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Do charter schools skim students or drain resources?

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Abstract

Two critical concerns with the rapid and ongoing expansion of charter schools are that they will segregate students and reduce the per-pupil resources available to conventional public schools. The contradictory prior evidence on such questions is based on potentially misleading cross-sectional comparisons. This study provides new evidence on these issues by conducting panel-based evaluations using school-level data from Arizona and neighboring states. These results suggest that the introduction of charter schools in Arizona has increased pupil–teacher ratios in traditional public schools by 6 percent and reduced the proportion of white non-Hispanic students by 2 percent.

JEL classification: I20; I28

Keywords: School choice; Resource allocation

1. Introduction

Proposals designed to introduce more choice and flexibility into public education (in particular, vouchers and charter schools) have arguably been the most significant educational policy topics of the last decade.[1] While school vouchers have generated a great deal of controversy and attention, they have only been adopted experimentally in a few localities [Peterson, Howell, Wolf, & Campbell (2001)]. In contrast, charter schools (i.e. independent public schools established by special agreements with state or local agencies) have proliferated. The first charter schools opened in Minnesota during the 1992–93 school year. By the fall of 2000, nearly 2000 charter schools were operating in 37 states and serving over a quarter-million students (US Department of Education, 2002a; Nelson, Berman et al., 2000). Proponents of school choice have argued that the introduction of charter schools can make improved educational opportunity available to our most disadvantaged youth as well as promote competitive pressures that will increase the quality of conventional public schools (e.g. Manno, Finn, & Vanourek, 2000). However, critics of charter schools raise a number of concerns, most notably that charter schools will simply lead to an increased segregation of students by socioeconomic status, race and ethnicity and that they will simultaneously erode overall public support for conventional public schools (e.g. Good & Braden, 2000a; Bernstein, 1999; Molnar, 1996).

This study provides empirical evidence on these two concerns by examining the consequences of our early experience with Arizona's charter schools. Arizona, which has been labeled the "Wild West" of education reform (e.g. Maranto & Gresham, 1999), should provide an outstanding opportunity to evaluate the concerns of

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[1] The other broad trend in educational reform has been towards establishing centralized standards and test-based accountability. However, the most prominent and recent example of such policies, the No Child Left Behind Act, also includes choice-based features (i.e. public-school choice for students whose schools receive Title I funding but fail to make testing targets for two consecutive years).

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school-choice critics. Arizona's fiscal and regulatory provisions are widely considered to be the most receptive to the formation of charter schools. Correspondingly, the state leads the nation with 15 percent of all operating charter schools (Nelson, Berman et al., 2000). In 1998, four percent of Arizona's public school students attended charter schools (Nelson, Berman et al., 2000). Only the District of Columbia had a larger share of its public school students in charter schools (4.4 percent).

Because charter schools are a relatively new phenomenon, the prior evidence on their consequences is limited (Wells, Holme, Lopez, & Cooper, 2000; Schnaiberg, 2000). The evidence that is available has focused largely on the segregation issue. The contradictory conclusions from these early studies are based on static, cross-sectional comparisons of the racial and ethnic composition of charter schools and the conventional public schools in given state or local area. We argue that these cross-sectional comparisons are subject to at least two potential sources of bias. First, they can be biased against skimming since charter schools are often located in communities with large minority populations but then compared to public schools that serve communities with a lower proportion of minorities (Schnaiberg, 2000; Good & Braden, 2000b, page 150). Second, they can also be biased in favor of skimming if charter schools disproportionately attracted non-minority students who otherwise would have attended *private* schools.

We attempt to circumvent these problems by relying instead on panel data from traditional public schools and variants of the basic "difference-in-differences" research design. More specifically, our evaluations are based on data drawn from the National Center for Education Statistics' (NCES) Common Core of Data (CCD) for the traditional public schools in Arizona and in neighboring states during the 1994–1995 and 1999–2000 school years. The results based on these data uniformly suggest that the introduction of charter schools disproportionately skimmed white non-Hispanic students and lowered the amount of resources available to conventional public schools. More specifically, this evidence suggests that charter schools led to a statistically significant reduction of approximately 2 percent in the proportion of white non-Hispanic students in conventional public schools and a statistically significant increase of 6 percent in their pupil–teacher ratios. We also find that these results are generally robust to the introduction of additional control variables as well as to variation in the set of "control" states.

2. Charter schools

Charter schools are independent public schools that are established under an agreement (or "charter") with a state or local agency. These agreements identify each school's goals and obligations and commit public support. Charter schools combine distinct features of both public and private schools (Finn, Manno, & Vanourek, 2000). Specifically, charter schools retain the critical features of public schools in that they must be non-selective in admissions, cannot charge tuition and are non-religious. Furthermore, these schools also retain at least some public accountability because of the provisions negotiated into their charters and because the charters must be renewed after a fixed period (typically, five years). However, unlike public schools, charter schools can be created by almost anyone and are highly autonomous since they are often exempt from some state and local regulations. This mix of increased choice and independence but within a public framework may explain the surprisingly broad political appeal of charter schools and their explosive growth over the last decade.

Arizona authorized the creation of charter schools in 1994 and opened its first charter schools for the 1995–1996 school year (Nelson, Berman et al., 2000). Earlier in Arizona's 1994 session, state legislators had narrowly defeated a highly controversial bill that would have provided low-income parents with vouchers to use at private schools (Keegan, 1999; Hartley, 1999). However, during the summer of that election year, the governor called the legislature back into a special session to address education reform. While there was insufficient support for vouchers, a bipartisan compromise on charter schools was reached.[2] Timmons-Brown and Hess (1999) provide a detailed, comparative study of why Arizona adopted aggressive charter school policies while the neighboring state of Nevada did not. They largely dismiss the relevance of each state's educational performance and socioeconomic background, which are actually quite similar. Instead, they suggest that Arizona's legislation reflected sharp and time-invariant differences across the two states in dynamics related to political parties and interest groups (specifically, Republican dominance and weak teacher unions in Arizona). This descriptive background on how Arizona came to introduce charter schools is important from an evaluation perspective since it strongly suggests that these policies were an independent event and not a response to other unobserved and possibly confounding within-state trends.[3] Specifically, the descriptions of Arizona's legislation wrangling suggest

[2] A key state legislator subsequently argued that the fiery voucher debate was actually a "smoke screen" meant to deflect union opposition to charter schools (Timmons-Brown & Hess, 1999.

[3] Furthermore, Arizona's NAEP scores in the years preceding the 1994 legislative session track the modest national gains closely (US Department of Education, 2002b, Table 128). This suggests that there were not idiosyncratic trends in student performance that motivated the state introduction of charter schools.

that the introduction of charter schools reflected a state-specific response to the growing national sentiment for education reform.

The features of Arizona's charter school legislation are widely considered the most aggressive in the nation (e.g. Hassel, 1999a,b). For example, in Arizona, charter schools can be authorized by two state agencies (the state Department of Education and a state charter board) as well as by local school districts. Furthermore, local school districts can also authorize charter schools that are located outside their geographic boundaries. Unlike most states, Arizona places no limits on the number of charter schools that can be created and grants charters a term of 15 years instead of five years (ECS, 2001).[4] Public school teachers in Arizona are also entitled to a 3-year leave of absence to work at a charter school. Furthermore, the charter schools in Arizona are automatically granted relatively broad waivers from most state and local educational regulations (Hassel, 1999a,b). All Arizona students are eligible to attend charter schools. Preference is only given to students with siblings who attend a given charter school and to district residents who wish to attend their district-sponsored charter schools. Nelson, Berman et al. (2000) estimate that, in the fall of 1999 Arizona's 222 charter schools enrolled 32,209 students—four percent of all the public school students in the state.

Charter schools in Arizona also have a great deal of autonomy because they are funded directly by the state and do not depend on local wealth or tax effort (Hassel, 1999a,b; Nelson, Muir, Drown, & To, 2000). State-authorized charter schools receive base support, transportation and capital funding from the state as if they were independent school districts. These state funds are adjusted for school size, the length of the school day as well as the presence of special-education students and bilingual programs (Nelson, Muir et al., 2000). The district-sponsored charters receive state funding under a similar formula but these calculations are based on the characteristics of the sponsoring districts instead of the school's traits. Since Arizona's charter schools receive their funding from state and Federal sources, their introduction should encourage participating parents (and possibly other voters) to reduce their tax effort for conventional public schools. However, these reductions may not occur if voters do not perceive these incentives or if contemporaneous increases in state funding offset any local reductions. We know of no study that has presented empirical evidence on whether the introduction of charter schools actually influenced the resource levels in conventional public schools. This study presents evidence on this issue by evaluating observed changes in school-level pupil–teacher ratios.

While there appears to be no prior evidence on how charter schools influenced resource levels, several studies have presented evidence (much of it conflicting) on whether charter schools have lead to increased segregation of students by race or ethnicity (e.g. Good & Braden, 2000b; Cobb and Glass, 1999). This evidence is typically based on simple comparisons of the racial and ethnic composition of charter schools and conventional public schools. For example, in a recent report from the US Department of Education, Nelson, Berman et al. (2000) compared data on the proportion of white students in charter schools to the state means as well as to the means for the schools in neighboring districts. They conclude that concerns about charter schools serving predominately white students have apparently not been realized since these proportions were "about the same". However, critics note that the report's definition of "about the same" (i.e. within twenty percent) is "awfully broad and forgiving" (e.g. Schnaiberg, 2000).

Another major concern with such comparisons is that the resulting inferences may be very sensitive to the choice of "neighboring" public schools (Schnaiberg, 2000]. For example, the state-level data collected by Nelson, Berman et al. (2000, page 33) indicate that Hispanic students comprised 31 percent of the students attending conventional public schools in Arizona but only 23 percent of those attending charter schools. This simple comparison suggests that charter schools did skim white non-Hispanic students from public schools. However, that inference could be misleading if charter schools were simply more likely to be located in communities with low concentrations of Hispanic students. Alternatively, this comparison would understate the amount of skimming if charters tended to form in communities with larger Hispanic populations. Another important complication to the conventional, cross-sectional comparisons of charter schools and public schools has apparently gone unnoticed. These simple comparisons can also be highly misleading simply because charter schools attract some of their students from *private* schools. For example, since the students who move from private schools to charter schools may be disproportionately white, charter schools could appear to be disproportionately white in cross-sectional comparisons even when they did not skim white students from traditional public schools. This study attempts to circumvent the many ambiguities inherent in static, cross-sectional comparisons by more directly evaluating the question of interest: whether the introduction of charter schools skimmed white students from traditional public schools. More specifically, we compare the changed racial and ethnic composition in Arizona's conventional public schools after charter schools were introduced to the con-

[4] There is a statutory limit on the number of charter schools that can be authorized annually by the state agencies. However, regulators and school operators often circumvent this restriction by operating multiple campuses under a single charter.

temporaneous changes in the public schools from neighboring states. We acknowledge that, like prior cross-sectional comparisons, this basic, panel-based research design also relies on important maintained assumptions. However, we attempt to assess the empirical relevance of these concerns for our results in a variety of ways that are discussed below.

3. Common core of data

These evaluations are based on school-level data from the last year before Arizona's charter schools became operational (1994–1995) and the most recent year for which data were available (1999–2000). We included data only from the most recent "post-treatment" year since Arizona had relatively few charter schools in the earlier years.[5] The data were drawn from the National Center for Education Statistics' (NCES) Common Core of Data (CCD). The CCD is based on an annual survey of all public schools and school districts in the United States. The school-level data include information on the racial and ethnic composition of students, on pupil–teacher ratios and on the number of students on free or reduced-price lunches.[6]

Most of the evaluations presented here are based on CCD responses from the traditional public schools in Arizona and the neighboring state of New Mexico. The choice of New Mexico as a "control" state is a critical one. In the next section, we discuss this choice as well as a variety of related robustness checks. This CCD extract began with all the school-level observations in these states for the 1994–1995 and 1999–2000 school years.[7] We then eliminated all charter schools. We identified charter schools first by relying on the charter school flags recently introduced into the CCD questionnaire. However, we also supplemented these responses with lists of charter schools from several outside sources.[8] These edits left 3955 school-year observations from Arizona and New Mexico. We then deleted the CCD records that reported zero enrollments or teachers as well as those that appeared to be from unconventional institutions such as administrative centers, juvenile detention and correctional centers, vocational schools, evening schools, other special programs and schools managed by the Bureau of Indian Affairs (BIA).[9] This reduced the sample to 3783 observations. Eliminating observations with missing data on student enrollments or teachers reduced the data set to 3670 observations.[10]

We then formed a pupil–teacher ratio for each school by dividing total school enrollments by full-time teacher equivalents.[11] The distribution of the pupil–teacher ratios included several implausible outliers that may reflect the unconventional status of remaining schools or coding errors in the relevant student and teacher counts. Therefore, we omitted all observations with pupil–teacher ratios that were greater than 150 percent of the 99th percentile value (roughly 45) or less than 50 percent of the 1st percentile value (3). These modest edits reduced the sample to 3657 observations. Finally, we deleted all observations that did not have valid data for both years since they could not contribute to our preferred panel-based estimates, which control for school fixed effects.[12] Our final data set consisted of 3396 school-by-year observations based on 1025 schools from Arizona and 673 schools from New Mexico.

The key variables in our school-level data set are the percentage of students who were white non-Hispanic and

[5] Furthermore, the available anecdotal evidence indicates that the earliest charter schools were typically small, idiosyncratic operations driven by the "missionary" zeal of parents and teachers (Maranto & Gresham, 1999). Though there is only one "post-treatment" year, the evaluations based on these data still generate statistically meaningful inferences. Furthermore, the use of only two periods should limit the pernicious influence of autocorrelation on the calculated standard errors (Bertrand, Duflo & Mullainathan, 2002).

[6] The data on free and reduced-price lunch status would have been useful to identify charter-induced patterns of socioeconomic segregation. However, these data are unavailable for Arizona in the CCD.

[7] The 1999–2000 data are from the CCD early release files. Information on the CCD and all the data are available on-line at http://nces.ed.gov/ccd.

[8] These included lists from state Departments of Education and the listings available on a web site sponsored by the US Department of Education (http://www.uscharterschools.org).

[9] After examining the distribution of school names, we eliminated those whose name contained any of the following character strings: VOC, TECH, JUVENILE, DETENTION, DET., JDC, EVENING, PROGRAM, CENTER, CNT, OFFICE, HOSPITAL, HOMEBOUND, TELETEACHING, SPECIAL and BIA.

[10] We considered imputing the missing data by simply carrying forward the responses to the CCD in the prior academic year. However, nearly all of relevant did not have valid data for the prior year as well.

[11] It should be noted that pupil–teacher ratios do not correspond perfectly with class sizes since not all teachers teach or do so in conventional settings.

[12] The relatively large number of "unbalanced" observations reflects the opening and closing of schools as well as the other school-year observations deleted because of missing data. However, we found that, when the unbalanced observations were included, models based on state and year fixed effects generated results quite similar to those reported here.

the pupil–teacher ratio.[13] However, in attempt to rule out other plausible explanations for why Arizona might have unique within-state changes in these outcomes over this period, we also merged in several other control variables. Because detailed demographic and economic data are seldom available at the school level (particularly for two relatively close intercensal years), we relied on county-level variables.[14] A particular concern with the model for the percent of white non-Hispanic students in conventional public schools is the possible bias due to community-specific trends in the racial and ethic composition of youths in the population. More specifically, if Arizona had relative reductions in the share of white non-Hispanics in the youth population, our research design could falsely attribute those changes to the stratification created by charter schools.[15] To address this concern, some of our models include as a control the percent of white non-Hispanics in the 5–19 year old county population. Economic circumstances could also influence the racial and ethnic composition of conventional public schools (e.g. through the choice of public versus private schools). Some of our models address this issue by including controls for real median household income and the percent of 5–17 year olds in poverty.[16] Since economic circumstances are also likely to influence the resources available to public schools, we also include these two control variables in our models for pupil–teacher ratios. Furthermore, state-specific changes in pupil–teacher ratios could also reflect idiosyncratic trends in the elderly share of state populations since older voters may be less likely to support public schools. To address this issue, some of our models for pupil–teacher ratios include the percent of the county population that is aged 65 or over. We also included in our extract identifiers for each school's status as an elementary or secondary school and for the type of locale (urban, suburban and rural) in which each school is located.[17] The urbanicity and grade spans of conventional public schools differ considerably across these states. Specifically, 45 percent of Arizona's schools are in urban settings compared to only 24 percent in New Mexico. And 70 percent of Arizona's schools are at the elementary level compared to 63 percent in New Mexico. Including information on these fixed school traits facilitates some important robustness checks, which are described in the next section.

4. Methods

The evaluations presented here are based on variants of the basic "differences-in-differences" research design. In this context, this approach effectively involves comparing the changes within Arizona's schools to the contemporaneous changes in the schools from some "control" state(s). An important, maintained assumption of this approach is that the changes in the control state reflect the unobserved, time-varying determinants also shared by Arizona (e.g. national or region-specific trends). Therefore, it was natural for us to initially consider the public schools from the states bordering Arizona as potential controls. However, all five of these states (California, Colorado, Nevada, New Mexico and Utah) also authorized the introduction of charter schools over this period. Fortunately, these states were starkly different in how aggressively they promoted the creation of charter schools. We rejected the schools in California and Colorado as controls because, like Arizona, each state has charter laws that have been classified as "strong" (Hassel, 1999a; CER, 2001)). And, as a consequence of these provisions, California and Colorado also have a relatively large number of charter schools (210 and 68 respectively in the fall of 1999, Nelson, Berman et al. (2000)). In contrast, the remaining three states passed extremely weak charter legislation (Timmons-Brown & Hess, 1999; Hassel, 1999a; CER, 2001) and had almost no charter schools in operation by the fall of 1999. By the fall of 1999, there was only 1 charter school in Nevada, 3 in New Mexico and 6 in Utah (Nelson, Berman et al., 2000). Furthermore, even if these few charter schools did have effects similar to those in Arizona, the direction of the implied bias would only

[13] We focus on this measure instead of percent black and percent Hispanic separately since most of the minority students in these states are Hispanic.

[14] The school-level CCD files do not include county identifiers. However, we were able to identify each school's county by cross-walking the school-level data with the district-level CCD files that did include county identifiers. There are 15 counties in Arizona and 33 in New Mexico. While there are many more schools than counties, the plausibly signed and statistically significant effects of many of these variables suggest that they are providing adequate controls.

[15] Our calculations based on recently released intercensal population estimates indicate that from 1994 to 1999, the percent white non-Hispanic among Arizona children aged 5 to 19 fell from 45.8 percent to 43.4 percent, a decline of 2.4 percentage points. Over the same period, the percent white non-Hispanic among New Mexico's children fell from 27.6 percent to 25.6 percent, a decline of 2.0 percentage points. These similarly sized reductions suggest that Arizona-specific demographic trends would only impart a relatively modest bias to our "skimming" results.

[16] These data were only available for the calendar years (1993 and 1998) preceding the relevant school years. We also experimented with including the county unemployment rate but found that it had small and statistically insignificant effects.

[17] We identified elementary schools as ungraded schools and those whose lowest grade was between pre-K and 6th grade. We identified secondary schools as those whose lowest grade was between 7th and 12th grades.

reinforce both of the key results reported here. For example, if New Mexico's schools actually had modest charter-related decreases in percent white non-Hispanic or increases in pupil–teacher ratios, the difference-in-differences approach implies that our results actually understate the corresponding effects of Arizona's charter schools.

However, the schools from New Mexico, Nevada and Utah could still be poor controls if there were yet other, idiosyncratic changes in these states that happened to coincide with the introduction of charter schools in Arizona. Our examination of these states over this period suggested that Nevada would be a poor control because, over this period, it had a regionally distinct trend in the growth of its population and, more critically, in its racial and ethnic composition (Weissenstein, 2001). Specifically, population data from the US Census Bureau indicate that between 1993 and 1999, the population share of white non-Hispanics in Nevada fell by 6.3 percentage points. In contrast, the corresponding declines in Arizona, New Mexico and Utah were only 2.6, 2.2 and 2.1 percentage points respectively. This implies that including data from Nevada would bias "difference-in-differences" evaluations against the existence of skimming because the share of white non-Hispanic students in Nevada's public schools should have relatively large and contemporaneous declines that are driven by its state-specific population trends. We also rejected the schools from Utah as potential controls because, over this period, the state authorized a substantial amount of new funds for a successful, 10-year initiative to reduce class sizes (Cortez, 1996; May, 2000). Utah's state-specific class-size reductions imply that including its data in these evaluations would lead us to overstate the degree to which Arizona's charter schools increased pupil–teacher ratios in its traditional public schools. In light of all this evidence, we chose to focus our attention on the traditional public schools from New Mexico as the "controls" for these evaluations. However, as a qualified robustness check, we also report the results of some evaluations that include similarly constructed school-level observations from Nevada and Utah.

The conditional means in Table 1 provide initial evidence on how the introduction of charter schools in Arizona might have influenced the ethnic composition and pupil–teacher ratios in traditional public schools. For example, the average share of white non-Hispanic students in Arizona's traditional public schools fell by 5.9 percentage points over this period. This suggests that the introduction of charter schools did lead to a substantial skimming of white students. However, an equally plausible conjecture is that this reduction merely reflects a contemporaneous trend in the composition of Arizona's total population that was unrelated to charter schools. The basic logic of this study's identification strategy is to rely on the corresponding change in New Mexico's public schools (a reduction of 3.1 percentage points) as a measure of the potentially confounding time-series changes unrelated to Arizona's state-specific charter school policies. The difference-in-differences estimate implied by these comparisons suggests that the introduction of Arizona's charter schools reduced the share of white non-Hispanic students by 2.8 percentage points ($\Delta_{AZ} - \Delta_{NM} = -0.059 - (-0.031)$). The conditional means in Table 1 indicate Arizona's pupil–teacher ratio also fell over this period. However, this reduction was modest relative to New Mexico's, suggesting that the expansion of Arizona's charter schools increased pupil–teacher ratio in traditional public schools by 1.2 ($\Delta_{AZ} - \Delta_{NM} = -0.9 - (-2.1)$).

There are a number of reasons to be concerned about the reliability of the inferences based on these simple comparisons. One concern is simply whether the relative differences within these two states are statistically distinguishable. We initially took an unrestrictive approach to this issue by conducting nonparametric comparisons of the within-school changes in each state based on Wilcoxon rank sum tests (Table 2). Specifically, we calculated the within-school changes in the percent of white non-Hispanic students for both states and ranked these pooled school-level observations from lowest to highest. Under the null hypothesis that the population distributions for these changes are equal, the sum of the ranks for Arizona's schools should equal 870737.5 (i.e. $E(T) = n_{AZ}(n_{TOTAL} + 1)/2$). However, the actual rank sum (800,517) was substantially lower, indicating that Arizona's traditional public schools tended to have larger reductions in the percent of white non-Hispanic students relative to New Mexico's schools. The implied z-statistic (-7.11) allows us to easily reject the null hypothesis that the distributions of these within-state changes are similar. For the within-school changes in pupil–teacher ratios, the rank sum for Arizona's schools exceeds the value implied by the null hypothesis, suggesting that these schools had relative increases in pupil–teacher ratios. The implied z-statistic (8.92) also allows us to reject the null hypothesis that these distributions are similar.

A more substantive concern with the inferences based on these comparisons is that the within-school changes unique to Arizona may simply reflect the influence of state-specific trends that are unrelated to its charter schools. We examine the empirical relevance of these concerns in a regression framework that allows us to introduce controls for other relevant determinants. More specifically, our regression results are based on the following two-way fixed effects model:

$$Y_{ist} = \mu_i + \nu_t + \gamma(\alpha_s \cdot \nu_t) + \varepsilon_{ist}$$

where Y_{ist} is the dependent variable for school i from state s in year t. The term, μ_i, represents *school* fixed effects and the term, ν_t, is a year fixed effect equal to 1

Table 1
Descriptive statistics, traditional public schools in Arizona and New Mexico, 1994–1995 and 1999–2000 school years

	Mean (standard deviation)					
	Arizona			New Mexico		
Variable	1994–1995	1999–2000	Difference	1994–1995	1999–2000	Difference
Percent white non-Hispanic	0.566 (0.303)	0.507 (0.304)	−0.059	0.384 (0.257)	0.351 (0.252)	−0.031
Pupil–teacher ratio	19.1 (3.9)	18.2 (3.6)	−0.9	17.2 (3.3)	15.1 (2.9)	−2.1
Observations	1025	1025		673	673	

Table 2
Wilcoxon rank sum tests comparing within-school changes in Arizona and New Mexico

Variable	Actual rank sum	Expected rank sum under H_o	Standard deviation	z-statistic
Within-school change in percent white non-Hispanic	800,517	870737.5	9883	−7.11
Within-school change in pupil–teacher ratio	958,867	870737.5	9883	8.92

This test compares the within-school changes in Arizona over the 1994–1995 and 1999–2000 periods to the within-school changes in New Mexico. The rank sums reported in the table are for Arizona. The null hypothesis is that the two population locations are the same.

for observations from the 1999–2000 school year and 0 for observations from the 1994–1995 school year. The term, α_s, is state fixed effect equal to one for Arizona's schools and zero for New Mexico's schools. The parameter of interest is the coefficient, γ, which identifies the changes unique to schools in Arizona after they introduced charter schools (i.e. the interaction of α_s and ν_t). Because the error term, ε_{ist}, is likely to be heteroscedastic, we report heteroscedastic-consistent standard errors.[18]

We first report the results for this specification, which essentially replicates the prior "difference-in-differences" comparisons. Then, we examine the robustness of these results to the incremental introduction of all the other, potentially relevant variables discussed earlier. The fact that the schools in Arizona are much more likely to be located in urban and suburban settings than those in New Mexico may imply another source of omitted variable bias. More specifically, if there were unobserved time-series determinants that were specific to urban and suburban locales (e.g. changed tastes for private schools), they could be confounded with the proliferation of Arizona's charter schools. To address this concern, we also estimate models where we introduce year fixed effects that are specific to each locale. Arizona's schools are also much more likely to be elementary schools than New Mexico's. To eliminate the possibly unobserved determinants that are specific to a school-level, we also introduce level-specific year fixed effects. As a final robustness check, we also report the results from several models where we expand the "control" group to include data from the traditional public schools in Utah and Nevada.

[18] Since we are estimating the effects of a state-level policy with sub-state data, the error structure could also reflect shocks specific to each state-year combination (Moulton, 1990). Therefore, we also examined heteroscedasticity corrections clustered on state-year. However, those standard errors are generally much smaller. Therefore, we report these conservatively large standard errors instead. The reported standard errors are also inflated by a finite-sample correction (Davidson & MacKinnon, 1993).

5. Results

Table 3 presents the key results from regression models where the dependent variable is the percent of white non-Hispanic students in Arizona and New Mexico's traditional public schools. The first model, which is based only on school and year fixed effects, suggests that char-

Table 3
Estimated effects of Arizona charter schools on percent white non-Hispanic in traditional public schools

Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arizona × 1999–2000 Year	−0.026[c]	−0.009[b]	−0.006[a]	−0.013[c]	−0.009[b]	−0.012[c]	−0.008[b]
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
County-level variables							
Percent white non-Hispanic in 5–19 year old population	–	1.82[c]	1.52[c]	1.16[c]	0.67[c]	1.12[c]	0.65[b]
		(0.21)	(0.24)	(0.23)	(0.25)	(0.23)	(0.25)
Real median household income	–	–	−0.053[c]	−0.039†	−0.038[b]	−0.037[b]	−0.036[b]
			(0.016)	(0.016)	(0.018)	(0.016)	(0.017)
Percent of 5–17 year olds in poverty	–	–	–	0.284[c]	0.115[a]	0.276[c]	0.111[a]
				(0.055)	(0.061)	(0.055)	(0.061)
Locale-specific year fixed effects?	no	no	no	no	yes	no	yes
Level-specific year fixed effects?	no	no	no	no	no	yes	yes
R^2	0.9848	0.9853	0.9854	0.9857	0.9860	0.9858	0.9862

Heteroscedastic-consistent standard errors are reported in parentheses. All models include school and year fixed effects. The subsequent models introduce year fixed effects specific to each locale (urban, suburban and rural) and each school level (elementary and secondary).

[a] Statistically significant at 10-percent level.

[b] Statistically significant at 5-percent level.

[c] Statistically significant at 1-percent level.

ter schools reduced this enrollment share by a large and statistically significant 2.6 percentage points. In the second model, which introduces the population control, this estimate falls to −0.9 percentage points but remains statistically significant at the 95 percent level. The sensitivity of this estimate implies that Arizona did have state-specific changes in the racial and ethnic composition of its youth population. Data from the Census Bureau confirm that, between 1994 and 1999, the percent white non-Hispanic among 5 to 19 year olds fell by 2.5 percentage points in Arizona but by only 1.9 percentage points in New Mexico. The results in Table 3 demonstrate that failing to control for those community-specific changes would have led to a large bias in the estimated "skimming" effect. The remaining controls are incrementally introduced in the next five specifications. Notably, the estimated effect of charter schools are generally similar across these specifications (i.e. within about 1.5 standard errors). Specifically, in the third model, real median household income is introduced and the key estimate falls to −0.6 percentage points and is weakly significant (p - value = 0.076).[19] In the remaining four models, other relevant controls are included and the estimated effect of charter schools is slightly higher and statistically significant at the 95 percent level. This evidence suggests that the introduction of charter schools in Arizona reduced the share of white non-Hispanic students in conventional public schools by approximately 1 percentage point. Given that the share of white non-Hispanic students in Arizona's traditional public schools was roughly 50 percent, a reduction of 1 percentage point implies an average decrease of approximately 2 percent.

In Table 4, we present the key results from similarly specified models for pupil–teacher ratios. The results suggest that the introduction of charter schools increased pupil−teacher ratios in conventional public schools by roughly 1.2, an average increase of roughly 6 percent. These point estimates are statistically precise and surprisingly robust to the incremental introduction of all the other control variables. An interesting ambiguity about these results involves whether Arizona's pupil–teacher ratio changed because of movement in the numerator or the denominator. Separate semi-log models for student enrollments and teacher full-time equivalents suggest that Arizona's pupil–teacher ratios rose because of significant changes in both. That is, after the introduction of charter schools, the pupil–teacher ratios in Arizona's conventional public schools rose because there were fewer teachers at a time when enrollments were growing. However, there is another potentially important source of bias in these evaluations. These results could also reflect the lagged hiring of new teachers in response to Arizona-specific enrollment pressures. To assess this possibility, we also constructed county-level variables for the size and recent growth in the youth population. We found that these measures had small and statistically insignificant effects on pupil–teacher ratios and led to no meaningful changes in our estimates.

A more fundamental concern is that these inferences reflect the maintained assumptions associated with using New Mexico as a "control" state. One ad-hoc way to assess this possibility is to consider the relative state trends in the period just prior to Arizona's experiment with charter schools. Between 1991 and 1994, the average pupil–teacher ratio in Arizona's public schools (19.3) was unchanged (US Department of Education, 1999, Table 67). Over the same period, the pupil–teacher ratio in New Mexico's public schools fell by 0.4 (i.e., from 17.6 to 17.2; a reduction of 2.3 percent). This relative reduction in New Mexico's pupil–teacher ratio—before Arizona's charter schools opened—suggests that the results in Table 4 could reflect unobserved trends specific to New Mexico. However, to the extent there is such a misspecification, the available evidence also suggests that the resulting bias is likely to be fairly small. In particular, the difference in state trends during this pre-reform period are small relative to those that occurred after Arizona allowed charter schools. Specifically, the means in Table 1 indicate that, between 1994 and 1999, the mean pupil–teacher ratios in New Mexico's public schools fell by 2.1 while those in Arizona fell by 0.9. In other words, the difference in the state changes after Arizona introduced charter schools is three times larger than the pre-reform difference. Furthermore, if we were to assume that the results in Table 4 reflect an upward bias of approximately 0.4, the implied point estimates would still be positive and statistically distinguishable from zero.

Overall, the results in Tables 3 and 4 suggest that the charter schools in Arizona did skim white non-Hispanic students from conventional public schools and lead to resource reductions for those schools. These panel-based results provide a novel complement to those based on cross-sectional comparisons. However, the quality of these inferences also relies on unique maintained assumptions. The most notable of these is that, conditional on the observables, New Mexico's public schools provided valid controls for the unobserved, time-series determinants that influenced Arizona's schools over this period. Violations of this assumption could generate biases of an uncertain direction. The robustness of our results to the introduction of other statistically relevant controls, along with other indirect and anecdotal evidence, suggests the absence of such biases. However, in Table 5, we present additional evidence on the robustness of our results to this assumption by introducing as controls observations from the traditional public schools

[19] However, it is worth recalling that there is increased precision (p - value = 0.023) in models that allow for heteroscedasticity specific to each state-year cell.

Table 4
Estimated effects of Arizona charter schools on pupil–teacher ratios in traditional public schools

Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Arizona × 1999–2000 Year	1.26[c]	1.13[c]	1.10[c]	1.21[c]	1.08[c]	1.25[c]	1.11[c]
	(0.16)	(0.18)	(0.19)	(0.20)	(0.21)	(0.20)	(0.21)
County-level variables							
Real median household income	–	1.39[b]	1.57[b]	2.15[c]	1.86[b]	2.26[c]	1.96[b]
		(0.67)	(0.69)	(0.76)	(0.84)	(0.76)	(0.84)
Percent of 5–17 year olds in poverty	–	–	2.4	0.6	3.4	0.2	3.1
			(2.4)	(2.6)	(3.3)	(2.6)	(3.2)
Percent of population aged 65 or older	–	–	–	23.1[a]	21.6[a]	24.0[a]	22.4[a]
				(12.9)	(12.9)	(12.8)	(12.8)
Locale-specific year fixed effects?	no	no	no	no	yes	no	yes
Level-specific year fixed effects?	no	no	no	no	no	yes	yes
R^2	0.8119	0.8124	0.8125	0.8128	0.8131	0.8139	0.8143

Heteroscedastic-consistent standard errors are reported in parentheses. All models include school and year fixed effects. The subsequent models introduce year fixed effects specific to each locale (urban, suburban and rural) and each school level (elementary and secondary).

[a] Statistically significant at 10-percent level.

[b] Statistically significant at 5-percent level.

[c] Statistically significant at 1-percent level.

Table 5
Estimated effects of Arizona charter schools by choice of control states

	Dependent variable					
	Percent white non-Hispanic			Pupil–teacher ratio		
Sample	Estimate	R^2	n	Estimate	R^2	n
Arizona and New Mexico	−0.013[a]	0.9857	3396	1.21[a]	0.8128	3396
	(0.004)			(0.20)		
Arizona, New Mexico and Nevada	−0.018[a]	0.9834	4172	0.86[a]	0.8190	4172
	(0.003)			(0.15)		
Arizona, New Mexico and Utah	−0.015[a]	0.9884	4764	1.63[a]	0.8491	4764
	(0.003)			(0.15)		
Arizona, New Mexico, Utah and Nevada	−0.016[a]	0.9861	5540	1.36[a]	0.8478	5540
	(0.003)			(0.14)		

Heteroscedastic-consistent standard errors are reported in parentheses. The model specifications are the same as Model (4) in Tables 3 and 4.

[a] Statistically significant at 1-percent level.

in the neighboring states of Nevada and Utah. The first row of Table 5 repeats for convenience the key results from the fourth model in Tables 3 and 4. The results in the remaining rows are based on the same specifications but also on samples that are expanded to include data from Nevada and Utah separately and then in combination. The results of all these models are quite consistent with the inferences from Tables 3 and 4.

6. Conclusions

The surprising proliferation of charter schools over the last decade has arguably been the most striking outcome of the many and varied efforts at educational reform. Our ongoing experiments with charter schools will be evaluated on a number of criteria that include their accountability and their success in developing innovative programs that successfully promote student growth and achievement. However, critics of school choice have also emphasized that the desirability of charter schools should also be judged by whether they increase the segregation of public school students by race or ethnicity and by whether they lead to a reduction in resources available to conventional public schools. The prior evidence on these questions, which is based on potentially confounded cross-sectional comparisons, has been limited and contradictory. In this study, we presented new empirical evidence on these issues by employing school-level panel data on enrollments and pupil–teacher ratios to evaluate the consequences the Arizona's intensive experiment with charter schools. The results of these evaluations clearly suggest that some of the concerns raised by critics of charter schools have been realized. More specifically, these evaluations suggest that Arizona's early experiences with charter schools led to a robust and statistically significant reduction in the percent of white non-Hispanic students in conventional public schools (i.e. roughly 1 percentage point or 2 percent of the mean) and an increase in pupil–teacher ratios (i.e. roughly 1.1 pupil per teacher or 6 percent of the mean).

However, in concluding, there are at least two caveats worth emphasizing. The first caveat involves the external validity of these inferences. The results presented here would not necessarily be replicated in other states implementing similarly aggressive charter-school provisions. In particular, Wells et al. (2000) suggest that the consequences of charter schools may be quite different in northern and eastern states where the impetus for charter schools has been relatively concentrated in segregated, urban districts. A second, important caveat involves the sizes of the effects reported here. The policy relevance of these results may come to be viewed as limited because the effects we find, though statistically significant, are surprisingly small. For example, in a recent assessment of the costs and benefits of class-size reductions, Krueger (2003) notes that reducing a class size from 22 to 15 increases test scores by 0.2 standard deviations. The increase in pupil–teacher ratios identified here is roughly 16 percent as large (i.e. 1.1/7) so the implied change in test scores would be about 0.032 standard deviations (i.e. 0.2×0.16). The introduction of charter schools may have also lowered student achievement in traditional public schools by reducing peer-group quality. However, a rough calculation suggests that this sort of effect is also likely to be quite small. Specifically, in 1994, the gap between the 4th grade NAEP reading scores of white and Hispanic students in Arizona was

roughly 32 (i.e. 220 and 188, respectively). Therefore, a 1 percentage-point reduction in the percent of white non-Hispanic students would reduce a school's reading score by roughly 0.32 points. Hoxby (2000) finds that a 1-point increase in the mean achievement of a student's peers increases achievement by as much as 0.40 points. Combining these results implies that the reduction in test scores among students in traditional public schools due to the charter-induced deterioration in peer-group quality would not be larger than 0.13 points (i.e. 0.32×0.40). This change is less than 0.1 percent of the mean NAEP score in Arizona (i.e. 206). Furthermore, such modest reductions in student achievement could coincide with larger and possibly offsetting gains that are driven by improvements in the productivity of conventional public schools. In fact, Hoxby (2002) presents evidence that the competition from charter schools in Arizona raised both the productivity and measured achievement of conventional public schools. These calculations clearly suggest that broad, normative assessments of the desirability of charter schools should be careful to acknowledge both their benefits and their costs.

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