

The Effects of School Construction on Student and District Outcomes: Evidence from a State-Funded Program in Ohio

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Abstract

I study an ongoing state-subsidized program of rebuilding and renovating Ohio's K-12 public schools and investigate the effect of improved facility quality on student and school district outcomes. The completion of a project increases public school enrollment and district property values. Test scores do not measurably improve upon completion and suffer significant reductions during construction. The implied willingness to pay for a project is lower than total costs but greater than the cost borne by district residents. While the program led to a narrowing in expenditures across district wealth, I find little evidence that it reduced disparities in student outcomes.

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1 Introduction

Every year, K-12 schools in the US spend upwards of \$50 billion on the operation, maintenance, and construction of their facilities (NCES 2010), approximately a tenth of all K-12 expenditures and an amount equal to over \$1000 per pupil. Despite this cost, a sizable fraction of school districts still suffer from facilities in poor conditions, and some argue that even more funding should be dedicated to bringing their buildings to an appropriate state (Alexander et al., 2014). Yet there is little research into the role that school facility quality plays in the educational process. Given the potential for expenditure increases, it is worth considering the return to investing in school facilities, both for student achievement and for district outcomes such as property values.

Though relatively few studies have been conducted on the relationship between facility quality and student outcomes, education scholars have suggested various ways in which building quality may be an important input in the education production function. Older facilities tend to have poorer air quality, which may lead to health risks that impair learning (Daisey et al., 2003), and inadequate lighting, which may inhibit teachers' instructional ability (Lemasters, 1997). Older buildings are less likely to be equipped to handle newer education technologies (Lyons, 1999). The discomfort of poorer facilities could lead to both increased truancy by students (Earthman, 2002) and reduced enthusiasm by teachers (Uline and Tschanen-Moran 2008). Despite these reasons, and broad public support for investments in infrastructure, it may not be immediately obvious that investments in new schools are beneficial. Construction and renovation projects often require that students learn in suboptimal temporary spaces while waiting for the completion of new buildings. More generally, investment in construction, particularly when mandated by state or federal authorities, may induce schools to reduce funding in other avenues potentially more beneficial to students, such as teacher pay. A recent review looking at various inputs into educational production found no significant evidence of facility quality affecting academic achievement (Hanushek 1997), though this review only used cross-sectional variation in school facility expenditures. Only recently have researchers used causal designs to examine this question.

Of the few recent papers to answer this question, the majority look at bond elections to raise funds for school construction and examine differences between districts just above and below the cutoff for passage of funding (Cellini, Ferreira and Rothstein, 2010; Hong and Zimmer, 2014; Martorell, Stange, and McFarlin, 2015). The main benefit of estimation using bond elections is the plausible exogeneity of variation in capital expenditures around the cutoff of approving funding. Another benefit is that, because all costs are borne locally by property taxes and capitalized into property values, any increase in housing prices in a

district indicates that the willingness to pay of residents is greater than the costs of the program. The disadvantages are the relative scarcity of observations around the cutoff and the difficulty in generalizing the treatment effect of such elections to districts far from the cutoff. In particular, for state and national policymakers the relevant treatment effect is often for poor districts, which tend to have worse school facilities, while districts that approve bonds for capital outlays tend to be wealthier.¹ Another disadvantage of such an approach is that the treatment effect being estimated using bond data is the treatment of passing the bond rather than an actual change in facility quality. Because there is a lag between bond approval and new investment in school construction, which varies across districts, estimates from such studies are likely to be biased towards zero when the true effect comes from the actual occupation of a new or renovated building.² The whole of these studies find positive but very weak evidence of improvements in test scores for treated students. Cellini et al. find positive effects on housing prices, suggesting a willingness to pay \$1.50 for \$1 of capital expenditures and implying that funding for school quality is significantly below the socially optimal level.

Another approach to estimation has been to look at large state-assisted funding projects for new construction. Neilson and Zimmerman (2014) investigate an impoverished school district in New Haven that spent \$1.4 billion ($\sim \$70,000$ per pupil), largely from state and federal funding, on improving facilities and employ a difference-in-difference strategy based on the staggered implementation of renovation/construction to different schools. They find a positive effect on home prices and a large effect on students' reading scores, comparable to attending a high achieving charter school, while finding no significant effect on math scores. Most other educational interventions find effects for math scores but not for reading, which suggests that facility quality may enter the production function distinctly from other inputs.

In this paper, I study an ongoing large scale program in Ohio that began in 1997 and has spent over \$10 billion in constructing and renovating schools throughout the state. This program is heavily state-subsidized, with the magnitude and timing of assistance favoring the poorest districts. I use this program to investigate the effect of improved facility quality on property values in the school district and enrollment and test scores in public schools. In light of previous research in this area, the primary benefits of this current study are three-fold: the construction project is statewide, and so allows me to plausibly estimate the effects of school facility construction for a wide distribution of district types. Secondly, while most studies can only look at the treatment of the passage of a bond (as they do not observe the

¹In California in particular, bond elections for construction are the only way that wealthy districts are able to use local funds to increase school spending (Cellini et al., 2010).

²These papers discuss the difference in estimating an ITT and TOT of bond passage, but only in regards to how a current bond passage affects the likelihood of a future bond passage, not in relation to how it affects the actual construction or renovation of facilities. Martorell et al. (2015) is the only study able to observe a measure of facility quality and show that bond passage leads to improvements in building quality.

status of actual school construction), I can look at both the treatment effect of the approval of funding for a district and the effect of the completion of a building project. This effect might be the more relevant treatment for student outcomes and may in part explain why previous studies have found small effects on test scores.

Thirdly, as this study is looking at a state-subsidized program, I can answer questions about fiscal federalism. A concern in the school finance literature is the incentives created by state finance equalization programs. Hoxby (2001) has shown that some state systems of progressively funding districts on the basis of wealth lead to a “levelling down” of funding to the poorest district. More generally, a large literature has looked at the tradeoff between equity and efficiency in centralized funding of local public goods (Besley and Coate, 2003). While progressive funding allows for reductions in inequality between regions or neighborhoods, it is challenging for the central government to elicit the preferences of the regions, who have no incentive to internalize the state cost of the program borne by other regions. This externality may lead to efficiency losses from costs being greater than the true willingness to pay for funding. We can answer the question of whether state-financing leads to efficiency losses by comparing program costs to the valuation of residents implied by changes in housing prices.

I estimate that the completion of a project leads to an increase in enrollment in public schools of over 5%, both for poor and non-poor districts. I find weak evidence that reading and math test scores increase for elementary school students upon completion, while all students who are in school during construction suffer statistically significant reductions in test scores. This reduction is worse for the 25% of poorest districts and is not ameliorated by a commensurate improvement post-completion. I find a substantial positive effect of the program on housing prices, on the order of 17-20% four years after project completion. For every dollar spent on construction, housing values increase by 75 cents. Total housing value capitalization is greatest in the wealthiest districts, despite roughly similar construction costs across districts.

Several papers looking at grant programs have investigated the so called “flypaper effect,” whereby grant money leads to a greater increase in local government spending than an identical increase in district wealth (Inman, 2008). In the current setting, government funding by law goes completely to construction, though we may expect that total funding for schooling would not increase by as much as the allotted grant through reductions in other inputs. However, I find that school districts respond positively to the program by increasing funding in other education inputs, lowering the pupil-teacher ratio and increasing teaching expenditures and overall per student expenditures. This “crowding-in” is potentially due to the freeing up of annual budgetary allocations to capital expenditures.

I conduct two cost-benefit analyses to assess the efficacy of the program. I first calculate the

implied per-pupil willingness to pay for the program using changes in housing prices. I find that the average district values the program slightly less than the total cost of construction, around \$26,500 per pupil compared to a cost of around \$31,000 (including the deadweight loss of taxation). However, there is significant heterogeneity across districts. Wealthier districts have much greater housing capitalization than poorer districts. For 52% of treated districts, the implied valuation of the program is lower than total cost of construction, though still higher than the district's share of the cost. My second cost-benefit analysis calculates the effect of construction on students' lifetime earnings using the point estimates of changes in test scores. I find that the completion of a construction project leads to a \$7,100 increase in lifetime earnings per pupil. This effect is still higher than local construction costs, though substantially lower than the average total cost and the implied willingness to pay for the program. This gap between the two cost benefit analyses suggests that the program is being primarily valued for its non-educational benefits, such as the aesthetic value of the buildings or their non-educational uses by the community. Furthering this hypothesis, I find that the housing price effect of the program remains even when looking only at homes with one or two bedrooms, which are less likely to have school-aged children.

This paper serves as a cautionary tale for state-subsidized construction programs. While the funding for the program did not lead to a crowding out of other education inputs, it did not go very far in ameliorating educational disparities across poor and wealthy districts. As a redistributive program, it was a success in terms of narrowing the gap in education expenses across districts, but it did not lead to measurable improvements in test scores, attendance, or discipline. Further, the program was illustrative of the potential drawbacks of heavily-subsidized school funding schemes. While districts internalized the local cost of construction projects, many did not internalize the large externality to other districts in the form of a large state subsidy. For the poorest districts, the implied willingness to pay for a project is significantly lower than the total cost.

The rest of the paper proceeds as follows. Part 2 describes Ohio's school rebuilding program and the specifics of its implementation. Part 3 describes the data and provides summary statistics of the program and Ohio public schools districts. Part 4 discusses the empirical strategy. Part 5 looks at the results, Part 6 presents the cost-benefit analyses, and Part 7 interprets the results. Part 8 concludes.

2 Program Description

The creation of Ohio's school construction program came about due to widespread unhappy

piness with the state of education funding in Ohio.³ Like most states, public school funding comes predominately from within districts, and construction projects are traditionally implemented through the issuing of district bonds to be paid back with property taxes. In 1997, the State Supreme Court ruled in the landmark case *De Rolph v State* that such a system of education funding was unconstitutional for allowing too much variation in students' academic opportunities based on place of residence. This ruling led to the passage of the Rebuilding Ohio's Schools bill and the creation of the Ohio School Facilities Commission (OSFC), which has overseen funding and construction for each school district. The program, still ongoing, has disbursed over \$10 billion, while even more has been expended in total once accounting for districts' matching funding, explained below (OSFC, 2013). Of the state's 609 public school districts,⁴ 248 have completed construction, and over 450 have at least begun construction.

Every year, the OSFC publishes the Ohio School Design Manual, which stipulates minimum standards for the quality and design of the buildings. These include requirements for square footage per student, lighting, sanitary conditions, and environmental testing pre-construction, meant to maintain uniform building quality across school districts. Since at least 2008, the OSFC has also required that all buildings comply with LEED certification for green buildings. Project managers are hired centrally by the OSFC and overlook multiple projects across districts. Prior to a project, an assessment is done by the OSFC on the state of the buildings to determine whether renovation or new construction is warranted.

The law specifies explicitly how districts will be funded. Each year the Ohio Department of Education (ODE) calculates the total assessed property value in a district divided by the average daily school attendance. Districts are then ranked on the basis of their 3-year moving-average valuations, and funding is offered down the line from poorest to wealthiest districts. School districts are required to contribute a percentage of the cost equal to its place in the distribution of valuation per pupil (e.g. a district at the 20th percentile must pay 20% of the costs of the construction project). The choice of renovation or new construction is based on an assessment done by the OSFC prior to disbursing of funds. I corresponded with an OSFC employee who reported that about 60-65% of projects are new facilities. If a school district is unable to raise the required funds for the project (usually done through bonds paid with property tax), their project is deferred to the following year, and funding is offered to the next poorest district. Districts are allowed to accelerate the construction process by beginning work using local funding while waiting on state funding, but the funding from the

³<http://www.toledoblade.com/State/2001/06/17/Coalition-legal-fees-3-6M-and-growing-Ohio-taxpayers-foot-bills-for-Columbus-firm-s-work.html>

⁴At some point throughout the period of study, Ohio had as many as 612 districts. This number dropped to 609 as very small districts merged into neighboring districts. This change over time does not affect this study, as these merges occurred in districts too wealthy to have had an OSFC project by this point.

state can only come when all poorer districts have received offers.⁵ While the local funding for construction comes largely from property taxes, the state contribution comes from the state's general revenue fund.⁶

3 Data

I use data from the Ohio School Facilities Commission annual reports to observe the status of construction in each district for the period 1998-2014 (while the program began in 1997, the first school district to complete construction was in 2001). Each annual report specifies whether a district has received funding, begun construction, or completed their project. The data also include the size of the expenditure for each project and in some years how many schools are built and renovated. I only see when construction begins and ends for the entire district, rather than each campus (e.g. K-8 v. 9-12). I can also only see year to year changes in the status of the program, so construction for a district may have begun halfway through a year and the status of the project only changes for the following year's annual report. While this level of detail is still an improvement over some previous studies, my construction variables are noisy in these respects. Using data from the Ohio Department of Education, I include information on test scores, daily attendance, and classroom discipline across districts for the school years 2005-06 to 2013-14. From the Common Core of Data I gather information on district demographics and expenditures for 1997-2011. I use these both as outcomes of interest affected by construction and controls in other regressions. From the Department of Taxation I collect data on property valuations per pupil for 1992-2013. Property valuation is calculated using all property in the district, including commercial. From Corelogic, I received data for housing transactions in all 88 Ohio counties in the period 2000-2015 and property deeds for the entire state. The deeds contain property characteristics, including lot size, building size, number of stories, bedrooms, bath, and building age. They also contain sale date and price for the last two arm's length transactions of the house. I use this information to extend the data back to 1992 to be able to assess the earliest programs (details of this are in the Housing data appendix). I link the houses to school districts using Census School District Boundary Files. For all analysis I restrict attention to single family residences sold in an arm's-length transaction. A more detailed discussion of the data and how they are linked across variables and years is discussed in the Data Appendix.

⁵This description is for the OSFC's main construction program, the Classroom Facilities Assistance Program (CFAP). The commission runs four separate programs, but CFAP is the primary one. In this paper, I restrict attention to CFAP programs, as they are the most uniform in their practice, are the largest in terms of funding per pupil, and utilize the allocation rule described above. For a detailed description of the other programs and when each one is used, see the Program Description Appendix.

⁶Beginning in 2007, a large part of the state's contribution came from annual payments received from the four major tobacco companies, due to a legal settlement from 1998: <http://www.osfc.ohio.gov/LinkClick.aspx?fileticket=M22zeXDD4%3D&tabid=79>; <http://www.dispatch.com/content/stories/local/2014/08/17/aretobaccobonds-really-toxic-debt.html>

The main variables of interest are the program statuses, namely when a district is eligible for funding, has received funding, started construction, and completed construction. The latter three of these are taken from the OSFC Annual Reports. I do not observe in the data when a district is eligible for funding. So, I infer their eligibility based on the explicit assignment rule and the property wealth of a district in each year:

$$\text{YearEligible}_i = \min_i \{t | \text{PVrank}_{it} \leq \max_j \{\text{PVrank}_{jt} | j \text{ funded in } t\}\}$$

where PVrank_{it} is the rank of the district based on their assessed property value per pupil. In other words, a district is eligible for funding the first year where their property value ranking is lower than the highest ranked district to receive funding that year.

3.1 Summary Statistics

Table 1 provides summary statistics for districts based on when they receive funding. Consistent with the allocation rule, the later districts are significantly wealthier, both in terms of property values and percentage of students eligible for free or reduced price lunches. This staggered rollout based on wealth is also shown in Figure 1. The overlap in wealth across years is due both to imperfect takeup of funding and to the fact that districts change rankings over time, due potentially to increased property value from a construction project. Time between funding and project completion is on average 4.6 years, with a range of 3 to 12 years. The average cost per pupil for a project is \$23,740. Though there is significant variation across districts in program cost, which depends on the needs of the district, this variation is not correlated with district wealth. As context for the magnitude of the program, average annual expenditure per pupil in the US was approximately \$12,000 in 2011-12 (Cornman, 2014), only \$1,000 of which is usually spent on capital projects.

The average per pupil contribution of the state is \$17,070 and comprises a larger part of total costs for the poorest districts. This contribution is 19% of the total property valuation for the average treated district. Figure 2 graphs this percent district-by-district, showing that state funding relative to district wealth is substantially higher for the poorest districts. Figure 3 shows how program takeup varies by year of eligibility. For districts eligible in or before 2006, the 3-year takeup rate hovers around 70%. For districts eligible later, the takeup rate drops significantly, down to 30% in 2008 and 2009.

Table 2 shows the size and scope of the program in comparison with other recent studies. Though other settings had greater per pupil funding or more districts, no previous study has, as far as I know, examined a construction project of this magnitude for so many districts.

4 Empirical Strategy

My estimation strategy takes advantage of the unique allocation rule established by the OSFC. I identify the effect of the program by grouping districts who were first eligible for funding in the same year and using variation in takeup of funding and construction within these groups. By only using differences within groups that receive eligibility in the same year, I can control for idiosyncratic variation that occurs for districts of similar wealth. For each outcome Y_{it} , I run the following regression:

$$Y_{it} = \alpha_{0i} + \alpha_{1i}t + \gamma_{e(i)t} + \sum_{p=1}^2 \theta_{-p} D_{it}^{-p} + \theta_{FC} D_{it}^{FC} + \sum_{k=0}^5 \theta_k D_{it}^k + \theta_{5+} D_{it}^{5+} + X_{it}\beta + \epsilon_{it}$$

α_{0i} and $\alpha_{1i}t$ are controls for district specific fixed effects and time trends, respectively. $\gamma_{e(i)t}$ are year fixed effects specific to groups of districts that are first eligible in the same year, $e(i)$. D_{it}^{-p} are the indicators for the years prior to funding, D_{it}^{FC} is for the multi-year period between funding and completion of the project (which includes construction), and D_{it}^k are the indicators for years after completion. The excluded period is 3 or more years prior to funding. I include D_{it}^{5+} as a coefficient for 5 or more years after program completion. This control allows me to include data far away from the program to help identify the district specific time trends and group fixed-effects, but I refrain from reporting $\theta_{>K}$ in the estimation tables because periods that long after project completion are only available for early takeup districts and thus suffers from composition concerns. The pre-trends D_{it}^{-p} allow me to check that the model is specified correctly. If there is no selection into the program and the dates of construction are properly measured, the coefficients for the pre-trends should be insignificant. The controls X_{it} include other school inputs (pupil-teacher ratio, instruction expenditures, student support, etc.).⁷ I use these regression specifications for all outcomes other than for test scores. For housing price analysis, the level of observation is a transaction, and the outcome is the price of the transaction. All treatment variables continue to be at the level of

⁷For the housing regressions, the inputs data does not cover all years of my sample, missing 1992-1996 and 2011-2013. To deal with this issue, I impute the values of inputs for years outside the window of availability using district specific time-trends and predicting the values of these inputs. The results look similar if I impute using a district average for years outside the window.

the district, but X_{it} now include property characteristics. For all regressions, standard errors are clustered at the district level.

The most unusual component of my regression is the group-specific year effects, $\gamma_{e(i)t}$. A more obvious choice would be to simply use sample-wide year fixed effects. My choice is primarily motivated by the fact that the housing market experienced a boom and bust during my period of study, and this rise and fall was heterogeneous across district types. Without accounting for this heterogeneity, my estimates for the effect of the program may pick up some variation from the housing bubble. More generally, one may be worried that variation in outcomes over time are correlated across similar districts. Accordingly, I use these fixed effects for all my outcomes, and I discuss when the results diverge from using sample-wide year fixed effects. In nearly all cases, the choice of year fixed effects makes no quantitative difference in my estimates.

My estimation approach relies on the standard fixed-effects assumption that there is no unobserved time-varying district-specific shock that may induce a change in the timing of construction and that also affects the outcome of interest. Because of the assignment process for state funding, there are essentially two ways that endogeneity may violate my identifying assumption. Firstly, we may be concerned that there is endogeneity in the takeup of the program. Conditional on being eligible, school districts that accept funding (and are able to raise their own funding) do so because they may already expect property values or enrollment to rise in the future, leading us to incorrectly assign this rise to the construction project. I account for expected increases by controlling for district time trends. However, another potential scenario of concern would be that a school district experiences a transitory drop in test scores or property values, responds by approving funding for construction, and test scores or property values revert to the mean while appearing to be the effect of the construction program, biasing our estimates upwards.⁸ While I cannot control for these unobserved time varying shocks, I can check for pre-trends in treatment, which is where we would see idiosyncratic changes that induce selection into the program. One might also be concerned about the endogeneity of eligibility. A district may experience a transitory drop in their property valuation and simultaneously receive eligibility and experience a reversion to the mean in their property valuation, which we incorrectly take to be induced by the receipt of funding. Because eligibility is determined by a 3-year smoothed average, I do not think selection into eligibility is a big concern. Further, the program effects we estimate go as far as four years after project completion, which is on average four years after funding. This length of time should mitigate any mean reversion effect in the estimates due to potential

⁸However, it may be the case that transitory shocks matter in the opposite direction. Districts may be more likely to approve funding after a transitory increase in property values because it becomes more feasible to fund the project. The resulting mean reversion would be captured in our estimates of post funding coefficients, which would then be biased downwards.

selection into eligibility.

Unlike traditional event studies, the program in question has three events, funding, construction, and completion. Further, the length of time a district spends in their funding and construction periods varies significantly. Therefore, I refrain from presenting raw event study graphs with averages of the data within each period and with event dummies prior to funding and after completion, as the total length of time elapsed will vary across districts. Including district and year fixed effects in my regressions resolves these concerns. I also refrain from showing raw event study graphs with all districts centered around completion, as it is difficult to interpret the pre-period values and there is no appropriate excluded period from which to compare the effect of completion.

4.1 Test Scores

For test scores, I use a different treatment approach to account for the fact that students may not have been in the school system for the entire construction or completion period. The treatment I use is a measure of their exposure to the program during and after construction:

$$\begin{aligned} \text{ConstExp}_{igt} &= \max(\min(\text{YearCompleted}_i, \text{Year}_t) - \max(\text{StartDate}_{igt}, \text{YearConstruction}_i) + 1, 0) \\ \text{CompExp}_{igt} &= \max(\min(\text{Grade}_{igt} + 1, \text{Year}_{it} - \text{YearCompleted}_i + 1), 0) \end{aligned}$$

In other words, a student's exposure to construction is the number of years he is in K-12 for which construction is ongoing. Similarly, completion exposure is the number of years for which a student is in a completed school building. For example, a 7th grade student in 2007 attending school in a district that began construction in 1998 and completed in 2004 has a construction exposure of 5 years and a completion exposure of 3 years. I then run a fixed effects regression using within-district within-grade variation in exposure to construction and completion:

$$P_{igt} = \alpha_{0i} + \alpha_{1i}t + \tau_g + \gamma_{e(i)t} + \sum_{k=1}^{6+} \theta_k^{\text{const}} D_{igtk}^{\text{const}} + \sum_{k=1}^{6+} \theta_k^{\text{comp}} D_{itk}^{\text{comp}} + X_{it}\beta + \epsilon_{igt}$$

where D_{igtk}^{const} and D_{itk}^{comp} are indicators for k years of construction and completion exposure, respectively, of grade g in district i in year t . I have indicators for 1 to 5 years and 6+ years of construction and completion. The controls X_{it} include school expenditures and student

demographics (percent black, hispanic, female, reduced-price lunch) to account for changes in composition induced by the program. The outcome P_{igt} is the percent of the students in a grade who received a grade of proficient or higher on their state-wide end-of-year exam. I do the analysis separately for math and reading scores.

4.2 Sample Selection

My main sample consists of all districts that take part in the CFAP program described above, did not have a prior program by the OSFC, and did not start construction prior to receiving funding by the state. Note that this sample does not include the six big urban school districts in Ohio, as they receive funding through a separate program and based on a different allocation rule. Because the construction program is ongoing and very few of my data extend prior to the creation of the program, my data is unbalanced around the treatment of the program. This unbalancedness may lead the event study estimates to be affected by composition changes. For example, if we see that housing prices increase gradually for the years after project completion, we cannot say whether that is due to a true increase or the treatment effects being estimated off of different districts across years, where the districts with more years of post-completion data had a greater treatment effect for all years than the average treated district. To address this concern, I include results for a balanced sample which requires that all districts in the sample have all years of the treatment variables and the excluded period. One important thing to note is that, because districts are staggered by wealth, balancing the sample changes the wealth composition of districts by removing the earliest or latest treated (or both) from the sample.

5 Results

5.1 Enrollment Effects

Figure 4 and Table 3 show the event study effect of a construction project on enrollment. All treatment effects are measured relative to 3 or more years prior to funding. We see in the figure that enrollment levels respond positively to the completion of a project. By 3 years after completion, enrollment has risen by 10.7%. The lack of pre-trends suggests that districts are not strategically choosing when to start funding based on transitory changes in enrollment. Note that this not does not mean that districts do not choose whether to construct without regard to expected changes in enrollment. By controlling for district-specific time trends, I account for this possibility. The positive results I find in the unbalanced sample are less significant and smaller in magnitude in the balanced sample, shown also in Figure 4. I find in the balanced sample that enrollment increases by 5.5% after 4 years of a completed project,

though the effect is not significant at the 5% level. The standard errors are significantly larger because of the reduction in sample size, with the number of districts falling from 278 to 46. The available enrollment data span the years 1997-2011, so the balanced sample only includes districts that receive funding as early as 2000 and completed as late as 2007, which removes the poorest and the wealthiest treated districts. It is possible that the treatment effect for the balanced group is smaller and most of the effect from the unbalanced sample is driven by early or late districts.

Columns 3 and 4 of Table 3 show the results for the poorest 25% of districts in the state. As of 2013, 48% of funded districts were in the bottom 25% of property wealth. The magnitude of the increase is very similar to the full sample, both unbalanced and balanced, suggesting that the enrollment effect is similar for poor and non-poor districts. There is a slight pre-trend for the poorest districts, which we did not find for the average district, though the magnitude is small relative to the ultimate estimated effect of the program. This pre-trend suggests that the poorest districts are more sensitive to idiosyncratic changes in enrollment needs and take up the program accordingly. It could also be that the poorest districts are much better able to predict when they will receive funding. Because their contribution to funding is much lower than the average project, they are more likely to successfully raise funds in their bond election. In the data I find that year of eligibility is more predictive of year of funding for poor districts than the average. For the poorest 25% of districts, 36% receive funding the year they become eligible; this number is only 24% for all other districts in the state.

One immediate question is from where schools are drawing the increase in enrollees. The increase may be due to movers from nearby districts, students from local private schools, or reductions in truancy. Unfortunately, data from the CCD on total number of school-age students in a district, public and private, are unreliable (and often less than the reported number of public school students), so I am unable to say whether this increase is due to students moving to the districts or local students choosing to leave private school. Throughout the period in question, public school enrollment is falling significantly. From 1997 to 2011, enrollment in public schools fell by over 10%, despite the total population of Ohio growing by about 3%. This decline suggests that the increased enrollment we observe is simply a reduction in the outflow of students from public schools rather than students moving into the district.

5.2 Student Outcomes

Figure 5 and Table 4 show the effect of the program on test scores for all districts, pooled across all grades. I find strong evidence of a negative effect during the construction period.

Construction leads to drops of 2.2% of students proficient in math and 1.6% proficient in reading after 4 years of exposure, off a base of 80% proficiency rates. I do not find any significant evidence for positive effects from project completion. Note that because of the size of my standard errors, it is possible there is a small positive effect that I cannot differentiate from no effect. Though not shown, I find positive effects of completion on math scores in a regression without district time trends. There is no evidence of reading scores improving in either specification, and if anything may continue to drop post-completion. When restricting the sample to the poorest 25% of districts, the negative effects of construction worsen, with no compensating improvement post-completion. Surprisingly, average proficiency is no lower in the poorest districts, which may explain why there are no greater effects. I then restrict attention to districts with many poor students, where over 25% are eligible for free or reduced-price lunches. I see a similar reduction in scores during construction, but weak evidence that math scores improve by six years of exposure to the program. Reading scores continue to be unaffected.

In Figures 6 and 7 and Table 5, I break down the test score regressions into elementary school (3rd-6th grade) and middle/high school (7th-12th grade). These results show that the gains from the program primarily appear for younger students, and the losses are borne by older students. Younger students have a 2% increase in proficiency, both in reading and math, due to program completion, and a 2-3% decline in math scores during construction. For older students, I find no evidence of score improvements after completion and strong negative effects during construction, with students suffering 5% declines in proficiency in both subjects.

Note that, by design, the sample is unbalanced in the test score regressions. Third grade students have a maximum exposure of four years to either construction or exposure, for example, so the later treatment effects are identified only on older grades. This fact may explain why I find variation in treatment effects by years of completion or construction exposure. Further, because I only have test score data available for the years 2005-2012, I suffer from a worsened problem of having a restricted window of data relative to the construction project years.

This study is not the first to find unconvincing evidence of school facility effects on test scores. Neilson and Zimmerman is the only previous study to find a strong positive effect of construction on reading scores (similar in magnitude to attending a high-achieving charter school), and the program in question had by far the highest per-pupil spending. My estimates are still surprising given that the original purpose of the program was to eliminate the educational opportunity gap across districts. I find no strong evidence that the construction projects led to a narrowing of this gap. It may be possible that construction led to improvements in other outcomes that may be valuable to parents. In Table 7, I present estimates of

the effect of the program on attendance and discipline. These regressions find no evidence of an improvement in either of these outcomes for students. While I am unable to find any tangible evidence of gains accruing to students from the construction projects, there may be benefits that are unobservable in the data.

I do find strong evidence that schools respond positively with regards to their other inputs. As shown in Table 6, districts increase their number of teachers and teaching expenditures in response to the program. The increase in teachers is more than just the required amount to respond to increased enrollments, as the pupil-teacher ratio also decreases in the years after completion, dropping by nearly one student three years after completion (off a base of 17.6 students per teacher). Non-capital expenditure per pupil increases by \$200 during the funding and construction period but drops to pre-funding levels once the project is complete. During non-project years, the districts spend on average \$750 per pupil on capital projects, so it is possible that the “crowding in” we observe is due to the freeing up of this allocation in the school budget during construction years.

5.3 Housing Price Analysis

The main challenge for understanding the effect of the construction program on housing prices is the large fluctuation in the housing market occurring during our sample period. Figure 8 shows a statewide year-by-year average of log housing transaction prices, clearly showing the boom and bust of the market in the mid-2000s. Further, this fluctuation was larger for wealthier and more urban districts, so we have to take care to adequately control for these differential effects across district types. To get an idea of how prices are moving during the periods of construction, I plot in Figure 9 a simple event study average around year of funding, separately by when districts are funded. These graphs clearly show the heterogeneity in trend across time. They also indicate how the economy-wide variation is much more important than any potential variation due to the program. To estimate the effect of the program on housing prices, I use the same estimation method as that used on enrollment effects. I control for district-specific time trends, group-specific year dummies, where groups are based on having eligibility in the same year, and characteristics of the house sold.

Figure 10 and Table 8 show the effect of the program on housing prices on both the balanced and unbalanced samples. In both cases, we see a positive and statistically significant effect of the program on housing values. By 4 years after completion, districts experience at least a 17% increase in housing values. Taking the average cost of construction (\$23,740 per pupil), this estimate corresponds to a 75 cent increase in housing values for every dollar spent on construction. This result is surprising when compared to Neilson and Zimmerman (2014),

who find a 6% increase in housing prices due to facility construction. Like my setting, the New Haven construction program was funded only 25% by local funds, and per pupil expenditures were much greater than expenditures in Ohio (\$70,000 v. \$23,000). Cellini, Ferreira, and Rothstein (2010) also find a 6% increase in housing values, but the projects they studied were completely funded by local district bonds, paid for through property taxes.

These regressions are the only case where including group-specific year fixed effects (discussed in Section 4) has a noticeable effect on regression estimates. When I only include sample-wide year fixed effects, the regression on an unbalanced sample has significantly positive pre-trends. This effect may be due to the fact that there are idiosyncratic housing shocks that are specific to districts of similar wealth, and districts choose to enter the program based on positive realizations of these shocks. This significant pre-trend is not present in the balanced sample regression.

To study the distributional effect of the program on housing values, I disaggregate the regression estimates to the district level. I do so by calculating the event study treatment effects separately for each district on the balanced sample. The left panel of Figure 11 shows the 4-year post-completion effect of the program in different districts, plotted against their percentile of wealth with a linear fit of the data. We see that there is no discernible difference in treatment effects across wealth. This uniformity is surprising when compared to Figure 2 showing that state assistance was much greater for the poorest districts, almost as large as total property value in some districts. Despite this uniformity across wealth, there is significant unexplained heterogeneity across district estimates. As mentioned earlier, this relatively muted response by the poorest districts is due to potentially many factors. Facility quality may be complementary with other inputs, leading to more positive educational outcomes for the wealthiest districts (though we could not find any direct positive educational outcomes). Poorer families may be less able to geographically relocate for the sake of accessing better public schools, or they may place lower relative value on school quality as a local amenity.

6 Cost-Benefit Analysis

To assess the efficacy of the program, I conduct two cost-benefit analyses. Firstly, I estimate the willingness to pay for the construction program of the average parent in a treated district and compare it to the average cost per pupil of construction. Secondly, I use a previous study on the lifetime income gains from improved test scores to measure the economic benefit of the program in human capital terms. I then compare this gain with program costs. The average per-pupil cost of each project is \$23,740, and the local contribution is \$6,669. To account for the deadweight loss of taxation, I add a 30% cost to each of these values, raising

the costs to \$30,862 and \$8,670, respectively.⁹

6.1 Housing-Price-Based Valuation

To infer the willingness to pay for the program, I first present a very simple model of housing valuation, based on Brueckner (1982) and used more recently in Barrow and Rouse (2004) and Cellini, Ferreira, and Rothstein (2010). The two essential elements are that changes in rents can be used to infer individual's valuation for an amenity, and that housing valuation is the present value of rents net of property taxes. Individuals have identical preferences $U(A, X, H, B)$, and they make a choice among neighborhoods to inhabit, which offer amenities A and X , and choose housing consumption H , and a numeraire composite good B . In our case, we let A be school facility quality, and X is a composite of all other local amenities. Each person is endowed with income I , which he spends on housing and the numeraire good, $R + B = I$. The key assumption of the model is that individuals are freely able to move across neighborhoods, so utility is equalized across regions for all individuals, i.e. $U(A, X, H, B) = k$. We can then rewrite their utility, incorporating their budget constraint, $U(A, X, H, I - R) = k$. Using the implicit function theorem and letting rents adjust to a change in amenity A , we have

$$\frac{U_A(A, C, H, I - R)}{U_B(A, C, H, I - R)} = R_A(A, C, H, B) \quad (1)$$

The ratio of derivatives of the utility function for A and B reflect the implicit price of A by indicating the increase in wealth an individual would need to compensate for a reduction in A . Therefore, we can infer the willingness to pay for an improvement in amenity A by the change in rental values induced by a change in A . Note, however, that R_A , reflects only the willingness to pay for *one year* of the amenity. Because the cost of the program accrues over multiple years, as do potentially the gains, we are interested in the net present value of the long run willingness to pay, $\frac{R_A}{\omega}$, where ω is the individual's discount rate. While we can't observe rental values, we do observe property values, which take the present discounted value of rents net of property taxation:

⁹The choice of 30% is based on Feldstein (1999). Various issues complicate the question of what deadweight loss percentage I should use. The district contribution is paid for by property taxes, while the state pays through their general revenue fund, collected mostly through income and sales taxes. Thus the deadweight loss may vary across types of taxes. Further, a large part of the state contribution after 2007 came from the legal settlement with the four major tobacco companies (see Note 6), which leads to no tax distortion. While I choose 30%, I get the same qualitative result that the estimated willingness to pay is lower than total cost but greater than local cost for any deadweight loss value greater than 5%.

$$P = \frac{R - (\tau_A + \tau_X)P}{\omega} = \frac{R}{\omega + \tau_A + \tau_X} \quad (2)$$

Here, τ_A and τ_X are separate tax rates for funding school buildings and all other neighborhood amenities, respectively. We assume each neighborhood government balances their budget but receives funding assistance from the state:

$$(\tau_A + \tau_X) \sum_{i=1}^N P_i = C^A(A) - S^A(A) + C^X(X) - S^X(X) = \text{LocalCost}^A + \text{LocalCost}^X$$

We now take these values, and plug them into Equation (2):

$$\sum_{i=1}^N P_i = \frac{1}{\omega} \left[\sum_{i=1}^N R_i - \text{LocalCost}^A - \text{LocalCost}^X \right]$$

Consider a change in the level of amenity A, caused by the construction program, one which we assume has no effect on the cost and quantity of other amenities X:

$$\sum_{i=1}^N \frac{\partial P_i}{\partial A} = \sum_{i=1}^N \frac{1}{\omega} \frac{\partial R_i}{\partial A} - \frac{1}{\omega} \frac{\partial \text{LocalCost}^A}{\partial A} \quad (3)$$

Note once again that the object of interest is long run willingness to pay for the program, which is why we scale everything by $\frac{1}{\omega}$. Further, the cost variables C^A and S^A reflect yearly costs for the amenity, so we again use $\frac{1}{\omega}$ to reflect the total change in costs. Property values respond to the program by internalizing both the improved amenity value of a district and the increase in local costs to pay for the program. To estimate the average willingness to pay for the program, I rearrange Equation 3 and make it at the per-pupil level:

$$\text{Willingness to Pay} \equiv \frac{1}{N} \sum_{i=1}^N \frac{1}{\omega} \Delta R_i = \frac{1}{N} \sum_{i=1}^N \Delta P_i + \frac{1}{\omega} \frac{\Delta \text{LocalCost}}{N}$$

The change in local cost in each year reflects the change in property taxes that homeowners must bear to pay for the program, with $\frac{1}{\omega}$ scaling the yearly costs to be in net present value. If the interest rate on the construction bond is similar to the discount rate of residents, then $\frac{1}{\omega} \frac{\Delta \text{LocalCost}}{N}$ will be very close to the listed cost of the bond. I therefore use that value for the cost of the program.¹⁰

From the housing price regressions, I estimate that construction increased home values by about 20% and did not differ significantly by district wealth (despite significant unexplained heterogeneity), so I set $\frac{1}{N} \sum_{i=1}^N \Delta P_i$ to be 20% of a district's per-pupil property value. The average treated district has \$89,000 in assessed property value per pupil in the year of funding (adjusted to 2010 Dollars) and a total and district cost of \$30,862 and \$8,670 per pupil, respectively. The formula above gives an implied willingness to pay of \$26,467. This number is slightly lower than total costs, though it hides significant heterogeneity across district wealth. Because the percentage increase in home values was uniform across district wealth, the amount of housing capitalization was much greater in the wealthiest districts. This fact is in line with Barrow and Rouse (2004), who look at the capitalization of state schooling aid in housing prices and find significantly greater effects in wealthier districts. Figure 12 compares total and local costs to the implied valuation of the program in each district. Every district has a greater valuation than local costs, while 52% have implied valuations lower than the total cost of the program, concentrated among the poorest districts in the state.

This efficiency loss reflects the main drawback of state financing of these projects, which is the misaligned incentives of the state and the district. Under the well-known Samuelson condition for the optimal expenditure on public goods, $\sum_{i=1}^N \frac{\partial R_i}{\partial A} = \frac{\partial C_A(A)}{\partial A}$, meaning the marginal cost of the good is equal to the total gain in welfare in a district from providing the good (Samuelson, 1954). From the perspective of the state, they should choose to fund any program where $\sum_{i=1}^N \frac{\partial R_i}{\partial A} > \frac{\partial C_A(A)}{\partial A}$, i.e. a case where property values increase by at least as much as the state contribution to the program. However, the district only bears the local cost, meaning that they would pass a bond for funding in any case where $\sum_{i=1}^N \frac{\partial R_i}{\partial A} > \frac{\partial \text{LocalCost}^A}{\partial A}$, i.e. where the willingness to pay is higher than their own cost. So it is not at all surprising that we find for many districts that the implied valuation is lower than the total cost of the project.

¹⁰Note that this model involves many simplifying assumptions relative to other papers in this literature. Individuals all have the same income and the same preferences, therefore I don't differentiate between the willingness to pay of the marginal versus the average homebuyer. Because there is no heterogeneity, I am not modelling the political economy of how districts choose to fund construction. I also assume that each household has one student in a K-12 school. Note further that this model does not include a firm or any commercial property. In bid-rent models with a firm (e.g. Brueckner, 1979; Roback 1982), land is freely mobile between housing and commercial usage, so a single rent value prevails within each district/region. While my estimate of the housing price response will come from data that only includes residential properties, my measure of total property value in a district (taken from the Ohio Department of Taxation), includes all assessed property value in the district. Therefore, my estimate of the willingness to pay for the program will be biased only if the price response of commercial property is different from the response of residential property.

It may not necessarily be a problem that the willingness to pay of the program is lower than costs in the poorest districts. Given that the primary concern of the court ruling against Ohio's existing funding system was equity of schooling quality across districts, we may not be worried about an efficiency loss in implementing the program. However, by calculating the difference between implied valuation and program costs, we can quantify the extent of the efficiency loss from implementing the program.

6.2 Test-Score-Based Valuation

The second cost-benefit analysis I conduct is based on the economic gain or loss due to the test score effects of the program. A large literature has looked at the economic returns to specific inputs in the education process as a means for assessing the efficacy of policy interventions such as reduced class size (for a review, see Speakman and Welch, 2006). However, no paper has causally estimated the economic impact of improved facilities by linking students to adult earnings in a setting with variation in facility quality. To approximate this impact, I use a recent paper, Chetty, Friedman, and Rockoff (2014), on the effect of improved teacher quality on student lifetime earnings. Their results imply that a one standard deviation increase in test scores (induced by teacher quality) leads to a 11.11% increase in math scores and a 14.8% increase in English scores.¹¹ To use these results, I need to assume that the conversion from a test score effect to an earnings effect is the same for facility quality as it is for teacher quality. This assumption is satisfied if we believe more specifically that the entire human capital gains from construction and teacher quality are captured by test scores.

For simplicity, I use my estimates of test score effects from the regressions with grades pooled (Table 4). In standard deviations, my estimates for the effects of a district's project (pooled across grades) are a negative .03 SD effect during years of construction for both reading and math and a .03 effect for math after completion. While the positive math effect is not significant in my main specification, I include it regardless because of the large standard errors in the estimates and because I find significant and quantitatively identical effects in the model with no time trends. Further, since the positive test score effects are so middling, this cost-benefit calculation is to be taken as a best-case scenario valuation of the educational effect of the program.

¹¹Specifically, Chetty et al. (2014) first create estimates of teacher value-added by regressing student test scores on teacher fixed effects and student demographics, $A_{it} = \beta X_{it} + \mu_{jt} + \epsilon_{it}$, where μ_{jt} is a teacher fixed effect that varies across years. A_{it} is test scores normalized to be mean 0 and standard deviation 1, so that a 1 unit increase in μ_{jt} corresponds a 1 standard deviation increase in test scores. They then regress earnings (net of observable individual characteristics) on teacher value added estimates, $Y_{it} = a + k_g m_{jt} + \eta_{it}$, where $m_{jt} = \mu_{jt}/\sigma_\mu$, and σ_μ is the standard deviation in teacher VA. For their main specification, they estimate $\hat{k}_g = .0165$, meaning a 1 SD increase in teacher VA leads to a 1.65% increase in a student's lifetime earnings. They calculate σ_μ to be .163 for math and .124 for English in elementary school and .134 for math and .099 for English in middle school. Taking the midpoint SD for each subject and dividing k_g by SD, we get that a 1 SD increase in test scores (caused by increase in teacher quality) leads to a 11.11% increase in math scores and 14.80% increase in English scores.

The average district in the sample waits 1.8 years between funding and the start of construction, and construction takes an average of 2.7 years to complete.¹² I then assume that the constructed facilities will be in use for 22 years. This time choice seems reasonable given that in Cellini et al. (2010) the expected time between locally funded capital projects is around 22 years.¹³ Using 2010 Census data, the average lifetime earnings in Ohio is \$364,300 in net present value at age 12 (the midpoint age in K-12 schooling). I discount all estimates to get a net present value for the year of funding, using a 5% discount rate.

Using these values, I calculate that the completion of a construction project led to a \$7,100 increase in lifetime earnings per pupil.¹⁴ This net effect consists of a \$6,700 *loss* during construction, followed by a \$13,800 gain post-completion. This estimate is significantly lower than the total cost of construction and slightly lower than the \$8,600 local cost. It is also significantly lower than the implied valuation by housing prices. This gap could both be due to imprecision in the test score estimates or due to homeowners valuing other benefits from the program.

7 Interpretation

The results above lead broadly to two conclusions, that residents valued the construction projects in their districts while experiencing no measurable improvement in student outcomes. This outcome leaves us to wonder why the student results are so mixed, why districts still elected to carry out these projects, and why, despite no student improvements, we still find positive responsiveness of enrollment and housing prices.

One possibility for why construction led to meagre student improvements was the need for uniformity in quality across district projects. While each district was allowed some discretion in school design, the need for minimum standards across districts may have led to the majority of funding being spent on improvements that would not have been carried out had the district funded construction independent of the state's program.

Another potential culprit for the meager student improvements is that the program did not actually target low-performing schools. District wealth is not very informative of public school quality in Ohio, insofar as grades are a measure of school quality, so targeting poor

¹²To account for the fractions in expected years, I perform four separate calculations, using 1 and 2 years between funding and construction and 2 and 3 years between construction and completion. I then take a weighted average over these calculations such that the expected times are 1.8 and 2.7, respectively. Details are in the Appendix.

¹³I calculate this number based on the fact that each year an average of 41 measures are passed in the 948 districts in California. Assuming that the passing of the bonds is not serially calculated (though in their data it is slightly), the expected time between projects is 22 years.

¹⁴This “per pupil” measure is technically an effect per slot at a school district, as the gains clearly accrue not only to current students but any students in the school within 22 years of completion. I denominate the total gains by per-current-pupil so as to compare the human capital gains to the cost of the program, which is also reported per-current-pupil.

districts is not identical to targeting poor-performing districts. Across all grades and both subjects, the mean difference in percent of students proficient in the public school system is 6-12% between the poorest and wealthiest districts, on a statewide average of around 80%. However, the regression R-squared ranges from .02 to .06. On the other hand, percentage of students in a district who are eligible for free or reduced-price lunch is significantly more predictive of test scores, with R-squared ranging from .3 to .4. This difference in explanatory power may be due to wealthier residents in a district both driving up property values and disproportionately sending their children to private schools. Due to this fact, it may have been a better targeted program to base eligibility on the number of poor students in the district rather than total property value. This eligibility scheme would have been more in line with traditional categorical aid, based on student demographics rather than districts wealth (Hoxby, 1996). Further, the strongest positive effect on test scores I find are in districts with many poor students.

It also seems unlikely that construction or renovation was necessary in every single district. By 2014, only 15 out of the 152 districts that were in the bottom quarter of property value in 1997 had not completed a project. It is possible that many of these districts were not in dire need of construction but accepted a heavily state-subsidised program because of the relatively small cost to residents. Because of the progressivity of state funding, voters in the poorest districts have the lowest threshold of student gains required to support construction. This tolerance may be why we see no improvements for the poorest districts.

Even so, that does not explain the positive effects I find for housing prices. It is possible that the positive housing prices observed are not due to some unobserved improvement in educational production, but instead an improvement in neighborhood amenities. The new buildings may be aesthetically pleasing for the neighborhood, or they may provide improved facilities for non-educational public purposes such as recreation or community meetings (Neilson and Zimmerman, 2014). Another possibility, put forth by Bayer et al. (2007), is that a strong determinant of housing prices is preference for one's neighbors, and construction led to an influx of desirable residents. To isolate a non-educational improvement in district amenities, I run the housing price analysis again on homes with one or two bedrooms, which are more likely to be owned by families with no children¹⁵. These results are in columns 3 and 4 of Table 8. The price response is nearly identical to the response in all single-family households, consistent with the hypothesis that construction is being valued as a non-educational amenity.

¹⁵While it might be more obvious to look at only one-bedroom homes, there are very few of these in our sample districts, which are largely suburban or rural.

8 Conclusion

This paper studies a large-scale school construction program in Ohio for the period 1997-2013. The program spent \$10 billion on renovating and building schools along the entire wealth spectrum of the state. The primary goal of such an effort was to mediate the disparity in school funding across districts and equalize educational opportunities for students of different economic backgrounds. I find that the program is treated favorably by residents, with positive enrollment responses and a substantial increase in property values for the average treated district. While the enrollment response is uniform across districts, housing value capitalization of the program is greatest in the wealthiest districts. Schools respond to funding from the state by increasing other educational inputs, leading to an overall increase in the number of teachers, expenditures on teaching, and non-capital expenditures per pupil and a decrease in the student/teacher ratio. Despite these positive reactions, I find little to no evidence of positive returns to students. Math and reading scores suffer during the construction process, and I find weak evidence of improvement after completion concentrated among elementary school students (though large standard errors make it difficult to detect small positive effects). I also find no evidence of improvements in other student outcomes such as attendance and discipline. My estimate of the average willingness to pay for the program is slightly lower than the cost of the program, though much greater for the wealthiest districts and significantly lower for the poorest districts. Districts were rational in carrying out their projects, as residents' valuations were greater than the local cost of construction. The willingness to pay for the program cannot be explained fully by human capital gains from improved school quality and is potentially due to valuing the facilities as a non-schooling amenity. I therefore conclude that, while facility quality is valued by residents and the program led to a narrowing in school spending across districts, school building construction is not an effective method for the desired goal of equalizing educational opportunities across students and districts.

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Appendix

A1: Human Capital Valuation Details

To be able to use estimates of the relation between test scores and lifetime earnings reported in Chetty et al. (2014), I need to convert my test score effects to standard deviations, where the standard deviation is measured at the student level. The results in my main specification are for changes in the school-level percentage of proficient students in math or reading. It is not obvious how to convert these results into a student level effect, nor how to rescale them into standard deviations. To do so, I create a new outcome variable that simulates a student-level grade. The data available on test scores in Ohio report what percentage of students in each school and grade scored “advanced,” “accelerated,” “proficient,” “basic,” or “limited” in the statewide exam. Supposing that those are the only scores a student can receive on an exam, we have the full distribution of outcomes within a school. I create an average score for a school in the following way:

$$\text{Score}_{jt} = .25 * \text{Basic}_{jt} + .5 * \text{Prof}_{jt} + .75 * \text{Acc}_{jt} + \text{Adv}_{jt}$$

where “Basic” etc. are measured out of 100, so the total score ranges from 0 to 100. Similarly, I get a measure of the variance of outcomes:

$$\begin{aligned} \text{Var}_{jt} = & \frac{\text{Basic}_{jt}}{100} (\text{Basic}_{jt} - \bar{\text{Score}})^2 + \frac{\text{Prof}_{jt}}{100} (\text{Prof}_{jt} - \bar{\text{Score}})^2 \\ & + \frac{\text{Acc}_{jt}}{100} (\text{Acc}_{jt} - \bar{\text{Score}})^2 + \frac{\text{Adv}_{jt}}{100} (\text{Adv}_{jt} - \bar{\text{Score}})^2 \end{aligned}$$

I then rerun the main test score regressions, but using Score_{jt} as the outcome variable. I then divide the treatment effects by the standard deviation. Using these estimates, I get that on average a year of construction causes a -.03 SD effect on reading and math scores, and on average a year of completion causes a .03 SD effect on math scores.

My calculation of the lifetime gain is the following. Suppose construction begins one year after funding, construction takes two years, and once completed the building is in use for 22 years. Relative to the year of funding, the calculated effect is

$$\begin{aligned}\delta_{12} &= \left[\frac{1}{1+r} + \frac{1}{(1+r)^2} \right] \cdot [(0.111) \times (-0.03) + (0.148) \times (-0.03)] \\ &\quad + \frac{1}{(1+r)^3} \cdot \left[\frac{1 - \left(\frac{1}{1+r}\right)^{23}}{1 - \frac{1}{1+r}} \right] \cdot [(0.111) \times 0.03]\end{aligned}$$

where δ_{12} is subscripted for the years of funding and construction. The values .111 and .148 are the effects of a 1 SD increase in math and English scores, respectively, taken from Chetty et al. To account for the fact that the true average times between funding and construction and between construction and completion are 1.8 and 2.7 years, respectively, I calculate total effects δ_{12} , δ_{13} , δ_{22} , and δ_{23} , and take a weighted average such that the expected times are 1.8 and 2.7:

$$\delta = .2 \times .3 \times \delta_{12} + .2 \times .7 \times \delta_{13} + .8 \times .3 \times \delta_{23} + .8 \times .7 \times \delta_{23}$$

I calculate δ to be .0195, meaning the introduction of the program led on average to a 1.95% increase in lifetime earnings per pupil. Finally, the economic gain from the program is $\delta \times \text{Lifetime Earnings}$. To calculate lifetime earnings, I look at all residents in Ohio surveyed in the 2010 American Community Survey. For every individual between ages 16 and 65, I discount their wages to be in net present value for age 12 (the midpoint age in K-12 schooling). I then average over all individuals and multiply by 50. This calculation gives me an average lifetime earnings value of \$364,300 at age 12, and a monetary gain from the program of \$7,100 per pupil.

A2: Data Description

Construction Status: The data on school construction projects are taken from the annual reports published by the Ohio School Facilities Commission for the years 2001-2014:

<http://www.osfc.ohio.gov/Library/Publications.aspx>

Though the format changes every year, the reports generally summarize all ongoing and completed projects, including total cost, year of funding, program of funding, and status of the project (design phase, building, completed). In some years, information is provided on the total number of buildings undergoing construction and renovation. Beginning in 2006, reports feature a section on all recently completed projects, describing the schools built and the main supervisors of the project.

There are no annual reports for 2005 and 2008. To find construction status for these years, I do the following: I treat the project as completed in 2005 if its status is construction in 2004 and completed in 2006, but it is not featured in the 2006 annual report as a recently completed project. I do the same for 2008.

Ohio Department of Education: From the ODE I get data on attendance, discipline, test scores, and enrollment for the school years for the school years 2005-06 to 2013-14. Discipline data records expulsions, suspensions, and all other disciplining actions per 100 students. Test scores are for Ohio's Achievement Assessment exams given in October of each year. Students in 3rd through 8th and 10th through 12th grade are tested in math and reading and, for some grades, science, social studies, and writing. The tests are given again in May for students who did not perform sufficiently well, but I do not use this data.

Property Valuation: Property Values are taken from the Department of Taxation and are available for 1992 onwards. This district-level measure is based on government property assessments and includes all commercial property in the district.

Eligibility Rankings: Ranking of school districts is based on “adjusted average valuation per pupil.” Total appraised property value in the district (including commercial property) is scaled by total public K-12 enrollment and averaged for the past 3 years. Then the adjustment is done as follows:

$$\text{AdjValuation} = \text{Avg.Valuation/Pupil} - \$30,000[1 - \frac{\text{median district income}}{\text{median state income}}]$$

The OSFC provides data on eligibility rankings for 2002 onwards. For my purposes, I need rankings going back to 1997, so I construct a measure of eligibility ranking based only on average property valuation per pupil, published by the Department of Taxation. While this ranking ignores median income in the district, the correlation between my ranking and the true ranking is over .99 in the period for which I have both measures. I then construct the variable YearEligible in the following way:

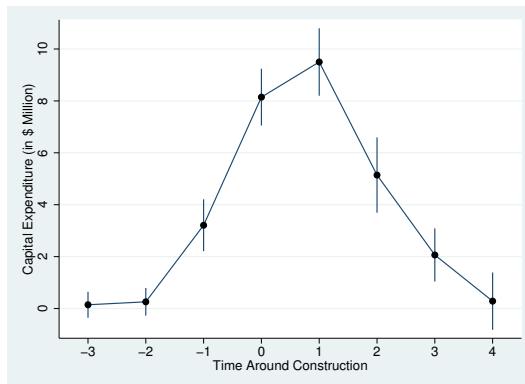
in other words, a district is eligible in the first year where their eligibility ranking is lower than the highest ranked district that was funded in that year.

Linking: Ohio records an Internal Reference Number (IRN) for every district, which remains the same for every year. Almost all data sets I use include the IRN, so I use it to link across data sets and years. The annual reports are the only data to not have an IRN. I match these observations to the Common Core of Data, which includes the name of a school district for

every year. While districts sometimes change names over time (necessitating the use of the IRN), disagreements within a year across the data are generally not large (e.g. Plymouth School District v. Plymouth-Shiloh School District) and simple to correctly match.

Timing: I line up the timing of my data based on school year. Therefore, the 2014 annual report, and the construction statuses it contains, are treated as occurring contemporaneously with the 2013-14 school year, all of which I designate as the year 2013. This timing means that it may be the case that construction finishes *after* school outcome variables are measured. However, this means our estimates of treatment effects for τ years after construction will be downwards bias. The alternative (assign the 2014 annual report to be for 2014-15) would risk having construction completed over a year prior to outcome measurements and treated as contemporaneous, leading to an upwards bias in my estimates.

Linking Check: As a verification that the linking across years and coding of program status has been conducted correctly, I plot capital expenditure (provided by the CCD) around the date of the start of construction, taken from the annual reports:



This graph shows that construction status corresponds well with reported capital expenditures by districts. The small pre-trend in the year prior to construction is consistent with the fact that construction may start mid-year and only be reported as beginning when the next year's annual report is published.

Housing Data

From CoreLogic, I received two separate data sources on the housing stock in Ohio.

- Property deeds for every plot of land in the state. These deeds record a unique Assessor Parcel Number, County of property, GPS coordinate, street address, current owner, and

characteristics of the land and current building on the property (e.g. square footage, number of stories, number of bedrooms, age, property type). The deeds also record the date and price of the previous two arms length sales of the houses. I link these assessments to school districts by their GPS coordinates and Census boundary files using ArcGIS.

- Grant deeds recording sales in the state for years 2000-2015. This data is linkable to property deeds using the Assessor Parcel Number, and includes the seller and buyer, sale price and date, whether the transaction is arms-length, and the amount of mortgage taken out, if any, by the buyer.

Since funding from the OSFC began in 1997, I would ideally have grant deeds going as far back as 1992 to cover a sufficient pre-period. However, I only have these deeds starting in 2000. To still be able to assess the effect of the early programs, I take advantage of the fact that each property deed lists the two most recent sales of the property. Therefore, I observe sales of houses in the 1990s which were either the last or second to last sale of that house as of 2015. Further, it is not uncommon for sales to go unrecorded on the property deed (e.g. the two most recent sales listed are 2006 and 2008, but there is a 2014 sale in the transactions data). Therefore, many observations of 1990s sales in the property deed listings are for properties that have been sold more than twice as of 2015.

The main concern with using 1990s data listed on the property deeds is that the observations may not be comparable to observations from the transactions data. I control for mean differences using the year-group fixed effects, and observable changes in the composition of types of houses are accounted for by controlling for property characteristics. This method leads to a biased estimate of the program effect if houses that were last sold in the 1990s have a different treatment effect from the average house sold in the period 2000-2015.

Some sales occur before the current form of the house. We can see this because the listed date of a house's construction will be later than the sale date. In this case, we do not know the true characteristics of the property being sold (including age). Though not ideal, I handle this issue by using property characteristics for the current property. For age, I have the following variables. New = $\mathbb{I}(\text{Saledate}=\text{ConstructionDate})$, Old = $\mathbb{I}(\text{Saledate}<\text{ConstructionDate})$, and a quadratic in house age, setting age to zero for sales prior to construction.

A3: Program Description

The OSFC oversees four construction programs. The largest one, the Classroom Facilities Assistance Program (CFAP), funds district-wide overhauls of school buildings, and priority of funding is based on the wealth of a district (explained above). If a district's buildings are in a state of disrepair that demand immediate renovation, they receive funding from the Exceptional Needs Program (ENP), which supports districts with below median wealth. The size of these projects are smaller ($\sim \$5,000$ per pupil rather than $\sim \$20,000$ per pupil), but they do not preclude districts from receiving CFAP funding later on. If a district wants to begin construction or renovation using local funding while waiting for eligibility from CFAP, they may do so under the Expedited Local Partnership Program (ELPP). Though this program is fully locally funded, it is coordinated with the OSFC to guarantee that the program accords with guidelines necessary for eventual CFAP funding. The final program is the Urban Initiative (UI), begun in 2002, which provides funding to the six big city school districts.

In this study, I restrict my analysis to the CFAP construction projects. These programs are more uniform in their practice, are the largest in terms of funding per pupil, and have the most transparent process for allocating funding per district. Further, I restrict my sample to districts who receive CFAP funding for their first project (i.e. excluding districts that first receive ENP, ELPP, or UI assistance). This restriction is done to guarantee that the treatment effect estimated for the construction project is relative to a state of no construction rather than a smaller project.

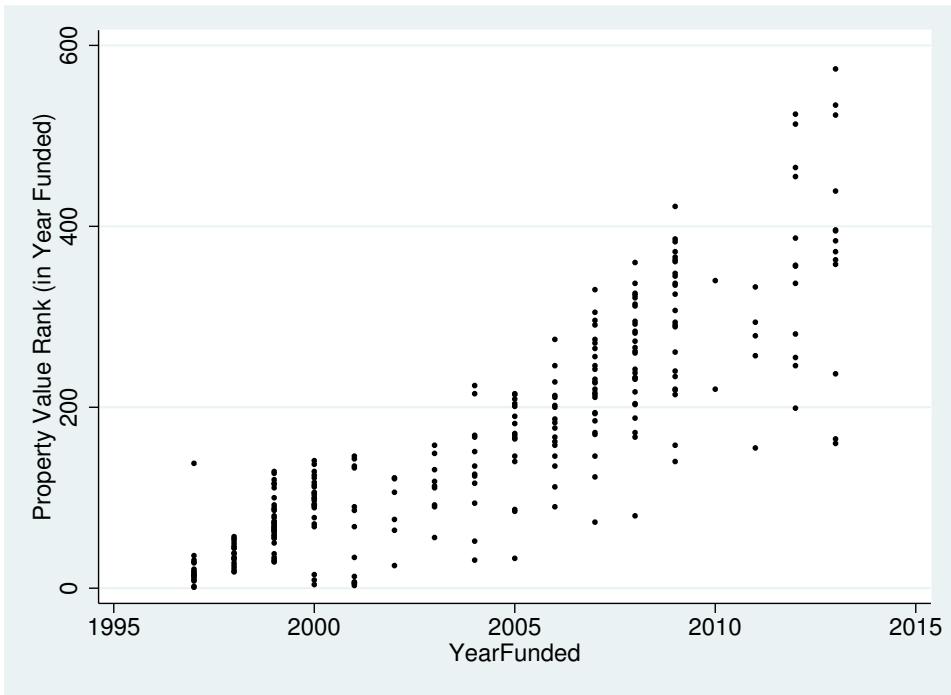
A4: Figures and Tables

A4.I Summary Statistics

	1997 - 2001	2002 - 2007	2008 - 2014	Not Treated	Total
K-12 Enrollment	1982.9 (2122.0)	2109.0 (1685.2)	2525.4 (2481.1)	3451 (2338.5)	2204.7 (2130.8)
Fraction Free/Reduced Price Lunch	0.296 (0.150)	0.229 (0.121)	0.194 (0.106)	0.206 (0.0314)	0.243 (0.135)
Property Value per Pupil	57514.5 (11852.2)	77991.6 (12817.4)	99648.5 (18978.0)	105530.2 (24391.3)	77266.3 (22997.9)
Construction Cost Per Pupil	23765.7 (7249.8)	25479.7 (5349.7)	21926.9 (9048.4)	.	23740.4 (7453.5)
State Contribution Per Pupil	20210.4 (6286.8)	18314.1 (4515.4)	11787.8 (7335.8)	.	17070.8 (7101.6)
Project Length (Funding to Completion)	4.571 (1.726)	4.854 (1.248)	4.156 (1.110)	.	4.616 (1.493)

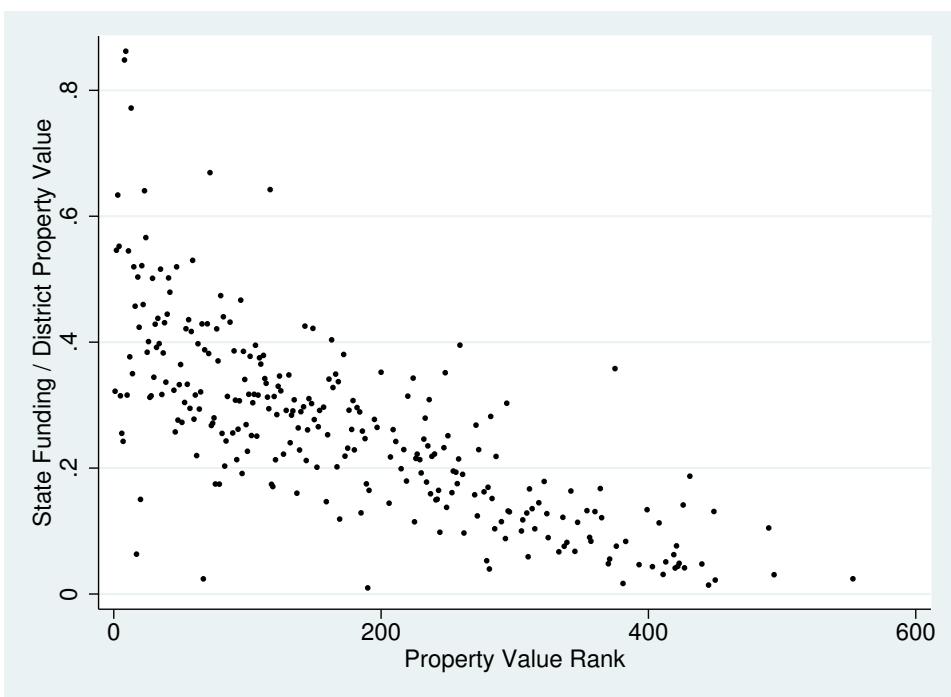
Notes: Enrollment, reduced-price lunch, and property values are measured in 1998. Property value and construction costs are in 2010 Dollars. Standard deviations are in parentheses. Enrollment and reduced-price lunch are from the Common Core of Data, Property values are from the Ohio Department of Taxation, and construction costs and duration are from the OSFC Annual Reports.

Table 1: Summary Statistics by Year of Funding



Notes: Data on property values taken from Department of Taxation. Data on year of funding are taken from the OSFC Annual Reports.

Figure 1: Wealth Rank of District by Year of Funding

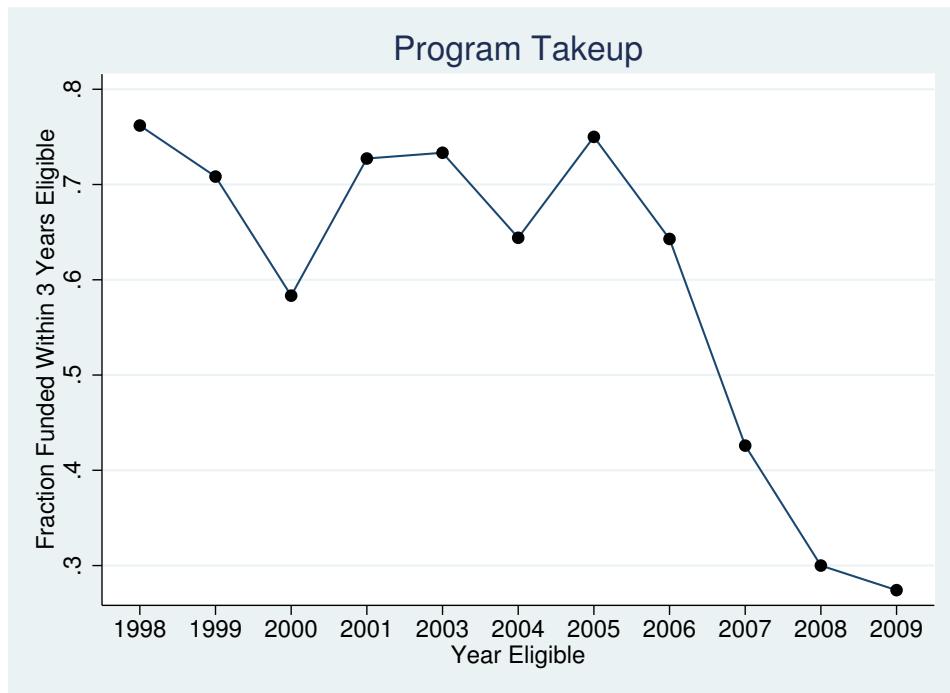


Notes: Property value measured the year district receives funding.

Figure 2: State Funding/District Wealth by Property Value Rank

Study	Sample	Avg. Cost Per Pupil (in treated districts)	Estimation Strategy
Cellini et al. (2010)	948 California districts	\$6,300	RD
Hong and Zimmer (2014)	~550 Michigan districts	\$7,000	RD
Martorell et al. (2015)	812 Texas districts	\$11,000	RD
This paper	609 Ohio districts (248 completed)	\$23,000	Fixed Effects
Neilson and Zimmerman (2014)	1 New Haven district	\$70,000	Fixed Effects

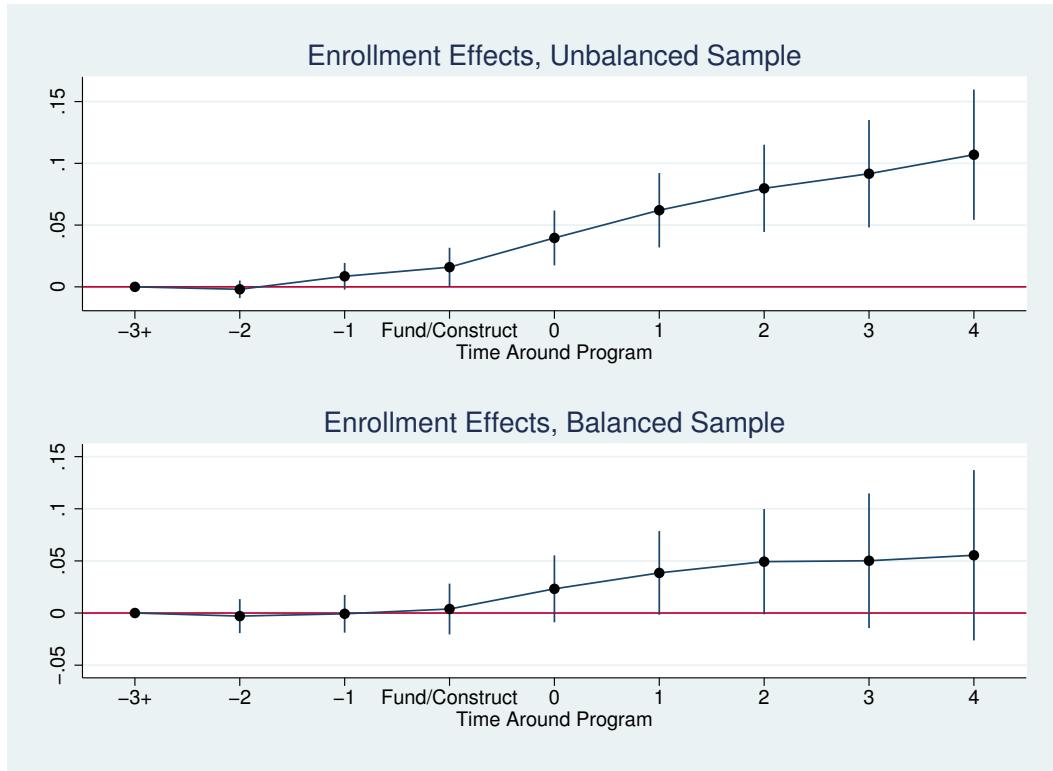
Table 2: Comparison of Program Size in various papers



Notes: Year of funding is taken from the OSFC Annual Reports. Year Eligible is inferred using property values from the department of taxation and which districts are funded each year.

Figure 3: 3-Year Program Takeup

A4.II District-Wide Results



Notes: graphs corresponds to columns 1 and 2 in Table 3. Regression includes district-specific fixed effects and time trends, eligibility-group year fixed effects, controls for school expenditures, and an indicator for 5+ years post-completion. Estimates are relative to 3+ years prior to funding, included as an implied zero in the plot above. Data on enrollment is for years 1997-2011, taken from the Common Core of Data. The Balanced Sample includes all districts that have data for at least 2 years prior to funding through 4 years post-completion. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Figure 4: Enrollment Effects

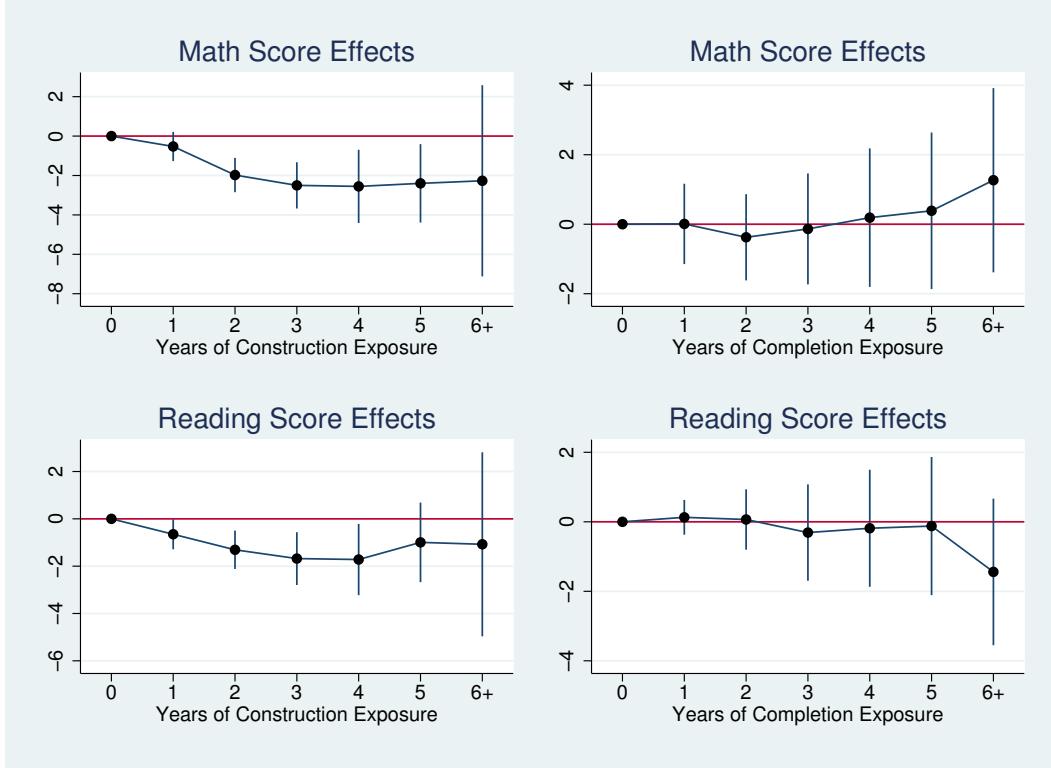
	(1) Log Enrollment	(2) Log Enrollment	(3) Log Enrollment	(4) Log Enrollment
2 yr. Pre-Funding	-0.00199 (0.00361)	-0.00293 (0.00809)	0.000872 (0.00449)	-0.00468 (0.00699)
1 yr. Pre-Funding	0.00855 (0.00548)	-0.000719 (0.00899)	0.0144* (0.00717)	0.00742 (0.00732)
Funding/Construction	0.0159* (0.00797)	0.00387 (0.0121)	0.0243** (0.00916)	0.0159 (0.0125)
0 yr. Post-Completion	0.0396*** (0.0113)	0.0232 (0.0159)	0.0505*** (0.0131)	0.0370* (0.0153)
1 yr. Post-Completion	0.0620*** (0.0153)	0.0385 (0.0199)	0.0653*** (0.0169)	0.0434* (0.0192)
2 yr. Post-Completion	0.0798*** (0.0179)	0.0493 (0.0251)	0.0855*** (0.0211)	0.0546* (0.0238)
3 yr. Post-Completion	0.0916*** (0.0221)	0.0502 (0.0321)	0.0949*** (0.0269)	0.0569 (0.0307)
4 yr. Post-Completion	0.107*** (0.0268)	0.0554 (0.0406)	0.109*** (0.0319)	0.0578 (0.0369)
N	3904	635	1896	495
R2	0.999	0.997	0.999	0.997
Balanced Sample	No	Yes	No	Yes
Districts	All	All	Poorest 25%	Poorest 25%

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: regressions all include district-specific fixed effects and time trends, eligibility-group year fixed effects, controls for school expenditures, and an indicators for 5+ years post-completion. Estimates are relative to 3+ years prior to funding. Data on enrollment is for years 1997-2011, taken from the Common Core of Data. The Balanced Sample includes all districts that have data for at least 2 years prior to funding through 4 years post-completion. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Table 3: Enrollment Effects



Notes: these graphs correspond to columns 1 and 2 in Table 4. Regressions all include district-specific fixed effects and time trends, eligibility-group year fixed effects, and controls for student demographics. Data on test scores is for years 2005-2012. Estimates are relative to 0 years of completion or construction, included in the graphs as implicit zeros. I refrain from controlling for other school expenditures, which would further reduce the sample size. The results look similar with expenditures included. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Figure 5: Test Score Effects

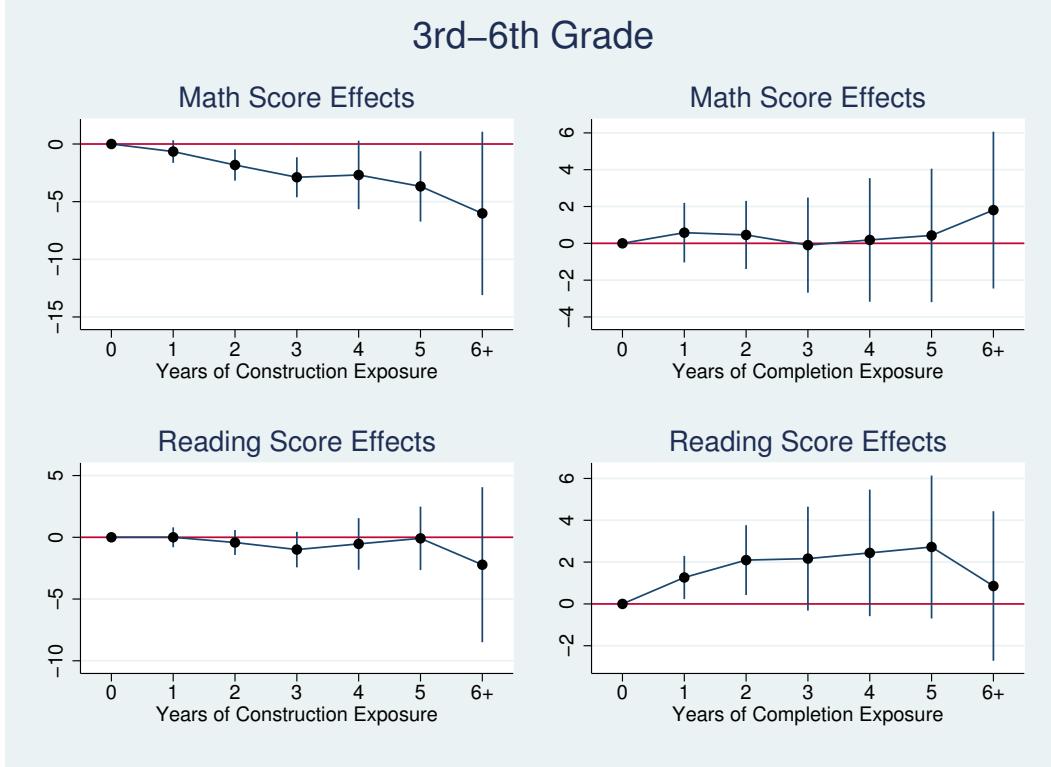
	(1) Math	(2) Reading	(3) Math	(4) Reading	(5) Math	(6) Reading
1 yr. Construction Exposure	-0.527 (0.376)	-0.651* (0.322)	-0.525 (0.541)	-0.878 (0.489)	-0.698 (0.620)	-1.426** (0.518)
2 yr. Construction Exposure	-1.977*** (0.442)	-1.308** (0.412)	-2.620*** (0.737)	-2.115* (0.835)	-3.460*** (0.727)	-2.767*** (0.697)
3 yr. Construction Exposure	-2.501*** (0.596)	-1.680** (0.566)	-3.723*** (0.979)	-3.364** (1.150)	-3.438*** (0.862)	-3.346*** (0.872)
4 yr. Construction Exposure	-2.553** (0.945)	-1.720* (0.765)	-4.273** (1.437)	-3.143* (1.454)	-4.610** (1.383)	-4.089*** (1.070)
5 yr. Construction Exposure	-2.399* (1.010)	-0.994 (0.854)	-4.001 (2.104)	-2.043 (1.832)	-4.280** (1.352)	-3.630** (1.182)
6+ yr. Construction Exposure	-2.269 (2.465)	-1.076 (1.975)	-7.408 (5.011)	-4.123 (5.054)	-4.260 (2.586)	-4.682* (2.283)
1 yr. Completion Exposure	0.00874 (0.587)	0.129 (0.254)	0.689 (0.589)	0.120 (0.484)	0.635 (0.687)	0.522 (0.324)
2 yr. Completion Exposure	-0.377 (0.630)	0.0665 (0.441)	-0.0853 (1.071)	-0.273 (0.804)	0.834 (0.884)	0.511 (0.676)
3 yr. Completion Exposure	-0.135 (0.811)	-0.310 (0.704)	0.508 (1.289)	-0.772 (1.118)	1.386 (1.261)	0.0463 (1.044)
4 yr. Completion Exposure	0.188 (1.013)	-0.186 (0.856)	0.652 (1.532)	-0.827 (1.300)	2.024 (1.537)	0.225 (1.245)
5 yr. Completion Exposure	0.386 (1.144)	-0.123 (1.010)	0.948 (1.739)	-0.568 (1.584)	2.688 (1.754)	-0.169 (1.511)
6+ yr. Completion Exposure	1.266 (1.347)	-1.442 (1.072)	2.216 (2.004)	-1.931 (1.669)	4.573* (2.066)	-1.107 (1.558)
N	14569	14570	7090	7091	5720	5720
R2	0.805	0.793	0.799	0.782	0.813	0.802
Avg. Proficiency	80.122	80.122	80.006	80.006	74.913	74.913
Districts	All	All	Poorest 25%	Poorest 25%	Lunch > 25%	Lunch > 25%

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: regressions all include district-specific fixed effects and time trends, eligibility-group year fixed effects, and controls for student demographics. Estimates are relative to 0 years of completion or construction. Data on test scores is for years 2005-2012. I refrain from controlling for other school expenditures, which would further reduce the available years. The results look similar with expenditures included. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Table 4: Test Score Effects



Notes: these graphs correspond to columns 1 and 2 in Table 5. Regressions all include district-specific fixed effects and time trends, eligibility-group year fixed effects, and controls for student demographics. Data on test scores is for years 2005–2012. Estimates are relative to 0 years of completion or construction, included in the graphs as implicit zeros. I refrain from controlling for other school expenditures, which would further reduce the sample size. The results look similar with expenditures included. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Figure 6: Test Score Effects, 3rd–6th Grade



Notes: these graphs correspond to columns 3 and 4 in Table 5. Regressions all include district-specific fixed effects and time trends, eligibility-group year fixed effects, and controls for student demographics. Data on test scores is for years 2005–2012. Estimates are relative to 0 years of completion or construction, included in the graphs as implicit zeros. I refrain from controlling for other school expenditures, which would further reduce the sample size. The results look similar with expenditures included. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Figure 7: Test Score Effects, 7th-12th Grade

	(1) Math	(2) Reading	(3) Math	(4) Reading
1 yr. Construction Exposure	-0.654 (0.498)	0.00215 (0.409)	-1.032* (0.457)	-0.880* (0.429)
2 yr. Construction Exposure	-1.821** (0.690)	-0.422 (0.512)	-2.367*** (0.695)	-2.303*** (0.541)
3 yr. Construction Exposure	-2.884** (0.882)	-0.994 (0.731)	-3.246** (1.088)	-3.071*** (0.886)
4 yr. Construction Exposure	-2.685 (1.509)	-0.536 (1.061)	-5.182*** (1.550)	-4.815*** (1.229)
5 yr. Construction Exposure	-3.673* (1.552)	-0.0862 (1.306)	-5.279** (1.910)	-4.868*** (1.418)
6+ yr. Construction Exposure	-6.018 (3.598)	-2.222 (3.189)	-5.917* (2.859)	-5.346* (2.142)
1 yr. Completion Exposure	0.580 (0.822)	1.264* (0.526)	0.479 (0.579)	0.139 (0.459)
2 yr. Completion Exposure	0.457 (0.940)	2.095* (0.848)	-0.580 (0.655)	-0.712 (0.890)
3 yr. Completion Exposure	-0.0965 (1.313)	2.166 (1.262)	-0.507 (1.188)	-1.448 (1.302)
4 yr. Completion Exposure	0.185 (1.706)	2.439 (1.537)	-0.349 (1.463)	-1.496 (1.492)
5 yr. Completion Exposure	0.428 (1.842)	2.721 (1.736)	-0.805 (1.716)	-1.935 (1.741)
6+ yr. Completion Exposure	1.805 (2.163)	0.856 (1.817)	-0.422 (1.968)	-1.586 (1.908)
N	6720	6721	6168	6168
R2	0.804	0.809	0.841	0.796
Avg. Proficiency	76.803	76.803	85.523	85.523
Districts	All	All	All	All
Grades	3rd-6th	3rd-6th	7th-12th	7th-12th

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: regressions all include district-specific fixed effects and time trends, eligibility-group year fixed effects, and controls for student demographics. Estimates are relative to 0 years of completion or construction. Data on test scores is for years 2005-2012. I refrain from controlling for other school expenditures, which would further reduce the available years. The results look similar with expenditures included. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Table 5: Test Score Effects, By Grade

	(1) Students/Teacher	(2) Log Teaching Exp	(3) Log No. of Teachers	(4) Non-Capital Exp
2 yr. Pre-Funding	-0.285* (0.120)	0.0168** (0.00569)	0.0121 (0.00770)	43.17 (62.02)
1 yr. Pre-Funding	-0.311 (0.190)	0.0198* (0.00804)	0.0233 (0.0119)	11.24 (93.92)
Funding/Construction	-0.435 (0.236)	0.0343*** (0.00976)	0.0324* (0.0150)	240.8* (112.4)
0 yr. Post-Completion	-0.544 (0.325)	0.0480** (0.0175)	0.0626** (0.0206)	307.7 (170.1)
1 yr. Post-Completion	-0.693 (0.376)	0.0572** (0.0188)	0.0873** (0.0280)	347.8 (209.1)
2 yr. Post-Completion	-1.092* (0.436)	0.0625** (0.0226)	0.123*** (0.0334)	272.9 (214.6)
3 yr. Post-Completion	-1.157* (0.453)	0.0719** (0.0260)	0.139*** (0.0338)	313.7 (239.8)
4 yr. Post-Completion	-1.106* (0.506)	0.0861** (0.0288)	0.149*** (0.0385)	398.7 (265.8)
N	4184	3933	4187	3930
R2	0.809	0.962	0.995	0.951

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: regressions are on an unbalanced sample and include district-specific fixed effects and time trends, eligibility-group year fixed effects, controls for school expenditures, and indicators for 4+ years post-completion and 4+ years prior to funding. Estimates are relative to 2 and 3 years prior to funding. Pupil-Teacher Ratio and Number of Teachers are available for years 1997-2011 and teaching expenditures and non-capital expenditures for years 1997-2010. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Table 6: Other School Expenditures

	(1) Attendance	(2) Attendance	(3) Discipline	(4) Discipline
2 yr. Pre-Funding	0.000592 (0.000589)		-1.232 (2.109)	
1 yr. Pre-Funding	0.000775 (0.000986)	0.000189 (0.000611)	-5.976 (4.026)	-4.754 (3.370)
Funding/Construction	0.000557 (0.00128)	-0.000201 (0.000946)	-5.063 (5.739)	-3.452 (5.111)
0 yr. Post-Completion	0.000119 (0.00167)		-7.137 (8.944)	
1 yr. Post-Completion	0.000714 (0.00219)		-5.550 (11.82)	
2 yr. Post-Completion	0.00196 (0.00261)		-7.835 (15.18)	
3 yr. Post-Completion	0.00278 (0.00310)		-9.274 (18.32)	
4 yr. Post-Completion	0.00321 (0.00351)		-9.751 (21.11)	
0-3 yrs. post-completion		-0.000955 (0.00140)		-4.860 (8.000)
N	1670	1670	1670	1670
R2	0.927	0.927	0.948	0.948
Balanced Sample	No	No	No	No
District Time Trends	Yes	Yes	Yes	Yes

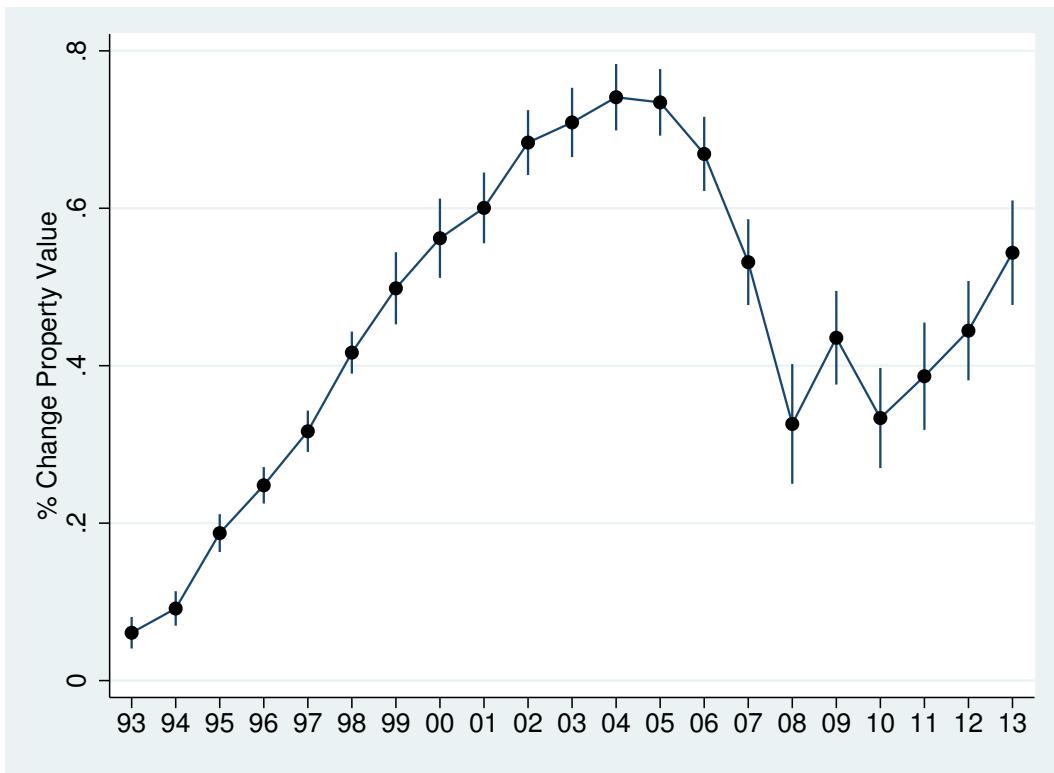
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: regressions are on an unbalanced sample and include district fixed effects, eligibility-group year fixed effects, controls for school expenditures, and indicators for 4+ years post-completion and 4+ years prior to funding. Estimates are relative to 2 and 3 years prior to funding. Data is available for years 2005-2013. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

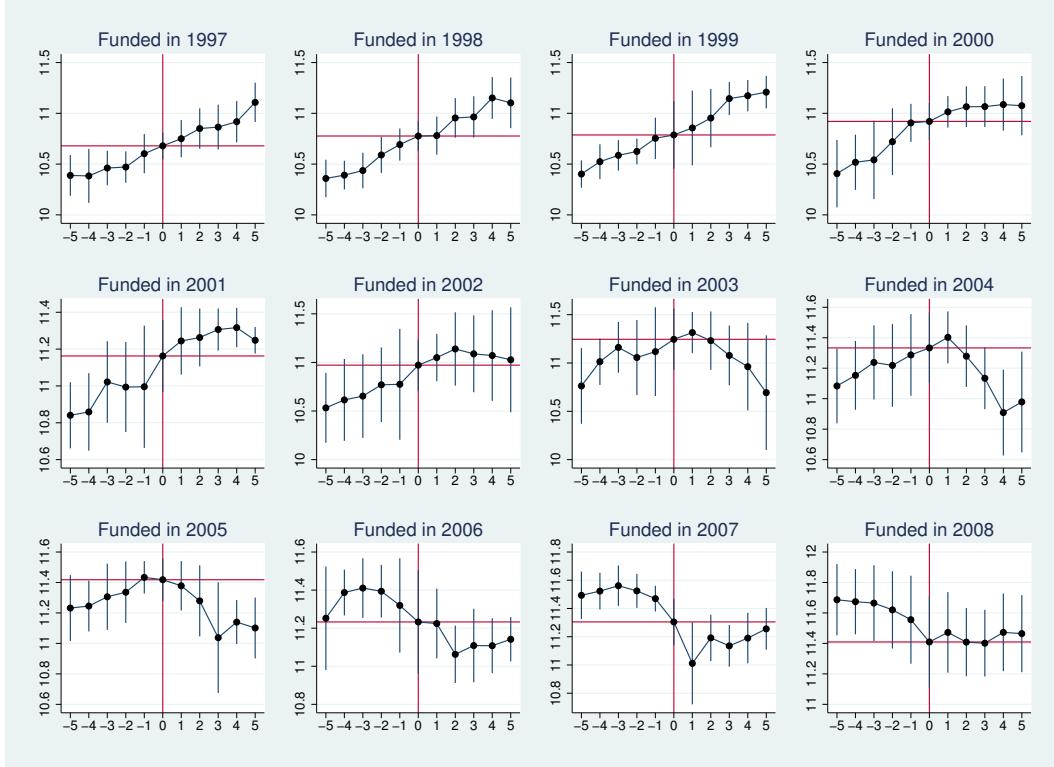
Table 7: Attendance and Discipline

A3.II Housing-Price Results



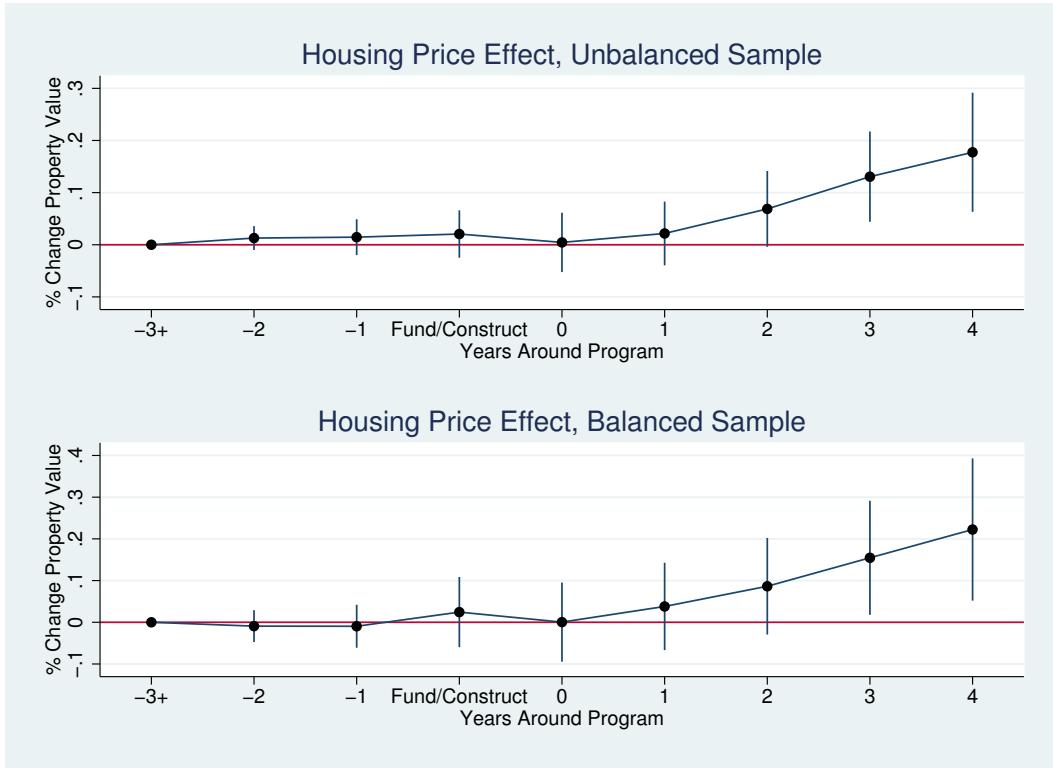
Notes: regression of all transactions on year indicators. Data spans the years 1992-2014 and are collected by Corelogic from county auditors in Ohio.

Figure 8: Statewide Housing Price Changes



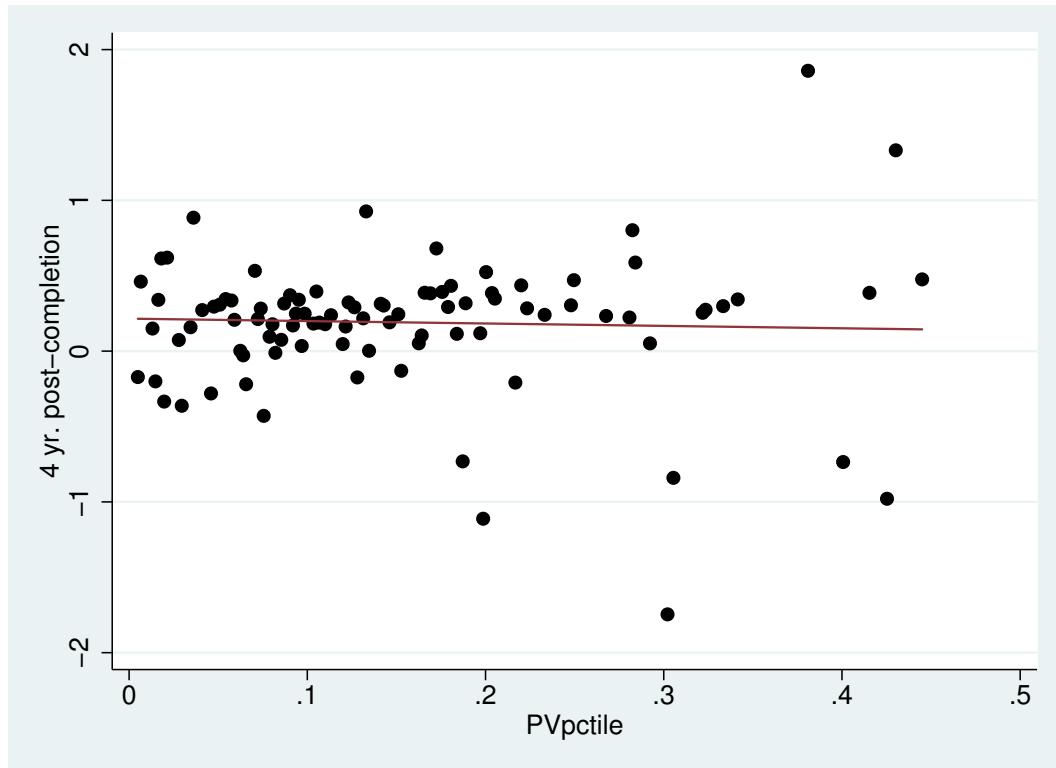
Notes: event studies of housing prices around date of funding, broken down by year of funding. Each regression restricts the sample to districts with data for 5 years before and after funding. No other controls are included in the regression. Data spans the years 1992-2014 and are collected by Corelogic from county auditors in Ohio.

Figure 9: Event Studies by Year of Funding



Notes: these graphs correspond to columns 1 and 3 in Table 8. Regressions are on log of transacted property values and include district-specific fixed effects and time trends, eligibility-group year fixed effects, controls for property characteristics, and an indicator for 5+ years post-completion. Estimates are relative to 3+ years prior to funding. Data spans the years 1992-2014 and are collected by Corelogic from county auditors in Ohio. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Figure 10: Housing Price Effects



Notes: comes from a balanced-sample event study regression on housing prices where event study indicators are interacted with school district. Regression includes district fixed effects, eligibility-group year fixed effects, controls for school expenditures, and an indicator for 5+ years post-completion. This graph plots 4-year post completion estimates against district property value percentile (measured in 1997).

Figure 11: Heterogeneous Effects

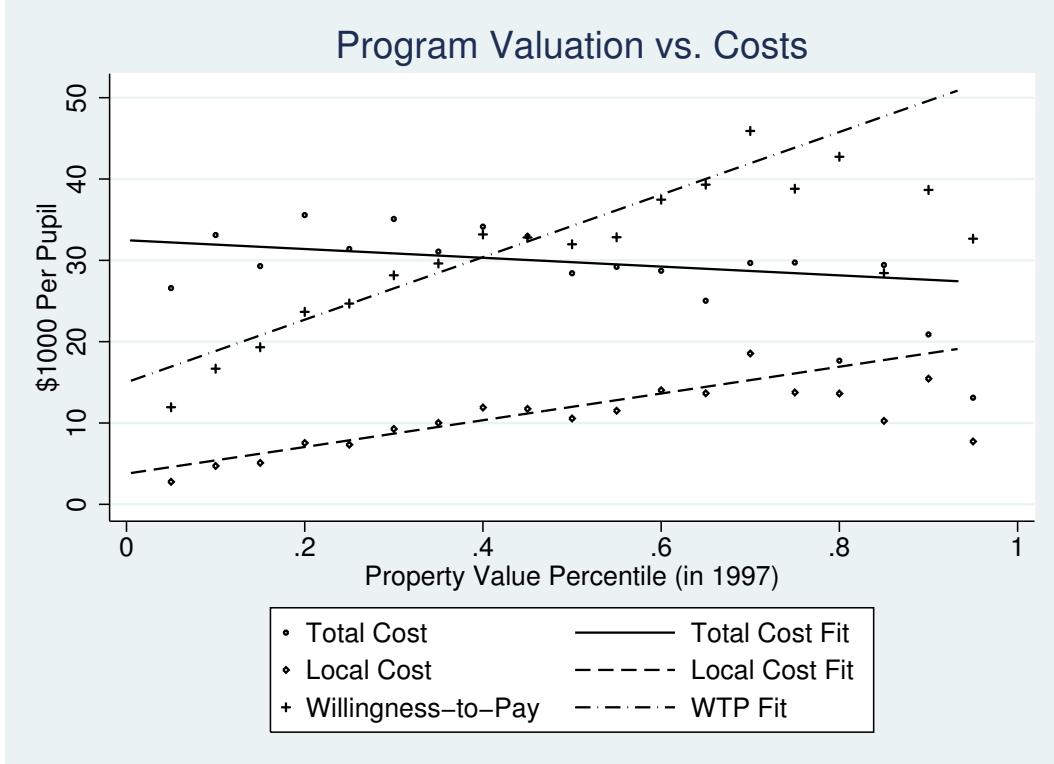
	(1) Log Price	(2) Log Price	(3) Log Price	(4) Log Price
2 yr. Before Funding	0.0129 (0.0139)	-0.00919 (0.0230)	0.00520 (0.0166)	-0.0158 (0.0231)
1 yr. Before Funding	0.0146 (0.0208)	-0.00951 (0.0310)	0.0157 (0.0265)	-0.0167 (0.0346)
Funding/Construction	0.0206 (0.0275)	0.0244 (0.0506)	0.0341 (0.0351)	0.0189 (0.0496)
0 yr. Post-Completion	0.00454 (0.0345)	0.000422 (0.0571)	0.0402 (0.0458)	0.00753 (0.0543)
1 yr. Post-Completion	0.0217 (0.0371)	0.0379 (0.0630)	0.0671 (0.0441)	0.0453 (0.0553)
2 yr. Post-Completion	0.0688 (0.0441)	0.0864 (0.0697)	0.0991 (0.0512)	0.0835 (0.0654)
3 yr. Post-Completion	0.131* (0.0525)	0.155 (0.0823)	0.156** (0.0567)	0.169* (0.0743)
4 yr. Post-Completion	0.177* (0.0692)	0.222* (0.103)	0.167* (0.0688)	0.162 (0.0961)
N	769112	190048	141899	42675
R2	0.498	0.443	0.433	0.419
Balanced Sample	No	Yes	No	Yes
Homes	All	All	≤ 2 Bedrooms	≤ 2 Bedrooms

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: regressions are on log of transacted property values and include district-specific fixed effects and time trends, eligibility-group year fixed effects, controls for property characteristics, and an indicator for 5+ years post-completion. Estimates are relative to 3+ years prior to funding. Data spans the years 1992-2014 and are collected by Corelogic from county auditors in Ohio. Standard errors are clustered at the district level, and observations are weighted by the public school enrollment of the district.

Table 8: Housing Effects



Notes: circles, diamonds, and pluses are bin averages of district level values, where bins are .05 percentile width. Data on total and local cost per pupil come from OSFC Annual Reports. Willingness to pay is calculated as in Section 6.1 and is based on costs and total property values in a district, taken from the Department of Taxation.

Figure 12: Housing Valuation Cost Benefit Analysis