Three Essays in Corporate Finance

by

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ABSTRACT

This dissertation comprises of three essays in corporate finance.

Chapter 1 studies the effects of carbon transition on shareholder value of publicly-traded utilities. We establish a causal link between carbon emissions and shareholder value using the passage of the Regional Greenhouse Gas Initiative (RGGI) that imposed a cap-and-trade policy for carbon emissions on electric utilities in several Northeastern and Mid-Atlantic states. The regulation was successful in significantly bringing down the level of CO_2 emissions from plants located in the RGGI states compared to unaffected plants. The affected plant's revenue and profitability decreased after the RGGI as they transitioned to cleaner technology. Publicly traded power utility companies in the affected states experienced a drop in their profitability as well. Yet, they had a higher market-to-book ratio after the implementation of the initiative. Increase in value came from increasing demand of the treated firm's stocks by institutional funds focused on environmental goals, and an increase in the expected future cash flows of the treated firms. Since CEO compensation is tied heavily to earnings in this industry, our results show that managerial focus on short-term profitability is likely to be a significant impediment to carbon transition.

In Chapter 2, I study how private equity invetments in fossil fuels affect the transition to clean energy. Contrary to the prevailing notion that private equity investment in fossil fuels adversely affects environmental outcomes, this paper shows that it can facilitate the green transition by allowing cleaner technologies to develop. I show that private equity (PE) acquisitions of fossil fuel power plants are followed by an 8% higher likelihood of solar development and a 10% increase in the number of solar plants in the same county. This increase comes from institutional investment in solar, specifically from the investors related to the PE owners of fossil plants. I establish a causal link between the PE acquisition of fossil plants and solar development using the intensity of sunlight that falls on fossil plants as a measure of solar investment opportunity and the passage of the investment tax credits that made solar power commercially attractive. In a difference-in-differences setting, I show that PE firms are more likely to buy fossil plants that provide higher solar investment opportunities. These findings suggest that regulations prohibiting PE investment in fossil fuels may prevent clean energy financing and impede the green transition.

In Chapter 3, I study the effects of creditor rights on bankruptcy outcomes of small businesses. I show that the presence of secured creditors impedes the successful reorganization of small businesses. There are two underlying mechanisms behind my finding: (i) excess market power of secured creditors decreases the owners' share of the going-concern surplus, which in turn reduces their incentives to exert costly effort to reorganize; (ii) in the presence of multiple creditors, the secured creditors suffer from conflicts of interest and coordination failure that make contract renegotiation more difficult. To achieve identification, I exploit the passage of the Small Business Reorganization Act of 2019 that exogenously and discontinuously changes the creditor's control on the bankruptcy process for eligible small businesses. Overall, my paper shows that in the presence of frictions such as information asymmetry, financial constraints, and coordination failure, creditors' market power and control can cause fewer successful reorganization.

CHAPTER 1

Carbon Emissions and Shareholder Value: Causal Evidence from the U.S. Power Utilities

1.1 Introduction

How do environmentally friendly policies such as transition to cleaner technology affect shareholder value? The nature and extent of trade-off between shareholder value and stakeholder outcomes has been at the center of academic and policy debates for decades (Friedman, 1970), with a renewed interest in the topic in recent years (Hart and Zingales, 2017; Edmans, 2021; Bebchuk and Tallarita, 2020; Edmans and Kacperczyk, 2022). Specifically, the issue of carbon-transition and its impact on shareholders has taken the center stage of several policy initiatives and academic debates on climate finance. Yet, it has been difficult to establish a causal link between transition to cleaner technology and shareholder value.

There are two primary reasons for this gap in the literature. First, environmental policies of a firm are likely to be correlated with other attributes of the firm that can independently affect shareholder value. For example, unobserved managerial preferences, technological differences, or a firm's investment opportunity set can affect both its investments in pollution control strategies and shareholder value at the same time. The second challenge relates to the measurement of environmental policies of the firm and their impact on pollution. For example, researchers often use ESG scores from outside rating agencies to measure a firm's environmental policies, making them prone to subjective assessment, error, and disagreement (Berg, Koelbel and Rigobon, 2019). Further, in the absence of a standardized reporting system of externalities across firms, environmental outcomes such as pollution are measured indirectly and often with noise and subjectivity. For example, a commonly used measure of carbon emission is based on the Greenhouse Gas (GHG) protocol that provides a set of guidelines to capture the extent of pollution emitted by corporation. These are indirect measures of pollution, the reporting is voluntary, and there are no enforcement mechanisms, all of which leads to a possibility of greenwashing (e.g., see Grewal, Richardson and Wang (2022)).

We overcome these challenges in this paper by focusing on the U.S. power utilities, a sector that contributes to more than 25% of the country's total carbon emission, and by exploiting a regulation that provides an exogenous variation in the pollution control policies of power utilities located in some of the Northeastern and Mid-Atlantic states of the country.[1] The Regional Greenhouse Gas Initiative (RGGI) is a mandatory, market-based, regional program to reduce CO2 emissions from the power sector in the U.S. It is a cap-and-trade policy where the participating Northeastern and Mid-Atlantic states set a regional cap on carbon emissions from utilities in their states. The initiative provides a meaningful variation in the adoption of clean technology across the RGGI-affected and non-affected states, a variation that is independent of managerial preferences, technological changes, and hidden investment opportunity set of the individual power plants. The governors of the RGGI states signed an MoU for this initiative in 2005 and the cap-and-trade program went into effect in 2008. We compare the environmental and shareholder outcomes across the affected and non-affected states before and after the adoption of the law, paying careful attention to the transition period between 2005-2008 in our empirical analysis.

Our empirical setting is attractive from the measurement viewpoint as well: all the power utilities in the U.S. are required to report their carbon emissions in a consistent manner at a very granular plant level. Specifically, our measure of pollution comes from a Continuous Emissions Monitoring System (CEMS) that all power utilities in the U.S. with more than 25MW generating capacity are required to install at every fossil fuel power plant. The CEMS measures particulate matter concentration or emission rate using pollutant analyzers and reports it continuously. We also obtain information on the level of electricity generation, revenue, and profits at the plant level, which allows us to directly link carbon emission with profitability outcomes. Finally, a subset of these utilities are publicly traded, which allows us to trace the effect of carbon transition on shareholder value.

The adoption of RGGI provides us with an exogenous measure of transition to green technology for three main reasons. First, the initiative required regional cooperation across neighboring states, making it less susceptible to endogeneity concerns that arise due to concomitant changes in other policies at the same time that are typically enacted either at the state level or at the national level. Related, the passage was possible due to the concerted efforts of the bureaucrats in these sates as argued by several leading policy scholars and observers (for example, see Rabe (2010)). Second, utilities in the RGGI states objected to

[1]See the data from the Environmental Protection Agency on CO2 emission by sector here: https://www.epa.gov/ghgemissions/sources-greenhouse-gas-emissions

the initiative when it was being actively debated by the lawmakers. Therefore, the policy is less likely to be affected by biases that come from lobbying efforts of the affected party. Finally, the passage of the law required the support of lawmakers from all the states in the region. Since democratic and republican voters and politicians often have considerable disagreement over environmental issues, and even within each party politicians differ on their commitment to green transition, the passage of the RGGI initiative was possible due to convergence in the viewpoint of lawmakers at a given point in time. In fact, whether to join the RGGI was often an important part of the election campaign of both the democratic and republican gubernatorial candidates in these states. New Jersey provides a visible example of the political differences that affected a state's decision to join the initiative: New Jersey initially joined the RGGI under a democratic governor and later withdrew from it in 2011 after the election of Governor Chris Christie, a republican candidate, in 2010. Therefore, the passage of the initiative was heavily influenced by political considerations.[2] The unique aspect of geographical clustering, bureaucratic cooperation, and political alignment that were needed for the passage of the RGGI makes it unlikely that the post-RGGI adoption of green technology was driven by any other systematic differences in omitted variables such as managerial ability, preferences, other state-specific laws, non-climate related technological differences, relative pice of coal versus other fuels, or hidden investment opportunity set of utilities in the RGGI states.

Our first set of results compare carbon emissions from fossil-fuel plants located in the treated states, i.e., the RGGI affected states, with those in all other states, i.e., the control states, in the country in a difference-in-differences setting. Carbon emissions from the treated and control plants show a parallel trend till 2005. But the emissions from the treated plants started to decline immediately after the signing of the MoU in 2005. Between 2005 and 2008, the treated plants reduced their carbon emission by 21% compared to the control plants. After 2008, i.e., after the implementation of the cap-and-trade policy, the reduction was an even larger 50%. These are economically large effects and show that the initiative had the intended effect of controlling pollution in these states.

In addition to comparing the treated states with all other states in the country, we also conduct two tests in which we compare them with power plants only in the states that voted for democrats in the presidential election of 2008 (Democrat states) and with power plants in the states with deregulated electricity market (Deregulated states).[3] These sets of

[2] Huber (2013) provides a detailed analysis of various factors, including business and political opposition, behind the passage of this law.

[3] Electricity markets in 17 states were deregulated in late 1990s and early 2000s that allowed consumers to select electricity provider of their choice and increased competition in electricity generation market. All RGGI states except Vermont were deregulated between 1996-2000. Details on deregulation can be found

counterfactual states allow us to alleviate concerns that our results are due to coordinated differences in political ideologies, policies or market structure across states. Our results remains similar. Therefore, the reduction in emission is driven by the passage of the initiative, rather than any other changes at the same time in the market for power or other climate related policies that coincide with the political affiliation of the ruling party. We now analyze the implications of the adoption of cleaner technology on shareholder outcomes and the trade-off between stakeholders and shareholders.

In terms of real operating decisions, we show that the affected plants significantly cut their electricity generation in the post-RGGI period. More specifically, they reduced their electricity generation from fossil fuel by around 52% compared to control plants. However, the reduction in emission was not driven entirely by the decisions to produce less power from fossil fuels. Even after controlling for the level of electricity generation, the treated plants reduced their carbon emission by almost 15%, i.e., carbon emission came down on a per unit of generation basis as well. There are two main mechanisms behind this. First, the power plants specifically reduced generation from the dirtiest fossil fuel: coal. We find that the coal consumption of the treated power plants declined by 70% post-RGGI. On the other hand, there was no significant decline in the consumption of natural gas, which is a relatively cleaner fossil fuel. Second, the affected plants began to use better quality of coal. Carbon emissions vary significantly depending on the type of coal used for power generation. For example, it varies by the mix of carbon, hydrogen, oxygen and sulfur contained in the coal (Hong and Slatick, 1994). Cleaner coals are more expensive. We show that after the RGGI shock, the treated plants started to use more expensive coal, consistent with the view that they adopted cleaner coal in their production decision.

As a result of these decisions, the affected plants experienced a sharp drop in their revenue and profits. Using our difference-in-differences design, we show that the fossil-fuel plants had almost 46%-51% reduction in their revenue in the post-RGGI period. Profits, defined as revenue minus fuel costs, decreased by an even higher amount. For an average power plant, profits decreased by around 70% in the post-RGGI period. Our unique setting allows us to estimate the elasticity of carbon emission to revenue and gross profits at a very granular level using an instrumental variable setting. Using RGGI as an instrument for carbon emission in a two-stage regression framework, we estimate the elasticity of plant revenue to carbon emission at 0.87-0.92, depending on the model specification. For an average power plant, the elasticity of gross profits is estimated at 0.84-0.92. These results show that shareholders experience a significant decline in profits when they move towards cleaner technology, and that reduction in CO_2 emissions comes at a cost to the bottom line, at least in the short-term.

here: https://www.electricchoice.com/map-deregulated-energy-markets/

We now ask what impact does reducing CO_2 emissions have on the shareholder value. To address this question, in the next part of the paper we analyze these outcomes at the company level. Power plants can be owned by publicly traded companies, private equity, or other investors. We analyze the effect of RGGI shock on profits and shareholder value for the publicly traded power utilities in the affected states as compared to publicly traded utilities in other states, using the same difference-in-differences design that we use for the plant level analysis. At the company level, we do not find any evidence of a decline in annual revenue. The affected utilities transitioned from coal based power generation to cleaner technology to protect their revenue and power supply to customers. However, their profitability decreased considerably: compared to all other utilities in the country, their return on assets (ROA) decreased by around 2.6% in the transition period, i.e., between 2005 and 2008, and over 2.3% thereafter. The decrease in ROA represents almost 70-80% of one standard deviation of the ROA in our sample. Similarly, the earnings per share (EPS) of the treated firms decreased by 54 cents during the interim period and 64 cents after 2008, representing 30-35% of one standard deviation of the EPS in our sample. Therefore, the transition to clean technology resulted in significant short term losses for the affected firm.

Did the shareholders lose as firms transitioned to cleaner technology that resulted in short-term losses? We next analyze the evolution of shareholder value, measured as the market-to-book value of equity and the market-to-book value of assets, of the utilities in the affected states before and after the passage of the initiative. In the interim period of 2005-08, there is some evidence of a decrease in shareholder value, but the results are economically small and statistically weak. The estimates vary depending on model specification, the measure of market-to-book ratio used, and the set of control firms employed for the counterfactual analysis. However, in the post-2008 period, we find a consistent pattern of an increase in value across model specifications and valuation measure used for the analysis: the treated utilities have 22-36% higher market-to-book ratio of equity, and 4-6% higher market-to-book ratio of assets in the post-2008 period compared to the control states. Therefore, despite a drop in profitability, shareholders gained in the long run as they switched to cleaner technology.

What are the sources of value creation for the treated utilities despite a drop in profitability? Green firms can obtain higher value if their future expected cash flow is higher or if their expected return is lower. As Pástor, Stambaugh and Taylor (2021) show, a green firm can have a lower expected return if investors have a preference for green stocks or if greener assets provide a better hedge against climate risk. Second, the treated firms can have higher value through the cash flow channel. For example, if customers prefer to buy clean energy or if suppliers care about clean firms, then the treated firms may have higher cash flows than the control firms in terms of market's expectation. Similarly, if the treated firms are likely to face lower regulatory costs in the future, then they may have higher expected cash flows.

A large emerging literature documents that institutional preference for green stocks can result in an upward shift in the demand curve of these stocks. Indeed, institutional investors have become increasingly concerned about climate risk of their portfolio companies and they also exert influence on ESG related outcomes (Krueger, Sautner and Starks, 2020; Chen, Dong and Lin, 2020). Stroebel and Wurgler (2021) document that practitioners, academic and policymakers alike recognize the increasing importance of climate risk. Gantchev, Giannetti and Li (2022) show the effect of institutional shareholder pressure on environmental policies of firms. Further, Baker et al. (2022) show that green municipal bonds are often issued at a premium and are more closely held, consistent with the view that some institutional investors exhibit a preference for non-pecuniary benefits from holding such assets. The realized returns on green stocks may go up due to the demand pressure generated by institutional flow into these stocks. At the same time, the expected return can decrease, which lowers the discount rate for the firms' cash flows (Pástor, Stambaugh and Taylor, 2021; van der Beck, 2021). Both of these channels can, in turn, result in higher market-to-book ratio of the treated firms. For example, van der Beck (2021) shows that flow of funds into sustainable mutual funds can substantially increase the value of green stocks.

Motivated by these studies, we investigate the flow of ESG-focussed mutual funds into the treated stocks after the RGGI shock to shed light on the institutional preference channel. A key advantage of our work, compared to the earlier literature, is that we are able to tease out the causal effect of institutional fund flow on valuations. We first show that the ESG-funds increased their shareholdings in the affected companies by a significant amount after the RGGI shock. Their shareholding, measured as a percentage of the total outstanding shares of the company, increased by 0.26 percentage point in the affected utilities after the shock. Similarly, as a fraction of their own assets, ESG funds held about 0.07 percentage point more shares in the affected utilities after the shock.

In our next test, we directly relate the valuation premium of treated stocks with the entry of ESG-funds in the market. Since the RGGI shock occurred in the mid-2000, our experimental setting provides a unique setting to assess the effect of ESG-funds on the value of green stocks. During this period, the ESG-focussed funds had just begun their entry into the financial markets, providing us with a rich time-series variation in the number of such funds over time. We show that over time, as the number of ESG-funds increased, the market-to-book ratio of treated stocks increased considerably. Thus a part of valuation increase, despite the decline in profits, can be attributed to the preference of institutional investors for greener firms.

We analyze the expected cash flow channel using analysts' long-term growth forecast of the treated and control firms. We show that the analysts expected about 2-3% higher long-term growth rate in the earnings of the treated firm after the shock compared to the control firms. We control for analyst fixed effects in these regression models; therefore, our estimates come from changes in forecast of cash flows that are independent of a particular analyst's time-invariant forecasting skills or attitude towards climate risk. It captures changes in cash flow expectation of market participants as the firm transitions to a cleaner technology. Combined with our earlier results that show a decline in realized profitability of the treated firms, these findings uncover the trade-off between short-term and long-term cash flow impact of carbon transition.

Our findings raise a natural question: if firms benefit in terms of higher market valuation, why didn't they adopt green technology even in the absence of the RGGI? There are two, not mutually exclusive, possibilities. First, our sample period comes from the early days of climate awareness and its acceptance by the finance community. Hence, ex ante, the benefits of carbon transition may not be clear to the managers. The second possibility is rooted in an agency theory view of corporate management. Since carbon transition results in a decrease in the short-term profits, corporate managers who are compensated based on their short-term earnings performance may not engage in green-transition even if it results in long-run value creation. We provide support for this view by showing that the compensation contract of utility CEOs depend heavily on short-term profits, namely the earnings per share of the firm. These results provide a tangible policy recommendation: deemphasizing the role of short-term performance on managerial compensation can be an important tool to encourage green transition.

Our paper contributes to a growing literature on climate finance and the effect of pollution on outcomes such as asset prices and shareholder wealth. Krüger (2015) use an event study methodology to show that shareholders react strongly negatively to negative news regarding a company's corporate social responsibility (CSR) policies. Bolton and Kacperczyk (2021) study the link between carbon emissions and stock returns. Acharya et al. (2022) study the effect of heat exposure on municipal bond yields. Chava (2014) document a significant link between ESG policies and cost of capital. Jiang, Li and Qian (2019) study the effect of a firm's exposure to rising sea level on its cost of bank loans. Sastry (2022) shows the effect of flood risk and government insurance on lender behavior in the mortgage market. Giglio, Kelly and Stroebel (2021) provide a comprehensive review of the growing literature on climate finance. Gillan, Koch and Starks (2021) review the literature on ESG policies and firm risk and value.

Although our empirical design is slightly different, namely a difference-in-differences design with carefully selected control groups, the first part of our paper that pollution decreased as a result of the RGGI has been documented earlier in the energy economic literature (Murray and Maniloff, 2015).[4] This literature has mainly focused on the effect of the RGGI on carbon emissions and collection and deployment of revenue from the auction of carbon caps by the state governments (Hibbard et al., 2018). Our key contribution comes from the analysis of shareholder-emission tradeoff. To the best of our knowledge, our paper is the first one to use this exogenous shock to analyze the implication of green technology on firm profitability and shareholder value.

1.2 RGGI Details

The Regional Greenhouse Gas Initiative (RGGI) is the first mandatory, market-based, regional program to reduce CO2 emissions from the power sector. As a regional 'cap-and-trade' program, states jointly decide on a regional cap on total CO_2 emissions from the power plants for a three-year control period. Each state then originates CO_2 allowances proportional to its share of the regional cap. Power plants with a capacity of 25MW or higher in these states are required to buy 1 allowance for every short tons of CO_2 they emit during this control period. The cap on allowance is set to decrease after every control period, reducing overall CO_2 emissions from the power plants.[5]

While RGGI is not the first cap-and-trade program aimed at reducing emissions, it is different from other similar programs in its distribution of allowances. Unlike other programs that gives out the allowances for free and then allow them to trade in the secondary market, RGGI auctions the CO_2 allowances every quarter. In doing so, it generates revenue for the states and adds actual costs to the power producers. In their comments to the first draft of the RGGI model rule, most of the power utilities companies and energy groups opposed this auction and wanted the allowances to be distributed for free.[6]

Figure 1.1 presents a brief timeline of the history of RGGI milestones. RGGI was formed as a result of discussions of several Northeastern and Mid-Atlantic states to reduce CO_2 emissions from the power sector. In December 2005, 7 of these states- Connecticut, Delaware, Maine, New Hampshire, New Jersey, New York, and Vermont- entered into a Memorandum of Understanding (MoU) to develop a regional cap-and-trade program to curb emissions from the power sector in these states. In 2006, these states issued a set of proposed regulations in

[4]The RGGI organization has also documented this effect. Details about the RGGI and their studies can be found on their website: https://www.rggi.org/program-overview-and-design/elements

[5]The regional cap decreased from 188 million allowances in 2009 to around 84 million in 2014, corresponding to an annual reduction of roughly 15%.

[6]Stakeholder comments and the RGGI program can be found here: https://www.rggi.org/program-overview-and-design/design-archive/original-stakeholder-comments

the form of a Model Rule that formed the basis for each state's individual legislation and allowance trading program. In 2007, Massachusetts, Maryland, and Rhode Island, which had participated in the early discussions, signed the MoU. The first CO_2 allowance auction happened in September 2008, and the first compliance period began on January 1, 2009. In 2012, New Jersey became the first (and only) state to withdraw from RGGI after the appointment of the republican governor Chris Christie. Therefore, we exclude New Jersey from our analysis.[7] In 2015, President Obama announced the Clean Power Plan, the first-ever, federal carbon pollution standard for all fossil-fueled power plant aiming to reduce carbon emissions by 32% in the next 15 years. We limit our sample period to the end of 2014 to ensure that our results are not contaminated by the differential effect of this federal announcement on RGGI versus non-RGGI plants.

In sum, the RGGI induced transition to cleaner technology was independent of several other correlated factors that can simultaneously affect pollution outcomes and financial performance of a company. For example, Azar et al. (2021) document the role of institutional investors in controlling pollution. Gantchev, Giannetti and Li (2022) document the importance of investor activism on a firm's environmental policies. Therefore, the presence or absence of institutional investors can affect the pollution strategy, as well as the firm performance. Our empirical setting is free from such biases.

1.3 Data and Descriptive Statistics

Plant level data: Our plant level data covers all the fossil fuel based plants in the country. We use three main sources of data to collect environmental and operational information for power plants and their owners: the US Environmental Protection Agency (EPA), the US Energy Information Administration (EIA), and the Federal Energy Regulatory Commission (FERC). The CO_2 emissions data comes from EPA's Clean Air Markets Division (CAMD). CAMD continuously monitors the level of CO2, NOx, So2, and mercury emissions from all fossil fuel-fired electric generating units (EGUs) over 25MW in nameplate capacity in the U.S.

The measurement of accurate and granular data on carbon emissions is a key advantage of our empirical setting. Prior studies have to often rely on carbon emission estimates from other industries. Most of the non-utility firms today report their emissions based on the standards developed by the Greenhouse Gas (GHG) Protocol Initiative, a multi-stakeholder partnership of businesses, NGOs, and governments. The GHG Protocol standard provides a step-by-step guide for companies to quantify and report their emissions in three categories

[7] Our results are robust to the inclusion of New Jersey in the sample.

or scopes based on the sources of the emissions. However, reporting of the emissions is voluntary, and there are no enforcement mechanisms, such as audits, for the reporting. The lack of standards and enforcement mechanisms exacerbate 'greenwashing' and incentivize firms to underreport or selectively report their emissions (Grewal, Richardson and Wang, 2022). Our setting allows us to overcome the issues of greenwashing. All fossil-fueled power plants in the US with a capacity of 25MW or higher are required to have the Continuous Emissions Monitoring System (CEMS), an equipment system that measures particulate matter concentration or emission rate using pollutant analyzers and reports it continuously.[8] We use the CO_2 emissions reported by the CEMS for our analysis. This overcomes the issues of managers self-reporting the numbers and using different metrics for their measurements.

Plant-level information on fuel consumption, and net electricity generation is obtained from the EIA Form 923. We aggregate fuel receipts that power plants report in the Form 923 to calculate total fuel costs. While information on electricity generation, carbon emissions, and fuel consumption are available on power plant level, data on revenue (in dollars) and sales (in megawatt hours) are only available at the utility-level. Electric utilities that are investor owned and own one or more power plants are required to report their sales and revenue data monthly to the EIA in the EIA-861M, also called the Monthly Electric Power Industry Report. We use this owner-level data to calculate electricity rate ($/MWh) for each power plant in a quarter. Using net generation and fuel costs on power plant level, we are able to calculate total revenue and gross profit, i.e. total revenue minus total fuel cost. We exclude power plants that have no gross generation of electricity in a quarter, and we winsorize all the variables at 2.5% on both the tails. Our results, however, are not affected by winsorization.

Panel A of Table 1.1 reports the summary statistics of the power plant-level data in our sample. The sample covers all fossil fuel based power plants in the country, including coal and gas-based plants. The unit of observation is plant-quarter. On average, there are 850 power plants each quarter in our sample. An average power plant generates 639 GWH of electricity in a quarter and emits 592,000 short tons of CO_2. It generate an average revenue of $53 million. While more than half of the power plants do not use coal as their primary source of fuel, those that do are larger and therefore produce more electricity from coal. A median power plant generates no electricity from coal but a 75th percentile plant consumes over 5,000 billion BTUs of coal and generates 469GWH of electricity from coal. In contrast, a median power plants generates 7.3 GWH of electricity from natural gas while a 75th percentile plant generates only 137.4GWH from natural gas.

Company level data: Our power plant level data is comprehensive. For the market value

[8]For more on CEMS, see here: https://www.epa.gov/emc/emc-continuous-emission-monitoring-systems

analysis, however, we are restricted to the set of publicly traded firms that own these power plants. We use Compustat and CRSP to get profitability and market valuation measures for publicly traded utilities. We start with all publicly traded, electric utilities companies that are engaged in the generation, transmission, and/or distribution of electric energy for sale (SIC: 4911, 4931) and exclude firms that do not generate or purchase power from fossil-fueled power plants. Following the power-plant level analysis, we restrict our sample to 2000-2014 and exclude New Jersey from the sample. We obtain profitability measure from the Financial Ratio Suite by the Wharton Research Data Service (WRDS). WRDS uses Compustat and CRSP to provide time series of profitability ratios per company. We use earnings per share (EPS) and return on earnings assets (ROEA) as our measures of earnings and profitability. ROEA is calculated as the operating income after depreciation as a fraction of average total earnings assets (TEA) based on most recent two periods, where TEA is defined as the sum of property, plant and equipment and current assets. We specifically focus on the earning assets of the company to measure profitability because it tells us about how profitable the assets of the firm that are used to generate income, as against assets such as intangibles and deferred revenue. We use market-to-book ratio of equity and assets as our measures of valuation. Market-to-book of equity is simply the market value of equity divided by the book value of equity. Market value of assets is calculated as the book value of assets minus the book value of equity plus the market value of equity. Market-to-book ratio of assets is then the market value of assets divided by the book value of assets.

Panel B of Table 1.1 presents the summary statistics of the profitability and valuation measures of electric utility companies in our sample. An average electric utility company in our sample has a total revenue of around $1.3 billion, the return on earning assets of 7.9%, and earnings per share of 2 dollars. The average market-to-book ratio of equity is 1.6 and market-to-book ratio of assets is 1.2.

Earnings forecast data: We obtain analysts' long-term EPS growth forecasts for publicly traded utilities from the Institutional Brokers' Estimate System (IBES). We use the Detail file from the IBES database that provides analyst-by-analyst historical earnings estimates. Our sample consists of around 3600 firm-analyst-quarter observations. A median firm in our sample is covered by 2 analysts each quarter. The median (average) long-term growth forecast in our sample is 5% (5.6%).

ESG ownership data: For ESG ownership analysis, we use CRSP mutual funds holdings database. To identify mutual funds that are ESG-oriented, we screen funds that have ESG-related words in their names.[9] We restrict our sample to funds that were first offered before

[9]Similar to Berg, Heeb, and Kolbel (2022), we select funds that have any of the following words in their names: 'ESG', 'SRI', 'Clean', 'Environment', 'Social', 'Sustain', 'Impact', 'Responsible', 'Climate', 'Green',

December 31, 2012 to ensure that we have their holdings information for at least two years before the end of our sample in 2014.

Figure 1.5 shows the total number of ESG-mutual funds in our sample. There were fewer ESG funds in the early 2000s and the number grew significantly from around 2007. There are on average 10-15 ESG funds each year in our sample before 2007. This number increases to around 40 in the year 2008 and around 60 in the 2010. As the number of fund-firm pairs in our transition period was small, we cannot detect the statistical effect of RGGI on ESG fund holdings in the transition period of 2005-2008. So, for ESG fund-related analysis, we focus on post-2008 as our only treatment period.

Executive compensation data: We obtain total compensation information for all executives of electric utilities from 1995 to 2014 from Execucomp. Specifically, we use the variable *TDC1*, which comprises of total salary, bonus, total value of restricted stock grants, total value of stock options granted, long-term incentive payouts, and all other compensation. Our sample has 6977 executive-firm-year observations covering 1084 executives in 54 electric utilities. The average (median) total annual compensation in our sample is \$1.8M (\$1.0M).

1.4 Results

We conduct our analysis both at the plant level and at the firm level. The plant level analysis allows us to precisely detect the relation between pollution control strategies and outcomes such as the level of electricity generated at the plant, cost of fuel used and the revenue and profits from the plant. Since we observe the level of CO_2 emission for each plant in our sample, our analysis directly ties emissions to these outcomes. In the second set of results, we focus on aggregate firm level outcomes that allows us to tease out the impact of carbon emissions on shareholder value.

For the plant level analysis, we use the following difference-in-difference research design using quarterly data for all fossil-fuel power plants in the sample:

$$y_{i,t} = Plant_i + yq_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_i + \beta_2 \times Post2008_t \times Treated_i + \epsilon_{i,t} \quad (1.1)$$

$y_{i,t}$ is the outcome variable such as the level of carbon emission or revenue generated by a plant. The model includes plant fixed effects to soak away time invariant differences in the technology used by the plant and other unobserved differences in the level of emissions each plant produces. The year-quarter fixed effects (yq_t) allows us to separate the effect of macroeconomic factors such as the relative cost of coals with varying degree of emission

'Impact'.

or the demand of electricity in the economy. $Treated_i$ equals one for plants located in a RGGI-state, zero otherwise. We interact the treatment variable to two indicator variables that capture the effect of the RGGI on outcomes. First, we interact it with $\mathbb{1}(2005\text{-}2008)_t$ to tease out the effect of changes in plant behavior soon after the RGGI states signed an MoU in 2005. The second variable $Post2008_t$ captures the differential effect of the enactment of the initiative on the affected plants after the passage of the act. Therefore, our model allows us to estimate the changes in the affected plant's behavior during the interim period, i.e., from the announcement to the enactment of the cap-and-trade policy, as well as during the post-enactment period. Firms can change their behavior while the legislative progress was under way. Our empirical specification captures this effect. More importantly, the inclusion of the interim period indicator variable in the model allows us to capture the market's expectation of the effect of RGGI on firm value when we conduct the valuation analysis later in the paper.

All plants located in the RGGI states are considered as the treated plants. We use three sets of control plants: (a) plants in all other states in the country, (b) plants located only in the states that voted Democrats in the presidential election of 2008 (Democrat states), and (c) plants located only in states with deregulated electricity market (Deregulated states). The first set of control plants has the advantage of larger data and it is free from any assumptions on our part in terms of comparability of the plants across the two sets of states. In our analyses, we ensure that the outcome variables, such as carbon emission, show a parallel trend between the treated and control group. The other two sets of control plants allow us to rule out any concerns that plants behaved differently over this time period because of the political leanings of the lawmakers in the state or the market based forces the firms faced. As we show below, our key results remain the same, no matter which set of control plants we use.

1.4.1 RGGI and carbon emissions

We begin our analysis by detecting whether the treated and control plants showed a parallel trend before the RGGI or not by estimating the following regression model:

$$Log(CO2)_{i,t} = Plant_i + QuarterYear_t + \sum_{\tau}(year = \tau) \times \beta_\tau \times Treated_i + \epsilon_{i,t} \qquad (1.2)$$

The model uses the set of all plants in the non-RGGI states anywhere in the country as the control plants. The dependent variable is the log of carbon emission measured in short tons per plant per quarter. We use 2005 as the base year and therefore the coefficients on the interaction term measures changes in emission relative to the year when the states signed the MoU. We present the coefficient estimates, and the 95% confidence interval in Figure 1.2. The figure shows that the two groups followed a parallel trend before the shock. There is no difference in carbon emissions across the treated and control plants in any of the years between 2000 and 2005. After 2005, however, there is a remarkable decrease in emissions from the affected plant.

Table 1.2 presents results of the regression model in equation 1.1. Column (1) shows that the affected plants cut their emissions by 40% in the post-2005 period in a difference-in-difference setting, which is both statistically and economically significant. Column (2) separates the effect between the interim period (2005-08) and the post-2008 period: the affected plants cut their emission by 20.5% in the interim period as they began adjusting to the RGGI regulations, and by 49.6% in the post-2008 period.

Carbon emissions at the plant level can come down from two broad operation decisions: (a) on extensive margin, cutting the level of generation from fossil fuel based plants while shifting towards renewable energy, and (b) on intensive margin, using environment-friendly operating decisions such as switching to cleaner and better quality of fossil-fuel.[10] Different types of fossil fuel emit different amounts of CO_2 per unit of energy produced. Natural gas, for example, is a cleaner fossil fuel and emits around 50% less CO_2 than coal. Even within the categories of coal, the extent of emission per unit of generation varies considerably depending on the chemical composition of coal. CO_2 emission across the type of coal varies in the following order from most polluting to least: anthracite, lignite, sub-bituminous, and bituminous (Hong and Slatick, 1994). Depending on the type of coal a plant uses, therefore, the level of pollution differs for the same level of electricity generation.

Is the decrease in emissions solely due to the lower level of generation or is it also due to the use of natural gas and cleaner coal? Column (3) answers this question by controlling for the level of power generation by the plant. Even after controlling for the level of electricity generation, we show that the emissions decreased by a significant 14-15% in the interim and post-2008 period. Therefore, the decrease in emissions came from both the extensive margin and intensive margin. Columns (4) and (5) repeat the analysis with just the democratic states and deregulated states the control plants. Our results remain similar.

[10]For example, Consolidated Edison's Annual Report in 2010 states "CECONY has participated for several years in voluntary initiatives with the EPA to reduce its methane and sulfur hexafluoride emissions. The Utilities reduce methane emissions from the operation of their gas distribution systems through pipe maintenance and replacement programs, by operating system components at lower pressure, and by introducing new technologies."

1.4.2 RGGI and real decisions

Table 1.3 presents the regression analysis of model 1.1 with four operating decisions as the outcome variable: (a) the level of electricity generated by the plant measured as the log of MWh, (b) the amount of coal used measured in the log of MmBTU, (c) the amount of gas used measured in the log of MmBTU, and (d) the rate paid for the coal used, measured as the total cost of coal per unit of heat input of the coal. The second and fourth regressions are estimated on coal-based plants only, and the third on gas-based plants separately.

Column (1) of the Table shows that the affected plants decreased their net generation by almost 23% in the interim period and 53% in the post-2008 period. This is an economically large reduction in the generation of fossil-fuel based energy. The magnitude of the reduction in the level of electricity generation closely matches the reduction in pollution level described in the previous section. Note that our sample covers fossil-fuel based plants only. At the firm level, the aggregate generation depends on the extent of switch towards renewable sources of energy a firm makes. Our firm level analysis, presented later in the paper, uncovers these effects.

Columns (2) and (3) focuses on reduction in the use of coal and gas, the two main sources of fossil fuels that the power plants use. Most of the reduction occurred in the coal-based plants. In the post-2008 period, the consumption of coal decreased by a significant 70% in the treated plants. The corresponding decrease in the use of gas is a much lower, and statistically insignificant, 13%. This shows that while the act reduced the generation of electricity from fossil-fueled power plants, much of this reduction came from burning the dirtier fossil fuel, coal.

Column (4) shows that the treated plants pay 38 cents more in the transition period, and 70 cents more post-2008 for each unit of coal as compared to all other plants in the country. As the average coal rate in our sample is $1.92, this increase in the rate is also economically significant.

1.4.3 RGGI and financial outcomes

We begin the analysis of financial outcomes with the effect of RGGI on plant revenue. Figure 1.3 presents the yearly coefficient estimates and the 95% confidence interval of the following regression model:

$$Log(revenue)_{i,t} = Plant_i + QuarterYear_t + \sum_{\tau}(year = \tau) \times \beta_\tau \times Treated_i + \epsilon_{i,t} \quad (1.3)$$

The estimation results provide us with an estimate of the difference in the revenue of treated and control plants every year from 2000 to 2014. We use 2005 as the base year. As shown in the figure, the revenue generated by the two groups is indistinguishable in the pre-2005 period, confirming that the two groups showed parallel trend before the RGGI initiative. Afterwards, the affected plants experienced a large decrease in revenue.

Columns (1) to (3) of Table 1.4 presents the regression result of equation 1.1 with log of revenue as our dependent variable. Since our measure of revenue is estimated based on plant level generation and the rate (i.e., $ per unit of power) at the utility level, we winsorize the revenue at 2.5% in both tails for this analysis to ensure that our findings are not driven by outliers. Our results are not sensitive to these choices.

As shown in the Table, the affected plants' revenue dropped by almost 48% in the post-2008 period compared to all other fossil fuel plants in the country. Compared to plants located only in democratic (deregulated) states, the drop is 46% (51%). Overall, these numbers are consistent with the results documented in the earlier section that shows a large drop in carbon emissions accompanied by a large drop in electricity generation.

Overall, these findings show that the affected plants had lower revenue and higher fuel costs in the post-RGGI period. Their gross profits, defined as revenue minus fuel cost, should come down as a result of lower revenue and higher per unit cost. Columns (4) - (6) of Table 1.4 present the regression result with gross profit as the dependent variable. Gross profit is calculated as the total revenue from electricity sales from the power plant minus total fuel cost of the power plant in that quarter. Similar to revenue numbers, we winsorize the gross profits at 2.5% on both the tails. As over 5% of the observations have negative profits in our sample, we add the absolute minimum value of the gross profit in our sample to each observation before taking the log.[11] The average plant in our sample experienced a decrease in gross profits of around 18% in the interim period, and 70% post-2008. Comparing with the coefficient estimates in columns (1) - (3) that estimates the effect of RGGI on revenue, the coefficient estimates on gross profits are larger in magnitude. This captures the fact that shareholders faced lower revenue with increasing costs as the firm switched to cleaner production.

[11]The minimum value of the profits is around 7% of the average gross profits. So, the interpretation of our estimates needs to account for that. Our results are robust to other specifications.

1.4.4 Causal estimate of the effect of emissions on financial outcomes

Our setting allows us to estimate the causal effect of a unit of carbon emission on plant revenue and profit using a two stage instrumental variable regression framework. We use the passage of the RGGI as an instrument in the first stage regression to get the predicted values of carbon emission. The second stage regression uses the predicted values of emission as the explanatory variable and the revenue or profits as the dependent variable.

Panel A of Table 1.5 presents the regression results for revenue as the dependent variable. For comparison purposes, Column (1) provides the OLS estimate for a regression of plant revenue on carbon emission. Since both the emissions and financial performance measures are based on the log transformed values, our regression coefficients provide us with the elasticity of financial performance to carbon emission. The OLS regression model provides us with an elasticity estimate of 0.95: a one percent increase in emissions increases revenue by 0.95. The corresponding IV estimate is 0.87 for the full sample, and around 0.92 when we restrict the control plants to democratic or deregulated states only. These estimates are similar to the OLS estimate. In sum, the power plant's revenue is highly sensitive to the level of emissions.

Panel B provides the elasticity for gross profits. The OLS estimate is 0.32: for a power plant with average profits, a 1% higher emission corresponds to 0.36% higher profits. However, the IV estimate for the corresponding full sample is a much higher at 0.79, which corresponds to an elasticity of 0.85 for an average power plant. The OLS estimates are likely to be downward bias, due to at least one unobserved factor. A higher ability manager can lower carbon emissions by performing better maintenance or by making better operational decisions. Such managers can also produce more from the same plant. Thus the hidden managerial ability correlates negatively with carbon emission and positively with the output. As a result, the OLS coefficient is likely to be biased downward.

A clear pattern emerges from the analysis. After the RGGI, the treated plants cut their coal-based power generation. As a result, the pollution came down. At the same time, the shareholders lost in terms of revenue and profits. However, the loss of revenue and profits from the fossil fuel plants need not come at the expense of shareholder value. Firms can switch to alternative sources of power, they can become more efficient, and the market may itself value their profits at a higher price than the other firms that have not yet made the switch. To assess the effect of emission control on shareholder value, we next focus on firm level analysis where we can also observe the market value of the firm's equity.

1.4.5 Firm level outcomes

Table 1.6 reports the effects of RGGI on firm-level measures of revenue and profitability. Specifically, the table shows the estimates from the following equation:

$$y_{i,t} = Firm_i + yq_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_i + \beta_2 \times Post2008_t \times Treated_i + \epsilon_{i,t} \quad (1.4)$$

where $Treated_i$ is a dummy that equals 1 if the publicly traded utility is located in the RGGI-state, and 0 otherwise. $\mathbb{1}(2005\text{-}2008)$ takes 1 if year is between 2005 and 2008. $Post2008$ takes 1 if $year > 2008$. The regression model is similar to the plant level regressions except that we now use the accounting measures for the entire firm, that includes revenue and profits from renewable sources of energy.

Columns (1) and (2) of Table 1.6 show that, at the firm level, there is no discernible change in revenue. The affected firms continued to serve their customers in the post-RGGI period. The key difference occurred in the type of power used. Instead of coal fired plants, the treated firms increasingly relied on renewable sources of power such as hydroelectricity. In addition to a switch in their own plants, the affected utilities also began to import hydroelectric power from Canada [12].

To directly assess the extent of switching to renewable sources of energy, we obtain data on renewable energy generation by the investor-owned utilities from EIA Form-923, and calculate the fraction of total electricity generated from renewable sources at the state-quarter level. We estimate the following regression model to assess whether utilities in the treated state increased electricity generation from renewable sources:

$$Renewable_{j,t} = State_j + QtrYear_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_j + \beta_2 \times Post2008_t \times Treated_j + \epsilon_{j,t} \quad (1.5)$$

The dependent variable measures the fraction of total electricity generated by renewable sources in the state j in quarter t by the investor-owned utilities. $Treated_j$ equals one for the RGGI state and zero otherwise. Table **??** presents the regression results. As shown in Column (1) of the Table, after the enactment of the RGGI, the treated states increased the fraction of renewable energy by a significant 8.15%. The estimates are even larger when we compare the treated states with only the other democratic states (11.15%) or deregulated states (15.66%). Figure 1.6 shows that the two groups of states followed a parallel trend before the enactment; it was only after 2008 that the treated states started to increase their renewable generation. Overall, these findings show that in the post-RGGI period, the affected

[12] Northeastern states significantly increased their hydroelctric imports from Hydro-Quebec, Canada. More on this can be found here: https://www.eia.gov/todayinenergy/detail.php?id=17671

states switched to renewable sources of energy that allowed them to protect their revenue despite a significant decline in power generation from fossil fuel plants.

While the firms are able to protect their revenue, it is not clear that their profits also stay the same. As the switch happens towards cleaner power, they are more likely to use expensive coal or incur a higher cost to produce and procure cleaner energy from other sources. They are also likely to incur higher costs in plant maintenance and other operational expenses to produce clean energy. We investigate the impact of clean energy transition on firm profitability measures using the same regression model as in equation 1.4. For earnings and profitability measures, we use the return on earning assets (ROEA) and earnings per share (EPS) as our dependent variables. ROEA is calculated as the operating income after depreciation as a fraction of average total earnings assets (TEA) based on most recent two periods, where TEA is defined as the sum of property, plant and equipment and current assets. We find that the ROEA declined by 2.6% in the interim period and 2.27% post-2008, as reported in columns (3) and (4) of Table 1.6 . This number is both statistically significant and economically large in magnitude as the average firm has a return on earning assets of 7.9%. Columns (5) and (6) show that EPS declined by 54 cents in the interim period, and 64 cents post-2008. As an average firm in our sample has an EPS of 2 dollars, this decline is economically significant.

Overall, the shareholders of power companies in the affected states experienced a decline in profits. We now analyze the changes in two measures of shareholder value - the market-to-book ratio of equity and market-to-book ratio of assets - to tease out the valuation effects. Subfigures (a) and (b) of Figure 1.4 show that the market-to-book ratio of equity and asset values followed a parallel trend before 2005. However, distinct from all our results so far, the treated firms perform better on this measure after the shock. Table 1.7 presents the results using the same regression model as in equation 1.4. We focus our discussion on market-to-book ratio of assets. As shown in Column (4) of Panel A, in the post-2008 period, the treated firms have 5.76% higher market-to-book ratio compared to the control firms that uses the full sample. The corresponding estimates are 3.17% and 4.74%, respectively if we use the sample of democratic states or deregulated states only. During the interim period, the treated firms have lower valuation ratios, but the statistical significance and economic magnitude is sensitive to model specification. For example, the market-to-book ratio for the treated firms is 2.52% lower when we use firms in the deregulated states as the control firms. The estimate is significant at 1%. However, the corresponding estimate is a statistically insignificant -2.44% for the entire sample. Overall, we conclude that the treated firms experienced some decline in market value in the interim period as the firms and market's expectations adjusted to the new regulation. In the long run, however, there is consistent pattern of value increase for the treated firms. Thus, despite a drop in profitability and a stable revenue, shareholders of the treated firms were better off in the post-2008 period.

1.4.6 Sources of value creation

As a firm cuts its carbon emissions, its value can improve either due to an increase in cash flows or due to a decrease in its expected return. As we showed earlier, in the short run, i.e., soon after the implementation of RGGI, the treated firms experienced a decline in their profitability. As these firms adjusted to renewable energy, their current profitability came down. However, if these firms are expected to earn higher cash flows in future, then their market value may still be higher. If power purchasers, for example, show a preference for clean sources of power, then utilities with higher proportion of renewable energy are like to have higher future cash flows. Additionally, if some institutional investors show a preference for green stocks, then the resulting fund flow into the treated stocks can increase their valuation (van der Beck, 2021). Our empirical setting provides an attractive setting to tease out these channels in a causal manner by comparing the treated and control firm's outcomes on these dimensions before and after the RGGI shock.

1.4.7 Investor preference for green companies

Several studies show that some institutional investors prefer green firms, i.e., firms with clean technology (Baker et al., 2022). Krueger, Sautner and Starks (2020) show that institutional investors have started to increasingly care about their portfolio companies' climate risk exposure. During our sample period, which covers a very early period of ESG investing and climate related investing decisions, there was a large increase in the number of mutual funds with a focus towards environment and sustainability performance (see Figure 1.5). Motivated by these findings, we now study whether such funds increased their shareholding in the treated firms as compared to the control firms. As the number of ESG fund-firm pairs before 2007 was too small to detect statistical effects, we limit this analysis to post-2008 as our only treatment period.

We proceed in two steps: first we study the holdings of ESG funds into the treated stocks and then we relate the entry of ESG-funds to the valuation premium for the treated stocks. For the analysis of ESG-holdings, we use two measures of dependent variable. In the first one, the dependent variable is the percentage of a company's shares held by the ESG funds every quarter. Since our sample covers all the utilities, this analysis allows us to focus on whether the ESG funds increased their holding in cleaner utilities as compared to utilities in other parts of the country. The second measure uses the ESG's holding in a firm as a percentage of its total holding as the dependent variable. Therefore, it measures whether the ESG fund increased its holding in the affected utilities on an overall basis in its portfolio. Both models include firm and fund fixed effects, which allows us to capture the effect of the treatment shock independent of fund-specific factors such as managerial style and past performance.

Table 1.8 presents the results. Column (2) shows that after 2008, the ESG funds increased their shareholding by 0.27 percentage point of the total shares outstanding of the treated firms. As a percentage of their own holdings, the ESG funds increased their exposure by 0.07 percentage point in the post-2008 period. These results paint a clear picture. ESG funds moved their portfolio towards cleaner power companies, consistent with the idea of investor preference for such stocks. Such a significant inflow of funds into the treated firms can increase their valuation consistent with the structural model of (van der Beck, 2021).

Next, we directly investigate the effect of the entry of ESG-funds into the financial market on the valuation premium of the treated stocks. Our empirical setting is especially suitable for such a study because this was an early period of ESG-related investing. Several new funds entered the market with specific focus on green stocks. Therefore, we are able to exploit the time-series variation in the number of ESG funds in the market to study its impact on the valuation of treated and control firms. We estimate the following model:

$$mtb_{i,t} = Firm_i + QtrYear_t + \beta \times Treated_i \times Log(no.\ of\ ESG\ funds)_T + \epsilon_{i,t} \tag{1.6}$$

Table 1.9 presents the results: Columns (1)-(3) use the market-to-book ratio of equity as the measure of valuation, whereas Columns (4)-(6) use the market-to-book ratio of firm. Across all six specifications, we find that the utilities in the RGGI states experienced an increase in valuation as the number of ESG funds increased in the market. These results are consistent with the view that part of green premium enjoyed by the treated stocks came from the flow of funds from investors with a preference for cleaner stocks.

1.4.8 Analysts expectation of future cash flows

To assess the effect of carbon transition on market's expectation of future cash flows, we rely on analysts' long term earnings growth forecast. We obtain analyst-firm-quarter level data on the long-term growth rate for all stocks in the sample to estimate the following regression model:

$$g_{i,a,t} = Firm_i + Analyst_a + yq_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_i + \beta_2 \times Post2008_t \times Treated_i + \epsilon_{i,t} \tag{1.7}$$

The regression model is similar to the earlier specification, augmented with the addition of the analyst fixed effects. Therefore, we are able to tease out changes in the expected cash flows of the treated firms compared to the control firms over the transition period, holding fixed the analysts' skill and time-invariant preferences. Results are provided in Table 1.10. Column (1) of the Table shows that the treated firms had 2.236% higher growth forecast in the post-2008 period, which is both economically and statistically significant. In economic terms, the estimates represent 40% of the average value of the long-term growth forecast in our sample. There was no difference in earnings forecast across the two sets of firms during 2005-08 period. These results suggest that the market's expectations about future cash flows of the treated firms changed only after the regulation was fully implemented.

Column (2) controls for the current earnings per share of the firm, and therefore it allows us to assess future expectations accounting for the decline in recent profitability. For example, it allows us to rule out the mechanical mean-reversion effects in earning dynamics. Our estimate remain similar. Columns (3) and (4) repeat the analysis for the democratic and deregulated states alone, and find a similar result.

1.4.9 CEO Compensation and Short-term Profits

Our analysis raises a natural question: if firms benefit from transitioning to greener technology, why did they not engage in such behavior even in the absence of the RGGI? There are at least two possibilities that could explain such reluctance. First, our empirical setting is based on a period when climate awareness and its implication for corporate valuation was in relatively early stages. It is, therefore, possible that some managers were reluctant to transition to greener technology simply due to the uncertainty caused by such a transition.

Another possibility arises from an agency cost perspective. Corporate managers are often compensated based on their company's short-term earnings, creating a preference for technologies that produce immediate cash flows even if it comes at the expense of long term value. We investigate this possibility by analyzing the importance of short-term earnings for the compensation of our sample firms. In Table 1.11, we regress trailing three-year average value of earnings-per-share (EPS) of a utility on the total compensation of its CEO. Column (1) shows that the total compensation strongly depends on the EPS measure of the firm. One standard deviation higher EPS is correlated with 5.12% higher total compensation for the utilities CEOs. Column (2) includes the average market-to-book ratio of the firm over the same time horizon: while EPS continues to explain the variation in total compensation, the market-to-book ratio does not. Similar results hold in Column (3) where we control for the cumulative stock return of the firm. These results show that the compensation level of an utility's CEO is strongly dependent on short term earnings of the firm, providing support to the view that managers may not engage in green transition if their compensation is tied to such measures.

Our results make a broader point. Green transition often comes at the expense of a reduction in short-term profits. Therefore, a key friction in such a transition could be the compensation contract of corporate managers. As shareholders and regulators think hard about encouraging firms to reduce their carbon footprint, it becomes important to pay a closer attention to managerial compensation contracts.

1.5 Robustness tests

1.5.1 Leakage

One of the main concerns with local regulations restricting carbon emissions in a region is the issue of leakage to regions that do not face the same restrictions (Bushnell, Peterman and Wolfram, 2007). As RGGI is state-specific regulation, firms may reduce their carbon emissions from treated states and switch to more pollution from the neighboring states that do not face same restrictions (Yan, 2021; Chan and Morrow, 2019; Fell and Maniloff, 2018).

This, however, is not a major concern for our firm-level analyses. While there are electric utilities that operate power plants in multiple states, there is a minimal overlap between RGGI and non-RGGI states. Out of all publicly traded electric utilities in treated states, only one utility (Constellation Energy) have around 18% of its power generation portfolio outside RGGI states. Similarly, of all the utilities in control states, only one (three) utilities have more than 15% (10%) of their power generation in RGGI states.

To further alleviate the leakage concerns, we run our main firm-level analyses excluding the states neighboring the RGGI states (Virginia, West Virginia, Washington D.C., Pennsylvania, and Ohio). Panel A of Table 1.12 presents results from this analysis. Specifically, it shows estimates from the Equation 1.4 for a sample that excludes firms in these neighboring states. The dependent variables in the first two columns are the profitability measures, ROEA and EPS, and in the next two columns are the valuation measures, market-to-book of equity and assets. The estimates are similar to our baseline results suggesting that the leakage is not a major concern for our empirical analysis.

1.5.2 Renewable Portfolio Standards (RPS)

Another concern with the timing of our empirical setting is the other concurrent changes in state-wide renewable policies. One of the most popular state-wide policy instruments for renewable electricity development in the U.S. has been the Renewable Portfolio Standard (RPS).[13] As many states passed RPS in the beginning of the 2000s, one concern may be that the effects we see are not due to RGGI, but the state-wide RPS policies. This is again not a major concern for us, as both RGGI states and non-RGGI states passed RPS during this time-period. Moreover, several studies, have shown that RPS has not been effective in reducing carbon emissions from states (Upton and Snyder, 2017a).

However, to further address these concerns, we conduct a similar robustness exercise as the previous analysis. Following Carley and Miller (2012), we keep all the states that have any non-voluntary, binding RPS as of 2008. This includes 8 of the 9 RGGI states in our sample, and 19 control states. Similar to the previous analysis, we estimate Equation 1.4 for a sample that only includes firms from these 27 states. Panel B of Table 1.12 reports results from this analysis. Our results hold when we restrict our sample to the states with RPS policies.

1.6 Conclusions

Using a regulatory intervention that limited the ability of power plants located in 10 Northeastern and Mid-Atlantic states to emit carbon, we tease out the causal effect of carbon emission on financial performance. Profits drop as a result of the switch to cleaner technology. However, shareholders benefit in the long run by obtaining higher market valuation. Part of this higher valuation comes from the increased expectation of future cash flows of cleaner utilities and the growth of ESG-related mutual funds that held more electric utilities from the treated states. Our results highlight the trade-off between short-term and long-term profits as a result of carbon transition. Further, we show that markets can play an important role along with regulations in reducing emissions, and shareholder value need not be at odds with societal welfare. Despite a decline in short-term profitability, the cleaner utilities ended up with better valuation due to the entry of institutional investors with green preference. Thus, corporate policies that encourage increased focus on short term profits may be a significant reason behind a firm's reluctance to transition to cleaner technology.

[13] RPS are the state requirement that electric utilities add certain fraction of their electricity from renewable sources by a target year. For an overview of RPS, see Citex).

Figure 1.1: Effect of RGGI on CO_2 emissions

Figure 1.1 presents a brief timeline of some major milestones in the implementation of the Regional Greenhouse Gas Initiative (RGGI).

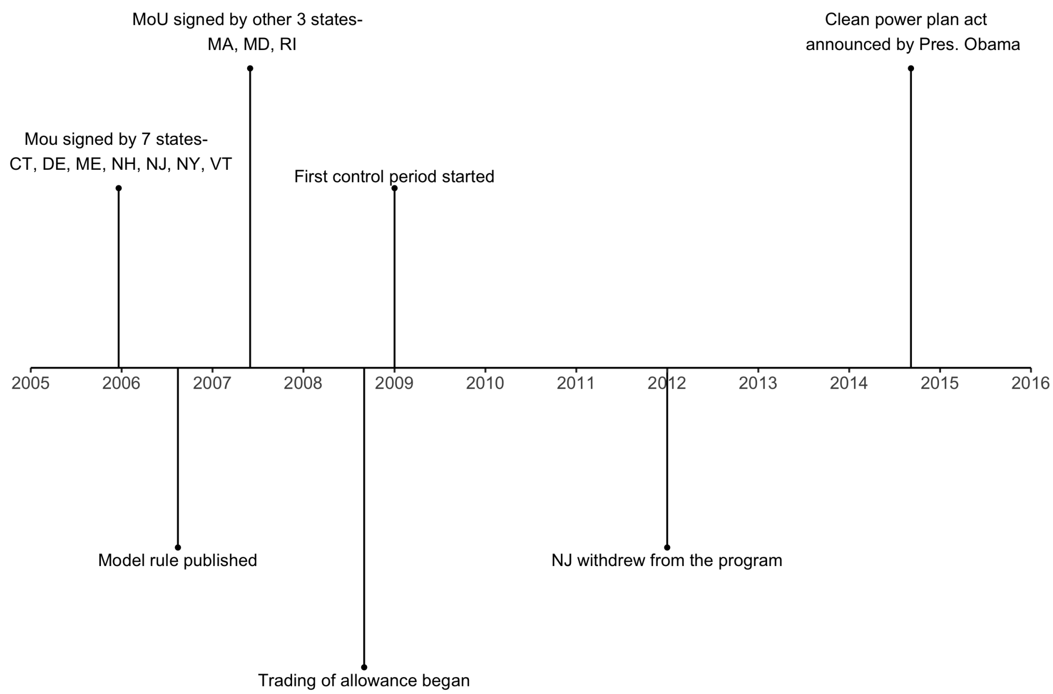


Figure 1.2: Effect of RGGI on CO_2 emissions

Figure 1.2 reports the dynamic differences-in-differences estimates of the effect of RGGI on reducing CO_2 emissions in the treated states. Specifically, the figure shows the point estimates and the 95% confidence intervals of β_τ from the following equation:

$$Log(CO2)_{i,t} = Plant_i + QuarterYear_t + \sum_{\tau}(year = \tau) \times \beta_\tau \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the power plant is located in the RGGI-state, and 0 otherwise. Solid gray vertical line is when the MoU for RGGI was signed; dashed blue vertical line represents the start of the cap-and-trade program. Standard errors are clustered at the state level.

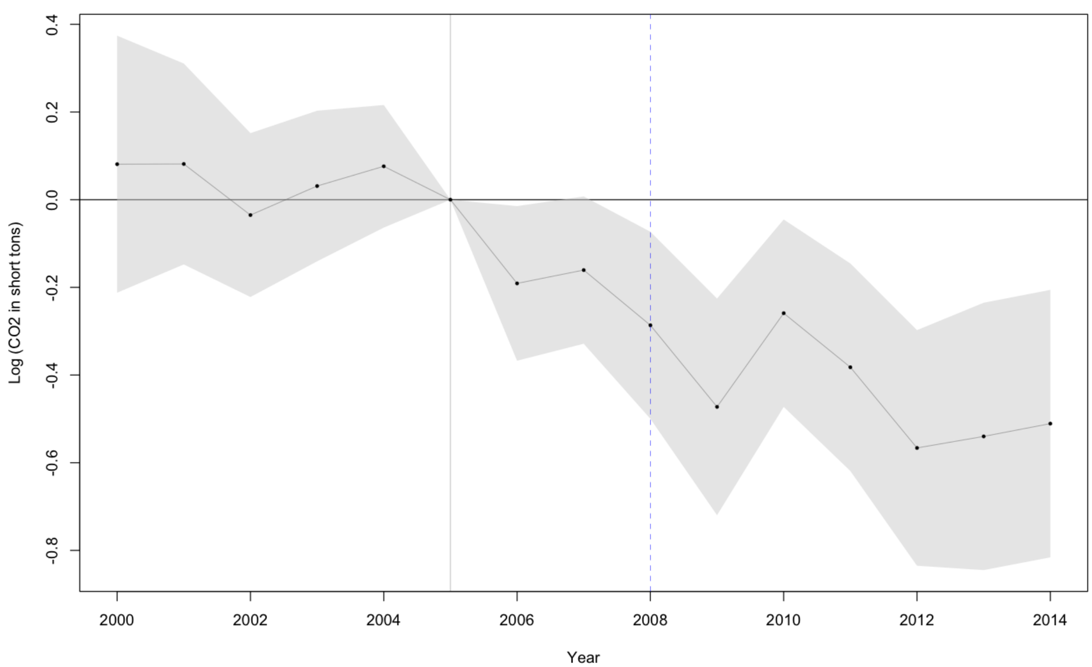


Figure 1.3: Effect of RGGI on power plant revenue

Figure 1.3 reports the dynamic differences-in-differences estimates of the effect of RGGI on revenue and profits in the treated states. Specifically, the figure shows the point estimates and the 95% confidence intervals of β_τ from the following equation:

$$y_{i,t} = Plant_i + QuarterYear_t + \sum_{\tau}(year = \tau) \times \beta_\tau \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. Solid gray vertical line is when the MoU for RGGI was signed; dashed blue vertical line represents the start of the cap-and-trade program. Standard errors are clustered at the state level.

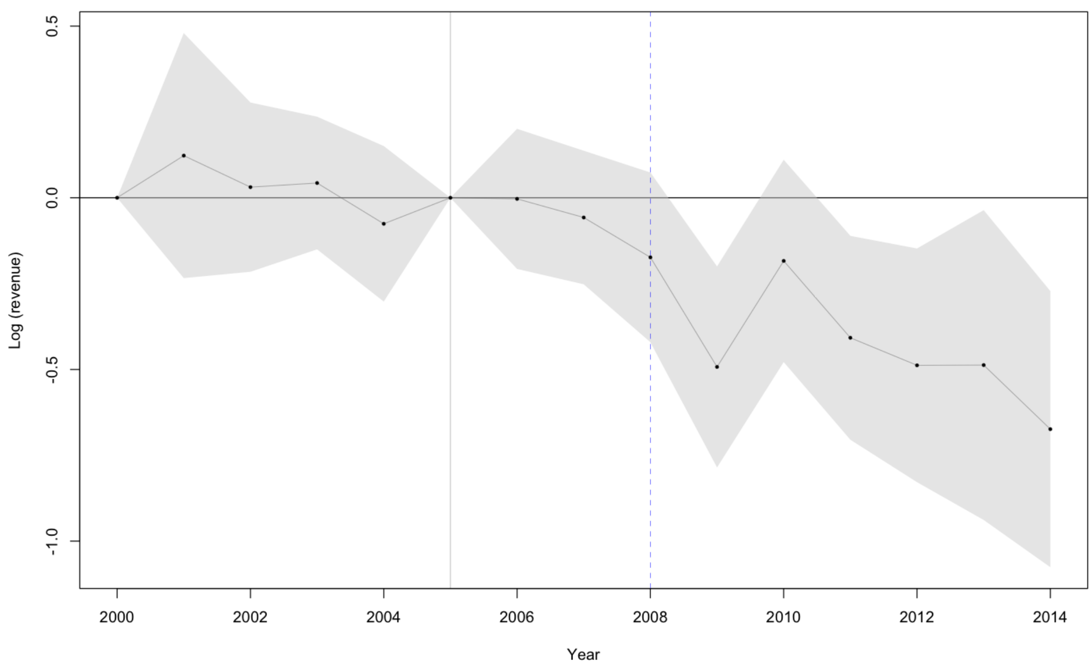


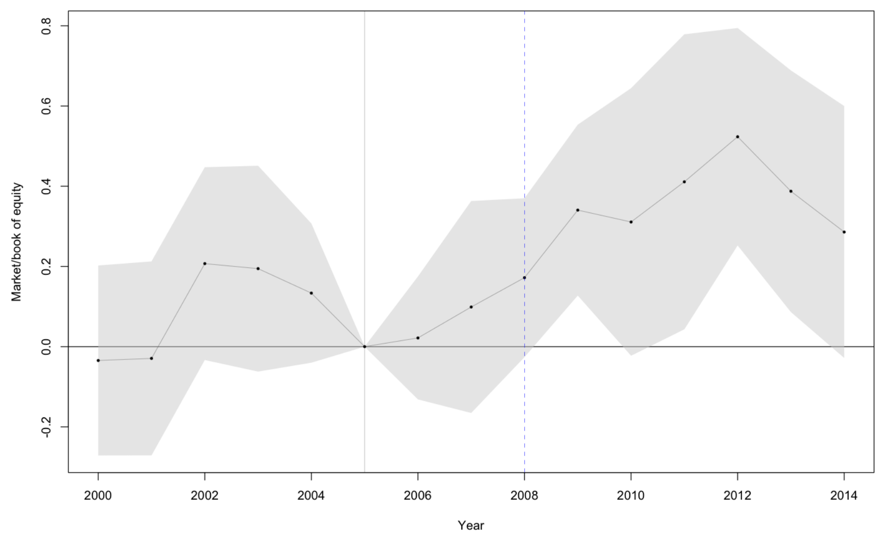
Figure 1.4: Effect of RGGI on Market valuation

Figure 1.4 reports the dynamic differences-in-differences estimates of the effect of RGGI on market valuations in the treated states. Specifically, the figure shows the point estimates and the 95% confidence intervals of β_τ from the following equation:

$$Market/Book_{i,t} = Firm_i + QuarterYear_t + \sum_{\tau}(year = \tau) \times \beta_\tau \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the Firm is located in the RGGI-state, and 0 otherwise. Solid gray line is when the MoU for RGGI was signed; dashed blue line represents the start of the cap-and-trade program. Standard errors are clustered at the firm level.

(a) Market / book of equity



(b) Market / book of assets

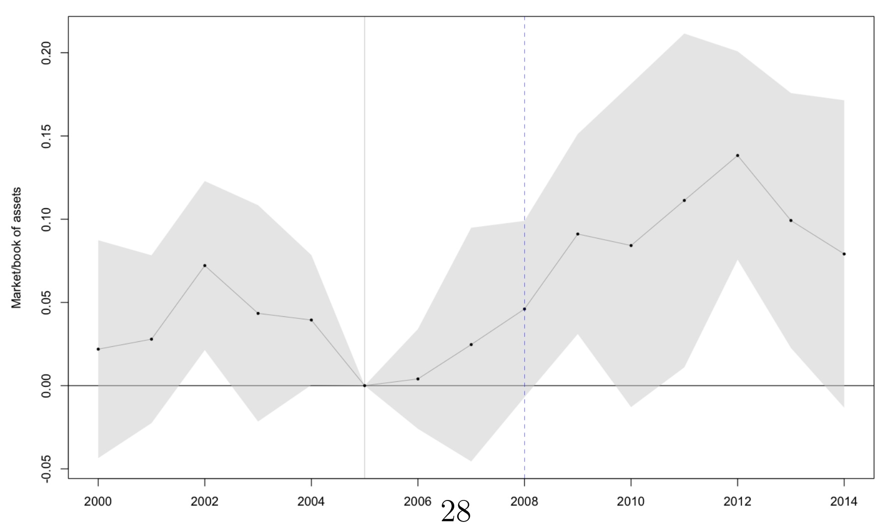


Figure 1.5: Number of ESG funds over year

Figure 1.5 shows the increase in the number of ESG-funds in our sample in each year. There are, on average, 15 ESG funds each year until 2006. This number increases to an average of 50 funds each year from 2007-2014.

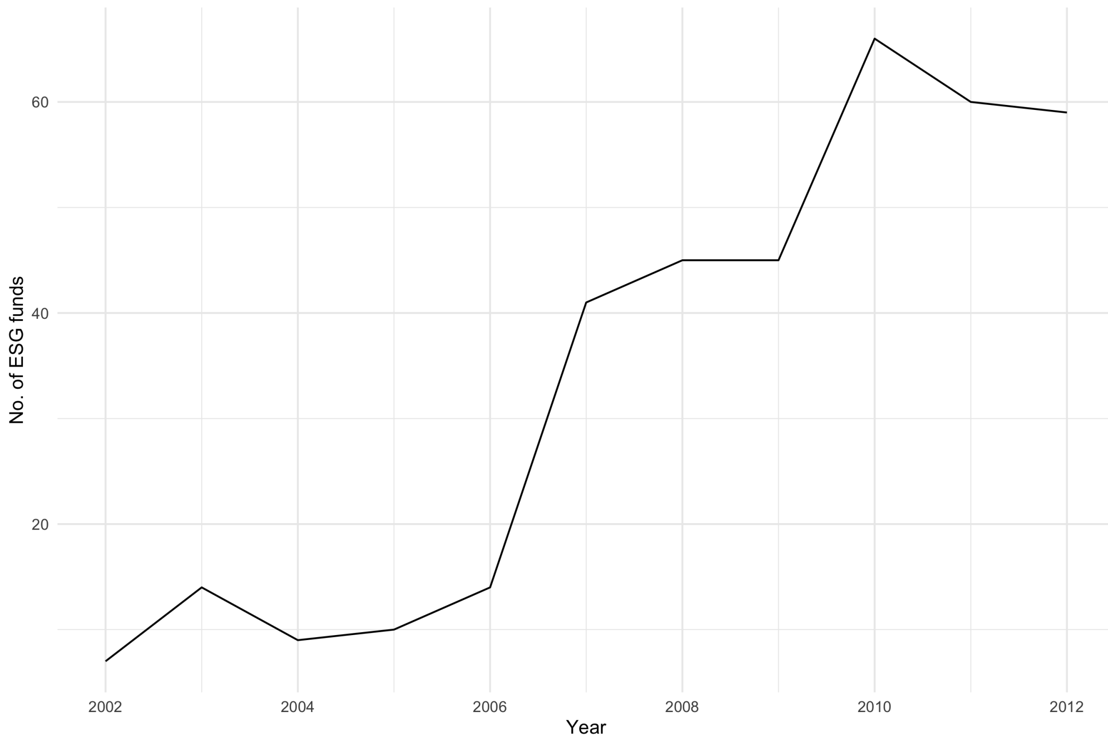


Figure 1.6: Effect of RGGI on state's renewable energy generation

Figure 1.6 reports the dynamic differences-in-differences estimates of the effect of RGGI on market valuations in the treated states. Specifically, the figure shows the point estimates and the 95% confidence intervals of β_τ from the following equation:

$$Renewable_{j,t} = State_j + QtrYear_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_j + \beta_2 \times Post2008_t \times Treated_j + \epsilon_{i,t}$$

where $Treated_j$ is a dummy that equals 1 if the state falls under RGGI, and 0 otherwise. The dependent variable, $Renewable_{j,t}$, is the fraction of total electricity generated by renewable sources in the state j in quarter t by the investor-owned utilities. Solid gray line is when the MoU for RGGI was signed; dashed blue line represents the start of the cap-and-trade program. Standard errors are clustered at the firm level.

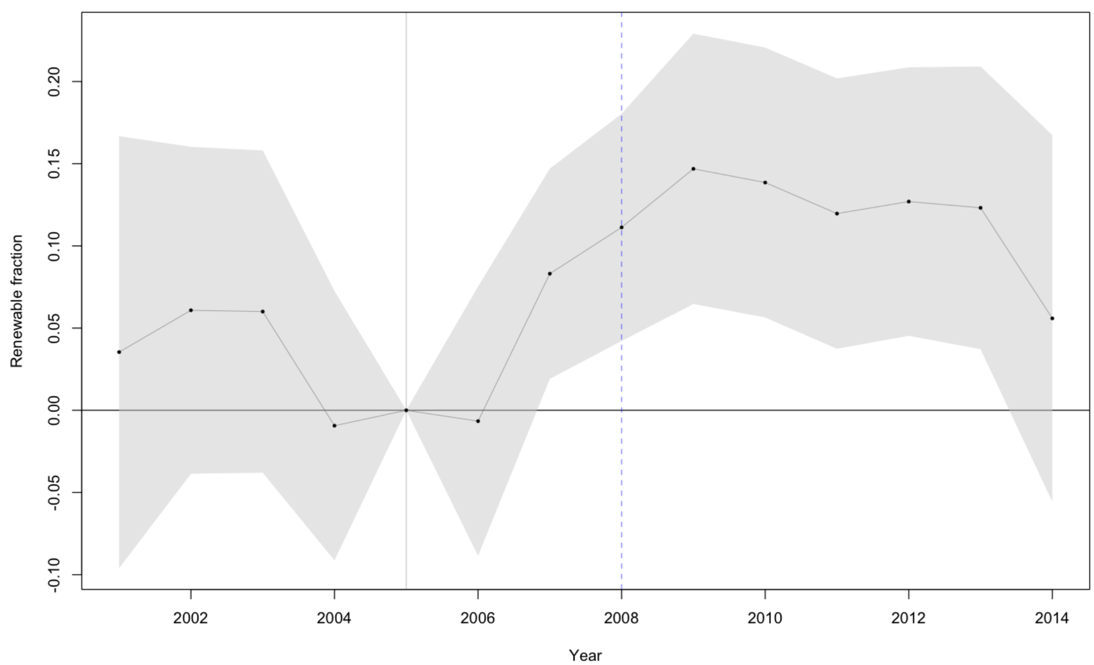


Table 1.1: Summary Statistics

Table 1.1 reports the descriptive statistics of the variables used in this paper. Panel A presents characteristics at the power plant- quarter level. CO_2 emissions data come from the U.S. Environmental Protection Agency (EPA). Power plant level operational data are from the U.S. Energy Information Administration (EIA). Plant-level gross profits are calculated as total revenue minus fuel costs, and is calculated only for plants that report fuel costs.

Panel B presents summary statistics of the profitability and valuation ratios used in the paper at the firm-quarter level. This data comes from Compustat/CRSP database provided by the Wharton Research Data Services (WRDS).

Panel A: Plant characteristics

Variable	N	Mean	SD	P25	Median	P75
Co2 (1000 short tons)	46344	592.1	909.3	14.4	183.1	739.8
Fuel used (billion BTUs)	43775	6252.1	8777.8	255.0	2504.1	8321.6
Coal used (billion BTUs)	43775	4607.4	8815.8	0.0	0.0	5019.1
Natuaral gas used (billion BTUs)	43775	1391.5	2600.6	0.0	88.3	1384.3
Net generation (GWH)	43775	639.1	886.5	21.5	247.1	883.6
Net generation- coal (GWH)	43775	446.1	866.9	0.0	0.0	468.5
Net generation- natural gas (GWH)	43775	165.5	328.4	0.0	7.3	137.4
Revenue (in million dollars)	36628	52.7	72.5	1.8	19.7	73.3
Gross profit (in million dollars)	17100	49.5	61.7	3.1	25.3	72.6

Panel B: Company valuations and profitability measures

Variable	N	Mean	SD	P25	Median	P75
Quarterly revenue (million dollars)	3580	1417.51	1454.53	302.95	856.00	2291.19
Market value of equity (billion dollars)	3577	6.57	8.20	1.28	3.30	8.77
Book value of equity (billion dollars)	3590	4.04	4.68	0.88	2.31	5.80
Earning assets (billion dollars)	3455	11.89	12.67	2.60	7.46	17.13
Return on earning assets (%)	3514	7.89	3.21	6.50	7.70	9.30
Earnings per share (EPS)	3542	2.05	1.86	1.29	2.01	2.84
Market / book of equity	3575	1.59	0.59	1.25	1.50	1.81
Market / book of assets	3575	1.15	0.14	1.06	1.13	1.22
Analysts long-term growth forecast	3638	5.60	5.32	3.70	5	7.50

Table 1.2: Effect of RGGI on CO_2 emissions

Table 1.2 reports the differences-in-differences estimates of the effect of RGGI on reducing CO_2 emissions in the treated states. Specifically, the table shows the estimates from the following equation:

$$log(CO2)_{i,t} = Plant_i + QtrYear_t + \beta \times Treated_i \times Post_t + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. In column 1, $Post_t = 1$ if $year > 2005$ and 0 otherwise. In columns (2) and (3), the variable $Post_t$ is divided into two time dummies: Yr(2005-2008) that takes 1 if year is between 2005 and 2008 and $Post$2008 that takes 1 if $year > 2008$. The dependent variable is log of CO_2 emissions (in short tons).

Columns (1)-(3) show results from the full sample. Column (3) also includes log of net generation of electricity (in MWh) as a control. Column (4) presents results from matched subsamples of power plants in states that voted Democrats in the presidential election of 2008. Column (5) is subsample of plants in the states with deregulated electricity markets. Standard errors are clustered at the state level; t-stats are shown in parentheses.

	Log (CO2 in short tons)				
		Full Sample		Democrat	Deregulated
	(1)	(2)	(3)	(4)	(5)
Post 2005*Treated	-0.4030***				
	(-4.115)				
Yr(2005-2008)*Treated		-0.2050**	-0.1469***	-0.2218**	-0.1844
		(-2.630)	(-3.157)	(-2.303)	(-1.493)
Post 2008*Treated		-0.4961***	-0.1548**	-0.4828***	-0.4886***
		(-4.205)	(-2.655)	(-3.756)	(-3.519)
Log (net gen, MWh)			0.7572***		
			(40.42)		
Plant ID FEs	✓	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓	✓
Observations	44,148	44,148	41,732	27,414	21,345
R^2	0.85	0.85	0.96	0.86	0.85

Clustered (State) co-variance matrix, t-stats in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.3: Effect of RGGI on real decisions

Table 1.3 reports the effects of RGGI on real decisions firms made. The table shows the estimates from the following equation:

$$y_{i,t} = Plant_i + QtrYear_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_i + \beta_2 \times Post2008_t \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. $\mathbb{1}(2005\text{-}2008)$ takes 1 if year is between 2005 and 2008. $Post2008$ takes 1 if $year > 2008$. The dependent variables are log of net generation (in MWh) in column (1), log of coal consumed (in MmBtu) in column (2), log of total natural gas consumed (in MmBtu) in column (3), and coal rate (in \$/MmBtu) in column (4).

Standard errors are clustered at the state level; t-stats are shown in parentheses.

	Log (net gen, MWh)	Log (coal used, MmBtu)	Log (gas used, MmBtu)	Coal rate (dollars/MmBtu)
	(1)	(2)	(3)	(4)
Yr(2005-2008)*Treated	-0.2275***	-0.0400	-0.1663*	0.3880***
	(-3.655)	(-0.6691)	(-1.873)	(5.838)
Post 2008*Treated	-0.5258***	-0.6943***	-0.1347	0.7016***
	(-4.749)	(-4.425)	(-0.8528)	(6.609)
Plant ID FEs	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓
Observations	43,775	16,222	32,360	10,665
R^2	0.83	0.85	0.78	0.82

Clustered (State) co-variance matrix, t-stats in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.4: Effect of RGGI on financial outcomes

Table 1.4 reports the differences-in-differences estimates of the effect of RGGI on the power plant revenue and profits in the treated states. Specifically, the table shows the estimates from the following equation:

$$y_{i,t} = Plant_i + QtrYear_t + \beta_1 \times \mathbb{1}(\text{2005-2008})_t \times Treated_i + \beta_2 \times Post2008_t \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. $\mathbb{1}$(2005-2008) takes 1 if year is between 2005 and 2008. $Post2008$ takes 1 if $year > 2008$. The dependent variables are the log of total revenue from electricity sales in columns (1)-(3) and log of gross profit, calculated as total revenue minus fuel costs, in columns (4)-(6).

Columns (1) and (4) show results from the full sample. Columns (2) and (5) present results from matched subsamples of power plants in states that voted Democrats in the presidential election of 2008. Columns (3) and (6) are subsamples of plants in the states with deregulated electricity markets. Standard errors are clustered at the state level; t-stats are shown in parentheses.

	Log (revenue)			Log(profits)		
	Full Sample	Democrat	Deregulated	Full Sample	Democrat	Deregulated
	(1)	(2)	(3)	(4)	(5)	(6)
Yr(2005-2008)*Treated	-0.0824	-0.0613	-0.0531	-0.1759***	-0.1998***	-0.1414
	(-0.9445)	(-0.5539)	(-0.3223)	(-3.532)	(-3.547)	(-1.619)
Post 2008*Treated	-0.4754***	-0.4633***	-0.5133**	-0.6451***	-0.6208***	-0.7158***
	(-3.163)	(-2.803)	(-2.563)	(-4.633)	(-4.340)	(-4.030)
Plant ID FEs	✓	✓	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓	✓	✓
Observations	36,628	24,708	16,441	16,672	8,879	3,654
R^2	0.83	0.83	0.83	0.86	0.85	0.84

Clustered (State) co-variance matrix, t-stats in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.5: Causal effect of CO_2 emissions on revenue and profits

Table 1.5 reports the impact of CO_2 emissions on plant's revenue and gross profit. The dependent variable is $log(revenue)$ in Panel A and $log(gross\ profit + c)$ in Panel B. In each panel, column (1) shows the estimate from the OLS regression. Columns (2), (3), and (4) show the instrumental variables estimates of the following $2nd$ stage:

$$y_{it} = \alpha_i + \gamma_t + \beta_{IV} log(\hat{CO2}_{it}) + \epsilon_{it}$$

where $log(\hat{CO2}_{it})$ is estimated using the following first-stage:

$$log(\hat{CO2}_{i,t}) = Plant_i + QtrYear_t + \hat{\beta}_1 \times \mathbb{1}(\text{2005-2008}) \times Treated_i + \hat{\beta}_2 \times Post2008_t \times Treated_i$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. $\mathbb{1}$(2005-2008) takes 1 if year is between 2005 and 2008. $Post2008$ takes 1 if $year > 2008$.

Columns (1) and (2) in both the panels show results from the full sample. Column (3) is the subsample of plants in states that voted Democrats in the presidential election of 2008. Column (4) is the subsample of plants in the states with deregulated electricity markets. Standard errors are clustered at the state level; t-stats are shown in parentheses.

Panel A: Revenue

	Log (revenue)			
	Full Sample		Democrat	Deregulated
	OLS	Instrumental Variables		
	(1)	(2)	(3)	(4)
Log (CO2 in short tons)	0.9510***	0.8720***	0.9200***	0.9182***
	(43.62)	(5.576)	(5.518)	(5.289)
Plant ID FEs	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓
Observations	34,815	34,815	23,115	15,171
R^2	0.95	0.95	0.94	0.94

Clustered (State) co-variance matrix, t-stats in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Panel B: Profits

	Log(profits)			
	Full Sample		Democrat	Deregulated
	OLS	Instrumental Variables		
	(1)	(2)	(3)	(4)
Log (CO2 in short tons)	0.3248***	0.7937***	0.7719***	0.8489***
	(13.94)	(3.452)	(3.254)	(3.517)
Plant ID FEs	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓
Observations	16,382	16,382	8,663	3,600
R^2	0.88	0.82	0.84	0.79

Clustered (State) co-variance matrix, t-stats in parentheses

Table 1.6: Effect of RGGI on firm revenue and profits

Table 1.6 reports the effects of RGGI on firm's revenue and profitability measures. Specifically, the table shows the estimates from the following equation:

$$y_{i,t} = Firm_i + QtrYear_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_i + \beta_2 \times Post2008_t \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. $\mathbb{1}$(2005-2008) takes 1 if year is between 2005 and 2008. $Post2008$ takes 1 if $year > 2008$. The dependent variables are log (revenue) in columns (1) and (2), return on earnings assets (ROEA) in % in columns (3) and (4), and earnings-per-share (EPS) in columns (5) and (6). ROEA is calculated as the operating income after depreciation as a fraction of average total earnings assets (TEA) based on most recent two periods, where TEA is defined as the sum of property, plant and equipment and current assets. Columns (2), (4), and (6) also include log of total assets as a control for the size of the firm.

Standard errors are clustered at the firm level; t-stats are shown in parentheses.

	Log (revenue)		ROEA		EPS	
	(1)	(2)	(3)	(4)	(5)	(6)
Yr(2005-2008)*Treated	0.0432	0.0538	-2.636***	-2.646***	-0.5400**	-0.5409**
	(0.3926)	(0.6622)	(-3.323)	(-3.251)	(-2.009)	(-1.998)
Post 2008*Treated	0.0672	0.0044	-2.269***	-2.176***	-0.6423*	-0.6369*
	(0.6677)	(0.0468)	(-2.956)	(-2.797)	(-1.808)	(-1.773)
Log (assets)		0.8285***		-1.256		-0.0725
		(12.79)		(-1.648)		(-0.3370)
Firm ID FEs	✓	✓	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓	✓	✓
Observations	3,579	3,577	3,514	3,512	3,542	3,540
R^2	0.95	0.97	0.48	0.49	0.44	0.44

Clustered (Firm ID) co-variance matrix, t-stats in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.7: Effect of RGGI on firm valuation measures

Table 1.7 reports the effects of RGGI on firm valuation measures. The dependent variables are market to book of equity in columns (1) and (2) of Panel A, and (1) and (3) of Panel B, and market to book of assets in (3) and (4) of Panel A and (2) and (4) of Panel B. Panel A shows results from the full sample. Panel B presents results from the states that voted Democrats in the presidential election of 2008 and the states with deregulated electricity markets. Standard errors are clustered at the firm level; t-stats are shown in parentheses.

Panel A: Full Sample

	Market/book of equity		Market/book of assets	
	(1)	(2)	(3)	(4)
Yr(2005-2008)*Treated	-0.0337	-0.0378	-0.0232	-0.0244
	(-0.3105)	(-0.3454)	(-0.9228)	(-0.9758)
Post 2008*Treated	0.2267**	0.2593***	0.0485*	0.0576**
	(2.246)	(2.832)	(1.689)	(2.186)
Log (assets)		-0.4165***		-0.1166***
		(-3.207)		(-3.420)
Firm ID FEs	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓
Observations	3,575	3,575	3,575	3,575
R^2	0.61	0.63	0.61	0.64

One-way (Firm ID) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Panel B: Matched Samples

	Deregulated only		Democrats only	
	M/B of equity	M/B of assets	M/B of equity	M/B of assets
	(1)	(2)	(3)	(4)
Yr(2005-2008)*Treated	-0.2874***	-0.1028***	-0.0222	-0.0252***
	(-3.485)	(-6.786)	(-0.4353)	(-2.868)
Post 2008*Treated	0.3024***	0.0317**	0.2458***	0.0474***
	(4.404)	(2.341)	(6.228)	(5.690)
Log (assets)	-0.4785***	-0.0963***	-0.2900***	-0.0904***
	(-6.426)	(-5.008)	(-5.741)	(-6.341)
Firm ID FEs	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓
Observations	1,624	1,624	2,442	2,442
R^2	0.66	0.66	0.65	0.68

One-way (Firm ID) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.8: Effect of RGGI on ESG fund holdings

Table 1.8 reports the effects of RGGI on ESG-mutual funds' holdings of the electric utilities in a differences-in-differences setting. Specifically, the table shows the estimates from the following equation:

$$y_{i,j,t} = Firm_i + ESG\ Fund_j + QtrYear_t + \beta \times Post2008_t \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the firm is located in the RGGI-state, and 0 otherwise. $Post2008$ takes 1 if $year > 2008$ and 0 otherwise.

The dependent variable in column (1) is log of total market value of $Firm_i$ that is owned by ESG fund. The dependent variable in (2) is % ownership of the firm by ESG mutual funds. In column (3), the dependent variables is the % of total net assets of $ESG\ Fund_j$ invested in $Firm_i$. Standard errors are clustered at the firm level; t-stats are shown in parentheses.

	Log (value of shares) (1)	% ownership of firm (2)	% of fund total net assets (3)
Post 2008*Treated	0.5403***	0.2685**	0.0691**
	(3.489)	(2.041)	(2.264)
Firm ID FEs	✓	✓	✓
Fund ID FEs	✓	✓	✓
Quarter-year FEs	✓	✓	✓
Observations	8,339	8,337	8,340
R^2	0.85	0.70	0.81

One-way (Firm ID) co-variance matrix, t-stats in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.9: Effect of number of ESG-funds on treated firm's Valuations

Table 1.9 reports the effects of the number of ESG funds on market valuations. Specifically, the table shows the estimates from the following equation:

$$y_{i,t} = Firm_i + QtrYear_t + \beta \times Treated_i \times Log(no.\ of\ ESG\ funds)_T + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. $Log(no.\ of\ ESG\ funds)_T$ is the log of total number of ESG-funds holding electric utilities companies in the year T. The dependent variables are Market to book of equity in Columns (1)-(3), and market to book of assets in Columns (4)-(6).

Columns (1) and (4) show results from the full sample. Columns (2) and (5) present results from subsamples of firms in states that voted Democrats in the presidential election of 2008. Columns (3) and (6) are the subsample of firms in the states with deregulated electricity markets.

The sample is restricted to the year 2005 and after to isolate the differential effects of ESG-investments on the treated states' valuations. Standard errors are clustered at the firm level; t-stats are shown in parentheses.

	Market/book of equity			Market/book of assets		
	Full	Democrat	Deregulated	Full	Democrat	Deregulated
	(1)	(2)	(3)	(4)	(5)	(6)
RGGI states ×	0.2007**	0.1930**	0.2782**	0.0547**	0.0504*	0.0561*
Log (no. of ESG funds)	(2.357)	(2.099)	(2.345)	(2.190)	(1.901)	(1.755)
Firm ID FEs	✓	✓	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓	✓	✓
Observations	2,238	1,497	982	2,238	1,497	982
R^2	0.73	0.68	0.74	0.70	0.69	0.69

Clustered (Firm ID) co-variance matrix, t-stats in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.10: Effect of RGGI on firm earnings growth forecasts

Table 1.10 reports the effects of RGGI on analysts' long term growth forecasts of earnings. Specifically, the table shows the estimates from the following equation:

$$y_{i,t} = Firm_i + Analyst_a + QtrYear_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_i + \beta_2 \times Post2008_t \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. $\mathbb{1}$(2005-2008) takes 1 if year is between 2005 and 2008. $Post2008$ takes 1 if $year > 2008$. The dependent variable is the long term growth forecasts by analysts.

Columns (1) and (2) show results from the full sample. Column (3) presents results from subsamples of firms in states that voted Democrats in the presidential election of 2008. Column (4) is the subsample of firms in the states with deregulated electricity markets. Columns (2), (3), and (4) also include log of total assets as a control for the size of the firm. Standard errors are clustered at the firm level; t-stats are shown in parentheses.

	LTG forecast			
	Full Sample		Democrat	Deregulated
	(1)	(2)	(3)	(4)
Yr(2005-2008)*Treated	0.4265	0.4028	0.1739	-0.2531
	(0.2812)	(0.2785)	(0.1116)	(-0.1609)
Post 2008*Treated	2.236**	2.171**	2.067**	3.049**
	(2.459)	(2.374)	(2.065)	(2.758)
EPS		0.1932*	0.2823***	0.2431**
		(1.940)	(3.170)	(2.591)
CUSIP ID FEs	✓	✓	✓	✓
Analyst FEs	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓
Observations	3,638	3,630	2,598	2,126
R^2	0.40	0.40	0.42	0.46

Clustered (CUSIP ID) co-variance matrix, t-stats in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.11: Effect of EPS on executive compensation

Table 1.11 reports the effects of the average EPS on executive compensation of electric utilities. Specifically, the table shows the estimates from the following equation:

$$log(Compensation)_{i,t} = Firm_i + \beta \times EPS_{i,t} + Controls_{i,t} + \epsilon_{i,t}$$

The variable $EPS_{i,t}$ is the average EPS of firm i over the last 3 years from the year t. Column (1) presents results without any controls. Column (2) includes average market-to-book ratio of the last 3 years of the firm as a control. Column (3) has cumulative stock return over the last 3 years of the utilities as the control.

The sample includes all executives of electric utilities from 1995 to 2014 that are reported in Execucomp. Standard errors are clustered at the firm level; t-stats are shown in parentheses.

	Log (total compensation)		
	(1)	(2)	(3)
Avg EPS (last 3 yrs)	0.0512**	0.0515**	0.0519**
	(2.174)	(2.222)	(2.435)
Avg MTB (last 3 yrs)		-0.0096	
		(-0.1873)	
Cum. return (last 3 yrs)			0.0004
			(0.1187)
Firm ID FEs	✓	✓	✓
Observations	5,773	5,752	5,483
R^2	0.27	0.27	0.29

Clustered (Firm ID) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.12: Effect of RGGI on firm profitability and valuation measures

Table 1.12 reports robustness exercise for main specifications. Specifically, the table shows the estimates from the following equation:

$$y_{i,t} = Firm_i + QtrYear_t + \beta_1 \times \mathbb{1}(2005\text{-}2008)_t \times Treated_i + \beta_2 \times Post2008_t \times Treated_i + \epsilon_{i,t}$$

where $Treated_i$ is a dummy that equals 1 if the plant is located in the RGGI-state, and 0 otherwise. $\mathbb{1}$(2005-2008) takes 1 if year is between 2005 and 2008. $Post2008$ takes 1 if $year > 2008$. The dependent variables are return on earnings assets (ROEA), earnings-per-share (EPS), market-to-book of assets, and market-to-book of equity in columns (1)-(4) respectively.

Panel A excludes neighboring states of Pennsylvania, Ohio, Virginia, West Virginia, and Washington DC from the full sample. Panel B only keeps all states that have non-voluntary, binding Renewable Portfolio Standards (RPS) as of 2008. Standard errors are clustered at the firm level; t-stats are shown in parentheses.

Panel A: Sample excludes neighboring states

	ROEA	EPS	Market/book of assets	Market/book of equity
	(1)	(2)	(3)	(4)
Yr(2005-2008)*Treated	-2.630***	-0.5313*	-0.0138	-0.0052
	(-3.204)	(-1.849)	(-0.5387)	(-0.0470)
Post 2008*Treated	-2.278***	-0.7865*	0.0660**	0.2709***
	(-2.907)	(-1.967)	(2.381)	(2.823)
Log (assets)	-1.280	0.1064	-0.1015***	-0.3645***
	(-1.513)	(0.4342)	(-3.124)	(-2.968)
Firm ID FEs	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓
Observations	3,067	3,246	3,282	3,282
R^2	0.47	0.45	0.63	0.61

Clustered (Firm ID) co-variance matrix, t-stats in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Panel B: Sample includes states with RPS

	ROEA	EPS	Market/book of assets	Market/book of equity
	(1)	(2)	(3)	(4)
Yr(2005-2008)*Treated	-2.904***	-0.7098**	-0.0412	-0.0646
	(-3.212)	(-2.276)	(-1.435)	(-0.5165)
Post 2008*Treated	-2.154**	-0.8193*	0.0624**	0.3068***
	(-2.440)	(-1.872)	(2.070)	(2.904)
Log (assets)	-0.9831	0.1063	-0.1047***	-0.4043***
	(-1.229)	(0.4211)	(-3.132)	(-3.156)
Firm ID FEs	✓	✓	✓	✓
Quarter-year FEs	✓	✓	✓	✓
Observations	2,678	2,849	2,893	2,893
R^2	0.50	0.40	0.66	0.65

CHAPTER 2

Getting Dirty Before You Get Clean: Institutional Investment in Fossil Fuels and the Green Transition

2.1 Introduction

Institutional investments in fossil fuel companies through private equity (PE) funds have grown significantly in the last decade, with over a trillion dollars invested since 2010.[1] Consequently, there is a growing concern that PE investment in fossil fuels delays the green transition by prolonging the lives of dirty assets.[2] This concern has led to mounting pressure on pension funds and asset managers to divest their fossil fuel holdings, not only from their public equity investments but also from private equity.[3]

Contrary to these concerns, I show that PE investments in fossil fuels may *facilitate* the green transition by allowing new, clean technologies to develop. The key insight is that old, dirty assets often provide opportunities for the development of new, clean technologies, and private equity can better realize these investment opportunities than other incumbent owners. I show in this paper that PE acquisition of fossil fuel power plants is followed by an increase in solar development in the area, especially from other institutional investors related to the PE owners of fossil plants. Moreover, PE firms are more likely to own fossil fuel plants that provide exogenously higher investment opportunities for solar development. To my knowledge, my paper is the first to establish this link between private equity investment in fossil fuels and the development of clean energy.[4]

[1]See this PE Stakeholder Project report titled 'Private Equity Propels the Climate Crisis.'

[2]See this article in The Economist titled 'Who buys the dirty energy assets public companies no longer want?' on media concerns related to PE investments in fossil fuels.

[3]New York City became one of the first cities to announce divestment of the future private equity holdings of its major pension funds from fossil fuels in April 2023. See this news report.

[4]While a growing literature studies PE investments (Andonov and Rauh, 2022; Shive and Forster, 2020;

Old, polluting assets often come with resources valuable for new, clean technologies. For example, fossil fuel power plants have access to grid infrastructure and the right to interconnect and transmit electricity. This resource is critical to the development of clean energy sources such as solar, which faces significant costs, delays, and regulatory hassles related to the interconnection of a new plant to the grid (Rand et al., 2023).[5] Solar development also requires several agreements and concessions with power purchasers, regulators, landowners, and the local community (Jarvis, 2021).[6] A fossil plant owner has relationships with these stakeholders and can help overcome these barriers for a new solar developer. Therefore, these fossil assets provide synergies for the development of future solar power plants.

Fossil plant owners and solar developers can contract with each other to jointly realize these opportunities. However, not all owners of fossil fuel plants have equal incentives and abilities to form such partnerships. First, publicly traded owners of these assets, such as electric utilities and independent power producers, face significant regulatory and public pressure not to own dirty assets (van Benthem et al., 2022). Moreover, the current ESG metrics, which are based on existing assets-in-place (Hartzmark and Shue, 2023), do not incentivize public utilities to partner with other solar developers since such partnerships do not affect the 'greenness' of the assets owned by utilities themselves. While large public firms lack incentives, small privately-held power producers lack the ability to realize these growth opportunities (Gilje and Taillard, 2016; Phillips and Sertsios, 2016) due to the lack of access to capital and relationships with other investors (Petersen and Rajan, 1994a; Bharath et al., 2009). Moreover, both traditional power plant owners face significant abandonment costs and legal liabilities associated with early plant retirements (Atanasova and Schwartz, 2019).

Private equity firms, which are increasingly important owners of power generation assets (Andonov and Rauh, 2022), are relatively less susceptible to these frictions. They have fewer disclosure requirements, face less regulatory and public pressure from owning dirty assets, and do not rely on public ESG ratings for the valuations of their portfolio companies (Bernstein, 2022). They are also better at renegotiating contracts (Shleifer and Summers, 1988) and avoiding future liabilities associated with plant retirements.[7] Moreover, PE firms have repeated interactions with other capital providers and institutional investors (Demiroglu and James, 2010; Ivashina and Kovner, 2011; Malenko and Malenko, 2015), which may ease the flow of information between investors and make contracting easier. Therefore, PE

Bellon, 2020) in and corporate divestment (Duchin, Gao and Xu, 2022) from polluting assets, it has focused on their effects on the polluting assets, not on the clean energy development.

[5]See this NYT article: "In a Twist, Old Coal Plants Help Deliver Renewable Power. Here's How."

[6]See this NYT article: "As Demand for Green Energy Grows, Solar Farms Face Local Resistance."

[7]PE firms often take minority stakes in a firm through club-deals and avoid being a 'trade or business' for liabilities purposes. See this court ruling on Sun Capital Partners v New England Teamsters Pension Fund.

firms may be better suited to realize these opportunities together with other investors who specialize in developing solar.

If fossil fuel assets provide solar development opportunities, and if PE firms are better able to realize these opportunities, we should expect a) solar development to increase in areas with PE-owned fossil plants, and b) fossil plants that offer more investment opportunities for solar to be reallocated to private equity. These are the key hypotheses I test and find support for in this paper.

Using a staggered difference-in-differences design, I find that the PE acquisition of a fossil power plant in a county leads to an 8% increase in the likelihood of new solar development in that county in the next five years, relative to other counties with fossil-fuel power plants. This increase is economically significant as only 37% of all counties have any solar development in my sample. On an intensive margin, I also find an additional 11% increase in the number of solar plants installed in that county and a 40% increase in new solar power capacity. These results hold after controlling for fossil fuel generation in these counties and county-level population to account for any time-varying changes in the overall energy demand in the area. My results also hold after including regulated-state-by-year fixed effects that control for varying time trends in deregulated and regulated states. This suggests that the results are not driven by deregulation in the electricity market, which is a key factor behind the ownership changes in the power sector (Andonov and Rauh, 2022). The results are also robust to new staggered difference-in-differences methodologies (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021).

Do PE firms acquire fossil fuel plants with the intention to exploit the solar development opportunity in the future? Establishing a causal link between PE investment in fossil fuels and solar development is challenging for two primary reasons: (i) it is difficult to measure the investment opportunity set, and (ii) the opportunity to develop solar plants may be correlated with other unobserved factors that also concurrently affect the PE's motivation to acquire the fossil fuel plant. For example, a state-wide renewable policy will affect both the potential for solar development and the generation prospects from existing fossil fuel plants.

I overcome these challenges by exploiting a unique technical feature of the solar power industry. Solar generation in a region depends on the intensity of sunlight that falls on an area, which is commonly measured as the Global Horizontal Irradiance (GHI). The National Renewable Energy Laboratory (NREL) provides hourly measures of GHI in segments of 4 km by 4 km (around 2.5 miles by 2.5 miles). I collect this data and calculate the average GHI for the segment that each fossil fuel plant is in and use this as a proxy for the investment opportunity set for future solar development in that area.

I also take advantage of the fact that the returns to solar development have changed over time for policy-related reasons. Solar generation became commercially attractive in the US only after the passage of the Energy Policy Act (EPA) of 2005. The Act created a solar Investment Tax Credit (ITC) that offered a 30% federal tax credit for investors in solar energy properties. It significantly reduced the cost of solar development and increased the value of the fossil fuel plant from a solar development perspective.

I exploit these variations in a difference-in-differences setting and compare the effect of solar radiance on the likelihood of PE ownership of fossil fuel plants before and after the passage of the investment tax credit. The identifying assumption is that solar radiance should not affect the value of a fossil fuel plant differentially before and after 2005, except for the option value of future solar development.

I find that private equity firms are more likely to buy a fossil fuel power plant in areas with higher solar radiation after solar generation becomes commercially more attractive. A one standard deviation increase in solar intensity within a state increases the likelihood of private equity firms buying a fossil fuel plant by 3.1% after 2005. This is an economically meaningful 41% increase relative to the likelihood of PE ownership of a fossil fuel plant in my sample (7.5%). This effect holds after including plant fixed effects that account for any time-invariant plant-level characteristics, such as the production quality of the power plants, and state-year fixed effects that control for time-varying state-level characteristics, such as states' renewable policies.[8] This result is also robust to the inclusion of plant-level time-varying characteristics, such as total generation and the efficiency of the plant, and county-level changes in population.

What kind of plants are more attractive from the future solar development perspective? Older plants have shorter remaining lives, and smaller plants generate less electricity. Such plants are more attractive from the solar point of view, as they offer all the benefits such as proximity to transmission lines and substations, without costing as much as large and new plants. The investment opportunity value of these assets, therefore, is much higher than the value of the assets-in-place. Consistent with this, I find that PE acquisition of fossil plants depends on sunlight only for smaller and older plants, and not for larger and newer plants.

These results, taken together, suggest a strong relationship between PE ownership of fossil plants and solar development in the area. Next, I explore the mechanisms through which private equity facilitates solar development. PE firms have strong relationships with other institutional investors (Ivashina and Kovner, 2011; Malenko and Malenko, 2015), who are also key players in greenfield solar development (Andonov and Rauh, 2022). The repeated interaction between these investors eases the flow of information about new opportunities

[8]Several states have adopted renewable policies, such as Renewable Portfolio Standards (RPS), during this period (Upton and Snyder, 2017b).

and makes contracting (e.g., power agreements) easier.

A simple case study illustrates this mechanism. CLECO is an electric utility that owns power plants in Louisiana. CLECO was acquired by a consortium of PE investors led by the Macquarie Group in 2016. In 2022, CLECO provided the interconnection rights of a coal power plant to the D.E. Shaw & Co. (a hedge fund) for solar development and entered into a long-term power off-take agreement with the hedge fund. While Macquarie and D.E. Shaw may seem unrelated, Macquarie had previously sold several solar plants to D.E. Shaw. The two investors, therefore, had prior relationships and expertise in solar-related agreements.

While we do not observe these agreements for all such partnerships, I provide some empirical evidence to show that this case study reflects a more general pattern. I find that the increase in solar development in PE counties (counties with PE-owned fossil plants) comes from institutional investment (such as from private equity, banks, and pension funds) in solar. Moreover, in more than half of the PE counties with institutional solar development, I find that the solar and fossil investors are either owned by the same parent company or related through prior limited partnerships. This suggests that the relationships between institutional investors may be valuable in realizing these investment opportunities.

Overall, contrary to the prevailing criticism that PE acquisition of dirty assets delays the green transition, I show that it can facilitate the transition by allowing new technologies to come up. PE firms, due to less regulatory and public scrutiny, offer institutional investors a path to entry into the energy sector that then increases their green investments in solar. My findings suggest that any future regulations prohibiting institutional investments in fossil fuels may unintentionally also reduce clean energy investments and hamper the green transition.
My paper contributes to several strands of literature. First, it relates to the growing literature on environmental finance. One important question in this literature is the effectiveness of divestment in achieving environmental and societal goals (Heinkel, Kraus and Zechner, 2001; Duchin, Gao and Xu, 2022; Green and Vallee, 2022; Broccardo, Hart and Zingales, 2022; Berk and van Binsbergen, 2021; Edmans, Levit and Schneemeier, 2022; Sachdeva et al., 2023). Another related strand studies the environmental effects of institutional ownership of dirty assets (Shive and Forster, 2020; Bellon, 2020; Bai and Wu, 2023). Existing research primarily examines how divestments from or investments in polluting assets impact those assets themselves. In contrast, my paper highlights that in the presence of barriers to entry for new technology and synergies between old and new assets, the ownership structure of existing assets may affect the development of new technology and should be taken into account. I show that prohibiting institutional investments in dirty assets may lead to fewer investments in clean technology.

Andonov and Rauh (2022), a paper close to mine in this strand, documents that institutional investors have increased their ownership in the power sector through development of new plants, especially renewables, and find deregulation to be the key driver behind these ownership changes. My paper, on the other hand, establishes a causal link between private equity ownership of fossil fuel assets and the development of renewable plants in those areas.

Second, this paper contributes to the literature studying the stakeholder impact of private equity.[9] While a few papers have studied the impact of PE on innovation (Mollica and Zingales, 2007; Popov and Roosenboom, 2009; Lerner, Sorensen and Strömberg, 2011), the evidence on the effects of PE on new business creation is scarce, primarily due to lack of data availability on new business creation. My paper bridges this gap in the literature by showing that PE acquisition of old assets leads to creation of new businesses, creating positive externalities for the local economy and the environment.

Third, my paper is related to the energy economics literature studying decarbonization of the electricity sector. One of the key impediments to decarbonizing the power sector has been the lack of sufficient network infrastructure, such as transmission lines, and regulatory mechanisms to allocate these transmission rights (Joskow and Tirole, 2000; Gonzales, Ito and Reguant, 2022). I provide empirical evidence that existing power plants with transmission rights and local knowledge can facilitate decarbonization if the contracting frictions between the owners are reduced, e.g., through a change in the ownership structure.

A related literature in energy economics studies the determinants and the effects of the ongoing ownership changes in the power sector (Borenstein and Bushnell, 2015; Cicala, 2022; Andonov and Rauh, 2022). To the best of my knowledge, my paper is the first to show that the ownership of fossil plants affects clean energy development due to asset synergies and owners' varying incentives and abilities to exploit them.

2.2 Hypothesis Development

I hypothesize that the relationship between PE ownership of fossil fuel plants and solar development is driven by three key features: 1) synergies between fossil fuel and solar plants; 2) frictions that prevent incumbent owners from exploiting these synergies; and 3) characteristics of PE firms that make them less susceptible to these frictions. In this section, I describe these points and use them to formulate my hypotheses.

[9] A large literature studies the impact of PE acquisition on competitors (Chevalier, 1995a,b), industry peers (Bernstein et al., 2017; Aldatmaz and Brown, 2020), and the environment (Bellon, 2020). For a review of the literature on stakeholder impact of PE, see Sørensen and Yasuda (2022).

2.2.1 Synergies between fossil and solar plants

There are two key challenges solar developers face in the US. The first relates to siting — the process of choosing a location to install solar panels. Utility-scale solar farms require large plots of land in flat, sunny areas that are also close to the grid and transmission lines and have access to electrical infrastructure, such as substations and transformers.[10] Often, such plots of land exist near population centers, where power plants were established historically and are used for pastoral and agricultural purposes. Development on these lands faces significant objections from local communities as it adversely affects the landscape and diminishes property value (Jarvis, 2021; Gaur and Lang, 2023).[11] In addition, siting requires permits from regulators and agreements with several other stakeholders, such as power purchasers and electric utilities.

The second key impediment to solar development is related to interconnection — the process of connecting a new power plant to the transmission lines (see Rand et al. (2023) for an overview of interconnection issues). The interconnection process requires a new plant to undergo a series of impact studies that estimate the total costs of new transmission equipment and upgrades needed to connect the plant to the grid. The new project bears this entire cost of transmission upgrade, even though the benefits are shared by every plant connected to the grid, leading to collective action problems (Shleifer and Vishny, 1986; Grossman and Hart, 1980). These costs can be significantly high, especially for smaller projects such as solar plants, as costs associated with equipment upgrades are generally fixed.[12] Moreover, the regulatory process to approve these projects is slow. Consequently, there is a large 'interconnection queue' — a backlog of projects waiting to be approved and connected to the grid. As of February 2023, over 2000 GW of new capacity (130% of current US electricity demand) is seeking interconnection to the grid, with an average wait time of over four years. Solar capacity accounts for the largest share at 947 GW (Rand et al., 2023), highlighting a key challenge solar developers face in the US.

Existing fossil plants may help overcome these challenges. First, these plants are already wired to the power grid and have the rights to interconnect and transmit electricity through the grid, called capacity interconnection rights (CIR). The power plants can share these rights with other developers, saving significant costs, time, and regulatory hassle for a new solar developer.[13] Moreover, fossil fuel plant owners have better information about the state of the electrical infrastructure and the load on local transmission lines and, therefore, may

[10] 1MW of power requires roughly five to eight acres of land (Ong et al., 2013).

[11] See these objections from https://www.clarkcoalition.com/solar.

[12] Many new renewable projects are withdrawn due to higher-than-expected interconnection costs.

[13] Different regional transmission organizations (RTOs) have different rules regarding the sharing and transfer of CIR to other developers.

help a solar developer plan where to request an interconnection. Fossil plant owners also have relationships with other stakeholders, such as regulators and local communities, that may help reduce information asymmetry between new solar developers and these players. The synergies between fossil and solar plants are also evident from the proximity of solar development in the US. Figure 2.1 (b) shows that around 85% (58%) of all solar plants are within 20 miles (10 miles) of a fossil fuel plant.

2.2.2 Frictions faced by incumbent owners

Fossil plant owners would ideally contract with solar developers to realize these opportunities. However, not all incumbent owners and buyers of fossil fuel plants have equal incentives and abilities to form such partnerships with solar developers. There are two main types of incumbent owners in the power generation industry- large, publicly traded utilities (e.g., Duke Energy) and power producers (such as Vistra Corp.) and small, privately held merchant owners of power plants. The large public companies lack incentives, and the small private firms lack the ability to partner with solar developers and realize these investment opportunities.

Large public companies face several financial frictions that affect their incentives to partner with specialized solar developers. First, publicly traded electric utilities face significant regulatory and public pressure not to own dirty assets (van Benthem et al., 2022; Bolton and Kacperczyk, 2023). Relatedly, the current ESG metrics rely heavily on existing assets-in-place that are owned by the firms themselves (Hartzmark and Shue, 2023). Therefore, public owners of fossil fuel plants are not incentivized to partner with other solar developers as it does not affect the 'greenness' of the fossil assets owned by utilities. Public firms are also prone to several managerial agency issues, which reduce their incentives to pursue long-term, risky projects. The dispersed ownership structure, for example, incentivizes the managers to have a quiet life (Bertrand and Mullainathan, 2003) and not pursue risky projects. Electric utility managers are also often compensated based on their short-term earnings, which reduces their incentives to undergo long-term carbon transition projects even if they are value-enhancing (Kumar and Purnanandam, 2022).

Privately-held, small power producers, on the other hand, do not face the same public pressure or suffer from such managerial agency issues. However, they have less access to capital and relationships with other investors.[14] Since developing new solar plants requires significant investments from several capital providers, these small firms are less able to realize these investment opportunities (Gilje and Taillard, 2016; Phillips and Sertsios, 2016; Mortal

[14]Petersen and Rajan (1994a) and Bharath et al. (2009) show that small businesses are often credit-constrained.

and Reisel, 2013). Moreover, both of these traditional owners of power generation assets face significant abandonment costs and legal liabilities associated with the retirement of the fossil fuel plants, which make any transition to green energy less valuable for them (see Atanasova and Schwartz (2019) for an overview of the impact of stranded assets on the value of firms that own fossil fuel reserves).

2.2.3 Role of private equity

Private equity firms are relatively less susceptible to these frictions. They have fewer disclosure requirements, face less regulatory and public pressure from owning dirty assets, and do not have to rely on public ESG ratings for their valuations (Bernstein, 2022). Moreover, private equity firms often avoid being a 'trade or business' for tax and liability purposes by taking minority stakes in companies and acquiring firms through club deals and co-investments. This shields them from certain future liabilities, such as pension obligations, and reduces their overall abandonment costs relative to other traditional owners.[15] PE firms also have relationships with other capital providers and institutional investors due to repeated interactions (Demiroglu and James, 2010; Ivashina and Kovner, 2011; Malenko and Malenko, 2015). This may ease the flow of information between investors and make contracting easier, enabling the PE firms to exploit the synergies with other investors looking to invest in renewables.

2.2.4 Hypotheses

In the presence of asset-side synergies and financial frictions, I formulate the following hypotheses:

H1: Solar development increases more in areas with PE-owned fossil plants.

If synergies between fossil and solar plants exist, and if private equity firms can better exploit the synergies than traditional owners due to financial frictions, we should expect solar development to increase in counties where PE owns fossil plants.

H2: Fossil plants that offer more synergies for solar development should be more likely to be owned by private equity.

In equilibrium, assets should be reallocated to their most efficient user. If PE firms are better able to manage the synergies, the plants that offer more synergies should have higher value for a PE firm than other firms. Therefore, in equilibrium, they should be more likely to own plants that are valuable from a solar development point of view.

[15] See this court ruling on Sun Capital Partners v New England Teamsters Pension Fund.

2.3 Data and Descriptive Statistics

Power plant-level characteristics: I obtain plant characteristics from the Energy Information Administration (EIA). EIA Form 860 provides generator-level information such as fuel type, technology type, and installed capacity for electric power plants with 1 MW or greater nameplate capacity. EIA Form 923 provides annual information on electricity generation and fuel consumption at the power plant-prime mover level for plants greater than 1MW capacity. I aggregate information from these forms for all solar plants and fossil-fuel plants that run on coal, natural gas, or oil from 2000-2022.

Panel A of Table 1 reports the summary statistics of plant-year-level information for fossil and solar power plants in my sample. An average fossil fuel plant in my sample is 28 years old, has a capacity of 272MW, and generates 1026GWh of energy per year. In contrast, an average solar plant is younger and smaller in capacity, with an average age of 3 years, capacity of 11MW, and generation of 31GWh per year.

Ownership data: I collect plant-ownership data for fossil-fuel and solar plants from S&P Capital IQ, which compiles asset-level ownership data for power plants in the US, and supplement this with hand-collected data from news articles, PR newswire, and homepages of private equity firms. I classify the owner of a power plant as an institutional investor if more than 25% of the plant is owned by private equity funds, asset managers, banks, or pension funds (through direct investments). All other owners are classified as energy companies, which include publicly traded electric utilities, private independent power producers, and other oil and gas companies. Around 10% of fossil plants and 17% of solar plants have an institutional owner for at least one year of the sample. Similar to Andonov and Rauh (2022), I find greater ownership of renewable plants by private equity and institutional investors compared to fossil plants.

Private equity data: I collect data on private equity investors and their limited partners and co-investors from Pitchbook. Pitchbook provides separate files with information on relationships between private equity funds and their limited partners and investors and their co-investors. I use these files to compile a relationship database of all private equity firms and institutional investors invested in fossil-fuel plants and solar plants and every investor they are affiliated with through limited partnerships or co-investments.

Solar radiation: There are three most common measurements of solar radiation: global horizontal irradiance (GHI), direct normal irradiance (DNI), and diffuse horizontal irradiance (DHI). Global horizontal irradiance (GHI) captures total solar radiation incident on a horizontal surface. It comprises both the DHI and the component of DNI that falls on the surface.[16] GHI is also used to estimate the capacity of total solar power that can be generated from an area and is, therefore, an important and highly relevant measure of solar investment opportunity from an area. While a few other papers in the energy literature, such as Sexton et al. (2021), have used irradiance as a proxy for solar development from the location of a solar power plant, mine is the first paper, to the best of my knowledge, that uses solar irradiance at a fossil-fuel plant location as a measure of investment opportunity for solar development.

I obtain this measure of solar radiation from the National Solar Radiation Database (NSRDB). NSRDB provides hourly values of the GHI at a spatial resolution of 4×4 km (around 2.5×2.5 miles). I collect this data for every fossil-fuel power plant location in the US for five years, from 2010 to 2014, and calculate the average daily GHI (in kWh/m^2) by adding hourly GHI for every day and taking an average over five years. While there is a significant temporal variation within a year, the average daily GHI at a location does not vary significantly across years.

There is a significant spatial variation in the GHI both across states and within-state. Figure 1 shows this variation. Panel A plots the Global Horizontal Irradiance (GHI) of all fossil-fuel power plants (with a capacity greater than 10MW). The size of the circle represents the plant's capacity, and the color shows the irradiance. The darker the red color, the more sunlight the area receives. On average, GHI varies from $5.83kWh/m^2$ in Arizona, the state with the most sunlight in my sample, to $3.34kWh/m^2$ in Washington, the state with the least sunlight on its power plants. Across states in the US, the standard deviation of solar radiation is 0.59 W/M2, roughly 13% of the average solar radiation ($4.57kWh/m^2$). There is also a significant within-state variation in solar radiation. Panel B plots the within-state variations for four states: California, Texas, New York, and Michigan. This within-state variation allows me to tease out the effect of solar radiance on outcomes after controlling for state-level changes in policies.

County-level aggregated sample: For the overall analysis of the impact of PE acquisition of fossil plants on solar development, I need to define an area close enough to a power plant that the solar plant benefits from the transmission synergies. I use the administrative county of the fossil-fuel power plant as the area of interest around it. I start with all the US counties with a utility-scale fossil-fuel power plant.[17] Next, I create county-year-level panel data of these counties from 2001-2020 and aggregate information such as the total number of generators, total capacity, and total generation from fossil fuel plants and solar plants in these counties.

[16]GHI is calculated as DNI * Cos(Z) + DHI, where Z is the solar zenith angle

[17]Utility-scale power plants are plants with at least 1 megawatt of total electric generating capacity.

I define a county to be PE county in a given year if at least one fossil plant in the county is owned by a private equity firm in the year. As counties with power plants differ in quality and potential for future power development from those without, I keep a county in my dataset if it has at least one fossil power plant in the year 2000. This allows me to compare solar development in counties with PE-owned fossil plants with those that have a fossil plant but is not PE-owned.

Panel B of Table 1 reports county-year-level summary statistics grouped by PE ownership in the county. PE counties have more number and capacity of fossil plants than non-PE counties. On average, a PE county has around 5.7 power plants in the county, with a total capacity of 1600MW. In contrast, a non-PE county has around two power plants and a capacity of 481MW. An average PE county also has more number (1.9 plants) and capacity (20.5MW) of solar plants in my sample than an average non-PE county at 0.33 plants and 4.8MW capacity.

2.4 PE Ownership of Fossil Fuel Plants and Solar Development

In this section, I investigate the effects of Private equity acquisition of fossil fuel power plants on solar development in the area. For this analysis, I aggregate fossil and solar plant information at the county-year level. I start with all the counties that has a fossil-fuel power plant at the start of my sample. I ask whether solar development increases in a county after the PE acquisition of a fossil plant in that county.

2.4.1 Empirical design

To study this, I start with the traditional difference-in-differences equation with two-way fixed effects (TWFE) that estimates the following model:

$$y_{c,t} = County_c + Year_t + \beta \times Post\,PE_{c,t} + Controls_{c,t} + \epsilon_{c,t} \tag{2.1}$$

where $County_c$ and $Year_t$ are county and year fixed effects, $Post\,PE_{c,t} = 1$ if there is a PE-acquired fossil plant in county c and year t and zero otherwise. The main dependent variable, $y_{c,t}$ takes 1 if there is a solar power plant in the county c in year t and zero otherwise. I cluster standard errors at the county level for this analysis. Under the parallel trends assumption and in the presence of homogenous treatment effects, β represents the effect of PE acquisition on the likelihood of solar development in the county.

Recent developments in econometrics suggest that this TWFE estimator may be biased when treatment is staggered and treatment effects heterogenous.[18] To address these concerns, I first balance the panel data by restricting my treated sample to have 5 years of data before and after the treatment.[19] Next, I follow the methodology proposed by Sun and Abraham (2021) that alleviates the concerns related to TWFE. This method uses "interaction-weighted" (IW) estimators that are formed by first estimating an average treatment effect with a regression that is saturated in the cohort and relative period indicators, and then averaging estimates across each cohort at a given period. More specifically, Sun and Abraham (2021) estimate an equation of the following form:

$$y_{c,t} = County_c + Year_t + \sum_{g} \sum_{k \neq -1} \beta_{g,k}(1\{G_g = g\} \cdot D^k_{c,t}) + Controls_{c,t} + \epsilon_{c,t} \tag{2.2}$$

where $County_c$ and $Year_t$ are county and year fixed effects, $1\{G_g = g\}$ is the cohort-indicator representing each treatment cohort g, $D^k_{c,t}$ is the relative period k from PE-acquisition of a fossil plant in county c and year t. $\beta_{g,k}$ represents the cohort-specific average treatment effect on the treated (CATT). The average treatment effect on the treated (ATT) is then the weighted average of these cohort-specific estimates, with weights equal to each cohort's respective sample share. I report this ATT as the main effect in this paper. My results are robust to alternative specifications, for example, as suggested in Callaway and Sant'Anna (2021).

2.4.2 Results

Figure 2.3 plots the dynamic effects of PE acquisition of fossil plants on the likelihood of solar development in the county and presents two key evidence in support of my empirical results. First, it shows that solar development in treated and control counties followed a parallel trend before the PE acquisition of a fossil plant. The parallel pre-trend alleviates the concerns that the treated and control counties had different pre-trends in their solar development. Second, the likelihood of solar development gradually increases over the next five years, with the increase in years 4 and 5 statistically significant at 5%. This suggests that the increase in solar development comes a few years after PE acquisition and is consistent with the average of 2-3 years it takes to plan and develop solar plants during my period (Rand et al., 2023).

Table 2.2 reports the estimates from the staggered differences-in-differences models in

[18] For an overview of the problems with TWFE and alternative estimators as solutions, see Baker, Larcker and Wang (2022).

[19] My results are robust to choosing other choices of pre- and post-treated periods.

Equations (2.1) and 2.2. In columns (1)-(4), the dependent variable is *solar* $dummy_{c.t}$, a dummy that takes the value of 1 if there is a solar development in county c and year t, and zero otherwise. Columns (1) and (3) present the baseline results estimating Equations (2.1) and (2.2) respectively. The likelihood of solar development in the county increases by around 8.1% (10.1% using TWFE), after the PE-acquisition of a fossil fuel plant in that county relative to control counties.

While county and year fixed effects in my analysis take care of time-invariant county-specific characteristics and time-varying macro trends, they do not address unobservable time-varying county-level characteristics such as higher demand for electricity due to increasing population or higher supply due to more number of power plants being developed in the area. While I cannot rule out these possibilities altogether, I control for changes in electricity demand by including controls for the number, capacity, and total generation of fossil fuel plants in the county each year and the county-year level population. The results are robust to the inclusion of these controls, as shown in columns (2) and (4).

Another possible concern is that the deregulation in the electricity markets in several states may be driving my results (Borenstein and Bushnell, 2015; Mansur and White, 2012). Several states have reformed their transmission access to ensure independent oversight and control of the transmission networks. Instead of an electric utility company managing transmission networks, these states have an Independent Systems Operator (ISO) as a balancing authority.[20] Andonov and Rauh (2022) find that states with deregulated electricity wholesale markets (states with ISOs) have higher ownership by private equity, and these states also have more renewable development. While much of the deregulation happened in the late 1990s, before the start of my sample, states with deregulated electricity markets may have different trends in ownership and power generation mix over the last two decades. To address this concern, I first add a Regulated state dummy-by-year fixed effects in column (5) to account for varying time trends in deregulated and regulated states separately. My results hold after including these fixed effects. I also restrict my sample to only deregulated states and estimate Equations (2.1) and (2.2) on the restricted sample and find the results to be the same as the full sample. Overall, across specifications, there is approximately a 9% increase in the likelihood of solar development in PE counties relative to non-PE counties. This increase is economically significant given that only 37% of all counties have any solar development in my sample.

In addition to the extensive margin, I also test whether, on an intensive margin, there is an increase in the number, capacity, and generation from solar plants in counties that would have had solar development without the treatment. To study this, I follow the methodology

[20] See Borenstein and Bushnell (2015) for an overview of electricity restructuring in the US.

suggested in Chen and Roth (2022) that shuts off the extensive margin channel. Specifically, I transform Y into $m(Y) = log(Y/Y_{min})$ for all $Y > 0$, and $m(Y) = 0$ for all $Y = 0$, where Y_{min} is the minimum non-zero value of the variable Y. This sets the zero values of the variable equal to the minimum non-zero value of that variable, shutting off the extensive margin.

I use this transformed log variable of the number of solar plants, the capacity of solar plants, and generation from solar plants as my dependent variable in columns (6)-(8), respectively. These columns also use the control variables and Regulated -by-year fixed effects as in column (5). The number of solar plants increases by around 11%, solar capacity by 40%, and total generation from solar by 80% in PE counties relative to non-PE counties. These effects are statistically significant at 1%. This set of results suggests that PE acquisition of fossil plants also leads to an increase in solar development on an intensive margin (i.e., the amount of solar power increases in counties that would have had solar development anyway).

2.5 PE Incentives to Acquire Fossil Fuel Plants

In this section, I establish a direct link between PE ownership of fossil fuel plants and solar development. More specifically, I test my second hypothesis. If private equity firms are better at realizing the investment opportunities that fossil assets provide for solar development, we should expect the assets that offer more opportunities to be reallocated to their more efficient user, in this case, private equity (Maksimovic and Phillips, 1998). Specifically, PE firms should be more likely to own fossil plants that offer more investment opportunities for solar development.

2.5.1 Empirical design

Studying whether PE firms acquire and own a fossil plant that offers more investment opportunities for solar development is empirically challenging. First, it is difficult to measure the investment opportunity set around a fossil plant for the development of solar power. Second, the opportunity to develop solar plants may be correlated with other unobserved factors that also simultaneously affect the PE's motivation to acquire the fossil fuel plant. For example, if an area implements a new renewable policy, it will affect not only the likelihood of new solar development but also the generation prospects from existing plants. Therefore, it is empirically challenging to isolate the variations in the solar investment opportunity that leave other factors unchanged.

I isolate this effect using the interaction of a spatial variation in the ability to generate solar power and a time-series variation in the costs of solar development. First, I use the intensity of the sunlight, measured in solar irradiance, that falls on a fossil-fuel power plant as a measure of future solar generation capacity from the area. A comprehensive measure of total solar irradiance on a surface is the Global Horizontal Irradiance (GHI). I collect this measure of solar intensity for an area of roughly 2.5 miles by 2.5 miles (4 km by 4 km) around the geographical coordinates of each fossil fuel power plant in the US over five years from 2010-2014 and calculate the average GHI. I use this average GHI as a proxy for the investment opportunity set for future solar development in that area.

Using the intensity of sunlight as a measure of the solar investment opportunity set has several advantages. First, there is a significant spatial variation in the intensity of solar radiation in the US, both across states and within a state. Second, the variation in solar irradiance is relevant for solar development, i.e., the sunlight that falls on an area strongly predicts the likelihood of solar development (Law et al., 2014; Sexton et al., 2021).

While solar radiation is plausibly exogenous to the quality of fossil-fuel plants in the area, there may be unobservable characteristics of fossil plants that are correlated with sunlight intensity, which might affect the likelihood of PE acquisition of these plants. Therefore, I use an additional time-series variation in the costs of solar development to strengthen my exogeneity claim. I use the passage of the Energy Policy Act (EPA), 2005, which introduced a 30% solar Investment Tax Credit (ITC) for residential, commercial, and utility investors in solar. This ITC substantially reduced the costs of solar development and made it commercially viable for the first time in the US. It has been one of the most significant federal policies to support the growth in solar generation in the US.[21]

I exploit these variations — the solar radiance of an area and the passage of the ITC — to achieve a plausibly exogenous change in the PE's incentive to acquire fossil plants for future solar investment opportunities. Specifically, I use a difference-in-differences model with continuous treatment and compare the effects of solar radiance on the likelihood of PE ownership of fossil plants before and after 2005. I estimate the following equation:

$$PE\ Owned_{i,t} = Plant_i + State \times Year_{s,t} + \beta \times Solar\ Radiance_i \times Post\ 2005_t + \epsilon_{i,t} \quad (2.3)$$

The dependent variable, $PE\ Owned_{i,t}$, is a dummy that takes one if a fossil-fuel plant i is owned by a PE firm in year t. $Solar\ Radiance_i$ is the average GHI (solar irradiance) around the area of the power plant i, standardized to have a mean of 0 and standard deviation (s.d.) of 1. $Post\ 2005_t$ is a dummy that takes one in the years starting 2005, and zero before. The model includes plant fixed effects to account for any time-invariant plant-level characteristics, such as production quality, and state-by-year fixed effects to soak away any time-varying

[21]See Stokes and Breetz (2018) for an overview of significant energy policies in the US.

state-level characteristics, such as changing state-level renewable policies and incentives. In additional specifications, I include plant-level controls for the plant's net generation and energy efficiency to account for any time-varying changes in the quality of the plant and annual county-level population to control for time-varying changes in the demographics of each county. Therefore, my model allows me to estimate the differential effects of the solar intensity of the PE likelihood of fossil fuel before and after 2005 within each plant after taking into account state-year-level changes, changes in plant quality, and county-level changes in demographics.

2.5.2 Results

I begin my analysis by showing how the effects of the radiance on the PE likelihood evolved by estimating the following event study-type regression model:

$$PE\ Owned_{i,t} = Plant_i + State \times Year_{s,t} + \sum_{\tau} (year = \tau) \times \beta_\tau \times Solar\ Radiance_i + \epsilon_{i,t} \quad (2.4)$$

The dependent variable $PE\ Owned$ takes 1 if plant i is owned by private equity in the year t. I use 2004, the year before the passage of the investment tax credits, as the reference year. Therefore, I compare the effect of solar radiance on the likelihood of PE ownership of fossil plants relative to 2004. Figure 2.4 presents the coefficient estimates and the 95% confidence intervals from this model. The figure suggests a parallel pre-trend and shows that prior to 2004, solar radiance had no significant effect on the likelihood of PE ownership of fossil plants. After 2004, however, there is a sharp increase in the effects, which persists in magnitude for the rest of my sample.

Table 2.3 presents the results of the regression model in Equation (2.3). Column (1) shows results for the baseline specification with no controls. It shows that a one standard deviation (s.d.) increase in the solar intensity of the plant location increases the likelihood of PE acquisition of the power plant by 3.1% after 2005 relative to before. This is an economically significant increase of 40% relative to the unconditional mean of PE ownership in my sample of 7.5%. Column (2) includes the time-varying plant-level controls for the net generation and the efficiency of the power plant. Column (3) adds further control for the county-level population to account for any time-varying changes in demographics across the counties. The results are robust to the inclusion of these controls. After including these controls, a one s.d. increase in the solar intensity of the plant location increases the likelihood of PE acquisition of the power plant by 3.3% after 2005 relative to before.

Next, I explore the cross-sectional variations in the characteristics of the plants that PE acquires with future solar investment motives. The value of a fossil fuel plant for its owner comes from the future cash flows it generates from the existing assets in place and the value it provides for future solar development. Therefore, the solar development value should be relatively more important for plants that have a lower present value of future cash flows from the existing assets. Older plants close to retirement and plants with smaller capacities that generate less electricity have lower future earnings than newer and larger plants. However, the options value of these plants, which comes from the infrastructure, such as access to transmission lines and relationships with stakeholders, are similar to larger, newer power plants. Therefore, such smaller and older plants should be more attractive from a solar development point of view. To test this, I split my sample of plants based on their installation year and capacity and estimate Equation (2.3) separately for these sub-samples. I classify plants built before the start of my sample in 2000 as old. Plants with less than 100MW of power are classified as small and others as large.

Table 2.4 presents the results. Columns (1) and (2) split the sample based on age and (3) and (4) on size. Columns (1) and (3) show that there is no significant relationship between solar intensity and PE acquisition for new and large plants respectively. On the other hand, columns (2) and (4) show that the likelihood of PE ownership of old and small plants significantly depends on the solar radiance post-2005 relative to before. A one s.d. increase in solar intensity increases the likelihood of PE acquisition post-2005 by 4.4% for old plants and 6.1% for small plants. These effects are also economically large in magnitude, given that the likelihood of PE ownership of an old plant (small plant) is only 5.9% (6.3%).

Overall, these results suggest that power plants that offer more synergies and higher investment opportunities for solar development are more likely to be owned by PE firms. This is consistent with my hypothesis that PE firms are better able to exploit these synergies. In equilibrium, assets are reallocated to reflect that.

2.6 Mechanisms

2.6.1 Relationship between owners of fossil plants and solar plants

PE firms have strong relationships with other institutional investors due to repeated interactions (Ivashina and Kovner, 2011; Malenko and Malenko, 2015). PE firms are also incentivized to maintain strong relationships with these capital providers for future fundraising purposes (Chung et al., 2012). These institutional investors are also key investors in greenfield solar development in the US (Andonov and Rauh, 2022). The repeated interaction between these investors eases the flow of information about new opportunities and makes contracting (e.g., power agreements) easier.

A simple case study, as shown in Figure 2.5, illustrates this mechanism. CLECO is an electric utility that owns power plants in Louisiana. CLECO was acquired by a consortium of PE investors led by the Macquarie Group (together with British Columbia Investment Management Corporation, John Hancock Financial, and other infrastructure investors) in 2016. In 2022, CLECO provided the interconnection rights of a coal power plant to the D.E. Shaw & Co. (a hedge fund) for solar development and entered into a long-term power off-take agreement with the hedge fund. While Macquarie and D.E. Shaw may seem unrelated, Macquarie and D.E. Shaw have prior interaction in the renewable energy space. For example, in 2013, Macquarie Group sold five solar farms in California to D.E. Shaw & Co. The two investors, therefore, have prior relationships and expertise in solar-related agreements.

While we do not observe these agreements for all such partnerships, I provide some empirical evidence to show that this case study reflects a more general pattern. If PE firms jointly realize these investment opportunities with other institutional investors, we should expect the solar development in PE counties to come from institutional investments in solar. To test for this, I classify the owners of the solar plants into two broad categories: (a) traditional energy companies, which include investor-owned utilities (IOUs), independent power producers, and energy companies, and (b) institutional investors, such as private equity, banks, asset managers, and pension funds.[22] Next, I define a county with solar development to be *institutional* solar if it has any institutional owner of a solar plant, and *non-institutional* otherwise.

I begin my analysis by showing some descriptive statistics in Table 2.5. Column 2 of the Table shows that solar development increased more in counties with PE-owned fossil plants (treated counties) than in counties without PE-owned fossil plants (control counties). It shows that around 34% of all control counties have solar development by the end of my sample, whereas this fraction is 59% in treated counties. Next, I divide the counties with solar development into those with institutional investment in solar and those without. This breakdown is shown in the 3rd and 4th columns. Counties with no institutional solar development have a similar share of solar across treated and control counties — 18% of all control counties and 22% of treated counties have solar development only by energy companies. On the other hand, there is a significant increase in institutional investment in solar in treated counties relative to control counties. Around 16% of the control counties have any institutional investment in solar, whereas around 37% of the treated counties have at least one institutionally owned solar. This shows that the increase in solar development in

[22]There are also government-owned and other non-profit owners, such as cooperatives, that are not included in my sample. (see Andonov and Rauh (2022) for different types of owners).

PE-owned counties mostly comes from institutional investment in these counties.

I more formally test this in Table 2.6. I classify a county with solar development as an institutional solar county if it has any institutional investment in solar and a non-institutional solar county with no institutional investment in solar. I then separately test whether the solar development in treated counties comes from institutional solar counties or non-institutional. More specifically, I estimate Equation (2.2) with two separate dependent variables, $institutional\ solar_{c,t}$ taking 1 if a county c has solar development by any institutional investors in year t, and zero otherwise (columns (1) and (2)), and $non\ institutional\ solar_{c,t}$ taking 1 if a county c has solar development in year t, but not by institutional investors (columns (3) and (4)). The likelihood of solar development increases in counties where institutional investors invested in solar by around 7%. This increase is statistically insignificant for counties with no institutional investor, suggesting that the PE acquisition of fossil plants helps institutional investors reduce their barriers to entry and develop solar.

I form fossil-solar institutional investor pairs that invest in the same county and classify them as *same investor* if the institutional investors investing in solar and fossil have the same parent company (e.g., Blackstone group), and *related investor* if they are related through prior limited partnerships (either one is a limited partner in another or they both share the same limited partners). Out of all the treated counties, 14% have the same investor investing in solar and fossil, and another 38% have a related investor. In total, more than half of all treated counties with solar development have a related institutional investor investing in solar. This suggests that relationships between the PE firms and the institutional investors are important for them to exploit the synergies together.

2.7 Robustness Tests

2.7.1 Matched difference-in-differences

In the previous section, I show that fossil plants that offer more investment opportunities for solar development are more likely to be reallocated and owned by private equity firms. Does the solar development increase post-PE acquisition solely due to the fact that PE acquires fossil plants in sunnier counties? This is not true, as I argue below. First, the relationship between solar radiance and the likelihood of PE acquisition that I find is within-state. Across the country, the difference between average solar radiance in counties with PE-acquired fossil plants and the others is statistically insignificant.

However, to further alleviate the concern that the treated counties are different from control counties in their radiance measure, I match every PE-acquired fossil fuel county with another county that has similar average radiation using a 1:1 nearest matching algorithm. I then use a matched difference-in-differences strategy by estimating Equations (2.1) and (2.2) on this set of matched sample. Table 2.7 presents results from this matched difference-in-differences strategy. Columns (1) and (2) show results from the TWFE estimation, and (3) and (4) from Sun and Abraham (2021). Across all of the specifications, the results are robust and similar to the baseline specifications.

2.7.2 PE acquisition and future growth potential of the area

My results so far point out that PE acquisition leads to more solar development, and PE acquires fossil fuel plants with more opportunities for solar generation. Do PE firms acquire fossil plants in areas with more solar generation potential or more economic potential and higher future energy demand? In this part, I rule out this alternative explanation. First, I include county-level population controls in all of my analyses, and the results do not change after the inclusion of these controls. This suggests that the population changes do not play a key role in driving my results. To further strengthen my claim, I test whether PE acquisition of a fossil fuel plant is followed by a growth in the population in the county. More specifically, I estimate Equations (2.1) and (2.2) with population change in the county as the dependent variable. Table 2.8 presents the results. Columns (1) and (2) show results from the TWFE estimation and (3) and (4) from Sun and Abraham (2021). Across all of the specifications, I find no evidence that PE counties have a higher population growth relative to non-PE counties.

2.8 Conclusion

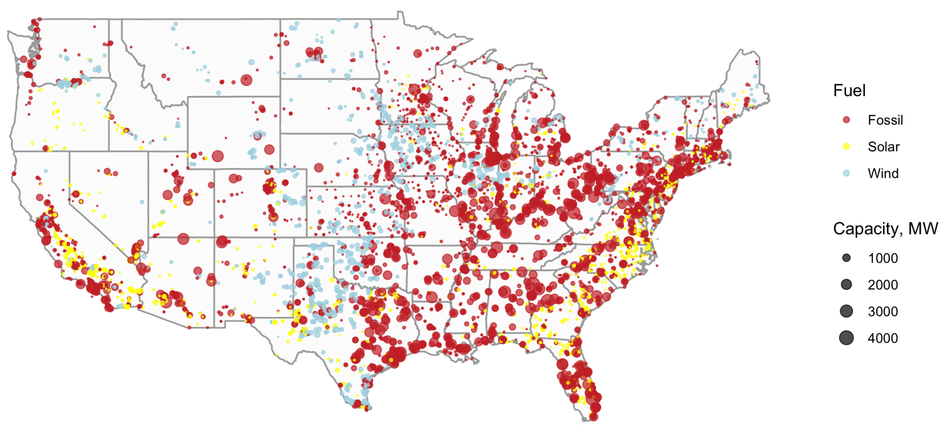
There is a growing concern among media, policymakers, and scholars that private equity investment in fossil fuel adversely affects environmental outcomes and delays the clean energy transition. Contrary to this prevailing notion, I provide evidence that PE acquisition of fossil fuel *facilitates* the green transition by allowing cleaner technologies to develop. I first show that PE acquisition of fossil plants leads to higher solar generation in the county. The increase in solar development in PE-owned fossil area comes from institutional investors, specifically the ones that are related to the PE owner of fossil plants. I show that PE firms are more likely to own fossil fuel plants that offer higher investment opportunities for solar development.

My results have important policy implications. As cities and states consider prohibiting their pension funds from investing in fossil fuels through private equity, it is important to consider the unintended consequences of such restrictions on the development of clean technology. My paper suggests that regulations prohibiting PE investment in fossil fuel may prevent financing of clean energy and impede the green transition.

Figure 2.1: Proximity between solar and fossil plants

Subfigure (a) of Figure 2.1 plots the location of fossil fuel, solar, and wind power plants in the US. The size of the circle represents the capacity of the power plant, and the color represents fuel type. The figure shows that solar development is more evenly distributed across the U.S., and are closer to the fossil-fuel power plants, relative to wind plants. Subfigure (b) plots the frequency of solar plants at different distance from a fossil-fuel plant. The figure shows that around 85% (58%) of all solar plants are within 20 miles (10 miles) of a fossil fuel plant.

(a): Location of power plants in the US



(b) Distance of solar plants from existing fossil plants

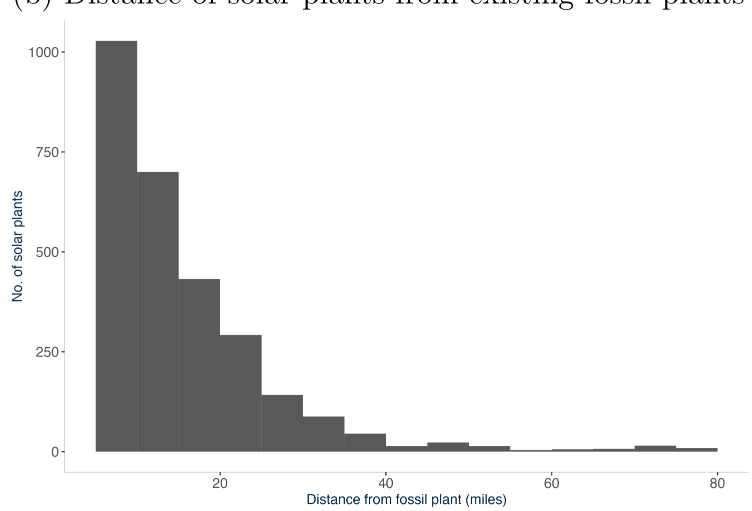
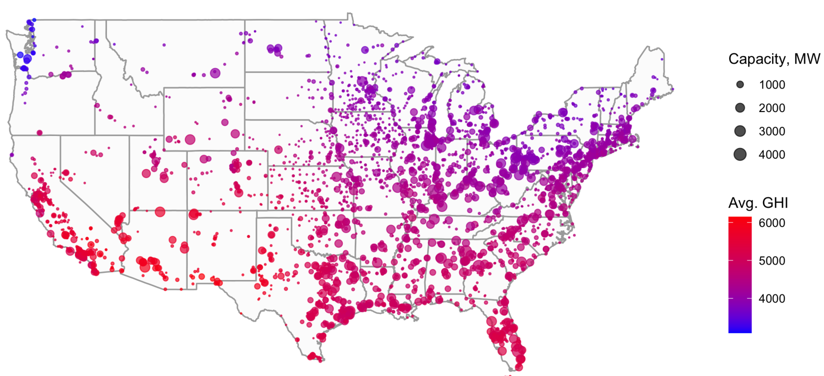


Figure 2.2: PE acquisition of fossil plants and solar development

Figure 2.2 plots fossil fuel power plants in the US and presents spatial variation in the intensity of the sunlight (measured in GHI) that falls on the power plants. The size of the circle represents the capacity of the power plant, and the color represents the GHI. The darker red color represents areas with greater sunlight. Subfigure (a) of Figure 2.2 shows the variations in GHI across all states in the US. Subfigure (b) shows within-state variations in the GHI for 4 states: Texas, California, New York, and Michigan.

(a) Across-state variations in GHI around fossil-fuel plants



(b) Within-state variations in GHI around fossil-fuel plants

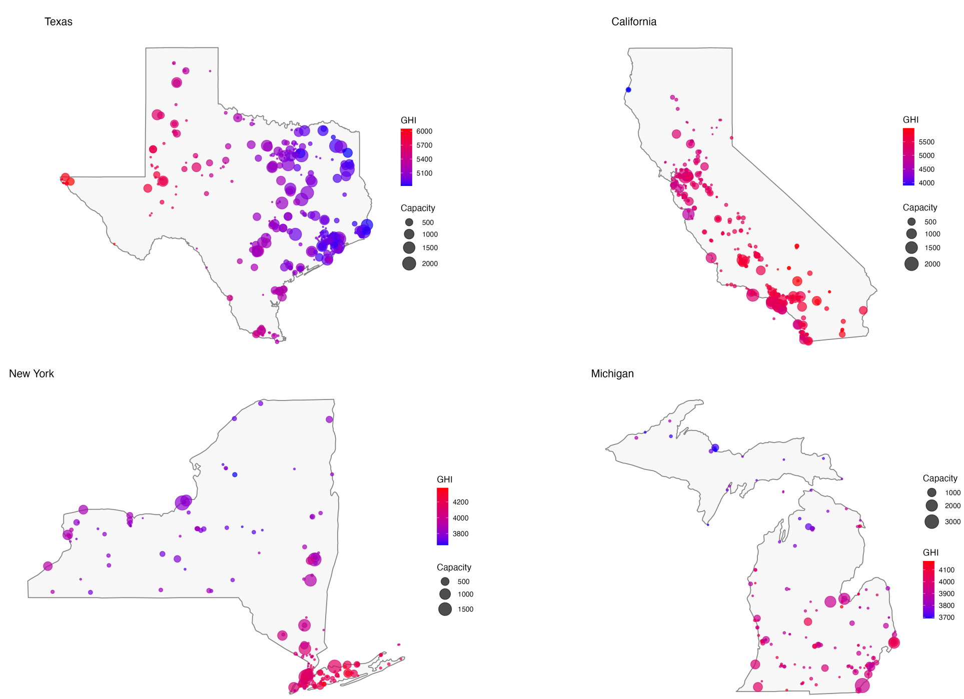


Figure 2.3: PE acquisition of fossil plants and solar development

Figure 2.3 reports the dynamic difference-in-differences estimates of the effect of the PE acquisition of fossil fuel plant on solar development following Sun and Abraham (2021) methodology.

The figure plots the $(\sum_g \beta_{g,k})_{k=-5,-4,...,4,5}$ from the following equation:

$$y_{c,t} = County_c + Year_t + \sum_g \sum_{k\neq -1} \beta_{g,k}(1\{G_g = g\} \cdot D_{it}^k) + \epsilon_{i,t}$$

where $1\{G_g = g\}$ is the cohort-indicator and $D_{c,t}^k$ is the relative period k from PE-acquisition of a fossil plant. The dependent variable $y_{c,t}$ takes 1 if there is a solar plant in county c in year t. Standard errors are clustered at the county level and confidence intervals at the 5% level are reported.

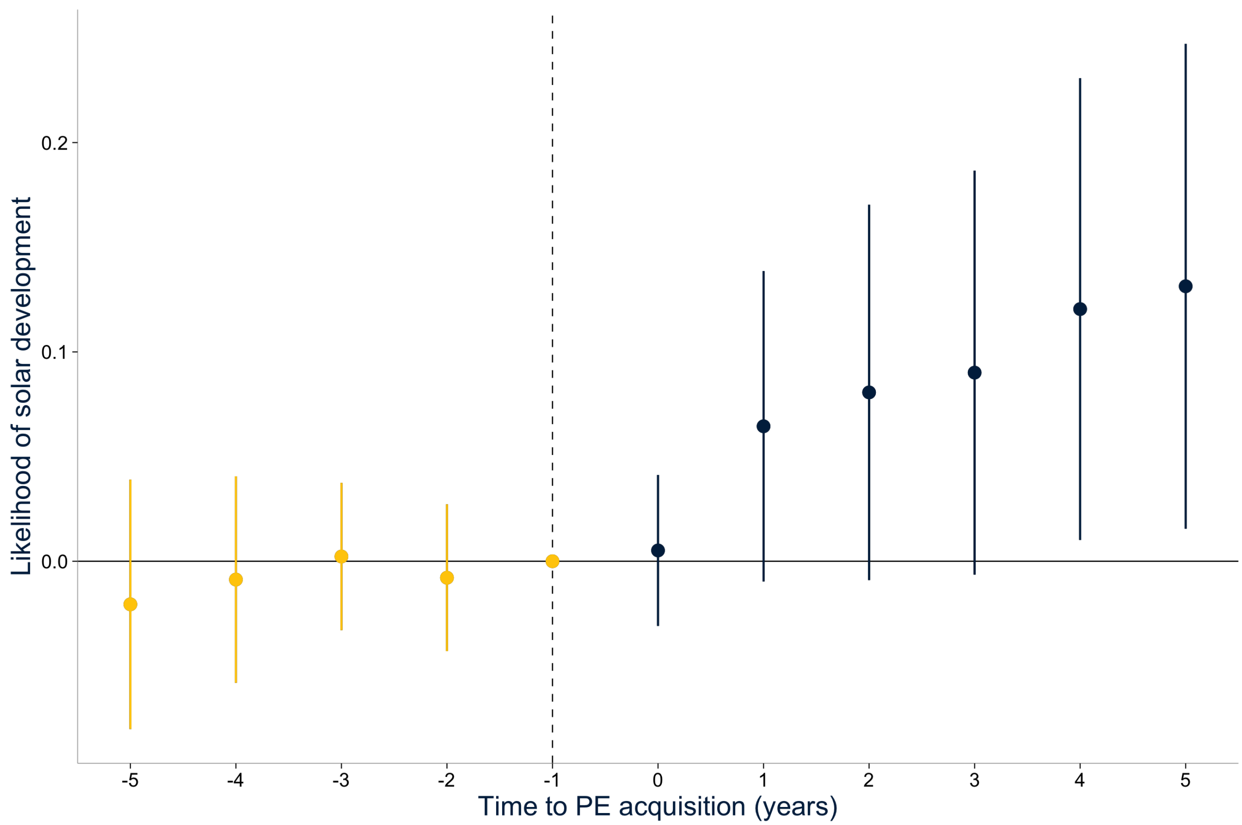


Figure 2.4: PE acquisition of fossil plants and solar development

Figure 2.4 reports the dynamic difference-in-differences estimates of the effect of the solar radiance on the likelihood of PE ownership of fossil fuel plant. Specifically, the figure plots the estimates and the 95% confidence intervals of β_τ from the following equation:

$$PE\ Owned_{i,t} = Plant_i + StateYear_{s,t} + \sum_{\tau}(year = \tau) \times \beta_\tau \times GHI_i + \epsilon_{i,t}$$

where the dependent variable $PE\ Owned$ takes 1 if plant i is owned by a private equity in the year t. Standard errors are clustered at the plant level and confidence intervals at the 5% level are reported.

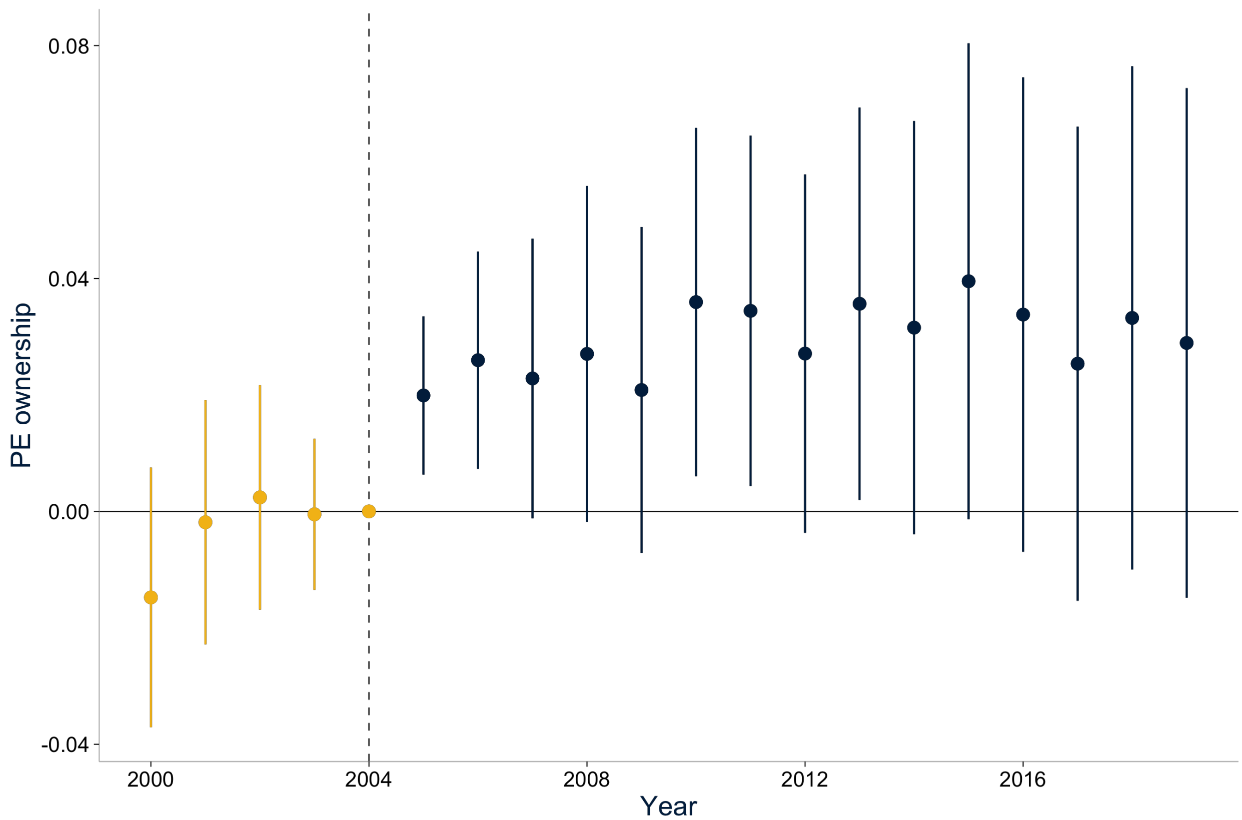


Figure 2.5: Contractual relationship between a coal plant and a solar plant

Figure 2.5 illustrates a contractual relationship between a coal plant owned by a private equity firm and a solar plant developed by an institutional investor.

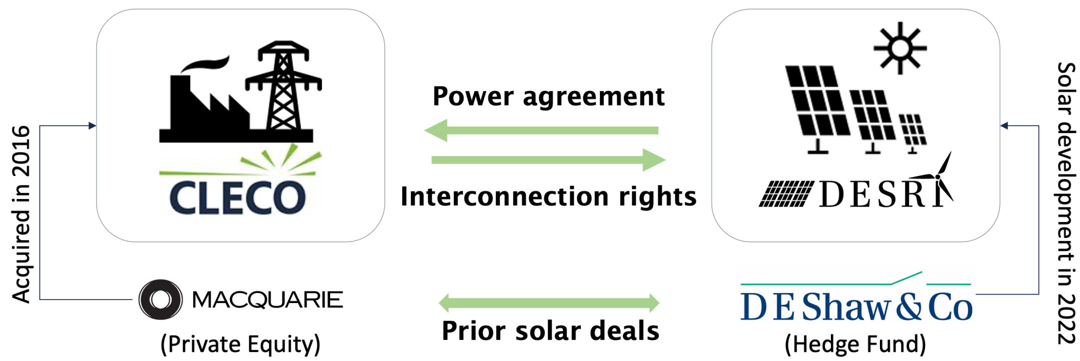


Table 2.1: Summary statistics

Table 2.1 reports the descriptive statistics of the variables used in this paper. Panel A presents characteristics at the power plant-year level. This data comes from the U.S. Energy Information Administration (EIA). Panel B presents aggregate data on county-year level grouped by observations that have PE-owned fossil plant in that county-year.

Panel A: Plant characteristics

Variable	N	Mean	SD	P25	Median	P75
Fossil-fuel:						
Age	55,894	27.51	21.38	8.00	24.000	45.00
Capacity (MW)	55,894	272.15	467.42	12.60	55.800	324.00
Generation (GWh)	50,312	1,026.23	2,471.81	2.36	49.880	654.71
Efficiency (MWh/MMBTU)	50,312	0.29	25.28	0.07	0.091	0.11
Fuel Type	55,894					
... Coal	8,485					
... Gas	29,629					
... Oil	12,135					
Solar:						
Age	15,552	2.83	3.37	1.00	2.000	4.00
Capacity (MW)	15,552	10.70	31.89	1.50	3.000	5.00
Generation (GWh)	13,835	30.86	291.07	1.95	4.020	9.66

Panel B: County-Level

	Non-PE counties			PE counties		
Variable	N	Mean	SD	N	Mean	SD
Fossil-fuel:						
No. of plants	26,040	2.06	2.03	3,560	5.74	7.20
Total Capacity (MW)	26,040	472.76	834.05	3,560	1,251.61	1,654.34
Total Generation (GWh)	26,040	1,510.77	3,478.75	3,560	3,625.53	5,674.16
Solar:						
No. of plants	26,040	0.33	1.90	3,560	1.86	7.82
Total Capacity (MW)	26,040	2.89	33.22	3,560	20.54	144.88
Total Generation (GWh)	26,040	4.78	70.88	3,560	36.63	309.47

Table 2.2: PE acquisition and solar development

Table 2.2 reports the staggered differences-in-differences estimates of the effect of PE acquisition of fossil fuel on solar development. Columns (1) and (2) report estimates from the following two-way fixed effects estimation:

$$y_{c,t} = County_c + Year_t + \beta \times Post\ PE_{c,t} + \epsilon_{c,t}$$

where $County_c$ and $Year_t$ are county and year fixed effects. $Post\,PE_{c,t} = 1$ if there is a PE-acquired fossil plant in county c and year t and zero otherwise. Columns(3) to (8) follow Sun and Abraham (2021) methodology that estimates Equation (2.2) as described in Section 2.4.1. Columns (1) to (5) have *solar dummy* as the dependent variable that takes one if there is a solar development in county c and year t. Columns (6), (7), and (8) use no. of solar plants, log (solar capacity), and log(solar net generation) as the dependent variables. Columns (2) and (4) to (8) include controls for the number of fossil plants, total capacity of fossil plants, total generation from fossil plants, and the population of the county. Columns (5) to (8) also include a more stringent Regulated-state-by-year fixed effects. Standard errors are clustered at the county level; t-stats are shown in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Model	TWFE		Sun and Abraham (2021)					
	Solar Dummy					Log (plants)	Log (cap)	Log (gen)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post PE	0.1009**	0.0993**	0.0809**	0.0803**	0.0820**	0.1128***	0.4034***	0.7989***
	(2.136)	(2.057)	(2.140)	(2.095)	(2.129)	(2.714)	(2.777)	(2.726)
Controls		✓		✓	✓	✓	✓	✓
County FEs	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓				
Regulated State-Year FEs					✓	✓	✓	✓
Observations	20,712	20,367	20,712	20,367	20,367	20,367	20,367	20,367
R^2	0.54	0.55	0.54	0.55	0.55	0.51	0.54	0.48

Table 2.3: Solar radiance and PE ownership of fossil-plants

Table 2.3 reports estimates from the following equation:

$$PE\ Owned_{i,t} = Plant_i + State \times Year_{s,t} + \beta \times Solar\ Radiance_i \times Post\ 2005_t + \epsilon_{i,t}$$

where $PE\ Owned_{i,t} = 1$, if a fossil-fuel plant i is owned by institutional investors through private equity in year t, and 0 otherwise. $Solar\ Radiance_i$ is the average GHI around the area of the plant i, standardized to have a mean of 0 and standard deviation (s.d.) of 1. $Post\ 2005_t = 1$ if $year \geq 2005$ and zero otherwise. $Plant_i$ are the plant fixed-effects and $State \times Year_{s,t}$ are the state-by-year fixed effects. Column (1) shows baseline results with no controls. Column (2) controls for log of net generation (MWh) and the efficiency of power plants (kWh/BTU). Column (3) adds further control for the county-level population. Standard errors are clustered at the power plant level; t-stats are shown in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	PE Owned		
	(1)	(2)	(3)
Solar Radiance × Post 2005	0.0311**	0.0332**	0.0328**
	(2.534)	(2.531)	(2.495)
Log net generation (MWh)		0.0006	0.0006
		(0.8426)	(0.8896)
Efficiency (kWh/BTU)		0.0060	0.0062
		(0.8162)	(0.8380)
Log population			0.0058
			(0.1155)
Plant FEs	✓	✓	✓
State-Year FEs	✓	✓	✓
Observations	53,488	48,002	47,288
R^2	0.61	0.61	0.61
Outcome mean	0.0754	0.0754	0.0754

Table 2.4: Solar radiance and PE ownership of fossil-plants

Table 2.4 presents cross-sectional variation in the likelihood of PE acquisition. Specifically, it reports estimates from the following equation:

$$PE\ Owned_{i,t} = Plant_i + StateYear_{s,t} + \beta \times Solar\ Radiance_i \times Post\ 2005_t + Controls_i + \epsilon_{i,t}$$

where $PE\ Owned_{i,t} = 1$, if a fossil-fuel plant i is owned by institutional investors through private equity in year t, and 0 otherwise. $Solar\ Radiance_i$ is the average GHI around the area of the plant i, standardized to have a mean of 0 and standard deviation (s.d.) of 1. $Post\ 2005_t = 1$ if $year \geq 2005$ and zero otherwise. $\alpha_{s,t}$ is the state-year fixed effects. Columns (1) and (2) report estimates from the sub-sample of plants that were built before and after 2000 respectively. Columns (3) and (4) present results from the sub-sample of plants that have a total capacity of over 100MW and under 100MW respectively. Standard errors are clustered at the power plant level; t-stats are shown in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	PE Owned			
	Age		Size	
	New	Old	Large	Small
	(1)	(2)	(3)	(4)
Solar Radiance × Post 2005	0.0011	0.0439***	0.0117	0.0608***
	(0.0323)	(3.040)	(0.6630)	(3.194)
Controls	✓	✓	✓	✓
Plant FEs	✓	✓	✓	✓
State-Year FEs	✓	✓	✓	✓
Observations	11,961	35,327	23,984	23,304
R^2	0.67	0.59	0.61	0.63
Outcome mean	0.126	0.059	0.101	0.052

Table 2.5: Relationship between institutional investment in fossil and solar

Table 2.5 describes the number of counties with institutional investment in solar grouped by counties with PE-owned fossil plants and counties with no PE-owned fossil plants.

Institutional solar refers to counties that have at least one solar development by an institutional investor. Non-institutional solar are counties with no institutional investor in solar. Each cell repsresent number of distinct counties. Numbers in parentheses represent percentage of total counties in that row.

	Total	Solar	Non-institutional solar (% of total)	Institutional solar (% of total)
Non-PE Fossil	1060	362	212 (20%)	150 (14%)
PE Fossil	174	103	42 (24%)	61 (35%)
Total	1234	465	254 (21%)	211 (17%)

Table 2.6: PE acquisition of fossil plants and solar development by PE

Table 2.6 reports the effects of PE acquisition of fossil plant on solar development in counties with institutional investment in solar and counties with no institutional investment. Columns (1) and (3) show TWFE estimates from the Equation (2.1). Columns (2) and (4) table follows Sun and Abraham (2021) method and estimates Equation (2.2). Columns (1) and (2) present results for solar development in *Any institutional* counties, and (3) and (4) in *No institutional* counties. Standard errors are clustered at the county level; t-stats are shown in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	Institutional Solar		Non-Institutional Solar	
	TWFE	SA2021	TWFE	SA2021
	(1)	(2)	(3)	(4)
Post PE	0.0759**	0.0665**	0.0249	0.0145
	(2.041)	(2.326)	(0.7355)	(0.5733)
County FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Observations	20,712	20,712	20,712	20,712
R^2	0.54	0.54	0.49	0.49

Table 2.7: PE acquisition and solar development

Table 2.7 reports the staggered differences-in-differences effects of PE acquisition of fossil plant on solar development after matching treated counties with a control county that has similar radiance. Columns (1) and (2) present results from TWFE estimation based on Equation (2.1). Columns (3) and (4) follow Sun and Abraham (2021) method and present estimates from Equation (2.2). Columns (1) and (3) have *solar dummy* as the dependent variable that takes one if there is a solar development in county c and year t. Columns (2) and (4) have log of solar capacity as the dependent variable. Standard errors are clustered at the county level; t-stats are shown in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Model	TWFE		Sun and Abraham (2021)	
	Development	Log (cap)	Development	Log (cap)
	(1)	(2)	(3)	(4)
Post PE	0.1137**	0.2907***	0.0908**	0.1935***
	(2.372)	(3.195)	(2.319)	(3.118)
County FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Observations	3,878	3,878	3,878	3,878
R^2	0.62	0.54	0.63	0.55

Table 2.8: PE acquisition and changes in demographics

Table 2.8 reports the staggered differences-in-differences effects of PE acquisition of fossil plant on changes in the population. Columns (1) and (2) present results from TWFE estimation based on Equation (2.1). Columns (3) and (4) follow Sun and Abraham (2021) method and present estimates from Equation (2.2). Columns (1) and (3) have no controls. Columns (2) and (4) include controls for the number of fossil plants and total capacity of fossil plants in the county. The dependent variable is the change in population of a county from the previous year (in%). Standard errors are clustered at the county level; t-stats are shown in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	Population change (in %)			
Model	TWFE		Sun and Abraham (2021)	
	(1)	(2)	(3)	(4)
Post PE	-0.0281	-0.0281	0.0068	0.0068
	(-0.2835)	(-0.2828)	(0.0771)	(0.0768)
Controls		✓		✓
County FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Observations	19,273	19,273	19,273	19,273
R^2	0.43	0.43	0.43	0.43

CHAPTER 3

Effects of Creditor Rights on Bankruptcy Outcomes: Evidence from the Small Business Reorganization Act of 2019

3.1 Introduction

One of the central problems in the design of the optimal bankruptcy law is the allocation of control rights between the creditors and the debtor in bankruptcy. Increasing creditor rights disciplines management, reduces their incentives for strategic default, and increases ex-ante access to finance (Aghion and Bolton, 1992; Hart and Moore, 1994). However, excess creditor control may also lead to inefficiencies in the form of a bias towards liquidation (Aghion, Hart and Moore, 1992; Hart et al., 1997). While there is a large literature on the ex-ante effects of creditor rights, the evidence on the ex-post effects of creditor rights on bankruptcy outcomes is scarce and ambiguous.[1] This paper seeks to understand whether, and to what extent, creditors affect bankruptcy filing decisions and outcomes. Specifically, I ask: how does reducing creditor rights affect the likelihood of Chapter 11 filing and successful reorganization?

Studying the impact of creditor rights on bankruptcy is empirically challenging for two key reasons. First, it is hard to find variations in creditor rights. Creditor rights of a country depend on the legal and political environment of a country, which also affects the quality of businesses filing for bankruptcy and the bankruptcy outcomes. Therefore, cross-country comparisons are mostly descriptive. Second, firms endogenously choose their creditors. Firms with different qualities have incentives to choose different types of creditors, which in turn affect both the creditors' influence and the bankruptcy outcomes. For example, firms may

[1] Agrawal, Gonzalez-Uribe and Martinez-Correa (2019) find that increased creditor rights reduces liquidation in Danish firms. Skeel Jr (2003a) finds that increased creditors' influence leads to excess (inefficient) liquidation. Thorburn (2000) finds that the 'harshness' of bankruptcy law does not lead to excess liquidation.

choose bank debt that has restrictive covenants as a credible signal of their credit quality. As the firm quality is unobserved to an econometrician, running a naïve regression of a proxy for creditor rights on bankruptcy outcomes would be plagued with omitted variables problem.

I overcome these empirical challenges by exploiting the passage of the Small Business Reorganization Act of 2019 (SBRA henceforth) that amends Chapter 11 bankruptcy by including a new subChapter V for small businesses. SubChapter V reduces the transaction costs and the creditors' influence on the reorganization process for small businesses with a total non-contingent, liquidated debt of less than \$7.5M.[2] Under SubChapter V, only the debtor can file a plan of reorganization, and creditors are not allowed to file competing plans. Moreover, the SBRA eliminates the absolute priority rule allowing the debtor to retain interests in the business even if all the senior claims are not paid in full. This act discontinuously makes the bankruptcy regime in Chapter 11 'pro-debtor' for firms below the debt threshold, while leaving the debtor-friendliness of Chapter 7 unchanged around the threshold. I exploit this discontinuity in a regression discontinuity design and compare the bankruptcy filing decisions and outcomes of firms around this debt threshold. To the best of my knowledge, this is the first paper that uses this shock to shed light on the issue of creditors' influence on bankruptcy outcomes.

The SBRA provides a nice empirical setting to answer the questions of this paper. By reducing creditor rights in bankruptcy, the SBRA shifts the bargaining power towards the debtor. This allows us to examine whether this increases the incentives of owner-managers[3] to exert costly effort to reorganize their business by testing for the discontinuity in the likelihood of Chapter 11 filing (Chapter 11 filing / total bankruptcy filing) at the threshold. Making Chapter 11 'pro-debtor', however, may also increase the incentives of economically inefficient firms to file for Chapter 11 in order to extract rent by continuing for too long and gaining bargaining power before liquidation.[4] However, before the debt could be discharged under Chapter 11, a bankruptcy judge has to confirm the management's plan. The plan is confirmed only if it is 'feasible'[5] and meets the 'best interests test', which says that the creditors must receive as much under the plan as they would in case of a hypothetical sale of all the assets (11 U.S.C. §1129(a)). To the extent that the judges are able to correctly screen feasible plans

[2]The original debt threshold for small businesses to qualify under the SBRA was \$2,725,625. The Coronavirus Aid, Relief, and Economic Security (CARES) Act, enacted and effective March 27, 2020, temporarily increased this debt threshold to \$7.5M for one year. The Covid-19 Bankruptcy Relief Extension Act of 2021 extends the debt limit for an additional 1 year.

[3]Most of the small businesses are privately owned.

[4]Weiss and Wruck (1998) argue agency issues such as managerial rent-extraction led Eastern Airlines to continue inefficiently for too long.

[5]Feasibility means that the "confirmation of the plan is not likely to be followed by the liquidation or the need for further financial reorganization".

from the infeasible ones, these requirements allow us to test whether reducing creditor rights increase successful reorganization by studying the discontinuity in the likelihood of the plan confirmation (total confirmed plan / total bankruptcy filing) at the threshold.

I find that the introduction of the SBRA discontinuously increases the likelihood of Chapter 11 filing by around 20% (s.d. of 5.2%) at the threshold. This suggests that making bankruptcy pro-debtor induces more firms to file for Chapter 11. However, I do not find a statistically significant discontinuity in the likelihood of plan confirmation at the threshold after the introduction of the act. The discontinuity in the likelihood of plan confirmation is only around 8% and is not statistically significant (s.d. of 4%). This is expected as reducing the costs of bankruptcy and creditors' influence should encourage both economically efficient and inefficient firms to file for Chapter 11. To further isolate the effects of creditor's influence on bankruptcy, I study the cross-sectional variation in the effects of the SBRA on firms with different extents of creditor control through a 'difference-in-discontinuities' approach. The idea is that while the SBRA reduces the administrative costs equally for everyone, reducing creditor's influence should have higher effects on firms with greater creditor control. I split the sample into firms with different measures of creditor control and study the heterogeneity in the treatment effects of the Act.

Secured creditors are known to have greater control rights and influence on the bankruptcy due to their liens on the collateral and post-petition financing of the firm. Secured creditors are also higher in priority than other creditors, and are therefore more biased towards liquidation, which follows the absolute priority rule (APR). Consistent with this, I find that for firms with more secured debt, reducing creditors' influence has a higher effect on the the plan confirmation. Specifically, for firms with above-median secured debt to total debt ratio, the act increases the Chapter 11 filing by 19.8% (s.d. of 6.7%) and the likelihood of plan confirmation by 15.8% (s.d. of 5.1%) at the threshold. These discontinuities are insignificant in the below-median secured debt to total debt sub-sample. This result contributes to the growing literature in bankruptcy that documents 'pervasive' control of secured creditors (Baird and Rasmussen, 2002; Skeel Jr, 2003a).

I explore two mechanisms to explain this cross-sectional variation in the treatment effect of the SBRA. First, I argue that secured creditors use their excess market power, influence, and resources to object to reorganization plans that leads to fewer plan confirmation and successful reorganization under regular Chapter 11. Reducing their rights-in-bankruptcy therefore increases the number of successful reorganization. The market power of secured creditors, and consequently, their control on bankruptcy is higher when their claims exceed the value of the collateral. Such 'under-secured' creditors can object to the reorganization on the grounds of ‘a lack of adequate protection’.[6] Moreover, such firms find obtaining external debtor-in-possession (DIP) financing more difficult, as they do not have excess collateral to pledge and need to rely on the existing secured creditors for liquidity needs. This exacerbates the financial constraints problem and causes inefficiencies in the bargaining between the firm and the secured creditors. Secured creditors gain more control by providing the DIP financing and use these financing agreements to influence the reorganization process (Skeel Jr, 2003a).

To the extent that under-secured creditors are able to exercise more control over the bankruptcy, we should expect that reducing creditors' influence has higher effects on firms with under-secured creditors. Consistent with this hypothesis, I find that the SBRA discontinuously increases the likelihood of plan confirmation by 16.3% (s.d. of 5.4%) for under-secured firm, but does not have similar effects on the over-secured firms. This difference in discontinuity is not present for unsecured debt to assets ratio, ruling out the alternative explanation that the effect comes from variations in leverage. Not all secured creditors can yield similar influence on bankruptcy. Banks, for example, have an expertise in monitoring, enforcement of debt covenants, and post-petition financing and negotiations.[7] In line with this, I find some evidence that banks have considerable influence on bankruptcy outcomes. For firms with a bank as one of their largest lenders, the Act increases the plan confirmation by around 35% (s.d. of 12.5%) below the threshold. These results suggest that creditors with enough market power and influence can cause fewer successful reorganizations when creditor rights are high.

Second, the presence of secured creditors exacerbate the renegotiation frictions among creditors. I find that firms with more creditors and dispersed claims are more likely to have their reorganization plans confirmed after the SBRA. For firms with more than 50 creditors, the provision of Sub Chapter V increases the likelihood of plan confirmation by 40%. Similarly, for claims that are dispersed across the creditors (debt HHI below median), Sub Chapter V increases plan confirmation by 44%. These effects are statistically insignificant for firms with fewer than 50 creditors and more concentrated claims. Moreover, I find that this heterogeneity is also only present when the majority of the firm's debt is secured. These results support the theory that secured creditors, in the presence of other dispersed creditors, suffer from higher conflicts of interest and information asymmetry that cause coordination failure. Such frictions make contract renegotiation more difficult and reduce the probability of plan confirmation (Berglöf et al., 2000).

This paper contributes to several strands of literature on bankruptcy and corporate finance. First, it is rooted in the literature on the effects of creditors' control rights on bankruptcy.

[6]Adequate protection is defined as the right of a secured creditor to receive protection against the decrease in value of its interest in the debtor's property during bankruptcy proceedings (§362(d)(1))

[7]Banks have dedicated personnel to negotiate and enforce loan contracts and lawyers to object to the debtor's plan and file a motion for relief from the automatic stay.

Several papers have documented a growing influence of secured creditors on the bankruptcy process in the US (Baird and Rasmussen, 2002; Skeel Jr, 2003a; Ayotte and Morrison, 2009). However, there is no consensus on the effects of the creditor control on the efficiency of reorganization outcomes. Baird and Rasmussen (2002) find that increased creditor control allows creditors to screen efficient firms and reduces inefficient continuation of non-viable firms. On the other hand, Skeel Jr (2003a) finds that increased creditors' influence leads to inefficient liquidation. Ayotte and Morrison (2009) find "pervasive" creditor control in the large, listed companies filing for Chapter 11, and show that creditor conflicts cause inefficient liquidation when secured creditors are in control and inefficient continuation when unsecured creditors are in control. Similar to Skeel Jr (2003a) and Ayotte and Morrison (2009), I find that secured creditors' control causes excess liquidation for small businesses. Such creditor influence not only affects reorganization outcomes but also deters businesses from filing for Chapter 11. However, unlike Skeel Jr (2003a) and Ayotte and Morrison (2009) who provide descriptive evidence, I present causal evidence of reducing creditor rights on bankruptcy outcomes. The only other related paper that provides a causal estimate of the effects of creditor rights on reorganization is Agrawal, Gonzalez-Uribe and Martinez-Correa (2019) that studies reorganization by Danish firms. Contrary to my results, however, Agrawal, Gonzalez-Uribe and Martinez-Correa (2019) find that increasing creditor rights in Denmark increases reorganization and reduces liquidation. This highlights the differences in the legal system and the extent of creditor control in the two countries.

This paper also contributes to the literature on the effects of debt ownership structure on bankruptcy outcomes. One strand of literature shows that dispersed creditors cause coordination failure and holdout problems (Gertner and Scharfstein, 1991; Bolton and Scharfstein, 1996) that may prevent efficient reorganization and cause excess liquidation. On the other hand, Bris and Welch (2005) argue that dispersed creditors also suffer from a free-riding problem that reduces the incentives for any creditor to exert costly effort to monitor or actively oppose management's reorganization plans, thereby making it easier for a firm to reorganize. My paper provides empirical evidence that the presence of coordination frictions among creditors hampers successful reorganization supporting the theories presented in Gertner and Scharfstein (1991); Bolton and Scharfstein (1996).

Last, much of the extant empirical work in bankruptcy focuses on large businesses mostly due to the lack of availability of small business data and empirical identification challenges. For example, the median firm that Ayotte and Morrison (2009) analyze has total assets of \$151 million before filing for bankruptcy. However, more than 50% of businesses that filed for federal bankruptcy in 2020 had assets under \$1 million, liabilities under \$10 million, and fewer than 10 employees [8]. In addition, small businesses suffer from greater frictions such as higher information asymmetry, financial constraints and excess market power of lenders over the firm. However, empirical evidence on the bankruptcy decisions and outcomes of small businesses is scarce. Bris, Welch and Zhu (2006) analyze a sample of both large and small businesses and find that Chapter 11 allows creditors to recover more than Chapter 7, but is costlier. They also show that the presence of secured creditors is associated with more Chapter 11 filings. They present this fact as the presence of coordination frictions among secured creditors, with debtors recognizing that Chapter 11 could overcome these coordination problems. Similarly, Morrison (2009) studies small businesses' decision to file for federal bankruptcy instead of using state workout law. The paper finds that a small business is more likely to file for federal bankruptcy (Chapter 7 and Chapter 11) when the business has secured debt and multiple senior lenders. I contribute to this literature by showing that the presence of secured creditors and multiple lenders not only affect the likelihood of filing for Chapter 11, but it also affects the likelihood of successful reorganization under Chapter 11 when creditors have higher control rights. I show that reducing creditor rights increases the likelihood of plan confirmation more for firms that have more secured creditors, and multiple lenders.

The remainder of the paper is organized as follows. Section 2 provides institutional details about the SBRA. Section 3 discusses the empirical design and identification strategy. Section 4 describes the data. Section 5 presents my results and section 6 concludes.

3.2 Institutional Details

3.2.1 The Small Business Reorganization Act (SBRA) of 2019

The Small Business Reorganization Act of 2019 (the "SBRA") was enacted on August 23, 2019, and took effect on February 19, 2020.[9] The SBRA enacts a new subChapter V of Chapter 11 of the Bankruptcy Code, codified as new 11 U.S.C. §§ 1181 – 1195. The act defines a 'small business debtor' as a person engaged in commercial or business activities that has an aggregate noncontingent liquidated debt of no more than $2,725,625 not less than 50% of which arose from the commercial or business activities of the debtor. The Coronavirus Aid, Relief, and Economic Security Act (the "CARES Act"), enacted and effective March 27, 2020, amended the SBRA to increase the debt limit for purposes of subChapter V to $7.5 million for one year. The Covid-19 Bankruptcy Relief Extension Act of 2021 amended the

[8]Source: bankruptcydata.com.

[9]For details, please see A Guide to the Small Business Reorganization Act of 2019, available on Judge Bonapfel's chambers website, http://www.ganb.uscourts.gov/content/honorable-paul-w-bonapfel.

CARES Act to further extend the increased debt limit for an additional year. This act aims "to streamline the process by which small business debtors reorganize"[10] and allows the small businesses "to file bankruptcy in a timely, cost-effective manner" which "not only benefits the owners, but employees, suppliers, customers, and others who rely on that business"[11].

The provision of subChapter V makes the bankruptcy 'pro-debtor' and gives the debtor significant control over the reorganization process. SubChapter V allows the debtor to remain in possession of the assets and operate the business unless the court removes the debtor (§ 1184). Instead of senior creditors supervising the reorganization process, a subChapter V trustee is appointed to monitor the case and to facilitate the confirmation of a consensual plan (§ 1183). Under the SBRA, only the debtor can file a plan of reorganization, and the debtor must do so within 90 days of the order for relief (§ 1189). These changes reduce the threat small business owners face of losing the ownership of the firm, allow them to keep a higher share of the going-concern surplus, and, consequently, incentivize them to exert costly effort to reorganize.

SubChapter V also significantly reduces the transaction costs of reorganization for small businesses. First, a committee of unsecured creditors is not appointed unless the court orders otherwise (§ 1102(a)(3)). Second, the debtor is not required to file a disclosure statement that provides adequate information to creditors (§ 1181). SubChapter V debtor is also exempt from paying the US Trustee quarterly fees (§ 1930). Other administrative expenses such as fees for professionals employed by the debtor and the trustee and claims for goods received by the debtor within 20 days before the petition can be paid throughout the plan (§ 1191(e)). In a general Chapter 11 case, the debtors are required to pay these claims upfront on the effective date of the plan (§ 1129(a)(9)(A)). These modifications substantially reduce the transaction costs of Chapter 11 filing and decrease the post-petition DIP financing burden for firms. As secured lenders, such as banks, usually use such DIP financing agreements to gain control over the bankruptcy process, the SBRA also reduces the effect of such creditors' control on bankruptcy procedure.

While the act makes it easier and less costly for firms to file for Chapter 11, it retains much of the Chapter 11 requirements on the contents and the confirmation of the reorganization plan. Although the debtor is not required to prepare a disclosure statement, the plan must include a brief history of the business operations of the debtor, a liquidation analysis, and projections regarding the ability of the debtor to make payments under the proposed plan (§

[10] H.R. REP. NO. 116-171, at 1 (2019), available at https://www.govinfo.gov/content/pkg/CRPT116hrpt171/pdf/CRPT-116hrpt171.pdf.

[11] Unofficial Transcript of Oversight of Bankruptcy Law and Legislative Proposals: Hearing Before the Subcommittee on Antitrust, Commercial, & Admin. Law of the H. Comm. on the Judiciary, 116^{th} Cong. 27 (2019) (on file with H. Comm. on the Judiciary staff).

1190 (1)). While a plan in a subChapter V case can be confirmed without the consent of any class of creditors, the plan must meet the other requirements for confirmation as stated in § 1129. First, the plan must be feasible under § 1129(a)(11) in that the confirmation of the plan is not likely to be followed by the liquidation, or the need for further financial reorganization, of the debtor unless such liquidation or reorganization is proposed in the plan. The plan also must meet the 'best interests test' pursuant to § 1129(a)(7), which states that each holder of an impaired claim should receive under the plan an amount that is not less than what they would so receive if the debtor were liquidated under Chapter 7 on that date. Third, the plan should be 'fair and equitable' to secured creditors, which means that the holders of secured claims should retain their liens and receive deferred cash payments whose net present value is at least as much as the current value of such holder's collateral (§ 1129(b)(2)(A)). These requirements allow the judge to screen efficient firms looking to reorganize from the inefficient firms trying to extract rent by filing for Chapter 11.

To the extent that the judge's ability to screen efficient firms from inefficient ones do not discontinuously change at the threshold, we should expect to see a discontinuous increase in plan confirmation at the threshold only if the jump in Chapter 11 filing at the threshold is by economically efficient firms. Consider an extreme case in which the jump in Chapter 11 filing at the threshold is because of inefficient firms filing for Chapter 11 in order to extract rent from the creditors. If judges are able to correctly screen them, the plans of these inefficient firms will not be confirmed. Consequently, we will not see a discontinuity in the likelihood of plan confirmation (total number of plan confirmation / total bankruptcy filings). In the other extreme case, if all the firms that filed for Chapter 11 as a result of increased debtor-friendliness were economically efficient, and if judges are able to correctly screen them as efficient, all of these plans will be confirmed. In such a case, the discontinuity estimate in the likelihood of plan confirmation at the threshold would be same as that of Chapter 11 filing. Therefore, testing for the discontinuity in both Chapter 11 filing and plan confirmation at the threshold gives us a better picture of the effect of the act on bankruptcy outcomes.

3.3 Empirical Design

I use a regression discontinuity framework (Imbens and Lemieux, 2008; Lee and Lemieux, 2010) to estimate the effects of the passage of the SBRA on the likelihood of Chapter 11 filing and plan confirmation for firms around the debt threshold of \$7.5M. Under the identifying assumption that all the factors that affect the outcome variables vary continuously across this debt threshold, any discontinuity in the outcome at the threshold can be attributed to the passage of the SBRA. I estimate this discontinuity using the following reduced-form equation:

$$Y_i = \alpha + \tau D_i + \beta X_i + \delta D_i * X_i + \theta C_i + \epsilon_i \tag{3.1}$$

where D is the dummy variable that indicates whether a firm is eligible for SubChapter V (net liabilities is less than \$7.5M). The running variable, X is the log of the net liabilities of the firm. C represents the covariates that I use in robustness checks. I allow the slopes of the fit to be different on both sides of the threshold by including an interaction term between the the running variable X and the dummy variable D. ϵ is the error term. τ is the coefficient of interest that estimates the discontinuity in the outcome variable at the threshold. This coefficient gives the local average effect of the act on the outcome variables.

The outcome variables, Y are the likelihood of Chapter 11 filing and the likelihood of plan confirmation. In an ideal world, we would like to calculate the likelihood of Chapter 11 filing (plan confirmation) as the number of Chapter 11 filing (confirmed plans) / all financially distressed firms looking to reorganize/liquidate. However, the data on all small businesses that are financially distressed is not available. Therefore, I limit my study to firms that file for any federal bankruptcy. As more than 99% of businesses file for either Chapter 7 or Chapter 11, I limit my study to these filings and calculate the outcome variables as follows:

$$\text{Chapter 11 filing} = \frac{\text{number of Chapter 11 filings}}{\text{total bankruptcy filings (Chapter 11 + Chapter 7)}}$$

and

$$\text{Plan confirmation} = \frac{\text{number of confirmed Chapter 11 plans}}{\text{total bankruptcy filings (Chapter 11 + Chapter 7)}}$$

To better understand the sources of the effects of the SBRA on bankruptcy filings and outcomes, I study the heterogeneity in the treatment effect across firms with different extents of creditor control and coordination frictions. There are two ways this is done in the RD literature. One common practice is to include an interaction term between the discontinuity dummy and the additional control of interest. This parametric method severely over-rejects under the model misspecification even if only the observations close to the cut-off are used (Hsu and Shen, 2019). A non-parametric method with better performance is to conduct a sub-sample regression discontinuity by splitting the sample in groups of interest and conducting separate RD analyses on them. I use this sub-sample regression discontinuity method to study the cross-sectional variation. However, specifications using interaction terms also give similar results.

3.4 Data

This study consists of all business bankruptcies of firms with total liabilities between $1M and $50M that were filed between April 1, 2020 and December 31, 2020. There are a total of 1471 firms in my sample. The primary data on business bankruptcy is obtained from the Integrated Database of the Federal Judicial Center (FJC). This database contains data on all bankruptcy case information reported by the Administrative Office of the United States Courts (AOUSC). It includes primary case-related information such as (i) date and Chapter of filing; (ii) financial characteristics of the firms such as total assets and liabilities; and (iii) breakdown of total liabilities in secured, unsecured priority and unsecured nonpriority claims. However, it does not include information about the company such as the name, industry and address of the firm. I use the "BankruptcyData" (BD), by New Generation Research, to supplement the data obtained from the FJC. In addition to firm-level information, this database also contains information on whether the firm filed for SubChapter V and includes updated status of the outcomes of the bankruptcy filings. To obtain claims-related information and breakdown of the creditors, I use the bankruptcy petition filed by the firms and available through Public Access to Court Electronic Records (PACER). Businesses that file for Chapter 7 or Chapter 11 bankruptcy are required to file the "Schedules of Assets and Liabilities" (Official Form 206) and report their assets and liabilities and list their largest secured and unsecured creditors and their total claims. I use the petition documents to collect information on the largest creditors of the firms. Specifically, I collect the (i) name of the creditor; (ii) the type of the claims, such as loans, wage, or trade debt; (iii) the lender type, such as banks, credit card companies, or financial services companies; and (iv) their total claims. I was able to collect these creditor-level information on 506 firms, which is around 34% of my total sample.

3.4.1 Summary Statistics

Table 3.1 reports the summary statistics of the financial characteristics of firms filing for Chapter 11 and Chapter 7 in my sample. On average, firms filing for Chapter 7 are much smaller in size of their assets and liabilities than Chapter 11 firms. The average Chapter 7 firm in my sample has total assets of $ 1.3 million (median of $94 thousand) as compared to $6.07 million (median of $1.7 million) for Chapter 11 firms. Chapter 7 firms have average (median) net liabilities of $6.2 million ($2.5 million) as compared to $10.2 million ($4.29 million) for firms filing for Chapter 11. Firms filing for Chapter 7 are significantly more leveraged than Chapter 11. The median Chapter 11 firm in my sample has debt to assets ratio of around 2 whereas that for Chapter 7 firm is around 30. This is not surprising as firms with higher leverage are typically more insolvent. Such firms are less likely to put in costly effort to salvage the firm and would instead look for quick discharge of their liabilities. In contrast to the debt/assets ratio, the secured debt / assets ratio of a median Chapter 7 firm is 0.83, slightly less than 0.96 of a median Chapter 11 firm . Firms looking to reorganize (Chapter 11 firms) also have more secured debt than liquidating firms. Median Chapter 11 firm has 78% of their total debt as secured, whereas the median Chapter 7 firm only has 7%. These findings are consistent with the idea that the incentives of small businesses to liquidate are higher when they have more unsecured debt. In such cases, they can get higher fraction of their debt discharged without significant losses to their assets. In contrast, small businesses with more secured debt do not have the same incentives to file for Chapter 7, as liquidation does not completely discharge the secured debt and the creditors are allowed to sell their collateral to recover their loans.

Table 3.2 provides information about the composition and type of creditors in my sample. On average, firms filing for Chapter 7 have fewer creditors and more concentrated claims. 63% of firms filing for Chapter 7 have fewer than 50 creditors, compared to 74% for Chapter 11 firms. However, in the sub-sample of firms that report their claims-level information, firms filing for Chapter 7 have relatively more concentrated claims. The debt HHI for Chapter 7 firms is around 50% whereas that for Chapter 11 firms is around 39%. The composition of their largest creditors are similar for firms filing for both Chapters. On average, 20% of these firms have a bank as their largest creditor; 50-60% have a bank as one of their 20 largest creditors; around 20% have a credit card company as one of their largest creditors; and around 25% have a financial services company as one of their largest creditors. These findings show support for the conventional wisdom that majority of small businesses are financed by banks and other financial services companies.

Table 3.3 compares the current status and outcomes of bankruptcy filings for general Chapter 11, subChapter V of Chapter 11 (sub V) and Chapter 7 cases. These outcomes are as of June 1, 2021. The reorganization plans of around 35% of sub V cases are confirmed as compared to only 20% of general Chapter 11 filings. This result is in line with the objective of the sub V to provide a 'streamlined reorganization process' for efficient small businesses. Moreover, only 14% of sub V cases are already dismissed as compared to 27% of Chapter 11 cases, almost double. To the extent that the plan dismissal is a sign of inefficient firms filing for Chapter 11, this shows that the introduction sub V cases do not increase the number of inefficient firms trying to reorganize. A large proportion of these cases have still not reached an outcome and are in the intermediary processes. Around 30-40% of all filings in my sample either have an upcoming deadline for the creditors to file for claims, object to the discharge, or attend the 341 creditors meeting.

3.5 Results

This section presents my empirical results. Figure 3.1 graphically shows the discontinuity in the likelihood of Chapter 11 filing at the threshold. Figure 3.1a takes the linear functional form to estimate the relationship between the running variable and the outcome, whereas figure 3.1b takes a non-parametric form. The figures show a clear discontinuous increase in Chapter 11 filing below the threshold. Panel A of Table 3.4 presents regression-based discontinuity estimates of the effects of reducing the creditors influence and bankruptcy costs on the likelihood of filing for Chapter 11 using different bandwidths. The first column is the baseline result with full sample; the second column uses a bandwidth of 1.5; the third uses Imbens and Kalyanaraman (2012) optimal bandwidth; and the fourth column uses a bandwidth of 1. A bandwidth of 1 means that the sample is restricted to firms with log of net liabilities between -1 and +1. The likelihood of Chapter 11 filing discontinuously increases by 18-22% (s.d. of 5-7%) below the threshold. The results are robust to different bandwidths. This discontinuity in the fraction of Chapter 11 filing suggest that the act encouraged more small businesses to file for Chapter 11.

An increase in Chapter 11 filing after bankruptcy becomes 'pro-debtor' is not surprising. Reducing the bankruptcy costs increases the going-concern surplus and, consequently, the incentives to reorganize for both efficient and inefficient firms. Therefore, a jump in Chapter 11 filing at the threshold does not imply an increase in efficient continuation of small businesses. For a business to reorganize under Chapter 11 and continue, the bankruptcy judge has to confirm the management's reorganization plan. As I argue in Section 2, a judge confirms the management plan only if it deems the plan to be feasible and the business to be economically efficient. If the judge is correctly able to filter efficient firms from the inefficient ones, we should expect to see a discontinuity in the plan confirmation at the threshold only if more efficient firms file for Chapter 11. Figure 3.2 presents the discontinuity in the plan confirmation at the threshold. Panel B of table 3.4 shows the regression discontinuity estimates of the effects of the act on the plan confirmation. I find a 8% increase in plan confirmation (s.d. of4%) at the threshold in the baseline specification (column 1).

However, this estimate is not statistically significant and is also not robust to other specifications. The large standard errors with respect to the coefficient estimate can be attributed to the fact that an RD design typically requires a much larger sample to get the same power as a natural experiment (Schochet, 2008). As the number of observations in my study is limited, this result may show a lack of conclusive evidence. Besides the statistical insignificance, however, the economic magnitude of the discontinuity (around 8%) is also less than half of the discontinuity in Chapter 11 filing. This result may suggest that around half of the increased Chapter 11 filing is by economically inefficient firms trying to extract rent. Therefore, on average, the SBRA seems to have induced both efficient and inefficient firms to file for Chapter 11.

3.5.1 Cross-sectional variation in the effects of the act on bankruptcy

The previous part of this section estimates the average effects of reducing creditor's influence on bankruptcy outcomes. This average effect, however, masks important cross-sectional variations across firms with different extents of creditor control. Since the SBRA discontinuously reduces the creditor's control on bankruptcy, the effects of the Act on bankruptcy filing decisions and outcomes should differ for firms with different creditor control. Specifically, firms that suffer from higher creditor control should see larger effects of reducing creditor rights under SBRA on Chapter 11 filings and plan confirmation. In this subsection, I test this hypothesis by splitting the sample across different measures of creditor control and studying the heterogeneity in the discontinuity in the subgroups.

Creditors with different seniority and security have different rights-in-default and preferences. Secured creditors have higher control rights and influence on the bankruptcy due to their post-petition financing of the firm, liens on the collateral, and priority in claims. Secured creditors, such as banks, extend secured lines of credit during the bankruptcy that limit the debtor's access to cash and impose requirements on their operations (Skeel Jr, 2003b; Ayotte and Morrison, 2009). Due to their liens on the collateral, they also have the ability to object to the management's reorganization plan on grounds of lack of adequate protection. Secured creditors are also higher in priority than other creditors and are therefore more biased towards liquidation, which follows the absolute priority rule (APR). On the other hand, unsecured creditors have relatively less control and more aligned incentives with the equity holders, as they only receive payments after the secured creditors. Therefore, if secured creditors have more influence on bankruptcy and are more biased towards liquidation, reducing creditors' rights should increase reorganization more for firms that have more secured creditors. I test this hypothesis first graphically in Figure 3.3. The figure shows a clear discontinuity in both Chapter 11 filing and plan confirmation for firms with above-median secured debt / total debt. No such discontinuity is visually evident in the below-median sub-sample. I estimate these discontinuities in panel A of table 3.5 that shows the heterogeneous effect of the act on the subgroups of firms with different extents of creditor control. The first two columns estimate the discontinuity in Chapter 11 filing and the next two in the plan confirmation. Panel A splits the sample of observations at the median value of secured debt to total debt ratio (median = 0.61). For firms with above-median secured debt to total debt ratio, reducing creditors influence increases Chapter 11 filing by 20% (s.d. of 6.7%) and the plan confirmation by 16% (s.d. of 5%) at the threshold. These effects are statistically insignificant for firms with below-median secured debt to total debt ratio. This heterogeneity in the effect shows that reducing creditor's control increases successful reorganization in firms that have more secured creditors.

I further examine the mechanisms through which secured creditors influence bankruptcy outcomes. First, I test whether it is the excess market power of secured creditors over the debtor that cause fewer continuation. When the creditors are under-secured, i.e., the current value of their collateral is less than their secured claims on them, they have more market power and control over the bankruptcy process. Firms with under-secured creditors find obtaining external debtor-in-possession (DIP) financing more difficult, as they do not have excess collateral to pledge, and have to rely on the existing secured creditors for liquidity[12]. Secured creditors gain more control and market power by providing the DIP financing and use these financing agreements to influence the reorganization process (Skeel Jr, 2003b). Such creditors can also seek a relief from the automatic stay if they can show a lack of adequate protection (§ 362(d)(1)) and gain control of the assets forcing the firm to liquidate. If secured creditors impede successful reorganization through their excess market power and control, we should expect that reducing their influence has higher effects on firms with more under-secured creditors.

Panel B of table 3.5 shows the cross-sectional variation in the effects of the act across firms with different secured credit to total assets ratio by splitting the sample at the median value (median of 0.95). Firms with above-median secured debt to total assets ratio exhibit an increase of 17% (s.d. of 5.3%) in plan confirmation at the threshold. In contrast, the act does not have statistically or economically significant effect on the plan confirmation for the below-median firms. I graphically show this variation in the discontinuity in figure 3.4. I rule out the alternative explanation that the cross-sectional variation present in Panel B is due to the leverage or solvency of the firm by testing for the variation in firms with different unsecured debt to assets ratio. Unlike secured leverage, firms with high and low unsecured debt to assets ratio does not exhibit similar cross-sectional variation. Panel C of table 3.5 splits the sample at the median unsecured debt to total assets ratio (median of 1.5). There is no significant increase in plan confirmation at the threshold for firms with above-median unsecured debt to assets ratio. If the variation in plan confirmation, as shown in Panel B,

[12]Existing creditors obtain market power during the course of lending relationships from the private information it collects about the firm (Petersen and Rajan, 1994b). External lenders are unwilling to lend due to higher information asymmetry.

was just due to leverage, we should have seen similar effects in Panel C.

Not all creditors can yield a similar influence on the bankruptcy process. Secured creditors such as banks have expertise in the monitoring of their borrowers and enforcement of their debt covenants. They also have dedicated personnel to represent them in bankruptcy courts, object to the plans that are not favorable to them, and file motions for relief from the automatic stay. Such lenders have higher market power and control over the bankruptcy process. If the market power of the creditors leads to fewer successful reorganizations, reducing the creditor's control should increase the plan confirmation more for firms that have such secured creditors as banks. I test this hypothesis in table 3.7 and find that when the firm has a bank as one of the main lenders, the SBRA increases plan confirmation by around 23% (s.d. of 8.4%). This further strengthens my argument that secured creditors with excess market power cause fewer successful reorganizations.

Next, I test for the presence of renegotiation frictions by examining the heterogeneity across firms with different debt ownership structure. In particular, the number and concentration of creditors have a potential to influence the coordination among them and their incentives to exert costly effort in reorganization. There are two competing theories on how the debt ownership structure may affect the bankruptcy outcomes. First, dispersed and more creditors suffer from higher conflicts of interest and information asymmetry that makes contract renegotiation more difficult. Such coordination failure and holdout problems may reduce the probability of confirmation of management's reorganization plans and cause excess liquidation (Berglöf et al., 2000). Dispersed creditors, however, also suffer from a free-riding problem that reduces the incentives for any creditor to exert costly effort to monitor or actively oppose management's reorganization plans. This free-riding problem implies that multiple creditors may cause excess continuation (Bris and Welch, 2005).

I test for these competing theories in table 3.6 that presents the cross-sectional variation in the effects of the act on firms with different number and concentration of creditors. First, I split the sample in subgroups of firms with fewer and more than 50 creditors in Panel A of table 3.6. The first two columns estimate the discontinuity in Chapter 11 filing and the next two in the plan confirmation. For both the sub-sample, the act increases the Chapter 11 filing by around 20-21% (s.d. of 6-9%). However, the effects of the act on plan confirmation are significantly different. For firms with more than 50 creditors, the act discontinuously increases the plan confirmation by around 18-19% (s.d. of 7%). This effect is both economically and statistically insignificant for firms with fewer than 50 creditors. This shows support for the coordination-failure theory as suggested in Berglöf et al. (2000).

The number of creditors, in itself, may not be a complete measure of coordination frictions among creditors. Many businesses have small claims from multiple suppliers, customers, and employees. As creditor's control rights depend on their relative amount of claims, these small creditors do not have the same influence as large lenders. To ensure that the variation in the effects on different number of creditors do not reflect the presence of small claimants, I calculate a measure of debt concentration following the Herfindahl–Hirschman Index (HHI), a common measure of market concentration. I calculate the debt HHI for each firm as:

$$hhi = \sum(\frac{x_i}{\Sigma x_i})^2$$

where i represents each of the top 20 largest creditors and x_i represents the claims of creditor i. Firms with high debt HHI have dispersed debt, while those with low debt HHI have concentrated debt. Panel B of table 3.6 splits the sample in firms with above-median and below-median debt HHI. Similar to the results from the number of creditors, this table suggests a significant heterogeneity in plan confirmation across firms with different debt concentration. The plan confirmation jumps by around 26-27% (s.d. of 9%) for firms with dispersed debt, while this effect is insignificant, both economically and statistically, for firms with concentrated debt. These findings suggest that when creditors have higher control, firms with dispersed creditors may suffer from coordination failure that may impede successful reorganization.

I show so far, in separate analyses, that reducing creditor control increases successful reorganization when the firm has more secured creditors (as compared to unsecured creditors), more under-secured claims, and a higher number of creditors. In order to put them together and speak about the mechanisms through which creditors influence bankruptcy outcomes, I study the interaction of these separate effects in table 3.8. Panel A shows the discontinuity in Chapter 11 filing and panel B in plan confirmation. Column 1 restricts the sample to firms that have secured debt to total debt of more than 50%. For this subgroup of firms, the plan confirmation increases by around 16% (s.d. of 5%) at the threshold. When I further restrict the sample to include only firms with under-secured creditors in column 2, this discontinuity increases to around 23% (s.d. of 6%). Column 3 is the sub-sample of firms with secured debt / total debt $>$ 50% and number of creditors $>$ 50. This column shows that the plan confirmation jumps by around 43% (s.d. of 12%) at the threshold.

The findings of this table first suggest that the secured creditors have enough market power and resources to exert influence on the bankruptcy outcomes; therefore, reducing creditor rights has effects on plan confirmation only in the presence of secured creditors. There are two underlying mechanisms through which secured creditors impede successful reorganization when they have higher control rights. First, their excess market power and 'dominant security interests' allow them to object to Chapter 11 plans and cause fewer successful reorganization.

Second, in the presence of multiple creditors, the secured creditors suffer from higher conflicts of interest and coordination failure that make contract renegotiation more difficult and result in excess liquidation. Reducing creditor rights in bankruptcy reduces these problems and leads to more plan confirmations.

3.5.2 RD Validity

One of the main assumptions concerning the validity of the RD design is that each individual has imprecise control over the assignment variable. In my setting, this means that the firms should not be able to precisely manipulate their total liabilities around the debt threshold. A standard test used for this in the RD literature is McCrary (2008) that tests for any discontinuity in the density of the observations at the threshold. However, this test is not very helpful in my setting because the idea of this paper is that reducing creditors' influence and costs of bankruptcy encourages more firms to file for Chapter 11. Therefore, a jump in the density below the threshold may just appear from more firms filing for Chapter 11. This is what I find. Figure 3.8 plots the combined density of Chapter 11 and Chapter 7 and shows an increase in total number of bankruptcy filings at the threshold. Figures 3.8b and 3.8c separately plots the density of firms filing for Chapter 7 and Chapter 11 respectively. As the figures suggest, there is an increase in the number of Chapter 11 filing at the threshold but no such increase in Chapter 7.

While a continuous density is neither necessary nor sufficient for identification, a discontinuity in the density may suggest possible manipulations by the firm. One manipulation concern is that firms just above the debt threshold may actively 'offload' their debt in order to be eligible for subChapter V. However, there are several reasons to believe that such manipulation is unlikely. First, the original debt eligibility for the subChapter V was \$2.75M which was increased to \$7.5M under the CARES act. While the SBRA 2019 may have been anticipated by the small businesses, the passage of the CARES act could not have been. Therefore, firms at the debt threshold of \$7.5M did not have enough time to actively 'manipulate' their reported liabilities before filing for Chapter 11. Second, a financially distressed firm may not have enough liquidity to pay off its debt right before filing for bankruptcy. Third, firms looking to file for bankruptcy usually tend to take on more debt (debt loading) rather than repay their debt. Last, the bankruptcy filing also requires the debtor to attest that it has not actively changed its financial structure with an intention to file for bankruptcy. Any such change may be considered "presumptive fraud" and can result in fines and imprisonment for the debtor (§§152, 1341, 1519, and 3571). Therefore, it is unlikely that firms are able to precisely manipulate their reported net liabilities. As a further test to alleviate the concerns of manipulation, I separately test for density discontinuity in firms filing for bankruptcy in the first 6 months of the SBRA versus the rest of the period. If the discontinuity in the density was because of firms paying off some debt in order to be eligible for the SBRA, we should expect the discontinuity to be higher in the later period. Figures xx plots this density discontinuity. As the figure suggests, there is no significant difference in density discontinuity in the first 6 months compared to the later period.

However, even with no threat of manipulation, Barreca, Lindo and Waddell (2016) note that a heaping at the threshold may bias the estimate. Therefore, following their recommendation, I employ a 'donut RD regression'[13] that excludes the observations that are very close to the threshold. In my setting, I drop the firms whose net liabilities are in the range of \$7.2M and \$7.5M. This excludes a total of 21 observations. All the results are robust to inclusion of these points.

A more informative test for possible manipulation in my setting is the test of discontinuity in the covariates at the threshold. If firms with certain characteristics are able to precisely manipulate the running variable, we should expect to see imbalances in the measure of those characteristics at the cut-off. Such discontinuity in covariates may bias the results if the covariates are also a determinant of the outcome variable. I test for discontinuity in a set of covariates that may affect the outcome variable and find no significant discontinuity. Figure 3.9 plots the discontinuity in these covariates. As the figures show, there is no significant discontinuity in any covariates that may affect the validity of the RD design.

3.5.3 Robustness checks

I conduct several robustness checks to ensure that the results are valid and not spurious. First, I conduct a placebo test that checks for any discontinuity away from the threshold. If we find significant discontinuity at points away from the cut-off, we may have reasons to believe that the significant discontinuity at the threshold is due to some other reasons. Figure 3.10 shows the discontinuity estimate for Chapter 11 filing at points away from the running variable. As the figure suggests, there is no other point away from the cut-off at which this discontinuity is significant.

I conduct another falsification test that checks for any discontinuity at the threshold in the time period before the SBRA was enacted. If the results we find were driven by some other regulations that were already in place from the past, we should expect similar discontinuity in Chapter 11 filing in pre-SBRA period. I use the bankruptcies from January 2019 - Mar 2020 as the control period and run the regression discontinuity. Figure 3.11 shows the results

[13] See Barreca, Lindo and Waddell (2016) for more details on donut RD design.

from this falsification test. As the figure shows, there is no discontinuity in the pre-SBRA period at the debt threshold of $7.5M.

Much of my results in this paper are from the cross-sectional analyses in which I split the sample in subgroups and conduct separate regression-discontinuity tests on them. In order to ensure that my results are not driven spuriously due to arbitrary sample splits, I conduct the heterogeneity analyses by splitting the sample at different points. As I argue before, if the effect of the SBRA is higher for firms with higher secured debt / total debt (and higher secured debt / total assets), we should expect to see the discontinuity in plan confirmation increasing as we restrict the sample to higher secured debt/ total debt (and secured debt/total assets). Figure 3.12 shows this variation. Each point in the figure is the RD estimate for a sub-sample of firms with the ratio greater than the tick on the x-axis. Subplot (a) presents the results by restricting sample on secured debt / total debt, and subplot (b) on secured debt / total assets. For example, the estimate at $x = 0.4$ in the right figure of panel (a) gives the discontinuity estimate in plan confirmation for a sub-sample of firms with secured / total debt > 0.4. The shaded area represents the 95% confidence interval. Consistent with my hypothesis, the plan confirmation discontinuity estimate increases as we restrict our sample to firms with higher secured debt/ total debt and secured debt / total assets.

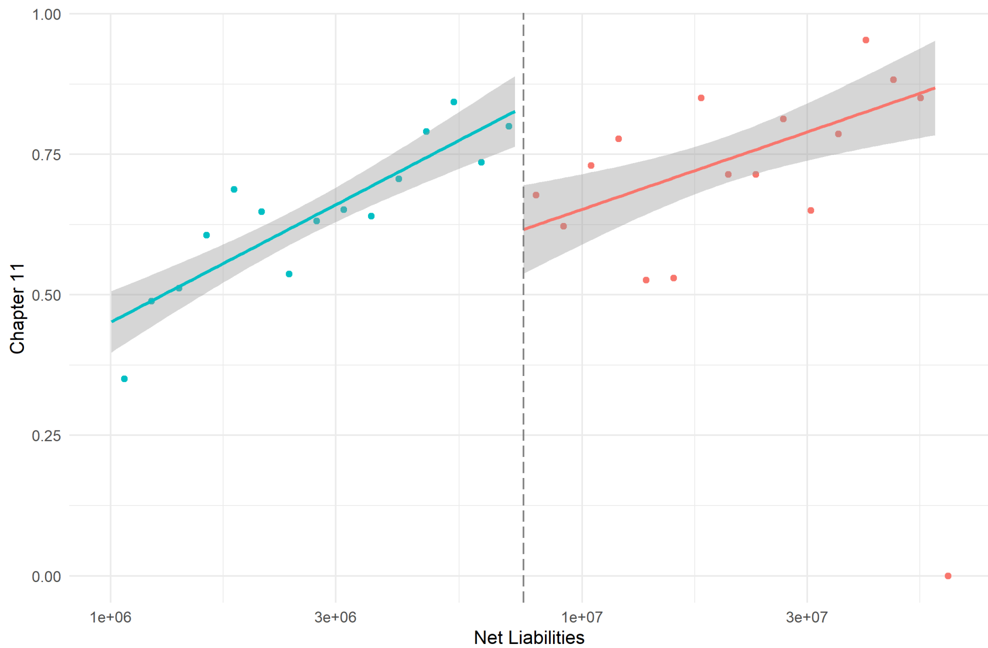
3.6 Conclusion

This paper studies the effects of creditor rights on bankruptcy outcomes of small businesses. Using a regression-discontinuity design (RDD), I find that reducing creditor's control in bankruptcy increases the plan confirmation for firms that have more secured creditors. This effect is greater when creditors have greater creditor market power and suffer from more coordination problems.

This finding has important policy implications for the design of the optimal bankruptcy law. My results imply that the existing reorganization process under Chapter 11 is too costly for small businesses. The provision of subChapter V increases the successful reorganization of firms that suffer from higher creditor control and bankruptcy frictions. My results also have important implications for the choice of the debt structure of small firms. While secured creditors, such as banks, are a good signal of the credit quality for many information-sensitive firms, my findings imply that they also impose serious costs in financial distress in the form of a bias towards liquidation. Similarly, having a dispersed debt structure may exacerbate coordination problems and cause the firm to inefficiently shut down.

Figure 3.1: Discontinuity in Chapter 11 filing

(a) Linear



(b) Non-parametric

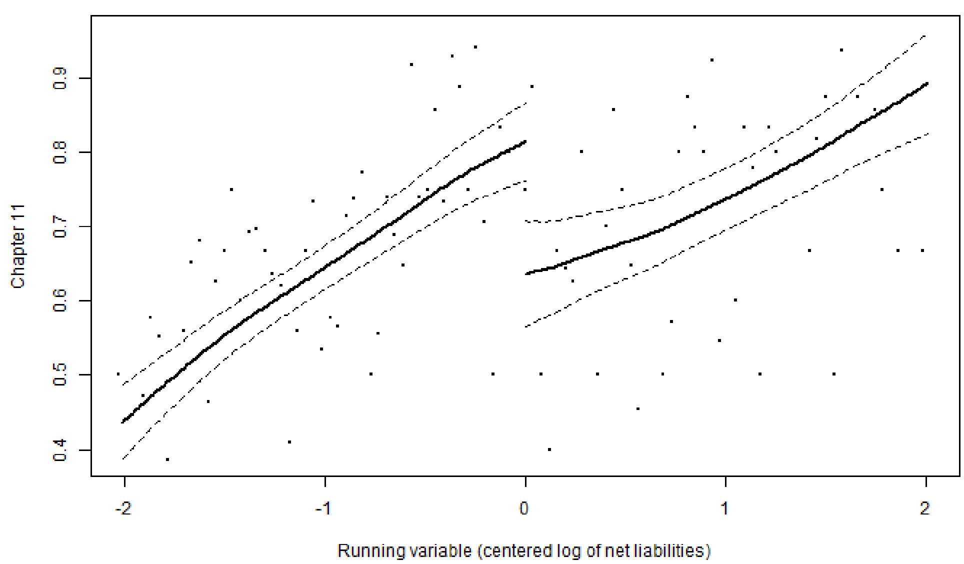
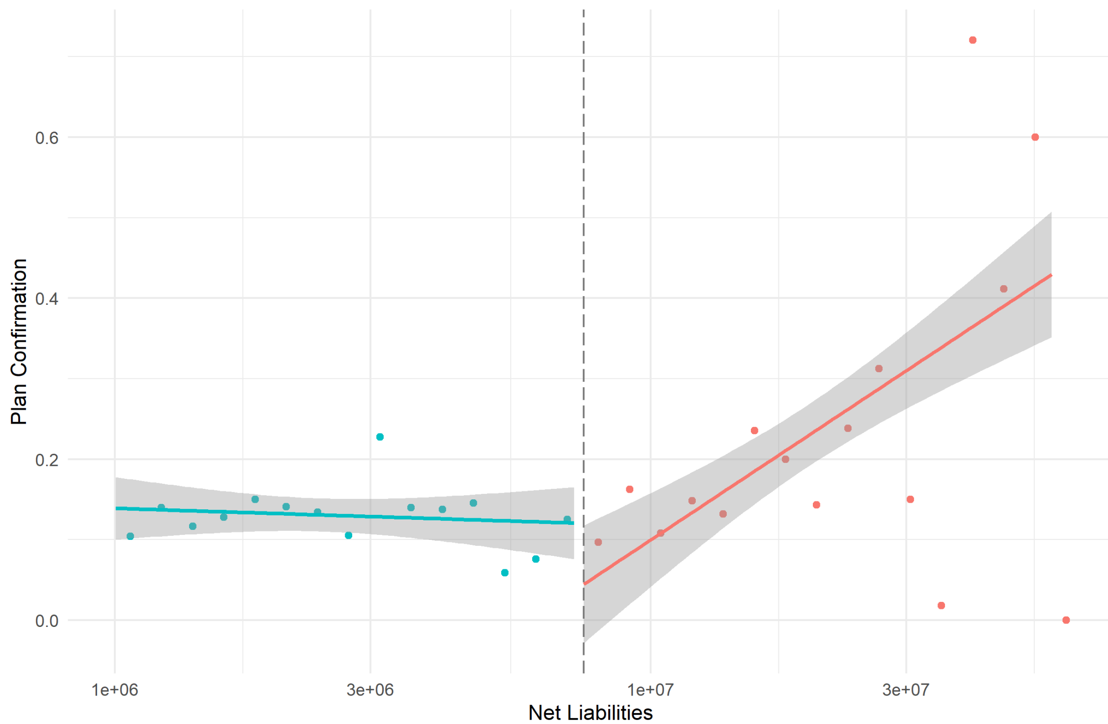


Figure 3.2: Discontinuity in plan confirmation

(a) Linear



(b) Non-parametric

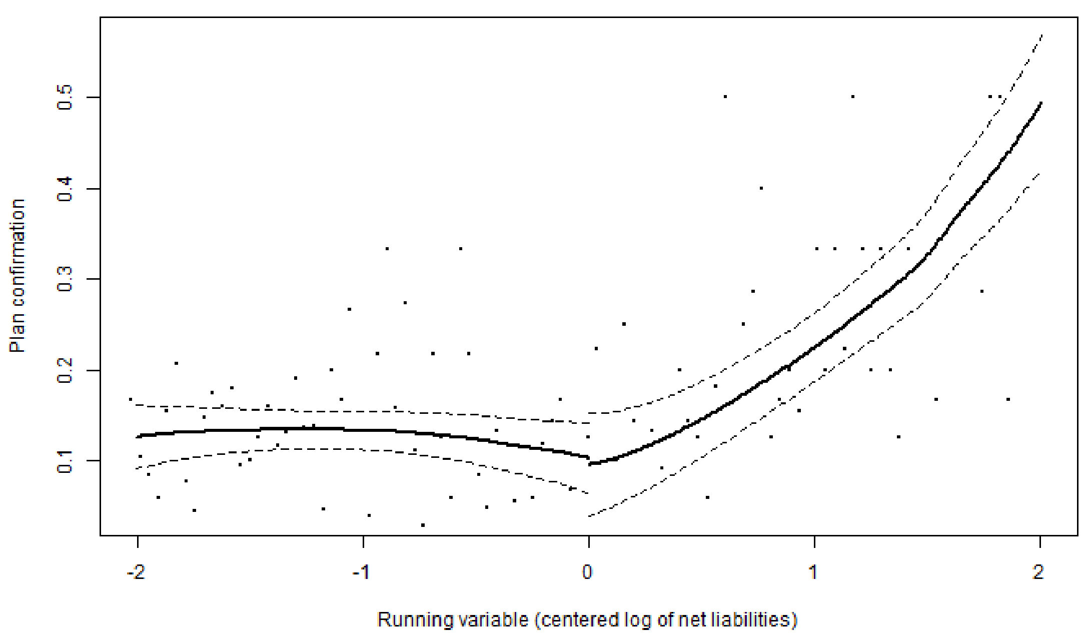


Figure 3.3: Heterogeneous discontinuity by secured debt / total debt

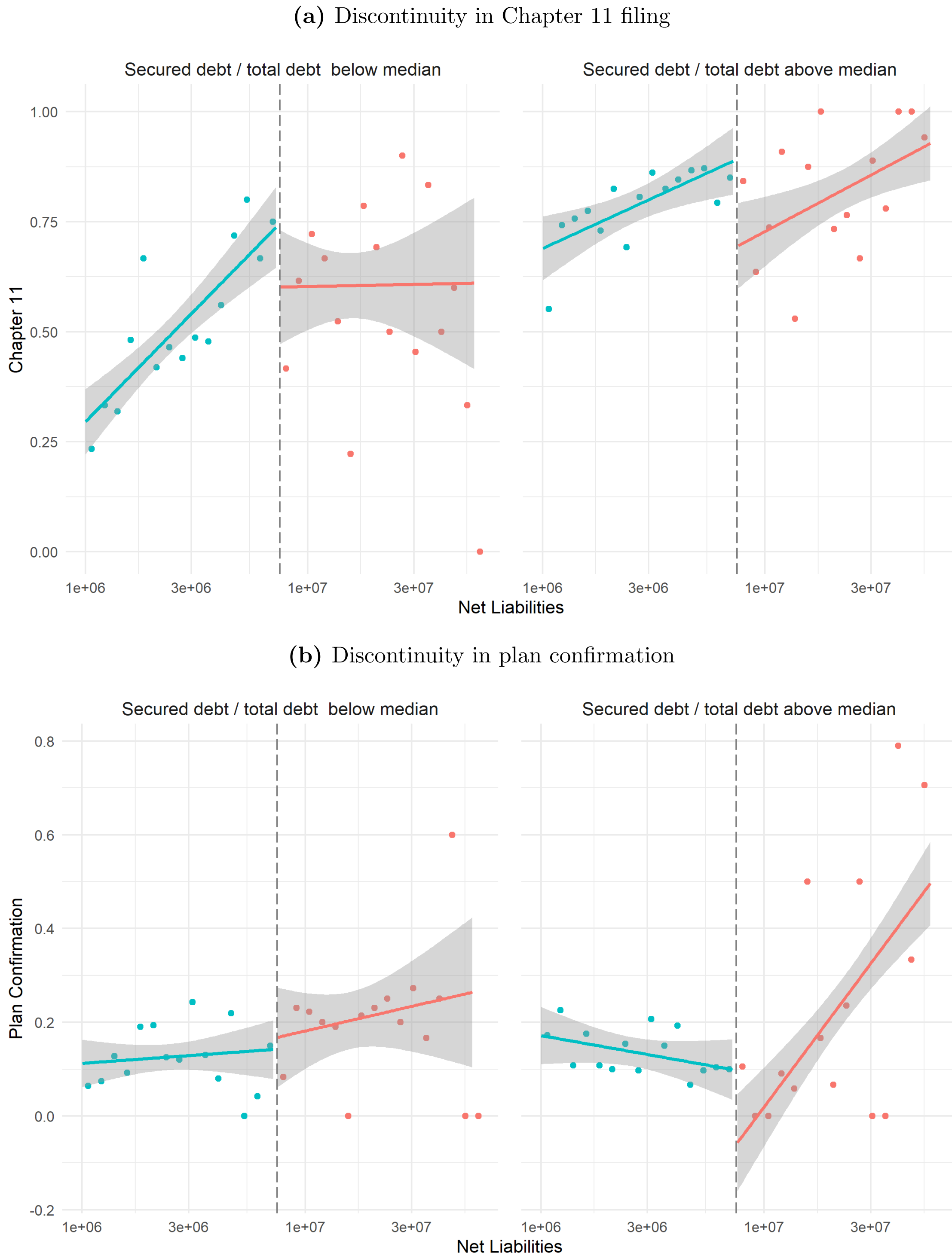
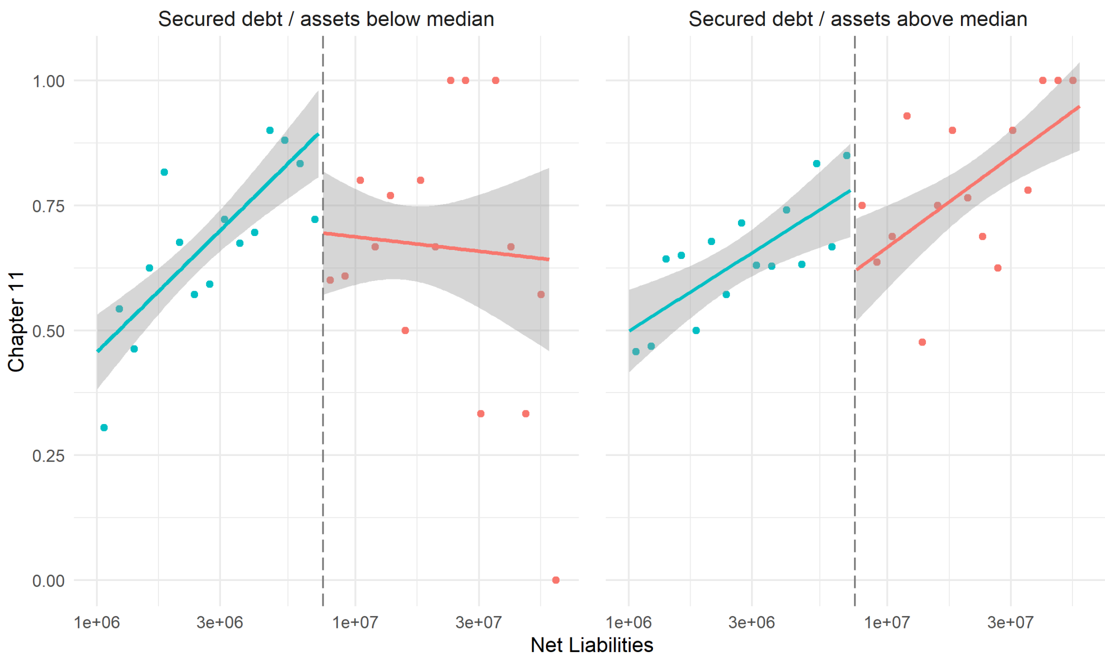


Figure 3.4: Heterogeneous discontinuity by secured debt / total assets

(a) Discontinuity in Chapter 11 filing



(b) Discontinuity in plan confirmation

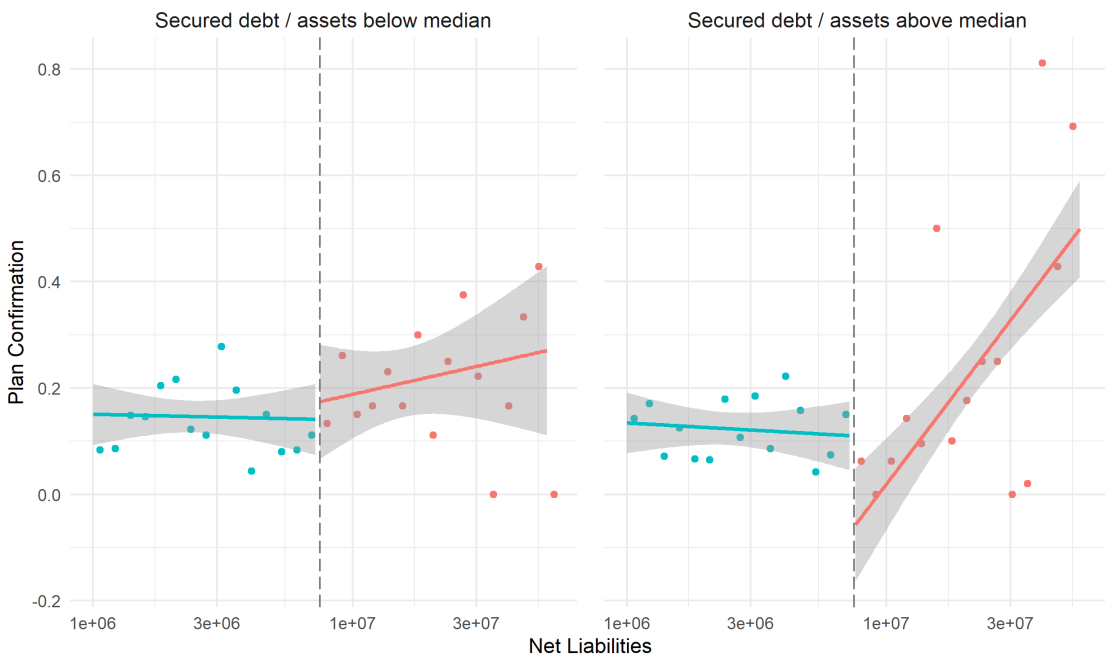
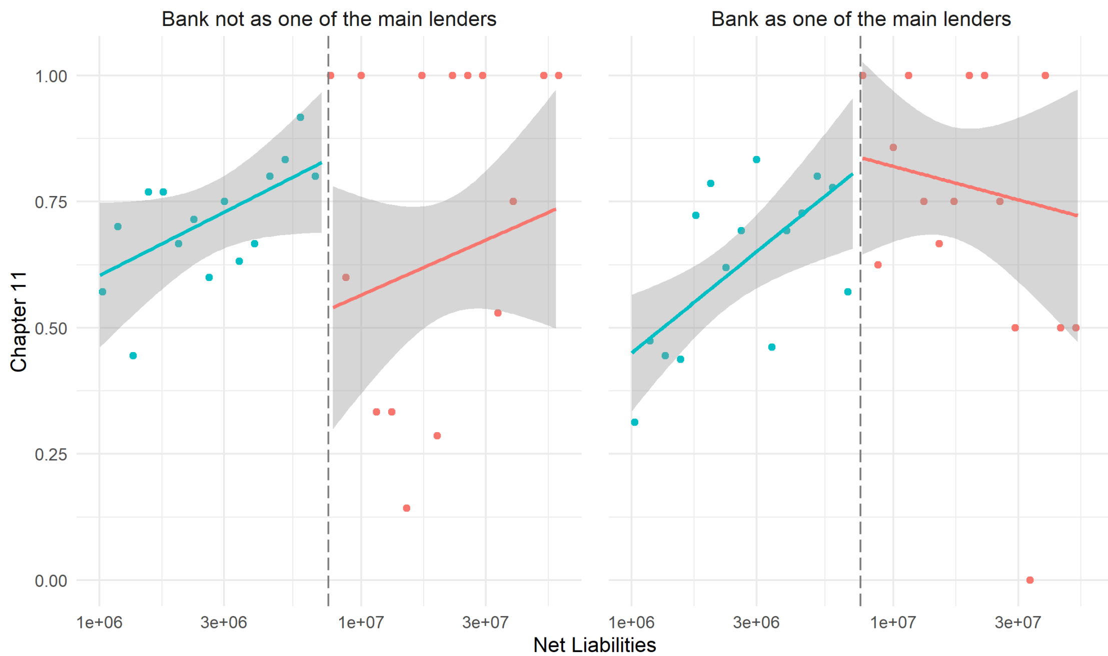


Figure 3.5: Heterogeneous discontinuity by the presence of bank as a lender

(a) Discontinuity in Chapter 11 filing



(b) Discontinuity in plan confirmation

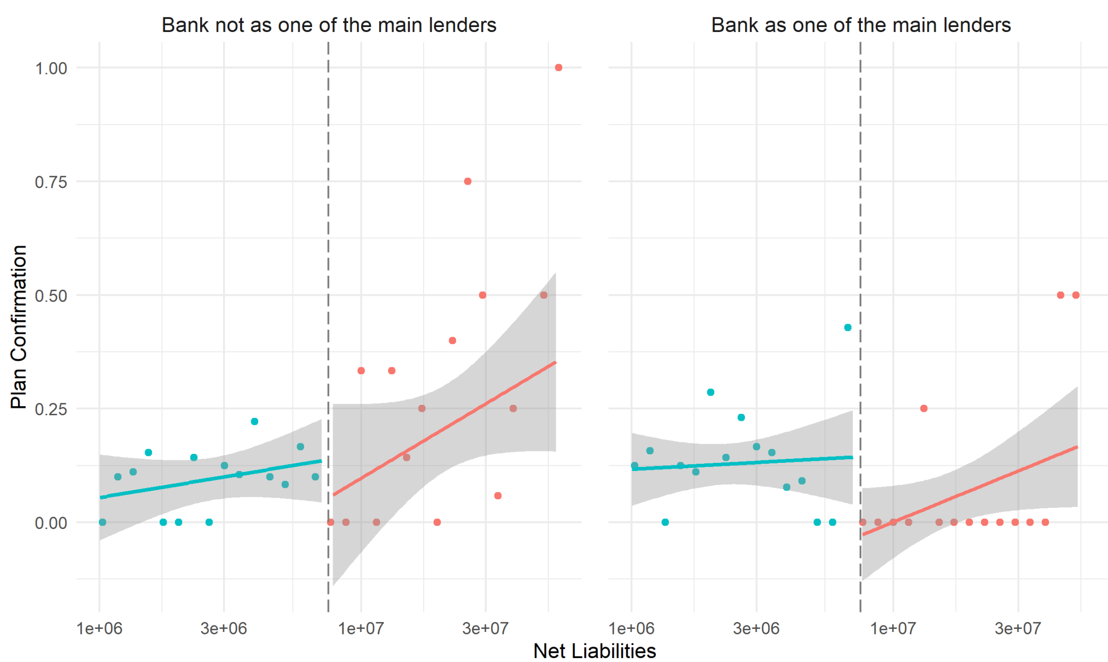
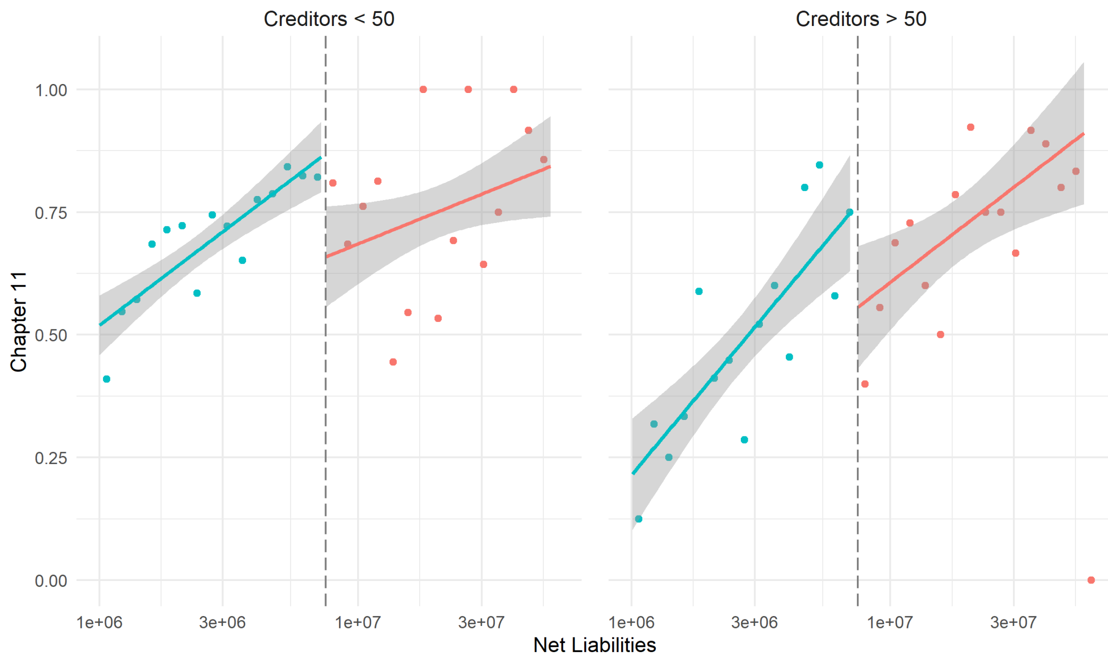


Figure 3.6: Heterogeneous discontinuity by number of creditors

(a) Discontinuity in Chapter 11 filing



(b) Discontinuity in plan confirmation

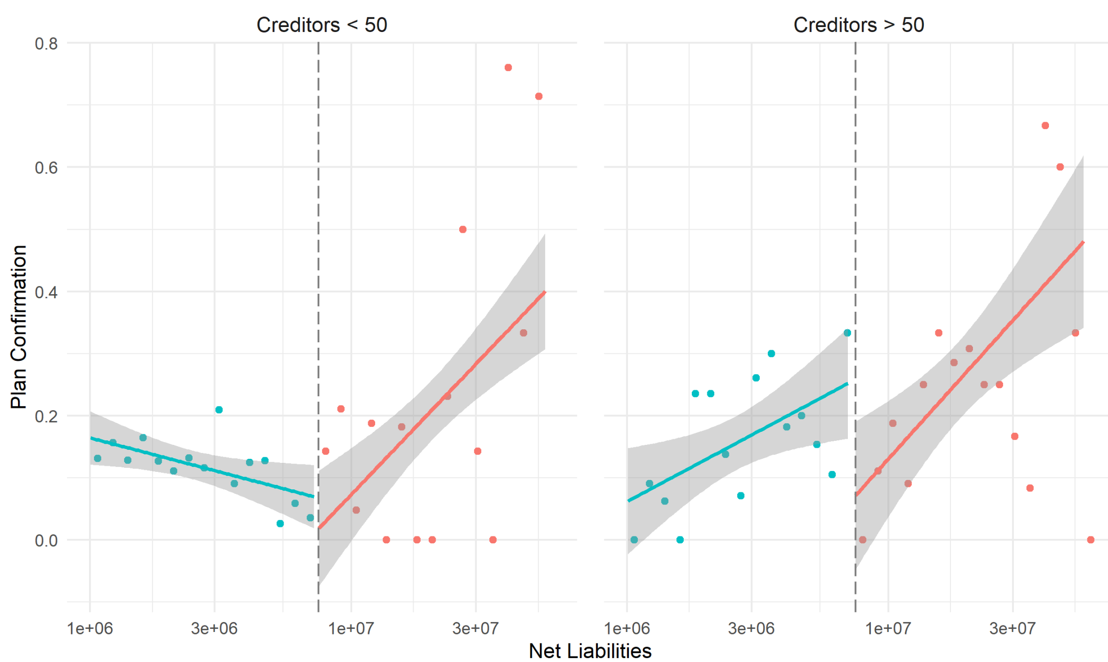
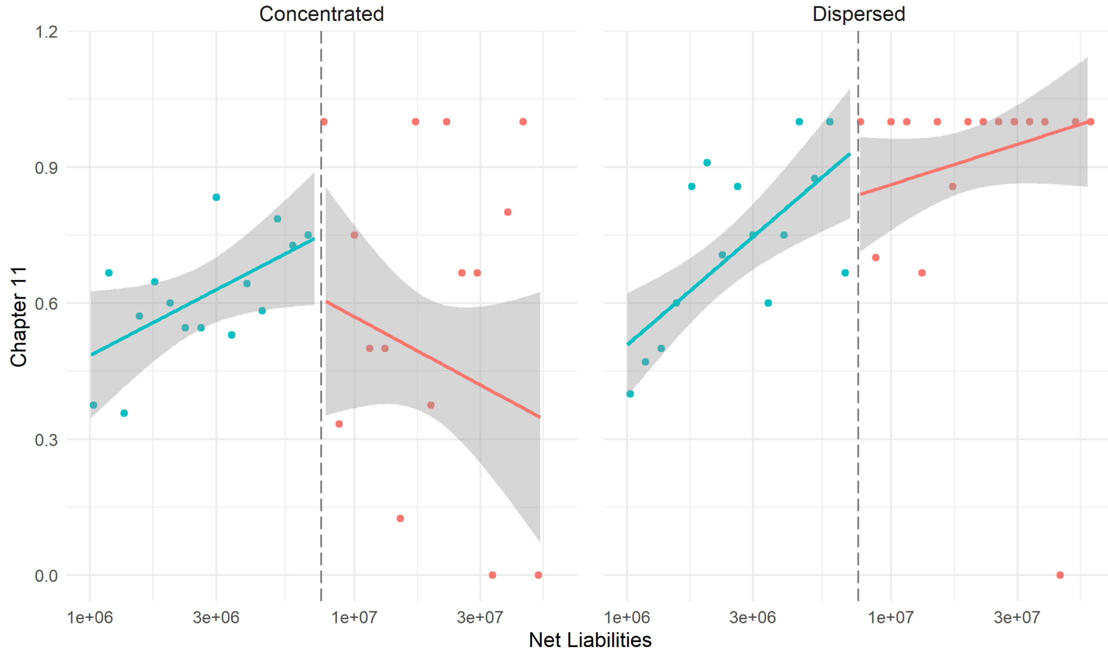


Figure 3.7: Heterogeneous discontinuity by debt concentration (debt HHI)

(a) Discontinuity in Chapter 11 filing



(b) Discontinuity in plan confirmation

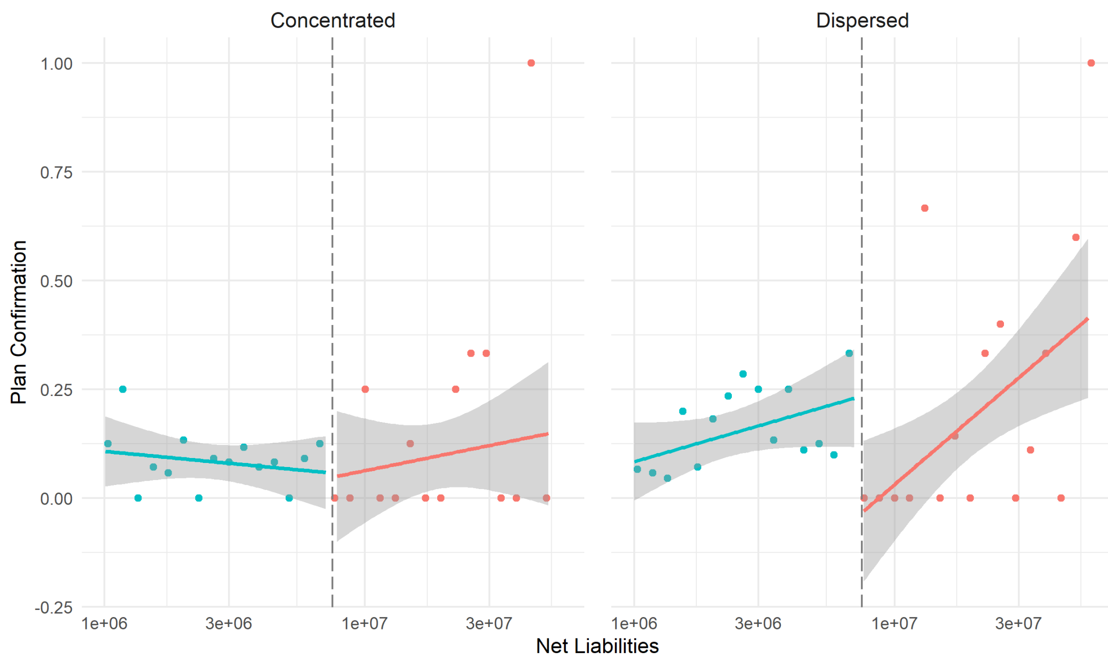
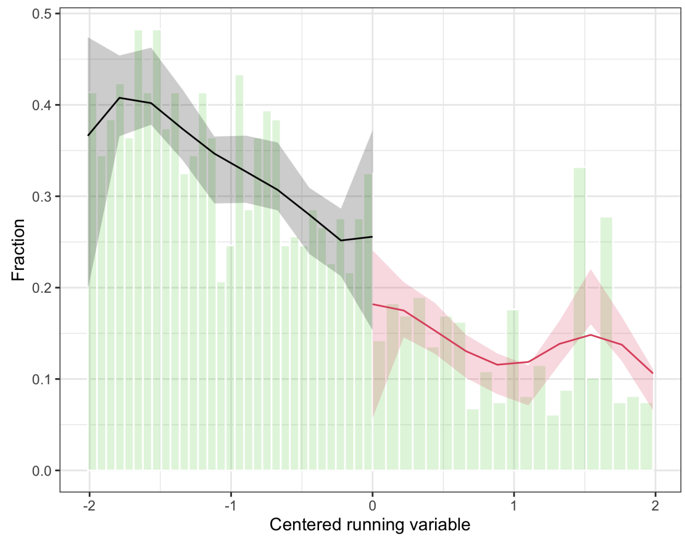
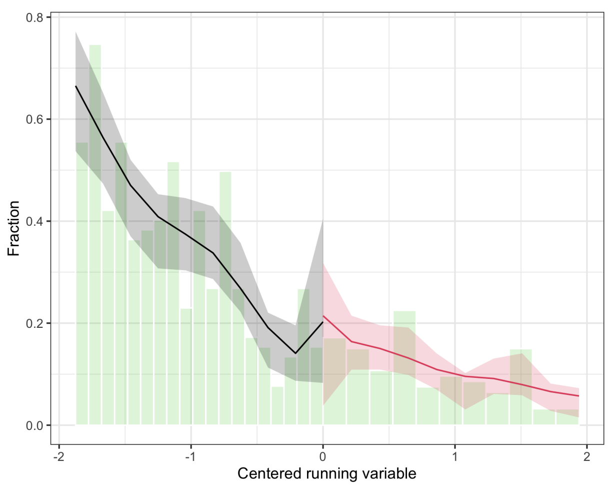


Figure 3.8: Density discontinuity test

(a) Full sample



(b) Chapter 7



(c) Chapter 11

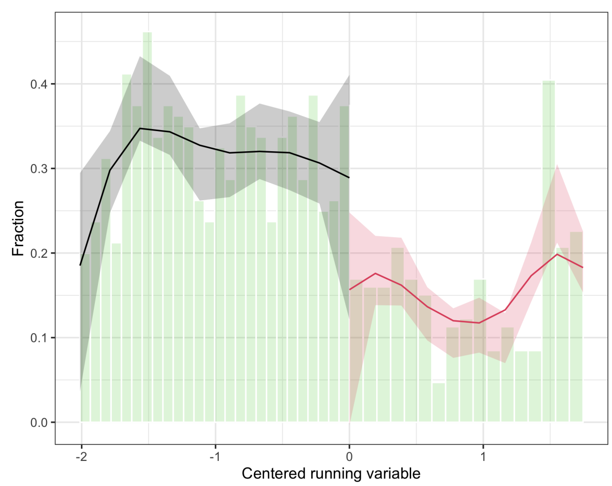


Figure 3.9: Balance of covariates test

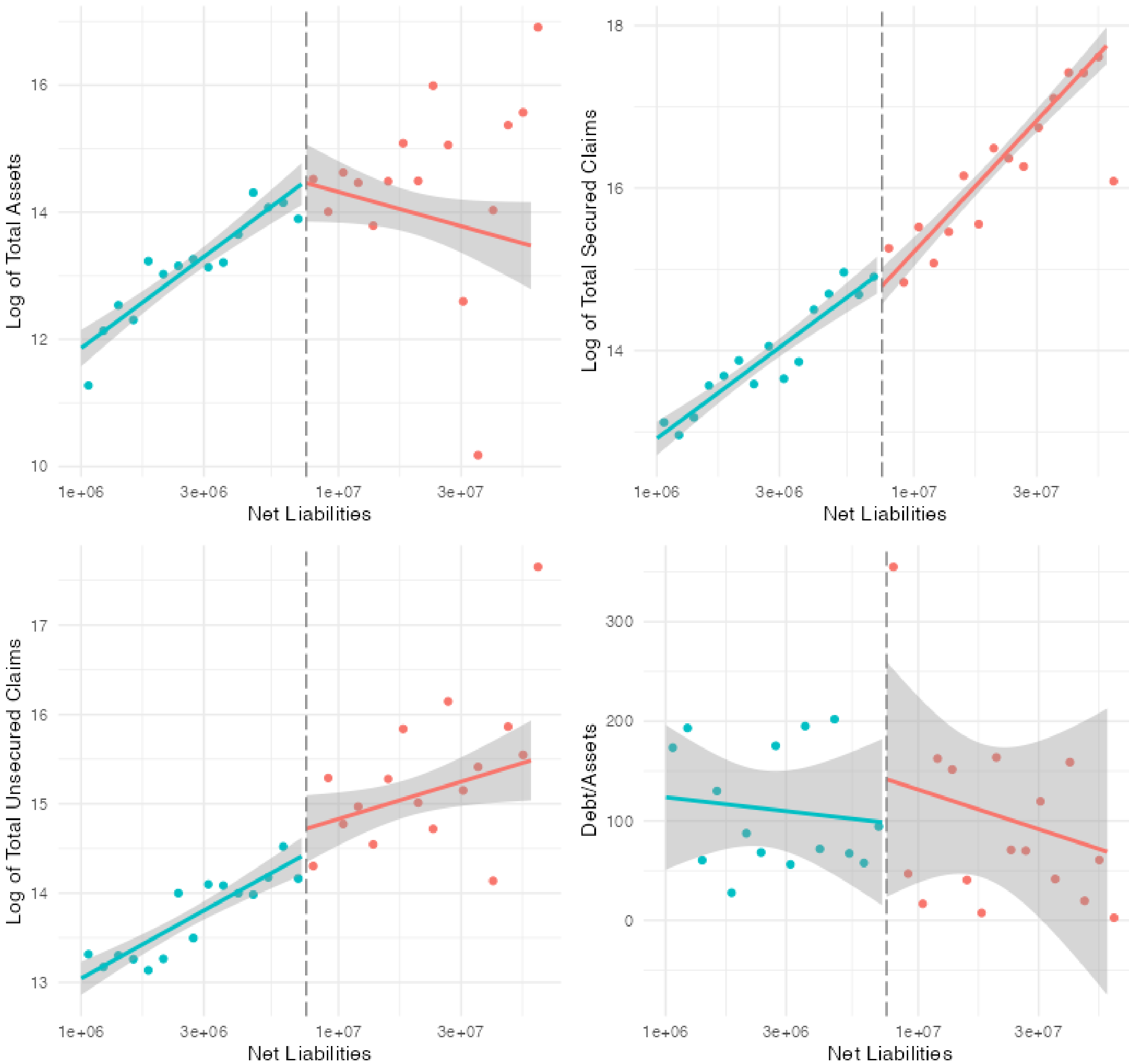


Figure 3.10: Placebo test: Alternate cut-off points

This figures shows regression discontinuity estimates at alternate cut off points farther away from the treatment thershold with Chapter 11 filing as the dependent variable.

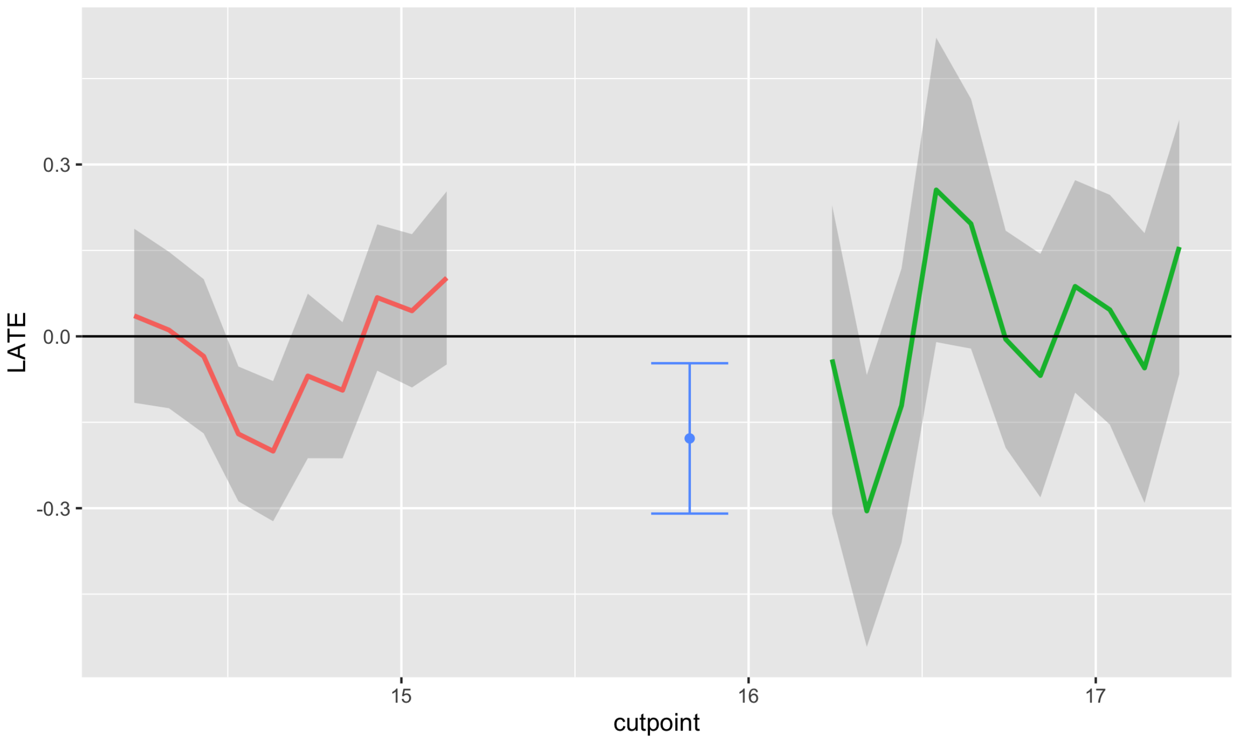


Figure 3.11: Placebo test: Different time period

This figures shows regression discontinuity estimates at the threshold for bankruptcy cases filed between Jan 1, 2019 and Mar 31, 2020. The Chapter 11 filing is the dependent variable.

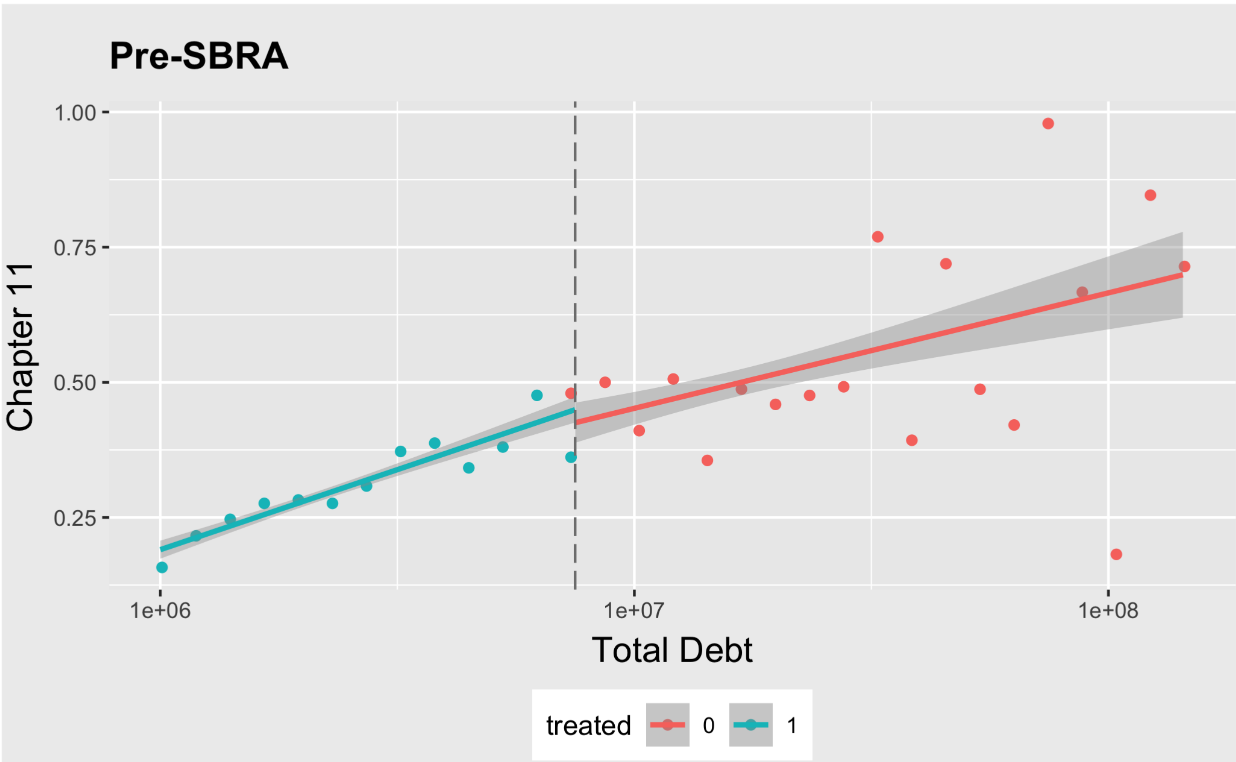
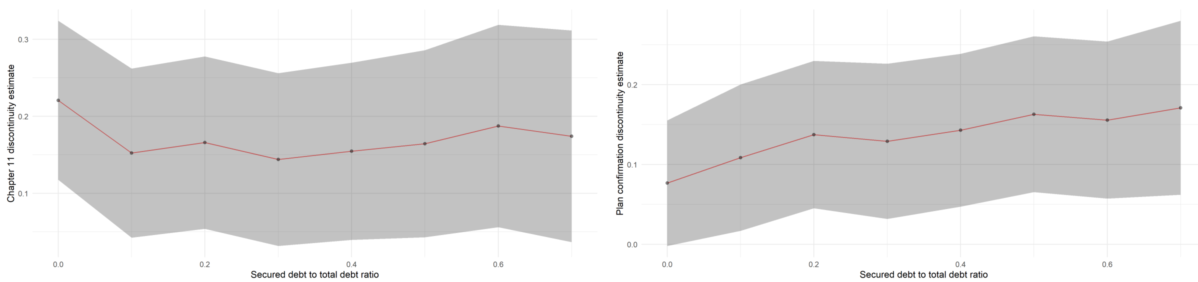


Figure 3.12: Sub-sample discontinuity estimates for firms with greater secured creditor control

This figure presents the sub-sample regression discontinuity estimates after restricting the sample to smaller groups based on two measures. Subplot (a) restricts the sample on secured debt / total debt, and subplot (b) on secured debt / total assets. Figures on the left show discontinuity estimates for Chapter 11 as the outcome variable, and figures on the right for plan confirmation. Each dot in the figure represents a point estimate of an RDD on a sub-sample of firms with the ratio greater than the tick on the x-axis. The shaded area represents the 95% confidence interaval around the point estimate.

(a) Firms with greater secured debt / total debt



(b) Firms with greater secured debt / total assets

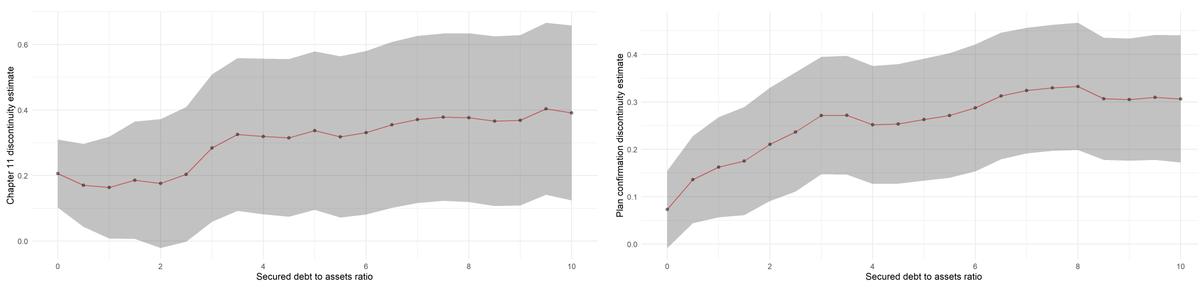


Table 3.1: Summary statistics: Financial characteristics

This table presents the summary statistics of some of the key financial characteristics of the firms filing for Chapter 11 and Chapter 7 bankruptcies.

(a) Panel A: Chapter 7

	Obs	Mean	St. Dev.	25th %ile	Median	75th %ile
Total Assets (in $M)	507	1.26	5.47	0.002	0.094	0.62
Secured Claims (in $M)	507	2.54	6.53	0.00	0.16	1.50
Unsecured Non Priority (in $M)	507	3.23	6.61	0.13	1.16	2.83
Unsecured Priority (in $M)	507	1.37	3.45	0.00	0.12	1.30
Net Liabilities (in $M)	507	6.20	8.97	1.43	2.47	6.34
Debt / Assets	507	Inf	NaN	4.27	30.65	1730.79
Secured Debt / Total Debt	503	0.32	0.39	0.00	0.069	0.69
Secured Debt / Assets	457	Inf	NaN	0.00	0.83	9.65

(b) Panel B: Chapter 11

	Obs	Mean	St. Dev.	25th %ile	Median	75th %ile
Total Assets (in $M)	964	6.07	21.81	0.34	1.72	4.46
Secured Claims (in $M)	964	7.32	11.56	0.87	2.25	6.56
Unsecured Non Priority (in $M)	964	2.70	5.00	0.065	0.78	3.10
Unsecured Priority (in $M)	964	0.39	1.67	0.00	0.00	0.11
Net Liabilities (in $M)	964	10.16	12.77	2.04	4.29	11.35
Debt / Assets	964	Inf	NaN	0.98	2.30	12.55
Secured Debt / Total Debt	960	0.63	0.36	0.34	0.78	0.96
Secured Debt / Assets	950	Inf	NaN	0.46	0.96	3.90

Table 3.2: Summary statistics: Creditor characteristics

This table presents the summary statistics of the composition and type of creditors. Data on creditors is obtained from the Form 206D of the bankruptcy petition where firms report information on their largest creditors.

	Chapter 7			Chapter 11			
Variable	Obs	Mean	St. Dev.	Obs	Mean	St. Dev.	Test
Creditors Fewer than 50	507	0.627	0.484	964	0.739	0.44	F= 19.87***
Debt HHI	170	0.495	0.305	336	0.384	0.319	F= 14.161***
Is Largest Creditor a Bank?	170	0.206	0.406	336	0.211	0.409	F= 0.02
Bank as a Large Creditor	170	0.588	0.494	334	0.527	0.5	F= 1.707
Credit Card Company as a Large Creditor	170	0.206	0.406	333	0.174	0.38	F= 0.749
Financial Services as a Large Creditor	170	0.271	0.446	334	0.222	0.416	F= 1.491

Statistical significance markers: * p<0.1; ** p<0.05; *** p<0.01

Table 3.3: Outcomes

This table presents the breakdown of the current status and outcomes of bankruptcy filings.

	Chapter 11		Chapter 11 SC V		Chapter 7	
	N	Percent	N	Percent	N	Percent
Outcome	560		338		501	
... 341 Creditors Meeting	54	9.6%	15	4.4%	96	19.2%
... Confirmed	116	20.7%	118	34.9%	0	0%
... Converted	43	7.7%	20	5.9%	2	0.4%
... Deadline Filing Claims	163	29.1%	123	36.4%	226	45.1%
... Deadline Objecting Discharge	16	2.9%	9	2.7%	7	1.4%
... Discharged	0	0%	0	0%	2	0.4%
... Dismissed	152	27.1%	46	13.6%	16	3.2%
... Terminated	16	2.9%	7	2.1%	152	30.3%

Table 3.4: Discontinuity in Chapter 11 filing and plan confirmation

This table presents the discontinuity estimates in the likelihood of Chapter 11 filing (in Panel A) and plan confirmation (in Panel B) using different bandwidths and specifications. The likelihood of Chapter 11 filing is calculated as the number of Chapter 11 filings divided by total bankruptcy filings (Chapter 11 + Chapter 7), and the likelihood of plan confirmation is calculated as the number of plan confirmations divided by total bankruptcy filings (Chapter 11 + Chapter 7). Each specification includes the log of net liabilities, X_i and $Below\ Threshold * X_i$ that allows the slopes of the fit on the running variable to be different on each side of the threshold. Heteroskedasticity-robust standard errors are shown in parentheses.

	(1)	(2)	(3)	(4)
Panel A	Outcome: Chapter 11 filing			
Below Threshold	0.218^{***}	0.154^{***}	0.178^{***}	0.191^{**}
	(0.052)	(0.059)	(0.066)	(0.074)
Obs	1471	1040	872	674
R^2	0.054	0.012	0.016	0.016
Bandwidth	Full	1.5	IK	1
Panel B	Outcome: Plan confirmation			
Below Threshold	0.076^{*}	-0.044	-0.007	-0.046
	(0.040)	(0.042)	(0.048)	(0.053)
Obs	1471	1040	872	674
R^2	0.043	0.001	0.008	0.004
Bandwidth	Full	1.5	IK	1

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 3.5: Heterogeneous RD effects for firms with different secured creditor control

This table presents heterogeneity in the RD treatment effects across firms with different extents of secured creditor control. Panel A splits the sample of observations at the median value of secured debt to total debt ratio (median = 0.61). Panel B splits the sample at median secured debt to total assets ratio (median = 0.95). Each specification includes the log of net liabilities, X_i and $Below\ Threshold * X_i$ that allows the slopes of the fit on the running variable to be different on each side of the threshold. Heteroskedasticity-robust standard errors are shown in parentheses.

Outcome:	Chapter 11 filing		Plan confirmation	
	Below median	Above median	Below median	Above median
Panel A: Sample split at median secured debt / total debt				
Below Threshold	0.153*	0.198***	−0.022	0.158***
	(0.081)	(0.067)	(0.062)	(0.050)
Obs	731	731	731	731
R^2	0.057	0.025	0.011	0.087
Panel B: Sample split at median secured debt / total assets				
Below Threshold	0.206***	0.167**	−0.032	0.170***
	(0.076)	(0.076)	(0.063)	(0.053)
Obs	703	703	703	703
R^2	0.050	0.068	0.007	0.094
Panel C: Sample split at median unsecured debt / total assets				
Below Threshold	0.140**	0.217***	0.118*	0.008
	(0.059)	(0.076)	(0.062)	(0.053)
Obs	625	812	625	812
R^2	0.024	0.079	0.109	0.013

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 3.6: Heterogeneous RD effects for firms with different numbers and concentration of creditors

This table presents heterogeneity in the RD treatment effects across firms with different numbers and dispersion of creditors. Panel A splits the sample of observations at the number of creditors. Panel B splits the sample at median debt HHI (median = 0.35). Debt HHI shows the concentration in claims for top 20 creditors, and is calculated using the formula:

$$hhi = \sum (\frac{x_i}{\Sigma x_i})^2$$

Each specification includes the log of net liabilities, X_i and $Below\ Threshold * X_i$ that allows the slopes of the fit on the running variable to be different on each side of the threshold. Heteroskedasticity-robust standard errors are shown in parentheses.

Outcome:	Chapter 11 filing		Plan confirmation	
	Panel A: Sample split at # of Creditors			
	Creditors < 50	Creditors > 50	Creditors < 50	Creditors > 50
Below Threshold	0.211***	0.212**	0.049	0.187***
	(0.064)	(0.092)	(0.050)	(0.072)
Obs	1030	441	1030	441
R^2	0.042	0.130	0.043	0.060
	Panel B: Sample split at median debt HHI			
	Below median	Above median	Below median	Above median
Below Threshold	0.141	0.107	0.009	0.268***
	(0.146)	(0.106)	(0.076)	(0.086)
Obs	252	254	252	254
R^2	0.033	0.117	0.004	0.046

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 3.7: Heterogeneous RD effects for firms with and without banks as their lenders

This table presents heterogeneity in the RD treatment effects across firms that do and do not have banks as their lenders. Each specification includes the log of net liabilities, X_i and $Below\ Threshold * X_i$ that allows the slopes of the fit on the running variable to be different on each side of the threshold. Heteroskedasticity-robust standard errors are shown in parentheses.

Outcome:	Chapter 11 filing		Plan confirmation	
	Has no bank	Has bank	Has no bank	Has bank
Below Threshold	0.184	−0.067	0.104	0.231***
	(0.123)	(0.127)	(0.089)	(0.084)
Obs	215	289	215	289
R^2	0.343	0.185	0.172	0.196

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 3.8: RD effects for subgroup of firms with greater creditor control and reorganization frictions

This table presents results from a set of sub-sample regression discontinuity analyses. Each column in the table corresponds to a subgroup of firms as defined in top row. Column 1 restricts the sample to firms that have secured debt to total debt of more than 50%; column 2 further filters to include only firms with secured debt more than total assets. Column 3 is the sub-sample of firms with secured debt / total debt > 50% and number of creditors > 50. Column 4 restricts the sample to firms with all of the 3 criteria. Panel A reports the discontinuity in the likelihood of Chapter 11 filing. Panel B reports the discontinuity in the likelihood of plan confirmation. Each specification includes the log of net liabilities, X_i and $Below\ Threshold * X_i$ that allows the slopes of the fit on the running variable to be different on each side of the threshold. Heteroskedasticity-robust standard errors are shown in parentheses.

	Sec debt/debt > 0.5	Sec debt/debt > 0.5, Sec debt/assets > 1	Sec debt/debt > 0.5, # of creditors > 50	Sec debt/debt > 0.5, Sec debt/assets > 1, # of creditors > 50
Panel A	Outcome: Chapter 11 filing			
Below Threshold	0.163***	0.220**	0.226	0.175
	(0.062)	(0.087)	(0.139)	(0.181)
Obs	816	525	163	111
R^2	0.023	0.044	0.143	0.220
Panel B	Outcome: Plan confirmation			
Below Threshold	0.164***	0.227***	0.427***	0.442***
	(0.050)	(0.058)	(0.115)	(0.146)
Obs	816	525	163	111
R^2	0.072	0.108	0.099	0.134

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

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