8-1-2019

Carbon Taxes and Stranded Assets: Evidence from Washington State

Stefano Carattini
Georgia State University, CESifo, London School of Economics and Political Science, University of St. Gallen

Suphi Sen
CESifo, ifo Institute at the University of Munich

International Center for Public Policy
Working Paper 19-10
August 2019

Carbon Taxes and Stranded Assets: Evidence from Washington State

Stefano Carattini
Suphi Sen

International Center for Public Policy
Working Paper 19-10

Carbon Taxes and Stranded Assets: Evidence from Washington State

Stefano Carattini
Suphi Sen

August 2019

*Note: This working paper has also been released by ifo Institute at the University of Munich

International Center for Public Policy
Andrew Young School of Policy Studies
Georgia State University
Atlanta, Georgia 30303
United States of America

Phone: (404) 413-0235
Fax: (404) 651-4449
Email: paulbenson@gsu.edu
Website: http://icepp.gsu.edu/

International Center for Public Policy
Andrew Young School of Policy Studies

The Andrew Young School of Policy Studies was established at Georgia State University with the objective of promoting excellence in the design, implementation, and evaluation of public policy. In addition to two academic departments (economics and public administration), the Andrew Young School houses seven leading research centers and policy programs, including the International Center for Public Policy.

The mission of the International Center for Public Policy (ICePP) at the Andrew Young School of Policy Studies is to provide academic and professional training, applied research, and technical assistance in support of sound public policy and sustainable economic growth in developing and transitional economies.

ICePP is recognized worldwide for its efforts in support of economic and public policy reforms through technical assistance and training around the world. This reputation has been built serving a diverse client base, including the World Bank, the U.S. Agency for International Development (USAID), the United Nations Development Programme (UNDP), finance ministries, government organizations, legislative bodies, and private sector institutions.

The success of ICePP reflects the breadth and depth of its in-house technical expertise. The Andrew Young School's faculty are leading experts in economics and public policy and have authored books, published in major academic and technical journals, and have extensive experience in designing and implementing technical assistance and training programs. Andrew Young School faculty have been active in policy reform in over 40 countries around the world. Our technical assistance strategy is not merely to provide technical prescriptions for policy reform, but to engage in a collaborative effort with host governments and donor agencies to identify and analyze the issues at hand, arrive at policy solutions, and implement reforms.

ICePP specializes in four broad policy areas:

- Fiscal policy (including tax reforms, public expenditure reviews, tax administration reform)
- Fiscal decentralization (including decentralization reforms, design of intergovernmental transfer systems, urban government finance)
- Budgeting and fiscal management (including local government budgeting, performance-based budgeting, capital budgeting, multi-year budgeting)
- Economic analysis and revenue forecasting (including micro-simulation, time series forecasting)

For more information about our technical assistance activities and training programs, please visit our website at http://icepp.gsu.edu or contact us by email at paulbenson@gsu.edu.

Carbon Taxes and Stranded Assets: Evidence from Washington State

Stefano Carattini[†‡§¶] and Suphi Sen[I‡]

August 2019

Abstract

The climate challenge requires ambitious climate policy. A sudden increase in carbon prices can lead to major shocks to the stock market. Some assets will lose part of their value, others all of it, and hence become "stranded". If the markets are not ready to absorb the shock, a financial crisis could follow. How well investors anticipate, and thus how large these shocks may be, is an empirical question. We analyze stock market reactions to the rejection of two carbon tax initiatives by voters in Washington state. We build proper counterfactuals for Washington state firms and find that these modest policy proposals with limited jurisdiction caused substantial readjustments on the stock market, especially for carbon-intensive stocks. Our results reinforce concerns about "stranded assets" and the risk of financial contagion. Our policy implications support the inclusion of transition risks in macroprudential policymaking and carbon disclosure and climate stress tests as the main policy responses.

Keywords: Carbon pricing; financial returns; systemic risk; macroprudential policies; voting
JEL codes: G12; H23; H71; L50; Q58

[†] Department of Economics, Andrew Young School of Policy Studies, Georgia State University
[‡] CESifo
[§] Grantham Research Institute on Climate Change and the Environment and ESRC Centre for Climate Change Economics and Policy, London School of Economics and Political Science
[¶] Department of Economics, University of St. Gallen
[I] ifo Institute at the University of Munich

We are grateful to Yoram Bauman, Baran Doda, Roger Fouquet, Marc Gronwald, Christian Traeger, and participants at various seminars and conferences for helpful comments. The usual disclaimer applies. We also thank Alexander Gordan and Byron Owusu-Ansah for excellent research assistance, and the LMU-ifo Economics and Business Data Center (EBDC) for providing access to the data. Carattini acknowledges support from the Grantham Foundation for the Protection of the Environment through the Grantham Research Institute on Climate Change and the Environment and from the ESRC Centre for Climate Change Economics and Policy as well as from the Swiss National Science Foundation, grant number PZ00P1_180006/1. Sen acknowledges support from the Bundesministerium für Bildung und Forschung research program "Economics of Climate Change," grant number 01 LA 1811.

Introduction

The climate challenge requires the immediate implementation of ambitious climate policies. However, global and national financial institutions have expressed concerns that an abrupt and coordinated increase in carbon prices could lead to major shocks to the stock market, with the potential for systemic risk. Some assets will lose part of their value, others all of it, and hence become “stranded”. If the markets are not ready to absorb the shock, a financial crisis could follow. The number of jurisdictions pricing carbon has increased substantially over the last few years, mainly following the Paris Agreement, to reach 57 jurisdictions and a coverage of about 20 percent of global greenhouse gas emissions (World Bank 2019). Very recently, the World Bank’s High-level Commission on Carbon Prices called for a global carbon price in the order of $40 to $80 per ton of CO2 (Stiglitz et al. 2017). While the increase in coverage and stringency of carbon pricing should be good news for climate change mitigation, in a recent and very influential speech the Bank of England’s governor Mark Carney expressed strong concerns for the potential role that stranded assets could play in destabilizing the global economy (Carney 2015).

A global financial crisis caused by climate policy would not only disrupt the lives of millions of people, but also represent a major setback for climate action. Macroprudential measures, such as climate stress tests, have become part of the policy options considered by institutes such as the European Systemic Risk Board (ESRB 2016), and are under consideration in Canada, France, Italy, the Netherlands, Sweden, and the United Kingdom (Caldecott et al. 2016; Campiglio et al. 2017, 2018). In the United Kingdom, the Streamlined Energy and Carbon Reporting regulations seek to mandate carbon disclosure for all quoted as well as large unquoted companies. In the United States, Senator Elizabeth Warren sponsored the Climate Risk Disclosure Act, a bill requiring public companies to disclose critical information about their exposure to climate-related risks.

How well investors anticipate, and thus how large these shocks may be, is an empirical question. In this paper, we analyze stock market reactions to the largely unexpected defeat of two carbon tax initiatives in Washington state, Initiatives 732 (I-732) and 1631 (I-1631). We leverage an important difference in the design of these carbon taxes, namely that one implied a revenue-neutral reform (I-732) while the other could have been classified as an environmental spending bill (I-1631), along the lines of the currently debated Green New Deal.

Our main findings are as follows: (i) both events led to significant readjustments in the value of Washington-based firms, (ii) these adjustments are stronger for carbon-intensive stocks, and (iii) all adjustments tend to be stronger for I-1631 than for the revenue-neutral I-732. These results have crucial implications for designing cost-effective policies in the face of climate change. In particular, the important swings in the stock market price of Washington-based firms observed in our context suggest that even a relatively modest policy with limited jurisdiction can represent an important shock to the stock market. Hence, the concern about systemic risk in the case of a coordinated implementation of ambitious carbon prices across countries seems justified. While our analyses do not offer a direct test of the usefulness of macroprudential measures such as mandated carbon disclosure and climate stress tests, they do support the rationale for such interventions.

Our empirical strategy takes advantage of the uniqueness of Washington state's framework and leverage the important differences that exist between these two initiatives. I-732, championed by stand-up economist Yoram Bauman and his grassroot organization Carbon Washington, was designed as a revenue-neutral, business-friendly carbon tax (Anderson et al. 2019). Following the example of the British Columbia carbon tax, implemented in 2008, I-732 had a pre-announced tax escalator and redistributed the money back to the economy. While British Columbia did so mainly through income tax rebates, the absence of an income tax in Washington state led the policy entrepreneurs behind I-732 to opt for a reduction in the sales tax. I-732 was announced in March 2015 and the ballot took place on November 8, 2016. While I-732 was supported by some business lobbies, environmentalists were strongly divided. Very powerful actors in the environmental justice community, such as the Sierra Club, opposed the initiative. Their opposition was not directed at carbon taxes *per se*, but rather at the specific, revenue-neutral design of I-732. Following the rejection of I-732 by Washington state's voters, environmental justice groups announced I-1631, a new carbon tax initiative, in March 2018. I-1631 also included a tax escalator. It was, however, no longer designed to be revenue neutral. Expected revenues of about US$1 billion would have been distributed among a clean air and clean energy fund (about $700 million), a clean water and healthy forest fund (about $250 million), as well as support towards low-income households, communities, and affected workers. Hence, the main difference between the two proposals lies on the different use of revenues. I-1631 was rejected on November 6, 2018.

We access Thomson Reuters database and collect financial data on all Russell 3000 companies, which we combine with firm-level characteristics from Compustat and other data sources from Thomson Reuters. We conduct a short-run event study analysis to estimate the abnormal returns on the event dates. Specifically, we estimate the causal effect of new information about carbon taxes on the returns of Washington- based firms. To this end, we estimate proper control portfolios for Washington-based firms, by matching on observable firm characteristics. Our treated sample consists of downstream firms, which use energy as an input to their production function. Our approach addresses potential confounders in both average and heterogeneous effects. We confirm our model's performance by conducting extensive placebo tests at non-event dates and with false treatment groups, among others.

Our unique approach contributes to the nascent literature on climate policy and systemic risk in several ways. First, we focus on new information released about the most prominent climate policy tool, carbon taxation. Second, we focus on the effect of new information on downstream firms. This approach is crucial to understand the true potential for systemic risk. Indeed, an important climate policy shock would not only affect fossil fuel companies, but the economy as a whole. Including downstream firms can lead to a tenfold increase in the exposure of financial intermediaries to a potential climate policy shock (Battiston et al. 2017). Third, we leverage differences in the design of the same policy tool, a carbon tax, to identify financial implications stemming from the considerably different economic impacts that actual policymaking can bring about. Fourth, we make use of our policy's clear tax base, carbon emissions, to analyze heterogeneous effects along this dimension. Heterogeneity, and in particular how affected are the most affected firms, is crucial for assessing systemic risk. Further, heterogeneous effects allow us to assess whether investors take advantage of available information about carbon intensity and exposure to potential carbon taxes.[1]

Our paper complements a subset of other studies, which analyze either stock market reactions to policy announcements with a focus on upstream energy suppliers, or release of scientific information with potential policy relevance. For instance, Griffin et al. (2015) study the impact on the stock market of two concurrent publications in *Nature*, calculating the implications for coal, gas, and oil reserves of keeping global temperatures within 2°C above pre-industrial levels (Allen et al. 2009 and Meinshausen et al. 2009). Byrd and Cooperman (2018) analyze the effect of new information about technological improvements in the capture and storage of carbon on the value of coal companies. Linn (2010) and Sen and Schickfus (2017) focus on energy utilities, and how their market value is affected by plans to implement an emissions trading scheme in the United States or a carbon tax in Germany, respectively. Hence, the existing evidence is either for energy suppliers or based on indirect inference from non-policy events.

The remainder of the paper is organized as follows. We discuss the literature and economic background in Section 2. Section 3 describes the data and empirical approach. Section 4 provides our empirical results. Section 5 concludes.

Background

Climate policy and stranded assets

About 20 percent of global greenhouse gas emissions are currently covered by a carbon price or soon will be. Among the countries currently pricing carbon, most of them

[1] In this way, we also contribute to this growing strand of literature. See, for instance, Beatty and Shimshack (2010), Kim and Lyon (2011), Oberndorfer et al. (2013), Flammer (2015), Krüger (2015), and Ramelli et al. (2018). Amel-Zadeh (2018) provides an extensive survey.

have tax rates, or permit prices, around or below $10 per ton of CO_2 (World Bank 2019). Tax rates, however, tend to increase over time, in some cases by construction, as many carbon tax schemes have tax escalators embedded in the policy design. Emissions trading schemes operate over phases, allowing the regulator to adjust the cap downward. Recent features such as the Market Stability Reserve in the European Union Emissions Trading System give regulators more power to ensure dynamic incentives (Hepburn et al. 2016). Hence, carbon prices are expected to keep rising over time and to cover an increasing portion of global emissions. Against this background, calls for a global carbon price in the order of $40 to $80 dollars per ton of CO_2 appear increasingly plausible (Stiglitz et al. 2017).

Concerns about stranded assets started to receive considerable attention following an influential speech by the Bank of England's governor Mark Carney (Carney 2015). In this 2015 speech, Carney highlighted three sources of risk for financial systems related with climate change. First, physical risks, related to the impact on financial assets of climate change, in particular through the intensification of natural disasters and extreme events (see Dietz et al. 2016). Second, liability risks, which would emerge if victims of climate change could seek compensation. Third, transition risks, which are the focus of our paper. According to Carney, changes in climate policy, and increases in stringency, could lead investors to reevaluate the value of a broad range of assets, potentially destabilizing the financial system. The term "stranded assets" has been used also more broadly, to define all capital investment that may lose value during the transition to a cleaner economy (see Asheim 2013).

Carney's speech, among other factors, led to both an emerging literature on stranded assets and the design of additional macroprudential policies by central bankers and financial stability boards. Since 2016, for instance, the European Systemic Risk Board (ESRB), an agency of the European Central Bank, considers late and abrupt implementation of climate policy (defined as "hard landing") as part of the systemic risks to the global financial system (ESRB 2016). The main concern is represented by the massive reserves of fossil fuels that would need to remain in the ground to avoid dangerous interferences with the climate system, but which are currently in the fossil fuel companies' books.[2] If the market value of these companies is readjusted belatedly and suddenly, potentially dangerous feedback loops could emerge. That is, the initial shock that climate policy would create by forcing the obsolescence of large fossil fuel assets could trigger systemically-relevant second-round effects. Following its analysis, ESRB's policy recommendations included the mandatory disclosure of carbon intensity by some firms as well as the inclusion of climate-related prudential risks in stress tests (leading to "climate stress tests") and other macroprudential strategies. Several other central banks and institutes in charge of financial stability are currently considering similar macroprudential policies, including in Canada, France, Italy, the Netherlands, Sweden, and the United Kingdom (Caldecott et al. 2016; Campiglio et al. 2017, 2018).

Although not yet mandatory, carbon disclosure has become increasingly common in recent years among publicly-owned firms (Doda et al. 2016). To address investors' concerns, as many as 600 companies have gone one step further and implemented an internal carbon pricing scheme. Internal carbon prices tend to be relatively low but could

[2] McGlade and Ekins (2015) estimate that about 80 percent of the current coal reserves should remain unused in order to keep global temperatures within 2°C above pre-industrial levels. The same applies to 49 percent (33 percent) of global gas (oil) reserves. McGlade and Ekins (2015) update the earlier analyses by Allen et al. (2009) and Meinshausen et al. (2009).

be easily ramped up, once the scheme is in place. Shadow carbon prices tend to be much larger, sometimes in the order of \$100 per ton of CO2 (CDP 2017; Gillingham et al. 2017). In terms of climate stress tests, Battiston et al. (2017) provide simulations for the European financial system. The authors use a network approach to account for interlinkages among financial institutions and examine the potential magnitude of second-round effects. They confirm the initial concerns that an abrupt implementation of climate policy could lead to a systemic risk, due to the important presence of carbon-intensive sectors in investors' portfolios. "High-carbon exposure" stocks (i.e. stocks of fossil fuel companies) represent about 5 percent of pension funds' assets, 4 percent of insurances', and 1 percent of banks' (Weyzig et al. 2014). Adding other sectors relevant for climate policy, as done in Battiston et al. (2017), can lead to much higher exposure, in the 36-48 percent range. Furthermore, financial actors own the equity of other financial actors in the order of 10-20 percent, implying substantial indirect exposure as well.

From a theoretical perspective, stranded assets could change the ranking of climate policy instruments. According to standard economic theory, carbon pricing dominates the ranking as the "first-best" instrument (Goulder and Parry 2008; Aldy and Stavins 2012). However, political economy issues have limited its adoption (Oates and Portney 2003; Carattini et al. 2018). A recent paper by Rozenberg et al. (2018) takes an Olsonian perspective and compares, with a simple theoretical model, different climate policy instruments in terms of stranded assets, which are considered visible losses of wealth concentrated in a few vested interests. Consistently with eco- nomic intuition, carbon pricing minimizes the (discounted) cost of climate policy. However, in the model, carbon pricing leads to stranded assets. This is not the case for "second-best" mandates, feebates, and standards, which in the model only affect new capital (e.g. new coal power plants, new buildings). Hence, the authors identify a trade-off between cost-effectiveness and the generation of stranded assets.[3] At the same time, however, the fact that carbon pricing leads to stranded assets can make climate policy more progressive (Rausch et al. 2010).

We complement the existing literature with a novel angle, looking at the most prominent climate policy, the carbon tax, under different declinations, and how it impacts firms with varying degrees of carbon exposure. By focusing on downstream firms, and assessing heterogeneity along the carbon intensity dimension, we aim at capturing the full extent of systemic risk. As shown in the literature, the exposure of financial firms increases dramatically when sectors other than fossil fuel companies are taken into account. Whether and to what extent substantial market fluctuations affect downstream firms is crucial to understand the potential for systemic risk.

Washington Initiatives 732 and 1631

Carbon taxes have become increasingly common in recent years, especially following the implementation of the Paris Agreement. Opposition from energy-intensive groups and from citizens remain, however, major obstacles. The first examples of carbon taxes date back to the early ’90s, when they were implemented in several Nordic countries. These schemes are known for their generous exemptions to energy-intensive industries. Switzerland implemented a carbon tax in 2008, but one covering only heating fuels (Conway et al. 2017; Narassimhan et al. 2017). More ambitious designs were rejected on the ballot first in 2000 and then again in 2015. Also in 2008, British Columbia

[3] Following Goulder and Schein (2013), a carbon tax with carefully designed exemptions may limit the extent of stranded assets, similarly to emissions trading schemes with some grandfathering (Goulder et al. 2010). Trade-offs, however, remain.

implemented a revenue-neutral carbon tax (Murray and Rivers 2015). More than 20 distinct carbon tax schemes currently exist around the world, to which one can add similar schemes such as the carbon floor and the climate change levy in the United Kingdom (World Bank 2019). None of these schemes was implemented following a public vote.

I-732 was designed as follows.[4] The tax would have started, in 2016, with a tax rate of $15 per ton of CO2, which would have increased in 2018 to $25 per ton of CO2. The tax rate would have kept increasing gradually by 3.5 percent per year (plus inflation), until reaching the target of $100 per ton of CO2 (in constant 2016 dollars). Fossil fuels from all sources would have been taxed upstream, including imports from other states used for electricity generation for the Washington market. I-732 was designed as a revenue-neutral reform, with the objective to appeal to an electorate of moderate Republican voters. Revenue neutrality would have been achieved, in theory, as follows. The state's sales tax would have been reduced from 6.5 percent to 5.5 percent. By reducing a regressive tax such as the sales tax, Carbon Washington planned to address, if partially, concerns related with the distributional effects of carbon taxes. To address the same concerns, some of the revenues would have been used to match the Federal Earning Income Tax Credit at 25 percent. Finally, local businesses would have benefitted from the elimination of the state's business and occupation tax for manufacturers (as high as 0.48 percent). In Washington state, the minimum number of valid signatures for an initiative to be successful is slightly above 250,000. The state legislature declined the opportunity to pass I-732 directly, or to suggest an alternative to voters, so that the initiative ended up on

[4] Please refer to Anderson et al. (2019), on which we largely rely as well, for a thorough analysis of I-732 (and I-1631).

the 2016 ballot. I-732 was on the ballot on November 8, 2016 and rejected at 59 percent.

Following the rejection of I-732, I-1631 was announced in March 2018. Its design was as follows. The initial tax rate was set at $15 per ton of CO_2, same as I-732, with implementation in 2020. The tax escalator implied an increase by $2 per ton of CO_2 per year, until reaching statewide emissions goals. The carbon tax was not designed to be revenue neutral. Rather, revenues would have been earmarked to promote several goals, through three funds. First, a fund for clean air and clean energy. Second, a fund promoting water quality and forest health. Third, a fund for community-related investments. The policy was labeled “fee”, in line with Washington state’s laws. I-1631 was on the ballot on November 6, 2018. The initiative was rejected at 57 percent.

I-723 and I-1631 have similar designs, in that they start with a relatively low tax rate, includes a tax escalator, and cover most CO_2 in the economy, upstream. As stressed, the most notable difference relates to the use of revenues. I-732 was designed as revenue neutral, I-1631 as a Green New Deal type of policy. The difference in design also mattered for part of the electorate, although how voters might have split was relatively hard to predict beforehand (see section 3.4). Supporters of I- 732 included proponents of carbon taxation such as the Citizens’ Climate Lobby, Audubon Washington, and minor environmental groups. Local Democratic party chapters and the renewable industry also supported it. Local chambers of commerce and carbon-intensive industries opposed it, but so did also progressive organizations such as labor and social justice groups, the most influential environmental groups (an alliance including the Sierra Club), and the State Democratic party. The split among environmentalists ultimately contributed to its rejection. Supporters of I-1631 included many environmental groups and was championed by those that opposed I-732. Opponents included business associations and carbon-intensive industries. For I-732, the yes-camp spent about $3 million, against $1.4 million spent by the no-camp. For I-1631, the yes-camp spent about $15 million, with $1 million each contributed by billionaires Bloomberg and Gates. The no-camp spent $32 million. Overall, the campaign spending for I-1631 was a record in the state's history, according to local media.

Data and empirical strategy

In this section, we describe our data and empirical strategy. We start by detailing the measure capturing the effects of an event on the stock market, namely cumulative abnormal returns (CARs). Second, we explain our estimation strategy and how we identify such event effects by controlling for confounding countrywide effects. Third, we describe our control set along with the underlying data sources and provide descriptive statistics. Finally, we discuss the events to be analyzed in relation to our identification strategy.

Cumulative abnormal returns

We use a standard short-run event study methodology to estimate the abnormal returns associated with a given event. We estimate the normal market performance by using three standard approaches, which are compared for sensitivity purposes (see Campbell et al. 1997). These are the market model, the Capital Asset Pricing Model (CAPM), and the Fama-French three-factors model (FFM).

The market model is given by $rit = \beta irmt + Eit$, where rit is the return of asset i at the trading date t, rmt represents returns to a market price index m, and Eit is an error term. The normal return is the predicted return given by $\hat{r}_{it} = \hat{\beta}_i r_{mt}$. We denote the event date with T, and specify the event window as a time period around the event date from $T_0 < T$ to $T_1 > T$. To control for potential feedback from the event to the normal market performance, we use an estimation window prior to the event window ending at $T_0 - 1$.

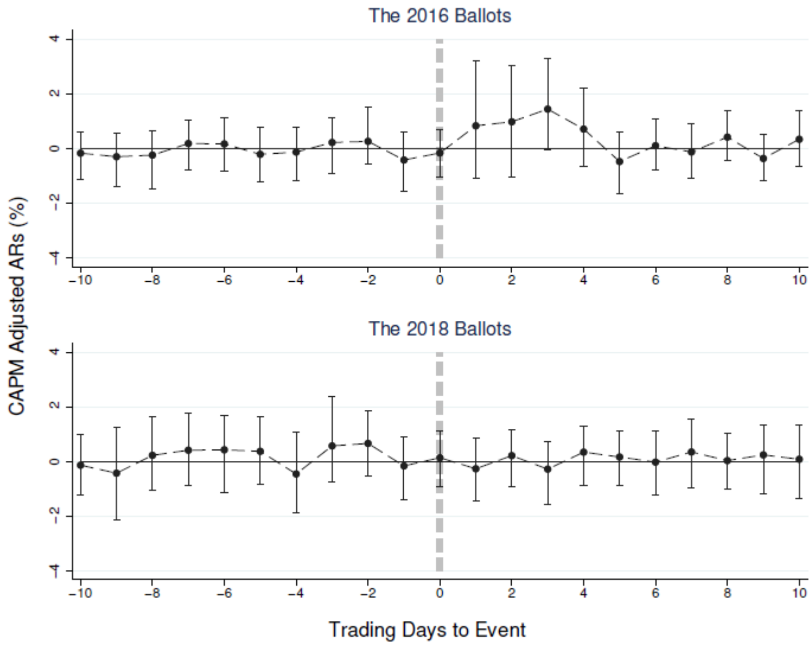
We define the relative time index $\tau = t - T$ to measure the distance to the event date. Then, the abnormal returns are estimated by the difference between realized returns and normal returns, given by the prediction errors $AR_{T+\tau} = r_{T+\tau} - \hat{r}_{T+\tau}$. The effect of the event is generally parametrized by cumulative abnormal returns (CARs), which are given by the sum of abnormal returns (ARs) over a number of consecutive days in the event window.

In the CAPM estimations, the returns are calculated in excess of a risk-free rate of return. Formally, the CAPM is given by $\tilde{r}_{it} = \beta_i \tilde{r}_{mt} + E_{it}$, where $\tilde{r}_{it} = r_{it} - r_{ft}$, $\tilde{r}_{mt} = r_{mt} - r_{ft}$, and r_{ft} is a risk-free rate of return. The Fama-French model augments the CAPM with size (s_t) and value risk factors (v_t) as additional covariates, such that $\tilde{r}_{it} = \beta_i \tilde{r}_{mt} + \lambda_{si} s_t + \lambda_{vi} v_t + E_{it}$.

Our sample consists of all firms in the 2018 Russell 3000 constituent list. We obtain their daily stock prices and the Russell 3000 price index from Thomson Reuters Datastream and calculate continuously compounded returns. As per standard procedure, we use one-month Treasury-bill rates as a proxy for the risk-free rate of return.[5]

[5] The data on the risk-free asset and the Fama and French (1993) factors are retrieved from http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html (last accessed, May 12, 2019).

Figure 1: Abnormal returns around the ballot dates



Note: This figure illustrates the median, and upper and lower 25th percentiles of CAPM-adjusted ARs. ARs are truncated at upper and lower fifth percentiles. The horizontal axis indicates the relative distance to the events in terms of trading dates, such that date zero stands for the event date.

In our baseline setting, we estimate the normal market performance by using a window length of 60 trading days ending at 10 days before the event date. Hence, our estimation window covers a sample between dates $\tau = -71$ and $\tau = -11$.[6]

Figure 1 illustrates the median, and upper and lower 25th percentiles of ARs from the CAPM estimations for all companies in the Russell 3000 constituency list, regardless of their location, around the dates when I-732 and I-1631 were on the ballot in Washington state. We exclude upper and lower fifth percentiles. In the figures, the horizontal axis indicates the relative distance to the event in terms of trading dates, such that date zero stands for the event date. The distribution of ARs in the pre-event windows

[6] Our results are robust to alternative estimation windows, including 90 or 150 trading days or leaving a 20-day gap between the estimation window and the event date. All additional estimations are available by the authors upon request.

are similar for both events and quite stable. The increase in the spread of ARs following the 2016 ballots is quite intuitive given the surprise election of Donald Trump on this date (see Wagner et al. 2018). On the other hand, the 2018 midterm elections do not seem to have had a major effect, on average. This result is in line with the common view that midterm election results tend to be less surprising. Incumbent presidents generally tend to lose seats in the first midterm, which also occurred in the 2018 election. In the next subsection, we explain how we control for nationwide confounding events to establish causality.

Estimation strategy

We are interested in identifying the effect of a series of events that are expected to affect firms active in Washington state. A potential threat to identification is the presence of nationwide contemporaneous shocks, which could act as confounders. In this section, we describe our estimation strategy, which explicitly accounts for such potential confounders by using a counterfactual comparison group represented by non-Washington-based firms.

Our empirical approach is as follows. Let the dummy variable $Di = \{0, 1\}$ stands for the treatment status. In our application, it takes value one for Washington-based firms and zero otherwise, which represents a conservative approach. Using the potential outcome framework, let $CAR1i$ denote the CAR of firm i on a single event date if it were a Washington-based firm, and by $CAR0i$ if it were a non-Washington- based firm. We are interested in estimating the causal effect of the treatment on CARs given by

$$\rho = E[CAR1i|Di = 1, Xi] - E[CAR0i|Di = 1, Xi],$$

which is the average treatment effect on the treated (ATET). Here, X_i is a vector of covariates, which we describe in the following subsection. The fundamental identification problem is that the second term is unobserved, as the CAR of a Washington-based firm ($D_i = 1$) in the case it was based elsewhere is never observed (CAR_{0i}). Consider the following specification to estimate the effect of a single event on the CARs:

$$CAR_i = \alpha + \rho D_i + X_i^t \gamma + \eta_i,$$

where α is a constant, η_i is the error term, and γ is a vector of parameters. If the error term and D_i are uncorrelated conditional on the covariate set, then ρ captures the ATET, that is, the causal effect of being subject to the treatment on the outcome variable *CAR*. The uncorrelatedness assumption can be stated as $E[CAR_{0i}|D_i = 1, X_i] = E[CAR_{0i}|D_i = 0, X_i]$, which means that, conditional on X_i, the potential CAR when firm i were not in Washington (CAR_{0i}) is independent of whether it is actually based in Washington state or not. Then, the observed difference between a control and a treatment unit, conditional on their observed characteristics X, reflects the event effect.

Our outcome variable is the estimated CARs. In the absence of any event on this date, the CARs should be equal to zero for both the control and treatment groups, as given by market efficiency. Further, in the absence of nationwide shocks, we could simply analyze the CARs of Washington-based firms. In presence of potential nationwide shocks, we can account for their effect by comparing treatment and control groups. Hence, the identification strategy relies on the ability of the covariate set to capture the effects of potential nationwide shocks.

An empirical strategy consistent with this conceptual approach is a matching estimation. A matching estimand constructs a counterfactual unit that best mimics the observed characteristics of a treated unit, such that a control unit is assigned a larger weight if it is closer to the treatment unit in terms of its observed characteristics. The advantage of a matching strategy is that it maximizes balance across compared units in terms of their observed characteristics, such as size and leverage. We estimate the weights by using the propensity score matching (PSM) algorithm. The propensity score represents the probability of receiving the treatment as predicted by observable characteristics (*X*). We estimate these scores by using a logit regression of treatment status on various covariate sets. We present our test results on the balance of covariates and overlap assumption in the Appendix together with the results from additional, standard estimators for treatment effects.

Covariate set

Our control set includes a rich set of characteristics describing companies in the Russell 3000 index, based on the 2018 list of constituents. All firm characteristics are obtained from Thomson Reuters. For all variables, we use the latest available accounting data prior to an event date. Table 1 presents a set of descriptive statistics with 2017 values for the variables in our baseline control set. We employ standard firm characteristics such as profitability, leverage ratio, market capitalization, and sales growth. We augment this set with additional variables that can further account for potential confounding events. I-732 was on the ballot on November 8, 2016, which was marked with Donald Trump's surprise election. It is reasonable to expect this surprise election to have nationwide effects on the stock market. For instance, Wagner et al. (2018) find that the victory of the Republican candidate in the 2016 election increased the stock value of firms with a stronger global orientation and a higher tax burden. In a conservative approach, we augment the standard control set by adding variables such as corporate income tax rate and foreign sales ratio.[7]

Table 1: Control set: descriptive statistics for 2017

	N	Mean	Median	St. Dev.	Min.	Max.
Leverage ratio (%)	2741	26.51	24.05	20.86	0.02	106.69
Profitability (%)	2883	0.17	2.37	16.70	-94.49	34.73
Annual net sales growth (%)	2819	11.34	7.68	25.55	-120.79	195.08
Market cap ($ bil.)	2894	10.57	1.79	41.97	0.01	867.51
Tax rate (5-year average) (%)	2575	23.46	26.51	12.84	0.00	65.93
Foreign sales ratio (%)	2949	18.67	0.00	26.68	0.00	100.00
Emissions (tons of CO2-e)	2341	1238.30	37.66	8843.15	0.08	294950.38
Carbon intensity:						
Emissions to net sales (tons of CO2-e/$)	2328	233.39	25.63	2064.55	0.01	93754.09
Emissions to physical assets (tons CO2-e/$)	2280	873.55	133.46	16859.83	0.17	782361.75

Note: This table presents descriptive statistics for our main control variables. Leverage is the ratio of total debt to total assets. Profitability is the ratio of pre-tax corporate income to total assets. Tax rate is the 5-year averaged ratio of corporate income tax to pre-tax income. Foreign sales ratio is the percentage of foreign sales over total sales. In the calculation of carbon intensity, physical assets consist of the value of plants, properties, and equipment.

Given that the tax burden of a carbon tax is proportional to a company's emissions, measured as CO2 equivalent (or CO2-e), we expect both I-732 and I-1631 to have heterogeneous effects depending on how carbon intensive a firm is. In particular, we consider the following two ratios as measures of carbon intensity: emissions to net sales and emissions to physical assets. Firm-level CO2 emissions are retrieved from the Thomson Reuters ESG Carbon database, which relies on self-reported emissions and imputes missing data based on past emissions, firm size, energy consumption, and industry characteristics. Another dataset measuring CO2 emissions, which is commonly employed in the literature, comes from the Carbon Disclosure Project (CDP) database. In our sample of firms, the correlation between our measure of emissions and scope 1 emissions reported in the CDP database is virtually perfect for 2015.

[7] Our results are robust to using cash-effective tax rates rather than corporate income tax rates.

Measurement of events and further considerations for identification

The main focus of our empirical analyses is the outcome of the vote on I-732, on November 8, 2016, and I-1631, on November 6, 2018. While both initiatives were eventually rejected, prior to the ballot day they were both heading in the opinion polls. Hence, the markets can be expected to have readjusted following the release of the actual outcome. Table 2 presents a summary of the opinion polls, which were realized by different organizations in the approach to the ballots. For I-732, the most recent opinion poll indicated 42 percent yes-votes against 37 percent no-votes. According to the online political encyclopedia Ballotpedia.org, in the final two weeks prior to the ballots, more than $1 million was spent by the opposition camp, most likely trying to counteract the consistent support observed in the opinion polls. For I-1631, the level of support in the polls was even high enough to exceed the 50 percent cutoff. In this case, recall that the opposition spent about $32 million.

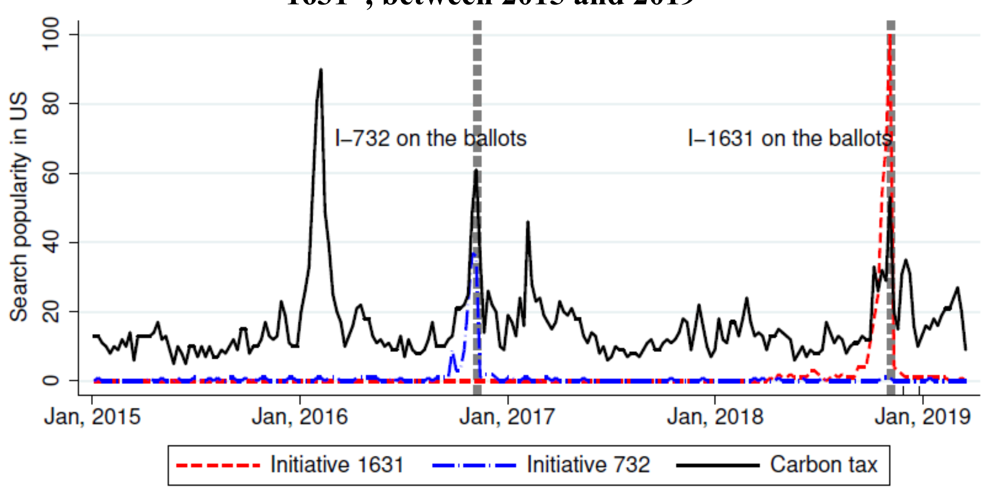
Table 2: Poll Results

Poll	Initiative	Date	Yes (%)	No (%)	Undecided (%)	Sample
Elway	I-732	Aug. 2016	34	37	30	500
KOMO News/Strategies 360	I-732	Sep.-Oct. 2016	42	37	21	500
Elway	I-732	Oct. 2016	40	32	28	502
YouGov	I-732	Oct. 2016	51	44	5	750
Crosscut/Elway	I-1631	Oct. 2018	50	36	14	400

Note: The source for all polls is Ballotpedia.org. Margin of error across the polls varies between +/- 4.4 and +/- 5.0.

Further, we consider natural that investors were made aware of the initiatives' outcomes on the ballot day or a few days after. While two emissions trading schemes are operational in California and in the Northeast, no carbon tax exists to date in the United States. If any of these initiatives had passed, it would have been the first carbon tax to be ever implemented statewide. Hence, there was considerable public interest around these initiatives. While considerable public interest is not a necessary element for investors to be informed about political events, it makes it even more plausible that the ballot outcomes rapidly became public knowledge. Illustrating this point, Figure 2 presents the search popularity of three related terms on Google: "carbon tax", "Initiative 732," and "Initiative 1631." As standard with Google search data, as provided by Google Trends, the maximum score during a given period is normalized at 100. Both the search term "Initiative 732" and the term "Initiative 1631" attain the maximum popularity score around the respective ballots. The search terms "Initiative 732" and "Initiative 1631" can partially reflect local interest. On the other hand, the popularity of the term "carbon tax" is more likely to be driven by countrywide interest. The term "carbon tax" attains its maximum popularity in February 2016 when the Supreme Court blocked the enforcement of the Clean Power Plan (CPP). This decision had a global importance, as the CCP was the Obama administration's major policy to curb CO2 emissions. Relative to this score, the search popularity of the three terms around the relevant ballot dates is substantial.

Figure 2: Google Trends outcomes for "carbon tax", "Initiative 732", and "Initiative 1631", between 2015 and 2019



Note: This figure presents the weekly search popularity on Google of the following three terms: "carbon tax", "Initiative 732," "Initiative 1631." All series come from Google Trends. All the illustrated scores are relative to the maximum, set by Google at 100.

Both in 2016 and 2018, there were other state initiatives on the ballot together with I-732 and I-1631. We discuss these initiatives in detail in Appendix A. In short, we are not concerned about these other initiatives acting as confounders for the following three reasons. First, some of them are largely irrelevant for the stock market, as only a small fraction of the economy is affected by either approval or rejection. Second, by the ballot date, the outcome of most of them was clearly predictable, with polls suggesting a clear outcome and the ballot results matching the polls' forecast very well. Third, none of the initiatives on the ballot relate directly to the carbon intensity of firms in the way that initiatives I-732 and I-1631 do. If the ATETs that we find in the main analyses were driven only by these confounders, then we should not find any heterogeneity along the carbon-intensity dimension.

In the next section, we show empirically that the rejection of the carbon tax initiatives led to positive and significant reactions in the stock market. This result implies that the stock markets had already (partially) priced in the effects of a potential approval. In order to validate this intuition, we further analyze the stock market reactions to the dates on which the initiatives were submitted and approved officially.

Results

In this section, we present our main results. We start by presenting average effects of the initiatives' ballot rejections on Washington-based firms. Next, we present our analysis on heterogeneity in the ATETs. Finally, we present a set of complementary findings leveraging the announcement dates for both initiatives.

Average effects of the ballot results on the valuation of Washington-based firms

This section presents our main results concerning the average effects of ballot results for I-732 and I-1631 on the stock performance of Washington-based firms. Table 3 presents the results from our OLS and PSM estimations. The dependent variables are either the CAPM or Fama-French Model (FFM) adjusted 10-day CARs calculated from a window starting with the next date following the event. Using CARs based on the market model and using CAPM-adjusted CARs yields similar results. We present the corresponding results from using CARs based on the market model in Appendix B.

Table 3 shows that the estimated effect of I-1631's rejection is positive and significant. This result is robust to employing different control sets, using CAPM- or FFM-adjusted CARs, and using OLS or PSM. For I-732, using OLS or PSM and using different control sets do not lead to drastic differences in the estimated effects. However, the estimations based on FFM-adjusted CARs yield systematically higher estimates compared to those based on CAPM-adjusted CARs. Hence, it seems that controlling for Fama-French risk factors is important to account for the role of confounders and the important noise surrounding the rejection of I-732. Given the predictive power of Fama-French risk factors and the insensitivity of our estimates to OLS or PSM, we present the results from OLS estimations with FFM-adjusted CARs in the rest of the main text. We provide the corresponding results from using CAPM- adjusted CARs, as well as PSM for all models, in Appendices B and C, respectively. In the rest of the paper, we use the full specification, which includes industry-specific fixed effects and our covariate set, as our preferred specification.

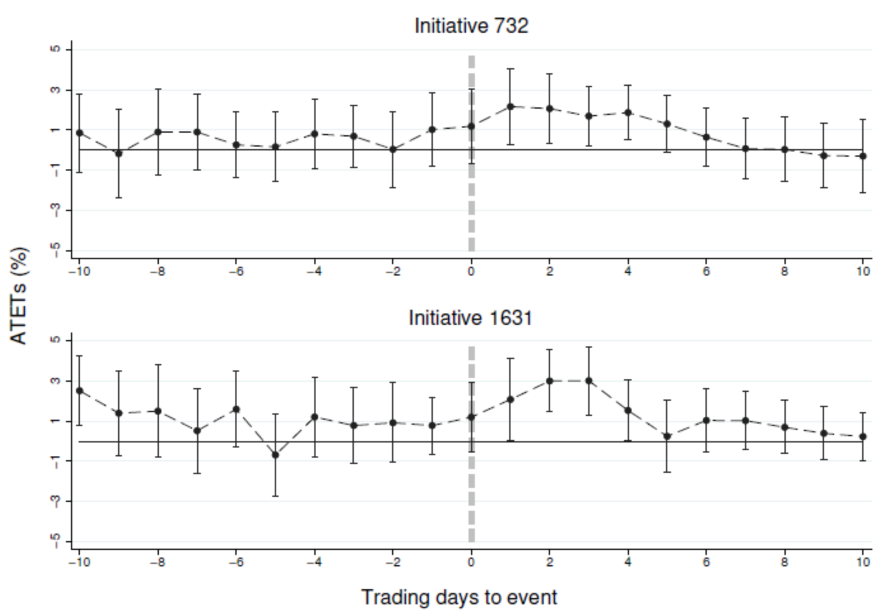
Table 3: Average event effects on Washington-based firms

	Initiative 732			Initiative 1631		
	(1)	(2)	(3)	(4)	(5)	(6)
CAPM - OLS	0.076	0.731	0.175	2.013**	2.326**	2.072*
	(0.974)	(0.995)	(0.910)	(0.812)	(1.112)	(1.061)
Treatment	41	32	32	42	29	29
Observations	2511	1959	1959	2658	2134	2134
CAPM - PSM	0.067	1.455	-0.278	2.011**	1.807	1.984**
	(0.876)	(1.168)	(1.045)	(0.803)	(1.156)	(0.794)
Treatment	41	32	32	42	29	29
Observations	2384	1959	1667	2526	2133	2027
FF - OLS	1.950**	2.800***	2.133*	2.171***	2.285**	2.082**
	(0.980)	(0.975)	(0.965)	(0.789)	(1.102)	(1.053)
Treatment	44	31	31	42	29	29
Observations	2517	1962	1962	2659	2136	2136
FF - PSM	1.945**	3.294***	2.395*	2.168***	1.598	1.988**
	(0.963)	(0.850)	(1.006)	(0.781)	(1.201)	(0.844)
Treatment	44	31	31	42	29	29
Observations	2406	1962	1534	2526	2135	2027
Industry fixed effects	Yes	No	Yes	Yes	No	Yes
Covariate set	No	Yes	Yes	No	Yes	Yes

Note: This table presents the OLS and PSM estimates of ATET where the dependent variables are CAPM-adjusted or Fama-French (FF) adjusted 10-days CARs from date 1 to 9. Robust standard errors are in parenthesis. The covariate set includes profitability ratio, leverage ratio, annual sales growth, market cap, 5-year corporate income tax rate, foreign sales ratio. Industry-specific fixed effects are based on ICB 1-digit industry classification (the Industry Classification Benchmark by FTSE International). Significance levels are indicated as $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

In Figure 3, we display graphically, for both ballot dates, the estimated ATETs from OLS regressions based on FFM-adjusted CARs, along with 95 percent confidence intervals. Here, we conduct in-time placebo tests around the event dates, which provide further insights to the stock market movements around the ballot dates. In these graphs, the horizontal axis measures the relative distance in time to the event date, in terms of trading days. For each date τ, we repeat our estimations by using 10-day CARs, calculated as the sum of ARs from date τ to τ +9. Hence, the ATETs on date 1 correspond to those presented in Table 3. As the events of interest are the announcement of the ballot results, we assume no informational leakage in the pre-event window. So, in the absence of other events of relevance, we expect the ATETs prior to the event dates to be generally insignificant, which serves as placebo test for our model's performance. Note, however, that the placebo ATETs can gradually move towards the event effect in the pre-event window, as the 10-days CARs that are closer to the event date pick more ARs from the post-event window. In Figure 3, the ATETs in the placebo windows are stable and virtually always insignificant. Hence, Figure 3 confirms the ability of our model in predicting the normal market performance and establishing a balanced control group. In line with Table 3, the ATETs are significant in date 1. ATETs, actually, remain significant up to date 4, indicating that while the market reactions started immediately, the adjustment process did take a few days to complete. Eventually, the market consumes all arbitrage opportunities, and the ATETs become insignificant again after date 4.

Figure 3: ATETs over the event window with FFM-adjusted CARs



Note: This figure presents the average effects of ballot results for I-732 and I-1631 on Washington- based firms, together with in-time placebo tests. The event day (date 0) is denoted by the vertical dashed line. The days prior to the event window are used as placebo-event days. The estimation window is given by the 60-day period prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are denoted by the capped vertical lines.

The estimation results in Figure 3 show that the effect of I-1631 tends to be much larger than that of I-732. Observing some difference between I-732 and I-1631 is in line with the initiatives' different designs. Since I-732 was designed as revenue neutral, and its revenues would have been used, mainly, to reduce sales taxes, such a design could have partially alleviated the burden of a carbon tax. On the other hand, I-1631 was not designed as a revenue-neutral reform. Most revenues would have been earmarked for environmental purposes, benefitting a relatively small group of firms active in the green economy but leading to higher net costs for virtually all other firms. These results seem to indicate that investors may pay attention to the design of policies in evaluating their effects on firms' profitability. We provide further evidence on this aspect in the following section.[8]

Both Table 3 and Figure 3 show substantial reactions to the rejection of the carbon tax initiatives. On average over the entire sample, the estimated effect is as high as 3 percent. It follows that the stock market had already priced in, at least to some extent, the possibility of a positive outcome in both 2016 and 2018. Consequently, the realization of an uncertain outcome required a market adjustment, which in this case affected the performance of Washington-based firms positively. Given our conservative approach and the limited jurisdiction and ambition of the policy proposals that we investigate, our first set of results already point to a potential for large readjustments in the stock market value of a broad range of firms in the case of a more abrupt increase in carbon prices. To fully gauge the extent that the market reacted to our events, and assess the potential for systemic risk, we analyze in the following section heterogeneous treatment effects along

[8] In both this section and the following, we use the Hausman (1978) specification test with robust variance-covariance matrices to test the difference in estimates between I-1631 and I-732. While we determine that these differences are not sufficiently large to reach statistical significance with the full sample, they are statistically significant and economically very meaningful for high carbon-intensity firms (see Figure 6). This is consistent with the main finding in the next section, which is that average treatment effects for both initiatives are mainly driven by high carbon-intensity firms.

the carbon intensity dimension.

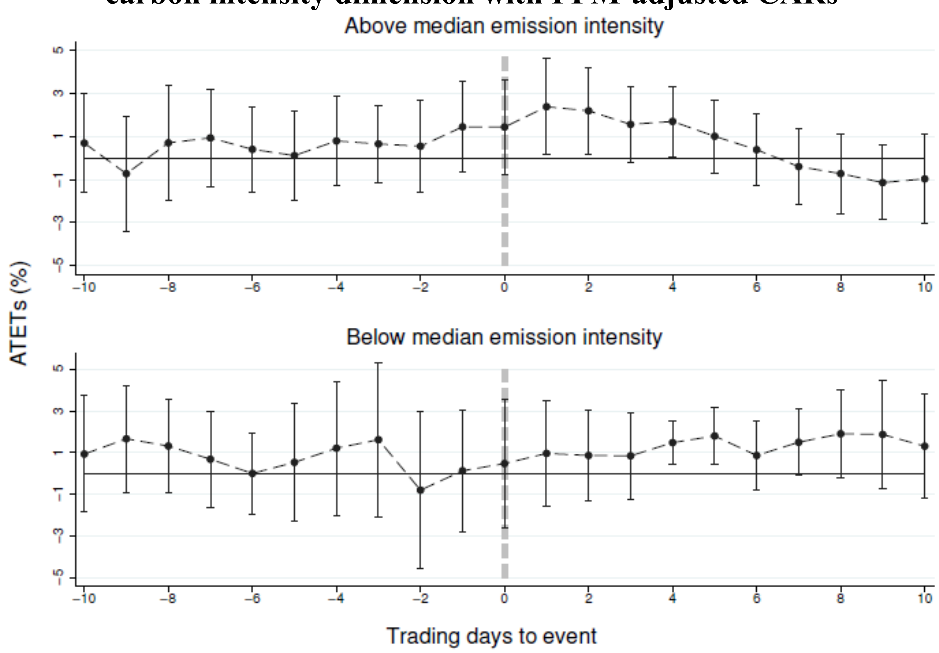
Note that our empirical approach is robust to a set of sensitivity tests, which are presented in the Appendix. In particular, in Appendix B we conduct our analysis by using CARs from the market model and the CAPM. Next, we present the results from various alternative estimators, such as doubly robust regressions (DRR), in Appendix C. In the same Appendix section, we also describe in detail the results from PSM estimations. For both DRR and PSM, we also show that the propensity scores and the individual covariates are balanced across treatment levels. Further, in Appendix D, we conduct in-space placebo tests by assuming false treatment groups. Specifically, we assume in turn that a state other than Washington is treated and estimate in-space placebo ATETs. These in-space placebo tests rely on non-parametric permutation tests, which do not require imposing any distributional assumption on the error term (MacKinnon and Webb 2019). In the same way, we also realize placebo tests by randomly assigning the treatment among all firms in the Russell 3000 index, regardless of their location. Results from this exercise are also presented in Appendix D. In this Appendix section, we show that our results are robust also to non-parametric inference. Such an approach is used in the literature also to pacify concerns about generated dependent variables, small-sample issues, cross-sectional correlation due to clustered assignment, and the absence of random assignment. In the following sections, we provide further insights into our results by analyzing heterogeneity along the carbon intensity dimension and by examining stock market reactions to the announcement of both initiatives.

Heterogeneity in stock market reactions

In this section, we investigate heterogeneity around the event effects. An obvious dimension on which to analyze stock market adjustments following the rejection of a carbon tax is carbon intensity. We follow the standard split-sample approach, so that we repeat our estimations by splitting the treatment and control groups into two sub-samples at the median carbon intensity of the treatment group.

The results for I-732 are illustrated in Figure 4. Here, we calculate the carbon intensity of firms by normalizing the carbon emissions variable with net sales. The emissions-to-sales ratio is particularly interesting for I-732, as this policy would have redistributed carbon-tax revenues by reducing the sales tax. In line with economic intuition, Figure 4 suggests that the effect of I-732's rejection, as detailed above, is mainly driven by the adjustments on the stock value of high-intensity firms.

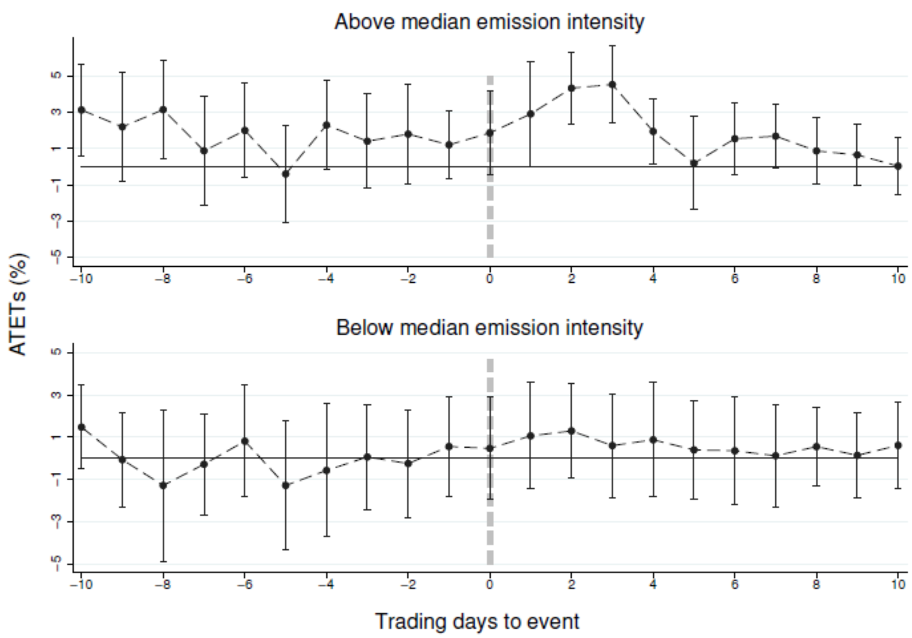
Figure 4: Heterogeneity in the ATETs of I-732 over the event window along the carbon intensity dimension with FFM-adjusted CARs



Note: This figure presents the average effect of ballot result for I-732 on above and below median carbon intensity firms in Washington state, together with in-time placebo tests. The event day (date 0) is denoted by the dashed vertical line. The days prior to the event window are used as placebo- event days. The estimation window is given by the 60-day period prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are denoted by the capped vertical lines.

The effect of I-732 on high-intensity firms is considerably larger than that on low-intensity firms. We now turn to I-1631. For this initiative, we investigate heterogeneity by normalizing emissions with the value of tangible assets such as plants, properties, and equipment. The results, presented in Figure 5, are consistent with Figure 4. Indeed, Figure 5 shows that, also for I-1631's rejection, the stock market adjustments are mainly driven by high-intensity firms.[9]

Figure 5: Heterogeneity in the ATETs of I-1631 over the event window with FFM-adjusted CARs



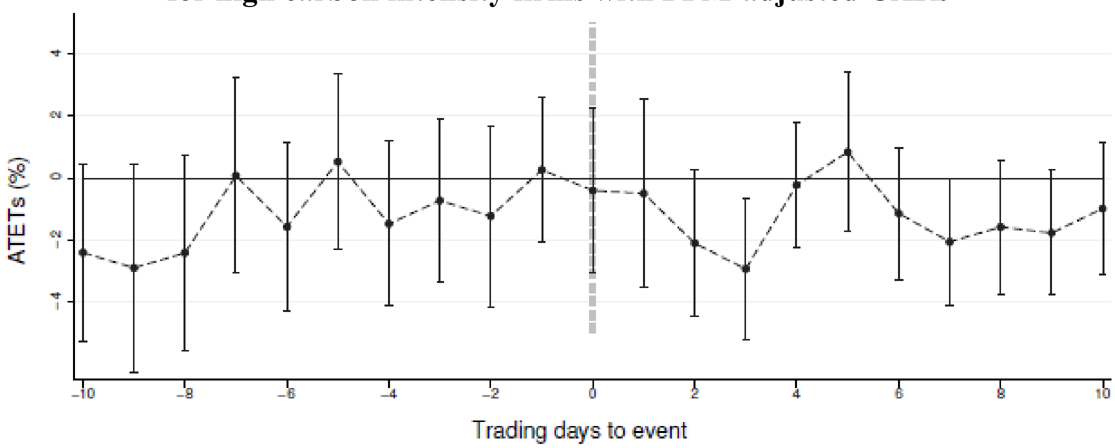
Note: This figure presents the average effect of ballot result for I-1631 on above and below median carbon intensity firms in Washington state, together with in-time placebo tests. The event day (date 0) is denoted by the dashed vertical line. The days prior to the event window are used as placebo- event days. The estimation window is given by the 60-day period prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are denoted by the capped vertical lines.

The results in Figure 5 show that the estimated average effect of I-1631 in the high carbon intensity sample tends to be as high as 5 percent. This estimate needs to be put

[9] Our findings are robust to alternative specifications of the carbon intensity ratio. Using sales as denominator leads to only slightly lower (higher) estimates for high-intensity (low-intensity) firms for I-1631. Using physical capital as denominator leads to a somewhat smaller difference between high- and low-intensity firms for I-732.

into perspective. First, it refers to an average over 50 percent of the universe of Washington-based firms, namely what we define as carbon-intensive firms. Second, it is the result of a very conservative approach, in the way the treatment is attributed. Third, it follows from the rejection of a proposal for which the odds of a defeat were not at 0 percent, despite the overall surprise with respect to the opinion polls. Fourth, both I-732 and I-1631 are policies with moderate ambition and limited jurisdiction. In this light, our results, which already suggest substantial readjustments in the value of 50 percent of the traded stocks, need to be interpreted as a lower bound for a potential national policy, or a regional policy with higher ambition.

Figure 6: Differences between the ATETs of I-732 and I-1631 over the event window for high carbon intensity firms with FFM-adjusted CARs



Note: This figure presents the differences between the average effects of ballot results for I-732 and I-1631 on Washington-based firms with above median carbon intensity. The event day (date 0) is denoted by the vertical dashed line. The days prior to the event window are used as placebo-event days. The estimation window is given by the 60-day period prior to the pseudo window. The 90 percent confidence intervals, based on robust standard errors, are denoted by the capped vertical lines.

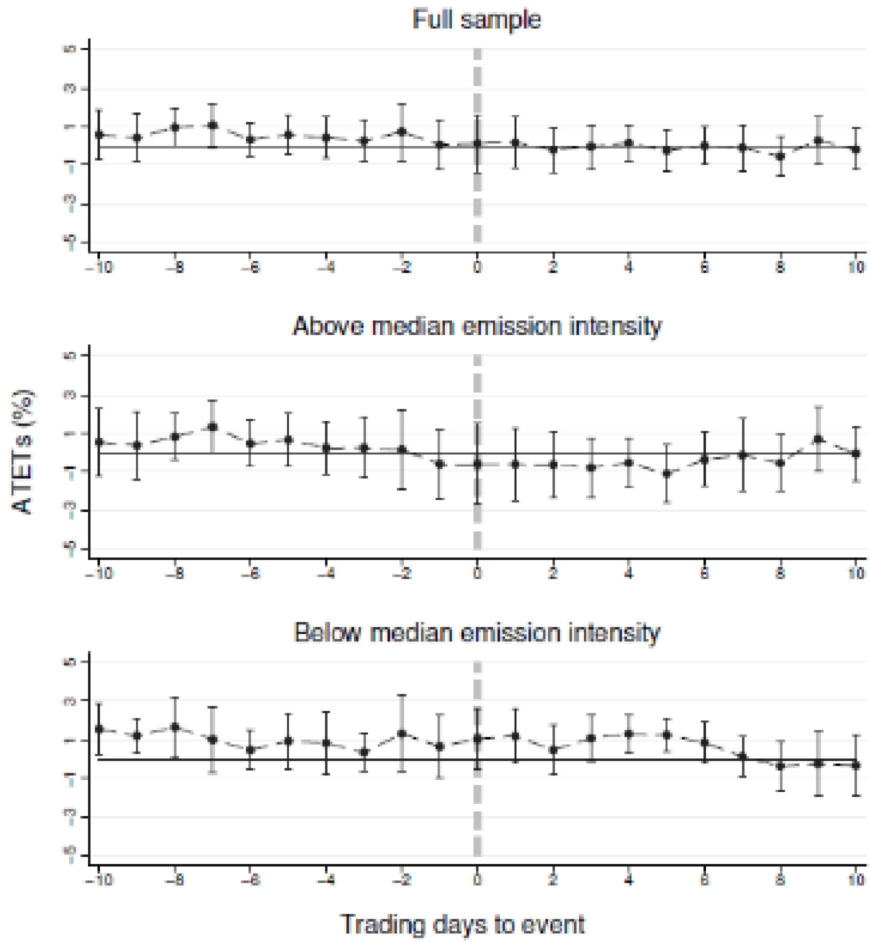
Revenue neutrality, however, may mitigate, if partially, the potential for systemic risk. In Figure 5, the estimated effects for high-intensity firms are considerably larger compared to I-732. We provide statistical tests on these differences in Figure 6, where we test the null of zero difference between the ATETs of each initiative, by using the standard specification test by Hausman (1978) with robust variance-covariance matrices. The results show that the estimated effect of I-1631 on high-intensity firms is significantly higher than that of I-732. These results are consistent with the different designs between the two carbon tax initiatives.

Stock market reactions on the path to the ballots

The positive stock market reactions that we observe following the rejection of both initiatives on the respective ballot days and shortly after both suggest that investors had already priced in, if only partially, the potential implications of a successful vote. In order to validate this intuition, we further investigate the reactions to the announcements of both initiatives. With respect to ballot days, announcements have one advantage. Since ballots take place on the same day of major elections, the presence of noise may make precise estimation of the ATETs harder to achieve. Such issue may not be present for announcements. However, with respect to ballot days, announcements also have disadvantages. Information about the plans to launch a new initiative may already circulate before its formal announcement. Hence, the adjustments that one may observe following a formal announcement may mainly capture the effect of investors readjusting their beliefs and upgrading carbon tax proposals from rumors to actual initiatives with potential to become policy. Further, announcements represent only the first of several steps before an initiative reaches the ballot box.

We start with the early days of I-732. On March 11, 2015, stand-up economist Yoram Bauman formally submitted the initiative to Washington's Secretary of State. The results for this date are presented in Figure 7. First, the estimated ATETs are negative for high-intensity firms and positive for low-intensity firms. The size of these reactions is comparable in absolute terms. As a result, the average reaction is close to zero. This pattern is similar, albeit not entirely symmetric, to what observed in Figure 4 for the ballot day. Note that the information set available to investors is different across the two dates. First, media coverage was relatively limited around the submission date. Second, the arguments of the opposition campaign may be absent, or less salient, at the time of the announcement. Hence, at the time of the announcement, investors might have rewarded low-intensity firms. Recall that I-732 was designed on the model of British Columbia's carbon tax. Empirical evidence has been circulating for some time suggesting that the British Columbia revenue-neutral carbon tax might have led to employment losses in energy-intensive firms, but employment gains in clean firms, especially small firms active in the local service sector (Azevedo et al. 2017; Yamazaki 2017). These findings have been considered evidence in favor of the "job-shifting hypothesis" of revenue-neutral carbon taxes, with which the evidence in Figure 7 is consistent. In this light, the more subdued effects on the ballot day may reflect a more conservative approach by investors, whose beliefs might have been influenced, over the course of about two years, by both campaigns as well as other stakeholders.

Figure 7: ATETs over the event window for submission of I-732 with FFM-adjusted CARs

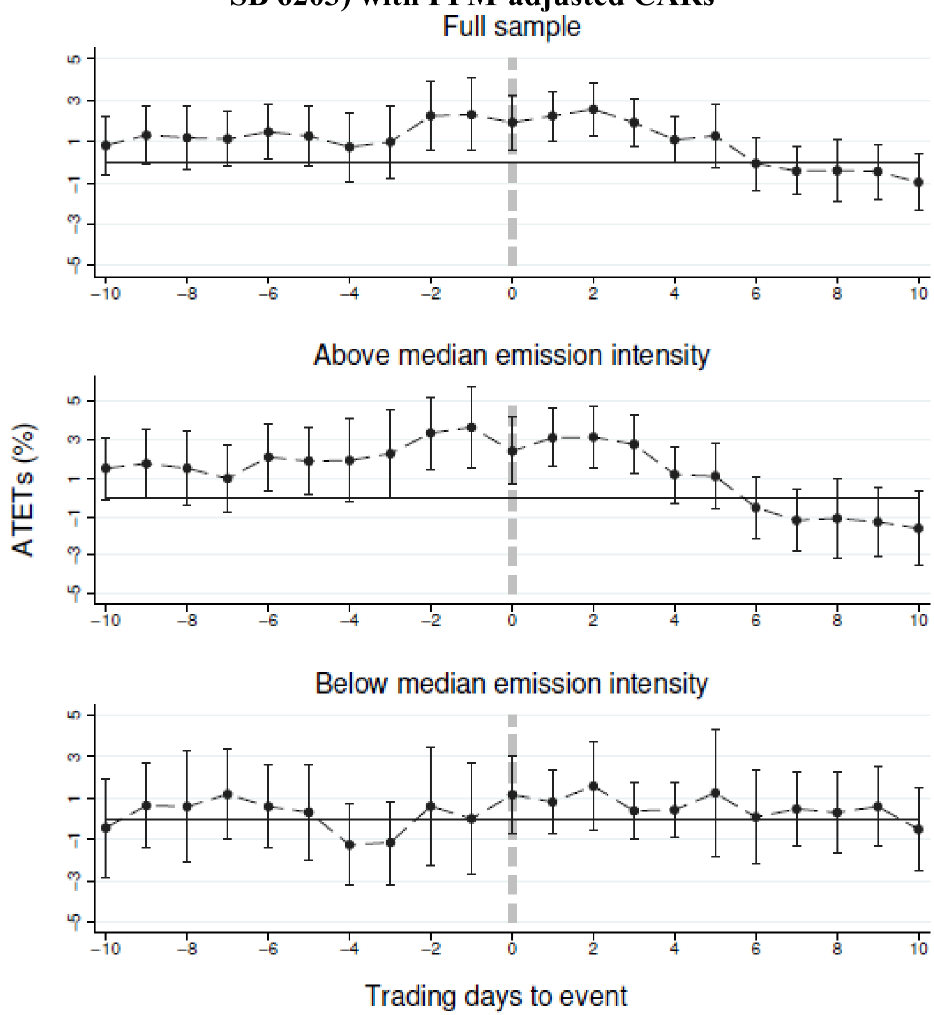


Note: This figure presents the average effect of I-732's submission on all Washington-based firms, as well as above and below median carbon intensity firms in Washington, together with in-time placebo tests. The event day (date 0) is indicated with the dashed line. The days prior to the event window are the placebo-event days. The estimation window is the 60 days just prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are indicated with capped-vertical lines.

We now turn to the early days of I-1631. As before, we start with the formal announcement, which took place on March 2, 2018. However, this announcement took place, intentionally, on the same date that Senate Bill 6203 (SB-6203), a carbon tax proposal championed by Governor Jay Inslee, failed to gather sufficient support among state legislators. The design of SB-6203 and I-1631 are similar, and these two overlapping events are linked. Immediately after the rejection of SB-6203, proponents of a carbon tax with earmarked revenues switched to the initiative toolbox to promote their policy design.

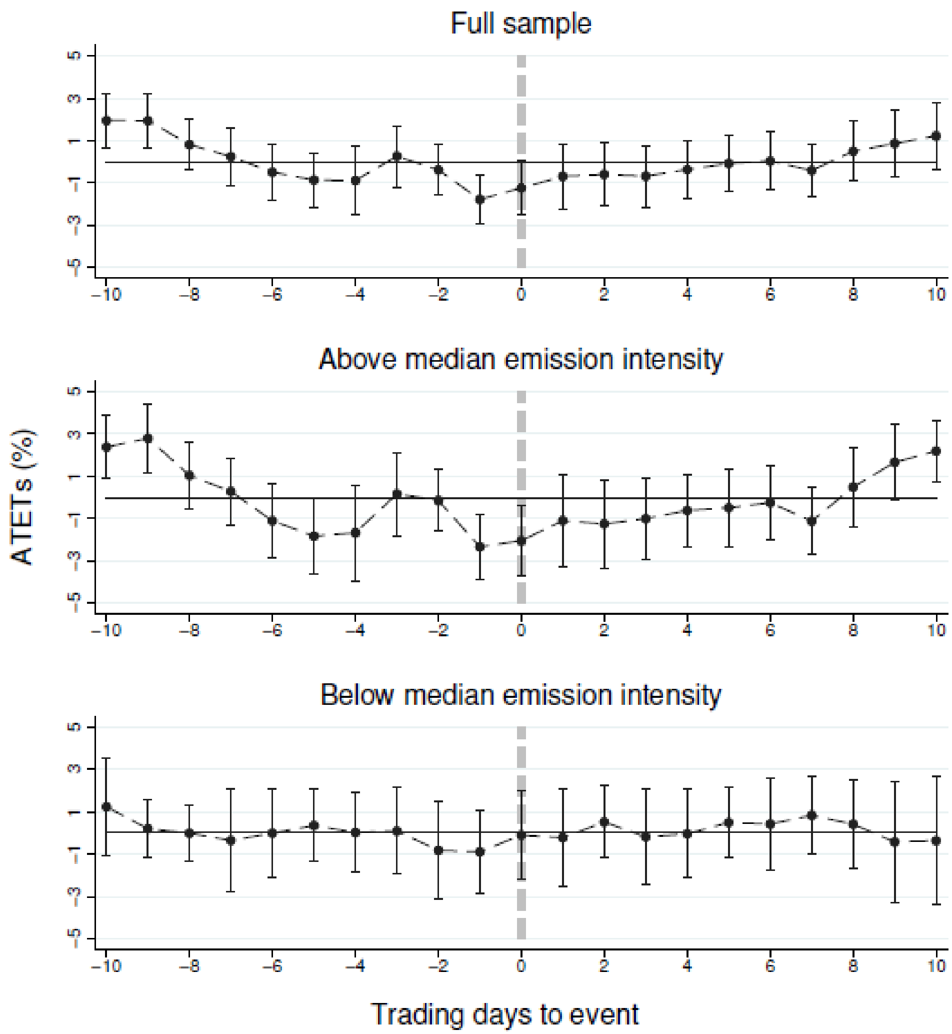
Figure 8 presents the estimation results for this date. The estimated reactions are very similar to those observed in Figure 5 following the rejection of I-1631: a positive market reaction driven mainly by high carbon-intensity firms. Hence, we may assume that the rejection of SB-6203, a carbon tax proposal that could have immediately become policy, dominated the effect of I-1631's announcement.

Figure 8: ATETs over the event window for submission of I-1631 (and rejection of SB 6203) with FFM-adjusted CARs



Note: This figure presents the average effect of SB-6203's rejection on all Washington-based firms, as well as above and below median carbon intensity firms in Washington, together with in-time placebo tests. The event day (date 0) is indicated with the dashed line. The days prior to the event window are the placebo-event days. The estimation window is the 60 days just prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are indicated with capped-vertical lines.

Figure 9: ATETs over the event window for the clearance for circulation of I-1631 with FFM-adjusted CARs



Note: This figure presents the average effect of I-1631's clearance for circulation on all Washington- based firms, as well as above and below median carbon intensity firms in the state, together with in-time placebo tests. The event day (date 0) is denoted by the dashed vertical line. The days prior to the event window are used as placebo-event days. The estimation window is given by the 60-day period prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are denoted by the capped vertical lines.

About two weeks after the submission date, on March 20, 2018, I-1631 was cleared for circulation by the state's Secretary of State. Figure 9 illustrates the results for the clearance date. Figure 9 shows that when I-1631 received formal clearance, the stock market reacted negatively, on average. Again, this reaction was driven mainly by high-intensity stocks. Note that, given the short time window between formal submission and formal clearance, the significant ATETs in the pre-event window are related to the rejection of SB-6203. The pattern that we observe for the date of clearance mirrors the effects found for the ballot date, when I-1631 was ultimately rejected. The fact that the market reacted when I-1631 received clearance also supports the idea that before the ballot date the market had partly priced in a potential success on the ballot.

Conclusions

About 20 percent of global greenhouse gas emissions are subject to carbon pricing. Many jurisdictions are considering implementing a carbon price while others are constantly increasing the stringency of their carbon pricing schemes. Following the Paris Agreement, a first acceleration in the implementation of carbon pricing schemes has been observed. A further acceleration may follow over the next few years, as countries are expected to ratchet up their climate goals, or Nationally Determined Contributions. Slowly but steadily, the world is moving forward towards a state in which coverage of greenhouse gas emissions by carbon pricing is much higher. Consistently, economists are increasingly vocal about the prospects of harmonizing carbon prices with the ultimate goal of achieving a global carbon price (Weitzman 2014; Stiglitz et al. 2017; Carattini et al. 2019).

A sudden convergence towards relatively high carbon prices, while justified from a climate perspective, may pose a threat to the stability of the financial system. As a result, central bankers have expressed deep concerns about the risk of financial contagion, driven by climate policy. Carbon pricing could lead investors to reevaluate a broad range of assets, of which many could become “stranded.” Given the considerable exposure of financial actors to carbon-intensive firms, beyond fossil fuel industries, and the strong interlinkages among such actors, there is high potential for a systemic risk. While central banks and other institutions in charge of financial stability have been investing important resources to refine their models of systemic risk and consider climate stress tests in their portfolio of macroprudential strategies, there is a need for empirical analyses, measuring whether and by how much investors reevaluate the value of stocks following a change in the probability of carbon pricing being implemented.

In this paper, we analyze the unique case of Washington state, where two carbon tax proposals were brought, two years apart, to the ballot box by two bottom-up initiatives. Furthermore, we leverage the different designs of these carbon tax proposals. I-732, rejected on the ballot in 2016, was designed as revenue neutral, with carbon tax revenues being compensated by lower sales taxes. I-1631, rejected in 2018, would have expanded the government’s budget. In particular, the carbon tax revenues would have been earmarked for environmental and social purposes.

We analyze the stock performance of Washington-based firms against their counterfactual scenarios and find important swings in the value of Washington-based firms following investors’ reassessment of the risks of a carbon price being implemented in the state. We identify significant reactions for both initiatives, suggesting that, if accepted, they would have led to a significant loss of market value for Washington-based firms. Consistent with its revenue-neutral design, we observe relatively smaller effects for I-732. Further, in both cases we observe important heterogeneity along the carbon-intensity dimension.

Hence, our findings support central banks' calls for macroprudential policies to anticipate the implementation of carbon pricing, which remains the most cost-effective policy even in the presence of stranded assets. While our results do not provide a direct test for the effectiveness of such macroprudential policies, they suggest that even a moderate carbon tax, with limited jurisdiction, can lead to important readjustments to the stock market, especially for carbon-intensive firms. Hence, the implementation of carbon pricing at the scale required to deal effectively with climate change, and potential global coordination on carbon taxes, could lead to important shocks to the financial sectors, if preemptive measures are not implemented.

References

Aldy, J. E. and Stavins, R. N. (2012). The promise and problems of pricing carbon: Theory and experience. *The Journal of Environment & Development*, 21(2):152– 180.

Allen, M. R., Frame, D. J., Huntingford, C., Jones, C. D., Lowe, J. A., Meinshausen, M., and Meinshausen, N. (2009). Warming caused by cumulative carbon emissions towards the trillionth tonne. *Nature*, 458(7242):1163–1166.

Amel-Zadeh, A. (2018). Social responsibility in capital markets: A review and framework of theory and empirical evidence. SSRN Scholarly Paper ID 2664547, Social Science Research Network, Rochester, NY.

Anderson, S., Marinescu, I. E., and Shor, B. (2019). Can Pigou at the Polls Stop Us Melting the Poles? SSRN Scholarly Paper ID 3400772, Social Science Research Network, Rochester, NY.

Angrist, J. D. and Pischke, J.-S. (2009). *Mostly harmless econometrics: an empiricist's companion*. Princeton University Press, Princeton.

Asheim, G. B. (2013). A distributional argument for supply-side climate policies. *Environmental and Resource Economics*, 56(2):239–254.

Azevedo, D., Wolff, H. L., and Yamazaki, A. (2017). Do carbon taxes kill jobs? Evidence of heterogeneous impacts from British Columbia.

Battiston, S., Mandel, A., Monasterolo, I., Schütze, F., and Visentin, G. (2017). A climate stress-test of the financial system. *Nature Climate Change*, 7(4):283–288.

Beatty, T. and Shimshack, J. P. (2010). The impact of climate change information: New evidence from the stock market. *The B.E. Journal of Economic Analysis & Policy*, 10(1).

Byrd, J. and Cooperman, E. S. (2018). Investors and stranded asset risk: Evidence from shareholder responses to carbon capture and sequestration (ccs) events. *Jour- nal of Sustainable Finance & Investment*, 8(2):185–202.

Caldecott, B., Harnett, E., Cojoianu, T., Kok, I., and Pfeiffer, A. (2016). Stranded assets: A climate risk challenge. Technical report, Inter-American Development Bank.

Campbell, J. Y., Lo, A. W., and MacKinlay, A. (1997). *The Econometrics of Financial Markets*. Princeton University Press.

Campiglio, E., Dafermos, Y., Monnin, P., Ryan-Collins, J., Schotten, G., and Tanaka, M. (2018). Climate change challenges for central banks and financial regulators. *Nature Climate Change*, 8(6):462.

Campiglio, E., Godin, A., Kemp-Benedict, E., and Matikainen, S. (2017). The tightening links between financial systems and the low-carbon transition. In Arestis,

P. and Sawyer, M., editors, *Economic Policies since the Global Financial Crisis*, International Papers in Political Economy, pages 313–356. Springer International Publishing, Cham.

Carattini, S., Carvalho, M., and Fankhauser, S. (2018). Overcoming public resistance to carbon taxes. *Wiley Interdisciplinary Reviews: Climate Change*, 9(5): e531.

Carattini, S., Kallbekken, S., and Orlov, A. (2019). How to win public support for a global carbon tax. *Nature*, 565(7739):289.

Carney, M. (2015). Breaking the tragedy of the horizon - Climate change and financial stability. Technical report, Bank of England, London.

CDP (2017). Putting a price on carbon: Integrating climate risk into business planning. Technical report, Carbon Disclosure Project.

Conway, D., Richards, K., Keenlyside, P., Mikolajczyk, S., Streck, C., Ross, J., Liu,

A. A., and Tran, A. (2017). Carbon tax case studies. Technical Report 129668, The World Bank.

Dietz, S., Bowen, A., Dixon, C., and Gradwell, P. (2016). ‘Climate value at risk’ of global financial assets. *Nature Climate Change*, 6(7):676–679.

Doda, B., Gennaioli, C., Gouldson, A., Grover, D., and Sullivan, R. (2016). Are corporate carbon management practices reducing corporate carbon emissions? *Corporate Social Responsibility and Environmental Management*, 23(5):257–270.

ESRB (2016). Too late, too sudden: Transition to a low-carbon economy and systemic risk. Technical Report No 6/February 2016, European Systemic Risk Board, Frankfurt am Main.

Fama, E. F. and French, K. R. (1993). Common risk factors in the returns on stocks and bonds. *Journal of Financial Economics*, 33(1):3–56.

Flammer, C. (2015). Does corporate social responsibility lead to superior financial performance? A regression discontinuity approach. *Management Science*, 61(11):2549–2568.

Gillingham, K., Carattini, S., and Esty, D. (2017). Lessons from first campus carbon-pricing scheme. *Nature*, 551(7678):27.

Goulder, L. H., Hafstead, M. A. C., and Dworsky, M. (2010). Impacts of alternative emissions allowance allocation methods under a federal cap-and-trade program. *Journal of Environmental Economics and Management*, 60(3):161–181.

Goulder, L. H. and Parry, I. W. H. (2008). Instrument choice in environmental policy.

Review of Environmental Economics and Policy, 2(2):152–174.

Goulder, L. H. and Schein, A. R. (2013). Carbon taxes versus cap and trade: A critical review. *Climate Change Economics*, 04(03):1350010.

Griffin, P. A., Jaffe, A. M., Lont, D. H., and Dominguez-Faus, R. (2015). Science and the stock market: Investors' recognition of unburnable carbon. *Energy Economics*, 52:1–12.

Hausman, J. A. (1978). Specification tests in econometrics. *Econometrica*, 46(6):1251–1271.

Hepburn, C., Neuhoff, K., Acworth, W., Burtraw, D., and Jotzo, F. (2016). The economics of the EU ETS market stability reserve. *Journal of Environmental Eco- nomics and Management*, 80:1–5.

Imai, K. and Ratkovic, M. (2014). Covariate balancing propensity score. *Journal of the Royal Statistical Society: Series B (Statistical Methodology)*, 76(1):243–263.

Imbens, G. W. and Rubin, D. B. (2015). *Causal Inference in Statistics, Social, and Biomedical Sciences*. Cambridge University Press.

Kim, E.-H. and Lyon, T. (2011). When does institutional investor activism increase shareholder value?: The Carbon Disclosure Project. *The B.E. Journal of Economic Analysis & Policy*, 11(1):1–29.

Krüger, P. (2015). Corporate goodness and shareholder wealth. *Journal of Financial Economics*, 115(2):304–329.

Linn, J. (2010). The effect of cap-and-trade programs on firms' profits: Evidence from the Nitrogen Oxides Budget Trading Program. *Journal of Environmental Economics and Management*, 59(1):1–14.

MacKinnon, J. G. and Webb, M. D. (2019). Randomization inference for difference- in-differences with few treated clusters. Technical Report 1355, Economics Department, Queen's University.

McGlade, C. and Ekins, P. (2015). The geographical distribution of fossil fuels unused when limiting global warming to 2 ◦C. *Nature*, 517(7533):187–190.

Meinshausen, M., Meinshausen, N., Hare, W., Raper, S. C. B., Frieler, K., Knutti, R., Frame, D. J., and Allen, M. R. (2009). Greenhouse-gas emission targets for limiting global warming to 2 ◦C. *Nature*, 458(7242):1158–1162.

Murray, B. and Rivers, N. (2015). British Columbia's revenue-neutral carbon tax: A review of the latest "grand experiment" in environmental policy. *Energy Policy*, 86:674–683.

Narassimhan, E., Gallagher, K. S., Koester, S., and Rivera Alejo, J. (2017). Carbon pricing in practice: A review of the evidence. Technical report, Climate Policy Lab, Medford, MA.

Oates, W. E. and Portney, P. R. (2003). The political economy of environmental policy. In Mäler, K.-G. and Vincent, J. R., editors, *Handbook of Environmental Economics*, volume 1 of *Environmental Degradation and Institutional Responses*, pages 325–354. Elsevier.

Oberndorfer, U., Schmidt, P., Wagner, M., and Ziegler, A. (2013). Does the stock market value the inclusion in a sustainability stock index? An event study anal- ysis for German firms. *Journal of Environmental Economics and Management*, 66(3):497–509.

Ramelli, S., Wagner, A. F., Zeckhauser, R. J., and Ziegler, A. (2018). Investor rewards to climate responsibility: Evidence from the 2016 climate policy shock. Working Paper 25310, National Bureau of Economic Research.

Rausch, S., Metcalf, G. E., Reilly, J. M., and Paltsev, S. (2010). Distributional implications of alternative U.S. greenhouse gas control measures. *The B.E. Journal of Economic Analysis & Policy*, 10(2).

Rozenberg, J., Vogt-Schilb, A., and Hallegatte, S. (2018). Instrument choice and stranded assets in the transition to clean capital. *Journal of Environmental Eco- nomics and Management*.

Sen, S. and Schickfus, M.-T. v. (2017). Will assets be stranded or bailed out? Expectations of investors in the face of climate policy. Technical Report 238, ifo Working Paper, No: 238.

Stiglitz, J. E., Stern, N., Duan, M., Edenhofer, O., Giraud, G., Heal, G., la Rovere,

E. L., Morris, A., Moyer, E., Pangestu, M., Sokona, Y., Shukla, P. R., and Winkler (2017). Report of the High-Level Commission on Carbon Prices. Technical report, Carbon Pricing Leadership Coalition.

Wagner, A. F., Zeckhauser, R. J., and Ziegler, A. (2018). Company stock price reactions to the 2016 election shock: Trump, taxes, and trade. *Journal of Financial Economics*, 130(2):428–451.

Weitzman, M. L. (2014). Can negotiating a uniform carbon price help to internalize the global warming externality? *Journal of the Association of Environmental and Resource Economists*, 1(1/2):29–49.

Weyzig, F., Kuepper, B., van Gelder, J. W., and van Tilburg, R. (2014). The price of doing too little too late. Technical report, Green European Foundation.

World Bank (2019). State and trends of carbon pricing – 2019. Technical report, World Bank, Washington, DC.

Yamazaki, A. (2017). Jobs and climate policy: Evidence from British Columbia's revenue-neutral carbon tax. *Journal of Environmental Economics and Management*, 83:197–216.

Appendices

A. Other Washington state initiatives on the ballot

In this section, we discuss other state initiatives that were on the ballot together with I-732 and I-1631. In 2016, Washington state voters were asked to vote on the following additional initiatives: Initiative 735 (I-735), Initiative 1433 (I-1433), and Initiative 735 (I-735). I-735 aimed at regulating political contributions, urging the Washington state congressional delegation to propose a federal constitutional amendment that reserves constitutional rights for people and not corporations, in response to *Citizens' United*. In the last month before the vote, opinion polls had 48 percent of the electorate in favor and 18 percent against. With no surprise, I-735 was accepted at 63 percent. I-1433 aimed at incrementally raising the state's minimum wage from $9.47 to $13.50 by 2020 and mandating employers to offer paid sick leave. In the last month before the vote, opinion polls had 57-62 percent of the electorate in favor and 27-31 percent against. With no surprise, I-1433 was accepted at 57 percent. I-1464 aimed at revising campaign finance laws and implement "democracy credits" with which residents could have redirected state funds towards qualifying candidates. In the last month before the vote, opinion polls had the largest share of voters as undecided. I-1464 was rejected at 54 percent.

In 2018, Washington state voters were asked to vote on the following additional initiatives: Initiative 940 (I-940), Initiative 1634 (I-1634), and Initiative 1639 (I-1639). I-940 aimed at limiting the use of deadly force by police. The most recent opinion polls had 68-69 percent of the electorate in favor and 21-18 percent against. With no surprise, I-1940 was accepted at 60 percent. I-1634 aimed at prohibiting local governments from enacting taxes on groceries. I-1634 was accepted at 56 percent. I-1639 aimed at restricting the purchase and ownership of firearms. In the last month before the vote, opinion polls had 59 percent of the electorate in favor and 34 percent against. With no surprise, I-1639 was accepted at 59 percent.

Both in 2016 and 2018, there were also advisory votes on the ballot. Advisory votes allow voters to share with legislators a preference about policies that are already enacted. Advisory votes are non-binding. In 2016, Advisory Vote 15 (AV-15) was on the ballot. It was aimed at repealing House Bill 2778 (HB 2778), which itself aimed to limit sales tax exemptions to alternative fuel vehicles. Such tax exemptions started in July 2016 and were supposed to run for three years or until one month after the state would have reached the goal of 7,500 electric vehicles sold. The proposal was approved at 60 percent, suggesting legislators repeal HB 2778. In 2018, Advisory Vote 19 (AV19) was also on the ballot. It was aimed at maintaining Senate Bill 6269 (SB 6269), which itself aimed to expand the oil response and administration taxes to include pipelines. SB-6269 was designed as an overall tax of 6 cents per barrel of oil transported via pipelines, estimated to yield on average around $1.3 million of annual revenues over 10 years. The effect of the price of oil, let alone the price of gas, would have been in the order of cents of cents. The total revenues collected by SB 6269 would have been a fraction of what was spent to promote and oppose I-1631, which, recall, was in the order of about $50 million dollars. The proposal was rejected at 54 percent, suggesting legislators repeal SB 6269.

As mentioned above, we are not concerned about these other initiatives acting as confounders for the following three reasons. First, some of them are largely irrelevant for the stock market, as only a small fraction of the economy is affected by either approval or rejection. Second, by the ballot date the outcome of most of them was clearly predictable, with polls suggesting a clear outcome and the ballot results matching the polls' forecast very well. Third, none of the initiatives on the ballot relate directly with the carbon intensity of firms in the way that initiatives I-732 and I-1631 do. If the ATETs that we find in the main analyses were driven only by these confounders, then we should not find any heterogeneity along the carbon-intensity dimension.

Table A.1: Average event effects on Washington-based firms using the market model

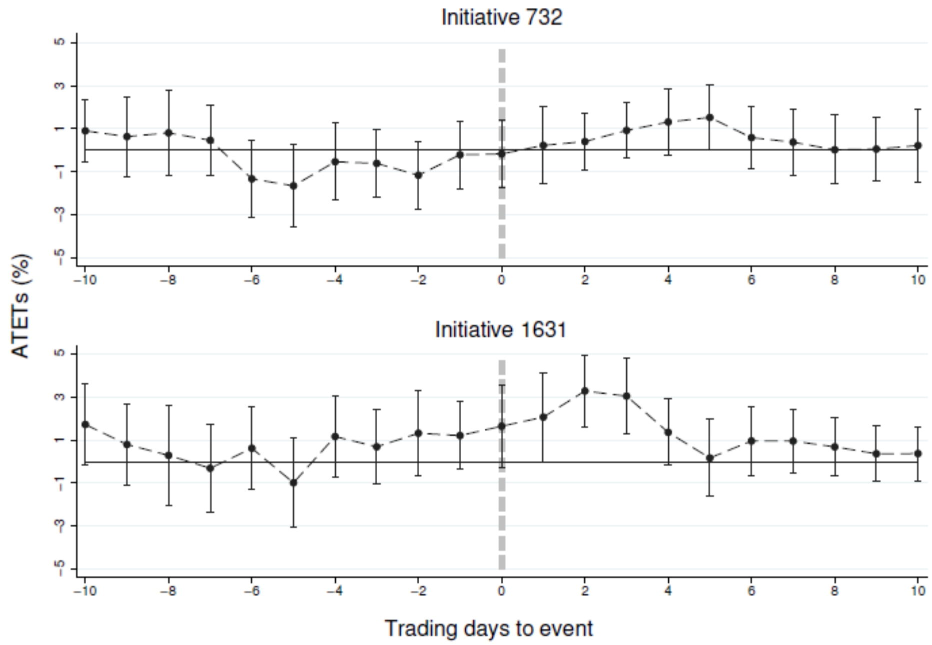
	Initiative 732			Initiative 1631		
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	0.076	0.731	0.175	2.012**	2.326**	2.072*
	(0.974)	(0.995)	(0.910)	(0.811)	(1.112)	(1.061)
Treatment	41	32	32	42	29	29
Observations	2511	1959	1959	2658	2134	2134
PSM	1.945**	3.294***	2.395**	2.168***	1.598	1.988**
	(0.963)	(0.850)	(1.006)	(0.781)	(1.201)	(0.844)
Treatment	44	31	31	42	29	29
Observations	2406	1962	1534	2526	2135	2027
Industry fixed effects	Yes	No	Yes	Yes	No	Yes
Covariate set	No	Yes	Yes	No	Yes	Yes

Note: This table presents the OLS and PSM estimates for the ATETs where the dependent variables are 10-days CARs from date 1 to 9 based on market model estimations. Robust standard errors are in parenthesis. The covariate set includes profitability ratio, leverage ratio, annual sales growth, market cap, 5-year corporate income tax rate, foreign sales ratio. Industry-specific fixed effects are based on ICB (the Industry Classification Benchmark by FTSE International) 1-digit industry classification. Significance levels are indicated as $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

B. Results with CAPM-adjusted CARs

In this section, we illustrate the robustness of our main results to using CARs based on the market model and the CAPM. Recall that our main results, presented in the main body of text, rely on FFM-adjusted CARs. The results from using the market model are presented in Table A.1. The estimated effects for I-732 are small and insignificant, while those for I-1631 are around 2 percent and statistically significant. Both in terms of size and significance, the results are close to the estimation results based on the CAPM presented in Table 3 in the main text. The results from the market model and the CAPM based estimations over time are also similar. In the rest of the section, we then only present CAPM based estimations.

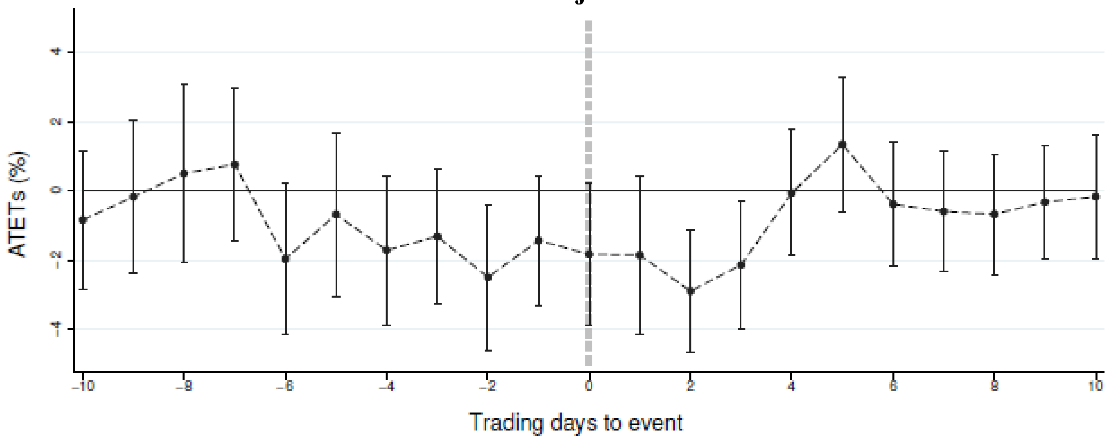
Figure A.1: ATETs over the event window with CAPM-adjusted CARs



Note: This figure presents the average effects of ballot results for I-732 and I-1631 on Washington- based firms, together with in-time placebo tests. The event day (date 0) is denoted by the vertical dashed line. The days prior to the event window are the placebo-event days. The estimation window is the 60 days just prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are indicated with capped vertical lines.

In Figure A.1, we present placebo and ATETs tests by using CAPM-adjusted CARs. The estimated pattern for I-1631 is very similar to that obtained with FFM-based estimations (see Figure 3 in the main body of text). For I-732, the ATET on the first date is insignificant and small, which corresponds to the results presented in Table 3. However, over the entire time window the estimated pattern for I-732 with CAPM-adjusted CARs also indicates a positive and gradual stock market reaction, which implies full consistency between CAPM- and FFM-adjusted CARs. The gradual adjustment pattern observed in this context is consistent with a relatively large amount of information to be digested after a major election.

Figure A.2: Differences in the ATETs of I-732 and I-1631 over the event window with CAPM-adjusted CARs



Note: This figure presents the differences between the average effects of the ballot results for I-732 and I-1631 on Washington-based firms with above median carbon intensity. The event day (date 0) is denoted by the vertical dashed line. The days prior to the event window are used as placebo-event days. The estimation window is given by the 60-day period prior to the pseudo window. The 90 percent confidence intervals, based on robust standard errors, are denoted by the capped vertical lines.

In Figure A.1, the differences between the estimated effects of I-732 and I-1631 are, if anything, larger than those implied by the FFM-based estimations. They also turn out to be statistically significant over dates 2 and 3, as shown in Figure A.2.

C. Balance of covariates and sensitivity to estimation methods

In this section, we present various diagnostic tests on the ability of our estimators to establish a balanced control group as well as estimation results from alternative estimation methods for treatments effects, such as PSM and other approaches from the standard toolkit. Both OLS and PSM rely on the conditional independence assumption for the identification of treatment effects (Angrist and Pischke 2009). In this section, we first start by relaxing the functional form specification imposed by OLS. We implement a regression-based approach known as doubly robust regression (DRR). In short, DRR is based on estimating an outcome model with OLS and correcting it for its potential misspecification with inverse-probability weights, which are obtained by estimating a treatment model. DRR estimates are unbiased even if one of the two models is misspecified. The rationale for using DRR is twofold.

Table A.2: Overall and covariate balance with DRR by using FFM-adjusted CARs

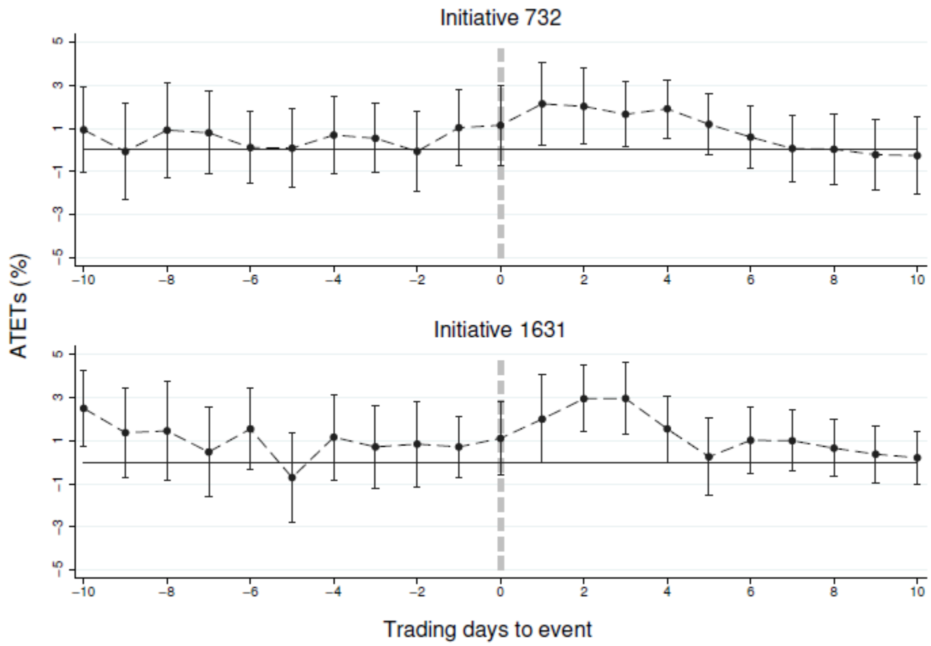
	I-732		I-1631	
Covariates / Sample	Full	Matched	Full	Matched
Leverage ratio	-0.400	0.001	-0.335	0.001
Sales to assets	-0.152	-0.002	0.007	0.003
Annual revenue growth	0.253	0.004	-0.062	0.000
Log of market cap	-0.098	0.004	-0.130	0.003
Corporate income tax rate	0.086	0.122	0.362	0.325
Foreign sales ratio	-0.364	-0.001	-0.145	-0.002
Tests on overall balance ($\chi2$)		5.159		3.248

Note: This table presents standardized differences in the means of covariates between the control and treatment groups in the full and matched samples. The last raw provides the $\chi2$ statistic to test the null hypothesis that the covariates are balanced across the treatment and control groups.

First, it provides an additional sensitivity check. Second, it allows us to test balancedness across treatment levels following a formal test introduced by Imai and Ratkovic (2014). In what follows, we start with DRR and then complete our series of robustness tests relying on PSM.

Table A.2 presents the standardized differences in the means of covariates and the test statistics for overall balance following our DRR approach applied to the ballot events. Even in the full sample without matching, the standardized differences are close to zero for half of our covariates for I-732 and all of our covariates for I-1631. In the matched sample, standardized differences are very close to zero for all the covariates. Most importantly, for all specifications we cannot reject the null that the covariates are balanced across the treatment and control groups. The $\chi2$ test statistics, provided in the last row, are, indeed, very large compared to the standard thresholds used in this exercise.

Figure A.3: DRR estimations for ATETs over the event window with FFM-adjusted CARs



Note: This figure presents the average effects of the ballot results for I-732 and I-1631 on Washington-based firms, together with in-time placebo tests. The event day (date 0) is denoted by the vertical dashed line. The days prior to the event window are the placebo-event days. The estimation window is the 60 days just prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are indicated with capped vertical lines.

Following this first sanity check, we use DDR as an alternative specification to test the robustness of our main results. The main estimates from DDR are presented graphically in Figure A.3. The estimates are virtually the same as those obtained with OLS and presented in Figures 3.

Table A.3: Overall and covariate balance with PSM and FFM-adjusted CARs

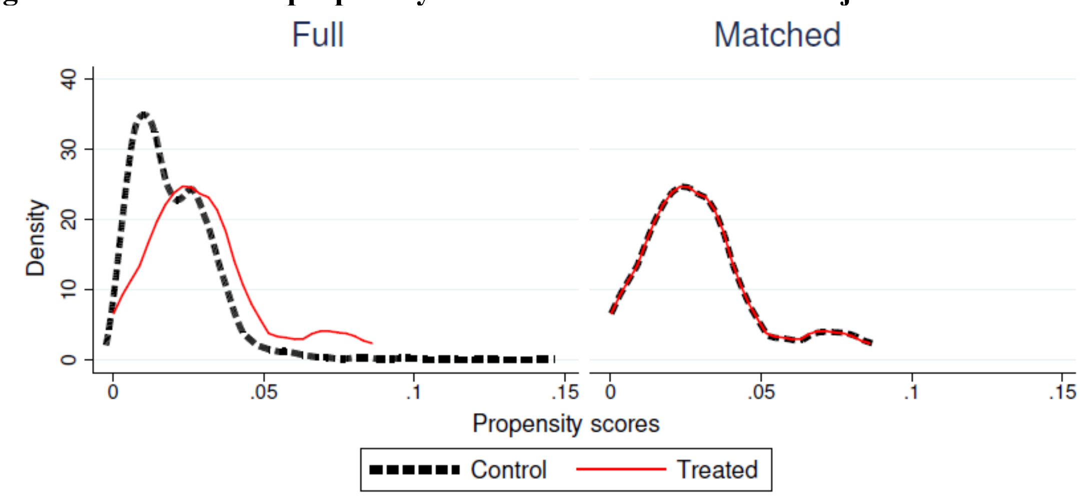
	I-732		I-1631	
	Full	Matched	Full	Matched
Leverage ratio	-0.400	-0.020	-0.335	0.066
Sales to assets	-0.152	0.039	0.007	0.058
Annual revenue growth	0.253	-0.084	-0.062	-0.104
Log of market cap	-0.098	0.040	-0.130	0.033
Corporate income tax rate	0.086	0.275	0.362	0.325
Foreign sales ratio	-0.364	-0.130	-0.145	-0.074

Note: This table presents standardized differences in the means of covariates between the control and treatment groups in the full and matched samples.

We now turn to the estimates from PSM. Following the same approach used with DDR, we first look at balancing between treatment and control groups and present the standardized differences in the means of covariates. Table A.3 presents the standardized differences in the means of covariates following our PSM estimations. The results closely resemble our previous results from DRRs.

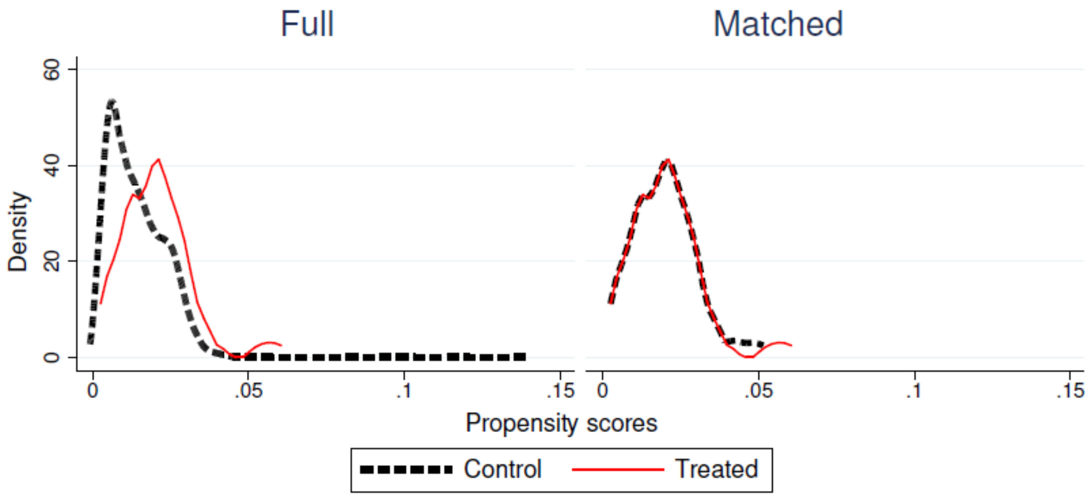
Following our examination of the balancedness between control and treatment groups, we display the distribution of propensity scores in Figure A.4 for I-732 and in Figure A.5 for I-1631. In the full sample, the mass of the estimated densities is in the same region, indicating “common support.” Furthermore, in either case, there is no probability mass near 1. Given these two observations, there is no evidence that the strict overlap assumption, which is required for estimating ATETs, is violated. As a result, the matching procedure balances the propensity scores successfully, as shown in the right panels of both Figures A.4 and A.5.

Figure A.4: Balance of propensity scores for I-732 with FFM-adjusted CARs



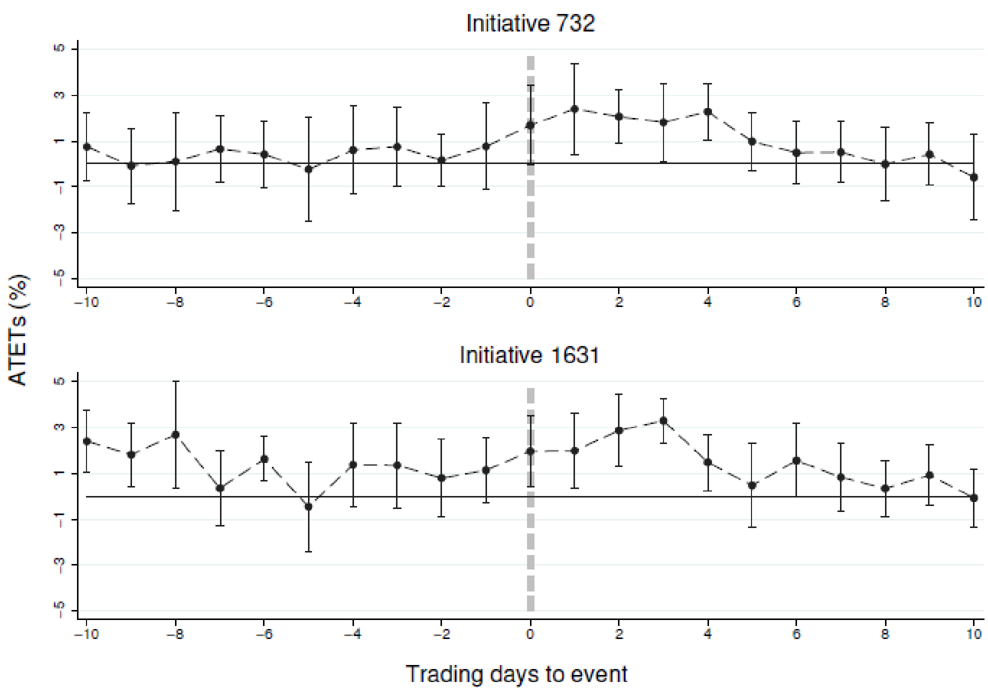
Note: This figure compares the distribution of propensity scores for I-731 across the treatment and control groups by using the full and matched samples.

Figure A.5: Balance of propensity scores for I-1631 with FFM-adjusted CARs



Note: This figure compares the distribution of propensity scores for I-1631 across the treatment and control groups by using the full and matched samples.

Figure A.6: PSM estimations for ATETs over the event window with FFM-adjusted CARs: Ballot results



Note: This figure presents PSM estimations for the average effects of the ballot results for I-732 and I-1631 on Washington-based firms, together with in-time placebo tests. The event day (date 0) is denoted by the vertical dashed line. The event day (date 0) is indicated with the dashed line. The days prior to the event window are the placebo-event days. The estimation window is the 60 days just prior to the pseudo window. The 95 percent confidence intervals, based on robust standard errors, are indicated with capped vertical lines.

D. Nonparametric inference and in-space placebo tests

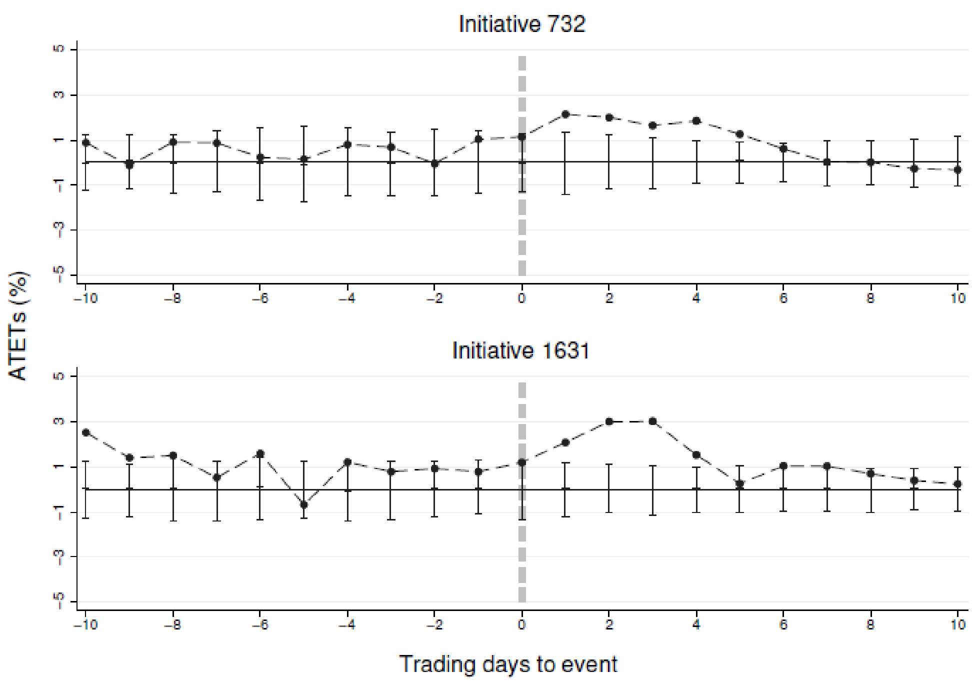
In this section, we relax all distributional assumptions and apply a non-parametric inference strategy to test the robustness of our methodological approach. Our non-parametric inference strategy relies on permutation methods. These tests are applied on the distribution of a test statistic, which is obtained through the random permutation of the treatment vector (Imbens and Rubin 2015). In our context, each permutation randomly assigns firms from the full Russell 3000 sample to either treatment or control groups, while preserving the original size of both groups.

We realize 1,000 random permutation of the treatment vector and present the results in Figure A.7. The capped lines indicate the median, 5th percentile, and 95th percentile of the distribution obtained with the random permutation. For inference purposes, we compute a *p*-value based on the rank of the estimated ATET in this empirical distribution. This *p*-value gives the probability of estimating, by pure chance, an estimate at least as large as our estimates for I-732 and I-1631. The results of this non-parametric exercise, presented in Figure A.7, show that this probability is below 5 percent. These results are robust to additional checks, such as combining randomization with stratification along the industry dimension. This additional check ensures that our results are robust also to random assignment of industries into the treatment group. That is, it confirms that the results are not driven by potential selection of industry clusters into treatment.

We now turn to another inference strategy suggested by MacKinnon and Webb (2019), in which we compare the estimated ATETs for I-732 and I-1631 against a distribution of placebo ATETs estimated from assigning the treatment to the states in the control group. This approach accounts for potential cross-sectional correlation due to treatment assignment in clusters.

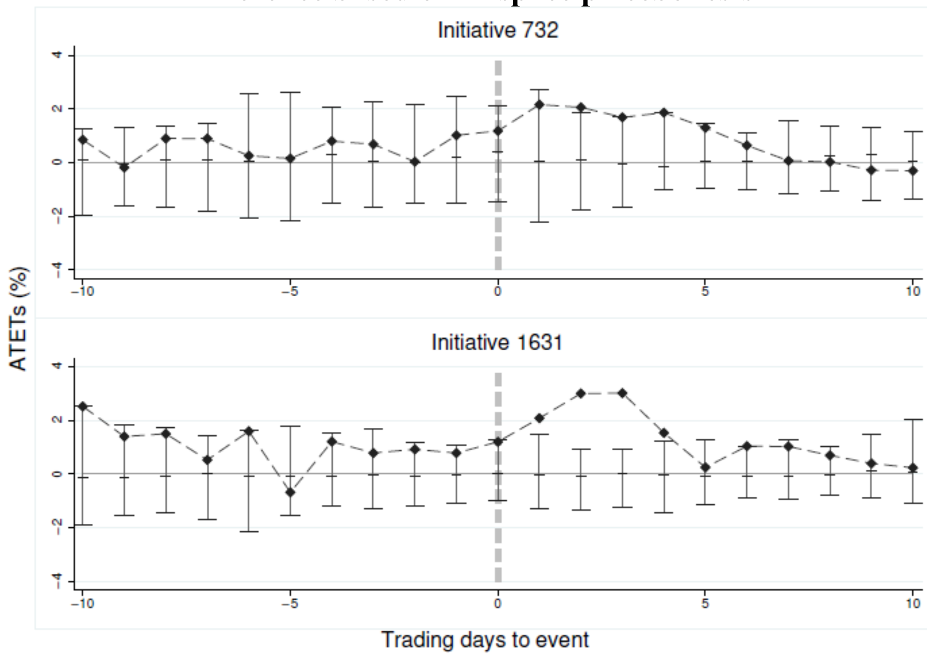
In our analysis, we exclude small states with fewer than 10 firms and focus on the remaining 33 states. Figure A.8 presents the results from these in-space placebo tests. Similarly to Figure A.7, the capped lines indicate the median, 10th percentile, and 90th percentile of the distribution of placebo ATETs obtained at each date. Figure A.8 shows that the estimated effects for I-732 and I-1631 are larger than 90 percent of the placebo ATETs. Note that, in the same spirit of all other analyses, we take a conservative approach and include in the distribution the estimated ATETs for Washington State (see MacKinnon and Webb 2019). We interpret these results akin to the non-parametric *p*-values from randomization inference techniques. That is, the probability of observing an estimate at least as large as our estimates for I-732 and I-1631 is at most 10 percent.

Figure A.7: ATETs over the event window based on FFM-adjusted CARs and randomization inference



Note: This figure presents the average effects of the ballot results for I-732 and I-1631 on Washington-based firms, together with in-time placebo tests. The event day (date 0) is denoted by the vertical dashed line. The days prior to the event window are the placebo-event days. The estimation window is the 60 days just prior to the pseudo window. The vertical capped lines show the median, 5th percentile, and 95th percentile of permutation tests.

Figure A.8: ATETs over the event window based on FFM-adjusted CARs and inference based on in-space placebo tests



Note: This figure presents the average effects of the ballot results for I-732 and I-1631 on Washington-based firms, together with in-time placebo tests. The event day (date 0) is denoted by the vertical dashed line. The days prior to the event window are the placebo-event days. The estimation window is the 60 days just prior to the pseudo window. The capped lines show the median, 10th percentile, and 90th percentile of in-space placebo ATETs from assigning the treatment to each state in the US.