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Coal-fired power plants and industrial development

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Abstract

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Past periods of industrial development have gone hand in hand with the burning of coal, but there is little evidence on the effects of coal infrastructure on manufacturing growth in today's industrializing economies. We quantify the direct and indirect effects of coal-fired power plant commissioning on local incumbent manufacturing firms in Indonesia during a coal phase-in period between 1984 and 2015. We analyze spatially and temporally explicit manufacturing and power plant data in a stacked difference-in-difference framework. Leveraging quasi-random variation in treatment timing, we show that coal-fired power plants have led incumbent larger firms to increase employment, inputs, and outputs. In contrast, smaller firms remained unaffected. We identify mediating channels including improved electricity supply and transportation infrastructure, and increased competition for labor. Ongoing efforts to reduce global coal capacity need to take such effects into account.

Keywords: Coal, Manufacturing, Industrialization, Indonesia, Difference-in-difference

JEL Codes: C55, L60, O12, O14, O53, Q40, R11

1 Introduction

Global investment in coal-fired power plant capacity continues. Approximately 27% of the coal-fired power capacity currently in operation, or more than 578 GW, is scheduled to start operation over the next decade (Global Energy Monitor et al. 2024). Most of this capacity will be located in today’s industrializing economies where it will help meet growing electricity demand¹. Given the required global phase-out of coal to meet international climate targets (Tong et al. 2019) and the steadily decreasing cost of low-emission energy conversion technologies (IPCC 2023), such continued investment in coal is puzzling.

One reason for such investment is that many governments in industrializing economies see coal-fired power plants and fossil-fueled electricity generation as the backbone for accelerating structural change and fostering industrial development processes². In turn, structural change and industrial development could lead to higher wages, infrastructure investment, and, consequently, economic growth and poverty eradication (McMillan and Rodrik 2011; Herrendorf et al. 2014). Indeed, economic history documents the important role of coal in industrial development in the U.S. (Wright 1990) and Europe (Hunt 1986; Fouquet and Pearson 1998; Wrigley 2013; Fouquet 2016; Fernihough and O’Rourke 2021): The operation of coal-fired power plants was beneficial to local manufacturing firms, e.g., by providing a reliable supply of electricity or by facilitating the development of transportation infrastructure such as roads, railways, and ports, which reduced transportation costs for local firms.

If the commissioning of new coal-fired power plants accelerates or even drives industrial development, the phasing out of coal capacity without adequate replacement could have a negative impact on the economic prospects of industrializing countries. However, there is little evidence on the prevalence and magnitude of direct or indirect effects of energy infrastructure investments (in general) and coal-fired power plants (in particular) on local manufacturing firms, especially in less mature stages of industrial development.

In this study, we examine the direct and indirect effects of coal-fired power plant commissioning on local and incumbent manufacturing firms in Indonesia between 1984 and 2015, a period of sustained coal expansion. Leveraging spatially and temporally explicit power plant and manufacturing firm data for more than 60,000 firms, we build on a stacked difference-in-difference (DID) design to address the identification challenge of endogenous

¹Analyses using data from the Global Energy Monitor (2024) show that most of the world’s coal-fired power capacity currently under construction, announced, or approved is being built in China (70%), India (13%), Indonesia (3%), and other low- and-middle-income countries (13%) such as Bangladesh, Vietnam, and Laos. Of all coal-fired power plants commissioned between 2018 and 2024, 65% were built in China, 9% in India, 6% in Indonesia, and 16% in other low- and-middle-income countries.

²For example, the Indian premier Modi said that “the consequences of the industrial age powered by fossil fuel are evident, especially on the lives of the poor. ... [T]he world’s billions at the bottom of the development ladder are seeking space to grow. ... [C]limate justice demands that, with the little carbon space we still have, developing countries should have enough scope to grow” (Government of India 2015).

power plant placement. The granularity of the available census data allows us to derive average effects on key indicators of firm performance, such as total employment, material inputs, outputs, and value added, as well as to uncover potential mediating mechanisms, such as improved electricity supply and transportation infrastructure, agglomeration effects, and spillovers to productivity.

We find that the commissioning of coal-fired power plants caused performance growth in larger, more capital-intensive firms. In the four years following the commissioning of coal-fired power plants, larger (medium-size) firms increased material inputs by 9.7% (9.6%), outputs by 10.6% (6.4%), and employment by 7.6% (3.5%). In contrast, we document a decrease in employment of 2.1% as well as small and insignificant effects on outputs and inputs in smaller firms with up to 100 employees. Analyzing firms in different industries, we document positive effects for firms in more capital-intensive industries, such as those producing basic metals, metal products, or chemical products. The effects are small and insignificant or even negative for firms in more labor-intensive industries such as food, textiles, wood, or paper.

Our analyses of the mediating mechanisms suggest that coal-fired power plants may have benefited incumbent manufacturing firms by improving the reliability of electricity transmission, leading to increased spending on electricity and reduced spending on generator fuel. We also document that firms increased their demand for transportation, as measured by the total shipping weight of inputs and outputs, but it is difficult to infer increased investment in local transportation infrastructure. We show, however, that coal-fired power plant commissioning has led to an increase in the number of new firms and an increase in the wages of both production and non-production workers. The resulting increase in competition for labor affects firms differently, with smaller firms reducing employment. Changes in productivity measures are also heterogeneous across firms of different sizes, but remain comparatively small and statistically insignificant.

Interpreting our estimates as causal requires the identification of appropriate counterfactual outcomes for treated firms, i.e., firms exposed to new power plants. In the context of coal-fired power plants, this is structurally difficult because the spatial distribution of such power plants is likely to be endogenous to local circumstances. We address such treatment endogeneity concerns by leveraging quasi-random variation in the treatment *timing* of power plant commissioning. In addition, the staggered roll-out of power plants between 1984 and 2015 helps to circumvent omitted variable bias from confounding but unobserved local shocks. We also use information on power plants that were announced but never built to address regional differences in endowments, infrastructure, or expectations that may affect both power plant allocation and firm performance.

Our research design, coupled with high-resolution panel data, allows us to test the robustness of our results: For example, we vary the treatment definition (by changing the inclusion criteria for manufacturing firms, such as distance to power plants), adjust

the cohort-specific control groups (by excluding never-, prior-, or later-treated units), or control for different circumstances at the local or regional level. Our complementary analyses confirm our main results of locally bounded and heterogeneous causal effects among heterogeneous firms.

We proceed as follows. In Section 2, we describe various mediating channels through which coal-fired power plant operations might affect the performance of local manufacturing firms. Then, in Section 3, we present our empirical strategy and our approach to addressing the challenges of identification. In Section 4, we present data on manufacturing firms and power plants. In Section 5, we present results from our investigation of the causal effects of coal-fired power plant commissioning on local manufacturing firms. In Section 6, we discuss possible mediating mechanisms that might help interpret our findings, before concluding in Section 7.

2 Literature: Coal-fired power plant commissioning and effects on local manufacturing firms

In this section, we identify possible mediating mechanisms between the commissioning of coal-fired power plants and the performance of local manufacturing firms. Increasing firm performance, as described by increasing employment, production, or productivity, is of interest to this study because it contributes to overall manufacturing growth, industrial development, and thus structural change.

Improvement of electricity supply Coal-fired power plants convert thermal energy from lignite or hard coal into electricity, which is an important input for many manufacturing firms. The arrival of a new power plant could affect local industries by providing electricity that may be cheaper or more reliable. The supply of cheaper electricity can facilitate a more productive use of inputs, leading to growth in output, productivity, and employment if electricity and labor are complements. Such effects have been observed in both industrialized (Fiszbein et al. 2020) and industrializing economies (Rud 2012; Lipscomb et al. 2013; Allcott et al. 2016; Abeberese 2017; Meeks et al. 2021). For Indonesia, Kassem (2024) shows that electricity grid expansion increased local firm turnover because enhanced electricity supply lowered entry barriers for new firms, crowding out less productive firms in the medium term. Similarly, improved access to electricity can increase manufacturing employment by expanding the local labor supply if available electricity reduces household demand for alternative fuel collection (Dinkelman 2011).

Beyond such improvements in electricity supply at the extensive margin, new power plants can lead to changes at the intensive margin, i.e., by increasing the reliability or quality of electricity transmission. Unreliable electricity supply is often detrimental to the

manufacturing sector, especially in the context of industrializing economies³ (Fried and Lagakos (2023), see also Cole et al. (2018)): Firms may respond to frequent power shortages by reducing inputs, resulting in lost revenue (Allcott et al. 2016), or by outsourcing production (Fisher-Vanden et al. 2015). An alternative option is to hedge against power
 135 outages through self-generation with more costly, fuel-based generators (Alby et al. 2013; Fried and Lagakos 2023). Although electricity supply naturally requires electricity *grids*, the commissioning of power plants is likely to lead to more reliable electricity supply closer to those plants due to the physical properties of electricity transmission. Such effects are more likely to occur in environments where the grid infrastructure is fragile.

140 If the supply of cheap and reliable electricity has been a constraint on production, the commissioning of new power plants with large generating capacity should increase the inputs, outputs, and productivity levels of firms, for example by reducing costly self-generation of electricity or by providing incentives to invest in productive capital. Employment effects are theoretically ambiguous and depend on whether capital and labor
 145 are complements or substitutes. In labor-intensive industries, enhanced electricity supply may be labor-saving, while in capital-intensive industries it may increase the demand for labor to take advantage of increasing machine productivity.

Improvement of transportation infrastructure Another channel through which power plants can affect local firms is through ancillary developments in local transporta-
 150 tion infrastructure. Coal-fired power plants often require improved roads, ports, or railways to deliver coal, which could also facilitate shipping for local manufacturing firms, thereby reducing their transportation costs. Road improvements can also expand labor markets and provide access to locally traded goods and services, especially in remote areas where transportation networks are sparse. Research documents such locally bounded
 155 effects of transportation infrastructure roll-out for several regions, including India (Datta 2012; Ghani et al. 2016; Donaldson 2018; Asher and Novosad 2020), China (Faber 2014; Banerjee et al. 2020), Sub-Sahara Africa (Storeygard 2016), and Peru (Volpe Martincus et al. 2017)⁴. In Indonesia, improved road conditions have helped manufacturing firms create new jobs, enabled workers to move out of informal employment, and increased
 160 household income from labor (Gertler et al. 2022). The commissioning of new coal-fired power plants that are not located in close proximity to coal mines may thus go hand in hand with locally concentrated improvements in transport conditions and subsequent positive effects on nearby manufacturing firms.

³In 2009 (2015), 34% (32%) of 1,176 (1,069) Indonesian manufacturing firms reported that electricity supply was a *moderate to very severe* obstacle to current operations. 9.2% (2.9%) of the firms surveyed cited electricity as their biggest obstacle in general. 52% (26%) of firms experienced electricity outages (World Bank 2009, 2015).

⁴For a literature review, see also Redding and Turner (2015).

Industrial agglomeration, productivity, and wages

Coal-fired power plants can have (indirect) benefits for local manufacturing by inducing industrial agglomeration (Rosenthal and Strange 2004; Ellison et al. 2010). Greenstone et al. (2010) demonstrate the spillover effects of so-called 'million dollar plants' on incumbent manufacturing firms: Large and locally concentrated investments have led to substantial productivity increases at nearby and incumbent firms by stimulating technological spillovers or by changing input prices of locally traded goods. Such price changes can lead to productivity gains if firms adjust their production inputs in response. Similar effects have been documented for early hydropower dams (Severnini 2022) and rural development programs (Kline and Moretti 2014).

In addition, industrial agglomeration involves the establishment of new firms. The resulting competition for labor can lead to rising wages and subsequent reallocation of labor across firms, unless there is sufficient in-migration of workers. In contrast, the operation of coal-fired power plants causes local externalities such as air pollution (Currie et al. 2015; Clay et al. 2022), which could potentially lead to out-migration and thus reduce the local labor supply. The impact of power plant commissioning on local wages is therefore a priori ambiguous.

In essence, coal-fired power plants can affect the performance of local and incumbent firms in the short to medium term by improving electricity supply, developing local transportation infrastructure, or inducing industrial agglomeration and thus changes in labor markets. Moreover, such effects are likely to be locally bounded, especially in settings where improved grid and transportation infrastructure is lacking. This is particularly true in the context of many industrializing economies, where most of the coal-fired power plants currently under construction are expected to operate. Following findings from the literature, we therefore hypothesize that the operation of coal-fired power plants has contributed to industrial development by positively affecting the performance of local and incumbent manufacturing firms.

3 Empirical strategy: Stacked difference-in-difference design with heterogeneous treatment effects

We estimate the direct and indirect effects of coal-fired power plant commissioning on the performance of local manufacturing firms in the early stages of industrial development. We exploit spatial and temporal variation in firm performance during periods of increasing coal infrastructure investment in Indonesia between 1984 and 2015. Thanks to various identifying assumptions, we can assume that time-variant performance shocks in firms located near coal-fired power plants are a credible estimate of causal effects.

3.1 Model: Main specifications

A conventional approach to disentangling causal effects of power plant commissioning is a difference-in-difference setup. Comparing outcomes between units that are subject to treatment and those that are not, i.e., the control group, before and after treatment time t yields a treatment effect that withstands causal interpretation if control group units represent a credible counterfactual for the treated units in the absence of the treatment.

In the context of this study, however, such a conventional DID setup may be subject to several biases. First, the coal-fired power plants in Indonesia were commissioned in several consecutive years, i.e., staggered. In this case, estimates from traditional DID may be misleading because the estimator would be comparing units that are treated at later time points to units that were treated at earlier time points. This could be a violation of the parallel trends assumption if the treatment causes prior-treated units to follow a different trend in the post-treatment period (Goodman-Bacon 2021). Second, but related, some units observe multiple treatments at different time points, which could bias our estimates, depending on the mechanism by which power plants affect local firm performance. Third, prior-treated units may differ from later-treated units because power plants came online during a period of rapid industrial development and efforts to increase electrification (see also Kassem (2024)). This relates to – fourth – a plausibly non-exogenous treatment, i.e., decisions about coal-fired power plant allocation are endogenous to local circumstances, such as expectations about future electricity demand. This could lead to selection bias if power plants were selected in areas where firms were expected to grow exceptionally.

To address such concerns, we propose a stacked difference-in-difference design to estimate the causal effect of coal-fired power plants on incumbent and local manufacturing firms. Recently, stacked difference-in-difference approaches have become increasingly popular (Cengiz et al. 2019; Deshpande and Li 2019; Fadlon and Nielsen 2019; Baker et al. 2022; Kraus et al. 2023) because they provide researchers with the flexibility to make reasonable comparisons between what was observed (the factual) and what could have been observed in the absence of the treatment (the counterfactual). When units cannot be considered for a reasonable counterfactual, for example because they were never treated, were treated in the past, or were exceptionally different from the treatment group ex ante, researchers can exclude such units from the control group by design. Our stacked difference-in-difference design thus helps to reduce biases that may be present in the context of staggered, possibly non-exogenous, placement of coal-fired power plants.

We begin by defining the commissioning of any coal-fired power plant in year t in village d as our treatment event θ . We choose commissioning, i.e., coming online, rather than the start of construction because we are unable to observe construction start dates in our data⁵. For each year t with any treatment event, we create a unique subsample of

⁵Pre-treatment differences between the treatment and control groups, if present, could partially ac-

firms, which we call cohort c_t . Each cohort c_t includes all firms i that are located in village d_i and observe the treatment in the respective cohort year t within distance $A_{d_i,d} \leq a$ ⁶, thus forming the treatment group of cohort c_t . If treatment occurs in consecutive years, differences between the treatment and control groups may capture both the treatment effect of each cohort year and the dynamic effect of prior treatment events. We therefore
 240 exclude units from the treatment group that were treated in the preceding three years⁷. All firms that are located in any other village d_i with distance $A_{d_i,d} > a$ in the respective cohort year are eligible for inclusion in the control group of cohort c_t .

For our main specification, we impose several restrictions for each firm i in each
 245 cohort c_t that we relax later for robustness checks. In each cohort, we include observations from four years before and after each cohort's treatment year (*event window*), because new coal-fired power plants are most likely to affect local firm performance in the first years after treatment. Including observations from the pre-treatment period serves the purpose of examining differences between the treatment and control groups in the
 250 pre-treatment period. Next, we remove from the sample firm-year observations in each cohort's control group if they were treated three years before or three years after the event window (*exclusion window*), thereby avoiding inappropriate comparisons due to violations of the parallel trends assumption. The flexibility of this approach allows us to vary both event and exclusion windows, as well as different inclusion criteria for control
 255 (e.g., excluding never-treated units or neighboring units) and treatment groups (e.g., excluding prior-treated units). A persistent concern is that regions with coal investment may differ substantially from never-treated villages. In response, we also remove firms from never-treated villages from our control group, taking advantage of the opportunity to credibly address potential selection bias. Since we are interested in the causal effects on
 260 *incumbent* firms, we remove firm-year observations from cohorts if firms lack observations in each cohort's pre-treatment period.

We stack all cohort-level subsamples to construct a large sample in which each observation can be uniquely identified by firm i , year t , and cohort c . Individual firm-year observations are likely to be included in multiple cohorts, but are unique within each
 265 cohort. Formally, we estimate the following specification on our stacked sample:

$$y_{i,j,k,d,t} = \beta_0(\theta_{d,c} * \rho_{c,t}) + \sum_c \beta_1^c * \theta_{d,c} * C_c + \sum_c \sum_\tau \beta_2^{c,\tau} * R_{t,c}^\tau * C_c + \lambda_i + \mu_{j,t} + \nu_{k,t} + \varepsilon_{i,t,c} \quad (1)$$

where $y_{i,j,k,d,t}$ denotes an indicator of firm performance in manufacturing firm i from industry j located in village (*desa*) d on island k in year t . We choose total employment,

count for the effects of construction.

⁶In our main specification we use $a = 50$ km. Later, as a robustness check, we vary a .

⁷This may result in some cohorts with no units in the treatment group. We remove such cohorts.

total material inputs, total outputs, and value added as main outcome variables because they indicate local industrial development at the extensive margin.

We denote the cohort of each observation by c and each (calendar) year by t . τ denotes the relative time to treatment in each cohort with $\tau \in \{x \in \mathbb{N} \mid -4 \leq x \leq 4\}$. $\theta_{d,c}$ is a binary indicator equal to one if village d is part of the treatment group in cohort c . C_c is a binary indicator equal to one if a plant-year observation is part of cohort c . $\varepsilon_{i,t,c}$ is the error term.

$R_{t,c}^\tau$ is a binary indicator equal to one if $\tau = t - c$. The interaction term of $\theta_{d,c}$ and C_c controls for cohort-specific differences between the treatment and control groups, effectively accounting for selection into earlier or later treatment. Likewise, the interaction of $R_{t,c}^\tau$ and C_c accounts for cohort-specific time trends before and after treatment in each cohort c , which is more precise than removing time-variant differences across cohorts by including only $R_{t,c}^\tau$.

The coefficient of interest is β_0 . β_0 captures the difference-in-difference point estimate between the treatment and control groups across all cohorts, averaged over four years after treatment compared to four years before treatment. In most of our specifications, we include a binary variable $z_{\theta,c}$ that removes differences between the treatment and control groups in the year of treatment. This is helpful because we are unable to observe the *exact* timing of coal-fired power plant commissioning in each respective treatment year.

Since β_0 expresses treatment effects across a range of different firms, we next extend our model to detect heterogeneous treatment effects. In essence, we exploit differences across three dimensions (time, treatment, and firm characteristics), which facilitates comparisons across firms that are more likely to form a reasonable counterfactual conditional on pre-treatment characteristics. Equation 1 hence expands to

$$y_{i,j,k,d,t,g} = \sum_g \beta_0^g (\theta_{d,c} * \rho_{c,t} * \phi_c^g) + \eta_{c,d,g} + \rho_{c,t,g}^\tau + \lambda_{i,g} + \mu_{j,t,g} + \nu_{k,t,g} + \varepsilon_{i,t,c} \quad (2)$$

and more specifically to

$$y_{i,j,k,d,t,g} = \sum_g \sum_\tau \beta_0^{g,\tau} (\theta_{d,c} * \rho_{c,t}^\tau * \phi_c^g) + \eta_{c,d,g} + \rho_{c,t,g}^\tau + \lambda_{i,g} + \mu_{j,t,g} + \nu_{k,t,g} + \varepsilon_{i,t,c} \quad (3)$$

where ϕ_c^g indicates whether a firm is eligible for group g in cohort c . In our preferred approach, firms are assigned to a firm group g conditional on the number of employees one year prior to each treatment year ($\tau = -1$) in each cohort c . Specifically, firms are considered *smaller* ($g = 1$) if they employ less than 100 workers one year before the

treatment. Similarly, firms are considered *medium-size* ($g = 2$) if they employ between 100 and 249 workers one year before the treatment, and as *larger* ($g = 3$) if they employ more than 249 workers⁸. The same firm-year observation can appear multiple times in our sample in different groups g , but not in different groups within the same cohort c . Note that we effectively drop all observations from firm i in cohort c if that firm's observation is missing at $\tau_c = -1$.

The cohort-treatment group-firm group-fixed effect $\eta_{c,d,g}$ accounts for non-deviating differences between treated and untreated firms as well as differences between firms from different firm groups g across cohorts. $\rho_{c,t,g}^\tau$ enters the equation as an cohort-event time-firm group-fixed effect to account for differences between firms from different firm groups g relative to treatment timing (τ) across cohorts. $\lambda_{i,g}$ captures time-invariant differences between firms, but differs when firms qualify for different firm groups in different cohorts. $\mu_{j,t,g}$ accounts for dynamic differences between firms from the same firm group and five-digit industries (industry-firm group-year fixed effect), while $\nu_{k,t,g}$ captures time- and firm group-variant differences at the island level (island-firm group-year fixed effect). $\varepsilon_{i,t,c}$ is the error term.

This approach is consistent with estimating Equation 1 separately for each stacked dataset of firm groups, i.e., comparing smaller firms, as identified in the year before each treatment year, to other smaller firms across cohorts, and larger firms to other larger firms across cohorts.

The coefficients of interest are β_0^g from Equation 2 and $\beta_0^{g,\tau}$ from Equation 3. β_0^g expresses differences between manufacturing firms in villages located within distance a of a commissioned coal-fired power plant and those in which no power plant was commissioned three years before or after a treatment year and which meet the criteria for our control group. While β_0^g captures an average treatment effect over four years after the treatment for each group of firms g , $\beta_0^{g,\tau}$ expresses differences relative to the outcomes one year before the treatment, i.e., $\tau = -1$. We examine $\beta_0^{g,\tau}$ to detect dynamic differences between the respective treatment and control groups.

Similarly, we assign firms to groups conditional on their main (two-digit) industry one year before each treatment year to uncover heterogeneous effects across industries. For this endeavor, we modify Equations 2 and 3 by removing $\mu_{j,t,g}$, the industry-firm group-year fixed effect. Instead, we account for firm size one year before the treatment,

⁸In fact, the term ‘small’ often refers to firms with less than 20 employees (Asian Development Bank 2020). Since our sample includes primarily firms with more than 19 employees, we deviate from this terminology for the purposes of this study. Badan Pusat Statistik (BPS-Statistics Indonesia) and the Asian Development Bank classify firms with more than 99 employees as *large*, while we additionally distinguish between *medium-size* firms and *larger* conglomerates (more than 249 employees), which is consistent with the U.S. Bureau of Labor Statistics (1982). Later, we assess the robustness of this classification by grouping firms according to annual gross revenue in $\tau = -1$ (as suggested by the Indonesian government (Republic of Indonesia 2008)) and the number of employees in $\tau = -4$. Note that unlike binning with total employment, using monetary values for a given year for binning can be sensitive to inflation.

330 measured by total employment, and add a firm-size firm-group fixed effect.

3.2 Challenges to identification

Credibly estimating the causal effects of coal-fired power plant commissioning on incumbent manufacturing firms requires acknowledging that our research design relies on several identifying assumptions. Here, we discuss potential pitfalls of our empirical strategy and
335 describe our approaches to addressing them.

An important concern is the endogeneity of the treatment. Unlike the canonical ‘million dollar plant’ setup (Greenstone et al. 2010), it is reasonable to assume that the decision to build a power plant in Indonesia depends on local circumstances, some of which could also alter firm performance, such as expectations about future electricity demand,
340 existing transportation infrastructure, proximity to coal mines, or resource endowments. It is therefore inherently difficult to account for structural differences between those firms that are treated at a given point in time and those that are not.

We propose three different ways to circumvent this challenge. First, we restrict the control group in our sample to those firms that are treated at a given point in time. This
345 allows us to decompose the causal effect even if there were local unobservable factors that affected both firm performance and power plant allocation. The exclusion of never-treated units from our control groups implies that β_0 captures an *average treatment effect on the treated* (ATT) rather than an *average treatment effect* (ATE)⁹.

Second, we leverage variation in the treatment timing of power plant *commissioning*,
350 which is likely exogenous to local firm performance and could therefore help distinguish treatment from anticipation effects (Deshpande and Li 2019; Montrone et al. 2022). Exploratory analyses of detailed, biannually released data on coal-fired power plants in Indonesia confirm that the time between the announcement of coal-fired power plants and the expected commissioning date differs substantially (see Panel A in Figure 1). Moreover,
355 the data reveal that the expected commissioning of coal-fired power plants is often delayed after the initial announcement (see Panel B in Figure 1), possibly because construction and operation require multiple permits and legislative processes with local authorities. In addition, we present evidence that suggests an additional delay in commissioning after the start of construction (see Panel C in Figure 1). Taken together, these arguments sup-
360 port the notion that individual firms are generally unable to predict the exact timing of power plant commissioning. Effects that occur in the post-treatment period are therefore unlikely to be biased by local time-specific but unobservable circumstances, such as tariff reforms, infrastructure development programs, or trade regulations. Our estimates should therefore capture the causal effects of coal-fired power plants that can be attributed to the

⁹Nevertheless, we argue that in most settings, power plant allocation decisions are likely to be driven by local circumstances. Thus, we strongly caution against interpreting our results as evidence that power plant construction is a likely intervention to affect the performance of *any* local firm.

365 *commissioning* of such plants rather than to accompanying changes in local institutions.

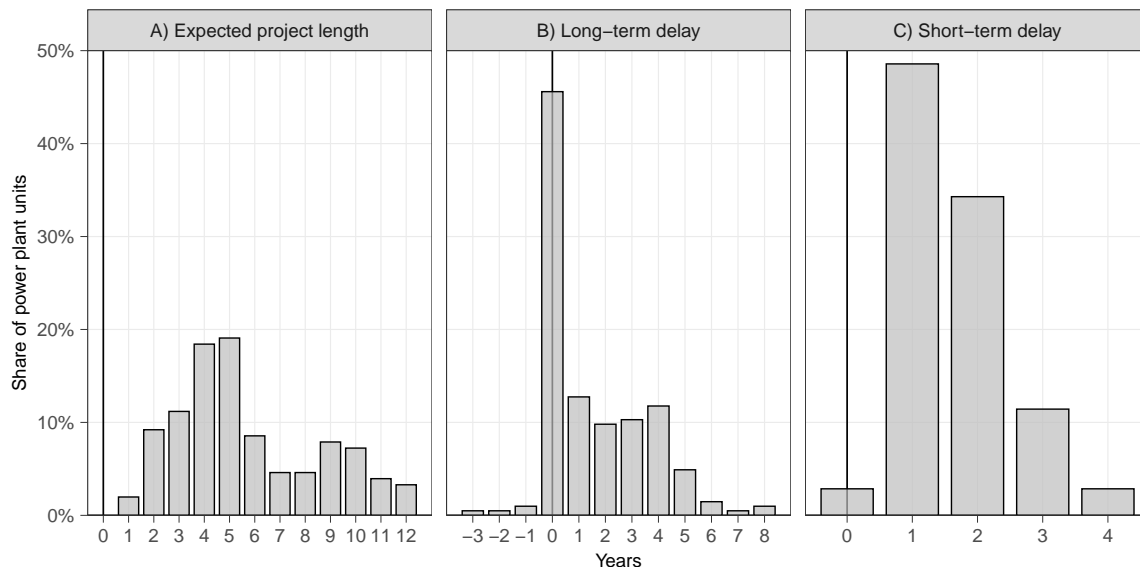


Figure 1: Project duration and delay for Indonesian coal-fired power plants

This figure shows data on coal-fired power plants in Indonesia from Global Energy Monitor (2021), which we collected twice a year between January 2016 and July 2021. Panel A shows the estimated project duration of coal-fired power plant unit construction in the year the unit appeared first in the data. Panel B displays the cumulative long-term delay of coal-fired power plant construction, which is the difference in years of expected commissioning between the year the unit first appeared in the data and the year it last appeared in the data. Panel C shows the short-term delay of coal-fired power plants that were commissioned in 2021 but not in 2016. It displays the difference in years between the expected commissioning date in 2016 and the actual commissioning date. Note the different scales on the x-axes in the different panels.

370 Third, we make use of data on coal-fired power plants that were announced but never commissioned. If there are local unobservable factors that affect both the treatment and the firm's performance, such factors should also be prevalent in the regions where operators eventually decided to operate a power plant. Expanding the control group to include firms from those areas where plants were announced but never actually built provides an opportunity to distill comparisons that reduce any remaining omitted variable bias¹⁰.

375 Furthermore, interpreting our estimates as causal requires that several propositions hold, most notably the parallel trends assumption. In our analysis, we assume that β_0 is the causal effect of power plant commissioning if treated units would have experienced the same growth in employment, inputs, outputs, or value added as control group units, conditional on our set of firm-, industry-year-, and island-year-level fixed effects. This proposition, by definition, cannot be tested because we do not observe the counterfactual performance of treated firms in the data. Instead, examining differences in trends in the *pre-treatment* period can help provide some confidence in our identification strategy.

¹⁰Note that we cannot use information on such announced plants for placebo checks because we structurally lack information on the commissioning year.

In addition, we cannot rule out the existence of confounding factors that coincide with the treatment timing. In response, the stacked difference-in-difference approach allows us to control for selection into earlier or later treatment by controlling for cohort-specific time-variant and -invariant characteristics. We therefore argue that the estimates are likely to be unbiased because the staggered roll-out would require unobservable confounding factors to affect treated firms in *each* cohort in order to bias our results.

We define the treatment as the commissioning of coal-fired power plants, which raises the question of whether the observed effects are unique to coal or rather reflect the effects of electrification. In response, we conduct additional analyses to detect the effects of gas-fired, hydropower, and geothermal power plants. If these are improvements in the local electricity infrastructure rather than fuel-specific effects, we should be able to observe similar effects for technologies other than coal.

Our research design relies on sharp spatial differences. Depending on the channel through which coal might affect firm performance, it is not a priori clear why effects should be locally bounded. We test this proposition, which is important for the interpretation of β_0 , by varying the distance a that defines the inclusion or exclusion of any firm in our treatment and control groups, respectively. In addition, we run our specifications excluding firms from villages in the control group that are neighbors of treated villages, thereby accounting for possible effects between treated and nearby non-treated villages. We also show effects in neighboring villages while excluding treated units.

The stacked difference-in-difference design allows for numerous choices about counterfactual outcomes. We demonstrate the robustness of our results by taking advantage of the ability to adjust the definition of the treatment (by varying a), the definition of cohort-specific control groups (by excluding never-, prior-, or later-treated units), or the inclusion of different sets of fixed effects. The flexibility of the stacked difference-in-difference approach therefore allows us to address several challenges to credible identification.

4 Data: Indonesian manufacturing firms and power plant infrastructure

We link high temporal and spatial resolution manufacturing data with detailed power plant infrastructure data. Our data on firms come from the Indonesian Manufacturing Census and report several performance measures across firms from different industries on an annual basis. Our power plant dataset covers all coal-fired power plants commissioned in Indonesia between 1984 and 2022. In further analyses, we also draw on data for gas-fired, hydropower, and geothermal power plants, as well as on regionally differentiated data on transportation infrastructure and the Indonesian population.

4.1 Firm-level data: The Indonesian Manufacturing Census

We use data from the Indonesian Manufacturing Census (Badan Pusat Statistik - *BPS*)¹¹. The data contain detailed information on annual firm performance, enriched with high-resolution spatial data. We infer the performance of individual firms by observing several
420 plant-level outcomes, such as the number of employees, total material inputs and outputs, productivity measures such as value added, electricity consumption (in kWh and Indonesian rupiah (IDR)), and (generator) fuel consumption (in liters and IDR). Our main outcome variables are comparatively well documented, while other variables, such as productive capital, are missing for some observations. Our total sample includes ob-
425 servations on 60,960 firms between 1980 and 2015. In addition, we have access to precise data on annual firm-level inputs and outputs (in physical units and IDR) between 1998 and 2012, from which we construct the total amount of goods transported (in tons) for each firm and year.

An important feature is that we are able to identify individual firms at the village
430 level. Our sample includes firms from 9,417 villages in 288 districts from 26 provinces on six main islands. 80% of the power plants in our sample are located on the island of Java, which turns out to be the center of Indonesian economic activity. Village and district boundaries have changed over the course of the sample. Therefore, we use crosswalks to identify village coordinates in 2000. Similarly, we transform district-level identifiers
435 (*kabupaten* and *kota*) to their 1993 boundaries, thereby addressing the recurring challenge of district splits in Indonesia (Burgess et al. 2012; Pierskalla 2016). We assume that firms are located at the centroid of each village polygon, which allows us to derive distances a to any other village d in which a power plant is located. Panel (a) in Figure C.1 shows villages that contain at least one manufacturing plant in our sample.

Our dataset includes industry codes at the five-digit level. The definition of industry
440 codes has undergone several changes, but we construct a crosswalk for industry codes at the two-digit level to allow the analysis of heterogeneous effects across heterogeneous industries and the entire sample. Note that our sample of manufacturing firms does not include plants whose main output is electricity, i.e., power plants. However, some
445 firms generate electricity as an input to production using generators, which we include in our analysis by using information on the consumption of generator fuel. Later, we remove from the sample firms that produce *chemical products*, *plastics*, *petroleum*, and *coke*, which may produce coal products.

We perform several cleaning steps on the data, such as removing duplicates that we
450 detect within and across firms (see Allcott et al. 2016; Kraus et al. 2023). Using input-output data, we clean aggregate information about total inputs and outputs. We also

¹¹This data fueled the analyses of a number of other researchers, including Blalock and Gertler (2004), Amiti and Konings (2007), Blalock and Gertler (2008), Bazzi et al. (2017), Pelzl and Poelhekke (2021), Kraus et al. (2023), and Kassem (2024).

remove outliers within and across firms. Our final sample contains 595,580 plant-year observations, from which we construct our cohort-specific treatment and control groups.

For each firm-year observation, we estimate several measures of productivity, such as labor productivity (value added per worker), output per worker, and total factor productivity (TFP). For TFP, we use the production function estimator at the 2-digit industry level presented by Levinsohn and Petrin (2003) with total material input (in IDR) as auxiliary intermediate input¹². An important caveat is that 60% of the firm-year observations in our sample lack some information needed to derive TFP, such as total productive capital, total employment, or firm-level industry. This means that our sample of firms with information on TFP is substantially smaller than our sample with information on labor productivity or output per worker.

Table 1 reports summary statistics for all Indonesian manufacturing firms in our sample in 1990, 2000, and 2010. Supplementary Table B.1 reports summary statistics for manufacturing firms in 1990, 2000, and 2010, differentiated by firm size. Note that the data do not include firms with less than 20 employees, except for earlier census years (1980 to 1989), when on average 10% of the firms in the sample reported having between 10 and 19 employees.

| Variable | 1990 | | | 2000 | | | 2010 | | |
|---------------------------|-------|--------|--------|--------|--------|---------|--------|--------|---------|
| | Mean | Median | SD | Mean | Median | SD | Mean | Median | SD |
| Number of workers | 228 | 64 | 560 | 203 | 48 | 608 | 201 | 49 | 565 |
| Inputs (1M IDR) | 4,488 | 334 | 20,242 | 14,559 | 648 | 64,866 | 43,257 | 2,356 | 169,628 |
| Outputs (1M IDR) | 6,662 | 526 | 33,344 | 23,152 | 1,119 | 101,399 | 69,120 | 4,343 | 249,996 |
| Value added (1M IDR) | 2,190 | 146 | 15,270 | 8,815 | 386 | 49,015 | 26,066 | 1,583 | 104,174 |
| Electricity (MWh) | 652 | 30 | 3,636 | 1,978 | 40 | 19,357 | 705 | 15 | 5,379 |
| Wages (1k IDR) | 889 | 675 | 870 | 3,800 | 3,038 | 4,153 | 13,635 | 10,763 | 15,757 |
| Total factor productivity | 8.84 | 8.65 | 1.42 | 9.23 | 8.96 | 1.61 | 10.37 | 10.16 | 1.67 |
| Labor productivity (log) | 7.77 | 7.58 | 1.22 | 9.03 | 8.86 | 1.32 | 10.37 | 10.23 | 1.38 |

Table 1: Summary statistics for firm performance

This table presents summary statistics for the main outcome variables in our sample of manufacturing plants based on data from the Indonesian Manufacturing Census. We estimate total factor productivity using production function estimators presented by Levinsohn and Petrin (2003) and total material inputs (in IDR) as auxiliary intermediate inputs.

Based on the firm-level data, we also construct a dataset that reports the total number of firms at the village or district level, including the number of firm entries, identified by the first appearance of individual firms in the data. We later use this measure to examine the impact on local firm entry.

¹²We also use different production function estimators, e.g., from Wooldridge (2009). The resulting estimates of TFP are of similar magnitude and lead to similar results.

4.2 Data on power plant infrastructure

We compile a comprehensive dataset of Indonesian power plant commissioning information from a variety of sources.

Coal-fired power plants Our analysis is based on unit-level power plant data from the Global Coal Plant Tracker (Global Energy Monitor 2021). The data include all coal-fired power plant units operating in Indonesia, with the first coming online in 1984. We collapse multiple plant units at the same location that came online in the same year into a single plant-year observation. In addition, the data include information on plants that are under construction or have been announced. Some of the announced power plant projects have been shelved or canceled altogether. Most operating power plants have precise geographic coordinates, which we cross-referenced with open access satellite data. For some of the announced (or canceled) plants and for 16% of the operating plants, information on exact locations is missing because approximate locations are given. We flag these plants and remove them later for robustness checks. The data track the year of commissioning, i.e., the year in which a power plant started operations and went online. In addition, our dataset includes information on the commissioned (or planned) capacity (in MW) and the operating company.

In total, our dataset includes information on 264 plants, of which 76 were commissioned by 2015, the last year for which we have plant-level information. As of early 2023, 72 additional units were commissioned and 32 units were under construction (see also Figure C.2). The vast majority of commissioned plants in our sample are located in Java (48%) and Sumatra (30% – see Panel (b) in Figure C.1), the Indonesian islands where manufacturing is most concentrated. The concentration of coal-fired power plants on Java and Sumatra also helps to address concerns about possible proximity to coal mines, which could be a confounding factor in the case of mine-to-mouth plants, which have become more attractive in recent years (Ministry of Energy and Mineral Resources 2020). Currently, most coal mines are located in Kalimantan (see Panel (e) in Figure C.1, data from the Global Coal Mine Tracker (Global Energy Monitor 2020)), and four out of 76 plants in our sample are located in direct proximity to an operating coal mine.

The first coal-fired power plants were commissioned in 1984, and coal-fired power capacity began to increase in 2004. In the early years of the coal phase-in, it was the state-owned utility Perusahaan Listrik Negara (PLN) that operated the coal-fired power plants as well as the electricity transmission grids. It is worth noting that the Indonesian government has set electricity prices (Burke and Kurniawati 2018) and guaranteed fixed prices to power plants (Chung 2017), implicitly subsidizing electricity generation¹³. Nevertheless, the construction and operation of coal-fired power plants has only recently

¹³The Indonesian government began reforming electricity price subsidies in 2013 (Burke and Kurniawati 2018).

become more attractive to non-state investors. In our final sample, PLN operates 53% of the coal-fired power plants commissioned between 1984 and 2015.

In our main specification, our treatment variable expresses whether a coal-fired power plant was commissioned in a given calendar year t at a given location at a distance a from each village. We compute the distances between each coal-fired power plant with existing coordinates and the centroid of each village d in which at least one plant from our manufacturing plant dataset is located. Therefore, any firm-year observation qualifies as treated if it is within distance a of any coal-fired power plant in year t . We also examine whether power plants were commissioned in neighboring villages, i.e., in villages adjacent to treated villages or in villages within distance a_1 of coal-fired power plants with $a_1 > a$. Table B.2 shows the number of villages within distance $a = 50$ km of coal-fired power plants that were commissioned in each year. It also shows the number of villages with firm-level information for each year and that were treated in the exclusion window and that we therefore remove from both the treatment and control groups. For our main specification, we remove firm-level observations from our sample if they were not treated until 2018, three years after 2015, the last year for which we have firm-level observations.

We also construct a dataset that identifies whether villages are exposed to a nearby power plant that came online after 2015, or that is currently under construction, approved, or in the pre-approval stage. We also include power plant projects that were once announced but ultimately shelved or canceled altogether. We drop observations if we are unable to identify the exact location of the proposed plant site. We later use this dataset, which includes 42 additional projects that were not realized, to help distinguish the effects of power plant *commissioning* from anticipation effects. Since the project developers rated the locations of the plants as suitable for operation, we can confidently assume that we do not increase the bias due to local unobserved factors by including manufacturing plants from such regions in our control group.

Gas-fired, hydropower, and geothermal power plants To identify fuel-specific effects, we compile a dataset of gas-fired, hydropower, and geothermal power plants that were built and operated during the period of interest. Such power plants also provide electricity, but may affect local manufacturing firms differently. For example, gas-fired power plants are more likely to require pipelines for fuel transport rather than roads, which may also be beneficial for manufacturing firms. We include information on hydropower and geothermal power plants because both technologies often cover baseload electricity demand, similar to coal-fired power plants¹⁴. In addition, hydropower is the largest source of renewable energy generation in Indonesia, while Indonesia has access to an estimated 40% of the world's geothermal resources (Nasruddin et al. 2016).

¹⁴We do not include information on alternative power generation technologies, such as wind or solar, because their share of Indonesian power generation is negligible.

We then merge data from the Global Powerplant Database (GPD, Byers et al. 2019) and the World Electric Power Plants Database (WEPP, PLATTS 2017). While data from GPD include information on the exact location of the plants, data from WEPP include exact information on the year of commissioning. For our analysis of the local impacts of non-coal-fired power plants, we include information on plants that were commissioned during our sample period between 1980 and 2015, dropping plants with a generating capacity of less than 10 MW. Our dataset includes 119 gas-fired power plants, 38 hydropower plants, and 19 geothermal power plants. Panel (d) in Figure C.1 shows the location of these plants in our final sample.

4.3 Regional infrastructure and population data

We enrich our dataset, which contains information on local firm performance and power plant operation at the village-year level, with data aggregated to the sub-district (*kecamatan*), district (*kabupaten* and *kota*), and province (*provinsi*) levels. These data include information on local transportation infrastructure at the village level, which we compile from the Village Potential Statistics (PODES) dataset of BPS, and information on local population and employment at the sub-district and district levels, which we compile from the National Socioeconomic Survey (SUSENAS) dataset of BPS and the Indonesia Database for Policy and Economic Research (INDO-DAPOER) dataset of the World Bank, respectively. In addition, we collect information on the reliability of electricity grids, such as the System Average Interruption Duration Index (SAIDI) and the System Average Interruption Frequency Index (SAIFI, see also (Falentina and Resosudarmo 2019)), at the province level from PLN’s annual statistical reports.

5 Results: Effects from coal-fired power plants on local and incumbent manufacturing firms

We show that the commissioning of coal-fired power plants had small and insignificant effects on local firm performance across the sample. We then provide evidence of heterogeneous treatment effects across heterogeneous firms. Our results show that the commissioning of coal-fired power plants has caused positive and significant shocks in labor demand, inputs, outputs, and value added for closely located and incumbent larger firms. In contrast, smaller firms do not appear to be positively affected, if at all, by the commissioning of power plants.

5.1 Effects on the entire sample

The commissioning of coal-fired power plants in Indonesia did not change the performance of incumbent and local firms relative to other firms that did not experience such a shock three years before or after the treatment year. Table 2 provides point estimates from Equation 1 with log-transformed levels of employment, total outputs and inputs to production, and value added (in log IDR) as dependent variable. Average differences between the treatment and control groups over four years post-treatment relative to four years pre-treatment are positive, but small relative to the standard errors and thus statistically insignificant at the 5% level.

| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) |
|----------------------------|------------------|------------------|------------------|--------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST | 0.013 (0.008) | 0.022 (0.017) | 0.018 (0.015) | 0.009 (0.015) |
| <i>Fixed effects</i> | | | | |
| Firm-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes |
| Industry (5-digit)-year-FE | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes |
| Observations | 868,518 | 831,474 | 835,532 | 868,470 |
| R ² | 0.91 | 0.88 | 0.90 | 0.88 |

Table 2: Effects of coal-fired power plant commissioning on employment, outputs, inputs, and value added

This table reports OLS estimates of the impact of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms. The outcome variables are labor demand (1) (in log total employees), total outputs (2), total material inputs (3), and value added (4) (all in log IDR). These are coefficients for β_0 from Equation 1. This table reports estimates and standard errors (in parentheses) clustered at the village level where the treatment is assigned. The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to the treatment or control group for each cohort year. We stack different cohorts with respect to event time. We exclude firms from our sample if they are located in villages that were never treated. All estimations include a set of fixed effects, namely firm FE, event time-cohort FE, treatment group-cohort FE, an industry-year FE at the 5-digit level, an island-year FE, and a binary variable equal to one for treated firms in the treatment year.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

Thus, the results from the full sample do not indicate strong effects of coal-fired power plant commissioning on the performance of local and incumbent manufacturing firms. In other words, our results do not support the notion that local manufacturing firms *as a*

whole flourish in response to the advent of coal-fired power generation. At first glance, this runs counter to conventional wisdom about the mechanisms of industrial development through which cheap and reliable energy supply enabled local manufacturing growth and subsequent industrialization. However, our difference-in-difference estimator expresses average effects across the entire sample and may therefore mask heterogeneous outcomes across heterogeneous firms.

5.2 Heterogeneous effects between firms of different sizes

We provide empirical evidence on the heterogeneous effects of coal-fired power plant commissioning on incumbent and local manufacturing firms in Indonesia. The commissioning of power plants has increased employment, inputs, outputs, and value added in larger firms (those with more than 249 employees in the year before the treatment) and those located within 50 kilometers, compared to other larger firms that observed the commissioning of coal-fired power plants earlier or later, but not three years before or after each treatment year. In contrast, comparing smaller firms to other smaller firms before and after the treatment yields small and negative effects on firm performance.

Table 3 reports pooled estimates for four years after the treatment compared to four years before the treatment, excluding the year of commissioning. Table 4 shows the coefficients from Equation 3, indicating $\beta_{0,\tau}^g$ for a set of firm performance indicators. We visually inspect these estimates using Figure 2, which plots these dynamic estimates for each firm group g in relative time τ to the treatment.

Firms employing between 20 and 99 workers one year before the treatment and located within 50 kilometers of the coal plant experienced a 2.1% reduction in employment, a 2.7% reduction in inputs, a 2.8% reduction in outputs, and a 3.2% reduction in value added in the four years after the commissioning of a coal-fired power plant, compared to manufacturing plants located in villages that experienced such a shock at some point but not within the exclusion window. The pooled regression coefficients for input, output, and value added are insignificant at the 5% level. Thus, it is reasonable to conclude that coal-fired power plants did not affect the performance of local small manufacturing firms and, if anything, reduced the demand for labor.

In contrast, medium-size (100 to 249 employees in the year before the treatment) and larger manufacturing firms (250 or more employees in the year before the treatment) increased employment, inputs, outputs, and value added: In the four years following the commissioning of a coal-fired power plant within 50 kilometers, larger (medium-size) firms increased employment by 7.6% (3.5%), production inputs by 9.7% (9.6%), manufacturing outputs by 10.6% (6.4%), and value added by 10% (2.3%) compared to larger or medium-size firms, respectively, in proximity to such a plant outside the exclusion window.

Examining the differences between the treatment and control groups in the pre-

| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) |
|--|---------------------|---------------------|---------------------|---------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST (Smaller firms) | -0.021** (0.009) | -0.027 (0.022) | -0.028 (0.019) | -0.032* (0.018) |
| Treatment group*POST (Medium-size firms) | 0.035** (0.016) | 0.096*** (0.033) | 0.064** (0.029) | 0.023 (0.031) |
| Treatment group*POST (Larger firms) | 0.076*** (0.017) | 0.097*** (0.031) | 0.106*** (0.027) | 0.100*** (0.029) |
| <i>Fixed effects</i> | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes | Yes |
| Observations | 868,518 | 831,474 | 835,532 | 868,470 |
| R ² | 0.93 | 0.90 | 0.92 | 0.90 |

Table 3: Effects of coal-fired power plant commissioning on employment, outputs, inputs, and value added, differentiated by firm size

This table reports OLS estimates of the effect of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size one year before the treatment. The outcome variables are labor demand (1) (in log total employees), total material inputs (2), total outputs (3), and value added (4) (all in log IDR). These are coefficients for β_0^g from Equation 2. This table reports estimates and standard errors (in parentheses) clustered at the village level where the treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to the treatment or control group for each cohort year. We stack different cohorts with respect to event time. We exclude firms from our sample if they are located in villages that were never treated. We assign firms to firm groups for each cohort according to the number of employees one year before the treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees. Note that we effectively remove firms from cohorts with missing information on employment in the year before the treatment year ($\tau = -1$).

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

625 treatment period ($\tau \in \{x \in \mathbb{N} \mid -4 \leq x \leq -1\}$) shows that such differences are not statistically significant at the 5% level. That is, the treatment and control groups exhibit similar trends in the years prior to treatment. The difference in total employment among larger firms in $\tau = -4$ is a notable exception that merits attention. Although such differences may, to some extent, reflect anticipation effects among larger firms, we observe
630 that pre-treatment differences diminish in subsequent years and increase substantially one

| Sample (Group name) | Smaller firms | | | | Medium-size firms | | | | Larger firms | | | |
|--------------------------------------|----------------------|-------------------|--------------------|--------------------|-------------------|--------------------|--------------------|--------------------|---------------------|---------------------|---------------------|---------------------|
| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) | Labor (5) | Input (6) | Output (7) | Value added (8) | Labor (9) | Input (10) | Output (11) | Value added (12) |
| <i>Variable</i> | | | | | | | | | | | | |
| Treatment group $\times t = -4$ | -0.002 (0.008) | -0.007 (0.026) | -0.004 (0.021) | 0.009 (0.021) | -0.016 (0.017) | -0.023 (0.041) | -0.036 (0.035) | -0.029 (0.037) | -0.040** (0.018) | -0.027 (0.039) | -0.033 (0.031) | -0.022 (0.034) |
| Treatment group $\times t = -3$ | 0.002 (0.007) | -0.010 (0.024) | -0.011 (0.020) | 0.0008 (0.022) | -0.014 (0.015) | -0.037 (0.040) | -0.040 (0.033) | -0.057 (0.036) | -0.026 (0.016) | -0.006 (0.037) | 0.013 (0.031) | 0.044 (0.034) |
| Treatment group $\times t = -2$ | -0.0009 (0.006) | -0.018 (0.021) | -0.012 (0.017) | 0.008 (0.019) | -0.016 (0.013) | -0.018 (0.032) | -0.042 (0.027) | -0.047 (0.031) | -0.018 (0.014) | -0.051 (0.034) | -0.029 (0.028) | -0.013 (0.031) |
| Treatment group $\times t = 0$ | -0.017*** (0.006) | -0.008 (0.019) | -0.023 (0.017) | -0.017 (0.018) | -0.002 (0.013) | 0.077** (0.032) | 0.041 (0.030) | 0.021 (0.034) | 0.005 (0.014) | 0.020 (0.031) | 0.004 (0.025) | 0.013 (0.029) |
| Treatment group $\times t = 1$ | -0.027*** (0.009) | -0.030 (0.025) | -0.032 (0.021) | -0.021 (0.023) | 0.014 (0.017) | 0.094** (0.041) | 0.065* (0.036) | 0.042 (0.042) | 0.059*** (0.017) | 0.089** (0.038) | 0.113*** (0.032) | 0.152*** (0.036) |
| Treatment group $\times t = 2$ | -0.020** (0.010) | -0.043 (0.027) | -0.037* (0.022) | -0.025 (0.024) | 0.036* (0.019) | 0.106** (0.043) | 0.056 (0.035) | 0.021 (0.040) | 0.057*** (0.020) | 0.134*** (0.040) | 0.128*** (0.034) | 0.132*** (0.039) |
| Treatment group $\times t = 3$ | -0.018 (0.011) | -0.028 (0.030) | -0.039 (0.026) | -0.029 (0.026) | 0.022 (0.020) | 0.059 (0.046) | 0.010 (0.039) | -0.067 (0.044) | 0.044** (0.022) | 0.090** (0.045) | 0.100*** (0.038) | 0.075* (0.042) |
| Treatment group $\times t = 4$ | -0.019* (0.011) | -0.040 (0.030) | -0.029 (0.026) | -0.045* (0.027) | 0.025 (0.023) | 0.041 (0.048) | -0.0005 (0.042) | -0.054 (0.049) | 0.064*** (0.024) | -0.036 (0.046) | 0.015 (0.039) | 0.018 (0.042) |
| <i>Fixed effects</i> | | | | | | | | | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 529,073 | 508,238 | 509,926 | 529,083 | 161,569 | 154,592 | 155,220 | 161,571 | 177,876 | 168,644 | 170,386 | 177,816 |
| R ² | 0.72 | 0.83 | 0.86 | 0.84 | 0.58 | 0.83 | 0.85 | 0.81 | 0.77 | 0.81 | 0.84 | 0.80 |

Table 4: Dynamic effects of coal-fired power plant commissioning on employment, outputs, inputs, and value added differentiated by firm size

This table reports dynamic OLS estimates of the effect of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size one year before the treatment. The outcome variables are labor demand (in log total employees) in columns (1), (5), and (9), total material inputs in columns (2), (6), and (10), total outputs in columns (3), (7), and (11), and value added in columns (4), (8), and (12) (all in log IDR). These are coefficients for $\beta_0^{g,\tau}$ from Equation 3. This table reports estimates and standard errors (in parentheses) clustered at the village level where the treatment is assigned. We also show coefficients and 95% confidence intervals in Figure 2.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to the treatment or control group for each cohort year. We stack different cohorts with respect to event time. We exclude firms from our sample if they are located in villages that were never treated. We assign firms to firm groups for each cohort according to the number of employees one year before the treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees. Note that we effectively remove firms from cohorts with missing information on employment in one year before the treatment year ($\tau = -1$).

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm group FE at the 5-digit level, and an island-year-firm group FE. As such, we interlink a standard set of fixed effects with firm group indicators. Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

year after treatment. In addition, we do not observe such differences in $\tau = -4$ for other outcome variables among larger firms, such as inputs, outputs, or value added, for which pre-treatment differences are statistically insignificant at the 5% level.

The absence of pre-treatment dynamics supports the notion that the parallel trends assumption is not violated. Thus, pre-treatment trends did not differ substantially between the treatment and control groups, providing further evidence that our estimates reflect the causal effect of coal-fired power plant commissioning. Our findings document heterogeneous effects that differ by firm size. While medium-size and especially larger firms increased their performance in response to the treatment, we do not observe similar

640 increases for smaller firms. For medium-size and larger firms, the effects on inputs, outputs, and value added in the post-treatment period become smaller three years after the treatment, but the effects for labor and for smaller firms remain stable across all outcome variables.

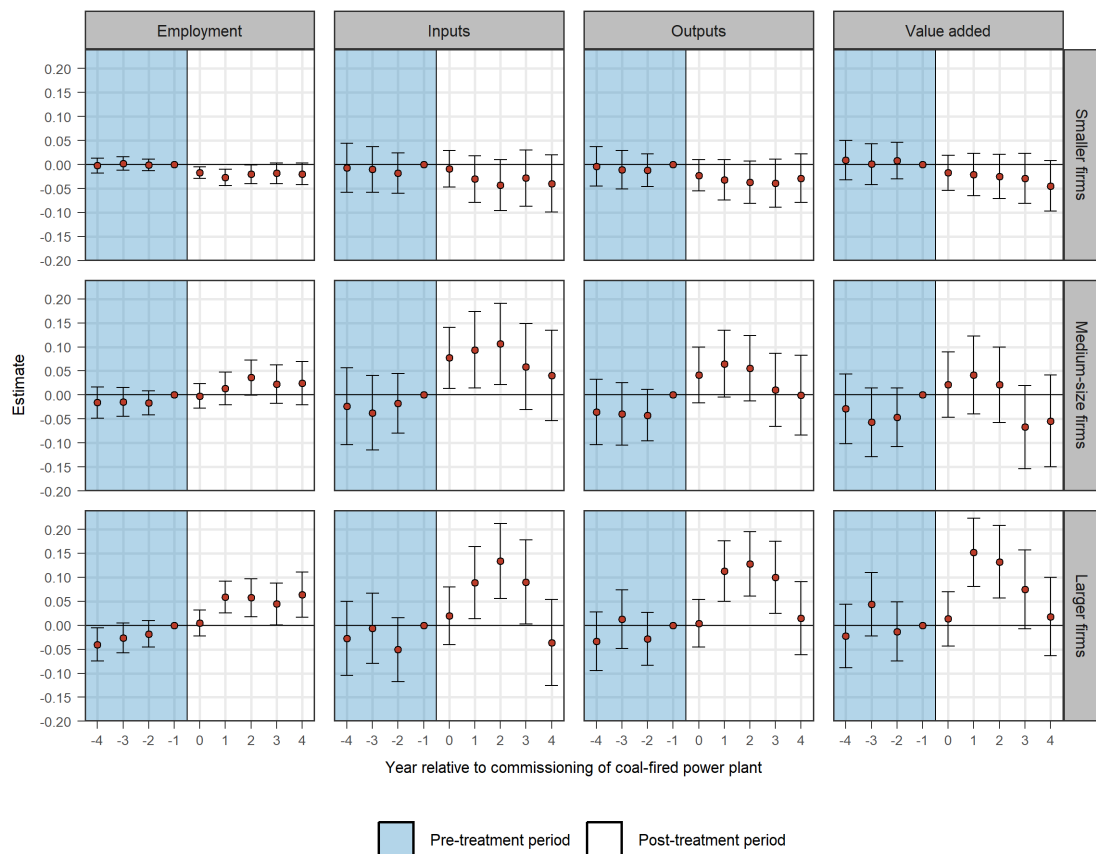


Figure 2: Estimates of the effects of coal-fired power plant commissioning on incumbent and local firm performance

This figure shows estimates of the effects of coal-fired power plant commissioning on incumbent and local manufacturing firms, differentiated by firm size one year before the treatment. The outcome variables are total employment (in log total employees), total material inputs, total outputs, and value added (all in log IDR). These are coefficients for $\beta_0^{g,\tau}$ from Equation 3, which we also report in Table 4. Error bars show the 95% confidence interval. We use the year before the treatment ($\tau = -1$) as the base year. We assign firms to firm groups according to the number of employees in the year before the treatment. Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees. Note that we effectively remove firms from cohorts with missing information on employment one year before the treatment ($\tau = -1$).

5.3 Heterogeneous effects between firms from different industries

We turn to heterogeneous effects between firms from different industries. We split our stacked sample according to the firm's industry one year before each cohort's treatment

year ($\tau = -1$). We document that coal-fired power plant commissioning caused firms in more capital-intensive industries to increase their performance.

Table 5 shows pooled estimates for four years after the treatment compared to four years before the treatment, excluding the year of treatment. Supplementary Figure C.3 visualizes such estimates.

The results exhibit heterogeneous effects of coal-fired power plant commissioning among different industries. For example, firms producing *metal products*, *machinery*, and *equipment* increase employment by 5%, material inputs by 14%, outputs by 12%, and value added by 12%. Our analyses also yield positive effects for firms producing *chemical products*, *plastics*, or *petroleum* (3-8%), *basic metal* (6-19%), or *non-metallic mineral* products (6-11%), but statistical power is low in some cases. In contrast, we document negative effects on employment in firms producing *textiles*, *wearing apparel* and *leather* (-4%) and on material inputs (-17%), outputs (-10%), and value added (-9%) in firms producing *pulp and paper*. The effect sizes are small and statistically insignificant for firms from the *food* industry, firms producing *wood* products, or firms producing *miscellaneous manufacturing* goods. In general, statistical power decreases for such analyses due to smaller subsample sizes.

Industries for which we document increases in firm performance are more capital intensive than industries for which we document small or negative effects on firm performance and that are more labor intensive¹⁵. In general, commissioning coal-fired power plants caused an increase in the performance of incumbent firms that are more capital intensive or comparatively larger, but there are no such effects for firms that are more labor intensive or comparatively smaller. Thus, coal-fired power plant commissioning has by no means led to manufacturing growth in firms of all sizes and sectors. Rather, our results suggest that such effects were concentrated in specific firms, leading to negligible effects on aggregate.

5.4 Robustness of results

We vary multiple parameters of our preferred specification to critically assess the robustness of our results and the validity of our identifying assumptions. Our flexible research design facilitates the comparison of multiple estimations, which allows us to test the sensitivity of our results using specification charts (Simonsohn et al. 2020). Specification charts allow us to compare numerous regression outcomes, for which we alter the definition of cohorts, change the inclusion criteria for the control group, change the definition of firm groups, include and remove several sets of fixed effects, and vary the definition of the treatment (see also Supplementary Information A.1).

Figures C.4 to C.15 show point estimates and confidence intervals (95%) for a set of

¹⁵Table B.3 shows production function coefficients for industries.

| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) |
|---|--------------------|--------------------|-------------------|--------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST (Food, beverages and tobacco) | -0.01 (0.01) | 0.001 (0.03) | -0.003 (0.03) | 0.003 (0.03) |
| Treatment group*POST (Textiles, wearing apparel, leather) | -0.04*** (0.02) | 0.003 (0.04) | -0.01 (0.03) | -0.05* (0.03) |
| Treatment group*POST (Wood and wood products, except furniture) | -0.02 (0.04) | -0.04 (0.08) | 0.009 (0.06) | 0.04 (0.06) |
| Treatment group*POST (Pulp, paper, publishing and printing of media) | 0.03 (0.03) | -0.17*** (0.06) | -0.10* (0.05) | -0.09* (0.05) |
| Treatment group*POST (Chemical products, plastics and petroleum) | 0.03* (0.02) | 0.08* (0.04) | 0.08** (0.04) | 0.08** (0.04) |
| Treatment group*POST (Non-metallic mineral products) | 0.06* (0.04) | 0.11* (0.06) | 0.09 (0.06) | 0.07 (0.07) |
| Treatment group*POST (Basic metals) | 0.12** (0.05) | 0.19 (0.15) | 0.16 (0.13) | 0.06 (0.12) |
| Treatment group*POST (Metal products, machinery and equipment) | 0.05** (0.02) | 0.14*** (0.05) | 0.12*** (0.05) | 0.12** (0.05) |
| Treatment group*POST (Miscellaneous manufacturing, transport equipment) | 0.03 (0.03) | -0.0008 (0.05) | 0.007 (0.04) | 0.03 (0.04) |
| <i>Fixed effects</i> | | | | |
| Firm-industry-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-industry-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-industry-FE | Yes | Yes | Yes | Yes |
| Island-year-industry-FE | Yes | Yes | Yes | Yes |
| Firm size-industry-FE | Yes | Yes | Yes | Yes |
| Year t = 0 -industry-FE | Yes | Yes | Yes | Yes |
| Observations | 868,518 | 831,474 | 835,532 | 868,470 |
| R ² | 0.92 | 0.87 | 0.90 | 0.88 |

Table 5: Dynamic effects of coal-fired power plant commissioning on employment, outputs, inputs, and value added, differentiated by industry

This table reports dynamic OLS estimates of the effect of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by the firm's industry one year before the treatment. The outcome variables are labor demand (in log total employees) in column (1), total material inputs in column (2), total outputs in column (3), and value added in column (4) (all in log IDR). These are coefficients for $\beta_0^{g,\tau}$ from a modification of Equation 3: All estimations include a set of fixed effects, namely firm-industry FE, event time-cohort-industry FE, treatment group-cohort-industry FE, a firm-group-industry FE indicating firm size defined by total employment (smaller, medium-size, and larger firms), and an island-year-industry FE. As such, we interlink a standard set of fixed effects with an indicator of firm industry. Estimations do not include an industry-year FE at the 5-digit level. This table reports estimates and standard errors (in parentheses) clustered at the village level where the treatment is assigned. We also show coefficients and 95% confidence intervals in Figure C.3.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to the treatment or control group for each cohort year. We stack different cohorts with respect to event time. We exclude firms from our sample if they are located in villages that were never treated. We assign firms to industries for each cohort according to the 2-digit industry of firms one year before the treatment ($\tau = -1$). We use a constructed industry identifier to account for changes in industry codes over time. Note that we effectively remove firms from cohorts with missing or ambiguous information on industry one year before the treatment year ($\tau = -1$).

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

alternative specifications and our main variables of interest, employment, inputs, outputs, and value added, differentiated by firm size one year before each cohort's treatment year. Heterogeneous effects between firms of different sizes are consistent across a set of alter-

native specifications. Positive effects for larger firms exceed those for medium-size firms, and effects for smaller firms are negative and often statistically insignificant across all outcome variables. This lends support to the credibility of our main results. We proceed to discuss several robustness checks and their implications for the interpretation of our main findings.

Inclusion of never-treated firms in control group In an alternative specification, we include never-treated firms in our control group. Table B.4 shows dynamic estimates from a regression, in which we leverage variation in both treatment and treatment timing. The heterogeneity of treatment effects across firms of different sizes remains unaffected by this variation. For all outcome variables, effect sizes tend to decrease in the post-treatment period among larger and medium-size firms, while becoming small and insignificant for smaller firms. Standard errors increase. This may indicate the need to compare appropriate counterfactual outcomes, as we would expect control group firms that are treated at some point in time to exhibit different dynamic trends than never-treated firms.

Inclusion of power plants in the control group that were once announced but not commissioned Endogeneity of treatment is an important concern with our research design. In our main specification, we address this concern by restricting the control group to firms that were treated at some point, but not within three years before or after the treatment. Nevertheless, the observed effects may tell us more about the treated units (and their anticipation behavior) than about the treatment. As an additional test, we include firms in our control group that are located in an area that was at some point identified by operators as suitable for coal-fired power plant operation, but that was ultimately never treated. Drawing on data on power plant projects that were announced but not yet (or never) commissioned, we thus add to the control groups firms that may have been subject to unobserved factors that could affect both power plant commissioning and firm performance. Our main results remain robust to this alteration (see Table B.5), supporting the notion that treatment effects are driven by the *commissioning* and subsequent operation of the plants rather than anticipation in the firm or unobserved confounding factors.

Definition of firm groups In our preferred specification, we demonstrate heterogeneous treatment effects by creating subsamples of all firms according to their total employment one year before the treatment ($\tau = -1$). We modify the definition of firm groups following two strategies: First, we bin firms according to their total employment four years before the treatment ($\tau = -4$), thereby excluding firms that may have started production in the three years before commissioning. If some firms respond to the *announcement* of a power plant by allocating close to it, this could constitute selection into the treatment

and therefore potentially bias our results. Second, we bin firms according to their total *revenue* one year before the treatment. For medium-size and larger firms, our results are robust to these alternative specifications (see Figures C.4 to C.15). The estimates remain in a comparable range and are statistically significant at the 5% level. In contrast, the estimates for small firms increase when grouped by total revenue. Nevertheless, grouping firms by total revenue does not account for inflation, thus combining comparatively larger firms from earlier years with smaller firms in later years. It is therefore likely that the positive estimates for smaller firms under the revenue grouping assumption reflect effects on larger firms. This is also consistent with the larger standard errors for larger firms due to the resulting smaller sample size.

Effects on later-treated firms We use data on manufacturing firms in the period between 1984 and 2015, during which Indonesia’s manufacturing sector evolved rapidly. This implies that the estimates from our main specification may be less informative about the effects of coal-fired power plant commissioning in more mature stages of industrialization. Table B.7 shows the estimates for the cohorts between 1998 and 2015. Estimates for smaller firms become smaller, but larger for larger firms. Estimates for medium-size firms become smaller and standard errors increase. These results are comparable in magnitude to our main specification, which confirms heterogeneous outcomes across heterogeneous firms for later periods of industrial development.

Effects on firms that are neighbors of treated villages In our main empirical setup, we identify treated villages by spatial distance a to commissioned power plants. Since this is a strict criterion for inclusion in the treatment or control group, we test for effects on neighboring villages to investigate whether power plants also affected plants beyond distance a . Such effects on firms from neighboring villages could bias our results if such untreated units, which may be included in the control group, changed their performance trends in response to the treatment, for instance through local industrial networks. This may violate the stable unit treatment value assumption (SUTVA). In an alternative specification, we therefore include such firms in our treatment group if they are located in villages that share a border with a treated village or are within distance a , where $50 \text{ km} < a < 100 \text{ km}$, of a commissioned coal-fired power plant. For this alternative specification, we drop firms from treated villages from our sample. Table B.6 shows the corresponding effects of coal-fired power plant commissioning on manufacturing firms in villages neighboring treated villages. The coefficients for large and medium-size firms are small and insignificant at the 5% level, justifying the inclusion of firms from neighboring villages in the control group¹⁶. Estimates from our sample of smaller firms tend to decrease relative

¹⁶As a robustness check, we drop plants from neighboring villages from the control group. Our estimates are robust to this variation. See Figures C.4 to C.15. As an alternative, we remove plants from all but neighboring villages from the control group.

to our main specification. These findings also provide evidence for the existence of locally concentrated effects and point to treatment effects in medium-size and larger firms that diminish with distance a to a new power plant, but which are not necessarily transmitted through local industrial networks.

Varying distance a to power plants In our preferred specification, we define treatment events as the commissioning of coal-fired power plants with distance $a = 50$ km. Since we exclude never-treated units in our preferred specification, the definition of a is an important inclusion criterion for both the treatment and control groups. We test whether our results are sensitive to the definition of a and compare the treatment effects on the performance indicators of treated firms while varying the definition of distance a . Figure C.16 shows a specification chart with point estimates and confidence intervals (95%) for different distances a in smaller, medium-size, and larger firms. The results support our main finding of heterogeneous treatment effects for firms of different sizes. Point estimates for smaller firms are comparable in size, but decreasing with greater distance a . For medium-size firms, positive effects tend to diminish for employment, inputs, and outputs at a distance of 80 kilometers or more. For larger firms, the effects for all outcome variables are comparatively insensitive to changes in a , with confidence intervals widening for lower levels of a . This is reasonable because smaller distances reduce the number of firms eligible for the treatment and control groups, respectively. Also, our preferred set of fixed effects may absorb some of the remaining variation. For larger firms, estimates for inputs, outputs, and value added decrease with increases in a . This supports locally bounded effects, which is consistent with an environment where the physical infrastructure through which coal-fired power plants might transcend their influence on local firms (such as grids or roads) is a constraint.

6 Discussion: Explanations and limitations

Our main results include estimates of the direct and indirect effects of coal-fired power plant commissioning in Indonesia on local incumbent manufacturing firms. We provide evidence of heterogeneous effects on firms of different sizes: Larger and medium-size firms increased their performance, while the output levels of smaller firms have remained virtually unaffected and employment decreased in response to the treatment. The interpretation of such results merits an investigation of the mechanisms through which power plant commissioning has affected manufacturing firms, which we turn to first. We then discuss the limitations of our study.

6.1 Investigation of mediating mechanisms

The commissioning of coal-fired power plants could affect local firm performance through several mechanisms with different implications. In this section, we use firm-, (sub-)district-, and province-level data to investigate the effects of coal-fired power plant commissioning through different lenses: In particular, we are interested in the role of increased availability of stable electricity supply, improved transportation infrastructure, induced firm entry, local employment effects, and productivity shocks. We proceed by estimating Equation 3 on additional outcome variables $y_{i,j,k,d,t,g}^M$, such as indicators of firms' energy consumption, transportation demand, or productivity. This yields estimates for $\beta_0^{g,\tau}$, which express the causal effect of coal-fired power plant commissioning on mediating variables that we assume are indicative of different channels through which power plants affect the main outcome variables.

In a second step, we estimate the following equation on our stacked sample:

$$y_{i,j,k,d,t,g} = \sum_g \beta_0^g (\theta_{d,c} * \rho_{c,t} * \phi_c^g) + \sum_g \beta_1^g * y_{i,j,k,d,t,g}^M + \eta_{c,d,g} + \rho_{c,t,g}^\tau + \lambda_{i,g} + \mu_{j,t,g} + \nu_{k,t,g} + \varepsilon_{i,t,c} \quad (4)$$

Here, our coefficient of interest is β_1^g . It expresses the variation in each outcome variable that can be attributed to variation in each additional, potentially mediating variable $y_{i,j,k,d,t,g}^M$ *controlling* for variation caused by coal-fired power plant commissioning. We present β_1^g for each firm group g , but also for the entire sample, which helps to investigate the role of different channels for firm performance.

Improvement of electricity supply Newly commissioned power plants could affect the performance of manufacturing firms due to the improved supply of electricity, which is a critical input to production for many firms. We estimate Equation 2 on total electricity used in production (in log kWh), total expenditure on electricity, and expenditure on generator fuel (both in log IDR) on our stacked firm sample. Column (1) of Table B.8 exhibits small and statistically insignificant effects on total electricity consumption (in kWh), which can be corroborated by a set of alternative specifications (see Figures C.17 to C.19). In contrast, Column (2) indicates that medium-size and larger firms increased their expenditures on *purchased* electricity by 9.6% and 10.3%, respectively, in the post-treatment period. It is unlikely that increases in expenditure on electricity reflect increases in electricity prices *in response to the treatment*, because the Indonesian government has tightly regulated national electricity tariffs. Instead, our estimates may indicate a switch from self-produced to purchased electricity, which is supported by a decrease in spending on generator fuel in response to the treatment for firms of all sizes (Column (3)). Estimates of generator fuel expenditures have large standard errors because this variable is missing

for a large fraction of our sample. During periods of electrification and grid expansion, it
825 appears that the local installation of generating capacity did not change the amount of
electricity consumed, but rather its source.

Tables B.9 to B.12 show estimates for the coefficient β_1^g from Equation 4. The esti-
mates suggest that increases in electricity expenditures can help explain some of increases
in employment, inputs, outputs, and value added. For example, a 1% increase in elec-
830 tricity expenditures is associated with a 0.32% increase in outputs in larger firms, which
implies that, on average, a 2.7% increase in outputs or a 0.8% increase in total employment
in response to coal-fired power plant commissioning is associated with the increased use
of purchased electricity, which is itself attributable to coal-fired power plant commission-
ing. Electricity expenditures are positively associated with all outcome variables across
835 firm sizes, which may help to understand why smaller firms reduce their performance:
Electricity expenditures decrease by 3.8% after the treatment. One reason for the higher
expenditure on electricity in larger firms compared to smaller firms may be that these
larger firms are often more capital intensive than smaller firms and thus more dependent
on a stable supply of electricity.

840 One possible explanation for the difference between the modest increase in electricity
consumption in Column (1) and the larger estimated increase in electricity expenditures
in Column (2) of Table B.8 could be a tariff switch to electricity supplied at higher ap-
parent power. Usually, PLN differentiates industrial tariffs according to apparent power,
measured in kilovolt-ampere (kVA). Some tariffs allow for a higher apparent power deliv-
845 ery to be supplied by the local electricity infrastructure, enabling industries to operate
more electricity-intensive machinery simultaneously. However, such tariffs are often more
expensive. One interpretation is that more capital-intensive or larger firms are more likely
to select more expensive tariffs in order to overcome previous production constraints and
increase their performance accordingly. In contrast, smaller firms in more labor-intensive
850 industries may not have been able to reap such benefits from improved electricity trans-
mission.

Thus, increased stability of electricity supply may be another reason for increased ex-
penditures on electricity and improved firm performance, while changes in electricity con-
sumption are comparatively small. In Indonesia, electricity supply suffers from frequent
855 interruptions (Falentina and Resosudarmo 2019), and Indonesian firms have consistently
reported that electricity supply is an obstacle to firm performance (World Bank 2009,
2015). In fact, proximity to coal-fired power plants, which often have large generating
capacities, may increase *local* grid stability and reduce interruptions, which may help
explain why treatment effects tend to diminish with distance and why larger firms and
860 firms in more capital-intensive industries increase their employment, inputs, and outputs
in the post-treatment period, but not the total electricity used in production¹⁷.

¹⁷As a follow up, we analyze data from PLN’s annual reports between 2010 and 2020 to estimate the

We consider it likely that improvements in grid stability and electricity transmission capacity contribute to explaining the heterogeneous effects on incumbent firms. In the absence of large local power-generating capacity and in environments where trans-regional transmission may have been suboptimal, energy-intensive firms may have chosen lower levels of employment, inputs, and outputs. The commissioning of coal-fired power plants – often with a total generating capacity of more than 100 MW – could therefore have altered decisions on the use of productive capital in those firms that require stable transmission of high-voltage electricity to operate machinery. This channel cannot be tested with the available data, but it might help explain the heterogeneous effects on firms of different sizes and capital intensities, the locally bounded treatment effects, and the negligible changes in aggregate electricity consumption (in kWh).

To shed light on local improvements in electricity supply, we compare the effects of coal-fired power plant commissioning with the effects of gas-fired, hydropower, and geothermal power plant commissioning. We estimate Equation 2 for different types of power plant technologies and define treatment θ_d for each village d and stacked datasets that we construct accordingly.

Columns (5) to (8) of Table B.14 show estimates of the effects of gas-fired power plant commissioning on employment, material inputs, outputs, and value added for firms of different sizes¹⁸. Similar to the results reported in Table 3, we drop never-treated observations from the sample. The heterogeneous effects on employment are comparable to those for coal-fired power plant commissioning, but the results become small and statistically insignificant for inputs, outputs, and value added in medium-size and larger firms. In response to gas-fired power plant commissioning, smaller firms reduced their inputs and outputs by 7.6% and 5.1%, respectively, which is a substantial decrease compared to the effects of coal-fired power plants.

The effects also differ for hydropower (B.15) and geothermal power plants (B.16). For hydropower plants, heterogeneous effects for firms of different sizes prevail and are comparable in magnitude to those for coal-fired power plants when all firms are included in the control group. For geothermal power plants, the effects diminish or even become

effects of coal-fired power plant commissioning on electricity grid stability. Data for earlier periods were not available. The outcome variables are the System Average Interruption Frequency Index (SAIFI) and the System Average Interruption Duration Index (SAIDI) of Indonesian provinces. We report the results in Table B.13. In the post-treatment period, SAIFI (SAIDI) decreases (increases) by 4% (0.7%), but the estimates are statistically insignificant. Note that this expresses province-level variation, which may not reflect village-level differences in electricity transmission. Also, improvements in electricity grid stability are likely to have been greater at earlier points in time, particularly during the 1990s, a period of extensive electrification in Indonesia. However, temporally and spatially disaggregated data on grid reliability are not available for this period. Another cause that could benefit grid reliability is the expansion and improvement of electricity transmission infrastructure. To our knowledge, however, detailed spatial information on transmission infrastructure is however not publicly available for Indonesia and the period of our study.

¹⁸It is worth noting that we create novel datasets by stacking new cohorts for gas-fired, hydropower, and geothermal power plants.

negative when never-treated observations are removed from the sample.

The effects of gas-fired, hydropower, or geothermal power plants may to some extent reflect differences in total generating capacity or local conditions required for each technology. For example, gas-fired power plants are less concentrated on Java (31%) than coal-fired power plants (50%), which means that the resulting firm samples may be difficult to compare. It is therefore difficult to understand how firms located near coal-fired power plants would have been affected by gas-fired power plants.

Nevertheless, our comparison of fuel-specific treatment effects shows that coal-fired power plants exhibit the largest positive effects on the performance of medium-size and larger firms, and that the effects of other technologies are not of comparable magnitude. This may suggest that power plants may have affected local firm performance through channels other than local improvements in electricity supply.

Improvement of transportation infrastructure The operation of coal-fired power plants often requires additional transportation infrastructure, such as roads or ports, which could have positive effects on local manufacturing firms (Donaldson 2018; Gibbons et al. 2019). We estimate Equation 2 on the aggregate transport weight of inputs and outputs at the firm level, which we derived from manufacturing plant input-output records between 1998 and 2012. The aggregate weight of inputs and outputs serves as a proxy for local firms' demand for transportation infrastructure. Column (3) of Table B.17 shows that aggregate transport weight increased by 15.2% for larger firms and by 5.1% for all firms in the post-treatment period, but the standard errors are large.

Table B.18 shows estimates for the coefficient β_1^g from Equation 4 with aggregate transport weights as the mediating variable. The results suggest that increases in transport demand are associated with increases in inputs and outputs (in log IDR) across the sample and for firms of all sizes: A 1% increase in aggregate transport weight is associated with an 0.10% increase in outputs across firms. On average, an increase in transport demand due to the commissioning of coal-fired power plants is thus associated with an increase in outputs of 0.51% and 1.3% for all firms and larger firms, respectively. In essence, such analyses indicate that transport demand has increased in response to coal-fired power plant commissioning for firms of all sizes. Nevertheless, such effects are comparatively small and insignificant for smaller and medium-size firms.

We also use data on local road conditions and infrastructure investment. Column (1) of Table B.19 shows that the sub-district-level share of villages with a main asphalt road *decreased* after the commissioning of coal-fired power plants and compared to sub-districts that were treated at some point in time. This is consistent with additional investment in transportation infrastructure in the *pre-treatment* period. However, identifying the causal effect of power plant *construction* on local road conditions is difficult because we are unable to observe the exact dates when power plants were announced. In addition,

analyzing data at the sub-district level may mask locally concentrated effects at the village level.

Such supplementary insights do not rule out the possibility that coal-fired power plants may have affected local manufacturing firms through the associated provision of local infrastructure. If anything, however, such effects are likely to have been most beneficial to larger firms, contributing to heterogeneous effects across firms of different sizes. This may help to shed light on locally bounded effects (see Figure C.16) and on differences between coal-fired and other types of power plants, since the operation of geothermal and hydropower plants does not usually require the transportation of fuel inputs.

Firm entries and increasing wages Changes in local energy supply infrastructure can affect firm performance by inducing agglomeration and subsequent effects on local labor markets. One reason is that improved energy availability may lower entry barriers for new firms, which in turn increases competition for local inputs and labor, thereby affecting the performance of less productive firms (Kassem 2024).

We document changes in the total number of firm entries at the village level by using village-year information on the total number of firm entries in a variation of Equation 1. Coal-fired power plant commissioning leads to an increase in firm entries of 0.042 per year in each village within 50 kilometers (Table B.20). The average number of new firms per year and village in our sample is 0.101. These entering firms are mostly small, which is reasonable because they require relatively less initial investment and resources in compared to the establishment of large firms.

Such firm entries affect the local demand for labor, as we show in Table B.21. An additional firm at the village level reduces employment in incumbent firms by 0.2%. Such effects appear modest, but given that new firms are mostly small and that new firms are likely to affect the demand for labor in neighboring villages, it is reasonable to conclude that new firms help explain the lower level of employment in smaller firms in the post-treatment period.

We also examine changes in local wages. Table B.22 shows estimates of Equation 1 with wages for production workers and non-production workers as the outcome variables. We find that wages for production workers (non-production workers) increased by 4.2% (5.4%) in the post-treatment period. Differentiated by firm size, wage increases are most pronounced for smaller and medium-size firms, with wage increases ranging from 4.2% to 6.7%, while wage changes are statistically insignificant for larger firms.

Table B.23 shows estimates for the coefficient β_1^g from Equation 4 with wages as the mediating variable. A 1% increase in production worker wages is associated with a 0.01% decrease in total employment. A 1% increase in the wages of non-production workers is associated with a 0.03% decrease in total employment. This suggests that wage changes caused by coal-fired power plant commissioning can help explain 0.18%

of the 2.1% decline in total employment at small incumbent firms. These changes were mainly driven by changes wages of non-production workers (0.16%). Wage changes also dampened employment in medium-size (-0.31%) and larger firms (-0.14%), but these effects were offset by positive employment effects from, for example, increased spending on electricity.

Increased entry of new, mostly small firms and rising local wages are likely to be correlated. Together, both mechanisms contribute to explaining the negative treatment effects on employment in *incumbent* smaller firms. In addition, Table B.24 shows the effects of coal-fired power plant commissioning on district-level population and employment. District-level population increases by 3.1% in the post-treatment period (Column (1)), but changes in (industrial) employment are statistically insignificant (Columns (3) to (4)). Table B.25 shows the treatment effects on employment at the sub-district level. In response to the treatment, changes in the labor force and formal employment are insignificant, while we estimate a reduction in the share of children in the labor force of 0.6%. Such findings add to the literature describing the relationship between economic shocks and child labor in non-industrialized economies (Edmonds and Pavcnik 2005; Edmonds et al. 2010); In Indonesia, reductions in child labor have also followed exposure to trade tariff reductions (Kis-Katos and Sparrow 2011).

Overall, the additional demand for labor by (incumbent) larger and medium-size firms and by new (smaller) firms is associated with rising local wages and declining employment in incumbent smaller firms. The effects on aggregate employment are modest, which may indicate that coal-fired power plant commissioning induces a reallocation of labor from smaller to larger or new firms rather than an overall increase in employment.

Changes in productivity Rising wages may to some extent reflect increases in labor productivity. Local changes in input prices in response to the treatment may also lead firms to use resources more efficiently. In Table B.26, we show the effects of coal-fired power plant commissioning on various measures of firm productivity (labor productivity, output per worker, and total factor productivity), broken down by firm size. The estimates are negative for smaller firms and positive for larger firms, but not statistically significant at the 5% level. It is therefore difficult to conclude that coal-fired power plants induced short-term increases in productivity across firms of different sizes.

Tables B.27 to B.29 show estimates for the coefficient β_1^g from Equation 4 with measures of productivity as the mediating variable. On aggregate, however, the estimates show that shocks to plant-level productivity could help explain up to 3.0% and 4.4% of the increase in inputs and outputs, respectively, for larger firms. This holds for different measures of TFP as mediating variables, but cannot be corroborated for measures such as labor productivity or output per worker. In essence, coal-fired power plants appear to have affected the performance of local and incumbent firms mostly at the extensive

margin.

Disentangling channels that might help explain changes in the performance of manufacturing firms can be challenging for several reasons: First, such channels may be interlinked, for example, if improvements in local transportation infrastructure may lead to the establishment of new firms. Second, for some channels, we have to rely on proxy variables that are sometimes poorly measured, missing in the data, or of low resolution. Third, our supplementary analyses leave us with some effect of coal-fired power plant commissioning on the performance of local incumbent firms that we cannot explain by induced variation in electricity supply, transportation infrastructure, wages, firm entries, or productivity. This direct effect of coal-fired power plants may include the effects of improvements in local grid quality, increases in local pollution, or changes in land rents. Such factors, which may counteract or exacerbate the effect, remain unobserved in the data.

6.2 Limitations

In this study, we assess the causal effects of power plant commissioning on local and incumbent manufacturing firms. Since we are unable to exploit random variation in treatment, we leverage quasi-random variation in treatment *timing* within our structure of fixed effects to assess the average treatment effect on the treated. Our research design thus facilitates the reduction of bias due to endogenous treatment, unobserved confounding factors, or selection into earlier or later treatment. Nevertheless, we discuss concerns about the internal and external validity of our results below.

We cannot conclude from our study that *any* firm in Indonesia would have experienced the same changes in outcomes as firms from the regions identified as suitable for power plant operation. This follows from the fact that our results capture average treatment effects on the treated, which accounts for unobserved endogeneity in power plant allocation decisions.

Since our sample period mostly covers phases of (early) industrial development in Indonesia, we cannot claim that our results are valid for future coal-fired power plants in Indonesia or for firms in different regions at similar stages of industrialization. Also, we do not provide any insights into the effects on firms that do not appear in the Indonesian Manufacturing Census, for example because they employ less than 20 workers. In addition, the documented increases in firm entry may be driven to some extent by employment growth in existing Indonesian microenterprises.

In this study, we also focus on the effects at the level of individual manufacturing firms in the four years post-treatment. We thereby refrain from investigating the effects on long-term industrial development and structural change, which would require several

hard restrictions for credible identification. In addition, the unavailability of spatially and temporally disaggregated data on electricity grids, local pollution, and population restricts our analysis to the performance of *firms*. Further analyses could benefit from drawing on additional data sources to yield a more comprehensive understanding of the effects of coal-fired power plant commissioning.

7 Conclusion

We analyze the causal effects of coal-fired power plant commissioning on local and incumbent manufacturing firms in Indonesia. Leveraging high-resolution manufacturing and power plant data and quasi-random variation in treatment timing, we find that larger and medium-size firms (with 100 employees or more) increased employment, inputs, outputs, and value added in the four years after commissioning. In contrast, the performance of small firms (20 to 99 employees) remains largely unaffected, with documented declines in employment being a notable exception. We suspect that greater reliability of electricity transmission, improved transportation infrastructure, and increased competition for labor help to explain some of the heterogeneous effects of coal-fired power plant commissioning.

Our results support heterogeneous effects of coal-fired power plant commissioning on the performance of local manufacturing firms during a period of industrialization in Indonesia between 1984 and 2015. It is evident that such power plants contributed to industrial development at the extensive margin, although such effects may have mainly benefited larger, more capital-intensive firms and were mostly temporally and spatially concentrated. Nevertheless, such effects on the performance of large firms, which are likely to be particularly visible, may help to understand why coal-fired power technology remains popular, particularly in industrializing economies. While the external validity of our results is questionable, international calls to reduce coal use or to discourage investment in new fossil-fueled power capacity must take into account such direct and indirect effects on industries. Whether alternative technologies, such as renewable energy technologies, can make a similar contribution to future industrial development remains to be understood, but is highly relevant to the prospects for sustainable economic development.

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Appendix

A Supplementary information

A.1 Alternative specifications

Figures C.4 to C.15 and C.17 to C.19 show different estimates from alternative specifications for the outcome variables labor, material inputs, outputs, value added, and electricity use. We start with changing single aspects with respect to the construction of stacked samples, definitions of treatment and control groups, or included sets of fixed effects. We continue by varying multiple parameters. It is important to note that estimates for alternative specifications encompass different interpretations of resulting estimation coefficients. Here, we provide supplementary information to such alternative specifications.

The preferred specification In our preferred specification, we include all firm-level observations that are available and construct cohort-level datasets for each treatment year between 1984 and 2015. 1984 is the year in which the first coal-fired power plant was commissioned in Indonesia. 2015 is the last year for which we have firm-level information. All firm-year-level observations are eligible for inclusion in the stacked dataset. Manufacturing firms that are in proximity of distance a to a newly commissioned coal-fired power plant in year t of commissioning belong to the treatment group of each cohort. In our preferred specification, we include only such firms in the control group that were treated at some point in time. We remove firms from the control group that were treated three years before or after the treatment year.

In our specification charts, we present estimates for heterogeneous firm groups and assign firms to firm groups conditional on the number of employees one year before the treatment. We include a set of fixed effects on the firm-, 5-digit-industry-year- and island-year level. We also account for cohort-level differences between control and treatment group (*cohort-treatment-group FE*), for cohort-level time-variant effects (*cohort-event-time FE*) and remove differences between the treatment and control groups in the treatment year because we are unable to observe the exact commissioning date. We remove firms from the treatment group that were treated in the three years before the treatment year. We choose a distance a of 50 km. Figure C.16 shows an analysis of different distances. For each cohort, the *treatment window* consists of the four years before and after the treatment year.

Cohorts We show estimates excluding earlier cohorts (*1998-2015*) that express the effects of coal-fired power plant commissioning in more mature stages of industrial development.

Control groups We leverage the flexibility to adjust firms in the control group.

- 1335 • We include never treated firms to the control group (*Including never treated*). See also Table B.4. Interpreting resulting estimates as causal requires the assumption that the treatment is unconditional on local exogenous circumstances, such as resource endowment or transportation infrastructure. It assumes that manufacturing firms in more remote regions are a reasonable counterfactual for firms from regions with investment in coal-fired power plant capacity at some point in time.
- 1340 • We exclude firms from the control group that were treated at some point in time, but not in the treatment year (*Including never treated only*). Estimates show the comparison of never treated manufacturing firms and the treatment group.
- 1345 • We exclude firms from the control group that were part of the treatment group in earlier cohorts (*Excluding treated before*). This may be warranted under the assumption that the treatment sets manufacturing firms on different trends and that such differences in trends are persistent three years after treatment. Note that we account for such short-term effect through the inclusion window three years before and post treatment.
- 1350 • We exclude manufacturing firms from the control group that are neighboring manufacturing firms from the treatment group. Neighboring manufacturing firms are defined as firms in villages that share a border with treated villages, or that are in distance $50 \text{ km} < a < 100 \text{ km}$ to a coal-fired power plant. See also Table B.6.
- 1355 • We include only manufacturing firms in the control group that are neighboring manufacturing firms from the treatment group. This may be warranted to capture regional dynamics, but rests on the assumption that treatment effects would be absent just after 50 km.
- 1360 • We exclude manufacturing firms from the 2-digit-industry *Coke, refined petroleum products, and nuclear fuel* to account for possible effects to firms that may use hard coal as an input to production.
- We include manufacturing firms to the control group that operate in villages that are in proximity to coal-fired power plants that have been announced, but never commissioned. See also Table B.5.

Firms We group firms for each cohort according to their total employment one year before the treatment. As an alternative, we present estimates for grouping firms for each cohort according to their total employment four years before the treatment or according to their revenue one year before the treatment. Note that grouping with revenue is subject to inflation which implies that effects on comparatively larger firms from earlier years mask effects on smaller firms in later years.

Fixed effects We change our set of fixed effects to control for different unobserved characteristics.

- We replace the fixed effect at the 5-digit-industry-year level with a fixed effect at the 2-digit-industry-year level. We leverage changes in industry codes over the sample period and construct more aggregated 2-digit-industry identifiers that allow for comparing industries over time. Building on such identifiers, we include more aggregated 2-digit-industry-year-level fixed effects.
- We replace the fixed effect at the 5-digit-industry-year level with a fixed effect at the 3-digit-industry-year level.
- We include a district-year-level fixed effect that soaks up time-variant variation at the district-level. This variation may to some extent reflect village-level variation in manufacturing performance caused by coal-fired power plant commissioning.
- We replace island-year-level fixed effects with province-year-level fixed effects. Our data includes information about manufacturing firms from six main islands and 26 provinces.
- We add fixed effects at the 2-digit-industry-island level, at the 2-digit-industry-island-year level and the 5-digit-island level, respectively, to additionally account for island-level time-variant differences between industries.
- We include differences between the treatment and control groups in the treatment year. This would be reasonable under the assumption that coal-fired power plants started operation in the beginning of the year.
- We include linear time trends that account for possibly existing different trends at different levels of aggregation. First, we replace the cohort-event-time-level fixed effect with a cohort-level linear time trend. Second, we replace the island-year-level fixed effect with an cohort-island-level linear time trend. Third, we replace the island-year-level fixed effect with a cohort-province level linear time trend.

Treatment type We change the definition of treatment with resulting qualifications for the interpretation of estimates.

- We show estimates in our specification charts for manufacturing firms in villages within a distance of 100 km to coal-fired power plants, respectively. See also Figure C.16 for a more nuanced analysis of different distances.
- We show estimates for specifications in which we define the treatment on the district level, including a variety of combinations of fixed effects. We have information about plants in 288 districts compared to 9,417 villages.
- We include firms to the treatment and control group that are located on Java only. Java is the Indonesian island comprising the largest share of economic activity.
- Exact spatial information is missing for some commissioned coal-fired power plants in our sample. We remove firms in proximity to power plants with approximate locations from the treatment group.

- We include firms to the treatment group that were treated in the three years before the treatment. The treatment effects may thus also capture lagging effects of prior treatment events.
- We exclude firms from the treatment group which were in proximity to gas-fired power plants that were commissioned in the treatment year or in the three preceding years.
- We exclude firms from the treatment group that were treated before.
- Our sample is unbalanced, i.e., some information is missing for some firms in some years. First, we exclude firms from cohorts with less than two observations in the pre-treatment period. Second, we exclude firms from cohorts with less than two observations in the post-treatment period. Third, we include only such firms in each cohort with at least two observations in both the pre- and post-treatment period.

Window For each cohort, we include observations for four years before and after the treatment year. We refer to this period as the *event window*. In alternative specifications, we change the definition of the event window. The resulting estimates express differences between the treatment and control group for different definitions of pre- and post-treatment periods.

- We prolong the pre-treatment period and include observations for seven years before the treatment year.
- We prolong the pre- and post-treatment period and include observations for seven years before and after the treatment year.
- We prolong the pre-treatment period by including observations for seven years before the treatment year and by accounting for possible anticipation effects in the three years preceding treatment.

B Tables

| Variable | Firm group | 1990 | | | 2000 | | | 2010 | | |
|---------------------------|-------------------|--------|--------|--------|---------|--------|---------|---------|---------|---------|
| | | Mean | Median | SD | Mean | Median | SD | Mean | Median | SD |
| Number of workers | Smaller firms | 43 | 37 | 21 | 39 | 31 | 20 | 40 | 32 | 20 |
| | Medium-size firms | 157 | 149 | 43 | 157 | 149 | 43 | 157 | 150 | 42 |
| | Larger firms | 868 | 530 | 1,033 | 914 | 545 | 1,260 | 899 | 555 | 1,151 |
| Inputs (1M IDR) | Smaller firms | 521 | 115 | 1,781 | 1,497 | 294 | 7,453 | 6,590 | 1,005 | 39,981 |
| | Medium-size firms | 4,234 | 1,194 | 13,691 | 14,112 | 3,521 | 37,206 | 57,416 | 13,513 | 144,437 |
| | Larger firms | 17,208 | 5,955 | 41,235 | 68,591 | 20,832 | 142,214 | 178,788 | 55,853 | 350,552 |
| Outputs (1M IDR) | Smaller firms | 739 | 189 | 2,407 | 2,389 | 543 | 13,970 | 10,056 | 1,849 | 52,848 |
| | Medium-size firms | 5,812 | 1,815 | 16,582 | 22,078 | 5,850 | 61,443 | 87,583 | 25,265 | 202,756 |
| | Larger firms | 25,688 | 8,898 | 69,743 | 108,901 | 34,617 | 219,765 | 291,437 | 107,078 | 511,686 |
| Value added (1M IDR) | Smaller firms | 210 | 57 | 810 | 892 | 192 | 7,766 | 3,518 | 675 | 17,155 |
| | Medium-size firms | 1,587 | 461 | 4,424 | 8,357 | 1,757 | 38,398 | 30,523 | 8,966 | 84,437 |
| | Larger firms | 8,881 | 2,368 | 33,200 | 41,484 | 9,522 | 107,060 | 113,539 | 40,634 | 218,323 |
| Electricity (MWh) | Smaller firms | 87 | 12 | 320 | 199 | 16 | 1,364 | 102 | 5 | 1,516 |
| | Medium-size firms | 480 | 131 | 1,461 | 1,475 | 298 | 5,958 | 787 | 110 | 4,494 |
| | Larger firms | 2,561 | 418 | 7,737 | 9,676 | 1,365 | 46,138 | 3,078 | 479 | 11,774 |
| Wages (1k IDR) | Smaller firms | 775 | 621 | 760 | 3,214 | 2,747 | 3,187 | 12,540 | 9,537 | 15,948 |
| | Medium-size firms | 1,024 | 740 | 964 | 4,947 | 3,635 | 5,811 | 17,058 | 13,326 | 15,465 |
| | Larger firms | 1,109 | 812 | 1,019 | 5,145 | 4,030 | 5,169 | 14,796 | 12,719 | 14,670 |
| Total factor productivity | Smaller firms | 8.08 | 7.93 | 1.06 | 8.66 | 8.56 | 1.23 | 9.78 | 9.68 | 1.36 |
| | Medium-size firms | 9.26 | 9.22 | 1.03 | 10.2 | 10.1 | 1.45 | 11.5 | 11.37 | 1.4 |
| | Larger firms | 10.26 | 10.34 | 1.19 | 11.04 | 10.97 | 1.58 | 12.24 | 12.2 | 1.44 |
| Labor productivity (log) | Smaller firms | 7.47 | 7.32 | 1.08 | 8.73 | 8.64 | 1.18 | 10.02 | 9.9 | 1.28 |
| | Medium-size firms | 8.19 | 8.08 | 1.29 | 9.58 | 9.32 | 1.41 | 11.08 | 10.96 | 1.37 |
| | Larger firms | 8.3 | 8.24 | 1.24 | 9.76 | 9.64 | 1.35 | 11.12 | 11.07 | 1.2 |

Table B.1: Summary statistics of firm groups in 2010

This table presents summary statistics for the main outcome variables in our sample of manufacturing plants based on data from the Indonesian Manufacturing Census. In this table, firms are clustered according to total employment in 1990, 2000, and 2010, respectively. Smaller firms are those with 20 to 99 employees. Medium-size firms are those with 100 to 249 employees. Larger firms are those with more than 249 employees.

| Cohort | Treatment group | | | Control Group | | | |
|--------|-----------------|----------------|-----------------|---------------|--------------|---------------|-------------------------|
| | Treated | Treated before | Treatment group | Control group | Treated ever | Never treated | Villages with exclusion |
| 1984 | 4 | 0 | 4 | 2,768 | 1,540 | 1,228 | 7 |
| 1985 | 9 | 9 | 0 | 2,941 | 1,623 | 1,318 | 405 |
| 1986 | 0 | 0 | 0 | 3,027 | 1,665 | 1,362 | 478 |
| 1987 | 0 | 0 | 0 | 3,216 | 1,751 | 1,465 | 534 |
| 1988 | 16 | 16 | 0 | 3,668 | 2,010 | 1,658 | 548 |
| 1989 | 20 | 19 | 1 | 3,897 | 2,122 | 1,775 | 625 |
| 1990 | 0 | 0 | 0 | 4,110 | 2,238 | 1,872 | 664 |
| 1991 | 0 | 0 | 0 | 4,461 | 2,369 | 2,092 | 726 |
| 1992 | 1,705 | 33 | 1,672 | 4,443 | 2,151 | 2,292 | 270 |
| 1993 | 626 | 537 | 89 | 5,150 | 2,750 | 2,400 | 741 |
| 1994 | 105 | 96 | 9 | 5,370 | 2,854 | 2,516 | 810 |
| 1995 | 3 | 3 | 0 | 5,598 | 2,977 | 2,621 | 840 |
| 1996 | 63 | 63 | 0 | 5,750 | 3,043 | 2,707 | 843 |
| 1997 | 72 | 72 | 0 | 5,998 | 3,159 | 2,839 | 1,180 |
| 1998 | 5 | 0 | 5 | 6,174 | 3,201 | 2,973 | 1,185 |
| 1999 | 196 | 0 | 196 | 6,306 | 3,243 | 3,063 | 1,489 |
| 2000 | 261 | 214 | 47 | 6,640 | 3,466 | 3,174 | 1,234 |
| 2001 | 0 | 0 | 0 | 6,898 | 3,591 | 3,307 | 1,205 |
| 2002 | 0 | 0 | 0 | 8,621 | 4,365 | 4,256 | 1,415 |
| 2003 | 0 | 0 | 0 | 8,787 | 4,436 | 4,351 | 1,585 |
| 2004 | 1,353 | 0 | 1,353 | 8,623 | 4,222 | 4,401 | 1,813 |
| 2005 | 0 | 0 | 0 | 8,700 | 4,319 | 4,381 | 2,607 |
| 2006 | 2,953 | 1,015 | 1,938 | 8,287 | 3,900 | 4,387 | 2,265 |
| 2007 | 1,644 | 989 | 655 | 8,348 | 4,052 | 4,296 | 2,718 |
| 2008 | 1,672 | 1,662 | 10 | 8,445 | 4,270 | 4,175 | 2,881 |
| 2009 | 134 | 0 | 134 | 8,209 | 4,138 | 4,071 | 2,838 |
| 2010 | 193 | 120 | 73 | 7,882 | 4,019 | 3,863 | 3,072 |
| 2011 | 2,940 | 174 | 2,766 | 7,032 | 3,259 | 3,773 | 2,424 |
| 2012 | 3,304 | 2,199 | 1,105 | 6,306 | 2,729 | 3,577 | 2,375 |
| 2013 | 392 | 157 | 235 | 6,048 | 2,680 | 3,368 | 2,606 |
| 2014 | 384 | 276 | 108 | 5,748 | 2,595 | 3,153 | 2,406 |
| 2015 | 368 | 30 | 338 | 5,406 | 2,402 | 3,004 | 2,192 |

Table B.2: Treated and non-treated villages in each cohort

This table reports the number of villages with firm-level observations in the treatment and control group for each cohort. Villages are eligible for the treatment group, if they are within distance of 50 km to commissioned coal-fired power plants in each cohort year ('Treated'). Column 'Treated before' indicates how many of those villages were treated in the past three years. For each cohorts' treatment group, we remove firms from such villages. Column 'Treatment group' indicates the number of villages that are left in the treatment group for each cohort. We mark in grey all rows without villages in the treatment group.

Column 'Control group' indicates how many villages are not within distance of 50 km to commissioned coal-fired power plants in each cohort year. Column 'Treated ever' indicates how many of those villages have been within distance of 50 km to commissioned coal-fired power plants at least once between 1984 and 2018. Column 'Never treated' indicates how many villages have never been within distance of 50 km to commissioned coal-fired power plants between 1984 and 2018. Column 'Villages with exclusion' indicates the number of villages that are excluded from the control group for our main specification because they were treated in the three years preceding and succeeding the cohort year.

| Industry | Firms | LP | | W | |
|--|--------|------------------|------------------|------------------|-------------------|
| | | Labor | Capital | Labor | Capital |
| Food products and beverages | 43,348 | 0.539 (0.012) | 0.177 (0.008) | 0.609 (0.006) | 0.176 (0.007) |
| Tobacco products | 7,778 | 0.4 (0.037) | 0.143 (0.018) | 0.5 (0.018) | 0.146 (0.016) |
| Textiles | 16,285 | 0.433 (0.018) | 0.084 (0.012) | 0.485 (0.01) | 0.095 (0.013) |
| Wearing apparel | 15,214 | 0.618 (0.017) | 0.15 (0.018) | 0.685 (0.009) | 0.152 (0.013) |
| Leather products | 4,413 | 0.531 (0.029) | 0.081 (0.033) | 0.605 (0.015) | 0.058 (0.023) |
| Wood and wood products | 27,223 | 0.311 (0.009) | 0.164 (0.011) | 0.345 (0.006) | 0.176 (0.008) |
| Pulp and paper products | 3,241 | 0.405 (0.043) | 0.121 (0.046) | 0.51 (0.025) | 0.102 (0.031) |
| Publishing and printing | 4,143 | 0.587 (0.039) | 0.108 (0.04) | 0.676 (0.022) | 0.097 (0.025) |
| Coke, refined petroleum products and nuclear fuel | 4,383 | 0.262 (0.026) | 0.108 (0.024) | 0.246 (0.017) | 0.131 (0.022) |
| Chemicals and chemical products, pharmaceuticals | 12,321 | 0.207 (0.014) | 0.154 (0.028) | 0.25 (0.011) | 0.156 (0.018) |
| Rubber and plastics | 10,714 | 0.473 (0.021) | 0.117 (0.022) | 0.515 (0.011) | 0.115 (0.016) |
| Other non-metallic mineral products | 22,771 | 0.277 (0.015) | 0.187 (0.01) | 0.315 (0.006) | 0.195 (0.008) |
| Basic metals | 3,747 | 0.257 (0.035) | 0.237 (0.039) | 0.25 (0.024) | 0.272 (0.032) |
| Fabricated metal products except machinery and equipment | 9,640 | 0.218 (0.028) | 0.221 (0.024) | 0.298 (0.013) | 0.232 (0.021) |
| Machinery and equipment | 3,800 | 0.209 (0.028) | 0.23 (0.045) | 0.226 (0.021) | 0.246 (0.03) |
| Electrical machinery | 1,886 | 0.401 (0.041) | 0.089 (0.052) | 0.557 (0.033) | 0.05 (0.05) |
| Radio, television, communication equipment | 721 | 0.551 (0.052) | 0.125 (0.115) | 0.602 (0.063) | 0.063 (0.088) |
| Medical, precision and optical instruments | 274 | 0.434 (0.1) | 0.022 (0.094) | 0.497 (0.072) | -0.022 (0.107) |
| Motor vehicles | 1,921 | 0.311 (0.049) | 0.076 (0.031) | 0.419 (0.031) | 0.11 (0.04) |
| Other transport equipment | 4,273 | 0.127 (0.023) | 0.185 (0.035) | 0.158 (0.021) | 0.22 (0.033) |
| Furniture and other manufacturing | 19,882 | 0.458 (0.016) | 0.147 (0.014) | 0.51 (0.008) | 0.139 (0.011) |
| Recycling | 498 | 0.449 (0.093) | 0.126 (0.122) | 0.412 (0.084) | 0.15 (0.079) |

Table B.3: Production function coefficients for each 2-digit manufacturing sector

This table shows production function coefficients for each 2-digit manufacturing sector in our sample. We estimate production functions following Levinsohn and Petrin (2003) (Columns LP) and Wooldridge (2009) (Columns W) with total material inputs (in log IDR) as auxiliary intermediate input, respectively. Note that a substantial fraction of firm-year-level observations is lacking information on variables that are important for production function estimation, including total capital, total employment, or firm-level industry.

| Sample (Group name) | Smaller firms | | | | Medium-size firms | | | | Larger firms | | | |
|--------------------------------------|-------------------|-------------------|-------------------|--------------------|---------------------|---------------------|--------------------|--------------------|---------------------|---------------------|--------------------|---------------------|
| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) | Labor (5) | Input (6) | Output (7) | Value added (8) | Labor (9) | Input (10) | Output (11) | Value added (12) |
| <i>Variable</i> | | | | | | | | | | | | |
| Treatment group $\times t = -4$ | -0.002 (0.007) | -0.034 (0.024) | -0.030 (0.020) | -0.017 (0.020) | -0.022 (0.016) | -0.010 (0.038) | -0.030 (0.032) | -0.045 (0.035) | -0.038** (0.018) | -0.079** (0.039) | -0.054* (0.030) | -0.028 (0.032) |
| Treatment group $\times t = -3$ | 0.009 (0.007) | -0.023 (0.023) | -0.019 (0.019) | -0.003 (0.020) | -0.017 (0.014) | -0.006 (0.037) | -0.018 (0.031) | -0.064* (0.035) | -0.026* (0.016) | -0.049 (0.036) | -0.007 (0.028) | 0.027 (0.031) |
| Treatment group $\times t = -2$ | 0.003 (0.006) | -0.013 (0.019) | -0.005 (0.016) | 0.012 (0.018) | -0.025** (0.011) | 0.012 (0.029) | -0.027 (0.026) | -0.053* (0.030) | -0.012 (0.014) | -0.056* (0.032) | -0.029 (0.025) | -0.009 (0.029) |
| Treatment group $\times t = 0$ | -0.008 (0.005) | 0.007 (0.017) | -0.003 (0.015) | 0.001 (0.016) | -0.004 (0.012) | 0.074** (0.029) | 0.038 (0.027) | 0.018 (0.033) | 0.010 (0.013) | 0.004 (0.028) | 0.0004 (0.024) | 0.013 (0.028) |
| Treatment group $\times t = 1$ | -0.004 (0.008) | 0.017 (0.022) | 0.011 (0.019) | 0.022 (0.020) | 0.003 (0.016) | 0.127*** (0.037) | 0.082** (0.032) | 0.044 (0.039) | 0.044*** (0.016) | 0.040 (0.036) | 0.070** (0.030) | 0.102*** (0.034) |
| Treatment group $\times t = 2$ | 0.014 (0.009) | 0.025 (0.025) | 0.022 (0.021) | 0.028 (0.022) | 0.022 (0.017) | 0.105*** (0.040) | 0.060* (0.032) | 0.041 (0.037) | 0.041** (0.019) | 0.061 (0.038) | 0.075** (0.033) | 0.103*** (0.038) |
| Treatment group $\times t = 3$ | 0.011 (0.010) | 0.025 (0.028) | 0.016 (0.024) | 0.014 (0.024) | 0.007 (0.019) | 0.074* (0.043) | 0.026 (0.036) | -0.038 (0.041) | 0.023 (0.021) | -0.0007 (0.042) | 0.031 (0.036) | 0.026 (0.040) |
| Treatment group $\times t = 4$ | 0.006 (0.010) | 0.031 (0.028) | 0.026 (0.024) | 0.003 (0.025) | 0.020 (0.021) | 0.050 (0.045) | 0.016 (0.040) | -0.017 (0.046) | 0.048** (0.023) | -0.072* (0.043) | -0.016 (0.038) | 0.005 (0.042) |
| <i>Fixed effects</i> | | | | | | | | | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1,012,919 | 978,079 | 981,022 | 1,012,934 | 251,704 | 241,716 | 242,748 | 251,706 | 272,975 | 259,849 | 261,997 | 272,928 |
| R ² | 0.71 | 0.83 | 0.85 | 0.83 | 0.54 | 0.84 | 0.86 | 0.82 | 0.75 | 0.80 | 0.84 | 0.80 |

Table B.4: Dynamic effects on employment, outputs, inputs, and value added by firm size including never treated firms in control group

This table reports dynamic OLS estimates of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size one year before treatment. The outcome variables are labor demand (in log total employees) in columns (1), (5), and (9), total outputs in columns (2), (6), and (10), total material inputs in columns (3), (7), and (11) and value added in columns (4), (8), and (12) (all in log IDR). These are coefficients from Equation 3. This table reports estimates and standard errors (in parentheses) clustered at the village-level where the treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. We assign firms to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit level, and an island-year-firm group FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) |
|--|---------------------|---------------------|---------------------|---------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST (Smaller firms) | -0.014* (0.009) | -0.013 (0.022) | -0.017 (0.019) | -0.021 (0.018) |
| Treatment group*POST (Medium-size firms) | 0.045*** (0.016) | 0.107*** (0.032) | 0.068** (0.028) | 0.030 (0.030) |
| Treatment group*POST (Larger firms) | 0.077*** (0.017) | 0.084*** (0.032) | 0.096*** (0.027) | 0.089*** (0.029) |
| <i>Fixed effects</i> | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes | Yes |
| Observations | 1,016,237 | 976,588 | 981,283 | 1,016,131 |
| R ² | 0.93 | 0.90 | 0.92 | 0.90 |

Table B.5: Effects on employment, outputs, inputs, and value added in firms including manufacturing firms from villages which are eligible for treatment

This table reports OLS estimates of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size one year before the treatment. The outcome variables are labor demand (1) (in log total employees), total outputs (2), total material inputs (3) and value added (4) (all in log IDR). These are coefficients from Equation 2. This table reports estimates and standard errors (in parentheses) clustered at the village-level where the treatment is assigned. Different from the estimations in Table 4, we include firms in the control group from villages, in which coal-fired power plant operation was announced, but where plants never have been built.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. We assign firms to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 firms. Larger firms are those with more than 249 employees.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) |
|--|-------------------|-------------------|-------------------|--------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST (Smaller firms) | 0.003 (0.005) | -0.012 (0.015) | -0.016 (0.011) | -0.014 (0.010) |
| Treatment group*POST (Medium-size firms) | -0.009 (0.012) | -0.020 (0.022) | -0.016 (0.019) | -0.005 (0.020) |
| Treatment group*POST (Larger firms) | 0.0006 (0.012) | 0.030 (0.022) | 0.029 (0.019) | 0.017 (0.021) |
| <i>Fixed effects</i> | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes | Yes |
| Observations | 805,143 | 735,122 | 737,271 | 805,125 |
| R ² | 0.94 | 0.91 | 0.93 | 0.91 |

Table B.6: Effects on employment, outputs, inputs, and value added in firms from neighboring villages

This table reports OLS estimates of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms from villages that neighbor treated villages, differentiated by firm size one year before the treatment. Neighboring villages comprise villages that either share a border with treated villages or that are located within 100 kilometers, but not within 50 kilometers to a power plant. We remove firms from treated villages from the treatment group. The outcome variables are labor demand (1) (in log total employees), total outputs (2), total material inputs (3), and value added (4) (all in log IDR). These are coefficients from Equation 2. This table reports estimates and standard errors (in parentheses) clustered at the village-level where the treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. We assign firms to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) |
|--|----------------------|---------------------|---------------------|---------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST (Smaller firms) | -0.028*** (0.010) | -0.039 (0.024) | -0.035* (0.021) | -0.040** (0.020) |
| Treatment group*POST (Medium-size firms) | 0.018 (0.018) | 0.079** (0.036) | 0.049 (0.031) | 0.025 (0.033) |
| Treatment group*POST (Larger firms) | 0.084*** (0.019) | 0.105*** (0.033) | 0.116*** (0.028) | 0.116*** (0.031) |
| <i>Fixed effects</i> | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes | Yes |
| Observations | 721,164 | 716,886 | 720,192 | 721,221 |
| R ² | 0.93 | 0.89 | 0.91 | 0.89 |

Table B.7: Effects on employment, outputs, inputs, and value added between 1998 and 2015

This table reports dynamic OLS estimates of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size one year before the treatment. In contrast to results in Table 3, results refer to effects in cohorts between 1998 and 2015. The outcome variables are labor demand (in log total employees) in columns (1), (5), and (9), total outputs in columns (2), (6), and (10), total material inputs in columns (3), (7), and (11), and value added in columns (4), (8), and (12) (all in log IDR). These are coefficients from Equation 3. This table reports estimates and standard errors (in parentheses) clustered at the village-level where the treatment is assigned. Coefficients and 95% confidence intervals are also shown in Figure 2.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event-time. We assign firms to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms comprise firms are those than 249 employees.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit level, and an island-year-firm group FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Electricity (log kWh) (1) | Electricity (log IDR) (2) | Generator fuel (log IDR) (3) |
|--|------------------------------|------------------------------|---------------------------------|
| <i>Variable</i> | | | |
| Treatment group*POST (Smaller firms) | -0.018 (0.027) | -0.038 (0.026) | -0.021 (0.087) |
| Treatment group*POST (Medium-size firms) | 0.009 (0.043) | 0.096** (0.045) | -0.033 (0.105) |
| Treatment group*POST (Larger firms) | 0.034 (0.045) | 0.103** (0.047) | -0.107 (0.108) |
| <i>Fixed effects</i> | | | |
| Firm-firm group-FE | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes |
| Observations | 797,330 | 771,055 | 157,935 |
| R ² | 0.82 | 0.85 | 0.83 |

Table B.8: Effects of coal-fired power plant commissioning on electricity and generator fuel consumption

This table reports OLS estimates of coal-fired power plant commissioning on electricity and generator fuel consumption in incumbent and local manufacturing firms. The outcome variables are total used electricity (1) (in log kWh), total expenditures on electricity (2), and total expenditures on generator fuels (3) (both in log IDR). These are coefficients from Equation 2. This table reports estimates and standard errors (in parentheses) clustered at the village-level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample, if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Labor | | | | | | | |
|------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | Full sample | | Smaller firms | | Medium-size firms | | Larger firms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST | 0.014* (0.008) | 0.027 (0.019) | -0.019** (0.009) | 0.010 (0.028) | 0.035** (0.016) | 0.021 (0.033) | 0.076*** (0.018) | 0.088*** (0.032) |
| Electricity (log IDR) | 0.086*** (0.003) | | 0.068*** (0.002) | | 0.059*** (0.003) | | 0.074*** (0.004) | |
| Generator fuel (log IDR) | | 0.047*** (0.003) | | 0.043*** (0.004) | | 0.030*** (0.003) | | 0.042*** (0.004) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (2-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 770,997 | 157,925 | 465,234 | 68,989 | 146,897 | 38,520 | 158,866 | 50,416 |
| R ² | 0.91 | 0.94 | 0.72 | 0.81 | 0.54 | 0.68 | 0.75 | 0.85 |

Table B.9: Effects of changes in electricity and generator fuel consumption, and coal-fired power plant commissioning on employment

This table reports OLS estimates of electricity consumption (in log IDR) and coal-fired power plant commissioning in columns (1), (3), (5), and (7), and ii) of generator fuel consumption (in log IDR) and coal-fired power plant commissioning in columns (2), (4), (6), and (8). The outcome variable is employment (in log number of workers) in incumbent and local manufacturing firms. These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village-level where treatment is assigned. Note that information about generator fuel consumption is missing for a large fraction of our sample.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample, if they are located in villages, which were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit-level, an island-year-firm group FE and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Input | | | | | | | |
|------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | Full sample | | Smaller firms | | Medium-size firms | | Larger firms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST | 0.034** (0.015) | 0.012 (0.033) | 0.007 (0.019) | 0.008 (0.052) | 0.089*** (0.029) | 0.012 (0.060) | 0.081*** (0.029) | 0.034 (0.058) |
| Electricity (log IDR) | 0.400*** (0.007) | | 0.432*** (0.008) | | 0.378*** (0.012) | | 0.322*** (0.010) | |
| Generator fuel (log IDR) | | 0.248*** (0.008) | | 0.272*** (0.013) | | 0.240*** (0.012) | | 0.217*** (0.011) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (2-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 743,926 | 157,423 | 449,827 | 68,758 | 141,257 | 38,390 | 152,842 | 50,275 |
| R ² | 0.90 | 0.91 | 0.85 | 0.88 | 0.84 | 0.89 | 0.81 | 0.86 |

Table B.10: Effects of changes in electricity, generator fuel consumption, and coal-fired power plant commissioning on inputs

This table reports OLS estimates of electricity consumption (in log IDR) and coal-fired power plant commissioning in columns (1), (3), (5), and (7), and ii) of generator fuel consumption (in log IDR) and coal-fired power plant commissioning in columns (2), (4), (6), and (8). The outcome variable is total material inputs (in log IDR) in incumbent and local manufacturing firms. These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village-level where treatment is assigned. Note that information about generator fuel consumption is missing for a large fraction of our sample.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event-time. Here, we exclude firms from our sample, if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit-level, an island-year-firm group FE and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Output | | | | | | | |
|------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | Full sample | | Smaller firms | | Medium-size firms | | Larger firms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST | 0.029** (0.013) | 0.015 (0.030) | 0.002 (0.017) | -0.013 (0.048) | 0.068*** (0.025) | 0.038 (0.054) | 0.087*** (0.026) | 0.056 (0.051) |
| Electricity (log IDR) | 0.331*** (0.006) | | 0.352*** (0.007) | | 0.315*** (0.010) | | 0.262*** (0.008) | |
| Generator fuel (log IDR) | | 0.206*** (0.007) | | 0.226*** (0.011) | | 0.198*** (0.010) | | 0.176*** (0.009) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (2-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 746,042 | 157,799 | 450,995 | 68,915 | 141,699 | 38,483 | 153,348 | 50,401 |
| R ² | 0.91 | 0.93 | 0.87 | 0.90 | 0.85 | 0.89 | 0.84 | 0.88 |

Table B.11: Effects of changes in electricity, generator fuel consumption, and coal-fired power plant commissioning on outputs

This table reports OLS estimates of electricity consumption (in log IDR) and coal-fired power plant commissioning in columns (1), (3), (5), and (7), and ii) of generator fuel consumption (in log IDR) and coal-fired power plant commissioning in columns (2), (4), (6), and (8). The outcome variable is total outputs (in log IDR) in incumbent and local manufacturing firms. These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village-level where treatment is assigned. Note that information about generator fuel consumption is missing for a large fraction of our sample.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event-time. Here, we exclude firms from our sample, if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit-level, an island-year-firm group FE and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Value added | | | | | | | |
|------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | Full sample | | Smaller firms | | Medium-size firms | | Larger firms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST | 0.027* (0.014) | 0.011 (0.034) | -0.0009 (0.017) | -0.037 (0.057) | 0.051* (0.029) | 0.033 (0.061) | 0.086*** (0.028) | 0.069 (0.059) |
| Electricity (log IDR) | 0.264*** (0.005) | | 0.277*** (0.006) | | 0.241*** (0.008) | | 0.201*** (0.007) | |
| Generator fuel (log IDR) | | 0.149*** (0.007) | | 0.162*** (0.010) | | 0.137*** (0.010) | | 0.124*** (0.009) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (2-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 771,055 | 157,935 | 465,244 | 68,993 | 146,899 | 38,522 | 158,912 | 50,420 |
| R ² | 0.88 | 0.89 | 0.82 | 0.85 | 0.79 | 0.82 | 0.78 | 0.83 |

Table B.12: Effects of changes in electricity, generator fuel consumption, and coal-fired power plant commissioning on value added

This table reports OLS estimates of electricity consumption (in log IDR) and coal-fired power plant commissioning in columns (1), (3), (5), and (7), and ii) of generator fuel consumption (in log IDR) and coal-fired power plant commissioning in columns (2), (4), (6), and (8). The outcome variable is total value added (in log IDR) in incumbent and local manufacturing firms. These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village-level where treatment is assigned. Note that information about generator fuel consumption is missing for a large fraction of our sample.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event-time. Here, we exclude firms from our sample, if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit-level, an island-year-firm group FE and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | SAIFI (log) (1) | SAIDI (log) (2) |
|---------------------------|--------------------|--------------------|
| <i>Variable</i> | | |
| Treatment group*POST | -0.040 (0.078) | 0.007 (0.123) |
| <i>Fixed effects</i> | | |
| Province-FE | Yes | Yes |
| Island-year-FE | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes |
| Event time-cohort-FE | Yes | Yes |
| Year t = 0 | Yes | Yes |
| Observations | 1,179 | 1,179 |
| R ² | 0.80 | 0.79 |

Table B.13: Effects on province-level SAIDI and SAIFI between 2010 and 2020

This table reports OLS estimates of coal-fired power plant commissioning on the System Average Interruption Duration Index (SAIDI, in log) and on the System Average Interruption Frequency Index (SAIFI, in log). These are coefficients from a regression similar to Equation 1, but on province-level outcomes instead of on firm-level outcomes. This table reports estimates and standard errors (in parentheses) clustered at the province level where treatment is assigned.

The unit of observation is an Indonesian province for which we have annual observations. We assign province-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time.

All estimations include a set of fixed effects, namely province FE, event time-cohort FE, treatment group-cohort FE, and a treatment group-year Zero FE.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Sample | All observations | | | | Treated ever | | | |
|--|---------------------|--------------------|--------------------|--------------------|---------------------|----------------------|----------------------|--------------------|
| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) | Labor (5) | Input (6) | Output (7) | Value added (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST (Smaller firms) | 0.026*** (0.007) | -0.006 (0.018) | 0.005 (0.015) | 0.015 (0.014) | 0.015* (0.008) | -0.076*** (0.025) | -0.051*** (0.019) | -0.034* (0.019) |
| Treatment group*POST (Medium-size firms) | 0.065*** (0.012) | 0.068** (0.027) | 0.055** (0.023) | 0.048** (0.024) | 0.035** (0.015) | 0.022 (0.034) | 0.010 (0.029) | -0.020 (0.031) |
| Treatment group*POST (Larger firms) | 0.068*** (0.013) | 0.038 (0.028) | 0.020 (0.024) | 0.024 (0.024) | 0.046*** (0.017) | 0.046 (0.036) | 0.006 (0.030) | 0.002 (0.030) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| All observations | Yes | Yes | Yes | Yes | No | No | No | No |
| Treated ever | No | No | No | No | Yes | Yes | Yes | Yes |
| Observations | 1,832,372 | 1,719,529 | 1,725,710 | 1,832,172 | 605,591 | 557,936 | 560,399 | 605,454 |
| R ² | 0.92 | 0.90 | 0.92 | 0.90 | 0.94 | 0.91 | 0.92 | 0.90 |

Table B.14: Effects of gas-fired power plant commissioning on employment, outputs, inputs, and value added differentiated by firm size

This table reports OLS estimates of gas-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size one year before treatment. The outcome variables are labor demand (1) (in log total employees), total material inputs (2), total outputs (3), and value added (4) (all in log IDR). These are coefficients from Equation 2. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event-time. Here, we exclude firms from our sample, if they are located in villages that were never treated. We assign firms to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees. Note that we effectively remove firms from cohorts with missing information about employment one year before the treatment year ($\tau = -1$).

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Sample | All observations | | | | Treated ever | | | |
|--|-------------------|--------------------|--------------------|--------------------|------------------|---------------------|---------------------|--------------------|
| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) | Labor (5) | Input (6) | Output (7) | Value added (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST (Smaller firms) | 0.009 (0.009) | 0.013 (0.023) | 0.021 (0.019) | 0.018 (0.017) | 0.004 (0.010) | 0.032 (0.028) | 0.038 (0.024) | 0.048** (0.023) |
| Treatment group*POST (Medium-size firms) | 0.041* (0.022) | 0.105** (0.042) | 0.077** (0.037) | 0.012 (0.035) | 0.034 (0.031) | 0.185*** (0.059) | 0.149*** (0.050) | 0.058 (0.048) |
| Treatment group*POST (Larger firms) | 0.043* (0.022) | 0.101** (0.049) | 0.079* (0.042) | 0.094** (0.039) | 0.026 (0.029) | 0.019 (0.058) | 0.031 (0.047) | 0.093** (0.046) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| All observations | Yes | Yes | Yes | Yes | No | No | No | No |
| Treated ever | No | No | No | No | Yes | Yes | Yes | Yes |
| Observations | 1,453,040 | 1,343,661 | 1,349,272 | 1,452,922 | 322,202 | 299,951 | 301,099 | 322,213 |
| R ² | 0.92 | 0.90 | 0.92 | 0.90 | 0.93 | 0.91 | 0.93 | 0.92 |

Table B.15: Effects of hydropower plant commissioning on employment, outputs, inputs, and value added differentiated by firm size

This table reports OLS estimates of hydropower plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size one year before treatment. The outcome variables are labor demand (1) (in log total employees), total material inputs (2), total outputs (3), and value added (4) (all in log IDR). These are coefficients from Equation 2. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample, if they are located in villages that were never treated. We assign firms to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees. Note that we effectively remove firms from cohorts with missing information about employment one year before the treatment year ($\tau = -1$).

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Sample | All observations | | | | Treated ever | | | |
|--|-------------------|-------------------|--------------------|---------------------|---------------------|---------------------|---------------------|----------------------|
| Dependent variable | Labor (1) | Input (2) | Output (3) | Value added (4) | Labor (5) | Input (6) | Output (7) | Value added (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST (Smaller firms) | 0.003 (0.007) | 0.015 (0.024) | 0.014 (0.018) | 0.010 (0.017) | -0.011 (0.013) | 0.018 (0.029) | 0.010 (0.025) | 0.008 (0.026) |
| Treatment group*POST (Medium-size firms) | -0.011 (0.017) | -0.039 (0.031) | -0.044* (0.027) | -0.061** (0.029) | -0.050** (0.025) | -0.072 (0.058) | -0.071 (0.044) | -0.093** (0.046) |
| Treatment group*POST (Larger firms) | -0.008 (0.017) | 0.009 (0.029) | 0.009 (0.027) | 0.007 (0.028) | -0.050** (0.024) | -0.087** (0.044) | -0.083** (0.038) | -0.109*** (0.040) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| All observations | Yes | Yes | Yes | Yes | No | No | No | No |
| Treated ever | No | No | No | No | Yes | Yes | Yes | Yes |
| Observations | 1,192,607 | 1,161,092 | 1,166,149 | 1,192,631 | 174,359 | 167,456 | 168,573 | 174,384 |
| R ² | 0.93 | 0.90 | 0.92 | 0.90 | 0.93 | 0.91 | 0.93 | 0.93 |

Table B.16: Effects of geothermal power plant commissioning on employment, outputs, inputs, and value added differentiated by firm size

This table reports OLS estimates of geothermal power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size one year before treatment. The outcome variables are labor demand (1) (in log total employees), total material inputs (2), total outputs (3), and value added (4) (all in log IDR). These are coefficients from Equation 2. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample, if they are located in villages that were never treated. We assign firms to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees. Note that we effectively remove firms from cohorts with missing information about employment one year before the treatment year ($\tau = -1$).

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Transported materials (log t) | | | |
|----------------------------|-------------------------------|------------------|-------------------|-------------------|
| Sample | Full sample | Smaller firms | Medium-size firms | Larger firms |
| | (1) | (2) | (3) | (4) |
| <i>Variable</i> | | | | |
| Treatment group*POST | 0.051 (0.038) | 0.040 (0.047) | 0.040 (0.086) | 0.152* (0.090) |
| <i>Fixed effects</i> | | | | |
| Firm-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes |
| Industry (5-digit)-year-FE | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes |
| Observations | 463,313 | 287,677 | 87,319 | 88,317 |
| R ² | 0.77 | 0.77 | 0.77 | 0.75 |

Table B.17: Effects of coal-fired power plant commissioning on transportation demand

This table reports OLS estimates of coal-fired power plant commissioning on transportation demand (in log t) in incumbent and local manufacturing firms. These are coefficients from Equation 1 and 2. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned. We report estimates for the entire sample (columns 1-2), smaller firms (columns 3-4), medium-size firms (columns 5-6), and larger firms (columns 7-8).

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample, if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Input Full sample (1) | Output (2) | Input Smaller firms (3) | Output firms (4) | Input Medium-size firms (5) | Output (6) | Input Larger firms (7) | Output firms (8) |
|-------------------------------|-----------------------------|---------------------|-------------------------------|------------------------|-----------------------------------|---------------------|------------------------------|------------------------|
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST | -0.021 (0.018) | -0.018 (0.015) | -0.033 (0.023) | -0.031 (0.020) | 0.015 (0.040) | 0.008 (0.035) | 0.019 (0.036) | 0.040 (0.032) |
| Transported materials (log t) | 0.128*** (0.003) | 0.101*** (0.003) | 0.134*** (0.004) | 0.105*** (0.003) | 0.106*** (0.005) | 0.084*** (0.004) | 0.112*** (0.006) | 0.086*** (0.005) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (5-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 461,820 | 462,757 | 286,764 | 287,280 | 87,001 | 87,236 | 88,055 | 88,241 |
| R ² | 0.90 | 0.92 | 0.85 | 0.86 | 0.85 | 0.86 | 0.85 | 0.86 |

Table B.18: Effects of changes in transportation demand and coal-fired power plant commissioning on inputs and outputs

This table reports OLS estimates of changes in transportation demand (in log t) and coal-fired power plant commissioning on incumbent and local manufacturing firms. The outcome variables are total material inputs (in log IDR) in columns (1), (3), (5), and (7), and total outputs (in log IDR) in columns (2), (4), (6), and (8). These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample, if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Villages with asphalt road (%) (1) | Villages with soil road (%) (2) | Villages with good road quality (%) (3) |
|---------------------------|---------------------------------------|------------------------------------|--|
| <i>Variable</i> | | | |
| Treatment group*POST | -0.027*** (0.010) | 0.010 (0.006) | 0.0008 (0.003) |
| <i>Fixed effects</i> | | | |
| Event time-cohort-FE | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes |
| Sub-District-FE | Yes | Yes | Yes |
| Observations | 50,445 | 50,445 | 50,445 |
| R ² | 0.78 | 0.66 | 0.60 |

Table B.19: Effects on transportation infrastructure

This table reports OLS estimates of coal-fired power plant commissioning on sub-district-level transportation infrastructure. The outcome variables are the share of villages in which the main road is an asphalt road (1) (in %), the share of villages in which the main road is a soil road (2) (in %), and the share of villages with roads that can be passed with motorized four-wheel vehicles all year (3) (in %). These are coefficients from a regression similar to Equation 1. This table reports estimates and standard errors (in parentheses) clustered at the sub-district level (columns (1) to (3)) and district-level (column (4)), i.e., where treatment is assigned.

For columns (1) to (3), the unit of observation is an Indonesian sub-district (*kecamatan*) for which we have annual observations. We assign sub-district-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude sub-districts from our sample, if they were never treated. Data comes from the PODES dataset.

All estimations include a set of fixed effects, namely sub-district-FE or district FE, event time-cohort FE, treatment group-cohort FE, an island-year-firm group FE, and a treatment group-firm group-year Zero FE.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | All firms (1) | Smaller firms (2) | Medium-size firms (3) | Larger firms (4) |
|---------------------------|---------------------|----------------------|--------------------------|---------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST | 0.042*** (0.009) | 0.030*** (0.007) | 0.007*** (0.002) | 0.005*** (0.002) |
| <i>Fixed effects</i> | | | | |
| Village-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes |
| Observations | 573,543 | 573,543 | 573,543 | 573,543 |
| R ² | 0.23 | 0.21 | 0.16 | 0.19 |

Table B.20: Effects of coal-fired power plant commissioning on firm entries

This table reports OLS estimates of coal-fired power plant commissioning on the number of entering manufacturing firms at the village level. The outcome variables are the total number of newly established manufacturing firms (1), the number of newly established smaller firms (20 to 99 employees) (2), the number of newly established medium-size firms (100 to 249 employees) (3), and the total number of larger firms (more than 249 employees) (4). We define the establishment of firms as the first year in which they appear in the Indonesian Manufacturing Census, which covers all Indonesian firms with more than 20 employees.

These are coefficients from a difference-in-differences estimation similar to Equation 1. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned. The unit of observation is an Indonesian village (*desa*) for which we have yearly observations. We assign village-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude villages from our sample that were never treated.

All estimations include a set of fixed effects, namely village FE, event time-cohort FE, treatment group-cohort FE, an province-year FE, and a treatment group-year Zero FE.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Labor | | | | | | | | | | | | | | | |
|----------------------------|-----------------------|-----------------------|----------------------|---------------------|----------------------|-----------------------|---------------------|---------------------|-----------------------|----------------------|----------------------|--------------------|-----------------------|---------------------|----------------------|----------------------|
| Sample | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
| <i>Variable</i> | | | | | | | | | | | | | | | | |
| Treatment group*POST | 0.013 (0.008) | 0.013 (0.008) | 0.013 (0.008) | 0.013 (0.008) | -0.022** (0.009) | -0.021** (0.009) | -0.021** (0.009) | -0.021** (0.009) | 0.035** (0.016) | 0.035** (0.016) | 0.035** (0.017) | 0.035** (0.017) | 0.077*** (0.017) | 0.076*** (0.017) | 0.077*** (0.017) | 0.076*** (0.017) |
| Entry of firms | -0.002*** (0.0006) | | | | -0.001** (0.0005) | | | | -0.003*** (0.0008) | | | | -0.003*** (0.0009) | | | |
| Entry of smaller firms | | -0.003*** (0.0008) | | | | -0.002*** (0.0006) | | | | -0.004*** (0.001) | | | | -0.002 (0.002) | | |
| Entry of medium-size firms | | | -0.008*** (0.002) | | | | 0.001 (0.002) | | | | -0.008*** (0.003) | | | | -0.011*** (0.003) | |
| Entry of larger firms | | | | -0.003** (0.001) | | | | 0.0001 (0.002) | | | | -0.003 (0.002) | | | | -0.005*** (0.002) |
| <i>Fixed effects</i> | | | | | | | | | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (5-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 868,518 | 868,518 | 868,518 | 868,518 | 529,073 | 529,073 | 529,073 | 529,073 | 161,569 | 161,569 | 161,569 | 161,569 | 177,876 | 177,876 | 177,876 | 177,876 |
| R ² | 0.91 | 0.91 | 0.91 | 0.91 | 0.72 | 0.72 | 0.72 | 0.72 | 0.58 | 0.58 | 0.58 | 0.58 | 0.77 | 0.77 | 0.77 | 0.77 |

Table B.21: Effects of firm entries and coal-fired power plant commissioning on employment

This table reports OLS estimates of firm entries and coal-fired power plant commissioning on employment (in log number of employees) in incumbent and local manufacturing firms. We show such estimates for the commissioning of coal-fired power plants and i) entries for all firms in columns (1), (5), (9), and (13), ii) for smaller firms in columns (2), (6), (10), and (14), iii) for medium-size firms in columns (3), (7), (11), and (15), and iv) for larger firms in columns (4), (8), (12), and (16). These are coefficients from Equation 4. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Wages (prod.) | Wages (other) | Wages (prod.) | Wages (other) | Wages (prod.) | Wages (other) | Wages (prod.) | Wages (other) |
|----------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|--------------------|------------------|------------------|
| Sample | Full sample | | Smaller firms | | Medium-size firms | | Larger firms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST | 0.042*** (0.010) | 0.054*** (0.016) | 0.048*** (0.013) | 0.067*** (0.021) | 0.042*** (0.016) | 0.052** (0.026) | 0.019 (0.018) | 0.020 (0.029) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (2-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 865,557 | 741,421 | 527,075 | 419,383 | 161,050 | 151,954 | 177,432 | 170,084 |
| R ² | 0.78 | 0.81 | 0.80 | 0.84 | 0.80 | 0.82 | 0.76 | 0.77 |

Table B.22: Effects of coal-fired power plant commissioning on wages

This table reports OLS estimates of coal-fired power plant commissioning on wages in incumbent and local manufacturing firms. The outcome variables are wages for production workers in columns (1), (3), (5), and (7) (in log IDR) and wages for non-production workers in columns (2), (4), (6), and (8) (in log IDR). These are coefficients from Equation 1. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned. We report estimates for the entire sample (columns 1-2), smaller firms (columns 3-4), medium-size firms (columns 5-6), and larger firms (columns 7-8).

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Labor | | | | | | | |
|------------------------------|----------------------|----------------------|---------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| | Full sample | | Smaller firms | | Medium-size firms | | Larger firms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST | 0.012 (0.008) | 0.019** (0.008) | -0.022** (0.009) | -0.016 (0.010) | 0.040** (0.016) | 0.046*** (0.016) | 0.082*** (0.018) | 0.077*** (0.018) |
| Wages (prod. - log) | -0.012*** (0.002) | | -0.005** (0.002) | | -0.025*** (0.004) | | -0.034*** (0.005) | |
| Wages (other - log) | | -0.033*** (0.003) | | -0.024*** (0.002) | | -0.041*** (0.004) | | -0.042*** (0.005) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (2-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 865,500 | 741,373 | 527,065 | 419,378 | 161,049 | 151,953 | 177,386 | 170,042 |
| R ² | 0.90 | 0.90 | 0.69 | 0.70 | 0.51 | 0.52 | 0.73 | 0.74 |

Table B.23: Effects of wage changes and coal-fired power plant commissioning on employment

This table reports i) OLS estimates of changes in wages for production workers (in log IDR) and coal-fired power plant commissioning in columns (1), (3), (5) and (7), and ii) of changes in wages for non-production workers (in log IDR) and coal-fired power plant commissioning in columns (2), (4), (6) and (8). The outcome variable is employment (in log number of workers) in incumbent and local manufacturing firms. These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | GDP p.c. (log) (1) | Population (log) (2) | Employment (log) (3) | Industrial emp. (log) (4) | Agricultural emp. (log) (5) |
|---------------------------|-----------------------|-------------------------|-------------------------|------------------------------|--------------------------------|
| <i>Variable</i> | | | | | |
| Treatment group*POST | 0.102* (0.060) | 0.031** (0.014) | 0.011 (0.017) | 0.013 (0.061) | -0.032 (0.030) |
| <i>Fixed effects</i> | | | | | |
| District-FE | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes |
| Observations | 25,083 | 42,966 | 17,969 | 17,887 | 17,912 |
| R ² | 0.99 | 0.99 | 0.99 | 0.95 | 0.96 |

Table B.24: Effects on local population and employment (district-level)

This table reports OLS estimates of coal-fired power plant commissioning on district-level (*kabupaten* and *kota*) population and employment. The outcome variables are GDP per capita (1), total population (2), total employment (3), employment in industry (4), and employment in agriculture (5) (all in log). These are coefficients from a regression similar to Equation 1. Data come from Indodapoer. This table reports estimates and standard errors (in parentheses) clustered at the district level where treatment is assigned.

The unit of observation is an Indonesian district for which we have annual observations. We assign district-year observations to treatment or control group for each cohort year. We stack different cohorts relative to event time. Here, we exclude districts from our sample if they were never treated.

All estimations include a set of fixed effects, namely district FE, event time-cohort FE, treatment group-cohort FE, an island-year FE, and a treatment group-year Zero FE.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Population (1) | Labor force (2) | Informal emp. (3) | Child labor (4) |
|---------------------------|-------------------|--------------------|----------------------|---------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST | 12.8** (5.03) | -0.003 (0.004) | -0.007 (0.006) | -0.006** (0.003) |
| <i>Fixed effects</i> | | | | |
| Sub-District-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes |
| Observations | 45,653 | 45,653 | 45,653 | 45,653 |
| R ² | 0.82 | 0.48 | 0.56 | 0.27 |

Table B.25: Effects on local population and employment (sub-district-level)

This table reports OLS estimates of coal-fired power plant commissioning on sub-district-level (*kecamatan*) population and employment. The outcome variables are total population (1), labor force (in %) (2), informal employment (in %) (3), and child labor (in %) (4). These are coefficients from a regression similar to Equation 1. Data come from SUSENAS. This table reports estimates and standard errors (in parentheses) clustered at the sub-district level where treatment is assigned.

The unit of observation is an Indonesian sub-district for which we have annual observations. We assign sub-district-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude sub-districts from our sample if they were never treated.

All estimations include a set of fixed effects, namely sub-district FE, event time-cohort FE, treatment group-cohort FE, an island-year FE, and a treatment group-year Zero FE.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable | Labor productivity (log) (1) | Output per worker (2) | TFP (LP-M) (3) | TFP (W-M) (4) |
|--|---------------------------------|--------------------------|--------------------|--------------------|
| <i>Variable</i> | | | | |
| Treatment group*POST (Smaller firms) | -0.011 (0.016) | -0.007 (0.016) | -0.035* (0.021) | -0.033 (0.021) |
| Treatment group*POST (Medium-size firms) | -0.012 (0.028) | 0.030 (0.025) | -0.011 (0.039) | -0.005 (0.039) |
| Treatment group*POST (Larger firms) | 0.022 (0.026) | 0.025 (0.024) | 0.064* (0.035) | 0.076** (0.036) |
| <i>Fixed effects</i> | | | | |
| Firm-firm group-FE | Yes | Yes | Yes | Yes |
| Event time-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Treatment group-cohort-firm group-FE | Yes | Yes | Yes | Yes |
| Industry-year-firm group-FE | Yes | Yes | Yes | Yes |
| Island-year-firm group-FE | Yes | Yes | Yes | Yes |
| Year t = 0 -firm group-FE | Yes | Yes | Yes | Yes |
| Observations | 868,412 | 835,474 | 337,565 | 339,931 |
| R ² | 0.83 | 0.86 | 0.88 | 0.88 |

Table B.26: Effects of coal-fired power plant commissioning on productivity measures

This table reports OLS estimates of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm size in $\tau = -1$. The outcome variables are labor productivity (in log) in column (1), output per worker (in log) in column (2) and total factor productivity (TFP) in columns (3) and (4). We estimate TFP following Levinsohn and Petrin (2003) (LP-M) in column (3) and following Wooldridge (2009) (W-M) in column (4) with total material inputs (IDR) as auxiliary intermediate variable, respectively. These are coefficients from Equation 2. This table reports estimates and standard errors (in parentheses) clustered at the village-level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event-time. Here, we exclude firms from our sample if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Input | | | | | | | | | | | |
|------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | (1) | Full sample (2) | (3) | Smaller firms | | (6) | Medium-size firms | | (10) | Larger firms | | (12) |
| <i>Variable</i> | | | | | | | | | | | | |
| Treatment group*POST | 0.025* (0.014) | 0.023 (0.019) | 0.018 (0.019) | -0.020 (0.019) | 0.022 (0.024) | 0.021 (0.024) | 0.101*** (0.029) | 0.065 (0.041) | 0.065 (0.041) | 0.088*** (0.028) | 0.035 (0.042) | 0.025 (0.042) |
| Labor productivity (log) | 0.516*** (0.007) | | | 0.559*** (0.008) | | | 0.455*** (0.012) | | | 0.407*** (0.012) | | |
| TFP (LP-M) | | 0.520*** (0.009) | | | 0.530*** (0.009) | | | 0.451*** (0.017) | | | 0.403*** (0.018) | |
| TFP (W-M) | | | 0.510*** (0.009) | | | 0.524*** (0.009) | | | 0.440*** (0.017) | | | 0.394*** (0.018) |
| <i>Fixed effects</i> | | | | | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (5-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 831,418 | 321,707 | 324,028 | 508,228 | 216,852 | 216,965 | 154,590 | 55,954 | 56,420 | 168,600 | 48,901 | 50,643 |
| R ² | 0.90 | 0.93 | 0.93 | 0.87 | 0.91 | 0.91 | 0.86 | 0.91 | 0.91 | 0.83 | 0.89 | 0.89 |

Table B.27: Effects of changes in productivity and coal-fired power plant commissioning on inputs

This table reports OLS estimates of changes in labor productivity (in log) and coal-fired power plant commissioning in columns (1), (4), (7) and (10), ii) of changes in total factor productivity (TFP) following Levinsohn and Petrin (2003) and coal-fired power plant commissioning in columns (2), (5), (8) and (11), and iii) of changes in total factor productivity (TFP) following Wooldridge (2009) and coal-fired power plant commissioning in columns (3), (6), (9) and (12). We estimate TFP with total material inputs (IDR) as auxiliary intermediate variable, respectively. The outcome variable is total material input (in log IDR) in incumbent and local manufacturing firms. These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village-level, where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year-observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Output | | | | | | | | | | | |
|------------------------------|---------------------|---------------------|---------------------|----------------------|---------------------|---------------------|--------------------------|---------------------|---------------------|----------------------|---------------------|---------------------|
| | (1) | Full sample (2) | (3) | Smaller firms (4) | (5) | (6) | Medium-size firms (7) | (8) | (9) | Larger firms (10) | (11) | (12) |
| <i>Variable</i> | | | | | | | | | | | | |
| Treatment group*POST | 0.020* (0.010) | 0.013 (0.013) | 0.011 (0.013) | -0.021 (0.013) | 0.015 (0.016) | 0.013 (0.016) | 0.070*** (0.021) | 0.032 (0.028) | 0.036 (0.028) | 0.093*** (0.020) | 0.028 (0.029) | 0.024 (0.029) |
| Labor productivity (log) | 0.679*** (0.005) | | | 0.725*** (0.006) | | | 0.623*** (0.009) | | | 0.583*** (0.010) | | |
| TFP (LP-M) | | 0.705*** (0.007) | | | 0.724*** (0.007) | | | 0.645*** (0.013) | | | 0.593*** (0.014) | |
| TFP (W-M) | | | 0.697*** (0.007) | | | 0.720*** (0.007) | | | 0.634*** (0.013) | | | 0.584*** (0.014) |
| <i>Fixed effects</i> | | | | | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (5-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 835,474 | 322,229 | 324,552 | 509,916 | 217,186 | 217,300 | 155,218 | 56,092 | 56,559 | 170,340 | 48,951 | 50,693 |
| R ² | 0.95 | 0.96 | 0.96 | 0.93 | 0.95 | 0.95 | 0.92 | 0.94 | 0.94 | 0.90 | 0.94 | 0.93 |

Table B.28: Effects of changes in productivity and coal-fired power plant commissioning on outputs

This table reports OLS estimates of changes in labor productivity (in log) and coal-fired power plant commissioning in columns (1), (3), (5), and (7), ii) of changes in total factor productivity (TFP) following Levinsohn and Petrin (2003) and coal-fired power plant commissioning in columns (2), (5), (8), and (11), and iii) of changes in total factor productivity (TFP) following Wooldridge (2009) and coal-fired power plant commissioning in columns (3), (6), (9), and (12). We estimate TFP with total material inputs (IDR) as auxiliary intermediate variable, respectively. The outcome variable is total outputs (in log IDR) in incumbent and local manufacturing firms. These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village-level where treatment is assigned.

The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample if they are located in villages that were never treated.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

| Dependent variable Sample | Value added | | | | | | | |
|------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | Full sample | | Smaller firms | | Medium-size firms | | Larger firms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| <i>Variable</i> | | | | | | | | |
| Treatment group*POST | 0.012 (0.008) | 0.007 (0.011) | -0.022** (0.009) | -0.025** (0.013) | 0.034** (0.016) | -0.002 (0.023) | 0.078*** (0.017) | 0.081*** (0.022) |
| Labor productivity (log) | 0.969*** (0.003) | | 0.986*** (0.002) | | 0.960*** (0.003) | | 0.931*** (0.005) | |
| Output per worker | | 0.820*** (0.005) | | 0.829*** (0.005) | | 0.822*** (0.007) | | 0.793*** (0.009) |
| <i>Fixed effects</i> | | | | | | | | |
| Firm-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Event time-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment group-cohort-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry (5-digit)-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Island-year-FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year t = 0 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 868,412 | 835,474 | 529,073 | 509,916 | 161,569 | 155,218 | 177,770 | 170,340 |
| R ² | 0.97 | 0.94 | 0.97 | 0.92 | 0.96 | 0.90 | 0.95 | 0.88 |

Table B.29: Effects of changes in productivity and coal-fired power plant commissioning on value added

This table reports OLS estimates of changes in labor productivity (in log) and coal-fired power plant commissioning in columns (1), (3), (5), and (7), and ii) of changes in output per worker (in log) and coal-fired power plant commissioning in columns (2), (4), (6), and (8). The outcome variable is value added (in log IDR) in incumbent and local manufacturing firms. These are coefficients from Equation 4. We show estimates for the full sample and differentiated by firm size in $\tau = -1$. This table reports estimates and standard errors (in parentheses) clustered at the village-level where treatment is assigned. The unit of observation is an Indonesian manufacturing firm for which we have annual observations. We assign firm-year-observations to treatment or control group for each cohort year. We stack different cohorts with respect to event-time. Here, we exclude firms from our sample if they are located in villages that were never treated.

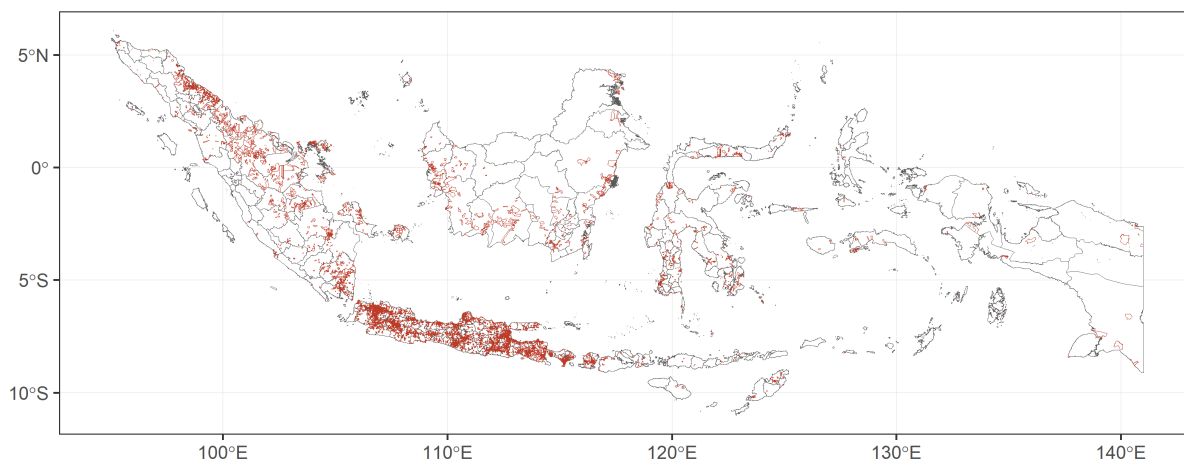
All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 5-digit-level, an island-year-firm group FE, and a treatment group-firm group-year Zero FE. As such, we interlink our standard set of fixed effects with firm group indicators.

Significance levels are: *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$

C Figures

Figure C.1: Villages, power plants, and coal mines in Indonesia

(a) Villages with manufacturing firms



(b) Commissioned coal-fired power plants between 1984 and 2020

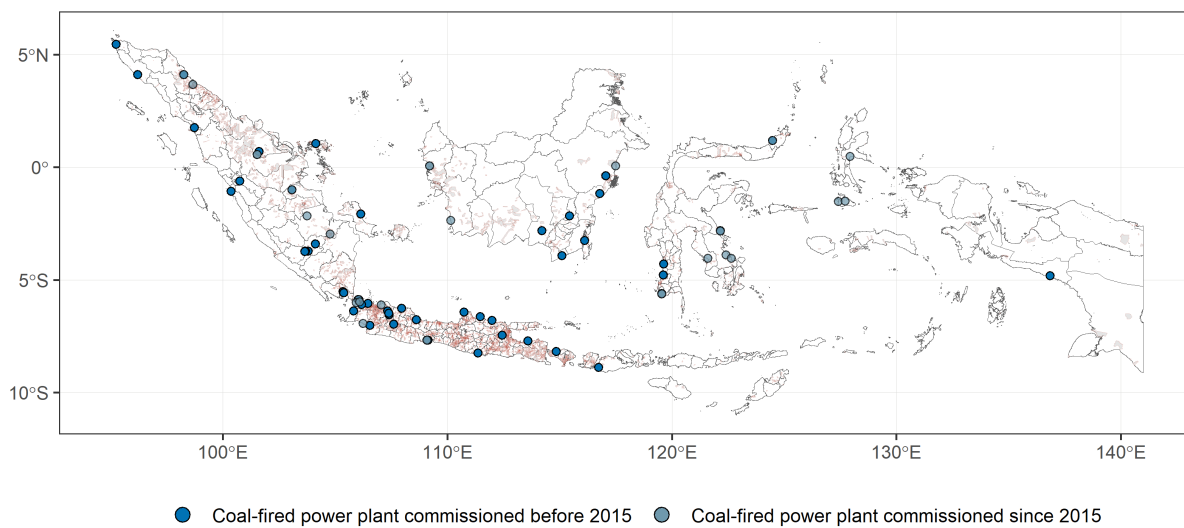
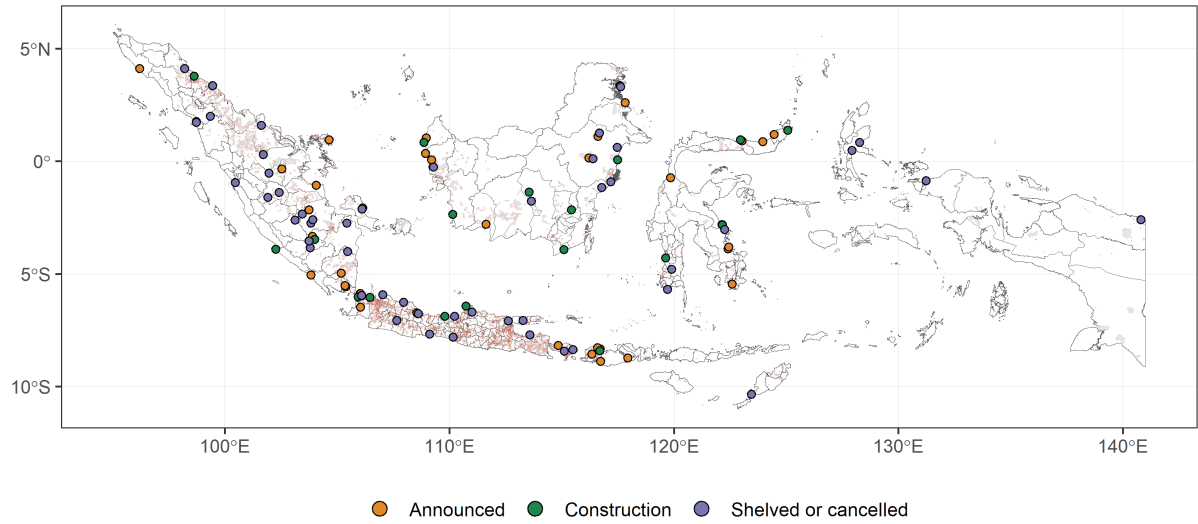


Figure C.1: Villages, power plants, and coal mines in Indonesia

(c) Coal-fired power plants in Indonesia that were announced, are under construction, were shelved, or cancelled



(d) Commissioned gas-fired, hydro-, and geothermal power plants in Indonesia between 1984 and 2015

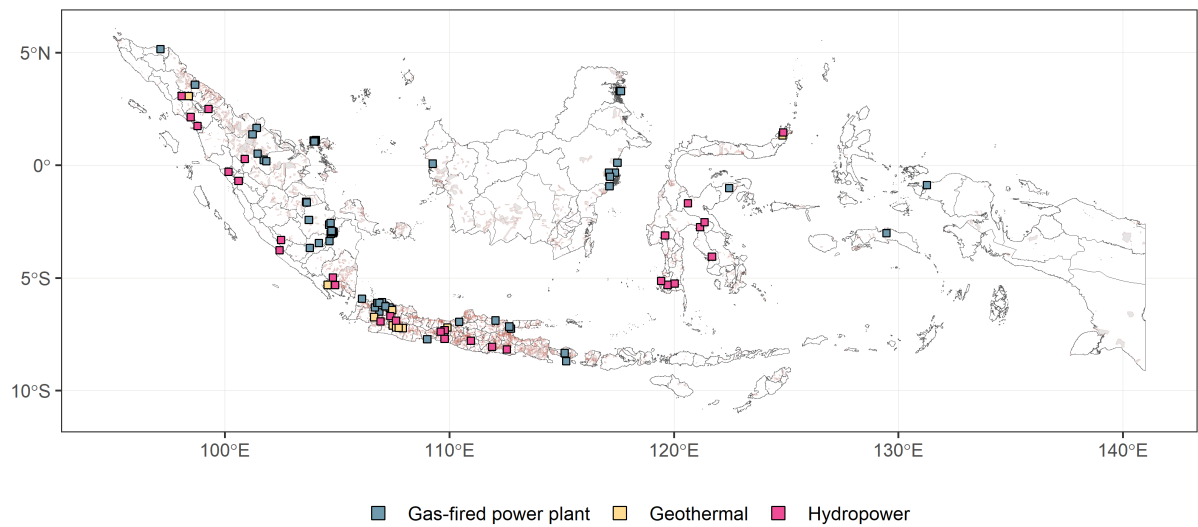
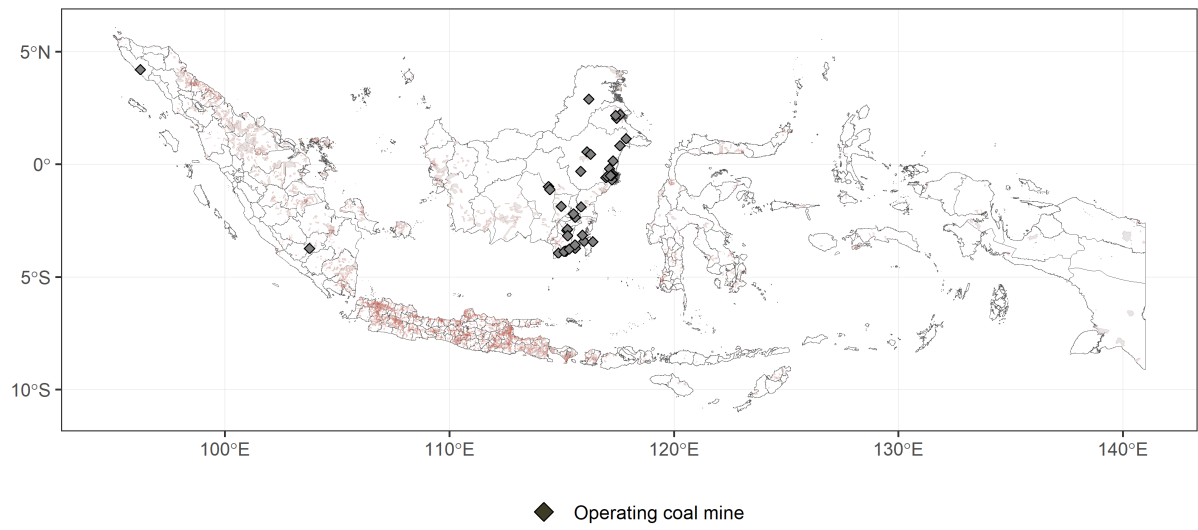


Figure C.1: Villages, power plants, and coal mines in Indonesia

(e) Operating coal mines in Indonesia



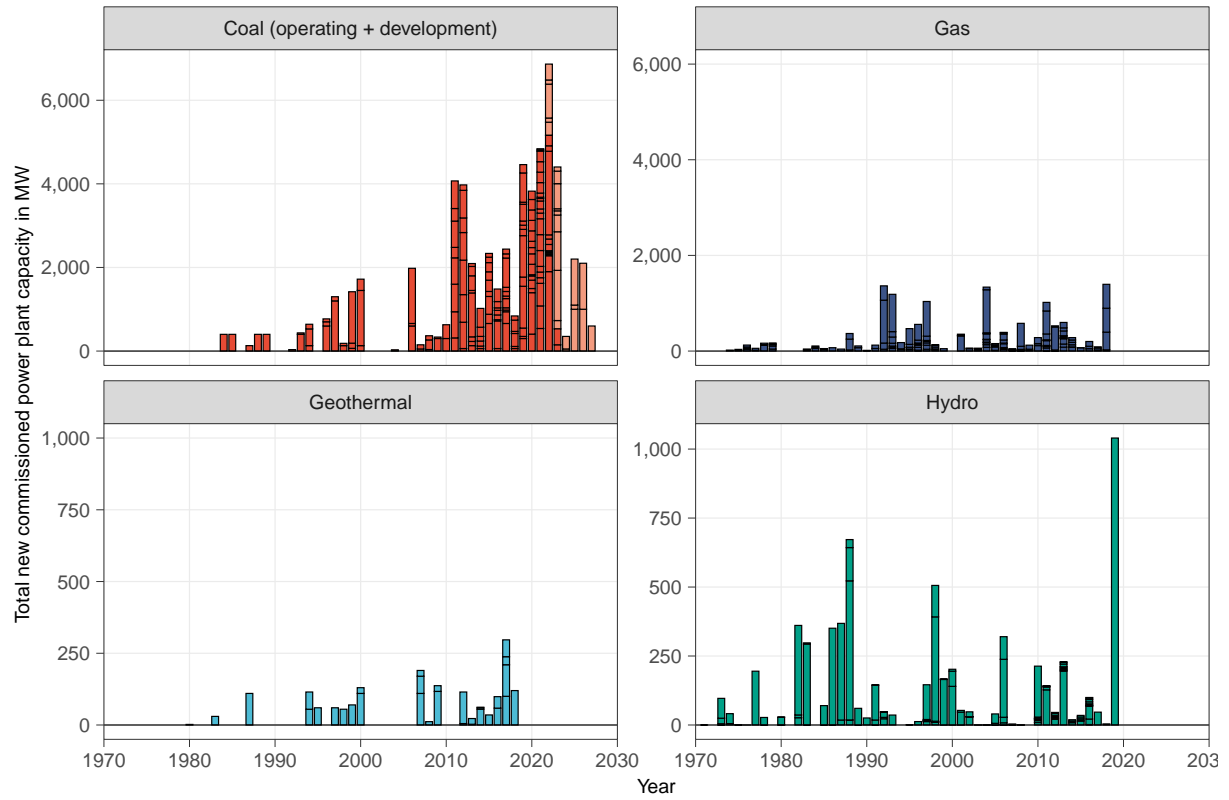


Figure C.2: Commissioned power plant capacity in Indonesia by technology

This figure shows the dispersion of power plant commissioning in our power plant sample across years and by different technologies. Data on aggregate power plant capacity (in MW) and the year of initial operation come from Global Energy Monitor (2023), Global Powerplant Database (Byers et al. 2019), and the World Electric Power Plants Database (PLATTS 2017).

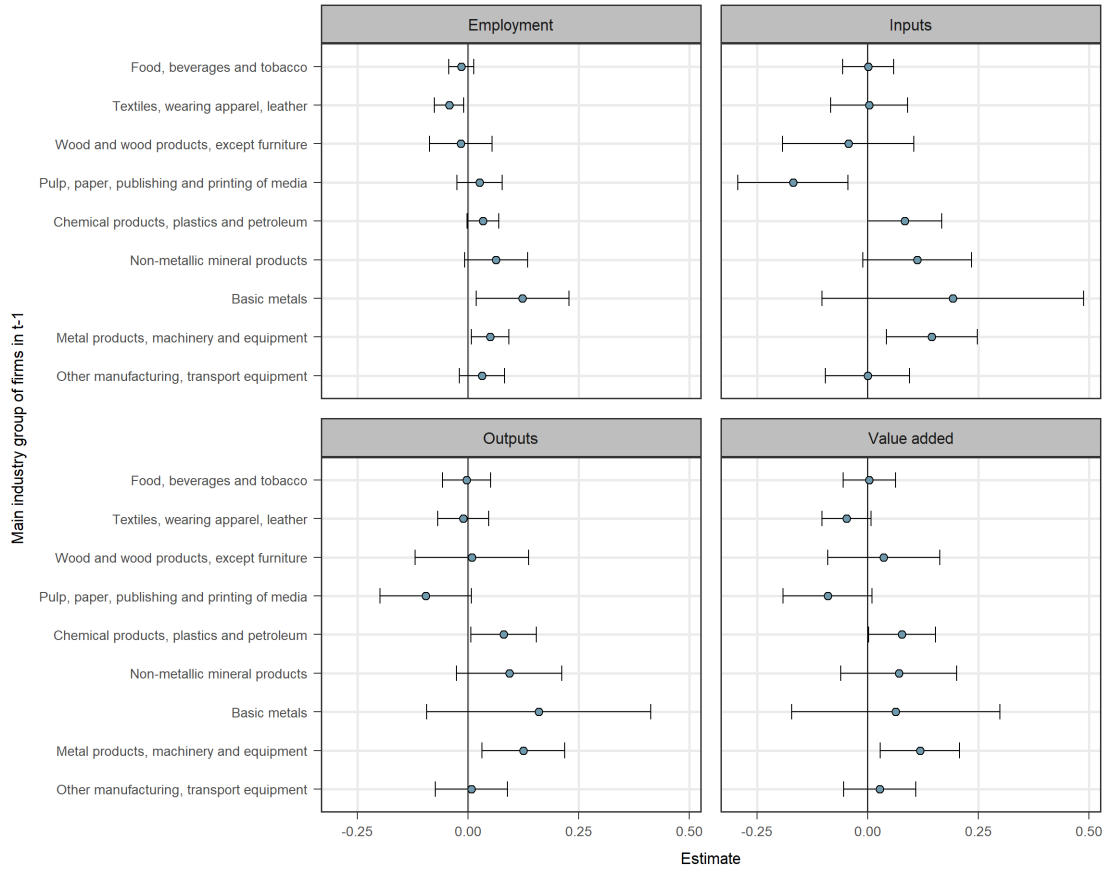


Figure C.3: Estimates of effects from coal-fired power plant commissioning on different industry groups

This figure displays estimates of coal-fired power plant commissioning on the performance of incumbent and local manufacturing firms, differentiated by firm industry one year before treatment. The outcome variables are total employment (in log total employees), total material inputs, total outputs, and value added (all in log IDR). These are coefficients from Equation 3 that we also show in table 5. Error bars display the 95% confidence interval.

We assign firm-year observations to treatment or control group for each cohort year. We stack different cohorts with respect to event time. Here, we exclude firms from our sample, if they are located in villages that were never treated. We group firms according to firm industry one year before treatment ($\tau = -1$). We use a constructed industry identifier to account for changes in industry codes over time. Note that we effectively remove firms from cohorts with missing or ambiguous information about industry one year before treatment ($\tau = -1$).

All estimations include a set of fixed effects, namely firm-industry FE, event time-cohort-industry FE, treatment group-cohort-industry FE, a firm-group-industry FE indicating firm size defined by total employment (smaller, medium-size, and larger firms), and an island-year-industry FE. As such, we interlink our standard set of fixed effects with an indicator for firm industry. Estimations do not include an industry-year FE at the 5-digit level.

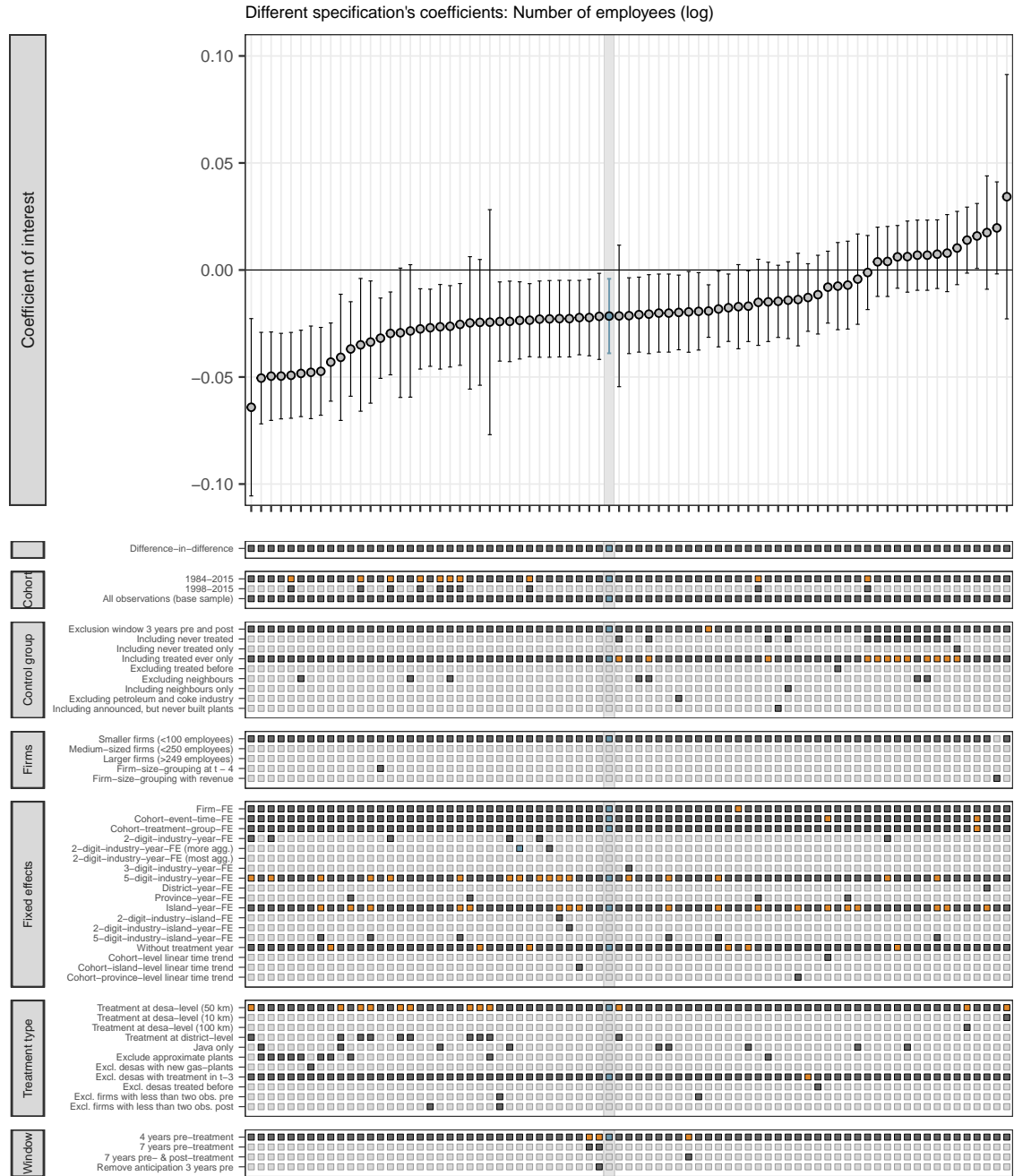


Figure C.4: Coefficients from multiple specifications: Number of employees (log) - smaller firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' labor demand in number of employees (log). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ less than 100 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

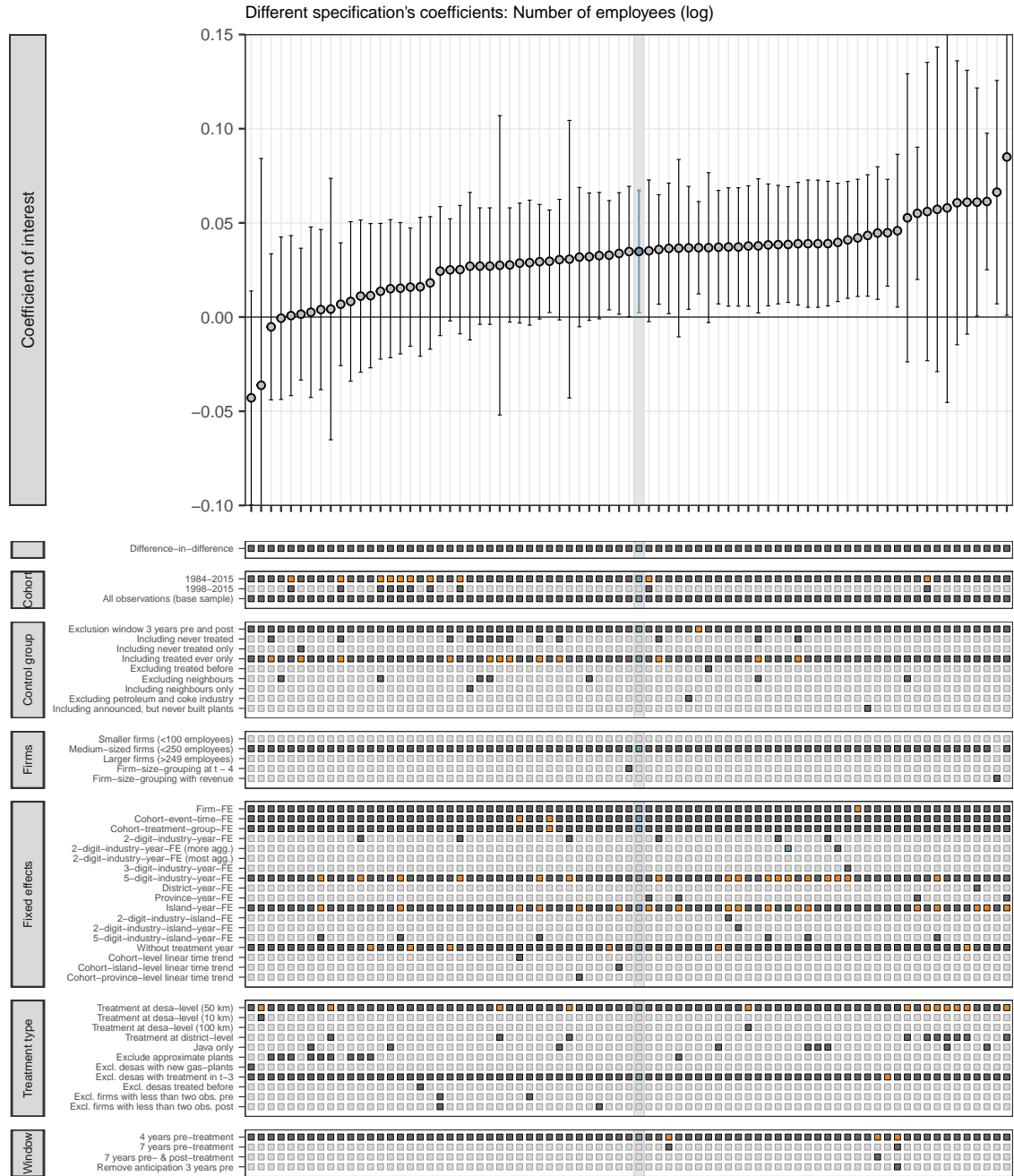


Figure C.5: Coefficients from multiple specifications: Number of employees (log) - medium-sized firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' labor demand in number of employees (log). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ between 100 and 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

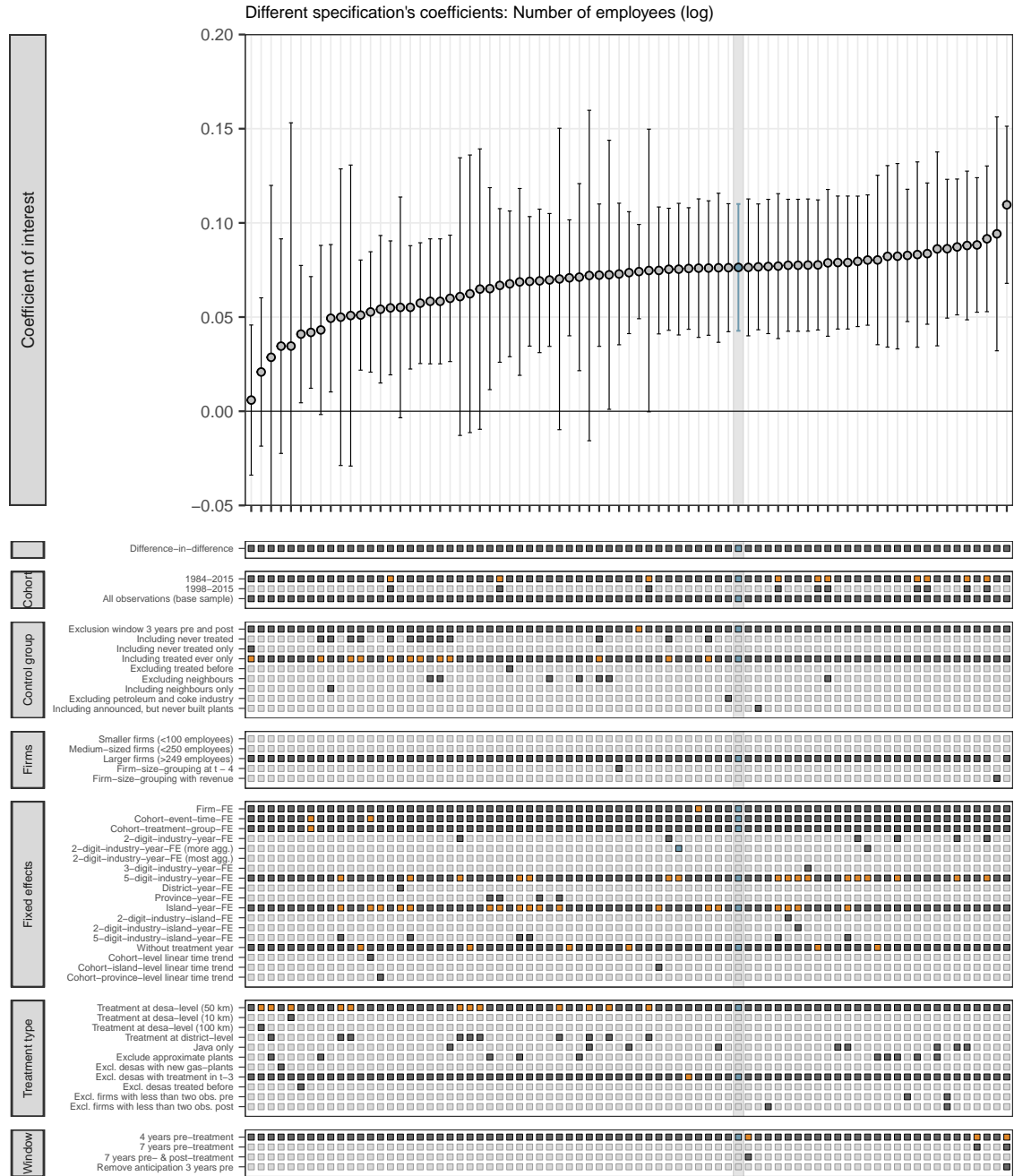


Figure C.6: Coefficients from multiple specifications: Number of employees (log) - larger firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' labor demand in number of employees (log). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ more than 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

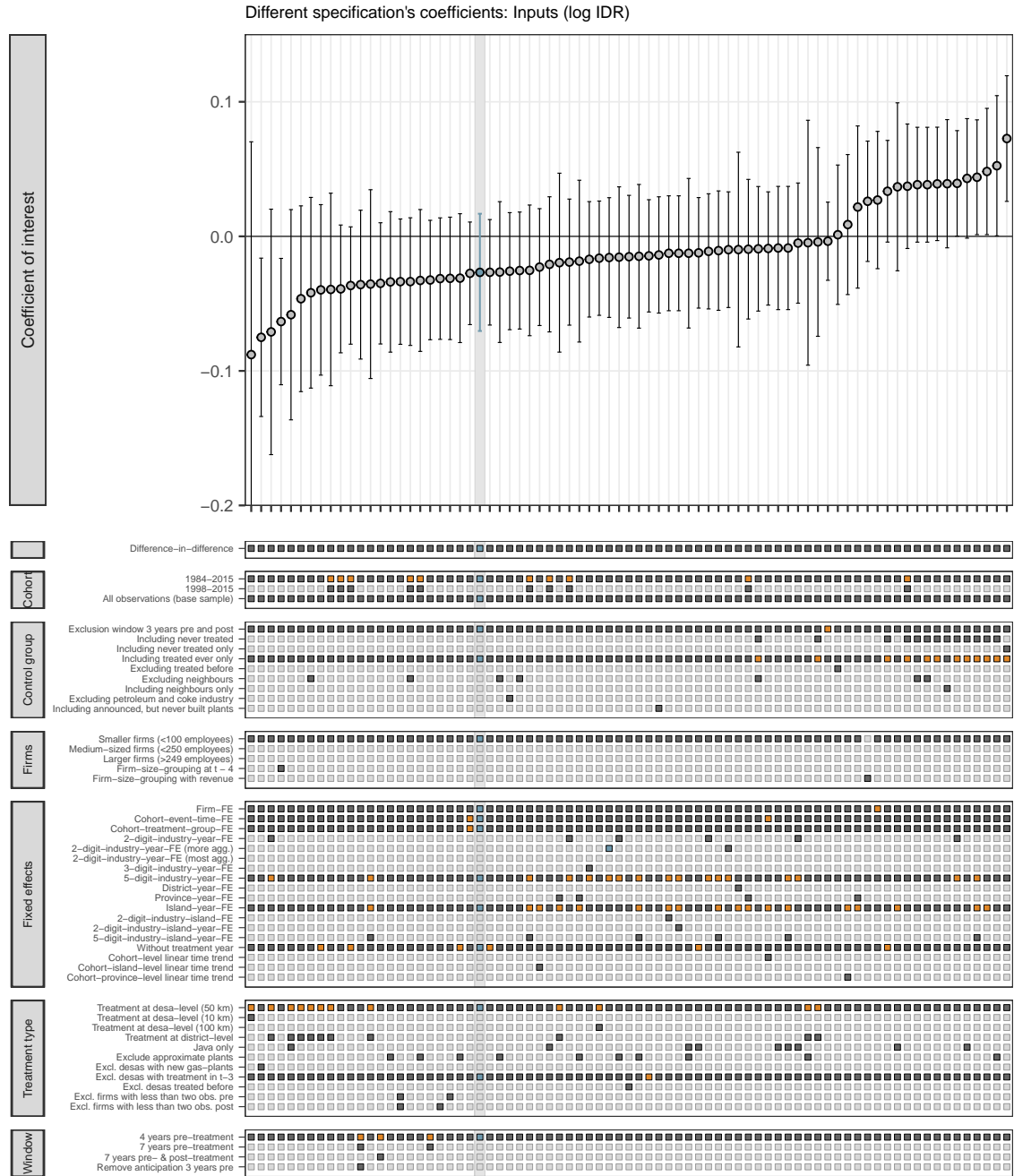


Figure C.7: Coefficients from multiple specifications: Inputs (log IDR) - smaller firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' total inputs to production (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ less than 100 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

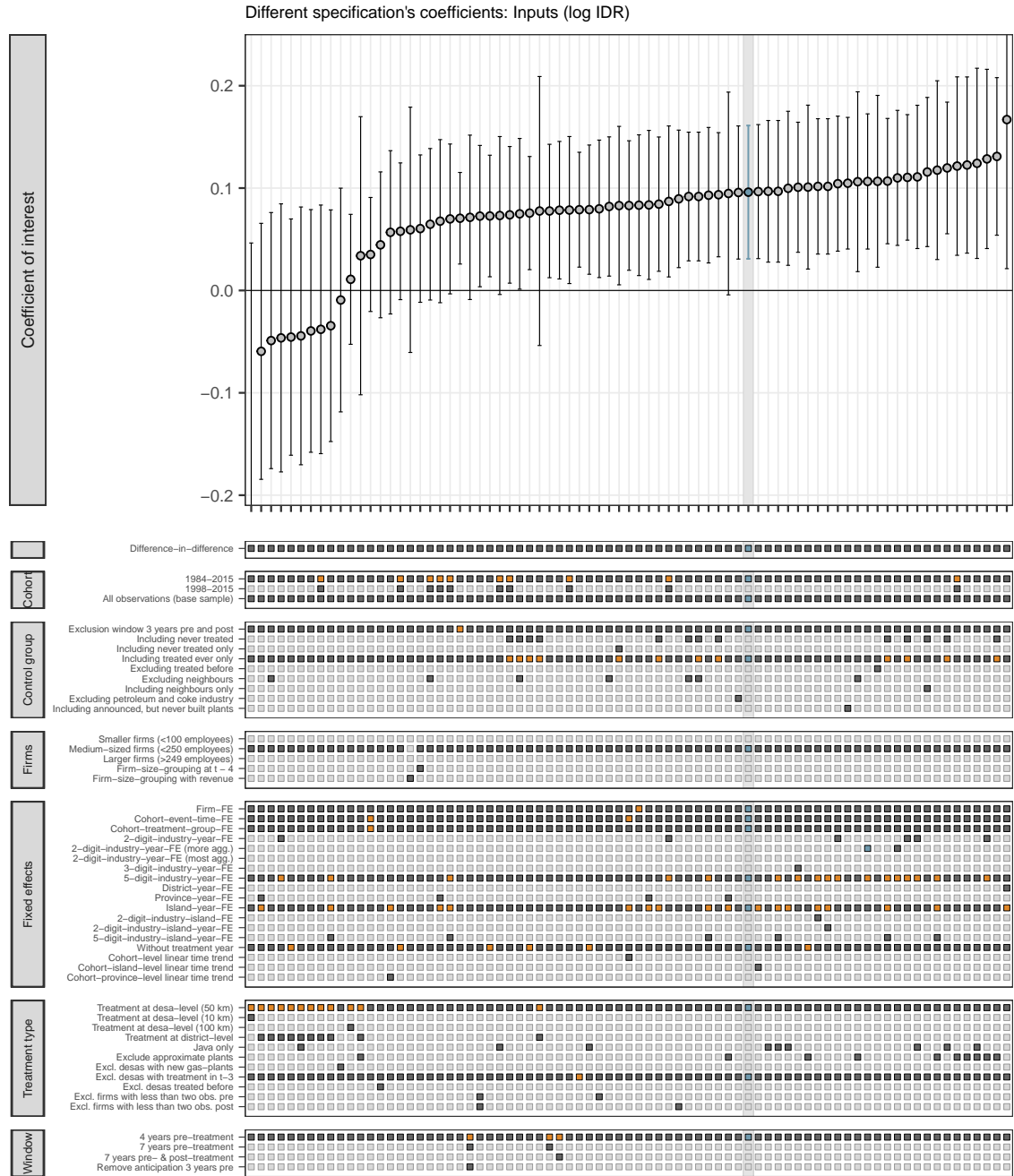


Figure C.8: Coefficients from multiple specifications: Inputs (log IDR) - medium-sized firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' total inputs to production (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ between 100 and 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

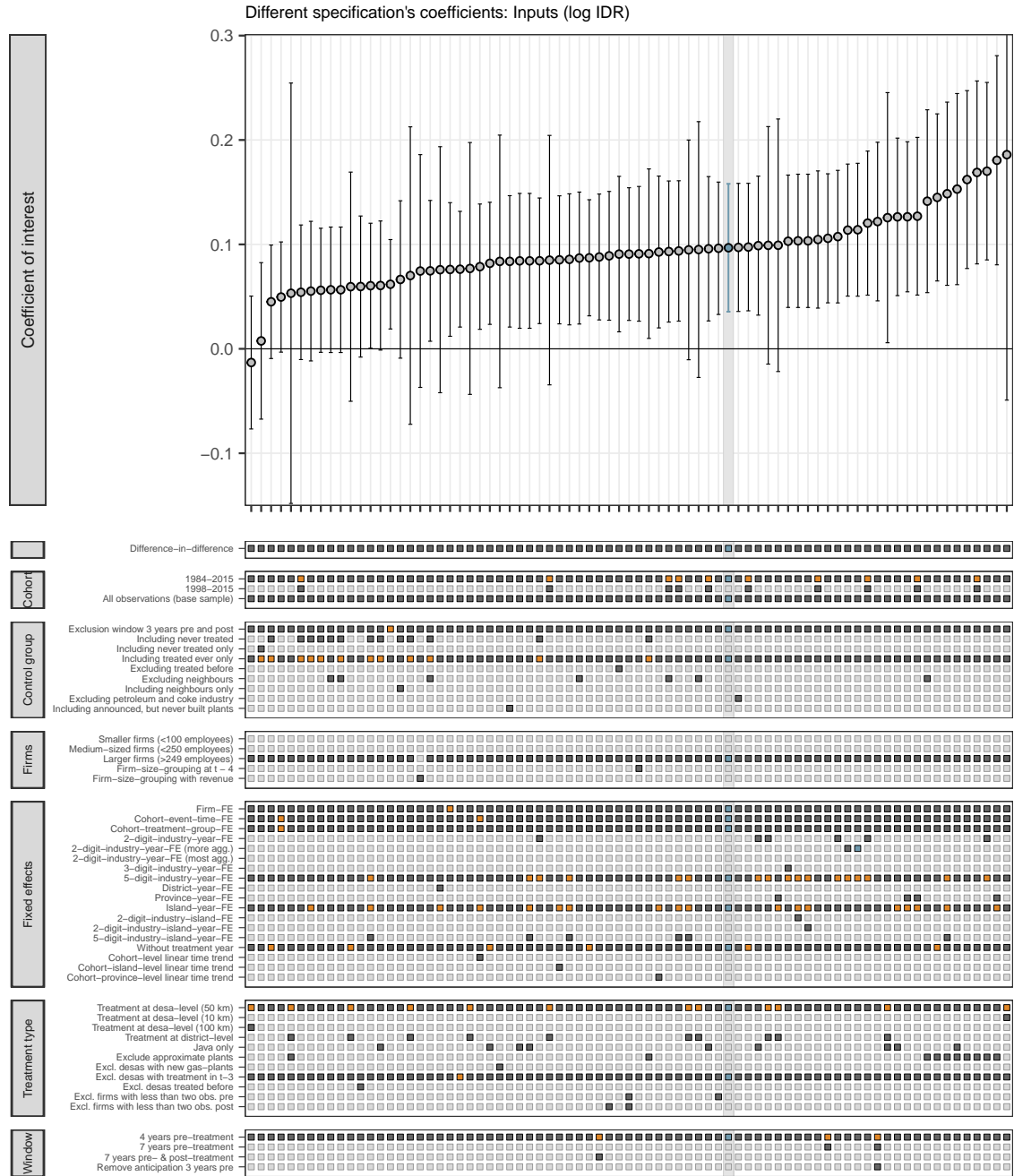


Figure C.9: Coefficients from multiple specifications: Inputs (log IDR) - larger firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' total inputs to production (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ more than 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

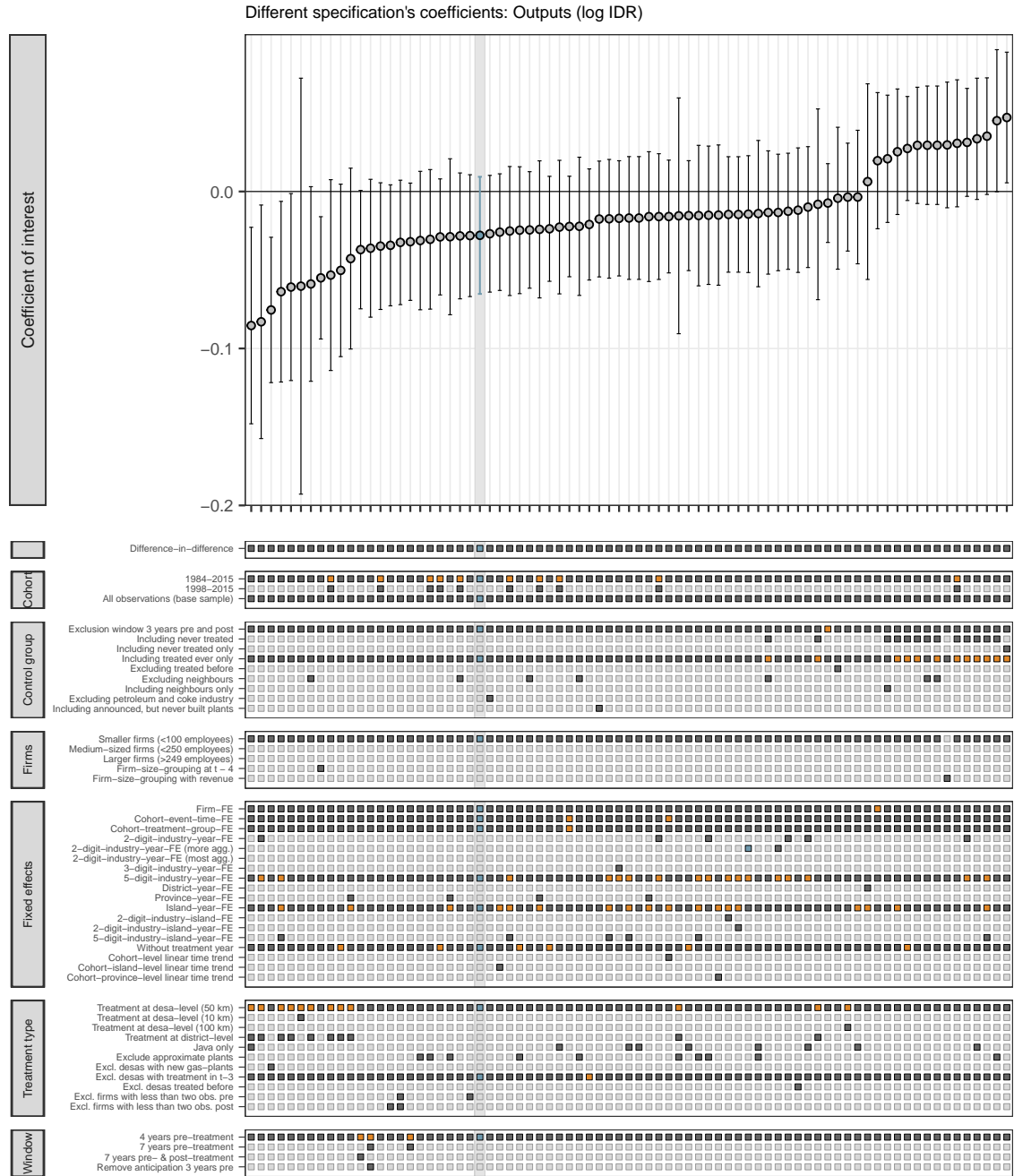


Figure C.10: Coefficients from multiple specifications: Outputs (log IDR) - smaller firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' total production outputs (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ less than 100 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

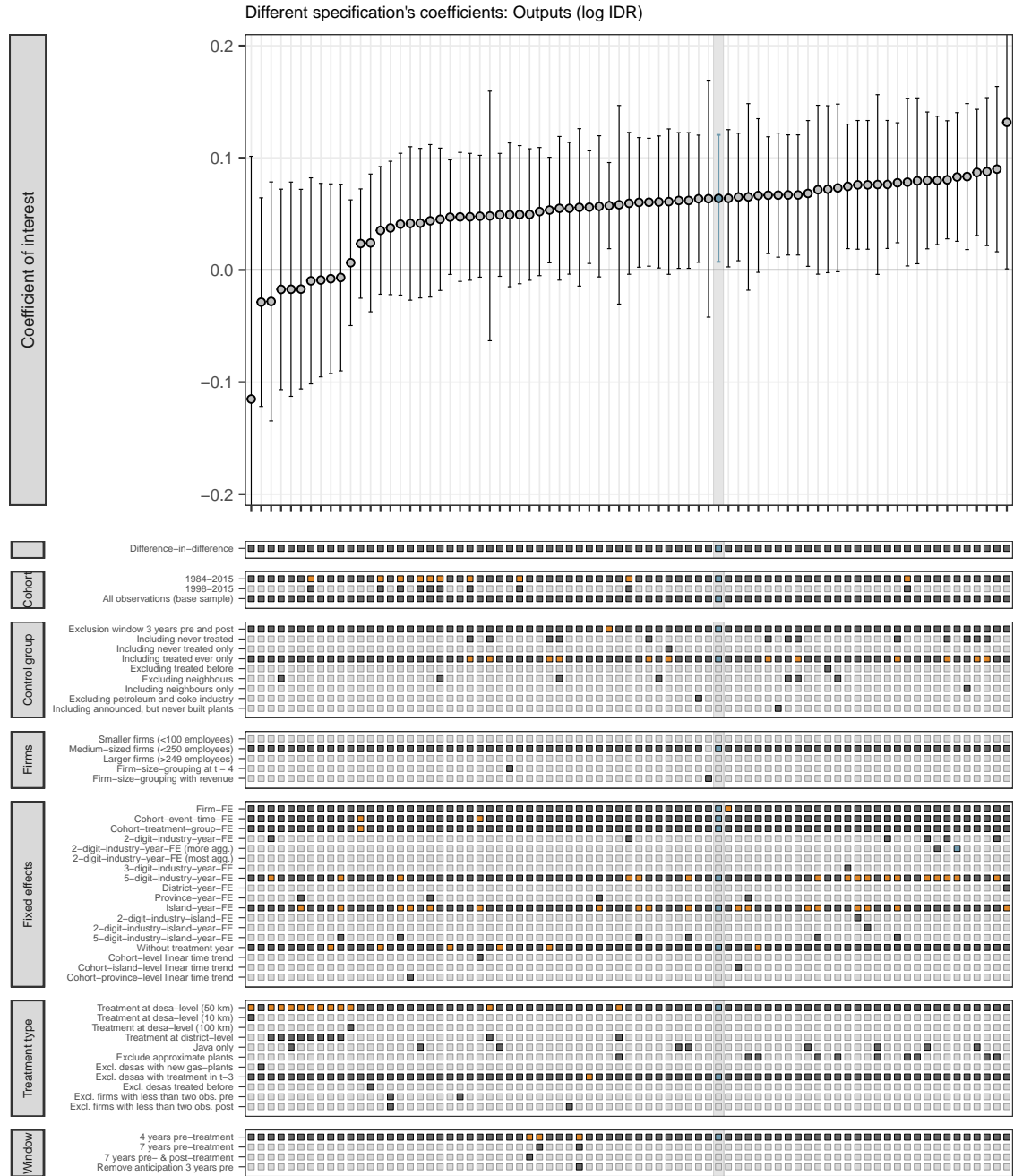


Figure C.11: Coefficients from multiple specifications: Outputs (log IDR) - medium-sized firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' total production outputs (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ between 100 and 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

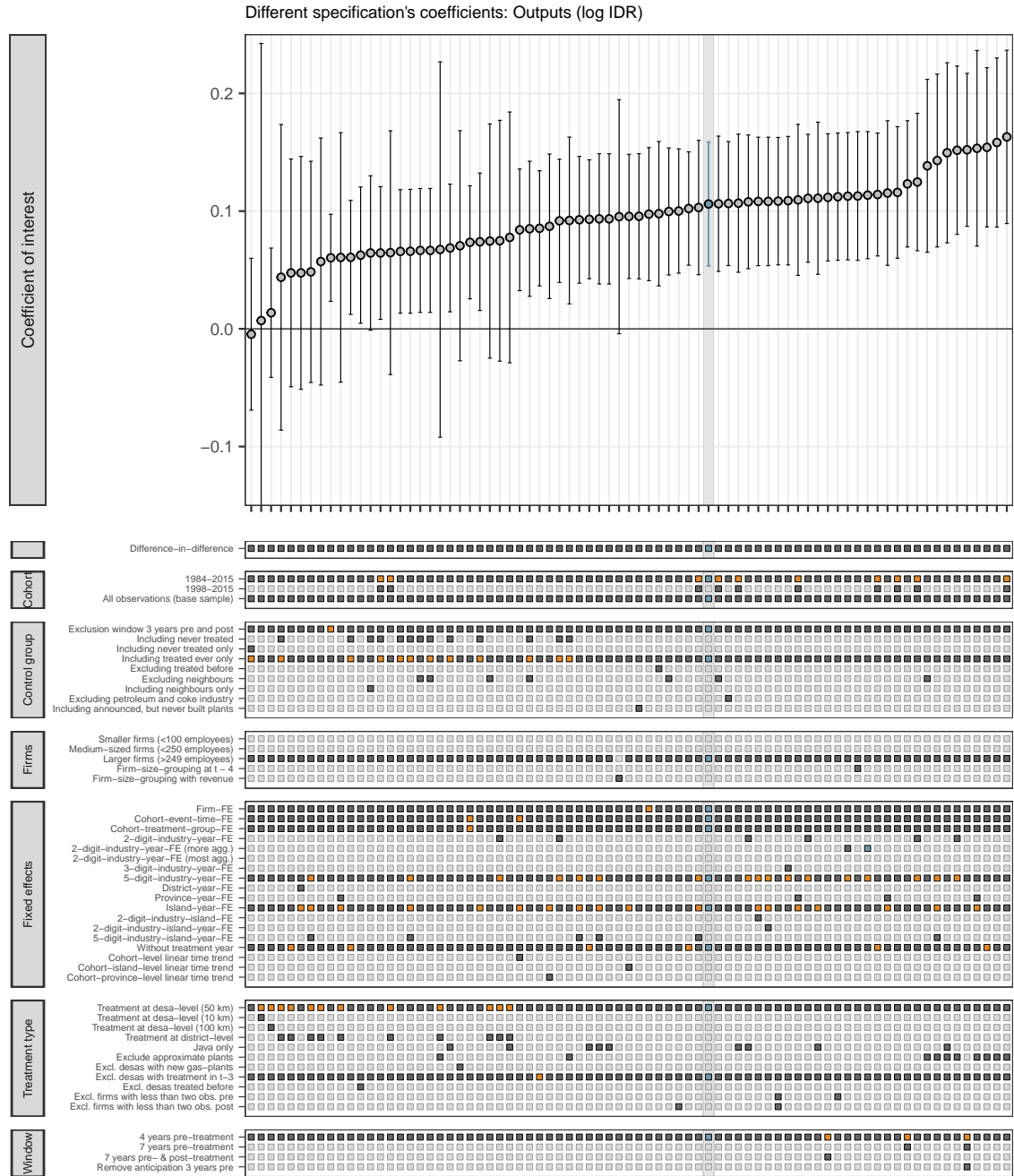


Figure C.12: Coefficients from multiple specifications: Outputs (log IDR) - larger firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' total production outputs (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ more than 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

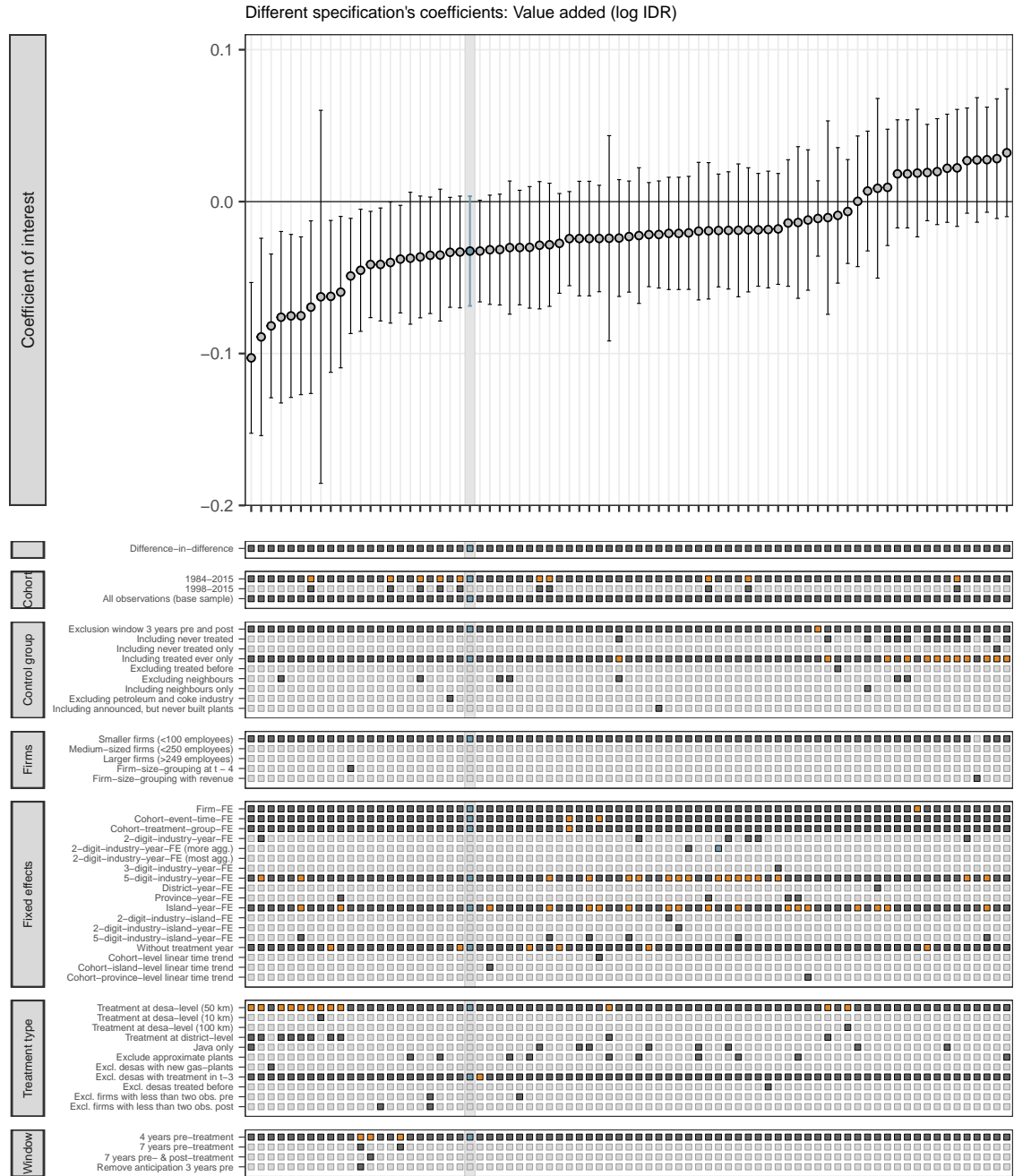


Figure C.13: Coefficients from multiple specifications: Value added (log IDR) - smaller firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' value added (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ less than 100 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

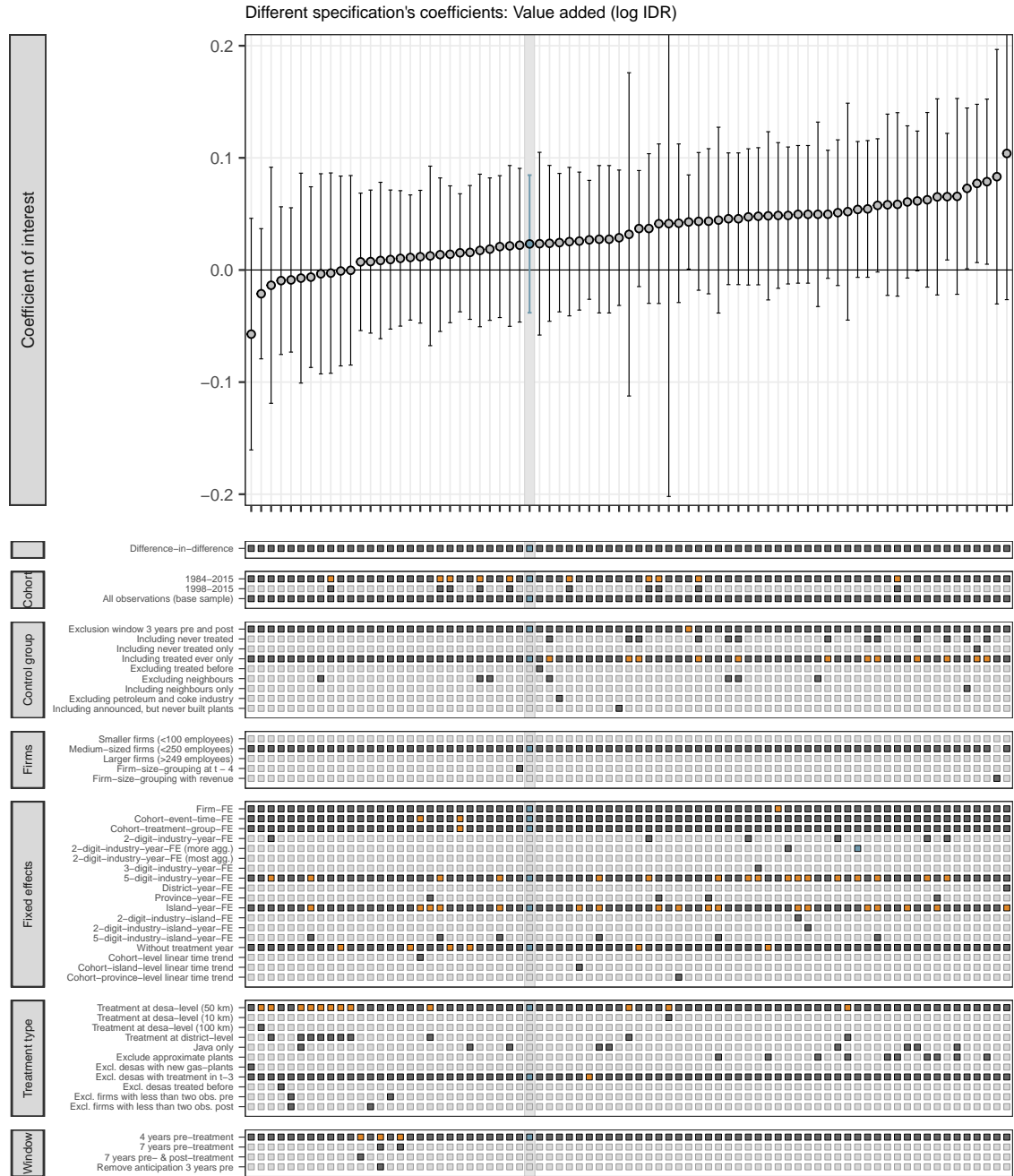


Figure C.14: Coefficients from multiple specifications: Value added (log IDR) - medium-sized firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' value added (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ between 100 and 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

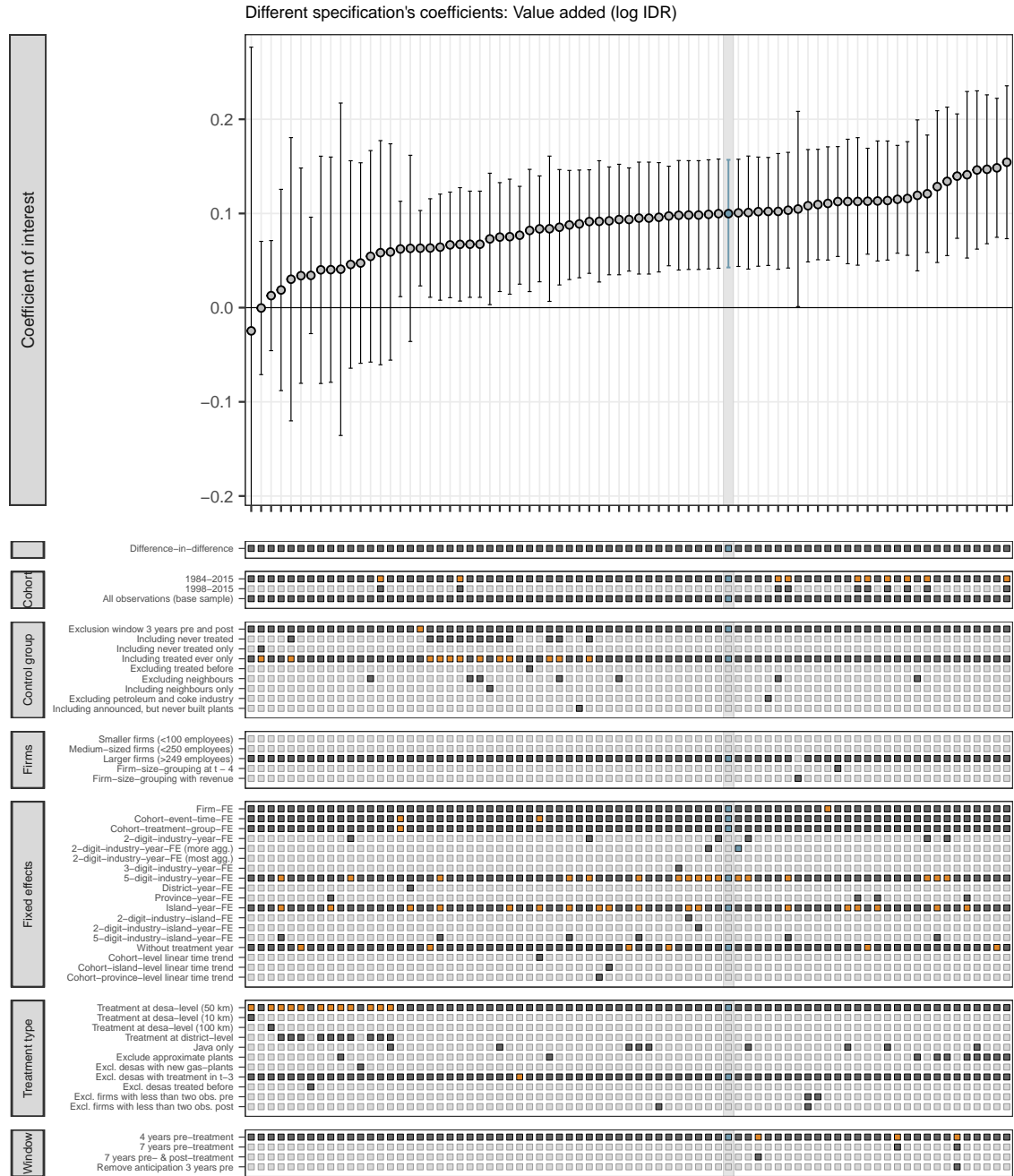


Figure C.15: Coefficients from multiple specifications: Value added (log IDR) - larger firms

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on firms' value added (in log IDR). Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ more than 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. We successively alter different parameters. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

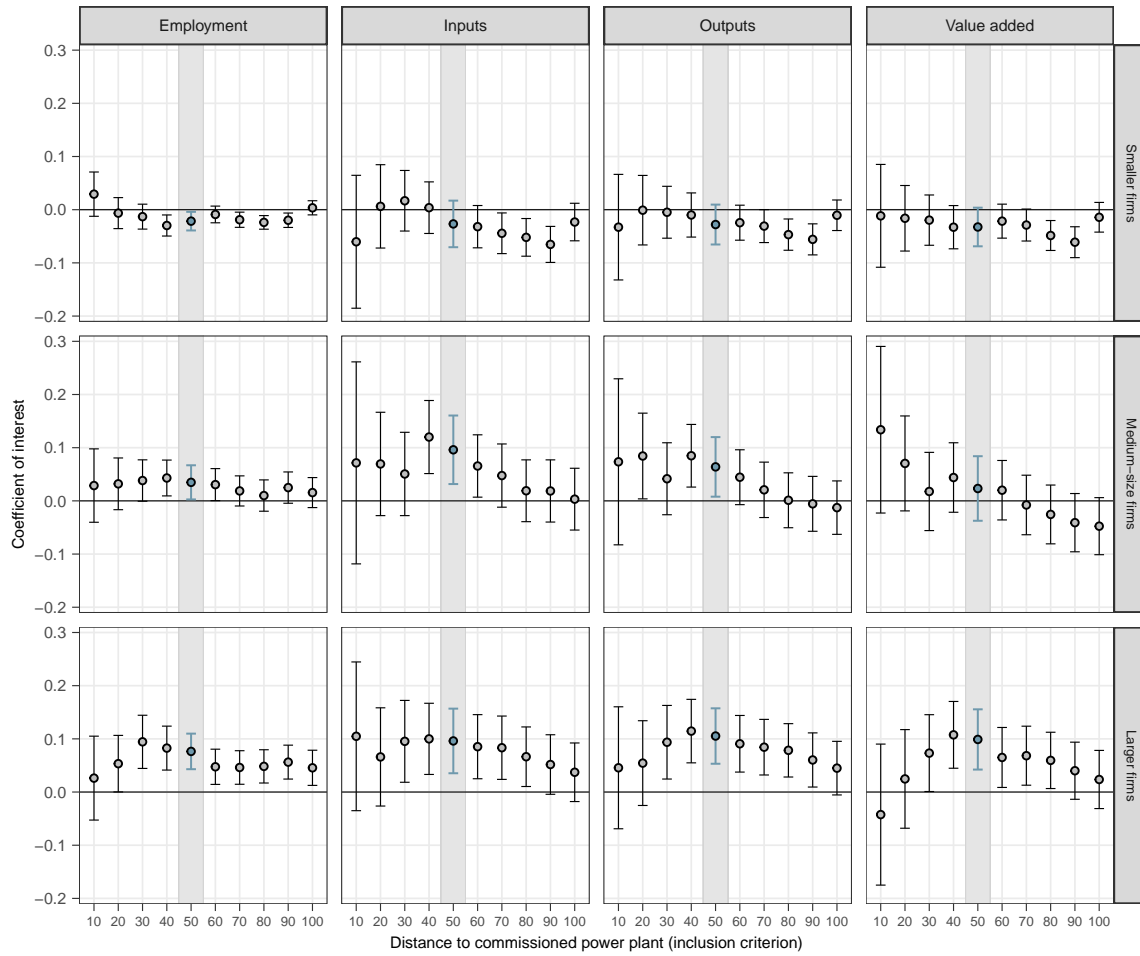


Figure C.16: Coefficients from multiple specifications with varying distance to treatment

This figure shows coefficients from various estimations of a variable indicating treatment by commissioning of a new coal-fired power plant on total employment (log), total inputs, total outputs, and value added (all in log IDR). Vertical bars indicate the 95% confidence interval. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Smaller firms are those with less than 100 employees. Medium-size firms are those with more than 99 and less than 250 employees. Larger firms are those with more than 249 employees.

In each specification (Equation 2), we vary distance a to a coal-fired power plant, which defines eligibility for treatment and control group. Our preferred specification ($a = 50$ km) is highlighted in blue and gray.

All estimations include a set of fixed effects, namely firm-firm group FE, event time-cohort-firm group FE, treatment group-cohort-firm group FE, an industry-year-firm-group FE at the 2-digit level, and an island-year-firm group FE. As such, we interlink our standard set of fixed effects with firm group indicators.

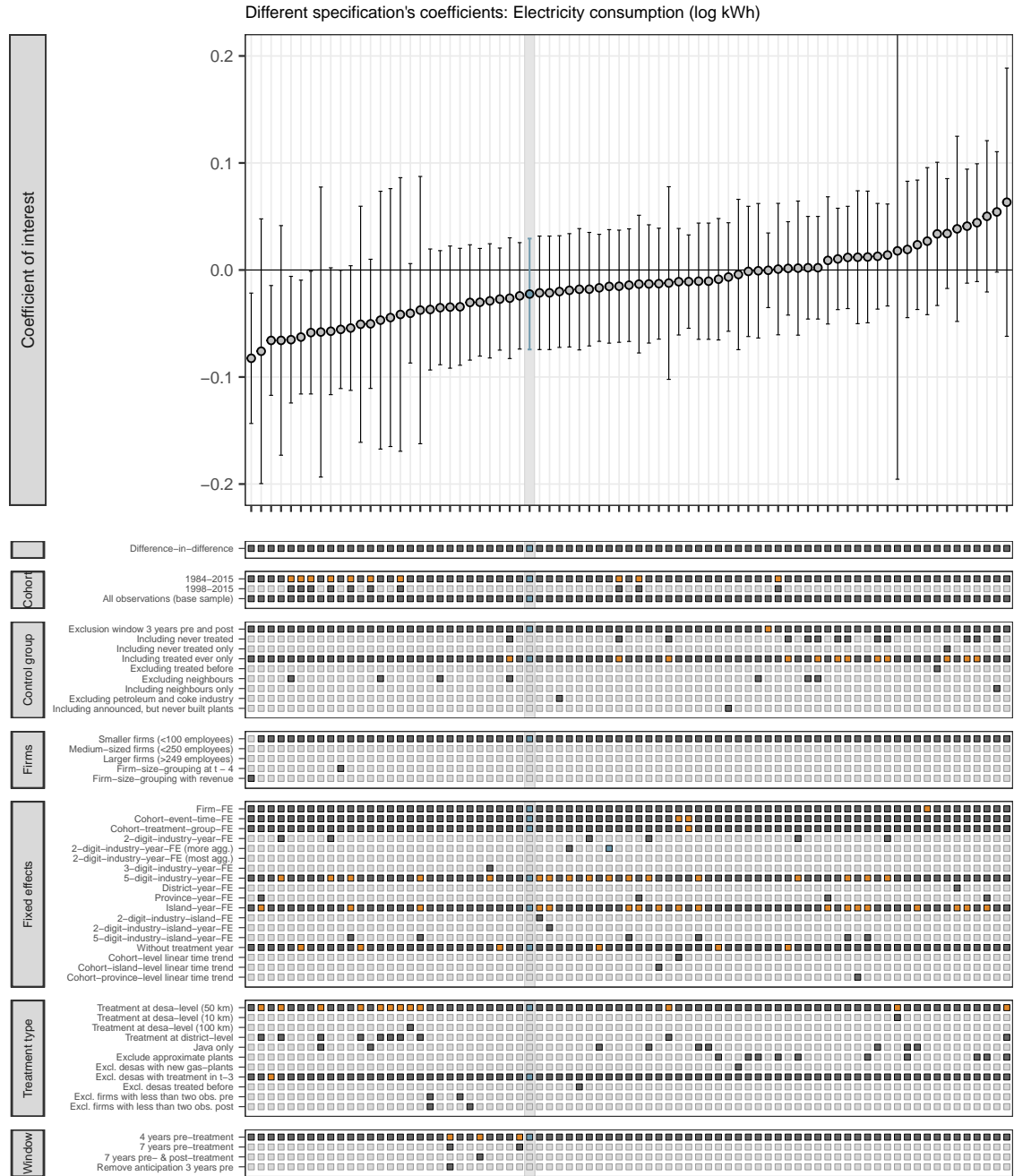


Figure C.17: Coefficients from multiple specifications: Electricity used in production (log kWh) - smaller firms

This figure shows coefficients from various estimations of a variable indicating the treatment by the commissioning a new coal-fired power plant on electricity consumption (in log kWh) in incumbent and local manufacturing firms. Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ less than 100 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

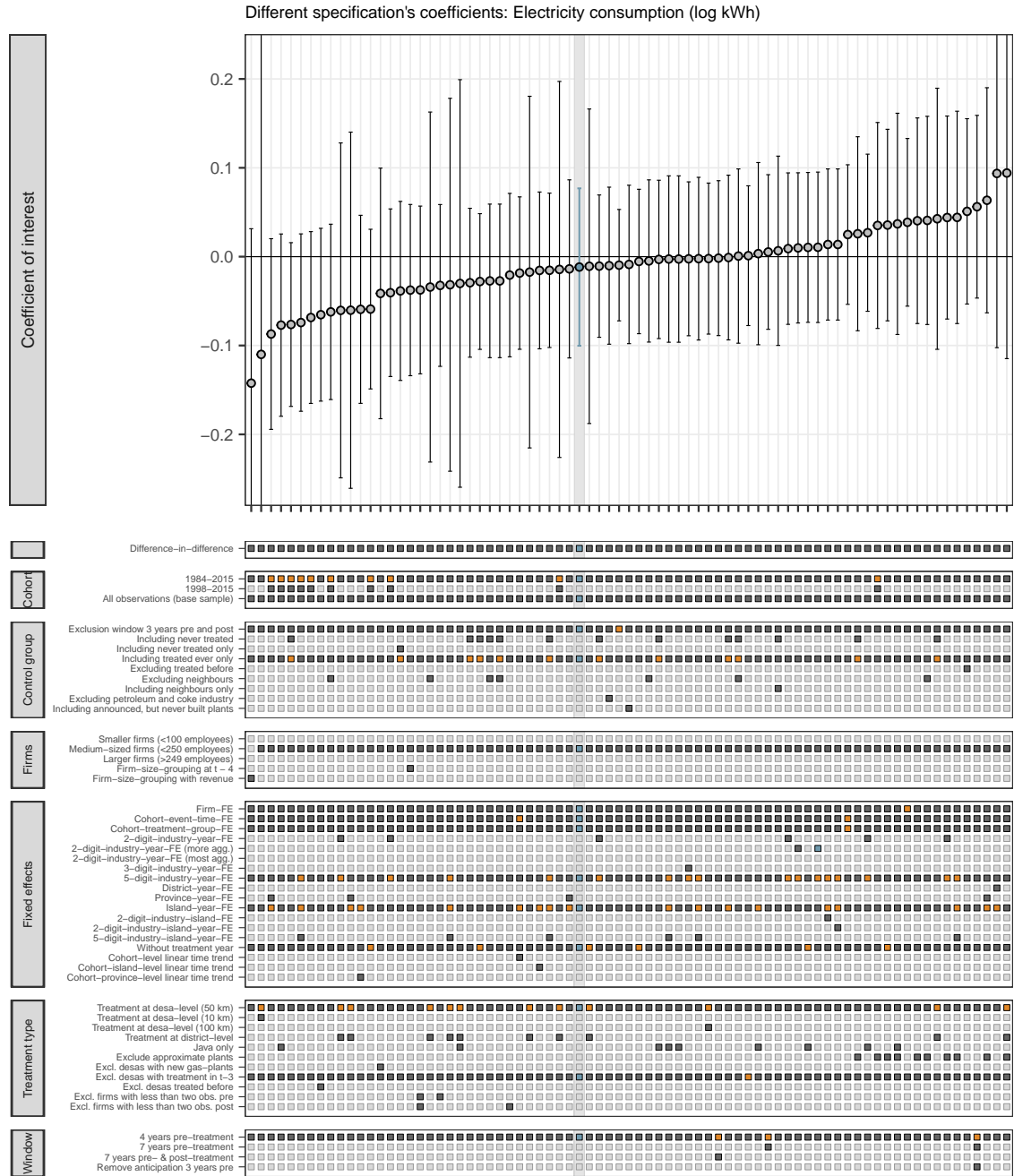


Figure C.18: Coefficients from multiple specifications: Electricity used in production (log kWh) - medium-sized firms

This figure shows coefficients from various estimations of firms' electricity used in production (in log kWh) Rp indicating treatment by commissioning of a new coal-fired power plant. Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ between 100 and 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').

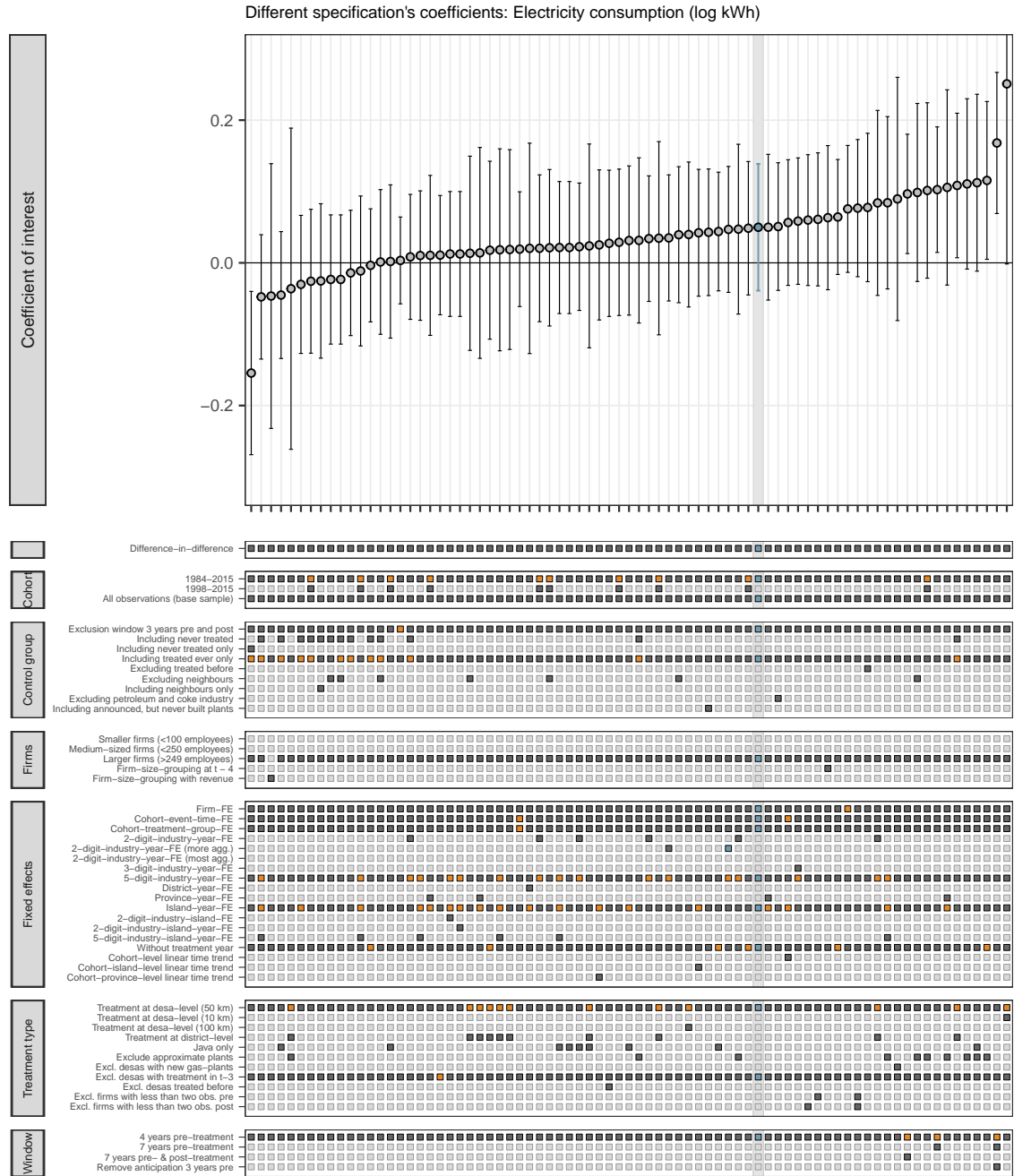


Figure C.19: Coefficients from multiple specifications: Electricity used in production (log kWh) - larger firms

This figure shows coefficients from various estimations of a variable indicating the treatment by the commissioning a new coal-fired power plant on electricity consumption (in log kWh) in incumbent and local manufacturing firms. Vertical bars indicate the 95% confidence interval. Firms are eligible for this sample, if they employ more than 249 employees one year before each cohort's treatment year ($\tau = -1$). Our preferred specification (Equation 2) excluding never treated villages is highlighted in blue and gray. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. Variations include changes to the sample from which we compose the stacked sample (panel 'Cohorts'), different definitions of control groups (panel 'Control group'), different attempts to assign firm groups (panel 'Firms'), different combinations of fixed effects (panel 'Fixed effects'), different definitions of treatment (panel 'Treatment type'), and multiple event windows (panel 'Window').