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ESSAYS ON THE SUBJECT OF EDUCATION FINANCE AND MOBILITY

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This dissertation is dedicated to my friends, who were understanding of milestones missed, and my wonderful family, who have been absolutely supportive throughout. I am lucky to have all of you in my life and I am truly thankful for your love and friendship.

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Abstract

This dissertation is an exploration of education finance. Most notably the passage of education financing bonds as well as the effects of the school construction that comes from the passage of education bonds. Chapter 1 explores the effect of elderly voting populations on the probability of passing a bond vote to improve school infrastructure. The addition to the literature is the inclusion of a measure of the community connection in the elderly community by including data regarding recent movers into the county within the last year. This inclusion separates the elderly community and shows that school districts with a large number of recent movers are more likely to not pass a school bond. Chapter 2 examines the effect of opening a new school campus on the students who are not able to attend the new school. Using a regression discontinuity design employing school bond votes and an event study analysis using school openings. The results show a small increase in testing results for students that stay, as well as a large positive effect on the lowest performing students and a large negative effect on the highest performing students who remain at existing schools. Chapter 3 attempts to determine the likelihood that the effect on student achievement of opening a new school comes from the increased mobility of quality teachers due to the opening of a new campus. The results show that there is a larger probability of experienced teachers changing position after a new school opens and therefore it is possible that any positive or negative effect on student achievement of opening a new school comes from increased teacher mobility.

Chapter 1: Financing Public Education Facilities: The Role of Elderly Populations and Geographic Mobility.

1. Introduction

The population of the United States is growing older. According to the U.N. Population Division, the United States' median age has increased by over seven years during the last five decades, and it is projected to rise four more years over the next five decades. The shift in persons age 65 and over may be even more pronounced, increasing from 13.1 percent of the population in 2010 to 22.4 percent of the population in 2060 forecasts. How this demographic shift will affect the median voter and, by extension, public policy is subject to speculation. Talk of a "Gray Peril", where an increasingly large elderly voting bloc reduces spending on public goods that do not provide direct consumption benefits, most notably local education, has been around for decades (Longino, 1988). However, the effect of larger elderly populations on the voter's appetites for education spending is subject to debate, with some results showing a positive correlation between the size of elderly populations and education funding, while others find a negative correlation.

This paper explores the contentious relationship between elderly populations and education funding by first identifying a component of the elderly population with potentially stronger negative views of education spending than the general elderly population. To that end, we analyze the determinants of passage for California school bond measures using a variable representing the expected degree of social connectivity the elderly population has with their community: rates of recent movers into the community. This choice follows the only previous study considering elderly mobility and education funding, Berkman and Plutzer (2004), who analyze per pupil spending in counties across the U. S. Our results suggest elderly residents that recently moved into an area lower the likelihood of passage.

"Gray Peril", a term used to represent the possibility that an organized and growing elderly population will begin to reduce public spending on education and other services that do not directly benefit the elderly voter, dates back at least to the work of Button and Resenbaum (1989, 1990). Surveys have shown a decrease in willingness to pay for education among elderly populations (Vinovski, 1993), and early empirical studies found that larger elderly populations within states have a negative effect on per pupil education spending (Poterba, 1997). However, other papers find insignificant effects on spending level when the relationship is measured at the county level (Ladd & Murray 2001). Importantly, recent studies have provided strong evidence of negative effects of aging on school spending. Recent survey data has shown that a respondent's age has a stronger negative effect on preferences regarding school spending when the respondent reports not having a strong connection with the community (Lambert et al, 2009). Additionally, in a recent paper from Figlio and Fletcher (2012), a novel approach was employed to create a population simulation in order to instrument for aging communities without the added complication of how a community may evolve through migration and Tiebout sorting. They found a negative effect of aging on per pupil spending. The authors simulated the community age distribution by assuming all members of the community aged in place, and members who passed away at typical life expectancy were replaced by a mean-aged new resident who would then age in the same manner as the other members of the community. In this way, the paper groups the elderly member of the community and instruments for the aging communities to remove various growth rates and migration patterns, finding that as communities aged, per pupil spending fell.

The primary goal of this paper is to explore whether or not it is possible, given current data availability, to demonstrate that elderly voters are best described as a heterogeneous entity when it comes to school finance. This goes to the heart of the Gray Peril concern that a growing elderly population will form a cohesive bloc that does not support school spending. Since connection to one's community is difficult to measure directly, this paper employs a variable capable of serving as a proxy for community connection from the American Community Survey (ACS): a school district specific measure of recent elderly mobility. This proxy for community connection is supported by health literature. When a household moves into a new area, they often leave behind friends and neighbors, which reduces the amount of meaningful social interaction (Dupuis-Blanchard, Neufeld, & Strang, 2009). At the individual level, it is also the case that the length of tenure has a positive effect on the sense of community a person feels (Royal & Rossi, 1996).

However, even given the importance of community connection and its linkage to mobility, the previously mentioned Berkman and Plutzer (2004) study marks the only contribution to the empirical literature that examines the recently mobile elderly population as a potential determinant for education spending. They use decennial census data measuring tenure of the elderly populations in counties across the United States. Specifically, they make a distinction between recent and longstanding elderly residents, which is similar to the method used in this paper. However, there exist key differences between the present study and theirs. For example, they examine per-pupil expenditures at the school district level across 40 states. However, for states that have strict equalization policies for student level spending, per-pupil spending is designed to have very small differences across school districts (Sonstelie, Brunner, & Ardon, 2000). Therefore, exploring the passage of education spending bonds provides a more direct link between voter preferences and local school funding. Additionally, school districts are used as the unit of analysis for the present study, whereas, Berkman and Plutzer use county level measures for their independent variables. A third advantage of the present study is that it benefits from the use of vote data from before and during the recent Great Recession, allowing an investigation of the role, if any, of that cycle on the likelihood of bond passage. Therefore, this paper can be viewed as an important contribution to the literature regarding elderly populations, community connection, and the determinants of school funding.

2. Elderly Voters and Education Finance

In order for Gray Peril to threaten future education spending, certain conditions must be met. The first is that the majority of voters' preferences must decline as they age. Applying the logic of the median voter theorem (Black, 1948), a decline in education spending comes only from an increase in the number of voters with a spending preference below the current spending level. However, a decline in the median preference is dependent on the presumption that the preference for education spending declines for the majority of individuals as they age. The Gray Peril argument generally assumes that the majority of the aged population no longer has school aged children and, therefore, do not receive a direct benefit from increased spending on education. Once their children have graduated from high school, the benefit a parent gains from education spending is the same benefit that childless households receive from having an educated population, and support for public education would be expected to fall (Rubinfeld, 1977). In addition, local governments have limited resources to provide programs and services, such that any spending on education services means money that could have been spent providing services directly to the elderly is no longer available (Jackson, 1985). Finally, many retired household have fixed income derived from savings, which works against preferences for increased public spending of all types.

However, a negative effect of aging on education spending is not assured (Poterba, 1998). In surveys, support for education spending is positively correlated with income and achieved level of education (Wyckoff, 1984), and when those variables are used as controls, the impact of older voters on outcomes of educational bond issues has been shown to dissipate (Button, 1992). Therefore, it is probable that higher income and education attainment would be related to higher perceived benefits to education in a voter's own life and therefore would correlated with support for education spending (De la Croix, 2001). While larger support of education spending from voters with higher incomes and education attainment explains why certain members of the elderly population would continue to support education spending, there are reasons why an aging population may wish to maintain or even increase education spending. The first reason the elderly population may wish to increase education spending is that education spending increases income for the younger generation. This increase in income will be taxed, and used to pay for more social services consumed by elderly households. This makes spending on education an intergenerational transfer that acts as an investment in the younger generation's earning potential that will be paid back once they enter into the work force. However, as a population ages the valuation for education spending is reduced as the benefit from educating the younger generation will not be fully realized by older generations. While education spending to increase incomes will be paid back through intergenerational transfers, at some point a voter cannot expect to see returns from younger generations which lowers the return on investment. Importantly, life expectancy is increasing and changing the nature of this calculation. According to the National Center for Health Statistics, life expectancy in the U.S. has increased from 69.7 in 1960 to 78.5 years in 2009. The added longevity of the elderly population means that more of the return from educating the young will be realized in their lifetime.

Another reason an aging population may support education spending is the positive effects that public education spending has on property values. Cellini, Ferreira, & Rothstein (2010), found that passing a school facilities bond in California had a positive effect on property values. As will be discussed in Section 3, the law in California requires that bonds pass by a two-thirds majority or a fifty five percent majority if the school district agrees to follow rules regarding increased transparency. Therefore, it is difficult to increase education spending as passage of a bond requires consensus beyond the median voter. Their results confirmed California's institutions leave the level of education spending low. In this environment, an increase in education spending moves the provision of education towards the median preference and resulted in an increase in the value of property in the area.

This capitalization effect in the real estate market would provide a benefit to anyone using home equity as a form a savings. Since elderly households commonly have reached an ownership outcome, increased home values serve as a direct benefit. However, this is an area where the incentive to increase education spending would depend on the type of aging taking place in a community. The migratory aging population has less of an incentive to see home values rise because the population is only going to hold the property for a short time, and therefore the return on holding a property is much lower (Haurin & Gill, 2002). However, the elderly who have aged in place are more likely to have owned their homes for a number of years and are using the increased property value to increase savings for retirement.

Additionaly, there may be an altruistic component of education spending or a generational connection beyond a single generation. The most readily apparent connection occurs if the household has an extended generational component or if there has not been any migration in the younger generation. In this case, it is possible that education is being consumed by grand-children, and therefore there is a more direct benefit to education spending. As mentioned previously, an aging-in-place household may feel a connection to the community, creating an older population that may wish to support educational spending, in order to benefit the community to which it has an attachment.

Since the models presented in this paper focus on educational bonds, the remainder of this section will focus on the available literature regarding school bond votes. An initial question to be addressed is the functional form that the regression should follow. The literature has not converged to an agreed upon method; rather it offers two ways to examine the effect of observable factors on voting outcomes. Studies generally either model the decision as a binary yes/no vote, using a probit or logit regression to model the probability of passage, or they model the percent of yes votes using OLS (or similar method) to examine how the identified independent variables affect the percentage of yes votes received. For instance, a recent paper by Zimmer et al. (2011) on education bonds used OLS and the percent of yes votes as the dependent variable, while Bowers, Metzger, and Militello (2010) used a logistic regression on binary pass/fail outcomes. Since the literature values both approaches, the current investigation presents results of both types.

While the effect of the elderly population on school bond votes is the focus of this paper, the models presented also control for other determinants of educational bond support that have been identified by previous studies. Balsdon, Brunner, and Reuben (2003), for example found a positive income elasticity for school spending. A 2013 paper from Bowers and Lee (2013) found that the first bond proposal was more likely to pass than subsequent bonds, suggesting that accounting for recent votes is important.

Finally, the influence of school bond ratings must be addressed. Due to data limitations, this paper does not contain the school bond rating, nor does it report or if the school decided to have the bond itself rated before the voting took place. Fortunately, there is a wide literature on municipal bonds that allows us to determine variables that would correlate with bond rating. Rubinfeld (1973) concludes that a community with a higher median household income will have a higher bond rating, since the community has a wealthier tax base, and hence, a higher likelihood of paying its debts. Simonsen, Robbins, and Helgerson (2001) report that larger communities are a safer bet, and will therefore have a better rating on municipal bonds. The present study includes both of these proxy variables for credit rating, as they should directly affect the interest rate attached to the bond, and therefore, the cost of servicing the retiring bond.

3. Data and Empirical Strategy

The data analyzed in the present study come from California. California offers a great opportunity since it provides a large number of bond votes and has a variety of school districts ranging from wholly urbanized to largely rural. The state also has a mobile population and is a common retirement destination. However, California does possess some eccentricities in terms of education finance and property taxes which should be discussed.

Education finance in California is designed to be equal for all students. In 1968, the Serrano v. Presti lawsuit and subsequent Supreme Court decision limited the allowed variance in the amount of spending per pupil in school districts across the state. A decade later, California voters passed Proposition 13, enacting strong property tax reform and further constraining the ability of local governing bodies to fund education. When Proposition 13 removed local discretion regarding school funding, it empowered the state to determine the level of school funding in all local districts in order to minimize differences in spending per pupil. To put the goal of equal distribution among school districts into perspective, the California Supreme Court, in a subsequent challenge of the Serrano ruling, stated that the difference in spending per pupil across districts should be reduced to considerably less than $100 per student (Henke, 1986).

Proposition 13 produced a number of consequences. Spending on education became more uniform, but at the expense of the high spending districts rather than increasing the funding of the low spending districts (Sonstelie, Brunner, & Ardon, 2000). The legislation also made it illegal for districts to raise money through general obligation bonds. Then, in 1984 Proposition 46 allowed school districts to issue bonds for infrastructure improvements, and bonds could only be adopted with a two thirds majority. Subsequently, in 2000 the state allowed school districts to issue bonds supported at a fifty five percent majority, as long as the district agreed annual performance and financial audits of the use of funds. This method accounts for over ninety percent of bond votes over the time period used in this paper. As such, the present analysis focuses on bond votes that use the fifty five percent majority.

In California, all property is subject to a uniform one percent property tax rate. By construction then, local revenues and the quality of all other local public services will be highly correlated with the median home value in the school district. Additionally, elderly populations are more likely to consume local services other than education, so decisions regarding where to live should largely follow Hamilton's (1975) benefits view, which characterizes property taxes as an efficient method of transferring the cost of local services to residents. Therefore, the taxation and spending policies that California employs motivates the idea that decisions of where to live for elderly populations should have little to do with education spending. This is a critical point, and one that should help alleviate any potential concerns related to non-random Tiebout sorting of elderly households into low property tax environments and the corresponding worry of reverse causality. Additionally, Figlio and Fletcher (2012) demonstrate that an instrumental variable correction ran counter to the story of tax avoidance, meaning that elderly movers were actually more likely to move into areas with higher education spending.

This paper examines the results of school bond votes in California for the years between 2005 and 2013, provided by EdSource. Parcel tax votes were also available, but our analysis uses only bond votes. While we considered including both types of education funding, it is possible for parcel taxes to be proposed with an exemption for the elderly population. The possibility of exemptions changes the cost of passage for elderly populations and the presence of an exemption is not available in the data; therefore, it cannot be accounted for in our regressions.

Bond votes in California are voted on at the school district level. There are different types of school districts in California: elementary school districts, high school districts, and unified school districts. A unified school district is simply a case where contiguous elementary and high school zones form one district. After removing any votes that were associated with school districts without data or that could not be linked to a unique school districts, the total number of observations is 503 bond votes in 401 unique school districts. [1]

Of the 401 school districts, seventy eight percent held only one bond vote over the nine year data widow, and another seventeen percent of school districts held two bond votes over the same time, and less than four percent of observations held three to four bond votes. Summary statistics are presented in Table 1.1. Bonds passed at an

[1] The naming convention of school districts poses a problem, in that within the data, some votes are reported without an elementary or a high school designation. This would not be a problem elementary and high school districts of the same name had identical demographics, but there exists variations between the two.

eighty one percent rate, and averaged sixty three percent support. The size of the bonds per household averaged $4,600 and the average median home value was $487,000. The percent of the elderly population averaged twelve percent and of the elderly population three percent on average were from outside of the county. The percent of the elderly from outside of the county and the value of the bond per household have the most variation. The percent of the elderly population from outside of the county has a maximum value of ten percent of the population, a minimum of zero, and the standard deviation is almost as large as the mean value while the value of the bond per household ranges between $90,000 and $3,000.

The Census Bureau provides data at the school district level in the main decennial census and in the American Community Survey (ACS). In addition to detailed socioeconomic and demographic data available at the school district level, the ACS provides data regarding the number of people surveyed who have moved into their residence within the last year. The is reported for five year age bands and can be used to construct the number of residents who have moved into their current residence from outside of the county. The age bands can then be used to determine what percentage of the elderly population has moved into the county during the last year. We argue that a new resident of the county will be less likely to have a strong connection to the community; this mobility measure will serve as our proxy for community connection. Unfortunately, the ACS does not provide the same measure for movement from outside of the school district, which would make an even more accurate measure than county movers. However, the advantages of this variable, when compared to using of growth of the aging population, are many. For example, growth of the elderly population can occur from increased mobility as well as from natural aging in place. If the elderly population growth comes from aging in place then it is possible that elderly population growth would have a positive effect on the likelihood of passing school funding bond. Indeed, when elderly population growth was used in Button and Rosenbaum (1989) it had a significant positive effect.

The most complete coverage of school districts is only available for the five year estimates for 2009, 2010, 2011, and 2012. A five year estimate uses data collected over a five year period to make up a single number that is then attributed to the last year of collection. For instance, the five year estimate of median income for 2009 would include data from 2005, 2006, 2007, 2008, and 2009. Theoretically, it is possible to double count responders in the survey. However, this is a trivial concern because the ACS does not survey the same residential address more than once in a five year period. In order for a resident to be counted more than once, the resident must move into a new residence during the five year window and also be surveyed by the census within that window. This circumstance is not impossible, but extremely unlikely. Using the five year estimate also poses a challenge when attempting to match data to specific dates of school bond votes. For a bond vote in 2007, the five year estimate for 2009, 2010, and 2011 will all contain data collected during the 2007 year. In order to address this problem, the data for each independent variable is averaged to create a cross-section of data for every public school district in California. This is done to in an attempt to address the timing issue while retaining data, but it does mean that if the outcome of a vote has an effect on the independent variables, the results will be overstated. Of some comfort is the fact that the dependent variables used in this paper are stable across time with the standard deviation being only five percent or less of the mean for all of the variables used except for the measurement used to capture elderly mobility. To further address the concern of the vote affecting independent variables we later present a robustness check, in Section 5, using a restricted data set where each bond vote is matched to the five year band that directly preceded it, instead of averaging all bands within school districts.

To provide a baseline of how the size of the elderly population affects school bond votes, we begin with a simple model using the variables that are specific to the bond that is being voted on, as well as the percent of the population that is aged sixty five years or more, the percent of the population aged sixty five years or more who have moved in to the school district in the last year from outside of the county, and an interaction term for the two variables. The inclusion of the interaction term is important because it weights the variable of interest, the recent elderly movers, by the size of the elderly population in the area. The bond characteristics included in the model are the amount of the bond per household in the school district, the number of times a school bond has come up for a vote and failed in the last two years, a dummy that accounts for the economic recession that took place during the period we analyze, the population of the district, and the median income of the district. The baseline regression also includes the district's median home value. The model is a binary outcome (passed or failed) logistic regression. The results are discussed in Section 4 and shown in Table 1.2. Two similar regressions are employed in order to provide a more complete understanding of the effects of recent elderly mobility. In regression (1), the model is specified with only the percent of the population sixty five or older and the interaction of the term with the percent of the population sixty five or older that moved into the school district from outside of the county in the last year. The model in regression (2) is similar, except for the inclusion of the percent of the population sixty five or older that moved into the school district from outside of the county in the last year as a standalone variable, as well as the interaction variable.

While these simple models provide a good point of reference for how the elderly population affects bond outcomes, they do not account for other relevant demographic data available at the school district level. Therefore, regressions (3) and (4) include all demographic controls. These controls include the child dependency ratio[2], a Herfindahl-Hirschman Index (HHI) for race[3], the percent of the population with a bachelor's degree or higher, the percent of the homes that are owner occupied, and the percent of the population living in a census designated urban area. The percent of the population that moved into the school district from outside the county in the last year for all ages is also included in order to determine if the effect that we are capturing is coming from a more general pattern of mobility. In addition, dummy variables are included for each region in California.[4] Similar to the comparison of regressions (1) and (2), the difference between regressions (3) and (4) is the inclusion of the percent of the population age sixty five or older as a standalone variable.

While our preferred models are the logistic regressions, we also estimate models in Section 4, Table 1.3 where the dependent variable is the percent of the affirmative

[2] The number of children age 0 to 17 divided by the number of adults age 18 to 64 in the school district.
[3] The sum of the squared percentage of the population in each school district for White, Hispanic, Black, Asian, and a single category that contains all other races reported by the census.
[4] Regions determined by the California Regional Economies Employment Series, sponsored by the California Workforce Investment Board.

votes the bond measure received. Because the dependent variable is a proportion that is bound by zero below and one hundred above, those extensions use a generalized linear model.

4. Results

Regressions (1) and (2), in Table 1.2, show the first main result of interest, that the coefficient for percent of the population sixty five or older is negative but statistically insignificant. Additionally, the coefficient for the interaction term is significantly negative, which suggests that a larger elderly population that is made up of more recent movers will have a detrimental effect on a district's ability to pass educational bonds. As regression (2) includes both the percent of recent movers as well as the interaction term, the linear combination of the two variables is also tested to determine if the two variables jointly have a negative effect on the probability of bond passage. The test rejects the null that the two coefficients sum to a value greater than or equal to zero, supporting the presence of a negative effect.

Once again the test of the two variables together in regression (4) suggests a significant negative effect. Compared to regressions (1) and (2), most variables do not change much in terms of magnitude or significance. Among the demographic variables, there are a number of factors that are directionally in line with theory, but that are statistically insignificant. The HHI for race is positive, supporting the findings of Barrow, 2006 and Ajilore, 2009, among others. The percent of the population with a college degree or higher is positive. The percent of the population that owns the house they are living is negative, which is consistent with the "renter's illusion" hypothesis that posits renters discount their own property taxes (Martinez-Vazquez, 1983 and

Oates, 2005). Significant demographic variables are the child dependency ratio and the percent of the population living in urban areas, which both have positive effects on the probability of passage. Turning to the main variables of interest, the estimated effect of the population sixty five or older becomes significant and positive. This gives support to the idea that a larger elderly population, in of itself, is not a detriment to education finance.

Regressions (5) and (6) follow the same pattern as previous regression pairs in Table 1.2. The added information is that these regressions include demographic data regarding residents aged twenty five to thirty four, measuring the presence of those most likely to have young children and to value education spending. The inclusion of an age range more likely to have children who attend public schools and therefore value education does not appear to have a significant effect on the model compared to regressions (3) and (4), with no directional changes in the estimated effects of the independent variables and only minor changes to significance level.

For each model the Akaike Information Criteria (AIC) is provided as well as the percent of observations correctly predicted by the model. There is no appreciable difference in the AIC between each pair of regressions, and a slight favoring of the second group of regressions that accounts for demographic information and region effects and without the specific inclusion of the younger age groups 25-34. In regards to the percent of the observations modeled correctly, regressions (3), (4), (5), and (6) outperform the simple model. However, as the percent of votes that are passed is high, the ability of any model to gain more accuracy is limited.

Shifting to Table 1.3, where the dependent variable becomes the percent of yes votes, we see the results are very similar. The coefficients for the percent of the population age sixty five or older as well as the recent mobility measures for the elderly population follow the same pattern, with a positive effect from the elderly percentage of population and a negative effect from the recently mobile elderly population and the interaction between the percent of recently mobile elderly and the percentage of elderly population. In regards to the control variables, there are a few minor differences in significance. For example, the amount of the bond is significant in Table 1.3, but not in Table 1.2. However, the directional effect of each variable is the same whether the model is based on passage/failure or upon the degree of support generated for the bond.

5. Robustness Checks

While the regressions presented in Section 4 account for a number of the factors likely influencing the outcome of interest, passage of a school bond is a complicated issue, framed in local politics and potentially unidentified independent variables. For instance, it is possible that timing issues exist regarding how long it has been since a district issued bonds that make those bond votes more likely to pass. Table 1.4 provides a robustness check that attempts to address these concerns. Regressions (1) and (2) now include a dummy variable for bond votes that are new, defined as cases where no bond votes were held in the previous three years. Regressions (3) and (4) include a dummy variable that defines new votes as having no votes held in the previous five years. Regressions (5) and (6) extend the window to the previous seven years. The variable to capture new activity is only significant in the strictest definition of new, with votes that take place very quickly after a previous bond measure being more likely to pass, given that there is a significant negative effect of having previous ‘No’ results in the last two years. The other definitions of new votes do not have any effect on the model. As such, we cannot find evidence that timing issues have an undue effect on school bond passage or failure.

Finally, additional robustness checks are also included regarding the method of averaging results across observations, and the choice to treat each district as only having one set of observations regarding independent variables. This can lead to problems if the recently mobile elderly are moving into areas that recently failed to pass a school bond initiative. Therefore, the regressions presented in Table 1.5 mirror the logistic regressions from Table 1.2, but the data used in these models comes exclusively from school bonds voted on in 2010 to 2013, where each bond can be directly linked to the five year ACS number from the year before the vote. For example, the bond votes in 2010 are linked to the 2009 ACS, the 2011 votes to the 2010 ACS, and so on. The results show similar directional effects for each variable, but many lose significance. Of course, a reduction in explanatory power is to be expected, given that the number of observations is reduced by more than fifty percent. For the mobility measures previously used in Table 1.2, the specifications with only the interaction term remain negative and significant. On the other hand, the specification where the measure of recently mobile elderly is included along with the interaction term no longer retains significance. Importantly though, another issue associated with not using the average of the five year ACS is that the possibility of having a zero observation for the percent of the elderly population that moved into the school district from outside the county in the last year. This is problematic, as a zero has a very large marginal effect compared to other small changes in the variable. The regressions in Table 1.6 use the same data used when constructing Table 1.5, but observations where the percent of the elderly population that moved into the school district in the last year equals zero are dropped from the regression. Comparing the results from Table 1.6 and Table 1.5, we can see that without the zero observations included, the percent of the population sixty five or older again becomes positive. Also, while the significance of the interaction term for recently mobile elderly is still below the ten percent confidence interval when the recently mobile elderly are also included in the model, the linear combination of the two coefficients also now returns to negative and significant.

6. Conclusion

Education finance is clearly one of the most critical issues facing society. Broadly speaking, it connects to larger concerns of income inequality and reduced generational income mobility, and serves as a litmus test for society's willingness to invest in future generation's human capital. In fact, equity concerns can be seen in the education financing landscape in California, which is flat by design. The state's goal is to create a terrain where there are no peaks and there are no valleys, which means that school districts in need of structural investments must turn to school bonds to find the necessary financing. Clearly then, it is important to know what characteristics make obtaining such financing easier or harder. There is concern that an aging population will make funding even more difficult to obtain, a "Grey Peril" identified by earlier work. However, the phenomenon does not appear to be driven by the elderly population in general. Our findings suggest that reduced support occurs when there is less engagement between the elderly population and the community; only in these environments is the passage of school funding bonds more difficult. Since the analysis only employs a proxy for community engagement, recent mobility, it is possible that the negative effect on passing school bonds is a more complex mechanism, for which elderly mobility is also serving as a proxy. Future research attempting to explore individualized reporting of connectedness and attitudes toward local education support would serve as a complement to this study.

An important strength of this paper is the ubiquitous nature of the control data used. The measures come from the American Communities Survey, which has data for every state. This means that if bond results from other states were acquired, similar investigations could be performed and compared to California to determine if this phenomenon is seen in other areas of the country, or only in states that share similar elderly migration patterns to California. It is possible that other states would require a different proxy for community connection, or that they may have different mobility patterns, and that the influence of recent elderly mobility is subject to these different intensities.

The presented results suggest that areas where seniors have recently moved have experienced a negative impact on school bond passage, which is an important aspect of school finance given the policies in place in California. This means that school districts can and should take into account the number of recently transferred elderly when determining the particulars of bond proposals and when marketing those proposals to the community. This paper shows that presenting a story to the entire elderly population of helping their grandkids or giving back to the community may have limited impact in some areas. On the other hand, the positive influence better education facilities have on property values (Cellini et al, 2010) may carry a more ubiquitous positive influence. It also means that school districts may be better served by engaging in community outreach with the elderly community in order to increase the amount of community support coming from that community.

Chapter 1 Tables and Figures

Table 1.1: Summary Statistics	Source	n	Mean	S.D.	Min	Median	Max
Result = "Yes"	EdSource Bond Data	503	0.81	0.39	0	1	1
Percent of Votes cast "Yes"	EdSource Bond Data	503	0.63	0.09	0.29	0.63	0.86
Amount of Bond per Household in $10,000	EdSource Bond Data	503	0.46	0.57	0.05	0.33	9.12
Total Population in 10,000 people	American Community Survey	503	8.56	29.81	0.02	2.93	447.12
Median Income in $10,000	American Community Survey	503	6.56	2.46	2.48	6.09	18.56
Median Home Value in $100,000	American Community Survey	503	4.87	3.40	0.90	3.93	16.95
# of Previous "No" Votes in the Last 2 Years	EdSource Bond Data	503	0.13	0.37	0	0	2
Child dependency ratio	American Community Survey	503	0.41	0.11	0.13	0.40	0.76
Racial Herfindahl-Hirschman Index	American Community Survey	503	0.51	0.15	0.24	0.48	0.96
Percent of the population with BS degree or higher	American Community Survey	503	0.28	0.18	0	0.23	0.82
Percent owner occupied	American Community Survey	503	0.64	0.13	0.19	0.64	0.93
Percent of the population living in urban area	2010 Decennial Census	503	0.79	0.31	0	0.94	1
Percent of the population aged 65+	American Community Survey	503	0.12	0.05	0.02	0.12	0.3
Percent of the population aged 65+ from outside the county	American Community Survey	503	0.03	0.02	0	0.02	0.1
Percent of the population aged 25-34	American Community Survey	503	0.12	0.03	0.03	0.12	0.28
Percent of the population aged 25-34 from outside the county	American Community Survey	503	0.1	0.06	0	0.09	0.44
Percent of the total population from outside the county	American Community Survey	503	0.05	0.03	0.01	0.05	0.21

TABLE 1.2: Logistic Regressions, Bonds from 2005-2013						
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Bond Amount per Household in $1K	-0.37*	-0.37*	-0.31*	-0.30	-0.27	-0.26
	(0.20)	(0.20)	(0.18)	(0.19)	(0.20)	(0.21)
Total Population in 1K people	0.14***	0.14***	0.07**	0.07**	0.07**	0.07**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Median Income in $1K	-0.09	-0.09	-0.12	-0.13	-0.10	-0.11
	(0.07)	(0.07)	(0.17)	(0.17)	(0.17)	(0.18)
Median Home Value in $100K	0.13	0.13	-0.03	-0.03	-0.018	-0.02
	(0.08)	(0.08)	(0.10)	(0.10)	(0.10)	(0.10)
# of Previous 'No' Votes Last 2 years	-0.55**	-0.55**	-0.64**	-0.64**	-0.57*	-0.56*
	(0.27)	(0.27)	(0.29)	(0.29)	(0.30)	(0.30)
Child Dependency Ratio			3.00*	3.18*	3.63**	3.95**
			(1.81)	(1.80)	(1.82)	(1.84)
Racial Herfindahl-Hirschman Index			0.16	0.41	0.34	0.61
			(1.12)	(1.15)	(1.13)	(1.15)
Percent of Pop. with BS			2.84	2.77	2.87	2.87
Degree or Higher			(2.47)	(2.51)	(2.54)	(2.60)
Percent Owner Occupied			-1.28	-1.30	-0.38	-0.42
			(1.84)	(1.82)	(2.03)	(2.00)
Percent of Pop. in Urban Area			1.22***	1.26***	1.02**	1.04**
			(0.44)	(0.43)	(0.45)	(0.45)
Percent of Pop. 65+	-0.30	2.74	4.25	9.82*	7.50	13.45**
	(2.92)	(4.1)	(4.46)	(5.47)	(5.23)	(6.300)
Percent of Pop. 65+ from		15.53		27.65		29.05
Outside the County		(15.18)		(18.07)		(19.01)
Percent of Pop. 65+ * Percent of	-1.16**	-2.30*	-1.40**	-3.44**	-1.32**	-3.42**
Pop. 65+ from Outside the County[a]	(0.52)	(1.20)	(0.61)	(1.45)	(0.62)	(1.55)
Percent of Pop. 25-34					10.07	9.40
					(7.92)	(10.34)
Percent of Pop. 25-34 from Outside						-1.85
of County						(6.88)
Percent of Pop. 25-34 * Percent of Pop.					9.56	21.77
25-34 from Outside the County					(27.82)	(64.48)
Percent of Pop. from Outside of County			-2.84	-3.49	-6.86	-7.42
			(4.17)	(4.22)	(7.09)	(7.22)
Vote occurred 2008-2013	0.26	0.28	0.43	0.48	0.45	0.49*
	(0.27)	(0.27)	(0.29)	(0.30)	(0.29)	(0.30)
Constant	1.34**	0.94				
	(0.57)	(0.69)				
H_0: $\beta_{65+\text{ from outside county}}$ + $\beta_{\text{perc pop 65+ * 65+ from outside county}} >= 0$		.022**		.007***		.012**
AIC:	453.02	454.25	440.48	440.6	442.48	444.34
Observations Correctly Predicted	81.3%	81.7%	83.1%	83.1%	83.3%	83.3%
Observations	503	503	503	503	503	503

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

a: Variable linearized by multiplying by 100 in order to display results more easily in the table.

TABLE 1.3: Generalized Linear Model, Bonds 2005-2013						
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Bond Amount per Household in $1K	-0.09***	-0.09***	-0.05***	-0.05***	-0.05***	-0.04**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Total Population in 1K	0.001*	0.001*	0.0002	0.0002	0.0002	0.0001
	(0.0007)	(0.0007)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Median Income in $1K	-0.04***	-0.04***	-0.037*	-0.037*	-0.034*	-0.035*
	(0.011)	(0.011)	(0.021)	(0.021)	(0.021)	(0.021)
Median Home Value in $100K	0.05***	0.05***	0.010	0.009	0.012	0.010
	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)
# of Prev. 'No' Votes Last 2 years	-0.082**	-0.082**	-0.073**	-0.07**	-0.064*	-0.064*
	(0.040)	(0.040)	(0.036)	(0.036)	(0.036)	(0.036)
Child Dependency Ratio			0.501**	0.506**	0.619***	0.586**
			(0.227)	(0.226)	(0.235)	(0.235)
Racial Herfindahl Index			0.014	0.030	0.053	0.067
			(0.124)	(0.124)	(0.126)	(0.125)
Percent of Pop. with BS			0.437*	0.438*	0.443*	0.450*
Degree or Higher			(0.262)	(0.263)	(0.263)	(0.265)
Percent Owner Occupied			-0.475**	-0.49**	-0.321	-0.314
			(0.220)	(0.217)	(0.253)	(0.249)
Percent of Pop. in Urban Area			0.269***	0.27***	0.23***	0.23***
			(0.068)	(0.068)	(0.073)	(0.072)
Percent of Pop. 65+	-1.34***	-1.198*	0.462	0.875	1.036*	1.606**
	(0.436)	(0.629)	(0.542)	(0.719)	(0.607)	(0.781)
Percent of Pop. 65+ from		0.703		2.145		2.588
Outside the County		(2.189)		(2.474)		(2.485)
Percent of Pop. 65+ * Percent of	-19.64**	-24.78	-26.1***	-41.11*	-24.9***	-44.66**
Pop. 65+ from Outside County	(7.875)	(17.47)	(8.83)	(21.12)	(8.94)	(20.91)
Percent of Pop. from Outside			-0.262	-0.342	-0.664	-0.695
County			(0.569)	(0.573)	(0.935)	(0.937)
Percent of Pop. 25-34					1.828*	2.628**
					(1.043)	(1.327)
Percent of Pop. 25-34 from Outside						0.780
County						(0.806)
Percent of Pop. 25-34 * Percent of					0.109	-6.006
Pop. 25-34 from Outside County					(3.389)	(7.005)
Vote occurred 2008-2013	0.053	0.054*	0.053*	0.056*	0.057*	0.061*
	(0.033)	(0.033)	(0.032)	(0.032)	(0.032)	(0.032)
Constant	0.78***	0.77***				
	(0.078)	(0.100)				
H_0: $\beta_{65+\ from\ outside\ county}$ + $\beta_{perc\ pop\ 65+\ *\ 65+\ from\ outside\ county}$ >=0		.061*		.020**		.012**
AIC:	460.35	462.34	485.54	487.52	489.45	493.41
Observations	503	503	503	503	503	503

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 1.4: Logistic Regressions, Bonds from 2005-2013						
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Bond Amount per Household in $1K	-0.26	-0.24	-0.27	-0.26	-0.27	-0.26
	(0.20)	(0.22)	(0.20)	(0.21)	(0.20)	(0.21)
Total Population in 1K people	0.08**	0.08**	0.07**	0.07**	0.07**	0.07**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Median Income in $1K	-0.08	-0.09	-0.10	-0.11	-0.10	-0.11
	(0.18)	(0.2)	(0.17)	(0.18)	(0.18)	(0.18)
Median Home Value in $100K	-0.054	-0.05	-0.02	-0.02	-0.02	-0.02
	(0.11)	(0.11)	(0.10)	(0.10)	(0.10)	(0.10)
# of Previous 'No' Votes Last 2 years	-1.44***	-1.43***	-0.53	-0.49	-0.67**	-0.64**
	(0.44)	(0.46)	(0.34)	(0.34)	(0.32)	(0.32)
New Bond Within Last 3 Years	-1.23**	-1.24**				
	(0.51)	(0.54)				
New Bond Within Last 5 Years			0.079	0.12		
			(0.32)	(0.32)		
New Bond Within Last 7 Years					-0.20	-0.17
					(0.30)	(0.31)
Child Dependency Ratio	3.72**	3.96**	3.63**	3.95**	3.60**	3.93**
	(1.82)	(1.85)	(1.82)	(1.84)	(1.82)	(1.84)
Racial Herfindahl-Hirschman Index	0.40	0.67	0.33	0.60	0.35	0.61
	(1.13)	(1.16)	(1.13)	(1.15)	(1.13)	(1.15)
Percent of Pop. with BS	2.76	2.73	2.88	2.88	2.85	2.86
Degree or Higher	(2.67)	(2.73)	(2.54)	(2.60)	(2.53)	(2.60)
Percent Owner Occupied	-0.48	-0.47	-0.37	-0.42	-0.47	-0.49
	(2.06)	(2.04)	(2.03)	(2.01)	(2.03)	(2.01)
Percent of Pop. in Urban Area	0.89*	0.90*	1.03**	1.04**	0.99**	1.01**
	(0.48)	(0.48)	(0.45)	(0.45)	(0.45)	(0.45)
Percent of Pop. 65+	8.32	14.41**	7.53	13.59**	7.36	13.09**
	(5.43)	(6.31)	(5.22)	(6.30)	(5.25)	(6.33)
Percent of Pop. 65+ from		29.24		29.59		27.93
Outside the County		(20.48)		(19.03)		(19.14)
Percent of Pop. 65+ * Percent of	-1.38**	-3.53**	-1.31**	-3.45**	-1.33**	-3.34**
Pop. 65+ from Outside the County[a]	(0.64)	(1.62)	(0.63)	(1.56)	(0.62)	(1.55)
Percent of Pop. 25-34	11.57	12.27	10.05	9.32	9.88	9.11
	(8.10)	(10.65)	(7.92)	(10.38)	(7.91)	(10.33)
Percent of Pop. 25-34 from Outside		-0.33		-1.89		-2.00
of County		(7.04)		(6.87)		(6.91)
Percent of Pop. 25-34 * Percent of Pop.	6.29	6.20	10.11	22.80	8.80	22.51
25-34 from Outside the County	(28.16)	(66.95)	(28.02)	(64.37)	(27.94)	(64.83)
Percent of Pop. from Outside of County	-6.80	-7.12	-7.00	-7.62	-6.60	-7.24
	(7.05)	(7.18)	(7.14)	(7.28)	(7.11)	(7.24)
Vote occurred 2008-2013	0.52*	0.58*	0.43	0.47	0.48	0.51*
	(0.30)	(0.31)	(0.29)	(0.30)	(0.29)	(0.30)
AIC:	440.15	438.06	446.21	444.42	446.04	444.02
Observations	503	503	503	503	503	503

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

a: Variable linearized by multiplying by 100 in order to fit results more easily in the table.

TABLE 1.5: Logistic Regressions, Bonds from 2010-2013						
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Bond Amount per Household in $10K	-0.39**	-0.41**	-0.26	-0.27	-0.27	-0.27
	(0.18)	(0.18)	(0.18)	(0.18)	(0.19)	(0.19)
Total Population in 10K people	0.13***	0.13***	0.11**	0.11**	0.11**	0.11**
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Median Income in $10K	-0.16	-0.16	0.03	0.03	0.05	0.07
	(0.14)	(0.14)	(0.28)	(0.22)	(0.22)	(0.22)
Median Home Value in $100K	0.27**	0.27**	-0.04	-0.04	-0.07	-0.10
	(0.13)	(0.13)	(0.14)	(0.14)	(0.13)	(0.13)
# of Previous 'No' Votes Last 2 years	-0.78**	-0.80**	-0.84**	-0.86**	-0.82*	-0.88**
	(0.39)	(0.38)	(0.43)	(0.42)	(0.42)	(0.42)
Child Dependency Ratio			-0.11	-0.10	-0.23	-0.62
			(2.07)	(2.07)	(2.07)	(2.11)
Racial Herfindahl-Hirschman Index			2.38	2.35	2.31	2.33
			(1.62)	(1.62)	(1.59)	(1.61)
Percent of Pop. with BS			1.37	1.37	1.45	1.38
Degree or Higher			(0.85)	(0.85)	(0.95)	(0.95)
Percent Owner Occupied			-3.87	-3.86	-4.29	-4.59*
			(2.55)	(2.55)	(2.63)	(2.65)
Percent of Pop. in Urban Area			1.13*	1.12*	1.25*	1.09
			(0.68)	(0.68)	(0.71)	(0.73)
Percent of Pop. 65+	-0.55	-3.53	3.97	2.07	2.63	2.31
	(6.08)	(8.39)	(8.07)	(9.32)	(8.62)	(9.89)
Percent of Pop. 65+ from		-13.01		-8.96		-7.16
Outside the County		(23.90)		(23.68)		(23.30)
Percent of Pop. 65+ * Percent of	-1.51**	-0.59	-1.80**	-1.16	- 1.77**	-1.40
Pop. 65+ from Outside County[a]	(0.73)	(1.89)	(0.84)	(1.84)	(0.84)	(1.86)
Percent of Pop. 25-34					-5.65	3.60
					(8.39)	(10.89)
Percent of Pop. 25-34 from Outside						11.12
of County						(7.01)
Percent of Pop. 25-34 * Percent of Pop.					13.26	-63.50
25-34 from Outside County					(28.92)	(45.30)
Percent of Pop. from Outside County			-5.59	-5.47	-8.17	-9.53
			(6.68)	(6.70)	(9.93)	(9.77)
Constant	1.47*	1.85*				
	(0.87)	(1.11)				
H_0: $\beta_{65+ \text{ from outside county}}$ + $\beta_{\text{perc pop } 65+ * 65+ \text{ from outside county}}$ >=0		.333		.222		.188
AIC:	199.65	201.43	203.56	205.49	207.27	209.58
Observations	219	219	219	219	219	219

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

a: Variable linearized by multiplying by 100 in order to display results more easily in the table.

TABLE 1.6: Logistic Regressions, Bonds from 2010-2013						
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Bond Amount per Household in $10K	-0.10	-0.08	0.39	0.41	0.40	0.28
	(0.55)	(0.56)	(0.57)	(0.57)	(0.57)	(0.60)
Total Population in 10K people	0.15***	0.15***	0.12**	0.12**	0.12**	0.12**
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Median Income in $10K	-0.17	-0.17	0.09	0.10	0.10	0.15
	(0.14)	(0.14)	(0.24)	(0.24)	(0.24)	(0.26)
Median Home Value in $100K	0.23*	0.22*	-0.117	-0.124	-0.141	-0.185
	(0.12)	(0.12)	(0.15)	(0.15)	(0.14)	(0.14)
# of Previous 'No' Votes Last 2 years	-0.78**	-0.77**	-0.76*	-0.73	-0.75*	-0.79*
	(0.38)	(0.38)	(0.45)	(0.44)	(0.44)	(0.45)
Child Dependency Ratio			0.12	0.11	-0.04	-0.16
			(2.45)	(2.46)	(2.47)	(2.56)
Racial Herfindahl-Hirschman Index			3.02	2.99	2.98	3.16*
			(1.89)	(1.89)	(1.85)	(1.90)
Percent of Pop. with BS Degree or Higher			1.08	1.07	1.11	0.98
			(0.93)	(0.93)	(1.06)	(1.06)
Percent Owner Occupied			-4.91	-4.94*	-5.29*	-5.72*
			(2.99)	(2.98)	(3.00)	(3.02)
Percent of Pop. in Urban Area			1.18	1.20	1.25	0.95
			(0.90)	(0.91)	(0.94)	(0.96)
Percent of Pop. 65+	3.45	5.78	9.80	12.55	8.440	12.95
	(6.28)	(10.31)	(9.81)	(13.03)	(10.74)	(14.17)
Percent of Pop. 65+ from Outside the County		9.02		11.39		10.80
		(28.33)		(28.67)		(27.98)
Percent of Pop. 65+ * Percent of Pop. 65+ from Outside the County[a]	-1.84**	-2.49	-2.22**	-3.02	-2.11**	-3.07
	(0.77)	(2.23)	(1.02)	(2.35)	(1.02)	(2.37)
Percent of Pop. 25-34					-4.24	6.30
					(8.96)	(12.39)
Percent of Pop. 25-34 from Outside County						11.37
						(7.54)
Percent of Pop. 25-34 * Percent of Pop. 25-34 from Outside County					18.32	-63.26
					(30.95)	(50.57)
Percent of Pop. from Outside County			-4.23	-4.36	-8.56	-8.74
			(7.78)	(7.79)	(11.32)	(10.93)
Constant	1.15	0.83				
	(0.94)	(1.43)				
H_0: $\beta_{65+ \text{ from outside county}} + \beta_{\text{perc pop } 65+ * 65+ \text{ from outside county}} >= 0$		.112		.082*		.082*
AIC:	183.36	185.28	190.76	192.68	194.54	197.11
Observations[b]	201	201	201	201	201	201

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

a: Variable linearized by multiplying by 100 in order to display results more easily in the table.

b: Data does not include observations where the Percent of Pop. 65+ from Outside the County = 0

Chapter 2: School Facilities and Educational Attainment: Quasi-experimental Evidence from a Regression Discontinuity in Voting Outcomes

1. Introduction

Government financing of public education has long been a topic of interest to social scientists. Education is viewed as an important micro level determinant of human capital and a macro level determinant of economic growth (e.g., Krueger and Lindahl, 2001; Hanushek, 2013). Widespread access to education has also been shown to reduce income inequality (Sylwester, 2002; De Gregorio and Lee, 2002). Scholars and policy makers recognize high quality education as a potential pathway towards social mobility for disadvantaged youth (Ladd, 2012). Put another way, both efficiency and equity related social goals seem to be enhanced through better education.

At the same time, public education is costly – primarily financed by state and local budgets that cover essentially all labor costs, more than 90% of the ongoing costs associated with existing facility maintenance/repair/utilities, and essentially all of the costs of building new facilities (NCEF, 2016).[5] Motivated by these tradeoffs, many studies have investigated the linkages between education spending and student outcomes.[6] However, widespread school finance reform has left most US states with funding equalization formulas that break the deterministic linkage between local property taxes and spending on public education (Hoxby, 1998; Downes and Shah,

[5] The National Clearinghouse for Educational Facilities has extensive data on facilities in the US.
[6] A review of the literature investigating public school spending and student outcomes lies beyond the scope of this paper. Interested readers could see Hanushek (1997) or Hattie (2008). Several papers focusing on the effects of class size, school facilities, and the school bonds are discussed below.

2006), creating an environment where per pupil spending is flat by design. In light of this constraint, we follow an approach others have used, turning attention to an important mechanism that local constituencies still retain autonomy over - publicly voted on property tax bonds used to fund construction of new schools and for expansions/upgrades to existing facilities. The intuition is that even in a fiscal landscape dominated by equalization formulas, local education bonds still capture marginal autonomous spending that can be related to attainment outcomes (commonly measured as reading and math scores on standardizes tests, which we also use).

We use a regression discontinuity design (RDD) and student level fixed-effects models to estimate the causal effect of arguably random infusions of capital resources for school districts in Texas that come from narrowly passed and failed bond votes.[7] As was discussed in Chapter 1, Tiebout sorting should generate conditions where some communities are more/less likely to pass educational bonds than others, leading to concerns over the endogeneity of bond passage. However, the frequency of voting over school related bonds in Texas facilitates an RDD approach that we complement with other supporting analyses. Of the 1,018 Texas school districts we have represented in our 4,342,285 student level panel, 104 school districts (10.2%) experienced a narrow bond passage (defined as falling within a 3 percent margin of victory) while 100 school districts (9.8%) experienced narrow failures. We show these districts are otherwise comparable on a number of observable dimensions, leading to the claim that exposure

[7] Regression discontinuity stems from Thistlethwaite and Campbell (1960) and has been the focus of much attention since. Imbens and Lemieux (2006) and Lee and Lemieux (2009) provide expositions. RDD studies appear in the education literature. Ou (2010) estimates the impact of failing a certification exam on dropout rates. Jacob and Lefgren (2004) show the impacts of access to a remedial instruction program on student achievement. We later discuss the two papers we know of that have applied technique to local education bonds votes to explore the effects of bond passage on student achievement.

to bond passage is the primary difference between students falling into our treatment and counterfactual control groups. As such, we argue the causal effects of the events that predictably follow bond passage (e.g., new construction, renovation/expansion, reshuffling students and/or teachers, changes in average class size and composition, etc) are aggregately approximated by our empirical models. While ‘lumping’ these independently interesting channels together can be viewed as a weakness of our work, we argue that when faced with the longstanding challenge of providing quality education to their students, school districts across the US and abroad turn to voters with bond requests for financing new or renovated facilities – motivating the aggregated effect we capture as worthy of attention.

Whether infusion of additional funds will affect student achievement is not at all clear, given the current literature. The prevailing attitude toward spending and student achievement has changed little since an influential 1986 article by Hanushek, which concluded there was not a significant positive effect of education spending on student achievement. In response though, many papers have tried to refute this claim. However, the majority of this research does not examine the causal effect of spending specifically, instead framing additional spending as a necessary first step toward changing the classroom environment (e.g., lowering student-teacher ratios; Card and Krueger, 1990; Wenglinsky, 1997) or improving teacher quality (Harris and Sass, 2011). Of particular interest are studies that focus on spending changes due to school finance reforms, for example Roy (2011) and Guryan (2001). These changes represent arguably exogenous shifts in district level spending that offer stronger identification. Roy found increased school spending in Michigan positively impacted the test scores of

4th grade students. Using data from Massachusetts, Guryan also found positive effects for 4th grade students, but did not find significant effects on the performances of 8th graders. Of relevance to our study, their results also suggest the largest benefactors of spending increases were low performing students and students in the low-spending districts (which although correlated, still represent distinct groups).

Based on the existing literature, we assume the effects of opening a new school on student achievement may be characterized by relatively long lasting positive/negative impacts. An exploration of the effect of education spending on home prices (Cellini, Ferreria, Rothstein, 2010) found the passage of school bonds in California led to increased home values. The authors posit that the effect is due to preexisting equilibrium levels of education spending that are inefficiently low. As we mentioned above, the benefits of expanding education spending have primarily been found in underfunded communities', lending additional support to the idea that underfunded and/or underserved school districts will experience the largest impacts on student performance. This highlights the importance of implementing a regression discontinuity design rather than simply registering the passage/failure of bonds, as the districts most likely to support spending for school construction (i.e., pass bonds) could easily be the districts that are the most severely underserviced in terms of current educational facilities. Note that an alternative story surfaces from the idea that wealthy communities may consistently fund public education to a high degree – leading to a case where the passages may come from the environments where impacts are the least likely to be significant. Regardless of which influence is stronger, or if the two offset one another, it would be difficult to generalize the results of empirical models that did not effectively control for the likelihood of band passage. Hence, we argue the RDD controls for this endogeneity and produces generalizable results.

While our study by no means resolves the longstanding disagreements in the literature, we do believe it contains valuable estimates of how school bond passage affected the performance of students over time in Texas, and that our results carry applicability to other environments, given the diversity of the students in Texas. Texas has significant urban and rural areas, considerable wealth and poverty, and displays significant racial/ethnic diversity in its student pool. In many ways, the differences between Texas and other US states actually make it an ideal target for our empirical analysis. Texas is the second largest and second fastest growing US state. Between the 2000 and 2010 census, the population aged eighteen or younger grew by nearly 1 million (a 16.5% increase from 5,906,301 to 6,878,896). Population growth places a strain on education infrastructure, which must expand to accommodate growth in the number of children. Unsurprisingly, Texas has been experiencing problematic school overcrowding, especially in population centers (Texas Education Agency, 1999). Additionally, according to a recent survey of school facilities (Taylor et al., 2005) the average (mean) age of permanent education structures in Texas was 24 years old, while roughly 50% of permanent structures were at least 27 years old. Given that the life expectancy of a public school building has been estimated as 50 years (Bureau of Economic Analysis, 2003), school districts in Texas have been forced to expand the stock of their educational facilities. Local property tax bonds are the primary mechanism to accomplish this.

The construction of these new facilities is costly; according to the Texas Comptroller of Public Accounts the cost of a new elementary school ranges between 5 and 25 million dollars. With these considerable resources at stake, policy makers and voters would benefit from a comprehensive understanding of the benefits that a new campus can provide. In the absence of building new schools, other solutions to dealing with overcrowding have of course been advanced. These include the use of portable classroom space, adopting multi-track year-round calendar systems (Graves, 2010), and redrawing service boundaries when districts try to smooth distributions between over and under populated schools (Elizondo, Boyd, and Beauregard, 1997). Still, each of these steps is typically characterized as a reaction to inadequately providing traditional facilities due to budgetary constraints. From this perspective, the implicit assumption is that an expanded stock of school facilities would be preferable to all parties involved in the education system (i.e., costs are the only deterrent).

However, studies have also shown students experience negative side effects of new school construction, including disruption of the classroom environment (i.e., changing peer groups and mobility of faculty) and being burdened with short-lived fixed costs associated with moving into a new environment. For instance, a study of school closures showed that students who moved to new schools had more instances of absenteeism (Engberg et al. 2012). And while the negative effect identified was temporary, it still represents a significant non-pecuniary cost. As such, measuring the net effect on student achievement that stems from opening new schools is a worthwhile endeavor. Importantly though, it is worth noting that the specific costs and benefits associated with transitioning to the newly constructed facility are only experienced by the relatively small fraction of students who attend it. That is to say, most students in the school district simply stay put. This is of great importance for policy makers and school board officials who are expected to follow the interests of parents/voters in circumstances where a majority of the students in the district may never attend a new school. We address this important question – focusing on how exposure to a newly passed school bond impacts the reading and math attainment of the students who stay placed at the same school – even after the bond – shedding light on the experience of a large, but surprisingly understudied, subgroup of students.[8]

Additionally, because we employ a lengthy panel, we are able to evaluate the potentially asymmetric effect of bond passage on students of various pre-existing levels of achievement. While our baseline models use the full sample, we also estimate specifications using only high performing students and only low performing students (defined by pre-treatment test scores), finding meaningful asymmetric effects between the two groups. Understanding the impacts for high and low performers is a valuable tool for policy makers, as the benefits (costs) that accrue to one group of students may differ from the impacts on others. For instance, poorly performing students have been found to be at higher risks for dropping out of school early (Cairns, Cairns, and Neckerman, 1989). These effects have been shown to exhibit path dependency from early ages, with poor performance in elementary school still carrying a positive correlation with the probability of dropping out after controlling for recent/current

[8] Note that estimating the average treatment effect for a student staying in place, and estimating the average treatment effect for all students in the district are not identical exercises. We later present evidence suggesting the characteristics of displaced students following quasi-random bond passage are not representative of the overall student population. A distinguishing feature of our analysis is that, to our knowledge, we provide the first estimates of how bond passage affects the group of students who stay put in the same schools – a plausibly interesting group and certainly a large one.

grades (Ensminger and Slusarcick, 1992). Therefore, a policy maker looking to increase retention rates among students may wish to heavily weight benefits to low performing students. Conversely, officials seeking the attention of high profile awards and scholarships (national merit scholars for example), may follow the opposite approach. Finally, early academic success carries long term impacts as a predictor for college performance and degree completion (Camara and Echternacht, 2000).

We find new facilities carry a small, but statistically significant, mean effect on reading and math scores. Once the full dynamic impact is registered, we estimate that achievement (measured by standardized reading and math test scores) rise by about 1/10th of one standard deviation. Additionally, when the analysis turns to the effects on the highest and lowest achievers, significantly larger effects (of opposing directions) surface. We document a negative effect on the highest performing students, while at the same time seeing a larger-than-mean positive effect on the test scores of the lowest performing students. Specifically, students in the lowest quintile have test scores that rise by nearly 4/10ths of one standard deviation when exposed to a new campus activation. Put another way, exposure to additional capital facilities in the district helps struggling students, but has a negative impact on the top students. Recalling the significance of our choice to focus on the large group of students who stay put, we later provide evidence to support the explanation that this asymmetry is largely driven by the non-random nature of student 'resorting' that plays out over the years following bond passage, tapping into the rich literature on student peer effects.

We next present a brief discussion of the literature considering school construction and student achievement. Section III describes our data and Section IV outlines the two main empirical methodologies we employ. A discussion of our results follows in Section V. Finally, Section VI concludes and offers policy implications.

2. Background and Theory

A. School Construction and Student Achievement

Three related literatures frame our study. The first investigates the effect of education spending on student achievement. Investigations of the effect of education spending on student achievement have a long history, likely due to the mixed results contained in the literature. Dating to the Coleman report in 1966, which concluded that government spending had limited benefits to student achievement, and to the work of Haneshek (1986), which also corroborated the lack of a systemic relationship between spending levels and student achievement – a theme in the literature is the claim that student achievement is not sensitive to the level of local spending. However, even within this environment, many influential studies have demonstrated positive effects of school spending on student achievement (e.g., Greenwald, Hedges and Laine, 1996; Card and Krueger, 1996; Holmlund, McNally, and Viarengo, 2010).

The branch of this literature that is most applicable to our paper comes from studies that investigate the effects of school construction. For instance, Bowers and Urick (2011) consider school facilities and their level of maintenance/upkeep, arguing the physical condition of the facility itself most directly influences the daily learning experience of students. They found no effect on student achievement. Since our work focuses directly on the benefits of expanding classroom facilities in school districts through school construction and/or major renovations (i.e., the passage of local school bonds), a particularly relevant contribution comes from Nielson and Zimmerman

(2014), who examine the effect of a comprehensive school construction program in New Haven, Connecticut on a number of outcomes, including student achievement on standardized math and reading exams. The authors examine students in affected neighborhoods using student level fixed effects - a method we employ in this paper - to control for unobservable time invariant characteristics, estimating the effect on students attending schools in the same area as the new school construction. They find a significant positive effect on student's reading scores, but see very little evidence to support the idea that math scores improved. The gain in reading scores is experienced in the first year following construction and grows in size with longer exposure to the new facility. The cumulative gain by the sixth year comes to roughly 0.15 standard deviations above the pre-exposure achievement level.

While our student level fixed effects models exhibit meaningful similarities to theirs, we extend their analysis in at least three critical ways. First, we carry the research question to another identification strategy – which we view as a form of a robustness check. The difference-in-difference approach used by Nielson and Zimmerman (2014) rests upon the fact that different neighborhoods were exposed to the new construction at different times, all according to a pre-selected master construction plan that did not correlate with the trends in neighborhood characteristics of student performance levels. Indeed, they include evidence in favor of the similar trends assumption needed to validate their models. Still, exposing this important question to another of the main quasi-experimental methods (regression discontinuity) is worthwhile. Second, we focus directly on the large pool of students who remain in the preexisting schools at the time of the quasi-random bond passage – shedding light on an important aspect of the policy dynamics at play: that enrollment into newly constructed schools is not random with respect to student ability. For this reason, classroom composition effects merit attention. This dovetails into our third extension; we examine the possibility that bond passage carries differential effects on high and low performing students. While exploiting quasi-experimental variation in school facilities spending through an RD approach can easily be viewed as a limited extension of the literature, we argue the other two extensions are non-trivial.

Another study with close motivational ties to our work comes from a policy brief by Welsh, et al (2012), which examines the effect of a large scale school building and renovation effort in Los Angeles – one of the most overcrowded urban school districts in the nation. The authors examine the effect of moving to a new school on the moving students, finding significant effects. They additionally examine the students left behind – as we do in the present analysis – arguing those students then experience less crowded learning environments. While they find no significant effect on math scores, they document positive effects in language arts. We extend their work by providing a more representative analysis of the school construction occurring in a wide variety of urban, suburban, and rural areas, with differing degrees of facility inadequacy, as opposed to focusing on a single intensely overcrowded urban district area. Policy makers may be appropriately skeptical of the applicability of the Los Angeles driven findings to other settings. Also, as was the case with Neilson and Zimmerman, the possibility of differential effects on the highest and lowest performing portions of the achievement distribution was not considered.

Another recent study that carries connections with our own is a 2015 NBER working paper by Martorell, Strange, and McFarlin (hereafter MSM). Similarly to our own work, MSM employ a RD approach to explore the effects of education bonds on student achievement using data from Texas. Although they clearly show bond passage is associated with substantial increases in capital expenditures and that new schools are more likely open in the 2-3 year window following bond passage, they document minimal effects of bond passage on student performance. Although their point estimates of effects on tests scores are generally positive, they are very small; and in the cases where school openings are specifically examined, they frame their findings as precise zero estimates of achievement effects. While their work represents a strong advancement of the literature, there are noteworthy methodological differences between their work and ours, as well as fundamental differences in the specific research questions taken to the data. Most importantly, MSM examines student performance averaged across school districts as the dependent variable of interest, whereas we examine student level outcomes over time using individual fixed effects to control for unobservable differences in student ability. The though experiments are highly related – but differ in a nuanced way. MSM asks what happens to a school district exposed to a random shock, whereas we ask what happens to a student exposed to a random shock. Since students are characterized by more considerable heterogeneity than school districts are, the shift facilities investigation of the asymmetries regarding high/low performing and staying/moving students that we find to be meaningful.

Additionally, MSM complement their quasi-experimental bond passage RD models with a focus on changes in school facilities (combining cases of new schools with cases of renovations), whereas our paper complements similar RD models with estimates that focus directly on new school construction – as observed through a data source that reports the specific year (and month) of school openings. While neither complement is necessarily preferable to the other, there is value in having both present in the literature, as intuition suggests the effects of new school openings and school renovations may not have identical effects. Additionally, while both studies use the bond voting results to generate a regression discontinuity, MSM use the entire population of education bond votes, whereas we only use votes that are described as leading to building new facilities or authorizing new land purchases (i.e., the subset of bonds most clearly leading to new school construction). Finally, another important difference between this paper and MSM's contribution is our focus on students who remain in existing schools when a new school is opened or a major renovation occurs, compared to their approach of measuring the mean effect across all students. This design difference complements our exploration of differential effects for high and low performing students, which is also not considered in the MSM study.

B. Facility Characteristics, Class Size, and Student Achievement

The second literature supporting our work comes from the large number of studies considering the effects of class size, overcrowding (a related but not identical phenomenon), and facility aging on student achievement. Perhaps due to the growing number of datasets reflecting student-level class size randomizations, class size has been investigated far more commonly than other school characteristics. Interest in class size effects is also longstanding; Glass and Smith (1979) explored a set of largely inconsistent empirical findings regarding class size effects nearly four decades ago.

Some would say the ‘modern’ literature on class size effects began in 1990, when Finn and Achilles published their influential analysis of the Tennessee STAR experiment. After this point, randomization of some form became largely required to defend empirical findings. They found positive and significant benefits for students from a reduction in class size. However, while the Tennessee STAR experiment employs the power of a designed natural experiment to examine the effect of class size on student achievement, the applications of this relatively small experiment are limited. Other studies using quasi-experimental designs come to similar conclusions. The quasi-regression discontinuity design facilitated by forced classroom splits (i.e., caused by exceeding the maximum student to teacher ratio following a policy change) used by Angrist and Lavy (1997) resulted in strong positive effects from reductions in class size surfacing. However, a similar examination of split classrooms from Hoxby (2000) finds no relationship between classroom size and student achievement. A recent study exploring the effects of class size using the number of local births in a given year as an instrumental variable for the endogenous class size variables of interest, estimates a much smaller gain in achievement than the Tennessee STAR experiment (Cho, Glewwe, and Whitler 2012), but still suggests the effects is positive.

We could continue this review for some time, as the number of excellent studies on class size effects is impressive and continues to increase. Instead, we summarize by noting the estimates of class size effects have varied. While most studies find smaller class sizes enhance achievement, well executed work supports both sides of this controversial coin. Unfortunately, the student level panel data used in this paper is not capable of directly linking students to their classroom, which means we are not able to contribute directly to this debate. However, since our empirical results are best interpreted as capturing the general treatment effect associated with exposure to the quasi-random passage of educational bonds, it is natural for us to point out that reduced class sizes are one potentially important influence on student achievement that is a component of our treatment effect.

C. Classroom Peer Effects and Student Achievement

The third avenue of relevant research comes from work considering the effect of classroom peers. Recall that we argue it is worth looking specifically at the large group of students who 'stay put' through the bond passage transition, since they represent a large sub-population worth attention. Attention for the group (relative to mean effects estimated for the entire population of students) is only merited under two conditions: 1. Selection out of old schools and into new schools is not random with respect to student ability, and 2. Peer effects (broadly defined) matter. We note a 'broadly defined' interpretation since changes in the composition of students in the classroom could lead to a number of other adjustments including teacher strategies, student attendance, and resource allocation decisions. We later present some evidence from our own data to support claim 1. Regarding claim 2, peer effects can have positive or negative effects on student outcomes with regards to motivation and disruptive behavior in the classroom (Kindermann, 1993 and DuPaul et al, 2013). Of particular interest to our work is the nature of peer effects with regards to student achievement. If peer effects are monotonic (Pivovarova, 2013), meaning that the highest benefit to students in classes is gained from having more high achieving students, then any change to the distribution would produce student level changes that are a zero sum game (e.g., taking high performing students from one classroom to another will reduce the positive peer effects for one set of students and increase them for others).

However, it is also possible that peer effects are non-monotonic, with the benefit of having high performing students always outweighing the benefit of average performers, but that the peer effect is asymmetric in terms of the effect of increasing the number of high performers versus increasing the number of low performers. It is also possible for peer effects to be monotonic, but for the overall net effects to still be something other than a zero sum game. For instance, Betts and Zau (2004) find asymmetric peer effects, but they still find that peer group effects always favor increasing peer test scores and that an increase in the performance level of one's peer group has a smaller positive effect on achievement than a similarly sized decreased performance level of the peer group. Put another way, exposure to high achieving students carries a small positive effect and exposure to low achieving students carries a larger negative effect.

It is also possible that peer effects are more nuanced than either of these cases. Instead of assuming that higher performing peers are always better for student achievement, it is possible that peers of a given level of achievement could have different effects on different types of students. Interesting cases can occur where subpopulations of students benefit more from average, or even below average peers. This possibility is noteworthy, since it facilitates instances where average test scores could increase for two different groups with the reallocation of students and the changing of peer groups. Burke and Sass (2013) demonstrate that low achieving students experience larger gains from exposure to more students with achievement in the middle of the distribution, compared to greater exposure to high achieving students. For example, this would be expected if it were more likely that study pairs formed when the size of the initial achievement gap between students was smaller.

Therefore, previous research suggests it is possible to increase the performance of low achieving students, not by increasing the exposure to high performing students, but in fact by reducing exposure to high performing students. Hence, high performing students may exhibit positive spillovers onto one another, but the idea that they exhibit a uniformly positive effect of the same magnitude on all students is less defendable (and perhaps even entirely incorrect). Of particular note, Hoxby and Weingarth (2005) find evidence in favor of the ‘Boutique model’ of peer effects, which finds positive benefits of being situated within homogeneous peer groups. According to the Boutique model, it is actually possible to produce benefits for both high and low performers through more homogenous ability grouping - with high performers improving other high performers and low performers improving by working with other low performers. Therefore, as the opening of a campus will result in the mobility of students and the change of classroom peer groups, the resulting peer effects could move in a number of possible directions and are worth attention.

D. Contributions and Extensions

In this paper we bring these three related literatures together and expand on previous work by implementing a regression discontinuity design focused specifically on bond votes in Texas that are the most likely to result in school construction. We do this to more precisely identify the effect of school construction in estimations that control for problematic endogeneity issues. We also separately explore the effects of campus activations on the limited population of students who remain behind after the activation. This shows policy makers an important effect that accrues to the large population of students that does not directly experience the costs and benefits of relocation and exposure to a newer (and likely structurally enhanced) facility. Additionally, while we estimate the net (mean) effects of school openings, we also explore the asymmetric effects on high and low performing students to illuminate which channels (i.e. class size effects, peer effects, facility quality effects) are most responsible for produced the net effects. This should be of direct interest to those focused on equity related concerns, as well as those interested in understanding the impacts of school facilities on particular portions of the achievement distribution.

3. Data

To investigate the relationship between the activation of a campus and student achievement, this paper estimates models using three primary data sources from the state of Texas: student level math and reading test scores, school district bond results, and school campus activation data. Our student level testing data was provided by the Texas education agency. It contains results for all reading and math standardized tests taken by Texas students in grades three through eight, starting with the 2003-2004 school year and running through the 2013-2014 school year. Beyond reporting the school of attendance, unique student ID variables allow us to track test scores from the same student over multiple years – an important detail since it facilities our use of student level fixed effects. The testing data spans two different standardized tests taken by Texas students. The Texas Assessment of Academic Skills (TAAS) test was in place from the beginning of our panel up through our 2010-2011 observations. At that time, the state switched to the State of Texas Assessment of Academic Readiness (STAAR) test, which covers 2011-2012 through the end of our panel. Unfortunately, the tests use different scales and are therefore not directly comparable. We normalized the reported math and reading scores for both exams through transformation into a standard normal distribution. Combined reading and math scores are standardized using the same procedure.

Our bond data contains results for Texas school district level bonds voted on during 1995 through 2015. We limit our sample to the 2,197 ballot initiatives proposing to generate facility construction and/or land acquisitions. While the vast majority of these bonds passed, our approach relies heavily on the 103 cases (4.7%) which passed by less than 3% and the 99 cases (4.5%) that failed by less than 3%. Since many bonds are used to fund other items (e.g., transportation needs, general maintenance, and athletic facilities), this choice increases the likelihood that passage of the bonds we consider will actually lead to building new school facilities. Unfortunately, the description of the bond's purpose is a simple labeling system that does not allow for precise determination of which bonds are requesting money to build new campuses (i.e., instead of significantly renovating/expanding existing campus facilities). This means that our later analysis of the effects of quasi-random bond passage on student achievement (our treatment of interest) cannot be directly linked to the building of a new campus. However, with the bond question limited to the school building and land acquisition classification, we expect passage to be highly correlated with campus construction, and to corroborate results from examining the effect on test scores from campus activations. Table 2.1 provides evidence supporting this expectation. School districts experiencing passage of a bond (by any margin) are significantly more likely to experience a new school opening in both the 3 year and 5 year windows following passage than districts experiencing no outcomes as well as the full sample at large. Furthermore, using similar data on Texas educational bonds, MSM demonstrate bond passage leads to significantly increased capital spending levels during the three year window that follows.

Because the circumstances leading districts to hold and/or pass bond votes are expected to be nonrandom, and since factors influencing these likelihoods could easily have their own independent effects on student achievement levels, standard OLS regressions seeking to determine the effect of bond passage on student test results could easily produce biased results. Therefore, our analysis mitigates this potential endogeneity bias by implementing a regression discontinuity design. Because the number of votes in favor and the number of votes against are reflected in the data, we are able to precisely calculate the percentage of support for a given bond. The percentage of supporting votes then leads to a quasi-experimental design under the assumption that districts narrowly passing votes are sufficiently similar to the districts that narrowly fail votes, such that they form reasonable treatment/control groups. This leaves the application of the treatment (bond passage) as quasi-randomly assigned. Hence, in addition to being assigned a dummy variable for the passage or failure of a bond (defined under the traditional measure), we also assign districts with dummy variables indicating the presence of marginal (within 3%) passage or failure.

Out of the 1,018 independent, combined, and municipal school districts operating in the state of Texas, 99 districts had a bond measure marginally fail, while

103 districts had a bond measure marginally pass. The demographic data reported in Figures 2.1, 2.2, and 2.3, which is reported for districts experiencing marginal failure, marginal passage, and all school districts (for reference), provides at least some evidence that the formation of our treatment/control groups is valid. For example, from Figure 2.1 which uses school district level information found in the 2010 decennial census, we find these groups have strikingly similar age distributions. In particular, we highlight the minimal difference between the marginal pass and marginal fail districts in the young and old categories. For a number of reasons, these are the two age groups that are expected to have the strongest effect on bond passage (e.g., more school age children puts pressure on the system and, most likely, means more voters in the district are parents with children in the school system).

Similar evidence that our regression discontinuity assumption holds comes from Figure 2.2, which reports the median district's percent of the population within different income brackets. This data comes from the American Communities Survey (the 2010 five year estimate) and is also reported at the school district level. Again, the difference between the marginal passage and the marginal failure districts is very small, as is the difference between districts experience passage or failure compared to all districts. Finally, Figure 2.3 reports the median district's percentage of the population split among urban and rural residents. Here, the median value for the marginal passage and failure districts are very similar to one another, although we do see that both are quite different from the overall median Texas school district (which is decidedly more rural than either group). While we do not interpret this as evidence that our RD assumptions are violated, it suggests more generally that districts with close votes are not representative of the full sample, which contains districts that less frequently hold votes and cases where voting outcomes are not close. Of course, the RD technique applied to bond votes has commonly been used in the literature, and the same critique could be made of those applications as well. On net, we are comfortable with the evidence we see that the RD strategy is valid in our application.

The final component of our data is the Texas directory of schools, a data set that is maintained by the Texas Education Agency. These files document the most recent date when a campus became categorized as an active school. The school district is also reported for each facility, allowing us to merge this information with the student level files that also report school districts. For the majority of school facilities, this file contains a straightforward recording of all the relevant information we need about the school. However, it is important to note that rare circumstances can lead to cases where the ‘activation’ date recorded is not necessarily the same as the date that a school opens. In cases where a previously operating campus experiences a temporary change in status (e.g., closes for renovation), the data entry for the facility will reflect the date of re-opening, not the original opening date. Of course, it is possible that a campus renovation and subsequent re-opening has a different effect on our main variables of interest than would a campus that is newly opened. Unfortunately, the data provide no help distinguishing the two cases, so our analysis treats these distinct cases as having the same impacts.

When a campus’ status is changed to active, the school year in which activation occurs is identified with a dummy variable as being year 0. Subsequent school years that follow are then given corresponding sets of sequential dummy variables (e.g., year

1, year 2, etc.). This process restarts for a school district once a new facility is activated and the sequence returns to 0. So for example, if a district opens new campuses in 2005 and 2010, then 2005-06 is recorded as 0, as is 2010-11, and the four school years in between register as year 1 through year 4. The result is a series of dummy variables that identify when the most recent campus activation occurred for any given school district/year observation. One advantage of this approach is that, when later paired with the inclusion of student level fixed-effects, the timing of campus activations provides an easily formed control group for our regression analysis. Since campus activations take place across various years, there are districts that experience openings early in the data and other districts that activate a campus later in the data. Students in a district/year not experiencing campus activations act as a control for students in a district/year experiencing activations. When paired with student level fixed effects that control for unobserved differences across students, our later regression analysis is able to isolate the variation in student achievement that is associated with campus activation.

The primary drawback of this approach is that it does not directly address the issue of multiple campus openings over a small span of years, as each opening is treated as an isolated occurrence. This could overstate the effect of a campus opening, as the opening could be paired with another opening that occurred just before it. However, this problem should diminish through our evaluation of lagged effects, as any observation with a lagged activation will not have experienced activation in the interim. Another shortcoming of this approach is that it limits the number of instances of observations representing long spans between new campus activations. We address this issue by creating a single variable to account for long lagged observations to increase these instances, and to reduce the possibility of a small number of observations driving the results of our later regressions.

Only districts with a campus activation during the study window are included in the event study. Of the available districts, 170 districts had at least one campus activation. These campuses contain 4,704 campuses, which account for over 60% of all of the campuses in independent, combined, or municipal school districts, and 3,177 campuses not activated during the time of the study.

4. Empirical Methodology

To investigate how opening a new school affects students' performance, two main types of regressions are presented in this paper. Both approaches use student results on standardized reading and math tests as the dependent variable to register the effect on student achievement. Both sets of models also employ student fixed effects and clustered standard errors (at the school facility level) to account for factors that may influence achievement that are not observed. Finally, the regressions each contribute to the analysis of student achievement in existing schools when a new campus is activated. The regressions differ in terms of the independent variables used and the empirical approach to measuring the activating a campus.

Before a campus activation takes place, a decision must be made by the school district regarding whether or not to build a new campus (or renovate/expand a current campus) in lieu of using portable buildings or other less costly alternatives. The benefit of starting at this early point in the process is that it provides an opportunity to control for the differences in districts that can affect the likelihood of deciding to move forward with construction, before campus activation ever takes place. The population of districts holding a bond vote may not be representative, as districts expecting to lose a bond request can opt to not hold a vote all together. We attempt to control for this issue by limiting the population of bonds that influence our estimation of the effect of interest through a regression discontinuity centered on the 50% passage threshold. The regression discontinuity limits the included data to students who attend campuses in school districts that have experienced bond votes passing or failing within a three percent margin of victory. Limiting the regression to only these students mitigates potential selection bias caused by including school districts with varying levels of need and support for education. We find evidence to support the randomness of passage around the forcing threshold (i.e., we have a 'smooth' distribution around 50%, following the common test to rule out bunching proposed by McCrary 2008) and similar observable characteristics for districts falling into the passing and failing groups around the cutoff. For a given outcome related to student achievement A (reading scores, math scores, aggregated total scores), from student *i*, in year *t*, attending school in district *j*, at school campus *k*, we estimate RD models as:

$$Y_{i,k,j,t} = \beta_1 \, MP_{j,t} + \beta_2 \, MF_{j,t} + s_i + \varepsilon_{i,k,t} \quad (1)$$

where $Y_{i,k,j,t}$ is the standardized test scores for reading, math, and summed total scores, $MP_{j,t}$ is a series of dummy variables denoting instances of a bond's marginal passage, $MF_{j,t}$ is a series of dummy variables denoting instances of a bond's marginal failure, $s_{(i)}$ is a vector of student level fixed effect dummies, and $\varepsilon_{i,k,t}$ is a residual term clustered at the school campus level and assumed to be normally distributed.

In addition to limiting the evaluated districts to just those with a marginal passage or a marginal failure, $MP_{j,t}$ and $MF_{j,t}$ include lagged dummy variables, going back eight years. It is important to keep track of previous bond results, and to include lagged votes in the regression for a number of years after the time of the vote, since it is likely that the benefits of passing a bond to construct a new school or purchase land to build a new school will not be experienced by students until years after the bond passage occurs. It is also possible that a failure to pass a bond will cause compounding problems over time, as a district that needed to hold a bond vote will continue to face the same (potentially worsening) problems that inspired the initial vote.

The main advantage of using regression discontinuity to examine student test scores is that it makes a strong case for the identification of any causal effects of band passage, lending confidence to whatever set of findings is eventually produced by the analysis. The weakness is that the independent variable of interest for most of the previous literature – the addition of a new school within the district of attendance – is measured imprecisely. Put another way, we argue to have precise identification of the effects of an interesting proxy for the construction of a new school.

Our second set of models essentially reverses these advantages/disadvantages, making them a natural complement to the RD approach. That is to say, they focus directly on the effect of new school openings on student test results, but at the cost of a weaker identification strategy. The ideal approach to constructing complementary regressions would be to link the activation of a campus with the passage of the bond parenting the facility, and to then use the same regression discontinuity technique outlined above to limit the sample of included student observations. This would offer strong identification and precision on the effect of new schools. Unfortunately, our data sources do not offer a direct connection between bond passage and campus activation that would allow the implementation of such a strategy. Instead, we follow an approach that relies heavily on the inclusion of student level fixed effects and the dispersion of campus activations over time to form a viable control group. The specific complementary models are estimated as follows:

$$(2) \qquad Y_{i,k,j,t} = \beta_1 A_{k,t} + \beta_2 X_{j,t} + s_i + \varepsilon_{i,k,t}$$

$Y_{i,k,j,t}$ is the standardized test scores for reading, math, and summed total scores, $A_{k,t}$ is a series of dummy variables denoting instances of a new school activation as well as lagged values from previous activations, $X_{j,t}$ contains control variables for district size and the amount of recent activity the district has experienced regarding school activation, s_i is the student level fixed effects, and $\varepsilon_{i,k,t}$ is a residual term clustered at the school district level and assumed to be normally distributed.

These regressions are performed using dummy variables generated from the Texas Education Agency directory of campuses regarding the year that a campus became active as the main independent variables of interest and the standardized test scores of the student as the dependent variable. Because the regression uses student level fixed effects, our ability to control for changes in campuses is quite limited. However, any unobserved differences across campuses or districts that students experience uniformly over time are effectively controlled for by this approach. The analysis uses the year a school becomes active, one year after activation, two years after activation, and three years after activation as independent dummy variables. All other observations (i.e., cases where the newest campus is at least 4 years old) are grouped together by a single independent dummy variable. Early explorations of the models suggested this cutoff was an intuitive choice. The observations taking place in the years before activation serve as the omitted reference group.

In addition, the percentage of the population of schools in the district activated in the previous three years is also included in the regression. The expected sign of this variable is ambiguous. It is possible that districts experiencing a larger percent of activated campuses are benefitting from an increased focus on education, which may result in better test scores for students. Conversely, it is possible that a large number of newly activated campuses signal a greater amount of disruption in the district that causes an unstable learning environment for students and reduces test scores.

Fortunately for our study, the considerable size of the data set presents a unique opportunity to stratify the data, in order to focus on specific portions of the student achievement distribution, to determine if any asymmetric effects of campus activations are present. Specifically, we estimate all our baseline/full models using only the top (bottom) twenty percent of achievers. It is possible that high and low performing students experience differential effects associated with adding new facilities, given that students of different levels of achievement may respond differently to the changing composition and dynamics in existing classrooms. In particular, the nonrandom patterns of student (and potentially teacher) mobility following passage should modify classroom composition. Of particular note is the fact that students who moved from an existing campus into a newly active campus have test scores that, on average, are significantly higher than students who do not move into newly active campuses (i.e., Table 2.1). This creates the possibility for the effect of a new school opening to have a differential effect on students through peer effects, which we reviewed in Section II and will further discuss after we show our results.

5. Results

Examining our main RD estimation results presented in Table 2.3, we find the quasi-random passage of a bond to have a delayed, but significant positive impact on student achievement. At the year of the vote, students perform worse than average (where average is defined as a typical score in the years leading up to the bond votes taking place) for both the treatment and control groups. Interestingly though, the negative effect on students in the treatment group is much smaller and only marginally significant. While we have no reason to believe a bond vote (of either outcome) would immediately change the physical environment in a district, it is reasonable to expect the attitudes of teachers, administrators, parents, and students could rapidly change in ways that are impacted by the outcome of the bond.

Relatively few benefits seem to accrue in the first few years following bond passage. With the exception of a few marginally significant effects that surface in the second and third years out, the effect is generally positive and insignificant over the first four years. This finding complements MSM (2015), which finds capital spending levels (following random bond passage in Texas school districts) increases in the three years following the vote, but not beyond that. We interpret that as suggesting the construction of new schools is most likely to begin and be in progress during the years we find no positive effect, but that facility completions occurring towards the end of this window are likely what triggers the positive effect we register in the years further out. Specifically, the benefits of the passing school bonds become readily apparent in years five through eight. We estimate the mean effect of exposure to the treatment of interest to be slightly less than 1/10th of a standard deviation when focusing on total test scores. So for example, it would move a student previously at the 50th percentile to the 53rd percentile by the seventh year after passage.

The effect of negative bond votes also shows an interesting pattern, as all instances of a negative bond vote have negative impacts on student test results. For example, focusing on math scores, the negative effects are both immediate and retained into the medium run. Interestingly, a negative vote does not produce a greater intensity of effect as time passes (as did the positive votes), but instead an effect that dissipates over time. A possible explanation for this finding is that school districts may turn to other lesser preferred solutions to the problems they face. For example, a district that does not pass a bond may respond by adding temporary portable capacity or by re-proposing a smaller or more tailored bond carrying a higher likelihood of success. The fact that the yes and no votes display the opposite patterns when it comes to comparing short run versus medium run effects, but that both of those results make intuitive sense given the setting, provides some reassurance that the RD model is registering a true effect. It is also reassuring that the patterns we observe hold for reading, math, and total scores. Perhaps due to our choice to focus on the students who remain attending the same school, we find a different result than most papers in the literature – that the effect on math scores seems larger than the effect on reading scores. Most studies obtaining significant results of school facilities on student achievement find the reading scores effect to be the larger of the two (e.g., Nielson and Zimmerman, 2014).

Table 2.4 presents the results of equation (2), the more direct investigation of the effect of a new school opening on students attending existing schools. Consistent with the RD results, the effect of opening a new school is decidedly positive for all three measures of achievement. Again, the point estimates of the effect are slightly larger for math scores than for reading, but both are highly significant. The weakest benefit of opening seems to come in the first year (i.e., the year of activation). The effect strengthens as the activation moves further in the past. The weak initial effect makes sense if the activation itself carries any turmoil/disruption effects (i.e., even for students who are retained at the same schools, since the conditions in those schools are different) that are short lived. It should also be noted that large school districts have lower tests scores on average, from the negative coefficient on number of school in the district. While we prefer the models including this variable to control for the general differences across large (urban/suburban) and small (largely rural) attendance school districts in Texas, the results on our key activation variables of interest are not at all sensitive to their inclusion.

Tables 2.5 and 2.6 revisit the estimations focusing on the top and bottom portions of the pre-existing achievement distribution, exploring the possibility of asymmetric effects among these two groups. We consider one of the main contributions of our work to be the strong result that asymmetries matter – with the lowest quintile of students retaining the sign of the mean effect (with a now larger magnitude) but the highest performing quintile seeing a full reversal of the effect to a negative impact on all three measures of academic achievement.

As discussed earlier, we know the test scores of students that move to a newly activated campus are higher than the scores of students who do not move. Therefore, upon activation of a new campus the number of high scoring students in an average existing classroom will fall and the number of lower performing students will rise. Using pre-activation test scores to form the distribution of interest, we created achievement level quintiles, and estimated model (2) using the highest and lowest quintiles. Those results are found in Tables 2.5 and 2.6, respectively. Table 2.5 considers high performing students – focusing directly on the group of students losing similar-achievement peers at the highest rates. We find that each period following the initial activation carries a significant negative effect on student test scores. In this case, the impact on reading and math scores registers as similar in magnitude. Besides reversing the direction of the effect to negative, these effects are relatively large (about double) when compared to the estimated mean effects presented in Table 2.4.

Table 2.6 show the effect of a new activation on low performing students. We estimate the effect on the lowest quintile of students to be the same direction as the mean effect (positive), but it now registers as much larger in magnitude (typically by a factor of three to ten times as large, depending on the coefficient of interest). Of course, the critical question is what explains these divergent impacts. Unfortunately, while the data allow the asymmetry to surface, they do not facilitate comprehensive testing of the specific channels driving this difference. However, we can still evaluate these effects in the context of the existing literature to provide at least some plausible explanations of what is happening. Specifically, we will examine our results in light of the best evidence coming from the literatures on the effects of class size and the effects of student peer effects.

If the asymmetry is viewed as being driven by changes in the average classroom size, our estimates are difficult to align with the most convincing studies in the literature. The simplest starting point would be to assume that both high and low performing students who stay put at their current schools following new educational facilities opening experience roughly similar changes in the average class size. While we have no direct evidence of this (because our data does not place students into classrooms, just into schools), we argue this is a reasonable assumption. In this case, since Table 2.4 suggests the highest performing students show a considerable negative effect of school activation, we would have to then believe that reductions in class size carried a negative effect on the performance of the top students – a narrative that runs counter to convincing evidence from studies like McKee, Rivkin, and Sims (2010), Ding and Lehrer (2005, 2011), and others that find the best students experience larger benefits from reductions in class size than do average and lower performing students. Therefore, while it is likely that class sizes are shrinking, and that those reductions carry a positive asymmetric effect on the highest and lowest performers, we are not able to explain our findings using this explanation. In fact, the asymmetric effects of class size reductions work against the pattern of our findings. We argue that this means the correct explanation must come from a different channel.

Conversely, when interpreted through a lens that focuses on the role of peer effects on student achievement, the combined results of Table 2.5 and 2.6 lend support to the Boutique model of peer effects that is largely attributed to Hoxby and Weingarth

(2006), who argue that students of all ability levels perform better when they are surrounded by students near their own ability level. Here, the removal of higher achievement students is expected to generate a negative effect on high scoring students (who lose similar ability peers), but a positive effect on low scoring students (who gain similar ability peers). Put another way, when a new campus is activated, the existing schools experience more homogeneous classrooms (as the higher performing students are more likely to leave), and the more homogenous campus would benefit the lowest scoring students but harm the highest scoring students. On net, since the effect on the highest achievement quintile run counter to the class size story but falls directly in line with the Boutique model of peer effects, we see our findings as primarily being driven by peer effects when it comes to the highest achievement group. Of course, the two literatures 'stack' in a sense when it comes to explaining the lowest achievement quintile since smaller class sizes and larger fractions of classmates at similar ability levels both would tend to increase performance – perhaps explaining in part the reason the gain becomes much larger in magnitude as we move further down the achievement distribution.

6. Conclusion

Exploiting a public voting process that creates quasi-experimental variation in facility investments, we investigate the effects of new schools on the academic performance of the large group of students who 'stay put' once they begin attending a particular elementary school. Across empirical methodologies displaying complementary advantages and disadvantages, we see essentially the same story surface – that additional school facilities increase student achievement. On average, opening a new school in the district was found to help the students that did not attend it, an important result to be aware of for policy makers who must convince local resident voters that raising property taxes to fund infrastructure expansions is desirable. So for example, this result could be helpful in combating reasonable concerns from parents in cases where they expected no benefits would come to their child from passing a bond (i.e., if based on the location of their home they know their children will not attend the new school). We estimate the treatment effect of quasi-random bond passage to be just less than 1/10th of one standard deviation in achievement.

However, we also find that not all students are winners, as the expansion of school facilities seems to carry asymmetric effects on the students who stay put in ways that correlate to their ability levels. Students scoring in the bottom quintile of the distribution experience very large positive effects, compared to students at the top of the distribution who actually experience negative effects. We stress that this effect is limited to the group of students who stay put – an important caveat given that we demonstrate the best students are most likely to attend the newly constructed schools. From the standpoint of public campaigns to pass education bonds, we argue that in addition to groups of voters who have little interest in investing in the human capital of students in the school system, one group possessing a reasonable set of concerns against opening new schools could be parents of high achieving students that live in the parts of the district that will 'stay put', while strong students from other areas (which evidence suggests tend to be newer and wealthier) leave to attend the new campus. Collectively, our findings are best explained by the boutique model of peer effects advanced by Hoxby and Weingarth (2006). Their work, as well as our findings, suggests that creaming off high performing students may simultaneously help the lowest performing students while harming the highest performing students. Further study of peer effects as a channel for the effects of new school openings (or simply as an avenue for additional work in general) is merited. Do teachers systematically modify curriculum in response to changing student pools in the classroom? Do the dynamics of peer-to-peer interactions change when fewer advanced students are present? Does teacher mobility play a role? How do the short and medium run effects compare to long term impacts, acknowledging that households can move and that new facilities age predictably. These and other questions remain.

Finally, as is common in observational studies, the empirical methods used in this paper are not ironclad. The estimations with arguably the strongest identification of causal effects, our RD models, use a dependent variable that is only correlated with the underlying independent variable of interest. Although we argue ‘bond passage’ is an interesting stand-alone outcome, many studies focus on the actual opening of a new school facility for good reasons. On the other hand, when we more closely link student achievement to observed openings of new schools, we move to a model carrying much stronger assumptions that must be met to ensure unbiased estimates. Fortunately, the panel nature of our data allows all our estimated models to include student level fixed effects – arguably the most important step we take in an environment where we observe student achievement but do not observe student level home/family characteristics. Also, it is both critical and reassuring that both methods provide such similar sets of results.

Chapter 2 Tables and Figures

Figure 2.1: Median age of district - percentage by RD group

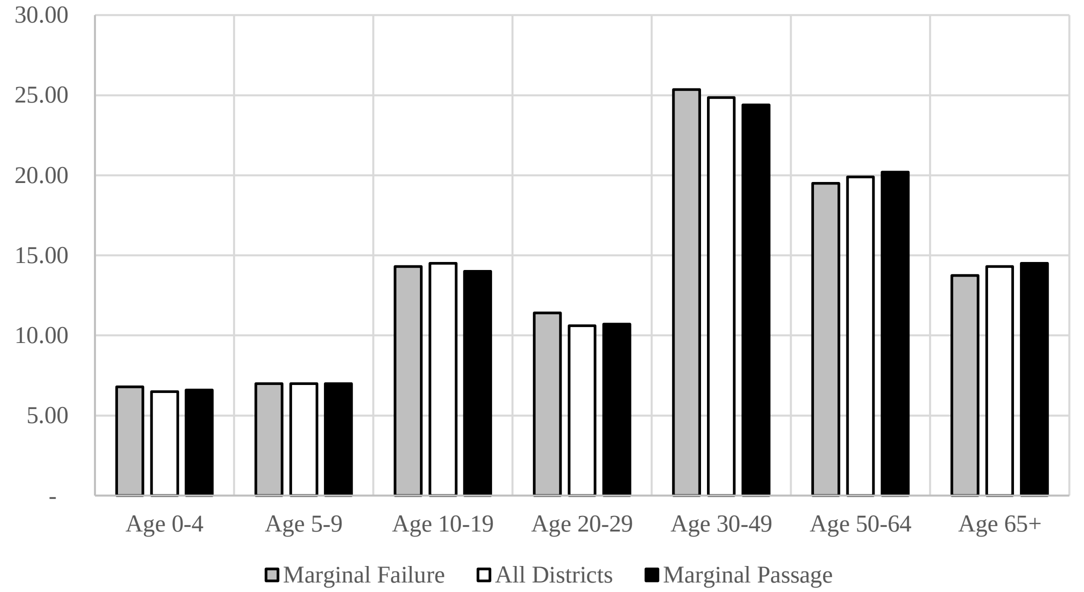

Figure 2.2: Median income of district - percentage by RD group

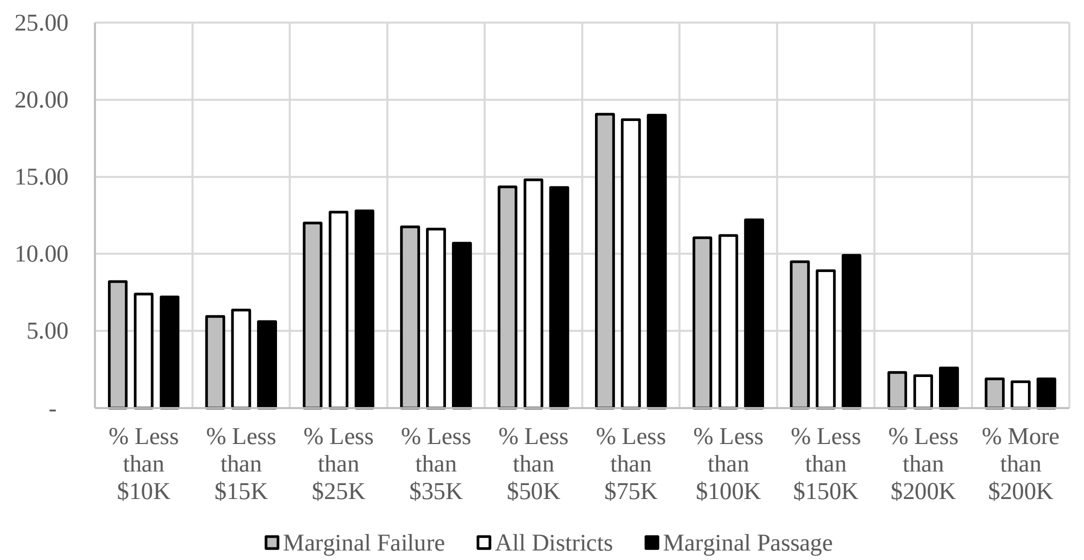

Figure 2.3: Median urban/rural - percentage by RD group

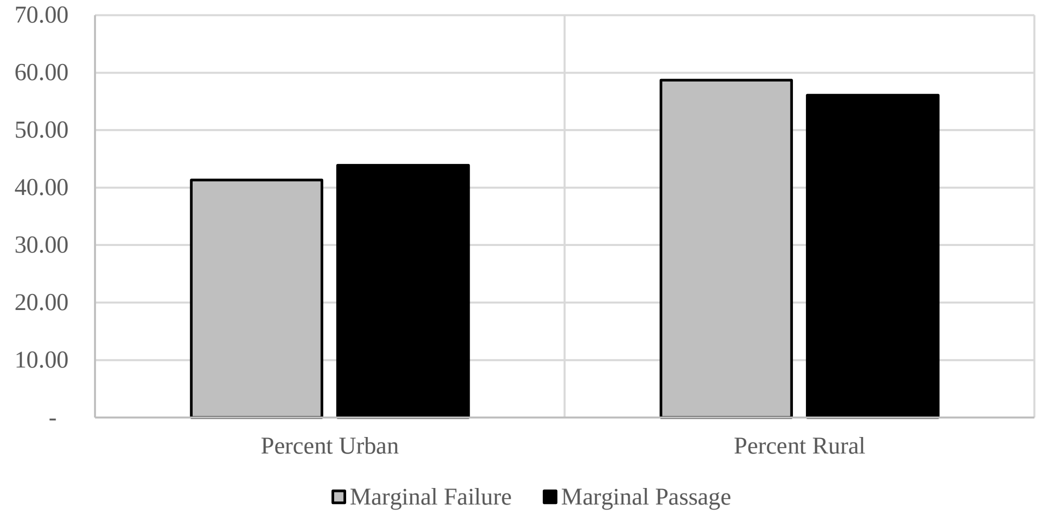

Table 2.1: Bond Results and New School Activations			
VARIABLES	% Total Districts with Activation	% Districts with Yes Vote and Activations	% Districts with No Votes and Activations
3 Year Window	22%	25%	15%
5 Year Window	22%	29%	22%

Table 2.2: Average Student Test Scores by Mobility			
VARIABLES	Reading Score	Math Score	Total Score
Mean of Never Movers	-0.006188	-0.0057096	-0.0063974
	(0.0003798)	0.0003805	0.0003816
Mean of Movers	0.0289878	0.0332633	0.0346787
	(0.0018131)	(0.0018014)	(0.0018185)
H_0: Mean of Movers – Mean of Never Movers = 0	-18.7781***	-20.7761***	-21.8245***
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1			

Table 2.3: Model (1) Regression Discontinuity Results – All Staying Students			
VARIABLES	Reading Score	Math Score	Total Score
Yes Vote	-0.018*	-0.030	-0.030**
	(0.011)	(0.0191)	(0.015)
Yes Vote, 1 Year Lag	0.009	0.009	0.005
	(0.012)	(0.020)	(0.016)
Yes Vote, 2 Year Lag	0.023*	0.020	0.019
	(0.012)	(0.021)	(0.017)
Yes Vote, 3 Year Lag	0.035***	0.034	0.032*
	(0.013)	(0.021)	(0.018)
Yes Vote, 4 Year Lag	0.019	0.017	0.014
	(0.013)	(0.020)	(0.017)
Yes Vote, 5 Year Lag	0.032**	0.035*	0.032*
	(0.013)	(0.021)	(0.017)
Yes Vote, 6 Year Lag	0.050***	0.076***	0.063***
	(0.013)	(0.021)	(0.017)
Yes Vote, 7 Year Lag	0.072***	0.089***	0.082***
	(0.013)	(0.022)	(0.018)
Yes Vote, 8 Year Lag	0.072***	0.094***	0.087***
	(0.014)	(0.022)	(0.018)
No Vote	-0.100***	-0.160***	-0.137***
	(0.018)	(0.026)	(0.022)
No Vote, 1 Year Lag	-0.088***	-0.150***	-0.125***
	(0.018)	(0.026)	(0.022)
No Vote, 2 Year Lag	-0.069***	-0.135***	-0.106***
	(0.016)	(0.025)	(0.020)
No Vote, 3 Year Lag	-0.059***	-0.129***	-0.098***
	(0.016)	(0.024)	(0.020)
No Vote, 4 Year Lag	-0.043***	-0.094***	-0.069***
	(0.016)	(0.024)	(0.020)
No Vote, 5 Year Lag	-0.024	-0.065***	-0.044**
	(0.015)	(0.024)	(0.020)
No Vote, 6 Year Lag	-0.023	-0.072***	-0.049**
	(0.015)	(0.023)	(0.020)
No Vote, 7 Year Lag	-0.045***	-0.082***	-0.065***
	(0.017)	(0.024)	(0.021)
No Vote, 8 Year Lag	-0.034*	-0.048*	-0.041*
	(0.017)	(0.026)	(0.022)
Constant	0.073***	0.106***	0.099**
	(0.011)	(0.018)	(0.015)
Observations	2,466,558	2,466,558	2,466,558
R-squared	0.777	0.793	0.823

Robust standard errors in parentheses, Student fixed effects included
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4: Model (2) New School Activations – All Staying Students			
VARIABLES	Reading Score	Math Score	Total Score
Number of Schools in District	-0.000525***	-0.000247**	-0.000427***
	(6.90e-05)	(9.81e-05)	(8.35e-05)
Percent of New Schools Last 3yrs	0.000381**	-0.000632*	-0.000208
	(0.000193)	(0.000323)	(0.000262)
0 Years Since Activation	0.0167***	0.0357***	0.0300***
	(0.00353)	(0.00517)	(0.00432)
1 Years Since Activation	0.0282***	0.0509***	0.0443***
	(0.00379)	(0.00557)	(0.00465)
2 Years Since Activation	0.0342***	0.0586***	0.0519***
	(0.00431)	(0.00652)	(0.00542)
3 Years Since Activation	0.0582***	0.0667***	0.0682***
	(0.00474)	(0.00685)	(0.00573)
4+ Years Since Activation	0.0637***	0.105***	0.0928***
	(0.00654)	(0.00902)	(0.00785)
Constant	-0.00967***	-0.0273***	-0.0203***
	(0.00364)	(0.00505)	(0.00428)

Robust standard errors in parentheses, Student fixed effects included
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Model (2) New School Activations – Highest Quintile Scores			
VARIABLES	Reading Score	Math Score	Total Score
Number of Schools in District	-8.27e-05	-0.000498***	-0.000139
	(9.08e-05)	(0.000114)	(0.000103)
Percent of New Schools Last 3yrs	-0.00131***	-0.000962**	-0.00151***
	(0.000313)	(0.000388)	(0.000372)
0 Years Since Activation	0.0699***	0.0392***	0.0654***
	(0.00616)	(0.00572)	(0.00578)
1 Years Since Activation	-0.244***	-0.0145**	-0.136***
	(0.00635)	(0.00626)	(0.00614)
2 Years Since Activation	-0.289***	-0.0374***	-0.175***
	(0.00786)	(0.00767)	(0.00781)
3 Years Since Activation	-0.319***	-0.0484***	-0.203***
	(0.0104)	(0.00878)	(0.00967)
4+ Years Since Activation	-0.332***	-0.0377***	-0.214***
	(0.0118)	(0.0117)	(0.0117)
Constant	1.193***	0.895***	1.143***
	(0.00458)	(0.00514)	(0.00483)

Robust standard errors in parentheses, Student fixed effects included
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6: Model (2) New School Activations – Lowest Quintile Scores			
VARIABLES	Reading Score	Math Score	Total Score
Number of Schools in	-0.000877***	-0.000938***	-0.000925***
District	(8.92e-05)	(0.000108)	(0.000100)
Percent of New Schools	0.000171	-0.000439	-0.000372
Last 3yrs	(0.000354)	(0.000464)	(0.000410)
0 Years Since Activation	-0.00311	0.0439***	0.0244***
	(0.00582)	(0.00729)	(0.00636)
1 Years Since Activation	0.304***	0.172***	0.256***
	(0.00653)	(0.00818)	(0.00721)
2 Years Since Activation	0.373***	0.216***	0.318***
	(0.00829)	(0.00967)	(0.00891)
3 Years Since Activation	0.403***	0.224***	0.335***
	(0.00827)	(0.0105)	(0.00919)
4+ Years Since Activation	0.411***	0.290***	0.379***
	(0.0101)	(0.0126)	(0.0114)
Constant	-1.280***	-0.964***	-1.218***
	(0.00552)	(0.00666)	(0.00608)

Robust standard errors in parentheses, Student fixed effects included
*** p<0.01, ** p<0.05, * p<0.1

Chapter 3: The Effect of School Construction on Teacher Mobility and Experience Distribution

1. Introduction

When a school district opens a new school, a substantial investment is made into improving education facilities for the children within that district. However, when researchers investigate the effect of opening a new school, the results do not always demonstrate a positive benefit on student achievement. It is possible that conflicting results in the literature are due to differences between the construction projects under study. For instance, the scale of the construction project, the effect on student demographics in new and existing schools, or staffing decisions made as districts reshuffle resources. This paper provides a novel exploration of the effect of opening a new campus on teacher mobility. We use hazard modeling and logistic regressions to examine the mobility patterns of teachers in Texas, in order to better understand this potential channel through which the opening of a new school could have an effect on student achievement. Attention is given to the question of whether experienced and inexperienced teachers are differentially affected.

When a new school is built, the district must find staff to fill the many position within the school, the most important being the teachers. An important task for any policy maker is how to staff a new school with quality teachers and retain those quality teachers, while still maintaining educational quality at existing campuses (Ingersoll, 2001). Depending on the scale of the construction project, the number of teachers needed may be substantial, which can put a strain on the pool of quality teachers available. A recent example of this comes from California, where a comprehensive push for smaller class sizes increased the demand for teachers and strained the system, causing a decline in teacher quality (Imazeki, 2003; Jepsen and Rivkin, 2009). While the outcome in California was driven by an increase in demand at the state level, the local supply of teachers may similarly be strained, depending on the size of communities and the number of recently activated schools. This is especially true in rural communities where it is difficult to recruit and retain teachers (Rosenkoetter, Irwin, and Saceda, 2004).

Teachers are an important contributor to student academic achievement. In fact, teacher quality has been shown to be the single largest contributing factor for student achievement gains (Sanders, Wright, and Horn, 1997). Standard estimates suggest that a one standard deviation increase in teacher quality results in an increase in test scores of .1 standard deviations (Rockoff, 2004; Aaronson, Barrow, and Sander, 2007). Student achievement gains from quality teachers are also cumulative (Sanders and Rivers, 1996), which makes each subsequent exposure to high quality teachers a further benefit to students. Therefore, while this study focuses directly on measuring the effect on teacher mobility of opening a new campus, it also speaks to how opening a new campus affects teacher quality, and in turn, how opening a new campus may affect student performance. Following recent work by Cellini, Ferreria, and Rothstein (2010), Martorell, Strange, and McFarlin (2015), and the research presented in Chapter 2 of this dissertation, we exploit quasi-experimental variation in teacher's exposure to new school facilities generated by narrowly passed and failed bond votes. Our analysis suggests teacher mobility does respond to the construction of new facilities and that these responses are subject to intuitively reasonable differentiation regarding a teacher's level of experience.

This rest of this paper will provide a brief review of the literature regarding teacher mobility and the effects of teaching experience on student achievement, a discussion of the data, a presentation of the methodology used, and an exposition of the results of the study.

2. Literature Review

When examining teacher mobility, the initial question to be answered is how a teacher makes the decision to transfer between schools. When examining the reasons why a teacher transfers schools, teachers report that the reason for moving is due to personal factors only 26% of the time (Keigher, 2010). This means the majority of the motivation is related to job specific factors or other non-personal factors. For instance, research has shown that teachers are more likely to change campuses when facing poor facilities and student ability (Feng, 2009). Teachers have also been shown to transfer away from schools with high minority populations (Scafidi, Sjoquist, and Stinebrickner, 2007). Forced transfers are commonly the result of enrollment changes and teaching seniority interacting with each other, rather than purely reflecting teacher preferences (Murnane, 1981). The practice of forcing a transfer of a teacher due to a reduction in school population or the cessation of a previously funded program is called "excessing". In most cases a principal or super-intendant is prohibited from hiring outside faculty until the excessed teacher is placed. Recently, a growing number of school districts have adopted a system of mutual consent between teachers and principals regarding teacher transfers, although such systems are not common (Sawchuck, 2010).

Because seniority is considered when making decisions regarding which faculty are excessed, teachers with more experience are less likely to find themselves without a position and forced to change campuses (Feng and Sass, 2012). However, in the case of a new school opening, the lure of a new facility may provide an incentive for more senior faculty to wish to transfer positions. According to a recent survey of school facilities (Taylor et al., 2005) the average (mean) age of permanent education structures in Texas was 24 years old, while roughly 50% of permanent structures were at least 27 years old. Given that the life expectancy of a public school building has been estimated as 50 years (Bureau of Economic Analysis, 2003), at least half of teachers in Texas are working in facilities that are on the decline or are outdated for modern teaching. It is easy to imagine that teachers, when given an option of working in a new facility, may want to transfer away from aging infrastructure. A study of school facilities and teachers in Washington D.C. found that low quality school facilities was a significant predictor in the likelihood of a teacher moving campuses (Buckley, Schneider, and Shang, 2005). That result is also supported by a recent study of teachers in North Carolina, which found that the condition of school facilities were as predictive as school demographics in determining the rate of teachers intending to move campuses (Ladd, 2011). This is unsurprising as school facilities have a comprehensive effect on all aspects of a teacher's ability to provide quality instruction and enjoy their work. For instance, poor air quality has been shown to have an adverse effect on teacher health, resulting in extra sick leave and lost teaching days (Schneider, 2003). Poor facilities also contribute to more student absences and further lost instruction time (Brahnam, 2004). These challenges can also cause more stress over meeting instruction goals (Chaplain, 2006) as well as reported stress from poor facilities as the root cause (Borg, Riding, and Falzon, 1991). While the literature contains these many studies that speak indirectly to the question of how opening new school facilities influences the mobility patterns of teachers, to our knowledge this study is the first to directly investigate the issue using micro level (teacher level) panel data. As such, we believe the contribution to the literature is one of interest to both scholars and practitioners of education finance.

While it is plausible that opening a new campus could entice teachers with seniority to transfer at a higher rate than normal, there is a separate question of whether such increased mobility would produce a benefit or a harm to student achievement. Therefore, the second related literature to our study asks, "does teacher experience translate into higher achievement gains?" The determination of such a question is difficult as the question of teacher experience is complicated by teacher attrition (Murnane and Phillips, 1981). Since there is high turnover in teacher populations, examining mean effects for teachers as a cross section potentially confuses the effects, as the earlier vintages of teachers include those who will eventually attrit from the population. If these lost teachers have lower than average quality, then the cross sectional correlation will produce a positive change in teacher performance do to experience that is a relic of the attrition, rather than a real effect of experience.

When panel data is employed the results show that, while teacher experience is positively correlated with achievement gains, the benefits of experience are strongest for the first five years of experience after which the benefits plateau (Rice, 2010;

Croninger et al, 2007). On the other hand, studies have shown that if teachers focus on continued training and collaboration, the five year plateau can be avoided, and additional experience can continue to provide enhanced teacher quality (Darling-Hammond, 2000; Harris and Sass, 2011). The overall effect of experience on student achievement is further confounded by the fact that greater teaching experience is correlated with greater sense of efficacy from teachers (Wolters and Daugherty, 2007), which is also correlated with increased student achievement gains (Aaronson, Greene, and Loewen, 1988). Due mainly to the importance of the five year plateau, the gains (losses) in teacher quality resulting from new school openings and associated teacher mobility are largely to be determined by whether or not school openings influence all teachers uniformly or if interesting asymmetric effects surface. We contribute to the literature by finding support for the latter story.

3. Data

To determine the relationship between a school district activating a new campus and teacher mobility, our study employs three primary data sources from the state of Texas: teacher level employment and experience data, school district level bond results, and school level campus activation data. The teacher data was provided by the Texas Education Agency. It contains the campus where each teacher worked, as well as their years of teaching experience in the state of Texas and if they hold a teaching certificate. Our data reporting starts in the 2003-2004 school year and is continuous through 2013-2014. Beyond reporting the school of employment, the data contains a unique ID variable, which allow us to track each teacher's school of employment over multiple years - an important detail - as it facilitates our use of teacher level fixed effects in certain models.

Our initial bond data contains the entire population of results for Texas school district level bonds voted on between 1995 through 2015. We limit our sample to the 2,197 ballot initiatives proposing to generate facility construction and/or land acquisitions. As bonds can be used to fund other items (e.g., transportation needs, general maintenance, and athletic facilities), this choice increases the likelihood that the passage of the bonds we consider will result in the building of a new school facility. Unfortunately, the description of the bond's purpose is generalized to the point that it does not allow for precise determination of which bonds are requesting money to build new campuses (i.e., instead of significantly renovating/expanding existing campus facilities). This means that our later analysis of the effects of quasi-random bond passage on teacher mobility (our treatment of interest) cannot be invariably linked to the building of a new campus. However, with the bond question limited to the school building and land acquisition classification, we expect bond passage to be highly correlated with new campus construction, and to corroborate results from examining the effect on teacher mobility from campus activations. Table 2.1 provides evidence supporting this expectation. School districts experiencing passage of a bond (of any type, not just focusing on narrow passages) are significantly more likely to experience a new school opening in both the 3 year and 5 year windows following passage than districts experiencing no outcomes as well as the full sample at large.

Out of the 1,018 independent, combined, and municipal school districts operating in the state of Texas, 99 districts had a bond measure marginally fail during our sample, while 103 districts had a bond measure marginally pass. The demographic data reported in Figures 2.1, 2.2, and 2.3, which is reported for districts experiencing marginal bond failure, marginal bond passage, and all school districts (for reference), provides evidence that the treatment/control groups are valid. In particular, we highlight the minimal difference between the marginal pass and marginal fail districts in the young and old categories. For a number of reasons, these are the two age groups that are expected to have the strongest effect on bond passage (e.g., more school age children puts pressure on the system and, most likely, means more voters in the district are parents with children in the school system).

Similar evidence that our regression discontinuity assumption holds comes from Figure 2.2, which reports the median district's percent of the population within different income brackets. This data comes from the American Communities Survey (the 2010 five year estimate) and is also reported at the school district level. There is little difference between the marginal passage and the marginal failure districts, and the same pattern is found between districts that experience passage or failure compared to all districts. Finally, Figure 2.3 reports the median district's percentage of the population split among urban and rural residents. Here, the median value of the marginal passage and failure districts are very similar to one another, however the overall median Texas school district is much more rural than the other groups. Therefore, the interpretation of our results employing a regression discontinuity should be mindful that the districts with instances of marginal passage or failure are somewhat less likely to be rural districts. While we do not interpret this as evidence that our RD assumptions are violated, it does suggest a minor divergence from the full sample, which contains districts that less frequently hold votes, as well as cases where voting outcomes are not close. Of course, the RD technique applied to bond votes has commonly been used in the literature (Imbens and Lemieux, 2008; Lee and Lemieux, 2009; Caughey and Sekhon, 2011), and the same critique could be made of those applications as well. On net, we are comfortable with the evidence we see that the RD strategy is valid in our application.

The final component of our data is the Texas directory of schools, a data set that is maintained by the Texas Education Agency. These files document the most recent date when a campus became categorized as an active school. The campus can then be merged with the teacher level data that also reports school campus. The directory file contains a straightforward recording of all the relevant information we need about the school. However, it is important to note that there are circumstances where the 'activation' date recorded is not the same as the date that a school first opens. In cases where a previously operating campus experiences a temporary change in status (e.g., closes for renovation), the data entry for the facility will reflect the date of re-opening, not the original opening date. Of course, it is possible that a campus renovation and subsequent re-opening has a different effect on our main variables of interest than would a campus that is newly opened. Unfortunately, the data provide no help distinguishing the two cases. As such, our analysis treats these distinct cases as having the same impacts.

We use the campus activation information for the current and the previous year to create a percent of total schools activated in each period. The percentage variable reflects the scale of the campus activations in a school district, as a single school opening in a district of 100 existing schools is a small change, compared to a new school opening in a district with only 6 schools. We use the current and previous year, since the effect on the needed teachers for a newly activated school is immediate, but the campus activation may take place at the end of the previous school year instead of the beginning of the current school year. In contrast, the teaching record will be for the campus where the teacher worked at the start of the year. By using the current and previous year, the immediate effect of a campus activation is weakened in exchange for a higher level of confidence that we are able to correctly capture all school activations. We argue it is preferable to include activations from the previous year and under estimate an effect than it is to systematically omit activations from the data.

Additionally, as the relationship that we wish to explore is how campus activations affect teacher mobility, school districts with three or fewer campuses in 2003 are not included in the sample as the effect of an activation in such a district would not reflect the changes in the majority of school districts or the preferences of the majority of teachers. It is also the case that teacher turnover is higher in small rural districts (Monk, 2007), which could mean including school districts with three of fewer campuses and would skew our results. In total, the number of school districts used in this study number 405, accounting for 401,241 teachers, and retaining all close bond votes.

4. Methodology

The dynamics of teacher mobility can be viewed as a survival model, where a teacher changing campuses is seen as an event transition (failure) point. This makes a Cox hazard model a natural methodology for examining teacher mobility. The benefit of using a Cox hazard model is that it employs a conditional probability for each time period that a change of campus could occur, rather than viewing each opportunity for mobility as an isolated incident. The complication in using such an approach is the fact that a teacher can, and in rare cases from out data actually does, change campuses multiple times during the panel. One approach would be to only include the first instance of a teacher changing campus. However, that would require us to ignore instances of teacher mobility that offer valuable information for regression analysis. Therefore, a preferable methodology that ensures that all available instances of teacher mobility are used is a survival model that allows for multiple failures, which can be found in Andersen and Gill (1982).

Some multiple failure models count time for all failures from a singular point of time. Although such methods are more in line with instances where failure states can occur concurrently (i.e. the formation of tumors). For the data reflecting teacher mobility, concurrent formation of failure states does not make sense. As such, the time to a failure state is counted either from the beginning of the available data, or from the time since the last move. A limitation of the data used in this paper is that the start of the observation of teacher mobility is not aligned with a treatment event, so that all teachers are treated as having received the same treatment before the data is collected. Such a limitation is clearly sub-optimal, but the fact that all teachers are treated the same whether they recently moved campuses or not is mitigated in a Cox hazard model, since the driving force behind the estimation is how the likelihood function differs between observations with different independent variables. The baseline hazard model is simple, with only two independent variables as seen in equation (1).

(1) $$Y_{i,j,t} = \beta_0 + \beta_1 TS_{j,t} + \beta_2 NS_{j,t} + \varepsilon_{i,t}$$

The dependent variable, Yi,j,t, shows whether or not a teacher changed campuses between observations, TSj,t, is the total number of schools in school district j, and NSj,t represents the percent of the schools that have been activated in the current or previous year in school district j. The baseline model is also repeated eight additional times, with each representing an individual slice of the teacher population based on the level of experience the teacher had at the time of the move.

Another issue with using the Cox hazard model is that the data must be formatted for multiple failure cases with resetting time frames (i.e., in cases of multiple moves), which can make employing the percent of the total schools that recently activated difficult, as the volatile variable may be ignored during times when a failure does not take place. In order to mitigate this problem, we also include separate regressions using a logistic approach with teacher fixed effects. The logistic regression trades the conditional aspect of a hazard model for the ability to more frequently update the percent new school variable. As discussed below, both methodologies eventually support the conclusion that recent activations increase the likelihood of teachers changing campuses, with a larger increase in the likelihood for more experienced teachers. The logistic model is still conceptually very similar to the hazard model in equation (1). The difference in equation (2) is the inclusion of teacher level fixed effects xi, which account for unobservable teacher specific characteristics.

(2) $$Y_{i,j,t} = \beta_0 + \beta_1 TS_{j,t} + \beta_2 NS_{j,t} + x_i + \varepsilon_{i,t}$$

It should also be noted that the data for the hazard models only provides a record when a move is made or at the end of the data set if no moves are made, while the data used in the logistic models is a true panel with an observation for every year that a teacher is active in the data. As such, the number of observations is completely different between the hazard model and the logistic model, with the logistic model using many times more observations.

Although the variable of interest in the previous equations is the percent of the total schools that have been recently activated, there is a possible problem with endogeneity. If a school district builds a new school, that construction began with a decision on whether or not to build based on the ability to acquire funding for the construction. In order to account for the omitted aspect of the data that is not accounted for in equations (1) or (2), we also use the results of education funding bond votes as a quasi-experiment. Using a regression discontinuity design means that the initial decision to construct a new campus is better accounted for in the regression and the endogeneity problem is mitigated. A hazard model and a logistic model are used that mirror equations (1) and (2), but instead include bond results instead of the percent of new schools. For reference, refer to equations (3) and (4) below, which illustrate the hazard model and the logistic model, respectively. In these models, MPj,t represents instances of a bond marginally passing (50 - 53 percent support) in the last three years, NMPj,t represents instances of a bond passing non-marginally (54 percent or greater support) in the last three years, MFj,t represents a marginal failure (47-49 percent support) in the last three years, and NMFj,t represents votes that failed non-marginally (less than 47 percent support) in the last three years.

(3) $Y_{i,j,t} = \beta_0 + \beta_1 TS_{j,t} + \beta_2 MP_{j,t} + \beta_3 NMP_{j,t} + \beta_4 MF_{j,t} + \beta_5 NMF_{j,t} + \varepsilon_{i,t}$

(4) $Y_{i,j,t} = \beta_0 + \beta_1 TS_{j,t} + \beta_2 MP_{j,t} + \beta_3 NMP_{j,t} + \beta_4 MF_{j,t} + \beta_5 NMF_{j,t} + x_i + \varepsilon_{i,t}$

5. Results

In order to make the interpretation of results easier, the hazard ratios that would normally be reported for a Cox hazard model, are instead reported as coefficients. This means that a negative effect on the likelihood of moving will be reported as a negative coefficient while an increased likelihood will result in a positive coefficient. When a Cox hazard model is used to examine the effect of a new school activation on teacher mobility, we find the effect of opening a new campus is to increase mobility. As shown in Table 3.2, when the total number of teacher moves is examined, the effect on the probability of changing campuses is positive and significant. When examining the results from the hazard models, the total effect is informative, but of particular interest is how the coefficient for the percent of the total schools recently activated changes as the model is repeated for specific groups of teaching experience. The coefficient on percent of schools recently activated is much smaller for the most inexperienced group of teachers compared to the total population. For the next two groups, representing teachers with two to three years and four to five years of experience, the effect of school activations grows larger. For teacher with more than five year of experience, the effect of a school activations fluctuates around a plateau that is larger than the most inexperienced group and more in line with the coefficient from the regression on the total population. However, as was discussed previously in the review of the literature, the effect of experience on student achievement increases during the first five years of a teacher's career and then plateaus for the yeas afterward. Therefore, the pattern of the coefficients shows a larger effect for activating a new school on the mobility of the teacher population where increased value of experience has been realized and experience returns begin to level off, 4-5 years of experience. The effect on mobility for teachers with more than five years of experience is much more muted, with a secondary peak in the effect of school activations on mobility occurring for teachers with twelve to thirteen years of experience.

Examining the results from the logistic models in Table 3.2, we find that the peak of the effect of new school activations on teacher mobility is found in the experience group of two to three years of experience. We also find that for experience levels after the peak the effect of school activations fluctuates around the total population effect. While the logistic model does not show a consistent increase in the probability of teachers transferring to a new campus, the model does show that the probability of changing campuses is significantly affected by the activation of new school in all levels of experience. Additionally, the smallest effect on mobility from school activations is for teachers who possess the least experience. The fact that both the logistic and hazard models return the smallest coefficient on school activations for teachers with zero or one year of experience is important for understanding how school openings affect the distribution of experienced teachers. The estimations both show the highest baseline mobility for the most inexperienced teachers, which means that these teachers are the most likely to transfer schools, a result that is expected with how teacher transfers occur. Combining this result with the fact that the constant term for the regressions on more experienced teachers is either significantly smaller or statistically insignificant, means that without the presence of any school activations the least experienced teachers are the clear majority of transferring teachers. However, compared to the likelihood of teachers moving without a school activation, in the presence of school activations experienced teachers are more likely to transfer, which changes the distribution of experienced teachers in new schools as well as the already existing schools.

As discussed previously, it is possible that using recent activations introduces an omitted variable bias because of the endogenous decision by school districts to construct a new school is not accounted for in the model. As a robustness check of the results from Table 3.2, we present a regression discontinuity design in Table 3.3. Examining the results for the hazard model, a comparison of the coefficients throughout the different experience groups for the marginally passed bonds and the coefficients for the marginally failed bonds shows that there is a distinct increase in the probability of a teacher changing campuses when a bond marginally passes compared to when a bond marginally fails. The differences between the coefficient for a marginal passage and the coefficient for a marginal failure are strongest in regressions on the teachers with the lowest levels of experience, but the difference is present (and significant in a joint f test) for all levels of experience. Interpreting these results in conjunction with the results in Table 3.3 gives credibility to the conclusions discussed regarding Table 3.2, as the effect on mobility is present in the case where issues of potential endogeneity are more effectively mitigated.

When similar regressions are performed with a logistic regression, the results are not as conducive for supporting previous conclusions. While some levels of experience show the positive differences between the effects of a marginal bond passages compared to a marginal bond failures, the effect is not present in every grouping, nor is it present in the regression using the total population. A possible reason for this outcome is that more of the differences is being captured in effect of the non-marginal passage and failure results, which shows positive differences between coefficients more often than the effect of marginal passages and failures. Unfortunately, finding the results in non-marginal results over marginal results does not provide the confidence that positive effect on teacher mobility exists regardless of endogeneity. However, as the results were found for hazard and logistic models examining school activations and hazard models employing a regression discontinuity design, we remain confident that there is a causal relationship between school openings and teacher mobility that is asymmetric with respect to teacher experience.

6. Conclusion

Educating the next generation is one of the most important responsibilities the government undertakes. The social and economic benefits are broad and far reaching, which is one reason why the question of how new school construction and school facility improvement affects student achievement is so important. A better understanding of how to properly and efficiently use public funds and manage facilities should provide long lasting benefits. This paper has not attempted to directly answer that question. However, this paper does focus on a related question, which is how exposure to new school facilities influences teacher mobility. The size of the effect of teacher quality on student achievement gains is second to none, and yet there is little understanding of how building new schools affects the location of teachers, and the quality of the teachers that move. Our study has shown that there is a measurable effect on teacher mobility of opening a new school in a school district. Teachers are more likely to change campuses when a new school is activated. This result then means that one of the largest influences on student performance has changed in the wake of the campus activation. Additionally, we have demonstrated that there is a differential effect depending on the experience of teachers, which is correlated over certain experience levels with improved teacher quality. Any school district with a concern for how the construction of a new school will affect student performance should be mindful of how such a school will be staffed, and how losing good teachers from existing schools will affect students who remain at those schools.

While the methods and results presented here are admittedly simple in the face of a complicated system, we believe this study is a valuable initial look at an untapped avenue of study, when determining the effect of new and improved facilities on student outcomes. Future research with more accurate measures of teacher quality, facility quality, and connection to student performance will hopefully provide more detailed expositions that will help inform school districts regarding how to account for teacher mobility when making school construction and staffing decisions.

Chapter 3 Tables and Figures

Table 3.1: Summary Statistics

Hazard Model	Mean	Standard Deviation	Median	Minimum	Maximum
Change Campus	0.54	0.498	1	0	1
Total Schools	54.39	63.487	37	4	269
Percent of Schools Recently Activated	0.038	0.060	0.014	0	0.714
Years of Experience	10.86	9.758	7	0	64
Logistic Model					
Change Campus	0.112	0.315	0	0	1
Total Schools	54.57	62.874	37	4	269
Percent of Schools Recently Activated	0.036	0.056	0.014	0	0.714
Years of Experience	12.65	9.174	10	0	89

Table 3.2: Percent New Schools Hazard Model and Logistic Model

Hazard Model	Total Transfers	Experience 0-1 years	Experience 2-3 years	Experience 4-5 years	Experience 6-7 years	Experience 8-9 years	Experience 10-11 years	Experience 12-13 years	Experience 14+ years
Total Number	-0.000810***	-0.00206***	-0.00124***	-0.00052***	-0.000645***	-0.00071***	-0.00129***	-0.00125***	-0.000626***
of Schools	(3.22e-05)	(9.69e-05)	(8.03e-05)	(8.59e-05)	(9.55e-05)	(0.000111)	(0.000126)	(0.000119)	(6.82e-05)
Percent Recently	0.897***	0.772***	0.913***	1.026***	0.823***	0.893***	0.862***	1.019***	0.889***
Activated	(0.00481)	(0.0305)	(0.0216)	(0.0168)	(0.0132)	(0.0145)	(0.0159)	(0.0161)	(0.00835)
Constant	0.247	1.383***	0.313	0.119	0.187	0.479	0.522	0.0843	-0.334
	(0.154)	(0.500)	(0.267)	(0.289)	(0.448)	(0.707)	(1.000)	(1.000)	(0.577)
Observations	491,007	61,283	70,977	62,783	51,376	38,265	29,763	35,581	140,979
Logistic Model									
Total Number	-0.000491***	-0.000207	-0.000404***	-0.00110***	-0.000934***	-0.00113***	-0.00103***	-0.000725***	-0.000260***
of Schools	(1.45e-05)	(0.000244)	(7.99e-05)	(7.40e-05)	(7.90e-05)	(9.50e-05)	(0.000103)	(7.95e-05)	(3.13e-05)
Percent Recently	0.449***	0.226	0.523***	0.436***	0.497***	0.449***	0.414***	0.415***	0.367***
Activated	(0.00503)	(0.165)	(0.0291)	(0.0274)	(0.0280)	(0.0299)	(0.0311)	(0.0210)	(0.00714)
Constant	0.121***	0.257***	0.150***	0.183***	0.161***	0.163***	0.150***	0.122***	0.0738***
	(0.000852)	(0.0140)	(0.00477)	(0.00447)	(0.00469)	(0.00548)	(0.00581)	(0.00437)	(0.00176)
Observations	2,085,884	68,713	240,101	254,910	227,094	191,105	162,499	203,163	738,299
R-squared	0.274	0.875	0.647	0.637	0.648	0.641	0.644	0.524	0.294

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.3: Regression Discontinuity Hazard Model and Logistic Model (previous 3 years of bond results)

Hazard Model

	Total Transfers	Experience 0-1 years	Experience 2-3 years	Experience 4-5 years	Experience 6-7 years	Experience 8-9 years	Experience 10-11 years	Experience 12-13 years	Experience 14+ years
Total Number	-0.00108***	-0.00219***	-0.00144***	-0.000815***	-0.00103***	-0.00105***	-0.00160***	-0.00144***	-0.000844***
of Schools	(3.27e-05)	(9.77e-05)	(8.11e-05)	(8.68e-05)	(9.68e-05)	(0.000113)	(0.000129)	(0.000121)	(6.94e-05)
Marginal Passage	0.0789***	0.106***	0.104***	0.108***	0.106***	0.0925***	0.0805***	0.0636***	0.0716***
	(0.00188)	(0.0107)	(0.00728)	(0.00569)	(0.00522)	(0.00541)	(0.00607)	(0.00554)	(0.00333)
Non-Marginal Passage	0.0498***	0.0512***	0.0749***	0.0638***	0.0619***	0.0488***	0.0423***	0.0372***	0.0367***
	(0.000803)	(0.00359)	(0.00314)	(0.00246)	(0.00227)	(0.00233)	(0.00251)	(0.00234)	(0.00145)
Marginal Failure	-0.00686***	-0.0254**	-0.0152	-0.00461	-0.00322	-0.00651	-0.00213	0.00215	-0.00447
	(0.00249)	(0.0113)	(0.0108)	(0.00762)	(0.00716)	(0.00750)	(0.00834)	(0.00716)	(0.00422)
Non-Marginal Failure	0.0498***	0.0450***	0.0981***	0.0948***	0.0757***	0.0586***	0.0673***	0.0689***	0.0406***
	(0.00161)	(0.00757)	(0.00625)	(0.00508)	(0.00478)	(0.00494)	(0.00513)	(0.00465)	(0.00280)
Constant	0.240	1.389***	0.364	0.108	0.121	0.447	0.581	0.0586	-0.321
	(0.154)	(0.500)	(0.267)	(0.289)	(0.448)	(0.707)	(1.000)	(1.000)	(0.577)
Observations	491,007	61,283	70,977	62,783	51,376	38,265	29,763	35,581	140,979

Logistic Model

	Total Transfers	Experience 0-1 years	Experience 2-3 years	Experience 4-5 years	Experience 6-7 years	Experience 8-9 years	Experience 10-11 years	Experience 12-13 years	Experience 14+ years
Total Number	-0.000506***	-0.000272	-0.000449***	-0.00114***	-0.000968***	-0.00116***	-0.00103***	-0.000738***	-0.000257***
of Schools	(1.45e-05)	(0.000244)	(8.01e-05)	(7.42e-05)	(7.92e-05)	(9.53e-05)	(0.000103)	(7.97e-05)	(3.13e-05)
Marginal Passage	0.00927***	0.00333	0.0332***	0.0206***	0.0192**	0.0215***	0.00104	0.00798	0.000132
	(0.00124)	(0.0521)	(0.00848)	(0.00759)	(0.00781)	(0.00804)	(0.00868)	(0.00569)	(0.00168)
Non-Marginal Passage	0.0144***	0.0207	0.00551*	0.00893***	0.00781**	0.000909	0.00518	0.00515**	0.00583***
	(0.000503)	(0.0168)	(0.00321)	(0.00297)	(0.00304)	(0.00318)	(0.00329)	(0.00222)	(0.000700)
Marginal Failure	0.0121***	-0.00765	0.000924	0.0263**	0.0286**	-0.00828	0.000106	0.00436	0.0125***
	(0.00173)	(0.0631)	(0.0125)	(0.0108)	(0.0113)	(0.0123)	(0.0125)	(0.00792)	(0.00234)
Non-Marginal Failure	0.00733***	-0.106***	0.00306	0.0102	0.00151	-0.0156**	-0.0148**	0.00562	0.00321**
	(0.00101)	(0.0352)	(0.00700)	(0.00637)	(0.00630)	(0.00679)	(0.00699)	(0.00459)	(0.00136)
Constant	0.131***	0.268***	0.169***	0.196***	0.177***	0.181***	0.164***	0.134***	0.0829***
	(0.000862)	(0.0147)	(0.00487)	(0.00453)	(0.00477)	(0.00556)	(0.00588)	(0.00442)	(0.00178)
Observations	2,085,884	68,713	240,101	254,910	227,094	191,105	162,499	203,163	738,299
R-squared	0.271	0.875	0.646	0.636	0.647	0.640	0.644	0.522	0.291

*** p<0.01, ** p<0.05, * p<0.1

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