with an energy cost share below 15% in 2008 and 2009 are excluded from the analysis. Each line represents a separate estimation, based on the MSE-optimal bandwidth selector (Calonico et al., 2019). The effect on electricity use in Columns 4-6 is not defined as it corresponds to the running variable we use in the main RD specification. Standard errors clustered at the firm level. * p<.1, ** p<.05, and *** p<.01. Source: AFiD Panel, own calculations.

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:

$$ATT^{DiD} = \frac{1}{N_1} \sum_{i \epsilon I_1} \left\{ (Y_{it}(1) - Y_{i0}(0)) - \sum_{k \epsilon I_0} W_{N_0,N_1}(i,k)(Y_{kt}(0) - Y_{k0}(0)) \right\}, \tag{3}$$

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 A.4). 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 energy intensive 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 exclude observations with an *energy* cost to gross value added ratio of less than 14% as these plants cannot pass the second eligibility criterion, which requires that the *electricity* cost to gross value added ratio is at least 14%. Our data trimming keeps all newly eligible plants, while ensuring that control plants have a similar size and energy intensity. 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. We condition on the following predetermined variables that directly influence the treatment status and plants' potential outcomes in 2013: (log of) electricity consumption, sales, employment, wages, and export share. To allow for a flexible relationship, our main propensity score specification includes both linear and quadratic terms of these variables. In addition, we exploit the longitudinal structure of our data by including sector fixed effects and lagged values for electricity use for up to three years prior to 2011, which helps us to match treated and control plants that share a similar economic history.

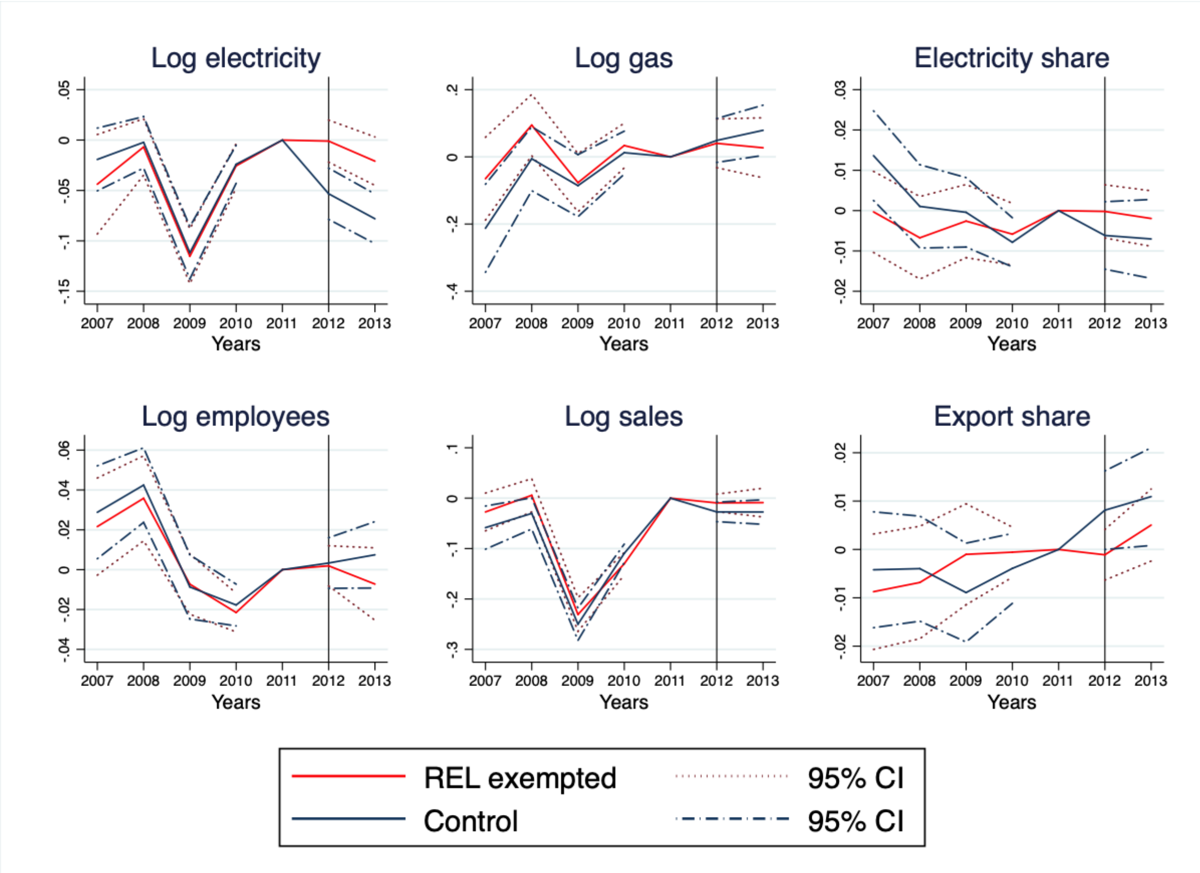
For matching, we use different algorithms based on nearest neighbor (NN) matching, NN matching with caliper and replacement, and one-to-many matching with caliper and replacement. Using caliper matching ensures that the characteristics of all nearest neighbors are close to those of the treated plants. 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 (2019).

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 rich set of covariates is possible. While untestable in principle, the assumption is more plausible if outcome trends are parallel prior to the policy intervention. For the years 2007 to 2013, Figure 6 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 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 and levels for the treatment and control group prior to 2011 (for details, see Appendix Table A.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 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.

Figure 6: 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. The vertical line indicates the reform in 2012. Standard deviations not defined for 2011.

5.2. Main results

Table 5 presents the results for the ATT^{DiD}, using the main propensity score specification and three different matching algorithms. Column 1 reports the ATT^{DiD} from one-to-one nearest neighbor (NN) matching, Column 3 from NN matching with caliper and replacement, and Column 5 from one-to-many matching with caliper and replacement, where we allow for up to 20 matched control plants for each treated plant. We calculate standard errors based on post-matching inference (Abadie and Spiess, 2019) for NN matching without replacement, and robust standard errors following Abadie and Imbens (2006) otherwise.[13] We express our out-

[13]In the matching literature, there exists a clear trade-off between bias and efficiency of the estimates. While the inference in Column 2 is correct, we need to rely on a smaller pool of control plants. The estimates in Columns 3 to 6 therefore provide important robustness checks.

Table 5: Results Matching DiD Estimates

Matching algorithm	1:1		1:1 caliper		1:20 caliper	
	ATT^{DiD}	SE	ATT^{DiD}	SE	ATT^{DiD}	SE
Δ 2013-2011	(1)	(2)	(3)	(4)	(5)	(6)
***Panel A:** Electricity & fuel usage*						
Log electricity consumption	0.057***	0.018	0.067**	0.029	0.051***	0.016
Log fossil fuel consumption	−0.039	0.046	0.001	0.049	−0.046	0.039
Share of total energy mix:						
Electricity [%]	0.005	0.006	0.003	0.008	0.010*	0.006
Fossil fuel [%]	−0.005	0.006	−0.007	0.007	−0.007	0.006
***Panel B:** CO2 emissions*						
Log CO_2, direct	−0.026	0.046	0.02	0.05	−0.042	0.039
Log CO_2, total	0.053***	0.018	0.07**	0.027	0.039**	0.015
***Panel C:** Competitiveness indicators*						
Log employment	−0.015	0.013	−0.011	0.017	−0.013	0.012
Log sales	0.018	0.020	0.036	0.022	0.019	0.017
Export share	−0.006	0.006	−0.009	0.012	−0.008	0.008
Indicator variable for investment	−0.011	0.021	0.004	0.028	−0.023	0.02
Indicator for investment in machinery	−0.011	0.017	−0.035	0.022	−0.009	0.017
Log investment	0.028	0.129	−0.012	0.139	−0.003	0.105
# of observations	916		770		2,359	
# of treated plants	458		458		458	

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), NN matching with caliper and replacement (Columns 3 and 4), and one-to-many matching with caliper and replacement (Columns 5 and 6) following Specification (3). Inference in Column 2 follows Abadie and Spiess (2019). Robust standard errors Abadie and Imbens (2006) in Columns 4 and 6. * p<.1, ** p<.05, and *** p<.01.

come variables as differences between the treatment year (2013) and the year that determines treatment eligibility (2011).[14]

Panel A shows estimates that the REL exemption under the reformed policy schedule led to an increase in electricity consumption by about 5.1 – 6.7%. This effect is very similar across specifications. For an average 'treated plant' in this group, with 5.5 GWh electricity consumption in 2013, the levy exemption represents an reduction of about 23% in electricity input costs.[15] Hence, our estimates imply a short-run price elasticity for electricity in the range between −0.22 and −0.29. In addition, we do not find evidence that plants change their consumption of fossil fuels. Our point estimates are mostly negative, but not statistically different from zero.

In Panel B, we focus on changes in CO_2 emissions. Our estimates suggest that the average total fuel emissions increased by approximately 4-7%, representing a carbon elasticity of −0.17

[14]When using the difference in levels as the outcome variable, we obtain an ATT^{DiD} of 0.249 GWh for the 1:1 matching algorithm, which is statistically significant at the 1% level. Dividing this value by the average electricity consumption of exempted plants (5.5 GWh), we obtain a very similar treatment effect of 4.5%.

[15]We obtain these value assuming a plant faces average electricity prices as presented in Figure 1, full payment for the REL for the 1 GWh of consumption and 10% of the REL for the remaining consumption.

Table 6: Test of Identifying Assumptions: SUTVA and Anticipation

	SUTVA		Anticipation	
	ATT^{DiD} (1)	SE (2)	ATT^{DiD} (3)	SE (4)
Panel A: *Electricity & fuel usage*				
Log electricity consumption	0.038*	0.021	0.042**	0.016
Log fossil fuel consumption	−0.013	0.058	−0.077*	0.043
Share of total energy mix:				
Electricity [%]	0.017**	0.008	0.016***	0.006
Fossil fuel [%]	−0.007	0.006	−0.012**	0.005
Panel B: *CO2 emissions*				
Log CO_2, direct	0.002	0.059	−0.074*	0.043
Log CO_2, total	0.022	0.021	0.03*	0.016
Panel C: *Competitiveness indicators*				
Log employment	−0.004	0.013	−0.018	0.011
Log sales	0.036	0.023	0.009	0.019
Export share	−0.001	0.007	−0.003	0.006
Indicator variable for investment	0.022	0.022	−0.011	0.02
Indicator for investment in machinery	0.025	0.021	0.002	0.016
Log investment	−0.006	0.158	−0.059	0.123
# of observations	644		908	
# of treated plants	322		454	

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 and Columns 3 and 4 condition the propensity score on 2010, the pre-annoucement year of the policy reform. Robust standard errors following Abadie and Spiess (2019). * p<.1, ** p<.05, and *** p<.01.

to −0.3. Direct CO_2 emissions, on the other hand, show a negative sign in most specifications in line with fuel substitution, yet these estimates are not statistically significant. Taken together, both finding shows that the increase in CO_2 emissions is again driven by an increase in electricity consumption in response to the exemptions.

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 on the grounds of improving the competitiveness of the industry.

Robustness

We next 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 Table 6). As the REL reform benefitted 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.[16] The point estimates presented in Column 1 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 year 2010 (Column 3 of Table 6). Plants that knew about the policy change in 2011 may have anticipated future exemptions and adjusted their production in that year already. To test for this possibility, we match the treatment and control group based on the pre-announcement year 2010, when plants were not yet informed about the reform. Again, we detect an increase in electricity consumption, which also implies higher total carbon emissions that can be attributed to a plant. With this specification, we find some evidence of reduced fossil fuel consumption, as shown by the negative estimates on fossil fuel consumption and the share of fossil fuels in total energy, which are statistically significant at the 5 and 10% level, respectively. Again, we do not find evidence that the REL exemptions had an impact on employment, sales, and investment.

In Online Appendix C.2, we show that our main results are robust to further sample selection and the choice of specification for the propensity score. Appendix Table C.4 presents the main treatment effect for a propensity score specification that matches strictly within economic sub-sectors.[17] In line with the main results, we find an increase of 4.6 to 7.7% in electricity consumption. We also detect a small negative impact on total employment and fossil fuel consumption of about 2% and 8%, yet this effect is only significant at the 10% level in some of our specifications.

6. Discussion of effect sizes

Qualitatively, both of our empirical evaluations in Sections 4 and 5 yield very similar results. We find that the REL exemption induces plants to increase their electricity consumption, while we do not find any significant impacts on competitiveness indicators. Yet, quantitatively, the

[16] We observe a total of 322 single-plant firms (about 70% of our sample) that have been newly exempted in 2013 (see also Appendix Table A.2).

[17] To guarantee convergence in the propensity score estimation, we group manufacturing sectors into five sub-sectors according to their average energy intensity (for details, see Table C.3 in the Appendix). Evidence on parallel pre-treatment trends is presented in Figure C.3 in the Appendix.

Table 7: Decomposition of Effect Sizes

Policy Regime (Years) (1)	Estimand (2)	Estimated Term from Model (3)	ATT (4)	ΔPrice (5)	Elasticity (6)
Notch (2010, 2011)	ATT^{RD} (10 GWh baseline use)	(1): $MPR + BR$	40.4%	21.1%	-1.92
No Notch (2013)	ATT^{DiD} (1-10 GWh baseline use)	(2): MPR	5.7%	31.4%	-0.18
		(1)−(2) = BR	34.7%		-1.73

Notes: Assuming that the average marginal price response (MPR) is the same for plants in both of our empirical evaluations, we can identify the average bunching response (BR) as the difference between the ATT^{RD} under the notched policy design (2010 and 2011) and the ATT^{DiD} estimated after the policy reform (2013). In Column 6, we take into account the change in the magnitude of the tax exemption over time and express the BR in terms of elasticities (see Appendix 1 for details).

effect sizes differ substantially, in particular with respect to the increase in plants' electricity use. In this section, we use the model introduced in Section 2.3, to recover the marginal price response (MPR) and the net bunching response (BR) from our empirical estimates.

Our RD design allows us to identify the treatment effect for plants close to the 10 GWh threshold, denoted by ATT^{RD}. As this treatment effect was estimated under a notched exemption policy, it captures both the marginal prices response and the net bunching response. Hence, we have $ATT^{RD} = MPR + BR$. After the policy change, the tax notch was largely removed, so that the ATT estimate from our DiD estimation, ATT^{DiD}, captures the marginal price response ($ATT^{DiD} = MPR$). Under the assumption that the MPR would have been the same in our fuzzy RD setting, we can infer the magnitude of the BR by taking the difference of both estimates, $BR = ATT^{RD} - ATT^{DiD}$. For ATT^{RD}, we use the conservative estimate of 40.4% (.404 GWh / 10 GWh), which is smaller than the result from the log specification. Our decomposition shows that the component of ATT^{RD} that can be attributed to a bunching response amounts to some 35 percentage points and thus accounts for 86% of the ATT^{RD} (see Column 4 of Table 7).

A first caveat concerning our decomposition is that the REL increased over time, which also led to an increase in the magnitude of an exemption, from about 21% of electricity prices in 2010/2011 to 31% in 2013 (see Column 5). A simple extrapolation of our MPR estimate from 2010/2011 to 2013 does not take into account such changes. To tackle this issue, we conduct our decomposition in terms of elasticities in Column 6. In Appendix B, we show that we can similarly decompose firms' input elasticity η^{RD} into $\eta^{RD} = \eta_{MPR} + \eta_{BR}$, where η_{MPR} denotes the price elasticity of input consumption in absence of a notch and η_{BR} denotes the component of the elasticity that can be attributed to the net bunching response. Again, we find that the net bunching response expressed as an elasticity (η_{BR}) amounts to -1.73. These extrapolations show that exemptions under a notched policy designs can induce plants to adjust their production inputs above a level that would have been chosen in the absence of the notch.

A second caveat is that our fuzzy RD and our DiD approach do not estimate treatment effects for the same group of plants. As all exempted plants need to pass the eligiblity criteria in our setting, our fuzzy RD and DiD approach both estimate an ATT (Battistin and Rettore, 2008). Yet, the fuzzy RD approach yields estimates for treated plants at the threshold with a baseline electricity use 10 GWh, while our DiD setting identifies the ATT for smaller plants with 1-10 GWh of electricity use.[18]

A third caveat relates to the precision of our estimates, which is lower for the fuzzy RD than for the DiD estimates. To explore the robustness of our findings, we make conservative assumptions on the magnitude of the effect size from our fuzzy RD approach. As a lower bound estimate of the effect, we use an intention-to-treat estimate that disregards the fuzzy nature of our RD design, where the probability of treatment jumps by 18 percentage points at the cutoff. This lower bound estimate implies an elasticity $\eta_{low}^{RD} = -1.92/(1/0.18) = 0.35$, which is still about twice as large as the elasticity implied by our ATT^{DiD} estimate.

7. Conclusion

This paper analyses the impact of a large electricity tax exemption scheme on German manufacturing. We examine how an exemption from the renewable energy levy (REL) affects the use of energy inputs and production outcomes of manufacturing plants under two policy designs. Our identifying strategy for evaluating the original, notched, tax exemption design exploits the fact that the financial crisis of 2008 and 2009 prevented plants from potentially manipulating their electricity use in order to reach eligibility for an exemption and identifies treatment effects via a fuzzy RD design. In addition, we employ a matching difference-in-differences approach to identify the effects of an exemption after a policy reform that eliminated the tax notch and substantially increased the group of eligible plants.

We find that, under the notched design, exempted plants increased their electricity use on average by 40-50% compared to the control group. By contrast, after the policy reform that largely eliminated the tax notch, we find that exempted plants increase their electricity use by

[18] Holding the estimation sample constant is not feasible as both approaches use different sources of identifying variation. To test whether smaller plants respond differently to an exemption, we exclude plants with 1-5 GWh of electricity use in the baseline year from the DiD estimation. Our estimates remain virtually unchanged (see Appendix Table C.2), which reduces concerns that differences in the estimation sample can explain the large difference in our ATT estimates.

about 5-7%. Based on a stylized model of production, we show that these differences can be rationalized by the incentives introduced by the notched tax design. When the eligibility for future exemptions depends on current input use and exemptions imply infra-marginal benefits, a reduction in the price for an input makes it profitable for more firms to increase their input use in order to reach eligibility for an exemption in subsequent years. The fact that exemptions can work as a 'subsidy for bunching' under a notched tax design highlights how policy design can co-determine firm production choices. In particular, our results show that notched tax exemption policies lead to unintended consequences by distorting production input choices.

We do not detect a statistically significant impact of the REL exemption on competitiveness indicators such as sales, export shares, employment, and investment. 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 of about 4 billion Euro per year (BAFA, 2013). Reducing the REL exemptions or tightening the eligibility criteria could avoid these costs and the associated distributional implications (for distributional implications of other climate policies, see e.g. Reguant 2019). Our evidence implies that such adjustments would not result in sizable reductions in production levels or employment, at least in the short run.

Our estimates exploit variation in exemption status to identify short-run effects on production in the year of the exemption. The dynamic nature of the REL exemption scheme complicates the causal evaluation of long-term effects, which we leave to future research. 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 certainly 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.

More broadly, our paper offers insights on the effectiveness of widely used exemption policies for energy-intensive and trade-exposed industries. Our findings highlight that defining eligibility criteria for exemption policies in terms of production inputs is problematic and that exemptions 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.

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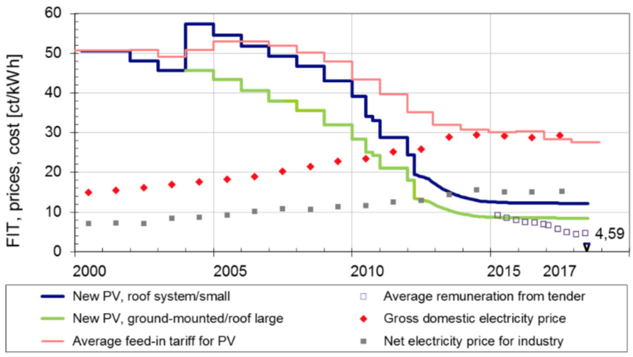
A. Appendix

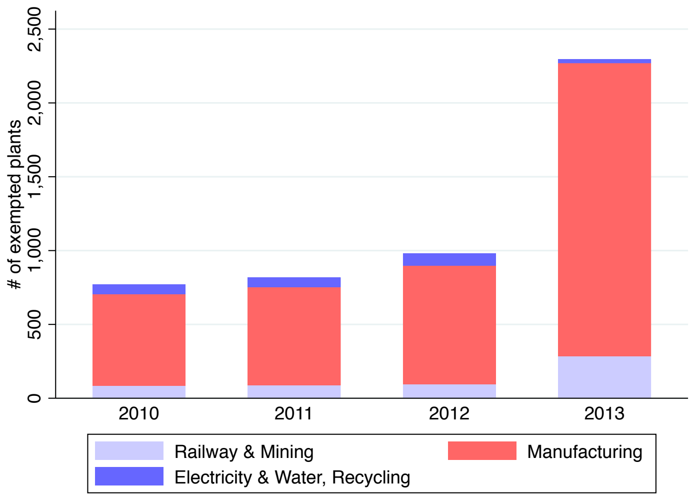
A.1. Additional Tables and Figures

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).

Table A.1: 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.2: 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).

Robustness: Fussy RD Design

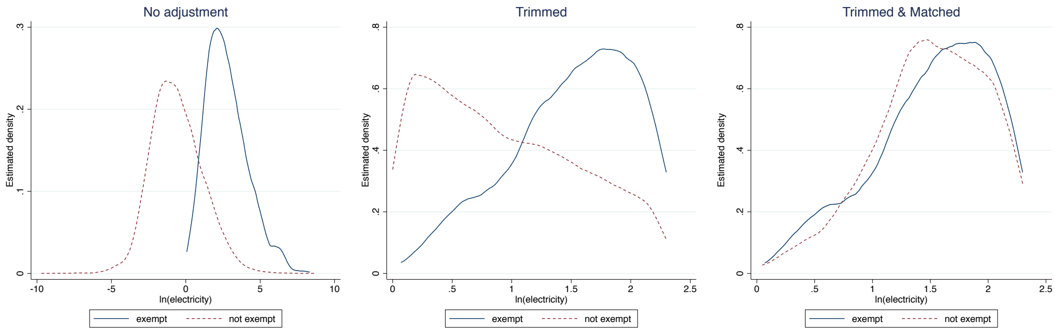
Table A.3: Fuzzy RD Estimates, Including Own Electricity Generation

	ATT^{RD}	Standard errors	n
Panel A: *Electricity & fuel usage*			
Electricity consumption [GWh]	7.134*	4.009	39,937
Log electricity consumption	0.709	0.474	40,475
Log fossil fuel consumption	−0.161	1.822	35,761
Share of total energy mix:			
Electricity [%]	0.293	0.321	40,056
Fossil fuel [%]	−0.267	0.335	40,041
Panel B: *CO2 emissions*			
Log CO_2, direct	−0.128	1.761	35,788
Log CO_2, total	0.953	0.743	40,292
Panel C: *Competitiveness indicators*			
Log employment	−1.657**	0.692	38,638
Log sales	−0.677	0.887	40,161
Export share	−0.453*	0.263	39,999
Indicator variable for investment	−0.464	0.298	41,821
Indicator for investment in machinery	−0.098	0.269	41,821
Log investment	0.146	1.447	32,922

Notes: Observations from non-eligible firms with an energy cost share below 15% in 2008 and 2009 are excluded from the analysis. Each line 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.

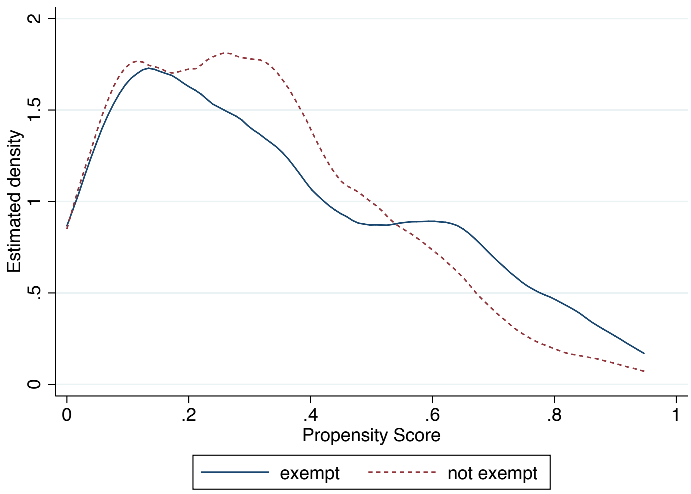
Robustness: Matching DiD

Figure A.3: 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).

Figure A.4: Overlap - Propensity Score



Notes: Overlap of the propensity score following our main matching specification and using nearest neighbor matching without replacement.

Table A.4: Propensity Score - Logistic Regression (2011)

Exempted 2013	Beta	Std. Error
Log electricity	2.785***	(0.579)
Log sales	-0.49**	(0.247)
Log employees	-0.118	(0.991)
Export share	-0.738	(0.717)
Log wages	3.102	(3.211)
Log employees × log employees	-0.26**	(0.118)
Log sales × log sales	0.004	(0.044)
Log electricity × log electricity	-0.433**	(0.182)
Export share × export share	0.229	(0.91)
Log wages × log wages	-0.703	(0.473)
Log electricity 2010	0.464	(0.326)
Log electricity 2009	0.324	(0.287)
Log electricity 2008	0.793***	(0.263)
Log electricity 2007	0.132	(0.159)
Sub-sector FE	Y	
Observations	8,522	
Pseudo R^2	0.422	

Notes: Dependent variable: REL exempted in 2013. Logistic regression. Sample trimmed to plants with 1-10 GWh electricity consumption in 2011. Explanatory variables measured in the year 2011, determining treatment status in addition to lagged electricity. The regression includes two-digit sub-sector fixed effects (20 manufacturing sub-sectors in total). Standard errors reported in parentheses. * p<0.1, **p<0.05, and ***p<0.01.

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

	Beta	SEM	P-value
Levels 2011			
Electricity	0.050	0.034	0.147
Sales	0.006	0.061	0.923
Employment	-0.033	0.040	0.400
Export share	-0.008	0.016	0.596
Wage	0.006	0.018	0.746
Electricity share	0.023	0.020	0.262
Total energy	-0.106	0.136	0.435
Gas	-0.091	0.165	0.582
Δ *2011-2010*			
Electricity	0.002	0.015	0.916
Sales	0.021	0.015	0.161
Employment	0.004	0.007	0.605
Export share	-0.003	0.005	0.460
Wage	-0.003	0.007	0.729
Electricity share	-0.002	0.005	0.688
Total energy	-0.011	0.036	0.755
Gas	-0.021	0.047	0.651
# of observations	916		

Notes: Test for parallel pre-treatment trends and level difference in key outcome variables for matched sample. Each line represents a separate regression to test for differences between the treatment and control group. Main coefficient (beta) and standard error of the mean (SEM) reported together with p-values. * $p<0.1$, **$p<0.05$, and ***$p<0.01$.

B. Appendix: Conceptual model for input tax notch

In this section, we describe how a tax exemption in the context of a notched tax schedule (as described in Section 2.3) affects production input choices.

Effect of a tax notch on input choices

Let us first consider the impact of the tax notch A^f on current 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 $A^f = 0$). The optimal inputs are implicitly defined by the two first order conditions for profit maximization, $\psi y_{\tilde{x}} = p + t$, and $y_z = q$, 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.

How does the presence of a notched tax schedule ($A^f > 0$) change the demand for x? Let us first define a "marginal buncher" as a firm with 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 become eligible for an exemption. This condition holds if $\pi(\psi^m \hat{x}, \bar{z}) = \pi(\psi^m x^c, z^c)$, or, equivalently:

$$y(\psi^m \hat{x}, \bar{z}) - q\bar{z}(\psi^m) - (p+t)\hat{x} + A^f = y(\psi^m x^c, z^c) - qz^c - (p+t)x^c,$$

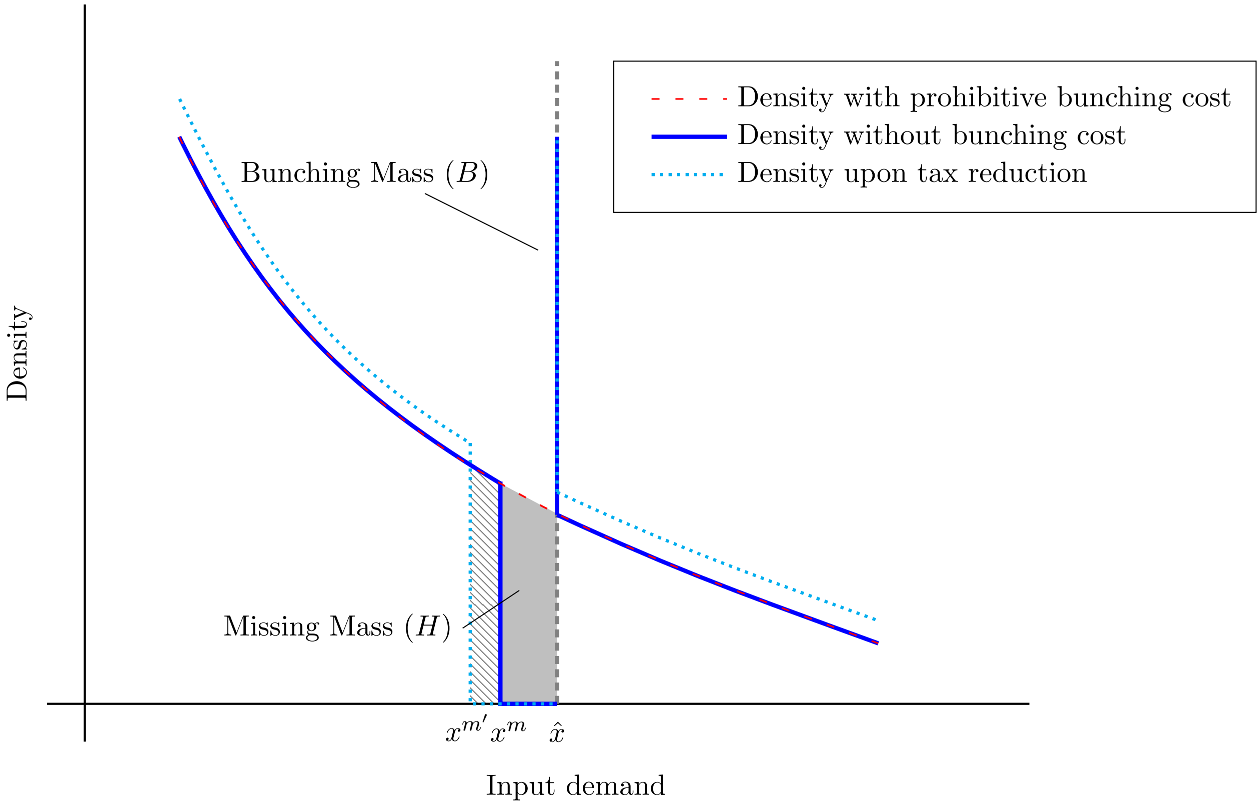
where $\bar{z}$ denotes a firm's profit maximizing choice of the input z, conditional on bunching to the notch threshold ($x = \hat{x}$). We can now determine the profit maximizing demand for the taxed input under the notched schedule:

$$x^*(\psi^m) = \begin{cases} x^c & \text{if } \underline{\psi} \leq \psi < \psi^m \text{ or } \psi^{\hat{x}} \leq \psi < \bar{\psi} \\ \hat{x} & \text{if } \psi^m \leq \psi < \psi^{\hat{x}}, \end{cases} \tag{4}$$

where $\psi^{\hat{x}}$ denotes the productivity of a plant that chooses x^c exactly equal to the threshold value, i.e. $x^c(\psi^{\hat{x}}) = \hat{x}$. For simplicity, let x^m be the quantity that the marginal buncher would use as an input, i.e. $x^m = x^*(\psi^m)$.

Figure B.1 illustrates how the presence of the tax notch changes the distribution of observable input choices x^*. For firms with sufficiently low productivities ($\psi < \psi^m$ or, equivalently, firms that choose $x^* < x^m$) 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 outweight the gains from obtaining A^f. For

Figure B.1: Bunching in stylized model



Notes: The figure plots the distribution of input demand x in the absence of an tax notch, its presence, as well as the distribution when a notch is present and the tax rate is reduced. The notch threshold is denoted by $\hat{x}$ and the input use of the marginal buncher before and after the tax reduction is given by x^m and $x^{m'}$, respectively.

more productive firms ($\psi^m \leq \psi < \psi^{\hat{x}}$ or, equivalently, $x^m \leq x^* < \hat{x}$), bunching is profitable. Accordingly, these firms will increase their electricity to the threshold, which leads to bunching at $\hat{x}$ and missing mass in the interval $x^m \leq x^* < \hat{x}$.

Effect of a tax exemption on input use in the presence of a tax notch

We now 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 $d\psi = \psi^{m'} - \psi^m$, where $\psi^{m'}$ denotes the productivity of the marginal buncher after the tax change.

We now determine the first derivative of $x^* = \int_0^\infty x^*(\psi)dG(\psi)$ with respect to a reduction in the tax rate t by t^{ex}. Using Equation (4), we take the first derivative of $x^*(\psi)$ with respect to t^{ex} for every ψ. We then integrate $\partial x^*(\psi)/\partial t^{ex}$ over the entire support of $G(\psi)$ and rearrange terms, which yields Equation 1 highlighted in the main text.

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

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 component 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 B.1 for the interval $x < x^{m'}$, for example. With a tax reduction, the first term is always positive, as $\partial x^c/\partial t < 0$, 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. In Figure B.1, this effect is illustrated by the change in the input of the "new" bunchers with input demand between x^m and $x^{m'}$ that bunch only after the tax reduction, as well as the (counterfactual) effect on firms with input consumption between x^m and $\hat{x}$ that also bunched prior to the tax change. The intuition of the BR in response to a tax exemption is as follows. A tax exemption increases the number of bunching firms ($\psi^{m'} > \psi^m$), yielding a second term that is positive. The third term reflects the counterfactual change in electricity consumption of all bunchers in response to a tax reduction, which is always positive. The BR can be calculated as the difference between the two and thus gives a net effect, which is only positive when the additional input use of new bunchers exceeds the counterfactual change of all bunchers in the absence of a notch.

When we log-transform the input variable x, we obtain:

$$\eta = \eta_{\text{MPR}} + \eta_{BR},$$

where $\eta = x^* = \int_0^\infty (\partial x^*/\partial t) \cdot (t/x^*) dG(\psi)$ denotes the average elasticity of the input use x relative to a tax change, $\eta_{\text{MPR}} = \int_0^\infty (\partial x^c/\partial t)/(t/x^c) g(\psi) d\psi$ denotes the average price elasticity of input in the absence of a notch, and $\eta_{BR} = \int_{\psi^{m'}}^{\psi^m} (log(\hat{x}) - log(x^c))\, tg(\psi) d\psi - \int_{\psi^{m'}}^{\psi^{\hat{x}}} (\partial x^c/\partial t)(t/x^c) g(\psi) d\psi$ gives the BR.

C. Appendix (for online publication)

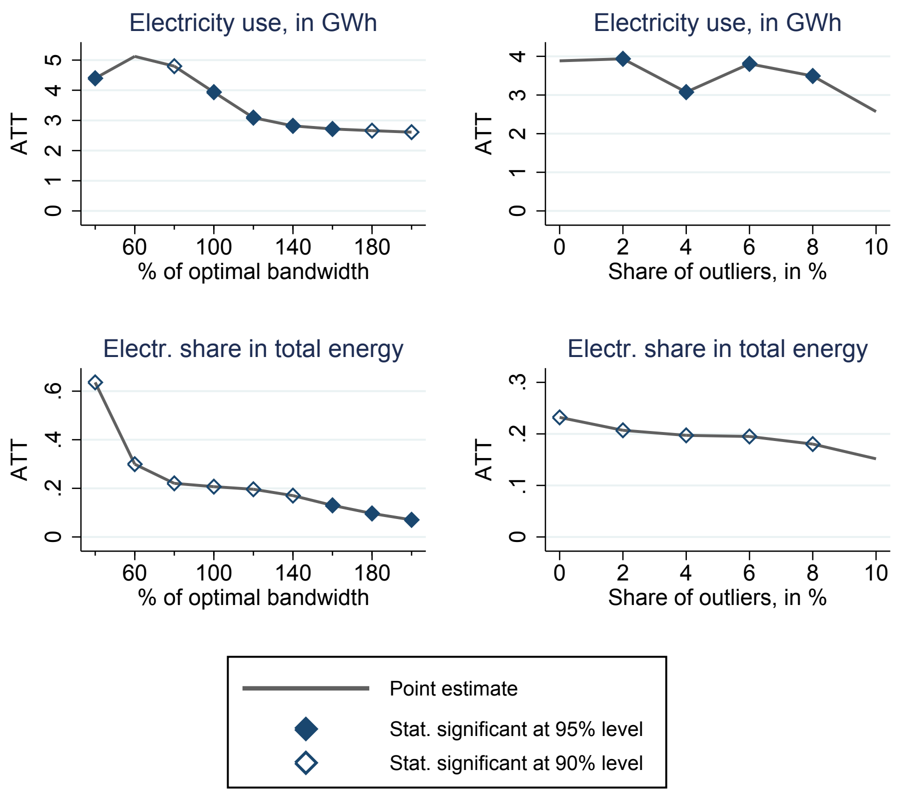
C.1. Additional Robustness: RD Design

This section provides additional robustness checks regarding the treatment of outliers, functional form assumptions, and choice of bandwidths. First, we focus on the bandwidth choice, which is characterized by a compromise between precision and bias (Lee and Lemieux, 2010). Larger bandwidths increase the number of observations in local linear regressions, for example, but can also increase bias due to a worse approximation of the conditional mean of the outcome variable. To ensure that the results are not due to a specific bandwidth choice, the left panel of Figure C.1 depicts the ATT^{RD} estimates for a variety of bandwidths ranging from 40 to 200% of the MSE-optimal bandwidth choice used in the main section, highlighting statistically significant ATT^{RD} by diamonds. We focus on the main electricity variables in this section. The figure illustrates that the estimates are robust to specific bandwidth choices and remain statistically significant at the 10% level throughout.

Next, we investigate whether the results are robust to different ways of excluding outliers. The right panel of Figure C.1 displays the ATT^{RD} estimates for a variety of excluded percentiles, ranging from 0%, i. e. no outlier removed, to excluding the 10% of the sample with the largest relative changes in electricity use between the period that determines eligibility and the outcome period two years later. It demonstrates that the size of the estimates remains virtually unchanged and that statistical significance is only lost for the ATT^{RD} estimate on electricity use in the extreme case of no outlier removal. Yet, the point estimates are highly aligned.

Another concern might be that the fit from local linear regressions is poor due to some curvature in the conditional mean that could better be captured by more flexible polynomial functions. We investigate this issue by estimating local quadratic regressions, using the bandwidth of the preferred specification to facilitate comparability. Table C.1 shows that the ATT^{RD} point estimates are very similar to the estimates from the main specification. Only their precision deteriorates, which is not surprising as additional parameters need to be estimated and degrees of freedom are lost.

Figure C.1: Treatment Effects (ATT^{RD}) under Different Bandwidths (Left Panel) and Alternative Outlier Definitions (Right Panel)



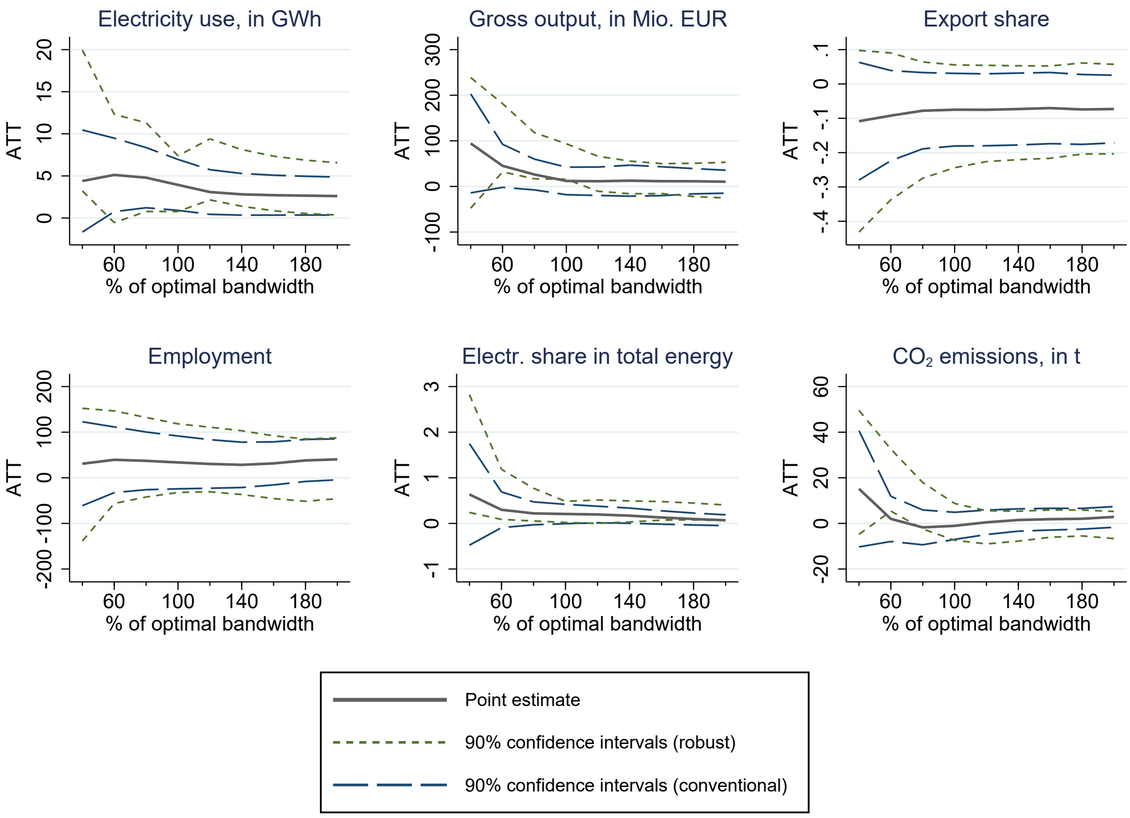
Notes: Optimal bandwidths correspond to the bandwidths from the footnote of Table 3. The share of outliers gives the percentage of observations that have been removed. For instance, a 2% share of outliers correspond to a deletion of the top and bottom 1% with regard to relative electricity use changes between the outcome year t and $t-2$. Source: AFiD Panel, own calculations.

Table C.1: Robustness Fuzzy RD Design

	Second order polynomials		
	ATT^{RD}	Std. Err.	# of Obs.
Electricity use, in GWh	6.54	4.65	36,834
Effects on the fuel mix			
Electricity share in total energy	0.34*	0.38	36,834
Oil share in total energy	-0.12*	0.08	36,834
CO_2 intensity of energy use	215.11*	236.64	36,843

Notes: Standard errors are clustered at the firm level. **,* denote statistical significance at the 5%, 10% level, respectively. The MSE-optimal bandwidth selector (Calonico et al., 2019) yields the following bandwidths: 2.62, 1.53, 4.16, 1.77 GWh (in order of appearance).

Figure C.2: Fuzzy RD Estimates for Varying Bandwidths



Notes: Confidence intervals are calculated as proposed by Calonico et al. (2014). Optimal bandwidths for the fuzzy RD analyses of all outcome variables are presented in the footnote of Table 3. Source: AFiD Panel, own calculations.

C.2. Additional Robustness: Matching DiD

Table C.2: Robustness Matching DiD - Sample Selection

	Potential outliers removed		5-10 GWh electricity	
	ATT^{DiD} (1)	SE (2)	ATT^{DiD} (3)	SE (4)
Panel A: *Electricity & fuel usage*				
Log electricity consumption	0.028**	0.013	0.038*	0.022
Log fossil fuel consumption	-0.037	0.049	-0.05	0.07
Share of total energy mix:				
Electricity [%]	0.009	0.006	0.001	0.007
Fossil fuel [%]	-0.01	0.006	-0.005	0.007
Panel B: *CO2 emissions*				
Log CO_2, direct	-0.03	0.049	-0.041	0.07
Log CO_2, total	0.009	0.015	0.026	0.023
Panel C: *Competitiveness indicators*				
Log employment	-0.018	0.012	0.008	0.017
Log sales	0.009	0.017	0.012	0.033
Export share	-0.007	0.007	-0.007	0.01
Indicator variable for investment	-0.021	0.022	0.004	0.029
Indicator for investment in machinery	-0.002	0.017	0	0.026
Log investment	-0.077	0.124	0.026	0.163
# of observations	858		458	
# treated plants	429		229	

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: potential outliers removed in line with RD design. Columns 3 and 4: Sample limited to 5-10 GWh electricity consumption in base year 2011. Robust standard errors following Abadie and Spiess (2019). * p<.1, ** p<.05, and *** p<.01.

Alternative Propensity Score Definition

Table C.3: Propensity score: Strict Sub-sector Matching

Exempt 2013	Sub-sector 1	Sub-sector 2	Sub-sector 3	Sub-sector 4	Sub-sector 5
Electricity 2011	1.897**	3.141***	1.959*	1.514	1.626***
	(0.952)	(0.693)	(1.143)	(1.426)	(0.318)
Electricity 2010	0.15	0.282	1.287	0.698	0.817**
	(1.187)	(0.509)	(1.107)	(1.853)	(0.351)
Electricity 2009	0.079	-0.128	-0.238	-0.376	1.359***
	(1.306)	(0.481)	(0.769)	(1.424)	(0.433)
Electricity 2008	0.376	-0.144	0.358	1.33	0.317
	(1.079)	(0.226)	(0.87)	(0.835)	(0.376)
Sales	0.377**	-1.139***	-2.229***	-0.456*	-1.38***
	(0.164)	(0.206)	(0.349)	(0.246)	(0.153)
Employment	-2.46***	-1.422***	-0.736**	-2.542***	-2.063***
	(0.209)	(0.216)	(0.341)	(0.327)	(0.24)
Observations	1,419	1,881	973	867	4,069
Pseudo R^2	0.4	0.3	0.4	0.35	0.51

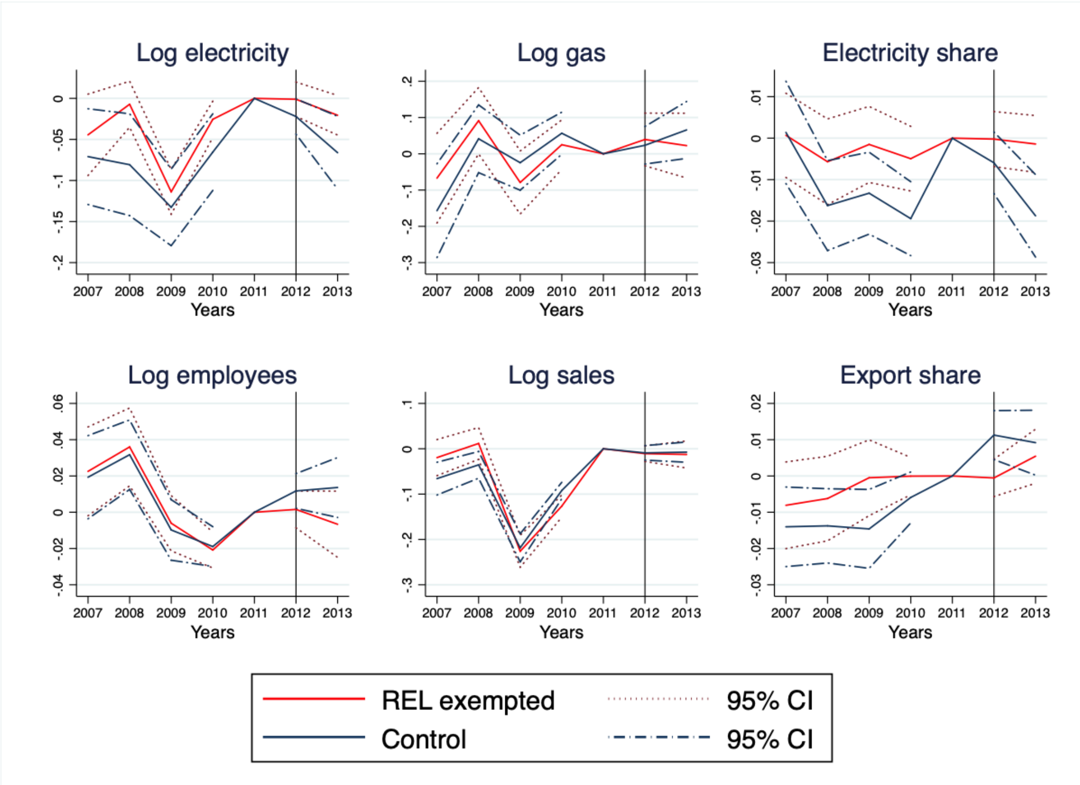
Notes: Main dependent variable: REL exempt 2013. Logit regression. Sample trimmed to plants with 1-10 GWh electricity consumption in 2011. All dependent variables refer to the base year, 2011. Unit of observation: plant. All variables are in logs, except for shares. Each column refers to a separate logit estimation of the propensity score (matches forced to be within same sub-sector). Sub-sectors defined according to the mean energy intensity (original WZ 2008 definition in parenthesis) sector 1: food (WZ 10,11), sector 2: chemicals & pharmaceuticals (WZ 19,20,21,22), sector 3: paper & cement (WZ 17,23), sector 4: metal, electrical equipment, machinery and cars (WZ 24,25,26,27,28,29,30,33), and sector 5: textiles, leather, wood processing and miscellaneous (WZ 13,14,15,16,18,31,32). BBGG algorithm, bootstrapped standard errors in parentheses. * p<0.1, **p<0.05, and ***p<0.01.

Table C.4: Robustness: Matching DiD - Strict Sub-sector Matching

Matching algorithm	1:1		1:1 caliper		1:20 caliper	
	ATT^{DiD} (1)	SE (2)	ATT^{DiD} (3)	SE (4)	ATT^{DiD} (5)	SE (6)
***Panel A:** Electricity & fuel usage*						
Log electricity consumption	0.046*	0.025	0.077**	0.031	0.046***	0.017
Log fossil fuel consumption	−0.042	0.042	−0.084*	0.051	−0.011	0.038
Share of total energy mix:						
Electricity [%]	0.017***	0.007	0.021***	0.008	0.007	0.005
Fossil fuel [%]	−0.009	0.006	−0.024***	0.008	−0.006	0.005
***Panel B:** CO2 emissions*						
Log CO_2, direct	−0.033	0.042	−0.08	0.05	0.003	0.039
Log CO_2, total	0.019	0.018	0.023	0.023	0.045***	0.015
***Panel C:** Competitiveness indicators*						
Log employment	−0.02*	0.012	−0.026*	0.014	−0.02*	0.011
Log sales	−0.004	0.019	−0.002	0.023	−0.01	0.018
Export share	−0.004	0.006	−0.011	0.009	−0.007	0.006
Indicator variable for investment	−0.013	0.021	−0.013	0.028	−0.003	0.02
Indicator for investment in machinery	0.009	0.017	−0.002	0.024	0.001	0.016
Log investment	−0.071	0.117	−0.137	0.158	−0.018	0.109
# of observations	912		765		2,245	
# treated plants	456		456		456	

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. Robust standard errors following Abadie and Spiess (2019). * p<.1, ** p<.05, and *** p<.01.

Figure C.3: Common trends: Strict Sub-sector Matching



Notes: Analysis of parallel pre-treatment trends for treated plants (REL exempted in 2013) and matched control plants based on nearest neighbor matching and relying on strict sector propensity score matching (Table C.3). The figure plots growth rate of the respective variables with respect to 2011, the year determining treatment status. The vertical line indicates the reform in 2012.