Estimating Education Quality Capitalization Using Time Series Modelling

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Defended on 4/4/2017 to:

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Abstract

This paper attempts to determine the degree to which variation in property values can be explained by variation of the quality of the public school district in which the property is located using time-conditional econometric modelling.

Introduction

It is unclear why the average citizen should keep abreast of education policy. For one thing, obtaining information on education policy is difficult. Some decisions are made at the national level, some decisions are made at the state level, and most are made at the local level. This diffusion of responsibility increases the costs of obtaining information. This is not to say that the issue is unimportant; there are many reasons why a society would care about education policy. In fact, most disciplines have a different theory about why education is a good thing. A macroeconomist might point to the innovation driven Solow-adapted models, and claim education is important because it fuels innovation and GDP growth. A microeconomist might point to uninternalized social benefits educated individuals have on society. Political Scientists may claim that education provides the intellectual tools necessary to be a discerning citizen capable of making good political choices. Sociologists might claim that public education provides a means through which young children can be socialized and understand societal mores. This is not an exhaustive list of the reasons, nor are they all watertight reasons. The point however, is that many of the reasons stated to justify caring about how well someone else's kid is educated tend to be abstract and societal rather than concrete and personal. However, this paper examines a subject which gives the average citizen a good reason to care about education policy: It might affect his property value.

This paper will discuss the degree to which Tiebout sorting is driven by local education quality, and more specifically the degree to which local education quality is capitalized into local property values. The Tiebout model is a unique way of analyzing the effect of education on the economy, because actual quality of education does not affect the model. Rather the effect is caused by perceived quality of education. This a useful fact; it allows us to entirely bypass most of the normative questions about what a quality education entails, and how accurately standardized tests measure it.

Previous research

Perhaps the most general form of this type of analysis is a method of non-market valuation known as hedonic price estimation, or sometimes, simply hedonics. The purpose of hedonics is to either associate a price to a specific level of a non-market

attribute associated with a good, or, more generally, to estimate a demand curve for all levels of a non-market attribute associated with a good. The theory is that for many non-market goods or attributes, consumer preferences for these goods or attributes will be capitalized into the price of a related good or service. An estimation of the demand for the non-market attribute is achieved through some form of statistical regression. In its simplest form, a dummy variable would be added to a regression to indicate presence or absence of an attribute. More complex examples involve regressing the price of the related good or service against the level of the attribute and other explanatory variables (Aizcorbe 2014). For the sake of an example, if an economist wished to use hedonic estimation to determine the effect that noise pollution from a highway had on nearby property values, she could model the property value as the dependent variable, and model distance to the highway as the primary explanatory variable, with other control variables, such as square footage and number of bedrooms being also present in the model.

Hedonic estimation is diversely applicable; it is used in courts to estimate damages in cases of environmental damage or monopolistic practice. It is used in the real estate and health industries to estimate prices. It is even used by the BLS while computing CPI to partially compensate for the changing quality of goods in the baskets it analyzes (Aizcorbe 2014). This wide use is largely due to the simplicity of the theory, which, stripped to its bare bones is: markets will take full account of all attributes of a good when determining price. There is no reason this method cannot be applied to the capitalization of education quality into the price of housing. This paper could easily be written, with only slight changes to methodology, on hedonic modeling instead of Tiebout modeling. While Tiebout begins from a different point, the implications of his model, and much of the subsequent research on it is highly reminiscent of the hedonics literature. I choose to analyze this question within the framework of Tiebout because I believe that the logic of the model is much more in line with the thought process of consumers making the choices, and because the literature surrounding Tiebout modeling provides a much richer context in which to discuss the implications of consumer choice.

Any discussion of local service provision must involve the work of economist Charles Tiebout. This is not because his *A Pure Theory of Local Expenditures* was the first writing on the subject, far from it. However, the influence of his work was large enough to require everything that came before it to be justified to the theory. Tiebout's Model originated as a solution to the two problems of preference revelation and preference aggregation in local governance. The problem of preference revelation is that citizens have an incentive to over-represent their desire for services which they could potentially free-ride on, but under-represent their willingness to pay for these services. Some examples of this might be spillover effects, where one jurisdiction implements and pays for a policy which benefits citizens in neighboring jurisdictions, or cutting public funding in hopes that private donors will pick up the bill. The problem with preference aggregation is much more intuitive. If every citizen has a different policy preference, not matter how slight the difference, the odds of more than a few citizens being perfectly happy with the policy which gets implemented is very small. Tiebout's solution to this problem is simply that there is no solution which needs to be implemented -- the system works just fine on its own. This is because constituents are

mobile. It is far more efficient for them to move to a community which already has policies in place which they prefer than to try to change the rules of the municipality they currently live in. This solves preference revelation because it makes it unnecessary; voters reveal their preference not when they are asked, nor when they vote, but when and where they move. This solves preference aggregation because the mobility effect has a strong homogenizing effect on the preferences of those within the municipality. The model describes a situation in which local governments provide a bundle of services at the cost of a vector of taxes. In effect, it creates a market for the provision of public services (Tiebout 1956,p 418).

This is also why the effect only works at the local level; the effect is entirely reliant upon an assumption of mobility, an assumption which is weakened over distance. Tiebout explicitly lists and numbers six other assumptions in his model. To summarize these assumptions: 1) Consumers are mobile. 2) Consumers are assumed to have perfect information about communities. 3) Consumers have many options when selecting a community in which to live. 4) Consumers live on dividend income, and thus do not face employment restrictions. 5) Public services exhibit no economies or diseconomies of scale. 6) Communities set policy according to the preferences of current members, and there is an optimal community size. Finally, 7) Communities above optimal size attempt to downsize, while communities below optimal size attempt to attract new citizens (Tiebout 1956, p419). A great deal of analysis exists which attempts to discern both the degree to which the effect is real, and the degree to which these assumptions hold.

There are conflicting opinions on which of the assumptions are robust to the world. I intend to focus on the first four assumptions. This is not implying that I find the other assumptions to be irreparably far from reality -- I take no stance on their reasonableness, they are simply not directly necessary to the theory as I intend to apply it.

The clearest obstacle to the mobility assumption is employment, which is why Tiebout assumed employment away in his original theory. The assumption of dividend income and no employment is perhaps the assumption which most bluntly diverges from reality, and it does so in a dramatic fashion. The Bureau of Labor Statistics puts labor force participation rate in December of 2016 at 62.7 for all civilians 16 years of age or older. This means that roughly 63 percent of adults are either employed or seeking employment. If possible the assumption of no employment restriction should be relaxed. It is true that employment restrictions dampen mobility; they require either movement to a location within commuting range, or a career change. The former limits choice, the second imposes a cost to relocation. There are of course mitigating factors; such as the ability to work from home, or transfer programs. However, work from home is still relatively infrequent, and transfer programs only allow movement to other areas in which the company has a presence. However, while employment does serve to limit mobility, it is likely not by very much. As long as a consumer is willing to accept a reasonable commute, in most metro areas she will have a large number of municipalities to choose from within the circle scribed by the radius of what she deems an acceptable commute. Perhaps this is reason to suspect that Tiebout sorting is more effective in urban than rural areas. In fact, employment can drive mobility as individuals relocate to be closer to a job they were just hired for, or as companies request

employees to relocate. To illustrate this, according to the US Census Bureau, 20.2% of movers in 2016 stated their reason for moving as “employment related”. Regardless, the main challenge to relaxing the unrealistic dividend income assumption is the strain it places on mobility, but this strain is not sufficiently burdensome as to, by itself, render the mobility assumption unrealistic.

It should be noted that nowhere in the literature does anyone assume relocation is free or frictionless, nor, is the argument made that relocation does not occur. All discussion on the mobility assumption is a matter of degree. The effect describes a competition driven incentive which can still occur as long as there is some mobility. The concern is that if mobility is constrained enough, the Tiebout effect may be so slight as to not a have a practical impact. With this in mind, there are other, more problematic, concerns about the mobility assumption. Firstly, homeowners are subject to market fluctuations (as will be described later in the methodology section, this is one reason to use a time series model with dependent variables). A homeowner in a down market is disincentivized to sell. This puts a time conditional dampener on mobility. There are other time-variant determinants of mobility. As stated earlier, one major reason for relocation can be employment. However, employment based mobility likely also depends on the strength of the economy. It is not entirely clear the net direction of this pull. Employment relocation might be stronger in strong economies, as decreasing unemployment causes more new hires to move to be in proximity to their new job, or employment relocation might be stronger in poor economic cycles, as individuals attempt to follow jobs to more prosperous areas, or are capable of moving to better areas following depressed housing prices.

While these theoretical considerations are important, especially in terms of research design, a simpler look at empirical data may be more useful in determining the actual degree of mobility. Census data does show that mobility occurs, listing the mobility rate for 2016 at 11.2%. There are some relevant demographic trends which are more revealing than just a simple population average. Assuming that the 2015-2016 mobility trends are reasonably generalizable across time, mobility forms a U shape across age, with mobility peaking from age 20 to age 30. Unemployed individuals are slightly more likely to relocate than employed individuals. Renters are much more likely to relocate than homeowners, and parents with children under 6 are more likely to relocate than any other category of parent, including non-parents. This effect seems to diminish as the age of the parent increases. However, on the whole, given the surprising fact that more than 1 in 9 Americans moved last year, it would seem that low mobility is unlikely to be a significant barrier preventing Tiebout competition.

One of Tiebout’s original assumptions is that consumers have perfect information about municipalities which they might consider moving to. This seems like an overly bold assumption. Even Tiebout admitted as such in the original paper, saying “Assumptions 1 and 2 should be checked against reality. Consumer-voters do not have perfect knowledge and set preferences, nor are they perfectly mobile. The question is how do people actually react in choosing a community (Tiebout 1956, p423).” It seems absurd that consumers would be aware of every tax policy and every service provided in the municipality in which they live, let alone the tax policies and services provided by the municipalities in which they could potentially live. However, the degree to which imperfect information is a problem is almost entirely dependent on consumer

preferences. If consumers dislike each type of tax equally, and like each potential service equally, imperfect information is a massive obstacle. If consumers have a small cluster of taxes and services which they care far more about than other taxes and services, acquiring sufficient information to make a choice is decidedly cheaper. Alternatively, if a consumer uses certain prominent and easily researchable services to heuristically represent the quality of other services in a municipality, that consumer only needs accurate information about the benchmark services to make a decision.

Preferences differ by consumer, meaning that not all consumers will evaluate municipalities identically. There should still be certain services which are more important to more people than most others. Additionally, it is assumed that all citizens are tax averse, although the degree to which they are tax averse may vary by person. The best way to measure these differences, according to Tiebout himself (1957) is through survey data, or micro-level data. Micro-level studies come in two flavors, entry and exit. As may be obvious from the name, entry studies examine pull factors, which induce consumers to move to a certain area, while exit studies examine push factors, which induce consumers to leave a certain area. In a literature review, Keith Dowding, Peter John, and Stephen Biggs (1994), examine some of the conclusions of the micro-level data. For example, they note that there is a lack of consensus about how accurately voters understand which level of government provides their services, as well as which level of government voters credit as being most important (Lowrey and Lyons, 1989; Percy and Hawkins 1992). As Dowding et al. note, most micro-level studies measure satisfaction or dissatisfaction, which is a step abstracted away from actual movement (p786).

This paper is about the degree to which education can drive sorting, thus it is useful to consult the micro-level data pertaining to what relative weight consumers place on education. Bayer and McMillan find a slight preference for education, but one which is overshadowed by neighborhood demographics and the year in which the house was built (2012). Percy, Hawkins, and Maier, in a survey of recent movers to the Milwaukee area, found that the rate at which respondents considered education a significant part of their decision varied considerably based on location, ranging from 12% to close to 50% (1995, p10). Bickers, Stein, and Salucci when studying push factors from major metropolitan areas also find significance for education, but the relationship was weaker than expected (2006). While significance is almost universal, the literature is not entirely clear on the strength, and there is also a slight suggestion of high variance by region.

The approach in the literature most similar to the one I take is known as the capitalization approach. Much like its name suggests, capitalization studies attempt to determine the effect that taxes levied by a municipality, or public services delivered by a municipality, have on property values within the municipality. The standard approach, first taken by Oates (1969, p965-967) is to regress property values against the physical properties of the home, such as the square footage, along with other metrics such as property taxes, or annual expenditure per pupil in public schools. Most, but not all capitalization models seem to diverge into two camps, those studying the capitalization of taxes, and those studying the capitalization of public services. Many studies use expenditure per pupil as a measure of school quality. I consider this a mistake. Expenditure per pupil is a poor predictor of school performance (Grubb 2006), and I

expect consumers are much less concerned with or aware of expenditure per pupil than they are of test score or graduation rates.

There is a school of thought which suggests that this type of analysis is incorrect and actually finds for a lack of Tiebout sorting. Edel and Sclar argue that evidence of capitalization is in fact evidence of a disconnect between market equilibrium and actual output on the supply side. Their theory is that if municipalities are undergoing Tiebout competition, prices should equalize with benefit; this, to them means that decreasing capitalization over time is a movement towards a market equilibrium as opposed to a movement away from a market (p944-948). Their criticism of the Oates approach is that, "Oates… simplifies the Tiebout model into a statement about consumer demand only, with no supply side made explicit, and no optimality issue raised…. His results, therefore, are never compared with the results predicted from a full Tiebout model. They are, rather, a demonstration that taxes and services have some effect on consumer preferences for town of residence, which is reflected in house price (948)." It is true that capitalization measures only provide evidence of consumer demand. This does not prevent them from being useful however. The implications of Tiebout's model are so wide reaching that any one approach would struggle to encapsulate every aspect of it if that were the goal. Edel and Sclar also fail to provide a suitable alternative; their approach of observing if service quality and price converge across boundaries over time as a measure of determining movement towards or away from equilibrium has its own problems. For one thing, presuming cities are modeled as perfectly competitive does not make sense. Consumer preferences and endowments are not uniform, and there are significant costs to mobility, which represents substitution. The wide array of possible public services offered by cities provide many different dimensions by which cities can differentiate themselves, even holding per capita spending constant. Many cities have qualities which are endemic to the city regardless of the actions of the local government – location being an obvious example. All of these facts indicate cities do have some slight market power. A lack of price convergence does not automatically imply disequilibrium.

These are not the only two approaches to studying capitalization. Black uses a clever pseudo-experiment where she studies neighborhoods which, while otherwise identical, are bifurcated by school attendance boundaries (1999), which has since developed into an approach sometimes referred to as boundary discontinuity regression. These three approaches are the most typical methods of studying capitalization.

If there is any gap in the literature it is a lack of studying the effect over time. Much research exists studying how capitalization within a population is different in different time periods, but few studies exist which observe direct changes in individual municipalities over time. This is not to say that researchers are insouciant to time-series analysis. Imbermann and Naretta (2015) use a time variant, fixed effects model while studying charter schools penetration, which, while not true time-series analysis, aims to achieve something similar. What I have not observed is the treatment of property value as a financial instrument and the use of statistical techniques suitable to financial analysis to study it. Especially given that the time frame I study includes the recession caused by the sub-prime mortgage crisis of 2008, I think this style of analysis can shed light on the relationship between the strength of Tiebout sorting and macroeconomic

conditions.

Research Design and Methodology

The purpose of this paper is to try to observe Tiebout sorting occurring, which is to say to observe a change in the chosen mechanism (school quality) and determine the effect it has on property values in the following time-period. This requires some sort of time-series model which is capable of predicting future values such that a comparison can be made between predicted future property values without the change in school quality and actual property values given the change in school quality.

However, my first step, before I get into time-series analysis, is to run a fairly simple multiple linear regression on the cross-sectional data of the last period observed to try to determine if there is evidence of Tiebout sorting having occurred. It would be somewhat pointless to try to observe sorting occurring in time if the end state does not support the conclusion of the theory. The theory in this case being that if consumers value education, there should be a positive relationship between school quality and property values in the catchment area for the school. The methodology here is very similar to but highly simplified from the methodology used in many capitalization studies, and the regression model is as follows:

$$New\ Jersey:\ log(MPV) = \beta_0 + \beta_1\, log(S) + \beta_2\, C + u$$

$$Massachusetts:\ log(MPV) = \beta_0 + \beta_1\, log(S) + \beta_2\, C + \beta_3\, P + u$$

MPV represents the median property value of a single-family residence, S represents the school quality as measured by standardized test scores, C represents the presence or absence of charter districts in the municipality, and likewise, for the Massachusetts regression, P represents the presence or absence of private schools. Additionally, a second regression was run using median rent list price as the dependent variable instead of housing price.

I obtained the property value and rental price data from Zillow, and I obtained all other data from the Department of Education of the state being studied. The municipalities studied were in New Jersey and Massachusetts. These two states were selected because for both states, school districts are almost completely coterminous with city boundaries (Fischel 2007). This allows the linkage of city level property value data with school district level test score data. Certain cities and school districts appear or disappear in the state during the period being examined. These are removed from the data set, as are cities or districts which cannot be matched up with an opposite entity, either due to the few exceptions where districts are not coterminous with city boundaries, or due to a naming discrepancy between data sets. Additionally, the handful of school districts serving pairs of small cities were eliminated unless the pair was considered as a single unit in the property value data.

Charter and vocational districts were not considered in the school quality metric, nor were private schools. As the Tiebout model is one of market choice, it seems unwise to ignore the possible substitution effect between traditional public schools and charter or private schools. It is possible to conceive of a situation in which, property values rise in an area, despite public school quality diminishing in that area, due to a concomitant greater than commensurate increase in the quality of private schools. Perhaps even, as discussed by Gemello and Osman in *Estimating the Demand for Private School Enrollment (1984)*, private schools and public schools may function as substitutes to the point where there is an inverse relationship between the quality of public and private schools. This would imply that every change in public school quality will cause a countervailing change in private school quality, providing an equilibrium pressure within a municipality which reduces the incentive to sort into another municipality with better public schools.

The addition of a variable to track charter schools is designed to partially ameliorate the lack of observation of this substitution effect. However, the capacity of this variable to fully solve the problem is very limited. Firstly, it does not track charter quality, but simply presence or absence. This is to say that it only represents the potential for substitution rather than observing the effects of actual substitution. Secondly, unlike traditional school districts in these states, charter districts are not coterminous with other municipal boundaries. Therefore, the variable is coded as the number of charter districts which are at least partially within the city. For the vast majority of cities this is actually 0, and most others only have one, but a in few large cities, multiple charter districts are present, with 6 being the most within any one city. Finally, only the Massachusetts regression contains a variable measuring private school presence. If I had access to data from New Jersey on private school location I would also track private schools in New Jersey. It is also worth noting that the effect of private schools is hard to study in this manner, as they are geographically unconstrained by formal attendance boundaries.

School quality is determined by summing the average score of the different subject tests given to high school students within the district. In New Jersey, simply a raw score is measured, in Massachusetts, the metric is the percentage of students scoring proficient or advanced. There is a possibility that limiting to high schools undercuts the effect, but I expect non-high school quality to be similar to high school quality within a district. Additionally, there may be a problem with abstracting the effect to the district level as opposed to examining the individual school level. I am limited to this level in terms of the data I have access to, but I do expect variance within a district to be lower than variance between districts. This is more of a concern for the later time-series analysis, than this first cross sectional analysis.

Both the variable for school quality and the variable for housing price are logarithmically transformed. These variables thus measures percentage changes as opposed to raw changes. It should be noted that this is a cross sectional percentage change; the measurement represents the percentage difference between municipalities in a fixed time. I choose to measure percentages for property values because property values likely exhibit large diminishing marginal utility: A change in property values of $10,000 is less important to the owner of a mansion than it is to the owner of a shotgun shack. Similarly, school score is measured in percentages because I expect desire to

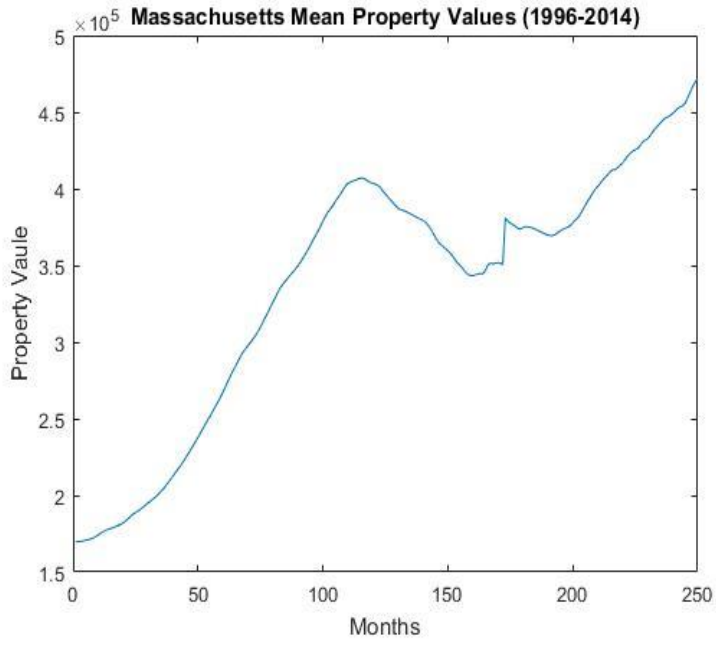
improve educational opportunities to diminish marginally. All else being equal, parents likely would spend more to move their children from an underperforming school to a mediocre one than to move them from a mediocre school to an excellent one.

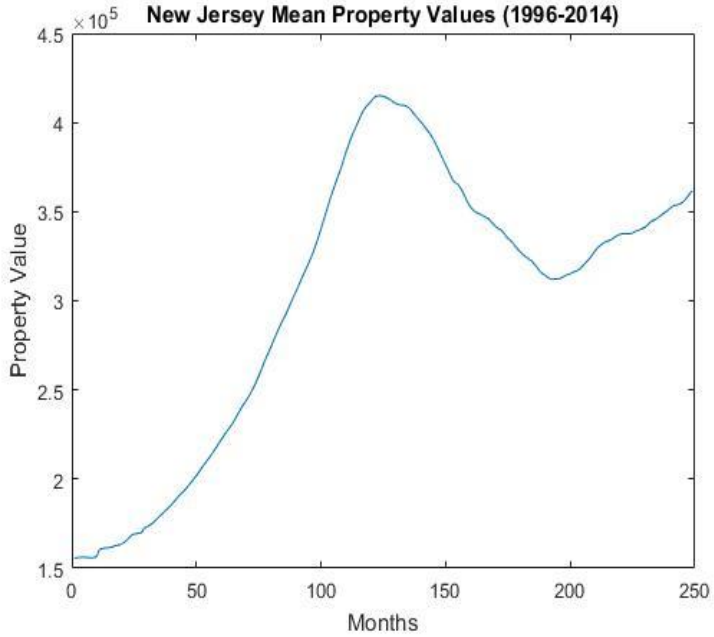
In order to study the sorting occurring, rather than studying sorting having occurred at some point in the past, a direct link between a change in school quality and a change in property values in the next period must be observed. However, property values vary according to numerous conditions, education quality being only a single one. Some of these are market-wide fluctuations, such as inflation or general macroeconomic strength, while others are location specific, such as local population size changes. Therefore, the change in property values measured cannot be between the initial time-period and the following time-period, but must be between the following time-period and what the following time-period would have been without the change in school quality. In other words, the change measured must be between the actual following time-period and a forecasted prediction of the following time-period. The initial regression models listed above in the purely cross-sectional analysis have been modified to reflect this:

$$\textit{New Jersey: } [\Delta MPV - E(\Delta MPV)] = \beta_0 + \beta_1 \Delta S + \beta_2 C + u$$

$$\textit{Massachusetts: } [\Delta MPV - E(\Delta MPV)] = \beta_0 + \beta_1 \Delta S + \beta_2 C + \beta_3 P + u$$

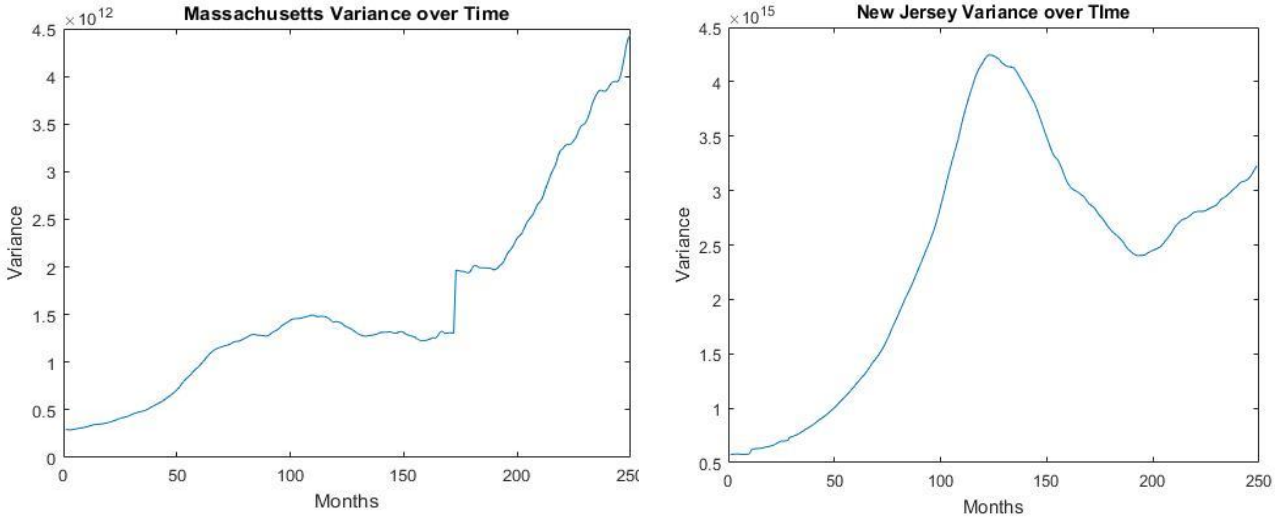
Where ∆MPV-E(∆MPV) represents the difference between the percentage change between the two observed time periods, and the expected percentage change between





the two time-periods, and additionally where ∆S represents the percentage change in school quality between the two time-periods. The accuracy of this method of analysis depends entirely on the accuracy of the forecast method.

The graphs above show how property values have changed over the span of years for which data is available from Zillow. It is clear from the graphs alone that the data in each time-period is not independent from the data in the previous time-period,



therefore the forecasting method must be one which accounts for conditional distribution of data. Additionally, from the graphs above, it is clear that variance of property values is conditional on time. I therefore opt for an ARIMA -- GARCH (Autoregressive Integrated Moving Average -- Generalized Autoregressive Conditional Heteroskedasticity) model, specifically an ARIMA model with one lag and a variance structure described by a GARCH(1,1) model. The exact model is:

$$EMPV= \phi_0 + \phi_1 MPV_{t-1} + (\alpha_0 + \alpha_1 X^2_{t-1} + \beta_1 h_{t-1})^{1/2} \varepsilon_t$$

$$\text{where: } h_t = \alpha_0 + \alpha_1 X^2_{t-1} + \beta_1 h_{t-1}$$

X represents the variance of MPV and ε_t is a normally distributed independent, identically distributed white noise innovation function. ϕ_1 represents the degree to which the value in the current time-period is affected by the value in the previous time-periods, α_1 represents how much the value in the current time-period is affected by the variance in the last time-period, and β_1 represents how much the variance of the rest of the time series affects the current value. Both variance contributions are subject to some random walk due to the white noise function. I choose this model for its general applicability and relative simplicity. I acknowledge that there is likely a more efficient and better specified model to describe the data, but this simple ARIMA – GARCH model is already pushing the limits of my abilities.

The housing data goes back to 1996, but the education data only goes back to 2005 in New Jersey, and 2010 in Massachusetts (due to the fact that they changed tests). While this does limit the number of years available for analysis, it does have the benefit of meaning that by the time education data is available for analysis, any data

eaten by lags are far in the past, and the model has had enough observations to become largely stable. This also allows the data set to be truncated to the period immediately prior to the education change without compromising the ability to accurately estimate parameters. However, the Rental data begins in 2011, and as such the model does not have enough time periods to form an accurate forecast. This means that rental data is limited to purely cross-sectional analysis using this data set until more time has passed.

I do not explicitly show the results of the time series analysis anywhere in this paper; this is because of the sheer space it would take – each year analyzed has a separate estimation for each municipality, and each estimation has five parameters with accompanying errors and test statistics. However, this forecasting is central to the accuracy of the overall analysis, so I will summarize trends of the model. ϕ_1 was always large and highly significant, α_1 was always highly significant except for three municipalities in New Jersey, and β_1 hovered on either side of significance depending on the year and municipality.

Data

New Jersey Cross Section Analysis (2014)

Homeowner	Estimate	SE	T-Statistic
Intercept(β_0)	5.466	0.703	77.727*
School Quality(β_1)	.0532	.006	9.621*
Charter Presence (β_2)	-.023	.0042	5.526*

n=158 Adjusted R^2=.494 ** represents significance at the .05 level using a 1-tailed test*

Rental	Estimate	SE	T-Statistic
Intercept(β_0)	-6.958	2.449	2.841*
School Quality(β_1)	2.383	.399	5.977*
Charter Presence (β_2)	-.024	.02	.824

n=152 Adjusted R^2= .213 ** represents significance at the .05 level using a 1-tailed test*

New Jersey Time-series and Cross-sectional (2006-2014)

Homeowner	Estimate	SE	T-Statistic
December 2006			
Intercept(β_0)	-.005	.001	5.29*
School Quality(β_1)	-.0016	.0015	1.078
Charter Presence (β_2)	-.03	.07	.435
December 2007			
Intercept(β_0)	.01	.0004	22.847*
School Quality(β_1)	-.0185	.0258	.717
Charter Presence (β_2)	-.0009	.0006	1.645
January 2009			
Intercept(β_0)	-.0046	.001	4.582*
School Quality(β_1)	.006	.001	.490
Charter Presence (β_2)	-.0538	.057	.929
January 2010			
Intercept(β_0)	-.004	.0007	5.63*
School Quality(β_1)	-.873	.0011	.939
Charter Presence (β_2)	.039	.053	.742
January 2011			
Intercept(β_0)	.006	.0002	22.462*
School Quality(β_1)	6.491E-5	.0003	.222
Charter Presence (β_2)	.011	.015	.743
February 2012			
Intercept(β_0)	.006	.0006	10.796*
School Quality(β_1)	-.0237	.0364	.6518

Charter Presence (β_2)	-.0002	.0007	.307
December 2012			
Intercept(β_0)	-.0017	.0005	2.92*
School Quality(β_1)	-.0034	.0354	.0981
Charter Presence (β_2)	-.0013	.0008	1.743*
November 2013			
Intercept(β_0)	-.0086	.0004	2.006**
School Quality(β_1)	-.014	.013	1.12
Charter Presence (β_2)	.0015	.0006	2.603*
November 2014			
Intercept(β_0)	.0032	.0004	6.565*
School Quality(β_1)	.0114	.0148	.75
Charter Presence (β_2)	-.0004	.0007	.646

n=158 Adjusted R^2: Varies, but is always <.04 ** represents significance at the .05 level using a 1-tailed test*

Massachusetts Cross-sectional Analysis (2014)

Homeowner	Estimate	SE	T-Statistic
Intercept(β_0)	6.961	1.267	5.494*
School Quality(β_1)	1.383	.229	4.617*
Charter Presence (β_2)	.054	.097	.558
Private Schools(β_3)	.025	.055	.467

n=56 Adjusted R^2=.251 ** represents significance at the .05 level using a 1-tailed test*

Rental	Estimate	SE	T-Statistic
Intercept(β_0)	4.290	.676	6.347*
School Quality(β_1)	.783	.160	4.895*
Charter Presence (β_2)	-.007	.050	.135

Private Schools(β_3)	-.002	.028	.055

n=51 Adjusted R^2=.312 ** represents significance at the .05 level using a 1-tailed test*

Massachusetts Time Series and Cross-sectional (2011-2014)

Homeowner	Estimate	SE	T-Statistic
2014			
Intercept(β_0)	-1.48	.885	1.671
School Quality(β_1)	-3.763	9.002	.418
Charter Presence (β_2)	.114	1.502	.076
Private Schools(β_3)	.482	.867	.555
2013			
Intercept(β_0)	-.471	1.023	.460
School Quality(β_1)	2.808	11.075	.254
Charter Presence (β_2)	-2.365	1.701	1.390
Private Schools(β_3)	-1.729	.977	1.769*
2012			
Intercept(β_0)	-3.443	1.517	2.27*
School Quality(β_1)	25.395	24.985	1.016
Charter Presence (β_2)	.431	2.425	.178
Private Schools(β_3)	1.067	1.410	.760
2011			
Intercept(β_0)	-2.909	1.372	2.12*
School Quality(β_1)	-12.238	27.507	.658
Charter Presence (β_2)	.348	2.294	.880
Private Schools(β_3)	.863	1.321	.658

n=56 Adjusted R^2:Varies, but is always <.05 ** represents significance at the .05 level using a 1-tailed test*

Analysis

For both states, the purely cross-sectional regression showed a significant and reasonably large relationship between school quality and property values. This is also true of median rental list price. In New Jersey, the estimated school quality coefficient was much greater in the rental regression than in the homeowner regression, in Massachusetts the reverse is true. Of the four regressions, the only other significant variable was charter presence in the New Jersey homeowner regression. This is an early indication of Tiebout sorting having occurred; and also a suggestion that charter and private schools matter less to the relationship than I had originally suspected.

In the Time Series portion, almost nothing was significant beyond intercepts. Occasionally charter presence was significant. I suspect what this is actually capturing is city size. Charter schools seems to be much more likely to be situated in large cities in these two states. This is probably because how concentrated the population is in large cities. Therefore, if charter presence is correlated with city size, perhaps the implication is not that charter schools cause property values to evolve differently, but rather that cities which have charter schools are more likely to be large, and property values evolve differently in large cities than they evolve in small cities.

One reason the cross-sectional analysis could show that there is a relationship between property value and education quality, while the time-series analysis fails to find significance while assuming that both analyses are correct is the possibility that the causality is reversed. This is to say that property values drive education quality rather than being driven by education quality. The logic would be that instead of quality schools attracting wealthy individuals to an area, wealthy areas attract quality students.

While certainly possible, I don't believe that the conclusion to draw from this data is that Tiebout sorting does not occur over time. While I have failed to find any evidence indicating that it does occur, the cross-sectional analysis does agree with the capitalization literature that education quality is capitalized into local property values. It seems likely therefore that I have in some way done a poor job of examining the mechanism for that capitalization. Four possible reasons occur to me why, if a difference does exist between forecasted property values and actual property values which is explainable by a change in education quality, I have failed to capture it. Firstly, I could have chosen a poor method of forecasting. While I do stand by the forecast method, it is possible that if education trends in recent periods were strong enough, the forecast model could be taking account of that trend and predicting a further increase or decrease in school quality. This does seem unlikely however, if only due to the fact that the forecast model was operating on monthly changes, while school quality reports were released roughly annually. While there are time series model powerful enough to pick up on trend occurring every, roughly, twelve time-periods, this should not be one of them. Secondly, I examined the change between the month in which the educational quality change was reported and the following month. It is possible that I selected the wrong months to analyze, or that there is some lag in the effect. No relationship would be expected if there was a mismatch between the periods in which the changes occurred. Thirdly, it is possible that, even if the effect begins immediately that it progresses slowly enough that the change is not visible on a month to month basis. For both these reasons, if I were to start over, I would increase the period of the change to six months, although this would also reduce the accuracy of the forecast, and would not fix the problem if the lack of variation is in the school quality metric. Finally, there is

always the possibility that the was some systematic error in either the data I used or the way I collected and labelled it. There is some weak evidence against this given the significance in the cross-sectional analysis.

Conclusion

While I have provided additional support to the body of literature suggesting that education quality is capitalized into property values, I have failed to provide evidence for anything beyond that, specifically how education quality and property values interact in time. Perhaps future work can be done to find a method which is more illuminating, either through a revision of the model described above, or through an entirely new method. An obvious first step would be to extend the time frame over which the change is observed to account for any lag in the effect and to capture more variation.

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