The Costs and Benefits of Renewable Portfolio Standards in the United States: Accounting for Policy Heterogeneity and Endogeneity

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PLAGARISM STATEMENT

I have written this project using my own words and ideas, except otherwise indicated. I have subsequently attributed each word, idea, figure, and table which is not my own to their respective authors. I am aware that paraphrasing is plagiarism unless the source is duly acknowledged. I understand that the incorporation of material from other works without acknowledgment will be treated as plagiarism. I have read and understand the Levy Economics Institute of Bard College statement on plagiarism and academic honesty as well as the relevant pages in the Student Handbook.

Ian Bowen
May 25^{th}, 2021

ABSTRACT

Renewable portfolio standards (RPS) have emerged as some of the main state-level policy tools addressing climate change. The central aim of this thesis is to investigate the costs and benefits of these policies in terms of their impacts on the share of non-hydro renewables and electricity prices, respectively. To accurately estimate these impacts, this paper argues that it is necessary to account for policy heterogeneity (i.e., differences in policy features across states) and endogeneity (i.e., the correlation between policy features and unobservable factors that affect the dependent variables). In the literature, there has been work addressing the former, and there is a modest consensus that RPS is effective when heterogeneity is considered. However, there has been little work addressing endogeneity. To address this gap in the literature, this thesis uses the instrumental variable (IV) and control function (CF) approaches to account for endogeneity and measures of RPS that capture policy heterogeneity. It compares the results from these approaches with the results of baseline regressions that account for heterogeneity but not endogeneity. In the results for the non-hydro renewable share, RPS is found to have significant impacts in the baseline but not in the IV and CF regressions. However, the validity of the results in the IV regressions depends on the strength of the instrument, which varies considerably depending on whether the instrument is lagged or if year fixed effects are included. For electricity prices, the IV approach indicates that RPS has no significant impact, while the CF approach indicates there is a significant and positive impact that is higher in magnitude than in baseline and in the literature. Due to this inconsistency between the two approaches, as well as other limitations, this thesis ends by discussing whether the results are useful for public policy. It argues that the literature on RPS has not reached the point where strong conclusions can be made about the impact of these policies.

Keywords: State Government; Renewable Portfolio Standards; Renewable Energy; Clean Energy Policy; Electricity; Instrumental Variable; Control Function

JEL Classifications: H70, Q42, Q48, Q58

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1. INTRODUCTION

In 2014, the Intergovernmental Panel on Climate Change (IPCC) released its fifth and most comprehensive assessment report to date, replete with numerous findings underscoring the urgency of climate change. These findings include unequivocal evidence for the warming of the climate system since the late 19th century. For example, the report presents multiple independently produced datasets indicating that the linear trend of global average surface temperatures has increased by 0.85 °C between 1880 and 2012. Moreover, since the early 1980s, each has successively decade has been warmer than any decade since 1850. The most significant factor driving these changes is the increasing atmospheric concentrations of greenhouse gases due to human activity. According to ice core records, these concentrations are higher than any concentration over the past 800,000 years, and over the past 22,000 years, the average rate of increase over the past century is unprecedented. The effects of these changes have already begun to be visible in both ecological and socio-economic systems.

However, perhaps more concerning than the past is the future. To assess future changes in the climate system, the IPCC has developed four "representative concentration pathways" that are each representative of a potential range of anthropogenic emissions over the 21st century. Under the scenarios with the lowest and highest emissions, by 2100, the global mean surface temperature anomalies relative to 1986–2005 are expected to increase by 1 and 3.7 °C, respectively. The scenario with the lowest emissions represents the emissions reduction that is likely necessary to keep average surface temperatures relative to the preindustrial period from surpassing 2 °C, which has become an established target for minimizing the impacts of climate change; the scenario with the highest emissions captures the cases where minimal further action is taken to mitigate future emissions. To achieve the former pathway, however, unprecedented action will be required. The consensus in the modeling literature indicates that substantial net negative emissions will be necessary by 2100. Furthermore, since, "in some regions and vulnerable ecosystems, high risks are projected even for warming above 1.5 °C" (UNFCCC, 2015), the notion of a "defense line" where warming is limited to 1.5 °C has been raised among climate experts. In 2018, the IPCC released a report focusing on this defense line, which provides modeling results suggesting that limiting warming to 1.5 °C will likely require annual emissions to reach net-zero by 2055 (IPCC, 2018).

It not clear whether the world is on track to meet these targets. Despite increasing awareness of climate change, global emissions have increased by 60% between 1990 and 2019 and nearly 10% between 2010 and 2019 (Ritchie and Roser, 2020; Global Carbon Project, 2020). The largest share of these emissions has come from the electricity and heat sectors (IEA, 2021); between 1990 and 2018 and 2010 and 2018, emissions from these sectors have risen about 83% and 12%, respectively. However, these aggregate statistics obscure important regional differences, particularly between developing and advanced economies; since 2007, growth in emissions from the electricity and heat sectors has come exclusively from developing countries, with emissions from these sectors in advanced countries falling by about 15% in large part due to increases in efficiency and decreases in the share of fossil fuels (Pavarini and Mattion, 2019).

The United States (US), the largest emitter among advanced countries, has played an important role in these trends. Between 2010 and 2019, power plant CO_2 emissions in the US have fallen 38.5% (EIA, 2020). Part of the reason for this decline is that while net generation has been nearly flat, the power mix has been changing substantially, with coal being replaced by less carbon-intensive sources. For example, over the period, net generation from wind power has increased by about 212%, bringing its share of total net generation to approximately 7.2%. Solar power, beginning from a negligible share of the US power mix in 2014, has since grown 270% and now supplies approximately 2.6% of the total load.

These developments have followed numerous policy actions to address climate change at a state level – in particular, renewable portfolio standards (RPSs). In general, RPSs require electricity providers to supply some percentage of their retail sales with energy from renewable sources. However, the details in these policies differ significantly across states. For example, states have adopted targets with different levels of stringency and different compliance mechanisms. These and other forms of policy heterogeneity are discussed in detail in Chapter 2.

The fact that there have been considerable reductions in electricity sector emissions and increases in renewable penetration following the implementation of RPSs does not mean there has been a causal link. It may be that the same outcomes would have been present if the RPSs had never been implemented. To determine whether this is the case, there have been numerous econometric studies measuring the effectiveness of RPSs, usually in terms of the effect on renewable penetration but also on emissions and renewable capacity. Furthermore, there have been studies examining what the costs of these policies are, particularly the impact of electricity prices. To measure these effects, a common approach has been to assume that variables capturing the RPSs are exogenous – that is, uncorrelated with the error term of the model. In the effectiveness literature, the consensus with this approach now appears to be that when the heterogeneity of RPSs is considered, the policy coefficients are positive and significant. In the cost literature, the policies are generally estimated to have statistically significant positive impacts on electricity prices, though there has been less work on accounting for policy heterogeneity.

An alternative to the exogeneity assumption is to assume RPSs are endogenous and explicitly model policy adoption. As discussed in Chapter 3, there are several theoretical reasons to believe that endogeneity is present. This paper argues that there are three channels affecting the estimation of the effectiveness of RPSs that stem from the role of special interests that could either benefit or be harmed by RPSs. Furthermore, reverse causality could affect the estimation of the effects of RPSs on costs. However, while several papers have modeled policy adoption alone, there has been little work where this has been done to account for endogeneity.

This paper aims to fill this gap in the literature. It provides estimates of the costs and benefits of RPSs, measured by the impacts on electricity prices and the share of non-hydro renewables, respectively. This is achieved by using a panel dataset with comprehensive data on policy features and two methodologies that account for endogeneity: the instrumental variable and control function approaches.

Many relevant questions stem from these estimates. In particular, how should policymakers and other stakeholders interpret these costs and benefits? After the results are presented in Chapter 4, Chapter 5 investigates this question. It proposes that to address this question, two additional questions must be answered. First, how should the findings in this study be interpreted if they are accurate? Second, how strongly should stakeholders weigh the uncertainty regarding the accuracy of the findings? Two potential answers to the first question are proposed. To answer the second, this paper notes that there are two aspects of the literature on RPSs that undermine the strength of its findings. As a result, the empirical research on RPSs is not ready to provide strong policy recommendations.

2. LITERATURE REVIEW

This section is divided into four subsections. In addition to providing background information on RPSs, the first subsection aims to demonstrate the considerable heterogeneity in these policies across states. This subsection is followed by three subsections on research on different aspects of RPSs. The first of these deals with the factors driving states to adopt these policies; as the methodology used in this paper explicitly models policy adoption, this paper draws upon this literature in the choice of control variables for the models presented in Chapter 3. The next subsections address the effectiveness of the policies and their costs, which is followed by a discussion of the limitations of the literature. Since one of the main contributions of this paper is methodological, special attention is paid to how the methodologies used in the literature have evolved to demonstrate how the methodology of this thesis fits into that evolution. In particular, it focuses on how these methodologies have and have not accounted for policy heterogeneity and endogeneity.

2.1. Background on Renewable Portfolio Standards

The first state to implement an RPS was Iowa in 1983, with the *Alternative Energy Law.* More than a decade, however, passed before any other states implemented an RPS, and it was not until the mid-2000s that adoption became widespread; as of 2019, 29 states plus DC have implemented some kind of RPS, with more than half coming between 2004 and 2009 (Barbose 2019). Most of these states have also updated their policies at least once over the past two decades.

In addition to the heterogeneity in adoption and revision dates, the details of these policies vary across states. One important dimension is the scope of the definition of the load-serving entities (LSEs) subject to the policy. For example, under New York State's *Clean Energy Standard*, the relevant LSEs include any investor-owned utility, municipal utility, electric cooperative, or retail supplier (DSIRE, 2020a). Other policies have narrower definitions, such as Missouri's *Clean Energy Act*, which only applies to investor-owned utilities in the state (DSIRE, 2018).

The policy targets also vary across states. First, the policies can target a share of generation or capacity. Most states require the former; only Iowa and Texas exclusively require the latter (Shields, 2021), which only indirectly affects the amount of renewable electricity produced. Furthermore, some states, such as Michigan, have both types of targets. Second, policies vary in the ambitiousness of their targets. Maine, for example, has a target of 84% of retail sales by 2030, while Connecticut has a target of 44% by 2030 (Barbose, 2019).[1] The policies also typically create a compliance schedule, which provides a set of annual targets leading up to the ultimate target; however, these annual targets may not increase at a constant rate per year.

To demonstrate compliance, states typically require the LSEs subject to the policy to accumulate renewable energy credits (RECs) that are equivalent to the relevant policy targets. These credits represent one megawatt-hour of renewable energy generated by LSEs. Some states also allow for REC trading; the LSEs that generate the RECs can sell them to others, potentially in other states, who can present them to authorities to meet the standard. As noted by Davies (2011), this mechanism "is directly analogous to other market-based forms of environmental regulation that permit one polluting business to achieve compliance not by reducing its own waste, but rather, by paying another polluter to trim more than its own share." This mechanism, therefore, introduces market forces into what would otherwise be a command-and-control policy.

In some cases, the pricing of RECs may also be determined in markets. In recent years, there has been a significant downward slide in REC prices in many states, particularly in the Northeastern US (Barbose, 2019). Other states, such as New York, fix the prices of their RECs. The RECs

[1] These figures are computed based on the sum of the applicable resource tiers

may also have a set lifespan; for example, in Maryland, a REC has a three-year lifespan after being generating (DSIRE, 2021).

The eligible technologies in RPSs are typically broad. They include solar, wind, hydro, geothermal, tidal, and various forms of waste-to-energy and biomass technologies, and they may include distributed generation, energy efficiency, and nuclear technologies. Policies may apply differential weights to these various technologies. Part of the reason for this is that to comply with the standards mandating a specific amount of generation or capacity, suppliers have often opted for the cheapest and most mature technologies, like wind, thereby neglecting technologies like solar (Kim and Tang, 2020); these short-term incentives can lead to grid reliability issues by locking-in certain technologies.

States have employed two primary mechanisms to combat this lock-in. First, states may have different targets – also referred to as carve-outs or set-asides – for each technology. Solar carve-outs are most common due to the short-term factors mentioned above; as of 2019, 15 states plus Washington, DC, have carve-outs for solar or distributed generation (Barbose, 2019). A related provision is the creation of tiers, with a primary tier for new resources and a secondary tier for resources that predate RPS and other less preferred technologies. The prices of RECs associated with these different categories of resources tend to differ systematically; the carve-outs often have the highest prices and the secondary tiers the lowest (Barbose et al., 2015). Second, states may use credit multipliers, which award more than one REC to each MWh generated by the preferred technologies. However, some states, such as Massachusetts and Nevada, have attempted to discourage technologies by awarding a fraction of a REC to certain technologies. Once again, solar technologies are typically given the greatest weight.

Eligibility rules can also affect the volume of new investment. Existing resources can reduce investment in new resources as these existing resources allow the LSEs subject to the policy to meet a portion of the requirement without any additional investment (Yin and Powers, 2010). Some have argued that granting eligibility to existing hydro resources results in a particularly significant reduction in the incentive to invest in new resources (Fischlein and Smith, 2013).

An LSE that does not meet the compliance requirements through power purchase agreements or RECs is typically subject to an alternative compliance payment (ACP) paid to state authorities. Wiser et al. (2010) note that this mechanism should not be understood as a financial penalty, as it is considered by authorities to be a legitimate channel for compliance with RPS requirements, and the costs of ACPs can sometimes be recovered by utilities in their rates. Penalties for non-compliance also exist. For instance, Washington imposes a penalty of $50/MWh, adjusted for inflation annually, for each MWh below the requirements (DSIRE 2020b). Some states also have separate ACPs for each resource tier and set-aside; solar ACPs, in particular, exist in numerous states as an alternative to meeting the solar set-aside (Wiser et al., 2010). The schedule for ACPs also varies; while some states, such as New Jersey and Massachusetts, have declining ACPs, states such as Connecticut have a constant ACP. The funds generated by the ACPs are often used to finance renewable energy projects.

States may also incorporate cost containment measures into their RPSs. There is a significant amount of heterogeneity in these measures (Stockmayer et al., 2012). Some states limit RPS compliance expenditures by capping the ratio of incremental costs attributable to the RPSs to utilities' expected annual revenues. Other states limit compliance expenditures based on the amount they are expected to impact consumer electricity bills. Others provide provisions for regulatory agencies to provide compliance waivers when deemed reasonable.

2.2. Research on the Adoption of Renewable Portfolio Standards

2.2.1. The Role of Interest Groups

Some researchers have suggested that interest groups could influence the probability of RPS adoption (Vachon and Menz, 2006; Lyon and Yin, 2010; Jenner et al., 2012a, 2012b, 2013). The theory in support of these arguments draws from the private interest theory of regulation developed by the Chicago School (Stigler, 1971; Peltzman, 1976; Becker, 1983). According to this theory, the state is a competitive domain in which various interest groups vie for regulation that promotes their interests. Typically, it is argued that these private interests are contrary to the interests of the public (e.g., by showing that regulation results in deviations from Pareto efficiency).

On the one hand, organized renewable energy interest groups in a state could have an incentive to advocate for RPSs as a means to strengthen their presence in the state; as argued by Rabe (2007), renewable energy developers have become increasingly present state legislative processes, and in some states, they have an even stronger impact on RPS debates than conventional environmental advocacy groups. On the other hand, fossil fuel interest groups could have an incentive to advocate against RPSs, as it could undercut their market share. Rabe and Mundo (2007) argue that there is often a reluctance among policymakers to challenge such interest groups and that this reluctance may partially explain why states relying heavily on non-renewable resources (such as Indiana, Michigan, Arkansas, Florida, Georgia, Kentucky, Mississippi, and South Carolina) have lagged in other parts of the nation in adopting RPS. As an example of this dynamic, in Colorado, RPS legislation was blocked by a coalition led by utilities and coal-mining interests in three consecutive sessions of the Colorado state legislature (Rabe, 2007). Nevertheless, an alternative coalition eventually managed to amass the 100,000 signatures required for a ballot proposition on RPS adoption. This was fiercely opposed by the state's predominant utility, the Public Service Company (PSC) of Colorado, which is a subsidiary of Xcel Energy, a Minnesota-based utility holding company. The PSC

> *spent more than $1.5 million in leading the campaign to oppose [the proposition] through an organization called Citizens for Sensible Energy Choices. This organization emphasized its concerns about potentially high costs that would be transferred to customers and the willingness of PSC to expand its own renewable offerings on a voluntary basis. The company was also clearly concerned about the impact of the proposition on its plans to build a massive coal-burning plant near Pueblo.* Rabe and Mundo (2007)

However, as Rabe and Mundo (2007) note that even when states decide to adopt an RPS, special interests do not abandon efforts to influence the process. These interventions have, in some cases, diluted policies that initially targeted renewable energy development by adding traditional energy sources to the list of eligible technologies. Thus, special interests not only affect RPS adoption but also the *stringency* of policies and thus heterogeneity across states. As an example, Rabe and Mundo discuss the influence of business interests on Pennsylvania legislators during the development of the *Alternative Energy Portfolio Standards Act.* They state that as "the legislation worked its way through the Pennsylvania legislature during 2004, [it] acquired so many environmentally suspect provisions that it ultimately became the first state RPS to be enacted in the face of active opposition from a large range of environmental groups." For example, the process resulted in a broadening of the definition of "renewable energy" to include decades-old waste coal in landfills, a decision linked to plans to build three power plants powered by such coal. Other sources that became classified as renewable were coal-mine methane and energy produced by incinerating trash and poultry waste.

Some empirical studies have supported the theory that special interests influence the probability of RPS adoption. Delmas and Montes-Sancho (2011) and Lyon and Yin (2010) find that the presence of organized renewable energy interests in each state has a significant impact on the probability that states adopt an RPS, measured as a binary variable. In the latter paper, renewable special interests are measured with a binary variable indicating the presence of staffed American Solar Energy Society (ASES)[2] chapters in each state; this also indicates how well renewable energy interests are organized. Furthermore, they use existing renewable capacity as a proxy for the foothold of renewables interests, as these generators have an incentive to preserve their market share with RPS. Finally, they include the fossil fuel interests that stand to lose from RPSs, which are measured by oil, natural gas, and coal industry employment per capita in 2002 and the (lagged) percentage of natural gas generation. Of these variables, only the presence of ASES chapters and the percentage of natural gas generation have significant effects, which are positive and negative, respectively. They also run a multinomial model to measure the impact of special interests on the adoption of policies with in-state requirements (e.g., REC multipliers for energy produced in-state). None of the variables above have significant effects.

However, Jenner et al. (2013) argue that that the measurement of organized renewable interests with a binary variable (i.e., the presence of ASES chapters) fails to capture the heterogeneity of different interests due to differences in their financial campaign contributions to politicians. That is, they argue that "money matters" – the magnitude of a special interest group's financial contribution is positively correlated with the likelihood that it will be able to successfully

[2] The authors indicate that the ASES advocates for all renewables, not just solar.

influence policy. Thus, they include in their regressions data on the magnitude of financial contributions by both conventional energy interests and renewable energy interests. To account for the fact that the magnitude of donations varies due to the timing of elections and the impact of inflation, they use the ratio of the contributions of these groups to total contributions. Using a proportional hazard model, they find that, consistent with expectations, the conventional energy contribution ratio has a statistically significant negative association with the probability of RPS adoption while the renewable energy contribution ratio has a statistically significant positive association. They also attempt to account for the impact of financial contributions on policy heterogeneity. To do so, they use a Tobit model with a measure of stringency from Yin and Powers (2010) as the dependent variable. In this case, they find that the renewable energy contribution ratio has a statistically significant impact while the convention energy contribution ratio does not.

2.2.2. Public Interest Theory (or Sentiment?)

Unlike the private interest theory, which views the state as a forum in which special interests compete to maximize their individual benefits, regardless of whether they maximize social benefits, the so-called positive public interest theory states governments adopt regulation in order to maximize social welfare. This theory has been explicitly cited by several authors studying RPS adoption (Ciocirlan, 2006; Lyon and Yin, 2010; Jenner et al. 2012b; Helwig, 2014).

Following Posner (1974), it has become common to juxtapose the Chicago School private interest theory of regulation with the public interest theory. According to this argument, the public interest theory is effectively a “normative as positive” theory of regulation (Joskow and Noll, 1981). In other words, the theory predicts that the state will adopt the optimal forms of regulations prescribed by normative welfare economics. Posner argued that this positive theory rests on two assumptions: 1) market failures are pervasive and 2) the transaction costs of regulation are non-existent or minimal. It was then argued that these assumptions do not hold in practice and many regulations in the real world fail to align with what is predicted by the public interest theory.

However, it might not be that there has ever been a positive public interest *theory*. Rather, as argued by Hantke-Domas (2003), it may be that there have only been political and legal appeals to the public interest, which do not constitute theories per se. Hantke-Domas documents these appeals throughout the case law and political discourse of Britain from the 17th century to the 20th century and in the US during the Progressive and New Deal eras, the latter capturing much of the period in which Posner argues the public interest "theory" was dominant. However, Hantke-Domas argues that in Posner (1974) and the papers that followed, there have been few citations of the alleged progenitors and followers of this theory. He argues that in the process, Posner and others have conflated normative welfare theory with a positive theory of regulation.

It is beyond the scope of this thesis to provide an evaluation of the history of economic thought surrounding this topic. But the suggestion by Hantke-Domas that the public interest "theory" is separate from the political or legal appeals to the public interest has ramifications for research on RPS adoption; it is not clear how or whether the "normative as positive" public interest theory would be used to explain the adoption of RPSs. For example, welfare economics generally advocates in favor of price-based approaches to internalize externalities (Tresch, 2014), not quantity-based approaches. If the *appeals* to the public interest are instead used as an explanation, there is no need to align the policy with any particular arguments made by welfare economics concerning optimal regulation. While this may be unsatisfying for those looking for a theory as such to explain RPS, it provides for greater consistency with real-world political and legal discourses.

In the case of RPSs, two aspects of the public interest could influence policymakers' willingness to adopt an RPS: environmental and economic factors. Some have argued that over time, the former has taken a back seat to the latter. As Davies (2011) notes,

> *[i]f RPSs started out as a way to promote environmentally friendly energy, the trend is largely a historical one. Listen to any press conference on the signing of an RPS today, and it quickly becomes clear that these laws' goals are increasingly lofty. The environmental protection aim remains, but many others have been injected as well…. [In*

particular,] the theme that has perhaps most clearly emerged from their adoption in the last two decades is economics.

Rabe (2007) concurs with this assessment, noting that there is a perception that promoting renewable energy via RPSs yields can promote economic development, which partially explains the growing diverse coalitions supporting them. Part of the linkage between development and renewables he identifies is that labor costs constitute a larger share of the total costs of renewables than of fossil fuels; therefore, the argument goes, RPSs can boost employment.[3] Furthermore, he notes that economic development can be spurred by reducing dependence on imported fossil fuels.

Empirical studies have found that several economic and environmental factors influence the probability of adopting RPS. These include a positive relationship with gross state product per capita (Chandler, 2009; Delmas and Montes-Sancho, 2011),[4] perhaps because policymakers consider the capacity of their constituents to absorb the costs of the programs (Matisoff, 2008). However, other studies have found no significant relation with gross state product per capita (Huang et al. 2007; Matisoff, 2008; Lyon and Yin, 2010; Carley and Miller, 2012).[5] As for environmental factors, studies have used variables capturing state-level emissions, with some finding a positive effect on adoption (Delmas and Montes-Sancho, 2011) and others finding no effect (Lyon and Yin, 2010).

2.2.3. Internal and External Determinants Models

Another approach that some researchers have used to analyze the determinants of RPS adoption is to separate factors internal to states from external factors. Many of the aforementioned factors can be classified as internal factors (e.g., economic conditions and pollution). In addition to these factors, relevant determinants include political and citizen ideology, since liberal voters tend to be more likely to support state action supporting renewables, and resource endowments, since

[3] Of course, this is not a comprehensive analysis of the employment impacts of renewables. But if this argument is widely held by policymakers, then this can explain some of the support for RPSs.
[4] Delmas and Montes-Sancho (2011) also find a significant negative effect with one model.
[5] The results in Huang et al. (2007) and in one model from Carley and Miller (2012) are significant at the 10% level. In both cases, the coefficient is positive.

states with greater endowments of renewables may be more able to successfully implement these policies. Variables capturing ideological factors that have been found to influence RPS adoption include the percentage of Democrats in state legislatures (Lyon and Yin, 2010; Delmas and Montes-Sancho, 2011) and proxy measures of citizen ideology (Matisoff, 2008; Carley and Miller, 2012) – in particular, a measure based on Berry et al. (1998).

As for external factors, there is large longstanding literature on state policy diffusion – originating with Walker (1969) – that has been linked to the RPS implementation literature (Carley and Miller, 2012; Carley et al., 2017). Researchers have examined whether states are more likely to adopt an RPS when a higher percentage of their neighbors also have an RPS in place; some papers have found no effect when the RPS adoption decision is a binary variable (Matisoff, 2008; Carley and Miller, 2012), while other have found that this variable has a significant positive effect with some models (Carley et al., 2017).

2.3. Research on the Effects of Renewable Portfolio Standards

There is a wide range of potential costs and benefits due to RPSs. On the benefit side, some studies have examined the impact of RPSs on green jobs, with some studies finding a positive impact (Wiser et al., 2016). Others have examined the impact on carbon intensity; in some of these studies, the impact is significant and negative (Yi, 2015), while others fail to find significant impacts (Greenstone and Nath, 2019). However, studies on the benefits of RPS have tended to focus on the impacts on renewables development.

The costs of RPSs primarily stem from their impacts on renewables development, and as with the benefits of RPS, there are several ways to measure these costs (Barbose, 2019). First, in terms of the levelized cost of electricity,[6] renewables have generally been more expensive than conventional sources, though this is changing; thus, increasing renewables can impose direct costs to LSEs subject to the policy. Second, since renewables are more intermittent than conventional sources, increasing penetrations of renewables result in socialized balancing costs, due to the management of increased forecast errors and balancing reserves (Wiser and Bollinger,

[6] The levelized cost of electricity is the present value of the various costs of generating electricity over the lifetime of a plant divided by the present value of the energy generated.

2019). Third, increased penetrations of renewables may also require transmission and distribution upgrades that are socialized.

This thesis focuses on two aspects of costs and benefits: electricity prices and the non-hydro renewable share, respectively. For the remainder of the section, the literature on each of these factors will be surveyed, beginning with the impact on the non-hydro renewable share. Since this variable is often the primary target of RPSs, the impacts of RPS on this variable will be viewed as an indication of policy effectiveness.

To evaluate effectiveness, the outcomes of states that have implemented RPSs can be compared to 1) the targets set out by legislation and 2) an estimate of the counterfactual in which the policies have not been implemented. The former is necessary at a minimum to determine whether the policies satisfied the goals set out by policymakers; furthermore, it can help determine whether the ACPs are too low to incentivize investment by LSEs or provide the revenue to state governments for investments. However, this form of evaluation does not indicate whether the same outcomes would have been achieved in the absence of the policies. Thus, econometric studies can be employed to provide the second form of evaluation, though true counterfactuals are impossible to determine.

The first form of evaluation is provided by Barbose (2019), who compares, for the period from 2000 to 2018, the actual growth in non-hydro renewable energy generation to the minimum amount of generation necessary for compliance. Between 2000 and 2007, the two grew in step by approximately 25 TWh. But after 2007, growth in actual generation exceeded growth in required generation, and the difference between the two has been increasing over time. As a result, from 2000 to 2018, actual generation has increased by 371 TWh while required generation has grown by 168 TWh. Thus, for the country as a whole, the targets for generation have been met.

Attempts to provide an econometric evaluation of RPSs, and thus estimates of a counterfactual, began in the mid-to-late 2000s following the proliferation of RPSs. Many of the early papers on this subject tended to capture the presence of RPSs with a binary variable. The initial results were mixed, with some providing estimates indicating that RPSs were effective in boosting either the level or share of renewable generation and others indicating that they were not effective in achieving their desired target.

Menz and Vachon (2006) is one of the earliest econometric papers on this topic. It provides a set of highly parsimonious cross-sectional models estimating the impact of several renewable energy policies, including RPSs, on four indicators of wind development: 1) capacity in 2003, 2) capacity growth between 2000 and 2003, 3) capacity growth between 1998 and 2003, and 4) the number of large projects. The results of these models estimate that, all else equal, RPSs measured with a binary variable have significant (at the 5% level) positive effects on dependent variables 1 and 3. With the number of years the RPSs have been in place as the RPS indicator, RPSs have a significant (at the 1% level) effect on dependent variables 1, 3, and 4. Carley (2009) builds on these results by using a significantly larger panel dataset with a more complete set of the controls – many of which appear in studies that followed, including this thesis – accounting for state-level economic factors (e.g., gross state product per capita), electricity market characteristics (e.g., whether a state has deregulated its electricity markets), renewable potential, and demographic factors (e.g., population growth). Furthermore, two models are used (a fixed-effects linear model and a fixed-effects vector decomposition model), and the (log of the) share of renewables is used as a dependent variable, as boosting the share of renewables is often the explicit goal of the policies. Interestingly, the results suggest that, with both models, RPSs do not have a significant impact on the share of renewables, but they do have a significant impact on capacity. That is, while the policies are not achieving their explicit goals in most cases (i.e., the share of renewables), the models suggest they are having some impact on the development of renewables.

Some early attempts to account for heterogeneity used the generation requirements dictated in RPS legislation as the independent variable. Kneifel (2008) and Shrimali and Kniefel (2011) include three binary variables that identify whether a state has a mandatory renewable sales requirement, a voluntary sales goal, or a renewable capacity requirement. They also differ from the previously mentioned papers in that they estimate models with the capacity shares of several renewable sources (i.e., wind, biomass, geothermal, and solar) as the dependent variable, in addition to total the renewable capacity share. They find that most RPS variables have either significant negative or insignificant impacts on most dependent variables; the exceptions are the significant effects of the mandatory renewable sales requirement on geothermal and solar, and a modestly significant (i.e., at the 10% level) effect of the capacity requirement on geothermal. However, they find that that excluding Maine eliminates the negative coefficient for the impact of the sale requirement on the total renewable capacity share. A related approach to account for the heterogeneity of nominal requirements is the trend-break difference-in-difference model adopted by Greenstone and Nath (2019). With their approach, they find mixed results, with RPS only having significant results on generation from some sources in some specifications.

Yin and Powers (2010) is a seminal paper in the literature as it more comprehensively addressed heterogeneity than any of the research before it. The paper focuses on four sources of heterogeneity, each of which is discussed in Section 2.1: coverage, or the types of LSEs subject to the requirements; eligibility of existing capacity, which makes the nominal requirements written into law a misleading indicator of stringency; whether in-state and out-of-state RECs are treated equivalently; and whether an ACP is included. The first two of these requirements are captured in a variable referred to as the incremental percentage requirement (IPR) – that is, the required increase in the share of renewables in total generation, which is adapted or replicated exactly in much of the subsequent literature. For the remaining two features, binary variables are used. The results of the regressions with these variables provide evidence that the heterogeneity of policies has a significant impact on measurements of their effectiveness, which they measure as the impact on the share of non-hydro renewable capacity. In regressions without the REC free trade and ACP binary variables and with either the RPS binary variable or the cumulative years the policy in each state has been in place as dependent variables, the predicted impact of RPSs is not significant. When the nominal requirement is used to represent RPS, the impact is negative and significant, which they note is driven entirely by idiosyncratic factors in Maine, as in Shrimali and Kniefel (2011).[7] However, when the IPR is used to represent RPS, the result is

[7] Maine's RPS took effect in 2000, but due to the eligibility of existing resources, they note that was possible to meet the nominal requirements without new investment. Immediately after taking effect, two biomass plants were retired and several natural gas plants became operational, thereby causing a fall in the share renewable capacity immediately after implementation. Yin and Powers note that when the regression is run without Maine, the nominal requirements variable no longer significant, but the remaining results in the paper are unaffected by the exclusion of Maine.

positive and significant. In other specifications that include the REC free trade and ACP binary variables, the positive and significant result for the IPR is unchanged. Furthermore, they find that the ACP variable is insignificant, and the REC free trade variable is negative and only significant in one of their specifications.

Following Yin and Powers (2010), several papers aimed to account for policy heterogeneity with similarly comprehensive methodologies. Shrimali et al. (2015) used the IPR and seven other RPS policy features to estimate the impact of RPSs on the renewable capacity share. They find that, in the specifications where only IPR is used to measure RPSs, the coefficient of IPR is positive and significant when controls are used. However, when they omit Maine, for the reasons mentioned in Yin and Powers (2010), they find that the IPR is no longer significant. They argue that this could be because the IPR alone does not sufficiently account for heterogeneity. When they include the other policy features (including variables representing the size of regional REC markets and the number of neighboring states with RPSs), the IPR and many of the policy features are significant. Similar results are provided in Carley et al. (2018), who use a similar RPS stringency index, which represents the mandated increase in the renewable generation share, divided by the number of years before the ultimate target must be met, multiplied by the total load subject to the policy. They also use they use a novel set of policy features and dynamic factor model estimation algorithm to create an index representing the stringency of RPSs based on seven of these features; these variables will be discussed further in Chapter 3, as this paper uses these variables as well. Their results show that the RPS stringency index and the dynamic factor index have significant positive impacts on the (logged) shares of renewable generation and capacity; in specifications with the other policy features, the variables representing cost recovery and RPS planning activities prove to have positive and significant effects on the same dependent variables.

Upton and Snyder (2017) make a novel contribution to the literature by using the synthetic control approach to control for non-random selection of RPS. For each state that has implemented RPS, this approach creates “synthetic control states” based on a weighted average of the political, economic, and natural resource endowments of the states that each state most resembles. The outcome with this approach is compared with the results from a standard difference-in-difference model. Using a binary variable to measure RPSs, their estimates indicate that RPSs have no impact on renewable generation. They also attempt to account for heterogeneity as a robustness check using the stringency measure from Carley and Miller (2012), but they find that this also does not yield significant results.

Similar to the research on effectiveness, research on the electricity price impacts of RPSs has taken non-econometric and econometric forms. Barbose (2019) provides "rough" estimates of the electricity rate impacts of RPSs by computing the net compliance costs to LSEs as a percentage of retail electricity bills. Three limitations of this approach are cited. First, they use a rather limited approach to computing compliance costs. For retail choice states, this is computed using REC prices plus ACP expenditures, while for vertically integrated states, they use cost estimates by utilities and public utility commissions, which are then compared to an estimated counterfactual based on market prices or projections. In addition to the lack of a unified methodology for vertically integrated states, this definition of compliance costs misses many of the potential costs and benefits of RPSs mentioned above. Second, the compliance costs based on this definition may not be fully passed through to electricity rates. Third, they note that "ACPs may be credited to ratepayers or recycled through incentive programs.” Nevertheless, for RPS states taken together (with appropriate weighting), the report estimates that costs as a percentage of retail bills have risen from 0.7% in 2012 to 2.6% in 2018, with large variability across states. The wide variability is attributed to several RPS design characteristics and state-specific factors, such as RPS target levels, resource tiering, and wholesale electricity prices.

Morey and Kirsch (2013) provide the earliest econometric estimates of the electricity rate impacts of RPSs in 48 states from 1990 to 2011. Separate impacts are estimated for residential, commercial, and industrial rates. The RPS variable is binary, which is also interacted with a binary variable indicating whether a state has retail access. According to their results, the RPS binary variable alone only had a significant impact on residential rates; the coefficient indicates that, all else equal, the average increase in retail rates based on 2011 US average retail prices is 3.8%. The coefficient for the interaction with the retail access variable is also significant when industrial rates are used as a dependent variable; since the coefficient for the RPS variable is not significant, this indicates that there were only significant impacts on industrial rates in retail choice states.

This study was followed by Tra (2015), which uses a difference-in-difference framework and utility-level data. Unlike Morey and Kirsch (2013), Tra (2015) notes that there may be endogeneity due to unobserved factors that impact both electricity prices and RPS adoption. The paper argues that the inclusion of state-by-year and utility-type-by-year fixed effects terms will be sufficient to control for this. While this may be the case, it is likely not the most efficient approach. Nevertheless, the results are similar to those in Morey (2013): RPSs, measured with a binary variable, are estimated to increase average residential and commercial rates by about 3%, but the commercial rate impact is only significant at the 10% level. Tra (2015) also provides an estimate of the impact of changes in the percentage requirements. When variables accounting for time-invariant factors are included, increases in requirements are estimated to have significant impacts on both residential and commercial rates, but this effect disappears in specifications accounting for time-invariant factors.

Wang (2016) is another difference-in-difference study – though using state-level data – that acknowledges the heterogeneity of RPSs. To account for this, the regressions include variables representing the years the RPSs were enacted and became binding and effective. Wang notes that, for example, "[a]lthough electricity producers can be forward looking, they might not immediately take any action to meet the RPS requirements when the policies are just enacted." In the models with each of the RPS variables alone, the results indicate that the binding year tends to yield a larger percentage increase in residential electricity rates (i.e., around 6.5 to 7.5% for models with controls) than the enactment and effective years (approximately 5% for models with controls).

Two other studies estimating electricity rate impacts are Greenstone and Nath (2019) and Upton and Synder (2017). The former provides some strong critiques of the limitations of the approach to electricity price impacts taken by Barbose (2019) noted above.[8] They estimate that seven years after initiation, the average retail price increase is around 11.1% over the price prior to implementation. Similar results are presented in Upton and Synder (2017), who estimate RPSs increase electricity prices by 11.4%, on average.

2.4. Limitations of the Research on Renewable Portfolio Standards

Many of the early papers on the effectiveness of RPSs employed models that abstracted away from much of the policy heterogeneity mentioned in Section 2.1. In the case of Menz and Vachon (2006), the model also likely suffers from omitted variable bias, as well as other forms of endogeneity, due to its simplicity. It also lacks an explicit time dimension due to the use of cross-sectional data. Furthermore, the dependent variables under study are not aligned with most states' RPS targets; that is, most states target the *share* of renewables, not capacity. While papers that followed (e.g., Carley, 2009; Shrimali and Kniefel, 2011; Shrimali and Jenner, 2013) rectified some of these limitations, they also have some shortcomings. First, they either treat RPS as a binary variable or have a limited treatment of heterogeneity. Second, though their comprehensive sets of control variables reduce the potential for omitted variable bias, there are still likely other sources of endogeneity; some of these will be discussed in Section 3.1. While some papers in the literature address the first limitation (Yin and Power, 2010; Shrimali et al., 2015; Carley et al., 2018), these papers do not address the second shortcoming.

The econometric literature on the electricity price impacts of RPSs differs from that on adoption and effectiveness in two important ways. First, there are fewer studies on electricity price impacts. Second, though the initial research on all three topics treated RPSs as a binary variable, the cost literature has not treated the heterogeneity of RPSs as meticulously as the effectiveness literature has. Thus, these papers may fail to adequately capture the effects of RPSs. The cost literature is similar, though, in that it also does not comprehensively address the potential for endogeneity.

[8] That being said, many of these limitations are noted by Barbose. Furthermore, the paper seems to argue that there had not yet been any econometric studies of electricity price impacts as of 2019, as none of the econometric studies in this section are cited.

However, in some papers, the potential for endogeneity is acknowledged. Shrimali et al. (2015), for example, note that their results may be biased due to two endogeneity channels:

> *[First,] states with faster growing [renewable energy] deployment also have stronger renewable energy political lobbies who can effectively strengthen RPS policies; and [second,] states more likely to deploy [renewable energy] technologies in the future enact RPS policies in anticipation of deployment, perhaps because anticipated [renewable energy] deployment lowers the political cost of enacting an RPS.*

That being said, they argue that the first channel is unlikely to be operative because the penetration of renewable energy is low and thus, they argue, the renewable energy lobby is likely to be weak. Furthermore, they note that "there is little evidence that RPS policies have achieved political acceptance due to anticipated [renewable energy] deployment that would have occurred even in the absence of the enacted RPS." However, as mentioned in Section 2.1.1, the lobbying power of renewable energy producers may be considerable. In addition, Shrimali et al. (2015) do not acknowledge the potential endogeneity stemming from lobbying by those who would lose from RPS adoption (e.g., fossil fuel producers).

There are a few papers in the literature that attempt to account for endogeneity. Delmas and Montes-Sancho (2011) is the earliest paper.[9] They note that "the decision to adopt a renewable policy, such as RPS, and investments in renewable capacity [are] likely to be influenced by the same factors" and that prior studies had not controlled for this. The potential confounding variables they suggest include differences in renewable endowments and differences in the motivations of policymakers. To control for these effects, they use a two-stage model based on Heckman (1978), which one can think of as explicitly modeling endogeneity. The first-stage regression is a binary logit model, which provides an estimate of the probability that each state adopts the policy in each period. The resulting predicted values are then included in the second

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[9] However, the authors do not explicitly use the term endogeneity. Instead, they argue that the results of these early papers likely suffer from "sample selection bias." This is a rather confusing use of terminology, as sample selection bias is usually used to refer to cases where a sample of a population overemphasizes certain groups. It could be that they are using this term to indicate that certain realizations of the stochastic processes under question are being systematically overrepresented because of omitted factors that affect both RPS and renewables investment. But if this is their intention, then it would be more intuitive to refer to this as endogeneity.

stage, which uses a Tobit model to estimate the impact of the policy on renewable capacity. Furthermore, in the second stage regression, they include an interaction between the RPS variable and a binary variable equal to 1 if a utility is investor-owned and 0 if it is publicly owned; this interaction is included because the authors hypothesize that investor-owned utilities are more responsive to renewable policies than publicly owned utilities. The results with the predicted values from the first stage are then compared with the results using the observed values. With the latter, the RPS binary variable is not significant alone, but the coefficient of the interaction is significant and positive. However, with the predicted values, the coefficient of the binary variable alone is significant and *negative* in the models with and without the interaction; the coefficient of the interaction is significant and positive, with a coefficient that is greater in absolute value than that for the binary RPS variable, resulting in a net positive effect for investor-owned utilities. Thus, they conclude that their results suggest that after correcting for endogeneity, RPSs have a positive effect for investor-owned utilities but a negative effect for publicly owned utilities.

While the attempt to control for endogeneity by Delmas and Montes-Sancho (2011) is a welcome development, their approach has several limitations. First, they measure the presence of RPSs with a binary variable. As shown in Yin and Powers (2010), while using the RPS binary variable yields a negative coefficient, replacing this variable with the IPR results yields a positive coefficient. In addition, Fischlein and Smith (2013) note that the failure to incorporate policy heterogeneity explains why they find that RPSs are only effective for investor-owned utilities; rather than this difference being due to differences in the governance of investor-owned and publicly owned utilities, it is likely because 11 states completely exempt publicly owned utilities from the requirements and six others impose weaker requirements on them. Second, it is not clear what variable they are using as an instrument. In the text, there is no discussion of the motivations for choosing an instrument. Since all variables in the first-stage regression appear in the second-stage regression, the reason for this omission appears to simply be that they did not choose an instrument. As a result, it is impossible to interpret their results in the second-stage regressions. Third, their use of a logit model as opposed to a hazard model in the first-stage model "appears to assume that the RPS is up for reconsideration each year" (Lyon, 2016), which may only be the case if one accounts for heterogeneity. Fourth, they use a two-stage predictor substitution approach, which, as discussed in Section 3.3 of this thesis, is inconsistent when nonlinear models are used.

Hitaj (2013) is another one of the few papers that attempts to address the endogeneity problem, but since it does so for a set of five policies supporting wind generation, problems arise that prevent the RPS endogeneity problem from being addressed. Specifically, the paper includes six instruments because the model has six endogenous variables, five of which are policies. When all the endogenous variables are instrumented simultaneously, the exogeneity tests indicate that the variables are endogenous at the 1% level. However, when each variable is instrumented individually, none remain significant. Therefore, the paper only instruments three variables at a time, and in each case, the RPS variable is never instrumented.

Finally, Upton and Snyder (2017) note that their synthetical control does not account for unobserved factors that impact both RPS adoption and renewables. That is, it does not directly control for endogeneity. To address the potential for endogeneity, they run a falsification test estimating the impact of RPSs on a variable they argue that RPSs should not affect: motor gasoline demand. Since no effect is identified, it is argued that endogeneity is likely not an issue. However, since their theoretical motivation for this test is related to unobserved factors pertaining to energy demand in general, it is possible that the endogeneity channels discussed in Section 3.1 will not be addressed by this test.

3. METHODOLOGY

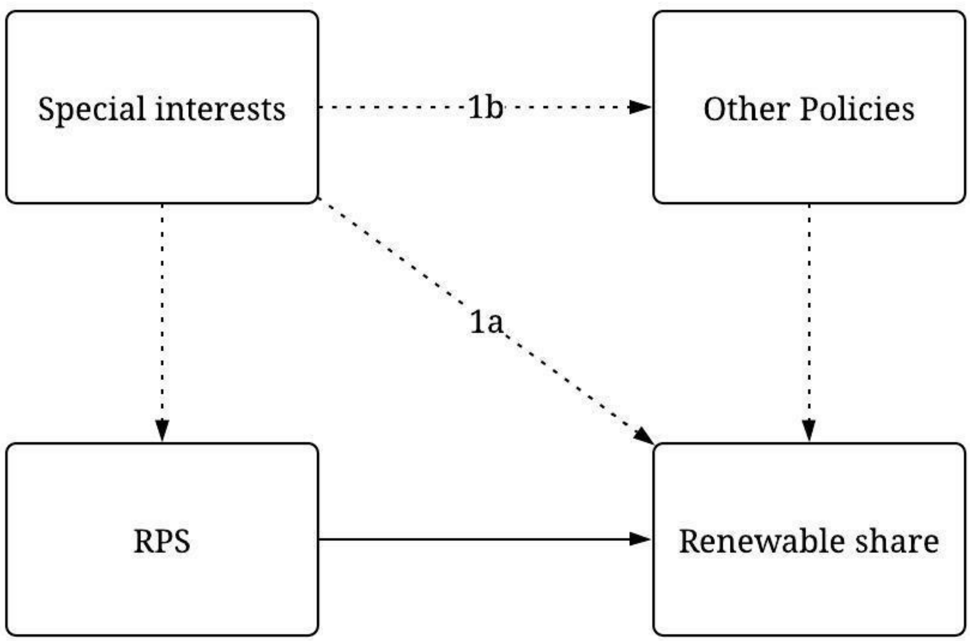
3.1. Potential Endogeneity Channels

This thesis argues that endogeneity stems from the presence of special interests, which, as mentioned in Section 2.2.1, plays an important role in the adoption of RPSs. While special interests may be measurable to some extent through proxy variables, these variables may not capture the full scope of the roles played by these groups. For example, special interests may not only have an explicit effect through campaign contributions; they may also have an implicit effect on politicians via the *potential* to finance attack ads or make campaign contributions to political opponents. There may also be informal ties between politicians (e.g., the potential for private sector positions for politicians after public service) that cannot be explicitly captured by a particular variable. How these unmeasurable special interests can affect the dependent variables will be made clear in the following three subsections.

3.1.1. Channel 1: Omitted Variable Bias

The simplest endogeneity channel is that states with stronger traditional energy lobbies may be less likely to invest in renewables, while states with stronger renewable energy lobbies are more likely to invest. In addition, since the ideal dependent variable for measuring the effectiveness of RPSs is the share of renewables, stronger special interests may tend to imply a higher share of fossil fuel generation that crowds out the share of renewables. This effect is labeled 1a in Figure 1.

Figure 1: Omitted Variable Bias Channels

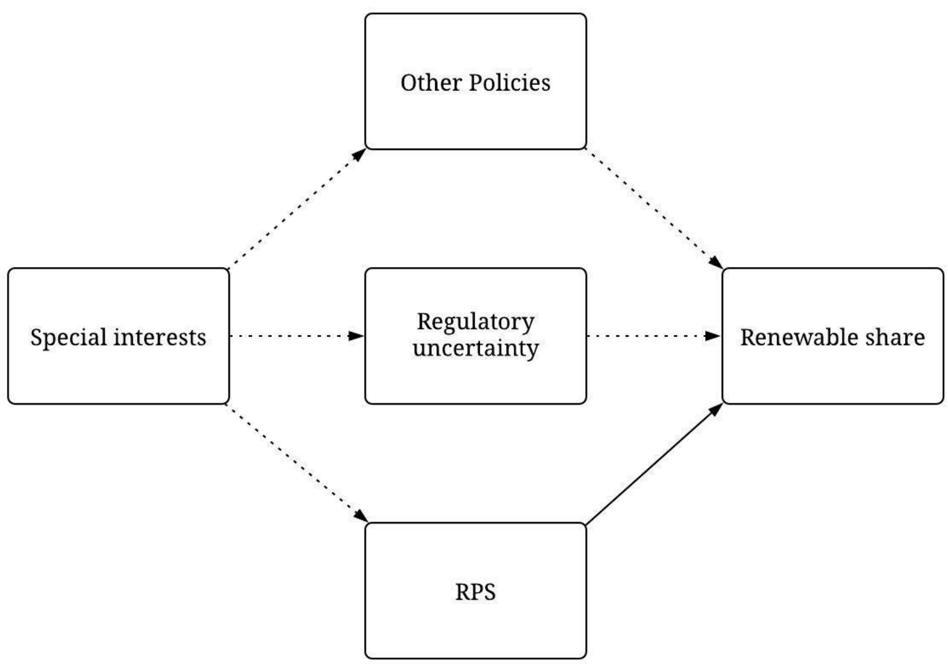


Special interest can also lobby for or against other policies that affect the renewables share. If these policies are not controlled for, there will be an indirect effect from special interests that will be included in the error term. This effect is labeled 1b in Figure 1. But what could these other policies be? Due to the crowding-out mechanism mentioned above, they need not just be related to renewables; they could also be related to investment in non-renewables. For example, in recent years, natural gas and renewables have become the only competitive forms of new generation, so states with weaker regulation on factors affecting gas investment (say, the availability of pipelines) may see faster natural gas investment and thus may tend to have a lower share of renewables; some of these policy factors may not be directly measurable, and it might not be possible to control for them. This omitted effect is labeled 1b in Figure 1.

3.1.2. Channel 2: Policy Expectations

The second omitted variable bias channel above relates to the effect of existing policies; this is represented by the top channel in Figure 2. There is also a potential endogeneity channel due to how strongly special interests believe they can affect future policies. In states where, say, natural gas special interests are stronger, agents may have more certainty about future regulations that affect investment and may be willing to invest. In states with weaker natural gas special interests, there may be more ambiguity about the potential for states to impose siting restrictions on pipelines and thus there may be more reluctance to invest in new natural gas plants.

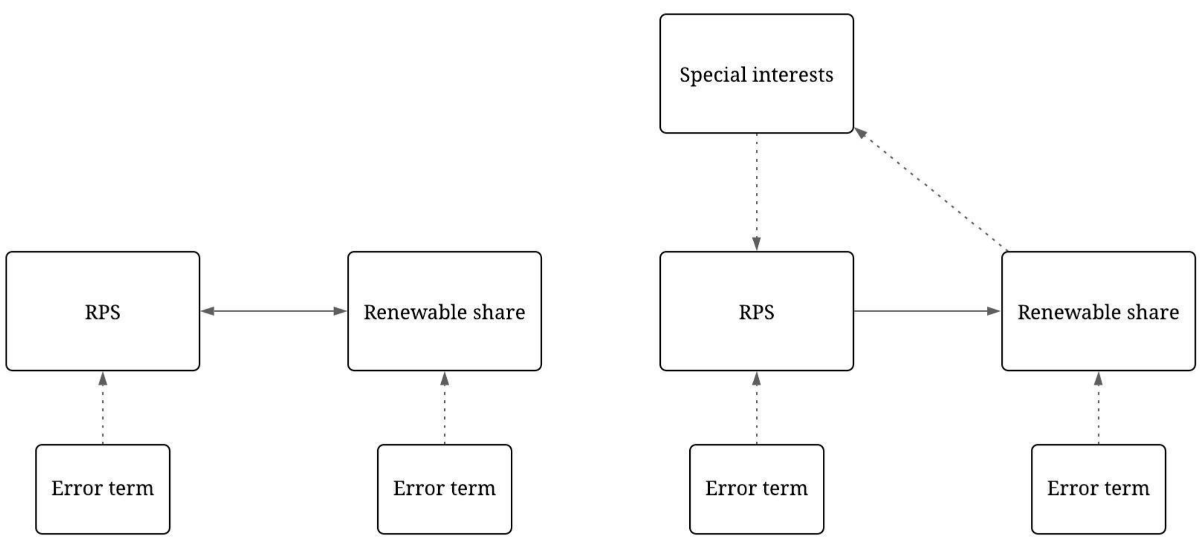
Figure 2: Policy Expectations Channel



3.1.3. *Channel 3: Reverse Causality*

This last channel stems from the possibility that the dependent variables and the RPS variables could affect each other, both directly and indirectly. Unlike channels 1a, 1b, and 2, this channel may affect the estimation of both the effectiveness of RPSs and the impact of RPSs on electricity prices. In the case of the latter, this interdependency is likely direct and thus not dependent on special interests, as shown in the left panel of Figure 3. For example, due to arguments in Section 2.2.2, states could be more reluctant to adopt RPSs when electricity prices are high. But RPSs could also affect electricity prices. In the case of the effectiveness of RPSs, special interests likely play a role again, as shown in the right panel. A higher share of renewables likely leads to stronger renewables special interests or weaker fossil fuel special interests (e.g., by impacting their revenue), and this can affect the likelihood of adoption of RPSs. A mathematical proof that this channel causes correlation between the RPS variable and the error term is provided in Appendix 1.

Figure 3: Reverse Causality Channels



3.2. Empirical Models

To account for these sources of endogeneity, two approaches are used: the instrumental variable (IV) and control function (CF) approaches. To understand how these approaches work, first consider the following model of the impact of RPSs on either electricity prices or the non-hydro renewable share:

$$y_{1it} = \boldsymbol{\beta_1 y_{2it}} + \boldsymbol{\beta_2 X_{it}} + \lambda_i + \delta_t + e_{1it} \tag{1}$$

where λ_i are individual fixed effects, δ_t are time fixed effects, and $\boldsymbol{y_2}$ is a $1 \times n$ vector of RPS indicators; these indicators could include the IPR and dummy or categorical variables for various RPS policy features, as the papers discussed in Section 2.3 have used. If for any $j \in n$, $corr(y_{2jit}, e_{1it}) \neq 0$ (e.g., through any of the channels in the previous section), then y_{2jit} is endogenous.

To consistently identify each β_{1j}, the IV–2SLS approach can be used. This requires finding a vector, $\boldsymbol{z_{it}}$, with at least n elements that

1) is strongly correlated with $\boldsymbol{y_{2it}}$.
2) can be excluded from (1).
3) is uncorrelated with e_{1it}.

If these conditions are met, then the following set of models can be run:

$$y_{2jit} = \boldsymbol{\alpha_1 z_{it}} + \boldsymbol{\alpha_2 X_{it}} + \theta_{ij} + \mu_{tj} + e_{jit}, \qquad \text{for all } j \in n \tag{2}$$

These regressions effectively break each y_{2j} into two components: 1) a predicted component $\hat{y}_{2jit}$ that is explained by $\boldsymbol{z_{it}}$ and orthogonal to e_{jit} and 2) a portion e_{jit} that is correlated with y_{2jit}. The first portion can be thought of as being "purged" of its endogenous component (Wooldridge, 2018). The vector these $\hat{y}_{2jit}$ terms can then substitute for $\mathbf{y_{2it}}$ in equation (1).

In the case of RPS, $\mathbf{y}_{2it}$ can be either vector of policy features or replaced by a scalar representing the binary RPS adoption decision or an index of policy features. As noted above, the second option is not adequate because it ignores the heterogeneity of RPSs. In the ideal scenario, the first option would be chosen over the third because it would avoid the difficulties associated with aggregating heterogeneous variables into an index. However, option 1 would be exceedingly difficult to implement given that the IV approach requires an instrument for each endogenous variable; even a single instrument is difficult to find. Thus, this thesis uses the third option; the details of this index will be discussed in Section 3.3.

If equation (1) or (2) is nonlinear, the control function (CF) approach must be used. To determine if the results in this thesis are robust to such models, this approach is also used. As with the IV approach, an instrument must be chosen satisfying the same three conditions discussed above. However, Terza et al. (2008) note that there is a distinction between the two-stage predictor substitution (2SPS) and two-stage residual inclusion (2SRI) versions of the CF approach. With the 2SPS approach, the predicted values from the first-stage regression – that is, equation (3) – are substituted for the endogenous variable(s) in the second-stage regression. With the 2SRI approach, the residuals are included. When both models are linear, then the results with either 2SPS or 2SRI will be identical to those using 2SLS. But if at least one of the two functions is nonlinear, the results with either 2SPS or 2SRI will differ; in particular, 2SPS will no longer be consistent. This discrepancy in the consistency of 2SPS and 2SRI with nonlinear models is relevant for this thesis because when modeling the probability of RPS adoption, nonlinear models are preferred; while the probability of RPS adoption could be modeled with a linear probability model, this would require treating RPS as a binary variable.

Therefore, either an ordered probit or logit model with 2SRI should be used. To create the response categories for this model, cluster analysis can then be used. However, two issues then arise. The first concerns the residuals that should be used in the approach; since multivariate logit and probit models do not have an additive error term, the residuals cannot simply be computed by subtracting the predicted values from the observed values for each observation. Wooldridge (2014) proposes an alternative method for the CF approach using the generalized residuals. But accepted techniques for computing these residuals are only available for the ordered probit models (Chiburis and Lokshin, 2007). Therefore, based on this criterion, the order probit model should be used. A description of this model is provided in Appendix 2.

Second, since this paper uses panel data, there is an additional difficulty in computing the generalized residuals: the formula in Chiburis and Lokshin (2007) is for cross-sectional data, not panel data. With nonlinear models, the use of fixed effects is also problematic due to the incidental parameter problem. To avoid these problems, this thesis uses an approach inspired by Mundlak (1978) and estimates correlated random effects models. Specifically, rather than controlling for individual fixed effects, the first stage ordered probit model includes the within-state averages for all time-varying variables:

$$y^* = \boldsymbol{\alpha_1 x} + \boldsymbol{\alpha_2 \overline{x}} + \boldsymbol{\alpha_3 \lambda} + e \tag{3}$$

where y^* is the latent variable in the cross-sectional ordered probit model, $\boldsymbol{\lambda}$ is a vector of dummy variables for each period, and $\boldsymbol{\overline{x}}$ is a vector of the within-state averages for the covariates. The approach in Chiburis and Lokshin (2007) is then used to compute the generalized residuals. Finally, following Wooldridge (2014), this paper incorporates the generalized residuals into the second-stage regression using the following "flexible" specification:

$$\begin{aligned} y_{it} = \alpha y_{2it} + \boldsymbol{\psi_1}\mathbf{x_{it}} + \eta_1 m_{it}\mathbf{1}\{y_{2it} = 2\} + \eta_2 m_{it}\mathbf{1}\{y_{2it} = 3\} + \\ \eta_3 m_{it} + \eta_4 m_{it}^2 + \lambda_t + \mu_i + e_{it} \end{aligned} \tag{4}$$

where m_{it} represents the generalized residuals from the ordered probit model, y_{2it} is the endogenous cluster variable, $\mathbf{x_{it}}$ is a vector of exogenous control variables, λ_t represents time fixed effects, and μ_i are state fixed-effects.

3.3. Dataset and Variables

This paper uses a highly comprehensive dataset that was graciously provided by Sanya Carley of Indiana University. The dataset was previously used in Carley et al. (2018). The dataset features many of the important policy design features for state RPSs between 1992 and 2014, and it captures changes in these features over time. The dataset also addresses some of the limitations of the datasets used in other studies incorporating RPS heterogeneity. For example, Carley et al. note that the dataset in Fischlein and Smith (2013) is based on policy features in a single year according to keywords, while Yin and Powers (2010) and Shrimali et al. (2015) used summaries of RPS policy design from secondary databases instead of deriving their policy variables directly from RPS legislation. In contrast, Carley et al. carefully examined the text of each initial RPS and amendment over the period. They then coded the policy features using a multi-step process.

A list of the variables from this dataset that are used in this thesis is provided in Table 1. As discussed in Section 2.3, the dependent variables in the analysis are the non-hydro share of renewables in total generation, $prengen$, and the real retail price of electricity, $elecprice$. The former is used to evaluate the effectiveness of RPS, while the latter is used to estimate the costs. As for the measurement of RPSs, two indicators are used. The first is $dynamic$, which is an index of seven RPS policy features computed using a dynamic factor algorithm presented in Zirogianni and Tripodis (2018). The seven RPS indicators are the percentage renewables target; the number of years the policy has been in place; a binary variable indicating whether the policy is mandatory; and four ordinal variables indicating the scope of the cost recovery provisions, planning requirements, geographical limits on compliance, and restrictions on REC trading. This indicator is used in the IV approach. The second indicator, $cluster$, is a categorical variable with three categories representing increasing levels of RPS stringency; this RPS indicator is used in the CF approach. This variable is computed using the k-means cluster analysis approach and the same seven policy features used to compute the $dynamic$. Then, the clusters are ranked according to their mean values for $dynamic$.

The third section of the table presents the instrument used in both methodologies: $citizen$. This variable is based on a methodology proposed by Berry et al. (1998) that locates the ideology of citizens in state-year along a liberal-conservative continuum. Recall from Section 3.2 that three conditions must be met for a variable to be used as an instrument. The arguments for why $citizen$ can be expected to satisfy these conditions are as follows:

1. Several studies have found that citizen ideology is a significant predictor of RPS adoption.

2. It likely has no *direct* impact on $prengen$ because survey research has indicated that there is strong bipartisan support for expanding renewables (Funk and Kennedy, 2016). Furthermore, several papers have found that ideology does not have a significant effect on renewables directly (e.g., Carley et al., 2018). In the electricity price regressions, it is difficult to propose a direct mechanism by which ideology and electricity prices could be linked. It could be that ideology is indirectly correlated with electricity prices; for example, more liberal states tend to have higher income, which can be correlated with electricity prices. However, in these cases, these intermediate factors can be controlled for.
3. While special interest may be able to affect $citizen$, these variables are likely only weakly correlated with special interests because ideology is determined by a wide range of factors. Furthermore, the indirect effect of citizen ideology on policy can be potentially controlled for. In the electricity price regressions, this potential effect can be at least partially controlled for by including $prengen$ as a control.

The third section in the table presents the control variables. These variables capture economic, social, and political conditions, and the bottom four variables are proxies for the presence of special interests. Each of these control variables was chosen based on the literature cited in the preceding sections.

Table 1: Summary of Variables

Variable	Description	Source
Dependent variables		
prengen	Share of non-hydro renewable net generation in total generation	EIA
elecprice	Real retail price of electricity in cents per kWh	EIA
RPS variables		
dynamic	Dynamic factor index of RPS characteristics	Computed by Carley et al. (2018)
cluster	Three-category ordinal variable based on dynamic factor index	Authors' computation
Instrument		
citizen	Index of citizen liberalism (0–100 scale)	†
Controls		
LcontRPS	Percentage of neighboring states with RPS in previous period	Computed by Carley et al. (2018)
deregulated	Binary variable indicating whether state has deregulated electricity sector	Delmas et al. (2007)
emissions	Carbon emissions from electricity generation per capita	EPA
growth	Population growth rate	US Census Bureau
GSPPC	Gross state product per capita	BEA
gasprice	Price of natural gas in USD per MMBTU	EIA
hydroshare	Share of hydro net generation in total generation	EIA
nucshare	Share of nuclear net generation in total generation	EIA
coalshare	Share of coal net generation in total generation	EIA
natshare	Share of natural gas net generation in total generation	EIA
solarest	Number of solar establishments	NAICS
windest	Number of wind establishments	NAICS

† Berry et al. (1998, 2010) and data from https://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/188/detail and http://www.bama.ua.edu/~rcfording/stateideology.html

4. RESULTS

4.1. Descriptive Statistics

Table 2 provides the summary statistics of the main variables used in the analysis. These statistics provide a summary of the overall dataset. For example, the mean column provides the averages for each value across all *state-years*, while the standard deviation (SD) summarizes the distribution of state-years around that mean. Overall, there are 49 states[10] over 23 years, resulting in 1,127 state-years observations. It is also possible to compute "between" and "within" statistics for the SD; the between SD provides a summary of the dispersion of the state-level means, while the within SD is the average of the SDs for each state. Of note is the distinction between the between SD and the within SD for the instrument, $citizen$. The former is nearly twice the latter – that is, 13.93 and 6.652, respectively. This indicates that, in general, there is more variation in $citizen$ across states than for each state across time. Indeed, a visual inspection of the time series plots (not shown but available upon request) indicates that $citizen$ has relatively little within variation. Furthermore, for most states, $citizen$ exhibits no clear trend over time.

For some states, there is also little variation in $cluster$ and $dynamic$. Thirteen states[11] remain at the minimum value for $dynamic$ and $cluster$ for the entire period and, besides South Carolina, they are the only states that do not have an RPS at any point during the period.[12] The values for $dynamic$ and $cluster$ for the remaining states vary over the period, as shown in Figure 4. The value for $dynamic$ for these states tends to increase sharply the year they implement their RPS. Thereafter, there are differences across states in the variability in the index depending on the content and frequency of amendments to the policies. For example, note how the index for Rhode Island increases suddenly in 2004, the year it implemented its RPS, and then appears to change little for the remainder of the period, whereas the index for Massachusetts appears to increase gradually throughout the period. Both indexes are actually continuously increasing since the index includes the number of years the RPSs have been in place. However, Massachusetts

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[10] Iowa is omitted because data for $dynamic$ is not available.

[11] These states are Alabama, Alaska, Arkansas, Florida, Georgia, Idaho, Kentucky, Louisiana, Mississippi, Nebraska, South Carolina, Tennessee, and Wyoming.

[12] Technically, South Carolina does not remain at the minimum value for the entire period. Its value for the index increases slightly in 2014 because it implemented a voluntary RPS in that year.

also revised its RPS twice during the period, while Rhode Island did not revise any of the features included in the index.

Figure 4 also shows the placement of each state into the three $cluster$ categories over time. The cluster analysis places all states into cluster 1 at the beginning of the period, when no state has an RPS. Then, in general, states are placed into either cluster 2 or 3 the year that they implement their RPS, which also corresponds with year that $dynamic$ increases sharply. For some states, however, the sharp increase in $dynamic$ and the transition of these states out of cluster 1 do not occur in the same year. Nevada and Texas are two clear examples. Even after they implement their RPSs, these states remain in the first clusters, which includes the 13 states that never implement an RPS. That being said, this paper still uses $cluster$ in the control function approach, as it avoids the arbitrariness of manually placing states into clusters. Figure 4 also shows that some states move through all three categories as their values for $dynamic$ increase gradually over the period.

Each state can also be categorized based on its placement in the clusters over the entire period. This categorization is useful in order to determine if key variables for states with similar placements tend to have similar trajectories. This can be thought of as a form of correlation analysis that can help to provide "empirical cues" that can then be probed further based on the results with more rigorous methods. Four clear groups[13] of states emerge from Figure 4:

1. The states that remain in cluster 1 for the entire period
2. The states that transition from cluster 1 to cluster 2 and remain in cluster 2
3. The states that transition through all three categories
4. The states that transition directly from cluster 1 to cluster 3

[13] The states in the first category are listed in footnote 19. The second category includes Arizona, Indiana, Iowa, Massachusetts, Michigan, Missouri, Montana, New Hampshire, North Carolina, North Dakota, Oklahoma, Pennsylvania, Rhode Island, South Dakota, Texas, Virginia, Washington, and Wisconsin. The third includes Colorado, Connecticut, Delaware, Hawaii, Illinois, Maryland, Minnesota, Nevada, New Jersey, and New Mexico. The fourth includes California, Kansas, Maine, New York, Ohio, Oregon, Utah, and West Virginia.

Table 2: Descriptive Statistics

Variable	Mean	SD	Min.	Max.
Dependent variables				
dynamic	–0.034	1.922	–1.272	4.718
cluster	1.357	0.630	1.000	3.000
prengen	0.032	0.047	–0.007	0.371
elecprice	8.063	3.156	3.370	34.040
Instrument				
citizen	50.280	15.313	8.450	95.972
Controls				
LcontRPS	0.299	0.355	0.000	1.000
deregulated	0.309	0.462	0.000	1.000
emissions	10.815	13.809	0.000	92.335
growth	0.011	0.011	–0.059	0.116
GSPPC	0.036	0.011	0.016	0.076
gasprice	5.306	2.827	0.320	32.390
hydroshare	17.74	18.62	-0.71	80.77
nucshare	10.92	20.06	0.00	94.28
coalshare	0.463	0.302	0.000	0.986
natshare	0.463	0.302	0.000	0.986
solarest	23.480	63.398	0.000	678
windest	80.924	150.738	0.000	1238

Figure 4: Dynamic Index and Clusters by State

In Figure 5, the state-level time series for states in each of these groups are plotted together. The first four graphs plot the time series of $prengen$ for the states in each group. For these time series, the proportion of states with a rising trajectory over time in groups 2 through 4 is higher than that in group 1.[14] Only three out of the thirteen states in group 1 have an upward trend for at least four years, whereas at least half of the states in each of the remaining three groups have such a trend. Upward trends in these groups become particularly present towards the end of the period – that is, after most states have implemented an RPS. This should not be interpreted as a causal argument; it just suggests that there is some correlation between placement in the clusters and $prengen$. While endogeneity may prevent a causal interpretation for this correlation, it is consistent with the previous research discussed in Section 2.3 that finds a relationship between RPSs and renewables development when heterogeneity is accounted for.

The bottom four graphs in Figure 5 plot the $elecprice$ time series.[15] These graphs show that there is a more general upward trend in $elecprice$ than in $prengen$; no states have a declining trend or stable values for $elecprice$ over the period as a whole. Furthermore, unlikely in the plots for $prengen$, there are no obvious differences among the plots for each group. The plots for groups 1 and 2 are more or less identical, except for the larger number of states in plot 2. In all years, states in group 3 tend to have higher electricity prices than the states in other groups, while the distribution of electricity prices in group 4 is more bimodal. However, none of the differences suggest that there is any correlation between placement in the $cluster$ categories and $elecprice$, though the more rigorous methods in the subsequent sections may contradict this.

In addition to considering large groups of states, empirical cues can be identified by comparing a small group of states, similar to comparative case studies. The comparisons are provided by plots in Figure 6, which present the $prengen$ time series and RPS implementation and amendment

[14] Note that the scaling of the y-axis is different in each plot in order to better compare the trajectories for each group. Furthermore, some readers may be confused as to why the values of $prengen$ for some states in some years appear to be negative. Note that $prengen$ is the share of *net* generation of renewables.

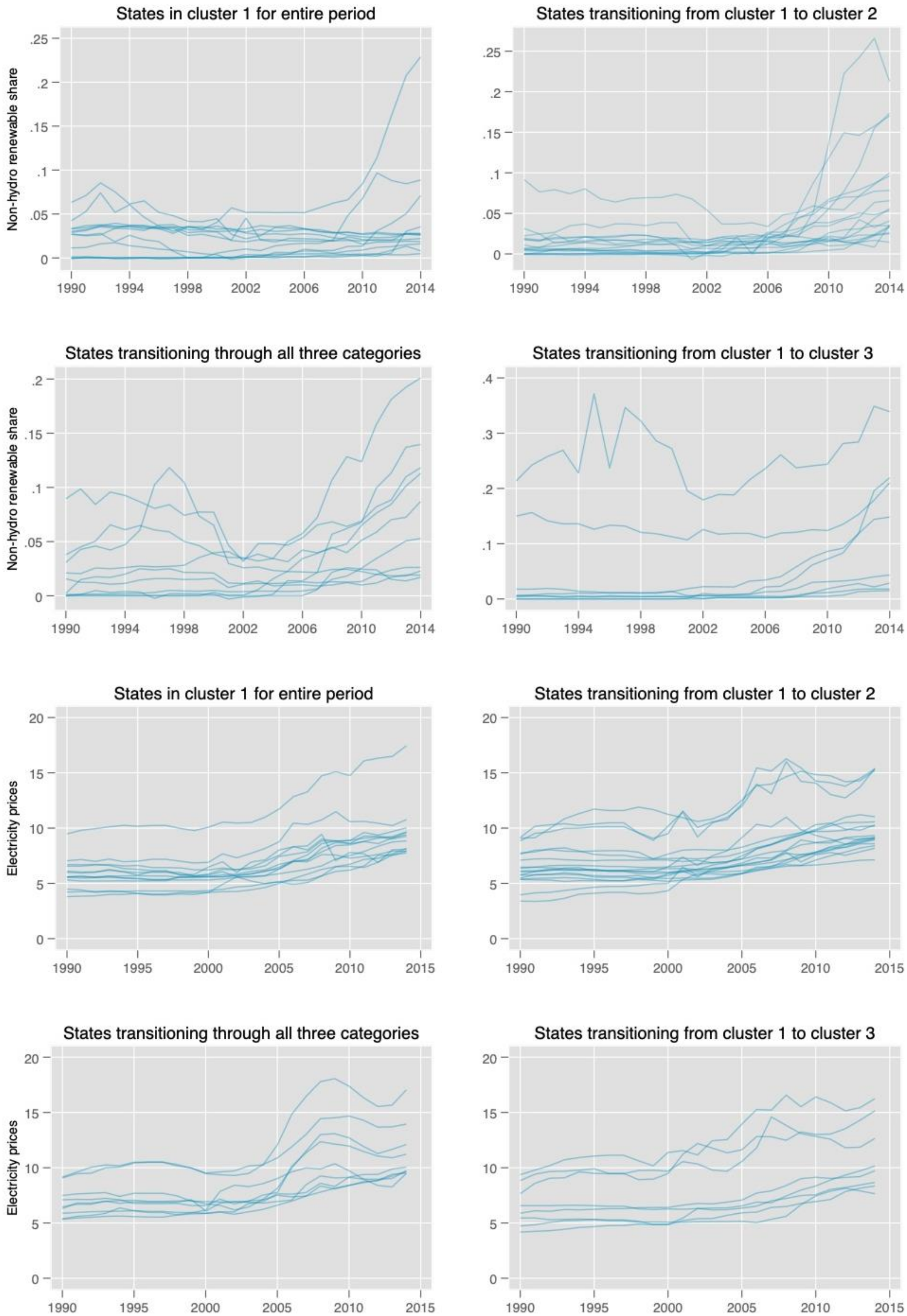
[15] Unlike the $prengen$ plots, the scaling is the same for each plot. In addition, one state, Hawaii, had to be removed from the plot for group 3 because it is an outlier and therefore made comparisons among states difficult with equivalently scaled axes for each graph. Hawaii begins the period with electricity prices similar to the states with the highest electricity prices in group 3, but it then experienced much higher electricity price growth. As a result, its electricity prices reached 34.04¢ per kWh in 2012. As Figure 5 shows, all other states have electricity prices far below this level.

dates of pairs of states. The aim of the first three panels in Figure 6 is to determine whether the trajectories of the time series correlate with differences in the RPS adoption and amendment dates and the content of these policies. In each figure, the first vertical dotted line represents the year RPS was implemented, and the subsequent lines represent amendment years; the vertical lines for each state share the color of the state's $prengen$ time series (e.g., the blue vertical lines in the graph with Massachusetts and Illinois indicate the implementation and amendment dates for Massachusetts).

The first panel of Figure 6 reveals that in Massachusetts, there was a sharp decline in the share of renewables just after RPS is implemented. This sharp decline may reflect idiosyncratic factors, similar to the sharp decline in the renewable share in Maine following the implementation of RPS (see footnote 7). The panel also shows that Connecticut experienced a similar decline just after it implemented RPS, though this decline appears to have started before RPS was implemented. In contrast, the second and third panels show that in New York and Colorado, there is a clear upward trajectory in $prengen$ that began approximately the year that their RPSs were implemented. In California, there is no decline in $prengen$ after implementation, but there is also no clear change in $prengen$ for several years. However, in all states but Connecticut, there is eventually an upward trajectory in $prengen$. In Illinois, this clearly begins the year RPS was amended, while in Massachusetts, it appears that it begins the year of the second amendment. In California, there is a clear upward trend a few years after the policy was amended, and this trend may have started the year of the amendment.

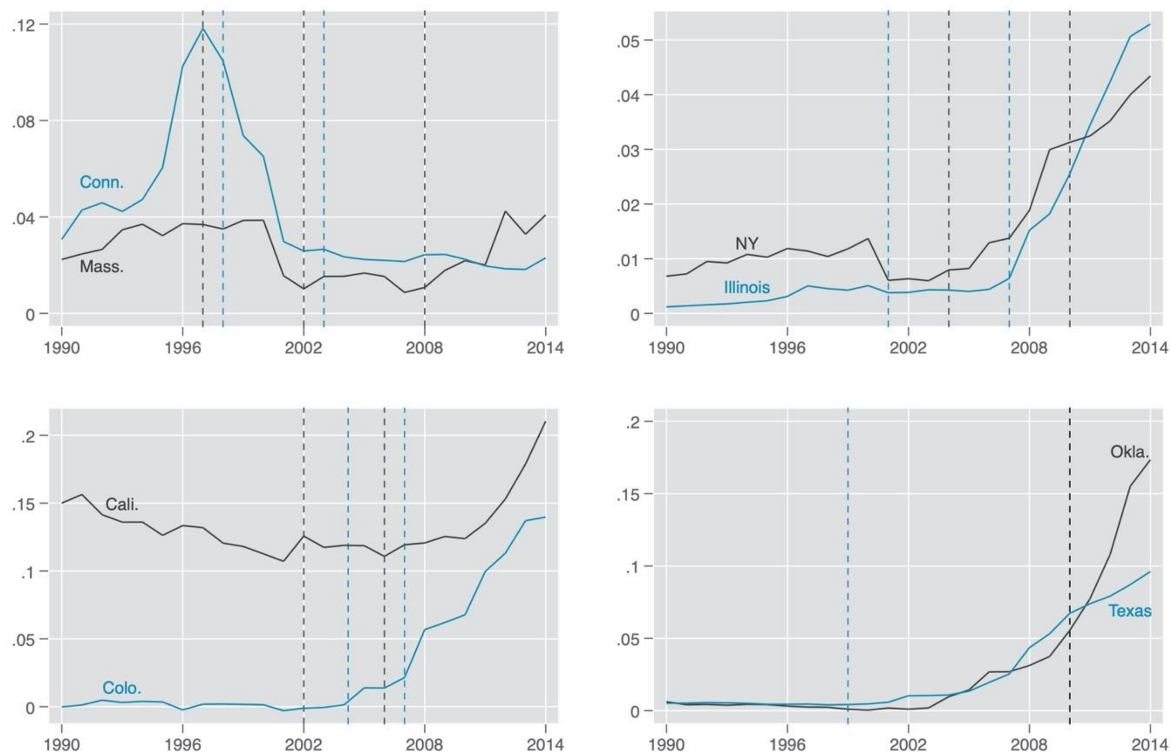
One empirical cue provided by these figures is that the initial RPS designs may have been too weak to spur renewable development. Indeed, Figure 4 shows that when Illinois and Massachusetts implemented RPS, their values for $dynamic$ increased little compared to other states in the years when they implemented RPS. The values for $dynamic$ then increase substantially as the policies were amended. This can perhaps be explained by the changing contents of the policies. Massachusetts introduced REC trading and planning requirements in 2002 and introduced geographic limits on REC trading and procurement in 2008; in Illinois, the initial RPS implemented in 2001 was only voluntary, and the amendment in 2007 made it mandatory.

Figure 5: Non-Hydro Renewable Share and Electricity Prices by Group



In California, there is a larger initial increase in $dynamic$, but it subsequently increases more sharply after the introduction of REC trading, which may have helped the LSEs subject to the policy more easily comply with it. This apparent correlation between increases in RPS stringency and $prengen$ is not necessarily a causal link. For example, the simple correlation is complicated by the lack of any correlation in the case of Connecticut. But it does provide some further informal evidence that accounting for policy heterogeneity is important.

Figure 6: Non-Hydro Renewable Share for Selected States



The final panel of Figure 6 is included to provide some empirical cues for what might complicate the simple correlations mentioned above. The figure shows the $prengen$ time series and RPS implementation dates for Texas and Oklahoma, two neighboring states with extremely high wind potential. Both states have virtually the same $prengen$ trajectory for most of the period, though Texas implemented its RPS over a decade before Oklahoma did. Two hypotheses arise from this finding. The first is that the Texas RPS was increasing wind development in Oklahoma, perhaps because LSEs in Texas subject to the policy satisfied their requirements with out-of-state RECs.

The second hypothesis is that wind development was driven by factors unrelated to RPS, such as the high wind potential in both states. While the first hypothesis is plausible, it is not supportable, as over the period, the LSEs subject to the policy were able to entirely comply with the policy using in-state RECs (Mack et al., 2011).

4.2. Baseline Regressions

This section provides the results of panel regressions without accounting for endogeneity. The results will be compared later with the results of the approaches accounting for endogeneity. It also presents the set of regression specifications that will be used in each approach. Only the coefficients of the RPS variables are presented here; the complete regressions can be found in Appendix 3. In addition, all regressions used individual fixed-effects, and the coefficients are presented with cluster-robust standard errors.

The first specification in the first panel of Table 3 is the simple regression of $prengen$ on $dynamic$. In the second specification, time fixed effects are added, which eliminates the significance of the coefficient in specification 1. The third specification adds several control variables, including two special interest proxies for the renewable energy sector. These additions leave the magnitude of the regression coefficient for $dynamic$ virtually unchanged, but it becomes significant again. Finally, the remaining two specifications add one fossil fuel proxy at a time; specification 4 uses $coalshare$ while specification 5 uses $natshare$. The reason that both are not added simultaneously is that the regression may begin to simply capture accounting relations rather causal effects; that is, in some states, the shares of renewables, coal, and natural gas make up virtually the entirety of the energy mix, so including all three in the regression would leave little else to be explained.

The estimates in Table 3 indicate for all models except for specification 2, increases in RPS stringency have statistically significant effects on $prengen$. This result is consistent with some of the informal correlation analyses in the previous section, as well as the literature accounting for heterogeneity. The magnitudes of the coefficients in specifications 3 through 5 indicate that on average and all else equal, a one-unit increase in $dynamic$ (recall from Table 2 that the range of $dynamic$ is about six units) results in an approximately 0.3 percentage point increase in $prengen$.

When $cluster$ is used instead of $dynamic$ as the RPS indicator variable, the results are less significant. Cluster 2 is only significant in two of the five specifications, while cluster 3 is only significant in specification 1. However, aside from specification 2, the coefficients are close to being significant at the 5% level. This is also the case when the coefficients for the two clusters are tested for joint significance, which is represented by the row labelled $F(2, 48)$. As for the sign of coefficients, the results are consistent with those using $dynamic$ as the indicator variable. Specifications 3 through 5 predict that, all else equal, states in cluster 2 have a non-hydro renewable share that is about 1.1 percentage points higher than those in cluster 1, while the share in cluster 3 is approximately two percentage points higher.

Table 3 also provides the results of the baseline models using $elecprice$ as the dependent variable. The specifications with controls use different variables than the specifications in Table 3. In particular, specification 5 uses the shares of all major generation sources. Since the dependent variable is no longer a generation share, there is no concern that including several generation sources as controls could explain most of the variation in the dependent variable.

The first specification in panel 3 is the simple regression of $elecprice$ on $dynamic$. Consistent with the literature, the coefficient from this regression indicates that a one-unit increase in $dynamic$ is associated with a highly significant increase (0.736¢ per kWh) in $elecprice$. However, the fades away as the specifications become more detailed. When time-fixed effects are added in specification 2, there is no longer a significant effect. The effect becomes significant again when $prengen$ is added as a control in specification 3, but it is weaker than in specification 1.[16] In specifications 4 and 5, various controls are added, and the coefficient for $dynamic$ becomes insignificant, though in specification 5, $dynamic$ is significant at the 10%

[16] It is also interesting that $prengen$ has a significant *negative* effect on $elecprice$ in this specification, since one of the channels through which RPS can influence is electricity prices is the relatively high cost of renewables. That being said, this effect becomes smaller and then insignificant as more controls are added.

level. This finding is consistent with empirical cues provided by the bottom four panels in Figure 5.

Table 3: Baseline Regression Results

	(1)	(2)	(3)	(4)	(5)
Dependent variable: $prengen$					
RPS variable: $dynamic$					
β_{RPS}	0.567***	0.274	0.318*	0.325*	0.285*
	(0.000)	(0.059)	(0.025)	(0.020)	(0.042)
RPS variable: $cluster$					
Clus. 2	2.081**	1.111	1.157	1.192*	1.059
	(0.004)	(0.073)	(0.054)	(0.041)	(0.064)
Clus. 3	3.024**	1.099	1.950	2.014	2.182
	(0.007)	(0.320)	(0.076)	(0.063)	(0.050)
$F(2,48)$	7.150**	1.910	2.880	3.320*	2.870
	(0.002)	(0.160)	(0.066)	(0.045)	(0.067)
Time FE	No	Yes	Yes	Yes	Yes
Controls	No	No	A	A+$coals.$	A+$natsh.$
Dependent variable: $elecprice$					
RPS variable: $dynamic$					
β_{RPS}	0.690***	0.109	0.135*	0.097	0.085
	(0.000)	(0.059)	(0.019)	(0.062)	(0.091)
RPS variable: $cluster$					
Clus. 2	2.299***	0.230	0.335	0.369	0.380
	(0.000)	(0.310)	(0.132)	(0.154)	(0.139)
Clus. 3	4.305***	1.501	1.604*	0.968**	0.941**
	(0.000)	(0.054)	(0.040)	(0.005)	(0.002)
$F(2,48)$	103.7***	2.120	2.610	5.240**	5.540**
	(0.000)	(0.131)	(0.084)	(0.009)	(0.007)
Time FE	No	Yes	Yes	Yes	Yes
Controls	No	No	$pren.$	B+$pren.$	B+C+$pren.$

Notes: Controls A includes $deregulated$, $LcontRPS$, $emissions$, $growthrate$, $elecprice$, $gasprice$, $windest$, $solarest$, and $GSPPC$. Controls B includes $deregulated$, $growthrate$, $gasprice$, and $GSPPC$. Controls C includes $coalshare$, $natshare$, $hydroshare$, and $nucshare$. The abbreviations $coals.$, $natsh.$, and $pren.$ refer to $coalshare$, $natshare$, and $prengen$, respectively. Results do not account for endogeneity. Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. Full results available in Appendix 3.
$*\ p < 0.05,\ **\ p < 0.01,\ ***\ p < 0.001$

The results in panel 4 reveal that for the $elecprice$ specifications, cluster 3 is generally much more significant than cluster 2; the former is significant or nearly significant for all specifications, while the latter is only significant in specification 1. As for the magnitude of the coefficients, the results indicate that states in cluster 3 have, on average and all else equal, electricity prices that are between 1 and 4 cents higher than states in cluster 1.

For both dependent variables and with $cluster$ as the RPS variable, the results in Table 3 can be summarized by computing the average predicted values of the dependent variables for each cluster. These figures provide estimates of the electricity prices experienced by an "average" state in each cluster – that is, a hypothetical state with average values for all covariates in the models. Table 4 provides these predicted values in columns 2 and 4 based on the results from specification 4 for both dependent variables. The third and fifth columns then provide the percentage difference between clusters 2 and 3 and cluster 1.

Table 4: Average Predicted Values from Baseline Regressions

Cluster	Predicted Elec. price	% diff.	Predicted RE share	% diff.
1	7.38	–	2.59%	–
2	9.56	29.4%	4.59%	77%
3	10.98	48.7%	5.02%	94%

4.3. Instrumental Variable Regressions

Table 5 presents the results for the first-stage regressions for both $prengen$ and $elecprice$ in order to gauge the strength of the instrument. For each dependent variable, four sets of regressions are provided; both contemporaneous and lagged values for the instrument are provided, and year fixed effects are included in omitted. Furthermore, note that each column in the table corresponds to the column with the same number in Table 3.

The results reveal that the magnitude and sign of the coefficients for each set of regressions are relatively consistent; that is, for each row in the table, the coefficients are similar. However, there are clear differences in the strength of the instrument (i.e., moving vertically in the table). With the instrument lagged, the instrument is stronger, though it never reaches significance at the 5% level. One reason for this could be that there is little variation in the instrument over time, as mentioned in Section 4.1. Since the regressions use fixed-individual effects, this lack of variation may cause the instrument to be absorbed in the individual error term. This lack of variation across time may also provide an explanation for why the instrument is stronger in the regressions without year fixed effects.

Table 6 presents the results of the second-stage models with $prengen$ as the dependent variable. The magnitude of p-value of the coefficients are fairly consistent with each other, regardless of the controls (i.e., the column in the table), instrument, and treatment of time fixed effects. The shared result is that after accounting for endogeneity, $dynamic$ does not have a significant effect on $prengen$. This result contradicts the baseline results in Table 3, which do not account for endogeneity, and are presented at the bottom of the table for comparison.

What does differ substantially across the columns and panels of Table 6 is the Cragg-Donald Wald F (CDWF) statistic. This statistic is used in the Stock-Yogo test for weak identification due to weak instruments. The test involves comparing the CDWF statistic to a set of critical values that determine the maximum bias of IV relative to OLS at the 5% level; that is, if the CDWF statistic is above the 20% critical value, there is a 95% chance that the IV results have a maximum bias of 20% compared to OLS. For the models used in this thesis, the critical values from Stock and Yogo (2005) are as follows:

Maximal relative bias	Critical value
10%	16.38
15%	8.96
20%	6.66
25%	5.53

Table 5: First-Stage Instrumental Variable Results

	(1)	(2)	(3)	(4)	(5)
Specifications for $prengen$					
Instrument: $citizen$					
w/ years	–	0.018	0.011	0.011	0.012
	–	(0.244)	(0.368)	(0.368)	(0.324)
w/o years	0.062***	–	0.011	0.013	0.011
	(0.000)	–	(0.196)	(0.153)	(0.193)
Instrument: $lcitizen$					
w/ years	–	0.029*	0.021	0.021	0.022
	–	(0.046)	(0.078)	(0.078)	(0.070)
w/o years	0.078***	–	0.021*	0.021*	0.021*
	(0.000)	–	(0.021)	(0.016)	(0.021)
Controls	No	No	A	A+$coals.$	A+$natsh.$
Specifications for $elecprice$					
Instrument: $citizen$					
w/ years	–	0.018	0.022	0.020	0.020
	–	(0.244)	(0.135)	(0.188)	(0.179)
w/o years	0.062***	–	0.071***	0.017	0.019
	(0.000)	–	(0.000)	(0.133)	(0.075)
Instrument: $lcitizen$					
w/ years	–	0.029*	0.033*	0.031*	0.031*
	–	(0.046)	(0.021)	(0.034)	(0.030)
w/o years	0.078***	–	0.083***	0.037**	0.036**
	(0.000)	–	(0.000)	(0.002)	(0.001)
Controls	No	No	$pren.$	B+$pren.$	B+C+$pren.$

Note: Controls A includes $deregulated$, $LcontRPS$, $emissions$, $growthrate$, $elecprice$, $gasprice$, $windest$, $solarest$, and $GSPPC$. Controls B includes $deregulated$, $growthrate$, $gasprice$, and $GSPPC$. Controls C includes $coalshare$, $natshare$, $hydroshare$, and $nucshare$. The abbreviations $coals.$, $natsh.$, and $pren.$ refer to $coalshare$, $natshare$, and $prengen$, respectively. Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. Full results available in Appendix 4. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Second-Stage Instrumental Variable Results for Non-Hydro Renewable Share

	(1)	(2)	(3)	(4)	(5)
IV with year fixed effects					
Instrument: $citizen$					
β_{RPS}	–	–2.971	–4.389	–4.424	–2.765
	–	(0.370)	(0.471)	(0.472)	(0.491)
CDWF	–	6.972	2.887	2.837	3.562
Instrument: $lcitizen$					
β_{RPS}	–	–1.707	–2.016	–2.039	–1.278
	–	(0.199)	(0.246)	(0.255)	(0.341)
CDWF	–	17.76	10.91	10.44	12.24
IV without year fixed effects					
Instrument: $citizen$					
β_{RPS}	–0.535	–	–3.809	–3.354	–3.498
	(0.148)	–	(0.285)	(0.253)	(0.295)
CDWF	70.61	–	4.554	5.707	4.644
Instrument: $lcitizen$					
β_{RPS}	–0.208	–	–2.030	–1.971	–1.813
	(0.384)	–	(0.098)	(0.091)	(0.112)
CDWF	120.6	–	16.34	17.26	16.59
Baseline results					
β_{RPS}	0.567***	0.274	0.318*	0.325*	0.285*
	(0.000)	(0.059)	(0.025)	(0.020)	(0.042)
Time FE	No	Yes	Yes	Yes	Yes
Controls A	No	Yes	Yes	Yes	Yes
Other controls	No	No	No	$coalshare$	$natshare$

Note: CDWF refers to the Cragg–Donald Wald F-statistic. Baseline results are taken from Table 3 and do not account for endogeneity. Full results for the IV regressions can be found in Appendix 4. Bottom two rows provides lists of controls for all regressions in each column. Controls A includes $deregulated$, $LcontRPS$, $emissions$, $growthrate$, $elecprice$, $gasprice$, $windest$, $solarest$, and $GSPPC$. Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses.
* $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

The CDWF statistics are higher in the models without year fixed effects than those with year fixed effects. Furthermore, those with the instrument lagged have higher CDWF statistics than those with the contemporaneous instrument. For some of the models without year fixed effects and/or with the lagged instrument, the CDWF statistic is greater than 10, indicating that the maximal relative bias for those models, assuming the other IV assumptions hold, is between 10% and 15%.

Table 7 provides the results of the second-stage regressions with $elecprice$ as the dependent variable. As in the baseline results, the coefficients in Table 8 are not significant in specifications 2, 4, and 5. In specification 3, the coefficient is also not significant in the model with year fixed effects, whereas it was significant in the baseline. Furthermore, for both $citizen$ and $lcitizen$ in all specifications, the CDWF statistics increase when year fixed effects are omitted.

4.4. Control Function Regressions

As with the IV approach, this section first presents the first-stage results to gauge the strength of the instrument. Here, however, since the regression uses a model inspired by the Mundlak approach, the regressions include both instrument and the mean of the instrument; in Table 8, the coefficients of these variables are labeled $\beta_{ctz.}$ (or $\beta_{lctz.}$) and $\beta_{\overline{ctz.}}$ (or $\beta_{\overline{lctz.}}$), respectively. To determine the strength of the instrument, the *joint* significance of the coefficients of these variables is necessary. This is achieved with a Wald test, the result of which is provided by the chi-squared statistic (i.e., labeled $\chi^2(2)$) in each panel of the table.

The coefficients of the instrument and the mean of the instrument together are never individually significant. In general, though, the mean of the instrument tends to be significant while the instrument alone does not, and if the mean is not significant, it is at least more significant than the instrument alone. One reason for this could be the relatively low variation in the instrument across time. That is, the mean alone is sufficient to capture the variation in the instrument, as it mainly occurs across states. In addition, within each panel in the table, there are no clear differences in the significance or magnitude of the coefficients for each of these variables across specifications across specifications 3 through 5. The main difference across specifications in each panel occurs between 1 and 2.

Table 7: Second-Stage Instrumental Variable Results for Electricity Prices

	(1)	(2)	(3)	(4)	(5)
IV with year fixed effects					
Instrument: *citizen*					
β_{RPS}	–	0.857	0.501	0.344	0.241
	–	(0.422)	(0.489)	(0.406)	(0.538)
CDWF	–	6.972	2.887	2.837	3.562
Instrument: *lcitizen*					
β_{RPS}	–	0.365	0.197	0.138	0.089
	–	(0.484)	(0.662)	(0.607)	(0.729)
CDWF	–	17.76	10.91	10.44	12.24
IV without year fixed effects					
Instrument: *citizen*					
β_{RPS}	1.062***	–	1.020***	–0.813	–0.687
	(0.000)	–	(0.000)	(0.588)	(0.567)
CDWF	70.60	–	109.8	8.677	11.32
Instrument: *lcitizen*					
β_{RPS}	0.956***	–	0.944***	0.206	0.107
	(0.000)	–	(0.000)	(0.664)	(0.822)
CDWF	115.7	–	154.0	44.47	43.26
Baseline results					
β_{RPS}	0.690***	0.109	0.135*	0.097	0.085
	(0.000)	(0.059)	(0.019)	(0.062)	(0.091)
Time FE	No	Yes	Yes	Yes	Yes
Controls B	No	No	No	Yes	Yes
Controls C	No	No	No	No	Yes
Other controls	No	No	*prengen*	*prengen*	*prengen*

Note: CDWF refers to the Cragg–Donald Wald F-statistic. Baseline results are taken from Table 3 and do not account for endogeneity. Full results for the IV regressions can be found in Appendix 4. Controls B includes *deregulated*, *growthrate*, *gasprice*, and *GSPPC*. Bottom rows provides lists of controls for all regressions in each column. Controls C includes *coalshare*, *natshare*, *hydroshare*, and *nucshare*. Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

However, the instrument and the mean are in most cases jointly significant at the 5% level. For both $prengen$, this joint significance tends to decrease as more controls are added, though specification 4, which controls for the share of coal generation, is more significant than specification 3. For $elecprice$, the addition of controls also tends to decrease the joint significance, though specification 5, which has controls for each major generation source, breaks this trend. Unlike with the IV regressions, though, the results do not change when the instrument is lagged instead of treated contemporaneously. As for the instrument alone and the mean of the instrument, using the $lcitizen$ as opposed to $citizen$ actually tends to reduce the significance of the coefficients.

Tables 9 and 10 provide the second-stage results with the baseline results from Table 3 provided for comparison. In each panel with the control function results, the endogeneity-adjusted coefficients for clusters 2 and 3 are provided. In addition, the table provides the results of a Wald test of the joint significance of the coefficients involving the generalized residuals are provided. There are four such coefficients: the generalized residuals alone, the square of the generalized residuals, and interactions between the generalized residuals and clusters 2 and 3. The statistics from the Wald tests have a similar interpretation as the CDWF statistics presented in the previous section; the higher the chi-squared statistics are, the more evidence there is that the instrument is strong enough to provide valid results, assuming all other assumption regarding the instrument hold.

The results in Table 9 indicate that after adjusting for potential endogeneity, the coefficients for both clusters are highly insignificant, and this finding is robust across specifications and instruments. These results differ from the baseline results presented at the bottom of the table, where many of the coefficients are significant at (or close to) the 5% level. However, among the specifications with controls, the magnitudes of the coefficients in the baseline and control function approach do not differ substantially. As for the validity of these results, the Wald tests indicate that among the specifications with controls, the adjustment is strongest for specification 3. Interestingly, the chi-squared statistic for specifications 4 and 5 differ rather substantially, even though they only differ in terms of the fossil fuel proxy used.

Table 8: First-Stage Control Function Results

	(1)	(2)	(3)	(4)	(5)
Specifications for $\boldsymbol{prengen}$					
Instrument: $citizen$					
$\beta_{ctz.}$	0.035**	–0.006	–0.002	0.000	–0.001
	(0.001)	(0.675)	(0.860)	(0.979)	(0.908)
$\beta_{\overline{ctz.}}$	–0.005	0.059**	0.043*	0.042	0.042*
	(0.676)	(0.004)	(0.045)	(0.057)	(0.046)
$\chi^2(2)$	21.61***	14.04**	6.190*	6.710*	5.740
	(0.000)	(0.001)	(0.045)	(0.035)	(0.057)
Instrument: $lcitizen$					
$\beta_{lctz.}$	0.044***	0.002	0.007	0.009	0.008
	(0.000)	(0.885)	(0.584)	(0.502)	(0.521)
$\beta_{\overline{lctz.}}$	–0.013	0.050*	0.032	0.033	0.032
	(0.275)	(0.017)	(0.125)	(0.132)	(0.131)
$\chi^2(2)$	29.97***	13.98**	6.450*	7.270*	5.970
	(0.000)	(0.001)	(0.040)	(0.026)	(0.051)
Time FE	No	Yes	Yes	Yes	Yes
Controls	No	No	A	A+$coals.$	A+$natsh.$
Specifications for $\boldsymbol{elecprice}$					
Instrument: $citizen$					
$\beta_{ctz.}$	0.038***	–0.005	0.001	–0.014	0.003
	(0.000)	(0.742)	(0.940)	(0.389)	(0.880)
$\beta_{\overline{ctz.}}$	–0.008	0.056**	0.047*	0.055*	0.058*
	(0.513)	(0.005)	(0.025)	(0.046)	(0.034)
$\chi^2(2)$	22.97***	12.88**	9.260*	4.690	11.76**
	(0.000)	(0.002)	(0.010)	(0.100)	(0.003)
Instrument: $lcitizen$					
$\beta_{lctz.}$	0.049***	0.005	0.014	0.002	0.016
	(0.000)	(0.751)	(0.355)	(0.907)	(0.281)
$\beta_{\overline{lctz.}}$	–0.018	0.046*	0.033	0.039	0.044
	(0.117)	(0.028)	(0.107)	(0.132)	(0.077)
$\chi^2(2)$	34.52***	13.04**	9.230*	4.470	13.30**
	(0.000)	(0.002)	(0.010)	(0.110)	(0.001)
Time FE	No	Yes	Yes	Yes	Yes
Controls	No	No	$pren.$	B+$pren.$	B+C+$pren.$

Note: $\beta_{ctz.}$ and $\beta_{lctz.}$ are the coefficients of the instruments, and $\beta_{\overline{ctz.}}$ and $\beta_{\overline{lctz.}}$ are the coefficients of the means of instruments. The $\chi^2(2)$ statistics test for the joint significance of two coefficients above each statistic. Controls A includes $deregulated$, $LcontRPS$, $emissions$, $growthrate$, $elecprice$, $gasprice$, $windest$, $solarest$, and $GSPPC$. Controls B includes $deregulated$, $growthrate$, $gasprice$, and $GSPPC$. Controls C includes $coalshare$, $natshare$, $hydroshare$, and $nucshare$. The abbreviations $coals.$, $natsh.$, and $pren.$ refer to $coalshare$, $natshare$, and $prengen$, respectively. Individual FEs and cluster-robust SEs used in all models. SEs in parentheses. Full results available in Appendix 5. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table 9: Second-Stage Control Function Results for Non-Hydro Renewable Share

	(1)	(2)	(3)	(4)	(5)
Control function regressions					
Instrument: $citizen$					
Cluster 2	–1.374	–0.389	0.856	0.941	0.930
	(0.158)	(0.798)	(0.532)	(0.509)	(0.491)
Cluster 3	–4.401	–3.772	1.871	1.918	2.915
	(0.240)	(0.129)	(0.513)	(0.505)	(0.288)
$\chi^2(4)$	21.00***	7.070	11.17*	9.470	4.970
	(0.000)	(0.133)	(0.025)	(0.050)	(0.290)
Instrument: $lcitizen$					
Cluster 2	–0.382	–0.438	0.843	0.931	0.899
	(0.597)	(0.771)	(0.531)	(0.509)	(0.500)
Cluster 3	–1.194	–3.818	1.880	1.936	2.909
	(0.647)	(0.122)	(0.502)	(0.493)	(0.280)
$\chi^2(4)$	21.77***	7.010	11.21*	9.610*	5.040
	(0.000)	(0.136)	(0.024)	(0.048)	(0.283)
Baseline regressions					
Cluster 2	2.080***	1.111	1.157	1.192*	1.059
	(0.002)	(0.073)	(0.054)	(0.041)	(0.064)
Cluster 3	3.143**	1.099	1.950	2.014	2.182
	(0.002)	(0.320)	(0.076)	(0.063)	(0.050)
Time FE	No	Yes	Yes	Yes	Yes
Controls A	No	Yes	Yes	Yes	Yes
Other controls	No	No	No	$coalshare$	$natshare$

Note: The $\chi^2(4)$ statistics are derived from tests of the joint significance of the terms with the generalized residuals. Bottom three rows apply to all regressions in each column. Controls A includes $deregulated$, $LcontRPS$, $emissions$, $growthrate$, $elecprice$, $gasprice$, $windest$, $solarest$, and $GSPPC$. Baseline results are taken from Table 3 and do not account for endogeneity. Full results for the CF regressions can be found in Appendix 5. Individual FEs and cluster-robust SEs used in all models. SEs in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

The results for $elecprice$, which are provided in Table 10, differ from those for $prengen$. First, the coefficients for the clusters tend to be positive and significant across all specifications and with either instrument. In many cases, the coefficients are close to being significant at the 1% level, even in the specifications with several controls. Second, the significance and the magnitude of the coefficients are higher than in the baseline. In particular, the control function approach substantially increases the significance of the second cluster relative to the baseline. Third, the chi-squared statistics across the specification and with each instrument are generally higher for $elecprice$ than for $prengen$.

Since there is a noticeable difference in the magnitude of the coefficients in the baseline and in the control function approach, it will be instructive to compare the average predicted values from each approach. Table 11 shows that these values also differ substantially. The differences between the percentage differences columns from the two approaches stems from the fact that the control function approach yields a lower average predicted value for cluster 1 and higher average predicted values for clusters 2 and 3. The difference is particularly large for cluster 3.

Table 10: Second-Stage Control Function Results for Electricity Prices

	(1)	(2)	(3)	(4)	(5)
Control function results					
Instrument: $citizen$					
Cluster 2	3.991***	0.972**	1.153**	0.998*	0.627
	(0.000)	(0.006)	(0.003)	(0.015)	(0.089)
Cluster 3	13.43***	6.838*	5.615	2.195**	1.302**
	(0.000)	(0.027)	(0.065)	(0.004)	(0.007)
$\chi^2(4)$	35.56***	8.850	14.80**	14.76**	10.69*
	(0.000)	(0.065)	(0.005)	(0.005)	(0.030)
Instrument: $lcitizen$					
Cluster 2	3.515***	1.001**	1.170**	1.037*	0.632
	(0.000)	(0.003)	(0.002)	(0.011)	(0.082)
Cluster 3	10.38***	6.804*	5.540	2.237**	1.302**
	(0.000)	(0.025)	(0.062)	(0.003)	(0.006)
$\chi^2(4)$	37.54***	9.420	15.76**	15.45**	10.75*
	(0.000)	(0.051)	(0.003)	(0.004)	(0.030)
Baseline results					
Cluster 2	2.307***	0.230	0.335	0.369	0.380
	(0.000)	(0.310)	(0.132)	(0.154)	(0.139)
Cluster 3	4.321***	1.501	1.604*	0.968**	0.941**
	(0.000)	(0.054)	(0.040)	(0.005)	(0.002)
Controls B	No	No	No	Yes	Yes
Controls C	No	No	No	No	Yes
Other controls	No	No	$prengen$	$prengen$	$prengen$

Note: The $\chi^2(4)$ statistics are derived from tests of the joint significance of the terms with the generalized residuals. Bottom three rows apply to all regressions in each column. Controls B includes $deregulated$, $growthrate$, $gasprice$, and $GSPPC$. Controls C includes $coalshare$, $natshare$, $hydroshare$, and $nucshare$. Baseline results are taken from Table 3 and do not account for endogeneity. Full results for the CF regressions can be found in Appendix 5. Individual FEs and cluster-robust SEs used in all models. SEs in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table 11: Average Predicted Electricity Prices from Baseline and CF Regressions

Cluster	Predicted Baseline	% Difference	Predicted CF	% Difference
1	7.38	–	7.13	–
2	9.56	29.4%	9.81	37.7%
3	10.98	48.7%	13.45	88.7%

Note: Predicted baseline and predicted CF refers to predicted values in the baseline and CF approach, respectively. The % difference column measure the percentage difference between the predicted value in the cell to the left and that for cluster 1.

5. DISCUSSION

5.1. Key Findings

In general, the findings with each approach and for both dependent variables tend to be robust across specifications; the main exception in some cases is that the coefficients of interest and their significance for specification 1 tend to differ from the other four specifications. This result suggests that the results are not attributable to a particular choice of control variables and provides some evidence that there are no unobservable factors related to both RPSs and the dependent variables. Furthermore, since these controls were selected ex-ante, this robustness cannot be viewed as a form of data mining, such as the stepwise regression approach.

For the regressions with the non-hydro renewables share as the dependent variable, the RPS coefficients from the IV approach are consistent with those from the CF approach. This is to be expected if the results stem from a fundamental relationship and are not simply the consequence of the empirical approach. The results are also economically meaningful; the significance of the RPS coefficient in the baseline, which was also suggested by the empirical cues discussed in Section 4.1, disappears in the approaches accounting for endogeneity. However, the validity of these results depends on the strength of the instrument. With the IV approach, the strength of the instrument various considerably depending on whether the instrument is lagged or if year fixed effects are included. With the CF approach, the strength of the instrument appears more robust to the choice of the instrument.

In contrast, with electricity prices as the dependent variable, the coefficients from the IV approach are not consistent with those from the CF approach: the results with the IV approach are insignificant while those with the CF approach are significant, positive, and higher in magnitude than in the baseline. That being said, the strength of the instruments with both approaches is generally higher than the strength of the instruments in the regressions with the non-hydro renewable share as the dependent variable. Moreover, the strength of the instrument is rather high regardless of whether it is lagged or if year fixed effects are included. Thus, while the inconsistency of the IV and CF results undermines the validity of the results, the relatively high strength of the instrument supports it.

There is a theoretical reason to prefer the IV results, though. It is rather difficult to identify a mechanism by which RPSs could affect electricity prices without affecting the non-hydro renewable share. The change in this share due to RPSs is the main channel through which RPSs are expected to affect electricity prices. Therefore, the fact that RPSs have no significant impact on both dependent variables is a reason to prefer the IV results.

5.2. Comparisons with Prior Literature

The findings in the IV and CF regressions with the non-hydro renewables share as the dependent variable are similar to the early research on RPSs. As noted in Section 2.3, this literature generally found that RPS did not have a significant impact on the non-hydro renewables share. But this literature did not account for heterogeneity. When papers began accounting for heterogeneity, a consensus developed that suggested that RPSs had significant impacts on the non-hydro renewables share. This thesis provides some evidence that the consensus rested on the assumption that RPS was exogenous.

The findings with the CF approach, though not the IV approach, that RPSs have positive price impacts is consistent with the literature. However, the price impacts identified with the CF approach are significantly larger than those in the literature. Table 12 uses the information in Table 11 to show the size of this disparity.

Table 12: Comparison of Electricity Price CF Results with the Literature

Study	*Timeframe*	*Estimated Effect*
Previous Literature		
Morey and Kirsch (2013)	1990–2011	Res.: 3.8%; Comm.: 1.8%; Indus. 1.3%
Wang (2014)	1990–2011	Increases ranging from 5 to 7.5%
Tra (2016)	2001–2012	3% inc. in residential and commercial rates
Upton and Snyder (2017)	1990–2013	11% increase
Greenstone and Nath (2019)	1990–2015	11% inc. after 7 yrs. and 17% after 12 yrs.
Average predicted effect from CF results		
Cluster 2	1990–2013	37.7% inc. relative to cluster 1
Cluster 3	1990–2013	88.7% inc. relative to cluster 1

Source: Barbose (2019) and author's calculations

5.3. Limitations

Although a reason to prefer the IV results was noted above, the regressions with relatively strong instruments depend on some assumptions that can be criticized as *ad hoc*. That is, aside from the specifications without controls, the only regressions with relatively strong IVs are those with the lagged instrument and/or without year fixed effects. Since there is no strong theoretical reason for either of these modeling choices, the models without strong instruments were also included to avoid accusations of "*p*-hacking." Future research should determine whether the modeling choices that yield strong instruments are theoretically well-founded.

This limitation relates to a more fundamental problem with the instrument. First, it has little variation across time, which complicates estimates of its impact on the dependent variables. Second, even when the instrument is lagged and year effects are excluded, it is still rather weak. Third, there are reasons to believe the instrument is somewhat correlated with the error of the dependent variable, which violates the third assumption for IV mentioned in Section 3.2.

Another limitation of this study is that some controls likely not exogenous. For example, if special interests affecting the share of non-hydro renewables also affect the share of natural gas generation, the latter may be endogenous. While the analysis here does not focus on the estimates of the control variables, it is possible that the endogeneity of these controls could distort the estimates of the coefficients of interest – that is, the coefficients for RPS. The inclusion of various specifications could account for this, however.

Finally, the dataset used for this thesis, though comprehensive, is relatively outdated; the last year used in the regressions above is 8 years prior to the publication of this thesis. Since much of the growth in renewables has come after in more recent years, particularly since 2015, running the regressions in this thesis with an updated dataset could yield significantly different results.

5.4. Are These Results Useful for Public Policy?

By virtue of the subject matter, the work of economists is necessarily intertwined in the public debate concerning public policy. It is often used by politicians to support their agendas, even if the results are misinterpreted. It can also be disseminated into the media, where it can then affect public opinion. It is therefore incumbent upon economists to address how their work can be interpreted by stakeholders and whether they can help inform public policy, particularly when there are reasons to question the validity of the results. In this case of this thesis, the recognition of these points leads to the following two questions:

1) How should the findings in this study be interpreted if they are accurate?
2) How strongly should stakeholders take the uncertainty regarding the accuracy of the findings into account?

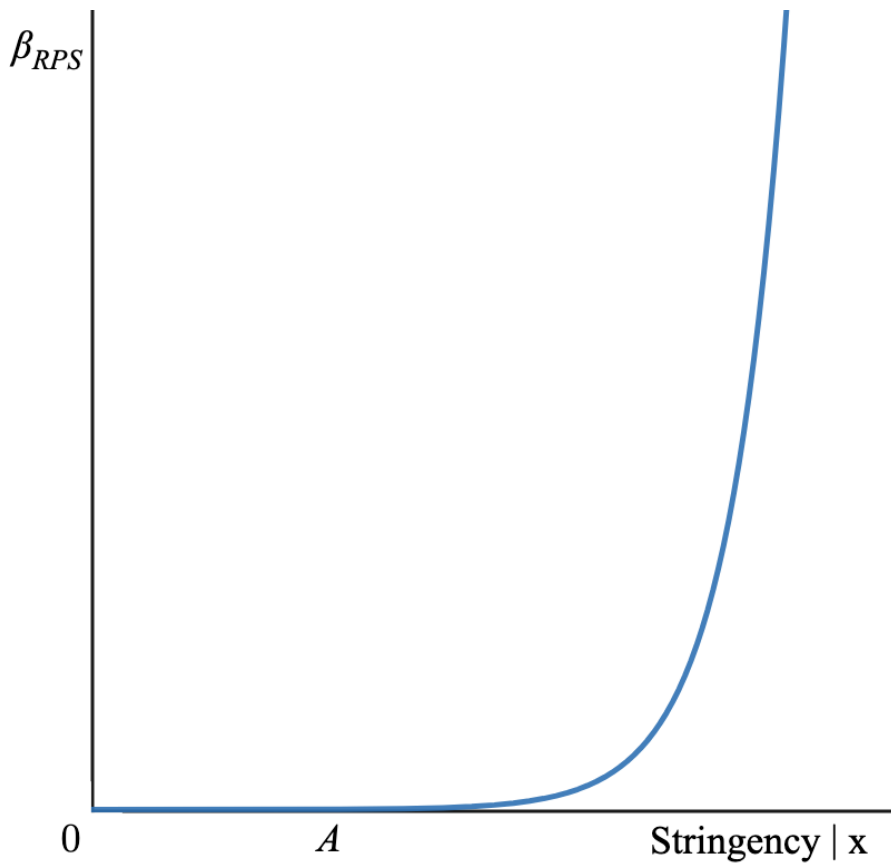
Two possible responses to the first question are considered here. First, if the evidence presented above is correct in identifying that RPSs have been ineffective, some may conclude this past ineffectiveness represents a fundamental property of RPSs. Thus, RPSs could be interpreted as either symbolic and/or inefficient policies, as some researchers have argued. One could then argue that the current set of RPSs should be replaced with an alternative policy. This conclusion would be consistent with the general preference among economists for price-based economy-wide measures to target GHGs, such as carbon taxes.

The second response is that RPSs may not have been stringent enough to achieve a higher non-hydro renewable share than would have otherwise occurred. Indeed, the findings indicating that RPSs have not been effective do not necessarily indicate whether they can be effective. For example, it is possible that there is a nonlinear relationship between RPS stringency and the dependent variable, and the model is not able to capture these nonlinearities. Figure 7 provides an "RPS impact function" that represents this hypothesis. The x-axis represents the stringency of RPS conditional on the other covariates, while the y-axis provides the impact of RPSs on the dependent variable, all else equal. The hypothesis implies that the current sample is contained within the $0A$ segment on the x-axis region, where there is no difference between the actual outcome and the counterfactual. However, a positive relationship emerges at some point outside of this region.

It may be that this nonlinear relationship occurs for different states at different levels of stringency. If so, taking the average impact at each level of stringency, conditional on the controls, may mask this effect; there may be enough states positioned along in region $0A$ in Figure 7 to offset the states in the region where the RPS impact function becomes nearly vertical, thereby resulting in an average partial effect of zero at each level of conditional stringency in the current dataset. This hypothesis is in line with some of the empirical cues mentioned in Section 4.1. For example, recall the differences in the trajectories of the non-hydro renewable shares of New York and Connecticut following the adoption of RPS. Thus, further work should aim to investigate not only how the heterogeneity of RPSs results in a range of average effects that is not captured by treating RPS as a binary variable but also the potential heterogeneity of the impacts of these different levels of RPS stringency across states.

This relationship could also exist for electricity prices, it could be that at some level of RPS stringency, bottlenecks emerge that cause large increases in prices. Beyond this point, perhaps the relationship in the figure would break down for the non-hydro renewable share, and instead, the graph would become horizontal again, thereby creating a sort of sigmoid function for the range of conditional stringency values. While this may result in pressures to free up resources from other sections, thereby causing shifts in the RPS impact functions, the resulting price increases during the adjustment process may be socially and politically unacceptable. If so, cost containment measures, such as those mentioned in Section 3.1, could be employed to prevent the policy from exacerbating the bottlenecks. These measures could be automatically triggered based on economic indicators in order to optimize response time. Furthermore, with such safeguards in place, policies could be designed to automatically increase stringency until some target is reached (e.g., 100% renewable share), as policymakers may not be able to accurately predict what level of stringency is necessary to enter the nearly vertical region of the RPS impact function.

Figure 7: A Representation of Nonlinear RPS Impacts



For those who accept the consensus that carbon taxes are superior to RPSs, this hypothesis to the first question above may be considered moot: that is, even if RPS is effective after a certain point, it will always be less efficient than carbon taxes. Therefore, the policy framework proposed above would be considered inferior. However, there may be reasons for policymakers to err on the side RPSs. First, there are numerous questions about how and whether the optimal carbon taxes can be calculated. Second, even if carbon taxes are superior in theory, it may be more politically difficult to implement carbon taxes than RPSs.

Determining which of these two answers to the first question above is correct leads directly to the second question. When two competing hypotheses are presented, favoring one over the other depends on the confidence one has in the evidence supporting it. This confidence, in turn, is derived from two sources. First, for each piece of evidence (say, the results of a given regression), there are margins of error that researchers aim to aim to minimize through their methodological choices. These margins of error are subject-dependent. For example, in this thesis, the strength of the instrument is one margin of error. While it is impossible to fully eliminate these errors or know their true magnitude, methodological improvements can reduce the estimates of them, thereby increasing the confidence in the findings. Second, confidence is derived from the integration of each piece of evidence into a comprehensive "web" of knowledge. The confidence in the links in this web is strengthened when various lines of evidence from independent sources converge on common conclusions. This phenomenon is referred to as *consilience*. For example, consilience is often invoked as an explanation for the scientific consensus in climate science (Oreskes, 2018).

How does the RPS literature fare on each of these fronts? One could argue that based on the review of the literature in Chapter 2, there have methodological improvements that may have helped to reduce the margins of error for each finding; in the case of the effectiveness literature, research has moved from overly simplistic models to more comprehensive ones and from models failing to account for policy heterogeneity to those that account for it. But throughout this development, the lurking endogeneity problem has never been adequately addressed. This thesis has provided some hypotheses indicating that it should be addressed and has presented some evidence that it is indeed a problem. However, the limitations discussed in Section 5.3 provide a reason to be skeptical about how much the methodologies used have helped to reduce the margins of error. Therefore, further work is needed to increase the accuracy of the findings.

However, the empirical literature on RPSs has a relatively limited range of independent lines of inquiry. While researchers have used methodologies such as case studies or interviews in addition to regression analyses, the independence of these alternatives from regression analysts is limited.

For example, interviewee respondents' perceptions of the effects of RPSs may be largely affected by the same data used in the regressions. Thus, while these alternatives are certainly welcome, more work is needed if a consilience of evidence in RPS empirical research is to be achieved.

With these points in mind, policymakers should view the research on RPSs, including this thesis, with a high degree of caution. Furthermore, they should weigh the high degree of uncertainty about this literature against the urgency of climate change, which is supported by greater degree of consilience. In this regard, the study of RPSs could be considered a branch of "post-normal science" – that is, a discipline "where facts are uncertain, values in dispute, stakes high and decisions urgent" (Funtowicz and Ravetz, 1993).

6. CONCLUSION

The central aim of this thesis has been to investigate the costs and benefits of renewable portfolio standards (RPSs), as represented by the impacts on the share of non-hydro renewables and electricity prices, respectively. To provide unbiased estimates of these impacts, this thesis has argued that two factors must be taken into account. The first is the heterogeneous nature of RPSs; unlike, say, vaccine trials, units do not receive a homogenous treatment. This point has been acknowledged in the literature on the impacts of RPSs, particularly the effectiveness literature, and various methodologies have been presented that account for this heterogeneity. The second factor is that there are reasons to believe that RPSs are endogenous; to use vaccine trials again for contrast, since there is no way to randomly assign RPSs to states, RPS indicators may be correlated with the error terms in regressions estimating the impacts of RPS on either dependent variable. This thesis has argued that this endogeneity likely stems from the presence of special interests.

To account for both the endogeneity and heterogeneity of RPSs, this thesis used the instrumental variable (IV) and control function (CF) approaches. The instrument chosen for these approaches is an index of citizen ideology. For the IV approach, a dynamic factor index capturing seven RPS policy features is used as the endogenous variable. For the CF approach, a categorical variable with three categories was constructed using cluster analysis and the same seven policy features used to create the dynamic factor index. The clusters were then ranked used each cluster's mean variables for the index.

The results provide some evidence that endogeneity is present. With the IV approach, RPSs are found to have, on average, no significant effect on either the non-hydro renewable share or electricity prices. With the CF approach, the RPSs once again have no significant effect on the non-hydro renewable share but have a significant positive effect, for some clusters in some models, on electricity prices.

The results have a number of limitations. Most importantly, the strength of the instrument in the IV approach varies considerably across models; reasonably strong results are only generated in the models where the instrument is lagged and/or year fixed effects are excluded. Therefore, future research should aim to determine whether these modeling choices are justified.

This paper also asks how policymakers should incorporate these findings into public policy. It argues that two questions must be answered. First, it asks how the findings in this study should be interpreted if they are accurate. Two answers to this question are proposed: the findings indicate that RPSs are inherently ineffective, or the findings indicate RPSs have not been stringent enough to yield results outcomes that are significantly different from what would have otherwise occurred. To support the latter, a simplified model of the relationship between stringency and the impacts of RPS is proposed. If this relationship exists, it could serve as the basis for an RPS policy wherein stringency is automatically increased until 100% renewables is achieved, while stopgap measures are triggered if the policy yields social or political undesirable increases in electricity prices.

Second, this thesis asks how strongly stakeholders should take the uncertainty regarding the accuracy of the findings into account. It argues that confidence in the evidence for a hypothesis stem from two sources: the margin of error of the evidence itself and the integration of the evidence into a consilient web of evidence. The empirical research on RPS, however, is limited on both fronts. Therefore, policymakers should be wary of using this research for policymaking. Moreover, while future research should of course further study these policies to increase the build a consilience of evidence, it is important to note that the climate crisis is too urgent to wait for such a body of work to emerge; it may be that policymaking regarding RPS in the near term will have to depend more on theory than empirical evidence.

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APPENDIX 1: PROOF OF ENDOGENEITY DUE TO CHANNEL 3

First, consider the following system of equations for the effectiveness case:

$$y_1 = \beta_1 y_2 + \boldsymbol{\beta_2 x_1} + e_1, \qquad x_u = \alpha_1 y_1 + \boldsymbol{\alpha_2 x_2} + e_2, \qquad y_2 = \pi_1 x_u + \boldsymbol{\pi_2 x_3} + e_3$$

where y_1 is the share of renewables, y_2 is RPS, x_u is a variable representing special interests,[17] and the $\mathbf{x}$'s and e's are exogenous regressors and error terms, respectively. Substituting the second equation into the third yields the following two equations:

$$\begin{aligned} y_1 &= \beta_1 y_2 + \boldsymbol{\beta_2 x_1} + e_1 \\ y_2 &= \pi_1 \alpha_1 y_1 + \pi_1 \boldsymbol{\alpha_2 x_2} + \boldsymbol{\pi_2 x_3} + \pi_1 e_2 + e_3 \end{aligned}$$

In the non-trivial case, $\pi_1, \alpha_1, \beta_1 \neq 1$. It can then be seen that y_2 is endogenous by expressing it in reduced form:

$$y_2 = \frac{\boldsymbol{\lambda x} + \pi_1 \alpha_1 e_1 + \pi_1 e_2 + e_3}{1 - \pi_1 \alpha_1 \beta_1}$$

where $\boldsymbol{\lambda z}$ is a linear combination of the exogenous regressors. Thus, y_2 is correlated with e_1 with a proportionality constant of $\pi_1 \alpha_1 / (1 - \pi_1 \alpha_1 \beta_1)$ when $\pi_1 \alpha_1 \beta_1 \neq 1$, which is ruled out by assumption.

Algebraically, it is simpler to demonstrate that the reverse causality between RPS and electricity prices causes endogeneity because there is no intermediate step through special interests. In this case, the system of equations can be written as

$$y_1 = \beta_1 y_2 + \boldsymbol{\beta_2 x_1} + e_1, \qquad y_2 = \alpha_1 y_1 + \boldsymbol{\alpha_2 x_2} + e_2,$$

[17] Note that the conclusions here would still hold if y_2 and x_u were vectors, thought the algebra would become considerably more complicated.

and the reduced form for y_2 is as follows:

$$y_2 = \frac{\boldsymbol{\psi x} + \alpha_1 e_1 + e_2}{1 - \alpha_1 \beta_1}$$

where $\boldsymbol{\psi x}$ is a linear combination of the exogenous regressors and $\alpha_1 \beta_1 \neq 1$ in the non-trivial case.

APPENDIX 2: OVERVIEW OF THE ORDERED PROBIT MODEL

An order probit model is used to model the probability that a set of observations adopt an observed response category, $y = \{y_0, y_1, \ldots, y_n\}$, such as an individual's responses to a question on a survey with a Likert scale. The model assumes that the response category observed for each unit is a function of an unobserved latent continuous variable, y^*. This variable is typically assumed to be predicted by a linear model:

$$y^* = \boldsymbol{\lambda}\mathbf{z} + e, \qquad e \mid \boldsymbol{z} \sim \text{Normal}(0,1) \tag{5}$$

The mapping of y^* to y is based on a set of increasing thresholds, $\tau = \{\tau_0, \tau_1, \ldots, \tau_{n+1}\}$, where $\tau_0 = -\infty$ and $\tau_{n+1} = \infty$. Formally, this can be written as follows:

$$y = \begin{cases} 0 \ \text{ if } \ \tau_0 < y^* \leq \tau_1 \\ 1 \ \text{ if } \ \tau_1 < y^* \leq \tau_2 \\ \vdots \\ n \ \text{ if } \tau_n < y^* \leq \tau_{n+1} \end{cases} \tag{6}$$

While this mapping is ordering preserving (i.e., for any j, $y_j^* < y_{j+1}^*$), no assumption is made about the distances between each of the response categories, as y is an ordinal variable. Equation (6) is then used to derive the probability (conditional on $\mathbf{z}$) of each observed response probability, $\text{P}(\text{y} = j \mid \mathbf{z})$, which is the probability that y^* for each j is bounded by the corresponding thresholds in equation (6) – that is, $\mathrm{P}(\mathrm{y} = j \mid \mathbf{z}) = \mathrm{P}(\{\mathrm{y} = \tau_j < y^* \leq \tau_{j+1}\} \mid \mathbf{z})$. Substituting equation (5) and rearranging yields

$$\begin{aligned}\mathrm{P}(\{\tau_j < \boldsymbol{\lambda}\mathbf{z} + e \leq \tau_{j+1}\} \mid \mathbf{z}) &= \mathrm{P}(\{\tau_j - \boldsymbol{\lambda}\mathbf{z} < e \leq \tau_{j+1} - \boldsymbol{\lambda}\mathbf{z}\} \mid \mathbf{z}) \\ &= \mathrm{P}(\{\tau_{j+1} - \boldsymbol{\lambda}\mathbf{z} \geq e\} \mid \mathbf{z}) - \mathrm{P}(\{\tau_j - \boldsymbol{\lambda}\mathbf{z} < e\} \mid \mathbf{z})\end{aligned}$$

Given the assumption that $e \mid \boldsymbol{z}$ is normally distributed, the normal cumulative density function can be substituted for the two conditional probabilities above. Thus,

$$\mathrm{P}(\mathrm{y} = j \mid \mathbf{z}) = \Phi(\tau_{j+1} - \boldsymbol{\lambda}\mathbf{z}) - \Phi(\tau_j - \boldsymbol{\lambda}\mathbf{z})$$

Given the definition of the thresholds, $\Phi(\tau_{j+1} - \boldsymbol{\lambda}\mathbf{z}) = 1$ when $j = n$ and $\Phi(\tau_j - \boldsymbol{\lambda}\mathbf{z}) = 0$ when $\tau = 0$.

APPENDIX 3: BASELINE REGRESSION TABLES WITH CONTROLS

Table A.1: Baseline Results for $prengen$ with Dynamic Index as RPS Variable

	(1)	(2)	(3)	(4)	(5)
$dynamic$	0.567***	0.274	0.318*	0.325*	0.285*
	(0.000)	(0.059)	(0.025)	(0.020)	(0.042)
$deregulated$	–	–	–0.590	–0.570	–0.287
	–	–	(0.210)	(0.230)	(0.461)
$LcontRPS$	–	–	–0.031	–0.057	0.332
	–	–	(0.973)	(0.950)	(0.666)
$emissions$	–	–	–0.117	–0.155	–0.175
	–	–	(0.262)	(0.269)	(0.116)
$growthrate$	–	–	11.02	9.629	2.513
	–	–	(0.509)	(0.595)	(0.885)
$elecprice$	–	–	–0.524***	–0.535***	–0.467***
	–	–	(0.001)	(0.000)	(0.000)
$gasprice$	–	–	0.213	0.212	0.163
	–	–	(0.103)	(0.104)	(0.147)
$windest$	–	–	0.001	0.001	0.002*
	–	–	(0.240)	(0.222)	(0.038)
$solarest$	–	–	–0.004	–0.004	–0.004
	–	–	(0.523)	(0.467)	(0.469)
$GSPPC$	–	–	146.4*	136.8	89.32
	–	–	(0.026)	(0.065)	(0.175)
$coalshare$	–	–	–	0.022	–
	–	–	–	(0.537)	–
$natshare$	–	–	–	–	–0.102***
	–	–	–	–	(0.000)

Notes: Complete results from corresponding panel of Table 3. Results do not account for endogeneity. Individual fixed effects and cluster-robust standard errors used in all models. Time fixed effects used in models 2 through 5. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.2: Baseline Results for $prengen$ with Cluster as RPS Variable

	(1)	(2)	(3)	(4)	(5)
Cluster 2	2.081**	1.111	1.157	1.192*	1.059
	(0.004)	(0.073)	(0.054)	(0.041)	(0.064)
Cluster 3	3.024**	1.099	1.950	2.014	2.182
	(0.007)	(0.320)	(0.076)	(0.063)	(0.050)
$deregulated$	–	–	–0.701	–0.683	–0.437
	–	–	(0.164)	(0.178)	(0.289)
$LcontRPS$	–	–	–0.123	–0.159	0.159
	–	–	(0.894)	(0.860)	(0.841)
$emissions$	–	–	–0.112	–0.154	–0.169
	–	–	(0.310)	(0.296)	(0.147)
$growthrate$	–	–	9.984	8.408	1.613
	–	–	(0.552)	(0.642)	(0.924)
$elecprice$	–	–	–0.553***	–0.566***	–0.505***
	–	–	(0.000)	(0.000)	(0.000)
$gasprice$	–	–	0.212	0.211	0.155
	–	–	(0.126)	(0.127)	(0.187)
$windest$	–	–	0.001	0.001	0.002*
	–	–	(0.275)	(0.255)	(0.033)
$solarest$	–	–	–0.004	–0.004	–0.005
	–	–	(0.502)	(0.440)	(0.373)
$GSPPC$	–	–	151.1*	140.4	93.14
	–	–	(0.022)	(0.058)	(0.158)
$coalshare$	–	–	–	0.025	–
	–	–	–	(0.486)	–
$natshare$	–	–	–	–	–0.106***
	–	–	–	–	(0.000)

Notes: Complete results from corresponding panel of Table 3. Results do not account for endogeneity. Individual fixed effects and cluster-robust standard errors used in all models. Time fixed effects used in models 2 through 5. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.3: Baseline Results for $elecprice$ with Dynamic Index as RPS Variable

	(1)	(2)	(3)	(4)	(5)
$dynamic$	0.690***	0.109	0.135*	0.097	0.085
	(0.000)	(0.059)	(0.019)	(0.062)	(0.091)
$prengen$	–	–	–0.094**	–0.075**	0.006
	–	–	(0.008)	(0.007)	(0.803)
$deregulated$	–	–	–	–0.055	–0.081
	–	–	–	(0.722)	(0.577)
$growthrate$	–	–	–	–8.780	–9.519*
	–	–	–	(0.063)	(0.035)
$gasprice$	–	–	–	0.648***	0.642***
	–	–	–	(0.000)	(0.000)
$GSPPC$	–	–	–	51.34*	38.55
	–	–	–	(0.018)	(0.059)
$coalshare$	–	–	–	–	6.690**
	–	–	–	–	(0.003)
$natshare$	–	–	–	–	5.433**
	–	–	–	–	(0.001)
$hydroshare$	–	–	–	–	0.056**
	–	–	–	–	(0.006)
$nucshare$	–	–	–	–	0.068***
	–	–	–	–	(0.000)

Notes: Complete results from corresponding panel of Table 3. Results do not account for endogeneity. Individual fixed effects and cluster-robust standard errors used in all models. Time fixed effects used in models 2 through 5. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.4: Baseline Results for $elecprice$ with Cluster as RPS Variable

	(1)	(2)	(3)	(4)	(5)
Cluster 2	2.299***	0.230	0.335	0.369	0.380
	(0.000)	(0.310)	(0.132)	(0.154)	(0.139)
Cluster 3	4.305***	1.501	1.604*	0.968**	0.941**
	(0.000)	(0.054)	(0.040)	(0.005)	(0.002)
$prengen$	–	–	–0.094**	–0.080**	–0.000
	–	–	(0.009)	(0.007)	(0.996)
$deregulated$	–	–	–	–0.147	–0.161
	–	–	–	(0.311)	(0.278)
$growthrate$	–	–	–	–8.772	–9.554*
	–	–	–	(0.058)	(0.027)
$gasprice$	–	–	–	0.613***	0.612***
	–	–	–	(0.000)	(0.000)
$GSPPC$	–	–	–	56.42**	41.94*
	–	–	–	(0.009)	(0.037)
$coalshare$	–	–	–	–	6.517***
	–	–	–	–	(0.000)
$natshare$	–	–	–	–	5.055***
	–	–	–	–	(0.000)
$hydroshare$	–	–	–	–	0.058***
	–	–	–	–	(0.001)
$nucshare$	–	–	–	–	0.067***
	–	–	–	–	(0.000)

Notes: Complete results from corresponding panel of Table 3. Results do not account for endogeneity. Individual fixed effects and cluster-robust standard errors used in all models. Time fixed effects used in models 2 through 5. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

APPENDIX 4: IV REGRESSION TABLES WITH CONTROLS

Section 4.3 provides the results of regressions with lagged and contemporaneous values for the instrument. Since the results for the control variables do not change substantially when the instrument is lagged, those results are excluded here. They are available upon request.

Table A.5: First-Stage IV Results for $prengen$, with Year Fixed Effects

	(2)	(3)	(4)	(5)
$citizen$	0.018	0.011	0.011	0.012
	(0.244)	(0.368)	(0.368)	(0.324)
$deregulated$	–	0.189	0.174	0.215
	–	(0.396)	(0.446)	(0.345)
$LcontRPS$	–	1.970***	1.979***	1.994***
	–	(0.001)	(0.001)	(0.000)
$emissions$	–	–0.028	–0.002	–0.034
	–	(0.731)	(0.984)	(0.687)
$growthrate$	–	–13.76	–12.72	–14.46
	–	(0.109)	(0.156)	(0.088)
$elecprice$	–	0.115	0.121	0.119
	–	(0.217)	(0.202)	(0.186)
$gasprice$	–	–0.011	–0.010	–0.015
	–	(0.880)	(0.889)	(0.829)
$windest$	–	–0.002	–0.002	–0.002
	–	(0.159)	(0.151)	(0.191)
$solarest$	–	0.004	0.004	0.004
	–	(0.062)	(0.053)	(0.069)
$GSPPC$	–	15.78	22.42	10.59
	–	(0.702)	(0.578)	(0.798)
$coalshare$	–	–	–0.016	–
	–	–	(0.342)	–
$natshare$	–	–	–	–0.009
	–	–	–	(0.349)

Notes: Complete results from corresponding panel of Table 5. Since specification 1 only differs from specification 2 in that it does not have year FEs, this table begins with column (2). Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.6: First-Stage IV Results for $prengen$, without Year Fixed Effects

	(1)	(3)	(4)	(5)
$citizen$	0.062***	0.011	0.013	0.011
	(0.000)	(0.196)	(0.153)	(0.193)
$deregulated$	–	0.194	0.193	0.200
	–	(0.344)	(0.361)	(0.336)
$LcontRPS$	–	2.353***	2.259***	2.377***
	–	(0.000)	(0.000)	(0.000)
$emissions$	–	–0.028	0.004	–0.028
	–	(0.746)	(0.961)	(0.745)
$growthrate$	–	–12.79*	–9.99	–13.23*
	–	(0.032)	(0.102)	(0.026)
$elecprice$	–	0.174*	0.165*	0.176*
	–	(0.012)	(0.013)	(0.011)
$gasprice$	–	–0.017	–0.010	–0.018
	–	(0.665)	(0.808)	(0.657)
$windest$	–	–0.001	–0.001	–0.001
	–	(0.165)	(0.171)	(0.174)
$solarest$	–	0.004	0.004	0.004
	–	(0.081)	(0.052)	(0.083)
$GSPPC$	–	35.75	32.70	36.36
	–	(0.095)	(0.120	(0.104)
$coalshare$	–	–	–0.022	–
	–	–	(0.134)	–
$natshare$	–	–	–	–0.003
	–	–	–	(0.691)

Notes: Complete results from corresponding panel of Table 5. Since specification 2 only differs from specification 1 in that it has year FEs, this table omits column (2). Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.7: First-Stage IV Results for $elecprice$, with Year Fixed Effects

	(2)	(3)	(4)	(5)
$citizen$	0.018	0.022	0.020	0.020
	(0.244)	(0.135)	(0.188)	(0.179)
$prengen$	–	0.077*	0.077	0.120*
	–	(0.047	(0.050)	(0.044)
$deregulated$	–	–	0.384	0.386
	–	–	(0.106	(0.118)
$growthrate$	–	–	–17.33*	–16.69
	–	–	(0.030)	(0.051)
$gasprice$	–	–	0.025	0.029
	–	–	(0.440)	(0.375)
$GSPPC$	–	–	34.01	26.98
	–	–	(0.505)	(0.590)
$coalshare$	–	–	–	1.580
	–	–	–	(0.577)
$natshare$	–	–	–	2.247
	–	–	–	(0.399)
$hydroshare$	–	–	–	0.049
	–	–	–	(0.183)
$nucshare$	–	–	–	0.036
	–	–	–	(0.171)

Notes: Complete results from corresponding panel of Table 5. Since specification 1 only differs from specification 2 in that it does not have year FEs, this table begins with column (2). Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.8: First-Stage IV Results for $elecprice$, without Year Fixed Effects

	(1)	(3)	(4)	(5)
$citizen$	0.062***	0.071***	0.017	0.019
	(0.000)	(0.000)	(0.133)	(0.075)
$prengen$	–	0.265***	0.116**	0.157**
	–	(0.000)	(0.002)	(0.001)
$deregulated$	–	–	0.410	0.355
	–	–	(0.053)	(0.120)
$growthrate$	–	–	–19.92**	–14.72*
	–	–	(0.003)	(0.030)
$gasprice$	–	–	0.022	0.047
	–	–	(0.570)	(0.202)
$GSPPC$	–	–	107.6***	88.98***
	–	–	(0.000)	(0.000)
$coalshare$	–	–	–	0.022
	–	–	–	(0.993)
$natshare$	–	–	–	3.545
	–	–	–	(0.120)
$hydroshare$	–	–	–	0.044
	–	–	–	(0.126)
$nucshare$	–	–	–	0.040*
	–	–	–	(0.048

Notes: Complete results from corresponding panel of Table 5. Since specification 2 only differs from specification 1 in that it has year FEs, this table omits column (2). Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.9: Second-Stage IV Results for $prengen$, with Year Fixed Effects

	(2)	(3)	(4)	(5)
$dynamic$	–2.971	–4.389	–4.424	–2.765
	(0.360)	(0.471)	(0.472)	(0.491)
$deregulated$	–	0.319	0.276	0.374
	–	(0.843)	(0.862)	(0.740)
$LcontRPS$	–	9.317	9.416	6.459
	–	(0.467)	(0.470	(0.452
$emissions$	–	–0.233	–0.145	–0.264
	–	(0.598)	(0.720)	(0.406)
$growthrate$	–	–54.35	–51.33	–41.77
	–	(0.543)	(0.547)	(0.481)
$elecprice$	–	0.037	0.064	–0.090
	–	(0.962)	(0.939)	(0.862)
$gasprice$	–	0.154	0.156	0.112
	–	(0.683)	(0.683)	(0.651)
$windest$	–	–0.006	–0.006	–0.002
	–	(0.584)	(0.582)	(0.741)
$solarest$	–	0.014	0.015	0.008
	–	(0.570)	(0.571)	(0.657)
$GSPPC$	–	224.6	247.4	126.1
	–	(0.355)	(0.339)	(0.442)
$coalshare$	–	–	–0.052	–
	–	–	(0.669)	–
$natshare$	–	–	–	–0.126**
	–	–	–	(0.006)

Notes: Complete results from corresponding panel of Table 6. Since specification 1 only differs from specification 2 in that it does not have year FEs, this table begins with column (2). Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.10: Second-Stage IV Results for $prengen$, without Year Fixed Effects

	(1)	(3)	(4)	(5)
$dynamic$	–0.535	–3.809	–3.354	–3.498
	(0.143)	(0.285)	(0.253)	(0.295)
$deregulated$	–	0.280	0.186	0.399
	–	(0.819)	(0.869)	(0.722)
$LcontRPS$	–	10.41	8.978	10.40
	–	(0.236)	(0.204)	(0.214)
$emissions$	–	–0.322	–0.184	–0.325
	–	(0.401)	(0.572)	(0.395)
$growthrate$	–	–46.36	–29.59	–55.49
	–	(0.371)	(0.428)	(0.261)
$elecprice$	–	0.720	0.606	0.734
	–	(0.229	(0.206)	(0.194)
$gasprice$	–	–0.309	–0.272	–0.317
	–	(0.136)	(0.168)	(0.099)
$windest$	–	–0.002	–0.002	–0.001
	–	(0.703)	(0.757)	(0.794)
$solarest$	–	0.018	0.017	0.017
	–	(0.364)	(0.337)	(0.378)
$GSPPC$	–	243.9	215.7	251.0
	–	(0.132)	(0.105)	(0.112
$coalshare$	–	–	–0.088	–
	–	–	(0.298)	–
$natshare$	–	–	–	–0.100*
	–	–	–	(0.021

Notes: Complete results from corresponding panel of Table 6. Since specification 2 only differs from specification 1 in that it has year FEs, this table omits column (2). Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.11: Second-Stage IV Results for $elecprice$, with Year Fixed Effects

	(2)	(3)	(4)	(5)
$dynamic$	0.857	0.501	0.344	0.241
	(0.422)	(0.489)	(0.406)	(0.538)
$prengen$	–	–0.120*	–0.095*	–0.012
	–	(0.016)	(0.041)	(0.819)
$deregulated$	–	–	–0.147	–0.131
	–	–	(0.475)	(0.511)
$growthrate$	–	–	–4.852	–7.320
	–	–	(0.552)	(0.326)
$gasprice$	–	–	0.623***	0.623***
	–	–	(0.000)	(0.000)
$GSPPC$	–	–	47.02*	38.46*
	–	–	(0.013)	(0.022)
$coalshare$	–	–	–	6.179**
	–	–	–	(0.003)
$natshare$	–	–	–	4.895**
	–	–	–	(0.003)
$hydroshare$	–	–	–	0.050
	–	–	–	(0.067)
$nucshare$	–	–	–	0.059**
	–	–	–	(0.010)

Notes: Complete results from corresponding panel of Table 7. Since specification 1 only differs from specification 2 in that it does not have year FEs, this table begins with column (2). Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.12: Second-Stage IV Results for Electricity Prices, without Year Fixed Effects

	(1)	(3)	(4)	(5)
$dynamic$	1.062***	1.020***	–0.813	–0.687
	(0.000)	(0.000)	(0.588)	(0.567)
$prengen$	–	–0.0783	0.158	0.309
	–	(0.156)	(0.441)	(0.178)
$deregulated$	–	–	0.446	0.247
	–	–	(0.452)	(0.573)
$growthrate$	–	–	–35.70	–24.96
	–	–	(0.200)	(0.139)
$gasprice$	–	–	0.348	0.403
	–	–	(0.192)	(0.141)
$GSPPC$	–	–	164.1	108.9
	–	–	(0.244)	(0.217)
$coalshare$	–	–	–	7.364**
	–	–	–	(0.006)
$natshare$	–	–	–	12.92*
	–	–	–	(0.018)
$hydroshare$	–	–	–	0.162
	–	–	–	(0.059)
$nucshare$	–	–	–	0.133*
	–	–	–	(0.047)

Notes: Complete results from corresponding panel of Table 7. Since specification 2 only differs from specification 1 in that it has year FEs, this table omits column (2). Individual fixed effects and cluster-robust standard errors used in all models. Standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

APPENDIX 5: CF REGRESSION TABLES WITH CONTROLS

In the tables below, the means of the regressors are excluded. Furthermore, the results for the control variables do not change significantly with the instrument is lagged, so on only regressions with $citizen$ are provided. Finally, the terms that include the generalized residuals are excluded from the second-stage regressions. The full results are available upon request.

Table A.13: First-Stage CF Results for $prengen$

	(1)	(2)	(3)	(4)	(5)
$citizen$	0.035**	–0.006	–0.002	–0.0003	–0.001
	(0.001)	(0.675)	(0.860)	(0.979)	(0.908)
$deregulated$	–	–	0.602*	0.639*	0.625*
	–	–	(0.031)	(0.022)	(0.026)
$LcontRPS$	–	–	0.274	0.292	0.213
	–	–	(0.556)	(0.515)	(0.630)
$emissions$	–	–	–0.0763	–0.0472	–0.0764
	–	–	(0.353)	(0.655)	(0.355)
$growthrate$	–	–	3.322	3.510	3.567
	–	–	(0.651)	(0.638)	(0.629)
$elecprice$	–	–	0.084	0.093	0.088
	–	–	(0.292)	(0.256)	(0.261)
$gasprice$	–	–	–0.0543	–0.0515	–0.0603
	–	–	(0.428)	(0.453)	(0.359)
$windest$	–	–	0.0004	0.00002	0.0004
	–	–	(0.801)	(0.987)	(0.794)
$solarest$	–	–	0.002	0.003*	0.002
	–	–	(0.053)	(0.019)	(0.077)
$GSPPC$	–	–	–5.422	7.971	–5.208
	–	–	(0.898)	(0.850)	(0.908)
$coalshare$	–	–	–	–0.020	–
	–	–	–	(0.297)	–
$natshare$	–	–	–	–	–0.004
	–	–	–	–	(0.768)

Notes: Complete results from corresponding panel of Table 8. Individual FEs and cluster-robust SEs used in all models. Time FEs used in models 2 through 5. SEs in parentheses. * $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.14: First-Stage CF Results for $elecprice$

	(1)	(2)	(3)	(4)	(5)
$citizen$	0.038***	–0.005	0.001	–0.014	0.003
	(0.000)	(0.742)	(0.940)	(0.389)	(0.880)
$prengen$	–	–	0.078*	0.111**	0.034
	–	–	(0.026)	(0.004)	(0.502)
$deregulated$	–	–	–	0.596	0.800*
	–	–	–	(0.095)	(0.021)
$growthrate$	–	–	–	–0.172	2.391
	–	–	–	(0.983)	(0.768)
$gasprice$	–	–	–	–0.0297	–0.0232
	–	–	–	(0.404)	(0.377)
$GSPPC$	–	–	–	–57.59*	–89.17*
	–	–	–	(0.047)	(0.016)
$coalshare$	–	–	–	–	–0.0593*
	–	–	–	–	(0.036)
$natshare$	–	–	–	–	–0.0561
	–	–	–	–	(0.053)
$hydroshare$	–	–	–	–	–0.0413
	–	–	–	–	(0.270)
$nucshare$	–	–	–	–	–0.0560*
	–	–	–	–	(0.037

Notes: Complete results from corresponding panel of Table 8. Individual FEs and cluster-robust SEs used in all models. Time FEs used in models 2 through 5. SEs in parentheses.
* $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.15: Second-Stage CF Results for $prengen$ with $citizen$ as instrument

	(1)	(2)	(3)	(4)	(5)
Cluster 2	–1.374	–0.389	0.856	0.941	0.930
	(0.158)	(0.798)	(0.532)	(0.509)	(0.491)
Cluster 3	–4.401	–3.772	1.871	1.918	2.915
	(0.240)	(0.129)	(0.513)	(0.505)	(0.288)
$deregulated$	–	–	–0.702	–0.706	–0.476
	–	–	(0.190)	(0.195)	(0.277)
$LcontRPS$	–	–	0.062	–0.010	0.220
	–	–	(0.958)	(0.994)	(0.840)
$emissions$	–	–	–0.095	–0.133	–0.154
	–	–	(0.416)	(0.394)	(0.193)
$growthrate$	–	–	4.883	4.559	–1.298
	–	–	(0.783)	(0.809)	(0.942)
$elecprice$	–	–	–0.497**	–0.517**	–0.480**
	–	–	(0.003)	(0.002)	(0.001)
$gasprice$	–	–	0.191	0.192	0.133
	–	–	(0.153)	(0.152)	(0.253)
$windest$	–	–	0.001	0.001	0.002
	–	–	(0.345)	(0.317)	(0.060)
$solarest$	–	–	–0.003	–0.003	–0.005
	–	–	(0.707)	(0.623)	(0.481)
$GSPPC$	–	–	163.6*	154.0*	103.7
	–	–	(0.014)	(0.041)	(0.112)
$coalshare$	–	–	–	0.020	–
	–	–	–	(0.582)	–
$natshare$	–	–	–	–	–0.104***
	–	–	–	–	(0.000)

Notes: Complete results from corresponding panel of Table 9. Individual FEs and cluster-robust SEs used in all models. Time FEs used in models 2 through 5. SEs in parentheses.
* $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$

Table A.16: Second-Stage CF Results for $elecprice$ with $citizen$ as instrument

	(1)	(2)	(3)	(4)	(5)
Cluster 2	3.991***	0.972**	1.153**	0.998*	0.627
	(0.000)	(0.006)	(0.003)	(0.015)	(0.089)
Cluster 3	13.43***	6.838*	5.615	2.195**	1.302**
	(0.000)	(0.027)	(0.065)	(0.004)	(0.007)
$prengen$	–	–	–0.129*	–0.098***	–0.018
	–	–	(0.011)	(0.001)	(0.451)
$deregulated$	–	–	–	–0.307*	–0.236
	–	–	–	(0.042)	(0.132)
$growthrate$	–	–	–	–5.468	–8.247
	–	–	–	(0.261)	(0.058)
$gasprice$	–	–	–	0.593***	0.604***
	–	–	–	(0.000)	(0.000)
$GSPPC$	–	–	–	47.97*	34.52
	–	–	–	(0.011)	(0.085)
$coalshare$	–	–	–	–	0.060***
	–	–	–	–	(0.000)
$natshare$	–	–	–	–	0.044***
	–	–	–	–	(0.000)
$hydroshare$	–	–	–	–	0.049**
	–	–	–	–	(0.001)
$nucshare$	–	–	–	–	0.059***
	–	–	–	–	(0.000)

Notes: Complete results from corresponding panel of Table 10. Individual FEs and cluster-robust SEs used in all models. Time FEs used in models 2 through 5. SEs in parentheses.
* $p < 0.05$; ** $p < 0.01$, *** $p < 0.001$