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"SCHOOL CHOICE AND SCHOOL PRODUCTIVITY (OR, COULD SCHOOL CHOICE BE A TIDE THAT LIFTS ALL BOATS?)

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School Choice and School Productivity
(or Could School Choice be a Tide that Lifts All Boats?)

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I. The Productivity Consequences of School Choice *Matter*

A school that is more productive is one that produces higher achievement in its pupils for each dollar it spends. Formally, a school's productivity is defined as achievement per dollar spent, controlling for incoming achievement differences of its students. Although a great deal of research has dealt indirectly with school productivity (most famously, the "Does Money Matter?" debate), productivity has been neglected by research on school choice. School choice research has concentrated on *allocation* questions, which include: who exercises school choice? who chooses which school? how does choice change the allocation of resources? how does reallocation of students change peer effects? The allocation questions are largely questions of redistribution. While it is theoretically possible that school choice could improve achievement for *all* students through reallocation, such an outcome would require that, for *every* student, the benefits of going to a school that was a better match exceeded the costs imposed upon him by school choice. The costs might include a worsening peer group or declining resources.[1]

In general, then, school choice allocation questions tend to raise redistribution tensions (which students gain, which students lose?). One way to relieve these tensions is to devise allocation-related remedies, such as controls on how resources and peers shift when choice is introduced. However, when advocates of school choice argue that every child would benefit from school choice, they are usually relying on the idea that school *productivity* would increase sufficiently to swamp any negative allocation effects that some students might experience. The basic logic is that choice would give schools greater incentives to be productive because less productive schools would lose students to more productive schools. That is, a school that could raise a given student's achievement for exactly the same money as his current school

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[1] Strictly speaking, what is required is that (1) the current distribution of peers or teaching methods is inoptimal and (2) that school choice would cause people to redistribute themselves in such a way that a *Pareto* improvement in the distribution of peers or teaching methods would occur. It is reasonable to think that families might redistribute themselves so as to achieve better alignment between teaching methods and their children's learning styles. It is less reasonable to think that families, acting independently, could implement a Pareto improvement in peer effects.

would be expected to draw the student away from his current school, thereby shrinking the less productive school and expanding the more productive one. (This is the broad idea; I discuss specific mechanisms through which choice might raise productivity later.) In other words, a general increase in school productivity could be a rising tide that lifted all boats, and the gains and losses from reallocation might be nothing more than crests and valleys on the surface of the much higher water level.

Thus, the first reason that the productivity consequences of school choice *matter* is that they potentially determine whether choice will benefit all children. For the rising tide scenario to be a realistic probability and not just a possibility, however, one must ask what productivity schools could reasonably be expected to achieve. That is, what is the range of productivity over which choice *could* cause productivity to vary? Recent history suggest that school productivity could be much higher than it is now–60 to 70 percent higher. Consider the simplest productivity calculation, achievement per dollar, without any attempt to control for student characteristics. Such a calculation (which I describe in detail below) suggests that average public school productivity was about 65 percent higher in 1970-71 than in 1998-99. This means that, if choice were simply to restore school productivity to its 1970-71 level, then the *average* student in the United States would be scoring at an advanced level where fewer than ten percent of students now score. This improvement in achievement would be so large that it would overwhelm any worst case scenario suggested by the literature on school choice allocation issues.

How does one make such a calculation? We have one measure of student achievement in the United States that reflects the achievement of the entire population of students, is nationally representative, and is designed for comparison over a long period of time and across schools: the National Assessment of Educational Progress (NAEP). Other measures of achievement tend to fail at least one, usually a few, of these requirements.[2] If one simply calculates NAEP points per thousand real dollars spent per pupil, one

[2] The high school drop-out rate, for instance, only reflects variation in the outcomes of low achieving students. Students' self-selecting into the tests generates incurable biases when the SAT or ACT

generates the results shown in Table I.[3] (All money amounts in this chapter are adjusted into 1999 dollars using the Consumer Price Index.) They show that, between the 1970-71 and 1998-99 school years, productivity fell by between 54.9 percent (based on math tests for 9 year olds) and 73.4 percent (based on reading tests for 17 year olds). The bottom panel of Table I shows actual NAEP scores in its upper row and, in its lower row, what NAEP scores would be if schools returned to 1970-71 productivity (1972-73 productivity, in the case of math). For all of the tests, the *average* American student would have a score that fewer than 10 percent of American students currently attain. In fact, the *average* 17 year old would have a score that fewer than 5 percent of American 17 year olds currently attain. The mean American student would be classified by the NAEP as an "advanced" student.

One might wonder whether demographic changes in the United States account for the fall in school productivity, as measured by the simple calculation described above. Perhaps schools were not losing productivity; perhaps they were simply working with students from worse family backgrounds. There is no definitive way to address this issue, but a standard approach is to:

(1) regress 1998-99 achievement on the characteristics of students who took the test in that year and thereby determine the effect of each characteristic (African-American, Hispanic, single-parent family, family income, and so on);

(2) predict what achievement would have been in 1998-99 if the student population were the same as the 1970-71 student population–that is, substitute 1970-71 characteristics into the prediction equation with 1998-99 coefficients;

(3) use predicted achievement to determine what 1998-99 productivity would have been if the student

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are used for comparisons over time or across schools. The SAT and ACT also only reflect variation in the outcomes of high achieving students. High school grades have been shown to be relative measures that cannot be compared successfully across schools.

[3] The sources for the table is United States Department of Education [2000].

population had remained what it was in 1970-71.[4]

If one uses this method, then one finds that constant-student test scores in 1998-99 would be slightly *higher* than actual test scores. For instance, the constant-student mean reading score of 17 year olds would be 290 points for 1998-99, while the actual mean reading score of 17 year olds is 288 points.[5] The fact that holding student characteristics constant makes NAEP scores rise is largely due to the fact that smaller shares of students had high school graduate or college graduate parents in 1971-72 than in 1998-99. Such students tend to score better on the NAEP exam than students whose parents are high school drop outs. (In addition, there were smaller shares of students in 1971-72 whose families had the incomes typical of families today.) The shares of students who are African-American and Hispanic have risen since 1971-72 and these students do tend to score worse on NAEP exam than non-Hispanic white students. However, the effect of changing racial composition is overwhelmed by the effect of changing parental

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[4] The calculation is

$$\rho = \frac{(X_{1971-72}\,\hat{\alpha}_{1998-99})}{pps_{1998-99}},$$

where ρ is productivity, $\boldsymbol{X_{1973}}$ is the vector of characteristics of the 1971-72 student population, pps_{1998} is per-pupil spending in 1998-99, and ˆ is the vector of estimated coefficients from the regression:

$$NAEP_{1998-99} = X_{1998-99}\,\alpha_{1998-99} + \epsilon .$$

$\boldsymbol{NAEP_{1998}}$ is a 1998-99 NAEP score (in reading, math, or science) and $\boldsymbol{X_{1998}}$ is the vector of characteristics of the 1998-99 student population.

Of course, one can do the parallel calculation: what would productivity have been in 1973 if the 1998 student population had existed then? A full Oaxaca decomposition can be used to divide the difference between 1973 productivity and 1998 productivity into (a) the difference caused by changing student characteristics and (b) the difference caused by changing coefficients (effects of those characteristics). The following two Oaxaca decompositions are true by definition:

$$\rho_{1998} - \rho_{1973} = (X_{1998} - X_{1973})\alpha_{1998} + X_{1973}(\alpha_{1998} - \alpha_{1973}) + \epsilon_{1998} - \epsilon_{1973} ,$$

$$\rho_{1998} - \rho_{1973} = (X_{1998} - X_{1973})\alpha_{1973} + X_{1998}(\alpha_{1998} - \alpha_{1973}) + \epsilon_{1998} - \epsilon_{1973} .$$

The variable ρ is productivity. When I do either Oaxaca decomposition, I find that more than 100 percent of the explained change in productivity is attributable to changing coefficients. In other words, changing student characteristics for none of the change in productivity.

[5] The source of data for the calculations described is United States Department of Education [1999].

education. Other changes in the composition of the student population, such as area of the country, have little effect on the adjustment.

In short, student characteristics explain almost none of the estimated decrease in productivity. This fact suggests that school conduct is the main source of the decline in productivity. Consequently, policies that improve school conduct could potentially generate very large increases in productivity.[6] Of course, it is not enough to point out that school productivity could plausibly be much higher than it is. One must investigate whether choice actually induces schools to raise productivity. Such investigations–both how one conducts them and what they show–are the main content of this paper. Before taking up such matters, however, one more vital point about school productivity must be made.

For as long as we have been able measure the factor content of American net exports and the sources of American economic growth, they have been intensive in human capital. This was observed early on by Leontief [1956] and confirmed by a series of other researchers [Keesing, 1966; Krueger, 1968; Jorgenson, 1984; and Jorgenson *et al*, 1989, 1992]. In other words, the United States has a comparable advantage in producing goods and services that make intensive use of educated labor. This comparative advantage has existed because America has always had a relative abundance educated labor. That is, the United States has always been able to produce education in its population relatively cheaply. America's "new economy" products (microprocessors, software, knowledge services) are some of the most human capital intensive products in the world. Yet, we know from basic trade theory that the human capital intensive economy is built on a foundation of American ability to produce education in its population

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[6] One could criticize the constant-student productivity by saying that some student characteristics mean different things in 1998 than in 197X. For instance, coming from a single-parent family is more common in 1998 than in 197X, and–thus–it may be a different experience now than it was in 197X. On the whole, however, such criticisms work in the *wrong* direction. Most of the student characteristics that are more common now than in the past are also less stigmatized–being a minority, from a single-parent family, from the South or Southwest, *et cetera*. The decline in productivity would be larger if one were to take account of the fact that having a single-parent, say, is not as bad for achievement as it was in 197X.

relatively cheaply. While it true that American can import some human capital (for instance, software engineers), imported human capital *cannot* be a source of comparative advantage in the mid- to long-run.[7] Thus, if Americans wish to continue enjoying a growing economy that is centered around human capital intensive products, they cannot be indifferent about rapidly falling productivity in their schools. A school sector with falling productivity translates into America's having relatively costly human capital, which translates into a loss of comparative advantage in human capital intensive goods.

In short, the effect of choice on school productivity is not interesting simply because it could overwhelm the allocation effects of choice on achievement, it is interesting because it also has broad implications for the macroeconomy, for trade, and for Americans' jobs.

II. How Productivity Fits into the School Choice Literature

The productivity implications of choice have been sadly neglected by the literature on school choice. This neglect has nothing to do with the importance of productivity (which is great, as has been discussed) and has everything to do with the roots of the theoretical literature. Models of school choice have grown out of, and used the same basic apparatus as, models of local public goods provision. (See, for instance, the chapters by Epple and Romano, Fernandez and Rogerson, and Nechyba in this volume.) Models of local public goods provision have traditionally focused exclusively on allocation problems, such as who gets what local public good and how one person's local public good choice affects other people. This focus has been inherited by the school choice literature, and–while allocation-focused models of choice are instructive–the intellectual history of the literature should not dictate neglect of productivity. Indeed, it is worthwhile taking a step back to look at some related literature that demonstrates how important

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[7] Other countries can import human capital too, so imported human capital cannot be cheap relative to its cost in the rest of the world. Moreover, under a logical extension of current trends, countries that are currently net exporters of human capital would become the world's net exporters of human capital intensive products.

productivity effects can be when competition is introduced into a market.

Health care is an obvious and recent example. Legislation passed in the late 1980s and early 1990s allowed managed care organizations to compete.[8] The competition has somewhat affected the allocation of health care, but a wealth of research also documents the dramatic effects of competition on productivity, which far exceeded what supporters of managed care had hoped. Competition in the health care market slowed the growth of costs, induced providers to adopt efficiency enhancing technology (such as computers that reduce paperwork), and eliminated methods that protected rents (such as doctors referring patients to their buddies without regard to cost or quality). From 1990 to 2000, health care costs grew just one-half as quickly as in the previous decade, but key health indicators (such as lifespan) grew just as rapidly in the 1990s as in the 1980s. These facts suggest that productivity surged in the more competitive environment.[9]

Trucking and parcel services are other examples. Many researchers have documented how, following deregulation in the 1970s, competition in trucking grew dramatically. The result was a sharp increase in productivity, as documented by Rose [1987], Michel and Shaked [1987], Traynor and McCarthy [1991], and others. For the same money, a trucking customer could get obtain faster, more specialized service after competition than before. In parcel services, the introduction of competition (by companies like United Parcel Services and Federal Express) not only improved productivity because the private firms had higher productivity and productivity growth than the United States Postal Service. The competition also induced the United States Postal Service to substantially increase productivity in its parcel services. Many commentators had doubted whether the United States Postal Service could rise to the

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[8] Managed care organizations include health maintenance organizations, primary provider networks, and certain other physician and hospital networks.

[9] See *Economic Report of the President* 2000. The literature on the productivity effects of managed care is voluminous but Cutler [1997] may serve as a good introduction to it. .

occasion and compete, but it has maintained a large market share in parcel delivery–largely through introducing services (like Express Mail and priority parcel post) that are competitive on price and quality with services offered by the private firms.

In fact, it is somewhat odd that school productivity should be so neglected in the school choice literature because–although productivity was also neglected in the local public goods literature–there is increasing interest among economists in the productivity of not-for-profit, semi-public, and regulated enterprises. Economists are increasingly interested in giving market-like incentives to such enterprises in order to keep workers from rent-seeking despite the fact that they lack conventional profit-maximizing incentives. For instance, yardstick competition among not-for-profit providers of social services (awarding contracts to training programs on the basis of their performance relative to other sites) is increasingly used as a method of inducing productivity gains.

Finally, it is worth noting that one type of school-related research does implicitly contain substantial evidence on productivity, though productivity is rarely mentioned and productivity calculations are never made. I refer to research that compares students' outcomes in public and private schools and that attempts to eliminate selection bias. (Selection bias is the potential bias caused by the fact that students who self-select into private schools might be unobservably different from students who remain in public schools.) The body of research on this topic is well-established and even includes some recent research comparing students who are assigned *by lottery* to a private school voucher or no voucher (so that they remain in the public schools). Peterson's chapter in this volume illustrates the best strain of such research. The consensus in public-private achievement research appears to be that private schools produce statistically significantly better achievement, at least among minority children and children from lower to lower-middle income households.

This body of research could be reformulated as comparisons of the productivity of public and private schools, since there is always an attempt to compare achievement and hold constant the quality of student inputs. Unfortunately, other inputs–especially spending–are *not* constant between private and public schools, and this body of research is often silent about this fact (and almost never controls for it). In particular, the typical private school in the United States spends only about 60 percent as much per pupil as the typical public school, but private school spending is also much more variable than is public school spending so that minimum private school spending is lower than minimum public school spending and maximum private school spending is higher than maximum public school spending. Thus, even if researchers were to find that public and private schools produced identical achievement, it would probably be true that private schools were considerably more productive (because they spend less on average). However, if one is to get an accurate comparison of public and private school productivity, one really ought to make a productivity calculation for each school (thereby taking account of differences in the distribution of spending) and compare these calculations for students with the same backgrounds.

III. Why Should Choice Affect School Productivity?

Why should choice be expected to affect productivity? That is, what mechanism guarantees that low-productivity producers will be driven out by high-productivity producers? Ultimately, this is a question about (1) what schooling producers maximize and (2) what the production function for schooling is like. In fact, there are several answers to this question, and the answer is different for different types of schooling producers: for-profit firms (like Edison Schools), not-for-profit private schools, charter schools, and regular public schools. In this section, I describe the mechanism by which choice might affect productivity for each of these types of schools. I maintain one assumption across all of the cases. It is that, for any given cost to them, parents will choose the school that produces the schooling that they value most. For convenience, I will hereafter call what parents value in schools “school quality,” but I do not assert that parents’ notion of school quality necessarily matches that of the reader. In other work [Hoxby, 1999a], I have presented empirical evidence that suggests that parents tend to prefer schools that have better academic achievement, emphasize academic standards, and promote a relatively structured (disciplined) school atmosphere.

A. A For-Profit School Producer that Takes Up Charter School Contracts

Let us start with a very straightforward case: a for-profit firm that opens a charter school. Such a case is fairly typical of Edison Schools and might become a common model if charter school programs were more widely enacted. The fee that the school could charge would be set by law and parents would not be able to "top up" the fee. Also assume that the school must accept charter school applicants at random (a typical charter school restriction) and is risk-neutral. In other words, a plan to include or exclude students cannot be part of the school's profit maximization strategy.

Then, the school would solve a problem such as:

$$\max_{q,l,k} \pi = p x(q) - c(q,l,w,k,r) x(q).$$

This problem simply says that the school maximizes the difference between revenues (the fixed fee p times the number of students who enroll x) and costs (per-pupil costs c times the number of students who enroll x). The school chooses the quality q that it offers, the staff or labor l that it hires, and the other inputs k that it employs (textbooks, equipment, and so on). The school accepts the going wage rate for staff w and the going price for other school inputs r. Per-pupil costs c are assumed to be increasing in quality, staff hired, and other inputs purchased. I have assumed that per-pupil costs are the same regardless of the school's scale. This is a good starting assumption, but is probably not true. I relax it below.

Given that we have said that parents choose the school that offers the highest quality for a given price, it is clear that enrollment x is increasing in quality q. Specifically, the school enrolls all of the public school students in an area if it offers quality that is higher than that of any other area charter school or regular public school (which would be equally free to parents). That is,

$$x(q_{j^*}) = \sum_{i=1}^{N} 1 \;\; if \;\; q_{j^*} > q_{j \neq j^*} \;\; for \;\; all \;\; j \, ,$$

$$x(q_{j^*}) = 0 \qquad if \;\; q_{j^*} < q_{j \neq j^*} \;\; for \;\; any \;\; j \, .$$

Assume that the school shares equally in enrollment if it offers exactly the same quality as another school.

In these circumstances, the best that the school can do is maximize quality subject to the constraint that its per-pupil cost must not exceed the charter school fee. Put another way, the school *must* maximize its productivity for a given cost or another school will enroll all of the students in the area. Unproductive schools will simply be driven from the market. Note that the firm earns only a slim profit on each student (just enough to repay the owners of its capital with market returns), so the best it can do is maximize the number of students on whom it earns the slim profit. It does this by offering the highest possible quality that the charter school fee can sustain.

Managers of for-profit schooling firms believe that there are economies of scale in schooling because a firm can pay lower prices for its inputs if it pools purchasing, curricular research and development, and information processing across multiple schools.[10] If there are economies of scale, then large firms may be able to earn economic profits (profits that exceed the profits necessary to pay the cost of capital) in local markets where they compete with schools that, for one reason or another, remain too small to take advantage of economies of scale.

B. A For-Profit School that Takes Up Vouchers

The case of a for-profit school producer that takes voucher students is quite similar to the case just examined, except that parents are assumed to be allowed to “top up” a voucher with extra tuition payments

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[10] Pooling may occur even if the schools are in different areas and offer somewhat different levels of quality.

from their own funds. Otherwise, assume that the case is the same: the school must accept voucher applicants on a random bias conditional upon their being willing to pay the school's fees with a combination of the voucher and extra tuition payments. Because the school can now set its fees, its problem is slightly more complicated:

$$\max_{p,q,l,k} \pi = p\,x(p,q) - c(x,q,l,w,k,r)\,x(p,q).$$

That is, when a school sets its fees, it takes into account that a higher fee means–on the one hand–greater revenue per student who enrolls but–on the other hand–lower enrollment (because a higher fee discourages enrollment for any given level of quality offered by the school. It can easily be seen that, for any given fee *p*, the school must maximize the quality it produces subject to the constraint that costs are less than or equal to *p*. In other words, the school must still maximize productivity if it is not to lose all its enrollment to another school that offers higher quality for the same price.

Note that, in the equation above, I have allowed for economies of scale. Thus, when a school considers aiming for a "better" but smaller niche of parents, who are willing to be charged higher fees for better quality, it must take into account the loss of economies of scale (and the consequent increase in its costs).

C. A Non-Profit School that Takes Up Charter School Contracts

The for-profit case is a nice place to begin because the firm's incentives to maximize productivity are obvious. The vast majority of school producers that take up charter school contracts or voucher students are, however, not-for-profits. At first glance, it might seem difficult to say what not-for-profits maximize, but–in fact–relatively simple modifications of the for-profit case capture not-for-profit behavior. The key difference between a non-profit and a for-profit organization is the distribution of surplus.[11] A for-

[11] This point has been discussed by numerous researchers. See Glaeser and Shleifer [forthcoming] for a recent model of non-profit entrepreneurs and for a review of the literature.

profit school distributes profits to its owners (private owners or shareholders). Thus, in the problem above, it was reasonable to assume straightforward profit maximization because owners benefit directly from profits. If a not-for-profit school has surplus (a difference between revenues and costs), it cannot pay them in a straightforward way to anyone. It can, however, use surplus in a variety of ways that make surplus a valuable thing to have. Surplus can be used to make working conditions pleasant for the school's staff (staff lunches, smaller classes, more classroom supplies, and so on) even if these conditions do not contribute to productivity. Surplus also allows a school to pursue social goals that its staff value: experiments with teaching methods, development of new curricula, a diverse student body, exposing students to nature. There are a few things to note about such distributions of surplus. First, they are nearly always inefficient compared to distribution of cash (which is fungible). That is, some of the surplus is lost in the process of being transformed in goods or services that the staff values. As a result, the school staff faces weaker incentives than they would face if they could be given cash incentives. Second, while it is relatively simple to distribute a non-profit school's surplus to its staff in the forms mentioned, it is difficult to distribute it (legally) to a single owner or even a concentrated subset of the staff. Thus, a school has less incentive to expand simply to increase the absolute size of the surplus: the surplus will increase as it expands, but so will the number of staff over whom the surplus must be divided. This is unlike the for-profit situation where owners have an incentive to expand their schooling production so long as they can earn some positive surplus on each additional enrollee.

One can incorporate these features of the distribution of surplus into a non-profit charter school's maximization problem:

$$\max_{q,l,k} \frac{(\alpha \cdot \pi)}{l} = \frac{\alpha \cdot [p\,x(q) - c(q,l,w,k,r)\,x(q)]}{l},$$

where enrollment is given by:

$$x(q_{j^*}) = \sum_{i=1}^{N} 1 \;\; \textit{if } q_{j^*} > q_{j \neq j^*} \;\; \textit{for all } j \,,$$

$$x(q_{j^*}) = 0 \qquad \textit{if } q_{j^*} < q_{j \neq j^*} \;\; \textit{for any } j \,,$$

just as before.

This problem simply says that a staff member at a non-profit charter school wants the school to maximize *(   )/l*, where   is total surplus (what the for-profit school would call profit),   is a factor that is less than 1 (the share of surplus that remains after it has been transformed into goods for the staff), and *l* is the number of staff. Under this maximization problem, the school's incentives to expand enrollment are weaker (than those of a for-profit school), but its incentives to maximize productivity are strong. The school will still be driven out by competitors if it does not produce the maximum quality *q* attainable given the constraint that its costs must not exceed the fixed charter school fee.

Two comments about the non-profit school's maximization problem are in order. First, if there are economies of scale, the school will have stronger incentives to expand enrollment than suggested just above. Second, one's measure of the productivity of a non-profit school may slightly understate its true productivity if the school earns surplus and buys staff rewards with it that appear to be inputs (although they really make no contribution to outcomes that parents value). The understatement will be slight because competition among non-profit schools will drive the surplus towards zero (even as each seeks to maximize its surplus).

D. A Non-Profit School that Takes Up Vouchers

The case of a non-profit school that takes voucher students is just like the case of a for-profit school that takes voucher students *except* that its surplus can be distributed only in the indirect way described above. That is, the voucher school's maximization problem is:

$$\max_{p,q,l,k} \frac{\alpha \cdot \pi}{l} = \frac{\alpha \cdot [p\,x(p,q) - c(x,q,l,w,k,r)\,x(p,q)]}{l}.$$

The non-profit voucher school must maximize productivity if it is not to lose its enrollment to a similar school that offers higher quality for the same fee *p*. The only complication is that the school needs to choose its fee and quality simultaneously, and the only caveat is that the school has weaker incentives to expand enrollment than a for-profit voucher school.

E. A Summary for Fee-Based Schools (For-Profit and Non-Profit)

In all of the cases above, the school's revenues are derived from student fees. It is this *fee basis* that is crucial because it means that parents' choices determine whether a school is finally viable or not. If a school's students are attracted away by a competing school that charges the same fees, the school naturally has to increase its productivity (either by raising its quality for the given fee or lowering the fee it charges for its quality). As shown, the for-profit or non-profit basis of the school is somewhat less crucial. A for-profit schooling firm will have stronger incentives to enter new markets and gain new enrollment, but both non-profits and for-profits have incentives to maximize productivity.

People often wonder whether there will be an elastic supply of charter or voucher schools. This is an important question, especially for non-profits, which do not have clear incentives to expand when they hope to earn only a slim surplus on additional students. If there are economies of scale, then the charter school fee or voucher that makes a school viable with a small number of students should guarantee that it is more than viable with higher enrollment. Thus, economies of scale suggest that both for-profits or non-profits should have elastic supply once they are in business. On the other hand, there are some factors that might function like *diseconomies* of scale. For instance, an charismatic principal might become uninspiring if she managed a large school and therefore had little direct contact with students.

Buildings are often discussed as a possible factor that would limit the elasticity of supply of charter or voucher schools. This, however, would seem to be a short-run phenomenon that mainly plagues the start-up of new charter or voucher programs. The total number of students to be taught does not increase simply because a new school has entered, so the introduction of charter or voucher school competition does not require much of a net increase in school building. As enrollment shifts from less productive to more productive schools, buildings should be sold by the shrinking or exiting schools and purchased by the expanding or entering schools. In fact, there *is* an active market for school buildings and related institutional buildings. If small fractions of school buildings could be sold easily, competition would require *no* net increase in school buildings. Schools are, however, somewhat indivisible–while parts of school buildings are often sold or leased to separate schools, only certain fractions of a building will generally make a viable school. (For instance, most schools require an entrance area, a set of bathrooms, and so on.) Realistically, then, competition requires a small increase in the total stock of school buildings, simply to allow more flexibility as parents' ability to choose makes enrollment more variable.

In any case, it is clear that some factors (economies of scale) suggest that school supply will be very elastic, while other factors (that function like diseconomies of scale) suggest that school supply will be less elastic. The elastic response of charter and voucher schools is, thus, an empirical question and will depend on features of the reform, such as funding for school building.

F. Competition and the Productivity of Regular Public Schools

Does competition give *regular* public schools incentives to be productive? We have seen that fee-based schools face straightforward incentives, but what about regular public schools that are funded mainly by taxes?

If a regular public school faces competition from a charter or voucher school, and the charter school fee or voucher comes directly from its budget, then the regular public school *is* fee-based at the margin and will have the similar marginal incentives to be productive. Whether these marginal incentives work well or not depends on the size of the fee or voucher. Some vouchers or fees are so small relative to regular per-pupil spending that they give public school perverse incentives to drive students away. That is, a voucher or charter school fee that is small relative to per pupil spending (or that is not financed from the public school's revenues) *raises* per pupil spending non-negligibly for each student who is driven away from the public schools. Public school staff may be able to enjoy greater surplus if they drive students away than if they try to attract them. Such perverse scenarios are easy to avoid by setting a sufficiently high voucher or charter school fee.

What if, however, a regular public school does not face competition from a charter or voucher school? It is not fee-based at the margin, so does it have any incentives to be productive? The answer is yes if the public school is financed by local property taxes and faces a high degree of traditional choice among public school districts.

Traditional choice among public school districts is what occurs when parents choose a school district by choosing a residence. This traditional form of choice is by far the most pervasive and important form of choice in American elementary and secondary schooling today. In order that this form of choice give schools incentives to be productive, it is essential that parents choose among *districts* that are fiscally independent. The mechanism that I am about to describe does not work with *intra*-district choice.

Conventionally, public school districts in the United States have revenues that depend largely on local property taxes. If parents in a metropolitan area can choose among a large number of districts, they will tend to favor districts that produce higher achievement for a given local property tax liability or, equivalently, have lower local tax liability for a given level of achievement. That is, parents will tend to favor districts with high productivity. If a school district's productivity falls, it will be avoided by parents who happen to be moving. The resulting decrease in the demand for its houses will drive down the prices of its properties. The lower property tax base will, in turn, drive down the school's budget, which is a function of the property tax base. The administrator will be encouraged to raise productivity, either through maintaining achievement in the face of a falling budget or through raising achievement sufficiently to make the district attractive to homebuyers again. Notice that, although only a fraction of households are moving at any given time, their observations of achievement and tax liabilities are "universalized" through the property market so that *every* family's house price changes in such a way as to give schools incentives to be productive. Notice also that this productivity-inducing mechanism is sustainable over the long-term because it depends on decentralized choices.[12] This is in contrast to centralized reward systems--for example, financial or other "merit" awards for successful school districts that are distributed by the state. Centralized rewards tend to be unsustainable because state governments cannot, *ex post*, credibly adhere to processes that reduce the amount of money going to failing school districts.

IV. Finding Evidence on How Choice Affects School Productivity

In the next two sections of this paper, I show evidence on how choice affects school productivity. The next section focuses on traditional forms of choice (parents choosing among independent school districts and parents choosing private schools). Section VI focuses on recent choice reforms: vouchers and charter schools. There are, however, some problems that arise in any analysis of how choice affects productivity, and the purpose of *this* section is to explain them.

The Endogenous Availability of Choice Options

One problem that plagues the economic analysis of choice is the fact that choice options do not arise randomly, but are frequently a response to school conduct. In particular, when people are dissatisfied with a particular school's conduct, they try to create alternative schools for themselves or maintain their access to existing alternative schools.

It is easy to see this phenomena with respect to the creation (or maintenance) of private schools,

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[12] The mechanism described is the subject of Hoxby [1999b], where it is described in much more detail.

charter schools, and voucher programs. In an area where the public schools are bad, parents are frustrated and are willing to make some effort or devote some money to obtaining alternative schooling. A collapse in the quality of local public schools (as sometimes occurs when an administrator or school finance laws change) tends to send families scurrying toward local private schools. The result is an area in which private schooling is common *because* the public schools are bad. A recent illustration of this phenomena is the substantial increase in private schooling that followed California's school finance equalization [Downes and Schoeman, 1998].

Recent voucher and charter initiatives also illustrate this phenomenon. It is no accident that Washington D.C. has both a voucher program and a rapidly growing population of charter schools. The Washington D.C. district has historically had low productivity: its per pupil spending is in the 99th (highest) percentile for the United States, yet its average student scores between the 10th and 20th percentiles on the National Assessment of Educational Progress. Reports of malfeasance in the D.C. public schools, including the theft of school supplies and payrolls padded with non-workers, are common.[13]

Although the mechanism is less obvious, choice options existing *because* the public schools are bad is a problem that also plagues traditional choice among public school districts. It turns out that voters resist district consolidation in areas where one or more districts (usually the largest central city district) has bad productivity. In areas where all the districts have good productivity, voters elect to consolidate them in order to enjoy economies of scale. Similarly, in districts with bad productivity, sub-areas are keen to secede and form another district. In districts with good productivity, no such secessions occur. The end result of such phenomena is that areas with many districts often contain one or more districts with bad productivity.

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[13] The sources are United States Department of Education, *Digest of Educational Statistics, 2000* and United States Department of Education, *National Assessment of Educational Progress Long Term Trend Summary Data Tables*.

Endogenous school choice in areas with bad public schools generates bias if a researcher estimates naively the effect of choice on productivity. Because schools with poor productivity induce the creation of choice, it can appear as though choice causes low productivity (instead of the other way around). Researchers can avoid this bias only by (1) comparing the same school district before and after a choice reform if panel data are available or (2) finding a source of variation in the availability of choice that is *not* correlated with the underlying causes of bad school productivity. The first solution typically generates differences-in-differences strategies, in which schools that are "treated" with choice reforms are compared, before and after the reform, to similar control schools (that did not experience the reform). The second solution typically generates instrumental variables strategies, two of which are illustrated below.

Unobserved Differences in Student Inputs that Appear to be Differences in Productivity

Some families provide many learning opportunities and resources for their children at home; other families provide few. Children also differ in motivation and innate ability. When measuring a school's productivity, one should fully account for differences in student inputs so that one avoids describing a mediocre school as highly productive simply because it has such good student inputs that achievement is high even if it adds very little learning (beyond what its students learn at home and pick up for themselves). It is not possible, however, to measure all student inputs. In particular, motivation and innate ability are usually not observed and cannot be controlled for.

For finding the effect of choice on productivity, there are three ways that researchers can deal with this problem. Suppose a researcher wants to compare productivity across schools that face strong choice-based incentives (such as voucher or charter schools) and schools that face weak choice-based incentives (such as a large public school district that dominates a metropolitan area). Then, the researcher must ensure that a random mechanism (such as a lottery) that is *not* correlated with unobserved motivation/ability assigns students to schools. If such a mechanism is at work, schools will have an equal allocation of unobserved motivation/ability, and the difference in achievement per dollar spent will accurately reflect true differences in productivity. This approach is illustrated by Peterson's chapter in this volume.

Another alternative is for a researcher to compare the achievement of *all* students from an environment in which there is little or no choice to that of *all* students from an environment in which there is a lot of choice. So long as the students cannot choose the environment to which they belong, this alternative generates good estimates. One example is comparing all students in a metropolitan area with little choice to all students in a metropolitan area with a great deal of choice. (Families are assumed to move among metropolitan areas for reasons *other* than the availability of choice.)

A final alternative is for a researcher to compare the achievement of students who are unlikely to have the unobserved traits that make people take advantage of choice options. An example will illustrate this alternative. Suppose that a researcher wishes to compare school productivity before and after a private school becomes available, and the researcher is worried that students with high unobserved motivation or ability will attend the private school and make it appear more productive than it is. Then, the researcher can compare measured productivity at the public schools *only*, in the knowledge that the "after" productivity of the public schools is, if anything, an underestimate. If the researcher finds that the introduction of the private school increases public school productivity, he may be confident that the result is not generated by unobserved motivation/ability rising at the public school.

Measuring Productivity

Productivity is achievement per dollar spent in a school, and measuring productivity raises a few measurement issues, mostly related to measuring achievement. It goes almost without saying that one should avoid using measures of achievement, such as grades, that have different meanings in different schools and times. One should also avoid using scores on standardized tests that are taken by only a small, self-selected share of students, such as the Scholastic Aptitude Test (SAT1) or American College Test (ACT). Use of such tests generates self-selection bias that is impossible to solve without the use of other standardized tests that are given to the entire population of students. If one has such a population-wide standardized test, however, one should use it instead of the SAT1 or ACT.

Supposing that one has a standardized test administered to the entire population of students, there remains the question of whether to measure productivity with reading scores, math scores, science scores, elementary grade scores, secondary grade scores, and so on. These are all valid measures of productivity, and the researcher is best off presenting several (especially math and reading). It perfectly normal to find that a school has better productivity in some subjects or grades than in others. One may use scale scores, national percentile scores, or any other score designed by the test-maker to be comparable across schools and time.[14]

Measuring per pupil spending presents few problems so long as the same definition is used for all schools. One may use either current spending or (preferably) total spending with smoothed capital expenditures.

V. The Effect of Traditional Forms of School Choice on Productivity

Parents' ability to choose among public school districts (through residential decisions) and to choose private schools are such established features of American education that they are taken for granted. Yet, through these mechanisms, American parents have traditionally exercised some choice over their children's schooling. These traditional forms of choice are useful for predicting the effects of choice on productivity, especially because the availability of traditional choice mechanisms varies greatly across

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[14] Wages and income later in life are additional measures of achievement that are useful complements to measures based on standardized tests. They are useful because they are meaningful to people in concrete way and because they are measured in dollars, as is the denominator of productivity. There are, however, several problems with using wages and income to measure achievement, including a paucity of data linked to schools, questionable validity for women, and the impossibility of analyzing a reform until at least 20 years after its occurrence. I do not present wage-based measures of productivity here, but see Hoxby [2000a] for some wage-based estimates.

metropolitan areas in the United States. Some metropolitan areas contain many independent school districts and a large number of affordable private schools. Other metropolitan areas are completely monopolized by one school district or have almost no private schooling.

In previous work, I have drawn upon traditional forms of choice to generate evidence about how choice affects productivity. I review this evidence here. In addition, I explain how traditional forms of choice generate important evidence on productivity that is otherwise unobtainable and illustrate empirical strategies for determining the effects of traditional forms of choice. For detail on the empirical work described here, see Hoxby [2000a] and Hoxby [2000b]. Rather than provide such detail here, I reserve space for evidence on the productivity effects of recent choice reforms (section VI).

Traditional Inter-District Choice

The first traditional form of choice occurs when parents choose among independent public school districts by choosing a residence. The degree to which parents can exercise this form of choice depends on the number, size, and housing patterns of districts in the area of the parents' jobs. There are some metropolitan areas in the United States that have many small school districts with reasonably comparable characteristics. Boston, for instance, has 70 school districts within a 30-minute commute of the downtown area and many more in the metropolitan area. Miami, on the other hand, has only one school district (Dade County) that covers the entire metropolitan area. Most metropolitan areas are, of course, somewhere between these two extremes. A typical metropolitan area has an amount of choice that corresponds to having four equal-sized school districts (or a greater number of less equally sized districts).

People with jobs in rural areas typically have only one or two school districts among which to choose. To avoid a much-choice/little-choice comparison that mainly reflects urban/rural differences in school productivity, it necessary to focus exclusively on metropolitan areas when analyzing traditional inter-district choice.

It is essential that parents choose among districts that are fiscally and legally independent if this traditional form of choice is to be useful guide to the productivity effects of choice. This is because the mechanism described above, by which parents' housing choices translate into incentives for a school to be productive, does not operate if, say, a district relies entirely on state revenue or is otherwise held harmless from repercussions associated with its inability to attract parents. Intra-district choice among schools provides no useful evidence about productivity effects because the schools are fiscally dependent by definition.

How does one measure the degree of traditional inter-district choice in a metropolitan area? A particularly good index of inter-district choice is the probability that, in a random encounter, two students in the metropolitan area would be enrolled in different school districts. If there were only one district, as in Miami, this probability would be equal to zero. If there were many districts, as in Boston, this probability would be very close to one (greater than 0.95). We can calculate this choice index, C_m, using the following equation:

$$C_m = 1 - \sum_{j=1}^{J} s_{jm}^2 ,$$

where s_{jm}^2 is the square of district *j*'s share of enrollment in metropolitan area *m*. Table II lists the names and choice indices of metropolitan areas in the United States that have very high or low degrees of inter-district choice. It is interesting to note that metropolitan areas as disparate as Saint Louis and Seattle have comparably high degrees of inter-district choice. Metropolitan areas as disparate as Las Vegas and Wilmington equal have zero inter-district choice.

Traditional Choice of Private Schools

The second way in which parents have traditionally been able to exercise choice in the United States is by enrolling their children in private schools. Traditionally, private school tuition in America is not subsidized by public funds (as it is in Canada and many European countries), so parents can only afford private school if they can pay tuition and also pay taxes to support local public schools. Partly as a result, private schools enroll only 12 percent of American students.

In the United States, 85 percent of private school students attend a school with religious affiliation, but such schools include a variety of Christian and non-Christian schools and have tuitions that range from token to over 10,000 dollars. The remaining 15 percent of private school students attend schools with no religious affiliation; these include most of the independent, college-preparatory schools that charge tuition of 5,000 dollars or more. The modal private school student in the United States attends a Catholic school that charges between 1,200 and 2,700 dollars.

A key feature of American private schools is that they typically subsidize tuition with revenues from donations or an endowment (or implicit revenues from an in-kind endowment such as buildings and land). The share of schooling cost that is covered by subsidies is larger in schools that serve low-income students, but even relatively expensive private schools charge subsidized tuitions. For instance, Catholic elementary schools, on average, cover 50 percent of their costs with non-tuition revenues.

The number of private school places (of a given quality) that are available at a given tuition varies greatly among metropolitan area in the United States.[15] For instance, in some metropolitan areas, 15 percent of the elementary student population is enrolled in private schools where tuition is about two-thirds of the schools' per-pupil expenditure. (Typical amounts would be tuition of 1,800 dollars and expenditure of about 2,700 dollars). In other metropolitan areas, fewer than 1 percent of the elementary school population is enrolled in such schools, although places might be available in schools where tuition is higher

[15] The quality of a private school can be measured in various ways, the simplest of which is simply the amount of money the private school spends on educating a student. Because private schools face strong incentives to be productive, their costs are a good guide to their quality. Private school expenditure sometimes understates the true cost of educating a private school student because, especially in schools with religious affiliation, labor is donated by volunteers and church buildings are used for educational purposes.

and there are no tuition subsidies. In short, the supply of private schooling varies among metropolitan areas, and–thus–the degree to which parents have choice between public and private schools varies among metropolitan areas.

It is reasonable to use the actual share of students who attend private school in a metropolitan area as a measure of private school availability *if* the measure is properly instrumented. The instruments must be variables that cause the non-tuition revenue of private schools to vary but are otherwise unrelated to local public school achievement. That is, one wants to use only the variation in private school availability that is generated by factors that affect the *supply* of private schooling, not by factors that affect the *demand* for private schooling (such as the local public schools being bad). I describe the best available instruments below.

Why Evidence from the Traditional Forms of Choice is Necessary

Evidence from the traditional forms of choice is necessary because it can reveal the long-term, general equilibrium effects of choice. Evidence based on recent reforms cannot.

In the short-term, an administrator who is attempting to raise his school's productivity to respond to competition has only certain options. He can induce his staff to work harder; he can get rid of unproductive staff and programs; he can allocate resources away from non-achievement oriented activities (building self esteem) and toward achievement oriented ones (math, reading, and so on). In the slightly longer term, he can renegotiate the teacher contract to make the school more efficient. If an administrator actually pursues all of these options, he may be able to raise productivity substantially.

Nevertheless, choice can affect productivity through a variety of long-term, general equilibrium mechanisms that are not immediately available to an administrator. The financial pressures of choice may bid up the wages of teachers whose teaching raises achievement and attracts parents. It may thus draw people into teaching (or keep people in teaching) who would otherwise pursue other careers. Indeed, it may change the entire structure of rewards in teaching and thereby transform the profession.[16] The need to attract parents may force schools to issue more information about their achievement and may thus gradually make parents into better "consumers." Because parents' decisions are more meaningful when schools are financed by fees they control, choice may make schools more receptive to parent participation. The need to produce results that are competitive with those of other schools may force schools to recognize and abandon pedagogical techniques and curricula that are empirically unsuccessful, even if they are philosophically appealing. Finally, in the long-term, choice can affect the size and very existence of schools. Choice makes districts' enrollment expand and contract; it makes private schools enter and exit. In the short term, we mainly observe how the existing stock of schools changes its behavior.

Both traditional forms of choice potentially create the long-term, general equilibrium effects that interest us.

The Effect of Traditional Inter-District Choice on School Productivity

We have a good measure of the degree of inter-district choice in a metropolitan area: C_m, defined above. We are concerned, however, that the inter-district choice available is endogenous to the conduct of local public schools–in particular, districts consolidate with productive districts but secede from unproductive districts. To obtain unbiased estimates, we need geographic or historical factors that increase a metropolitan area's tendency to contain many independent districts but that have no direct effect on contemporary public school conduct. As explained in Hoxby [2000a], streams and rivers provide good instruments because, early in American history, they were natural barriers that influenced the drawing of district boundaries. They increased students' travel time to school, causing school districts to be drawn smaller initially.[17] They probably have no direct effect on how schools conduct themselves now.

[16] See Hoxby [2000c] for more on this point.

[17] This typically took place about the time of Anglo-American settlement, which varies with the area of the country. Many of the original petitions for district boundaries cite streams as a reason for not

Formally, the set of instruments for C_m is a vector of variables that measure the number of larger and smaller streams in a metropolitan area. I estimate the effects of inter-district choice using a regression in which the dependent variable is productivity (achievement divided by the natural log of per-pupil spending). The log of per pupil spending is used in the denominator of productivity so that when we compare productivity at two schools, we are comparing the change in achievement for a given *percentage* change in per pupil spending. It is best to use the percentage change in spending when comparing schools across areas with a variety of costs of living. The key independent variable is the choice index (instrumented). The key variation in the regression is at the metropolitan area level, but I am able to control for a wide range of background variables that might also influence schools or students. For instance, I control for the effect of household income, parents' educational attainment, family size, family composition (single-parent households), race, region, metropolitan area size, and the local population's income, racial composition, poverty, educational attainment, and urbanness. Because I have good measures of racial, ethnic, and income segregation by school and school district, I can even control for self-segregation (which may be increased by inter-district choice).

The principal results of this regression are shown in Table III, which displays only the coefficients of interest, not the coefficients on control variables. The estimates show that inter-district choice has a positive, statistically significant effect on productivity. For instance, a metropolitan area with maximum inter-district choice (index approximately equal to one) has school productivity that is 0.308 higher than a metropolitan area with zero inter-district choice, if we base the measure of productivity on tenth grade math scores. This means that, if we compare two schools, the school in the metropolitan area with maximum choice has math scores that rise by more (0.308 percentile points more) for every 100 percent increase in

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extending the district lines further. Streams are by far the most common natural boundary for school districts. Note, however, that many of the streams that are preserved in boundaries are small and have never had industrial importance. Today, many of the boundary streams are of negligible importance in travel.

per pupil spending than the school in the metropolitan area with minimum choice.

It is somewhat difficult to interpret the effect of inter-district choice on productivity, so it is useful to break the effect down into its two parts: the effect of choice on achievement and the effect of choice on per pupil spending. If one runs a regression with the natural log of per pupil spending as the dependent variable, one finds that schools in metropolitan areas with maximum inter-district choice spend 7.6 percent *less* than schools in areas with minimum inter-district choice.[18] If one runs a regression with achievement as the dependent variable, one obtains the results shown in Table IV. Compared to schools in metropolitan areas with zero inter-district choice, schools in areas with maximum choice have eighth grade reading scores that are 3.8 national percentile points higher, tenth grade math scores that 3.1 national percentile points higher, and twelfth grade reading scores that are 5.8 national percentile points higher. Recall that these higher levels of achievement are achieved with spending that is 7.6 percent lower, and one has a sense of the effect on school productivity.

The Effect of Traditional Private School Choice on School Productivity

Recall that availability of private schooling varies among metropolitan areas in the United States. To estimate the effects of varying private school competition for public schools, we need factors that affect the supply of private schooling, but have no direct effect on achievement. Such factors include historical differences in metropolitan areas' religious composition because religious groups left endowments that today generates differences in the amount of non-tuition revenue enjoyed by private schools. A private school presented by history with a generous endowment can provide a given quality of schooling at a lower tuition (and can thus be more competitive with public schools) than a private school with little or no endowment.

Formally, the set of instruments for the share of enrollment in private schools is a vector of

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[18] The regression is otherwise identical to the regression described above, which generated the estimates shown in Table III.

variables that measure the population densities of nine major religious denominations in 1950. So long as I control for *current* religious composition of metropolitan areas (which might affect the demand for private schooling), these historical religious population densities should only affect the supply of schooling and should have no direct effect on the achievement of public school students.[19] I estimate the effects of private school choice using a regression in which the dependent variable is productivity (achievement divided by the natural log of per-pupil spending) and the key independent variable is the percentage of metropolitan area students in private schools (instrumented). I control for the same background variables that I used for inter-district choice (see above).

The key estimates from this regression are shown in Table V, which displays only the coefficients of interest, not the coefficients on control variables. The table shows that private school choice has a positive, statistically significant effect on *public* schools' productivity. For instance, compare two metropolitan areas, one with a moderately high degree of private school supply (about 17 percent of students in private schools) and the other with a moderately low degree of private school supply (about 7 percent of students in private schools). The difference between moderately high and low private school choice is, thus, a 10 percentage point difference in the share of students in private schools. This means that we can interpret the first coefficient shown in the table as follows. A *public* school in the metropolitan area with moderately high private school choice has eighth grade reading scores that rise by more ($0.27=0.027\times10$ percentile points more) for every 100 percent increase in per pupil spending than a *public* school in the metropolitan area with moderately low private school choice.

Again, it is useful to separate the effect of choice on productivity into its two parts: the effect on achievement and the effect on per pupil spending. If one runs a regression with the natural log of per pupil spending as the dependent variable, one finds that a one percentage point increase in the supply of private

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[19] See Hoxby [2000b] for further comment on this point.

school choice (as measure by the share of metropolitan area enrollment in private schools) *raises* per pupil spending in public schools by 0.5 percent. That is, traditional private school choice has almost no effect on public school spending. This is probably because of offsetting effects. Increased availability of private school choice draws some students away from the public schools, raising per pupil spending through the reduction in the number of pupils served but lowering per pupil spending through the reduction in voters who will support higher public school spending.

In any case, nearly all of the effect of private school choice on productivity occurs through its effect on achievement. Table VI shows these achievement effects. Compared to public schools in metropolitan areas with moderately low private school choice, schools in areas with moderately high private school choice have eighth grade reading scores that are 2.7 national percentile points higher, eighth math scores that are 2.5 national percentile points higher, twelfth grade reading scores that are 3.4 national percentile points higher, and twelfth grade math scores that are 3.7 national percentile points higher. (To get these numbers, multiply the coefficients in Table VI by the 10 percentage point difference between a moderately high and low degree of private school choice.) Recall that these higher levels of public school achievement are achieved with spending that is approximately the same, and one has a sense of the effect on productivity.

<u>Discussion of the Effects of Traditional Forms of School Choice</u>

Are the effects of traditional choice on productivity large or small? One way to answer this question is to ask how much higher American school productivity would be if every school were to experience a high level of inter-district choice and private school choice, as opposed to zero inter-district choice and moderately low private school choice. There would be a 28 percent improvement in American school productivity, based on the estimates described above. 28 percent is close to half of the decline in American school productivity since 1970.

One should keep in mind, however, that both traditional forms of choice provide rather weak incentives compared to choice reforms like vouchers and charter schools. Moreover, the cost of exercising the traditional forms of choice can be quite high and is certainly prohibitive high for truly poor families.

VI. The Effect of Recent Choice Reforms on School Productivity

As mentioned above, recent choice reforms can only partially answer our questions about how competition affects productivity. The recent vintage of most reforms means that we are unlikely to witness major changes in the supply of schools. Also, short-term reactions to choice can differ from long-term reactions. For instance, consider a regular public school that has had low productivity for years and that has become the target of voucher or charter school competition. Under pressure, the school might make dramatic productivity gains in the short run. The principal might quickly eliminate unsuccessful instructional programs or personnel. She might quickly reallocate resources towards core instructional programs in reading, language, math, history, and science. The rate of productivity increase might, however, slow after the first few years as good policy changes become less obvious. On the other hand, even a school that is raising its productivity might appear to have productivity losses in the short-run if it faces adjustment costs when it makes changes. For instance, a school that puts an academic monitoring system in place may face short-run costs for computers and training.

Can we learn much, then, from recent choice reforms? The answer is "yes" if we follow a few principles. First, although it is interesting to examine the productivity of the choice schools themselves (as Peterson implicitly does in this volume), it is even more important to study the productivity reactions of regular public schools that are newly facing competition. This is because the productivity reactions of regular public schools are in much more doubt than the productivity of choice schools. An unproductive choice school is unlikely to enter and even less likely to survive, but critics of school choice doubt whether regular public schools even have the knowledge or tools to raise their productivity. Second, we should focus on the productivity reactions of regular public schools that face non-negligible incentives due to a choice reform. This immediately limits our investigation to a few choice reforms that meet the following requirements: (1) there is a realistic possibility that at least five percent of regular public enrollment could go to choice schools, (2) the regular public schools lose at least some money (not necessarily the entire per-pupil cost) when a student goes to a choice school, and (3) the reform has been in place for a few years. Three reforms that satisfy these basic requirements are school vouchers in Milwaukee, charter schools in Michigan, and charter schools in Arizona. I describe each of these reforms below in the course of examining the reaction to it. Apart from these three reforms, most choice reforms fail to meet at least one of these requirements. In fact, choice reforms are typically characterized by constraints on enrollment (for instance, no more than one percent of local students can attend choice schools) or perverse financial incentives (for instance, the local district loses no money when it loses a student to a choice school).[20]

The Effect of Vouchers on Achievement in Milwaukee Public Schools

Vouchers for poor students in Milwaukee were enacted in 1990 and were first used in the 1990-91 school year. Currently, a family is eligible for a voucher if its income is at or below 175 percent of the federal poverty level (at or below 17,463 dollars for a family of four). For the 1999-00 school year, the voucher amount was 5,106 dollars per student or the private school's cost per student, whichever was less. For every student who leaves the Milwaukee public schools with a voucher, the Milwaukee public schools lose state aid equal to half the voucher amount (up to 2553 dollars per voucher student in 1999-00). Milwaukee's per pupil spending in 1999-00 was 8,752 dollars per pupil, so the district was losing 29 percent of the per pupil revenue associated with a voucher student. Currently, the vouchers may be used at secular and non-secular private schools.[21]

[20] See Rees [2000] for a thorough review of current school choice reforms. In most cases where I have not used materials directly obtained from the relevant state's department of education, I have relied upon Rees for a description of reforms.

[21] The information on the Milwaukee program and Wisconsin schools is obtained from several publications of the Wisconsin Department of Instruction [all 2000].

The voucher program had a difficult start. While approximately 67,000 students were initially eligible for vouchers, participation was initially limited to only 1 percent of Milwaukee enrollment. In 1993, the limit was raised to 1.5 percent and, in 1998, to 15 percent of enrollment. The 1998 changes followed a prolonged legal dispute in which most voucher students had to use privately donated, not publicly funded, vouchers. For instance, in 1997-98, only 1,500 students (about 1.4 percent of Milwaukee students) were able to use publicly funded vouchers. Also, until 1998, the future of the program was very much in doubt. Its future is still somewhat in doubt pending possible review by the United States Supreme Court. Overall, while the voucher program began in 1990 and might have been expected to have had a small impact on the Milwaukee Public Schools beginning with the 1990-91 school year, the program generated very little potential competition until the 1998-99 school year. However, because the program was already somewhat established and familiar to Milwaukee residents by 1998, one would expect a quicker response to the program than one would expect for a completely new program. In short, it is plausible to look for a productivity impact, if any, over the few most recent school years. The 1996-97 school year effectively predates serious competition.

Not all schools in Milwaukee experienced the same increase in competition as the result of the voucher program. The greater was a school's share of poor children, the greater was the potential competition because the greater was the potential loss of students (after 1998). Some Milwaukee schools had as few as 25 percent of their schools eligible for vouchers, while other Milwaukee schools had as many as 96 percent eligible. Also, because private elementary schools cost significantly less than private high schools, more than 90 percent of vouchers were used by students in grades one through seven in 1999-00. Thus, only elementary schools in Milwaukee faced significant potential competition.

These facts about the voucher program suggest that the following type of evaluation is most appropriate for examining the productivity response of Milwaukee public schools. First, one should focus on the productivity of Milwaukee schools in grades one through seven. Second, schools' productivity should be compared from 1996-97 (before significant competition) to 1999-00 (after significant competition). Third, schools in Milwaukee can be separated into those that were "more treated" by competition because a large number of students were eligible and those that were "less treated." More treated schools are likely to have responded more strongly to the program. We can think of the less treated schools in Milwaukee as partial control group. but *all* schools in Milwaukee were eligible for non-negligible treatment. Therefore, it is desirable to have a control group of schools from Wisconsin that were truly unaffected by the voucher program. It turns out that it is not easy to find such schools in Wisconsin because Milwaukee's schools are much poorer and have much larger shares of black and Hispanic students than most other schools in the state. I chose the most similar schools available for the evaluation, but it is likely that the results will understate the productivity effects of school competition. We expect understatement because schools that have fewer poor and minority students typically enjoy greater productivity and higher productivity *growth* than schools with more poor and minority students. Thus, the control schools, which are richer than the treated schools, would probably have higher productivity growth (all else equal) than the treated group of schools. Also, the less treated schools in Milwaukee would probably have higher productivity growth (all else equal) than the more treated schools.[22]

Because my evaluation compares *treated and control* schools *before and after* 1998, it is what is sometimes called a difference-in-differences evaluation. It has a fairly obvious analog in scientific experiments.

Table VII shows some demographic indicators for the three groups of elementary schools: most

[22] It is fairly obvious that better-off schools will have better productivity if one does not control for demographic differences among students. It is less obvious that better-off schools will also have better productivity growth, but they do in fact. For instance, prior to 1996, Wisconsin elementary students took statewide tests in reading (only). In the pre-voucher period, productivity growth was negative (based on these reading tests) in the schools that were later to become most treated and somewhat treated. In contrast, productivity growth (based on reading tests) was positive in the schools that form the untreated comparison group.

treated (Milwaukee schools where at least two-thirds of students were eligible for vouchers), somewhat treated (Milwaukee schools where less than two-thirds of students were eligible for vouchers, and untreated comparison schools. Note that 30 percent was the minimum share of students eligible for vouchers among the somewhat treated Milwaukee elementary schools. There are 32 most treated and 66 somewhat treated elementary schools. All of the Milwaukee elementary schools have enrollment of about 71-72 students in a grade.

In the most treated schools, an average of 81.3 percent of students were eligible for free or reduced-price lunches (and thus eligible for vouchers), 65.4 percent of students were black, and 2.9 percent of students were Hispanic. In the somewhat treated schools, an average of 44.5 percent of students were eligible for vouchers, 49.1 percent of students were black, and 13.7 percent of students were Hispanic.[23]

I included a Wisconsin elementary in the untreated comparison group if it (1) was not in Milwaukee, (2) was urban, (3) had at least 25 percent of its students eligible for free or reduced-price lunch, and (4) had black students compose at least 15 percent of its students. There were only 12 schools in Wisconsin that met these criteria. It was not possible to choose a group of untreated schools that were more closely matched to Milwaukee schools. In the untreated comparison schools, average enrollment in a grade was 51 students, 30.4 percent of students were eligible for free or reduced-price lunch (and, thus, would have been eligible for vouchers had they lived in Milwaukee), 30.3 percent of the students were black, and 3.0 percent of students were Hispanic.

Students in Wisconsin take state-wide examinations in grades 4, 8, and 10. Because I am necessarily focusing on the productivity reactions of elementary schools, I measure productivity by dividing a school's fourth grade score (expressed in national percentile points) by its per pupil spending in

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[23] Note that all of these demographic numbers reflect what the schools looked like in 1990, *before* the voucher program was enacted. This is the correct method for choosing treated and control schools. One does not want to measure the extent of treatment using measures of student composition that potentially reflect how students reacted to the voucher program.

thousands of real (1999) dollars. Achievement is measured on five tests: mathematics, science, social studies, language, and reading. It is worth noting that, during the period in question, Wisconsin enacted a controversial new reading curriculum that emphasized whole-language methods, as opposed to phonics.

Table VIII shows productivity growth rates in most treated, somewhat treated, and untreated comparison schools in Wisconsin between 1996-97 and 1999-00. The statistics in the table are based on regressions in which the dependent variable is productivity and the independent variables are an indicator for each school, a time trend for most treated schools, a time trend for somewhat treated schools, and a time trend for untreated comparison schools. This regression incorporates the best differences-in-differences method, given the application, because it allows *each* school to have its own starting point for productivity. Intuitively, the regression is based on the idea that productivity growth rates might look like one of the following figures.

Figures 1 and 2 show what productivity might look like in nine schools, three of which are most treated, three of which are somewhat treated, and three of which are untreated. It would be fairly typical to find that the most treated schools had the lowest initial productivity if we did not correct for differences in student demographics, for the simple reason that poorer students tend to have lower achievement and the most treated schools have more poor students. Thus, both figures show the most treated schools generally having the lowest initial productivity, somewhat treated schools generally having medium initial productivity, and untreated schools generally having the highest initial productivity. Now, if competition made schools raise their productivity, then the time trends might look like those in Figure 1. The most treated schools raise their productivity most quickly, somewhat treated schools raise their productivity less quickly, and untreated schools raise their productivity least quickly (or perhaps not at all). If competition had little or no effect on productivity, then all the schools might have the same time trend (same slope to their productivity line) or the time trends might look like those in Figure 2, where the untreated schools have the fastest productivity growth. It would not be surprising to see trends like those in Figure 2 if competition does not affect productivity because richer schools often have better productivity growth all else equal. Recall that this tendency may generate underestimates of the effect of competition on productivity.

Formally, the regression equation can be written as follows:

$$\frac{Ach_{it}}{PPExp_{it}} = \alpha_1 I_1 + \dots + \alpha_N I_N +$$

$$\beta^{most\ treated} I_i^{most\ treated}\, time_t + \beta^{somewhat\ treated} I_i^{somewhat\ treated}\, time_t + \beta^{untreated} I_i^{untreated}\, time_t + \epsilon_{it}$$

where Ach_{it} is a national percentile rank score for students at school i in year t, $PPExp_{it}$ is the per pupil expenditure at school i in year t, I_1 through I_N are indicator variables for schools, $_1$ through $_N$ are initial productivity levels at individual schools, $I^{most\ treated}$ is an indicator variable for the school being most treated, $I^{somewhat\ treated}$ is an indicator variable for the school being somewhat treated, $I^{untreated}$ is an indicator variable for the school being untreated, and $time_t$ is the school year. The coefficients $^{most\ treated}$, $^{somewhat\ treated}$, and ^{untreated} pick up the different productivity growth rates for most treated, somewhat treated, and untreated schools, respectively.

The left-hand column of Table VIII shows that, based on mathematics achievement, productivity grew annually by about 0.7 national percentile points per thousand dollars between 1996-97 and 1999-00 in the most treated schools. It grew more slowly in somewhat treated schools (about 0.5 national percentile points per thousand dollars) and yet more slowly in untreated schools (about 0.3 national percentile points per thousands dollars). Productivity growth based on science, social studies, and language (grammar) is shown in the next three columns, all of which show patterns that are similar to the mathematics-based pattern. In all these columns, productivity growth in the most treated schools is statistically significantly different than that in the untreated schools with a 95 percent confidence level. Reading-based measures of productivity are falling in all the schools over the period in question, perhaps because of whole language methods. However, reading-based productivity is falling least quickly in schools that were most treated to voucher school competition.

Table IX shows statistics that are very similar to those in Table VIII. They are easier to interpret for those unfamiliar with regression analysis, but they are less ideal because each school does not have its own initial level of productivity. For instance, examine the top panel, which shows productivity calculations based on the mathematics exam. In 1996-97, the most treated schools earned 4.18 national percentile points for every thousand dollars of per pupil spending. In the same year, the somewhat treated and untreated schools earned 4.08 and 5.65 national percentile points (respectively) for every thousand dollars. Over the next few years, however, productivity growth was the highest in the most treated schools, second highest in the somewhat treated schools, and lowest in the untreated schools (see right-hand column). In fact, by 1999-00, productivity in the most treated schools was closer to that of the untreated schools than it was to that of the somewhat treated schools! The productivity growth rates shown in Table IX are dramatic for the most treated schools. The basic pattern (highest productivity growth in the most treated schools) is repeated in the other panels of the table, for the science, social studies, language, and reading examinations.

Tables X and XI are very much like Tables VIII and IX, except that they show achievement growth instead of productivity growth. That is, they leave out the changes in productivity that come about as a result of changes in per pupil spending. An examination of them shows that achievement growth displays patterns like that of productivity growth, which suggests that the improvements in productivity in the most treated and somewhat treated schools occurred because achievement was rising in those schools, not because achievement was holding steady while per pupil spending fell. (Indeed, use of the vouchers causes per pupil spending to rise in the Milwaukee public schools, so if achievement were to hold steady, productivity would fall if schools did not respond to competition by raising it.)

Look, for example, at Table X. It shows that math scores rose by about 7 percentile points per year in the most treated schools, by about 5 percentile points per year in somewhat treated schools, and by about 4 percentile point in untreated schools. Alternatively, examine Table XI. It shows that social studies scores in the most treated schools rose by 4.2 percentile points per year, while social studies scores in untreated schools rose by only 1.5 percentile points per year.

Overall, an evaluation of Milwaukee suggests that public schools have a strong, positive productivity response to competition from vouchers. The schools that faced the most potential competition from vouchers had the best productivity response. In fact, the schools that were most treated to competition had dramatic productivity improvements. On the one hand, such bursts of productivity growth may slow down after a few more years of competition. On the other hand, the productivity effects of competition may be understated because the control group of schools was a slightly unfair comparison group with fewer poor and minority students.

The Effect of Charter Schools on Achievement in Michigan Public Schools

In 1994, Michigan enacted a charter school law as part of a series of changes in its method of financing schools. Michigan charter schools receive a per pupil fee that is essentially the same as the state’s foundation level of per pupil spending (the state’s minimum level of per pupil spending, given the characteristics of the school’s student population). For instance, in 1999-00, the average charter school student in Michigan had 6,600 dollars spent on his education, while the average regular public school student had about 7,440 dollars spent on his education. Detroit public schools spent 8,325 dollars per pupil and the average charter school student in Detroit had about 6,590 dollars spent on his education. A district that loses a student to a charter school loses approximately the foundation level of per pupil revenue. Charter competition tends to be most substantial in the elementary grades because the charter fees more adequately cover costs for the lower grades. By the 1999-00 school year, approximately 3.5 percent of all non-private elementary students in Michigan were enrolled in charter schools. The corresponding number for secondary students was 0.7 percent. Charter schools can receive their charters from state-wide organizations, such as universities, so they can compete with local public schools, unlike charter schools in many other states that have their charters granted and renewed by their local district.[24]

Evaluating the effect of charter school competition on Michigan public schools is slightly more difficult than evaluating the effect of Milwaukee's vouchers. This is because it was relatively easy to define *ex ante* the treatment and control schools in Wisconsin: no school outside of Milwaukee received any voucher treatment and the scale of treatment within Milwaukee schools varied with students' poverty, a variable that we observe. In Michigan, it is misleading simply to compare public schools that did and did not face much charter school competition. Only some Michigan districts actually had to face much competition from charter schools, and the districts that did were not randomly selected or pre-selected according to a rule. Instead, charter schools formed as a response to local opportunities. Charter schools were more likely to form in districts where parents were frustrated, teachers were frustrated, or the local district spent money inefficiently (because the charter school fee was more likely to finance a comparable environment there). That is, charter schools were likely to form in districts that had unproductive regular public schools. Thus, if we compare, at a point in time, public schools that were targeted and not targeted by charter school competition, we are essentially comparing public schools that have a history of being less productive and more productive. Such a comparison would clearly generate biased estimates of the effect of charter schools on public school productivity. A good evaluation strategy needs to recognize (a) that public schools targeted by charter school competition were likely to be less productive initially and (b) that the best control group for schools treated to charter competition may be those schools themselves *before* they were hit with charter competition. I therefore adopt the strategy of looking for *changes* in regular public schools' productivity that are associated with critical levels of charter school competition for their

[24] The information on Michigan charter schools and all the data on Michigan schools are taken from publications of the Michigan Department of Educaton [all 2000].

students.

For instance, one might look for patterns like those shown in Figures 3a through 3c. In Figure 3a, there is a district that initially enjoys strong positive productivity growth (solid line). After 1994, this district faces only very minor charter school competition. The dashed line represents charter school enrollment as a percentage of local enrollment. It never exceeds more than one half of one percent of local enrollment. Perhaps only a “niche” charter school, like one with training for a particular vocation, could survive in this productive district. The district’s productivity is unaffected by the charter school law, which does not force it to face much meaningful competition.

Figure 3b represents a district that initially has no productivity growth. Immediately after the passage of the charter law in 1994, the district faces only minor charter school competition and does not respond much. However, when charter schools begin to take a critical share of local students, such that public school administrators actually feel a bite in their budgets, the public schools respond by increasing their productivity to fight off competition. Observe in the figure how productivity growth increases when charter enrollment reaches the critical percentage of local enrollment.

Figure 3c is similar to Figure 3b except that it illustrates the opposite possible response: the public school responds to competition by giving up. Its productivity growth falls when local charter enrollment reaches the critical level where administrators notice the bite from their budget. This is a scenario under which public school administrators get discouraged or start spending their time on political opposition to charter schools (rather than instruction).

Note well that in comparing schools like those in Figures 3a through 3c, I compared only productivity *growth* and looked to see whether a school’s own productivity *growth* (the slope) changed when charter competition reached the critical level. It is difficult to compare the measured productivity of schools that have very different demographics because good student inputs can make a school appear to be more productive than it really is (and poor student inputs have the opposite effect). One can more safely compare a school to itself.

Of course, an empirical researcher does not know what the critical percentage (of local enrollment) might be. It is likely to be greater than one or two percent because the mean year-to-year change in a Michigan school's enrollment prior to 1994 was 5.1 percent. Therefore, a very small drawing away of enrollment by a local charter school would be hard to differentiate from normal year-to-year variation in enrollment. However, a persistent drawing away of enrollment of more than 5 percent, say, would be likely to be noticed and attributed to charter schools. I initially looked for a critical level of 6 percent and, because it worked well, I kept it. A critical level of 7 or 8 percent works quite similarly, but it is hard to choose a critical level higher than 8 percent because so few districts in Michigan ever face that much competition.

Table XII shows which Michigan districts have had as much as six percent of local enrollment entering charter schools (left hand column) or between three and six percent of local enrollment entering charter schools (right hand column). There are 597 districts in Michigan and only 35 shown on the table, so a non-negligible charter school presence is still the exception and not the rule. Michigan's largest city districts are over-represented among the districts that face charter school competition: Pontiac, Grand Rapids, Lansing, Flint, Detroit, Livonia, Dearborn, and Kalamazoo all have at least six percent of enrollment in charter schools. The lack of such large cities among the districts that face no or negligible charter competition is yet another reason for comparing districts to their own pre-competition selves (rather than comparing districts with and without a charter presence at a point in time).

I use regression to carry out the analysis represented by Figures 3a through 3c. The dependent variable in the regression is a school's productivity *growth* in a school year. In particular, it is the change in a school's scale score divided by the percentage change in a school's per pupil spending.[25] Thus, if a

[25] I divide the change in achievement by the percentage change in per pupil spending because, unlike in Wisconsin, the regression includes schools with a variety of different demographics and spending

school's scale scores rose by one point for every two percent increase in its per pupil spending, then its productivity growth would be measured to be 0.5. Of course, productivity growth rates can be negative as well as positive. The regression allows districts that were never targeted by charter competition to have different *preexisting* productivity growth rates than districts that were eventually targeted. The regression also allows districts that were eventually targeted to change their productivity growth at two times: when they face any charter competition (a charter school enrolls at least 0.1 percent of local enrollment) and when charter competition reaches a critical level (initially picked to be 6 percent). The results of this exercise are shown in Table XIII. Formally, the regression used is:

$$\frac{Change\ Ach_{ijt}}{\%\ Change\ PPExp_{ijt}} = \delta^{not\ targeted} I_{ij}^{not\ targeted} + \delta^{eventually\ targeted} I_{j}^{eventually\ targeted} + \delta^{charter\ entry} I_{jt}^{charter \geq 0.1\%} + \delta^{critical\ charter\ competition} I_{jt}^{charter \geq 6\%} + \epsilon_{ijt}$$

where $Change\ Ach_{it}$ is the change in scale scores for school i in district j in year t, $\%\ Change\ PPExp_{it}$ is the percentage change in per pupil expenditure for the same school, $I^{not\ targeted}$ is an indicator variable for the school being in a district that is not targeted by charter competition, $I^{eventually\ targeted}$ is an indicator variable for the district's being eventually targeted, $I^{charter \geq 0.1\%}$ is an indicator variable for the district's having at least 0.1 percent of enrollment in charter schools, and $I^{charter \geq 6\%}$ is an indicator variable for the district's having at least 6 percent of enrollment in charter schools. The coefficients ${}^{not\ targeted}$, ${}^{eventually\ targeted}$, ${}^{charter\ entry}$, and ${}^{critical\ charter}$ pick up the different productivity growth rates for schools that are not targeted,

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levels. It is believed that some students are more expensive to educate than others. An absolute change (such a 500 dollar change) in per pupil spending would not allow for differences in the cost of raising a student's scores depending on his demographics. For instance, 500 dollars would mean less to a school that had had to spend 8,000 dollars to reach a certain level of achievement than to a school that had had to spend only 6,000 dollars to reach the same level of achievement.

eventually targeted, facing charter entry, and facing a critical level of charter competition.

Table XIII shows productivity calculations based on Michigan's statewide math and reading exams, which are administered in the fourth and seventh grades. Looking at the first and second rows of Table XIII, one immediately observes that initial productivity growth rates in Michigan were all close to zero. In fact, none of the productivity growth rates for nontargeted schools or the initial productivity growth rates for targeted schools are statistically different from zero at a conventional confidence level. However, the point estimates consistently suggest that productivity growth was slightly higher in nontargeted schools than in schools that were eventually targeted. For instance, the growth rate based on fourth grade reading score was 1.48 (scale points per 1 percent increase in per-pupil spending) among nontargeted schools and 0.42 among schools that were eventually targeted. Across the board, there appears to be little or no immediate response to the mere entry of charter schools: none of the productivity growth rates change enough for the change to be statistically significantly different from zero. However, the point estimates consistently suggest that productivity growth rates may fall very slightly as an immediate response to charter entry. This may be because the departure of students for charter schools mechanically raises per pupil spending in the local public schools (because the local public schools lose less than their per pupil spending to the charter school). Thus, productivity growth would mechanically fall if public schools had no immediate response to charter entry but did experience the automatic increase in their per pupil spending.

The last row of Table XIII shows its most important results: when a regular public schools faces charter competition at or above six percent of local enrollment, its productivity rises. The productivity measures based on fourth grade exams show particularly dramatic increases over the schools' initial productivity growth. For instance, according to the fourth grade reading exam, initial productivity growth among eventually targeted schools was 0.42, but productivity growth rose to 6.30 after the targeted schools began to face the critical level of competition. According to the fourth grade math exam, initial productivity growth among eventually targeted schools was 1.43, but productivity growth rose to 2.85 after the targeted schools began to face the critical level of competition. These differences are significantly different from zero with 95 percent confidence.

Productivity measures based on the seventh grade exams show the same basic patterns, but the changes are less dramatic. This may be because charter competition is weaker at the middle school level than it is among the lower grades. For instance, according to the seventh grade reading exam, initial productivity growth among eventually targeted schools was 0.49, but productivity growth rose to 1.61 after the targeted schools began to face the critical level of competition.

Table XIV is very much like Table XIII, except that it shows achievement growth instead of productivity growth. That is, it leaves out the changes in productivity that come about as a result of changes in per pupil spending. Table XIV shows that achievement growth displays patterns like that of productivity growth, which suggests that Michigan's regular public schools raised their productivity by raising their achievement (for a given level of per pupil spending), rather than by maintaining a steady level of achievement and cutting their per pupil spending. For instance, the left hand column of Table XIV shows that fourth grade reading scores were rising by 3.19 scale points per year in nontargeted schools. In contrast, they were initially rising by only 2.15 scale points in schools that were to become targets of charter school competition. (The difference between 3.19 and 2.15 is statistically significant with 95 percent confidence.) Targeted schools' immediate response to charter school entry may have been a slight decrease in achievement growth (from 2.15 to 1.98 points per year), but the difference is too small to be statistically significant. However, their response to charter competition above the critical level (6 percent of enrollment) was a dramatic increase in achievement growth. Targeted schools fourth grade reading scores began rising by 6.42 scale points per year. In other words, targeted schools' achievement began to catch up with that of more privileged public schools. This burst of achievement growth is large, but not unrealistically large, particularly when one considers that targeted districts are making up for years of slower achievement growth. If there were more years of Michigan data after the charter reform, one might see targeted schools catching up for several years and then settling into a new rate of achievement growth that is lower than 6.42 points per year but higher than their pre-reform growth rate of 2.15 points per year. Note that, even with a growth rate of 6.42 points per year, a district like Detroit would take approximately 8 years to catch up with the achievement of one of its affluent suburbs, like Grosse Point. (Of course, it is possible that, as Detroit caught up, a suburb like Grosse Point would feel competitive pressure to increase its own rate of achievement growth. This would lengthen the catch up period but further raise Michigan students' scores.)

Fourth grade math, seventh grade reading, and seventh grade math scores show a pattern of response to charter competition that is similar to, though less dramatic than, the pattern for fourth grade reading (just described). For instance, seventh grade reading scores rose by 1.61 scale points per year in nontargeted schools. In contrast, they were initially rising by only 0.99 scale points in schools that were to become targets of charter school competition. Targeted schools' immediate response to charter school entry was negligible: a change from 0.99 to 0.89 points per year (not statistically significant). In response to charter competition above the critical level of six percent of enrollment, targeted schools' fourth grade reading scores began rising by 3.68 scale points per year.

Overall, the picture that one draws from Michigan is the following. Charter competition focused on schools that initially had poor productivity growth. These public schools responded with a burst of productivity growth once charter competition reached a critical level that happens to coincide with the enrollment at which charter competition would be easily discernible from regular fluctuations in enrollment. The increase in productivity was large, especially in fourth grade subjects, perhaps because elementary schools faced the most competition.

The Effect of Charter Schools on Achievement in Arizona Public Schools

Like Michigan, Arizona enacted a charter school law in 1994. Arizona's charter school law is widely regarded as the most favorable to charter schools in the United States, as it allows charter schools to have considerable fiscal and legal autonomy. There are also few constraints on the growth of charter schools in Arizona. As a result, 5.3 percent of Arizona's non-private enrollment was in charter schools in 1999-00. This percentage is the highest of any American state.

In Arizona, state sponsored charter schools get a fee equal to the state's share of revenue (45 percent of total revenue for a regular public school). District sponsored charter schools get a fee equal to local per pupil revenue, but are less able to compete with the regular public schools because they must seek renewal of their charters from the very districts with which they compete.

My evaluation of Arizona follows the same strategy as I employ for Michigan, so I will merely highlight a few differences between the Michigan and Arizona situations here. In Arizona, a municipality may contain multiple districts: for instance, a few elementary districts, a middle school district, and a high school district. A local charter school may therefore be competing with multiple districts. Therefore, I associate regular public schools and charter schools with a municipality, not a district. All Arizona fourth and seventh graders were required to take the Iowa Test of Basic Skills (ITBS) through 1995-96 and have been required to take the Stanford 9 test since then. The shift in the test does not pose problems for the analysis because both tests offer national percentile rank scores (which have a 0.97 correlation at the school level). Moreover, all of the schools switched tests in the same year, so it is simple to establish each school's pre-reform trend and post-reform trend allowing for a state-wide shift in the intercept (with a separate shift allowed for each ITBS national percentile rank). [26]

In Arizona, charter schools entered disproportionately in two types of markets: large urban municipalities (as in Michigan) and municipalities with new housing developments, where charter schools grew up side-by-side with new public schools. In both Michigan and Arizona, charter competition

[26] The information on Arizona charter schools and all the data on Arizona schools are taken from publications of the Arizona Department of Education [1988 through 1995, various 2000].

apparently gravitated toward large urban municipalities because there were frustrated parents and teachers there and because the public schools' low productivity allowed charter schools to provide comparable environments with fees below local per pupil revenue. The growth of charter schools in new housing developments was quite different. In these areas, parents had little relationship with existing public schools because they (the parents) were new or because the public schools were just being built. In other words, parents had to send their child to an unproven school whether or not they sent him to a charter school. Some parents simply preferred charter schools, so that charter schools and public schools grew up side by side. Thus, in municipalities dominated by new housing, there should be no presumption that the local public schools initially had particularly low productivity or frustrated parents. It is difficult to learn much from these municipalities, both because local enrollment is small (so that achievement is measured imprecisely) and because the composition of the local population is changing rapidly (so that pre-1994 achievement is not a good control for post-1994 achievement). In short, although I do not exclude such municipalities from the analysis, one should expect to learn little from them, despite the fact that they sometimes have large percentages of local enrollment in charter schools.

Table XV shows the Arizona municipalities that had between three and six percent of local enrollment in charter schools (left hand column), six to ten percent of local enrollment in charter schools (middle column), and more than 10 percent of local enrollment in charter schools (right hand column). Arizona's largest cities are over-represented among the municipalities listed in the table. Phoenix, Tempe, and Gilbert all have more than six percent of local enrollment in charter schools. Mesa, Tucson, Scottsdale, Chandler, and Glendale all have between three and six percent of local enrollment in charter schools. The municipalities with more than ten percent of local enrollment in charter are all small and fast-growing. Their mean one-grade enrollment (including charter school students) was 150 students in 1999-00, but was only 116 students in 1992-93: that is, their student populations grew 29 percent in seven years. It should be fairly clear that these municipalities will not be helpful in the analysis because their pre-reform student populations are not a good control for their post-reform student populations. In fact, we shall see that these small, fast growing municipalities generate estimates that are very imprecise.

Table XVI shows productivity calculations based on Arizona's statewide math and reading exams, for which I have fourth and seventh grade scores from 1988-89 onwards. Looking at the first and second rows of Table XVI, one sees that initial productivity growth rates were lower among schools that were eventually targeted by charter competition. In fact, the pre-existing productivity growth rate based on fourth grade reading was negative in these schools and that based on seventh grade reading was not significantly different from zero. The pre-existing productivity growth rates based on math are better, but they are less than a third as high as the productivity growth rates in nontargeted schools. Targeted schools appear to have made little or no immediate response to the mere entry of charter schools: none of the productivity growth rates change enough for the change to be statistically significantly different from zero. The targeted schools do, however, appear to respond to more significant charter school competition and the critical level at which they respond appears to be lower than it is in Michigan. For instance, one row of Table XVI (third from the bottom) shows that when a regular public school faces charter competition between three and six percent of local enrollment, its productivity rises. The new productivity growth rate is 2.99 based on fourth grade reading, 6.93 based on fourth grade math, 2.92 based on seventh grade reading, and 7.51 based on seventh grade math. These growth rates are substantially (and statistically significantly higher) than the schools' initial productivity growth rates. When the targeted schools face even more charter competition (six percent of local enrollment or more), their productivity growth rates are even higher. The rates are 3.74 based on fourth grade reading, 8.01 based on fourth grade math, 5.33 based on seventh grade reading, and 7.57 based on seventh grade math. These are large increases in productivity, but not so large as to be unreasonable, especially when one considers that many of the targeted schools are making up for years of slower productivity growth. (I quantify this statement below when describing the achievement results.) The bottom row of Table XVI shows quite high point estimates for productivity growth rates among schools in small, rapidly growing municipalities that face very substantial charter competition. The standard errors of the estimates are so large, however, that none of the estimates is statistically different from zero.

Table XVII is very much like Table XVI, except that it shows achievement growth instead of productivity growth. That is, it leaves out the changes in productivity that come about as a result of changes in per pupil spending. The achievement growth shown in Table XVII displays patterns much like that of productivity growth, which suggests that Arizona's regular public schools raised their productivity by raising their achievement (for a given level of per pupil spending), rather than by maintaining a steady level of achievement and cutting their per pupil spending. For instance, the left hand column of Table XVII shows that fourth grade reading scores were rising by 0.32 national percentile points per year in nontargeted schools. In contrast, they were initially falling by 0.29 percentile points in schools that were to become targets of charter school competition. (The difference between 0.32 and -0.29 is statistically significant with 95 percent confidence.) Targeted schools did not respond (to a statistically significant degree) to mere charter entry, but when charter competition was between three and six percent of local enrollment, their fourth grade reading scores began rising 1.12 national percentile points per year. When charter competition was even more substantial (6 percent of enrollment), their fourth grade reading scores began rising 1.39 national percentile points per year. These changes in the rate of achievement growth are significant, but not unrealistically large. Even at the top rate of 1.39 national percentile points per year, the average targeted school in the Phoenix metropolitan area would take ten years to catch up with the average nontargeted school in the Phoenix metropolitan area.

Fourth grade math, seventh grade reading, and seventh grade math scores show a pattern of response to charter competition that is similar to the pattern for fourth grade reading. The only real difference in the pattern is that mathematics achievement growth is uniformly higher in Arizona than is reading achievement growth.

In both Tables XVI and XVII, it appears as though Arizona schools respond to charter competition as small as three percent of local enrollment. I have checked and found that Michigan schools do not respond to critical levels much below six percent (a reaction is just barely discernable at five percent). Why are Arizona schools more responsive? It is not because their normal year-to-year variation in enrollment is smaller than that of Michigan: it is about the same (just over five percent). There are two possible reasons why Arizona schools might appear to respond to a lower level of competition. First, Arizona's enrollment data for charter schools appears to suffer from underreporting (unlike Michigan's), so that Arizona charter enrollment may be somewhat understated–four percent of local enrollment is really five percent, say. Second, the growth of Arizona charter schools is less constrained and more supported by the state department of education than that of Michigan charter schools. Thus, an Arizona public school that observes four percent of its enrollment go to charter schools may foresee that five or six percent will go in the next school year if it does not respond quickly.

Overall, the evaluation of Arizona suggests conclusions that broadly similar to those one draws from the Michigan evaluation. Charter competition focused on public schools that initially had poor productivity growth, with the caveat that new housing developments also experienced substantial charter growth. Targeted public schools had a positive productivity response once charter competition reached approximately three percent of local enrollment. The positive productivity response was even higher when charter schools could successfully compete for six to ten percent of local enrollment.

Discussion of the Effects of Recent School Choice Reform

Are the productivity effects of the Milwaukee vouchers, Michigan charter schools, and Arizona charter schools sufficient to make us think that choice could remedy the American school productivity problem? All three forms of choice did boost productivity. If all schools in the United States were to enjoy productivity growth rates like those in Milwaukee's most treated schools, American schools could return to their 1970-71 productivity levels in under a decade. Of course, it is dangerous to extrapolate so much from the short voucher and charter school experiences described in this section. On the one hand, the bursts of productivity growth seen in Milwaukee may settle down to a lower level of growth. On the other hand, many of the long-term, general equilibrium effects of choice are not yet in operation.

In order to get a sense of the magnitude of the productivity effects, without having to extrapolate so much, consider the following alternative question. Is it likely that the productivity effects of Milwaukee's voucher program (the "rising tide") are likely to overwhelm the allocation effects for students who experience the worst possible allocation changes in Milwaukee? We can get a sense of the students who are available in Milwaukee to be reallocated if we examine the very high scoring (top decile) and very low scoring (bottom decile) elementary schools in the city. Such schools score about 32 national percentile points apart on the math exam. Thus, a Milwaukee student's worst case scenario would be to experience a fall of about 32 national percentile points in his peer group. Moreover, let us make the extreme assumption that the student is *very* influenced by his peers so that his scores fall by 32 points. This scenario is truly pessimistic! It is not strictly impossible, but it is so pessimistic that is barely plausible. Nevertheless, if the student enjoys the achievement growth rates that Milwaukee students in the most treated schools are enjoying now, he will "grow out of" the bad allocation effects within 4.5 years. That is, he will be better off for having experienced vouchers within 5 years of the voucher program affecting his schooling.

VII. Conclusions

In this paper, I have presented evidence that suggests that we should care deeply about the productivity effects of school choice, not only because they potentially relieve that tensions generated by the allocation effects of choice but also because American schools are in a productivity crisis. Policies that boost American schools' productivity are sorely needed, if only to return the schools to their 1970 productivity levels.

I have also explained how schools that face choice-driven incentives can be induced to raise their productivity. I presented models of for-profit choice schools, non-profit choice schools, and even regular public schools that just face inter-district choice.

In section V of the paper, I show evidence that traditional forms of choice raise school productivity. I present results for traditional forms of choice because they can have long-term, general equilibrium effects of productivity, such as may arise when schools enter or exit or when a different reward system draws better individuals in teaching. If all schools in the United States experienced high levels of the traditional forms of choice, school productivity might be as much as 28 percent higher than it is today.

In section VI of the paper, I present evidence on three recent choice reforms: vouchers in Milwaukee, charter schools in Michigan, and charter schools in Arizona. In each case, I find that regular public schools boosted their productivity when exposed to competition. In fact, the regular public schools responded to competitive threats that were, if anything, surprisingly small. In each case, the regular public schools increased the growth rates of their productivity by raising achievement, not by lowering spending by while maintaining achievement. This achievement-oriented response may, of course, be related to the nature of the actual reforms. One can summarize the productivity effects of a reform like Milwaukee's voucher program by noting that a student would have better achievement in five years under the voucher program *even if* his peer group plunged in quality and he adopted the lower achievement of his new peer group.

Of course, one must be cautious about extrapolating unduly from recent reforms or from traditional school of choice that only partially mimic choice reforms. Nevertheless, it seems safe to conclude that analyses that ignore the productivity effects of choice are likely to be misleading. Improvements in productivity may be *the* key effects of choice.

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Figure 1

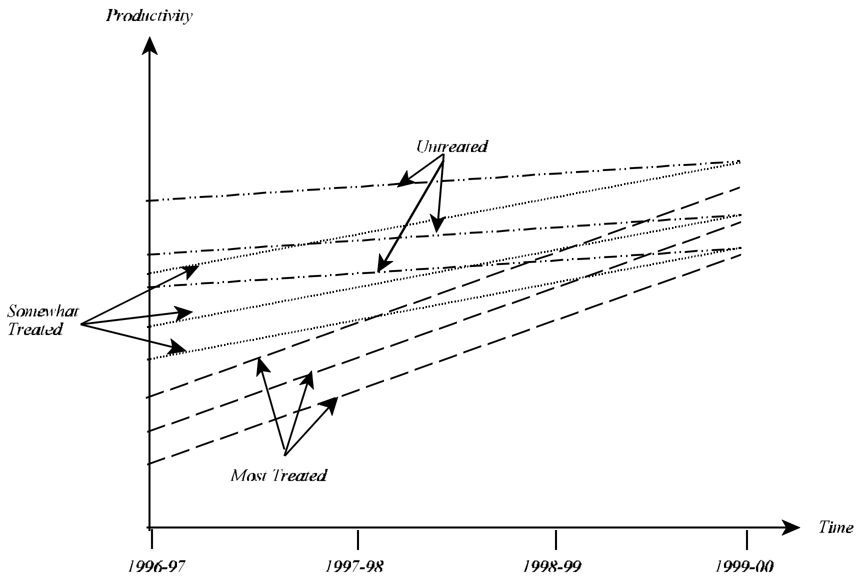

Figure 2

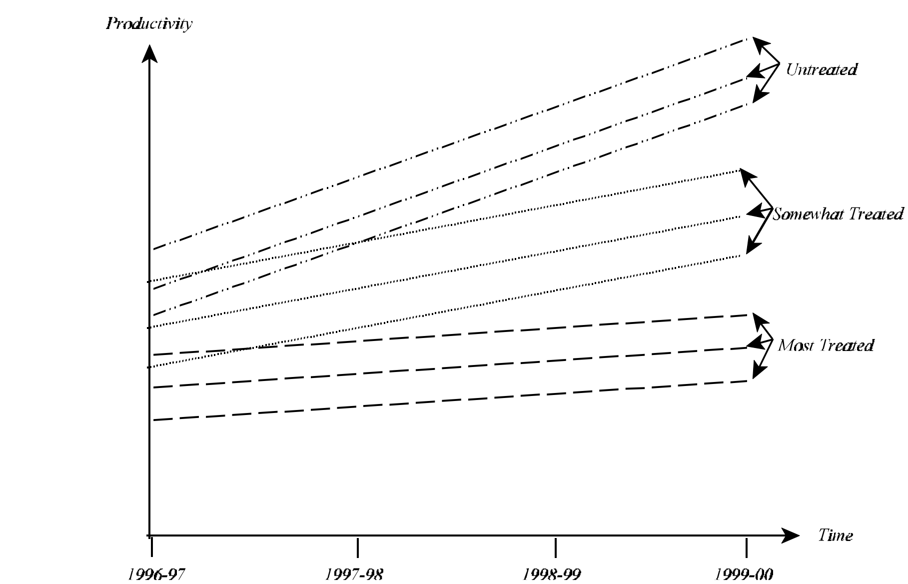

Figure 3a

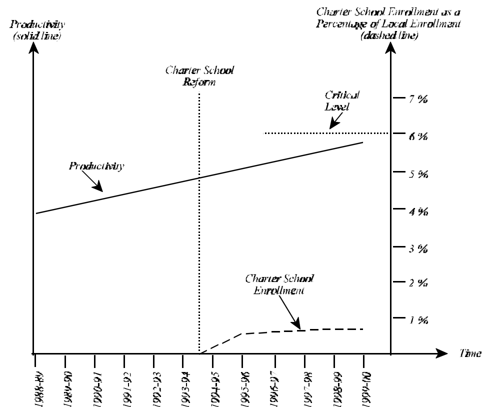

Figure 3b

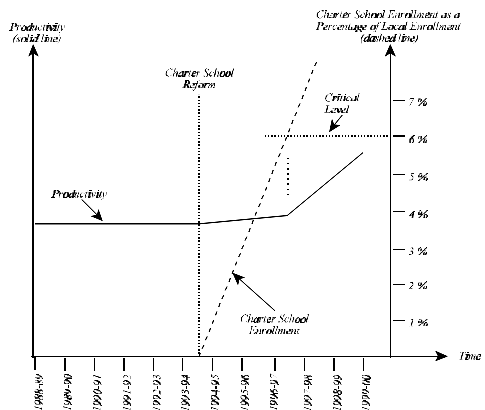

Figure 3c

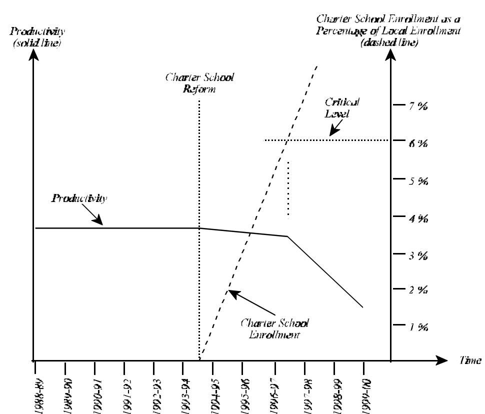

Table I
School Productivity in the United States, 1971 to 1999

	Productivity (NAEP Points Per Thousand Dollars of Per Pupil Spendng) is Based on the Test of					
School Year	Reading for 9 Year Olds	Math for 9 Year Olds	Reading for 13 Year Olds	Math for 13 Year Olds	Reading for 17 Year Olds	Math for 17 Year Olds
1970-71	46.0		56.6		63.3	
1972-73		45.5		55.3		63.2
1974-75	41.7		50.8		56.7	
1977-78		41.3		49.7		56.5
1979-80	41.3		49.6		54.8	
1981-82		40.8		50.1		55.7
1983-84	36.4		44.4		49.9	
1985-86		34.8		42.2		47.4
1987-88	30.5		37.1		41.8	
1989-90	28.9	31.8	35.5	37.3	40.1	42.2
1991-92	29.0	31.7	35.8	37.7	40.0	42.3
1993-94	28.7	31.4	35.1	37.2	39.2	41.6
1995-96	28.1	30.6	34.3	36.3	38.0	40.6
1998-99	26.8	29.4	32.8	35.0	36.5	39.0
Productivity was this Percent Higher in Earliest Year Shown	71.5%	54.9%	72.5%	58.1%	73.4%	62.0%
Actual Mean NAEP Score in 1998-99	212	232	259	276	288	308
What NAEP Score would Have Been in 1998-99 at Productivity of Earliest Year Shown	363	359	447	437	500	499

The source for this table is United States Department of Education *Digest of Education Statistics 1999* and United States Department of Education, *National Assessment of Educational Progress Long-Term Trend Summary Data Tables* [2000]

Table II

Metropolitan Areas with the *Most* Choice Among Public School Districts		Metropolitan Areas with *Very Little* Choice Among Public School Districts	
Metropolitan Area	Choice Index	Metropolitan Area	Choice Index
Albany, NY	0.97	Honolulu, HI*	0
Bergen-Pasaic, NJ	0.97	Miami, FL	0
Boston, MA	0.97	Las Vegas, NV	0
Pittsburgh, PA	0.96	Fort Lauderdale, FL	0
Riverside-San Barnardino, CA*	0.96	Daytona Beach, FL	0
Monmouth-Ocean, NJ	0.96	Fort Myers, FL	0
Minneapolis, MN	0.96	Albuquerque, NM	0
Atlantic City, NJ	0.95	Hagerstown, MD	0
San Francisco, CA*	0.95	Jacksonville, NC	0
St. Louis, MO	0.95	Sarasota, FL	0
Binghamton, NY	0.94	Odessa, TX	0
York, PA	0.94	Cheyenne, WY	0
Scranton, PA	0.94	Lakeland/Winter Haven, FL	0
Johnstown, PA	0.94	Reno, NV	0
San Jose, CA*	0.94	Boca Raton, FL	0
Dayton, OH	0.94	Wilmington, NC	0
Allentown, PA	0.94	Ocala, FL	0
Anaheim-Santa Ana, CA*	0.94	Melbourne/Palm Bay, FL	0
Seattle, WA	0.94	Panama City, FL	0
Rochester, NY	0.94	Bradenton, FL	0
Phoenix, AR	0.94	Portland, OR	0.07
Youngstown, OH	0.94	Midland, TX	0.11

*Hawaii is *one* school district, so that the school district is larger than the metropolitan area of Honolulu. California have school districts that have almost no fiscal independence, so it is somewhat deceptive to describe metropolitan areas like Riverside-San Bernardino, San Francisco, San Jose, and Anaheim-Santa Ana as having significant choice among school districts. The source for this table is United States Department of Education, *School District Data Book*.

Table III
Effect of Traditional Inter-District Choice on Productivity

	dependent variable is a measure of productivity based on:		
	8th grade reading score	10th grade math score	12th grade reading score
Coefficient on the Index of Inter-District Choice	0.290 (0.140)	0.308 (0.158)	0.579 (0.236)
mean of dependent variable	5.917	5.937	5.957
standard deviation of dependent variable	1.177	1.172	1.170

Notes: Each dependent variable is formed by dividing a measure of achievement by the log of per-pupil spending. The notes below the next table apply to this table.

Table IV
Effect of Traditional Inter-District Choice on Achievement

	dependent variable is a measure of achievement:		
	8th grade reading score	10th grade math score	12th grade reading score
Coefficient on the Index of Inter-District Choice	3.818 (1.591)	3.061 (1.494)	5.770 (2.208)

Test scores are measured in national percentile points. The coefficients shown come from instrumental variables estimation of regressions in which the dependent variable is one of the achievement measures shown. The independent variables in the regression include the index of choice (instrumented by a vector of streams variables, see text), several family background variables (household income, gender, race, parents' education), several neighborhood variables (mean household income in district, income inequality in district, racial composition of district, racial and ethnic homogeneity of district, educational attainment of adults in district), and several characteristics of the metropolitan area (population, land area, mean household income, income inequality, racial composition, racial homogeneity, ethnic homogeneity, educational attainment of adults, homogeneity of educational attainment, region of the country). The regressions are weighted by school enrollment. Standard errors are in parentheses and use formulas (Moulton 1986) for data grouped by districts and metropolitan areas.

The main source for this table is Hoxby [2000a]. Observations are metropolitan area students from the National Education Longitudinal Study. The number of observations in each column are: 10,790 (from 211 metropolitan areas), 7,776 (from 211 metropolitan areas), and 6,119 (from 209 metropolitan areas). The number of observations varies due to the availability of the dependent variable. Other data sources are the School District Data Book, Common Core of Data, City and County Data Book, Geographic Names Information System, and United States Geographic Survey.

Table V
Effect of Traditional Private School Choice on Productivity of *Public* Schools

	dependent variable is a measure of productivity based on:			
	8th grade reading score	8th grade math score	12th grade reading score	12th grade math score
Coefficient on the Percentage of Metropolitan Area Students Enrolled in Private Schools	0.027 (0.009)	0.025 (0.009)	0.035 (0.017)	0.038 (0.017)

Notes: Each dependent variable is formed by dividing a measure of achievement by the log of per-pupil spending. See Table III for typical means and standard deviation of the dependent variables.

Table VI
Effect of Traditional Private School Choice on Achievement of *Public* School Students

	dependent variable is a measure of achievement:			
	8th grade reading score	8th grade math score	12th grade reading score	12th grade math score
Coefficient on the Percentage of Metropolitan Area Students Enrolled in Private Schools	0.271 (0.090)	0.249 (0.090)	0.342 (0.172)	0.371 (0.171)

Test scores are measured in national percentile points. The coefficients shown come from instrumental variables estimation of regressions in which the dependent variable is one of the achievement measures shown. The independent variables in the regression include the percentage of metropolitan area student enrolled in private schools (instrumented by a vector of religious composition variables from 1950, see text), several family background variables (household income, gender, race, parents' education), several neighborhood variables (mean household income in district, income inequality in district, racial composition of district, racial and ethnic homogeneity of district, educational attainment of adults in district), and several characteristics of the metropolitan area (population, land area, mean household income, income inequality, racial composition, racial homogeneity, ethnic homogeneity, educational attainment of adults, homogeneity of educational attainment, region of the country). The regressions are weighted by school enrollment. Standard errors are in parentheses and use formulas (Moulton 1986) for data grouped by districts and metropolitan areas.

The main source for this table is Hoxby [2000b]. Observations are metropolitan area students from the National Education Longitudinal Study. Other data sources are the School District Data Book, Common Core of Data, and City and County Data Book.

Table VII
Demographics of Wisconsin's Most Treated, Somewhat Treated, and Untreated Comparison Schools

	Percentage of Students Eligible for Free/ Reduced-Price Lunch	Percentage of Students who are Black	Percentage of Students who are Hispanic
most treated schools	81.3	65.4	2.9
somewhat treated schools	44.5	49.1	13.7
untreated comparison schools	30.4	30.3	3.0

"Most Treated" schools were Milwaukee elementary schools where at least two-thirds of students are eligible for free or reduced price lunches (and thus eligible for vouchers). There are 32 "Most Treated" elementary schools, each of which has an average fourth grade enrollment of 72 students.

"Somewhat Treated" schools were Milwaukee elementary schools where fewer than two-thirds of students are eligible for free or reduced price lunch (and thus eligible for vouchers). In all of these schools, at least 30 percent of students are eligible for free lunch. There are 66 "Somewhat Treated" elementary schools, each of which has an average fourth grade enrollment of 71 students.

The untreated comparison schools are all the Wisconsin elementary schools that:

(1) are urban;
(2) have at least 25 percent of their students eligible for free lunch;
(3) have at least 15 percent of their students being black.

There are 12 untreated comparison elementary schools, each of which has an average fourth grade enrollment of 51 students.

The sources for this table are Wisconsin Department of Public Instruction [various 2000] and United States Department of Education, *School District Data Book*.

Table VIII
Productivity Time Trends in Wisconsin Most Treated, Somewhat Treated, and Untreated Comparison Schools, from Regressions with School Fixed Effects

	Productivity Calculation is Based on Exam in:				
	Math	Science	Social Studies	Language	Reading
Annual Change in Productivity for Most Treated Schools	$0.732^{**\dagger\dagger}$ (0.071)	$0.889^{**\dagger\dagger}$ (0.072)	$0.475^{*\dagger\dagger}$ (0.070)	$0.248^{**\dagger}$ (0.066)	$-0.035^{*\dagger}$ (0.066)
Annual Change in Productivity for Somewhat Treated Schools	0.527 (0.056)	0.729^{**} (0.057)	0.327 (0.055)	0.123^{*} (0.052)	-0.141 (0.052)
Annual Change in Productivity for Untreated Comparison Schools	0.342 (0.172)	0.255 (0.176)	0.188 (0.170)	-0.081 (0.160)	-0.235 (0.162)

** indicates that the time trend for “Most Treated” or “Somewhat Treated” schools is statistically significantly different from the time trend for untreated comparison schools at less than the 0.05 level.

* indicates that the time trend for “Most Treated” or “Somewhat Treated” schools is statistically significantly different from the time trend for untreated comparison schools at the 0.05 to 0.15 level.

†† indicates that the time trend for “Most Treated” is statistically significantly different from the time trend for “Somewhat Treated” schools at less than the 0.05 level.

† indicates that the time trend for “Most Treated” is statistically significantly different from the time trend for “Somewhat Treated” schools at the 0.05 to 0.15 level.

Productivity is measured in national percentile points per thousand dollars of per pupil spending, where per pupil spending is measured in 1999 dollars. The deflator used is the Consumer Price Index. Each regression includes a fixed effect for each school, a time trend for “most treated” schools, a time trend for “somewhat treated” schools, and a time trend for untreated comparison schools. The observations are school level averages based on fourth graders scores, and the regressions are therefore weighted by the schools’ fourth grade enrollment.

The sources for this table are Wisconsin Department of Public Instruction [various 2000] and United States Department of Education, *School District Data Book*.

Table IX
Productivity in Wisconsin's Most Treated, Somewhat Treated, and Untreated Comparison Schools

		1996-97	1999-00	annual change
Productivity Calculation is Based on Math Exam	most treated schools	4.18	6.09	0.64
	somewhat treated schools	4.08	5.50	0.47
	untreated comparison schools	5.65	6.65	0.33
Productivity Calculation is Based on Science Exam	most treated schools	3.87	6.04	0.72
	somewhat treated schools	3.91	5.67	0.59
	untreated comparison schools	6.33	6.92	0.20
Productivity Calculation is Based on Soc Stud Exam	most treated schools	5.05	6.19	0.41
	somewhat treated schools	5.26	5.80	0.18
	untreated comparison schools	6.90	7.21	0.10
Productivity Calculation is Based on Language Exam	most treated schools	5.07	5.64	0.19
	somewhat treated schools	5.07	5.28	0.07
	untreated comparison schools	6.04	5.85	-0.06
Productivity Calculation is Based on Reading Exam	most treated schools	5.35	5.31	-0.01
	somewhat treated schools	5.46	4.98	-0.16
	untreated comparison schools	6.68	6.04	-0.21

Productivity is measured in national percentile points per thousand dollars of per pupil spending, where per pupil spending is measured in 1999 dollars. The deflator used is the Consumer Price Index. Statistics are based on weighted averages over schools in the relevant group, where each school is weighted by its enrollment.

The sources for this table are Wisconsin Department of Public Instruction [various 2000] and United States Department of Education, *School District Data Book*.

Table X
Achievement Growth in Wisconsin's Most Treated, Somewhat Treated, and Untreated Comparison Schools, from Regressions with School Fixed Effects

	Math	Science	Social Studies	Language	Reading
Annual Change in Test Scores for Most Treated Schools	7.06**†† (0.61)	8.39**†† (0.62)	4.97*†† (0.59)	2.98**† (0.56)	0.57*† (0.57)
Annual Change in Test Scores for Somewhat Treated Schools	5.27 (0.48)	6.99** (0.49)	3.68 (0.46)	1.88* (0.44)	-0.37 (0.45)
Annual Change in Test Scores for Untreated Comparison Schools	3.71 (1.48)	2.96 (1.50)	2.40 (1.43)	-0.10 (1.37)	-1.42 (1.38)

** indicates that the time trend for "Most Treated" or "Somewhat Treated" schools is statistically significantly different from the time trend for untreated comparison schools at less than the 0.05 level.

* indicates that the time trend for "Most Treated" or "Somewhat Treated" schools is statistically significantly different from the time trend for untreated comparison schools at the 0.05 to 0.15 level.

†† indicates that the time trend for "Most Treated" is statistically significantly different from the time trend for "Somewhat Treated" schools at less than the 0.05 level.

† indicates that the time trend for "Most Treated" is statistically significantly different from the time trend for "Somewhat Treated" schools at the 0.05 to 0.15 level.

Test scores are measured in national percentile points. Each regression includes a fixed effect for each school, a time trend for "most treated" schools, a time trend for "somewhat treated" schools, and a time trend for untreated comparison schools. The observations are school level averages for fourth graders, and the regressions are therefore weighted by the schools' fourth grade enrollment.

The sources for this table are Wisconsin Department of Public Instruction [various 2000] and United States Department of Education, *School District Data Book*.

Table XI
Fourth Grade Test Scores in Wisconsin's Most Treated, Somewhat Treated, and Untreated Comparison Schools

		1996-97	1999-00	annual change
Math	most treated schools	34.5	53.3	6.3
	somewhat treated schools	33.7	48.2	4.8
	untreated comparison schools	50.0	60.6	3.5
Science	most treated schools	31.9	52.8	7.0
	somewhat treated schools	32.3	49.7	5.8
	untreated comparison schools	56.0	62.9	2.3
Social Studies	most treated schools	41.6	54.2	4.2
	somewhat treated schools	43.4	50.7	2.4
	untreated comparison schools	61.0	65.6	1.5
Language	most treated schools	41.8	49.4	2.5
	somewhat treated schools	41.8	46.2	1.5
	untreated comparison schools	53.4	53.2	-0.1
Reading	most treated schools	44.2	46.5	0.8
	somewhat treated schools	45.1	43.6	-0.5
	untreated comparison schools	59.0	55.0	-1.3

Test scores are measured in national percentile points. Statistics are based on weighted averages over schools in the relevant group, where each school is weighted by its enrollment.

The sources for this table are Wisconsin Department of Public Instruction [various 2000] and United States Department of Education, *School District Data Book*.

Table XII
Michigan Districts with a Charter School Presence

Michigan School Districts where the Largest Share of Students Entered Charter Schools (at least Six Percent of Local Enrollment entered Charter Schools)	Michigan School Districts where at least Three but less than Six Percent of Local Enrollment entered Charter Schools
Pontiac**	Harrison
West Ottawa*	Greenville
Grand Rapids**	Plainwell
Petoskey	Ann Arbor**
Hillsdale	Ionia
Lansing**	Oscoda
Flint**	Monroe*
Mount Pleasant	Midland*
Jackson*	Utica**
Detroit**	
Livonia**	
Dearborn**	
Kalamazoo**	
Chippawa Hills	
Sault Sainte Marie	
Howell*	
Manistee	
Muskegon*	
Battle Creek*	
Luddington	
Saginaw*	
Coldwater	
Lapeer*	
Benton Harbor	
Grand Ledge	
Escanaba	

** indicates a very large city district (enrollment in one grade typically exceeds 1,000).

* indicates a large city district (enrollment in one grade is typically between 500 and 1,000).

Shares of students who entered charter schools cannot be calculated exactly because a charter school located in one district can attract students from surrounding districts. The above statistics are calculated under the assumption that students attend a charter school located in their district.

Table XIII
Effects of Charter School Competition on Michigan Productivity Trends

	Productivity Calculation is Based on:			
	Fourth Grade Reading Exam	Fourth Grade Math Exam	Seventh Grade Reading Exam	Seventh Grade Math Exam
Trend in Productivity among *Nontargeted* Schools	1.48 (1.44)	1.69 (0.51)	0.77 (1.01)	1.41 (0.47)
Pre-Existing Trend in Productivity among Schools that were Eventually *Targeted* by Charter School Competition	0.42 (1.71)	1.43 (0.64)	0.49 (1.19)	0.53 (0.60)
Trend in Productivity among Schools that were *Targeted* by Charter School Competition, Immediately after Charter School Entry	0.08 (1.92)	1.25 (0.71)	0.26 (1.39)	0.46 (0.75)
Trend in Productivity among Schools that were *Targeted* by Charter School Competition, After at least 6% of Local Enrollment is in Charter Schools	6.30** (2.30)	2.85** (0.78)	1.61 (1.63)	1.12 (0.94)

This table is based on a regression of school level data from 1992-93 to 1999-2000. The dependent variable is the change in productivity–specifically, the change in a school's scale scores divided by the percentage change in a school's real per-pupil spending. The independent variables are a constant (which picks up the trend in productivity among nontargeted schools), an indicator for a district eventually becoming a target of charter school competition (which picks up the differential trend for such districts), an indicator for a district having at least 0.1 percent of local enrollment in charter schools (which picks up the change in trend associated with charter school entry), and a final indicator for the district having at least six percent of local enrollment in charter schools (which picks up the change in trend associated with non-negligible competition). The two last variables "turn on" when a school begins to face competition and rarely "turn off" thereafter. The regression also includes year indicator variables that allow for minor state-wide changes in the test from year to year. The regression is weighted by the school's number of test takers. The inflator for per-pupil spending is the Consumer Price Index.

The numerator for productivity is the school's scale score on the Michigan Assessment of Educational Progress (MEAP) tests, which are administered to fourth and seventh graders. See the notes to the previous table for details on the tests.

** indicates that the time trend in targeted schools when six percent of local enrollment was in charter schools is statistically significantly different from their pre-existing time trend at the 0.05 level. * indicates the same thing, but at the 0.10 level.

The sources for this table are Michigan Department of Education [2000 various].

Table XIV
Effects of Charter School Competition on Michigan Achievement Trends

	Fourth Grade Reading Exam	Fourth Grade Math Exam	Seventh Grade Reading Exam	Seventh Grade Math Exam
Trend in Scale Scores among *Nontargeted* Schools	3.19 (0.32)	3.52 (0.25)	1.61 (0.36)	2.27 (0.24)
Pre-Existing Trend in Scale Scores among Schools that were Eventually *Targeted* by Charter School Competition	2.15 (0.38)	3.28 (0.29)	0.99 (0.43)	1.75 (0.29)
Trend in Scale Scores among Schools that were *Targeted* by Charter School Competition, Immediately after Charter School Entry	1.98 (0.58)	3.01 (0.45)	0.89 (0.75)	1.60 (0.50)
Trend in Scale Scores among Schools that were *Targeted* by Charter School Competition, After at least 6% of Local Enrollment is in Charter Schools	6.42** (0.69)	4.12** (0.54)	3.68** (0.85)	2.60 (0.57)

This table is based on a regression of school level data from 1992-93 to 1999-2000. The dependent variable is the change in a school's achievement–specifically, the change in a school's average scale score. The independent variables are a constant (which picks up the trend in achievement among nontargeted schools), an indicator for a district eventually becoming a target of charter school competition (which picks up the differential trend for such districts), an indicator for a district having at least 0.1 percent of local enrollment in charter schools (which picks up the change in trend associated with charter school entry), and a final indicator for the district having at least six percent of local enrollment in charter schools (which picks up the change in trend associated with non-negligible competition). The two last variables "turn on" when a school begins to face competition and rarely "turn off" thereafter. The regression also includes year indicator variables that allow for minor state-wide changes in the test from year to year. The regression is weighted by the school's number of test takers.

The statistics shown in the table are based on scale scores on the Michigan Assessment of Educational Progress (MEAP) tests, which are administered to fourth and seventh graders. From 1992 to 2000, the means and standard deviation of schools' average scores (weighted by the number of test takers) were: mean of 611, standard deviation of 19 on fourth grade reading; mean of 528, standard deviation of 16 on fourth grade math; mean of 600, standard deviation of 17 on fourth grade reading; mean of 521, standard deviation of 14 on fourth grade math.

** indicates that the time trend in targeted schools when six percent of local enrollment was in charter schools is statistically significantly different from their pre-existing time trend at the 0.05 level. * indicates the same thing, but at the 0.10 level.

The sources for this table are Michigan Department of Education [2000 various].

Table XV
Arizona Municipalities with a Charter School Presence

Arizona Municipalities where at least Three but less than Six Percent of Enrollment entered Charter Schools	Arizona Municipalities where at least Six but less than Ten Percent of Enrollment entered Charter Schools	Arizona Municipalities where at least Ten Percent of Enrollment entered Charter Schools - *All Are <u>Small</u>*[†]
Nogales*	Show Low	Keams Canyon
Mesa**	Vail	Clarkdale
Hereford	Fountain Hills	Golden Valley
Scottsdale**	Flagstaff*	Queen Creek
Lake Havasu	Chino Valley	Camp Verde
Avondale*	Gilbert**	Ehrenberg
Chandler**	Prescott	Higley
Apache Junction	Mayer	Coolidge
Glendale**	Tempe**	Sedona
Bullhead	Sierra Vista*	Tuba
Florence	St Johns	Concho
Tucson**	Cottonwood	Globe
	Benson	Green Valley
	Cave Creek	Kingman
	Winslow	Pima
	Safford	
	Phoenix**	
	Page	
	Bisbee	

** indicates a very large city district (enrollment in one grade typically exceeds 1,000).

* indicates a large city district (enrollment in one grade is typically between 500 and 1,000).

Shares of students who entered charter schools cannot be calculated exactly because a charter school located in one municipality can attract students from surrounding municipalities. The above statistics are calculated under the assumption that students attend a charter school located in their municipality.

† The mean one-grade enrollment of municipalities in this (right-hand) column was only 150 in 1999-00, including charter school enrollment.

The sources for this table are Arizona Department of Education [2000 various].

Table XVI
Effects of Charter School Competition on Arizona Productivity Trends

	Productivity Calculation is Based on:			
	Fourth Grade Reading Exam	Fourth Grade Math Exam	Seventh Grade Reading Exam	Seventh Grade Math Exam
Trend in Productivity among *Nontargeted* Municipalities	1.28 (0.41)	3.80 (0.74)	0.97 (0.85)	3.05 (0.37)
<u>Pre-Existing</u> Trend in Productivity among Municipalities that were Eventually *Targeted* by Charter School Competition	-0.83 (0.27)	1.17 (0.44)	0.17 (0.55)	0.96 (0.22)
Trend in Productivity among Municipalities that were *Targeted* by Charter School Competition, <u>Immediately after Charter School Entry</u>	0.08 (0.73)	1.31 (1.69)	0.88 (0.68)	1.15 (1.87)
Trend in Productivity among Municipalities that were *Targeted* by Charter School Competition, <u>After at least 3% of Local Enrollment is in Charter Schools</u>	2.99** (1.56)	6.93** (3.42)	2.92** (1.37)	7.51** (3.79)
Trend in Productivity among Municipalities that were *Targeted* by Charter School Competition, <u>After at least 6% of Local Enrollment is in Charter Schools</u>	3.74** (1.79)	8.01** (3.78)	5.33** (1.75)	7.57** (3.63)
Trend in Productivity among Municipalities that were *Targeted* by Charter School Competition, <u>After at least 10% of Local Enrollment is in Charter Schools</u>	2.88 (3.48)	6.28 (5.97)	2.38 (3.15)	6.33 (7.38)

† This table is based on a regression of school level data from 1988-89 to 1999-2000. The dependent variable is the change in productivity–specifically, the change in a school's national percentile rank (NPR) score divided by the percentage change in a school's real per-pupil spending. The independent variables are a constant (which picks up the trend in productivity among nontargeted municipalities), an indicator for a city eventually becoming a target of charter school competition (which picks up the differential trend for such municipalities), an indicator for a city having at least 0.1 percent of local enrollment in charter schools (which picks up the change in trend associated with charter school entry), an indicator for the city having at least three percent of local enrollment in charter schools (which picks up the change in trend associated with competition), and a final indicator for the city having at least six percent of local enrollment in charter schools (which picks up the change in trend associated with more substantial competition). The three last variables "turn on" as a school begins to face greater competition and rarely "turn off" thereafter. The regression also includes year indicator variables that allow for state-wide changes in the test from year to year. Intercepts are allowed to reset for each national percentile rank between 1995 and 1996, which allows for the switch from the Iowa Test of Basic Skills (1988 through 1995) to the Stanford 9 test (1996 to 2000). The regression is weighted by the school's number of test takers. The price inflator for per-pupil spending is the Consumer Price Index.

** indicates that the time trend in targeted schools is statistically significantly different from their pre-existing time trend at the 0.05 level. * indicates the same thing, but at the 0.10 level.

Table XVII
Effects of Charter School Competition on Arizona Achievement Trends

	Fourth Grade Reading Exam	Fourth Grade Math Exam	Seventh Grade Reading Exam	Seventh Grade Math Exam
Trend in NPR† Scores among *Nontargeted* Municipalities	0.32 (0.09)	0.96 (0.16)	0.25 (0.19)	0.78 (0.08)
Pre-Existing Trend in NPR† Scores among Municipalities that were Eventually *Targeted* by Charter School Competition	-0.29 (0.08)	0.41 (0.13)	0.06 (0.16)	0.34 (0.06)
Trend in NPR† Scores among Municipalities that were *Targeted* by Charter School Competition, Immediately after Charter School Entry	-0.07 (0.25)	0.64 (0.58)	0.33 (0.23)	0.86 (0.64)
Trend in NPR† Scores among Municipalities that were *Targeted* by Charter School Competition, After at least 3% of Local Enrollment is in Charter Schools	1.12** (0.53)	2.65** (1.17)	1.09** (0.48)	2.80** (1.25)
Trend in NPR† Scores among Municipalities that were *Targeted* by Charter School Competition, After at least 6% of Local Enrollment is in Charter Schools	1.39** (0.66)	2.96** (1.29)	2.00** (0.61)	2.78** (1.36)
Trend in NPR† Scores among Municipalities that were *Targeted* by Charter School Competition, After at least 10% of Local Enrollment is in Charter Schools	0.91 (0.85)	2.01 (1.60)	0.87 (0.82)	2.17 (1.77)

† This table is based on a regression of school level data from 1988-89 to 1999-2000. The dependent variable is the change in a school's achievement–specifically, the change in a school's average national percentile rank (NPR) score. The independent variables are a constant (which picks up the trend in achievement among nontargeted municipalities), an indicator for a city eventually becoming a target of charter school competition (which picks up the differential trend for such municipalities), an indicator for a city having at least 0.1 percent of local enrollment in charter schools (which picks up the change in trend associated with charter school entry), an indicator for the city having at least three percent of local enrollment in charter schools (which picks up the change in trend associated with competition), and a final indicator for the city having at least six percent of local enrollment in charter schools (which picks up the change in trend associated with more substantial competition). The three last variables "turn on" as a school begins to face greater competition and rarely "turn off" thereafter. The regression also includes year indicator variables that allow for state-wide changes in the test from year to year. Intercepts are allowed to reset for each national percentile rank between 1995 and 1996, which allows for the switch from the Iowa Test of Basic Skills (1988 through 1995) to the Stanford 9 test (1996 to 2000). The regression is weighted by the school's number of test takers.

** indicates that the time trend in targeted schools is statistically significantly different from their pre-existing time trend at the 0.05 level. * indicates the same thing, but at the 0.10 level.

The sources for this table are Arizona Department of Education [1988 through 1995, and 2000 various].