Burning Coal, Flourishing Industries? Evidence from the Indonesian Manufacturing Sector

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Abstract

Historically, industrial development concurred with burning coal. However, little evidence exists on spillovers from coal-fired power plants to manufacturing firms, especially in today's industrializing economies, which account for the major share of future coal capacity. We quantify spillovers of coal-fired power plant commissioning on local incumbent manufacturing firms in Indonesia during a period of coal phase-in between 1984 to 2015. We analyze spatially and temporarily explicit manufacturing and power plant data in a stacked difference-in-differences framework. Leveraging quasi-random variation in treatment timing, we show that coal-fired power plants caused local firms with more than 100 workers to increase inputs, outputs and employment. Additional research suggests that coal-fired power plants influence firm performance through enhancing electricity reliability, developing local transport infrastructure and inducing labour reallocation. Our results indicate that phasing down global coal capacity will likely need to take such positive externalities from coal-fired power plants into account.

Keywords: Coal, Manufacturing, Industrialization, Indonesia, Difference-in-Differences
JEL Codes: C55, L60, O12, O14, O53, Q40, R11

1 Introduction

Burning coal to generate electricity entails various local and global externalities. Consequently, scenarios frequently find that achieving international climate targets requires a global phase out of unabated coal-fired power plant capacity (Tong et al. 2019). However, despite constantly decreasing costs of low-emission energy conversion technologies (Creutzig et al. 2017; IRENA 2021), more than 500 GW of *additional* coal-fired power capacity is envisaged to start operation until 2030 (Global Energy Monitor 2021a).

Most of the power plants, which are currently active or under construction, will operate in today's industrializing economies (Global Energy Monitor 2021b). In developing economies, coal-fired power plants are often framed as beneficial for industrial development processes[1], which will in turn trigger higher wages, improvements in infrastructure and consequently economic growth. Historically, insights from early industrial development in Europe (Fernihough and O'Rourke 2021; Hunt 1986) underpin this link between burning coal and industrialization: Coal-fired power plants have been helpful to local manufacturing by the provision of reliable electricity and by facilitating the development of transport infrastructure, such as roads, rails and harbours, which resulted in reduced transport costs for local firms. Prevailing spillovers to industrialization might help explain why the use coal has historically been correlated with poverty eradication (Kalkuhl et al. 2019).

If spillovers from coal-fired power plants to manufacturing were indeed prevalent during industrialization processes, ambitions to phasing down coal might be perceived as a slow-down or even circumvention of industrial development, since most coal-fired power plants are built in economies with evolving manufacturing sectors[2]. Yet, little evidence exists on spillover effects from energy infrastructure (in general) and coal-fired power plants (in particular) on local industries in less stages of economic development.

In this study, we investigate spillover effects of local coal-fired power plant commissioning on incumbent manufacturing firms during a period of coal phase-in in Indonesia between 1984 and 2015. Leveraging spatially and temporally explicit manufacturing firm and power plant data, we use a stacked difference-in-differences setup to deduct that coal-fired power plants have led to local booms in large manufacturing firms, thereby consolidating industrialization processes.

We find that coal-fired power plant commissioning causes large and medium-sized firms to expand. Over the course of four years after the advent of coal-fired power plants, large (medium-sized) firms have increased inputs by 12.6% (9.8%), outputs by 11.4% (10.6%) and employment by 8.5% (5.3%). To the contrary, we document a decrease in employment of 2.4% as well as small and insignificant effects on outputs and inputs in smaller firms (with up to 100 workers). This substantial heterogeneity among different firms results in no meaningful effect of power plant commissioning in the pooled sample.

Interpreting our estimates as causal requires the identification of reasonable counterfactuals for treated units. In the context of coal-fired power plant commissioning, this is challenging, since allocation of such power plants is likely endogenous to local circumstances. We address such concerns on endogeneity of treatment by leveraging quasi-random variation in treatment *timing* of power plant commissioning. In addition, the staggered roll-out of power plants helps to circumvent omitted variable bias by confounding, but unobserved local shocks. We also exploit information on power plants, which were announced but not built, to address regional differences in endowment, infrastructure or expectations, that might drive both, power plant allocation and

[1]"Justice demands that, with what little carbon we can still safely burn, developing countries are allowed to grow.", said Indian premier Modi(Financial Times 2015). In India, approximately four million people work in sectors related to coal (Ramachandran and Pai 2021). In the context of Indonesia see also Ordonez et al. (2021)

[2]Most of global coal-fired power capacity, which is currently under construction, announced or permitted, is built in China (54%), India (11%), Indonesia (6%) and other emerging economies (26%), such as Vietnam, Bangladesh or Turkey (Global Energy Monitor 2021b).

firm performance.

Our flexible research design enables us to confirm the robustness of our results, for instance by varying the definition of treatment (by varying inclusion criteria for manufacturing firms, such as distance to power plants) or by tailoring the cohort-specific control groups (by excluding never-, prior- or later-treated units). Our supplementing analyses confirm our main results of heterogenous and locally bounded spillover effects.

This study proceeds as follows. In the following section 2, we discuss channels, through which coal-fired power plant investments might affect local manufacturing firms. We then introduce our empirical strategy and assess challenges to identification in section 3. Section 4 introduces manufacturing and power plant data. In section 5 we present results from our investigation of causal spillovers from coal-fired power plant commissioning to local manufacturing firms. Section 6 discusses probable channels that might help to interpret our findings and section 7 concludes.

2 Literature: Energy Infrastructure and Spillovers to Local Industries

Coal-fired power plants might spill over to local manufacturing firms via several channels: They produce an important production input (electricity), require transport infrastructure, which also facilitates the supply of labour, goods and services to local firms, and might lead to productivity shocks by inducing industrial agglomeration. Contrarily, local pollution externalities might lead to emigration and increase wages. If those spillover effects existed, quantifying them could help to understand why industrializing economies continue investing in coal-fuelled infrastructure projects. Here, we link our study to prior findings from development economics, energy economics and economic history and demonstrate possible channels, through which coal-fired power plants might exert influence on local firm performance and thus local manufacturing growth.

Coal-fired power plants convert thermal energy of lignite or hard coal into electricity. As electricity is a central input to production of many firms, the advent of a new power plant might influence local manufacturing firm performance through the provision of electricity, which might also be cheaper or more reliable. Consequently, the supply of cheaper electricity might facilitate a more productive usage of inputs, growth in output, in employment (if electricity and labour are complements) and productivity, as it is observed in both, industrialized (Fiszbein et al. 2020) and industrializing economies (Allcott et al. 2016; Lipscomb et al. 2013; Meeks et al. 2021; Rud 2012). In the context of Indonesia, Kassem (2020) documents an increase in firm turnover in response to electricity grid expansion, as power provision lowers entry barriers for new competitors, which enforces less productive firms to exit. In a similar fashion, electrification in the domain of private households could increase manufacturing employment, since the use of electricity for lighting increases local labour supply by lowering the demand for household fuel collection, especially among women (Dinkelman 2011).

In contrast to the benefits of expanding electricity transmission, the unreliable electricity provision via *existing* infrastructure is often detrimental to manufacturing firm performance, especially in the context of industrializing economies[3] (Fried and Lagakos (2020), see also Cole et al. (2018)): Firms have responded to power shortages by outsourcing production in China (Fisher-Vanden et al. 2015) and by reducing inputs resulting in revenue loss in India (Allcott et al. 2016). Abeberese (2017) documents reduced levels of output and productivity among Indian manufacturing firms responding to increases in electricity prices. Even though the transmission of electricity requires electricity *grids*, improved reliability of electricity transmission is

[3]In 2009 (2015), 34% (32 %) of 1,176 (1,069) Indonesian manufacturing firms reported electricity to be a moderate to very severe obstacle to current operations. 9.2% (2.9%) of surveyed firms named electricity to be the biggest obstacle in general. 52% (26%) of firms experienced electricity outages (World Bank 2009, 2015).

likely to correlate with spatial proximity to power plants, especially in settings, in which grid infrastructure is weak.

Another channel through which power plants might spill over to local firms is the development of local transport infrastructure. Coal-fired power plants often require improved roads, harbours or railways for operation, which could also facilitate shipping for local manufacturing firms, thereby lowering transport costs. Road development might also expand labour markets and enable access to locally traded goods and services, especially in remote areas, in which transport networks are sparse. Research documents local spillover effects from transportation infrastructure development for several regions including India (Donaldson 2018) or China (Banerjee et al. 2020).

Even beyond the provision of electricity and transport infrastructure, coal-fired power plants can exhibit substantial benefits to local manufacturing by inducing industrial agglomeration (Ellison et al. 2010; Rosenthal and Strange 2004). Greenstone et al. (2010) demonstrate spillover effects to incumbent firms after allocation of so-called 'Million Dollar Plants' in the U.S.. In a quasi-experimental research design, the authors identify counties, which were initially selected as alternative locations for large manufacturing firms, as counterfactuals to counties, in which plants were eventually built. Consequently, the operation of new local plants lead to a substantial increase in productivity among incumbent firms by enabling technological spillovers or altering input prices of locally traded goods. This leads to productivity gains, if firms adjust production inputs accordingly. In the U.S., early hydropower dams (Severnini 2014) and rural development programs (Kline and Moretti 2014) exhibit similar effects.

If new firms allocate in agglomerating areas, competition for labour might lead to rising wages and subsequent immigration. To the contrary, operating coal-fired power plants often entails negative externalities (for instance pollution) at the local level (Clay et al. 2016; Currie et al. 2015), which might lead to emigration, thereby decreasing local labour supply. Expected effects on local wages in response to power plant commissioning are therefore a priori uncertain.

A different strand of the literature is concerned with impacts of broader energy infrastructure to local manufacturing. The mining of fossil fuel minerals relates to industrial agglomeration (Glaeser et al. 2015), impacts regional manufacturing employment (Marchand and Weber 2018) and productivity, especially during periods of boom and bust (Allcott and Keniston 2018; Black et al. 2005; Maniloff and Mastromonaco 2017; Pelzl and Poelhekke 2021). Similiar effects are documented for the use of renewables for electricity generation (Ejdemo and Söderholm 2015; Jackson et al. 2018; Severnini 2014; Slattery et al. 2011).

The literature has identified several channels by with coal-fired power plant commissioning could affect local firm performance. Since those plants require local transportation infrastructure and produce cheap and reliable electricity, which is a pivotal input to many industries, it is reasonable to hypothesize that the operation of coal-fired power plants spills over to local manufacturing firms. Also, effects are likely to be locally bounded, especially in settings, in which grid and transport infrastructure is unreliable.

To the best of our knowledge, our study is the first to empirically investigate spillover effects from coal-fired power plants on local manufacturing firms. Since most of today's coal-fired power plants under construction are envisaged to operate in industrializing economies, we turn to Indonesia, thereby paying special attention to differing trends and mechanisms in less mature stages of industrial development. Our rich sample of firm and power plant data allows us to empirically investigate the roll-out of coal-fired power technology in Indonesia and its links to local manufacturing performance. Comparing coal-specific spillovers to local effects from different energy-conversion technologies (such as hydropower, gas-fired or geothermal power plants) allows us to separate electrification effects from from fuel-specific impacts on local industries.

3 Empirical Strategy: Stacked Difference-in-Differences with Hetereogeneous Treatment Effects

We estimate spillovers to local manufacturing firms from coal-fired power plant infrastructure in early stages of industrial development. Towards this end, we exploit spatial and temporal variation in firm performance during phases of coal infrastructure investments in Indonesia between 1984 and 2015. Owing credit to various identifying assumptions, we can presume time-variant performance shocks in those firms, which are located closely to coal-fired power plants, to be a credible estimate for causal spillover effects.

3.1 Model: Main Specifications

One conventional way to distill causal spillover effects from power plant commissioning would be a difference-in-differences (DID) framework. DID-setups comprise the comparison of two units, treatment and control group, in time periods before and after the treatment, usually accompanied by time- and unit-specific fixed effects. The treatment group consists of those units, which a receive a treatment θ (here: observing a power plant coming on-line in near distance). The control group consists of those units, which do not. Comparing outcomes in treatment and control group before and after treatment time t yields a treatment effect, that withstands the causal interpretation, if units from the control group represent a credible counterfactual for treated units in the absence of treatment.

However, in the context of this study, conventional DID might be subject to several biases. First, coal-fired power plants in Indonesia come on-line in multiple years, that is, in a staggered fashion. In this case, estimates from traditional DID can be misleading, since units, which are treated at later points in time would be compared to units, which were treated at earlier points in time. This might constitute a violation of the parallel trends assumption, if treatment causes prior-treated units to follow a different trend (Goodman-Bacon 2021). Second, some units observe multiple treatments at different points in time, which might bias spillover effects, depending on the mechnanism by which power plants affect local firm performance. Third, prior-treated units might differ from later-treated units, since power plants come on-line during a period of rapid industrial development and electrification programs (see also Kassem (2020)). This relates to - fourth - plausibly non-exogenous treatment, i.e. decisions on power plant allocation being endogenous to local circumstances, such as expected electricity demand. This might lead to selection bias, if power plants select into areas in which firms are expected to grow exceptionally.

As a solution, we propose a *stacked* difference-in-differences setup to estimate local spillover effects from coal-fired power plants to incumbent manufacturing firms. Recently, stacked difference-in-differences techniques have become more popular (Baker et al. 2021; Cengiz et al. 2019; Deshpande and Li 2019; Fadlon and Nielsen 2019; Kraus et al. 2021), since they provide more flexibility to allow the researcher to exert reasonable comparisons between what was observed (the factual) and what could have been observed in the absence of treatment (the counter-factual). Units, which might not be eligible for a reasonable counterfactual, because they were never treated, treated before or exceptionally different *ex ante* from the treatment group, can be excluded from the control group by design. Our stacked difference-in-differences setup helps to reduce biases that might accrue in the context of staggered, possibly non-exogenous placement of coal-fired power plants in Indonesia.

We define 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 on-line instead of construction, since we are unable to observe dates of construction start in our data[4]. For each year t with any

[4]Pre-treament differences might partially account for spillovers from construction of power plants.

treatment event, we next create a unique sample of firms, which we denote cohort c_t. Each cohort c_t includes all firms i, which are located in village d_i and observe a treatment in the respective cohort-year t within distance $A_{d_i,d} < a$ [5], thereby constituting the treatment group of cohort c_t. If treatment occurs in consecutive years, differences between treatment and control group might capture both, the treatment effect of each cohort-year and the dynamic effect of prior treatment events. We therefore exclude units from the treatment group, which were treated in the preceding three years. All firms, which 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 cohort c_t, which we later relax for robustness checks. In each cohort, we include observations four years before and after each cohort's treatment-year (*event window*), because coal-fired power plants are most likely to influence local firm performance in the first years past commissioning. The inclusion of observations from the pre-treatment period serves the purpose of inspecting differences between treatment and control group pre-treatment. Next, we remove firm-year-observations in each cohort's control group from the sample, if they were treated three years before or three years after the event window (*exclusion window*), thereby avoiding unreasonable comparisons following violations of the parallel trends assumption. The flexibility of this approach allows us to vary both, event window and exclusion window, and various inclusion criteria for control (e.g. excluding never treated units or neighbouring units) and treatment groups (e.g. excluding prior treated units). To address the concern that regions, which observe coal investments might differ substantially from never treated villages, we also remove firms from never treated villages from our control group, thereby seizing the opportunity to credibly address potential selection bias.

We stack all subsamples into a large sample, whereby each observation can be uniquely identified by firm i, year t and cohort c. Firm-year-observations are therefore likely to be included in multiple cohorts.

Formally, we estimate this 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 any firm performance indicator in manufacturing firm i from industry j located in village (*desa*) d on island k in year t. Each observation's cohort is denoted by c, each (calendar) year by t. τ indicates relative timing to treatment in each cohort with $\tau \in \{-4; 4\}$. $\theta_{d,c}$ is a binary indicator equaling one if village d is eligible for the treatment group in cohort c. C_c is a binary indicator equaling 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 equaling one if $\tau = t - c$. The interaction term of $\theta_{d,c}$ and C_c controls for cohort-specific differences between treatment and control group, thereby effectively accounting for selection into earlier or later treatment. Likewise, the interaction of $R_{t,c}^\tau$ and C_c addresses cohort-specific time trends before and after treatment in each cohort c, which is more precise than removing time-variant differences across all cohorts by including $R_{t,c}^\tau$ only.

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

Since β_0 expresses treatment effects across a range of possibly heterogeneous firms, we next extend our model to discover heterogeneous treatment effects. Effectively, we thereby exploit

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

differences across three dimensions (time, treatment and firm characteristics), which facilitates comparisons between firms, which are more likely to form a reasonable counterfactual conditional on pre-treatment characteristics. Equation 1 hence expands to

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

and more specifically to

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

where ϕ_c^g denotes whether a firm is eligible for group g in cohort c. In our preferred approach, firms are assigned to a group g conditional on the number of workers one year prior to each treatment year ($\tau = -1$) in each cohort c. Specifically, firms are considered *small* ($g = 1$), if they employ less than 100 workers in $\tau = -1$. Similarly, firms qualify as *medium-sized* ($g = 2$), if they employ between 100 and 499 people in $\tau = -1$, and *large* ($g = 3$), if they employ more than 500 people[6]. The same firm-year-observation might appear in our sample multiple times in different groups g, but never in different groups within the same cohort c. Note that we effectively drop all observations from firm i in cohort c, if firm's i 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 as well as for differences between firms from different 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 groups g relative to treatment timing (τ) across cohorts. $\lambda_{i,g}$ soaks up time-invariant differences between firms, but differs, if firms qualify for different firm groups in different cohorts. $\mu_{j,t,g}$ addresses dynamic differences between firms from the same firm group and two-digit industries ('industry-firm group-year-fixed effect'), while $\nu_{k,t,g}$ addresses 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 in line with estimating Equation 1 separately for each stacked dataset of firm groups, i.e. comparing small firms to other small firms across cohorts and large firms to other large firms across cohorts.

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

3.2 Identification Challenges

In order to distill a credible estimate of causal spillover effects from coal-fired power plants to incumbent manufacturing firms, our research design rests on several identifying assumptions.

[6]In fact, the term 'small' often refers to firms with less than 20 employees (Asian Development Bank 2020). Since firms are only included in our sample, if they employ more than 19 workers, we deviate from this terminology. BPS and ADB classify firms with more than 99 workers as 'large', whereby we additionally distinguish between *medium-sized* firms and *large* conglomerates (more than 499 workers), which is in line with U.S. Bureau of Labor Statistics (1982). Later, we assess the robustness of this classification by grouping firms according to yearly gross revenue in $\tau = -1$ (as suggested by the Indonesian government (Republic of Indonesia 2008)) and the number of workers in $\tau = -4$. Note however that in contrast to binning with total employment, using revenues for binning is sensitive to inflation.

Here, we discuss potential pitfalls of our design and offer robustness checks as a solution.

One lingering concern addresses the endogeneity of treatment. Other than in the canonical Million Dollar Plant setup (Greenstone et al. 2010), it is reasonable to assume that the decision of constructing coal-fired power plants in Indonesia is contingent on local circumstances, some of which could also alter firm performance, such as expected electricity demand, existing transport infrastructure, proximity to coal mines or resource endowment. Addressing structural differences between those firms, which are treated at some point in time, and those, which are not, is challenging.

We propose three different ways to circumvent this challenge. First, we restrict the control group in our sample to firms, which receive treatment at any point in time. This enables us to decapsulate a more precise causal spillover effect, even if local non-observable factors existed, which affect firm performance and power plant allocation. Note that any β_0 should now capture an *average treatment effect on the treated* (ATT)[7].

Second, we leverage variation in treatment timing of power plant *commissioning*, which is likely exogenous to local firm performance and could therefore help to distinguish treatment from anticipation effects (Deshpande and Li 2019). Explorative analysis of detailed biannual data on coal-fired power plants in Indonesia confirms threefold: First, the time between announcing and expected commissioning date differs heavily for coal-fired power plants (see Panel A in Figure 1). Second, the data reveals that the expected commissioning of coal-fired power plants often delays after announcement (see Panel B in Figure 1), since construction and operation require several permissions and legislative procedures with local authorities. Third, we are able to present supporting evidence indicative of additional delay even during construction periods (see Panel C in Figure 1). Pulling those arguments together supports the notion that individual firms are not able to predict the exact timing of power plant commissioning. Effects occurring in the post-treatment period are therefore unlikely biased by local time-specific, but unobservable circumstances, such as tariff reforms, infrastructure development programs or trade regulations.

Third, we exploit data on coal-fired power plants, which were announced, but never commissioned. If local non-observable factors existed, that would influence both, treatment and firm performance, they would also need to be prevalent in those regions, in which operators were at some point convinced to operate a power plant. Expanding the control group by firms from those areas, where plants have been announced, but never established on real grounds, provides an opportunity to distill comparisons, in which bias from omitted variables would be reduced.

Furthermore, several propositions are required to hold in order to interpret our estimates as causal, most prominently the parallel trends assumption. In our analysis, we assume β_0 to be the causal effect of power plant commissioning, if treated units would have observed the same growth in employment, manufacturing or productivity as units from the control group do, conditional on several firm-, industry- and island-specific fixed effects. Note that this proposition can - by definition - not be tested, since we do not observe counterfactual performances of treated firms in the data. Inspecting differences in trends *pre-treatment* can however help to provide some confidence in that our research setup expresses plausible causal estimates.

In addition, we can not rule out the existence of confounding factors coinciding with treatment timing. However, the stacked difference-in-differences approach allows us to address selection into earlier ot later treatment by controlling for cohort-specific time-variant and -invariant characteristics. We therefore argue that estimates are likely unbiased nevertheless, since the staggered role-out would require unobservable confounding factors to impact treated firms in *every* cohort to bias our results.

We define treatment as the commissioning of coal-fired power plants, which elicits the question, of whether observed spillover effects are unique to coal or rather expressing the impact of

[7]However, we argue that in most practical settings decisions on power plant allocation will in fact always obey to local circumstances. Our results should not be interpreted as evidence supporting power plants construction as a probable intervention to influence *any* local firms' performances.

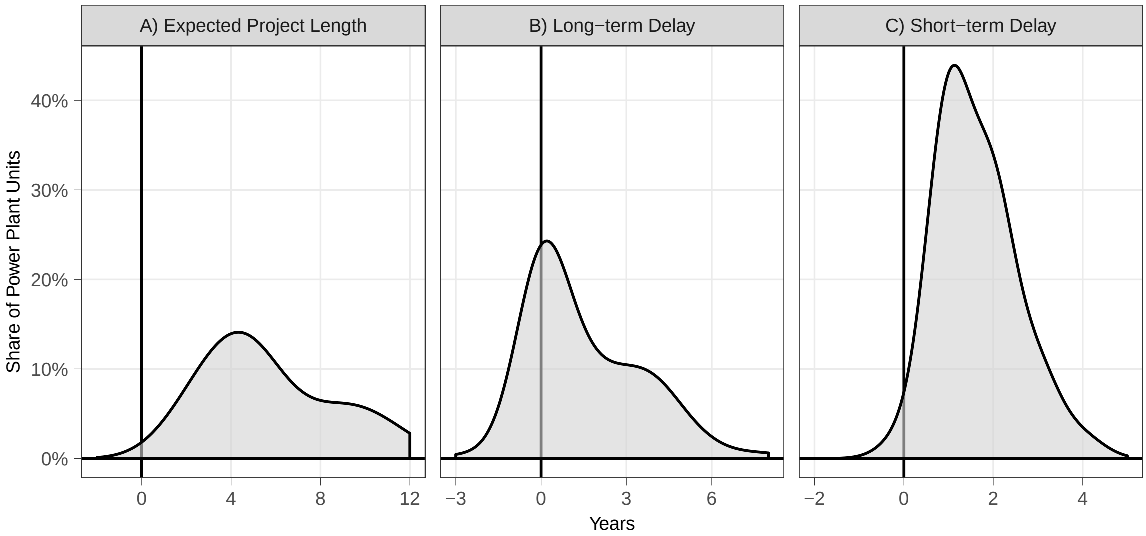

Figure 1: Project Length and Delay for Indonesian Coal-Fired Power Plants

This figure shows data from Global Energy Monitor (2021b), which was collected biannually between January 2016 and July 2021. Panel A displays the estimated project length of coal-fired power plant unit construction in the year, in which the unit appeared first in the data. Panel B displays the accumulated long-term delay of coal-fired power plant construction, that is the difference in years of expected commissioning between when the unit appeared first in the data and when it appeared last in the data. Panel C displays 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 expected commissioning date in 2016 and actual commissioning date. Note different x-axis scales across panels.

electrification. In response, we run additional analyses to detect the effects of gas-fired power plants, hydropower and geothermal power plants. If it were improvements in the local electricity infrastructure rather than fuel-specific spillovers, we should be able to observe similar trends for technologies other than coal.

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

For robustness checks, we also seize the opportunity to tailor the definition of treatment (by varying a) or the definition of cohort-specific control groups (by excluding never-, prior- or later-treated units). The flexibility of the stacked difference-in-differences approach therefore allows us to address several challenges to credible identification.

4 Data: Indonesian Manufacturing Firms and Power Plant Infrastructure

We link manufacturing data with high temporal and spatial resolution to detailed data on power plant infrastructure. Our data on firms come from the Indonesian Manufacturing census, reporting several yearly performance measures across firms from different industries. Our dataset on power plants covers all coal-fired power plants commissioned in Indonesia between 1984 and

2021. In further analyses we also draw on data on gas-fired, hydro- and geothermal power plants as well as on district-level infrastructure data.

4.1 Indonesian Manufacturing Census

We use data from the Indonesian Manufacturing Census (Badan Pusat Statistik - *BPS*), which fuelled the analyses of a number of other researchers including Amiti and Konings (2007), Bazzi et al. (2017), Blalock and Gertler (2004, 2008), Kassem (2020), Kraus et al. (2021), and Pelzl and Poelhekke (2021). The data includes detailed information on yearly firm performance enriched by highly disaggregated spatial data. We infer on single firm's performance by observing several plant-level outcomes such as number of workers, production inputs and outputs, productivity measures such as value added, electricity consumption (in kWh and IDR) and use of (generator) fuels (in liters and IDR). The total sample includes observations from 62,831 firms between 1976 and 2015. In addition, we have access to confidential and precise data on yearly firm-level inputs and outputs (in physical units and IDR) between 1998 and 2012, from which we construct the total amount of transported goods (in tons).

Single firms can be identified on the village-level. Our sample contains firms from 8,622 villages in 286 districts from 26 provinces on 6 major islands. 80% of the plants in our sample are located on the island of Java, which proves to be the center of Indonesian economic activity. Village- and district-level boundaries have been subject to changes over the course of the sample. Therefore, we use crosswalks to identify village coordinates in 2000. Likewise, we transform district-level identifiers into their borders of 1993, thereby addressing the recurring challenge of district splits in Indonesia (Burgess et al. 2012). We assume firms to be located at the centroid of each village polygon, which enables us to derive distances a to any other village d, in which a power plant is located. Panel (a) in Figure A.2 displays villages incorporating at least one manufacturing plant in our sample.

Our dataset includes five-digit-level industry codes. In addition, we are able to identify each firm's main output at the nine-digit commodity-level through examination of input-output data between 1998 and 2012. Note that our sample of manufacturing firms does not include plants whose main output is electricity, i.e. power plants. However, some firms generate electricity as an input to production with the help of generators, which we proxy by observing the consumption of generator fuels.

We perform several cleaning steps on the data, such as removing duplicates, which we detect within and across firms (Allcott et al. 2016; Kraus et al. 2021). Making use of confidential input-output data, we clean aggregate information about total inputs and outputs. In addition, we remove outliers within and across firms. Our final sample contains 625,841 plant-year observations, from which we construct our cohort-specific treatment and control groups.

For each firm-year observation we estimate total factor productivity (TFPR) with the help of various production function estimators (Ackerberg et al. 2015; Levinsohn and Petrin 2003; Wooldridge 2009) and different auxiliary intermediate inputs, namely total electricity (in kWh) and materials (in IDR) used in production. However, the derivation of TFPR requires information on total productive capital, which is missing for 40% of firm-year observations in our sample.

Table 1 compiles summary statistics for Indonesian manufacturing firms in years 1998 and 2015. Table A.1 reports summary statistics for manufacturing firms in 2010, differentiated by firm size. Note that firms with less than 20 workers are not included in the manufacturing census.

We also construct a dataset that reports the total number of firms at the village- or district-level including the number of firms that enter or exit operation, identified by the first and last appearance of single firms in the data. We later use this measure of local firm entries and exits to investigate impacts on firm turnover (Jarmin and Miranda 2002). In this, it is consistent

	1998			2015		
Variable	Mean	Median	SD	Mean	Median	SD
Labour	209	50	597	232	59	582
Inputs (IDR)	13,370,519	577,470	61,774,354	76,879,485	5,887,353	231,317,566
Outputs (IDR)	20,067,222	963,006	88,744,748	129,872,114	11,979,377	370,357,698
Value Added (IDR)	673,148	291,014	37,144,025	52,811,649	4,577,707	167,795,720
Electricity (kWh)	1,098,630	28,823	7,916,110	1,558,495	33,000	11,033,083
Wages (IDR)	2,622	2,043	2,939	22,219	19,506	14,091
Total Factor Productivity	1.45	2.65	4.11	3.82	4.38	4.82

Table 1: Summary statistics of firm groups

This table displays summary statistics for main outcome variables in our manufacturing plant sample based on data from the Indonesian manufacturing census.

with the measure of Kassem (2020).

4.2 Power Plant Infrastructure

We craft a comprehensive dataset comprising information on the commissioning of Indonesian power plants from various sources.

Coal-fired Power Plants We draw unit-level power plant data from Global Coal Plant Tracker (Global Energy Monitor 2021b). Data includes all coal-fired power plant units, which operated in Indonesia with the first one starting operation in 1984. We collapse units at the same location, which come on-line in the same year, into a single plant-year observation. Additionally, the data contains information on plants in the pipeline, that is plants, which are currently under construction or were announced. Some of the announced power plant project were shelved or cancelled entirely. Most operating power plants accommodate exact geographic coordinates, which we cross-checked with open access satellite data. Information on exact locations are missing for some of the announced (or cancelled) plants and for 14% of operating plants, which only report approximate locations. We flag those plants and remove them later in robustness checks. Data keep track of commissioning years, that is, the year in which a power plant came on-line. In addition, our dataset contains information on commissioned (or scheduled) capacity (in MW) and the operating company.

In total, our dataset includes information on 264 plants, of which 76 started operation until 2015, which is the last year, for which we observe manufacturing plant-level information. 30 plants were commissioned until early 2020 and 29 units were under construction (see also Figure A.1). The vast majority of commissioned plants in our sample is located in Java (48%) and Sumatra (30% - see panel (b) in Figure A.2), the Indonesian islands on which manufacturing is most concentrated. This also helps to address concerns on possible proximity to coal mines, which might be a confounding factor in case of mine-to-mouth plants, which have been becoming more attractive in recent years (Ministry of Energy and Mineral Resources 2020). However, most of the coal mines are located in Kalimantan (see panel (e) in Figure A.2, data from Global Coal Mine Tracker (Global Energy Monitor 2020)) and only 4 out of 76 plants in our sample are located in direct proximity to an operating coal mine.

Coal phase-in started in the 1984 and ramped up from 2004 onwards. In early years of coal phase-in, most power plants were operated by the stated-owned Perusahaan Listrik Negara (PLN), which also operates electricity transmission grids. The Indonesian government sets electricity prices (Burke and Kurniawati 2018) and guarantees fixed prices to power plants (Chung

2017), thereby subsidizing electricity generation [8]. Nevertheless, the construction and operation of coal-fired power plants has only recently become more attractive to non-governmental investors (PwC Indonesia 2018). In our final sample, PLN operates 53% of coal-fired power plants, which were commissioned between 1984 and 2015.

In our main specification, our treatment variable expresses whether a coal-fired power plant was commissioned in any calendar year t at a specific location in distance a to each village. We calculate distances between each coal-fired power plant with existing geocoordinates and each village d, in which at least one plant from our manufacturing plant dataset is allocated. Consequently, any firm-year observation qualifies as treated, if it is within distance a to any coal-fired power plant in year t. We also inspect whether power plants were commissioned in neighbouring villages, that is, villages bordering treated villages or villages within distance a_1 to coal-fired power plants with $a_1 > a$. Table A.2 displays the number of villages within distance $a = 50km$ to coal-fired power plants, which were commissioned in the respective year.

We also construct a dataset which identifies whether villages are exposed to a nearby power plant that came on-line after 2015, is currently under construction, permitted or in stages of pre-permit development. We also include power plant projects, that were once announced. Some of the announced projects were shelved or cancelled entirely. We drop observations, if we are unable to identify the exact location of the plant site. We later deploy this dataset entailing 42 additional projects to help distinguishing the effects of power plant *commissioning* from anticipation effects. Since project developers have evaluated plant sites to be eligible for operation, we can confidently assume that we reduce bias from local unobserved factors when including manufacturing plants from such regions to our control group.

Gas-fired, Hydro- and Geothermal Power Plants Towards the end of distinguishing fuel-specific spillover effects, we compile a dataset on gas-fired, hydro- and geothermal power plants, which were constructed and operated during the period of our interest. Those power generation facilities also deliver electricity, but spillover effects might nevertheless differ. Gas-fired power plants, for instance, are more likely to require pipelines for fuel shipping rather than roads, that could be used by firms. Hydropower accounts for the largest source of renewable power generation in Indonesia, while Indonesia has access to estimated 40% of the world's geothermal resources (Nasruddin et al. 2016). We disregard information on wind or solar power generation facilities, since they do not contribute a meaningful share to Indonesian electricity generation (PwC Indonesia 2018). Similar to coal-fired power plants, hydropower and geothermal power generation often covers baseload electricity demand.

We merge data from the Global Powerplant Database (Byers et al. 2019) and the World Electric Power Plants Database (PLATTS 2017). While the former contains information on exact geocoordinates, the latter includes information on exact commissioning dates. For our analysis of local spillovers from non-coal-fired power plants, we include information on plants, which were commissioned within our sample period between 1980 and 2015 and drop those plants with a power generation capacity lower than 10 MW. Our dataset comprises 120 gas-fired power plants, 38 hydropower plants and 19 geothermal power plants, that were commissioned in our time period of interest. Panel (d) in Figure A.2 displays the location of those power plants in our final sample.

4.3 Regional Infrastructure Data

We enrich our dataset, which contains information on local firm performance and power plant operation on the village-year-level, by district- and province-level aggregate information. We compile data on transport infrastructure (such as district share of improved roads), local labour

[8] Since 2013, the Indonesian government has started to reform electricity price subsidies (Burke and Kurniawati 2018).

demand (total and sectoral employment, poverty indicators, total population) and electricity reliability (system average interruption duration index (SAIDI) and system average interruption frequency index (SAIFI)). Data on the former stems from the Indonesia Database for Policy and Economic Research (INDO-DAPOER) compiled by the World Bank, while the latter is similar to the one used by Falentina and Resosudarmo (2019) and includes information on the province-year level compiled from yearly PLN statistic reports.

5 Results: Spillover Effects from Coal-Fired Power Plants to Manufacturing Firms

We show next that over the pooled sample the commissioning of coal-fired power plants leads to small and insignificant effects on local firm performance. However, we are able to provide evidence for heterogeneous treatment effects across heterogeneous firms, which are masked in our pooled regression. Commissioning coal-fired power plants has led to large and significant shocks in labour demand, inputs, outputs and value-added among closely located large firms. To the contrary, small firms appear not be positively affected by power plant commissioning, if anything.

5.1 Spillover Effects in Pooled Sample

Commissioning Indonesian coal-fired power plants did not alter the performance of local incumbent firms in comparison to other firms, which 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 level of employment, total outputs and inputs to production and value added (in log IDR) as dependent variable y. Average differences between treatment and control group across four years post- in comparison to the pre-treatment period can hardly be distinguished from zero.

Results in the pooled sample are not indicative of strong local spillovers from coal-fired power plants to local manufacturing performance. In fact, we can rule out that local industry clusters flourish *as a whole* post the advent of coal-fired electricity generation. At a first glance, this is contrary to prior beliefs about conventional mechanisms in economic development, by which cheap and reliable energy provision enabled local economic growth and subsequent industrialization. However, our difference-in-differences estimator expresses average effects and might therefore mask heterogeneous spillover impacts on different firms.

5.2 Hetereogeneous Spillover Effects

We provide empirical evidence for heterogeneous spillover effects from coal-fired power plants to local incumbent manufacturing firms in Indonesia. The commissioning of power plants increased employment, inputs, outputs and value added in large firms (employing more than 500 employees in the year before power plant commissioning), which are located within 50 kilometers distance, compared to other large firms, which observed the commissioning of coal-fired power plants earlier or later, but not three years before or past treatment. To the contrary, comparing small firms to other small firms before and after treatment yields small and negative impacts on firm performance.

Table 3 reports pooled estimates for four years after treatment in comparison to four years before treatment, excluding the year of commissioning. Table 4 shows coefficients from Equation 3, which expresses $\beta^{g}_{0,\tau}$ for a set of firm performance indicators. We inspect those estimates visually with the help of Figure 2, displaying those dynamic estimates for each firm group g in relative time τ to treatment.

Dependent Variables:	Labour (1)	Output (2)	Input (3)	Value Added (4)
Variables				
Treatment Group*POST	-0.002 (0.008)	0.004 (0.013)	0.014 (0.016)	-0.004 (0.014)
Fixed-Effects				
Firm-FE	Yes	Yes	Yes	Yes
Event Time-Cohort-FE	Yes	Yes	Yes	Yes
Treatment Group-Cohort-FE	Yes	Yes	Yes	Yes
Industry (2-Digit)-Year FE	Yes	Yes	Yes	Yes
Island-Year-FE	Yes	Yes	Yes	Yes
Year t = 0	Yes	Yes	Yes	Yes
Observations	1,396,445	1,333,477	1,326,184	1,396,368
R^2	0.89334	0.88930	0.86667	0.86530

Table 2: Effects on Labour, Output, Input and Value Added

This table reports OLS estimates of coal-fired power plants coming on-line on local manufacturing firms. Outcome variables are labour demand (1) (in log total employees), total output (2), total material inputs (3) and value added (4) (all 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. The unit of observation is an Indonesian manufacturing firm, for which we have yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative 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-FE, event time-cohort-FE, treatment group-cohort-FE, an industry-year-FE at the 2-digit level, an island-year-FE and a binary variable equaling one for treated firms in treatment year.

Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

Firms, which employ between 20 and 99 workers one year before treatment and are located within 50 kilometers distance, decrease employment by 2.4%, outputs by 2.3%, inputs by 1.1% and value added by 2.1% in the four years after the commissioning of a coal-fired power plant compared to manufacturing plants in villages, which experience the shock at some point in time, but not within in the exclusion window. Pooled regression coefficients for input, output and value added are insignificant at the 5%-level. It is thereby reasonable to conclude that coal-fired power plants did not spill over to local small manufacturing firms and, if anything, reduced labour demand.

To the contrary, medium-sized (100 to 499 workers in the year before treatment) and large manufacturing firms (500 workers or more in the year before treatment) increase employment, outputs, inputs and value added: In the four years after the commissioning of a coal-fired power plant within 50 kilometers distance large (medium-sized) firms increased employment by 8.5% (5.3%), manufacturing outputs by 11.4% (10.6%), inputs to production by 12.6% (9.8%) and value added by 9.7% (7.0%) in comparison to large and medium-sized firms, that are in proximity to such a plant outside the exclusion window.

The inspection of differences between treatment and control group in the pre-treatment period ($\tau \in \{-4; -1\}$) shows that differences are not statistically significant at the 5%-level. That is, treatment and control groups exhibit similar trends in the years preceding treatment.

Dependent Variables:	Labour (1)	Output (2)	Input (3)	Value Added (4)
Variables				
Treatment Group*POST (Small Firms)	-0.024***	-0.023	-0.011	-0.021
	(0.009)	(0.019)	(0.022)	(0.019)
Treatment Group*POST (Medium Firms)	0.053***	0.106***	0.098***	0.070***
	(0.014)	(0.026)	(0.029)	(0.026)
Treatment Group*POST (Large Firms)	0.085***	0.114***	0.126***	0.097**
	(0.023)	(0.039)	(0.041)	(0.039)
Fixed-Effects				
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	1,049,371	975,879	994,894	1,049,307
R^2	0.92309	0.89982	0.88273	0.88139

Table 3: Effects on Labour, Output, Input and Value Added by Firm Size

This table reports OLS estimates of coal-fired power plants coming on-line on local manufacturing firms, differentiated by firm size one year before treatment. Outcome variables are labour demand (1) (in log total employees), total output (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 treatment is assigned.

The unit of observation is an Indonesian manufacturing firm, for which we have yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Here, we exclude firms from our sample, if they are located in villages, which were never treated. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment. Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 firms. Firms with more than 499 employees are considered large firms.

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. Thereby, we interlink the standard set of fixed effects with firm group indicators.

Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

Performance in total output, input and value added among medium-sized are notable exceptions that merit special attention. Here, we however observe a substantial trend break in differences post-treatment. Pre-trend differences among medium-sized firms, which might be heterogenous among themselves, are flat for employment. For all firm groups, differences in the post-treatment period become smaller after three years of power plant commissioning. Our results are indicative of spillover effects from coal-fired power plant commissioning to large and medium-sized local manufacturing firms. Especially, coal-fired power plant commissioning appears to drive local employment as well as input and output levels in large and medium-sized firms, whereas similar increases in labour demand are not observed at the level of smaller firms.

Dependent Variables:	Labour	Output	Input	Value Added	Labour	Output	Input	Value Added	Labour	Output	Input	Value Added
Firm Group	Large Firms				Medium-Sized Firms				Small Firms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Treatment Group (t = -4)	-0.016	0.014	-0.070	0.012	-0.011	-0.043	-0.026	-0.065**	-0.007	-0.030	-0.020	-0.002
	(0.024)	(0.045)	(0.056)	(0.046)	(0.013)	(0.030)	(0.034)	(0.030)	(0.008)	(0.022)	(0.026)	(0.022)
Treatment Group (t = -3)	-0.026	-0.016	-0.025	0.037	-0.009	-0.056**	-0.072**	-0.051*	0.007	-0.023	-0.012	-0.001
	(0.022)	(0.042)	(0.047)	(0.045)	(0.012)	(0.027)	(0.031)	(0.027)	(0.007)	(0.020)	(0.023)	(0.021)
Treatment Group (t = -2)	-0.027	-0.031	-0.045	0.006	-0.003	-0.025	-0.022	-0.035	-0.0008	0.009	6.1×10^{-5}	0.017
	(0.018)	(0.035)	(0.041)	(0.037)	(0.009)	(0.024)	(0.027)	(0.024)	(0.005)	(0.018)	(0.020)	(0.018)
Treatment Group (t = 0)	0.010	-0.002	0.036	-0.008	0.006	0.024	0.030	0.011	-0.015***	-0.024	0.013	-0.004
	(0.018)	(0.032)	(0.036)	(0.036)	(0.010)	(0.024)	(0.026)	(0.025)	(0.006)	(0.016)	(0.018)	(0.018)
Treatment Group (t = 1)	0.040*	0.119***	0.075	0.129***	0.038***	0.088***	0.103***	0.090***	-0.027***	-0.028	-0.007	0.005
	(0.023)	(0.045)	(0.048)	(0.047)	(0.013)	(0.029)	(0.034)	(0.032)	(0.008)	(0.021)	(0.023)	(0.022)
Treatment Group (t = 2)	0.071***	0.154***	0.178***	0.171***	0.064***	0.102***	0.089**	0.066**	-0.017*	-0.022	-0.018	-0.009
	(0.026)	(0.050)	(0.051)	(0.051)	(0.015)	(0.031)	(0.036)	(0.032)	(0.009)	(0.023)	(0.026)	(0.024)
Treatment Group (t = 3)	0.077**	0.103**	0.113**	0.095*	0.033*	0.053	0.048	-0.024	-0.023**	-0.041	-0.016	-0.031
	(0.030)	(0.051)	(0.054)	(0.055)	(0.017)	(0.034)	(0.039)	(0.035)	(0.011)	(0.025)	(0.029)	(0.026)
Treatment Group (t = 4)	0.098***	0.024	-0.013	0.017	0.058***	0.055	0.025	-0.021	-0.028**	-0.044*	-0.037	-0.052*
	(0.033)	(0.053)	(0.059)	(0.058)	(0.019)	(0.036)	(0.040)	(0.038)	(0.012)	(0.027)	(0.031)	(0.028)
Fixed-Effects												
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	119,798	112,493	112,313	119,741	295,506	280,093	279,359	295,486	634,067	583,293	603,222	634,080
R^2	0.66225	0.78444	0.74832	0.74632	0.61949	0.80944	0.78884	0.76806	0.69336	0.83690	0.81668	0.81668

Table 4: Dynamic Effects on Labour, Output, Input and Value Added by Firm Size

This table reports dynamic OLS estimates of coal-fired power plants coming on-line on local manufacturing firms, differentiated by firm size one year before treatment. Outcome variables are labour demand (in log total employees) in columns (1), (5) and (9), total output 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 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 yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Here, we exclude firms from our sample, if they are located in villages, which were never treated. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 employees. Firms with more than 499 employees are considered large firms.
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. Thereby, we interlink a standard set of fixed effects with firm group indicators. Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

5.3 Robustness of Results

We critically assess the robustness of our results and the validity of our identifying assumptions by varying multiple parameters of our preferred specification. Since our research design facilitates the comparison of multiple estimations, we test the sensitivity of our results with the help of specification charts (Simonsohn et al. 2020). Specification charts enable the comparison of multiple regression outcomes, in which we alter the definition of cohorts, change 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 treatment. Figures A.3 to A.14 show point estimates and 95% confidence intervals for a set of alternative specifications and our main variables of interest, employment, inputs, outputs and value added. We show that our results are consistent in a set of alternative specifications. We confirm evidence for heterogeneous spillover effects to firms of different sizes with estimates of large firms usually exceeding those of medium-sized firms. Subsequently, we highlight central results, which help to examine the credibility of our main results.

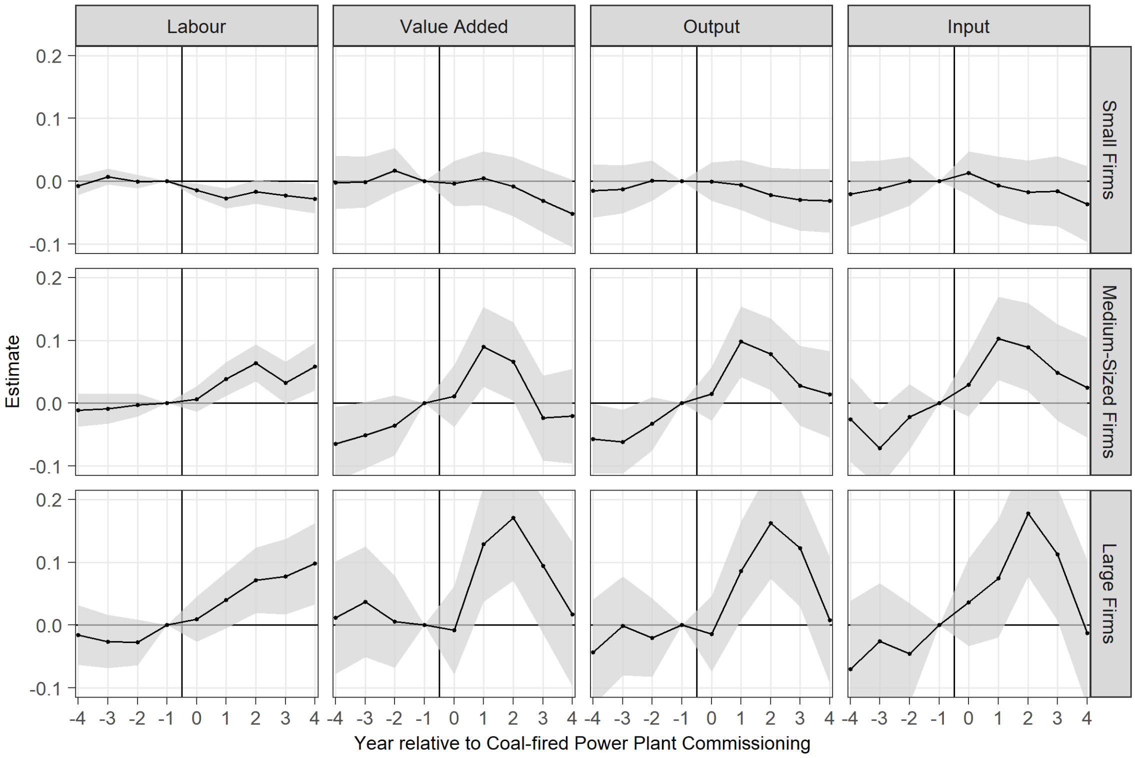

Figure 2: Estimates of Spillover Effects from Coal-Fired Power Plants Establishment on Local Firms

This figure displays estimates of coal-fired power plants coming on-line on local manufacturing firms, differentiated by firm size one year before treatment. Outcome variables are labour demand (in log total employees), total output, total material input and value added (all in log IDR). These are coefficients from Equation 3, which are also presented in Table 4. 95% confidence intervals are represented by grey area. The year before treatment ($\tau = -1$) is used as the base year. Firms are assigned to firm groups according to the number of employees in this year before treatment. Small firms comprise firms with less than 100 employees. Medium-sized firms comprise firms with more than 99 and less than 500 employees. Large firms refer to firms with more than 499 employees.

Definition of firm groups In our preferred specification, we demonstrate heterogeneous treatment effects by creating sub-samples of all firms according to their total employment one year before treatment ($\tau = -1$). We alter the definition of firm groups by following two strategies: First, we bin firms according to their total employment in $\tau = -4$, thereby excluding firms, which might have started production in the three years before commissioning. If some firms might react to the *announcement* of a power plant by allocating, this might constitute selection into treatment and therefore potentially bias our results. Second, we bin firms according to their total *revenue* one year before treatment. However, for medium-sized and large firms our results are robust to this alternative specifications (see Figures A.3 to A.14). Estimates remain of comparable magnitude and are statistically significant at the 5%-level. To the contrary, estimates for small firms turn positive. Note though that deploying total revenues as inclusion criterion for firm groups does not account for inflation, thereby excluding comparable smaller firms from earlier years out of our sample. It is likely that positive estimates for small firms under the assumption of revenue-based binning partly reflect effects on larger firms. This is also reflected by larger standard errors for large firms, since sample size becomes more restricted.

Including never treated firms in control group We assess the robustness of our results by including never treated units to our control group. Table A.3 displays dynamic estimates from a regression, in which we leverage variation in both, treatment occurence and treatment timing. Heterogeneity of treatment effects among firms of different sizes remains unaffected to this change. Effects on employment in large and medium-sized firms increase in size, while they become small and insignificant for small firms. Dynamic DID-estimates in levels of input and output become smaller, but standard errors increase. This might be indicative of the necessity to compare reasonable counterfactuals, since we would expect ever treated units to exhibit different dynamics around treatment than never treated units.

Leveraging plants which were announced but not commissioned Endogeneity of treatment is an important concern about our research design. In our main specification, we address this concern by restricting the control group to firms, which are treated at some point, but not within three years before or after treatment. Nevertheless, observed effects might tell us more about the treated units (and anticipation behaviour) than about the treatment. To address this possible threat to identification, we include firms to our control group, which reside in an area, which operators identified at some point to be suitable for coal-fired power plant operation, but have never been treated. Drawing on data on power plant projects, which were announced, but have not yet (or never) been commissioned, we thereby help to reduce bias from unobserved factors, which might influence both, power plant commissioning and firm performance. Our main results remain robust to this alteration (see Table A.4), which supports the notion that treatment effects are driven by the *commissioning* and subsequent operation of plants rather than anticipation within firms or locally unobserved confounders.

Neighbouring treated villages In our main empirical setup, we identify treated villages via spatial distance a to commissioned power plants. Since this is a sharp criterion for inclusion in treatment and control group respectively, we test for spillovers to neighbouring villages to rule out that local spillovers also affect plants beyond distance a. This could bias our results, if untreated units changed performance trends in response to treatment, for instance through local industrial networks. In an alternative specification, we therefore include firms in our treatment group, if they are located in villages, that share a border with a treated village or are within distance a with $50km < a < 100km$ to a commissioned coal-fired power plant. For this alternative specification, we remove firms from treated villages from our sample. Table A.5 reports the effects of coal-fired power plant commissioning on manufacturing firms in villages neighbouring treated villages. Coefficients for large and medium-sized firms are small and insignificant at the 5%-level, which justifies the inclusion of firms from neighbouring villages in the control group[9]. Estimates from our sample of small firms are similar in magnitude as in our main specification. This test also provides evidence for the existence of locally concentrated spillover effects and hints to treatment effects in medium-sized and large firms, which are diminishing with distance a to a new power plant.

Varying distance a In our main specification, we define treatment events as the commissioning of coal-fired power plants with distance $a = 50km$. Since we exclude never treated units in our preferred specification, the definition of a constitutes an important inclusion criterion for both, treatment and control group. To test whether our results are sensitive to the definition of a, we compare treatment effects on treated firm's performance indicators while varying the definition of a. Figure A.15 shows a specification chart including point estimates and confidence intervals (95%) for different distances a in small, medium-sized and large firms. Results support our main finding of heterogenous treatment effects for firms of different sizes. Point estimates for small firms are similar in size, but decreasing with larger levels of a. In medium-sized and large firms, effects on employment are insensitive to changes in a with confidence intervals expanding

[9]We remove plants from neighbouring villages from the control group as a robustness check. Our estimates are robust to this variation. See Figures A.3 to A.14. As an alternative, we remove plants from all, but neighbouring villages from the control group.

for lower levels of a. This is reasonable, since lower distances decrease both, the number of firms, which are suitable for treatment or control group. Also, our fixed effects structure might capture remaining variation to some extent. On the other hand, we document a slight decrease in estimates for increases in a on total output, inputs and value added. This is supportive of locally bounded spillover effects, which is consistent in a setting, in which physical infrastructure, by which coal-fired power plants might exceed influence on local firms (such as grids or roads), is a constraint.

6 Discussion: Explanations and Implications

Our main results provide estimates on spillover effects from coal-fired power plant commissioning in Indonesia on local incumbent manufacturing firms. We provide evidence on heterogeneous effects on firms of different size: Large and medium-sized firms increase performance, while output levels of local small firms remain virtually unaffected and employment decreases in response to treatment. The interpretation of these results merits an investigation on the mechanisms, through which power plant commissioning affect manufacturing firms, which we discuss first. In addition, we also assess further implications of our results for international climate and energy policy and discuss the limitations of our study.

6.1 Possible Channels of Spillover Effects

Theoretically, the commissioning of coal-fired power plants could influence local firm performance via several channels with differing implications. In this section, we use firm-, district- and province-level data to investigate the effects of power plant commissioning on several mechanisms: increased availability of electricity, advanced stability in local electricity grids, improved transport infrastructure, induced firm turnover and local employment effects.

Electrification Newly commissioned power plants might influence manufacturing firm performance through the supply of electricity, which is a pivotal input to production in many firms. If the supply of electricity is a constraint to production, local electricity generation should increase firms' levels of productivity and output, for instance by providing incentives for investment or by reducing the use of costly electricity generators (Fried and Lagakos 2020). The expected effects on employment are theoretically ambiguous and depend on whether capital and labour are complements or substitutes. In labour-intensive industries, the advent of electricity might be labour-saving, while capital-intensive firms might expect increases in employment to leverage increased levels of productivity.

We estimate Equation 2 on total electricity used in production, total expenditures on electricity and expenditures on generator fuels on our stacked manufacturing sample. Column (1) in Table 5 exhibits small and statistically insignificant effects on total electricity consumption, which is confirmed by a set of alternative specifications (see Figures A.16 to A.18). To the contrary, column (2) indicates that medium-sized and large firms increased expenditures on *purchased* electricity by 9.2% and 11.9% respectively in the post-treatment period. This indicates a switch from self-produced electricity to purchased electricity, which is supported by decreasing expenditures on generator fuels in response to treatment across firms of all size (see column (3)). It is unlikely that increases in expenditures on electricity reflect increases in electricity prices in response to treatment, since the Indonesian government regulates national electricity tariffs (PwC Indonesia 2018). During periods of electrification and grid expansion, it appears that local installation of power generation capacity has not altered the amount of consumed electricity, but the source. Since larger firms are more likely to produce capital- and energy-intensive products (such as metal, rubber or chemicals) rather than small firms, whose production is more labour-intensive (such as tobacco, food products or textiles), the complementarity or substitutability of labour and electricity might explain differences in effects on total employment.

Dependent Variables:	Used Electricity (log kWh) (1)	Electricity Exp. (log IDR) (2)	Generator Fuel Exp. (log IDR) (3)
Variables			
Treatment Group*POST (Small Firms)	-0.014	-0.034	-0.140**
	(0.027)	(0.026)	(0.057)
Treatment Group*POST (Large Firms)	0.033	0.092	-0.635***
	(0.068)	(0.068)	(0.223)
Treatment Group*POST (Medium Firms)	0.027	0.119***	-0.557***
	(0.036)	(0.037)	(0.119)
Fixed-Effects			
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	961,954	925,655	899,426
R^2	0.82534	0.85459	0.60308

Table 5: Effects on Electricity and Generator Fuel Consumption by Firm Size

This table reports OLS estimates of coal-fired power plants coming on-line on local manufacturing firms, differentiated by firm size one year before treatment. Outcome variables are used electricity (1) (in log kWh), total expenditures on electricity (2) and total expenditures on generator fuels (3) (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 yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Here, we exclude firms from our sample, if they are located in villages, which were never treated. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment. Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 firms. Firms with more than 499 employees are considered large firms.
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. Thereby, we interlink the standard set of fixed effects with firm group indicators.
Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

In addition, we compare estimates from coal-fired power plants to the effects of the commissioning of gas-fired power plants, hydropower capacity and geothermal units on local firms. If altered conditions of electricity supply were affecting local firms and were unrelated to specific fuels, we would expect similar plant-level outcomes in villages, which observed the commissioning of power plants propelled by different fuels in near distance. We estimate Equation 2 with four different types of power plant technologies that constitute treatment θ_d in any village d and construct stacked datasets accordingly. Results in Table A.6 display effects from gas-fired power plants on employment, which are of comparable magnitude compared to effects from coal-fired power plants. This is an indication for the intuition that coal-fired power plants spill over to local plants via the electricity channel. However, effects for commissioning of hydropower generation capacities are small and insignificant and even negative for geothermal power plants, if never treated villages are excluded from the control group. Note that this might partially reflect differences in total generation capacity or local circumstances. To some extent, this hints towards fuel-specific spillover effects.

Enhanced Reliability Our results suggest that treatment effects diminish with further distance to commissioned power plants (see Figure A.15) and that medium-sized and large firms increase inputs, output and employment in the post-treatment period, but not total electricity used in production. This might reflect changes in the reliability of electricity supply, which suffers from frequent interruptions in the case of Indonesia (Falentina and Resosudarmo 2019; PwC Indonesia 2018). Indeed, Indonesian firms have constantly reported electricity to be an obstacle for firm performance (World Bank 2009, 2015) and research suggests that grid expansion in Java has increased local employment and manufacturing output (Kassem 2020). Drawing on data from PLN annual reports between 2010 and 2020, we estimate the effects of power plant commissioning on the system average interruption frequency index (SAIFI) and system average interruption duration index (SAIDI) of Indonesian provinces. We report results in Table A.7. In the post-treatment period, SAIFI (SAIDI) decreases (increases) by 3.6% (0.9%), but estimates are statistically insignificant. Note that this expresses province-level variation, which might not reflect village-level differences in electricity transmission. Also, electricity grid performance has likely increased more substantially during earlier points in time, such as our sample period. For the period of our manufacturing sample, more temporarily and spatially disaggregated data is unavailable.

However, increased grid stability and enhanced electricity transmission might help to explain heterogeneous effects on firms. In the absence of large local power capacity and in settings, in which trans-regional transmission might be sub-optimal, energy-intensive firms might choose lower levels of inputs, outputs and production. Consequently, the advent of coal-fired power plants, which often entail a total generation capacity of more than 100 MW, could alter decisions on production inputs in those firms, which require stable transmission of high-voltage electricity to operate machines. This channel, which cannot be tested with the data available at hand, might help to explain both, heterogenous effects on firms of different sizes and locally bounded treatment effects. It might also help to shed light on small and insignificant effects on productivity (see Table A.8).

Infrastructure Development The operation of coal-fired power plants often requires physical transport infrastructure, such as roads or harbours that could encompass positive spillover effects to local manufacturing firms (Donaldson 2018). Based on estimating Equation 2 on our data derived from confidential input-output records in manufacturing plants, we show in column (3) of Table A.11 that the transport weight of manufacturing inputs and outputs increased by 6.9% in the post-treatment period. Note that we use total shipped weight as a proxy of demand for transport infrastructure in local firms. We also exploit data on road conditions and infrastructure investments in Indonesian districts and show in Table A.10 that the commissioning of coal-fired power plants is associated with an increase in villages with asphalt roads (column (2)), kilometers of roads classified as ‘good’ (column (3)) and district-level investments in road infrastructure (column (4)) in the post-treatment period. We therefore provide supporting insights that coal-fired power plants spill over to local manufacturing firms by local transport infrastructure provision, which might help to explain locally bounded effects and differences to geothermal and hydropower capacities (see Table A.6), since the operation of such plants does not require shipping of fuel inputs. However, this channel might be blocked in the case of mine-to-mouth plants, where coal is burned in direct proximity to mining.

Agglomeration Effects, Firm Entries and Wages As Kassem (2020) argues, changes in the local energy infrastructure might influence firm performance by lowering entry barriers for new firms, who increase competition for local inputs and labour. This might lead to the subsequent exit of less productive firms, which Kassem identifies as a reaction to grid expansion in Java. In a similar fashion, we document changes in the total number of entering firms on the village-level by using village-year-level information on the total number of firm entries in a variation of Equation 1, in which we add province-year and village-level fixed effects. The commissioning of coal-fired power plants leads to an increase in 0.046 firm entries per year in each village within 50 kilometers distance. Note that the average number of entering firms per year in our sample is 0.108. Entering firms are mostly small, which is reasonable, given that it requires less investments and resources compared to establishing a large firm.

We also inspect differences in local wages in Table A.11, separated by production workers (1) and non-production workers (2). We find that wages increased by 4.3% (5.8%) for production workers (non-production workers) in the post-treatment period.

Both mechanisms, the increase in entering firms, who are mostly small, and in local wages, might help to shed light on negative treatment effects on employment in *incumbent* small firms (column (1) in Table 4). Since changes in aggregate population, labour force and industrial employment remain insignificant post-treatment (column (1) in Table 2 and column (1), (2) and (3) respectively in Table A.12), additional labour demand by (incumbent) large and medium-sized firms as well as by entering (small) firms lead to higher local wages, which might in turn reduce labour demand in smaller firms. This suggests that the commissioning of coal-fired power plants induces reallocation of labour from small (incumbent) manufacturing plants to medium-sized or large or entering firms.

6.2 Coal and Industrialization: Implications for Climate Policy

Early industrialization in Europe has gone hand in hand with an expansion of coal-fired power generation. Burning abundant fossil fuel resources enabled manufacturing growth, which contributed substantially to overall economic development. But even beyond the provision of cheap electricity, coal-fired power infrastructure is likely to having enabled industrial development, for instance by facilitating the roll-out of local transport infrastructure. It therefore becomes apparent why policy makers in today's industrializing economies frequently highlight the necessity of energy infrastructure in general and coal-fired power plants in particular in processes of economic development.

Whether coal-fired power plant commissioning causes manufacturing firms to grow is therefore a question of concern to global prospects of sustainable development. Admittedly, this question is almost impossible to be answered, since power plant allocation usually obeys to specific local circumstances, such as expected electricity demand or access to coal supply. Leveraging quasi-random variation in timing of power plant commissioning in Indonesia, we do however show that such power plants caused growth in output, input and employment among large and medium-sized firms during phases of industrial development. We also provide evidence indicative of local labour reallocation towards larger firms, which are usually more productive than smaller firms.

Even though the external validity of our results is in doubt, it becomes apparent why (industrial) policy makers, especially in industrializing economies, often favour investments in coal-fired power technology. Any calls to phase down coal or to inhibit investments in new fossil-fuelled power generation capacities need to take these positive externalities from power plants into consideration. After all, successful economic development is likely to require flourishing industries. How the resulting additional demand for energy in general and electricity in particular is met is hence of importance to global climate policy.

6.3 Limitations

In this study, we assess the causal spillover effects from power plant commissioning to local 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 thereby facilitates the reduction of bias from endogenous treatment, from unobserved confounding factor or from selection into earlier or later treatment. Nevertheless, we subsequently discuss concerns about internal and external validity of our results.

We cannot conclude from our study that *any* firm in Indonesia would have observed the same changes in input, output and employment as those firms from those regions, which were identified to be suitable for power plant operation. This follows from our results capturing treatment effects on the treated, which accounts for the unobserved endogeneity in decisions on power plant allocation.

Since our sample period mostly covers phases of (early) industrial development in Indonesia, we are unable to claim validity of our results 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 on spillovers to firms, which do not appear in the Indonesian Manufacturing Census, for instance because they employ less than 20 workers. In addition, increases in firm entries in response to coal-fired power plant commissioning might to some extent be driven by employment growth in existing Indonesian micro-enterprises.

In this study, we also focus on the effects at the individual manufacturing plant-level in the four years post-treatment. We thereby refrain from investigating 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*. For instance, local environmental externalities might influence local labour markets by induced emigration, thereby exacerbating increments in local wages. Those factors, which might also counteract or exacerbate spillover effects, remain unobserved by the researcher.

7 Conclusion

We observe substantial heterogeneity in spillover effects from coal-fired power plant commissioning to incumbent manufacturing firms in Indonesia. Leveraging high-resolution manufacturing and power plant data and quasi-random timing of treatment, we show that large and medium-sized firms (with more than 100 workers) increase employment, input, output and value added in the four years after commissioning. To the contrary, firm performance in small firms (20 to 99 workers) remains largely unaffected, with a documented decrease in employment of 2.4% being a notable exception. We suspect enhanced access to electricity, local transport infrastructure development and regional labour reallocation mechanisms to explain our results.

Spillovers to local manufacturing firms are apparent in the context of early stages of industrialization in Indonesia between 1984 and 2015. In the past years, researchers have drawn attention to avenues, which could help to render the phase *out* of coal-fired power plants acceptable to different stakeholders. How to enable economic development in regions and industries, which are eager to phase *in* coal-fired electricity generation aspiring to boost local manufacturing, appears to be a fruitful direction for future research.

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A Appendix

A.1 Tables

	Small Firms			Medium-Sized Firms			Large Firms		
Variable	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Labour	39.83	32	20.29	217.80	185	103.7	1342.07	925	1406.8
Inputs (IDR)	6,605,611	1,010,570	39,741,250	77,321,481	17,888,327	207,726,529	226,540,159	91,939,209	378,268,119
Outputs (IDR)	10,036,008	1,860,000	52,353,865	116,415,411	33,436,030	280,053,225	382,455,049	169,686,258	568,380,651
Value Added (IDR)	3,482,742	681,692	16,651,311	39,529,880	11,915,687	101,667,608	157,304,269	67,056,100	259,545,251
Electricity (kWh)	103,867	5,320	1,532,087	1,160,187	150,000	6,481,858	4,051,828	734,789	13,412,803
Wages (IDR)	12,564	9,556	15,998	17,024	13,418	15,012	13,193	11,772	15,139
Total Factor Productivity	3.43	3.84	4.46	2.17	3.5	6.2	2.27	3	10.1

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

This table displays summary statistics for main outcome variables in our manufacturing plant sample based on data from the Indonesian manufacturing census. In this table, firms are clustered according to total employment in 2010. Small firms comprise firms with 20 to 99 workers. Medium-sized firms comprise firms with 100 to 499 workers. Firms employing more than 499 workers qualify as large.

		Control Group	
Cohort	Treated	Treated Ever	Never Treated
1985	9	1621	1316
1988	10	2010	1658
1989	13	2121	1775
1992	520	2381	2293
1993	159	2727	2399
1994	79	2847	2506
1995	3	2957	2589
1996	26	3017	2661
1997	28	3113	2768
1998	4	3151	2875
1999	129	3181	2941
2000	162	3299	3027
2004	321	4090	4006
2006	687	3709	3989
2007	320	3799	3909
2008	349	3788	3807
2009	73	3846	3715
2010	83	3725	3537
2011	738	3213	3451
2012	958	2764	3368
2013	214	2669	3276
2014	187	2576	3103
2015	194	2404	2973

Table A.2: Treated and Non-Treated Villages in each Cohort

This table displays the number of villages within distance of 50 km to commissioned coal-fired power plants in each respective cohort year ('Treated'). Manufacturing firms, which are located in those villages, are eligible for the treatment group in each cohort. Firms from all other villages are eligible for the control group, which might - depending on the respective specification - include or exclude villages, which were ever ('Treated Ever') (or never '(Never Treated)') within distance to a coal-fired power plant.

Dependent Variables:	Labour	Output	Input	Value Added	Labour	Output	Input	Value Added	Labour	Output	Input	Value Added
Firm Group		Large Firms				Medium-Sized Firms				Small Firms		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Treatment Group (t = -4)	-0.007	-0.027	-0.088**	0.006	-0.013	-0.056**	-0.041	-0.062***	0.0006	-0.038**	-0.037*	-0.011
	(0.019)	(0.035)	(0.043)	(0.033)	(0.011)	(0.022)	(0.026)	(0.023)	(0.006)	(0.018)	(0.020)	(0.017)
Treatment Group (t = -3)	-0.006	-0.025	-0.019	0.016	-0.011	-0.029	-0.027	-0.029	0.006	-0.010	-0.017	0.002
	(0.017)	(0.029)	(0.033)	(0.029)	(0.009)	(0.019)	(0.021)	(0.020)	(0.005)	(0.016)	(0.017)	(0.015)
Treatment Group (t = -2)	0.011	-0.019	-0.032	0.016	0.001	0.004	-0.002	0.001	0.004	0.020*	-0.003	0.017
	(0.012)	(0.021)	(0.024)	(0.022)	(0.006)	(0.014)	(0.015)	(0.015)	(0.004)	(0.012)	(0.013)	(0.012)
Treatment Group (t = 0)	0.037***	0.005	0.007	0.009	0.020***	0.022	0.029*	0.023	0.0008	0.0006	0.016	0.016
	(0.012)	(0.024)	(0.023)	(0.023)	(0.007)	(0.015)	(0.016)	(0.015)	(0.004)	(0.011)	(0.012)	(0.011)
Treatment Group (t = 1)	0.074***	0.081**	0.055*	0.069**	0.034***	0.068***	0.079***	0.055***	0.003	0.008	0.012	0.013
	(0.017)	(0.032)	(0.033)	(0.033)	(0.009)	(0.020)	(0.022)	(0.020)	(0.006)	(0.015)	(0.017)	(0.015)
Treatment Group (t = 2)	0.089***	0.079**	0.060	0.101***	0.049***	0.076***	0.059**	0.054**	0.015**	0.014	0.015	0.013
	(0.021)	(0.036)	(0.038)	(0.039)	(0.011)	(0.024)	(0.026)	(0.023)	(0.007)	(0.018)	(0.021)	(0.018)
Treatment Group (t = 3)	0.092***	0.051	0.036	0.066	0.043***	0.063**	0.050*	0.026	0.014	0.026	0.034	0.025
	(0.025)	(0.040)	(0.043)	(0.043)	(0.013)	(0.026)	(0.029)	(0.026)	(0.009)	(0.021)	(0.024)	(0.021)
Treatment Group (t = 4)	0.099***	0.035	0.021	0.023	0.036*	0.065**	0.058*	0.034	0.024**	0.034	0.055**	0.030
	(0.028)	(0.047)	(0.050)	(0.050)	(0.018)	(0.029)	(0.033)	(0.030)	(0.011)	(0.025)	(0.027)	(0.024)
Fixed-Effects												
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	191,511	180,714	180,126	191,467	470,008	448,042	447,190	469,993	1,224,659	1,139,447	1,173,075	1,224,683
R^2	0.63898	0.79183	0.75751	0.75454	0.59982	0.82719	0.80505	0.77902	0.69243	0.82917	0.81856	0.81699

Table A.3: Dynamic Effects on Labour, Output, Input and Value Added by Firm Size Including Never Treated Firms in Control Group

This table reports dynamic OLS estimates of coal-fired power plants coming on-line on local manufacturing firms, differentiated by firm size one year before treatment. Outcome variables are labour demand (in log total employees) in columns (1), (5) and (9), total output 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 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 yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 employees. Firms with more than 499 employees are considered large firms.

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. Thereby, we interlink a standard set of fixed effects with firm group indicators. Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

Dependent Variables:	Labour (1)	Output (2)	Input (3)	Value Added (4)
Variables				
Treatment Group*POST (Small Firms)	-0.016*	-0.004	0.003	-0.011
	(0.009)	(0.019)	(0.021)	(0.018)
Treatment Group*POST (Medium Firms)	0.060***	0.099***	0.111***	0.079***
	(0.014)	(0.025)	(0.029)	(0.026)
Treatment Group*POST (Large Firms)	0.090***	0.107***	0.118***	0.090**
	(0.023)	(0.036)	(0.042)	(0.039)
Fixed-Effects				
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	1,157,663	1,105,357	1,100,033	1,157,538
R^2	0.92350	0.90581	0.88326	0.88172

Table A.4: Effects on Labour, Output, Input and Value Added in Firms including Manufacturing Firms from Villages which are eligible for Treatment

This table reports OLS estimates of coal-fired power plants coming on-line on local manufacturing firms, differentiated by firm size one year before treatment. Outcome variables are labour demand (1) (in log total employees), total output (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 treatment is assigned.
Other than in 4, we include firms to the control group from villages, in which coal-fired power plant operation was announced, but that were never treated.
The unit of observation is an Indonesian manufacturing firm, for which we have yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 firms. Firms with more than 499 employees are considered large firms.
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. Thereby, we interlink the standard set of fixed effects with firm group indicators.
Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

Dependent Variables:	Labour (1)	Output (2)	Input (3)	Value Added (4)
Variables				
Treatment Group*POST (Small Firms)	0.004	-0.029**	-0.022	-0.028**
	(0.006)	(0.014)	(0.016)	(0.013)
Treatment Group*POST (Medium Firms)	-0.012	-0.035*	-0.039*	-0.025
	(0.012)	(0.020)	(0.023)	(0.020)
Treatment Group*POST (Large Firms)	0.008	0.004	0.019	0.040
	(0.019)	(0.031)	(0.035)	(0.032)
Fixed-Effects				
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	1,154,384	1,081,966	1,077,279	1,154,248
R^2	0.92475	0.91926	0.89993	0.89269

Table A.5: Effects on Labour, Output, Input and Value Added in Firms from Neighbouring Villages

This table reports OLS estimates of coal-fired power plants coming on-line on local manufacturing firms from villages that neighbour treated villages, differentiated by firm size one year before treatment. Neighbouring 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. Firms from treated villages are removed from the treatment group. Outcome variables are labour demand (1) (in log total employees), total output (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 treatment is assigned.

The unit of observation is an Indonesian manufacturing firm, for which we have yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 firms. Firms with more than 499 employees are considered large firms.

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. Thereby, we interlink the standard set of fixed effects with firm group indicators.

Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

Dependent Variable:	Labour							
Plant Type:	Coal		Gas		Hydropower		Geothermal	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Treatment Group*POST (Small Firms)	-0.023**	0.004	0.017**	0.026***	0.012	0.010	-0.020*	0.002
	(0.009)	(0.008)	(0.008)	(0.006)	(0.010)	(0.008)	(0.011)	(0.008)
Treatment Group*POST (Medium Firms)	0.053***	0.040***	0.059***	0.073***	0.044*	0.048***	-0.057***	-0.019
	(0.014)	(0.013)	(0.013)	(0.011)	(0.024)	(0.018)	(0.020)	(0.014)
Treatment Group*POST (Large Firms)	0.083***	0.094***	0.086***	0.125***	0.037	0.020	-0.069**	0.007
	(0.023)	(0.022)	(0.024)	(0.018)	(0.042)	(0.031)	(0.032)	(0.022)
Fixed-Effects								
Firm-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
Event Time-Cohort-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
Year t = 0 -Firm Group-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
All Observations	No	Yes	No	Yes	No	Yes	No	Yes
Treated Ever	Yes	No	Yes	No	Yes	No	Yes	No
Observations	861,582	1,527,388	600,517	1,823,825	323,471	1,456,293	174,698	1,195,002
R^2	0.92235	0.92114	0.92495	0.91767	0.91567	0.91779	0.91302	0.92085

Table A.6: Effects on Labour by Firm Size and Power Plant Technology

This table reports OLS estimates of power plants coming on-line on local manufacturing firms, differentiated by firm size one year before treatment and by technology of power plant. The outcome variable is labour demand (in log total employees). For each type of power plant, we estimate Equation 2 on a sample, in which we exclude never treated villages (columns (1), (3), (5) and (7)), and on the full sample (columns (2), (4), (6) and (8)). 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 yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 firms. Firms with more than 499 employees are considered large firms.
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. Thereby, we interlink the standard set of fixed effects with firm group indicators.
Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

Dependent Variables:	SAIFI (log)	SAIDI (log)
	(1)	(2)
Variables		
Treatment Group*POST	-0.036	0.009
	(0.110)	(0.161)
Fixed-Effects		
Province-FE	Yes	Yes
Treatment Group-Cohort-FE	Yes	Yes
Event Time-Cohort-FE	Yes	Yes
Year t = 0	Yes	Yes
Observations	1,295	1,295
R^2	0.68770	0.68268

Table A.7: Effects on Province-Level SAIDI and SAIFI between 2010 and 2020

This table reports OLS estimates of coal-fired power plants coming on-line on system average interruption duration index (SAIDI, in log) and on system average interruption frequency index (SAIFI, in log). These are coefficients from a regression similar to Equation 1. 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 yearly observations. Province-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative 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-firm group-year Zero-FE.
Significance levels are: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Dependent Variables:	Labor Productivity (log) (1)	Output per Worker (2)	Total Factor Producitivity (3)
Variables			
Treatment Group*POST (Small Firms)	0.002	0.008	-0.007
	(0.016)	(0.016)	(0.017)
Treatment Group*POST (Medium Firms)	0.017	0.040*	-0.003
	(0.023)	(0.022)	(0.025)
Treatment Group*POST (Large Firms)	0.010	0.018	0.014
	(0.035)	(0.032)	(0.037)
Fixed-Effects			
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	1,049,246	999,804	675,261
R^2	0.79970	0.83799	0.80911

Table A.8: Effects on Productivity Measures by Firm Size

This table reports OLS estimates of coal-fired power plants coming on-line on local manufacturing firms, differentiated by firm size one year before treatment. Outcome variables are labour productivity (1) (in log), output per worker (2) (in log) and total factor productivity (TFP) (4). TFP is estimated according to Levinsohn and Petrin (2003) with electricity consumption (kWh) as the instrumental variable. 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 yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Here, we exclude firms from our sample, if they are located in villages, which were never treated. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 firms. Firms with more than 499 employees are considered large firms.
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. Thereby, we interlink the standard set of fixed effects with firm group indicators.
Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

Dependent Variables:	Total Number of Firms	Small Firms	Medium-Sized Firms	Large Firms
	(1)	(2)	(3)	(4)
Variables				
Treatment Group*POST	0.046***	0.030***	0.012***	0.004**
	(0.009)	(0.007)	(0.003)	(0.002)
Fixed-Effects				
Village-FE	Yes	Yes	Yes	Yes
Treatment Group-Cohort-FE	Yes	Yes	Yes	Yes
Event Time-Cohort-FE	Yes	Yes	Yes	Yes
Province-Year-FE	Yes	Yes	Yes	Yes
Year t = 0-Treatment Group-FE	Yes	Yes	Yes	Yes
Observations	760,695	760,695	760,695	760,695
R^2	0.24979	0.21384	0.22231	0.16791

Table A.9: Effects on Firm Entries

This table reports OLS estimates of coal-fired power plants coming on-line on the number of entering manufacturing firms at the village-level. Outcome variables are the total number of newly established manufacturing firms (1), the number of newly established small firms (20 to 99 workers) (2), the number of newly established medium-sized firms (100 to 499 workers) (3) and the total number of large firms (more than 500 workers) (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 workers. 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. Village-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Here, we exclude villages from our sample, if they 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	GDP (Log) (1)	Asphalt Road Share (Log) (2)	km of Good Road (Log) (3)	Road Infr. Investments (Log) (4)
Variables				
Treatment Group*POST	0.081	0.031	0.059	0.259**
	(0.062)	(0.027)	(0.270)	(0.117)
Fixed-Effects				
District-FE	Yes	Yes	Yes	Yes
Island-Year-FE	Yes	Yes	Yes	Yes
Treatment Group-Cohort-FE	Yes	Yes	Yes	Yes
Event Time-Cohort-FE	Yes	Yes	Yes	Yes
Year t = 0	Yes	Yes	Yes	Yes
Observations	3,702	1,931	1,033	1,753
R^2	0.99174	0.91612	0.79151	0.86371

Table A.10: Effects on District-Level GDP and Transport Infrastructure

This table reports OLS estimates of coal-fired power plants coming on-line on district-level GDP and transport infrastructure. Outcome variables are GDP (1) (log), the share of villages with asphalt roads (2) (log), the total district-level kilometers of roads classified as good (3) (log) and the total amount of district-level invesments in road infrastructure (4) (log IDR). These are coefficients from a regression similar to Equation 1. 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 yearly observations. District-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked 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-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 Variables:	Wages (Prod. - Log) (1)	Wages (Other - Log) (2)	Transported Materials (log t) (3)
Variables			
Treatment Group*POST	0.043***	0.058***	0.069*
	(0.010)	(0.016)	(0.037)
Fixed-Effects			
Firm-FE	Yes	Yes	Yes
Event Time-Cohort-FE	Yes	Yes	Yes
Treatment Group-Cohort-FE	Yes	Yes	Yes
Industry (2-Digit)-Year FE	Yes	Yes	Yes
Island-Year-FE	Yes	Yes	Yes
Year t = 0	Yes	Yes	Yes
Observations	1,046,268	898,391	509,936
R^2	0.80053	0.81619	0.74627

Table A.11: Effects on Local Wages and Transport Capacity

This table reports OLS estimates of coal-fired power plants coming on-line on local manufacturing firms. Outcome variables are wages for production workers (1) (in log IDR), wages for non-production workers (2) (in log IDR), and total transported inputs and outputs (in t). These are coefficients from 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 manufacturing firm, for which we have yearly observations. Firm-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked relative to event-time. Here, we exclude firms from our sample, if they are located in villages, which were never treated. Firms are assigned to firm groups for each cohort according to the number of employees one year before treatment ($\tau = -1$). Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 firms. Firms with more than 499 employees are considered large firms.
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. Thereby, we interlink the standard set of fixed effects with firm group indicators.
Significance levels are: * p<0.1, ** p<0.05, *** p<0.01

Dependent Variable	Population (log) (1)	Labor Force (Log) (2)	Under employed (in 1000) (3)	Employed in Industry (Log) (4)	People in Poverty (Log) (5)
Variables					
Treatment Group*POST	-0.003	-0.011	-0.018	-0.023	-0.021
	(0.019)	(0.015)	(0.022)	(0.070)	(0.040)
Fixed-Effects					
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	8,007	2,157	2,157	2,156	3,574
R^2	0.98685	0.99629	0.98378	0.96935	0.96465

Table A.12: Effects on Local Population and Employment

This table reports OLS estimates of coal-fired power plants coming on-line on district-level population and employment. Outcome variables are total population (1), total labour force (2), total underemployment (3), employment in industry (4) and people in poverty (5) (all in log). These are coefficients regression similar to Equation 1. 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 yearly observations. District-year-observations are assigned to treatment or control group for each cohort year. Different cohorts are stacked 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

A.2 Figures

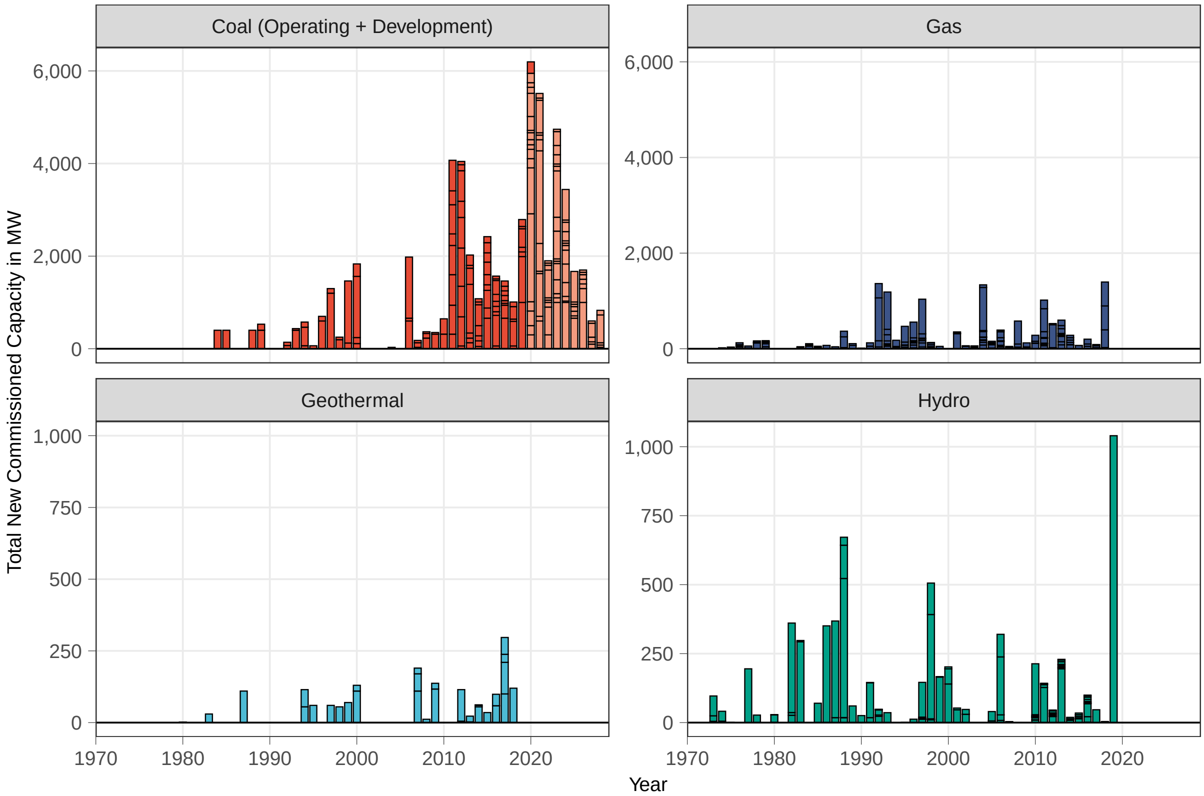
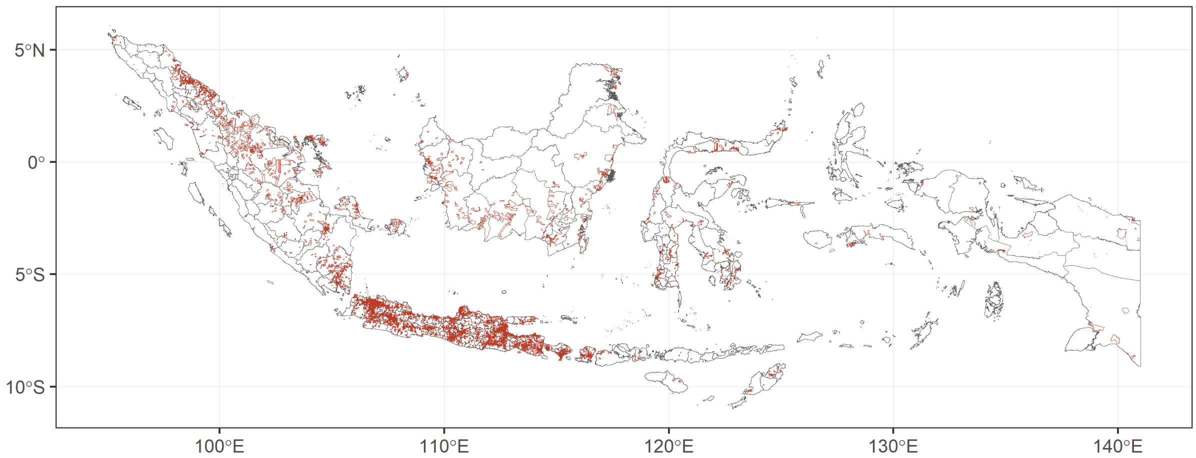
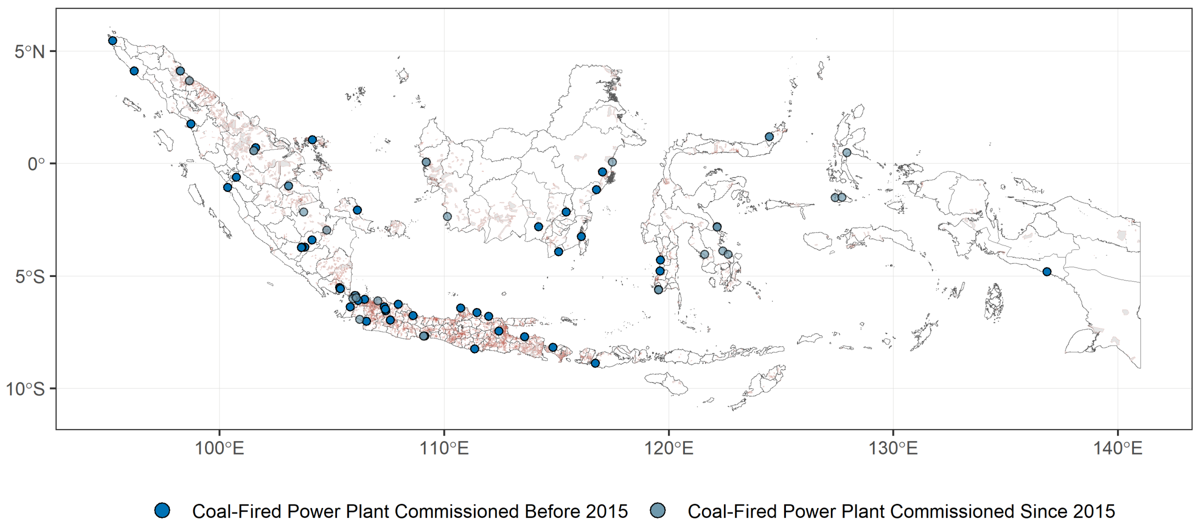


Figure A.1: Commissioned Power Plant Capacity in Indonesia by Technology

This figure displays the dispersion of power plant commissioning in our power plant sample. Data on aggregate power plant capacity (in MW) and the year of initial operation stem from Global Energy Monitor (2021b), Global Powerplant Database (Byers et al. 2019) and the World Electric Power Plants Database (PLATTS 2017).

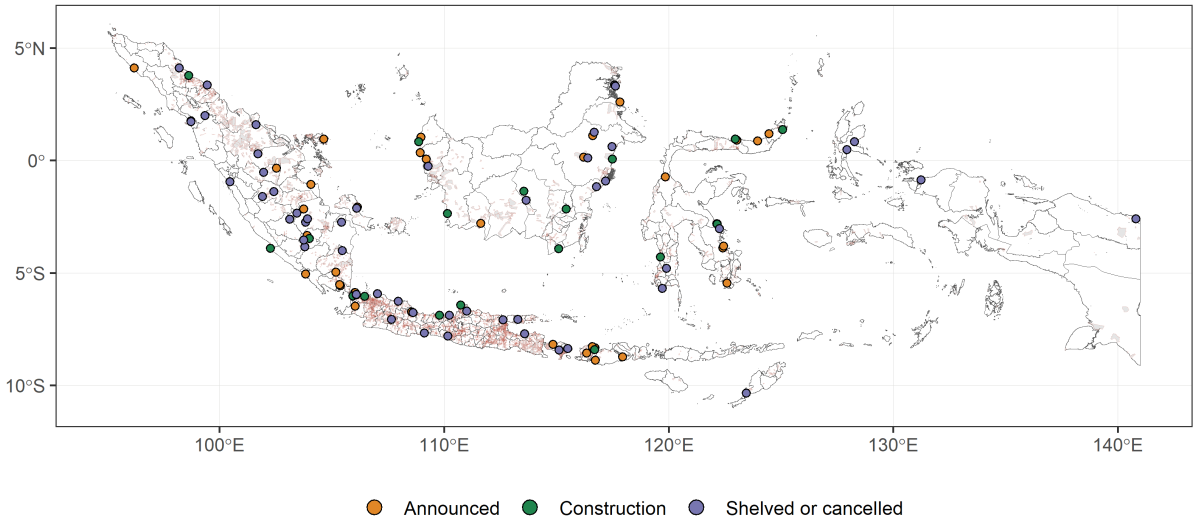


(a) Villages with Manufacturing Firms

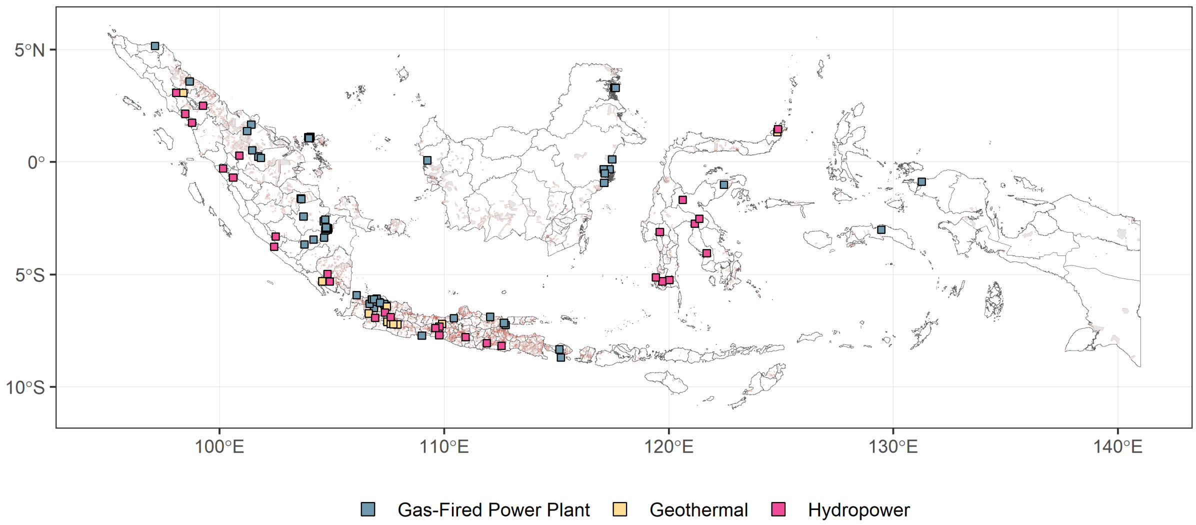


(b) Commissioned Coal-Fired Power Plants between 1984 and 2020

Figure A.2: Villages, Power Plants, Coal Mines in Indonesia

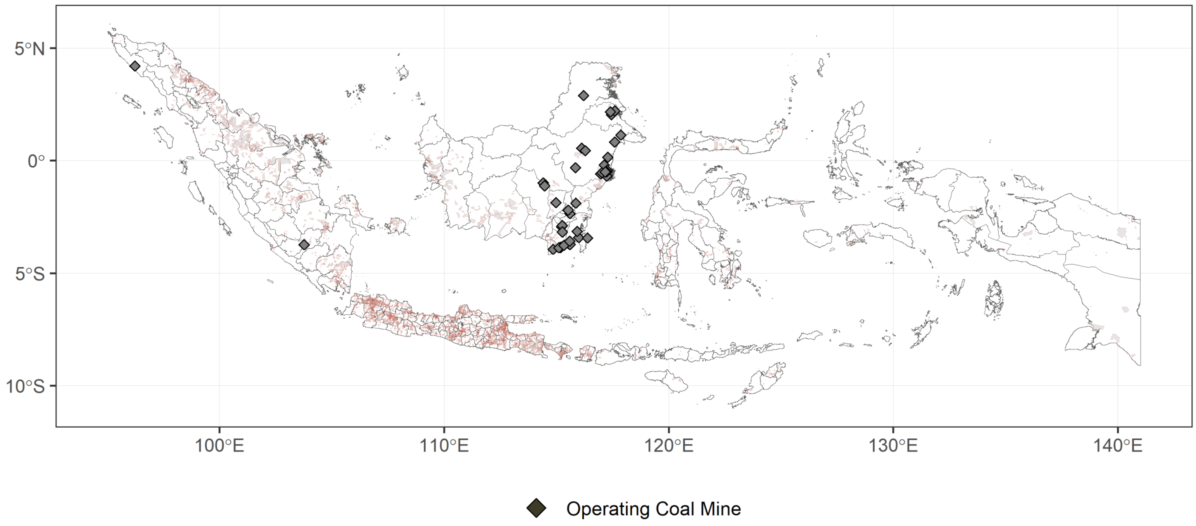


(c) Coal-Fired Power Plants in Indonesia which were announced, are under construction or were shelved or cancelled



(d) Commissioned Gas-fired, Hydro- and Geothermal Power Plants in Indonesia between 1984 and 2015

Figure A.2: Villages, Power Plants, Coal Mines in Indonesia



(e) Operating Coal Mines in Indonesia

Figure A.2: Villages, Power Plants, Coal Mines in Indonesia

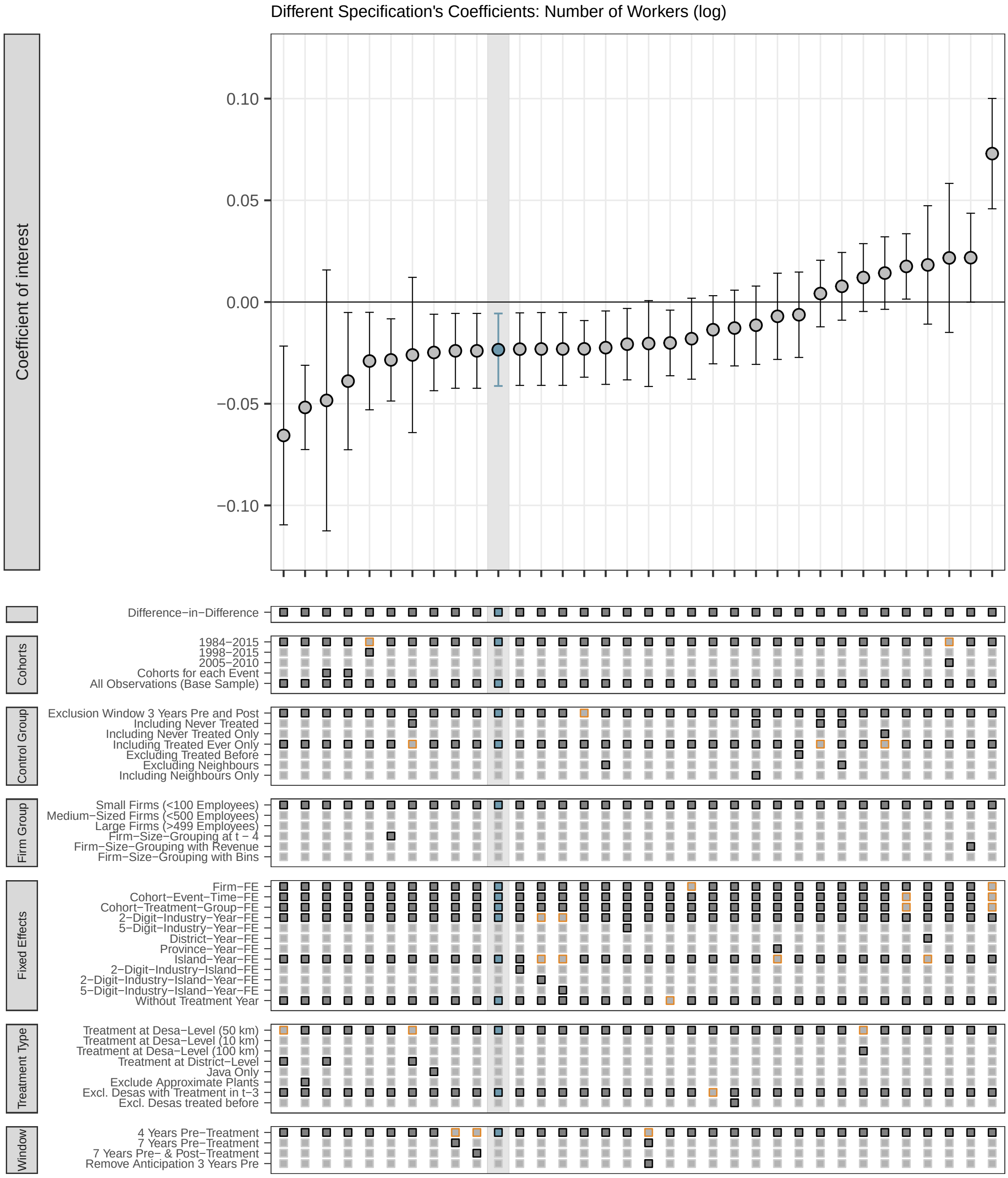


Figure A.3: Coefficients from multiple specifications: Number of Workers (log) - Small Firms

This figure shows coefficients from various estimations of firms' labour demand in number of employed workers (log) on a variable 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 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

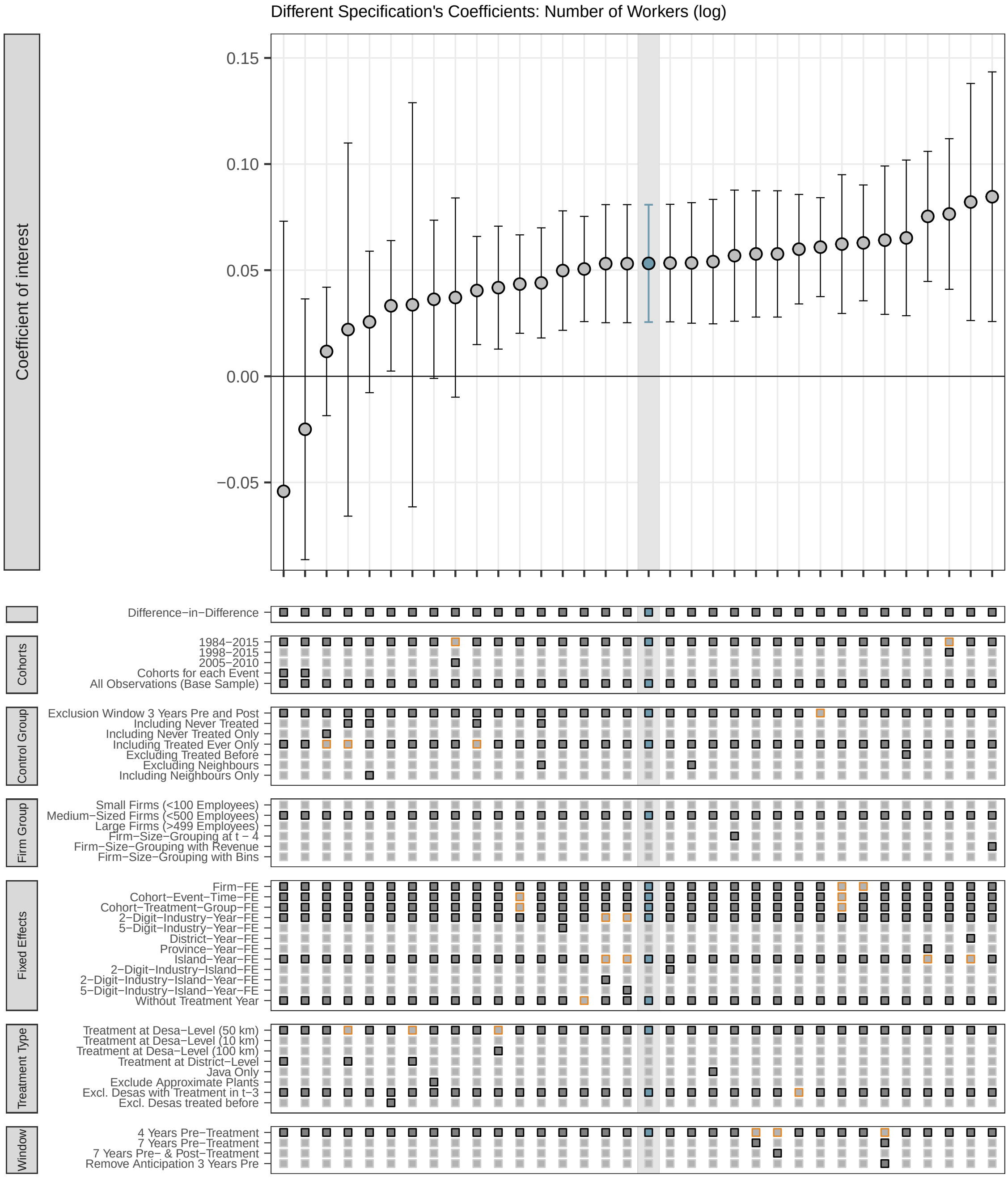


Figure A.4: Coefficients from multiple specifications: Number of Workers (log) - Medium-Sized Firms

This figure shows coefficients from various estimations of firms' labour demand in number of employed workers (log) on a variable 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 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

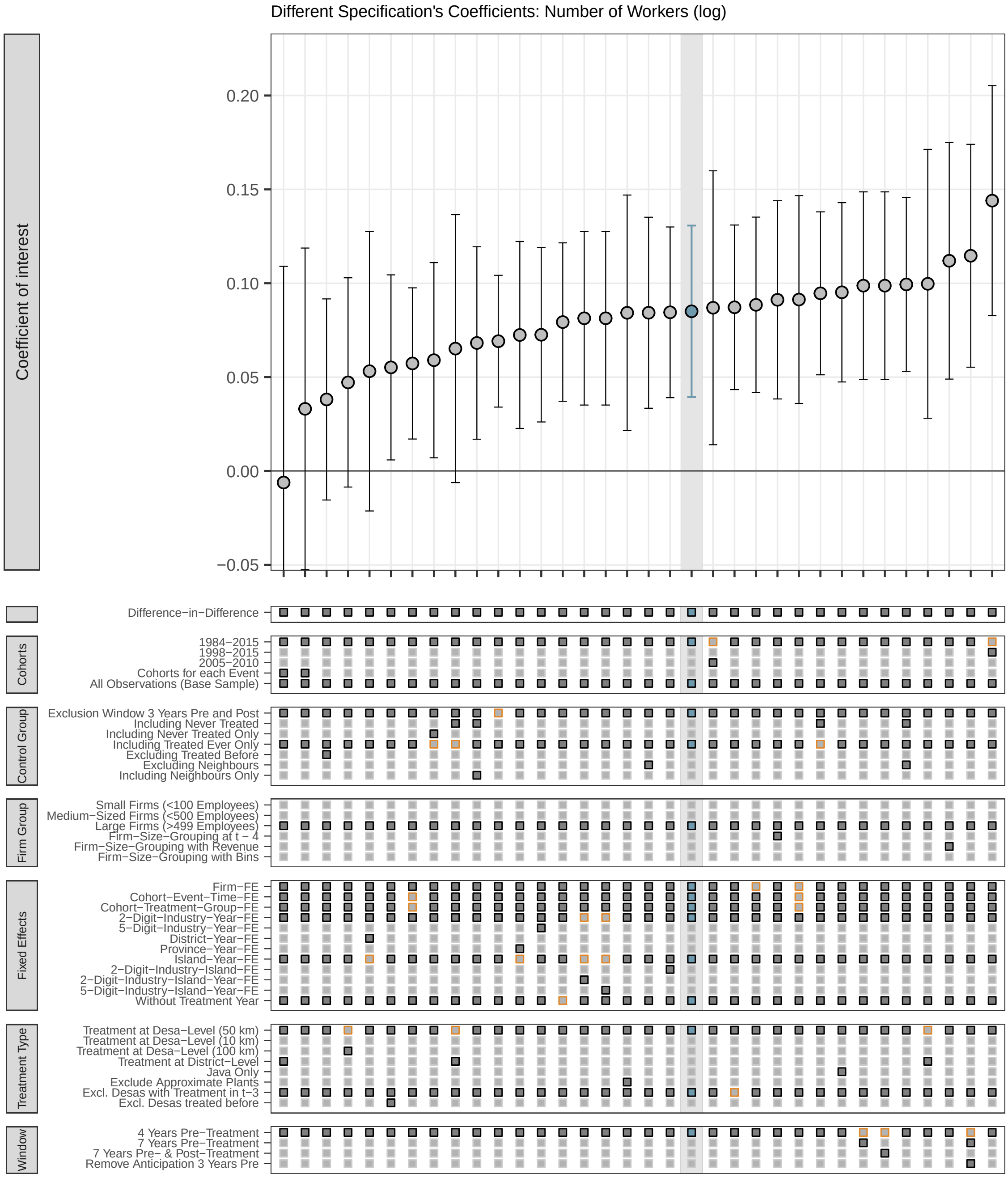


Figure A.5: Coefficients from multiple specifications: Number of Workers (log) - Large Firms

This figure shows coefficients from various estimations of firms' labour demand in number of employed workers (log) on a variable 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 more than 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

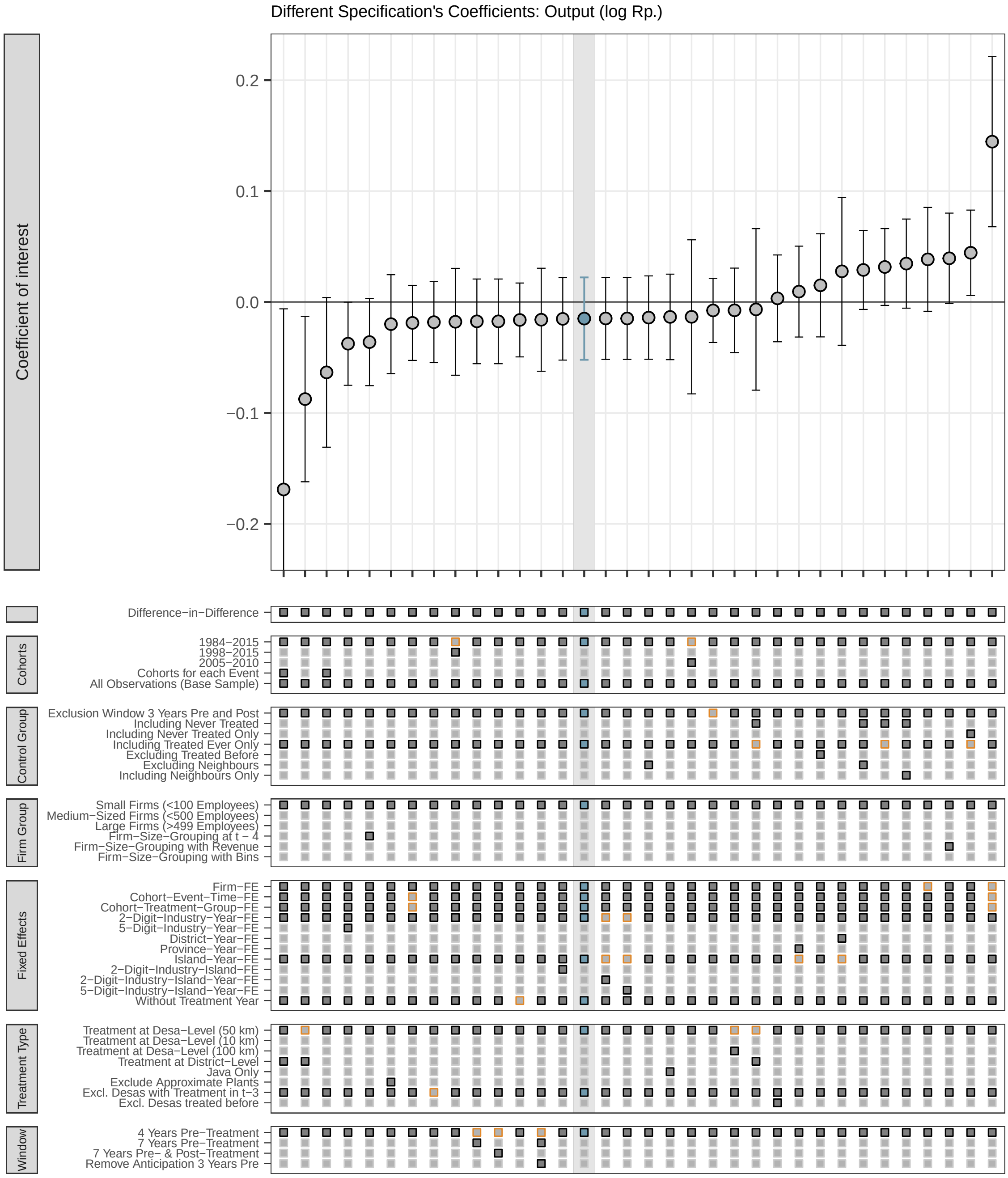


Figure A.6: Coefficients from multiple specifications: Output (log IDR) - Small Firms

This figure shows coefficients from various estimations of firms' total production output (in log IDR) on a variable 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 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

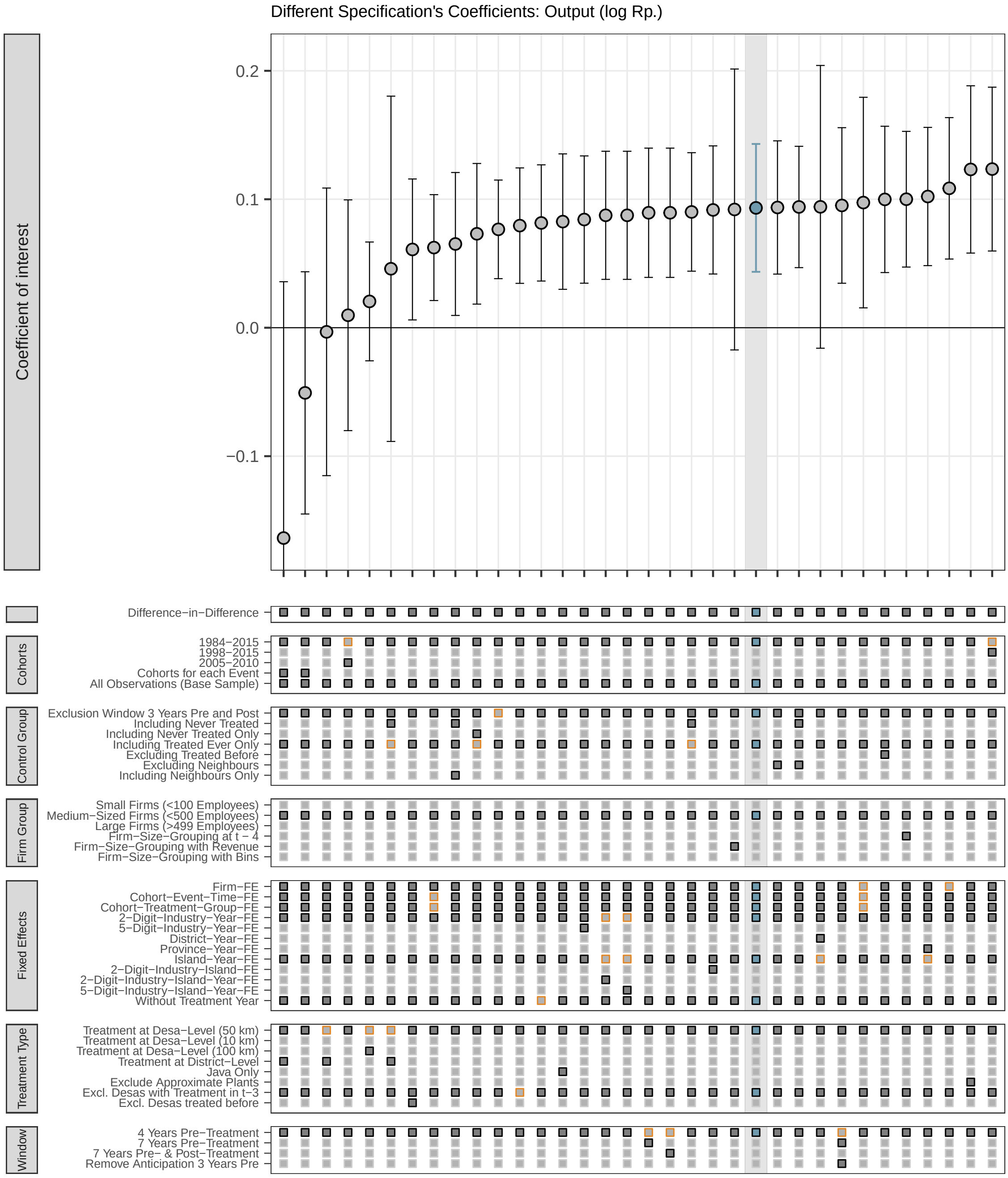


Figure A.7: Coefficients from multiple specifications: Output (log IDR) - Medium-Sized Firms

This figure shows coefficients from various estimations of firms' total production output (in log IDR) on a variable 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 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

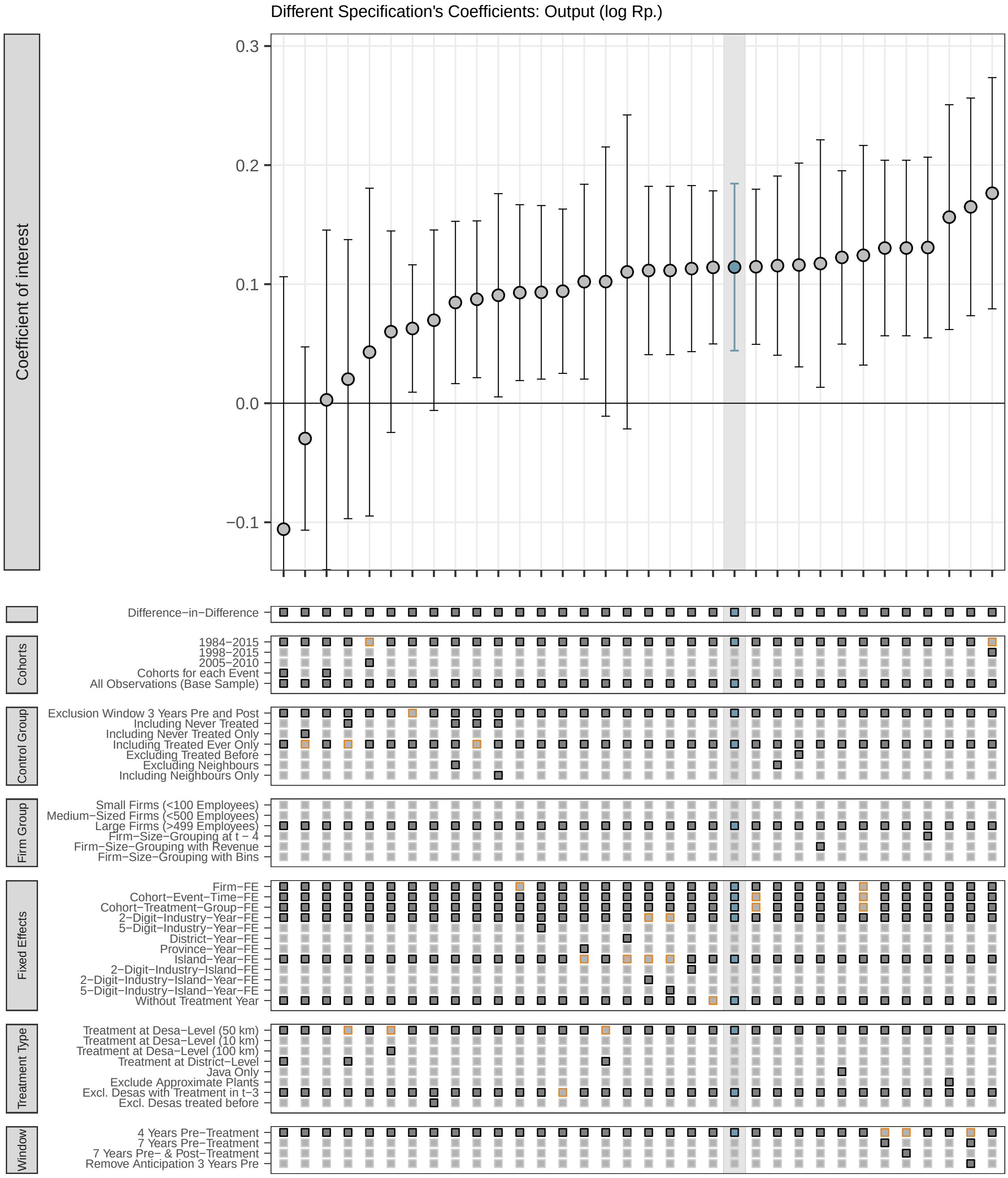


Figure A.8: Coefficients from multiple specifications: Output (log IDR) - Large Firms

This figure shows coefficients from various estimations of firms' total production output (in log IDR) on a variable 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 more than 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

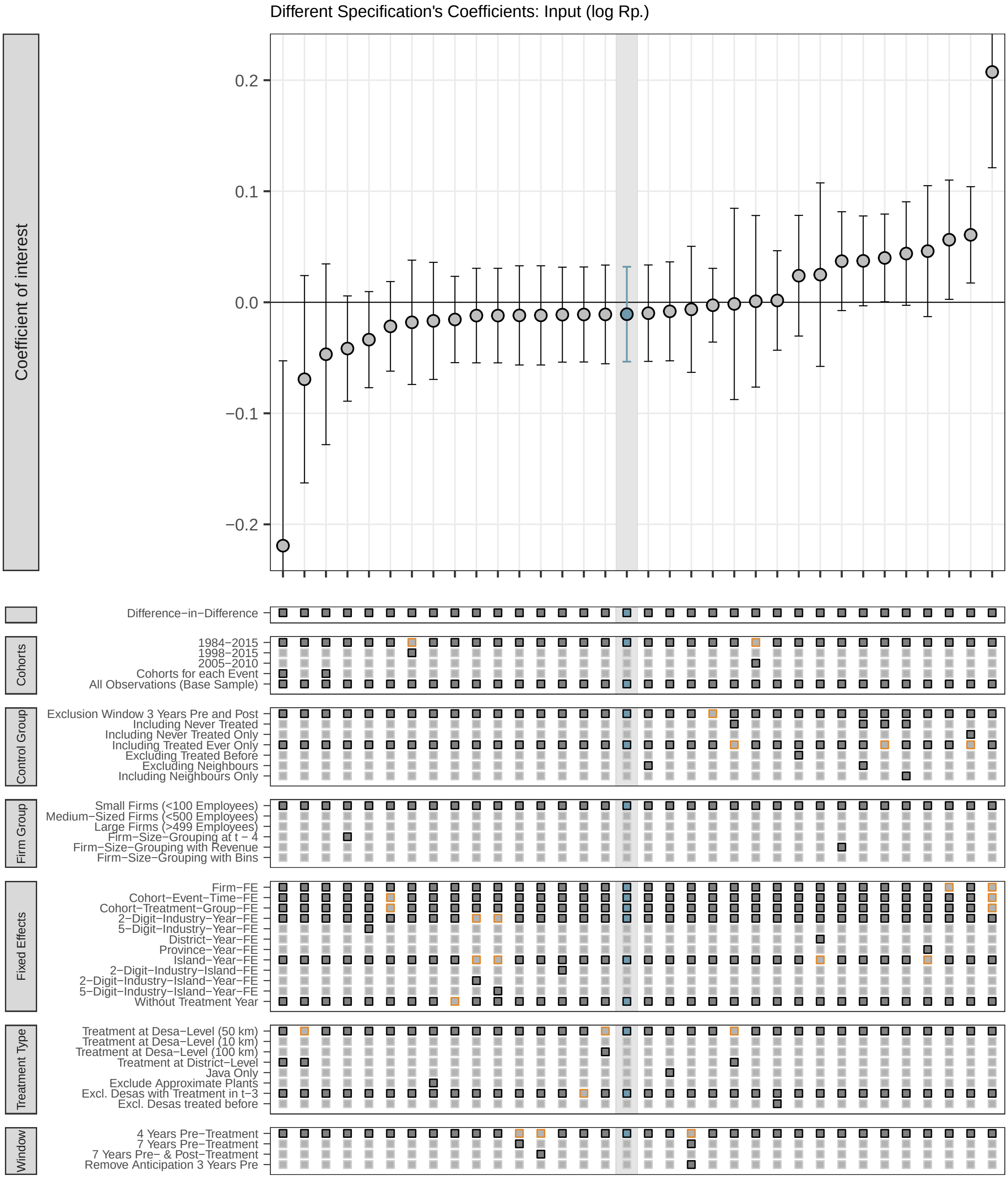


Figure A.9: Coefficients from multiple specifications: Input (log IDR) - Small Firms

This figure shows coefficients from various estimations of firms' total inputs to production (in log IDR) on a variable 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 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

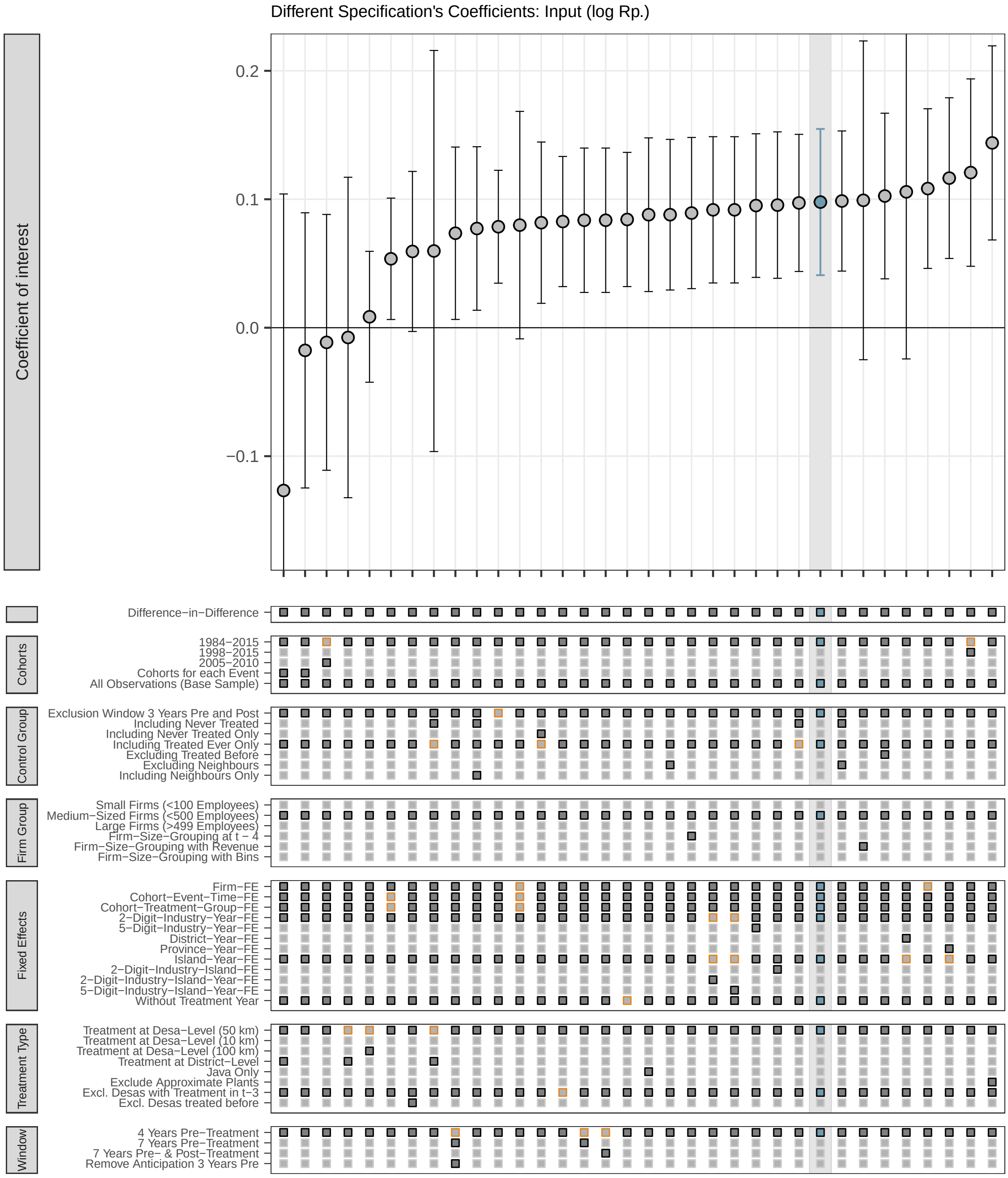


Figure A.10: Coefficients from multiple specifications: Input (log IDR) - Medium-Sized Firms

This figure shows coefficients from various estimations of firms' total inputs to production (in log IDR) on a variable 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 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

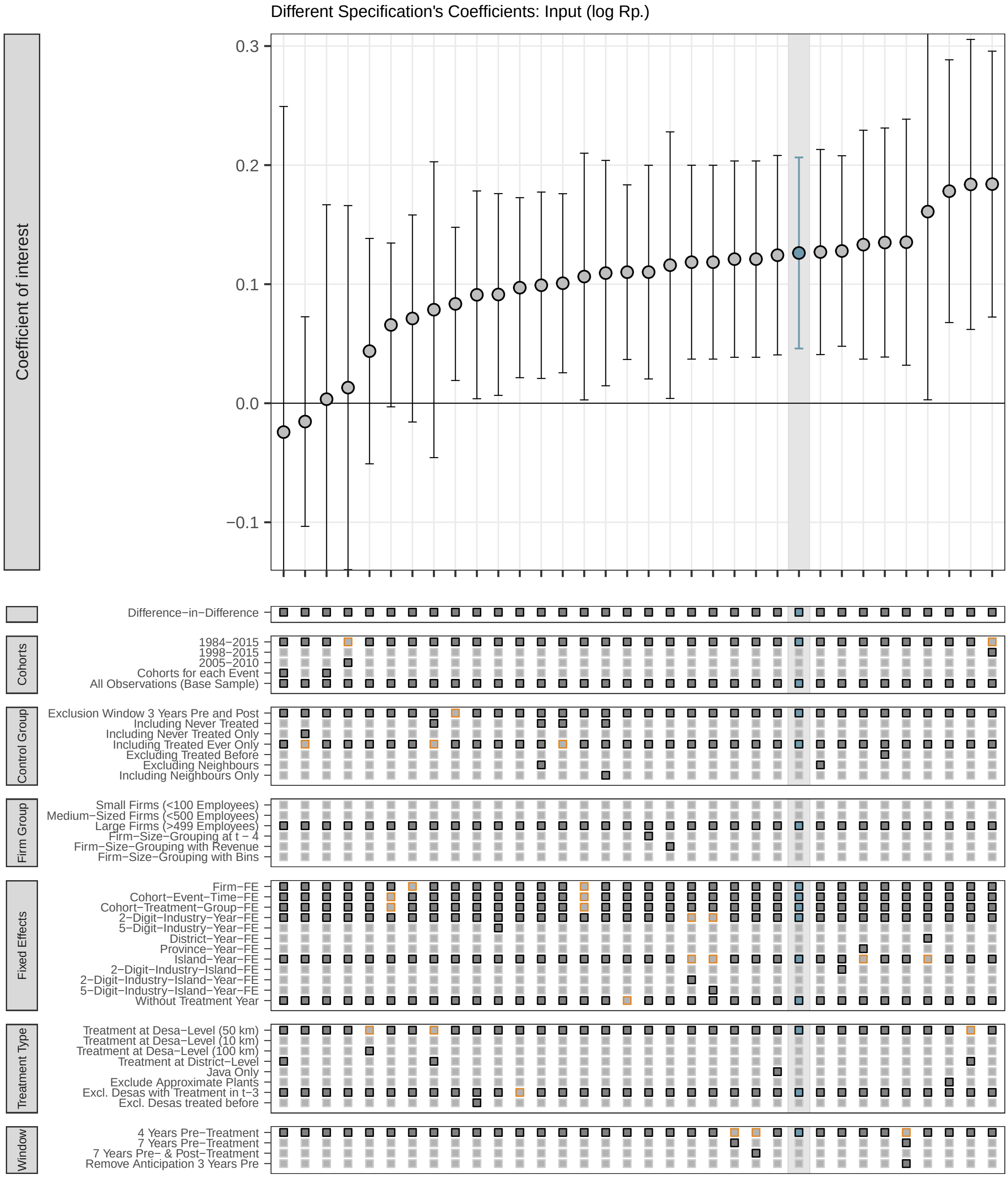


Figure A.11: Coefficients from multiple specifications: Input (log IDR) - Large Firms

This figure shows coefficients from various estimations of firms' total inputs to production (in log IDR) on a variable 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 more than 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

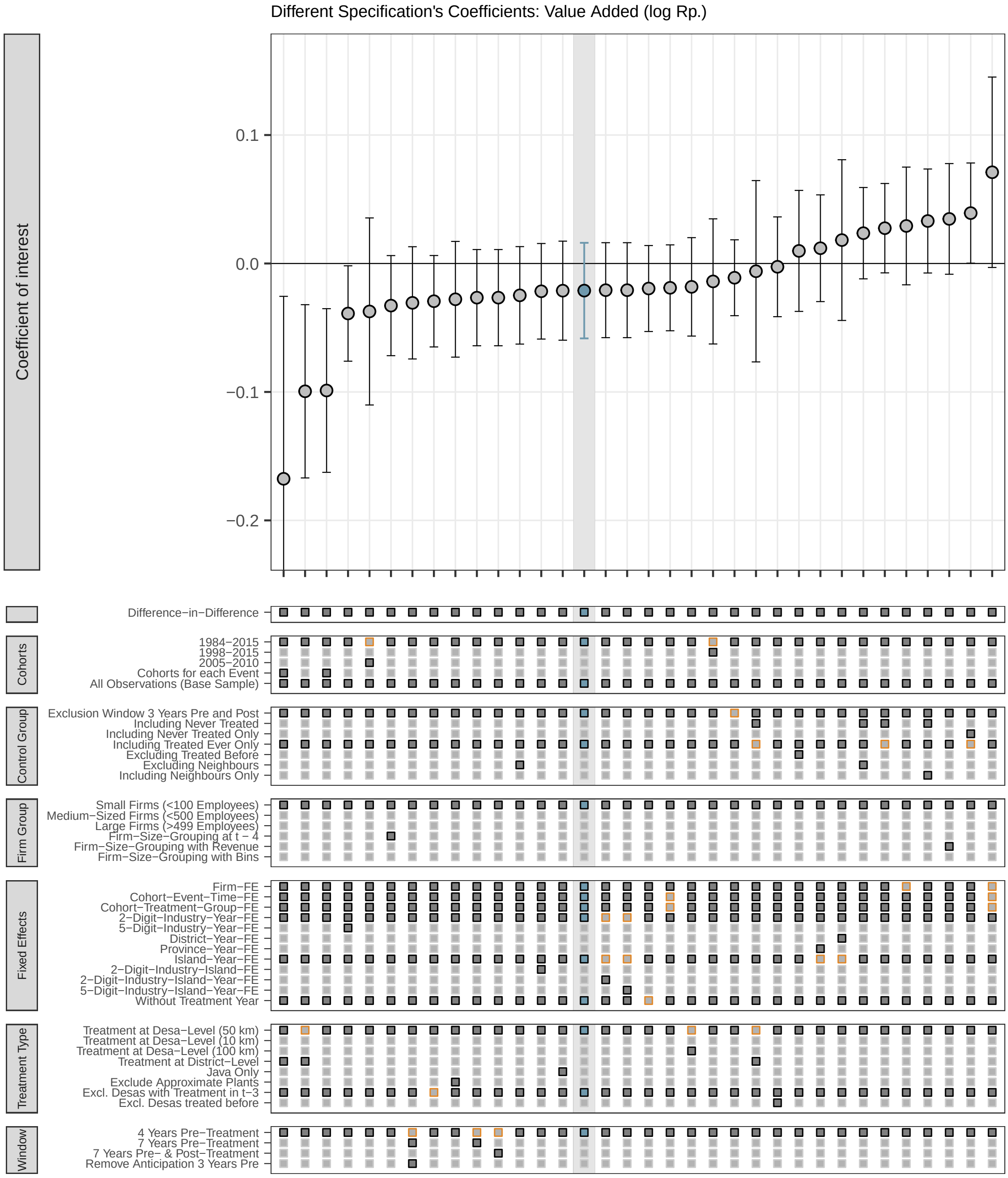


Figure A.12: Coefficients from multiple specifications: Value Added (log IDR) - Small Firms

This figure shows coefficients from various estimations of firms' value added (in log IDR) on a variable 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 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

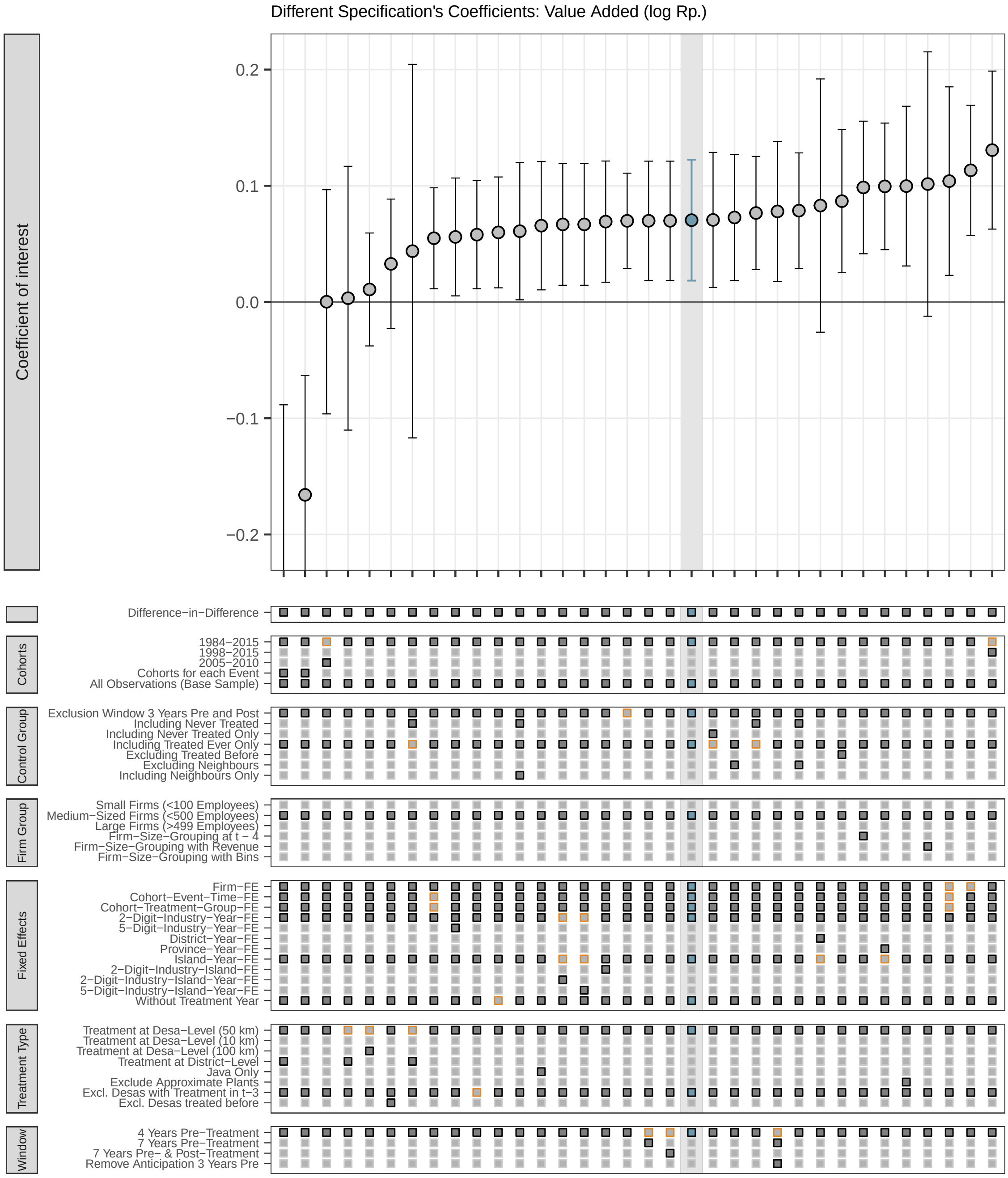


Figure A.13: Coefficients from multiple specifications: Value Added (log IDR) - Medium-Sized Firms

This figure shows coefficients from various estimations of firms' value added (in log IDR) on a variable 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 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

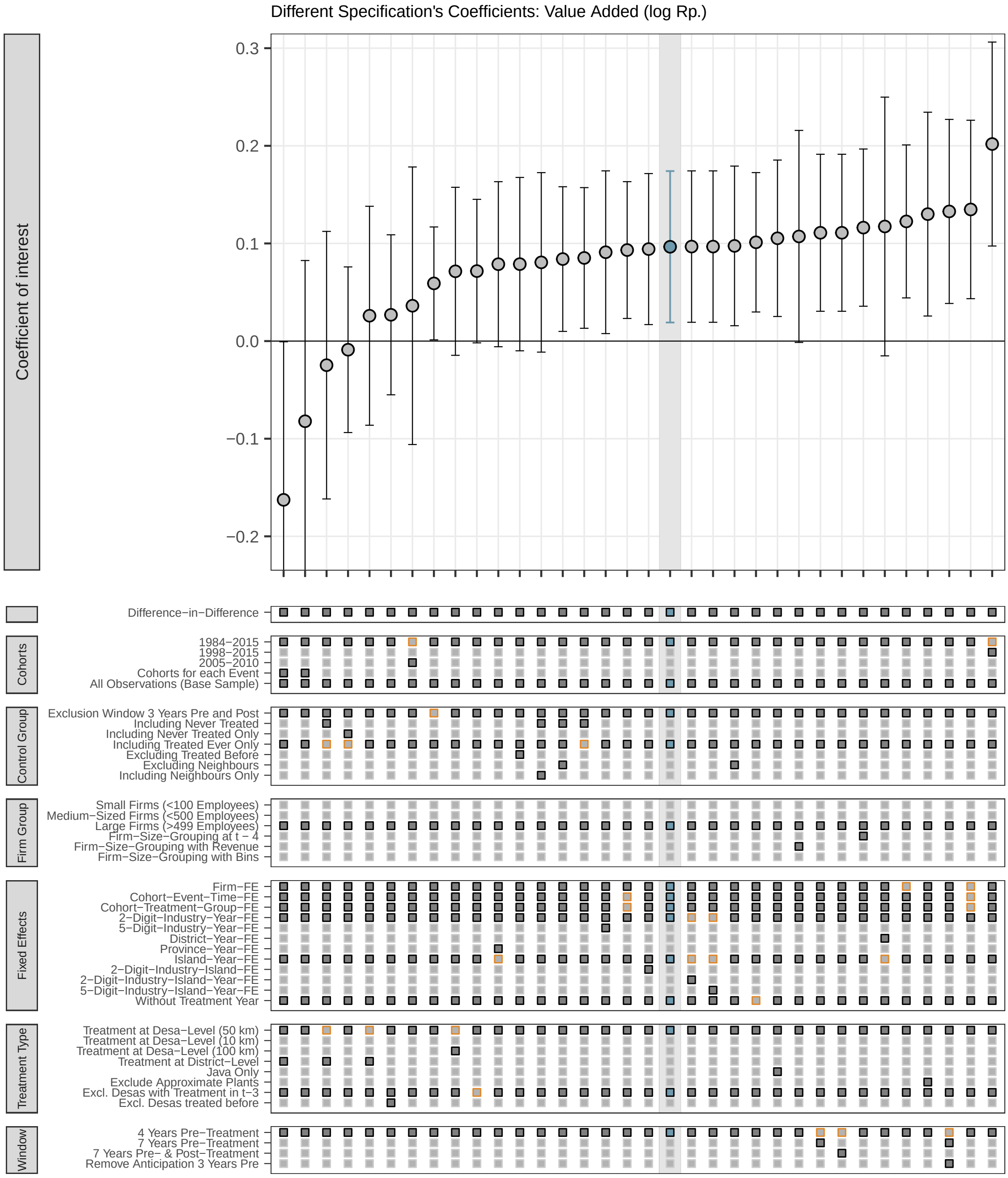


Figure A.14: Coefficients from multiple specifications: Value Added (log IDR) - Large Firms

This figure shows coefficients from various estimations of firms' value added (in log IDR) on a variable 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 more than 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

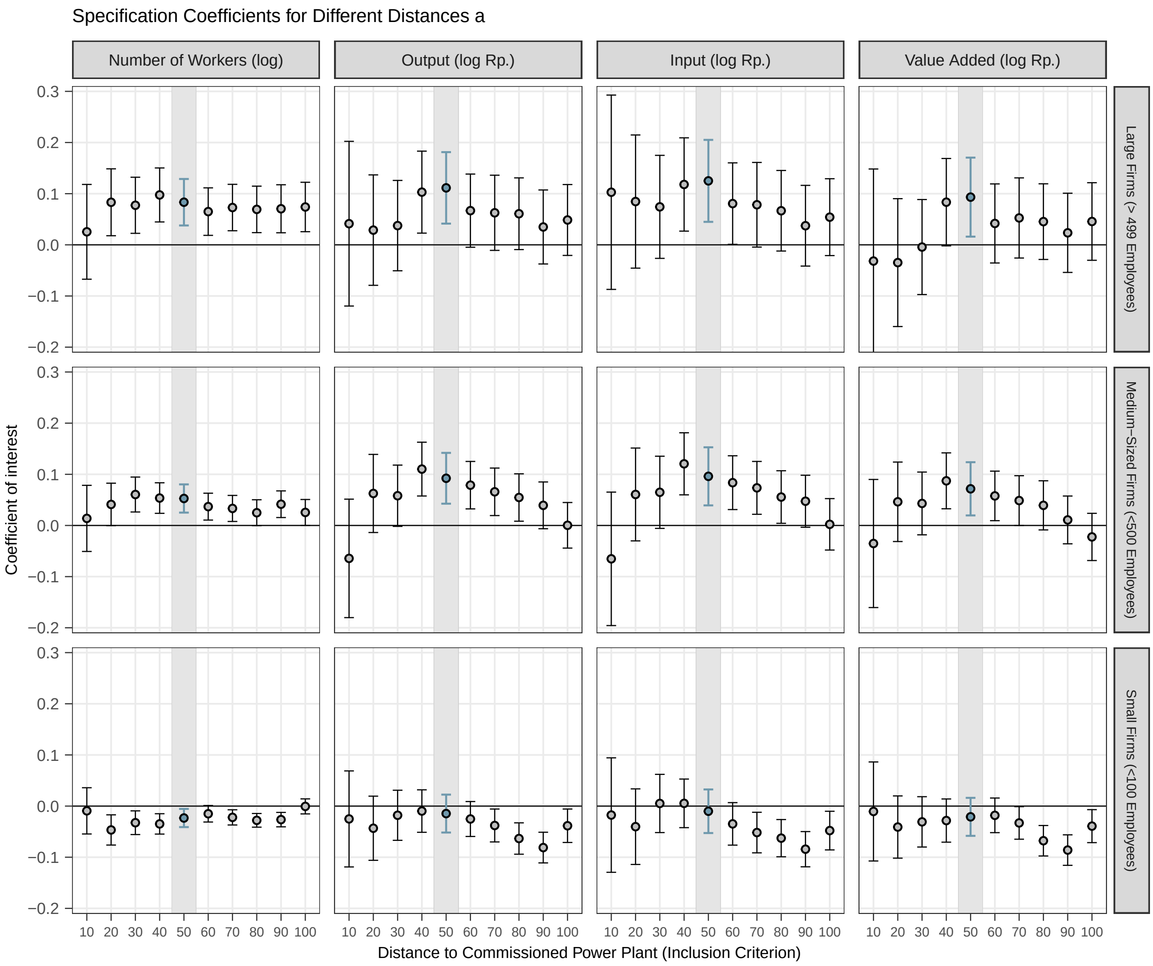


Figure A.15: Coefficients from Multiple Specifications with Varying Distance to Treatment

This figure shows coefficients from various estimations of multiple total employment (log), total inputs, total outputs and value added (all in log IDR) on a variable indicating treatment by commissioning of a new coal-fired power plant. 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 ($\tau = -1$). Small firms comprise firms with less than 100 employees. Medium-sized firms consist of firms with more than 99 and less than 500 firms. Firms with more than 499 employees are considered large firms.
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 grey.
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. Thereby, we interlink a standard set of fixed effects with firm group indicators.

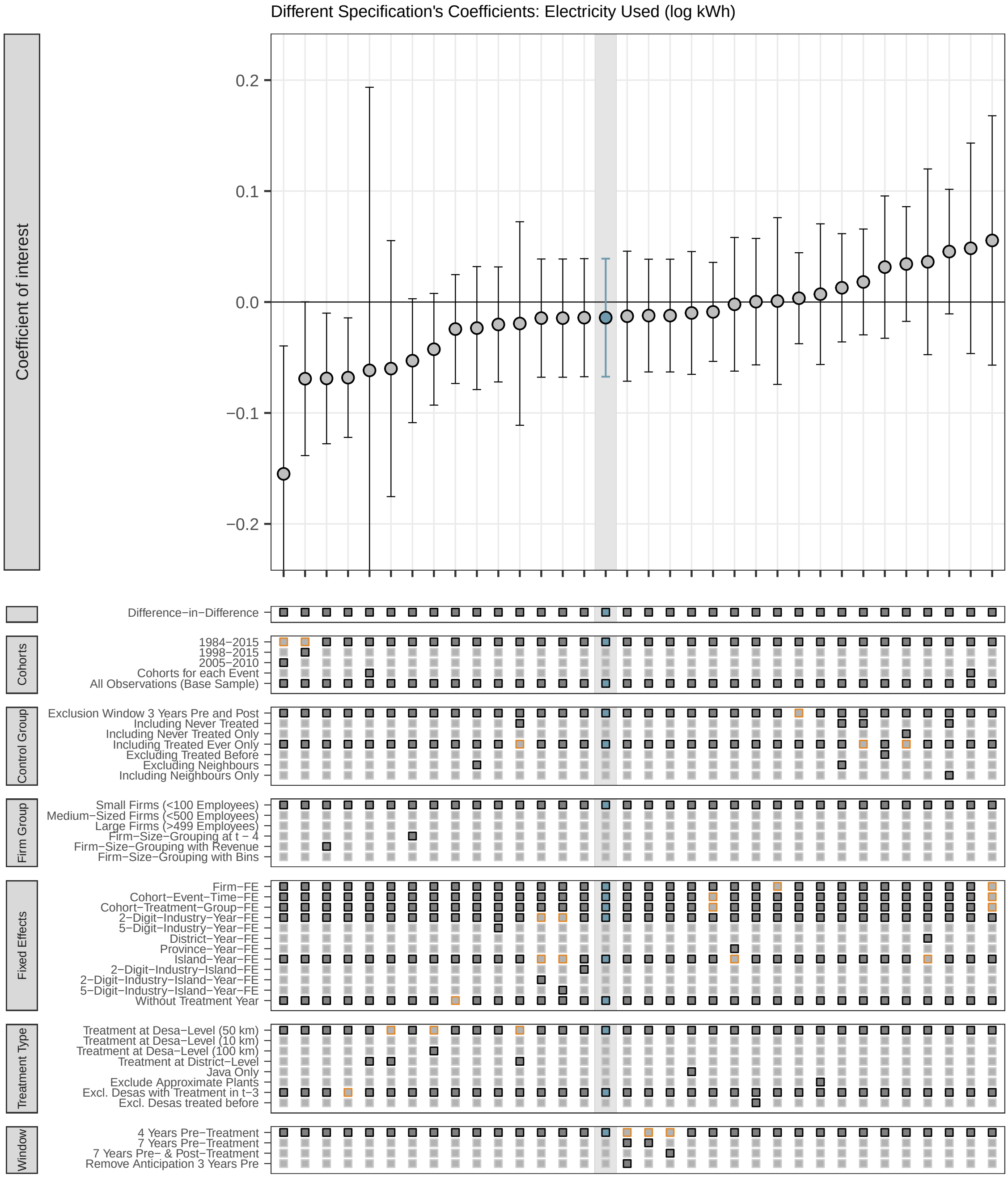


Figure A.16: Coefficients from multiple specifications: Electricity Used in Production (log kWh) - Small Firms

This figure shows coefficients from various estimations of firms' electricity used in production (in log kWh) on a variable 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 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

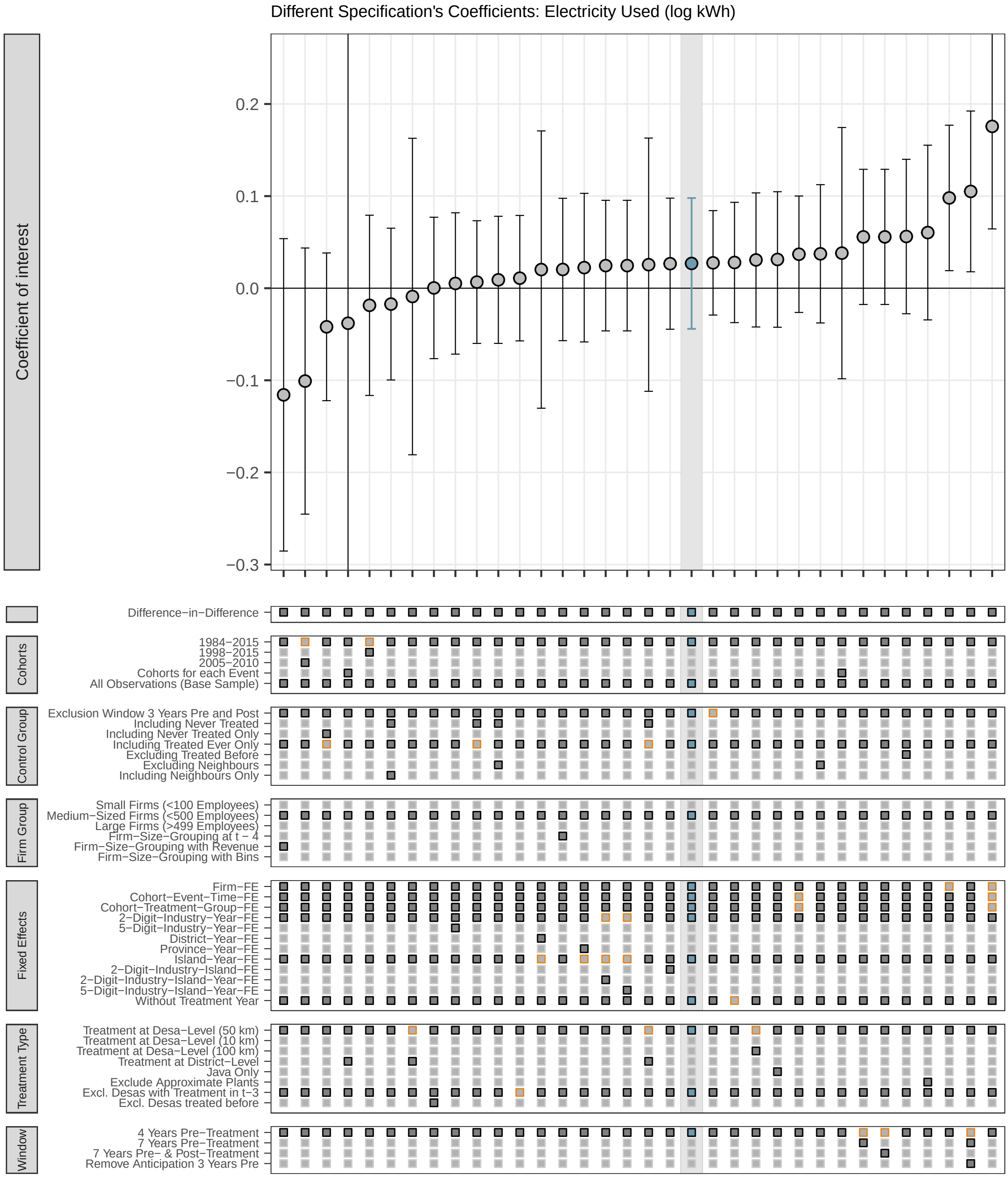


Figure A.17: Coefficients from multiple specifications: Electricity Used in Production (log kWh) - Medium-Sized Firms

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 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').

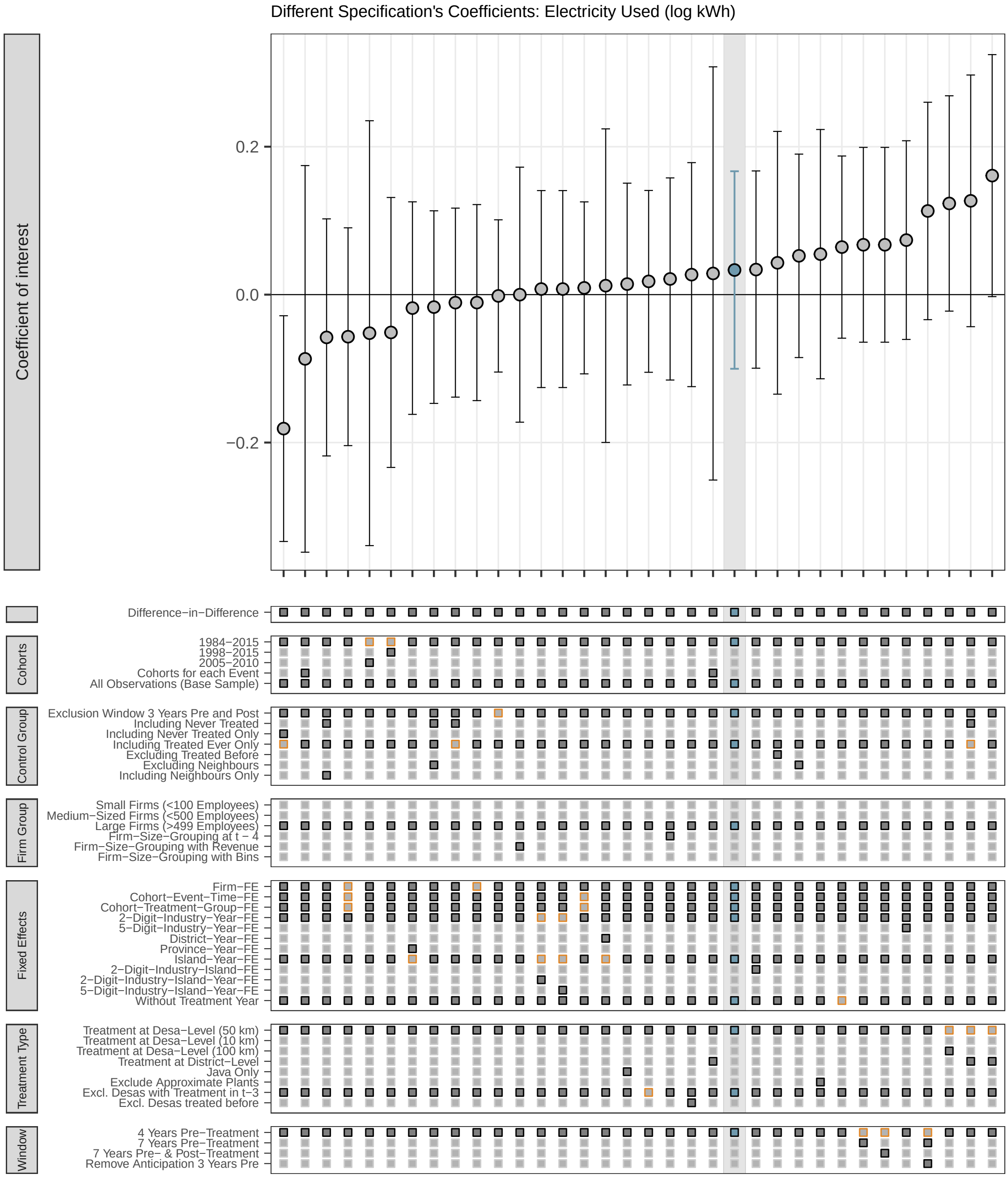


Figure A.18: Coefficients from multiple specifications: Electricity Used in Production (log kWh) - Large Firms

This figure shows coefficients from various estimations of firms' electricity used in production (in log kWh) on a variable 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 more than 499 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 grey. We present estimates with alternative specifications as indicated by squares in lower panels as robustness checks. All parameters except for one remain constant. 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 'Firm Group'), different combinations of fixed effects (panel 'Fixed Effects'), different definitions of treatment (panel 'Treatment Type') and multiple event windows (panel 'Window').