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Oust the Louse: Does Political Pressure Discipline Regulators?

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Abstract:
We consider a possible determinant of regulatory decisions by public utility commissioners: the desire to remain in office. We examine regulatory exit, where a regulator leaves a commission during a term or is not re-appointed/re-elected. With data from US states, we empirically investigate several hypotheses motivated by a political agency model of regulatory decision-making. Our empirical results generally support the hypotheses, including that higher electricity prices lead to ousting, that ousting is less common where it is more costly for the principal to whom the regulator reports, and that ousting is more likely where regulators are more accountable or are more likely to share the principal's preferences. Furthermore, the results provide limited evidence that regulatory exit is not due mainly to the revolving door. Ousting also appears to lower future electricity prices.

JEL Codes: L51, D72, D78, L43, L94, L97
Keywords: public utility commission, public service commission, electricity regulation, interval censored duration data, hazard rate, dynamic panel data, public choice theory, capture theory, revolving door, political agency model.

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I. Introduction

Regulatory agencies have played an important role in network industries for almost a century in the U.S., and regulatory authorities often argue that their independence is a crucial aspect of their design.[1] Regulators, however, do not make decisions in a vacuum. We look at whether regulators have incentive to align their decisions with the preferences of those who appoint or elect them, or whether their decisions are independent of such influence. That is, does political pressure discipline regulators? We examine the tenure of state public utility commissioners and regulated electricity prices to address both meanings of "discipline". First, are non-reappointment, non-reelection, and political pressure to leave office before a term is up used to punish regulators who contradict the preferences of the appointing or electing body? Such punishment we refer to collectively as "ousting". Furthermore, does ousting regulators discipline their replacements, in the sense that future regulatory decisions are more in line with the preferences of the electorate or the governor? We demonstrate econometrically that ousting is of empirical significance in both questions. We contribute to the political economy literature on regulation by showing that in the absence of incentive-pay contracts, which are usually not possible to offer to state regulators,[2] ousting appears to be another mechanism used to motivate and restrain regulators. The results are also of

[1] For example, in its regulatory guide for foreign regulators, the FCC states that "Establishing an independent regulatory authority is a crucial factor in the success of any country's effort to introduce competition and to privatize and liberalize the telecommunications sector" (http://www.fcc.gov/connectglobe/sec1.html, visited July 1, 2010).

[2] The pay of regulators is typically determined by the legislature, sometimes directly and elsewhere by pegging commissioner salaries to that of other public positions (e.g., in Maine the salary of commissioners is equal to that of an Associate Justice of the Superior Court; Maine Rev. Stat. §6-A). One exception is that in some states the governor determines which commissioner serves as chair, and the position of chair commands a higher salary (e.g., in Maine the salary of the chair is equal to that of the Chief Justice of the Superior Court). No state determines the pay of PUC commissioners based directly on their regulatory decisions, and in some states there are explicit statutory restrictions on receiving any compensation other than salary and travel expenses for serving as a commissioner.

practical interest to policymakers concerned about the independence of regulatory decision-making.

Ousting is related to both prominent explanations in the political economy literature for regulatory decision-making: public choice theory and capture theory (or more broadly in the latter case, the economic theory of regulation). Public choice theories look at the role of constituents' preferences in shaping regulatory outcomes. Public choice theory indicates that institutions for choosing regulators are important, and many studies have looked at whether elected regulators are more pro-consumer than are those appointed by governors and legislatures.[3] Boyes & McDowell (1989) summarize the early literature (e.g., Costello, 1984) as finding no consistent pattern of elections leading to pro-consumer outcomes. Later work by Besley and Coate (2003), however, finds convincing evidence that electing officials leads to lower prices and less pass through of costs to electricity rates.[4]

Instead of following the literature in looking only at the determinants of regulated prices, our work takes ousting as the main phenomenon to explain, thus providing an explanation for why regulators might make good on their promises. In the public choice approach to regulation, the mechanism by which a past appointment or election translates into current regulatory behavior is typically not modeled explicitly. It is either assumed, as in Downsian public choice models,[5] that regulatory candidates can pre-commit to policies, or that the regulator's type (and hence future actions) is known (see Besley and

[3] Such studies are but one strand of the large literature looking at the determinants of regulatory decisions. See Dal Bó (2006) for a review.
[4] Elections matter through less-direct channels as well. Guerriero (2006) finds evidence that when states switch to electing the administrative law judges that can overturn regulatory decisions, less cost is passed through to consumers of regulated services.
[5] Downs (1957) shows that when politicians are able to commit to policies, their choices satisfy the preferences of the median voter.

Coate (2003) for a model incorporating the latter assumption). In neither case is it clear why the regulator should be viewed as an automaton acting in a predetermined, predictable way once in office. This assumption is not innocuous, because once in office the regulator's incentives may change. The choice to support a particular policy before serving office may be dynamically inconsistent. For example, to be elected, a candidate for a public utility commission (PUC) may promise to lower electricity prices for consumers, but once in office may find that supporting higher prices will secure better post-commission employment from the regulated industry. Alesina (1988) shows that lack of commitment causes the predictions of Downsian models to break down when voters are forward looking and rational.

Our work explores an explanation for why regulators might follow through on their promises, even in the absence of binding commitments to platforms: regulators care about keeping their positions and act to please those who have the power to terminate them.[6] Our approach is thus similar in spirit to the "retention rule" literature based on Holmstrom's career concerns model (Banks and Sundaram, 1998; Ashworth, 2005).[7] By accepting a seat on a PUC, a commissioner occupies a highly political position, exposed to pressure from the governor, the legislature, voters, and special interest groups (Schuler, 1996), and ousting is not an idle threat. In some cases, ousting of regulators is public and dramatic. For example, in 2006, the Maryland legislature attempted to remove all of the state's public service commissioners by dissolving the commission after a regulatory decision that greatly increased retail electricity prices. For every public ousting of a commissioner, there are probably far more cases in which back-channel pressure from the

[6] We do not suggest that regulators have this as their only goal, merely that it is one factor (perhaps among many) that may have empirical relevance.
[7] Unlike in these models, the regulator in our model does not undertake costly actions to reveal his type.

governor's office or a sense that the electorate is unhappy with regulatory performance causes commissioners to seek other employment "voluntarily" or to decline to run for another term.

The other main economic approach to explain regulatory decision-making is capture theory, which argues against allowing complete regulatory independence.[8] Both capture theory (Stigler, 1971; Posner, 1974) and its expanded version, interest group theory (Peltzman, 1976), assume that industry can pay off regulators in order to influence outcomes toward industry.[9] Such payments include "revolving door" offers of post-regulatory employment in the regulated industry,[10] which is widespread (Eckert, 1981).[11] The empirical literature on capture theory regarding whether the revolving door leads to pro-industry regulatory decisions is mixed (Cohen, 1986; Dal Bó, 2006), and there is apparently no recent evidence from the energy industry.[12] Nonetheless, given the potential empirical relevance of capture theory for regulatory performance, any public choice theory of regulation ignoring capture theory is incomplete. We explore the implications of capture theory as an alternative explanation for ousting in our empirical

[8] See Boehm (2007) for a guided survey of the literature on capture as it applies to regulation.

[9] Industry can also offer the stick instead of the carrot to keep regulators in line by protesting regulatory decisions. Leaver (2009) finds evidence to support that regulators seek to minimize industry "squawking" to enhance their reputation and post-commission employment prospects. Industry can also shape regulatory outcomes by channeling campaign contributions to the state legislators who appoint or confirm regulators (de Figueiredo and Edwards, 2007).

[10] Che (1995) points out that the revolving door may also take subtler forms such as consulting arrangements with the regulated firm after the commissioner steps down from the PUC.

[11] A former advisor at the FCC states that in the federal context, the revolving door (from a regulatory agency to the regulated industry) is "a problem much bigger than the FCC and it's all around town." (Curran, 2009).

[12] The revolving door remains of current policy interest. Upon attaining office, President Obama imposed new restrictions on lobbying by senior level government and regulatory appointees after they leave public service. Commissioners who leave the FCC (for example) are now barred for two years from appearing before the Commission (twice as long as before) and are indefinitely barred from lobbying FCC officials (Buskirk and Bender, 2009).

investigation. Our empirical work thus adds to the large body of econometric work testing the implications of the economic theory of regulation (interest group theory).[13]

To explore the link between regulatory decisions and ousting, we examine the impact that pricing decisions have on regulators' tenure with a simple model of political agency. That is, we look at whether prices that are out of line with the preferences of the political master of the regulator (the principal) lead to ousting. We assume that the principal prefers lower to higher prices, after controlling for generation cost. We also assume that the cost of ousting is lower for governors. When regulators are appointed, they must please not only the median voter (who elects the regulators' masters) but also the master (governor's office or legislature) itself. The theory also indicates that ousting will be more common when the regulator holds preferences that are farther from those of the governor or voters and when accountability for individual regulators is higher.

In the empirical model, we take the theory to the data to test various hypotheses suggested by the theoretical model. We proceed by finding regressors that proxy for various parameters in the theoretical model. The regression estimates reveal interesting patterns in the data and provide evidence in support of the political economy model as an explanation for ousting. The empirical work also points toward—but does not prove that— politically motivated ousting causes regulatory exit more by than capture does. Our exploration relies on an exhaustive dataset of state public utility regulators from 1970 to 2005, including dates of service, whether the regulators completed their terms, and whether they were reappointed or reelected. The regulatory decisions we focus on are retail electricity prices. Other variables such as generation costs, the political

[13] See section 3 of Noll (1989) for citations.

proclivities of the regulators' superiors (the party of the governor and an ideological score for voters), and the status of deregulation in each state also are included.[14]

In the econometric examination of the data, we analyze mid-term ousting with a hazard rate model of the regulators' tenure, and analyze the outcome at time of re-appointment/re-election with a binary dependent variable model. Results suggest that ousting is not an empty threat: turnover of commissioners is higher when regulatory commissioners allow higher prices, both for mid-term ousting and at time of re-appointment/re-election. Ousting may therefore be a mechanism to punish regulators who step out of line, and the degree of independence that regulators enjoy is thus open to question.[15] Electricity prices for industrial customers, who face lower costs to mount political pressure than do general consumers, have a larger impact on ousting than do standard retail rates.

Turning to political economy variables, we find that mid-term ousting is more prevalent where regulators are appointed and where the governor has strong power to remove commissioners unilaterally. Regulatory tenure is shorter where the median voter is more liberal and where there is a greater difference between the ideology of the median voter and the commissioner. Ousting is more common when the commissioner is in the majority party on the PUC or is a member of a smaller PUC, both of which make a commissioner more accountable for outcomes. All of these results are in accord with the political agency model. Furthermore, most of these factors also decrease the likelihood

[14] The present paper builds on the work of Jamison, Hauge, and Chiang (2009), who use similar data on regulators to examine determinants of commissioners leaving office before their terms expire. Our work differs by looking at individual commissioners instead of aggregating to the state level, examining re-appointments and re-elections, and placing the empirical results in the context of a theoretical model.

[15] Our work does not address the optimal degree of regulatory independence. We discuss this in our concluding section.

of the commissioner serving another term. In addition, commissioners who were not appointed by the current governor and those whose party affiliation does not match that of the governor are also less likely to serve again, results that are again consistent with the model.

To explore capture theory as an alternative explanation of why we might see regulators leaving office after what could be perceived as a pro-industry decision (as Cohen (1986) found), we also explore the impact of restrictions on post-regulatory employment. We find that regulatory tenure is shorter where there are such employment restrictions, which points toward ousting and away from capture as the explanation for the observed exit of commissioners. A suite of robustness tests lends further support to the conclusion that the political economy of regulation determines ousting.

In the final part of our study, we address the deterrent effect of ousting by considering whether the ousting of regulators lowers future electricity prices. Our results show that there appears to be a curbing effect from punishing regulators by ousting: subsequent electricity prices are lower than they otherwise would be. While the magnitude of the effect is not large, the result is robust to several changes in specification.

The paper is organized as follows. In section II, we review a model of political agency. In section III, we describe our data, link the empirical model to the theoretical model, and formulate hypotheses to test. In section IV, we present our econometric models for ousting and the regulatory determination of prices. In section V, we present our empirical results. In a concluding section we discuss avenues that our results open for future work on the optimal degree of regulatory independence. An appendix contains some details of the econometric model not presented in the main text.

II. Theoretical Considerations

To motivate the econometric work and to assist in interpreting the empirical results, in this section we briefly describe a model of political agency from Besley (2006). Instead of taking a typical mechanism design approach in which the principal offers incentive pay or budgetary contracts to the regulator, we choose a model in which the principal's only instrument is to oust the agent, which is more realistic for our application. While the original context of Besley's model is a game between voters and an elected politician, we apply it more generally to describe the regulatory process. Consider a two period game between a principal, which may be the state's governor or voters (i.e., the median voter) and the agent, the regulator. The agent's task is to perform an unobserved action, which we take to be setting the markup of utility prices p over cost. The action, in conjunction with the state of nature (also unobserved by the principal), determines the principal's payoff. There are two types of regulators, congruent and dissonant. Congruent regulators, who compose proportion π of the pool of potential regulators, hold preferences identical to the principal's, who wants prices to move with cost. Dissonant regulators earn a random, period-specific rent r from acting contrary to the principal's wishes and raises prices out of line with cost. Both types of regulators benefit from staying in office, however. The principal's task is to decide after observing its utility in period one whether to oust the regulator.

In Besley's model, ousting when the prices are out of line with the principal's preferences is part of an "oust the louse" perfect Bayesian equilibrium. In the second period, both types of agents act in their short-term interest. For the dissonant type, this leads to an outcome that the principal does not wish—prices that are high relative to cost, in our interpretation. Therefore, the principal screens agents on whether they acted congruently in period one. Good behavior leads to re-appointment or re-election, and bad behavior leads to ousting and another draw of a regulator for period two. A dissonant type acts congruently in period one only if his draw of dissonance rent in period one is too small to make forgoing the expected benefits of holding office in period two worthwhile.[16] We make one conceptual addition to Besley's model. If the principal incurred a random utility cost with mean θ when ousting the agent, then the amount of ousting observed in replications of the game would be decreasing in θ.

III. Data and Hypotheses

In this section, we describe our data and posit several hypotheses concerning the relationships between ousting on the one hand and prices and the political characteristics of the regulatory setting on the other hand. We also present an alternative hypothesis that helps distinguish between ousting and the revolving door as the explanation for regulators leaving office.

Our data includes information on all US state public utility commissioners from 1970 through 2005. We collected information including starting dates for each term, statutory and actual term ending dates, method of selection (election or appointment),[17] and the political party of the commissioner. These data are from NARUC,[18] Beecher (2007), and various PUC web sites.[19] The party affiliation of the commissioner is

[16] See section 3.3 of Besley (2006) for the mathematical details of the model.
[17] In S. Carolina and Virginia, the legislature elects the PUC commissioners. We count commissioners from S. Carolina and Virginia as elected instead of appointed. The members of the Tennessee Regulatory Authority are appointed by the governor, the lieutenant governor, and the Speaker of the House, and we count them as appointed. The members of the D.C. Public Service Commission are appointed by the mayor. In all other cases of appointment, the principal is the governor.
[18] The National Association of Regulatory Utility Commissioners (NARUC) published annual lists of commissioners on every US PUC in its *Annual Report on Utility and Carrier Regulation* (1973-1990), and *Profiles of Regulatory Agencies of the United States and Canada: Yearbook* (1991-1996).
[19] Additional data on the party affiliation of regulators was obtained from Joe Craig, who collected the data for Craig (2009).

missing for about 6% of the observations, and so we estimate models with and without controlling for party. We include all commissioners that were serving in 1970 or started terms thereafter. States generally have three to five commissioners serving concurrently, usually with overlapping terms, although some seats may be vacant at any given time. In 13 states, commissioners are elected during the entire sample period,[20] and in four others, they are elected at the beginning of the sample but appointed by the end.[21] The length of service varies significantly both within a state and across states depending on statutory length of term, how often commissioners are re-appointed or re-elected, and how often commissioners leave office before their terms end. We analyze all states and Washington, D.C. over 35 years, giving us complete observations on 1,164 commissioners who collectively serve 1,816 terms.

In the model in section II, ousting happens after period one. This formalization of the interaction between the regulator and his political master stands in for all possible opportunities for ousting. In actual regulatory practice, commissioners can be ousted (formally or informally) in the middle of a term or between terms. The data show that 22% of commissioners leave office before the expiration of their term, and that 49% of commissioners eligible to serve a succeeding term do not. The reason for this career exit by regulators is the focus of our analysis.

The electricity industry in the U.S. has been subject to regulation by state public utility commissions, and we take the retail electricity price as a key variable of interest. During the period of our study, states imposed different forms of price regulation, from

[20] These states are Alabama, Arizona, Georgia, Louisiana, Mississippi, Montana, North Dakota, Nebraska, New Mexico, Oklahoma, South Carolina, South Dakota, and Virginia.
[21] These states are Florida, Minnesota, Tennessee, and Texas. The trend away from election in recent decades continues the longer trend triggered by the receding tide of early 20th century populism.

traditional rate of return regulation, in which every attempted retail price change necessitates a rate hearing at the PUC, to alternative forms of regulation such as price caps and rate freezes. In the later years of our study, some states also deregulated (and re-regulated, in some cases) parts of the electricity industry, although retail prices are typically still regulated. Changes in utility costs, whether due to fluctuating fuel input prices for electricity generation or restructuring of wholesale electricity markets, have led to many opportunities over the years for PUCs to set new retail prices. When approving changes, in most cases—77% of the time for residential prices in our data—PUCs *raise* electricity prices. Despite the fact that PUCs in all states were designed to function as independent regulatory agencies (Schuler, 1996),[22] public outcry frequently greets even necessary rate increases, not only from residents but also from politicians such as governors eager to support their constituents.

Given these facts, we hypothesize that higher electricity prices lead to a higher probability that a commissioner is ousted. In terms of the model, we thus associate congruence with lower prices.

Hypothesis 1: *Higher prices lead to more regulatory exit.*

To test our hypothesis we use data on retail electricity prices charged to residential and industrial customers. These data, calculated separately for residential and industrial customers as the average revenue from electricity service (in $/KWH), are available annually at the state level from the Department of Energy for the sample

[22] The PUCs' decisions are "not subject to revision by higher administrative authority, and nearly all claim quasi-judicial, quasi-legislative power" (Schuler, 1996).

period.[23] To control for input costs, we use the weighted average fossil-fuel input cost specific to inputs used in each state and year.[24]

If politics and pressure groups are the cause of ousting, then the more effective the interest group, the greater the impact on ousting should be. Compared to residential consumers, large industrial users of electricity have more at stake and are fewer in number. They are also likely to be more efficient at pressuring regulators, because they can form a more organized, focused interest group. All these characteristics make industry a more effective interest group to counter the power of the regulated utilities (Becker, 1983). Indeed, Anderson (1981) found that large electricity users play an important role in determining energy policy. Thus, we refine Hypothesis 1 with the expectation that industrial price changes have more effect on ousting than residential prices.

One limitation of the commissioner data is clear. When a commissioner leaves the PUC in the middle of a term, the data indicate neither the reason for leaving nor whether exit was voluntary. Similarly, we do not see in the data whether a commissioner ran for re-election (or wished to be re-appointed) if he[25] did not serve a second (or subsequent) term. Even if we observed whether a commissioner ran for re-election, we would not limit our definition of ousting to losing an election because the choice to run is

[23] The data are from series ESRCD from the DOE Energy Information Administration (EIA) State Energy Data System (SEDS) datafile "Prices, 1970-2006," available at http://www.eia.doe.gov/emeu/states/_seds.html. Raw data are in dollars per Btu; we converted to $/KWH assuming the EIA conversion factor of 3,412 Btu/KWH.

[24] The data for 1970-1990 are from EIA database EIA-906, available at http://www.eia.doe.gov/cneaf/electricity/page/eia906u.html. The fossil-fuel input cost index is created for each state and year by multiplying the fraction of generation by electricity utilities from input i by its price, where i = coal, oil, and natural gas. For years after 1990, the data source is the same database, available for these years from http://www.eia.doe.gov/cneaf/electricity/epa/epa_sprdshts.html, and the same method is used to create the cost index, except that all generators are included, not just utilities, since independent power producers become much more important in latter years of the sample and because a few states had no generation from utilities at all by 2005.

[25] Since four-fifths of regulators are male in our data, we use the male pronoun throughout.

endogenous. A choice not to run for a second term might still effectively be an ousting, since it may have been clear to the commissioner that there was little chance of re-election.[26] A major question of the paper, however, is whether regulators exit because they are ousted—forced out for political reasons—or for unrelated reasons. Some commissioners may move on to other employment for reasons other than political pressure, retire before serving the maximum allowed number of terms, or even die in office. Such reasons apart from the political economy of regulation will show up in the regressions as additional noise. In econometric terms, exit for such reasons apart from the political economy variables is incorporated in the error term in the regression equation (or by the inherent randomness of the duration process in the Weibull regressions). If a large proportion of exit is due to unrelated factors apart from ousting or capture, then the large error term will cause imprecision in the coefficient estimates. In part for this reason, we focus more on the signs of the coefficients than on the magnitudes.

In the absence of directly observing the cause of exit, we instead rely on suggestions from economic theory to inform us which causes predominate. The theory suggests that the cost of the principal to remove an agent is a key factor.

Hypothesis 2: *Where the principal's cost of ousting the regulator is lower, there will be more regulatory exit.*

Rejecting Hypothesis 2 would indicate that regulatory exit has little to do with ousting and instead is driven mainly by retirements or other apolitical career concerns.

[26] Similarly, a commissioner may leave during a term due to political pressure or seeing "the hand writing on the wall" without formally being removed from office, and we would consider that to be an ousting by our definition as well.

Observed variables that pertain to θ, the cost of ousting, include the method of commissioner selection, how much the leeway the state governor has to remove commissioners from office, and a measure of how liberal is the citizens' ideology. We assume that regulators appointed by governors are easier to oust, because recall elections for regulatory officials are expensive and require organization of a disperse interest group, compared to the relative ease with which a governor can act. For the latter, we create a binary variable *strong governor*, which takes a value of one if the governor can remove commissioners without a trial or the concurrence of the state legislature.[27] About half the states have a strong governor by this definition. Strong governors have a lower cost of ousting than do other governors or voters. We also a collected a measure of citizen ideology, *CI* (originally on a 100 point scale; 0 = extremely conservative, 100 = extremely liberal, although we apply a logit transform to the variable in the regressions) by state and year from Berry *et al.* (1998; data updated through 2005 on the authors' website). Relying on literature from empirical political science showing that activism is associated with liberal political ideology,[28] we assume that the cost of ousting is lower where the electorate is more liberal.[29] We expect that commissioners are more susceptible to ousting when their views differ more from those of the citizens.

The theory also suggests that where π is lower and congruence is less likely, ousting is less likely:

[27] We created the *strong governor* variable from information in the NARUC Yearbooks cited above and from additional checking of state statutes and constitutions. Removal is usually "for cause" except in a few states where removal can be "at will".
[28] Levenson and Miller (1976) state that "[P]ast research has indicated that activism is positively related to a liberal political ideology…."
[29] Cost, in this case, encompasses both psychic and organizational costs.

Hypothesis 3: *Where commissioners are less likely to hold preferences that are congruent with those of the governor and the electorate, there will be more regulatory exit.*

We expect that commissioners are less likely to be congruent, and therefore more likely to be ousted, when their party affiliation differs from that of the governor and when the governor at time of reappointment did not originally appoint the commissioner.[30] We can test the effect of congruence of the commissioner's and the median voter's ideology in the state only indirectly. The measure of liberal citizen ideology (*CI*) described above can reflect small differences in political attitudes, but for commissioners only the political party affiliation is available. If all commissioners were at the extremes of the ideological spectrum, then we would assume that increases in *CI* would lead to more congruence with and less ousting of Democrat commissioners, and less congruence with and more ousting of Republican commissioners. To the extent that commissioners are not ideological extremists, this conclusion is blurred, because (for example) increases in an already liberal state's *CI* may move citizens further from a commissioner who is a moderate Democrat. Furthermore, Hypothesis 2 indicates that even for Democratic commissioners, a more liberal citizenry may lead to more ousting. Thus, a weaker prediction is that any positive impact of ideology on ousting will show up stronger for Republican commissioners than for Democrats.

An important question is whether regulators exit because they are ousted or because of capture and the revolving door. While there is no explicit revolving door in the model, if the enticement of capture is the source of the dissonance rent we can

[30] Data on governors and their party affiliations are from answers.com.

associate capture with parameter *r*. We cannot measure dissonance rent or the regulated industry's contribution to it directly. However, we proxy systematic changes in *r* across time and states with an indicator for state having a restriction on how quickly former commissioners can take jobs with firms in the regulated industry (*revolving door restrictions*).[31] About three-fifths of the states have such restrictions. Ceteris paribus, states with such revolving door restrictions have a lower present value of the gains from dissonance than other states.[32] If capture is the source of dissonance rent and the motivation for regulator exit, then revolving door restrictions will be negatively correlated with exit. If exit is due to ousting unrelated to capture, then revolving door restrictions will not be negatively associated with exit. Thus, the sign and significance of this variable can suggest whether politically motivated ousting or capture predominate in the data.

Hypothesis 4 (null – $D4_0$): *Commissioners leave involuntarily because they are ousted for political reasons after raising prices.*

Hypothesis 4 (alternative – $D4_A$): *Commissioners leave because they are captured and take advantage of the revolving door.*

We include other variables in the regressions that are not directly motivated by the theoretical model. While there is no exogenous parameter reflecting the degree of

[31] Information on the laws governing the removal of commissioners from office and the time restrictions on the revolving door to industry is taken from NARUC Yearbooks, and supplemented with data helpfully shared with us by Marc Law. Where data were in conflict between the sources, we generally gave precedence to the NARUC Yearbooks. See Law and Long (2011) for a description of the alternate data source.

[32] We do not associate the revolving door restrictions with β because the discount factor pertains to more than just the revolving door offer in the model.

accountability in Besley's (2006) model, it is reasonable to expect that when a commissioner can be held more accountable for the actions of the entire commission that there will be more ousting. We have two proxy variables for how accountable the regulator is for outcomes. One is whether the commissioner is in the majority party of the PUC, under the assumption that members of the minority party are outvoted more often. Another is the size of the PUC. If the commission has more members, then any individual member is less responsible for outcomes, because the probability that the individual is the median voter is lower. Larger PUCs therefore correspond to less individual accountability and less ousting.

Hypothesis 5: *Where commissioners can be more readily identified as responsible for commission actions, there will be more regulatory exit.*

The regressors of interest are summarized in Table 1, where the expected impacts are with reference to the hypotheses above. We supplement the dataset with US Census estimates of state population to control for heterogeneity in market size, per capita income, and the status of electricity deregulation in the state.[33] These variables are included as control variables only, and absorb some of the main relevant differences among states. Summary statistics for all the data are in Table 2.

The final implication of the political economy model is that ousting leads to prices that are lower than the counterfactual world of no political discipline. First, ousting a dissonant regulator leads to a chance that the replacement will be congruent the

[33] Deregulation status data is from the EIA, following links available at http://www.eia.doe.gov/cneaf/electricity/page/restructuring/restructure_elect.html.

next period. Second, in the "oust the louse" equilibrium, the threat of ousting deters dissonant regulators from raising prices, as long as their dissonance rent is not too large. The model thus implies that ousting leads to lower future prices, because of regulatory turnover and the demonstration that ousting is a credible threat.

Hypothesis 6: *Past ousting leads to lower regulated prices.*

IV. Empirical Model for the Determinants of Ousting

We are interested in modeling the determinants of a commissioner's tenure at a PUC, which comprises two parts: the present term and possible re-appointment or re-election to a subsequent term. We associate leaving mid-term or failing to obtain a subsequent term with leaving office after period 1 in the theoretical model. Since the commissioner need not serve the entire term, and indeed often does not, we model the first part of tenure as a duration. To the duration model we append a binary dependent variable model for the second part, the re-appointment/election outcome. The bipartite structure of our empirical model thus does not constrain regressors such as electricity prices to have the same effect on whether a commissioner leaves mid-term and whether he serves another term.

Let i index commissioners and j index terms served by a commissioner. Let t_{ij} be the duration of commissioner i's stay in office for term j and T_{ij} be the term length if served in full. Thus $0 < t_{ij} \leq T_{ij}$. Let y_{ij} be a binary variable taking a value of one if the commissioner was re-appointed or re-elected (zero otherwise), and d_{ij} be an indicator taking a value of one if the commissioner served his entire term and was eligible (by statute)[34] in that term for re-election or re-appointment (zero otherwise). Let $f(t)$ be the density function for the duration process, the hazard rate (the rate at which tenure ends given that it did not end before time t) of which may depend on the contemporaneous level of time-varying covariates (regressors) $x_{ij}(t)$. We take f to be the Weibull distribution, and so the hazard rate at t is

$$h(t;x_{ij}(t)) = p\exp(x_{ij}(t)'\beta)t^{p-1} \tag{1}$$

where β is an unknown coefficient vector to be estimated and p is a shape parameter determining the duration dependence of the distribution.[35] Let r_{ij} be the probability that $y_{ij} = 1$, which also depends on regressors. We model r_{ij} with probit regression.[36] The likelihood of an observation (t_{ij},y_{ij}) is then

$$L(t_{ij}, y_{ij}) = \left\{ f(t_{ij})^{1(t_{ij}<T_{ij})} \left[1-\int_0^{T_{ij}} f(s)ds\right]^{1(t_{ij}=T_{ij})} \right\} \left[(1-r_{ij})^{1(y_{ij}=0)} r_{ij}^{\,1(y_{ij}=1)}\right]^{1(d_{ij}=1)} \tag{2}$$

The covariates are not made explicit in the notation to avoid clutter. The first part (in the curly braces) takes the form of a standard right-censored duration model. In this application, the mass at the censoring point T_{ij} (which looks like the contribution of a right-censored spell to the likelihood) is the probability that term ij is served to completion. The second part of equation (2) in the square brackets is the likelihood for a binary dependent variable estimation, but only for the observations where the

[34] Only three states have term limits that affected commissioners serving during our study period. In Arizona, commissioners could serve no more than one consecutive term during 1992-2000 and no more than two consecutive terms thereafter. In Montana, commissioners could serve no more than 8 years in any 16 year period after 1995. In New Mexico, commissioners could serve no more than two consecutive terms after 1996. Term limits potentially affected only 10 opportunities for reappointment or reelection in the data.

[35] Equation (1) is the "proportional hazards" specification of the Weibull model. When $p > 1$, the hazard function increases with time (positive duration dependence); $p < 1$ leads to a declining hazard and negative duration dependence.

[36] Thus with regressors z_{it} and coefficients γ, $p(z_{it}'\gamma) = \Phi(z_{it}'\gamma)$, where Φ is the cumulative normal density function.

commissioner actually could have served another term. If the stochastic processes generating t_{it} and y_{it} are independent, which we maintain for simplicity, each part of the likelihood can be consistently estimated by itself. Joint estimation can improve efficiency if common covariates enter both parts of the likelihood.[37]

For actual estimation, the likelihood for the duration part of the model has to be adjusted to account for time-varying covariates and for the fact that t_{it} is often not observed directly but is instead interval censored.[38] The censoring occurs because for some commissioners we know they left office between two dates but do not know exactly when. These complications in estimation, which do not affect the interpretation of the hazard rate coefficients in (1), are explained in the appendix.

We do not include state fixed effects in the regressions because *revolving door restrictions* never changes during the sample period in about half the states, other key variables (such as *appointed*, *large PUC*, and) vary over time in only a handful of states, and *strong governor* varies only in the cross section. All estimation, however, include two-way fixed effects for the 11 North American Electricity Reliability Council (NERC) regions and years.[39] Thus, in our "laboratory of the states," identification of the impacts of the political economy variables comes from deviation from national trends across time within a state and from variation among states within the same region. Using regional instead of state-level fixed effects is a compromise between pure cross-sectional and within-unit identification. Other political economy studies of regulation have also used

[37] Given that joint estimation changes only the standard errors, not the coefficient estimates (because while regressors may appear in both the duration and probit models, no parameters are common to both), and that the improvement in the *p*-values of the estimates was small, we do not report the results from the joint estimation.
[38] Our work appears to be the first application in the economics literature of hazard rate modeling for interval-censored data with time-varying covariates.
[39] See http://www.eia.doe.gov/cneaf/electricity/chg_str_fuel/html/fig02.html for a map of the regions.

regional instead of state fixed effects (e.g., Quast, 2008). We comment on the results of adding state fixed effects in the results below.

In the absence of state fixed effects, it is possible that some of the political economy variables are endogenous. For some variables, endogeneity would appear to be unlikely. For example, state law usually determined *strong governor* long before the sample period and sometimes applies broadly to removing any state appointee, not just utility commissioners, and so is unlikely to be endogenous in an ousting regression. For other variables, such as *revolving door restrictions*, endogeneity may be more problematic (but even there, the law usually pertains to state employees generally). In such cases, it is best to interpret the results as illuminating correlations in the data to test whether they are in accord with theory, in the spirit of reduced-form estimation, rather than as precisely measuring the causal impact of a structural parameter.

V. Empirical Results

As described above, we separately estimate whether a commissioner serves his full term and whether he is re-appointed or re-elected. The reader may refer to Table 1 for a summary of how the regressors relate to the hypotheses. In the final part of the section, we look at whether past ousting affects future electricity prices.

a. Mid-term Ousting – Main Results

The results of the hazard rate regressions for being ousted mid-term are in Table 3. We investigate prices rather than margins because complete cost data are not available. However, we control for cost in part by including the index of fuel input prices. In the first few estimations, we do not control for any variables related to the party affiliation of the commissioner, to allow use of the full sample. In Estimation D1 (with *D* for duration model) we use residential electricity prices (in logs), and apart from prices do not include any of the other political economy variables related to the theoretical model. The reported coefficients are exponentiated, to show the proportional effect on the baseline hazard rate from a unit increase in the variable. Thus figures greater than one imply a higher likelihood of ousting.

Higher retail electricity prices significantly increase the hazard rate of the commissioner. Its multiplier of $\exp(\beta) = 2.138$ means that an increase of one unit in the log price slightly more than doubles the hazard rate. Stated in elasticity terms, an increase of one percent in the residential price increases the hazard rate by $\beta = 0.76$ percent. Thus, higher prices appear to lead to ousting and we accept Hypothesis 1. This conclusion is the same whether prices are in nominal or real terms.[40] We also verified that replacing the current prices and cost index with one-year lagged variables leads to the same results. Most of the control variables besides cost are individually insignificant. We leave them in the estimation because they are jointly significant ($p = 0.012$) and because they help capture differences among states. The Weibull shape parameter is significant, and is estimated to be about 0.5 in logs, which implies $p = 1.6$.[41]

In estimation D2, we add the industrial electricity price in the specification, for the reasons discussed above regarding Hypothesis 1. The hazard rate increases with industrial prices, and by more than from residential prices. In accord with Becker's (1987) notions of effective interest groups, the impact (a multiplier of 2.26) is significant

[40] Since price is in logs and year fixed effects are included, the coefficient on nominal and real log prices are identical.

[41] The significance of p implies that it is not appropriate to use an exponential duration model, because there is evidence of duration dependence. Since $p > 1$, the hazard of being ousted increases with time. This may be due to a simple "record" effect: with more time in office, a commissioner has a larger accumulated record of decisions to which the principal may object.

and much larger than the residential price effect in the same estimation, which is insignificant.

In estimation D3, reported in Table 4, we add the other political economy variables pertaining to the theoretical model, except those involving the party affiliation of the commissioner. In the model, the parameters π, θ, and so forth determine the regulator's choice of prices, and therefore including prices in the empirical model along with the variables proxying for the primitives of the model is redundant. We therefore drop the price variables in the following estimations.[42] Our focus in estimation D3 is on the direction of the effects of the political economy variables, to allow us to both test implications of the model (Hypotheses 2, 3, and 5) and help differentiate between the two explanations for regulatory exit (Hypothesis 4).

Ousting is 61% more common in states with appointed commissioners, in accord with Hypothesis 2. When the governor has authority to remove commissioners unilaterally (*strong governor*), the hazard rate is 46% higher, lending further support to Hypothesis 2 and the model. Without the party variables in the specification, we do not test Hypothesis 3 yet.[43]

The effect of the revolving door restrictions is to increase the hazard rate by 39%, in accord with null Hypothesis 4. If industry uses the revolving door to reward commissioners who raise prices, then we would expect that the hazard rate would be

[42] The issue is similar to reduced form estimation of a supply and demand system. After solving for price and quantity in terms of the primitives of the model, only the primitives are included as regressors in reduced form estimation. However, the magnitude and significance of the coefficients on the variables of interest in estimations D3-D5 change little even if we do include the price variables.

[43] We could add the variable for the current governor being the same as at the beginning of the term to test Hypothesis 3, even though we expect this variable to have more effect at the time of reappointment or re-election. If we add this variable to estimation D3, its coefficient is in the direction in accord with D3 but is insignificant, probably because there is not a lot of variation in the variable (since during much of a commissioner's term the original governor is still in office). The likelihood barely improves with the addition of the variable, and the other coefficients change little.

larger in states that have no restrictions on how soon former commissioners can join firms in a regulated industry, the opposite of our finding. While not a definitive test of capture theory versus ousting, the result does suggest that the latter is more prevalent in the data. Regarding commissioner accountability, we find that when the commissioner serves on a PUC with five or more members, the hazard rate is 24% lower. The finding is in accord with Hypothesis 5.

We add the party affiliation variables in estimation D4, which requires dropping the observations for which the commissioner's party is unknown. The political economy coefficients that were also in estimation D3 have little change in their magnitudes and retain their significance, although the estimates for large commissions and revolving door restrictions lose a significance star due to the smaller sample size. For the new variables to test Hypothesis 2, the more liberal the citizen's ideology, the more ousting, for both Democrats and Republican commissioners. Furthermore, the coefficient is larger for Republicans, in accord with Hypothesis 3.[44] The other new variable testing Hypothesis 3 is for the commissioner having the party affiliation of the governor. When the governor and the commissioner are not in the same party, the commissioners are less likely to serve out their terms, but the effect is too small to be significant (although its direction is in line with the hypothesis). Regarding the new variable pertaining to commissioner accountability and Hypothesis 5, we find that when the commissioner is in the majority party, the hazard rate is 34% higher, although the coefficient is significant only at the 10% level.

[44] The coefficient for Republicans is significantly larger than that for Democrats, with a one-sided *p*-value of 0.09.

With the inclusion of two-way fixed effects, identification of the impacts of the political economy variables comes from deviation from national trends within a state and variation among states within the same region. Apparently this latter source of variation is important in the hazard estimations (unlike our finding below for the re-appointment/re-election regressions), for if we replace the regional fixed effects in estimation D4 with state fixed effects, none of the variables in the model retain significance. Therefore, while the "laboratory of the states" reveals interesting and suggestive correlation between the political economy variables and ousting, the laboratory within the states apparently does not have enough variation in ousting and the regressors with which to work. The results including state fixed effects are strong for the probit estimations, as we show below.

b. Mid-term Ousting – Robustness Tests

The results in the previous section give support to each of the hypotheses following from the political economy model of ousting. Before turning to the model for re-appointment or re-election, we examine the determinants of not completing a term further to check the robustness of our conclusions. First, one may suspect that commissioner salary may be an important determinant of whether commissioners serve their full terms instead of seeking employment elsewhere. It is also worth investigating whether salary is correlated with the political economy variables in the specification, for if so the estimates may be biased. For example, perhaps in states with revolving door restrictions, it is more difficult to attract high-quality talent to public service, so that commissioners have fewer qualifications or less experience, and are ousted more for making poor decisions.[45]

Our investigation found no comprehensive publicly available data on the salaries of individual commissioners. However, the *Book of the States* serial published by the Council of State Governments, a source used by other researchers, reports the salary of the "chief administrative official" in charge of public utility regulation. This would appear to be the chair of the commission. However, in some states the chair earns a higher salary than other commissioners, and the Book of the States reports salary ranges for only a few states. Some of the salary data are not updated each year, and we interpolated in such cases.

Examination of the correlation between salary and the political economy variables does not indicate cause for concern. Log salary has positive correlation greater than 0.1 with the following variables only: *appointed* (corr. = 0.21), *revolving door restrictions* (0.47), *CI×Democrat* (0.23), and *large PUC* (0.14). The other political economy variables show little correlation with log salary. If higher salaries lead to less exit, then omitting salary from the estimation may bias the coefficients of these positively correlated variables downward.[46] With the exception of the final variable, however, this bias would be *against* the confirmation of the hypotheses above. It is also notable that salaries tend to be higher where there are revolving door restrictions, perhaps as a compensating differential to attract higher quality talent to public service.

Even though the discussion above indicates that omitting salaries are not likely to cause incorrect acceptance of the hypotheses, except perhaps for the large PUC variable,

[45] Several of the robustness tests in this section were suggested by anonymous referees.
[46] Of course, this statement is only heuristic, as correlation among the entire set of regressors in a multiple regression makes the direction of omitted variable bias indeterminate in general.

we add log salary to estimation D5 (also in Table 4). There are no changes in the direction of the impacts of the political economy variables, although some of the significance levels change. *Commissioner in majority party* becomes significant at the 5% level. The salary impact itself is insignificant, and none of the conclusions above change. Perhaps the largest change is for the coefficient for revolving door restrictions, which as expected increases in size and gains significance at the 5% level.[47]

The unobserved quality of the regulator may also be an important omitted factor. Presumably, some regulators prove to be inept or corrupt and rightfully lose their office. Thus, perhaps a strong governor exercises the option to oust a commissioner because of demonstrated incompetence rather than dissonant preferences. We refine our specification in two ways to lend credence to the political economy model as the explanation for ousting. First, we repeat estimation D4 but interact the strong governor variable with an indicator for the commissioner serving a second or higher term, under the assumption that it is likely that incompetence or corruption would have become known during their first term. The results (not reported) show that the coefficient for a strong governor during a commissioner's initial term ($e^{\beta} = 1.26$, $p = 0.19$) is even lower than during subsequent terms ($e^{\beta} = 1.41$, $p = 0.02$). Thus, the apparent impact of the strong governor variable does not appear to be only an artifact of quality differences among regulators.

In a second test to check whether strong governors oust mainly for reasons of dissonant preferences, we repeat estimation D4 with an interaction between *strong governor* and *same governor*, an indicator for whether the current governor is the same as

[47] Since the salary variable is usually really the chair's salary, we repeated estimation D5 interacting salary with an indicator variable for whether the commissioner was serving as chair. While the impact of salary was stronger for chairs (p-value = 0.10), no other results changed.

when the term began. Presumably, the degree of preference alignment is greater when the commissioner was appointed by (or at least began office under) the current governor. The results (not reported) show that the coefficient for *strong governor* × *same governor* (e^{β} = 1.39, p = 0.07) is lower than the coefficient for *strong governor* × *different governor* (e^{β} = 1.59, p = 0.01), as congruence of preferences in the model suggests.

Finally, we argued above that it is appropriate to leave the electricity price out of the estimations with the political economy variables, since these are reduced form regressions and model predicts that prices are determined *by* the other variables. Nonetheless, we examined the results of adding the residential electricity price from estimation D1 to the specification from estimation D4. As expected, once controlling for the political economy variables, the price coefficient is insignificant (although the coefficient is still positive). Changes in the other coefficients were negligible, there were no changes in significance levels, and an LR test rejects the need for including the price variable.

We also perform a falsification test directed at Hypothesis 4. Since we do not include state fixed effects in the model, it may be the case that *revolving door restrictions* proxies a political climate in the state where commissioners are over scrutinized and undervalued, leading to more turnover. We explored how the political economy variables would affect another high-level state board member: the university regent. Since public university regents "regulate" a not-for-profit, state-run enterprise, there is nothing equivalent to the revolving door between the PUC and private industry. Thus, the indicator for PUC revolving door restrictions, even if they apply generally to all state employees, should not be significant when applied to university regents' tenures. Given that regents are still politically appointed or elected, the other political economy variables related to the cost of ousting or the degree of accountability should still be in line with our other hypotheses, however.

Finding data on university regents proved to be much more difficult than for PUC commissioners. However, we were able to find complete enough data on regents' term dates from a variety of sources to assemble a dataset covering 832 terms of 609 regents from ten states.[48] The results of a specification similar to estimation D3 are in Table 5.[49] Whether region fixed effects are included (estimation D6) or not (estimation D7), *revolving door restrictions* has no significant effect. To make sure the result is not driven by a single state in our small sample, we repeated estimation D6 ten times leaving out each state in turn; in no iteration was *revolving door restrictions* significant at even the 20% level.[50] For the other political economy variables, *appointed* and *strong governor* still increase the hazard, in accord with Hypothesis 2, and larger regent boards are still associated with longer tenure, in accord with Hypothesis 5.[51]

We also replicated estimation D1 for an additional falsification test (results not shown), for electricity prices should have nothing to do with the tenure of university regents unless prices in reality only proxy for omitted factors affecting the political economy of a state. The residential electricity price coefficients were insignificant as expected, whether region fixed effects were included ($e^{\beta} = 0.65$, $p = 0.64$) or not ($e^{\beta} = 0.95$, $p = 0.94$).

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[48] See note to Table 5 for the list of covered states.
[49] We used D3 instead of D4 because party data were not available for regents.
[50] The insignificance of the coefficient is also not due merely to the smaller sample size. Even if there were as many observations as in estimation D3, a simple adjustment of the standard error in line with root-N asymptotics suggests that the coefficient would still lack significance.
[51] We again use the median board size as the threshold for a "large" board. Whereas for PUCs this was four members, for regent boards it is eight.

c. Ousting at the End of a Term

We turn now to the second part of the model for ousting, that for re-appointment or re-election. The results from the probit model for commissioners who served out their terms and did not face binding term limits are reported in Table 6. Apart from the price variable, we include similar variables as in the hazard estimations except we do not include *strong governor*, since mid-term removal from office is not at issue here. We also include another political variable, *same governor*, which here is an indicator for whether the governor at time of re-appointment or re-election is the same as when the preceding term began.

Table 6 reports the marginal effects from the probit estimations, which are the increase in the probability (expressed in percentage points) of re-appointment or re-election.[52] Initial exploration of the data found that log residential electricity prices best entered the specification by allowing the bottom 5% of outliers their own slope coefficient, and so in Estimation R1 prices enter as a spline with the knot at the fifth percentile of the price distribution.[53] For all but the bottom 5% of prices, for which there is no significant effect, higher residential electricity prices are significantly associated with a lower probability of serving another term. Every percentage increase in the electricity price in that range reduces the likelihood of serving another term by 21 percentage points, in accord with Hypothesis 1. As in the hazard estimations, several of the control variables are insignificant, but we again leave them in the estimation to absorb

[52] Replacing probit with logit estimation does not change any of our conclusions in Table 6.

[53] We investigated the nonlinearity in the marginal effect of residential prices on serving another term with a generalized additive model (GAM, see Hastie and Tibshirani (1990)), allowing prices to enter the specification nonparametrically. The plot of the fitted relationship revealed that there is no effect of prices below the 0.05 quantile of prices and a nearly strictly linear negative relationship above that quantile.

some of the heterogeneity among states remaining after the inclusion of the regional fixed effects.

In Estimation R2, we drop the price variable and add the political economy variables except for those related to the commissioner's party.[54] Being appointed instead of elected is associated with a 14.2 percentage point drop in the probability of serving another term, in accord with Hypothesis 2. The large size of this effect may also be due to the well-known incumbency advantage enjoyed by elected officials (Ansolabehere and Snyder, 2002). Based on the incumbency effect for regulators of about six percentage points estimated by Ansolabehere and Snyder (2002),[55] it appears unlikely that all of the effect we observe is merely due to the advantage of incumbency when the position is elected.

When the same governor is in office as at the start of the previous term, there is a 17.2 percentage point increase in the probability of serving the new term again, in accord with Hypothesis 3. *Revolving door restrictions* has no significant impact, and so we do not reject null Hypothesis 4. When the commissioners serve on a larger board, they are more likely to serve again, as implied by Hypothesis 5. The latter effect is insignificant in estimation R2, but gains significance at the 10% level in estimations R3 and R4 once the party regressors are included.

[54] If we leave the price variable in estimation R2, it is significant at the 10% level.

[55] Ansolabehere and Snyder (2002) are the only researchers we found who consider the incumbency advantage for state regulatory officials, although they group them with other "lower" state officers. Gordon and Landa (2009) review several factors that may lead to an incumbency advantage, including the *campaign discount* (it is "less costly for an incumbent to run for reelection than for a challenger to mount a campaign"), *pro-incumbent endorser bias* (created "from the unique opportunities officeholders have to cultivate relationships with influential interest groups or elites"), and *district partisan bias* toward the incumbent's party. The latter is not likely to be important for PUC elections, since commissioners typically represent the entire state.

In estimation R3, we add the party-related variables, causing a small reduction in the sample size. For the new variables, the citizen ideology coefficients are negative for commissioners of both parties, in accord with Hypothesis 2, although the effect is significant only for Democrats. While the coefficient on liberal ideology is larger for Democrats, the estimates are not precise enough to reject the hypothesis that the true coefficient is larger for Republicans (one-sided p-value = 0.16), and so Hypothesis 3 is not supported, it is not rejected. In line with Hypothesis 3, on the other hand, *same governor* has a positive impact. While the effect for being in the majority party is insignificant, being in the same party as the governor is significantly associated with a 7.3 percentage point increase in the probability of serving a succeeding term. The latter is also in accord with Hypothesis 3.

In summary, the results from the empirical investigation of regulators leaving office—both mid-term and at time of re-appointment or re-election—generally support that the cause of exit is ousting in accord with the political economy model. The results also suggest that the revolving door plays a minor role compared to ousting. While not every regressor in every specification has a statistically significant impact in the direction suggested by theory, many do, and none of them has a significant impact that points in the other direction.

d. Ousting at the End of a Term – Robustness Tests

As with the hazard estimations, we examine the determinants of serving a succeeding term further to check the robustness of the results of our hypothesis tests. We add log salary to estimation R4 (also in Table 6). There are no changes in the direction, magnitude, or significance of the political economy estimates. The salary impact is significant, in the expected direction: a one percent increase in salary leads to an 18.4 percentage point increase in the probability of serving again. In another check, we add electricity prices back into the specification from estimation R3 (results not reported). The second spline component of price, while smaller, retains significance (marginal effect = -18.7, *p*-value = 0.04). There are no changes in the direction of the impacts of the other political economy variables, although the coefficients for *CI* × *Democrat* and *large PUC* lose a significance star.

Even though several of the political economy variables have little variation over time, in estimation R5 we replace the regional fixed effects with state fixed effects. Without relying on cross-sectional variation within regions to identify the impacts of the political economy variables, the significance level of many of them falls. However, none of the coefficients changes sign, and the coefficients for *large PUC* and *revolving door restrictions* gain significance. The latter is in the direction predicted by null Hypothesis 4.

e. The Impact of Ousting on Regulatory Behavior

We close our empirical work with a final question: does ousting effectively deter future dissonant regulatory behavior? If ousting does not curb the approval of higher electricity prices, then there would be little point in punishing regulators by ousting them, apart from gratifying a principal's desire for retribution. In the estimations in Table 7, we regress prices on past ousting to test Hypothesis 6. In these panel regressions, the unit of observation is a state and the dependent variable is the log of real electricity prices.[56] Since regulators are likely to consider recent experience in the state regarding ousting as more germane to their pricing decisions than ousting from long ago, we construct a

[56] We convert all monetary variables to real terms because we do not include year fixed effects in estimations P1-P3.

measure, *past ousting*, that weights recent years most heavily. In particular, treating the amount of mid-term and end-of-term ousting in the state and year as a flow, *past ousting* is the stock of ousting from the beginning of the sample through the previous year, with a ten percent depreciation rate.[57]

The specification of the model requires care, for the preceding results demonstrate that higher prices are positively correlated with ousting. Thus, one of the expected effects of a larger amount of past ousting is a *higher* price level. To control for ousting's impact before the current period on the price level, we add the lagged price to the model. Doing so allows the coefficient on *past ousting* to reflect only the impact on the forward-looking behavior of the regulators.

In the first price regression, we include regional fixed effects as before. We do not include year fixed effects for comparability with the following estimations. Estimation P1 in Table 7 shows that higher *past ousting* is associated with lower prices in the period, after controlling for the lagged price level, fuel cost, and other control variables. The coefficients from these log-linear regressions are multiplied by 100 in the table, to express in percentage points the marginal impact on the dependent variable of a one-unit increase in a regressor. A unit increase in *past ousting* is associated with a 2.2% lower price.[58] Although the estimate is significant, there are reasons the figure might be biased. Because of the presence of the lagged dependent variable, for consistency the model requires that there is no serial correlation in the error terms, which may not be the

[57] We tested the stationarity of *past ousting* before using it as a regressor. The null hypothesis of a unit root common to all panels was soundly rejected with the tests of Levin, Lin, and Chu (2002) and Harris and Tzavalis (1999), where we used the BIC to determine the number of lags for the tests.

[58] Note that while the coefficient implies that an extra ousting at period t-1 lowers price in period t by 2.2%, the dynamics in the model also imply that the impact of a one-period change in ousting is carried forward to all future periods. Since the coefficient on lagged price is positive, the lower price in period t results in a price lower than otherwise in period t+1, and so forth.

case since the estimation does not include panel fixed effects. Another reason to include state fixed effects is that there may be many unobserved determinants of electricity prices within a state that vary within the region. Adding state fixed effects also helps account for the unobserved initial stock of ousting in a state.

In estimations P2 through P4, we include panel fixed effects. Since standard fixed effects models are inconsistent for dynamic panels, we use Blundell and Bond's (1998) GMM system estimator for dynamic panel data. In the GMM estimator, lagged differences in y instrument for lagged y in a level equation, and lagged y instruments for Δy in a second equation that is differenced to remove the fixed effects.[59] Given the theoretical model and the ousting estimations, we suspect that the error term in the price regression may have feedback on future realizations of *past ousting*. That is, shocks that increase prices in one period may lead to ousting, which will increase future values of *past ousting.* An advantage of the GMM framework is that we can treat *past ousting* as weakly predetermined instead of strictly exogenous, and assume only that the error term is uncorrelated with past realizations of the regressor. The estimations also account for the presence of residual serial correlation of up to two lags in differenced model.[60]

The result in estimation P2 is again in accord with Hypothesis 6, for *past ousting* still has a negative and statistically significant impact on prices. However, the coefficient falls to less than half of its magnitude from P1. The Hansen J statistic fails to reject the

[59] The Blundell and Bond (1998) model performs better than the more familiar Arellano and Bond (1991) estimator for dynamic panels when the coefficient on the lagged dependent variable is near unity. We performed the estimations with the xtabond2 package in Stata 11.

[60] Arellano-Bond (1991) and Hansen J tests rejected the hypothesis of no first and second order serial correlation in estimations P2-P4, which leads us to exclude an extra two lags from the instrument set for the lagged dependent variable. We also limited the number of lags included as instruments to avoid the problems of using too many instruments that be describe below. In particular, we used $\Delta y_{t\text{-}3}$ and $\Delta y_{t\text{-}4}$ to instrument $y_{t\text{-}1}$ in the level equation, and $y_{t\text{-}4}$ and $y_{t\text{-}5}$ to instrument $\Delta y_{t\text{-}1}$ and $\textit{past ousting}_{t\text{-}2}$ and $\textit{past ousting}_{t\text{-}3}$ to instrument for $\Delta \textit{past ousting}_t$ in the differences equation. The other differenced regressors serve as their own instruments.

validity of the instruments. In estimation P3, we add political economy variables as regressors. The coefficient on *past ousting* falls a bit but remains significant. Most of the additional variables are insignificant, except for *revolving door restrictions*, which has a negative impact on prices. This finding is consistent with the ousting estimations, which showed that *revolving door restrictions* was associated with more ousting, which the present estimations show leads to lower prices.

To avoid using too many instruments, which can lead to bias in the coefficients for the endogenous variables and low power of the instrument validity tests (Roodman, 2009), we do not include year fixed effects in estimations P2 and P3.[61] The exclusion of year fixed effects also allows *past ousting* to evolve naturally within each state, which may be important to the interpretation of its coefficient.[62] To test whether year fixed effects change any conclusions, we add indicator variables for three-year periods in estimation P4.[63] The impact of *past ousting* rises; now a unit increase in last period's ousting is associated with a 3.1% lower price the next period. While the inclusion of the extra instruments may bias the estimate, it is nevertheless the case that across the range of models we find consistent support for Hypothesis 6.[64]

[61] Each regressor requires another instrument in the GMM, and with a complete set of year indicator variables the number of instruments is about the same or greater than the number of panels. If year fixed effects are included in estimations P2 and P3, the Hansen overidentification tests have a *p*-value of 1.0, which is a sign of severe overfitting of the endogenous variables (Roodman, 2009). Since we cannot include a full set of year fixed effects, we verified that including a linear time trend did not remove the significance of the coefficient on *past ousting* in estimations P2 and P3.

[62] Adding year fixed effects is the same as demeaning each regressor year by year across the states, which would destroy the time series properties of *past ousting*, which are important because it is a stock variable.

[63] We cannot add fixed effects for pairs or single years without the number of instruments approaching or exceeding the number of panels and resulting in signs of overfitting the model (Hansen statistics near 1.0) (Roodman, 2009).

[64] We also explored other depreciation rates—0, 5, and 15%--in the construction of *past ousting*. As the rate falls, the statistical significance of the coefficient rises, perhaps indicating that regulators have long memories when it comes to ousting.

VI. Conclusions

Our empirical results support the five hypotheses stemming from the political agency model of ousting. Retail electricity prices are associated with ousting, both mid-term and at time of selecting for a second term. A greater cost of ousting and a greater prevalence of congruent regulators are associated with less exit, in accord with the theoretical model. Where accountability is higher, there is more ousting. Furthermore, the finding that revolving door restrictions on post-regulatory employment is associated with more ousting suggests that politically motivated ousting is more likely than a revolving-door story as the main reason for turnover. Past ousting also appears to discipline regulators' decisions, leading to lower future prices, although the magnitude of the effect is mild.

While we have presented evidence that political principals can apparently bring pressure to bear on regulators through ousting, we have not addressed the question of the optimal degree of control the principal should exert over the regulator. Insulating the regulator from political interference is important to preserve regulatory credibility and commitment. The ability of the regulator to commit to prices and policies is important in the presence of long-lived capital investment, to avoid the hold-up problem (Newbery, 1999).[65] Cambini and Rondi (2010) find that the higher the degree of independence among European regulatory institutions, the higher is the associated investment by the regulated firms.[66] In part for this reason, PUCs in the US were established as independent regulatory agencies (Schuler, 1996). On the other hand, commitment can

[65] When public utility regulators cannot commit to long-term policies, they have incentive to reduce the prices that the regulated utilities are allowed to charge after the firm has sunk its investment. Recognizing this time inconsistency problem, regulated utilities may invest less than is optimal.

[66] See Trillas (2010) for a survey of related empirical work on regulatory independence, which generally comes to similar conclusions.

lead to bad policies being locked in when the regulator may not be benevolent.[67] Armstrong and Sappington (2006; sec. 4.5) discuss why complete independence of the regulator is not optimal when capture is a possibility, and Martimort (1999) shows that a higher potential for capture may call for placing stricter constraints on the regulator.

In general, the regulator's independence, which allows it to act contrary to the wishes of the principal, may either increase social welfare (when the principal does not want prices to reflect cost) or decrease it (when the principal wants prices to be aligned with cost). In existing empirical studies of regulated industry performance or investment, regulatory independence, however measured, typically enters the econometric models in linear fashion. Thus, the corpus of empirical work (see Trillas (2010) for a review) generally finds that "more independence is better" with models that do not allow for nonlinear impacts of regulatory oversight. A promising avenue for future research would be an empirical examination of whether (and which) regulatory institutions embody the *optimal* amount of independence.

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[67] See chapter 16 of Laffont and Tirole (1993).

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Econometric Appendix

Here we present further details on the duration estimation for mid-term ousting. There are two complications not reflected in equation (2) in the main text. First, some of the covariates entering hazard rate specification (1) vary between years, and thus vary over the tenure of commissioners whose service spans more than one calendar year (time-varying covariates). Second, some observations are censored. Right censoring occurs in the data for two reasons. As mentioned in the text, commissioners serving their full term are never ousted and are treated as right-censored observations at time of term end. Furthermore, terms still in progress at the end of the observation period for the data are also right censored. Less common in duration data models is the interval censoring of some of the observations. When a commissioner disappears from the NARUC yearbooks before his term ends, the publication does not state the date he left office. Thus, in the absence of other information, we only know that the commissioner's tenure ended at some point between the record dates of the last yearbook listing the commissioner and the first yearbook not listing him. In some cases, we can narrow the range of the interval of departure, using data from Beecher (2007), the starting date of the succeeding commissioner, or other information we found through extensive searching of PUC websites and news media. Thus, we have duration data that may be completely observed, right censored, or interval censored. The likelihood for interval-censored Weibull duration data with time-varying covariates is presented in Sparling *et al*. (2006), who work out the likelihood for a more general parametric model that nests the Weibull. We estimate the model via MLE in Stata 11.1 (using the default options for the “lf” method, which include using the Newton-Raphson maximization method with numeric derivatives), which converged readily in all specifications.

Table 1: Expected impacts of regressors on ousting

Variable	Related to parameter	Related to hypothesis	How related	Expected impact under Hypothesis
Residential price	p	1	$x\uparrow \Rightarrow p\uparrow$	More exit
Industrial price	p	1	$x\uparrow \Rightarrow p\uparrow$	More exit than for residential p
Commissioner is appointed	θ	2	$x\uparrow \Rightarrow \theta\downarrow$	More exit
Strong governor	θ	2	$x\uparrow \Rightarrow \theta\downarrow$	More exit
Liberal citizen ideology	θ	2	$x\uparrow \Rightarrow \theta\downarrow$	More exit
Liberal citizen ideology	π	3	$x\uparrow \Rightarrow \pi\downarrow$	More exit for Republicans than for Democrats
Commissioner and governor in same party	π	3	$x\uparrow \Rightarrow \pi\uparrow$	Less exit
Same governor as at start of term	π	3	$x\uparrow \Rightarrow \pi\uparrow$	Less exit
Revolving door restrictions	r	4 (null)	$x\uparrow \Rightarrow r\uparrow$	More exit or no effect
Revolving door restrictions	r	4 (alt.)	$x\uparrow \Rightarrow r\downarrow$	Less exit
Commissioner is on a large PUC	a^*	5	$x\uparrow \Rightarrow a\downarrow$	Less exit
Commissioner is in majority on PUC	a^*	5	$x\uparrow \Rightarrow a\uparrow$	More exit

*Parameter *a* refers to accountability.

Table 2: Summary Statistics of the Data

Variable	Mean	Std. Dev.	Minimum	Maximum
Data for Mid-Term Ousting Analysis				
Retail residential electricity prices	6.901	2.738	1.075	23.347
Retail industrial electricity prices	4.383	1.981	0.337	17.957
Commissioner is appointed	0.714	0.452	0.000	1.000
Strong governor	0.505	0.500	0.000	1.000
Revolving door time restrictions	0.607	0.488	0.000	1.000
Commissioner’s party = majority pty	0.681	0.466	0.000	1.000
PUC has > 4 commissioners	0.413	0.492	0.000	1.000
Cmmssr’s party = governor’s pty	0.585	0.493	0.000	1.000
Citizen ideology score CI_{it}	46.971	15.206	6.860	95.972
CI_{it} x Democrat commissioner*	25.605	25.412	0.000	95.972
CI_{it} x Republican commissioner*	18.752	24.940	0.000	91.566
Commissioner’s party = Democrat	0.568	0.495	0.000	1.000
Commissioner’s party = Republican	0.389	0.487	0.000	1.000
Commissioner’s party = Other	0.043	0.203	0.000	1.000
Deregulation is active	0.068	0.252	0.000	1.000
Deregulation is suspended	0.007	0.086	0.000	1.000
State per capita income, log	9.620	0.629	7.954	1.000
State population, log	15.001	1.044	12.664	10.982
Data for Re-appointment/Re-election Analysis				
Re-appointed/re-elected	0.494	0.500	0.000	1.000
Retail residential electricity prices	7.005	2.725	1.145	23.347
Commissioner is appointed	0.697	0.460	0.000	1.000
Revolving door time restrictions	0.587	0.493	0.000	1.000
Cmmssr’s party = majority pty	0.675	0.468	0.000	1.000
PUC has > 4 commissioners	0.445	0.497	0.000	1.000
Cmmssr’s party = governor’s pty	0.551	0.498	0.000	1.000
Citizen ideology score CI_{it}	46.971	15.273	6.860	91.236
CI_{it} x Democrat commissioner*	26.369	25.113	0.000	91.236
CI_{it} x Republican commissioner*	17.737	24.850	0.000	86.465
Same governor as previous term	0.469	0.499	0.000	1.000
Commissioner’s party = Democrat	0.591	0.492	0.000	1.000
Commissioner’s party = Republican	0.364	0.481	0.000	1.000
Commissioner’s party = Other	0.045	0.206	0.000	1.000
Deregulation is active	0.079	0.270	0.000	1.000
Deregulation is suspended	0.010	0.101	0.000	1.000
State per capita income, log	9.652	0.608	7.954	10.982
State population, log	14.978	1.024	12.695	17.402

*Variable is divided by 100 and passed through a logit transform to create a regressor.

Table 3: Hazard Rate Estimation for Leaving Mid-Term

Y=time until ousted	*Estimation D1* *exp(β)* *(s.e.)*	*Estimation D2* *exp(β)* *(s.e.)*
Regressors of interest		
Residential electricity prices, log	2.138**	0.949
	(0.735)	(0.494)
Industrial electricity prices, log		2.256**
		(0.832)
Other control variables		
Fuel input cost index, log	1.057	1.035
	(0.083)	(0.076)
Deregulation is active	0.544*	0.520**
	(0.178)	(0.170)
Deregulation is suspended	0.915	0.763
	(0.585)	(0.498)
Per capita income, log	0.631	0.567
	(0.246)	(0.221)
State pop, log	1.191***	1.215***
	(0.073)	(0.073)
Weibull shape parameter (log)	0.479***	0.480***
	(0.048)	(0.043)
Likelihood	-2100.1	-2098.0
χ^2 statistic (d.o.f.) and *p*-value	24.4 (6) 0.000	31.07 (7) 0.000
Clusters (commissioners)	1,164	1,164
N	1,816	1,816

*** p<0.01, ** p<0.05, * p<0.10

Notes: Exponentiated coefficients are reported, which can be interpreted as the multiplier on the hazard rate. Robust standard errors for the multipliers are in parentheses (clustered on the individual). Estimation is censored Weibull duration regression (see appendix). Two-way fixed effects for years and the 11 Electricity Reliability Council regions are included but not reported. χ^2 statistic is for the null hypothesis that all regressors (excluding fixed effects) have zero coefficients (degrees of freedom are in parentheses).

Table 4: Hazard Rate Estimation for Leaving Mid-Term

Y=time until ousted	*Estimation D3* *exp(β)* *(s.e.)*	*Estimation D4* *exp(β)* *(s.e.)*	*Estimation D5* *exp(β)* *(s.e.)*
Regressors of interest			
Appointed	1.612**	1.511**	1.547**
	(0.311)	(0.300)	(0.312)
Strong governor	1.463**	1.464**	1.481**
	(0.223)	(0.239)	(0.238)
Citizen ideology x		1.332**	1.249*
Democrat commissioner		(0.179)	(0.169)
Citizen ideology x		1.778***	1.575**
Republican commissioner		(0.362)	(0.311)
Commissioner and		0.916	0.897
governor in same party		(0.130)	(0.126)
Revolving door restrictions	1.386**	1.330*	1.422**
	(0.208)	(0.216)	(0.228)
Large PUC (> 4	0.763**	0.797*	0.805*
commissioners	(0.095)	(0.103)	(0.102)
Commissioner in majority		1.337*	1.346**
party		(0.200)	(0.203)
Commissioner salary, log			0.799
			(0.311)
Other control variables			
Fuel input cost index, log	1.104	1.158	1.120
	(0.079)	(0.106)	(0.091)
Republican commissioner		0.987	0.973
		(0.124)	(0.121)
Independent/other party		1.055	1.009
commissioner		(0.304)	(0.288)
Deregulation is active	0.562	0.534*	
	(0.202)	(0.194)	
Deregulation is suspended	1.547	1.354	
	(0.968)	(0.865)	
Per capita income, log	0.371*	0.598	1.103
	(0.221)	(0.192)	(0.375)
State pop, log	1.274***	1.206**	1.198**
	(0.093)	(0.091)	(0.095)
Weibull shape parameter,	0.471***	0.486***	0.490***
Log	(0.050)	(0.049)	(0.054)
Likelihood	-2051.5	-1937.5	-1946.5
χ^2 stat (d.o.f.) and *p*-value	43.7 (9) 0.000	61.5 (15) 0.000	35.86 (14) 0.001
Clusters (commissioners)	1,164	1,074	1,074
N	1,816	1,683	1,683

*** p<0.01, ** p<0.05, * p<0.10
See notes to previous table.

Table 5: Hazard Rate Estimation for Regents Leaving Mid-Term

	Estimation D6		*Estimation D7*	
Y=time until regent is ousted	*exp(β)*	*p-value*	*exp(β)*	*p-value*
Regressors of interest				
Appointed	3.711**	0.011	3.091***	0.000
Strong governor	27.407***	0.000	5.149***	0.000
Revolving door restrictions	0.694	0.313	1.257	0.396
Large board (> 8 regents)	0.447***	0.006	0.621**	0.030
Other control variables				
Per capita income, log	12.823*	0.068	15.678***	0.004
State pop, log	2.058**	0.039	0.961	0.640
Weibull shape parameter, log	0.154*	0.053	0.153*	0.055
Likelihood	-929.6		-938.9	
χ^2 stat (d.o.f.) and *p*-value	28.49 (6) 0.000		56.03 (6) 0.000	
NERC region fixed effects	yes		no	

*** p<0.01, ** p<0.05, * p<0.10

Notes: Both estimations have 609 clusters (regents) and *N*=832. Regents are included from the Alaska Board of Regents, the Regents of the University of California, The Trustees of Indiana University, the Regents of the University of Minnesota, Montana University System Board of Regents, University of Nebraska Board of Regents, the Ohio Board of Regents, The University of Texas System Board of Regents, the Utah System of Higher Education Board of Regents, and the University of Virginia Board of Visitors. See also notes to previous table.

Table 6: Probit Estimation for Re-appointment or Re-election

Y = 1 if Re-appointed or re-elected, 0 if not	Marginal effects × 100 (with std. errors in parentheses)				
	Estimation R1	*Estimation R2*	*Estimation R3*	*Estimation R4*	*Estimation R5*
Regressors of interest					
Res. electricity prices, log (spline: bottom 5%)	27.392 (26.144)				
Res. electricity prices, log (spline: top 95%)	-21.182** (8.403)				
Appointed		-14.18*** (3.95)	-11.04*** (4.11)	-12.13*** (4.19)	-21.44* (11.90)
Citizen ideology x Democrat			-7.18** (3.56)	-7.12** (3.55)	-8.92* (4.77)
Citizen ideology x Republican			-2.30 (4.29)	-2.45 (4.30)	-6.57 (5.45)
Commissioner and governor in same party			7.31** (3.31)	7.22** (3.30)	6.10* (3.23)
Same governor as previous term		17.20*** (2.84)	16.03*** (3.09)	16.32*** (3.09)	14.76*** (3.11)
Revolving door restrictions		-2.53 (3.51)	-0.95 (3.71)	-2.07 (3.77)	-10.33* (5.62)
Large PUC (> 4 commissioners		3.78 (3.17)	5.70* (3.27)	6.29* (3.29)	11.08** (4.56)
Commissioner in majority party			-0.82 (3.30)	-1.15 (3.30)	-0.58 (3.17)
Commissioner salary, log				18.42** (9.16)	
Other control variables					
Fuel input cost index, log	2.710* (1.393)	1.40 (1.28)	1.26 (1.33)	1.61 (1.35)	1.97 (2.78)
Republican commissioner			-1.51 (3.02)	-0.85 (3.05)	-3.12 (2.93)
Independent/other party commissioner			-4.46 (5.75)	-4.60 (5.75)	-8.77 (5.94)
Deregulation is active	5.836 (6.634)	4.01 (6.71)	3.24 (6.76)	3.89 (6.77)	0.81 (6.76)
Deregulation is suspended	9.094 (14.828)	5.76 (16.13)	6.80 (15.37)	7.16 (15.51)	3.59 (15.85)
Per capita income, log	-9.815 (7.740)	-6.04 (7.57)	-5.25 (7.91)	-18.86* (10.38)	-10.26 (8.37)
State pop, log	-4.333*** (1.619)	-5.45*** (1.71)	-5.83*** (1.89)	-7.68*** (2.06)	-17.85 (15.19)
Likelihood	-804.5	-779.0	-709.0	-707.1	-665.8
χ^2 statistic (d.o.f.) and *p*-value	39.8 (7) 0.00	39.8 (7) 0.00	48.3 (13) 0.00	52.3 (14) 0.00	33.0 (13) 0.00
Pseudo *R* squared	0.081	0.099	0.108	0.110	0.162
N	1,263	1,247	1,147	1,147	1,147
Clusters (commissioners)	848	840	772	772	772

*** p<0.01, ** p<0.05, * p<0.10

Notes: Marginal effects are the change in the probability of serving a directly succeeding term from a one unit change in *x* (calculated as the average in the sample), expressed in percentage points. Marginal effects are calculated with the derivative for continuous *x* and discrete changes for discrete *x*. Robust standard errors for the marginal effects are in parentheses (clustered on commissioner). Marginal effects for the spline in prices measure the slopes for the relevant interval in prices. Two-way fixed effects for years and regions are included but not reported. See previous table notes on χ^2 statistic.

Table 7: Dynamic Panel Estimations for Electricity Prices

	Coefficients × 100 (with std. errors in parentheses)			
Y = real residential electricity price, log	*Estimation P1*	*Estimation P2*	*Estimation P3*	*Estimation P4*
Regressor of interest				
Past ousting	-2.150**	-0.789**	-0.695**	-3.273***
	(0.862)	(0.314)	(0.331)	(0.661)
Other control variables				
Lagged real residential electricity price, log	25.011***	69.254***	69.266***	85.124***
	(6.486)	(4.985)	(5.039)	(7.891)
Appointed			0.214	1.197
			(0.450)	(1.033)
Citizen ideology			0.194	-0.678
			(1.365)	(1.380)
Governor Democrat			-0.266	-0.490
			(0.466)	(0.624)
Governor independent			0.619	0.610
			(0.503)	(0.576)
Revolving door restrictions			-1.438***	-2.191**
			(0.376)	(0.846)
Fuel input cost index, log (real)	6.263***	2.078*	2.088*	1.468
	(1.383)	(1.160)	(1.164)	(1.014)
Deregulation is active	-12.343***	-3.723***	-3.500***	-3.583***
	(3.207)	(1.113)	(1.125)	(1.121)
Deregulation is suspended	-14.280	-4.874	-4.626	-2.658
	(12.596)	(4.163)	(4.216)	(4.335)
Per capita income, log (real)	-81.362***	-28.962***	-31.562***	-19.303***
	(12.593)	(6.263)	(5.646)	(5.673)
State pop, log	9.531***	22.074***	23.588***	13.684***
	(2.191)	(4.145)	(3.769)	(3.716)
Fixed effects	ERC region	state	state	state & 3-year groups
Serial correlation	none	2 lags	2 lags	2 lags
Treatment of ousting	exogenous	endogenous	endogenous	endogenous
R^2	0.604			
F or χ^2 statistic (d.o.f.)	34.7 (7,50)	2,589.5 (7)	2,143.6 (12)	5,155.1 (12)
p-value	0.000	0.000	0.000	0.000
Hansen overID stat. (d.o.f.)		5.09 (4)	3.90 (4)	3.38 (4)
p-value		0.278	0.419	0.496
N	1,730	1,730	1,696	1,696
Number of instruments		11	16	27
Clusters for SE's	51	51	50	50

Notes: An observation is for a state in a year. The first test statistic is F for estimation P1 and χ^2 for the others; the latter excludes coefficients for year fixed effects in estimation P4. *P*-values are for the statistic in the row above. The second test statistic is Hansen's statistic with null hypothesis that the overidentifying restrictions are valid in the GMM estimations. A constant is included in P1. Std. errors are robust to clustering at the state level. Real variables are deflated with the CPI-U.