The Effect of Voter Control on Public Policy

by

Michael William Sances

B.A. Political Science, University of Massachusetts Amherst (2006)

Submitted to the Department of Political Science in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy
at the
Massachusetts Institute of Technology

September 2014

Signature redacted

Signature of Author: ______________________
Department of Political Science
June 30, 2014

Signature redacted

Certified by: ______________________
Andrea Louise Campbell
Professor of Political Science
Thesis Supervisor

Signature redacted

Accepted by: ______________________
Roger Petersen
Arthur and Ruth Sloan Professor of Political Science
Chair, Graduate Program Committee

The Effect of Voter Control on Public Policy

by

Michael William Sances

Submitted to the Department of Political Science on June 30, 2014
in Partial Fulfillment of the Requirements
for the Degree of Doctor of Philosophy in Political Science

ABSTRACT

In democracies, the public make decisions that affect policy. In some situations, these decisions are only indirectly related to policy: voters choose an elected executive, who then appoints an unelected policy-maker, who in turn decides policy. In other situations, these decisions are more directly related to policy: voters bypass the executive and elect the policy-maker directly. In still other situations, voters bypass the electoral process altogether, deciding policy for themselves. Do these different configurations matter? While centuries of debate over the merits of democracy have been premised on the assumption they do, there is still limited evidence that voter control affects policy. In this dissertation, I provide three empirical tests of the claim that voter control institutions matter for public policy.

The first empirical chapter examines what happens when voters lose control over property tax policy in New York towns. Consistent with expectations, voter control has large impacts on property tax policy. The second empirical chapter examines what happens when voters gain control over local education policy in Virginia school districts. In this case, policy is unaffected when voter power is increased. The third and final empirical chapter examines what happens when voters gain control over fire protection policy in Illinois special district governments. In this case, the increase in voter control happens via two channels: elections and referendums. While elections have no effect on policy, referendums cause significant changes in both policy and performance.

The final chapter concludes by considering several outstanding questions raised by the results, including the precise conditions under which voter control will matter, the implications of these results for debates over citizen competence, and the degree to which the results may be driven by elites capturing the democratic process.

Thesis Supervisor: Andrea Louise Campbell
Title: Professor of Political Science

Dedicated to the memory of Jack P. Hajj.

Acknowledgments

I began graduate school with a rather pugnacious commitment to populism, believing that voters deserve to get what they want, no matter how inconsistent or self-defeating their preferences might seem. My journey from optimism to skepticism has been long and rewarding, and has been made all the more enriching thanks to the support of numerous friends and colleagues. First and foremost, I thank my thesis committee of Andrea Campbell, Adam Berinsky, Jens Hainmueller, and Gabriel Lenz. Their constant engagement with this project, from the substantive questions that motivate it to the technical aspects that help provide the answers, has been essential to its success.

Several members of the scholarly community at MIT, Harvard, and beyond have also provided crucial pieces of feedback along the way. For their comments, encouragement, and words of wisdom, I thank Matt Baum, Devin Caughey, Joshua Clinton, Danny Hidalgo, Horacio Larreguy, Krista Loose, Michele Margolis, Marc Meredith, Kai Quek, Kay Schlozman, Jim Snyder, BK Song, Lucas Stanczyk, Jeff Tessin, Jessica Trounstine, Chris Warshaw, Yiqing Xu, and Teppei Yamamoto. I also thank seminar participants at conferences where this work was presented, including the 2011 American Political Science Association meeting, the spring 2012 Political Economy Breakfast at MIT, the 2013 American Politics conference at Yale, the 2012 and 2013 Midwest Political Science Association meetings, and the 2012 annual meeting for the Society for Political Methodology.

The data collection effort that went into this project would not have succeeded without the support of many generous organizations and individuals. For financial assistance, I thank the National Science Foundation, which provided me with a Doctoral Dissertation Improvement Grant (SES-1223187) that helped pay for public records requests; and the MIT Political Experiments Research Lab, which allowed me the use of Internet software for surveying local officials. For sharing his data on transitions to elected school boards in Virginia, I thank John Moffat. Last but not least, I extend a special thank you to the many hardworking state and local officials who provided me with their time and assistance, including those who responded to my numerous records requests, helped guide me toward the right data sources, responded to my online surveys, and participated in semi-structured interviews.

Finally, I thank my wife, Maia Hajj, for her constant support throughout graduate school and beyond. While my ideas about politics may have changed radically over the past few years, my appreciation for her has only grown.

Contents

	Abstract	**3**
	Acknowledgments	**7**
1	**Introduction**	**11**
2	**Assessing Policy Effects: Decreasing Democracy in New York Towns**	**23**
3	**Failing the Test: Increasing Democracy in Virginia School Districts**	**79**
4	**Playing with Fire: Increasing Democracy in Illinois Special Districts**	**111**
5	**Conclusion**	**149**

Chapter 1

Introduction

In democracies, the public make decisions that affect policy. In some situations, these decisions are only indirectly related to policy: voters choose an elected executive, who then appoints an unelected policy-maker, who in turn decides policy. In other situations, these decisions are more directly related to policy: voters bypass the executive and elect the policy-maker directly. In still other situations, voters bypass the electoral process altogether, deciding policy for themselves.

Do these different configurations matter? Centuries of debate over the merits of democracy have been premised on the assumption they do. On one hand, democratic critics argue voters are incapable of seeing beyond their own narrow interests, and that policy control must be indirect. Others argue that such concerns are elitist at worst, and misguided at best. Voters, these defenders argue, do the best with the information they have, and policy would benefit if elites simply stepped out of the public's way. While both sides fervently disagree about the quality of voter opinion, they share the belief that the level of voter control matters for policy.

Yet despite the long pedigree of this debate, whether direct control actually matters for policy is still very much an open question. This dissertation is primarily an exercise in empirically testing this core assumption. The substantive content is three quantitative studies, all cases in which voters are granted more or less control over policy. While the studies are quantitative, the methodology is straightforward: when voters gain control over policy, does policy change?

How is it possible that such a fundamental question about democracy – if voter control changes, does policy change? – is still largely unsettled? Certainly, this lacuna is not without justification. On one hand, scholars of public opinion have focused on assessing the quality of mass opinion. If voters can be shown to be incompetent, then this implies something about the effects of voter control on policy. Yet for all the studies of citizen competence that draw conclusions about the impact of institutions, none has actually tested whether institutions matter. On the other hand, scholars of institutions have attempted to assess the effects of voter control, but have been frustrated by several empirical challenges. For one, institutions of voter control rarely vary, which means simple comparisons are difficult to come by. For another, when institutions do vary, they are likely correlated with many other factors that influence policy, confounding comparisons.

In the following section, I review existing studies of whether voter control affects policy. I then briefly describe the three studies that make up this dissertation, discuss the research design elements that help me to overcome the empirical challenges in the existing literature, and offer a summary of my findings. In the final section, I offer some concluding thoughts that I expand upon in the final chapter of the dissertation.

Direct Control and Policy Outcomes: A Review

As mentioned, that voter control matters for policy is something on which supporters and critics of democracy agree. Evidence for this agreement may be found in the numerous studies of citizen competence, which typically end in prescriptions for institutional design. James Madison in Federalist 10 is perhaps the best example: in this essay, Madison first describes the public as incompetent, and then concludes that, given this incompetence, the public's influence over policy should be limited to the selection of competent leaders.

Following Madison's lead in Federalist 10, many academic studies of public opinion will first describe citizen incompetence, and then conclude with a call for limiting (or increasing, depending on the results) voter control. For example, Achen and Bartels (2004) argue that voters are myopic in their assessments of the economy. From this, they conclude that democracy must be limited to "not policymaking power but a veto, with regularly scheduled opportunities to exercise it" (42). Kuran and Sunstein (1998) follow a similar pattern in their discussion of the mass public's statistical illiteracy (Kahneman 2003). After cataloging these limitations, these authors argue that bureaucrats must be protected from the public, and they offer "proposals...to give civil servants better insulation against mass demands for regulatory change" to this end (683). Those who argue that citizens are in fact competent also make prescriptions. For example, Page and Shapiro (1992), who argue that the mass public is much more competent in the aggregate than at the individual level, declare that "The chief cure for the ills of American democracy is to be found not in less but in more democracy" (3).

Yet as much as these prescriptions are made, none of these studies has tested whether voter control matters for policy. While it is surely important to assess the public's competence – as competence likely conditions the impact of institutions – the simple fact is that these studies do not observe variation in voter control, and so have no information about the effects of such variation.

Separate from the literature on citizen competence, other scholars have attempted to assess the effects of giving voters more power through various institutional means. Most closely related to the competence literature are studies of direct democracy in the American states. In a review of this literature, Lupia and Matsusaka (2004) note that "Questions about voter competence are a common facet of direct democracy debates. Many people believe that ordinary citizens are incompetent because they base their political choices on limited factual foundations," and thus, "it is difficult to imagine that voters are competent to make the kinds of policy decisions with which direct democracy confronts them" (467). Despite these concerns, however, these authors surmise that voters are able to use cues to make sound decisions about direct legislation. For example, Lupia (1994) finds that voters in a California referendum use endorsements by industry groups to make reasoned choices, despite knowing next to nothing about the technical details of the proposal.

Evidence of cue-taking, however, is not in itself informative about the impact of institutions. The reason is that such evidence is typically gathered in contexts where direct democracy does not vary, such as Lupia's study of the California insurance referendum. In effect, these studies truncate the data by focusing only on cases where direct democracy has already been implemented. Thus, like the competence literature more generally, they draw conclusions about the effects of institutional change from situations in which institutions do not vary.

This limitation is easy to understand, once the analyst begins to search for situations where voter control does vary. It turns out that in many contexts, it rarely does: political institutions, particularly at the national level, tend to remain in place once established, which makes it impossible to compare national policy under more or less voter control. As a result, scholars of direct democracy have looked to the states, some of which allow voters to make decisions via the initiative, and some of which do not. This allows researchers to compare policy between the two groups of states, and hopefully learn something about the impact of direct democracy. As Lupia and Matsusaka (2004) write:

> A common approach is to regress a policy variable on a set of control variables and a dummy variable that equals 1 for states with the initiative process. If policy differences remain after controlling for other known determinants of policy outcomes, such as demographics and political variables, the differences are ascribed to the availability of the initiative process. (473)

Lupia and Matsusaka note that several studies using this approach have concluded that direct democracy matters for fiscal policy, typically by lowering taxes and decreasing government budgets (e.g., Matsusaka 1995; Matsusaka 2000; Feld and Matsusaka 2003).

However, while these studies have overcome a fundamental problem in estimating the effect of voter control – namely, an absence of variation in voter control – there is a more pernicious issue that they have failed to address. As Lupia and Matsusaka write, existing studies of the effects of voter control "face the familiar problems associated with nonexperimental data" (474). The contrast to experiments here is instructive: in an experiment, the assignment of the treatment is arbitrary, meaning that it is unrelated to any other characteristics by design. In contrast, political institutions are not arbitrarily assigned, meaning that it is very difficult to attribute differences in outcomes to differences in institutions.

For example, suppose some states have direct democracy, some states do not, and we observe that states with direct democracy have lower taxes. One explanation is that direct democracy *caused* taxes to be lower. But a radically different explanation is no less plausible: perhaps the states with direct democracy are also different on some other dimension that also affects taxes. To see this, suppose that the true effect of direct democracy on taxes is zero, and that citizens who hate taxes live in states with direct democracy. In this scenario, taxes would have been low in direct democracy states even in the absence of direct democracy. However, if we simply compared states with and without direct democracy, we would observe the direct democracy states have lower taxes. Alternatively, suppose again that the true effect of direct democracy is zero, that voter prefer high taxes, and that voters in low-tax states adopt direct democracy with the hope of achieving higher taxes. In this scenario, we would again observe that states with direct democracy have lower taxes, but we would be wrong to conclude that direct democracy caused taxes to be lower.[1]

[1]As the excerpt above suggests, existing studies have attempted to deal with these problems by adjusting for "other known determinants of policy outcomes" in a regression.

Finally, many economists have tested for an effect of voter control in the form of direct elections. Rather than studying direct voter control via the initiative, these studies test whether policy changes when officials are directly elected by voters as opposed to appointed by other officials. In the best-known study, Besley and Coate (2003) conclude that electing state energy regulators results in lower utility bills for consumers. Yet many other studies have found that direct elections fail to make an impact. Partridge and Sass (2011) review 30 studies of direct elections, 15 of which compare city spending under elected mayors vs. appointed managers. They note that 11 of these 15 studies found no difference in spending, two found it to be lower with elected mayors, and two found it to be higher.

Many of these studies face the same empirical challenges as the direct democracy literature. For example, Besley and Coate (2003) use states as their unit of analysis, which means that there could be many other differences across states that explain the observed difference in energy prices. On the other hand, many of the studies cited by Partridge and Sass use cities as the unit of analysis. This approach is more promising, given that cities, unlike states and countries, tend to exhibit more variation in political institutions. Especially promising are cities that change their institutions across time: by comparing outcomes before and after the switch, the researcher can rule out many sources of confounding.

However, the studies reviewed by Partridge and Sass have not fully exploited this design: only 8 of the 30 studies reviewed use data in which the same units are observed over time. For instance, in one study of state judges, the number of states that switch between

---

Such a strategy assumes that the researcher has correctly measured and specified all possible determinants of policy that correlate with institutions. This assumption is usually quite strong, but is even more so when studying institutions, which are often the result of unobservable and strategic behavior on the part of elites and voters (Acemoglu 2005).

election and appointment is 3 out of 48 (Besley and Payne 2005). In the studies comparing elected mayors to appointed city managers, the ratios of switching to non-switching cities are 15/204 (Jung 2006), 10/119 (Vlaicu 2008), 25/2,563 (Coate and Knight 2009), and 102/1,546 (Enikolopov 2010). The small number of switchers in these cases creates several problems. For one, there may not be enough variation in the treatment variable to detect an effect on policy. More importantly, it is likely that the small group of treated cities are different from the much larger group of untreated cities on several other important dimensions, differences that city fixed effects may not fully account for.

Local Governments as Testing Grounds

To summarize, the existing literature on voter control has faced two key problems. First is the lack of variation in institutions of voter control, preventing comparisons. Second is that the variation that does exist at the level of countries, states, and cities is often correlated with other factors that affect policy, confounding comparisons.

To address these issues, I go one step beyond the existing literature – and one level below – be leveraging the large amount of subnational variation in political institutions in the United States. Local institutions in the United States, as in other federalist democracies, are highly dynamic, with many different methods of voter control that differ across states and often change over time. That the institutions of control change *over time* is crucial for my research design. When voter control varies within units, and across time, I can estimate the effect of voter control on policy while holding numerous other factors constant. Additionally, each of my studies focuses on units within a particular state. This means that the untreated units provide a more plausible comparison group than would be the case if, for example, I were to compare different cities across states.

The main institution of voter control I study is the direct election. In the first case, I examine what happens when voters lose the power to elect property tax officials. In the state of New York, property tax administration is largely left to the 932 towns, where officials known as tax assessors decide how often property should be revalued. Originally, all towns elected their tax assessors. Over the past four decades, towns gradually shifted toward appointed assessors. In this chapter, I examine the effects of limiting voter control on local assessment policy. I find that elected tax assessors are much less likely to conduct revaluations than their appointed counterparts, and that this has large implications for the equity of the tax.

In the second case, I ask what happens when voters gain the power to elect local education officials. Beginning in 1992, and continuing throughout the decade, most of Virginia's 132 school districts embraced democracy by switching from appointed to elected school boards. In contrast to the property tax case – and the dominant assumption in the existing literature on school boards – I find no impact of voter control on policy, here measured as spending, revenues, teacher salaries, and class sizes.

In the third case, I expand my focus to an additional institution of voter control: the referendum. In rural and suburban Illinois, fire protection is provided by over 800 fire protection districts, a type of "special district government," that are governed by boards of trustees. In the 1990s, two reforms gave voters more power over the policy decisions of these board members. First, some districts transitioned from appointed to elected trustees. Second, some districts became subject to property tax limitations, which mandated that any tax increases be approved by voters in a referendum. I test whether these institutions affected districts' fiscal policy, as well as the quality of fire protection. I find that referendums decrease tax revenue and increase emergency response times, whereas there is no discernible effect of electing board trustees.

Outstanding Questions

In this dissertation, I provide empirical evidence for a core assumption in democratic politics: that when voters gain control over policy, policy will change. I perform this test in three diverse cases, in which I assemble original data and apply a novel methodology that allows me to rule out many confounding influences. I find that sometimes voter control has large impacts on policy, while at other times it does not. It is therefore natural to ask what explains the divergent outcomes across these cases. In the concluding chapter, I discuss some possible explanations for this divergence.

It is also noteworthy that, in the cases where voter control does affect policy, the policy effects are easily seen as negative for social welfare. For example, when tax assessors are elected, the equity of the property tax suffers; when voters gain control over fire department budgets, emergency response times increase. Critics of democracy, who have long pointed to the dangers of too much voter control, may find new ammunition in these results. Yet, defenders of democracy will probably not yield that easily: how can we fairly judge whether "welfare" actually suffered in these cases? Who are we to decide what is good for the voters, when voters tell us otherwise? Such normative debates are informed by my results, but are ultimately beyond the scope of this dissertation. Nonetheless, in the conclusion I offer some thoughts along these lines.

Finally, it is worth unpacking "voter control" by disaggregating "voters" as a whole. When voter control is increased, it is unlikely that all voters participate equally, given what we know about participatory biases. Thus, results showing less equity, lower taxes, and worse performance may make intuitive sense if we believe that "voter control" in theory is actually "elite capture of democracy" in practice. My results, which do not include data on participatory bias, can not speak directly to this question. However, in the conclusion I consider how this may alter the interpretation of my results.

References

Acemoglu, Daron. 2005. “Constitutions, politics, and economics: A review essay on Persson and Tabellini's *The Economic Effects of Constitutions*.” *Journal of Economic Literature* 43(4): 1025-1048.

Achen, Christopher H., and Larry M. Bartels. 2004. “Musical chairs: Pocketbook voting and the limits of democratic accountability.” Paper presented at the Annual Meeting of the American Political Science Association, Chicago.

Besley, Timothy, and Stephen Coate. 2003. “Elected versus appointed regulators: Theory and evidence.” *Journal of the European Economic Association* 1(5): 1176-1206.

Besley, Timothy, and A. Abigail Payne. 2005. “Implementation of Anti-Discrimination Policy: Does Judicial Selection Matter?” lSE STICERD Research Paper No. PEPP04.

Coate, Stephen, and Brian Knight. 2009. “Government form and public spending: Theory and evidence from US municipalities” Working Paper, National Bureau of Economic Research.

Enikolopov, Ruben. 2008. "Politicians, bureaucrats and targeted redistribution: the role of career concerns." Working Paper, Barcelona Institute for Political Economy and Governance.

Feld, Lars P., and John G. Matsusaka. 2003. “Budget referendums and government spending: evidence from Swiss cantons.” *Journal of Public Economics* 87(12): 2703-2724.

Jung, Changhoon. 2006. “Forms of government and spending on common municipal functions: a longitudinal approach.” *International Review of Administrative Sciences* 72(3): 363-376.

Kahneman, Daniel. 2003. “Maps of bounded rationality: Psychology for behavioral economics.” *American Economic Review* 93(5): 1449-1475.

Kuran, Timur, and Cass R. Sunstein. 1998. “Availability Cascades and Risk Regulation.”

Stanford Law Review 51: 683-768.

Lupia, Arthur. 1994. "Shortcuts versus encyclopedias: information and voting behavior in California insurance reform elections." *American Political Science Review* 88(1): 63–76

Lupia, Arthur, and John G. Matsusaka. 2004. "Direct Democracy: New Approaches to Old Questions." *Annual Review of Political Science* 7: 463–82.

Matsusaka, John G. 1995. "Fiscal effects of the voter initiative: Evidence from the last 30 years." *Journal of Political Economy* 103(3): 587-623.

Matsusaka, John G. 2000. "Fiscal Effects of the Voter Initiative in the First Half of the Twentieth Century." *The Journal of Law and Economics* 43(2): 619-650.

Page, Benjamin I., and Robert Y. Shapiro. 1992. *The Rational Public: Fifty Years of Trends in Americans' Policy Preferences*. Chicago: University of Chicago Press.

Partridge, Mark, and Tim R. Sass. 2011. "The productivity of elected and appointed officials: the case of school superintendents." *Public Choice* 149(1-2): 133-149.

Vlaicu, Razvan. 2008. "Executive performance under direct and hierarchical accountability structures: Theory and evidence." Working Paper, Department of Economics, University of Maryland.

Chapter 2

Assessing Policy Effects: Decreasing Democracy in New York Towns

Critiques of public opinion often end in prescriptions for institutional design. "As long as the reason of man continues fallible," James Madison writes in Federalist 10, "and he is at liberty to exercise it, different opinions will be formed." These different opinions inevitably lead citizens to form "factions," whose "impulse of passion" is at odds with "the permanent and aggregate interests of the community." From these observations, Madison concluded that policy should be left to elites, "a chosen body of citizens, whose wisdom may best discern the true interest of their country." Many years later, Achen and Bartels (2004) conclude their study of myopic voting in national elections by warning against excesses of democracy at the state and local level. "Our self-deceptions about our own wisdom," they write, "sometimes have real consequences, particularly at the state level, where elite safeguards are likely to be less institutionalized" (Achen and Bartels 2004, 43).

In the years separating these two critiques of public opinion, extensive evidence has accumulated that public opinion is in fact driven by "impulses of passion" and incapable of "discerning the true interest of the country." To be sure, the degree to which the public is competent is still a subject of debate. Yet it is notable that the conclusions of these critiques– that limits on democratic control can sometimes advance the public interest

– remain relatively untested. Thus, while we now know a great deal about the flaws of public opinion, we know much less about what these flaws mean for the design of democratic institutions.

My goal in this paper is to empirically test whether limits on democracy can improve welfare in the manner suggested by Madison, Achen and Bartels, and other critics of popular democracy. To perform this test, I exploit a quasi-experiment involving 920 towns in New York state. Between 1970 and 2010, almost all towns imposed limits on democratic control over property tax policy, gradually shifting from electing to appointing their property tax assessor. I show that this greater insulation from voters improves welfare: property valuations are more accurate, updates to these valuations are more frequent, and the distribution of the tax burden is more uniform. As a robustness check, I focus on a subset of the transitions induced by a plausibly exogenous state law, finding the same effect as in the main sample. Taken together, the results show that limiting democracy can have large, positive, effects on public welfare, and that evidence of voter incompetence has real implications for the design of institutions.

Voter Competence and Limits on Democracy

As it was written well before scientific polling, Madison's negative view of public opinion might be dismissed as merely anecdotal. The same could also be said for the complaints of the early 20th century writer Walter Lippmann, who concluded that policy is safer when controlled by "a specialized class whose interests reach beyond the locality" (Lippmann 1922, 310). Yet systematic data on public attitudes, provided by scientific polls beginning in the 1950s and 1960s, proved no less unsettling. It turned out that most citizens were ignorant of basic political facts and concepts (Converse 1964; Delli Carpini and Keeter 1996). Thus, the prospect of voters forming detailed policy preferences and judging politicians based on "issues," "facts," "alternatives," and "consequences" (Berelson, Lazarsfeld, and McPhee 1954, 308) suddenly seemed dubious.

In light of this evidence, models of voter behavior were scaled back considerably. Instead of making detailed judgments on complicated policy issues, voters were said to simply judge incumbents on how well the economy performed over the past four years (Fiorina 1981). Yet even within this more limited view, the debate over the quality of public opinion continues. Achen and Bartels (2004) argue the public is incapable of competent retrospective voting, as voters myopically weight the election year more when intending to judge the incumbent's entire term (see also Healy and Lenz 2013). Others find voters judge incumbents not only on economic performance, but also on unrelated events such as natural disasters (Achen and Bartels 2013) and sporting contests (Healy, Malhotra, and Mo 2010; Miller 2013).[1]

Thus, the concerns of Madison, Lippmann, and other critics of democracy now appear well-founded in empirical research: voters lack information, have trouble effectively using what little information they do have, and even factor irrelevant information into their political choices. Whether these concerns justify the conclusions of these critics – namely, that limits on democracy would improve welfare – is often argued, but rarely tested. For example, Achen and Bartels (2004) argue that popular control over policy decisions will cause harm due to citizens' misunderstanding of the issues. Kuran and Sunstein (1998) argue that government officials in charge of risk should be more insulated from a public incapable of statistical reasoning (Kahneman 2003). Defenders of the public's wisdom also make prescriptions for institutional design; for example, Page and Shapiro write that "The chief cure for the ills of American democracy is to be found not in less but in more democracy" (1992, 3). Yet empirically, all three of these studies examine contexts where institutions do not vary, and thus where nothing can feasibly be learned about the effects

[1]Though see Ashworth (2012) and Ashworth and Bueno de Mesquita (2013) for a critique of the literature on seemingly irrelevant events and voting behavior.

of limiting democracy via institutional change.

Studies of direct democracy, in contrast, do often connect voter competence debates to data on institutional design. Reviewing this literature, Lupia and Matsusaka (2004) write that "Questions about voter competence are a common facet of direct democracy debates. Many people believe that ordinary citizens are incompetent because they base their political choices on limited factual foundations," and thus, "it is difficult to imagine that voters are competent to make the kinds of policy decisions with which direct democracy confronts them" (467). Despite these concerns, however, these authors surmise that voters are able to use cues to make sound decisions about direct legislation (Lupia 1994; Bowler and Donovan 1998). Unfortunately, the studies of cue-taking cited by the authors are once again conducted in contexts where direct democracy does not vary. In effect, these studies often truncate the data by focusing on cases where direct democracy has already been implemented. As a result, whether direct democracy can harm policy remains an open question. Indeed, Lupia and Matsusaka caution that "Research specifically devoted to questions of voter competence in direct democracy is a relatively new phenomenon" (470).[2]

---

[2]As this brief review shows, there is no single definition of voter competence. For example, one view of competence implies a knowledge of facts, including facts about what policies are "correct" in the sense that they lead to better social outcomes (Madison; Achen and Bartels 2004). In an alternative view, competence simply means forming consistent preferences and holding officials accountable for failing to satisfy those preferences (Druckman 2001; Healy and Malhotra 2009). While the definition of competence employed in this paper is closer to the first, I also present evidence in the penultimate section that the second, more subjective definition also fails to hold in this case.

Challenges to Estimating the Effect of Limiting Democracy

Three empirical challenges have prevented a more thorough accounting of the concerns raised by Madison, Lippmann, and other critics regarding the implications of public opinion for democratic design. The first challenge is that "limits on democracy" are rare at the level of states or countries. Madison himself was concerned with the design of federal institutions, yet the effects of national institutions are notoriously hard to quantify. Comparisons of state institutions face the same issues. For one, there are only a finite amount of countries and states, and differences between institutions are typically accompanied by differences in other factors that also affect policy. Thus, existing cross-sectional comparisons of institutions, including comparisons between states with and without direct democracy, are prone to omitted variable bias. Moreover, because political institutions may themselves be a result of policy, there is a reverse causality problem that simply adjusting for measurable covariates will not address (Acemoglu 2005).

The second challenge is that measures of public welfare are often controversial. Most studies of the policy effects of direct democracy, for example, use total government spending as an outcome measure. While intuitive, the normative value of spending is debatable. Further, such an aggregated measure can mask large variations in citizens' actual welfare. Lupia and Matsusaka do cite a handful of studies that attempt to measure the effect of direct democracy on more granular measures of performance. Yet they conclude these studies "face the familiar problems associated with nonexperimental data ... so the findings should be viewed as preliminary" (Lupia and Matsusaka 2004, 474).

The third challenge is to link the policy effects of institutions back to public opinion. While there are existing studies of the effects of democratic institutions, these are largely disconnected from the literature on voter competence. For example, Besley and Coate

(2003) compare energy prices between states with democratically elected energy regulators, and states with more insulated appointed regulators. They interpret lower electricity prices in states with elected regulators as consistent with responsiveness to public opinion, but they do not speculate as to whether these preferences are well-informed. Similarly, Gordon and Huber (2007) find that Kansas judges subject to competitive elections are more punitive than judges subject to retention elections; but again, these authors do not connect their result to questions of voter capabilities. Indeed, in both these studies, it is not clear that citizen preferences are the driving force behind the observed effects. Neither study presents evidence that voters feel one way or the other about the policy issue in question, so it is possible that the effect is due to some other factor, such as stronger professional norms among appointed officials. To the extent this is true, it is difficult to interpret the results of such studies in terms of public opinion.

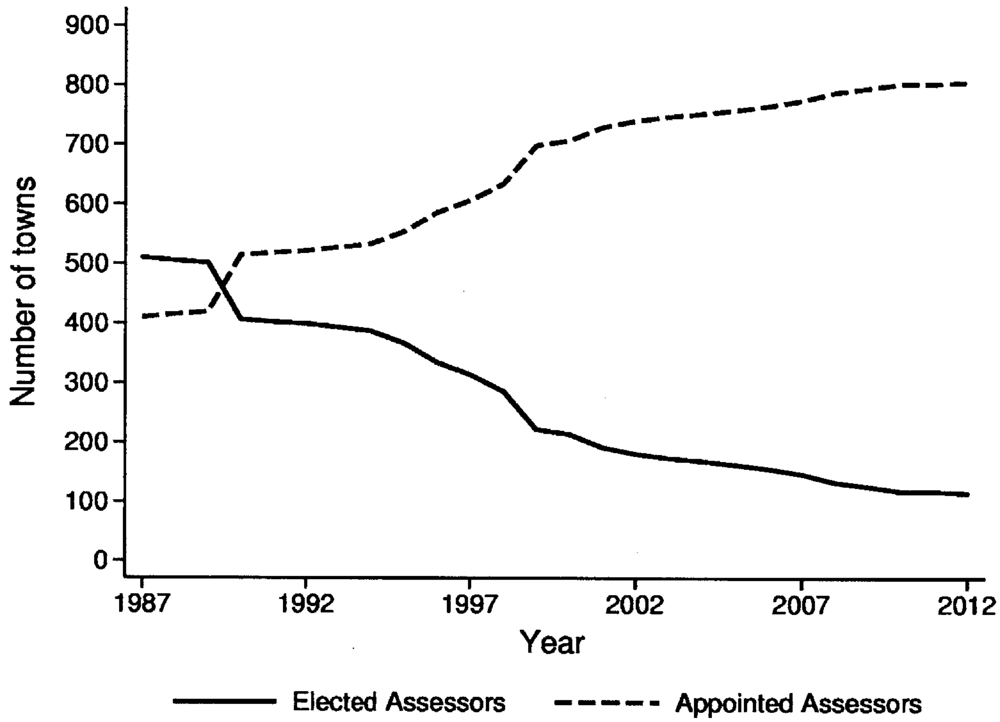
Limiting Democracy in New York Towns: A Quasi-Experiment

Overcoming omitted variables bias and reverse causality requires a research design that approximates a randomized experiment, which ensures the only relevant factor separating the treated group from the comparison group is the value of the treatment variable (Angrist and Pischke 2008). Lacking a truly randomized trial, I leverage a quasi-experiment involving New York towns. In these towns, property tax policy is determined by an official known as the tax assessor. Over time, most towns in the state have limited democracy by changing from electing to appointing their assessors. Figure 2.1 plots these transitions across time, from 1982 to 2012.[3]

Studying towns in a particular state ensures that many factors, such as state institutions

[3] I describe data collection in the Appendix.

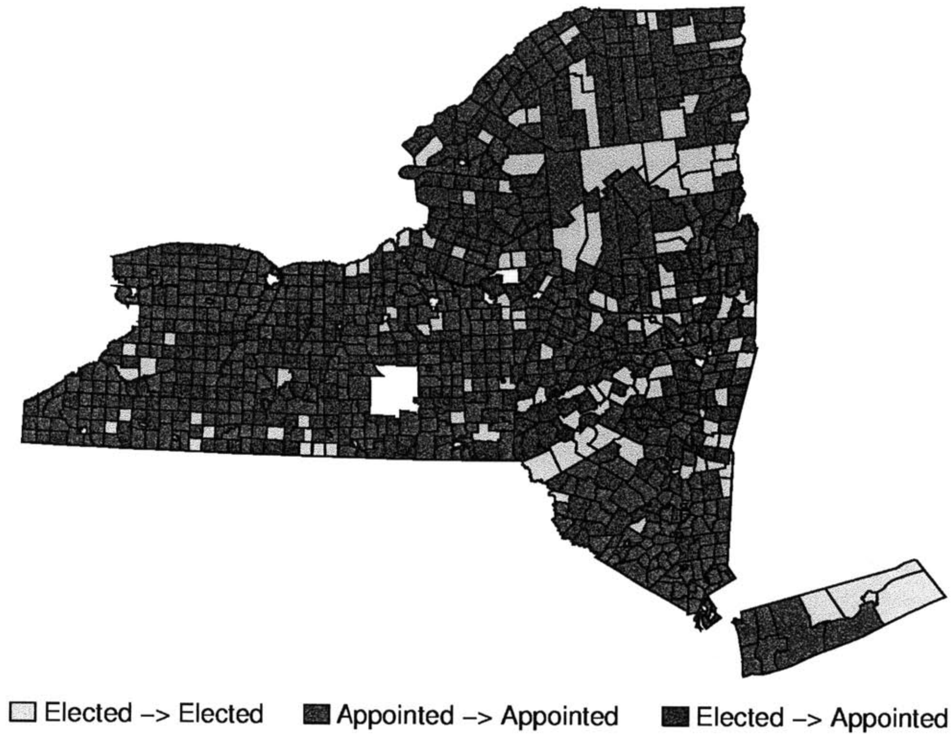
Figure 2.1: Limiting the democratic control of tax assessors in New York towns.



Notes: This figure plots the number of New York towns with elected and and appointed property tax assessors between 1987 and 2012. Over time, there has been a marked shift away from democratically elected assessors and toward appointed assessors.

and culture, are held constant when comparing institutions. Further, because the switches happen in multiple years, I am able to use a difference-in-differences design (Angrist and Pischke 2008). First, I compare outcomes within towns that switch in a given cohort, before and after these towns limit democracy (first difference). Then, I compare this to the difference in outcomes within towns that do not switch in this period, before and after towns in the first cohort limited democracy (second difference). The advantage of this design is that it rules out any potential omitted variable that is fixed over time, such as a town's inherent policy preference. Further, the ability to compare outcomes before and after the switch to less democracy, but within the same town, bolsters the assumption of

Figure 2.2: Transitions from elections to appointments, 1987-2012.



Notes: This figure maps transitions from elected to appointed tax assessors in the 920 sample towns. All New York cities, as well as all municipalities in Herkimer and Nassau counties, are omitted from this map.

no reverse causality.

Figure 2.1 shows that the majority of towns, about 500 out of 920, were electing their assessors in the late 1980s; by the early 1990s, the majority were appointing. As of 2012, only about 110 of the 920 towns retain the elected system.[4] Figure 2.2 maps these

---

[4]There are 932 towns, 62 cities, and 551 villages (sub-town units) in New York state. Because institutions differ across these three types, I focus only on towns. The state has 57 counties, and two counties, Nassau and Tompkins, have county-wide assessors (New York State Department of Taxation and Finance 2012a). Excluding the three towns in Nassau and the nine in Tompkins, the population of interest is 920 towns.

transitions, which have not been limited to any particular area of the state. This is to be expected, as originally all towns in New York elected a three-member board of assessors with a chair, and thus all towns are eligible for the "treatment." Pressure to change comes from the state government, which in 1970 mandated all towns switch to appointments by the town council, unless they passed a referendum to keep the elected system (New York State Department of State 2011). Since 1970, the state has continued to encourage towns to switch. Town councils ultimately make these decisions, typically with little input from voters or assessors.[5]

Measuring Welfare: The Assessor and Local Tax Policy

The case of tax assessors also provides measures of welfare that are less ambiguous than typical indicators such as spending. Assessors play a key role in administering the local property tax by estimating the value of each property. These estimated values are used to calculate each property owner's tax bill. For example, if two homes are each worth $100,000, both are assessed at 100% of their market value, and the tax rate is one percent, then each homeowner will pay $100,000 * 100% * 1% = $1,000. Yet if one of these homes is assessed at 90% of its market value, and the other is assessed at 50%, then the first pays $900 and the second pays $500. In effect, the first homeowner subsidizes a tax break for her neighbor. While assessors have no control over tax rates or revenues, the valuation process can have substantial distributional consequences.

Because there was no pre-existing list of these transitions, I relied on several sources to measure the year that each town switched. In the cases where I could not pinpoint the exact year of the switch year, I constructed lower and upper bounds. I show in the Appendix that dropping the uncertain cases does not change the basic results.

[5]I describe accounts of some of these transitions in the Appendix.

State governments have long been concerned about assessments due to their impact on equity between and within localities. For one, inaccurate estimates of local property wealth could lead to towns receiving more local aid than they should. When wealth is misrepresented in this way, the state has to adjust local estimates of property wealth using a correction factor. Within a town, variations in assessments can translate into certain homeowners paying much more, as a percentage of their home's value, than their neighbors. For these reasons, New York state strongly encourages valuing all property at 100% of market value, and legally requires that all properties be valued at a uniform percentage of what they are worth (New York State Office of Real Property Services 2007).[6]

Assessment inequities typically result from a failure to update assessments with changing market conditions. As homes increase in value, the assessor's most recent estimate of value will stray further and further from the truth. To the extent that different properties appreciate at different rates, lag between assessments will mechanically create winners and losers who pay disproportionate shares of the tax burden. As the New York State Department of Taxation (2012b) advises: "The fairness, or equity, of the real property tax depends on whether similar properties are treated alike ... Municipal-wide reassessments are the best way to ensure that assessments are fair and accurate." Academic scholarship on the uniformity of the property tax also advises regular updates. As McMillen and Weber (2008, 654) summarize this literature, "The primary explanation put forth for" inequalities in assessments "is that higher–priced properties may appreciate more quickly relative to the natural lag in assessments."

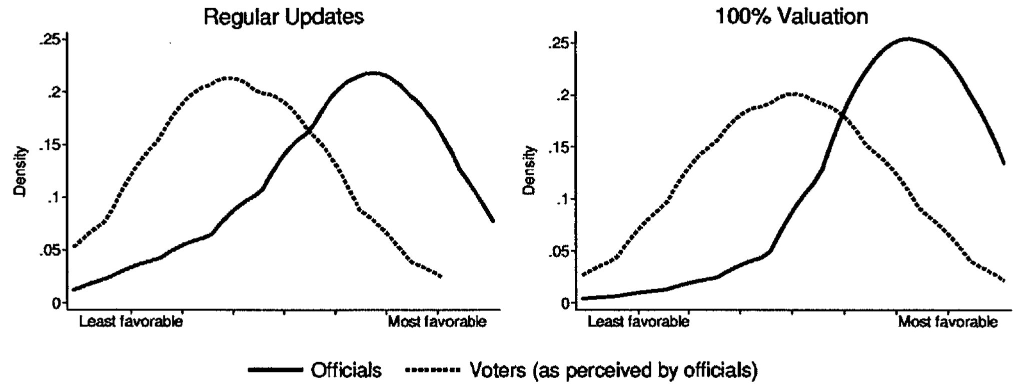
[6]These standards are consistent with those in place in many other states (Malme 1991), as well as the guidelines of the International Association of Assessing Officers (2013).

The Role of Public Opinion

An additional benefit of this case is that property taxes are highly salient to voters. National surveys consistently reveal voters dislike the property tax more than any other state or federal tax (Gallup 2012). Further, voters appear to have intimate knowledge of the assessment process and hold strong views on assessment policy. A recent sociological account of the tax revolts of the 1970s and 80s traces these episodes back to voter opposition to accurate assessments (Martin 2008). In New York state, public opinion has long been blamed for poor assessment administration. In the 1920s, a state state survey found that "Politics – assessors looking toward reelection" was among the most common reasons given by assessors for why they do not value property at 100% (Pond 1931). A 1938 state constitutional convention report complained that "assessors who are dependent for the continuance of their office on pleasing the voters are not free to make fair assessment" (New York State Constitutional Convention Committee 1938, 157). In the 1970s, another state survey of assessors found "entrenched hostility and much misinformation" among voters regarding assessment updates (Murphy 1984, 16). Newspaper accounts of revaluations from the 1990s and 2000s describe assessments as a "political hot potato," "politically perilous," "political suicide," and a "Pandora's box that few politicians really want to touch."[7]

[7]These accounts are described in more detail in the Appendix.

Figure 2.3: Public vs. official opinion on tax assessment policy in New York towns.



Notes: This figure compares the opinions of town officials on assessment policy to the perceived opinions of town voters. While town officials strongly support assessing all properties at 100% of market value and holding regular town-wide updates, they perceive voters as opposing these policies.

My own survey of town officials in New York supports the view of public opinion as a key factor in assessment policy. I e-mailed a survey to all assessors and town councils in the approximately 800 towns with accessible e-mail addresses, receiving responses from 233 officials in 153 unique towns.[8] I asked these officials their own views on regular assessment updates 100% valuation; I also asked them to characterize how their voters feel about these issues. I show the results in Figure 2.3. The left panel of Figure 2.3 plots the density of officials' favorability toward performing assessment updates on a regular basis (solid line), as well as how officials' perceive voters favorability toward this position (dashed line); the right panel plots these densities for the issue valuing all properties at 100% of market value. For both issues, there is a clear divide between official and voter opinion: officials' perceive voters to be skewed toward opposing these policies, but they

[8]I received at least one response from 18.5% of towns surveyed, and the towns that completed surveys do not appear to be unrepresentative. Further details on the survey are available in the Appendix.

see themselves as strongly supporting them.[9]

Thus, in contrast to many policy domains, tax assessments provide a clear case of a public ruled by "impulses of passion" that conflict with broader societal goals of efficiency and equity. Surveys of town officials, however, suggest that local elites are able to look beyond narrow interests and support policies that lead to more uniform treatment of homeowners. These facts would appear to support the claims of Madison, Lippmann, and others that limiting democracy would improve public welfare in this case. In the the next section, I empirically test this prediction.

Limiting Democracy Improves Welfare

In this section I show that limiting democracy has positive effects on three measures of welfare. The first outcome is simply a binary measure of whether a town conducted a municipal-wide update to assessed values in a given year. As stressed previously, conducting such updates is essential for ensuring a uniform distribution of tax burdens in light of market changes.

The second outcome measures the degree to which, in the aggregate, assessed values deviate from market values in a given town and year. This statistic, henceforth referred to as the *assessment rate*, represents the town's total assessed value divided by the state's estimate of total market value in the town, with the state's estimate based on recent real estate sales and market trends. Higher values indicate that properties are valued closer to 100% of what they are worth.

Data on these first two indicators were available for most years between 1987 and

---

[9]That these officials likely perceive of themselves as faithful representatives of voter opinion likely masks the true extent of disagreement.

2012.[10] For each measure of welfare, I estimate a difference-in-differences regression of the form,

$$outcome_{jt} = \beta * appoint_{jt} + \sum_{k=1}^{K} \pi^k * x_{jt}^k + town_j + year_t + e_{jt}$$

where j indexes towns and t years; $appoint_{jt}$ is an indicator equal to 1 if the town appoints its assessor and 0 if it elects; x_{jt} is a vector of K town demographic variables;[11] $town_j$ is a town fixed effect; $year_t$ is a year fixed effect; and e_{jt} is an error term assumed to have a conditional mean of zero. The key advantage of this design over previous studies is the inclusion of town fixed effects, which ensure that β represents the change in outcomes that occurs when $appoint_{jt}$ changes from 0 to 1 *holding all time-invariant confounders constant*. Further, the inclusion of time-varying covariates partially accounts for potential confounders that change over time, while the inclusion of year fixed effects account for changes over time that affect all towns equally. Finally, to account for the fact that outcomes are correlated within towns and across years, I cluster standard errors at the town level.

The third outcome measures uniformity: the degree to which deviations from 100% valuation vary across homeowners. Using data on all residential, single-family home sales between 2003 and 2011, I measure individual assessment rates for each property i that sold in town j and year t.[12] I then test the degree to which these rates vary as a

[10] Data on updates were available only through 2011. Please see the Appendix for details on data collection.

[11] These include population, population density, percent White, median income, percent under age 18, percent age 65 and older, percent farmer, percent unemployed, and percent with a high school degree. All demographic variables are from the decennial census and are linearly interpolated between Census years.

[12] To remove extreme observations, I restrict the data to homes selling at between $10,000

function of the sale price. Formally, I estimate a regression of the form,

$$\begin{aligned} assessment\ rate_{ijt} &= \beta_1 * appoint_{jt} + \beta_2 * price_{ijt} + \beta_3 * appoint_{jt} * price_{ijt} \\ &\quad + \sum_{k=1}^{K} \pi^k * x_{jt}^k + town_j + year_t + u_{ijt} \end{aligned}$$

In these regressions, β_2 represents the degree to which assessment rates decline as sale prices increase, conditional on a town electing its assessor. Given the legal requirement of uniform assessments, it should be the case that $\beta_2 = 0$. How this relationship changes when a town changes from electing to appointing its assessor is captured by β_3. Thus, higher values of β_3 indicate that limiting democracy brings assessments more in line with the normative and legal benchmark of uniformity.

I show the results of these regressions in Table 2.1. Columns 1 and 2 present the results for town-wide updates, first without covariates and then with covariates included. Limiting democracy by switching to an appointed assessor increases the probability of conducting a town-wide update to assessments by about 9 percentage points. Given that the average probability of conducting an update in towns that elect, shown in the footer of the table, is about 12 percentage points, this represents a sizable increase over the baseline. The effect is also precisely estimated in both specifications, with standard errors less than 2 percentage points.

The next two columns show that this increase in town-wide updates has real effects on how homeowners are assessed. Columns 3 and 4 show the impact on aggregate assessment rates: the degree to which assessed values deviate from market values overall. Switching to an appointed assessor causes an increase of 15 points in this overall rate, with a standard error of less than 2 points. In an average town that elects, properties are valued at 58% of what they are worth; thus the switch to less democratic control causes assessments to get much closer to the benchmark of 100% valuation that town officials

---

and $1,000,000.

Table 2.1: Limiting democracy improves public welfare: difference-in-differences results.

	Updates		Assessment rate			
	(1)	(2)	(3)	(4)	(5)	(6)
Appoint	8.78*	9.29*	15.03*	15.00*	-7.51+	-8.52+
	(1.70)	(1.71)	(1.88)	(1.85)	(4.46)	(4.71)
Sale price					-58.01*	-58.77*
					(5.36)	(5.27)
Appoint*Price					24.54*	24.99*
					(6.55)	(6.55)
Average outcome	11.81	11.71	58.00	57.95	44.11	44.11
Covariates		Y		Y		Y
Year FE	Y	Y	Y	Y	Y	Y
Town FE	Y	Y	Y	Y	Y	Y
Time period	1987-2011	1987-2011	1987-2012	1987-2012	2003-2011	2003-2011
# Towns	920	910	920	910	912	904
# Switchers	392	391	392	391	54	54
# Observations	23,000	22,750	23,920	23,660	411,298	409,009

Notes: This table presents estimates of the effect of limiting democracy (changing to an appointed assessor) on three measures of social welfare, calculated using a difference-in-differences regression. Cell entries are point estimates with town-clustered standard errors in parentheses. ($+\ p < 0.10$, $*\ p < 0.05$)

and technical experts favor.

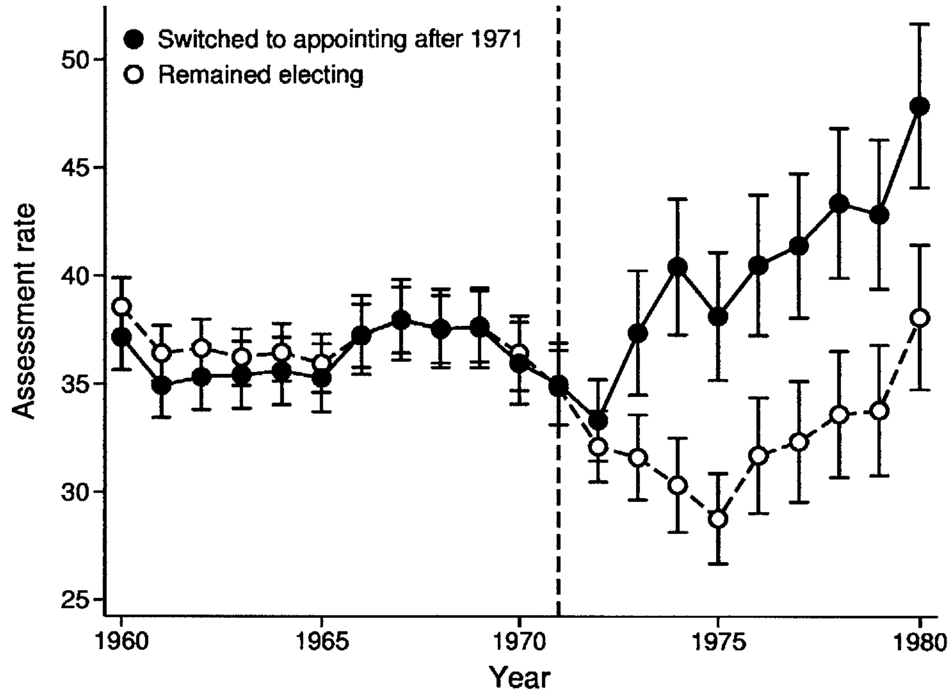
Finally, the last two columns of Table 2.1 show the impact on uniformity. Columns 5 and 6 show that the increase in assessments affects different homeowners differently. The coefficient on sale price – which is logged and rescaled to lie between zero and one – is -58 and -59, depending on the specification. This means that, in towns that elect their assessors, moving from the lowest- to the highest-priced home is associated with a decrease in the assessment rate of almost 60%. Thus, in these towns the wealthiest homes pay taxes on 60% less of their actual property value than do the poorest homes (the standard error is about 5 points). Yet when a town limits democracy by changing to an appointed assessor, the regressive incidence of the tax is significantly lessened: the slope of assessment rates to sale prices increases about 25 percentage points. While the legal benchmark of perfectly uniform assessments – which would correspond to a slope of zero – is still a ways off, it is much closer as a consequence of abandoning direct democratic control of assessors.

Robustness Tests

While using over-time variation in institutions represents a significant advance in estimating these effects, the design still relies on the assumption of no unmeasured time-varying confounders. One violation of this assumption would be if towns strategically timed their transitions to appointed assessors. Another would be if something else important, such as a broader package of fiscal reforms, also changed with the switch to appointed assessors. I address these possibilities in two ways. First, in the Appendix I show that the effect is robust to a variety of alternative specifications.[13] Second, I exploit a 1970 state law regarding assessor selection. In 1970, the state passed a package of assessment reforms which included a requirement that all towns change from electing to appointing their assessors. Only if a town pro-actively held and passed a referendum to prevent this change between 1970 and July, 1971 would they avoid being forced into the appointed regime (New York State Office of Real Property Services 2007). Thus while towns still had discretion over whether they would switch, the timing of the reform was plausibly

[13]These include dropping any towns where I am uncertain of the switch year, and including county-by-year fixed effects, which control for any confounder that flexibly varies within counties and across time. I also estimate a series of "placebo" regressions where I change the outcome to something that should not be affected by the switch: population, the number of home sales, the median home price, the percent of revenue from property taxes, total revenues, and total expenditures. None of these placebo outcomes are significantly of substantively impacted by the treatment.

Figure 2.4: Limiting democracy improves welfare: robustness check using 1970 state reform.



Notes: Prior to 1970, nearly all towns elected their assessors. In 1971, about half of towns were induced by a state law to change to an appointed assessor. This figure plots the average assessment rate (a measure of social welfare) for each of these two cohorts. Prior to 1971 (the year of the switches), the two cohorts are indistinguishable in terms of levels or trends. After 1971, there is a clear divergence, with towns that changed to appointing showing marked gains in welfare.

exogenous, which should rule out strategic timing as an explanation. And while the law also reformed other elements of the assessment system, such as creating county offices of assessment assistance and imposing new training requirements, these other changes should have affected both electing and appointing towns equally.

In Figure 2.4, I plot the average assessment rate for the 494 towns that passed referendums between 1970 and 1971 to maintain their elected system and the 426 that did not, over the period from 1960 to 1980. Hollow circles plot the average outcome for towns that elected throughout the period, and filled circles plot the averages for towns that were pushed into appointing in 1971; vertical bars represent 95% confidence intervals, and a dashed horizontal line at 1971 denotes the year of the referendums.[14] The figure shows a striking degree of parallelism between the two cohorts prior to 1971. Indeed, not only are the trends parallel prior to 1971, but there is no distinguishable baseline difference. After 1971, however, there is a clear divergence, with the difference gradually rising to about 10 points by 1974 and persisting at that level for the rest of the series. A difference-in-differences regression, reported in the Appendix, yields a point estimate of about 9 points, with a standard error of 1.6. While this effect at first seems smaller than that reported in Table 2.1, the baseline accuracy in the elected towns in this period is much lower, at about 35, which means the substantive magnitude of the effect is similar to that observed in the more recent period. The replication of this result in a different time period, with a more plausibly exogenous shift in treatment assignment, reinforces the interpretation of a large, positive causal effect on welfare as a result of limiting democracy.[15]

---

[14]Unfortunately, the aggregate assessment rate is the only measure of welfare available in this earlier time period. I obtained the referendum data by tabulating paper records of local laws at the New York State Library in Albany, NY. While some of the 426 non-referendum towns may have already appointed their assessors prior to 1971, this should only bias the observed effect of the reform downward. Any towns that refused to comply with the law should cause a similar downward bias. According to Conneman (1979), 95% of towns elected assessors prior to 1970.

[15]An additional source of confounding is possible difference in technical training between elected and appointed assessors. While I am unable to measure training and include it as a covariate, three points cast doubt on this explanation. First, both elected and appointed assessors in New York have roughly the same education and training requirements. As I describe in the Appendix, there is a baseline level of training required

Alternative Models of Competence

Thus far, I have been discussing competence in the Madisonian terms of "discerning the public interest." That is, when there exists a clearly "correct" policy that would maximize social welfare, can voters be trusted to arrive at the right result? In this case, the voters appear to fail. However, it is worth considering an alternative model of competence: expressing consistent preferences and holding officials accountable (Druckman 2001; Healy and Malhotra 2009). Superficially, at least, it would seem that voters are behaving competently under this alternative definition: voters oppose accurate assessments and regular updates, and they get less of them when the assessor is elected.

Yet even this more subjective definition of competence places a priority on internally consistent preferences (Druckman 2001, 232). To evaluate the results in terms of this definition of competence, then, it is necessary to ask why voters oppose accurate assessments. According to Martin's (2008) account of the tax revolts, opposition to fair assessments during the tax revolts was due primarily to voters' association between higher assessments and higher tax bills. In other words, voters see their homes being valued at higher levels, and immediately assume that this will result in higher tax bills. Historical

---

of all newly elected or appointed assessors, as well as an ethics course required of all newly re-appointed or re-elected assessors (New York State Office of Real Property Services 2007). The key difference is that appointed assessors must meet additional experience and continuing education requirements. Second, I present survey evidence in the Appendix showing both elected and appointed assessors meet the training baseline requirements, and that even many elected assessors participate in continuing education. Finally, I found similar results using the 1971 referendums. Prior to these referendums, there were few training requirements, and afterward, the newly created requirements were applied equally to both elected and appointed assessors.

and press accounts from revaluations in New York state support this view. According to the assessors surveyed in the 1920s by Pond (1931), the number one reason for lack of revaluations was voters' "Belief that high assessment means high taxes." A 1985 New York Times article on the towns of New Castle and Mount Kisco characterizes reassessing as "politically difficult because homeowners were fearful that larger assessments might mean a larger tax" (Brown 1985).

Thus voters' actual objective seems to be lower tax bills. We can then ask, are voters' association between assessments and taxes based on facts? And are voters actually using elections to achieve their goal of lower bills? In Table 2.2, I present evidence that the answers to these questions are both negative. In these tables, I estimate difference-in-differences regressions similar to those presented earlier. Now, however, the outcome in columns 1 and 2 is the (logged) aggregate tax rate; the outcome in columns 3 through 6 is the individual homeowner's (logged) tax bill (calculated by multiplying the town's tax rate by the homeowner's assessed value).

Columns 1 and 2 show that limiting democracy actually results in a *decrease* in the overall tax rate of between 0.13 and 0.14 log points, with a standard error of 0.08. Thus, tax rates are actually higher when assessors are elected (which is to be expected, given the lower tax base that accompanies inaccurate assessments). Columns 3 and 4 examine the effect on tax bills. The effect on (log) tax bills is only 0.02, with a standard error of 0.05, in each specification. Unlogging, this estimate is about 9 dollars, but the confidence interval implies it could be as high as a 59 dollar increase, or it could be as low as a 52 dollar decrease.[16] Voters' association between assessments and tax bills appears unwarranted,

[16]The unlogged effect estimate is calculated by,

$$\frac{\exp\{E[bill_{ijt}|\widehat{appoint}_{jt}=0]+\hat{\beta}\}-\exp\{E[bill_{ijt}|\widehat{appoint}_{jt}=0]\}}{\exp\{E[bill_{ijt}|\widehat{appoint}_{jt}=1]\}-\exp\{E[bill_{ijt}|\widehat{appoint}_{jt}=0]\}}$$

Table 2.2: Are voters getting what they want?

	Tax rate		Tax bill			
	(1)	(2)	(3)	(4)	(5)	(6)
Appoint	-0.13	-0.14^{+}	0.02	0.02	-0.26*	-0.24*
	(0.08)	(0.08)	(0.05)	(0.05)	(0.08)	(0.08)
Sale price					3.03*	3.05*
					(0.08)	(0.08)
Appoint*Price					0.42*	0.41*
					(0.12)	(0.13)
Mean outcome	2.05	2.05	6.15	6.15	6.15	6.15
Covariates		Y		Y		Y
Year FE	Y	Y	Y	Y	Y	Y
Town FE	Y	Y	Y	Y	Y	Y
Time period	2000-2009	2000-2009	2003-2009	2003-2009	2003-2009	2003-2009
# Towns	903	895	894	886	894	886
# Switchers	79	78	39	39	39	39
# Observations	8,900	8,823	339,165	337,212	339,165	337,212

Notes: This table presents estimates of the effect of limiting democracy (changing to an appointed assessor) on tax rates and tax bills, calculated using a difference-in-differences regression. Cell entries are point estimates with town-clustered standard errors in parentheses. (+ $p < 0.10$, $*$ $p < 0.05$)

and elections do not actually help voters achieve lower bills.

Finally, in columns 5 and 6 I interact the appointed variable with sale price to examine

---

where the second term comes from the average outcome in towns that elect (6.15) and the first term is the sum of this average and the point estimate of 0.02. Then the 95% confidence interval estimate is,

$$\Big[\exp\left(E[bill_{ijt}|\widehat{appoint}_{jt}=0]+(\hat{\beta}-1.96*\hat{SE}(\hat{\beta}))\right)-\exp\left(E[bill_{ijt}|\widehat{appoint}_{jt}=0]\right),$$
$$\exp\left(E[bill_{ijt}|\widehat{appoint}_{jt}=0]+(\hat{\beta}+1.96*\hat{SE}(\hat{\beta}))\right)-\exp\left(E[bill_{ijt}|\widehat{appoint}_{jt}=0]\right)\Big].$$

the distributional impact. The coefficient on sale price indicates that, in towns that elect their assessor, moving from the lowest- to the highest-valued property is associated with an increase of 3 log points in the tax bill. As would be expected, higher-priced homes pay larger tax bills in absolute terms. When a town limits democracy, however, the coefficient on appoint indicates that the poorest homeowners actually save money: the estimate is between -0.24 and -0.26, with a standard error of 0.08. Unlogged, this translates into about a 100 dollar decrease. The only increases are concentrated at the very top: the interaction term indicates that for the highest priced homes, tax bills increase by between 0.41 and 0.42 log points, with a standard error of 0.13%. Unlogged, this represents an increase of about 237 dollars.[17]

The stark distributional impact of assessments raises an additional possibility: namely, that the subset of wealthy homeowners are behaving "competently" in the sense that they are supporting policies that benefit themselves, and are using elections to achieve these policies. This interpretation would seem unlikely, given that conventional models of political agency predict *more* elite capture when officials are appointed (Besley and Coate 2003). However, even if elite capture were driving these results, it would imply that a large swath of voters are either going along with elites and supporting policies that harm them; or, that most voters are failing to express their preferences for fair assessments via elections. In either case, aggregate voter preferences may still fairly be described as inconsistent, and voters as a whole could not be described as holding elected assessors

[17] These numbers assume the same baseline, namely the average tax bill, for all homeowners. Alternatively, we could calculate the tax savings for the poorest homes using the tax bills of the poorest homes as the baseline; and the tax increase for the wealthiest homes using the bills of the wealthiest homeowners as the baseline. This yields a savings of about 20 dollars for the poorest homeowners (or a 70% decrease) and about 1,290 dollars for the wealthiest (a 49% increase).

accountable. Thus, in this case voters are not behaving competently, whether we define competence as discerning the public welfare, or as simply expressing consistent preferences through elections,

Conclusion

Critics of democracy have long pointed to the fallibility of public opinion to justify limits on popular control. Despite an extensive literature on the flaws of public opinion, however, there has been little study of the claim that limiting popular control can improve welfare. In this paper, I have provided a novel test of this claim. Using original data on New York towns and exploiting plausibly exogenous shifts in institutions to estimate causal effects, I have shown that limits on democracy can indeed have large, positive impacts on social welfare.

The debate over the proper balance between elite and popular control in democracies has been ongoing for centuries, and this study is just one piece of evidence in the longer exchange. Obviously, the conclusion should not be to discard democracy altogether; even Madison and Lippmann did not go this far. Voters, they seem to have recognized, are better than elites at certain tasks, but much worse than them at others. My results suggest that voters are worse at choosing policies that maximize social welfare; indeed, the results on tax rates and bills suggest they are bad at choosing policies that maximize even their own narrow self-interest. What, then, is the role of voters in a democracy, and when should they be given a veto over elites?

My results suggest that decisions on policy means, as opposed to judgments on policy outcomes, are safer when left to the elites. This conclusion is slightly ironic, given that the early competence literature bemoaned voters' apathy toward the technical aspects of policy. Retrospective voting – the idea that voters judge politicians based on results, while remaining rationally ignorant of the means used to achieve these results – was offered in no small part as a second-best alternative to the ideal of voting on policy means.

Yet my findings, coming from a situation where voters do know and care about the issues, suggest that judging politicians on policy means can be disastrous for social welfare. The reason is that knowledge and attention do not necessarily imply a full understanding of the issues, or an appreciation of the connection between policy means and policy ends. Seen in this light, the model of voters simply registering their general satisfaction at the polls, ignoring policy means altogether, appears somewhat less dismal. For despite the occasional mis-weighting of evidence by voters (Achen and Bartels 2004), or their consideration of irrelevant information (Healy, Malhotra and Mo 2009), this model still provides a modicum of accountability that – so far – has not caused the republic to collapse. Should the "ideal" of voting on policy means ever to become reality, my results suggest, the continuance of this stability would be much less certain.

References

Acemoglu, Daron. 2005. "Constitutions, politics, and economics: A review essay on Persson and Tabellini's *The Economic Effects of Constitutions*." *Journal of Economic Literature* 43(4): 1025-1048.

Achen, Christopher H., and Larry M. Bartels. 2004. "Musical chairs: Pocketbook voting and the limits of democratic accountability." Paper presented at the Annual Meeting of the American Political Science Association, Chicago.

Achen, Christopher H., and Larry M. Bartels. 2013. "Blind Retrospection: Why Shark Attacks Are Bad For Democracy." Vanderbilt University Center for the Study of Democratic Institutions Working Paper 5-2013.

Angrist, Joshua D., and Jörn-Steffen Pischke. 2008. *Mostly harmless econometrics: An empiricist's companion.* Princeton University Press.

Ashworth, Scott. 2012. "Electoral Accountability: Recent Theoretical and Empirical Work." *Annual Review of Political Science* 15: 183–201.

Ashworth, Scott, and Ethan Bueno de Mesquita. 2013. "Disasters and Incumbent Electoral Fortunes: No Implications for Democratic Competence." Working paper, Harris School of Public Policy Studies, University of Chicago.

Berelson, Bernard R., Paul F. Lazarsfeld, and William N. McPhee. 1954. *Voting: A Study of Opinion Formation in a Presidential Campaign.* Chicago: University of Chicago Press.

Besley, Timothy, and Stephen Coate. 2003. "Elected versus appointed regulators: Theory and evidence." *Journal of the European Economic Association* 1(5): 1176-1206.

Bowler Sean, and Todd Donovan. 1998. *Demanding Choices: Opinion, Voting, and Direct Democracy.* Ann Arbor: University of Michigan Press.

Brown, Betsy. 1985. "New Castle Fairest in State for Taxes." *The New York Times*

(February 3): 11WC.

Conneman, George J. 1979. "Real estate taxation, assessment and revaluation: Implications for agriculture." Technical report, Department of Agricultural Economics, Cornell University.

Converse, Philip E. 1964. "The nature of belief systems in mass publics." In *Ideology and Discontent*, ed. David Apter. Glencoe, IL: Free Press, pp. 206-261.

Delli Carpini, Michael X., and Scott Keeter. 1996. *What Americans Know About Politics and Why It Matters*. New Haven: Yale University Press.

Druckman, James N. 2001. "The implications of framing effects for citizen competence." *Political Behavior* 23(3): 225-256.

Fiorina, Morris P. 1981. *Retrospective Voting in American National Elections*. New Haven: Yale University Press.

Gallup. 2012. "Taxes | Gallup Historical Trends." Accessed via http://www.gallup.com /poll/1714/taxes.aspx on August 23, 2012.

Gordon, Sanford C., and Gregory A. Huber. 2007. "The effect of electoral competitiveness on incumbent behavior." *Quarterly Journal of Political Science* 2(2): 107–38.

Healy, Andrew, and Gabriel S. Lenz. 2013. "Substituting the End for the Whole: Why Voters Respond Primarily to the Election-Year Economy." *American Journal of Political Science*, forthcoming.

Healy, Andrew, and Neil Malhotra. 2009. "Myopic Voters and Natural Disaster Policy." *American Political Science Review* 103(3): 387-406.

Healy, Andrew J., Neil Malhotra and Cecilia Hyunjung Mo. 2010. "Irrelevant events affect voters' evaluations of government performance." *Proceedings of the National Academy of Sciences* 107(29):12804–12809.

International Association of Assessing Officers. 2013. "Standard on Ratio Studies." Accessed via www.iaao.org/uploads/standard_on_ratio_studies.pdf on August 25, 2013.

Kahneman, Daniel. 2003. "Maps of bounded rationality: Psychology for behavioral economics." *American Economic Review* 93(5): 1449-1475.

Kuran, Timur, and Cass R. Sunstein. 1998. "Availability Cascades and Risk Regulation." *Stanford Law Review* 51: 683-768.

Lippmann, Walter. 1922. *Public Opinion.* NY: Harcourt, Brace and Company, Inc.

Lupia, Arthur, and John G. Matsusaka. 2004. "Direct Democracy: New Approaches to Old Questions." *Annual Review of Political Science* 7: 463–82.

Lupia, Arthur. 1994. "Shortcuts versus encyclopedias: information and voting behavior in California insurance reform elections." *American Political Science Review* 88(1): 63–76

Malme, Jane H. 1991. "Policies and practices that promote assessment equity: case studies of alternative models." Technical report, Lincoln Institute of Land Policy. Accessed via http://dev.lincolninst.edu/subcenters/property-valuation-and-taxation-library/dl/malme_1.pdf on August 25, 2013.

Martin, Isaac W. 2008. *The permanent tax revolt: how the property tax transformed American politics*. Stanford University Press.

McMillen, Daniel P., and Rachel N. Weber. 2008. "Thin Markets and Property Tax Inequities: A Multinomial Logit Approach." *National Tax Journal* 61(4): 653-671.

Miller, Michael K. 2013. "For the Win! The Effect of Professional Sports Records on Mayoral Elections." *Social Science Quarterly* 94(1): 59-78.

Murphy, Barbara. 1984. *1982 Survey of New York State Assessors, Volume 3: Revaluation.* Albany, NY: State Board of Equalization and Assessment.

New York State Constitutional Convention Committee. 1938. *Problems Relating to Taxation and Finance.* Albany, NY: New York State Government.

New York State Department of State. 2011. Local Government Handbook. Albany: New York Department of State. Accessed via www.dos.ny.gov/lg/publications/Local_Gov ernment_Handbook.pdf on August 25, 2013.

New York State Department of Taxation and Finance. 2012a. "Municipal Options for More Efficient Assessment Administration." Department of Taxation Web Site. Accessed via http://www.tax.ny.gov/pubs_and_bulls/orpts/munioptions.htm, July 11, 2013.

New York State Department of Taxation and Finance. 2012b. "What is a reassessment, and why are they needed?" Department of Taxation Web Site. Accessed via http://www.tax.ny.gov/research/property/assess/reassessment/reassess_what.htm, July 11, 2013.

New York State Office of Real Property Services. 2007. "A History of the Real Property Tax and Equalization in the State of New York." Technical report. Accessed via http://www.victorny.org/DocumentCenter/View/148 on August 26, 2013.

Pond, Chester Baldwin. 1931. *Full value real estate assessment as a prerequisite to state aid in New York.* Albany, NY: New York State Legislature.

Page, Benjamin I., and Robert Y. Shapiro. 1992. *The Rational Public: Fifty Years of Trends in Americans' Policy Preferences*. Chicago: University of Chicago Press.

Appendix

Data Collection

The state Office of Real Property Services provided me with a list of all towns and their selection methods between 2006 and 2010. For prior to 2006, I used several sources. In 1987 and 1992, the U.S. Census of Governments (COG) survey included questions about the structure of local government, including the number of elected and appointed officials occupying particular positions. Thus, the COG provides two complete cross-sections for 1987 and for 1992.

The third source was county election records. County governments in New York oversee the administration of local elections. Examining election results gives an indicator for what offices are elected in what years: if the assessor is listed on the ballot for all years prior to 2001 but then disappears, I infer that the town has switched. A fourth source came from a web search for local laws, which towns legally must file in order to switch, and which are sometimes posted to New York town web sites, and sometimes to the eCode360 web site at http://www.generalcode.com/. Fifth, the New York Department of State provided me with records for some of of these laws for the years 2000-2006.

Even after combining these data sources, I could not identify the assessor selection method for all towns in all years. I therefore used two important facts to impute the treatment indicator for missing years. First, once a town switches to the appointed regime, it is forbidden by law from switching back (New York State Department of Taxation and Finance 2013a). Second, if a town is electing in year t, we can assume it has elected for all years prior. This yielded a precise switch year for 162 of the 392 switching towns, and bounds for 230.

For determining the towns affected by the 1970 state reform, I searched through paper records of historical local laws at the State Library in Albany, NY. I browsed laws filed between 1970, when the state law mandating the referendums was passed, and 1971, the deadline for passing the referendums. I coded towns as passing a referendum if a relevant local law was present in the law books, and as not passing a referendum otherwise.

Table 2.A1 summarizes the data collection for the analysis in the main text. Table 2.A2 gives the distribution of bounds on the treatment indicator.

Table 2.A1: Summary of data sources.

Category	Variable	Sources
Treatment	Appointed assessor	New York State Office of Real Property Services
		U.S. Census of Governments
		County election results
		Town web sites
		eCode360
		New York State Department of State
		New York State Archives
Outcomes	Town-wide update	New York State Office of Real Property Services
	Assessment rate (aggregate)	New York State Office of Real Property Services
	Assessment rate (individual)	New York State Office of Real Property Services
	Tax rate	New York State Comptroller's Office
Covariates	Population	U.S. Census
	Population density	U.S. Census
	Percent White	U.S. Census
	Median income	U.S. Census
	Percent under age 18	U.S. Census
	Percent age 65 and older	U.S. Census
	Percent farmer	U.S. Census
	Percent unemployed	U.S. Census
	Percent with a high school degree	U.S. Census
Placebo outcomes	Population	U.S. Census
	Number of sales	New York State Office of Real Property Services
	Median sale price	New York State Office of Real Property Services
	Total tax revenue	New York State Comptroller's Office
	Percent revenue from property taxes	New York State Comptroller's Office
	Total expenditure	New York State Comptroller's Office

Table 2.A2: Distribution of bounds on year of switch to appointed assessors.

	Number of towns
0 years	162
5 years	108
7 years	24
9 years	15
11 years	26
13 years	19
14 years	38
Total	392

Accounts of Transitions to Appointed Assessors

The state agency in charge of overseeing property taxation has long been encouraging towns to change to appointed assessor. In an information packet directed at towns considering the option to switch, the agency gives nine "Advantages of Sole Assessor." Of those purported advantages, the following are relevant to the distinction between election and appointment: the appointed assessor is directly responsible to the town board; the appointed assessor must meet minimum qualification standards, as opposed to the age and residency requirements of the elected assessors; the appointed assessor is more likely to be a "professional" assessor, as opposed to the elected assessors, who may use the office as a "stepping-stone" to higher office, "meaning voters must be kept friendly"; the appointed assessor is more "insulated from political pressures," because they do not seek re-election every four years; and lower turnover. ORPS also cites "overall savings" due to lower costs of paying salary, training, and administration costs for one assessor as opposed to three. Finally, ORPS cites the fact that appointment is what the majority of towns in the state do, and that "every year, the number of towns opting for 'sole' increases." "Better Assessing Practices and More Equity" is mentioned at the bottom of the list (New York State Department of Taxation and Finances 2013a).

News accounts of decisions to switch cite many of the same reasons. In 1978, the town of Lyons in Wayne county failed to pass a switch to an appointed regime; those on the pro-appointed side cited greater efficiency in having one assessor; those on the anti-appointed side cited greater responsiveness to the public under the elected system (Crosby 1978). In 2009, in the town of Somerset in Niagara County, the motion to switch was debated at a public hearing. Anti-appointment forces claimed the appointed assessor would be "controlled politically," and that a switch to appointments would violate voters' right to choose their representatives. Pro-appointed forces made arguments similar to the state literature (Town of Somerset 2009).

In 2010, in the town of Crown Point in Essex County, those in favor cited cost savings, the fact that most other towns in the county had switched, and that the state government favored it; those against cited the benefit of being able to vote out poor quality officials (McKinstry 2010). In 2011, in the town of Taghkanic in Columbia County, pro-forces cited the fact that most other towns in the county had already switched; general cost savings; greater professionalism; greater fairness; and cost savings specifically due to reduced litigation due to contested assessments (Taghkanic Neighbors 2011). Also in 2011, in the town of Lowville in Lewis County, pro-forces cited a lack of interested candidates, despite strong opposition to a switch when it was last proposed in 1993. They pointed to the current assessors' impending retirements as the main impetus this time around (Virkler 2011). Also in 2011, in the town of Lyme in Jefferson County, pro-forces cited cost savings and an impending vacancy; speaking in favor of the elected system, a former elected assessor cited the advantage of being able to remove bad assessors through elections (Madsen 2011).

A public hearing on the issue was held in the town of Western in Oneida County in 2011. Those in favor of elections cited the ability of voters to control the assessor. Those in favor of appointments cited cost savings in terms of smaller salaries, and that it is difficult finding people to run (Town of Western 2011). In 2012, in the town of Minden in Montgomery County, pro-forces cited a lack of qualified candidates and costly state training requirements for elected assessors (Kellett 2012). Also in 2012, in the town of Moriah in Essex County, the pro-forces pointed to the state's literature on the advantages of switching, and also stated they believed the state would eventually force them to switch. The current assessor disagreed, claiming that elected assessors are more available to help the public (Herbst 2012).

Accounts of Town-Wide Updates to Assessments

Newspaper accounts indicate widespread voter opposition to updates to assessments. A news article concerning Westchester County in 2012 describes revaluations as "a political hot potato: Few leaders want to take on this issue since there is a belief that some taxpayers will pay more, and that's not good for votes" (McKinstry 2012). A New York Times article from 1992, concerning the town of East Hampton, concurs with this view. This article mainly describes a "cottage industry" of property tax appeals specialists, which for a fee will help voters appeal their assessments and receive a refund. According to one of these specialists, "The history books tell us that when reassessment occurs, elected officials lose their jobs" (Barbanel 1992). A *New York Times* journalist, surveying wide disparities in valuations between towns in Westchester County, similarly describes revaluation as "a Pandora's box that few politicians really want to touch. So for now Scarsdale will continue to assess its properties at roughly 4 percent of their value while Mount Kisco will assess properties at 40 percent of their value, and each will retain wildly divergent tax rates" (Berger 1994a). Again reporting on the problem of disparities, this same journalist wrote later that year that "The problem could largely be corrected by periodic reassessments of all properties. But county officials say local politicians have been loath to do that, because they fear that homeowners who end up paying higher taxes would never forgive them at the polls" (Berger 1994b).

The fear of punishment at the polls is mentioned repeatedly in these reports. According to another journalist in 1995: "Mention "reassessment" and elected officials run for cover. It doesn't take a political insider to know that people whose taxes are raised tend to express their displeasure in the voting booth." The journalist went on to describe revaluation as "political suicide" in the eyes of elected officials, which led many towns to avoid the issue (Lombardi 1995). And again in 1995, the same paper quoted a lawyer who claimed that "The reason politicians say 'no' to reassessment is because they are afraid they'll be clobbered at the polls" (Shaman 1995). Another lawyer, who is described as working with property taxes, put it this way in 2008: "There is such a general knee-jerk reaction against revaluation that any official who takes it on has to ask themselves, 'How long do I want to remain in office?'" (Gruen 2008). And in the words of a town supervisor quoted by the *Times* in 2003, revaluation "is an easily distorted issue that incites people's fears, and nobody wants to take the political heat" (Rubenstein 2003).

Other accounts describe voter ire in more detail. For example, in the town of Rye in 2003, the issue was debated publicly. The *Times* described how the discussion "exploded into a loud and public argument, including lawsuits, accusations of fiscal wrongdoing by officials, and even a death threat and an order of protection" (West 2003). The *Buffalo News* in 2005 reports on how voters respond to revaluations by challenging the assessor's decisions. The assessor of the town of Lancaster reported receiving 1,200 of these challenges from "home and business owners disputing their property assessments." Reportedly, the Town Board had been handed a petition, signed by 400 residents, calling for a complete nullification of the revaluation (McNeil 2005). And in the town of Newstead in 2007, "fears of a huge property tax hit" due to a revaluation led to residents flooding the assessor's office with visits, e-mails, letters, and phone calls. Similar to Lancaster, residents were said to be "organizing a 'tax revolt' at the Town Board meeting" that evening (Tan 2007).

Details on Survey of Town Officials

The New York Department of State provided me with contact information for town officials. In many cases, the particular officials lacked individual e-mail addresses. Thus I sent the survey to all 837 town clerks with an e-mail address and requested they forward it on to other officials. The full launch was conducted in June 2013, and was preceded by a pilot survey sent to 100 randomly selected clerks in May, 2013. Recruitment consisted of an initial invitation followed by reminders sent after one and two weeks. A total of 236 officials from 155 unique towns responded and completed the survey (the numbers in the main text differ slightly due to item non-response). Table 2.A3 shows how many officials responded by office and by whether the town currently elects or appoints its assessor; Figure 2.A1 maps towns with at least one responding official; and Figure 2.A2 compares responding towns with non-responding towns on measurable characteristics. Question wordings for data shown in Figure 2.3 in the main text are below:

- How much do you agree with the following statement: All properties in my town should be assessed at 100% of their market value. [Strongly Agree, Agree, Somewhat Agree, Neither Agree nor Disagree, Somewhat Disagree, Disagree, Strongly Disagree]
- Generally speaking, how would you describe voters' reactions to the idea of assessing at 100% of market value in your town? [Extremely negative – they do not like 100% assessment at all, Very negative, Somewhat negative, Neutral, Somewhat positive, Very positive, Extremely positive – they like 100% assessment very much]
- How much do you agree with the following statement: Town-wide revaluations should be done on a regular basis in my town, such as every year or every three years. [Strongly Agree, Agree, Somewhat Agree, Neither Agree nor Disagree, Somewhat Disagree, Disagree, Strongly Disagree]

- Generally speaking, how would you describe voters' reactions to the idea of regular town-wide revaluations in your town? [Extremely negative – they do not like revaluations at all, Very negative, Somewhat negative, Neutral, Somewhat positive, Very positive, Extremely positive – they like revaluations very much]

Table 2.A3: Number of officials completing survey by office.

	Electing towns	Appointing towns	Total
Assessor	7	55	62
Town board	13	112	125
Town clerk	5	27	32
Other	1	16	17
Total	26	210	236

Figure 2.A1: Map of sample towns responding to the survey.

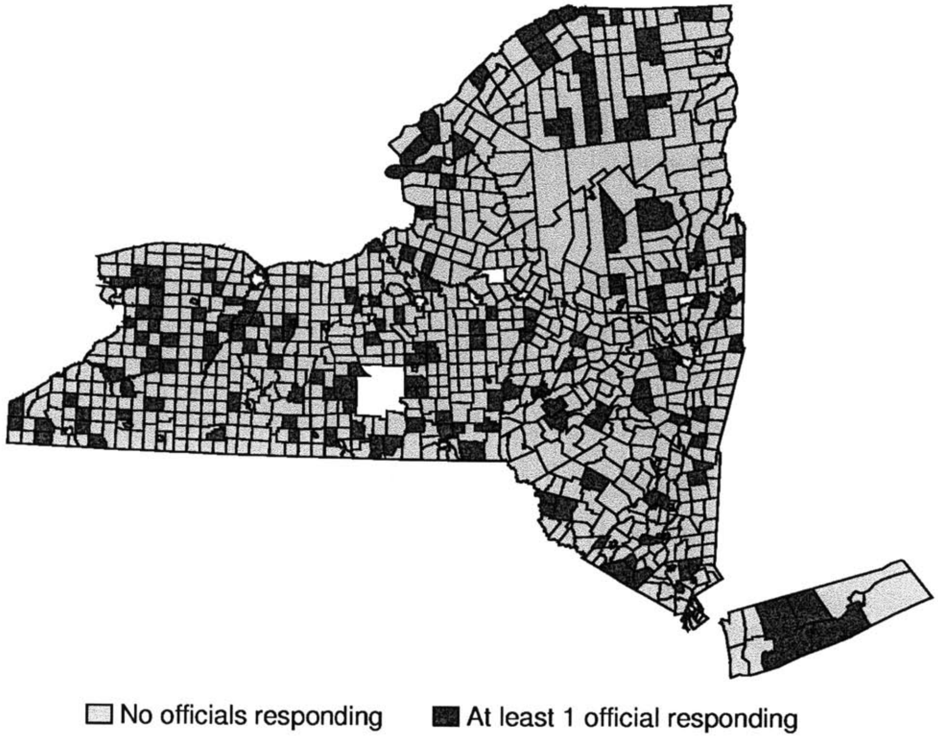
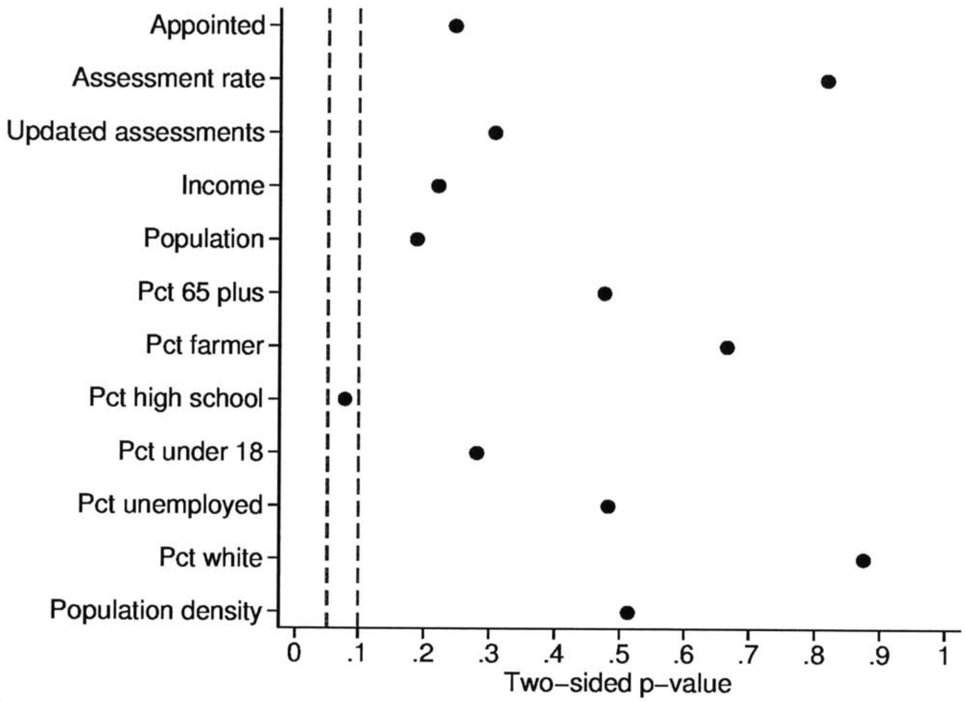


Figure 2.A2: Comparing responding and non-responding towns.



Notes: This figure plots two-sided p-values from t-tests of differences in means, between towns that did and did not respond to the survey. All variables are from 2010.

Robustness Checks for Difference-in-Differences Results

Table 2.A4: Replication of Table 2.2.1, dropping towns with uncertain switch years.

	Updates		Assessment rate			
	(1)	(2)	(3)	(4)	(5)	(6)
Appoint	11.52*	12.60*	13.87*	14.26*	-8.53+	-9.80+
	(2.71)	(2.73)	(2.70)	(2.71)	(4.58)	(5.08)
Sale price					-57.80*	-58.83*
					(5.36)	(5.40)
Appoint*Price					26.60*	27.37*
					(6.72)	(6.87)
Average outcome	13.18	13.04	59.69	59.64	44.11	44.11
Covariates		Y		Y		Y
Year FE	Y	Y	Y	Y	Y	Y
Town FE	Y	Y	Y	Y	Y	Y
Time period	1987-2011	1987-2011	1987-2012	1987-2012	2003-2011	2003-2011
# Towns	690	680	690	680	683	675
# Switchers	162	161	162	161	54	54
# Observations	17,250	17,000	17,940	17,680	376,645	374,356

Table 2.A5: Replication of Table 2.2.1, including county-by-year fixed effects.

	Updates		Assessment rate			
	(1)	(2)	(3)	(4)	(5)	(6)
Appoint	6.85*	6.85*	10.78*	10.78*	-9.24*	-9.50*
	(1.37)	(1.37)	(1.54)	(1.54)	(3.97)	(4.10)
Sale price					-55.52*	-55.85*
					(4.73)	(4.62)
Appoint*Price					20.87*	21.38*
					(5.78)	(5.75)
Average outcome	11.80	11.80	57.99	57.99	44.11	44.11
Covariates		Y		Y		Y
Year FE	Y	Y	Y	Y	Y	Y
Town FE	Y	Y	Y	Y	Y	Y
Time period	1987-2011	1987-2011	1987-2012	1987-2012	2003-2011	2003-2011
# Towns	920	920	920	920	912	904
# Switchers	392	392	392	392	54	54
# Observations	22,988	22,988	23,908	23,908	411,298	409,009

Table 2.A6: Replication of Table 2.2.2, dropping towns with uncertain switch years.

	Tax rate		Tax bill			
	(1)	(2)	(3)	(4)	(5)	(6)
Appoint	-0.14+	-0.15+	0.03	0.02	-0.26*	-0.24*
	(0.08)	(0.08)	(0.05)	(0.05)	(0.08)	(0.09)
Sale price					3.03*	3.04*
					(0.08)	(0.08)
Appoint*Price					0.41*	0.41*
					(0.13)	(0.14)
Average outcome	2.05	2.05	6.15	6.15	6.15	6.15
Covariates		Y		Y		Y
Year FE	Y	Y	Y	Y	Y	Y
Town FE	Y	Y	Y	Y	Y	Y
Time period	2000-2009	2000-2009	2003-2009	2003-2009	2003-2009	2003-2009
# Towns	674	666	667	659	667	659
# Switchers	79	78	39	39	39	39
# Observations	6,631	6,554	311,391	309,438	311,391	309,438

Table 2.A7: Replication of Table 2.2.2, including county-by-year fixed effects.

	Tax rate		Tax bill			
	(1)	(2)	(3)	(4)	(5)	(6)
Appoint	-0.11	-0.11	-0.00	0.00	-0.26*	-0.27*
	(0.08)	(0.08)	(0.05)	(0.05)	(0.08)	(0.08)
Sale price					3.09*	3.08*
					(0.08)	(0.08)
Appoint*Price					0.43*	0.45*
					(0.12)	(0.12)
Average outcome	2.05	2.05	6.15	6.15	6.15	6.15
Covariates		Y		Y		Y
Year FE	Y	Y	Y	Y	Y	Y
Town FE	Y	Y	Y	Y	Y	Y
Time period	2000-2009	2000-2009	2003-2009	2003-2009	2003-2009	2003-2009
# Towns	903	903	894	886	894	886
# Switchers	79	79	39	39	39	39
# Observations	8,890	8,890	339,165	337,212	339,165	337,212

Table 2.A8: Placebo regressions.

	Population	# Sales	Med Sale Price	% Rev from Prop Tax	Revenues	Expenditures
Appoint	-0.00	0.00	0.02	0.01	-0.01	0.00
	(0.00)	(0.07)	(0.02)	(0.01)	(0.01)	(0.02)
Average outcome	8.09	3.04	11.67	0.48	14.32	14.34
Year FE	Y	Y	Y	Y	Y	Y
Town FE	Y	Y	Y	Y	Y	Y
Time period	1987-2012	2003-2011	2003-2011	1996-2010	1996-2010	1996-2010
# Towns	910	920	913	878	878	878
# Switchers	391	56	55	202	202	202
# Observations	23,660	8,280	8,097	13,108	13,108	13,108

Difference-in-Differences Estimate for Figure 2.2.4

Table 2.A9: Replication of Figure 2.2.4 using a difference-in-differences regression.

	Assessment rate	
	(1)	(2)
Appoint	8.66*	7.39*
	(1.59)	(1.57)
Average outcome	35.40	35.50
Covariates		Y
Year FE	Y	Y
Town FE	Y	Y
Time period	1960-1980	1960-1980
# Towns	920	910
# Switchers	426	417
# Observations	19,320	19,110

Training Requirements for Elected and Appointed Assessors

Figure 2.A3 reproduces a comparison of the requirements of serving as an elected or appointed assessor comes from the state Department of Taxation and Finance (2013b). This chart shows that all assessors, regardless of selection method, must meet certain basic training requirements, including re-certification upon re-appointment or re-election to office. The chief difference is that appointed assessors must meet mandatory continuing education requirements, while these courses are optional for elected assessors.

Figure 2.A3: Comparison of elected and appointed assessors.

Comparison of Elected and Appointed Assessor Positions			
	Single Appointed	**Sole Elected**	**Elected to 3 Member Board**
Length of term	6 years	6 years	4 years
Current term dates	October 1, 2007 – September 30, 2013	Jan. 1, 2012 – December 31, 2017	1/01/12-12/31/15 (2 members) 1/01/10 - 12/31/13 (1 member)
Qualifications	Must meet experience and education standards pursuant to 8188-2.2 of 20 NYCRR 8188.	Must meet residency and age requirements.	Must meet residency and age requirements.
Basic Required Training	Must earn State Certified Assessor designation. Recertification required upon re-appointment to office.	Must earn State Certified Assessor designation. Recertification required upon re-election to office.	Must earn State Certified Assessor designation. Recertification required upon re-election to office.
Required Continuing Education Training	Continuing Education requirement of 12 credits per year.	Continuing Education requirement of 12 credits per year.	Continuing Education training is optional.
Method of Acquiring Office	Appointed by Municipal Board. Professional, career-oriented position.	Must run for elective office in locality. Subject to voter approval.	Must run for elective office in locality. Subject to voter approval.

The following description of the qualifications for appointed assessors also comes from the Department of Taxation and Finance (2013b). This description shows that the minimum qualifications are not necessarily onerous: for example, the appointee may be deemed qualified if she has a four-year college degree and six months of experience volunteering in an assessor's office.

> § 8188-2.2 Minimum qualification standards for appointed assessors.
>
> (a) The minimum qualification standards for appointed assessors are as follows:
>
> (1) (i) graduation from high school, or possession of an accredited high school equivalency diploma; and
>
> (ii) two years of satisfactory full-time paid experience in an occupation involving the valuation of real property, such as assessor, appraiser, valuation data manager, real property appraisal aide or the like. Such experience shall be deemed satisfactory if it is demonstrated that the experience primarily was gained in the performance of one or more of the following tasks: collection and recording of property inventory data, preparation of comparable sales analysis reports, preparation of signed valuation or appraisal estimates or reports using cost, income or market data approaches to value. Mere listing of real property for potential sale, or preparation of asking prices for real estate for potential sale, using multiple listing reports or other published asking prices is not qualifying experience; or,
>
> (2) graduation from an accredited two-year college and one year of the experience described in subparagraph(1)(ii) of this subdivision; or
>
> (3) graduation from an accredited four-year college and six months of the experience described in subparagraph (1)(ii) of this subdivision or graduation from an accredited four-year college and a written commitment from the county director that the county will provide training in assessment administration, approved by ORPTS, within a six-month period; or,
>
> (4) certification by ORPTS as a candidate for assessor.
>
> (b) In evaluating the experience described in subparagraph (1)(ii) of subdivision (a), the following conditions shall apply:
>
> (i) if the assessor has been previously certified by ORPTS as a State certified assessor pursuant to section 8188-2.1 of this Subpart while serving as an elected assessor, such certification is equivalent to one year of the experience described in subparagraph (1)(ii) of subdivision (a) if it has not expired;

(ii) for the purpose of crediting full-time paid experience, a minimum of 30-hour per week shall be deemed as full-time employment;

(iii) three years of part-time paid experience as sole assessor or as chairman of the board of assessors shall be credited as one year of full-time paid experience, and five years of part-time paid experience as a member of a board of assessors shall be credited as one year of full-time paid experience. Additional paid part-time experience in excess of these amounts shall be credited;

(iv) volunteer experience in an assessor's office may be credited as paid experience to the extent that it includes tasks such as data collection; calculation of value estimates; preparation of preliminary valuation reports; providing routine assessment information to a computer center; public relations; and review of value estimates, computer output and exemption applications; and

(v) in no case shall less than six months of the experience described in subparagraph (1)(ii) of subdivision (a) be acceptable with the exception of county training as provided for in paragraph (3) of subdivision (a).

Finally, table A10 compares the characteristics of elected and appointed assessors as revealed through the survey of local officials. All officials were asked to report on the training backgrounds of their town's assessors, while assessors themselves were asked about their formal education and other demographics. This table shows that, as required by law, both elected and appointed assessors meet the state's basic training requirements, consisting of five courses; moreover, even elected assessors report completing two continuing education courses. Additionally, there are no significant differences in terms of age, gender, education, or income (if anything, elected assessors are slightly more likely to be female and more educated; they report making on average 5,000 dollars less per year). The chief difference is in years of experience: elected assessors were in office for 22 years on average, while appointed assessors were in office 10 years on average. This difference no doubt reflects that the shift to appointed assessors is a more recent phenomenon.

Table 2.A10: Characteristics of elected and appointed assessors from survey of local officials.

	Appointed	Elected
Basic training courses completed (#)	5	5
	(83)	(10)
Continuing education courses completed (#)	3	2
	(62)	(4)
Years in office	10	22
	(56)	(7)
Age	53	57
	(57)	(6)
Female (%)	51	57
	(57)	(7)
Four year degree (%)	25	29
	(57)	(7)
Graduate degree (%)	9	14
	(57)	(7)
Personal income	70,102	65,000
	(49)	(4)

Notes: Cell entries are averages with the number of valid responses in parentheses.

Additional References

Barbanel, Josh. 1992. "Tax Quixote Shakes Town Hall." *The New York Times* (May 29): B1.

Berger, Joseph. 1994a. "Despite Assurances, Complex Factors Drive Up Tax Bills." *The New York Times* (January 21): B5.

Berger, Joseph. 1994b. "They Fight City Hall on Taxes and They're Winning Refunds." *The New York Times* (December 27): B1.

Crosby, Steve. 1978. "Assessor plan is defeated." *Finger Lakes Times* (October 4): 22.

Gruen, Abby. 2008. "Mamaroneck Weighing Move on Tax Inequities." *The New York Times* (November 16): SECTWE.

Herbst, Fred. 2012. "Sole assessor question arises in Moriah: Current assessor opposes any change." *Times of Ti* (August 2).

Kellett, Linda. 2012. "Elected vs. appointed: Minden officials consider (again) assessor change." *Courier-Standard-Enterprise* (August 7).

Lombardi, Kate Stone. 1995. "Property Reassessment Plan Put Forth." *The New York Times* (April 9): 13WC.

Madsen, Nancy. 2011. "Lyme Council sets deadline for residents to send back wind surveys." *Watertown Daily Times* (August 12).

McKinstry, Lohr. 2010. "Crown Point may end elected assessors." *Press-Republican* (September 20).

McKinstry, Gerald. 2012. "Westchester County tax inequalities show need for reassessments." *The Journal News* (January 22).

McNeil, Harold. 2005. "Assessment Challenges Lower than Expected." *Buffalo News* (May 12).

New York State Department of Taxation and Finance. 2013a. "Volume 11 - opinions of counsel sbrps no. 57." Accessed via http://www.tax.ny.gov/pubs_and_bulls/orpts/leg al_opinions/v11/57.htm, August 22, 2013.

New York State Department of Taxation and Finance. 2013b. "Towns Changing from Three Member Boards of Elected Assessors." Accessed via http://www.tax.ny.gov/research/property/assess/training/qualcert/threememberbd.htm on August 25, 2013.

Rubenstein, Carin. 2003. "An Especially Taxing Burden." *The New York Times* (November 16): 14WC.

Shaman, Diana. 1995. "In the Region: Long Island: A Civic Group Scrutinizes Nassau Revaluation." *The New York Times* (December 17): Real Estate 9.

Taghkanic Neighbors. 2011. "Taghkanic to Vote on Sole Appointed Assessor." *Taghkanic Town Watch* 7: 2.

Tan, Sandra. 2007. "Organizers press for 'tax revolt' over assessments." *Buffalo News* (May 14).

Town of Somerset. 2009. "Regular meeting May 12 2009." Accessed August 22, 2013 via http://somersetny.livewebdev.com/files/Mary%2012%202009%20Regular%20Meeting.pdf.

Town of Western. 2011. "Town of Western Public Hearing April 11, 2011." Accessed August 22, 2013 via http://townofwestern-ny.org/Minutes/Town/TS110411.pdf.

Virkler, Steve. 2011. "Lowville now will appoint one assessor." *Watertown Daily Times* (April 22).

West, Debra. 2003. "Feud Erupts in Rye, In Print and in Public." *The New York Times* (December 21): 14WC.

Chapter 3

Failing the Test: Increasing Democracy in Virginia School Districts

Perhaps in no other policy area are elite concerns about "too much democracy" voiced more loudly than education. Beginning with the work of Chubb and Moe (1990), scholars of education policy have consistently traced alleged policy failures back to the democratic institutions governing local school boards. Outside the academy, similar concerns have prompted mayoral takeovers of elected school boards in several large cities since the 1990s (Hess 2008). Despite these concerns, and the reforms they have led to, relatively little is known about the actual effects of democratic control in this area. As with the dearth of evidence of the effects of democratic control in general, the problem is a lack of data: the vast majority of the nearly 15,000 school boards in the United States are elected, and have been since their beginning. This makes it extremely difficult to know whether changing these institutions would actually improve policy.

In this paper, I offer a novel test of the claim that democratic control matters for education policy, using the case of Virginia as a natural experiment. Until the 1990s, all school boards in Virginia were appointed. This changed in 1992, when the state legislature passed a law that allowed local districts to transition to elected boards via referendums. I use these transitions to estimate the effect of elected school boards on spending, revenue, teacher salaries, and class sizes, as well as the descriptive representation of African

Americans.

In stark contrast to those who argue that democratic control is to blame for poor education policy – as well local officials in Virginia, the majority of whom believe there is an impact of elected boards – I find no effect of elections on fiscal policy and class sizes. Voters did not appear to use their newfound power to implement their views about spending; nor is there any detectable impact on teacher salaries, even in districts with higher rates of union membership.

School Board Politics: A Failure of Democracy?

Writing in an era of seeming crisis, two political scientists sharply criticized reform proposals for not addressing the fundamental issue. In *Politics, Markets and America's Schools*, John Chubb and Terry Moe traced America's educational woes to political institutions:

> It is our view that the most fundamental causes [of poor schools] are far less obvious, given the way schools are commonly understood, and far less susceptible to change. They are, in fact, the very institutions that are supposed to be solving the problem: the institutions of direct democratic control. (Chubb and Moe 1990, 2)

The key problem with democratic control in school boards, according to these authors, is that certain vested interests – in particular, bureaucrats and unions – overpower the concerns of ordinary voters in school board elections. While in an ideal democratic system, voters would achieve their objectives by "voic[ing] their preferences through the democratic control structure," in practice, the mass of voters is overwhelmed by "well armed and organized" interests (Chubb and Moe 1988, 1068).

How is it that ordinary voters are so overwhelmed? Chubb and Moe, as well as other scholars, have pointed to the low salience of school board politics for the average voter, relative to the high salience for special interest groups. In the words of Hess (2008), the primary critique of elected school board is that "a lack of attention and electoral involvement makes it difficult for voters to hold their representatives even loosely accountable" (3). In other words, voters are accused of being insufficiently competent to handle the responsibility of school board elections.

This incompetence is alleged to come in several forms. First, they are often accused of not having clear preferences on education policy. For example, Berry and Howell (2007) speculate that voters might not gather the information needed to form preferences. "Most voters, after all," they write, "do not have school-age children, and hence may decide not to collect information on the changing quality of public schools" (848). Second, even if they form preferences, voters may not connect these preferences to candidates in elections. Should they actually show up to vote on election day, "student learning ... may not be at the forefront of citizens' minds when they enter the voting booth and choose from a slate of candidates." Instead, a plethora of irrelevant concerns – such as "safety issues, the football team's record, the convenience of the busing system, or the attractiveness of the buildings" – may enter into voters' electoral calculus (Berry and Howell 2005, 157). Such behavior would be consistent with voters' consideration of irrelevant factors in national elections, such as natural disasters and sporting contests (Achen and Bartels 2004; Healy, Malhotra, and Mo 2009).

Finally, even if voters form preferences and connect these preferences to candidates, there is no guarantee their preferences will be for good education policy. Hess (2008) makes an analogy between elected regulators here. While there is some evidence that "elected regulatory commissions do a better job than appointed commissions of keeping prices down and appeasing public appetites," this may lead to costs in terms of "fiscal discipline." In other words, such a finding may mean that "hard decisions are being rejected in favor of popular short-term decisions," and that elected school boards "may be insufficiently resolute when improvement demands unpopular short-term measures" (3).

While such critiques are commonplace in discussions of local education policy – and have even helped motivate real institutional change in the form of mayoral takeovers of urban school boards – there is very little evidence that local education policy would be any different were school boards unelected. Howell (2005) cites a dearth of research on school board governance generally, glumly speculating that "more is known about the operation of medieval merchant guilds than about the institutions that govern contemporary school districts" (15). Among research that is concerned with school board governance, the claim that direct democratic control matters is made far more often than it is tested. Hess (2008) counts over four hundred studies speculating that elected school boards matter, compared to "fewer than a dozen [that] explicitly examine their impact" (3; see also Land 2002). Chubb and Moe (1988), who began the attack on direct democratic control, did not actually test whether elected school boards mattered; rather, they compared outcomes between public and private school districts. Justifying this decision, they pointed to the simple fact that the vast majority of school boards in the United States are elected. As they asked, "How can we study institutional effects if there is only one, all-encompassing institution?" (1066-1067).

Two studies that have partially tested for an effect of elected boards are Berkman and Plutzer (2005, 103-104) and Wong and Shen (2005). The former study tests whether the relationship between voter attitudes and policy outputs is stronger in cases when boards are elected, finding that this relationship is stronger when the board is appointed. However, this study does not test for an impact on policy, but rather the relationship between policy and opinion. Berkman and Plutzer also conduct their test by comparing roughly 10,000 elected boards and 342 appointed boards, most of which were concentrated in a few states. This means that there are likely many other factors that differentiate the treatment and control group aside from political institutions. Similarly, Wong and Shen test whether mayoral takeovers affect fiscal policy, finding no differences. Yet as in the former study, given that the sample cities come from many different states, there may be unobservable differences between treatment and control cities that confound comparisons.

Increasing Democracy in Virginia School Districts

To overcome these issues, I use the case of Virginia as a test of the hypothesis that direct democratic control affects education policy. Until 1992, Virginia was the only state without "direct democratic control" of its school boards. This status originated in the post-Civil War era, when the state legislature in 1877 gave the power of appointing county-level school trustees to a local selection commission consisting of the circuit judge, the school superintendent, and the commonwealth's attorney (Moffat 2000, 17). At the state Constitutional Convention of 1901-1902, delegates, openly citing the desire to disenfranchise black voters, rejected a proposal to write a system of elected school boards into the new constitution (Moffat 2000, 4; see also Morris and Bradley 1994, 286). Eventually counties gained the option to give the appointing power to the elected board of supervisors; city school boards were appointed by municipal councils throughout this period (Moffat 2000, 21).[1]

The effort to change to elected boards was largely the effort of one state legislator,

[1]Virginia is also unlike some other states in that its school boards are fiscally dependent, meaning that while they write their own budget, they rely on another government to levy taxes in order to fund the budget. Despite this arrangement, and as I mention in more detail below, a majority of local officials in the state cite education spending as one of the biggest impacts of the transition to elections, including a majority of officials from the boards on which school boards depend for revenue. According to the Edu-

David G. Brickley, a Democrat representing parts of Prince William County. Between 1976 and its ultimate success in 1992, Brickley introduced a bill to provide for elected school boards in the state General Assembly (Massie 2010, 1). While the details of the plan varied over the years, the key element was to empower local governments to decide for themselves, via local referendum, whether to change. The measure failed many times due to opposition from local officials, who were loath to give up their appointment power (Massie 2010, 2). Yet Brickley's bill slowly gained co-sponsors with every re-introduction; by the time the bill passed in February 1992, many of the plan's stalwart opponents had either retired or been voted out (Massie 2010, 3). The governor signed the legislation in April 1992, and the first referendums were held later that year.

Right up until the year they became reality, elected school boards divided elite opinion in Virginia. In 1988, the state branch of the American Civil Liberties Union had sued Virginia, alleging that appointed boards discriminated against blacks (Massie 2010, 2). The Virginia School Boards Association believed that that *elected* boards would dilute minority representation, and lobbied against the reform in 1992. Yet the state chapter of the National Association for the Advancement of Colored People supported the reform (Moffat 2000, 5). Partisan politics also played a role: Brickley, a Democrat, was virtually the lone supporter of elected boards in his party for many years; the proposal gained some momentum when state Republicans recognized it as a winning issue. Meanwhile, the Virginia Education Association – the state affiliate of the National Education Association teachers' union – consistently supported Brickley's proposals, believing that they would achieve higher salaries and more political influence under the elected system (Massie 2010, 4).

---

cation Commission of the States (1997), fiscally dependent school boards are also the norm in Alabama, Alaska, California, Connecticut, Hawaii, Maryland, Massachusetts, Mississippi, Nevada, Rhode Island, Tennessee, New Hampshire, and Maine.

While the initial plan may have been primarily the work of one crusading legislature, and may have divided political elites, the measure had widespread support among the electorate. Moffat (2000) describes the ensuing series of referendums as a "flash flood of change" that "clearly had its genesis in grass-roots populism" (296). Voters enthusiastically embraced democracy: the majority of districts voted to elect, with an average margin of 5 to 1 in favor, by the end of the decade. In 1992 alone, 43 jurisdictions held elected school board referendums; in 9 of these districts, turnout was higher than in the concurrent presidential race (Moffat 2000, 298).[2] According to Moffat, traditional cleavages such as race, party, and interest group alignment played little or no role in voters' decisions. The reason was that political elites largely stepped aside. "In the face of grass-roots sentiment to get the issue on the ballot, most politicians either ignored the issue, took no official position, or assumed an unassailable position – declaring themselves ready to support whatever choice the voters made" (296).

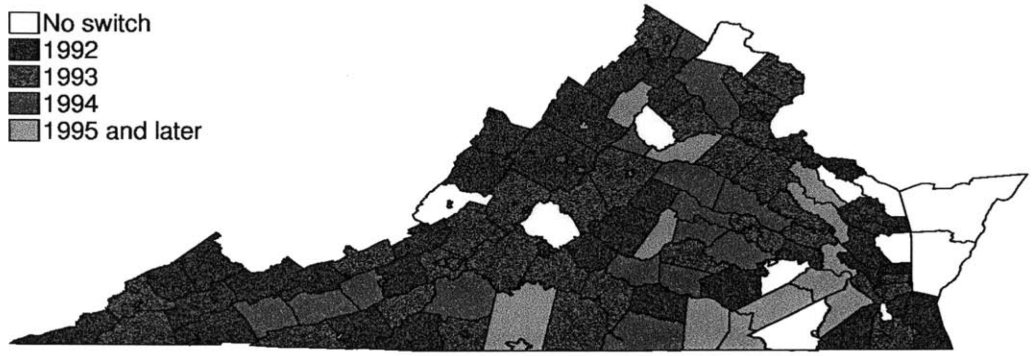
Figure 3.1 maps the transitions to elected school boards that occurred between 1992 and 2010 in the 95 counties, 35 cities, and two towns that make up Virginia's 132 school districts.[3] Darker shadings represent switches in earlier years, while lighter shadings represent later switches; districts in white did not switch over this period, and have retained their appointed boards. Over this period, 113 districts passed referendums enabling the switch to elected school boards; 19 districts (12 cities and 7 counties) did not. Consistent

---

[2]Only three districts held referendums that failed over this period.

[3]I describe the collection the data used to produce this map in the Appendix. Unlike some other states, Virginia's school districts are coterminous with county and city boundaries. According to Massie (2010), there are 136 districts total. My sample is 132, as there are several joint city-county districts that I treat as single districts (Bedford County and Bedford City; Fairfax County and Fairfax City; Greensville County and Emporia City; James City County and Williamsburg City).

Figure 3.1: Increasing democracy in Virginia school districts.



Notes: This figure maps the transitions to elected school boards that occurred between 1992 and 2010 in the 95 counties, 35 cities, and two towns that make up Virginia's 132 school districts. Darker shadings represent switches in earlier years, while lighter shadings represent later switches; districts in white did not switch over this period, and have retained their appointed boards.

with Moffat's "flash flood" description, the majority of referendums were passed early on in the decade: 43 in 1992, 36 in 1993 and 18 in 1994, with an average of one every two years thereafter.

Elite Expectations about Elected Boards

As summarized in the review above, scholarly opinion is quite convinced that "institutions of direct democratic control" have important consequences for education policy. Likewise, elites in Virginia clearly expected that the switch to elected school boards would have an impact on policy – though they were often divided about just what the effect would be. Brickley, the sponsor of the reform legislation, claimed the appointed school board in his home county of Prince William was unresponsive to local concerns regarding class sizes (Massie 2010, 1). The state teachers' union organization supported the reform, arguing it would result in higher salaries for teachers. The ACLU argued that elected boards would improve minority representation; the NAACP and the Virginia

School Boards Association believed elected boards would harm minority representation. Meanwhile, the voters themselves appeared to be taken with the general idea of increasing the accountability of officials who controlled such a large portion of local budgets (Hall 1993).

Two decades post-reform, local elites in Virginia still believe that elected school boards have made an impact. In an original survey of local officials in Virginia – including members of City Councils, County Boards of Supervisors, and School Boards – I asked respondents whether they believed the switch to elected school boards had a positive impact, a negative impact, or no impact. Of the 223 officials who gave valid responses to this question, 44% said that elections had a positive impact, 39% said they had a negative impact, and 17% said elections had no impact. Thus, the overwhelming majority of local officials, 83%, believe that elected boards have an impact of some kind.

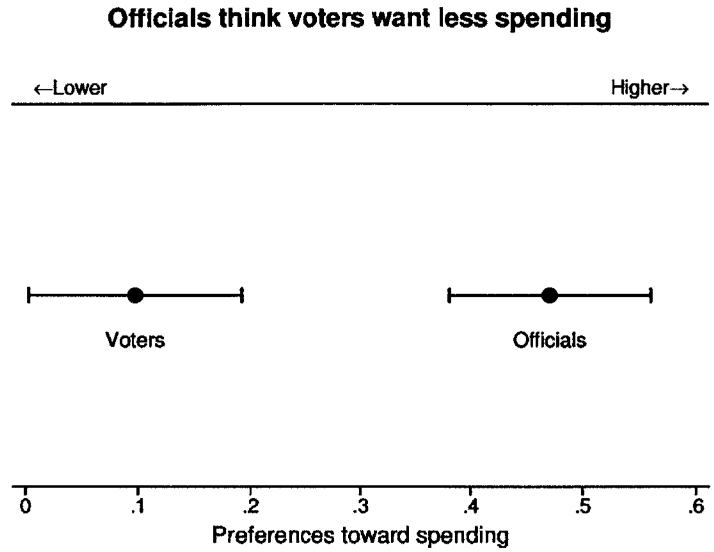
I also asked officials about the impact of elections on specific policy areas. Here, elite opinion is slightly less confident in the importance of elections. Sixty-six percent of officials believed that elected boards had an impact on local education spending. In terms of policy outcomes, 37% believed that class sizes were impacted while 44% believed student achievement was affected.[4]

That spending was ranked so highly as an area impacted by elections would make sense, if we were to find that voters and officials hold different preferences over the correct amount of education spending. If voters and officials had the same preferences, the electoral incentive would not be necessary for officials to adopt policies in line with voter preferences: they would be free to follow their conscience.

To gauge the amount of preference disagreement, I asked local officials whether they themselves believe that education spending in their district should be increased, decreased,

[4]These frequencies did not vary substantively by office; I show responses by office in the Appendix. The Appendix also contains more details on the elite survey.

Figure 3.2: Local officials believe voters want lower spending.



Notes: This figure shows responses to questions about whether officials believe education spending in their district should be increased, decreased, or kept the same; as well as how officials believe voters in their district feel about this issue. Responses are on a three-point scale (decrease = -1, no change = 0, increase = 1). Points are means with horizontal lines spanning 95% confidence intervals. N = 225.

or kept the same; I also asked them how they believed voters in their district felt about this issue. The result was a three-point scale on which officials and voters (as perceived by officials) can be ranked in terms of their preferences toward education spending. I show responses to these questions in Figure 3.2. As the figure shows, officials perceive a surprising amount of disagreement between themselves and the voters. On the three-point scale, officials strongly place themselves in favor of greater spending, at about 0.48. At the same time, they perceived voters as at about 0.1.

In sum, while elite opinion was divided over the precise nature of the effect, the vast majority of officials believed, and continue to believe, that elected school boards make a difference for policy. The most salient area of impact appears to be education spending; this is intuitive, given that officials perceive themselves as further from the voters on this issue. The question is then whether voters actually used their newfound power to obtain their policy goals.

Effect of Elections on Fiscal Policy

To estimate the effect of elections on policy, I use a difference-in-differences estimator of the form,

$$y_{jt} = \delta * elected_{jt} + district_j + year_t + \sum_{k=1}^{K} x_{kjt} * \beta_k + u_{jt}$$

where y_{jt} is the outcome for district j in year t; *elected* is a binary variable equal to 1 if district j held and passed a referendum to switch to elected boards as of year t; *district* and *year* are district- and year-fixed effects; x_{jt} are time-varying covariates; and u_{jt} is an error term. This design effectively holds all time-invariant confounding variables constant; further, the inclusion of time-varying covariates should capture sources of confounding that vary over time.[5] Finally, because all the outcomes are highly serially correlated – the year-to-year correlations range from 0.95 to 0.99, depending on the outcome – I express all outcomes in terms of annual percentage changes.

I show the results in Table 3.1. The first two columns show the effect of elected school boards on spending per-pupil. As shown in the header to the table, spending per-pupil rises by about 1.83% every year. As indicated in the first column, when a district changes to elected school boards, this increases by 0.29; however, the standard error is 0.36, meaning we can not reject the null hypothesis of no actual effect on spending. When adding

[5]The time-varying covariates include log population, the proportion of white residents, the log of average income, the proportion of residents over age 65, the proportion of residents younger than 18, and the proportion of home-owners. I show balance on these covariates between treatment and control districts in the Appendix.

Table 3.1: Direct democratic control does not matter for fiscal policy or class sizes.

Outcome	Spending		Revenue		Salaries		Class sizes	
Average	1.83		1.95		0.14		-0.84	
Elected	0.29	0.34	-0.73	-0.37	0.05	0.11	0.32	0.39
	(0.36)	(0.36)	(0.76)	(0.81)	(0.25)	(0.25)	(0.31)	(0.30)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: District-clustered standard errors in parentheses. All regressions include district and year fixed effects. Sample period is 1988-2012 for all regressions. N = 132 districts and N = 3,300 total observations. All outcomes are measured as annual percentage changes.

covariates in the second column, the estimate is essentially unchanged: the point estimate is 0.34, with a standard error of 0.36.

The second pair of columns test for an effect on local revenue (primarily through property taxes). On average, revenue grows by about 2% each year. When a district changes to elected boards, this declines by 0.73. Again, the large standard error of 0.76 means that we can not reject the null hypothesis of no effect of elections. Similarly, when adding covariates, the point estimate declines to -0.37, with a standard error of 0.81.

The third pair of columns tests for an effect on average teacher salaries. On average, these salaries increase by 0.14% each year. When a district changes to elected boards, this increases by between 0.05 (standard error = 0.25) and 0.11 (standard error = 0.25) depending on whether covariates are adjusted for. That there is no effect on teacher salaries is particularly surprising, given concerns about voter inattention leading to "interest group capture." I return to this point in the conclusion.

Finally, the fourth pair of columns in Table 3.1 test for an effect on class sizes, calculated as the number of students in a district divided by the number of instructors. Over this period, class sizes were decreasing by -0.84% per year; when a district changes to elections, the point estimates suggests that this rate increases by 0.32 or 0.39, depending on whether covariates are adjusted for. Again, however, the standard errors (0.31 and 0.30, respectively) are so large that we can not reject the hypothesis that the true effect is zero.

While these estimates provide a gross indicator of how outcomes changed after a district switched to elections, they might mask important dynamic variation. For example, it might be that spending increased sharply in the year following the switch, and then returned to the mean. Alternatively, effects may take time to manifest themselves. It is also important to check whether districts were trending in a certain direction prior to the switch. For example, if switching districts saw a spike in spending just before they switched, this could violate the identifying assumptions of the estimator.

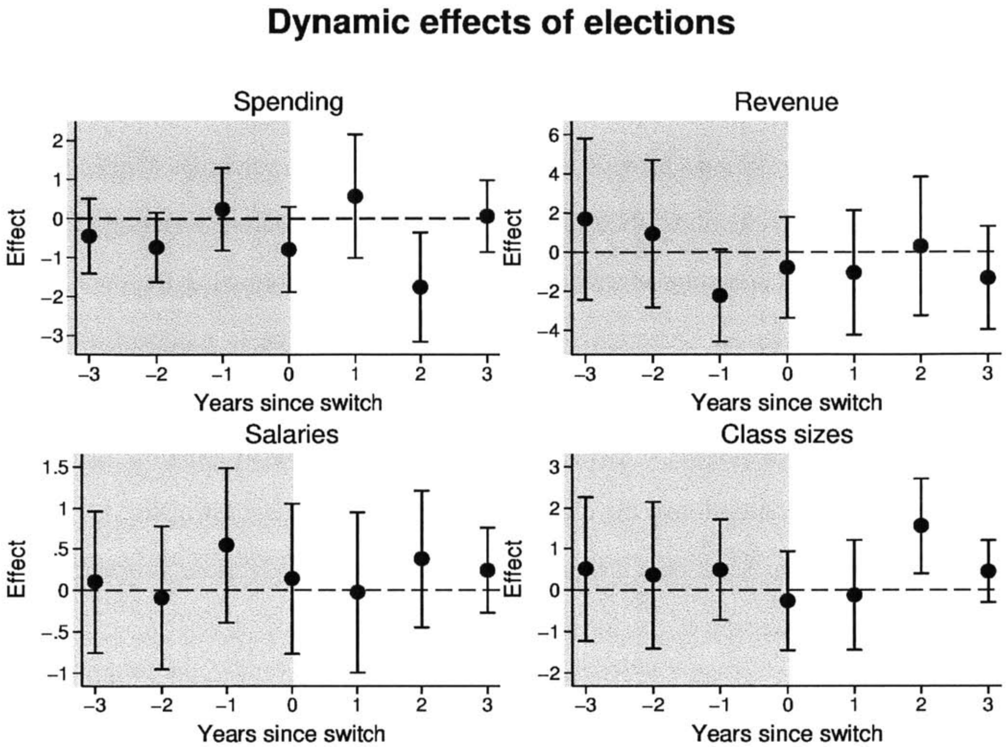
To address these concerns, I show how the non-effects of elections vary over time. First, I estimate a regression of the form,

$$\begin{aligned} y_{jt} &= \sum_{\tau=-3}^{2} \delta_\tau \mathbf{1}_{\{switchyear_j - year_t = \tau\}} + \delta_3 \mathbf{1}_{\{switchyear_j - year_t \geq 3\}} \\ &\quad + district_j + year_t + u_{jt} \end{aligned}$$

The δ_τ coefficients represent the "effect" of elections in the years prior to the switch (when there should be no effect) as well as in the years after; the last coefficient, δ_3, represents the summary effect of elections when the switch is at least three years past. Second, I plot these coefficients, and their 95% confidence intervals, in Figure 3.3.

Figure 3.3 confirms that elections have no effect on any of these outcomes: there is no discernible difference in trends between switching and non-switching districts, either in any of the years prior to the switch, or in the years after. The sole exceptions are the growth rates for spending, which appears to decline by two percentage points two years after the switch; and class sizes, which appears to increase by about 1.5% two years after the switch. However, neither of these apparent effects appear to persist over time; and

Figure 3.3: The dynamic effects of school board elections on policy.



Notes: Vertical bars span 95% confidence intervals.

they are generally not robust. In the Appendix, I show that the "effect" for spending disappears when changing the treatment from the years since the referendum to the years since the switch to elections completed.[6]

Finally, I also explore whether the null results in Table 3.1 mask heterogeneous effects. For example, the impact of elections could vary based on local policy preferences or interest group strength. In the Appendix, I show this is not the case: when interacting the treatment with the proportion of teacher union members, the proportion of white residents, Democratic presidential voteshare, or the proportion of school-age residents, the

[6]The "effect" for class sizes survives this test, but it is difficult to see it as anything but an artifact given that salaries and spending remain constant.

null results persist across the four outcome variables. I also show that these null results are robust to iteratively dropping different districts and cohorts.

Conclusion

A key assumption of democracy is that voters will hold officials accountable for failing to meet their policy preferences. This turns out to be a tall order for the voters, who often lack preferences and fail to pay attention to what their representatives are doing. As a result, blaming democratic control for policy failures is highly intuitive: with voters not paying attention, special interests may pervert the political process. Perhaps nowhere else is this argument heard more than in the context of the American school board.

Yet my results suggest these concerns, while not entirely wrong, are incorrect in important ways. Voters do appear to have preferences over education spending, the key policy issue in school board politics. Yet they do not appear to succeed at using elections to achieve their preferences: when districts change to elected boards, there is no impact on spending or any other fiscal indicator.

With voters not holding officials to account for policy decisions, we might expect special interests to enter in. Yet this is not what I find: in the aggregate, teacher salaries do not increase as a result of elections. Nor do they change in districts with more or less teacher union strength. One explanation for this result is that teacher unions are just as adept at capturing elected board members as they are at capturing appointed board members. Yet given that the average salary increase is just 0.14% in all districts, whereas the average spending increase is 1.83%, it is hard to see how teachers have "captured" the school board under either regime. A more likely explanation is that Virginia, as a "right to work" state, has uniformly weak unions in all districts. Finally, it is also possible that while voters do not pay attention to policy in elections, they are attuned to charges of interest group capture that could be made by challengers or the media (Arnold 1992, Chapter 10). Such an "auditing" mechanism could be at work with equal effectiveness under both the elected and the appointed regime, which would result in a null effect of elections on salaries.

References

Achen, Christopher H., and Larry M. Bartels. 2004. "Blind retrospection. Electoral responses to drought, flu, and shark attacks." Estudios/Working Papers (Centro de Estudios Avanzados en Ciencias Sociales) 199.

Berkman, Michael B., and Plutzer, Eric. 2005. *Ten thousand democracies: Politics and public opinion in America's school districts*. Georgetown University Press.

Berry, Christopher, and William G. Howell. 2005. "Democratic accountability in public education." In *Besieged: School boards and the future of education politics*, edited by William G. Howell. Brookings Institution Press. pp 150-198.

Berry, Christopher R., and William G. Howell. 2007. "Accountability and local elections: rethinking retrospective voting." *Journal of Politics* 69(3): 844-858.

Chubb, John E., and Terry M. Moe. 1988. "Politics, markets, and the organization of schools." *American Political Science Review* 82(4): 1065-1087.

Chubb, John E., and Terry M. Moe. 1990. *Politics, markets, and America's schools*. Brookings Institution Press.

Education Commission of the States. 1997. "Finance: Fiscally Dependent/Independent School Districts." *ECS Information Clearinghouse*. Accessed June 4, 2014 via http://dev.ecs.org/clearinghouse/13/22/1322.htm.

Hainmueller, Jens and Hangartner, Dominik. 2013. "Does Direct Democracy Hurt Immigrant Minorities? Evidence from Naturalization Decisions in Switzerland." MIT Political Science Department Research Paper No. 2013-1. Available at SSRN: http://ssrn.com/abstract=2022064.

Hall, Charles W. 1993. "Local Leaders Battle School Board Elections; Some Say New System Would Hurt Minorities." *Washington Post* September 23.

Healy, Andrew J., Neil Malhotra and Cecilia Hyunjung Mo. 2010. "Irrelevant events affect voters' evaluations of government performance." *Proceedings of the National Academy of Sciences* 107(29):12804–12809.

Hess, Frederick. M. 2008. "Assessing the Case for Mayoral Control of Urban Schools." *American Enterprise Institute Education Outlook* 4 (August).

Howell, William G. 2005. "Introduction." In *Besieged: School boards and the future of education politics*, edited by William G. Howell. Brookings Institution Press.

Land, Deborah. 2002. "Local School Boards Under Review: Their Role and Effectiveness in Relation to Students' Academic Achievement," *Review of Educational Research* 72(2): 239.

Massie, L. A. 2010. *Perceptions of Superintendents and School Board Members Who Experienced the Transition from Appointed to Elected School Boards.* Doctoral dissertation, Virginia Polytechnic Institute and State University.

Moffat, John W. 2000. *Populism and Civil Rights: The Impact of the Voting Rights Act on Changing School Governance in Virginia.* Doctoral dissertation, University of Maryland.

Morris, Thomas, and Neil Bradley. 1994. "Virginia." In *Quiet Revolution in the South: The Impact of the Voting Rights Act*, 1965-1990, edited by Chandler Davidson, and Bernard Grofman. Princeton, NJ: Princeton University Press. 279-98.

Wong, Kenneth K., and Francis X. Shen. 2005. "When Mayors Lead Urban Schools: Assessing the Effects of Takeover." In *Besieged: School boards and the future of education politics*, edited by William G. Howell. Brookings Institution Press. pp 81-101.

Appendix

Data Collection

To determine which districts switched to elections when, I relied primarily on a data set provided to me by John Moffat, who collected data on the first wave of transitions for his own study (Moffat 2000). Because these data only went up to the end of the 1990s, I then completed the series through 2010 by examining local election results from the Virginia Board of Elections.

I obtained data on per-pupil spending, revenues, teacher salaries, and class sizes by examining the annual state Department of Education's Superintendent's Reports. While the most recent years of these reports are available on the state's web site, I obtained the earlier years by requesting paper copies from the Department of Education and digitizing the relevant tables.

Data on demographics come from the decennial Census for 1980, 1990, 2000, and 2010. The years 1980 and 1990 came pre-aggregated to the city, county, and town level; the years 2000 and 2010 required aggregating from the block level. I then linearly interpolated between Census years.

Data on the proportion of teacher union members comes from the Census of Governments, 1987: Employment Statistics. I calculate this ratio as the number of organized full-time instructional employees to the number of organized full-time employees.

Survey of Local Officials

I obtained contact information for local officials from the Virginia Review Directory of State and Local Government Officials at http://vareview.com/. This directory had at least the name, and usually an e-mail and mailing address, for all school board members, school superintendents, local council (i.e. Board of Supervisors if a county; City Council if a city; Town Board if a town), and local executive (i.e. County Manager or Executive if a county; Mayor or Manager if a city or town) for all local government units in the state. In some cases where officials' e-mail addresses were missing, I obtained e-mail addresses via web searches. The result was a list of 1,666 officials with e-mail addresses, and 245 with no e-mail address but a mailing address.

The survey was e-mailed to the officials with e-mail addresses in February 2014. The initial e-mail simply informed respondents that a survey would be coming in one week. A week later, the survey itself was e-mailed, followed by two weekly reminders. In March 2014, postcards were sent to all 245 officials with no e-mail address, as well as any appointed school board members who had not yet replied to the e-mail survey; the total number of postcards was 481. The first postcard alerted respondents about the survey and informed them they could e-mail the author if they preferred to receive the survey via e-mail. The second postcard included a unique, shortened survey link that recipients could use to access the online survey.

The end result was 226 officials who both consented to and completed the survey, for a response rate of 12%. Figure 3.A1 maps the number of responses received from each district. At least one valid response was received from 122 out of 132 school districts. Table 3.A1 shows the distribution of offices responding to the survey, and Figure 4.A2 shows that the responses to survey questions discussed in the main text do not vary appreciably by office.

Figure 3.A1: Number of responses to elite survey by school district.

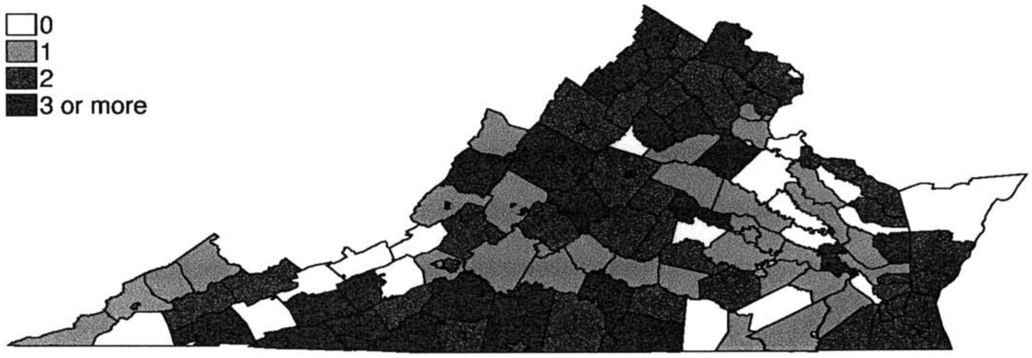
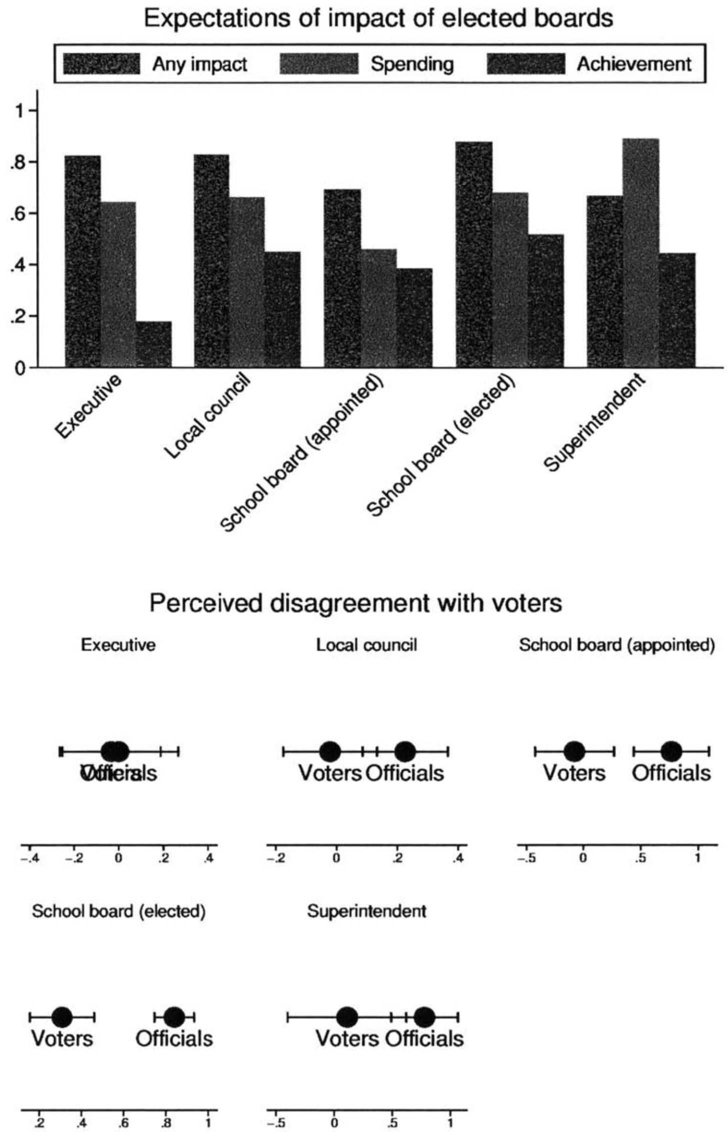


Table 3.A1: Number of responses by office.

	Number
Executive	30
Local council	93
School board (appointed)	13
School board (elected)	81
Superintendent	9
Total	226

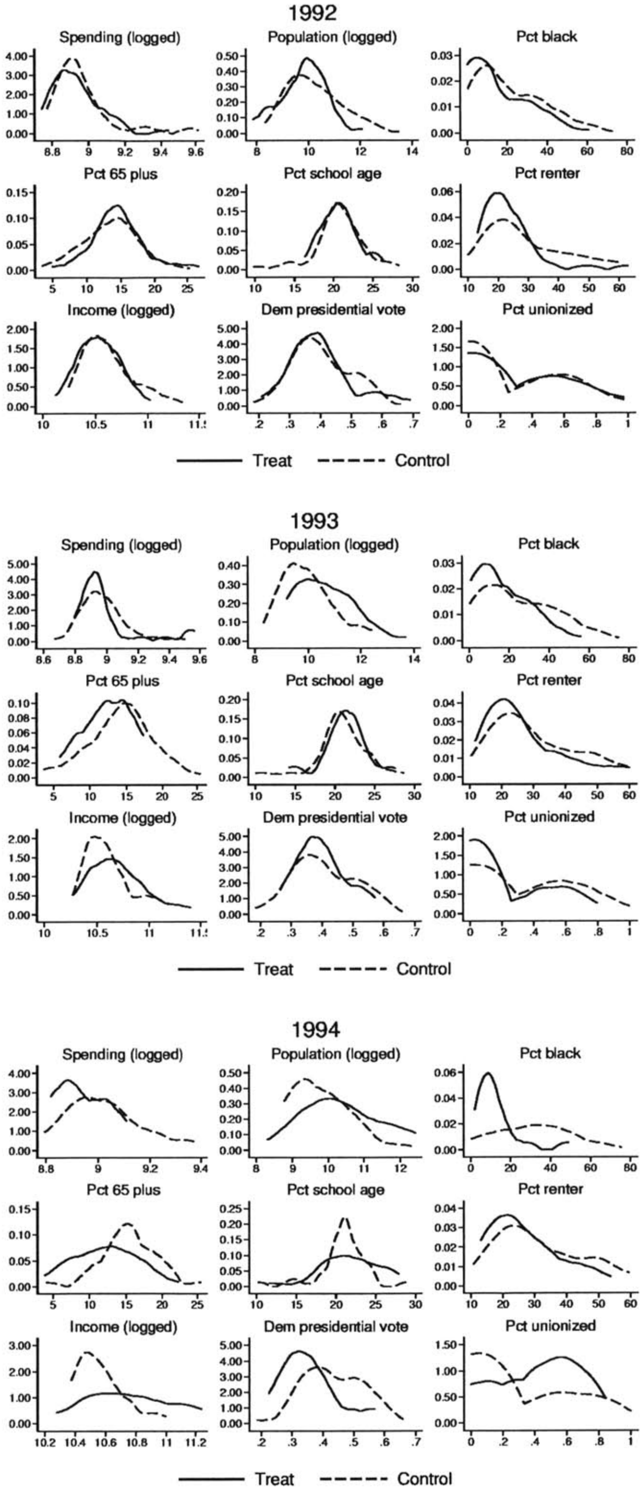
Figure 3.A2: Survey responses by office.



Covariate Balance

Figure 3.A3 compares treated and control districts on observable characteristics, for the three largest cohorts of switching districts. In these figures, the densities represent the difference between elected and appointed districts in the year of the switch, with the exception of spending which is lagged one year. These figures show considerable overlap between treated and control districts in each cohort, though the balance is worst for the cohort that switched in 1994.

Figure 3.A3: Balance on observable characteristics.



Robustness Checks

Alternative Definition of Treatment Year

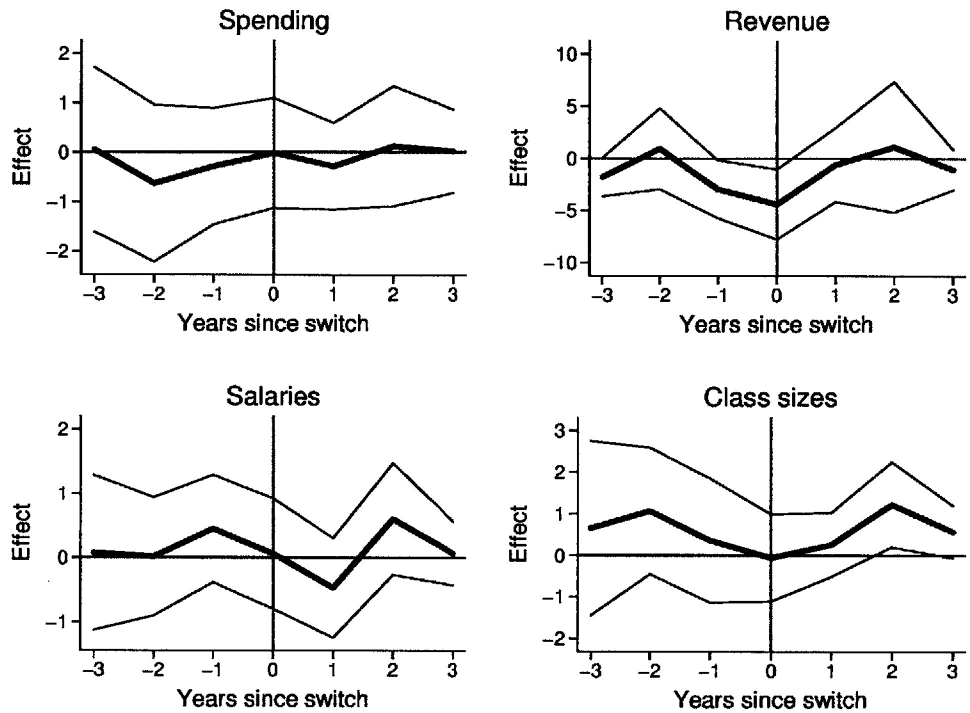
The estimates in the text set the treatment equal to 1 once a district passes a referendum to switch to elected school boards. This might induce noise if there is a lag between referendum passage and the actual transition. In practice, districts can take a few election cycles to fully replace their boards with elected members. I replicate the results in the main text using the year this transition to fully elected members completed; I show these results in Table 3.A2 and Figure 3.A4. Table 3.A2 shows that the difference-in-differences estimate of no effect is unchanged using this alternative definition. Figure 3.A4 shows that there are no dynamic effects of the recoded treatment: the bump observed for spending in the main text has now disappeared, though there is still a slight uptick in class sizes two years post-treatment.

Table 3.A2: Replication of Table 3.1 using recoded treatment.

Outcome	Spending		Revenue		Salaries		Class sizes	
Average	1.83		1.95		0.14		-0.84	
Elected	0.10	0.13	1.11	1.56	-0.07	-0.02	0.35	0.43
	(0.28)	(0.29)	(1.09)	(1.13)	(0.18)	(0.19)	(0.24)	(0.25)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: District-clustered standard errors in parentheses. Sample period is 1988-2012 for all regressions. N = 132 districts and N = 3,300 total observations. All outcomes are measured as annual percentage changes. * p<0.05, ** p<0.01, *** p<0.001

Figure 3.A4: Replication of Figure 3.3 using recoded treatment.



Notes: Thick lines connect point estimates; thin lines represent 95% confidence intervals.

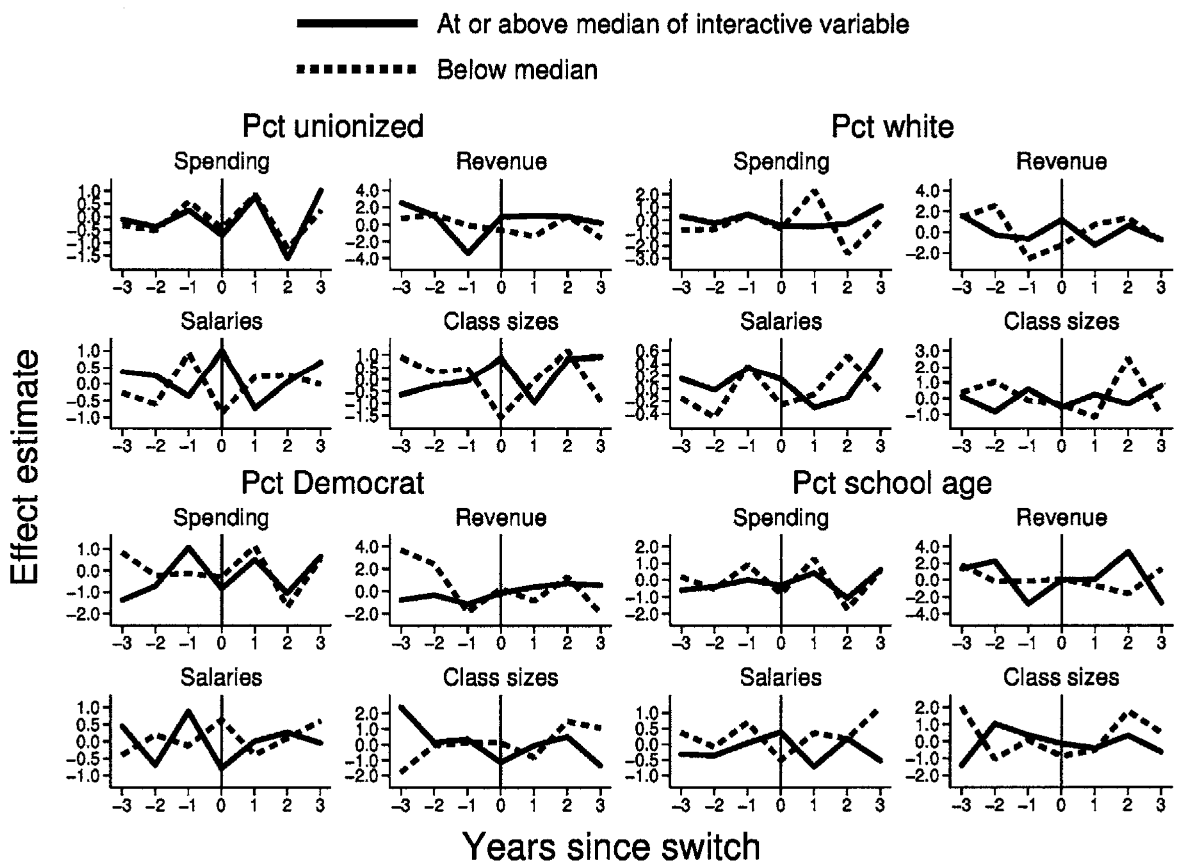
Testing for Heterogeneity by District Characteristics

While there is no average effect of school boards on fiscal policy, there could be effects that vary by district characteristics. I test whether the effect of elections varies by the percentage of unionized school employees, the percentage of white residents, partisanship (proxied with 1988 Democratic presidential voteshare), and percentage school age children. I test for these interactions graphically using the following procedure. First, I estimate a regression of the form,

$$y_{jt} = district_j + year_t + u_{jt}$$

Second, I plot the residuals from these regressions against the time until a district switched for districts that are above the median on the interactive variable of interest, or below the median of this variable. I show the results in Figure 3.A. Each of the four groups of four panels represents a different interactive variable; each graph within a panel represents a different outcome. For example, the first graph plots spending per pupil pre- and post-switch, for districts that are above the median unionization level (solid line) or below (dashed line). The graph shows no difference between the null effects of elections between more or less unionized districts. The remaining 15 graphs all tell a similar story.

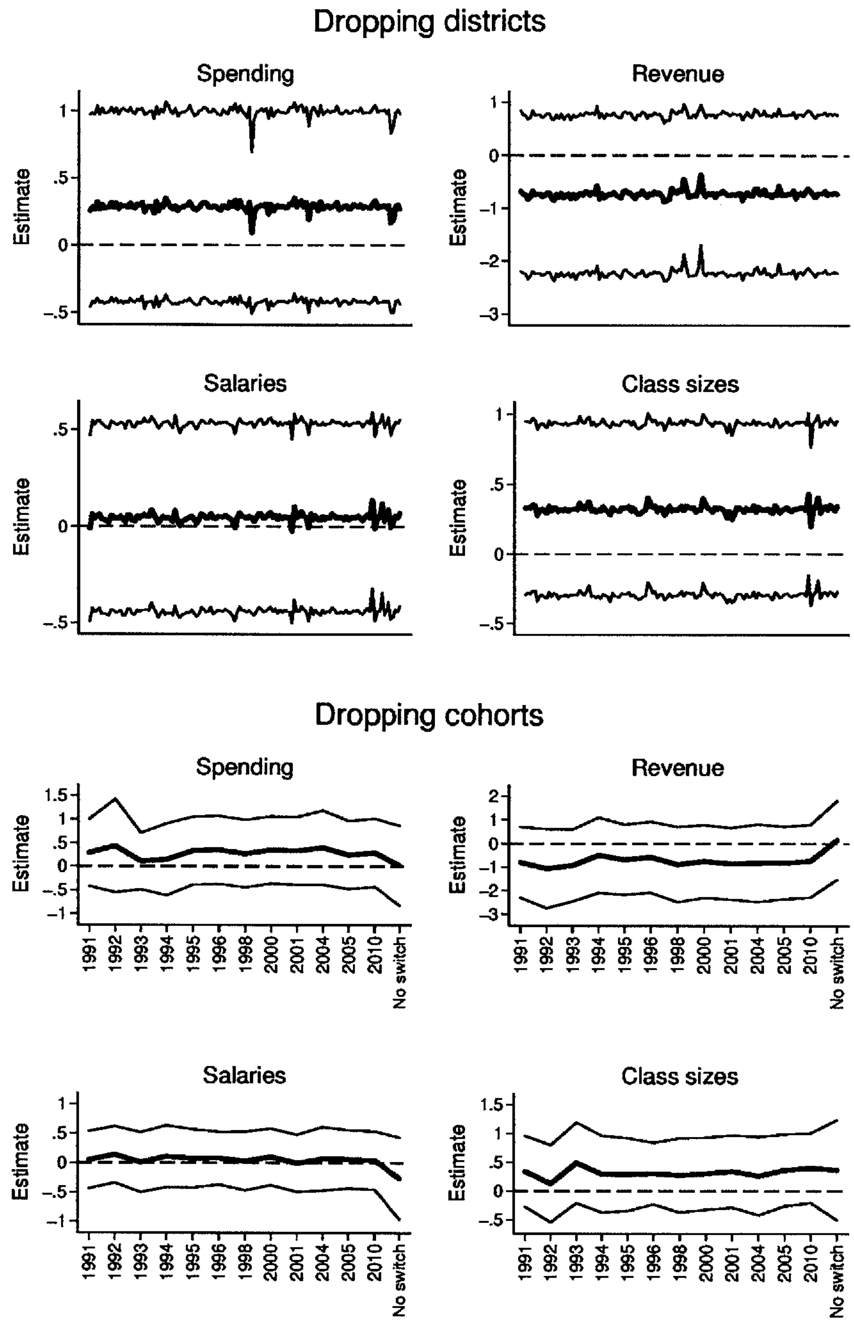
Figure 3.A5: Dynamic interactive effects of elected school boards.



Sensitivity to Particular Districts and Cohorts

Figure 3.A6 tests the sensitivity of the results to the exclusion of particular districts and cohorts. The top panel replicates the point estimates in Table 3.1 by iteratively dropping one district at a time. This figure shows that the results are not driven by any one particular district exerting disproportionate weight on the results. The bottom panel repeats this exercise by iteratively dropping one cohort at a time. Again, the estimates are not sensitive to the exclusion of any particular cohort.

Figure 3.A6: Sensitivity of estimates to dropping particular districts.



Chapter 4

Playing with Fire: Increasing Democracy in Illinois Special Districts

In the preceding two chapters, we have seen that granting voters more power via elections sometimes affects policy, but sometimes does not. In the case of assessors in New York, voters used elections to achieve their policy preference. But in the case of school boards in Virginia, voters could only form preferences, but not implement them. What explains these divergent results?

One explanation is that voters in the first case were more capable than voters in the second. For some reason, perhaps voters in New York towns are better equipped to use elections than voters in Virginia counties. While a possibility, this explanation is not very helpful for making predictions about the potential effects of increased democracy in other contexts, as it begs the question of what makes voters more inherently "capable" in a particular context.

An alternative explanation is that voters are of equal ability in both cases, but that features of the institutional environment allowed them to meet their roles with greater ease. For example, a visit by the assessor to one's home for revaluation purposes is probably much more salient than an obscure school board meeting to discuss the budget. Similarly, assessments as policy decisions are trivially attributed to the assessor, whereas budget decisions are more difficult to observe and attribute to board members.

While both of these explanations are possible, it is difficult to adjudicate between them beyond simply speculating, given that the data come from two separate case studies. In this chapter, however, I present strong evidence in support of the second explanation. I do so by focusing on a single case, fire protection districts in Illinois, where two reforms grant voters more power over policy: elections, which allow voters to choose their representatives; and referendums, which give voters a veto over the decisions of these representatives. I show that only the referendum enables voters to achieve their policy preference for lower spending. Because voter characteristics are held constant in this case, this result suggests that the institutional environment makes accountability more or less likely.

Crucially, however, while referendums simplify the accountability process, they do not help voters' form preferences for good policies. Similar to the assessor case, when voters achieve their policy preference – in this case, less spending and thus less revenue – the quality of public services suffers. Emergency response times increase by about 40 seconds, on average, when voters gain veto power over tax increases.

Increasing Democracy in Illinois Fire Districts

In many rural and suburban parts of the country, "special district governments" provide many local services, such as fire protection, libraries, and public transportation. These governments are distinct in that they focus on providing a single service, unlike "general purpose" governments such as counties or cities; they also operate independently of these other units, typically within borders that overlap other jurisdictions. According to the Census of Governments, there are over 37,000 such special district governments nationwide. Fire protection is by far the largest category of special district government, and the state of Illinois contains over 800 such districts. Typically, these districts are established in rural areas where residents of various localities decide to pool resources in order to provide fire protection for the larger area. The districts are governed by boards of trustees, who write the budget and have the power to levy property taxes on district residents.

With the number of overlapping governments growing over time in Illinois, rural and suburban residents became concerned about issues of accountability in the 1990s. These concerns manifested themselves in two reforms. First, district residents began to demand electoral control over district trustees, who by default are appointed by county or township governments.[1] This resulted in a series of transitions from appointed to elected boards of trustees.

Second, the state legislature passed the Property Tax Extension Limitation Law, or PTELL. This law mandated that property tax revenues could not be increased by the minimum of 5% or the annual increase in the national Consumer Price Index, unless voters approve such an increase via referendum. The initial state legislation applied only to the Chicago-area "collar" counties (DuPage, Kane, Lake, McHenry, and Will) in 1991, but gave all of the other 97 counties the option to adopt the referendum regime. While enacted at the level of counties, the referendum requirement applies to all non-home rule governments within the county, including school districts, park districts, and fire districts (Illinois Department of Revenue 2012).

---

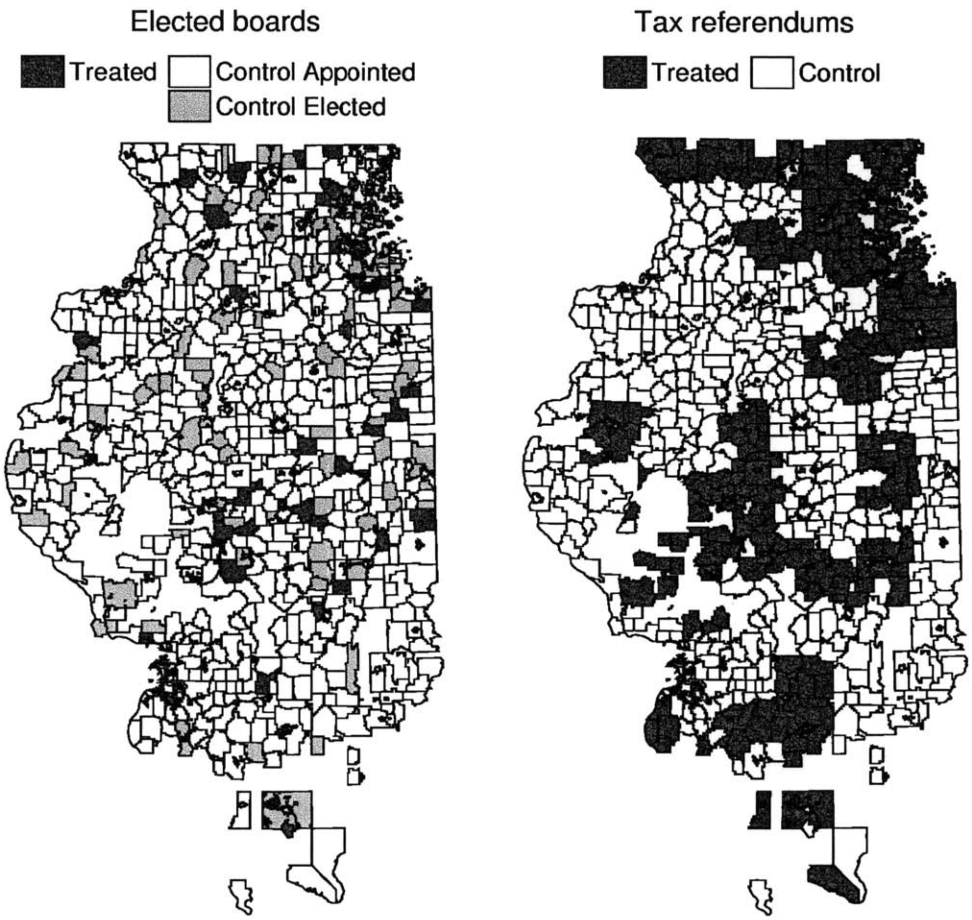
[1]Districts apparently had the power to switch to elected boards prior to the 1990s, though the exact date at which this began is difficult to pinpoint. The statute enabling the establishment of fire districts was enacted in 1927. A review of the available legislative history between the years of 1927 and 1990 yields conflicting information. For example, the Center for Governmental Studies at Northern Illinois University reports that only park and drainage districts are elected, and all other special district boards in Illinois are appointed (Rehfuss and Tobias 1977). Yet a 1979 publication from the University of Illinois Springfield discusses whether voters must register for fire trustee elections (University of Illinois Springfield 1979).

Figure 4.1 maps these transitions between 1992 and 2009 in the 739 districts in my sample.[2] The left map shows districts that switched to elected boards in dark gray, districts that remained elected throughout this period in light gray, and districts that remained appointed throughout in white. The right panel shows districts that became subject to tax referendums in dark gray, and those that did not in white. Over this period, 38 counties adopted the tax referendum regime, including 330 fire protection districts.[3] Likewise, 40 districts changed from appointed to elected trustees over this period; 111 districts had elected boards throughout; and 588 had appointed boards throughout.

[2]While the actual number of districts is sometimes listed at over 800, depending on the source, my final sample is 739 because I restrict the analysis to cases that meet the following criteria: no year missing for political institutions; at least one valid year of property tax revenue data; and at least one valid year of emergency response time data. I describe data collection in the Appendix.

[3]Occasionally, a fire district will span multiple counties. The law states that a district is subject to the referendum regime if a majority of its taxable property lies in that county. To proxy for this, I assigned districts to the county in which a majority of their land area lies. In the Appendix I show that dropping these ambiguous cases has no impact on the results.

Figure 4.1: Increasing democracy in Illinois fire protection districts.



Notes: This figure maps the transitions to greater democratic control in Illinois fire protection districts between 1992 and 2009. The left map shows districts that switched to elected boards in dark gray, districts that remained elected throughout this period in light gray, and districts that remained appointed throughout in white. The right panel shows districts that became subject to tax referendums in dark gray, and those that did not in white.

Theoretical Expectations

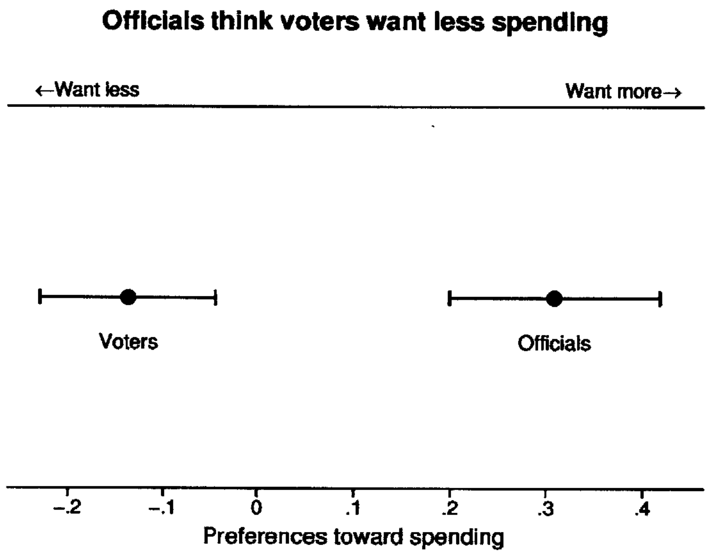
Did policy become more responsive to voter preferences as a result of these reforms? And if so, was government performance harmed?

Previous studies of both elections and referendums strongly predict a positive answer to the first question. Several formal models suggest that elected representatives will be more responsive to the median voter than will appointed representatives (Besley and Coate 2003; Maskin and Tirole 2004; Alesina and Tabellini 2007). These models are supported by a series of empirical studies that tend to find policy differences between elected and appointed officials (e.g., Besley and Coate 2003; Partridge and Sass 2011; Whalley 2013). Likewise, the "setter" model (Romer and Rosenthal 1978; Gerber 1996) implies that referendums give voters veto power over the decisions of representatives, constraining them to propose policies they know that voters will accept. Several studies have also found evidence consistent with this prediction (Gerber 1996; Matsusaka 2010).

Elite and voter opinion in Illinois agreed with the scholarly consensus that both institutions would improve responsiveness to voter opinion. Supporters of the PTELL referendum did not expressly call for less government services; rather, they accused local governments and school districts of wasteful spending, and argued referendums would both reduce waste and allow voters to hold officials accountable for budget increases (Tessin 2009, 64-66). Similar sentiments were expressed by proponents of elected boards. In 2011, residents of the Cerro Gordo fire district in Macon and Piatt counties set up a web site calling on citizens to "Increase accountability regarding the use of your tax dollars" by supporting the switch to elections (ElectOurFireTrustees.com 2011). Residents of the McHenry Township district, in McHenry County, similarly posted a site promoting elections as a method of ensuring "fiscal responsibility" and "accountability for your tax dollars" (Elect McHenry Fire Trustees 2014). An editorial in a Northern Illinois newspaper argued that all fire districts should be elected, so that citizens may have "the final say over the operations of local governmental agencies" (Ledger-Sentinel Editorial Board 2013).

Local officials in these districts, and in the counties that oversee them in some cases,

Figure 4.2: Local officials believe voters want lower spending.



Notes: This figure shows responses to questions about whether officials believe fire protection spending in their district should be increased, decreased, or kept the same; as well as how officials believe voters in their district feel about this issue. Responses are on a three-point scale (decrease = -1, no change = 0, increase = 1). Points are means with horizontal lines spanning 95% confidence intervals. N = 110.

appear to recognize the extent of voter unrest. In an original survey sent to the executive officers of all county boards and fire districts in the state, I asked officials whether they believed spending on fire protection in their district should be increased, decreased, or kept the same. I also whether they believed voters wanted fire spending increased, decreased, or kept the same. This allows me to assess the level of disagreement between officials and voters (as perceived by the officials themselves).

I show responses to these questions, from 110 officials, in Figure 4.2. While officials themselves believe that fire spending should be increased – the average response is at about 0.3 on the three-point scale – they perceive voters as more in favor of spending cuts – the average response here is at about -0.15.[4] Left to their own devices, officials

[4] I provide more details of the survey in the Appendix. I also show that this pattern does

would be making policy decisions that they believe their own constituents would oppose. This supports the expectation that elections and referendums could potentially increase accountability by imposing constraints on officials.

However, preference formation by itself does not imply accountability. Once preferences are formed, voters must then observe policy decisions, attribute these decisions to officials, and then form evaluations of these officials to use in casting their vote (Healy and Malhotra 2013). That voters would be able to accomplish this task appears highly unlikely in this case, given the extremely low salience of special district elections (Berry 2009) that are probably even less well-known than school boards (for which there is no effect of elections; see Chapter 2).

Referendums, in contrast, are a much easier accountability mechanism than elections. In the latter case, voters must form policy preferences; observe policy decisions; attribute these decisions to an official; judge whether this official will represent their policy views in the future; and then cast a ballot for that official (Healy and Malhotra 2013). In the former case, voters merely form policy preferences, and then cast a ballot for that preference. The causal chain of accountability is simplified greatly when voters are given referendum power, which suggests that while elected boards may have no effect on policy, referendums will.

However, even a preference backed up with effective sanctioning may not be sufficient for competent voter behavior. The second question that opened this section is whether government performance was harmed by expanding voter control. The answer to this question depends on whether voters were correct in their assessment that local officials were wasting tax dollars on needless expenses. While supporters of the PTELL in Illinois appeared confident in this assertion, the literature on public opinion suggests that voters often overestimate the extent of government waste, and misjudge the amount of spending

---

not vary by the type of official surveyed.

needed to fund a given service (Sears and Citrin 1982; Tessin 2009). If a similar bias was present in this case, this would imply that the increased responsiveness to voter opinion comes with a significant cost to government performance.

Effects of Voter Control on Policy

To test whether these institutions affected policy, I conduct a series of "difference-in-differences" comparisons. First, for districts that switched in a given year, I compare outcomes before and after the switch occurred (first difference): Next, I compare how outcomes changed before and after the first group of districts switched, but only among districts that did not switch (second difference) Finally, I subtract the second difference from the first. Because I have many different cohorts of switches, I use a regression specification that produces an average difference-in-difference,

$$y_{jt} = \delta * reform_{jt} + district_j + year_t + u_{jt}$$

where y_{jt} represents the outcome, $reform_{jt}$ takes a value of 1 if a district j is subject to the reform (either elections or referendums) in year t, and the next two terms are district and year fixed effects.[5]

Because the intent of the law was expressly to limit property tax growth rates, I use the annual percentage change in property tax revenue my first outcome. I show the estimated effects of the reforms on revenue growth in Table 4.1. As shown in the header, the av-

[5]The specification assumes that the error term u_{jt} is mean-zero. As is well known, this design removes any sources of confounding that result from fixed characteristics of districts, as well as common trends. Any remaining sources of confounding must come from within-district, across-time variation. I use robustness checks to account for these possibilities later in the paper.

Table 4.1: The effect of increasing democracy on property tax revenue.

	Revenue growth in percentage points (Average = 4.86)		
Elected board	1.84		2.05
	(1.38)		(1.39)
Tax referendums		-1.57**	-1.53**
		(0.51)	(0.54)
Elected X referendums			-0.33
			(1.11)

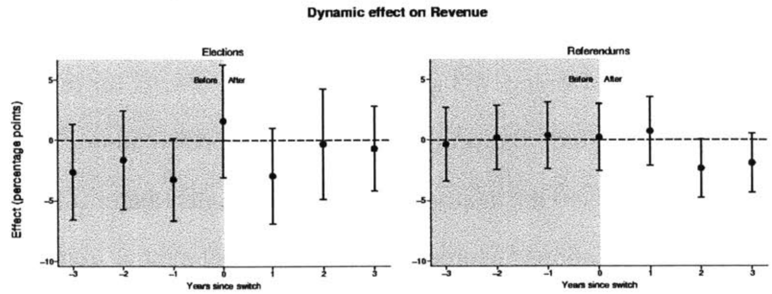
Notes: Time period is 1995-2009, number of districts is 732, and total sample size is 9,780. Standard errors in parentheses, clustered by district. * $p<0.05$, ** $p<0.01$, *** $p<0.001$

erage growth rate over this period was 4.86%.[6] The first column shows that this rate is essentially unchanged when a district shifts from appointed to elected trustees. The point estimate of 1.84 suggests a large increase in growth rates, but the large standard error of 1.38 means that we can not reject the hypothesis of no effect.

The second column shows the effect for tax referendums. In contrast to the null effect for elections, referendums cause revenue growth to decline by 1.57 percentages points, an effect that is precisely estimated and significantly different from zero (standard error = 0.51).

[6] With no pre-processing of the data, the average growth rate is 960%, the 10th percentile is -100%, and the maximum is 11.9 million. To remove these implausible values, which are likely a result of data entry errors on the part of the state Department of Revenue, I trim the outcome to lie between -50% and 50%. I show in the Appendix that the results are robust to other ways of treating these outlying observations.

Figure 4.3: The dynamic effects of voter control on property tax revenue.



Notes: Thick lines connect point estimates; thin lines represent 95% confidence intervals.

Finally, in the third column I test for an interactive effect between the two reforms. The coefficient on the interaction is both substantively small and very noisy (estimate = -0.33, standard error = 1.11), implying that elected boards do not become more or less responsive when they are also subject to tax referendums.

While these estimates give the average difference between the pre- and post-treatment period, they might mask interesting variation in effects across time. To be unbiased estimates, they also rely on the assumption that switching districts were not trending in a different direction from non-switching districts prior to the reform. To explore these potentially dynamic effects, I next estimate regressions of the form,

$$\begin{aligned} y_{jt} &= \sum_{\tau=-3}^{2} \delta_\tau \mathbf{1}_{\{switchyear_j - year_t = \tau\}} + \delta_3 \mathbf{1}_{\{switchyear_j - year_t \geq 3\}} \\ &\quad + district_j + year_t + u_{jt} \end{aligned}$$

Thus each δ represents the difference between treated and control districts by year; the final δ represents the long-term difference. I then plot estimates of δ against the number of years before and after the switch, in Figure 4.3.

The top panel in Figure 4.3 shows the dynamic effect of elected boards. As in the regression results, there is a slight uptick in revenue growth in the post-treatment period; however, the graph shows that this uptick is concentrated in the immediate year of the switch, and does not persist thereafter. As before, however, we never reject the hypothesis that any of the dynamic effects are actually zero.

The bottom panel of Figure 4.3 shows the dynamic effects for referendums. Here we see a clear negative effect that occurs two years after referendums are implemented, an effect that then persists to the next year and beyond. Again the estimate is more precisely estimated than for elections. Moreover, there is no differential trend in outcomes in the pre-treatment period. Up until two years after the treatment, districts with and without referendums were identical in terms of revenue growth.

Thus, contrary to the predictions of both the scholarly literature and public opinion in Illinois, only referendums had an effect on policy. This is consistent with a conception of accountability as a multistep process, which referendums help to greatly simplify. A remaining question is whether voters were able to complete a final test of competence: choosing policies that lead to good performance.

Effects of Voter Control on Performance

To test whether the greater responsiveness to voter opinion affects performance, I conduct the same difference-in-differences comparison as in the preceding section. Instead of property tax revenues, I now use the average emergency response time. The data are originally measured in minutes, but I convert the outcome into seconds to ease interpretation. I show the results in Table 4.2.

As the header to Table 4.2 shows, the average response time over this period was seven minutes.[7] The first column shows the estimated effect of elections. When a district adopts elected boards, this is essentially unchanged: the point estimate suggests a difference of

[7]Tessin (2009) reports slightly larger effect estimates, but also reports the average response time at 10-11 minutes. Our estimate of the average response time is more in line with estimates reported by the state fire marshall (Illinois Office of the State Fire Marshal

Table 4.2: The effect of increasing democracy on emergency response times.

	Response times in seconds (Average = 7 minutes)		
Elected board	-9.96		0.64
	(22.96)		(27.56)
Tax referendums		38.62**	41.43**
		(12.69)	(13.54)
Elected X referendums			-16.96
			(26.36)

Notes: Time period is 1992-2009, the number of districts is 706, and the total sample size is 5,464. Standard errors in parentheses, clustered by district. * $p<0.05$, ** $p<0.01$, *** $p<0.001$

about 10 seconds, but the standard error is about 23 seconds. This makes sense, given that we saw no effect on revenue in the previous section.

The second column shows the estimated effect of tax referendums, which in the previous section were shown to result in large revenue decreases. Here, the point estimate implies an increase in response times of 39 seconds; the effect is precisely estimated, with a standard error of 13 seconds. Finally, the third column shows there is no interaction between elections and referendums (estimate = -17 seconds, standard error = 26 seconds).

Thus, although referendums help voters achieve their policy preference for lower revenues, this responsiveness comes with a cost. Voters appear to have overestimated the amount of wasteful spending in these districts, and misjudged the amount of revenue needed to maintain good performance. As a result, voters in districts with greater democ-

2014).

racy must wait nearly a minute longer for emergency responders to arrive.

Robustness Checks

I have shown that tax referendums have large effects on revenues and response times, whereas elections do not. As mentioned, the key strength of the design is that the institutional variation is within districts and across time. This means that any difference in outcomes can be plausibly attributed to the treatment, as opposed to unobserved differences between treated and untreated districts.

This design relies on the assumption of "parallel trends" – that the districts that switch provide a good counterfactual for those that do not. The evidence presented in Figure 4.3 strongly supports this assumption, because the treated and control districts have very similar outcomes prior to the switch. The assumption is that these similar trends would persist in the absence of the treatment. However, we may also wish to know how these districts differed on factors aside from the outcome – and if so, adjust for those differences. For example, suppose districts that switched have smaller populations than districts that did not, and that the referendum only has an effect in small districts.

While such concerns are relatively minor compared to heterogeneity bias and divergent pre-treatment trends, it would be ideal to explore them by bringing covariates into the analysis. While data on district demographics and other covariates are limited, given the obscure nature of fire districts, I address this concern as much as possible in the Appendix. I explore covariate balance between treated and untreated districts on population, the percent over aged 65, the percent home-owner, property values, the number of fire incidents, the percent of paid firefighters, and the number of firefighters. Density plots show that the covariate distributions for treated and control districts are very similar. I also adjust for my difference-in-differences estimates for these covariates; while the sample size shrinks considerably, the substantive results are unchanged.

A separate concern is how the standard errors are calculated. While elections were applied at the level of districts, referendums were applied at the level of counties. The clustered standard errors reported in Tables 4.1 and 4.2 assume both treatments vary at the level of districts. In the Appendix, I show that clustering the standard errors at the county level leaves statistical significance basically unchanged (from the 1% to the 5% level for tax revenues, and no change for response times).

Another concern is that the analyses presented earlier mix together the urban Chicago area counties with the more rural downstate counties (Tessin 2009). As I show in the Appendix, dropping the Chicago-area counties and re-running the analysis has no substantive impact on the results.

Finally, one concern with comparing elections and referendums is statistical power: there are only about 40 districts that adopted elections over the study period, whereas about 300 districts adopted referendums. Thus the true effect of elections may be as large as that of referendums, but we simply do not have enough data to detect this effect. One response to this concern is that the actual level of variation might be more comparable than at first glance, given that the 300 referendum districts are clustered within about 40 counties. Thus, in principle a district-level treatment should be easier to detect than a county-level treatment.

However, to more systematically address this concern, I conduct a series of simulations that ask how often we would detect an effect of referendums if we only had 30 (to be on the conservative side) treated units. For each of 500 iterations, I first randomly select 30 of the districts that adopted referendums; I then drop all of the other districts that switched, and conduct the referendum analysis presented in Table 4.1, saving the point estimate. Finally, I examine the density of point estimates across the 500 simulations. I show this density plot in the Appendix. The figure shows that even if we only had 30 referendum districts, we would achieve the observed point estimate for elections exactly 0 out of 500 times.

Conclusion

When will increases in democratic control impact policy? Using a unique case study featuring two separate increases in democratic control, I have shown that not all institutions are created equal. While granting voters the power to select their representatives via elections has no impact on policy, granting voters veto power via referendums does. Because both institutions are implemented in the same state – even the same districts in some cases – they grant power to the same population of voters. Thus the design allows me to hold voter characteristics constant, and the difference in effects can be attributed to features of the institutions themselves.

If the difference in effects is not due to voter characteristics, what is it about the institutions that explains the opposing results? As discussed earlier, a candidate explanation is that referendums drastically shorten the causal chain of accountability that voters must process in order to get what they want. Rather than learn about the connections between their policy preferences, official actions, and vote choice, they can register their preference in a single step. The implied model of accountability here, as well as the null result for the effect of elections, may help explain why the "setter" model (Romer and Rosenthal 1978; Gerber 1996, 11) assumes elected legislators are entirely unconstrained, in terms of policy choices, in the absence of direct democracy institutions.

References

Alesina, Alberto, and Guido Tabellini. "Bureaucrats or politicians? Part I: a single policy task." *The American Economic Review* 97(1): 169-179.

Berry, Christopher R. 2009. *Imperfect Union: Representation and taxation in multilevel governments.* Cambridge University Press.

Besley, Timothy, and Stephen Coate. 2003. "Elected versus appointed regulators: Theory and evidence." *Journal of the European Economic Association* 1(5): 1176-1206.

ElectOurFireTrustees.com. 2011. "Home." Accessed March 11, 2011 via http://www.electourfiretrustees.com/.

Elect McHenry Fire Trustees. 2014. "Elect McHenry Fire Trustees." Accessed January 14, 2014 via http://www.electmtfpdtrustees.com/.

Gerber, Elisabeth. 1996. "Legislative Response to the Threat of Popular Initiatives." *American Journal of Political Science* 40(1), 99-128.

Healy, Andrew, and Neil Malhotra. 2013. "Retrospective Voting Reconsidered." *Annual Review of Political Science* 16: 285-306.

Illinois Department of Revenue. 2012. "An Overview of the Property Tax Extension Limitation Law by Referendum." Accessed June 7, 2014 via http://tax.illinois.gov/Publications/PIOs/PIO-62.pdf.

Illinois Office of the State Fire Marshall. 2014. "Illinois Fire Incident Reporting: Statewide Reporting Overview for 2011 - 2013." Accessed June 7, 2014 via http://www.sfm.illinois.gov/reports/NFIRSDashboard03-06-14.pdf.

Ledger-Sentinel Editorial Board. 2013. "Let voters elect fire district boards." *Ledger-Sentinel* March 28. Accessed July 19, 2013 via http://www.ledgersentinel.com/article.asp?a=11071.

Maskin, Eric, and Jean Tirole. 2004. "The politician and the judge: Accountability in government." *The American Economic Review* 94(4): 1034-1054.

Matsusaka, John G. 2010. "Popular control of public policy: A quantitative approach." *Quarterly Journal of Political Science* 5(2): 133-167.

Partridge, Mark, and Tim R. Sass. 2011. "The productivity of elected and appointed officials: the case of school superintendents." *Public Choice* 149(1): 133-149.

Rehfuss and Tobias. 1977. "Special districts: The little governments providing specialized services." *Illinois Issues* September. Accessed December 4, 2011 via http://www.lib.niu.edu/1977/ii770922.html.

Romer, Thomas, and Howard Rosenthal. 1978. "Political Resources Allocation, Controlled Agendas, and the Status Quo." *Public Choice* 33 (4): 27-43.

Sears, David O., and Jack Citrin. 1982. *Tax Revolt: Something for Nothing in California.* Cambridge: Harvard University Press.

Tessin, Jeff. 2009. *Representation and Government Performance.* PhD Thesis, Department of Politics, Princeton University.

University of Illinois Springfield. 1979. "Executive Report." *Illinois Issues* March. Accessed December 4, 2011 via http://www.lib.niu.edu/1979/ii790329.html.

Whalley, Alexander. 2013. "Elected versus Appointed Policy Makers: Evidence from City Treasurers." *Journal of Law and Economics* 56(1): 39-81.

Appendix

Data Collection

To measure districts subject to tax referendums, I used a map of PTELL status by year published online by the Illinois Department of Revenue. To measure whether districts elect or appoint their trustees in a given year, I began with the U.S. Census of Governments, which measured this in 1992. To measure transitions from 1992 onward, I used switching referendum outcomes published to the Illinois State Board of Elections web site.

I obtained property tax revenue from the Illinois Comptroller's Office, which has this data in levels back to 1994. I obtained response times data from the National Fire Incident Reporting System (NFIRS), a national database to which fire departments voluntarily report incident-level data. I obtained this data via a request to the federal Department of Homeland Security, which currently maintains the database. I calculated response times as the absolute value of the difference in minutes between the arrival time and the call time. I then dropped any incidents with response times greater than 25 minutes, treating these as data entry mistakes. Finally, I aggregated the incident-level data to district-year averages.

I obtained district demographic data via the U.S. Census. Unlike counties or towns, these data are not pre-aggregated to the level of districts. I therefore aggregated block-level demographic data to district boundaries using GIS software. I did so for 1990, 2000, and 2010 (the three years for which block-level data are available) and linearly interpolated between Census years.

Data on the total property values in each district comes from the Comptroller's Office. The number of fire incidents, the percent paid firefighters, and the number of firefighters are all included as auxiliary variables in the NFIRS data set.

Survey of Local Officials

The Illinois State Comptroller provided me with a list of official names and e-mail addresses for each government unit in the state. For each unit, the data contain two officials, labeled as the Chief Executive (typically the chairman of the board) and Chief Financial Officer (typically the treasurer). After dropping cases with no e-mail address and where the same e-mail address was listed for both the CEO and CFO, this left 1,169 officials (168 county officials and 1,001 fire district officials).

I e-mailed the survey to all 1,169 officials in March of 2014. I first sent officials a notification that they would soon be receiving the survey. One week later, I sent the first invitation, followed by reminders one and two weeks later. One hundred and twelve officials from 56 counties consented to and completed the survey.

A map of the number of responses by county is shown in Figure 4.A1. Table 4.A1 gives the distribution of responses by the type of official. Figure 4.A2 shows that the pattern of perceived disagreement observed among the full sample in Figure 4.2 does not vary by office.

Figure 4.A1: Number of responses to survey by county.

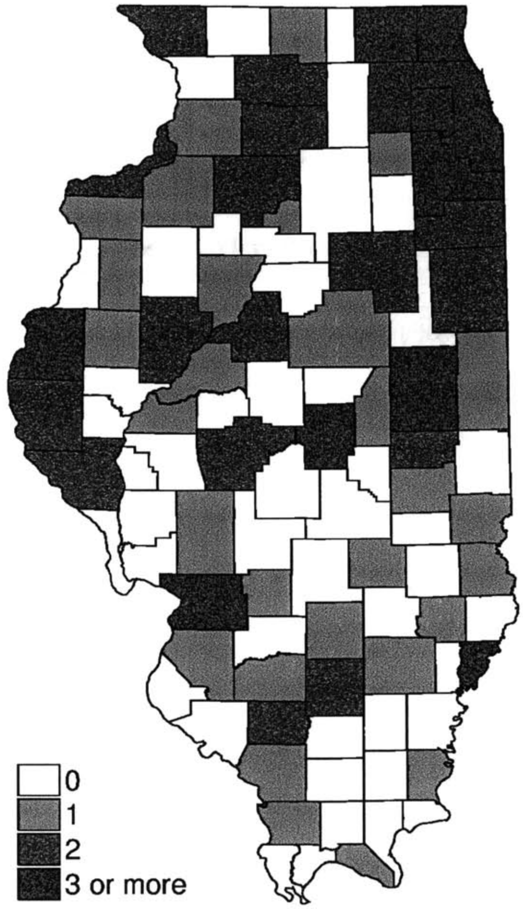
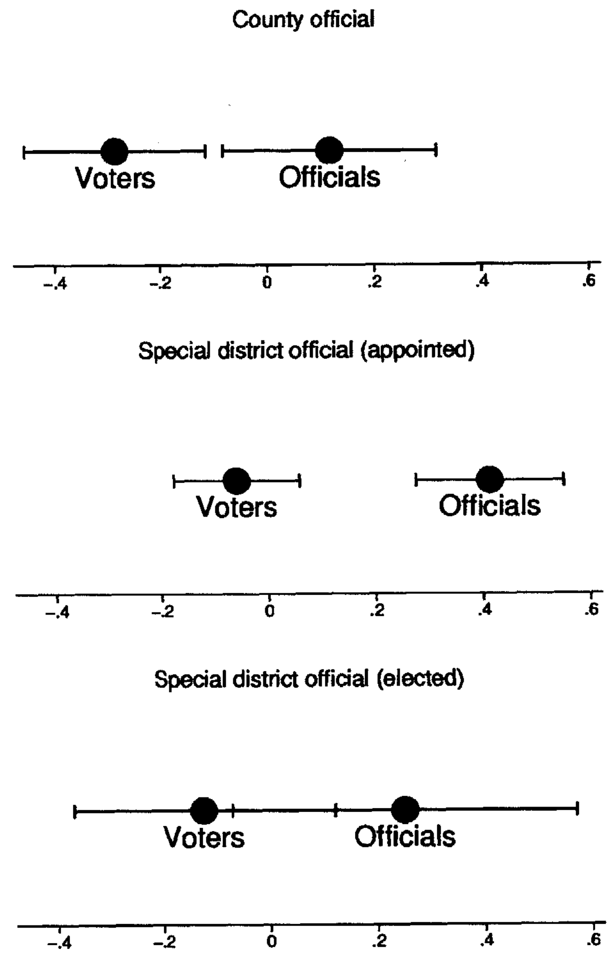


Table 4.A1: Number of responses to survey by office.

	Number
County official	36
Fire district official (appointed)	68
Fire district official (elected)	8
Total	112

Figure 4.A2: Perceived disagreement between officials and voters by office.



Robustness Checks for Difference-in-Differences Results

Dropping Overlapping Districts

Tables 4.A2 and 4.A3 repeat the analysis reported in the main text, dropping any districts that overlap multiple counties. As mentioned, a district is subject to a county's adoption of the referendum regime if a majority of its taxable property lies within that county. I used the county with a majority of a district's geographic area as a proxy, but this may induce measurement error. These two tables show that the results are unchanged if I drop these ambiguous cases.

Table 4.A2: Replication of Table 4.1, dropping districts overlapping multiple counties.

	Revenue growth in percentage points (Average = 4.88)		
Elected board	1.42		1.30
	(1.79)		(1.54)
Tax referendums		-1.64**	-1.66*
		(0.63)	(0.68)
Elected X referendums			0.14
			(1.16)

Notes: Sample period is 1995-2009 for all specifications. The number of districts is 478, and the number of observations is 6,352.

Table 4.A3: Replication of Table 4.2, dropping districts overlapping multiple counties.

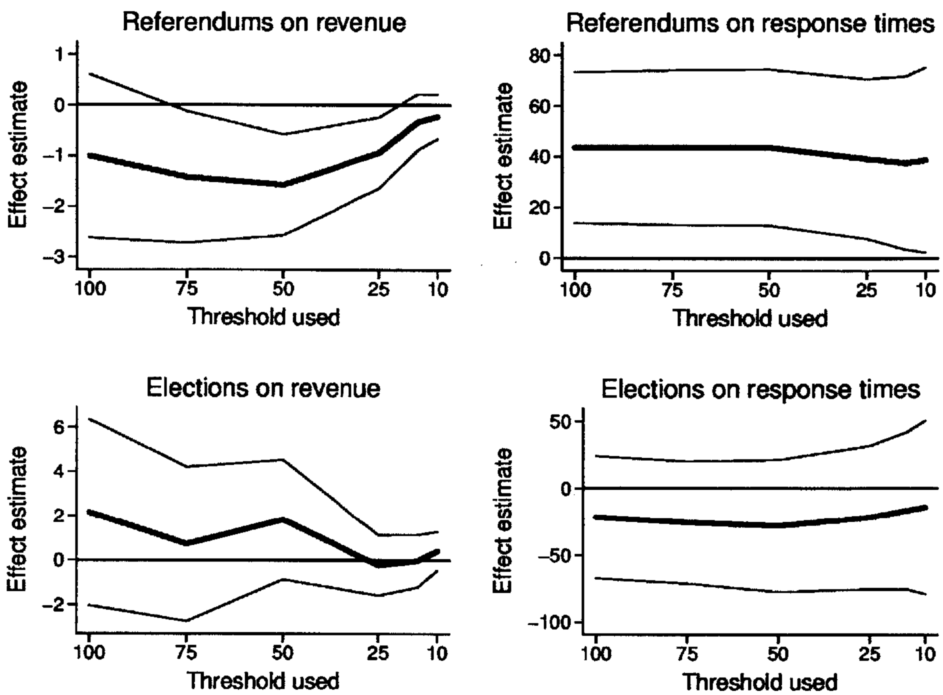
	Response times in seconds (Average = 7 minutes)		
Tax referendums	33.49*		33.67*
	(15.56)		(16.34)
Elected board		-8.24	-5.34
		(27.35)	(35.90)
Referendums X elected			-1.44
			(34.55)

Notes: Sample period is 1995-2009 for all specifications. The number of districts is 461, and the number of observations is 3,677.

Alternative Treatments of Outliers

In the analysis reported in the text, I trim the revenue outcome by excluding observations with an absolute percentage change of more than 50% (i.e., districts whose revenue is reported to be cut or to be increased by half). In Figure 4.A3, I show how the estimates vary when I change this threshold. Rows represent institutions (referendums or elections) and columns represent outcomes (revenue growth or response times). The top left panel shows that the effect of referendums on revenue is always negative, but the precision depends somewhat on how we exclude extreme observations. The top right panel, however, shows that the effect on response times is invariant to these choices.

Figure 4.A3: Robustness of estimate to alternate deletions of extreme observations.



Adjusting for Covariates

Due to limited availability, the analysis in the main text does not report estimates with covariates. In Figure 4A.4, I present balance on observable characteristics between districts that did and did not adopt each treatment in the study period. A visual inspection reveals little difference between either groups of districts for either treatment.

In Table 4.A4, I replicate the estimated effects on revenue growth adjusting for these covariates in a stepwise fashion. The magnitudes of the point estimates are virtually unchanged across the specifications. Statistical significance becomes an issue only when all covariates are included; however, the sample size has decreased from 9,475 to 2,776 in the final estimates, which are probably underpowered.

In Table 4.A5, I replicate the estimated effects on response times. The approximate magnitude of the effects, as well as their statistical significance, does not vary depending on which set of covariates is included. If anything, the estimated effects become larger.

Figure 4.A4: Balance on observable characteristics.

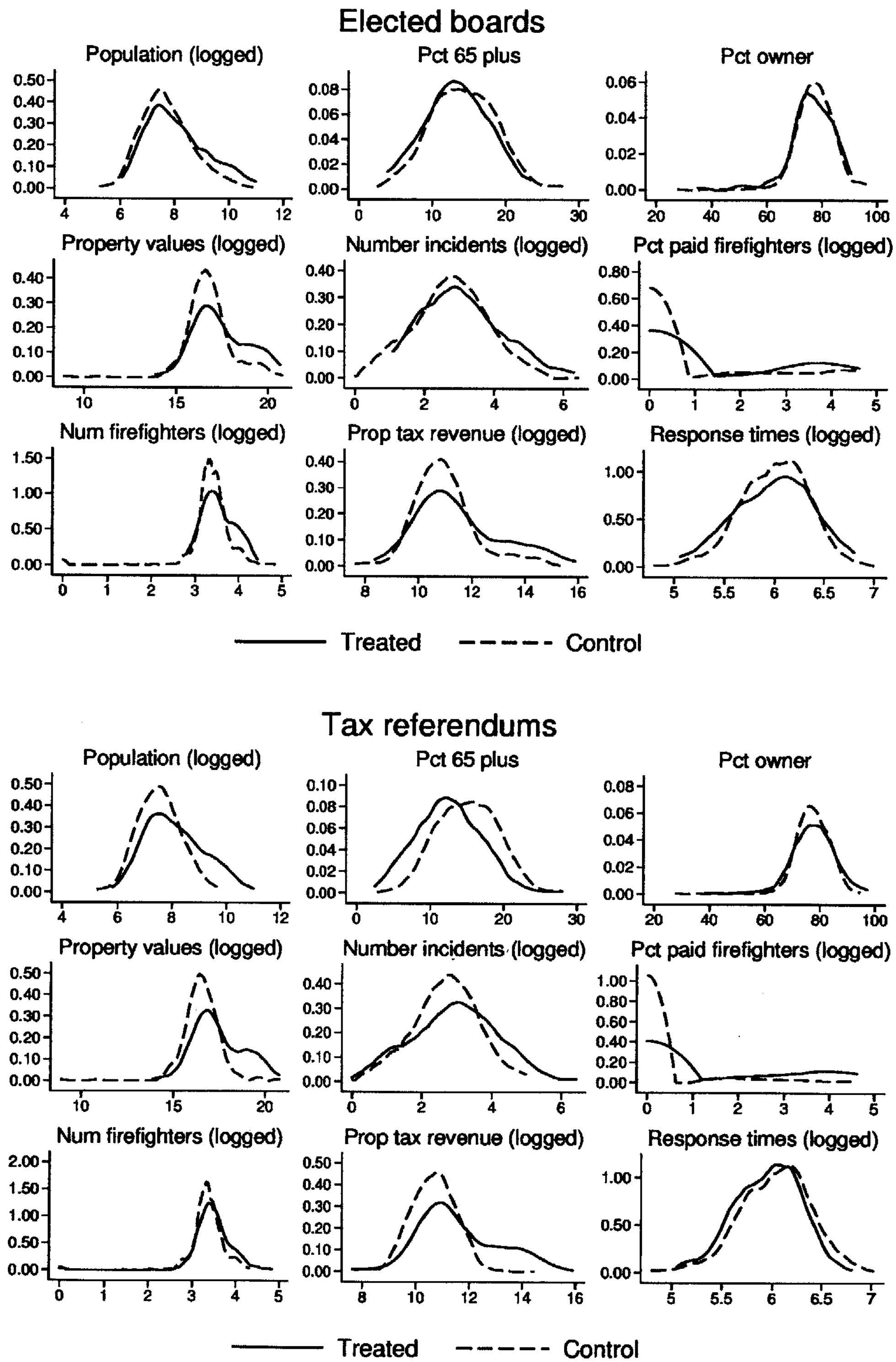


Table 4.A4: Replication of Table 4.1, adjusting for time-varying covariates.

	Revenue growth in percentage points								
Elected board	1.90		1.92	1.99		1.84	5.06*		4.60
	(1.40)		(1.39)	(1.42)		(1.44)	(2.35)		(2.87)
Tax referendums		-1.52**	-1.51**		-1.50**	-1.54**		-1.63	-1.71
		(0.51)	(0.54)		(0.52)	(0.55)		(1.11)	(1.17)
Elected X referendums			-0.04			0.27			0.70
			(1.14)			(1.17)			(2.56)
Census covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
Property values				Y	Y	Y	Y	Y	Y
Firefighter covariates							Y	Y	Y
Number of districts	708	708	708	708	708	708	634	634	634
Sample size	9,475	9,475	9,475	8,992	8,992	8,992	2,776	2,776	2,776

Notes: Sample period is 1995-2009 for all specifications.

Table 4.A5: Replication of Table 4.2, adjusting for time-varying covariates.

	Response times in seconds								
Elected board	-22.06		-42.63	-21.71		-37.15	-9.27		-31.45
	(22.43)		(29.19)	(25.53)		(31.35)	(30.31)		(36.48)
Tax referendums		45.09**	41.12**		50.34**	47.56**		57.98**	54.30**
		(14.46)	(14.88)		(15.88)	(16.57)		(18.90)	(20.07)
Elected X referendums			31.92			23.02			31.51
			(33.04)			(34.23)			(38.64)
Census covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
Property values				Y	Y	Y	Y	Y	Y
Firefighter covariates							Y	Y	Y
Number of districts	664	664	664	662	662	662	653	653	653
Sample size	4,470	4,470	4,470	4,088	4,088	4,088	3,333	3,333	3,333

Notes: Sample period is 1994-2009 for all specifications.

Clustering Standard Errors by County

Because the referendum regime is applied at the level of counties, a case can be made that standard errors should be clustered at the level of counties. The estimates in the main text cluster at the district level. In Tables 4.A7 and 4.A8, I show that clustering at the level of counties has no effect on the results.

Table 4.A6: Replication of Table 4.1, clustering at the county level.

	Revenue growth in percentage points		
Elected board	1.84		2.05
	(1.35)		(1.21)
Tax referendums		-1.57*	-1.53*
		(0.68)	(0.70)
Elected X referendums			-0.33
			(1.13)

Notes: For all specifications, the time period is 1995-2009, the number of counties is 96, and the sample size is 9,780. Standard errors in parentheses, clustered by county. * p<0.05, ** p<0.01, *** p<0.001

Table 4.A7: Replication of Table 4.2, clustering at the county level.

	Response times in seconds		
Elected board	-9.96		0.64
	(24.04)		(29.92)
Tax referendums		38.62**	41.43**
		(13.69)	(15.15)
Elected X referendums			-16.96
			(30.06)

Notes: For all specifications, the time period is 1992-2009, the number of counties is 95, and the sample size is 5,464 Standard errors in parentheses, clustered by county. * p<0.05, ** p<0.01, *** p<0.001

Dropping Chicago-area Counties

The estimates in the main text lump together potentially heterogeneous parts of the state, which may differ in important ways. In particular, the more urban Chicago-area counties in the northeast may differ from the more rural, downstate districts. Among other differences, the Chicago-area counties were immediately affected by the PTELL legislation in 1991, whereas the downstate counties adopted the referendum regime via referendums. To address these concerns, I re-estimate the effects dropping the counties of Cook, DuPage, Kane, Lake, McHenry, and Will. I show these estimates in Tables 4.A8 and 4.A9. The effect on revenue is reduced to -0.95, with a standard error of 0.51; the effect on revenue is now 32.99 seconds (standard error = 14 seconds). Thus, dropping these counties does not appreciably affect the results.

Table 4.A8: Replication of Table 4.1, dropping Chicago-area counties.

	Revenue growth in percentage points		
Elected board	0.39		1.03
	(1.37)		(1.41)
Tax referendums		-0.95+	-0.77
		(0.51)	(0.54)
Elected X referendums			-1.26
			(1.10)

Notes: For all specifications, the time period is 1995-2009, the number of districts is 621, and the sample size is 8,309. Standard errors in parentheses, clustered by district. + p<0.10 * p<0.05, ** p<0.01, *** p<0.001

Table 4.A9: Replication of Table 4.2, dropping Chicago-area counties.

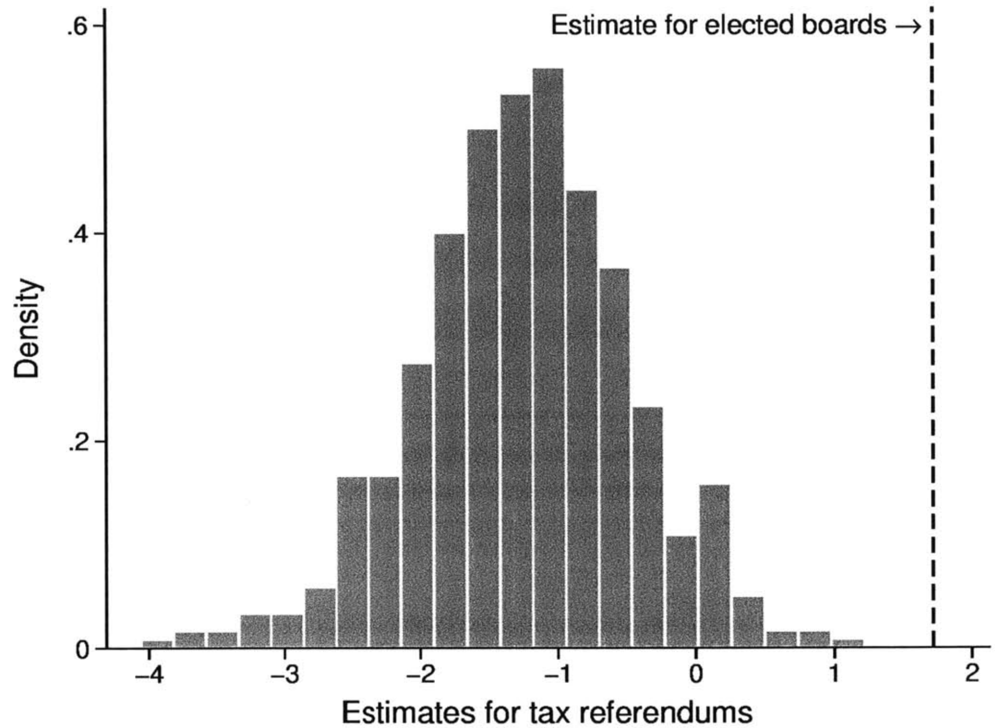
	Response times in seconds		
Elected board	15.12		12.18
	(30.37)		(32.69)
Tax referendums		32.99*	32.64*
		(14.48)	(15.40)
Elected X referendums			1.40
			(33.13)

Notes: For all specifications, the time period is 1992-2009, the number of districts is 596, and the sample size is 4,270. Standard errors in parentheses, clustered by district. * p<0.05, ** p<0.01, *** p<0.001

Statistical Power

One explanation for the null results for elections and the significant result for referendums is statistical power. About 300 districts were affected by the referendum treatment, whereas only about 40 were affected by the elections treatment. To test whether statistical power could drive this difference, I perform the following simulation. I first randomly select 30 (to be conservative) of the districts that adopted referendums, and drop all the other switching districts. I then re-estimate the effect on revenue from the main text. I repeat this exercise for 500 iterations, and I plot the density of the resulting estimates in Figure 4.A5. Even if we had only 30 districts that adopted referendums, we would observe a negative effect the vast majority of the time; we would observe an effect equal to the estimate for elections precisely zero times.

Figure 4.A5: Is the null effect of elections a result of statistical power?



Chapter 5

Conclusion

In this final chapter, I briefly review some outstanding questions raised by the results of the three empirical studies. First, I consider possible explanations for the divergence in effects within and across cases. Second, I discuss the implications of my findings for debates about citizen competence. Third, I ask whether the results are driven by elites capturing the electoral process.

When Will Voter Control Matter?

An exhaustive theoretical treatment of voter control would offer predictions about the conditions under which that control will matter. To my knowledge, no theorist, either formally or informally, has offered such a treatment. As discussed in the fire district chapter, existing formal theoretic treatments simply predict that voter control – whether operationalized as direct elections or referendums – will move policy toward the median voter's preferred outcome. Thus, existing treatments have not even considered that elections and referendums may diverge, let alone offered predictions about when either of these institutions by themselves will have divergent results.

My goal in this dissertation was to offer empirical tests of these baseline predictions, as opposed to developing a full-fledged model of voter control. Nonetheless, by looking back on the results, we can perhaps gain some insights into the conditions under which voter control will matter. Most notably, the sole case in which direct elections mattered concerned the property tax, arguably the most salient policy issue at any level of government. In contrast, when elections did not matter, the issues were much less salient, concerning school and fire budgets.

Why would salience make the difference? One reason is that salience helps voters form an opinion about policy. Voter control institutions are predicted to resolve divergence between officials' preferences and the median voter's preference. If the median voter does not have a preference, then voter control can not plausibly make a difference. Thus, salience could be a key determinant of whether such institutions will matter.

On the other hand, this would not explain why referendums matter in the fire district case. Here, direct elections did not matter, but referendums did. Salience can not explain this difference, as the salience of fire protection is the same for both direct elections and referendums.

An alternative explanation is that the effect of voter control depends on the salience of the *process* as well as the policy. To see this, assume that voters have clear preferences in all of my cases. They may wish to use direct elections to get what they want, but it is no easy task. As Healy and Malhotra (2013) argue, the use of elections to enforce accountability involves monitoring officials' actions, attributing these actions to officials, and using these attributions to make voting decisions. This can be easy or difficult, depending on the context. In the case of assessors, monitoring and attribution are simple: the assessor interacts one-on-one with the voter, and it is clear who is making the decision to reassess. In the case of fire and school boards, monitoring and attribution are harder: the voter needs to make an effort to learn about budgetary decisions, and may have trouble understanding just who is responsible for these decisions.

Now suppose that voters, who we have assumed have preferences, are granted referendum power over budget decisions. In contrast to elections, the process is much more accessible. Rather than monitor officials and attribute their decisions correctly, voters need only assert their preference. Referendums, then, give voters more accountability not via issue unbundling, but by simplifying the cognitive processing required in making voting decisions.

Implications for Citizen Competence Debates

As noted at the outset of this dissertation, whether voter control matters for policy is a core assumption in debates over citizen competence. Critics of popular democracy argue that citizens are incompetent, and so giving them more power will lead to harmful public policies. Defenders argue that citizens do quite well with the information they have, and so citizen control ought to be increased.

In this dissertation, I have shown that voter control can make a difference for policy. What this means for debates about citizen competence turns, crucially, on whether we believe these policy effects are good or bad for public policy. This is a thorny problem in the study of direct democracy and competence: if citizens believe that a certain policy is best, and they achieve this policy via institutions, who are we to argue that this is a bad thing? Because of this issue, it is unlikely that my results will do much in resolving the disagreement between defenders and critics of democracy.

Yet while these results may not resolve this debate, they do offer some suggestive evidence about the nature of competence, which I define as the ability of citizens to use democratic institutions to achieve the policies and performance they desire. Note that competence, as I have defined it, involves policy and performance. Voters only use institutions to achieve their preferred policies in some of my cases: in the assessor case, when they use elections to induce less frequent reassessments; and in the fire case, when they use referendums to induce lower taxes. Yet, even when voters achieve their desired policy, government performance suffers: in the tax case, less frequent assessments lead to more inequity; in the fire case, lower taxes increase response times. Thus, voters only get their preferred policy some of the time, yet it is precisely these cases in which performance suffers.

Whether this implies voters are incompetent requires some additional assumptions. Why do voters support policies that lead to bad performance? Do they misunderstand the connection between policy and performance? Do they place a greater value on policy than on performance? Do politicians (or other elites) manipulate voters into focusing on policy instead of performance? Or might voters simply have different conceptions of performance than the researcher?

Scholars of competence have not fully grappled with these issues, focusing instead on the more fundamental question of whether voters form preferences and whether they use elections to achieve these preferences. While addressing these basic questions is important, my results reveal a need for empirical engagement with these more nuanced, more difficult questions.

Popular Control or Elite Capture?

One counter that defenders of democracy may offer is that my results are not actually about voter control, but about the perversion of democracy by elites. That is, my explanatory variable does not measure voter control, but the opportunity for control. Given that those who participate in elections are quite different from those who do not (Verba, Schlozman, and Brady 1995), perhaps my explanatory variable is actually measuring the elite capture of democracy. Elites benefit from more inequality and lower taxes, so it makes sense that I find these outcomes in my cases.

This is a definite possibility that I am unable to rule out. Yet, it does not change the fundamental message of this dissertation: giving voters more power, regardless of who uses it, can affect policy. It may, however, change the interpretation of the results, particularly as regards citizen competence. For my results to be interpreted as evidence of citizen competence, we need to assume that the median voter desires policies that harm their own interests: for example, less frequent reassessments or lower fire district revenue. If the median voter does not desire these policies, but wealthy voters (who would benefit from them) do, then perhaps the issue is not incompetence, but hyper-competence on the part of the economic elite.

Of course, this would also mean that non-wealthy voters are unable to use their numerical majority to achieve policies that benefit them. Is this incompetence, or just a bad deal? To my knowledge, existing studies of voter competence have all treated voters as an aggregate, and so have not considered this issue.

This points to a more fundamental question of why democracies sometimes produce inequitable policies: because the mass of voters is incompetent, or because a tiny minority captures the democratic process? A recent review (Bonica et al. 2013) considers only the latter possibility, yet there is good reason to believe that the former is equally plausible (Bartels 2005). As inequality becomes more of a pressing policy issue in the United States, adjudicating between these competing explanations will become more important. However, it is unlikely that this question will be resolved using national-level data. For the same reasons outlined in the introductory chapter, the best hope for adjudicating between these two empirical claims is likely to be found at the subnational level.

References

Bartels, Larry M. 2005. "Homer gets a tax cut: Inequality and public policy in the American mind." *Perspectives on Politics* 3(1)1: 15-31.

Bonica, Adam, Nolan McCarty, Keith T. Poole, and Howard Rosenthal. 2013. "Why Hasn't Democracy Slowed Rising Inequality?" *The Journal of Economic Perspectives* 27(3): 103-123.

Healy, Andrew, and Neil Malhotra. 2013. "Retrospective Voting Reconsidered." *Annual Review of Political Science* 16: 285-306.

Verba, Sidney, Kay Lehman Schlozman, and Henry E. Brady. 1995. *Voice and equality: Civic voluntarism in American politics*. Cambridge, MA: Harvard University Press.