A Rising Tide for All or Wave for One?: The Effect of Charter School Competition on District Achievement

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Abstract

We analyze the impact of each additional charter school on the achievement of its local school district. We use a national dataset of 3^{rd}–8^{th} grade students in the 2015–2016 school year from the Stanford Educational Opportunity Project and the US Department of Education Common Core of Data. We instrument our specifications with a measure of district location and the strength of state charter laws measured by the Center for Education Reform. Our primary specifications indicate that each additional charter school decreases math achievement by 0.0202SD and decreases ELA achievement by 0.0115SD, declines that are statistically significant at the 5% level and are equivalent to 1-2 weeks of learning. Analysis by racial subgroups indicate that white students are harmed relative to all minority groups, but no differences by gender. These results indicate that while charter schools reduce racial disparities in education, they lower achievement in both math and ELA for all but one subgroup.

Since Minnesota passed the first charter school legislation in 1991, charter schools have grown to serve over 3 million students in the United States. As of 2019, 45 states (alongside DC, Guam, and Puerto Rico) have laws permitting charter schools to form, with nearly 7,500 charter schools serving over 5% of American students [2]. Charter schools are publicly-funded but privately-run, and often are exempt from many of the requirements of traditional public schools. While charters are often praised for their innovative approaches, they also receive criticism for their impact on public school finances, lack of regulation, and under-representation of students with disabilities. Charter schools are incredibly contentious — data from the 2019 Education Next Poll shows that parents are split 50/50 on whether they support charter schools or not [2]. The Harvard Graduate School of Education describes charter schools as "one of today's most contentious debates in education" [18].

What is important from the perspective of educational policy is not just whether charter schools have positive impacts on their own students, but whether they have positive impacts on education at-large, particularly in the districts where they are located. In this paper, we estimate the causal impact of each additional charter school on the academic performance *all* students in their district, not just those that attend the charter school.

We use data from the Educational Opportunity Project at Stanford and the US Department of Education Common Core of Data. Our dataset covers 3^{rd}–8^{th} grade students in 2016 from all public school districts in the United States. A naive estimation may have reverse causality issues, as the decision to create a charter school in a certain district may be in part due to the academic achievement of the existing students. To account for this, we instrument our regressions with the interaction between a district's locale (urban, suburban, and rural) and the strength of the state's charter policy laws, as quantified by the Center for Education Reform.

Our primary specifications show that the entrance of a charter school has small but statistically significant negative impacts on the achievement of its district. A new charter school will decrease the achievement in math of the local school district by 0.020S2D, and decrease achievement in English/Language Arts (ELA) by 0.0115SD. These effects are measured quite precisely, and translate to learning decreases on the order of 1-2 weeks of learning for the typical student in 3^{th}–8^{th} grade [16].

Analysis by racial and gender subgroups shows that charter schools may impact different populations in different ways. White students fare worse due to charter school competition than any group of minority students in both math and ELA. While minorities are relatively benefited, our point estimates of effects are negative for all but one of our specifications by race and subject; as such, we do not believe that charter schools are an optimal policy instrument for reducing racial achievement gaps. No statistically significant differences are found when analyzing the impacts by gender.

This study makes two primary contributions to the literature. First, it is the only study known to the authors to estimate the causal impact of charter competition on traditional public-school achievement using a national dataset. This study furthers existing evidence on the impacts of charter schools in their communities. Hoxby (2003) analyzes charter schools in Michigan and Arizona, showing that charter school competition raises math and reading test scores in the district, describing competition as a "tide that lifts all boats" [3]. Gilraine et al. (2019) find small gains from North Carolina charter school competition caused by increased performance in charters with similar curricula to public schools and increased performance in traditional public schools themselves [14]. Apperson (2010) uses New York City zoning laws to find that competition raises neighborhood student achievement by small amounts, but also causes substantial sorting by race into schools [7]. Griffith (2019) finds correlations between a higher share of students in charter schools and academic performance, especially among urban minorities, using the SEDA dataset that is also used in this paper (see Subsection 1.1) [15]. In a meta-analysis of 11 different studies, Epple et al. report that all 11 studies had neutral to positive effects [13].

Second, this paper furthers existing evidence that the impacts of competition in educational settings are quite limited. Perhaps the most famous paper in this literature is Hoxby (2000), who uses stream density as an instrument for natural school barriers, finding that Tiebout choice raises student achievement while lowering school spending. Card et al. (2010), studying competition between a public school system and a Catholic-only school system in Ontario, find that expanding school choice to all students would increase 6$^{\text{th}}$ grade achievement by $.06 - .08$SD [10].

The remainder of the paper proceeds as follows. Section 1 discusses the sources of data used in this analysis. Section 2 describes our estimation strategy and the motivation behind our instrumental variables approach. Section 3 presents our results of our regressions, including various checks for robustness and discusses its implications. Section 4 concludes.

1 Data Sources

1.1 District and School Data

Our primary data source on school information comes from the Educational Opportunity Project at Stanford University, which created the Stanford Education Data Archive (SEDA) to "help scholars, policymakers, educators, and parents learn how to improve educational opportunity for all children" [20].

The dataset measures educational outcomes across the United States at the school, district, and state level. This data includes the average achievement of each public school (traditional and charter) with grades 3-8 through school years 2008–09 through 2015–16. We analyze data from the 2015-16 school year at the district-grade level – that is, each grade of each district is a separate observation in our analysis. Test score data has been normalized across states and years using data from the National Assessment of Educational Progress (NAEP), in such a way that the average test score for each observation is 0 and that a grade in a district with a score of 1 has students that achieve one standard deviation higher than a typical student in that grade. SEDA also includes test score data grouped by race and gender, enabling subgroup analysis along these dimensions.

SEDA has a rich set of covariates, including racial demographics and categorical variables for the school's location (urban, rural, town, or suburban). SEDA also computes a socioeconomic composite index on a scale with mean 0 and variance 1 with respect to all districts in the United States, using data from the American Community Survey (ACS) and incorporating median income, unemployment rate, SNAP receipt rates, and other similar information.

SEDA additionally redefines school districts in a way that allows us to estimate the impact of charters on district achievement. Many charters are not part of any school district; in SEDA, these schools are "considered to be part of the district in which they are physically located" [5]. While all data was already publicly available, SEDA combines these all into more succinct format.

SEDA does not report data for many district-grade observations for a variety of reasons, which limits the external validity of our analysis. Many "small" districts lack data, as SEDA suppresses data if it represents less than 20 students or if the score normalization created imprecise estimates of test score means, removing the test scores of 34% of districts. In addition, observations with data anomalies, such as those with errors in reporting, incomparable tests, schools that could not be located, and states with low participation rates, are all dropped from the dataset, observations that represent around 6% of the sample. As such, our analysis is done with the caveat that it may not generalize to all schools within the U.S., particularly those with few students.

We use data from the National Center for Education Statistics Common Core of Data (CCD) to find the number of charter and non-charter public schools for each district and grade in the 2015–2016 school year.[1] Of the 258,679 school-grade observations in CCD, all but 414 were able to be located in the SEDA dataset, which appears to be due to random errors in reporting. The remaining 258,265 school-grade observations are a part of 78,738 distinct district-grade observations. We additionally drop 234 virtual school districts, 3,967 with incomplete or missing covariates, and 20,947 with missing math and ELA test results.

[1] SEDA is based on the the ED*Facts* database, though both CCD and ED*Facts* are managed by the U.S. Department of Education. As such, CCD and SEDA can be linked using Locale Education Agency (LEA) numbers.

This leaves 53,590 districts with full controls and either math or ELA reported for the 2015-16 school year that we use for our analysis.

Summary statistics for the variables of interest in the SEDA dataset are included in Table 1. Note that the districts with charters tend to have lower test scores, be in urban areas, have a lower average socioeconomic status, and have larger minority populations.

1.2 State Charter Laws Data

The Center for Education Reform (CER) is an advocacy group that aims to "expand educational opportunities that lead to improved economic outcomes for all Americans" [6]. Since 1996, CER has analyzed charter school laws to see how well they facilitate the creation of new charter schools. In particular, CER has released annual rankings and scorecards of charter school laws every year since 1999 [11]. The policy score is assigned at the state level, not the district level.

These rankings evaluate each law on a scale from 0 to 55, with points awarded depending on how well the state's law allows multiple independent entities that can authorize charter schools, no caps on the number of schools, operational independence, fiscal equity with other schools, and the implementation of the law. These categories are chosen because they "have the most impact on the development and creation of charter schools" [1]. We use CER's 2013 report in this study, normalize scores to a scale from 0 to 1, and summarize its data in Figure 1 [4].

2 Methods

We estimate the causal impact of each additional charter school per grade on the achievement of the same grade in the district where it is located. We do so using the regression

$$TestScore_i = \alpha Charters_i + \beta \mathbf{X}_i + \varepsilon_i$$

Here, i represents a district-grade observation, $TestScore_i$ is the average test score (either math or reading) of the district-grade observation, $Charters_i$ is the number of charter schools present in a district for a given grade, and $\mathbf{X}_i$ is a vector of district-level controls (racial demographics, number of students, socioeconomic status, urban/suburban/town/rural location, grade fixed effects, and state fixed effects). Note that state fixed effects control for $Policy$, as $Policy$ is fixed for each state.

In addition to our main analysis, we perform a subgroup analysis on two dimensions: race and gender. As shown in Table 1, charter schools have higher populations of minority students, and so we may expect effects of charter schools to differ across this subgroup. Note that because of SEDA's data suppression, as described in Subsection 1.1, not all districts will have data reported for each subgroup. For example, consider a district with 100 students in the 8^{th} grade where 50 are white, 30 are black, 10 are Hispanic, and 10 are Asian.Test scores would be available in SEDA for the whole district, white students, and black students, but no data would be available for Hispanic and Asian students.

We restrict each subgroup analysis to observations that report test scores for the relevant subgroup. Our hypothetical district would be included in the analyses for white and black students, but would not be included in analyses for Asian and Hispanic students. As a result, the estimates for each subgroup represent different sets of districts, and the analysis performed for each subgroup may not be directly comparable to the results for other subgroups. These estimates represent the effect of an additional charter school on test scores for a specific subgroup when the population of that subgroup is at least 20.

However, there may be issues with reverse causality — charters may selectively enter into districts where existing schools have lower performance. Further, there may be additional unobserved characteristics of districts that correlate with test scores that impact whether charter school entry occurs. To account for these issues, we instrument $Charters_i$ in all of our specifications using the variable of $Urban_i \times Policy_i$ in a 2SLS approach. Here, $Urban_i$ is an indicator variable for if the district is located in an urban locale. $Policy_i$ is the strength of each state's public charter laws in 2013 (see Subsection 1.2). Note that the nature of this instrument means that we estimate the local average treatment effect (LATE) for urban school districts in states with a high $Policy$ score.

We expect this instrument to be relevant due to the nature of charter school entry. It is well-documented that most charter schools tend to be located in urban areas, as reported by the New York Times [12], the Atlantic [19], and the Brookings Institution [17]. Further, the strength (and existence) of charter policy laws has a clear impact on charter school development. We use a lagged variable for policy because founding charter schools is not an instantaneous process. Researchers from the Peabody College at Vanderbilt University report that founding a charter school is a process that can take 1–2 years [9]. As our school data covers the 2015–16 school year, this implies that the 2013 policy measure will have an appropriate lag. We use the interaction between $Urban_i$ and $Policy_i$, rather than just $Policy_i$, to increase the relevance of our instrument.

We can also explicitly test for relevance by estimating the first stage of this regression, and using an $F-$test of the first-stage to see whether this instrument are strong. We find that this instrument for strong for each of our specifications, and we report the result of relevance tests alongside the main results in Section 3.

We would expect these instruments to be exogenous of if holding the controls fixed, there is no correlation between $Urban_i \times Policy_i$ and $TestScores_i$ except through the number of charter schools. We control for the location of each district directly and $Policy_i$ indirectly through state fixed-effects. As $Policy_i$ is assigned at a state level, $Policy_i$ is collinear with state-fixed effects and including it in our specification is redundant.

The most direct channel by which exogeneity may be violated because of omitted variable bias is that our instrument may be related to other education policies. In particular, districts with a high $Urban \times Policy$ value may be correlated with public school funding, school accountability measures, and curricula choices. However, given the bipartisan support for public schools through 2012, it is plausible that states with a high $Urban \times Policy$ are uncorrelated with other partisan educational policies and thus performance on standardized tests [8]. As such, it seems implausible that this channel could violate the exogeneity assumption.

We also do not believe threats to exogeneity to occur due to reverse causality. First, low test scores in specific areas of a state cause the state to pass charter law policies to target those locations. Given that we are already controlling for the location of each district, this does not appear to be plausible. Second, urban areas with strong charter law policies leads to student sorting across districts, and likewise for different locales and weak charter laws. Given the challenges and costs associated with moving between districts (and doing so solely for a child's schooling), we do not believe this to be a significant factor. Finally, success of said laws *prior* to 2013 may impact both laws in 2013 and current educational outcomes. We believe the impact of this channel to be minimal, given the wide variety of factors that impact the passage of laws, such as government regulations, which party is in power, and the lobbying of advocacy groups.

3 Results

3.1 Primary Results

Results of our main specification is shown in Table 2. The impact of each additional charter school on the achievement of its school district is negative and statistically significant. A new charter school reduces the math achievement of its district by 0.020SD and decreases the district's ELA achievement by 0.012SD. To give context for these estimates, the typical 3^{th}–8^{th} grade student learns 0.30SD in a given school year [16]. As such, these learning decreases are equivalent to 2 weeks and 1 weeks of school, respectively. While the negative impacts of each additional charter school are relatively small, they are significant at the 5% level.

In addition, we note the validity of our instruments in both specifications. The math specification has a first-stage $F = 166.4$, while for the ELA specification has $F = 159.3$. These results show that our instrument is quite strong and causes much variation in the number of charter schools.

We can additionally perform a robustness check of these results using two additional instruments – $Suburban \times Policy$ and $Rural \times Policy$, defined in the same way as $Urban \times Policy$ but with a different indicator variable for location. Given these three instruments, we can test the consistency of results using all possible combinations of these three instruments. Results of the robustness checks for the math specification are reported in Table 3, while those for ELA are reported in Table 4.

For math, the results of our main specification are robust to using different sets of instruments; all specifications estimate that the impact of a new charter school is close to -0.0202SD, though not all are statistically distinguishable from 0. In addition, all specifications but one pass the $F-$test. The specification which fails is the one using only

Suburban × *Policy* as an instrument, which may be due to limited numbers of charters in suburban areas.

However, for ELA scores, our results are less consistent, with only 3 of the 6 new regressions having impacts of a new charter school close in magnitude to -0.0115SD. Note that the specifications using only *Suburban* × *Policy* and using only *Rural* × *Policy* both have positive results, indicating that there are heterogeneous effects of charter schools in urban, suburban, and rural locations. This result is not surprising, as some anecdotal evidence already exists for this phenomenon [17,19]. Again, the lone specification that fails the $F-$test is instrumented using only *Suburban* × *Policy*, likely for the same reason as that this instrument failed in the robustness checks for math.

3.2 Effects by Race

In Table 5, we report results the effect of charter schools on Math and ELA achievement for white, black, Hispanic, and Asian students. These results are also depicted graphically in Figure 2.

We observe significant changes in the sample size of each specification. As noted in Subsection 1.1 and Section 2, this is because our data set only includes test scores for a racial subgroup when that subgroup has at least 20 students, among other factors. As such, each specification contains a different set of district-grade observations. In addition, we note that as $F > 10$ for each of our specifications, our instrument is strong even when performing our analysis by racial subgroups.

In math, the largest difference between two subgroups is between Asian and white students; white students are harmed by an additional charter school in math by nearly two weeks of achievement relative to Asian students, though this difference is not statistically significant. Further, white students are harmed relative to all minority groups because of charter school competition, but no difference is statistically significant. Perhaps most importantly, for all subgroups, the impact of an additional charter school on district achievement is negative, indicating that charter schools lower achievement in mathematics across the board.

Turning to ELA achievement, we find even larger differences between racial subgroups. In particular, the difference in effects of charter school competition for black and white students is 0.0325SD ($p = 0.001$), a difference equivalent to a month of learning in ELA for the typical student. Further, the point estimate for the impact on black student achievement in ELA is *positive*, albeit statistically indistinguishable from 0. Smaller differences occur between all other subgroup pairs, but we find that on charter schools benefit minority students, particularly black students, relative to white students. However, the net effect on district achievement in ELA is negative, and all subgroups have either negative impacts or impacts statistically indistinguishable from 0.

In summary, we find that white students are harmed from charter school competition relative to minority students, indicating that charter schools may reduce existing racial achievement gaps. However, the net effect of charter schools (and for almost all subgroups) is *negative*. As such, we do not find that charter schools are an effective policy intervention on the basis of increasing educational equity.

3.3 Effects by Gender

Results for subgroup analysis by gender are shown in Table 6 and Figure 3. We again have that our instrument is relevant for each subgroup that we consider, with $F > 150$ for each specification. In addition, the difference in sample sizes for these specifications is much smaller than those for our subgroup analysis by gender.

The effect of each charter school on math and ELA achievement is remarkably similar for both male and female students. While point estimates indicate that females fare slightly worse than males because of charter schools, the differences are not statistically significant at the 5% level. Further, our estimated differences are incredibly small in magnitude and are equivalent to just around one day of learning in the typical 180-day school year for ELA, and one-fifth of a day of learning in math [16]. As such, policy-makers should not be concerned about any differential impacts of charters on each gender.

4 Conclusion

We find small but statistically significant declines in math and ELA scores caused by the entrance of each additional charter school. Subgroup analysis indicates that white students are impacted more negatively than students of other minority groups. Results indicate that charter schools reduce racial achievement gaps, but do so by lowering achievement for all students. No such differences exist when disaggregating test scores by gender.

Our results run counter to the bulk of research that exists for charter schools, which show small but positive gains when a charter school enters. Future research is needed to determine the factors which lead certain school districts to benefit positively from charter school competition, and whether this is something that can be influenced by policy. Further analysis is also needed regarding the equity impacts of charter school legislation. In this study, we find that charter schools negatively impact white students compared to minorities. Careful econometric analysis is needed to further understand the welfare impacts of charter school policy, especially given the high minority population that typically enrolls in charter schools.

It is imperative that the results of studies like these are not interpreted in a vacuum; the performance of students on standardized is just one of many outcomes that all schools try to impact. Schools also try to develop pro-social behaviors and career motivation, impacts which we are unable to study. Further, given the nature of charter school finances (privately operated but publicly funded), charter schools may have desirable impacts on school finances. Analyzing the impact of test scores is but one factor that policy-makers should use when considering public charter legislation.

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Table 1: Summary statistics of SEDA dataset for 3rd–8th grade students in the 2015–16 school year. Observations are at the district-grade level. Data construction is described in Subsection 1.1.

	All Schools			No Charters			Contains Charters		
	mean	sd	median	mean	sd	median	mean	sd	median
Outcome Variables									
Math Scores	0.02	0.44	0.02	0.04	0.44	0.04	-0.14	0.41	-0.13
ELA Scores	0.05	0.39	0.06	0.07	0.39	0.07	-0.08	0.36	-0.07
Regressors									
Number of Charters	0.34	2.57	0.00	0.00	0.00	0.00	3.11	7.14	1.00
Number of Schools	3.68	11.24	1.00	2.29	3.89	1.00	14.84	29.64	7.00
Total Enrollment (100s)	3.47	11.07	1.17	2.14	4.22	1.02	14.16	28.85	6.06
Socioeconomic Status	0.20	0.91	0.26	0.23	0.89	0.28	-0.08	1.02	0.01
Urban	0.07	0.25	0.00	0.04	0.19	0.00	0.29	0.45	0.00
Suburban	0.22	0.41	0.00	0.20	0.40	0.00	0.32	0.47	0.00
Town	0.21	0.41	0.00	0.22	0.41	0.00	0.17	0.37	0.00
Rural	0.50	0.50	1.00	0.54	0.50	1.00	0.22	0.42	0.00
White (%)	0.72	0.28	0.84	0.75	0.27	0.86	0.50	0.29	0.51
Black (%)	0.08	0.17	0.02	0.07	0.16	0.01	0.17	0.23	0.06
Hispanic (%)	0.15	0.22	0.06	0.14	0.20	0.05	0.28	0.26	0.19
Asian (%)	0.02	0.05	0.01	0.02	0.05	0.01	0.04	0.06	0.02
Native American (%)	0.02	0.10	0.00	0.02	0.10	0.00	0.01	0.05	0.00
Observations	53590			47644			5946		

Notes: District-grade level observations. Does not include virtual school districts, incomplete covariates, or observations with no reported math or ELA test scores. More details about dropped observations in Subsection 1.1. Source: Stanford Education Data Archive (SEDA) [5].

Table 2: The impact of the number of charter schools per grade on district-wide academic achievement. Achievement is normalized nationally to mean 0 and each unit represents a standard deviation of achievement. Both specifications are instrumented with a measure of location and policy in a 2SLS approach, as described in Section 2. Results are interpreted in Subsection 3.1.

	(1) Math	(2) ELA
Number of Charters	-0.0202***	-0.0115**
	(0.00489)	(0.00416)
Total Enrollment	0.0000157**	0.00000576
	(0.00000599)	(0.00000370)
Socioeconomic Status	0.233***	0.210***
	(0.00247)	(0.00201)
Number of Schools	0.00213**	0.00158*
	(0.000804)	(0.000621)
Urban	0.0123	0.0362***
	(0.00635)	(0.00494)
Suburban	0.0293***	0.0560***
	(0.00488)	(0.00399)
Rural	-0.0502***	-0.0336***
	(0.00349)	(0.00286)
White (%)	0.340***	0.365***
	(0.0268)	(0.0203)
Black (%)	-0.150***	-0.0520*
	(0.0292)	(0.0219)
Hispanic (%)	-0.0236	-0.00644
	(0.0273)	(0.0206)
Asian (%)	1.268***	1.089***
	(0.0427)	(0.0359)
Observations	50385	52988
Adjusted R^2	0.541	0.576
First-stage F	166.4	159.3
State/Grade Fixed Effects	yes	yes

Notes: District-grade level observations. Due to data suppression, each regression considers only district-grade observations which report scores; see Subsection 1.1 for more information. Each specification instrumented with $Urb \times Pol$ as described in Section 2. Robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3: Robustness checks for our main specification for math scores. Each column uses a different set of instruments in a 2SLS approach. Column (1) is identical to column (1) of Table 2. Results are interpreted in Subsection 3.1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of Charters	-0.0202***	-0.0183	-0.200	-0.0202***	-0.0217***	-0.0115	-0.0216***
	(0.00489)	(0.0160)	(0.193)	(0.00489)	(0.00489)	(0.0153)	(0.00489)
Total Enrollment	0.0000157**	0.0000146	0.000120	0.0000157**	0.0000166**	0.0000107	0.0000166**
	(0.00000599)	(0.0000104)	(0.000126)	(0.00000597)	(0.00000628)	(0.00000947)	(0.00000627)
Socioeconomic Status	0.233***	0.233***	0.212***	0.233***	0.233***	0.234***	0.233***
	(0.00247)	(0.00300)	(0.0232)	(0.00247)	(0.00247)	(0.00294)	(0.00247)
Number of Schools	0.00213**	0.00187	0.0268	0.00212**	0.00233**	0.000934	0.00233**
	(0.000804)	(0.00224)	(0.0273)	(0.000805)	(0.000826)	(0.00211)	(0.000826)
Urban	0.0123	0.0127	-0.0314	0.0123	0.0119	0.0144*	0.0119
	(0.00635)	(0.00736)	(0.0577)	(0.00635)	(0.00642)	(0.00706)	(0.00642)
Suburban	0.0293***	0.0300***	-0.0291	0.0294***	0.0289***	0.0322***	0.0289***
	(0.00488)	(0.00695)	(0.0653)	(0.00488)	(0.00488)	(0.00679)	(0.00488)
Rural	-0.0502***	-0.0503***	-0.0390**	-0.0502***	-0.0501***	-0.0507***	-0.0501***
	(0.00349)	(0.00361)	(0.0129)	(0.00349)	(0.00349)	(0.00360)	(0.00349)
White (%)	0.340***	0.339***	0.426***	0.340***	0.340***	0.336***	0.340***
	(0.0268)	(0.0282)	(0.0983)	(0.0268)	(0.0268)	(0.0281)	(0.0268)
Black (%)	-0.150***	-0.153***	0.0878	-0.150***	-0.148***	-0.162***	-0.148***
	(0.0292)	(0.0363)	(0.260)	(0.0292)	(0.0292)	(0.0357)	(0.0292)
Hispanic (%)	-0.0236	-0.0232	-0.0615	-0.0236	-0.0239	-0.0218	-0.0239
	(0.0273)	(0.0274)	(0.0513)	(0.0273)	(0.0273)	(0.0274)	(0.0273)
Asian (%)	1.268***	1.271***	1.012***	1.268***	1.266***	1.281***	1.266***
	(0.0427)	(0.0472)	(0.280)	(0.0427)	(0.0427)	(0.0468)	(0.0427)
Observations	50385	50385	50385	50385	50385	50385	50385
Adjusted R^2	0.541	0.542	0.124	0.541	0.541	0.544	0.541
State/Grade Fixed Effects	yes	yes	yes	yes	yes	yes	yes
Instrumented with $Urb \times Pol$	yes	no	no	yes	yes	no	yes
Instrumented with $Rur \times Pol$	no	yes	no	yes	no	yes	yes
Instrumented with $Sub \times Pol$	no	no	yes	no	yes	yes	yes
First-stage F	166.4	159.4	1.778	97.00	113.7	80.02	78.34

Notes: District-grade level observations. Due to data suppression, each regression considers only district-grade observations which report scores; see Subsection 1.1 for more information. Each specification instrumented with the set of instruments indicated. Robust standard errors in parentheses.

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

Table 4: Robustness checks for our main specification for math scores. Each column uses a different set of instruments in a 2SLS approach. Column (1) is identical to column (2) of Table 2. Results are interpreted in Subsection 3.1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of Charters	-0.0115**	0.0122	0.0881	-0.0103*	-0.00895*	0.00932	-0.00888*
	(0.00416)	(0.0124)	(0.0516)	(0.00414)	(0.00405)	(0.0123)	(0.00405)
Total Enrollment	0.00000576	-0.00000741	-0.0000497	0.00000511	0.00000435	-0.00000581	0.00000431
	(0.00000370)	(0.00000759)	(0.0000342)	(0.00000353)	(0.00000332)	(0.00000732)	(0.00000331)
Socioeconomic Status	0.210***	0.213***	0.221***	0.210***	0.210***	0.212***	0.210***
	(0.00201)	(0.00240)	(0.00640)	(0.00201)	(0.00201)	(0.00238)	(0.00201)
Number of Schools	0.00158*	-0.00166	-0.0120	0.00142*	0.00123*	-0.00127	0.00122*
	(0.000621)	(0.00174)	(0.00759)	(0.000609)	(0.000588)	(0.00171)	(0.000588)
Urban	0.0362***	0.0413***	0.0576***	0.0364***	0.0367***	0.0406***	0.0367***
	(0.00494)	(0.00568)	(0.0169)	(0.00491)	(0.00487)	(0.00558)	(0.00487)
Suburban	0.0560***	0.0638***	0.0890***	0.0563***	0.0568***	0.0629***	0.0568***
	(0.00399)	(0.00559)	(0.0181)	(0.00398)	(0.00395)	(0.00556)	(0.00395)
Rural	-0.0336***	-0.0351***	-0.0398***	-0.0337***	-0.0338***	-0.0349***	-0.0338***
	(0.00286)	(0.00294)	(0.00438)	(0.00286)	(0.00286)	(0.00293)	(0.00286)
White (%)	0.365***	0.354***	0.320***	0.364***	0.363***	0.355***	0.363***
	(0.0203)	(0.0213)	(0.0315)	(0.0204)	(0.0203)	(0.0212)	(0.0203)
Black (%)	-0.0520*	-0.0815**	-0.176*	-0.0534*	-0.0551*	-0.0779**	-0.0552*
	(0.0219)	(0.0269)	(0.0691)	(0.0220)	(0.0219)	(0.0267)	(0.0219)
Hispanic (%)	-0.00644	-0.00259	0.00977	-0.00625	-0.00603	-0.00306	-0.00602
	(0.0206)	(0.0207)	(0.0230)	(0.0206)	(0.0206)	(0.0207)	(0.0206)
Asian (%)	1.089***	1.128***	1.254***	1.091***	1.093***	1.123***	1.093***
	(0.0359)	(0.0407)	(0.0933)	(0.0359)	(0.0359)	(0.0405)	(0.0358)
Observations	52988	52988	52988	52988	52988	52988	52988
Adjusted R^2	0.576	0.575	0.469	0.576	0.577	0.576	0.577
State/Grade Fixed Effects	yes	yes	yes	yes	yes	yes	yes
Instrumented with $Urb \times Pol$	yes	no	no	yes	yes	no	yes
Instrumented with $Rur \times Pol$	no	yes	no	yes	no	yes	yes
Instrumented with $Sub \times Pol$	no	no	yes	no	yes	yes	yes
First-stage F	159.3	168.3	7.997	98.53	112.4	84.47	76.98

Notes: District-grade level observations. Due to data suppression, each regression considers only district-grade observations which report scores; see Subsection 1.1 for more information. Each specification instrumented with the set of instruments indicated. Robust standard errors in parentheses.

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

Table 5: The impact of the number of charter schools per grade on district-wide academic achievement, broken down by racial subgroups. Achievement is normalized nationally to mean 0 and each unit represents a standard deviation of achievement. Both specifications are instrumented with a measure of location and policy in a 2SLS approach, as described in Section 2. Results are interpreted in Subsection 3.2.

	Math				ELA			
	(1) White	(2) Black	(3) Hispanic	(4) Asian	(5) White	(6) Black	(7) Hispanic	(8) Asian
Number of Charters	-0.0192**	-0.0166	-0.00503	-0.00452	-0.0243***	0.00821	-0.00994	-0.0156
	(0.00652)	(0.00866)	(0.00602)	(0.0121)	(0.00719)	(0.00766)	(0.00618)	(0.0158)
Total Enrollment	0.0000102*	0.0000175*	-0.00000255	0.00000805	0.00000945	0.00000875	0.00000370	0.0000250
	(0.00000514)	(0.00000711)	(0.00000494)	(0.0000124)	(0.00000567)	(0.00000571)	(0.00000507)	(0.0000140)
Socioeconomic Status	0.274***	0.133***	0.122***	0.398***	0.242***	0.152***	0.119***	0.331***
	(0.00273)	(0.00551)	(0.00436)	(0.0115)	(0.00233)	(0.00465)	(0.00402)	(0.0102)
Number of Schools	0.00389**	0.00130	0.000988	0.00131	0.00498***	-0.00237*	0.00141	0.00179
	(0.00127)	(0.00127)	(0.000839)	(0.00142)	(0.00123)	(0.00121)	(0.000864)	(0.00223)
Urban	0.0731***	-0.0363***	0.0131	0.172***	0.0837***	0.00537	0.0256***	0.121***
	(0.00765)	(0.0107)	(0.00845)	(0.0295)	(0.00677)	(0.00913)	(0.00764)	(0.0292)
Suburban	0.00625	0.0161	0.0688***	0.120***	0.0196***	0.0789***	0.0899***	0.0892**
	(0.00549)	(0.0123)	(0.00875)	(0.0302)	(0.00490)	(0.0109)	(0.00809)	(0.0322)
Rural	-0.0674***	-0.0302***	0.00719	0.0755*	-0.0503***	0.00139	0.0124	0.0546
	(0.00374)	(0.00863)	(0.00755)	(0.0316)	(0.00312)	(0.00747)	(0.00640)	(0.0299)
White (%)	-0.217***	0.0571	0.229*	5.294***	-0.185***	-0.0347	0.289***	5.247***
	(0.0431)	(0.128)	(0.0940)	(0.703)	(0.0395)	(0.138)	(0.0857)	(0.658)
Black (%)	-0.0602	-0.0569	-0.00163	5.425***	0.0208	-0.0577	0.145	5.417***
	(0.0460)	(0.128)	(0.0944)	(0.704)	(0.0418)	(0.138)	(0.0864)	(0.655)
Hispanic (%)	-0.174***	-0.105	-0.0107	5.245***	-0.139***	-0.100	0.0397	5.221***
	(0.0439)	(0.126)	(0.0927)	(0.692)	(0.0402)	(0.137)	(0.0846)	(0.651)
Asian (%)	0.308***	0.346*	0.342***	5.722***	0.304***	0.241	0.369***	5.321***
	(0.0598)	(0.139)	(0.102)	(0.700)	(0.0579)	(0.146)	(0.0942)	(0.658)
Observations	43070	10771	14793	5778	45377	11434	15569	5404
Adjusted R^2	0.420	0.407	0.395	0.598	0.415	0.426	0.394	0.577
First-stage F	127.0	60.87	116.4	51.71	138.4	58.72	101.2	34.02
State/Grade Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes

Notes: District-grade level observations. Due to data suppression, each regression considers only district-grade observations which report scores; see Subsection 1.1 for more information. Each specification instrumented with $Urb \times Pol$ as described in Section 2. Robust standard errors in parentheses.

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

Table 6: The impact of the number of charter schools per grade on district-wide academic achievement, broken down by gender subgroups. Achievement is normalized nationally to mean 0 and each unit represents a standard deviation of achievement. Both specifications are instrumented with a measure of location and policy in a 2SLS approach, as described in Section 2. Results are interpreted in Subsection 3.3.

	Math		ELA	
	(1) Male	(2) Female	(3) Male	(4) Female
Number of Charters	-0.0192***	-0.0195***	-0.0119*	-0.0143***
	(0.00522)	(0.00493)	(0.00464)	(0.00430)
Total Enrollment	0.0000128*	0.0000158**	0.00000502	0.00000708
	(0.00000620)	(0.00000596)	(0.00000399)	(0.00000423)
Socioeconomic Status	0.247***	0.218***	0.208***	0.208***
	(0.00273)	(0.00261)	(0.00223)	(0.00219)
Number of Schools	0.00212*	0.00199*	0.00156*	0.00204**
	(0.000857)	(0.000796)	(0.000694)	(0.000677)
Urban	0.0237***	0.00484	0.0395***	0.0330***
	(0.00669)	(0.00646)	(0.00535)	(0.00518)
Suburban	0.0385***	0.0265***	0.0599***	0.0547***
	(0.00528)	(0.00514)	(0.00438)	(0.00430)
Rural	-0.0472***	-0.0397***	-0.0264***	-0.0279***
	(0.00391)	(0.00383)	(0.00326)	(0.00317)
White (%)	0.343***	0.251***	0.386***	0.326***
	(0.0322)	(0.0290)	(0.0242)	(0.0231)
Black (%)	-0.199***	-0.212***	-0.0688**	-0.0805**
	(0.0342)	(0.0309)	(0.0257)	(0.0245)
Hispanic (%)	-0.0370	-0.110***	0.00660	-0.0672**
	(0.0321)	(0.0290)	(0.0242)	(0.0232)
Asian (%)	1.286***	1.140***	1.082***	1.043***
	(0.0487)	(0.0444)	(0.0398)	(0.0391)
Observations	41567	40682	43592	43000
Adjusted R^2	0.586	0.547	0.589	0.602
First-stage F	157.6	160.8	150.1	154.0
State/Grade Fixed Effects	yes	yes	yes	yes

Notes: District-grade level observations. Due to data suppression, each regression considers only district-grade observations which report scores; see Subsection 1.1 for more information. Each specification instrumented with $Urb \times Pol$ as described in Section 2. Robust standard errors in parentheses.

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

Figure 1: Distribution of Center for Education Reform charter school law scores (normalized) for each district-grade observation, as described in Subsection 1.2. Scores are assigned at the state level. Source: Center for Education Reform [4].

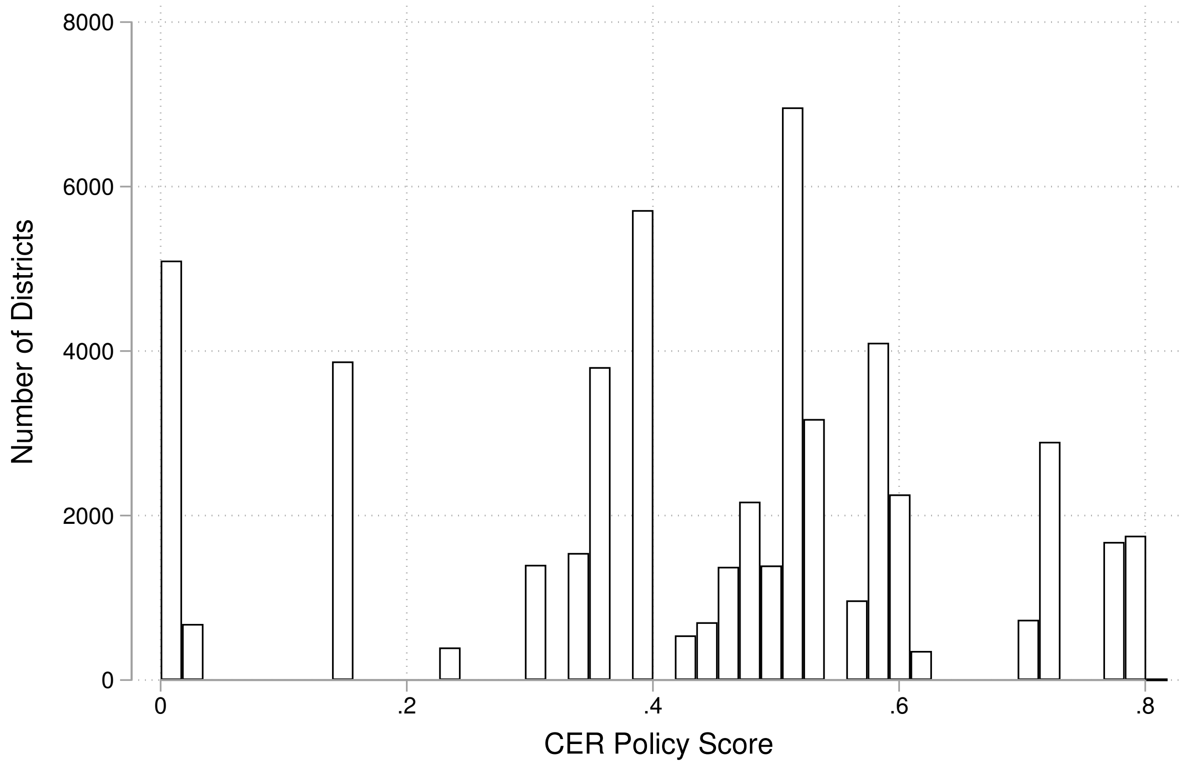
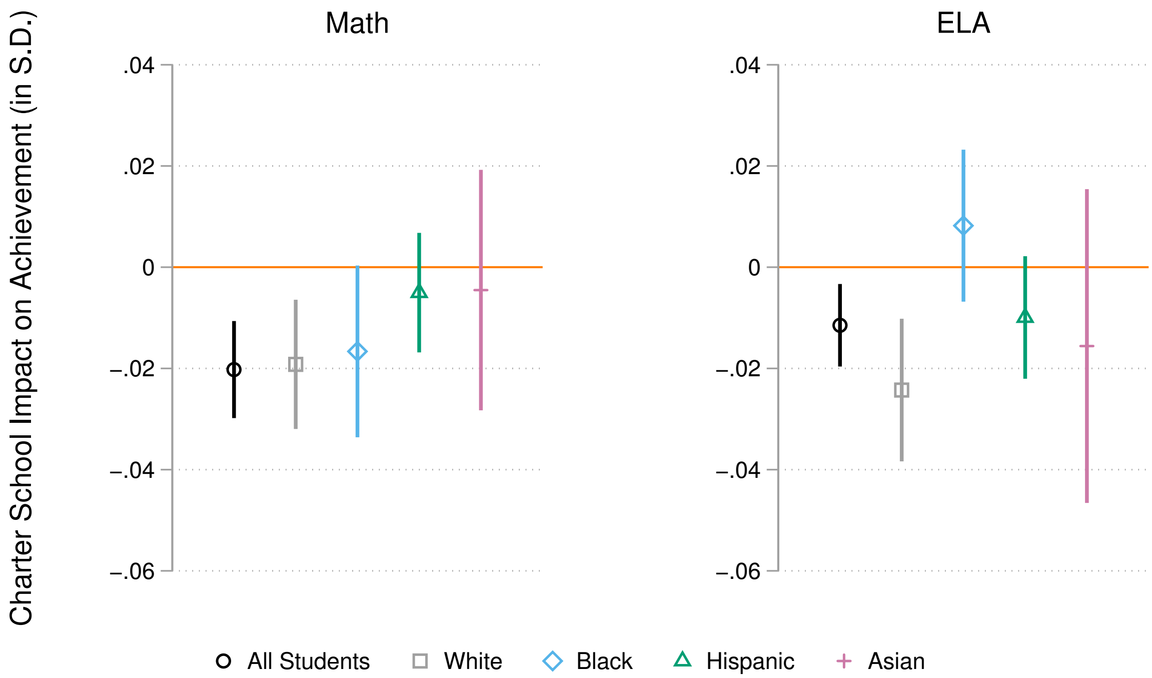
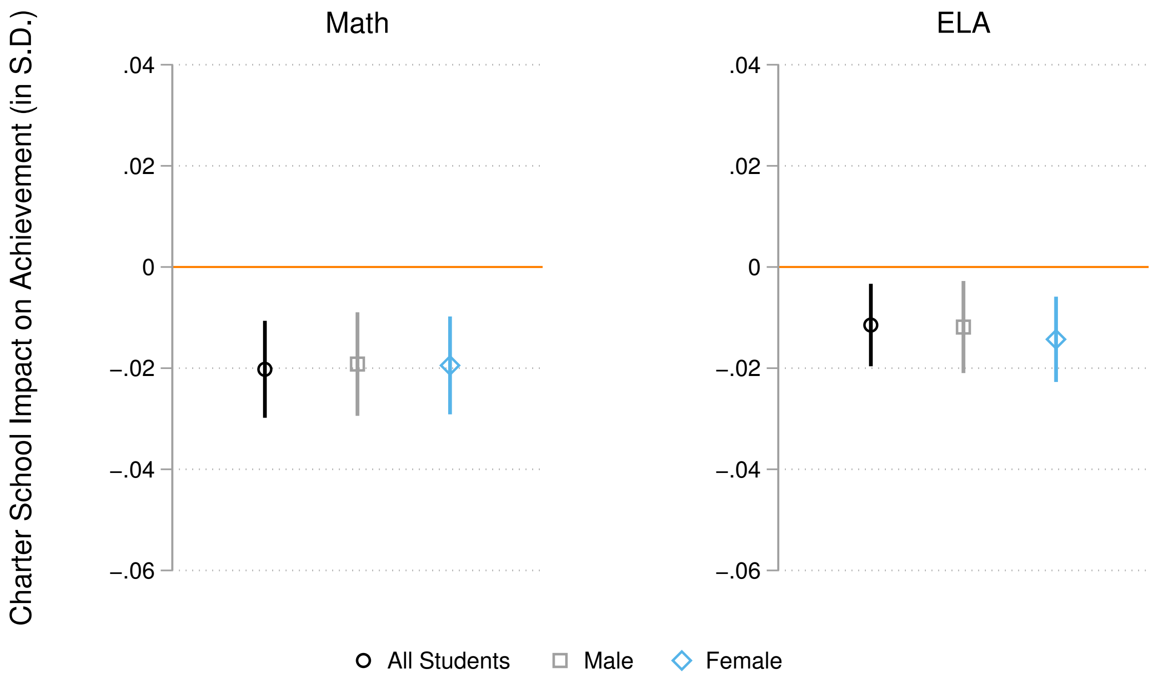


Figure 2: The 95% confidence interval for effect of new charter school on district achievement by subject and race, as reported in Table 5.



District−grade level observations. Due to data suppression, each regression considers only district−grade observations which report scores. Each specification instrumented with measure of location and policy. Sample sizes from left to right for Math are 50385, 43070, 10771, 14793, 5778; for ELA, 52988, 45377, 11434, 15569, 5404.

Figure 3: The 95% confidence interval for effect of new charter school on district achievement by subject and gender, as reported in Table 6.



District−grade level observations. Due to data suppression, each regression considers only district−grade observations which report scores. Each specification instrumented with measure of location and policy. Sample sizes from left to right for Math are 50385, 41567, 40682; for ELA, 52988, 43592, 43000.