# Do Charter Schools Crowd Out Private School Enrollment? Evidence from Michigan*

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**Abstract**

Charter schools have been one of the most important dimensions of recent school reform measures in the United States. Though there have been numerous studies on the effects of charter schools, these have mostly been confined to analyzing their effects on student achievement, student demographic composition, parental satisfaction, and the competitive effects on traditional public schools. This study departs from the existing literature by investigating the effect of charter schools on enrollment in private schools. To investigate this issue empirically, we focus on the state of Michigan where there was a significant spread of charter schools in the nineties. Using data on private school enrollment from biennial NCES private school surveys, and using a fixed effects as well as an instrumental variables strategy that exploits exogenous variation from Michigan charter law, we investigate the effect of charter school penetration on private school enrollment. We do not find any causal evidence that charter schools led to a decline in enrollment in the private schools. Further, we do not find evidence that enrollments in Catholic or other religious schools were affected differently from those in non-religious private schools. Our results are robust to a variety of sensitivity checks.

**Keywords:** Charters, Private Schools, Instrumental Variables

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# 1 Introduction

Since the publication of A Nation at Risk in 1983, efforts to improve public school quality have been at the forefront of national and state policy debates. Concerned over the academic achievement of U.S. students, particularly in comparison to students in other developed as well as developing countries, policy makers have proposed and implemented several reform measures. One of the most important dimensions of these school reform measures has been school choice, and charter schools in particular. Since the first charter school opened in 1991-92 in Minnesota, there has been a rapid spread of charter schools throughout the U.S. and most states now have charter schools. As of 2013, there were more than 6,000 charter schools enrolling over two million students spread across the 40 U.S. states, the District of Columbia, and Puerto Rico (National Alliance for Public Charter Schools).

This study investigates the effect of charter schools on private school enrollment in the context of Michigan. While there is a rich literature that studies the effect of charter schools, these have mostly been confined to analyzing their effects on student achievement, student demographic composition, parental satisfaction, and the competitive effects on traditional public schools. Surprisingly, the impacts of charter schools on private school enrollment have been largely overlooked. While there is a general impression that charter schools have negatively impacted private school enrollment, there is no convincing evidence either to this effect or to its contrary. However, understanding the effect of charter schools on private school enrollment is of paramount importance from various perspectives. First, private schools are an integral part of our K-12 education system, and hence it is important to understand whether and how charter schools affect private school enrollment. Moreover, any policy that affects enrollment in these schools has the potential to have significant consequences on school quality and educational outcomes of students (both public and private). Second, an important factor is the potential impact on per pupil spending in public schools. If it is indeed the case that a significant number of private school students are now transferring to charter schools (which are publicly financed) then this may reduce the amount of per pupil spending in public schools unless

total school spending increases at a corresponding rate. On the other hand, if there are more children in the public sector, this increases the number of people with stakes in the quality and performance of public schools. This in turn may lead to demands for more resources in the public sector. Thus, while the exact direction of the net effect on funding is not clear, charter school impacts on private school enrollment can surely have ramifications on per pupil funding in public schools.[1]

Also of importance is whether charter schools affect enrollment in different types of private schools differently. As President Bush said in his 2008 State of the Union address, faith-based schools are disappearing at an "alarming" rate in many of America's inner cities, and there is a general perception (Cech, *Education Week*, 2008) that charter schools are one of the major drivers of the decline in Catholic school enrollment. In spite of these perceptions, there is virtually no literature that studies the impact of charter schools on private school enrollment, either overall or separately for religious schools (including Catholic schools) and non-religious schools. This paper begins to fill this gap and takes an important step forward in this direction. In our study, in addition to investigating the impact of charter school entry on overall private school enrollment, we disaggregate the universe of private schools into Catholic schools, non-Catholic religious schools, and secular schools, analyzing whether the effects varied by the religious nature of the schools.

This paper can also speak to one of the most hotly debated questions relating to charter schools, the question of relative efficacy of charter and public schools. The existing literature typically addresses this question by comparing the achievement of public and charter school students, after controlling for their observable and unobservable characteristics. However, the effect of charter schools on private enrollment, as analyzed in this paper, can also inform this question, from the point of view of parents' perceptions (or parental valuation) of these two types of schools.

This study is the first to point out that private school enrollment patterns in the presence of charter schools contain important information relating to the relative quality of charter and public

[1] Preston (1984) argues that one of the main reasons that poverty rates among the elderly fell significantly from 1950 to 1980 despite the increase in their numbers is the fact that the increase in the number and percentage of elderly people led to a redistribution of resources towards them.

schools. The intuition is that any movement from private schools to charter schools will depend not only on the relative qualities of these two types of schools, but will also depend in an important way on the quality of the neighboring traditional public school. This is because if the new charter school is regarded as an improved alternative in comparison to the traditional public school, then some private school households (the ones marginal or close to marginal between public and private schools) will now take advantage of this and switch from private to charter schools. Note that a transfer of students from private schools to charter schools does not necessarily imply that these parents value charter schools more than private schools (as private schools are costly). On the other hand, such a transfer implies that parents prefer charter schools to the traditional public schools, since both are free. Thus the effect of charter schools on private school enrollment gives us valuable information relating to relative attractiveness of charter and public schools, as perceived by private school parents.

Note that moves of public school students to charter schools also imply that their parents value charters more than the public schools. However, there might be differences in the responsiveness of public and private families to school quality in their school choice decision. For example, Hanushek et al. (2007) found that in Texas, the parental decision to exit a charter school is significantly related to charter school quality and that the magnitude of this relationship is substantially larger than the relationship between the probability of exit and quality in the traditional public school sector. Since parents of children currently in private schools have already exercised the choice option once, they may be more sensitive to school quality, and arguably more motivated.

It should however be noted that such moves (moves of private school students to charter schools or moves of public school students to charters for that matter) do not necessarily imply that charter schools are better than public schools in terms of academic achievement. It merely implies that parents 'value' charter schools more than the traditional public schools, and these may be for reasons other than academic achievement. For example, some parents may have a preference for a particular curriculum (e.g., fine arts theme or Afro-centric theme), or the fact that charter schools might be

targeted to particular groups like special education students or at-risk students, or the fact that they may be somewhat different in nature from the traditional public or private schools (like being technical schools), etc. Of course, it is also possible that being new schools, charters initially attract some families who are shopping around for better school quality.

The empirical part of this paper investigates the effect of charter penetration on private school enrollment. We focus on the state of Michigan where there was a significant spread of charter schools in the 1990s. Data used include biennial data from private school surveys conducted by the U.S. Department of Education and charter school data obtained from the Michigan Department of Education. We use a fixed effects as well as an instrumental variables strategy that exploits exogenous variation from the Michigan charter law, following Bettinger (2005). We find no causal evidence that charter schools in Michigan impacted private school enrollment. Further, we do not find evidence that enrollments in Catholic or other religious schools were affected differently from those in non-religious private schools. Our results are robust to a series of sensitivity and falsification tests.

Our study is related to the burgeoning literature on various effects of charter schools. However, most of this literature focuses on either the effect on students enrolled in such schools [Hoxby and Rockoff (2004), Bettinger (2005), Bifulco and Ladd (2006), Sass (2006), Booker et al. (2007), Hoxby and Murarka (2007), Imberman (2011), Abdulkadiroglu et al. (2009), Abdulkadiroglu et al. (2011), Angrist et al. (2010, 2011, 2012, 2013)], or the competition effect on surrounding traditional public schools [Hoxby (2003a, 2003b), Bettinger (2005), Sass (2006) and Booker et al. (2008), Imberman (2011)], or the effects on sorting of students across public and charter schools, particularly based on racial composition (Dee and Fu, 2004). In contrast, our study analyzes the enrollment effects of charter schools on private schools.

This study is most closely related to Toma et al. (2006)—the only other paper that looks at enrollment consequences of charter schools. It also focuses on Michigan, and finds that charter schools are attracting a significant number of students from the private sector. Considering enrollments at

the county level, they find that private schools lose 0.31 student for every student gained by charter schools.

This study differs from Toma et al. (2006) in several fundamental ways. First, Toma et al. (2006) does not account for the fact that the location of charter schools might be endogenous to (unobserved components of) private and public school quality in the neighborhood, so that simple OLS or even fixed effect regressions can yield biased estimates. This paper, on the other hand, pursues an instrumental variables strategy by exploiting exogenous variation in Michigan's charter school law. Second, unlike this paper, Toma et al. (2006) does not control for pre-program differences in enrollment trends across individual private schools, nor does it allow the impact on private school enrollment to trend over time. Third, Toma et al. looks at the effect of *county-level* charter enrollment on *county-level* private enrollment. However, neither charter enrollment nor private school enrollment is restricted by county boundaries. Proximity rather than county boundaries determines choice of these schools. Therefore, in our analysis we use proximity to define the competition variable. Specifically, we estimate the effect of charter competition within certain reasonable radii of private schools on private school enrollment. Fourth, while we use data from 1989-90 through to 2001-02, Toma et al. (2006) only uses data from 1994-95 through 1998-99. The time-period considered in Toma et al. (2006) is too narrow to control for pre-program trends (the first charter schools in Michigan were established in 1994-95). Fifth, we decompose private schools into secular schools and parochial schools—and parochial schools further into Catholic schools and other religious schools—to investigate potential heterogeneity of effects. Finally, unlike Toma et al. (2006) we use several alternative ways to measure the extent of competition and employ a battery of sensitivity checks to ensure the validity of our results. Our findings, outlined above, differ significantly from Toma et al. (2006) in that we find no causal evidence of any impact of charter schools on private school enrollment.

## 2 Charter schools in Michigan

The law creating charter schools—also known as public school academies—was passed by Michigan's legislature in December 1993. It is regularly ranked as one of the strongest in the nation. For example, the Center for Education Reform, an advocacy group for school choice, ranks Michigan's law to be among the most-accommodating charter laws in the country (Center for Education Reform, 2008). Among other things, the law allows for a wide range of authorizers for charter schools, including local school boards, intermediate school boards[2], and community colleges and public universities in the state;[3] does not impose automatic collective bargaining agreements for teachers in charter schools, unless these schools have been authorized by local school districts (Mead, 2006); and allows charter schools to receive per pupil state funding at the same level as traditional public schools[4]. Still, the composition of funding differed between charter and public schools. Local funding constituted a markedly smaller proportion of charter school revenue. During the post-charter period under consideration for bulk of the paper (1996-2002), local revenue constituted 3% (18%), state revenue constituted 92% (77%), and federal revenue constituted 5% (5%) of charter (public) school budgets (Izraeli et al. (2012)). Of note is that charter school students must take the same state tests that students in traditional public schools do, and they are subject to the same accountability provisions as traditional public schools.

Figure 1 maps the distribution of private and charter schools across Michigan. As can be seen, both private and charter schools are spread across the whole state. However, most of the charter schools are located in the big cities and their suburbs. For example, Detroit and its suburbs account

[2] In Michigan, an intermediate school district is a government agency usually organized at the county or multi-county level that assists a local school district in providing programs and services. The board of an intermediate school district can authorize charter schools within its jurisdiction—however, few has done so.

[3] Public universities are allowed to authorize charter schools anywhere in the state, but other authorizers can only authorize schools within their jurisdictions.

[4] This is particularly important in Michigan as following a major school finance reform (Proposal A) which occurred around the same time, the share of state revenues in total per pupil spending rose to around 70-80 percent, unlike many other states where state funds account for a much smaller share of per pupil spending. See section 6.6 below for more on Proposal A.

for a significant number of charter schools, as do cities like Grand Rapids, Lansing and Flint. A report to the Michigan legislature estimated that in 2003-04 about 20 percent of the charter schools were located in small town or in rural areas (Michigan Department of Education, 2005). Most charter school students are in the elementary grades, followed by the middle grades.

# 3 Data

In this study, we utilize data from the Private School Surveys (PSS) conducted by the National Center for Education Statistics (NCES), an arm of the U.S. Department of Education. The PSS have been conducted biennially since 1989-90 for the purpose of collecting data on private schools in the U.S. and their teachers and students.[5] These surveys provide information on enrollments in private schools in Michigan, and are available for every other year beginning 1989-90. Since the first charter schools in Michigan opened in 1994-95, the PSS data allow us to control for differences in pre-program trends of private schools and also to test for any differential effect of charter schools on private school enrollment as the charter schools matured and expanded.

Another important feature of the PSS data is that these surveys have detailed information on the characteristics of the private schools. Of particular importance to us are several variables related to religious affiliation (whether school has a religious purpose, whether the school is Catholic, etc.)—as it is possible that religious private schools will have different enrollment effects than secular private schools. Most of the analysis in this paper uses private school data from 1989-90 through 2001-02. However, in one robustness exercise, we also use data through 2005-06 to see whether the patterns are robust to the time period of analysis. We use charter school data on location and enrollment for the period 1994-95 through 2005-06—these data are obtained from the Michigan Department of Education.

[5] The target population for this survey consists of all private schools in the U.S. that meet the NCES definition (i.e., a private school is not supported primarily by public funds, provides instruction for one or more of grades K-12 or comparable ungraded levels, and has one or more teachers). Details about the PSS, including its survey design and its components, are available at http://nces.ed.gov/surveys/pss/.

Data on demographic composition of private schools are also available from the PSS, but these data are available starting from the 1993-94 school year. For the purposes of our instrumental variables strategy, we use data on location of private schools obtained from the PSS, and data on location of Michigan public universities obtained from the Integrated Postsecondary Education Data System (IPEDS).

# 4 Empirical Strategy

We focus on the state of Michigan where there was a significant spread of charter schools in the 1990s. Michigan is an interesting state to analyze as it has several distinct advantages. First, it is one of a handful of states where charter schools spread very rapidly. This allows researchers to analyze the effect of charter schools in a setting where potential confounding factors like secular time trends are less important. Second, Michigan presents a very diverse perspective in terms of its private schools. This allows us to examine whether the effects of charter schools on enrollment are different for different types of private schools. Third, and importantly, the presence of a unique feature in Michigan's charter school law allows us to pursue an instrumental variables strategy, following Bettinger (2005).

Since charter schools are disproportionately concentrated in the elementary grades, we focus on elementary schools in this paper. We look at middle and high schools as part of some robustness checks later.

The significant spread of charter schools in Michigan is shown in Table 1. The total number of charter schools increased from 33 in 1995-96 to over 200 in 2001-02, and enrollment in charter schools increased from 4,449 in 1995-96 to over 64,000 in 2001-02. We investigate whether the spread of charter schools was associated with a fall in private school enrollment.

### 4.1 Estimation using Private School Surveys

The Private School Survey data are available for every other year, beginning in 1989-1990. Using data from 1989-90 to 2001-02,[6] we first estimate the following model. Controlling for year-specific dummy variables to absorb any common shocks, we investigate whether there was any shift in private school enrollment following charter penetration.

$$y_{it} = \alpha_0 + \sum_{k=1992}^{2002} \alpha_{1k}.D_k + \alpha_2.charter_{it} + f_i + \varepsilon_{it} \qquad \ldots (1)$$

Here $y_{it}$ denotes enrollment in private school $i$ in year $t$, while $charter_{it}$ is a measure of charter competition faced by school $i$ in year $t$. $D_k$s refer to the year dummies for the various years and control for year specific shocks, $f_i$ denotes private school fixed effects that control for time-invariant characteristics of schools.[7] The coefficient of interest here is $\alpha_2$, which represents the effect of charter competition on enrollment in private schools.

As mentioned earlier, neither charter school enrollment nor private school enrollment is restricted by county boundaries—households can cross county boundaries to go to their preferred private or charter school. Therefore, we use proximity to define the competition variable. We estimate the effect of charter competition within certain radii of private schools on private school enrollment.[8] For the purposes of our analysis, we geocode every private and charter school in the state of Michigan during our period of analysis, and compute number of charter schools and charter school enrollment within certain specified radii of each private school. For instrumental variables analysis and robustness checks later, we also geocode each public university and compute their distances from private schools.

We use three measures of charter competition: (i) an indicator variable denoting whether or

[6] Henceforth in the paper, we refer to school years by the calendar year of the spring term—example, 1990 refers to school year 1989-90, and so on.

[7] We do not include demographic controls in our main specification as the data on demographics is only available from the 1993-94 school year.

[8] Since charter schools are open to any resident, they can in principle attract students from a wide catchment area. However, in practice transportation costs are an important barrier—Kleitz et al. (2000) find that a majority of all racial and economic groups cited location as important in their choice of a charter school, with minorities and low-income households attaching significantly higher weights to this factor. So distance to the nearest charter school should be an important indicator of the competitive pressures faced by private schools.

not there was a charter school within that radius, (ii) number of charter schools within that radius of a private school, and (iii) charter school enrollment within that radius. The analysis reported here mainly pertains to 2 mile and 5 mile radii, but we have experimented with 1 mile and 10 mile radii also. The results remain qualitatively similar and hence are not reported here.[9] Since the schools are of different sizes, the regression errors are likely to be heteroscedastic. So we employ heteroscedasticity-robust standard errors in all regressions reported here. In addition, to control for serial correlation across observations for the same county, we cluster the standard errors at the county level.

#### 4.1.1 Existence of Differential Pre-Program Trends

The above specification does not control for any pre-existing trends of private schools, so the estimates of the effects of charter competition will be biased if there are differential pre-program trends. We next control for the presence of such pre-program trends in our regressions. For this purpose, we follow the strategy used in Hoxby (2003b). Based on Hoxby (2003b), we estimate a linear time trend for each private school based on only its own pre-program enrollment data and generate predicted trend values for the entire period ($trend_{it}$).[10,11] We estimate the following model.

$$y_{it} = \beta_0 + \sum_{k=1992}^{2002} \beta_{1k}.D_k + \beta_2.charter_{it} + \beta_3.trend_{it} + f_i + \varepsilon_{it} \quad \ldots (2)$$

Here $trend_{it}$ denotes school i's predicted enrollment trend, and allows us to control for pre-program differences in trends across individual private schools.

[9] While specification (1) as well as the other specifications (discussed below) are fixed effects (FE) specifications that include private school fixed effects, we also estimate OLS versions of these regressions. For brevity, we only report results from the FE regressions—the OLS results are similar and available on request.

[10] We run a separate regression for each private school using only its pre-program data, regressing enrollment on a linear time trend. We then generate predicted enrollment trend values for each private school for both the pre-charter and post-charter periods and call this variable $trend_{it}$. We next introduce $trend_{it}$ as an estimated regressor in our specifications. This controls for any differences in pre-charter trends across private schools.

[11] It is worth noting here that following Hoxby (2003b) we use the predicted trend strategy because the alternative strategy of including explicit interactions between time trend and individual private school dummies requires much more statistical and computing power, and computing time. While the results from such an analysis are qualitatively similar, they are sometimes less precise and have lower goodness of fit values. These results are available on request.

Our most preferred specification is specification (3) below, where we estimate a variant of model (2). Here we interact the charter competition variable with time trend to allow for the fact that the effect of charter schools may be trending over time.

$$y_{it} = \gamma_0 + \sum_{k=1992}^{2002} \gamma_{1k}.D_k + \gamma_2.charter_{it} + \gamma_3.charter_{it} * trend + \gamma_4.trend_{it} + f_i + \varepsilon_{it} \qquad ... \ (3)$$

Here the variable *trend* denotes time trend; it takes a value of zero in 1994, the last year before the introduction of charter schools. Recall that we have biennial data—our trend variable increments by 1 biennially in the post-program period and decrements by 1 biennially in the pre-program period.[12] The coefficients of interest here are $\gamma_2$ and $\gamma_3$, respectively denoting any intercept and trend shifts in private school enrollment following charter competition. These together represent the effect of charter competition. In particular, $\gamma_3$ shows whether the effect of competition from charter schools had any significant trend component, that is whether the effect of charter competition (on private school enrollment) changed over time. It is possible that with time there was better information dissemination. As parents come to know about charter schools, they may have wanted (or not) to enroll their children in these schools.

#### 4.1.2 Allowing for Heterogeneity in effects of Charter Competition

As noted earlier, the PSS data contain information on religious affiliations of private schools. Since it is possible that charter school competition might affect secular private school enrollment differently from religious private school enrollment, we check for heterogeneity in effects across different types of private schools. We estimate the following model to see if enrollment in religious private schools showed different patterns than those in secular private schools.

$$y_{it} = \delta_0 + \sum_{k=1992}^{2002} \delta_{1k}.D_k + \delta_2.charter_{it} + \delta_3.charter_{it} * trend + \delta_4.(charter_{it} * religious\ purpose)$$
$$+\delta_5.trend_{it} + f_i + \varepsilon_{it} \qquad ... \ (4)$$

The variable *religious purpose* is a 0-1 binary variable, taking the value of 1 if the private school in question has a religious purpose, 0 otherwise. Note that *religious purpose* separately is not

[12] Specifically, trend equals -1 in 1992, -2 in 1990, 0 in 1994, 1 in 1996, 2 in 1998, 3 in 2000, and 4 in 2002.

included in the above regression as it is absorbed by private school fixed effects. A statistically significant coefficient for ($charter_{it} * religious\ purpose$) would imply that competition from charter schools had a different effect on religious private schools compared to secular ones.

Since many of the private schools in the U.S. have traditionally been Catholic, we also estimate a variant of specification (4) where we disaggregate all schools into three types—Catholic, other religious, and those without a religious affiliation. We include separate dummies for Catholic schools and schools affiliated with other religious organizations, secular private schools serve as the omitted category.

#### 4.1.3 Addressing Concerns about endogenous Locations of Charter Schools

One important concern here is that charter school location is likely to be endogenous—there might be unobserved characteristics that affect both private school quality and charter school location. Charter schools may open in areas where the private schools are not very good—alternately, they may open in districts where there is a high demand for good quality schooling and existing schools are already quite good.[13] Since our specification above includes private school fixed effects, any school or neighborhood-specific characteristic that does not vary over time will be absorbed and not bias the results. However, there might be time-varying features that affect both charter school location and enrollment in private schools. To address this issue, we use an instrumental variables estimation strategy that also includes private school fixed effects.

Our choice of instrument follows Bettinger (2005)—this study investigates the effect of charter competition in Michigan on student performance in public schools. One interesting and unique feature of Michigan's charter law is that public universities are allowed to grant charters. Notably, universities whose boards are appointed by the governor authorized a lot of charter schools (more than 85 percent) during this period (1995-2002) to please the then governor, John Engler, who was

[13] There are a few studies that look at the location decisions of charter schools. Glomm et al. (2005) argues that charter schools are more likely to locate in districts whose populations are more diverse—they find that school districts with more diverse populations in terms of race and adult education are more likely to attract charter schools.

a big supporter of charter schools. In contrast, the other public universities did not authorize any charter school. The list of universities where the governor appoints board members is set forth in the Michigan Constitution (Michigan Constitution, Article 8, sections 5 and 6). The boards of the other public universities are elected by the general public in statewide elections. In other words, the decision to appoint boards at some universities and not others is historically determined, and is not dependent on current economic, social, political or educational trends. Since the authorizers had to supervise and monitor the charter schools, charter schools were more likely to be set up closer to these public universities than farther. Bettinger (2005) exploits this exogenous variation created by the charter law—he uses as his instrument for charter school location the distance of public schools from public universities that had governor appointed boards.

Our study also exploits this exogenous variation created by the charter law, however there are some differences with Bettinger (2005). First, since we are interested in the effect of charter school competition on private school enrollment we use distance of private schools from public universities that had governor appointed boards ("authorizing universities" from now on). More specifically, we compute the distance of each private school from the various authorizing universities and use the minimum of these distances ("minimum distance"). There were fifteen public universities in Michigan during the period under consideration—ten were authorizing universities and five were "non-authorizing" universities (universities whose boards were not appointed by the governor). The authorizing and non-authorizing universities are mapped in Figure 2. As can be seen, both authorizing and non-authorizing universities were concentrated in the southern part of Michigan. (For more discussion on authorizing and non-authorizing universities and demographic characteristics of areas near authorizing and non-authorizing universities, see section 6.2.) The distribution of "minimum distance" is graphed in Figure 3—the mean minimum distance is 24.25 miles; the median is 19.21 miles; the 25th and 75th percentiles respectively are 12.34 miles and 30.37 miles.

Second, unlike the cross-sectional nature of the Bettinger study, this study is a panel data analysis and we need an instrument that provides exogenous variation over time. We use as instruments

the interactions of minimum distance with post-program year indicator variables. As a robustness check, we also use a related but alternate set of instruments where the instruments are interactions of minimum distance with program dummy, and program dummy and trend respectively. We focus on results from the first set of instruments, results from the second set of instruments are similar and are available on request. In addition, we consider the prospect that the location of public universities may not be exogenous. Most public universities have been set up decades before and their locations were historically determined, so this may not be a big problem. But as a robustness check we carry out falsification tests exploiting the location of public universities that do not have governor appointed boards. We also carry out falsification tests using pre-program data and institutional details of charter school law. We discuss results from these falsification tests in section 6.1.

# 5 Results

## 5.1 Results from Fixed Effects Analysis

Table 2 shows results from estimating specifications (1)-(2). The first six columns (marked (1)-(6)) are from regressions where we use charter school competition within 2 miles of a private school as our main variable of interest, while columns (7)-(12) are from regressions where we use 5 miles as the radius. Columns (1)-(3) and columns (7)-(9) report results from estimation of specification (1) and include charter competition as an intercept term. The results from this basic "first pass" specification show a significant decline in private school enrollment following the spread of charter schools. Columns (4)-(6) and columns (10)-(12) control for pre-existing trends of individual private schools, in addition, using the strategy described in section 4.1.1. The results from this specification also show statistically and economically significant declines in private school enrollment, but these effects are more moderate than those from specification (1). The presence of charter schools within 2 miles of a private school leads to a fall in private school enrollment by 14 students (column (4)), whereas a private school loses about 4 students for *each* charter school within a 2-mile radius (column (5)). An additional charter school student within its 2 mile radius reduces private school enrollment

by 0.01 students (column (6)). The effects of charter schools are much attenuated when we consider charter competition within a larger 5-mile radius as opposed to a 2-mile radius. This is expected—the presence of the same number of charter schools within a 2-mile radius should generate much more opportunities to transfer out of private schools (assuming that the charter schools are more or less evenly spread out). This general trend is repeated in all the specifications we estimate—while qualitatively similar, the effects from charter competition are economically larger when we consider a smaller distance (1 mile, 2 miles) around private schools rather than a larger distance (5 miles, 10 miles). For brevity, most results report estimates from regressions using 2 miles as the radius—the other results are available on request.

Table 3 shows results from estimation of our preferred specification (3). Here we continue to allow each private school to have a separate pre-program trend, and as in the regressions above, we also include individual year dummies to control for year specific shocks. In addition, to see whether the effect of charter schools might be trending over time we interact the charter competition variable in each regression with a linear time trend.

The results in Table 3 columns (1)-(3) pertain to 2 mile radius, while the columns (4)-(6) pertain to 5 mile radius. First, consider the results for 2-mile radius. The results from using charter presence as the competition measure shows a positive intercept shift, though it is statistically not significant. This is associated with an economically larger and statistically significant negative trend shift. The results from using number of charter schools as the competition measure shows a negative and statistically significant intercept shift and a smaller (statistically insignificant) negative trend shift. When the competition measure is charter enrollment, the estimation shows a negative statistically insignificant intercept shift along with a smaller positive insignificant trend shift. The results for the 5-mile radius are qualitatively similar, although they are more muted. As the table shows, the treatment effects are jointly significant, except in column (4).

Since there are two treatment effects—an intercept shift and a trend shift—it is instructive to look at the total treatment effect. When the competition measure is "number of charters", the results

(Table 3 Column 2) suggest that an additional charter school within a two mile radius decreases private school enrollment by 4.29 students or 2.75% after 7 years, and this effect is statistically significant. When the competition measure is "charter enrollment", an additional charter school student decreases private school enrollment by 0.006 student or 0.004% after 7 years (Table 3 Column 3), however this effect is not statistically significant. For space constraints, we only report p-values corresponding to the F-test of statistical significance of the total treatment effect for $trend = 1$ in the various tables in this paper. However, we discuss the total treatment effects for $trend \in \{1, 2, 3, 4\}$ (that is, one year, three years, five years, seven years after) in the text. P-values corresponding to $trend = 1$ that is, one year after, reveal that the total treatment effects are statistically different from zero in 50% of the regressions in Table 3 (three columns out of six). When we individually consider the regressions where the total treatment effects are not statistically significant in the first year, in only one of these regressions (2-mile charter presence measure) does the total treatment effect become statistically different from zero after 7 years - the total treatment effect never becomes statistically different from zero in the other two regressions. Similarly, while the total treatment effect when the charter competition measure is charter enrollment within 5 mile radius is statistically significant in the first year, it becomes statistically insignificant by the end of our sample period. To summarize, while the fixed effects estimations for specification (3) show some evidence of a negative impact of charter competition on private school enrollment, this evidence is neither consistent nor robust. It remains to be seen what our preferred instrumental variables estimations (which yield causal estimates) will reveal.

Table 4 investigates whether the effect of charter competition differed for the religious private schools. We estimate specification (4), controlling as earlier for pre-existing trends of individual private schools and include year dummies to control for any common shocks. In columns (1)-(3) we use the *religious purpose* variable, which takes the value of 1 if the school in question has a religious purpose, 0 otherwise. In columns (4)-(6) we employ the three-way classification of private schools and include dummies for Catholic schools and other religious schools (secular private schools being

the omitted category). While the effect on the non-religious private schools remain similar to those in Table 3, the results do not provide much evidence that competition from charter schools had a bigger negative effect on religious private schools. While the interaction terms between competition variable and different measures of religiosity are in most cases negative (and often economically meaningful), they are never statistically significant. Also at the risk of foreseeing later results, the coefficients on these interaction terms are not even negative in the instrumental variables results and they are never statistically significant.

### 5.2 Results from Instrumental Variables Strategy

Table 5 reports the results from our instrumental variables estimation strategy. These regressions continue to include private school fixed effects, while instrumenting for charter school competition. Recall that we employ two alternate sets of instruments: (i) interactions of minimum distance with post-program year dummies and (ii) interactions of minimum distance with program dummy, and program dummy and trend respectively. Since the results are similar, we only report results from the first set of instruments—the other results are available on request.

The first stage results (not reported here, but available on request) show that for each of the endogenous regressors, the instruments are highly significant and always have the expected sign. Also each of the first stage regressions passes the Angrist-Pischke multivariate test of excluded instruments, as seen from the corresponding p-values in Table 5.[14]

For brevity, Table 5 (and tables moving forward) report results from two competition measures—number of charter schools and enrollment in charter schools. The results for the other (relatively coarse) competition measure—presence of charter schools—are qualitatively similar and available on request. Columns (1)-(2) pool all private schools together, while columns (3)-(6) distinguish between religious and non-religious schools. Columns (3)-(4) use *religious purpose* as the measure of religiosity while columns (5)-(6) use the three-way classification distinguishing between Catholic schools,

[14] Because the R-squared has no natural interpretation in an instrumental variables context, we do not report them corresponding to our instrumental variables regressions. See Wooldridge (2008) for further explanation.

religious schools, and non-religious schools (the latter serving as the omitted category). Impact estimates from both competition measures show a positive intercept shift along with a smaller negative trend shift. But these effects are not statistically significant; more importantly, the treatment effects are never jointly significant.

Given that the total treatment effect is a combination of intercept and trend shifts, it is more instructive to study the total treatment effect (rather than the shifts separately). Figure 4 presents the total treatment effects and corresponding 95% confidence intervals during the various years in our period of consideration ($trend \in \{1, 2, 3, 4\}$, that is, 1 year, 3 years, 5 years, and 7 years after program). The left panel presents total treatment effects for the competition measure "number of charter schools", while the right panel presents these when the competition measure is "charter school enrollment".

As can be seen from Figure 4, the point estimates for the total treatment effects are economically small and they are never statistically different from zero during the period of our analysis (1 year, 3 years, 5 years or 7 years).[15] It is important to note that the confidence intervals around the point estimates (of total treatment effects) are quite tight for the various years under consideration and for both the competition measures. For example, when the measure of charter school competition is number of charter schools, an additional charter school leads to an increase of 1.51 private school students after 7 years with a confidence interval of [-4.14, 7.16][16]. Given an average private school enrollment of 156 students during our study period, this is equivalent to a percentage effect of 0.97% with confidence interval [-2.65%, 4.59%]. In other words, we can rule out total treatment effects larger than 4.59% and less than -2.65% after 7 years with an addition of a charter school.

When the measure of competition is charter enrollment, the results imply that an additional charter school student leads to an increase of 0.003 private school student after 7 years. The confidence interval around this point estimate is [-0.012, 0.018]. This is equivalent to a percentage effect

[15] For lack of space, we only report p-values for Chi-squared tests of statistical significance of total treatment effect when $trend = 1$ in Table 5.

[16] All confidence intervals reported in this paper are 95% confidence intervals.

of 0.002% with confidence interval [-0.008%, 0.012%]. This means that after 7 years we can rule out total treatment effects larger than 0.012% and smaller than -0.008% with a unit increase in charter enrollment. Thus, the total treatment effects are economically small and never statistically different from zero; the estimates are precise enough to rule out modest to large sized effects.

It might be worthwhile to compare the overall treatment effects obtained from the fixed effects (FE) analysis to those obtained from the instrumental variables (FE-IV) results. We limit our discussion here to comparisons where we pool all private schools together, and do not distinguish between types of private schools[17]—that is we compare total treatment effects from FE-IV regressions in columns (1)-(2) in Table 5 to the total treatment effects from the corresponding FE regressions in Table 3. When the competition measure is number of charters, the FE results imply a total treatment effect of a decline of 4.29 students or 2.75% after 7 years, and this effect is statistically significant. In contrast, the FE-IV results imply a total treatment effect of an increase of 1.51 students or 0.97% after 7 years, and this effect is not statistically significant. When the competition measure is charter enrollment, the FE results imply a total treatment effect of a decline of 0.006 student or 0.004% after 7 years, and this effect is not statistically significant. On the other hand, the FE-IV results imply a total treatment effect of an increase of 0.003 student or 0.002% after 7 years, and this effect is not statistically significant.

Thus, while the FE results provide some evidence of negative impacts on private school enrollment, this evidence is not consistent, and the effects are also economically small. In contrast, the FE-IV results consistently suggest a zero effect and the effects are precise enough to rule out modest to large effects. The differences in results between the two estimation strategies suggest that the FE results are likely biased. The patterns of the results suggest that charter schools tended to locate in areas that were losing private school enrollment for reasons unrelated to charter schools, and this was being captured by the FE results. It is worth reminding the reader here though that while the FE results seem to suggest a negative effect, this effect is not robust.

[17] Results from the other comparisons are similar.

# 6 Robustness Checks

## 6.1 Falsification tests: Testing validity of using distance from public authorizing universities as an instrument

Recall that our instrument is based on minimum distance of private school from public authorizing universities. One concern here is the fact that these authorizing universities may be located in areas which differ from other areas in characteristics unobservable to the researcher. Though we include school fixed effects in our regressions which absorb all time-invariant characteristics of private schools (and their neighborhoods), there may be time trends in these areas around the time charter schools were set up which in turn may bias our results.

Note, however, that these public universities have been established decades earlier and their locations are thus historically determined. So this is unlikely to introduce bias in our regressions. But we carry out falsification tests using pre-program data to check whether in the immediate pre-charter period areas which were closer to the authorizing universities witnessed any differential growth in private school enrollment. That is, using data from 1989-90 to 1993-94 we regress private school enrollment on year dummies, private school fixed effects, minimum distance, and the interaction of minimum distance with time trend.[18] The intuition is that if indeed distance from these public universities affects private school enrollment only through its effect via charter schools, then we should not expect any significant effect in the pre-charter period. To check robustness, we also estimate alternate specifications where we replace interactions of minimum distance with trend by (i) interaction of minimum distance with 1994 dummy and (ii) interactions of minimum distance with 1992 and 1994 dummies respectively.

The results are in Table 6. There is no evidence of any differential change in enrollment in private schools in the pre-charter period that varies with the minimum distance from authorizing universities. The coefficients are small and statistically not significant, suggesting that such changes were nonexistent or small and unlikely to bias our results.

[18] Note that the variable minimum distance is absorbed by fixed effects.

We also conduct another falsification test to examine the validity of our instrument. Here we use the locations of public universities that do not have governor appointed boards (or non-authorizing universities). Recall that our identification strategy for the instrumental variables estimation exploits the fact that public universities in Michigan are allowed to grant charters, and universities whose boards are appointed by the governor authorized more than 85 percent of the charter schools during this period (1995-2002), while the other public universities did not authorize any. It might be argued that places which are host to colleges and universities in general are different from other places, particularly in terms of the demographic and socioeconomic composition of their populations, and this may affect both the location decisions of charter schools as well as private school enrollment. However, this effect is unlikely to be different across authorizing public universities and non-authorizing public universities.[19] Therefore, we use the distance from non-authorizing public universities as a robustness check and investigate whether distance from non-authorizing universities also had a similar effect on private school enrollment during our period of analysis. Note that if indeed charter schools are driving our results and not characteristics of areas around universities, then we should not expect distances from non-authorizing universities to have any effect on private school enrollment.

The results are in Table 7. The presence of non-authorizing public universities does not seem to have had any effect on private school enrollments during this period. Though some of the coefficients are negative, they are never statistically significant and are economically small.

Another point is worth noting here. University of Michigan Ann Arbor, and Michigan State University fall in the set of non-authorizing universities (see Figure 2). Since these two universities are arguably different from the other public universities, we repeat this exercise after dropping these two universities. The results are similar to above and are available on request.

[19] As stipulated in Michigan Constitution (Michigan Constitution, Article 8, sections 5 and 6), the governor appoints board members of only some of the public universities in Michigan, while the boards of the other public universities are elected by the public in statewide elections. For more details, see section 4.1.3.

### 6.2 Exploring the Exclusion Restriction Further: Are School Districts Located Close to Authorizing Universities Different?

In this subsection, we further explore the validity of the exclusion restriction assumption. Specifically, it is important to investigate whether school districts located close to the authorizing universities are different from those located farther away. We investigate this in three alternative ways. Recall that our instrument is based on minimum distance of private schools from the authorizing universities. Our first strategy compares the demographic characteristics of school districts within median minimum distance of authorizer (school districts located "close" to authorizers) to demographic characteristics of school districts between median and 2∗median distance of authorizers (school districts located "far" from authorizers).

Our second strategy compares the demographic characteristics of school districts within the same county of the authorizer to those in school districts located in neighboring counties. Third, we compare demographic characteristics of school districts within median minimum distance of authorizer to demographic characteristics of school districts within median minimum distance of non-authorizers.

The results from these three sets of comparisons are reported in the three panels of Table 8 respectively. In each panel, columns (3) and (4) respectively report the means of the "close" and "far" school districts, as defined above. There is no evidence that the demographic characteristics of the "close" districts were statistically different from those in the "far" school districts, and they are also economically similar. This gives us further confidence in the validity of our instrument.

For easier visualization, we also represent these comparisons graphically. Appendix Figures A1-A3 map the distribution of demographic characteristics across school districts in Michigan and superimpose the authorizing and non-authorizing universities in the maps. Also represented by blue and black circles respectively are the median and 2*median minimum distance from the authorizers. Figures A1-A3 respectively map the distribution of % white, % black, and % Hispanic. In each case, the demographic characteristics of districts "close" to the authorizers are similar to those in "far" school districts, and are in general representative of Michigan. The maps also show that demographic

characteristics close to authorizing universities are similar to those close to the non-authorizing universities.

In Table 9, we report the characteristics of the authorizing and non-authorizing public universities in Michigan. Apart from percent black, none of the other characteristics are statistically different between these two groups of universities. Also of note here is that with a large number of comparisons, one expects a few to be statistically different from zero by sheer random variation. It is worth noting here that the more important comparison is really the comparison of the demographic characteristics of the school districts that are close, versus those that are not. This is because school district demographics more appropriately represent the demographics of the school going population rather than university characteristics. And, as seen above demographic characteristics of districts close and those that are not are well balanced.

### 6.3 Are the Results Robust to Dropping Rural Areas?

One might argue that the areas farthest from the universities are more likely to be rural, and it would be inappropriate for the instrumental variables strategy to implicitly use rural areas as a control group for urban areas. Also of importance is that the appropriate radius for perceptible competition likely differs between rural and urban areas.

To address these issues, we investigate whether our results are robust to dropping rural areas. Table 10 presents results of this analysis. Columns (1)-(2) present results from estimating specification (3) using private school fixed effects, while columns (3)-(4) use instrumental variables (FE-IV) strategy to estimate specification (3). As can be seen, both the FE and FE-IV results are similar to the results obtained above for the whole sample (Tables 3 and 5). This analysis gives us confidence in our results and affirm that our results above are not driven by rural areas.

### 6.4 Is Private School Size Driving the Results?

Within Michigan's private school sector (as in most states), schools differ noticeably in size. Figure 5 shows the distribution of private school enrollment in Michigan during our period of analysis—the mean enrollment is 156 students; the median is 110 students; the 25th and 75th percentiles respectively are 40 and 214 students. In this section we investigate whether our results above are driven by differences in private school size. For this purpose, we use log private school enrollment as the dependent variable, and investigate whether the results above are sensitive to the rescaling of our dependent variable.

The results from this analysis are presented in Table 11. Using log private school enrollment as the dependent variable, the first two columns present results from estimation of specification (3) using private school fixed effects, while columns (3)-(4) present results from estimating the same specification using our instrumental variables (FE-IV) strategy. The results remain qualitatively similar to above. Also of note here is that the treatment effects are not jointly significant in any of the columns in Table 11. In sum, the results confirm our previous finding that charter schools in Michigan did not have any meaningful effect on private school enrollment.

### 6.5 Are the Results Robust to a More Flexible Functional Form of the Instrument?

To investigate the robustness of our instrumental variables results to the functional form of the instrument, in this section we allow the instrument (minimum distance from the authorizer) to take a quadratic form. The purpose is to assess the sensitivity of our findings to changes in the functional form of the instrument. The results from this analysis are presented in Table 12. They remain very similar to those obtained above (Table 5), and confirm the robustness of our findings.

### 6.6 Are the estimates biased by the effects of school finance reform (Proposal A) in Michigan?

One factor that might bias the above results is the school finance reform enacted in Michigan during this time. In 1994, Michigan embarked on a comprehensive overhaul of its school finance program when it enacted a new plan called Proposal A. It significantly increased the state share of K-12 spending in all Michigan school districts. It also entailed giving large sums of money to the lowest spending districts, which were allowed to increase their spending at a much faster rate than others. Concurrently, Proposal A also ended local discretion over school spending. It is the state that now decides the amounts by which each school district can raise its expenditures, based on a formula.

However, Proposal A is unlikely to bias our results for several reasons. First, the districts most affected by Proposal A were the ones that were spending at relatively low levels before the reform—however, these are not the districts which witnessed the rapid spread of charter schools. As Roy (2011) shows, these lowest spending districts are predominantly rural, while charter schools in Michigan mostly serve urban and suburban children and are located in the higher spending districts.[20] In regressions not reported here, we reran our regressions excluding school districts in the lowest spending quintile—the results are qualitatively similar.[21] Second, though it is possible for a school finance

[20] As noted earlier, in 2003-04 only about 20 percent of the charter schools were located in small town or in rural areas (Michigan Department of Education, 2005).

[21] Some observers argue that school finance reforms, and the associated equalization of per pupil spending, can directly affect private school enrollment. Individual households that value education more may be less able to use Tiebout sorting to achieve their preferred spending levels and thus may have a greater incentive to exit the public system. However, Nechyba (2003) argues that there are two additional effects that may counter this incentive—first, such equalization of spending may improve public school quality in previously low-spending districts, and second, private school attendees who previously chose to live in poor districts under local public school financing in order to take advantage of depressed housing values and lower property tax payments lose both these incentives under a move to centralized public school financing. The empirical evidence on this is mixed—Downes and Greenstein (1996) find that California experienced a sizeable growth in the number of private schools after its school finance reform while Sonstelie et al. (2000) argue that the move from local to state finance had little impact on private school enrollments in California. Epple and Ferreyra (2008) also argue that the school finance reform in Michigan would have little impact on the allocation of students between public and private schools. Chakrabarti and Roy (2015) does not find evidence of any impact of the Michigan school finance reform on private school enrollment. Since in Michigan, districts most affected by Proposal A were generally not the same ones most affected by charter penetration, it is unlikely that the above mechanisms played any significant role here.

reform to have broader effects, particularly on socieoeconomic stratification/segregation, studies of Proposal A in Michigan typically find that such effects were modest or small (Epple and Ferreyra (2008), Ferreyra (2009), Chakrabarti and Roy (2015)).

Third, one way to separate out the effect of charter competition from the effect of Proposal A is to exploit the fact that while charter competition is more likely to impact elementary schools (due to the fact that charter schools predominantly serve elementary grades in Michigan), the effect of Proposal A should be more general and affect all grades. In keeping with this intuition, we next examine the impact of charter competition on enrollment in middle and high schools.[22]

Table 13 shows results when the sample is restricted to middle and high schools. The results show that there is not much evidence of an effect of charter competition on private school enrollment in middle and high schools. All of the estimated effects are small, often do not have the right sign and never statistically significant. This result suggests that our findings are not driven by school finance reform, since (unlike charter schools) school finance reform affected all schools similarly.

Finally, our instrumental variables strategy also serves to address issues relating to confounding effects from school finance reform, as long as the incidence of low-spending districts—who were most affected by the reform—is not correlated with the minimum distance from the authorizer. The robustness checks of the instruments (section 6.1) show that this was indeed the case.

### 6.7 Inter-district Choice Program

There is also a small inter-district choice program in Michigan, which was introduced around the same time as charter schools and Proposal A. This program allows students to transfer across schools and school districts, but only within the Intermediate School District (ISD) boundaries which are roughly the same as the county boundaries. However, this program was quite small during the period under consideration here—most districts had either opted out or refused to accept transfer students or set strict limits on the number of transfer students and the grades in which they would

[22] We would like to thank Julie Cullen for suggesting this strategy.

be accepted. For example, in 1997-98, only 0.68 percent of all students in Michigan had used this inter-district choice option (Plank and Sykes, 1999).

The presence of this inter-district choice program would have biased our results only if it were true that the areas which witnessed growth of charter schools were also the areas where this particular choice program was most popular. In such a case, we might erroneously attribute to charter schools transfers of students from private schools when in fact these students are leaving private schools to attend public schools in neighboring districts. First, our instrumental variable technique ensures that the patterns we obtained above were not confounded with the effect of inter-district choice. As long as *the propensity to move out of private schools to attend the choice program is not correlated with the minimum distance from the authorizer*, our results from the instrumental variables approach will not be conflated with the effects of inter-district choice program. The results in section 6.1 where we conduct falsification tests on the validity of the instrument suggest that this is indeed the case. Second, public school choice has been mainly concentrated in and around Detroit.[23] To investigate whether the existence of this choice program affected our results, we re-estimate the regressions above after omitting the counties that fall within the Detroit Metropolitan Area. The results remain qualitatively similar—they are not reported here to save space, but are available on request.

### 6.8 Decline of the auto and manufacturing industry in Michigan

A related concern is the secular decline in auto and manufacturing industries in Michigan throughout the last two and a half decades which may have led to demographic shifts and thus biased our results. First, note that our instrumental variables approach ensures that this should not be the case. The instrument serves to isolate exogenous variation pertaining to charter school location. As long as the minimum distance from authorizing universities is not correlated with changes in the manufacturing sector, our instrumental variables strategy should be able to extricate the causal impact of charter competition. The falsification tests (section 6.1) provide assurance relating to the validity of the

[23] See, for example, Cullen and Loeb (2004, page 242): "Student participation in schools of choice has largely been a Detroit phenomenon, with more than one-third of all transfers taking place within the Detroit metropolitan area."

instrument. Second, using the decennial census data we look at the changes in the percentage of workforce employed in manufacturing. The results (not reported here for space constraints, but available on request) do not suggest differential movement across Michigan school districts during this period. Third, Wayne county, which included Detroit, is a hub of manufacturing activity in Michigan. To investigate whether our analysis is being driven by the decline of the auto and manufacturing industry, we repeat our analysis above after excluding Wayne county. Our results (not reported here, but available on request) remain very similar.

### 6.9 Are the results robust to a longer time period?

In this section, we extend our analysis through the 2005-06 school year. The purpose is to investigate whether the results above were an artifact of the time period considered, and whether the patterns survive when we extend our time period of analysis.[24] The results from this extended time period are reported in Table 14. The first two columns report results from fixed effects estimation of specification (3) using data from the longer time period; columns (3)-(4) report results from instrumental variables (FE-IV) estimation of specification (3). The results closely mirror the patterns obtained above. While the FE regressions provide some evidence of a negative effect on private school enrollment, our preferred FE-IV specifications do not find evidence of causal impact of charter schools on private school enrollment. The treatment effects are jointly significant for the FE regressions, however they are not so for the FE-IV regressions (which provide the causal estimates). The results from the FE-IV estimation of specification (3) imply a total treatment effect of -1.26 students at the end of the longer period, that is after 11 years, when the charter competition measure is number of charter schools. Further, it implies a total treatment effect of -0.01 student after 11 years when the charter competition measure is charter enrollment. These effects are economically very small (and precisely

[24] Note that the implementation of No Child Left Behind (NCLB) should not bias our results here, as AYP-failed school zones (zones of schools that failed "Adequate Yearly Progress" and hence faced NCLB sanctions/stigma) do not coincide with areas corresponding to private schools that faced more charter school concentration. Also, it is worthy of note here that our instrumental variables strategy rules out any such bias. As long as the propensity of schools to face sanctions/stigma under NCLB is not correlated with the minimum distance from the authorizing universities, our estimates from the longer time period should not be affected by NCLB.

estimated) and none of these effects are statistically different from zero. Thus, once again we do not find any causal evidence that charter schools led to a decline in private school enrollment in Michigan.

# 7 Conclusion

In this study, we examine the impact of charter schools on private school enrollment. We do not find any causal evidence that charter schools in Michigan led to declines in private school enrollment. In addition, we do not find evidence that charter schools affected Catholic or other religious school enrollment differently. The results are robust to employing alternate measures of competition, controlling for individual pre-charter trends of the private schools, instrumenting for charter school location (using exogenous variation from Michigan charter law) and a variety of other sensitivity and falsification checks.

It is interesting to observe here that these results go against general public perception (often reflected in the media) that charter schools are draining private school enrollment, and especially Catholic school enrollment. For example, Sarah Scheitzer notes, "Nationwide, about 10% of the charter school student population came from private schools in 2000" (Scheitzer, Sarah, St. Petersburg Times, 2000). Reverend Ronald J. Nuzzi, the director of the Alliance for Catholic Education leadership program at the University of Notre Dame, called charters "one of the biggest threats to Catholic schools in the inner city, hands, down" (Scott Cech, *Education Week*, 2008). The reason behind this apparent inconsistency may be the quality of charter schools. It is possible that the charter schools in Michigan were not good enough or did not appeal to the private school parents enough. This hypothesis is consistent with the findings of Bettinger (2005) where he finds that test scores of charter school students in Michigan did not improve and may have actually fallen relative to their counterparts in public schools.

Still another reason may be that raw enrollment numbers (as is often cited in the media) do not always tell the whole story. Enrollment levels in charter and private schools do not reflect

causal trends. These numbers may be caused (or affected) by pre-existing trends in private school enrollment, regular mobility of students between schools, and state or district wide policies or events. Furthermore, the location of charter schools is not random. These factors are not accounted for when simply looking at the raw enrollment numbers. Similarly, the decline in Catholic school enrollment (often reported in the media) does not necessarily imply that charter schools caused the change. The decline in Catholic school enrollment could be a part of a larger trend—the result of recent tuition increases in Catholic schools or recent scandals in the Catholic Church. As Reverend Nuzzi noted, "Catholic school tuition, once very low, has had to increase over the years, and now runs thousands of dollars a year". "The increased tuition", he said,"is needed to pay for lay teachers, who earn salaries much closer to public school teachers than the pay provided the teaching nuns of decades past" (Scott Cech, *Education Week*, 2008). Catholic schools are often seen as an attractive alternative to secular private schools because of their lower tuition costs, so the recent increases are likely to make Catholic schools less desirable. Rigorous empirical analysis aimed at ruling out other potential confounding factors done in this paper reveals that at least in Michigan charter schools did not lead to a relative decline in Catholic school enrollment.

It might be worth discussing the implications of the above findings for the relative attractiveness of public and charter schools, as perceived by private school parents. Our causal analysis above finds no evidence that charter school entry led to declines (or any impact for that matter) on private school enrollment. It is comforting to see that parental valuations matched well with the objective relative measures of quality found in Bettinger (2005). To summarize, the findings of this paper suggest that, at least in Michigan, the private school parents did not perceive charter schools as an improved alternative over the traditional public schools.

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**Table 1: Spread of Charter Schools in Michigan**

(1995-96 to 2001-02)

| Year | Number of Charter Schools | Total Enrollment in Charter Schools |
|---|---|---|
| 1995-96 | 33 | 4,449 |
| 1997-98 | 108 | 21,175 |
| 1999-00 | 176 | 46,833 |
| 2001-02 | 202 | 64,103 |

Source: Authors' calculations from Bulletin 1014's issued by the Michigan Department of Education.

**Table 2: Effect of Charter Competition on Private School Enrollment**

(Using PSS data, 1989-90 to 2001-02)

| | Dependent Variable: Enrollment | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 2 Mile Radius | | | | | | 5 Mile Radius | | | | | |
| | FE (1) | FE (2) | FE (3) | FE (4) | FE (5) | FE (6) | FE (7) | FE (8) | FE (9) | FE (10) | FE (11) | FE (12) |
| Charter Presence | -18.16*** | | | -14.31*** | | | -6.02 | | | -3.83 | | |
| | (5.16) | | | (3.24) | | | (4.41) | | | (2.83) | | |
| Number of Charter Schools | | -5.66*** | | | -3.87*** | | | -1.62*** | | | -1.30*** | |
| | | (1.29) | | | (1.29) | | | (0.42) | | | (0.37) | |
| Charter Enrollment | | | -0.012*** | | | -0.008** | | | -0.004*** | | | -0.003*** |
| | | | (0.004) | | | (0.003) | | | (0.001) | | | (0.001) |
| Controls for Pre-Existing Trends | N | N | N | Y | Y | Y | N | N | N | Y | Y | Y |
| Year Dummies | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Observations | 6625 | 6625 | 6625 | 5405 | 5405 | 5405 | 6625 | 6625 | 6625 | 5405 | 5405 | 5405 |
| R-squared | 0.95 | 0.95 | 0.95 | 0.96 | 0.96 | 0.96 | 0.95 | 0.95 | 0.95 | 0.96 | 0.96 | 0.96 |

Columns (1)-(6) are from regressions where we use charter school competition within 2 miles of a private school, while columns (7)-(12) are from regressions where we use 5 miles as the radius. Columns (1)-(3) and (7)-(9) are obtained from estimation of specification (1). Columns (4)-(6) and (9)-(12) are obtained from estimation of specification (2). *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity and are clustered at the county level to control for serial correlation across observations for the same county. To put results in perspective, the average private school enrollment during our study period (1990-2002) was 156.

**Table 3: Effect of Charter Competition on Private School Enrollment: Allowing for differential pre-program trends and trending effect of charter schools**

(Using PSS data, 1989-90 to 2001-02)

| | Dependent Variable: Enrollment | | | | | |
|---|---|---|---|---|---|---|
| | 2 Mile Radius | | | 5 Mile Radius | | |
| | FE | FE | FE | FE | FE | FE |
| Charter Presence | 2.64 | | | -1.84 | | |
| | (3.52) | | | (2.61) | | |
| Charter Presence * Trend | -6.32*** | | | -0.83 | | |
| | (1.75) | | | (1.48) | | |
| Number of Charter Schools | | -1.93* | | | -1.33*** | |
| | | (1.03) | | | (0.43) | |
| Number of Charter Schools * Trend | | -0.59 | | | 0.01 | |
| | | (0.37) | | | (0.07) | |
| Charter Enrollment | | | -0.010 | | | -0.005* |
| | | | (0.010) | | | (0.003) |
| Charter Enrollment * Trend | | | 0.001 | | | 0.000 |
| | | | (0.002) | | | (0.001) |
| Controls for Pre-Existing Trends | Y | Y | Y | Y | Y | Y |
| Year Dummies | Y | Y | Y | Y | Y | Y |
| Joint sig. of treatment effects[1] | 0.00 | 0.02 | 0.00 | 0.37 | 0.00 | 0.00 |
| p-value[2] | 0.15 | 0.01 | 0.27 | 0.20 | 0.00 | 0.06 |
| Observations | 5405 | 5405 | 5405 | 5405 | 5405 | 5405 |
| R-squared | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 |

See specification (3) in the text. Columns (1)-(3) are from regressions where we use charter school competition within 2 miles of a private school, while columns (4)-(6) are from regressions where we use 5 miles as the radius. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity and are clustered at the county level to control for serial correlation across observations for the same county. [1] p-value of test of joint significance of treatment coefficients. [2] p-value of test of statistical significance of total treatment effect for Trend=1. To put results in perspective, the average private school enrollment during our study period (1990-2002) was 156.

**Table 4: Effect of Charter Competition on Private School Enrollment: Do Private Schools with Religious Purpose or Orientation have Different Effects?**

(Using PSS data, 1989-90 to 2001-02)

| | Dependent Variable: Enrollment | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Charter Presence | 6.67 | | | 6.95 | | |
| | (8.25) | | | (8.00) | | |
| Charter Presence * Trend | -6.24*** | | | -6.32*** | | |
| | (1.75) | | | (1.68) | | |
| Charter Presence * Religious Purpose | -4.73 | | | | | |
| | (8.06) | | | | | |
| Charter Presence * Catholic | | | | -8.47 | | |
| | | | | (7.21) | | |
| Charter Presence * Other | | | | -1.11 | | |
| | | | | (11.87) | | |
| Number of Charter Schools | | -1.64 | | | -1.68 | |
| | | (2.12) | | | (2.12) | |
| Number of Charter Schools * Trend | | -0.58 | | | -0.55 | |
| | | (0.35) | | | (0.36) | |
| Number of Charter Schools * Religious Purpose | | -0.33 | | | | |
| | | (2.39) | | | | |
| Number of Charter Schools * Catholic | | | | | -2.14 | |
| | | | | | (1.97) | |
| Number of Charter Schools * Other | | | | | 2.00 | |
| | | | | | (3.48) | |
| Charter Enrollment | | | -0.008 | | | -0.007 |
| | | | (0.007) | | | (0.007) |
| Charter Enrollment * Trend | | | 0.001 | | | 0.000 |
| | | | (0.002) | | | (0.002) |
| Charter Enrollment * Religious Purpose | | | -0.002 | | | |
| | | | (0.006) | | | |
| Charter Enrollment * Catholic | | | | | | -0.007 |
| | | | | | | (0.005) |
| Charter Enrollment * Other | | | | | | 0.003 |
| | | | | | | (0.007) |
| Controls for Pre-Existing Trends | Y | Y | Y | Y | Y | Y |
| Year Dummies | Y | Y | Y | Y | Y | Y |
| Observations | 5405 | 5405 | 5405 | 5405 | 5405 | 5405 |
| R-squared | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 |

Notes: See specification (4) in the text. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity and are clustered at the county level to control for serial correlation across observations for the same county. To put results in perspective, the average private school enrollment during our study period (1990-2002) was 156.

**Table 5: Effect of Charter Competition on Private School Enrollment: Results from Fixed Effects Instrumental Variables (FE-IV) Estimation**

(Using PSS data, 1989-90 to 2001-02)

| | Dependent Variable: Enrollment | | | | | |
|---|---|---|---|---|---|---|
| | FE-IV (1) | FE-IV (2) | FE-IV (3) | FE-IV (4) | FE-IV (5) | FE-IV (6) |
| Number of Charter Schools | 4.91 | | 6.19 | | 6.26 | |
| | (3.82) | | (4.06) | | (4.17) | |
| Number of Charter Schools * Trend | -0.85 | | -1.31 | | -1.31 | |
| | (0.75) | | (0.88) | | (0.90) | |
| Number of Charter Schools * Religious Purp. | | | 1.56 | | | |
| | | | (1.27) | | | |
| Number of Charter Schools * Catholic | | | | | 0.30 | |
| | | | | | (1.41) | |
| Number of Charter Schools * Other | | | | | 2.95 | |
| | | | | | (2.54) | |
| Charter Enrollment | | 0.036 | | 0.058 | | 0.059 |
| | | (0.029) | | (0.041) | | (0.042) |
| Charter Enrollment * Trend | | -0.008 | | -0.015 | | -0.015 |
| | | (0.006) | | (0.010) | | (0.011) |
| Charter Enrollment * Religious Purp. | | | | 0.008 | | |
| | | | | (0.006) | | |
| Charter Enrollment * Catholic | | | | | | 0.002 |
| | | | | | | (0.005) |
| Charter Enrollment * Other | | | | | | 0.015 |
| | | | | | | (0.011) |
| Controls for Pre-Existing Trends | Y | Y | Y | Y | Y | Y |
| Year Dummies | Y | Y | Y | Y | Y | Y |
| Nonlinear Instruments | Y | Y | N | N | N | N |
| Joint sig. of treatment effects[1] | 0.30 | 0.46 | 0.27 | 0.46 | 0.42 | 0.64 |
| p-value[2] | 0.13 | 0.37 | 0.11 | 0.14 | 0.11 | 0.13 |
| P-value of A-P F-test excluded instruments from first stage (1) | 0.02 | 0.10 | 0.02 | 0.10 | 0.02 | 0.10 |
| P-value of A-P F-test excluded instruments from first stage (2) | 0.10 | 0.05 | 0.03 | 0.02 | 0.03 | 0.04 |
| P-value of A-P F-test excluded instruments from first stage (3) | - | - | 0.05 | 0.07 | 0.08 | 0.09 |
| P-value of A-P F-test excluded instruments from first stage (4) | - | - | - | - | 0.04 | 0.06 |
| Observations | 5392 | 5392 | 5392 | 5392 | 5392 | 5392 |

*, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity and are clustered at the county level to control for serial correlation across observations for the same county. [1] p-value of test of joint significance of treatment coefficients. [2] p-value of test of statistical significance of total treatment effect for Trend=1. Also reported are Angrist-Pischke multivariate F-test of excluded instruments for weak identification for the various first stage regressions. To put results in perspective, the average private school enrollment during our study period (1990-2002) was 156.

**Table 6: Using Pre-Program Data to do Falsification Tests: Testing Exogeneity of Instruments**

[(Using PSS data, 1989-90 to 1993-94)]

| | Dep. Var. = Number of Students | | |
|---|---|---|---|
| | FE | FE | FE |
| | (1) | (2) | (3) |
| Distance * Trend | -0.01 | | |
| | (0.08) | | |
| Distance * 1992 | | | 0.03 |
| | | | (0.04) |
| Distance * 1994 | | -0.02 | 0.00 |
| | | (0.02) | (0.00) |
| Observations | 2695 | 2482 | 2482 |
| R-squared | 0.98 | 1.00 | 1.00 |

*, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity and are clustered at the county level to control for serial correlation across observations for the same county.

**Table 7: Is distance from the authorizing public universities driving results? Robustness check using distance from non-authorizing public universities**

| | Dep. Var. = Number of Students | | |
|---|---|---|---|
| | FE | FE | FE |
| | (1) | (2) | (3) |
| Distance from non-authorizer * Program | -0.025 | 0.060 | 0.061 |
| | (0.021) | (0.036) | (0.037) |
| Distance from non-authorizer * Program * Trend | -0.020 | -0.020 | -0.020 |
| | (0.012) | (0.012) | (0.012) |
| Distance from non-authorizer * Program * Religious Purpose | | -0.057 | |
| | | (0.039) | |
| Distance from non-authorizer * Program * Catholic | | | -0.087 |
| | | | (0.057) |
| Distance from non-authorizer * Program * Other Religious | | | -0.069 |
| | | | (0.046) |
| Controls for pre-existing trends | Y | Y | Y |
| Year Dummies | Y | Y | Y |
| Observations | 5392 | 5392 | 5392 |
| R-squared | 0.96 | 0.96 | 0.96 |

*, **, ***: significant at the 10, 5, and 1 percent level, respectively. The standard errors, shown in parenthesis, are robust to heteroscedasticity and are clustered at the county level. All regressions control for pre-program trends of private schools, and include year dummies and private school fixed effects.

**Table 8: Are school districts located close to authorizing universities different?**

| Panel A | Comparing Districts within median distance of Authorizer to those between median and 2*median distance of authorizer | | | |
|---|---|---|---|---|
| Variable | Sample Size - Close | Sample Size - Far | Mean - Close | Mean - Far |
| | (1) | (2) | (3) | (4) |
| Percent White | 232 | 367 | 88.97 | 89.23 |
| | | | (16.60) | (17.10) |
| Percent Black | 232 | 367 | 5.48 | 5.42 |
| | | | (15.63) | (16.11) |
| Percent Hispanic | 232 | 367 | 1.64 | 1.89 |
| | | | (3.07) | (3.69) |
| Panel B | Comparing Districts in the same county as an Authorizer to those in neighboring counties | | | |
| Variable | Sample Size - Close | Sample Size - Far | Mean - Close | Mean - Far |
| | (1) | (2) | (3) | (4) |
| Percent White | 82 | 389 | 87.62 | 88.64 |
| | | | (18.34) | (17.84) |
| Percent Black | 82 | 389 | 6.93 | 6.36 |
| | | | (16.61) | (17.32) |
| Percent Hispanic | 82 | 389 | 1.80 | 1.56 |
| | | | (3.76) | (2.79) |
| Panel C | Comparing Districts within median distance of Authorizer to those within median distance of non-Authorizing universities | | | |
| Variable | Sample Size - Close | Sample Size - Far | Mean - Close | Mean - Far |
| | (1) | (2) | (3) | (4) |
| Percent White | 243 | 155 | 89.08 | 87.46 |
| | | | (16.27) | (19.34) |
| Percent Black | 243 | 155 | 5.27 | 7.66 |
| | | | (15.31) | (18.96) |
| Percent Hispanic | 243 | 155 | 1.56 | 1.16 |
| | | | (3.02) | (1.58) |

**Table 9: How do Authorizing Universities Compare with non-Authorizing Universities?**

| Variable | Number of Authorizers | Number of Non-Authorizers | Mean: Authorizers | Mean: Non-Authorizers |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Percent Black | 10 | 5 | 5.30 | 10.62** |
| | | | (3.56) | (5.95) |
| Percent Hispanic | 10 | 5 | 1.42 | 1.51 |
| | | | (0.89) | (1.00) |
| Percent Undergraduate | 10 | 5 | 85.86 | 76.07 |
| | | | (11.33) | (14.37) |
| Percent Graduate | 10 | 5 | 16.77 | 24.79 |
| | | | (9.55) | (14.93) |
| Percent Female | 10 | 5 | 86.72 | 83.47 |
| | | | (16.49) | (23.09) |
| Percent Over 25 years old | 10 | 5 | 34.64 | 38.55 |
| | | | (10.50) | (13.93) |
| Total number of degrees conferred | 10 | 5 | 2721.90 | 5085.20 |
| | | | (1834.68) | (3906.11) |
| Percent of Degrees at Bachelors level | 10 | 5 | 70.49 | 70.43 |
| | | | (8.12) | (15.36) |
| Percent of Degrees below Bachelors | 10 | 5 | 8.91 | 0.23 |
| | | | (12.15) | (0.51) |
| Percent of Degrees Above Bachelors | 10 | 5 | 20.60 | 29.34 |
| | | | (11.56) | (15.47) |

Note for tables 9 and 10: *, **, *** indicate whether column 4 is statistically different from column 3 at the 10, 5, and 1 percent levels, respectively. Standard deviations reported in parentheses below means.

**Table 10: Are the Results Robust to Dropping Rural Areas?**

(Using PSS data, 1989-90 to 2001-02)

| | Dependent Variable: Enrollment | | | |
|---|---|---|---|---|
| | FE | FE | FE-IV | FE-IV |
| | (1) | (2) | (3) | (4) |
| Number of Charter Schools | -1.54 | | 5.18 | |
| | (1.07) | | (4.83) | |
| Number of Charter Schools * Trend | -0.65* | | -0.70 | |
| | (0.39) | | (1.01) | |
| Charter Enrollment | | -0.008 | | 0.057 |
| | | (0.010) | | (0.046) |
| Charter Enrollment * Trend | | 0.000 | | -0.011 |
| | | (0.002) | | (0.010) |
| Controls for Pre-Existing Trends | Y | Y | Y | Y |
| Year Dummies | Y | Y | Y | Y |
| Joint sig. of treatment effects[1] | 0.03 | 0.00 | 0.32 | 0.44 |
| p-value[2] | 0.03 | 0.35 | 0.13 | 0.21 |
| Observations | 4626 | 4626 | 4626 | 4626 |
| R-squared | 0.96 | 0.96 | - | - |

Table excludes rural school districts. *, **, and *** denote significance at 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity. [1] p-value of test of joint significance of treatment coefficients. [2] p-value of test of statistical significance of total treatment effect for Trend=1.

**Table 11: Is Private School Size Driving the Results?**

| | Dependent Variable: Log Enrollment | | | |
|---|---|---|---|---|
| | FE | FE | FE-IV | FE-IV |
| | (1) | (2) | (3) | (4) |
| Number of Charter Schools | -0.0042 | | 0.0364 | |
| | (0.0098) | | (0.0283) | |
| Number of Charter Schools * Trend | -0.0009 | | -0.0036 | |
| | (0.0023) | | (0.0066) | |
| Charter Enrollment | | -0.000003 | | 0.000389 |
| | | (0.000054) | | (0.000236) |
| Charter Enrollment * Trend | | -0.000003 | | -0.000067 |
| | | (0.000013) | | (0.000050) |
| Controls for Pre-Existing Trends | Y | Y | Y | Y |
| Year Dummies | Y | Y | Y | Y |
| Joint sig. of treatment effects[1] | 0.16 | 0.16 | 0.06 | 0.11 |
| Observations | 5405 | 5405 | 5392 | 5392 |
| R-squared | 0.94 | 0.94 | - | - |

*, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity and are clustered at the county level to control for serial correlation across observations for the same county. [1] p-value of test of joint significance of treatment coefficients.

**Table 12: Impact of Charter Competition Allowing for a More Flexible Functional Form for the Instrument**

| | Dependent Variable: Enrollment | |
|---|---|---|
| | FE-IV | FE-IV |
| | (1) | (2) |
| Number of Charter Schools | 5.10 | |
| | (3.94) | |
| Number of Charter Schools * Trend | -0.94 | |
| | (0.63) | |
| Charter Enrollment | | 0.033 |
| | | (0.026) |
| Charter Enrollment * Trend | | -0.007 |
| | | (0.005) |
| Controls for Pre-Existing Trends | Y | Y |
| Year Dummies | Y | Y |
| Observations | 5392 | 5392 |

This table uses a quadratic functional form for the instrument. See text for details. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity and are clustered at the county level to control for serial correlation across observations for the same county.

**Table 13: Robustness Test Using Middle and High Schools:**
**Analyzing Effects of Charter Competition on Private Enrollment in Middle and High Schools**

| | Dependent variable: Number of Students | | |
|---|---|---|---|
| | FE | FE | FE-IV |
| | (1) | (2) | (3) |
| Number of Charter Schools | -6.54 | | |
| | (9.95) | | |
| Number of Charter Schools * Trend | 2.88 | | |
| | (3.29) | | |
| Charter Enrollment | | 0.004 | 0.094 |
| | | (0.024) | (0.095) |
| Charter Enrollment * Trend | | 0.000 | -0.020 |
| | | (0.005) | (0.026) |
| Controls for pre-existing trends | Y | Y | Y |
| Year dummies | Y | Y | Y |
| Observations | 477 | 477 | 477 |
| R-squared | 0.96 | 0.96 | - |

Notes: *, **, ***: significant at the 10, 5, and 1 percent level, respectively. The standard errors, shown in parenthesis, are robust to heteroscedasticity and are clustered at the county level.

**Table 14: Did the Impacts of Charter Competition Change After NCLB? Fortifying the Sample With Post-NCLB Data**

[(Using PSS data, 1989-90 to 2005-06)]

| | Dependent Variable: Enrollment | | | |
|---|---|---|---|---|
| | FE | FE | FE-IV | FE-IV |
| | (1) | (2) | (3) | (4) |
| Number of Charter Schools | -1.39 | | 6.42 | |
| | (1.23) | | (4.11) | |
| Number of Charter Schools * Trend | -1.23*** | | -1.28 | |
| | (0.38) | | (1.37) | |
| Charter Enrollment | | -0.013* | | 0.050 |
| | | (0.008) | | (0.031) |
| Charter Enrollment * Trend | | 0.000 | | -0.010** |
| | | (0.001) | | (0.005) |
| Controls for Pre-Existing Trends | Y | Y | Y | Y |
| Year Dummies | Y | Y | Y | Y |
| Joint sig. of treatment effects[1] | 0.01 | 0.00 | 0.11 | 0.13 |
| Observations | 6567 | 6567 | 4659 | 4659 |
| R-squared | 0.92 | 0.92 | - | - |

*, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively. The standard errors, shown in parentheses, are robust to heteroscedasticity and are clustered at the county level to control for serial correlation across observations for the same county. [1] p-value of test of joint significance of treatment coefficients.

**Figure 1: Michigan Charter and Private Schools (2002)**

Gray lines represent county boundaries

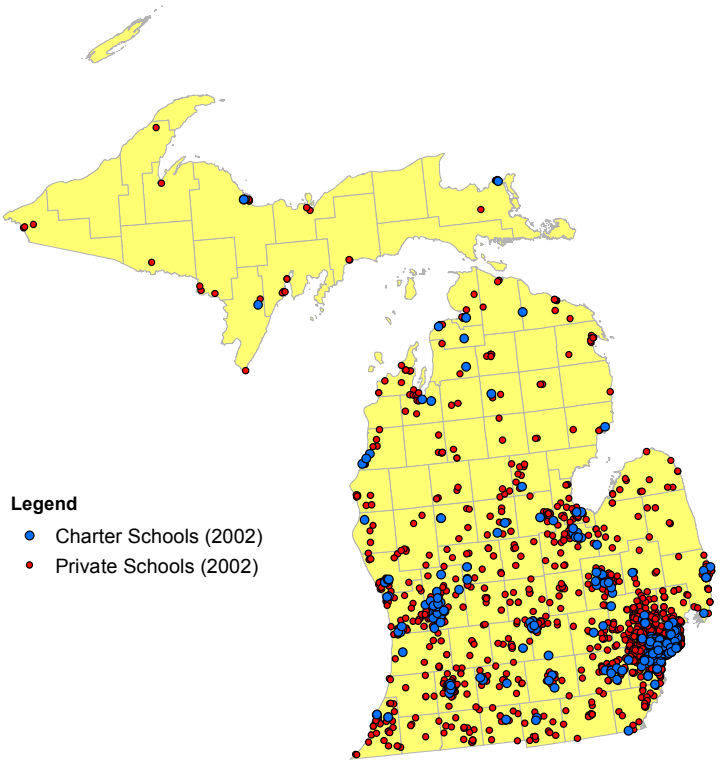
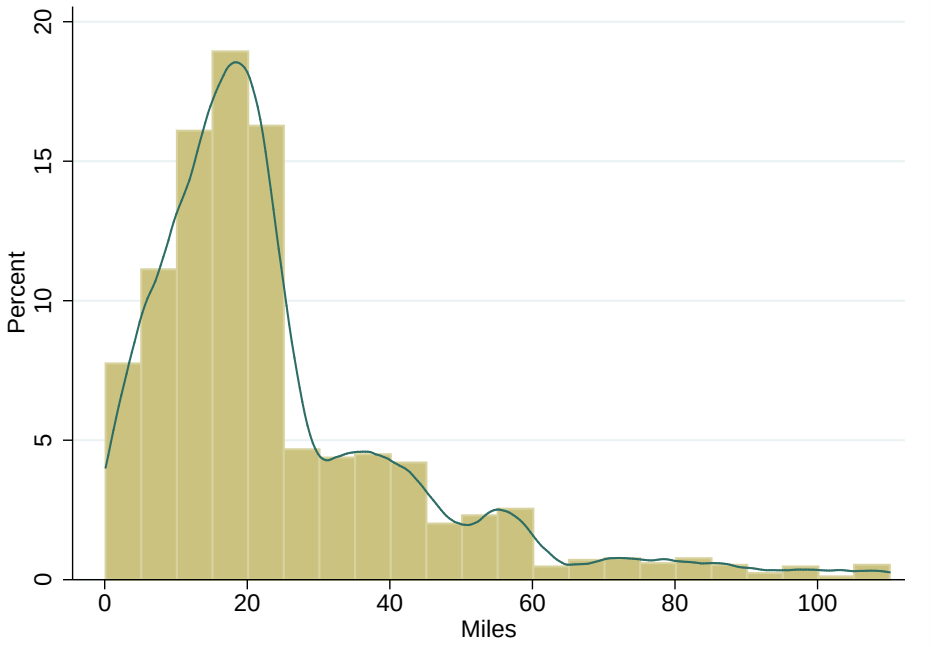



**Figure 2: Location of authorizing and non-authorizing universities**

Source: Integrated Post-Secondary Data System (IPEDS)
Gray lines represent county boundaries

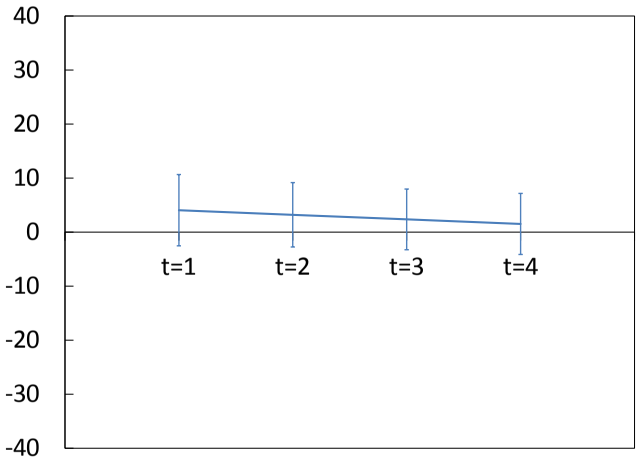
**Figure 3: Distribution of Distance of Private Schools from closest Public University with Governor Appointed Board**



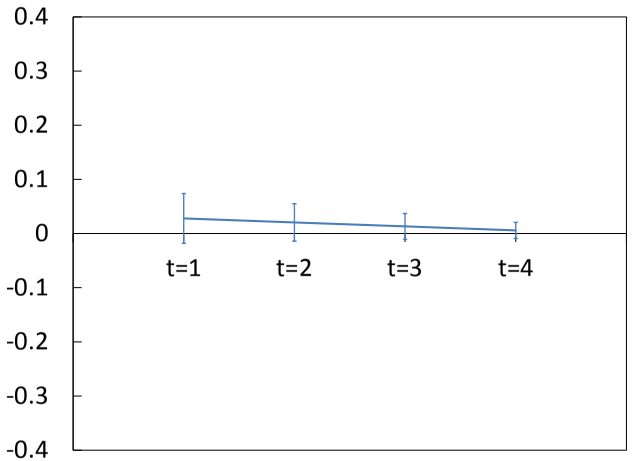

Bars represent groups of 5 miles. The mean distance is 24.25 miles. The 25th percentile of distance is 12.34 miles, median is 19.21 miles, and the 75th percentile is 30.37 miles.

**Figure 4: Total Treatment Effects and Confidence Intervals**

Competition Measure: Number
FE-IV

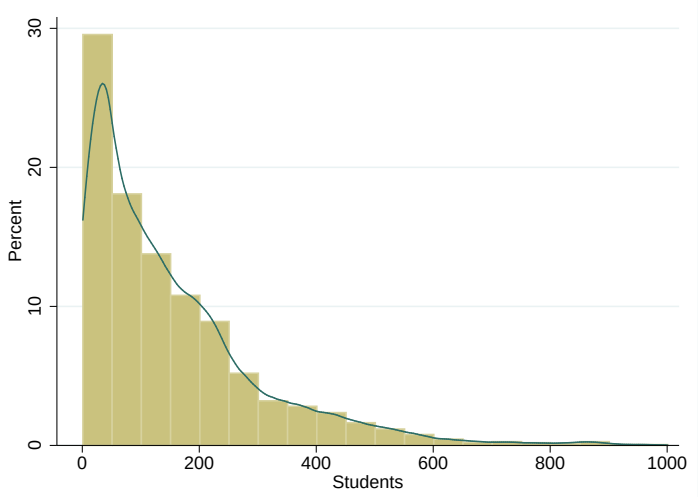



Competition Measure: Enrollment
FE-IV




Vertical bars represent 95% confidence intervals.

## Figure 5: Distribution of Private School Enrollment, All years




Bars represent groups of 50 students. Mean enrollment is 156.15 students. The 25th percentile of enrollment is 40 students, median is 110 students, and the 75th percentile is 214 students.

## Figure A1: Percent White

Source: Common Core of Data, School District Data.
Gray lines represent school district boundaries. Bins divide quintiles.

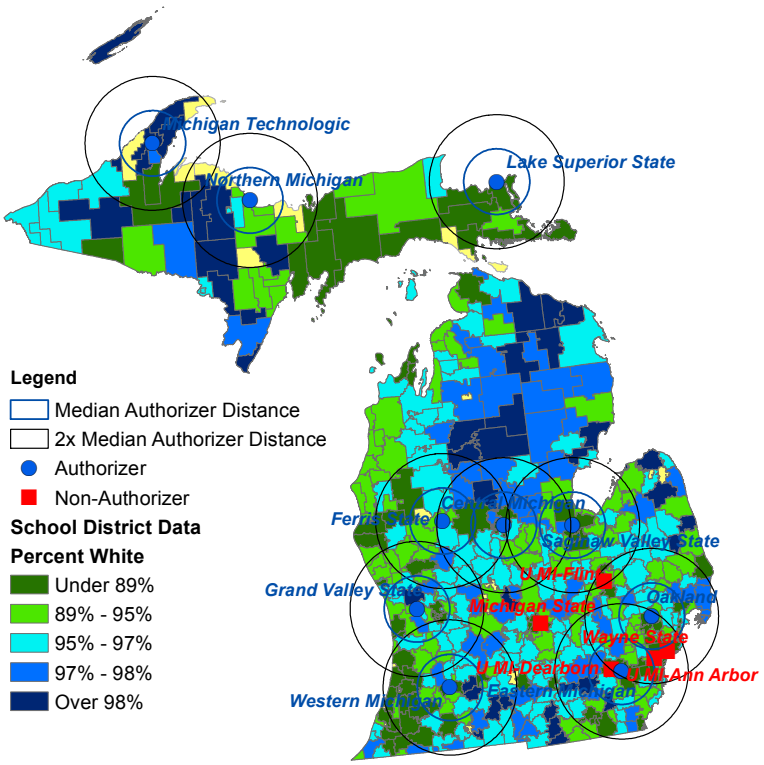


**Figure A2: Percent Black**

Source: Common Core of Data, School District Data.

Gray lines represent school district boundaries. Bins divide quintiles.

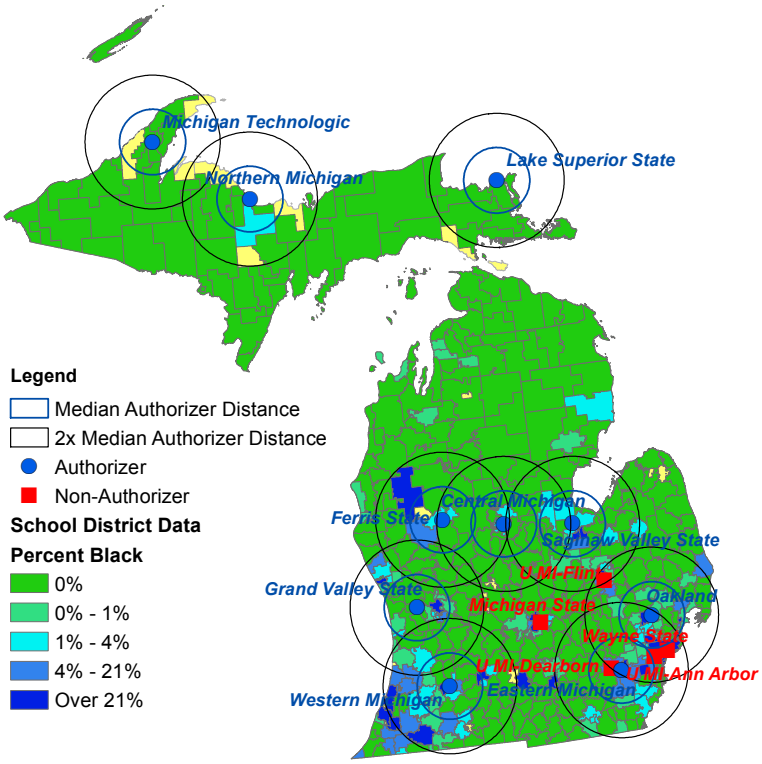



**Figure A3: Percent Hispanic**

Source: Common Core of Data, School District Data.

Gray lines represent school district boundaries. Bins divide quintiles.