You Get What You Pay For: Electricity Quality and Firm Response

Meera Mahadevan*

November 30, 2022

Abstract

Electricity sectors in many parts of the developing world are composed primarily of state-owned electricity utilities that make massive losses. In India, the sector often costs the central government billions of dollars a year to bail out. These losses are driven by the prevailing low prices of electricity, non-payment of bills or theft, and non-metering of electricity use. This creates a vicious cycle where electricity is treated as an entitlement, with low utility revenues, unreliable supply, and low willingness-to-pay for electricity. Using a large reform in the sector, I show that differing implementation of this reform across states generated variation in the institutional structure of electricity provisioning. In particular, I find that some states were able to improve the reliability of their electricity supply. In response, manufacturing firms scaled up their consumption of grid electricity, showing a willingness to pay higher average prices for reduced blackouts. Firms also re-optimize their production decisions: increasing purchased electricity, worker hours, and manufacturing output. These results demonstrate that some institutional changes could potentially propel the sector out of the low-price/low-electricity equilibrium.

Keywords: Institutions, India, Public Service Delivery
JEL Classification Codes: H41, Q41, O13, O38

*University of California, Irvine, meera.m@uci.edu, meeramahadevan.com. I am grateful to Ryan Kellogg, Shaun McRae, Catherine Hausman, Steve Cicala, Julie Cullen, Fiona Burlig, Paige Weber, Martha Bailey, Dmitriy Stolyarov, Dean Yang, and Hoyt Bleakley for their helpful discussion and comments. Thanks also to the seminar participants at the University of Michigan, the University of California at Santa Barbara, Young Scholars Symposium 2022, and Camp Resources XXVII.

1 Introduction

Public provision of goods and services is omnipresent across the world, particularly for large utilities such as water and electricity. However, the quality of these services depends heavily on institutional structures, pricing, reforms, and investment. Policy-makers around the developing world grapple with several considerations in trying to provide affordable and reliable electricity access. Yet, utilities make heavy losses while having little requirement to meet demand in a reliable manner, skewing the marginal valuation of electricity for consumers. In such contexts, therefore, the aims of universal access or equitable pricing face several institutional and structural challenges, that may require large-scale reform.

In India, millions of consumers connected to the electrical grid face blackouts and brownouts primarily because state-owned utilities make large financial losses, and are forced to restrict electricity supply (Burlig, Jha, and Preonas, 2020). Some of the large reasons for utility losses are the lower-than-market prices for most consumer groups, high bill non-payment, and theft or unmetered supply: Indeed, Burgess, Greenstone, Ryan, and Sudarshan (2020) discuss the presence of a vicious cycle, where low electricity prices and bill non-payment contribute to financial losses for utilities and unreliable electricity. Consumers, in turn, are not willing to pay for an irregular electricity supply,[1] and hours of supply are often a binding constraint on their consumption. Yet, in order to break out of this low-electricity equilibrium, either revenue collection or reliability has to change.[2]

In this paper, I derive variation from a large set of electricity reforms in India that were designed to improve production efficiency in electricity production and distribution by unbundling vertically-integrated energy utilities. Specifically, some states implemented the reforms to end up with a single electricity utility while others ended up with multiple distributors, with likely unintended consequences on both prices and reliability. This variation provides a natural experiment where different states end up with different electricity-provisioning institutions. I leverage the differential effects of single versus multiple distributors on prices and reliability and examine how institutional factors may contribute to better management of utilities and a way out of the low-electricity equilibrium.

I leverage variation in changing institutional structures across states from these reforms, allowing me to estimate impacts on price, reliability, and overall electricity consumption in

[1] Potentially perpetuating low infrastructure investments (McRae, 2015).

[2] Chatterjee (2020) provides multiple examples of successful lobbying by farmers' groups and urban residents in several South Asian countries, including India, to keep electricity tariffs low. Once cheap electricity is seen as a right, any price increases are met with public opposition.

Indian manufacturing. A majority of surveyed Indian firms report electricity outages as one of the biggest obstacles to production (World Bank, 2014). Electricity outages damage firm productivity in the short run (Fisher-Vanden et al., 2015), and even modest improvements can boost productivity significantly (Allcott et al., 2016). Indeed, in the longer term, improvements in reliability could increase the productivity of firms by as much as 25% (Fried and Lagakos, 2020). This is in addition to the wide-ranging implications extensive-margin electricity access has for economic development more broadly (Dinkelman, 2011; Lipscomb, Mobarak, and Barham, 2013), and for industrial development (Kassem, 2021) and manufacturing (Meeks et al., 2021) in particular. Studies examining the effect of outages on firms often rely on survey data, theoretical models, or temporary exogenous shocks that vary power availability, and focus on a relatively static state of the electricity sector. But, there is little empirical evidence on what factors may fix electricity reliability issues in the long run, and how permanent transitions or large-scale reforms to institutional frameworks may affect firm outcomes.

From 1998 to 2003, the Indian government passed the Electricity Act reforms, which sought to unbundle generation from the transmission and distribution sectors in order to improve efficiency in provision (Ministry of Law and Justice, 2003). In India, electricity is largely state-owned and provided.[3] For decades, electricity had been generated, transmitted, and distributed via large, inefficient vertically-integrated utilities leading to large supply problems such as widespread blackouts. Importantly (for empirical identification), different states split up the three sectors – generation, transmission, and distribution – in different manners. Some partially unbundled the components, whereas others completely unbundled all electricity firms. As a result of the reforms, a few states ended up with a single statewide electricity distributor, while others ended up with multiple. I focus on this particular tenet of the reforms in institutional structures to understand what factors resulted in better electricity supply.[4]

Since, historically, electricity is under-priced (Burlig et al., 2020), these utilities often make annual appeals to raise prices, citing their costs and requesting price increases. However, regulators have a few different considerations: equitable prices within a state, and preventing steep price rises that would hurt consumers. Therefore, their incentives are often at odds with each other. Theoretically, under a single statewide distributor, one possible outcome is that revising prices upwards is easier to negotiate with the regulator, who does

[3] As is the case in several countries across South Asia, Central Asia, and Latin America.

[4] Other provisions of the reform were largely uniformly implemented across states (Pargal and Banerjee, 2014).

not need to compare prices with another utility potentially charging lower rates.[5] Higher revenues may lead to greater electricity reliability as the utility is better able to meet demand. On the other hand, states with multiple utilities may perform better as the competition among utilities on revenue collection and electricity supply can incentivize reliable quality. As such, it is theoretically ambiguous which configuration may perform better.

I use an event study design to estimate the causal effects of each type of reform on the manufacturing sector outcomes. I simultaneously estimate the effects of the different types of unbundling, comparing outcomes in states that received a specific type of unbundling to states that would receive it in the future or never receive it at all. In doing so, I can simultaneously discern the dynamics in outcomes as a result of each type of institutional structure. The control group for each type of unbundling includes states that have not unbundled or have not adopted any form of unbundling yet. Using an event study allows there to be level differences across these states, but requires parallel trends in the absence of reforms. Since each state chose its own configuration, my event study design helps isolate the Average Treatment Effect on the Treated (ATT), rather than an Average Treatment Effect (ATE), as a different assignment rule may produce different estimates.[6]

I investigate outcome dynamics in two stages. First, I examine the effects of each reform-institution structure on the quality of electricity supply and the retail unit price of electricity paid by firms (and relatedly, the amount of electricity consumed). Second, I study how firms respond to changing prices and reliability. The reforms were phased in over a six-year period, and the staggered implementation of reforms allows me to study the impacts free of coincident changes in any one particular year.[7] I study the effect of these reforms on manufacturing firms in India, using data from the Annual Survey of Industries (ASI) between 1998 and 2012, spanning a major portion of the enactment period of these reforms. I examine how the reforms affected the average price-per-unit of electricity (reported by firms), the total

[5]With a single distribution entity for the state, there is greater accountability, and therefore better bill-collection rates and more efficiently designed cross-subsidies in tariffs (Pargal and Banerjee, 2014), leading to better electricity reliability. This is in contrast with the difficulties of coordinating tariff increases or designing jurisdiction-spanning cross-subsidies in states with multiple non-competing utilities (Foster and Rana, 2020, discuss this in the context of Peru).

[6]I follow recent work on staggered-entry difference-in-differences designs (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; de Chaisemartin and D'Haultfuille, 2020) and adapt the proposed solutions outlined by Callaway and Sant'Anna (2020) and Sun and Abraham (2020) and present event study graphs.

[7]I empirically investigate challenges to the identification assumptions by testing for differential pre-trends. The identification assumption, as is the case with such designs, is that the trends across states with different configurations would have been similar in the absence of the reform – the baseline levels across states could still be quite different.

amount of electricity purchased, total output, and own-generated electricity.[8] I further obtain satellite data on night-time lights as a proxy for electricity outages (Alam, 2015).[9]

I find that in states with one unified distribution network, there was greater reliability in electricity supply, while there is no evidence of positive impacts on reliability under other institutional structures. Interestingly, firms responded to the greater electricity reliability with higher consumption of grid-generated electricity, despite the higher per-unit prices, suggesting that firms may be willing to pay more for better electricity quality. Consequently, these manufacturing firms benefited in terms of increased worker hours. The price increases that accompanied the increased reliability do induce a fall in the extensive margin of electricity consumption: smaller firms may switch away from grid power when prices rise, to generating their own electricity, which I find also increases on the extensive margin.

It may sound counter-intuitive that less competition from just a single distributor is good for consumers. Yet, with a single distribution entity for the state, there is likely greater accountability, and therefore better bill-collection rates and more efficiently designed cross-subsidies in tariffs (Pargal and Banerjee, 2014), leading to better electricity reliability, in contrast with the difficulties of coordinating tariff increases or designing jurisdiction-spanning cross-subsidies in states with multiple non-competing utilities (Foster and Rana, 2020).[10] While the reforms led to a reduction in power outages overall (Malik et al., 2011), this paper shows that these reductions were driven by certain regions, and this result is crucial in determining how future policy should be designed if we are to break out of the vicious cycle in Indian electricity. Such unexpected results demonstrate the unintended effects of reform interpretation and implementation (Sen and Jamasb, 2012), and highlight the importance of understanding these varied implications when designing policies.

It is difficult to unequivocally establish why some states chose one reform implementation strategy over another – indeed, there may be lingering questions over whether the estimates of reforms by certain states are applicable to other states that did not choose a similar reform (as with many analyses that estimate a Treatment on the Treated ATT rather than an Average Treatment Effect ATE). While states that chose certain types of reforms likely differ in levels-outcomes, reassuringly, the event study results indicate that for the primary variables, such as average electricity price and electricity consumption, parallel pre-trends

[8]Utility reported prices are not available for a majority of states studied.

[9]This is also commonly used in the literature as a proxy for electrification (Mahadevan, 2021; Mann et al., 2016; Min and Golden, 2014; Burlig and Preonas, 2017).

[10]Qualitative evidence from India's Annual Revenue Reports for multiple states 2003-2011 suggest that cross-subsidies in tariffs (charging industrial users more and residential users less) were made more efficient in states with single utilities, and they also do better on bill collection rates.

may be established.

Most broadly, this paper contributes to the growing literature on the importance of energy, and specifically electricity supply, in developing countries. Prior work has focused more on electrification itself, and its effects (Lipscomb et al., 2013; Dinkelman, 2011; Greenstone and Jack, 2015; Burlig and Preonas, 2017), more than reliability. I find that increases in power supply reliability enable firm growth.

This paper also contributes to the literature on the political economy of electricity prices. Electricity, being important to voters (Chhibber et al., 2004), there is political pressure to keep prices low (Chatterjee, 2018). State electricity utilities in India widely under-price their electricity to subsidize select groups (Burgess et al., 2020; Burlig et al., 2020) or engage in corrupt practices with the same end (Mahadevan, 2021). Such subsidies sometimes lead to perverse incentives and reduce infrastructure investment and electricity reliability (McRae, 2015). However, I show that firms are willing to pay higher prices for better quality supply, which could facilitate a move towards a better overall equilibrium. This indicates that blackouts were likely a binding constraint on consumption previously, and when relaxed due to reduced outages, consumption goes up despite higher marginal prices.

Finally, I contribute to a broader literature on the consequences of electricity reforms, by providing among the first studies in a developing-country context (Malik et al., 2011). In contrast to the previous literature studying electricity restructuring in the US, there is scope for increased prices and reliability in developing countries. I find that the reforms enabled utilities to charge higher prices, and as a result, were able to provide more reliability. In contrast, there is mixed evidence from the work on the effects of restructuring in the US. There may be gains in the efficiency of power generation from reforms (Fabrizio et al., 2007; Knittel, 2002; Davis and Wolfram, 2012; Cicala, 2015, Forthcoming), but because electricity access is universal, there are no clear benefits to higher prices, and indeed prices not falling were considered adverse outcomes (Apt, 2005; Mansur, 2007; Wolfram, 1999; Borenstein et al., 2002; Joskow and Kahn, 2002).

The rest of the paper is organized as follows: Section 2 discusses the institutional context in India, while Section 3 describes the datasets used in this study. Section 4 lays out the empirical strategy, followed by results in Section 5. Finally, Section 6 discusses the results and describes how to interpret them, and Section 7 concludes.

2 The Power Sector in India

The Indian power sector was historically characterized by insufficient generating capacity, frequent power blackouts, and inefficient transmission and distribution infrastructure. The tariff structure that was in place did not allow cost recovery for power plants and distributors, and therefore the electricity sector was highly subsidized to keep it operational (Tongia, 2003). The low prices charged to households and agricultural users of electricity were partially compensated for by charging higher prices for industrial users. However, often, these industrial consumers would generate their own electricity to avoid paying such high tariffs. The power sector was making losses in the late 1990s to the tune of Rs. 250 billion (almost $6 billion), with cost-recovery continuously declining (Pargal and Banerjee, 2014), followed by government bailouts.

Given the unsustainability of these losses, in 1998 and 2003, the Indian government passed the Electricity Acts (EAs) in order to consolidate the reforms enacted in the past decade and address issues facing the sector. Eighty-five percent of the state-owned coal-based power plants were unbundled from the transmission and distribution sectors. While the generation companies remained state-owned, they were now independent of the distribution and transmission companies and therefore had greater autonomy in their decisions. While the separation of generation from downstream supply remains the main goal of the reforms, there were some differences in how states executed this separation: with some taking an additional step of separation transmission from distribution. Due to historical institutional structures, it was administratively easier for some states to end up with a single distribution firm, while others ended up with multiple firms. Ex-ante, there was no reason to expect states with multiple distribution companies to perform any differently from those with single distribution companies.

As a part of the EA 2003, apart from unbundling generation companies from the vertically integrated power firms, independent regulators were made mandatory at the central (Central Electricity Regulatory Commission, CERC) and state level (State Electricity Regulatory Committees or SERCs, Singh (2006)). This was done to increase the internal accountability of the power utilities and separate them from the lack of transparency and politically motivated decisions that marked the sector when it was under the State Electricity Boards (SEBs), who monitored them before the reform.

3 Data and Variables

My main source of data on manufacturing firms is from the Annual Survey of Industries (ASI) for the years 1998-99 until 2011-12. This data spans the period covering the Electricity Acts, through the 2000s, which saw the enforcement of the 2003 Act.

To study the impact of the restructuring, one of the key variables I look at is the price paid by the firm for per unit of electricity. I calculate the price per unit by dividing the total purchase amount of electricity by the number of units consumed. When either one of these variables is missing, I use the reported rate per unit when it is available.[11]

Table 1 presents summary statistics of the main variables I use for this paper. I use data on the number of days a firm is operational, number of worker days, wages, output produced, and inputs to the firms, such as amount of electricity consumed. I compute worker days and total wages by summing across all the different employees across each firm. Based on the data, around a third of the firms surveyed had some electricity generation capacity, and among those who did, they were able to self-generate about 20% of their total electricity consumption.

The calculated per-unit prices of electricity are consistent with average annual tariff prices for manufacturing firms reported by the World Bank. Finally, I restrict the dataset to only those firms that are privately owned. This is because government-owned firms may get preferential rates, and would not accurately reflect the effect of these reforms. I also restrict the data to include firms from only relatively electricity-intensive manufacturing industries (full list in table A2).[12]

I obtain information on the unbundling status of various states from their respective State Electricity Regulatory Commission (SERC) websites, and verify this data with Pargal and Banerjee (2014). The states are listed in Appendix Table A1.

Finally, I use luminosity data from the Defense Meteorological Satellite Program's Operational Landscan System (DMSP-OLS). This data is constructed as an annual average of satellite images of the earth taken daily between 20:30 and 22:00 local time. The raw data

[11] In certain cases, the quantity of electricity consumed is reported as 0, but the price of electricity is reported. In these cases, I report these observations as missing with respect to the price of electricity. I lose about 1% of observations through this step, because of the number of firms who reported consuming no electricity.

[12] Some variables, such as the quantity of electricity consumed and generated, total worker days, value of output and wages have outliers. I winsorize the data for these variables at the 1st and 99th percentile.

Table 1: Summary statistics for outcome variables of manufacturing firms (in a year)

Restricted Sample			
Variable	**Obs.**	**Weighted mean**	**SD**
No. of manufacturing days	436151	219.87	123.74
No. of non-manufacturing days	436151	8.38	38.32
No. of total operational days	436151	245.16	108.96
Quantity of electricity consumed (KWh) (mill.)	399344	0.45	1.28
Price of electricy consumed (Rs. per KWh)	399344	4.85	1.36
Quantity of electricity generated (KWh) (mill.)	139927	0.24	0. 84
Total number of worker days	415134	16080.13	32747.04
Total wages (all employees) (Rs.) (mill.)	415134	4.05	11.6
Value of finished goods (Rs.) (mill.)	294132	6.39	20.5
Worker productivity (wages/worker days)	415134	186.09	368.3
Worker productivity (total output/worker days)	151760	526.57	1934.58

Note: Annual Survey of Industries (1998-2012). SD - standard deviation of variable. Obs. - number of observations. Dataset restricted to privately-owned, electricity-intensive manufacturing firms. Time-horizon for outcome measures is one year (e.g., 219 manufacturing days per year). Monetary variables in current rupees (Rs.). Exchange rate approximately Rs. 66 per 1 USD, over the time period.

is at a 30-second resolution, which implies that each pixel in the raw data is roughly one square kilometer. I average over pixels within sub-districts. The raw luminosity data for each pixel is reported as a six-bit integer ranging from 0 to 63. Following the literature (Henderson et al., 2012), I use the log of the light density per capita. Since the average within a district is never 0, the natural log is taken. In the absence of any data on the quality of electricity and blackouts, this data serves as a good measure of extensive electricity supply and reliability (Burlig and Preonas, 2017; Alam, 2015), and an intensive margin indicator of electricity consumption as well (Mahadevan, 2021; Mann et al., 2016).

4 Empirical Strategy: Event Study Design

Using an identification strategy similar to Cicala (2015); Malik et al. (2011); Markiewicz et al. (2004), I exploit the variation in timing and nature of the restructuring in various Indian states in order to find their effect on electricity price, electricity reliability, and other manufacturing outcomes. I explore three forms of restructuring and identify (i) the marginal effects of separating only generation and distribution from transmission, (ii) separating all three sectors and setting up a single distribution company, and finally, (iii) setting up multiple distribution companies.

Given the various ways in which reforms could affect the electricity sector, I estimate the net effect of unbundling on various outcome variables. This would capture the overall effects on the manufacturing sector regardless of the channel through which restructuring could affect manufacturing. I conduct an event study analysis in order to understand the effects of these reforms on the outcomes of interest. These outcomes include the unit price of electricity, the amount of electricity consumed by a firm, their own-generated electricity, days worked, and total output of the firm. I estimate the following specification for my event study.

$$Y_{fist} = \sum_{z=1}^{3} \left[\sum_{y=-9, y \neq 0}^{7} \delta_{zy} Unbundled_{zs} \mathbb{1}(t - T_s = y) \right] + SERC_{st} + \theta_s + \theta_t + \theta_i + \nu_{fist} \quad (1)$$

The outcome is Y_{fist} for firm f, industry i, state s, and year t. The variable $Unbundled_{zs}$ refers to the nature of restructuring in a particular state s. When $z = 1$, it is an indicator variable that takes the value 1 for states where generation and distribution were separated from the transmission sector. This minimal unbundling occurs in all states that acted on the reform, but some states go further. Therefore, $z = 1$ indicates only the states that implemented this low level of unbundling. When $z = 2$, it is an indicator variable that takes the value 1 for states that separate generation from distribution and transmission, i.e. separate all three sectors, and end up with one state-wide distribution company. When $z = 3$, $Unbundled_{zs}$ is an indicator variable that takes the value 1 for states that set up multiple distribution companies. The omitted category include the states that did not undergo any unbundling at all.

So, for instance, the state of Maharashtra restructured in 2005 ($T_s = 2005$), and separated all three sectors (generation, transmission, and distribution), resulting in a single state-wide distribution company ($Unbundled_2 = 1$). Here, when I have a firm-level observation in year 2000 ($t = 2000$), the expression within the indicator function in Equation 1 is 1 when $t - T_s = -5$. Therefore, we would estimate the effects of unbundling in the year 2000, and state of Maharashtra through period$\delta_{2,-5}$. The EA 2003 also included the setup of an independent State Electricity Regulatory Commission (SERC). I include the indicator variable $SERC_{st}$, which takes a value of 1 for all years, including and after the year y in which a state s sets up the SERC, and 0 otherwise.

This regression specification also includes state, year, and industry fixed effects, as well as a linear time trend for each state. Given that my data is based on self-reported prices and outcomes by manufacturing firms, I use these fixed effects to control for year-specific, state-specific, and industry-specific differences. In my main specification, I exclude the year of unbundling, and so all outcomes should be interpreted as relative to that period. (Borusyak and Jaravel, 2017) also suggest robustness checks that avoid under-identification issues; and so in alternative appendix exercises, I restrict the pre-period to be zero for all periods (See Appendix Section A.3). I cluster my standard errors at the state level.[13]

In Figure 1b, the pre-trends of average electricity price are similar across all states, after controlling for the above fixed effects, providing some evidence that there were no pre-existing trends in the price data. The event study graphs allow us to understand the full set of dynamic changes and visually inspect the pre-trends, and by relying on changes that occurred in many different years, the variation is not driven by one single year that may have witnessed other aggregate shocks to the economy. I find that my results from this identification strategy on overall outages are consistent with those in Allcott et al. (2016); Malik et al. (2011), who use different identification strategies. Since the configurations were not randomized across states, states may have chosen the type of unbundling that may have best suited their context. As such, we are estimating a treatment on the treated (ATT) rather than an average treatment effect (ATE). In this case of multiple treatments, we should interpret the ATT of a specific configuration as relative to the other types of states.

I discipline my analysis with recent work on staggered-entry difference-in-differences designs (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; de Chaisemartin and D'Haultfuille, 2020). These papers show that in many instances, a traditional two-way fixed effects model does not recover easily interpretable estimates of the Average Treatment Effect (ATE) or the Treatment on the Treated (ATT). This is especially relevant in my instance, since the effects of the policy may evolve over time (i.e., the treatment effects are "dynamic"), and there may be heterogeneity in responses across states. I follow the proposed solutions outlined by Callaway and Sant'Anna (2020) and Sun and Abraham (2020), as their frameworks adhere closely to my context.[14] Since I have multiple treatments, I assume that the 'never treated with configuration of Type 1' control group includes any

[13] Given the multiple treatments, and the possibility of simultaneous confidence intervals Callaway and Sant'Anna (2020), inference is not straightforward. In Appendix A.2 I wild bootstrap my errors following Sant'Anna and Zhao (2020).

[14] Another set of solutions suggested by Cengiz et al. (2019) in their Online Appendix D uses "stacked-events" when studying changes to the minimum wage. In my context (unlike the minimum wage), a unit is treated only once, and the units are treated in relatively short spans of time.

state that did not ever implement the Type 1 reforms (only transmission separated).[15]

Following these papers, instead of a traditional two-way fixed effects model (pooled Diff-in-Diff), the event study I estimate carefully considers the effective comparison units (for instance, not having as a control, the previously treated units). Given the differential timing of the treatments, this implies that certain units will have more pre-treatment periods, and certain others will have more post-treatment outcome measures.

In Appendix A.2, I explore further specification tests suggested by Callaway and Sant'Anna (2020) and Sant'Anna and Zhao (2020), which include their doubly-robust inverse probability tilting and weighted least squares specifications, and inverse probability weighted OLS design with wild bootstrapped errors. I also vary the definition of the control groups, including (a) states that received no reform at all, (b) states that received a different reform, or (c) a combination of the two. My results are not sensitive to these definitions.

Rather than a traditional single ATT, the event study, in my case, estimates an ATT that varies over time and by treated group.[16] As in Sun and Abraham (2020), I estimate a dynamic treatment effect for each group, and fit the full set of possible relative time-indicator variables in the event study.[17]

5 Results

In this section, I present the results of the event study analysis using Equation 1. In appendix figures, I show confidence intervals for these estimated coefficients, clustered at the state level.

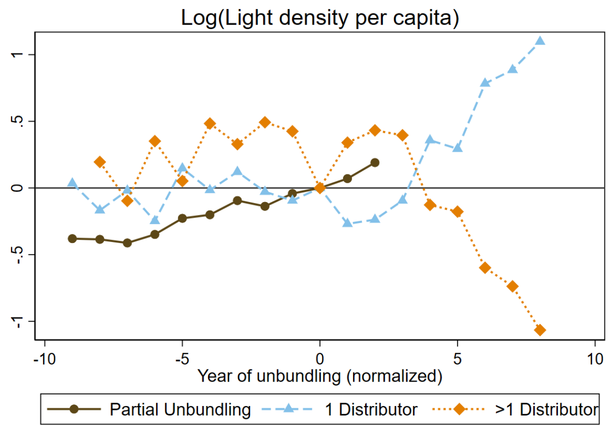
The line representing complete separation, with one distribution company (coefficient of $Unbundled_2$: δ_{2y}) shows the effect of completely separating the three sectors and ending up with a single distribution firm, compared to states that did not unbundle, or have not unbundled yet. The total effect in the case of this kind of complete separation with one distribution firm on electricity reliability and electricity consumption appears to be positive (Figures 1a, 1b and 1c). The net effect of complete separation with multiple distribution

[15] In Appendix A.2 I show robustness to the various definitions of the control group.

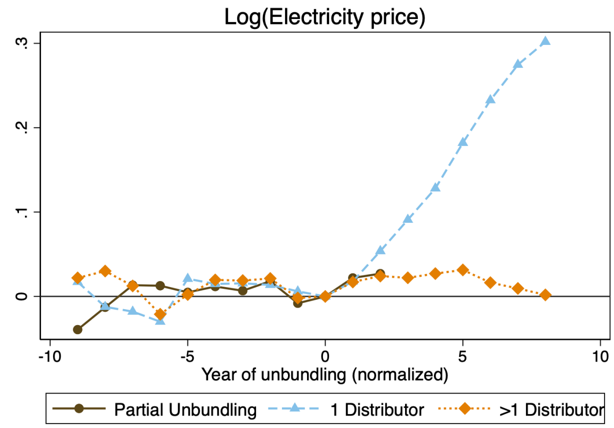
[16] See Goodman-Bacon (2018) for a decomposition of how the traditional two-way fixed effects ATT is a weighted average of each of the ATTs that vary over time and group. The paper also suggests diagnostic tests for when it is appropriate to use the traditional two-way fixed effects model.

[17] My setting has a few attractive features that minimize complications raised in these papers. First, the treatment does not switch on and off. Second, the treatment type does not change. And third, we do not necessarily need to condition on covariates (Callaway and Sant'Anna, 2020).

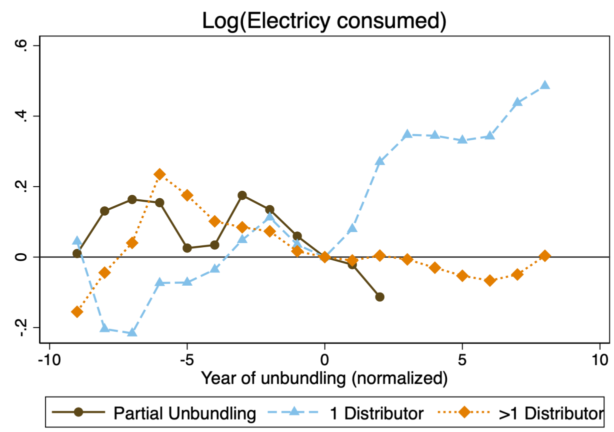
Figure 1: The effect of restructuring on electricity reliability and firm response



(a) Electricity Reliability



(b) Electricity price



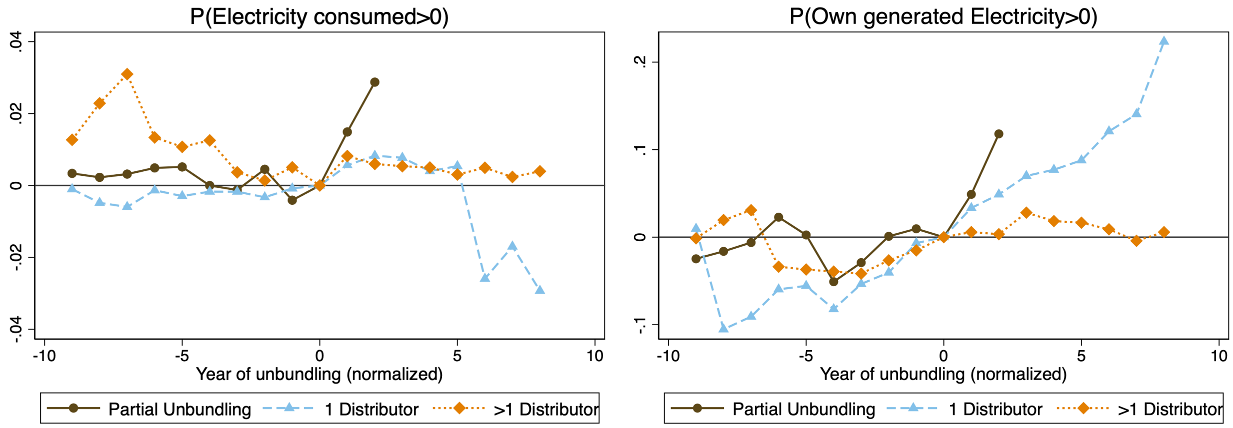
(c) Electricity purchase (intensive margin)

Note: Dataset restricted to privately-owned, electricity-intensive manufacturing firms. The graphs plot coefficients from regressions following Equation 1. Figure 1b shows results from a regression run on 507,942 observations, while Figure 1c shows the results from a regression with 507,224 firm level observations. For confidence intervals, please see Figures A1, A2 and A3 in the Appendix. Standard errors clustered at the state level. For other specification tests, see Appendix A.2 and A.3.

companies on electricity purchase appears to be near zero (coefficient of $Unbundled_3$: δ_{3y}). The line representing the simplest level of restructuring, i.e. only separating generation and distribution from transmission (δ_{1y}) stops two years after the treatment year. This is because only 4 states had this limited level of structuring, and the last year available in the data for these states is a maximum of two years after restructuring.

Figure 1a shows an increase in electricity reliability in states that completely unbundled all three sectors, and formed a single distribution company. Given the lack of pre-trends, we

Figure 2: The effect of restructuring on electricity purchase and own-generation



(a) Electricity purchase (extensive margin) **(b)** Probability of using a generator

Note: Dataset restricted to privately-owned, electricity-intensive manufacturing firms. The graphs plot coefficients from regressions following Equation 1. The left panel shows the results from a regression with 514,724 firm level observations, while the right-hand side panel shows results from a regression run on 520,184 observations. For confidence intervals, please see Figures A4 and A5 in the Appendix. Standard errors clustered at the state level. For other specification tests, see Appendix A.2 and A.3.

may be able to causally infer that blackouts differentially fell in these states.

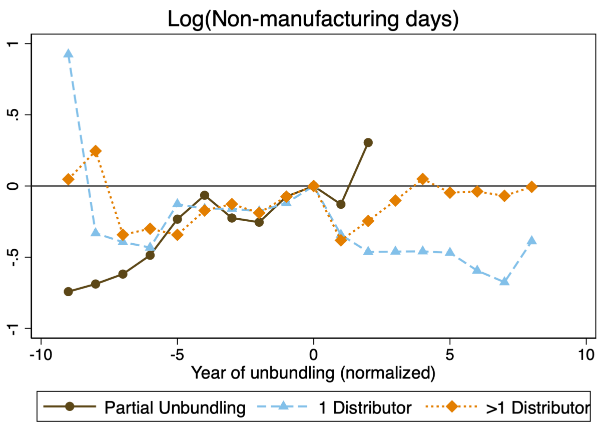
From Figure 1c, I observe a differentially and significantly higher level of electricity consumption in states with a single distributor, despite these same states also facing a differentially higher average electricity price (Figure 1b).[18] This may be consistent with prices increasing in these states if higher consumption levels pushed these firms to higher tiers of the price tariff, raising the average price per unit of electricity consumed given increasing block price tariffs. However, coupled with the decreased blackouts, the increase in consumption despite higher prices points to the fact that electricity unreliability was previously a binding constraint to production.

The figures show how the magnitudes evolve over time. Given the dynamic nature, and the fact that each state was treated at a different point of time, the ATTs differ by when each state was treated. Callaway and Sant'Anna (2020) describe the concept of a group-specific ATT, where each group is defined by the cohort when they received the reform. For instance, for our main reform of interest (unbundled with one distributor), and the large group (2005 cohort), the ATT(g) for Log(electricity price) as an outcome, ranges from 0.0611

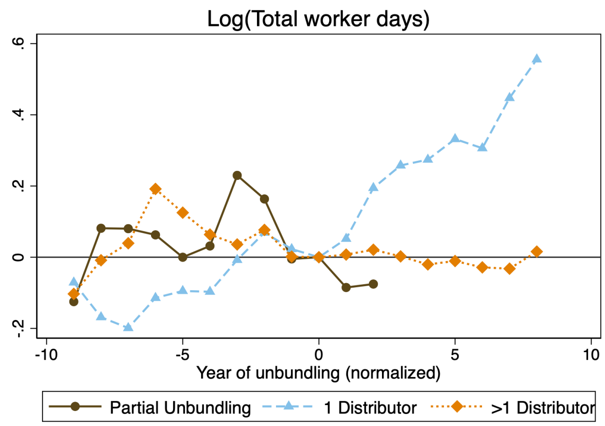
[18] Confidence intervals in Figures A1, A2 and A3.

and 0.0686.[19] That is, on average, states that unbundled, but created just one distributor saw an increase in electricity prices of between 6 and 7%. Similarly, the group-specific ATT(g) for Log(electricity consumed), ranges between 0.145 and 0.193.[20]

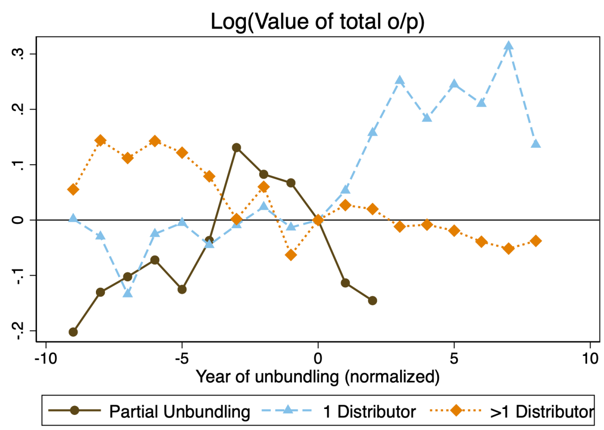
Figure 3: The effect of restructuring on firm outcomes



(a) Non-manufacturing days



(b) Total worker days



(c) Total output

Note: Dataset restricted to privately-owned, electricity-intensive manufacturing firms.The graphs plot coefficients from regressions following Equation 1. The left hand panel shows the results from a regression with 80,750 firm level observations, while the right hand side panel shows results from a regression run on 506,734 observations. For confidence intervals, please see Figures A6, A7, A8 in the Appendix. Standard errors clustered at the state level. For other specification tests, see Appendix A.2 and A.3.

While some firms may respond to reduced blackouts by increasing electricity purchases, even if average electricity price tariffs increase to some extent, there may be some firms who respond more to the increasing price of electricity, as opposed to the quality. For

[19]The range is because there are two different estimators described in Sant'Anna and Zhao (2020).
[20]These ATT(g)'s are statistically different from zero at the 95% using wild bootstrapped errors.

these firms, a reduction in electricity purchases, as well as an increase in own-generated electricity, is plausible. Figure 2a shows that on the extensive margin, a small fraction of firms may exit the market in states with a single distributor; although this result is statistically indistinguishable from zero (Figure A4). The firms that do remain increase their own-generated electricity to complement their increased purchase of grid-electricity (Figure 2b).[21] I find no meaningful effects for states with other configurations.

The results in Figures 3a and 3b further support the idea that for a meaningful fraction of firms, for whom the quality of electricity is a major factor, increase their electricity consumption (both purchased and own-generated), and re-optimize production.[22] These figures show that non-manufacturing days (when factories are forced to shut operations) slightly fall, while the total worker days rises in the presence of more reliable electricity. Once again, this occurs only in states with complete unbundling and one distribution company. This may be interpreted as evidence that these manufacturing firms also increase their labor input, which complements their increased electricity purchases. This is consistent with the observation that blackouts were a major constraint to their production, since they may have had to shut down production on days when there was no power.

This hypothesized re-optimization by firms is borne out in Figure 3c, where an increase in electricity purchases, and subsequent increase in worker days is accompanied by an increase in the total output.[23]

In Appendix A.2 and A.3 I discuss various specification tests suggested by Callaway and Sant'Anna (2020); Sant'Anna and Zhao (2020); Borusyak and Jaravel (2017) and Sun and Abraham (2020) to the event study designs.

6 Discussion

The patterns observed in this paper show that higher prices, greater consumption of electricity, and higher reliability, differentially occur in states with a single distribution company. A question that follows is why they did not occur, for instance, in states with the same separation of the three sectors, but with multiple distribution companies. According to analyses by Pargal and Banerjee (2014), multiple utilities in a single state may result in better outcomes if benchmark competition exists, but in practice, because they serve different areas and dif-

[21]Figures A4 and A5 include the confidence intervals for the coefficients of $Unbundled_2$.
[22]Figures A6 and A7 in the Appendix include the confidence intervals for the coefficients of $Unbundled_2$.
[23]Figure A8 in the Appendix include the confidence intervals for the coefficients of $Unbundled_2$.

ferent consumer bases, they do not compete. In fact, this may prevent them from effectively cross-subsidizing across consumer groups if, for instance, one firm serves only agricultural users and another only industrial users. Difficulties associated with cross-jurisdictional coordination and cooperation across multiple utility management groups within a state have also been observed in other countries (Foster and Rana (2020) in the context of Peru).

Among the states that ended up having a single distributor, there are a number of unique features that may explain their differentially better performance. They are able to effectively cross-subsidize across consumer groups, adopting progressive tariffs with the highest prices for industrial users.[24] All but one of the states with a single utility set tariff rates at or above cost-recovery levels by 2011, while considerably fewer states with multiple utilities achieve that. To provide a more specific example, in Maharashtra (where there is one unified distribution company, Bhiwandi Power sector), bill collection efficiency increased from 58% in 2006/07 to 99% in 2010-11. In fact, the majority of states that ended up with a single distributor were among the top performers in terms of utility cost recovery (Deloitte, 2013). While states like Maharashtra (single distributor) reduced losses from distribution, the worst performing states were Uttar Pradesh and Orissa (with multiple distributors), where losses from distribution worsened over time after the reforms.

One of the biggest reasons why states with single utilities may have performed better is that they were able to coordinate a successful cross-subsidization schedule as they cover all consumers. Further, Pargal and Banerjee (2014) note that regulators impose higher accountability standards for single distributors as the onus of state-wide supply is on a single entity. Finally, it may be possible that in states with only one distribution company, a hike in electricity tariffs, at least to cover all costs, is administratively easier to pass and justify. With multiple distribution companies, with varying levels of overhead and, therefore costs, it may be harder to explain price tariff increases in one distribution area versus another. State regulators are also responsible for ensuring parity in prices across states, and therefore the worst-managed utilities may serve as the binding constraint on price hikes.

[24] These figures are drawn from metadata in Pargal and Banerjee (2014), combined with statistics from the Annual Revenue Requirement (ARR) reports of Maharashtra, Orissa, Uttar Pradesh, Bihar, West Bengal, Karnataka, Assam, Himachal Pradesh, Punjab and Andhra Pradesh from 2003-2011. These reports are submitted by utilities to the independent regulatory council.

7 Conclusion

The Indian experience with restructuring appears to have mixed results. This paper shows that the 2003 electricity reforms resulted in different state configurations for electricity provision. Surprisingly, whether a state ends up with a single distributor or multiple results in highly divergent patterns. States with single distributors increase reliability, manufacturing firms pay more and consume more electricity, and increase output. I show supporting evidence that single distributors face fewer coordination hurdles and are able to cross-subsidize or restructure their electricity tariffs in a more revenue-increasing manner. They successfully increase bill payments, and consequently, electricity reliability, as they face greater accountability from regulators who have only one utility to manage.

States with multiple distributors, however, are amongst the worst performers across the country. The losses these utilities face create a vicious cycle of poor-performing utilities and unreliable supply. The central government in India has a fund called UDAY to systematically bail these utilities out, perpetuating this loss-making cycle and reducing accountability (Chatterjee, 2017). Apart from the opportunity cost of taxpayer funds being channeled towards utility bailouts, low revenues reduce investment in generation and transmission (Kumi, 2017, examines the case of Ghana). Other evidence from Colombia shows how government subsidies can perpetuate a cycle of low investment in electricity (McRae, 2015).

This paper is among the first to show evidence in the Indian case of how institutions and politics matter for effective electricity supply, and when they allow for better tariff-redesign, electricity reliability improves in a way that increases firms' willingness-to-pay for electricity. The latter is crucial if the country is to break out of the low-quality, low-revenues equilibrium it is in. Making the link between greater reliability and paying higher bills more salient is crucial in increasing consumer willingness-to-pay for electricity. Power debt in India amounts to almost 2.4% of national GDP (Engelmeier, 2015). Unbilled electricity and non-payment of bills that perpetuate this debt are problems with solutions. It is important to consider the institutional and political contexts of energy policy more broadly in order to fix them.

References

Muneeza Alam. Coping with Blackouts: Power Outages and Firm Choices. *Yale University (mimeo)*, 2015.

Philippe Alby, Jean-Jacques Dethier, and Stéphane Straub. Firms Operating under Electricity Constraints in Developing Countries. *The World Bank Economic Review*, 27(1): 109–132, 06 2012. ISSN 0258-6770.

Hunt Allcott, Allan Collard-Wexler, and Stephen D. O'Connell. How Do Electricity Shortages Affect Industry? Evidence from India. *American Economic Review*, 106(3):587–624, March 2016.

Jay Apt. Competition Has Not Lowered U.S. Industrial Electricity Prices. *The Electricity Journal*, 18(2):52–61, 2005.

Robert W. Bacon and John Besant-Jones. Global Electric Power Reform, Privatization and Liberalization of the Electric Power Industry in Developing Countries. *Energy and Mining Sector Board Discussion Paper Series*, (2), June 2002.

Thushyanthan Baskaran, Brian Min, and Yogesh Uppal. Election cycles and electricity provision: Evidence from a quasi-experiment with indian special elections. *Journal of Public Economics*, 126:64–73, 2015.

Subhes C. Bhattacharyya. Sustainability of Power Sector Reform in India: What Does Recent Experience Suggest? *Journal of Cleaner Production*, pages 235–246, 2007.

Seth Blumsack, Jay Apt, and Lester B. Lave. A Cautionary Tale: U.S. Electricity Reform. *Economic and Political Weekly*, 40(50):5279–5290, December 10-16 2005.

Severin Borenstein. The Trouble With Electricity Markets: Understanding California's Restructuring Disaster. *Journal of Economic Perspectives*, 16(1):191–211, 2002.

Severin Borenstein and James B. Bushnell. The U.S. Electricity Industry After 20 Years of Restructuring. *Annual Review of Economics*, 7(1):437–463, 2015.

Severin Borenstein, James B. Bushnell, and Frank A. Wolak. Measuring Market Inefficiencies in Californias Restructured Wholesale Electricity Market. *American Economic Review*, 92 (5):1376–1405, December 2002.

Kirill Borusyak and Xavier Jaravel. Revisiting Event Study Designs. *Available at SSRN: https://ssrn.com/abstract=2826228*, 2017.

Robin Burgess, Michael Greenstone, Nicholas Ryan, and Anant Sudarshan. The Consequences of Treating Electricity as a Right. *Journal of Economic Perspectives*, 34(1): 145–69, February 2020.

Fiona Burlig and Louis Preonas. Out of the Darkness and into the Light? Development Effects of Rural Electrification. *Working Paper*, 2017.

FIona Burlig, Akshaya Jha, and Louis Preonas. Out-of-Merit Costs and Blackouts: Evidence from Indias Electricity Market. 2020.

James B. Bushnell and Celeste Saravia. An Empirical Assessment of the Competitiveness of the New England Electricity Market. *Center for the Study of Energy Markets Working Paper 101*, 2002.

James B. Bushnell and Catherine Wolfram. Ownership Change, Incentives and Plant Efficiency: The Divestiture of U.S. Electric Generation Plants. *Center for the Study of Energy Markets, Working Paper 140*, 2005.

Brantly Callaway and Pedro H. C. Sant'Anna. Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics*, 2020.

Doruk Cengiz, Arindrajit Dube, Attila Lindner, and Ben Zipperer. The Effect of Minimum Wages on Low-Wage Jobs. *The Quarterly Journal of Economics*, 134(3):1405–1454, 05 2019. ISSN 0033-5533.

Elizabeth Chatterjee. Reinventing State Capitalism in India: A View from the Energy Sector. *Contemporary South Asia*, 25(1):85–100, 2017. doi: 10.1080/09584935.2017.1290583.

Elizabeth Chatterjee. The Politics of Electricity Reform: Evidence from West Bengal, India. *World Development*, 104:128–139, 2018.

Elizabeth Chatterjee. The Asian Anthropocene: Electricity and Fossil Developmentalism. *The Journal of Asian Studies*, 79(1):324, 2020. doi: 10.1017/S0021911819000573.

Pradeep Chhibber, Sandeep Shastri, and Richard Sisson. Federal Arrangements and the Provision of Public Goods in India. *Asian Survey*, 44(3):339–352, 2004.

Steve Cicala. When Does Regulation Distort Costs? Lessons from Fuel Procurement in US Electricity Generation. *American Economic Review*, 105(1):411–444, 2015.

Steve Cicala. Imperfect Markets versus Imperfect Regulation in U.S. Electricity Generation. *American Economic Review*, Forthcoming.

Lucas W. Davis and Catherine Wolfram. Deregulation, Consolidation, and Efficiency: Evidence from US Nuclear Power. *American Economic Journal: Applied Economics*, 4(4): 194–225, 2012.

Clément de Chaisemartin and Xavier D'Haultfuille. Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–96, September 2020. doi: 10.1257/aer.20181169.

Deloitte. Review of Reforms Implementation in the Indian Power Sector. *Report prepared for the World Bank, Washington, DC*, 2013.

Taryn Dinkelman. The Effects of Rural Electrification on Employment: New Evidence from South Africa. *American Economic Review*, 101:3078–3108, 2011.

Avinash Dixit. Power of Incentives in Private versus Public Organizations. *The American Economic Review*, 87(2):378–382, May 1997. ISSN 00028282.

Tobias Engelmeier. Will Indias Power Sector Run Out of Debt? *https://www.linkedin.com/pulse/indias-power-sector-run-out-debt-tobias-engelmeier/*, 2015.

Kira R. Fabrizio, Nancy L. Rose, and Catherine D. Wolfram. Do Markets Reduce Costs? Assessing the Impact of Regulatory Restructuring on US Electric Generation Efficiency. *American Economic Review*, 97(4):1250–1277, 2007.

Karen Fisher-Vanden, Erin T. Mansur, and Qiong (Juliana) Wang. Electricity shortages and firm productivity: Evidence from China's industrial firms. *Journal of Development Economics*, 114:172–188, 2015. ISSN 0304-3878.

Forum of Regulators. Introducing Competition in Retail Electricity Supply in India. *Government of India*, 2013.

Vivien Foster and Anshul Rana. Rethinking Power Sector Reform in the Developing World. *Sustainable Infrastructure Series, Washington DC: World Bank*, 2020.

Stephie Fried and David Lagakos. Electricity and Firm Productivity: A General-Equilibrium Approach. May 2020.

Andrew Goodman-Bacon. Difference-in-Differences with Variation in Treatment Timing. NBER Working Papers 25018, National Bureau of Economic Research, Inc, September 2018.

Michael Greenstone and B. Kelsey Jack. Envirodevonomics: A Research Agenda for an Emerging Field. *Journal of Economic Literature*, 53(1):5–42, March 2015.

J. Vernon Henderson, Adam Storeygard, and David N. Weil. Measuring Economic Growth from Outer Space. *American Economic Review*, 102(2):994–1028, 2012.

International Energy Agency. *India Energy Outlook*. International Energy Agency, 2015.

B. Kelsey Jack and Grant Smith. Pay As You Go: Prepaid Metering and Electricity Expenditures in South Africa. *American Economic Review*, 105(5):237–241, 2015.

P. L. Joskow and Edward Kahn. A Quantitative Analysis of Pricing Behavior in California's Wholesale Electricity Market During Summer 2000. *The Energy Journal*, 23(4):1–36, 2002.

Paul L. Joskow. Restructuring, Competition and Regulatory Reform in the U.S. Electricity Sector. *The Journal of Economic Perspectives*, 11(3):119–138, 1997.

Paul L. Joskow and Richard Schmalensee. *Markets for Power: An Analysis of Electrical Utility Deregulation*. The MIT Press, 1983.

Matthew E. Kahn and Erin T. Mansur. How Do Energy Prices, and Labor and Environmental Regulations Affect Local Manufacturing Employment Dynamics? A Regression Discontinuity Approach. *NBER Working Paper No. 16538*, 2010.

Dana Kassem. Does electrification cause industrial development? grid expansion and firm turnover in indonesia. 2021.

Madhu Khanna and David Zilberman. Barriers to Energy-Efficiency in Electricity Generation in India. *Energy Journal*, 20(1):25–41, 1999.

Christopher Knittel. Alternative Regulatory Methods and Firm Efficiency: Stochastic Frontier Evidence the US Electricity Industry. *The Review of Economics and Statistics*, 84(3): 530–540, 2002.

Ebenezer Nyarko Kumi. The Electricity Situation in Ghana: Challenges and Opportunities. *Washington, DC: Center for Global Development*, 2017.

Kenneth Lee, Edward Miguel, and Catherine Wolfram. Does Household Electrification Supercharge Economic Development? *Journal of Economic Perspectives*, 34(1):122–44, February 2020.

Molly Lipscomb, A. Mushfiq Mobarak, and Tania Barham. Development Effects of Electrification: Evidence from the Topographic Placement of Hydropower Plants in Brazil. *American Economic Journal: Applied Economics*, 5(2):200–231, 2013.

Meera Mahadevan. The Price of Power: Costs of Political Corruption in Indian Electricity. 2021.

Kabir Malik, Maureen Cropper, Alex Limonov, and Anoop Singh. Estimating the Impact of Restructuring on Electricity Generation Efficiency: The Case of the Indian Thermal Power Sector. *NBER Working Papers 17383*, 2011.

Michael L. Mann, Eli K. Melass, and Malik Arun. Using VIIRS Day/night Band to Measure Electricity Supply Reliability: Preliminary Results from Maharashtra, India. *Remote Sensing*, 8(9):711, 2016.

Erin T. Mansur. Upstream Competition and Vertical Integration in Electricity Markets. *Journal of Law and Economics*, 50:125–156, 2007.

Erin T. Mansur. Measuring Welfare in Restructured Electricity Markets. *The Review of Economics and Statistics*, 90(2):369–386, 2008.

Kira Markiewicz, Nancy Rose, and Catherine Wolfram. Has restructuring improved operating efficiency at us electricity generating plants. *Center for the Study of Energy Markets, Working Paper 135*, 2004.

Shaun McRae. Infrastructure Quality and the Subsidy Trap. *American Economic Review*, 105(1):35–66, 2015.

Robyn Meeks, Hope F. Thompson, and Zhenxuan Wang. Electrification to Grow Manufacturing? Evidence from Nepal. *Duke Global Working Paper Series No. 36*, 2021.

Virgiliu Midrigan and Daniel Yi Xu. Finance and misallocation: Evidence from plant-level data. *American Economic Review*, 104(2):422–58, February 2014.

Brian Min and Miriam Golden. Electoral Cycles in Electricity Losses in India. *Energy Policy*, 65:619–625, 2014. ISSN 0301-4215. doi: 10.1016/j.enpol.2013.09.060.

Ministry of Law and Justice. The electricity act. *Government of India*, 2003.

Sheoli Pargal and Sudeshna G. Banerjee. More Power to India The Challenge of Electricity Distribution. *World Bank Report*, 2014.

Nicholas Ryan. The Competitive Effects of Transmission Infrastructure in the Indian Electricity Market. *American Economic Journal: Microeconomics*, 2020. Forthcoming.

Pedro HC Sant'Anna and Jun Zhao. Doubly Robust Difference-in-Differences Estimation. *Journal of Econometrics*, 219(1):101–122, 2020.

Anupama Sen and Tooraj Jamasb. Diversity in Unity: An Empirical Analysis of Electricity Deregulation in Indian States. *The Energy Journal*, 33(1):83–130, 2012.

Anoop Singh. Power Sector Reform in India: Current Issues and Prospects. *Energy Policy*, 34(16):2480–90, 2006.

Liyang Sun and Sarah Abraham. Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects. Working Paper, 2020.

John Taber, Duane Chapman, and Tim Mount. Examining the Effects of Deregulation on Retail Electricity Prices. *Working Paper, Department of Applied Economics and Management, Cornell University*, 2006.

Rahul Tongia. The Political Economy of Indian Power Sector Reforms. *Program on Energy and Sustainable Development Working Paper 4*, 2003.

Frank A. Wolak. Diagnosing the California Electricity Crisis. *The Electricity Journal*, 16 (7):11–37, August-September 2003.

Catherine Wolfram. Measuring Duopoly Power in the British Electricity Spot Market. *American Economic Review*, 89(4):805–826, 1999.

Catherine Wolfram. The Efficiency of Electricity Generation in the U.S. After Restructuring. *Center for the Study of Energy Markets Working Paper 111R*, 2003.

World Bank. World Bank Enterprise Survey. Technical report, World Bank Group, 2014.

A Appendix

Table A1: Different styles of restructuring in various states

States			
Type 1 **Treatment 1**	**Type 2** **Treatment 2**	**Type 3** **Treatment 3**	**No Unbundling** **Control Group**
Tamil Nadu	Maharashtra	Andhra Pradesh	Goa
Punjab	Meghalaya	Delhi	Jammu and Kashmir
Himachal Pradesh	Uttaranchal	Gujarat	Jharkhand
	Assam	Haryana	Kerala
	West Bengal	Karnataka	Manipur
	Chattisgarh	Madhya Pradesh	Nagaland
		Orissa	Sikkim
		Rajasthan	Tripura
		Uttar Pradesh	
		Bihar	

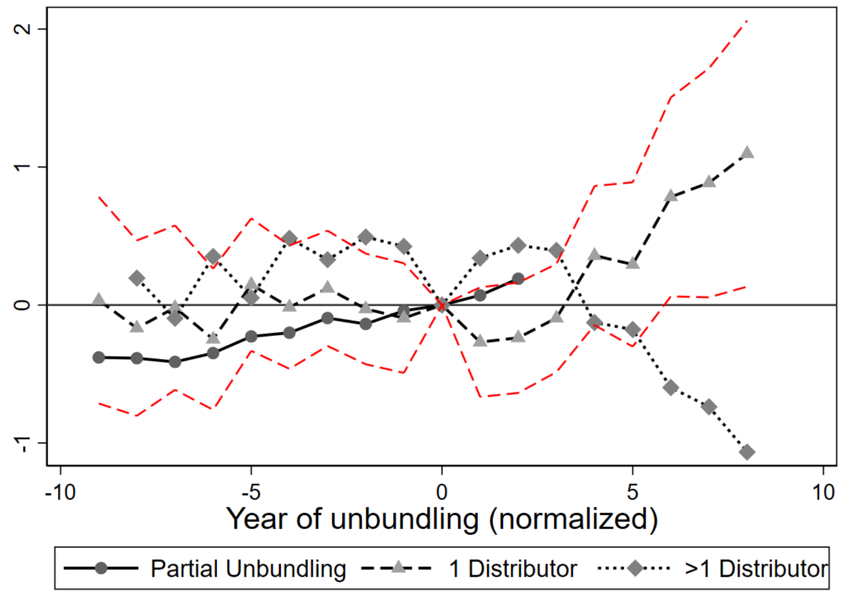
Type 1 Generation and distribution separated from transmission
Type 2 Generation, transmission and distribution separated, single distribution company
Type 3 Generation, transmission and distribution separated, multiple distribution companies
No Unbundling States that did not restructure during the study period

Table A2: Electricity-intensive manufacturing industries included in the ASI data

List of included manufacturing industries
Manufacture Of Food Products And Beverages
Manufacture Of Textiles
Manufacture Of Wood And Of Products Of Wood And Cork,Except Furniture
Manufacture Of Articles Of Straw And Plating Materials
Manufacture Of Paper And Paper Products
Manufacture Of Coke, Refined Petroleum Products And Nuclear Fuel
Manufacture Of Chemicals And Chemical Products
Manufacture Of Other Non-Metallic Mineral Products
Manufacture Of Basic Metals
Manufacture Of Fabricated Metal Products, Except Machinery And Equipments
Manufacture Of Machinery And Equipment
Manufacture Of Electrical Machinery And Apparatus
Manufacture Of Other Transport Equipment
Manufacture Of Furniture
Manufacture Of Office, Accounting And Computing Machinery
Manufacture Of Radio, Television And Communication Equipment And Apparatus
Manufacture Of Medical, Precision And Optical Instruments, Watches And Clocks
Manufacture Of Motor Vehicles, Trailers And Semi-Trailers

A.1 The Effect of Restructuring: Inference

Figure A1: The effect of restructuring on luminosity (satellite data)



Note: Standard errors clustered at the state level. This graph uses satellite nighttime light density data as a proxy for electricity consumption. I use 228 observations to run a regression following Equation 1, and the coefficients are plotted in this graph.

Figure A2: The effect of restructuring on electricity price

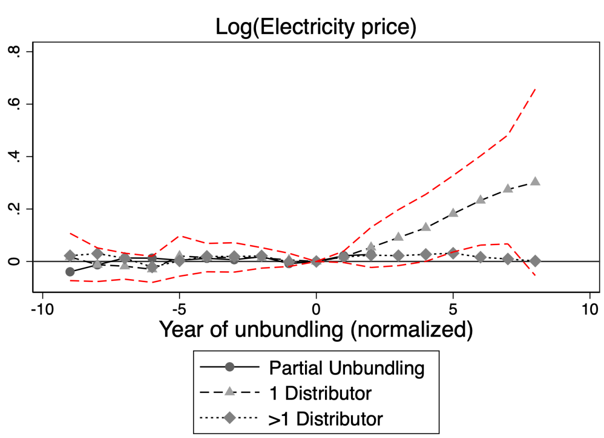
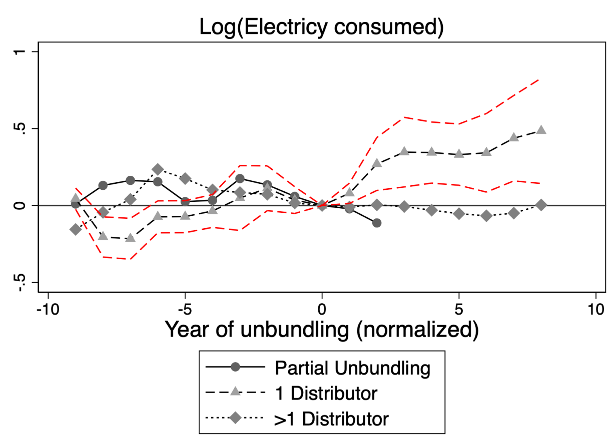


Figure A3: The effect of restructuring on electricity purchase (intensive margin)



Note: Standard errors clustered at the state level. Dataset restricted to privately-owned, electricity-intensive manufacturing firms.The coefficients are reported from Equation 1.

Figure A4: The effect of restructuring on electricity purchase (extensive margin)

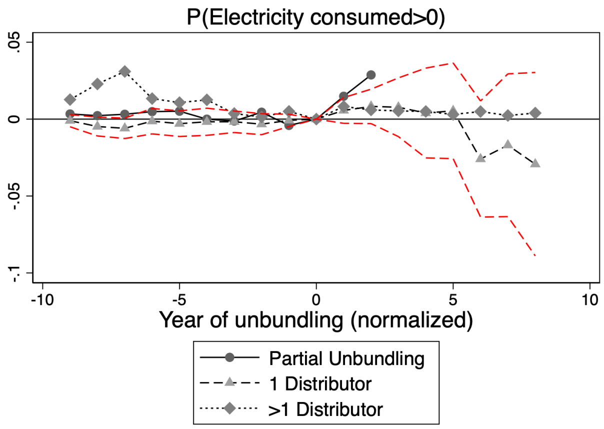
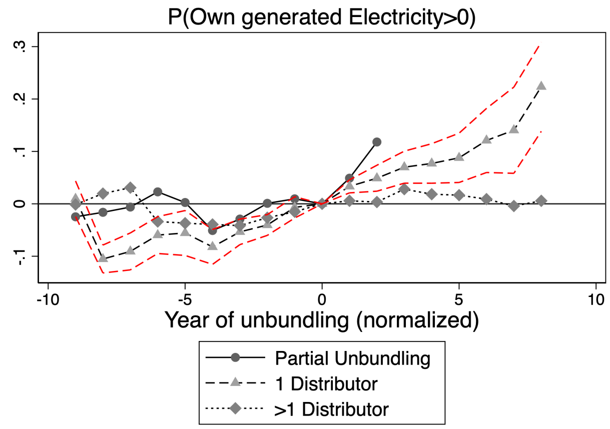


Figure A5: The effect of restructuring on the probability of using a generator



Note: Standard errors clustered at the state level. Dataset restricted to privately-owned, electricity-intensive manufacturing firms.The coefficients are reported from Equation 1.

Figure A6: The effect of restructuring on non-manufacturing days

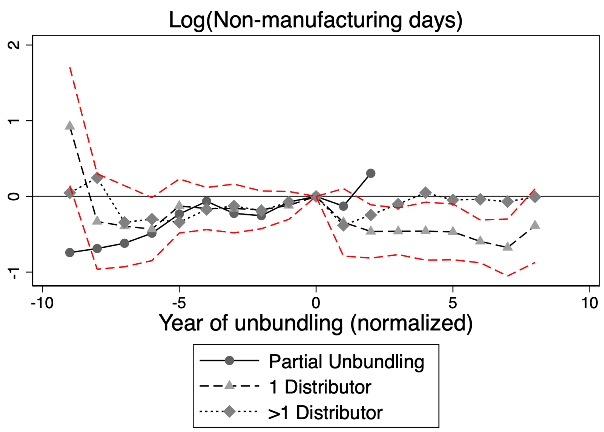
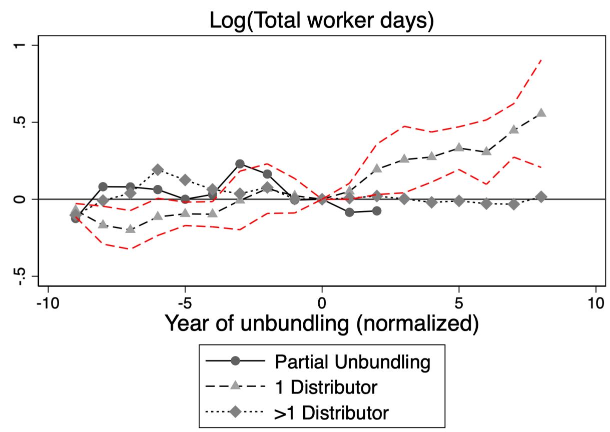
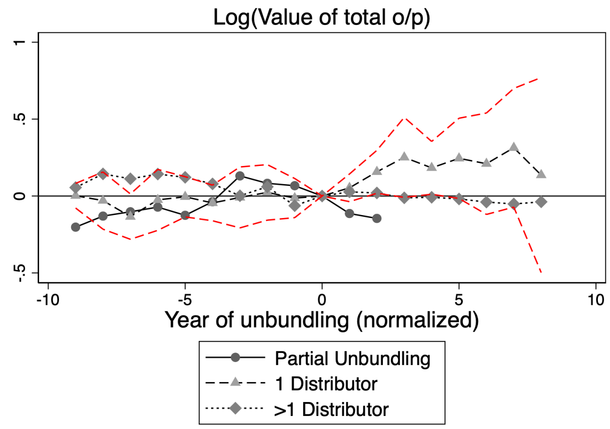


Figure A7: The effect of restructuring on total worker days



Note: Standard errors clustered at the state level. Dataset restricted to privately-owned, electricity-intensive manufacturing firms.The coefficients are reported from Equation 1.

Figure A8: The effect of restructuring on total output



Note: Standard errors clustered at the state level. Dataset restricted to privately-owned, electricity-intensive manufacturing firms.The coefficients are reported from Equation 1.

A.2 Robustness to Event-Study Designs

Recent developments in the study of staggered-entry difference-in-differences designs suggest numerous specification tests (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; de Chaisemartin and D'Haultfuille, 2020). While my main estimates try to adhere to the recommendations, in this appendix I follow various other specification tests suggested by the authors. First, I test the various estimators proposed by Callaway and Sant'Anna (2020), and described in detail by Sant'Anna and Zhao (2020). In the following figures, the left-hand side columns show use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on inverse probability tilting and weighted least squares. The right-hand side columns use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and OLS. These estimators produce very similar results.

Second, I test robustness to the control groups. The unique setting in my case suggests a few alternative tests. In the traditional Difference-in-Differences framework, there is one type of treatment, whereas here we have three different types of unbundling. This raises the question of what is the control group. While this is not a question of bias, it does change the interpretation of the estimated ATT and what 'treatment' necessarily implies in this case.

In the following figures, I explore three different definitions of 'treatment.' In the top rows, I use any other state (whether they received no treatment, or received a different type of unbundling) as the control group. In the middle rows, I use any other reform (type of unbundling) as a control group, and exclude the non-reform states altogether. This may compare somewhat more similar states, but changes the interpretation of the ATT to be relative to other types of reform. Yet, the results are similar. Finally, in the bottom row, I use only the non-reform states as the control group. This again produces similar results.

In implementing the specific Callaway and Sant'Anna (2020) specifications, I follow their recommendations and wild-bootstrap the errors, and unlike my main specifications, the omitted period is the first period of the data (instead of $t = -1$).

For tractability sake, I show results for the group that drive my primary set of results: those states that unbundled with a single distributor (note that the other treatments had almost null effects on net). Figures A13 to A14 show the electricity consumed as the outcome. Figures A19 to A20 shows electricity price, and Figures A25 to A26 show manufacturing output. These results closely mirror the results in the main paper.

Log(Elec. Consumed) for Unbundled with 1 Distributor – Callaway and Sant'Anna (2020)

Figure A9: IMP Spec; Control Group: Any other state

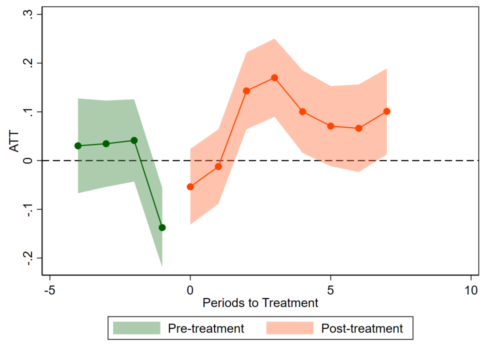


Figure A10: IPW Spec; Control Group: Any other state

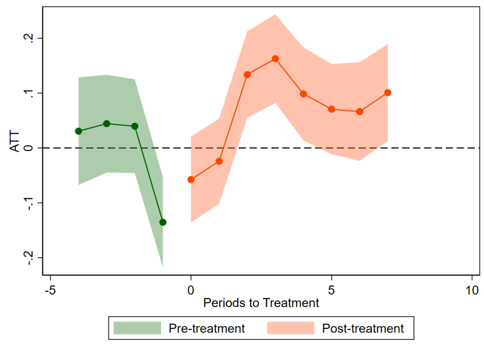


Figure A11: IMP Spec; Control Group: Any other reformed state

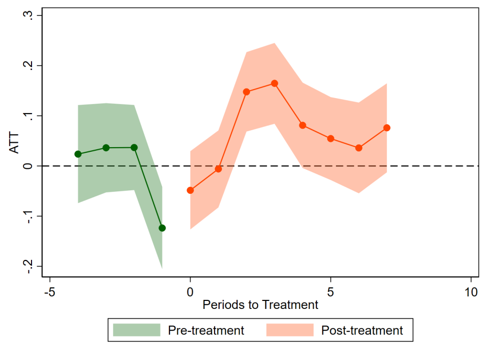


Figure A12: IPW Spec; Control Group: Any other reformed state

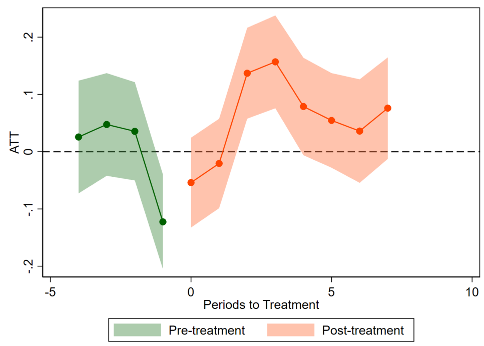


Figure A13: IMP Spec; Control Group: Only non-reformed states

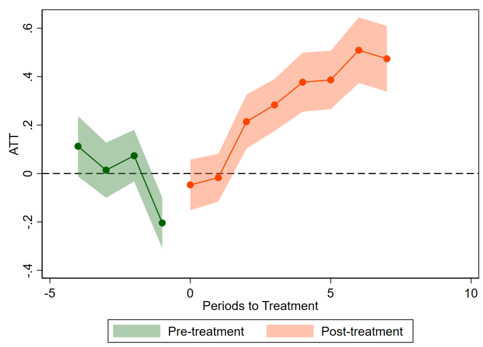
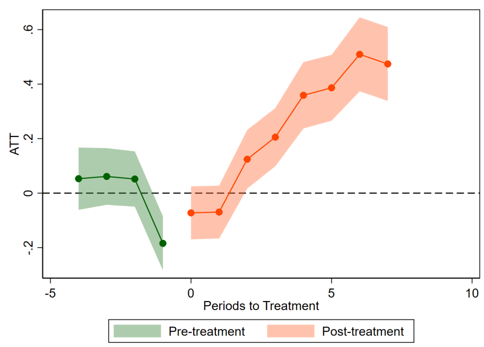


Figure A14: IPW Spec; Control Group: Only non-reformed states



Note: Privately-owned, electricity-intensive manufacturing firms. Wild bootstrapped standard errors. Omitted periods are before $t = -5$. Left columns show the "IMP Spec", which use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on inverse probability tilting and weighted least squares. The right columns use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and OLS. In the top row, the control group is any other state (i.e., those that received no reform, or received a different reform.) In the second row, the control group is any other state that received a reform. In the last row, the control group is only states that received no reform at all.

Log(Electricity Price) for Unbundled with 1 Distributor – Callaway and Sant'Anna (2020)

Figure A15: IMP Spec; Control Group: Any other state

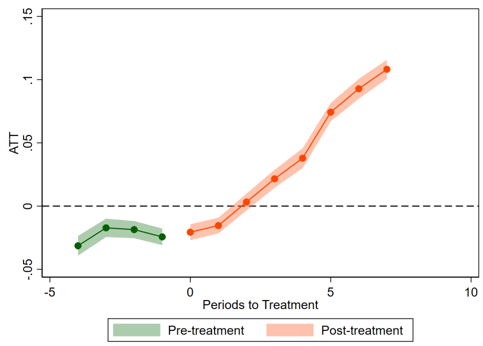


Figure A16: IPW Spec; Control Group: Any other state

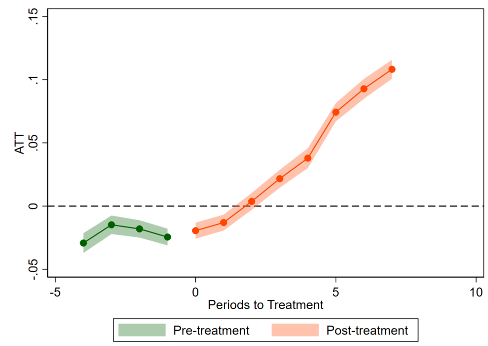


Figure A17: IMP Spec; Control Group: Any other reformed state

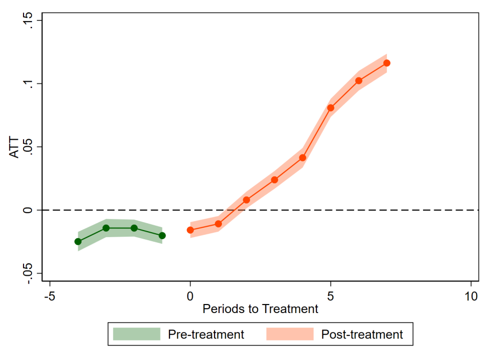


Figure A18: IPW Spec; Control Group: Any other reformed state

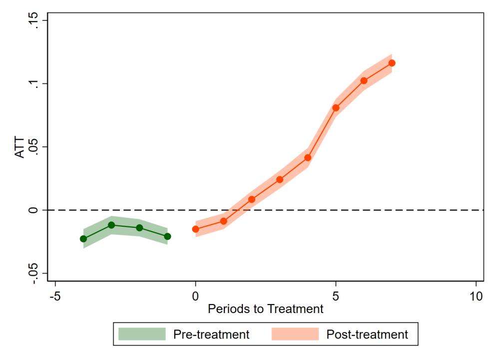


Figure A19: IMP Spec; Control Group: Only non-reformed states

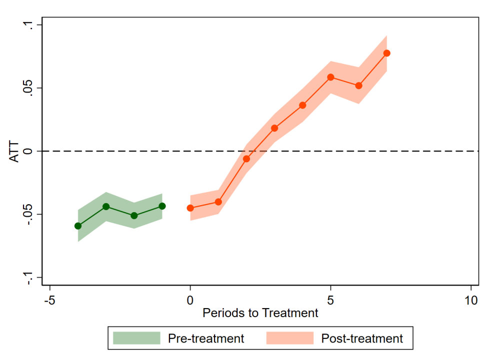
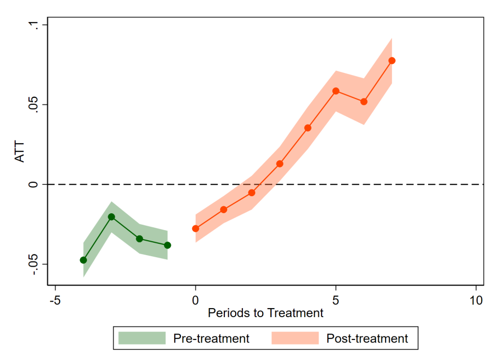


Figure A20: IPW Spec; Control Group: Only non-reformed states



Note: Privately-owned, electricity-intensive manufacturing firms. Wild bootstrapped standard errors. Omitted periods are before $t = -5$. Left columns show the "IMP Spec", which use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on inverse probability tilting and weighted least squares. The right columns use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and OLS. In the top row, the control group is any other state (i.e., those that received no reform, or received a different reform.) In the second row, the control group is any other state that received a reform. In the last row, the control group is only states that received no reform at all.

Log(Mfg. Output) for Unbundled with 1 Distributor – Callaway and Sant'Anna (2020)

Figure A21: IMP Spec; Control Group: Any other state

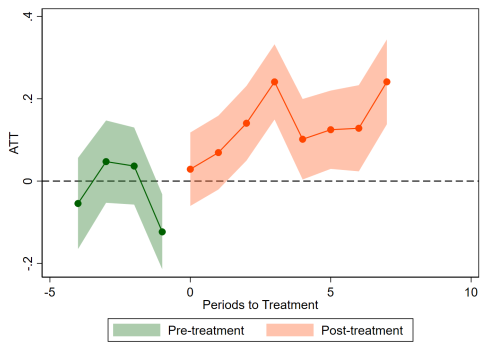


Figure A22: IPW Spec; Control Group: Any other state

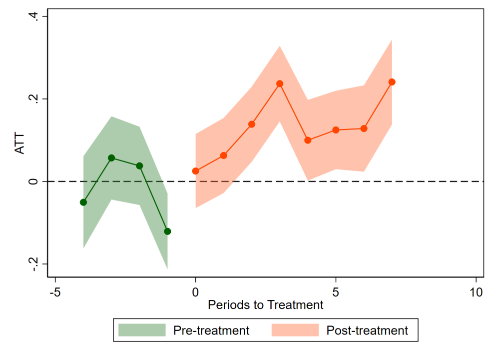


Figure A23: IMP Spec; Control Group: Any other reformed state

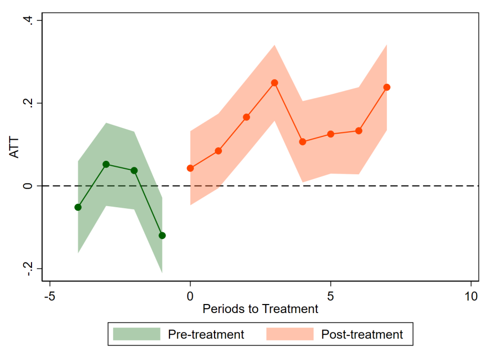


Figure A24: IPW Spec; Control Group: Any other reformed state

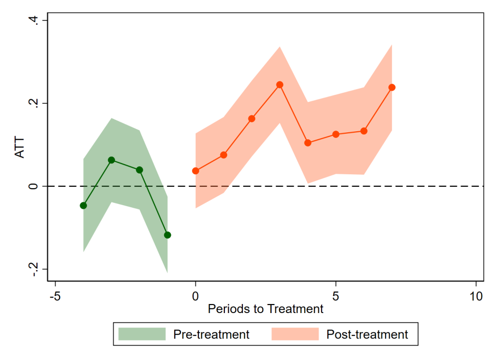


Figure A25: IMP Spec; Control Group: Only non-reformed states

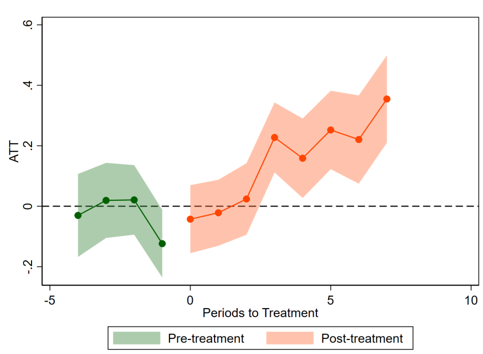
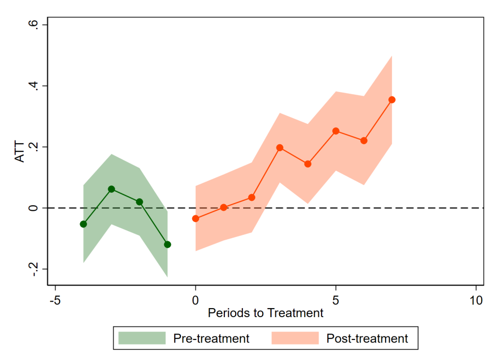


Figure A26: IPW Spec; Control Group: Only non-reformed states



Note: Privately-owned, electricity-intensive manufacturing firms. Wild bootstrapped standard errors. Omitted periods are before $t = -5$. Left columns show the "IMP Spec", which use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on inverse probability tilting and weighted least squares. The right columns use the Sant'Anna and Zhao (2020) doubly robust DiD estimator based on stabilized inverse probability weighting and OLS. In the top row, the control group is any other state (i.e., those that received no reform, or received a different reform.) In the second row, the control group is any other state that received a reform. In the last row, the control group is only states that received no reform at all.

A.3 Under-Identification in Event Studies

I follow Borusyak and Jaravel (2017)'s suggestion to include robustness checks that avoid under-identification issues; and so in the following exercise, I restrict the pre-period to be zero for all periods. Imposing the condition that there are no pre-reform trends is a very strong restriction, given that I do not see clear evidence in the data that there are no pre-trends, particularly for the outcome variables other than electricity price. However, I use the results from this specification only to test whether the effect of a particular type of restructuring is jointly different from 0 for all the post-treatment years. I use the following specification for this model:

$$Y_{fist} = \gamma + \sum_{z=1}^{3}\left[\sum_{y=1}^{7}\delta_{zy}Unbundled_{zs}\mathbb{1}(t - T_s = y)\right] + SERC_{st} + \theta_s + \theta_t + \theta_i + \nu_{fist} \quad (2)$$

I show results using the above specification in the Appendix. I use these results only to check whether there is a significant aggregate effect of restructuring on the outcomes of interest. I find the trends confirm my main results.

Figure A27: The effect of restructuring on electricity purchase (intensive margin) - Model with 0 pre-trends

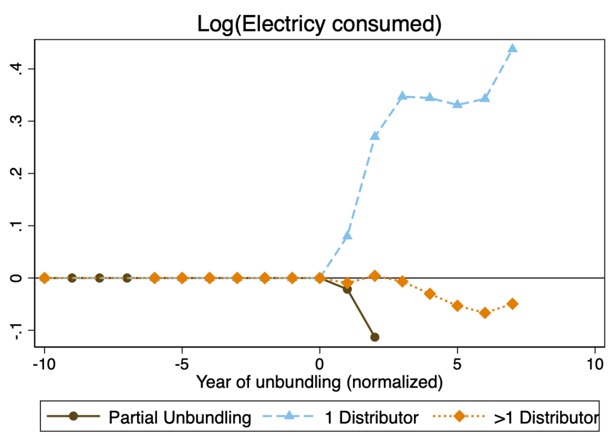
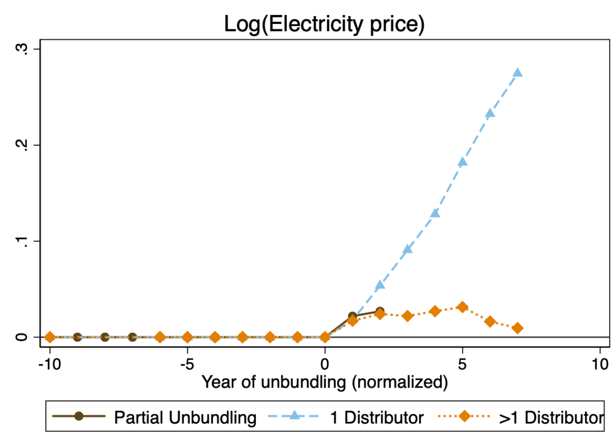


Figure A28: The effect of restructuring on electricity price - Model with 0 pre-trends



Note: Dataset restricted to privately-owned, electricity-intensive manufacturing firms.The coefficients are reported from Equation 2