Forecasting the Cost to Firms of Climate Policy using Prediction Markets and Lobbying Records

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Abstract

Forecasting the cost of proposed policies may be difficult when prior experience is limited. This paper develops a novel forecasting method that combines prediction market prices with stock returns to estimate the expected cost to firms of the Waxman-Markey climate policy bill. I find Waxman-Markey would have reduced the value of listed firms by $150 billion with greater losses for carbon intensive sectors. A regression discontinuity design finds sectors entitled to free allowances under the bill experienced larger gains. Lobbying records are used to estimate a political influence function for listed firms and to partially identify costs for unlisted firms.

*Princeton University and University of California, Santa Barbara, email: kmeng@princeton.edu. I thank Ethan Kaplan, Wolfram Schlenker, and in particular Bernard Salanié for their guidance. This paper has benefited from critical suggestions from Joe Aldy, Doug Almond, Scott Barrett, Linda Bui, Olivier Deschênes, Ray Fisman, Olle Folke, Larry Goulder, Michael Greenstone, Rema Hanna, Geoff Heal, Solomon Hsiang, Nat Keohane, Wojciech Kopczuk, Gary Libecap, Erich Muehlegger, Suresh Naidu, Matt Neidell, Tom Vogl, Gernot Wagner, Reed Walker, Marty Weitzman, and seminar participants at Columbia University, Environmental Defense Fund, Georgia Tech, Harvard Kennedy School, NBER EEE Summer Institute, Rutgers, UC Irvine, UC Santa Barbara, University of Connecticut, and University of Michigan. I am grateful to Richard Goettle, Jennifer Morris, and Sergey Paltsev for detailing CGE methodology and sharing output from the IGEM and EPPA models. All remaining errors are my own. Research support from the NSF GRFP. This version: February, 2014.

1 Introduction

Policy making relies on forecasts of the magnitude and distribution of costs and benefits associated with proposed policies. However, forecasting models may have trouble constraining key parameters when prior experience is limited. This difficulty characterizes current efforts to forecast the costs of relatively novel climate change policies using computable general equilibrium models. Given existing levels of uncertainty, Pindyck (2013) recently called for alternative approaches noting that current methods may not be practical. In particular, availability of other methods may be beneficial for studying proposed policies in the U.S. which, despite being the largest cumulative emitting nation, has yet to implement a national climate policy.

This paper develops a method for forecasting the cost to firms of U.S. climate policy. In the spirit of Hayek (1945), my approach acknowledges that while climate policy parameters may be unknown to the researcher, market participants and firms may reveal "local" information that can be incorporated into an estimation framework. Specifically, I combine prediction markets data with stock prices and lobbying expenditures to forecast the firm-level expected cost of the Waxman-Markey bill, a cap-and-trade policy that passed the House of Representatives in 2009 but not the Senate and thus was never implemented. I find that the expected incidence of Waxman-Markey would have reduced the total value of U.S. listed firms by $150 billion or roughly 1% of total market value.

This paper's central insight is that the prediction market event study pioneered by Snowberg, Wolfers and Zitzewitz (2007) can be extended into a forecasting setting. Snowberg, Wolfers and Zitzewitz (2011) show that prediction market prices, which approximate market beliefs over event probabilities, can be used to address bias from event window selection in traditional event studies. However, an arguably greater benefit of observing market beliefs is the ability to estimate abnormal returns associated with a probable, but ultimately unrealized, event. While Snowberg, Wolfers and Zitzewitz (2012) have noted this potential, to the best of my knowledge, forecasting using prediction markets has yet to be implemented in the literature. Thus, the general estimation framework developed in this paper can be applied to forecasting the cost to firms of other proposed, and perhaps unrealized policies. Recent examples with available prediction markets include U.S. legislation on immigration, social security, and health care policy.

A proposed cap-and-trade system should also affect unlisted firms, for whom market values are not observed but who may respond with political activity. Enlisting this observation, I use Congressional lobbying records to recover bounds for the cost borne by an important subset of unlisted firms that lobbied on Waxman-Markey. I first estimate a Becker

(1983)-type lobbying influence function for listed firms using my estimated firm-level effects and observed Waxman-Markey lobbying expenditures. The marginal returns to lobbying is symmetric for firms that gain and lose under Waxman-Markey as predicted by Becker (1983). I then use this function to bound the cost for unlisted firms that lobbied using a formal partial identification procedure (Manski, 2003). This procedure allows me to recover the magnitude but not the sign of cap-and-trade costs for unlisted firms and thus produces fairly conservative bounds between $70 and $240 billion for the total cost borne by all firms.

The presence of a strong lobbying influence function for listed firms provides my first validity check. In addition, I conduct several tests to detect whether markets responded to policy features that are both particular to the Waxman-Markey bill and general to climate policy incidence. First, Waxman-Markey allocated free allowances to manufacturing sectors with historical energy intensity greater than 5%. Using this threshold rule as the basis for a regression discontinuity design, I find that firms expecting an allocation of free allowances experience a relative gain in value which suggests that markets were aware of the distributional consequences of the policy. Second, Waxman-Markey allowed limited trading with the EU-Emissions Trading Scheme (EU-ETS) which would have lowered the price of EU-ETS allowances beginning in 2012. Indeed, I find that prediction market prices had a negative effect on 2012 EU-ETS futures prices which is statistically different from the effect on 2011 EU-ETS futures prices. Finally, in a series of heterogeneity analyses examining general climate policy incidence, I find the strongest effects in sectors with greater carbon intensity and energy intensity, import penetration, and exposure to U.S. product markets.

The prediction market used in this paper is more thinly traded than those used previously in the literature. As such, I develop a general empirical framework with explicit identifying assumptions to address estimation concerns such as price volatility, potential market manipulation, and the selection of benchmark controls. In particular, my identification is based on a subsample of high volume trading days during which there were major political developments that were likely exogenous to Waxman-Markey prospects. I furthermore conduct a series of indirect tests employing transactions-level data from the cap-and trade prediction market and show that my main result is robust to concerns about trading volume, individual large volume traders, and the overall competitiveness of the bidding environment as captured by a Herfindahl index. Thus, while the available data does not allow me to completely rule out biases due to thin trading, the combined weight of validity checks already mentioned and indirect tests for thin trading suggest it is unlikely that any remaining bias would substantially alter my main result.

This paper's use of decentralized private information has two important advantages for climate policy forecasts. First, the induced innovation hypothesis (Hicks, 1932) suggests that climate policy could trigger significant technological advances (Jaffe, Newell and Stavins, 2003). While this has been explored theoretically (Goulder and Schneider, 1999; Nordhaus, 2002; Buonanno, Carraro and Galeotti, 2003; Acemoglu et al., 2012), induced technological change presents modeling difficulties for many computable general equilibrium (CGE) models of climate policy (Jacoby et al., 2006). By relying on the expectations of market participants, my approach incorporates their dispersed information on the potential technological frontier. Second, because climate policy is typically designed to last several political cycles, future rent-seeking behavior will likely alter the policy's distributional consequences and thus the policy itself. Estimates from this paper incorporate such political dynamics to the extent that they are anticipated by market participants. At the same time, using market expectations has certain drawbacks. While my estimates capture the expected cost of the implemented policy, I am unable to confirm that this policy corresponds exactly to the Waxman-Markey bill. As such, this paper provides an important but fundamentally different estimate from that offered by CGE models which evaluate costs for a known policy.

My approach recovers the expected cost to firms and not to consumers and thus do not capture the full welfare effects of the policy. However, estimates of the overall cost to firms and its heterogeneity across firm characteristics are crucial for understanding the political economy of climate policy in which firms have historically played a pivotal role in policy formation (Bovenberg and Goulder, 2001). In particular, the heterogeneity analysis in this paper can inform future discussions on how free allowances may be allocated to secure political support during future legislative efforts. Finally, this analysis does not address issues of policy optimality as I do not consider global climate damages which are included in integrated assessment models of optimal climate policy (Stern, 2006; Nordhaus, 2008).

To the best of my knowledge, this paper provides the first forecast of the cost to firms of a proposed cap-and-trade policy outside CGE modeling efforts and as such provides an important input for future U.S. climate policy deliberations. There is, however, a long tradition of employing traditional event study methodology to evaluate, ex-post, the costs of realized regulation which includes recent event studies examining the cost of realized U.S. (Linn, 2010) and E.U. (Bushnell, Chong and Mansur, 2013) environmental regulations. Using a similar context, Lemoine (2013) conducts a traditional event study to evaluate the response of various energy commodity markets to a political event related to the Waxman-Markey bill. However, Lemoine (2013) does not normalize estimates according to changes in Waxman-Markey probabilities, which is particularly important for a policy that is never realized.

In the next section, I provide institutional details on cap-and-trade and the Waxman-Markey bill. Section 3 develops an empirical framework which details how a potential cap-and-trade policy can be forecasted using prediction markets. Section 4 presents the prediction market event study estimates along with robustness checks for key identifying assumptions and detection tests for policy features. Section 5 details a partial identification framework to bound costs for unlisted firms using lobbying expenditures and presents the lower and upper bound for the aggregate effect on all firms. Section 6 discusses how my approach and estimates compare with forecasts by prevailing CGE models of climate policy and is followed by a brief conclusion. The online appendix provides a general theoretical framework, details on several numerical simulations and empirical procedures, a data description, and further background on the Waxman-Markey bill and CGE climate policy models.

2 Background: Waxman-Markey

Over the past two decades, emissions trading, known popularly as "cap-and-trade", has become an increasingly important regulatory instrument for controlling regional and global pollutants such as greenhouse gases (Stavins, 1998; Aldy et al., 2010). In a typical cap-and-trade system, a limit on cumulative emissions is set for the lifetime of the policy allowing the regulator to issue annual emission allowances. Regulated firms are then either given, or must purchase, allowances to cover their annual emissions. Following the success of the U.S. SO_2 trading system introduced in the Clean Air Act Amendments of 1990, variants of cap-and-trade have been implemented domestically and internationally. Well-known systems currently in operation include provisions of the Kyoto Protocol, the European Unions Emissions Trading System (EU-ETS), the U.S. Regional Greenhouse Gas Initiative (RGGI), and the California cap-and-trade system. Economically, the compliance flexibility provided by cap-and-trade has been shown to yield lower costs than traditional command-and-control policies (Carlson et al., 2000; Ellerman et al., 2000). Politically, and in particular for the United States, this regulatory tool is considered more palatable than comparable Pigouvian tax schemes.

This backdrop has made cap-and-trade the centerpiece of U.S. domestic climate policy efforts over the last decade. After a series of failed Senate cap-and-trade bills in the early 2000s, the Democratic-led 111th House of Representatives introduced the American Clean Energy and Security Act in the spring of 2009. Known informally as the Waxman-Markey bill after its primary sponsors, the legislation specified a declining annual limit on emissions beginning in 2012 which would eventually cover 85% of greenhouse gas emitting sectors (see Figure A.1).[1] Waxman-Markey required that covered emissions decline by 17% in 2020, 42%

[1] While central to the Waxman-Markey bill, cap-and-trade was not the only component of the legislation. Alongside emissions trading were supply-side interventions such as a renewable energy portfolio standard as

in 2030, and 83% in 2050, all relative to 2005 levels.

Waxman-Markey contained two specific features that inform validity tests discussed later in the paper. First, to further build political support for the policy, policy makers allowed a large share of annual allowances to be freely distributed in the early years of the regulation. In particular, manufacturing industries deemed both energy intensive and trade sensitive were to be granted free allowances for the initial years of the policy.[2] Second, Waxman-Markey included provisions for trading with external cap-and-trade systems such as the EU-ETS which would alter the supply of available allowances in these other systems.

The Waxman-Markey bill passed the House of Representatives on June 26, 2009, marking the first time cap-and-trade legislation had passed either Houses of Congress.[3] Despite President Obama's support for a Senate bill with a similar cap schedule, prospects for cap-and-trade declined shortly after House passage. With the exception of Republican Senator Lindsay Graham joining Senate cap-and-trade efforts on Nov 4, 2009, the rest of 2009 and 2010 witnessed the gradual demise of cap-and-trade. Prospects for cap-and-trade were affected by the failure to reach a new binding international agreement at the UNFCCC Copenhagen negotiations and further declined following Scott Brown's Senate victory which weakened the filibuster-proof supermajority needed by the Democrats. On April 23, 2010, Senator Lindsay Graham withdrew support for cap-and-trade. Three months later, on July 22, 2010, a little over a year after House passage of Waxman-Markey, the Senate formally dropped deliberation over a comparable cap-and-trade bill (see Appendix F for a summary of these events). As prima-facie evidence that these events affected stock prices, Figure 1 plots the cumulative stock returns for several prominent companies during four of these major events. These companies, which were the seven highest spenders on Waxman-Markey related lobbying (see Table 5), generally exhibited negative abnormal stock returns on June 26, 2009 and Nov 4, 2009 and positive abnormal returns on Apr 23, 2010 and July 22, 2010.[4]

This politically turbulent period provides a suitable setting to study the market effects of cap-and-trade regulation for two reasons. First, as documented above, political developments

well as demand-side interventions such as incentives for electric vehicles. This analysis, therefore, evaluates the joint effect of cap-and-trade in conjunction with other components of Waxman-Markey.

[2]Specifically, Waxman-Markey deemed a 3-digit NAICS manufacturing or iron and steel production related sector as eligible for free allowances if for that sector both 1) energy intensity (measured as cost of energy inputs over total output) or carbon intensity (measured as 20 times sum of direct and indirect tons of CO_2 emissions over total output) was over 5% and 2) trade intensity (measured as sum of import and export value over sum of output and import value) was over 15%. The number of free allowances were initially set based on recent output levels and designed to decline in later years. Altogether, 60% of cumulative allowances were to be distributed freely over the lifetime of the policy.

[3]In the bicameral U.S. legislative system, a piece of legislation must pass both Houses of Congress before being sent to the President. Thus, passage of Waxman-Markey by the House of Representatives needed to be followed by a similar cap-and-trade bill approved by a Senate filibuster-proof supermajority.

[4]General Motors returns were excluded because it was not continuously listed during period of interest.

within this period provided large variation in cap-and-trade prospects from its peak in the summer of 2009 to its eventual decline one year later. Second, in contrast to earlier periods in which Congress considered several cap-and-trade bills simultaneously, the 111th Congress only seriously deliberated over the Waxman-Markey bill and its Senate variant. Without the potentially confounding effects of other climate and energy-related legislation, estimates from this period should better reflect direct concerns over Waxman-Markey incidence.

3 Empirical methodology

3.1 Prediction market event studies

The typical prediction market contract is a bet on the realization of an event at a certain date. When that date is reached, holders of a contract receive \$1 if the event is realized and zero otherwise with contract prices fluctuating within the unit interval prior to the termination date.[5] Under certain assumptions about prediction market participants,[6] the price of the contract can be interpreted as the real-time average market belief over event realization. When combined with stock returns, prediction markets can be used in an event study to estimate the abnormal returns attributed to that event.

The prediction market event study has two important advantages over traditional event studies. In a traditional event study, market beliefs are not observed and so the researcher must approximate the moment when markets first become aware of the possibility of an event. This is typically manifested in the selection of an event window in which one assumes that the probability of policy realization is 0 prior to the window. Any "fuzziness" in the release of information may violate this assumption resulting in estimates that are sensitive to event window selection as demonstrated in Snowberg, Wolfers and Zitzewitz (2007) and Snowberg, Wolfers and Zitzewitz (2011) on the macroeconomic effects of U.S. presidential elections. To avoid event window selection, Snowberg, Wolfers and Zitzewitz (2007) approximate market beliefs using prediction market prices such that each trading day with an active prediction market, known as an "event period", is used to estimate abnormal returns.

The second and arguably more important advantage is that prediction market prices allow researchers to estimate abnormal returns for a probable event even if this event is never

[5]Actual Intrade contract prices range from \$0 - \$10. I normalize prices to match probabilities.

[6]Wolfers and Zitzewitz (2006) show that two assumptions are required in order for prediction market prices to equal mean beliefs: 1) utility has a log form and 2) trader wealth and beliefs are independent. For other standard utility functions, the divergence between prediction market prices and mean beliefs is shown generally to be quite small when 1) traders are risk averse, 2) prices are within the \$0.20 – \$0.80 range, and 3) the distribution of beliefs exhibit relatively low dispersion. In the case where trader wealth and belief are correlated, the prediction market price reflects the wealth weighted average belief in the trading population.

realized. As such, the use of prediction markets can transform event studies into a tool for policy forecasting, a feature that is widely applicable to different policy issues but has thus far been little explored in the literature (Wolfers and Zitzewitz, 2009; Snowberg, Wolfers and Zitzewitz, 2012). There are, during any legislative period, a number of important policies that fail to become law but whose costs remain of interest, perhaps to inform future legislative efforts. Prediction markets have been offered for recent efforts to reform immigration, social security, and health care regulation in the U.S.[7] The following section formalizes how prediction markets can be used to forecast the cost to firms of a proposed climate policy.

3.2 Estimation framework

Let $i = 1...L$ index a listed firm and denote the difference in discounted present value of firm i at time t under Waxman-Markey and business-as-usual as $\Delta v_{it} = v_{it}(R) - v_{it}(R^o)$ (see Appendix A for the full theoretical framework). Unfortunately, neither is directly observed because the U.S. government has never passed cap-and-trade legislation nor was the probability of cap-and-trade realization ever zero within the event period. Instead, at each date t I observe the pair $[\widetilde{v}_{it}, \theta_t]$, denoting the actual market value of firm i and the prediction market price. Observed market value lies between my two values of interest, that is $\widetilde{v}_{it} \in [v_{it}, v^o_{it}]$, and is a function of the prediction market price.

To show this formally, suppose there are three policy states, $p \in [w, a, o]$, indicating the Waxman-Markey, alternative, and no-policy states respectively. For simplicity, I define the alternative policy as all possible climate mitigation policies that are not Waxman-Markey and should include a policy with identical abatement parameters but with a later implementation date. Define the random variable $q^p_t \in [0, 1]$ as the true average population belief at time t that potential climate policy p will be realized. Applying the law of total probability for a risk-neutral representative trader, I write:

$$\widetilde{v_{it}} = q^w_t v_{it} + q^a_t v^a_{it} + (1 - q^w_t - q^a_t) v^o_{it}$$

where v^a_{it} is firm value under the alternative policy. Thus, the observed market value of firm i at time t is the expected value given uncertainty about climate policy passage. Defining the effect of Waxman-Markey as $\gamma_i = \frac{v_{it} - v^o_{it}}{v^o_{it}}$ and likewise for the alternative policy effect, γ^a_i, results in:[8]

$$\widetilde{v_{it}} = v^o_{it}(1 + \gamma_i q^w_t + \gamma^a_i q^a_t) \tag{1}$$

[7] A list of all Intrade prediction markets is available here:`www.intrade.com/v4/reports/special/all-intrade-markets/all-intrade-markets.xlsx`

[8] The definition of the policy space implies that γ_i is time-invariant within the event period.

Taking logs and first differences of Equation 1 yields an expression for stock returns, r_{it}:

$$r_{it} = \ln(1 + \gamma_i q_t^w + \gamma_i^a q_t^a) - \ln(1 + \gamma_i q_{t-1}^w + \gamma_i^a q_{t-1}^a) + (\ln v_{it}^o - \ln v_{it-1}^o)$$

Note that for sufficiently small $\gamma_i q_t^w + \gamma_i^a q_t^a$, $\ln(1 + \gamma_i q_t^w + \gamma_i^a q_t^a) \approx \gamma_i q_t^w + \gamma_i^a q_t^a$, and thus:[9]

$$r_{it} = \gamma_i \Delta q_t^w + \gamma_i^a \Delta q_t^a + \Delta \ln v_{it}^o \tag{2}$$

To obtain an econometric specification, I enlist the two assumptions. The first assumption states:

Assumption 1 *$\Delta\theta_t$ is an unbiased estimate of Δq_t^w*

Assumption 1 allows for the prediction market price to be used as a proxy for average market beliefs over Waxman-Markey realization. A second assumption states:

Assumption 2 *$E[\Delta\theta_t \Delta q_t^a | \Delta \ln v_{it}^o] = 0$, within the event period*

That is, changes in average market beliefs on Waxman-Markey prospects are uncorrelated with beliefs over other plausible climate policy within the event period after controlling for normal market performance. With Assumption 2, one can replace Δq_t^a with an error term ϵ_{it} which together with Assumption 1 yields:

$$r_{it} = \gamma_i \Delta\theta_t + \Delta \ln v_{it}^o + \epsilon_{it} \tag{3}$$

I now discuss potential concerns with Assumptions 1 and 2 in my empirical setting.

3.3 Concerns about Assumption 1

Assumption 1 states that prediction market prices must be an unbiased proxy for average market beliefs over Waxman-Markey realization. From May 1, 2009 to Dec 31, 2010,[10] the online trading exchange Intrade hosted a prediction market contract on the prospects of a U.S. cap-and-trade system. This contract was titled: "A cap and trade system for emissions trading to be established before midnight ET on 31 Dec 2010." Intrade further defined this contract by noting:

[9]During the event period, the average $\theta_t = 0.24$ while the average estimated Waxman-Markey effect is $\gamma = -0.02$. Average beliefs and effects for alternative climate policies are likely even lower. Such small values allow the approximation to be reasonable. Econometrically, this results in attenuation bias.

[10]Intrade began offering this contract on March 25, 2009. However, trading began only on May 1, 2009, which marks the start of the event period.

"A cap and trade system will be considered established once federal legislation authorizing the creation of such a system becomes law, as reported by three independent and reliable media sources. Emissions trading under the system does not need to begin for the contract to be expired."

Figure 2 plots the price time series for this contract. A price of $0.50 indicates that market participants believed, on average, that cap-and-trade had a 50% chance of being realized before the end of 2010. Each solid red line identifies a major political event mentioned in Section 2 that had direct effects on the prospects of cap-and-trade passage in the U.S. Senate. Dashed gray lines indicate events with indirect effects. Importantly, Figure 2 shows that this prediction market was responsive to major cap-and-trade political developments (see Appendix F for a summary of these events).

Two aspects of this prediction market has the potential to violate Assumption 1. First, the contract describes a generic cap-and-trade system without explicit mention of Waxman-Markey, its particular abatement levels, and associated auxiliary policies. However, one can be reasonably confident that prediction market participants were reacting primarily to Waxman-Markey. This is in part because President Obama explicitly supported a cap-and-trade bill with a cap schedule similar to Waxman-Markey during the event period, a point that was noted on Intrade's cap-and-trade message board at the time.[11] Furthermore, whereas some details of a legislation can vary across House and Senate versions, important features such as the abatement schedule are usually unaltered in order for the two bills to be reconciled without additional floor votes. Thus, Senate efforts were likely constrained by the abatement levels specified in Waxman-Markey.[12]

A second, and potentially more troubling concern, is the thinness of this market relative to other prediction markets used in the literature. During the event period, 11,260 contracts were traded for a total value of $190,000. An average of 30 contracts were transacted every 2 days. By comparison, the prediction market used in Snowberg, Wolfers and Zitzewitz (2007) had an average of 129 trades for every 10-minute interval during election night. Transaction-level data acquired privately from Intrade indicates that there were 143 unique traders participating in the market.[13] It also reveals the presence of two large volume traders.

[11] Intrade cap-and-trade message board available here: `http://bb.intrade.com/intradeForum/posts/list/4343.page`

[12] Nonetheless, one cannot eliminate the possibility of Intrade participants betting on different cap-and-trade systems. Indeed, a cursory examination of Intrade's cap-and-trade message board reveals that some participants, though perhaps not those involved in betting, thought sectoral-level emissions trading schemes were more plausible in 2010.

[13] While Intrade does not provide information on where traders are located, Intrade has said in a public letter to the U.S. CFTC that "our 82,000 plus membership are predominantly resident in the United States" and that "78% of traffic to Intrade.com in the period 1 January to 30 June [2008] was from the U.S." Available here: `http://www.intrade.com/news/misc/CFTC_Intrade_Comment_Reg_Treatment_Event_Mkts.pdf`

Figures A.3 and A.4 plot the buying and selling volumes of Large Traders 1 and 2 relative to the total traded volume for the event period. Large Trader 1, a major buyer, was responsible for 38% of all contracts sold before the contract expired. Conversely, Large Trader 2 was responsible for 22% of all contracts purchased.

I address concerns over thin trading through specific estimation choices and tests. First, my main point estimate uses only trading days with major political developments during which trading volume was more than double the full sample mean (see Table 1). Second, all my variables are differenced over 2-day intervals which allows for the dissipation of short-term market overreactions and other transitory distortions. The use of longer time intervals also account for Intrade prediction markets having later closing hours than the primary U.S. stock exchanges as well as the occurrence of after-hours stock trading.[14] In addition to these estimation choices, I furthermore conduct a series of increasingly demanding statistical tests that fail to detect the effects of price manipulation. Specifically, I show that my main estimate is robust to interactions with trading volume, the presence of either large traders, and the overall competitive bidding environment as captured by a Herfindahl index.

This confirms several lines of graphical evidence shown in Figures A.3 and A.4. First, the direction of Intrade price fluctuations shown in Figure 2 for major event days intuitively match the cap-and-trade implications of those political developments. In particular, Large Trader 1's buying activity could not prevent the fall in prediction market prices following Senator Graham's exit. Second, the buying and selling patterns of Large Traders 1 and 2 respectively do not appear to be consistent with active price manipulation. That is, one would expect the buying volume of Large Trader 1 to be largest on the major event days, as indicated by vertical dashed lines in Figures A.3 and A.4, and similarly for the selling activity of Large Trader 2. With the exception of Senator Graham's exit, the observed pattern of transactions appears to suggest noise trading rather than price manipulation.

This stability of prediction market prices to the activity of individual traders is consistent with several prior empirical findings. Camerer (1998) places temporary bets designed to manipulate racetrack markets and concludes that successful long-term manipulation was unlikely even when considering efforts to distort relatively thinly traded markets. A similar conclusion is reached for both historical presidential betting markets (Rhode and Strumpf, 2004) and recent presidential prediction markets (Rhode and Strumpf, 2008). In particular, Rhode and Strumpf (2008) find that experimental efforts to manipulate the 2000 Iowa Electronic Market during thinly traded moments and observed efforts to manipulate the 2004

[14]Intrade closing prices are observed 2am on weekdays and 3am on weekends. If after-hours stock trading were to occur, the effect of information released after 4pm ET on trading days or over weekends would not be picked up using observed daily returns.

Tradesport market had effects that dissipated hours afterwards. Similarly, recent experimental work shows that price manipulators in prediction markets were unable to distort price accuracy (Robin, Oprea and Porter, 2006) nor influence the beliefs of third party observers (Hanson et al., 2011). A notable exception is Rothschild and Sethi (2013) who find evidence of possible manipulation in the 2012 Intrade presidential prediction market.

A number of additional issues related to Assumption 1 are worth noting. Wolfers and Zitzewitz (2006) show that with certain utility functions, a favorite-longshot bias and reverse favorite-longshot bias can occur for prediction market prices below \$0.20 and exceeding \$0.80 respectively. To address this concern, my estimation sample uses only trading days when prediction prices lie between \$0.20-\$0.80. Assumption 1 would also be violated if prediction market prices reflect some degree of concern over contract expiration prior to the expected realization of the event. In Section 4.2, I discuss an adjustment procedure using a similar 2009-expiring Intrade market to correct for impending contract expiration.

3.4 Concerns about Assumption 2

Assumption 2 requires that prediction market prices are uncorrelated with alternative climate policies after controlling for normal market performance. However, there is trade-off between identification and precision in finding suitable proxies for normal market performance when prediction markets are thinly traded. To see this, suppose I write $\Delta \ln v_{it}^o = \alpha_i + \beta_i \eta_t$. The most natural procedure for estimating the aggregate effect is to run the following value-weighted time series regression on aggregate market returns:

$$mkt_t = \tilde{\alpha} + \tilde{\beta}\eta_t + \tilde{\gamma}\Delta\theta_t + \epsilon_t \tag{4}$$

where $mkt_t = \sum_i \frac{\overline{v_i^o}}{\sum_i \overline{v_i^o}} r_{it}$, and $\tilde{\beta} = \sum_i \frac{\overline{v_i^o}}{\sum_i \overline{v_i^o}_i} \beta_i$ and $\overline{v_i^o}$ is average firm value under the no-policy scenario. The aggregate coefficient of interest is $\tilde{\gamma} = \sum_i \frac{\overline{v_i^o}}{\sum_i \overline{v_i^o}} \gamma_i$, which is the aggregate value-weighted effect of Waxman-Markey across all firms. However, as Roll (1977) has noted, η_t is not directly observed. Instead, one can run the following feasible bivariate time series regression:

$$mkt_t = \dot{\alpha} + \dot{\gamma}\Delta\theta_t + \dot{\epsilon}_t \tag{5}$$

Observe that because normal market performance is excluded in Equation 5, estimates of $\dot{\gamma}$ will typically suffer from omitted variable bias unless one has cause to believe $cov(\eta_t, \Delta\theta_t) = 0$. This assumption may be plausible during the night of a presidential election which allows Snowberg, Wolfers and Zitzewitz (2007) to run bivariate regressions similar to Equation 5 at 10-minute intervals. Thin trading in the cap-and-trade prediction market prevents me from using high-frequency returns during major event periods. Instead, I estimate Equation

5 using 2-day returns for the five days with major political developments directly affecting Waxman-Markey prospects as discussed in Section 2 and shown in Figure 2. Table 1 provides summary statistics for various indicators of cap-and-trade interest for both the full event period and the subset of major event days. The average absolute 2-day change in Intrade price during these five days is three times larger than changes across the full sample. When examining changes in media and internet interest, Google news headlines and Google search volume indicating "cap-and-trade" jump by over 9% and 1% in weeks when major events occurred compared to the full sample average of 3% and -0.002% respectively.

While point estimates from Equation 5 may be unbiased during these major events, precision of my estimates is low given the small sample size. To increase precision of my estimates, I also perform firm-level regressions for the entire event period with benchmarks for normal market performance to mitigate concerns about omitted variable bias in the full sample. A firm-level approach would also allow me to explore heterogeneity of Waxman-Markey effects across firms. My general firm-level specification is:

$$r_{it} = \gamma_i \Delta\theta_t + \boldsymbol{F_t}\boldsymbol{\beta_i} + \epsilon_{it} \tag{6}$$

where $\boldsymbol{F_t}$ is a vector of controls for normal market performance discussed below. I estimate Equation 6 for all firms continuously listed on NYSE, AMEX, and NASDAQ during the policy period.[15] However, with over 5,000 listed firms, panel regression of Equation 6 requires joint estimation of between 10,000 - 20,000 parameters depending on the controls for normal market performance which is computationally demanding. Instead, I estimate Equation 6 firm-by-firm using a seemingly unrelated regression (SUR) model and report both the equally and value-weighted average effect for all listed firms.[16] In the standard SUR framework, errors are correlated across firms but are iid over time and block homoscedastic.[17] Thus, the resulting standard errors are not robust to serial correlation and heteroscedasticity. To address this concern, I also perform panel regressions for a random subsample of firms imposing both heteroscedastic-robust and sector-level clustered standard errors to examine whether the SUR error structure is too restrictive.

I use several benchmarks for normal market performance because none is ideal on its own. The finance literature provides two standard benchmark models. The CAPM model includes a firm fixed effect and an aggregate market return index. The seminal work of Fama and French (1993) advises the use of returns from a value-based portfolio and a size-

[15]I exclude firms that are not continuously listed during this period because firm entry and exit in response to cap-and-trade regulation is not explicitly modeled.

[16]In a system of equations, if regressors are identical, firm-by-firm SUR is identical to systems GLS and achieves any efficiency gains provided by GLS (see (Greene, 2003, p. 341-344))

[17]Specifically, denoting Σ as the NTxNT variance-covariance error matrix from Equation 6, the element $\sigma_{it,js} = E[\epsilon_i \epsilon_j' | \boldsymbol{F_t}]$ $\forall$ $t = s$ and 0 otherwise.

based portfolio as additional controls in a 3-factor model to account for common risk factors associated with book-to-market ratio and firm size. These two standard models, however, have one major drawback in this context. An implicit assumption in event studies is that benchmark controls are not affected by the treatment of interest. While this may be likely for firm or sector specific treatments, cap-and-trade is expected to affect the entire economy. It is therefore possible that changes in Waxman-Markey prospects directly affect benchmark indices in the CAPM and 3-factor Fama-French models in the same direction as most firms which would bias my estimates towards zero. To address this concern over treatment spillover, I employ separate benchmarks using the value-weighted returns of listed firms with low carbon intensity. Specifically, I construct value-weighted indices for all firms in 6-digit NAICS sectors in which 2006 carbon intensity from own operations and inputs was below 0.05, 0.10, and 0.15 mton CO_2 per billion dollar output.[18] Controlling for the performance of low carbon intensive firms would also provide a cleaner control for the no-policy scenario if one believes that the prospects of alternative climate policies are priced into aggregate benchmarks used by the CAPM and 3-factor Fama-French models. However, low carbon intensive benchmarks may also be problematic. First, abnormal returns relative to the performance of low carbon intensive firms would not eliminate all treatment spillover if low carbon intensive firms experience any pecuniary effects of cap-and-trade through changes in input and output prices. Furthermore, low carbon intensity benchmarks constructed from a specific subset of firms may not fully capture common risk factors that are correlated with cap-and-trade prospects leading to omitted variable bias.

In light of these empirical limitations, I rely on multiple lines of evidence to obtain identified and precise estimates. My aggregate time series approach following Equation 5 using major event days provides arguably unbiased point estimates but at the cost of low precision. On the other hand, my full sample, firm-level SUR approach using various benchmarks for normal market performance provides potentially precise estimates that may be biased due to treatment spillover and omitted variables. If, however, point estimates from both approaches are similar, it is possible that my second approach provides estimates that are both identified and precise.

4 Listed firms: prediction market event study

This section presents event study results for listed firms (see Appendix E for a data summary). First, I show estimates from the aggregate time series regression on major events and

[18]There are 5, 23, and 82 6-digit NAICS sectors that are below 0.05, 0.10, and 0.15 carbon intensity in 2006.

firm-level SUR regressions for the full event period. Next, I provide a series of robustness results testing the validity of Assumptions 1 and 2. Finally, as further validation checks checks, I present a series of tests designed to detect market behavior in response to specific features of the Waxman-Markey bill and to climate policy incidence in general.

4.1 Main result

Table 2 shows the equally-weighted average effect, value-weighted average effect, and total aggregate cost of the Waxman-Markey bill for all continuously listed firms on NYSE, AMEX, and NASDAQ from May 01, 2009 to December 31, 2010. All variables are in 2-day intervals to address possible concerns about price volatility, investor overreaction, price manipulation, and the different closing hours for stock and Intrade markets. To avoid favorite-longshot bias and reverse favorite-longshot bias in the prediction market prices, I only include trading days for which $\theta_t \in [0.2, 0.8]$.

In Panel (a), I estimate a time series regression of aggregate market returns on the difference in prediction market price following Equation 5. The sample includes only the five major events with political developments that directly affected Waxman-Markey prospects and are plausibly uncorrelated with macroeconomic shocks.[19] My point estimate shows that had Waxman-Markey been implemented, listed firms would have lost a total of \$160 billion. However, because the time series is conduct over a small sample, precision of these estimates are low.

To obtain more precise estimates, I estimate firm-level SUR regressions using Equation 6 in Panel (b) of Table 2 with different benchmarks for normal market performance. In Rows (2) and (3), I use the standard CAPM and 3-factor Fama-French models. To address concerns about treatment spillover, in Rows (4)-(6) I use benchmarks constructed from the value-weighted returns of listed firms with carbon intensity below 0.05, 0.10, and 0.15 mton CO_2 per billion dollar output.

All models in Panels (b) show negative and statistically significant effects. Across the five models, the equally-weighted average effect for listed firms range from -1.4% to -2.2% while the value-weighted effect range from -0.66% to -0.1%. This translates to a total cost ranging from -\$120 to -\$190 billion with a mean of -\$150 billion. A few points are worth noting. The CAPM model yields slightly greater losses than the 3-factor Fama-French model

[19] These special events, corresponding to the red vertical lines in Figure 2, were 11/4/2009, 12/20/2009, 1/27/2010, 4/23/2010, and 7/22/2010. I exclude the day of Waxman-Markey passage, 6/26/2009, because prediction price activity in response to that event occurred entirely over the weekend during which stock markets were closed. The events marked with dotted gray lines in Figure 2 likely affected the prospects of other policies in addition to cap-and-trade and thus are excluded. See Appendix F for a summary of these events.

but do not fall outside the latter's 95% confidence interval. This suggests that cap-and-trade prospects may be positively correlated with the profitability of small market cap and high book-to-market firms. As discussed in Section 3.2, treatment spillover using standard models of normal market performance may lead to estimates that are biased towards zero. Benchmarks based on the performance of low-carbon intensive firms help partially alleviate concerns over treatment spillover but possibly at the cost of introducing omitted variable bias. Indeed, estimates in Rows (4)-(6) using low-carbon intensity benchmarks exhibit larger losses than those shown in Rows (1) and (2) thought this difference is small with estimates in Rows (4) and (5) being within the confidence interval of the estimate from the 3-factor Fama-French model.

The critical comparison is between the point estimates in Panels (a) and (b). The similarity in point estimates between the aggregate approach for major events and the firm-level approach for the full sample suggests that the aforementioned tradeoff between identification and precision is addressed by the joint presentation of these two estimation approaches. Finally, Figure A.2 presents the 3-factor Fama-French model result in a scatterplot of abnormal returns averaged across all firms against the change in prediction market price. It shows that abnormal returns are roughly linear in prediction market changes within the support of observed prediction price changes.

I now turn to a few important points regarding the standard errors shown in Table 2. While the point estimates are similar across the two approaches, standard errors are much lower for the firm-level regressions in Panel (b). There are three possible reasons for this difference in estimation precision. First, models in Panel (b) have a larger sample size. Second, the firm-level SUR estimation procedure in Panel (b) explicitly models the variance-covariance error structure across firms. Garrett (2003) and Veredas and Petkovic (2010) have shown that the firm-level regressions can result in different standard errors from aggregate-level regressions in the presence of non-zero covariance in the error structure across firms. In particular, if the sum of the covariance terms are positive as is the case with each SUR model in Panel (b), estimates from firm-level regressions would have greater precision than those from aggregate regressions. Finally, models in Panel (b) include controls for normal market performance which also increases estimation precision. In Appendix C, I detail a numerical simulation procedure to determine the relative contribution made by each of these three statistical properties and find that 85% of the difference in the uncertainty between the two methods can be attributed to increased sample size from 5 to 111 days.

Within Panel (b), it is apparent that standard errors from the equally-weighted average effect is much larger than standard errors from the value-weighted affected, or identically from the estimated total cost which is the product of the value-weighted effect and market value.

Figure A.5 plots the estimated mean squared error for each firm against its market value showing that larger firms tend to have less volatile residual stock returns as is expected given that large cap firms tend to be more heavily traded. This implies that the equally-weighted average effect, which does not scale estimates by firm size, would overstate the aggregate uncertainty. Finally, Section 3.2 noted concerns about block heteroscedasticity and serial correlation in SUR standard errors. In Table A.1, I conduct a joint panel regression for a 2% random sample of firms allowing for heteroscedastic- and cluster-robust standard errors at the 3-digit NAICS level. The latter allows arbitrary forms of cross-sectional and serial correlation within the same 3-digit NAICS sector. I find that concerns over a restrictive SUR error structure are not warranted as the alternative standard errors displayed in Table A.1 are similar to SUR standard errors in Table 2. Furthermore, it appears that 3-digit NAICS clustered standard errors yield more precise estimates possibly due to the presence of negative cross sectional and serial correlation within a 3-digit NAICS sector.

4.2 Testing Assumption 1

In this section, I present the equally-weighted average Waxman-Markey effect for all listed firms. My robustness results will be compared against the SUR regression using the 3-factor Fama-French benchmark model as shown in Row (3) of Table 2. The sample is again restricted to just trading days in which $\theta_t \in [0.2, 0.8]$.

Assumption 1 fails when prediction market prices are a biased estimate of the average market belief over Waxman-Markey realization. Table 3 presents a sequence of tests designed to explore whether thin trading and price manipulation might generate bias. Column (1) replicates the main result. To examine whether thin trading affects my estimates, I restrict the sample in Column (2) to days in which trading volume exceeded the sample mean and find that the point estimate is little affected. In Column (3), I conduct a less arbitrary test by interacting the daily trading volume with the prediction market variable. The interacted coefficient is small and statistically insignificant while the uninteracted coefficient becomes slightly smaller in magnitude.

As an initial test of price manipulation, I restrict in Column (4) the sample to just trading days in which neither Large Traders 1 nor 2 were participating in the prediction market. While the sample size drops by one-third, the Waxman-Markey effect falls within the 95% confidence interval of my main result. Simply examining days without the involvement of Large Traders 1 and 2, however, does not preclude other trading days in which the market was dominated by relatively few traders. Using transaction-level data with unique trader identifiers, I construct a daily buyer-based normalized Herfindahl-Hirshman Index (HHI).[20] This index captures the relative competitiveness of the prediction market for any given day. In Column (5), I restrict the sample to just days with HHI<0.25. The standard error for Column (5) is large as the sample is reduced to only 9 days but the point estimate is similar to my main result in Column (1). The HHI cutoff used in Column (5) is nonetheless arbitrary. My final and most stringent test interacts the prediction market variable with the daily HHI. The uninteracted prediction market term in Column (6) can be interpreted as the average effect of Waxman-Markey after removing the influence of prediction market bidding competitiveness. The Waxman-Markey effect in Column (6) is larger, but still within the 95% confidence interval of my main result in Column (1). However, because the interacted coefficient is not statistically significant, one cannot rely on the functional form modeled used in Column (6).

Another possible violation of Assumption 1 concerns Intrade contract expiration. The cap-and-trade prediction market used for this analysis expired on December 31, 2010, regardless of whether cap-and-trade regulation were to eventually pass Congress. Thus, while the prospects of cap-and-trade realization might indeed be declining in 2010, a component of the price movements shown in Figure 2 might also reflect expectations that policy realization is unlikely to occur before the end of 2010. In practice this was unlikely, as any legislation, having failed in the current Congress, is rarely reintroduced with identical features in a subsequent Congress. However, it is difficult to ascertain whether markets expected Waxman-Markey prospects to exist following the end of the 111th Congress. If so, a bias is introduced between the prediction market price and average market beliefs which increases as the expiration date nears. In Appendix B, I detail an adjustment procedure to separate average market beliefs, the true variable of interest, from concerns over contract expiration. This procedure uses information from a similar Intrade prediction market with an earlier expiration date at the end of 2009 (see Figure A.6). Under certain assumptions, I can use the period of overlap between the 2009 and 2010 expiring contracts to separate the effects of concerns over contract expiration with the true market belief in cap-and-trade prospects.

In Table A.2, I find that adjusting for contract expiration yields a coefficient similar to my main result. While using the adjusted prediction market price in general yield smaller effects, they fall well within the uncertainty of my main results shown in Table 2. This is because whereas the adjustment procedure illustrated in Figure A.8 inflates prediction market price levels to account for concerns of impending contract expiration, much of this

[20]Formally, for trading day t, there are $j = 1...J_t$ traders each purchasing s_{jt} share of all contracts transacted that day. The normalized Herfindahl-Hirshman Index is $H_t^* = \frac{H_t - 1/J_t}{1-1/J_t}$ where H is the Herfindahl-Hirshman Index, $H_t = \sum_j s_{jt}^2$.

adjustment is already removed from the unadjusted prediction market price after the data was first-differenced.

4.3 Testing Assumption 2

Assumption 2 requires that Waxman-markey beliefs, as approximated by prediction market prices, are uncorrelated with alternative climate policies after controlling for normal market performance. As discussed in Section 2, cap-and-trade dominated climate policy debates in the United States during the event period. Figure A.9 plots the number of U.S. news article compiled by Google that contained the term "cap-and-trade" and terms capturing several alternative climate policies during the event period.[21] Observe that the U.S. media cited cap-and-trade far more than alternative climate policies during the event period. However, it also appears that media interest in cap-and-trade declined in 2010 as coverage of alternative policies intensified.

To see whether this poses a violation of Assumption 2, I augment the controls for normal market performance to include indices that possibly capture the contemporaneous prospects of alternative climate policies in Table 4. Column (1) replicates my main result. Column (2) shows that the estimated Waxman-Markey effect is unperturbed by the inclusion of a linear trend, suggesting that first-differencing effectively removes common trends in the data. 2009-2010 was a particularly volatile period for oil prices, witnessing both a historic high and decline in global prices. Given the tight coupling between oil prices and carbon emissions, one might be concerned that prediction market prices were driven by daily oil price movements. In Column (3), I include oil price returns as a control which has little effect on the coefficient of interest. In Column (4), I control for beliefs over alternative climate policies by including changes in the frequency of alternative climate policy headlines from Google News as shown in Figure A.9. These controls have little effect on the coefficient of interest. Finally, in Column (5) a kitchen sink regression is performed using a vector of monthly macroeconomic indicators commonly used in the finance literature to predict the aggregate market risk premium (see Welch and Goyal (2008)).[22] Unsurprisingly, given the known low predictive power of these variables, the estimated effect differs little from Column (1).

[21] Google News tabulates any news articles containing a particular term of interest. Thus, it is possible that an article about "cap-and-trade" would also include mention of "energy policy".

[22] Due to computational limits, I am unable to control for the entire set of Welsh-Goyal variables, instead only choosing those with predictive power (Welch and Goyal, 2008). These controls include the variance of returns on the S&P 500 (svar), the book-to-market value of the DJIA (bm), the long-term yield (lty) and rate of return (ltr) on U.S. government bonds, a 12-month moving sum of net NYSE issues over total capitalization (ntis), and inflation from the CPI (infl).

Previous prediction market event studies noted concerns about reverse causality with time series analyses (Snowberg, Wolfers and Zitzewitz, 2011). My panel approach partly addresses this concern by removing common risk factors. Table A.3 explores this further by adding lead and lag terms to my 3-factor Fama-French specification. Across all models, my main result attenuates slightly but remain statistically significant at the 5% level. Column (1) replicates my main result while for Column (2) a 1-period lagged return is included and is not statistically significant. This result also mitigates concerns about serial correlation in the residuals. Column (3) includes a lagged prediction market term which is not statistically significant suggesting that markets incorporate information on policy prospects within a 2-day window, obviating the need for longer return intervals. In Column (4), a lead prediction market term is not statistically significant implying there is no evidence of stock markets anticipating future prediction market activity.

Finally, in Table A.4, I consider different trading day subsamples. Figure A.9 and Intrade's message board suggest that media and investor beliefs over alternatives climate policy prospects were increasing at the start of 2010 as Waxman-Markey beliefs were declining, possibly violating Assumption 2. In Columns (2) and (3), I estimate my model for trading days in 2009 and 2010 showing that the coefficient is relatively stable across the two years. In Columns (4) and (5), I test whether market participants responded asymmetrically to the direction of prediction market changes by restricting the sample to trading days in which $\Delta\theta_t \geq 0$ and $\Delta\theta_t < 0$ respectively. I find no evidence of asymmetric effects and is consistent with the linear response shown in Figure A.2.

4.4 Additional validity tests

This section presents a series of validity checks for my prediction market approach. In particular, I combine my firm-level prediction market estimates of Waxman-Markey costs with additional data to detect patterns of behavioral response that are consistent with climate policy incidence in general and with specific features of the Waxman-Markey bill.

Expenditures on Waxman-Markey lobbying

The magnitude, scope, and distributional consequences of U.S. climate policy has led some observers to call the associated rent-seeking activity as the "sum of all lobbies."[23] Following Becker (1983)'s political competition framework, there may exist an influence function relating political lobbying and the equilibrium level of subsidy or tax a firm receives from a

[23]For example: `http://e360.yale.edu/feature/an_army_of_lobbyists_readies_for_battle_on_the_climate_bill/2131/`

redistributive policy such as cap-and-trade. Moreover, the marginal effect of lobbying may be symmetric - that is, in equilibrium, an increase in subsidies from additional lobbying by a policy winner should equal the decrease in taxes resulting from additional lobbying by a policy loser.

To test this framework, I identify all firms that have explicitly lobbied on Waxman-Markey and related climate bills during the 111th Congress as indicated in lobbying records collected by the Senate Office of Public Records.[24] Special care was taken to drop lobbying firms, trade organizations, and advocacy organizations that lobbied on Waxman-Markey but represent industry-level interests and not that of individual firms.[25] These records reveal that 459 separate firms lobbied on Waxman-Markey of which 234 were listed firms. Overall, \$1.5 billion worth of lobbying records indicated Waxman-Markey lobbying.[26] Table 5 lists the firms with the highest lobbying expenditure which as expected is dominated by energy intensive firms. I then estimate the following regression in Table 6:

$$\log\left|\widehat{\gamma_i}\widehat{v_i^o}\right| = \alpha + \eta \log LobbyExpense_i + c_s + \mu_{is} \tag{7}$$

where $\widehat{\gamma_i}$ is the estimated effect of Waxman-Markey from Section 4 and $\widehat{v_i^o}$ is the predicted market value under business-as-usual.[27] An absolute value operator was applied to ensure that an elasticity can be computed for firms that lose value under Waxman-Markey. $LobbyExpense$ is the total amount spend lobbying on Waxman-Markey and c_s are 3-digit NAICS sector fixed effects. Column (1) of Table 6 estimates the marginal returns to lobbying for firms estimated to gained value under Waxman-Markey while Column (2) presents the estimate for firms that lose value under the policy. Interpreting the cross-sectional elasticities, a 1% increase in lobbying expenditures is associated with a 0.36% increase in the cap-and-trade subsidy for positively affected firms. Conversely, a 1% increase in lobbying

[24]Each lobbying form indicates the lobbying institution (a private company if internal lobbying or lobbying firm if external lobbying), the client served, names of lobbyists employed, a list of lobbying issues, and the total amount paid by the client to the lobbying institution (see Appendix E for further details). To isolate cap-and-trade related lobbying, I extract the names of clients from lobbying records that indicate either H.R. 2454, H.R. 587, H.R. 2998, S.1733, or S.1462 in the "specific lobbying issues" entry on the lobbying form. If multiple issues are noted on a lobbying form, total lobbying expense will include all issues. This would mean that not all amount indicated was spent on cap-and-trade lobbying. However, a spot check of lobbying records showed that most forms noting cap-and-trade lobbying largely included issues that were closely related.

[25]In general, it is difficult to identify which firms are associated with certain trade or advocacy organizations. Fortunately, expenditures by trade and advocacy organizations constitute only 5% of total Waxman-Markey lobbying expenditures.

[26]Because of the structure of the lobbying records, it is unclear if \$1.5 billion was spent only lobbying on Waxman-Markey. For the purposes of this analysis, what matters is the order of lobbying expenses for firms and not its actual value.

[27]Obtained by rearranging Equation 1 so that $\hat{v}_i^o = \frac{\overline{\widetilde{V}_i}}{\overline{\theta}\hat{\gamma}_i+1}$, where the bar denotes the average over the event period.

expenditures is correlated with a 0.45% decrease in the cap-and-trade tax for negatively affected firms. Consistent with Becker (1983), the marginal returns to lobbying is not statistically different for winners and losers. Column (3) presents the pooled estimate for all listed firms that lobbied. Figure 3 plots the pooled relationship for all listed firms.

Regression Discontinuity at 5% energy intensity

As discussed in Section 2, Waxman-Markey granted free allowances to manufacturing sectors with historical energy intensity greater than 5%. Using 6-digit NAICS energy intensity data from the NBER-CES database, I examine whether there is a discontinuity in estimated Waxman-Markey effects at 5% energy intensity. Figure 4 provides graphical evidence by plotting my estimated Waxman-Markey effects as local polynomial functions of energy intensity on both sides of the 5% cutoff suggesting that a discontinuity exists. Sectors with energy intensity slightly higher than 5% experience greater abnormal returns from the expected allocation of free allowances than sectors with energy intensity immediately less than 5%. A density continuity test using the McCrary (2008) procedure do not find a discontinuity in the distribution of firms at the 5% cutoff (not shown) suggesting that markets did not expect firms to sort around the discontinuity. Turning to regression results, Table 7 shows estimates from the following local linear model for manufacturing firm i in sector s within various bandwidths around the cutoff value:

$$\widehat{\gamma}_{is} = \alpha_1 + \alpha_2 \mathbf{1}[EnInt_{is} > .05] + \alpha_3(EnInt_{is} - .05) + \alpha_4 \mathbf{1}[EnInt_{is} > .05](EnInt_{is} - .05) + c_s + \mu_{is} \quad (8)$$

where $EnInt_{is}$ is the 6-digit NAICS energy intensity matched to the firm, and c_s are 3-digit NAICS fixed effects. Standard errors are clustered at the 6-digit NAICS level. The discontinuity of interest is captured by α_2 and is displayed in Table 7. In Column (1), I estimate a discontinuity of 6% regardless using the optimal bandwidth selected by the Imbens and Kalyanaraman (2012) procedure. In Columns (2)-(5) I estimate Equation 8 for a range of bandwidths and find coefficients that do not differ statistically from Column (1).[28] Furthermore, placebo tests shown in Figure A.11 indicates that discontinuities are not present at other energy intensity cutoffs. Remarkably, detection of this discontinuity in my firm-level estimates suggests that market participants were anticipating the benefits of free allowance distribution in their valuation of Waxman-Markey effects. This result is also consistent with Bovenberg and Goulder (2001) who modeled that firm profit neutrality can

[28]The standard errors presented in Table 7 do not explicitly use the uncertainty associated with my estimated Waxman-Markey effects. Regressions using a parametric bootstrap procedure drawing from the estimated variance-covariance matrix of Waxman-Markey effects were also conducted. Resulting standard errors are nearly identical to those shown in Table 7.

be preserved in a cap-and-trade system by freely allocating only 15% of historical emissions for the oil and gas industry and only 4.3% for the coal industry.

Other dimensions of heterogeneity

I now consider other heterogeneous effects that are anticipated for climate policies in general. To motivate my analyses, consider the following static profit function at the optimal emissions level e_i^*:

$$v_i^* = pq_i(e_i^*) - C_i(q_i(e_i^*), w) + \tau(A_i^f - e_i^*) \tag{9}$$

where $C_i()$ is a cost function separable in output q_i and input prices w_i with $\frac{\partial C_i}{\partial q_i} > 0$ and $\frac{\partial C_i}{\partial w_i} > 0$. The marginal impact of cap-and-trade on profits can be obtained via the envelope theorem:

$$\frac{dv_i^*}{d\tau} = -\underbrace{\frac{\partial C_i}{\partial w}\frac{dw}{d\tau}}_{\text{input costs}} + \underbrace{q_i(e_i^*)\frac{dp}{d\tau}}_{\text{pass-through}} + \underbrace{A_i^f}_{\text{allowances}} - e_i^* \tag{10}$$

Cap-and-trade affects profit through changes in input and output prices. The first term, which is negative, suggests that firms with carbon and energy intensive inputs would exhibit greater losses. The second term summarizes the pass-through effect. Firms that can pass-through a greater portion of regulatory costs onto output markets should experience lower losses. High pass-through is captured directly by a low elasticity of demand or indirectly by low rates of import penetration. The third term captures the positive effect from the distribution of free allowances already explored above. Equation 10, however, does not capture the regulatory exposure of a firm with both domestic and international revenue. Intuitively, all else equal, firms with a greater share of exposure to U.S. output markets would experience greater losses than firms with higher international market exposure.

I first explore aggregate sectoral heterogeneity before examining whether specific patterns conform to the predictions in Equation 10. Table A.5 displays the equally-weighted and value-weighted Waxman-Markey effect for each 2-digit NAICS sector. As expected, large negative, though not statistically significant effects, are observed for the mining, utilities, and construction sectors. Statistically significant and large negative effects are experienced by the information, finance, real estate, management, waste remediation, and accommodation sectors. However, the average effect for the manufacturing sector is small possibly due to the allocation of free permits already discussed.

Carbon and energy intensive sectors have cap-and-trade sensitive input costs and should experience higher relative losses. Unfortunately, carbon intensity cannot be easily compared across 2-digit NAICS sectors.[29] For more valid comparisons, I examine 3-digit NAICS manufacturing sub-sectors for which I observe both average carbon and energy intensity. Figure 5 plots coefficients estimated separately for each 3-digit NAICS manufacturing sector against average carbon intensity, defined as mton of CO_2 per billion output, obtained from the U.S. Department of Commerce for 2006. A clear negative relationship is shown. A similar relationship is shown in Figure A.10 using average energy intensity, which is defined as cost of energy inputs over value of total output and is provided by the NBER-CES Manufacturing Industry Database for 2005. Table 8 supports this evidence showing analogous firm-level regression results. Coefficients are unaffected by the inclusion of 2-digit fixed effects in Columns (2) and (4). Interpreting these coefficients, a one unit increase in CO_2 per billion output increases the estimated Waxman-Markey effect by 3%. Similarly, a one percentage increase in energy input share increases the estimated Waxman-Markey effect by about 30%. It is likely that this relationship has been muted by distribution of free allowances for some energy intensive sectors as already discussed.

Equation 10 indicates that cap-and-trade effects are lower for firms that pass-through a greater share of regulatory costs onto output markets. One proxy for pass-through is the import share for a firm's output market. All things equal, higher import shares imply lower pass-through rates as households can more readily substitute regulated domestic goods with unregulated international goods. In Table A.6, I estimate the average Waxman-Markey effect separately for firms with different 4-digit NAICS import shares. All estimated firm effects are first demeaned at the 3-digit NAICS level. While the standard errors are large given the reduced sample size, point estimates in Table A.6 for each 10% import share bin display a generally negative relationship. That is, sectors with higher import shares experience greater losses from Waxman-Markey incidence.

A U.S. climate policy should have different effects for firms operating primarily in the U.S. than firms with more internationally oriented portfolios. The absence of equally stringent climate regulation in other major emitting countries, cap-and-trade regulation in the U.S. will have either zero or even slightly positive effects for firms with greater non-U.S. market exposure in the presence of regulatory leakage. Table A.7 estimates the average effects for firms within different bins of average U.S. revenue share in 2009-2010 after removing 3-digit NAICS means.[30] Columns (1)-(4) show estimates for bins widths of 0.25. Column (5) includes firms with only US-based revenue. Point estimates generally decrease with increased U.S. market exposure though coefficients are not significant due to the reduced sample size.

[29] In most standard carbon accounting frameworks, emissions associated with the utilities sector are considered indirect emissions and thus not comparable to direct emissions generated by other sectors.

[30] I use firm-level geographic business segment data which subdivide firms into country and region-specific segments allowing construction of a U.S. market exposure index.

Interestingly, firms with low U.S. product market exposure in Column (1) yield small positive effects hinting at potential gains due to international leakage of a U.S. cap-and-trade policy.

Differential effects on EU-ETS futures

My final validity check aims to detect the effects of prediction market prices on international carbon allowance prices. Waxman-Markey permitted limited external trading with the EU-ETS at the start of the policy in 2012. Because the EU has both a more stringent abatement schedule than Waxman-Markey and greater price distortions due to conflicting energy policies, it was believed that the EU-ETS would be a net buyer of U.S. allowances starting in 2012 if Waxman-Markey was implemented (EPA, 2009). Thus, one may expect U.S. cap-and-trade prediction market prices to have a negative effect on EU-ETS futures delivered on and after 2012 and not on 2011 futures.[31]

In Table 9, I test whether changes in the prediction market price has differential effects on EU-ETS futures with different vintages. Specifically, I regress the percent change in the spread between EU-ETS spot and futures on the change in prediction market price for futures to be delivered at the end of 2011, 2012, 2013, and 2014. Following the reasoning in Section 3.4, I only use the subsample of major event days for which unbiased estimation is more plausible. I do not find that changes in prediction market price has an effect on 2011 EU-ETS futures relative to the spot price in Column (1). In Column (2), I find that changes in prediction market price has the anticipated negative effect on 2012 EU-ETS futures relative to the spot price. While this coefficient itself is not statistically significant due to the small sample size, a seemingly unrelated regression comparison across models shows that the coefficient in Column (2) is statistically different from that of Column (1). The coefficient for EU-ETS futures for 2013 and 2014 are similar to that for 2012 futures. This differential response to prediction market prices between pre-2012 and post-2012 EU-ETS futures provides further evidence that the Intrade prediction market had relevant informational content.

5 Unlisted firms: bounding analysis

It is unlikely that cap-and-trade would only affect publicly listed firms. Cap-and-trade should alter the profitability of firms regardless of ownership structure. The challenge, however, is that the market value of unlisted firms is typically not observed.

[31] In practice, the EU-ETS allows borrowing of future allowances to meet current compliance obligations implying that any post-2012 supply shocks should also affect pre-2012 futures. However, the amount of borrowing permitted is limited across periods.

My particular solution employs the Congressional lobbying expenditures already detailed in Section 4.4 which revealed the Waxman-Markey lobbying expenses of 225 unlisted firms. Having already estimated the lobbying influence function for listed firms, my procedure amounts to projecting that relationship onto unlisted firms for whom I observe lobbying expenditure but not cap-and-trade effects. However, whereas for listed firms I can separate between policy winners and losers, I am unable to do so for unlisted firms. This implies that lobbying expenditure may reveal the magnitude of cap-and-trade costs borne by unlisted firms but it does not indicate the effect sign. Without knowing the distribution of positive or negative effects borne by unlisted firms, the conservative approach is to assign costs to be either positive or negative for all unlisted firms.

5.1 Partial identification framework

I formalize this exercise by adopting the partial identification framework with non-random missing outcomes introduced by Manski (2003). Continuing with prior notation, I describe L listed and U unlisted firms, with $N = L + U$, by the random variables $(\Delta v, Z, X)$, where Δv is the cost of Waxman-Markey, Z is a binary variable equaling unity if a firm is listed and X is a scalar denoting lobbying expenditures on Waxman-Markey in a space $\Omega \subset \mathbb{R}^{\geq 0}$. Δv is only observable when $Z = 1$. The total cost of the policy is:

$$N \cdot E[\Delta v] = E[\Delta v|Z = 1] \cdot L + E[\Delta v|Z = 0] \cdot U \tag{11}$$

$E[\Delta v|Z = 0]$ is not observed. Importantly, in this context and others, it would be unreasonable to assume that $E[\Delta v|Z = 0] = E[\Delta v|Z = 1]$. That is, the distribution of cap-and-trade costs differs for listed and unlisted firms. One could bound $E[\Delta v|Z = 0]$ using the empirically observed lower bound, $\underline{\Delta v} = \min_{Z=1} \Delta v$, and upper bound, $\overline{\Delta v} = \max_{Z=1} \Delta v$, for listed firms such that $\underline{\Delta v} \leq E[\Delta v|z = 0] \leq \overline{\Delta v}$. However, as Lee (2009) has argued, in applications where the range of observed costs are large, this "worst-case" scenario procedure generates bounds that may be too wide to be informative. In my context, it would be unreasonable to assign unlisted firms with bounds equalling the lowest and highest cost estimated for a listed firm.

I make two assumptions in order to perform the bounding analysis. First, I assume that unlisted firms that do not lobby are on average unaffected by Waxman-Markey. Second, the absolute cost of Waxman-Markey for an listed firm weakly bounds that of a unlisted firm conditional on positive lobbying expenditure. The first assumption can be written:

Assumption *3* *Revealed preference:* $E[|\Delta v|\,|Z = 0, X = 0] = 0$

In words, Assumption 3 states that unlisted firms that did not lobby on average will not experience cap-and-trade costs. While this assumption might appear strong, it is fairly innocuous as the bounds I estimate for unlisted firms that lobbied are relatively wide given the overall value of unlisted firms in the U.S. economy. My second assumption states:

Assumption 4 *Bounding:* $E[|\Delta v|\,|Z=0, X=x, X>0] \leq E[|\Delta v|\,|Z=1, X=x, X>0]\ \forall\ x \in \Omega$

Assumption 4 states that conditional on positive lobbying expenditures, the absolute costs of Waxman-Markey for a listed firm weakly bounds the costs absolute costs borne by an unlisted firm. Because both assumptions are based on the costs borne by unlisted firms which is unobserved, they are fundamentally non-refutable. For Assumption 3, concerns about free-riding in the lobbying market are partly assuage by the fact that one quarter of firms lobbying on Waxman-Markey spend between \$6,000 - \$125,000 suggesting that the lobbying cost of entry is fairly low. In Figure A.12 and Table A.8, I provide suggestive evidence that Assumption 4 is reasonably valid. Figure A.12 shows that lobbyists hired exclusively by listed firms to lobby on Waxman-Markey have higher average total lobbying revenue (across all lobbying activity) than lobbyists hired exclusively by unlisted firms. Table A.8 shows that this is largely true even conditional on the sector of the hiring firm.[32] I can now rewrite the second term in Equation 11:

$$\begin{aligned}
E[\Delta v|Z=0]\cdot U \leq\ & E[|\Delta v|\,|Z=0]\cdot U \\
=\ & \sum_{x\in\Omega} E[|\Delta v|\,|Z=0, X=x]\cdot U_x \\
=\ & \sum_{x\in\Omega, x>0} E[|\Delta v|\,|Z=0, X=x, X>0]\cdot U_x \\
\leq\ & \sum_{x\in\Omega, x>0} E[|\Delta v|\,|Z=1, X=x, X>0]\cdot U_x
\end{aligned} \tag{12}$$

where U_x is the number of unlisted firms spending x amount on lobbying. The first line applies the property of the absolute value, the second line uses the law of total probability, the third line employs Assumption 3, and the final line uses Assumption 4. The expression above illustrates why Assumption 3 is needed. While the overall value of unlisted firms in the U.S. economy is only 9%, they make up 97% of all incorporated firms according to the Bureau Van Dijk Orbis database. This implies a large value for U_0 and thus large uninformative bounds in the absence of Assumption 3. Observe that implicit in Assumption 4 is the notion that absolute Waxman-Markey costs can be predicted by lobbying expenditures. This has already been empirically shown in the lobbying influence function presented in Table 6 and Figure 3. Applying the property of the absolute value, I can now recover an identification region for the total cost of cap-and-trade:

[32] Unfortunately, because I only observe total lobbying revenue and not lobbying wages, I cannot infer units of lobbying effort purchased by each firm.

$$
\begin{aligned}
H\{N\cdot E[\Delta v]\} =&[E[\Delta v|Z=1]\cdot L-\sum_{x\in\Omega,x>0}E[|\Delta v|\,|Z=1,X=x,X>0]\cdot U_x,\\
&E[\Delta v|Z=1]\cdot L+\sum_{x\in\Omega,x>0}E[|\Delta v|\,|Z=1,X=x,X>0]\cdot U_x]\\
=&[\sum_{i=1}^{L}\widehat{\gamma}_i\widehat{v}_i^o-\sum_{u=1}^{U}|\widehat{\Delta v_u}|,\ \sum_{i=1}^{L}\widehat{\gamma}_i\widehat{v}_i^o+\sum_{u=1}^{U}|\widehat{\Delta v_u}|]
\end{aligned}
\tag{13}
$$

To summarize, the bounding analysis is performed in three steps. First, I estimate the relationship between absolute cap-and-trade costs and lobbying for listed firms that appear in the lobbying records using Equation 7. In a second step, I predict out-of-sample absolute cap-and-trade costs for unlisted firms lobbying on Waxman-Markey. Finally, I assign predicted costs to be either negative or positive for all unlisted firms. As an illustration of these generated bounds, Figures A.13 and A.14 plot the distribution of firm-level costs estimated for all listed firms in the lobbying record (red) along with the predicted negative and positive costs for matched unlisted firms (gray).

5.2 Aggregate costs

Panel (a) of Table 10 displays the total change in firm value according to the various benchmark models shown in Panel (b) of Table 2. Averaging across the five benchmark models, Waxman-Markey is expected to lower the value of listed firms by \$150 billion. Using Equation 13 to bound costs for unlisted firms, total losses due to Waxman-Markey have a lower bound of \$70 billion and an upper bound of \$240 billion. The large width of these bounds is due to the difficulty of determining the sign of predicted costs for unlisted firms. They are also sufficiently wide such that Assumption 3 seems reasonable. My conservative upper bound estimate attributes 35% of the upper bound total Waxman-Markey cost to unlisted firms that lobbied. This value is large relative to the 9% share of annual U.S. corporate profits attributed to unlisted firms and suggests that my bounds may be wide enough to include all unlisted firms and not just those that lobbied.

To conduct statistical inference on the lower and upper bounds of the identification region, I follow the principle developed by Imbens and Manski (2004) for a confidence interval that asymptotically covers the true parameter with fixed probability. This is implemented using a parametric bootstrap procedure which draws from the estimated listed firm effects and associated variance-covariance matrix (see Appendix D for further details). Figure A.15 plots the two layers of uncertainty associated with the estimates shown in Table 10. For each model, I plot the estimated loss for all listed firms along with a 90% confidence interval in thick black lines corresponding to Panel (b) of Table 2. The interval shown by thin brown lines indicates the identification region for the effect on all listed and unlisted firms with dashed gray lines showing the 90% confidence interval for the lower and upper bounds of the identification region.

6 Comparing with CGE models

Unfortunately, direct benchmarks for my estimates are not available because Waxman-Markey was never implemented. To date, multi-sector computational general equilibrium (CGE) models are the prevailing technique for evaluating the cost of cap-and-trade policy (see Appendix G for a summary) and thus serve as a potential benchmark for my estimates. Such comparisons, however, require a degree of caution. In particular, CGE estimates may differ from this paper for reasons relating to the structural assumptions of CGE models as well as the scope of their analyses.

CGE forecasts are based on structural representations of the economy with parameters that capture, among other features, expected future prices, demand elasticities, and technological change. Parameters assumed for CGE models may differ from market expectations. In particular, while demand elasticities may be well-approximated using available empirical evidence, expectations over prospects of low-carbon technologies may diverge widely if there is disperse information regarding the technological frontier.

The scope of analysis also differs across these two methods. CGE models typically analyze the total costs of a specific, stand-alone cap-and-trade policy at the domestic sectoral level. This differs from my approach which excludes the household sector but includes the entire suite of policies under Waxman-Markey in addition to the cap-and-trade component. Because I use firms as my unit of analysis, I also cannot exclude non-U.S. effects on firms with international operations nor can I capture the dynamics of future firm entry within a sector.[33] Second, while my analysis focuses on the cost to firms of climate policy, I am unable to exclude the possibility that markets also anticipated benefits from climate policy that may result from implementation of the Waxman-Markey bill. Expecting markets to anticipate benefits from climate policy, however, requires strong assumptions on investor foresight. In particular, while the U.S. is one of the world's largest emitters, reductions from the U.S. alone is unlikely to have a significant impact on global atmospheric greenhouse gas concentrations. Market participants responding to benefits from the policy must therefore forecast not just U.S. emissions reductions but also the likelihood that U.S policy triggers

[33] Ryan (2011) shows that the latter is particularly relevant for estimating the cost of the 1990 Clean Air Act Amendments on the US cement industry.

carbon mitigation elsewhere.

Finally, insofar as market participants expected political activity to alter final policy details before implementation, my estimates may correspond to a slightly different policy than that examined by CGE models which do not endogenize political dynamics. This final point should draw the most pause when considering any comparison exercise. The estimates from this paper correspond to the cost of a policy that markets expected to be implemented. While the validity checks in Section 4.4 gives confidence that this policy resembles the Waxman-Markey bill, I am unable to confirm that markets expected the eventual policy to follow Waxman-Markey line for line.

For these reasons, the following comparison exercise should be interpreted with caution. In Table 10 Panel (b), I display comparable estimates from the IGEM and EPPA models, the two most prominent academic CGE climate policy models. The statistic provided is the CGE forecasted change in net-present discounted capital income which is the closest proxy for firm profits within the CGE environment.[34] Unfortunately, capital income reflects accounting and not economic profits.[35] To produce a more valid comparison across the two methods, I consider a scenario whereby capital investments within the CGE environment are adjusted for the market cost of capital.[36]

Using this measure for comparison, my lower and upper bound estimates are 16% - 54% of the average CGE estimate for the IGEM and EPPA models as shown in Figure 6. Though it is difficult to isolate which of the reasons noted above may explain the difference in estimates between these two methods, it is illustrative to explore why actual costs of environmental policies may have diverged from ex-ante structural forecasts in the past. In the case of the Montreal Protocol, overestimates were attributed to a failure in forecasting the development of CFC substitutes (Cook, 1996). For the U.S. SO_2 cap-and-trade system, models did not foresee the lowering of transport costs for low-sulfur coal following railroad deregulation (Carlson et al., 2000; Ellerman et al., 2000). The blue bars in Figure 6 indicate the ratio of actual costs to ex-ante structural forecasts for the Montreal Protocol, U.S. SO_2 cap-and-trade system, and the E.U. Emissions Trading System (EU-ETS). An evaluation of structural forecasts for these past major environmental policies suggests that actual costs were between 30% - 75% of ex-ante forecasts.[37] In this light, CGE modeling choices regarding future input

[34] I am grateful to Larry Goulder for this suggestion.

[35] Constant returns to scale and perfect competition in most CGE models implies a zero-profit condition.

[36] Discounting within a CGE model is conducted using the risk-free interest rate. The discount rate for stock prices, on the other hand, is the sum of the risk-free interest rate and a risk premium associated with holding the risky asset. IGEM uses an endogenous risk-free interest rate of 2.63%. EPPA has an exogenous risk-free interest rate of 4%. I increase the discount rate used for net present value calculations for CGE outputs to the sum of the risk-free interest rate inherent in each model and 3.3% equity risk premium obtained from Robert Shiller's data (data available: `http://www.econ.yale.edu/~shiller/data.htm`).

[37] Not all prior structural forecasts of environmental regulations were performed using CGE models. See

prices and technological change may explain some of the difference between Waxman-Markey estimates from this paper and CGE models. Future research aims to better understand which of these features are driving the divergence in estimates produced by these two methods.

7 Conclusion

This paper develops a novel method for forecasting the cost to firms of proposed climate policy. Through an event study using prediction markets, I estimate the expected cost to firms of the Waxman-Markey cap-and-trade bill, had it been implemented. Validity checks confirm that markets responded to features that are both particular to the Waxman-Markey bill and general to climate policy incidence. Lobbying records are used to estimate a political influence function for listed firms and to bound costs for unlisted firms.

To the best of my knowledge, this paper provides the first forecast of the cost of climate policy to firms outside CGE modeling efforts. Results from my heterogeneity analyses could inform the design of redistributive schemes needed to secure political support for future climate policy proposals. More generally, the method developed in this paper can serve as a framework for using prediction markets as a forecasting tool for other polices.

Estimates from this paper and CGE models may not be directly comparable. The main advantage of this method is that it exploits the diffuse information revealed by market participants and firm behavior. However, while this method recovers the expected cost of the implemented policy, I am unable to confirm that this policy corresponds exactly to the Waxman-Markey bill, which limits my method from informing debates on alternative policy options. CGE models, on the other hand, structurally evaluate cap-and-trade policies for a known policy and can conduct counterfactual policy evaluations. It is likely therefore that these two methods will serve complementary roles during future climate policy debates.

Appendix G for more details on prior forecasts.

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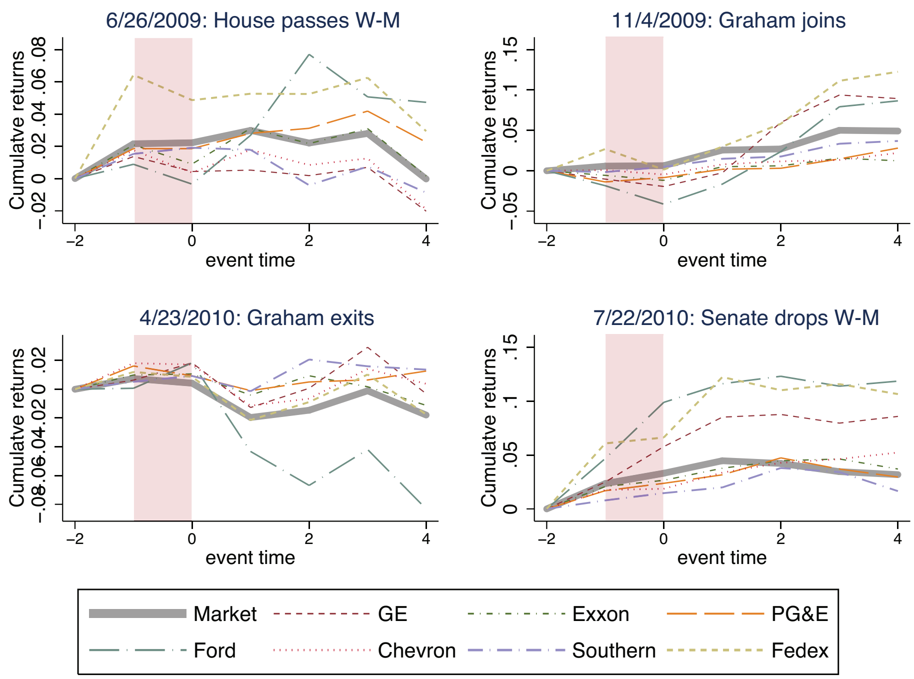
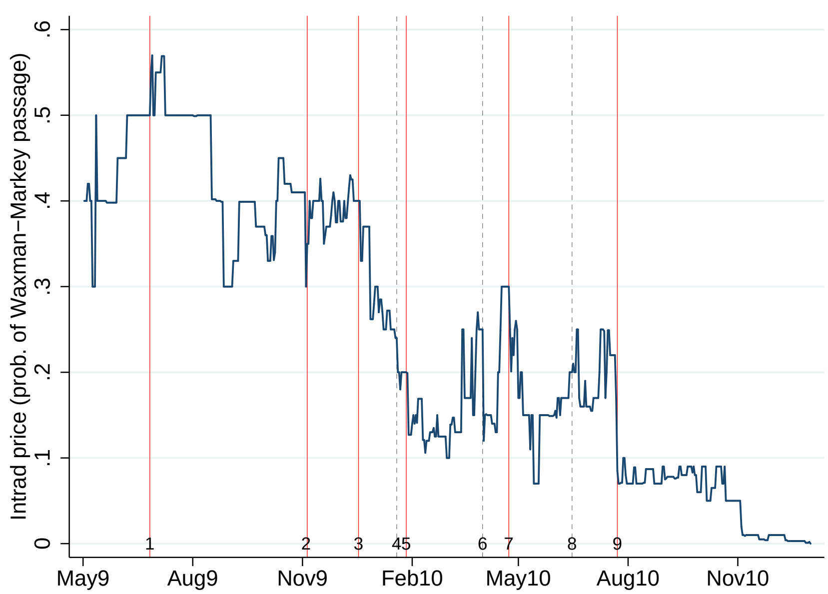
Figures

Figure 1: Stock returns for prominent firms during major events



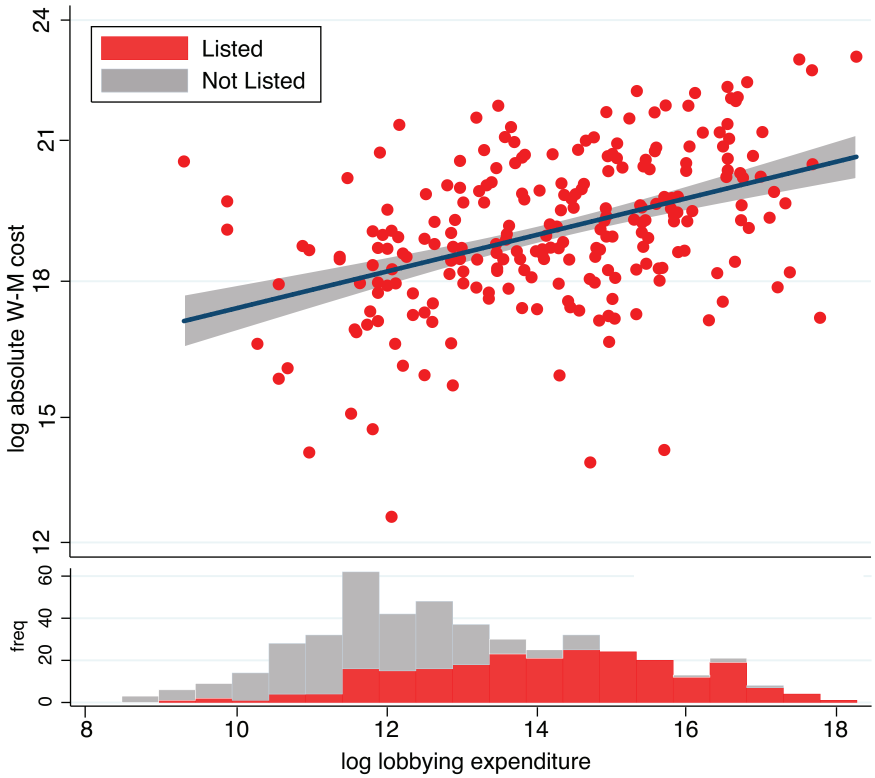
Notes: Each plot shows cumulative returns before and after a major event for the aggregate value-weighted market index and stock returns of firms with the highest Waxman-Markey lobbying spending.

Figure 2: Cap-and-trade Intrade market prices



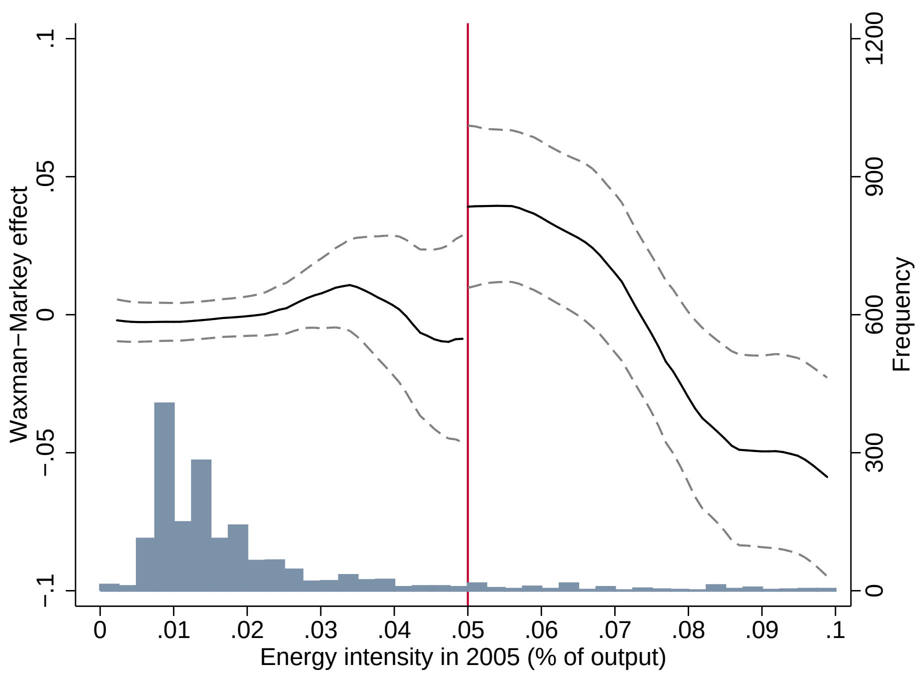
Notes: Red solid (gray dashed) lines mark days with events directly (indirectly) related to cap-and-trade prospects. (1) 6/26/2009: House passes Waxman-Markey. (2) 11/4/2009: Graham joins Senate effort. (3) 12/20/2009: Copenhagen negotiations concluded. (4) 1/19/2010: Scott Brown wins Mass. Senate seat. (5) 1/27/2010: Graham-Kerry-Lieberman seeks non cap-and-trade alternatives. (6) 3/31/2010: Obama supports offshore drilling. (7) 4/23/2010: Graham drops support. (8) 6/15/2010: Obama oval office speech. (9) 7/22/2010: Senate drops cap-and-trade legislation. See Appendix F for further detail.

Figure 3: Firm-level cost of cap-and-trade versus lobbying expenditure



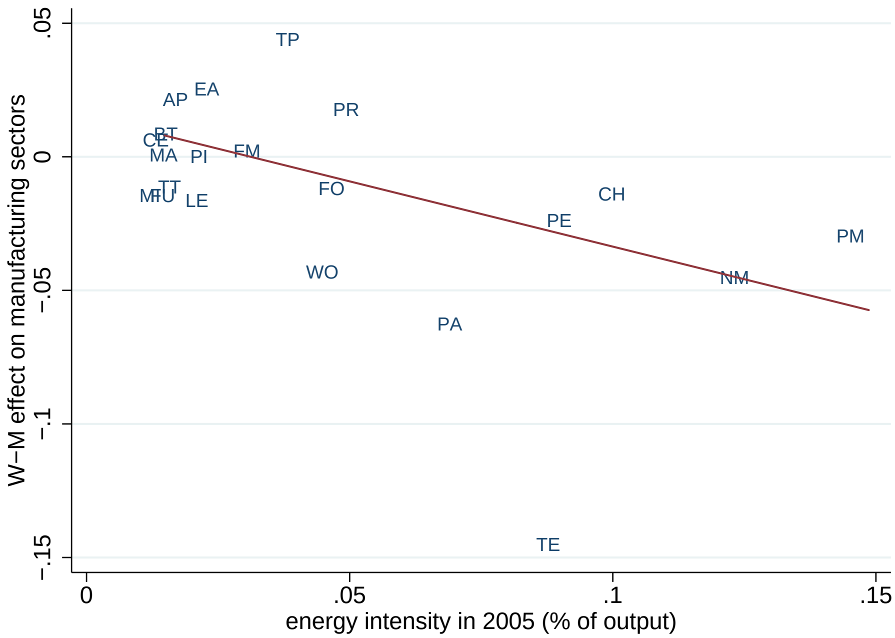
Notes: Log absolute cost of cap-and-trade estimated from Equation 6 against log lobbying expenditure for listed firms that have lobbied on W-M. Linear model with 90% CI shown in gray. Stacked histogram showing total spending on Waxman-Markey lobbying by listed and unlisted firms.

Figure 4: Regression discontinuity of Waxman-Markey effects at 5% energy intensity



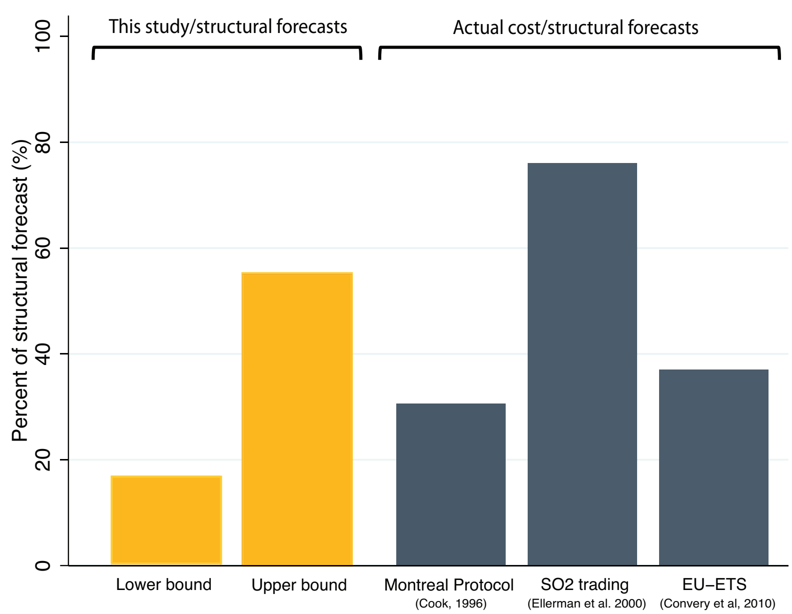
Notes: Discontinuity of estimated Waxman-Markey effects at 5% energy intensity with 3-digit NAICS average removed. Local polynomial regression using an Epanechikov kernel with optimal bandwidth (Fan and Gijbels, 1996). 90% confidence intervals shown.

Figure 5: Manufacturing subsector effects vs. carbon intensity



Notes: Average W-M effects for firms within a 3-digit NAICS manufacturing sector plotted against carbon intensity (mton CO_2/billion output) in 2006. AP: apparel. BT: beverage & tobacco products. CE: computer & electronic products. CH: chemicals. EA: electrical appliances. FM: fabricated metal products. FO: food. FU: furniture. LE: leather. MA: machinery. MI: misc. NM: non-metallic mineral. PA: paper. PE: petroleum & coal products. PI: printing. PM: primary metals. PR: plastic & rubber products. TE: textile mills. TP: textile mill products. TT: transportation equip. WO: wood products.

Figure 6: Comparing with past structural forecasts



Notes: Orange bars show percentage of total cost estimates relative to the change in average risk-adjusted discounted capital income for the IGEM and EPPA CGE models. IGEM (EPPA) uses an endogenous (exogenous) risk-free interest rate of 2.63% (4%). Risk-adjusted NPV capital income obtained by adding market risk premium of 3.3% to existing risk-free interest rates. Blue bars show percentage of actual costs relative to structural forecasts for the Montreal Protocol, SO_2 cap-and-trade program, and the EU-ETS.

Tables

Table 1: Cap-and-trade related activity for full sample and major event days

	Full sample	Major events
Number of obs	111	5
2-day interval		
Absolute change in Intrade price ($\lvert\Delta\theta\rvert$)	0.024	0.078
	[.032]	[.033]
Intrade volume	30	69
	[89]	[100]
Weekly interval		
Google News Headlines:	0.031	0.094
"cap and trade" (% change)	[0.70]	[0.51]
Google search volume:	-0.0016	0.011
"cap and trade" (% change)	[0.12]	[0.0073]

Standard deviation in brackets.

Table 2: Prediction market event study: main result

Model	Controls	Days	equal-wt. avg. eff. $\frac{1}{L}\sum_\ell \hat{\gamma}_\ell$	value-wt avg. eff. $\sum_\ell \frac{v^o_\ell}{\sum_\ell v^o_\ell}\hat{\gamma}_\ell$	total cost $\sum_\ell v^o_\ell \hat{\gamma}_\ell$
	Panel (a): Dep. var. is 2-day value-weighted market returns				
(1) Aggregate		5		-0.011	-163.77
				[0.18]	[2741.73]
	Panel (b): Dep. var. is 2-day firm-level returns				
(2) Firm-level	CAPM	111	-0.020*	-0.0080***	-146.74***
			[0.011]	[0.0024]	[43.40]
(3) Firm-level	3-factor FF	111	-0.014**	-0.0066***	-120.54***
			[0.0067]	[0.0022]	[40.42]
(4) Firm-level	< 0.05 CI	111	-0.020*	-0.0086**	-157.12**
			[0.012]	[0.0041]	[75.30]
(5) Firm-level	< 0.10 CI	111	-0.020*	-0.0085**	-154.47**
			[0.012]	[0.0043]	[77.90]
(6) Firm-level	< 0.15 CI	111	-0.022*	-0.010**	-190.99**
			[0.012]	[0.0046]	[84.21]

Panel (a) from bivariate regression of aggregate value-weighted market returns on change in prediction market price for 5 major event days (see Eq. 5). Panel (b) from firm-level SUR regressions of 5,342 firm-level returns on change in prediction market price with CAPM, 3-factor Fama-French model, and value-weighted returns of low carbon intensive firms ($<$ 0.05, 0.10 and 0.15 mton CO_2/billion output) as benchmark control (see Eq. 6) . Only days with $\theta_t \in [.2, .8]$. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Prediction market event study: thin trading and price manipulation

	Dep var is 2-day stock return					
	(1)	(2)	(3)	(4)	(5)	(6)
	main	high volume	volume interaction	without big trader	high concentration	HHI interaction
$\Delta\theta_t$	-0.014**	-0.018*	-0.011	-0.022**	-0.017	-0.027*
	[0.0067]	[0.0097]	[0.0082]	[0.011]	[0.027]	[0.016]
$\Delta\theta_t$ x volume			-0.000072			
			[0.00014]			
$\Delta\theta_t$ x HHI						0.019
						[0.021]
Number of days	111	21	111	37	9	111

Equally weighted average effect shown for 5,342 firms. Only days with $\theta_t \in [.2, .8]$. Column (1) replicates 3-factor Fama-French result. Column (2) includes only high volume trading days (> sample mean volume of 30 trades). Column (3) adds an interaction of prediction market price with trading volume. Column (4) includes only trading days without top 2 influential traders. Column (5) includes only days with HHI<0.25. Column (6) adds an interaction of prediction market price with daily HHI index. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Prediction market event study: additional controls

	Dep var is 2-day stock return				
	(1)	(2)	(3)	(4)	(5)
	main	trend	oil price	Google News	Welsh-Goyal
$\Delta\theta_t$	-0.014**	-0.013*	-0.015**	-0.014**	-0.017**
	[0.0067]	[0.0067]	[0.0066]	[0.0069]	[0.0072]
Number of days	111	111	111	111	111

Equally weighted average effect shown for 5,342 firms. Only days with $\theta_t \in [.2, .8]$. Column (1) replicates 3-factor Fama-French result. Column (2) includes a linear trend. Column (3) includes change in crude oil price. Column (4) includes Google News volume for "climate change", "carbon tax", "energy policy", and "nuclear policy". Column (5) includes monthly Welsh-Goyal controls. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Top 40 firms lobbying on Waxman-Markey by expenditure

	Lobby expenses ($)	Listed
GEN ELECTRIC	89,650,000	1
PG&E	55,140,000	1
FEDEX	50,037,074	1
EXXON MOBIL	49,580,000	1
CHEVRON	41,729,000	1
SOUTHERN	36,940,000	1
GEN MOTORS	36,351,000	1
FORD MOTOR DEL	34,769,000	1
KOCH IND	34,613,000	0
BOEING	31,286,000	1
MARATHON OIL	29,830,000	1
AMERICAN ELECTRIC POWER	28,152,466	1
BP	25,560,000	1
UNITED TECH	24,963,415	1
NORFOLK SOUTHERN	22,545,177	1
PEABODY ENERGY	21,266,000	1
JP MORGAN CHASE	20,800,000	1
LOCKHEED MARTIN	19,710,000	1
ROYAL DUTCH SHELL	19,390,582	1
UNITED PARCEL SERVICE	19,220,828	1
DUKE ENERGY	18,987,464	1
CONOCOPHILLIPS	18,372,210	1
WAL MART STORES	17,890,000	1
TOYOTA MOTOR	17,729,578	1
MONSANTO	16,800,000	1
ALTRIA	16,390,000	1
DELTA AIR LINES	16,105,879	1
UNION PACIFIC	16,039,854	1
JOHNSON & JOHNSON	16,015,000	1
DOW CHEM	16,007,000	1
DU PONT EI DE NEMOURS	15,793,514	1
EXELON	15,106,248	1
BERKSHIRE HATHAWAY	15,027,438	1
HEWLETT PACKARD	15,015,720	1
PRUDENTIAL FINANCIAL	14,430,000	1
ENERGY FUTURE HLDGS	12,591,447	0
HONEYWELL INT	12,492,000	1
CSX	11,512,078	1
PROCTER & GAMBLE	10,375,530	1
PUBLIC SERVICE ENTERPRISE	10,010,000	1

Table 6: Estimates of the lobbying influence function

Dep var is log abs. cap-and-trade cost

	(1) Winners	(2) Losers	(3) All
log W-M lobbying expense	0.36**	0.43***	0.44***
	[0.17]	[0.11]	[0.088]
Number of firms	117	117	234

Includes 3-digit NAICS fixed effects and standard error clustering. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Prediction market event study: regression discontinuity at 5% energy intensity

	(1)	(2)	(3)	(4)	(5)
Discontinuity at 5% energy intensity	0.058*	0.064	0.078**	0.071**	0.057*
	[0.030]	[0.045]	[0.038]	[0.032]	[0.029]
Bandwidth	0.044 ±	0.02	0.03	0.04	0.05
Number of firms	1,647	203	411	1,122	1,678

Regression discontinuity of estimated firm-level Waxman-Markey effects at 5% energy intensity in 2005. Local linear model with triangular kernel and 3-digit NAICS sector fixed effects. ± in Column (1) indicates bandwidth using Imbens and Kalyanaraman (2012) optimal bandwidth selection procedure. Columns (2)-(5) show discontinuity at different bandwidths. 13σ outlier firm dropped (PERMNO=88729). Standard errors clustered at 6-digit NAICS level. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Prediction market event study: carbon intensity and energy input share

Dep var is 3-digit manufacturing coefficient

	(1)	(2)	(3)	(4)
carbon intensity	-0.0364***	-0.033***		
	[0.0112]	[0.0096]		
energy input share			-0.31***	-0.24*
			[0.11]	[0.12]
Number of firms	1,663	1,663	1,663	1,663
2-digit NAICS fixed effect	NO	YES	NO	YES

Regressions of estimated firm-level cap-and-trade effects on 2005 sectoral carbon intensity (CO_2/billion output) and energy input share (% of output) at the 3-digit NAICS manufacturing level. Only includes manufacturing firms. Columns (2) and (4) include 2-digit NAICS2 fixed effects. Standard errors clustered at 4-digit NAICS level in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 9: EU-ETS futures response to Intrade price

	Dep. var is EU-ETS spot-futures spread			
	(1) 2011 Futures	(2) 2012 Futures	(3) 2013 Futures	(4) 2014 Futures
$\Delta\theta_t$	0.17	-0.27	-0.25	-0.21
	[0.26]	[0.14]	[0.13]	[0.17]
Number of days	5	5	5	5
Testing difference from Col 1 (p-value)		0.0271	0.1075	0.1804

Bivariate regressions of percent change in EU-ETS allowance spot-futures price spread change in Intrade price. Column (1)-(4) uses the spread between spot and EU-ETS allowance futures to be delivered at the end of 2011, 2012, 2013, and 2014 respectiively. P-values from a SUR procedure for the difference between coefficients in Columns (2)-(4) relative to Column (1) shown. Heteroscedasticity-robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Estimated change in profits for listed and unlisted firms

Panel (a) Event study aggregate cost estimates

	(1) CAPM	(2) 3FF	(3) <0.05 CI	(4) <0.10 CI	(5) <0.15 CI	(6) Avg.
Listed firms	-146.74	-120.54	-157.12	-154.47	-190.99	-153.97
Unlisted firms (absolute cost)	82.04	78.52	86.21	87.87	81.87	83.30
Listed & unlisted firms (lower bnd)	-64.7	-42.02	-70.91	-66.59	-109.12	-70.67
Listed & unlisted firms (upper bnd)	-228.78	-199.06	-243.33	-242.35	-272.86	-237.28

Panel (b) CGE model estimates

CGE model	
MIT EPPA	-410
Harvard IGEM	-460

All values in billion 2009 dollars. Listed firm estimates based on Panel (b) of Table 2. Each column in Panel (a) uses a different benchmark model. Unlisted firm bounds based on estimated relationship shown in Table 6. For CGE models in Panel (b), change in risk-adjusted NPV capital income is shown obtained from private communication with IGEM and EPPA modeling teams. See Section 6 on construction of risk-adjusted NPV capital income.

Appendix to *Forecasting the Cost to Firms of Climate Policy using Prediction Markets and Lobbying Records*

FOR ONLINE PUBLICATION

Appendix A Theoretical framework for cap-and-trade

This section presents a theoretical framework which maps market values onto the marginal abatement cost under a cap-and-trade system. Following the modeling framework of Montgomery (1972) and Rubin (1996), I first explore the joint cost minimization problem for firms and households which solves for an aggregate marginal abatement cost. In practice, however, the regulator can never implement the joint cost problem, but can instead set up a cap-and-trade system. To that end, I analyze the cost minimization problem under emissions trading in which the equilibrium allowance price equals the aggregate marginal abatement cost. An extension of this equivalence result yields an expression approximating the aggregate marginal abatement cost for the policy. Because I do not observe the impact of the policy on households, I can only recover the portion of the aggregate marginal abatement cost attributed to firms. Throughout, I use a general objective function to avoid specifying market structure and production technology.

Joint-cost minimization

Banking and borrowing provisions in most cap-and-trade legislations allow aggregate annual emissions caps to be ostensibly treated as a cumulative stock.[38] This insight allows one to translate the dynamic setting of a cap-and-trade policy into the canonical Hotelling model of optimal extraction for a known stock of nonrenewable resource (Hotelling, 1931). This was explored in Rubin (1996)'s dynamic model of emissions trading which extended the canonical static model of Montgomery (1972) first establishing the cost effectiveness of emissions trading. Following this framework, I explore a joint cost minimization problem in which $N-1$ firms and a representative household choose annual emissions e_{it} to optimally deplete a fixed known stock of R emissions over $t \in [0, T]$, the lifetime of the policy.[39] For simplicity, firms and the representative household are treated identically within this framework, a point I return to later.

I define a concave, twice-differentiable, general profit function with emissions e_{it}. The

[38] Waxman-Markey permits unlimited banking and limited borrowing of future allowances. Specifically, borrowing of allowances vintage 2 to 5 years into the future are subject to a 15% interest. Such constraints result in allowance price increases below the rate of interest (Rubin, 1996; Schennach, 2000). Because I am primarily interested in estimating the allowance price during the first year of the policy, for simplicity, this model sets allowance prices to follow Hotelling's rule.

[39] This setup differs from the Montgomery (1972) model along three dimensions. First, I introduce a household production sector in which the representative agent maximizes "profit" from the household production of a utility good. Second, the objective function is written in terms of firm profits and not the difference between unconstrained and constrained profits. Lastly, I deviate from Rubin (1996)'s setup by writing an equation of motion in terms of depletion rather than accumulation. These choices were made for expository simplicity but are mathematically immaterial.

optimal control problem with choice variable e_{it} and state variable s_t is written as:

$$V = \max_{e_{it}} \int_0^T e^{-\delta t} \sum_{i=1}^{N} \pi_i(e_{it}) dt$$
$$s.t. \quad \dot{s}_t = -\sum_{i=1}^{N} e_{it}$$
$$s_0 = R, \quad s_T \geq 0, \quad e_{it} \geq 0 \ \ \forall i$$

where δ is the exogenously determined rate of interest.[40] Solving the current value Hamiltonian yields the following first order conditions:

$$\pi_i'(e_{it}) = \Lambda_t \ \ \forall i \tag{A.1}$$
$$\dot{\Lambda}_t - \delta \Lambda_t = 0 \tag{A.2}$$
$$\Lambda_T s_T e^{-\delta T} = 0 \tag{A.3}$$

where Λ_t is the positive current value shadow price at year t and can be naturally interpreted as the marginal abatement cost as it corresponds to the marginal profit associated with an extra unit of allowed emissions. Equations A.2-A.3 summarizes two well-established features of the Hotelling problem. First, a simple rearrangement of Equation A.2 yields Hotelling's rule, $\Lambda_t = \Lambda_0 e^{\delta t}$, which states that the marginal abatement cost rises at the rate of interest. Second, observe that Hotelling's rule together with the transversality condition in Equation A.3 yield $\int_0^T \sum_{i=1}^N e_{it} dt = R$. That is, total emissions must equal R by the end of the policy period. Define the optimal allocation of emissions for the joint problem $E_t^{**} = (e_{1t}^{**}...e_{Nt}^{**})$. The value function at the optimum can be written as a single-valued function of the cumulative cap, such that $V(R) = \int_0^T e^{-\delta t} \sum_{i=1}^N \pi_i(e_{it}^{**}) dt$. An envelope theorem-type argument implies:[41]

$$\Lambda_0(R) = V'(R) \tag{A.4}$$

Furthermore, a concave, nondecreasing, and nonnegative value function, together with a positive shadow price, yields $\Lambda_0 \geq 0$ and $\frac{d\Lambda_0(R)}{dR} < 0$. That is, the marginal abatement cost rises as the cumulative cap under the policy tightens. Now consider a linear approximation for Λ_0 between the optimum value for a no-policy, business-as-usual scenario with cumulative emissions R^o, and the optimum value under a policy with cumulative emissions constrained at R:

[40] I assume that cap-and-trade regulation ends in 2050 as written in Waxman-Markey to avoid explicit assumptions about both business-as-usual emissions and cap-and-trade regulation beyond 2050.

[41] See (Weitzman, 2003, p. 159)

$$\Lambda_0(R) \approx \frac{V(R) - V(R^o)}{R - R^o} \tag{A.5}$$

Observe that given the concavity of $V(R)$ and since $R < R^o$, a linear approximation understates $\Lambda_0(R)$ to a degree that depends on the concavity of $V(R)$.

Cap-and-trade

In practice, however, the regulator never solves the joint cost problem, but can introduce a cap-and-trade system. Here, the regulator's role is to create R cumulative allowances such that in each period A^f_{it} is given freely to firm or household i and A^a_t is auctioned off.[42] Denote y_{it} as the number of allowances sold (>0) or purchased (<0). The firm or household's dynamic problem is to choose e_{it} and y_{it} with allowance banking:

$$\begin{aligned} v_i = \max_{y_{it}, e_{it}} \int_0^T e^{-\delta t} \left[\pi_i(e_{it}) + \tau_t y_{it}\right] dt \\ s.t. \quad \dot{s}_{it} = A^f_{it} - e_{it} - y_{it} \\ s_{i0} = 0, \quad s_{iT} \geq 0, \quad e_{it} \geq 0 \ \ \forall i \end{aligned}$$

where τ_t is the allowance price. First order conditions for the current value Hamiltonian are:

$$\pi'_i(e_{it}) = \lambda_{it} \tag{A.6}$$

$$\tau_t = -\lambda_{it} \tag{A.7}$$

$$\dot{\lambda}_{it} - \delta\lambda_{it} = 0 \tag{A.8}$$

$$\lambda_{iT} s_T e^{-\delta T} = 0 \tag{A.9}$$

where λ_{it} is the positive current value shadow price. Defining the market equilibrium as $E^*_t = (e^*_{1t}...e^*_{Nt})$, $Y^*_t = (y^*_{1t}...y^*_{Nt})$, and τ^*_t, I further impose market clearing and terminal conditions:

$$\textstyle\sum_{i=1}^N y^*_{it} + A^a_t = 0 \ \ \forall t \tag{A.10}$$

$$\tau^*_T \left[\textstyle\int_0^T \sum_{i=1}^N (A^f_{it} - e^*_{it} - y^*_{it}) dt\right] = 0 \tag{A.11}$$

Rubin (1996) shows that the market equilibrium satisfying Equations A.6 - A.11 achieves $E^{**}_t = E^*_t$ and $-\Lambda_t = \tau^*_t$. That is, the decentralized emissions trading solution yields the same efficient emissions allocation as the joint cost problem and the marginal abatement cost obtained from the joint cost problem equals the equilibrium allowance price. Now, suppose

[42] Observe that Montgomery (1972) and Rubin (1996) assume that all allowances are distributed freely, that is $A^a_t = 0$ $\forall t$. This is inconsistent with Waxman-Markey.

one could observe the aggregate difference in optimal firm and household values under a cap-and-trade policy and business-as-usual scenario, $\sum_{i=1}^{N} \Delta v_i = \sum_{i=1}^{N}(v_i(R) - v_i(R^o))$. This can be written:

$$\sum_{i=1}^{N} \Delta v_i = \int_0^T e^{-\delta t} \sum_{i=1}^{N} \left[\pi_i(e_{it}^*) + \tau_t^* y_{it}^* - \pi_i(e_{it}^o)\right] dt \tag{A.12}$$

$$= \int_0^T e^{-\delta t} \sum_{i=1}^{N} \left[\pi_i(e_{it}^{**}) - \pi_i(e_{it}^o)\right] dt - \int_0^T e^{-\delta t} \Lambda_t \sum_{i=1}^{N} y_{it}^* dt \tag{A.13}$$

$$= V(R) - V(R^o) + \Lambda_o A^a \tag{A.14}$$

where the second line uses Rubin (1996)'s equivalence result. The third line employs the definition for the current value shadow price, uses Equation A.10, sets $A^a = \int_o^T A_t^a dt$, and substitutes the optimal value from the joint cost problem. Dividing Equation A.14 by the cumulative abatement under Waxman-Markey, $R - R^o$, applying Equation A.5, and after some rearranging, yield:

$$\Lambda_o(R) \approx \frac{\sum_{i=1}^{N} \Delta v_i}{R - R^o + A^a} \tag{A.15}$$

Equation A.15 states that the marginal abatement cost can be recovered by estimating the differences in firm and household values under business-as-usual and Waxman-Markey scenarios. Furthermore, it requires no further assumptions on the function $\pi_i(e_{it})$. The numerator can be interpreted as the total level of abatement adjusted for the number of auctioned allowances. Observe that the Coase independence property, whereby the equilibrium allowance prices are unaffected by the initial distribution of allowances, holds throughout this framework (Coase, 1960; Montgomery, 1972; Hahn and Stavins, 2010). However, recovering the policy's underlying marginal abatement price using potentially observable market values requires specifying the cumulative number of auctioned allowances. This is because, as evident from the objective function, changes in market values depend on the share of total allowances that are freely distributed.[43] Because Δv_i and $R - R^o$ are both negative, a greater share of free allowances A^a would lower losses due to the policy which increases the numerator in Equation A.15. Thus, neglecting allowance auctioning would understate the true marginal abatement cost.

Thus far, I have treated firms and the representative household alike. However, notice that I cannot recover the aggregate marginal abatement cost because I do not observe cap-and-trade effects on households. Instead, I can only recover the total cost to firms, which is simply denoted as $\sum_{i=1}^{N-1} \Delta v_i$.

[43] I thank Michael Greenstone for raising this point.

Appendix B Adjusting for contract expiration

Intrade prediction markets are traded up to a certain date upon which contract holders are paid \$1 if the event is realized for each contract held. For the cap-and-trade prediction market, that expiration date was December 31, 2010, coinciding with the end of the 111th Congress. Because it is rare that a piece of legislation, having failed passage in the current Congress, is reintroduced with identical features in a subsequent Congress, this expiration date should coincide with the expected final possible date of Waxman-Markey approval.

However, it is difficult to ascertain whether markets expected Waxman-Markey prospects to exist following the end of the 111th Congress. If so, this introduces a bias between the prediction market price and average market beliefs which would increase as the expiration date nears. One solution to this problem is to estimate Equation 6 in first-differences, which removes a linear time trend from the price time series. However, one might still be concerned about nonlinearities in this bias as a function of remaining trading days not fully captured by a linear trend. To remove this bias, one would like to weight prediction price levels using a kernel that varies with the number of remaining trading days.

Formally, the true variable of interest is $q_t(\overline{T})$ where $\overline{T} = 12/31/2011$, the date in which the cap-and-trade system begins under the policy. I do not observe $q_t(\overline{T})$. Instead, I observe a prediction market price for a contract expiring on date $T^1 = 12/31/2010 < \overline{T}$. I now define this as $\theta_t(d, T^1)$, where $d = T^1 - t$, the number of remaining days until expiration. Specifically, it has the following piece-wise form:

$$\theta_t(d, T^1) = \begin{cases} k(d) q_t(\overline{T}), & \text{if } d < \widehat{D} \\ q_t(\overline{T}), & \text{otherwise} \end{cases} \tag{B.1}$$

where $k(d)$ is a weighting kernel which is a function of d and exists only when the remaining number of days is less than some threshold $\widehat{D}$. In other words, $k(d)$ captures any concerns about an impending contract expiration. Importantly for this exercise, I assume $k(d)$ to be discontinuous such that prediction market participants only become concerned about contract expiration after a certain point when there are fewer than $\widehat{D}$ days remaining.

The problem lies in estimating $k(d)$. Fortunately, the availability of additional Intrade data allows for an empirical estimate of $k(d)$. The prediction market contract shown in Figure 2 was not the first cap-and-trade contract offered by Intrade. Around the same time that the 2010-expiring contract begin trading, InTrade offered an identical contract with an earlier expiration date set for $T^2 = 12/31/2009 < T^1 < \overline{T}$. This contract, with prices denoted as $\theta_t(d, T^2)$, lasted only eight months and is shown as a dashed line in Figure A.6.

Estimating $k(d)$ requires the following assumption: for all trading days in which both contracts exist, $d \geq \widehat{D}$ for $\theta_t(d, T^1)$ and $d < \widehat{D}$ for $\theta_t(d, T^2)$. That is during 5/1/2009-12/31/2009, prices from the 2010-expiring contract were unadulterated by concerns over contract expiration while prices from the 2009-expiring contract incorporated such concerns. Thus:

$$k(d) = \frac{\theta_t(d, T^2)}{\theta_t(d, T^1)} \quad \forall t \in [5/1/2009, 12/31/2009] \tag{B.2}$$

The solid line in Figure A.7 plots $k(d)$ and appears trend stationary. To remove noise in $k(d)$, the following linear regression is performed:

$$k(d) = \alpha_0 + \alpha_1 d + \epsilon_d \tag{B.3}$$

where ϵ_d is a mean zero disturbance. The predicted kernel, $\widehat{k(d)}$, is shown as the dashed line in Figure A.7. The threshold $\widehat{D}$ is defined as the point at which $\widehat{k(d)} = 1$. To recover q_t, I simply rewrite Equation B.1 to obtain:

$$q_t(\overline{T}) = \text{adjusted } \theta_t(d, T^1) = \begin{cases} \frac{\theta_t(d, T^1)}{\widehat{k(d)}}, & \text{if } d < \widehat{D} \\ \theta_t(d, T^1), & \text{otherwise} \end{cases} \tag{B.4}$$

Figure A.8 plots the original $\theta_t(d, T^1)$ against the adjusted $\theta_t(d, T^1)$ using the predicted kernel from Equation B.3. Observe that the two time series begin diverging at the beginning of 2010 when $d < \hat{D}$. This divergence, which increases until the end of the 2010, inflates the original price series to remove any concerns about contract expiration. Thus, while the prospects for cap-and-trade indeed collapsed when the Senate formally withdrew cap-and-trade legislation on July 23, 2010, market beliefs over cap-and-trade prospects were actually higher than what the original prediction market indicated.

Table A.2 replicates Panel (a) of Table 2 using the adjusted Intrade prices. These estimates are slightly smaller but are not statistically different than those presented in Table 2.

Appendix C Standard error simulations

The two estimation procedures presented in Section 4 yield similar point estimates but different uncertainty. The procedures differ in sample size, unit of analysis, and inclusion of controls. In this section, I use a numerical simulation to explore the relative contribution of these three properties in explaining the difference in estimated uncertainty. To start, I assume that the true data generating process follows the CAPM model:

$$DGP : r_{it} = \alpha_i^o + \gamma_i^o \Delta\theta_t + \beta_i^o mkt_t + \epsilon_{it}$$

To mimic actual stock returns I assume the parameters estimated in my firm-level CAPM model (Row (2) in Table 2) are the true parameters so that $\alpha_i^o = \hat{\alpha}_i$, $\gamma_i^o = \hat{\gamma}_i$, $\beta_i^o = \hat{\beta}_i$, and $\epsilon_{it} \sim N(\mathbf{0}, \hat{\Sigma})$. Predictors are drawn to match empirical distributions such that $\Delta\theta_t \sim N(0.0028, 0.0016)$ and $mkt_t \sim N(0.0028, 0.00029)$. The exercise includes four statistical models designed to incrementally examine each of the three properties that differ across the two approaches. Model 1 is identical to the aggregate time series model for major event days shown in Row (1) of Table 2. Model 2 examines the implication of larger sample size by estimating Model 1 for 111 days. Model 3 estimates a firm-level SUR regression to examine the implications of covariance in residuals (Garrett, 2003; Veredas and Petkovic, 2010). Model 4 includes a control for normal market performance which matches the true DGP and corresponds to results shown in Row (2) in Table 2. Specifically, for each iteration $b = 1...500$, the procedure is:

i) Draw: $\epsilon_{it}^{(b)} \sim N(\mathbf{0}, \hat{\Sigma})$
ii) Apply DGP: $r_{it}^{(b)} = \hat{\alpha^o}_i + \hat{\gamma^o}_i \Delta\theta_t + \hat{\beta^o}_i mkt_t + \epsilon_{it}$
iii) Calculate aggregate returns: $\tilde{r}_t^{(b)} = \sum_i \frac{\overline{v_i^o}}{\sum_i \overline{v_i^o}} r_{it}^{(b)}$
iv) Est. $\hat{\gamma_1}^{(b)}$ from Model 1: $\tilde{r}_t^{(b)} = \alpha_1^{(b)} + \gamma_1^{(b)} \Delta\theta_t + \epsilon_{1t}$ for random draw of T=5.
v) Est. $\hat{\gamma_2}^{(b)}$ from Model 2: $\tilde{r}_t^{(b)} = \alpha_2^{(b)} + \gamma_2^{(b)} \Delta\theta_t + \epsilon_{2t}$ for T=111.
vi) Est. $\hat{\gamma_3}^{(b)} = \sum_i \frac{\overline{v_i^o}}{\sum_i \overline{v_i^o}} \hat{\gamma}_{3i}$ from Model 3: $r_{it}^{(b)} = \alpha_{3i}^{(b)} + \gamma_{i3}^{(b)} \Delta\theta_t + \epsilon_{3it}$ for T=111.
vii) Est. $\hat{\gamma_4}^{(b)} = \sum_i \frac{\overline{v_i^o}}{\sum_i \overline{v_i^o}} \hat{\gamma}_{4i}$ from Model 4: $r_{it}^{(b)} = \alpha_{4i}^{(b)} + \gamma_{4i}^{(b)} \Delta\theta_t + \beta_{4i} mkt_t + \epsilon_{4it}$ for T=111.

Table A.9 shows the mean and standard deviation for the value-weighted Waxman-Markey effect for the four models. Both the mean and standard devision for Models (1) and (4) closely match estimates in Rows (1) and (2) of Table 2 suggesting that parameters for the simulation are well calibrated. When the sample size increases to 111 in Model (2), uncertainty decreases dramatically, accounting for 85%[44] of the difference in uncertainty between Models (1) and (4). The firm-level analysis in Model (3) explicitly estimates the covariance in error terms across firms which further decreases the difference in uncertainty between Models (1) and

[44] 0.85=(0.11-0.018)/(0.11-0.0022)

(4) by 5%. Finally, inclusion of a control for normal market performance in Model (4) covers the remaining 10% difference in uncertainty between Models (1) and (4). Thus, simulations suggest that most of the precision gain in the firm-level, full sample analysis comes from the increased number of trading days.

Appendix D Aggregate cost uncertainty

This section estimates of uncertainty in the bounding analysis. In Section 5, I constructed the following identification region using $i = 1...L$ listed and $u = 1...U$ unlisted firms:

$$\begin{aligned} H\{N \cdot E[\Delta v]\} =& [\sum_{i=1}^{L} \widehat{\gamma}_i \widehat{v}_i^o - \sum_{u=1}^{U} |\widehat{\Delta v_u}|,\ \ \sum_{i=1}^{L} \widehat{\gamma}_i \widehat{v}_i^o + \sum_{u=1}^{U} |\widehat{\Delta v_u}|] \\ =& [\widehat{LB},\ \ \widehat{UB}] \end{aligned} \tag{D.1}$$

Because $\widehat{LB}$ and $\widehat{UB}$ are estimated, one can conduct statistical inference on the identification region. I follow the principle developed by Imbens and Manski (2004) and extended by Stoye (2009) which provide a confidence interval for a general partial identification framework that asymptotically covers the true parameter of interest with fixed probability. Specifically, a $(1 - \alpha)$ confidence interval has the general form:

$$CI_\alpha = [\widehat{LB} - c_{LB} \cdot se(\widehat{LB}),\ \ \widehat{UB} + c_{UB} \cdot se(\widehat{UB})] \tag{D.2}$$

where $se(\widehat{LB})$ and c_{LB} are the standard errors and critical values for the estimated lower bound and analogously for the estimated upper bound. Unfortunately, I am unable to use the critical values suggested by Stoye (2009) because the bounds for unlisted firms are estimated using the particular functional form shown in Equation 7 and generates covariance terms between listed and unlisted firms that are not analytically tractable. Instead, I perform a parametric bootstrap procedure. In Section 4, I used a seemingly unrelated regression procedure to estimate $\hat{\gamma}$, the vector of Waxman-Markey effects for all listed firms, and an associated L x L variance-covariance matrix $\hat{\Omega}$. The parametric bootstrap procedure begins by drawing from this L x L multinominal normal distribution and follows the steps described in the bounding analysis of Section 5. Specifically, for each iteration $b = 1...250$:

i) Draw: $\hat{\gamma}^{(b)}$ from $N(\hat{\gamma}, \hat{\Omega})$

ii) Calculate: $\hat{v}_i^{o(b)} = \frac{\overline{\widetilde{V}}_i}{\overline{\theta}\hat{\gamma}_i^{(b)}+1}$ and $\Delta v_i^{(b)} = \widehat{\gamma}_i^{(b)} \widehat{v}_i^{o(b)}$

iii) Regress: $log|\Delta v_i^{(b)}| = \alpha + \eta \log LobbyExpense_i + \mu_i$ for listed firms that lobbied.

iv) Predict: $|\Delta \hat{v}_u^{(b)}| = e^{\hat{\alpha} + \hat{\eta} \log LobbyExpense_u}$ for unlisted firms.

v) Calculate: $\widehat{LB}^{(b)} = \sum_{i=1}^{L} \hat{\gamma}_i^{(b)} \hat{v}_i^{o(b)} - \sum_{u=1}^{U} |\widehat{\Delta v_u}^{(b)}|$, $\quad \widehat{UB}^{(b)} = \sum_{i=1}^{L} \hat{\gamma}_i^{(b)} \hat{v}_i^{o(b)} + \sum_{u=1}^{U} |\widehat{\Delta v_u}^{(b)}|$

This procedure produces an empirical distribution for both the lower and upper bounds of the identification region. The $(1 - \alpha)$ confidence interval can now be written as:

$$\widehat{CI}_\alpha = [\widehat{LB}^{(\alpha/2)},\quad \widehat{UB}^{(1-\alpha/2)}] \tag{D.3}$$

Appendix E Data summary

Prediction market event study

Individual daily stock returns obtained from the Center for Research in Security Prices (CRSP). Intrade provides daily closing prices and trading volume for the 2010-expiring and 2009-expiring cap-and-trade contract. Transaction-level data for the 2010-expiring contract acquired privately from Intrade. Fama-French factors and monthly Welsh-Goyal variables were downloaded from Kenneth French's[45] and Amit Goyal's[46] websites respectively. Daily crude oil prices come from the U.S. DOE Energy Information Agency.[47] EU-ETS futures prices obtained from the Intercontinental Exchange.[48] The 3-digit manufacturing NAICS energy intensity was constructed from the NBER-CES Manufacturing Industry Database.[49] Recent sectoral level carbon intensity was provided by the U.S. DOC Economics and Statistic Administration.[50] 4-digit NAICS trade import data obtained from U.S. Census Bureau's Foreign Trade Division[51] with related output from U.S. DOC's Bureau of Economic Analysis.[52] Geographic business segment level revenue data constructed from the merged CRSP-Compustat database. Business-as-usual emissions obtained from the U.S. Department of Energy Information Agency's Annual Energy Outlook 2009.

Lobbying expenditure bounding analysis

Since the Lobbying and Disclosure Act of 1995, all individuals engaged in lobbying members of the federal government are required to register with the Clerk of the House of Representatives and the Senate Office of Public Records (SOPR).[53] Each lobbying record indicates lobbyist name (or names in the case of a team of lobbyists), name of the firm hiring lobbying services, amount spent, and in some cases the specific issue or legislation targeted by lobbying efforts (see Blanes i Vidal, Draca and Fons-Rosen (2012) for further background on reports). A copy of these publicly available records are maintained and organized by the Center for Responsible Politics which has examined the records allowing the data to be collapsed to the lobbying firm level.[54] To standardize company names for matching with CRSP data, I use Bronwyn Hall's name standardization code developed originally for patent data. Spot checks were subsequently employed to check that listed firms match CRSP data.

[45] Available: `www.mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html`

[46] Available: `www.hec.unil.ch/agoyal/`

[47] Available: `www.eia.gov/dnav/pet/pet_pri_spt_s1_d.htm`

[48] Available: `http://data.theice.com/`

[49] Available: `www.nber.org/data/nbprod2005.html`

[50] Available: `www.esa.doc.gov/Reports/u.s.-carbon-dioxide`

[51] Available: `http://data.usatradeonline.gov/usatrade/Browse/browsetables.aspx`

[52] Available:`http://www.bea.gov/iTable/index_industry.cfm`

[53] The Lobbying and Disclosure Act defines a lobbyist "any individual who is employed or retained by a client for financial or other compensation for services that include more than one lobbying contact, other than an individual whose lobbying activities constitute less than 20 percent of the time engaged in the services provided by such individual to that client over a six month period." Lobbyists were required to file reports on a semi-annual basis from 1998-2006 and on a quarterly basis since 2007.

[54] The SOPR does not require lobbying firms to provide standard company identifiers used in other databases. There is thus a problem of whether firms filing lobbying reports are truly separate entities. For example in 2009, General Electric, General Electric Transportation, and General Electric Healthcare all filed lobbying records. CRP manually identifies the subsidiaries of a parent company so that aggregation can be performed at the parent company level.

Appendix F Specific cap-and-trade related events

The period between the passage of Waxman-Markey on June 26, 2009 and the withdrawal of cap-and-trade from the Senate on July 23, 2010 marked the peak and decline of cap-and-trade prospects in the US. A number of important events during this period were instrumental in defeating cap-and-trade. This section provides a short summary of each event along with a news link. Some important events probably affected stock returns for other reasons besides Waxman-Markey prospects. For example, Scott Brown's election affected the likelihood of various policies. An asterisk (*) notes that this event is likely to have only affected cap-and-trade policy prospects and hence was examined separately in this paper. As shown by the vertical lines in Figure 2, these events were well captured by prediction market price movements.

June 26, 2009: House passes Waxman-Markey[55]
Initial hearings on draft legislation were held on the week of April 20, 2009 with the full bill introduced into the House shortly thereafter on May 15, 2009. The bill was approved on June 26, 2009 by a vote of 219-212 with 8 supporting Republicans and 44 Democrats opposed.[56]

November 4, 2009: Lindsay Graham joins Senate climate effort ($\Delta\theta_t = 0.05$)*
After passage of Waxman-Markey, efforts to pass legislation in the Senate were lead by Senators Lieberman, an independent, and Kerry, a Democrat. The arrival of Lindsay Graham, a Republican Senator from South Carolina buoyed cap-and-trade prospects.[57]

December 20, 2009: UNFCCC Copenhagen negotiations concluded ($\Delta\theta_t = -0.07$)*
With the Kyoto Protocol expiring in 2012, countries were expected to negotiate a new international climate treaty at Copenhagen. While a general agreement was reached in the final hour, the agreement was non-binding and was generally regarded as not substantial enough to succeed the Kyoto Protocol.[58]

January, 19, 2010: Scott Brown wins Mass Senate seat
The Democrat's tenuous supermajority in the Senate was lost when Scott Brown won Edward Kennedy's Massachusetts Senate seat in a special election.[59]

January 27, 2010: Graham, Kerry, Lieberman seek cap-and-trade alternatives ($\Delta\theta_t = -0.073$)*
With cap-and-trade looking unlikely, Senate sponsors look for alternative policy ideas.[60]

[55] No prediction market price movement recorded because all related fluctuations occurred during the weekend when stock markets were closed.

[56] Article:`www.nytimes.com/2009/06/27/us/politics/27climate.html`

[57] Article:`abcnews.go.com/blogs/politics/2009/11/graham-joins-dems-wh-to-write-new-climate-change-bill/`

[58] Article:`nytimes.com/cwire/2009/12/21/21climatewire-obama-negotiates-copenhagen-accord-with-senat-612 html`

[59] Article:`www.denverpost.com/latin/ci_14337907`

[60] Article:`nytimes.com/cwire/2010/01/27/27climatewire-got-ideas-about-a-climate-bill-kerry-graham-64375 html`

March 31, 2010: Obama supports offshore drilling
After months of political pressure, President Obama agrees to expand domestic oil production.[61]

April 20, 2010: BP Deepwater Horizon spill begins
An explosion on the Deepwater Horizon oil platform spills up to 4.9 million barrels of oil. Senator Graham had pushed for offshore drilling as part of the Senate climate bill to engage Senate Republicans.

April 23, 2010: Lindsay Graham drops support of Senate bill ($\Delta\theta_t = -0.06$)*
After political pressure from his constituents and party, Senator Graham criticizes Senate Democratic Leadership over disagreements regarding immigration reform on April 23, 2010. Graham formally withdrew from Senate climate efforts on April 24, 2010.[62]

June 15, 2010: Obama oval office speech
President Obama focuses on energy issues in his first oval office speech.[63]

July 22, 2010: Senate drops cap-and-trade legislation ($\Delta\theta_t = -0.14$)*
Without a filibuster-proof supermajority, Senate democrats drop consideration of cap-and-trade bill.[64]

Appendix G Models of environmental policy

CGE models for cap-and-trade regulations

During deliberations for Waxman-Markey, several CGE modeling groups were contracted by organizations and government agencies. The Environmental Protection Agency hired RTI and Dale W. Jorgenson Associates to run the ADAGE and IGEM models respectively.[65] Kolstad et al. (2010) provide a detailed peer review of ADAGE and IGEM commissioned by the EPA. With the exception of IGEM which estimates parameters econometrically, parameters within CGE models are calibrated to match observed macroeconomic activity. The offset usage assumptions adopted in this paper were based on EPA analysis (EPA, 2009). The EPPA model is run by the Joint Program on the Science and Policy of Climate Change at MIT. [66] Model runs were also commissioned by several advocacy organizations. The American Council for Capital Formation (ACCF) and National Association for Manufacturers (NAM) hired SAIC to run the U.S. EIA's National Energy Modeling System (NEMS).[67]

[61] Article: nytimes.com/gwire/2010/03/31/31greenwire-obama-proposes-opening-vast-offshore-areas-to-74696.html
[62] Article: nytimes.com/2010/04/25/us/politics/25graham.html
[63] Article: nytimes.com/2010/06/16/us/politics/16obama.html
[64] Article: www.nytimes.com/2010/07/23/us/politics/23cong.html
[65] Available: www.epa.gov/climatechange/economics/economicanalyses.html
[66] Available: globalchange.mit.edu/files/document/MITJPSPGC_Rpt173_AppendixC.pdf
[67] Available: www.accf.org/news/publication/accfnam-study-on-waxman-markey-bill

The National Black Chamber of Commerce hired CRA international to run the MRN-NEEM model.[68] The Heritage foundation hired Global Insight to run its IHS model.[69]

These models differ along many dimensions (see Fawcett, Calvin and de la Chesnaye (2009) for a recent review). One important distinction pertinent for this analysis is whether agents in the models are myopic or exhibit perfect foresight. Myopic CGE models are solved iteratively at each time step while in models with perfect foresight agents optimize simultaneously over the entire policy time-horizon. The Hotelling model introduced in Section Appendix A exhibits perfect foresight. Of the CGE models analyzing Waxman-Markey, IGEM, ADAGE, and MRN-NEEM have perfect foresight whereas EPPA, NEMS, and IHS are myopic.

Another important area of distinction is whether the CGE models incorporated non-cap-and-trade components of the Waxman-Markey bill. ADAGE, NEMS, and MRN-NEEM models include many non-cap-and-trade provisions. IGEM and EPPA do not model those provisions. It is not clear from available IHS documentation whether non-cap-and-trade provisions are modeled.

Models for previous environmental regulations

Most of the EU-ETS modeling forecasts summarized in Convery et al. (2010) are similar to the models used for evaluating the Waxman-Markey policy described above. Structural models for earlier environmental regulations were primarily partial equilibrium linear dynamic optimization models and thus not directly comparable to modern CGE models. For many of the ex-ante Title IV SO_2 forecasts under the 1990 Clear Air Act Amendments, the EPA hired ICF consulting to run the Integrated Planning Model (IPM).[70] A similar methodology was used by the EPA for forecasting costs under the Montreal Protocol. Cook (1996) notes that ex-ante EPA estimates for a 50% phase-out of CFCs by 1998 was $3.55 per kg while ex-post estimates for a 100% phase-out of CFCs by 2000 was $2.20 per km. To make ex-ante and ex-post estimates comparable, I conservatively assume that abatement costs are linear implying an ex-ante forecast cost of $7.1 per kg for a 100% phase-out by 1998.

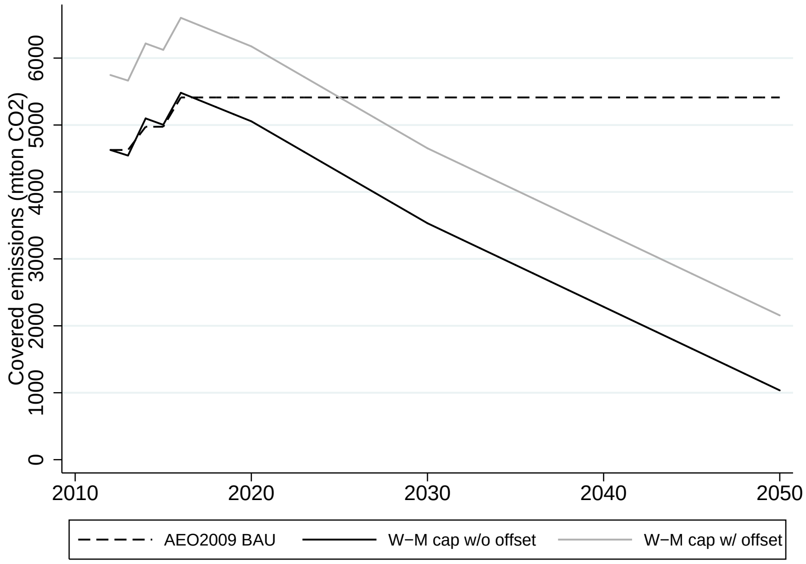
[68] Available: www.nationalbcc.org/images/stories/documents/CRA_Waxman-Markey_Aug2008_Update_Final.pdf

[69] Available: www.heritage.org/research/reports/2009/08/the-economic-consequences-of-waxman-markey-an-ar

[70] A summary of IPM available: http://pdf.usaid.gov/pdf_docs/PNACE423.pdf

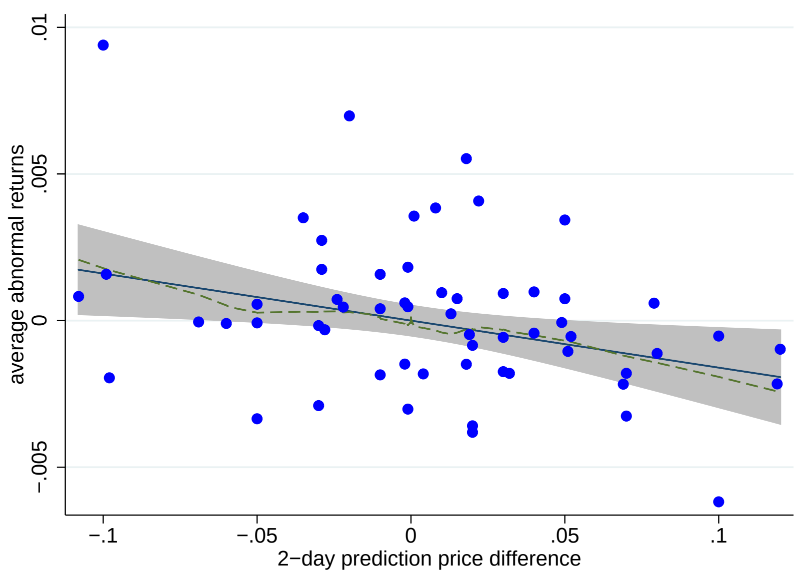
Appendix Figures

Figure A.1: Waxman-Markey annual cap versus AEO2009 business-as-usual



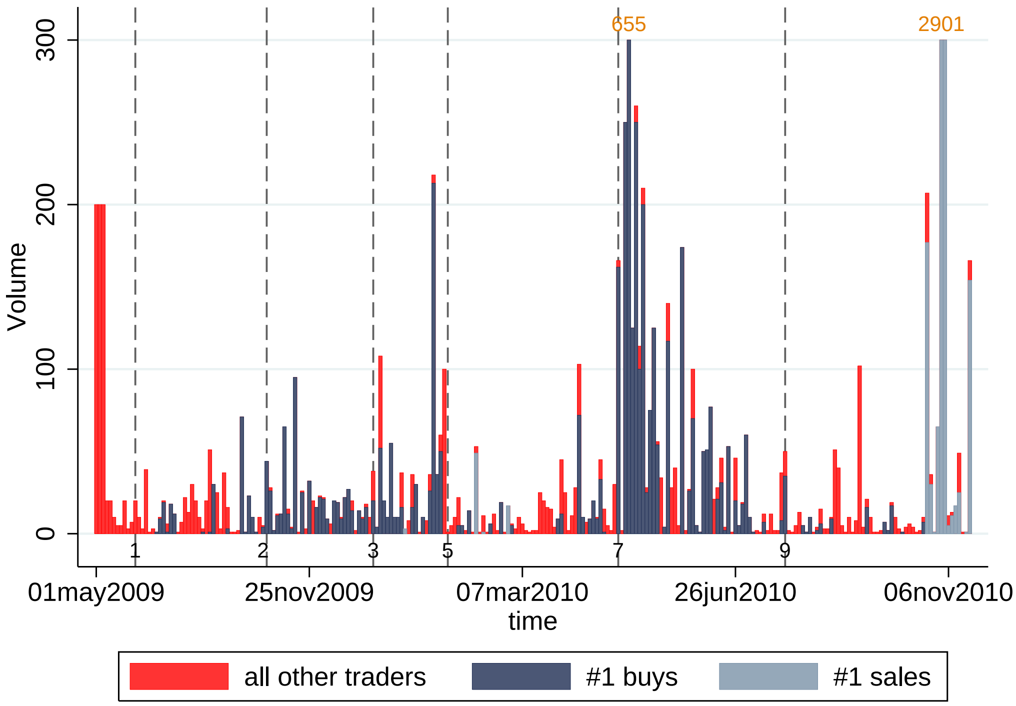
Notes: Dark solid line shows annual cap under Waxman-Markey for covered sectors. Gray solid line shows Waxman-Markey cap with offsets set at 1,400 mton per year. Coverage of emissions cap is 68.2% in 2012, 75.7% in 2014 and 84.5% in 2016. Dotted line shows business as usual under U.S. DOE Annual Energy Outlook 2009 projection.

Figure A.2: Average abnormal return vs 2-day Intrade price difference



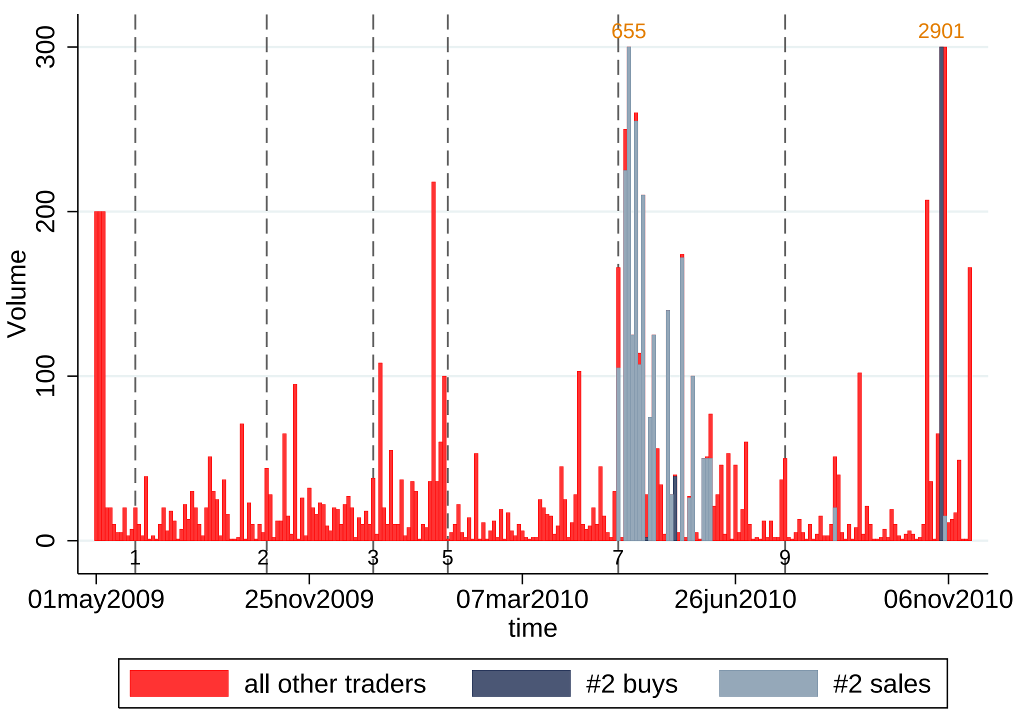
Notes: Average 2-day abnormal returns with 3-factor Fama-French normal returns removed plotted against change in cap-and-trade prediction market price. Only trading days with $\theta_t \in [0.2, 0.8]$. Linear model (solid) with 90% confidence interval shown along with local linear model (dashed).

Figure A.3: Large Trader 1 versus total market trading volume



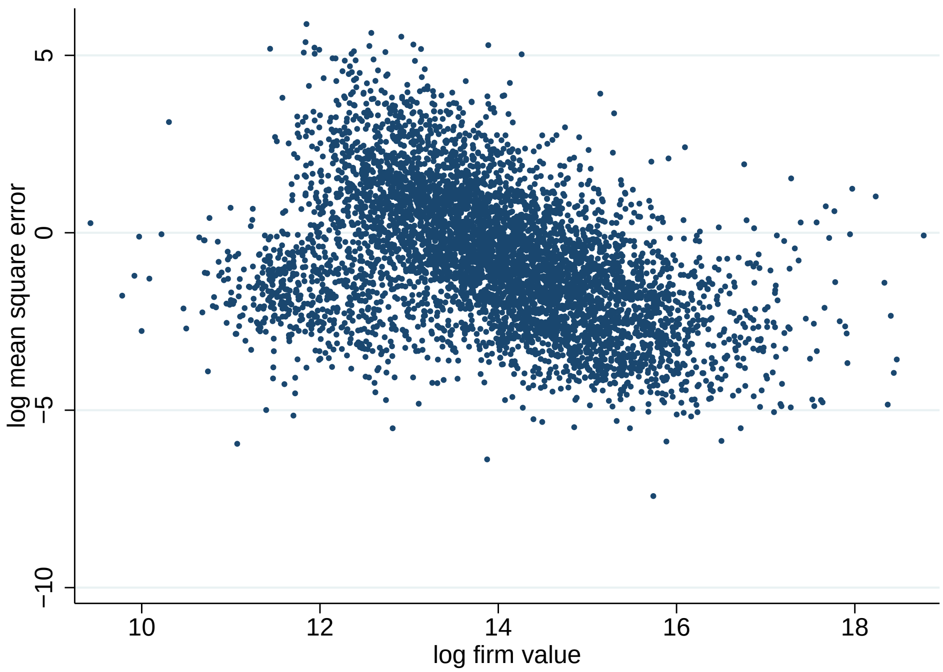
Notes: Time series of trading volume for entire cap-and-trade prediction market (red), shares bought by Large Trader 1 (dark blue), and shares sold by Large Trader 1 (light blue).

Figure A.4: Large Trader 2 versus total market trading volume



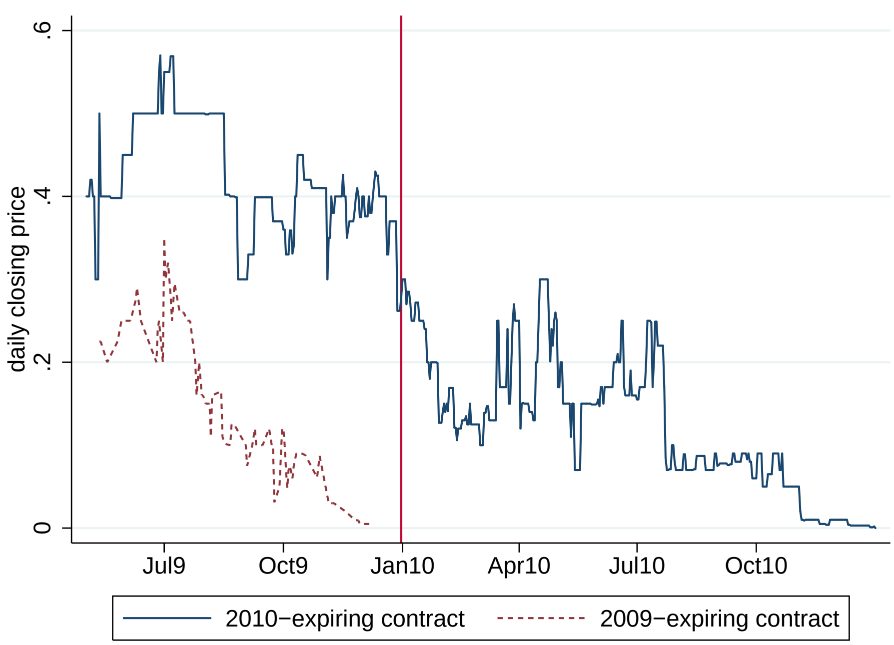
Notes: Time series of trading volume for entire cap-and-trade prediction market (red), shares bought by Large Trader 2 (dark blue), and shares sold by Large Trader 2 (light blue).

Figure A.5: Estimated mean square error vs. firm value



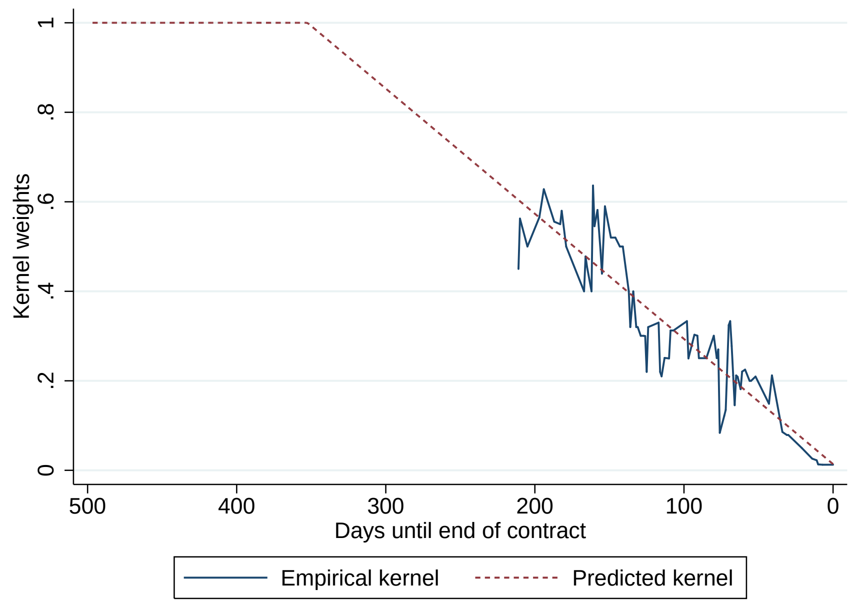
Notes: Estimated using 3-factor Fama-French model shown in Row (3) of Table 2

Figure A.6: Price for Intrade 2009-expiring and 2010-expiring cap-and-trade contracts



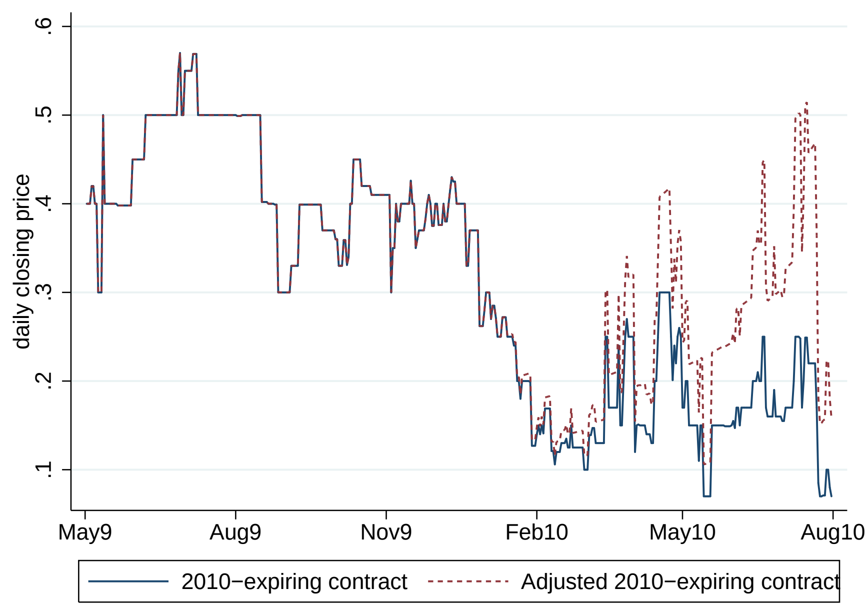
Notes: Time series of daily prices for Intrade cap-and-trade contracts expiring at end of 2009 (dashed) and 2010 (solid). Red vertical line marks start of 2010.

Figure A.7: Empirical and estimated weighting kernel for expiring cap-and-trade contracts



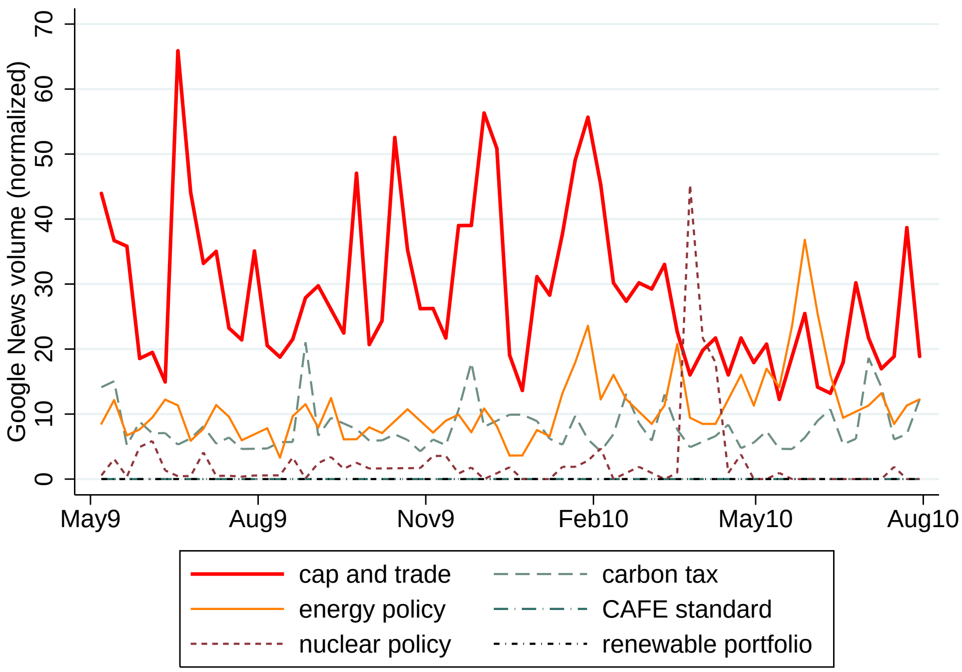
Notes: Time series of empirical (solid, blue) and predicted (dashed, red) weighting kernel, $\widehat{k(D)}$ as a function of D days remaining until contract expiration.

Figure A.8: Price for Intrade 2010-expiring contract with termination date adjustment



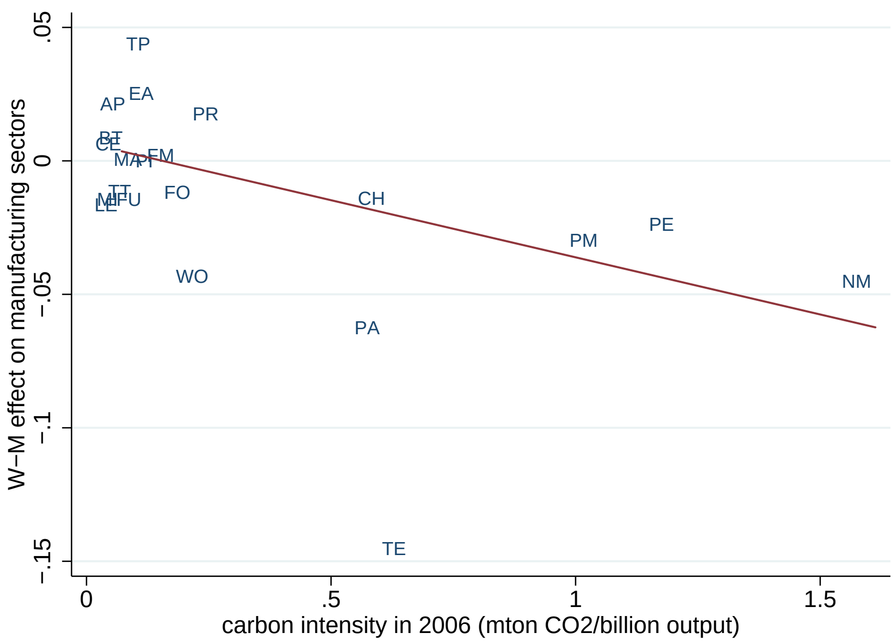
Notes: Time series of daily prices for Intrade cap-and-trade contracts expiring in 2010 (solid) and with adjustment for termination date using predicted weighting kernel in Figure A.7.

Figure A.9: Google News volume for climate policy terms



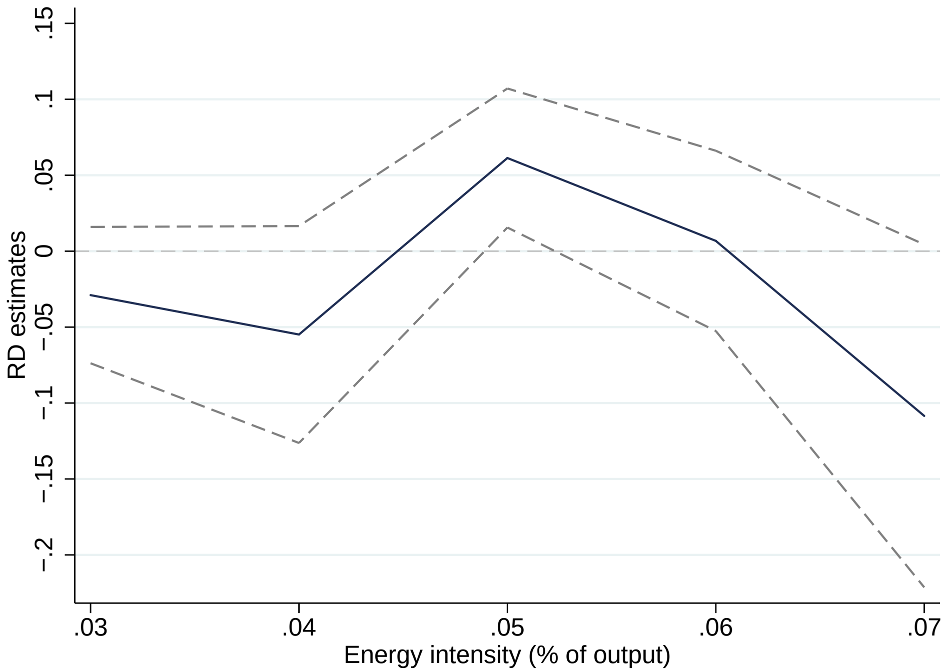
Notes: U.S. Google News volume for climate policy related terms from May 1, 2009 - July 31, 2010. Values normalized by "cap-and-trade" volume.

Figure A.10: Manufacturing subsector effects vs. energy input share



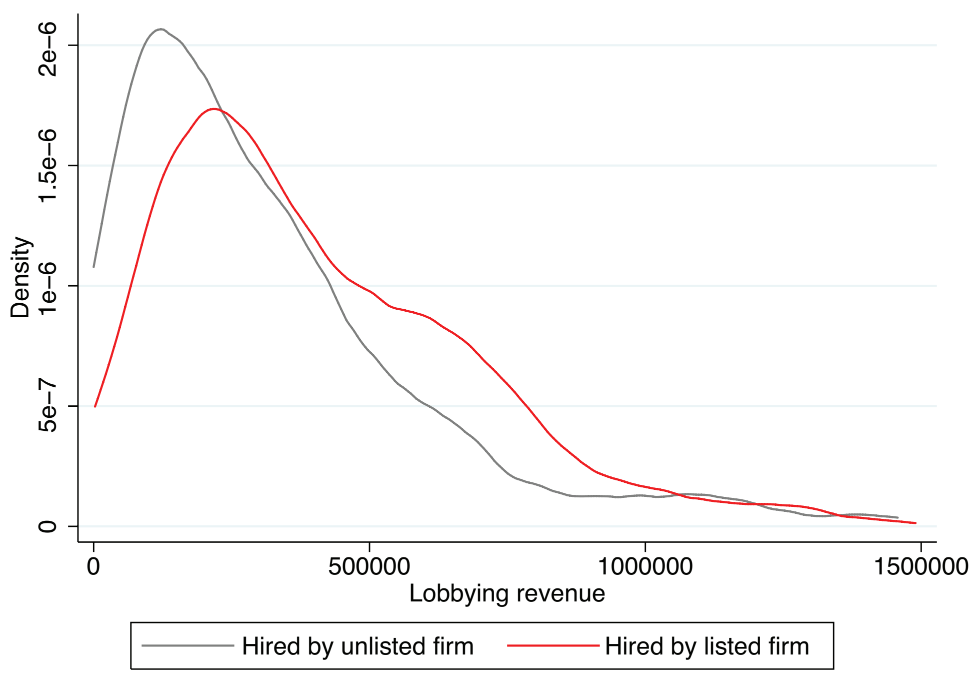
Notes: Average cap-and-trade effects for firms within a 3-digit NAICS manufacturing subsector plotted against energy intensity (% per output) in 2005. See Figure 5 for sector codes.

Figure A.11: Placebo discontinuity tests at different energy intensity levels



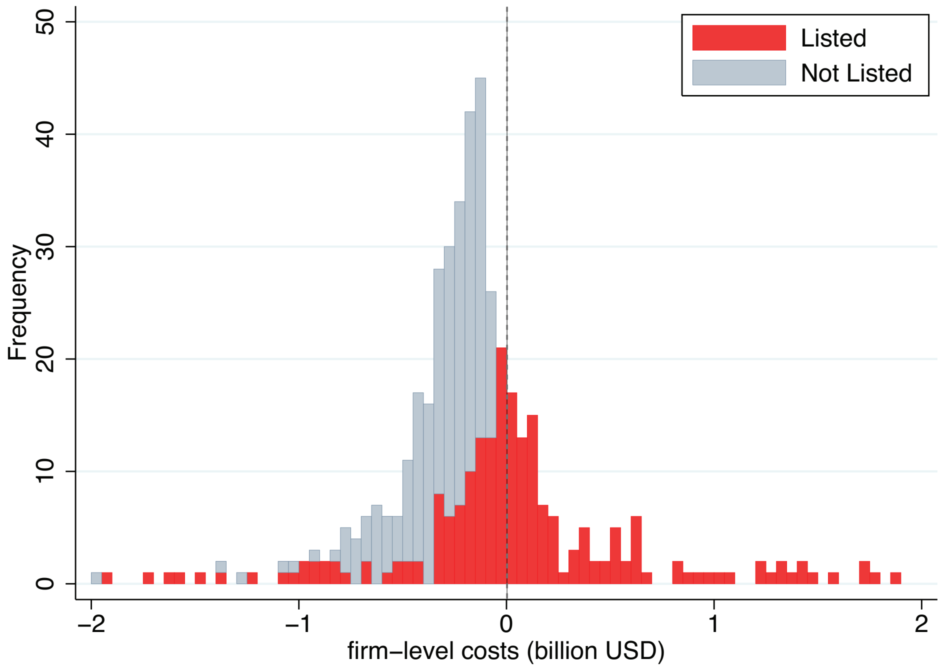
Notes: Estimated effects at different placebo discontinuities using a local linear model with 0.03 wide bins. 90% confidence intervals shown.

Figure A.12: Distribution of lobbying revenue for lobbyists hired by listed and unlisted firms



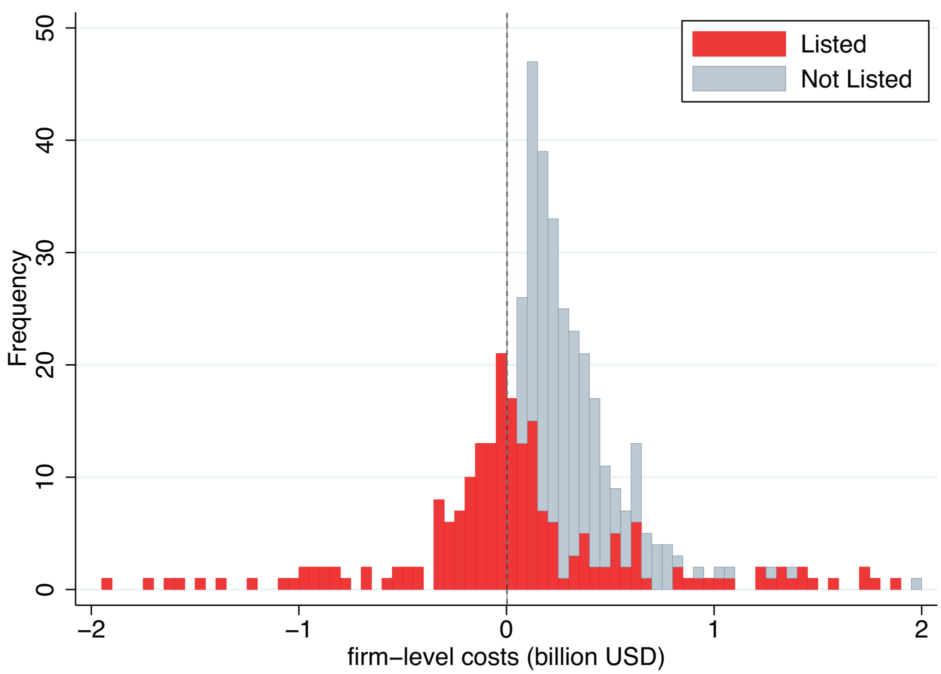
Notes: Kernel density shows distribution of total lobbying revenue for lobbyists hired by unlisted and listed firms to lobby on Waxman-Markey.

Figure A.13: Distribution of cap-and-trade costs with negative bounds for unlisted firms



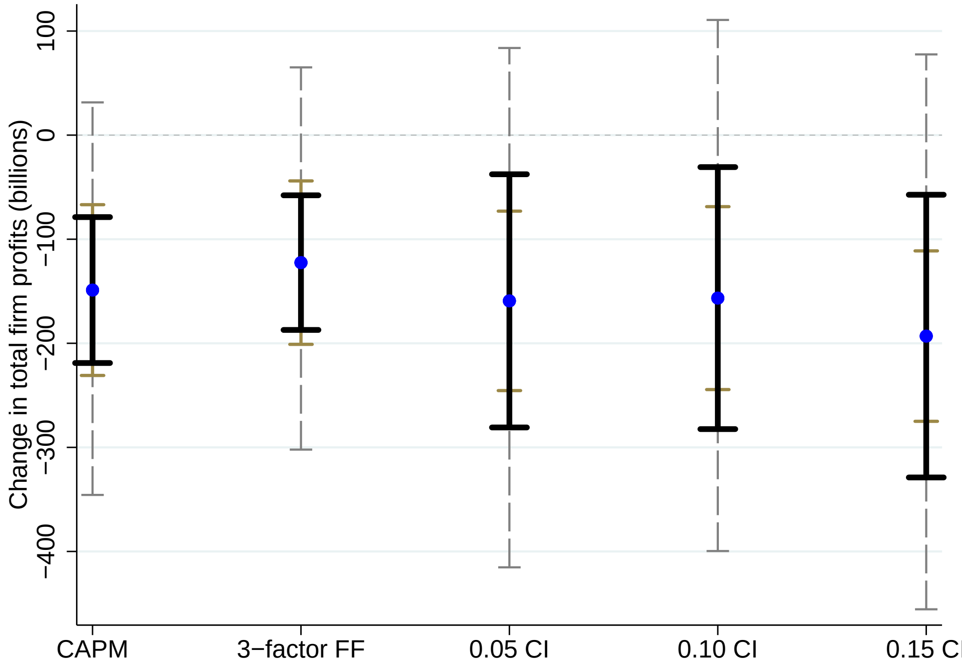
Notes: Stacked histogram of estimated firm-level cap-and-trade costs for listed firms and negative bound costs for unlisted firms that lobbied on Waxman-Markey. Distribution truncated at ± \$2 billion.

Figure A.14: Distribution of cap-and-trade costs with positive bounds for unlisted firms



Notes: Stacked histogram of estimated firm-level cap-and-trade costs for listed firms and positive bound costs for unlisted firms that lobbied on Waxman-Markey. Distribution truncated at ± \$2 billion.

Figure A.15: Aggregate cost uncertainty



Notes: Blue dot shows mean change in profit from Waxman-Markey for all listed firms with associated 90% confidence interval shown as solid thick black lines. Solid thin brown lines indicate identification region for total change in profit for listed and unlisted firms with thin dashed gray lines representing the associated 90% confidence interval for the identification region (from 250 bootstrap draws).

Appendix Tables

Table A.1: Prediction market event study: standard errors

	Dep var is 2-day stock return	
	(1)	(2)
	mkt	3 FF
$\Delta\theta_t$	-0.029	-0.026
Std. Errors		
SUR	[0.015]*	[0.013]*
OLS	[0.013]**	[0.013]**
ROBUST	[0.013] **	[0.014]**
NAICS3 CLUSTER	[0.0091]***	[0.0093]***
Number of firms	104	104
Number of days	111	111

Comparison of firm-by-firm SUR standard errors and panel regression standard errors using a 2% random sample of firms. Only days with $\theta_t \in [.2, .8]$. Uncertainty shown using firm-by-firm SUR, panel OLS, panel OLS with heteroscedasticity-robust standard errors, and panel OLS with 3-digit NAICS clustered standard errors. Column (1) uses the CAPM model with an aggregate value-weighted market index. Column (2) uses a 3 factor Fama-French model. *** p<0.01, ** p<0.05, * p<0.1

Table A.2: Prediction market event study: using expiration adjusted prices

Model	Controls	Days	equal-weighted avg. eff. $\frac{1}{L}\sum_\ell \hat{\gamma}_\ell$	value-weighted avg. eff. $\sum_\ell \frac{v_\ell^o}{\sum_\ell v_\ell^o}\hat{\gamma}_\ell$	total cost $\sum_\ell v_\ell^o \hat{\gamma}_\ell$
(1) Panel	CAPM	111	-0.016	-0.0063***	-114.81***
			[0.0096]	[0.0020]	[36.62]
(2) Panel	3-factor FF	111	-0.011*	-0.0051***	-92.5.54***
			[0.0057]	[0.0018]	[34.15]
(3) Panel	< 0.05 CI	111	-0.016	-0.0066*	-120.87*
			[0.099]	[0.0035]	[63.32]
(4) Panel	< 0.10 CI	111	-0.016	-0.0064*	-118.29*
			[0.0098]	[0.0036]	[65.50]
(5) Panel	< 0.15 CI	111	-0.018*	-0.008**	-153.73**
			[0.0098]	[0.0039]	[70.73]

Specification using expiration adjusted prediction market prices (see Appendix B). Each row from panel regressions (see Equation 6) of 5,342 firm-level returns on change in prediction market price with CAPM, 3-factor Fama-French, and value-weighted returns constructed from firms with carbon intensity below 0.05, 0.10 and 0.15 mton CO_2 per billion output as benchmark controls. Only days with $\theta_t \in [.2, .8]$. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Prediction market event study: leads and lags

	Dep var is 2-day stock return			
	(1)	(2)	(3)	(4)
	main	lagged returns	lagged prediction	lead prediction
$\Delta\theta_t$	-0.014**	-0.013**	-0.013**	-0.013**
	[0.0067]	[0.0064]	[0.0064]	[0.0066]
$r_{i,t-1}$		-0.0027		
		[0.0096]		
$\Delta\theta_{t-1}$			0.0021	
			[0.0064]	
$\Delta\theta_{t+1}$				0.0068
				[0.0066]
Number of days	111	110	110	110

Equally weighted average effect shown for 5,342 firms. Only days with $\theta_t \in [.2, .8]$. Column (1) replicates 3-factor Fama-French result. Column (2) includes lagged stock returns. Column (3) includes lagged prediction price. Column (4) includes lead prediction price. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Prediction market event study: other trading day samples

	Dep var is 2-day stock return				
	(1)	(2)	(3)	(4)	(5)
	main	2009	2010	$\Delta\theta_t \geq 0$	$\Delta\theta_t < 0$
$\Delta\theta_t$	-0.014**	-0.017*	-0.010	-0.014	-0.013
	[0.0067]	[0.0091]	[0.010]	[0.011]	[0.016]
Number of days	111	84	27	81	30

Equally weighted average effect shown for 5,342 firms. Only days with $\theta_t \in [.2, .8]$. Column (1) replicates 3-factor Fama-French main result. Column (2) includes only 2009 trading days. Column (3) includes only 2010 trading days. Column (4) includes only days with $\Delta\theta_t \geq 0$. Column (5) includes only days with $\Delta\theta_t < 0$. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Prediction market event study: sectoral effects

2-digit NAICS sector	equal-wt avg. eff.	value-wt avg. eff	Number of firms
Agriculture (11)	0.024	0.0045	11
	[0.041]	[0.16]	
Mining (21)	-0.012	-0.0085	307
	[0.033]	[0.032]	
Utilities (22)	-0.017	-0.021	123
	[0.016]	[0.020]	
Construction (23)	-0.018	-0.018	47
	[0.025]	[0.026]	
Manufacturing (31-33)	-0.0028	0.0060	1,663
	[0.0094]	[0.0048]	
Wholesale trade (42)	-0.0038	0.0022	76
	[0.015]	[0.018]	
Retail trade (44-45)	0.0058	0.015	210
	[0.018]	[0.019]	
Transportation & Warehousing (48-49)	-0.0034	0.012	161
	[0.018]	[0.017]	
Information (51)	-0.027**	-0.011	403
	[0.012]	[0.011]	
Finance and Insurance (52)	-0.023***	-0.011	1,399
	[0.0087]	[0.018]	
Real Estate (53)	-0.049*	-0.038	147
	[0.026]	[0.035]	
Professional, Scientific, & Technical Services (54)	-0.0093	-0.021*	279
	[0.013]	[0.011]	
Company management (55)	-0.048**	-0.038	124
	[0.02322]	[0.035]	
Administrative, Waste Mgmt & Remediation Services (56)	-0.035**	-0.027*	76
	[0.016]	[0.015]	
Education Services (61)	0.016	0.033	24
	[0.037]	[0.053]	
Health Care and Social Assistance (62)	-0.0011	-0.014	75
	[0.022]	[0.029]	
Arts, Entertainment, & Recreation (71)	-0.011	-0.048	37
	[0.025]	[0.034]	
Accommodation & Food Services (72)	-0.052**	-0.034	72
	[0.026]	[0.025]	

3-factor Fama-French model using 2-day returns. Only days with $\theta_t \in [.2, .8]$. Each row shows a separate seemingly unrelated regression for firms within a 2-digit NAICS sector. Includes only firms continuously listed within the same NAICS category during event period. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table A.6: Prediction market event study: import share heterogeneity

	Dep var is 2-day stock return				
	(1)	(2)	(3)	(4)	(5)
	imp share $\in [0, .1)$	imp share $\in [.1, .2)$	imp share $\in [.2, .3)$	imp share $\in [.3, 4)$	imp share >=.4
$\Delta\theta_t$	0.0050	0.013	0.0022	0.00081	-0.0032
	[0.013]	[0.012]	[0.010]	[0.016]	[0.012]
Number of firms	468	239	410	567	268
Number of days	111	111	111	111	111

Equally weighted average effect shown. 3-factor Fama-French model. Only days with $\theta_t \in [.2, .8]$. All regressions with 3-digit NAICS average removed. Import share variation at 4-digit NAICS level. Column (1) just firms with import share $\in [0, .1)$. Column (2) just firms with import share $\in [.1, .2)$. Column (3) just firms with import share $\in [.2, .3)$. Column (4) just firms with import share $\in [.3, .4)$. Column (5) just firms with import share $\geq .4$. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Prediction market event study: US revenue share

	Dep var is 2-day stock return				
	(1)	(2)	(3)	(4)	(5)
	US share $\in [0, .25)$	US share $\in [.25, .5)$	US share $\in [.5, .75)$	US share $\in [.75, 1)$	US share =1
$\Delta\theta_t$	0.020	0.0018	0.0070	-0.0027	-0.0048
	[0.016]	[0.0089]	[0.0081]	[0.0072]	[0.0091]
Number of firms	238	361	457	556	1203
Number of days	111	111	111	111	111

Equally weighted average effect shown. 3-factor Fama-French model. Only days with $\theta_t \in [.2, .8]$. All regressions with 3-digit NAICS average removed. US revenue share variation at 4-digit NAICS level. Column (1) just firms with US revenue $\in [0, .25)$. Column (2) just firms with US revenue $\in [.25, .5)$. Column (3) just firms with US revenue $\in [.5, .75)$. Column (4) just firms with US revenue $\in [.75, .1)$. Column (5) just firms with US revenue=1. SUR standard errors with correlation across firms. *** p<0.01, ** p<0.05, * p<0.1

Table A.8: Lobbying revenue for lobbyists hired by listed and unlisted firms by sector

	Listed	Unlisted	Difference
All sectors	456,101	353,290	-102,810***
	N=952	N=461	[28517]
Agribusiness	483,269	220,956	-262,313***
	N=54	N=42	[57998]
Comm/Elec	302,308	222,183	-80,125
	N=97	N=15	[49476]
Construction	326,005	355,777	29,772
	N=34	N=23	[82877]
Energy	551,979	342,697	-209,282***
	N=310	N=180	[63040.24]
Finance	516,820	392,577	-124,243**
	N=83	N=95	[38,444]
Health	496,607	351,135	-145,472
	N=14	N=12	[71106]
Trans	478,796	373,606	-105,120
	N=132	N=26	[58373]
Misc	366,404	428,873	62,469
	N=228	N=68	[65130]

Each row conducts a t-test for differences in means allowing unequal variance. Standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

Table A.9: Simulations for comparing uncertainty across models

Model	Obs	Parameter	Mean	Std. Dev.
(1) Aggregate time series	5	$\hat{\gamma}_1{}^{(b)}$	-0.012	0.11
(2) Aggregate time series	111	$\hat{\gamma}_2{}^{(b)}$	-0.0075	0.018
(3) Firm-level SUR w/out control	111	$\hat{\gamma}_3{}^{(b)}$	-0.0087	0.013
(4) Firm-level SUR w/ control	111	$\hat{\gamma}_4{}^{(b)}$	-0.0079	.0022

Simulations described in Section Appendix C. 500 draws.