

Gold-Plate or Glint: The Effect of Competitive Markets on Capital Investment in U.S. Power Plants

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Abstract: A primary goal of competitive reforms in electricity generation was to alter the incentives for new investment in power plants. I use the divestment of U.S. power plants to identify the causal effect of competitive reform on capital investment. Between 1990 and 2010, I find fossil power plants are 9.1% smaller, on average, after divestment. The effect is robust and precisely estimated. The change in size avoided approximately \$25 billion in investment and is likely less than lost revenue, suggesting a gain in economic efficiency. The paper provides the first causal evidence that competitive reforms altered the magnitude and efficiency of investment in U.S. electricity generation. I then discuss implications of the findings for decarbonization, which will require large-scale investment in renewable power plants.

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In the late 1990’s, the U.S. introduced changes to the way the electricity system was regulated, following the deregulation of other industries like telecommunications, airlines, railroads, and trucking. Among the most significant reforms occurred within electricity generation: federal policy enabled the creation of open wholesale power markets (Cicala, 2022a), and many states removed generation from the functions over which electric utilities were granted a monopoly (Cicala, 2022b). Economists have long theorized that cost-of-service regulation, which states used to govern utilities and set electricity prices, could distort utilities’ investment decisions, biasing production towards capital (Averch and Johnson, 1962). Reformers hoped that competition would lead to more efficient investment decisions. As Paul Joskow wrote, reviewing the bevy of possible benefits of competitive reforms: “The most important opportunities for cost savings are associated with long-run investments in generation capacity” (Joskow, 1997).

Despite a large body of economic research on the effects of electricity restructuring, the literature has yet to identify how competitive reforms have affected capital investment in electricity generation (Borenstein and Bushnell, 2015; Bushnell, Mansur and Novan, 2017).¹ The gap in understanding has direct consequences for modern policy discussions. Mitigating climate change will require a large amount of new renewable power plants, to both shift existing supply and to meet higher demand, as the economy electrifies. By one estimate, total power plant capacity in the U.S. would need to more than double by 2035, in order to reach 100% clean energy (Denholm et al., 2022), and the change could require on the order of \$1.7 trillion in capital investment over the next decade (Phadke et al., 2020). Decarbonization thus rests upon large-scale investment in electricity generation, and the potential effect of different market structures, among U.S. states, on the total cost of the transition remains poorly understood.

Identifying the effect of competitive reforms on investment presents a number of challenges. Power plants are long-lived assets that operate for decades, and new capacity typically requires years of planning and development before it is operational. As a result, sufficient time needs to elapse, after reform, before we expect any effects to be observable in data, and this fact motivated an early focus on operational outcomes at power plants in the literature (e.g., Fabrizio, Rose and Wolfram (2007)). Second, electricity regulation, on the whole, is remarkably stable. After the California Energy Crisis in 2000-1, many states halted or repealed competitive reforms, and there has been no meaningful deregulation efforts at the state-level in the subsequent two decades. This limits the extent to which policy variation can be used to identify causal effects, requiring researchers to either rely on cross-sectional variation or to impose stronger structural assumptions. Finally, while most regulated utilities are required to report detailed information on their investments to the Federal Energy Regulatory Commission (FERC), independent-power producers are not. This means that data on capital investment in power plants is censored: observable when a power plant is regulated, and unobservable when it is not.

In this paper, I quantify how competition affects investment in electricity generation. To do so, I take advantage of a natural experiment that exposed a select set of power plants to both regulated and competitive market structures. As part of restructuring, investor-owned utilities sold or transferred hundreds of power plants to unregulated entities. These “divestitures” were largely prompted by policymakers in reform states who sought to preempt a single company from

¹As Bushnell, Mansur and Novan (2017) state in their literature review: “... we are unaware of any empirical paper that tests for the causal effects of restructuring on either the type or magnitude of (generation) investment” (p. 37).

exerting undue market power in newly-formed wholesale electricity markets. Once a plant was divested, competitive markets defined the economic incentives for new investment, rather than cost-of-service regulation. After an asset was sold or transferred to an unregulated company, the primary way for the power plant to earn revenue was to bid and sell its generation into wholesale exchanges. By studying how a power plant evolves after it was divested, it's thus possible to identify the effect of competitive reforms in electricity generation on capital investment, relative to a regulated counter-factual.

The divestiture of U.S. power plants has been widely used to study how competitive markets affect a range of outcomes in electricity, including market power (Mansur, 2007), investment crowd-out (Ishii and Yan, 2008), fuel procurement (Cicala, 2015), and the operating efficiency of power plants (Bushnell and Wolfram, 2005; Davis and Wolfram, 2012) and firms (Kwoka, Pollitt and Sergici, 2010). As an identification strategy, it has several strengths. First, unobserved, unit-specific characteristics of power plants can be controlled for, because divestment allows researchers to observe the same power plant under both regulated and competitive market structures. Second, the sheer number of plants that were divested, as part of reform, has not been replicated in the decades since. This creates a singular opportunity to study the effect of competitive markets in a sample with sufficient statistical power to detect changes. Finally, divestiture shortens the time-horizon on which medium- to long-run effects on investment could normally be observed. A generator added to an existing power plant can be brought online faster than a completely new plant, because major bottlenecks, such as permission to interconnect, are already overcome.

I address the final challenge—that investment is unobserved, once a power plant is deregulated—using a proxy variable: the size or “capacity” of a power plant. The logic behind this approach is simple. Power plants are modular and comprised of individual generators. If a power plant becomes larger, it necessarily signals that capital was invested to either uprate existing generators or to add new ones. (Put differently: it requires only an assumption that generators cost money to build.) In addition, while exact data on investment at a specific power plant is not always observable, data on the average cost of different generator types is consistently published within the industry. As a result, given an assumption about generator technology, it's straightforward to calculate the approximate investment equivalent of any given change in power plant size. This technique has been used successfully by other papers in the literature, such as Ishii and Yan (2008) and Davis and Wolfram (2012).²

In my main specification, I use a difference-in-difference design to measure how the capacity of a power plant changes, before and after divestment, relative to similar power plants that remain regulated over time. I focus the analysis on a sample 800 power plants that were initially owned by a utility subject to cost-of-service regulation and that primarily use fossil fuel, such as coal, oil, and natural gas, which are more likely to be modular than other generating technologies. Of these power plants, 273 are eventually divested. I observe the power plants annually for 21 years, from 1990 to 2010, and I show that, after divestments begin, regulated fossil power plants

²There are three other approaches used in the literature, to overcome that capital investment is only observed when regulated: modeling the amount of investment using context-specific knowledge about the assets purchased, like pollution controls, as in Fowlie (2010); focusing on the effect of competitive markets on investment in a function that was not deregulated, such as distribution, as in Cicala (2022b); or, simulating investment decisions in power plant capacity using a structural model, as in Gowrisankaran, Langer and Reguant (2024).

become larger over time, while divested plants become smaller. The difference is not driven by differential rates of retirements, but rather, a shift in the capacity distribution of plants that remain operational.

The difference-in-difference identifies the effect of competitive reform on investment at a single power plant, rather than a market-level change. A natural concern with this approach is that electricity markets are interconnected, and the level of overall investment or new build within a region can confound the investment decisions at any individual unit. For example, divested fossil power plants might become smaller if competitive reforms led to a higher rate of entry, crowding out the need for other operating capacity. However, I show that, in practice, the rate of new build near divested and regulated fossil power plants was remarkably similar during the study period. This result is consistent with MacKay and Mercadal (2024), who demonstrate that entry was also comparable across regulated and deregulated utility territories. As a further robustness check, I include a specification that limits the regulated plants in the sample to those physically closest to—and thus likely experience the same market conditions as—divested fossil plants, mirroring the approach in Cicala (2015). I find minimal change in the treatment effect estimated on the geographically restricted sample, relative to the full sample. While differential entry would be a direct validity threat, the evidence suggests the actual risk of bias, in this setting, is low, supporting the within-plant design.

The results show that exposure to competitive reforms leads to a large, precise, and robust reduction in capital investment at fossil power plants. After divestment, a fossil plant is 9.1% smaller, on average across years, relative to a plant that remains regulated. This change is equivalent to a decrease of 47 megawatts (MW) from the mean plant size of 522 MW. The effect is stable across alternative specifications; I demonstrate that the reduction is not driven by outlier additions, plant characteristics, plants' locations, the effect of new ownership, or selection among assets, when a utility could choose which plant to divest. The reduction in capacity corresponds to a sizable amount of avoided investment that would have been recovered through regulated prices. If we assume the counter-factual generator for a divested plant would have aligned with its original, dominant type, the reduction in capacity is equivalent to avoiding approximately \$25.4 billion (\$9-41.4 billion, in the 95% confidence interval) in capital investment. This estimate accounts only for the 273 divested fossil plants in the sample and is likely to underestimate the aggregate, market-wide cost savings.

The decrease in investment appears to be efficient. Among a sub-sample of 310 fossil power plants (72% of capacity in the sample), for whom detailed generation data is available, I find divested power plants perform similarly to regulated plants on multiple dimensions, despite the lower apparent level of capital investment. I show there is no detectable difference in divested fossil plants' rate of generation or capacity factor, when operating; the efficiency with which they use fuel; nor the carbon intensity of their fuel mix, which measures fuel switching (e.g., coal to natural gas). While divested fossil power plants do operate for fewer hours during the year, I illustrate that the cost of lost revenue among divested plants only exceeds the value of avoided investment under implausible assumptions regarding capacity costs and wholesale market prices. Because the value of plants' output likely declined by less than their input, it suggests that the reduction in capital investment, due to competitive markets, likely represents a gain in economic efficiency.

The effect of competitive markets on investment is not uniform, but rather varies across the operating characteristics of power plants. I find that divestment leads to a larger proportional decrease in investment among power plants that likely served intermediate (which are 13.5%, or 92 MW, smaller) or peak load (12.4%, or 12 MW, smaller), relative to plants that likely served base load (3.5%, or 32 MW, smaller, on average). In other words, fossil power plants that are likely to have higher marginal generating costs—and that are less likely to be dispatched within the merit-order of wholesale markets—experience a larger reduction in investment. This dynamic is consistent with prior work; Davis and Wolfram (2012) showed that nuclear power plants, which have very low marginal generating costs, receive slightly more investment once divested. Taken together, the evidence suggests that competitive reforms may both improve the efficiency of investment and *reallocate* it from high to low marginal cost generating technologies. The latter has direct implications for the clean energy transition. The marginal production costs of solar and wind, in the absence of fuel, are negligible—implying competitive reforms might lead to greater investment in renewables, relative to a regulated counter-factual, all else equal.

Finally, I show that additions at regulated fossil power plants coincided with the operating conditions under which cost-of-service regulation is most likely to lead to overcapitalization (Joskow, 1974). This finding has two implications for the external validity of the treatment effect estimated to other generating technologies and time periods. First, it suggests at least a portion of the identified effect of competitive markets on capital investment is likely driven by utility-level changes in investment. This indicates that the efficiency gains in investment are less likely to be specific to fossil power plants and more likely to hold for other generating technologies. Second, the relative benefit of any future competitive reforms on investment will depend, in part, on the operating conditions of regulated utilities at the time of change. For example, the risk of overcapitalization may be lower in periods when regulated utilities’ average production costs are decreasing, such as when load is growing, which may reduce the initial gains from competitive reform.

The paper contributes to three literatures in economics. First, the results help expand our understanding of the benefits of restructuring to the U.S. electricity system. Prior work has shown that competitive markets led to gains in how power plants operate (Cicala, 2015; Fabrizio, Rose and Wolfram, 2007; Davis and Wolfram, 2012; Craig and Savage, 2013; Cicala, 2022a), but had yet to establish if similar changes occurred within investment (Borenstein and Bushnell, 2015; Bushnell, Mansur and Novan, 2017). Similarly, a central policy question, after the creation of wholesale markets, was whether revenue from energy and ancillary service sales would be sufficient to incentivize new build and how market design might address possible shortfalls (Bushnell, 2005; Joskow, 2006; Cramton, Ockenfels and Stoft, 2013; Bushnell, Flagg and Mansur, 2017), but the discussion occurred in advance of empirical evidence that deregulation did, in fact, alter investment decisions. This paper speaks to both literatures and provides the first direct, causal evidence that competitive reforms changed the magnitude and efficiency of capital investment in power plants. The two closest papers to my analysis are Hill (2021) and Knittel, Metaxoglou and Trindade (2019). Relative to Hill (2021), I study changes in power plants’ capacity directly, rather than a ratio of electricity supply to demand, and I use divestments to isolate the specific effect of competitive reform in generation, exclusive of retail reform. Relative to Knittel, Metaxoglou and Trindade (2019), which studies how natural gas generation responded to fuel price shocks across regulated and deregulated markets, I study

investment at the power-plant level, instead of state-level aggregates, and analyze a broader set of generating technologies, inclusive of coal, oil, and natural gas.

Second, the analysis adds to the broader literature on how regulation can alter investment in electricity infrastructure. Much of the early work in this space, during the 1970's and 1980's, sought to find evidence of the Averch-Johnson effect (Averch and Johnson, 1962), but prior to restructuring, often lacked meaningful sources of identifying variation and reached mixed conclusions (Joskow and Rose, 1989). The findings in the modern empirical literature appear more consistent. Fowlie (2010), using a discrete choice model, showed that regulated utilities were more likely to adopt capital-intensive pollution controls for coal power plants, relative to independent power producers. Cicala (2022*b*), using a difference-in-difference, showed that deregulation of generation increased utilities' investment in transmission and distribution, which remained in the rate base. Gowrisankaran, Langer and Reguant (2024), using a structural model, illustrated how, during the shale gas revolution, regulation likely led utilities to invest less in natural gas plants and favor existing coal assets, in order to maximize the value of their rate base. Similar to prior work, I find, within a reduced-form framework, that regulation led utilities to invest a larger amount in fossil power plants; and, uniquely, I demonstrate that competitive reforms may be able to establish more efficient incentives for investment.

Finally, the paper highlights a key gap in the separate literature on decarbonization. Existing research has primarily studied the ability of new incentive-based policies, such as carbon taxes or clean energy standards, to encourage efficient investment in clean energy (Goulder, Hafstead and Williams, 2016; Kellogg, 2020; Stock and Stuart, 2021; Borenstein and Kellogg, 2023; Bistline et al., 2024), but overlooks the role of existing electricity regulation and the incentives it creates for capital investment. A primary implication of this paper is that market structure is likely to have a direct effect on the magnitude and efficiency of investment decisions in clean energy. Further research is needed to understand how different regulation structures, across the U.S., can affect investment in decarbonization, and whether competitive reform warrants explicit consideration as a policy mechanism, alongside more traditional environmental policies.

The remainder of this paper proceeds as follows. Section 1 describes the data. Section 2 provides background on divestment, details the identification strategy, and gives descriptive evidence to support the difference-in-difference design. Section 3 estimates the effect of divestment on the capacity of power plants and the approximate amount of investment avoided. Section 4 then explores if there are coincident changes in plants' operation, after divestment, and if cost-of-service regulation likely distorted utilities' investment decisions in the post-period. Section 5 summarizes and concludes.

1 Data

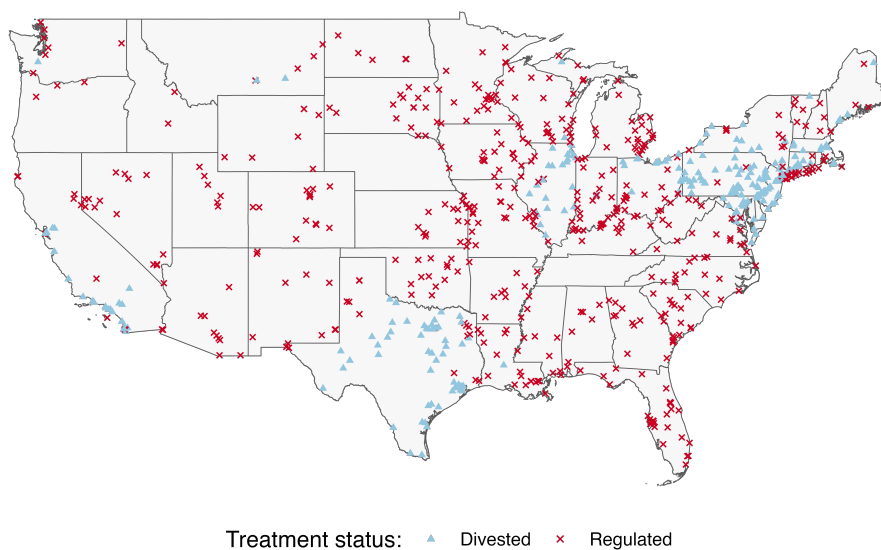
The data I use for the analysis comes from three federal agencies: (i) the Energy Information Administration (EIA), (ii) the Environmental Protection Agency (EPA), and (iii) the Federal Energy Regulatory Commission (FERC). What follows is a brief description of the source and construction of the primary variables used in the analysis. Additional details on data processing are provided in the Supplementary Information.

1.1 Capacity and other characteristics of power plants

The measure of plant-level capacity, and my primary outcome variable, comes from EIA Form-860 (EIA, 1990-2010*a*). Beginning in 1990, the annual survey has collected detailed information about the technical characteristics of all U.S. power plants above 1 MW. In each year of the panel, I observe the total operable capacity of each power plant, as well as the underlying characteristics of its generators, such as their age, primary fuel source, and turbine type. “Operable” generators include those that are actively in-service and those on standby and exclude those that are retired or under construction. I use the reported nameplate capacity of each generator, because summer de-rated capacity is not available consistently for all years of the survey. I limit the sample to plants that, in 1990, were located in the continental U.S. and that were owned by utilities subject to cost-of-service regulation. I identify regulated utilities using the type of each plant’s owner in EIA Form-861 (EIA, 1990-2010*b*), which is an annual census of the activities of U.S. electric utilities, and limiting to investor-owned utilities that served end-load customers. Finally, I include only fossil plants that burn primarily coal, oil, or natural gas to generate power, removing hydroelectric, nuclear, and a small number of geothermal plants from the sample. I identify fossil plants based on their historic fuel use during the decade prior to the panel (1980-1990), obtained from EIA Form-759 (EIA, 1980-1990). The resulting sample has a total of 800 fossil plants, operating in 1990, that were built and initially owned by regulated utilities. A map of the location of plants in the sample is shown in Figure 1.

I observe power plants in the sample annually from 1990 to 2010, for a total of 21 observations per plant. I end the panel in 2010 to reduce the risk that structural changes to electricity supply over the subsequent decade—in particular, the rise of shale gas production and the growth of renewables—confound the treatment effect estimate.

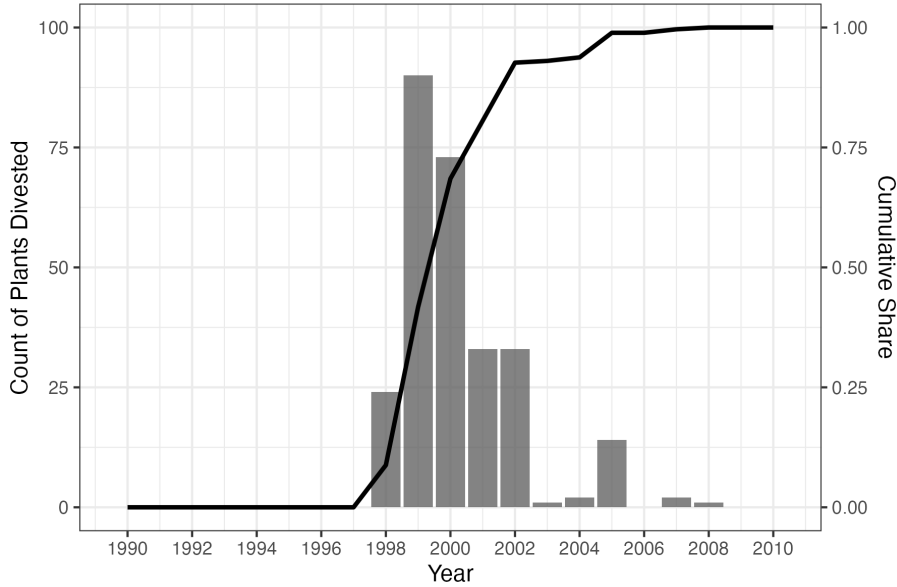
Figure 1: Spatial Distribution of Fossil Power Plants in Sample



1.2 Divestment

There is no single, definitive data source that reports divestments of power plants. Following the approach in Davis and Wolfram (2012) and Cicala (2015), I code a plant as divested during the first year in which it reports generation to the EIA as a non-utility (EIA, 1990-2010*c*).³ Each plant that reported any non-utility generation was considered a “candidate” divestment, and I then cross-verified the fact and timing of each sale with a second data source, such as the Securities and Exchange Commission (SEC) filings of the original owner. Because I aim to use divestments to identify how competitive markets affect capital investment, I exclude any sales of power plants to a government-owned or cooperative utility, which are subject to different forms of regulation. I also remove four power plants whose treatment status was non-absorbing (i.e., they were sold back to an investor-owned utility or re-regulated). This left a total of 273 plants that were divested within the sample. The timing of divestments is shown in Figure 2. Divestments are staggered between 1998 and 2008, but the majority (93%) occur between 1998 and 2002.

Figure 2: Timing of Divestments



1.3 Fuel use, generation, and carbon emissions

I use data on power plants’ fuel use, generation, and carbon emissions to explore if the change in capital investment is likely to indicate a gain in economic efficiency. While the EIA does collect data on monthly generation from utility-scale power plants, many divested plants stopped reporting temporarily during the late 1990’s and early 2000’s, due to initial ambiguity in reporting requirements for non-utility power plants.⁴ Given this, I instead obtain data on plants’ operation from the EPA Continuous Energy Monitoring System (‘CEMS’) (EPA, 1997-2010).

³I note that the EIA Plant ID is stable across owners. While a plant’s name may change, after divestment, I am not aware of an instance where its identifier changes.

⁴I find non-reporting is a particular problem among divested fossil power plants, relative to other technologies, such as nuclear. It is especially difficult to distinguish between instances of non-reporting and a true zero for fossil plants that may operate infrequently across and within years, such as peaking plants. In addition, early versions of EIA generation data did not report units for fuel consumption, which means the calculation of plants’ heat rates in the pre-period requires guessing the heat intensity of fuel volume. This would introduce measurement

The CEMS data began in 1995 to track compliance among plants regulated under the federal Acid Rain Program (which aimed to reduce sulfur dioxide emissions through a cap-and-trade program), though its scope has expanded over time in line with EPA air quality regulations. The two key strengths of the CEMS data are its granularity (variables are collected hourly, at the sub-plant level) and quality (it is based on physical monitors, rather than self-reported data). The trade-off for quality is scope: not all plants that reported capacity to the EIA were initially required to monitor their operations for the EPA.

The analysis on plant operations is thus based on a subsample of power plants for whom complete CEMS data is available. I limit the analysis to plants who reported for all 14 years of available data (1997-2010) and whose operational data did not contain obvious measurement error (e.g., reported generation without fuel use). This identified a total of 310 power plants (the ‘CEMS Sub-Sample’) that comprise 72% of the capacity in the full sample in 1997. Relative to the EIA capacity sample (Table 1), the plants in the CEMS sub-sample are larger, more efficient, and more likely to burn coal, which reflects the type of plant included in the initial cohorts of the Acid Rain Program. The mean plant was around 1 GW in 1990, obtained about 60-73% of its fuel from coal in the decade prior, and had an average heat rate near 10,500 Btu per kWh. There are also comparatively fewer control plants in the Northeast.⁵

One additional note about the EPA data is worth addressing. The EPA collects information on power plants’ gross generation, which does not net out the amount of electricity used to operate the plant itself. This means that estimates of heat rates from the data may slightly overstate the thermal efficiencies of plants, which are ideally calculated using net generation values. However, the discrepancy is likely to be small in this particular subsample of power plants, because they tend to operate consistently throughout the year; the gap between net and gross generation is largest for peaking plants, which generate for a handful of hours each year and may frequently have net negative generation.

1.4 Financial data for utilities that own regulated power plants

I use data in the Form 1 filing from the Federal Energy Regulatory Commission (FERC) to analyze the financial context in which regulated utilities made their investment decisions (FERC, 1994-2010). Form 1 is an annual filing, available digitally from 1994 onwards, that is required of all major utilities (annual sales exceed 1 million MWh), and it contains detailed financial information for the companies’ operations and investments. Among the utilities which own power plants in the control group in 1990, I limit the sample to the 74 who report to the FERC and who have a full panel of data (i.e., the reporting company definition is stable from 1994-2010).⁶ These companies owned 94% of the control plant capacity at the start of the panel.

error in a central outcome of the analysis (the relative efficiency with which divested and regulated plants used fuel). For these reasons, I view EPA CEMS as the best available source of operational data, given the time period and type of power plants in the sample.

⁵These values are shown in a balance table for the CEMS sub-sample in the Supplementary Information.

⁶Within the sample, it is possible for a utility to own both control and treated plants. This occurs for one of three reasons: (i) the utility is in a state that required divestment, but the control plant retires before restructuring occurs; (ii) the utility voluntarily divested one or two of its power plants; or (iii) the utility is in a state, such as California or New York, where divestment prompted by restructuring legislation was partial. For the FERC data, I exclude utilities in category (i) and include those in (ii) and (iii) for whom the majority of plants owned by the utility in 1990 remain regulated.

2 Empirical Strategy

The goal of this paper is to identify the effect of competitive market structures on capital investment in electricity generation. To do so, I take advantage of a natural experiment that occurred when U.S. electricity markets were restructured: the large-scale divestment of power plants. In the following sections, I explain the key building blocks of this empirical approach: what divestments are and how they occurred; the difference-in-difference research design and its identifying assumptions; and descriptive evidence to support the validity of using a difference-in-difference, in this setting.

2.1 Divestment as a Treatment

Historically, electric utilities in the U.S. were treated as vertically-integrated, natural monopolies, and regulation primarily occurred at the state-level. In exchange for exclusive access to customers in a given region, investor-owned utilities ceded price-setting authority to state public utility commissions.⁷ How commissions set prices, in turn, determined incentives for utilities' investment. The dominant approach is called "cost-of-service," in which the retail price is set so that the utility earns sufficient revenue to recover its operating and depreciation costs, plus a return on its capital investment, under an assumption about expected sales. A central consequence of cost-of-service ratemaking is that it creates a direct incentive for utilities to invest in capital projects, as a means to increase profit. The potential for cost-of-service regulation to bias firms towards capital is referred to as the Averch-Johnson effect, after the eponymous authors of the original model (Averch and Johnson, 1962).

I use "divestment" to refer to the sale or transfer of a power plant from an investor-owned utility to an unregulated company. "Unregulated," in this context, means that the owner is not subject to price-oversight or cost-of-service regulation administered by states. From the perspective of the investor-owned utility, divesting a power plant removes it from the company's rate base—meaning, its capital value is no longer included in setting prices. Instead, once divested, the power plant earns revenue primarily by selling generation in newly-established wholesale markets.⁸ The markets are viewed as "competitive" because participation is open and energy prices are set on a least marginal-cost basis. Divestment thus switches the economic incentives for new investment at a power plant: it removes those set by cost-of-service regulation and introduces those established by wholesale markets. By tracing how a plant evolves, before and after it's divested, one can isolate the effect of competitive reforms on investment, relative to a regulated counter-factual.

It's worth noting that divestment can involve two underlying changes: the power plant changes owners, and it is exposed to new economic incentives. Both changes—ownership and incentives—can affect the level of resulting investment, and the interpretation of the effect is altered based on which mechanism dominates. As a sensitivity test, I will leverage heterogeneity across the

⁷Other types of utilities in the U.S.—such as federal, municipal, state, or cooperatives—are regulated differently.

⁸Wholesale markets can vary in their design and how they compensate generators. As a treatment, divestment will identify the average effect of competitive reforms, across independent system operators, inclusive of payments generators may receive in energy, ancillary services, and, when available, capacity markets. The owner of a divested plant can also sign a power purchase agreement with another party, for a fixed off-take of generation; this would enable a fixed revenue stream, separate from the markets facilitated by independent system operators. In all cases, the power plant loses access to revenue from regulated retail prices.

type of divestment—sale or transfer—that occurred. We expect that the effect of ownership will be minimal when a power plant is transferred to an unregulated subsidiary of the same parent company, versus when it is sold to a different entity. By estimating the effect of divestment separately among each group, I can thus assess how the change in ownership and the change in economic incentives separately affect investment.

Treatment assignment—meaning, which power plants were selected to be divested—could occur one of three ways, based on the state in which the investor-owned utility that owned the power plant was located (Andrews, 2000; FTC, 2000). First, a number of states passed legislation that required investor-owned utilities to divest all of their fossil generating assets. These include Connecticut, Massachusetts, Maryland, Maine, New Jersey, Pennsylvania, and Texas. A second group of states passed legislation that did not require full divestment but did encourage it; for example, the state may have provided financial incentives for investor-owned utilities to divest. States in this bucket include California, Delaware, Illinois, Michigan, New York, and Ohio. Finally, a number of utilities voluntarily chose to divest power plants in the absence of state legislation. Power plants in D.C., Indiana, Louisiana, Montana, Virginia, Vermont and Washington fall into this category.

We can therefore think about the possible selection effects among divested power plants along two dimensions. The first is whether or not the owner could choose among its power plants and select which to divest. If a utility had a choice, we might be concerned that the company opted to only offload those power plants that were the equivalent of “lemons.” Importantly, the fear here is that the firm would have private knowledge about plant characteristics that would be unobservable to buyers ex-ante (or to researchers ex-post). Luckily, the heterogeneity in state policy provides a way to test if this form of selection occurred. Intuitively, we expect that selection among assets will not play a role in states that required utilities to divest of all their power plants. If the overall treatment effect is similar to the effect estimated among required divestments, it implies that selection within a utility’s portfolio does not dominate the overall estimate.

The second dimension of selection is the location of divested power plants. As shown in Figure 1, divested power plants are primarily located in states that required or encouraged divestments, and there is thus a clear difference in the spatial distribution across treatment arms. Why does this matter? Many aspects of electricity markets are determined by place: the density of the population (and the magnitude of demand), the local weather patterns (and the timing of demand), and the availability and cost of different fuels (and the composition and cost of supply). As a result, we might be concerned about initial differences in the types of power plants that were divested, as a function of where they are and the market conditions in which they were built to operate. This dimension of selection affects the suitability of regulated plants to set the counter-factual investment path within the difference-in-difference. It is easily explored by assessing the balance on mean characteristics across regulated and divested plants (i.e., asking, “How similar are they, in practice?”).

However, even if divested and regulated plants are similar on observables, there is a final aspect of selection, as a function of location, that may be unobservable and that can affect how we interpret the estimated effect of divestment. Unlike other industries, deregulation of electricity in the U.S. was partial: while all states opened proceedings to consider deregulation, many

withdrew or halted efforts after the California Energy Crisis in the early 2000’s. States that adopted restructuring legislation *first* tended to be those with higher retail electricity prices (Andrews, 2000; Kwoka, 2008*b*; Borenstein and Bushnell, 2015). As explained above, under cost-of-service regulation, capital investment—particularly in power plants—can directly contribute to higher prices. It follows that we may worry divested plants were owned by the “worst offenders”—meaning, the utilities that displayed the highest degree of bias towards capital, while regulated, which motivated early reform. Given this, it’s important to remember that the identified treatment effect is the local average among places where divestments happened to occur. Caution is warranted before assuming external validity to other regions.

2.2 Difference-in-Difference

I use a difference-in-difference to trace how the capacity of a fossil power plant changes, before and after divestment, relative to similar power plants which remain regulated. It is estimated using a two-way fixed effect model:

$$y_{pt} = \beta d_{pt} + \theta_p + \gamma_t + \epsilon_{pt} \quad (1)$$

Here, p indexes a power plant, t indexes a year, and d is a binary variable equal to 1 once a plant has been divested. The outcome, y , measures the operable capacity of plant p in period t ; if the power plant retires, y is 0. The plant-level fixed effect, θ_p , captures time-invariant characteristics of each power plant that can affect investment decisions, and the year fixed effect, γ_t , captures common shocks to new build, such as changes in resource availability, fuel prices or macro-economic shifts to demand. The coefficient of interest is β : the average change in capacity, after a plant is divested, in a given year.

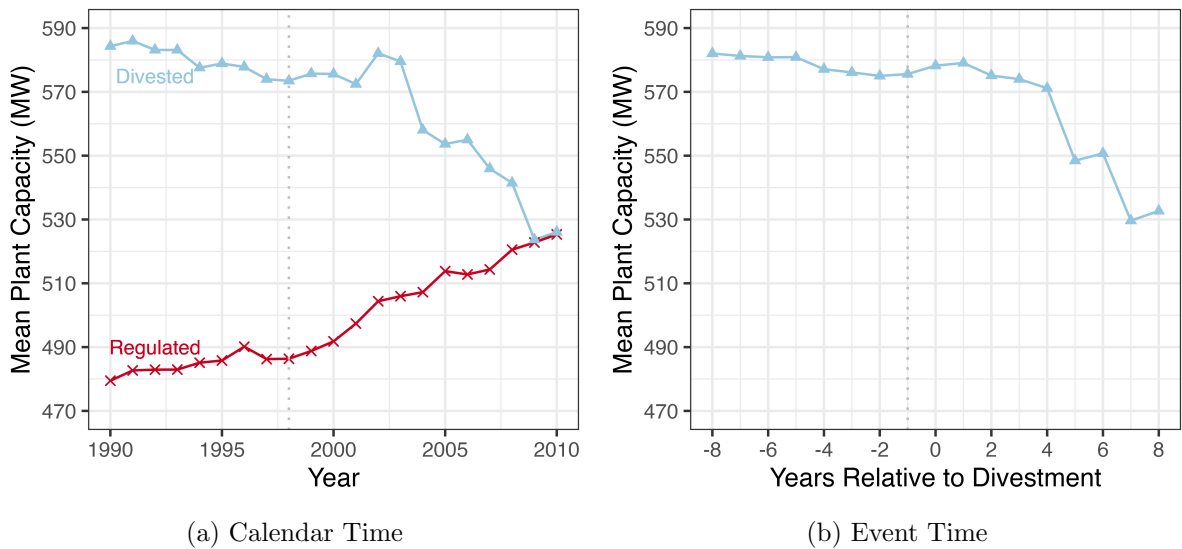
In Equation 1, I use the capacity of a power plant acts as a proxy for capital investment. This is because, once a plant is deregulated, its owner is no longer obligated to report investment data to the FERC through the Form-1 filing. The “capacity” of a power plant is the technical name for its size—roughly, it can be thought of as how much energy the plant is “capable” of generating—and is measured in megawatts (MW). Because power plants are modular and can consist of multiple generators, a plant will become bigger when capital is spent to either add a generator or uprate existing units, and vice versa. Capacity further acts as a conservative proxy for investment, because capital can be spent on the plant in other ways, such as pollution controls (Fowlie, 2010), that would not be reflected in a larger size. Later in the paper, I convert the identified change in capacity (MW) to an estimated change in investment (\$) using average cost factors (\$/MW) for different generator types. With capacity used as a proxy for capital, the coefficient of interest in Equation 1, β , identifies the effect of competitive markets on capital investment in power plants, relative to a regulated baseline.

Because plants are divested in different years (Figure 2), and the effect of divestment is unlikely to be homogenous across both plants and time, the estimate of β in Equation 1 can be biased due to problematic comparisons between adopting groups embedded in the simple two-way fixed effect model (Goodman-Bacon, 2021; Wing et al., 2024). To address the issue, I include additional estimators that are robust to staggered treatments and heterogeneous treatment effects (Callaway and Sant’Anna, 2021; Gardner, 2022; Wing, Freedman and Hollingsworth, 2024). However, because I find, in practice, that the simple two-way fixed effect estimates are

very similar to those from stagger-robust estimators, I maintain the two-way fixed model as the main specification and provide the results from the alternative estimators as robustness checks.

The mean evolution of power plant capacity among divested and regulated plants is shown in Figure 3. Two points are evident. First, divested and regulated plants appear to evolve similarly in the pre-period, with minimal change in plant capacity prior to 1998 when divestments begin, as demarcated by the gray dotted line. This implies that plants, on average, follow a common trend prior to treatment, which supports the choice of the difference-in-difference design. Second, there is a clear change in the post-period: the mean deregulated power plant becomes smaller, while the mean regulated plant becomes larger. The estimate of β in Equation 1 will measure the net effect of both changes—that, once divested, power plants become smaller, and in the counter-factual, they would have become larger.

Figure 3: Mean Power Plant Capacity by Treatment Arm and Year



Causal interpretation of β in Equation 1 relies upon the strict exogeneity, SUTVA, and the parallel trend assumptions holding. In this context, strict exogeneity can be violated if the original power plant owners under-invested in anticipation of divestiture. Assuming sufficient statistical power, this should manifest as a differential pre-trend that can be detected within an event study, which I provide. Because electricity markets are interconnected, SUTVA violations are a natural concern. This threat is a weakness of many reduced-form studies in electricity and difficult to overcome with existing empirical methods. Finally, the parallel trend assumption asserts that regulated plants form an appropriate counter-factual for how divested plants would have evolved over time, but for divestment. It also implies the absence of group-specific trends. Below, I provide descriptive evidence to support the validity of the parallel trend assumption in this setting. I also address the possibility of outliers within the sample, in light of prior work in the literature (Han et al., 2021; Cicala, 2021).

2.3 Descriptive Evidence

2.3.1 Covariate Balance

Table 1 shows the mean characteristics of power plants in the sample, by treatment arm, in 1990. The Cohen’s d statistic provides the standardized difference of means between the regulated and divested power plants, in (pooled) standard deviation units. Plants in both arms burn a mix of coal, natural gas, and oil; are roughly 25 years old at the start of the panel; and use a similar distribution of generator technologies. The high average heat rate within the sample reflects that the power plants in both arms serve base, intermediate, and peaking load.⁹ Differences emerge in location and exposure to specific fuels. Relative to the mean regulated plant, the mean divested plant is more likely to be located in the Northeast, as shown in Figure 1, reflecting differences in state policy discussed in Section 2.1. It is also less likely to burn coal.

Table 1: Baseline Characteristics of Regulated and Divested Power Plants in the Sample

	Regulated	Divested	Difference	Cohen’s d
Count of Plants	527	273		
Total Capacity	252,682	159,514		
Average Size (MW)	479.47	584.30	-104.83	0.16
Generators per Plant	3.50	4.18	-0.68	0.22
Year In-Service	1966.21	1965.61	0.60	0.06
Reported Heat Rate	12,021.90	12,068.93	-47.03	0.02
Fuel Use, Share for Average Plant:				
Coal	39.89	30.31	9.57	0.21
Natural Gas	29.95	33.33	-3.38	0.08
Oil	30.12	36.35	-6.23	0.14
Other (Wood/Waste)	0.04	0.01	0.04	0.06
Generator Types, Share of Average Plant that Uses:				
Combined Cycle	1.46	2.19	-0.73	0.06
Steam Turbine	57.72	62.04	-4.32	0.09
Gas Turbine	28.03	32.74	-4.71	0.11
Internal Combustion	12.79	3.03	9.76	0.34
Other Generator	0.00	0.00	0.00	0.05
Plant Location, Share in:				
Northeast census region	10.44	44.32	-33.89	0.89
Midwest census region	38.14	16.12	22.02	0.49
West census region	16.89	10.62	6.27	0.18
South census region	34.54	28.94	5.60	0.12

Notes: Fuel use is based on each plant’s annual average consumption between 1980-1990, as reported in EIA Form-759. Heat rates are self-reported by plant operators in EIA Form-860 in 1990, and the average shown excludes 39 plants that did not report values.

One small difference which could have an outsized impact is the slightly higher use of natural gas among divested power plants, because of the particular realization of natural gas prices in the

⁹‘Heat rate’ is the inverse of the thermal efficiency of the plant. It captures the amount of fuel a plant uses as input, in British thermal units (Btu), to produce a single kWh of electricity output. The higher the heat rate, the less efficient the plant.

post-period. Between 2000 and 2010, prior to the rise of shale gas, natural gas prices both rose and became more volatile, spiking multiple times above \$10 per MMBtu, relative to a baseline near \$2.50 in 1999, as traded at the Henry Hub (Joskow, 2013). Fuel price shocks directly impact the profitability of operating a fossil power plant, influencing the owner’s resulting investment decisions. Because divested plants are slightly more likely to use natural gas than regulated, it suggests they could be differentially vulnerable to gas price swings. This would manifest as a differential, time-varying trend in the post-period, confounding the treatment effect estimate.

To thus address any concerns regarding baseline differences in the characteristics of divested and regulated plants, I include robustness checks that refine the set of control plants to those that are most similar to divested plants. I do so using two strategies: re-weighting regulated plants according to the inverse propensity-score, and using only a fixed number of the most similar (“nearest neighbor”) regulated plants as the counter-factual, for each divested plant. The former is similar to the double-robust estimator proposed by Callaway and Sant’Anna (2021) while the latter mirrors the matched difference-in-difference strategy employed by Deryugina, MacKay and Reif (2020) and MacKay and Mercadal (2024) and defined formally by Imai, Kim and Wang (2023).

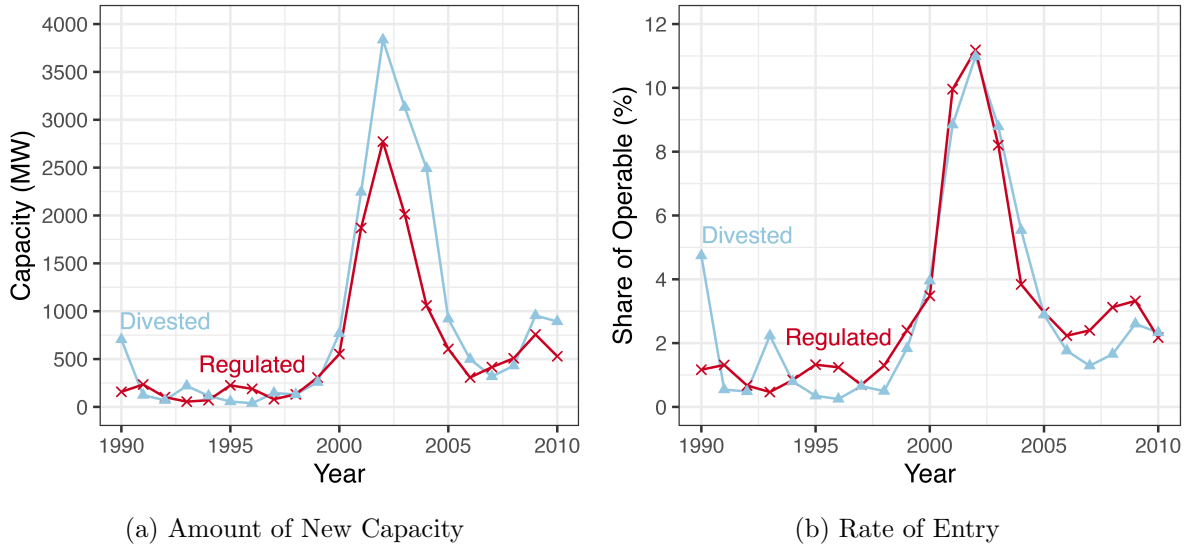
2.3.2 Entry and Demand

Spatial differences in where divested and regulated fossil plants are located create the possibility of group-specific time trends, because of the regional nature of electricity markets, which can confound the estimate of β in Equation 1. A primary example, in this context, is entry—or, the amount of new power plants that are built near each divested and regulated plant. For example, a divested power plant could choose to retire to a generator because, as part of broader deregulation, a glut of new, independently-owned power plants were built in its region. Conversely, a regulated power plant could expand because there are higher barriers to entry in its area, limiting the amount of independently-owned capacity that is built. In this case, the observed difference in plants’ capacity over time (Figure 3) may reflect different levels of overall supply near divested and regulated plants, rather than the causal effect of competitive markets on an individual plant’s outcomes.

The threat of entry can be assessed empirically. Figure 4 tallies the amount of new power plant capacity built within a 200 mile radius of each divested and regulated power plant in the sample over time. Figure 4a illustrates that, in the years immediately following restructuring (2002-4), more power plant capacity was built in the vicinity of divested fossil plants. However, divested plants are also more likely to be located in populated areas of the country that have larger electricity markets (Figure 1). Normalizing the amount of new build by the total operable capacity, also within 200 miles of each plant, Figure 4b shows that the *rate* of entry is very similar, on average, across divested and regulated fossil power plants in the sample. This result mirrors a finding in MacKay and Mercadal (2024), who show that power plant entry is similar across regulated and deregulated utility territories. It also aligns with stakeholder discussions at the time, which were concerned about the lack of entry in restructured regions in the decade post-reform (Kwoka, 2008a).

Similarly, we may be concerned that regulated plants become larger over time (Figure 3) because load is growing in areas where regulated plants are located, and vice versa. Figure 5 shows the

Figure 4: Entry Near Regulated and Divested Fossil Plants



Note: “Near” is defined as being located within a 200 mile radius from each power plant in the sample. Because the EIA did not begin reporting exact coordinates of power plants in Form-861 until 2012, the location of each plant is identified using the best available information: if exact coordinates are not available, the centroid of the plant’s zip code is used; if the plant’s owner reported an invalid zip code, the centroid of the plant’s county is used. “New Capacity” refers to capacity that begins operation in each calendar year, as indicated by its reported in-service year. Plants within the sample are excluded from the new and total operable capacity tallies. The graphs show the simple mean of each outcome, by treatment arm, across the sample.

opposite is true: on average, regulated fossil plants are located in states with a *lower* rate of load growth, compared to those divested. In other words, additions among regulated plants are unlikely to be explained by higher levels of demand growth.

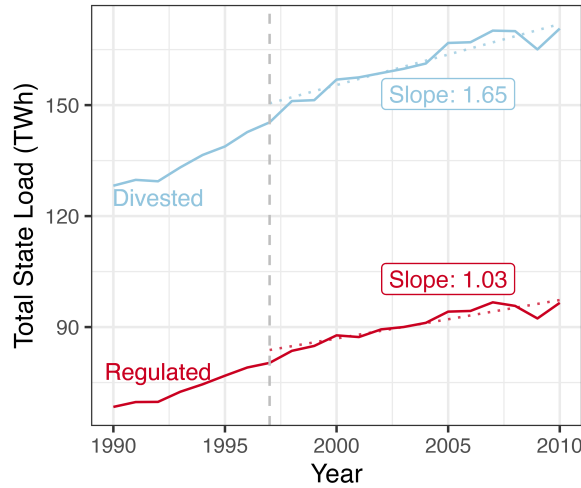
To further rule out any potential confounds, due to differences in the spatial distribution of regulated and divested fossil plants, I include a robustness check that limits regulated power plants to those that fall within a specified physical radius of each divested power plant, similar to the design in Cicala (2015). In this set-up, physical distance acts as a proxy for the market conditions (i.e., entry and demand) to which each power plant is exposed. In other words, only regulated plants that experience very similar conditions to those divested are allowed to establish the counter-factual investment path.

Finally, it’s worth clarifying the relationship between resource adequacy standards and the amount of new build across regions in the U.S. Resource adequacy standards are meant to ensure that there is enough operating capacity available to meet a given reserve margin above expected peak demand, in order to reduce the likelihood an outage occurs from a supply shortage (Bushnell, Flagg and Mansur, 2017). Intuitively, one concern is that these standards could function as a kind of quota for generating capacity, thereby confounding the relative effect of competitive reforms identified by divestment. However, in practice, no entity in the U.S. has the ability to require market participants build new generating capacity to meet an approved resource adequacy standard.¹⁰ Instead, delegated organizations¹¹ are given the authority to

¹⁰See Energy Policy Act of 2005 (2005), Section 215 (a)(3).

¹¹Such as the North American Electric Reliability Corporation (NERC), its regional entities, the regional transmission or independent system operators, or local balancing authorities.

Figure 5: Average Change in State-Level Electricity Demand by Treatment Arm



Note: The graphs shows the simple mean of total retail sales, at the state-level, across divested and regulated plants. Historic state-level retail sales is obtained from the EIA (<https://www.eia.gov/electricity/data/state/>).

impose fines to ensure compliance. As a result, resource adequacy standards do not function as a quota, limiting or requiring new build, but rather serve as the basis for planning or setting incentives, such as capacity auctions or prices. To the extent that standards are affecting new build, shortfalls are likely to be regional, and thus can be addressed by limiting the control group to the regulated plants closest to those divested, as described above.

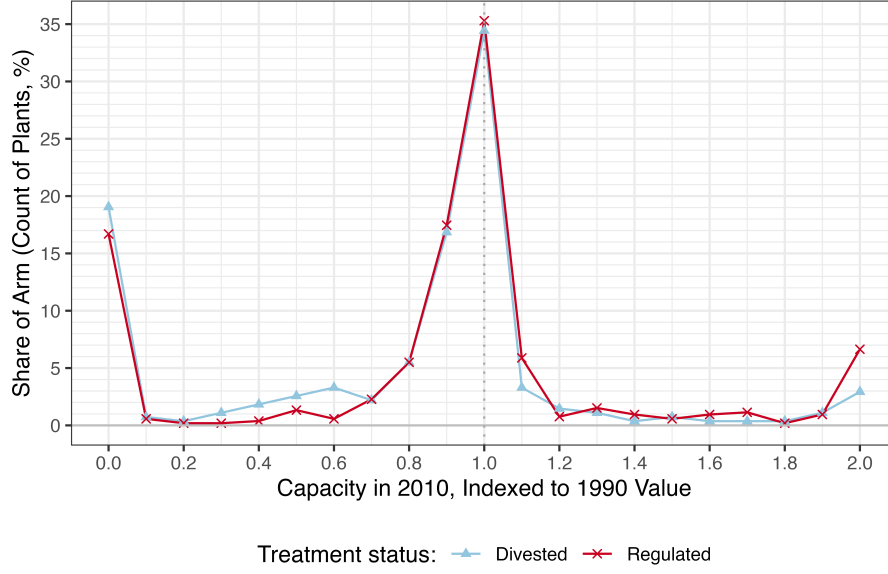
2.3.3 Exit and Outliers

One concern, when viewing the decrease in divested plant capacity over time in Figure 3, is that the exit or retirement of a few plants could drive the observed change in the mean. However, divested and regulated plants in the sample retire at similar rates, with 19% and 17% of plants retired by 2010, respectively. This is shown in Figure 6, which displays the distribution of plant capacities in 2010, as indexed to their starting values in 1990. In the graph, a value of 0 indicates a plant fully retired, a value of 1 indicates the plant stayed the same size, and a value of 2 indicates it expanded to two-times or greater its beginning capacity.

The figure illustrates three additional points. First, the most common outcome for plants in the sample is to remain the same size; around one-third of plants in both arms end the panel in 2010 at the same capacity at which they started in 1990. Second, divested power plants are more likely than regulated to be slightly smaller by 2010: 34% of divested plants end with operating capacities below their starting size, 6% more than regulated. Third, regulated power plants are more likely to become larger: 20% of regulated plants were larger by 2010, 7% more than divested. (It's also worth noting that regulated plants are more likely to become "a lot" larger. More than 6% doubled or more in size by 2010, compared to just 3% of divested plants.) In other words, divestment appears to shift the probability mass function among the two-thirds of power plants that change size over time. This further supports the difference-in-difference design, which would be inappropriate if within-plant change in capacity was uncommon.

While Figure 6 shows the distribution of relative changes in capacity, the treatment effect es-

Figure 6: Distribution of Changes in Plant Size in 2010, Relative to 1990



Note: The values plotted at 2.0 include plants that are at or above twice their initial size in 2010.

timated in Equation 1 can still be affected by outliers in the absolute *magnitude* of capacity added or retired at a single plant. For example, among plants that expand, I find three clear outlier additions in the sample, all of which happen to be regulated plants owned by one utility, Florida Power and Light. The utility’s Fort Meyers, Sanford and Martin plants each more added 1.5-2.5 GW of new capacity per plant by 2010. (For scale, compare that to the mean plant size of 480-580 MW in Table 1.) Similarly, regulated plants that retire happen to be, on average, smaller in size, relative to both the mean of all regulated plants and the mean of divested plants which retired. To assess the sensitivity of the treatment effect estimate to outliers, I thus include specifications which condition on operability in 2010, excluding plants which retire across both arms, and which drop the Florida Power and Light’s Fort Meyers, Sanford and Martin plants and their large expansions.

3 Results

3.1 The Effect of Divestment on Power Plant Capacity

Table 2 shows the results of the difference-in-difference model (Equation 1). Divestment leads to a reduction in plant capacity of 47 MW per year, on average. This effect is equivalent to an 9.1% reduction in plant capacity, relative to the mean plant size among untreated observations (Column 2).¹² The treatment effect is precisely estimated and does not appear to be driven by plant retirements (excluded in Column 3) nor the three outlier expansions among the regulated plants, owned by Florida Power and Light (excluded in Column 4). As mentioned earlier, the simple two-way fixed effect estimate in Column (1) can be biased if the treatment effect is heterogeneous, due to the staggered nature of divestment timing. However, stagger-robust estimators provide a similar treatment effect estimate to the two-way fixed effect model, suggesting the

¹²Note that 9.1% is obtained by transforming the coefficient from the Poisson model using: $(1 - \exp(\hat{\beta}_{pois})) * 100$.

likelihood of bias is low (Columns 5-7).

Table 2: The Effect of Divestment on Operating Capacity

	Operating Capacity (MW)						
	Full Sample		No Retire	No FP&L Out.	Stagger-Robust		
	(1) OLS	(2) Poisson	(3) OLS	(4) OLS	(5) Gardner	(6) Stacked	(7) C&S'A
Divested	-46.80*** (10.28)	-0.0871*** (0.0186)	-40.69*** (9.888)	-40.22*** (9.569)	-48.93*** (11.84)	-35.16*** (8.598)	-41.00*** (10.747)
Squared Correlation	0.97124	0.97191	0.97738	0.97575	—	—	—
Observations	16,800	16,800	13,902	16,737	16,800	49,096	16,800
Dependent variable mean	521.83	521.83	610.09	515.32	—	—	—
Plant fixed effects	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓

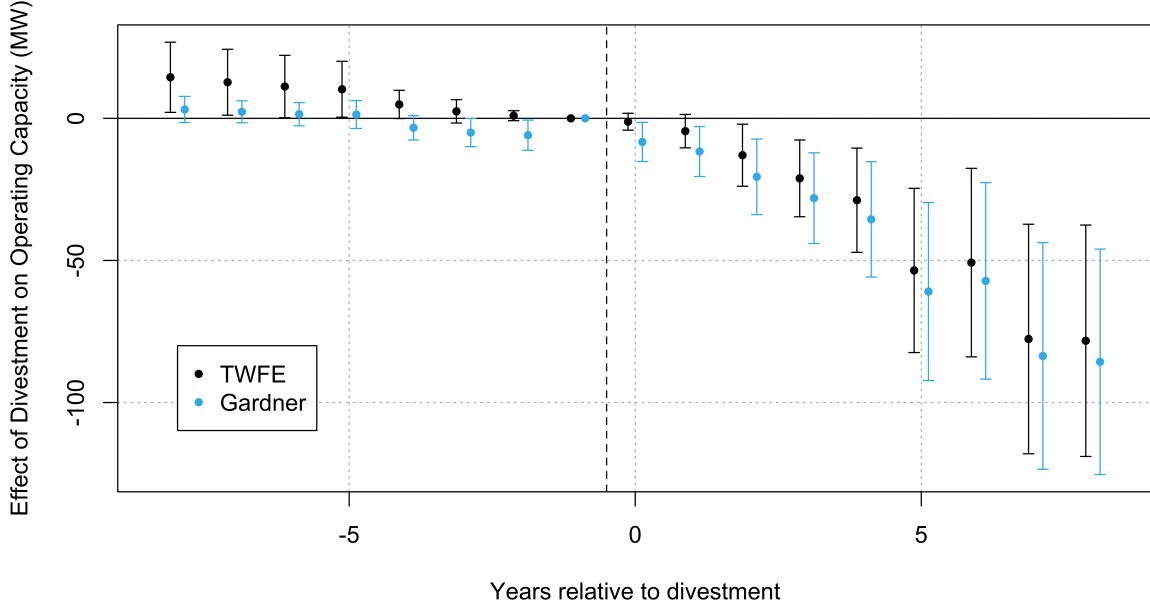
Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the plant level. The sample in Column (3) includes only plants that remain operable at the end of the panel. The sample in Column (4) excludes the three largest absolute expansions—Fort Meyers (EIA Plant ID 612), Sanford (EIA Plant ID 620), and Martin (EIA Plant ID 6043) power plants, all owned by Florida Power and Light—from the control group. Column (5) is estimated using the two-stage difference-in-difference estimator proposed by Gardner (2022). Column (6) is estimated using the stacked estimator as proposed by Wing, Freedman and Hollingsworth (2024). The stacked estimator in Column (6) uses 8 pre-periods and 8 post-periods and includes only plants that were divested in 1998-2002. Column (7) is estimated using Callaway and Sant’Anna (2021), and the aggregate coefficient shown is the average of the mean treatment effect for each timing group.

An event study shows that divested plants do not receive statistically different investment in the pre-period, prior to sale (Figure 7). The figure includes both the simple two-way fixed effect estimate, as well as a stagger-robust estimator (Gardner, 2022). This supports the assumption that parallel trends likely holds in the pre-period. In addition, the event study is sufficiently powered to identify economically meaningful violations of this assumption (Roth, 2022). A pre-trend with slope 2.56 MW per year and 0.65 MW per year would be identified 99% of the time under the two way fixed effect and stagger-robust event studies, respectively—both of which are much smaller than the estimated treatment effect in the post-period (47 MW per year). This suggests the risk of confounding due to an un-identified pre-trend is minimal. Finally, in the post-period, Figure 7 shows that the effect of divestment on capacity is dynamic and grows larger the longer a plant has been deregulated. (The overall coefficient of 47 MW per year, from Table 2, should be interpreted as, in any given year, after divestment, a power plant is on average 47 MW smaller, relative to its regulated peers.)

Fossil power plants are designed to meet different portions of electricity demand, based on the underlying generator technology used. For example, steam turbines that burn coal have a low average cost when operated consistently, but are slow to increase generation; plants that use them usually serve “base” load and will generate throughout the year. In contrast, internal combustion or gas turbines that burn oil or natural gas can ramp generation quickly but incur high marginal costs to do so; plants that use these generators are typically “peaking” capacity that will operate infrequently, during the hours with the highest demand. In Table 3, I examine if the effect of divestment is heterogeneous across plants that share similar operating fundamentals. I assign plants to one of three categories, based on the type of load a plant is likely to serve as a function of its baseline characteristics.¹³ I find that the effect of divestment is consistently

¹³Fuel use and operation at the plant level can be quite complex and defy simple categorization, due to the many permutations with which generators of different types can be combined within a single plant. For example,

Figure 7: Event Study of the Effect of Divestment on Plant Capacity



negative and significant across plant types, but that it has a relatively larger effect on the size of plants that initially served intermediate and peaking load versus those that served base load. This suggests that the magnitude of the effect of deregulation on capacity may be mediated by the specific way a plant’s economics are affected by a competitive market structure.

Table 3 uses a simple form of matching, by splitting the sample based on three overall types of power plants. We may still be concerned that small baseline differences in the types of plants that were divested and regulated could confound the overall treatment effect estimate, as discussed in Section 2.3.1. Here, I implement two alternate and more precise matching methodologies. The goal is to refine the set of regulated power plants to those which are most similar to those divested on observable characteristics. First, I re-weight regulated plants according their inverse propensity-score. The propensity score is estimated using a logit model of divestment on the age, capacity, heat rate, and fuel mix of each plant in 1990 (Table 1). Second, I identify a fixed number of “nearest neighbor” regulated plants, to act as the counter-factual for each divested power plant, using an equally-weighted distance metric of the log of capacity, fuel mix, and the plant’s generation profile (share of generation by month). The two strategies achieve similar improvements in the balance on mean characteristics within the sample; the key difference is that all regulated plants are included as controls within the inverse-propensity score model (but may have a weight of 0), while only selected plants are included in the matched approach (and may be included in the counter-factual for multiple divested plants). The results, in Table 4, show that the magnitude and precision of the estimated treatment effect remains stable when

it is not uncommon for a coal plant, which I assign to the “baseload” type, to have a single gas turbine generator that it uses in a peaking capacity. As a result, these categories should be viewed as a coarse but well-informed guess as to the operating profile of the plant overall, as a way to group plants that share similar fundamentals. I also caveat that plant type is endogenous over time and can change based on the addition or retirement of specific generators, which is why I assign type based on plants’ baseline characteristics.

Table 3: Heterogeneous Effects by Plant Type

	Operating Capacity (MW)					
	Baseload		Intermediate		Peaking	
	(1) OLS	(2) Poisson	(3) OLS	(4) Poisson	(5) OLS	(6) Poisson
Divested	-31.57** (13.88)	-0.0359** (0.0159)	-92.35*** (25.53)	-0.1449*** (0.0388)	-12.10** (4.943)	-0.1322*** (0.0500)
Squared Correlation	0.98329	0.98343	0.91861	0.92215	0.96129	0.96755
Observations	6,048	6,048	4,788	4,788	5,964	5,964
Dependent variable mean	878.43	878.43	608.68	608.68	90.472	90.472
Plant fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Plant N	288		228		284	
Divested N	81		106		86	
Coal (%)	97.62		3.9		0.48	
Natural Gas (%)	1.34		65.88		33.41	
Oil (%)	0.96		30.23		66.1	
Steam Turbine (%)	96.15		85.84		0.34	
Gas Turbine (%)	3.48		8.1		73.44	
Internal Combustion (%)	0.15		0.46		26.13	

Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the plant level. Plant type is assigned based on a plant's historic fuel use in the decade prior to the panel (1980-1990), as reported in EIA Form-759, as well as the technology of its generators, as reported in EIA Form-860. Further details are provided in the Online Appendix. The coal, natural gas, and oil percentages report the average use of each fuel, at the plant-level, between 1980-1990, as reported in EIA Form-759. The steam turbine, gas turbine, and internal combustion turbine percentages report the average share of each generator type, at the plant-level, in 1990, as reported in EIA Form-860.

refining the control plants to those that are most similar to divested plants. This suggests that baseline differences in the observed characteristics are not confounding the estimated effect of divestment on plant capacity.

A primary concern, given the difference in the spatial distribution between divested and regulated fossil power plants (Figure 1), is that power plants in each arm could be exposed to differential market conditions over time, biasing the treatment effect estimate in Table 2. Section 2.3.2 showed, descriptively, that there are similar rates of entry near divested and regulated plants, and that regulated plants are, on average, located in states with lower rates of load growth than divested. Both provide confidence that regulated and divested plants experience similar market conditions over time, limiting the risk of group-specific trends in the post-period. As an additional robustness check, Table 5 limits the regulated power plants in the sample to those that are located near divested plants. I define “near” as falling within a specified physical radius (i.e., within 250 miles). In this set-up, physical distance acts as a proxy for the market conditions (supply, demand, fuel prices) to which each power plant is exposed; in other words, it controls for potential confounds by excluding regulated power plants which are located far away and thus more likely to experience differential trends over time.

The results in Table 5 show that, when regulated plants are required to be within 300 or 250

Table 4: Sensitivity of Results to Differences in Baseline Plant Characteristics

	Operating Capacity (MW)			
	IPS Weights	Nearest-Neighbor		
	(1)	(2)	(3)	(4)
Divested	-45.41*** (11.26)	-50.80*** (14.97)	-58.36*** (14.61)	-54.61*** (12.76)
Squared Correlation	0.97115	0.96320	0.96996	0.97114
Observations	16,716	9,135	13,923	15,435
Dependent Variable Mean	521.83	576.39	575.89	556.56
N Controls	—	1	5	10
Plant fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the plant level. In Column (1), “IPS” stands for inverse-propensity score. In Columns (2)–(4), divested plants are matched to their 1, 5, and 10 most-similar controls. For the inverse propensity-score weights, all divested plants receive a weight of 1, while control plants are weighted according to $\hat{e}_p/(1 - \hat{e}_p)$, where $\hat{e}$ is the estimated propensity score. For the nearest-neighbor control sample, the model is estimated using pooled OLS, where control plants are re-weighted according to the frequency with which they are chosen.

miles of a divested plant, the sign, magnitude and precision of the estimated effect of divestment remains stable, relative to the full sample (Table 2). At smaller radii of 200 and 100 miles, the effect divestment is still negative and precisely estimated, but the magnitude of the effect decreases by about 5 MW/year. I note, however, that the composition of divested power plants begins to change at these radii; a divested plant is dropped if there is not at least one regulated plant that falls within the specified radius. There is a detectable effect of divestment up to a radius of 50 miles, which is likely to be an overly restrictive distance to establish similar market conditions. (The sample also shrinks considerably at this radius.) Taken together, the results suggest that any local differences in market conditions that could affect the size of a power plant—such as new build or load—are not driving the overall estimated effect of divestment on capacity.

Table 6 assess the role of selection in treatment assignment among power plants owned by a utility—i.e., is there evidence that owners chose to divest their lowest-quality power plants? The analysis leverages heterogeneity in state policy; intuitively, we would expect selection bias to be minimal when utilities were required to divest their generation assets and did not have a choice on which specific plants to offload. I split divested plants into three groups: those in states which passed policy requiring the deregulation of all fossil assets (Column 1); those in states where policy only required partial divestment, or where divestment was encouraged but not required (Column 2); and those in states without any legislation that sought to restructure generation (Column 3). Owners of plants in both Columns 2 and 3 had a degree of choice over whether and which plants to divest. The results show a larger effect of divestment on capacity among plants where divestment was partial or encouraged (Column 2), suggesting selection may play a role in these regions.¹⁴ However, I also find that the treatment effect among plants where divestment was required (Column 1) is similar in magnitude and precision to the overall treatment effect

¹⁴Interestingly, I find no effect of divestment among plants in states without legislation (Column 3); however, the number of divested plants is very small in this subgroup.

Table 5: Sensitivity of Results to the Location of Regulated Plants

	Operating Capacity (MW)					
	(1) OLS	(2) Poisson	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Divested	-37.94*** (9.461)	-0.0703*** (0.0170)	-34.05*** (9.272)	-30.53*** (9.019)	-25.33*** (9.679)	-13.16 (11.01)
Squared Correlation	0.97580	0.97593	0.97604	0.97699	0.97394	0.97739
Observations	13,986	13,986	13,251	12,138	9,324	5,481
Dependent variable mean	510.59	510.59	510.87	521.2	543.95	509.1
Radius	300 mi	300 mi	250 mi	200 mi	100 mi	50 mi
<i>N</i> Divested Plants	270	270	270	266	243	140
<i>N</i> Regulated Plants	396	396	361	312	201	121
Plant fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓

Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the plant level. Because the EIA did not begin reporting exact coordinates of power plants in Form-861 until 2012, the location of each plant is identified using the best available information: if exact coordinates are not available, the centroid of the plant's zip code is used; if the plant's owner reported an invalid zip code, the centroid of the plant's county is used. The three plants without any regulated plants within 300 miles (and excluded from all samples) are the J.L. Bates (EIA ID 3438), Laredo (EIA ID 3439), and La Palma (EIA ID 3442) power plants, all of which are located in South Texas near the Mexican border.

in Table 2. This implies that, if selection effects are present, they do not dominate the overall treatment effect estimate.

Finally, the treatment effect for divested plants that were sold will capture the combined effect of two underlying changes: exposure to new economic incentives and to a new owner. One concern is that the observed effect of divestment may be driven entirely by the effect of ownership, rather than incentives, such that attributing the reduction solely to competitive markets would be inappropriate. To explore the relative contribution of each factor, I re-estimate the effect separately among divested plants that were transferred to an affiliate of the original owner and among plants that were sold to an unaffiliated entity (Table 7). Plants that were transferred remain within the same parent company as the original, regulated utility, such that we would expect the effect of new ownership to be minimal, whereas plants that were sold will reflect both changes. The results show that the effect of divestment is larger among plants that were sold, indicating that a change in ownership contributes to the observed reduction in investment. However, the treatment effect among plants that were transferred is similar in magnitude to the overall estimate (Table 2), which suggests that the change in economic incentives is responsible for the majority of the observed effect of divestment.

In summary, I find that divestment reduces the capacity of fossil power plants. The overall effect of 47 MW per year (Table 2) is precisely estimated and robust to multiple alternative specifications. The reduction does not appear to be driven by retirements (Table 2); outlier additions (Table 2); differences in plant characteristics (Table 4); differences in market conditions, based on where plants are located (Table 5); nor selection among assets, when utilities could choose which plants to divest (Table 6). The likelihood of bias due to the staggered timing of

Table 6: Exploring Concerns About Selection Into Divestment

	Operating Capacity (MW)		
	Required (1)	Partial or Voluntary (2)	No Legislation (3)
Divested	-42.78*** (10.67)	-68.81*** (20.97)	29.38 (38.16)
Squared Correlation	0.97570	0.96999	0.97590
Observations	14,553	13,083	11,298
Dependent variable mean	506.67	518.32	499.55
<i>N</i> Divested Plants	166	96	11
States	CT, MA, MD ME, NJ, PA TX	CA, DE, IL MI, NY, OH	DC, IN, LA, MT, VA, VT, WA
Plant fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓

Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the plant level. State coding relied primarily upon Andrews (2000); FTC (2000) and individual state's regulation, as needed. Treated plants in each model are limited to those that fall in the states specified.

divestments is low (Table 2); there is not evidence of differential pre-trends (Figure 7); and the event study is sufficiently powered to identify economically meaningful violations of the parallel trend assumption in the pre-period. The effect of divestment is larger among plants that likely serve intermediate and peaking load, relative to those that serve base (Table 3), and the change in economic incentives, rather than a change in ownership, appears to be responsible for the majority of divestment's effect (Table 7).

3.2 Translating Capacity to Capital Investment

Power plants are capital intensive. For example, 1 MW of new capacity can cost from about \$800,000 to over \$2 million to build, depending on the generator technology used. Translating the effect of divestment on capacity into dollars thus requires two things: an assumption about generator technology, and a set of benchmark cost factors (\$ in capital cost per unit of capacity). Cost factors are easy to obtain from the EIA, which publishes representative capacity costs, by generator type, as part of its Annual Energy Outlook.¹⁵ For the counter-factual generator technology, I examine two scenarios: first, that power plants would have remained the same type; and second, that they would have converted to natural gas.

The results are shown in Figure 8. The left-hand graph, Figure 8a, shows the estimated magnitude of avoided investment for a single power plant. The right-hand graph, Figure 8b, aggregates across the 273 divested power plants in the sample to provide a total value. I use the coefficients from Table 3, which estimates the effect of divestment on capacity separately for power plants that share operating fundamentals; power plants are grouped into those that likely served base, intermediate, and peaking load, at the start of the panel. The first range in Figure 8a shows how much investment was avoided by the reduction in capacity, due to divestment, assuming

¹⁵The capacity cost assumptions for each Annual Energy Outlook are available at: <https://www.eia.gov/outlooks/aeo/archive.php>.

Table 7: Separating the Effects of Incentives and Ownership

	Operating Capacity (MW)	
	Transfer	Sale
	(1)	(2)
Divested	-41.95*** (10.92)	-57.77*** (17.51)
Squared Correlation	0.97606	0.96994
Observations	14,112	13,755
Dependent variable mean	508.61	516.72
<i>N</i> Divested Plants	145	128
Plant fixed effects	✓	✓
Year fixed effects	✓	✓

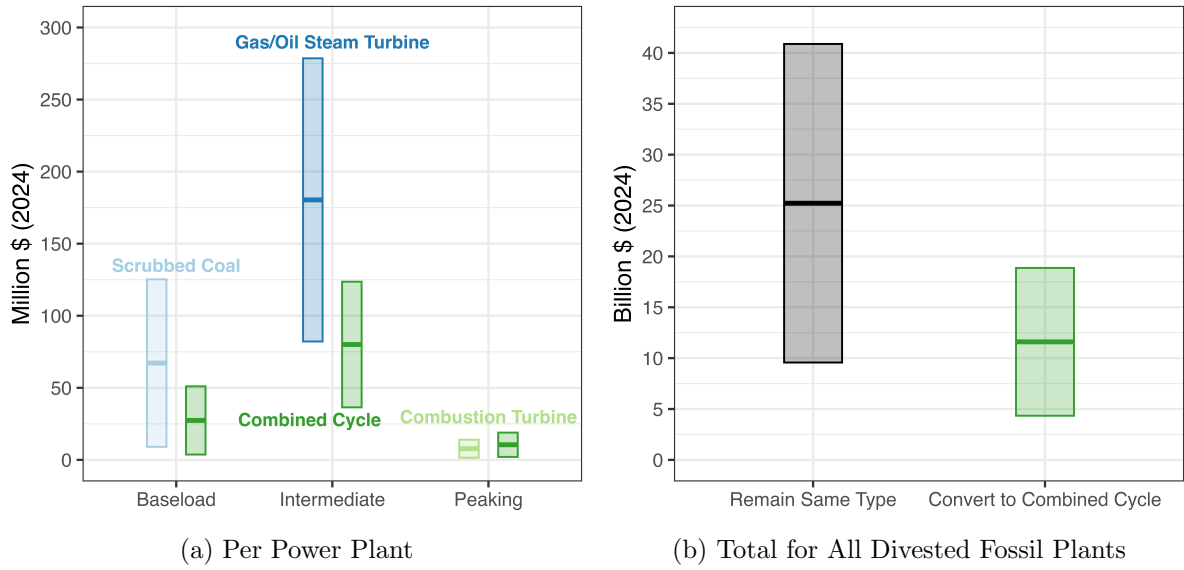
Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the plant level. ‘Transfer’ includes divested plants that were transferred to a company that is an unregulated affiliate of the original utility. ‘Sale’ includes divested plants that were sold to a different entity that was not associated with the original owner.

the avoided capacity was the same dominant generator type as the original plant: scrubbed coal for baseload, gas/oil steam turbine for intermediate, and combustion turbine for peaking plants. The second range assumes the plant converted to natural gas, and the avoided generator was a combined cycle turbine.

The results show that the estimated reduction in capacity, due to divestment, likely corresponds to a large magnitude of avoided investment in fossil power plants, across plant type and scenario. At the plant level, savings are the largest among fossil power plants that likely served intermediate load (\$182 million, if a steam turbine, and \$81 million, if combined cycle, for the mean plant) because the estimated effect of divestment on capacity is largest, in absolute terms, for these plants (Table 3). Because the capital cost of a combined cycle generator (\$867 per kW) is lower than that of a coal (\$2,127 per kW) or steam (\$1,953 per kW) generator, the total magnitude of avoided investment is lower if I assume plants would have converted to natural gas, instead of remaining their original type. Under the status quo scenario, I estimate that divestment would have avoided on the order of \$25.4 billion in capacity costs (\$9-41.4 billion in the 95% confidence interval), in total, across all divested power plants in the sample. Under the natural gas conversion scenario, I estimate divestment would have avoided about \$11.7 billion (\$4.4-18.9 billion) in capital costs, in total.

A careful reader may recall that the estimated effect of divestment measures the combination of two underlying changes: in the post-period, regulated fossil power plants became larger over time, on average, while divested plants became smaller (Figure 3). When converting the effect on capacity into an amount of investment, we may wonder if both dynamics are equivalent, in terms of dollars avoided. For example, if a divested power plant retires a generator, leading to a smaller overall size, has capital investment been saved? Here, I argue the evolution of divested power plants reveals a version of their counter-factual size, had they been built under a competitive market structure. Separately, the growth of regulated plants indicates capacity that divested plants would have added, but did not. In both cases, the overall effect of divestment captures the sum of capacity that could have been or was avoided, and so is appropriate to use

Figure 8: Estimated Magnitude of Avoided Capital Investment due to Divestment



Notes: Capacity costs are obtained from the EIA 2000 Annual Energy Outlook, which benchmark projects initiated in 1999, the first full year after divestments began in 1998. These assumptions are available in Table 37 of the Assumptions file, available at: <https://www.eia.gov/outlooks/aeo/archive.php>. Capacity costs are converted to \$ real 2024. The capital costs for scrubbed coal generator are assumed to be \$2,126.86 per kW; for a oil/natural gas steam turbine are \$1,953.16; for a combustion turbine are \$640.76; and for a combined cycle turbine are \$866.57. Figure 8a multiplies by the point and 95% confidence interval of the coefficients in Table 3 by the specified cost factor for each generator type. Figure 8 then multiplies the range at the plant-level by the number of divested plants in each category—81 baseload plants, 106 intermediate plants, and 86 peaking plants—to arrive at the total values for the cohort.

in estimating an equivalent magnitude of avoided investment.

4 Interpreting the Results

In this section, I provide evidence two additional pieces of evidence to help contextualize the observed reduction in fossil power plant capacity, from divestment, documented in Section 3. First, a decrease in capital investment at a power plant is not necessarily a “good” thing. That is because the value of the avoided cost has to be weighed against its potential benefit for how the power plant operated. For the change in capacity to suggest a gain in economic efficiency, we expect the cost of any change in generation to be less than the value of avoided capital. This question is addressed in Section 4.1. Second, part of the effect of divestment is a function of the growth of regulated fossil power plants during the post-period (Figure 3). In Section 4.2, I explore if there is evidence to suggest the change is driven by the incentives cost-of-regulation creates for capital investment. The answer can affect how we understand the mechanisms behind the effect of divestment, as well as the likelihood it may be externally valid for other generating technologies, beyond fossil.

4.1 How Fossil Power Plants Operated, after Divestment

Investment in capacity at a power plant can change how it operates. For example, a new generator is more likely to be fuel efficient than older technologies. All else equal, this might allow a power plant to burn less fuel and emit less pollutants while producing the same amount of electricity. Similarly, a new generator may enable a power plant to burn a different fuel, such

as switching from coal to natural gas. Because natural gas is roughly half as carbon intensive as coal, the new capacity could significantly reduce the carbon dioxide emissions from the power plant. Both of these changes have corresponding social value. As a result, it's essential to catalogue if the counter-factual, higher level of investment among regulated power plants led to any detectable benefits.

In Table 8, I use data from the EPA for a sub-sample of 310 plants (72% of capacity), from 1997 onwards, to assess the effect of divestment on how fossil plants operated. (The ‘CEMS sub-sample’ is explained further in Section 1.3.) Each coefficient is obtained from a two-way fixed effect regression, following the main specification in Equation 1, and Figure 9 graphs the corresponding mean of each outcome. (In the Supplemental Information, I show the results are not sensitive to the use of stagger-robust estimators.) Before moving to the new operational outcomes from EPA data, I first re-estimate the effect of divestment on plant capacity, to assess the stability of the coefficient between the full and sub-samples (Column 1). I find the effect is nearly identical in magnitude, sign and precision, suggesting plants in the sub-sample are, on average, representative of changes in investment within the full sample.

Table 8: Changes in Plant Operation due to Divestment in CEMS Sub-Sample

	Capacity (1)	Capacity Factor (2)	Operating Hours (3)	Heat Rate (4)	CO2 Intensity (5)	CO2 Emissions (6)
<i>Panel A: Outcomes in Levels</i>						
Divested	-47.44*** (14.97)	0.3445 (1.507)	-581.2*** (171.0)	-16.68 (145.1)	-0.1487 (1.209)	-101,393.4 (110,018.6)
Mean Outcome	994.22	53.673	7,518.2	10,619.4	91.422	4,596,126.6
Squared Correlation	0.97456	0.87505	0.78904	0.67788	0.89390	0.97985
Observations	4,340	4,340	4,340	4,340	4,340	4,340
<i>Panel B: Logged Outcomes</i>						
Divested	-0.0442** (0.0193)	-0.0151 (0.0388)	-0.1392*** (0.0485)	-0.0027 (0.0124)	-0.0039 (0.0134)	-0.2046*** (0.0726)
Squared Correlation	0.98394	0.85555	0.74064	0.69597	0.84727	0.92626
Observations	4,337	4,337	4,340	4,340	4,340	4,340
Plant fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓

Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the plant level. Sample is limited to the 310 plants in the CEMS sub-sample. Capacity is obtained from the EIA, while all other outcomes are from the EPA. The units for capacity are MW; for capacity factor are percentage points; for heat rate are MBtu of heat input per MWh of gross generation; for CO2 intensity are short tons per MBtu; and for CO2 emissions are short tons.

Moving from left to right in Table 8, I first assess how divestment affected the total generation of a plant (Columns 1-3), before estimating the impact on how the plant used fuel (Columns 4-5), and ending with the net effect on the total carbon dioxide emissions (Column 6). These six variables, including capacity, have the convenient relationship that:

$$CO_{2,t} = Generation_t \cdot HeatRate_t \cdot CO_2Intensity_t \quad (2)$$

$$= \{Capacity_t \cdot (CapacityFactor_t|Op.) \cdot OpTime_t\} \cdot HeatRate_t \cdot CO_2Intensity_t \quad (3)$$

$$= \{MW_t \cdot \frac{MWh_t}{MW_t \cdot Hours_t} \cdot Hours_t\} \cdot \frac{MBtu_t}{MWh_t} \cdot \frac{st_t}{MBtu_t} \quad (4)$$

In other words, the total carbon emissions from a plant (Column 6) can be obtained from the product of its capacity, capacity factor, operating time, heat rate and the carbon intensity of its fuel mix (Columns 1-5). It follows, by taking the log of both sides of the equation, that the percentage change in carbon emissions is approximately equal to the sum of the percentage change in the other factors. This is shown in Panel B of Table 8, which reports coefficients from log-linear, two-way fixed effect specifications.

Starting with how much plants generate, I find that divestment reduces the number of hours a plant operates: operating time declines by approximately 13.0%, after divestment (around 581 hours each year) (Column 3 and Figure 9c). This result suggests that divested plants may have been dispatched less frequently within competitive market structures. However, I do not find that divestment affects the *rate* at which the plant produces power in the hours when it does operate. This is captured by the capacity factor (Column 2 and Figure 9b), which normalizes how much generation each unit of capacity produces, on average, within a given hour.¹⁶ The null result indicates that divestment does not affect the average productivity of plants, once operating. The combined effect of the changes in capacity and operating time is an overall decrease in annual generation among divested plants, relative to regulated, of approximately 18.0%, on average.

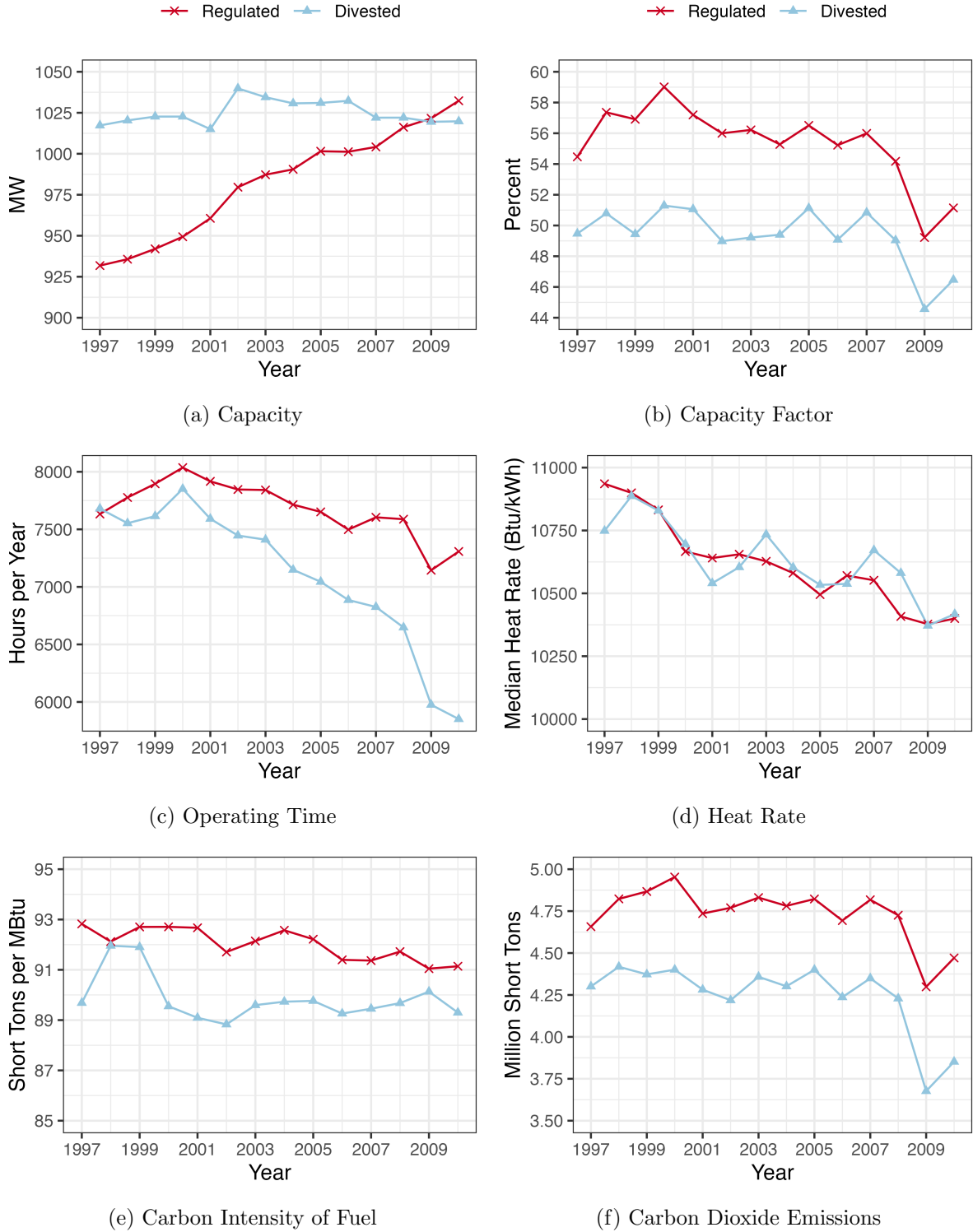
Turning to fuel use, I find no effect of divestment on the annual average heat rate of plants nor the carbon intensity of their fuel mix (Columns 4-5 and Figures 9d-9e). Both results are somewhat striking. Heat rate quantifies the amount of fuel a plant needs to burn as input, to produce each unit of output. It thus measures the thermal efficiency of the plant—the heat rate declines (less fuel, per unit output) when fuel efficiency improves—and, as described earlier, is expected to be lower among newer technology. As shown in Figure 9d, both divested and regulated plants demonstrate markedly similar gains in average heat rates over time. This suggests that the expansions at regulated plants did not lead to a disproportionate gain in the overall thermal efficiency of the plants, and that divested plants were able to achieve similar efficiency improvements, with less apparent capital input. Similarly, the carbon intensity of the plant’s fuel mix would decline if a plant switched from coal towards natural gas. The fact that there is no detectable change in the carbon intensity of heat input among divested and regulated plants indicates that the additions at regulated plants did not substantially displace their use of coal. The end effect is an overall decrease in carbon emissions, after divestment, of about 18.5%, driven nearly entirely by the relative reductions in capacity and operating time.¹⁷

One concern about the above results is that the impact of new natural gas generators among regulated plants may have been muted, due to the specific (high) realization of natural gas prices in the post-period. As a result, natural gas generators may have been less economic to

¹⁶For example, a 50 MW plant with a 10% capacity factor would generate 5MWh of electricity in one hour. Here, I have defined the capacity factor to be conditional on operation, meaning that the capacity in the denominator is multiplied by the number of hours the plant operates within a year, rather than 8,760 (the total number of hours in a non-leap year). This is shown in Equation 4.

¹⁷Why is there no detectable effect on the *level* of carbon dioxide emissions, in Panel A? As a variable, the mass of carbon dioxide emissions has a large range across plants in the sample: a power plant at the 75th percentile of emissions would have released over five times more carbon than a plant at the 25th in 1997. As shown in Figure 9f, the mean level of carbon emissions—which will be affected by high-emitting plants—is relatively stable over time and similar between groups, providing the intuition for the insignificant coefficient in Panel A. The decrease in carbon dioxide is instead concentrated among lower-emitting, infra-marginal plants, whose generation declines as they operate less.

Figure 9: Mean Operational Outcomes Among CEMS Sub-Sample



Notes: The CEMS sub-sample contains 310 power plants that comprise 72% of the capacity in the full sample in 1997. See Section 1.3 for a description of the underlying data.

run during these years, in a way that would not reflect their long-term economics after prices fell due to fracking. Luckily, we are able to learn something about plant behavior during “low” natural gas prices by limiting our attention to the final two years of the panel, 2009-10, when the

annual average Henry Hub price fell to \$3.94 and \$4.37 (compared to \$8.86 in 2008; all values in nominal \$ per MMBtu). Even when allowing for a differential treatment effect during low (2009-10) and high (2008 and earlier) natural gas prices, I still do not find a statistical difference in average heat rates or carbon intensity among regulated and divested plants (Table 9). While the share of generation from natural gas was 2.97 percentage points higher among regulated plants under low prices (Column 5), this marginal increase was not large enough to detectably shift the overall carbon intensity nor thermal efficiency at the plant-level.¹⁸

Table 9: Sensitivity of Operational Outcomes to Natural Gas Prices

	Heat Rate		CO2 Intensity		% Natural Gas
	Level	Log	Level	Log	Level
	(1)	(2)	(3)	(4)	(5)
Divested (Low NG Prices)	-15.12	-0.0006	0.6824	0.0078	-2.970*
	(192.4)	(0.0168)	(1.500)	(0.0158)	(1.779)
Divested (High NG Prices)	-17.03	-0.0032	-0.3382	-0.0065	-0.8114
	(141.4)	(0.0120)	(1.158)	(0.0131)	(1.181)
Squared Correlation	0.67788	0.69598	0.89397	0.84735	0.96377
Observations	4,340	4,340	4,340	4,340	4,340
Mean Outcome	10,619.4	9.2618	91.422	4.4894	24.173
Plant fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓

Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the plant level. Sample is limited to the 310 plants in the CEMS sub-sample. ‘Low NG Prices’ limits the post period years to 2009, and ‘High NG Prices’ includes all post years in 2008 and earlier. ‘% Natural Gas’ is the share of generation using natural gas as the primary fuel. The units of heat rate are MBtu of heat input per MWh of gross generation; of CO2 intensity are short tons per MBtu; and ‘% Natural Gas’ are percentage points.

It’s worth noting that the CEMS data does not help us identify the effect of divestment on the operation of peaking plants, specifically, which comprise an estimated 5.72% of capacity in the full sample in 1990. The sub-sample studied here consists primarily of plants that would serve base and intermediate load, based on the frequency of their operation at baseline. (The mean plant in the CEMS sub-sample operated for 7,646 hours—87% of the year—in 1997, and only 9% of plants operated for half the year or less.) It is difficult to anticipate if divestment would affect peaking plants differently than those serving base or intermediate load. On the one hand, peaking plants already operate infrequently during the year, such that they may be less likely to experience the changes in operating time observed here. On the other hand, because heat rates are nonlinear and increasing at low levels of generation, it’s possible any small shift in operating time among peaking plants could differentially affect their average thermal efficiencies. Given this, how competitive markets may affect peaking plants, in particular, remains an open question for future work. In addition, the CEMS data only provides one year of pre-period data for power plants that were divested in 1998. As a result, I view the results as strong suggestive evidence, but caution against strict causal interpretation.

¹⁸Cullen and Mansur (2017) find evidence of meaningful coal-to-natural gas switching among US power plants, in response to natural gas price fluctuations. I note that this paper asks a slightly different analytical question—not, “Did plants switch?”, but, “Did divested plants switch by a greater amount than regulated plants?”—and studies a more limited sample of power plants (only those owned by regulated utilities and operating in 1990).

Given information on how divested fossil power plants operated, we can assess whether the decrease in capital investment is likely to represent a gain in economic efficiency. Noting that capital is an input to each power plant’s production function, the reduction in investment would signal a gain in efficiency if its value is less than the cost of lost revenue, due to the lower level of generation or output.¹⁹ The mean power plant in the CEMS sub-sample generates 268,463 MWh less, each year, after divestment. If we assume a baseline capacity cost of \$1,000 per kW, the wholesale power price would need to have been at or above \$179 per MWh, on average across all hours, for the reduction in revenue to exceed the magnitude of avoided investment. Conversely, at an average wholesale price of \$60 per MWh, capacity costs would need to be \$339 per kW or cheaper. (I assume labor and gross fuel expenditures are constant; because generation decreases among divested plants, this is a conservative assumption.) Both scenarios are highly improbable. In 2022, the EIA listed only one generator type with capacity costs below \$1,000 per kW: combustion turbines, at \$785 per kW (\$ 2021).²⁰ Similarly, the weighted average wholesale price in the PJM market, where many divested plants are located, was \$57 per MWh between 2001 and 2010.²¹ As a result, it’s highly unlikely that the magnitude of lost revenue exceeded the change in investment for mean divested plant in the CEMS sub-sample. This suggests that the reduction in capital investment likely represents a gain in economic efficiency among divested fossil power plants.

4.2 Exploring the Growth Among Regulated Fossil Power Plants

The effect of divestment on capacity is identified from two underlying changes: regulated fossil power plants became larger over time, while divested plants became smaller (Figure 3). The evidence presented so far suggests that the change is driven by the economic incentives introduced by competitive markets, relative to those created by cost-of-service regulation. In this section, I provide additional evidence to support that the change in economic incentives is likely the primary mechanism behind the estimated effect of divestment. Specifically, I show that additions among regulated fossil power plants coincided with the conditions under which the Averch-Johnson effect—or, an increase in the capital intensity of a firm subject to cost-of-service regulation—is most likely to emerge.

The intuition for these conditions rests on a difference between how Averch and Johnson modeled cost-of-service regulation and how it is implemented by U.S. states to govern electric utilities. The Averch-Johnson model assumes that the regulator fixes the return on investment that a firm earns (Averch and Johnson, 1962). In practice, states regulate electric utilities’ price, rather than their return (Lazar, 2016). Policymakers do use a “regulated” return as an input to determine prices within a rate case, but once the price is fixed, regulators do not control nor oversee the actual returns utilities achieve. As Paul Joskow showed, under this set-up, the regulated return is most likely to act as a binding constraint during periods when the utility’s average production costs are increasing (Joskow, 1974). It follows that Averch-Johnson style effects are most likely to be observed when utilities’ average production costs are rising.

¹⁹This follows the logic in Davis and Wolfram (2012), who study the reverse scenario: an increase in output among divested nuclear plants.

²⁰See the “Assumptions to the AEO2022”, Electricity Market Module, Table 3: <https://www.eia.gov/outlooks/archive/aeo22/assumptions/>.

²¹See “Historical Wholesale Market Data”, 2001 thru 12/31/2013, PJM-West Hub: <https://www.eia.gov/electricity/wholesale/>.

Figures 10 and Table 10 show how average production costs evolved, during the panel, among utilities which own regulated fossil power plants in the sample. (See Section 1.4 for a description of the data.) The analysis shows that average production costs for these companies increased significantly during the post-period. The utilities’ operating expenses began to rise around 2000 (Figure 10a), leading average production costs to be 33.6% higher, across the sample, from 1998-2010 (Figure 10b and Column 4 of Table 10).²² In line with Joskow’s prediction, we observe a coincident and significant increase in overall capital investment: the utilities invested 31.1% more on average, each year, during the post-period, when production costs were higher (Figure 10c and Column 6 of Table 10). These changes led to a decrease in estimated returns on investment earned by the utilities, from a high of 8.9% in 1995-96 to 6.4% in 2010 (Figure 10d). This suggests that the “regulated” return was more likely to bind during the post-period.

Table 10: Change in Regulated Utility Operating Costs and Investment in the Post-Period

	Operating Expenses (\$ mil)		Average Cost (\$/kWh)		Net Plant Investment (\$ mil)	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Poisson	OLS	Poisson	OLS	Poisson
Post-Period (1998-2010)	610.2*** (73.52)	0.4270*** (0.0314)	0.0238*** (0.0025)	0.2899*** (0.0288)	810.5*** (112.4)	0.2707*** (0.0244)
Squared Correlation	0.85844	0.87785	0.62497	0.63776	0.88982	0.89939
Dependent variable mean	1,612.2	1,612.2	0.08916	0.08916	3,226.8	3,226.8
Observations	1,258	1,258	1,255	1,255	1,258	1,258
Utility fixed effects	✓	✓	✓	✓	✓	✓

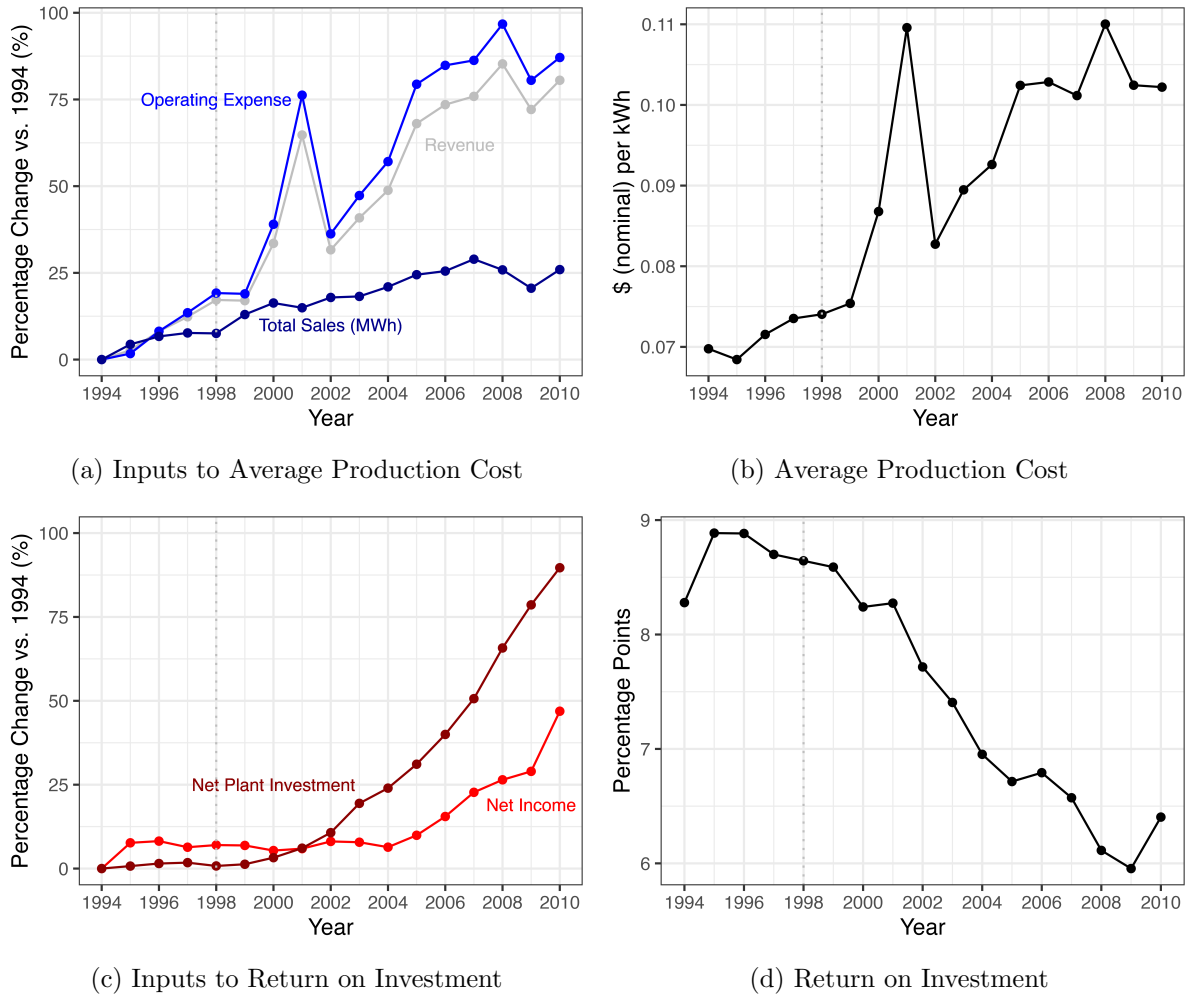
Notes: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Standard errors are clustered at the utility level. Please see additional notes in Figure 10.

Regulated fossil power plants in the sample establish the counter-factual evolution for divested plants. A key point of Joskow’s paper is that the distortionary effect of cost-of-service regulation on capital investment, as highlighted by Averch and Johnson, is not an inevitable outcome, but rather depends on the financial conditions in which utilities operate (Joskow, 1974). The evidence here shows that these conditions were present, on average, during the post-period, for regulated utilities that owned fossil power plants in the sample. This helps clarify the specific counter-factual that these power plants benchmark in the difference-in-difference estimation. Their size reflects the investment decisions made under cost-of-service regulation, when cost-of-service regulation is most likely to bias production decisions towards capital. This further supports the interpretation that the likely mechanism behind the estimated effect of divestment is the change in economic incentives between regulated and competitive markets.

There are two additional implications of this understanding for how we view the external validity of the treatment effect estimates in Section 3 to different generating technologies and time periods. First, the Averch-Johnson model describes a change in behavior at the firm-level, not at the plant-level. This implies that at least part of the estimated effect of divestment that I measure reflects a portfolio-wide change in investment among utilities, rather than a change in investment in fossil power plants, specifically. Second, the effect of competitive markets on capital investment in electricity generation, relative to cost-of-service regulation, may be smaller in periods when regulated utilities’ average production costs are falling, rather than rising. While

²²The spike in 2000 is driven by an increase in purchased power costs, observed across utilities (i.e., it does not appear to be driven by a single company).

Figure 10: Assessing if the Averch-Johnson Effect Distorted Regulated Utility Investment



Notes: All data is obtained from FERC Form 1. The sample consists of 74 utilities who own regulated fossil power plants in the sample and who report consistently to the FERC. Operating expense is total of operation and maintenance costs, amortization, depreciation, and taxes, net of deferrals and adjustments. It captures the amount of operating costs a utility would recover within regulated rates, prior to any adjustments by regulators. Average cost is operating expense divided by total sales. Net plant investment is the total un-depreciated value of capital investment made by the utility across functions. It represents the investment on which a utility would earn a return within regulated rates, prior to any adjustments by regulators. Net income is the difference between revenue and operating expense. The return on investment is net income divided by net plant investment. The values shown are the mean outcome across the sample. The average production cost is given in nominal dollars (Joskow, 1974), because regulated prices do not typically contain inflation adjustments. An equivalent chart in real dollars is provided in the Supplementary Information.

there is preliminary evidence that the increase in operating costs and decrease in returns for regulated utilities has continued beyond 2010 (Yozwiak, 2023), data centers supporting artificial intelligence and electrification are both sources of expected load growth in the near-future, which could apply downward pressure on utilities' average costs. The operating conditions of electric utilities are thus likely to be an important mediator of the effect of competitive markets, when estimated against a cost-of-service counter-factual.

5 Conclusion

How do competitive markets affect capital investment? In this paper, I document that competitive reforms led to a large and robust decrease in capital investment among fossil power plants

in the United States. I identify the effect of competition by using the large-scale divestiture of power plants that occurred in the late 1990s, in which regulated utilities sold or transferred hundreds of power plants to unregulated entities, as part of broader reform. By comparing fossil power plants before and after divestment, I find that power plants are 47 MW (9.1%) smaller, on average, after exposure to competitive markets, relative to similar units that remain regulated. The reduction in size corresponds to approximately \$25 billion in avoided investment, in total, across the cohort of 273 divested plants. Power plants do generate less power, after divestment, but the cost of lost revenue is likely smaller than the value of the avoided investment, implying the change in investment is likely to represent a gain in economic efficiency. Finally, I show that counter-factual additions at regulated fossil plants occurred during a period when cost-of-service regulation was most likely to lead to over-capitalization among utilities. This suggests that at least a portion of the estimated treatment effect reflects a change in utility-level investment behavior and is less likely to be specific to only fossil power plants.

I also find a connection between the operating characteristics of a power plant and the effect of competitive reforms. The effect of divestment is smaller, proportionally, among fossil power plants that likely served base load (which are 3.5%, or 32 MW, smaller, on average) versus those that likely served intermediate (13.5%, or 92 MW, smaller) or peak load (12.4%, or 12 MW, smaller). In other words, competitive reforms appear to reduce investment more among power plants that are likely to have higher short-run marginal generation costs. This is consistent with the findings of Davis and Wolfram (2012), who show that capital investment at nuclear power plants—which have very low short-run marginal generation costs—increases slightly, after divestment. In both cases, the change appears to be efficient. Taken together, the results suggest that competitive markets may lead to a *reallocation* of capital investment across different power plant types—based on plants’ differential ability to compete within wholesale markets—rather than a uniform change in investment levels.

This interpretation can provide some intuition for how the effect of competitive markets may change for future investment in renewables, relative to past investment in fossil, based on the operational differences between the two technologies (Joskow, 2011). For example, unlike a coal, oil, or natural gas plant, solar and wind do not require fuel to produce electricity; they thus bid in to wholesale markets at zero or very low short-run marginal costs, similar to nuclear plants (Bushnell and Novan, 2021; Jha and Leslie, 2025). We might therefore expect competitive market structures to *increase* renewable investment, relative to a regulated counter-factual.²³ In addition, unlike both nuclear and fossil power plants, renewable technology is not typically dispatchable.²⁴ In markets with high levels of renewable penetration, solar and wind can be curtailed, if the supply of renewable generation exceeds demand (Novan and Wang, 2024), directly affecting a plant’s revenue. We might imagine, as a result, that the effect of competitive reform could vary based on the level of renewable capacity that is already operating within a region, creating interesting patterns of spatial and time heterogeneity.

It’s worth restating the key caveats to and limitations of the empirical analysis. First, the

²³There is some descriptive evidence this is occurring: to date, regulated utilities have not invested heavily in renewables (Fogler and Ver Beek, 2023; Andonov and Rauh, 2024). However, I note the competitive counter-factual is confounded by the fact that states which enacted competitive reforms were also more likely to introduce stringent renewable portfolio standards, which is a major driver of renewable build (Carley et al., 2018).

²⁴Exceptions are photovoltaics paired with battery storage, concentrated solar-thermal, and some types of geothermal systems.

estimated effect of competitive reforms represents an average of the places where divestments happened to occur, which largely reflects states that first adopted restructuring legislation. Caution is warranted before assuming the effect is externally valid for other regions of the country. Second, the effect was estimated during a period when average production costs for regulated utilities were rising. The relative effect of competition may be lower in periods when cost-of-service regulation is less likely to lead to over-capitalization among firms. Third, high quality operational data is not available for all years and power plants in the sample. In particular, I observe only one year of pre-period data for plants divested in 1998, and there is limited information on how peaking power plants operate, after divestment. The results for fossil plants' output should therefore be viewed as strong suggestive, but not strictly causal, evidence. Fourth, the effect of competitive reforms reflects the wholesale market designs in place during the post-period (1998-2010), and changes to market rules can alter the estimated effect of reforms. Finally, the exact amount of capital investment spent at a power plant is unobserved, once it is divested. While there is a high degree of precision and stability in the estimated change in capacity, the equivalent value of avoided investment is necessarily approximate and relies on assumptions of generator type and average capital costs.

A central policy goal of competitive reforms in electricity generation was to alter the incentives for new investment in power plants. The hope was that competition could establish more efficient incentives for investment, relative to cost-of-service regulation. Among fossil power plants, between 1990 and 2010, I find evidence this goal was likely achieved. The magnitude of avoided investment represents billions in cost-savings for consumers in markets served by divested power plants. The results provide direct evidence of the causal effect of market structure on capital investment in electricity generation. Further work is needed to understand how differences in regulation, across U.S. states, could affect the future capital costs of decarbonization.

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