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# From Power Shortages to Reliable Electricity: Grid Upgrading and Firm Performance in Nepal

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# Abstract

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### Keywords

electricity reliability, grid infrastructure, firm performance, labor productivity, electricity intensity, difference-in-differences-in-differences, Nepal

### JEL Classification

D24, L94, O12, O18, Q43



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# From Power Shortages to Reliable Electricity: Grid Upgrading and Firm Performance in Nepal

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**Abstract**

Reliable electricity is critical for firms in developing economies, yet improvements in electricity infrastructure may not translate immediately into better firm performance. This study examines whether grid upgrades were associated with changes in electricity reliability and firm performance in Nepal. We combine three waves of the World Bank Enterprise Survey (2009, 2013, and 2023) with city-level information on major grid upgrades and predetermined industry electricity intensity from the 2006/07 Census of Manufacturing Establishments. Using a difference-in-differences-in-differences (DDD) framework, we exploit variation across time, upgraded and comparison cities, and firms' predetermined industry electricity intensity. The baseline estimates suggest that a one-standard-deviation increase in electricity intensity is associated with a 32.3-percentage-point larger decline in outage incidence and approximately 14.1 fewer hours of monthly outage exposure in upgraded cities relative to comparison cities. However, statistical significance is sensitive to model specification, and the outage-exposure estimate depends substantially on the composition of the comparison group. We find no statistically significant effects on real sales, employment, or labor productivity, while the positive estimates for total factor productivity provide only suggestive evidence of improvement. Pre-treatment placebo tests reveal no statistically significant differential changes, although the limited pre-treatment observations constrain their interpretation. Overall, the findings suggest that grid upgrading may improve electricity reliability, but the evidence is sensitive to identification and inference choices, and its implications for broader firm performance remain uncertain.

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# 1 Introduction

Reliable electricity is important for firm production and growth, yet power outages remain a major constraint in many developing economies. Electricity interruptions can stop production, leave labor and capital idle, and force firms to rely on costly alternatives or reorganize production around periods when electricity is available. A growing literature shows that unreliable electricity can reduce firm output and revenues and affect firms' production decisions and productivity (Fisher-Vanden et al., 2015; Allcott et al., 2016; Cole et al., 2018; Abeberese et al., 2021). These studies establish the economic costs of unreliable electricity, but they leave an important policy question: when governments invest in the electricity grid, do these investments actually improve the reliability experienced by firms, and do those improvements translate into better firm performance?

This question becomes increasingly important as electricity systems in developing countries mature. Much of the infrastructure literature has focused on expanding access to electricity. For example, Kassem (2024) shows that grid expansion in Indonesia increased the number of manufacturing firms, employment, and output, with firm entry accounting for much of the increase in output. Once firms are already connected to the grid, however, the relevant infrastructure constraint may shift from access to the quality and reliability of electricity supply. Improving an existing grid may substantially reduce electricity disruptions without producing the same economic responses as extending electricity to previously unconnected locations. Evidence on this intensive margin remains more limited.

Nepal provides a useful setting for studying this question. The country experienced severe electricity shortages and widespread load shedding for much of the period from 2007 to 2017. Previous research documents substantial economic costs from these shortages and a high value placed on reliable electricity service (Timilsina and Steinbuks, 2021; Alberini et al., 2022). Following the electricity crisis, Nepal also experienced a substantial expansion and strengthening of its transmission and substation network. Nepal Electricity Authority (NEA) records show that high-voltage substation capacity increased from 2,132 MVA in FY 2014/15 to about 7,149 MVA in FY 2021/22, while transmission lines of 66 kV and above increased from 2,624 to 5,329 circuit-km over the same period (NEA, 2022). By FY 2022/23, total high-voltage substation capacity had reached 8,867 MVA (NEA, 2023). These investments included new substations, transformer-capacity additions, and reinforcement and upgrading of existing substations. This expansion provides an opportunity to examine whether improvements in grid infrastructure translated into better electricity reliability for firms that were already connected to the system.

A central challenge in estimating these effects is that grid investments are not randomly located. Cities

receiving major upgrades may differ from other cities in electricity demand, economic activity, infrastructure needs, or other characteristics that also affect firm outcomes. We address this challenge by combining geographic exposure to grid upgrades with predetermined differences in firms' technological dependence on electricity. The underlying idea is straightforward: if grid upgrading improves electricity reliability, firms whose production technologies depend more heavily on electricity should experience larger improvements.

We combine three waves of the Nepal World Bank Enterprise Survey (WBES), covering 2009, 2013, and 2023, with city-level information on major grid upgrades and industry-level electricity intensity from Nepal's 2006/07 Census of Manufacturing Establishments (CME). Electricity intensity is measured before the baseline period and therefore does not reflect firms' responses to subsequent improvements in electricity supply. Our main analysis uses a difference-in-differences-in-differences (DDD) framework that compares changes between 2013 and 2023 across upgraded and comparison cities as predetermined industry electricity intensity increases. Firm fixed effects absorb persistent differences across firms, while the DDD specification allows changes over time to differ by upgrade status and predetermined electricity intensity.

The results show that grid upgrading is associated with substantial improvements in electricity reliability. A one-standard-deviation increase in predetermined industry electricity intensity is associated with an additional 14.1-hour reduction in monthly outage exposure in upgraded cities relative to comparison cities between 2013 and 2023. We also find an additional 32.3 percentage-point decline in the probability of reporting an outage. These results are statistically significant using Webb wild-cluster bootstrap inference, which we use because the main estimation sample contains only 11 city clusters. The strongest result is for monthly outage exposure, which combines the frequency and duration of electricity interruptions and therefore captures the overall monthly burden of outages.

The effects on downstream firm outcomes are more limited. We find no statistically significant effects on real sales, employment, or labor productivity. The estimates for total factor productivity (TFP), however, are positive across two alternative productivity measures. The DDD coefficient is 0.131 for Cobb–Douglas TFP and 0.109 for Wooldridge-based TFP, with Webb bootstrap (p)-values of 0.084 and 0.082, respectively. We therefore interpret these estimates as suggestive evidence of productivity improvement rather than strong evidence of a productivity effect. Pre-treatment placebo estimates using the 2009–2013 period show no statistically significant differential changes before the qualifying grid upgrades. The magnitude of the main outage-exposure estimate is also generally stable across alternative treatment definitions and sample restrictions.

The difference between the strong reliability results and the weaker firm-performance results is consistent

with an important feature of the existing literature. Firms can respond to unreliable electricity by changing their input use and production decisions. Fisher-Vanden et al. (2015), for example, show that Chinese industrial firms responded to electricity scarcity by substituting materials for energy, allowing them to avoid substantial productivity losses but at higher production costs. Allcott et al. (2016) find that electricity shortages reduced revenues and producer surplus among Indian manufacturers by about 5–10 percent, while productivity losses were smaller because firms could adjust some inputs during outages. More broadly, Fried and Lagakos (2023) show that eliminating outages can have relatively modest short-run productivity effects but substantially larger long-run effects as firms adjust their scale, entry decisions, and use of productive resources. These findings help explain why a substantial improvement in electricity reliability need not immediately appear as higher sales or employment among incumbent firms.

This paper makes three contributions. First, it shifts attention from the consequences of electricity shortages to the infrastructure investments intended to reduce them. Much of the firm-level literature takes electricity shortages as the treatment or explanatory variable. We instead begin with changes in the underlying grid infrastructure and examine whether those changes improve the electricity reliability that firms actually experience. This allows us to distinguish the infrastructure intervention from the service-quality mechanism through which it is expected to affect firms.

Second, the paper combines geographic infrastructure exposure with predetermined technological exposure. A comparison based only on upgraded and non-upgraded cities could capture other changes occurring across locations. Our DDD design instead asks whether firms that were technologically more dependent on electricity before the treatment period experienced relatively larger changes following grid upgrading. Electricity dependence is measured using industry-level data from the 2006/07 CME, well before the main treatment period. This provides a predetermined, mechanism-based source of variation in firms' exposure to improvements in electricity reliability.

Third, the paper distinguishes between improvements in infrastructure service quality and downstream changes in firm performance. This distinction complements evidence from settings where electricity access itself is expanding. Kassem (2024), for example, finds substantial industrial-development effects from grid expansion in Indonesia, including responses through firm entry and exit. Our setting examines a different stage of electricity-sector development, in which firms are already connected and the relevant change is improved reliability. The results show that grid upgrading can substantially reduce the electricity constraint faced by incumbent firms even when immediate effects on their sales and employment are limited.

The findings have a broader implication for infrastructure policy in developing economies. Expanding

electricity access remains important, but the quality of service after connection also matters. Investments that strengthen transmission and substation infrastructure can reduce the electricity disruptions experienced by firms, particularly those whose production depends more heavily on electricity. At the same time, removing an infrastructure constraint does not necessarily generate an immediate expansion in firm activity. Understanding the development effects of electricity investment therefore requires distinguishing between access, reliability, and the subsequent adjustment of firms.

The remainder of the paper is organized as follows. Section 2 describes Nepal's electricity shortages and grid-upgrading process. Section 3 presents the data and empirical strategy. Section 4 reports the main results and robustness analyses. Section 5 discusses the findings and limitations, and Section 6 concludes.

# 2 Electricity shortages and grid upgrading in Nepal

## 2.1 Electricity shortages and improvements in the grid

Nepal experienced severe electricity shortages for much of the period from 2007 to 2017. Rapid growth in electricity demand, limited domestic generation, seasonal variation in hydropower production, and constraints in the transmission and distribution system contributed to prolonged load shedding. At the height of the crisis, scheduled power cuts lasted many hours per day, creating substantial disruptions for households and businesses. Timilsina and Steinbuks (2021) estimate that these shortages imposed large economic costs through lower production and investment.

Electricity conditions improved substantially after the mid-2010s. The improvement reflected several changes in Nepal's electricity system, including increased domestic generation, electricity imports, better load management, and expansion and reinforcement of the transmission and distribution network. The end of widespread load shedding represented an important change in the overall electricity environment, but improvements in the physical grid continued as Nepal Electricity Authority (NEA) expanded substations, added transformer capacity, constructed transmission lines, and reinforced existing infrastructure.

The scale of this expansion was substantial. NEA records show that high-voltage substation capacity increased from 2,132 MVA in FY 2014/15 to approximately 7,149 MVA in FY 2021/22, while transmission lines of 66 kV and above increased from 2,624 to 5,329 circuit-km over the same period (NEA, 2022). By FY 2022/23, high-voltage substation capacity had reached 8,867 MVA (NEA, 2023). These changes indicate that the improvement in Nepal's electricity system involved not only additional electricity supply but also substantial investment in the infrastructure used to transmit and deliver that electricity.

These grid investments are directly relevant to electricity reliability. NEA reports describe substation reinforcement and capacity expansion as measures intended to reduce interruptions associated with inadequate substation capacity and to improve the reliability and quality of electricity supply. For example, NEA's Grid Substation Reinforcement and Capacity Expansion Project upgraded eight existing grid substations, including Butwal and Banepa, adding a total of 280 MVA of substation capacity. NEA also describes the Kathmandu Valley Transmission Capacity Reinforcement Project as an investment designed to increase substation capacity and improve electricity reliability by reducing distribution-system overload (NEA, 2019). NEA subsequently reported that reinforcement and upgrading works helped reduce electricity interruptions associated with inadequate substation capacity (NEA, 2021).

The timing and location of these investments varied across Nepal. Some cities received substantial additions to transformation or substation capacity during the period between the 2013 and 2023 WBES surveys, while other cities in the survey did not receive a comparable qualifying upgrade. This geographic variation forms the infrastructure-treatment dimension of our empirical design.

## 2.2 Defining grid-upgrade treatment

We classify a city as treated when NEA records document a substantial grid or substation capacity expansion that can be linked to the city in which the firm was located at baseline and that became operational between the 2013 and 2023 WBES surveys. Qualifying investments include the construction of a new grid substation, the addition of substantial transformation capacity, or the replacement or reinforcement of existing substation equipment when it resulted in a clear increase in grid capacity.

We do not classify every electricity-related project as a treatment. Routine maintenance, repair work, capacitor installation, line rehabilitation without a documented capacity expansion, distribution-level improvements without a qualifying grid-capacity change, and projects that remained planned or under construction by the end of the treatment period are excluded. This distinction is important because the purpose of the treatment variable is to identify substantial changes in the capacity of the local grid rather than general electricity-sector activity.

This definition is consistent with NEA's own descriptions of grid reinforcement and capacity expansion. NEA reports separately identify additions of transformers, replacement of existing transformers with higher-capacity units, construction of new substations, and reinforcement of existing substations as measures used to expand grid capacity and improve reliability. NEA also distinguishes these investments from routine maintenance and repair activities.

Using these criteria, the treated group consists of firms located at baseline in Banepa, Biratnagar, Butwal, Dhangadhi, Hetauda, Kathmandu, Lalitpur, Nepalgunj, and Simara. Firms in Bhadrapur and Pokhara form the comparison group because we do not identify a comparable qualifying grid-capacity upgrade for these cities during the treatment period. Appendix Table A1 reports the underlying project information, upgrade type, capacity change, operational timing, and source used to classify each city.

The evidence linking the documented grid investment to the predetermined baseline city is less direct for Biratnagar and Hetauda than for the other treated locations. Rather than dropping these cities from the main specification based on a more subjective classification, we retain them in the baseline treatment definition. As a robustness check, we estimate a stricter specification that excludes firms in Biratnagar and Hetauda from the estimation sample. The main result is very similar under this restriction.

The final matched 2013–2023 estimation sample contains 55 firms observed in both survey years. Of these, 37 firms are located in the nine upgraded cities and 18 are located in the two comparison cities. Because treatment varies at the city level and the number of cities is small, the empirical analysis clusters standard errors by city and uses Webb wild-cluster bootstrap inference, as discussed in Section 3.

## 2.3 Predetermined electricity dependence

The effects of improved grid infrastructure are unlikely to be the same for all manufacturing firms. Production processes differ substantially in their dependence on electricity. An interruption may create a relatively small disruption for a firm whose production uses little electricity, while the same interruption can stop machinery or an entire production process in an electricity-intensive industry. Improvements in electricity reliability should therefore be more valuable to firms whose technologies depend more heavily on continuous electricity supply.

We capture this technological difference using industry-level electricity intensity from Nepal's 2006/07 Census of Manufacturing Establishments (CME). Electricity intensity is measured as purchased electricity quantity in kilowatt-hours per Rs. 1,000 of industry output and is assigned to firms according to their two-digit manufacturing industry. Importantly, the measure is constructed several years before the 2013 baseline and before the qualifying grid upgrades. It therefore captures predetermined differences in technological dependence on electricity rather than firms' electricity use after reliability improved.

This predetermined measure is central to the empirical design. Comparing firms only across upgraded and comparison cities could confound grid investment with other changes occurring across locations. Our DDD approach instead asks whether firms in industries with greater predetermined dependence on electricity

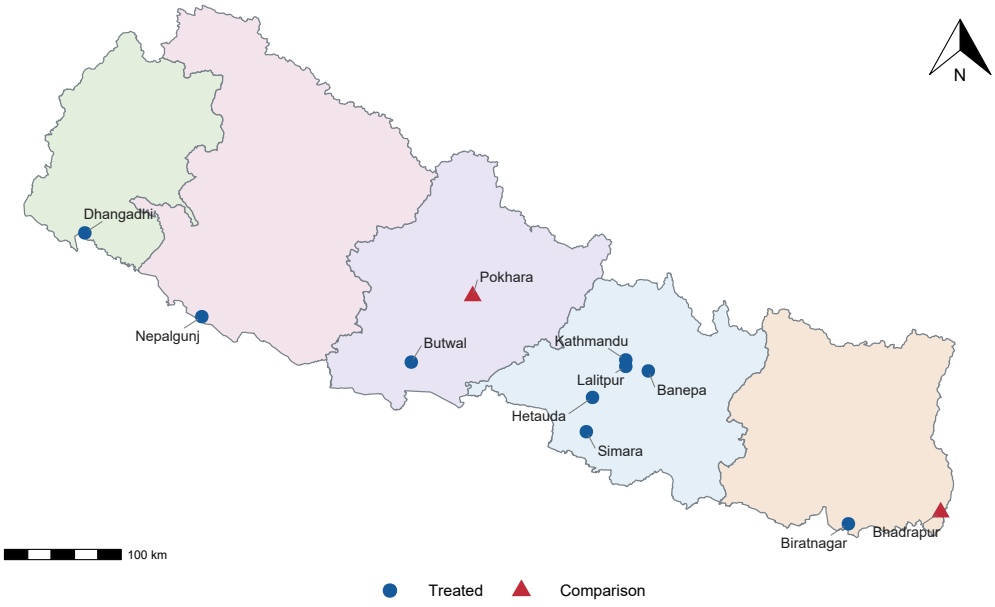



Figure 1: Figure 1. Geographic distribution of treated and comparison cities in Nepal

*Notes:* The map shows the predetermined baseline cities of firms included in the estimation sample. Nine cities are classified as treated and two as comparison cities based on the grid-upgrade treatment definition. City locations represent approximate urban centers rather than individual firm locations or substation service areas.

experienced relatively larger changes after grid upgrading. If the documented grid investments improved electricity reliability, the reduction in outage exposure should be stronger among firms whose production was more dependent on electricity before the upgrades.

Electricity intensity also varies meaningfully within both groups of cities. In the final baseline sample, firms in upgraded and comparison cities show substantial overlap in their predetermined industry electricity intensity, although the treated group has a somewhat wider range. Figure A1 shows this distribution, while Appendix Table A3 reports electricity intensity across the manufacturing industries represented in the data. This overlap allows the DDD specification to compare firms with different levels of technological electricity dependence within both upgraded and comparison locations.

# 3 Data and empirical strategy

## 3.1 Data sources and sample construction

This study combines data from three main sources: the World Bank Enterprise Surveys (WBES), Nepal's 2006/07 Census of Manufacturing Establishments (CME), and the World Development Indicators (WDI) database. We supplement these data with information on local grid infrastructure upgrades from Nepal Electricity Authority (NEA) reports and project records, as described in Section 2.

The firm-level data come from the Nepal World Bank Enterprise Surveys conducted in 2009, 2013, and 2023. The surveys provide information on firm characteristics, electricity outages, sales, employment, production inputs, and other aspects of the business environment. The availability of three survey waves allows us to use the 2013–2023 period for the main analysis and the earlier 2009–2013 period for pre-treatment placebo tests.

The main analysis uses firms that can be matched between the 2013 and 2023 surveys. We retain manufacturing firms for which predetermined industry electricity intensity can be assigned using the 2006/07 CME and for which predetermined city information from the 2009 survey can be linked to the grid-upgrade classification described in Section 2. The final balanced panel contains 55 firms observed in both 2013 and 2023, giving 110 firm-year observations across 11 predetermined baseline cities. Of these firms, 37 are located in cities classified as treated and 18 are located in comparison cities. Appendix Table A2 reports the distribution of firms across the 11 cities in the final estimation sample.

The second main data source is Nepal's 2006/07 Census of Manufacturing Establishments. We use the CME to construct predetermined industry-level electricity intensity. Because the census predates the baseline

Enterprise Survey and the grid upgrades examined in this study, it provides a measure of industries' underlying dependence on electricity that cannot itself be affected by the subsequent infrastructure improvements. We describe the construction of this measure in Section 3.3.

We also use the World Bank's World Development Indicators (WDI) database to obtain Nepal's GDP deflator, with 2011 as the base year. We use the deflator to convert nominal monetary variables into real 2011 values. In particular, sales and the monetary inputs used in constructing the productivity measures are expressed in constant 2011 prices. This ensures that changes in monetary values across survey years reflect changes in real economic activity rather than changes in the general price level.

Together, these data allow us to link four dimensions of the analysis: firms' outcomes and characteristics from the WBES, predetermined technological dependence on electricity from the CME, local exposure to documented grid upgrades from NEA records, and the price information needed to construct comparable real monetary measures across survey years from the WDI.

Appendix Table A5 summarizes the construction of the estimation sample. Of the 482 firms observed in 2013, 202 are also observed in 2023. Requiring predetermined baseline-city information reduces the sample to 119 firms, and matching these firms to predetermined industry electricity intensity from the 2006/07 CME leaves 55 firms in the final DDD sample. Appendix Table A6 compares these firms with the other firms observed in both 2013 and 2023. Baseline characteristics are generally similar across the two groups, although firms in the final sample have significantly higher real sales. The reduction in sample size raises concerns about sample selection and the generalizability of the findings. Although the use of predetermined city information and historical electricity intensity strengthens the empirical design, it restricts the analysis to a relatively small group of surviving and reinterviewed firms. Moreover, the significantly higher baseline sales of firms retained in the final sample suggest that these establishments may differ systematically from other firms. Consequently, the findings may not be representative of smaller establishments or firms that entered or exited during the study period.

## 3.2 Grid-upgrade treatment

We construct a city-level treatment indicator based on the grid-upgrade classification described in Section 2. A city is classified as treated if a qualifying grid or substation transformation-capacity expansion became operational between the 2013 and 2023 Enterprise Survey waves. Treatment status is assigned using each firm's predetermined city reported in the 2009 survey and remains fixed throughout the analysis.

The treatment classification is based on documented grid and substation upgrades reported by the Nepal

Electricity Authority (NEA). We focus on major infrastructure improvements that strengthen electricity delivery capacity rather than routine maintenance or general improvements in electricity supply. However, the precise geographic coverage of some substations is not always available. Therefore, treatment status represents city-level exposure to documented grid upgrading rather than verified infrastructure exposure for each individual firm. This distinction is particularly relevant for cities served by substations outside their administrative boundaries.

Under the main treatment definition, Banepa, Biratnagar, Butwal, Dhangadhi, Hetauda, Kathmandu, Lalitpur, Nepalgunj, and Simara are classified as treated, while Bhadrapur and Pokhara form the comparison group. The treatment indicator, $Upgrade_c$, equals one for firms whose predetermined baseline city is classified as treated and zero for firms in comparison cities. Figure 1 shows the geographic distribution of these cities, while Appendix Table A1 presents the documented infrastructure changes, timing, and NEA sources supporting their classification.

The main classification includes Biratnagar and Hetauda, where the connection between the documented infrastructure improvements and the baseline city is less direct than in other treated locations. As a robustness check, we therefore estimate a stricter specification that excludes firms in these two cities to examine whether the results depend on these less certain treatment assignments.

### 3.3 Predetermined industry electricity intensity

We measure firms' predetermined dependence on electricity using industry-level electricity intensity from Nepal's 2006/07 Census of Manufacturing Establishments (CME). For each two-digit manufacturing industry, electricity intensity is calculated as

$$\mathrm{EI}_j = \frac{\text{Purchased Electricity quantity}_j}{\text{Output}_j}, \tag{1}$$

where $j$ denotes the two-digit manufacturing industry. Purchased electricity quantity is measured in kilowatt-hours (kWh) and output in thousands of Nepalese rupees, so electricity intensity is expressed as kWh per Rs. 1,000 of output. We assign each WBES firm the electricity intensity of its corresponding manufacturing industry.

Using the 2006/07 CME provides a measure of electricity dependence that predates the 2013 baseline and the subsequent grid upgrades. This avoids using firms' contemporaneous electricity use, which could itself respond to changes in electricity reliability. Electricity intensity ranges from approximately 0.49 to

10.13 kWh per Rs. 1,000 of output across the industries represented in the final sample, with substantial overlap between firms in upgraded and comparison cities (Figure A1). Appendix Table A3 reports electricity intensity by industry.

For the empirical analysis, we standardize electricity intensity using the broader sample with available CME intensity data to have a mean of zero and a standard deviation of one. The DDD coefficient can therefore be interpreted as the differential post-treatment change associated with a one-standard-deviation increase in predetermined electricity intensity in upgraded cities relative to comparison cities.

### 3.4 Electricity-reliability outcomes

We examine four measures of electricity reliability reported in the WBES. The first is outage incidence, an indicator equal to one if a firm reports experiencing an electricity outage during the fiscal year. The second is monthly outage frequency, measured as the reported number of electricity interruptions in a typical month. The third is average outage duration, measured in hours per interruption. Our preferred measure combines the frequency and duration of outages into monthly outage exposure:

$$\text{OutageExposure}_{it} = \text{OutageFrequency}_{it} \times \text{OutageDuration}_{it}. \tag{2}$$

This measure approximates the total number of hours in a typical month during which firm $i$ is exposed to electricity interruptions. It captures changes in both the number and duration of outages, which may not be apparent when the two dimensions are considered separately.

Because frequency and duration are self-reported, their product can occasionally exceed the number of hours physically available in a month. We therefore cap monthly outage exposure at 720 hours:

$$\text{OutageExposure}_{it}^{720} = \min(\text{OutageFrequency}_{it} \times \text{OutageDuration}_{it}, 720). \tag{3}$$

The 720-hour threshold corresponds to the total number of hours in a 30-day month. The cap affects only two observations in the 2009 data and none in the main 2013–2023 estimation sample.

### 3.5 Firm-performance and productivity outcomes

We examine four firm-performance outcomes: real sales, employment, labor productivity, and total factor productivity (TFP). Nominal sales are converted to constant 2011 Nepalese rupees using Nepal's GDP

deflator from the World Development Indicators (WDI), World Bank database, and expressed in logarithms. Employment is measured as the number of permanent full-time employees and is also expressed in logarithms. Labor productivity is measured as real sales per permanent full-time employee and expressed in logarithms.

To measure firm-level productivity, we estimate a non-dynamic Cobb–Douglas production function:

$$\ln Y_{ijt} = \beta_L \ln L_{ijt} + \beta_K \ln K_{ijt} + \beta_M \ln M_{ijt} + \lambda_t + \delta_j + \varepsilon_{ijt}, \tag{4}$$

where $Y_{ijt}$ is real annual sales of firm $i$ in two-digit industry $j$ and survey year $t$. $L_{ijt}$ is permanent full-time employment, $K_{ijt}$ is the real replacement value of machinery, and $M_{ijt}$ is the real cost of raw materials and intermediate inputs used in production. Sales, capital, and material inputs are expressed in constant 2011 Nepalese rupees. The specification includes survey-year fixed effects, $\lambda_t$, and two-digit industry fixed effects, $\delta_j$. We estimate the production function using the broader manufacturing sample with complete information on the required inputs rather than restricting it to firms in the final DDD sample.

Firm-level TFP is then constructed using the estimated input coefficients:

$$\text{TFP}_{ijt}^{CD} = \ln Y_{ijt} - \hat{\beta}_L \ln L_{ijt} - \hat{\beta}_K \ln K_{ijt} - \hat{\beta}_M \ln M_{ijt}. \tag{5}$$

We use this non-dynamic Cobb–Douglas measure as our main TFP outcome because the WBES waves are separated by long and unequal intervals—four years between 2009 and 2013 and ten years between 2013 and 2023. As a robustness check, we also estimate the production function using the Wooldridge (2009) approach and use the estimated coefficients to construct an alternative residual-based TFP measure for firms with complete production-function inputs. The Wooldridge production-function coefficients are estimated using the 171 observations satisfying the estimator's dynamic panel requirements; we then apply the estimated input coefficients to observations with complete production-function inputs to construct the alternative residual-based TFP measure. The two TFP measures are highly correlated, with a correlation coefficient ($r = 0.976$). Appendix Table A4 reports the production-function estimates used to construct the two TFP measures, along with the corresponding sample sizes and tests of constant returns to scale.

### 3.6 Difference-in-differences-in-differences specification

Our empirical strategy exploits variation along three dimensions: time, exposure to grid upgrades, and firms' predetermined industry electricity intensity. The main analysis compares firms observed in 2013 and 2023.

Let $Post_t$ equal one for 2023 and zero for 2013. $Treat_c$ equals one if firm $i$'s predetermined baseline city received a qualifying grid upgrade between 2013 and 2023, and zero for firms in comparison cities. $EI_j$ denotes the standardized predetermined electricity intensity of firm $i$'s two-digit manufacturing industry, constructed from the 2006/07 Census of Manufacturing Establishments. We estimate

$$Y_{ict} = \alpha_i + \lambda_t + \beta_1(\text{Post}_t \times \text{EI}_j) + \beta_2(\text{Post}_t \times \text{Treat}_c) + \beta_3(\text{Post}_t \times \text{Treat}_c \times \text{EI}_j) + X'_{ict}\gamma + \varepsilon_{ict}, \qquad (6)$$

where $Y_{ict}$ is an electricity-reliability or firm-performance outcome for firm $i$, located in city $c$, in year $t$. Firm fixed effects, $\alpha_i$, absorb time-invariant differences across firms, while year fixed effects, $\lambda_t$, capture common changes between 2013 and 2023. $X_{ict}$ includes manager experience and exporter status.

The coefficient of interest is $\beta_3$. It measures whether the change in an outcome between 2013 and 2023 varies more strongly with predetermined electricity intensity in upgraded cities than in comparison cities. Because electricity intensity is standardized, $\beta_3$ can be interpreted as the additional post-period change associated with a one-standard-deviation increase in predetermined industry electricity intensity in upgraded cities relative to comparison cities.

This specification allows common changes over time to differ both by firms' predetermined electricity intensity and by whether their city received a grid upgrade. The identifying variation therefore comes from whether more electricity-intensive firms experienced different changes after grid upgrading, relative to less electricity-intensive firms and to firms with similar electricity intensity in comparison cities.

## 3.7 Identification

Identification comes from comparing changes between 2013 and 2023 across firms that differ in two dimensions: whether their baseline city received a qualifying grid upgrade and how electricity-intensive their industry was before the study period. The key identifying assumption is that, in the absence of grid upgrades, the relationship between predetermined industry electricity intensity and changes in firm outcomes would have evolved similarly in upgraded and comparison cities. Under this assumption, the triple-interaction coefficient captures the differential change associated with grid upgrading among firms with greater predetermined dependence on electricity.

The use of predetermined industry electricity intensity is important for this design. Electricity intensity is measured using the 2006/07 Census of Manufacturing Establishments, several years before the 2013 baseline

and before the grid upgrades examined in the main analysis. It therefore does not reflect firms' responses to subsequent improvements in electricity reliability. In addition, electricity intensity is measured at the two-digit industry level rather than from each firm's contemporaneous electricity use, reducing concerns that the exposure measure itself responds to firm-specific shocks during the study period.

Firm fixed effects absorb time-invariant differences across firms, including persistent differences in location, industry, and other firm characteristics. Year fixed effects account for shocks common to all firms between 2013 and 2023. The design therefore does not attribute Nepal's overall improvement in electricity supply between 2013 and 2023 to the local grid upgrades. Rather, it tests whether the relationship between predetermined electricity dependence and changes in firm outcomes differed systematically between upgraded and comparison cities. The DDD specification also allows the change over time to differ by electricity intensity and by upgrade status through the lower-order interactions. As a result, identification of the coefficient of interest comes from whether the relationship between predetermined electricity intensity and outcome changes differs between upgraded and comparison cities.

Grid upgrades were not randomly assigned across Nepal, and their placement may have been influenced by local electricity demand, industrial growth, or infrastructure needs. These factors could also affect firm outcomes, particularly in electricity-intensive industries. Although the DDD framework accounts for time-invariant firm characteristics and common changes over time, it cannot fully eliminate concerns about unobserved city-specific developments correlated with electricity intensity. We examine this concern using a pre-treatment placebo test for 2009–2013 and alternative treatment classifications and sample restrictions. However, these tests cannot completely rule out differential changes unrelated to grid upgrading. The estimated coefficients should therefore be interpreted with this limitation in mind.

We also examine the sensitivity of the results to alternative treatment definitions and sample composition. In particular, we estimate a stricter specification that excludes firms in Biratnagar and Hetauda from the estimation sample, and we re-estimate the main specification after excluding Kathmandu and Bhadrapur. These checks assess whether the main results are driven by treatment classification or by individual cities in the relatively small estimation sample.

### 3.8 Inference with a small number of clusters

Because grid-upgrade treatment varies at the city level, we cluster standard errors by city. This allows for arbitrary correlation in the regression errors across firms within the same city. The main 2013–2023 estimation sample, however, contains only 11 cities, making conventional cluster-robust inference potentially

unreliable in finite samples (Cameron et al., 2008).

To address the small number of clusters, we base statistical inference on the wild cluster bootstrap using Webb weights (Webb, 2023), which are designed to improve inference when the number of clusters is small. For each coefficient of interest, we use 99,999 bootstrap replications and report the resulting wild-cluster bootstrap $p$-values. We also report conventional city-clustered standard errors and, in the robustness analyses, the corresponding conventional $p$-values.

Our statistical conclusions are therefore based primarily on the wild-cluster bootstrap $p$-values rather than conventional asymptotic $p$-values. This approach is applied consistently to the main DDD estimates, robustness checks, and pre-treatment placebo analysis.

### 3.9 Pre-treatment placebo

We use the 2009–2013 period to examine whether the main DDD estimates could reflect differential changes that were already present before the grid upgrades considered in the main analysis. We assign cities their subsequent grid-upgrade status and estimate the same DDD specification using 2009 as the baseline and 2013 as the post period. Predetermined industry electricity intensity is defined using the same 2006/07 CME measure used in the main analysis.

The coefficient of interest is the interaction between the 2013 indicator, subsequent grid-upgrade status, and predetermined electricity intensity. A statistically significant placebo coefficient would indicate that the relationship between electricity intensity and outcome changes differed between the two groups of cities even before the upgrades, which would raise concern about the identifying assumption. Conversely, the absence of such a pattern provides supporting evidence for the DDD design. We apply the same city-level clustering and wild cluster bootstrap inference used in the main analysis.

## 4 Results

### 4.1 Descriptive analysis

Table 1 reports electricity reliability and firm characteristics across the 2009, 2013, and 2023 WBES waves. Electricity reliability improved substantially over this period, although the different outage measures do not follow the same pattern. The share of firms reporting an outage fell from 98.4% in 2009 to 60.8% in 2013, before increasing to 75.4% in 2023. Monthly outage frequency followed a similar pattern, falling from 49.8 interruptions in 2009 to 7.4 in 2013 and then increasing to 14.8 in 2023.

The severity of outages, however, declined more consistently. Average outage duration fell from 7.0 hours per interruption in 2009 to 1.1 hours in 2013 and 0.7 hours in 2023. Monthly outage exposure, which combines frequency and duration, declined sharply from 221.5 hours in 2009 to 31.6 hours in 2013 and 13.7 hours in 2023. Reported sales losses due to outages also fell from 28.1% of annual sales in 2009 to 10.5% in 2013 and 3.0% in 2023. These patterns show why monthly outage exposure is useful in this setting: although firms continued to report outages in 2023, the interruptions were substantially shorter and imposed a much smaller overall burden.

Firm outcomes also changed over the period. Mean log real sales increased from 16.11 in 2009 to 17.00 in 2023, while mean log real labor productivity increased from 13.28 to 13.98. Mean log employment was relatively stable between 2009 and 2013 before increasing in 2023. The unconditional Cobb–Douglas TFP mean increased from 2.71 in 2009 to 3.02 in 2013 and then declined slightly to 2.92 in 2023.

These descriptive changes should not be interpreted as effects of grid upgrading. The number and composition of firms differ across survey waves, and Nepal experienced broader changes in electricity supply and economic conditions over this period. We therefore turn to the DDD estimates, which compare changes within matched firms while exploiting variation in grid-upgrade status and predetermined industry electricity intensity.

## 4.2 Effects of grid upgrades on electricity reliability

Table 2 presents the DDD estimates for electricity reliability. The coefficient for outage incidence is negative and statistically significant ($-0.323$, Webb $p = 0.043$). Because electricity intensity is standardized, this estimate indicates that a one-standard-deviation increase in predetermined industry electricity intensity is associated with an additional 32.3 percentage-point decline in the probability of reporting an outage in upgraded cities relative to comparison cities between 2013 and 2023.

The strongest result is for monthly outage exposure. The DDD coefficient is $-14.135$, with a Webb wild-cluster bootstrap $p$-value of 0.007. Thus, a one-standard-deviation increase in predetermined electricity intensity is associated with an additional reduction of about 14.1 hours of monthly outage exposure in upgraded cities relative to comparison cities between 2013 and 2023. This result indicates that firms in industries with greater predetermined dependence on electricity experienced larger improvements in overall electricity reliability following grid upgrading.

The coefficients for monthly outage frequency ($-2.759$, Webb $p = 0.733$) and average outage duration ($-0.425$, Webb $p = 0.287$) are also negative but statistically insignificant. Overall, the results are strongest

Table 1: Descriptive Statistics Across Nepal World Bank Enterprise Survey Waves

| | 2009 | | | 2013 | | | 2023 | | |
|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | SD | N | Mean | SD | N | Mean | SD | N |
| **Panel A. Electricity reliability** | | | | | | | | | |
| Outage incidence | 0.984 | 0.127 | 368 | 0.608 | 0.489 | 482 | 0.754 | 0.431 | 582 |
| Monthly outage frequency | 49.837 | 20.474 | 367 | 7.402 | 13.127 | 482 | 14.802 | 19.360 | 582 |
| Average outage duration (hours) | 6.956 | 7.511 | 366 | 1.056 | 1.985 | 482 | 0.713 | 1.144 | 582 |
| Monthly outage exposure (hours) | 221.473 | 64.385 | 366 | 31.635 | 77.045 | 482 | 13.672 | 28.114 | 582 |
| Sales lost due to outages (%) | 28.135 | 18.134 | 326 | 10.490 | 14.078 | 459 | 3.014 | 4.634 | 575 |
| **Panel B. Firm performance** | | | | | | | | | |
| Log real sales | 16.110 | 1.877 | 363 | 16.226 | 2.133 | 471 | 17.002 | 2.201 | 580 |
| Log employment | 2.829 | 1.124 | 368 | 2.757 | 1.210 | 482 | 3.033 | 1.252 | 582 |
| Log real labor productivity | 13.278 | 1.350 | 363 | 13.483 | 1.333 | 471 | 13.979 | 1.460 | 580 |
| TFP, Cobb–Douglas | 2.705 | 0.659 | 126 | 3.020 | 0.425 | 221 | 2.920 | 0.388 | 201 |
| **Panel C. Firm characteristics** | | | | | | | | | |
| Manager experience (years) | 15.318 | 9.057 | 368 | 15.936 | 9.320 | 482 | 20.292 | 11.744 | 582 |
| Exporter | 0.101 | 0.301 | 368 | 0.133 | 0.340 | 482 | 0.292 | 0.455 | 582 |
| Predetermined electricity intensity | 3.737 | 2.512 | 137 | 3.516 | 2.413 | 240 | 3.508 | 2.234 | 216 |
| Survey observations | | | 368 | | | 482 | | | 582 |

*Notes:* The table reports descriptive statistics using all available observations in each Nepal World Bank Enterprise Survey wave. Outage incidence equals one if an establishment experienced an electricity outage. Monthly outage frequency is the number of reported outages in a typical month. Average outage duration is measured in hours per interruption. Monthly outage exposure combines outage frequency and duration and is capped at 720 hours, the maximum number of hours in a 30-day month. Sales lost due to outages is expressed as a percentage of annual sales. Real sales and real labor productivity are expressed in logarithms. TFP is based on the preferred Cobb–Douglas productivity measure. Exporter is an indicator for firms reporting direct or indirect exports. Predetermined electricity intensity is measured as purchased electricity quantity (kWh) per Rs. 1,000 of industry output using the 2006/07 Census of Manufacturing Establishments. Sample sizes vary across variables because of missing observations and differences in the samples available for constructing TFP and predetermined electricity intensity.

for monthly outage exposure, which captures changes in both the frequency and duration of electricity interruptions.

Table 2: Effects of Grid Upgrades on Electricity Reliability and Firm Performance

| | **Panel A. Electricity reliability** | | | | **Panel B. Firm performance and productivity** | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Outage incidence | Outage frequency | Outage duration | Monthly outage exposure | Log real sales | Log employment | Log labor productivity | Cobb–Douglas TFP | Wooldridge TFP |
| Post × Upgrade × Electricity intensity | -0.323** | -2.759 | -0.425 | -14.135*** | 0.248 | 0.038 | -0.316 | 0.131* | 0.109* |
| | (0.072) | (4.478) | (0.400) | (1.510) | (0.381) | (0.262) | (0.479) | (0.080) | (0.065) |
| Webb $p$-value | [0.043] | [0.733] | [0.287] | [0.007] | [0.635] | [0.917] | [0.705] | [0.084] | [0.082] |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 110 | 110 | 110 | 110 | 108 | 110 | 108 | 96 | 96 |
| City clusters | 11 | 11 | 11 | 11 | 11 | 11 | 11 | 11 | 11 |

*Notes:* The table reports the coefficient on the triple interaction Post × Upgrade × Electricity intensity from the difference-in-differences-in-differences specification. Post equals one for 2023. Upgrade identifies firms located in cities with qualifying grid upgrades between 2013 and 2023. Electricity intensity is predetermined industry-level electricity intensity from the 2006/07 Census of Manufacturing Establishments and is standardized to have mean zero and standard deviation one. All specifications include firm and year fixed effects and controls for manager experience and exporter status. Standard errors clustered at the city level are reported in parentheses. Webb wild-cluster bootstrap $p$-values based on 99,999 replications are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% level based on Webb wild-cluster inference.

### 4.3 Effects on firm performance and productivity

Panel B of Table 2 examines whether the improvements in electricity reliability were accompanied by changes in firm performance. The estimated effects on real sales, employment, and labor productivity are not statistically significant. The DDD coefficients are 0.248 for real sales, 0.038 for employment, and −0.316 for labor productivity, with Webb $p$-values of 0.635, 0.917, and 0.705, respectively. These estimates therefore provide no clear evidence of changes in firm sales, employment, or labor productivity associated with grid upgrading over the study period.

The TFP results provide more suggestive evidence. The DDD coefficient for the preferred Cobb–Douglas TFP measure is 0.131 (Webb $p = 0.084$). Because electricity intensity is standardized, this estimate indicates that a one-standard-deviation increase in predetermined electricity intensity is associated with an additional 0.131-log-point increase in TFP in upgraded cities relative to comparison cities between 2013 and 2023. This corresponds to approximately 14 percent when expressed in percentage terms. The alternative Wooldridge-based TFP measure produces a similar coefficient of 0.109 (Webb $p = 0.082$), equivalent to approximately 11.5 percent. Although the two productivity measures produce similar estimates, they are constructed using closely related firm-level information and therefore do not provide independent confirmation of productivity gains. Furthermore, changes in measured TFP may reflect differences in capacity utilization or input measurement rather than improvements in underlying technical efficiency. Given that both estimates are significant only at the 10 percent level, we interpret the productivity results as suggestive rather than conclusive.

Overall, the results show stronger evidence that grid upgrading improved electricity reliability than that it changed firm performance. We find suggestive evidence of higher TFP among more electricity-intensive firms in upgraded cities, but no statistically significant effects on sales, employment, or labor productivity.

### 4.4 Robustness of the electricity reliability effects

Table 3 examines the robustness of the main result for monthly outage exposure to alternative treatment definitions, sample restrictions, and model specification. The baseline DDD coefficient is −14.135 (Webb $p = 0.007$). Using the stricter treatment definition, which excludes Biratnagar and Hetauda because the evidence linking their documented upgrades to the baseline city is less direct, produces a very similar estimate of −14.865 (Webb $p = 0.007$).

The estimate is also stable when potentially influential cities are excluded. Dropping Kathmandu gives a coefficient of −13.378 (Webb $p = 0.036$), while excluding Bhadrapur produces an estimate of −15.303 (Webb

$p = 0.025$). The magnitude of the estimated reduction therefore remains close to the baseline result across these alternative treatment and sample definitions.

We also examine the sensitivity of the results to the inclusion of firm-level controls. Excluding manager experience and exporter status produces a DDD coefficient of $-13.805$, close to the controlled estimate of $-14.135$. However, the Webb wild-cluster bootstrap $p$-value increases substantially from 0.007 to 0.463. Thus, while the estimated magnitude is relatively stable, statistical significance is highly sensitive to the model specification. Given the small number of city clusters, we interpret the evidence cautiously and distinguish stability in coefficient estimates from robustness of statistical inference.

To examine this sensitivity further, we re-estimate the model using alternative control specifications. Appendix Table A7 shows that the DDD coefficient remains stable, ranging from $-12.4$ to $-15.6$ hours across specifications with no firm-level controls, individual controls, and predetermined baseline characteristics. The estimates are statistically significant across all specifications using conventional city-clustered inference. However, the Webb wild-cluster bootstrap $p$-values vary across specifications. Overall, the stability of the point estimates suggests that the estimated magnitude is not sensitive to the choice of controls, while inference is more sensitive when accounting for the small number of city clusters.

Table 3: Robustness of the Effect of Grid Upgrades on Monthly Outage Exposure

| Specification | DDD coef. | Clustered SE | Cluster $p$ | Webb $p$ | Obs. | Clusters |
|---|---|---|---|---|---|---|
| Baseline specification | $-14.135^{***}$ | (1.510) | $< 0.001$ | [0.007] | 110 | 11 |
| Strict treatment definition | $-14.865^{***}$ | (1.634) | $< 0.001$ | [0.007] | 106 | 9 |
| Excluding Kathmandu | $-13.378^{**}$ | (4.230) | 0.010 | [0.036] | 80 | 10 |
| Excluding Bhadrapur | $-15.303^{**}$ | (1.257) | $< 0.001$ | [0.025] | 100 | 10 |
| Without firm-level controls | $-13.805$ | (2.724) | $< 0.001$ | [0.463] | 110 | 11 |
| Firm and year fixed effects | Yes | | | | | |

*Notes:* The dependent variable is monthly outage exposure. The reported coefficient is the interaction of 2023, grid-upgrade treatment, and standardized predetermined industry electricity intensity. All specifications include firm and year fixed effects and, except for the final row, controls for manager experience and exporter status. The strict treatment definition excludes Biratnagar and Hetauda. Standard errors clustered by baseline city are reported in parentheses. Cluster $p$-values are based on conventional city-clustered inference. Webb wild-cluster bootstrap $p$-values based on 99,999 replications are reported in brackets. Significance levels are based on Webb $p$-values: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Appendix Figure A2 presents leave-one-city-out estimates for monthly outage exposure. The estimated coefficient remains negative and relatively close to the baseline estimate when individual treated cities or Bhadrapur are excluded. However, excluding Pokhara reverses the sign of the estimated coefficient. This is an important limitation because Pokhara accounts for 13 of the 18 comparison firms, and its exclusion leaves only five comparison firms in Bhadrapur. Moreover, Pokhara experienced electricity-system improvements during the study period, although these were not classified as qualifying grid-capacity upgrades under our treatment definition. The results therefore depend substantially on Pokhara's role as a comparison city, raising concerns about the stability of the estimated reliability effect and its interpretation.

### 4.5 Pre-treatment placebo tests

Table 4 reports the pre-treatment placebo estimates using the 2009–2013 period. We assign cities their subsequent grid-upgrade status and estimate the same DDD specification used in the main analysis. If the main results were driven by pre-existing differential changes between more and less electricity-intensive firms in upgraded and comparison cities, we would expect to observe a similar pattern during this earlier period.

We find no statistically significant placebo effects for any of the electricity-reliability outcomes. The placebo coefficient for monthly outage exposure is $-72.465$, but it is imprecisely estimated and statistically insignificant under the Webb wild-cluster bootstrap ($p = 0.519$). The placebo estimates for outage incidence, frequency, and duration are also statistically insignificant, with Webb $p$-values of 0.749, 0.710, and 0.649, respectively.

We similarly find no statistically significant placebo effects for firm performance. The Webb $p$-values are 0.144 for real sales, 0.491 for employment, and 0.122 for labor productivity. The placebo estimates for both productivity measures are also insignificant: $-0.151$ for Cobb–Douglas TFP ($p = 0.358$) and $-0.174$ for Wooldridge-based TFP ($p = 0.303$).

Overall, we find no statistically significant evidence of differential pre-period changes associated with pre-determined electricity intensity between future upgraded and comparison cities during 2009–2013. However, the placebo test has limited statistical power, particularly given the small number of comparison cities and firms. Moreover, the availability of only two pre-treatment survey waves allows us to examine a single pre-period difference rather than dynamic pre-treatment trends. Therefore, the insignificant placebo estimates should not be interpreted as definitive evidence that the identifying assumption holds.

Table 4: Pre-Treatment Placebo Tests, 2009–2013

| | **Panel A. Electricity reliability** | | | | **Panel B. Firm performance and productivity** | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Outage incidence | Outage frequency | Outage duration | Monthly outage exposure | Log real sales | Log employment | Log labor productivity | Cobb–Douglas TFP | Wooldridge TFP |
| 2013 × Future upgrade × Electricity intensity | -0.050 | 5.615 | -2.495 | -72.465 | -0.512 | 0.021 | -0.518 | -0.151 | -0.174 |
| Clustered SE | (0.134) | (8.757) | (2.360) | (29.469) | (0.314) | (0.035) | (0.319) | (0.159) | (0.163) |
| Webb $p$-value | [0.749] | [0.710] | [0.649] | [0.519] | [0.144] | [0.491] | [0.122] | [0.358] | [0.303] |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 202 | 202 | 200 | 200 | 198 | 202 | 198 | 164 | 164 |
| City clusters | 11 | 11 | 11 | 11 | 11 | 11 | 11 | 10 | 10 |

*Notes:* The table reports pre-treatment placebo DDD estimates using 2009 and 2013, both of which precede the qualifying grid upgrades. The coefficient of interest is the interaction 2013 × Future upgrade × Electricity intensity. Future upgrade uses the same treatment classification as the main 2013–2023 analysis. Electricity intensity is predetermined industry-level electricity intensity from the 2006/07 Census of Manufacturing Establishments, standardized to mean zero and standard deviation one. All models include firm and year fixed effects and controls for manager experience and exporter status. Standard errors clustered at the city level are reported in parentheses. Webb wild-cluster bootstrap $p$-values based on 99,999 replications are reported in brackets.

# 5 Discussion and limitations

The main finding of this study is that grid upgrading is associated with substantial improvements in the electricity reliability experienced by firms. The effect is strongest for monthly outage exposure, which captures both the frequency and duration of electricity interruptions. More electricity-intensive firms experienced larger reductions in outage exposure in upgraded cities relative to comparison cities. This pattern is consistent with the idea that improvements to the electricity network matter most for firms whose production processes depend more heavily on a continuous supply of electricity.

This finding adds to a broader literature showing that unreliable electricity can impose substantial costs on manufacturing firms. Fisher-Vanden et al. (2015) find that Chinese industrial firms responded to electricity shortages by changing their input mix, particularly by substituting materials for energy, which allowed them to limit productivity losses but increased production costs. Allcott et al. (2016) similarly find that electricity shortages reduced revenues and producer surplus among Indian manufacturers by about 5–10 percent, while productivity losses were smaller because firms could adjust some inputs during outages. Evidence from Ghana also shows that eliminating outages can improve manufacturing productivity and that firms adjust their product mix in response to unreliable electricity (Abeberese et al., 2021). Our results complement this literature by examining the other side of the relationship: rather than estimating the cost of electricity shortages, we show that improvements to the underlying grid are associated with substantial reductions in the electricity disruptions experienced by firms.

The findings are particularly relevant for Nepal, where unreliable electricity was a major economic constraint for many years. Timilsina and Steinbuks (2021) estimate that the load-shedding crisis imposed large costs on the Nepalese economy and that reliable electricity supply would have substantially increased GDP and investment. Alberini et al. (2022) also show that Nepalese households place considerable value on avoiding electricity outages. Our findings provide complementary firm-level evidence. They suggest that improvements in grid infrastructure were associated not only with broader improvements in Nepal's electricity system but also with lower outage exposure among connected manufacturing firms, particularly those in more electricity-intensive industries.

The firm-performance results are more modest. We do not find statistically significant effects on real sales, employment, or labor productivity. The TFP estimates are more encouraging but should be interpreted cautiously. Both the Cobb–Douglas and Wooldridge-based measures produce positive DDD coefficients, and both are statistically significant at the 10 percent level under the preferred Webb wild-cluster bootstrap inference. The similarity of the two estimates provides suggestive evidence that improved electricity reliability

may have been accompanied by gains in production efficiency, but the evidence is not strong enough to establish a clear productivity effect.

This difference between the strong reliability results and the weaker firm-performance results is consistent with previous research. Allcott et al. (2016) show that firms can partly adjust to electricity shortages by changing how they use inputs, which can make measured productivity effects smaller than the underlying cost of unreliable electricity. Fisher-Vanden et al. (2015) similarly show that firms can reorganize production in response to electricity scarcity. More generally, Fried and Lagakos (2023) show that the short-run productivity effects of eliminating outages can be relatively modest even when the longer-run effects are substantially larger. Their model emphasizes that longer-run gains can arise through changes in firm scale, entry, and the utilization of productive resources. Our findings are consistent with this broader distinction between removing an infrastructure constraint and observing an immediate expansion in firm outcomes.

The absence of statistically significant effects on sales and employment should therefore not be interpreted as evidence that improved electricity reliability has no economic value. Reliable electricity can reduce production disruptions and make the use of existing inputs easier without necessarily causing firms to expand employment or sales over the same period. At the same time, our data do not allow us to identify the specific mechanism behind the positive TFP estimates. We therefore view the TFP results as suggestive evidence of productivity improvement rather than evidence that grid upgrading affected firms specifically through a particular efficiency channel.

The economic value of improved electricity reliability may extend beyond the firm-performance outcomes examined here. In the baseline specification, the estimated reduction of approximately 14 hours of monthly outage exposure could represent a meaningful improvement for electricity-intensive firms, although this estimate is sensitive to the comparison-group composition and statistical specification. Table 1 also shows that reported sales losses attributed to outages declined from 10.5 percent in 2013 to 3.0 percent in 2023, although these descriptive changes cannot be attributed solely to grid upgrading. Evidence from Zambia similarly documents the economic costs of electricity outages for manufacturing firms Ahmed et al. (2019), the implications of hydropower dependence for backup diesel generation and emissions Ahmed et al. (2020), and the consequences of outages for households Ahmed et al. (2023). These studies highlight that improvements in electricity reliability may generate economic benefits through reduced production disruptions and reliance on alternative power sources, even when changes in measured sales or employment are difficult to identify.

Our results also differ in an important way from studies of initial electrification. Kassem (2024), for example, finds that electricity-grid expansion in Indonesia increased manufacturing output, employment,

and the number of firms, with firm entry accounting for an important part of the increase in output. The firms in our study, however, were already connected to the electricity system. The relevant change in Nepal is therefore an improvement in the quality and reliability of an existing service rather than initial access to electricity. This distinction may help explain why the strongest effects in our setting appear in electricity reliability rather than in firm expansion. It also suggests that the economic effects of electricity infrastructure may differ depending on the stage of electricity-sector development.

Several limitations should be considered when interpreting the results. First, grid upgrades were not randomly assigned across cities. Upgrade decisions may have been related to electricity demand, existing infrastructure constraints, or other characteristics of local economies. Our DDD design reduces this concern by combining geographic exposure to grid upgrading with predetermined industry electricity intensity measured using the 2006/07 Census of Manufacturing Establishments. Firm fixed effects also absorb persistent differences across firms, and the 2009–2013 placebo analysis provides no statistically significant evidence of a similar differential pattern before the qualifying upgrades. However, because the main analysis spans 2013–2023, other changes during this period may also have affected firms differently across locations. Although the DDD specification accounts for common time changes and differential changes associated with predetermined electricity intensity, it cannot rule out all city-specific shocks that may have coincided with grid upgrading. In addition, with only two pre-treatment survey waves, the placebo analysis can assess differential changes over 2009–2013 but cannot establish longer pre-treatment trends.

Second, the main analysis is based on a relatively small matched sample of 55 firms located in 11 cities. Only two cities form the comparison group, and 13 of the 18 comparison firms are located in Pokhara. This concentration limits the amount of independent geographic variation available for identifying the effects. We address the small number of city clusters using Webb wild-cluster bootstrap inference and examine sensitivity to alternative treatment definitions and city exclusions. The estimated magnitude of the main outage-exposure effect is generally stable across these checks, although inference becomes less precise in some specifications. The results should therefore be interpreted in light of the relatively small number of cities supporting the DDD comparison.

Third, the long interval between the 2013 and 2023 WBES waves prevents us from observing how firms adjusted during the years between the two surveys. Grid upgrades also became operational at different points during this period. The estimates therefore capture changes over the broader 2013–2023 period rather than a common short-run response immediately following an upgrade. More frequent firm-level data would be needed to distinguish immediate from gradual responses and to examine how effects evolve with the length of exposure to improved grid infrastructure.

Finally, the balanced-panel design focuses on firms that can be matched across the 2013 and 2023 surveys. This is useful for controlling for time-invariant firm characteristics, but it means that the estimates describe the experience of surviving and reinterviewed firms rather than all manufacturing firms in Nepal. Appendix Table A6 shows that the final estimation sample is similar to the other matched firms on most observed baseline characteristics, although firms in the final sample have higher baseline real sales. The analysis does not capture possible effects on firm entry and exit, which may be an important part of the longer-run response to electricity infrastructure. This is particularly relevant given evidence from other settings that electrification can affect industrial development through firm turnover as well as changes among incumbent firms (Kassem, 2024).

Overall, the findings point to an important distinction for infrastructure policy. Improving the electricity grid can substantially reduce the reliability constraint faced by firms, especially those whose production is more dependent on electricity. But removing that constraint does not necessarily produce immediate increases in sales or employment among existing firms. Understanding the broader economic returns to grid investment therefore requires attention not only to whether infrastructure improves service quality, but also to how firms adjust over time and how improved reliability interacts with investment, technology, finance, skills, and other constraints on firm growth.

# 6 Conclusion

This study examines whether improvements in electricity-grid infrastructure translated into better electricity reliability and firm performance in Nepal. We combine firm-level data from the 2009, 2013, and 2023 World Bank Enterprise Surveys with city-level information on grid upgrades and predetermined industry electricity intensity from the 2006/07 Census of Manufacturing Establishments. Our DDD design examines whether firms in more electricity-intensive industries experienced larger changes following grid upgrades.

The results show a clear improvement in electricity reliability. A one-standard-deviation increase in predetermined electricity intensity is associated with an additional 14.1-hour reduction in monthly outage exposure in upgraded cities relative to comparison cities between 2013 and 2023. We also find a significant reduction in the probability of experiencing an outage. These results indicate that grid upgrading was associated with larger reliability improvements among firms whose production was more dependent on electricity.

The evidence for downstream firm performance is more limited. We find no statistically significant effects on real sales, employment, or labor productivity. The TFP estimates are positive across both productivity

measures and statistically significant at the 10 percent level under Webb wild-cluster bootstrap inference, providing suggestive evidence of productivity improvement. The pre-treatment placebo estimates show no statistically significant differential changes during 2009–2013, and the magnitude of the main outage-exposure estimate is generally stable across alternative treatment definitions and sample restrictions.

These findings highlight an important distinction for electricity infrastructure policy. Improving grid infrastructure can substantially improve the quality and reliability of electricity available to already-connected firms, particularly those that depend more heavily on electricity. However, better electricity reliability does not necessarily translate into immediate increases in sales or employment. Removing an important infrastructure constraint may be an important first step, while broader firm responses can depend on how firms adjust over time and on other constraints they face.

For developing economies moving beyond basic electricity access, the findings suggest that infrastructure policy should pay attention not only to expanding connections and generation capacity but also to the reliability of electricity delivered to firms. More frequent firm-level data and longer-term evidence would help determine whether improvements in reliability eventually translate into investment, firm expansion, employment growth, and broader productivity gains.

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# A Additional Figures and Tables

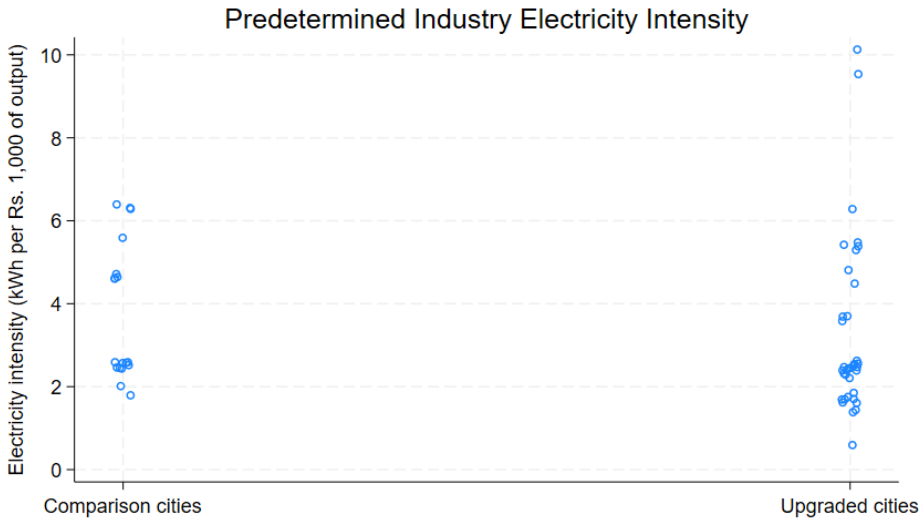



Figure A1: Predetermined industry electricity intensity by grid-upgrade status.

The figure shows predetermined industry-level electricity intensity for the 55 firms in the 2013 baseline sample used in the 2013–2023 DDD analysis. Electricity intensity is constructed from Nepal's 2006/07 Census of Manufacturing Establishments and measured as kWh per Rs. 1,000 of industry output. The sample includes 18 firms in comparison cities and 37 firms in upgraded cities.

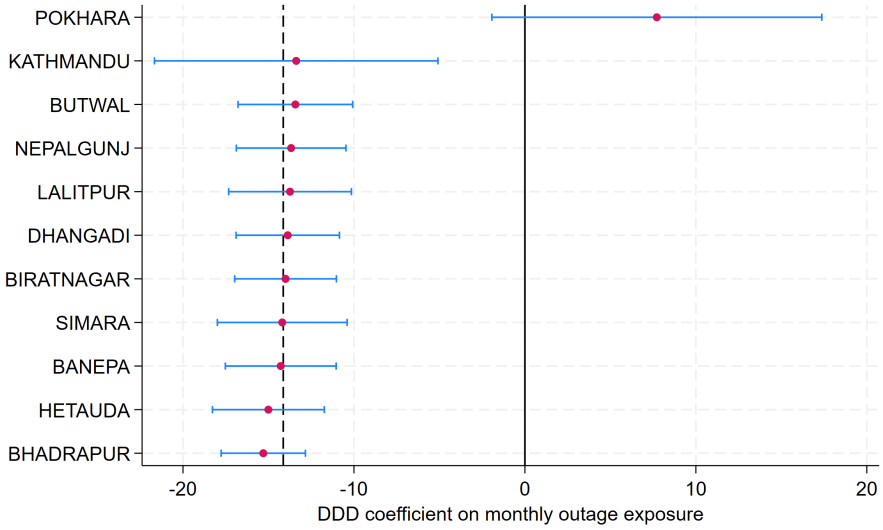



Figure A2: Leave-one-city-out DDD estimates for monthly outage exposure

The figure reports leave-one-city-out estimates of the DDD coefficient for monthly outage exposure. Each estimate is obtained by excluding the indicated city and re-estimating the baseline specification. The dashed

vertical line shows the baseline estimate using all 11 cities −14.135, while the solid vertical line indicates zero. Error bars show 95% confidence intervals based on city-clustered standard errors and are presented for descriptive purposes. Formal inference in the main analysis is based on Webb wild-cluster bootstrap p-values. Pokhara contains 13 of the 18 comparison firms; excluding Pokhara therefore leaves Bhadrapur, with five firms, as the only comparison city.

Table A1: City-Level Grid Upgrade Evidence and Treatment Classification

| Baseline city | Treatment status | Grid/substation | Documented infrastructure change | Timing | Source |
|---|---|---|---|---|---|
| Banepa | Treated | Banepa substation | Transformer capacity upgraded to $2 \times 22.5$ MVA. | 2017 | Nepal Electricity Authority (NEA), Transmission Directorate/Project Management Directorate, FY 2020/21, p.117 |
| Bhadrapur | Control | Chandragadhi/Bhadrapur supply system | Electricity-system improvements are documented during the study period, but these are not classified as a qualifying city-level grid-capacity treatment under the study definition. | 2013–2023 | NEA Distribution and Consumer Services Directorate yearbooks and annual reports. |
| Biratnagar | Treated | Duhabi–Biratnagar grid system | Additional 63 MVA transformer bay at Duhabi, which supplies the Biratnagar area. | 2017 | NEA Annual Report/transmission records, FY 2015/16, p.37. |
| Butwal | Treated | Butwal substation | New 16.6 MVA transformer bay, increasing 33/11 kV transformation capacity. | Post-2013 | NEA Grid Development Directorate, Grid Year Book FY 2018/19, p.85. |
| Dhangadhi | Treated | Dhangadhi 33/11 kV substation | Substation capacity reinforcement involving a 20/24 MVA transformer. | 2020/21 | NEA Annual Report FY 2018/19 Table 7. |
| Hetauda | Treated | Hetauda substation | Existing lower-capacity transformers replaced by $2 \times 45$ MVA transformers, substantially increasing transformation capacity. | 2015 | NEA Annual Report FY 2014/15; NEA Transmission Year Book FY 2022/23, p.99. |

Table A1: City-Level Grid Upgrade Evidence and Treatment Classification (continued)

| Baseline city | Treatment status | Grid/substation | Documented infrastructure change | Timing | Source |
|---|---|---|---|---|---|
| Kathmandu | Treated | Chapali 132 kV substation | New 132/11 kV, 30 MVA substation serving northern Kathmandu and intended to increase capacity and improve reliability. | 2016/17 | NEA Annual Report FY 2016/17, project discussion and Appendix: p.46 for project discussion. |
| Lalitpur | Treated | Patan 66/11 kV substation | New 18 MVA transformer bay added. | 2014 | NEA transmission/project Directorate FY 2020/21, p.112. |
| Nepalgunj | Treated | Nepalgunj Old substation | Capacity upgrading and rehabilitation of the Nepalgunj supply system completed. | 2018/19 | NEA Distribution and Consumer Services Directorate, FY 2018/19, p.36. |
| Pokhara | Control | Pokhara grid/substation | Replacement and network-reliability improvements occurred, but no qualifying major transformation-capacity addition meeting the study treatment definition was identified as operational before the 2023 survey; the major new Birauta project was not operational during the treatment window. | 2013–2023 | NEA annual reports and transmission/project records. |
| Simara | Treated | Simara 66/11 kV substation | Existing lower-capacity transformers replaced with $2 \times 15$ MVA transformers, increasing installed transformation capacity to 30 MVA. | 2016 | NEA Annual Report FY 2015/16 and subsequent transmission records. |

*Notes:* Treatment status is assigned using the firm's predetermined baseline city and documented grid infrastructure changes between the 2013 and 2023 Enterprise Survey waves. A city is classified as treated when NEA records document a substantial grid or substation transformation-capacity expansion that is sufficiently linked to electricity supply in the firm's baseline location and became operational during the study period. Routine maintenance, replacement without a qualifying capacity expansion, distribution-level improvements, line rehabilitation, capacitor installation, and projects that were planned or under construction but not operational during the treatment window are not classified as treatment. Bhadrapur and Pokhara serve as control cities under this definition. Sources are Nepal Electricity Authority annual reports, Distribution and Consumer Services Directorate yearbooks, Grid Development Directorate yearbooks, and transmission/project records.

Table A2: Composition of the Final Estimation Sample

| Baseline city | Treatment status | Firms | Firm-year observations |
|---|---|---|---|
| Banepa | Treated | 1 | 2 |
| Bhadrapur | Comparison | 5 | 10 |
| Biratnagar | Treated | 1 | 2 |
| Butwal | Treated | 7 | 14 |
| Dhangadhi | Treated | 1 | 2 |
| Hetauda | Treated | 1 | 2 |
| Kathmandu | Treated | 15 | 30 |
| Lalitpur | Treated | 2 | 4 |
| Nepalgunj | Treated | 4 | 8 |
| Pokhara | Comparison | 13 | 26 |
| Simara | Treated | 5 | 10 |
| Treated total | | 37 | 74 |
| Comparison total | | 18 | 36 |
| **Total** | | **55** | **110** |

*Notes:* The table reports the composition of the final 2013–2023 estimation sample by firms' predetermined baseline city. Treatment status is based on documented qualifying grid infrastructure upgrades during the study period. Bhadrapur and Pokhara constitute the comparison group. Each firm is observed in both 2013 and 2023, resulting in two firm-year observations per firm.

Table A3: Predetermined Industry Electricity Intensity in the Estimation Sample

| ISIC | Manufacturing industry | Electricity intensity | Standardized intensity | Firms |
|---|---|---|---|---|
| 21 | Paper and paper products | 10.128 | 2.768 | 1 |
| 17 | Textiles | 9.562 | 2.529 | 1 |
| 26 | Other non-metallic mineral products | 6.295 | 1.151 | 2 |
| 25 | Rubber and plastic products | 5.571 | 0.846 | 6 |
| 20 | Wood and wood products | 4.613 | 0.442 | 4 |
| 36 | Furniture; manufacturing n.e.c. | 3.328 | -0.100 | 4 |
| 29 | Machinery and equipment n.e.c. | 3.213 | -0.148 | 4 |
| 31 | Electrical machinery and apparatus n.e.c. | 2.592 | -0.410 | 2 |
| 15 | Food products and beverages | 2.569 | -0.420 | 12 |
| 27 | Basic metals | 2.529 | -0.437 | 9 |
| 28 | Fabricated metal products | 2.393 | -0.494 | 2 |
| 24 | Chemicals and chemical products | 1.714 | -0.780 | 5 |
| 22 | Publishing, printing and recorded media | 1.631 | -0.815 | 2 |
| 16 | Tobacco products | 0.492 | -1.296 | 1 |
| **Total** | | | | **55** |

*Notes:* Electricity intensity is constructed from the 2006/07 Nepal Census of Manufacturing Establishments (CME) and is measured as purchased electricity quantity (kWh) per Rs. '000 of total output. Electricity use and output are first aggregated to the 2-digit industry level before calculating the ratio. The measure therefore predates the 2013–2023 grid-upgrade period. Standardized electricity intensity is the continuous measure used in the DDD specifications. The final column reports the number of firms from each industry in the 2013–2023 estimation sample. Industries are ordered from highest to lowest electricity intensity.

Table A4: Production-Function Estimates Used to Construct TFP

| | (1) Cobb–Douglas | (2) Wooldridge |
|---|---|---|
| Log permanent full-time employment | 0.263*** | 0.270*** |
| | (0.030) | (0.056) |
| Log real machinery capital | 0.054*** | 0.034* |
| | (0.018) | (0.020) |
| Log real materials and intermediate inputs | 0.760*** | 0.728*** |
| | (0.023) | (0.027) |
| Year fixed effects | Yes | – |
| Two-digit industry fixed effects | Yes | – |
| Observations used in estimation | 548 | 171 |
| Firms in underlying TFP sample | 373 | 373 |
| $R^2$ | 0.960 | – |
| Sum of input coefficients | 1.077 | 1.033 |
| CRS test $p$-value | 0.0001 | 0.460 |

*Notes:* The dependent variable is log real annual sales. Column (1) reports estimates from the non-dynamic Cobb–Douglas production function with survey-year and two-digit industry fixed effects. Standard errors clustered at the firm level are reported in parentheses. Column (2) reports estimates from the Wooldridge (2009) proxy-variable estimator, with real machinery capital as the state variable and real materials and intermediate inputs as the proxy variable. Monetary variables are expressed in constant 2011 Nepalese rupees using Nepal's GDP deflator from the World Development Indicators. The CRS test evaluates the null hypothesis that the labor, capital, and materials coefficients sum to one. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A5: Construction of the 2013–2023 DDD Estimation Sample

| Sample restriction | Firms remaining | Firms excluded at step |
|---|---|---|
| Firms observed in the 2013 WBES | 482 | – |
| Also observed in the 2023 WBES | 202 | 280 |
| Have predetermined city information from the 2009 survey | 119 | 83 |
| Matched to predetermined industry electricity intensity | 55 | 64 |
| Upgraded cities | 37 | |
| Comparison cities | 18 | |

*Notes:* The table shows the construction of the balanced 2013–2023 DDD estimation sample. Starting from the 482 firms observed in the 2013 World Bank Enterprise Survey, we retain firms that are also observed in 2023. Treatment status is assigned using predetermined city information from the 2009 survey. Predetermined industry electricity intensity is constructed from the 2006/07 Census of Manufacturing Establishments (CME). The final sample contains 55 firms observed in both 2013 and 2023, yielding 110 firm-year observations. Of these, 37 firms are located in upgraded cities and 18 in comparison cities.

Table A6: Baseline Characteristics of Firms Included and Not Included in the Final DDD Sample

| Baseline characteristic | Other matched firms | Final DDD sample | Difference | $p$-value |
|---|---|---|---|---|
| Monthly outage exposure (hours) | 18.401 | 21.255 | 2.853 | 0.678 |
| Log real sales | 16.710 | 17.591 | 0.880 | 0.017 |
| Employment | 67.891 | 83.655 | 15.763 | 0.573 |
| Log labor productivity | 13.844 | 13.757 | -0.087 | 0.701 |
| Manager experience (years) | 15.912 | 17.291 | 1.379 | 0.355 |
| Exporter | 0.102 | 0.127 | 0.025 | 0.611 |
| Number of firms | 147 | 55 | | |

*Notes:* The table compares baseline characteristics of firms in the final DDD sample with those of other firms observed in both the 2013 and 2023 World Bank Enterprise Surveys. All characteristics are measured in 2013. The final DDD sample consists of firms with predetermined city information from the 2009 survey and industry electricity-intensity information required for the DDD analysis. Differences are calculated as the mean for the final DDD sample minus the mean for other matched firms. The reported $p$-values are from two-sample tests of differences in means. Sales and labor productivity are available for 143 firms in the other matched group and 54 firms in the final DDD sample; the remaining variables are available for all 147 and 55 firms, respectively.

Table A7: Sensitivity of Monthly Outage Exposure Estimates to Alternative Controls

| Specification | DDD coefficient | Clustered SE | Cluster $p$ | Webb $p$ |
|---|---|---|---|---|
| Baseline: manager experience + exporter | −14.135 | (1.510) | $< 0.001$ | 0.007 |
| No firm-level controls | −13.805 | (2.724) | $< 0.001$ | 0.463 |
| Manager experience only | −12.379 | (1.963) | $< 0.001$ | 0.005 |
| Exporter only | −15.565 | (2.704) | $< 0.001$ | 0.243 |
| Baseline employment × post | −14.141 | (3.027) | 0.001 | 0.487 |
| Baseline firm age × post | −13.006 | (2.201) | $< 0.001$ | 0.326 |
| Baseline foreign ownership × post | −14.462 | (2.592) | $< 0.001$ | 0.356 |
| Observations | 110 | | | |
| City clusters | 11 | | | |

*Notes:* The table reports the coefficient on the triple interaction between the 2023 indicator, grid-upgrade status, and predetermined industry electricity intensity. The dependent variable is monthly outage exposure, measured in hours. All specifications include firm and year fixed effects. Baseline employment, firm age, and foreign ownership are measured in 2013 and interacted with the post-period indicator. Standard errors clustered by baseline city are reported in the table. Cluster $p$-values are based on conventional city-clustered inference. Webb wild-cluster bootstrap $p$-values are based on 99,999 replications with Webb weights. The baseline specification includes manager experience and exporter status.