THREE ESSAYS ON THE ECONOMICS OF EDUCATION

By

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ABSTRACT

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Community colleges are a large part of the nation's higher education system and provide an important access point to post-secondary education for many students. Transfer to a four-year institution is one of the many functions served by community colleges. Despite the importance of the transfer function, the process of transferring between higher education institutions can be confusing for students. In order to reduce the uncertainty surrounding the transfer process, states have formalized and expanded pre-existing institutional transfer agreements to provide clearer linkages between two-year and four-year institutions of higher education, and many schools also maintain institution-to-institution agreements. Chapters 1 and 2 provide a closer look at institution-to-institution policies, and changes in state policies, respectively.

Chapter 1 explores the effects of the transfer admission guarantees (TAG) between California Community Colleges and some University of California (UC) campuses. Specifically, I investigate the impact of TAG policies on transfer to and bachelor's degree completion at UC campuses. These analyses indicate that TAG is positively related to transfer rates and bachelor's degree attainment, but not to the rate at which transfer students graduate. There is no association between TAG policies and the grade point average (GPA) attained by transfer students.

Chapter 2 adds to a growing literature examining the relationship between state post-secondary transfer and articulation policies and the final educational attainment of students who begin at public two-year institutions. Researchers have used both the National Education Longitudinal Study of 1988 and various cohorts of the Beginning Postsecondary Students

Longitudinal Study (BPS). Previous studies use cross-sectional differences in state policies to investigate the effect of such policies on educational outcomes. Most of these studies conclude there is little cross-sectional relationship between state articulation policies and education outcomes such as transfer, credit accumulation, persistence, and degree attainment. In Chapter 2 I build on the existing literature by using multiple cohorts of the BPS, allowing for the examination of changes in state policies over time. I find no evidence of a relationship between state transfer policies and either transfer or degree attainment for beginning public two-year or public four-year students. However, these results are sensitive to the sample used, as well as the policy definition.

Chapter 3 contributes to the important but small body of research on the role of private schools in Indian education. It uses a household dataset from India with a rich set of household covariates and student performance data on reading, writing, and mathematics. For both rural and urban India the results from regression analyses indicate that private school students perform better on tests controlling for covariates. In both contexts, however, the private school benefit becomes largely, statistically, insignificant after conducting multivariate analysis on data balanced using the propensity score matching technique. The paper also makes an initial attempt to identify 'low-fee' private schools; within the regression framework it finds that children in such schools may perform no better than their public school counterparts. The data and methods used in this paper are not without limitations; however these analyses call into question the claim that private school effect may be unequivocally positive and highlights the potential heterogeneity in private school performance in the Indian context.

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Introduction

Community colleges educate a growing number of students, with enrollment growing from roughly 27 percent of undergraduate post-secondary students in 1970 to over 36 percent by 2010 (U.S. Department of Education 2011). Over the past several decades, policymakers have paid increasing attention to student transfer between two-year and four-year institutions. State governments and postsecondary institutions enacted policies aimed at easing transfer in response to perceived low levels of transfer. The effectiveness of these policies has implications for both individuals and state governments, because a year of education at a community college is less expensive for both students and states than a year of education at a four-year public university.

In this paper, I study a transfer admission guarantee (TAG) for students transferring from California community colleges to particular University of California (UC) campuses. A February 1996 Research Synopsis from UC Davis discusses the admission guarantee policy as it relates to both students and the university:

> "Transfer Admission Agreements [later TAG] benefit both students and the campus. By concentrating on a specific set of courses, students can reduce the time spent preparing for transfer. The campus gains by enrolling students with more focused preparation for upper division major coursework; such preparation could lead to improved student performance and reduce the time needed to complete a degree." (p. 1)

It is clear from this statement that the administrators and campuses who put this policy in place believed that it would benefit transfer students in two ways. First, the TAG program would presumably reduce time-to-transfer by focusing students on specific courses and a required grade point average (GPA). Second, transfer students would perform better at the four-year campus and graduate more quickly due to better pre-transfer preparation. The goal of this paper is to investigate whether these expectations are met.

Previous studies in this area concentrated on state-level policies and generally found little association between transfer policies and postsecondary student outcomes. Focusing instead on detailed data from California allows for an analysis of institution-to-institution policies, which may be particularly relevant to students. California is an especially rich source of data on transfer policies, as its 112 community colleges form the largest higher education system in the nation, serving almost 750,000 full-time equivalent (FTE) students in 2008. California has a long history of transfer agreements that guarantee the option to transfer to selected University of California (UC) campuses for community college students who have completed a required number of credits and maintained a minimum GPA. I study the effects of these TAG policies on transfer between two-year and four-year institutions, and on bachelor's degree outcomes of transfer students.

I find evidence that TAG policies increase transfer to UC campuses and bachelor's degrees completed by transfer students at those campuses. Campuses may worry that the quality of these transfers might decline if the marginal students affected by the policy are less prepared for upper-level coursework. However, the data suggest that transfer student quality, as measured by graduation rate or junior-year GPA, does not decline. The magnitude of the transfer and bachelor's degree effects are similar, suggesting that graduation rates among transfer students do not change. Further analysis confirms that graduation rates for students who transferred from community colleges to the UC system are not related to TAG, implying that the average quality of transferring students does not decline in the presence of TAG policies even though the number of transfers increases. Additionally, I find no change in transfer student grade point average (GPA) at UC campuses.

Section 2 gives background on higher education in California, discusses the TAG policy, and reviews the literature. The data used in the analysis are presented in Section 3. Section 4 discusses the methods used to evaluate this particular policy. Section 5 gives the results and Section 6 provides a discussion and conclusion.

Background

Transfer in California

California public higher education consists of three systems: the University of California (UC), California State University (CSU), and the California Community Colleges (CCC). There are currently 9 UC, 23 CSU, and 112 CCC campuses serving undergraduates in California[1].

Figure 1.1 shows transfers to the UC system over time. Transfers rose dramatically between 1989 and 2010. The downward trend in transfers to the UC system beginning in 1993 prompted the UC campuses to agree to try to increase transfer students, which resulted in a 1997 policy change. Figure 1.1 also graphs the average number of UC TAG agreements per community college. The number of TAG agreements per community college rises between 1997 and 2009. I only have complete policy information on TAGs after 1997.

[1] See the working version of this paper for maps of the UC and CCC systems.

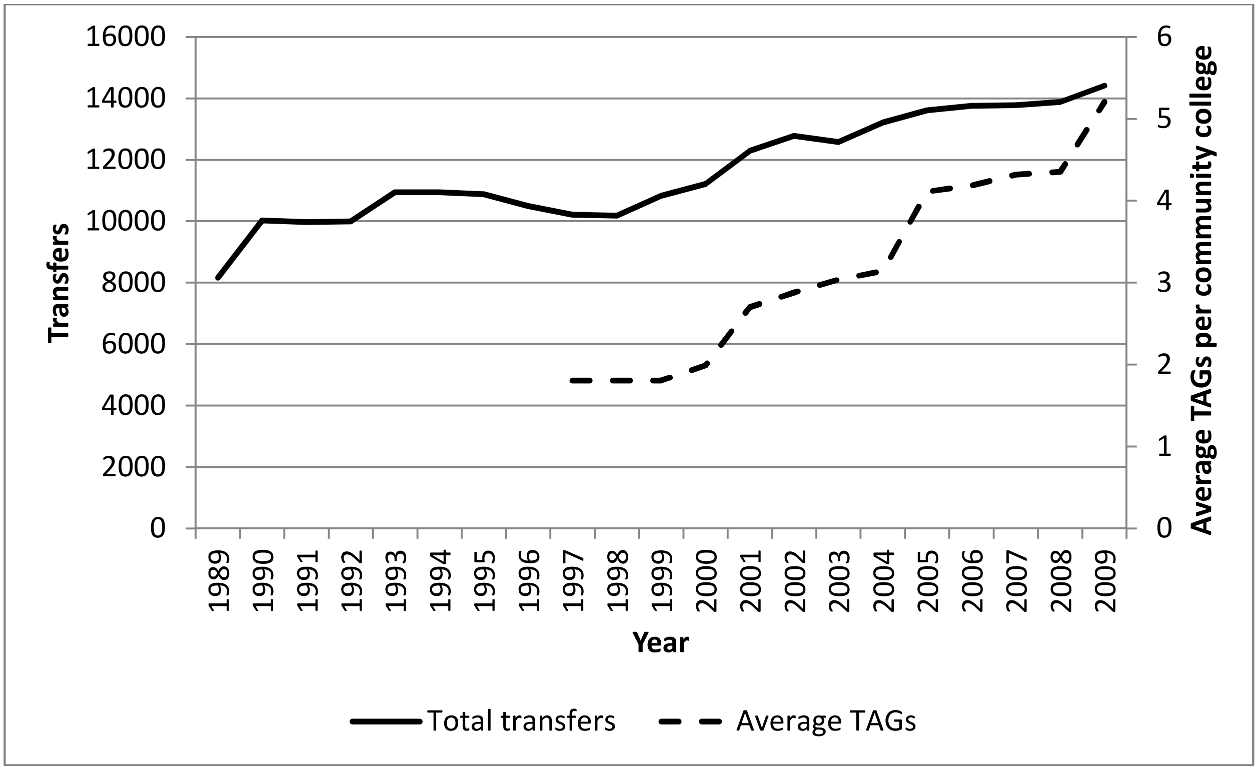

Figure 1.1 Transfers from the California Community Colleges to the UC campuses over time

In 1997 a Memorandum of Understanding (MOU) re-asserted the transfer role and set transfer targets. The signing of this agreement set off a new wave of expansion of the transfer guarantee programs in California. UC campuses that already had transfer agreements in place, such as UC Davis, UC San Diego and UC Santa Cruz extended those agreements to more community colleges. Other campuses initiated a transfer guarantee: for example, UC Irvine and UC Santa Barbara began their programs after 1997.

There were several other transfer-related policies in place in California during this time. One is the Intersegmental General Education Transfer Curriculum (IGETC), which fulfills lower-division general education requirements at both UC and CSU campuses. The "Access to Transfer Information for Community College Students Act", passed by the California State Legislature in 2000, required community colleges to publicize IGETC so that students would know what courses and credits were transferable. The passage of this act suggests that the IGETC may not have been effective prior to 2000 because students may not have known about its existence.

The Articulation System Stimulating Interinstitutional Student Transfers (ASSIST) website (www.assist.org) lists all of the course articulation agreements between each community college and four-year campus in California. While the ASSIST website guides students about particular courses that transfer, the admission guarantee policy is much broader in that it is a guarantee of admission if a student meets certain requirements.

Transfer Admission Guarantee (TAG)

The Transfer Admission Guarantees (TAG) in California, begun at UC Davis in the mid-1980's, expanded to other UC campuses during the 1990s and early 2000s. The TAG policies have also been called Transfer Admission Agreements (TAA), Guaranteed Admission for Transfer Entry (GATE – at UC Santa Cruz), and Preliminary Admission in the Field (PAIF – at UC Irvine). Students typically sign these agreements at the beginning of their second year of community college to apply for admission to a UC campus in the following fall. Students using TAG are considered junior-level transfers. In order to sign the agreement, students generally must have completed 30 transferable semester (45 quarter) units. In addition, many TAG agreements require a minimum grade point average (GPA), which may vary by campus and by major within campus. The GPA requirements ranged from 2.8 to 3.2 during the years I consider below. Students must maintain the minimum GPA and complete a specified number of credits by the spring before they transfer.

Students do not need to sign a TAG agreement in order to be admitted to a UC campus. The UC campuses give priority to junior-level community college applicants over other transfer applicants, including students from other four-year institutions. However, signing a TAG allows for several additional benefits besides priority consideration, such as early review of student records and a guarantee of admission to the campus. In general, the requirements for signing a TAG are more stringent than those needed for regular transfer admission. For example, the GPA minimum for TAG agreements is higher than regular transfer admission GPA. Nonetheless, the benefits of guaranteed admission are sufficient to encourage a non-trivial portion of transfer students to sign a TAG.

The number of students signing TAGs varies from campus to campus. According to the UC Davis Research Synopsis reports from 1996 and 2000, the number of TAGs (then called TAAs) signed at UC Davis was 202 in 1987-88, 792 in 1994-95, and 716 in 1998-99. These agreements accounted for 23%, 44%, and 40%, respectively, of entering community college transfer students at UC Davis in those academic years. In 1994-95, 35% of all entering transfer students signed an agreement. The number of TAGs submitted to UC Davis for review in 2009-10 exceeded 3,000. However, the number of TAGs signed was much lower at UC Merced, with only around 200 students signing a TAG in 2009-10. Students who sign TAGs are more likely to enroll in UC Davis than transfer students admitted without signing a TAG. For example, in 1998-99, 62% of TAG signers enrolled at UC Davis, compared to 50% of other advanced standing applicants.

In 2007, the seven UC schools that use admission guarantees agreed to a common name – Transfer Admission Guarantee (TAG). In addition, the UC campuses decided to use a common TAG application form for all campuses. The TAG application became available on-line in the summer of 2010 for students seeking fall 2011 admission. While UC Berkeley and UCLA do not participate in the TAG program, they do offer priority admission to CCC transfer students.

In many ways the California experience with Transfer Admission Guarantees (TAG) provides ideal variation to study the effect of policies on transfer students' experiences. Since 1997, the TAG agreements have grown in two ways. First, they rolled-out across the participating UC campuses over time. Second, for some UC campuses the agreements generally began regionally and then expanded to include community colleges across the state of California. For example, UC Davis expanded its program from 56 community colleges in 2000 to 94 partner colleges by fall 2008. UC Santa Cruz expanded its TAG program from 20 community colleges in 2000 to 102 by 2008. The empirical analysis in this paper uses both of these sources of variation in exposure to the TAG policy to identify the effects of TAG on post-secondary outcomes.

Relevant Literature

Several papers examine whether there is an association between state transfer policies and student outcomes. The most common outcomes studied are the probability of transfer and, conditional on transfer, the probability of receiving a bachelor's degree as well as time-to-degree. So far, the bulk of research concludes that the presence of a state policy does not increase the transfer rate between 2-year and 4-year institutions. The datasets used are the National Education Longitudinal Study (NELS) 88/2000, and the Beginning Postsecondary Students (BPS) 89/94 longitudinal study.

The studies that use the NELS:88/2000 are Goldhaber and Gross (2009), Roksa and Keith (2008) and Reynolds (2007). Goldhaber and Gross (2009) attempt to classify 'strong' and 'weak' articulation policies. However the authors find only small effects on transfer. Gross and Goldhaber also conclude that state articulation policies are associated with higher odds of transfer for Hispanic students but not for other minority groups or first generation college attendees. Roksa and Keith (2008) use a simple indicator for whether a state has a transfer policy to investigate the outcomes of transfer, bachelor's degree attainment, and time-to-degree. They find no effect of a state transfer policy on these outcomes. Reynolds, in a 2007 dissertation, looks at the effect of state policies on students by using propensity score matching. He matches students who have similar characteristics on the outcome of living in a state with a transfer policy. He also runs his analysis separately for men and women. Reynolds' finds that articulation agreements raise educational attainment for male college attendees, but not overall attainment for the cohort of high school graduates.

Anderson, Sun, and Alfonso (2006) use the BPS 89/94 to look at transfer rates between two-year and four-year institutions. They define their policy as presence of a legislated transfer policy in a state by 1991. They find no effect of presence of a transfer policy on transfer in a state.

Two studies examine the relationship between California's transfer agreements and early post-secondary outcomes of transfer and junior-year GPA at the transfer UC campus. The first study finds a relationship between a community colleges transfer rate and the use of Transfer Admission Agreements (TAAs) and Transfer Admission Guarantees (TAGs) (Transfer Velocity Project, RP Group, 2010). In particular, the Transfer Velocity Project showed a positive association between a community colleges transfer rate and the number of students signing TAAs or TAGs with a UC or CSU institution.

The second study by Dupraw and Michael (1995) studies the early outcomes of TAG transfer students at UC San Diego (UCSD). They compare junior GPA at UCSD for students who transferred with a TAG to community college students who transferred without a TAG, and to students who began their studies as freshman at UCSD (known as native students). Their data covers three cohorts of transferring students, from fall 1988 to spring 1991. This period was in the very early stages UCSD's TAG program with only a few local community colleges participating. The authors find that both types of transfer students obtain roughly the same GPA, and that this GPA is only slightly lower than that received by students who entered the university as freshman. Transfer students who earned a higher community college GPA were less likely to face academic probation at UCSD due to poor academic performance. The authors relate this higher level of academic success to the increase in the GPA requirement for TAG students from 2.4 in fall 1988 to 2.8 in fall 1990. This paper expands on these two studies by relating the *expansion* of the TAG policy to transfer and bachelor's degree outcomes, as well as several other post-secondary outcomes.

None of the studies listed above is able to take advantage of policy changes over time, which may be one reason why they find little relationship between state transfer policies and post-secondary outcomes. Another reason these studies may find little effect on post-secondary outcomes is that state-level policies generally supplement existing institution-to-institution agreements. Studying institutional transfer policies may reveal more about what types of policies can affect post-secondary outcomes for students.

Data

The data used in the analysis come primarily from publicly available data at the California Postsecondary Education Commission (CPEC) website (http://www.cpec.ca.gov). The data include transfers between each two-year and four-year public institution in California[2], and bachelor's degree outcomes for transfer students at the four-year campuses. The transfer data

[2] Data on transfers between community colleges, and from community colleges to in-state private or out-of-state institutions is not available for all years and missing for some institutions. As a result, this transfer data will not be used in the analysis.

cover both fall-term and full-year transfers from each community college to each UC campus. Transfer data are coded as occurring in the fall of the academic year.

Bachelor's degree data consist of the number of bachelor's degrees received each year at each public four-year institution by transfer students from each sending community college. Unlike the transfer data, the bachelor's degree data is coded as occurring in the spring of the academic year. That is, students who receive a bachelor's degree in the 1998-1999 academic year are coded as receiving that degree in 1999. The data publicly available from CPEC provide snapshots of transfer and graduation, but does not follow cohorts of students over time.[3]

I compile the policy variable mainly from information in the Answers for Transfers publication from the University of California. Other sources, including campus reports, email correspondence with Admissions and TAG representatives at the UC campuses, and on-line searches supplemented the Answers for Transfers information. I consider a TAG policy in effect the fall of the first academic year that transfer students were accepted using TAG. This paper analyzes the implementation of the TAG policy between 1997 and 2006. Appendix A provides information on the number of community colleges that had a TAG with each UC campus over time.

Other covariates account for possible outside labor market opportunities in the county where the community college is located. These include county-level employment rates, median household income, county population, county population growth rate, percent male, percent white, and percent in age categories zero to 14, 15 to 29, and 30 to 49.

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[3] Pair-level bachelor's degree data reports the number of transfer students from community college *j* that received a bachelor's degree at four-year campus *h* in year *t*, but does not note when students transferred. Data linking students over time was not available.

Currently, there are 112 community colleges in California. I restrict the analysis to the 107 community colleges that were open for the entire period of the study. There were eight UC campuses open during the entire study period (UC Merced opened in 2005-06). UC Berkeley and UCLA never had a TAG policy while UC Riverside had a TAG with all California community colleges by 1997. Therefore, the policy variation comes from schools added to the TAG program at UC Davis, Irvine, San Diego, Santa Barbara, and Santa Cruz. Of these five UC campuses, UC Davis had the most agreements, with 56 community colleges, as of 1997. On the other hand, UC Irvine and UC Santa Barbara did not have a guaranteed transfer program in place in 1997. By 2009, the UC campuses with a TAG program, with the exception of UC Santa Cruz, had added all community colleges.

Methods

I now turn to the empirical specification, which uses the expansion of the TAG policy to estimate a differences-in-differences model.

Empirical Specification

I perform the analysis at the community college level by relating the expansion of UC TAGs at the community college to transfer and bachelor's degree outcomes for students from that college.[4] For each community college the outcomes of interest are aggregate transfers to UC campuses, and aggregate bachelor's degrees given to transfer students at UC campuses in

[4] I also conducted several analyses examining the impact of TAG between a community college – UC campus pair on transfers between the pair and bachelor's degree outcomes of transfer students at the UC campus. These analyses can be found in Appendix C.

each academic year. Transfer and bachelor's degree outcomes at UC Merced are not included in this analysis.[5]

When the outcome is transfers, Y_{jt} is the log of transfer students from community college j to the UC system in year t. The year t is the fall of the academic year in which the student transferred. Specifically, I estimate

(1) $Y_{jt} = \alpha + \beta \#TAG_{jt} + \eta X_{jt} + \lambda_t + \theta_j + \varepsilon_{jt}$,

The variable of interest, $\#TAG_{jt}$, defines exposure to TAG as the number of TAG agreements the community college has with UC campuses. X_{jt} is a set of county labor market and demographic characteristics defined in the data section above. Equation (1) also includes a set of year fixed effects, λ_t, community college fixed effects, θ_j, and a random error term ε_{jt}.

Transfer is a direct outcome, but the goal of transfer policies is to help students attain bachelor's degrees. Therefore, degree completion is perhaps a better way to evaluate transfer policies. To analyze the effect of the TAG policy on graduation with a bachelor's degree I estimate

(2) $Y_{jt} = \alpha + \beta \#TAG_{j,t\text{-}3} + \eta X_{jt} + \lambda_t + \theta_j + \varepsilon_{jt}$,

where Y_{jt} is the log bachelor's degrees obtained by transfer students from community college j in the UC system in year t. The variable of interest, $\#TAG_{j,t\text{-}3}$, is a three-year lag of the TAG policy variable in (1). For example, the three-year lag means that students obtaining a bachelor's

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[5] For example, when aggregating transfers or bachelor's degrees at UC campuses, I include all UC campuses except UC Merced. The same applies to aggregating the independent variable for all UC campuses.

degree in the 1999-2000 school year (coded as 2000), are given the value of the TAG policy in 1997. All other variables are defined above in equation (1).

Equations (1) and (2) relate the number of TAG agreements to community college level outcomes. However, there may be differential impacts of the TAG policy based on how far the community college is from the UC campus. Therefore, I define $TAGclosest_{jt}$ as an indicator variable for having a TAG with the closest UC campus, while also controlling for the number TAG agreements with other UC campuses, $\#TAGnotclosest_{jt}$. Then, I estimate

$$(3) \qquad Y_{jt} = \alpha + \beta TAGclosest_{jt} + \gamma \#TAGnotclosest_{jt} + \eta X_{jt} + \lambda_t + \theta_j + \varepsilon_{jt}.$$

Y_{jt} again represents transfer and bachelor's degree outcomes at either the closest UC campus[6] or in the UC system. When analyzing bachelor's degrees, the two TAG policy variables are lagged by three years as shown above in equation (2). All other variables are defined as above in equation (1).

Results

Baseline Results

Table 1.1 contains descriptive statistics for the analysis sample. The average number of annual transfers from a community college to the UC system is 115.29, while there are 56.04 transfers to the closest UC campus. The corresponding averages for bachelor's degrees received by transfer students range from 52.11 at the closest UC campus to 106.77 in the UC system.

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[6] The closest UC campus is defined without considering UC Merced.

Table 1.1 Descriptive statistics of the analysis sample

	Observations (1)	Mean (2)	SD (3)
Transfers			
Year	1,040	2001.49	2.87
UC system	1,040	115.29	137.63
Closest UC campus	1,003	56.04	78.61
Bachelor's degrees			
UC system	1,040	106.77	130.03
Closest UC campus	1,003	52.11	74.52
Policy Variables			
#TAG	1,040	2.76	1.19
TAG with closest	1,040	0.53	0.50
#TAG not closest	1,040	2.24	1.17
Other Community College Outcomes			
Associate's	1,040	658.93	394.25
First-year UC GPA for transfers	716	2.90	0.19
1-year UC system persistence	716	90.71	6.34
2-year UC system graduation rate	716	45.07	11.13
CSU system transfers	1,040	471.94	324.92
Community College County Demographics			
Employment rate	1,040	93.93	2.713
Total population (in 10,000s)	1,040	291.90	344.20
% Male	1,040	50.01	1.295
% White	1,040	79.52	9.459
% 0-14 years old	1,040	22.48	2.873
% 15-29 years old	1,040	21.47	2.096
% 30-49 years old	1,040	30.53	2.161
% population change	1,040	1.086	1.068
Median household income	1,040	48,500	11,643

Table 1.2 contains baseline estimates of specifications (1)-(3).[7] Multiplying the reported coefficients by 100 gives an approximation to the percent change in the corresponding dependent

[7] Table 1.7 in Appendix B contains baseline results using a negative binomial specification.

variable. Column (1) implies that a community college adding one more TAG policy with a UC campus is associated with a seven percent increase in transfers to the UC system.

Table 1.2 Baseline estimates of the TAG effect on transfer to and bachelor's degrees from UC campuses

	Ln(Transfers to UC system) (1)	Ln(BA at UC system) (2)	Ln(Transfers to UC system) (3)	Ln(BA at UC system) (4)	Ln(Transfers to closest UC) (5)	Ln(BA at closest UC) (6)
#TAG	0.07***					
	[0.02]					
#TAG (lag 3)		0.08***				
		[0.02]				
TAG w/ closest			0.02		0.13*	
			[0.05]		[0.07]	
#TAG not closest			0.08***		0.11***	
			[0.02]		[0.03]	
TAG w/ closest (lag 3)				-0.01		0.14**
				[0.05]		[0.07]
#TAG not closest (lag 3)				0.09***		0.07***
				[0.02]		[0.03]
Observations	1,040	1,040	1,040	1,040	1,003	1,003
R-sq	0.15	0.15	0.15	0.15	0.11	0.08
#CC	106	106	106	106	105	105

Note. -- All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49. All models also include year and community college fixed effects. Standard errors, clustered at the community college level, are in brackets.
*** p<0.01, ** p<0.05, * p<0.1

An additional TAG agreement is associated with an eight percent increase in bachelor's degrees, as shown in column (2). Taken together, the estimates in columns (1) and (2) suggest that TAG is associated with a similar percent increase in transfers and bachelor's degrees, implying no change in the graduation rate of transfer students.

Columns (3) and (4) repeat the analysis in columns (1) and (2), but split the policy variable into two parts: whether the community college has a TAG with the closest UC campus and the number of TAG agreements with the other UC campuses. The coefficient on having a TAG with the closest UC campus is not statistically significant in either the transfer or bachelor's degree regression. An additional TAG with a UC campus not closest to your community college is associated with an eight percent increase in transfers to the UC system, and a nine percent increase in bachelor's degrees. Therefore, after accounting for the number of other TAG agreements, adding a TAG with the closest UC campus does not affect transfers to the UC system.

Columns (5) and (6) of Table 1.2 consider slightly different dependent variables – specifically transfer and bachelor's degree outcomes at only the closest UC campus. Adding a TAG with the closest campus is associated with a 13% increase in transfers to the closest UC campus and a 14% increase in bachelor's degrees. The coefficient in the transfer regression is only statistically significant at the ten percent level, while the coefficient in the bachelor's degree regression is significant the five percent level. The coefficient on the number of TAG agreements with other UC campuses is positive and statistically significant in both columns (5) and (6). It is not clear why the number of other TAG agreements significantly impacts transfers to and graduation rates at the closest UC campus after accounting for the TAG with that campus. One possible explanation is that the number of TAG agreements captures the amount of publicity of the TAG program at the community college campus. Therefore, even conditional on a TAG with the closest UC campus, students will be more informed about transfer requirements to UC campuses. This broader awareness of transfer options may lead to the positive and statistically significant coefficient on the number of other TAG agreements.

The baseline results suggest that additional TAG agreements increase both transfers and bachelor's degrees at UC campuses. The associated percent changes in transfers and bachelor's degrees are larger when looking at the TAG agreement with the closest UC campus and outcomes at that campus. Overall, the results suggest similar percent changes in transfers and bachelor's degrees, implying no change in the graduation rate of transfer students at UC campuses.

Further Analysis Using Additional Community College Level Data

The University of California StatFinder contains publicly available persistence, graduation, and grade point average (GPA) information for transfer students from each community college to the UC system (UC StatFinder http://statfinder.ucop.edu/default.aspx). One-year persistence rate and one-year GPA are measured from 2000 to 2007, and two-year graduation rates from 2000-2006.[8] Panel A of Table 1.3 uses the number of TAG agreements as the independent variable of interest, while Panel B uses TAG with the closest UC campus and number of other TAG agreements as the policy variables. Columns (1) and (2) reproduce the baseline results of Table 1.2, but only include the years for which the StatFinder data is available. Column (1) of Panel A shows a statistically significant seven percent increase in transfers to the UC system related to adding one more TAG agreement with a UC campus, identical to the baseline result in Table 1.2. However, the impact on bachelor's degrees is different from the baseline results. However, the impact on bachelor's degrees is different from the baseline results. The coefficient on TAG in column (2) is three percent, smaller than the eight percent from Table 1.2, Column (2) and no longer statistically significant. The coefficients

[8] Table 1.8 in Appendix B contains additional results using logs the three- and four-year graduation rate, and the graduation GPA as outcomes. The three-year graduation rate is measured from 2000-2005, and the four-year graduation rate and graduation GPA are measured from 2000-2004.

Table 1.3 Further community college level analysis using restricted years

	Ln(Transfers to UC system) (1)	Ln(BA at UC system) (2)	Ln(1-yr persistence at UC system) (3)	Ln(1-yr GPA at UC system) (4)	Ln(2-yr graduation rate from UC system) (5)
			A. #TAG		
#TAG	0.07***		0.01	<0.01	0.02
	[0.02]		[0.01]	[0.01]	[0.02]
#TAG (lag 3)		0.03			
		[0.03]			
			B. Closest TAG		
TAG w/ closest	0.03		0.01	-0.01	-0.02
	[0.06]		[0.02]	[0.02]	[0.05]
#TAG not closest	0.07***		<0.01	<0.01	0.03
	[0.02]		[0.01]	[0.01]	[0.02]
TAG w/ closest (lag 3)		-0.11			
		[0.09]			
#TAG not closest (lag 3)		0.04			
		[0.03]			
Observations	716	716	716	716	716
#CC	106	106	106	106	106

Note. -- All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49. All models also include year and community college fixed effects. Standard errors, clustered at the community college level, are in brackets.
*** p<0.01, ** p<0.05, * p<0.1

on the TAG with closest UC variable from columns (1) and (2) in Panel B of Table 1.3 match in sign and significance with the baseline results from columns (3) and (4) of Table 1.2. The coefficient on the TAG with closest UC variable in column (2) is more negative than that in Table 1.2, Column (4) although neither coefficient is statistically different from zero.

Column (3) reports results using the log of the one-year persistence rate of transfer students in the UC system as the dependent variable. The coefficient on the #TAG variable suggests that there is no association between persistence and the number of TAG agreements. Similarly, the estimates in column (3) of Panel B show that there is no statistically significant relationship between adding a TAG with the closest UC campus and changes in one-year persistence. Columns (4) and (5) report results from regressions where the dependent variable is the log of first-year UC GPA and the log of two-year UC graduation rate, respectively. None of the coefficients on the TAG policy variables in Panels A or B are statistically different from zero. The results in column (5) support the results from the baseline analysis that finds no change in the graduation rate. Column (4) suggests there is no relationship between additional TAG agreements and changes in the quality of transfer students to the UC system as measured by GPA. Overall, Table 1.3 confirms the baseline results suggesting that the graduation rate among transfer students does not change in response to changes in TAG policies.

Event History Analysis Using TAG with the Closest UC Campus

To provide a more complete picture of the effect of the TAG policies, I also estimate event history models that trace out the time path of the effects of TAG:

(4) $$Y_{jt} = \alpha + \sum \pi_k D_j 1(t - T_j = k) + \eta X_{jt} + \lambda_t + \theta_j + \varepsilon_{jt},$$

where Y_{jt} measures log transfers from community college j either to the UC system or to the closest UC campus in year t. D_j is a dummy variable equal to one if the community college ever got a TAG agreement with its closest UC campus, and equal to zero otherwise. The indicator function $1(t - T_j = k)$ equals one if the community college is k years from the enactment of the TAG agreement with the closest UC campus. The omitted category is two years prior to when TAG is enacted between the community college and its closest UC campus. Community colleges are observed two years pre- and post-policy. All specifications include year and community college fixed effects.

Table 1.4 presents the results from the event history specification given in equation (4). Columns (1) and (2) use the log of transfers to the UC system as the outcome variable, while columns (3) and (4) use transfers to the closest UC. Columns (2) and (4) present results from equation (4). Columns (1) and (3) present estimates of the TAG policy variables from using the specification in equation (3) on the event-history sample. Columns (1) and (2) show positive but statistically insignificant variables related to TAG with the closest UC campus. The imprecise estimates are likely a result of the small number of community colleges and observations in the event history sample. These results mirror the findings in column (3) of Table 1.2 that a TAG with the closest UC campus is not related to transfers to all UC campuses. However, the coefficient on the number of other TAG agreements is negative but statistically insignificant here, while it was positive and statistically significant in the baseline results in Table 1.2. Taken together, these results suggest that adding a TAG with the closest UC campus does not impact transfers from the community college to the UC system as a whole. However, when looking at the impact on transfers to the closest UC campus, column (4) shows positive and statistically significant coefficients beginning the first year the community college gets a TAG with the closest UC campus. These estimates are larger in magnitude but have the same sign as the results from column (5) of Table 1.2. Column (3) shows a negative but statistically insignificant coefficient on the TAG with closest UC campus variable. Overall, the results in Table 1.4 are broadly consistent with the findings in Table 1.2. However, the small number of community colleges in the event study sample results in imprecise estimates, limiting what can be learned from these specifications.

Table 1.4 Event history analysis using a balanced panel

	Ln(Transfers to UC system) (1)	Ln(Transfers to UC system) (2)	Ln(Transfers to closest UC) (3)	Ln(Transfers to closest UC) (4)
Excluded: -2				
-1		-0.01		0.26
		[0.25]		[0.26]
0		0.38		0.74**
		[0.29]		[0.27]
1		0.27		0.88*
		[0.39]		[0.43]
2		0.44		1.53**
		[0.44]		[0.59]
#TAG not closest	-0.12	-0.13	-0.10	-0.12
	[0.08]	[0.08]	[0.13]	[0.14]
TAG closest	0.31		-0.02	
	[0.22]		[0.20]	
Observations	113	113	105	105
R-sq	0.43	0.45	0.53	0.55
#CC	24	24	24	24

Note. -- The event study analysis is based on when the community college gets a TAG with the closest UC campus. All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49. All models also include year and community college fixed effects. Standard errors, clustered at the community college level, are in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Additional Community College Outcomes

Table 1.5 examines other community college level outcomes to look for evidence of TAG impacts outside of the intended target of transfers to and graduation from UC campuses.[9] It is possible that the estimated increase in transfers to UC campuses in Table 1.2 does not measure an overall increase in transfers from each community college, but rather a shift in transfers from

[9] Table 1.9 in Appendix B contains results using a negative binomial specification.

CSU campuses to UC campuses. I use additional data on transfers and bachelor's degrees at CSU campuses from the CPEC website to investigate the possible diversion effects of the UC TAG policy. The CSU transfer and bachelor's outcomes are aggregated for each community college to the CSU campus system. Panel A shows the coefficient on the #*TAG* variable while Panel B reports results from regressions using the closest TAG.

Columns (1) and (2) use the log of transfer and bachelor's degree outcomes in the CSU system as the dependent variables. Panels A and B show similar small, positive coefficients on the TAG policy variables. None of the coefficients are statistically significant. The results using outcomes in the CSU system suggest no impact of the UC TAG policy on transfer, bachelor's degree completion, or graduation rates at the CSU campuses. It should be noted that some CSU campuses also had an admission guarantee during this period. I was not able to get full policy information for the CSU campuses to conduct an analysis of CSU TAG. However, it appears that the expansion of the UC TAG policy did not simply divert transfers from the CSU campuses to UC campuses.

The UC TAG policy may also affect associate's degree completion at the community colleges. While students do not need to complete an associate's degree to take advantage of TAG, the TAG course and GPA requirements may affect associate's degree attainment. Information on the number of associate's degrees granted by each community college in each year comes from the CPEC website. Column (3) of Table 1.5 shows the coefficients on the policy variables using the log of associate's degrees as the outcome. Panel A shows no statistically significant relationship between the number of TAG agreements and associate's degrees awarded at the community college. There are no statistically significant coefficients in Panel B. The results using associate's degrees suggest no major impact of TAG on associate's degrees received at the community college.

Table 1.5 The relationship between TAG and additional community college level outcomes

	Ln(Transfers to CSU system) (1)	Ln(BA at CSU system) (2)	Ln(AA from community college) (3)
		A. #TAG	
#TAG	0.02		-0.00
	[0.01]		[0.02]
#TAG (lag 3)		0.01	
		[0.01]	
		B. Closest TAG	
TAG w/ closest	0.02		0.03
	[0.03]		[0.05]
#TAG not closest	0.01		-0.01
	[0.01]		[0.03]
TAG w/ closest (lag 3)		0.02 [0.03]	
#TAG not closest (lag 3)		0.01 [0.01]	
Observations	1,040	1,040	1,040
#CC	106	106	106

Note. -- All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49. All models also include year and community college fixed effects. Standard errors, clustered at the community college level, are in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Conclusion and Discussion

This paper assesses the effects of inter-institution guaranteed transfer policies on post-secondary outcomes for students who begin their postsecondary education at a community college. Estimates suggest that TAG is associated with increases in transfers to and bachelor's degree completions at UC campuses. However, the similar magnitude of the transfer and degree impacts suggests no change in the graduation rates of transfer students. In addition, TAG has little effect on persistence or GPAs of transfer students into the UC system. Overall, these results suggest that TAG is associated with increases the number of students transferring and attaining bachelor's degrees with little change in the quality of these transfer students.

What aspects of the UC TAG program are likely to be responsible for these patterns in the data? From a student perspective, TAG provides both information and certainty in the transfer process. Information comes from both the specified course and GPA requirements, as well as early review of student records to ensure the student is on track to meet those requirements prior to transfer. The GPA and course requirements may also be related to maintaining the quality of transfer students, as measured by GPA and graduation rate, even though the number of transfers increases.

In addition, TAG provides certainty to the admissions directors UC campuses. Students may only sign a TAG with one UC campus. This rule was *de facto* enforced during my study period due to each UC campus having separate TAG forms. UC Davis noted that TAG signers were more likely to enroll than transfer students admitted without signing a TAG. After the study period, the UC campuses experimented with letting students sign multiple TAGs through a common application form or an online form. When students could sign TAGs with multiple UC campuses, it lowered the probability that a TAG applicant would enroll at a particular UC campus. While the TAG application remains online, the UC campuses now enforce that students may only sign a TAG with one UC campus, although students can still apply to multiple campuses. The one-TAG policy likely ensures that students who sign TAG agreements are more likely to enroll than other advanced-standing transfer applicants. Therefore, the TAG program provides greater certainty to UC campuses about the number of students they will enroll in the following year.

While I can speculate about the potential mechanisms of the TAG policy, future research should explore other transfer policies to shed light on policy components that are particularly effective. Do other institution-to-institution or state-level guaranteed transfer policies lead to the same outcomes? What is the effect of transfer policies that do not contain a guarantee? In addition to examining other transfer policies, future research may benefit from longitudinal student-level data. First, student-level data would allow for construction of cohort transfer and graduation rates for each pair of campuses, which is not possible with the current publicly available data used in this paper. Second, student data would allow for a more in-depth analysis of how students from different racial, ethnic, or socioeconomic groups respond to the policy.

APPENDICES

APPENDIX A

Policy Appendix

Table 1.6 Number of community college campuses with a TAG agreement with each UC campus in selected years

	UCB	UCD	UCI	UCLA	UCM	UCR	UCSD	UCSB	UCSC
1986	None	3	None	None	.	None	None	None	None
1988	None	25	None	None	.	None	3	None	None
1995	None	56	None	None	.	?	?	None	?
1997	None	56	None	None	.	All	14	None	17
1998	None	56	None	None	.	All	14	None	17
1999	None	56	None	None	.	All	14	None	17
2000	None	56	16	None	.	All	15	None	20
2001	None	60	16	None	.	All	16	None	92
2002	None	70	19	None	.	All	17	3	92
2003	None	81	19	None	.	All	17	9	94
2004	None	81	19	None	.	All	24	10	97
2005	None	82	22	None	All	All	26	All	99
2006	None	90	22	None	All	All	27	All	99
2007	None	90	29	None	All	All	33	All	101
2008	None	94	29	None	All	All	33	All	101
2009	None	All	29	None	All	All	All	All	103
2010	None	All	All	None	All	All	All	All	103

APPENDIX B

Additional Community College Level Tables

Table 1.7 Baseline results using negative binomial specification

	UC System				Closest UC	
	Transfers	BA	Transfers	BA	Transfers	BA
	(1)	(2)	(3)	(4)	(5)	(6)
#TAG	0.06***					
	[0.01]					
#TAG (lag 3)		0.06***				
		[0.01]				
TAG w/ closest			0.03		0.25***	
			[0.03]		[0.04]	
#TAG not closest			0.06***		0.06***	
			[0.01]		[0.01]	
TAG w/ closest				0.03		0.17***
(lag 3)				[0.02]		[0.04]
#TAG not closest				0.06***		0.05***
(lag 3)				[0.01]		[0.01]
Observations	1,062	1,055	1,062	1,055	1,070	1,060
#CC	107	106	107	106	107	106
Year FE	X	X	X	X	X	X

Note. -- All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49. All models also include year and community college fixed effects. Standard errors, clustered at the community college level, are in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.8 Further community college level analysis using restricted sample

	UC Grad. Rate		Graduation
	Ln(3-year)	Ln(4-year)	Ln(GPA)
	(1)	(2)	(3)
	A. #TAG		
#TAG	0.01	0.01	-0.00
	[0.01]	[0.01]	[0.00]
	B. Closest TAG		
TAG w/ closest	0.00	-0.01	-0.01
	[0.04]	[0.03]	[0.01]
#TAG not closest	0.01	0.01	-0.00
	[0.01]	[0.01]	[0.01]
Observations	615	510	510
R-squared	0.06	0.05	0.08
#CC	106	105	105

Note. -- All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49, as well as year and community college fixed effects. Standard errors, clustered at the community college level, are in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.9 Additional community college level outcomes using negative binomial specification

	Associate's	CSU System	
	AA	Transfers	BA
	(1)	(2)	(3)
#TAG	0.02**	0.03***	
	[0.01]	[0.01]	
#TAG (lag 3)			-0.01
			[0.01]
TAG w/ closest	0.02	0.02	
	[0.02]	[0.02]	
#TAG not closest	0.02**	0.02**	
	[0.01]	[0.01]	
TAG w/ closest (lag 3)			-0.06***
			[0.02]
#TAG not closest (lag 3)			<0.01
			[0.01]
Observations	1,062	1,062	1,055
#CC	107	107	107
Year FE	X	X	X

Note. -- All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49, as well as year and community college fixed effects. Standard errors, clustered at the community college level, are in brackets. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX C

Pair-level Tables

Table 1.10 Pair-level estimates of the TAG effect on transfers to and bachelor's degrees from UC campuses

	Ln(Transfers)	Ln(BA)	Ln(Transfers)	Ln(BA)	Ln(Transfers)	Ln(BA)	Ln(Transfers)	Ln(BA)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TAG	0.04		0.02		0.12***		0.12***	
	[0.03]		[0.03]		[0.04]		[0.04]	
TAG (lag 3)		0.06*		0.03		0.14***		0.14***
		[0.03]		[0.03]		[0.04]		[0.04]
# other TAG	0.05***		0.05***		0.03		0.04**	
	[0.02]		[0.01]		[0.02]		[0.01]	
# other TAG		0.08***		0.08***		0.05***		0.06***
(lag 3)		[0.02]		[0.01]		[0.02]		[0.01]
Observations	4,723	4,723	6,502	6,502	4,723	4,723	6,502	6,502
R-squared	0.06	0.05	0.07	0.05	0.12	0.09	0.12	0.09
#Pair	607	607	811	811	607	607	811	811
Pair FE	X	X	X	X	X	X	X	X
UC campus*year FE					X	X	X	X
UC campuses	TAG campuses	TAG campuses	All	All	TAG campuses	TAG campuses	All	All

Note. -- All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49, as well as year and pair fixed effects. Standard errors, clustered at the pair level, are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 1.11 Negative binomial pair-level estimates

	Ln(Transfers)	Ln(BA)	Ln(Transfers)	Ln(BA)
	(1)	(2)	(3)	(4)
TAG	0.10***		0.10***	
	[0.02]		[0.02]	
TAG (lag 3)		0.10***		0.09***
		[0.02]		[0.02]
# other TAG	0.05***		0.05***	
	[0.01]		[0.01]	
# other TAG		0.07***		0.06***
(lag 3)		[0.01]		[0.01]
Observations	6,183	6,183	8,273	8,273
#Pair	619	619	828	828
Pair FE	X	X	X	X
	TAG campuses	TAG campuses	All	All

Note. -- All models include controls for county employment rate, median household income, percent male, percent White, total population, population growth, and population aged 0 to 14, 15 to 29, and 30 to 49, as well as year and pair fixed effects. Standard errors, clustered at the pair level, are in brackets. *** p<0.01, ** p<0.05, * p<0.1

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CHAPTER 2
EFFECTS OF STATE-LEVEL TRANSFER POLICIES ON POST-SECONDARY OUTCOMES

Introduction

Community colleges are the access point to postsecondary education for millions of students due to low tuition and open enrollment policies (American Association of Community Colleges http://www.aacc.nche.edu/AboutCC/Trends/Pages/default.aspx). These institutions serve many purposes, providing workplace training and remedial education, as well as preparing students for transfer to other postsecondary institutions. While the transfer mission has remained important, in the late 1980s falling transfer rates from community colleges caused concern for policymakers (Barry & Barry, 1992). Therefore, over the past several decades, states have instituted a series of policies aimed at making higher education seamless; that is, students can begin at any public postsecondary institution and transfer to other institutions without major loss of credit. The effectiveness of statewide articulation policies has implications for students as it could change the cost of a bachelor's degree, as well as labor market outcomes.

This paper extends the literature on the effects of statewide transfer and articulation policies. To do this, I use data from three cohorts of the Beginning Postsecondary Students (BPS) longitudinal study. I supplement the BPS data with state articulation policy information from two different sources. The BPS tracks students who first begin postsecondary education in 1989-90, 1995-96, and 2003-04, allowing me to look at state policy changes over time. Previous studies look at the cross-sectional relationship between articulation agreements and postsecondary outcomes, generally finding little impact on transfer and degree attainment outcomes.

Baseline estimates suggest that state articulation policies are not related to transfer or degree attainment outcomes for students who begin at public two-year or public four-year institutions. These results are sensitive to the BPS cohorts used in the analysis, as well as to the policy definition. The latter two BPS cohorts show a negative impact on certificate completion, and a positive, marginally significant impact on bachelor's degree attainment. Using a transfer policy definition focused on the transfer of academic credit does show a positive, statistically significant impact on transfer.

Section 2 gives background on state-level transfer and articulation policies, and reviews the literature. The data used in the analysis are presented in Section 3. Section 4 discusses the methodology used in this paper, and Section 5 provides results. Section 6 provides a discussion, and Section 7 concludes.

Background

State-Level Transfer and Articulation Policies

States have a variety of policies pertaining to articulation and transfer between postsecondary institutions. A report from the Education Commission of the States (ECS) in 2010 describes the types of transfer policies, and indicates whether the state had that policy by 2010. Policies include cooperative agreements, transfer data reporting, incentives and rewards for transfer students, statewide articulation guides, common general education core requirements, and common course numbering. For example, Alabama has a common core, but no common course numbering system. Additionally, states may have legislated policies or policies adopted by higher education governing boards that apply statewide. Associate's degree transfer policies – where associate's degree completion ensures transfer or credit acceptance at public four-year institutions in the same state – are generally included as part of legislation. Ignash and Townsend (2000) also discuss types of state transfer policies.

As the ECS report highlights, there is considerable variation in the presence of state policies. This variation extends to the timing of these policies, with some states having long-standing transfer agreements, while other states have only recently added state articulation policies. I will be exploiting this policy variation to identify the effect of state articulation policies on postsecondary outcomes of students.

To define state transfer/articulation policies, I use data from several sources. The first source is a report on state transfer and articulation policies from ECS (2010). I define policies based on the information from the 'Statewide Policy' column in the report. These policies include legislation as well as policies put in place by higher education systems. I use the ECS policy data for my baseline analysis because I can define state policies from the ECS for all three BPS cohorts. The second policy data source is from a Government Accountability Office report in 2005. State policies are defined from the year of the state legislation listed in Appendix II of the report. These policies are related to the transfer of academic credit. I use state policies from the GAO report as a sensitivity check on the definition of the state policy. I can only define state policies from the GAO accurately for the latter two BPS cohorts. Previous studies use policy data from Ignash and Townsend (2000), who define transfer policies based on a survey of the states. However, since the policy data only go through 1999, these data do not cover the needed time span for this study.

Table 2.8 in the Policy Appendix contains information on the policy year from each data source. In some states, the data sources agree on the policy year. However, in other states there is disagreement on the policy year, and sometimes disagreement on the whether a statewide articulation policy exists. There are 12 states where the ECS and GAO data disagree on the timing of the policy; in half of those states the ECS policy comes before the GAO policy. Additionally, there are six states where the ECS and GAO policy data disagree about the presence of a state articulation policy; in one of these states the disagreement occurs because the ECS policy is put in place after the GAO report in 2005. Some of these differences in the timing and presence of state policies can be explained by the different policy definitions used by the ECS and GAO reports. The GAO study looks strictly at policies related to the transfer of academic credit, while the ECS policy definition is broader and includes legislation and higher education board policies governing transfer of credit as well as transfer of associate's degrees. These alternate definitions of state transfer policies seem to impact different postsecondary outcomes, as discussed in the Results section below.

Literature Review

A small literature examines whether there is an association between state transfer policies and postsecondary outcomes for students. The main outcomes of interest are the probability of transfer and, conditional on transfer, the probability of receiving a bachelor's degree as well as time-to-degree. The conclusion of the literature so far is that the presence of a state policy does not increase the transfer rate between two-year and four-year institutions for students who begin postsecondary at a two-year institution. Past research has used student data from the National Education Longitudinal Study (NELS) 88/2000, and the Beginning Postsecondary Students (BPS) 89/94 longitudinal study to investigate this research question.

The paper closest to this one is Anderson, Sun, and Alfonso (2006) because they use the first cohort of the BPS. Their study looks at transfer rates between two-year and four-year institutions for students initially enrolled at public two-year colleges. The state articulation policy is defined as the presence of a legislated transfer policy in the state by 1991. They find no effect of presence of a transfer policy on transfer in a state.

Other studies use NELS:88/2000 to investigate the relationship between articulation policies and transfer (Goldhaber and Gross (2009), Roksa and Keith (2008) and Reynolds (2007)). Similar to Anderson, Sun, and Alfonso (2006), Roksa and Keith (2008) use a simple indicator for whether a state has a transfer policy to investigate the outcomes of transfer, bachelor's degree attainment, and time-to-degree. They find no relationship between state transfer policy and these outcomes. Goldhaber and Gross (2009), in addition to using an indicator for the presences of a state policy, also attempt to classify 'strong' and 'weak' articulation policies. However the authors find only small effects on transfer. The authors also use indicators for various types of state policies (automatic transfer of associate's degree, common course numbering across institutions). Again, there is little relationship between type of transfer policy and postsecondary outcomes. Gross and Goldhaber also conclude that state articulation policies are associated with higher odds of transfer for Hispanic students but not for other minority groups or first generation college attendees. Reynolds (2007) looks at the effect of state policies on students by using propensity score matching. He matches students who have similar predicted probabilities of living in a state with a transfer policy, running his analysis separately for men and women. Reynolds finds that articulation agreements raise educational attainment for male college attendees, but not overall attainment for the cohort of high school graduates. He also studies an associate's degree policy in North Carolina using data from the Integrated Postsecondary Education Data System (IPEDS). He finds increases in associate's degree completion, but no impact on bachelor's degree attainment.

None of the studies listed above is able to take advantage of policy changes over time, which may be one reason why they find little relationship between state transfer policies and post-secondary outcomes. The aim of this paper is to extend the analysis on state-level transfer policies by looking at changes over time, so I use three cohorts of the BPS to examine the impact of transfer policies on students who begin at public two-year institutions.

Data

The primary data source for this paper is the Beginning Postsecondary Students (BPS) longitudinal study. The BPS follows cohorts of students who have enrolled in postsecondary education for the first time. The sample does not include students concurrently enrolled in high school. I will use all three waves: BPS: 1990-94, 1996-01, and 2004-09.

Each initial cohort comes from the National Postsecondary Student Aid Study (NPSAS), a large nationally representative sample of postsecondary students and institutions. The first wave follows students for five years (through 1994), while the last two waves follow students for six years (through 2001 and 2009, respectively). Students in each BPS cohort are interviewed three times: at the end of their first year in postsecondary education, two years later, and then again two or three years later depending on the BPS cohort. So, students in the BPS: 90-94 cohort are first interviewed in the spring of 1990 at the end of the 1989-90 academic year.

The BPS tracks students postsecondary outcomes, including degree attainment, transfer between institutions, and stopout. I use this information to create indicators for whether students ever transferred or attained a degree during the study. Specifically, I create an indicator for whether a student ever transferred, and ever transferred to a four-year institution. Similarly, I create indicators for whether students ever receive an associate's degree, or ever receive a bachelor's degree. These measures of transfer and degree attainment are the main outcomes of interest. The baseline estimates use all three cohorts of the BPS, so outcomes are defined five years after first entering post-secondary. For analysis that only includes the latter two cohorts of the BPS, outcomes are defined six years after entering post-secondary.

The main independent variables are policy indicators for each state. These policy variables indicate whether a state had any articulation policy in a particular year. The three data sources for the policy variables are listed in the previous section. I code a state as having a transfer policy if it is in place by December 1989 for BPS:90/94, December 1995 for BPS:96/01, and December 2003 for BPS:04/09.

The BPS datasets also contain information on student demographic and background characteristics such as sex, race/ethnicity, age when first enrolled, parental education, dependent status, marital status, presence of children, and whether the student received a high school diploma. Additionally, I include the student's degree program, attendance intensity, degree goal when first enrolled, and indicators for having a job while enrolled, taking any remedial courses, whether they receive financial aid, and whether the student is in the top or bottom income quartile (excluded category is the middle 50% of income). These variables are commonly used in other papers in the literature. Table 2.9 describes the variables from the BPS used in this study.

Other studies also include state-level variables, in addition to the state transfer policy indicator, to account for different state environments. I use variables from Reynolds (2007) dataset, which he generously shared with me. State-level covariates include, unemployment rate, appropriations, financial aid, per capita personal income, population distribution (percent 5-17, 18-24, and 65 and over), tuition at universities, other four-year institutions, and two-year institutions, and the number of public and private two-year and four-year institutions (per 18-24 population). All dollar values are in 2004 dollars.

Analysis Sample

Table 2.10 in Appendix A shows the sample size for each BPS cohort as I impose sample restrictions. The BPS:90-94 is the smallest cohort with a starting sample size of 7,253 students. The BPS:96-01 and BPS:04-09 cohorts contain 12,085 and 16,684 students respectively. After conditioning on beginning postsecondary studies at a public two-year campus, the sample sizes fall to 704, 1516, and 5549 in each of the three BPS cohorts. The analogous sample sizes for beginning at a public 4-year institution are 1,613, 5,161, and 4,643 in each of the BPS cohorts. The next cut to the data keeps only students who responded to both the first and last BPS interview, which is needed to observe postsecondary attainment outcomes. Additionally, I drop students with missing information on background characteristics or postsecondary outcomes. Finally, the analysis sample only contains students who attend their first institution in a state that is present in two of the three BPS cohorts. The final sample sizes for beginning public two-year students is 622 in BPS:90/94, 742 in BPS:96/01, and 5378 in BPS:04/09, which forms the baseline analysis sample.

The BPS provides analysis weights to account for the complicated survey design. I use analysis weights in all descriptive tables and regression analysis in this paper. Additionally, I cluster standard error by state in all regression analysis.

Methodology

My research question is what is the effect of state articulation policies on education outcomes of students who begin their postsecondary studies at public two-year institutions? I use variation over time in state policies on articulation to identify this relationship.

I use a differences-in-differences strategy as shown in the equation below:

$$(1) \qquad Y_{ist+5} = \alpha + \eta X_{ist} + \beta_4 Policy_{st} + \lambda_t + \theta_s + \varepsilon_{ist},$$

where i indexes individual students, s indexes state, and t indexes BPS cohort. X_{ist} is a vector of individual-level covariates. Y_{ist+5} denotes the transfer or degree attainment outcome five years after the student begins post-secondary education. I include only those students who begin their postsecondary education at two-year institutions. $Policy_{st}$ refers to one of the state transfer and articulation policies described above. All policy and background demographic characteristics are defined as of the student's first year in post-secondary education. Background characteristics include the student's sex, race, completion of a high school diploma, dependent status, and whether the student received any financial aid. Additionally, I include the beginning degree program in which the student enrolled and an indicator variable for whether the student had a goal of obtaining a bachelor's degree. These variables are common in other studies in this literature.

Results

Descriptive statistics

Table 2.1 shows descriptive statistics by BPS cohort for students who began postsecondary at either a public two-year or four-year institution[1]. The table shows demographic changes in beginning postsecondary students with higher percentages of female and minority students in the latter cohort. Student's goals are also changing over time; around 68% of beginning public two-year students want to get a bachelor's degree or higher in the first two BPS

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[1] Descriptive statistics for students with no missing information in all states is in Appendix Table 2.11.

Table 2.1 Descriptive statistics of the analysis sample of first-time beginning Public two-year and four-year students

	1989-1990		1995-96		2003-04	
	2-yr	4-yr	2-yr	4-yr	2-yr	4-yr
Student Background						
Female	50.6%	53.5%	54.1%	54.6%	56.2%	55.1%
White, non-Hispanic	75.9%	82.6%	71.8%	72.5%	60.3%	71.6%
Black, non-Hispanic	8.0%	8.4%	11.9%	11.5%	14.2%	8.9%
Hispanic	11.8%	4.1%	11.3%	9.1%	15.9%	8.5%
Other race	4.3%	4.9%	5.0%	7.0%	9.6%	11.0%
HS diploma	92.4%	98.5%	87.7%	98.4%	85.6%	96.1%
Certificate program	11.6%		10.0%		4.4%	
Assoc's degree program	68.8%		72.9%		79.6%	
Bachelor's program		76.8%		92.2%		91.7%
Age 22 or less	76.8%	95.9%	68.6%	94.6%	69.9%	95.1%
Dependent	50.0%	82.9%	59.7%	91.7%	62.9%	92.7%
Received any aid	27.5%	48.3%	50.4%	74.9%	53.9%	76.7%
Goal: BA or higher	68.1%	94.8%	68.1%	82.4%	81.3%	98.5%
Parent ed. BA or higher	26.3%	42.3%	25.6%	43.7%	27.9%	54.6%
Attend full-time	37.2%	76.2%	47.0%	87.6%	48.8%	90.3%
Married	19.6%	3.6%	22.3%	3.4%	17.5%	2.9%
Have a child	18.1%	3.3%	24.7%	4.5%	22.2%	3.5%
Work while enrolled	82.7%	78.8%	79.7%	61.3%	77.7%	59.0%
Take remedial courses	18.9%	16.2%	24.0%	17.0%	29.4%	19.4%
Income (bottom quartile)	26.0%	20.3%	28.7%	25.2%	25.1%	18.5%
Income (top quartile)	17.3%	27.7%	17.1%	24.0%	25.5%	30.9%
Postsecondary Outcomes						
Ever transfer	43.0%	28.1%	39.6%	25.5%	36.3%	24.2%
Attain certificate	13.0%	3.8%	10.4%	3.1%	8.0%	1.9%
Attain associate's	22.2%	6.1%	19.5%	4.6%	15.0%	3.9%
Attain bachelor's	6.5%	46.6%	7.4%	46.6%	5.8%	48.9%
State characteristics						
Transfer policy (ECS)	22.0%	14.9%	54.2%	32.5%	74.6%	68.5%
Transfer policy (GAO)	.	.	64.5%	44.3%	87.4%	82.5%
Unemployment rate	5.3%	5.4%	5.6%	5.3%	6.1%	5.8%
Sample size (unweighted)	622	1509	742	3230	5378	4281

Source: Author calculations from BPS:90/94, BPS:96/01 and BPS:04/09.

Note. -- Student background characteristics are defined for the first year in postsecondary while all degree and transfer variables are defined as of five years after beginning postsecondary. BPS analysis weights are used. States in at least two of three BPS cohorts.

cohorts, while that rises to over 81% in the latter BPS cohort. Beginning public four-year students show a different pattern, with almost 95% of students aiming for at least a bachelor's degree in 1989-90, dropping to 82.4% in 1995-96, and rising again to 98.5% in 2003-04. Despite these changes in student's aspirations, five-year transfer and degree attainment outcomes change little over time. Only around six percent of beginning public two-year students completes a bachelor's degree within five years. While five-year bachelor's degree completion rates are higher for beginning public four-year students, they are still below 50%. Finally, states are adding articulation policies over time, so more students attend school in states with transfer policies in the later BPS cohorts. Only 22.0% of beginning public two-year students in 1989-90 attends an institution in a state with an articulation policy (as defined by ECS), while that number rises to 74.6% in 2003-04. More students attend institutions in states with policies as defined by the GAO.

Table 2.2 expands on the descriptive statistics in Table 2.1 by dividing beginning public two-year students by their beginning degree program: no degree program, certificate, and associate's degree. Students who begin at public 2-year institutions are transferring to other institutions. Between 37.1-44.5% of students who begin in an associate's degree program transfer to another institution within five years. Students who begin in a certificate program have the lowest rates of transfer. In addition, the percentage of students who begin at public 2-year institutions and want to get a bachelor's degree is quite high (with the exception of students enrolled in certificate programs), but actual rates of (five-year) bachelor's degree attainment are much lower. While this may be optimal (students realize they do not need a bachelor's degree for their chosen career path, or are not prepared academically to complete a bachelor's degree),

Table 2.2 Student background characteristics and five-year postsecondary outcomes by initial degree program for first-time beginning public two-year students (means in %)

	1989-1990			1995-96			2003-04		
	None	Cert.	Assoc.	None	Cert.	Assoc.	None	Cert.	Assoc.
Student Background									
Female	45.9	48.5	52.3	61.3	52.7	52.6	52.6	55.2	57.0
White, non-Hispanic	69.3	75.2	77.9	70.5	66.7	72.8	61.1	62.1	60.0
Black, non-Hispanic	7.6	8.8	7.9	11.6	20.4	10.8	7.5	20.6	15.2
Hispanic	13.0	10.9	11.6	9.4	12.0	11.6	21.1	11.1	15.1
Other race	10.1	5.2	2.6	8.6	0.9	4.8	10.3	6.2	9.6
HS diploma	88.6	92.8	93.5	85.1	74.9	90.0	80.7	78.8	87.0
Age 22 or less	67.1	69.0	80.9	54.2	48.6	74.7	61.0	44.9	73.1
Dependent	41.9	36.7	54.6	46.8	27.3	67.2	56.5	38.8	65.6
Received any aid	17.3	27.6	30.4	41.6	62.1	50.9	40.8	67.3	55.7
Goal: BA or higher	59.8	60.2	71.8	62.2	40.3	73.4	74.3	45.1	84.7
Parent ed. BA or higher	26.6	17.3	27.8	25.7	9.6	27.8	28.0	12.9	28.7
Attend full-time	20.0	33.8	42.7	28.6	35.3	52.9	40.0	46.0	50.7
Married	32.0	19.2	16.1	31.2	34.6	18.6	21.8	35.2	15.6
Have a child	23.2	19.6	16.4	31.8	53.2	19.1	25.7	38.8	20.6
Work while enrolled	85.8	83.2	81.8	86.7	70.5	79.3	78.6	72.2	77.8
Take remedial courses	21.7	14.6	18.8	11.6	10.4	28.8	22.8	28.0	30.8
Income (bottom quartile)	27.1	26.3	25.6	26.9	32.0	28.7	18.8	23.3	26.5
Income (top quartile)	17.5	6.1	19.1	25.1	12.3	15.9	32.4	30.8	23.8
Postsecondary Outcomes									
Ever transfer	37.1	44.2	44.5	33.0	18.9	44.0	35.7	24.5	37.1
Attain certificate	15.4	19.4	11.2	13.0	26.6	7.6	8.4	49.2	5.7
Attain associate's	15.3	14.4	25.6	11.6	9.9	22.7	6.2	5.1	17.3
Attain bachelor's	5.8	2.8	7.4	2.7	0.9	9.4	4.8	1.2	6.3
State characteristics									
Transfer policy ECS	18.4	18.6	23.5	53.3	57.6	54.0	84.8	60.3	73.3
Transfer policy (GAO)	.	.	.	59.5	73.2	64.4	90.0	67.8	88.0
Unemployment rate	5.3	5.5	5.2	5.6	5.3	5.6	6.2	5.7	6.1
Sample size (unweighted)	110	83	429	114	60	568	667	296	4415

Source: Author calculations from BPS:90/94, BPS:96/01 and BPS:04/09.

Note: Student background characteristics are defined for the first year in postsecondary while all degree and transfer variables are defined as of five years after beginning postsecondary.

BPS analysis weights are used. States in at least two of three BPS cohorts (37 states).

state transfer policies are often put in place to address the gap between degree goal and degree attainment. Is there a relationship between the state transfer policy and degree attainment?

Pooled Cohort Analysis

Table 2.3 presents a pooled cohort analysis using all three BPS cohorts[2,3]. Each column uses a different dependent variable: transfer to another institution, attainment of a certificate, associate's degree, and bachelor's degree. Results are from a linear probability model. State articulation policies are positively related certificate and bachelor's degree attainment, and negatively related to transfer and associate's degree completion. However, none of the coefficients are statistically different from zero. Conversely, several other variables of interest are significantly related to postsecondary outcomes. Specifically, a student's goal to attain at least a bachelor's degree is positively related to both transfer and bachelor's degree completion with coefficients significant at the 1% level. A beginning public two-year student who wants to attain a bachelor's degree is 19.8% more likely to transfer than students without those aspirations. A bachelor's degree goal lowers a student's probability of receiving a certificate by just over four percent. Full-time attendance in the first year of postsecondary increases a student's probability of transferring, as well as of attaining an associate's or bachelor's degree. Younger students are more likely to transfer than older students, but no more likely to attain a degree.

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2 Coefficients are only shown for selected variables. Complete regression analyses are available from the author upon request.

3 Cross-sectional analysis for each BPS cohort is presented in Appendix Table 2.12.

Table 2.3 Pooled cohort analysis of the relationship between state articulation policy and postsecondary outcomes

	(1) Transfer	(2) Certificate	(3) AA	(4) BA
Transfer policy (ECS)	-0.010	0.004	-0.000	0.015
	(0.025)	(0.013)	(0.016)	(0.012)
Attend full-time	0.105***	-0.001	0.098***	0.060***
	(0.017)	(0.013)	(0.015)	(0.010)
Goal: BA or higher	0.198***	-0.041**	0.035*	0.052***
	(0.018)	(0.016)	(0.019)	(0.007)
Age 22 or less	0.089**	-0.023	0.029	0.011
	(0.040)	(0.019)	(0.021)	(0.010)
R-squared	0.145	0.056	0.080	0.070
N	6742	6742	6742	6742

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions, and year fixed effects. * p<0.10, ** p<0.05, *** p<0.01

Differences-in-differences

Multiple BPS cohorts allow for the investigation of whether changes in state transfer policies impact student postsecondary outcomes. Table 2.4 presents results from Equation (1)[4,5]. Each column represents a different postsecondary outcome. There is a negative but

[4] Appendix Table 2.13 contains results from a differences-in-differences specification not using the BPS sample weights.

[5] Additional analyses, available upon request, run separate regressions for males and females, as well as different racial subgroups. Some results differ from the baseline analysis: there is a large positive, though marginally significant, relationship between state transfer policy and transfer for

statistically insignificant coefficient on the policy variable in columns (1) and (2) relating to student transfer and attainment of a certificate within five years. State transfer policies are positively related to associate's and bachelor's degree attainment, but the coefficients are not statistically different from zero. It is possible that five years is not a long enough time to detect a bachelor's degree impact, given the lengthening amount of time students take to complete bachelor's degrees, even when they begin at four-year institutions.

Table 2.4 Baseline estimates of the state policy effect on postsecondary outcomes

	(1) Transfer	(2) Certificate	(3) AA	(4) BA
Transfer policy (ECS)	-0.014	-0.008	0.034	0.025
	(0.047)	(0.023)	(0.032)	(0.016)
Goal: BA or higher	0.199***	-0.039**	0.039**	0.051***
	(0.018)	(0.017)	(0.019)	(0.007)
R-squared	0.160	0.066	0.092	0.075
N	6742	6742	6742	6742

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions. In addition, models include state and year fixed effects. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

The patterns in this analysis are similar to, though not as strong as, what Reynolds (2007) found when looking at the associate's degree transfer policy in North Carolina. His analysis showed an increase in associate's degrees, but no detectable change in bachelor's degree receipt following the policy. One common policy implemented by states is to designate the associate's degree as a transfer degree. It is possible that these policies are impacting associate's degree

Black students. Additionally, there is a large positive and statistically significant impact of state transfer policy on associate's degree completion for Hispanic students.

completion. However, it is difficult to pin down when associate's degree policies are added using the policy data sources I have. As a result, it is not possible to distinguish the impact of specific transfer policy elements (specifically relating to associate's degrees) versus the presence of any policy in the state. If I were able to accurately define associate's degree transfer policies, I might be able to find similar results to Reynolds (2007).

As in the pooled cohort analysis above, the student's goal of receiving at least a bachelor's degree is positively associated with both transfer to other institutions and eventual associate's or bachelor's degree receipt for beginning public, two-year students. However, this goal is negatively related to attaining a certificate. Having a high school diploma is also positively and statistically significantly related to receiving an associate's degree within five years.

Additional Analysis

While the baseline results do not show any relationship between state transfer policies and post-secondary outcomes, I investigate alternate specifications to look for evidence of policy impacts. Specifically, I analyze the sensitivity of the baseline results to the timing of the policies, different policy definitions, and a different sample of students.

First, I look at the impact of defining whether a state has a policy at a different time. In the baseline analysis in Table 2.4, a state is considered to have a policy if it is in place by December of the first academic year for the BPS cohort. To check the timing, I now consider a state to have a policy if it is in place by December of the second academic year; specifically, by December of 1990, 1996, and 2004 for each BPS cohort respectively (compared to December of 1989, 1995, and 2003 for Table 2.4). The effect of this different policy classification is to define some states as having a policy in place in an earlier cohort than previously. This specification does not test for endogeneity of the policy in the traditional sense, but it is a sensitivity check on the timing of the policy.

Table 2.5 shows the results in the differences-in-differences framework using the lagged policy definition. The coefficients on the lagged policy variable are all small and statistically insignificant. The coefficients on the lagged policy variable have the same sign and significance as the coefficients on the policy variable in Table 2.4. Therefore, the impact of state policies on associate's degree completion is not sensitive to when the policy is defined.

Table 2.5 Additional analysis using a lagged policy variable

	(1) Transfer	(2) Certificate	(3) AA	(4) BA
Transfer policy (ECS lagged)	-0.023	-0.027	0.007	0.023
	(0.049)	(0.022)	(0.033)	(0.016)
Goal: BA or higher	0.199***	-0.039**	0.039*	0.051***
	(0.018)	(0.017)	(0.019)	(0.007)
R-squared	0.160	0.066	0.091	0.075
N	6742	6742	6742	6742

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions. In addition, models include state and year fixed effects. * p<0.10, ** p<0.05, *** p<0.01

Second, I check the sensitivity of the analysis using the ECS policy definition, to using policies defined from the GAO report. Unfortunately, I cannot define GAO policies reliably for the BPS:90/94 cohort, so this analysis only uses the last two BPS cohorts. As a result of using a slightly different sample, I use both the ECS and GAO policy definitions in the differences-in-

Table 2.6 Sensitivity analysis using two policy definitions with the latter two BPS cohorts

	(1) Trans.	(2) Cert.	(3) AA	(4) BA	(5) Trans.	(6) Cert.	(7) AA	(8) BA
Transfer policy (ECS)	-0.005	-0.095**	-0.009	0.044*				
	(0.075)	(0.044)	(0.058)	(0.022)				
Transfer policy (GAO)					0.206**	-0.031	0.009	0.059*
					(0.085)	(0.061)	(0.068)	(0.031)
Goal: BA or higher	0.164***	-0.025	-0.002	0.040***	0.162***	-0.026	-0.002	0.040***
	(0.018)	(0.016)	(0.023)	(0.007)	(0.017)	(0.016)	(0.024)	(0.008)
R-squared	0.171	0.091	0.096	0.081	0.173	0.089	0.096	0.081
N	6028	6028	6028	6028	6028	6028	6028	6028

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions. In addition, models include state and year fixed effects. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

differences analysis in Table 2.6[6]. Columns (1) thru (4) use the ECS definition, while columns (5) thru (8) use GAO. Using the new sample, there is now a positive, and marginally significant (at the 10% level), relationship between the ECS state policy variable and bachelor's degree attainment. Additionally, there is a negative and statistically significant relationship between attaining a certificate

[6] Appendix Table 2.14 contains descriptive statistics for the sample used in Table 2.6, and Table 2.15 provides an analysis using six-year transfer and degree attainment outcomes.

and the ECS policy variable. The negative but statistically insignificant relationship between ECS policy and transfer remains. The impact on associate's degree completion is now negative, but not statistically different from zero.

The coefficients on certificate and bachelor's degree attainment in Table 2.6 are larger than those in Table 2.4, while the coefficients on transfer and associate's degree completion are smaller. There may be two reasons for the difference in coefficient size. First, Table 2.6 drops the first BPS cohort. Second, there are fewer states present in the analysis sample in Table 2.6. In each analysis, I kept states present in at least two BPS cohorts, which kept 37 states in the sample in Table 2.4, but only 34 states in the sample in Table 2.6. Using a restricted sample of states gives results similar to the baseline analysis in Table 2.4[7]. Therefore, it appears that dropping the first BPS cohort drives the change in estimated coefficients in Table 2.6 as compared to the baseline results. Next, I investigate the impact of changing the policy definition on the results.

Column (5) shows a large, positive and statistically significant relationship between the state policy variable as defined from the GAO reports and transfer. Adding a state transfer policy as defined by the GAO increases a student's probability of transfer by 20.6%. Additionally, the coefficient on the GAO policy is positive and marginally significant in the bachelor's degree regression in column (8). This matches the result using the ECS policy in column (4) of Table 2.6. Notably, there is no statistically significant relationship between the GAO state articulation policy and the probability a student completes an associate's degree within five years. However, the GAO policies are related to the transfer of academic credit. Easing transfer of credit between institutions does not necessarily encourage students to

[7] See Appendix Table 2.16 for a specification of the baseline results using only the states present in the sample in Table 2.6.

complete an associate's degree. The strong relationship between transfer and GAO policy is likely explained by the GAO policy focus on transfer of academic credit. The ECS definition of statewide policies is broader than that of GAO and seems to include policies that are related to students completing associate's degrees.

Finally, I investigate the relationship between state articulation policies and postsecondary outcomes for students who begin at public four-year institutions. Much of the previous literature focuses solely on students who begin at public two-year institutions. However, many state transfer policies, while aimed at easing transfer for public two-year students, cover all public postsecondary students in the state. For example, general education policies which require that general education credits transfer between public institutions in the state can impact transfer and degree outcomes for both two-year and four-year beginners. In Table 2.7, I use all three BPS cohorts in a differences-in-differences analysis with beginning public four-year students (instead of public two-year students as in Table 2.4). Adding a state transfer policy is related to a 3.5% increase the probability of transfer. However, the coefficient is not statistically different from zero. There is a positive relationship between state transfer policies and completion of a certificate or associate's degree, and a negative relationship for bachelor's degree completion. However, none of these coefficients are statistically different from zero. These results match those in Reynolds (2007), who found no statistically significant relationship between state transfer policy and degree attainment for four-year students.

Table 2.7 Analysis using students beginning at public four-year institutions

	(1) Transfer	(2) Certificate	(3) AA	(4) BA
Transfer policy (ECS)	0.035	0.012	0.018	-0.020
	(0.027)	(0.009)	(0.013)	(0.027)
Goal: BA or higher	0.027	-0.018	-0.041***	0.087***
	(0.026)	(0.012)	(0.013)	(0.023)
R-squared	0.022	0.013	0.052	0.152
N	9020	9020	9020	9020

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions. In addition, models include state and year fixed effects. * p<0.10, ** p<0.05, *** p<0.01

Discussion

This paper, as well as much of the previous literature, finds little relationship between state transfer policies and postsecondary outcomes. However, several papers, focusing on one policy in one state, do find stronger impacts on specific outcomes. As mentioned previously, Reynolds (2007) found a positive relationship between an associate's degree policy in North Carolina and associate's degree completion. Quin (2013) showed that adding a transfer guarantee policy at community colleges in California increased transfer and bachelor's completion. However, Quin (2013) found no impact on associate's degree completion. There was no associate's degree component to the admission guarantee. Taken together, these findings suggest that the postsecondary outcomes impacted depend on the type of transfer policy put in place. Therefore, combining different policies that may affect different outcomes may not produce strong results.

Using a single indicator for whether a state has any policy may be one reason why the literature has found little relationship between state transfer policies and postsecondary outcomes. State transfer policies are quite varied, as displayed in ECS (2010). A state may have none of these policies, or may combine components of several different types of policies. The patterns of types of state policies in place are difficult to classify. Therefore, using an indicator for whether a state has a policy without distinguishing the type of policy may mask the true impact of different policy components. The one exception to using a single policy indicator is Gross and Goldhaber (2009), who investigate the impact of specific transfer policy components. They include indicators for each type of transfer policy, such as automatic transfer of an associate's degree, but find no impact on transfer. However, they do not look at associate's degree attainment as an outcome.

Moving forward, researchers should focus on thinking about what outcomes they expect particular policies to impact. Researchers should also consider whether these policies have impacts prior to transfer; for example, do state transfer policies impact who goes to college, or what college they initially choose. Reynolds (2007) found that state policies did not affect college attendance, but were related to an increase in the probability of women attending two-year colleges. I conducted a similar exercise using first-time enrollment data from the Integrated Postsecondary Education Data System (IPEDS), but found no relationship between state transfer policy and initial college type.[8] Thinking about expected outcomes may help researchers begin to classify policies into clear groups. Additionally, it may help identify particular policies that may be working counter to each other.

[8] Results are available upon request to the author.

Conclusion

This paper expands on previous research investigating the relationship between state articulation policies and postsecondary outcomes by looking at policy changes over time. Estimates suggest that state articulation policies are not related to transfer or degree completion for either beginning public two-year or public four-year students. However, these results are sensitive to the definition of the policy used, as well as the number of BPS cohorts in the analysis. Aspirations to receive a bachelor's degree or higher is positively associated with bachelor's degree completion in all specifications.

This paper uses data from three cohorts of the BPS, which has several strengths and weaknesses. One advantage of the BPS data is that it tracks multiple cohorts of entering postsecondary students, allowing for the investigation of trends over time. In addition, the BPS includes older students; this is more representative of the postsecondary population, particularly for two-year institutions. However, the tradeoff is that there is no high school to college link. As a result it is not possible, using the BPS, to investigate the effect of state transfer policies on whether students attend college, or possible shifting between four-year and two-year institutions. In addition, the BPS only tracks students for five to six years. This is a relatively short period to look at bachelor's degree outcomes, especially for transfer students, as even students who begin at four-year institutions may now take six years to graduate.

APPENDICES

APPDENDIX A

Policy Appendix

Table 2.8 State-level transfer and articulation policy year by source

State	GAO (2005)[2]	ECS (2010)[3]
Alabama	1994	1994
Alaska		
Arizona	1996	1996
Arkansas	1989	1989
California	1988	1991
Colorado	1985	1985
Connecticut	1996	1996
Delaware		
Florida	1975	1975
Georgia		
Hawaii		
Idaho		
Illinois	1973	1973
Indiana	1987	1987
Iowa		
Kansas	1991	1991
Kentucky	1997	July, 1990
Louisiana	1999	1999
Maine	1985	
Maryland	1996	1996
Massachusetts	by 1991	1988
Michigan		1999
Minnesota	1985	2001
Mississippi		
Missouri	by 1994	
Montana		
Nebraska	1991	1994
Nevada	1997	2008
New Hampshire[1]		
New Jersey	1994	
New Mexico	1995	1995
New York	by 1987	
North Carolina	1997	1997
North Dakota		2007
Ohio	2003	2003

Table 2.8 (cont'd)

Oklahoma	1995	1995
Oregon	1987	1987
Pennsylvania	2000	2006
Rhode Island	1987	1979
South Carolina	1994	2009
South Dakota	1998	1998
Tennessee	2000	2000
Texas	1997	1997
Utah	2001	1997, repealed 2001
Vermont		
Virginia	2004	1992
Washington	1983	1983
West Virginia	2001	2000
Wisconsin	1973	
Wyoming	1997	1997
States with articulation policy	39	36

[1] States with no policy year do not have a state policy, with the exception of New Hampshire, for which reliable data about the state policy year was not available.
[2] Policy year is the year of the state legislation listed in Appendix II.
[3] Policy year is the year of the legislation/policy listed in the "Statewide Policy" column.

APPENDIX B

Data Appendix

Table 2.9 BPS variables

Label	My variable name	BPS:90/94 variable	BPS 96/01 variable	BPS 04/09 variable
Sex	female	H_GENDR	SBGENDER	GENDER
Age when first enrolled	age	AGE	AGE	AGE
Race/ethnicity	race	H_RACE	SBRACE	RACE
Any remedial education participation	remedial	REMEDIAL	RMANYY1	REMETOOK
Dependency status	dependent	EC_DEPE1	SBDEP1Y1	DEPEND
Receive any financial aid	anyaid	ANYAID89	AIDANY1	TOTAID
Attend full-time	fulltimeattend	ATTNSTAT	ATTNSTAT	ATTNSTAT
Degree goal	goalbaorhigher	ASPIRE	EPHDEGY1	HIGHLVEX
Parents highest education	paredbaorhigher	RPARED	PBEDHI3	PAREDUC
Degree program during first year	degreeprogram	PROGTYP	DGPGMY1, PGM2Y1	UGDEG
Have a high school diploma	hsdiploma	H_HSDIP	HSDEG	HSDEG
Marital status	married	MARITAL	SBMAFAY1	SMARITAL
Have a child	child	KIDS8990	SBMRCHY1	DEPCHILD
Work while enrolled	job	EMWKHR3	J1HOURY1	JOBENR2
Income	topquart, bottomquart	FAMINCPR	PCTALL2	PCTALL
First institution type	beginatpublic2yr	OFCO8990	ITNPSAS	FSECTOR
First institution state	firstinststate	FIPS	INSTATE	INSTSTAT
Ever transfer institutions	evertransfer	TRANTO	TRINTY2B	TFNUM6Y
Ever transfer to a four-year institution	transferto4yr	EVER4YR	TRINTY2B	IT4Y6Y
Ever obtain a certificate	getcert	RECDCT	DGDTCT2B	ATCTDT6Y
Ever obtain associate's degree	everaa	DEGASTAT	DGRETY2B	ATTYPE6Y
Ever obtain bachelor's degree	everba	DEGASTAT	DGRETY2b	ATTYPE6Y
BPS analysis weights	weight1and3	BPS94AWT	B01LWT2	WTA000

Table 2.10 Sample sizes (unweighted) for each BPS cohort

	BPS:90/94		BPS:96/01		BPS:04/09	
Beginning sample size	7253		12085		16684	
	Public 2-yr	Public 4-yr	Public 2-yr	Public 4-yr	Public 2-yr	Public 4-yr
Students beginning at institution sector	704	1613	1516	5161	5549	4643
Responded to first and last interview wave	689	1603	1061	3916	5546	4541
No missing information	629	1509	749	3230	5401	4301
First institution state present in 2 of 3 BPS cohorts	622	1509	742	3230	5378	4281

Source: Author calculations from BPS:90/94, BPS:96/01, and BPS:04/09
Note: There are 37 states with beginning public 2-year students in two of the three BPS cohorts, and 43 states with beginning public 4-year students in two of the three BPS cohorts.

APPENDIX C

Additional Tables

Table 2.11 Descriptive statistics for all first-time beginning public two-year and four-year students by BPS cohort (means in %)

	1989-1990		1995-96		2003-04	
	Public 2-year	Public 4-year	Public 2-year	Public 4-year	Public 2-year	Public 4-year
Student Background						
Female	51.2	53.2	51.2	53.9	56.3	55.2
White, non-Hispanic	76.6	82.1	72.8	73.5	60.4	71.1
Black, non-Hispanic	8.0	8.6	11.5	10.7	14.1	8.8
Hispanic	11.1	3.9	11.0	8.9	15.9	8.5
Other race	4.2	5.4	4.6	7.0	9.6	11.5
HS diploma	92.2	98.6	88.2	98.5	85.6	96.2
Certificate program	11.6		8.8		4.4	
Associate's degree program	67.4		72.9		79.7	
Bachelor's program		75.9		92.5		91.7
Age 22 or less	75.1	95.9	72.5	94.6	69.9	95.2
Dependent	47.9	83.0	65.5	91.8	62.8	92.7
Received any aid	27.6	48.2	42.5	68.7	53.9	76.4
Goal: BA or higher	67.0	94.6	67.2	81.4	81.1	98.5
Parent ed. BA or higher	25.8	42.5	26.2	46.4	27.9	54.4
Attend full-time	36.2	76.3	46.8	86.6	48.9	90.1
Married	20.4	3.8	21.7	3.6	17.5	2.9
Have a child	19.1	3.2	21.0	4.2	22.3	3.6
Work while enrolled	82.7	78.6	81.8	60.7	77.6	59.0
Take remedial courses	18.2	16.1	23.2	16.5	29.4	19.3
Income (bottom quartile)	26.2	20.4	28.5	23.4	25.1	18.6
Income (top quartile)	17.3	27.6	16.9	27.1	25.4	30.8
Postsecondary Outcomes						
Ever transfer	42.7	28.1	39.5	25.3	36.3	24.1
Transfer to 4-year	24.5	12.6	.	.	.	.
Attain certificate	13.1	3.9	11.0	3.0	8.0	1.9
Attain associate's	21.6	6.2	19.3	4.3	15.1	3.9
Attain bachelor's	6.1	46.7	6.9	47.6	5.8	48.7
State characteristics						
Transfer policy (ECS)	21.1	14.5	55.1	33.0	74.3	67.0
Transfer policy (GAO)	.	.	66.2	44.2	87.3	81.1
Unemployment rate	5.2	5.3	5.6	5.4	6.1	5.8

Table 2.11 (cont'd)

Sample size (unweighted)	663	1575	980	3852	5546	4541

Source: Author calculations from BPS:90/94, BPS:96/01 and BPS:04/09.

Note: Student background characteristics are defined for the first year in postsecondary while all degree and transfer variables are defined as of five years after beginning postsecondary. BPS analysis weights are used. States in at least two of three BPS cohorts.

Table 2.12 Cross-sectional analysis of the relationship between transfer policies and postsecondary outcomes

	(1) Transfer	(2) Certificate	(3) AA	(4) BA
	A. BPS:90/94 (N = 622)			
Transfer policy (ECS)	-0.059*	-0.024	0.147***	0.008
	(0.035)	(0.033)	(0.036)	(0.022)
Goal: BA or higher	0.252***	-0.069	0.132***	0.083***
	(0.045)	(0.047)	(0.035)	(0.016)
R-squared	0.160	0.013	0.104	0.055
	B. BPS:96/01 (N = 742)			
Transfer policy (ECS)	0.097**	0.006	-0.018	0.045***
	(0.047)	(0.025)	(0.027)	(0.016)
Goal: BA or higher	0.170***	0.021	-0.005	0.032*
	(0.035)	(0.034)	(0.028)	(0.017)
R-squared	0.208	0.045	0.114	0.087
	C. BPS:04/09 (N = 5378)			
Transfer policy (ECS)	-0.008	0.017	-0.054***	-0.006
	(0.022)	(0.012)	(0.015)	(0.009)
Goal: BA or higher	0.156***	-0.061***	-0.014	0.045***
	(0.022)	(0.013)	(0.023)	(0.005)
R-squared	0.120	0.127	0.064	0.057

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 2.13 Baseline analysis without sample weights

	(1) Transfer	(2) Certificate	(3) AA	(4) BA
Transfer policy (ECS)	0.007	-0.007	0.016	0.015
	(0.031)	(0.019)	(0.030)	(0.017)
Goal: BA or higher	0.184***	-0.062***	0.024*	0.058***
	(0.013)	(0.009)	(0.014)	(0.006)
R-squared	0.133	0.112	0.071	0.069
N	6742	6742	6742	6742

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions. In addition, models include state and year fixed effects. * p<0.10, ** p<0.05, *** p<0.01

Table 2.14 Descriptive statistics for first-time beginning public two-year students in Table 2.6

	1995-96	2003-04
	Public 2-year	Public 2-year
Student Background		
Female	54.1%	56.1%
White, non-Hispanic	71.8%	59.9.%
Black, non-Hispanic	11.9%	14.4%
Hispanic	11.3%	16.1%
Other race	5.0%	9.7%
HS diploma	87.7%	85.5%
Certificate program	10.0%	4.4%
Associate's degree program	72.9%	79.7%
Age 22 or less	68.6%	70.4%
Dependent	59.7%	63.5%
Received any aid	50.4%	53.3%
Goal to get BA or higher	68.1%	81.6%
Parent education BA or higher	25.6%	28.0%
Attend full-time	47.0%	48.7%
Married	22.3%	17.0%
Have a child	24.7%	21.6%
Work while enrolled	79.7%	77.6%
Take remedial courses	24.0%	29.4%

Table 2.14 (cont'd)

Income (bottom quartile)	28.7%	25.2%
Income (top quartile)	17.1%	25.3%
Postsecondary Outcomes (after 5 yrs)		
Ever transfer	39.6%	36.5%
Attain certificate	10.4%	8.0%
Attain associate's	19.5%	15.0%
Attain bachelor's	7.4%	5.8%
Postsecondary Outcomes (after 6 years)		
Ever transfer	42.3%	40.1%
Transfer to 4-year	30.2%	27.5%
Attain certificate	11.4%	9.6%
Attain associate's	20.7%	18.1%
Attain bachelor's	11.1%	11.4%
State characteristics		
Transfer policy (ECS)	54.2%	74.5%
Transfer policy (GAO)	64.5%	87.2%
Unemployment rate	5.6%	6.1%
Sample size (unweighted)	742	5286

Source: Author calculations from BPS:96/01 and BPS:04/09.

Note: Student background characteristics are defined for the first year in postsecondary while all degree and transfer variables are defined as of five or six years after beginning postsecondary. BPS analysis weights are used. States in at least two of three BPS cohorts.

Table 2.15 Sensitivity analysis using two policy definitions with the latter two BPS cohorts (six year outcomes)

	(1) Transfer	(2) Certificate	(3) AA	(4) BA	(5) Transfer	(6) Certificate	(7) AA	(8) BA
Transfer policy (ECS)	0.050	-0.084**	0.002	0.007				
	(0.069)	(0.038)	(0.064)	(0.030)				
Transfer policy (GAO)					0.243***	-0.021	0.030	0.043
					(0.075)	(0.056)	(0.070)	(0.042)
Goal: BA or higher	0.165***	-0.028	-0.005	0.069***	0.163***	-0.028	-0.005	0.069***
	(0.021)	(0.019)	(0.025)	(0.012)	(0.020)	(0.019)	(0.025)	(0.012)
R-squared	0.172	0.077	0.085	0.115	0.174	0.076	0.085	0.115
N	6028	6028	6028	6028	6028	6028	6028	6028

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions. In addition, models include state and year fixed effects. * p<0.10, ** p<0.05, *** p<0.01

Table 2.16 Baseline results restricted to the states used in Table 2.6

	(1) Transfer	(2) Certificate	(3) AA	(4) BA
Transfer policy (ECS)	0.002	-0.015	0.032	0.027
	(0.044)	(0.024)	(0.032)	(0.016)
Goal: BA or higher	0.200***	-0.039**	0.039*	0.052***
	(0.018)	(0.017)	(0.020)	(0.007)
R-squared	0.159	0.064	0.092	0.077
N	6636	6636	6636	6636

Note. -- All models include controls for sex, race/ethnicity, receipt of high school diploma, dependent status, degree program in the first year, receipt of any financial aid, marital status, indicator for having a child, having a job while enrolled, taking remedial courses parent's education, and being in the top or bottom income quartile. Models also include state-level controls for unemployment rate, appropriations, tuition at universities, other four-year campuses, and two-year campuses, personal income per capita, population demographics, state aid, and the number of public and private two-year and four-year institutions. In addition, models include state and year fixed effects. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

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CHAPTER 3
RELATIONSHIP BETWEEN PRIVATE SCHOOLING AND ACHIEVEMENT: RESULTS FROM RURAL AND URBAN INDIA

Introduction

Several recent developments in Indian education have led to a growing interest in the performance of private schools, especially in their relative ability to improve student achievement compared to public schools. In the western literature, which focuses primarily on the United States, several researchers have investigated this issue using a range of datasets and econometric techniques. In the Indian context, however, such research is still quite limited, so this study makes a useful contribution. We use the propensity score matching technique along with ordinal logit and ordinary least squares regressions on recent, nationally representative, household data from rural and urban India, and investigate the relative performance of public and private schools.

Indian education, relevant background

Several interrelated factors are driving the interest in how private schools are performing in India. In recent years India has enjoyed tremendous success in enrolling millions of previously un-enrolled children in school (e.g., Government of India, 2009-2010). As more children enroll in school, academics, policymakers and parents have become more interested in how well students are performing, regardless of what school they attend.

At the same time, this massive surge in enrollments has placed additional pressure on the already strained government school system which is also the largest provider of education in India.[1] Several studies point to widespread parental dissatisfaction with the performance of the

[1] According to 2008-2009 data at the elementary level 70-80 percent of all rural enrollments are in government school where as 36-50 percent of all urban enrollments are in government

government schools. They argue that government schools are fast losing their enrollments to private providers (e.g., Muralidharan & Kremer, 2006). Guided at least in part by this growing dissatisfaction with the government school system, the nation has seen a steady growth in private school enrollments (e.g., Kingdon, 2007; Wadhwa, 2009). This growth in the private school sector is yet another factor that guides the growing interest in their performance compared to that of public schools.

The phenomenon of the growth of private schooling is also nuanced. Based on recent research, James Tooley and his colleagues argue that much of the growth in private schooling is actually occurring in the so-called 'low-fee' private school sector: privately-run schools that cost far less than elite private schools. One of the most important cost-saving strategies among these schools is lower teacher salaries. According to Tooley and Dixon (2003), at least in certain parts of urban India such schools may actually dominate and outnumber public schools. This fascination with a potentially low-cost yet more effective alternative to public schools is the third factor that has led to an interest in the performance of private schools in India.

Finally, and most recently, the Indian government passed the Right to Education Bill. Among other things this landmark bill mandates that all private schools must reserve 25% of their seats for poor and marginalized children, and that the cost of these seats will be paid by the government. The bill also mandates that the government will regularize the operation of private schools (*Gazette of India*, 2009). Once again, this highly debated and discussed bill has underscored the need to better understand how well private schools are performing in India.

To summarize; general growth in school enrollment, increased performance pressure on government school systems, growth in the private school sector, the emergence of low-fee

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schools. Since about 70 percent of India population still resides in the rural area this makes government the largest provider of elementary education.

private schools, and the added focus on private schools because of the Right to Education bill have led to great interest in the performance of private schools in India. Do these schools produce higher achievement and do they produce such outcomes more efficiently (at lower costs)? These questions have been at the center of several civil society and policy debates.[2]

Private school performance: Existing research from India

Little empirical evidence is available on the performance of private schools in India. Researchers have found that, in general, without accounting for covariates (or in raw terms) children in private schools out-perform those in public schools (e.g., Wadhwa, 2009). Even after controlling for covariates, various studies that rely on varied samples and varied methods generally tend to find a private school advantage (e.g., Kingdon, 2007; Muralidharan & Kremer, 2006; Tooley, Dixon, Shamsan, & Schagen, 2010; Goyal & Pandey, 2009).

However, to truly identify the effect of private schooling it is important not just to control for covariates in a regression framework but also to account for the selection issue. That is because children do not enroll in schools randomly; their school participation reflects their parents' explicit choices and the value their family places on education, factors that may in turn be related to the children's achievement. Conventional data indicate that children 'sort' into school types: typically, the children from better off and better informed families tend to enroll in private schools (e.g., Goyal & Pandey, 2009). This sorting leads to the possibility that in the

[2] For instance, a group of citizens in New Delhi, India's capital, have formally come together to demand a more systematic implementation of school choice via school voucher programs. This School Choice Campaign (SCC) spearheaded the Delhi school voucher program. To give another example, in 2009 India's leading Economics and Policy journal *Economic and Political Weekly* carried an impassioned exchange spanning several issues between policy scholars, educational and economic researchers about the role and importance of private schools in Indian education.

regression framework cov (PRIVATE, e_i) $\neq 0$, where e_i is the error term in the regression equation which in turn would lead to a biased estimation of the 'private' school effect.

In India, to our knowledge, four studies have attempted to explicitly account for, or correct for, the selection issue (Kingdon, 1996; French & Kingdon, 2010; Desai, Dubey, Vanneman, & Banerji, 2008; Goyal, 2009). In general these studies find a positive effect for private schools; after appropriate corrections are made for the selection issue, this effect may be attenuated but it does not disappear. Kingdon (1996) uses the selection approach developed by Heckman, Ichimura, and Todd (1997) to correct for selection bias, and Goyal (2009) uses a technique proposed by Altonji, Elder and Taber (2005, cited in Goyal 2009) to measure the selection bias. The key limitation of these studies is that their findings have only limited generalizability. Kingdon's study is based on data from one district in one state, and Goyal's study uses data from eight districts in just one Indian state.

French and Kingdon (2010) had access to data from all of rural India, with a set of household covariates including maternal age, maternal grade level education, and maternal reading ability. Like Goyal (2009), they measured selection bias using the technique proposed by Altonji, Elder and Taber. They used household fixed effects to correct for selection and measure the effect of private schooling at the child level (which is the focus of the present paper). They similarly used a 3-year panel of village level data to correct for selection and measure the private school effect at the village level. Using the household fixed effects approach they compared the performance of two children within the same household where one child was enrolled in private school and another in public school. This approach allowed them to account for all the observed and unobserved household covariates. However, the household fixed effects approach is also not without limitations. The problem with using household fixed effects is that parents often invest differently in children within the family depending on the child's observed potential (observed by the parent, not by the researcher) or due to some other form of favoritism (in India for example preferential investment in the male child is well documented) . Thus a more 'heavily invested' child attending private school may be doing better simply because he or she has more academic talent to begin with or because he or she is being favored in other ways, in addition to being 'selected' for (rather than being 'randomly allocated' to) the more expensive private education. The other issue potentially with the household fixed effects approach is the representativeness of the sample which may have implications on the generalizability of these findings. The households that make different schooling decisions for their children may be a small subset of the broader population and they may be unique compared to the rest of the population[3].

Finally, Desai et al. (2008) use the India Human Development Survey (IHDS) data, which we also use for this study. They also use the Heckman et al. selection approach to first model private school choice, and then measure the achievement levels associated with private schooling. Citing Stolzenberg and Relles (1997) they acknowledge that the use of the Heckman approach is sensitive to the exclusion restrictions; therefore, like French and Kingdon (2010)

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[3] A descriptive analysis of our nationally representative data (which is different than the data used by French and Kingdon, 2010) upheld both these concerns. We find that families where siblings attend different school types do treat public and private school attending siblings differently. This was reflected in the private school attending sibling receiving additional educational support at home (in terms of receiving private tuition and longer private tuition hours) and missing fewer days at school, in spite of attending schools that were much farther away from home. Also, confirming the male-child bias observed elsewhere in the Indian literature, private school attending children in families where other siblings attended public schools were disproportionately more male than the overall proportion of male children attending private schools. We also found that only 5-6 percent of rural and urban children reside within such families that engage in such differential decisions for their children. In rural areas such children came from families that were more educated and economically better off. In both rural and urban areas such children were disproportionately likely to come from female headed households.

they also use the household fixed effects approach which suffers from the limitations discussed above. In addition Desai et al. analyze rural and urban data within a single econometric framework. Given the differential spread and demand for private schooling in rural and urban India and given the large overall differences between these two settings, such a unified treatment of rural and urban schools may not be appropriate for such an analysis.

In summary, several existing studies have corrected or attempted to correct for selection bias and thus present a valuable addition to the literature on private schooling in India; still, each leaves room for improvement and additional analysis in terms of data, methods and modeling approaches. Also, despite the recent growing interest in low-fee unaided private schools, none of these four recent studies has attempted to evaluate their performance. Given this background, and given the growing interest in the performance of private schools in India among policymakers, academics, and members of civil society, the present study using nationally representative data makes an important contribution to the literature. We treat rural and urban data separately, make an initial attempt to distinguish the performance of low-fee private schools, and most importantly attempt to correct for the inherent selection bias in private school choice using propensity score matching approach- a "powerful method for reducing bias" (Imai, 2005, pp.295) when using a dataset with an extensive set of relevant covariates, like the one we use for this study.

Materials and methods

Dataset and key variables

For our analysis we used the propensity score matching technique (Rosenbaum & Rubin, 1983a) to compare the performance of public and private schools. We supplemented this analysis with regressions on appropriate covariates.

Dataset

We used the India Human Development Survey 2005 (IHDS), a nationally representative survey of 41,554 households, conducted jointly by researchers from the University of Maryland, in the United States and the National Council of Applied Economic Research in India. This survey is unique: in addition to several standard household variables, variables pertaining to individuals in the household, and basic descriptors for children's schools, it also includes assessments of the children's reading, writing, and arithmetic skills. Standard test-score data, which are often used to compare the performances of children in public and private schools, are collected from children within classrooms and schools. Because such datasets rely on the children's reports about their household (parental education, household possessions, household economic status, etc.) they often cannot describe the students' home environments completely accurately. The IHDS data, however, provide both a rich set of household descriptors and test-score data (collected from children in the household aged eight to eleven), making it an ideal dataset to address the issue of selection into private schools.

To conduct this analysis we worked with data on those children who were enrolled in school at the time of data collection and for whom the test-score data were available (which constrained us to the age group eight to eleven). Missing data were not a serious concern for our study. Less than one percent children in rural and urban data had missing information on one of the three key outcome variables (discussed in detail below). These observations simply dropped out of the regression analysis since it is not advisable to impute missing data on the dependent variables. Even fewer observations if any, were missing for the other variables. Depending on the definition of private school utilized the final sample covered around 7,000 children from rural India and around 3,000 children from urban India. Various relevant tables provide the precise sample sizes as applicable.

Definitions of private school

Broadly, India has at least three types of schools. Public schools are the schools owned and run by the government. The private school category can be divided into two groups: private-aided, and private-unaided. In private-aided schools the government provides all or the majority of the funding (aid) but the school is run privately. Private-unaided schools are both funded and run privately. Private-aided schools are more regulated, as most of their decision-making is overseen by the government, unlike the situation in the private unaided schools. Prominent researchers have argued that for all practical purposes 'private aided' schools should not be included in the 'private' category, as they bear much greater resemblance to regular public schools in most regards including their fee structure (Kingdon, 2007). For the purpose of this paper, therefore, we use two different definitions of private school. We define PRIVATE to include only private non-aided schools[4]; we group both public and private-aided schools in the 'public' category. We define PRIVATE_ONLY to include once again private non-aided schools in the private category, but this time in the public category we include *only* government schools, i.e. we compare non-aided private schools with government schools only. Thus using PRIVATE_ONLY effectively drops the observations where the child is enrolled in private-aided schools. This will result in not using little over 2 percent of rural observations and close to 6 percent of the urban observations. For the sake of simplicity throughout the rest of the paper we refer to 'private non-aided' as simply 'private,' and indicate the reference category by identifying if we are using the PRIVATE or PRIVATE_ONLY as the independent variable.

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[4] From our data we are unable identify if a school is recognized by the government or not.

An imperfect effort to identify low-fee private schools

In addition to comparing private and public schools, we made an effort, albeit imperfect, to identify 'low-fee' private schools. We did so explicitly to see if our data would provide any evidence for or against the benefits of low-fee private schools relative to government schools. Corresponding with the two private school definitions we classified a private school as 'low-fee private school' (LFPVT and LFPVT_ONLY) if its reported fees were lower than the *maximum* fees for government schools in the same district for the same grade level (primary or middle) and for the same setting (rural or urban). The remaining private schools for the same grade level, setting, and district were identified as 'high-fee private schools' (HFPVT and HFPVT_ONLY).

Dependent variables

The key outcome of interest is student performance. The assessment data that we analyzed were collected using widely used tests developed by the NGO Pratham. The tests are described in detail by Desai, Dubey, Vanneman, and Banerji (2008). These tests are limited in their psychometric abilities but they do provide a useful benchmark of student achievement levels in India where such data has traditionally been hard, if not impossible, to obtain. The children were given separate reading, math, and writing tasks. Their performance in reading was graded on a scale of 0 to 4 (from cannot read at all, to can read a whole story), in math on a scale of 0 to 3 (cannot recognize 2-digit numbers to can divide a 3-digit number by a 1-digit number) and in writing on a scale of 0 to 1 (cannot write, to can write with 2 mistakes or less).

In our analysis, none of the scores on these three tasks were distributed normally—which was not unexpected given the nature of the items. We therefore generated two primary dependent variables. We generated a dependent variable SCORE which simply is the summation of the student's performance on the three tasks. Such a continuous variable allows us to harness the maximum amount of variation in child achievement available in the dataset. However, a 'unit change' in SCORE has no clear meaning in terms of changes in the child's skill-levels[5]. So we additionally generated an average score for each student across the three tasks. We then ranked students based on where they fall in the national distribution of average proficiency scores. This dependent variable Proficiency (PROF) is an ordinal variable with three values: 1 = low average proficiency (a child in the bottom third of the national average proficiency distribution), 2 = medium average proficiency (a child in the middle third) and 3 = high average proficiency (a child in the top third). While this dependent variable is more complex to understand, it also carries a clearer substantive meaning. In addition to these two primary dependent variables, we conducted all the analysis separately for each of the three test-scores (READ, WRITE, MATH).

Methods

While we are primarily interested in the results from the propensity score matching analysis, we also conducted regression analysis. We conducted this analysis to compare our findings with a large body of existing literature that only corrects for selection through covariates in the regression framework. For the sake of simplicity we discuss the methods with respect to independent variable PRIVATE, but the same details apply also to independent variable PRIVATE_ONLY.

Ordinal Logit and Ordinary Least Square Regressions

Equations 1 and 2 express the regressions we estimated. Equation (1) quantifies the private benefit with appropriate covariates. Equation (2) uses the low-fee, high-fee definition along with the covariates. We conducted each analysis separately for both the rural and urban data. Each regression model includes appropriate sample weights and accounts for clustering at

[5] A visual inspection reveals that this variable is also not normally distributed.

the district level. The covariates are derived from the literature. The HOUSEHOLD variables are the family's caste, income, assets, the number of people in the household, and the sex and education of the household head; the CHILD variables are the child's gender and age, the years of education completed by the child, whether the child receives private tuition, and whether or not the child works in addition to attending school.

$$Y = \beta_0 + \beta_{PVT}\,\text{PRIVATE} + \boldsymbol{\beta_{HH}}\,\textbf{HOUSEHOLD} + \boldsymbol{\beta_c}\,\textbf{CHILD} + e \qquad (1)$$

$$Y = \beta_0 + \beta_{LFPVT}\text{LFPVT} + \beta_{HFPVT}\text{HFPVT} + \boldsymbol{\beta_{HH}}\,\textbf{HOUSEHOLD} + \boldsymbol{\beta_c}\,\textbf{CHILD} + e \qquad (2)$$

We estimate equations (1) and (2) for the dependent variables SCORE, READ, WRITE, and MATH using OLS. We used ordinal logistic regression for dependent variable PROF. The ordered logit model assumes that there is an underlying, continuous, latent dependent variable y* mapped onto the observed variable PROF. In our case, that would mean there is some underlying ability to read, write, and do arithmetic. Formally, the underlying process looks like $y^* = \mathbf{x}'\beta + \varepsilon$, where y* is the unobserved dependent variable, **x** is a vector of independent variables, and β is the vector of regression coefficients. However, we only see the proficiency group that each student is in. As a result, we can write our observed dependent variable PROF, which is proficiency, as a function of the underlying y*. This can be written as PROF = 1 if $0 \leq y^* \leq \tau_1$, and PROF = 2 if $\tau_1 < y^* \leq \tau_2$, and PROF=3 if $\tau_2 < y^*$ where τ's are called thresholds or cutpoints. The use of the ordinal logit model also assumes that the error term ε has a logistic distribution with a mean of 0 and a variance of $\pi^2/3$ (Long Scott, 1997).

The results from ordinal logit regressions are presented as odds ratio, where an odds ratio of greater than 1 associated with PRIVATE (or PRIVATE_ONLY) indicates greater odds of being ranked in the higher proficiency level nationally for a child attending private school versus the reference category. In the OLS scenario a positive coefficient associated with PRIVATE (or PRIVATE_ONLY) indicates the units by which the given dependent variable (SCORE, READ, WRITE, MATH) will increase for a student attending private school versus the reference category.

Propensity score matching

The problem with regression analysis is that the estimates of β_{PVT} are only as good as the controls included in the model. And while the results are sufficient to establish associations, they are not sufficient to generate causal claims. This is because children are not allocated to schools randomly. The potential family factors associated with sending a child to private school (such as greater access to resources, greater involvement in the child's educational experiences, greater willingness and ability to invest in the child's education) may also be associated with the child's achievement. In the standard regression framework therefore a positive β_{PVT} coefficient may at least in part be reflecting not the actual effect of attending private schools, but perhaps the effect of greater parental attention and involvement.

More precisely, let PRIVATE=1 indicate attending private school (treatment) and PRIVATE =0 indicate attending public school (control). Let Y once again indicate the student outcome on the test. To estimate the effect of private school on outcomes, ideally, we would like to see $Y_{i1} - Y_{i0}$ for each student *i.* But we cannot observe both treatment and control test scores on the same student. We can only see either Y_{i1} or Y_{i0} for each child, not both. As a result, we cannot estimate $Y_1 - Y_0$ for our sample. This problem could be addressed if it was possible to allocate children randomly to different school types. But this is also not feasible.

However, with retrospective data we can estimate the average treatment on children in the treatment (private school) group, (ATT), $\tau_{ATT} = E(Y_1 - Y_0 \mid \mathbf{X}, PRIVATE = 1)$ where **X** denotes a set of observed covariates used to calculate propensity score (Smith & Todd, 2001). With this approach we use an extensive set of covariates **X** and a standard regression model for dichotomous outcome to calculate each student's probability of enrolling in private school (propensity score). Next we compare students who have the same or similar propensity of being enrolled in a private school but a group that was enrolled in private schools (treatment) and group that was not (control). This approach, of comparing students based on their propensity score rather than matching students on all the confounding covariates, reduces the dimensionality of the problem (Rubin, 1997).

The use of the propensity score method is predicated on the assumption of 'strong ignorability' (Rosenbaum & Rubin, 1983b). This assumption states that:

$$(Y_0) \perp PRIVATE \mid \mathbf{X} \tag{3}$$

In other words, conditional on the observed covariates, the unobserved potential outcomes are independent of treatment assignment. This assumption states that the observed covariates (**X**) contain sufficient information for the counterfactual outcome Y_0 to be independent of the treatment. The IHDS dataset are uniquely suited for this purpose since they allow us to observe a number of relevant covariates that describe the child, their home and family, and their learning environment including covariates that may be relevant to modeling private school choice.

A second important assumption is overlap or common support. This assumption states that everyone in the defined population has some chance of being treated or not treated based on their observed characteristics. Formally:

$$0 < P(\text{PRIVATE}=1 \mid \mathbf{X}) < 1 \text{ for all } \mathbf{X} \tag{4}$$

In the propensity score framework, we instead work with the probability of being assigned to the treatment, as calculated by a standard model for binary outcomes. This means that the fitted propensity score values for the treated and untreated groups must overlap.

In addition, Heckman, Ichimura, and Todd (1997) identify several features of such evaluation studies which if attained will further reduce bias. For both treatment and control groups these include (1) same distribution on unobserved attributes (2) same distribution on the observed attributes (3) using the same instrument for data collection; and (4) ensuring that both groups were placed in the same economic environment. They further suggest that fulfilling conditions (2) through (4) is relatively more important than fulfilling condition (1).

Our study fulfills criteria (2) and (3). We cannot make claims about the identical distribution of unobserved attributes. In fact this is one of the key limitations of the propensity score matching approach. Like several standard statistical techniques propensity score matching relies on variables that are observed. Matching between treatment and control groups on these observed variables no matter how perfect is still unable to comment on the matching between the two groups on unobserved variables. Similarly, we are unable to ensure the same environment for both groups since states in India vary considerably in their rates of private school enrollment[6]. However, by running the analysis separately for rural and urban India we made at least a partial attempt to compare treatment and control in not altogether dissimilar environments.

We conducted this analysis separately for rural and urban data and for the two definitions of private schools. To calculate the propensity scores we used probit regressions for binary

[6] These limitations are not uncommon; a recent paper by Doyle (2009) using propensity score matching analysis in a different context identifies the same two limitations of his study.

outcomes, to predict the probability of a child enrolling in private school given their background. We follow Ruben and Thomas (1996) and include an extensive set of relevant covariates. Specifically, this regression included all the controls except 'age' and 'years in school' that we mentioned earlier in the regression framework. In addition the probit estimation included three other sets of attributes. These were, first, attributes related to the child: the number of days the child was absent from school, and whether the child receives support from a government agency regardless of school type, hours spent on private tuition. The second group of attributes is related to the family and the importance it places on educating the child: the highest education level among adults over age 21, whether the household is below the poverty line, whether the household head can speak English, and the number of children in the household. The third group of attributes related to the child's school experience; that are not *due to* the school choice but factors that may influence parental decisions to enroll their children in one type of school versus another. For example, how far is the school from the child's home, how early does the school start teaching English, and does the school provide meals? (The second factor was used by Desai et al. (2008) and the third by Goyal (2009) as controls in their respective studies). A detailed list of these variables, their coefficients in the probit model, and associated descriptive statistics for the control and treatment groups are available upon request.

After fitting the probit regressions, we used the psmatch2 command in Stata to match observations. We employed nearest neighbor matching without replacement with caliper to match the data for rural and urban data and for PRIVATE and PRIVATE_ONLY (Guo & Fraser, 2010). We checked for balance in the matched sample using the pstest command at 5% level of significance[7] and we also ensured that the p-value associated with the likelihood-ratio test of the

[7] One variable in rural PRIVATE_ONLY balanced at 10% level of significance.

joint insignificance of all the regressors after matching was greater than 0.10. Following Guo and Fraser (2010) we began with checking for balance with a caliper size of 0.25 σ_{PS} (where σ_{PS} is the standard deviation of the estimated propensity score) and tried different caliper sizes to attain balance while maintaining the largest sample possible. The final caliper size for the rural data were 0.17 σ_{PS} and 0.20 σ_{PS} and for urban data 0.09 σ_{PS} and 0.10 σ_{PS} for PRIVATE and PRIVATE_ONLY respectively. The literature is not clear on the value of using sample weights while generating a propensity score analysis. We did not use sample weights at this stage since propensity score analysis is used to match treatment and control groups within the sample (Zanutto, 2006). Intuitively the matched subsample generated in this manner identifies comparable children (based on the available covariates) where one group attended private non-aided school and the other attended the reference category schools (depending on whether we used PRIVATE or PRIVATE_ONLY variable). Comparing these 'similar' children who received different types of schooling provides one way to address the non-random allocation of children to different types of schools. As may be anticipated, this matched sample is a smaller subset of the complete dataset (we discuss this reduction in sample size in the results section below).

To arrive at the estimate or the treatment effect, after the matching, we conducted regression analysis including ordered logit regression analysis for outcome PROF and OLS for outcomes SCORE, READ, WRITE and MATH on the matched sample. For these analyses we controlled for age and year in school. These regression analyses used the sample weights in the dataset as we were now interested in generating population-level estimates of the treatment effect (Zanutto, 2006).

Results

Table 3.1 presents descriptive data for rural and urban India. The first panel compares children's performance on each of the five dependent variables separately for each of the school groups (public schools including private aided schools, public schools excluding private aided schools, private schools, low fee private schools, high fee private schools). The second panel provides a similar comparison on school fee and a selected set of variables used for matching. As the first panel shows, comparing raw average performance of public-private children on five different outcome variables across rural and urban areas shows a clear private school advantage. Private school children significantly outperform their public school counterparts on all the outcomes consistently. Interestingly, some of the private school heterogeneity is already visible in this data. When we group children in to low fee private and high fee private school, we find that the low fee private school children perform less well compared to the high fee private school children. The second panel reveals that these private school attending children may belong to systematically different household than their public school counterparts. The descriptive data show the relatively privileged position of children who attend private schools. These differences in background and school fees are especially stark when we compare high fee private school attending children to public school attending children. Their families tend to be well off in terms of both income and assets and their households tend to be better educated. The Table also reveals that, overall, children in urban areas tend to belong to better-off families regardless of the school type, compared to rural children. And in many instances the home background of a child attending a public school in an urban area is comparable to that of a child attending a private school in a rural area. This observation underscores the importance of conducting separate rural,

Table 3.1 Means for the full sample, for dependent variables, propensity scores and selected matching variables, by rural, urban and by different school groups[b]

	Rural					Urban				
	Public (including pvt. aided)	Public (excluding pvt. aided)	Private	Low fee private	High fee private	Public (including pvt. aided)	Public (excluding pvt. aided)	Private	Low fee private	High fee private
Dependent Variables										
PROF	1.76	1.75	2.17	2.04	2.24	2.03	2.00	2.32	2.22	2.37
SCORE	4.44	4.41	5.66	5.28	5.86	5.26	5.18	6.05	5.79	6.17
READ	2.42	2.41	2.98	2.81	3.07	2.78	2.76	3.16	3.09	3.20
WRITE	0.63	0.63	0.79	0.74	0.81	0.74	0.74	0.84	0.79	0.86
MATH	1.39	1.37	1.89	1.73	1.98	1.73	1.68	2.05	1.91	2.12
Selected matching variables (and Propensity Score)										
School fee[a]	481	466	2152	1348	2587	816	742	3564	2649	4023
Household income[c]	35,486	35,231	70,793	56,127	78,693	54,876	52,969	86,843	78,071	91,220
Household assets	8.84	8.75	13.47	12.86	13.80	13.47	13.27	17.88	17.28	18.17
Household head's educ[c]	3.86	3.80	6.08	5.36	6.47	5.92	5.80	8.76	8.04	9.12
Highest level of educ. for female age[c] > 21	2.51	2.43	4.94	4.35	5.26	4.70	4.51	7.47	6.77	7.83
Propensity Score[a]	0.08	0.07	0.67			0.25	0.25	0.77		
N	5748	5574	1374	481	893	1458	1285	1577	525	1052

[a] The sample sizes for the propensity score and school fee are slightly reduced (127 and 28 less observations respectively in the rural data, and 74 and 15 less observations respectively in the urban data).

[b] All public, private differences statistically significant with p<=0.05. (This includes 12 comparisons; public (including private aided) vs. private (PRIVATE), low fee private, high fee private, and public (excluding private aided) vs. private (PRIVATE_ONLY), low fee private, and high fee private for both rural and urban data).

[c] Income is measured in Indian Rupees and Education levels are measured in years.

urban analysis, especially when trying to generate 'matched' data. The Table also highlights the need to make appropriate corrections for selection in order to discern the true private effect.

Regression analysis: Ordinary Least Square and Ordinal Logit models

Table 3.2 presents the results of the regression results for all the five outcomes for rural and urban data separately by the two definitions of the private school variable. We conducted ordinal logit regression analysis for dependent variable PROF and OLS for the other four dependent variables including SCORE, READ, WRITE and MATH. It is important to remind the readers that the coefficients across two different regression methods are not directly comparable. In each panel, columns (1)-(2) correspond to equations (1)-(2). Focusing first on column (1), the results show that, in agreement with earlier studies, there is indeed a positive association between attending private school and having a higher test performance. This positive private effect after controlling for household covariate is somewhat smaller than the raw differences noted in Table 3.1. Also, this effect is consistently positive and significant across all the outcome variables, two separate definitions of private schools and across rural and urban India. It is worth noting that across both dependent variables and independent variables specifications, in absolute terms this association between the school's status as private and the child's test performance is higher in rural areas than in urban ones.

Column (2) adds an additional nuance to this 'private effect.' As we mentioned earlier, we do not believe that our classification of low- and high-fee private schools is by any means perfect; it is merely an attempt to identify what parents might consider 'cheap' private schools within their area, given the public school costs. But based on this imprecise definition of 'low/high fee private' we find a few patterns worthy of note. First, in both rural and urban settings, we find a strong positive association between a child attending a 'high fee' private

Table 3.2 Maximum Likelihood Estimates of a set of Ordinal Logistic Regressions (PROF) and Ordinary Least Square Regressions (SCORE, READ, WRITE, MATH) on full sample, by Rural, Urban and by Private school definitions

Dependent variable		Public (including pvt. Aided) vs. Private [PRIVATE]				Public (excluding pvt. Aided) vs. Private [PRIVATE_ONLY]			
		Rural		Urban		Rural		Urban	
		(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
PROF	Private	2.04 [0.19]***		1.38 [0.11]***		2.02 [0.20]***		1.41 [0.09]***	
	High fee private		2.66 [0.24]***		1.48 [0.10]***		2.64 [0.25]***		1.51 [0.10]***
	Low fee private		1.27 [0.22]		1.22 [0.17]		1.27 [0.23]		1.25 [0.16]*
SCORE	Private	0.73 [0.09]***		0.30 [0.07]***		0.73 [0.10]***		0.34 [0.07]***	
	High fee private		0.96 [0.08]***		0.35 [0.06]***		0.96 [0.08]***		0.38 [0.06]***
	Low fee private		0.33 [0.09]***		0.22 [0.13]*		0.33 [0.09]***		0.26 [0.13]*
READ	Private	0.37 [0.06]***		0.16 [0.04]***		0.37 [0.06]***		0.16 [0.04]***	
	High fee private		0.47 [0.05]***		0.15 [0.04]***		0.47 [0.05]***		0.15 [0.04]***
	Low fee private		0.19 [0.06]***		0.18 [0.08]**		0.20 [0.06]***		0.18 [0.07]**
WRITE	Private	0.08 [0.02]***		0.05 [0.02]***		0.08 [0.02]***		0.04 [0.02]***	
	High fee private		0.12 [0.01]***		0.06 [0.02]***		0.12 [0.01]***		0.05 [0.02]***
	Low fee private		0 [0.05]		0.03 [0.02]		0 [0.05]		0.02 [0.02]

Table 3.2 (cont'd)

MATH	Private	0.28		0.09		0.28		0.14	
		[0.04]***		[0.04]***		[0.04]***		[0.04]***	
	High fee private		0.36		0.14		0.36		0.18
			[0.03]***		[0.04]***		[0.03]***		[0.04]***
	Low fee private		0.14		0.01		0.14		0.05
			[0.07]*		[0.07]		[0.08]*		[0.06]
	N	7122	7122	3035	3035	6948	6948	2862	2862

Note. Standard errors in brackets. * 0.10 significance level. ** 0.05 significance level. *** 0.01 significance level.

school and their test performance. This association is larger in magnitude than simply the 'private' effect observed in column (1). More interesting, however, is the lack of significance of low-fee private schools at 5% level of significance in the rural and urban data for the ordinal logit model with dependent variable PROF and for dependent variables WRITING and MATH using OLS, regardless of the public school comparison group used. When we use the OLS approach with dependent variable SCORE, we find that the low-fee private schools in the rural area have a positive significant albeit smaller coefficient associated with them. In the urban area the low-fee private effect attains significance only at 10% level of significance for both comparisons. Finally for outcome READ both low-fee and high-fee private school effect is positive and significant for both rural and urban data at 5% level of significance. It is likely that the overall of significance of SCORE may in part be explained by the positive findings for READ, since READ ranges from 0-4 and is the single largest component of SCORE.

At the very least, the data seem to indicate that children may not do well simply because they are enrolled in a 'private' school, and especially in urban areas there may be 'private' schools identified by some threshold of costs associated with private schooling within a given area, region, or grade level that in fact fail to significantly improve student achievement outcomes simply because they are ‘private.’ Most important, perhaps, these results highlight the importance of considering the heterogeneity in private schools rather than treating them as a single, homogenous type of schooling experience.

Propensity score matching

Table 3.3 presents the results of matching the data using the propensity score technique separately for two definitions of private schools. After we conducted the matching, the rural and urban sample reduced as expected. (The rural sample is 1056 and 982 after matching on PRIVATE and PRIVATE_ONLY and urban sample is now 912 and 794 respectively for the two definitions of private schools. In other words, the matched rural sample contains 14-15% of the total rural sample and the matched urban contains 25-30% of the total urban sample). As Table 3.3 indicates, after the matching, in both rural and urban samples the mean observations match on several of the key home background variables. Figures included in the published version of the paper, show the distribution of the propensity score and some key variables before and after matching, for rural and urban data for the two separate definitions of private schools. Once again the figures reveal that after matching the matched data are almost identically distributed on several key attributes.

The Table also presents the mean score comparisons on the five outcomes on the matched data (ATT) before we conduct the regression analysis on the matched data. Both for rural and urban data and for both definitions of private schools we now find no statistically significant difference in the performance of children in public and private schools post-matching. In fact, in a few separate cases in the rural and the urban data we find a positive public school effect at 10%

level of significance ($t_{critical} = |1.64|$) and even at 5% level of significance in one case ($t_{critical} = |1.96|$).

Table 3.3 Differences in means for the matched sample, for dependent variables, propensity scores and selected matching variables, by Rural, Urban and by Private school definitions

	Rural			Urban		
	Public	Private	t-test	Public	Private	t-test
PRIVATE (Public including pvt. aided vs. Private)						
Dependent Variables						
PROF	2.14	2.10	-0.64	2.26	2.18	-1.35
SCORE	5.59	5.44	-1.00	5.88	5.71	-1.12
READ	2.94	2.90	-0.47	3.05	3.01	-0.56
WRITE	0.78	0.77	-0.44	0.81	0.81	0.00
MATH	1.87	1.77	-1.55	2.01	1.89	-1.85
Propensity Score and selected matching variables						
Propensity score	0.47	0.50	-1.61	0.58	0.60	-1.18
Household income	52,579	54,643	-0.46	71,864	76,391	-0.90
Household assets	12.02	11.69	1.01	16.07	16.71	-1.87
Household head's education	5.43	5.37	0.21	7.59	7.99	-1.20
Highest level of education for female age>21	4.33	4.15	0.64	6.51	7.10	-1.74
N	528	528		456	456	
PRIVATE_ONLY (Public, excluding pvt. aided vs. Private)						
Dependent Variables						
PROF	2.19	2.09	-1.84	2.22	2.12	-1.70
SCORE	5.65	5.42	-1.52	5.85	5.60	-1.60
READ	2.99	2.90	-1.07	3.06	2.95	-1.31
WRITE	0.79	0.74	-1.65	0.81	0.81	0.00
MATH	1.88	1.78	-1.55	1.98	1.84	-2.03
Propensity Score and selected matching variables						
Propensity score	0.47	0.50	-1.84	0.58	0.60	-1.09
Household income	52,378	55,228	-0.64	70,264	71,929	-0.33
Household assets	12.09	11.96	0.38	15.92	16.45	-1.46
Household head's education	5.19	5.49	-0.99	7.23	7.86	-1.76
Highest level of education for female age>21	4.19	4.21	-0.07	6.24	6.88	-1.76
N	491	491		397	397	

Note. Income is measured in Indian Rupees and Education levels are measured in years.

While the simple comparison of mean on the matched data presented in Table 3.3 already indicates the loss of significance associated with private schools in our data, we conclude with a final set of regression analysis. Table 3.4 presents the results of the ordinary least square and ordinal logit analysis on the matched data for the five outcome variables controlling for child's age and years of completed schooling, as presumably these attributes will have their own independent influence on how well a child does on the tests. After the matching we found that the years of education the child had received and in certain cases their age was positively associated with the child's performance levels. However, in both the rural and urban data, regardless of the definition of the independent variable used (PRIVATE or PRIVATE_ONLY) we find no statistically significant private affect for dependent variables SCORE, WRITE and MATH at 5% level of significance. In the rural data when using PRIVATE as the definition of the key independent variable we find a positive and significant effect associated with private schooling at 5% level of significance for PROF and READ but not at a more stringent 1% level of significance. Thus evidence from matched data presented in Tables 3 and 4 provide insufficient evidence to make an unequivocal claim about the superiority of private schools.

Table 3.4 Maximum Likelihood Estimates of a set of Ordinal Logistic Regressions (PROF) and Ordinary Least Square Regressions (SCORE, READ, WRITE, MATH) on matched sample, by Rural, Urban and by Private school definitions

		Public (including pvt. Aided) vs. Private [PRIVATE]		Public (excluding pvt. Aided) vs. Private [PRIVATE_ONLY]	
Dependent variable		Rural	Urban	Rural	Urban
PROF	Private	1.5	1.00	1.23	0.96
		[0.29]**	[0.14]	[0.23]	[0.15]
	Years of education completed	1.74	1.57	1.92	1.72
		[0.13]***	[0.09]***	[0.17]***	[0.12]***
	Age	1.11	1.20	1.07	1.06
		[0.11]	[0.10]**	[0.11]	[0.09]
SCORE	Private	0.45	0.10	0.27	-0.02
		[0.24]*	[0.14]	[0.22]	[0.15]
	Years of education completed	0.61	0.57	0.75	0.64
		[0.07]***	[0.06]***	[0.08]***	[0.07]***
	Age	0.19	0.15	0.11	0.06
		[0.10]*	[0.10]	[0.12]	[0.09]
Reading	Private	0.31	0.06	0.22	0
		[0.13]**	[0.08]	[0.12]**	[0.08]
	Years of education completed	0.33	0.27	0.39	0.31
		[0.04]***	[0.04]***	[0.04]***	[0.04]***
	Age	0.05	0.06	0.05	0.02
		[0.06]	[0.06]	[0.06]	[0.05]
Writing	Private	0.04	0.05	0.06	0.04
		[0.04]	[0.03]*	[0.04]	[0.03]
	Years of education completed	0.07	0.07	0.09	0.07
		[0.01]***	[0.01]***	[0.01]***	[0.01]***
	Age	0.04	0	0.01	-0.01
		[0.03]*	[0.02]	[0.02]	[0.02]
Math	Private	0.09	-0.02	0.04	-0.06
		[0.09]	[0.06]	[0.09]	[0.07]
	Years of education completed	0.22	0.24	0.26	0.25
		[0.03]***	[0.03]***	[0.03]***	[0.03]***
	Age	0.10	0.09	0.05	0.05
		[0.04]**	[0.04]**	[0.04]	[0.04]
	N	1056	912	982	794

Note. Standard errors in brackets. * 0.10 significance level. ** 0.05 significance level. *** 0.01 significance level.

Limitations and conclusion

Our study makes an important contribution to the limited body of empirical knowledge on private school performance in India. We used recent, nationally representative data from rural and urban schools and investigated the relative performance of private versus public schools. Capitalizing on a rich set of home background covariates, we used the propensity score matching technique to balance the data and arrive at an estimate of the 'private effect' for the rural and urban data.

Before we discuss the main findings from our study we must acknowledge its limitations. The key outcome variables (SCORE and PROF) used in this study cannot claim to have several of the desirable psychometric properties that may be available in large scale test-score data sets such as the Trends in International Math and Science Studies (TIMSS) or PISA. On the other hand, India unfortunately has not participated in any such study in recent years, and in general very few sources of nationally representative student performance data from India are publicly available along with the set of household covariates that would be necessary for such an analysis. As an attempt to avoid overreliance on these dependent variables, we presented all our analysis using the three separate test-score variables which suffer from their own limitations.

Second, we must remind our readers that while propensity score matching is an attempt to minimize the selection bias issue by comparing children who are alike on several attributes, the estimates generated from this analysis are still only as good as the covariates available to us and this analysis cannot make claims about matching children or families on their unobserved traits. Also, the reader should note that creating matched data inherently reduces sample size available to the researcher. We cannot rule out the possibility that with a larger nationally representative sample of children beyond this sample that was collected from 41,554 households and a larger matched sample our findings may look different. In this regard too, while we acknowledge the limitations of our data we must note that to our knowledge this is the only recent, nationally representative, dataset available from India that provides both achievement data and extensive home background data.

Moreover, we are not able to comment on the cost-effectiveness of private schools in this study. In other words, perhaps children at private schools do not perform differently than those in public schools; however, as Tooley et al. (2010) and others argue, these schools simply produce the same achievement levels more cheaply. That scenario is possible, but our data do not allow us to comment on the cost of producing these achievement outcomes from the school's perspective.

Finally, we also acknowledge that without the benefit of longitudinal data, it is always possible to argue that certain aspects of the home environment may be affected by the child's school type rather than the other way around; in that case, such variables will not form suitable controls for our analysis. We have paid careful attention to this potential pitfall of propensity score matching in which the researcher may include a whole host of covariates, sometimes without proper justification. Instead, we have tried to ensure that all the covariates we included in the analysis are supported by the existing literature and/or are appropriate based on the language used in the questionnaire, the distribution of the variables, etc. For instance, we excluded from the analysis variables that related to child's perception of their teacher, or the time they spent on school work at home. While it is likely that these variables simply reflect the child and/or their family's disposition/commitment towards education, it is equally likely that these are post-treatment variables.

Having noted these limitations, our analysis reveals some patterns that we believe make a valuable contribution to the ongoing conversation on the role of private schooling in India. The regression results on the full sample show the importance of accounting for even simple child and home covariates. These results also highlight the importance of conducting separate analyses for rural and urban India where possible, as the results from these two areas are often divergent. Overall the regression results on the full sample agreed with the existing literature that has found a positive association between attending private schools and having better achievement outcomes. However, when we distinguished private schools by their fee levels (using an imperfect measure of 'low/high' fee private schools) we found that children in such schools may not always perform better than those in public schools. As noted earlier, this finding says nothing about the cost-effectiveness of these so-called 'low-fee' private schools, but at the very least this finding highlights the importance of recognizing that the term 'private school' in the Indian context ought not to be treated as a homogenous unit. Rather, possible variations in the quality of private schools may be associated with student achievement levels. Not surprisingly, these results show that children in private schools, whose parents can afford higher fees, do end up doing significantly better than children attending public schools.

After we matched students on a series of covariates using propensity score matching technique, the regression analysis revealed a different result. We found that in both the rural and urban data at 5% level of significance the coefficients associated with private schooling was no longer statistically significant in 18 out of 20 cases we analyzed. We found no negative effect associated with attending private schools; however, unlike earlier researchers we failed to find that private school attendance is associated with any systematic and specific benefit in terms of increased achievement. Overall, the regression results on the matched data failed to produce a clear result that may indicate that children in private schools may be outperforming those in public school in our dataset.

Discussion

After the impressive strides India has taken to improve school enrollment, there is now deep interest in understanding the factors associated with improved achievement levels. A simultaneous and growing dissatisfaction with the public school system has led many researchers and policy advocates to argue that increased privatization and greater reliance on private schools may be important to improve school performance in India. The proponents often argue that privatization in any shape or form (even in terms of low fee/low cost private schools) may be preferable to public provision of education. While the arguments on either side about the roles and limitations of public schools are impassioned, we have limited empirical evidence to inform this debate. Not surprisingly, children from families of higher socio-economic status are more likely to enroll in private schools; to our knowledge, however, few studies have attempted to correct for this non-random selection issue in the Indian case. The limited evidence on this important issue is concerning from the Indian perspective and also from the perspective of other smaller developing countries facing similar pressures yet even less systematic research evidence.

Our study using data, on children aged eight to eleven from a representative sample of rural and urban households in India makes a contribution to this limited empirical literature. In disagreement with some of the other recent studies from India, we find insufficient evidence to claim that children in private schools outperform those in public schools in India. As we discuss clearly in our limitations section better data are needed and there is certainly room to improve analytical approaches beyond those we used in this study. However, our findings generated using appropriate and sophisticated analyses are important because they call into question the consistency of ‘positive’ private effect. Policymakers in the developed world have for decades benefited from nuanced, extensive and multi-faceted conversation on the implications of privatization in education (for instance, Witte (1992), Goldhaber (1996), Rouse (1998), Bettinger (2005), Cohen-Zada (2009), Zimmer, Gill, Booker, Lavertu, Witte (in press)). In the same manner, we hope that this study and other similar studies lead to a more robust conversation on the benefits and limitations of privatization in the developing world.

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