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When Educators are the Learners: Social Learning and the Choice of Comprehensive School Reform Providers

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Abstract

I. Introduction

How do consumers learn about the quality of experience goods? Do they only learn from their own consumption of the good, or can they learn from the experience of others? If consumers learn, how efficient is the learning process? These questions lie at the heart of the analysis of markets for experience goods and the answers to those questions have substantial impact on the welfare analysis of such markets. For example, Bergemann and Välimäki (2006) derive optimal price and sales paths in experience goods markets and point out that, in their model, allowing for social learning changes the optimal price path and can substantially alter the result of their social welfare analysis.[1]

The analysis of markets for experience goods – i.e., goods whose quality cannot be observed prior to consumption - dates back to Nelson (1970) and has traditionally assumed that consumers learn only from their own consumption experience.[2] Such markets give rise to adverse selection, which may be overcome if firms can establish a reputation for good quality with consumers who purchase the product repeatedly. However, in many such markets [give examples] it is natural to assume that consumers communicate with each other about the quality of the product which they experienced. Moreover, in some cases [examples] objective information about the quality of the product may become available after some initial consumers have purchased the product. In this paper, we study an empirical setting in which such information becomes available over time and we investigate to what extent this information influences consumer choices.

[1] In their model, the case with social learning may lead to no product being offered in a competitive market whereas, for the same parameter values, the product would be offered if individuals can only learn from their own experience.

[2] [Other references.]

Our setting is the Comprehensive School Reform (CSR) program, a federal program which awarded grants to public schools to purchase consulting services from private firms. This market provides an excellent opportunity to study how information about experience goods is learned because the market essentially did not exist prior to the start of the federal grants program, but once the program was established a very large number of firms, most of them very small, entered the market and began selling their products to public schools. These products involved relatively complex services, such as teacher training and curriculum development, whose quality would have been difficult to assess prior to purchase.[3] As a result, the first wave of schools receiving grants had to choose among products with unknown quality. Each school only received a single grant and thus there were no repeat purchases. However, schools in later waves could potentially learn from the experience of other schools in previous waves. This paper focuses on understanding whether and how such learning occurs.

To this end, we exploit another interesting feature of our setting, namely the fact that – for the state of Texas – schools' student achievement data as well as their choice of CSR provider are public information throughout the entire period of the program. This allows us to test separately whether schools respond simply to their neighbors' past choices or also to the effectiveness of the plans that were chosen by the school's neighbors. In addition, we can test whether learning is predominantly local, i.e. from geographic neighbors, or whether schools take into account all the information that is available for the entire state.

Models of social learning assume that individuals learn from their neighbors or their peers (e.g. Banerjee, 1992, Ellison and Fudenberg, 1993, 1995). This theory has

[3] [We discuss this further in section II.]

motivated a number of empirical contributions that test for social learning in different contexts.[4] For example, Munshi (2004) shows that wheat growers in India respond strongly to the experiences of their geographic neighbors, whereas the effect is weaker for rice, which is a crop that is more sensitive to unobserved farm characteristics so that information from neighbors is less valuable. Sorensen (2006) tests for social learning in health plan choice at the University of California and finds that new employees are substantially influenced by their colleagues in the same department after controlling for observables and allowing for unobserved, department-specific heterogeneity.

The remainder of the paper is organized as follows. Section II provides institutional background on the Comprehensive School Reform Program. Section III outlines our theoretical framework and section IV presents the data sources and descriptive statistics. Section V presents the estimation and results. A final section concludes.

II. Background on Comprehensive School Reform

The Department of Education (2007) states that "[t]he Comprehensive School Reform program is designed to foster coherent schoolwide improvements that cover virtually all aspects of a school's operations, rather than piecemeal, fragmented approaches to reform."[5] This schoolwide improvement concept and the creation of CSR as a defined product is due not to any organic market-based evolution of demand, but rather to a largely academic research and development effort begun in 1991 by New American Schools, a joint public-private venture (see Berends *et al.* for more on New

[4] There are also a few papers that empirically examine individual learning in experience goods markets, e.g. Israel (2005) and Osborne (2005).
[5] Department of Education (2007) further specifies eleven elements that are required in a CSR program.

American Schools). A federal effort to bring CSR to scale began first with the Comprehensive School Reform Demonstration grants in 1997, and became the Comprehensive School Reform grants program in 1998. Under these programs, public schools could apply for competitive federal grants to purchase CSR plans from private CSR providers or for school districts to develop programs locally. The program was last funded in 2005.

Schools also purchased CSR products using other revenue sources over this time period, but we only observe the school-CSR provider match for federally-funded programs. The most notable change in the federal CSR program was with the No Child Left Behind Act of 2001 and its mandate that federally-funded programs be supported by "scientifically-based research." This mandate increased the importance of state education agencies by creating a gatekeeper role for them in which they review CSR characteristics and approve particular models. In practice, a wide variety of programs has been considered CSR and historically has received federal funding. These programs range from those highly specific in their intervention to those in which it is difficult to detect what exactly the CSR treatment is. At one end of the CSR spectrum are programs like the Coalition for Essential Schools: on its website, it describes "school reform as an inescapably local phenomenon" and acknowledges that "no two Essential schools are alike." It instead emphasizes the shared commitment to a set of principles that are both relatively uncontroversial and vague, such as personalized instruction and an atmosphere of trust and high expectations. In contrast, Success For All(SFA) is an example of a tightly scripted program, with teachers adhering to a prescribed curriculum and methods, with rhyming mnemonics included. In order to create this uniformity, SFA must provide

many more curricular, assessment, and training materials than would a plan emphasizing locally specific needs.

Such great variety in CSR program design naturally leads to the question of the efficacy—and heterogeneity in efficacy—of these programs. There is little existing large-scale quantitative evidence on this question. Mason (2005) investigates the achievement effects of five CSR programs that were implemented in Los Angeles Unified School district. He finds that, of the five programs in his study, none led to uniform improvements in achievement. [Add reference to Borman et al. in this paragraph.]

We attempt to fill this gap by analyzing test score gains of schools which received CSR intervention in Texas, using standardized test scores to control for pre-existing trends and changes in test difficulty over time [Add reference to Cullen-Reback]. We restrict our attention to Texas because Texas implemented standardized testing in ??, thus providing us with XX years of test-score data which we can use to control for pre-While the original impetus for CSR came from national and federal efforts, schools and districts have purchased CSR from a variety of funding sources.

There is no comprehensive accounting of which particular schools use CSR, or how many schools use particular CSR plans, overall. There is, however, a complete database of all schools purchasing CSR plans with federal funds from 1997 to 2003. Federal funds were awarded to State Education Agencies, which would distribute the funds through competitive grants to school districts that applied on behalf of specific schools within their districts. Applications from schools had to specify the CSR provider that the school intended to use, and grants were awarded for use with this provider only.

States typically selected a (long) list of providers that conformed to the federal and any possible state guidelines for CSR, which the schools could choose from. Award criteria were set by the state. Some of the federal grants were reserved for Title I schools, but other funds could be used for CSR efforts of any kind of public school. Schools could apply for CSR grants only once, and grants were given for one year with the possibility of being extended to up to three years. The federal legislation mandated a minimum grant amount of $50,000 per year.

We focus our analysis on a single state, Texas, for several reasons. Most importantly, Texas began testing students and collecting achievement data in 1992 (?), several years before the start of the CSR program. We can thus establish a school's initial achievement level before CSR intervention. Furthermore, TX has accountability standards during this period, which simplifies our analysis by making improvements in pass rates an explicit objective. In addition, since there are cross-state differences in which plans were approved for CSR and potentially also differences in how CSR grants were awarded, focusing on a single state reduces the noise in our estimations. Given that Texas is a large state that received many CSR grants, restricting our attention to this state still leaves us with a sufficient number of observations.

III. Theoretical Framework

We assume that schools, or their administrators, have the objective to improve student pass rates at their school. CSR intervention may affect pass rates, but the effect of each CSR plan on pass rates is unknown. School administrators form an expectation of the effect of each CSR plan on pass rates and of the match quality between their school

and individual CSR plans. In our model, the match quality is based on observable characteristics of the school, such as demographic composition and the school's initial pass rate.

As described in the previous section, CSR grants in Texas were awarded in three waves. Prior to the first wave, none of the CSR providers had operated in the state.[6] As a result, schools in the first wave have no observable information about any of the providers. Schools in waves 2 and 3 have the potential to learn about the quality of CSR providers from the experience of other schools in previous waves. Recall that each school only receives a single grant and thus only chooses a CSR provider once. Thus, schools cannot react to the information that they learn from their own experience with a provider.

Learning may happen in a variety of ways. It may be purely local, for example through word-of-mouth communication with nearby schools or through observation of the experience of those schools. One interesting feature of our setting is that, in principle, school administrators in the later waves have access to a rich set of information that they could base their choice of CSR provider on. The timing of the CSR grants is such that schools in wave 2 or 3 could observe at least one year of test score data *after* the programs of the prior wave were implemented.[7] Student pass rates for each school in Texas for each year were public information, as was the allocation of CSR providers to schools. Thus, school administrators could have used this information to assess the quality of each CSR plan. Our empirical investigation will look for evidence of these

[6] Other states had already awarded grants in 1998 and 1999 to schools using some of the same firms that were active in Texas. However, those other states did not collect systematic test score information that schools in Texas could have used to assess the effectiveness of any of the plans. [Check.]
[7] [Add specific dates of grant applications and test score availability, obtained from our PIR.]

different types of learning. In particular, we will be interested in whether any learning occurs and whether we can distinguish between these types of learning.

IV. Data Sources and Descriptive Statistics

We use three main data sources. The first of these is the Comprehensive School Reform Awards Database from which we obtained our data on federal CSR grants in 2005. This database was collected and maintained by the Southwest Educational Development Laboratory (SEDL), a largely federally funded not-for-profit research firm. SEDL collected these data from state CSR administrators and maintained an online database. The federal Department of Education later took over responsibility for this database, and then removed it from the Internet.[8] The CSR Awards Database is organized at the school level of observation; the grants were three years in duration, and no school received more than one grant during the seven year span of the program, so this is essentially a school-grant level of observation. For each school, we use data on the "models used" (equivalent to the CSR firm with whom the school contracted, and in most cases, only one model) and the first year of the grant.

Each school is identified with its unique seven-digit National Center for Education Statistics (NCES) identification number, allowing us to link these data with the NCES Common Core of Data. From the Common Core, we use data on the racial and ethnic composition of each school, as well as data on school enrollment. Table 1.A provides descriptive statistics comparing school demographics for schools that receive CSR to schools that do not receive CSR. We see that CSR schools have a lower

[8] See http://csrprogram.ed.gov/ for details on accessing the offline data. All data in the paper are public and available from the authors upon request.

percentage of White students and higher percentages of Black, Hispanic and Limited-English Proficient (LEP) students. CSR schools are similar to non-CSR schools in their percentage of Special Education students. Finally, CSR schools have more students and are in larger districts (as measured by the number of schools in the district).

Our final data source is the Academic Excellence Indicator System available from the Texas Education Agency. These data contain annual information on each school in Texas, including the percentage of participating students at each school who passed the statewide achievement test in that year ("pass rate") and the percentage of students at the school who are exempt from the test.[9] [Add details on accountability set.] Pass rates are reported separately for math and reading. We use the average of those two pass rates as our measure of school performance. [Describe Table 1.B here.]

[Describe Table 2 here.]

V. Empirical Approach and Results

Our empirical approach consists of two parts. First, we create measures of the effectiveness of each CSR plan by estimating a set of perceived effects of the plan on pass rates at the schools where it was implemented. These estimated perceived effects range from quite naïve (e.g., xxx) to highly sophisticated (e.g., incorporating existing school-level time trends as an econometrician might do). The second part of the empirical investigation then explores whether there is any evidence, from the choices made by schools with new CSR grants, that school administrators learned from their peers' prior experience with CSR and, if so, how.

[9] [Add Cullen-Reback reference on manipulating test participation here.]

V.A Effectiveness of CSR Plans

In this first part of the empirical analysis, our goal is to develop proxies for the kind of information school administrators could have taken into account at the time of making their decision. Since we know little about the decision-making process within the schools, our strategy is to develop a range of proxies and test how well each of these performs in explaining the choice of CSR plan. Importantly, these proxies will assume different levels of sophistication on the part of school administrators.

The first proxy we use is the most naïve one: It assumes that schools only consider past experiences of other schools within their own district with a given plan and that the measure of effectiveness that schools use is differences in raw pass rates. That is, for a given school that makes its choice in wave 2 or 3, we go through each plan in its choice set and if this plan was chosen by another school in the same district in the past, we compute the change in pass rates that occurred in the school after the implementation of the plan. If more than one school in the district used that plan in the past we use the average change in pass rates across all schools with that plan. This measure obviously ignores many confounding factors that would also have affected pass rates, but we are interested in testing whether such a naïve metric can explain school behavior.

Our second measure increases slightly in sophistication: We still assume that schools only consider past experience within their own district, but we now standardize the pass rates of all Texas schools within each year so that the distribution of pass rates has a mean zero and a standard deviation of one. The main effect of this change is that this metric considers deviations of an individual school's performance from the mean school's performance in the state, i.e. it takes out a common trend. Again, for each plan

we consider the average change in standardized pass rates across all schools within the district that used this plan in the past.

Next, we allow for schools using information not only from their own school district, but also from geographically neighboring districts. We compute both of these metrics, i.e. changes in raw pass rates and changes in standardized pass rates, for schools of the same grade level in other districts that are up to 100 miles away. [To be added: Do this also for schools up to 50 miles away.]

Finally, we consider a more sophisticated approach, in which the decision-maker takes into account all publicly available information. Here, we assume that schools collect all test score information for the entire state and all information on CSR interventions available at the time of their decision and run a regression to assess the effectiveness of each CSR plan. The remainder of this section will describe these regressions. Note that since we attempt to mimic the information that school administrators would have had at the time of their decision, we use different sets of information for schools in waves 2 and 3. Specifically, for schools which submitted their applications during the 2000-2001 school year we use information on pass rates through Spring 2000.[10] For schools applying during the 2003-2004 school year, we use pass rates through Spring 2002. We do not include Spring 2003 data since Texas started using a new test that year.

Table 3 shows regressions that estimate the effect of CSR interventions on school pass rates. The first half of the table shows the results using pass rates from 1994-2000, whereas the second half shows results using data through 2002. As the dependent

[10] We have verified that the information on Spring 2000 pass rates was indeed published before applications for the next wave of CSR grants had to be submitted.

variable we use school-level pass rates, standardized by year so that the state-level distribution of pass rates has a mean of zero and a standard deviation of one. Our variables of interest are dummies that are equal to one after a CSR program has started in a school. All regressions include school fixed effects. Some of the regressions also include school-level demographics, such as the percentage of White students, the percentage of students with Limited English Proficiency, and the school enrollment. We further control for the percentage of students that are exempted from taking the test and for the initial pass rate of the school. Our sample includes all schools in Texas, i.e. we compare schools that receive CSR to all other schools in the state. We do not have information on applicants that were denied grants. Note that our focus in this table is on what kind of information school administrators could have derived from these data.

We begin in column 1 by including only a dummy for the school receiving any CSR treatment, in addition to the school level fixed effects, and find a positive and sizeable coefficient of 0.189. [Need to report marginal effects.] The effect becomes somewhat smaller but remains positive when we add demographic controls and the initial pass rate in column 2. In columns 3 and 4 we consider the effect of individual plans without and with controls, respectively. We find that five of the fourteen plans that we explicitly consider had significantly positive effects on pass rates. The remaining plans had statistically insignificant effects, some with positive and some with negative coefficients. No plan had a significantly negative effect on pass rates.[11] The results in columns 5-8 that include waves 1 and 2 of the CSR intervention are quite similar to our findings in columns 1-4. The most important difference is that two additional plans are

[11] A plan might have a negative effect of pass rates if it is so poorly designed that it reduces the effectiveness of the school's resources. Or, a very contentious plan might lead to resignations of experienced teachers.

now estimated to have a positive effect on achievement, as opposed to a statistically insignificant effect in the previous results.

[Report falsification exercise and put results in an appendix table.]

V.B Learning about CSR Plan Effectiveness

In this second part of our estimation, we are interested in testing whether information about the effectiveness of CSR plans affects the decisions of schools choosing among these plans. To this end, we estimate a multinomial logit model in which schools can choose among thirteen large plans. As described above, we exclude small plans which are chosen fewer than five times and schools choosing those plans. We allow for the school's demographic composition (Percent White and Percent Limited English Proficient) and its initial pass rate to affect the school's choice.[12]

Panel A of Table 4 presents a version of the model without any learning to show the basic effects of these demographic controls. We find that "model 1" (AVID, the 2nd largest plan) is inherently more likely to be chosen than other plans and is more likely to be chosen by schools with a higher percentage of White students. "Model 5" (CRISS, one of the smallest plans) is inherently less likely to be chosen than other plans. The other demographics also have significant effects on the choice of some of the plans, but most are not statistically significant. These controls are included in all of the following regressions, but due to space constraints we do not explicitly report them there.[13]

In Panel B of Table 4 we test whether the experiences of other schools in the same district and in neighboring districts affect a school's choice of CSR provider. We

[12] Our results are robust to including additional demographic controls.
[13] The results are available upon request.

estimate the same multinomial logit model as in Panel A, with the proxies for each plan's effectiveness as attributes of the choices. We begin in column 1 by simply including a dummy for whether another school in the school's own district chose this plan in the past. Note that this dummy can pick up many possible effects besides learning about the plan's effectiveness, such as economies of scale at the local level, or bureaucratic costs of dealing with multiple plans in the same school district. We find that the dummy has a positive effect on a school's choice, i.e. a school is more likely to choose a plan that has already been chosen by another school in the same district. In order to tease out a possible effect of learning about the plan's effectiveness, column 2 shows a specification that includes the dummy for whether another school in the same district used that plan in the past and, for plans that were used before, the average effect of the plan on standardized pass rates. We find that the effect of the dummy remains positive, but it is reduced in magnitude, and the effect of CSR on pass rates is positive as well. This suggests that at least part of the reason why schools tend to choose the same CSR plan as other schools in their district is that they learn about positive effects of these plans.

Next, we test whether information from schools in neighboring districts has an effect on CSR choice. In column 3, we include dummies for whether the plan was chosen in the schools own district in the past and whether it was chosen by schools in neighboring districts in the past. The effect on the own district dummy remains positive, but the effect on the neighboring districts dummy is insignificant. Next, we add an interaction effect for the two dummies since a school may only choose to learn from neighboring districts if there is no prior experience in its own district. The results in column 4 suggest that this may indeed be the case. The effect on the neighboring district

is now positive, but its interaction with the own district dummy is negative and of similar magnitude, suggesting that schools choose the same plan as their neighbors only if they have no prior experience in their own district.

In columns 5 and 6 we add the plans' effects on pass rates in the schools that they were active in. We still find that the effect on pass rates by plans used in the own district matters, but we find no such effect for the effect on pass rates in neighboring districts. This suggests that schools may learn from neighbors – if their own districts have no prior experience – but that they may use less detailed information than if they were to learn from schools within their own districts.

[Fix falsification exercise.]

[Add results with statewide regression effect of each plan instead of average change in pass rates within neighborhood.]

Our final set of results test whether there is evidence that schools use statewide information on plan effectiveness to determine their plan choice. To this end, we use the regression coefficients on each CSR plan from columns 4 and 8 of Table 3 for schools in wave 2 and wave 3, respectively, and a dummy for whether the effect is statistically significant as attributes of the plans in our multinomial logit model. We report these results in Panel C of Table 4. We find no effect of the plan's effectiveness as measured by the regression coefficient from Table 3, whether or not we control for the statistical significance of the effect (see the results in columns 1 and 2, respectively). However, we show in column 3 that if we include only a dummy for whether the plan had any statistically significant effect in the statewide regressions, this dummy has a significantly

positive effect. We interpret this as weak evidence that schools may use some information from statewide data when choosing their CSR plan.

We have also estimated models similar to those in Panel C considering different types of other schools that a school may choose to learn from. In particular, a school administrator may decide to use information on CSR effects at demographically similar school to assess the likely effectiveness of the plan in its own school. Our results in these regressions provide no evidence of any learning of this sort. [Add some of these results to the paper.]

VI. Conclusion

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Table 1: Comparing CSR and non-CSR schools in Texas

Panel A: Means and standard deviations of selected variables

Variable	CSR schools	Non-CSR schools
Percent White	27.0	49.5
	(27.6)	(31.7)
Percent Hispanic	51.6	35.0
	(34.2)	(31.2)
Percent Limited English Proficiency	21.5	11.6
	(21.3)	(17.1)
Percent Black	19.5	13.4
	(24.5)	(19.6)
Percent Asian	1.7	1.8
	(3.3)	(3.7)
Percent Special Education	11.8	12.6
	(4.8)	(6.5)
School enrollment (in thousands)	0.780	0.593
	(0.522)	(0.449)
Number of schools in the district	51.8	36.3
	(67.4)	(60.3)
Observations	3746	53788

This is for all years. Get obs. in 2000 instead.

Notes: Standard deviations in parentheses.

Panel B: Distribution of passrates across both types of schools

Percentile of passrate	CSR-schools	Non-CSR schools
5	38.5	43.8
25	51.7	61.2
50	60.1	71.05
75	69.9	79.5
95	81.8	90

Table 2: CSR plans in the sample, by year of grant

Panel A: Number of Texas schools choosing this plan

CSR plan	Plan number	2000	2002	2005	Total
Accelerated Schools	2	30	30	13	72
AVID	1	26	18	11	55
Direct Instruction	6	9	1	0	10
El Paso Collaborative	8	9	15	1	25
Success for All	14	9	10	2	21
Roots and Wings	13	7	1	0	8
Creating Independent Student-owned Stragegies	5	6	1	0	7
Lightspan	9	6	8	0	14
Co-nect	3	4	10	3	17
Coalition of Essential Schools	4	4	7	0	11
Literacy Collaborative	10	4	3	0	7
Effective Schools	7	3	3	0	6
Modern Red Schoolhouse	11	2	1	2	5
Renaissance Learning	12	2	1	3	6
other	15	31	110	47	188
Total		152	219	82	452
Observations in our sample:		115	101	35	250

Panel B: Percent of Texas schools choosing this plan

CSR plan	Plan number	2000	2002	2005	Total
Accelerated Schools	2	19.7	13.7	15.9	15.9
AVID	1	17.1	8.2	13.4	12.2
Direct Instruction	6	5.9	0.5	0.0	2.2
El Paso Collaborative	8	5.9	6.8	1.2	5.5
Success for All	14	5.9	4.6	2.4	4.6
Roots and Wings	13	4.6	0.5	0.0	1.8
Creating Independent Student-owned Stragegies	5	3.9	0.5	0.0	1.5
Lightspan	9	3.9	3.7	0.0	3.1
Co-nect	3	2.6	4.6	3.7	3.8
Coalition of Essential Schools	4	2.6	3.2	0.0	2.4
Literacy Collaborative	10	2.6	1.4	0.0	1.5
Effective Schools	7	2.0	1.4	0.0	1.3
Modern Red Schoolhouse	11	1.3	0.5	2.4	1.1
Renaissance Learning	12	1.3	0.5	3.7	1.3
other	15	20.4	50.2	57.3	41.6
Total		100	100	100	100
Observations in our sample:		75.7	46.1	42.7	55.3

Panel C: Schools with prior experience in own or neighboring district

	Neighboring district had prior experience		
Own district had prior experience	**No**	**Yes**	**Total**
No	198	176	374
	(44.2%)	(39.29%)	(83.48%)
Yes	17	57	74
	(3.79%)	(12.72%)	(16.52%)
Total	215	233	448
	(47.99%)	(52.01%)	(100%)

Notes: Dummy for own district's experience turns on if district had *any* prior experience. Dummy for neighboring district only turns on if neighboring school *of same grade level* has used CSR.

Table 3: Achievement effects of CSR plans on standardized passrates

Dependent variable: Standardized passrate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years in sample		1994-2000				1994-2002		
Any CSR	0.189***	0.168***			0.209***	0.192***		
	(0.042)	(0.042)			(0.023)	(0.024)		
Percent Exempt		1.447***		1.438***		1.703***		1.700***
		(0.112)		(0.112)		(0.096)		(0.096)
Percent White		0.018***		0.018***		0.019***		0.019***
		(0.001)		(0.001)		(0.001)		(0.001)
Percent LEP		-0.017***		-0.017***		-0.022***		-0.022***
		(0.001)		(0.001)		(0.001)		(0.001)
Initial passrate		0.032***		0.032***		0.034***		0.035***
		(0.008)		(0.008)		(0.009)		(0.009)
Enrollment		-0.443***		-0.441***		-0.434***		-0.435***
		(0.058)		(0.058)		(0.048)		(0.048)
Enrollment squared		0.063***		0.063***		0.067***		0.067***
		(0.018)		(0.018)		(0.014)		(0.014)
Plan 1 (post)			0.210**	0.185*			0.265***	0.229***
			(0.100)	(0.099)			(0.061)	(0.060)
Plan 2 (post)			0.143	0.096			0.209***	0.153***
			(0.095)	(0.099)			(0.057)	(0.059)
Plan 3 (post)			0.860***	0.746***			0.542***	0.445***
			(0.255)	(0.251)			(0.123)	(0.123)
Plan 4 (post)			0.104	0.047			-0.094	-0.205
			(0.252)	(0.249)			(0.137)	(0.141)
Plan 5 (post)			-0.237	-0.110			-0.084	0.051
			(0.200)	(0.197)			(0.126)	(0.123)
Plan 6 (post)			0.199	0.161			0.284**	0.265**
			(0.170)	(0.168)			(0.115)	(0.112)
Plan 7 (post)			0.584**	0.535*			0.417***	0.381**
			(0.280)	(0.275)			(0.154)	(0.150)
Plan 8 (post)			0.647***	0.531***			0.282***	0.187**
			(0.170)	(0.168)			(0.090)	(0.089)
Plan 9 (post)			0.010	-0.035			0.205*	0.137
			(0.209)	(0.206)			(0.115)	(0.115)
Plan 10 (post)			-0.241	-0.029			0.031	0.092
			(0.249)	(0.279)			(0.146)	(0.170)
Plan 11 (post)			0.282	0.268			0.106	0.063
			(0.361)	(0.355)			(0.227)	(0.221)
Plan 12 (post)			1.451***	1.471***			1.306***	1.360***
			(0.347)	(0.341)			(0.206)	(0.200)
Plan 13 (post)			0.046	0.020			0.090	0.077
			(0.192)	(0.189)			(0.128)	(0.124)
Plan 14 (post)			-0.099	-0.126			0.009	-0.049
			(0.179)	(0.177)			(0.101)	(0.101)

Plan 15 (post)			0.149	0.143			0.187***	0.217***
			(0.093)	(0.093)			(0.041)	(0.041)
Observations	42622	41744	42622	41744	55750	53547	55750	53547

Notes: All regression also include school fixed effects and year dummies (not reported).
* significant at 10% level, ** significant at 5% level, *** significant at 1% level
Robust standard errors in parentheses.

Table 4: Learning Results

Panel A: Base Case, no learning

	Direct effect	Interaction with:		
		Percent White	Percent LEP	Initial passrate
model_1	2.710*	0.032*	-0.025	-0.030
	(1.471)	(0.019)	(0.023)	(0.028)
model_2	1.255	0.052**	0.022	-0.029
	(1.213)	(0.021)	(0.023)	(0.025)
model_3	2.283*	-0.120	-0.014	-0.023
	(1.266)	(0.078)	(0.022)	(0.029)
model_4	-0.754	-0.133***	-0.056**	0.042
	(2.036)	(0.039)	(0.026)	(0.038)
model_5	-8.782***	0.030	0.000	0.102***
	(1.999)	(0.019)	(0.022)	(0.027)
model_6	3.154*	0.017	0.017	-0.082***
	(1.826)	(0.017)	(0.022)	(0.023)
model_7	0.189	0.010	0.002	-0.025
	(2.504)	(0.053)	(0.042)	(0.041)
model_9	0.754	-0.015	-0.029	-0.003
	(1.386)	(0.023)	(0.027)	(0.032)
model_10	-2.894	0.047***	0.014	-0.001
	(2.819)	(0.018)	(0.024)	(0.048)
model_11	0.893	-0.057	-0.025	-0.018
	(1.644)	(0.042)	(0.032)	(0.035)
model_12	-2.581	0.049**	-0.041	0.006
	(5.168)	(0.022)	(0.029)	(0.081)
model_13	1.392	-0.026	-0.008	-0.031
	(1.381)	(0.029)	(0.020)	(0.021)
Observations	2756			

Notes: Models 8 and 15 not included in regressions. Model 14 is omitted category.

Panel B: Learning from own district's and neighboring districts' past experiences

	(1)	(2)	(3)	(4)	(5)	(6)
District had same plan in the past	1.805***	1.470***	1.830***	1.792***	1.443***	1.429***
	(0.537)	(0.518)	(0.542)	(0.525)	(0.539)	(0.507)
Plan's std. effect in same district in past		14.026*			14.455*	13.831**
		(7.376)			(7.877)	(6.906)
Neighbor had same plan in the past			0.400	0.885**	0.535*	0.953**
			(0.333)	(0.401)	(0.299)	(0.394)
District had same plan * Neighbor				-1.055*		-0.971*
had same plan in the past				(0.570)		(0.511)
Plan's std. effect in neighboring district in past					-0.445	-0.565
					(0.418)	(0.365)
District had same plan *						0.326
Plan's std. effect in neighboring district in past						(1.204)
Observations	2756	2756	2756	2756	2756	2756

Notes: Models include all controls presented in Panel A.
Notes to self: Cannot estimate 6F. One of the variables drops out b/c of collinearity.

Panel C: Learning from state-wide data

	(1)	(2)	(3)
District had same plan in the past	1.830***	1.984***	1.953***
	(0.561)	(0.575)	(0.559)
Plan's std. effect in same district in past	14.171**	14.443**	14.276**
	(7.172)	(7.323)	(7.143)
Statewide regression coefficient	0.889	-1.724	
	(2.061)	(2.379)	
Dummy for significant effect in statewide		1.756	1.617*
regressions		(1.150)	(0.879)
Statewide regression coefficient * Significant		0.253	
		(3.113)	
Observations	1378	1378	1378

Notes: Regression include only 2002 and 2005 grants.