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DISCUSSION PAPER

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Do Climate Policies Lead To Outsourcing? Evidence From Firm-Level Imports

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Do Climate Policies Lead to Outsourcing? Evidence from Firm-Level Imports

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Abstract

Rising energy prices might lead to adjustments along the supply chain and make firms outsource energy-intensive processes. This could lead to carbon leakage. I provide empirical evidence whether energy price-induced offshoring occurs using firm-level data on energy use, imports, and material purchases. I document that import shares in German industry have increased between 2009 and 2013, and that energy prices correlate positively with imports. Despite this positive correlation, I show in a quasi-experimental setup that a sudden drastic drop in electricity prices has not led firms to significantly reduce their imports or their domestic material purchases relative to an unaffected control group. This holds for very electricity-intensive firms; for firms using easily tradable goods; and both for regular importers with a trade network and occasional/non-importers.

Keywords: Offshoring, Energy Prices, Climate Policy, Manufacturing

JEL-Classification: F14, F18, L60, Q41, Q56

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1 Introduction

Against the backdrop of the Ukrainian war and the ensuing high gas prices in many European countries, debates about industry relocation are heating up. To what extent are high energy prices a competitive disadvantage in a globalized world? This question is not only relevant from the perspective of an individual country trying to compete internationally, but also in the face of climate change in a world of incomplete regulation: Offshoring from countries with high energy prices to countries with low energy prices might involve carbon leakage (i.e., the relocation of emissions due to climate policy, counteracting unilateral efforts to limit global warming). This concern is aggravated as energy prices correlate positively with emissions policy stringency (Sato *et al.*, 2015).

Despite its relevance, as of today, we know little about the causal relation between energy prices/climate regulation, offshoring, and emissions. Cherniwchan *et al.* (2017) state that "the interaction between trade liberalization, offshoring, and aggregate pollution emissions is complex and currently not well understood". Theoretical contributions have outlined the potential of environmental regulation to induce offshoring (see, e.g. Copeland and Taylor 2004 for a discussion of classical comparative advantages, Weisbach *et al.* 2022 and Weisbach and Kortum 2021 for conclusions from a Ricardian model, Aichele and Felbermayr 2015 from a Dixit-Krugman-style model, or Bolz *et al.* 2022 for an example with heterogeneous firms). Anecdotal evidence supports these hypotheses: According to a survey of more than 3,500 German firms conducted by the German Chamber of Commerce and Industry, in 2023, 31.7% of firms were considering relocation of capacities abroad due to high energy prices (DIHK, 2023). Yet, hard empirical evidence remains scarce.

In this paper, I use firm-level import data on German manufacturing for the years 2009 to 2013 to study responses to rising energy prices along the supply chain. First, I document that imports and import shares of German manufacturing firms have increased over the observation period. I also show that firm-level energy prices correlate positively with firm-level imports. I then move on to study causal effects in a quasi-experimental setup, and analyse whether a substantial exogenous change in a firm's electricity price leads to a response in its input use. Specifically, I study whether firms subject to a regulation-induced decrease in electricity prices first effective in 2013 reduced their domestic material purchases and/or their imports relative to an unaffected control group, i.e., offshored less, in the same year. The detailed micro-data allow me to differentiate effects along various dimensions such as the firms' electricity cost intensity, the electricity-intensity of the inputs used in their sector, and the ease of tradability of their sector's input mix.

My results can be summarised as follows: Firms subject to a sudden drop in electricity prices subsequently neither significantly increased their value added, nor decreased their imports or their domestic material purchases relative to an unaffected control group. This is also true for the very electricity cost-intensive firms that are heavily affected by changing electricity prices; and for sectors that use a high share of electricity-intensive inputs in their production process and might be more prone to offshore when electricity prices increase. I also find no effect for firms for which offshoring might be easier, because they use a high share of easily tradable intermediates in production or because they generally import more.

There are several potential explanations for these findings which will be subject to future work: 1) Adjustments along the supply chain take longer to materialize and only appear in the medium to long run; 2) only the imports of the most electricity-intensive intermediates respond, and this effect is not visible in aggregate imports; 3) effects of climate policies on imports mostly operate through the decision on what country to source intermediates from, not through the make-or-buy margin.[1]

I add to different strands of literature. First, I add to the literature studying determinants of trade. Traditionally, this stream of literature has focused on the role of relative factor endowments (see, e.g., Baldwin 1971 or Romalis 2004). More recent contributions have stressed the importance of institutional settings such as national tax rates (see, e.g., Feldstein and Krugman 1990 or Melvin 1979). With regards to environmental taxation, the potential of tax differences to affect trade flows has spurred intense debate about pollution haven effects: All else equal, an increasing stringency of environmental regulation will decrease a country's net exports of dirty goods, as discussed in the seminal paper by Copeland and Taylor (2004). Empirical evidence consistent with this hypothesis has been provided, e.g., by Cherniwchan (2017) in the context of local pollutants. I contribute to

[1] Antràs *et al.* (2017) discusses how the optimality of sourcing from one country depends on the other sourcing countries of a firm. Changes in the input cost in one country (e.g., through climate policies) affect the attractiveness of this country in terms of marginal cost savings, and through the interdependencies involved affect the complete sourcing strategy of a firm.

this literature by explicitly focusing on the role of electricity prices as a determinant of trade flows, rather than focusing on the more frequently studied regulation of local pollutants.

More specifically, I contribute to the strand of literature studying the relation between climate policies and trade flows (Aichele and Felbermayr, 2015; Aldy and Pizer, 2015; Naegele and Zaklan, 2019; Sato and Dechezleprêtre, 2015). Most of these studies make use of sector-level data on trade flows, which may lead to aggregation bias and mask effect heterogeneities. Also, these studies cannot disentangle the exact channel through which trade flows adjust. Broadly speaking, the studies identify no (Naegele and Zaklan, 2019) or small (Aldy and Pizer, 2015; Sato and Dechezleprêtre, 2015) effects of energy prices and climate policies on imports; only Aichele and Felbermayr (2015) find a larger effect of differential Kyoto commitment on imports of 5%. For all of these studies, it is not clear whether imports increase because firms start to import intermediates they purchased domestically before, or whether firms directly offshore energy-intensive tasks they used to carry out in-house. This difference seems subtle, but is crucial for policy makers in order to design policy measures that address the group of firms considering offshoring in a targeted manner. I contribute by specifically analysing firms' make-or-buy decisions. To the best of my knowledge, only Colmer *et al.* (2023) and Dussaux *et al.* (2023) use firm-level data to study the topic. While the former focus on the EU ETS under which permit prices have been low in the observation period until 2012, the latter investigate effects of offshoring on firms' emission-intensities, rather than the effect of climate policies on offshoring.

Second, I contribute to the studies analysing the effects of climate policies on firm or plant performance using micro-data. My paper examines a potential channel for the quite prevalent finding that, while climate policies lead to reductions in firms' energy consumption and carbon emissions, they neither significantly increase investments nor reduce sales (Gerster and Lamp, 2022; von Graevenitz and Rottner, 2022; Lehr *et al.*, 2020; Marin and Vona, 2021). One potential explanation for this finding is that offshoring energy-intensive intermediates is an alternative abatement strategy to technological abatement efforts, or to reductions in final output.

Lastly, the paper touches on the literature studying the effects of energy prices on firm re-location (Manderson and Kneller, 2020; Panhans *et al.*, 2017; Saussay and Sato, 2018). Rather than studying the binary decision of complete re-location or setting up a new foreign affiliate, I study responses along the more marginal dimension of offshoring single production processes. In fact, given that most firms in my sample import, I analyse the intensive margin of *how much more* to import when energy prices rise.

2 Framework and suggestive evidence

2.1 The framework: To produce or to offshore?

Why do firms offshore, and how do electricity prices play into this decision? A growing literature stresses that importing involves fixed costs, just as exporting in standard trade models (Antràs *et al.*, 2017; Antràs and Helpman, 2004; Blaum *et al.*, 2018; Gopinath and Neiman, 2014; Halpern *et al.*, 2015; Kasahara *et al.*, 2016; Kasahara and Rodrigue, 2008). Fixed cost of importing vary across origins, and are lower domestically. The fixed cost can be interpreted as arising from searching suppliers, as well as monitoring and communicating with them. Firms can both outsource domestically by purchasing intermediates from a domestic supplier that were previously produced in-house, or offshore abroad. Offshoring abroad may yield higher marginal cost savings than outsourcing domestically if factor prices in the foreign country are lower than at home. Domestically, outsourcing can be beneficial even when firms face identical factor prices if the supplier is more productive than the offshoring firm. Due to the fixed costs involved, only the most productive firms select into importing (from higher cost origins).

Fixed cost can also vary across imported varieties (Gopinath and Neiman, 2014). Intuitively, some goods are more easily tradable than others as they are more homogeneous, reducing the effort required for searching a supplier. Hence, firms can not only adjust along the extensive (importing or not importing) or intensive margin (how much of a given good to import), but also at the "sub-extensive" margin (how many goods to import). Gopinath and Neiman (2014) provide evidence that this sub-extensive margin played a more significant role than the extensive margin in the trade adjustments in Argentina between 2000 and 2002.

In this framework, changes in the offshoring decisions of firms can result both from changes in the marginal cost savings from importing, and changes in the associated fixed cost. Domestic electricity price increases will, all else equal, make importing more attractive. In Germany, retail electricity prices (net of recoverable taxes and levies) for industrial consumers with an electricity consumption between 2 and 20 GWh increased by about 24% between 2009 and 2013. This increase was larger than the EU average (+10%).[2] The increase in German industrial electricity prices also stands in contrasts to developments in major non-European trading partners such as the US and China, where prices decreased by roughly 10%.[3]

Consequently, all else equal, the marginal cost difference between Germany and other countries increased, and imports should increase – especially for countries with lower fixed cost for importing, and for electricitiy-intensive intermediates. In the German setting, most industrial firms are importers, such that these increases will be driven less by the extensive margin, and more by the intensive and sub-extensive margins.[4] The observed electricity price divergence between Germany and other countries could lead to a general trend to offshore in industry. In the next section, I provide suggestive evidence for this general trend and for the role of energy prices for this development. To identify causal effects on offshoring, in Section 3 I check whether German industrial firms for whom electricity prices did *not* increase resorted *less* to offshoring than similar firms experiencing an electricity price increase.

2.2 Two stylized facts: Importing and energy prices

Consistent with a general trend to offshore, between 2009 and 2013, on average, industrial imports have increased. This can be seen when regressing firm-level imports on a series of time dummies following equation (1). The regression is based on the import expenditures

[2]In France, e.g., electricity prices for industrial users increased by 17% between 2009 and 2013. In Poland and the Czech Republic, they even decreased (-5% and -3%).

[3]The numbers are available on the EU's Energy Dashboard under https://energy.ec.europa.eu/data-and-analysis/energy-prices-and-costs-europe/dashboard-energy-prices-eu-and-main-trading-partners_en.

[4]Each year, roughly 82-86% of the approximately 15,000 manufacturing firms on which I have trade statistics available, import, and 75-78% both import and export. The sample of firms with trade data overrepresents large manufacturing firms. Therefore, these shares of importing/exporting firms might be considered an upper bound; yet, the numbers highlight the prevalence of international trade in German manufacturing, and stand in stark contrast with other countries: Halpern *et al.* (2015) report that half on Hungarian firms did not import at all between 1992 and 2003. Bernard *et al.* (2007) find an even lower share of importers of 14% in the US in 1997.

of an unbalanced panel of roughly 15,000 manufacturing firms per year. The data base will be discussed in more detail in Section 3.2.

$$log(y_{i,t}) = \beta_0 + \delta_t + \mu_i + \epsilon_{i,t} \quad (1)$$

In the regression, μ_i represents a fixed effect for firm i, and δ_t dummies for different years t. The left panel of Figure 1 depicts estimated coefficients and confidence intervals for these time dummies. The right panel of the figure shows results from running the regression using import shares as dependent variable, thus controlling for inflation and firm growth. Import shares are defined as the ratio of imports from total material purchases. On average, not only import values, but also import shares have increased in German manufacturing firms. This conclusion holds when controlling for the effects of changing labour cost and capital deepening, by controlling for the wage to sales ratio and the (log) capital to employment ratio, as in Kee and Tang (2016) and Illanes and Villegas-Sánchez (2022), as shown in Figures 7 and 8 in the Appendix.

This increase in the import share is unlikely to be driven by trade liberalisation, as only few EU trade agreements entered into force between 2009 and 2013.[5] The recession in 2009 (in response to the financial crisis) might affect manufacturing import shares in 2009. By 2013, however, import shares were also significantly higher than in the post-crisis year 2010. Aggregate statistics on German imports of intermediate goods are available for a longer time period. These aggregate statistics show indeed that in 2009,

[5] Croatia acceded the EU in 2013 and became member of the single market. However, Croatia already had a free trade agreement with the EU before the accession (the Stabilization and Association Agreement, SAA) which entered into force in 2005 and granted Croatia almost unlimited free access to EU markets. Other than that, the following agreements fall into the observation period: trade agreement with Colombia, Ecuador and Peru (provisionally applied since 2013); Association Agreement with a strong trade component with Central America (provisionally applied since 2013); Partnership and Cooperation Agreement with Iraq (provisionally applied since 2012); Economic Partnership Agreement with Eastern and Southern Africa States (provisionally applied since 2012), Interim Partnership Agreement with Pacific States (provisionally applied since 2013), and the Stabilisation and Association Agreement with Serbia (in force since 2013). None of these agreements concerns large trading partners of Germany and is likely to significantly drive up import shares. For a full overview of EU trade agreements, see https://policy.trade.ec.europa.eu/eu-trade-relationships-country-and-region/negotiations-and-agreements_en.

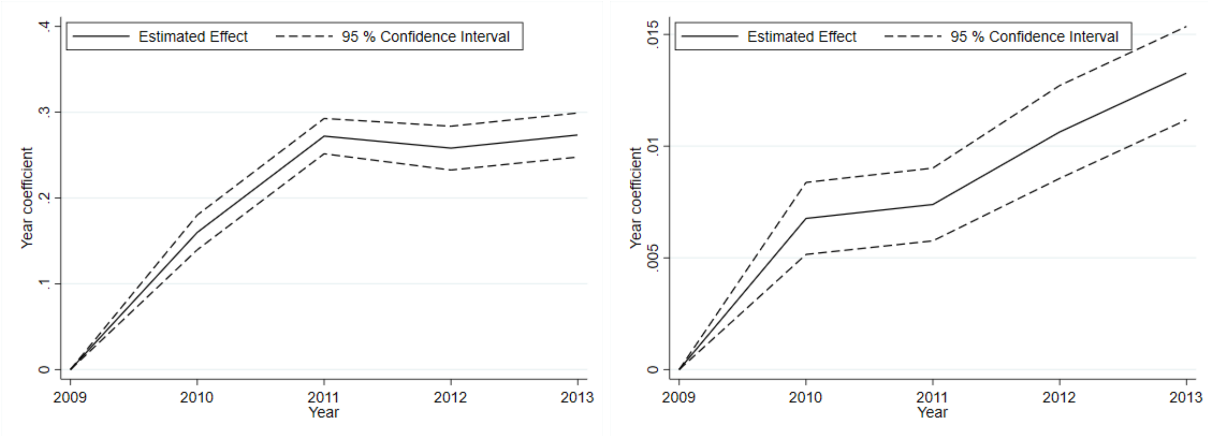


Figure 1: Development of imports (left) and import shares (right) relative to 2009

Notes: Source: Own calculations, based on the unbalanced MLD data in combination with the German Manufacturing Census. Information on the respective DOIs is relegated to the Appendix.

import levels were substantially lower than in 2008. Yet, already in 2010, imports of intermediate inputs exceeded pre-crisis levels, as shown in Figure 9 in the Appendix.[6]

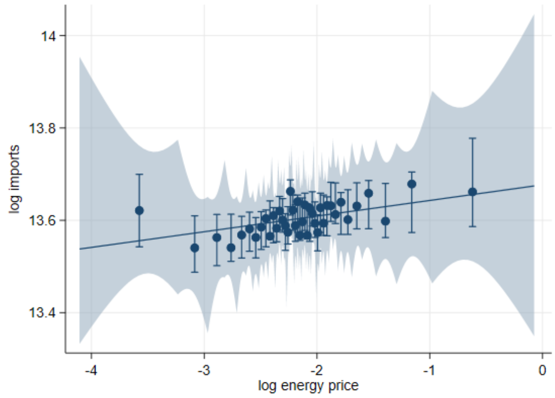
Imports are becoming increasingly important in German industrial production. (How) do energy prices contribute to this development? By running a within firm regression of log imports on log energy prices, I find indicative evidence for a positive relationship:

$$log(imports_{i,s,t}) = \beta_0 + \gamma log(energyprice_{i,t}) + \delta_{s,t} + \mu_i + \epsilon_{i,s,t} \quad (2)$$

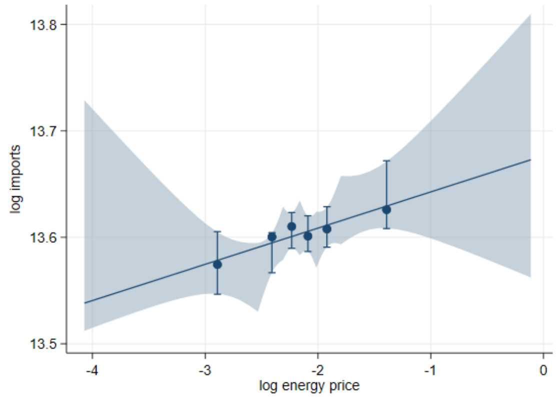
In the regression, I control for firm fixed effects (μ_i) and sector-by-year fixed effects at the 2-digit level ($\delta_{s,t}$). Firm-level energy prices are calculated by dividing a firm's energy expenditures (in Euros) by its energy consumption (in kWh). Standard errors are clustered on the firm-level. The estimation again is based on an unbalanced panel of approximately 15,000 manufacturing firms.

Regression results are graphically depicted in binned scatterplots in Figure 2. Each dot in the figure represents predicted imports in each bin, evaluated at the mean of the covariates included, and net of the fixed effects used. The line shows the linear regression fit. Vertical lines and shades reflect point-wise confidence intervals for each bin and a

[6]Intermediate goods are defined following the Commission regulation (EC) no 656/2007 of 14 June 2007 on Main Industrial Groupings.



(a)



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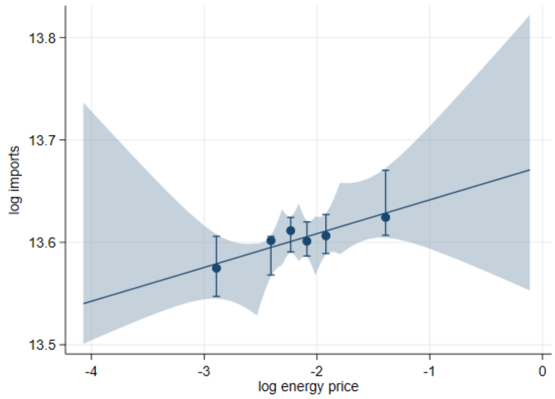

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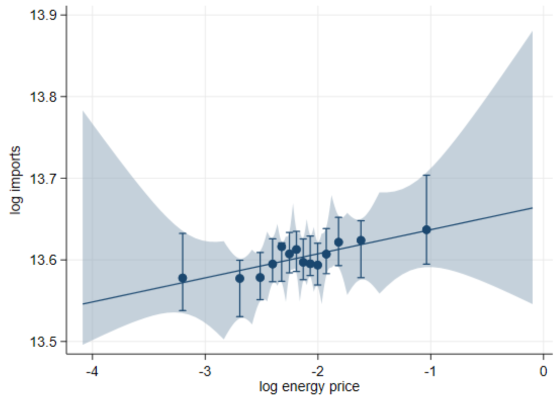

(d)

Figure 2: Results from regressing imports on energy prices (a) within firm and with 2-digit sector-by-year fixed effects (b) additionally controlling for firm wages (c) additionally controlling for non-parametric federal state time trends (d) instead using 4-digit sector fixed effects

Notes: Source: Own calculations, based on the unbalanced MLD data in combination with the German Manufacturing Census. The regression contains observations from 2009 to 2013. Observations outside the 1st to 99th percentile range with respect to energy prices are excluded as outliers. This concerns firms with an average energy price below 1.5 of above 97 cents per kWh. The dependent variable is the log of firm-level imports. Standard errors are clustered at the firm-level. The number of bins are selected automatically in a data driven way. The automatic procedure leads to the number of bins varying depending on the covariates included.

uniform confidence band.[7] Numerical regression results are shown in Table 10 in the Appendix.

There is a statistically significant and clearly positive relation between a firm's energy price and its imports. A linear model provides a good approximation of the relationship in logs. A 1% increase in firm-level energy prices is associated with a 0.034% increase in its imports on average.[8] The effect is of a similar magnitude as the estimate by Sato and Dechezleprêtre (2015) who find that a 10% increase in the energy price differential at the country pair-sector level increases imports by 0.2%.

This estimate does not constitute a causal effect: Firm-level energy prices are endogenous, as firms can negotiate contracts with suppliers, or benefit from quantity discounts. If growing firms face decreasing energy prices and growing firms also increase their import levels, the estimate will be biased downwards. Yet, the regression result provides more than a naive correlation: Any effect of firm size (e.g., larger firms generally paying lower energy prices) are controlled for. Including proxies for alternative drivers of imports, namely firm-level wages (panel (b)), as well as federal state-by-year fixed effects that broadly capture changes in physical trade cost or effects of regional (industrial) policies (panel (c)) leave the positive correlation between energy prices and imports unaffected. So does controlling for finer 4-digit sector-by-year fixed effects that better capture any potential changes in tariffs (panel (d)). In the next section, I use a regulatory change through which electricity prices decreased for a subset of treated firms as an exogenous price change to estimate causal effects.

[7]I use the method by Cattaneo *et al.* (2023) to automatically select the number of bins, as well as include covariates and fixed effects in the binscatter plots. The number of bins is selected in a data driven way and is optimal in minimizing the integrated mean squared error, trading off bias and variance. Therefore, the number of bins depend on the covariates included and the variation available for estimating the relationship between log imports and energy prices.

[8]Note that within-firm, energy prices also correlate positively to domestic material purchases. This is shown in Table 11 in the Appendix.

3 The causal effects of electricity prices on firm-level imports

3.1 The quasi-experiment: the exemption scheme from the Renewable Energy Surcharge

To estimate the causal effects of electricity price changes on firm-level imports in Germany, I exploit a quasi-experiment: the amendment of the exemption scheme for the Renewable Energy Surcharge. The Renewable Energy Surcharge (RES) was used to finance guaranteed feed-in prices (per kWh) for renewable energy providers in Germany. These feed-in tariffs were introduced to expand renewable generation in the German energy mix. When wholesale electricity prices were below the guaranteed price, the wedge had to be financed. This was achieved by charging all electricity users a surcharge on electricity prices, i.e., the RES. The RES was charged from 2000 until its abolishment in 2022. Whereas in early years, the RES amounted to less than 1 cent per kWh, the expansion of renewables in the German electricity grid led to a substantial increase: In 2013, the RES made up 5.28 cents/kWh (and continued to increase afterwards). It accounted for roughly a third of total electricity prices in 2013. The share of the RES from total electricity prices for industrial consumers is shown in Figure 3. Between 2006 and 2013, increases in electricity prices were driven by the RES to a large extent. Without the RES, electricity prices would have been rather constant from 2006 onwards.[9]

The surge in the RES came along with concerns about its effects on firm competitiveness. Therefore, electricity-intensive users have been eligible for an exception from paying the full RES under the so-called "special equalisation scheme" if they satisfied certain requirements. Prior to 2012, users with an electricity procurement of above 10 GWh and a ratio of electricity cost to value added above 15% were eligible. The former condition applied at the plant-level, whereas the latter condition had to be satisfied at the firm-level.

In 2012, this exemption scheme was changed by the Amendment to the Renewable Energy Law (EEG): The threshold in terms of electricity procurement was lowered from

[9]Without the RES, renewable energies might not have expanded as much in Germany, arguably leading to higher cost for electricity generation. In that sense, the development of the RES is not clearly separable from the development of electricity prices.

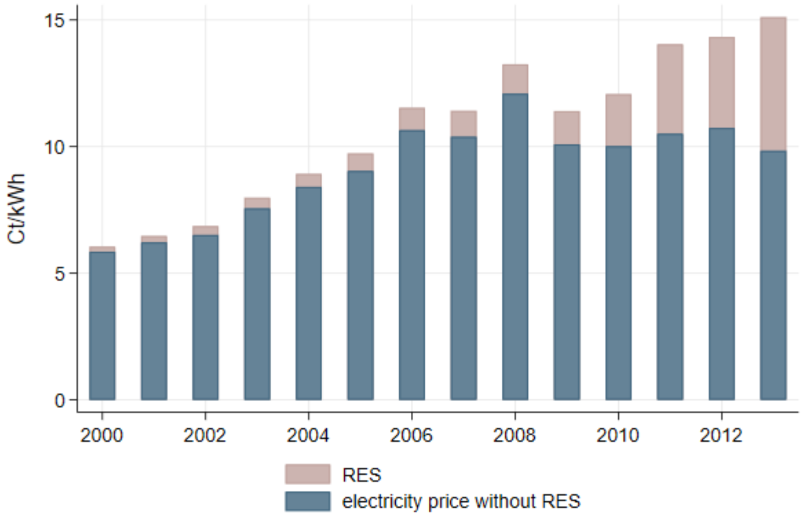

Figure 3: Average electricity prices and the Renewable Energy Surcharge from 2000 to 2013

Notes: The Figure depicts an average of electricity prices applying to new contracts for industrial users with an annual consumption between 0.16 GWh and 20 GWh. The electricity user is assumed to be connected to the electricity grid at the medium voltage level. Prices are nominal and include all taxes and levies. The numbers are taken from the German Association of Energy and Water Industries (BDEW, 2022).

10 to 1 GWh. The threshold for the electricity cost ratio was lowered from 15% to 14%. Suddenly, a lot more manufacturing users were eligible for the exemption. In 2011, there were 554 industrial beneficiary companies; this number more than tripled by 2013. Correspondingly, the amount of (industrial) exempt electricity rose from 71,784 GWh in 2011 to 90,724 GWh in 2013 (i.e., by roughly 19 TWh) (BMWi and BAFA, 2014). These newly exempt firms experienced a decrease in their electricity prices, while the RES continued to increase for the users not subject to the exemption.[10] The size of the price reduction for the newly exempt depended on firm-level electricity procurement (as the reduction followed a step function), and electricity cost intensity (determining whether users became partial or full beneficiaries of the exemption scheme). Note that throughout the observation period, electricity generated onsite was fully exempt from the RES. Appendix 9.1 details the exact regulatory rules on reduced rates.

To become exempt, firms had to submit an application proving they met the eligibility criteria. The application had to be submitted in a given year for exemption in the following year, and based on the electricity procurement and electricity cost intensity of the prior year. This procedure introduced a two-years lag between the electricity usage behaviour determining exemption, and the exemption actually applying. The change in the exemption scheme from 2012, therefore, resulted in newly eligible users being exempt for the first time in 2013, based on their electricity usage of 2011.

I exploit this regulatory change in the exemption from the RES as a quasi-experiment. Effectively, I compare firms that had to pay the full RES throughout with firms that experienced a strong drop in the RES in 2013 through the exemption. In contrast to total electricity or energy prices, the development of the RES is exogenous to manufacturing firms and could not be negotiated. I follow in the footsteps of Gerster and Lamp (2022) who have already shown that plants newly benefiting from the exemption increased their electricity procurement in 2013 as compared to control plants facing the full RES. In contrast to the study by Gerster and Lamp (2022), I focus on whether this increase in electricity consumption relative to the control group was driven by adjustments along the supply chain. The thought experiment is the following: Did firms that got access to cheaper electricity subsequently offshore less than otherwise very similar firms for which electricity became more expensive?

[10]The reduced RES, as of 2012, only applied to the electricity procurement exceeding 1 GWh.

The comparison used for identification is shown in Figure 4. The blue line depicts the development of the RES in Euros per kWh over time. The red line shows the development of the effective RES paid per kWh of electricity consumption for firms that became newly exempt in 2013.[11] Note that the paths of the RES effectively paid are slightly different for firms that generated some of their electricity onsite between 2009 and 2013. This is because, as noted, electricity generation has been exempt from the RES, such that for onsite generating firms the effectively paid RES per kWh of electricity *consumption* was lower (while the RES paid per kWh of electricity *procurement* of course was identical to non-generators).[12] Since electricity procurement and generation are perfectly substitutable, firms that had the capacity to generate electricity onsite had some control over the RES they had to pay (in average terms, not marginally). Therefore, the RES is not fully exogenous for this group of firms, and I drop them in a robustness check.

Using the change in the exemption scheme from the RES as a quasi-experiment to study responses along the supply chain offers some advantages over prior studies. First, as compared to Colmer *et al.* (2023) who look at the EU ETS, the change in electricity prices I study is large. In 2011 (i.e., prior to exemption), electricity expenditures made up on average 4% of total cost and 11% of material cost in the newly exempt firms, such that the regulatory change led to a notable change in total firm-level cost. In contrast, according to Naegele and Zaklan (2019), the emission costs due to the EU ETS were below 0.65% of material cost for large parts of European manufacturing between 2004 and 2011. Therefore, a response in the form of a re-optimization of the input mix is more likely in case of the RES than in case of the EU ETS. Second, the time lag introduced

[11]For new beneficiaries, the effective RES paid per kWh in 2013 constitutes an average that depends on the electricity procurement of new beneficiaries and the corresponding reduction applicable according to the step function. Exempt firms had, e.g., to pay the full RES on the first GWh of their electricity procurement. A reduced rate only applied to the electricity procurement exceeding 1 GWh. Consequently, a firm procuring 1.5 GWh of electricity paid a higher RES per kWh than a firm procuring 10 GWh of electricity. The average is computed using information on electricity procurement from the German Manufacturing Census.

[12]The RES paid per kWh of electricity consumption depends on the share of electricity consumption that was generated onsite. As these shares were not necessarily identical for the newly exempt and the non-exempt firms, the RES per kWh of electricity consumption can deviate between the onsite generators of these two groups of firms even before the regulatory change. Information on the shares of electricity generated onsite for the two groups are taken from the German Manufacturing Census.

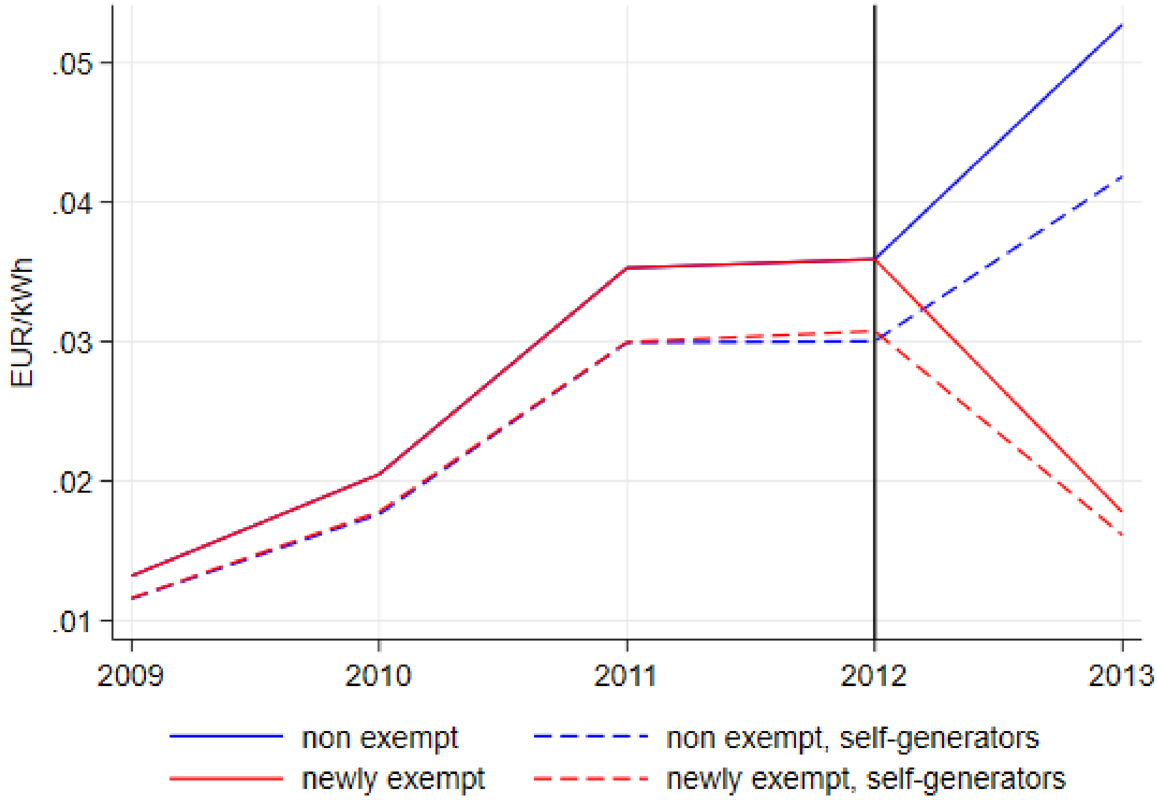

Figure 4: Development of the average Renewable Energy Surcharge per kWh of electricity consumption for different firm groups

Notes: The Figure depicts the effective Renewable Energy Surcharge in cents per kWh of electricity consumption for different firm groups: Those that paid the full RES throughout; those that paid the full RES on their electricity procurement but generated part of their electricity consumption onsite; those that became exempt after the regulatory change in 2012; and those that became exempt in 2013 and were also electricity self-generators. The numbers are own calculations based on the EEG 2012 and data from the German Manufacturing Census.

by the application procedure for the RES exemption means that firms generally knew ahead of time that their electricity prices would drop substantially. Adjusting sourcing strategies might take time, such that imports might not respond immediately to price shocks (e.g, because of longer-term contracts with suppliers, or fixed cost necessary for adjustment).[13] The setting I study gives firms one year to make these adjustments, which makes it more likely that I can detect an effect.

3.2 Data

To estimate the effects of the RES exemption on firm level offshoring, I combine administrative micro-data from the official German Manufacturing Census with a newly available module on firm-level trade flows: the so-called micro-data linking panel (MDL) from the Federal Statistical Office of Germany. While the Census data contains, among others, information on plant-level electricity and energy consumption, employment, or sales, the MDL covers firm-level import expenditures and export revenues for the years 2009 to 2013. From 2011 onward, trade flows are differentiated by country of origin and destination. While the MDL covers more sectors, in this paper, I limit my attention to manufacturing: Manufacturing is more prone to leakage effects than, e.g., the energy or construction sectors as manufacturing goods are easily tradable. I aggregate all plant-level information from the Manufacturing Census to the firm-level to match the trade statistics. The data sets are readily matched, as both contain the same firm identifier.

The Manufacturing Census generally covers all German industrial plants with more than 20 employees.[14] In contrast, the MDL covers a stratified sample of firms. This sample is based on the cost structure survey – a Census module containing information on firm-level cost components. The cost structure survey (and hence the MDL) covers a sample of around 18,000 firms per year, equivalent to roughly 45% of firms in manufacturing and mining and quarrying. Bigger firms are more likely to be sampled, and

[13] There is consensus among researchers that especially when it comes to international suppliers and to specialized components, firms engage in long-term stable relationships due to the high search cost involved. Forming a trade network might also involve training provided by buyers to suppliers, knowledge transfer, or specific investment. For a discussion of these issues, see e.g. Rauch (1999, 2001); Rauch and Watson (2003) or Defever *et al.* (2016). Import relations – at least for some goods – are designed for the long-term and might exhibit some delay in their adjustment response.

[14] More details can be found e.g. in Petrick *et al.* (2011) or von Graevenitz and Rottner (2023).

firms with more than 500 employees are always included in the survey. Every four years, participation in the cost structure survey is rotated to keep the effort for responding firms as low as possible. Such a rotation took place in 2012. For this reason, the panel of firms with trade data is highly unbalanced: Most firms are either observed for the years 2009 to 2011, or for the years 2012 and 2013. Only about 20% of manufacturing firms are observed throughout. As the rotation in the sample introduces considerable noise I restrict my attention to a balanced panel of firms to uncover causal effects of the RES exemption. In consequence, the focus of analysis in this paper lies on large firms which are more likely to be surveyed every year. The larger firms are arguably more relevant to the research question: From all firms in the MDL, the subset of balanced firms accounts for roughly three quarters of imports, exports and sales, as reported in Table 9 in the Appendix.[15,16] Comparing import expenditures from the MDL with aggregate statistics shows that the MDL makes up roughly 70-80% of total German imports of intermediate goods (see Table 8 in the Appendix). Assuming that all imports by industrial firms concern intermediate inputs, in 2013, an analysis restricted to the balanced panel of MDL firms covers 64% of German intermediate goods imports.

Trade flow information in the MDL come from the foreign trade statistics. Specifically, extra-EU trade data are collected by the customs administrations, while trade flows within the EU are directly reported by firms.[17] In the foreign trade statistics, trade flows are in parts reported by holdings instead of single firms, i.e., the holding reports trade flows for all its members. In preparing the MDL, the Federal Statistical Office redistributed

[15]It is well known that the firm distribution is highly skewed, especially with regards to trade activity. Bernard *et al.* (2007) documented that in the US, in 2000, 0.4% of firms made up 96% of US exports. Table 9 indicates that in Germany, trade is somewhat less concentrated.

[16]Focusing on the large firms also offers the advantage that there are only few zeros in trade flows, reducing the need to explicitly deal with selection, as noted by Bas *et al.* (2017). I hence focus on studying the intensive and sub-extensive margins in terms of trade responses.

[17]In the foreign trade statistics, within EU trade features a reporting threshold to relieve the burden: Firms with total within EU exports of below 500,000 Euros and total within EU imports of below 800,000 Euros are exempt from reporting that trade flow. In the MDL, trade flows below these thresholds are filled in with information from the VAT advance return. As the VAT advance return does not collect information on the trading partner, in these cases, the trading partner is unobserved even after 2011. Trade flows not even contained in the VAT advance return are of low importance and empirically less interesting.

trade flows of holdings on the member firms by revenue shares. This introduces some measurement error.[18,19]

I combine the detailed firm-level data from the Manufacturing Census and the MDL with a list of plants that benefit from a RES reduction. The Federal Office for Economic Affairs and Export Control (BAFA) compiles lists of granted reductions for the years 2010 to 2013. The matching procedure with the Manufacturing Census relies on Bureau van Dijk identifiers, location (i.e., municipalities), and tax identification numbers.[20] The lists on granted exemptions are available on the plant-level, while trade flow information are available at the firm-level. Conducting the analysis at the firm-level brings about new challenges such as changes in ownership structure. I drop all firms that buy or sell plants from or to other firms. For these firms, changes in the exemption status can be grounded in the acquisition of an eligible plant. Changes in the firm's composition conflate the effects of the RES exemption to be identified.[21] A graphical illustration of the data structure, the different data sources and their linkage can be found in Figure 6 in the Appendix. Overall, I have information on roughly 300 newly exempt and 14,100 potential control firms (see Table 1).

3.3 The estimation strategy

I treat all firms as treated if they owned at least one plant that was first exempt from paying the full RES in 2013, and that had an electricity procurement of below 10 GWh in 2011 (such that it only became eligible to the exemption after the regulatory change).[22]

[18]In some cases, this is apparent by reported imports being larger than firm-level material expenditures. In these cases, I set import numbers to missings. More information on the data cleaning can be found in the Appendix Section 6.2

[19]The data base for the MDL, its preparation and according challenges are described in more detail by Jung and Käuser (2016) and Knaus and Leppert (2017) (only available in German).

[20]I thank Andreas Gerster for generously providing his list of exempt plants already equipped with these identifiers.

[21]This leads to the drop of 29 treated, and 2,181 untreated firms. About 67% of treatment firms, and 77% of potential controls are single-plant firms. For single-plant firms that simply change the firm identifier over time without any apparent structural change, I construct a firm identifier that is consistent over time. Firm identifiers are changed by the Statistical Offices, e.g., in case of a change in the legal form, the name, or a change in the NACE sector at the 4-digit level.

[22]Also plants with an electricity procurement above 10 GWh only became eligible for exemption through the regulatory change if they belonged to firms with an electricity cost intensity of exactly

Conversely, firms are treated as controls if they were not subject to the exemption scheme between 2009 and 2013.[23] The basic idea is to use the control firms to calculate how outcomes in the treatment firms would have developed in the absence of treatment, to back out the causal effect of said treatment. Such difference-in-differences designs have frequently been used in program evaluation, also for environmental and climate policies (see, e.g., Colmer *et al.* 2023; Fowlie *et al.* 2012; Greenstone 2002; Martin *et al.* 2014a; Walker 2013).

Table 1 shows differences between newly exempt firms and the non-exempt firms in the base year 2011 for the full (unbalanced) sample of firms for which trade statistics are available. Clearly, in 2011, firms that became exempt from paying the full RES two years later were structurally different from the non-exempt firms: They tended to use and procure more electricity on average, and had higher energy cost. Electricity cost made up a considerably higher share of their value added.[24] At the same time, they were generally smaller in terms of employment. The sectoral composition between treatment and control group also differed, as shown in the left panel of Table 2. Treatment firms were much more prevalent in the food (NACE 10), textiles (NACE 13), wood (NACE 16), rubber and plastics (NACE 22) and metals (NACE 24) sectors, and less prevalent in the electronics (NACE 26), electronic equipment (NACE 27), machinery (NACE 28), and motor vehicles (NACE 29) industries.

These structural differences complicate the estimation of causal effects: As pointed out by Imbens (2015), conventional regression methods generally are not robust to substantial

between 14% and 15%. However, since I measure electricity cost intensity inaccurately, I abstain from trying to identify these cases.

[23]Some of the control firms benefited from the exemption scheme in later years. In specific cases, using "not-yet treated" units introduces bias in the regression. This is the case if the not-yet treated firms were actively increasing electricity use and electricity cost intensity over my observation period in order to become exempt in the future. As I describe below, I trim my sample to exclude firms that are far from meeting the eligibility criteria, such as firms with an electricity procurement below 1 GWh. This limits the extent to which the not-yet treated are expected to follow different paths than the never-treated. For a discussion of the (dis)advantages of using not-yet versus never treated as a control group, see Callaway and Sant'Anna (2021).

[24]Data on electricity cost are not available for 2011, but only for 2010. Therefore, they have to be imputed to calculate the 2011 electricity cost intensity. Due to inaccuracies in this imputation, the average electricity cost intensity of treatment firms in 2011 is below the 14% threshold they actually have to satisfy.

Table 1: Differences between newly exempt and other firms in 2011

	Rest of firms		**Newly exempt firms**		Difference
	Mean	Count	Mean	Count	
Electricity consumption (GWh)	7.5	14,084	27.7	295	-20.2**
Electricity procurement (GWh)	6.6	14,084	25.5	295	-18.9**
Energy use (GWh)	25.0	14,084	124.0	295	-99
Electricity cost from value added (in 2010, %)	5	9,184	18	193	-13***
Electricity cost from value added (in 2011, imputed, %)	5	9,184	12	193	-7***
Energy cost from value added (%)	8	13,892	31	269	-23***
Energy cost (mio.)	1.4	14,084	3.7	295	-2.3***
Employees	233	14,084	160	295	73***
Sales (mio.)	77.7	14,084	71.8	295	5.9
Imports (mio.)	12.8	13,665	9.3	286	3.55
Domestic material purchases (mio.)	32.5	13,665	42.2	286	-9.7
Import share from materials (%)	18.5	13,665	18.1	286	0.4

Notes: Own calculations, based on the MDL and the Manufacturing Census. The table reports results from t-tests for 2011. Firms that own at least one newly exempt plant constitute the treatment group. **, ** and *** indicate significance of differences at 10%, 5% and 1%, respectively.*

Table 2: Sectoral composition of treatment and control firms in 2011

	Before trimming		After trimming	
	Rest of firms	Newly exempt	Rest of firms	Newly exempt
	Share of firms (%)		Share of firms (%)	
NACE 10: food	11.82	22.07	28.26	34.43
NACE 11: beverages	1.48	2.07	1.57	2.46
NACE 12: tobacco	0.13	0	x	0
NACE 13: textiles	2.46	5.17	4.40	4.92
NACE 14: wearing apparel	1.26	x	0	x
NACE 15: leather	0.62	x	x	x
NACE 16: wood products	2.50	5.86	3.45	x
NACE 17: pulp, paper	2.44	1.72	2.51	x
NACE 18: printing, publishing	2.08	x	1.73	0
NACE 19: coke, petroleum	0.25	x	0.63	x
NACE 20: chemicals	4.96	6.90	6.91	8.20
NACE 21: pharmaceuticals	1.22	0	0.63	0
NACE 22: rubber, plastics	5.73	15.52	5.34	4.10
NACE 23: non-metallic minerals	4.54	8.97	9.26	11.48
NACE 24: metals	3.66	9.31	13.81	x
NACE 25: metal products	14.01	11.38	7.22	x
NACE 26: electronics	4.81	x	1.41	x
NACE 27: electronic equipment	5.87	x	2.20	x
NACE 28: machinery	15.02	2.07	2.51	2.46
NACE 29: motor vehicles	3.83	1.72	3.77	x
NACE 30: other transport	1.14	0	1.10	0
NACE 31: furniture	2.38	x	1.10	0
NACE 32: other	3.88	x	0.78	0
NACE 33: repair, installation	3.19	0	0	0

Notes: Own calculations, based on the Manufacturing Census. The table reports the share of firms in different two-digit sectors in 2011. Firms that own at least one newly exempt plant constitute the treatment group. The left panel contrasts the sectoral composition of treatment and control firms before trimming, the right panel after trimming. "x" denotes cases in which confidentiality does not permit to report the numbers due to the lack of observations.

differences in the covariate distribution between treatment and control groups, given their reliance on extrapolation. I therefore trim the sample to make treatment and control groups more comparable. I make use of the two eligibility criteria for trimming. Specifically, I drop all control firms with an electricity procurement below 1 GWh in 2011. I also drop all control firms with a ratio of electricity expenditures to value added below 8.1% in 2011.

Trimming on electricity procurement directly follows the eligibility criterion of 1 GWh. For the trimming on electricity cost intensity, I use a value below the eligibility criterion of 14%. I do so because a considerable share of treatment firms in the sample fails to satisfy the 14% requirement. This might be due to an inaccurate imputation of electricity expenditures in 2011. Data on electricity expenditures are part of a specific Census module and only available for 2010. I therefore impute electricity expenditures in 2011 for the calculation of electricity cost intensities. I describe the procedure for the imputation in more detail in the Appendix 6.3.[25] The imputation arguably is one reason why not all treatment firms exceed an electricity cost intensity of 14% in 2011.[26] 90% of treatment

[25] Outliers that change their (imputed) electricity cost intensity substantially between 2010 and 2011 are not used as controls, as detailed in the Appendix, which increases the signal-to-noise ratio and contributes to identifying controls that are electricity cost intensive throughout. One potential reason for large changes in the electricity cost intensity is that non electricity intensive firms might have tried to game the system to benefit from the RES exemption by artificially increasing their electricity cost intensity in 2011. Policy reports have recognized the potential for manipulation of electricity cost intensity to achieve exemption: Strategies include the reorganisation of firms and the formation of spin-offs bundling the most electricity intensive operations of a firm, but also agreeing on special contractual conditions with energy suppliers to procure electricity at high cost (possibly in exchange for more favourable conditions for other energy carriers), or the reduction of gross value added, e.g., through temporary employment (Neuhoff *et al.*, 2013). Since a draft legislation for the EEG amendment has already been published in June 2011, it is possible that firms in 2011 were actively manipulating their electricity cost intensity. To check for whether firms were artificially decreasing their value added, I run regressions (before trimming) explaining a firm's value added by a series of year dummies and firm fixed effects. Results are shown in Table 17 in the Appendix. On average, I do not find that value added in 2011 was significantly lower than in 2007, neither among control nor among treatment firms.

[26] Other reasons include misreporting in the Census data, or a mismatch between firm- and plant-level data, where firm-level value added can capture activities outside of manufacturing, while information on electricity procurement – and in extension, imputed 2011 electricity expenditures – is restricted to manufacturing plants. If firms open up plants outside of manufacturing, the imputed electricity expenditures will be too low.

firms however do exceed an electricity intensity of 8.1%, and I set the threshold for trimming at this value. Trimming on values ranging from 9 to up to 14% generally yields the same results as using the 8.1% threshold. Results are shown in Table 21 in the Appendix.[27]

Table 3: Differences between newly exempt and other firms in 2011 after trimming

	Rest of firms		**Newly exempt firms**		Difference
	Mean	Count	Mean	Count	
Electricity consumption (GWh)	29.2	637	40.7	124	-11.5
Electricity procurement (GWh)	28.2	637	35.4	124	-7.2
Energy use (GWh)	138.0	637	257.0	124	-119
Electricity cost from value added (in 2010, %)	10.9	637	16.6	98	-5.7***
Electricity cost from value added (imputed, %)	12.9	637	19.0	98	-6.1***
Energy cost from value added (%)	20.9	632	31.1	113	-20.2***
Energy cost (mio.)	5.3	637	5.9	124	-0.6
Employees	330	637	256	124	74
Sales (mio.)	120	637	131	124	-11
Imports (mio.)	18.5	616	17.3	120	1.2
Domestic material purchases (mio.)	56.6	616	80.4	120	-23.8
Import share from materials (%)	23.5	616	19.6	120	3.9*

*Notes: Own calculations, based on the MDL and the Manufacturing Census. The table reports results from t-tests for 2011 on the balanced and trimmed sample. Firms that own at least one newly exempt plant constitute the treatment group. *, ** and *** indicate significance of differences at 10%, 5% and 1%, respectively.*

Trimming leads to a considerably more homogeneous sample, as is shown in Table 3. After trimming, firms are more similar with respect to their electricity use and their energy expenditures.[28] The sectoral composition also becomes more balanced which is shown in the right panel of Table 2. Yet, some imbalances remain in terms of import shares, as well

[27] Given the imputations necessary for trimming on electricity cost intensity and the inaccuracies involved, one might suggest to only trim the sample on the more accurately measurable criterion of electricity procurement. Doing so however keeps treatment and control groups highly imbalanced, with control firms being much larger in terms of employees, sales and imports, and much less electricity intensive. This is shown in Table 14 in the Appendix.

[28] Imbens (2015) suggests to measure imbalance in terms of normalized differences instead of t-tests, to avoid large sample sizes rendering even small differences statistically significant. I report standardized differences in Tables 12 and 13 in the Appendix. They confirm the results from the t-tests. The only

as energy and electricity cost intensities.[29] Figure 5 shows kernel plots of the electricity cost intensity of treatment and control group in 2011 before trimming (on the left), and after trimming (on the right). After trimming, there is a strong overlap at the lower end of the distribution. At the upper end, only a few control firms display electricity cost intensities comparable to treatment firms. Trimming brings the distributions closer together, but cannot fully eliminate differences in terms of averages.[30]

I implement the difference-in-differences regression design to identify the effect of the exemption from the RES using a standard two-way fixed effects model.[31] The regression equation is given by:

$$y_{ist} = exp(\sum_{t=2009}^{2013} [\mathbf{1}\{t \in \Theta_\tau\} \times \beta_\tau \times RES_i] + \lambda_{st} + \mu_i + \epsilon_{ist}) \tag{3}$$

In this equation, i indexes firms, s sectors and t years. y is the outcome variable of interest, such as electricity procurement, imports, or domestic material purchases. 2-digit sector-by-period fixed effects λ_{st} control for time-varying unobservables at the sector-level, such as changes in technology, policy or demand. Time-invariant firm-specific characteristics are controlled for by the firm fixed effect μ_i. Standard errors are clustered on the firm-level. RES denotes firm-level treatment, and is equal to 1 for firms that became exempt from paying the full RES in 2013. With the coefficients β_t, I estimate

variables with standardized differences above 0.3 (which Imbens (2015) calls moderate) are the electricity and energy cost intensities.

[29]Differences in import shares might be grounded in the different sectoral composition of treatment and control group at the 4-digit level. Additionally limiting the sample to observations from 4-digit sectors in which there is at least one treatment firm renders import shares balanced. Roughly 65% of control firms operate in 4-digit sectors with at least one treatment firms. Regression results do not change with this additional restriction, as shown in Table 20 in the Appendix.

[30]It might be surprising that there actually are so many firms that reach eligibility thresholds but are not exempt. Gerster and Lamp (2022) make the same observation. They argue that applying for an exemption comes with considerable compliance cost, e.g., in the form of hiring accountants to verify eligibility status. Starting in 2008, applicants had to let accredited experts survey them and assess their energy saving potentials which might have deterred some applications.

[31]Recent research has strongly improved awareness for the conditions under which two-way fixed effects estimation yields unbiased estimates of the average treatment effect on the treated (ATT) (Athey and Imbens, 2018; Borusyak *et al.*, 2022; de Chaisemartin and D'Haultfoeuille, 2020; Callaway *et al.*, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). I discuss the reasons why the criticisms to two-way fixed effects estimation do not apply in my case in the Appendix Section 10.

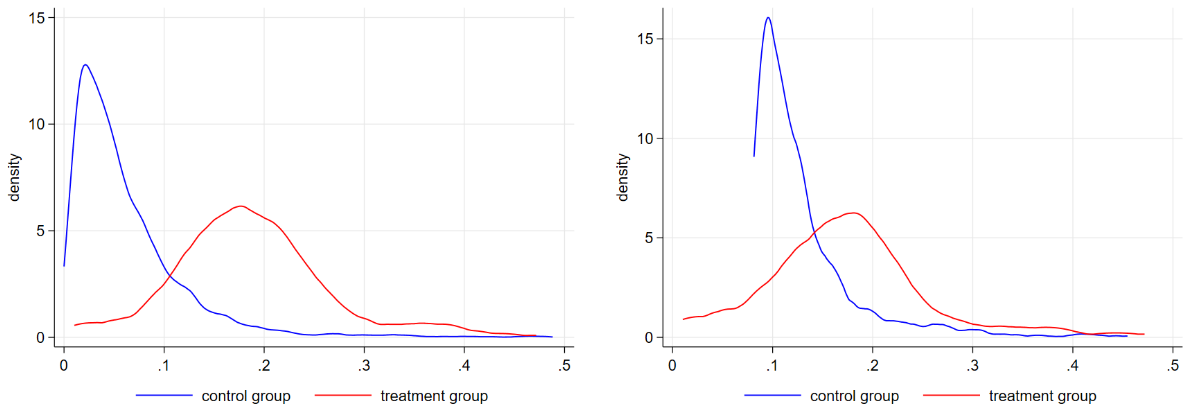

Figure 5: The distribution of electricity cost intensity in 2011 in treatment and control group before (left) and after (right) trimming

Notes: Source: Own calculations, based on the Manufacturing Census.

whether treated firms developed differently than non-treated firms in different years. The baseline is the year 2011.

I estimate equation (3) in its multiplicative form using pseudo Poisson maximum likelihood (PPML). As compared to log-linearized models, PPML yields consistent estimates in the presence of heteroskedasticity (Santos Silva and Tenreyro, 2006). The estimator is also a natural choice for cases in which the dependent variable can take on zero values. While this is not the case for variables such as electricity use, there are non-importers in German manufacturing. Using PPML allows me to retain these observations.[32]

Two-way fixed effects regression recovers causal effects if specific assumptions are fulfilled. Crucially, the difference-in-differences logic underlying this estimation requires the so-called Stable Unit Treatment Assumption (SUTVA) as well as the Parallel Trends Assumption to be satisfied. First, SUTVA requires that the treatment of the treated units has no direct effect on the outcomes of the control group (i.e., there are no spillovers). The assumption would be violated if the 2012 EEG amendment led not only to some additional firms becoming exempt from the RES, but also to the RES increasing for the remaining firms – because fewer electricity users had to cover the fixed subsidisation of renewables in Germany. As in the context of the regulatory change in 2012 exemptions granted to other industries (such as water supply, recycling, and public transportation) were removed, this

[32]PPML can also be applied to non-count data, as long as the dependent variable is nonnegative. For consistency, only the conditional mean has to be correctly specified (Gourieroux *et al.*, 1984).

was in fact not the case (Gerster and Lamp, 2022).[33] Intra-firm spillovers are excluded by construction as the analysis is carried out on the firm-level. Lastly, SUTVA could be violated through general equilibrium effects, given that treatment and control firms compete in product and factor markets. This point is extensively discussed by Barrows *et al.* (2023). Concerns about this channel are mitigated by the fact that I find no statistically significant effects of the RES exemption on competitiveness indicators such as sales, as Gerster and Lamp (2022). Still, the estimator I apply does not formally account for general equilibrium effects.

Second, parallel trends requires that treatment and control firms would have followed similar trajectories absent treatment. While untestable, I provide empirical support for parallel trends by checking whether in pre-treatment periods, control and treatment firms exhibit statistically significantly different trends (i.e., the coefficient β_t is statistically significant for $t \neq 2013$). Note that variables from the Manufacturing Census are available for a longer time period than the MDL. For dependent variables not taken from the MDL, I use data starting in 2007, allowing me to test pre-trends over a longer period. Results however have to be interpreted with caution: Roth (2022) shows that tests for pre-trends of this kind often lack power against violations of parallel trends that would produce non-negligible bias in the treatment effect estimates. For transparency, therefore, underneath each result I report the size of a linear pre-trend that would go undetected in my setting with a power of 50/80.[34]

4 Results and discussion

4.1 Two-way fixed effects

Table 4 shows the results from running the two-way fixed effects regression from equation (3). Column (1) shows that the reduction in the RES led to an increase in electricity

[33] According to the BMWi and BAFA (2014), the privileged electricity use changed by 0.4 TWh only through the regulatory change, due to the combination of the change in the eligibility threholds in manufacturing and the removal of exemptions for other industries.

[34] For this purpose, Roth (2022) introduced the following procedure: First, a test is conducted to check whether any pre-treatment coefficient is statistically significant. Then, the power of this test against a linear pretrend with a given slope is calculated analytically, up to a slope at which the probability of rejecting the pre-test is 50 or 80 percent.

procurement of about 8.4% for the treated firms relative to the control group. This estimate corroborates earlier findings and lies in between the effect estimates of Gerster and Lamp (2022) (4-6%) and Lehr (2022) (12-14%). The estimate is significantly larger than a pre-trend that could not be detected in this setting. As Gerster and Lamp (2022), I find no evidence that the exemption of the RES had a statistically significant impact on sales which is shown in column (2).

Table 4: The effects of becoming exempt from paying the RES – TWFE

	Electricity procurement	**Sales**	**Employment**	**Value added**	**Domestic material purchases**	**Imports**
	(1)	(2)	(3)	(4)	(5)	(6)
RES_{2007}	0.029	-0.034	-0.014	-0.056		
	(0.043)	(0.073)	(0.039)	(0.063)		
RES_{2008}	0.015	0.002	0.005	-0.022		
	(0.045)	(0.042)	(0.022)	(0.053)		
RES_{2009}	-0.061	-0.044	-0.003	-0.105	-0.098	0.010
	(0.055)	(0.052)	(0.018)	(0.076)	(0.070)	(0.089)
RES_{2010}	-0.025	-0.002	-0.003	-0.035	-0.024	0.088
	(0.031)	(0.017)	(0.013)	(0.044)	(0.022)	(0.067)
RES_{2012}	-0.008	0.060	-0.001	-0.049	0.160	0.040
	(0.028)	(0.067)	(0.006)	(0.044)	(0.135)	(0.087)
RES_{2013}	0.084*	0.085	-0.013	-0.004	0.133	0.114
	(0.043)	(0.068)	(0.016)	(0.041)	(0.133)	(0.091)
			Slope of linear undetectable pretrend			
Power 50	0.013	0.013	0.007	0.018	0.020	0.049
Power 80	0.021	0.021	0.011	0.028	0.030	0.074
N	5,046	5,046	5,046	5,046	3,648	3,648
number firms	755	755	755	755	743	743

Notes: The regressions include observations from 2007–2013, except for when they are taken from the MDL which is only available from 2009–2013. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with 2-digit sector time trends. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** *and* *** *indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

If firms were expanding output and at the same time reducing prices, resulting in an insignificant effect on sales, they should use more input factors such as labour. Column (3) shows that employment remained unaffected. This speaks against treatment firms simply passing on lower electricity prices to their customers. In consequence, the explanation for rising electricity procurement in response to the RES exemption does not lie in an expansion of output, but might be grounded in adjustments along the supply chain, e.g., less outsourcing relative to the control group.

Column (4) shows that value added does not respond significantly to the exemption.[35] If treated firms did in fact outsource less than the untreated controls (be it by sourcing less domestically or internationally), value added of treatment firms would increase relative to the control group. The insignificant estimate on value added does not lend support to treatment firms offshoring less than control firms.

(Aggregate) responses might not be visible if only certain types of intermediate input expenditures respond. (Not) being exempted from the RES might switch the cost differential between making or buying only for some source countries, not for all of them. Given lower fixed cost of offshoring domestically than internationally, e.g., only domestic material inputs might develop differently in response to the RES exemption, not imported ones. Columns (5) and (6) report estimates separately for domestic material purchases and imports. Domestic material purchases are calculated by subtracting imports from total material inputs. As can be seen, neither of the two variables significantly responds to the RES exemption.[36] There is hence no evidence for offshoring as a channel for adjustment of electricity consumption.[37]

[35]With constant output, electricity use and other intermediate inputs, a decrease in electricity price, such as through the exemption from the full RES, leads to an increase in value added by construction. However, as documented in column (1), electricity procurement did in fact not stay constant but increased, which can counteract the mere price effect.

[36]In Table 18 in the Appendix, I further split up import effects according to whether imports originate from other EU-countries or from outside of the EU. For neither of the two, I find a statistically significant effect.

[37]My estimates do however include scale effects. Other studies such as Illanes and Villegas-Sánchez (2022) use import shares as dependent variables, or normalize imports by output values (Ederington and Minier, 2003). This way, a shift in the composition of input factors can directly be estimated, net of any scale effects. I abstain from doing so as using shares as dependent variables comes with other issues, and specifically implicitly assumes a certain relation between explanatory variable, nominator and denominator. With a log ratio as dependent variable, e.g., one implicitly assumes a 1:1 relationship between nominator and denominator. I tested this in a regression and found that this assumption does not hold: Imports do in fact increase by less than 1% if material expenditures increase by 1%. For a thorough discussion of challenges and implicit assumptions when using ratios as dependent variables,

4.2 Robustness

I test the robustness of these results in various ways. Results on electricity procurement and value added are shown in Table 5, and on domestic material purchases and imports in Table 6. Column (1) shows the baseline effects from the previous section.

In column (2), I estimate regressions in log-form instead of PPML. Generally, effects are similar, albeit smaller. The robustness against the functional form chosen provides support for the general comparability of treatment and control group after trimming such that results are not sensitive to minor changes in the specification (Imbens, 2015).

My trimming strategy is based on imputed values and might include measurement error. Column (3) reports results where I additionally trim on the directly observed 2010 electricity cost intensity.[38] This also ensures that I select firms that are electricity-intensive throughout, and not only in 2011. The additional restriction leads to the loss of 49 control firms. Results are qualitatively unaffected by this change. They also do not change substantially when trimming on electricity cost intensity values from 9 to 14%, instead of 8.1% as in the base specification. Results are shown in Tables 21 and 22 in the Appendix.

The main specification included firms that generated electricity onsite, even though electricity generated onsite was not subject to the RES over my observation period. Onsite generating firms had some control over the RES they had to pay. The control group without onsite generators might have reduced electricity procurement by less in response to the rise in the RES, as they could not substitute procurement with own generation. For the treatment group, it is not clear whether onsite generators respond more/less strongly than non-generators; this crucially depends on whether after the exemption applies, onsite generation was still cheaper than electricity procurement. Therefore, effects without onsite generators may be larger or smaller than in the baseline including onsite generators. In column (4), I redo my main analysis, excluding onsite generators both among the treated and controls. This renders the effect on electricity procurement statis-

see Bartlett and Partnoy (2020). While my effect estimate on imports does not rule out the presence of scale effects, simply controlling for variables such as material inputs or sales might raise concerns about bad controls, and I refrain from that approach.

[38]The threshold again is set at the 10th percentile of treatment firms, i.e. at 6.2% in 2010.

Table 5: The effects of becoming exempt from paying the RES: electricity procurement and value added

	Base specification	Logs	Alternative trimming	No onsite generators	CEM	Callaway & Sant'Anna
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: electricity procurement			
RES_{2007}	0.029	-0.008	0.033	0.019	0.171***	
	(0.043)	(0.064)	(0.042)	(0.044)	(0.053)	
RES_{2008}	0.015	-0.080	0.012	-0.016	0.098**	-0.117**
	(0.045)	(0.055)	(0.045)	(0.040)	(0.045)	(0.049)
RES_{2009}	-0.061	-0.052	-0.061	-0.092	-0.077	0.033
	(0.055)	(0.050)	(0.055)	(0.064)	(0.076)	(0.039)
RES_{2010}	-0.025	0.013	-0.031	-0.040**	0.013	0.070
	(0.031)	(0.015)	(0.031)	(0.016)	(0.042)	(0.056)
RES_{2011}	0	0	0	0	0	0.009
						(0.018)
RES_{2012}	-0.008	0.036	-0.010	-0.004	-0.006	0.026
	(0.028)	(0.023)	(0.028)	(0.030)	(0.034)	(0.024)
RES_{2013}	0.084*	0.042*	0.079*	0.011	0.120*	0.024
	(0.043)	(0.024)	(0.044)	(0.027)	(0.065)	(0.020)
robust to linear pre-trend	yes					
robust to non-linear pre-trend	no					
			Panel B: value added			
RES_{2007}	-0.056	0.143*	-0.054	0.023	-0.064	
	(0.063)	(0.080)	(0.070)	(0.064)	(0.083)	
RES_{2008}	-0.022	0.106	-0.004	0.017	0.021	0.023
	(0.053)	(0.070)	(0.054)	(0.056)	(0.064)	(0.057)
RES_{2009}	-0.105	0.051	-0.088	-0.108	-0.108	-0.040
	(0.076)	(0.075)	(0.077)	(0.094)	(0.109)	(0.062)
RES_{2010}	-0.035	0.014	-0.009	-0.064	-0.043	-0.033
	(0.044)	(0.062)	(0.046)	(0.049)	(0.054)	(0.059)
RES_{2011}	0	0	0	0	0	-0.069
						(0.054)
RES_{2012}	-0.049	-0.004	-0.028	0.013	-0.011	0.027
	(0.044)	(0.074)	(0.045)	(0.042)	(0.045)	(0.081)
RES_{2013}	-0.004	0.088	0.010	0.059	0.030	0.036
	(0.041)	(0.065)	(0.042)	(0.041)	(0.047)	(0.064)
robust to linear pre-trend	positive					
robust to non-linear pre-trend	positive with 1%					
N	5,046	5,046	4,720	4,408	2,084	4,429
number firms	755	755	706	661	309	755

*Notes: The regressions include observations from 2007–2013. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with 2-digit sector time trends. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

Table 6: The effects of becoming exempt from paying the RES: domestic material purchases and imports

	Base specification	**Logs Sant'Anna**	**Alternative trimming**	**No onsite generators**	**CEM**	**Callaway &**
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel C: domestic material purchases			
RES_{2009}	-0.098	-0.013	-0.055	-0.043	-0.032	
	(0.070)	(0.049)	(0.049)	(0.054)	(0.059)	
RES_{2010}	-0.024	0.001	-0.022	-0.037	-0.033	-0.002
	(0.022)	(0.039)	(0.023)	(0.027)	(0.029)	(0.040)
RES_{2011}	0	0	0	0	0	0.013
						(0.037)
RES_{2012}	0.160	0.017	0.177	0.023	-0.004	0.038
	(0.135)	(0.037)	(0.143)	(0.029)	(0.026)	(0.038)
RES_{2013}	0.133	-0.042	0.157	0.028	-0.004	-0.018
	(0.133)	(0.045)	(0.139)	(0.034)	(0.035)	(0.038)
robust to linear pre-trend	yes					
robust to non-linear pre-trend	positive with $\geq$1%					
			Panel D: imports			
RES_{2009}	0.010	-0.020	0.012	-0.014	-0.004	
	(0.089)	(0.105)	(0.093)	(0.086)	(0.108)	
RES_{2010}	0.088	0.025	0.095	0.059	-0.005	-0.026
	(0.067)	(0.103)	(0.069)	(0.077)	(0.089)	(0.073)
RES_{2011}	0	0	0	0	0	0.019
						(0.098)
RES_{2012}	0.040	-0.018	0.047	-0.059	-0.096	-0.109
	(0.087)	(0.111)	(0.091)	(0.065)	(0.067)	(0.110)
RES_{2013}	0.114	0.124	0.102	0.031	0.018	0.227*
	(0.091)	(0.098)	(0.095)	(0.082)	(0.094)	(0.119)
robust to linear pre-trend	yes					
robust to non-linear pre-trend	yes					
N	3,612	2,984	3,393	3,171	1,477	3,198
number firms	735	631	692	645	300	735

Notes: The regressions include observations from 2009–2013. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with 2-digit sector time trends. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** *and* *** *indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

tically insignificant.[39] There are three potential reasons for why the effect estimate turns statistically insignificant after the exclusion of onsite generating firms. First, dropping observations given the generally low sample size, especially concerning the number of treatment firms, leads to a loss of power. The signal-to-noise ratio decreases, and outliers have more weight. Second, in the control group, onsite generators were structurally different from the non-generators. Specifically, they tended to display larger electricity use and procurement as well as higher energy cost than the remaining controls.[40] The share of controls operating in the same 4-digit NACE industries as the treatment firms was higher among generators than among non-generators. Dropping them, therefore, renders the control group less similar to the treatment group. The control firms that were electricity-intensive, not subject to the RES exemption, and yet never started to generate electricity onsite might constitute a select sample of firms. Third, the effect vanishing could be grounded in heterogeneous effects. If, e.g., the most electricity-intensive firms are the ones that respond most strongly, and they are also the ones that tend to generate some of their electricity onsite, then effects naturally become smaller without these firms. Alternatively, the main part of the effects that were previously estimated by Gerster and Lamp (2022) and Lehr (2022) does in fact stem from that plants and firms benefiting from the RES exemption increased their onsite generation by less or even decreased it compared to other plants and firms.

Even after trimming, structural differences between treatment and control groups remain, as documented in Table 3. This fact might cast doubt on the parallel trend assumption holding exactly. Motivated by this, I test the robustness of the two-way fixed effect estimates in three more ways: First, in column (5), I apply a matching approach, i.e., coarsened exact matching, matching exactly on 2-digit sectors as well as sector-specific quartiles of electricity procurement and electricity cost intensity. Second, I test whether results hold up when allowing for (slightly non-)linear trends, following the

[39] Gerster and Lamp (2022) perform a similar robustness check. For them, the effect on electricity procurement remains in place after dropping onsite generating manufacturing plants. However, they only drop plants that were generating electricity in the base year 2011, while I also drop all firms that start generating electricity in 2012 or 2013. In that sense, my robustness check is more conservative. More than a third of self-generating control firms, i.e., 200, start to generate electricity onsite in 2012 or 2013. 14 treatment firms start in 2012. No treatment firm switches into onsite generation in 2013.

[40] These differences do not extend to the treatment group in which onsite generators were very similar to the rest of firms.

procedure of Rambachan and Roth (2023). Confidence sets account for the uncertainty in terms of the magnitude of pre-trends. Results are reported underneath column (1). Third, in column (6), I apply the estimator by Callaway and Sant'Anna (2021) that requires parallel trends to hold only conditional on covariate-specific trends. I condition on two-digit sectors, as well as pre-treatment electricity cost intensity in a doubly-robst estimator.[41] The implementation of all three methods is described in more detail in Appendix 10.

Effects on electricity procurement are relatively robust to these modifications. Qualitatively, results on a matched sample are identical for the post-treatment year 2013, even if a statistically significant pre-trend is estimated for the years 2007 and 2008. Allowing treatment and control firms to follow different linear trends does not alter conclusions either. The effect however turns statistically insignificant using the estimator by Callaway and Sant'Anna (2021).[42]

Results on other variables are less robust: Under the assumption of different linear trends, the effects on value added and sales turn positive and statistically significant and remain so even if the post-treatment trend deviates by 1% from the pretrend. This goes along with a statistically significant increase in domestic material purchases under the assumption of a post-treatment trend that deviates from a linear pretrend by 1% or more. Imports do not respond to the RES exemption under any trend assumption. Under the assumption of treatment and control firms following different secular trends, effects seem to be driven rather by treatment firms growing (such that sales, value added and domestic material purchases increase), and less by offshoring (for which the effect on domestic material purchases should move in the opposite direction or at least remain constant). Implementing the estimator by Callaway and Sant'Anna (2021), I get a positive treatment

[41]Note that the estimator is conceptually different from the other robustness checks in that a separate difference-in-differences design is run for each year. I.e., the estimate for RES_{2013} represents the effect from a difference-in-differeces estimator between 2012 and 2013. For that reason, there is no base year, and correspondingly, there is also an effect estimate for 2011, which is the difference in development between treatment and control group between 2010 and 2011.

[42]The point estimate remains positive. This seems to be driven by an outlier observation. Due to the Bavarian legal interpretation of the Federal Statistics Act, generally, data on Bavarian firms cannot be directly accessed by researchers. Without Bavarian firms, effects from using the Callaway and Sant'Anna (2021) estimator are qualitatively identical to the two-way fixed effects estimator, pointing to some outlier firms in Bavaria which could unfortunately not be inspected.

effect on imports in 2013. Becoming exempt from the RES and facing lower electricity prices, treatment firms, if anything, significantly increased their imports as compared to the control group. This again counterdicts concerns about offshoring. The positive effect would rather be in line with treatment firms generally growing due to the electricity price decrease.

Overall, results on electricity procurement, domestic material purchases and value added prove sufficiently robust to changes to the exact identification strategy. The robustness checks do however point to effects potentially being heterogeneous across manufacturing firms, and to the presence of some outlier firms. The difference between a standard two-way fixed effects model and the estimator by Callaway and Sant'Anna (2021) indicates that covariate specific trends play a role in the estimation of treatment effects. Given the structural difference between treatment and control group with respect to electricity cost intensity, results therefore have to be interpreted with caution.

4.3 Heterogeneity analyses

Arguably, changes in electricity prices do not lead to identical supply chain adjustments across all firms. Is there a subset of firms which respond to decreasing electricity prices by offshoring less than similar unaffected firms? I explore heterogeneity along two dimensions: (1) the severity of the price reduction and (2) the ease of adjustment in the make-or-buy decision for different firms. As the two-way fixed effect regression generally yields similar results to more complex identification strategies, as shown in Section 4.2, each heterogeneity analysis is carried out by interacting the treatment-by-time indicator with an additional measure reflecting dimensions (1) or (2).

To operationalize the severity of the price reduction (dimension 1), I use two different measures: First, I use the firms' 2011 electricity cost intensity (i.e., a firm's ratio of electricity expenditures to value added). Firms with a high intensity experience a stronger price decrease – relative to their value added –, and hence might respond more strongly to the exemption. I estimate effects for two groups of firms, one with an electricity intensity above 14.2% in 2010 (which is the median across treatment firms), and one below.[43]

[43]While it would be appealing to define more categories and estimate effects along more moments of the distribution, I refrain from doing so due to the low number of treatment firms. Separating the effect for two groups already leads to effects being estimated on 49 treatment firms only in each group. For

Second, I use a measure of the pre-treatment electricity intensity of the inputs of sectors. Specifically, I measure the average kWh of electricity contained in each Euro of input at the 4-digit sector level in 2010.[44] I again distinguish between firms belonging to sectors below or above the median electricity intensity of inputs of treatment firms in my sample. This median amounts to 1.02 kWh/EUR. The rationale of this measure is the following: Firms in sectors that heavily rely on electricity-intensive inputs might be more ready to offshore any electricity-intensive production processes that are yet conducted in-house when electricity prices rise. The control group might be subject to a stronger trend to offshore, and relative differences to the behaviour of treatment firms correspondingly higher. The measure is similar to the electricity cost intensity at the firm-level to the extent that firms are electricity cost intensive because they produce goods for which they need electricity-intensive intermediate inputs. Yet, the sector-level measure smooths out potential measurement error and manipulations/peculiarities of value added in single years. The measure of sector-level electricity-intensity of inputs is also less dependent on the electricity prices a single firm faces – at the cost of losing variation within 4-digit sectors.

To operationalise the ease of adjustment in the make-or-buy decision (dimension 2), I again use two measures. First, I use a measure of the share of easily tradable inputs in production. Rauch (1999) discusses that for many goods, there are no straightforward international markets (e.g., for electronic components, or combustion engines). Instead, importing of many goods requires the formation of (stable) networks. Consequently, short-run adjustments along the margin of importing might be more likely for firms in sectors that use a lot of easily tradable inputs (such as lead, which is traded, e.g., on the London Metal Exchange). To test this hypothesis, I classify 4-digit sectors according to their pre-treatment (i.e., 2010) share of intermediate inputs coming from sectors in which such international markets exist. These inputs are highly homogeneous and can be traded

the control group, 96 observations (15%) fall in the more electricity-intensive category, and 541 in the less intensive one.

[44]For each 4-digit sector, I calculate the average kWh of electricity necessary to produce one EUR of value added in Germany in 2010. For each 4-digit sector, I then compute the average electricity intensity of intermediate inputs by computing a weighted average of these values, based on the sector's specific input mix in 2010. As electricity expenditures, firm level inputs are only available in 2010, and only for firms with more than 50 employees. In Table 23 in the Appendix, I report the mean electricity intensity of intermediate inputs in different sectors.

internationally quite easily, owing to the existence of international exchanges or reference prices.[45] I use the classification provided by Rauch (1999). Appendix 12.2 provides the list of 4-digit sectors that I classify as "easily tradable". I differentiate effects among firms belonging to 4-digit sectors below or above the median share of easily tradable inputs in treatment firms which constitutes 82%.

Offshoring might also be easier for firms that already maintain a trading network and that are experienced in trading internationally. To check for that, I distinguish between firms that were importers according to the definition by Eurostat pre-treatment, i.e., that imported more than 5,000 EUR in values and had an import intensity exceeding 5% (defined as the share of imports in purchases of goods and services). This way, firms that only occasionally import are not counted as importers.[46]

Table 7 depicts the results. As there are no statistically significant pre-trends, for the ease of exposition, I only depict post-treatment effects. The firms that are particularly electricity-intensive indeed tend to respond more strongly with respect to their electricity procurement, supporting that these firms are heavily affected by the electricity price decrease. Yet, even these firms do not significantly reduce imports or domestic material purchases or increase value added as compared to the control group. The same pattern holds for firms that belong to 4-digit sectors in which intermediates tend to be more electricity-intensive.

Both firms that are regular importers and firms that do not or only occasionally import respond to decreasing electricity prices by increasing electricity procurement – and neither of the two groups responds along any other of the margins I check.

With respect to the ease of tradability of inputs used, it is mostly the firms in sectors using easily tradable goods that respond to the RES exemption by increasing their electricity procurement. This can be explained by the sectors using a high share of easily tradable goods also tending to be more electricity-intensive such as the metals sector (see Table 23 in the Appendix). Contrary to expectation, firms in sectors with a high share of easily tradable inputs do not respond to decreasing electricity prices by offshoring less

[45] Rauch (1999) refers to goods with reference prices as goods that are not branded and for which price information is easily available. One example are certain chemicals whose prices are weekly reported in the "Chemical Marketing Reporter".

[46] Roughly 70% of control firms and 65% of treatment firms are importers according to this definition. Detailed statistics on the importer-share by sector are displayed in Table 23 in the Appendix.

Table 7: Heterogeneous effects of becoming exempt from paying the RES

	Electricity procurement	**Sales**	**Employment**	**Value added**	**Domestic material purchases**	**Imports**
	(1)	(2)	(3)	(4)	(5)	(6)
			Firms with a high vs low electricity cost intensity			
RES_{2013} low intensity	0.050	0.104	-0.021	-0.011	0.235	0.126
	(0.036)	(0.105)	(0.022)	(0.056)	(0.215)	(0.138)
RES_{2013} high intensity	0.112*	0.055	-0.007	0.011	0.020	0.036
	(0.061)	(0.040)	(0.018)	(0.043)	(0.048)	(0.073)
N	4,883	4,883	4,883	4,883	3,529	3,529
number firms	731	731	731	731	719	719
			Firms in sectors with more or less electricity-intensive inputs			
RES_{2013} non intensive inputs	0.042	0.029	-0.004	0.018	-0.007	0.005
	(0.035)	(0.036)	(0.029)	(0.082)	(0.039)	(0.113)
RES_{2013} intensive inputs	0.090*	0.146	-0.018	-0.010	0.358	0.172
	(0.055)	(0.137)	(0.018)	(0.050)	(0.309)	(0.144)
N	4,701	4,701	4,701	4,701	3,438	3,438
number firms	704	704	704	704	697	697
			Firms in sectors with more or less easily tradable inputs			
RES_{2013} non easily tradable	-0.002	0.031	-0.019	0.095	-0.025	0.199***
	(0.033)	(0.044)	(0.026)	(0.065)	(0.043)	(0.068)
RES_{2013} easily tradable	0.106*	0.118	-0.005	-0.050	0.238	0.048
	(0.054)	(0.102)	(0.018)	(0.045)	(0.198)	(0.125)
N	4,701	4,701	4,701	4,701	3,438	3,438
number firms	704	704	704	704	697	697
			Regular importers vs non-/sporadic importers			
RES_{2013} non importer	0.064**	0.055	-0.003	-0.072	0.018	0.109
	(0.031)	(0.038)	(0.025)	(0.072)	(0.051)	(0.135)
RES_{2013} importer	0.089*	0.096	-0.015	0.011	0.192	0.115
	(0.050)	(0.087)	(0.018)	(0.046)	(0.184)	(0.100)
N	5,046	5,046	5,046	5,046	3,648	3,648
number firms	755	755	755	755	742	742

*Notes: The regressions include observations from 2007–2013, except for when they are taken from the MDL which is only available from 2009–2013. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with 2-digit sector time trends. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

than the control group. On the contrary, firms using less easily tradable goods increase their imports relative to the unaffected control group. As the point estimates on sales and value added are positive too, albeit not statistically significant, possibly, these firms are growing and for that reason increase imports.

Overall, none of the heterogeneity checks provides support for offshoring due to rising electricity prices even for certain subgroup of firms.

4.4 Discussion

In section 2.2, I have documented that the share of imported intermediates has increased between 2009 and 2013 in German manufacturing, and that firm-level energy prices correlate positively to imports and domestic material purchases. Yet, studying the response of German manufacturing firms to a substantial, regulation-induced change in electricity prices does not provide evidence that firms respond to an electricity price decrease by offshoring less than an unaffected control group: Neither domestic material purchases nor imports seem to increase by less than in the control group. If anything, I find that treatment firms increase their imports relative to the control group. What explains the difference to the correlation between energy prices and imports? Does offshoring due to energy prices in fact not take place?

There are a couple of potential reasons why I might not identify any offshoring response despite offshoring taking place. First of all, I estimate very short-run effects, as I only observe import-data up to the year 2013, i.e., the first treatment year. While treated firms generally knew already in 2012 about the granting of their application and their future lower electricity prices, and control firms knew at least some months ahead in October about the increase in the RES for the next year, adjustments along the supply chain might take longer to materialize. Firms might exhibit some lag in their response, preventing me from identifying an effect. Gerster and Lamp (2022) and Lehr (2022) both find that responses to the exemption from the RES with respect to electricity procurement tend to become larger over time. Contributing to that, potentially, the relatively strict labour laws in Germany render outsourcing infeasible in the short-run.

Second, owing to data availability, I have been unable to examine the goods that are traded. Arguably, only the very electricity intensive-inputs are offshored less by treatment than by control firms. Only for these goods, the decrease in electricity prices for the exempt firms, and increase in electricity prices for the remaining firms leads to a strong price signal requiring adjustments in the supply chain. The average effects I have estimated might simply mask effects occurring only for specific intermediates. New data that recently have been made available on the exact intermediates imported would allow me to use the variation in electricity-intensity of traded goods for identification in future research.

Third, it is possible that the firms newly exempt from paying the full RES pass on the price decrease they experience to their customers and therefore are not really benefiting from electricity price decreases themselves. My findings do not support that hypothesis, as sales, employment and material inputs do not increase in response to the exemption. If firms indeed increased output due to the tax exemption, and at the same time decreased prices, such that sales remained unaffected, factor inputs should significantly increase; and such an increase could not be identified for employment. Also, Lehr (2022) found no evidence for 100% passthrough. Even if passthrough occurred, newly exempt firms on average use a higher share of intermediates from sectors with many newly exempt firms. In that sense, newly exempt firms should be treated more strongly than the control group even if treatment took place in the form of passthrough rather than the direct exemption. Yet, the degree to which passthrough might explain my findings is not completely clear.

Relatedly, I could not completely rule out scale effects. While the estimates of the exemption scheme on sales are generally statistically insignificant, point estimates are positive throughout. In some specifications, and in some heterogeneity analyses, I found the RES exemption induced decrease in electricity prices to have positive effects on firm-level imports, value added, sales and material purchases. This could be indicative for the exempt firms growing due to the low electricity prices faced. The lack in the response of labour might be grounded in the electricity-intensive firms generally being rather capital, and less labour-intensive.

Fourth, I have studied offshoring very specifically in the context of a make-or-buy decision. I have studied whether electricity price changes lead to firms buying intermediate inputs that previously were produced in-house. It is possible, however, that trade adjustments to rising energy prices first lead to a shift in firms' existing sourcing strategies – i.e., before switching between making or buying, firms start to import intermediates they used to source domestically before. This margin will be subject to future analyses.

Fifth, I have not controlled for a potential change in the product composition of firms. Bernard *et al.* (2011) have noted (– with a focus on the effects of trade policies and on exporting, but the same logic applies here –) that policies that lead to changes in product composition thereby also affect firm-level trade behaviour. If the exemption from the RES makes firms switch products they produce, this switch also might lead to adjustments of import values. Rottner and von Graevenitz (2021) have documented that in German industry, the production composition changed substantially between 2005 and 2017. Against this background, it cannot be ruled out that RES induced changes in firm-level product mixes play a role in explaining my findings. In future work, I will analyse the effect of the RES on firms' product switching and thereby assess the extent to which this channel matters for imports.

Lastly, the relatively few observations on which the analyses are based set limits in terms of power. Again, the new data available as of this summer provide a remedy against such concerns.

Alternatively, indeed, energy prices might not have led to offshoring in the past. This is also plausible if goods produced in-house are just no close substitutes to inputs produced abroad. Halpern *et al.* (2015) estimate that imperfect substitutability between domestic and foreign inputs play a large role in the import behaviour of Hungarian firms. Also, Martin *et al.* (2014b) found evidence of only a moderate risk of offshoring, conducting telephone interviews with managers of 761 manufacturing firms in six European countries. In all countries and in most industries studied, firms reported an average risk of outsourcing below 10 percent. If this is the case, the question remains how control firms have managed to reduce their electricity procurement by more than the treatment group, without following significantly different paths with respect to outcomes such as sales or employment. Different trends of firms with respect to electrification efforts and expansion of onsite generation might be candidates for an explanation, even though neither Gerster and Lamp (2022) nor Lehr (2022) found any effects of the exemption on investments.

5 Conclusion

Increasingly stringent unilateral climate policies and rising energy prices have fueled debates about industrial firms offshoring and re-locating. Using a data set on firm-level imports, in this paper, I have documented that between 2009 and 2013, the share of imported intermediates has increased in German manufacturing. Also, in within firm regressions, energy prices correlate positively with imports. Motivated by these facts, I have exploited a tax exemption for electricity-intensive firms to provide causal evidence on how firms respond to a substantial decrease in electricity prices. Contrary to expectations, I do not find evidence that firms subject to decreasing electricity prices offshored less than a group of similar control firms that experienced increasing electricity prices.

Even among the electricity-intensive users subject to the tax exemption scheme, firms differ in terms of the importance of electricity prices in production, and in terms of their ability to offshore. I have explored these dimensions by conducting various heterogeneity analyses. I neither find evidence for offshoring in response to rising electricity prices for the most electricity-intensive firms. Nor do I find a stronger response for sectors that use a high share of easily tradable intermediates in their input mix. This too speaks against offshoring of single production processes in response to rising energy prices being a major cause for concern. Concerns about production shifts abroad seem exaggerated in the public debate.

Studying aggregate trade flows is not necessarily meaningful to study carbon leakage. Even if I could not identify offshoring responses in terms of trade values, the emission-intensity of imports could respond in a way to induce carbon leakage. Firms might not increase import values in response to rising electricity prices, but the emission-intensity of imports. This could be the case if the optimal sourcing strategy is adjusted to rising energy prices by importing more energy-intensive intermediates, and reducing imports of less energy-intensive inputs. The present paper could not analyse this dimension as information on the exact goods imported was not available. This questions is left to future research.

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Appendix

6 Additional data details

6.1 Micro-data used

The analyses in this paper are based on several modules of the official micro-data from the Statistical Offices of Germany. Specifically, information on electricity use, electricity onsite generation, energy use, etc. are taken from the energy use module of the German Manufacturing Census (DOIs 10.21242/43531.2007.00.03.1.1.0 – 10.21242/43531.2013.00.03.1.1.0).

Information on sales and employment are from the monthly reports (DOIs 10.21242/42111.2007.00.01.1.1.0 – 10.21242/42111.2013.00.01.1.1.0).

Data on the cost structure of industrial firms as well as their value added is from the cost structure survey (DOIs 10.21242/42251.2097.00.01.1.1.0 – 10.21242/42251.2013.00.01.1.1.0).

Lastly, import data are from the Micro Data Linking-Panel (MDL) (DOIs 10.21242/48121.2009.00.05.1.1.0 – 10.21242/48121.2013.00.05.1.1.0).

The data structure for the construction of the sample is graphically depicted in Figure 6.

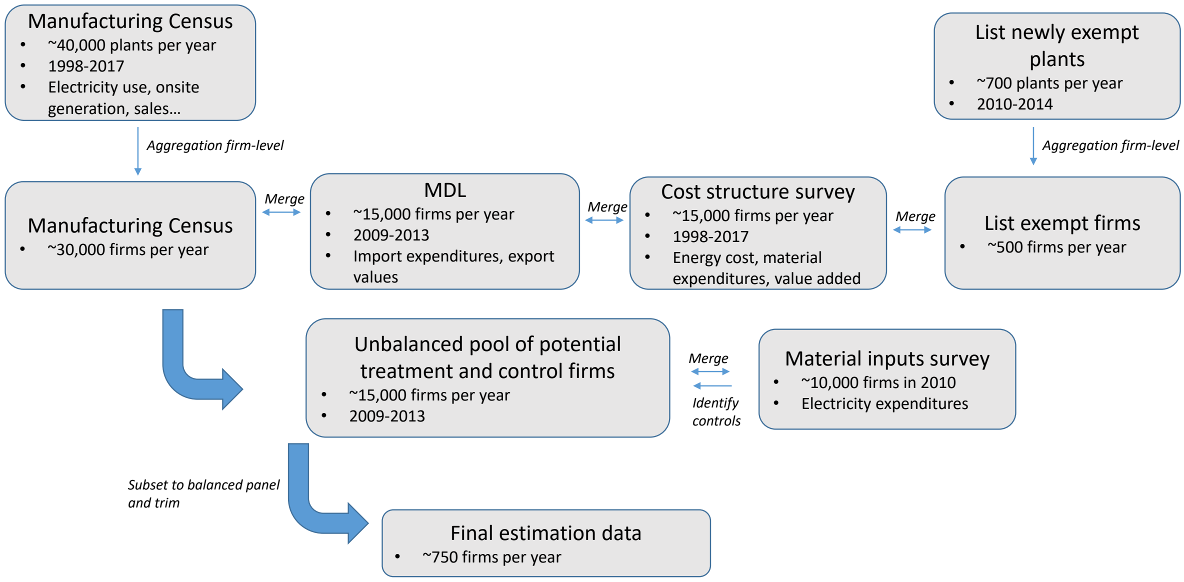

Figure 6: Structure of the data used

6.2 Data cleaning

While the reporting in the different modules of the Manufacturing Census and the MDL are checked by the Statistical Offices for plausibility, inevitably, some errors remain. This section gives an overview over the data cleaning steps I conduct.

I drop

- firms with an unreasonably large variation in fuel use. I define these as firms in which the standard deviation within firm is 75 times larger than the median. This leads to the loss of 83 firms, among which there are 3 treatment firms.
- firms with an unreasonably large variation in electricity procurement. I define these as firms in which the standard deviation within firm is 75 times larger than the median. This leads to the loss of 127 firms, among which there is no treatment firm.
- firms with an electricity procurement of 1 kWh. This concerns 1,011 firms in total, among which there are less than 3 treatment firms.
- firms that report generating electricity with fossil fuels, but report no fossil fuel usage. I drop 36 control firms because of this, and no treatment firm.

- firms for whom I calculate a negative total energy use, i.e. that sell more energy than they buy and/or generate. This concerns 38 control firms.

Additionally, I replace some entries by missings. Specifically, I replace imports by missings if they are below 10 EUR in a given year, but not 0. When imports exceed total firm-level material expenditures, I replace both imports and domestic material expenditures by missings. Most likely, these differences originate in the Federal Statistical Office redistributing trade values from the level of fiscal unities onto the member firms by revenue share. Lastly, I replace implausible outlier values by missings in case of energy prices and electricity prices (i.e., firms paying less than 2.8/1.5 or more than 64.2/97 cents per kWh of electricity or energy, respectively).

6.3 Imputation of electricity cost intensity in 2011

As information on electricity expenditures is only available for the year 2010, I have to impute 2011 electricity expenditures to measure the electricity cost intensity in 2011. To do so, I calculate the average electricity price firms paid in 2010.[47] I then approximate electricity prices applying in 2011 by adding the change in the RES between 2010 and 2011. Electricity expenditures are then calculated by multiplying the imputed 2011 price with a firm's 2011 electricity procurement.[48] With these imputed electricity expenditures, I can calculate a firm's electricity cost intensity in 2011.

I drop firms as controls that fall outside the 1st to 99th percentile range with respect to their variation in electricity cost intensity between 2010 and 2011. That amounts to firms that de- or increase their electricity cost intensity over consecutive years by more than minus 16 or plus 14 percentage points. Some of these cases are apparent reporting errors (e.g., firms report electricity procurement in kWh in one year and MWh in another); in other cases, there are structural changes in firms (e.g., firms change their industrial

[47] I replace electricity prices as missing for outliers outside the 1st to 99th percentile range, i.e. for firms that report electricity prices below 2.8 or above 64.2 cents per kWh.

[48] Figure 3 shows that on average, between 2010 and 2011 industrial electricity prices were rather constant except for the change in the RES, lending credibility to this procedure. Checking the Eurostat time series nrg_pc_205, I confirm that for industrial users with more than 1 GWh electricity use, electricity prices net of taxes only changed by 1% on average between 2010 and 2011. The price including taxes and levies in contrast increased by 12% on average, such that accounting for the change in the RES likely captures most of the electricity price change over the two years.

sector, or apparently go bankrupt).[49] By not using these firms as controls in the analysis, I achieve two things: First, I increase the signal-to-noise ratio by excluding firms that are on very specific paths due to structural changes. This is especially relevant given my small sample size: In the balanced panel, I have 124 treatment firms. Second, I improve comparability of treatment and control firms by ensuring that the sample of control firms is continuously electricity-intensive, and not only in one specific year. For robustness, I additionally trim the sample on both 2010 and 2011 electricity cost intensity.

7 Additional descriptive statistics

7.1 Comparing trade data in the MDL to aggregate trade statistics

Table 8: Share of MLD imports from total German imports of intermediates

	total imports MDL (million EUR)	total imports intermediates (million EUR)	%
2009	141,741	195,277	73
2010	186,204	247,159	75
2011	209,506	278,913	75
2012	211,164	261,084	81
2013	205,037	256,813	80

Source: Own calculations, based on the MDL and Genesis Table 51000-0054 "Imports and Exports (Volume, Indices) (Foreign Trade): Main Industrial Groupings"

7.2 Coverage in the balanced MDL panel as compared to the full MDL

[49] Most of the variation in electricity cost intensity stems from the denominator variable, i.e. value added which exhibits strong variation: Firms at the 1st/99th percentile of the distribution change their value added by -134% and +63% between 2010 and 2011, often without an accompanying adjustment in electricity use.

Table 9: Share of different variables covered by the balanced panel of MDL firms in 2013

	%
Electricity consumption	74
Electricity procurement	73
Energy use	83
Energy cost	75
Employees	67
Sales	77
Imports	80
Exports	76

Notes: Own calculations. The ratios reflect the share of a given variable that is contained in the balanced panel of firms (roughly 20% of firms) as compared to the full MDL panel in 2013.

7.3 Developments of imports and import shares over time

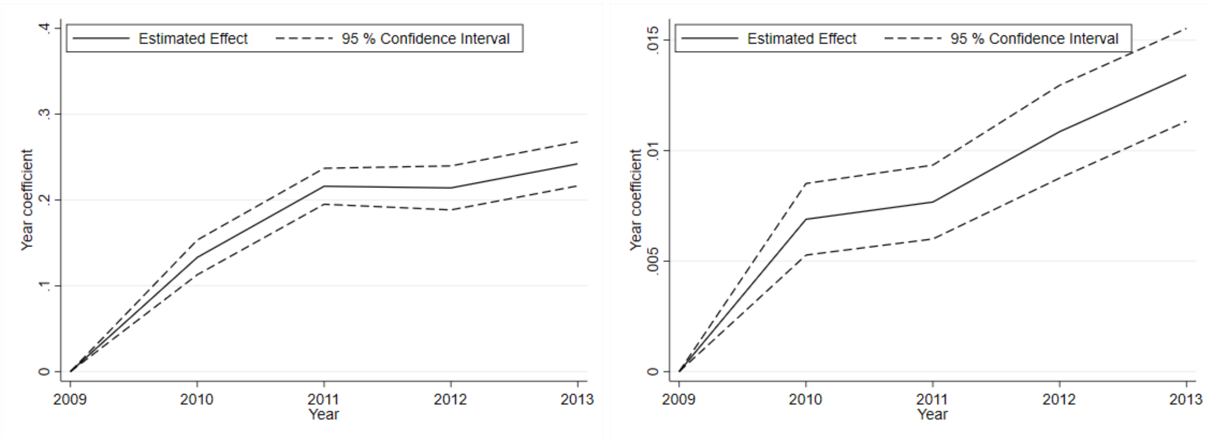

Figure 7: Development of imports (left) and import shares (right) relative to 2009

Notes: Source: Own calculations, based on the MLD data. The Figure depicts the results from estimating equation (1)*, additionally controlling for the wage to sales ratio.*

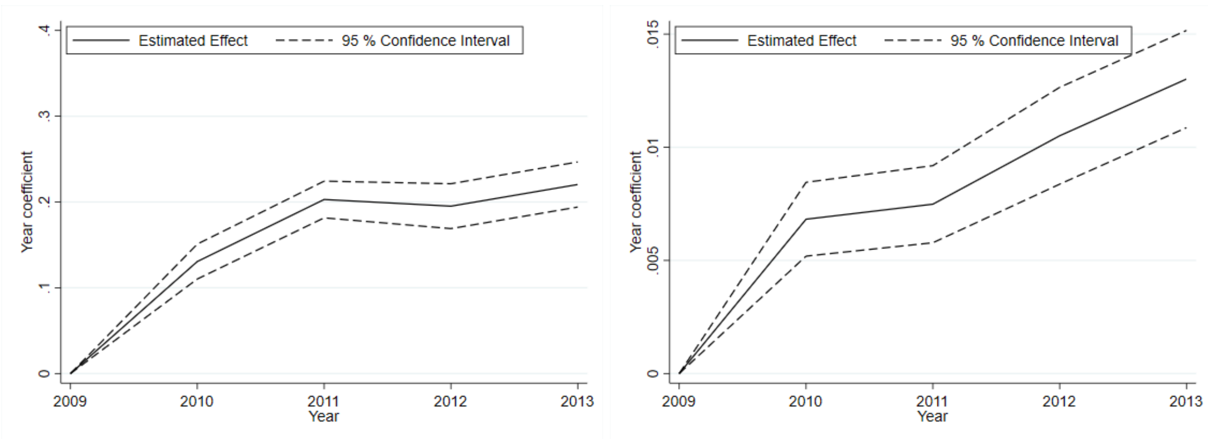

Figure 8: Development of imports (left) and import shares (right) relative to 2009

Notes: Source: Own calculations, based on the MLD data. The Figure depicts the results from estimating equation (1)*, additionally controlling for the wage to sales ratio as well as the log capital to employment ratio.*

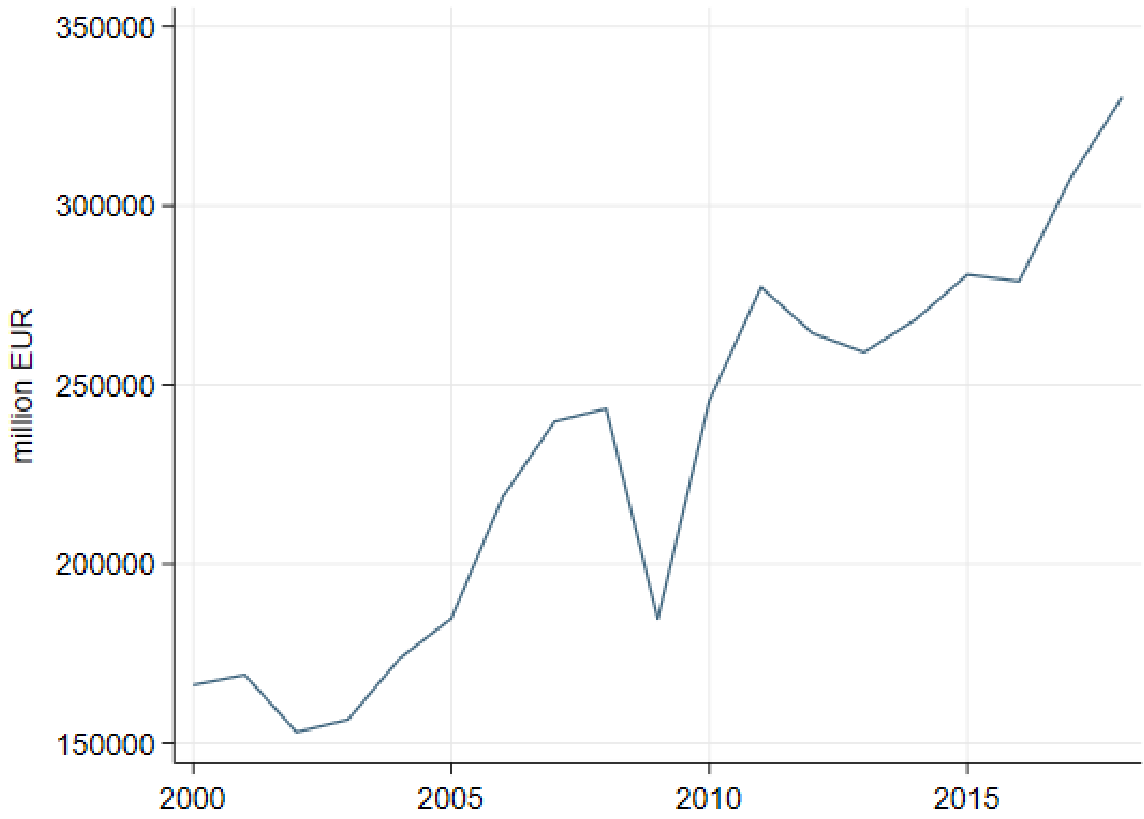

Figure 9: Development of aggregate imports of intermediate goods between 2000 and 2018 in Germany

Notes: Source: Eurostat: EU trade 1988-2022 by CPA 2008 for intermediate goods and Germany

7.4 The relation between firm-level energy prices and domestic material purchases and imports

Table 10: The relationship between firm-level energy prices and imports

	log(imports)			
	(1)	(2)	(3)	(4)
Energy price	0.034**	0.034**	0.033*	0.029*
	(0.017)	(0.017)	(0.017)	(0.017)
Sector FEs	2-digit	2-digit	2-digit	4-digit
Wage controls	NO	YES	YES	YES
Federal-state by year FEs	NO	NO	YES	YES
N	62,418	62,384	62,384	62,362
number firms	20,437	20,423	20,423	20,418

*Notes: Based on the unbalanced panel of the intersection between MDL and German Manufacturing Census. The regression contains observations from 2009 to 2013. Observations outside the 1st to 99th percentile range with respect to energy prices are excluded as outliers. This concerns firms with an average energy price below 1.5 of above 97 cents per kWh. The dependent variable is the log of firm-level imports. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.*

Table 11: The relationship between firm-level energy prices and domestic material purchases

	log(domestic material purchases)			
	(1)	(2)	(3)	(4)
Energy price	0.023***	0.023***	0.022**	0.022**
	(0.009)	(0.009)	(0.009)	(0.009)
Sector FEs	2-digit	2-digit	2-digit	4-digit
Wage controls	NO	YES	YES	YES
Federal-state by year FEs	NO	NO	YES	YES
N	70,260	70,224	70,224	70,205
number firms	23,073	23,060	23,060	23,056

Notes: Based on the MDL and the German Manufacturing Census. The regression contains observations from 2009 to 2013. Observations outside the 1st to 99th percentile range with respect to energy prices are excluded as outliers. This concerns firms with an average energy price below 1.5 of above 97 cents per kWh. The dependent variable is the log of firm-level domestic material purchases. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** *and* *** *indicate significance at 10%, 5% and 1%, respectively.*

8 Balance tables and matching

8.1 Standardized differences before and after trimming

Imbens (2015) suggests to calculate standardized differences instead of conducting t-tests, as t-tests might lead researchers to conclude treatment and control groups are statistically different from each other simply because of a large sample, while actually differences are substantively small. Standardized differences are calculated in the following way:

$$\Delta_{xk} = \frac{\bar{X}_{tk} - \bar{X}_{ck}}{\sqrt{(S^2_{Xtk} + S^2_{Xck})/2}}$$

where k indices a certain covariate of the vector X, $\bar{X}$ constitutes averages among treatments t and controls c and S the standard deviation.

Table 12: Standardized differences between newly exempt and other firms in 2011

	Rest of firms	**Newly exempt firms**	Standardized difference
	Mean	Mean	
Electricity consumption (GWh)	7.5	27.7	-0.17
Electricity procurement (GWh)	6.6	25.5	-0.19
Energy use (GWh)	25	124	-0.12
Electricity cost from value added (in 2010, in %)	5	18	-0.91
Electricity cost from value added (imputed, in %)	5	12	-0.8
Energy cost from value added (in %)	8	31	-1.76
Energy cost (mio.)	1.4	3.7	-0.17
Employees	233	160	0.06
Sales (mio.)	77.7	71.8	0.01
Imports (mio.)	12.8	9.3	0.03
Domestic material purchases (mio.)	32.5	42.2	-0.02
Import share from materials (%)	18.5	18.1	0.02

Notes: Own calculations, based on the MDL and the Manufacturing Census.The table reports standardized differences as in Imbens (2015) for 2011 before trimming. Statistics are included for the balanced panel of firms. Firms that own at least one newly exempt plant constitute the treatment group.

Table 13: Standardized differences between newly exempt and other firms in 2011 in the trimmed sample

	Rest of firms	**Newly exempt firms**	Standardized difference
	Mean	Mean	
Electricity consumption (GWh)	29.2	40.7	-0.09
Electricity procurement (GWh)	28.2	35.4	-0.07
Energy use (GWh)	138	257	-0.09
Electricity cost from value added (in 2010, in %)	10.9	16.6	-0.47
Electricity cost from value added (imputed, in %)	12.9	19.0	-0.27
Energy cost from value added (in %)	20.9	31.1	-0.67
Energy cost (mio.)	5.3	5.9	-0.03
Employees	330	256	0.16
Sales (mio.)	120	131	-0.03
Imports (mio.)	18.5	17.3	0.02
Domestic material purchases (mio.)	56.6	80.4	-0.10
Import share from materials (%)	23.5	19.6	0.18

Notes: Own calculations, based on the MDL and the Manufacturing Census. The table reports standardized differences as in Imbens (2015) for 2011 on the balanced and trimmed sample. Trimming is implemented by dropping control firms with less then 1 GWh electricity procurement in 2011. Firms that own at least one newly exempt plant constitute the treatment group.

8.2 Balance if only trimming on electricity procurement

Table 14: Differences between newly exempt and other firms in 2011

	Rest of firms		Newly exempt firms		Difference
	Mean	Count	Mean	Count	
Electricity consumption (GWh)	21.0	3,161	40.7	124	-19.7
Electricity procurement (GWh)	17.1	3,161	35.4	124	-18.3
Energy use (GWh)	76.8	3,161	257.0	124	-180.2
Electricity cost from value added (in 2010, %)	4.9	2,971	16.6	98	-11.7***
Electricity cost from value added (imputed, %)	5.5	2,971	19.0	97	-13.5***
Energy cost from value added (%)	9.8	3,145	31.1	113	-21.3***
Energy cost (mio.)	3.6	3,161	5.9	124	-2.3
Employees	606	3,161	256	124	350***
Sales (mio.)	235	3,161	131	124	104*
Imports (mio.)	40.8	3,032	17.3	120	23.5***
Domestic material purchases (mio.)	105	3,032	80.4	120	24.6
Import share from materials (%)	25.2	3,032	19.6	120	5.6***

*Notes: Own calculations, based on the MDL and the Manufacturing Census. The table reports results from t-tests for 2011 on the balanced and trimmed sample. Firms that own at least one newly exempt plant constitute the treatment group. *, ** and *** indicate significance of differences at 10%, 5% and 1%, respectively.*

8.3 Balance after CEM in the trimmed sample

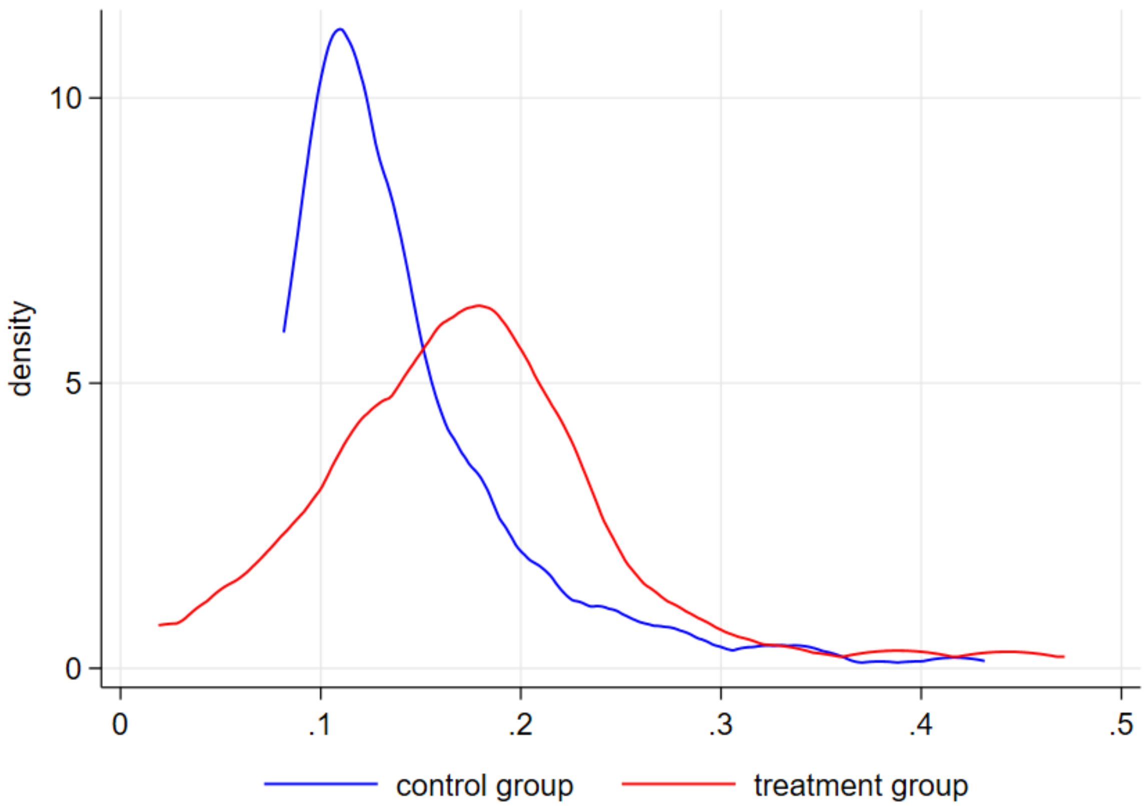

Figure 10: Distribution of electricity intensity in treatment and control firms in 2011 after CEM

Notes: Own calculations.

Table 15: Differences between newly exempt and other firms in 2011 after CEM matching

	Rest of firms		**Newly exempt firms**		Difference
	Mean	Count	Mean	Count	
Electricity consumption (GWh)	40.8	225	31.2	84	9.6
Electricity procurement (GWh)	40.1	225	31.0	84	9.1
Energy use (GWh)	248	225	102	84	146
Electricity cost from value added (in 2010, in %)	12.3	225	16.4	85	-4.1**
Electricity cost from value added (imputed, in %)	14.1	225	20.9	85	-6.8***
Energy cost from value added (in %)	23.8	222	28.4	80	-4.6**
Energy cost (mio.)	7.8	225	5.3	85	2.5
Employees	370	225	294	85	76
Sales (mio.)	173	225	123	85	50
Imports (mio.)	26.0	216	14.1	81	11.9
Domestic material purchases (mio.)	88.2	216	65.4	81	22.8
Import share from materials (%)	20.2	216	18.5	81	1.7

Notes: Own calculations, based on the MDL and the Manufacturing Census. The table reports results from t-tests for 2011. Firms that own at least one newly exempt plant constitute the treatment group. *, ** *and* *** *indicate significance of differences at 10%, 5% and 1%, respectively.*

9 RES regulation

9.1 Regulatory details

Table 16: The change in regulation through the EEG amendment in 2012

	Before 2012		After 2012	
	partial exemption	full exemption	partial exemption	full exemption
Minimum electricity procurement	10 GWh	100 GWh	1 GWh	100 GWh
Minimum electricity intensity	15%	20%	14%	20%
Reduced rate	full RES on 10%	0.05 Ct/kWh	full RES on first GWh	0.05 Ct/kWh
	0.05 Ct/kWh on rest		10% of RES on the next 9 GWh	
			1% of RES on the next 90 GWh	
			0.05 Ct/kWh on rest	
Self generated electricity	exempt		exempt if generation installation spatially linked	

Notes: The Table summarises the regulation for exemption from paying the full RES prior to and after the amendment of the Renewable Energy Law (EEG) from 2012. Information are taken from the EEG amendment 2012, and the preceding version from 2009.

10 Details on empirical methods

10.1 Two-way fixed effects versus new difference-in-differences methods

In light of the growing focus on these new difference-in-differences methods, it is worthwhile to point out why the criticisms to two-way fixed effects estimation do not apply in my case. In my application, the three conditions outlined by de Chaisemartin and D'Haultfoeuille (2022) for the unbiasedness of β in a two-way fixed effects setup are satisfied: First, treatment is arguably binary. Firms are or are not exempt from paying the full RES. While the exemptions are granted on the plant-level which could introduce treatment intensity, in the vast majority of cases all (or none of the) plants of a firm are subject to the exemption scheme (also owing to the fact that roughly 67% of treatment firms are single-plant firms).[50] Also, as the RES was a national regulation, the exact

[50] In 2013, more than 75% of treatment firms had a treatment intensity of 1, i.e. all plants within the firm were subject to the exemption. On average, the share of exempt plants in treatment firms was 94%. This is in line with results by Gerster and Lamp (2022) who find that a plant is more likely to be exempt from paying the full RES if other plants within the same firm are also subject to the exemption, pointing to the importance of firm-level barriers for applying to the exemption. Conversely, if firms have built up the expertise of applying for exemption, they do so for all plants.

same rules applied to all firms. Second, the design is staggered, i.e. firms can switch in, but not out of treatment. While this is not generally true for the exemption scheme, this simply follows from the fact that I observe one post-treatment period (2013) only. Third, there is no variation in treatment timing. This again follows from the fact I am studying the regulatory change through the 2012 amendment of the EEG and its effects on the newly exempt firms, and abstain from estimating general effects of exemption from the RES over time.

10.2 Details on robustness checks

Researchers frequently support the parallel trends assumption by referring to pre-treatment balance in observable covariates. If treatment and control groups are similar in terms of observables, it is more likely they are also similar in terms of unobservables, so the logic. As shown in Table 3, even after trimming, some imbalances in 2011 remain. Motivated by this fact, I test the robustness of the two-way fixed effects estimator in three ways.

First, I apply coarsened exact matching (CEM). While propensity score matching has been the prevalent matching method for many years, it may result in increased imbalance, model dependence and bias (King and Nielsen, 2019).[51] CEM in contrast by design guarantees a reduction in imbalance to the extent specified ex-ante. The basic idea of CEM is to define for each matching variable ("coarsened") ranges of values on which exact matching is performed. CEM hence returns a set of strata in which units have the same coarsened values of matching variables. The process results in eliminating imbalances also in terms of multivariate nonlinearities or interactions beyond the chosen level of coarsening. Differences remain only within coarsened strata. For a more thorough discussion of the properties of CEM, see Iacus *et al.* (2011) and Iacus *et al.* (2012).

I match exactly on two digit sectors. Additionally, I match on sector-specific quartiles of electricity use and electricity cost intensity.[52] In the process, 40 treatment firms drop

[51] Intuitively, the reduction in dimensions in form of the propensity score means that balancing on said score does not balance the single matching variables. Indeed, preparatory analyses showed that in my application, even estimating a propensity score on two variables only (electricity use and electricity cost intensity) yields a high share of matches in which treated and control units are not similar in any of the two dimensions (with almost identical propensity score, however). Propensity score matching also did not increase balance in my case.

[52] Balance tables for the matched sample are shown in Table 15 in the Appendix.

out. This is not because no suitable match would exist for these treatment firms, but because information on electricity cost intensity is missing: Electricity expenditure data are not collected for firms with less than 50 employees. Given the treatment status of these firms, electricity cost intensity had to be above 14% in 2011, but it is not clear by how much which prevents matching for these treatment firms. It is important to note, therefore, that the estimate from the CEM regression is a local ATT for firms with more than 50 employees. After matching, I estimate equation (3) on the matched sample. I also implement CEM by including an additional stratum-by-sector-by-time fixed effect in equation (3).

Second, I check for robustness of results against (slightly non-)linear deviations from parallel trends following Rambachan and Roth (2023). Effectively, I check whether allowing for different secular trends between treatment and control group changes conclusions. This approach offers an advantage over pruning the sample via matching: Roth (2022) shows that conditioning the analysis on passing a pretest can lead to large biases in estimated effects, if treatment and control groups truly follow different trends. In many cases, this "pretest" bias exacerbates the unconditional bias from the underlying difference in trends. Intuitively, the controls that survive a pretest constitute a selected sample from the true data-generating process. Choosing the controls for which the pre-trend is not visible does not make the fundamentally different trends go away, and instead introduces an additional bias. The approach by Rambachan and Roth (2023) avoids testing whether pre-trends are zero altogether, and develops confidence sets that account for the uncertainty in terms of the magnitude of pre-treatment trends. Using this approach, I can show whether allowing for linear pre-trends changes conclusions, and by how much the post-treatment trends would have to differ from pre-trends for specific effects to break down.

Third, I apply the estimator by Callaway and Sant'Anna (2021) instead of the standard two-way fixed effects estimator. This allows to test robustness against a more flexible specification: In contrast to the two-way fixed effect estimator, the estimator of Callaway and Sant'Anna (2021) allows for covariate-specific trends and does not restrict heterogeneity with respect to observables. Given the differences in the distributions between treatment and control group with regards to electricity cost intensity after trimming, I implement the Callaway and Sant'Anna (2021) estimator by conditioning not only on sectors (equivalent to the sector-by-year fixed effect in the two-way fixed effects estimation), but also on (log) 2010 pre-treatment electricity cost intensity. The approach estimates all single feasible difference-in-differences designs in the sample over all periods (the so-called "group-time average treatment effects") and aggregates them. I estimate these difference-in-differences designs using the doubly-robust estimator introduced by Sant'Anna and Zhao (2020). The doubly-robust estimator offers consistency in including covariates into the model, if either the working model for the propensity score or the working model for the outcome evolution for the control group is correctly specified.

11 Additional results

11.1 The development of value added in treatment and control group

Table 17: The development of value added

	Control group	**Treatment group**
	(1)	(2)
2008	-1.76e+07	-9825575
	(1.28e+07)	(8199045)
2009	-2.76e+07**	-1.44e+07*
	(1.27e+07)	(8137002)
2010	-1.56e+07	-1.07e+07
	(1.27e+07)	(8137002)
2011	-1.13e+07	-1.07e+07
	(1.27e+07)	(8137002)
2012	-1.25e+07	-1.13e+07
	(1.27e+07)	(8137002)
2013	-1.48e+07	-1.08e+07
	(1.27e+07)	(8149135)
N	21,306	800

*Notes: Own calculations, based on the Manufacturing Census. Column (1) regresses value added on year dummies for the control group (before trimming). Column (2) performs the same regression on the treatment group. The regressions include observations from 2007–2013. 2007 constitutes the base year. Regressions are estimated with OLS. All regressions are run with 2-digit sector fixed effects. Standard errors are displayed in parentheses. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

11.2 The effect of the RES exemption on import from different regions

Table 18: The effects of becoming exempt from paying the RES on imports from different regions

	Imports EU	**Imports non EU**
	(1)	(2)
RES_{2012}	0.056	-0.242
	(0.081)	(0.197)
RES_{2013}	0.126	-0.042
	(0.087)	(0.178)
	Slope of linear undetectable pretrend	
Power 50	0.159	0.387
Power 80	0.228	0.553
N	2,187	1,983
number firms	729	664

*Notes: The regressions include observations from 2011–2013. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with 2-digit sector time trends. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

11.3 The effect of the RES exemption estimated with CEM

Table 19: The effects of becoming exempt from paying the RES – CEM

	Electricity procurement	**Value added**	**Domestic material purchases**	**Imports**
	(1)	(2)	(3)	(4)
RES_{2007}	0.188***	-0.012		
	(0.034)	(0.055)		
RES_{2008}	0.114***	0.066		
	(0.026)	(0.066)		
RES_{2009}	-0.061	-0.080	-0.054	0.128
	(0.075)	(0.110)	(0.039)	(0.099)
RES_{2010}	0.019	0.006	-0.059***	0.111
	(0.018)	(0.051)	(0.019)	(0.087)
RES_{2012}	-0.011	0.009	-0.002	-0.148**
	(0.031)	(0.037)	(0.024)	(0.073)
RES_{2013}	0.126*	0.059	-0.007	-0.005
	(0.067)	(0.060)	(0.02)	(0.114)
N	2,070	2,019	1,489	1,475
number firms	309	309	303	300

Notes: The regressions include observations from 2007–2013, except for when they are taken from the MDL which is only available from 2009–2013. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with stratum-by-sector-by-year fixed effects. Standard errors are clustered at the firm-level and at the stratum-by-sector level and displayed in parentheses. *, ** *and* *** *indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

11.4 Restricting the control group to 4-digit sectors with at least one treatment firm

Table 20: The effects of becoming exempt from paying the RES – identical 4-digit sectors

	Electricity procurement	**Value added**	**Domestic material purchases**	**Imports**
	(1)	(2)	(3)	(4)
RES_{2007}	0.037	-0.030		
	(0.044)	(0.057)		
RES_{2008}	0.021	-0.026		
	(0.044)	(0.060)		
RES_{2009}	-0.060	-0.104	-0.108	0.012
	(0.058)	(0.088)	(0.073)	(0.104)
RES_{2010}	-0.032	-0.015	-0.032	0.060
	(0.034)	(0.050)	(0.023)	(0.075)
RES_{2012}	-0.019	-0.025	0.179	0.031
	(0.026)	(0.048)	(0.148)	(0.103)
RES_{2013}	0.089*	0.005	0.161	0.099
	(0.049)	(0.044)	(0.145)	(0.110)
N	3,378	3,305	2,457	2,426
number firms	510	510	503	496

*Notes: The regressions include observations from 2007–2013, except for when they are taken from the MDL which is only available from 2009–2013. The control group is restricted to only contain firms in 4-digit sectors in which there is at least one treatment firm. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with sector-by-year fixed effects. Standard errors are clustered at the firm-level and at the stratum-by-sector level and displayed in parentheses. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

11.5 Trimming on electricity cost intensity at different thresholds

Tables 21 and 22 show results from running the two-way fixed effects regression on different dependent variables, when the sample of control firms is trimmed not at 8.1% of electricity cost intensity in 2011 (which is the 10th percentile of treatment firms), but at higher values up to 14% (which constitutes the official eligibility criterion).

Trimming at higher values leads to a loss of observations. It also renders control firms increasingly larger, with higher electricity and energy use numbers. When trimming at values of 12% or higher, the electricity cost intensity measures are balanced between treatment and control firms. However, at the same time, the control firms become significantly larger than treatment firms with respect to employee numbers, among others.

As shown in the Table, generally, results hold up to the various thresholds for trimming: There are no significant effects of the RES exemption on imports or domestic material purchases, while the exemption scheme leads firms to increase their electricity procurement. Depending on the trimming, however, treatment firms differ from control firms in their 2007 electricity procurement. There is at least suggestive evidence that treatment firms tended to be more affected by the economic crisis in 2009, when they decreased value added by more than the control firms.

Table 21: The effects of becoming exempt from paying the RES – different electricity cost intensity thresholds for trimming

	9%	**10%**	**11%**	**12%**	**13%**	**14%**
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: electricity procurement						
RES_{2007}	0.036	0.060	0.076**	0.082*	0.076	0.096*
	(0.043)	(0.043)	(0.045)	(0.047)	(0.051)	(0.053)
RES_{2008}	0.016	0.021	0.025	0.050	0.055	0.070
	(0.046)	(0.047)	(0.050)	(0.045)	(0.047)	(0.048)
RES_{2009}	-0.059	-0.058	-0.064	-0.063	-0.062	-0.062
	(0.056)	(0.056)	(0.056)	(0.058)	(0.060)	(0.063)
RES_{2010}	-0.024	-0.018	-0.015	-0.013	-0.022	-0.019
	(0.032)	(0.034)	(0.036)	(0.038)	(0.040)	(0.042)
RES_{2012}	-0.007	-0.005	-0.002	-0.001	-0.002	-0.000
	(0.029)	(0.029)	(0.030)	(0.032)	(0.033)	(0.034)
RES_{2013}	0.083*	0.089*	0.091*	0.104*	0.109*	0.117*
	(0.045)	(0.050)	(0.055)	(0.058)	(0.063)	(0.068)
Panel B: value added						
RES_{2007}	-0.067	-0.041	-0.050	-0.069	-0.098	-0.111
	(0.067)	(0.055)	(0.063)	(0.067)	(0.075)	(0.083)
RES_{2008}	-0.037	-0.051	-0.065	-0.078	-0.091	-0.084
	(0.056)	(0.063)	(0.069)	(0.070)	(0.077)	(0.086)
RES_{2009}	-0.122	-0.136*	-0.158*	-0.175**	-0.188*	-0.182*
	(0.077)	(0.080)	(0.086)	(0.088)	(0.097)	(0.107)
RES_{2010}	-0.045	-0.044	-0.047	-0.062	-0.091*	-0.088
	(0.045)	(0.048)	(0.050)	(0.049)	(0.055)	(0.060)
RES_{2012}	-0.055	-0.057	-0.068	-0.092*	-0.078*	-0.080*
	(0.045)	(0.045)	(0.048)	(0.049)	(0.042)	(0.044)
RES_{2013}	-0.014	-0.024	-0.048	-0.075	-0.064	-0.062
	(0.043)	(0.047)	(0.051)	(0.052)	(0.050)	(0.056)
N	4,387	3,606	3,026	2,553	2,140	1,839
number firms	656	539	452	382	319	273

*Notes: The regressions include observations from 2007–2013. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with 2-digit sector time trends. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

Table 22: The effects of becoming exempt from paying the RES – different electricity cost intensity thresholds for trimming

	9%	**10%**	**11%**	**12%**	**13%**	**14%**
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel C: domestic material purchases			
RES_{2009}	-0.094	-0.050	-0.073	-0.074	-0.063	-0.058
	(0.072)	(0.050)	(0.049)	(0.051)	(0.053)	(0.054)
RES_{2010}	-0.022	-0.028	-0.018	-0.020	-0.013	-0.012
	(0.023)	(0.024)	(0.027)	(0.029)	(0.033)	(0.036)
RES_{2012}	0.182	0.195	0.020	0.018	0.034	0.024
	(0.144)	(0.155)	(0.033)	(0.036)	(0.041)	(0.043)
RES_{2013}	0.147	0.163	-0.017	-0.017	0.009	-0.016
	(0.142)	(0.154)	(0.050)	(0.055)	(0.061)	(0.064)
			Panel D: imports			
RES_{2009}	-0.004	-0.020	-0.081	-0.068	-0.114	-0.124
	(0.096)	(0.107)	(0.125)	(0.136)	(0.158)	(0.168)
RES_{2010}	0.075	0.073	0.058	0.071	0.019	0.012
	(0.069)	(0.075)	(0.090)	(0.098)	(0.107)	(0.122)
RES_{2012}	0.028	0.030	-0.069	-0.065	-0.096	-0.103
	(0.093)	(0.105)	(0.053)	(0.058)	(0.064)	(0.066)
RES_{2013}	0.110	0.101	0.036	0.054	0.017	0.038
	(0.099)	(0.111)	(0.065)	(0.071)	(0.077)	(0.083)
N	3,161	2,598	2,176	1,830	1,519	1,293
number firms	645	530	444	374	311	264

Notes: The regressions include observations from 2009–2013. 2011 constitutes the base year. Regressions are estimated with PPML. All regressions are run within-firm and with 2-digit sector time trends. Standard errors are clustered at the firm-level and displayed in parentheses. *, ** *and* *** *indicate significance at 10%, 5% and 1%, respectively. Source: Own calculations.*

12 Sector heterogeneities

12.1 Electricity intensity of firms and intermediates used by sector

Table 23: Electricity intensity and intermediate inputs by sector

	Electricity cost intensity	**Electricity intensity intermediates**	**Share specific intermediates**	**Share importers**
NACE 10	0.113	0.598	0.800	0.477
NACE 11	0.149	0.646	0.332	0.231
NACE 12	x	x	x	x
NACE 13	0.144	1.377	0.765	0.853
NACE 14	x	x	x	x
NACE 15	x	x	x	x
NACE 16	0.152	1.663	0.384	.778
NACE 17	0.119	3.263	.808	0.900
NACE 18	0.109	3.023	0.659	0.545
NACE 19	0.102	1.405	0.526	0.600
NACE 20	0.111	2.020	0.837	0.868
NACE 21	0.097	1.269	0.945	0.750
NACE 22	0.125	1.579	0.811	0.975
NACE 23	0.126	1.288	0.547	0.644
NACE 24	0.123	2.540	0.953	0.830
NACE 25	0.099	1.872	0.655	0.673
NACE 26	0.095	0.627	0.143	1.000
NACE 27	0.129	1.443	0.486	0.813
NACE 28	0.088	0.698	0.289	0.842
NACE 29	0.088	0.928	0.431	0.769
NACE 30	0.089	0.642	0.224	0.857
NACE 31	0.074	1.267	.444	1.000
NACE 32	0.116	0.667	0.432	0.800

Notes: The table reports the average of the factors used for heterogeneity analysis in different two-digit sectors in 2010. Firms with at least 50 employees are represented in the table. "x" denote cases in which confidentiality does not permit to depict numbers. Column (1) contains information on average electricity cost intensity. Column (2) shows the average electricity intensity of intermediate inputs used, where electricity intensity is expressed in kWh per EUR of value added. Column (3) shows the share of specific intermediates from total intermediate input use. Column (4) shows the share of importers according to the definition by Eurostat (i.e., $\leq 5,000$ EUR imports and import intensity $\leq 5\%$).

12.2 List of easily tradable goods

To identify intermediate inputs that are more easily tradable and whose imports might hence respond quicker to changes in electricity prices, I rely on the list of sectors identified by Rauch (1999) to either have an organized exchange or at least a reference price. The list is provided in the Standard International Trade Classification (SITC) rev. 2 classification. I recode it to the NACE classification. The result is provided in Table 24.

Note that the recoding is in some cases ambiguous. If that is the case, I compute the share of a 4-digit NACE sector falling into the reference price/organized exchange category by taking the share of recoding candidates that are in those categories.

Table 24: List of specific and easily tradable intermediate inputs

NACE code	Sector description	Market organization
0111	Cultivation of cereals (excluding rice), pulses and oilseeds	Organized exchange
0112	Rice	Organized exchange
0113	Potatoes and other roots and tubers	Organized exchange
0114	Sugar cane	Organized exchange
0115	Tobacco	Organized exchange
0116	Fibre plants	Organized exchange
0119	Other annual plants	Organized exchange
0121	Wine and table grapes	Organized exchange
0126	Oleaginous fruits	Organized exchange
0128	Aromatic plants, narcotic/pharmaceutical plants	Organized exchange
0129	Other perennial plants	Organized exchange
0130	Plants for propagation purposes	Reference price
0141	Dairy cows	Organized exchange
0143	Horses and donkeys	Organized exchange
0146	Pigs	Reference price
0147	Poultry	Reference price
0149	Other animals	Reference price
0311	Marine fisheries	Reference price
0322	Freshwater aquaculture	Reference price
0710	Iron ore mining	Organized exchange
0721	Uranium and thorium ores	Reference price

NACE code	Sector description	Market organization
0729	Other non-ferrous metal ore mining	Reference price
0811	Natural stone, limestone and gypsum, chalk and slate	Reference price
0812	Gravel, sand, clay and kaolin	Reference price
0891	Chemical and fertiliser minerals	Reference price
0893	Salt	Reference price
0899	Stone and earth n.e.c.	Reference price
1011	Slaughter (excluding poultry)	Reference price
1013	Meat processing	Reference price
1020	Fish processing	Reference price
1031	Potato processing	Reference price
1032	Fruit and vegetable juices	Reference price
1039	Other processing of fruit and vegetables	Reference price
1041	Oils and fats (excluding margarine)	Organized exchange
1042	Margarine and similar food fats	Organized exchange
1051	Milk processing (excluding ice cream)	Reference price
1052	Ice cream	Reference price
1061	Grinding and peeling mills	Reference price
1062	Starch and starch products	Reference price
1081	Sugar	Organized exchange
1091	Feed for farm animals	Reference price
1092	Feed for other animals	Reference price
1101	Spirits	Reference price
1200	Tobacco processing	Reference price
1310	Spinning	Reference price
1320	Weaving mill	Reference price
1621	Veneer, plywood, wood fibre and chipboard panels	Reference price
1711	Wood and pulp	Reference price
1712	Paper, cardboard and paperboard	Reference price
1721	Corrugated paper and paperboard, packaging materials	Reference price
1910	Coke	Reference price
1920	Mineral oil processing	Reference price
2011	Industrial gases	Reference price
2012	Dyes and pigments	Reference price

NACE code	Sector description	Market organization
2013	Other inorganic basic materials and chemicals	Reference price
2014	Other organic basic chemicals	Reference price
2015	Fertilisers and nitrogen compounds	Reference price
2016	Plastics in primary moulds	Reference price
2017	Synthetic rubber in primary forms	Reference price
2060	Man-made fibres	Reference price
2110	Basic pharmaceutical products	Reference price
2120	Pharmaceutical specialities, other pharmaceuticals	Reference price
2221	Sheets, foils, hoses and profiles from plastics	Reference price
2222	Packaging materials from plastics	Reference price
2223	Building materials from plastics	Reference price
2229	Other plastic products	Reference price
2351	Cement	Reference price
2352	Lime and burnt gypsum	Reference price
2410	Pig iron, steel and ferroalloys	Reference price
2420	Steel pipes, pipe/steel pipe fittings, pipe closures	Reference price
2431	Bright steel	Reference price
2432	Cold rolled strip (width less than 600 mm)	Reference price
2433	Cold profiles	Reference price
2434	Cold drawn wires	Reference price
2441	Precious metals	Organized exchange
2442	Aluminium	Organized exchange
2443	Lead, zinc and tin	Organized exchange
2444	Copper	Organized exchange
2445	Other non-ferrous metals	Organized exchange
2451	Iron foundries	Reference price

Source: Based on the classification of Rauch (1999).

Goods which have lower search cost for international trade, as they either are traded on organized exchanges, or at least possess a reference price, are concentrated in certain sectors of the economy: Many of these goods belong to the agricultural, mining and quarrying, food processing, textiles, coke and petroleum, chemical, and metals sectors. In contrast, e.g., no 4-digit NACE code of the wearing apparel, electronics, machinery or car industries are classified as easily tradable inputs.

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