The Cost of Climate Policy to Capital: Evidence from Renewable Portfolio Standards*

Harrison Hong [†] Jeffrey Kubik [‡] Edward Shore [§]

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Abstract

Climate policy targeting emissions-intensive sectors imposes a significant cost to capital. Exploiting features of state-level renewable portfolio standards (RPS) that govern US utilities that issue debt to finance abatement, we estimate that RPS reduces firm carbon emissions by 2.7 million tons at a cost of 66 bps wider credit spread. Our novel identification strategy compares investor-owned utilities to RPS-exempt municipal producers within the same state. Using a corporate-bond pricing model in which abatement costs increase default risk, the market expects an abatement cost of $50 per ton of emissions. This large effect is consistent with a small cost passthrough in our data.

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[†]Columbia University and NBER

[‡]Syracuse University

[§]Columbia University

1 Introduction

Many jurisdictions around the world have started to implement climate policy, such as emissions trading systems (ETS) and renewable portfolio standards (RPS), that target emissions-intensive firms in the energy and utilities sectors. While there are plentiful studies on the damages associated with carbon emissions[1], far less research has been done on the abatement costs that these firms have to bear, i.e. the cost of climate policy to capital. But having causal estimates of the policy impact on investors' forecasts of abatement costs and risks is crucial for understanding the long-run implications for investments and welfare[2], as well as for addressing financial regulatory concerns about transition risks [3]

To this end, we estimate the response of bond markets to renewable portfolio standards (RPS), the main climate policy covering power firms in 40% of the major carbon-emitting countries globally, including the US, India, and South Korea.[4] Because power firms or utilities rely on debt to finance their investments, we have plentiful data on bond yields with which to measure the response of investors in bond markets to RPS, which requires firms to switch from fossil fuels toward more expensive renewables, typically solar and wind farms.

Since an empirical challenge is that the enactment of climate policy is endogenous and dependent on underlying economic conditions that also affect firms' cost of capital, a key step in our approach is to exploit institutional features of the RPS system in the United States to estimate the causal effect of climate policy on firms' financial

[1]See, e.g., Deschênes and Greenstone (2007), Schlenker and Roberts (2009), Dell et al. (2012), Dell et al. (2014), and Bilal and Rossi-Hansberg (2023).

[2]For instance, these abatement costs are a key outcome in integrated assessment models (Nordhaus (2017), Golosov et al. (2014), Jensen and Traeger (2014), Barnett et al. (2020),Hong et al. (2023)).

[3]For policy discussions of transitions risks, see (Task Force Climate-Related Financial Disclosures, European Systemic Risk Board, and De Nederlandsche Bank and Jung et al. (2021).

[4]Emissions trading systems and renewable portfolio standards are two types of regulations used for emissions-intensive sectors, while national carbon taxes are enacted to address gaps for other sectors (see, e.g., Carhart et al. (2022) for overview of climate policy globally).

health. Of the 32 states in the US that enacted RPS over the period of 1991-2020, 14 of them require investor-owned producers to meet RPS targets, but exempt municipal producers. The municipal producer exemption allows us, in a panel regression setting, to address implementation timing or endogeneity concerns by using firm and state-by-year fixed effects.

By combining carbon emissions and bond-issue data with our novel identification strategy for RPS in the US to identify the reduction in carbon emissions, we are able to estimate the elasticity of credit spreads to a reduction of carbon emissions. We find the benefits of RPS in terms of lower carbon emissions come at a significant cost to capital in terms of higher credit spreads. Our conservative estimate is that RPS leads to a reduction of carbon emissions of around 2.7 million tons per year for a typical producer, which comes at a cost to capital of around 66 bps wider credit spreads.

Our emissions reduction figure is consistent with, but more conservative than, earlier RPS studies (Greenstone and Nath (2020), Upton Jr and Snyder (2017), Deschenes et al. (2023)). There are two reasons for this. First, we use a different identification strategy. These studies exploit the staggered implementation of RPS across states along with state-level controls to address endogeneity concerns. Second, measuring emissions using plant level data is complicated since firms can meet RPS standards through the purchase of renewable energy certificates (RECs). We get around this issue by using RPS enforcement outcomes at the state level by producer type to estimate the emissions reduction by a typical producer.

To arrive at our credit spreads findings, we run four sets of distinct analyses using bond-issue data. The first is a difference-in-differences design that pools states with municipal-producer exemptions and has state by year fixed effects. Rather than pooling observations from all these states, we also run our difference-in-differences analysis for each individual state, since our control firms are municipal producers in the same state

(Sun and Abraham (2021), Goodman-Bacon (2021)). The second is an event-study design where we can assess the pre-trends and the persistence of the treatment effects over time.

The third is a placebo analysis, where we implement the same procedure as in our main analysis but using bond issues from firms in the 18 states that passed RPS without a municipal exemption. Finally, we pool observations from exempt and non-exempt states and run a panel regression specification that compares the estimates for exempt versus non-exempt states — a triple difference estimate. The latter two designs address concerns that unobserved differences in features of bonds are somehow correlated with the timing of RPS implementation.[5]

Treatment with RPS significantly elevates bond issuance and the characteristics-adjusted bond yield spreads of treated utilities by around 100 bps compared to non-treated municipals in the same state.[6] There are no pre-trends; and the effect of RPS on bond issuance and yield spreads persists over several years. We expect and indeed find in our 'placebo' analysis that the effects we documented in the states with municipal exemptions are absent in these states without exemptions. Our triple difference estimate of 66 bps is more conservative than the 100 bps from our difference-in-difference estimates. Our paper is the first causal estimate that links emissions reduction to asset prices in the climate finance literature (Hong et al. (2020), and Giglio et al. (2021)).[7]

We then use a structural corporate bond-pricing model (Merton (1974), Longstaff and Schwartz (1995), Leland and Toft (1996)) — that maps yield spreads to the tax

[5]Bonds differ in many features such as callability that are not easily controlled for due to potential missing data.

[6]These yields are adjusted for issue-level characteristics including credit ratings, maturity, and in the case of municipal debt, its purpose and tax treatment.

[7]Studies have typically examined the response of asset prices to the Paris Treaty Agreement, which is non-binding (see, e.g., Seltzer et al. (2022)). Several papers have examined the effects of regional cap-and-trade programs or greenhouse gas policies (Kumar and Purnanandam (2022), Ivanov et al. (2023)). They suggest that banks might reduce lending but find no effects on asset prices.

burden of RPS through a distance to default channel — to infer the abatement costs per ton of emissions.[8] Investor-owned firms subject to RPS have wider credit spreads compared to firms not subject to RPS because abatement costs, all else equal, reduce firm cash flow and hence asset value, which then brings the debt of the firm closer to default.

Using post-RPS data, we estimate the model to match credit spreads for our bonds from the power sector, that already reflect the effects of the RPS, as well as the spread differences in yields between treated and control firms. A key assumption in this comparison, which is true in practice, is that investor-owned and municipal producers compete in different and segmented markets. To match our credit spread findings, we estimate an abatement cost to capital stock ratio of 1.3%. This translates to a cost of $50 to reduce emissions by one ton.

Our abatement cost estimate is larger than the roughly $10 per ton that Meng (2017) estimates using the differential response of equity prices of firms granted versus not granted free permits under the proposed 2009-2010 Waxman-Markey Bill that passed the House but failed in the Senate. Our sizeable expected abatement cost is consistent with a marginally statistically significant increase in electricity prices of around 4%.

That is, producers seem to bear more of the cost of RPS than consumers. This finding is different from conclusions of climate policy such as carbon taxes or ETS in Europe where the impact of producers is mild and consumers bear most of the cost (Känzig (2021), Metcalf and Stock (2020), Känzig and Konradt (2023)). These studies look at equity prices as opposed to debt prices and do not have identification strategies that utilize exemptions. Furthermore, the electricity price increases in the

[8] In our quantitative analysis, we ignore the mitigation benefits of decarbonization for risk premia (Hong et al. (2021)) as such an effect is likely to be small in our sample since aggregate reduction in carbon emissions due to RPS is small.

power sector are determined by regulators. Since rate setting in the power sector of most countries are determined by regulators, our estimates suggest that conclusions on whether producers or consumers bear the cost of climate policy will differ across emissions-intensive versus non-emissions intensive sectors.

2 Data

Our analysis merges a number of sources of data that are required to measure the RPS intervention on output, electricity prices and emissions by different types of producers, as well as the endogenous response of corporate debt issuance and credit spreads.

RPS. At the broadest level, we have state level data on RPS in the US from Barbose (2021). For each state, the data gives the year of implementation, the required amount of output that has to be produced from renewables, the year when firms in that state are to have reached that requirement, and most importantly for us, the types of firms that are covered by the RPS in the state. One of the major groups of firms that are exempt in a fraction of states are municipal producers (see Section 3.1). But in general, state exemptions can apply even to a single name, though these instances are rare.

Producers. Using the U.S. Energy Information Administration's Annual Electric Generator Report (Form EIA-860), we collect information on the electric utilities in the United States. This annual form gives information on ownership type (e.g. investor-owned versus municipal producer), where the utilities operate, along with a host of variables including total sales, megawatt capacity in different types of fuel sources, and the cost of installing these different types of capacity.

Renewable capacity. In order to meet RPS, firms respond in one of two ways: they buy renewables from a supplier (via a Renewable Energy Certificate, or REC), or build solar and wind farms. Data on the former is spotty, while we can precisely track firm investments in renewable plants.

CO2 emissions. We calculate the CO_2 emissions of the investor-owned firms and municipal utilities (separately) in our sample of states from 2001 to 2020. We use a different methodology for calculating the emissions of the two sectors. For investor-owned utilities in our sample that are subject to the RPS mandates, we need to take into account that they can meet these mandates either by cutting back emissions from their own electricity generation or they can buy RECs. Our methodology for calculating the emissions of investor-owned utilities needs to take into account these RECs, but for the municipal utilities that are not subject to the RPS mandates, we can ignore them.

For the investor-owned utility sector, we calculate CO_2 emissions by first measuring the amount of electricity sales in this sector each year by state that is not constrained by the RPS mandate. From EIA State Electricity Profiles, we know the total sales of investor-owned utilities each year (measured in MWhr). From Barbose (2021) we know the RPS mandate (measured in percentage of sales) for each state and year and the average compliance rate of firms.[9] This compliance rate takes into account the change in emissions made by the utilities and their purchases of RECs. Thus, the amount of sales not covered by the mandate is equal to total sales multiplied by one minus the RPS mandate times the compliance rate.

We next need to convert this state/year measure of the non-green electricity sales of investor-owned firms (measured in MWhr) into a measure of CO_2 emissions (measured in tons). For this, we use a similar methodology to Greenstone and Nath (2020).

[9] We only have information on compliance rates at the state/year/sector level. This is why our measure of emissions is also at that level.

Using EIA Forms 906 and 923, we can calculate for each year the average mix of fossil fuels that utilities use to generate non-green electricity.[10] Assuming that investor-owned utilities generate non-green electricity using this average fossil fuel mix, we can calculate how much of the sector's non-green electricity sales are produced with each type of fossil fuel. The EPA estimates carbon conversion factors for every fossil fuel, allowing us to calculate the amount of CO_2 (measured in ton) released by every state investor-owned utility sector each year.[11]

For the municipal utility sector in our sample of states, we do not need to worry about them meeting RPS mandates through RECs; therefore, we measure their CO_2 emissions in a simpler way. We measure for every municipal utility in our sample the fossil fuel use of their plants by type (again from EIS forms 906 and 923). With this information, we again use the EPA conversion factors to calculate the CO_2 emissions of the plants. We then sum up those emissions to state/year/sector observations.

Electricity prices. We collect yearly data on the average retail price of electricity from investor-owned and municipal utilities (separately) from 2001 to 2021.[12] These data come from various years of EIA State Electricity Profiles and are measured as cents/KWhr. Data on retail prices allows us to assess the degree of cost passthrough from firms to consumers in the wake of RPS passage, which we discuss in Section 3.2.

Corporate debt. We draw our data for corporate bonds from Mergent FISD, a standard corporate bond database. This dataset contains information on the yields, maturity, issue amount, bond rating, industrial sector, and issuer name of corporate

[10] We calculate this mix every year because over our sample period utilities have been moving away from using coal as the main fossil fuel in electricity generation towards natural gas. Coal use generates more CO_2 emissions than natural gas, so we want to adjust our emissions measures over time for this shift.

[11] The carbon factors are estimated by the U.S. Environmental Protection Agency, Inventory of U.S. Greenhouse Gas Emissions and Sinks: 1990-2021.

[12] Data for electricity prices is not broken down at the producer level.

bond issues in the United States, plus a host of other issue-relevant variables.

We filter on firms in the power sector. Although this data contains information on the state of the head office of the issuer, it does not typically contain information on the state or states in which the issuer operates.

To address this issue, we integrate our bond data with our dataset on utility operations, the details of which are outlined above. We match these two databases using the legal name of the issuer, as given in the 'Bond Issuers' dataset within Mergent, and perform a string distance match to our dataset on production. We are able to perform an exact match to roughly a third of issuers from Mergent, though these issuers make up roughly 72% of all issues in our dataset. When we cannot match exactly, we assume that the state of operation is the same as the state of the head office.[13] In a robustness check, we run our analysis on only the issuers we are able to match exactly; our results are essentially unchanged.

One technical issue with analyzing at the issue level is that many investor-owned utilities operate across several states. To resolve this problem, and ensure that issues are appropriately assigned to states, we perform the following procedure: first, we calculate the average exposure of an investor-owned utility in each of the states where it has a presence by taking the time-series total of sales in each state and dividing by the total sales. For a utility with a presence in only one state, this results in a value of 1.

We then replicate any issues for utilities that operate in multiple states, but weight that observation by the previously calculated exposure. Therefore, if utility 'A' operates in, for example, Kansas, Kentucky, and Tennessee, and has sold roughly 20%, 20%, and 60% of its output in each state respectively, then an issue from utility 'A' appears three times in our dataset, with one assignment to each state, where each

[13]By looking at the discrepancy between the state of operation and state of the head office in our exact matches, we find that this assumption is correct roughly 87% of the time.

observation is weighted by 0.2, 0.2, and 0.6 respectively.

Municipal bonds. For municipal bonds, we use the SDC Muni database. This dataset contains information on the yields, maturity, issue amount, bond rating, industrial sector, state, and issuer name of municipal bond issues in the United States, plus a host of other issue-relevant variables.

Given our interest in assessing the impact of RPS, we restrict attention to municipal bond issues in the 'Electric & Public Power', 'Combined Utilities', and 'Gas' sectors. Across our sample period of 1990 to 2021, we find complete data on 2,049 municipal issues.

3 RPS Exemptions: Renewables, Carbon Emissions and Electricity Prices

3.1 States with and without Municipal Producer Exemptions

In Table 1, we report the RPS details for the 14 states that exempted their municipal producers. The details of our classification procedure are in the Appendix Section A. Many states implemented their RPS in the mid-to-late 2000s. Investor-owned producers are allowed to gradually ramp up their mix of renewables before hitting the required or steady-state amount.

Consider the state of Illinois, which implemented its RPS in 2007. It gave firms a runway of around 20 years to reach a required renewable mix of 25% of output. Hence, investor-owned producers had to increase their mix by roughly a percent a year. States typically vary the length of the transition period to a steady-state requirement depending on how stringent those requirements are. There is variation across states in

Table 1: Summary of RPS Legislation in States with Municipal Exemptions

This table presents summary details of the passage of Renewable Portfolio Standards regulation in the 14 states that have thus far enacted the legislation with municipality exemptions. Note that Virginia also has an exemption for its small investor-owned producers. For the number of municipal and investor-owned suppliers, and their sales in gigawatt hours, we take the time series average.

State	Mandate Start	Maximum Renewable %	Year Max Achieved	No. Municipal	No. Investor-Owned	Municipal Sales (gwhrs)	Investor-Owned Sales (gwhrs)
Arizona	2001	15	2025	0	2.9	0	37,785
Colorado	2004	30	2020	8.4	1.65	4,780	28,987
Hawaii	2004	100	2045	0	3.1	0	9,393
Iowa	1991	1	2000	57.3	2.15	4,201	33,160
Illinois	2007	25	2026	18.4	4.2	3,580	15,599
Kansas	2009	20	2020	45.9	4	5,914	25,839
Minnesota	2007	30	2020	46.15	3.65	6,124	42,171
Missouri	2008	15	2021	2.05	2	427	22,663
North Carolina	2007	12.5	2021	2.95	3	2,490	96,816
New Hampshire	2007	12.8	2025	1	1.8	19	7,846
New Mexico	2004	80	2040	2.55	3	1,663	14,861
Ohio	2008	8.5	2026	14.75	8.25	5,148	85,027
Oregon	2007	50	2040	1	4.6	2,624	33,212
Virginia	2020	100	2050	8.55	3.2	3,397	90,430

terms of this stringency, which can be as high as 100 percent in Hawaii (in 2045) and Virginia (in 2050).

In Table 2, we report summary statistics for the other 18 states that do not exempt their municipals. Other than the municipal exemption, the distributions of mandate start dates, maximum green requirements and year the maximum target is achieved are not dissimilar to those from the 14 states with exemptions.

The literature on the determinants of RPS finds that political ideology aligned with concerns about global warming and affluence of households in the state predict whether a state implements an RPS (Lyon and Yin (2010), Carley and Miller (2012)). Local economic concerns play a minor role. That is, policymakers in these states are driven by a desire to contribute to carbon abatement to mitigate the risks of global warming. Justifications for the exemption include that municipal producers do not cause as much damage to the climate in the first place; or they might not have the

Table 2: Summary of RPS Legislation in States without Exemptions

This table presents summary details of the passage of Renewable Portfolio Standards regulation in the 18 states that have thus far enacted the legislation without municipality exemptions. For the number of municipal and investor-owned suppliers, and their sales in gigawatt hours, we take the time series average for a given year.

State	Mandate Start	Maximum Green %	Year Max Achieved	No. Municipal	No. Investor-Owned	Municipal Sales (gwhrs)	Investor-Owned Sales (gwhrs)
California	2002	60	2030	13.15	7.55	38,027	190,115
Connecticut	1998	40	2030	1.65	1.7	387	2,718
District Columbia	2005	90	2041	0	0	0	0
Delaware	2005	21.5	2026	1.82	0	222	0
Maine	1999	84	2030	0	1.83	0	1,689
Maryland	2004	50	2030	1.6	0	284	0
Massachusetts	2002	100	2090	8.85	3.55	2,829	15,156
Michigan	2008	15	2021	18.05	8.75	4,631	91,907
Montana	2005	15	2015	0	2.1	0	1,076
Nevada	1997	50	2030	0	3.65	0	30,303
New Jersey	1999	52.5	2045	1	3.7	627	46,869
New York	2004	70	2030	4.25	9.25	951	95,247
Pennsylvania	2004	7.5	2020	1	7	292	27,979
Rhode Island	2004	100	2033	0	1	0	11
Texas	1999	5	2025	11.8	4.55	40,173	47,342
Vermont	2015	75	2032	4.75	2.2	529	4,244
Washington	2006	15	2020	3	3.95	14,204	32,038
Wisconsin	1999	10	2015	9.9	8	2,068	50,272

Table 3: Producer-Type Summary Statistics

This table presents summary statistics for our data at the producer-type level. This data covers all 14 states that passed RPS with a municipal exemption. The left three columns refers to data for investor-owned suppliers. The right three columns show the same summary statistics for municipal suppliers. We have $N = 200$ investor-owned-type-by-year observations and $N = 167$ municipal-type-by-year observations.

	Investor-Owned			Municipal		
Variable	N	Mean	SD	N	Mean	SD
Number of Producers	200	2.4	1.1	167	23	21
Observations in Post Period	200	0.71	0.45	167	0.72	0.45
Renewable/Non-Renewable Capacity	200	0.014	0.039	167	0	0
Per Firm CO2 Emissions (metric tons)	200	5,998,148	4,685,296	167	253,942	476,152
Electricity Prices (per KWhr)	200	$0.10	$0.49	167	$0.11	$0.73

resources to implement an aggressive abatement plan. Such ideological motivations are similar to the sorts of policy variations used in the studies of European climate policies by Känzig (2021) and Metcalf and Stock (2020) for identification.

Numbers and sales by producer type. In Table 1, we also report for each state the time-series average of the number of producers of each type and the time-series average of the total sales of the two types of producers. Investor-owned firms mostly operate in one state, but around 5.4% operate in more than one state. While there are a greater number of municipal producers compared to investor-owned ones, investor-owned producers' sales are much higher than those of the municipal producers. For instance, in the state of Illinois, there are on average in a typical year around 4.2 investor-owned producers who generate 15,599 gigawatt hours. There are 18.4 municipal producers who generate 3,580 gigawatt hours.

3.2 Renewables, Carbon Emissions and Electricity Prices

Earlier work (Greenstone and Nath (2020), Upton Jr and Snyder (2017), Deschenes et al. (2023)) uses the staggered timing of RPS introduction across all 32 states along with state level controls to show that in the wake of RPS, ratios of renewable to non-renewable capacity go up, emissions intensities go down, and consumer prices increase. Using states with municipal producer exemptions, we examine the extent to which RPS affects these same variables using just the 14 states that exempt their municipal producers. This affords us a control group that offers within state-year variation. Hence, we can implement a restrictive identification procedure whereby we control for a state-year fixed effect. This inclusion directly addresses the concern that unlike states will be compared to one another.

Table 3 reports the summary statistics for our variables of interest at the producer-type-state-year level. To test the impact of RPS, we use a difference-in-differences approach, wherein we estimate the following expression:

$$y_{i,s,t} = \alpha_{s,t} + \beta_0 corp_i + \beta_1 \left(corp_i \times post_{s,t}\right) + e_{i,s,t}, \tag{1}$$

where y_{ist} represents the variable of interest. The indicator variable $corp_i$ equals one if the observation is for the investor owned producer type and subject to the RPS and zero otherwise. The key independent variable of interest is interaction of $corp_i$ with the indicator $post_{st}$ which equals one if state s has an RPS policy in place at time t.

We always include a state-year fixed effect $\alpha_{s,t}$ that allows us to restrict our comparison to corporate and municipal suppliers operating in the same state at the same time. We also include the investor-owned dummy to capture any fixed systematic differences between investor-owned and municipal suppliers.

The findings are presented in Table 4. In column (1), we find that RPS leads to an increase in the renewable to non-renewable capacity ratio of treated firms by 0.0224, which is statistically significant at the 1% level. This is similar to the finding in the literature that RPS is enforced and had a significant effect on the build up of renewables

Table 4: Firm/Producer-Type Level Difference in Differences

This table presents results from a difference-in-differences estimation design that examines the impact of RPS on three producer-type variables. In Column (1), we look at how the ratio of renewable to non-renewable capacity ratio of investor-owned suppliers changed relative to exempt municipal suppliers in the post-RPS period. In Column (2), we compare the average firm CO2 emissions in metric tons of corporate producers within a state to that of exempt municipal peers. In Column (3), we compare the log of the average consumer prices for corporate to the log average of consumer prices for municipal producers.

Dependent Variables:	Renewable/Non-Renewable Ratio	Average Firm Emissions (CO2)	Log of Electricity Prices
Model:	(1)	(2)	(3)
Variables			
corp	0.0006**	7,756,508.5***	-0.0974***
	(0.0003)	(826,846.7)	(0.0175)
corp × post	0.0224***	-2,701,193.7***	0.0416*
	(0.0044)	(904,025.5)	(0.0237)
Fixed-effects			
State-Year	Yes	Yes	Yes
Fit statistics			
Observations	367	367	367
R^2	0.56537	0.77278	0.95976
Within R^2	0.18555	0.63509	0.15853

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

capacity.

In column (2), the dependent variable is the average firm emissions in tons of CO2 for investor-owned and municipal producers. We find that investor-owned producer types reduce their emissions by roughly 2.7 million tons after passage of RPS.

In column (3), the dependent variable is the average electricity price for investor-owned versus municipal producers. RPS leads to around 4% higher electricity prices, with a statistical significance at the 10% level. This value is slightly more conservative than the figures reported previously in the RPS literature (around 11% in both Greenstone and Nath (2020) and Upton Jr and Snyder (2017)). In other words, some of the cost is passthrough to consumers, which is similar to findings for other types of climate policy such as the European ETS (Känzig and Konradt (2023)). However, this price effect appears smaller in magnitude than for these policies, suggesting that producers bear relatively more of the cost associated with RPS.

4 Bond Issue Level Analysis: Impact of RPS on Credit Spreads

Having established existing results regarding the benefits of RPS in terms of carbon emissions reduction through building renewables capacity and the passthrough of some of these costs to consumers, we next turn to the effect of RPS on cost of capital for producers. We address this question using bond issue level data as firms need to issue debt to finance their investments in renewables capacity. Hence, we can see if RPS has any effects on firm financial health, either in terms of debt issuance or credit spreads.

4.1 Comparing investor-owned versus municipal producer debt

Unsurprisingly, municipal bond issues differ in systematic ways from investor-owned bond issues. Table 5 reports the mean and standard deviation of the variables of interest by municipal versus investor-owned issues. First, we have 322 issues by municipals, versus 1,739 corporate issues. For both types of producers, we find there are more debt issues for investor-owned utilities than municipals post RPS — 39% compared to 27%.

The typical municipal debt issue is 54 million dollars with a standard deviation of 103 million dollars. For investor-owned, the mean is 241 million dollars, with a standard deviation of 231 million dollars. Municipals borrow at longer maturities — 19 years, compared to 16 years for investor-owned.

The mean yield of municipal issues is 4.3%, while it is 5.8% for investor-owned. The standard deviation of yields is also larger for investor-owned, 1.9% compared to 1.4% for municipals. This difference in yields reflects the fact that municipals typically have a higher Moody's rating, 1.3 compared to 6.7 for investor-owned.[14] All municipal

[14] Here a value of 1 corresponds to Aaa, and a value of 17 corresponds to Caa1. A value of 7 indicates a Moody's rating of A3.

debt is investment grade, while 95% of the investor-owned debt is investment grade.

Characteristics-adjusted bond yields and issue amounts. Since systematic differences exist between municipal and investor-owned issues that could distort our findings, we perform an adjustment to bond yields, issue amounts, maturity, and bond rating at issuance using a characteristics-based benchmarking as in Daniel et al. (1997). Specifically, we form 5x5x5 portfolios based on Moody's ratings, maturity, and issue size for adjusting yields. For each of these 125 portfolios, we calculate the median yield at issuance. We then subtract this median yield from the yields of all bonds within the same grouping. The means and standard deviations of these characteristics-adjusted yields are also given in Table 5.

We conduct a similar benchmark adjustment for issue amounts, but we base the adjustment on Moody's ratings, maturity, and yields. The resulting distributions are shown in Figure 1. This figure shows that for both yields and issue amounts, the two distributions after adjustment are not too far apart from each other.[15]

In addition to issue amount, maturity yield, and credit rating information, we also have information on the tax treatment of the various bonds as well as the security type. We include these additional variables as covariates in our issuance and yield regressions below. As corporate bonds do not vary in their tax codes, nor do they have specified purposes as is the case for municipal bonds, we simply assign a single tax and security type identifier to these issues.

[15] An additional significant distinction between municipal and corporate bonds lies in their callability. Although adjusting for this difference can be challenging, it should not impact the identification process as long as the rates of callability remain relatively stable over the period in question.

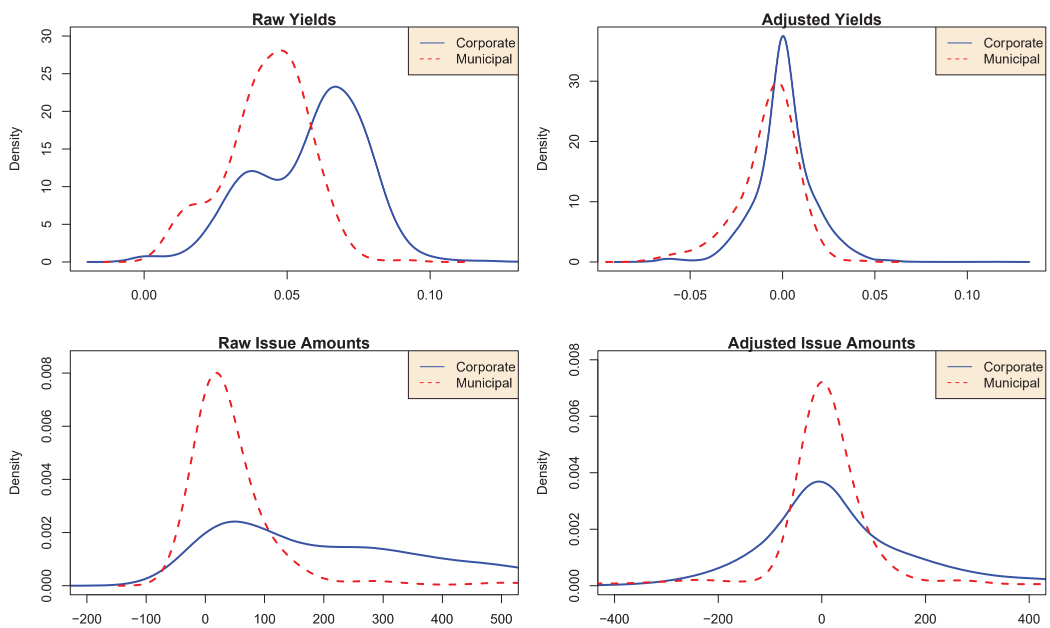
Table 5: Summary Statistics of Bond Issue Level Data

This table presents summary statistics on our final dataset of bond data. The data we collect runs from 1990 to 2021. Adjusted Yields are constructed using a characteristic benchmarking approach as described in Section 4.1. Tax code refers to one of four options: CB is taxable corporate bond, E is municipal bond exempt from federal tax, A is municipal bond taxable subject to AMT (Alternative Minimum Tax), and T is taxable municipal bond. Security Type refers to one of three options: CB is simply for corporate bonds, GO is general obligation municipal bond, and RV is revenue municipal bond.

	Municipal			Investor-Owned		
Variable	N	Mean	SD	N	Mean	SD
Yield	322	0.043	0.014	1739	0.058	0.019
Maturity (years)	322	19	7.1	1739	16	11
Issue Amount ($mn)	322	54	103	1739	244	233
Moody Rating (rank)	322	1.3	0.98	1739	6.7	2.5
Investment Grade	322	1	0	1739	0.95	0.21
Observations in Post Period	322	0.27	0.44	1739	0.39	0.49
Adjusted Yield	322	-0.0063	0.013	1739	0.0011	0.013
Adjusted Issue Amount ($mn)	322	15	84	1739	39	141
Year	322	2002	5.6	1739	2004	9.6
Security Type	322			1739		
... CB	0	0%		1739	100%	
... GO	32	10%		0	0%	
... RV	290	90%		0	0%	
Tax Code	322			1739		
... A	14	4%		0	0%	
... CB	0	0%		1739	100%	
... E	275	85%		0	0%	
... T	33	10%		0	0%	

Figure 1: Distributions of Adjusted Yields and Issue Amounts

This figure plots binned kernel density estimates of the distribution of the adjusted yield and issue amount of bond issues from municipal and investor-owned utilities, adjusted using a characteristic benchmark approach similar to Daniel et al. (1997). We construct benchmarks by forming 5x5x5 portfolios on Moody's rating, maturity, issue size, and yields. We then subtract the median yield/issue amount/maturity/bond rating in each portfolio from the actual value for each issue inside that portfolio.



4.2 Difference-in-Differences

We first conduct a difference-in-differences estimation design that focuses on the bond issue level measures. We compare how yields and issue amounts change relative to municipal issuers in the post RPS period. Yields and issue amounts are adjusted using the approach outlined in Section 4.1.

In each case, our estimation follows the same basic specification described by Equation 2:

$$y_{i,j,s,t} = \alpha_{s,t} + \varphi_j + \tau_j + \phi_i + \beta \left(corp_i \times post_{s,t} \right) + \Psi \mathbf{K}_{i,j,t} + e_{i,s,t}, \tag{2}$$

where the subscript i indicates firm and subscript j indicates the bond issue. We always include a state-year fixed effect, $\alpha_{s,t}$, that allows us to restrict our comparison to corporate and municipal suppliers operating in the same state at the same time. We also include a firm fixed effect (ϕ_i), a security type fixed effect (φ_j), and a tax code fixed effect (τ_j).[16]

For both yields and issue amounts, we control for the log of maturity, and an indicator for the rating band that the bond is assigned by Moody's, $\mathbb{I}(rating_{i,j,t} \in g)$.[17] These bands group bond ratings into similar risk profiles, and allow us to non-linearly control for the impact of bond rating on yield. These bands distinguish between high investment grade, low investment grade, and various junk bond statuses, which are likely to have strongly discontinuous impacts on bond yields, thus justifying the use of the non-linear specification. When estimating the impact on yield spreads, we also control for the log of issue amount. We include both raw controls, and controls interacted with the corporate dummy.

[16]The security type of issue j takes a value of CB for corporate bonds, GO for general obligation municipal bonds, and RV for revenue municipal bonds; the tax code takes a value of CB for corporate bonds, A for municipal bonds taxable subject to AMT (Alternative Minimum Tax), E for municipal bonds exempt from federal tax, and T for taxable municipal bonds.

[17]We make one adjustment to these bands, which is to include 'Aaa' rated bonds with 'Aa1', 'Aa1', and 'Aa3' bonds.

Table 6: Bond Issuance and Credit Spreads Difference in Differences

This table presents results from a difference-in-differences estimation design that examines the impact of RPS on bond issue-level variables. In Column (1), we assess the impact on adjusted yields, and in Column (2), on adjusted issue amounts. We include state-year, and issuer fixed effects in all three regressions. In columns (1) and (2), we control for Moody's ratings band and the log of maturity. In column (1) we also control for the log of issue amount.

Dependent Variables:	Adjusted Yields	Adjusted Issue Amount
Model:	(1)	(2)
Variables		
corp × post	0.0099***	51.66
	(0.0026)	(32.70)
Controls	Yes	Yes
Fixed-effects		
State-Year	Yes	Yes
Issuer	Yes	Yes
Security Type	Yes	Yes
Tax Code	Yes	Yes
Fit statistics		
Observations	2,050	2,050
R^2	0.76049	0.67895
Within R^2	0.19169	0.05921

Clustered (state-year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Our results are shown in Table 6. We find evidence that RPS led to higher yield spreads for corporate issuers of around 99 basis points. The coefficient on issue amounts is positive, though statistically insignificant.

State by state estimates. Given that states that pass RPS with exemptions for municipal producers have a natural treatment and control group, we can also estimate bond market effects for each state separately and then aggregate. We do this by running the following specification for each of the 14 states with municipal exemptions in our sample:

$$y_{i,j,t} = \phi_i + \tau_t + \beta_s \left(corp_i \times post_t\right) + \Psi \mathbf{K}_{i,j,t} + e_{i,j,t} \tag{3}$$

Note that we no longer include a state-time fixed effect, as we are comparing within state by construction. We use the same set of controls as in Section 4.2. This procedure generates a set of fourteen $\{\beta_s\}$ coefficients.

We aggregate by taking a weighted average using one of two procedures: (i) we weight by the number of observations, and (ii) we weight by the inverse of the standard error of the coefficient (precision weighting). When weighting by number of observations, we construct standard errors by bootstrapping over the estimated coefficients. When weighting by precision, we construct the standard error using the following formula:

$$\text{Variance of the weighted average} = \frac{\sum(w_i^2 \cdot \sigma_i^2)}{(\sum w_i)^2} \tag{4}$$

where w_i is the weight for the i^{th} estimate, i.e. the precision weight (the inverse of the variance, σ_i^2), and σ_i^2 is the variance of the i^{th} estimate.

Details can be found in Table 7. We find statistically significant and positive coefficients in both cases, with a magnitude very similar to our findings using the entire sample and controlling for a state-year fixed effect as in the standard difference-in-differences approach (Table 6).

Table 7: State-by-State Results

This table presents results from a difference-in-differences estimation design that aggregates individual state-level regressions into a weighted average. We estimate Equation 3 for each of the 14 states that passed RPS with a municipal exemption, and then aggregate using either the number of observations or the precision of the estimates as weights. Standard errors are constructed with bootstrapping for observation weighting, and using the formula in Equation 4 for precision weighting.

Dependent Variables:	Adjusted Yields		Adjusted Issue Amounts	
Weighting:	Observations	Precision	Observations	Precision
Variables				
corp × post	0.0084*	0.0114***	28.74	104.69***
	(0.0044)	(0.0029)	(58.65)	(30.34)
Controls	Yes	Yes	Yes	Yes
Fixed-effects				
Year	Yes	Yes	Yes	Yes
Issuer	Yes	Yes	Yes	Yes
Security Type	Yes	Yes	Yes	Yes
Tax Code	Yes	Yes	Yes	Yes

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.3 Panel Regression Specifications

We run the following issue-level regression, where $r_{j,s,t}$ denotes the year of the issue relative to the passage of RPS in state s:

$$\begin{aligned} y_{i,j,s,t} =& \phi_{s,t} + \alpha_i + \varphi_j + \tau_j + \beta_{-5} D_{r_{s,t} \leq -5} \times corp_i \\ &+ \sum_{-4 \leq r_{j,s,t} \leq -2} \beta_r D_r \times corp_i + \sum_{0 \leq r_{j,s,t} \leq 8} \beta_r D_r \times corp_i \\ &+ \beta_9 D_{r_{j,s,t} \geq 9} \times corp_i + \Psi \mathbf{K}_{i,j,t} + \varepsilon_{i,j,t} \end{aligned} \tag{5}$$

Here $y_{i,j,s,t}$ is a measure of the issue j, by firm i, operating in state s, in year t; $\phi_{s,t}$ is a state-year fixed effect; ψ_i is a firm fixed effect; φ_j and τ_j are fixed effects for security type and tax code of issue j respectively; $corp_i$ is an indicator taking a value of 1 if the firm i is a corporate/investor-owned firm; D_r is an indicator that takes a value of one if the year of the issue t, is r years relative to the passage of the RPS legislation in state s. Note that, as before, we bin all observations 3 years before and all observations 7 years after passage.

We also include a vector of issue-level controls, $\mathbf{K}_{i,j,t}$. We control for the log of the maturity in years of the debt issuance, the log of the value of the issuance in \$mns, and an indicator for the rating band that the bond is assigned by Moody's, $\mathbb{I}(rating_{j,s,t} \in g)$.[18] These bands group bond ratings into similar risk profiles, and allow us to non-linearly control for the impact of bond rating on yield. These bands distinguish between high investment grade, low investment grade, and various junk bond statuses, which are likely to have strongly discontinuous impacts on bond yields, thus justifying the use of the non-linear specification.

We remove a control when it measures the same bond level characteristic as the dependent variable, i.e. the regression for issue amounts does not include the log of the issue amount as a control. In all cases, we include both the control, and the control interacted with the $corp_i$ indicator.

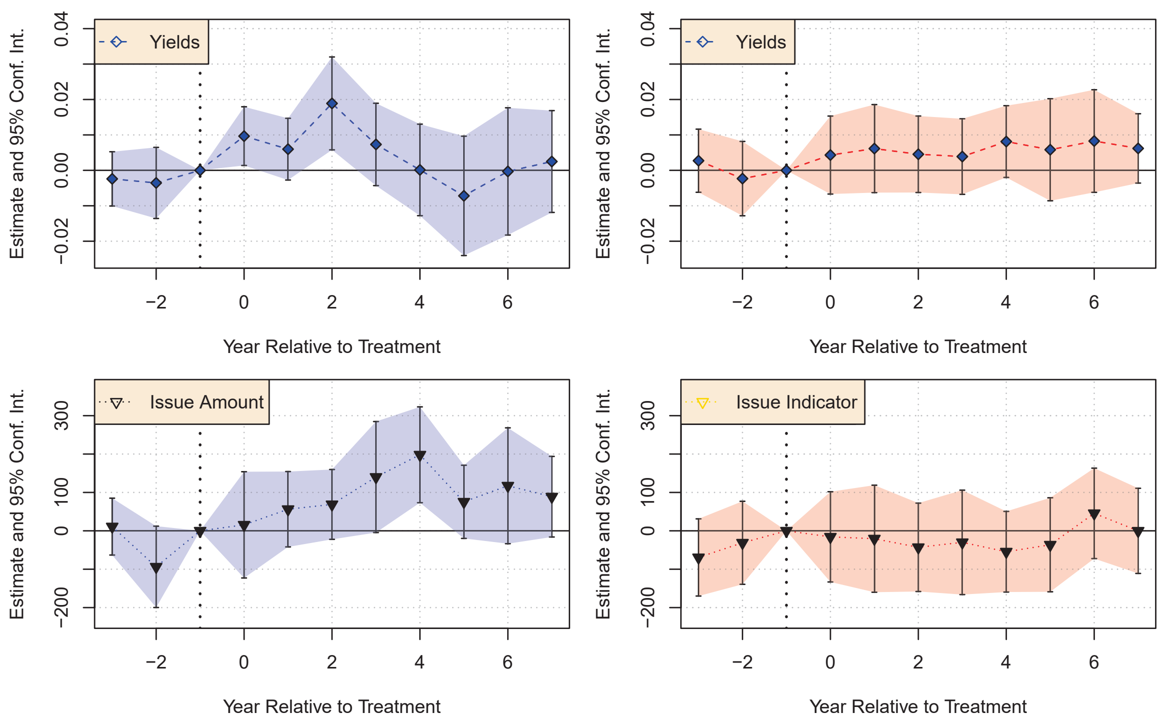
We plot the event studies from our estimations for states with and without exemptions in Figure 2. Specifically, we plot the values of the fitted coefficients, $\{\beta_{-3}, ..., \beta_7\}$, that capture the differential response of corporate (treated) to municipal producers (exempted) from the RPS mandates.

The figures in the left column show our findings at the issue-level for states that instituted municipal exemptions. We plot coefficients for the yields and bond issue amount, respectively. Reassuringly, we find limited evidence of significant pre-trends in all our specifications. The figures in the right column show the same results found using bond issues in states that instituted RPS without exemptions.

[18] We make one adjustment to these bands, which is to include 'Aaa' rated bonds with 'Aa1', 'Aa1', and 'Aa3' bonds.

Figure 2: States with and without exemptions– Event Study

In this figure we plot the results of our event study specification. We include results for states with municipal exemptions (left column, in blue) and without exemptions (right column, red). The top row shows results for yields, and the bottom row for issue amounts. We adjust these dependent variables using the procedure outline in Section 4.1. We winsorize adjusted yields and adjusted issue amounts at the 5% level. Confidence bands are given at the 95% level.



For states with municipal exemptions, we see economically and statistically significant impacts across all variables of interest. First, there is a spike in characteristics-adjusted bond yields coinciding with debt issuance in the early years.

Second, bond issue amount increases significantly following RPS treatment. In the years before treatment, the coefficient of interest is close to zero, indicating that bond issuance by investor-owned firms did not differ from their municipal counterparts. But there is a very rapid increase in bond issuance following treatment, peaking at around 4 years after treatment. There is a reversion in years 6 to 7 from the peak, but the mean of these years is still far above the pre-treatment years. However, the standard error bands on these later years are wider than the early years following treatment.

4.4 Placebo Analysis: States without Municipal Exemptions

Our identification strategy supposes that investor-owned issuers affected by RPS legislation are comparable to municipal issuers in the same state-year that are exempt from the legislation. As a test of the validity of this result, we run the same exercises as in our main analysis, but restrict to the states that did not allow municipal exemptions. If our main finding is robust, we should not see a significant difference in the yields or issuance of investor-owned relative to municipalities in the same state-year in the wake of the RPS legislation.

There are 18 states that enacted RPS legislation without municipality exemptions, so in this exercise we restrict to municipal and investor-owned bond issues in these 18 states. Our specification is the same as in our main analysis.

Finally, consistent with the validity of our identification strategy, we do not observe any significant response in the investor-owned to municipal spread in states without municipal exemptions.

4.5 Triple Difference-in-Differences

We conduct a triple difference-in-differences estimate of the credit spread and issue amount by taking advantage of the fact that many states passed RPS without municipal exemptions. This additional difference allows us to compare the yield spread between corporate and municipal suppliers post RPS passage of states with exemptions, versus states without exemptions. Specifically, we estimate the following expression, using all issuances in the 32 combined states:

$$\begin{aligned} y_{i,j,s,t} = &\phi_{s,t} + \alpha_i + \varphi_j + \tau_j + \beta_0 \times corp_i \times post_{s,t} \\ &+ \beta_1 \times exempt_s \times corp_i \times post_{s,t} + \Lambda \mathbf{K}_{i,j,t} + \nu_{i,j,t} \end{aligned} \tag{6}$$

where $y_{i,j,s,t}$ is the yield of issue j, by firm i, operating in state s, in year t; $\phi_{s,t}$ is a state-year fixed effect; ψ_i is a firm fixed effect; φ_j and τ_j are fixed effects for security type and tax code of issue j respectively; $corp_i$ is an indicator taking a value of 1 if the firm i is a corporate/investor-owned firm; $post_{s,t}$ is an indicator that takes a value of one if the issue occurs after RPS passage in that state; $exempt_s$ is an indicator that takes a value of 1 if the state that the firm operates in instituted a municipal exemption as part of RPS legislation; and $\mathbf{K}_{i,j,t}$ is a vector of issue level controls identical to those in our main specification (Equation 5).

The key coefficient of interest is β_1, i.e. the one associated with interaction term, $exempt_s \times corp_i \times post_{s,t}$. This coefficient tells us the difference in corporate-to-municipality spreads/issue amounts between states with and without exemptions. Note that the coefficient β_0 captures the impact of RPS legislation on corporate to municipal yield spreads in states without municipal exemptions. A test of our identification strategy is that β_0 is not statistically different from zero.

Our results from the triple difference-in-differences are presented in Table 8. Once

Table 8: Triple Difference-in-Differences for States with and without Exemptions

This table presents results of our triple difference-in-differences estimation. Here we pool issue level observations from all 32 states that passed RPS legislation. We include an indicator, $exempt_s$, that takes a value of 1 if the state instituted a municipal supplier exemption. The coefficient on $corp \times post$ captures the change in post RPS legislation spreads between corporate and municipal suppliers in states without municipal exemptions, and the coefficient on $exempt \times \times post$ captures the differential effect in states with exemptions.

Dependent Variables:	Adjusted Yields	Adjusted Issue Amt.
Variables		
corp × post	0.0029	57.98***
	(0.0018)	(18.60)
exempt × corp × post	0.0066**	4.764
	(0.0032)	(35.96)
Controls	Yes	Yes
Fixed-effects		
State-Year	Yes	Yes
Issuer	Yes	Yes
Security Type	Yes	Yes
Tax Code	Yes	Yes
Fit statistics		
Observations	6,668	6,668
R^2	0.77530	0.70137
Within R^2	0.13157	0.03803

Clustered (State-Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

again, consistent with our identification, we do not find a significant impact on corporate to municipal spreads in states without exemptions. By contrast, we find a positive and statistically significant coefficient for adjusted yields in exempt states of 66bps.

5 Quantitative Analysis

In this section, we combine our reduced-form estimates with a structural corporate-bond pricing model from Longstaff and Schwartz (1995) to impute the annual abatement costs to firms to meet RPS. This abatement cost is a key quantity in the evaluation of climate policy.

5.1 Model

The value of a firm's assets (V) evolves according to a geometric Brownian motion:

$$dV = \mu V dt + \sigma V dZ_1, \tag{7}$$

where σ is a constant representing asset volatility, and Z_1 is a standard Wiener process.

The short-term riskless interest rate is defined by the following process:

$$dr = (\zeta - \beta r)dt + \eta dZ_2 \tag{8}$$

where ζ, β, and η are constants and Z_2 is another standard Wiener process.

We first construct the value of a hypothetical riskless discount bond, $D(r,T)$, with short-term riskless interest rate r and maturity T following Vasicek (1977):

$$D(r,T) = \exp\left(A(T) - B(T)r\right), \tag{9}$$

where $A(T) = \left(\frac{\eta^2}{2\beta^2} - \frac{\alpha}{\beta}\right)T + \left(\frac{\eta^2}{\beta^3} - \frac{\alpha}{\beta^2}\right)(\exp(-\beta T) - 1) - \left(\frac{\eta^2}{4\beta^3}\right)(\exp(-2\beta T) - 1)$ and $B(T) = \frac{1-\exp(-\beta T)}{\beta}$.

We then construct the value of a risky discount bond, $P(X,r,T)$, in the following way:

$$P(X,r,T) = D(r,T) - \omega D(r,T)Q(X,r,T), \tag{10}$$

where ω represents the proportion of the debt not recovered in the case of default, $Q(X,r,T)$ is a measure of the cumulative default probability, and X represents the distance to default, which is defined as the ratio of firm value at issuance (V) to the lower bound value of the firm that triggers default ($\underline{V}$), which is assumed to be constant. When $X = 1$, the firm defaults. We construct $Q(X,r,T)$ using the same method as in

Longstaff and Schwartz (1995).

To price coupon bonds, we simply construct a compound portfolio of risky discount bonds, each with a face value equal to the coupon rate, C. We do this by creating several bonds with increasing maturities and face value C, and then form a composite cash flow portfolio that covers the entire range of the maturity of the coupon bond, including the final face value payment. Formally:

$$P^c(C, X, r, T) = P(X, r, T) + C \sum_{j=1}^{TN} P\left(X, r, \frac{j}{N}\right) \tag{11}$$

We select a value of N that is sufficiently large to approximate continuous time compounding, in our case 200. This gives us $P^c(C, X, r, T)$. Then, we can calculate the yield-to-maturity that equates the present value of a coupon bond to its price as defined by $P^c(C, X, r, T)$, and subtract this from the same object for a risk-free coupon bond equivalent to calculate the implied yield spread, $y(C, X, r, T)$.

To better connect the hit to credit spreads from climate policy, we consider a tax representation of RPS. Let τ be the annual tax rate expressed as a fraction of asset value, i.e. the abatement cost to meet RPS. We convert these annual taxes into a lump sum impact on the initial value of firm assets, V_0. We assume a N-year distance from V_0, and discount the annual tax impact, τ, over this period to arrive at V_0^τ. As such, we construct the tax impact of RPS by adjusting our estimated model to a hypothetical in which the firm was not subject to RPS restrictions: i.e. did not face the tax. We construct this adjustment in the following way:

$$V_0^\tau = (1 + \delta^\tau) V_0 \tag{12}$$

$$\delta^\tau = \tau \sum_{t=1}^{N} (1 + r)^{-t} \tag{13}$$

Given that the expected value of V is linearly related to V_0, and X is simply the ratio of V to the constant default boundary, $\underline{V}$, this adjustment can then be directly applied to X to give the implied valuation of the firm after RPS, $X^\tau = (1 + \delta^\tau)X$. The yield spread impact of RPS relative to the counterfactual bonds that are not taxed (i.e. municipal bonds) is then given by the following expression:

$$\Delta y^{RPS} = y(C, X, r, T) - y(C, X^\tau, r, T) \tag{14}$$

5.2 Model Estimation

To account for the fact that issuance changes after RPS, we estimate our model using data in the post-RPS implementation period. That is, credit spreads have already priced in the implications of the RPS tax.

Parameters: We abstract away from interest rate risk by setting $\eta = 0$ and $\beta = 1$. The remaining parameters in the model are: the risk-free interest rate, r, the recovery rate, $(1-\omega)$, the coupon rate, C, the volatility of firm assets, σ, the distance to default, X, and the RPS-implied annual tax rate, τ.

We use data values for r, $(1 - \omega)$, C, and σ. We use a risk-free interest rate (r) of 4.05%, which is the average yield of a 10-year treasury bond in the post-RPS sample period. For the recovery rate $(1 - \omega)$, we use a value of $(1 - 0.5131)$, consistent with Huang and Huang (2012). We set the coupon rate at 5.8%, which is the average coupon rate in our reduced form sample. We calculate a volatility of 15.9% using daily returns of public firms operating in the power sector (SIC code 4911).[19]

[19] Specifically, we calculate measures of historical volatility of firms in the power sector by calculating the standard deviation of daily returns from 2007 to 2023. We choose 2007 as our start point as this is the median year in which RPS is enacted in our sample of states that institute municipal exemptions. We then form a value-weighted industry portfolio of all the firms in our sample and construct a portfolio variance that accounts for correlations across stocks. This process leads us to a value of σ of 15.9%.

Table 9: Parameters

This table documents the parameters of our model. Values for the risk-free interest rate (r), the recovery rate, $(1-\omega)$, the coupon rate, C, the volatility of firm assets, σ, are calculated using historical data. The values for distance to default for each ratings band ($\{X^{Aaa}, ..., X^{B}\}$), and the implied tax rate of RP (τ), are estimated. Details of estimated values for these parameters can be found in Table 10.

Parameter	Symbol	Source
Risk-free interest rate	r	Sample average from reduced form dataset (4.03%)
Debt not recovered after default	ω	Huang and Huang (2012) (0.5131)
Coupon Rate	C	Sample average from reduced form dataset (5.8%)
Volatility	σ	Calculated using daily returns in power sector (15.9%)
Distance to Default	$\{X^{Aaa}, ..., X^{B}\}$	Estimated in Section 5.2
Implied Tax Rate of RPS	τ	Estimated in Section 5.2

We estimate the remaining parameters, matching model generated moments to their data equivalents. To account for variation in distance to default for different bonds, we estimate X for each rating band: Aaa, Aa, A, Baa, Ba, and B. We therefore have six parameter values for X, i.e. $\{X^{Aaa}, ..., X^{B}\}$. We estimate the tax rate, τ, across the whole sample.

Moments: To estimate our parameters, we target seven moments. We use yield spreads, calculated for each of the six Moody's ratings bands in our sample, to identify our distance to default (X) parameters. We use our triple difference-in-differences estimate of the impact of RPS on yields to identify the tax rate, τ.

For the yield spread, we use the post-RPS sample average of the yield at issue, minus the 10-year treasury bond yield, binned by ratings band.

We use the triple difference-in-difference estimate from our reduced form as the final moment. To construct the model moment equivalent, we use the procedure outlined in Section 5.1. For each possible τ tax rate, we calculate the yield spreads for each of our ratings bands under the new value for X from avoiding the tax net the original estimated yield spread. We then take the weighted average of these yield differences, weighting by the number of bonds in each rating band in our dataset.

Estimation Approach: We estimate our parameters using GMM. This is a deliberate choice, as it allows us to incorporate both bond-level data on yields, and also our reduced form finding of the impact of RPS on credit spreads.[20]

We search for parameters that minimize the squared gap between our model moments and our observed data moments, $\hat{\theta}$:

$$\hat{\theta} = \arg\min_{\theta \in \Theta}(g(\theta)'Wg(\theta)) \tag{15}$$

where $g(\theta)$ is the squared distance between the model moments implied by θ and the data moments, and W is the weighting matrix (we use the identity matrix). We then construct standard errors using the typical sandwich formula:

$$Var(\hat{\theta}) = (G'WG)^{-1}G'W\Omega WG(G'WG)^{-1} \tag{16}$$

where G is the Jacobian of $g(\theta)$, evaluated at $\hat{\theta}$, and Ω is the covariance matrix of $g(\theta)$. We construct Ω by bootstrapping the construction of our data moments 1,000 times, and then calculating the covariance matrix across these simulations.

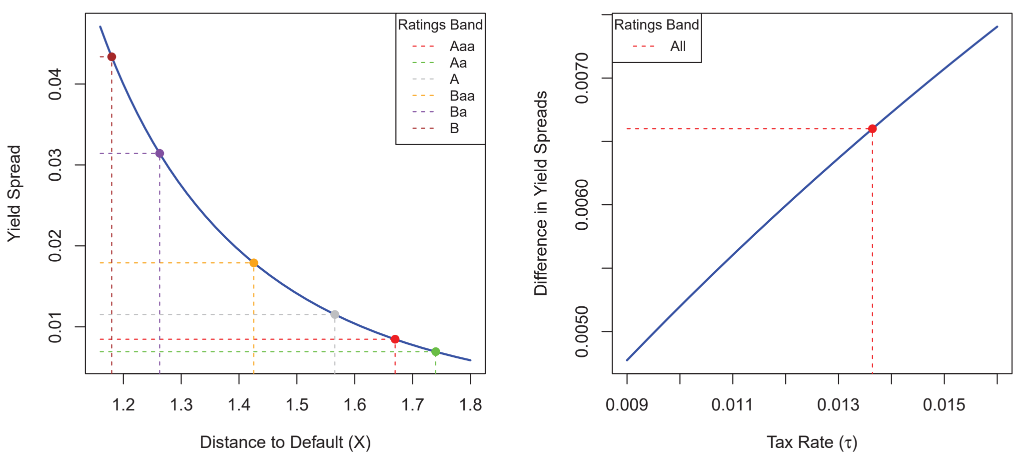
Identification: Given the nature of the model outlined in Section 5.1, we have a tight link between each of our distance to default parameters and the corresponding yield spread, and between the implied tax rate and the difference in implied yield spreads. This can be illustrated visually as in Figure 3.

In Figure 3, we plot how our model moments vary with the parameters we seek to estimate. In the left panel, we show how distance to default (X) impacts yield spreads. Consistent with the intuition of the model, there is a clear and monotonically

[20] Several papers employ a Maximum Likelihood approach to estimating corporate-treasury yield spreads (Duan (1994), Ericsson and Reneby (2005)). This approach, while having advantages over GMM, does not easily afford the flexibility of also using reduced form results to estimate parameters.

Figure 3: Identification of Model Parameters

This figure shows how the moments we use to identify our model parameters vary as we modify those parameters. In the left panel, we show how distance to default (X) impacts model yield spreads. For each of the ratings bands, we plot the data moment as a horizontal dashed line. The vertical dashed line from the intersection of these data moments and the model generated moment function shows how we identify X at the rating band level. In the right panel, we show how the tax rate (τ) affects the difference in yield spreads induced by RPS. As this is calculated using data across all ratings bands, we plot a single data moment: the triple difference-in-differences estimate of the impact of RPS on bond yields.



decreasing relationship between yield spreads and distance to default. As such, for a given yield spread, there exists one and only one value of X that would generate this value in the context of our model.

In the right panel, we show how the difference in yield spreads varies with the implicit tax rate, τ. Again, consistent with the model's intuition, the relationship between τ and the difference in credit spreads is monotonically increasing. This also guarantees that, for a given difference in yield spreads, there exists a single τ that leads our model to generate that value.

Results: Our results can be found in Table 10. In Panel A we show, for each of the ratings bands, the estimated values of distance to default (X), the data yield spread we aim to match, and the implied model yield spreads. We are able to generate a perfect match to the data.

The results here are also broadly consistent with what one would expect, i.e. firms with lower rated bonds are closer to default. The only exception to this pattern is the estimated parameter for *Aaa* bonds. This is likely a feature of the limited number of such bonds in our dataset, as we only observe 6 *Aaa* rated bonds, compared to 113 for *Aa*.

In Panel B we show the estimated value of the tax (τ), the data moment (i.e. the triple difference-in-differences estimate from our reduced form), and the model implied difference in average credit spreads in the untaxed vs. taxed setups. Again, we are able to match the data moment exactly with an imputed tax parameter (τ) of 0.0136, or 1.36%.

Table 10: Model Estimation Results

This table shows the estimated parameters alongside the data moments to be matched, and the implied model moments for these parameters. We include standard errors for parameters in brackets. In Panel A, we show our values for distance to default, X, for each ratings band. X is defined as the ratio between the value of the firm's assets and the default-inducing lower boundary valuation. In Panel B, we show our value for the RPS implied tax, τ. In Panel C, we show the model implied dollar cost to firm value induced by RPS passage. For Panel A, We use post-RPS data on 10-year maturity corporate bond issues to construct yield spreads. In Panel B, we use the triple difference-in-differences result from our reduced form as a measure of the tax impact on yield spreads. In Panel C, we perform the calculation described in Equation 17 to convert the tax rate into a dollar cost to firm value. We set the risk-free interest rate to the sample average of 10-year Treasury bonds $r = 4.05\%$. We set the coupon rate to 5.8% which is the sample average for corporate bonds in our sample period.

Panel A: Distance to Default

Ratings Band	X	Data Yield Spread	Model Yield Spread
Aaa	1.67 (0.21)	85bps	85bps
Aa	1.74 (0.02)	69bps	69bps
A	1.57 (0.03)	115bps	115bps
Baa	1.43 (0.04)	179bps	179bps
Ba	1.26 (0.02)	314bps	314bps
B	1.18 (0.01)	434bps	434bps

Panel B: Tax Rate

Ratings Band	τ	Data Diff in Yield Spreads	Model Diff in Yield Spreads
All	1.36% (0.16%)	66bps	66bps

Panel C: Cost to Firm Value

Ratings Band	Cost to Firm Value
All	\$53.81 (\$6.30)

5.3 Model-implied abatement cost

Using our estimated tax parameter, τ, we can infer in dollar terms how much firm value was reduced by RPS passage. We can then compare this change to the reduction in emissions result we describe in Section 3.2. This will give us an indication of the abatement cost for firms per metric ton of eliminated CO2.

We use Enterprise Value (EV) Multiples tables for US firms to construct asset value. These values represent typical firm ratios of asset value to earnings before interest, taxes, depreciation, and amortization (EBITDA), calculated using Compustat data. For the power sector, this ratio is 11.69.

We then collect income statement data from FERC on firms operating in the Utilities sector. We construct EBITDA, and back out a proxy of firm value using the EV multiple.

Once we have this measures of asset value, we can then establish in dollar terms the annual impact of RPS using our estimated tax parameter, τ. We divide this dollar hit by our estimates of absolute emissions reduction to arrive at a per-firm cost of abatement for one metric ton of CO2, and take the sample average. This approach is described in Equation 17 below:

$$\mathbb{E}[\mathit{Abatement\ Cost\ (\$)}] = \sum_{i \in N} \sum_{t \in T} \frac{\tau \times \mathit{Asset\ Value}_{i,t}}{\beta^{CO2}} \tag{17}$$

where *Abatement Cost (\$)* is the cost to the firm in dollars of eliminating one metric ton of CO2, τ is our estimated tax parameter that reconciles observed yield spread effects from RPS with an equivalent tax to firm value, $\mathit{Asset\ Value}_{i,t}$ is the estimated asset value of firm i at year t, using the Enterprise Value multiple, and β^{CO2} is our estimated coefficient of the total absolute annual drop in CO2 induced by RPS.

Our results can be found in Panel C of Table 10. We find that the cost to firm asset value of reducing one ton of CO_2 through RPS is \$53.81, with a standard error of \$6.30 cents. Given that the function relating the tax to firm value changes is linear, we simply multiply the standard error for τ by the firm value to CO_2 coefficient ratio to arrive at the standard error for our cost.

To put these numbers into perspective, we perform some simple calculations that link CO_2 reduction to power generation. According to numbers collected by the US Energy Information Administration, the power sector generates an average of 0.86lbs of CO_2 for every kilowatt-hour of electricity produced. This implies that every ton of CO_2 generated corresponds to roughly 2.56 megawatt-hours of power. Given that the price of a kilowatt-hour of electricity is around \$0.11 for investor-owned utilities in our sample, this means that the ratio of revenue from electricity generation to CO_2 production is roughly \$281.60 per ton of CO_2. While our measure of cost relates to firm value rather than revenue, this calculation nonetheless gives some indication as to the scale of the cost associated with RPS policy.

6 Conclusion

Reduction of carbon emissions in the emissions-intensive power sector due to climate policy leads to wider credit spreads due to abatement costs. By combining a novel identification strategy for renewable portfolio standards (RPS) in the US that govern investor-owned utilities but exempt municipal producers with emissions and bond issue data, we estimate that the reduction in carbon emissions of 2.7 million tons per producer from RPS comes at a cost of 66 bps wider credit spreads. We show that this trade-off can be explained with a structural corporate-bond pricing model in which RPS narrows distance to default by reducing firm cashflows. We use the model to infer that the abatement costs that firms have to bear is \$50 per ton of emissions abated.

Firms seem to bear more the tax burden of RPS as there is only a small pass through of higher renewable costs to consumers.

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Appendix

A Classifying the RPS Programs of States

We use two data sources to determine whether a state's RPS program regulates the emissions of investor-owned utilities differently than other utilities. The first source is Barbose (2021). This study contains a spreadsheet that describes the RPS targets of each state over time. The spreadsheet contains notes that describe whether the RPS targets of the state are applicable to all utilities located in the state or only a subset (e.g. investor-owned utilities). We crosscheck these descriptions of state RPS programs with a second source: the Database of State Incentives for Renewables and Efficiency (DSIRE). This website maintained by the North Carolina Clean Energy Technology Center at North Carolina State University and contains descriptions of each state's RPS program. As part of these descriptions, the website indicates which types of utilities are covered by the RPS.

For states that only include investor-owned firms in their RPS mandate, it is an easy call to classify them as part of our sample of states that differentially treat utilities. But there are a handful of states that are more complicated. There are a few states that have more stringent mandates for investor-owned firms than other utilities. For example, in Colorado in 2022, investor-owned utilities were required to meet a 30% RPS target but municipal utilities only had to meet a 10%. We include such states that have higher mandates for investor-owned utilities in our sample. Also, a handful of states treat different types of investor-owned utilities differently. For example, in Virginia, the RPS only covers the two large investor-owned utilities in the state and not a handful of smaller investor-owned utilities. We classify the entire investor-owned sector in states such as Virginia as being covered by the mandate.