The Distributional Impact of Greater Responsiveness: Evidence from New York Towns

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Making leaders more responsive to voters is a frequent goal of institutional reforms in democracies. Given that some citizens participate more in politics than others, however, there is a risk that increased responsiveness may conflict with another democratic value: the equality of policy outcomes. This article studies this trade-off using institutional reforms in New York towns, where officials known as assessors are charged with ensuring equitable treatment in property taxation. Over time, hundreds of towns reduced responsiveness by making their assessors appointed instead of elected. The local context thus allows for more precise measurement of who gets what from government, as well as more credible estimates of the effects of institutions. Results show that local policy decisions are biased against low-income residents and that elections serve only to compound this bias.

The responsiveness of public policy to public opinion is a key benchmark of democratic performance. When government is seen as insufficiently attentive to the will of the people, institutional reforms are often proposed to make public sentiment more salient in officials' minds. Yet while undoubtedly an important goal, this responsiveness may also conflict with another democratic value, namely, the equality of policy outcomes. Certain institutional choices may succeed in heightening leaders' incentives, but there is no guarantee the public they hear from will favor equality. Indeed, given well-known biases in who participates in politics, there is a risk that increasing responsiveness may in fact work to exacerbate inequality in policy outcomes.

This article studies this trade-off in the context of direct elections versus appointments. According to several studies, subjecting officials to elections causes them to be more responsive to public opinion than would be the case if they were appointed by other officials. To test whether this responsiveness also leads to more inequality, I compare the behavior of elected and appointed property tax assessors in New York towns, a local-level case that has two key advantages.

First, the assessor is directly responsible for managing the distributional burden of local property taxes. Thus, studying this office gives more direct insight into the link between elite decisions and the equity of policy outcomes than would be possible at the state or national level, where the link between decisions and outcomes is much more obscure. Second, hundreds of towns changed from electing to appointing their assessor since the 1980s, which means that changes in elite decisions can plausibly be attributed to changes in institutions. In contrast, institutions at the state and national levels rarely change over time, making it much harder to separate the effects of institutions from other factors.

Consistent with my claim that responsiveness and equity may conflict, I find that direct elections lead to policies that severely undertax wealthier homes relative to poorer homes. The effects are substantial. At the level of towns, elections cause a 9 percentage point drop in the probability that the assessor updates property values. As I explain below, this failure to update values is the single most important driver of inequality in effective tax rates. I also verify this claim empirically: using property-level data on home sales, I show that elections increase the difference in effective tax rates between the richest and poorest homes by 26 percentage points. Additional tests show that these effects are driven by incentives. According to theories of indirect accountability, appointed

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officials are typically free to ignore voter opinion because of the lower salience of their task. Consistent with this prediction, I find that appointed assessors are just as responsive as elected assessors in towns where property tax salience is highest.

These results have numerous implications for how we evaluate findings of a relationship between public opinion and public policy, as well as potential institutional reforms intended to increase this link. For one, they replicate numerous state and national studies showing policy decisions to be biased against the economically disadvantaged, regardless of the institutional configuration. Yet they also go a step further, demonstrating that reforms intended to increase responsiveness can actually compound this bias. Additionally, these findings contribute to our understanding of how different accountability institutions affect policy outcomes. Practically speaking, they suggest that elections for numerous obscure—yet demonstrably consequential—offices at the state and local levels constitute a barrier to greater equity in public policy.

DIRECT ELECTIONS AND RESPONSIVENESS

A key mechanism by which officials may be made more responsive is the manner in which they are chosen. In contrast to direct elections, making officials appointed places an additional intermediary between voters and the policy maker, weakening the link between voter preferences and public policy. While there is still a chance that voters can exercise indirect accountability—punishing the official they do elect for the actions of the official they do not—this is unlikely for two reasons. First, without an electoral campaign to inform them, voters likely know little about the appointee's performance (Gailmard and Jenkins 2009). Second, even if voters had this information, they might not wish to "waste" their vote if the appointer's job is more salient (Besley and Coate 2003). For instance, voters might be hesitant to judge their governor, in charge of the general ideological direction of the state, on the basis of the performance of an appointed regulator, who is in charge of only one particular policy area.

The result of these information and salience problems is that—except in cases in which these problems do not exist—appointed officials will be less responsive to the public. Consistent with these arguments, directly elected officials are often found to be more responsive to public opinion than appointed officials. At the national level, several studies examine the 17th Amendment to the US Constitution, which changed the method of selecting federal senators from appointment by state legislatures to election by voters (Bernhard and Sala 2006; Crook and Hibbing 1997; Rogers 2012). For instance, Gailmard and Jenkins (2009) find that the relationship between state public opinion and senator roll call voting is strengthened following the passage of the amendment.

Others look for evidence of increased responsiveness at the state level. Besley and Coate (2003) find that elected state utility regulators are associated with lower consumer energy bills, relative to appointed regulators. Examining state courts, Canes-Wrone, Clark, and Kelly (2014) find that elected judges are more likely to rule in favor of capital punishment in states where voters are more in favor of the death penalty. And at the local level, Whalley (2013) finds that California cities switching from elected to appointed treasurers see a 25% decrease in their borrowing costs. This suggests that elected treasurers are more responsive to voters, who may desire costly policies or who may be poorly equipped to judge performance (Whalley 2013, 43). Finally, at an even more micro level, Grossman (2014) finds that Ugandan farm association members perceive elected association leaders as more responsive than appointed leaders.

WHY MORE RESPONSIVENESS MAY MEAN LESS EQUALITY

While numerous studies have found evidence of greater responsiveness under direct elections, the implications for the equity of policy outcomes have not been examined. However, there is a distinct possibility that responsiveness and equity may conflict. Although politicians may strive to respond to their publics, their estimates of public preferences may be biased because of unequal participation. Participation in politics is increasing with wealth, education, and interest, and these participatory biases distort the messages sent to public officials (Schlozman, Verba, and Brady 2012; Verba, Schlozman, and Brady 1995; Wolfinger and Rosenstone 1980). Thus, if their electoral incentives are increased, officials will seek to respond more to these distorted messages.[1]

Whether this increased responsiveness to an anti-equity median voter actually increases inequality depends on how officials would behave in the absence of electoral incentives. If appointees are actually more opposed to redistribution than the median voter, moving closer to voter opinion could decrease inequality. Yet in many cases, the appointee will likely

1. If reforms that increased incentives also mobilized new voters, then the messages sent to officials may become less biased. Yet new opportunities for participation, such as an additional elected office, tend to exhibit even greater participatory bias, as those already predisposed to participate are more likely to take advantage of these new opportunities (Enos, Fowler, and Vavreck 2014; Grimes and Esaiasson 2014; Hinnerich and Pettersson-Lidbom 2014). Consequently, to the extent that direct elections change the makeup of the electorate, they will only shift it even more toward those with an intense interest in the official's task (Anzia 2011; Berry 2009).

be pushed in a more pro-equity direction than voters because of additional constraints on their behavior, such as personal ideology, professional norms, or administrative review. For instance, Huber and Gordon (2004) argue that trial judges seek to carefully consider the details of a particular case, as well as their own personal ideology, in sentencing defendants. Thus in the absence of electoral incentives, judges may be more lenient because of their own ideology, as well as their "desire to judge" (250)—to consider the facts of the case as they have been trained. Pointing to a similar mechanism in a very different context, Hainmueller and Hangartner (2015) argue that Swiss town officials are less likely to make ethnically biased decisions than voters, even though these officials are no less prejudiced. In contrast to voters, however, town officials' decisions are subject to judicial review. Similarly to the case of judges, these officials are incentivized by nonelectoral constraints to fairly assess naturalization applicants.

Note that in these two examples, as in general, there is still room for voter opinion to play a role in officials' decision calculus. Appointed officials are directly accountable to the appointing official, who may be directly accountable to voters. Thus while it is useful to consider how officials would behave in the absence of electoral incentives, empirically we will typically see incentives lessened, not eliminated. In turn, greater insulation of officials from voter opinion does not imply the elimination of inequality altogether, but rather a decrease relative to the more responsive counterfactual.[2]

While nonelectoral constraints may not be present in all cases, the theoretical discussion suggests that the risk of greater inequality due to responsiveness is quite real. Many existing studies have found evidence of increased responsiveness under direct elections, yet they may have also overlooked important distributional consequences. In part, this neglect is due to the fact that studying the effects of institutions on policy outcomes is extremely difficult at the state and national levels. In the following section, I discuss how studying local governments allows me to better examine these effects.

2. Similar dynamics could also operate in other institutions that increase responsiveness, such as direct democracy. Several scholars have concluded that giving voters the power to vote on legislation directly increases responsiveness (e.g., Gerber 1996; Matsusaka 2004). In such cases, it is also plausible that those who participate are more opposed to equality than the general population and that the legislature would act in a more pro-equity manner than voters in deciding policy. This may be why scholars have consistently found that states with direct democracy tend to spend less than states without direct democracy (Lupia and Matsusaka 2004, 473). However, at such an aggregated level (in terms of both geography and budgets) it is difficult to say whether smaller state budgets reflect greater efficiency or less redistribution.

INEQUALITY AND INSTITUTIONS IN NEW YORK TOWNS

Testing the theoretical prediction of greater inequality under direct elections faces two key challenges. First, understanding the effect of elections on "who gets what" requires a policy area with measurable distributional consequences. Second, separating the effect of direct elections from other factors requires variation in institutions that is unrelated to other determinants of policy. In this section, I introduce the case of property tax assessors in New York towns as a means of addressing these challenges.

Measuring "who gets what"

Local governments present an excellent opportunity for measuring who gets what from government. As Trounstine (2009a) writes, "Scholars interested in determining whether or not the government works, and for whom it works, may find more precise (and collectable) measures at the local level than at state or national levels" (614). While local politics in general has this feature, I focus on an office that is particularly well suited for measuring for whom government works: the property tax assessor. This official is charged with estimating the value of each property in a jurisdiction; these estimates are then used to determine how much property wealth the town has and, ultimately, each property owner's tax bill. For example, imagine a town consisting of two properties, each worth $100,000, which decides it must raise $10,000 in revenue. If both homes are assessed at 100% of market value, then the town council believes it has a tax base of $200,000 and thus sets a tax rate of 10,000/200,000 = 5%. Each homeowner then has a tax bill of $5,000. Now suppose that while the first home is still assessed at 100% of market value, the second is assessed at 50% of market value. Now, the first homeowner pays an additional $1,667 in taxes, while the second pays $1,667 less. While the total amount of revenue does not change, the first homeowner is now subsidizing a substantial tax break for her neighbor. The relative proportion of market value subject to tax therefore has significant consequences for who pays for government.

According to state governments and academics who study the property tax, the most important cause of inequality in tax assessments is the reassessment process. As housing markets change, different homes rise or fall in value, and extant estimates of value stray further and further from the truth. This is why the assessor's most important job is to regularly reassess all properties in a town, so that assessed values keep up to date with market conditions. As the New York State Department of Taxation and Finance (2012b) advises local officials, "Municipal-wide reassessments are the best way to ensure that assessments are fair and accurate." Summarizing academic research on this issue, McMillen and Weber (2008, 654) concur that "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."

Thus for measuring responsiveness and inequality, the case of assessors offers two key advantages. First, we can easily trace the chain of events from the assessor's policy decision to the voter's tax bill, unlike many state or national policy decisions whose impact on voters is more opaque. Second, assessments are a zero-sum game: if certain homeowners are paying less in property taxes than they should, other homeowners must pay more. Because one voter's loss is another's gain, this makes it much easier to determine who wins and loses as a result of government decisions.

To make these distributional impacts more concrete, I plot effective tax rates for two groups of homes in figure 1. In this figure, which uses data on all single-family residential home sales in New York State between 2003 and 2011, the vertical axis represents a home's effective tax rate, or the proportion of the home's value that is being taxed.[3] For example, if a home sells for $1 million but is assessed at $500,000, the effective tax rate is 50%. Such a scenario would be analogous to someone earning an income of $1 million but paying income taxes on only $500,000. The horizontal axis represents the number of years since a town conducted a reassessment. The solid line represents the average effective tax rate for the least expensive homes (those with sale prices in the lowest quintile) and the dashed line for the most expensive homes (sale prices in the highest quintile); dashed and dotted thin lines represent 95% confidence intervals.

In New York State, town assessors have discretion over when these townwide reassessments will occur. As the state Department of Taxation explains to voters, "The assessor is a local government official who estimates the value of real property within a city, town, or village's boundaries" and "is obligated by New York State law to maintain assessments at a uniform percentage of market value each year." To meet this obligation, "Where assessments need to be changed, in some cases, your assessor will be able to increase or decrease the assessments of a neighborhood or group of properties" (New York State Department of Taxation and Finance 2012a).

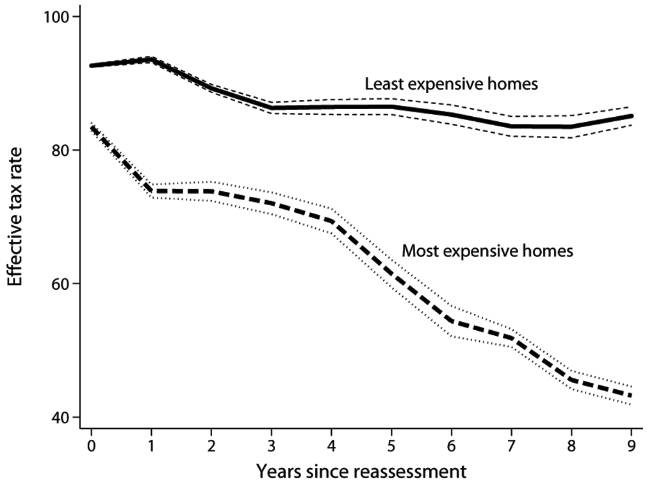

Figure 1. Who gets what: reassessments and inequality. This figure shows the relationship between effective tax rates and the number of years since a town last conducted a reassessment. The solid line represents effective tax rates for the least expensive homes (those in the bottom quintile of sale prices) and the dashed line for the most expensive homes (top quintile). Lines connect averages, and dotted and dashed thin lines reflect 95% confidence intervals.

Thus the assessor decides both when to conduct a reassessment and what value should ultimately be assigned to each property.

Figure 1 shows that these choices have significant distributional consequences: for towns that conducted a reassessment in that year, the least expensive homes pay taxes on about 90% of their actual value, while the most expensive homes are taxed on about 85% of their value. As the time from reassessment increases, however, this disparity grows dramatically: five years from a reassessment, the poorest homes are effectively taxed at 87% and the richest at 62%; nine years out, the ratio is 85% to 43%. Without regular reassessments, wealthier homes become severely undertaxed relative to poorer homes.[4] Given the importance of these decisions, I use an indicator for whether a town conducts a reassessment in a particular year as the primary outcome measure in my analysis. However, I also verify whether the difference in reassessment activity, in turn, affects effective tax rates.

Estimating the effects of institutions

While reassessments are a powerful measure of who gets what, a second challenge lies in estimating the impact of institutions without bias. Political institutions tend to covary

3. These sales are restricted to the 920 sample towns used in this study, described in more detail below. I also trim the sample to include homes selling for between $10,000 and $1,000,000, after adjusting for inflation, and limit the number of lags to nine years. The total number of observations used to generate this figure is 207,743.

4. That the effective tax rate is never quite 100% for any group likely reflects that there is inevitably some lag between home sales and reassessments, as well as assessors generally trying to err on the side of under- rather than overestimating values.

with other factors that also affect policy, such as local policy preferences, economic institutions, and pretreatment policy outcomes. As a result, estimating the effects of institutions using cross-sectional designs requires very strong "all else equal" assumptions that are unlikely to hold (Acemoglu 2005; Keele and Minozzi 2013).

In contrast, local governments can exhibit significant over-time variation in institutional configurations, allowing for much stronger designs (Trounstine 2009a, 613). While local institutions may be resistant to change on their own (Baqir 2002, 1324), state or federal government actions often induce large numbers of localities to undergo reforms (Anzia 2012; Hainmueller and Hangartner 2015). When such changes do occur, analysts are able to observe the same political unit under different institutions, which means that any confounding variables—provided they do not also change with the change in institutions—will be held constant. The potential for time-varying confounders is also more limited when studying lower-level governments, given that changes in local government institutions are often influenced by higher-level mandates. Additionally, if long panel data are available, we can test for time-varying confounding directly by looking for changes in the outcome variable that might have occurred prior to treatment.

Fortunately, the case of assessors in New York features substantial over-time variation in institutions as a result of state pressure. Originally, all towns elected a three-member board of assessors, consisting of a chair and two deputies. Over time, the state government became increasingly concerned with poor assessment administration, a practice that they blamed on elected assessors (New York State Constitutional Convention Committee 1938, 157). In the 1970s, the state legislature passed a law requiring all towns to switch to an appointed assessor system, with appointments made by the town council, unless they passed a referendum in favor of keeping the elected system. This law effectively converted about half the towns in the state, while the remaining half retained their elected assessors. Since that time, towns continued to shift from elected to appointed assessors, and I describe these transitions since 1987 in figure 2.[5] While 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 2011, only about 110 of the 920 towns retain the elected system.[6] All told, 392 of 920 towns changed their institutions in various years between 1987 and 2011. Note that the switches occur only from election to appointment, not the other way around, and are permanent. There is no state-sanctioned process for converting from an appointed to an elected assessor (New York State Department of Taxation and Finance 2013b).

While the state continues to advise towns to switch from election to appointment, the decision is ultimately up to the town council, creating the potential for selection bias. One important alternative hypothesis is that voters in towns that switch to appointment may be more in favor of reassessments, such that reassessments would be higher even if these towns used elections. This would lead us to falsely conclude that responsiveness is higher under elections. However, there are several reasons why this is less of a concern here. For one, voter preferences in these towns are likely stable over time, which means they are accounted for by town fixed effects and are a threat to inference only if they happen to change simultaneously with the change in institutions. Media accounts of these transitions cast doubt on this possibility: rather than pointing to voter preferences, town officials will cite state pressure, the fact that they are among the last of the towns in their county to maintain the elected system, cost savings from not having to pay salaries and training expenses for multiple assessors, and a shortage of interested candidates.[7] All this is to suggest that switches are part of a broader trend driven by the actions of the state government rather than changes in the preferences of local actors or other town-level factors that may also influence policy. The robustness checks reported below are strongly supportive of this interpretation.

EFFECT OF DIRECT ELECTIONS ON REASSESSMENTS

As discussed previously, the key way that assessors maintain equity is by conducting townwide reassessments, or updating estimated property values to reflect current market conditions. The more frequently these reassessments are conducted, the more equitable are effective tax rates. In this section, I use a difference-in-differences design to test whether

5. While the state tracks the aggregate number of switches over time, it does not maintain a complete list of towns by switch year. I use multiple sources, described in the appendix, to determine the year when each town switched. Despite this effort, I can determine only intervals for many towns (e.g., I can determine that the switch took place between 1992 and 1998). For these cases, I code the switch year as the midpoint of the interval (e.g., 1995). Because the missingness on the treatment occurs for idiosyncratic reasons—e.g., whether the home county had town election results posted on its website—this should only bias my estimates toward zero. Indeed, as I show in the appendix, the effects are significantly stronger when excluding the uncertain cases.

6. There are 932 towns, 62 cities, and 551 villages (subtown units) in New York State. Because assessment systems differ across these three types, I focus only on towns. The state has 57 counties (excluding the five boroughs of New York City), and two counties, Nassau and Tompkins, have countywide assessors. After the three towns in Nassau and the nine in Tompkins are excluded, the sample consists of 920 towns.

7. These accounts are summarized in the appendix.

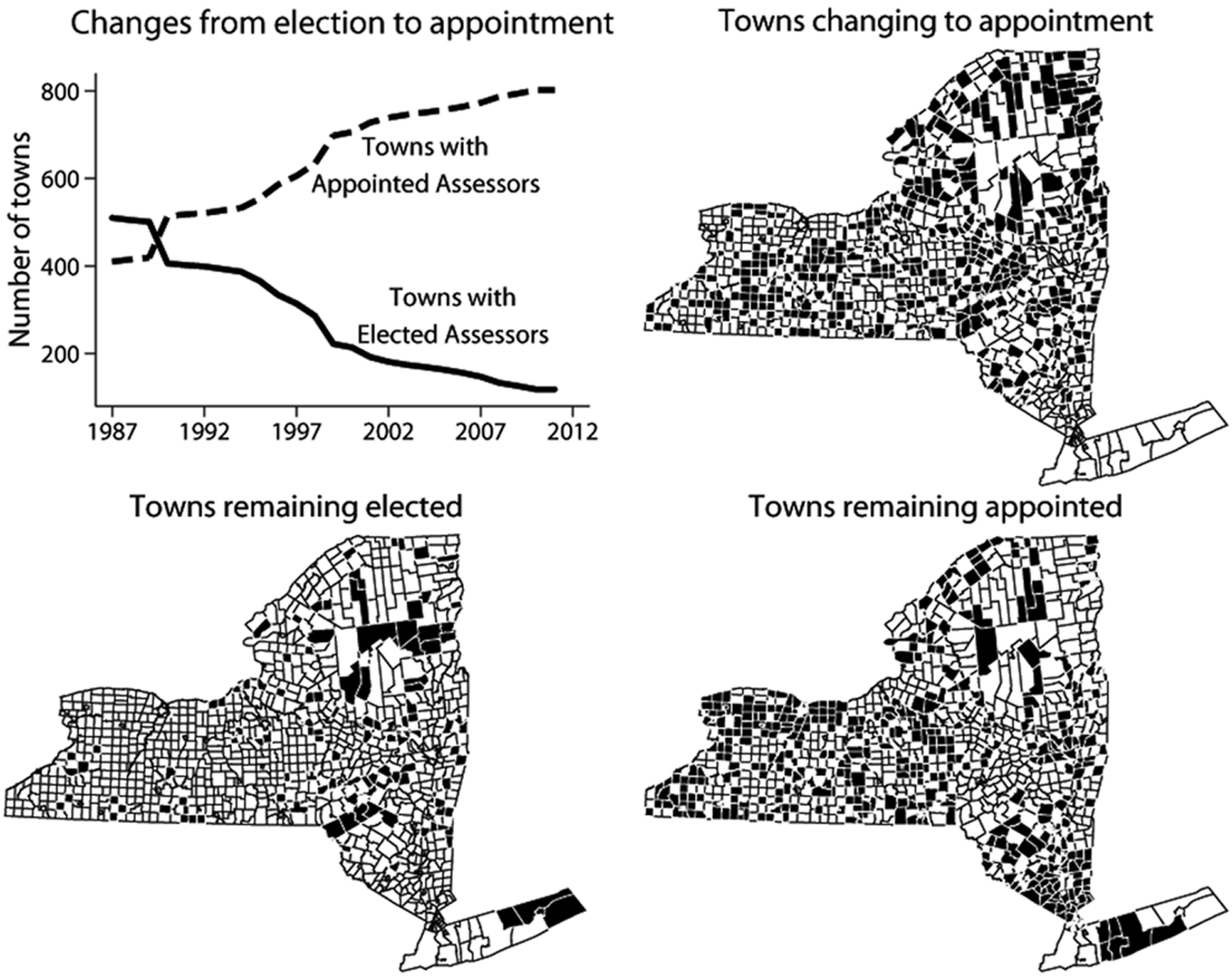

Figure 2. Towns changing from elected to appointed assessors, 1987–2011. The top-left panel plots the number of New York towns with elected and appointed property tax assessors between 1987 and 2011. The remaining panels map towns that did and did not switch from election to appointment over this period, with shaded polygons indicating the respective class of town.

elected assessors are more or less likely to conduct reassessments than appointed assessors. In the simplest case, this design consists of two groups, treated and control, and two comparisons, before and after. The first difference compares outcomes within the treated group (in this case, those that change to appointment, or the top-right map in fig. 2), before and after the change to appointment. The second difference compares outcomes within the control group (those that do not change their institutions, or the bottom two maps in fig. 2), before and after the treated group changes. Because the first comparison is done within towns, any persistent confounding variables are held constant. The second comparison, moreover, reflects how outcomes would have changed in the absence of the switch.

Because my data set consists of multiple cohorts that switch in different years, I use a difference-in-differences regression (Angrist and Pischke 2009, 233) that includes a treatment indicator and group and period fixed effects. This regression extends the logic of the two-group, two-period case, asking how outcomes change before and after the treatment changes for multiple cohorts. That is, I estimate a regression of the form

$$\text{Reassessment}_{jt} = \beta \times \text{Elected}_{jt} + \text{Town}_j + \text{Year}_t + \varepsilon_{jt},$$

where Reassessment_{jt} is an indicator for whether a town j conducted a reassessment in year t; Elected_{jt} is equal to one if a town elects its assessor and zero if it appoints; and Town_j and Year_t are fixed effects for town and year. I assume that the error term ε_{jt} has mean zero and contains no other factors that correlate with both direct elections and reassessments, though I will show that the results are robust to relaxing this assumption. To account for dependence within towns and across years, I cluster the standard errors at the town level.[8]

Before discussing the point estimates from this regression, it is worth noting that the average level of reassessment in these towns, as shown in the header to table 1, is quite low, at about 20 percentage points. In other words, towns on average are updating their assessments once every five years. Given the patterns in figure 1, it is likely that this frequency of reassessment is insufficient for maintaining equity. Indeed, later in the article I will show that the baseline level of regressivity in effective tax rates is quite large, even in the absence of direct elections. Nonetheless, direct elections serve only to make this problem worse: the point estimate in the first column of table 1 indicates that direct elections cause about a 9 percentage point decline in the probability of conducting a reassessment, with a standard error of less then 2 percentage points. That is, towns go from conducting a reassessment about once every five years to about once every nine years as a result of elections. Again, given figure 1, this effect is likely meaningful for the equity of effective tax rates, a claim I verify below.

Table 1. Effect of Elected Assessor on Reassessments: Town Analysis

	Outcome: Reassessment (Average = 20.27%)		
	(1)	(2)	(3)
Elected assessor	−8.78***	−8.77***	−8.77***
	(1.70)	(1.70)	(1.69)
Demographic controls	No	Yes	Yes
Fiscal controls	No	No	Yes
Observations	23,000	23,000	22,925

Note. All specifications include town and year fixed effects. Town-clustered standard errors are in parentheses.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

ROBUSTNESS CHECKS AND ALTERNATIVE EXPLANATIONS

The baseline difference-in-differences estimate rules out confounding from any time-invariant factor but assumes that changes in institutions are unrelated to any other changes that might also influence reassessments. While this assumption is much weaker than in a typical cross-sectional design, it is still possible that other determinants of reassessments also change when a town changes its institutions, which could bias the estimates reported previously. For this reason, in this section I consider some particular forms of time-varying confounding, discuss how they may affect the results, and conduct several empirical tests to address them.

First, it may be that changes in institutions correlate with changes in voter preferences for reassessments. In this scenario, appointed assessors may be just as responsive as elected assessors but are simply responding to different preferences. While an annual, town-level measure of reassessment preferences does not exist, we can proxy for changing preferences using time-varying measures of demographic variables, including population, median income, unemployment, the percentage of residents over age 65, the percentage of white residents, the percentage renting their home, and the percentage employed in agriculture.[9] In column 2 of table 1, I add these measures to the baseline specification. Aside from decreasing the absolute magnitude of the point estimate by 0.01 percentage points, the inclusion of these variables has no impact on the result.

Second, towns experiencing changes in assessor selection method might also experience other institutional or policy changes that could affect reassessments. Estimating a value for each individual property in a town is a resource-intensive endeavor, as reflected in the low base rate of reassessments even among appointing towns. If towns that change to appointments also experience changes in factors that affect their capacity to conduct reassessments, this could also bias the baseline estimates. To account for this possibility, column 3 of table 1 adds total revenues, the proportion of revenue that comes from property taxes, surplus (total revenue less total expenditure), and total full-time employment. Again, the inclusion of these variables does not substantively change the estimated effect: the point estimate now indicates a decline of 8.77 percentage points in the probability of conducting a reassessment, with a standard error of 1.69.[10]

Third, what looks like the impact of a new institutional system may simply reflect the arrival of a new assessor. If

8. I use a linear probability model for this analysis, as opposed to a logit or probit regression, as the latter are inconsistent in the presence of fixed effects because of the "incidental parameters problem" (Angrist and Pischke 2009). Moreover, the linear model yields consistent estimates of the average treatment effect regardless of the true functional form (Wooldridge 2010, 562).

9. In particular, and as explained in more detail below, the percentage of residents over age 65 is likely the best proxy for reassessment preferences.

10. The number of observations declines slightly in col. 3 as three of the 920 towns are missing data for the fiscal outcomes. As expected, rerunning the regression in col. 1 but using these covariates as "placebo" outcomes yields point estimates that are substantively and statistically insignificant. Results are also the same if I use year-over-year changes in the fiscal variables.

assessors are generally more likely to reassess early in their terms, this would bias estimated effects upward. I explore this possibility using data on assessor turnover for a subset of 143 towns. As described in full in the appendix (available online), this analysis yields two key results. First, turnover is in fact about 20 percentage points more likely when the assessor is elected. If turnover is positively correlated with reassessments, then this result implies that the estimates in table 1 understate the true impact of appointments. Second, turnover is in fact not positively correlated with reassessments: the estimates imply a statistically significant decrease of about 3 percentage points, an effect that is the same under both the elected and appointed systems.

I next directly test whether towns that changed their institutions were already becoming more likely to conduct reassessments, prior to the change in institutions. Such a pattern would be consistent with any number of selection bias explanations, including but not limited to those discussed above. To conduct this test, I simply plot the difference in outcomes between switchers and nonswitchers, before and after the switch occurs. That is, I estimate a regression of the form

$$\begin{aligned}
\text{Reassessment}_{jt} = {} & \beta_{-5} \times (\text{5 years before switch})_{jt} \\
& + \beta_{-4} \times (\text{4 years before switch})_{jt} \\
& + \beta_{-3} \times (\text{3 years before switch})_{jt} \\
& + \beta_{-2} \times (\text{2 years before switch})_{jt} \\
& + \beta_{-1} \times (\text{1 year before switch})_{jt} \\
& + \beta_{0} \times (\text{Year of switch})_{jt} \\
& + \beta_{1} \times (\text{1 year after switch})_{jt} \\
& + \beta_{2} \times (\text{2 years after switch})_{jt} \\
& + \beta_{3} \times (\text{3 years after switch})_{jt} \\
& + \beta_{4} \times (\text{4 years after switch})_{jt} \\
& + \beta_{5} \times (\geq 5 \text{ years after switch})_{jt} \\
& + \text{Town}_j + \text{Year}_t + \varepsilon_{jt}.
\end{aligned}$$

In this regression, the 11 β coefficients represent the difference in outcomes (net of town and year effects) between switchers and nonswitchers for each year relative to the switch, from five years prior to five years after.[11] If, for example, β_{-3} is positive and significantly different from zero, towns that changed to appointments were already seeing more reassessments three years prior to the switch, a result that would cast doubt on a causal interpretation of table 1.

I plot these coefficients in figure 3, with the horizontal axis representing the number of years since the switch to appointments occurred. The figure shows that the estimates are statistically no different from zero prior to the switch to appointments. In contrast, the estimates are uniformly positive and significant following the year of the switch, rising to as much as 20 points the second year. Thus, the observed effect of elections is not driven by pretreatment differences in trends between switching and nonswitching towns, and the simple specification in table 1, column 1, likely captures the causal effect of elections on reassessments.[12]

Finally, I report the results of several additional robustness tests in the appendix. The first series of tests shows that the result is not driven by any particular comparison group. For instance, one concern when looking at the maps in figure 2 is that the result may differ if I use the always-elected (the bottom-left map) or the always-appointed (bottom-right) towns as the control group, rather than pooling the two groups as in table 1. However, in the appendix I show that the effect is very similar when excluding either group from the analysis or when restricting the analysis to only towns that switched over this period. Further, the effect is not an artifact of measurement error in the independent variable and is in fact stronger when dropping all towns with an uncertain treatment year. Finally, I show that the effect is not driven by any particular year or group of towns by sequentially reestimating the baseline regression while dropping each year and each county. The smallest estimates from each set of regressions are still substantively and statistically comparable to those in table 1.

EFFECT ON EFFECTIVE TAX RATES

How much does elected assessors' failure to reassess affect inequalities in taxation? Figure 1, which shows that inequality

11. The final lag variable captures the effect of being greater than or equal to five years after treatment; observations that are more than five years before the switch are included in the intercept. For this analysis, I also drop towns with uncertain switch years given that interest lies in estimating changes at precise years before and after the switch.

12. While the final coefficient would seem to indicate a downward trend, recall that this coefficient represents the effect of being five or more years after treatment. The correct interpretation is thus that the short-term effect is larger than the long-term effect, not that the long-term effect is zero. In the appendix, I replicate fig. 3 using 10 lags; this figure also shows an initial surge in reassessments, followed by a moderate decline that is still about 10 percentage points at 10 or more years after reform. One explanation for this pattern is that most towns conduct a reassessment soon after the switch but that some of these towns reassess more frequently than others thereafter. This would explain why the estimated differences are the most precise in the initial two years after the reform (when all towns conduct their first postelection reassessment), then become noisier afterward (when some towns continue to reassess annually or semiannually, while others wait longer). In the appendix, I show that the distribution of years between the reform and the first and second reassessments is consistent with this explanation.

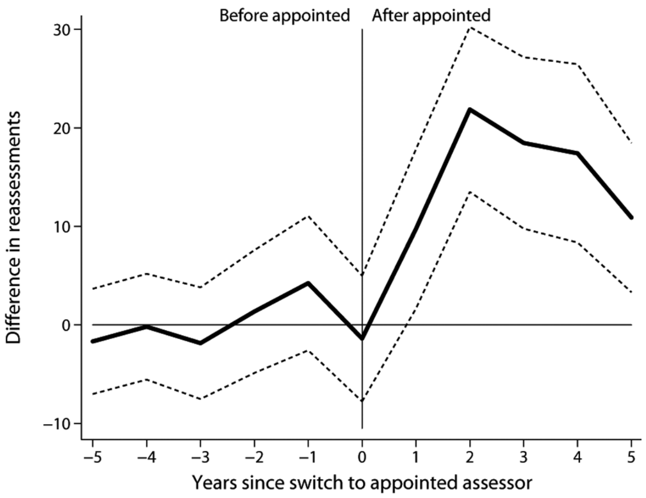

Figure 3. Effect of elected assessor on reassessments: checking for pre-reform differences in outcomes. This figure plots coefficient estimates (with dashed lines spanning 95% confidence intervals) representing the difference between switching and nonswitching towns, from five years prior to the switch to five years after.

grows as a function of time since reassessments, suggests that the impact of elections could be severe. To directly test this claim, I use data on all real estate sales in the state between 2003 and 2011 to estimate the effect on individual effective tax rates.[13] While using these data limits the period of analysis compared to earlier, it deepens our understanding of the consequences of reassessment policy by examining impacts at a disaggregated level, something that is virtually impossible when studying state and national institutions.

For this analysis, I first compute an effective property tax rate for each property i in town j in year t:

$$\text{Effective tax rate}_{ijt} = \left(\frac{\text{Assessed value}_{ijt}}{\text{Sale price}_{ijt}}\right) \times 100,$$

where both assessed values (the assessor's estimate of the home's value) and the sale price are in real 2012 dollars.[14]

I then estimate a regression of the form

$$\begin{aligned}\text{Effective tax rate}_{ijt} = {} & \beta_1 \times \text{Elected}_{jt} \\ & + \beta_2 \times \ln(\text{Sale price}_{ijt}) \\ & + \beta_3 \times \text{Elected}_{jt} \times \ln(\text{Sale price}_{ijt}) \\ & + \text{Town}_j + \text{Year}_t + \varepsilon_{jt}.\end{aligned}$$

This specification tests how elections affect the equity of effective tax rates. For instance, β_2 represents how effective tax rates vary as a function of sale prices when towns appoint their assessors. A negative value of β_2 implies that wealthier homes pay lower effective tax rates than poorer homes in towns with appointed assessors. Similarly, β_3 represents how this relationship changes when towns change between electing and appointing, such that a negative β_3 means that the baseline level of regressivity gets worse when towns elect their assessor. Finally, β_1 represents the effect of elections on effective tax rates when sale price is equal to zero, a quantity whose interest depends on the scale of sale price. For the tabular results below, sale price is coded such that zero represents the minimum of \$10,000 and one the maximum of \$1 million; when interactions are included, β_1 is therefore the effect of elections on effective tax rates for the least expensive homes.[15]

In the first column of table 2, I show the results of a regression in which I restrict β_2 and β_3 to be zero; this specification tests for an average effect on effective tax rates, before asking how these effects vary by sale price. The first estimate suggests that the average effective tax rate is about 6 percentage points lower when assessors are elected; this estimate falls just short of statistical significance, with a standard error of about 3. Adding demographic and fiscal controls in column 2 increases the precision of this estimate: the coefficient is now about −7 and the standard error is still about 3.

13. I limit the analysis to single-family, residential homes that sold at "arm's length," meaning in the absence of family ties or coercion. Unfortunately, micro-level sales data were not available prior to 2003. In the appendix, I show that the results obtained in this section hold whether restricting the sample to before or after 2007, when the housing market collapsed.

14. The assessed value of the home is determined by the assessor in the most recent reassessment year. The sale price is for the year of the sale. Each observation is included only in the year of the sale, and thus the analysis is restricted to only homes that sold. While this may induce sample selection bias, this would not affect the internal validity of the design provided that the difference between homes that do and do not sell does not change with the switch in institutions. Separate difference-in-differences regressions show that neither the median sale price nor the total number of sales changes as a result of a switch to appointments, which suggests that any differences between homes that do and do not sell remain constant (results available on request). Real estate economists disagree as to whether sample selection is a problem in studies that rely on real estate sales (McMillen and Weber 2008). While the use of sales data is not without problems, it is still considered the best available measure by professionals and state and local governments interested in evaluating tax uniformity (Ihlanfeldt 2004).

15. Although the presence of sale price in both the left-hand side (as the denominator in effective tax rates) and the right-hand side may cause spurious bivariate correlations, this is not a problem in multivariate regression analysis (Firebaugh 1988). Dispensing with ratios and using log assessed values as the outcome yields statistically similar results, but theoretical interest lies in the effective tax rate itself rather than how its components change.

Table 2. Effect of Elected Assessor on Effective Tax Rates: Property Analysis

	Outcome: Effective Tax Rate			
	(1)	(2)	(3)	(4)
Elected assessor	−6.35	−7.18*	7.74	6.76
	(3.25)	(3.30)	(4.83)	(5.00)
Sale price (log)			−41.13***	−41.79***
			(4.55)	(4.37)
Elected × price			−25.84***	−25.07***
			(7.29)	(7.46)
Controls	No	Yes	No	Yes
Observations	411,731	411,731	411,731	411,731

Note. All specifications include town and year fixed effects. Town-clustered standard errors are in parentheses. "Controls" include both the demographic and fiscal controls as included in table 1.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

The third and fourth columns of table 2 confirm that the effect of elections is primarily redistributive, as the decreases in effective tax rates are much larger for the wealthiest homes. The second row of column 3 indicates that, in towns with appointed assessors, the richest homes pay effective tax rates that are 41 percentage points lower than those of the poorest homes (standard error = 5). This baseline effect is consistent with the town-level results shown earlier: even in towns with appointed assessors, policy decisions are biased toward wealthier voters. Just as with the town-level results, however, we also see that elections make this situation worse: the interaction between price and direct elections is −26 percentage points, with a standard error of 7 points. This means that the gap between effective tax rates for the richest and poorest homes grows an additional 26 points when towns elect their assessors. Not surprisingly, given the robustness of the town-level results, adding controls in column 4 leaves these estimates unaffected.

In figure 4, I use the coefficients from a specification similar to table 2, column 3, to plot the effect of elected assessors as sale price varies.[16] Consistent with table 2, the least expensive homes actually see a slight increase of about 8 percentage points in effective tax rates as a result of elections, though the effect is just shy of statistical significance. In contrast, the only homes that benefit from elections are those above $100,000, with million dollar homes, which see a decline in effective tax rates of 18 points, benefiting the most.

IS THE EFFECT DUE TO ELECTORAL INCENTIVES?

I have argued that increasing responsiveness risks increasing inequality in public policy, because of the unequal participation of rich and poor. I have also shown that direct elections cause a substantial increase in the inequality of local taxes. I now consider whether these effects are plausibly a result of heightened electoral incentives and not some alternative mechanism.

I first directly test a key implication of the incentives mechanism. Recall that while the appointing official may be subject to elections, voters will be hesitant to make the performance of the appointee an issue in the appointer's election. Because the appointing official's task is more salient to voters, they would rather not judge the appointer on the basis of his or her appointee (Besley and Coate 2003). Elections should therefore have the strongest effects when the appointee's task is least salient. Likewise, when the issue becomes extremely salient, both appointments and elections should lead to responsiveness. The effect of elections on reassessments should therefore be decreasing in issue salience (Mullin 2009; Vlaicu and Whalley 2014).

As a proxy for issue salience, I use the percentage of the town's residents aged 65 and older. Because most senior citizens rely on fixed incomes, rising property taxes and assessments are especially salient for this group. Indeed, recent evidence indicates that such increases can even have a mar-

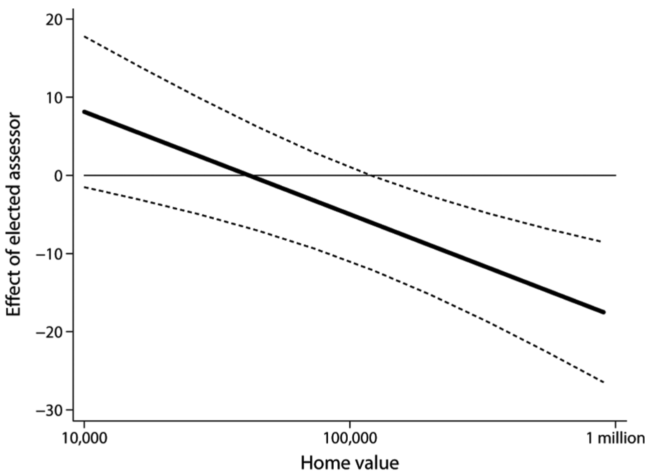

Figure 4. Effect of elected assessor on effective tax rates. This figure plots the marginal effect of elected assessor for different values of home price. The marginal effect is calculated using coefficients from the specification in table 2, column 3. Dashed lines span 95% confidence intervals.

16. The only difference from the table is that I do not rescale sale price before estimating the regression, which allows me to predict the marginal effect for actual values of sale price. Although this specification assumes that the effect of sale price is linear, I show in the appendix that I obtain similar results when I instead use indicators for quintiles.

ginal impact on residential mobility, in spite of seniors' strong emotional attachments to their homes (Shan 2010). Senior citizens are also much more likely to own their homes free and clear, which means they see their property tax bill every quarter rather than having it obscured by an escrow payment. As Cabral and Hoxby (2013) show, the use of an escrow account, in which homeowners combine their mortgage and tax bills into a single payment, drastically reduces the salience of the property tax to homeowners. Thus, senior citizens are likely exempt from the salience and information problems that, on average, prevent appointed officials from responding to voter preferences.

I test this implication using the following regression:

$$\begin{aligned}\text{Reassessment}_{jt} = \beta \times \text{Elected}_{jt} + \alpha \times \text{Elected}_{jt} \\ \times \text{Senior}_j + \text{Town}_j + \text{Year}_t + \varepsilon_{jt},\end{aligned}$$

where Senior_j is the proportion of residents age 65 and older in 1990.[17] Table 3 shows the results, which are essentially the same regardless of the controls used. As predicted, elections have their largest impact where salience is lowest: the negative effect on reassessments is about 20 points, with a standard error of about 5 points, when salience is at its minimum.[18] This is roughly twice as large as the magnitude of the baseline estimate in table 1. Likewise, the interaction between elections and the proportion of seniors is positive and statistically significant, at about 30 points, with a standard error of about 10. Although this suggests that elections actually increase reassessments in places where salience is highest, we do not reject the null hypothesis that the effect in such towns (i.e., $\beta + \alpha$) is zero ($p = .22$). Figure 5, which plots the marginal effect of elections as a function of the proportion of seniors, illustrates the effect. Precisely as theory would suggest, elections have an impact only in towns where assessments are less salient. Once we remove the key enabling factor of nonsalience, the effects of elections disappear.[19]

Table 3. Effect of Elections as a Function of Property Tax Salience: Town Analysis

	Outcome: Reassessment		
	(1)	(2)	(3)
Elected assessor	−20.52***	−19.79***	−19.32***
	(4.64)	(4.71)	(4.68)
Elected × percent 65 and older	31.30**	29.33*	28.07*
	(11.18)	(11.40)	(11.34)
Demographic controls	No	Yes	Yes
Fiscal controls	No	No	Yes
Observations	23,000	23,000	22,925

Note. All specifications include town and year fixed effects. Town-clustered standard errors are in parentheses. "Controls" include both the demographic and fiscal controls as included in table 1.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

I next consider the chief alternative mechanism. Given the technical nature of tax administration, elected assessors may simply be less competent than appointed assessors. They may seek to perform more reassessments but are unable to do so because of a lack of training, not a lack of political will. Ideally, I would measure competence and include it as a control variable in the regression reported above; unfortunately, such a measure is not available. However, an indirect test of this channel comes from a 1970 state reform that varied incentives for some towns while effectively holding competence fixed.

This reform, known as the Assessment Improvement Law, had two major components (New York State Office of Real Property Services 2006). First, the law caused some towns to change from electing to appointing their assessors. Prior to the law, only about 5% of towns appointed their assessors (Conneman 1979). As a result of this law, about 300 towns changed to appointments while the remaining 600 stayed electing. Second, it increased capacity and technical expertise in all towns, both by creating county offices of assessment to assist localities and by imposing training and certification requirements for all assessors. Prior to 1970, both training and county assistance were virtually nonexistent. Thus the law varied competence for all towns but varied incentives for only

17. This regression essentially adds a third difference to the baseline regression, comparing how the "difference in differences" itself differs as a function of salience. I use the proportion from 1990 as it is plausibly pretreatment; in contrast, posttreatment measures may be confounded if switches affect the proportion of seniors. The results are robust if I use the proportion of seniors from 1980 or if I ignore the confounding issue and allow the percent senior variable to vary over time and as a function of elections. Note that because the proportion from 1990 does not vary over time, the "main effect" is absorbed by the town fixed effect.

18. I code the percent senior variable as zero if it is at its sample minimum (about 5%) and one if it is at the maximum (about 30%).

19. I show that the interaction is robust to delinearizing the percent senior variable in the appendix. This regression also shows that the impact of elections in the "most salient" towns—in the top quintile of percent senior—is essentially zero (estimate = −1.29, standard error = 3.08), suggesting that the "positive" impact for the most salient towns is likely an artifact of the linearity assumption.

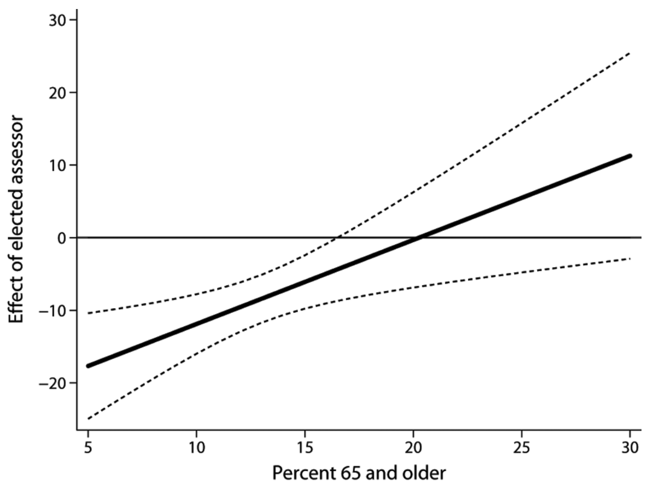

Figure 5. Effect of elections as a function of property tax salience. This figure plots the marginal effect of elected assessor for different values of percent 65 and older. The marginal effect is calculated using coefficients from the specification in table 3, column 1. Dashed lines span 95% confidence intervals.

a third. Because all towns experienced the same shock to competence, any difference in outcomes between the two groups can be attributed to incentives.[20]

Because measures of reassessment activity are not available prior to the 1980s, I use a town's average effective tax rate, or the ratio of assessments to market value, as the outcome for this analysis. In figure 6, I plot average outcomes in a 10-year window around the reform for these two groups of towns: those that were subject to increased training, but not decreased incentives (solid line), and those that were subject to both increased training and decreased incentives (dashed line). The vertical line at 1971 marks the year the law took effect. This figure has several notable features. First, there is no difference in outcomes between the two groups of towns prior to 1971, in either trends or base rates; this supports the interpretation of the reform as a natural experiment. Second, after 1971 there is a general upward trend for both groups, which is consistent with the reform increasing competence for all towns. Third, postreform accuracy is markedly higher for the group that varied both competence and incentives: the difference in accuracy between the two groups is about 7 points in 1973 and is between 13 and 14 points for the rest of the period. (The difference in differences is -11 points, significant at the $p < .001$ level clustering by town.)

This test is not without caveats. It is impossible to verify if electing towns actually complied with the training and capacity provisions, and formal training may not fully capture competence. However, together with the salience interaction, as well as additional tests reported in the appendix, the evidence is broadly supportive of the incentives mechanism.[21] While a lack of technical training may prevent some elected assessors from doing their job, the heightened incentives they face would likely prevent them from supporting equity even if they were fully competent to do so.[22]

CONCLUSION

Institutional designs that heighten electoral incentives are often seen as unambiguously positive for democracy. Given the reality of unequal participation, however, making officials more responsive may also lead to greater inequality in policy outcomes. This article explored this danger using the novel case of tax assessors in New York towns. Using a large number of over-time changes in institutions and analyzing original data at both the town and property levels, I have revealed considerable inequity in local tax policy. Moreover, I have shown that direct elections only increase these inequalities. As predicted, core democratic values of responsiveness and equality can sometimes be in conflict.

In demonstrating these effects, this article has showcased the dual advantages of studying local politics for measuring inequality and estimating causal effects. Of course, it is also worth speculating as to how these effects may differ in

20. As described in the appendix, the law required all towns to change their system unless they held and passed a referendum to the contrary. Towns did ultimately have the power over whether they would switch, but selection is less of a concern given that, eventually, nearly all towns did. In a situation in which all units are eventually treated, selection bias can occur only if units can manipulate the timing of the treatment. That is also unlikely here, given that the date of the switches was dictated to towns and local officials may not have anticipated the law's passage.

21. In the appendix, I present evidence that assessor elections are frequently contested, a seemingly necessary condition for the incentives mechanism. Moreover, both elected and appointed assessors are legally bound to meet training and certification requirements in the contemporary period (New York State Department of Taxation and Finance 2013a). As shown in the appendix, conditional on going through with a reassessment, elected assessors do not produce assessments that are less accurate than those of appointed assessors.

22. Having shown that the effect is due to voter preferences, we may then ask what determines these preferences. I have argued that the median voter is economically advantaged in this context (certainly true in national [Verba et al. 1995] as well as local [Fischel 2009; Hajnal 2010; Oliver 2012] elections) and so opposes reassessment out of self-interest. Of course, it may also be the case that voters who would benefit from reassessments oppose them, just as many low-income citizens support redistribution in principle but oppose it in practice (Bartels 2005; Hochschild 1986). While I do not rule out this possibility, the interaction with the proportion of seniors provides clear evidence that self-interest plays an important role. Given the very real economic impact of the tax on this subgroup, it does not seem plausible that seniors' opposition arises from ambivalence or confusion.

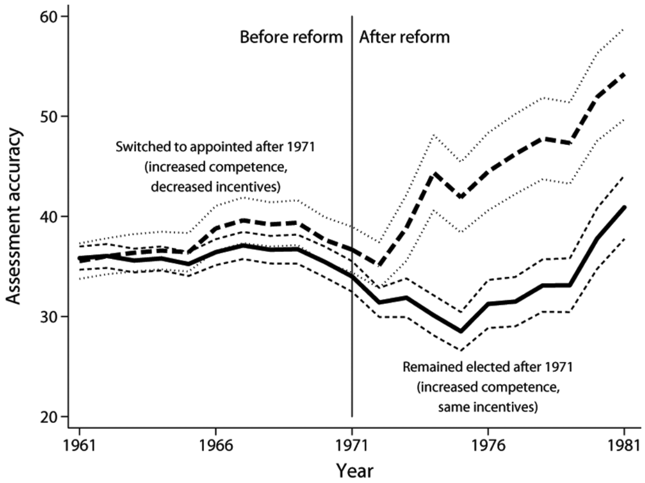

Figure 6. Varying competence for all towns, but incentives for only some. Thick lines represent group averages, and thin lines span 95% confidence intervals (1.96 × the group average).

other cases. An important moderating variable here is the nature of the preference gap between officials and voters. According to the theory, responsiveness may increase inequality when the median voter is less pro-equity than officials would be in the absence of direct voter control. It is therefore important to consider, in turn, how likely this preference divergence would be in other contexts.

In particular, one key determinant of divergence appears to be the existence of nonelectoral checks on official behavior. In my case, unresponsive assessors are incentivized to be pro-equity by state law, which mandates uniform assessments. As the state explains to voters, assessors are "obligated by New York State law to maintain assessments at a uniform percentage of market value each year" (New York State Department of Taxation and Finance 2012a). Given the substantial baseline bias in policy that was found, this case demonstrates that such checks by themselves are likely insufficient. Thus even though electoral accountability makes inequality worse, this does not mean that other institutional reforms are not needed. Moreover, in some cases nonelectoral constraints may be either insufficient or altogether absent. For instance, a primary goal of municipal reformers was to exclude the broader public from the decision-making process altogether (Bridges 1997; Trounstine 2009b). In such cases, where officials act independently of both voters and any nonelectoral checks, more responsiveness may have the opposite effect than was observed here.

Moreover, there may also be cases in which the median voter favors measures to reduce inequality. In my case, I have argued that the median voter opposes such measures because of unequal participation. While inequality in participation is one of the most robust findings in political science, there could be cases in which this factor does not exist, such as when voting is compulsory (Fowler 2013). It is interesting to consider how my findings might differ in a context in which other institutional reforms have already eliminated the bias in participation.

Speaking generally about subnational politics in the United States, however, the necessary conditions that underlie these results are, unfortunately, quite common. Hundreds of thousands of elected officials make important policy decisions in the over 90,000 state and local governments. In New York towns alone, this includes judges, town clerks, highway superintendents, and tax collectors as well as tax assessors. Over half of the 3,000 US counties elect the person responsible for determining the cause of death in criminal cases (Harris 2013), and numerous states ask voters to elect utility regulators, auditors, and secretaries of state. It is virtually always the case that, were they not directly elected, these officials would be subject to nonelectoral constraints. Although policy decisions in these cases would likely still be biased in the absence of elections, the results of this study suggest that electoral incentives only further distort accountability.

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