Are Changes in Equity Deceptive? The Responsiveness of Supplementary Education Spending to Changes in Local Spending

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Introduction

Be it to back-fill the loss of federal funding, decreased state aid or evaporating local property tax revenues, the popular press is replete with examples of school districts using fees to close funding gaps (e.g., Maxwell, 2013; The Pew Charitable Trusts, 2012). In addition, the growing use of fees has spawned lawsuits in Idaho (*Joki v. State of Idaho*) and California (*Doe v. The State of California*). Yet, in a survey of school administrators conducted by AASA, only 17 percent of the respondents indicated that they were "shifting funding of extracurricular activities to families/community/boosters" in response to federal aid cuts resulting from sequestration (AASA, 2013). These survey results are consistent with the finding of relatively limited use of non-property tax revenues in the sparse literature on school district utilization of these alternative revenue sources (Wassmer and Fisher, 2002). In recent work (Downes and Killeen, 2014), we documented trends over the last two decades in school district use of user fees and charges and other local sources of non-tax revenues. While use of fees and other non-tax revenues has grown and has shown some sensitivity to constraints on traditional revenues created by economic downturns and by tax and expenditure limits (TELs), these non-tax sources of revenue have remained a relatively unimportant source of district resources (Downes and Killeen, 2017).

The use of fees has been little changed in part because districts have been able to respond to declines in state and federal aid by increasing property taxes, even when constraints on local revenue-raising ability exist. But property tax increases have only filled part of the gap (Dye and Reschovsky, 2008; Chakrabarti, Livingston, and Roy, 2014). Supplementary education spending, particularly by more affluent families living in districts most constrained by school finance reforms and other fiscal institutions, could further help close the gap between public spending and desired provision. Thus the absence of substantive growth in fee revenues in

education could be explained by historically unobservable supplemental or shadow spending on education by families. The goal of this study is to estimate the causal link between supplemental spending and local public education spending, with an eye towards improving our understanding of the extent to which supplemental spending mutes the impact of state policies designed to equalize opportunities.

In the U.S. context, analysis of supplementary spending on education is limited. Kornich and Rodriguez (2016) and Kornrich and Furstenberg (2013) look at the relationship between supplementary spending and family characteristics using the Consumer Expenditure Survey; Hao and Yeung (2015) do the same with the Panel Study of Income Dynamics. But neither of those papers looks at the link between local spending on public education and this supplementary spending. And while Downes (2007) attempts to look at how supplementary spending responds to constraints imposed by tax and expenditure limits and education finance reforms, the analysis is done using the Before- and After-School Programs and Activities Survey, making it impossible to document how substitution might respond to the heterogeneous effects within a state of finance reforms or tax and expenditure limits (TELs). Here, we link data on families' expenditures to measures of public school spending to quantify the impacts of state policy on this supplementary spending.

In particular, we combine data on local school districts drawn from the Common Core of Data and the Stanford Education Data Archive (Reardon, et al, 2017) with data from the Panel Study for Income Dynamics (PSID) to examine the extent to which families adjust spending on education activities in response to K-12 spending. Jackson and Johnson (2017; with Persico, 2016), Lafortune, Rothstein, and Schanzenbach (2016), and Brunner, Hyman, and Ju (2017) establish that school finance reforms can be treated as exogenous changes to the fiscal landscape.

As they do, we use school finance reforms to instrument for local spending on education and thus generate causal estimates of the link between supplementary spending and local education spending. This research posits that, in some localities, significant K-12 education spending among families occurs outside of the purview of traditional education finance, and that spending is sensitive to what is happening locally. Further, we explore whether accounting for supplementary spending alters our assessment of the equity of schooling provision.

We find that, while there is evidence that failing to account for the possibility that public education spending is endogenous leads to biased estimates of its impact on supplementary spending, we cannot rule out the possibility that supplementary spending is independent of local provision. Thus, we find no evidence that supplementary spending is mitigating the impact of finance reforms. We also find, however, that supplementary spending is higher in states in which TELs are imposed on local governments. One explanation for these seemingly contradictory results is that recent finance reforms have leveled-up (Lafortune, Rothstein, and Schanzenbach, 2016) and, therefore, have affected relative but not absolute spending in high-income districts. Local TELs, when combined with state-level TELs, reduce spending and send a signal about a state's commitment to education (Downes and Figlio, 2015). Our findings suggest that families' decisions about supplementary spending may be sensitive to perceived public school quality but do not appear to be driven by a desire to maintain relative advantage.

We also find that families who reside in districts with higher fractions of minority students spend more. Again, this suggests that perceptions matter. This finding parallels results in both the public-private choice literature (e.g., Lankford and Wyckoff, 1992) and the hedonics literature (e.g., Downes and Zabel, 2002).

All of these results appear to be driven by the families with the highest spending. We

suspect that many of the families with the highest levels of spending are sending some or all of their children to private schools, partly because of the parallels between our findings and findings in the public-private choice literature and partly because of the nature of the results when we limit the sample to families with spending levels below the average tuition for Catholic schools.

The next section reviews the evolving literature on the effects of school finance reforms and provides context for our analysis of supplementary spending. We then describe the data we use, followed by an overview of the models we estimate and the steps we take to address the possibility that local education provision and the demographic composition of local schools are endogenous. The final two sections provide the results from our estimation of these models and summarize the implications of these estimates.

Supplementary Spending: Its Importance and Its Determinants

Jackson, Johnson, and Persico (2016) and Lafortune, Rothstein, and Schanzenbach (2016) document the reductions in spending inequality that have resulted from reforms in the systems states use to finance public elementary and secondary education.[1] But Lafortune, Rothstein, and Schanzenbach note that, while the most recent finance reforms have reduced cross-district inequalities in both spending and scores on the National Assessment of Education Progress (NAEP) test, those reforms have no appreciable impact on inequality of NAEP scores by income. They argue that the persistence of inequality by income results from the fact that poor children do not live disproportionately in low spending districts that are the principle beneficiaries of finance reforms.

[1] Evans, Schwab, and Wagner (forthcoming) argue that inequality improvements have been dissipated since 2001. However, this dissipation is driven by growing cross-state inequality. The within-state equity gains from finance reforms do shrink over time, but they are not fully dissipated (Jackson, Johnson, and Persico, 2016).

The recent literature on the growth of shadow education and parental spending on supplementary education suggests a second possibility; higher income families living in districts in which relative spending has declined have increased their supplementary spending. Downes (2007) provides a simple theoretical model which shows that the types of constraints on spending in previously high-spending districts that are likely to accompany finance reforms and TELs, which are often concomitant (Downes and Figlio, 2015), are likely to increase families' incentives to spend on supplementary education services. Kornrich and Furstenberg (2013) show that, from 1972 to 2007, spending on children grew more grew more rapidly in the top than the bottom income deciles.[2] And that growth was driven by increases on spending on education and childcare.

But, as is always true, this simple correlation between the growth of supplementary spending by high income families and the imposition of finance reforms and TELs cannot support the conclusion that this growth is a response to constraints on public spending in previously high spending districts. In fact, we know very little about the relationship between supplementary spending and the perceived quality of local education provision. Much of the focus of the literature on supplementary spending, particularly on tutoring services, has been on non-U.S. contexts; see Dang and Rogers (2008) and Park, et al (2016) for excellent reviews. And, while authors have suggested that parents spend more when they perceive the quality of local schools is lower, few have examined that relationship empirically. Das, et al (2013) show that, in both Zambia and Andhra, Pradesh, India, families reduced their spending when local

[2] Spending on children as a share of income grew in all deciles, but the growth in that share was largest in the lowest income decile. However, for the lowest income deciles, the growth in the share occurred between 1972-73 and 1983-84, with declines in the share since then. The share in the highest income deciles has increased from 1983-84 to 2006-07, which is the period of greatest activity in finance reforms (Jackson, Johnson, and Persico, 2016) and TELs (Downes and Figlio, 2015).

schools received anticipated grants. Unanticipated grants led to no changes in family spending. Dang (2007) finds that parents in Vietnam spend more on private tutoring when the share of qualified teachers is lower. On the other hand, Davies (2004) sees no relationship between satisfaction with local schools and the likelihood of hiring private tutoring in a survey of Canadian parents.

Davies' results on the relationship between public school quality and supplementary spending could signal a weak relationship in developed country contexts. Alternatively, they could be driven by bias, since families are likely to choose where to live on the basis of public school quality. Families that demand high levels of education provision could sort into districts that are perceived to be better for their children and still have higher that average levels of supplementary spending.[3] Cross-sectional regressions like those of Dang and Davies are likely to tell us little about how families will respond to changes in school quality following finance reforms or TELs.

We address this challenge in isolating the causal link between public provision and supplementary spending in two ways. First, since we use reports on education expenditures and spending on childcare from families in the PSID, we can take advantage of the panel nature of the data to control for temporally-stable unobservables. Second, we draw on a growing literature (Jackson, Johnson, and Persico, 2016; Jackson and Johnson, 2017; Lafortune, Rothstein, and Schanzenbach, 2016; Brunner, Hyman, and Ju, 2017) that has established that school finance reforms can be treated as exogenous events. As a result, we can use school finance reforms to construct instruments for public school expenditures. Details on the instruments are given below.

[3] If Tiebout-like sorting is imperfect, communities will be heterogeneous and some, even in high provision communities, will want more than is publicly provided.

Data

The primary source of our data is the Panel Study of Income Dynamics (PSID). The PSID, which began in 1968 and has been biannual since 1997, added questions on supplementary education and child care expenditures in 1999. Li, Schoeni, Danziger, and Charles (2010) show that the expenditure data in the PSID matches well with similar expenditure data in the Consumer Expenditure Survey, which is explicitly designed to collect expenditure data and which has been the source of most analyses of supplementary spending (e.g., Kornrich and Furstenberg, 2013). To preserve as many observations as possible, we use the imputed values of supplementary education expenditures, child care expenditures, and family income, though Li, Schoeni, Danziger, and Charles argue that the low nonresponse rate in the PSID means that it matters little on how nonresponses are handled.

To create our panel data set from the PSID, we identified each family's head in each survey year. Our rule was that a family would be identified as being the same if the head was unchanged. We then needed to place families in school districts in order to be able to match them to school district level data. For each family in each interview year, the restricted use version of the PSID provides each household's census block based on the 2010 definition of census blocks. We matched each family to the school districts that includes their block of residence using the Census Bureau's Block Assignment file for 2010. Every family living in a K12 district were matched to a single district. Families in both elementary and high school districts were assigned to the elementary district if all school age children in the household were of elementary school age. Families with only high school age children were assigned to the high school. For families with both elementary and high school age children, we used the weighted average of the elementary school district and high school district data, with weights based on the

number of children in each age bracket. Finally, we drop from the sample all families with college-age children. Since our measure of supplementary education expenditures includes tuition payments, we omitted families with college-aged children to avoid the possibility of counting college tuition payments as supplementary spending.

Once we had located families in school districts, we matched those families to data on the school districts in which they lived. The data on school districts was drawn from the National Center for Education Statistics' Common Core of Data (CCD) and the Stanford Education Data Archive (Reardon, et al, 2017). The CCD proved data on district spending, staffing, and student demographics. Test score data for most districts in the U.S. were available beginning in 2008-09 from the Stanford Education Data Archive. We have used the score estimates that are linked to the NAEP scale in order to have scores that are comparable across stares.[4] We combine these financial data with demographic data from the 1980 and 1990 Decennial Censuses and with school district enrollment data from the 1987 Census of Governments.

As we noted above, our strategy for instrumenting for school finance reforms depends critically on the geographic and temporal variation in school finance reforms. Recent papers that use national information on finance reforms (Downes and Killeen, 2013; Jackson, Johnson, and Persico, 2016; Lafortune, Rothstein, and Schanzenbach, 2016; Brunner, Hyman, and Ju, 2017) vary in how they identify finance reforms. While most have shown that results do not hinge on a particular strategy for identifying the timing of finance reforms (see Lafortune, Rothstein, and Schanzenbach, for example), we have chosen to use the reform timings of Brunner, Hyman and Ju. They identify the first year of a reform and then set their reform dummy to 1 in that year and all subsequent years. We do the same. Since Brunner, Hyman, and Ju show that the finance

[4] See the Stanford Education Data Archive for more detail on the process used to generate each district's score estimates.

reforms are effectively exogenous, they can serve as the basis of our identification strategy.

Brunner, Hyman, and Ju only identify finance reforms from 1989 onwards. Since several key finance reforms came before 1989, using their strategy to classify states as having finance reforms might cause us to fail to identify as reform states certain states that had earlier reforms.[5] To see if this matters, we used Springer, Liu, and Guthrie (2009) and Jackson, Johnson, and Persico (2016) to identify states with pre-1989 reforms.

Since TELs could also impose the types of local constraints that would encourage families to increase their supplementary spending, we started with Mullins and Wallin (2004) to identify states with local limits. The Lincoln Institute of Land Policy's *Significant Features of the Property Tax* provided the information needed to bring up to date information on local limits. Following the tradition in the literature, school districts were classified as being potentially subject to a limit if in the state or the county in which that district was located there existed limits on expenditures, limits on revenues, or combined limits on nominal tax rates and assessment growth. If any one of these three limits was present, a district was treated as having a limit on the ability to raise revenues and spend those revenues.[6]

Table 1 includes summary statistics on the key covariates in our analysis. We present both weighted means, using the family weights given in the PSID, and unweighted means. Between 1997 and 2011, mean spending on supplementary education increased from $588.57 to $2232.31 for families in our sample, consistent with the trends described by Kornrich and

[5] Lafortune, Rothstein, and Schanzenbach (2016), whose identification procedure is very similar to that of Brunner, Hyman, and Ju, argue that post-1988 reforms, which are primarily adequacy reforms, differ fundamentally from earlier reforms, which were primarily equity reforms. However, Springer, Liu, and Guthrie (2009) found little evidence that there is difference in the effects of court mandated equity and adequacy reforms.

[6] In Illinois, residents of individual counties can choose to impose limits. As a result, we coded the timing of limits in counties in Illinois using the January 2016 version of the History of PTELL map provided by the Property Tax Division of the Illinois Department of Revenue.

Furstenberg (2013).[7] Spending on childcare was $331.57 in 1997 and $158.89 in 2011. By 2011, 51.27 percent of the families resided in school finance reform states according to the Brunner, Hyman, and Ju classification. That percentage rises to 55.65 if we add in the pre-1989 reforms. Also, in 1999 the percent of the families in school districts in the bottom quartile of the within-state distribution across school districts of per capita income in 1980 is 15.58. The percentages in the second and third quartiles are 19.26 and 18.69, respectively.

We cannot report minimum and maximum values of supplementary education expenditures, but we can note that about 46 percent of our observations are zero. To address potential complications created by the high frequency of zeros, we estimated our models using OLS, Tobit, and Poisson methodologies.[8]

We also can note that the maximum value is large. That fact highlights a reality in the data – a small subset of families have large expenditure levels. Others using similar expenditure data have suggested that the high spenders are paying for private school (Farre, Ortega, and Tanaka, 2018). To limit our analysis to families who are unlikely to be paying for private school, we generate estimates restricting our sample to all the families in each interview year who have spending below $5000 in 1990 dollars. While the average tuition on private schools was less than $5000, in real terms, at the beginning of our sample period, average tuition exceeded $5000 by the end of our period.[9] Thus, it seems likely that most families with spending below $5000 were not paying for private school.

[7] All dollar figures are inflation adjusted to 1990 dollars using the CPI-U.
[8] Nichols (2010) has suggested the Poisson specification as a method to generate better inferences in situations when zero values of the dependent variable are common.
[9] The *Digest of Education Statistics* was our source of information on average private school tuition.

Models and Methods

To estimate the link between changes in public school spending and family spending decisions, we estimate models of the form

$$y_{idt} = \boldsymbol{\alpha} + S_{dt}\beta + X_{it}\delta + Z_{d}t\gamma + Q1_{d}t\theta_1 + Q2_{d}t\theta_2 + Q3_{d}t\theta_3 + \tau_t + \eta_i + \varepsilon_{idt}\ , \quad (1)$$

where y_{idt} is a measure of spending by family i residing in district d in year t. The variable S_{dt} includes measures of per pupil expenditures, pupil-teacher ratio, or 5th grade math test scores[10] and fraction minority in district d in year t, and X_{it} are time-varying family attributes. Each district's fraction minority equals the sum of the fractions Black, Native American, and Hispanic. To control for the demographics of each district's residents that could influence both district spending and the spending choices of families, we follow Jackson, Johnson, and Persico (2016) and Brunner, Hyman, and Ju (2017) and interact district demographic measures (Z_d) in 1980 with a time trend.[11] Using pre-determined demographic measures eliminates the possibility that these demographics are affected by school finance reforms and thus endogenous. The variables Q1, Q2, and Q3 are indicators of whether the district fell in the first, second or third quartile of the state's cross-district distribution of per capita income in 1980. The variables τ_t, η_i, ε_{idt} represent, respectively, year-effects, family fixed effects, and a random error term. In our estimation, we cluster by family.

As we argued above, school district provision and student demographics are likely to be endogenous. To address this problem, we take advantage of the fact that school finance reforms have been shown to be "exogenous quasi-experimental shocks" (Jackson, Johnson, and Persico, 2016, p. 159). We also build upon the work of Lafortune, Rothstein, and Schanzenbach (2016),

[10] We used 5th grade math scores to maximize the number of included observations.
[11] Since the Decennial Census includes no district enrollment information, we use enrollment measures from the 1987 Census of Governments.

who show that lower income districts benefit relatively from finance reforms. As a result, our first stage is

$$S_{dt} = \alpha^1 + \beta_1{}^1 Q1_d SFR_t + \beta_2{}^1 Q2_d SFR_t + \beta_3{}^1 Q3_d SFR_t$$

$$+ Q1_d t\varphi_1 + Q2_d t\varphi_2 + Q3_d t\varphi_3 + Z_d t\pi + \varsigma_t + \rho_d + \upsilon_{dt} . \quad (2)$$

The variable SFR is a dummy variable that equals 1 in the year a district's state has a finance reform and in all subsequent years.

Table 2 gives the results of estimation of this first stage when we include pre-1989 reforms in defining SFR and use per pupil spending and the fraction minority as our measures of public school provision.[12] While, as expected, spending in the second and third quartiles increases relative to the omitted fourth quartile post-reform, we do not see the expected effect for the first quartile. That may reflect the fact that spending of all first quartile districts, even those in non-reform states, trended up throughout our period of analysis. The trends in the second and third quartile districts are much weaker. Thus, even non-reform states may have been tweaking their finance formulae to increase spending in poorer districts, making the separate effect of finance reforms on these districts less evident. We also see that the fraction minority increased in finance reform states. Finally, the value of the Cragg-Donald F statistic suggests that any small-sample bias attributable to weak instruments is limited.[13,14]

Results

Table 3 provides estimates of equation (1) when we use spending and fraction minority as

[12] Appendix Table 1 gives the first stage estimates when we exclude pre-1989 reforms in defining SFR. We find that the first-stage fit is better when we include the pre-1989 reforms.
[13] Mayoral (2015) notes that the Cragg-Donald statistic is only suggestive when clustered standard errors are used.
[14] We have estimated specifications that replace per pupil spending with either pupil-teacher ratio or 5th grade math score. Our instruments are weak when we use these measures of schooling provision, so we do not report those results.

our measures of provision. In the first two columns are the ordinary least squares estimates; instrumental variables estimates are in the final two columns.[15,16]

Most of the estimates differ little between the first two and the last two columns. When we use the full sample and do not account for potential endogeneity (column 1), supplementary spending is positively related to public education expenditures, though not significantly so. That does not change when we account for potential endogeneity (column 3). And, when we limit the sample to families with spending less than $5000, the estimated effects continue to be positive but insignificant. Taken as a whole, the estimates do not support the conclusion that supplementary spending mitigates the impact of equity-enhancing finance reforms.

That conclusion is clouded a bit by the results for fraction minority. The estimates indicate that supplementary spending is higher in districts with higher fractions minority. The implied effect is particularly large in the full sample; the estimated impact is insignificant when we limit the sample to families with spending below $5000. Since, as we noted above, we suspect that high-spending families are purchasing private education, then these results suggest that the decision to choose private school is sensitive to the racial/ethnic composition of the local school district. Other supplementary spending seems to be less sensitive to district demographics, so any effects on equity happen because of the decision of a few families to opt out to the private sector.[17]

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[15] Appendix Table 2 includes the instrumental variable estimates when we include pre-1989 reforms in defining SFR. The qualitative implications of those estimates are the same as those implied by the estimates in Table 3.

[16] Table 3 includes OLS and traditional IV estimates. We have also explored the sensitivity of our results to the high fraction of zeros by using Poisson or Tobit methodologies to generate estimates. The conclusions implied from these alternative estimates matched those reported here.

[17] We interacted fraction minority with an indicator of the head's minority status in order to see if the relationship between supplementary spending and fraction minority depended on the race or

The other estimated effects generally match expectations. Families with higher incomes spend more on supplementary education, though the magnitude of the effect is small. In the full sample, each additional child reduces supplementary spending by about $300. Additional school age children have no impact on spending when we limit the sample to families spending less than $5000, suggesting that families set an overall budget for supplementary spending and not a target amount per child. Families with a spouse present spend more than do single parent households, possibly because the logistics of private schools, tutoring, and other supplementary education programs are easier when two adults are present.

Family composition changes, other than the head or spouse departing, result in a significant increase in supplementary spending. Kornich and Furstenberg (2013) show that supplementary spending spikes up near the end of high school and then drops sharply. This pattern would lead us to expect that supplementary spending would decline after a family composition change; we need to explore further why we get a result counter to expectations.

In the full sample, families who live in school districts subject to TELs spend substantially more, an added $242, on supplementary education. This result, which is not apparent when we limit the sample to families spending less than $5000, is a bit surprising since it is happening over and above any direct impact of the TEL on local spending. But Downes and Figlio (2013) note that one way to understand some of the impacts of TELs, such as declines in the flow into the teaching profession, is that TELs may be taken by some as a signal about the long-term commitment to education. If families interpret TELs in that way in the short run and act on the signal by choosing private schools, this increase in supplementary spending is less surprising. The pattern of changes in private schooling in California in the aftermath of

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ethnicity of the head. The interaction was never significant.

Proposition 13 fits this pattern. Downes and Schoeman (1998) find substantial increase in private schooling in the immediate aftermath of Proposition 13; Brunner and Sonstelie (2006) note that the private school share in California dropped after families discovered that the impact of the limits were less than might have originally been expected. Spending changes of the type we observe are consistent with that pattern of behavior. Thus, TELs matter, even if they have little impact on spending levels in the long run.

Concluding Remarks

A growing body of research (Jackson and Johnson, 2017; with Persico, 2016; Lafortune, Rothstein, and Schanzenbach, 2016) has shown that school finance reforms have led to permanent changes in state school finance systems that have translated into short- and long-term benefits for affected students. But those gains have not resulted in shrinking gaps in performance between students from high- and low-income families (Lafortune, Rothstein, and Schanzenbach, 2016). The persistence of those gaps in performance might be a result of the fact that benefits of finance reforms are not necessarily targeted at students from low-income families (Lafortune, Rothstein, and Schanzenbach, 2016). Another possibility is that high-income families have preserved their relative standing by increasing their supplementary education spending. We explore that second possibility in this paper.

To analyze the links between supplementary spending and spending on the local public schools, we merge data on education spending from the National Center for Education Statistics' Common Core of Data with data on a panel of families drawn from the Panel Study of Income Dynamics. In addition, to address the potential endogeneity of public education spending, we instrument for that spending using the varied timing across states of school finance reforms, which has been shown to be exogenous (Jackson and Johnson, and Persico, 2016; Lafortune,

Rothstein, and Schanzenbach, 2016; Brunner, Hyman, and Ju, 2017).

Our estimates suggest that instrumenting for public spending on education is necessary, but we cannot rule out the possibility that supplementary spending is unresponsive to changes in public education spending. Spending does seem to be sensitive to the racial/ethnic composition of local school districts, with the pattern of the estimates suggesting that private school is a more commonly chosen option when the share of the student population that is minority is higher.

We do find, however, that institutional changes to local fiscal conditions matter. Imposition of local tax and expenditure limits leads to large increases in supplementary spending, which again appear to be driven by the decision of some to opt into private schools in the aftermath of limits.

The Panel Study of Income Dynamics, which has been shown to be a good source of information on supplementary education (Hao and Yeung, 2015; Bouffard, et al, 2006), seems to be particularly appropriate for analyzing the relationship between supplementary spending and public provision. We have shown that family income is not the only driver of supplementary spending, the nature of public provision matters. The next step is to separate spending on tuition from other supplementary spending. The Child Development Survey of the PSID might offer an avenue for doing that (Hao and Yeung, 2015) and for better understanding why the decision to choose private school appears to be very different from other supplementary spending decisions.

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Table 1
Summary Statistics

Variable	Observations	Mean (unweighted)	Standard Deviation (unweighted)	Mean (weighted)	Standard Deviation (weighted)
Supplementary Education Expenditures	28276	965.11	3348.61	507.68	1840.29
Family Income	28276	44718.01	67040.98	31286.29	35505.45
Number of School Age Children in the Family	28561	1.79	0.95	1.88	1.05
Moved Since Last Survey	28561	0.350	0.477	0.439	0.496
Spouse Present in the Family	28561	0.635	0.482	0.508	0.500
Family Composition (other than Head or Spouse) Changed Since Last Survey	28561	0.402	0.490	0.500	0.500
Family Resides in a Town (NCES designation)	28276	0.081	0.273	0.046	0.211
Family Resides in a Rural Area (NCES designation)	28276	0.155	0.362	0.101	0.301
Per Pupil Expenditures	28561	5618.48	1962.62	5844.05	1741.58
Fraction Minority	28561	0.437	0.324	0.633	0.284
District Subject to TEL	28287	0.765	0.424	0.639	0.480

Table 2
First-Stage Estimates for Per Pupil Expenditures and Fraction Minority[1]
(Standard Errors based on Clustering by Family in Parentheses)

	No limit on supplementary educational expenditures		Supplementary educational expenditures < $5000	
Variable	**Per Pupil Expenditures**	**Fraction minority**	**Per Pupil Expenditures**	**Fraction minority**
Interaction of school finance reform with first quartile of 1980 income distribution	38.3329 (92.8847)	0.0551*** (0.0139)	16.3181 (95.5933)	0.0511*** (0.0149)
Interaction of school finance reform with second quartile of 1980 income distribution	237.8791** (109.4886)	-0.0016 (0.0128)	293.1339*** (109.5006)	-0.0107 (0.0136)
Interaction of school finance reform with third quartile of 1980 income distribution	476.3623*** (73.9503)	0.0111 (0.0123)	470.6886*** (77.9684)	0.0144 (0.0135)
District subject to TEL	222.8255*** (44.9405)	-0.0169*** (0.0055)	219.1811*** (44.6426)	-0.0158*** (0.0057)
Family Income	-0.000248 (0.000216)	5.06e-08** (2.27e-08)	-0.000214 (0.000419)	2.59e-08 (3.19e-08)
Number of School Age Children in the Family	-28.0225** (11.2960)	0.0014 (0.0014)	-20.6745** (10.0205)	0.0031** (0.0015)
Moved Since Last Survey	-0.2629 (24.0597)	-0.0073*** (0.0020)	1.7807 (17.5759)	-0.0083*** (0.0021)
Spouse Present in the Family	-150.7299 (180.5227)	-0.0100 (0.0076)	-77.7915 (197.7884)	-0.0031 (0.0083)
Family Composition Changed Since Last Survey	26.0501 (17.4050)	0.0034** (0.0016)	20.4538 (14.8704)	0.0037** (0.0018)
Family Resides in a Town (NCES designation)	-86.5704** (36.5006)	-0.0147*** (0.0049)	-87.2013** (36.9338)	-0.0124** (0.0051)
Family Resides in a Rural Area (NCES designation)	-56.8837 (42.6986)	-0.0468*** (0.0044)	-25.7850 (49.3924)	-0.0448*** (0.0050)
Interaction of trend with 1987 enrollment	0.0001689*** (0.0000294)	-4.12e-09 (4.12e-09)	0.0001588*** (0.0000344)	-5.91e-09 (5.00e-09)
Interaction of trend with 1980:				
Fraction high school (and not college) graduates	-64.8366* (30.6624)	0.0108*** (0.0030)	-74.2455** (33.8568)	0.0127*** (0.0034)
Fraction college graduates	82.5228*** (25.8948)	0.0033 (0.0039)	70.2272** (27.8056)	0.0050 (0.0045)
Per capita income	0.0050*** (0.0013)	-6.45e-07*** (1.52e-07)	0.0054*** (0.0014)	-7.35e-07*** (1.75e-07)

Fraction of population living in poverty	13.1209 (11.3006)	-0.0032** (0.0015)	23.0957* (12.2586)	-0.0043** (0.0017)
Fraction of population Black	103.1052*** (11.3129)	0.0361*** (0.0017)	125.0503*** (12.4573)	0.0391*** (0.0020)
Fraction of population Native American	47.6513 (40.6956)	-0.0008 (0.0028)	69.9807 (53.5065)	-0.0019 (0.0032)
Fraction of population Asian American	18.5234 (37.7115)	-0.0067 (0.0052)	45.1117 (50.0406)	-0.0048 (0.0054)
Fraction of population Hispanic	-88.7964*** (14.3156)	0.0467*** (0.0028)	-69.2102*** (15.8829)	0.0492*** (0.0030)
District in first quartile of 1980 income distribution	62.3148*** (6.5753)	-0.0034*** (0.0009)	59.6328*** (7.0115)	-0.0030*** (0.0010)
District in second quartile of 1980 income distribution	27.4294*** (7.1832)	-0.0002 (0.0007)	19.3672*** (6.6960)	-0.000005 (0.0008)
District in third quartile of 1980 income distribution	13.5432*** (4.0955)	-0.0008 (0.0007)	9.8474** (4.4123)	-0.0008 (0.0008)
Number of observations	24498	24498	20187	20187
Number of families	5825	5825	5009	5009
Cragg-Donald F-statistic	18.96		21.32	
Sanderson-Windmeijer F-statistic	21.43	8.22	19.88	6.96

Note: 1) All specifications include family-specific effects and year effects.
* significant at 10 percent level, ** at 5 percent level, *** at 1 percent level .

Table 3
Determinants of Families' Supplementary Education Expenditures[1]
(Standard Errors based on Clustering by Family in Parentheses)

Variable	Ordinary Least Squares Results		Instrumental Variables Results	
	No limit on supplementary Education Expenditures	**Supplementary educational expenditures < $5000**	**No limit on supplementary Education Expenditures**	**Supplementary educational expenditures < $5000**
Per Pupil Expenditures	0.037*	0.003	0.066	0.023
	(0.020)	(0.012)	(0.435)	(0.178)
Fraction Minority	1211.414***	257.199**	5793.237**	251.228
	(298.464)	(106.320)	(2814.458)	(1180.769)
District subject to TEL	169.713	-46.696	242.209	-59.139
	(138.193)	(49.1985)	(185.090)	(65.093)
Family Income	0.006**	0.001**	0.006**	0.001**
	(0.003)	(0.001)	(0.003)	(0.001)
Number of School Age Children in the Family	-261.330***	57.498***	-266.335***	57.946***
	(41.718)	(16.179)	(44.542)	(16.843)
Moved Since Last Survey	-15.950	-53.236***	17.742	-53.351**
	(47.816)	(19.938)	(53.327)	(21.874)
Spouse Present in the Family	242.233**	134.706***	289.503**	136.208***
	(100.680)	(48.377)	(123.370)	(50.252)
Family Composition Changed Since Last Survey	402.467***	81.865***	386.673***	81.543***
	(49.765)	(18.644)	(51.507)	(19.424)
Family Resides in a Town	12.794	13.133	87.421	14.766
	(113.583)	(46.764)	(133.585)	(50.547)
Family Resides in a Rural Area	0.728	-14.565	224.186	-14.310
	(81.130)	(40.829)	(166.979)	(69.376)
Observations	26657	22145	24498	20087
Families	7984	6967	5825	5009
F	6.485	3.256	6.412	3.398

Note: 1) All specifications include family-specific effects and year effects. In addition, all specification include interactions between a time trend and 1987 enrollment, 1980 percent high school graduate, 1980 percent college graduate, 1980 per capita income, 1980 fraction below poverty, 1980 fraction Black, 1980 fraction Native American

* significant at 10 percent level, ** at 5 percent level, *** at 1 percent level .

Appendix Table 1
First-Stage Estimates for Per Pupil Expenditures and Fraction Minority[1]
(Standard Errors based on Clustering by Family in Parentheses)

	No limit on supplementary educational expenditures		Supplementary educational expenditures < $5000	
Variable	**Per Pupil Expenditures**	**Fraction Minority**	**Per Pupil Expenditures**	**Fraction Minority**
Interaction of school finance reform with first quartile of 1980 income distribution	-25.1011 (81.5419)	0.0084 (0.0118)	-54.8310 (82.2361)	0.0019 (0.0125)
Interaction of school finance reform with second quartile of 1980 income distribution	399.6261*** (154.7216)	-0.0043 (0.0111)	3437.1998** (156.9384)	-0.0115 (0.0117)
Interaction of school finance reform with third quartile of 1980 income distribution	323.9298*** (62.4481)	-0.0214 (0.0097)	306.1900*** (66.7973)	-0.0239** (0.0107)
District subject to TEL	236.6746*** (44.4135)	-0.0176*** (0.0055)	232.2661*** (44.1627)	-0.0168** (0.0057)
Family Income	-0.000252 (0.000218)	4.96e-08** (2.25e-08)	-0.000221 (0.000419)	2.54e-08 (3.16e-08)
Number of School Age Children in the Family	-27.6065*** (11.1530)	0.0013 (0.0014)	-20.0392** (9.9467)	0.0029* (0.0015)
Moved Since Last Survey	-2.0548 (28.2634)	-0.0073*** (0.0020)	0.4557 (17.1530)	-0.0083*** (0.0021)
Spouse Present in the Family	-142.1059 (177.8222)	-0.0095 (0.0076)	-67.7637 (192.7151)	-0.0025 (0.0084)
Family Composition Changed Since Last Survey	25.4316 (17.7454)	0.0032** (0.0016)	19.5946 (15.2841)	0.0035* (0.0018)
Family Resides in a Town (NCES designation)	-73.1763* (37.8476)	-0.0159*** (0.0049)	-74.2474* (39.9708)	-0.0133*** (0.0051)
Family Resides in a Rural Area (NCES designation)	-52.7303 (43.7828)	-0.0484*** (0.0044)	-21.2972 (51.7950)	-0.0463*** (0.0050)
Interaction of trend with 1987 enrollment	0.0001789*** (0.0000295)	-3.43e-09 (4.02e-09)	0.0001664*** (0.0000346)	-5.08e-09 (4.85e-09)
Interaction of trend with 1980:				
Fraction high school (and not college) graduates	-67.5569** (29.6533)	0.0113*** (0.0030)	-76.1328** (32.9195)	0.0133*** (0.0035)
Fraction college graduates	84.5235*** (25.8695)	0.0040 (0.0038)	72.2408*** (27.4445)	0.0060 (0.0045)
Per capita income	0.0051*** (0.0013)	-6.65e-07*** (1.54e-07)	0.0055*** (0.0014)	-7.60e-07*** (1.78e-07)

Fraction of population living in poverty	15.9288 (11.3702)	-0.0032** (0.0015)	26.5767** (12.3125)	-0.0043** (0.0017)
Fraction of population Black	105.5005*** (11.1439)	0.0363*** (0.0017)	127.2483*** (12.5052)	0.0393*** (0.0019)
Fraction of population Native American	47.5404 (40.5098)	-0.0013 (0.0026)	70.3741 (53.3592)	-0.0019 (0.0028)
Fraction of population Asian American	4.8988 (36.1325)	-0.0078 (0.0054)	27.8554 (46.8826)	-0.0058 (0.0055)
Fraction of population Hispanic	-98.1712*** (15.0600)	0.0473*** (0.0029)	-77.0721*** (16.4532)	0.0501*** (0.0032)
District in first quartile of 1980 income distribution	63.8434*** (6.4449)	-0.0019** (0.0008)	62.8971*** (6.8814)	-0.0012 (0.0009)
District in second quartile of 1980 income distribution	26.8451*** (7.0407)	-0.0001 (0.0007)	18.1413*** (4.9374)	0.00001 (0.0008)
District in third quartile of 1980 income distribution	12.2688*** (3.9353)	0.0001 (0.0007)	15.5671** (4.2391)	0.0004 (0.0008)
Number of observations	24498	24498	20187	20187
Number of families	5825	5825	5009	5009
Cragg-Donald F-statistic	2.20		0.82	
Sanderson-Windmeijer F-statistic	1.08	0.96	0.34	0.35

Note: 1) All specifications include family-specific effects and year effects.
* significant at 10 percent level, ** at 5 percent level, *** at 1 percent level .

Appendix Table 2
Determinants of Families' Education-Related Expenditures[1]
(Standard Errors based on Clustering by Family in Parentheses)

	No limit on supplementary Education Expenditures	**Supplementary educational expenditures < $5000**
Per Pupil Expenditures	0.386 (0.532)	0.308 (0.734)
Fraction Minority	7170.821 (10887.389)	8889.919 (11826.488)
District subject to TEL	194.050 (219.808)	16.510 (102.699)
Family Income	0.006** (0.003)	0.001* (0.001)
Number of School Age Children in the Family	-258.957*** (46.361)	37.915 (29.981)
Moved Since Last Survey	27.754 (102.478)	18.840 (103.386)
Spouse Present in the Family	350.779* (197.302)	177.549 (118.846)
Family Composition Changed Since Last Survey	374.532*** (68.970)	45.072 (58.861)
Family Resides in a Town (NCES designation)	136.701 (226.136)	151.189 (222.997)
Family Resides in a Rural Area (NCES designation)	309.544 (547.527)	390.909 (567.647)
Observations	24489	20087
Families	5825	5009
F	5.99	1.868

Note: 1) All specifications include family-specific effects and year effects. In addition, all specification include interactions between a time trend and 1987 enrollment, 1980 percent high school graduate, 1980 percent college graduate, 1980 per capita income, 1980 fraction below poverty, 1980 fraction Black, 1980 fraction Native American

* significant at 10 percent level, ** at 5 percent level, *** at 1 percent level .