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

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Abstract

The Indian Electricity Sector is composed primarily of state-owned electricity utilities that make massive losses, often costing the central government billions of dollars a year to bailout. 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 resulted in 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 worker productivity. These results demonstrate that some institutional changes could potentially propel the sector out of the low-price/low-electricity equilibrium in India.

Keywords: Electricity, India, Manufacturing
JEL Classification Codes: H41, Q41, O13, O38

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1 Introduction

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 leverage 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 some others ended up with multiple distributors, with unintended consequences on both prices and reliability. This variation provides a natural experiment where different states ended 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 derive variation in changing institutional structures across states from these reforms, allowing me to estimate impacts on price, reliability and overall consumption in 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 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

[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.

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 such 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 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 state-wide 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. Under a single statewide distributor, one possible outcome is that revising prices upwards is easier to negotiate with the regulator who does 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 competition among utilities on revenue collection and electricity supply can incentivize reliable quality. As such, it is theoretically ambiguous which configuration may perform better.

[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).

[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).

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.

I do so 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.[6] 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. I study the effect of these reforms on manufacturing firms in India, using data from the Annual Survey of Industries 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), total unit of electricity purchased, total output, worker productivity and own-generated electricity.[7] I further obtain satellite data on night-time lights as a proxy for electricity outages (Alam, 2015).[8]

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 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 and productivity. 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.

[6]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). A different assignment rule may produce different estimates. 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. To mitigate concerns of violating this assumption, I test for differential pre-trends. 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]Utility reported prices are not available for a majority of states studied.

[8]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).

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 enables 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 utilites 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 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 from the distribution and transmission companies and therefore had greater autonomy in their decisions.

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 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.[9]

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,

[9]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 report consuming no electricity.

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, full-states with more than 1000 observations. Exchange rate approximately Rs. 66/$.

and inputs to the firms such as amount of electricity consumed. I define two measures of firm productivity. I refer to one of them as wage productivity, which is simply the total wages divided by the total worker days. Similarly, I define output productivity as the total value of output (sum of finished and semi-finished output value) divided by the total workers days. 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 there 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). And finally, I only include states for which the data has more that 1000 observations (firm-year level).[10]

I obtain information on the unbundling status of various states from their respective

[10]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.

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 of 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 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 quality of electricity and blackouts, this data serves as a good measure of electricity supply and reliability (Burlig and Preonas, 2017; Alam, 2015).

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, total output of the firm, and measures of productivity. I use the following specification for my event study.

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

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 in addition to the separation referenced by $z = 1$, also separate generation from distribution, 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 additional to the steps taken into account by $z = 1$ and $z = 2$ also set up multiple distribution companies. 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 ($Unbundled_1 = 1$ and $Unbundled_2 = 1$). Here, when I have a firm-level observation in year 2000 ($t = 2000$) the expression within 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 periods $\delta_{1,-5}$ and $\delta_{2,-5}$. 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.[11]

[11] Given the multiple treatments, and the possibility of simultaneous confidence intervals Callaway and

Table 2: Explanation of indicator variables used in event study

Features	**Variables** $Unbundled_1$	$Unbundled_2$	$Unbundled_3$
Generation and distribution separated from transmission	1	0	0
Generation, transmission and distribution separated, single distribution company	1	1	0
Generation, transmission and distribution separated, multiple distribution companies	1	1	1

I can reasonably argue for conditional exogeneity considering that in Figure 1b, the pre-trends of average electricity price are similar across all states, after controlling for the above fixed effects. The event study graphs allows 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.[12] Since I have multiple treatments,

Sant'Anna (2020), inference is not straightforward. In Appendix A.2 I wild bootstrap my errors following Sant'Anna and Zhao (2020).

[12]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.

I assume that the 'never treated with configuration of Type 1' control group includes any state that did not ever implement the Type 1 reforms (only transmission separated).[13]

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.[14] 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.[15]

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.

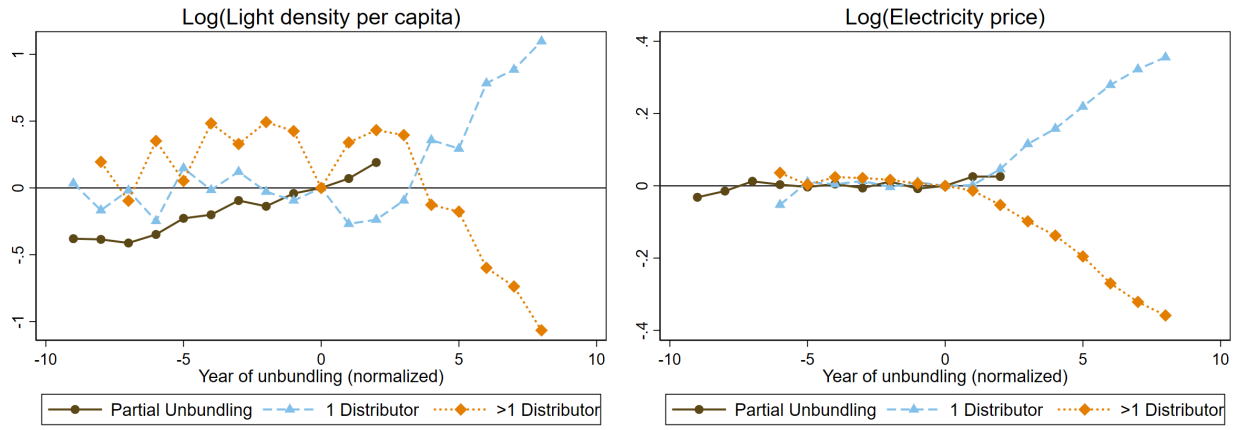
The line representing complete separation, with one distribution company (coefficients of $Unbundled_2$: δ_{2y}) shows the additional effect of completely separating the three sectors over merely separating generation and distribution from transmission. Therefore, the total effect of complete separation can be calculated by adding the coefficients of $Unbundled_1$ and $Unbundled_2$ ($\delta_{1y} + \delta_{2y}$). The total effect in the case of this kind of complete separation with

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

[14] 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.

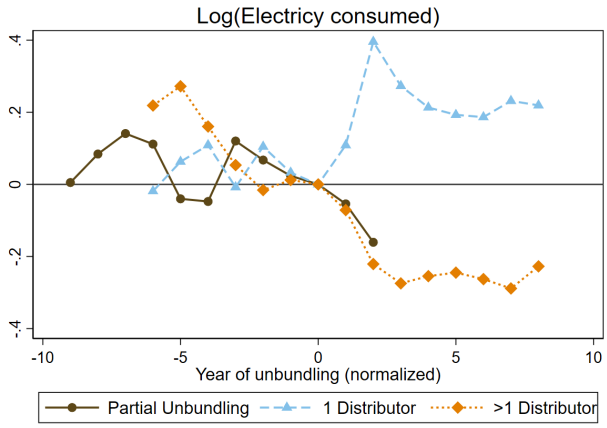
[15] My setting has a few attractive features that minimize complications raised in these papers. First, the treatment does not switch on and off. Second, the treatment type does not change. And third, we do not necessarily need to condition on covariates (Callaway and Sant'Anna, 2020).

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



(a) Electricity Reliability

(b) Electricity price

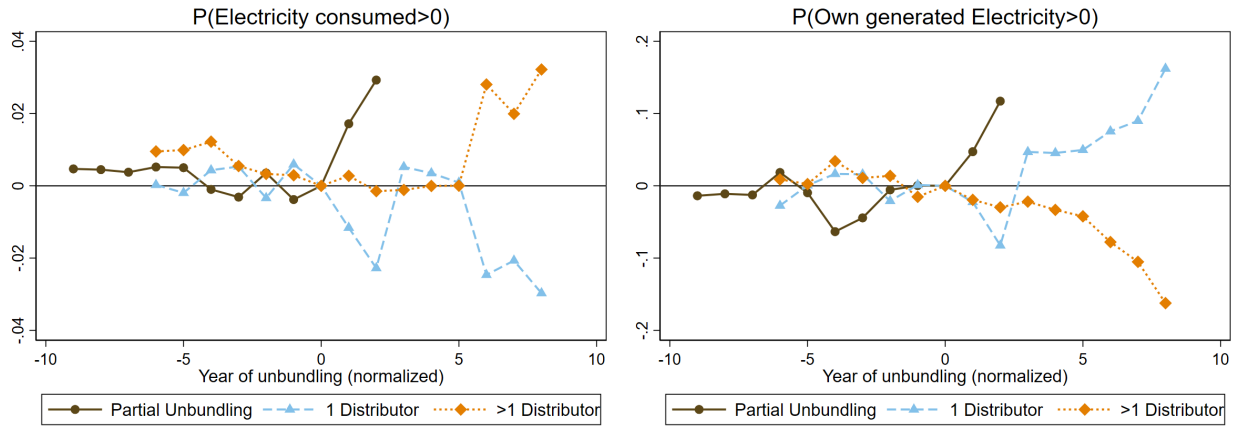


(c) Electricity purchase (intensive margin)

Note: Dataset restricted to privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations. 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.

one distribution appears to be positive. The net effect of complete separation with multiple distribution companies on electricity purchase appears to be zero (adding the coefficients of $Unbundled_1$, $Unbundled_2$ and $Unbundled_3$: $\delta_{1y} + \delta_{2y} + \delta_{3y}$). The line representing the baseline 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 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, full-states with more than 1000 observations.The graphs plot coefficients from regressions following Equation 1. The left hand 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.

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 can causally infer that blackouts differentially fall in these states.

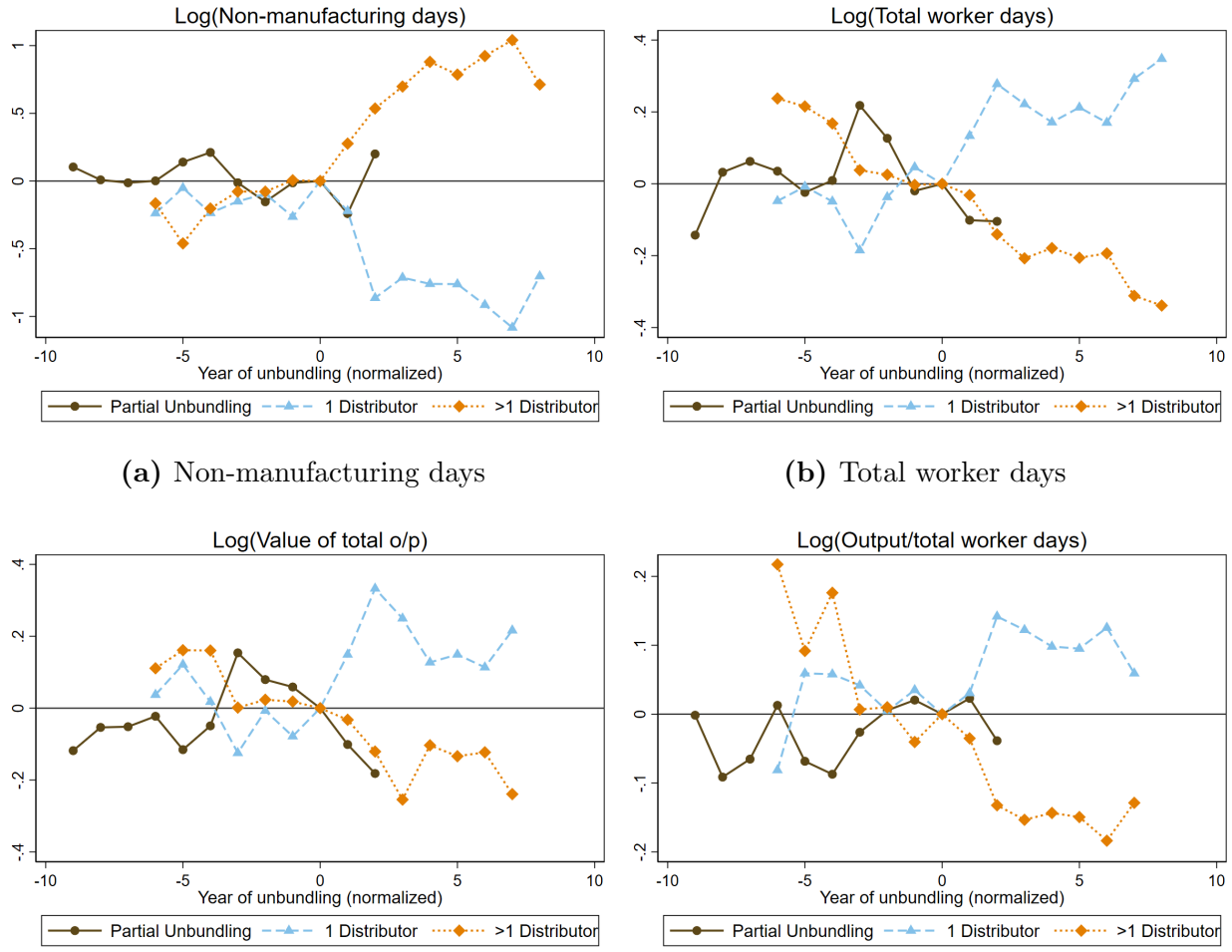
From Figure 1c, I observe a differentially and significantly higher level of electricity consumption in states with a single distributor, despite these same states also facing a differentially higher average electricity price (Figure 1b).[16] This is consistent with prices increasing in these states if higher consumption levels pushed these firms to higher tiers of the price tariff, raising the average price per unit of electricity consumed due to 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

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

group (2005 cohort), the ATT(g) for Log(electricity price) as an outcome, ranges from 0.0611 and 0.0686.[17] 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.[18]

Figure 3: The effect of restructuring on firm outcomes



(a) Non-manufacturing days

(b) Total worker days

(c) Total output

(d) Total o/p productivity

Note: Dataset restricted to privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations.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 and A9 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 purchase,

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

even if average electricity price tariffs increase to some extent, there may be smaller firms who respond more to the increasing price of electricity, as opposed to the quality. For 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, smaller firms may exit the market in states with a single distributor. However, the firms that do remain increase their own-generated electricity to complement their increased purchase of grid-electricity 2b.[19] I find no significant effects for states with other configurations.

The results in Figures 3a and 3b further support the idea that larger firms, for whom the quality of electricity is a major factor and who remain in the market, increase their electricity consumption (both purchased and own-generated), and re-optimize production.[20] These figures show that non-manufacturing days (when factories are forced to shut operations) falls, 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 complement 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 larger firms is borne out in Figures 3c and 3d, where an increase in electricity purchases, and subsequent increase in worker days is accompanied by an increase in the total output as well as output productivity (units of output per worker day).[21]

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 analysis by

[19] Figures A4 and A5 include the confidence intervals for the coefficients of $Unbundled_2$.
[20] Figures A6 and A7 in the Appendix include the confidence intervals for the coefficients of $Unbundled_2$.
[21] Figures A8 and A9 in the Appendix include the confidence intervals for the coefficients of $Unbundled_2$.

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 different 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.[22] 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 (multiple distributors), where losses from distribution worsened over time after the reforms.

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 productivity. 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.

[22]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.

States with multiple distributors, however, are amongst the worst performers across the country. The losses these utilities face creates 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 bill collection and 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 that have solutions, and it is important to consider the institutional and political contexts of energy policy more broadly in order to fix them.

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A Appendix

Table A1: Different styles of restructuring in various states

States		
Type 1	**Type 2**	**Type 3**
Tamil Nadu	Maharashtra	Andhra Pradesh
Punjab	Meghalaya	Delhi
Himachal Pradesh	Uttaranchal	Gujarat
	Assam	Haryana
	West Bengal	Karnataka
	Chattisgarh	Madhya Pradesh
		Orissa
		Rajasthan
		Uttar Pradesh
		Bihar

Type 1 Generation and distribution separated from transmission

Type 2 Generation, transmission and distribution separated, single distribution company

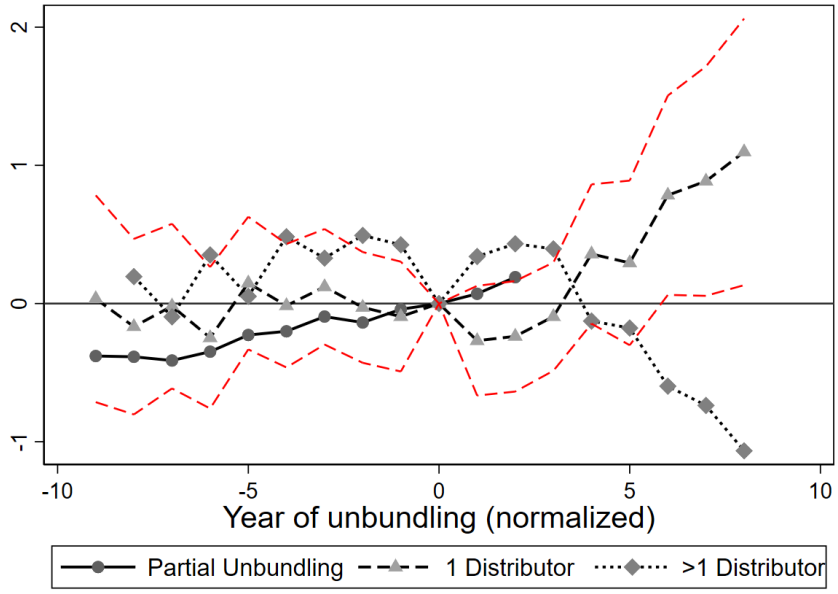
Type 3 Generation, transmission and distribution separated, multiple distribution companies

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

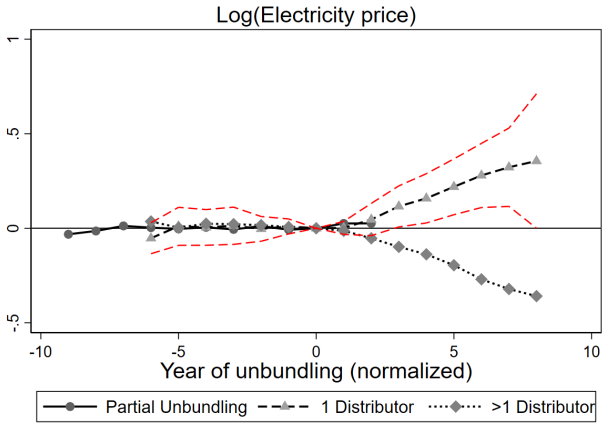
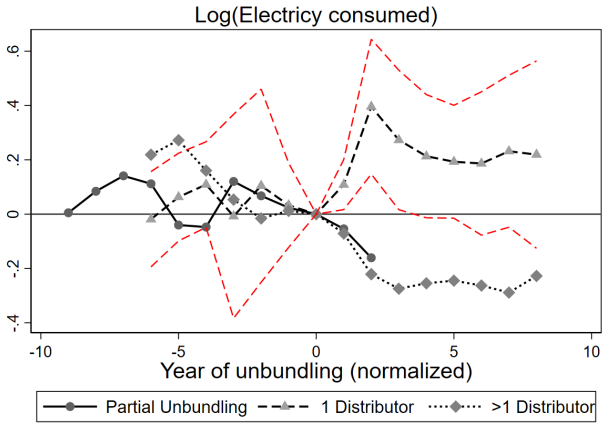


Figure A3: The effect of restructuring on electricity purchase (intensive margin)



Note: Standard errors clustered at the state level. Dataset restricted to privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations.The coefficients are reported from Equation 1.

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

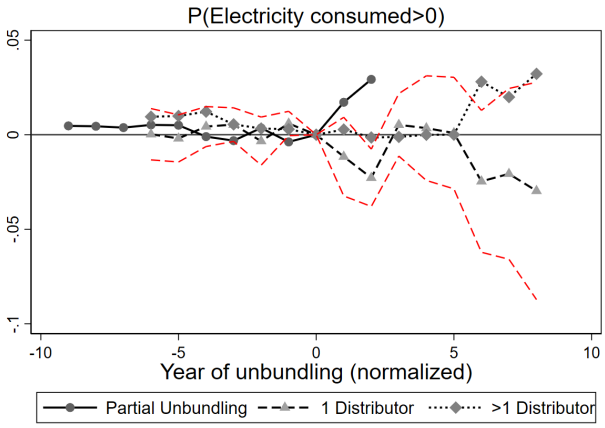
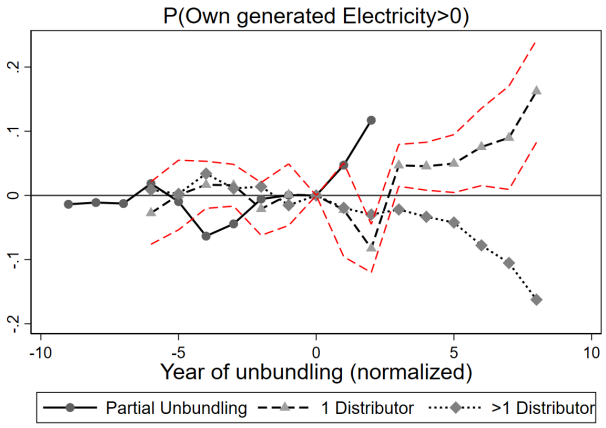


Figure A5: The effect of restructuring on the probability of using a generator



Note: Standard errors clustered at the state level. Dataset restricted to privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations.The coefficients are reported from Equation 1.

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

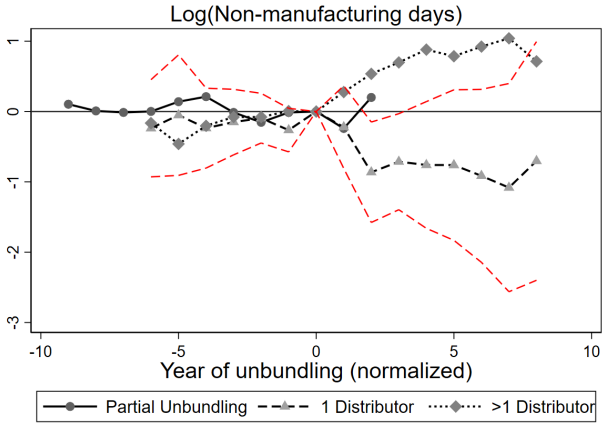
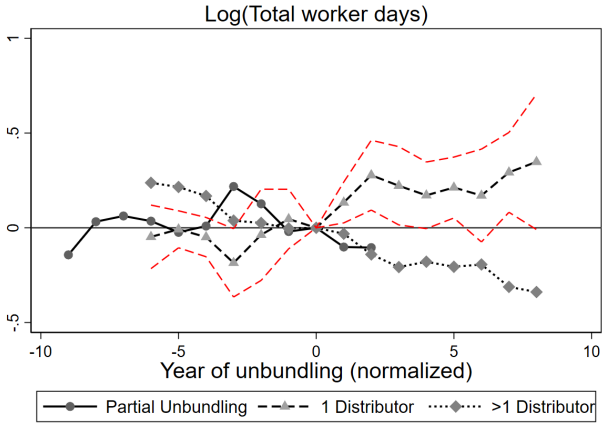


Figure A7: The effect of restructuring on total worker days



Note: Standard errors clustered at the state level. Dataset restricted to privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations.The coefficients are reported from Equation 1.

Figure A8: The effect of restructuring on total output

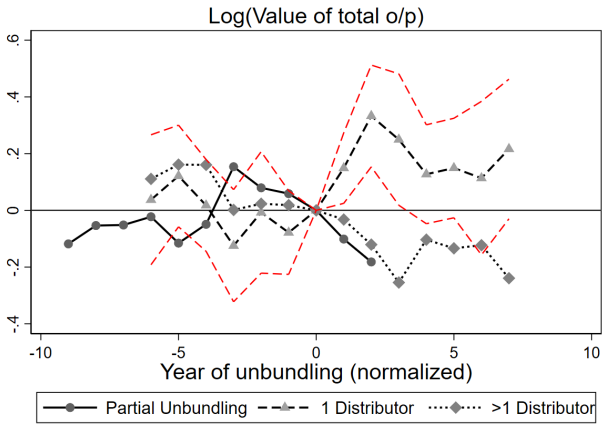
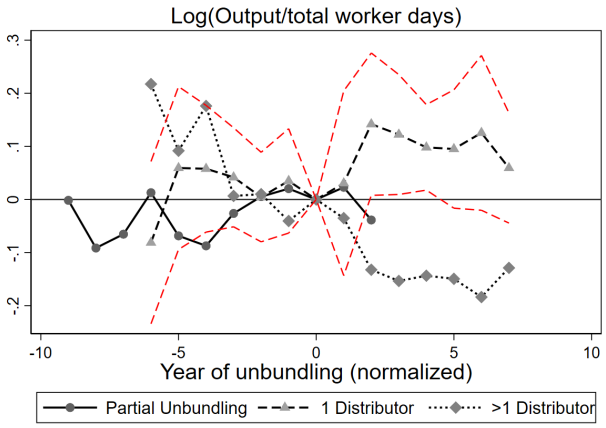


Figure A9: The effect of restructuring on total output productivity



Note: Standard errors clustered at the state level. Dataset restricted to privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations.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 A14 to A15 show the electricity consumed as the outcome. Figures A20 to A21 shows electricity price, and Figures A26 to A27 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 A10: IMP Spec; Control Group: Any other state

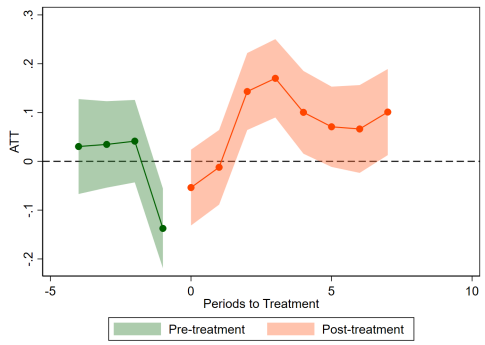


Figure A11: IPW Spec; Control Group: Any other state

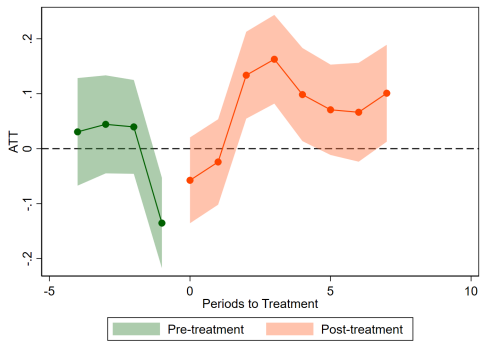


Figure A12: IMP Spec; Control Group: Any other reformed state

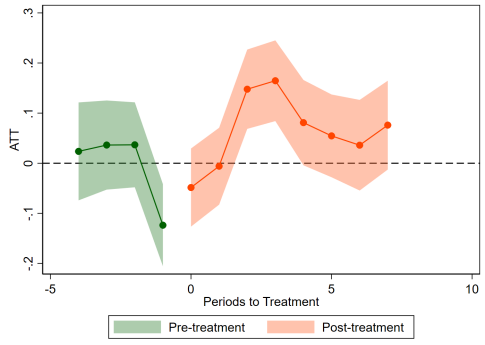


Figure A13: IPW Spec; Control Group: Any other reformed state

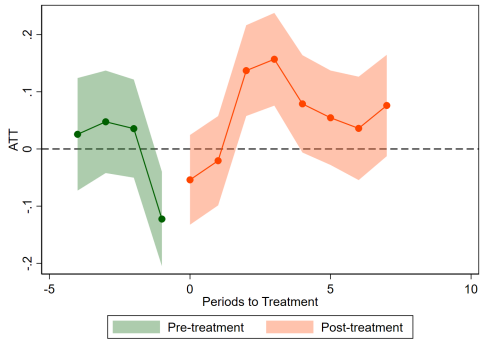


Figure A14: IMP Spec; Control Group: Only non-reformed states

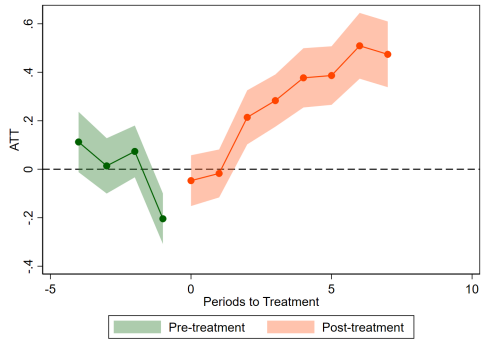
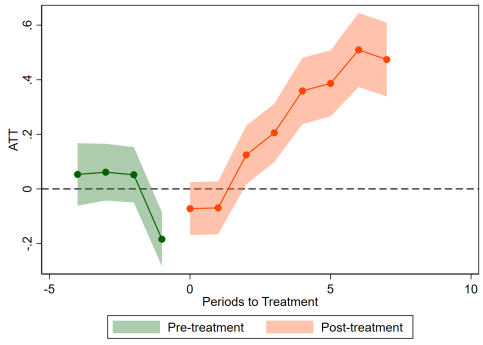


Figure A15: IPW Spec; Control Group: Only non-reformed states



Note: Privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations. 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 A16: IMP Spec; Control Group: Any other state

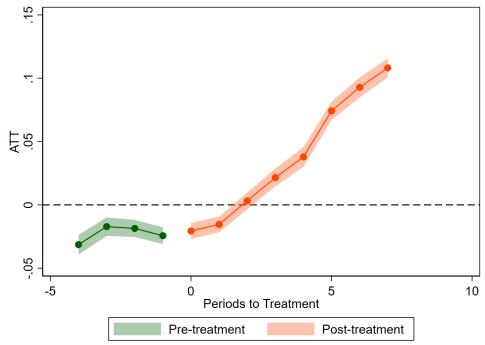


Figure A17: IPW Spec; Control Group: Any other state

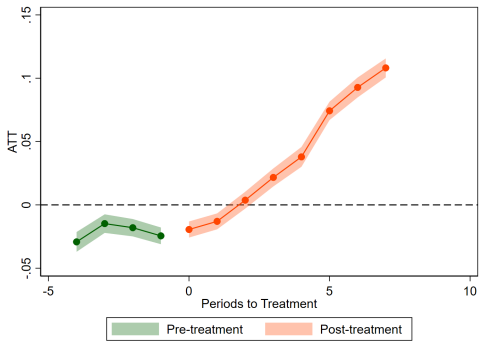


Figure A18: IMP Spec; Control Group: Any other reformed state

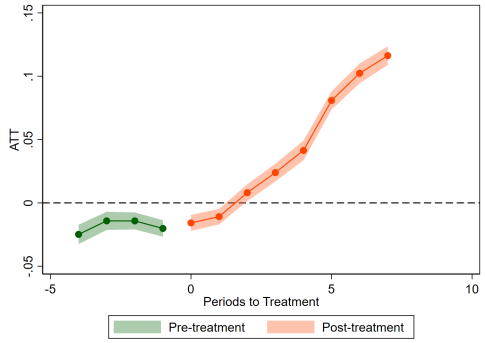


Figure A19: IPW Spec; Control Group: Any other reformed state

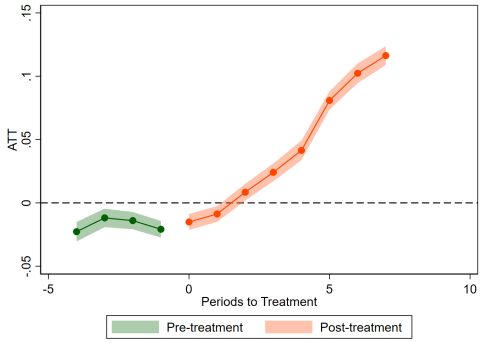


Figure A20: IMP Spec; Control Group: Only non-reformed states

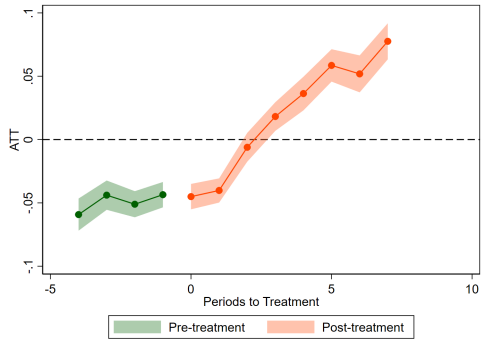
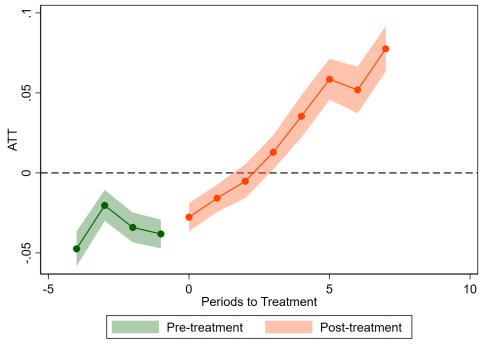


Figure A21: IPW Spec; Control Group: Only non-reformed states



Note: Privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations. 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 A22: IMP Spec; Control Group: Any other state

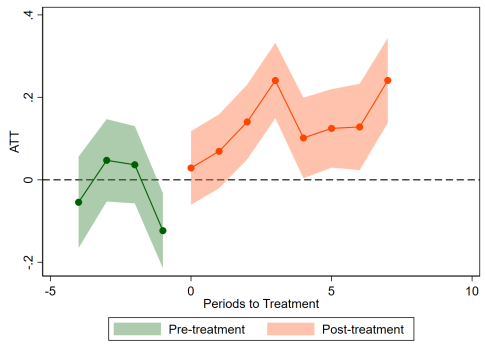


Figure A23: IPW Spec; Control Group: Any other state

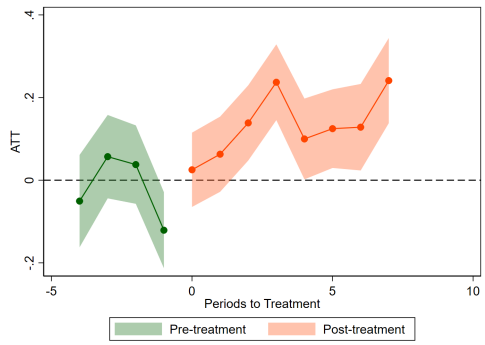


Figure A24: IMP Spec; Control Group: Any other reformed state

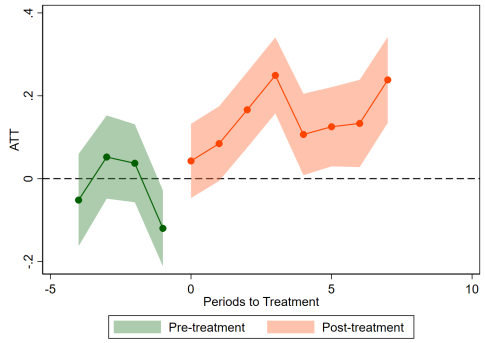


Figure A25: IPW Spec; Control Group: Any other reformed state

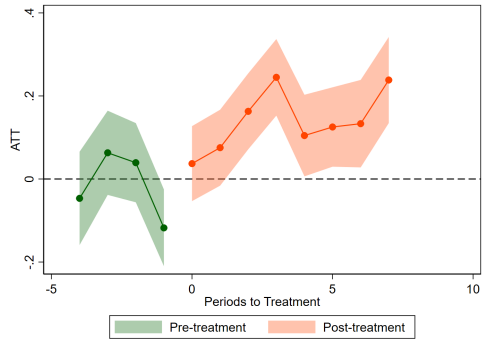


Figure A26: IMP Spec; Control Group: Only non-reformed states

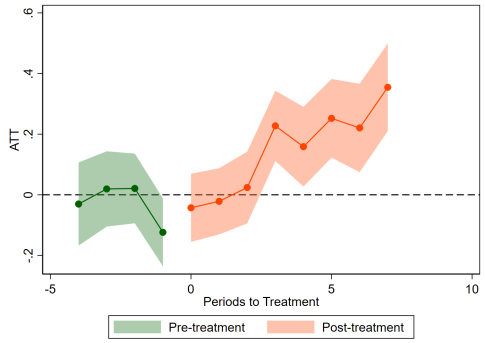
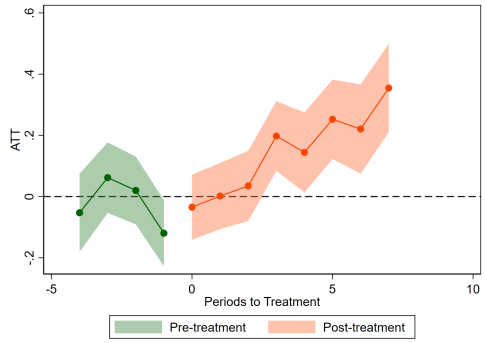


Figure A27: IPW Spec; Control Group: Only non-reformed states



Note: Privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations. 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 A28: The effect of restructuring on electricity purchase (intensive margin) - Model with 0 pre-trends

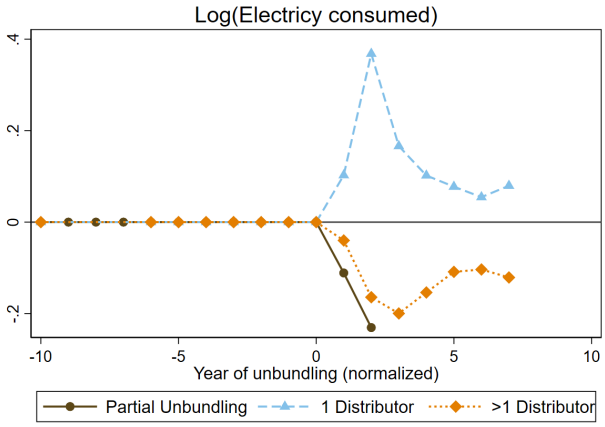
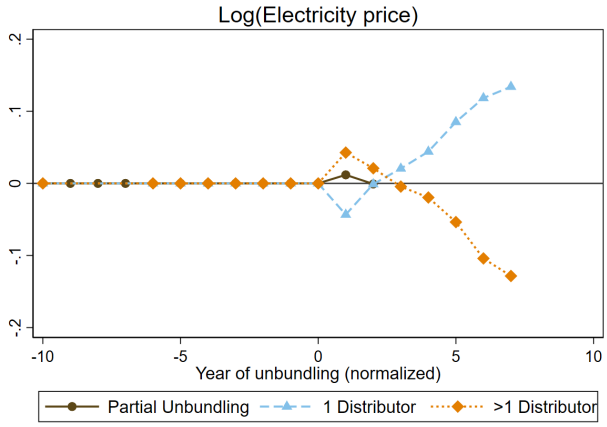


Figure A29: The effect of restructuring on electricity price - Model with 0 pre-trends



Note: Dataset restricted to privately-owned, electricity-intensive manufacturing firms, full-states with more than 1000 observations.The coefficients are reported from Equation 2