Like a Good Neighbor: Spatial Effects and Proposition $2^{1/2}$ Override Votes in Massachusetts

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Econometrics 312

INTRODUCTION

Proposition $2^{1/2}$ was passed into law by Massachusetts voters in 1980, and went into effect in 1982. The proposition serves two main functions: to limit both the overall size of the property tax levy that can be raised by a municipality, and the amount that levy can change (mainly increase) on a year-to-year basis. Since property taxes are a large source of income for a local government, any sort of limit on them has serious fiscal implications.

The overall limit on the amount that Massachusetts local government can raise from property taxes is set at no more than 2.5% of the total value of all taxable property; this is referred to as the *levy ceiling*. Further, the *levy limit* refers to the restriction that the property tax cannot increase by more than 2.5% on a year-to-year basis. This means that the levy limit must be at or below the levy ceiling, unless there is a vote to override. The proposition does not include debts from prior to 1980 and from any debt incurred from water or sewage projects.

Keep in mind that the new year's levy limit is not based on the levy the municipality uses but on the levy limit of the previous year. Further, the limit does increase by not only the 2.5% each year, but also due to "new growth" (properties' that increased in value due to development, new housing etc.). However, issues can arise when inflation is *larger* than 2.5%, which will result in decreased revenue.

Proposition $2^{1/2}$ does allow municipalities the flexibility to move above and below the 2.5% mark with a vote. Communities are able override the property tax limit, allowing them to raise revenue in excess of the normal limit. However, the override still needs to fall below the levy ceiling (defined earlier as no more than 2.5% of the value of all taxable property). This can be accomplished by a communities' city council calling for an override vote.

The following analysis examines what factors influence the use of the override vote as a policy tool. In particular, we want to look beyond fiscal distress to see if spatial weighting techniques are predicative of deciding to hold a vote. If the voting behavior of one locality strongly affects a neighboring one, it implies that the override is not being used effectively and for its original purpose. Over the course of the following paper we examine the effect of local and spatial variables on three different demotions of the policy process: the decision to hold a vote in a given year, the first year a vote was held, and the total number of votes overall for each municipality. Each one of these measures is a different way of examining the same question: are local governments deciding to hold a vote because they need the money, or because they see other governments doing so?

DATA AND EMPIRICAL IMPLICATIONS

Following in the footsteps of a number of authors (Case et al. 1993, Bruce et al. 2007, Brown and Rork 2005) we view the voting behavior of a given city, town, or municipality (P_{it}) as a policy tool, the use of which can be influence by any number of factors. In particular, characteristics of the locality (Z_{it}) and voting behavior of neighboring areas (P_{jt})[1] should both affect the decision of a local council to hold an override vote. It should be noted that P_{jt} should be seen as $\sum w_{ij}P_{jt}$, so as

[1] A full discussion of the definition of "neighbor" will follow in the discussion section. Depending on how the term is viewed, different weighting schemes should be used in our spatial analysis.

to take into account the presence of more than one neighboring jurisdiction. The full model is as follows below:

$$P_{it} = \beta Z_{it} + \theta P_{jt} + \xi_i + p_t + u_{it}$$

Where ξ_i and p_t are geographic and year fixed effects, respectively. The fixed effects are intended to control for variables that might have a fixed common effect on voting in a given year or a given area, such as public opinion, political leanings, demographic factors, etc…[2] (Case et al 1993). The purpose of the fixed effects are summarized well by Bruce et al.: "The state fixed effects control for unobserved time-invariant characteristics within each [municipality] while the year fixed effects control for variables that impact all [municipalities] in each particular year, such as national macroeconomic conditions, federal tax law changes" (Bruce et al. 2007).

Our data is drawn from 351 municipal areas within Massachusetts from 1980 (when proposition 2 ½ was passed), through 2008. While our examination of this dataset found no reason to exclude any one municipality, the absence of data for a range of variables caused us to start our dataset at 1990. It is important to note here that we have tried two separate models in this project. The first used all the data from 1990 to 2006, with a panel dataset spanning 17 years and 351 observations per year. However, this attempt uses fewer variables in Z_{it}. In addition, in order to avoid the potential effects of the financial crisis, we ended this dataset during 2006. The second removes around ten years from the sample, starting the dataset in 2000. In this attempt, we extend the regression to 2009[3]. This allows us to include a number of municipal area variables, which will be explained further below.

Local Characteristics

The available literature suggests that there are three general categories that might effect the complex decision to hold or not hold an override vote: political, fiscal, and demographic. Political factors can be defined as variables that effect the political environment of a given region. Previous literature has claimed that a number of these – election year, party politics, unity of party control over different branches – influence the use of tax policy tools (like state lotteries). If we view the override vote as a tax policy tool in a similar vein, such measures should be considered as independent variables (Besley and Case, 1995, Brown and Rork, 2005). However, despite the relevance of these factors, they were not available for all cities and for all years, and the decision was made to leave them out of the dataset.

Fiscal variables are designed to control for fiscal stress inside of a municipality, which would pressure a local government to use this policy tool (Alm et al. 1993). This group of variables seems important, given that the difficult decision to attempt to raise taxes should be made only if a government actually needs additional funding, given the political and economic cost of increased taxation. There are several ways to measure this stress. First, there are changes in city-level expenditures. In our dataset, we have total expenditures, and a number of other expenditure categories, both in total and change over time.[4] The theory is that an increase in expenditures from

[2] There are some variables that are folded into fixed effects that the literature suggests should be included in the regression as local characteristics (political leaning, demographics, etc…) but are left out due to data constraints.

[3] Given potential concerns about a break in the data at the financial crisis, we will re-run the 2000 – 2009 regression split at 2007 to check for radical different results.

[4] General government, safety (police, fire, other safety), education, public works, health, recreation, debt, fixed costs, interest payments and other

one period to the next indicates fiscal stress or an increased need for income, which the override vote provides. In addition, the change in total aid coming from the state of Massachusetts should be another indicator of fiscal stress. Finally, local debt levels will be observed as well, the notion being that an increase in debt. Therefore, state fiscal factors will include changes in debt, aid, and expenditures.

The final set of variables cover the general demographic characteristics of the local area. These include unemployment, per capita income, education, age, and a range of other. Unfortunately, for most of these variables the data was only available for 2000-2010, and here the reason for the separation of our project into two different regressions emerges. In the first attempt, it was decided that the extra ten years were more important that the additional variables. While their impact is theoretically present, it could be that it is not worth the loss of some large portion of our observations. We do have data on the proportion of the population over 65 and the proportion of the population under 18 for all time periods. These indicators of the elderly and youth populations is important, as both segments have particular needs that are more expensive to provide then for the 19-64 year old slice of the population. Our second regression leaves out data prior to 2000, and includes variables for proportion of the population that is non-white, female, unemployed, college educated, and the per capita income of the city. For this regression, we also expanded the dataset to include 2007-2009, on the assumption that year fixed effects should control for the financial crisis.

In summary, the Z_{it} in the model will be different in the two time periods. For the regression from 1990 – 2006, we begin with include changes in total debt, state aid, expenditures, proportion of the population that is young and the proportion of the population that is old. For the regression from 2000 – 2009, we start with changes in total debt, state aid, expenditures, and the non-white, female, unemployed, in kindergarten population, proportion young and old, and the per capita income of the city. See Tables 1 and 2 below for summary statistics.

<u>Fixed Effects</u>

In the simplest terms, a fixed effect is an effect over space or time that is constant (or nearly so) for that dimension. In our case, a fixed effect is either a set of characteristics – not accounted for in the model – that is constant for a given year or for region. It is worth discussing to what degree fixed effects captures demographic or political variables. To begin with, there are clear trends from year to year that reflect nationwide political change, which seems to argue that the year fixed effects would capture some political variables, and political structures should fall under municipal fixed effects. In the same vein, depending on the variability in racial or wealth inside of a city, municipal fixed effects should account for some portion of the demographic characteristics. However, the fixed effects will be at least imperfect as a measure for most variables, that that should be taken into account.

One large question that we faced in our econometric analysis was whether to use city fixed effects (one dummy for each locality) or to group the localities together and have a fixed effect for a larger geographic region (one dummy for each county). We choose the latter for several reasons. First, it is closer to the path taken by the literature, which used state fixed effects. It is more likely that larger regions like counties will share certain characteristics (demographics, etc…) over time than individual towns. Secondly, using 351 fixed effects subtracts from the total degrees of freedom, which in this case can be avoided. Third, our econometric ran into some trouble preforming municipal fixed effects, and spit out a series of errors that seemed to indicate correlation troubles between some – if not all – of the fixed effect dummies and the other variables. These errors did not occur when we used fixed effects for counties.

It is important to note that fixed effects estimation – present in every article using spatial analysis we have read – requires a large number of observations, part of the reason we were hesitant to eliminate 1990 – 2000. However, the potential effects of the relevant demographic variables are worth considering, even at the cost of the additional degrees of freedom. At the very least, having the two models side by side will provide an additional test of robustness. [5]

Table 1: 1990 – 2006

Variable	Obs	Mean	Std. Dev.	Min	Max
vtyear	5967	.190548	.3927662	0	1
rchtotdebt	4671	29.64322	191.5626	-100	5966.176
rchextotal	5966	25.03907	618.6655	-99.63837	43824.38
rchatotal	5966	126.2174	4221.035	-99.93317	308461.4
oldper	5947	13.20392	4.493377	2.7	36.4
youngper	5947	24.39129	3.814474	8	33.6
wvtyrtotal	5967	.5498967	1.10728	0	15.5
wvtsuc	5967	.2112129	.501306	0	9

Table 2: 2000 – 2009

Variable	Obs	Mean	Std. Dev.	Min	Max
vtyear	3510	.1586895	.3654383	0	1
rchtotdebt	1993	25.05266	148.6411	-100	2628.677
rchextotal	2457	3.262837	9.32825	-57.42494	171.2604
rchatotal	2457	-.1513474	14.71928	-71.66674	290.942
oldper	3500	13.37471	4.520766	3.7	36
youngper	3500	24.73014	3.941189	8	33.6
notwhite	3510	2496.547	14604.21	0	254457
female	3510	9374.02	19386.07	36	305553
unemployed	3510	2202.829	12018.93	3	119256
kindergarten	3490	247.7765	515.1067	0	7934
ownerhouse	3510	4296.444	5774.47	17	77226
percapinc	3490	27309.42	8647.693	13360	79640
wvtyrtotal	3510	.3418197	.7185647	0	14.5
wvtsuc	3510	.1780428	.4295552	0	7.5

SPECIAL CONSIDERATION: SPATIAL ECONOMETRICS

Until the advent of the mid-twentieth century, econometric technique was unable to capture the rich and dynamic relationships existed across physical space. In his seminal 1954 paper, *On Stationary Processes In The Plane*, Peter Whittle constructs the theoretical framework upon which spatial econometrics is built. Whittle co-opts the stationary processes prevalent in time series econometrics and co-opts them to model how physical proximity may affect econometric results. He builds upon the foundation provided by time-series econometrics by extending the dependence process from Euclidean one-space (i.e. a line) to Euclidean two-space (i.e. a plane). Whereas time interdependence

[5] For more information on the creation of fixed effects, see Appendix 1.

can only travel in one direction (past values can affect future values, but *not* vice versa), spatial interdependence can travel in *multiple* directions.

Out of Whittle's work grew two major concerns about how spatial processes may affect regression analysis: *spatial dependence* and *spatial heterogeneity*. Spatial dependence refers to the phenomena that one observation in location *j* may depend on observations located elsewhere, whereas spatial heterogeneity refers to the phenomena that a different regression model may hold for different points in space (LeSage, 1998). These concerns are in no way rare or negligible – many economic relationships economists deal with in theory display some form of spatial interdependence or heterogeneity.

Two common spatial econometric models used to deal with the concerns listed above are *spatial lag* and *spatial error models*. The spatial lag model views the spatial correlation between observations to be substantive and desirable. In other words, the spatial lag model is employed when we believe that the regressand for one location is directly affected by neighboring locations' regressands in a manner that is above and beyond regressors specific to the location in question. The spatial error model views the spatial correlation between observations to be "noise" obscuring the true information in the regressand and attempts to control for spatial correlation such that it does not affect the estimators. In essence, the spatial error model disregards the possibility that the spatial relationship between observations may have useful information within (Ward, 2008).

All of our voting data has been weighted according to a "naïve" spatial matrix, in which the weights are determined by the presence of touching borders. We will employ these variables to determine if neighboring vote characteristics influence any given town's decisions about the override.

METHODOLOGY

There are a number of different ways to approach the question of why municipalities use an override vote as a policy tool. First, we will be using the probit model to examine what affects the presence or absence of a vote (or votes) in a given year. Second, a hazard model will allow us to look at what factors make it more likely for a city to hold their first vote. Third, the Poisson and Negative Binomial models allow us to examine what affects the number of override votes a city holds. Each model allows a different view of the policy process: first in a given year, then looking at the decision to adopt the use of the tool, and finally the total use of the tool over time.

Probit Model

The probit model is used when we have a dichotomous outcome for our dependent variable, in this case, whether a city holds a vote (dependent variable = 1) or not (dependent variable = 0). The model uses a cumulative normal distribution function to predict the effect a change in some independent variable x has on the probability that the dependent variable y will be 0 or 1. Our goal for this regression is to determine what variables affect the probability of having a vote. For example, does having a neighbor vote render a locality more likely to hold an override vote as well? We will be running two probit regressions, one for 1990 – 2006 and the other for 2000 – 2009. This model will have both time and geographic fixed effects.

Hazard Model

Originally developed by biologists to examine time until death, duration models (commonly referred to as hazard models) allow us to examine what characteristics influence the possibility of success.

We will be using a discrete time duration model, since the length of time used in our data is in years, rather than being viewed as continuous. Each city is contributing several entries to one large likelihood function, where each year the city is at "risk" of holding a vote. Once the city holds the vote, they are dropped out of the model. A higher coefficient in this model indicates a greater likelihood of having a vote, or as the biologists originally modeled it, dying. The concerning factor of this model is that – given our starting date of 1990 or 2000, voting has been occurring in some cities since the early 1980s.

<u>Poisson/ Negative Binomial</u>
Poisson regression is the preferred technique for modeling count data. The probability distribution underlying Poisson regression is the Poisson distribution. The Poisson distribution models the number of events occurring in a given time or space interval. Furthermore, the Poisson distribution assumes that there is a known average rate of occurrences per discrete time/space interval and that every event is independent to the time prior to the previous event. We employ the Poisson regression model to determine which factors may influence why a given town may have more votes than another town. The negative binomial regression model is an alternative count-data modeling technique that allows the variance used in the Poisson regression to differ from the sample mean. The importance of this difference will be discussed later in the text.

RESULTS

<u>Probit Model</u>
We begin by looking at table 1 above, reflecting the 1990 – 2006 portion of the dataset. The first important realization is the variable for change in debt – *rchtotdebt* – has approximately 1000 fewer observations than the other variables. We ran into similar trouble with the 2000 – 2009 regarding availability of data. The change in state aid and local expenditure data only goes until 2007, while change in debt is only present until 2006. As such, we will leave debt out all together and run two different regressions for the second time period, one with the change in aid and local expenditure variables, one without them.

Our results are reported below. In total, we ran 5 final probit regressions. Regression 1 examines change in expenditures, change in state aid, percentage young (under 18), and percentage old (over 65), as well as two spatial variables; total votes in a year and total successful votes in a year. In addition, there are fixed effects (not included below) for both year (1990 – 2006 or 2000 – 2009) and county. The results for the first regression show that percentage old, percentage young, and the spatial success variables have statistically significant coefficients. Of those three, by far the strongest effect is for the spatial variable. A one-unit increase in neighbor voting success increases the probability of having a vote in a given year by 4.3%. That is compared to an increase of 0.4% and 0.9% for percentage old and percentage young, respectively. This seems to indicate that, once you control for some fiscal and demographic factors, as well as fixed effects, the successful votes of a geographically close city encourages municipalities to hold their own vote.

The second regression looks at 2000 - 2009, and in addition to the variables already mentioned, includes variables corresponding with female, unemployed, kindergarten, home ownership, as well as per capita income. Four of those new demographic variables are statistically significant but have very weak effects, while the successful voting special variable has fallen to only be statistically significant at the 10% level. One problem with this regression was mentioned earlier in this section – namely, that we are missing several years of observations for change in expenditures and change in aid. We run a third regression without those two variables, and the results align with those of the 1990 – 2006 data. Namely, a one-unit increase in neighbor voting success increases the probability of having a vote in a given year by 6.3%. The four demographic variables remain statistically significant and weak. In addition, the other special variable – which measured neighbor voting, not just voting successes – becomes statistically significant, with a coefficient of 0.031 (3.1% effect).

A potential issue with the 2000 – 2009 timeframe is the inclusion of the beginning of the financial crisis that began in 2007 / 2008. An informed observer might wonder if the inclusion of the early crisis years would reflect a radically different fiscal atmosphere. While year fixed effects might control for this, it is worth examining our dataset with 2008 and 2009 removed. To this end, we re-run regression three without those years. The result of the removal of those two years is that the spatial voting variable (not the success variable) is only statistically significant at the 10% level. In essence, it appears that the removal of the years reduces the effect of neighbor's voting behavior, which raises the question: is this due to a reduction in degrees of freedom or the elimination of the effect of the great recession? While there is no way to approach this question without additional data, it is worth considering. We also ran a regression on 2007 – 2009 for the sake of completeness, but decided that it focused on a different period entirely, and had fewer degrees of freedom.

The result of the probit model regressions is the general indication that the successful votes of a neighboring city increase the chance of a municipality having a vote by between 4.3% and 6.3%. The demographic and fiscal variables are split between statistically significant and insignificant, but always have a weak effect on voting behavior. While we have found some instances of statistical significance at the 5% and 10% levels for the number of votes held in a neighboring state, we are less confident with those results, which indicate that a one unit increase in the number of votes held by neighbors decreases the probability of holding a vote by between 2.6% and 3.1%.[6]

Table 3: Probit Model

VARIABLES	(1) 1990-2006 Change Variables	(2) 2000-2009 Change Variables	(3) 2000-2009 Without Change Variables	(4) 2000-2007 Without Change Variables	(5) 2007-2009 Without Change Variables
rchextotal	0.000	0.001			
	[0.06]	[1.51]			
rchatotal	-0.000	0.000			
	[-1.64]	[0.42]			
oldper	0.004**	0.002	-0.001	0.001	-0.007**
	[2.50]	[1.14]	[-0.35]	[0.60]	[-2.45]
youngper	0.009***	0.003	0.003	0.003	0.003
	[5.40]	[1.19]	[1.64]	[1.24]	[0.92]
notwhite		0.000***	0.000***	0.000***	0.000***
		[3.20]	[4.61]	[3.14]	[2.76]
female		-0.000***	-0.000***	-0.000***	-0.000***
		[-4.10]	[-5.23]	[-4.48]	[-3.11]
unemployed		-0.000	-0.000	-0.000	-0.000
		[-0.13]	[-0.87]	[-0.35]	[-1.05]
kindergarten		-0.000	-0.000	-0.000	-0.000
		[-0.84]	[-1.08]	[-0.30]	[-0.85]
ownerhouse		0.000***	0.000***	0.000***	0.000**
		[3.01]	[3.86]	[3.15]	[2.43]
percapinc		0.000***	0.000***	0.000***	-0.000
		[9.41]	[8.19]	[9.32]	[-0.11]
wvtyrtotal	0.006	-0.022	-0.031**	-0.026*	-0.074**
	[0.80]	[-1.37]	[-2.18]	[-1.65]	[-2.46]
wvtsuc	0.043***	0.050*	0.063***	0.055**	0.095*
	[2.95]	[1.89]	[2.68]	[2.15]	[1.96]

[6] For the full regression outputs For the Probit model, see Appendix 2.

Observations	5,946	2,436	3,480	2,784	1,041

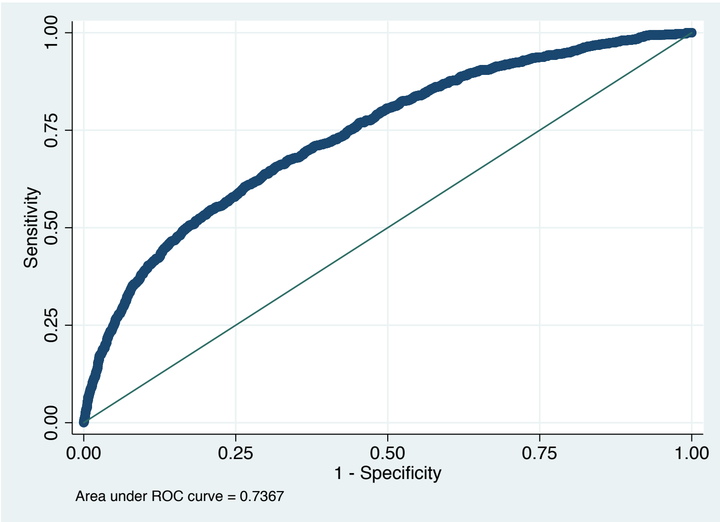
z-statistics in brackets
*** p<0.01, ** p<0.05, * p<0.1

Following these 5 regressions, we proceeded to preform a number of post-estimation tests. The first main test is the *estat gof* test of goodness-of-fit, which computes a Pearson Chi ^ 2 test that examines the observed against expected observations. The first thing to examine in this test is if the number of covariate patterns is near the number of observations. If the number is close (as it is in the example below, from regression 1) then the literature[7] indicates that the data should be grouped into 10 sections, representing ordered probabilities, and observing the results. This is done using the *estat gof, group(10)* command. The result is a new Chi ^ 2 test, called the Hosmer-Lemeshow Chi ^ test. Now, we can say we reject the null hypothesis that the probit model is inappropriate for our data.

```
. estat gof

Probit model for vtyear, goodness-of-fit test

       number of observations =      5946
 number of covariate patterns =      5946
           Pearson chi2(5909) =      5898.16
                  Prob > chi2 =         0.5373

. estat gof, group(10)

Probit model for vtyear, goodness-of-fit test

  (Table collapsed on quantiles of estimated probabilities)

       number of observations =      5946
             number of groups =        10
      Hosmer-Lemeshow chi2(8) =        11.03
                  Prob > chi2 =         0.1998
```

For the four regressions previously discussed, for (1), (2) and (4) we can reject the null hypothesis that the probit model is inappropriate. However, for regression (3) we cannot reject the null hypothesis; a finding that raises questions about the usefulness about that regression. As a reminder, (3) examined 2000 – 2009, and remove the change in debt, expenditure, and state aid as variables.

The second test we ran was based on the *lroc* command, which generated a graph that compared sensitivity and specificity. Sensitivity is the probability of predicting voting among the portion of the population that actually votes. Specificity is the probability of predicting the absence of votes among the portion of the population that doesn't vote. The *lroc* command places sensitivity on the y-axis and 1 – specificity on the x-axis. In essence, a model with no predictive power would show as a 45-degree line originating at point (0,0). Our model is judged by how it curves above that line (depicted below). The numeric form of that measurement is the area under our curve (Receiver operating characteristic (ROC) curve) with 0.5 being no predictive power, and 1 being perfectly predicative.



All four of our regressions have area measurements between 0.76 and 0.79, indicating that we models that are predictive, but not perfectly so.

All in all the post-estimation commands leave us doubtful about regression 3, but generally confident in the conclusion that the successful votes of a neighboring city increase the chance of a municipality having a vote by between

4.3% and 6.3%. We would lean towards the lower estimates, however, given our questions about regression 3, which produced the 6.3% estimate. [8]

Poisson / Negative Binomial Regression Model

Data Remediation

Extensive data remediation was necessary prior to running any Poisson regression. Because our unit of analysis was the township rather than the year, it became necessary to collapse the dataset from having multiple years of observations for each town to a single observation per town (thus reducing the dataset to 351 observations). Of primary concern when collapsing the data was the method by which each regressor would be collapsed:

- *Median*: The expenditure, aid, and debt information were collapsed onto their medians rather than their means. This decision was made due to the presence of numerous observations that were more than two absolute standard deviations away from the mean for each variable. Choosing to collapse on the median abated concerns about the undue leverage multiple outlying points might have had on the model.
- *Mean*: The spatial variables were collapsed onto their means. The spatial weighting technique controls for the presence of high leverage points - hence, there was little to no concern about the possibility for biased means when choosing to collapse the spatial variables in this particular manner.
- *Sum*: The vote count data per year were collapsed onto their sums in accordance with the structure of Poisson regression.

Regression (1990-2006)

Table 4: Poisson and Negative Binomial 1990 - 2006

	1	2	3	4
	1990-2006	1990-2006	1990-2006	1990-2006
	No Fixed Effects	County Fixed Effects	No Fixed Effects	County Fixed Effects
VARIABLES	Poisson	Poisson	Negative Binomial	Negative Binomial
youngper	0.032***	0.035***	0.030	0.042*
	[5.41]	[5.64]	[1.44]	[1.78]
oldper	0.032***	0.031***	0.011	0.015
	[7.78]	[6.66]	[0.65]	[0.81]
rchextotal	0.032**	0.039***	0.026	0.043
	[2.57]	[2.88]	[0.61]	[0.93]
rchtotdebt	0.001	-0.001	-0.001	-0.004
	[0.52]	[-0.66]	[-0.12]	[-0.83]
rchatotal	-0.001***	-0.001***	-0.000***	-0.000***
	[-10.47]	[-10.31]	[-3.72]	[-4.07]
wvtyrtotal	0.118**	0.011	0.398	0.126
	[2.27]	[0.19]	[1.37]	[0.41]
wvtsuc	0.828***	0.640***	0.837	1.177*
	[8.44]	[5.33]	[1.55]	[1.76]
Constant	0.737***	0.610***	0.843	0.442
	[4.03]	[2.94]	[1.30]	[0.59]
lnalpha			0.319***	0.254***
			[3.69]	[2.89]
Observations	348	348	348	348

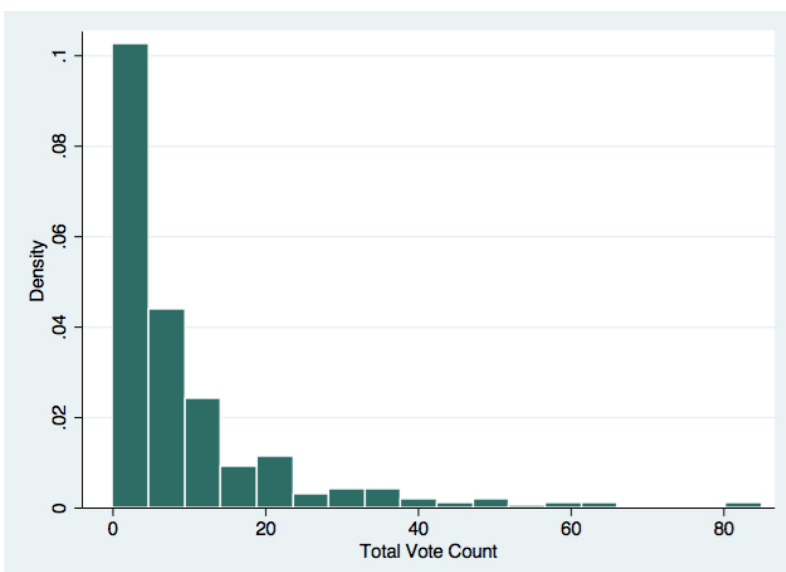
[8] For the full post-estimation results for the probit model, see Appendix 3.

The above regression output table displays four separate regressions run on the same data set containing the collapsed observations for all 351 Massachusetts towns for the years 1990-2006. The regressors employed were chosen due to theoretical consideration about their importance towards determining the rate of occurrence of votes:

- Proportion of minors (*youngper*) and seniors (*oldper*): Economic (and quite possibly sociological) theory suggests that the proportion of minors and seniors in any given town's population will have an effect on the vote occurrence rate. Namely, we may cities with higher proportions of minors to have override votes more frequently due to constituency concerns about school funding. Cities with higher proportions of seniors may either have more override votes due to constituency demand for more public good provision, or less override votes due to constituency age-related apathy.
- Changes in total expenditure, aid, and debt: Economic theory suggests that the financial indicators for a given city may drive a higher rate of override votes. Fiscal stress may pressure local governments into voting to override the 2 ½% debt ceiling levied by the proposition.
- Spatial variables: Spatial analysis lends an interesting perspective into how relationships across space may affect the rate of voting in a given city. The spatial variables capture spillover and influence effects that ordinary variables may not be able to capture.

County fixed effects are included in two of our regressions to control for demographic, financial, and political data that is constant over counties. Ultimately, regression (4) was chosen as the true regression for analysis. There were several reasons for this choice:

- *Overdispersion*: We call a regressand overdispersed if there exists higher variability than would be expected given a distribution model. The Poisson distribution is an extremely simple parametric model that assumes that the predicted variance is equivalent to the predicted mean. Overdispersion is a problem because although overdispersion does not render our estimators inconsistent, overstating the significance of our regressor and may bias the coefficients (Breslow, 1984). We start by examining a histogram of our regressand (sum *vtyear*):



The histogram implies that we may be experiencing overdispersion. Notice how the density of the total vote counts is heavily skewed towards the left. We examine the summary statistics for our regressand:

Mean: 9.33
Standard Deviation: 13.07
Variation: 170.98

It is quite clear that the assumption that our predicted variance and predicted mean our equivalent is violated, thus rendering the Poisson distribution a poor choice for modeling our estimators. This result is also confirmed by our post-estimation goodness of fit tests following our Poisson regressions:

TEST REGRESSION ONE - H_0 : Data are not overdispersed | REJECT at 99% confidence level [Deviance χ^2 - 3606.59/Pearson χ^2 - 4548.29 | P-Value: 0.00]
TEST REGRESSION TWO - H_0 : Data are not overdispersed | REJECT at 99% confidence level [Deviance χ^2 - 3400.94/Pearson χ^2 - 4049.47 | P-Value: 0.00]

The negative binomial regression model is an alternative modeling method for count data. Unlike the Poisson regression model, the negative binomial regression model does not suffer from the same problems due to overdispersion. The negative binomial distribution improves upon the Poisson regression model by implementing an extra parameter that allows for the sample variation to differ from the sample mean. Because our data displays the problem of overdispersion, we choose to employ the negative binomial regression model. The extremely significant *lnalpha* reported by both negative binomial regressions strongly imply that the Poisson distribution was not a good fit for the data.

- *Fixed effects*: It was determined that including county-level fixed effects were justified due to the overwhelming evidence given in the literature and the theoretical evidence that shows that variations in spending and demographic patterns happen within each county subunit.

Unlike some other limited dependent models, we can actually meaningfully interpret the regression coefficients in a negative binomial distribution model. Because we are using a negative binomial distribution, we interpret the coefficients by first exponentiating them and then treating them as factors:

a) A one percent increase in the proportion of minors results in a 1.04 times higher expected total override event count
b) A one unit weighted successful vote increase results in a 3.24 times higher expected total override event count.

Table 5: 1990 – 2006 Marginal Effects and Incedent Rate Ratios

	1	2
	1990-2006	1990-2006
VARIABLES	Marginal Effects	Incidence Rate Ratios
youngper	0.313*	0.043*
	[1.78]	[1.79]
oldper	0.111	0.015
	[0.80]	[0.80]
rchextotal	0.316	0.043
	[0.93]	[0.93]
rchtotdebt	-0.026	-0.004
	[-0.82]	[-0.82]
rchatotal	-0.003***	-0.000***
	[-4.16]	[-4.05]
wvtyrtotal	0.907	0.123
	[0.40]	[0.40]
wvtsuc	8.825*	1.199*
	[1.78]	[1.80]
Constant		0.468
		[0.62]
lnalpha		0.255***
		[2.90]
Observations	348	348

For the sake of interpretation, we examine both the marginal effects and the incidence rate ratios given by the regression. In particular, we note that the only statistically significant regressors are the proportion of young people in a given city, changes in aid, and the number of successful votes neighboring towns had. Interpretation of our marginal effects columns shows that:

a) For each one percent increase in the proportion of minors in a town raises the expected number of override vote events by a factor of 0.313. Hence, we expect that a 6~8% change in the proportion of minors in a town may cause the expected number of override votes to double – this result is significant at the 90% confidence level.
b) Neighboring cities who have successful override initiatives will increase the expected number of override vote events by a factor of nearly 8.825. This result is dramatic and suggests that

The incidence rate ratio is an epidemiological concept that explains the *rate* at which new events occur. In our analysis, this means we are examining the rate at which cities are having new override vote events. Similarly, interpreting the incidence rate ratios column shows that:

a) Each one percent increase in the proportion of minors causes the rate at which override vote events occur to increase by a factor of 0.043. This result is significant at the 90% confidence level.
b) Successful override initiatives in neighboring cities causes the rate at which a given town has override vote events to increase by a factor of 1.199. This result is significant at the 90% confidence level.

Regression (2000-2009)

Table 6: 2000-2009 (FE)

	1	2
	2000-2009	2000-2009
VARIABLES	No Fixed Effects	County Fixed Effects
youngper	0.022	0.032
	[0.99]	[1.28]
oldper	0.009	0.011
	[0.56]	[0.55]
rchextotal	0.050	0.087**
	[1.30]	[2.04]
rchtotdebt	-0.004	-0.007*
	[-0.86]	[-1.67]
rchatotal	-0.021	-0.030
	[-0.80]	[-1.12]
wvtyrtotal	0.539*	0.319
	[1.75]	[0.99]
wvtsuc	0.723	1.038
	[1.27]	[1.48]
Constant	0.898	0.464
	[1.35]	[0.59]
lnalpha	0.365***	0.294***
	[4.23]	[3.34]
Observations	343	343

We repeat the regressions for our second block of time analysis with the same variables. When analyzing the time period in between 2000-2009, a different set of variables become significant. Again, the model with fixed effects is chosen for reasons discussed above. For the sake of interpretation, we examine both the marginal effects and the incidence rate ratios given by the regression:

Table 7: 2000 – 2009 (Marginal Effects and IRR)

	1	2
	2000-2009	2000-2009
VARIABLES	Marginal Effects	Incidence Rate Ratios
youngper	0.250	0.032
	[1.27]	[1.28]
oldper	0.083	0.011
	[0.55]	[0.55]
rchextotal	0.687**	0.087**
	[2.02]	[2.04]
rchtotdebt	-0.056*	-0.007*
	[-1.66]	[-1.67]
rchatotal	-0.234	-0.030
	[-1.11]	[-1.12]
wvtyrtotal	2.511	0.319
	[0.99]	[0.99]
wvtsuc	8.162	1.038
	[1.47]	[1.48]
Constant		0.464
		[0.59]
lnalpha		0.294***
		[3.34]
Observations	343	343

Interpretation of these results leads to the following conclusions:

a) Each one percent increase in the change in total expenditures raises the expected number of override vote events by a factor of 0.687. This result is significant at the 95% confidence level.
b) Each one percent increase in the change in total debt lowers the expected number of override vote events by a factor of 0.056. This result is significant at the 90% confidence level.
c) Each one percent increase in the change in total expenditures causes the rate at which override vote events occur to increase by a factor of 0.087. This result is significant at the 95% confidence level.
d) Each one percent increase in the change in total debt causes the rate at which override votes occur to decrease by a factor of 0.007. This result is significant at the 90% confidence level.

Overall Interpretation

The negative binomial model seems to imply a few fundamental facts about the factors that influence the rate at which override events occur and ultimately why a certain city will have *x* number of override events:

a) The success of neighbors has a powerful effect: Some of our most dramatic (and significant) coefficients are found for the weighted successful override regressor. This result suggests that city governments receive "inspiration" from neighboring towns that have successful overrides and then decide to have a few overrides themselves
b) Weakness of financial factors: Many of the financial variables are either insignificant or have negligible effect. This suggests that city governments rarely consider their financial straits when deciding to have an override vote.
c) Overall fragility/weakness of regression models: The regression models all seem to be fragile to various specifications and suggest that accurate prediction/modeling of vote count data is difficult.

<u>Hazard Model</u>

Table 8:
Hazard Model

	(1)	(2)
	1990-2006	2000-2009
VARIABLES	Fixed Effects	Fixed Effects
youngper	1.00	0.981***
	[0.25]	[-2.62]
oldper	0.992	0.983**
	[-0.73]	[-2.33]
kindergarten		1.00*
		[1.74]
female		0.999
		[-0.14]
notwhite		1.000
		[1.38]
ownerhouse		1.000
		[0.87]
wvtyrtotal	1.02	1.15***
	[0.55]	[2.73]
wvtsuc	1.13	0.714**

	[1.66]	[-2.18]
rchextotal	1.00	
	[0.50]	
rchatotal	0.99	
	[-1.49]	
Observations	1,367	824

Hazard ratios given, robust z-statistics in brackets
*** p<0.01, ** p<0.05, * p<0.1

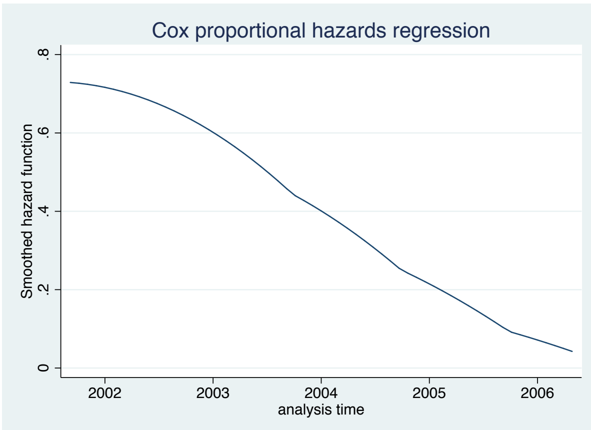
For our hazard model, we ended up running two regressions: one running from 1990 to 2006 featuring spatial variables, changes in real expenditures (both total and aid), as well as the percentage data for the elderly (>65 years) and the young (<18 years). The other regression ran from 2000 to 2009, and included a number of municipal feature variables (female, notwhite, ownerhouse, and kindergarten). We did remove both changes in real expenditure and real state aid variables from our second regression due to issues with multicollinearity and a lack of data for those variables after 2007. For our 1990 to 2006 model, we were unable to include any of the municipal features due to a lack of data (these variables only go back as far as 2000).

When examining our regression output table, keep in mind that the coefficient values listed for each variable are hazard ratios. They are to be interpreted as a ratio of the hazard rate between the treatment and a control group, where the treatment is including the variable being examined, and the control is without it. A hazard ratio close to or equal to 1 therefore, implies that there is no difference on "survival" if you include the given variable or if you don't. Less than 1 implies that there is a lower chance of the given event (in our case, a vote occurring, literature usually simply refers to the event as "death") happening with that given variable. Conversely, a hazard ratio greater than 1 implies that there is a higher chance of the given event happening with that given variable. Our regressions were also run with robust estimates of variance; doing so reduces our standard errors. The best explanation for our rationale comes from Stata, "The robust variance estimator uses a one-term Taylor series approximation. In linear regression, the coefficient estimates, b, are a linear function of y. Thus the one-term Taylor series is exact and not an approximation. For logistic regression and other MLEs, the ignored higher-order terms in the Taylor series are nonzero. So it's truly an approximation in these cases." (Bill Sribney, Stata Corp., 1998) Finally, like all the other regressions, included in both these hazard regressions are fixed effects for time and county; they are excluded for parsimony.

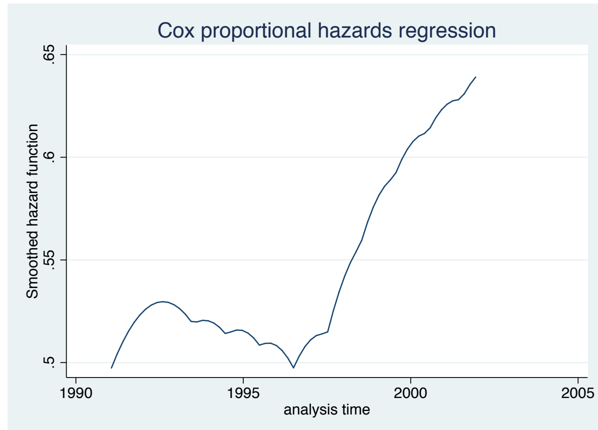
Examining our first regression, very little can be gleaned from it. None of our variables are considered significant, and the hazard ratios are also all at 1.00 or close to it, implying that none of the variables have any more of an effect on whether a municipality holds a vote than if there was no variable there.

Our second regression had much better results, with several variables, most notably our spatial variables, being both statistically significant and also not ~1.00. Both youngper and oldper imply that having higher per capita amounts of youth and the elderly slightly decreases the chance that a city will hold a vote (or rather, that the city will "survive" slightly longer compared to not having said variables). Most interestingly, our spatial variable wvtsuc implies that a city is *less* likely to hold a vote if a neighboring county has held a successful vote. Further, wvtyrtotal implies that a city is *more* likely to hold a vote the more a neighboring county has held a vote. [9]

2000-2009



1990-2006



For both regressions, running a post-estimation test of the assumptions of the Cox proportional hazards model, *estat phtest*, we reject the null hypothesis of the test, which implies that there is no evidence against the assumptions of the Cox proportional hazards model (primarily that we need to have constant hazard ratios across time). We also plotted our hazard function over time, and for our 2000-2009 model, we see a mostly linearly declining function as time moves from 2000 to 2009. This makes sense intuitively, as time increases, we have fewer municipalities that are not considered "dead" (in this case, having voted already), and our spatial variable wvtsuc implies that as more of your neighbors have called a vote, the less likely your municipality is to call a vote. Examining the plot of the hazard function over time for our 1990 to 2006, we see the hazard ratio increase at first then drop, then skyrocket up. However, it's hard to draw any conclusions from this since no element of that model was remotely statistically or economically significant.

DISCUSSION AND POTENTIAL ISSUES

Summary of Results

Probit: After running four models from 1990 – 2006 and 2000 – 2009, there seems to be a general trend in our data that having a neighboring region vote successfully increased the probability that the region in question will hold a vote in that period. However, the post estimation analysis poses questions about the validity of one of our models, which generally makes us more hesitant about the results. However, we are still cautiously optimistic that successful votes of a neighboring city increase the chance of a municipality having a vote by between 4.3% and 6.3%.

Hazard: Our results indicate that over the period 2000 to 2009, having a neighboring region vote successfully decreases the probability that your municipality will hold the first vote. Percentages of youth and the elderly also had negative effects on the probability of holding a vote, but the magnitude and size of the hazard ratio is so close to 1 that they can be considered negligible.

[9] For the full regression output for the Hazard Model, see Appendix 4.

Poisson: Our results indicate that the only regressor that seems to have a significant and large effect on override event occurrence rates and numbers is the number of successful override votes your neighbors had. Otherwise, the negative binomial / Poisson model has very little prediction power.

Spatial Variables:

The past literature that has made use of spatial variables has followed a number of conventions that we were unable to replicate here due to data and time limitations. The first of these is the use of multiple different weighting mechanism for geographically contiguous regions, as seen in Case et al., Bruce et al., and several other papers. The four most common weights are *contiguity* (which we have used), *population-contiguity*, *center*, and *city*. *Population-contiguity* makes it such that the contiguity effects are weighted based on the population of the neighboring areas. *Center* creates a weighted variable based on the distance from the center of one region to another neighboring region. *City* does the same, but measures the distance between the two regions' largest cities. Each of these measures a different relationship between regions. Others that could be considered in the framework of a policy discussion would be demographic or political similarity (New York and California, while geographically distant, would effect each other given certain similarities), but these are not the focus of the current discussion. Given limited time and limited data, we choose to just use *contiguity* weighting of spatial variables. We are not overly concerned by this omission because of the clear pattern in the literature that the results from one weighting system (*contiguity*) are mirrored in the other three (*population-contiguity*, *center*, and *city)*. This can be seen in a number of texts {Brown and Rork, 2005} {Bruce et al., 2007} {Case et al., 1993}.

One additional convention has been a test run by most authors we read that checks to see if the use of any generic spatial variable is significant. In essence, we would create a weighting system for spatial variables with absolutely no meaning. We could do this by weighting regions based on their spatial relation in the alphabet. The nearest 2 (or 3, 4, etc…) regions would be treated as if there were, for example, geographically close to one another. The idea behind this is to check to make sure the effect is coming from the weighting measure we choose and not the presence of a spatial variable at all. Unfortunately, due to the same time and data constraints, we chose to not do this test.

CONCLUSION

Data Issues:

Just to briefly summarize data issues that we have already addressed a number of times over the course of this paper, the absence of data forced us to not use political and municipal factors. In order to use what limited demographic factors available, we were forced to split our regressions into two time periods, complicating the three existing models and our overall analysis. In an idea world, our panel would be complete, and we could have begun our analysis at the inception of Proposition 2 ½. This would allow the hazard in particular to function better, as we could start at a year without any prior override votes.

Future Issues:

We would encourage future researchers to attempt to collect additional data in order to complete this dataset and further the analysis. This would involve collected municipal data from the mid – 1980s to the mid 2000s, and in addition adding political data, such as party control, government structure, and voter political culture. We would also encourage the use of multiple weighting systems, and continued attention paid to spatial variables, especially variables related to neighboring voting successes, which our data seems to indicate is important and relevant.

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