When Schools Spend Less, Do Families Spend More?

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When Schools Spend Less, Do Families Spend More? The Responsiveness of Supplementary Education Spending to Changes in the Local Schooling Context

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Abstract: This working paper explores the relationship between local public education spending, local public school demographics, and family spending on supplemental education or private schooling. Basic to its analysis are data on school spending from the National Center for Education Statistics as well as data on family spending from the Panel Study of Income Dynamics. The analysis finds that spending on supplemental education or private schooling is not significantly related to local public education spending. However, such spending is significantly related to the racial/ethnic composition of school districts. Where school districts are more racially and ethnically diverse, families spend more on supplemental education or private schooling. This result is driven by the families with the highest spending, who are paying tuition for private schooling.

NEPC Topic Search Terms (these terms may be used to find this working paper in the NEPC website publication archive): School Finance and Funding; School Segregation; Diversity; Private Schools; Privatization.

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Introduction

Research points to a wide array of reasons that motivate families to invest in their children's education, including status attainment, social class reproduction, advancement in higher education, and academic remediation (Vincent and Ball, 2007; Smyth, 2015; Roska and Potter, 2011). However, researchers have struggled to understand the intricacies of family decision making about their children given the variety of environments in which families reside. For example, Rabe (2019) notes that scholars know little about why parents choose to supplement or substitute for the traditional, publicly available schooling option and the degree to which conditions in the public schools interact with family characteristics to elicit such substitutions. Changes in the demographics of a school district or changes in that school district's spending may stimulate families to act but may do so differently across family types and income levels. In developing our focus on the determinants of supplementary spending on education—which for the purposes of this paper we define as spending on substitutes for or complements to traditional public education—we explore how the local public schooling context influences family spending behavior.

Specifically, we estimate the links between supplemental spending by families, local public education spending, and local public school demographics. We build on a limited body of research on U.S. families with children to examine connections between public school conditions and family spending on supplemental educational services for their children. We combine data on local school districts drawn from the NCES Common Core of Data with data from the Panel Study of Income Dynamics (PSID) to examine the extent to which families

adjust their spending on education activities for their children in response to K-12 spending and the racial/ethnic composition of the local public schools.

Measuring the direct influence of public school expenditures and context on family spending is a difficult challenge because unobserved attributes of families that influence where they live might also affect their supplemental spending choices. Jackson et al. (2016), Lafortune et al. (2018), and Brunner et al. (2020) 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 also show that these school finance reforms can serve as valid instruments for the racial/ethnic composition of school districts, allowing us to estimate the causal link between a family's supplemental spending and the racial/ethnic composition of the public school district in which that family resides.

We find that families spend more when they reside in districts with higher fractions of minority students. That is, the proportion of minorities in school districts serves as a signal, of some sort, in the family-spending calculus. Again, this suggests that perceptions matter. This finding parallels results in the literatures about public school choice (e.g., Bifulco et al., 2009; Ladd and Turaeva, 2020), public-private choice (e.g., Lankford and Wyckoff, 1992), and hedonics (e.g., Downes and Zabel, 2002). But our results suggest that family decisions to spend on enrichment education is independent of local public education spending. Whether schools spend more or less seems to have little effect on parent choices on supplemental education.

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 send some or all of their children to private schools. We suspect this private school linkage partly because of the parallels between our findings and findings in the public-private choice literature and partly because of the results when we limit the sample to families with spending levels below the average tuition for Catholic schools.[1] Our findings thus suggest some families' decisions about supplementary spending may be sensitive to perceived public school quality, but that families who are paying for tutoring or lessons do not appear to be driven by a desire to maintain relative advantage.

The following section provides context for our analysis of supplementary spending. We then describe the data we use, followed by an overview of the models we estimate. Because we are attempting to establish the causal relationship between families' spending on supplemental education for their children and local conditions, we also describe the steps we take to address the possibility that public education levels 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

Review of the Literature

Scholarship about the determinants of supplementary spending in education has been historically focused on why parents spend on substitutes for traditional public schooling. A large body of literature in this area has examined parental choice and preferences for private education (Lankford and Wyckoff, 1992; Egalite and Wolf, 2016), selection of charter schooling (Epple et al., 2016), and even instances of housing mobility associated with schooling choices (Lareau and Goyette, 2014; Brasington, 2021). Each of these spending choices seeks to *supplant* formal education and tends to involve large expenditures and commitments of family resources, generally in response to the push and pull of school quality concerns. What is less studied are parental investments (e.g., time or money) in *supplemental* education activities outside of formal schooling. These can include enrichment instruction (e.g., music or language lessons), academic tutoring, or physical development (e.g., sports teams and coaching). Rabe (2019) notes that scholars know very little about why parents choose to substitute for formal schooling and the degree to which conditions in the schools interact with family characteristics to elicit such substitutions. Conditions like changes in the demographics of a school, staffing levels, or budget constraints may stimulate families to act, but may do so differently across family types and income levels.

Our empirical approach to understanding why families make economic investments in their children is based on the model in Todd and Wolpin (2003), which they use to show that family input decisions might be responsive to schooling inputs. Further, they observe that (2003, F19) "[e]conomic models in which parents care about a child's cognitive development imply that the amount of resources allocated to the child, in the form of purchased goods and parental time, will be responsive to the parent's perception of a child's ability".

The discussion in Todd and Wolpin is not specific about the nature of either family or schooling inputs. Schooling inputs could mean traditional measures, such as per-pupil spending, class size, and teacher quality, but they could also encompass the broader environment of schools like demographic conditions (Allen and Fraser, 2007).

There are also important linkages between family spending and residential choice. As was noted above, families often choose where to live based on available schooling and likely make time and spending decisions based on the choice of schooling. One of the challenges apparent from the Todd and Wolpin discussion is that identifying the relationship between schooling and family inputs is hard because schooling inputs are likely to be endogenous. The literature on time allocation, reviewed by Rabe (2019), includes a growing number of papers that provide plausibly causal estimates of how family time allocations respond to variation in schooling inputs. Much of the recent work takes advantage of the revelation of new information to quantify parental responses to school quality. Both Dizon-Ross (2019) and Bergman (2021) establish that parents' perceptions of their children's effort and ability are typically inaccurate. When improved information is provided, parents increase their

monitoring (Bergman, 2021) and alter their educational investments (Dizon-Ross, 2019). Dizon-Ross also establishes that responses are heterogeneous by income because of heterogeneity in information availability by income. While both Bergman and Dizon-Ross establish the influence of information on parental decisions, in the situations they analyze, the type of information available to parents differs from the type of information available to parents in traditional schooling contexts.

Recent work looking at revelation of test scores and of objective measures of school quality produces results that provide clearer benchmarks for the analysis in this paper. Both Cobb-Clark et al. (2021) and Greaves et al. (2021) leverage variation in the timing of interviews in longitudinal surveys and the reality that the timing of those interviews does not align with the release of administrative data. Cobb-Clark, et al. look at how families in the Growing Up in Australia survey respond to test score information, taking advantage of the fact that some families receive test score information before they are interviewed. They find that tutoring activity increases, and participation in extracurriculars declines, among families who know test scores before the interview, relative to those who do not have that information. While this suggests families' spending and time allocation decisions are sensitive to student performance, Cobb-Clark, et al. also find that changes in tutoring and extracurricular participation occur independent of whether the family receives "good news" or "bad news" (Cobb-Clark et al., 2021, 3). Since test scores in Australia are released at the same time to all families, this latter result suggests they may actually be establishing that, independent of new information, through the academic year, tutoring expenditures increase and extracurricular participation declines.

The timing of the release of school ratings that are the new information used by Greaves, et al. (2021) varies through the academic year, strengthening their identification strategy. However, the authors have no information on families' supplemental spending. They are able to establish that parental time allocated to homework assistance declines when school ratings are better than expected, suggesting that family and school inputs are substitutes.

Chan (2022), who looks at how families respond to gifted and talented identification, generates results that suggest that responses in the realms of parental time and spending might not necessarily move in the same direction. Chan finds that, after identification, non-minority parents are less likely to provide homework help but are more likely to pay for tutoring. Minority parents, on the other hand, increase both homework help and paid tutoring post-identification.

In summary, these recent papers suggest that, while parental time allocations may be responsive to information about their children and the schools those children attend, the direction and magnitude of the responses is still unclear. Even less is known about how supplemental spending decisions respond, with almost no information about the extensive margin (Rabe 2019).[2] The recent literature on the growth of shadow education and parental spending on supplementary education offers some indirect evidence about how family spending responds, suggesting that higher income families living in districts in which relative spending on education has declined have increased their own supplementary spending. Kornrich and Furstenberg (2013) show that, from 1972 to 2007, spending on children grew more rapidly in the top than the bottom income deciles.[3] And that growth was driven by

increases on spending on education and childcare. But this simple correlation between the growth of supplementary spending by high- income families and the imposition of finance reforms cannot support the conclusion that this growth is a response to constraints on public spending in previously high-spending districts.

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 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, in a survey of Canadian parents, Davies (2004) sees no relationship between satisfaction with local schools and the likelihood of hiring private tutoring.

Davies' results on the relationship between public school quality and supplementary spending could signal a weak relationship in developed country contexts. Alternatively, supplementary spending 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 than average levels of supplementary spending.[4] Cross-sectional regressions like those of Dang (2007) and Davies (2004) are likely to tell us little about how families will respond to changes in school quality following finance reforms.

Fiscal signals are unlikely to be the only determinants of supplementary education spending. As Schneider et al. (2018) and Lunn and Kornich (2017) suggest, more affluent families' attitudes towards supplementary spending could differ from those of their less affluent counterparts. Since familial spending contributes to the perpetuation of advantage (Potter and Roksa, 2013), attitudinal differences or responses to public efforts to equalize opportunities could preserve existing wealth inequality. Schneider et al. (2018) use the Consumer Expenditure Survey (CEX) to explore how inequality in income begets inequality in supplementary spending. These findings align with the results of Dizon-Ross (2019) and Chan (2022) which suggest that, if spending does respond, the nature of the responses varies with family income.

Our Identification Strategy

One major challenge to establishing the causal link between public school conditions and families' supplemental spending is available data. The surveys that have been used in the papers mentioned above typically "lack measures of parents' other inputs such as monetary investments in their children" (Rabe, 2019, 7). But surveys like the PSID do not lend themselves to the identification strategies described above.

We address the challenge in isolating the causal link between public spending on education and supplementary education spending by families in two ways. First, since we use reports

on family education expenditures from families in the PSID, we can take advantage of the panel nature of the data to control for temporally stable unobservables by accounting for family-specific fixed effects. Second, we can draw on a growing literature (Jackson et al., 2016; Jackson and Johnson, 2017; Lafortune et al., 2018; Brunner et al., 2020) 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 and the racial/ethnic composition of the local schools, and thereby explore the independent effect of local conditions on the supplementary spending behavior of families. 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 expenditures in 1999. The variable we use is total family education expenditure, which combines school-related expenses and other schooling expenses. Li et al. (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; Kornrich and Rodriguez, 2016). To preserve as many observations as possible, we use the imputed values of supplementary education expenditures and family income, though Li et al. (2010) 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 place families in school districts in order 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 include their block of residence using the Census Bureau's Block Assignment file for 2010. Every family living in a K-12 district was 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 district. 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 dropped from the estimation sample all families with college-age children. Since our measure of supplementary education expenditures included 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 the school districts in which they lived. Data on school districts were drawn from the National Center for Education Statistics' Common Core of Data (CCD) and the Stanford Education Data Archive (Reardon et al., 2019). The CCD proved data on district spending, staffing, and student

demographics. We combined these financial data with demographic data from the 1980 and 1990 Decennial Censuses and 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, 2014; Jackson et al., 2016; Lafortune et al., 2018; Brunner et al., 2020) 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 (e.g., Lafortune et al.), we have chosen to use the reform timings of Brunner, et al. 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, et al. show that the finance reforms are effectively exogenous with regard to per-pupil expenditures, they serve as the basis of our identification strategy.

Brunner, et al. (2020) do not establish that finance reforms are effectively exogenous with regard to community composition. And yet community composition could well be affected by finance reforms. While the research on the impact of finance reforms on community composition, such as Aaronson (1998), Chakrabarti and Roy (2015), and Zabel and Zuckerman (2018), has tended to focus on sorting by income, the policies will also affect the racial/ethnic composition of school districts directly, because the link between residential choice and a community's racial/ethnic composition is so strong (Downes and Zabel, 2002; Zabel and Zuckerman, 2018). That sensitivity of residential and schooling choices to the racial/ethnic composition of the schools also means that, for the same reason that per-pupil spending is endogenous, the racial/ethnic composition of the schools will be endogenous. But, since finance reforms are likely to directly affect community composition, we could use the timing of finance reforms to instrument for racial/ethnic composition if we can establish that the finance reforms are effectively exogenous.

Figure 1 plots the estimates of non-parametric event study models we estimated to check the validity of the parallel trends assumption that must hold in order to treat the finance reforms as effectively exogenous.[5] For none of the income distributions is there evidence of pre-trends. Individually, the pre-reform coefficients are insignificant. Jointly, none of the sets of pre-reform coefficients are significant at the five percent level, with the largest p-value for the joint test being 0.0652 for the fourth quartile. Visually, the fourth quartile shows no evidence of a pre-trend. Thus, the finance reforms are effectively exogenous with respect to the racial/ethnic composition of school districts.

Figure 1 suggests that the finance reforms had no impact on the fraction of students who were Black or Hispanic in districts that had been in the first three income quartiles in 1980. The fraction minority in districts in the fourth income quartile fell after the finance reforms; a finding that aligns with Chakrabarti and Roy's (2015) results for Michigan. Sensitivity to peer effects seems to have accentuated sorting post-reform.

Table 1 includes summary statistics on the key covariates in our analysis.[6] We present both weighted means, using the family weights given in the PSID, and unweighted means.[7] Between 1997 and 2019, mean spending per school-aged child on supplementary education

fell, going from $1800.72 to $1504.40 for families in our sample. This contrasts with the trends described by Kornrich and Furstenberg (2013).[8] By 2019, 66.7 percent of the families resided in school finance reform states according to the Brunner, et al. (2020) classification. 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 15.15. The percentages in the second and third quartiles are 17.98 and 21.67, respectively.

We cannot report minimum and maximum values of supplementary education expenditures, but we can note that about 49 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.[9]

We also note that the maximum value is large, highlighting that 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 et al., 2018). To limit our analysis to families who are unlikely to be paying for private school, we generate estimates restricting our sample to all families in each interview year with spending below $5000 in 2017 dollars. The average tuition of private schools exceeded $5000, in real terms, throughout our sample period.[10] As a result, it is likely that most families with spending below $5000 were not paying for private school.

Models and Methods

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

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

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 current expenditures[11] 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, et al. (2016) and Brunner, et al. (2020) and interact district demographic measures (Z_d) in 1980 with a time trend.[12] 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,[13] family fixed effects, and a random error term. In our estimation, we cluster by family.

As we asserted above, school district provision and student demographics are likely to be endogenous. 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. By interacting SFR with the quartile dummies, we are drawing on the work of Lafortune, et al. (2018), who show that lower income districts benefit relatively from finance reforms.

Table 2 gives the results of estimation of this first stage. 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. This result differs from first-stage results in papers like Brunner, et al. (2020). In fact, the result differs from what we get when we use all districts in the CCD and limit the sample to fiscal years that match with the PSID data.[14] This suggests that our counter-intuitive first-stage estimates result from the fact that the school districts represented in the PSID are a nonrandom sample of all districts in the U.S. The coefficients on the trend for the first quartile districts hint at the nature of our sample and align with other results in the literature. The trend on spending of all first quartile districts, even those in non-reform states, are far stronger than the trends for the second and third quartile districts. This finding aligns with the results of Shores et al. (2022), who also find considerable heterogeneity in the extent to which finance reforms raised spending in low-income districts, with spending losses in low-income districts in a number of school finance reform states. Our sample, it appears, overrepresents the finance reform states in which low-income districts experienced spending losses.

All that said, what matters for the validity of our second-stage results is the randomness of our sample of *families* and the strength of our first stage. The PSID is known to be a good sample. The strength of our instruments is less clear; the Kleibergen-Paap statistics in Table 2 are 1.36 and 0.96. However, in all but one of the first-stage regressions, the coefficients on two of the three instruments are significant at the 10 percent level and, as is evident in Table A1 in the Appendix, these instruments work well for the full CCD. Thus, we view the instrumental variable estimates below as suggestive evidence of the possible endogeneity in the determination of supplementary spending of public school spending and racial/ethnic composition.

Results

Table 3 provides estimates of equation (1) when we control for local public school spending and fraction minority. In the first two columns are the ordinary least squares estimates; instrumental variables estimates are in the final two columns.[15]

In Table 3, 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. The coefficient has the expected negative sign when we account for potential endogeneity (column 3), though the coefficient is not significant. This latter result suggests biased estimates due to endogeneity along the lines we discussed. But, when we limit the sample to families with spending less than $5000, the estimated effects in both the ordinary least squares and instrumental variables specifications are positive but insignificant. There-

fore, since even the single negative estimate is insignificant, we have weak evidence for the conclusion that the OLS estimates are biased. Taken as a whole, the estimates do not support the conclusion that supplementary spending is particularly responsive to equity-enhancing finance reforms.[16]

That conclusion is clouded when the results take into consideration the fraction of minority students in a district. 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 10 percent as large 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, these results suggest that the decision to choose private school is particularly sensitive to the racial/ethnic composition of the local school district. Other supplementary spending seems to be less sensitive to district demographics with a 10-percentage point increase in the fraction minority translating into about a $25 increase in supplementary spending. Nevertheless, all the estimates support the conclusion that growth in the fraction minority in the public schools leads some families to increase their supplementary spending, with the largest disequalizing responses occurring because of the decision of a few families to opt towards private sector education.[17]

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 per child by about $480. When we limit the sample to families spending less than $5000, families with additional school age children appear to increase supplemental spending per child by about $160, suggesting that families may seek to maintain some intra-family equity in spending. Heads with a spouse present spend more than do single parent households, possibly because the logistics of private schools, tutoring, and a child's participation in other supplementary education programs are easier when two adults are present, though the differences are never significant.

Family composition changes that include new marriages with an additional adult present (male or female) result in a significant increase in supplementary spending. These findings suggest that the family composition changes that solidify the relationships of adults in the household, or that result in a remarriage, are strongly associated with additional investments in children. Given the controls associated with income in the models, these finding suggest that there may be additional capacity allowing new attention to be focused on household children.

Families that moved since the last survey have lower levels of supplementary spending, about $65 less when we restrict the sample to families spending less than $5000. We explored this result further by estimating separate models for movers and stayers; none of the coefficients differed substantively between the models. This suggests that the reduction in spending is linked to the dislocation of the move, since movers and stayers respond in the same ways to changes in the characteristics of the public schools.

The full sample estimates indicate that families who live in school districts subject to TELs spend between $200 and $300 more on supplementary education, though the estimates are

not significant. This result is a bit surprising since it occurs over and above any direct impact of the TEL on local spending. If short run response of some families to the imposition of a TEL is to choose private school, this increase in supplementary spending is less surprising. The pattern of changes in private schooling in California in the aftermath of Proposition 13 fits this pattern, for example. Downes and Schoeman (1998) find substantial increase in private schooling in the immediate aftermath of Proposition 13.

While supplementary spending is more prevalent in Asian countries, the inclusion of fixed effects meant that we could not ask whether Asian-American families spend more, all else equal. We did look at whether the determinants of supplementary spending were different for Asian-American families. The estimates for Asian-American families aligned with the results in Table 3. But the relatively small number of Asian-American families in the PSID was likely a barrier to isolating any differences that might exist.[18]

As a further check on our results, in Table 4 we include reduced-form estimates. These estimates suggest any changes in supplemental spending attributable to finance reforms occurred primarily in districts in the bottom income quartile, districts that should have been among the primary beneficiaries of the reforms. And the effects are only evident when we do not limit supplemental spending, suggesting that the finance reforms might have led to increases in private schooling in the lowest income districts. We discuss potential explanations for this result below.

Concluding Remarks

Recent work by Sund (2023) uses experimental evidence to show that "a substantial share of parents (35.0%) are willing to forgo the principle of equal opportunities by helping their child in [a] competition even when they know it is at the expense of another child's opportunity to succeed" (p. 3). Is this just an extreme example of the willingness of parents to improve the relative standing of their children? And, outside of the lab, is that willingness sensitive to the perceived conditions in the local public schools and the peers in them? Our analysis provides evidence on sources of this willingness by examining the determinants of families' supplemental spending on education, with a focus on the sensitivity of that spending to per-pupil spending and the fraction of students who are African-American and Hispanic in the local public schools.

To analyze the links between supplementary spending and spending on the local public schools, we merged 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 and the racial/ethnic composition of the public schools, we instrumented for those variables using the varied timing across states of school finance reforms, which has been shown to be exogenous (Jackson et al., 2016; Lafortune et al., 2018; Brunner et al., 2020).

Our estimates provide mixed evidence on whether instrumenting for public spending on education is necessary. Further, we cannot rule out the possibility that supplementary spending is unresponsive to changes in public education spending. Supplementary spending does

seem to be sensitive to the racial/ethnic composition of local school districts, with the pattern of the estimates suggesting more families choose private school when the share of the student population in the public schools that is minority is higher.

Reduced-form estimates that relate supplemental spending and our instruments, the interaction between a school district's income quartile in 1980 and whether that district was located in a state with a school finance reform, suggest that families residing in the districts in the lowest income quartile may have been most sensitive to the changes occurring in their public schools. One possible explanation, suggested by Figure 1 and by the reduced-form estimates and the instrumental variable estimates, is that the fraction minority in the school district might have become more cogent in the aftermath of finance reforms. That is plausible, since school finance reforms focus increased attention on the quality of services provided to disadvantaged students. A second possibility is that the finance reforms might have induced some more affluent families to choose to move to or remain in districts that benefitted from the reforms, even while those families chose private schooling for their children. That explanation is consistent with the findings of Pearman, II (2020), who shows that schools in gentrifying areas experience enrollment decline with no change in the share of students who are white. That is true even in areas where the gentrifiers are white, because 'gentrifying White households opt out of the neighborhood schools' (Pearman, II 2020, 209).

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.

Endnotes

1 Using the Consumer Expenditure Survey (CEX), Vinson (2021, p. 10) observes that "only 2.4 percent of households that spend less than $1,000 are sending a child to private school."

2 The one apparent exception is Vinson (2021), who argues that familial spending appears to complement public sector spending. However, Vinson uses state-level spending, thus missing much of the important variation. Further, he only finds complementarity when he accounts for the probability that the family attends private school. But he uses a function of family spending to predict the probability of private school attendance. Including this function of the dependent variable as a control variable necessarily introduces bias in the estimates.

3 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).

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

5 We estimated these models using the full Common Core of Data, the same data set that underlies the estimates in Brunner, Hyman, and Ju (2020), and allowed the effects to vary depending on whether the district fell in the first, second, third, or fourth quartile of the state's cross-district distribution of per capita income in 1980. We censor at 7 years prior and 10 years after a reform.

6 One of our covariates is an indicator of whether a district is subject to a tax or expenditure limit (TEL). We use the process outlined in Downes and Killeen (2014) to identify the presence of TELs.

7 All of the estimates presented below are from unweighted models, since weighting is typically neither necessary nor appropriate when the variables that are the basis for oversampling are included in the regression (Wilson, 2000). That said, we have estimated weighted versions of our models. Those results align with the results presented below.

8 All dollar figures are inflation adjusted to 2017 dollars using the CPI-U.

9 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.

10 The Digest of Education Statistics was our source of information on average private school tuition.

11 We also explored the pupil-teacher ratio and 5th-grade math test scores as alternative measures of perceived public school quality. We chose 5th-grade math scores, drawn from the Stanford Education Data Archive, to maximize the number of included observations. When we estimated specifications that replaced per-pupil spending with either pupil-teacher ratio or 5th-grade math score, our instruments were weak. As a result, we do not report those results.

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

13 While we do not report the year effects, it is worth noting that there is no clear pattern in the estimated year effects. When we impose no restriction on the level of supplemental spending, spending is higher in years leading up to recessions, but the recession years did not differ significantly from 1999, the reference year. When we restricted supplemental spending to less than $5000, there was no pattern in year effects.

14 These results are included in Appendix Table 1.

15 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.

16 We have estimated all of our specifications with instructional expenditures per pupil replacing current expenditures per pupil. None of the substantive conclusions change when we do this.

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 ethnicity of the head. The interaction was never significant.

18 We also explored controlling for the fraction of students in the district who are Asian American, with an eye toward exploring the possibility of peer effects in spending. The coefficient on the fraction Asian American in the school was never significant.

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Table 1

Summary Statistics

Variable	Observations	Mean (unweighted)	Standard Deviation (unweighted)	Observations (weighted)	Mean (weighted)	Standard Deviation (weighted)
Supplementary Education Expenditures	33014	1653.83	6054.17	32646	2389.91	8132.745
Family Income	33014	87593.05	121983.10	32646	107090.00	150042.10
Number of School Age Children in the Family	33014	1.81	0.96	32646	1.777	0.918
Moved Since Last Survey	33014	0.353	0.478	32646	0.289	0.453
Spouse Present in the Family	33014	0.632	0.482	32646	0.711	0.453
Family Composition (other than Head or Spouse) Changed Since Last Survey	33014	0.401	0.490	32646	0.340	0.474
Family Resides in a Town (NCES designation)	33014	0.088	0.284	32646	0.095	0.294
Family Resides in a Rural Area (NCES designation)	33014	0.161	0.367	32646	0.169	0.375
Per-pupil Current Expenditures	33014	11338.23	3768.59	32646	11384.78	3851.48
Fraction Minority	33014	0.442	0.320	32646	0.351	0.305
District Subject to TEL	33014	0.790	0.407	32646	0.841	0.366

Note: Weights are missing for 368 of the observations.

Table 2

First-Stage Estimates for Per-pupil Current Expenditures and Fraction Minority

	No limit on supplementary educational expenditures		Supplementary educational expenditures < $5000	
	Log of Per-pupil Expenditures	Fraction minority	Log of Per-pupil Expenditures	Fraction minority
Interaction of finance reform with 1980 first quartile indicator	0.0098	0.0267***	0.0094	0.0262***
	(0.0233)	(0.0066)	(0.0230)	(0.0067)
Interaction of finance reform with 1980 second quartile indicator	0.0713***	0.0018	0.0688***	0.0015
	(0.0269)	(0.0091)	(0.0263)	(0.0093)
Interaction of finance reform with 1980 third quartile indicator	0.0634***	-0.0304**	0.0664***	-0.0325***
	(0.0121)	(0.0116)	(0.0124)	(0.0124)
District subject to TEL	0.0314***	-0.0291***	0.0314***	-0.0316***
	(0.0113)	(0.0085)	(0.0116)	(0.0086)
Interaction of trend with 1987 enrollment	2.44e-08***	-1.93e-08***	2.40e-08***	-2.04e-08***
	(5.26e-09)	(4.77e-09)	(5.49e-09)	(5.32e-09)
Interaction of trend with 1980:				
Fraction high school (and not college) graduates	0.0134***	-0.0015	0.0132**	-0.0023
	(0.0052)	(0.0033)	(0.0053)	(0.0035)
Fraction college graduates	0.0057	-0.0172***	0.0055	-0.0174***
	(0.0063)	(0.0040)	(0.0065)	(0.0042)
Per capita income	-6.68e-07**	5.89e-07***	-7.19e-07**	6.57e-07***
	(3.06e-07)	(2.10e-07)	(3.21e-07)	(2.22e-07)
Fraction of population living in poverty	-0.0100***	0.0101***	-0.0103***	0.0106***
	(0.0025)	(0.0021)	(0.0026)	(0.0021)
Fraction of population Black	0.0055	-0.0096***	0.0053	-0.0095***
	(0.0035)	(0.0021)	(0.0035)	(0.0022)
Fraction of population Native American	-0.0278***	-0.0002	-0.0270***	0.0002
	(0.0041)	(0.0027)	(0.0042)	(0.0028)
Fraction of population Asian American	0.0345**	-0.0086	0.0352**	-0.0080
	(0.0172)	(0.0109)	(0.0175)	(0.0106)
Fraction of population Hispanic	-0.0160***	-0.0001	-0.0157***	-0.0004
	(0.0046)	(0.0022)	(0.0047)	(0.0023)
District in first quartile of 1980 income distribution	0.0063***	-0.0032***	0.0060***	-0.0031***
	(0.0020)	(0.0010)	(0.0020)	(0.0010)
District in second quartile of 1980 income distribution	0.0033**	-0.0027***	0.0032**	-0.0026***
	(0.0014)	(0.0010)	(0.0014)	(0.0010)
District in third quartile of 1980 income distribution	0.0005	-0.0017**	0.0003	-0.0016**
	(0.0010)	(0.0007)	(0.0011)	(0.0007)
Number of observations	33014	33014	29442	29442
Number of districts	2996	2996	2866	2866
Kleibergen-Paap rk LM statistic	1.39		0.96	
F for excluded instruments	11.68***	7.41***	11.89***	6.97***

Note: Standard Errors based on Clustering by School District in Parentheses. All specifications include district-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

	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
(Fitted value of) Log of Per-pupil Current Expenditures	652.365	105.389	-632.543	134.316
	(402.142)	(108.544)	(756.102)	(150.852)
(Fitted value of) Fraction Minority	2401.641***	263.929***	3242.314***	255.882***
	(505.369)	(91.006)	(720.496)	(95.069)
District subject to TEL	211.501	47.729	300.858	48.291
	(292.834)	(54.528)	(291.754)	(46.395)
Log of Family Income	199.724***	57.760***	199.615***	57.897***
	(74.832)	(15.281)	(59.057)	(15.987)
Number of School Age Children in the Family	-478.545***	161.495***	-481.705***	161.229***
	(69.405)	(20.369)	(74.485)	(25.042)
Moved Since Last Survey	-34.460	-66.502***	-29.983	-66.254***
	(78.857)	(22.172)	(78.869)	(25.455)
Spouse Present in the Family	247.740	27.650	254.104	28.607
	(199.177)	(48.918)	(201.248)	(55.658)
Family Composition Changed Since Last Survey	772.223***	36.928*	768.844***	36.864**
	(90.308)	(19.492)	(86.379)	(18.816)
Family Resides in a Town	97.747	-49.985	76.410	-52.930
	(202.795)	(62.159)	(189.7713)	(73.005)
Family Resides in a Rural Area	61.388	-24.128	67.814	-27.656
	(163.809)	(47.195)	(180.531)	(47.190)
Observations	32834	29280	32834	29280
Families	9249	8709	9249	8709
F (Wald)	6.52***	4.09***	421.06***	731.44***

Note: Standard errors in parentheses – Clustered by family for OLS, bootstrapped for IV. 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.

Table 4

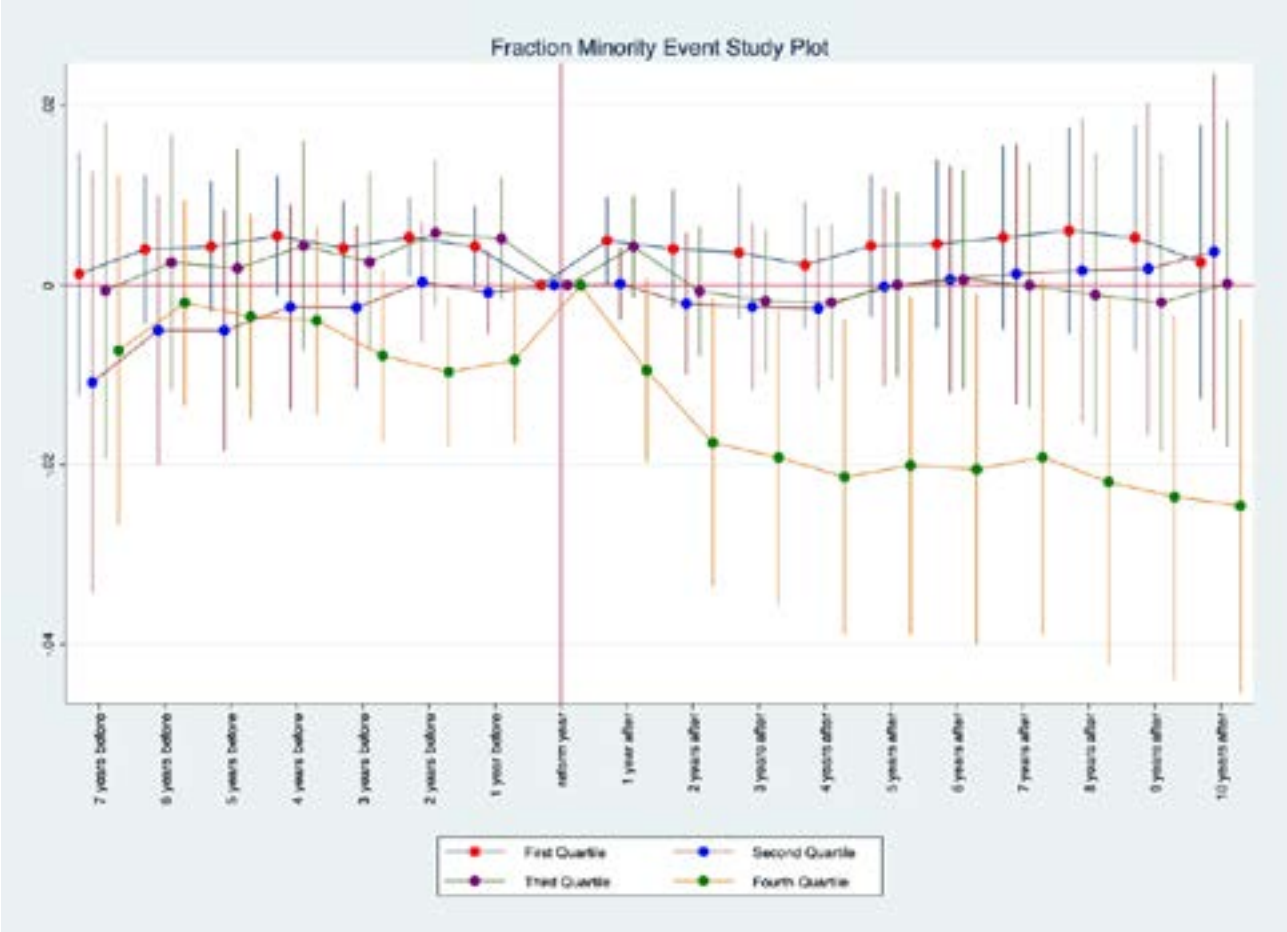
Reduced-Form Specification of Determinants of Families' Education-Related Expenditures

	No limit on supplementary Education Expenditures	Supplementary educational expenditures < $5000
Interaction of finance reform with 1980 first quartile indicator	428.991*	-0.652
	(249.951)	(68.015)
Interaction of finance reform with 1980 second quartile indicator	170.523	0.331
	(282.044)	(72.573)
Interaction of finance reform with 1980 third quartile indicator	2.312	88.232
	(277.708)	(86.471)
District subject to TEL	195.691	48.556
	(292.172)	(55.516)
Log of Family Income	198.438***	57.282***
	(75.078)	(15.280)
Number of School Age Children in the Family	-477.000***	162.228***
	(69.753)	(20.440)
Moved Since Last Survey	-56.082	-68.803***
	(77.873)	(22.182)
Spouse Present in the Family	217.645	24.497
	(198.541)	(48.877)
Family Composition Changed Since Last Survey	775.207***	37.235*
	(90.604)	(19.491)
Family Resides in a Town (NCES designation)	39.899	-58.049
	(203.896)	(62.588)
Family Resides in a Rural Area (NCES designation)	-60.165	-38.017
	(161.302)	(46.900)
Observations	32834	29280
Families	9249	8709
F	6.25***	3.89***

Note: Standard errors based on clustering by family in parentheses. 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 .

Figure 1


Fraction Minority Event Study Plot
.02
0
-.02
-.04
7 years before
6 years before
5 years before
4 years before
3 years before
2 years before
1 year before
reform year
1 year after
2 years after
3 years after
4 years after
5 years after
6 years after
7 years after
8 years after
9 years after
10 years after
First Quartile
Second Quartile
Third Quartile
Fourth Quartile

Appendix

Table A1

First-Stage Estimates for Per-pupil Current Expenditures and Fraction Minority using Full Common Core of Data

	Log of Per-pupil Expenditures		Fraction Minority
Interaction of school finance reform with first quartile of 1980 income distribution	0.0610***		0.00299**
	(0.00548)		(0.00150)
Interaction of school finance reform with second quartile of 1980 income distribution	0.0709***		0.00487***
	(0.00551)		(0.00155)
Interaction of school finance reform with third quartile of 1980 income distribution	0.0588***		0.00327*
	(0.00525)		(0.00167)
District subject to TEL	0.0409***		-0.00892***
	(0.00299)		(0.000978)
Interaction of trend with 1987 enrollment	-1.15e-08		1.05e-08
	(2.09e-08)		(1.01e-08)
Interaction of trend with 1980:			
Per-capita income	6.88e-07***		4.73e-07***
	(1.87e-07)		(4.15e-08)
Fraction high school (and not college) graduates	0.0134***		0.000679
	(0.00186)		(0.000577)
Fraction college graduates	0.0185***		-0.0101***
	(0.00324)		(0.000809)
Fraction of population living in poverty	-0.00127		0.00506***
	(0.00121)		(0.000356)
Fraction of population Black	-0.000147		-0.00248***
	(0.00129)		(0.000500)
Fraction of population Native American	-0.00417**		8.83e-06
	(0.00198)		(0.000440)
Fraction of population Asian American	-0.0249**		0.0167***
	(0.0103)		(0.00507)
Fraction of population Hispanic	-0.0116***		0.00517***
	(0.00119)		(0.000539)
District in first quartile of 1980 income distribution	0.00576***		-0.00159***
	(0.000520)		(0.000178)
District in second quartile of 1980 income distribution	0.00347***		-0.00105***
	(0.000421)		(0.000160)
District in third quartile of 1980 income distribution	0.00177***		-0.000436***
	(0.000352)		(0.000145)
Observations	130,449		129,369
Number of districts	13,685		13,689
Kleibergen-Paap rk LM statistic		12.93	
F for excluded instruments	124.28		5.49

Note: Standard errors based on clustering by district in parentheses. All specifications include district-specific effects and year effects.

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