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

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; Heung 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 local spending data drawn from the Common Core of Data with data from the Panel Study for Income Dynamics (PSID). The PSID offers a nationally representative sample of more than 18,000 Americans living in more than 5,000 family groups—the longest running longitudinal data set in the US. Following guidance from Jackson, Johnson, and Persico (2016), and using the rich set of demographic information available on PSID families, we 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. We ask under what conditions American households spend on educational enrichment activities (e.g. extra-curricular, tutoring, after-school programming) and at what levels. 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 expect that accounting for supplementary spending will alter our assessment of the equity of schooling provision. The PSID therefore offers a rich basis for understanding the conditions (state, district and family level) under which benefit driven financing in public education may flourish.

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.

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 spending is 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.

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,

1Evans, 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).

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 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. 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) finds no relationship between satisfaction with local schools and the likelihood of hiring private tutoring in a survey of Canadian parents.

These mixed results on the relationship between public school quality and supplementary spending could signal a weak relationship. 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

2Spending 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).

demand high levels of education provision could sort into high spending districts 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 spending 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.

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 bn]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,

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

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 this draft, we turned to the PSID's Child Development Supplement (CDS) to locate families in school districts. The CDS, which was started in 1997, was designed to provide more detailed information on the lives of children in PSID families. The restricted-use version of the CDS includes information on the school districts of the children in the survey at three points in time, 1997, 2002, and 2007. We matched the children present in the CDS in 1997 to their families. The families that included children in the CDS were thus placed in school districts at three points in time. We filled in locations in other years using PSID family reports on moving behavior. For example, if in 1999 a family reported that they had not moved since the previous interview (or the Spring of 1998), that family was assigned the same school district location as they had in 1997. Using this process, we filled in locations for CDS families from 1999 to 2011.[4] We were left with a sample that included 706 families in 1999, 562 families in 2001, 930 families in 2003, 729 families in 2005, 722 families in 2007, 521 families in 2009, and 433 families in 2011.[5]

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

4If a family indicated they had moved in between times when we had precise locations, we set their location to missing.

5In most years, we were observing between five percent and twelve percent of the PSID families. Since CDS participants were chosen randomly, this should not create nonrandomness. Nevertheless, our future plan is to place all PSID families in school districts so as to expand our sample and rule out any possible nonrandomness created by omission.

Center for Education Statistics' Common Core of Data (CCD). The financial data on school districts are collected by the Census Bureau using the F-33 survey. 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 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.[6] 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

6Lafortune, 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.

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.[7]

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. They differ little, since the CDS participants were drawn disproportionately from the regular, as opposed to the low income, sample. 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 Furstenberg (2013).[8] 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.

Models and Methods

To estimate the link between changes in public school spending and family spending

7In 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.
8All dollar figures are inflation adjusted to 1990 dollars using the CPI-U.

decisions, we estimate models of the form

$$y_{idt} = \alpha + \beta Spend_{dt} + X_{it}\delta + Z_d t\gamma + Q1_d t\theta_1 + Q2_d t\theta_2 + Q3_d t\theta_3 + \tau_t + \eta_i + \varepsilon_{idt}\ , \quad (1)$$

where y_{idt} is a measure of spending by family i residing in district d in year t. The variable $Spend_{dt}$ measures per pupil expenditures by district d in year t, and X_{it} are time-varying family attributes. To control for district demographics that could influence both district spending and the spending choices of families, we follow Brunner, Hyman, and Ju (2017) and interact district demographic measures (Z_d) in 1980 with a time trend.[9] 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, per pupil spending is 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), who show that lower income districts benefit relatively from finance reforms. As a result, our first stage is

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

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

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.

The results of estimation of this first stage are given in Table 2.[10] While, as expected,

9Since the Decennial Census includes no district enrollment information, we use enrollment measures from the 1987 Census of Governments.

10Appendix Table 1 gives the first stage estimates when we include pre-1989 reforms in

spending in the second and third quartiles increases relative to the omitted forth 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. Similar trends are not evident for second and third quartile districts. Thus, even non-reform state 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. Finally, the value of the Cragg-Donald F statistic suggests that any small-sample bias attributable to weak instruments is limited.[11]

Results

Table 3 provides estimates of equation (1). In the first two columns are the ordinary least squares estimates; instrumental variables estimates are in the final two columns.[12]

Most of the estimates differ little between the first two and the last two columns. Public education expenditures are never a significant determinant of supplementary spending. But the direction of the estimated effect of public spending switches signs from positive to negative, consistent with the potential bias suggested above. And the magnitude of the effect implied by the instrumental variable estimate is large, suggesting supplementary spending would increase by more than a dollar for each dollar reduction in public spending. Nevertheless, the estimates do not support the conclusion that supplementary spending mitigates the impact of equity-enhancing finance reforms.

The other estimated effects generally match expectations. Families with higher incomes

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defining SFR.

11Mayoral (2015) notes that the Cragg-Donald statistic is only suggestive when clustered standard errors are used.

12Appendix 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.

spend more on supplementary education, though the magnitude of the effect is small. Each additional child reduces supplementary spending by about $800. Families with a spouse present spend about that same amount more than do single parent households, possibly because the logistics of 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 substantial (over $1100) drop in supplementary spending. Kornich and Furstenberg (2013) show that supplementary spending spikes up near the end of high school and then drops sharply. This estimate could be picking up that fall off, which would coincide with a child's departure from the household.

Families who live in school districts subject to TELs spend substantially more, an added $1300 to $1500), on supplementary education. This result 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, this increase in supplementary spending is less surprising. TELs matter, even if they have little impact on spending levels.

Our model does little to explain variation in childcare expenditures. And the only consistent result, that childcare expenditures drop off when families reside in districts subject to TELs, is difficult to explain. The absence of parallels to the results for supplementary education spending is, however, notable. Decision making around childcare appears very different from that around supplementary education spending. That may not be surprising since, for most families, childcare decisions are driven by parental work decisions (National Academies, 2018).

The choice to participate in the shadow education system tends to be motivated by parental efforts to improve their children's educations and the likelihood that those children are admitted to a more highly ranked college or university (Park, et al, 2016).

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 and that there could be large increases in families' supplementary spending on education in

response to reductions in that public spending. But we cannot rule out the possibility that supplementary spending is unresponsive to changes in public education spending.

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. And our results suggest that the determinants of supplementary spending are very different from those for expenditures on childcare.

The Panel Study of Income Dynamics, which has been shown to be a good source of information on supplementary education (Heung and Yeung, 2015; Bouffard, et al, 2006), seems to be particularly appropriate for analyzing the relationship between supplementary spending and public provision. We have only begun exploring what can be done with these data; expanding our data to include more families is a first step in reefing our answers. In addition, we can explore whether families' perceptions of the quality of public schools are based on per pupil spending. Since other research has shown that the composition of the teaching staff (Brunner, Hyman, and Ju, 2017) and even test scores (Lafortune, Rothstein, and Schanzenbach, 2016) are affected by finance reforms, our methods will allow us to determine if families use information other than public spending in making their own spending decisions. Thus, the nature of the link between public provision and private spending on supplementary remains an open question.

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Table 1
Summary Statistics

Variable	Mean (unweighted)	Mean (weighted)	Standard Deviation (weighted)	Standard Deviation (weighted)	Minimum	Maximum
Supplementary Education Expenditures	1085.14	1436.21	3599.31	4396.36	0	64955.41
Childcare Expenditures	250.93	265.32	895.67	961.42	-196.76	10198.61
Family Income	52284.13	61241.20	63720.68	75372.86	-72117.48	1490736
Number of Children in the Family under 18	1.74	1.70	1.19	1.18	0	11
Moved Since Last Survey	0.049	0.042	0.215	0.200	0	1
Spouse Present in the Family	0.705	0.775	0.456	0.418	0	1
Family Composition (other than Head or Spouse) Changed Since Last Survey	0.751	0.778	0.432	0.415	0	1
Per Pupil Expenditures	5427.06	5557.11	1421.44	1599.08	2913.89	15787.57
Number of Observations	4055					

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

Variable	Coefficient
Interaction of school finance reform with first quartile of 1980 income distribution	-169.5949 (124.8622)
Interaction of school finance reform with second quartile of 1980 income distribution	413.5666** (174.1283)
Interaction of school finance reform with third quartile of 1980 income distribution	398.9827*** (152.1441)
District subject to TEL	119.4548* (64.3838)
Family Income	-0.000396* (0.000214)
Number of Children in the Family under 18	-13.5286 (18.9226)
Moved Since Last Survey	-73.7662 (65.2595)
Spouse Present in the Family	40.7462 (81.5306)
Family Composition Changed Since Last Survey	-4.4390 (19.7406)
Interaction of trend with 1987 enrollment	0.0000445 (0.0000565)
Interaction of trend with 1980:	
Fraction high school (and not college) graduates	-35.6834 (92.4724)
Fraction college graduates	1.7940 (85.6006)
Per capita income	0.01232** (0.004887)
Fraction of population living in poverty	45.8646 (38.1177)
Fraction of population Black	134.045*** (36.8547)
Fraction of population Native American	337.7139*** (125.1774)
Fraction of population Asian American	-230.6562* (128.5466)
Fraction of population Hispanic	-63.1428 (40.9987)
District in first quartile of 1980 income distribution	78.8746*** (17.5027)
District in second quartile of 1980	14.0502

income distribution	(14.4031)
District in third quartile of 1980 income distribution	8.5470 (13.7410)
Number of observations	4093
Number of families	1213
Cragg-Donald F-statistic	12.07
Stock-Watson F-statistic	5.09

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' Education-Related Expenditures[1]
(Standard Errors based on Clustering by Family in Parentheses)

Variable	Ordinary Least Squares Results		Instrumental Variables Results
	Supplementary Education Expenditures	Childcare Expenditures	Supplementary Education Expenditures
Per Pupil Expenditures	0.3837 (0.2720)	0.4509* (0.2640)	-1.4816 (1.2995)
District subject to TEL	1225.121* (667.2136)	-186.143** (81.0894)	1447.78** (687.3115)
Family Income	0.005167* (0.002906)	0.0002657 (0.000305)	0.004406 (0.002906)
Number of Children in the Family under 18	-793.4148*** (126.6951)	-11.0425 (28.6088)	-816.7775*** (141.956)
Moved Since Last Survey	84.6697 (246.3343)	82.8779 (63.6466)	-41.5349 (273.3485)
Spouse Present in the Family	840.0137** (359.9155)	89.1159 (107.9114)	864.9048** (389.3507)
Family Composition Changed Since Last Survey	-1156.006*** (214.2337)	23.2345 (28.7351)	-1167.589*** (212.9122)
Observations	4386	4386	4093
Families	1506	1506	1213
F	4.07	4.13	4.01

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[1]
(Standard Errors based on Clustering by Family in Parentheses)

Variable	Coefficient
Interaction of school finance reform with first quartile of 1980 income distribution	-201.5014 (220.3421)
Interaction of school finance reform with second quartile of 1980 income distribution	756.6625*** (219.2335)
Interaction of school finance reform with third quartile of 1980 income distribution	491.2153** (204.5284)
District subject to TEL	113.9914* (64.9772)
Family Income	-0.0003762* (0.000213)
Number of Children in the Family under 18	-14.0896 (19.5157)
Moved Since Last Survey	-67.5030 (65.5968)
Spouse Present in the Family	11.4093 (83.1228)
Family Composition Changed Since Last Survey	-6.8180 (19.5748)
Interaction of trend with 1987 enrollment	0.0000507 (0.0000556)
Interaction of trend with 1980:	
Fraction high school (and not college) graduates	-32.9625 (92.1921)
Fraction college graduates	-11.1680 (86.6499)
Per capita income	0.01274*** (0.004916)
Fraction of population living in poverty	37.0203 (38.6250)
Fraction of population Black	138.9654*** (37.1958)
Fraction of population Native American	373.6894*** (129.2358)
Fraction of population Asian American	-194.6981 (135.3691)
Fraction of population Hispanic	-66.9666* (40.6685)
District in first quartile of 1980 income distribution	78.9786*** (18.8039)
District in second quartile of 1980	8.6670

income distribution	(14.5916)
District in third quartile of 1980 income distribution	7.7846 (14.4948)
Number of observations	4093
Number of families	1213
Cragg-Donald F-statistic	17.451
Stock-Watson F-statistic	5.81

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)

Variable	Instrumental Variables Results
	Supplementary Education Expenditures
Per Pupil Expenditures	-0.1637 (1.1661)
District subject to TEL	1290.457** (674.917)
Family Income	0.004944* (0.002925)
Number of Children in the Family under 18	-800.2703*** (133.1467)
Moved Since Last Survey	-47.6364 (261.3701)
Spouse Present in the Family	847.3701** (363.0491)
Family Composition Changed Since Last Survey	-1159.405*** (213.2176)
Observations	4093
Families	1213
F	4.07

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 .