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I did **not** use data posted by the authors
My substantive addition is: N/A

Replication Paper of "Do charter schools skim students or drain resources?"

Introduction

In "Do charter schools skim students or drain resources?," Thomas Dee and Helen Fu (2004) investigated two primary concerns with the charter school movement: racial segregation and decreased resources for traditional public schools (TPSs). Using school-level panel data from the Common Core of Data (CCD) and county-level intercensal estimates from U.S. Census data, Dee and Fu used a difference-in-differences design to explore how the opening of Arizona's first charter schools in the 1995-1996 school year changed the percentage of white non-Hispanic students and the student-teacher ratio in TPSs between the 1994-1995 and 1999-2000 school years (hereafter referred to as SY 1995 and SY 2000).

The charter school movement gained bipartisan support at the turn of the century (Murphy & Shiffman, 2002), as it promised to provide an alternative to the often "abysmal" local public schools available to disadvantaged populations (Manno et al., 1999, p. 429). By subverting the bureaucratic hindrances that often limit the scope and impact of district schools, charter schools—in theory—are meant to use innovative teaching techniques to drastically improve student achievement (Finn et al., 2001). In exchange for this increased autonomy, charter schools also have increased accountability, as they are required to apply for frequent renewals (usually every five years)—otherwise they risk forced closure (Manno et al., 2000).

Despite their enormous potential, skeptics raised concerns over the oft homogenous student populations commonly present in charter schools (Bernstein, 1999). Because many

charter schools elect to serve a niche population and because parents have control over selection, charter school critics fear that the expansion of these schools of choice will increase racial segregation (Logan & Burdick-Will, 2016).

A second concern with the charter school movement is the negative effects on the local district schools. Ongoing research continues to investigate whether per pupil resources—including total revenue per student, student-teacher ratios, etc.—in traditional public schools are affected by the institution of a local charter school (Arsen & Ni, 2012; Molnar, 1996).

As a metric for segregation, Dee and Fu chose to compare the proportion of white non-Hispanic students in TPSs in Arizona, the treated state, and New Mexico, the control state, before and after the opening of charter schools. To measure how charter schools affect per pupil resources in TPSs, the authors chose student-teacher ratio in non-charter public schools as an outcome variable.

The endogeneity problem

When investigating the impacts of charter schools on segregation and per pupil resources in TPSs, the fact that charter schools are "school of choice" presents an inherent endogeneity problem. Firstly, when trying to make a within state comparison of the percentage of white students (Dee and Fu's metric for segregation) in charter schools versus TPSs, the choices of charter management organizations (CMOs) present a problem. Dee and Fu (2004) highlight the main issue with past "static, cross-sectional comparisons of the racial and ethnic composition of charter schools and the conventional public schools" (p. 260). CMOs often choose to place charter schools in predominantly black and Hispanic, low-income neighborhoods due to their potential to provide a strong option for families apart from the local district school (Schnaiberg,

2000), so they are often more available to students of color. Within a state, a standard comparison of racial breakdowns in charter schools versus TPSs would not capture this choice. Similarly, simply comparing the racial breakdowns of two different states (which will inevitably have different populations of low-income students of color) could skew the data disproportionately to make charter schools appear to promote segregation, since states with higher populations of black and Hispanic students are likely to have higher numbers of charter schools.

We also have several other factors to consider. The authors did not run a simple OLS regression to determine the effect of charter schools on segregation and TPS resources because the elective nature of charter schools provides ample opportunity for selection bias. Parental motivations, for example, could be an important unobserved omitted variable. The types of parents who are motivated to sign their child up for the charter school lottery may be the type of parent to also pull their child out of an integrated school and go into a more racially segregated one—thus contributing to segregation. Thus, parental motivation is an example of a variable that is correlated with both the treatment (living in an area with charter schools), and the outcome (segregation). Other potential omitted variables that can affect both segregation and per pupil resources are socioeconomic status, locale, school level, and age distribution of the voting population.

Additionally, comparing the impact of a charter policy between two states presents the challenge of differing time trends. If Arizona and New Mexico experience different changes in the patterns of segregation and per pupil resources over time, we will not know how the policy impacted these outcomes. To solve these endogeneity issues, Dee and Fu employ a difference-in-differences design that compares the white non-Hispanic percentage and student-teacher ratio in

TPSs before and after the implementation of the first charter schools in Arizona and nearby control state New Mexico. This method eliminates the selection bias that the cross-sectional comparisons could not, while simultaneously eliminating the time effects between the two states (Cunningham, 2021). A difference-in-differences design is a common design for evaluating a policy (such as the introduction of charter schools) at scale, and it is applicable when we have clear observations before and after the institution of the policy, as well as plausible controls (Angrist & Pischke, 2014). To use this design, however, the data must meet the parallel trends assumption. This concept will be discussed further in the Extension section.

Arizona and New Mexico

Dee and Fu chose to investigate the opening of charter schools in Arizona, since this state has consistently been a leader in the school choice movement. It had a very strong charter school presence, with one out of every twenty-five students in Arizona attending a charter school by 1998 (Nelson et al., 2000).

The authors chose to use New Mexico as the control state due to its proximity to Arizona and low number of charter schools. They began their search for a control state by looking at all of Arizona's neighbors, saying "it was natural for us to initially consider the public schools from the states bordering Arizona" (Dee & Fu, 2004, p. 263). Then they narrowed their search by examining each state's charter school presence. Colorado and California were immediately disqualified due to their similarly large numbers of charter schools. Nevada was ruled out due to its rapid decline of white non-Hispanic students between 1993 and 1999, which would have disproportionately skewed the estimates in favor of segregation—since the reason for this reduction in white TPS students would have been assigned to the introduction of charter schools. Utah was also disqualified due to a new initiative to reduce class size during the period of

interest, which would have affected the teacher-pupil ratio estimates. By default, New Mexico was the only border state left to use as the control. I discuss other potential control states in the Extension section.

Data

The authors and I used data from the Common Core of Data (CCD), housed on the National Center for Education Statistics' (NCES) website. Following Dee and Fu, I downloaded school-level CCD data from SY 1995 and SY 2000, then filtered out all charter schools since the study only focuses on the effects on traditional public schools. Dee and Fu (2004) state that they eliminated charter schools from the data set "first by relying on the charter school flags recently introduced into the CCD questionnaire, [then] supplemented these responses with lists of charter schools from several outside sources" (p. 262). One of the "outside sources" listed in a footnote is a website that is no longer active, so my sample size at this stage had 262 more observations than that of Dee and Fu due to my inability to identify some of these schools.

I then attempted to replicate their data cleaning process by filtering out all observations with zero students and/or full-time teachers. Next, I created the student-teacher ratio variable. The CCD data does not explicitly include a student-teacher ratio variable, so I, like the original authors', divided school enrollment by the number of full-time teachers to obtain this variable. Similarly, I created a variable for the percentage of white non-Hispanic students by dividing the number of white non-Hispanic students by total enrollment. Then, the original authors and I eliminated questionable outliers by filtering the schools that were "greater than 150 percent of the 99th percentile value (roughly 45) or less than 50 percent of the 1st percentile value (3)" (Dee & Fu, 2004, p. 262). Then I eliminated all observations that did not have data for both years of study. Lastly, I removed all observations that appeared to be from "unconventional institutions"

(Dee & Fu, 2004, p. 262). Similar to when I was filtering out charter schools, this step likely caused my dataset to deviate slightly from the original authors since they did list which specific schools they eliminated. They did, however, specify that they eliminated school from "administrative centers, juvenile detention and correctional centers, vocational schools, evening schools, other special programs and schools managed by the Bureau of Indian Affairs (BIA)" (Dee & Fu, 2004, p. 262). My dataset downloaded from the Common Core of Data had a category for school type, so I eliminated all schools that were not "Regular" and "Special Education Schools," which removed all alternative and vocational schools. In the end, my dataset contained 3,374 observations (including both years of data), compared to Dee and Fu's 3,396 observations.

Dee and Fu also used several covariates from intercensal estimates, which I located on the U.S. Census' website. Unlike the CCD data, these files were only available at the county level. To control for potential bias due to "community specific tends in the racial and [ethnic] composition of youths in the population" (Dee and Fu, 2004, p. 263), the authors included the intercensal estimate variable for the percent of white non-Hispanics who are between 5 and 19 years old. The datafiles proved difficult to locate on the Census' website, and I was only able to obtain files for years 1995 and 1999, since 2000 was not available in the same format. This deviation from the original authors' process led to discrepancies in estimates, to be discussed in more detail in a later section.

Once downloaded, these datafiles were filtered for the states of interest (including the other control states—Nevada, Utah, New York, and New Jersey—also to be discussed in a later section). Another variable that the authors and myself pulled from these data were the percent of the population that was 65 years and older. This variable was meant to control for the voting

habits of older people who may not support public schools which, supposedly, impacted the student-teacher ratios.

The authors also wanted to control for income and poverty levels, so we downloaded two more variables from the intercensal data. These two variables, however, were only available in 1993 and 1998, so myself and the authors' were restricted to data from these two years. Specifically, I merged the median household income and percent of 5-17 year olds in poverty in 1993 and 1998 with the rest of the data, treating them as 1995 and 2000 data, respectively.

Methods

Dee and Fu employed a difference-in-differences design to find the causal impact of the introduction of charter schools in Arizona on segregation—measured by the percentage of white non-Hispanic students in TPS—and per-pupil resources—measured by student-teacher ratios in TPSs. The use of this strategy is justified due to the many potentially confounding variables, as discussed above. The comparison of Arizona to the control state of New Mexico is meant to account for the time trend changes that may have occurred in Arizona between 1995 and 2000. The basic logic of the difference-in-differences design is first displayed in Table 1. It shows the average percentage of white non-Hispanic students in Arizona's TPSs for SY 1995 and SY 2000, along with the differences between them. The same is displayed for New Mexico. My replication of these summary statistics yielded a decrease of 2.2 percentage points in the share of white non-Hispanics in TPSs in Arizona, compared to Dee and Fu's 2.8 percentage point decrease. Additionally, Table 1 shows similar averages for student-teacher ratios, with my replication ultimately producing an increase of 1.3 teachers per student due to the introduction of charter schools, compared to Dee and Fu's 1.2.

While helpful in interpreting the structure of a difference-in-differences research design, these initial approximations do not adequately control for the many confounding variables discussed above. So Dee and Fu used the following regression equation to more accurately produce estimates of charter school impact:

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

"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, v_t, represents fixed effect equal to 1 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" (Dee & Fu, 2004, p. 265).

Dee and Fu also account for two more potential omitted variables: locale and school level. The motivation for these variables came from the fact that Arizona has much more urban and suburban schools than New Mexico, and Arizona has a greater proportion of elementary schools than New Mexico. Lastly, as a robustness check, they included regression models that utilized Utah and Nevada TPS data. I will discuss the effectiveness of this robustness check in the Extension section.

Results

In the first regression model which uses the percentage of white non-Hispanic students in Arizona TPSs as the outcome variable, the authors find that charter schools lead to a 2.6 percentage point decrease in white students left in TPSs. My own analysis yielded a percentage point decrease of 2.7 percentage points. After controlling for all the covariates, however, Dee and

Fu's effect size dropped to – 0.8 percent; mine dropped to -1.9 percent. All these effects were statistically significant. This means, according to the data, the presence of charter schools in Arizona in 2000 caused a 0.8 percent decrease (according to Dee and Fu), or an approximately 0.2 decrease (according to my own data), in the percentage of white students remaining in TPSs. See Table 2 in the Appendix for a summary of mine and the original authors' results. While the results are statistically significant, practically, this number is underwhelming, especially in a state that experiences so much immigration and has commonly fluctuating percentages of Hispanic and white non-Hispanic students.

It is worth nothing that the introduction of the census data, particularly the real median household income covariate, changed my effect sizes to be quite different from the original authors'. I believe this is due to the fact that much of my census data came from 1999, rather than 2000, as previously discussed. Nevertheless, the effect sizes were small.

Similarly, the impact of charter schools on TPS student-teacher ratios was relatively small. The authors found an increase of 1.26 pupils per classroom, compared to my 1.32, before controlling for any other variables. Once introducing all the covariates, the authors' effect size was reduced to 1.11, and mine to 1.26. A more detailed summary can be found in Table 3 in the Appendix. While these effects were also statistically significant, the increase of approximately one student per classroom is not a compelling argument that charter schools are draining resources from TPSs.

The original authors also highlight the small effect sizes and provide two helpful pieces of conetxt. Firstly, the reduction in the share of white non-Hispanic students is would lead to a decrease in test scores that "would not be larger than 0.13 points" out of 100 (Dee & Fu, 2004, p. 270). Secondly, the added number of students to the average Arizona classroom would cause a

"change in test scores [of] about 0.032 standard deviations" (Dee & Fu, 2004, p. 269), with my replicated effects producing similar changes in test scores.

While the small effect sizes appear to imply that charter schools are not harming traditional public schools in a meaningful way, I will present evidence in the following section that I believe Dee and Fu's causal argument to be flawed, as I do not believe the parallel trends assumption not met. Therefore, we cannot draw any conclusions from this analysis.

Extension

New States for Robustness Check

As a robustness check, the original authors expand their control samples to include the states of Utah and Nevada. As discussed previously, Dee and Fu ruled out these states as strong controls for Arizona due to Nevada's changing white population and Utah's new class size reduction policy. Rather than using two states that were not suitable controls as a check for robustness, I sought out new states to be used as viable controls against Arizona.

To choose these states, I did not want to be limited, like Dee and Fu, to Arizona's border states. To ensure adequate comparability, I wanted to find states that were similar to Arizona in racial demographics (for white and Hispanic students) and locale breakdown. I also looked for states that, to my knowledge, had no policies that would impact pupil-teacher ratios or notable changes in demographics. First, I eliminated any state with a charter school percentage greater than 1 percent of its total public school count in 2000, since this new state was meant to be used as the control which required absence of the treatment (charter school presence in 2000). Then I calculated the percentage of white students, percentage of Hispanic students, and percentage breakdowns by locale status (rural, small town, suburb, large city). I ranked each state according

to these percentages and then selected the states who were closest overall to Arizona. According to this method, I found that New York and New Jersey were both adequate control states.

Table 4 in the Appendix displays both mine and Dee and Fu's results to this robustness check, using the estimates which controlled for all variables besides locale and level fixed effect. The first row of the table contains the estimates already presented in Tables 2 and 3, but the next row adds Nevada data to the mix and shows a decrease of 0.5 percentage points in white non-Hispanic students and a decrease of 0.35 students per classroom for the pupil-teacher ratio variable—compared with my increases of 0 and 0.2 percentage points, respectively. Though I got an increase rather than a decrease in effects when adding Nevada to the control data, the differences in size were quite small. In the next row, we added Utah data and in the fourth row included data for New Mexico, Utah, and Nevada. Both mine and the original authors' effect sizes in this fourth category were similar to the original control data in row 1.

To extend this table, I added two rows where I used New York then New Jersey as controls for the treatment in Arizona. The effect size in New York was almost double that of New Mexico (-0.030) for the share of white non-Hispanic students and comparable for student-teacher ratio (+1.31 students per classroom). The New Jersey data was also comparable. I believe that these states serve as a stronger robustness checks than Nevada and Utah due to the reasons the authors themselves disqualified them from being adequate control states.

Alternate Segregation Variable

I further extended this paper by choosing a different variable to measure segregation. The authors used the percentage of white non-Hispanic students in a TPS as a metric for segregation, which I did not believe was an adequate measure of how racial breakdowns evolve within a

district. So I created a (more district focused) ratio in which I divided the share of white non-Hispanic students in each school by the share of white non-Hispanic students in a district. Table 5 shows the results of my regression, which yielded a large (-18 percent) but statistically insignificant effect of charter schools on segregation. This high p-value could be due to several factors, including the lack of an effect—which is plausible given the already small effect sizes obtained by Dee and Fu—or the variability of the outcome—since adding in another ratio may make it more difficult to detect a statistically significant effect. Despite my attempt, I believe that finding an accurate measure of segregation is inherently difficult due to the many complex societal factors.

Parallel Trends

The parallel trends assumption states that "in the absence of treatment, treatment and control group outcomes would need to move in parallel" (Angrist & Pischke, 2014, p. 204). Though this assumption is untestable, we can observe pre-trend graphs and infer whether the assumption has been met by visually assessing the parallelism of the pre-treatment plots. The authors briefly discuss the importance of the parallel trends assumption, saying:

> "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" (Dee & Fu, 2004, p. 267).

Despite mentioning this crucial assumption upon which their findings rest, they do not provide much discussion as to how they tried to ensure this assumption was met. In fact, the only other

time the paper references this assumption is when the authors discuss the potential that it was *not* met. They mention that the student-teacher ratios in New Mexico decreased by 0.4 students per classroom between 1991 and 1994, which "could reflect unobserved trends specific to New Mexico" (Dee & Fu, 2004, p. 267). Dee and Fu (2004) justify this reduction by stating "the resulting bias is likely to be fairly small [and] the difference in the state changes after Arizona introduced charter schools is three times larger than the pre-reform difference" (p. 267).

Instead of rationalizing the causality of this analysis due to the small size of the potential bias like Dee and Fu, I plotted the raw averages of the share of white non-Hispanic students in Arizona and New Mexico (see Figure 1 in the Appendix). I used data from 1991-2000, with the treatment year being 1996. The trends are slightly parallel before the treatment year, but appear to be even more parallel after the treatment year. This is the opposite of what we should expect to see in such a graph. I also plotted parallel trends of the raw averages for all the other potential control states, including my additions of New York and New Jersey (see Figures 2, 3, 4, and 5). These graphs similarly are less parallel before the treatment than after. I repeated the process for student-teacher ratios in Arizona and New Mexico (see Figure 6 in the Appendix) and also found inconsistencies in the parallelism of the plots in the pre-treatment years.

These graphs, however, are not enough to debunk Dee and Fu's findings since they present the raw averages of the outcome variables, without controlling for any other variables. To present a plot of the trends conditional on the covariates, I ran a regression on just the covariates and plotted the average residuals. I only had sufficient data (which included all the control variables from the intercensal estimates) for two pre-treatment years: 1993 and 1995. I completed this process for both outcome variables (see Figures 7 and 8 in the Appendix). Both plots clearly show that the average residuals between Arizona and New Mexico are not parallel

in the pre-treatment years, thus this assumption was not met in the pre-treatment years for either outcome variable. This presents a gaping hole in Dee and Fu's causal argument, as this assumption must be met to utilize a difference-in-differences design.

Conclusion

I sought out to replicate Dee and Fu's analysis of how the presence of charter schools in Arizona impacted segregation and per pupil resources in traditional public schools between 1995 and 2000, using New Mexico as the control state. Using a difference-in-differences design, the original authors found a small but statistically significant negative effect of charter schools on the percentage of white non-Hispanic students in TPSs, and small but statistically significant positive effect on student-teacher ratio. They used Nevada and Utah data as a robustness check and found the effect sizes to be comparable to their original estimates. In general, my replication yielded similar effects to those of the original authors.

I extended this paper by searching for better states to add to the robustness check, which included New York and New Jersey. I found the effects from these states to be comparable to the authors' control states, though New York's charter schools had a slightly stronger effect on segregation than any of the other states. I further extended the paper by testing a new metric for a district-level measure of segregation, the ratio of the share of white students in each school to that in a district. Ultimately, this metric yielded statistically insignificant results.

Lastly, and most importantly, I plotted parallel trends—both using raw averages and the average residuals from a regression on just the covariates—for both outcome variables. I found that the parallel trends assumption was not met, and thus the entire causal argument used by Dee and Fu is flawed. Therefore, we cannot draw any meaningful conclusions from this analysis.

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Table 1: Yearly averages 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*	
	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu
Percent white non-Hispanic	0.583 (0.304)	0.566 (0.303)	0.521 (0.305)	0.507 (0.304)	-0.062	-0.059	0.398 (0.258)	0.384 (0.257)	0.358 (0.253)	0.351 (0.252)	-0.040	-0.031
Pupil-teacher ratio	19.1 (3.9)	19.1 (3.9)	18.2 (3.6)	18.2 (3.6)	-0.9	-0.9	17.3 (3.6)	17.2 (3.3)	15.1 (2.9)	15.1 (2.9)	-2.2	-2.1
Observations	1011	1025	1011	1025			676	673	676	673		

* *Note*: This is a replication of Dee & Fu's Table 1

Table 2: Estimated effects of Arizona charter schools on percent white non-Hispanic in traditional public schools

Independent Variables	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu
Arizona x 1999-2000 Year	-0.027[c] (0.006)	-0.026[c] (0.004)	-0.020[c] (0.005)	-0.009[b] (0.004)	-0.017[c] (0.005)	-0.006[a] (0.004)	-0.018[c] (0.005)	-0.013[c] (0.004)	-0.019[c] (0.005)	-0.009[b] (0.004)	-0.018[c] (0.005)	-0.012[c] (0.004)	-0.019[c] (0.005)	-0.008[b] (0.004)
County-level variables														
Percent white non-Hispanic 5-19 year old population		-	1.25[c] (0.04)	1.82[c] (0.21)	1.30[c] (0.05)	1.52[c] (0.24)	1.30[c] (0.05)	1.16[a] (0.23)	1.30[c] (0.05)	0.67[c] (0.25)	1.30[c] (0.05)	1.12[c] (0.23)	1.31[c] (0.05)	0.65[b] (0.25)
Real median household income		-		-	-0.001 (0.001)	-0.053[c] (0.016)	-0.001 (0.001)	-0.039 (0.016)	-0.001[a] (0.001)	-0.038[b] (0.018)	-0.001[b] (0.005)	-0.037[b] (0.016)	-0.001 (0.001)	-0.036[b] (0.017)
Percent of 5-17 year olds in poverty		-		-		-	0.001[c] (<0.001)	0.284[c] (0.055)	0.001[c] (0.001)	0.115[a] (0.061)	0.001[c] (0.001)	0.276[c] (0.055)	0.001 (0.001)	0.111[a] (0.061)
Locale-specific fixed effects?	no		no		no		no		yes		no		yes	
Level-specific year fixed effects	no		no		no		no		no		yes		yes	
R^2	0.9535	0.9848	0.9686	0.9853	0.9687	0.9854	0.9687	0.9857	0.9690	0.9860	0.9690	0.9858	0.9700	0.9862

* *Note*: This is a replication of Dee & Fu's Table 3

[a] Statistically significant at 10 percent level
[b] Statistically significant at 5 percent level
[c] Statistically significant at 10 percent level

Table 3: Estimated effects of Arizona charter schools on student teacher ratios traditional public schools

Independent Variables	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu
Arizona x 1999-2000 Year	1.32[c] (0.17)	1.26[c] (0.16)	1.32[c] (0.17)	1.13[c] (0.18)	1.29[c] (0.18)	1.10[c] (0.19)	1.31[c] (0.18)	1.21[c] (0.20)	1.27[c] (0.18)	1.08[b] (0.21)	1.31[c] (0.18)	1.25[c] (0.20)	1.26[b] (0.18)	1.11[c] (0.21)
County-level variables														
Real median household income		-	0.001[c] (<0.001)	1.39[b] (0.67)	0.001[c] (0.001)	1.57[b] (0.69)	0.001[c] (0.001)	2.15[c] (0.76)	0.001[c] (<0.001)	1.86[b] (0.84)	0.001[c] (0.001)	2.26[c] (0.76)	0.001[c] (0.001)	1.96[b] (0.84)
Percent of 5-17 year olds in poverty		-		-	0.02 (0.02)	2.4 (2.4)	0.02 (0.02)	0.6 (2.6)	.03 (0.02)	3.4 (3.3)	0.02 (0.02)	0.2 (2.6)	0.03[a] (0.02)	3.1 (3.2)
Percent of population aged 65 or older		-		-		-	4.09 (6.33)	23.1[a] (12.9)	2.07 (6.34)	21.6[a] (12.9)	4.61 (6.37)	24.0[a] (12.8)	2.84 (6.44)	22.4[a] (12.8)
Locale-specific fixed effects?		no		no		no		no		yes		no		yes
Level-specific year fixed effects		no		no		no		no		no		yes		yes
R^2	0.7965	0.8119	0.7965	0.8124	0.7966	0.8125	0.7966	0.8128	0.7989	0.8131	0.7967	0.8139	0.8001	0.8143

* *Note*: This is a replication of Dee & Fu's Table 4

[a] Statistically significant at 10 percent level
[b] Statistically significant at 5 percent level
[c] Statistically significant at 10 percent level

Table 4: Estimated effects of Arizona charter schools by control states

	Dependent Variable											
	Percent white non-Hispanic						Pupil-teacher ratio					
Sample	Estimate		R^2		n		Estimate		R^2		n	
	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu	Luna	Dee & Fu
Arizona and New Mexico	-0.018[c] (0.005)	-0.013 (0.004)	0.9687	0.9857	3,374	3,396	1.31[c] (0.18)	1.21[a] (0.20)	0.7989	0.8128	3,374	3,396
Arizona, New Mexico, and Nevada	-0.018[c] (0.006)	-0.018 (0.003)	0.9385	0.9834	3,750	4,172	1.29[c] (0.18)	0.86[a] (0.15)	0.8199	0.8190	3,750	4,172
Arizona, New Mexico, and Utah	-0.018[c] (0.007)	-0.015 (0.003)	0.9405	0.9884	4,114	4,764	1.37[b] (0.21)	1.63[a] (0.15)	0.8382	0.8491	4,114	4,762
Arizona, New Mexico, Utah, and Nevada	-0.020[c] (0.006)	-0.016 (0.003)	0.9352	0.9861	4,886	5,540	1.19[c] (0.18)	1.36[a] (0.14)	0.8428	0.8478	4,886	5,540
Arizona and New York	-0.030[c] (0.007)		0.9792		7,230		1.31[c] (0.18)		0.9040		7,230	
Arizona and New Jersey	-0.017[c] (0.008)		0.9569		5,546		1.32[c] (0.18)		0.8621		5,546	

* *Note*: This is a replication of Dee & Fu's Table 5

[a] Statistically significant at 10 percent level
[b] Statistically significant at 5 percent level
[c] Statistically significant at 10 percent level

Table 5: Estimated effects of Arizona charter schools by segregation variable

	Percent white non-Hispanic			
Segregation variable used	*Estimate*	R^2	*n*	*p*
Using percent of white non-Hispanic students	-0.018 (0.005)	0.9687	3,374	<0.001
Using ratio of white non-Hispanic students in a school to a district	-0.18 (0.112)	0.9524	3,316	0.11

Figure 1: Average Percent White non-Hispanic over Time in Arizona and New Mexico

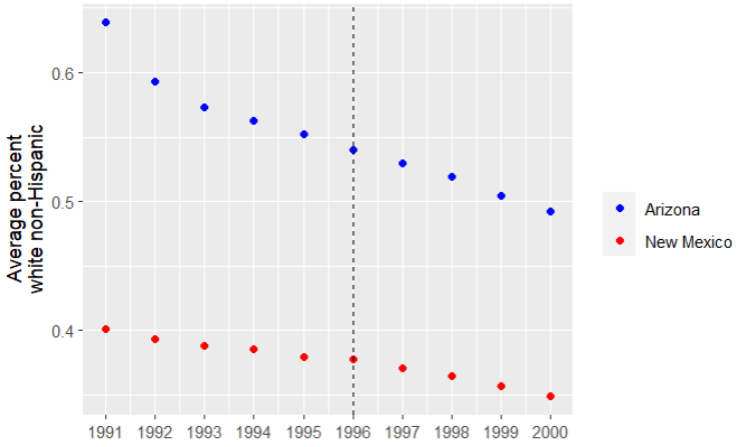

Figure 2: Average Percent White non-Hispanic over Time in Arizona and Nevada

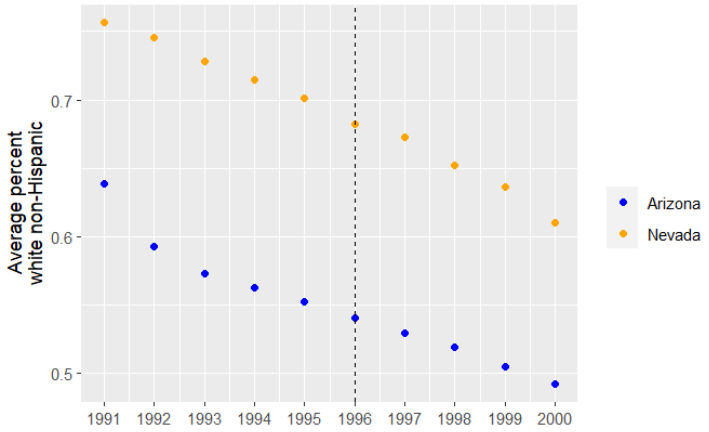

Figure 3: Average Percent White non-Hispanic over Time in Arizona and Utah

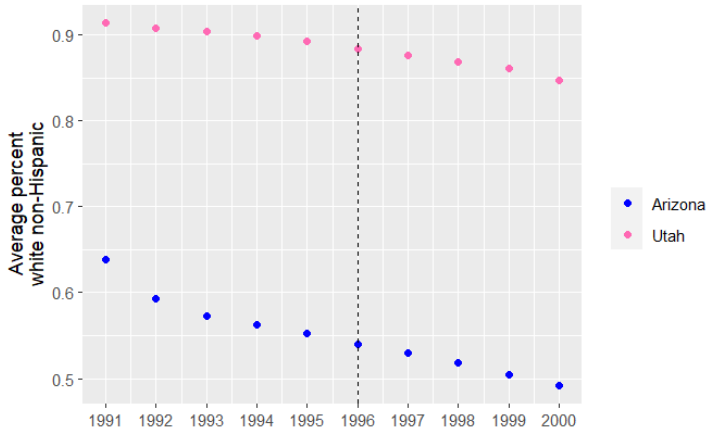

Figure 4: Average Percent White non-Hispanic over Time in Arizona and New York

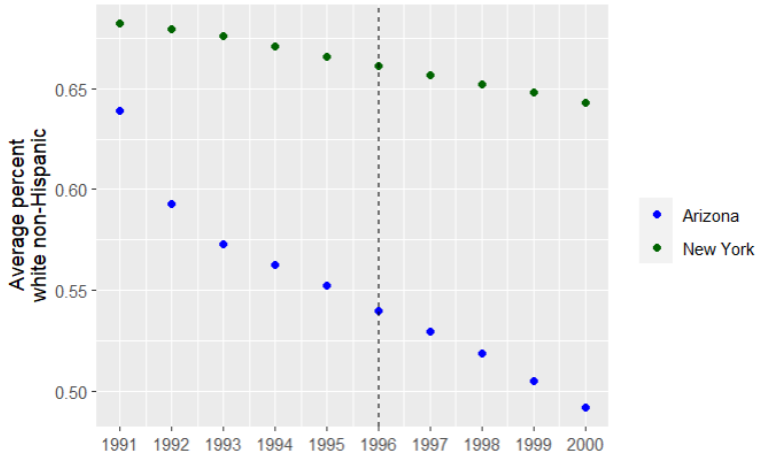

Figure 5: Average Percent White non-Hispanic over Time in Arizona and New Jersey

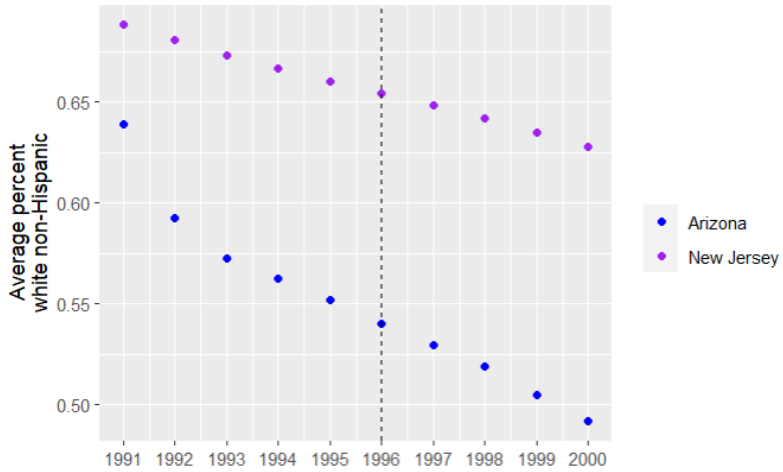

Figure 6: Average Student-Teacher Ratio over Time in Arizona and New Mexico

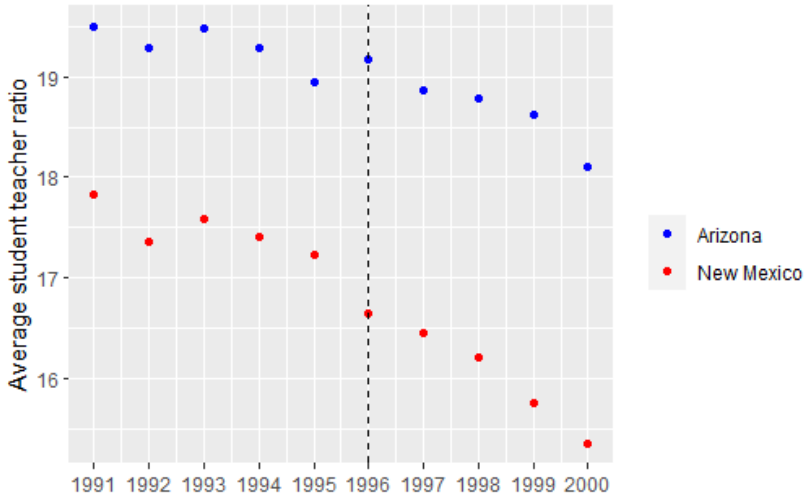

Figure 7: Average Residuals over Time for Percent White non-Hispanic

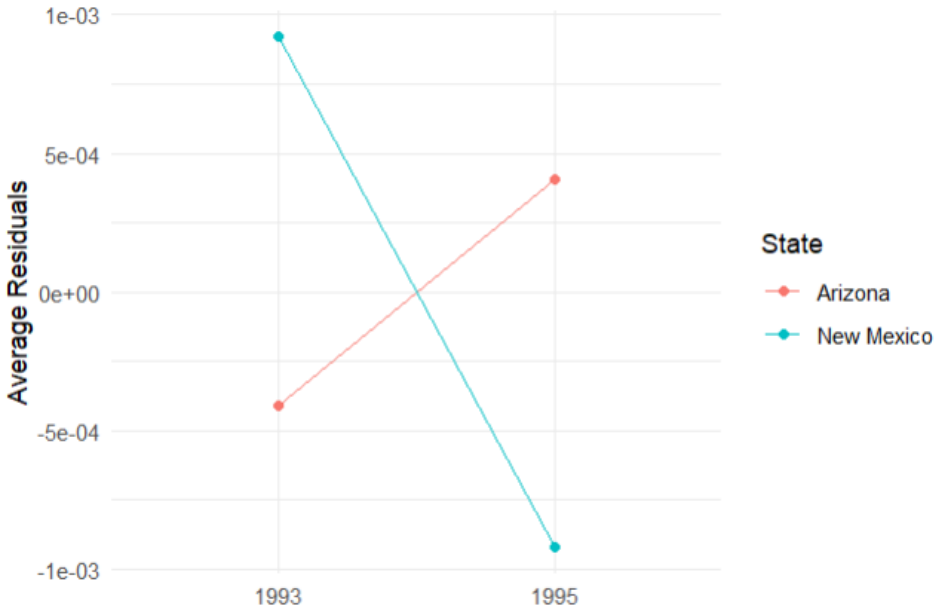

Figure 8: Average Residuals over Time for Student-Teacher Ratio