Unbundling, Regulation, and Pricing: Evidence from Electricity Distribution

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ABSTRACT

Unbundling of vertically integrated utilities has become an integral element in the regulation of network industries and has been implemented in many jurisdictions. The idea of separating the network, as the natural monopoly, from downstream retailing, which may be exposed to competition, is still subject to contentious debate, as there is much empirical evidence that unbundling eliminates economies of vertical integration, though evidence on overall price effects is still lacking. In this paper, we study the effect of legal unbundling on grid charges in the German electricity distribution industry. Using panel data on German distribution system operators (DSOs), we exploit the variation in the timing of the implementation of legal unbundling and the fact that not all DSOs had to implement unbundling measures. We are also able to identify heterogeneous effects of legal unbundling for different types of price regulation because we observe a switch in the price regulation regime from rate-of-return regulation to incentive regulation during our observation period. Our findings suggest that legal unbundling of the network stage significantly decreases grid charges in the range of 5% to 9%, depending on the type of price regulation in place.

Keywords: Vertical integration, Electricity distribution, Unbundling, Regulation

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1. INTRODUCTION

Unbundling of vertically integrated utilities (VIU) has become a major instrument when it comes to the regulation of network industries. Given that the network represents an essential input for downstream retailers, VIUs may have incentives to disadvantage retail competitors by setting excessively high grid charges, which the VIUs themselves can cross-subsidize. Thus, unbundling of the network stage intends to eliminate incentives for price discrimination against rivals and to foster competition in the retail segment. Besides direct price discrimination, unbundling is also deemed to reduce the incentive for non-price discriminatory measures, such as delaying consumers' supplier switching, or withholding important information from competitors. Despite its potential benefits, unbundling remains a controversial topic. The reason is that much evidence points to a pronounced loss of vertical economies of scope (e.g. Arocena et al., 2012; Gugler et al., 2017; Triebs et al., 2016), while evidence on the potential benefits of unbundling, such as lower prices, is still lacking.

In the past two decades, the regulatory principle of unbundling, in combination with third party access, has been implemented in a variety of forms in many infrastructure industries around the

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globe, especially in electricity markets. For instance, the European Union requires legal unbundling of the electricity and gas *distribution* networks for VIUs with more than 100,000 customers.[1] The unbundled distribution system operator (DSO) must be independent with respect to its legal form, organization, and decision making from other activities not related to distribution (CEER, 2013). Ownership unbundling of the electricity and gas *transmission* network has been compulsory for EU Member States since the Third Energy Package came into force in 2009. Not only in Europe but also in the US, vertical unbundling is widely applied in the electricity and natural gas industries (Höffler and Kranz, 2011b). Apart from energy markets, unbundling is also practiced in many other infrastructure markets, such as railway transportation (van de Velde, 2015; Finger, 2014), telecommunications (Bourreau and Doˇgan, 2005; Pindyck, 2007), and internet broadband (Nardotto et al., 2015).

However, despite wide application and more than one decade of experience with unbundling measures in many industries, empirical evidence on competition-fostering effects of unbundling is still scarce. The lack of evidence on the positive aspects of unbundling is even more worrisome, as there is a body of empirical literature highlighting the costs associated with unbundling in the form of lost vertical economies (see, e.g., Arocena et al., 2012; Gugler et al., 2017; Triebs et al., 2016, for recent contributions). Also, theoretical (e.g. Buehler et al., 2004; Cremer et al., 2006) and empirical studies (Gugler et al., 2013) warn that unbundling may lead to reduced investments in the network, leading to an increase in equilibrium prices. Because of these counteracting effects—ambiguous effects on competition on the one hand and a loss of vertical economies on the other—there are still controversial debates among scholars and policy-makers about the costs and benefits of unbundling. For example, Japan's announcement in 2015 to legally unbundle its electricity transmission and distribution sectors by April 2020 has received much media attention insofar as the unpredictable effects were criticized.[2] However, some theoretical papers (see, e.g. Höffler and Kranz, 2011b) argue that legal unbundling may serve as a 'golden mean' between full ownership unbundling and vertical integration, as it minimizes the loss in vertical economies but is supposed to be similarly effective in fostering competition. Again, however, empirical evidence is lacking and it is frequently questioned if legal unbundling is in fact effective in inducing lower prices and fostering downstream competition.

To fill the literature gap, we study the competitive effect of legal unbundling in the German electricity distribution industry on grid charges. We make use of the fact that VIUs with more than 100,000 customers had to legally unbundle, while utilities below this threshold have been allowed to stay vertically integrated. Moreover, we exploit additional variation in the timing of the implementation of unbundling measures, as not all DSOs implemented legal unbundling in the same year.

A key feature of our analysis is our longitudinal dataset on grid charges and network characteristics of German DSOs for the period 2005–2014. This period covers the unbundling transition of several DSOs and an important regulatory change—the switch from rate-of-return regulation to incentive regulation in 2009—also falls within this period. This allows us to test the effect of legal unbundling on grid charges under different types of price regulation using a *triple-differences approach*.

To preview results, we find that the implementation of legal unbundling leads to a decrease in distribution grid charges in the range of 5% to 9%, depending on the on the type of price regulation. During rate-of-return regulation, firms subject to legal unbundling set around 5% lower prices. After 2009, when incentive regulation was introduced in the German electricity distribution sector, legally unbundled firms charge 9% lower prices than vertically integrated utilities. The re-

1. Electricity Directives 2003/54/EC, 2009/72/EC, and Gas Directives Directive Directive 2003/55/EC, 2009/73/EC.
2. For critical reactions, see e.g. Boyd (2016).

sults remain robust to several robustness checks, such as Lewbel (2012)'s heteroskedasticity-based instrumental variables approach, split sample analyses, dropping DSOs that unbundled voluntarily, as well as running placebo tests where we randomized the year in which legal unbundling measures were implemented.

Our results suggest that legal unbundling is indeed effective in reducing grid charges. Moreover, we also find that the magnitude of the price reducing effect depends on the price regulation regime in place and is more pronounced under incentive regulation than under rate-of-return regulation. From this perspective, our results are 'good news' to many jurisdictions that already apply legal unbundling in combination with incentive regulation. For example, this combination applies to electricity and gas distribution markets in most EU Member States and in many States of the USA. For Japan, currently planning to introduce legal unbundling, our results may be promising.[3]

2. INSTITUTIONAL BACKGROUND

Electricity distribution in Germany is divided between approximately 880 local DSOs (BNetzA, 2015).[4] As a result of the high (sunk) costs of building a new electricity grid, competition cannot emerge in this sector. For historical reasons, a DSO is typically part of a VIU and before the German electricity market was liberalized in 1999 VIUs were local monopolists in the retail market. The liberalization ended the era of local monopolies by allowing entry to local retail markets and giving consumers the choice to switch from their former local incumbent—the VIU—to alternative suppliers. Prior to market liberalization, the local incumbent served all customers in its distribution grid area at a regulated tariff. Since the liberalization, retail prices are not regulated anymore and customers can freely choose their electricity supplier. With respect to entry, the liberalization was a great success and the incumbents now compete with a large number of retailers for customers.[5]

However, the large number of retailers does not guarantee fair competition. Having control over the distribution grid, vertically integrated incumbents may have the incentive to affect their competitors' costs at the retail stage by increasing grid charges, whereas the VIUs themselves are able to cross-subsidize. Moreover, the VIUs may also have incentives to further hinder competition by exercising non-price discriminatory measures (e.g. withdrawing important information from rivals, delaying customer switching to rivals, etc.). The policies of price regulation and unbundling aim at preventing such unfair practices.

The initial implementation of rate-of-return regulation in 2006 aimed at repressing exorbitant grid charges. In 2009, rate-of-return regulation was replaced by incentive regulation. Both regulations involved the prohibition of price discrimination between similar customers[6] and included restrictions on how to set grid charges. Under rate-of-return regulation, the regulator calculated revenue caps based on DSOs' operational costs for the distribution of electricity[7] in the last fiscal

3. We acknowledge that other factors, such as the market structure, the investment environment, or the policy framework are likely to have an influence on the effect of unbundling. However, in our setting these factors are to a large extent time-invariant. Thus, we are not able to study them separately as this information falls victim to the DSO fixed effects we use in our estimations. Nevertheless, for the external validity of our results, such factors have to be taken into account.

4. Appendix Figure 3 illustrates the number of DSOs in Germany over time.

5. In 2014, there were on average 156 local electricity retailers in a zip code (Gugler et al., 2018).

6. Groups of similar customer are determined by the voltage level of a meter point, the kind of electricity meter, and usage hours.

7. For determining DSOs' operational distribution costs, the regulatory agency examines the costs for operating the grid as well as other costs that are not under the immediate control of the DSO (e.g. legal obligations, concession fees, business taxes, upstream transmission charges, investments related to network operations, extra costs of underground cabling, or collective agreements about nonwage and social benefits).

year and allowed for a given rate-of-return predetermined by the regulator.[8] DSOs thus hardly had incentives to become more efficient because any investments or cost changes related to the network operations were accounted for in the revenue cap and thus did not change their rate of return.

This is in stark contrast to incentive regulation, which was introduced in 2009. Under incentive regulation, annual revenue caps intend to simulate competitive pressure in the regulated industry.[9] The annual revenue caps consist of two parts: (1) A general component, which applies equally to all DSOs and essentially covers the productivity growth in the regulated industry (Gugler and Liebensteiner, 2019), and (2) a DSO-specific component that is based on the DSO's efficiency score as determined in a benchmarking process. The idea is that inefficient DSOs get lower (i.e. more pressing) revenue caps as a 'motivation' to become more efficient (i.e. to catch up to the efficiency frontier). With these two components, revenue caps are determined for regulatory periods of five years, whereas the benchmarking process relates back to a base year (i.e. four years before the start of the regulatory period; see Appendix Figure 4). The individual revenue cap determines a DSO's scope of price setting for access to its distribution grid. A price set too high would result in a revenue above the revenue cap that cannot be retained, while a low price might yield suboptimal profits (RAP, 2014). [10] Thus, under both regulatory regimes (rate-of-return regulation and incentive regulation), utilities have some scope for setting their grid charges, even though the revenue caps set strong boundaries.

With the 2005 amendment of the German Energy Law (Energiewirtschaftsgesetz, EnWG) the EU Electricity Directive 2003/54/EC was transposed into national law, which required DSOs to *legally unbundle* from downstream retail. The law, however, requires mandatory legal unbundling only for utilities with more than 100,000 customers. Only 45 of around 880 DSOs are large enough to fall under the legal unbundling requirements in Germany in 2015 (BNetzA, 2015). Smaller utilities are exempt from this regulation, as a result of the amount of structural changes required, and are thus still allowed to remain vertically integrated. Although these small utilities have the right to voluntarily legally unbundle, this rarely happens in practice, as outlined in more detail in Section 4. Legal unbundling represents an intermediate approach between the extreme forms of *ownership unbundling* (i.e. full vertical divestiture) and *vertical integration*. It forces vertically integrated utilities to partition their distribution activities by creating a new legal entity. Using this approach, the distribution grid can remain in the ownership of the integrated enterprise, but it must be managed by a legally distinct DSO.

Legal unbundling thus intends to decrease discrimination incentives and foster downstream competition by strengthening the formal independence of the DSO. At the same time, legal unbundling is likely to reduce vertical economies less than ownership unbundling. Furthermore, legal unbundling is easier to implement in practice, as the unbundled DSO remains in the ownership of the formerly VIU.[11] It is assumed that with full ownership unbundling, the new system operator may be fully neutral against any downstream retailer. However, with *legal unbundling*, the integrated utility still possesses the network even though it must be managed by a legally independent operator. Hence, it is possible for incentives to discriminate, such as setting high grid charges

8. Compare § 1 and § 15 of the 2005 Electricity Network Charges Ordinance (Stromnetzentgeltverordnung, StromNEV).

9. Matschoss et al. (2019) provide an extensive description of the German incentive regulation regime and the formula for estimating revenue caps.

10. Obtained revenues above or below the actual revenue cap are booked to a regulatory account and transferred at the end of the regulatory period to the next one (Matschoss et al., 2019).

11. Most transmission system operators (TSOs) in Europe are subject to ownership unbundling. One reason may be that only few TSOs exist (usually one per country; four in Germany) so that ownership unbundling is feasible, while with many DSOs the costs of ownership unbundling would be tremendous.

to harm downstream competitors, may remain with legal unbundling. It is thus open to empirical investigation whether legal unbundling is indeed successful in reducing grid charges compared to vertical integration.

The effects of rate-of-return and incentive regulation in combination with LU on grid charges crucially depend on three different channels: (1) the incentives they impose on DSOs to invest, (2) the incentives they impose on DSOs to reduce their costs, and (3) the space they leave for DSOs to overstate their costs to receive higher revenue caps and grid charges. Averch and Johnson (1962) pointed out that rate-of-return regulation creates potentials for inefficiencies. as regulated firms may not minimize costs but build up inefficiencies and misreport costs to receive higher grid charges and revenue caps. Incentive regulation is thus supposed to erode the inefficient behavior of regulated firms by simulating a competitive environment (Braeutigam and Panzar, 1993). In that sense, incentive regulation leaves less room for regulated firms to misreport their costs. The theoretical prediction is thus that under incentive regulation, the scope for overstating costs, in order to receive higher grid charges and revenue caps, is more limited. However, the empirical evidence is scarce. Hellwig et al. (2018), for example, show that German electricity DSOs exhibit lower costs under a weaker form of regulation compared to a higher-powered regulatory regime. Cambini and Rondi (2010) show that energy utilities under incentive regulation have higher investment rates than under rate-of-return regulation. Considering the effects of unbundling, for instance, Buehler et al. (2004) show theoretically, while Gugler et al. (2013) empirically, that investments are higher under vertical integration than under unbundling, which implies that integrated firms may set higher grid charges. However, no study so far investigates the interplay of the regulatory regime with LU. Again, the effect of legal unbundling under rate-of-return and incentive regulation on grid charges boils down to an empirical question.

3. LITERATURE

We now survey the related literature on the different effects of unbundling such as grid charges, investments, retail prices, economies of scope, and cost efficiency.

The only empirical paper we are aware of that (directly) examines the effect of unbundling measures on grid charges itself is Nikogosian and Veith (2012). The authors show in a theoretical setup that vertically integrated electricity utilities have incentives to discriminate against downstream retail competitors (they argue for demand-decreasing and cost-increasing non-price discrimination, e.g. through withholding crucial information or delaying supplier switching), which may result in higher retail prices. Using cross-sectional data of German DSOs from August 2008, they empirically test their theoretical predictions but do not find statistically significant effects of unbundling measures on grid charges. Unfortunately, the authors provide no sound explanation for why they do not find an effect of LU on grid charges. Although this paper is related to our analysis, its results are likely to suffer from substantial unobserved heterogeneity between DSOs due to the cross-sectional dimension of the dataset, as well as other limitations.[12] In contrast, we make use of a panel dataset and apply a difference-in-differences estimator with DSO fixed effects.

In a theoretical setup, Cremer and Donder (2013) examine the effects of legal and ownership unbundling on grid investments. They find that, under ownership unbundling, the unregulated VIU limits its investments in grid capacity in order to increase equilibrium grid charges and maximize its profits from the grid operation. With legal unbundling, the upstream firm takes downstream

12. E.g. Nikogosian and Veith (2012) do not consider the fact that DSOs with less than 30,000 customers are subject to a different type of price regulation.

profits into account as well. Hence, given that downstream profits increase with grid capacity, the upstream firms then invest more in grid capacity under legal unbundling, and grid charges are lower. Similarly, Buehler et al. (2004) suggest that, under reasonable assumptions on demand, investment incentives turn out to be smaller under vertical separation than under vertical integration. The finding that ownership unbundling decreases investment is empirically supported by Gugler et al. (2013) in their examination of grid investments in European countries.

Several other papers have examined the effect of unbundling on retail prices in the energy sector. For the electricity sector, Steiner (2012) analyzed a panel dataset of 19 OECD countries for the period of 1987–1996. Using a random effects estimator, she finds that the unbundling of generation and transmission had no significant effect on end-user prices. Sen and Jamasb (2012) use panel data for 19 Indian states between 1991 and 2007 and assess the effect of several regulatory variables on electricity prices for end-consumers (and other variables). With respect to unbundling measures, they do not find a significant effect on consumer prices. However, the paper does not provide any details on the type of unbundling. Nillesen and Pollitt (2011) investigate New Zealand's introduction of ownership unbundling between the stages of distribution and retail in 1998. They conduct a before-and-after analysis and find that average end-consumer prices did not change significantly after ownership unbundling, though residential prices increased and commercial prices decreased. For the gas sector, Growitsch and Stronzik (2014) and Brau et al. (2010) empirically analyze the effect of unbundling for a panel of EU countries. Both papers do not find that ownership unbundling has a significant effect on consumer prices for natural gas. Moreover, Growitsch and Stronzik (2014) find that consumer prices are significantly reduced with legal unbundling of the transmission network, whereas unbundling of the distribution stage has no statistical influence.[13]

A larger strand of the literature on vertical separation focuses on the empirical measurement of vertical economies (usually based on the estimation of a multi-output cost function), which are assumed to be lost with unbundling (e.g. Arocena et al., 2012; Fetz and Filippini, 2010; Greer, 2008; Gugler et al., 2017; Triebs et al., 2016). These papers highlight that vertical integration is associated with vertical synergies (e.g. coordination advantages, usage of common inputs and staff, sharing of information and hedging against risk, avoidance of double marginalization) and that successful unbundling has to outweigh the dissipated scope economies by its benefits (e.g. lower retail prices through competition, lower grid charges, avoidance of cross-subsidization, mitigation of sabotage and discriminatory access in the network segment).

However, despite the cost-increasing effect of unbundling due to the elimination of vertical economies, there are also papers suggesting that there may be a countervailing effect due to an increase in cost efficiency. Using a cost function, Nillesen and Pollitt (2011) analyze New Zealand's introduction of mandatory ownership unbundling of distribution from retail in 1998 and find that ownership unbundling has significantly reduced the unit-operation costs of electricity distribution and that grid quality (proxied by an electricity supply interruption index) has improved. Similarly, Filippini and Wetzel (2014) find that ownership unbundling improved the cost efficiency of New Zealand's DSOs based on Stochastic Frontier Analysis (which, in contrast to a standard cost function estimation, deals explicitly with firms' inefficiencies). A novel result is that the short-run efficiency improvement (evaluated from a variable cost function) is higher than the long-run efficiency

13. Regarding emerging economies, Nagayama (2007) analyzed the influence of a set of regulatory measures on electricity retail prices with no statistical effect of ownership transmission distribution. Gao and Biesebroeck (2014) find that ownership unbundling of the electricity transmission network from upstream generation together with the introduction of wholesale price liberalization improved productivity (i.e. treated firms reduced employment by 7% and material inputs by 5% relative to control firms) in the Chinese electricity sector two years after the reform took place.

(evaluated from a total cost function). Kwoka et al. (2010) derive efficiency scores of U.S. DSOs by data envelopment analysis and then apply a difference-in-differences approach to infer about how unbundling affects efficiency. The findings are that mandatory divestiture of the distribution grid from upstream generation had a decremental effect on efficiency, while voluntary divestiture did not significantly distort efficiency. These findings are in line with Delmas and Tokat (2005) who find that stronger integration leads to higher efficiency. A limitation of Kwoka et al. (2010)'s difference-in-differences approach is that they do not provide any tests or evidence that the treatment (unbundling) and control (no unbundling) groups are similar.

To sum up, the effects of unbundling remain ambiguous, according to the current literature. Regarding the competition-fostering effects of unbundling, the scarce empirical evidence suggests that unbundling neither has a significant effect on grid charges nor on retail prices. However, all cited studies suffer from different non-negligible weaknesses. Moreover, it is not clear if an increased cost efficiency can outweigh the loss of vertical economies from unbundling. Also, most studies focus on the effects of ownership unbundling, while the effects of legal unbundling are clearly under-researched. This is surprising, especially against the background that legal unbundling represents the standard requirement in the energy sectors and is considered a 'golden mean' between full ownership unbundling and vertical integration. (see, e.g. Höffler and Kranz, 2011a,b). Though the economies of vertical integration should be less affected by legal unbundling than by full ownership unbundling, it is still not clear if legal unbundling is effective in reducing prices. Our paper naturally extends the literature by investigating the latter.

4. DATA

We construct a panel dataset for the period 2005–2014, which combines information on grid charges and grid characteristics of German DSOs, renewable energy capacities connected to the grid, and—of utmost importance for our purpose—the status of DSOs' vertical integration. The data stem from several sources. Proprietary data on *grid charges* and *grid characteristics* are from *e'net*, one of the largest private information service providers for Germany's energy industry. Data on all wind and solar plants, their capacities, and geographical locations are gathered from *EnergyMap*, an open source project by the German branch of the International Solar Energy Society, which combines and adjusts data on wind and solar capacities to the corresponding distribution grid.

Our main variable of interest is the status of vertical integration of the DSOs. Even though legal unbundling became mandatory for all DSOs with more than 100,000 customers, there were also cases where DSOs with less than 100,000 customers had to unbundle, for instance due to holdings in other grid operators. To identify the status of vertical integration, we manually skimmed the DSOs' financial statements of the respective sample years for announcements regarding their form of unbundling as well as information on their vertical structures such as electricity generation, the absence thereof, and retail activities. If a utility's financial statement indicated legal unbundling, we identified the legal connection to its former retail and/or generation unit so as to double check the information contained in the financial statements. Furthermore, we determined if legally unbundled DSOs had implemented legal unbundling *voluntarily* by browsing their financial statements, homepages, and in many cases by contacting them via phone or e-mail.

We only consider DSOs with at least 30,000 customers in our application, as the DSOs below this threshold are under a different regulatory regime.[14] There are also a few DSOs with slightly

14. In Germany, DSOs with less than 30,000 customers can choose between taking part in the benchmarking process of the incentive regulation to receive an individual efficiency score based on their characteristics, or receiving the average efficiency

more than 30,000 customers that are regulated in the different regime due to historical regulatory exceptions. Those were also excluded from our data. DSOs with more than 170,000 customers were also disregarded to avoid too large structural differences between DSOs.[15] In the end, our dataset contains 123 DSOs, which have 6.6 million connected customers in total. Table 1 provides an overview about the distribution of DSOs that are included in our sample and those which we exclude in our main specifications.

Table 1: Distribution of DSOs by size

Meter points	Number DSOs	Average number of meter points per DSO	Total number of meter points (in million)
<30,000	662	10.64	4.83
30,000–170,000	123	76.54	6.61
>170,000	54	691.79	30.90
Total	839	70.96	42.34

Out of the 123 DSOs in our sample, 46 have implemented legal unbundling, seven of which have unbundled voluntarily. None of the 123 DSOs has implemented ownership unbundling. 77 DSOs in our dataset did not unbundle at all until the end of our observation period 2014. Our observation period ranges from 2005 to 2014 and we observe (1) both states of vertical integration and legal unbundling for the 31 DSOs that unbundled 2006 or later, (2) only the state of vertical integration for the 77 DSOs that did not unbundle at all, (3) and only the state of legal unbundling for the 15 DSO that unbundled in 2005 or earlier. In Table 2, we provide more details on the distribution of the unbundling dates.

VIUs with more than 100,000 connected customers were required to incorporate legal unbundling by 2007 at the latest (according to Article 7 of the EnWG issued in 2005). 20 of them unbundled before 2007 (17 of them in either 2005 or 2006) and 20 in 2007 or 2008 (19 of them in 2007) as shown in Table 2. Some utilities unbundled even later because they passed the threshold of 100,000 connected customers, either through organic growth or following a merger, and some even unbundled voluntarily.

Table 2: Timing of Legal Unbundling

Year	2000	2004	2005	2006	2007	2008	2011	2013	no LU
Number of DSOs	2	1	12	5	19	1	3	3	77

Note: The Table shows how many distribution system operators (DSOs) implemented legal unbundling per year.

The data on grid charges are composed of a fixed component and a variable part that depends on the usage level. We calculate the grid charges for a representative residential household customer with an annual consumption of 4,000 kWh of electricity. As a robustness check, we also compute grid charges for an average business customer that consumes 50,000 kWh of electricity per year.

score of a regulatory period. According to Hellwig et al. (2018), this is a soft form of rate-of-return regulation, “which provides lower incentives for cost reduction.”

15. Hence, we include the DSOs in a range of 70,000 below and above the 100,000 connected customers threshold. While the 70,000 below the threshold emerge naturally due to the different grid charge regulation for DSOs with less than 30,000 connected customers, the 170,000 connected customers threshold was chosen to ensure symmetry around the 100,000 connected customers threshold. However, our results remain robust for alternative choices of the threshold as shown in the robustness section.

We also use information on the yearly maximum possible electricity generation capacity (in kW) of the wind and solar plants connected to a DSO's distribution grid. These two variables are included to control for the additional time variation in costs imposed on DSOs through the connection of renewable energy sources (RES). In general, these costs are created due to the unreliable nature of RES and their expansion in recent years. More specifically, DSOs are responsible for the stability of the electricity grid. However, the grid stability can be endangered by grid congestions or voltage and frequency deviations. Therefore, DSOs need to use balancing power to counteract frequency deviations, and redispatch measures or feed-in management to compensate grid congestions. As a result of the unpredictable nature of weather, an increase in the connected solar and wind capacity increases the risk of grid instability, therefore raising a DSO's expenses for the mentioned countervailing measures.

Furthermore, the existing grid is not designed to handle the high amount of locally generated electricity that results from the expansion of RES. Before the expansion of RES, electricity was typically received from a transmission system operator's extra-high voltage trans-regional transmission network and distributed by the responsible DSO in its grid area. However, the share of renewable energies on Germany's total electricity consumption increased from 15.1 percent in 2008 to 31.6 percent in 2015 and more than 90 percent of renewable energy plants were directly connected to the distribution grid in 2015 (BNetzA, 2015, 2016). Therefore, the total amount locally generated cannot be locally consumed but rather needs to be distributed from the generation facility to a TSO's extra-high voltage transmission line. This creates additional investment costs, since the existing grid needs to be expanded to secure grid stability. Finally, according to §20 of the German Energy Act, DSOs are legally forced to connect newly built electricity plants, under fair and objective conditions, to their distribution grid. Thus, each newly built electricity plant creates additional investment costs if a grid expansion is needed to connect it.

We also include the population density (residents per square kilometer within a grid area), and the number of meter points (equivalent to connected customers) into the set of control variables in order to consider the network usage and the grid size, respectively. Table 3 provides descriptive sample statistics of our main variables employed in the regressions.

Table 3: Summary statistics

	Mean	SD	Min	Max	Obs
Grid charge (€/4,000 kWh)	208	32.7	105	320	832
Meter points (#)	75,222	39,838	31,017	167,080	832
Population density (#/km^2)	964	620	81	3,315	832
Solar capacity (kW)	17467	24,507	59	162,218	832
Wind capacity (kW)	7,907	18,911	0	133,217	832

5. IDENTIFICATION AND RESULTS

We are interested in the effect of legal unbundling of VIUs on their grid charges (in the following simply *prices*). The suspicion is that VIUs may set higher prices in order to increase downstream costs for their competitors at the retail level while the integrated enterprise itself can cross-subsidize these costs. With legal unbundling, this incentive should be mitigated by strengthening the independence of the DSOs. Thus, we would expect that DSOs decrease their prices once they become legally unbundled. If this was not the case, the VIUs either do not engage in price discrimination (and thus do not charge higher prices at all) or legal unbundling is not effective in incentivizing DSOs against discriminatory behavior.

5.1 Identification

As we observe several VIUs that underwent the transition from vertical integration to legal unbundling during our period of investigation, 2005–2014, we are able to perform a difference-in-differences estimation. Formally, we estimate the following equation:

$$\log(p_{it}) = \beta \times LU_{it} + \log(X_{it}) \times \gamma + \xi_i + \xi_t + \varepsilon_{it}, \tag{1}$$

where p_{it} represents the grid charge set by DSO i in year t. LU_{it} is our variable of interest and takes a value of 1 if a firm i is legally unbundled in year t and 0 otherwise. X is a set of (log-transformed) control variables, ξ_i and ξ_t are DSO and year fixed effects, and ε_{it} represents the noise term. Finally, we cluster the standard errors at the DSO level.

An advantage of our data is that not all treated firms unbundled in the same year (see Table 2). This allows us to examine the effect for different control groups. The firms can be categorized into three groups depending on their timing of unbundling:

(a) *LU 2006 or later*
For these firms, the transition from vertical integration to legal unbundling takes place during our sample period. Hence, we observe their prices in years in which they are still vertically integrated and in years in which they are legally unbundled.

(b) *LU before 2006*
These firms already unbundled before 2006. Hence, we do not observe pre-treatment prices for these firms as our sample period ranges from 2005 to 2014.

(c) *No LU*
These firms did not legally unbundle at all during our sample period (i.e. firms that did not receive the treatment). For these firms, we only observe prices for the status of vertical integration.

Hence, we have (a) as the treatment group, and two different control groups, (b) and (c).

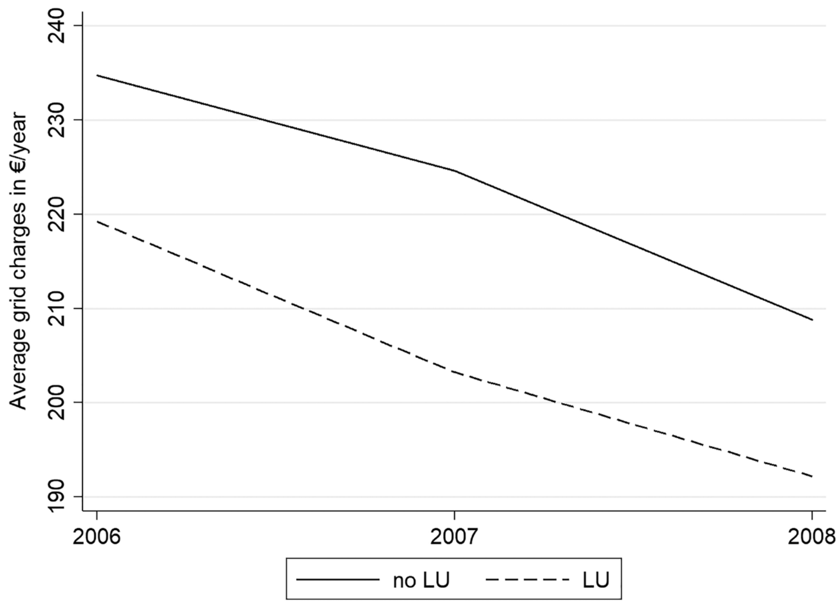
For a causal interpretation of β in Equation (1), the parallel trends assumption must hold. It requires that the developments of the prices of the treatment and the control group would have been parallel in the absence of the treatment. However, it may be that VIUs that were obliged to legally unbundle their operations already had a different trend in their pricing before unbundling was implemented. For instance, their prices may have experienced a stronger increase or decrease, on average, than the prices of DSOs that were not forced to legally unbundle.[16] In this case, our estimates would be biased.

We can compare the pre-treatment developments of prices of (a) with (c), but not with (b), given that our dataset starts in 2005. Figure 1 plots the pre-treatment prices of treated (a) and untreated (c) DSOs until 2008, since in 2009 incentive regulation came into effect (for which we cannot graphically disentangle the effect on prices). For (a) and (c), Figure 1 suggests that the development of the prices of firms that unbundled and those that remained vertically integrated is indeed parallel before the treatment. This gives us confidence that prices of these firms would have developed in parallel in the absence of unbundling.

The firms that unbundled before 2006 account for one third of the unbundled firms (15 out of 46, 12 of which unbundled in 2005). However, it is reasonable to assume that the unbundling year was randomly chosen by these firms. In particular, we assume that firms that unbundled in 2005 had

16. Recall that the VIUs that had to unbundle are larger (more than 100,000 customers) and hence, differ from the VIUs that were allowed to remain vertically integrated in this regard.

Figure 1: Pre-treatment development of prices of firms that unbundled and firms that did not



the same development of prices before they legally unbundled as the VIUs that unbundled in 2006 or 2007. To add further confidence that this is the case, we compare if firms that unbundled before 2006 differ systematically from those that unbundled afterwards in their observable main characteristics: the number of connected customers (meter points), the population density in the DSOs' grid areas, and the installed wind and solar capacity per grid area.

To test this, we conduct a two-sample t-test of equal means of these characteristics, as shown in Table 4. The null hypothesis of equal means of both groups cannot be rejected for any of these characteristics, suggesting that the DSOs that unbundled before 2006 and those that unbundled afterwards do not differ statistically in this regard.[17] Nevertheless, it may still be the case that firms that unbundled earlier differ in unobserved characteristics. However, as we use DSO fixed effects in all estimations, we account for endogeneity issues caused by unobserved heterogeneity.[18] To sum up, we are confident that firms that unbundled before 2006 may also serve as a valid control group.[19]

5.2 Effect of legal unbundling on prices under rate-of-return regulation

We now estimate Equation 1 for different subsamples reflecting the different control groups. If both (b) and (c) are appropriate control groups, the effect of LU on prices should be similar. We

17. We additionally estimated a probit model with the binary variable *legal unbundling before 2006* being the dependent variable and meter points, population density, wind capacity and solar capacity as covariates. A corresponding likelihood-ratio test of the joint insignificance of all regressors cannot be rejected, suggesting that there are no differences in the joint distribution of the observable covariates between the two samples. This provides additional confidence that the groups do not differ in terms of the covariates.

18. In the robustness section we will also estimate Lewbel IV models to ensure that the results are not caused by time-varying unobserved differences between DSOs.

19. A further assumption is that the implementation of LU *immediately* translates into pricing. If this was not the case, we would not find a contemporaneous effect of LU on prices. However, we think it is realistic to assume that firms immediately adjust their pricing strategies once the link between the grid and the retail activities is cut under the assumption of profit maximization.

Table 4: t-test of equal means in the year 2006 of DSOs that legally unbundled before 2006 and in 2006 or later

	Mean	SD	t-test) (p-value)
Meter points			
LU before 2006	120,048	40,187	
LU 2006 or later	100,748	41,728	
			0.24
Population density			
LU before 2006	2,547	1,761	
LU 2006 or later	3,461	1,241	
			0.19
Wind capacity			
LU before 2006	8,168	11,957	
LU 2006 or later	3,528	4,905	
			0.22
Solar capacity			
LU before 2006	2,579	3,583	
LU 2006 or later	4,158	7,211	
			0.48

Note: The H0 of the t-test is "means are equal for both groups (a) and (b)"

will initially focus on the period 2005–2008. We do this to prevent coincidental and potentially heterogeneous effects of a switch from rate-of-return regulation to incentive-based regulation in 2009 from affecting our results. However, we will also consider the full period and explicitly account for this switch of the price regulation regime later.

In the first subsample, we include only firms that unbundled before 2009 and exclude all firms that unbundled after 2008, or not at all. Hence, the treatment group consists of the firms that experienced a transition from vertical integration to legal unbundling between 2006 and 2008 and the control group consists of all firms that unbundled before 2006 (as we do not observe their pre-treatment prices). The results are reported in Column (1) of Table 5 and suggest that LU caused a price decrease of approximately 6.9%.[20]

In the second subsample, we replace the control group and include only firms that unbundled from 2006 on, or not at all. Whereas before the control group consisted of firms that were legally unbundled during the entire sample period, it now consists of firms that unbundled after 2008 (when incentive regulation came into force), or not at all. The treatment group remains the same as before, namely firms that unbundled during 2006–2008. The results are reported in Column (2) of Table 5. The effect of LU is now 5.4% (=exp(–0.055)–1) and hence, similar in magnitude to that reported in Column (1).[21] This gives us further confidence that the estimated effect is not due to different pre-treatment price developments.

Finally, we use the full sample of firms. The control group now consists of all firms that remained either vertically integrated or legally unbundled during all sample years. The results are shown in Column (3) and suggest that LU causes an overall price reduction of around 5.9% (= exp(–0.061)–1). Again, the finding compares well with the previously reported results.

20. To interpret the coefficients as percentage changes, they are transformed to $100 \times \exp(\text{coefficient}) - 1$; in this case $\exp(-0.072) - 1 = -0.069$.

21. We evaluate the difference between the estimated coefficients in Column (1) ($\hat{\beta}_{(1)}$) and 2 ($\hat{\beta}_{(2)}$) based on a z-value: $z = (\hat{\beta}_{(1)} - \hat{\beta}_{(2)}) / \sqrt{SE(\hat{\beta}_{(1)})^2 + SE(\hat{\beta}_{(2)})} = (-0.072 + 0.055) / \sqrt{(0.036^2 - 0.026^2)} = -0.38$ (Clogg et al., 1995). The critical value for the 90% significance value is -1.96, so we cannot reject the H0 of equal coefficients. Thus, the two estimated coefficients are statistically non-distinguishable.

Table 5: Effect of LU on grid charges for the period 2005–2008

	(1)	(2)	(3)
LU	–0.072**	–0.055**	–0.061***
	(0.036)	(0.026)	(0.022)
log(meter points)	–0.074	–0.205***	–0.194***
	(0.109)	(0.035)	(0.027)
log(population density)	0.028**	0.009	0.011
	(0.011)	(0.012)	(0.011)
log(solar capacity)	0.104***	0.046	0.049*
	(0.028)	(0.030)	(0.028)
log(wind capacity)	0.082***	-0.047	0.025
	(0.015)	(0.054)	(0.036)
DSO FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
R^2	0.75	0.59	0.59
Observations	82	171	208
Treatment group:	2006≤LU≤2008	2006≤LU≤2008	2006≤LU≤2008
Control group:	LU≤2005	No LU, LU≥2009	LU≤2005, LU≥2009, no LU

Dependent variable is log(price). Period of investigation is restricted to 2005–2008 (i.e. before the regime switch from rate-of-return regulation to incentive regulation). Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

5.3 Interaction of the status of vertical integration with price regulation

As mentioned before, we are also interested in how the effect of LU on prices depends on the type of price regulation in place. To study this, we take advantage of a regulatory reform, which took place in 2009—a switch from rate-of-return regulation to incentive-based regulation. We therefore apply a triple-differences approach and estimate the following fixed-effects model:

$$\log(p_{it}) = \beta_1 \times LU_{it} + \beta_2 \times LU_{it} \times IR_t + \log(X_{it}) \times \gamma + \xi_i + \xi_t + \varepsilon_{it}, \qquad (2)$$

where *IR* takes a value of 1 during the time of incentive regulation from 2009 on, and 0 before 2009. As we now explicitly account for the regulatory reform, we can make use of the entire observation period 2005–2014 in this regression.

The results from estimating Equation (2) are presented in Column (1) of Table 6. The effect of LU is similar as before and suggests that LU causes a price decrease of 5.35% ($=100 \times (\exp(-0.055)-1)$) under rate-of-return regulation. Moreover, the estimate of the interaction term $LU \times IR$ suggests that the switch from rate-of-return regulation to incentive regulation causes an additional price decrease of 3.34% ($=100 \times (\exp(-0.034)-1)$) for legally unbundled utilities under incentive regulation compared to vertically integrated ones. In sum, under incentive regulation, prices are 8.69% lower for legally unbundled than for vertically integrated utilities.[22] This suggests that the type of price regulation matters indeed with regard to the efficacy of LU, and that the price effect of LU comes particularly into play under incentive regulation.[23]

22. The combined effect of *LU* and $LU \times IR$ is computed as $100 \times (\exp(-0.055)-1+\exp(-0.034)-1) = -5.35+(-3.34) = -8.69$.
23. To give a sense about the potential of legal unbundling regarding consumer welfare arising from the 9% difference in grid charges between DSOs that underwent legal unbundling and those that did not, we provide some back-of-the envelope calculations. In 2014, the last year of our observation period, the grid charge of a representative household with an electricity consumption of 4,000 kwh/year, which is located in a supply area with a legally unbundled electricity utility, was on average 257.4/year. Hence, using a simplified calculation, grid charges would have been 282.9/year per representative household in the absence of legal unbundling (257.4/0.91). Thus, under the current incentive regulation regime the 41.3 million households

In addition to the fixed-effects model, as given in Equation (2), we also estimate a model in which we replace the year fixed effects ξ_t by a yearly time trend (T) and its squared term (T^2). Although this specification may be less precise (i.e. time fixed effects are more flexible than the polynomial time trend), it has the advantage that it additionally allows for estimating the effect of the introduction of incentive regulation, which would otherwise be captured by the year fixed effects:

$$\log(p_{it}) = \beta_1 \times LU_{it} + \beta_2 \times LU_{it} \times IR_t + \beta_3 \times IR_t + \log(X_{it}) \times \gamma + \delta_1 T + \delta_2 T^2 + \xi_i + \varepsilon_{it}. \qquad (3)$$

The results are shown in Column (2) of Table 6 and are fully robust to the fixed-effects results as in Column (1). We find a price decrease of 4.7% (=100 × (exp(–0.048)–1)) following the implementation of LU. Again, the parameter estimate for the interaction term $LU \times IR$ suggests that under incentive regulation, there is an additional price effect for legally unbundled utilities in the magnitude of –3.44% (=100 × (exp(–0.035)–1)) compared to firms that remained vertically integrated. As all coefficients in Column (2) have similar magnitudes as the estimates in Column (1), the quadratic trend appears to mimic the year fixed effects reasonably well.

Table 6: Effects of LU and IR on grid charges for the period 2005–2014

	(1)	(2)	(3)	(4)
LU	–0.055**	–0.048**	–0.078***	–0.077***
	(0.023)	(0.023)	(0.019)	(0.021)
IR		0.049***		
		(0.010)		
LU × IR	–0.034**	–0.035**		
	(0.017)	(0.018)		
log(meter points)	–0.161**	–0.155**	–0.153**	–0.149**
	(0.068)	(0.068)	(0.069)	(0.070)
log(population density)	–0.004	–0.007	–0.005	–0.008
	(0.008)	(0.007)	(0.006)	(0.008)
log(solar capacity)	0.008	0.009	0.008	0.012
	(0.013)	(0.012)	(0.010)	(0.012)
log(wind capacity)	0.009***	0.008***	0.009***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.36	0.33	0.36	0.32
Observations	764	764	764	764

Dependent variable is log(price). Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

The estimates suggest that prices for vertically integrated firms are approximately 5% (=100 × (exp(0.049)–1)) higher under incentive regulation than under rate-of-return regulation. In contrast, we do not find that the type of price regulation affects pricing of legally unbundled firms as the combined effect of IR and $LU \times IR$ is only 1.58% (=(exp(–0.049) – 1 + exp(–0.035) – 1)), and statistically insignificant.[24] Hence, our estimates suggest that vertically integrated firms increased their prices as a result of the switch from rate-of return regulation to incentive regulation, whereas it had no effect for legally unbundled firms.

in Germany would have to pay 1,051 million per year less on grid charges if all DSOs were unbundled, compared to a scenario where all DSOs remained vertically integrated.

24. A t-test with the null hypothesis $\hat{\beta}_2 + \hat{\beta}_3 = 0$ gives a p-value of 0.39, indicating that the effect is not distinguishable from zero.

We also estimate the models without consideration of the introduction of the regulatory reform. This gives us an overall (weighted average) effect of LU on prices. The results with year fixed effects and with a polynomial time trend are reported in Columns (3) and (4) of Table 6, respectively. They suggest that without consideration of the price regulation regime, legal unbundling reduces prices by approximately 8% compared to vertical integration.

5.4 Robustness

Sensitivity to chosen threshold

Our main application excludes all DSOs with less than 30,000 or more than 170,000 connected customers. While the lower threshold emerges naturally, as DSOs with less than 30,000 customers are subject to a different price regulation scheme, the upper threshold was chosen to have symmetry around the threshold. However, it can be argued that the latter threshold is chosen arbitrarily. To ensure that our results are not driven by the choice of the upper threshold, we additionally estimate models for different thresholds. In the estimations presented in Table 7 we reduce the upper threshold by half (135,000) and in Table 8 we put no limit on the threshold and also include DSOs with more than 170,000 connected customers. The results remain almost unchanged.

Alternative consumption levels

A concern may be that grid charges are non-linear in consumption because they are set in the form of two-part tariffs consisting of a fixed and a per-unit price component. As a robustness check, we therefore also estimate the specifications from Table 6 for business consumers with a yearly electricity consumption of 50,000 KWh. The results are fully robust and are reported in Table 9 in the Appendix. For example, in specification (2) of Table 9, we find that the switch from vertical integration to LU leads to a price decrease for business customers by 4.4% ($=100\times(\exp(-0.045)-1)$), and that the switch from rate-of-return to incentive regulation brings about an additional decrease by –4.2% ($=100\times(\exp(-0.043)-1)$) compared to firms that remained vertically integrated.

To complete the picture, we also examine which component reacts to the unbundling measures and the incentive regulation. The results are reported in the Appendix Tables 10 and 11 and suggest that the per-unit charged price component is the strategic variable, while the fixed component remains unaffected by changes in the regulation.[25]

Endogeneity

There may also be concerns about endogeneity. One issue is that seven firms in our dataset unbundled voluntarily, hence self-selection into treatment may have an effect on our results. To test this, we exclude the seven voluntarily unbundled DSOs from our dataset and estimate the specifications from Table 6 again. The results remain robust as shown in Table 14.

Moreover, it is possible that some VIUs may have sold parts of their grid prior to the introduction of the legal unbundling regime in order to prevent mandatory legal unbundling. Hence, excluding the voluntarily unbundled VIUs may not be sufficient to fully exclude potential bias due to endogeneity arising from self-selection into treatment. However, there was no situation where a DSO initially exceeded the 100,000 customers threshold and fell below that threshold later on. Nevertheless, as a further robustness check we excluded those DSOs with between 90,000 and

25. Estimations for the fixed price component are in levels instead of logs because the fix price component is zero for a significant share of the DSOs in our sample (32%).

110,000 customers. The idea is that the risk of self-selection is highest for those DSOs. The results are presented in Table 13 and remain similar.

As a final robustness check aiming to address potential endogeneity concern, we apply the identification strategy recently proposed by Lewbel (2012, 2018). Lewbel (2012) provides an estimator for linear regression models containing an endogenous regressor when no outside instrument is available. In a nutshell, the method works by exploiting the model heteroskedasticity to construct instruments using the available regressors. Lewbel (2018) shows that the assumptions required for the proposed estimator can also be satisfied when an endogenous regressor is binary, as is the case with our LU variable. The results of the Lewbel IV estimations are reported in Table 15.[26]

As Lewbel (2012) shows, the model is identified if the errors from a regression of the endogenous variable on covariates from the main model are heteroskedastic and the variance of these errors is correlated with at least some of the covariates but not with the covariances of these errors and the second stage errors. We test the heteroskedasticity requirement based on the residuals of the first stage regression, using a modified Wald statistic for groupwise heteroskedasticity. The test rejects the null hypothesis of constant variance, as can be seen in Table 15. Table 15 also shows that the generated instruments are sufficiently strong to identify the LU variables according to the First-Stage F-statistic and the Kleibergen-Paap F-statistic, as the Stock and Yogo (2005) critical values are always exceeded. The instruments are also not correlated with the error term, as shown by the Hansen *J* test. The Durbin-Wu-Hausman test does not point towards an endogeneity issue, as it does not reject the exogeneity hypothesis of the unbundling introduction. Nevertheless, the results turn out to be consistent with the estimations presented earlier, supporting our main results.

Placebo test

Finally, we conduct a placebo test in order to exclude other possibilities that could drive our results. As discussed earlier, an important assumption for the validity of the difference-in-differences estimation is that prices in the treatment and the control group would have had developed parallel in the absence of LU. We have already shown that the grid charges of treated and untreated firms are parallel prior to treatment and that treated and untreated firms do not differ significantly in their observable characteristics. Hence, we do not think that our results are due to a violation of the common trends assumption.

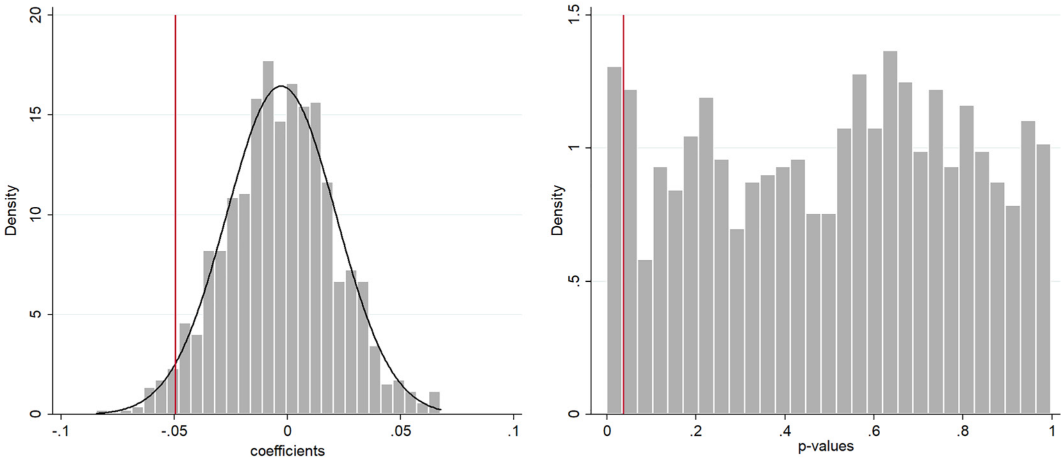
Another common concern about difference-in-differences estimation is that the standard errors may understate the standard deviation of the estimators due to serial correlation (Bertrand et al., 2004). As with the previous concern, this problem is also unlikely in our case, as we correct for arbitrary serial correlation by clustering standard errors at the utility level.

In any event, to give us more confidence, we randomly assign a placebo LU year to each utility from the treatment group (i.e. DSOs that implemented LU during our sample period). We then run the specification as given in Equation 2 and store the coefficient as well as the p-value of the LU dummy. We repeat this procedure 1,000 times. Figure 2 shows the distribution of the resulting placebo coefficients and their p-values. The vertical lines represent the actual LU coefficient and its p-value from Column (3) in Table 6. The placebo coefficients are centered around zero (the mean is 0.003) and their p-values exceed the 10% value in 89.5% of the cases. Moreover, the p-values of the placebo coefficients exceed the true p-value of our actual LU coefficient in 97.3% of the cases.[27]

26. A technical description of the required assumptions for the Lewbel IV estimation and a brief description on the procedure itself are provided in the Appendix.

27. The p-value for the LU coefficient from Column (1) in Table 6 is 0.017.

Figure 2: Placebo results



The left panel presents distribution of the placebo coefficients (1,000 repetitions), the right panel the distribution of the corresponding p-values. The red vertical lines present the values from the estimation in Column (3) of Table 5. The black solid line illustrates a normal distribution.

This is very close to random chance, giving us further confidence that our findings are not caused by a violation of the common trends assumption or by autocorrelation.

6. CONCLUSION

The unbundling of vertically integrated utilities has become a cornerstone in the regulation of network industries and is applied in many jurisdictions around the globe. However, though enforced unbundling constitutes a significant market intervention, its actual efficacy is still ambiguous and debated controversially. Particularly in the case of ownership unbundling, the literature has found that it is associated with a loss of vertical scope economies. The literature argues that the loss of vertical economies may be less pronounced with *legal unbundling*, whereas it may similarly reduce incentives to distort downstream competition. However, it is particularly not clear if the latter effects actually translate into practice, since the empirical literature on the deemed competitive effects of (any form of) unbundling is still scarce and, to this date, there is no robust evidence on its price effects.

This paper contributes to the discussion by examining the effect of legal unbundling on grid charges using a panel dataset of German electricity utilities, some of which underwent legal unbundling, while others did not. Moreover, during our sample period 2005–2014, we observe a switch in the regulatory regime from rate-of-return to incentive regulation. This additionally enables us to examine the heterogeneity of the effect of unbundling on grid charges for the two most prevalent price regulation regimes.

We find that legal unbundling is indeed successful in reducing grid charges by around 5% to 9%, depending on the type of price regulation in place. While legally unbundled utilities subject to rate-of-return regulation charge around 5% lower grid charges compared to VIUs, under incentive regulation the prices are even around 9% lower. Our findings are important insofar as we extend the relatively limited literature on the potential benefits of unbundling in general and of legal unbundling in particular. Given that the loss of vertical economies should become minimized with legal unbundling, while it still has a price decreasing effect, our findings suggest that legal unbundling may indeed represent a ‘golden mean’.

Fortunately, the combination of legal unbundling together with incentive regulation is applied widely in electricity and gas distribution markets in Europe and the USA. Our results are also promising for other countries that are considering introducing unbundling measures, such as Japan. Nevertheless, we have to point out that our study also has limitations due to the limited sample period, which only allows us to examine short-term effects but not long-term effects (e.g. long-run investment incentives). This may provide room for future research on this topic. Finally, it may be helpful to develop theoretical models in order to attain a better understanding of the interaction between different price regulation regimes and unbundling measures.

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APPENDIX

A.1 Figures

Figure 3: Number of DSOs in Germany

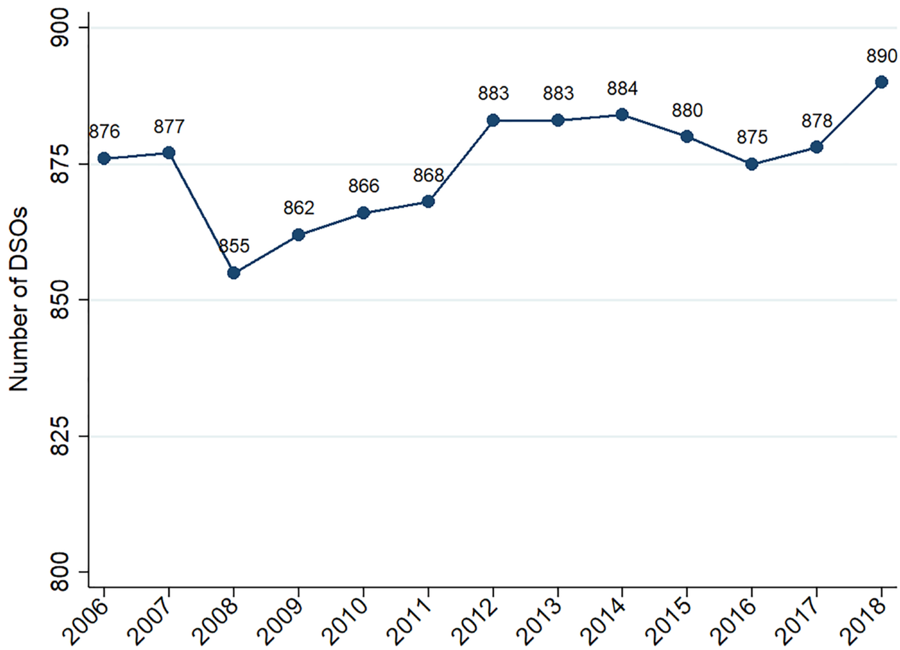
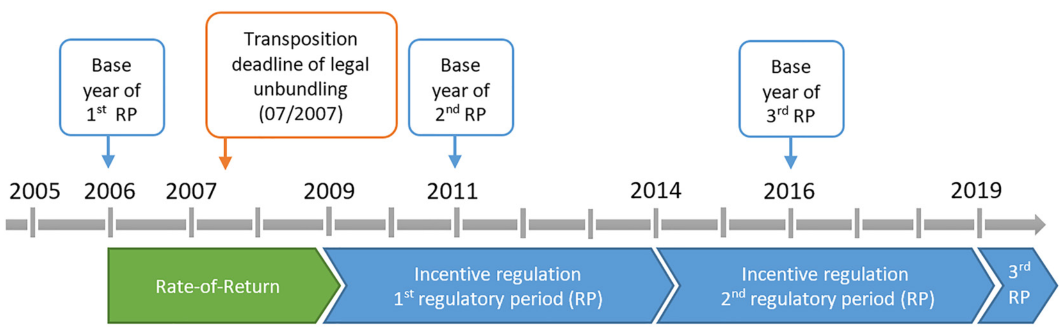


Figure 4: Regulatory regimes



A.2 Robustness checks

Table 7: Effects of LU and IR on grid charges for the period 2005–2014: DSOs with more than 135,000 connected customers excluded

	(1)	(2)	(3)	(4)
LU	–0.050**	–0.041*	–0.078***	–0.074***
	(0.023)	(0.024)	(0.021)	(0.022)
IR		0.051***		
		(0.011)		
LU × IR	–0.041**	–0.042**		
	(0.019)	(0.020)		
log(meter points)	–0.167**	–0.161**	–0.156**	–0.152**
	(0.068)	(0.068)	(0.071)	(0.071)
log(population density)	–0.004	–0.007	–0.005	–0.008
	(0.008)	(0.007)	(0.006)	(0.008)
log(solar capacity)	0.012	0.012	0.012	0.015
	(0.014)	(0.013)	(0.011)	(0.013)
log(wind capacity)	0.008***	0.007**	0.008***	0.007**
	(0.003)	(0.003)	(0.003)	(0.003)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.335	0.305	0.328	0.291
Observations	660	660	660	660

Dependent variable is log(price). Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 8: Effects of LU and IR on grid charges for the period 2005–2014: DSOs with more than 170,000 customers also included

	(1)	(2)	(3)	(4)
LU	–0.042**	–0.035*	–0.066***	–0.067***
	(0.021)	(0.020)	(0.017)	(0.018)
IR		0.049***		
		(0.009)		
LU × IR	–0.035**	–0.036**		
	(0.016)	(0.016)		
log(meter points)	–0.052	–0.052	–0.049*	–0.050
	(0.032)	(0.032)	(0.030)	(0.032)
log(population density)	0.009	0.013	0.009	0.010
	(0.018)	(0.017)	(0.016)	(0.018)
log(solar capacity)	0.013	0.013	0.012	0.015
	(0.013)	(0.012)	(0.009)	(0.011)
log(wind capacity)	0.007***	0.006**	0.007***	0.006**
	(0.002)	(0.003)	(0.003)	(0.003)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.39	0.36	0.39	0.35
Observations	1087	1087	1087	1087

Dependent variable is log(price). Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 9: Estimating the effect of LU and IR for the period 2006–2014: Estimation for business customers with a yearly electricity consumption of 50,000 kWh

Dependent variable is *log(price)*	(1)	(2)	(3)	(4)
LU	–0.050**	–0.045*	–0.079***	–0.079***
	(0.023)	(0.023)	(0.020)	(0.021)
IR		0.055***		
		(0.012)		
LU × IR	–0.043**	–0.043**		
	(0.019)	(0.019)		
log(meter points)	–0.108	–0.103	–0.099	–0.095
	(0.074)	(0.074)	(0.073)	(0.074)
log(population density)	–0.002	–0.006	–0.003	–0.006
	(0.008)	(0.008)	(0.006)	(0.008)
log(solar capacity)	0.009	0.012	0.010	0.014
	(0.014)	(0.013)	(0.010)	(0.012)
log(wind capacity)	0.011***	0.010***	0.011***	0.010***
	(0.003)	(0.003)	(0.003)	(0.003)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.31	0.27	0.30	0.26
Observations	764	764	764	764

Dependent variable is log(price). Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 10: Effects of LU and IR on the per-unit charged price component of the grid charges for the period 2005–2014

	(1)	(2)	(3)	(4)
LU	–0.046*	–0.042*	–0.073***	–0.074***
	(0.024)	(0.023)	(0.020)	(0.021)
IR		0.055***		
		(0.013)		
LU × IR	–0.041**	–0.041**		
	(0.019)	(0.020)		
log(meter points)	–0.169**	–0.162*	–0.162*	–0.157*
	(0.086)	(0.086)	(0.085)	(0.087)
log(population density)	–0.004	–0.007	–0.004	–0.008
	(0.007)	(0.007)	(0.006)	(0.007)
log(solar capacity)	0.010	0.012	0.010	0.014
	(0.014)	(0.013)	(0.010)	(0.013)
log(wind capacity)	0.011***	0.009***	0.011***	0.010***
	(0.003)	(0.004)	(0.003)	(0.004)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.314	0.284	0.307	0.271
Observations	760	760	760	760

Dependent variable is log(per-unit charged price component) of the grid charges. Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 11: Effects of LU and IR on the fix price component of the grid charges for the period 2005–2014

	(1)	(2)	(3)	(4)
LU	–2.381	–1.630	–1.579	–0.951
	(1.671)	(1.552)	(1.308)	(1.520)
IR		–0.838		
		(0.720)		
LU × IR	1.228	0.991		
	(0.894)	(0.877)		
log(meter points)	0.694	1.261	0.481	1.094
	(4.557)	(4.422)	(4.401)	(4.468)
log(population density)	–0.372	–0.287	–0.358	–0.276
	(0.371)	(0.353)	(0.346)	(0.357)
log(solar capacity)	–0.028	–0.250	–0.045	–0.273
	(0.411)	(0.440)	(0.306)	(0.435)
log(wind capacity)	–0.172	–0.176	–0.182	–0.186
	(0.356)	(0.355)	(0.258)	(0.360)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.054	0.042	0.050	0.040
Observations	760	760	760	760

Dependent variable is fix price component of the grid charges. Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 12: Estimation of the effects of LU and IR for the period 2005–2014: DSOs that unbundled before 2005 excluded

	(1)	(2)	(3)	(4)
LU	–0.055**	–0.048**	–0.079***	–0.078***
	(0.024)	(0.023)	(0.019)	(0.021)
IR		0.049***		
		(0.011)		
LU × IR	–0.035**	–0.036*		
	(0.018)	(0.018)		
log(meter points)	–0.161**	–0.156**	–0.153**	–0.150**
	(0.069)	(0.069)	(0.069)	(0.071)
log(population density)	–0.001	–0.004	–0.001	–0.004
	(0.006)	(0.006)	(0.006)	(0.007)
log(solar capacity)	0.006	0.008	0.006	0.010
	(0.013)	(0.012)	(0.010)	(0.012)
log(wind capacity)	0.008***	0.007**	0.009***	0.007***
	(0.003)	(0.003)	(0.003)	(0.003)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.37	0.34	0.37	0.33
Observations	734	734	734	734

Dependent variable is log(price). Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 13: Estimation of the effects of LU and IR for the period 2005–2014: DSOs with between 90,000 and 110,000 connected customers excluded

	(1)	(2)	(3)	(4)
LU	–0.071***	–0.065***	–0.096***	–0.095***
	(0.025)	(0.025)	(0.021)	(0.022)
IR		0.045***		
		(0.011)		
LU × IR	–0.039**	–0.040**		
	(0.017)	(0.018)		
log(meter points)	–0.098	–0.090	–0.090	–0.076
	(0.109)	(0.109)	(0.112)	(0.110)
log(population density)	–0.008	–0.012	–0.009	–0.013
	(0.009)	(0.009)	(0.008)	(0.009)
log(solar capacity)	0.008	0.008	0.009	0.011
	(0.013)	(0.012)	(0.010)	(0.011)
log(wind capacity)	0.009***	0.008***	0.010***	0.009***
	(0.003)	(0.003)	(0.003)	(0.003)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.36	0.33	0.35	0.32
Observations	692	692	692	692

Dependent variable is log(price). Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 14: Estimation of the effects of LU and IR for the period 2005–2014 after exclusion of voluntarily unbundled DSOs

	(1)	(2)	(3)	(4)
LU	–0.062***	–0.055**	–0.079***	–0.080***
	(0.023)	(0.023)	(0.019)	(0.021)
IR		0.049***		
		(0.010)		
LU × IR	–0.027	–0.027		
	(0.017)	(0.018)		
log(meter points)	–0.160**	–0.154**	–0.154**	–0.150**
	(0.070)	(0.071)	(0.070)	(0.072)
log(population density)	–0.000	–0.003	–0.000	–0.003
	(0.006)	(0.006)	(0.005)	(0.006)
log(solar capacity)	0.003	0.005	0.003	0.008
	(0.013)	(0.012)	(0.010)	(0.011)
log(wind capacity)	0.007***	0.006**	0.008***	0.007**
	(0.002)	(0.003)	(0.003)	(0.003)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
R^2	0.40	0.37	0.39	0.35
Observations	723	723	723	723

Dependent variable is log(price). Standard errors clustered at the DSO level in parenthesis. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

Table 15: Lewbel (2012) IV estimation of the effects of LU and IR for the period 2005–2014

	(1)	(2)	(3)	(4)
$\widehat{\text{LU}}$	–0.064**	–0.059**	–0.088***	–0.085***
	(0.025)	(0.026)	(0.025)	(0.027)
IR		0.052***		
		(0.011)		
$\widehat{\text{LU} \times \text{IR}}$	–0.038**	–0.045**		
	(0.017)	(0.018)		
log(meter points)	–0.163**	–0.158**	–0.155**	–0.149**
	(0.068)	(0.067)	(0.073)	(0.071)
log(population density)	–0.003	–0.006	–0.004	–0.007
	(0.008)	(0.007)	(0.008)	(0.008)
log(solar capacity)	0.008	0.009	0.009	0.012
	(0.013)	(0.012)	(0.013)	(0.011)
log(wind capacity)	0.009***	0.008***	0.009***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
DSO Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	N	Y	N
Time trend (T & T^2)	N	Y	N	Y
First-stage Wald test for group heteroskedasticity (p-val.)	0.00	0.00	0.00	0.00
First-stage F stat.	—	—	364.21	48.19
Kleibergen-Paap F stat.	74.85	37.88	—	—
Critical value for weak instruments (10%) by Stock and Yogo	11.06	10.89	11.52	11.12
Hansen J stat. (p-val.)	0.52	0.75	0.70	0.34
Durbin-Wu-Hausman test	0.34	0.70	0.42	0.73
Observations	778	778	778	778

Dependent variable is log(price). Standard errors corrected for using generated instruments and clustered at the DSO level in parenthesis. *LU* and *IR* × *LU* treated as endogenous and are instrumented using Lewbel's (2012) heteroskedasticity based IV approach. Estimation is done by GMM. *** $p < 1\%$, ** $p < 5\%$, * $p < 10\%$.

A.3 Technical description of Lewbel's (2012) IV method

Consider the linear relationship $Y = X\beta + Z\gamma + \varepsilon_1$, where Z is the potentially endogenous variable (the LU dummy in our case) and γ is the parameter we wish to estimate. The equation that determines Z is $Z = X\alpha + \varepsilon_2$, where ε_1 and ε_2 may be correlated and no element of X can be used as an instrument, i.e. there is no outside instrument available. As usual, the requirement is that $E\left(X\varepsilon_1\right) = 0$, $E(X_i\varepsilon_2) = 0$, and that $E\left(XX'\right)$ is non-singular. The additional assumptions for the identification in the absence of an outside instrument are that $Cov\left(X, \varepsilon_1\varepsilon_2\right) = 0$ and that there is some heteroskedasticity in the error of the first-stage, $Cov\left(X, \varepsilon_2^2\right) \neq 0$. If these assumptions hold, the variation in ε_2 can be used to identify the model parameters. γ (and β) can then be estimated consistently by using interactions of the mean-centered control variables and the residuals ($\left(X - \bar{X}\right)\hat{\varepsilon}_2$) to instrument for Z.

The estimation procedure is then as follows:

1. Estimate $\hat{\alpha}$ by an OLS regression of Z on X to obtain $\hat{\varepsilon}_2 = Z - X\hat{\alpha}$.
2. Use the interactions of the residuals $\hat{\varepsilon}_2$ and the mean-centered covariates $(X - \bar{X})$ as instruments for Z and estimate $Z = X\alpha + \gamma\left(X - \bar{X}\right)\hat{\varepsilon}_2 + \varepsilon_3$.
3. Obtain $\hat{\beta}$ and $\hat{\gamma}$ by estimating $Y = X\beta + \hat{Z}\gamma + \varepsilon_4$.

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