Scandal-driven Catholic school closures and charter school response: Dynamics of competition for educational services

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Abstract

The paper asks whether public school services fill in gaps left by private school failures. Specifically, it explores what type of schools enter the market and experience an increase in enrollment after reports of abuse by Catholic priests lead to Catholic Schools closures. I use a two-way fixed effects event study method to estimate a change in enrollments and number of different types of schools after a report of priest abuse within the same zip code, school district, or county. I find there are 0.2 fewer Catholic schools and Catholic school enrollment falls by 75 students after six years, which are offset by a 0.2 and 50-student increase in charter school counts and enrollments on average. These increases are unique to charter schools and is not observed in other public or non-Catholic private schools. Altogether, these results suggest that former Catholic schooled families show a preference for charter schools over other public schools, which may be due to the low-cost and similar emphasis on discipline and academic achievement.

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1 Introduction

On January 6, 2002 the Boston Globe Spotlight reported that over 130 people had alleged sexual abuse by a former priest, John J. Geoghan, during his three-decade tenure in the Greater Boston area.[1] Furthermore, the story implicated Boston Archdiocese Cardinal Bernard F. Law in a coverup of Geoghan's crimes. Spotlight's reporting spurred similar reporting that brought to light further accusations around the country and world. ProPublica drew on news reporting and official press releases by the archdioceses to identify 6,754 credibly accused clergy through 2019[2]. Researchers at John Jay College of Criminal Justice[3] identified allegiations by 10,667 from 1950 to 2002 and the Georgetown University Center for Applied Research in the Apostolate (CARA) identified 8,694 allegations from 2004 to 2017[4].

These accusations affected the Catholic church, its members, and its institutions in a number of ways ranging from spiritual to financial. Dioceses and archdioceses around the country paid out settlements and legal fees for accusations, subsequently reducing support for local churches and institutions. Overall religiosity and charitable giving declined in areas in which an accused priest works or has worked (Bottan and Perez-Truglia, 2015) with substitutions of members from Catholicism to other Christian faiths (Hungerman, n.d.), leading to a decline in donations to the Catholic church. Furthermore, enrollments at Catholic schools fell in dioceses and zipcodes that experienced such a scandal (Bottan and Perez-Truglia, 2015; Dills and Hernández-Julián, 2012). The decline in donations and enrollments coupled with an increase in legal fees and settlement contributed to the shutting down of Catholic schools in areas that experienced scandals (Bottan and Perez-Truglia, 2015; Dills and Hernández-Julián, 2012; Carattini et al., 2012).

But what are the effects of these Catholic school closures on the educational market? Carattini et al. (2012) used the number of times a diocese and its scandal was mentioned in the news as an instrument for Catholic enrollment in each diocese and found a 2.7 percentage point increase in

[1] https://www.bostonglobe.com/news/special-reports/2002/01/06/church-allowed-abuse-priest-for-years/cSHfGkTIrAT25qKGvBuDNM/story.html

[2] https://projects.propublica.org/credibly-accused/

[3] http://www.usccb.org/issues-and-action/child-and-youth-protection/upload/The-Nature-and-Scope-of-Sexual-Abuse-of-Minors-by-Catholic-Priests-and-Deacons-in-the-United-States-1950-2002.pdf

[4] https://www.americamagazine.org/faith/2018/09/05/cara-study-indicates-decline-abuse-reports-worst-behind-us

pass rates of a standardized math test in public schools. The decline could suggest public schools had fewer incentives to teach well without competition from Catholic schools, but the study does not account for compositional effects of the student body or educational market. For example, if only the lowest-achieving students transfer into public schools after a Catholic school closes, then this weakens evidence of a competition mechanism. Furthermore, Catholic schools changing sectors or new schools entering the local educational market would complicate the competition story.

McShane and Kelly (2014) identified 18 such schools that "sector-switched" from Catholic to charter in 2008 and 2010. Their case study identifies that enrollment generally increased at these schools. Additionally, Waddington (2012) shows that charter schools may contribute to the decline in Catholic enrollments, but his work remains agnostic on the effect of scandals, which omits a critical variable.

I seek to further characterize the competition between charter and Catholic schools by establishing a causal relationship: charter schools open after Catholic schools close. I correct for the possible endogeneity that charter schools for force Catholic schools to close by using public accusations against local Catholic officials as an exogenous shock to Catholic school closures. My analysis relies on an event study framework, which assumes the timing, not the occurrence of a scandal is exogenous. My results show that an average of 0.1 charter schools move into a zipcode within 10 years of a public accusation against a Catholic official. Furthermore, charter school enrollments increase as much as 10 percent in the long run. No such effect seems to exist for traditional and magnet public schools or secular or other religious private schools. I expand these results to the school district level and find similar results. These results also hold whether I restrict to accusations against priests that currently or formerly worked in a church or institution in the zipcode or diocese.

Of course, charter schools may just advantageously go into areas with Catholic schools, which inadverently appears to show that charter schools replace Catholic schools closing after a scandal. I address this by regressing my results on areas that ever have a Catholic school. I find that those areas with scandals and an open Catholic school at some point in the sample are far more likely to see charter schools open than those areas without scandals, but with an open Catholic school at some point in the sample. When I restrict to those areas that ever have a charter school, I find

similar results. In general, this evidence suggests that charter schools seek out areas where Catholic schools are likely to close.

These results suggest the Catholic scandals may have accelerated the expansion of the charter system in certain areas. This information suggests that charter schools may advantageously select to move into areas and replace outgoing private schools, which would compromise estimates of the competitive effect of charter or private school enrollments on public school performance. Last, while I do not have adequate price data to investigate the claim, the finding indicates that charter schools may have been the most ideal substitute for students leaving private schools.

The rest of the paper proceeds as follows. Section 2 expands on the literature concerning private-public educational competition and charter schools. Section 3 introduces the data used to establish my reuslts. Section 4 introduces my event study framework. Section 5 shows my results and robustness checks. Section 6 concludes.

2 Literature

The literature on competition in schooling services goes back to Tiebout (1956), who argued that citizens can vote for public goods, like education, with their feet by choosing to live in areas with super educational services. Improving school choice available to students would then improve educational outcomes for students because it would force public school's to stay innovative and pull in more students. Two large empirical literatures have followed which attempt to (1) measure a causal effect of school choice on economic outcomes and (2) look whether sorting and selection violate Tiebout choice. This paper focuses on extending this second strand of literature, but I will expand on both below.

Work in the first strand of literature focuses on finding a plausibly exogenous increase to private or non-traditional public schools, and looking at the effect on public school educational outcomes. In a highly influential paper, Hoxby (2010) instrumented for endogenous school district formation using natural boundaries (rivers and streams), which influenced historical district formation. Her findings suggested that metropolitan areas with more districts, and thus more Tiebout choice, had schools with higher test scores and fewer private schools. In a paper that focused on individual students, Neilson (2013) suggested that targeted vouchers in Chile raised school competition in

poorer neighborhoods and seemed to push schools to improve their academic quality. As previously mentioned, Carattini et al. (2012) found dioceses with more private schools driven had slight improvements in standardized test pass rates. While these studies suggest an effect of private schools on public school outcomes, several mechanisms may drive this relationship besides Tiebout choice.

One possible mechanism might be sorting of students with means into other schools, which allows public schools to allocate more resources to their enrolled students. Epple and Romano (n.d.) show that in equilibrium students would sort on ability and income with high-ability, low-income students in schools with low-ability, high-income students. Stratification combined with a preference for "good" peers may lead to sorting into a separating equilibrium with students not considering public schools (Macleod et al. (2015) MacLeod and Urquiola (2015)).

Stratification creates an incentive for schools to "cream skim" the best students in traditional public schools. Angrist et al. (2013) notes some charters may draw in high-quality students and offer minimal value-added, while "No Excuses" charters are far more productive than public schools. Singleton (2019) builds a model of charter school entry that notes how traditional funding formulas provide incentives to cream-skim and models charter schools as heterogeneous in quality, cost structures, and objectives. He finds that cost-based cream skimming is by charter schools, which is largely driven by non-No Excuses and for-profit charter schools, while No Excuses charter schools enter into areas with historically underserved black and Hispanic populations. Counterfactual models suggest that targeted start-up grants or cost-adjusted formula that favored underserved areas would largely improve charter services in underserved areas.

This work provides an important causal-link to the story that Singleton (2019) tells, which largely relies on cross-sectional data to make his estimates. Noting that charter school entry and enrollment decisions are both highly endogenous, I use scandals involving Catholic officials as an instrument for Catholic exits and enrollment declines and show that charter schools increase almost parallel to these declines. My work suggests that contrary to the hypothesis of Waddington (2012), charter school entries seem to follow private school closures. Furthermore, my estimates suggest that students move directly from private Catholic programs to charter schools.

3 Data

I draw on several data sources to show how different types of schools respond to scandals in the Catholic church. First, I use data on scandals from 1980-2012 put together by Bottan and Perez-Truglia (2015) and used with the authors' permission. Second, I use enrollment data for private schools from the biennial Private School Survey (PSS) and public schools in the annual Common Core of Data (CCD), both of which are put together by the National Center for Education Statistics (NCES). Third, I draw on data from the 1990 U.S. Census and Association of Religious Data Archives to control for the fact that the probability of a scandal occurring increases in the prevalence of Catholicism and other demographic characteristics. I explain each of these datasets in turn below.

3.1 Scandals Data

Bottan and Perez-Truglia (2015) put together their scandals dataset by drawing on allegations reported on bishopaccountability.org. This website lists all public allegations against priests in the Catholic church around the world along with their diocese, order, number of victims, and relevant public information. While these allegations are not necessarily proven to be true, they are associated with a public news report. The effect of a scandal likely operates through its publicity and less through the validity of the allegation.

Bottan and Perez-Truglia (2015) researched each accusation and geolocated the location of each alleged abuse with an address, latitude and longitude, zipcode, county, and state. I used the zipcode-level data for my analysis and used shapefiles from the NCES[5] to match each geolocated scandal to its corresponding school district in order to analyze school district level changes in private, charter, and public schools.

In addition to geocoding the data, Bottan and Perez-Truglia (2015) go an extra step and classify the scandals into "Type-A" and "Type-B," based on the location of the alleged crime and location of the priest at the time of the accusation. Type-A scandals are those allegations against a clergy member who currently works in a Catholic institution of abuse in the past, while working at another

[5] https://nces.ed.gov/programs/edge/Geographic/DistrictBoundaries

institution. The location of a type-A scandal is the location in which the priest currently works and the date is the date of the first newspaper article about the scandal. Type-B scandals cover institutions in which a priest was accused of committing abuse while working at the institution in the past. The location of the scandal is the institution where the alleged abuse occurred and the date is the date of the first newspaper reporting the abuse which circulates in the area of the relevant institution.

These typings help the authors separate out the way scandals have different costs on a local parishes and institutions. Parishes have to remove a priest after a Type-A scandal, but parishes with type-B scandals may have to face lawsuits. Both types of scandals should have a negative effect on the perceptions of the Catholic church in an area. I regress my outcome variables on both and do not find a difference, as such my main specification relies on both.

The database covers 3,024 scandal events from 1980-2010 with 1,125 Type-A and 1899 type-B scandals. I include a map of scandals by zip code and timeline of scandal counts by type in figure 1. Together these suggestive a robust amount of geographical and time heterogeneity, which lends itself to an event-study format that I will expand on in section 4.

3.2 Schooling Data

My outcomes of interest are the number of schools in an area and student enrollment by type of school. I use the PSS and CCD to measure each of these outcomes.

In order to make the surveys comparable I drop the years of the CCD that do not match the PSS years. One issue with this method is that some schools close or open during a year when they do not start providing data. I impute an observation for the school back one year and backfill its relevant geographical and institutional details to ensure that it is included in my counts. I leave the enrollment as missing to keep from blending post-event data with pre-event data.[6]

Next, I classified the schools using indicators for schooling level, religiosity, and other typing information. Schooling level is restricted to "Elementary," "Secondary," "Ungraded," and "Combo" schooling levels, though CCD also includes "Pre-K." I could have imputed a more detailed metric

[6] As robustness checks that are available upon request, I regress whether a school "opened" or "closed." Where possible I match this information to the actual year a school opens or closed and not from when it first appears in my panel with information that it actually opened a year before.

using the highest and lowest grade in each school, but I decided not to. I do largely exclude Pre-K from my analysis as it is not common across both datasets.

For type, I had differing levels of granularity in my categorizations. The broadest category listed public schools as "Public" and assigned private schools to "Catholic," "Non-Catholic," or "Secular." The next most granular assigned the typology of the school within each religiosity. Public schools fell into "Public," "Special Education," "Vocation," "Alternative," and "Reportable," a self-contained program that may share a principal with a traditional public school. The vast majority were public schools. Among private schools, the typologies were "Parochial Catholic," "Diocesan Catholic," "Private Catholic," "Conservative Christian," "Other Religious Affiliation," "Other Religious Unaffiliated," "Secular," "Special Emphasis," "Special Education." Other categorizations were possible, but usually involved such a small N, they were not worth displaying.

One category that did not nest itself neatly in the above categorizations is charter schools. charter schools could be several types of public schools. As a result, I typed each public school as charter or Non-charter in its own dummy variable.

The PSS is not matched to a local educational agency, or school district, which would allow me to look at private schooling changes within a relevant geographical unit for public schools. To solve that I used the NCES geographical relationship files to match zipcodes and FIPS county codes to local educational agencies. I used each private school by its zipcode and FIPS code to a school district.

Once I have the CCD and PSS variables created and cleaned, I append one on the other and then collapse on year, geographical level (zipcode or school district), schooling level, religiosity, and typology and then reshaped at the schooling, religiosity, and typology level. I combined each interaction and dropped those columns with insufficient observations. I collapsed charter schools at the year, geographical level, and schooling level, and reshaped at the schooling level, which I merged on at the year-geography level. Finally, I merged the scandal data collapsed at the relevant geography-year level to these collapsed schoolin datasets for analysis.

The schooling data on their own illustrate some important patterns about schooling in the United States. First, figure 2 shows that public schools have been increasing while private schools have largely flatlined in the last twenty years. Second, figures 2 and 2 breaks down the growth

of private schools by the religiosities and the growth of charter schools. Catholic schools have steadily declined from being the plurality since the 1990s with that trend accelerating after 2001 and the Spotlight report. Meanwhile, secular and non-Catholic religious private school counts have continued to increase. Together, these three lines explain the relative flatlining of private schools in figure 2. Along with the opposite trends by religiosity, the yellow line in the graph shows the monotonic increase to charter schools for the last two decades. These opposite trends for charter and Catholic schools suggest one replaced the other.

A relationship between these aggregate trends would likely require at least some geographical overlap between schools. Figure 3 shows the percentage of zipcodes with at least one school of a type conditional on if that zipcode ever had a Catholic school and the percentage of zipcodes with at least one zipcode that ever have one of the other types of schools during the sample period. The solid black line shows the percentage of zipcodes with a public school if that zipcode ever had a Catholic school. Meanwhile, the dotted black line shows the percentage of zipcodes with a Catholic school that ever had a public school. Note that I look at zipcodes that ever have a school, so the charter school line can extend before 2000 when the CCD started counting charter schools.

There are two things to note about the these graphs. First, putting aside charter schools, the levels are relatively stable for all the graphs with a slight decline in the percentage of zipcodes with a Catholic school that ever have another type of school. This implies relatively little change in the competition between types of schools over the sample period. Second, the growth of charter schools seems to target areas with Catholic schools, with the percentage of zipcodes with a Catholic school in a zipcode that ever has a charter school noticeable falling, while the percentage of charters that ever have a Catholic church notably rising. These flat trends for other school types and opposite trends for charter and Catholic schools are suggestive of the relationship I will present in the next section: charter schools seemed to open in areas with Catholic schools after a Catholic scandal.

Figure 3 shows that this upward trend in charter schools is common in zipcodes that ever have every type of school. These parallel trend suggests that charter schools are not targeting any one type of competitor schools, which is useful for my analysis if the timing of Catholic scandals are truly exogenous. Of course, this pattern also brings home that I must control for time trends in regressions on the number of schools or students in a locality to control for aggregate trends.

Last, I want to focus on zipcodes with some overlap between types of schools. Therefore, the regressions I run will either condition on there ever being a Catholic school in the sample period or ever having the same type of school as my outcome in the sample period. In either case, I do not look at the effects of scandals in areas where there are no relevant schools or no Catholic student population.

3.3 Other Data

Last, I use data from the 1990 Census and Association of Religious Data Archives (ARDA) to control for possible non-parallel trends in areas with scandals. The 1990 Census data come in both the zipcode and school district level, but the ARDA is at the county level. I map the ARDA county data to the zipcode and school district level using the FIPS to zipcode key provided by the Missouri Census Data Center and the FIPS to school district key provided by the NCES.

Figure 4 shows the percentage of religious adherents that are Catholic by zipcode. This map alongside the map in figure 1 shows the prevalence of Catholicism is predictive of having one or multiple scandals. ../analysis/output/tables 1 and 2 show t-test comparisons for areas with and without scandals. A variety of demographics do not differ much, but scandal areas are clearly more Catholic, more religious, higher income, and more urban. As a result, I control for quartiles of each of the demographic variables and the percentage Catholic in each zipcode or school district in all regressions.

4 Empirical Setup

Ideally scandals would be completely exogenous events, but as shown in section 3.3, they are predicted by the Catholic population among other demographics, which are possibly and likely predictive of the number of Catholic schools and students enrolled in Catholic schools in an area. Therefore, I assume the timing of scandals is plausibly exogenous, in line with Bottan and Perez-Truglia (2015) and estimate an event study framework. Given that some areas have multiple scandals in multiple years, I cannot use a traditional event study framework, but instead rely on the framework for multiple event studies with heterogeneous magnitudes introduced by Sandler and Sandler (2014) and extended by Schmidheiny and Siegloch (2019) shown in equation 1.

Essentially, the multiple event study method takes into account that (1) if an event occurs after a prior event it may or may not have the same effect and (2) if multiple events happen in the same year it may have a larger effect than a single event happening in that year. While neither (1) nor (2) is likely to be a linear effect, a more flexible model like this one removes possible bias from excluding events in other years or collapsing multiple events into dummy variables.

$$y_{z,t} = \sum_{w \in W} \gamma_w d^w_{z,t} + \mu_z + \delta_t + X_{z,t}\beta + \epsilon_{c,t} \tag{1}$$

The key variable of interest is $d^w_{z,t}$, which equals one in area z that occured in year $t - w$ where t and w is all years in the event window W. The event window W is chosen to be wider than the window over which the outcomes are observed. Given that I have events from 1980 to 2010 and observe outcomes from 1990 to 2010, or 2000 to 2010 for charter schools, I drop the coefficients from relative time $w \notin [-6, 6]$. Additionally, given that the my outcome data are biennial, I bin my $d^s_{z,t}$ in two-year increments.

The $y_{z,t}$ is my outcome of interest, like the number of students enrolled in a Catholic school or charter school. μ_z and δ_t are locality fixed effects and time fixed effects, respectively. $X_{z,t}$ are my set of controls. These controls are demographic quartiles in 1990 interacted with time fixed effects and state fixed effects interacted with year fixed effects to account for differential government trends.

My standard errors are clustered at the diocese and archdiocese level. A diocese is the local ruling body of the Catholic church run by a bishop or archbishop. I present a map of the diocese in figure 5. Bishops and archbishops often became the face of failures by the Catholic church to properly address past scandals in the news and as such a scandal in one area, likely spilled over into a scandal in another area. If I do not account for these spillover effects in my point estimates, I will just underestimate the effect of the scandal. If I do not account for the spillovers in my standard errors and assume uncorrelated errors between zipcodes or school districts, then I will underestimate them and find significance where none really exists in my data.

Notably, these results rely on heterogeneous timing of scandals, which may have heterogeneous treatment effects over that period. The recent literature (summarized by Goodman-Bacon (2021))

on two-way fixed effects methodologies highlights a number of biases that could arise from this approach as the early treated observations serve as controls for later treated observations. One potential solution to this critique is to have a large control or untreated group, which I have, leading me to be less concerned about these critiques. Nevertheless In Appendix B, I show results from the Sun and Abraham (2021) estimator, which are largely consistent with the results from the rudimentary event study framework above.

5 Results

5.1 Catholic & charter Schools

Figures 6 show the baseline event studies in the enrollment and number of Catholic and charter schools at the zipcode level. These demonstrate that by six years out, there were 0.2 fewer Catholic schools and 75 fewer students enrolled in Catholic schools, both on average. Conversely, there were about 0.2 more charter schools and 50 more students enrolled in charter schools. These results are consistent with the results of Bottan and Perez-Truglia (2015). I present similar analysis broken out by Type A and Type B scandals in appendix figure 14.

Among the number of schools, there are zero pretrends, while there are slight (but imprecisely measured) pretrends in the enrollment graphs. These results suggest that some scandals were known locally ahead of public reporting. As a result, more flexible decisions, like a family changing a child's school happened ahead of the reports, while less flexible decisions, like a town opening a new school, only happened after public reports.

The zipcode level results are more precise, but may miss out on effects because schooling services are provided by the school district. As a complement, I present the same graphs at the school district level in figures 7. Catholic enrollments and school count decreases are somewhat noisier, but fall by about 75 and 0.3 on average, respectively. Meanwhile, charter school enrollments and school count increases are about 50 and 0.1 on average, respectively.

Next, I show the break down by the type of Catholic school: parochial, diocesan, or generally private and Catholic in figure 10. While the dioceses typically took on the brunt of negative news coverage after a scandal, their directly affiliated schools seemed relatively safe through this period

with enrollments and school counts staying relatively flat through the following years. The same holds true for private Catholic schools, which are the minority of Catholic schools. Parochial schools are hardest hit as there are 0.25 fewer schools on average and mean enrollments fall over 70 students in six years. This is suggestive that Parochial schools dominate the effects for all Catholic schools. At the school district level in appendix figure 13, these same patterns generally hold, though results are less precise.

5.2 Other Schools

While the magnitude of the charter school increases is in line with the decrease among Catholic schools, it is possible that other religious or public schools could experience a change in schooling. For example, maybe the charter increase is capturing an overall boom in all public schools at this time period. Alternatively, other schools may experience a downturn if families were likely to move away from towns after scandals, leading to a decrease in demand for these types of schools. Lastly, other religious schools may also see a downturn because families start to view all religious scholastic options more negatively after scandals.

First, figure 11 shows that enrollment and count are flat among private secular and non-Catholic religious schools. Figure 8 show an increase in all public schools after Catholic scandals at the zipcode level, while enrollments decrease, though point estimates are imprecise. As public schools are a superset of charter schools, this suggests that the increase in public schools is locally driven by charter schools. Meanwhile, the noisy enrollment decline might be driven by a charter school opening in a nearby zipcode and as such drawing some public school students.

The distinction between charter and public schools is further clarified in figure 9, which shows the same event studies at a school district level. While less precisely estimated, these results show point estimates of public school increases track slightly higher than charter school increases to both the number of schools and enrollments. Given that charter schools are a subset of public schools, this suggests that if a zip code does not open a charter school, students shift to the nearest public school. Additionally, public schools increase by one after six years after tracking with charter schools, suggesting non-charter public schools are slower to open relative to charter schools.

All other school types do not differ at the school district level as shown by 12. Here both types

of private schools remain flat around scandals.

5.3 Catholic-Charter Substitution

I next ask about the substitution between Catholic and charter schools. I hypothesize that charter schools will gain students and open as Catholic enrollments fall and schools close. While an OLS regression could serve here, I am concerned about the endogeneity of the Catholic and charter school counts and enrollment. On one hand, charter schools may increase competition and lead to Catholic schools closing, introducing reverse causality and biasing my coefficient down (negative). On the other hand, Catholic and charter schools may be more likely to open and have more students enrolled in areas where populations are on the rise, introducing omitted variable bias and biasing my coefficient estimates up (positive).

I summarize the apparent substitution between Catholic and charter schools using an instrumental variable analysis, using a dummy for whether it is a post-scandal year as an instrument as shown in equation 2. Essentially, I assume the scandals only affect the creation and enrollment of charter schools via the Catholic enrollment and count channel, satisfying the exclusion restriction. Similarly, I assume that scandals are revealed to the public in a way that is not correlated with the other forces in the education market, satisfying the exogeneity condition.

$$\begin{aligned} \text{School outcome}_{zt} &= \beta_0 + \beta_1 \text{Catholic outcome} + \gamma_z + \lambda_t + \varepsilon_{zt} \\ \text{Catholic outcome}_{zt} &= \alpha_0 + \alpha_1 \text{Post scandal} + \delta_z + \mu_t + \eta_{zt} \end{aligned} \tag{2}$$

Table 3 summarizes the results for charter and public schools in both the OLS and IV specifications. For counts, the OLS estimates show a decrease of -0.1172, suggesting that for every one Catholic school that closes, 0.1 charter schools open on average. The IV estimates show a magnitude of -1.4, suggesting a relationship of slightly one more charter opening for each closed Catholic school. For enrollments, the OLS estimates show a magnitude of -0.15, suggesting that for every 100 Catholic student that leaves, about 15 students enroll in charter schools. The IV estimates show a magnitude of -1.14, suggesting a one-to-one relationship or that the majority of Catholic students shift to charter schools. The smaller magnitude, negative estimates for charter

school counts and enrollment suggest some underlying positive correlation between Catholic and charter schools, driven by an omitted variable bias channel – the two are likely to open and have more students in areas where populations are on the rise. The public school results highlight that this is a unique role among charter schools.

These IV estimates for charter counts and enrollment hover just above a one-to-one replacement of Catholic with charter schools and a 1:1 shifting in students from Catholic to charter schools. On average there are 0.32 Catholic schools and 0.15 charter schools, suggesting an elasticity of -3.02. The corresponding enrollment averages are 96.4 and 4.06 for Catholic and charter schools, respectively, suggesting an elasticity of -27.06. These imply a highly elastic relationship, in part because of the small market share of charter schools at this point, and in part evidence that the two are close substitutes for families in the education market.

6 Conclusion

This chapter builds on past shows that charter schools fill the gap in educational services for formerly Catholic-schooled children. This shift to charter schools is driven by both demand- and supply-side factors. Demand-side factors include that families are less likely to send children to Catholic schools after scandals, while supply-side factors include that Catholic schools close after scandals. This chapter shows that Catholic scandals seem to accelerate the growth of charter schools in areas that have Catholic schools. This suggests that charter schools are the closest substitute to Catholic schools in most education markets.

My findings open up several avenues for future research. First, I plan to interact the scandals with state-level variation in the charter licensing requirements to estimate the "willingness-to-fund" and associated supply curve for charter schools. Second, one could use student-level data to explore whether scandals generate sorting as non-Catholic students primarily change schools, while Catholic students are more likely to stay or seek out nearby Catholic schools that remain open. Third, one could ask whether the shift toward charter schooling leads to an increase in scholastic outcomes like test scores, and high school graduation and college acceptance rates, and non-scholastic outcomes like teenage pregnancy and drug use. Furthermore, it would be useful to know whether any changes are driven by switching students, staying students, or students in charter schools that receive new

students.

Overall, these results provide a useful first pass that Catholic scandals partially helped drive the growth of charter schools. This suggests that charter schools are a viable alternative to Catholic schools for many families, and that the growth of charter schools is not just a response to demand-side factors like parental preferences. In future work, I will explore the short- and long-run economic effects of this shift in greater detail.

7 Tables

Table 1. Means T-test of religious variables across scandal zones in 1990

	(1)				
	(Scandal)	(No Scandal)	Diff.	SE	Obs.
Pct.religious	0.53	0.46	-0.07***	0.01	28656
Pct Evang. Prot.	0.03	0.04	0.01***	0.00	28656
Pct Mainline Prot.	0.03	0.03	0.00	0.00	28656
Pct Catholic	0.87	0.60	-0.28***	0.01	28656
Pct Other	0.00	0.00	0.00	0.00	28656
Pct Orthodox	0.00	0.00	-0.00***	0.00	28656
Pct Black Prot.	0.00	0.01	0.00***	0.00	28656
Catholic cong.	2.29	0.67	-1.62***	0.03	28656
Other cong.	1.47	0.49	-0.98***	0.06	28656
Evang. Prot. cong.	7.48	4.44	-3.04***	0.19	28656
Mainline Prot. cong.	7.76	3.41	-4.35***	0.13	28656
Orthodox cong.	0.15	0.03	-0.12***	0.00	28656
Black Prot. cong.	0.12	0.07	-0.06***	0.01	28656
Observations	28656				

Table 2. Means T-test across scandal zones for 1990

	(1)				
	(Scandal)	(No Scandal)	Diff.	SE	Obs.
Pct. popUnder18	0.25	0.27	0.02***	0.00	31004
Pct. Priv. School	0.09	0.05	-0.04***	0.00	26393
Pct. urbPop	0.81	0.28	-0.54***	0.01	31004
Pct. womPop	0.51	0.50	-0.01***	0.00	31004
Pct. HS Deg.	0.31	0.38	0.07***	0.00	26414
Pct. Coll. Deg.	0.14	0.11	-0.03***	0.00	26414
Real Per Cap Inc. (thou. 2018 USD)	282.80	229.35	-53.45***	2.77	27595
Pct. white	0.81	0.88	0.07***	0.00	31004
Observations	32185				

Table 3. Charter and public school count and enrollment (OLS and IV)

Method	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	Charter Count		Public Count		Charter Enrollment		Public Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Catholic Count	-0.1172***	-1.420***	0.1244**	-1.241**				
	(0.0246)	(0.2996)	(0.0542)	(0.5570)				
Catholic Enrollment					-0.1461***	-1.141***	0.5300***	1.442**
					(0.0280)	(0.1895)	(0.0986)	(0.5781)
Observations	163,404	163,404	163,404	163,404	163,404	163,404	163,404	163,404
R^2	0.73977	0.57035	0.94872	0.94383	0.65574	0.58247	0.95750	0.95692
Dependent variable mean	0.15263	0.15263	4.0633	4.0633	45.951	45.951	2,050.4	2,050.4
F-test (1st stage), Catholic Count		710.69		710.69				
F-test (1st stage), Catholic Enrollment						1,175.5		1,175.5
Zipcode and Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Note: The IV and OLS estimates of the counts and enrollment at public and charter schools with respect to the count and enrollment of Catholic schools. Instrument is a dummy for being after an abuse scandal in the Catholic church.

Table 4. Charter and public school count and enrollment (OLS and IV)

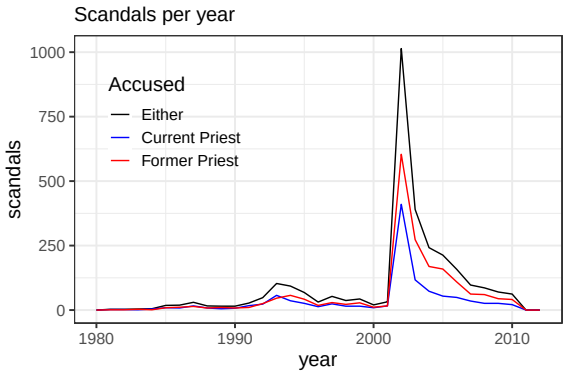
Method	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	Charter Count		Public Count		Charter Enrollment		Public Enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Catholic Count	-0.0904**	-0.4116*	4.233	-1.705*				
	(0.0365)	(0.2320)	(3.290)	(0.9447)				
Catholic Enrollment					-0.1930	-0.1740	11.43	-3.643**
					(0.1185)	(0.1767)	(6.958)	(1.544)
Observations	98,064	98,064	98,064	98,064	98,064	98,064	98,064	98,064
R^2	0.78475	0.76442	0.91518	0.88089	0.81977	0.81969	0.91192	0.85117
Dependent variable mean	0.19244	0.19244	5.5804	5.5804	53.447	53.447	2,777.1	2,777.1
F-test (1st stage), Catholic Count		104.77		104.77				
F-test (1st stage), Catholic Enrollment						123.53		123.53
District and Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Note: The IV and OLS estimates of the counts and enrollment at public and charter schools with respect to the count and enrollment of Catholic schools. Instrument is a dummy for being after an abuse scandal in the Catholic church.

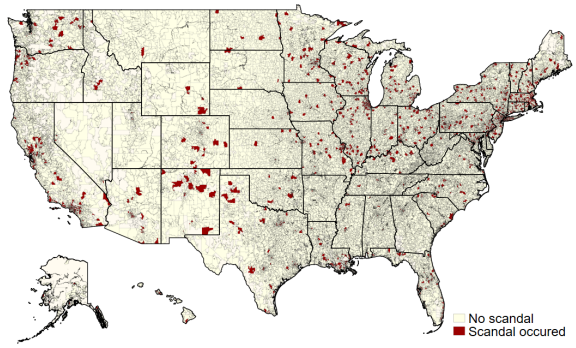
8 Figures

Figure 1. Time and geographic variaton of scandals

(a) Timeline



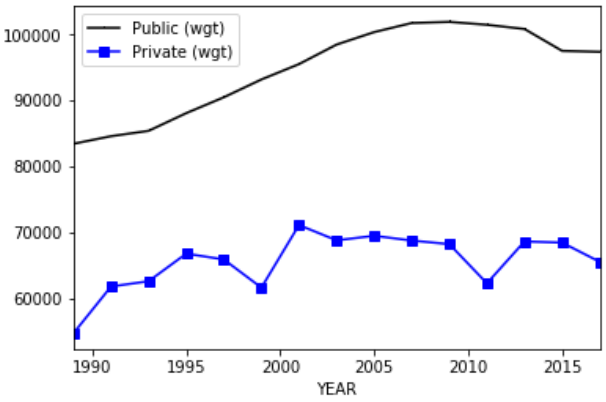
(b) Geographic Variation



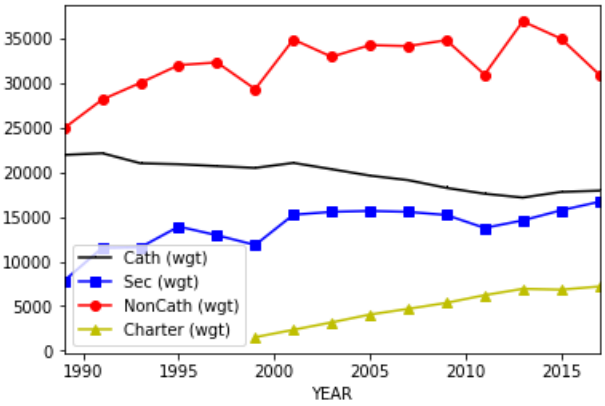
Notes: Count of scandals over time and indicator for if a zipcode ever has a scandal.

Figure 2. Counts of schools by different types of schools

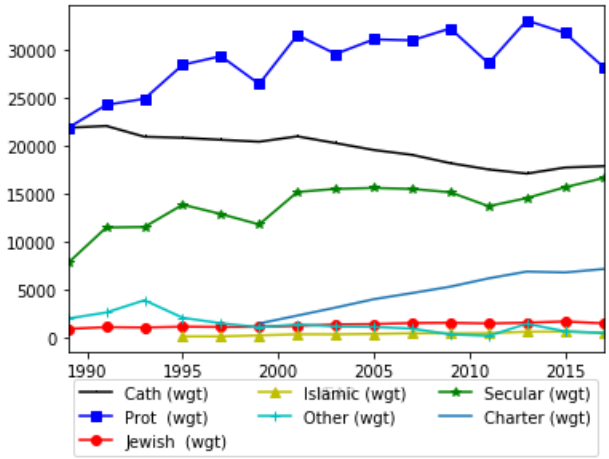
(a) All Counts



(b) Private



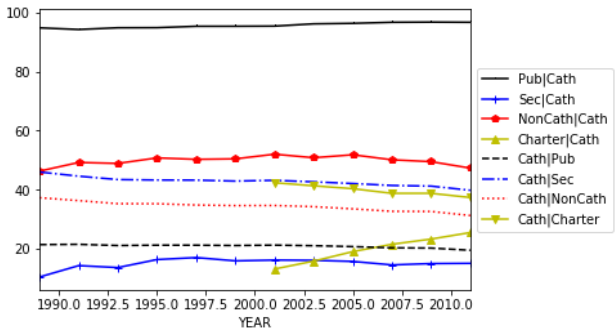
(c) Private Breakdown



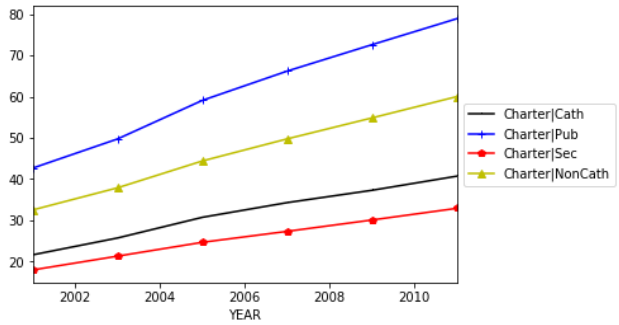
Notes: Figures of counts of schools and overlap of these schools by year. Figure (a) shows private and public school counts. Figure (b) shows private school counts. Figure (c) show Catholic school counts. All counts weighted by the population weights given by the National Center for Education Statistics.

Figure 3. Overlap of different types of schools

(a) Percentage overlaps

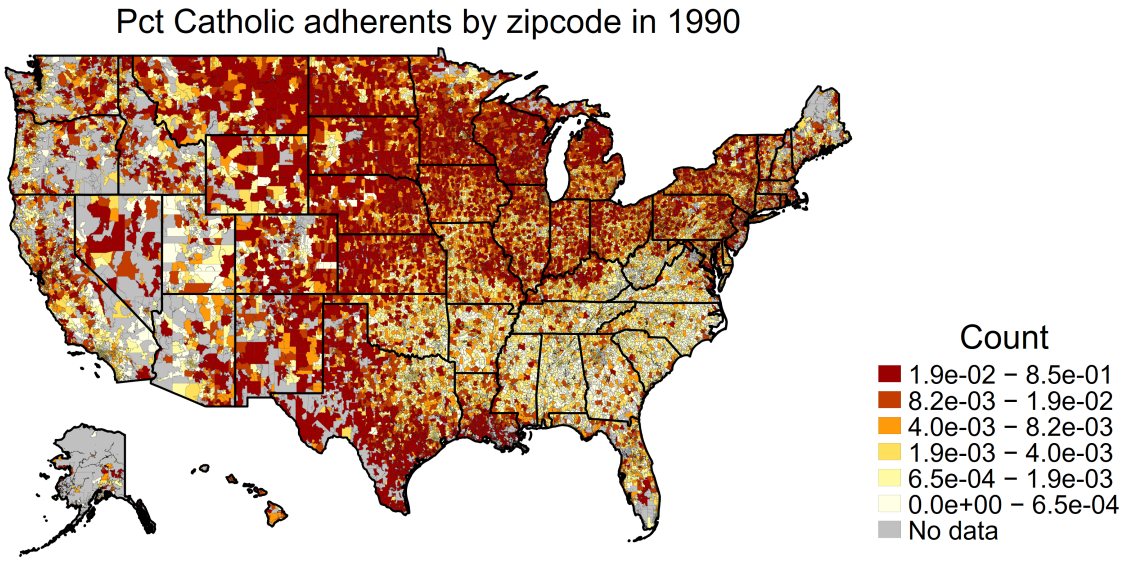


(b) Charter overlaps



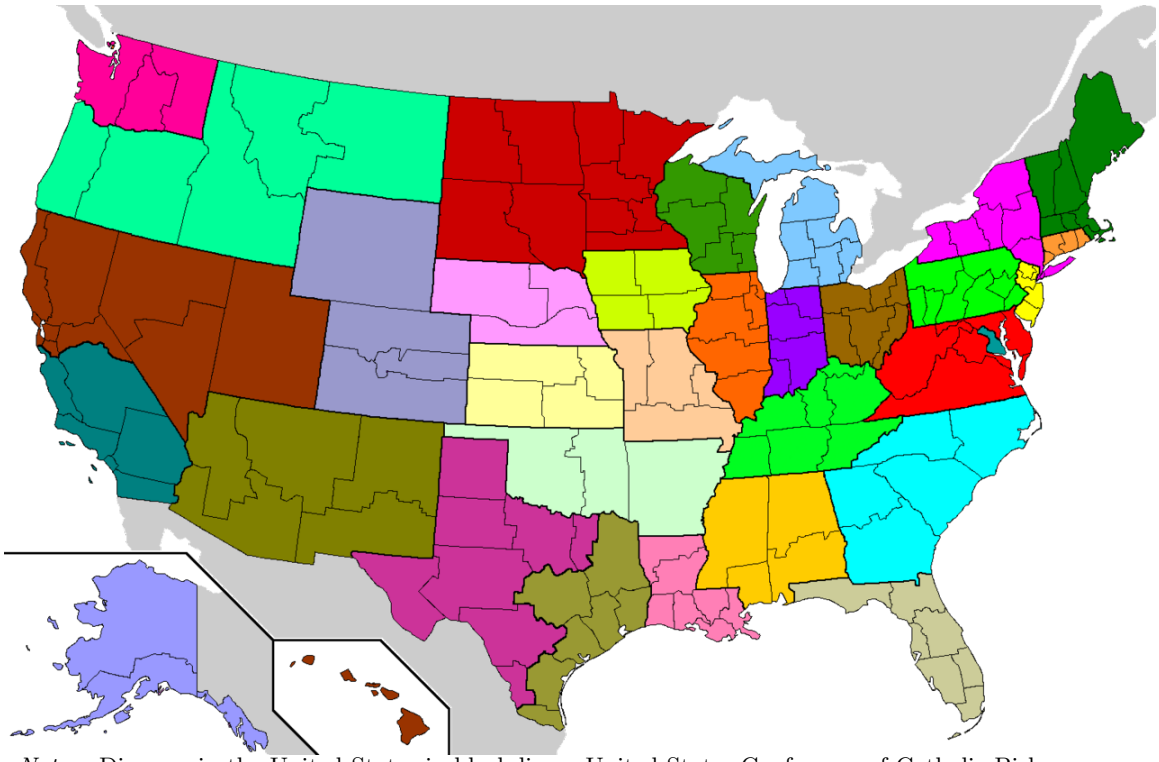
Notes: Figures of counts of overlap of these schools by year. Figure 3 show the share of one school in the same zipcode as another type of school – as indicated in the legend. Figure 3 is the same for charter schools only. All counts weighted by the population weights given by the National Center for Education Statistics.

Figure 4. Catholic adherence by county



Notes: Catholic adherence by county as measured in the 1990 Association of Religious Data Archives.

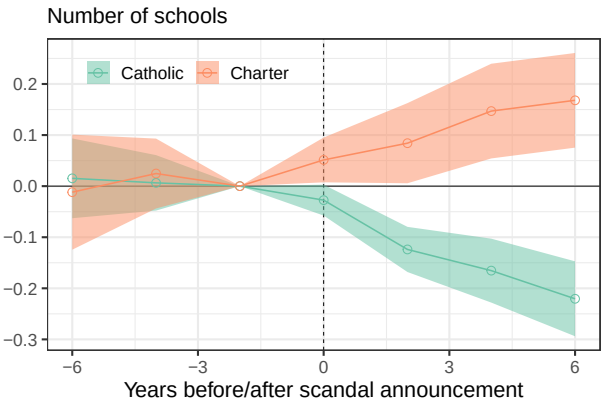
Figure 5. Catholic Diocese Map



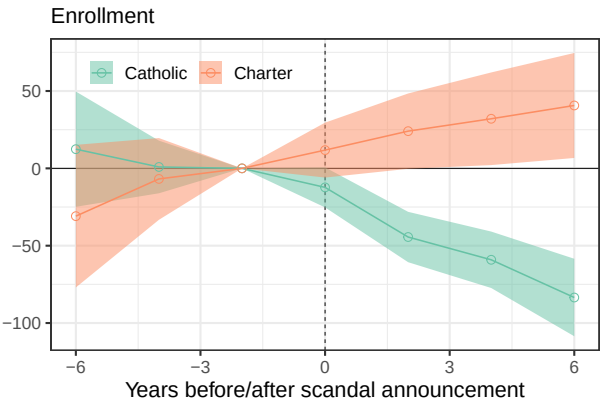
Notes: Dioceses in the United States in black lines. United States Conference of Catholic Bishops areas colored. Source: https://upload.wikimedia.org/wikipedia/commons/7/77/USCCB_Regions_map.png

Figure 6. Event study of count and enrollment at Catholic and charter schools

(a) Counts



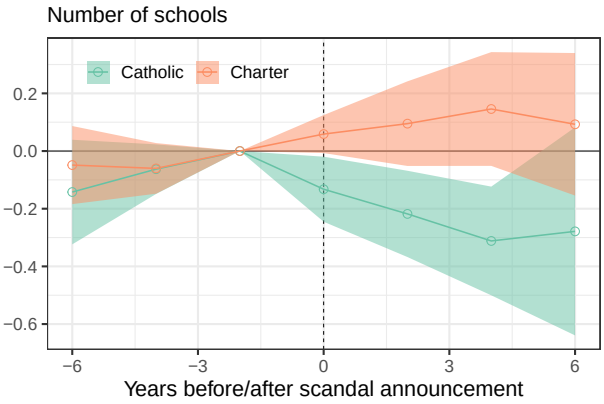
(b) Enrollment



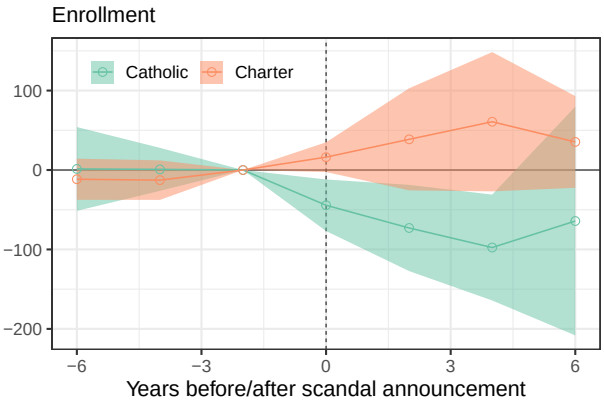
Notes: Event studies of the counts and enrollments at all schools from 2000-2012. Unit of analysis is zip code. Standard errors clustered at diocese.

Figure 7. Event study of count and enrollment at Catholic and charter schools

(a) Counts



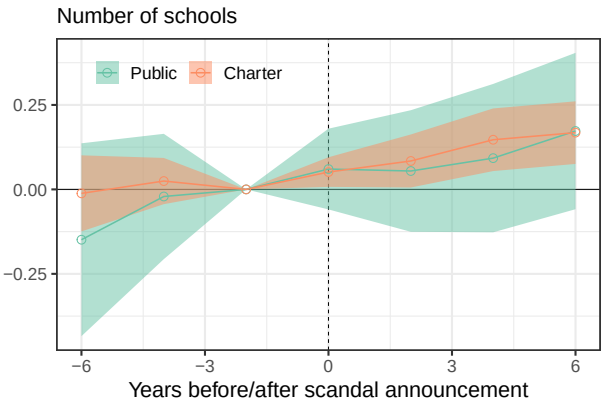
(b) Enrollment



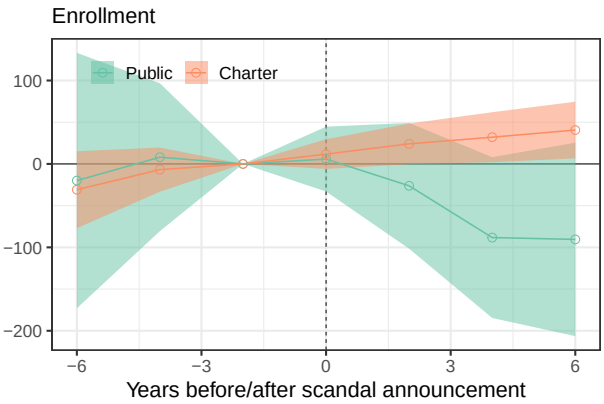
Notes: Event studies of the counts and enrollments at all schools from 2000-2012. Unit of analysis is school district. Standard errors clustered at diocese.

Figure 8. Event study of count and enrollment at all schools

(a) Counts



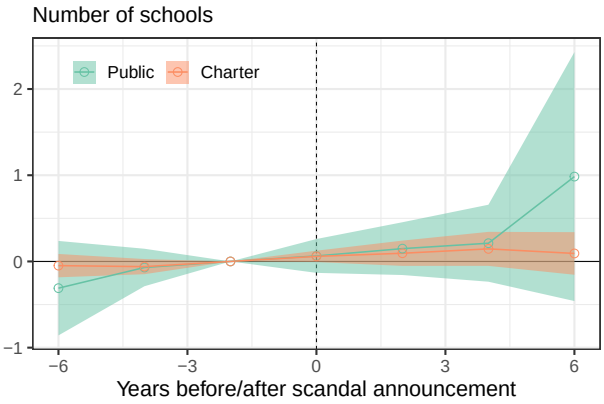
(b) Enrollment



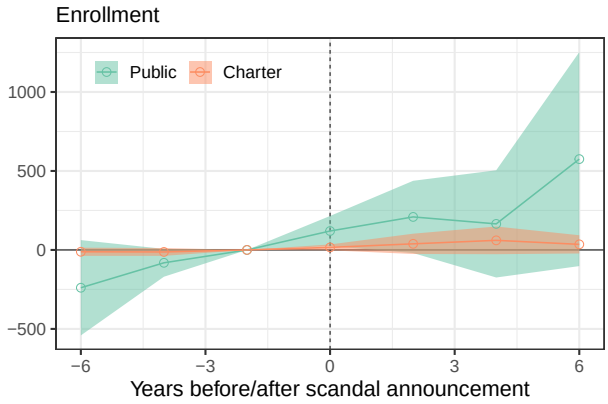
Notes: Event studies of the counts and enrollments at all schools from 2000-2012. Unit of analysis is zip code. Standard errors clustered at diocese.

Figure 9. Event study of count and enrollment at all schools

(a) Counts



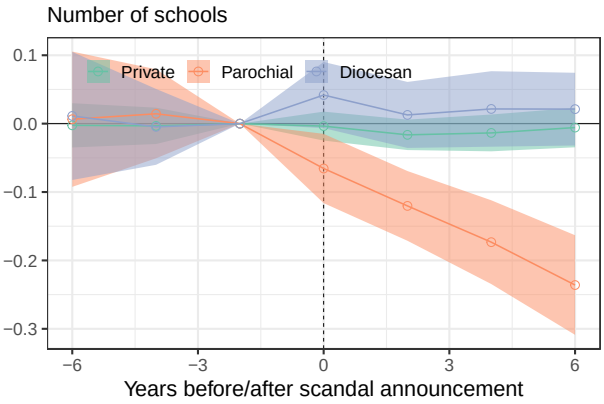
(b) Enrollment



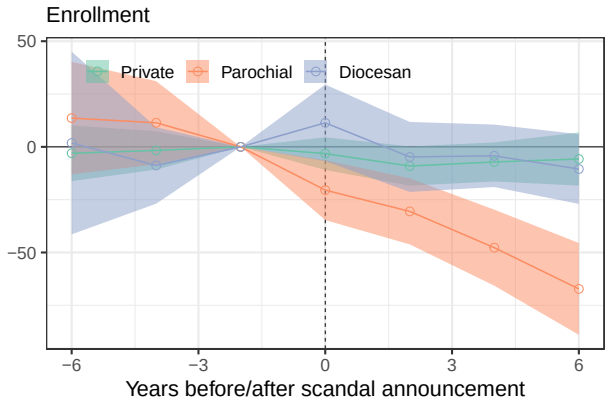
Notes: Event studies of the counts and enrollments at all schools from 2000-2012. Unit of analysis is school district. Standard errors clustered at diocese.

Figure 10. Count and enrollment by type of Catholic school

(a) Counts



(b) Enrollment



Notes: Catholic school counts and enrollment at zipcode level from 2000 to 2010. Standard errors clustered at Diocese level.

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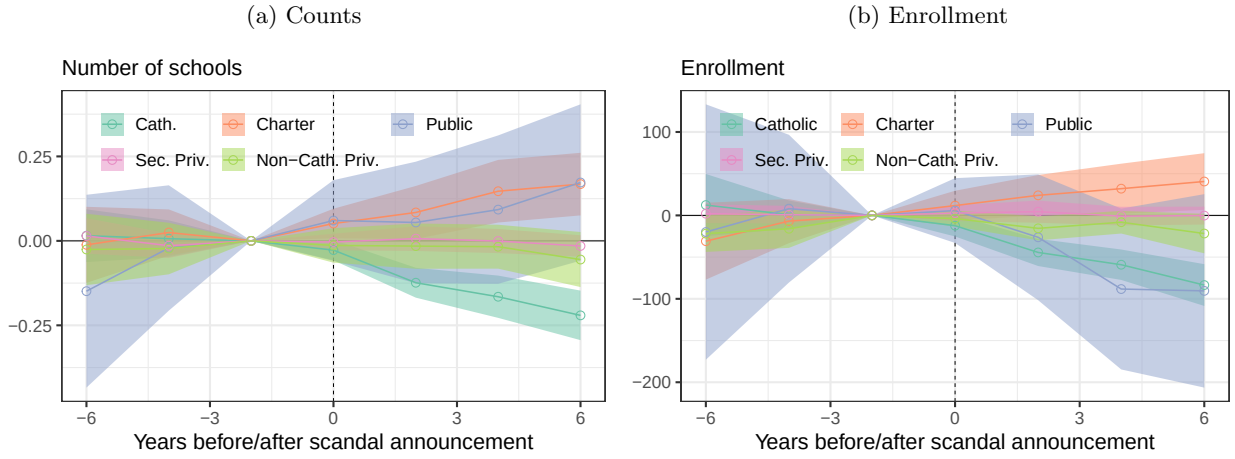
A Figures

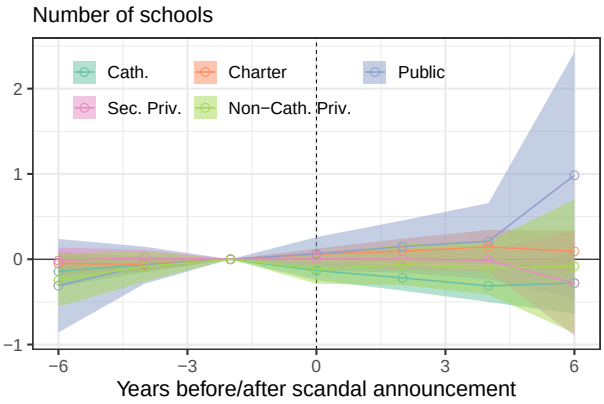
Figure 11. Event study of count and enrollment at all school types



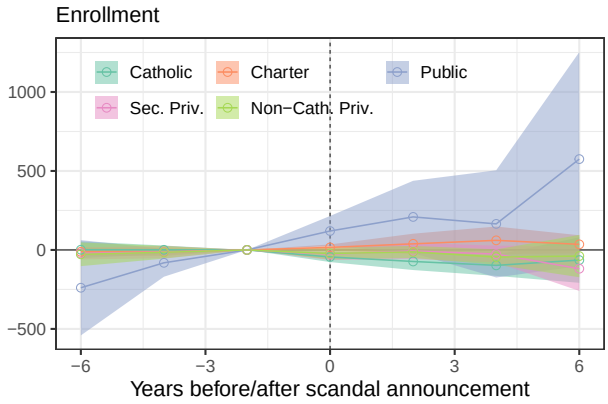
Notes: Event studies of the counts and enrollments at all schools from 2000-2012. Unit of analysis is zip code. Standard errors clustered at diocese.

Figure 12. Event study count and enrollment at all schools

(a) Counts



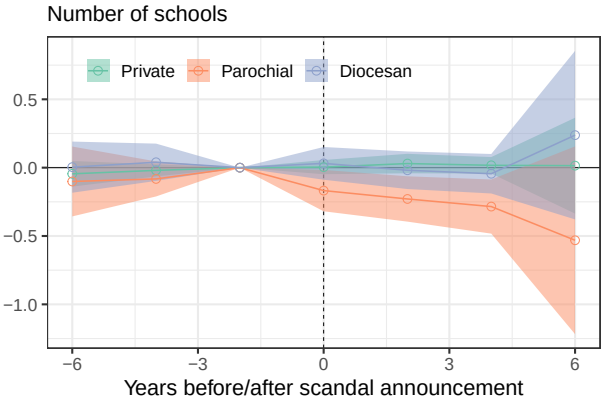
(b) Enrollment



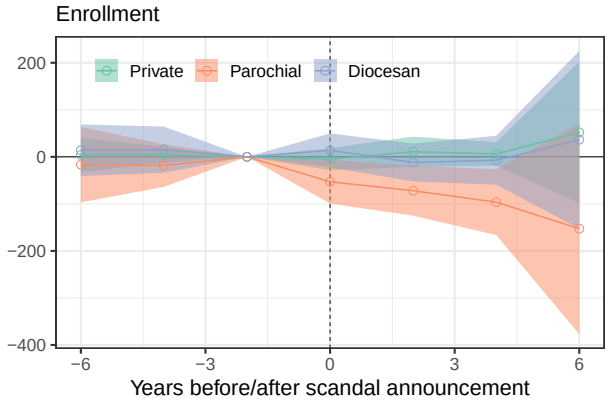
Notes: Event studies of the counts and enrollments at all schools in school district from 2000-2012. Standard errors clustered at diocese.

Figure 13. Count and enrollment by type of Catholic school

(a) Counts



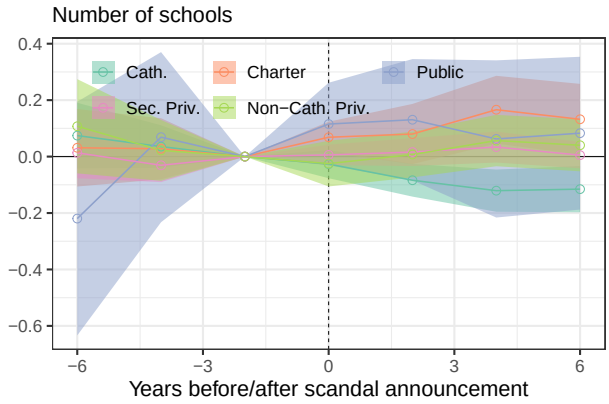
(b) Enrollment



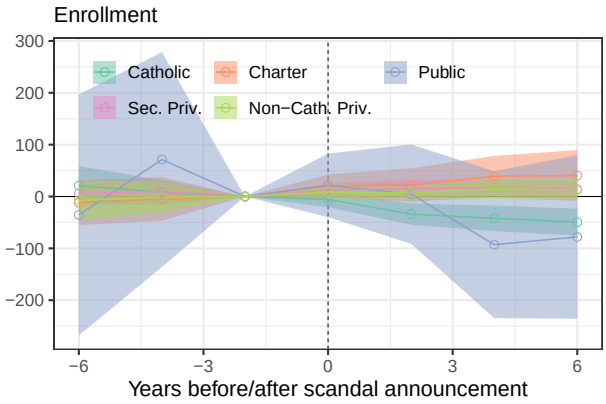
Notes: Catholic school counts and enrollment at school district level from 2000 to 2010. Standard errors clustered at Diocese level.

Figure 14. Event studies of count and enrollment at all schools for Type A scandals

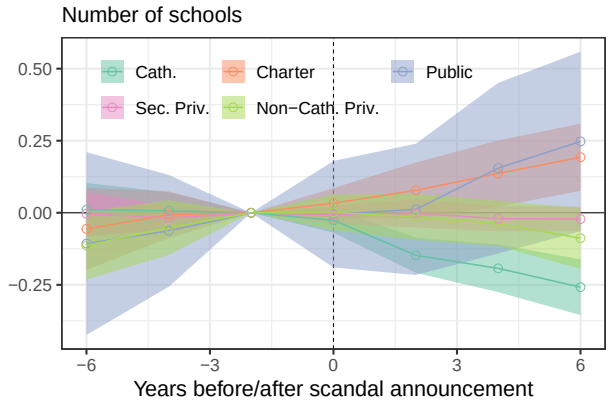
(a) Counts (Type A)



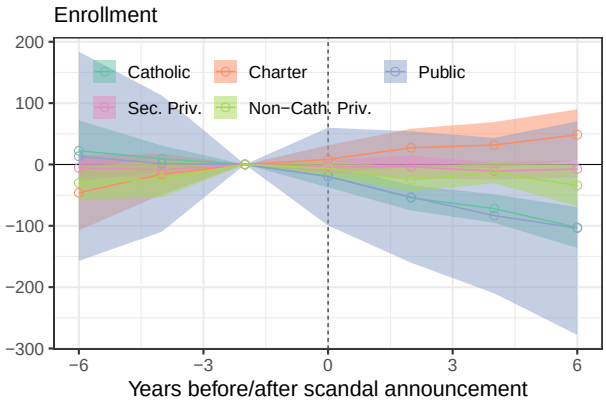
(b) Enrollment (Type A)



(c) Counts (Type B)



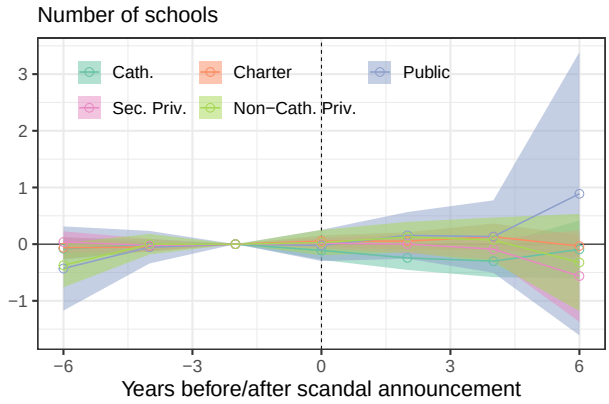
(d) Enrollment (Type B)



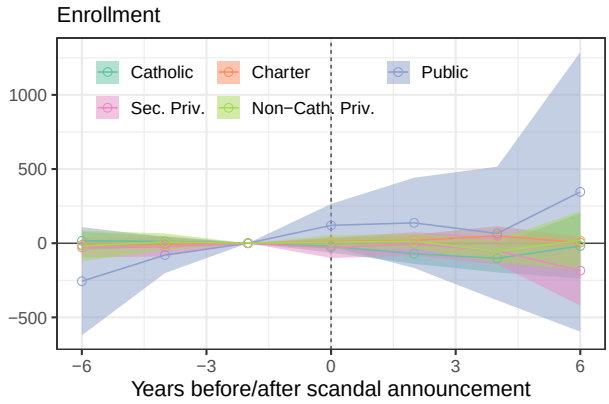
Notes: Event studies of the counts and enrollments at all schools from 2000-2012. Standard errors clustered at diocese.

Figure 15. Event study of count and enrollment at all school types

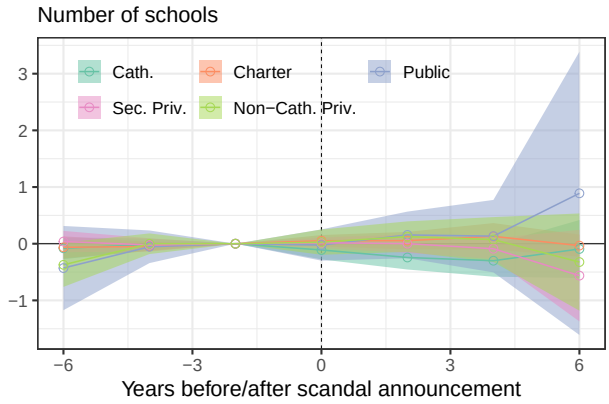
(a) Counts (Type A)



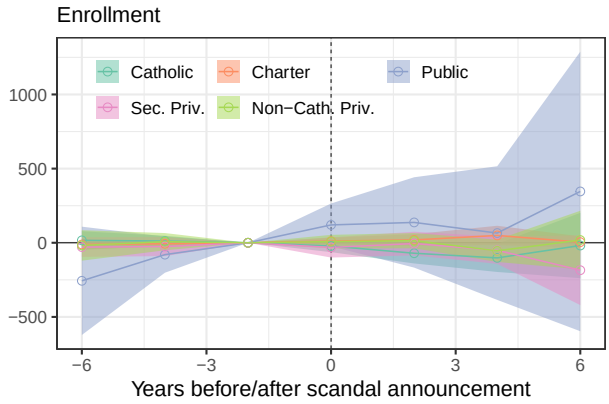
(b) Enrollment (Type A)



(c) Counts (Type B)



(d) Enrollment (Type B)



Notes: Event studies of the counts and enrollments at all schools from 2000-2012. Top row has Type A scandals, second row has Type B scandals. Unit analysis is school district. Standard errors clustered at diocese.

B Sun & Abraham Robustness

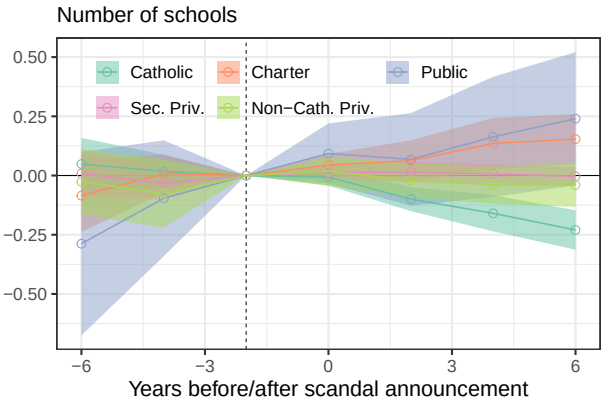
In this section, I implement the Sun and Abraham (2021) estimator of treatment effects with heterogeneous treatment timing and treatment effects. The underlying problem of heterogeneity is that early treated groups are not "clean controls" for later treated groups, but are implicitly used in a vanillatwoway fixed effects OLS estimator. For example, if treatment effects increase in relative time, then the early treated groups will be of greater magnitude than the later treated groups, leading to a negative "treatment" effect on later treated groups on average and biasing treatment effects down.

The Sun and Abraham (2021) estimator addresses this problem by estimating cohort-specific average treatment effects on the treated (CATTs) and then averaging these CATTs across cohorts to get the ATT. The CATTs are the comparison to the untreated group in the sample, here all of the population. Then these CATTS are aggregated across cohorts using non-negative inverse probability weights to get the ATT.

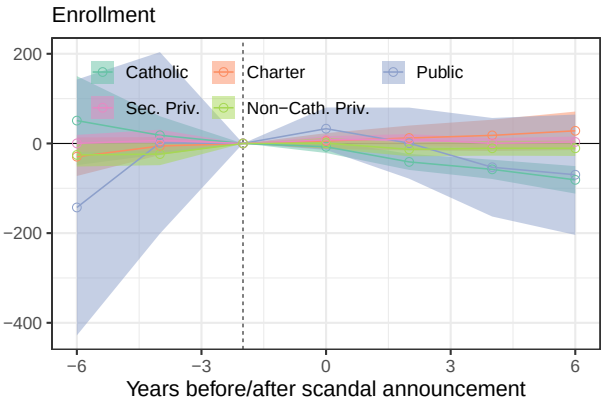
Figure 16 shows the results of the Sun and Abraham (2021) estimator at the zipcode and school district level. These results are qualitatively and quantitatively similar to the results in the main text. Given the large "untreated" or "clean" control group that has not experienced a scandal, it makes sense that the OLS estimator would not suffer from bias due to heterogeneous treatment timing and early-late-treated comparisons. s

Figure 16. Sun & Abraham count and enrollment at all schools

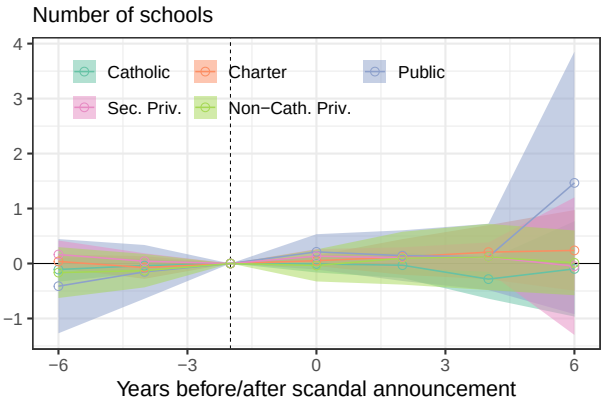
(a) Counts (Zipcode)



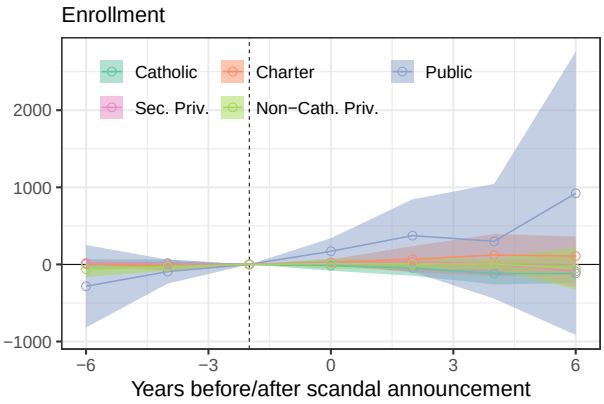
(b) Enrollment (Zipcode)



(c) Counts (School District)



(d) Enrollment (School District)



Notes: Event study using the Sun and Abraham (2021) event study methodology for counts and enrollments at all schools from 2000-2012. Standard errors clustered at diocese.