

OUR FAVORITE METHOD OF REDISTRIBUTION: SCHOOL SPENDING EQUALITY, INCOME EQUALITY, AND GROWTH

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Abstract

Equalizing spending on the education of children from rich and poor families is one of the most popular methods of redistribution in the world. It is undoubtedly the method on which the U.S. primarily relies, given the relatively low level of social benefits in America. Redistributing through education is potentially very efficient compared to other methods of redistribution. This is because children from poor families are likely to underinvest in education, judging not only by social returns but even by private returns. Also, if implemented well, redistribution through education poses few incentive problems (parents would not readily make themselves poorer to obtain more school aid). In short, redistribution through education could not only suppress income inequality, but could potentially raise growth. Currently, support for these hypotheses is based solely on calibration exercises. In this paper, I directly test whether U.S. states that practiced more redistribution through education ended up producing adults whose incomes were less unequal and faster growing. I exploit substantial within-state variation in states' school finance policies. Owing to rather arbitrary implementation of state Supreme Court judgements, the changes in the redistributive consequences of these policies are relatively uncorrelated with changes in the states' income and education distributions. I find partial support for the hypothesis that redistribution through education reduces income inequality among adults. I do not find support for the hypothesis that it raises income growth. Because the U.S. relies so heavily on redistribution through education, weak effects are problematic and suggest that understanding where the method "breaks down" is important.

*Department of Economics, Harvard University, Cambridge MA 02138. The author gratefully acknowledges the helpful comments from Larry Blume, Sheila Murray, William Evans, David Ellwood, Richard Freeman, David Cutler, Sandy Jencks, Eric Wanner, Jeffrey Domonitz, Yannis Ioannides, Lisa Berkman, Sandy Jencks, Theda Skocpol, and Sidney Verba. She also thanks Sarah Reber and several undergraduate research assistants for work on the pre-1972 school finance dataset.

I. Our Favorite Method of Redistribution

Equalizing educational opportunity is probably the world's, and is certainly America's, favorite method of redistribution. A poor child in the United States who spent every year between the ages of 5 and 18 living on welfare would typically receive income transfers that were smaller than the school revenue transfers received on his behalf by his school district.¹ That is, he would get more state and federal redistribution through the education system than through the welfare system. In fact, this example understates society's preference for redistributing through educational opportunities: there is a five-year limit on welfare use, but a child can typically receive up to 17 years of redistribution in the elementary-secondary system before running into age limits. While redistribution through education may not be so clearly preferred all over the world, its popularity is revealed by any survey of the policy efforts of non-governmental organizations (such as the World Bank) that work in developing countries.

Redistribution through education is appealing primarily because of its logic. This works as follows. It is hypothesized that children's ability to benefit from education is relatively uncorrelated with their parents' income. Therefore, if society does not redistribute through education but instead relies on parents for private funding (or highly local funding), some able children will suffer underinvestment in their human capital. As a consequence, each generation will grow up to have incomes that are more unequal than is necessary, given nature's distribution of ability. Moreover, because poor, able children have inadequate educational opportunities, society will have misallocated investment and incomes will be lower on average than they need to be. Finally, income inequality will be more persistent, within an

¹ These estimates are based on comparing maximum welfare payment to the per-pupil revenue received by a district when it gets an additional poor child. I made this calculation separately for each state. I used the maximum welfare payment for a family of three and divided by three to get a crude per-person amount. For the school revenue calculation, I assumed that each poor child displaces a non-poor child and that poor children are associated with property wealth at the 20th percentile of the state's per-pupil property wealth distribution. I assumed that the typical non-poor child is associated with property wealth at the median of the state's per-pupil property wealth distribution. If this calculation is of general interest, I may show the state-by-state comparison in this or a related paper.

extended family, from generation to generation. Such persistence may have negative effects on individuals' behavior, above and beyond the effects of impermanent inequality. For instance, persistent inequality may erode social notions of justice or make corruption more acceptable. In summary, redistribution through education can reduce income inequality, raise the growth of average income, and reduce the intergenerational transmission of income status. This logical process is what I will call "hypothesis 1" and denote "H1" in this paper.

In addition, a potential advantage of redistributing through education is that should be possible to implement it without generating significant incentive problems. It is unlikely that parents would readily make themselves significantly poorer in order to obtain more aid for their child's elementary and secondary schooling. In contrast, redistribution through direct transfers to parents, say, raises the likelihood that parents will reduce labor supply or other efforts to earn income.

H1, or a refined version of it, underlies *many* models of income growth and equality. There are too many to survey accurately here, especially since hypothesis 1 is often stated informally. There are, however, some formal models that state H1 explicitly and built on it. Recent and influential examples include models of Benabou (1996a, 1996b), Boldrin (1993), Fernandez and Rogerson (1995, 1996), and Somanathan (1996). All of these models use an overlapping generations framework to demonstrate that, if H1 operates, societies that practice redistribution through education can achieve higher social welfare than those that do not.

H1 is, however, only an hypothesis and does not necessarily hold. If we disassemble H1, we see that it contains a few key sub-hypotheses, one or more of which may not hold.

First, children's ability to benefit from education may be quite strongly correlated with their parents' income. Such correlation could presumably not arise through native ability alone because there are genetic limits to such correlation. However, it might arise through the family transmission of habits or behavior that are complementary to investments in education. If children's ability to benefit from

education is quite strongly correlated with their parents' income, then optimal redistribution through education might be very small. It might be more efficient to have education funding be private or highly local so that most investment goes to the children who make the best use of it. These children would then earn high incomes later in life and fund income transfers to people who are poor.

Second, H1 implicitly assumes that redistribution through education actually reaches poor children, the intended targets, in the form of superior schools. This is not obvious because the redistribution is very indirect. Between the appropriation and the classroom, there are many interested parties who can divert some of the funds intended for redistribution to themselves: educational bureaucracies, unions that represent school employees, contractors who build schools, firms that supply school equipment, attorneys who involve school systems in expensive litigation ranging from asbestos or school finance itself. There are two reasons why such diversion, or rent-taking, may peculiarly affect schools that serve poor families. First, many of funds intended for such families come from non-local governments and the transfers among governments are opaque. The resulting lack of transparency in redistribution through education may make diversion of funds easier. Unless poor parents are literally school finance *experts*, they cannot figure out what funds the district is receiving on their behalf or what these funds ought be able to buy. Lack of transparency makes redistribution through education differ from social benefits in which the transfer is reasonably obvious: welfare, food stamps, disability benefits, unemployment compensation, and so on. Second, it is not clear that poor families have tools to punish parties that divert funds intended for their children. Unlike richer families, they typically cannot choose a different school if they find that funds are being diverted and their school is inadequate as a result. Thus, even if they know that their children are being exploited, they cannot necessary effect a change. The basic point is that, if a considerable share of funds intended for redistribution through education do not actually show up as superior schooling, then H1 need not hold. It might be more efficient either to give income transfers to people who turn out to be poor or to give education transfers

in a form that is more transparent and fungible (such as a voucher).²

Third, H1 contains some implicit assumptions about politics that may not hold. H1 is usually stated as though policy makers could adjust the degree of redistribution in the educational system without affecting the average level of education spending.³ While this is somewhat true, a combination of politics and behavioral responses to higher tax rates limits the degree to which redistribution can occur before the average level of education spending begins to fall. Above some level, additional redistribution through education will not be optimal because the problems of having lower average investment in education will more than offset the benefits of redistribution. For various reasons, however, American society is unlikely to be at this frontier, so I will not further emphasize this qualification to hypothesis 1.⁴

Although H1 is popular, belief in it is not universal. Moreover, the magnitudes of the relationships described in H1 are very important. If H1 is a powerful force for promoting income equality and growth, it is more reasonable to rely on it than if it is a weak force. The many models built

² If we were find that the main reason H1 does not hold is that too little money makes its way to the poor children who are the intended targets of redistribution, then we might "resurrect" H1 by making the transfer more direct (as in a voucher). The most basic form of H1 is silent on whether redistribution through education needs to happen through publicly provided schools or merely through public funding. In many other social benefit programs (higher education, health care, housing), an individual can receive public funds without using a publicly-owned provider.

However, it is not possible to do a direct test of H1 with equalized public funding in a form other than public provision. This is because, so far, no state has used more than a tiny fraction of its funds for redistribution through elementary and secondary education on a program other than public schools. Thus, in this paper, I can only test the version of H1 in which redistribution occurs indirectly, through schools.

³ There are exceptions to this rule. For instance, Fernandez and Rogerson (1995, 1996) model ways in which redistribution affects the politics that control the average level of school spending.

⁴ I will not emphasize this point because most states have *not* figured out how to maximize the amount of redistribution they can do before average education spending starts to fall. Currently, many states use such poorly designed school finance formulas that their average education spending does fall substantially when they redistribute, even though (with a better formula) they could achieve more redistribution with little or no fall in average spending. Thus, what one actually sees in the data is somewhat confusing unless one is an expert in school finance and local public economics. Some states apparently face an enormous trade-off between redistribution and average spending: every time they raise redistribution, they level down. Other states apparently face little trade-off. If states had more optimal formulas, they would all be on a policy frontier in which they would face some trade-off between redistribution and average spending but not as large a trade-off as appears in some states. For more on why some school finance formulas level down while others level up, see Hoxby (2001). A review of the literature on equalization plan should also include Evans, Murray, and Schwab (1997).

on an H1 foundation all need estimated magnitudes to deliver results. Currently, such models use a hodge-podge of partial results from other studies – they take an estimated relationship between parents' ability and children's ability from one source, they take an estimated relationship between school spending and later income from another source, and so on. Such methods leave a good deal to be desired. Not only is researchers' choice of estimates crucial (and there are usually a wide range among which researchers must choose), but the overall results still depend greatly on assumptions about the structure of relationships in H1. What we really need is direct evidence on H1.

Oddly enough, given H1's popularity, there are few if any reasonably credible, direct tests of it. Why is this? We only have as many "experiments" as there are changes in school finance policy for a given distribution of income. In most countries, school finance operates at the national level, changes slowly, and often changes along with a variety of other social benefit programs. Using national data from such countries, it is extremely difficult to identify the effect of redistribution through education, separately from the effect of other policies and time-trends that affect the country. Indeed, one cannot rely much on the results of studies that compare countries using cross-sectional data or short panels of data. Think of a developing country with low income growth, unequal family incomes, and unequal educational spending. Is its growth low and are its family incomes unequal because its educational spending is unequal? Or, is its educational spending unequal because its family incomes are unequal and low?⁵

Fortunately, in the United States, school finance policy is very largely controlled by the states, so we have 50 separate experiments happening at any given time. Even so, school finance does not change rapidly, so it is not possible to test H1 without having several decades of data on school spending and

⁵ Hanushek (1995) reviews the literature on school resources and growth. There are a large number of empirical papers that use cross-sectional data on countries or short panels of data on countries and that claim to find relationships between growth and education spending (or, more often, measures of educational attainment). I have yet to see a paper in the cross-country literature that deals at all well with the inherent endogeneity problems. I will therefore be silent on this literature, rather than give a list of papers only to criticize them.

family incomes from the states. The further back we go in history, the more inadequate the data become. One cannot construct really credible tests without using at least some very recent data. Moreover, it is always difficult to separate the effects of school finance from the effects of changes in the distribution of family income. For instance, suppose that we see a state's school spending become more equal at the same time that family incomes or wealth become equal. (It is normal for more equal incomes or wealth to generate more equal school spending –indeed, under most school finance formulas, the relationship is mechanical.) If we then see better or more equal outcomes among the children raised in the more equal environment, should we attribute these effects to their having experienced more equal school spending or to their having experienced more equal family incomes and wealth? So long as the equality of school spending changes in concert with the equality of incomes and wealth, separating their effects is very difficult. Happily for research, over the last few decades, school spending equality in American states has not been closely related to income equality. This is because states have actively pursued school finance policy and much of the timing and structure of their policies has been quite arbitrary, in part because state supreme courts have played a large and erratic role in policy timing and structure. In addition, school finance is perhaps the largest-scale public finance policy in the United States over which economists have little say and that is very poorly understood by the bureaucrats, politicians, and judges who do influence policy.⁶ While these conditions make for relatively random (and therefore sometimes unfortunate) public policy, they are useful for empirical research. Below, I show that, for the United States between 1960 and 2000, there was a good deal of variation in school spending equality for a *given* level of income equality. Moreover, this variation is not just among states, but within states over time. These are favorable conditions for testing H1.

⁶ School finance is the largest non-federal area of government spending, yet the typical school finance "expert" is not familiar with basic concepts of public finance such as marginal tax rates, deadweight loss, or capitalization. This statement is based on the author's own work with many states' state finance policy makers.

II. An Empirical Strategy for Testing the Effects of Redistribution through Education

My empirical strategy for testing H1 in this paper is straightforward. I use variation within the 50 states, over the last four decades, in the degree to which their school spending is equal and is correlated with family income. I go to some pains to demonstrate that changes in the equality of school spending do not merely reflect changes in the equality of family incomes, over this period. States that are otherwise similar pursue different school finance policies. In each decade, there are some states that have school spending equality and family income equality going in opposite directions. Such differences are the source of identification in this paper; one cannot credibly test H1 without them. I rely on changes in school spending and family incomes *within* states because states differ for many reasons and it would be naive to attribute most of the differences in their children's outcomes to school finance. Just as we cannot rely on the results of cross-sectional studies among countries, we cannot rely on cross-sectional variation among the states.

Although the empirical strategy is straightforward (given the variation I will show), two issues do arise: migration among states and policy endogeneity. Let us consider them in turn.

II.A. Migration among states

Suppose H1 holds in a straightforward fashion and that a poor child who is educated in a state with a high degree of school spending equality gets a good, rather than a poor, education. This child, once grown up, may migrate to another state with better earnings opportunities for someone with his human capital. Indeed, this may be one of the benefits of a better education –the skills to move flexibly in response to labor demand. Such migration (migration among labor markets once one has acquired one's human capital) will change how states reflect H1 over the long-term but it cannot prevent us from testing H1. It simply means that we have to associate people with the state where they were educated, rather than the state where they currently reside. In others words, we must give Arkansas credit for the

incomes of all its students regardless of whether they have moved outside the state.⁷

Another type of migration that can occur (although it is surely less prevalent) is migration that follows school finance policy. In other words, poor parents who have an especially able child or who care a lot about education might deliberately relocate to states that practice more redistribution through education. Rich parents who want to keep all of their education dollars for themselves might deliberately relocate to states that practice less redistribution through education. Such policy-pursuing migration is more problematic for testing H1 because it implies that the distribution of ability and motivation among a state's students could depend on its school finance policy. It would then be difficult to determine whether the outcomes we observe are caused by the policy itself or the unobserved ability and motivation of people who pursue the policy.

What can one do about policy-pursuing migration? First, I will assert that policy-pursuing migration is unlikely to be prevalent. Families largely choose their state of residence for work-related and other reasons, not in pursuit of school finance policies. For most families, the losses associated with taking a less optimal job would exceed the gains from enjoying more or less redistribution through school finance. Second, one can somewhat control for this type of migration by associating children with the state where they were born, regardless of where they later lived. If a family only moves to follow school finance policy once it has children and something about those children is known, then state of birth will not be as endogenous to policy as state of schooling is. Finally, I will note that policy-pursuing migration will probably bias the evidence in favor of H1. Among low income families, unobserved ability and motivation should be associated with states that practice more redistribution. Among high income families, there will be little pattern of unobserved ability and motivation because high income families

⁷ The main problem that labor market migration poses for H1 is that it can disrupt the virtuous political cycle that supports spending on education. If poor, able children in a state who benefit from redistribution through education consistently leave, then the state may find itself with too little income to support much education spending. Also, a state that is a net recipient of such children may see little reason to redistribute toward its own poor children.

can be repelled by a state that practices redistribution through education regardless of their own children's ability to benefit from education: Taxes that a high income families does not pay can be spent on anything, not just their own children's education. Overall, given a state's distribution of family income (something for which I control), migration that follows policy should make us expect greater unobserved child ability and motivation in states with more redistribution. The reader will see that I deliberately make several empirical choices in this paper that bias the evidence in favor of H1. Below, it will become clear why I proceed in this fashion.

II.B. Endogenous Policy Formation

School finance policies do not just create themselves. They are created in response to perceived problems. Such endogeneity in policy formation can pose a problem for tests of H1. I will first describe some situations in which policy endogeneity is not particularly worrisome, then discuss some where it is, and finally discuss some remedies.

The typical history of school finance policy is something like the following. A state's has had bad outcomes (such as a high drop-out rate or low test scores) among students from poor families for some time. An interest group that is concerned about these outcomes attributes them to the state's system of school finance and gets the system changed, either by gaining influence over the legislature or by suing the state and thereby creating an occasion for the courts to put pressure on the legislature. The alterations to the school finance system that result from this process are quite unpredictable, largely because school finance is poorly understood so that actual formulas exhibit a high degree of idiosyncrasy. Indeed, owing to the fact that most "experts" do not account for predictable behavioral responses, formulas do not always move in an equalizing direction even when the intention of the legislature is clear.

The typical history just described is not a serious problem for testing H1. This is because the policy forms in response to conditions that already exist and would probably remain stable in the absence

of intervention. By seeing whether student outcomes *improve* in a state in response to the *effect* of its school finance policy, we can test H1.⁸ The state's initial conditions will be differenced out.

It sometimes happens that the interest groups that gain sway over the legislature not only get school finance policy changed but also get other policies oriented toward the poor (especially poor children) changed. For instance, many child interest groups are interested in education, health care, and child care. Legislation that they influence sometimes bundles such policies. To the extent that their rise in influence allows them to get multiple policies enacted, the evidence will be oversupportive of H1: redistribution through education will appear to be more efficacious than it really is.

So far, policy endogeneity would only seem to bias circumstances in favor of H1. It is possible, however, to construct a scenario in which policy endogeneity is more problematic. Suppose that policy makers in a state are forward looking and they see that child outcomes will become more and more unequal if they do not intervene. Then, equalizing school spending may reduce the rise in the inequality in outcomes that would otherwise occur, but it may not actually cause outcomes to become more equal than they were when the legislation was passed. Unless we know the correct counterfactual (rising inequality of outcomes), we will conclude that redistribution through education is less efficacious than it truly is. I deal with such possible policy endogeneity in two ways. First, it is fortunate that very similar states do not consistently enact similar policies or enact policies with similar timing. For instance, Ohio, Michigan, Indiana, Pennsylvania, and Illinois have experienced many similar industrial and demographic trends but their school finance policies exhibit no coordination. Thus, these states serve as counterfactuals for one another. Second, one can control explicitly for the trends in outcomes that policy makers could have observed at the time. If the evidence is more favorable to H1 when such controls are introduced, then the results suggest that forward-looking policy endogeneity is a problem. If adding

⁸ Note that I look at the *effect* of a policy rather than its intention. This is because the intention of policies is often not fulfilled and poor children's lives are potentially changed only by the actual effect of the policy.

controls has no such effect, it is likely that policy is based on more stable conditions (as described above) or that other states are providing effective counterfactuals.

III. Data on School Spending, Family Incomes, and their Relationship

Before demonstrating that the last four decades contain sufficient variation for a credible test of H1, I need to describe my data sources, their advantages, and their limitations.

III.A. Per-Pupil Spending

The most straightforward data are those on school spending per pupil. Data on school districts' expenditures and enrollment come from their administrative records and are compiled in various censuses. Since 1992, the United States Department of Education and the United States Bureau of the Census have conducted annual censuses of school finance called *Public Elementary-Secondary Education Finances*. Between 1972 and 1992, school districts' expenditures were reported in the quinquennial *Census of Governments*.⁹ In the five year intervals between these censuses, many districts' expenditures were reported in the *Annual Survey of Governments*, which sampled districts and always included the largest ones. Enrollment information is reported in the *Censuses of Governments* (except for dependent school districts) but the most accurate, annual information for 1969 to 1992 comes from annual censuses of enrollment conducted by United States Department of Education. From 1980 onwards, these have been known as the *Common Core of Data: Public School District Universe Data*. Prior to 1987, they were known as *Elementary and Secondary General Information System: Public School District Universe Data*. There are *Censuses of Governments* for 1957, 1962, and 1967, but electronic data from them is only available at the level of counties or independent cities (found in states like Virginia, Maryland, Pennsylvania, and Missouri). I used the 1957, 1962, and 1967 *Censuses of*

⁹ A common misconception is that the *Censuses of Governments* do not contain revenue, expenditure, or employment, information on dependent school districts. They do. Users must map each dependent district to its municipality.

Governments for data on school districts are coterminous with counties or independent cities. The remaining districts' data were filled using their states' biennial reports on education.¹⁰ Briefly put, the per-pupil spending data are very accurate for 1992 onwards, for all districts. From 1972 to 1991, they are also very accurate for large districts, but a typical small district's data is obtained only twice in every five years and its per-pupil spending must be interpolated in other years. In practice, such interpolation is not a serious problem: per pupil spending does not change radically very often. From 1957 to 1971, per-pupil spending data are obtained only every five years and must be interpolated in other years. Clearly, this is the period with the least accurate spending data and results from the period should be discounted accordingly. (I will remind readers of this when interpreting the results).

III.B. Family Income Data Matched to School Districts

In order to understand the relationship between school spending and family incomes in a state, it is necessary to associate family income data with school district boundaries. For the 1980, 1990, and 2000 *Censuses of Population*, there is a school district level summary of the census that covers all school districts. The summary files report mean and median income as well as the number of families in each of several income categories (17 categories in 1980, 25 categories in 1990, 16 categories in 2000). I mainly use the category data, in a manner described below. For the 1970 *Census of Population*, there are three files that are useful for obtaining district-level census data: the *1970 Census Fourth Count: School District Data Tape* (the 82 percent of school districts that have at least 300 students), the *National Institute of Education Special Tabulation and Fifth Count Data File* (additional variables for school districts with at least 300 students, if they did not alter their boundaries between 1969 and 1973), and the *Office of Revenue Sharing 1970 Fifth Count Census and ORS Revenue Sharing File* (for filling in data on

¹⁰ For the past two years, Sarah Reber and I have been electronically coding school districts' data from the hard copy biennial reports of state superintendents of education, working backwards from 1972 (the least recent, fully available Census of Governments). We hope eventually to make all of these data public, but the project is still very much ongoing. These data are listed as Hoxby and Reber (2003) in the references.

small districts).¹¹ For 1970, family income data at the district level include the mean, median, and number of families in 15 income categories. For the 1960 *Census of Population*, there are no published district-level summary data, so the best one can do is construct a dataset from three sources. For the 41 percent of districts in tracted areas (mainly metropolitan counties in the 1960 *Census*), the best available data are contained in the *Census-Tract Level Data 1960*. It is generally possible to associate tracts with districts using the full set of geographic identifiers. For most metropolitan districts, the match appears to be accurate. For the 24 percent of districts that are untraced but coterminous with a city or county, the *County and City Data Book 1962* (which contains data from the 1960 Census) is an accurate source of family income data. The remaining 35 percent of districts can only be matched to county-level family income data. Note that this 35 percent of districts represents only 9 percent of American elementary and secondary enrollment; they tend to be small, rural districts outside of the South (where county-level districts are common). As with the per-pupil spending data, the earliest data (that from 1960) is the least accurate and results from the period should be discounted accordingly.

III.C. Students' Long-term Outcomes

Data on students' long-term outcomes must meet three criteria: they must be available over several decades; they must be sufficiently numerous to permit me to compute accurate measures of diversity of outcomes (not just the mean outcome); and they must allow me to associate a person with (preferably) his state of birth. The data that best fulfil these criteria are the micro-data of the decennial censuses (IPUMS). For each of the 1970 through 2000 censuses, I use the largest representative sample available (5 percent in 1980 and 1990, 1 percent in 1970 and 2000).¹² The outcomes on which I focus are

¹¹ In order to use the Office of Revenue Sharing data, one must determine which place or municipality corresponds most closely with the district.

¹² The IPUMS data for the 2000 census are still only partially available. I use the one percent sample for the 16 states for which it is available. I use the 1/250 sample for the remaining states. I will update the data in this paper as they become available.

a person's total earned income, computed hourly wage, and educational attainment. Because women's labor supply has changed so much over the period, I show results for income and the hourly wage that include only men and other results that include all persons. I also make minor use of the 1950 and 1960 IPUMS data (see below).

III.D. The Cohorts and Timing

It might be supposed to be a disadvantage that I can observe family income and long-term outcomes only at ten year intervals. Actually, this is not so. As emphasized previously, we only have as many "experiments" as there are changes in states' school finance policies. Adjacent birth cohorts in a state tend to experience very similar school finance policies and distributions of family incomes. Thus, if there were data to "fill in" between the censuses, such data would not add much explanatory power. Such data would, however, introduce considerable and problematic serial correlation. Another way to see the situation is in terms of the recent literature on difference-in-differences estimation in the presence of serial correlation (Bertrand *et al*, 2000). That literature provides a reinterpretation for the well-known stricture that there are only as many experiments as there are changes in regime. The authors note that, when researchers ignore this stricture, they tend to use (say) annual panel data as though each year of data provided independent evidence on the effect of policy change. If researchers ignore the fact that such data exhibit a high degree of serial correlation, they can seriously underestimate standard errors.

For a test of H1, it makes sense to focus on cohorts who experience school finance regimes that are largely independent of one another. Conveniently, elementary and secondary school are twelve years long, so cohorts separated by ten years have schooling histories that overlap only slightly. Figure 1 shows how I choose cohorts and associate per-pupil spending and family income data with them. Let us consider the 1950 birth cohort and start with their outcomes. I observe them as twenty-year-olds in the 1970 census and again as thirty-year-olds in the 1980 census. I record their incomes and wages at age 30 (when most will have settled into a job pattern), and I record their educational attainment at both age 20

and age 30. This cohort would have attended elementary and secondary school from 1957 to 1968. Thus, I associate each of them with per-pupil spending variables from 1957 to 1968 in the state of their birth. I record median per-pupil spending and several measures of the diversity of per-pupil spending in their state of birth in the years that they attended school. Finally, I need to associate the 1950 birth cohort with state-level data on the level and diversity of family incomes that they experienced. I observe their family incomes three times: in 1950 when they are born but less than one year old, in 1960 when they are ten, and in 1970 when they are twenty. I associate them with the incomes of families with newborns from their state of birth in 1950 and with the incomes of families of ten-year-olds from the state of birth in 1960. It would be unwise to associate them with the incomes of families of 20-year-olds in 1970, however. Many twenty-year-olds live apart from their parents, so that their family incomes would reflect not their parents' circumstances but their own earnings. Since all I need is state-level data to associate with them, I use the incomes of families with 17-year-olds in their state of birth in 1970. I chose 17-year-olds because 98 percent of them still live at home and because it is likely that the distribution of incomes among parents of 17-year-olds is very similar to the distribution of incomes among parents of 20-year-olds. Finally, I use the observations from the 1950, 1960, and 1970 census to compute median family income and several measures of the diversity of family incomes experienced by children in their cohort, from their state of birth.

III.E. Aggregation

It may be useful to remind the reader that the "experiments" in redistribution through education all operate at the state level. Also, independent "experiments" are held only once for each generation (adjacent cohorts participate in virtually the same experiment). Thus, we really have only as many experiments as we have states and decades: 50 states times four decades equals 200 experiments. Thus, regardless of how much data I use, I essentially have only 200 observations for each variable. The main usefulness of micro-data, therefore, is that it allows me to accurately compute numerous state-level

measures of the diversity of individuals' outcomes and families' incomes. The main usefulness of the district-level data on family income is that it allows me to accurately estimate the relationship between family income and per-pupil spending, by state and decade. When I actually run regressions, many of them will be entirely at the state-by-decade level –for instance, the diversity of income among people from the 1950 birth cohort from state X regressed on measures of the level and diversity of their parents incomes; measures of the level and diversity of per-pupil spending they experienced, and measures of the strength of the relationship between family income and per-pupil spending that they experienced. When I do run a regression with individual-level data, it is only to gain efficiency from relating certain individual background characteristics (such as race) to outcomes; there are still only 200 observations of the policy and family income variables and the standard errors are computed with appropriate clusters to reflect this fact (see Moulton, 1986).

It is interesting to note that even if I had longitudinal data and could link each 30-year-old with the exact schools that he attended, I could make only limited use of such data. Because a family's choice of schools is endogenous to numerous unobserved variables (such as their preference about education and motivation), I would have to instrument for the actual per-pupil spending experienced by each person with the per-pupil spending we would have expected him to experience, given his family's income and his state's relationship between per-pupil spending and family income. This would be a good exercise but not terribly different from the estimates I show in this paper. Because longitudinal data would be of limited use, I have chosen to rely mainly on census data (where the number of observations per state is very useful) rather than, say, Panel Survey of Income Dynamics (PSID) data, which is longitudinal but in which the number of observations per cohort per state is very small. In the final section of this paper, however, I do consider how the PSID might be used to examine intergenerational income correlations, by state and decade.

IV. The Environment for Testing Redistribution through Education:

Four Decades of Family Income and Per-Pupil Spending

Let us examine the time patterns of income inequality and per-pupil spending inequality in the United States. I want especially to focus on within-state income and spending inequality because within-state variation will be the source of identification for testing H1. In order to focus on within-state patterns, I will make considerable use of median ratios. These also (conveniently) provide a metric that is consistent over time and insensitive to changes in nominal prices. In order to see how the median ratios work, consider the income of family i in state k in year t . We can decompose its income as follows:

$$\begin{aligned} & \text{family income}_{ikt} = \\ & \frac{\text{family income}_{ikt}}{\text{median family income in state}_{kt}} \cdot \frac{\text{median family income in state}_{kt}}{\text{median family income in US}_t} \cdot \text{median family income in US}_t \end{aligned} \quad (1)$$

In this section, I will show the patterns of income for all three terms on the right-hand-side of equation (1), but I will focus on the first term on the right-hand-side, which is the median ratio. When the distribution of families' median ratios is tight in a state, the state's income distribution is compressed, and vice versa. The distribution of the second term on the right-hand-side shows how similar the states are to one another. The third term on the right-hand-side is obviously useful for tracking median family income in America over time. We can perform a similar operation on the per-pupil spending (pps) of district j in state k in year t :

$$\text{district's } pps_{jkt} = \frac{\text{district's } pps_{jkt}}{\text{median } pps \text{ in state}_{kt}} \cdot \frac{\text{median } pps \text{ in state}_{kt}}{\text{median } pps \text{ in US}_t} \cdot \text{median } pps \text{ in US}_t \quad (2)$$

IV.A. The Distribution of Family Income over Four Decades

Figures 2 through 5 show how the distribution of family income changed from 1960 to 2000. Each one shows kernel density estimates of the median ratios based on all the individual families in the 1960, 1970, 1980, and 2000 IPUMS data. Keep in mind that, because the figures show the median ratios, what we are looking at is how compressed or dispersed the average state's income distribution is. The patterns of family income inequality that I show here are not surprising: changes in the income distribution are well documented. However, they are worth reviewing, both so that we can understand the environment for school finance and so that we can focus on *within-state* changes in the family income distribution (which is what the later empirical work relies upon).

Figure 2 shows the distribution of family income median ratios in 1960 and 1970. They have the characteristic shape of typical income distributions: roughly bell-shaped with a truncated left-hand tail and an elongated right-hand-tail. The two distributions lie almost on top of one another, showing that there was little change in within-state income inequality between 1960 and 1970. In contrast, Figure 3 shows that the distribution of family income became more dispersed within states between 1970 and 1980: the distribution flattened, with the mass being pushed leftwards. That is, family income inequality rose because families were being pushed away from median family income in a downwards direction. Figure 4 shows the distributions of family income in 1980 and 1990 and suggests that inequality rose again during the 1980s. In fact, for within-state income inequality, the 1980s look much a scaled-down version of the 1970s. Finally, Figure 5 shows that the within-state distributions of family income were relatively stable with a small decrease in inequality due to the fact that there were significantly fewer poor families in 2000 than in 1990.

In the empirical work that follows, I rely on the within-state changes in the distribution of family income. Nevertheless, for completeness, we ought to know how the between-state distribution of family income has changed (that is, what has happened to the second term on the right-hand-side of equation

(1)). Figure 6 shows the distribution of states' median family incomes in 1960, 1970, 1980, 1990, and 2000. Essentially, the states' median income levels became more dispersed over the 1960s, slightly less dispersed over the 1970s, and significantly more dispersed over the 1980s and 1990s. For completeness, Figure 7 shows the national trend in American median family incomes (that is, what has happened to the third term on the right-hand-side of equation (2)). Not surprisingly, median family incomes have risen—from \$31,721 in 1960 to \$53,200 in 2000 (both in 2001 dollars, adjusted using the CPI-U).

To recap, the within-state distribution of family incomes was stable in the 1960s, became more unequal in the 1970s and 1980s, and was largely stable in the 1990s except for a small decrease in inequality because the share of families that was poor fell. Now, let us see whether changes in the distribution of per-pupil spending mimics that of family income.

IV.B. The Distribution of Per-Pupil Spending Over Four Decades

Figure 8 shows the distribution of per-pupil spending median ratios in 1960 and 1970. They are roughly bell shaped but the mass is much more concentrated at the median than is the distribution of family income. The distribution is not as smooth as that of family income; this is because there are large districts that create groups of students who experience the same per-pupil spending. Unlike the family income distribution, the within-state per-pupil spending distribution tends to be approximately symmetric—in particular, there is no elongated right-hand-tail. The 1960 distribution is more unequal than that of 1970. The easiest way to see this is to note the height of the distribution at the state's median. Recall that the within-state family income distribution was stable between 1960 and 1970 so that the falling inequality of within-state per-pupil spending was not caused by changes in the income distribution.

Figure 9 shows that inequality in within-state per-pupil spending continued to fall between 1970 and 1980, especially because the mass of low spending districts really disappeared, shifting much closer to the median per-pupil spending in their state. (The mass moves to a level above 70 percent of the median.) It is important to observe that, during the 1970s, the within-state per-pupil spending

distribution is shifting is the opposite way that the family income distribution is shifting. Remember that family income was becoming significantly more unequal over the 1970s, with a significant mass shifting to toward low levels of income.

Figures 10 and 11 show that within-state per-pupil spending inequality continued to fall further during the 1980s and 1990s. The best way to see these changes is to look at the height of the distribution around the median. The mass around states' median is rising and is coming from the tails of distribution—for instance, schools with per-pupil spending below 80 percent of the median and steadily disappearing between 1980 and 2000. To put this in perspective, recall that, during the 1980s, family income was becoming less equal and that, during the 1990s, the distribution of family income became slightly more equal because the share of families who were poor decreased. In summary, we have four decades with somewhat different experiments: 1960s, stable income distribution, slightly falling inequality in per-pupil spending; 1970s, substantially rising income inequality, substantially falling inequality in per-pupil spending; 1980s, rising income inequality, falling inequality in per-pupil spending; 1990s, slightly decreasing income inequality, falling inequality in per-pupil spending. Note, however, that there is considerable variation among states within each decade. This is merely the overall pattern; individual states do not always follow it.

Before looking at the variation among states, let us look briefly at the between-state distribution of per-pupil spending (the second term on the right-hand-side of equation (2)) and the national trend in median per-pupil spending (the third term on the right-hand-side of equation (2)). Figure 12 shows that states' median per-pupil spending levels became more compressed with each decade. Notice the very large compression among states that occurred in the 1990s. This is interesting because, by the 1990s, the rate of within-state compression in spending had slowed, although it continued. Figure 13 shows that American median per-pupil spending rose substantially in real terms between 1960 and 2000: it was \$2,626 in 1960 and \$7,355 in 2000 (both in \$2001, adjusted using the CPI-U).

IV.C. Variation among States and Decades in the Family Income – Per-Pupil Spending Relationship

What is essential for identification in this paper is that states and decades vary in the relationship between family income and per-pupil spending. If we are to learn about how changes in school finance policy affects the distribution and growth of students' incomes (later in life), we need to see states' policies change over time and we need to see that family income and per-pupil spending do not always change in the same direction. The next four figures demonstrate these points. For instance, consider Figure 14. The figure is arranged so that its axes form a quadrant. Each observation on the figure represents a state. The horizontal axis records the 1960s change in each state's 90-10 ratio of family income: the 1970 ratio minus the 1960 ratio. The vertical axis records the 1960s change in each state's 90-10 ratio of per-pupil spending: the 1970 ratio minus the 1960 ratio. (I have constructed these figures with various measures of the diversity of family income and per-pupil, not just the 90-10 ratio. The 90-10 ratio shows the same patterns we would see in other measures, though naturally it changes in a somewhat more muted fashion than the 95-5 ratio and a somewhat less muted fashion than the 75-25 ratio or coefficient of variation.) Thus, if a state's observation is in the lower, left-hand quadrant, then both its family income and per-pupil spending are becoming more equal over the 1960s. If it is in the upper, right-hand quadrant, both its family income and per-pupil spending are becoming less equal over the 1960s. In the upper, left-hand quadrant, family income is becoming more equal and per-pupil spending is becoming less equal. In the lower, right-hand quadrant, family income is becoming less equal and per-pupil spending is becoming more equal. Although all states' observations can help us with identification, observations are especially helpful if they do not lie along an upward-sloping line. Also, the observations in the upper, left-hand and lower, right-hand quadrants are especially useful for identification because they represent "experiments" in which a state's school finance policy is *necessarily* active and independent. This is because per-pupil spending equality would naturally follow family income equality in the absence of active policy.

Figure 14 shows that there are states in all four quadrants in the 1960s. In fact, the four quadrants are roughly balanced. This shows that, though family income was stable over the decade and per-pupil spending became slightly more equal over the decade, different states were on different paths. States did not simply mimic the national trends. Thus, the 1960s are a good decade for identifying the effects of redistribution through education (except for the fact that the 1960s explanatory variables suffer more from measurement error than later data do). Figure 15 shows the 1970s. In the 1970s, most states' income distributions were becoming more unequal –thus, most of the observations fall into the right-hand side of the figure. However, a fifth of the states' family income distributions compressed. Almost all states' per-pupil spending became more compressed over the decade. In short, many of the observations are grouped in the helpful quadrant where family income inequality is rising and per-pupil spending inequality is falling. The remainder are in the lower, left-hand quadrant where both family income inequality and per-pupil spending are falling. Within these quadrants, note the substantial variation among states: the 1970s also provides us with useful "experiments." Figure 16 shows that the 1980s mainly generate experiments in the two right-hand quadrants, where family income inequality is rising but states differ in whether per-pupil spending inequality is both rising or falling. Although the helpful, lower right-hand quadrant again has most of the observations, about a third of the observations are in the upper, right-hand quadrant where both family income and per-pupil spending inequality are rising. This is good because this quadrant was quite empty in the 1970s. Finally, in Figure 17, we see the 1990s, when plurality of states experienced falling inequality in both family income and per-pupil spending, but some states experienced rising family income inequality and falling per-pupil spending inequality and a few states experienced falling income inequality and *rising* per-pupil spending inequality.

In short, every quadrant of our figures has quite a number of "experiments," suggesting that the four decades from 1960 to 2000 are a good period for identifying the effects of redistribution through education. There is variation from decade to decade, and—within each decade—states certainly do not line

up neatly in upward-sloping lines (as they would in the absence of active state policy). We are dealing with a period in which family income is changing and different states are addressing those changes differently. This means that, even if school finance policy is endogenous, the sheer variety in the states' endogenous response is helpful for identification.

Another way to see this same point is to note the variation, among states and within a decade, in the estimated relationship between family income and per-pupil spending. Separately for each state in each decade, I ran a regression of per-pupil spending on family income, using the census data matched to school districts. To do this, I assigned each family income category its mean value, and I weighted each category by the number of families in it.¹³ Thus, each regression approximates a regression of per-pupil spending on individual family income data. Note that the 1960 coefficients may be attenuated by measurement error. Table 1 records the estimated slope coefficients –specifically, the number of dollars by which per-pupil spending rises for every additional \$1,000 in family income. The purpose of Table 1 is simply to give the reader some sense of the variety in the relationships among family income and per-pupil spending, over states and (especially) within-states over time. I readily admit that most states' relationship are non-linear, so that the linear slope coefficients shown are only a crude indicator of the differences among states. However, there is no way to show 50 estimated, non-linear relationships for each decade (200 relationships in all) in a sufficiently concise way for the reader to get any sense of the differences among states. Having said this, the reader will see that active school finance policy over the four decades made most states' relationships jump around. To take just one example, consider Connecticut. In 1960s, it had almost no relationship between family income and per-pupil spending. This was because its cities, where its poor people were concentrated, spent above-average amounts per-

¹³ The bottom family income category was assigned an income equal to 0.8 of its upper limit, and the top family income category was assigned an income equal to 1.15 of its upper limit. I derived these adjustment factors (which are common) from a survey of adjustments made to Current Population Survey incomes over the last forty years. My results are not sensitive to reasonable variations in these adjustment factors.

pupil owing to the concentration of property wealth in those cities. In the 1970s and 1980s, it exhibited a positive relationship between family income and per-pupil spending: every \$1,000 increase in family income was associated with a \$3 to \$5 increase in per-pupil spending. This was because its cities were decreasingly wealthy but the state was increasingly taking steps to redistribute through education (mainly relying on foundation aid formulas). Finally, during the 1990s, a combination of compensatory aid (including special education aid) and stringent school finance equalization reversed the sign of the coefficient on family income. By 2000, every \$1,000 increase in family income was associated with a small *decrease* (\$0.84) in per-pupil spending. Connecticut's story is not particularly dramatic (compare its coefficients to those of some other states) but an easily understood one and we can see why it generates experiments. States that have persistent negative coefficients (such as Montana) are typically those in which small schools in sparsely populated areas are among the highest spenders in the state.

Later, I will use the estimated coefficients shown in Table 1 as indicators of the steepness of the relationship between per-pupil spending and family income, in a state in a decade.

V. The Effect of Redistribution through Education on Students' Later Outcomes

In this section, I exploit the "experiments" shown in the previous section to estimate the effect of redistribution through education on students' later life outcomes: income, wages, and educational attainment. The basic estimating equations are straightforward. I run them using median regression, interquartile regression, and mean (conventional) regression.

V.A. Measuring the Effect of Redistribution through Education

To determine how students' later incomes are affected by their state's educational environment, I estimate:

$$\begin{aligned}
\text{later outcome}_{ikc} = & \beta_0 + \beta_1 \text{median income of parents}_{kc} + \beta_2 \text{diversity of parents' income}_{kc} + \\
& \beta_3 \text{median per-pupil spending experienced}_{kc} + \beta_4 \text{diversity of per-pupil spending}_{kc} + I_k^{\text{state}} \beta_5 + I_c^{\text{cohort}} \beta_6 + \\
& I_{ikc}^{\text{female}} I_c^{\text{cohort}} \beta_7 + I_{ikc}^{\text{black}} I_c^{\text{cohort}} \beta_8 + I_{ikc}^{\text{Hispanic}} I_c^{\text{cohort}} \beta_9 + I_{ikc}^{\text{Asian}} I_c^{\text{cohort}} \beta_{10} + I_{ikc}^{\text{NativeAmer}} I_c^{\text{cohort}} \beta_{11} + u_{kc} + u_{ikc} .
\end{aligned} \quad (3)$$

All variables are in \$2001, adjusted using the CPI-U. In equation (3), i indexes individual students, k indexes states, and c indexes birth cohorts (the 1950, 1960, 1970, and 1980 birth cohorts). Thus, *median income of parents*_{kc} is the variable mentioned previously, which is calculated for all parents of children who were born in state k in cohort c . Recall that their incomes are observed on three occasions during their children's lives. This variable is the average of those three observations. In the estimation, I will experiment with several alternative measures of the diversity of parents' incomes and per-pupil spending. This is why I have merely written "*diversity of parents' income*_{kc}" and "*diversity of per-pupil spending*_{kc}" and have not specified a measure of diversity. The two per-pupil spending measures are calculated for state k during the years when cohort c would typically have attended grades one through twelve. Equation (3) includes a set of state effects because I rely on within-state variation. The year effects control for national influences on outcomes.

In equation (3), there are few covariates measured at the individual level: indicators for sex and race/Hispanic ethnicity. It is the inclusion of these covariates that makes me estimate equation (3) using observations of the dependent variable that are measured at the individual level. It is important to understand that, although there are many observations for each state and cohort, there are still only 200 possible "experiments." The only reason for using individual-level data is that they increase the precision of the other estimates because the relationships between the individual-level covariates and the outcome are estimated. Put another way, we gain precision by accounting for the fact that a changing sex or racial/ethnic composition might affect a state's outcomes. Notice that I allow the coefficients on sex

and race/ethnicity indicators to be different for different cohorts. This is because social movements have affected how woman and minorities interact with the labor market.¹⁴ The first of the two error terms in equation (4) reminds us that the standard errors are computed to reflect the fact that the observations are clustered by state-cohort and that, for the purpose of estimating β_1 through β_4 there are really only as many observations as there are state-cohort clusters.

I estimate equation (3) for several outcomes. The first set of outcomes shows the effect of the educational environment on *diversity* of students' later outcomes: the diversity of income at age 30, the diversity of wages at age 30, the diversity of educational attainment at age 20, and the diversity of educational attainment at age 30. I use three measures of diversity for the outcomes: the coefficient of variation, the 90-10 ratio, and the 75-25 ratio. I discuss these measures more below. I also estimate equation (3) for a second set of outcomes that show the effect of the educational environment on the *growth* of outcomes in the state: median and mean income at age 30, median and mean wages at age 30, median and mean educational attainment at age 30, and median and mean educational attainment at age 20. Note that equations for education attainment at age 20 includes 200 "experiments" because all four birth cohorts (1950, 1960, 1970, and 1980) have usable data. Equations for the outcomes age 30 include only 150 "experiments" because the 1980 birth cohort has not reached age 30 by the year 2000.

V.B. Results: How Redistribution through Education Affects Outcome *Diversity*

Table 2 through 4 show the effect of redistribution through education on the diversity of four key outcomes: earned income at age 30, (calculated) hourly wage at age 30, educational attainment in years at age 30, and educational attainment at age 20. In the tables, I alternate the measure of diversity used because we do not have *ex ante* information that makes us prefer one measure of inequality over another. I examined about a dozen measures and show the most revealing three here: the coefficient of variation,

¹⁴ Alert readers may wish to see what happens if I impose the restriction that these coefficients are equal for all cohorts. See the notes to the appropriate table.

the 90-10 ratio, and the 75-25 ratio.¹⁵ The coefficient of variation is sensitive to the existence of outliers but also weighs observations more equally than the two other measures. The 90-10 ratio picks up changes in diversity in the tails of the distribution; the 75-25 ratio picks up changes in diversity closer to the median. Within each table, I vary the measure of diversity used for the explanatory variables but use a single type measure for the dependent variables. The organization of the tables is as follows:

| | Method | Measures of Diversity Used for Explanatory Variables (in turn) |
|---------|---|--|
| Table 2 | estimate coefficient of variation of outcomes using state-level variables only | coefficient of variation, 90-10 ratio, 75-25 ratio |
| Table 3 | estimate 90-10 ratio of outcomes using individual level data and interquantile regression | coefficient of variation, 90-10 ratio, 75-25 ratio |
| Table 4 | estimate 75-25 ratio of outcomes using individual level data and interquantile regression | coefficient of variation, 90-10 ratio, 75-25 ratio |

Let us first examine Table 2. In each panel of Table 2, the first row shows the effect of median parents' income on diversity of later outcomes. It turns out that median parents' income does not have a discernible effect on the diversity of later outcomes; this is not terribly surprising, as theory does not lead us to expect any particular relationship. The next row shows the effect of the diversity of parents' income on the diversity of their children's later outcomes. The effect is positive for all of the outcomes, and is statistically significant for all the outcomes except wages. For instance, if the coefficient of variation of parents' income rises by one (about 4 standard deviations), the coefficient of variation of their children's later income rises by 0.11. If the 90-10 ratio for parents' income rises by one (about two-

¹⁵ The 90-10 ratio is the ratio of the value at the 90th percentile to the value at the 10th percentile. The 75-25 ratio is defined similarly.

thirds of a standard deviation), the coefficient of variation of their children's later income rises by 0.27. If the 75-25 ratio for parents' income rises by one (about 2.6 standard deviations), the coefficient of variation of their children's later income rises by 0.24. The point estimates for children's later wages suggests similar effects, but are statistically insignificant. The estimates for educational attainment suggests that if the coefficient of variation of parents' income rises by one, the coefficient of variation of their children's completed years of education rises by 0.05 years by age 30 and 0.02 years by age 20. Overall, these results are not surprising. We expect that a generation of parents with more unequal incomes will have children with more unequal incomes, all else equal. Also, we expect that the results for children's wages are least likely to be statistically significant owing to the measurement error generated when we compute wages from income, usual hours, and hours worked last year. Notice that, even though the effect of parents' income inequality on children's outcome inequality is consistently positive, it is not enormous in magnitude. If the magnitude seems small to the reader, he or she should recall that naive estimates, in which state fixed effects are not controlled for, overstate the transmission of inequality from generation to generation. This is because many of the underlying factors that cause a state to have unequal incomes persist from generation to generation. Their persistent influence makes inequality correlated across generations, even when parents are not actually transmitting their income inequality in a causal fashion.

Appendix Table 1 contains estimates that are exactly parallel to those in Table 2, except that the outcomes are calculated only for males, in order to avoid problems of interpretation caused by women's changing role in society and the labor force. Looking at Appendix Table 1, one can see that the results I have discussed so far (in the first two rows of the table) are similar when we examine outcomes for males only.

The next row of estimates shows the effect of median per-pupil spending on the inequality of children's later income and wages. For income, the effect appears to be negative, and there may indeed

be some negative effect. I will not, however, emphasize this result because it turns out to be very fragile. The rest of Table 2 shows that it does not hold up for other outcomes (indeed, the point estimates are often of the wrong sign). Also, Appendix Table 1 shows that the result does not hold up for males only. We should not be surprised that the median level of per-pupil spending does not have strong effect on the diversity of later outcomes. In intergenerational models of income inequality, strong assumptions are necessary to generate the prediction that higher overall spending leads to more equal outcomes (or, indeed, any change in the diversity of outcomes).

Now, let us examine the coefficients on the two explanatory variables that are crucial for testing the effect of education through redistribution: the inequality of per-pupil spending and the slope of the relationship between per-pupil spending and family income. The first measure tells us how different are the per-pupil investments experienced by different children in the state, but does not tell us whether more spending tends to go along with higher parent incomes. The second measure indicates whether differences in parental income are reinforced by correlated differences in the educational investment that a child experiences.

The estimated coefficients on the coefficient of variation of per-pupil spending give us no indication that there is a relationship between inequality in per-pupil spending and inequality of students' later outcomes. None of the estimated coefficients is statistically significantly different from zero at conventional levels and even their signs are not consistently positive. However, the story is different for the coefficients on the estimated slope of the relationship between per-pupil and family income. These coefficients are consistently positive in sign. They are statistically significantly different from zero at the 5 percent level for income and educational attainment at age 20. (They are positive but not statistically significantly different from zero at the 5 percent level for wages or educational attainment at age 30.) A standard deviation in the estimated slope is 0.00287, so a one standard deviation increase in the slope would raise the later coefficient of variation of income among students by 0.042 to 0.045. (For reference,

the mean of the coefficient of variation of income is 0.903 and its standard deviation is 0.132.) A one standard deviation increase in the slope would raise the coefficient of variation of educational attainment of 20-year-old students by 0.006 to 0.008. (For reference, the mean of the coefficient of variation of education attainment of 20-year-olds is 0.133 and its standard deviation is 0.037.)

Summing up, the estimated coefficients suggest that income inequality and educational attainment inequality are generated when students experience a tight relationship between per-pupil spending and their families' income. The magnitude of the effects of moderate: a increase of one standard deviation in the slope of the per-pupil spending-family income relationship raises inequality by about one-third of a standard deviation when inequality is measured by the coefficient of variation. These results support the inequality part of H1 (as opposed to the growth part, which is yet to be tested). Before we become too assured about this result, however, let us consider other measures of inequality. Table 3 uses 90-10 ratios as the dependent variables and Table 4 uses 75-25 ratios as the dependent ratios. Recall that the estimates in Tables 3 and 4 are somewhat to be preferred to those in Table 2 because they make better use of the individual-level data (the estimates in Table 2 are necessarily based on regressions run purely at the state level).

Looking at the second rows in Table 3, we see that parents with more diverse incomes generate children with more diverse later outcomes, as measured by 90-10 ratios. This confirms the results of Table 2. However, in the bottom rows in Table 2, we see little evidence that children have more diverse outcomes (as measured by the 90-10 ratio) if they experience a more steeply sloped relationship between per-pupil spending and family income. Only two out of twelve coefficients are statistically significant and the point estimates for income are all negative. Also, the coefficients in the fourth rows of Table 3 indicate that the experience of more dispersed per-pupil spending does not have consistent effect on disparity of outcomes (only 2 of the twelve estimates are statistically significantly different from zero and the point estimates are not consistent in sign). In short, Table 3 confirms that parents with more unequal

incomes have children with more unequal outcomes, but the table does not otherwise confirm Table 2 and it provides little support for H1.

The evidence we draw from Table 4, which shows results for 75-25 ratios of the dependent variables, is similar to the evidence we draw from Table 3. We get the familiar result that the diversity of parents' income matters, but there is little to no evidence that disparity in the educational environment matters. Among the coefficients on the slope of relationship between per-pupil spending and family income, only one of twelve is statistically significantly different from zero and a few have the "wrong" sign. The coefficients on the disparity of per-pupil spending present us with a similar pattern. Overall, Table 4 does not provide support H1. Appendix Tables 2 and 3 contain the same regressions as Tables 3 and 4, but they are estimated for males only. They corroborate the evidence in Tables 3 and 4.

Given the mixed results from Tables 2, 3 and 4, we must conclude that the estimated effects of redistribution through education are sensitive to changes in the measure of inequality. Thus, a reasonable course of action is to focus on the most promising results in Table 2 and see whether they measure a true effect or merely reflect spurious correlation, such as might be generated by unobserved variables.

We were concerned, at the outset, that results might merely reflect unobserved factors that were trending within states –that is, unobserved state time trends. It is feasible to estimate an expanded version of equation (3) that allows each state to have its own linear time trend as well as its own constant. (Allowing each state to have its own non-linear time trend is infeasible, given the number of "experiments.") In Table 5, I show estimates of the equations that were most promising in Table 2, except that I allow for state-specific linear time-trends. The important thing to observe is that there are no longer statistically significant coefficients on the slope of the per-pupil spending-family income relationship. The statistically significant coefficients do not disappear only because the standard errors are higher in Table 5 than in Table 2. The coefficients themselves have been approximately halved and would be statistically insignificant even if their standard errors had remained the same. This halving of

the coefficients suggests that the Table 2 equations might have reflected omitted state-level time trends in the diversity of outcomes.

Another concern that we had at the outset is that school finance might be endogenous to policy makers' contemporaneous perceptions of the way in which inequality was heading. If this is the case, then the estimates in Table 2 might understate the true effects of redistribution through education. To test for this possibility, I try controlling for the time trend in income inequality (average annual rate of change in the diversity of family incomes) that a policy maker would have observed over the period that the students were in school.¹⁶ These results are shown in Table 6. The control for policy endogeneity in Table 6 is imperfect, but it should at least indicate the direction in which the estimates change when we attempt to account for policy endogeneity.

Looking at the estimates in Table 6, we see that controlling for the time trends that might have sparked policy does not make the key coefficient estimates rise. It appears that attempting to account for policy endogeneity *decreases* the estimated efficacy of redistribution through education: there is a substantial decrease in the estimated coefficients on the slope of the relationship between per-pupil and family income. In results that are not shown, I have experimented with controlling for the annual rate of change in a variety of other indicators that might have sparked policy changes: the unemployment rate, the size of the population, racial/ethnic diversity, and so on. Controlling for such trends produces results similar to those shown in Table 6: the estimated efficacy of redistribution through education falls. In short, the results suggest that policy endogeneity is not a primary concern –not a surprising result given states' diverse responses to similar economic trends (as shown in the quadrant figures).

Summing up, it appears that the evidence for the first part of H1 (more unequal per-pupil spending generates more unequal outcomes) is limited. An optimistic reading of the evidence would

¹⁶ I have also tried giving the policy makers more time to react by controlling for the time trend in income inequality that they would have observed for a period starting a few years earlier than the student's own time in school.

suggest that unequal per-pupil spending does increase the inequality of outcomes but that the magnitudes of the effects are so small that they are indiscernible over much of the distribution of outcomes. If one is looking for the virtuous cycle that is key to H1, one may not find it simply because the small effects allow the impact of school spending to dissipate before the cycle gets going strongly.

V.C. Results: How Redistribution through Education Affects Outcome *Growth*

Let us now turn to the effect of redistribution through education on the growth in outcomes. To study growth, I examine the level of students' outcomes, *given* their parents' outcomes. In other words, I estimate equation (3) with median and mean regression, not interquartile regression as I did in the previous section.

Table 7 shows the effects of educational environment on the median student's outcomes later in life. As expected, parents with higher median incomes have children with higher income and wage outcomes. (In Table 7, we see that their children's median educational outcomes are not statistically significantly higher. However, in Table 8, we shall see that their children's *mean* educational outcomes are statistically significantly higher. Therefore, it does not seem worthwhile to focus on the difference between the effects of parents' income on financial outcomes versus educational outcomes.)

Table 7 also shows that the diversity of parents' incomes does not affect the median child's outcomes in any consistent way. This is a reasonable result, as well, given that models generally require strong assumptions to link parents' diversity to children's outcomes levels. In addition, children who experience higher median per-pupil spending do not have outcomes that are typically higher (that have a higher median). This result might be surprising if there were not such a large literature showing that it is very difficult to prove that average school spending has a causal effect on achievement.¹⁷ While this paper is not meant primarily to contribute to the "does money matter?" debate, it should be noted that the lack of relationship between median per-pupil spending and median outcomes (a finding that appears in

¹⁷ See Betts (1996), Hanushek (2002), or Card (1999) for a review.

both Tables 7 and 8) does conflict rather directly with well-known results by Card and Krueger (1992) that also depend on state-by-cohort variation in school resources. Heckman, Layne-Ferrari and Todd (1996) have shown that Card and Krueger's results depend crucially on upward bias that their estimates receive from endogenous migration. (Essentially, people who migrate from low wage states to high wage states are positively self-selected on ability and motivation. Constructing estimates that rely on a comparison of them to non-migrants generates an upward bias in estimates of the effects of school quality because the migrants tended to complete an unusually large number of years of education, given their state of birth. For a complete understanding of this point, it is necessary to examine Card and Krueger's peculiar specification, in which returns to quality are channeled through the multiplier on a year of education, in defiance of conventional models of investment in human capital.) Perhaps because my estimates do not rely on the comparison of endogenous migrants to non-migrants, I do not find results similar to those of Card and Krueger. Instead, I equally weigh all people from a state of birth when computing the effects of its per-pupil spending.

The "does money matter" debate is a digression from my main purpose in this paper, so let us now focus on the estimates of primary interest: the coefficients of the diversity of per-pupil spending and the slope of the relationship between per-pupil spending and family income. In Table 7, these coefficients are roughly half negative and half positive. None of the coefficients on the slope is statistically significantly different from zero. Only one of the twelve coefficients on the diversity of per-pupil spending is statistically significantly different from zero and its sign suggests that the typical student's income *rises* when per-pupil spending becomes more diverse. Table 7 provides little evidence that the diversity of per-pupil spending or its correlation with family incomes sufficiently affects the efficiency by which educational investment are allocated to students to affect income growth.

There are several ways to check the median regression results in Table 7. Table 8 shows mean regressions. The pattern of results is very similar to that of Table 7. Table 8 indicates that higher income

parents have, on average, children with higher incomes, wages, and educational attainment. It also confirms the finding that children's average outcomes are not affected significantly by median per-pupil spending, the diversity of per-pupil spending, or the slope of the relationship between per-pupil spending and family income.

Table 9 adds state time trends to the median and mean regressions. Table 10 adds controls for policy endogeneity to the median and mean regressions. Making these changes to the specifications does not increase the support for H1. Indeed, a small number of the key coefficients become statistically significantly different from zero with their signs in the wrong direction for support of H1. Finally, Appendix Table 4 shows the median regressions for males only. It confirms the results in Tables 7.

Overall, the evidence provides little to no support for the second part of H1 (income growth will increase if per-pupil spending is made more equal and less correlated with family income). In fact, the evidence for the first part of H1, which is limited, turns out to be the strongest evidence for H1. Perhaps this is to be expected. The first part of H1 relies on a straightforward logic and assumptions: allowing educational opportunity to vary with parents income will exacerbate (or at least not suppress) the intergenerational transmission of income inequality. The second part of H1 relies on more complex assumptions: (a) that ability to benefit from education is relatively uncorrelated with parents' income, (b) that the rents taken from school resources are unrelated to how much of the revenue comes from state as opposed to local parents, and (c) that resources make their way to students just as efficiently in schools that serve low-income students (who typically cannot move if dissatisfied) as in schools that serve high-income students (who typically can move if dissatisfied), and so on.

V.D. Future Results: Intergenerational Transmission in the Panel Survey of Income Dynamics

Future versions of this paper may include results that attempt to link state-by-cohort specific rates of intergenerational income transmission to the educational environment variables found on the right hand side of equation (3). These results use the PSID. Preliminary estimates provide little

indication that there are statistically significant relationships, but we must be cautious because state-by-cohort cells are small in the PSID, and small cells generate imprecise estimates of intergenerational transmission. There is a trade-off between merging cohorts into the same cell in order to make the cells larger and creating cells whose cohorts have overlapping (and therefore non-independent) school experiences.

VI. Conclusions

In this paper, I attempt to test directly the popular hypothesis ("H1") that income inequality is reduced and income growth is increased when educational opportunity is made more equally available to children from all family income backgrounds students. As described at the outset, this hypothesis' broad appeal probably owes most to its logic. However, it may also get support from the often indirect and nearly always non-credible evidence associated with it. This paper takes seriously the task of finding evidence that is both direct and credible. The goal is not only to find evidence of a "yes" or "no" variety, but also to understand the magnitude of the relationship described by H1. Currently, the United States depends heavily on H1 as a method of redistribution. If the magnitude of H1 is small, then it may be useful to shift some government spending to alternative methods of redistribution, such as income transfers. Alternatively, it may be useful to re-examine how redistributed money is spent in schools, so this the H1 channel can be more efficient. What is clear is that we ought to know about the magnitude of the H1 mechanism if we are to rely on it.

Using data from four decades, I exploit circumstances that should be propitious for testing H1. In particular, I demonstrate that there is considerable within-state variation in school finance policy and that the changes in educational spending equality are not correlated with changes in family income equality. Thus, the variation should allow the econometrician to identify separately the effects of family income and school spending. Finding such circumstances and separating these effects is precisely what

the typical cross-national study of growth does not do. This is why such studies tend not to be credible.

Overall, I find weak evidence for the first part of H1 (that allowing educational opportunities to be unequal causes children's later outcomes to be more unequal). The evidence is weak because it is not robust to reasonable variation in the measure of outcome inequality and it is not robust to allowing state-specific linear time trends. One interpretation of the evidence is that the relationship that I describe as the "first part of H1" does exist but is so small that it is not discernible over much of the distribution of outcomes. I find no evidence for the second part of H1 (that making educational opportunities more equal or less correlated with family income causes growth). The tests that I conduct in this paper do not reveal *why* H1 fails to be substantial in size. There are several possibilities (correlation between parents' income and children's ability to benefit from school resources, opportunities for rent extraction being negatively correlated with parents' income, etc.). These possibilities require separate testing and, indeed, there are studies that implicitly test them. Clearly, if the United States is to rely heavily on education as a channel for redistribution, it will be effective only if the magnitude of the H1 process is substantial. If (as the evidence suggests) it is not substantial now, then understanding how and why it breaks down is important.

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* please note these references are preliminary and may be incomplete

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Figure 1: Example of Data Collection for a Birth Cohort

1950 Birth Cohort

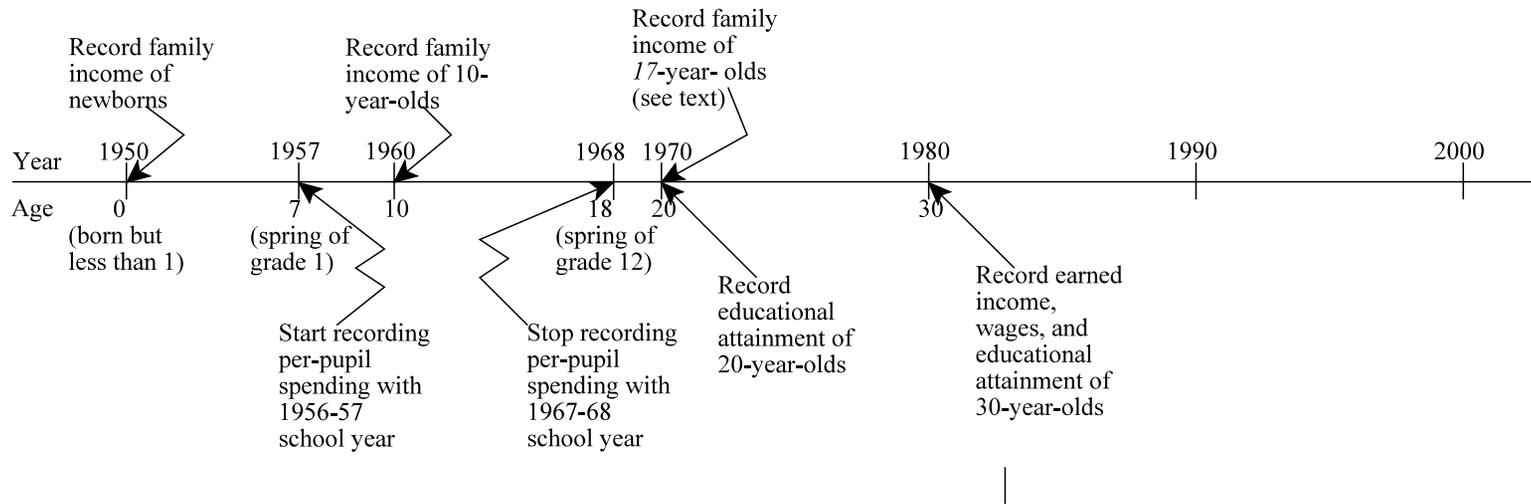


Figure 2: The Distribution of Family Incomes *Within* States, 1960 and 1970

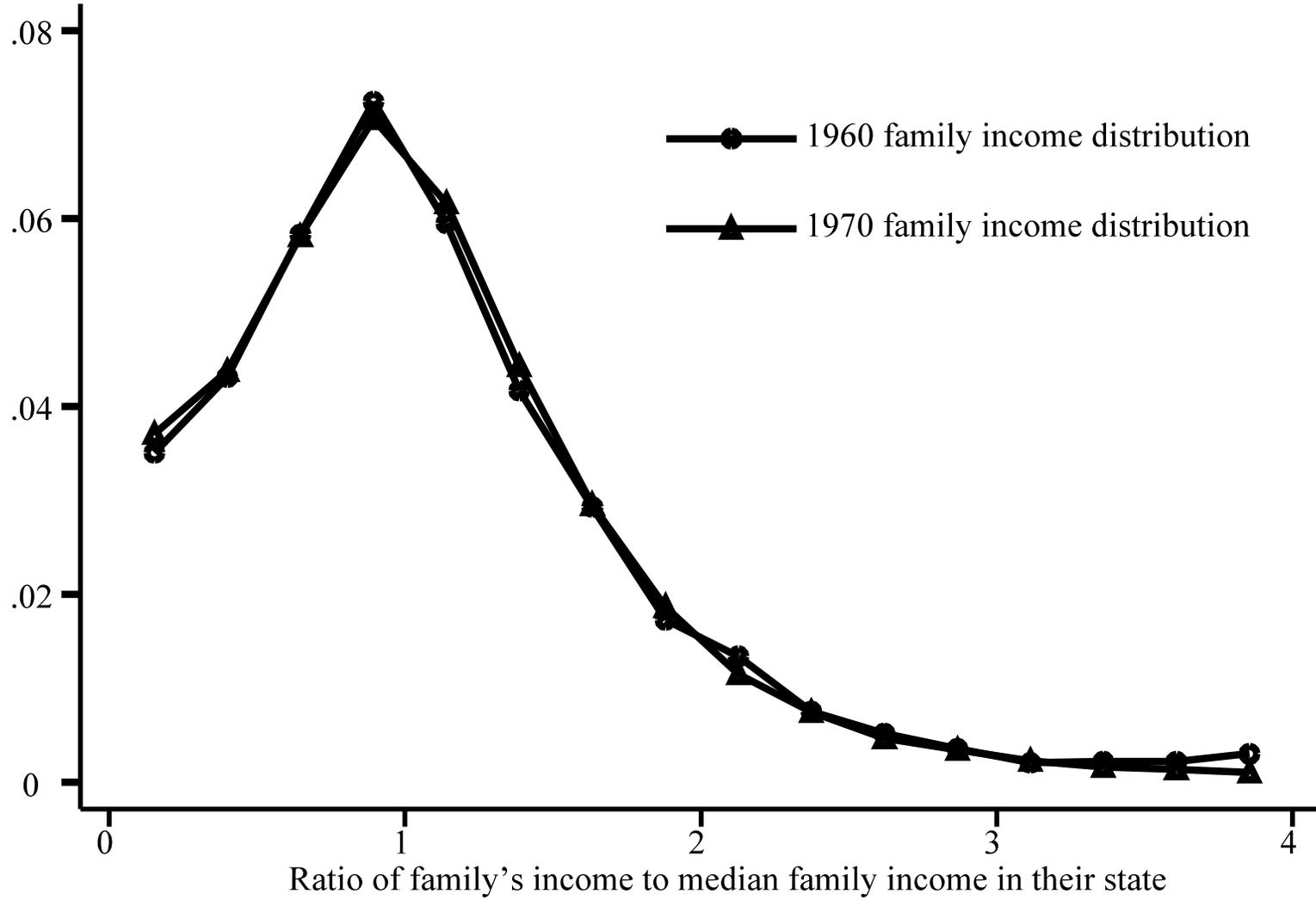


Figure 3: The Distribution of Family Incomes *Within* States, 1970 and 1980



Figure 4: The Distribution of Family Incomes *Within* States, 1980 and 1990

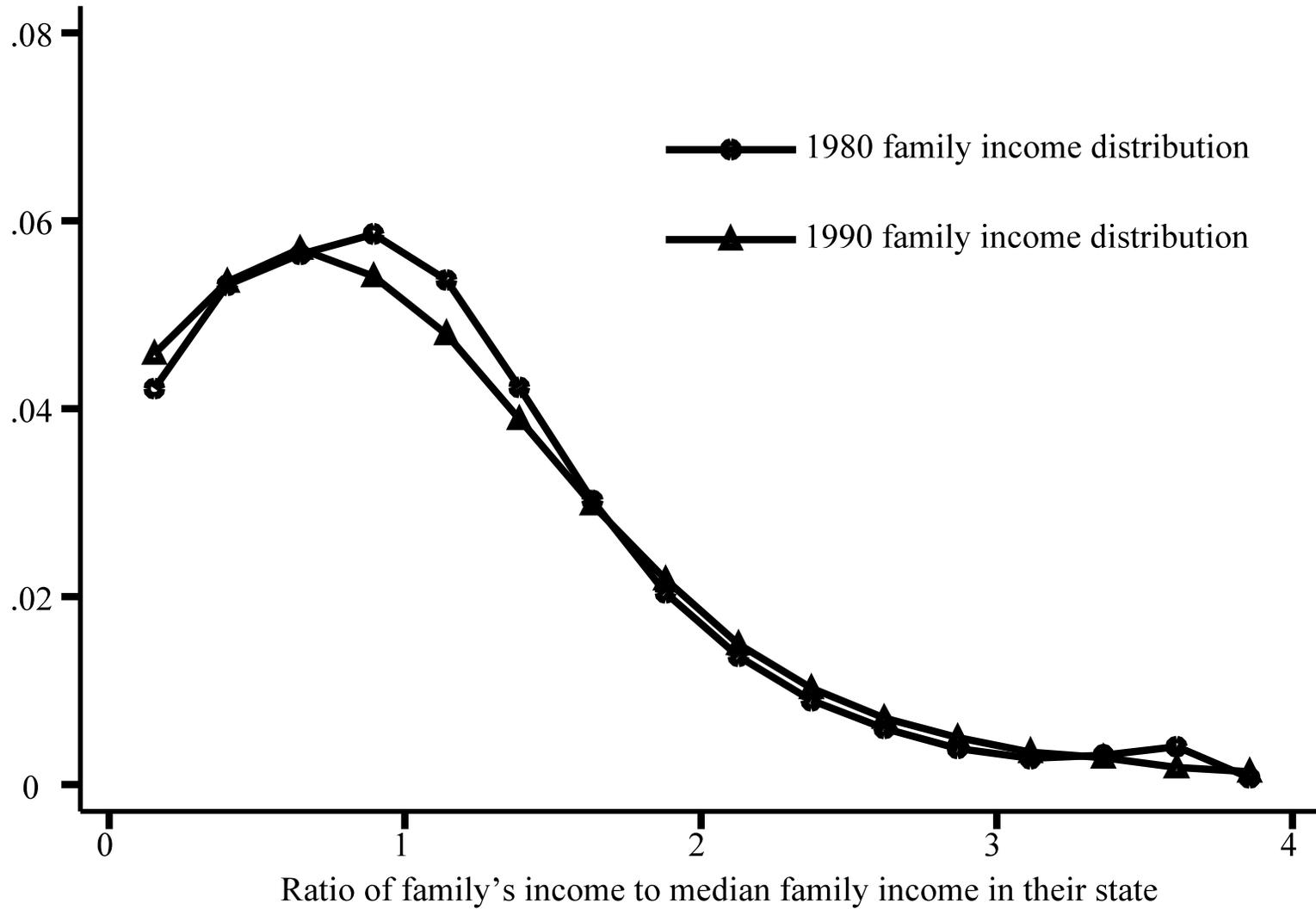


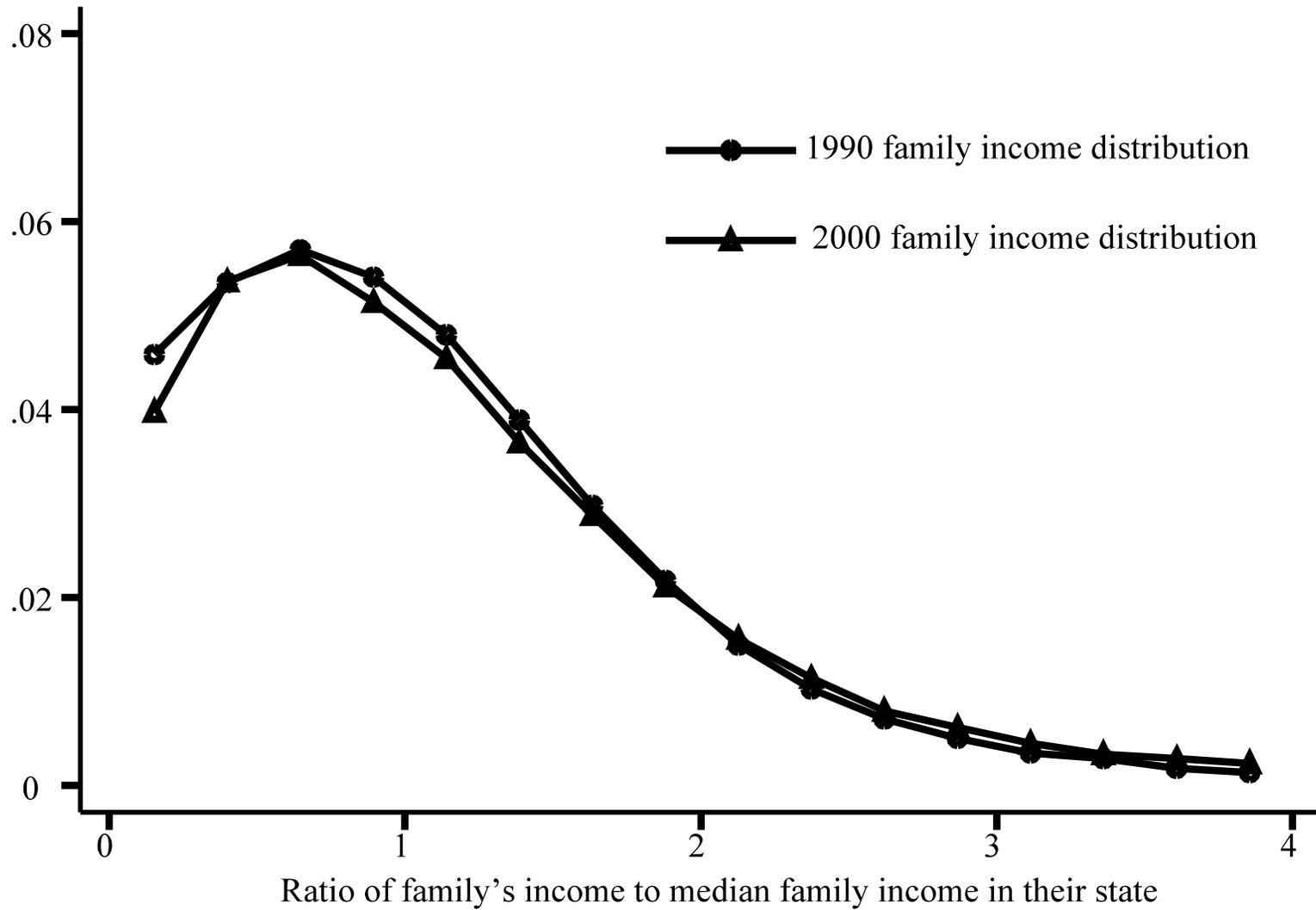
Figure 5: The Distribution of Family Incomes *Within* States, 1990 and 2000

Figure 6: The Distribution of Family Incomes *Between* States, 1960 to 2000

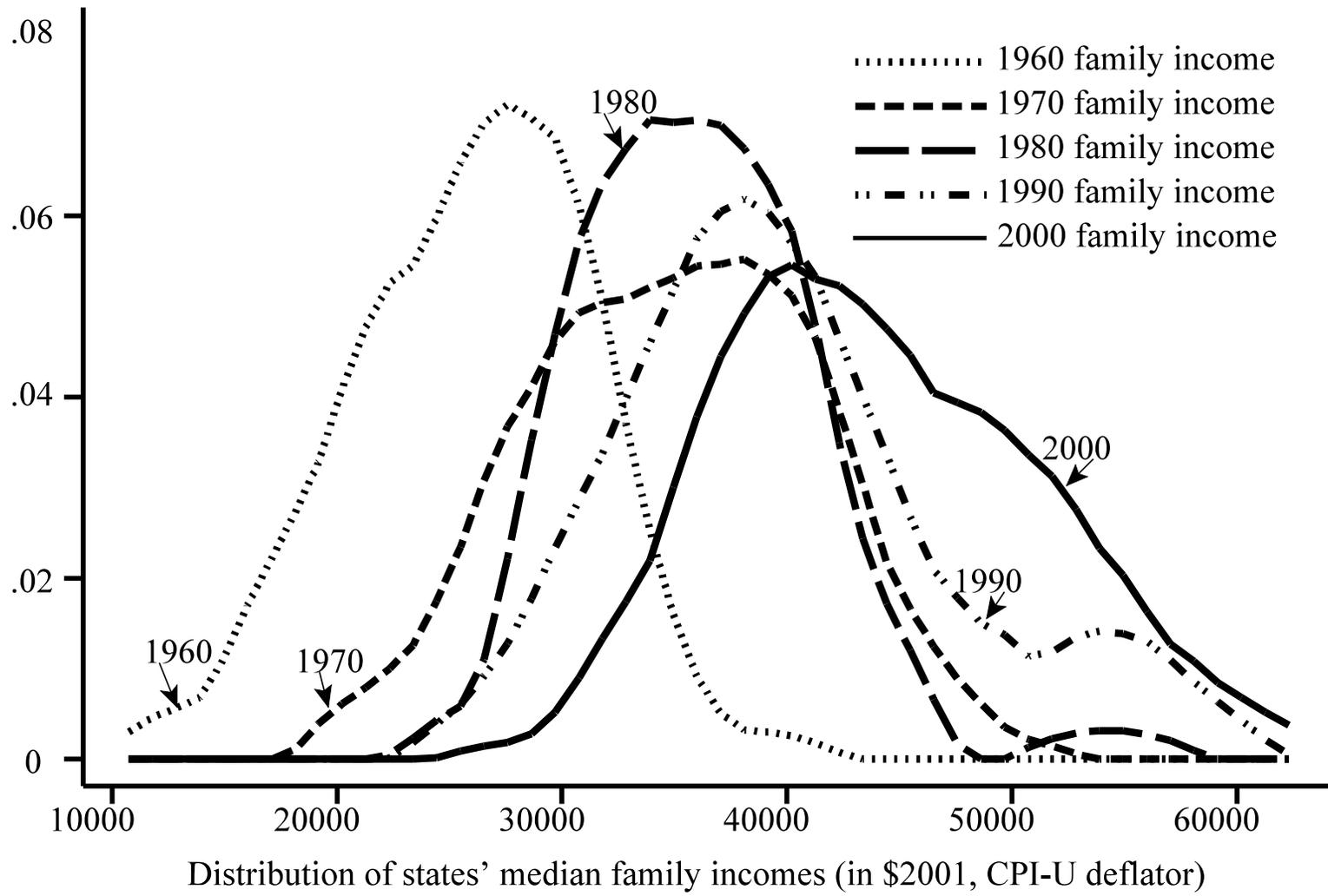
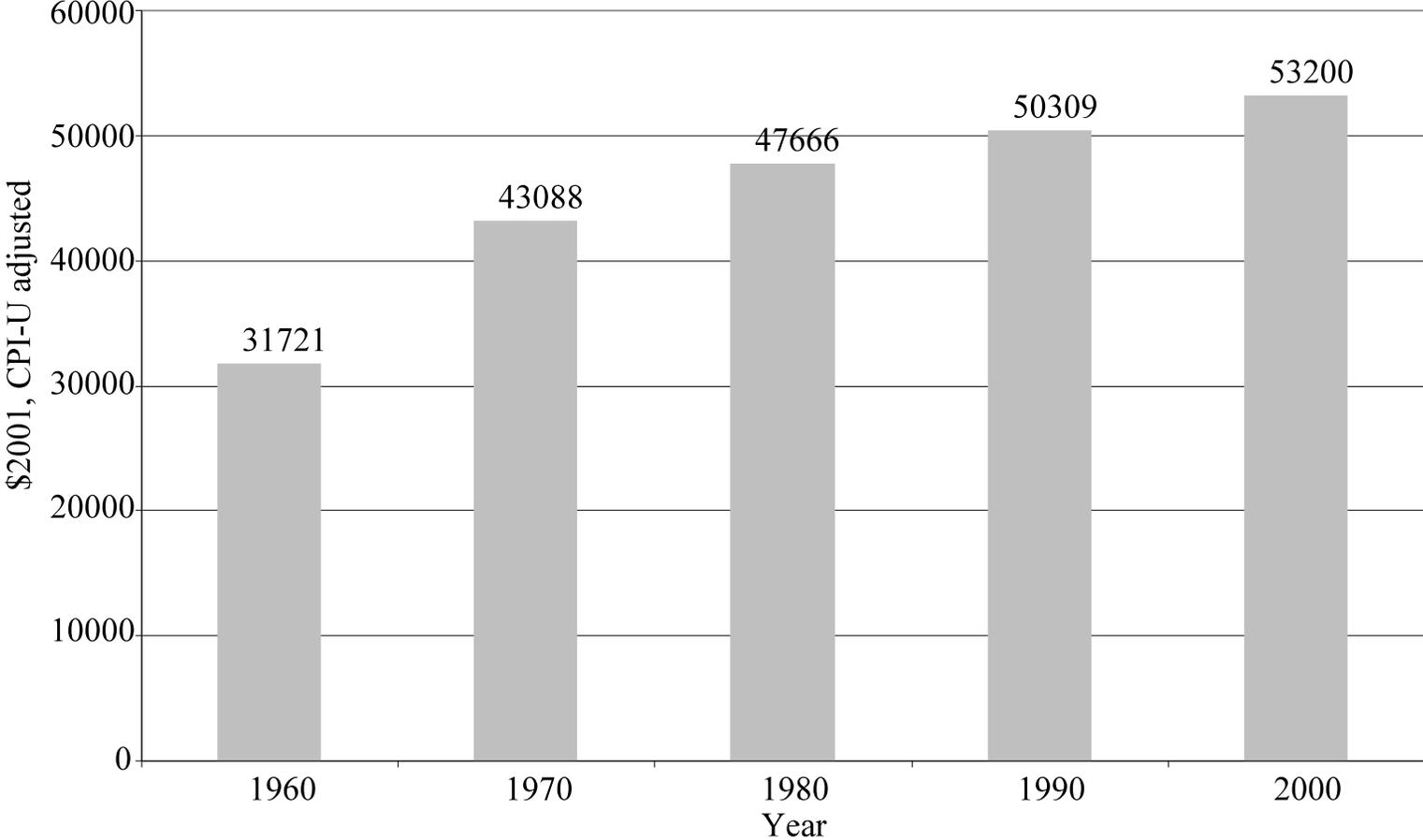


Figure 7: Median Family Income in the U.S. (\$2001, CPI-U adjusted)



Source: U.S. Bureau of the Census, Table S2

Figure 8: The Distribution of Per-Pupil Spending *Within* States, 1960 and 1970

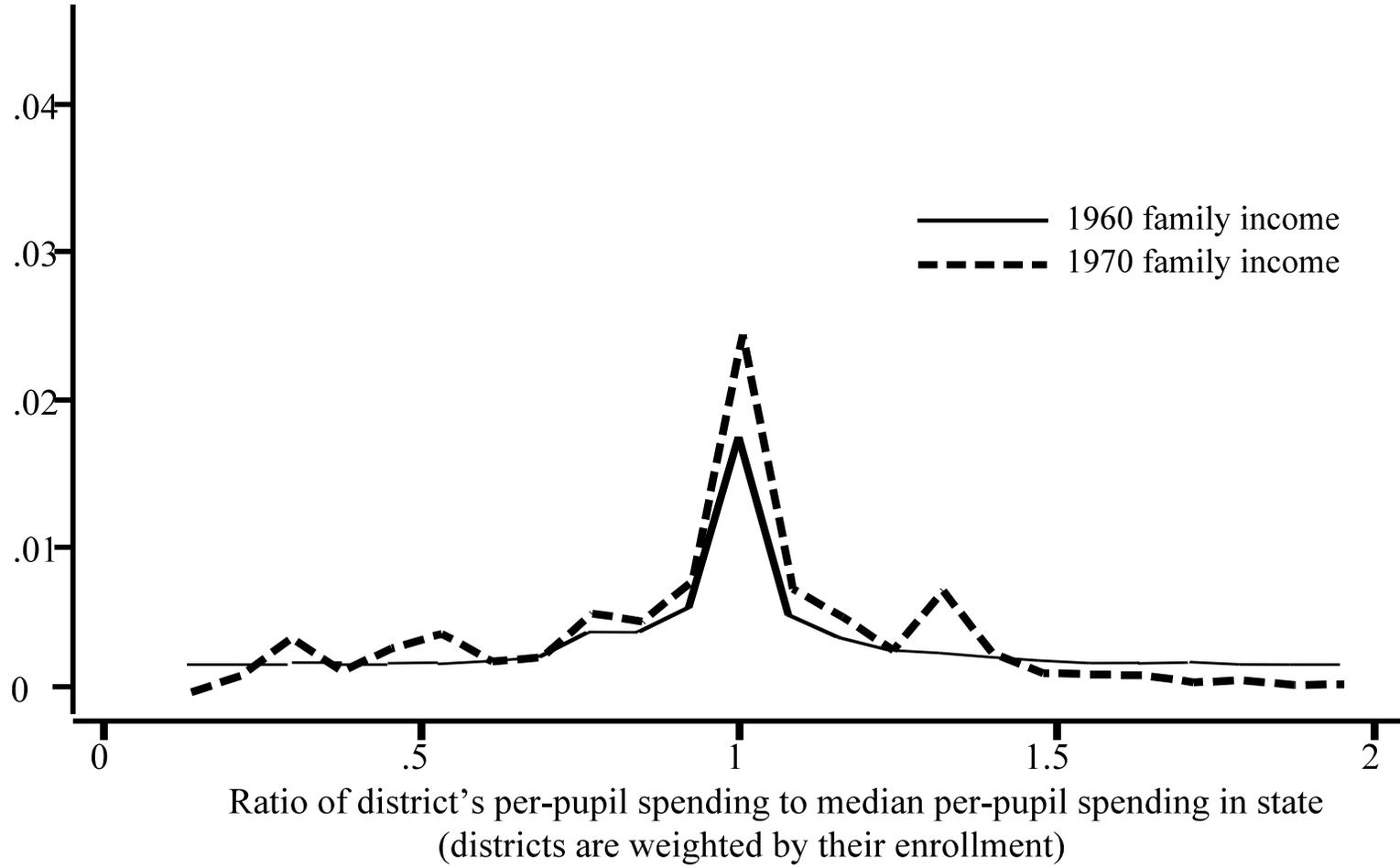


Figure 9: The Distribution of Per-Pupil Spending *Within* States, 1970 and 1980

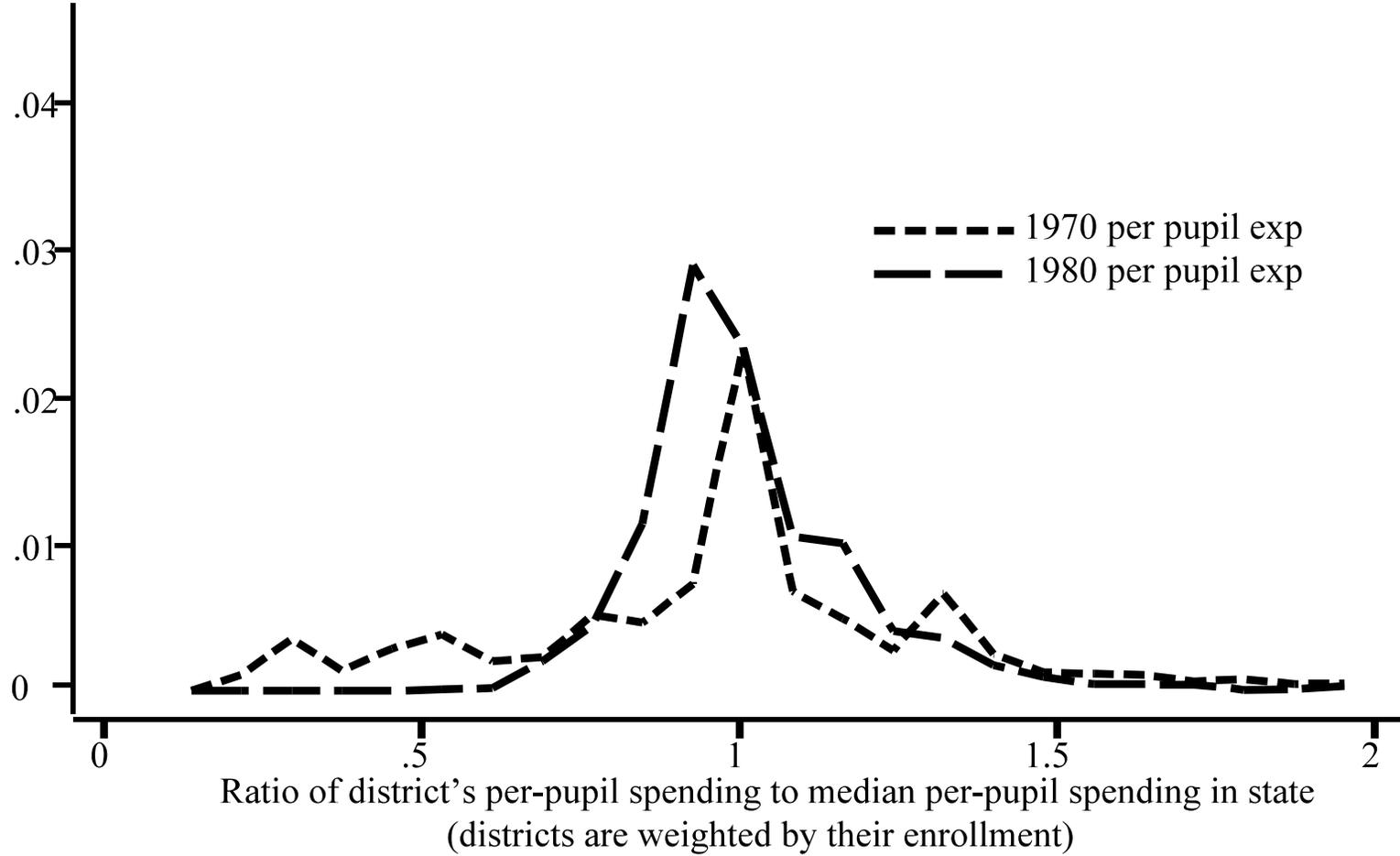


Figure 10: The Distribution of Per-Pupil Spending *Within* States, 1980 and 1990

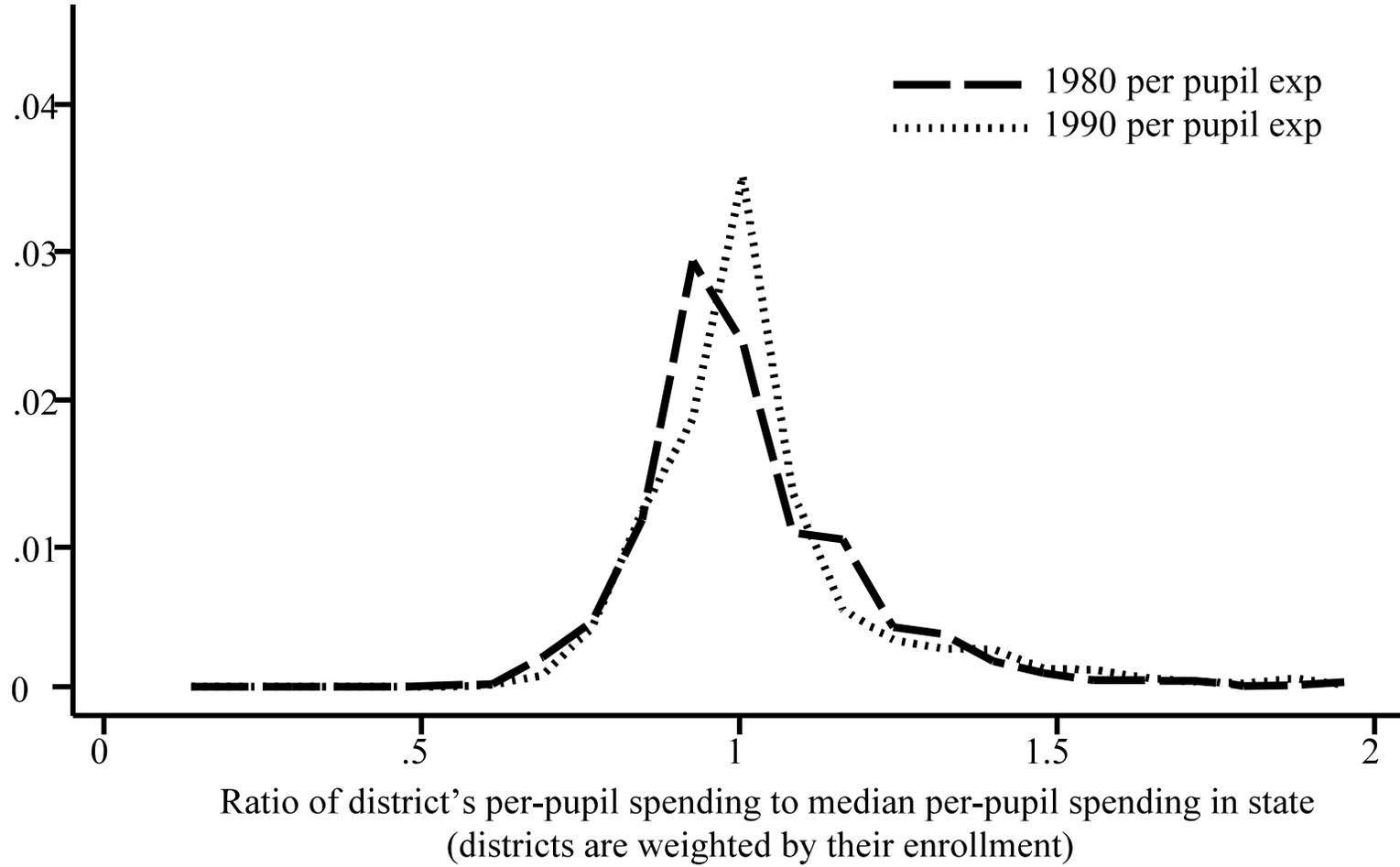


Figure 11: The Distribution of Per-Pupil Spending *Within* States, 1990 and 2000

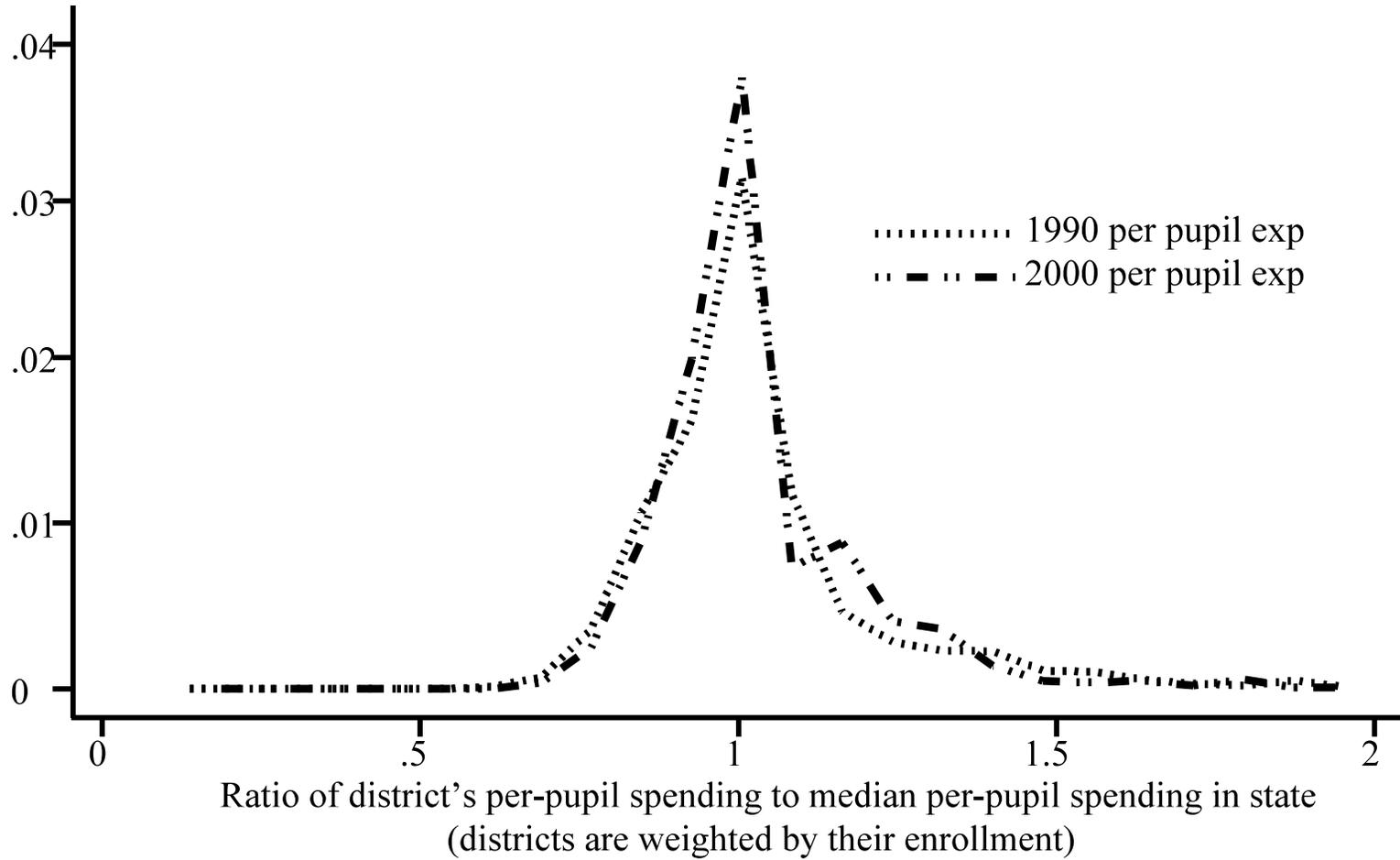


Figure 12: The Distribution of Per-Pupil Spending *Between* States, 1960 to 2000

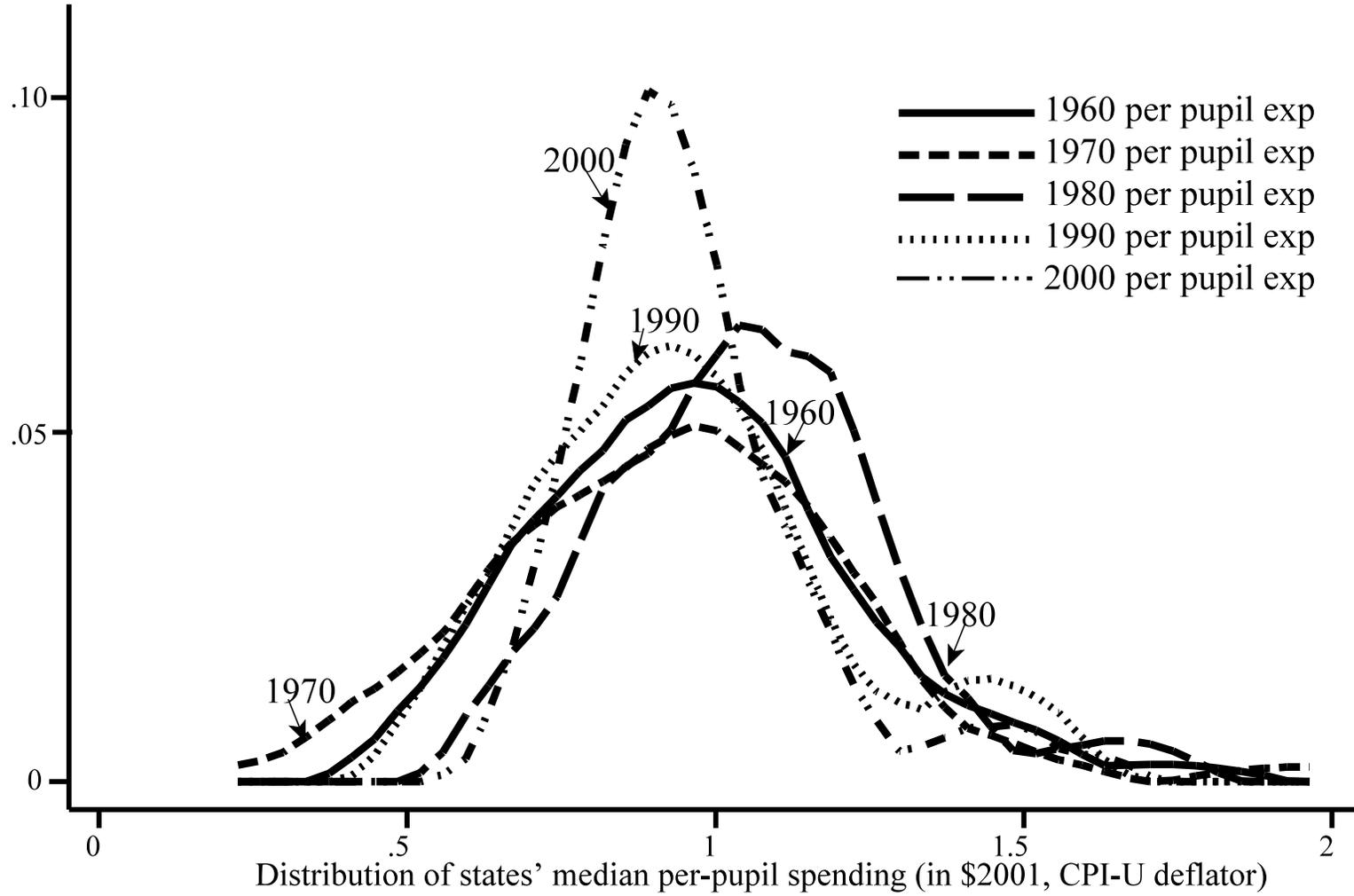
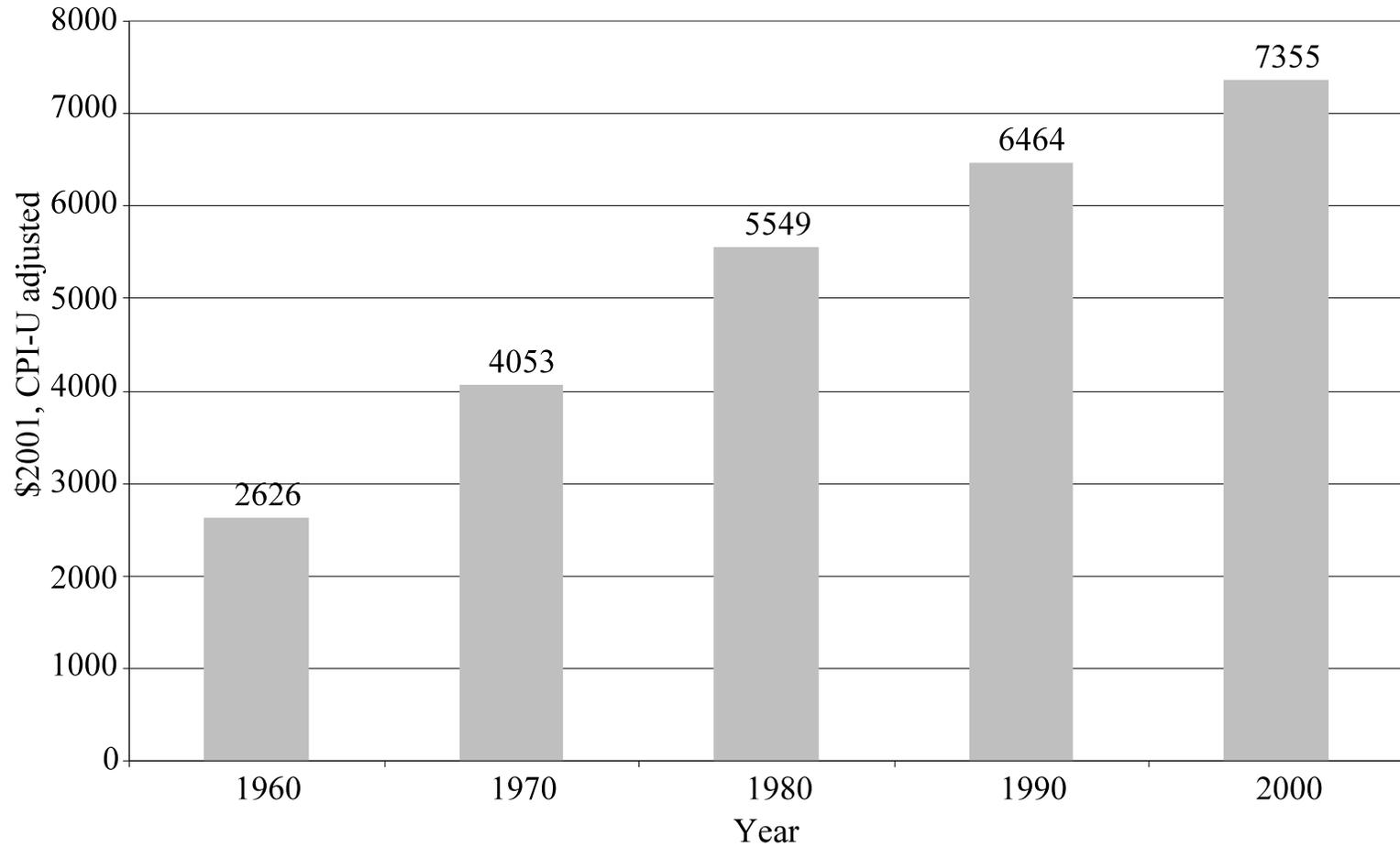


Figure 13: Median Per-Pupil Spending in the U.S. (\$2001, CPI-U adjusted)



Note: For calculations, each district's per-pupil spending is weighted by its enrollment

Figure 14
The 1960s: Change in Per-Pupil Spending Diversity versus
Change in Family Income Diversity, by State

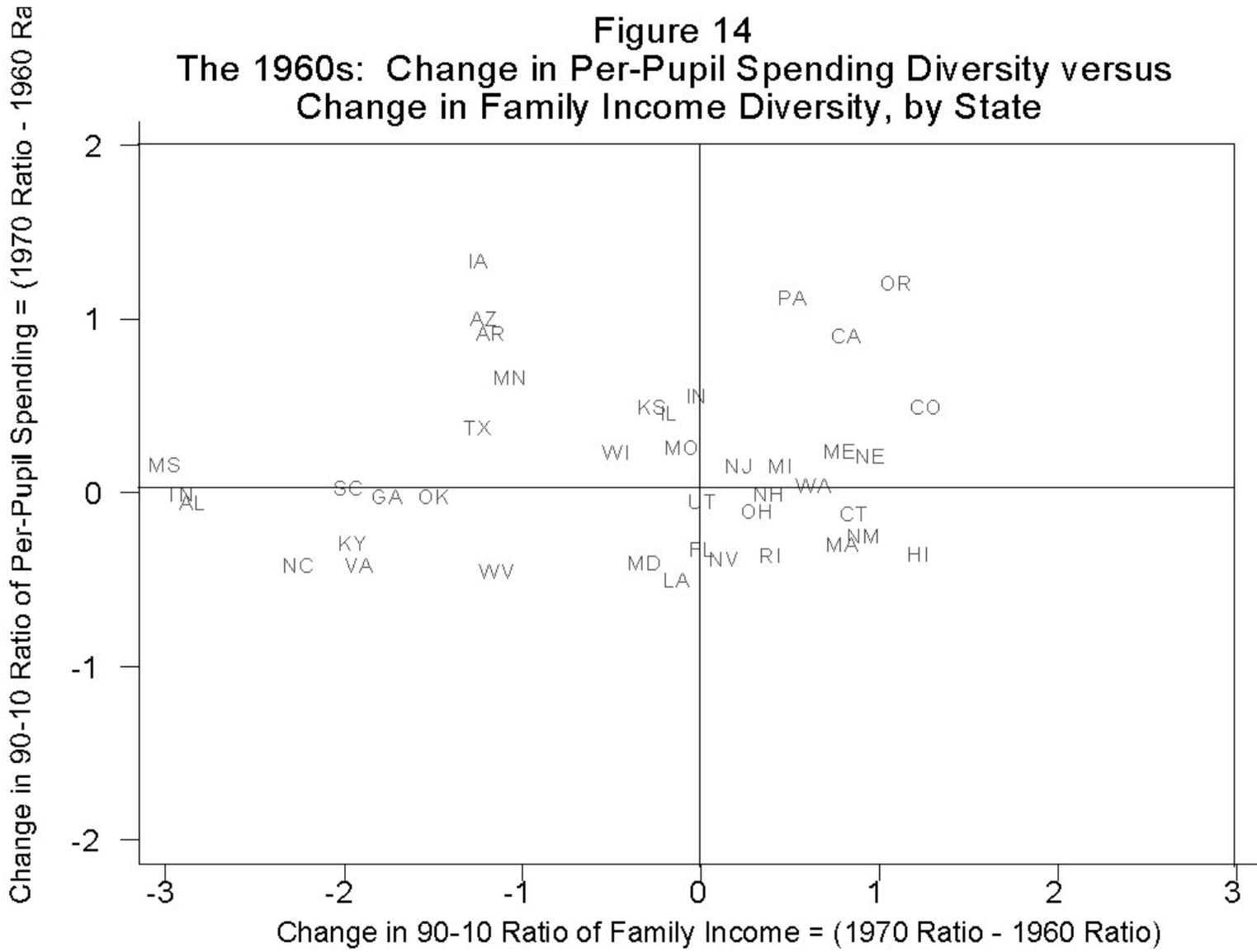


Figure 15
The 1970s: Change in Per-Pupil Spending Diversity versus
Change in Family Income Diversity, by State

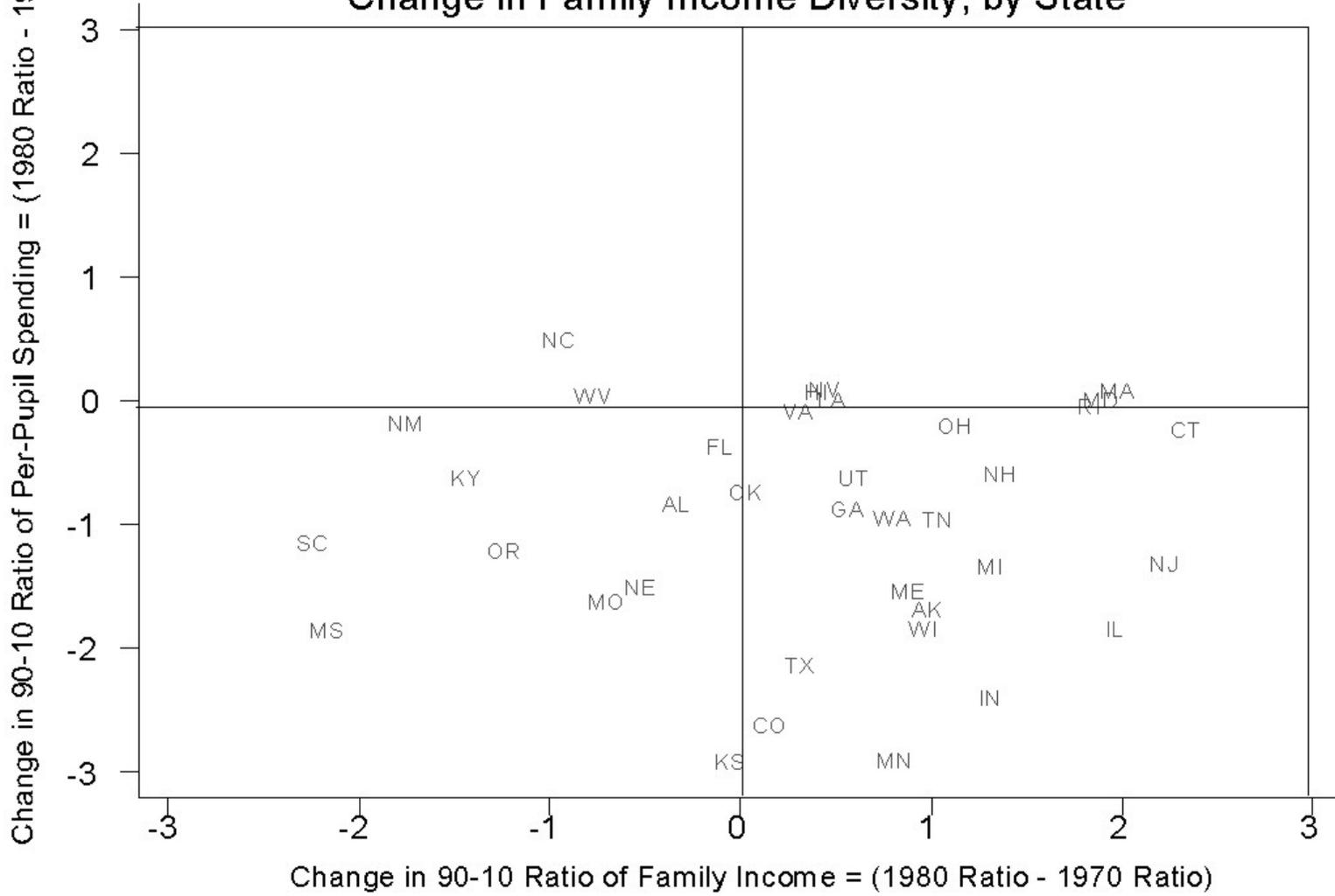


Figure 16
 The 1980s: Change in Per-Pupil Spending Diversity versus
 Change in Family Income Diversity, by State

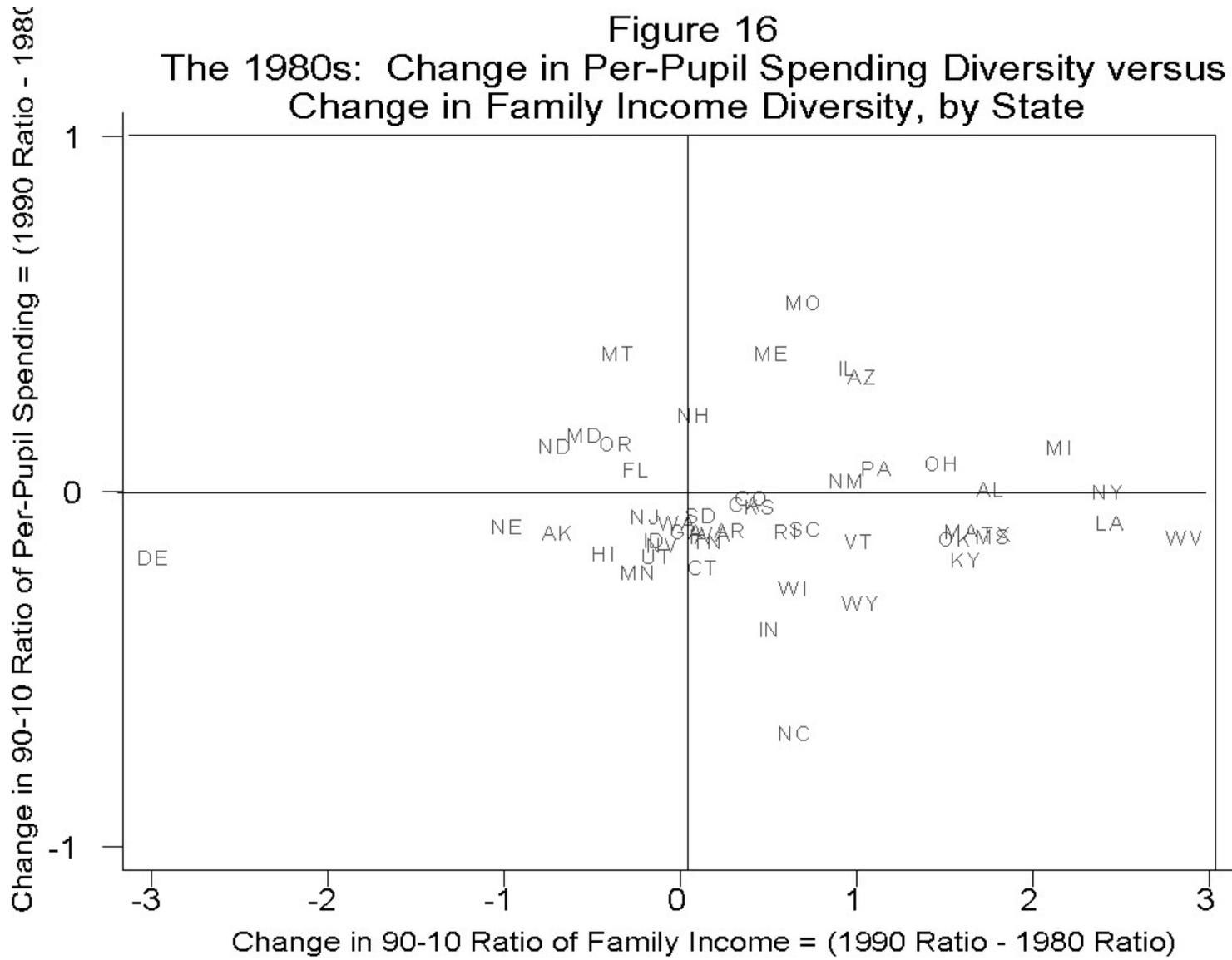


Figure 17
The 1990s: Change in Per-Pupil Spending Diversity versus
Change in Family Income Diversity, by State



Table 1

| Estimated Coefficient from a Univariate Regression of Per-Pupil Spending on Family Income | | | | | | |
|---|-------|--------|---------|--------|--------|--------|
| State: | Year: | 1960 | 1970 | 1980 | 1990 | 2000 |
| Alaska | | 14.530 | 0.971 | -3.030 | -1.858 | -6.164 |
| Alabama | | 0.732 | 1.420 | 0.413 | 2.382 | 3.351 |
| Arkansas | | 1.195 | -8.989 | 0.764 | 1.537 | 0.599 |
| Arizona | | 1.019 | 9.977 | -1.493 | 0.552 | -1.580 |
| California | | 0.827 | 1.429 | -0.513 | -0.016 | 2.535 |
| Colorado | | 0.964 | 5.000 | 0.518 | 1.170 | 2.626 |
| Connecticut | | -0.241 | 4.827 | 3.295 | 3.242 | -0.837 |
| Delaware | | -2.184 | 1.837 | 1.147 | 1.892 | 1.793 |
| Florida | | 0.235 | 0.695 | 1.251 | 2.006 | 0.249 |
| Georgia | | 1.999 | -0.273 | -0.296 | 4.781 | 2.233 |
| Hawaii | | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Iowa | | 0.127 | -1.133 | -0.276 | -0.016 | -0.419 |
| Idaho | | 0.518 | -1.902 | -0.883 | 0.162 | 0.102 |
| Illinois | | 0.242 | -5.733 | 4.728 | 11.875 | 10.304 |
| Indiana | | 0.610 | -0.445 | -0.355 | 0.293 | -1.393 |
| Kansas | | 2.615 | 1.546 | -0.991 | -0.203 | -1.113 |
| Kentucky | | 3.134 | -0.379 | 1.924 | 2.166 | 1.011 |
| Louisiana | | -1.664 | 0.579 | 1.914 | 0.985 | 1.068 |
| Massachusetts | | 1.700 | 2.584 | 1.774 | 5.407 | -0.975 |
| Maryland | | 4.111 | 5.430 | 4.874 | 9.964 | 3.304 |
| Maine | | 0.755 | 3.135 | 0.442 | 0.692 | 2.291 |
| Michigan | | 1.186 | -0.745 | 3.015 | 8.472 | 5.876 |
| Minnesota | | 0.933 | -10.376 | -3.871 | 2.816 | -1.386 |
| Missouri | | 2.241 | 5.591 | 2.795 | 1.700 | 3.079 |
| Mississippi | | 2.075 | 2.397 | 0.288 | -0.034 | -0.857 |
| Montana | | -0.186 | -3.987 | -0.934 | -0.922 | -3.026 |
| North Carolina | | 3.117 | 0.375 | 1.955 | 1.629 | 2.006 |
| North Dakota | | 1.434 | -7.576 | -2.120 | -2.712 | -0.008 |
| Nebraska | | -0.098 | -5.759 | -1.176 | -0.517 | 0.043 |
| New Hampshire | | 0.099 | -0.098 | 4.159 | 4.526 | 2.715 |
| New Jersey | | 1.075 | 2.985 | 5.093 | 6.306 | -0.306 |
| New Mexico | | 0.003 | -1.000 | -1.525 | -1.488 | -1.297 |
| Nevada | | -0.132 | -0.461 | -0.320 | -0.559 | -0.527 |
| New York | | 3.482 | -8.638 | 10.638 | 10.181 | 6.733 |
| Ohio | | 0.410 | 2.504 | 0.594 | 1.706 | 2.806 |
| Oklahoma | | 0.271 | -6.681 | 0.764 | -0.821 | -1.341 |
| Oregon | | -0.108 | -1.470 | 0.242 | 1.606 | 0.943 |
| Pennsylvania | | 0.856 | 1.994 | 3.418 | 8.777 | 8.576 |
| Rhode Island | | 0.416 | -0.020 | 0.654 | 1.234 | -1.226 |
| South Carolina | | 1.104 | 2.078 | 0.612 | 1.492 | 0.391 |
| South Dakota | | 1.708 | -7.469 | -2.537 | -2.045 | -4.349 |
| Tennessee | | 1.737 | 0.271 | 1.227 | 1.481 | 1.374 |
| Texas | | 1.532 | -1.319 | 1.687 | 1.569 | 1.309 |
| Utah | | 0.120 | -1.719 | -2.529 | -1.740 | 0.041 |
| Virginia | | 5.649 | 7.811 | 7.246 | 11.501 | 6.986 |
| Vermont | | 0.237 | 1.455 | 3.030 | 2.997 | 0.935 |
| Washington | | 0.753 | -1.284 | -0.166 | 1.740 | 2.071 |
| Wisconsin | | -0.173 | -8.589 | 0.895 | 3.521 | 0.824 |
| West Virginia | | 1.698 | -0.123 | 0.227 | 0.698 | -0.607 |

| Estimated Coefficient from a Univariate Regression of Per-Pupil Spending on Family Income | | | | | | |
|---|-------|--------|-------|-------|--------|-------|
| State: | Year: | 1960 | 1970 | 1980 | 1990 | 2000 |
| Wyoming | | -3.242 | 1.439 | 4.031 | -1.836 | 1.942 |

Notes to Table 1: The estimated coefficients shown are from a regression of per-pupil spending on a constant and family income. A separate regression is run for each state and year. The observations in the regression are at the district-by-family income category level. Each observation is therefore weighted by the number of families in the district-by-family income category cell. Data sources are: the 1962, 1972, 1982, and 1992 Census of Governments; the Public Elementary-Secondary Education Finances, 2000-01; Hoxby and Reber (2003); the Common Core of Data: Public School District Universe Data, 1990-91 and 2000-01; the Elementary and Secondary General Information System: Public School District Universe Data, 1970-71 and 1980-81; the 1970 Census Fourth Count: School District Data Tape; the National Institute of Education Special Tabulation and Fifth Count Data File; the Office of Revenue Sharing 1970 Fifth Count Census and ORS Revenue Sharing File; the Census-Tract Level Data 1960; the County and City Data Book 1962; the 1980 Census of Population and Housing Summary Tape File 3F; the School District Data Book for 1990; the School District Demographic data for 2000.

Table 2
The Diversity of Students' Later Outcomes (as measured by the coefficient of variation)
Regressed on Educational Environment

| dependent variable: | coefficient of variation in earned income at age 30 | | | coefficient of variation in wage at age 30 | | |
|---|--|---------------------------------|---------------------------------|--|--------------------------------|--------------------------------|
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | 0.003 (0.005) | 0.005 (0.005) | 0.005 (0.005) | 0.003 (0.008) | 0.002 (0.007) | 0.002 (0.007) |
| diversity of parents' income in student's state (see column heading for measure) | 0.112 (0.056) | 0.027 (0.012) | 0.024 (0.012) | 0.152 (0.123) | 0.035 (0.023) | 0.031 (0.023) |
| median per-pupil spending experienced by student | -0.057 (0.033) | -0.066 (0.031) | -0.064 (0.031) | 0.017 (0.048) | 0.001 (0.046) | 0.001 (0.046) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.089 (0.130) | -0.001 (0.026) | 0.078 (0.080) | 0.128 (0.189) | 0.015 (0.037) | 0.063 (0.117) |
| slope of relationship between per- pupil spending and family income | 14.612 (6.647) | 14.371 (6.744) | 15.765 (6.386) | 7.843 (9.698) | 15.401 (9.882) | 13.946 (9.379) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: | coefficient of variation in highest grade completed by age 30 | | | coefficient of variation in highest grade completed by age 20 | | |
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | 0.001 (0.001) | 0.001 (0.001) | 0.001 (0.001) | 0.001 (0.001) | -0.001 (0.000) | -0.001 (0.001) |
| diversity of parents' income in student's state (see column heading for measure) | 0.049 (0.016) | 0.010 (0.002) | 0.012 (0.003) | 0.022 (0.011) | 0.005 (0.002) | 0.004 (0.002) |
| median per-pupil spending experienced by student | 0.001 (0.003) | -0.001 (0.003) | -0.002 (0.003) | 0.001 (0.003) | 0.002 (0.003) | 0.002 (0.003) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.008 (0.012) | 0.001 (0.003) | 0.001 (0.008) | -0.018 (0.017) | -0.001 (0.003) | 0.007 (0.010) |
| slope of relationship between per- pupil spending and family income | 0.519 (0.603) | 1.046 (0.671) | 1.079 (0.634) | 2.215 (0.797) | 2.652 (0.794) | 2.702 (0.770) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Table 2: Table shows coefficient estimates from regressions. Each regression is in a column. Dependent variable is indicated at the top of each column. Standard errors are in parentheses. Observations are at the cohort-year level where the cohorts are the 1950, 1960, 1970, and 1980 birth cohorts. Regressions with age 20 outcomes contain 200 observations. Regressions with age 30 outcomes contain 150 observations. Data sources are: the 1962, 1972, 1977, 1982, 1987 and 1992 Census of Governments; the 1972-76, 1978-81, 1983-86, and 1987-91 Annual Surveys of Governments; the Public Elementary-Secondary Education Finances, 1992-93 through 2000-01; Hoxby and Reber (2003); the Common Core of Data: Public School District Universe Data, 1986-87 through and 2000-01; the Elementary and Secondary General Information System: Public School District Universe Data, 1969-70 through

1985-86; the 1970 Census Fourth Count: School District Data Tape; the National Institute of Education Special Tabulation and Fifth Count Data File; the Office of Revenue Sharing 1970 Fifth Count Census and ORS Revenue Sharing File; the Census-Tract Level Data 1960; the County and City Data Book 1962; the 1980 Census of Population and Housing Summary Tape File 3F; the School District Data Book for 1990; the School District Demographic data for 2000; the 1950, 1960, 1970, 1980, 1990, and 2000 Integrated Public Use Microdata System for the Census of Population and Housing.

Table 3
The Diversity of Students' Later Outcomes (as measured by the 90-10 ratio)
Regressed on Educational Environment

| dependent variable: | 90-10 ratio of earned income (coefficients divided by 1000 for convenience) | | | 90-10 ratio of wage | | |
|---|---|--------------------------------|--------------------------------|---|---------------------------------|---------------------------------|
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | -0.026 (0.014) | -0.012 (0.010) | -0.012 (0.010) | -0.149 (0.046) | -0.113 (0.044) | -0.119 (0.045) |
| diversity of parents' income in student's state (see column heading for measure) | 1.070 (0.531) | 0.238 (0.099) | 1.132 (0.533) | 3.310 (1.621) | 0.549 (0.201) | 2.385 (1.160) |
| median per-pupil spending experienced by student | 0.149 (0.120) | 0.105 (0.108) | 0.134 (0.107) | 0.978 (0.295) | 0.742 (0.284) | 0.784 (0.286) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 1.018 (0.531) | 0.101 (0.056) | 0.392 (0.182) | 2.297 (1.166) | 0.328 (0.232) | 0.571 (0.727) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | -22.020 (17.761) | -19.177 (18.188) | -23.554 (16.802) | 11.373 (59.853) | 25.823 (61.082) | 5.363 (58.057) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: | 90-10 ratio of highest grade completed by age 30 | | | 90-10 ratio of highest grade completed by age 20 | | |
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | 0.005 (0.004) | 0.004 (0.004) | 0.004 (0.004) | 0.002 (0.002) | 0.001 (0.002) | 0.001 (0.002) |
| diversity of parents' income in student's state (see column heading for measure) | 0.369 (0.139) | 0.035 (0.017) | 0.201 (0.100) | 0.126 (0.055) | 0.013 (0.006) | 0.075 (0.040) |
| median per-pupil spending experienced by student | -0.002 (0.025) | -0.002 (0.024) | -0.005 (0.025) | -0.023 (0.012) | -0.021 (0.012) | -0.022 (0.012) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.046 (0.100) | -0.018 (0.020) | 0.006 (0.063) | -0.095 (0.067) | -0.014 (0.013) | 0.012 (0.041) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | -0.446 (5.125) | 3.932 (5.234) | 3.876 (5.028) | 5.581 (3.239) | 6.950 (3.221) | 7.923 (3.152) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Table 3: All table notes from Table 2 apply, with the following exceptions. The observations in the regressions shown above at the individual level. Individuals are classified by state of birth and the birth cohort (the cohorts in the data are the 1950, 1960, 1970, and 1980 birth cohorts). Most of the explanatory variables are not at

the individual level, but the cohort-by-state level. Therefore, the standard errors shown in parentheses are calculated using Moulton's (1986) procedure for dealing with clustered observations. Regressions with age 20 outcomes contain 200 cohort-by-state clusters. Regressions with age 30 outcomes contain 150 cohort-by-state clusters.

Table 4
The Diversity of Students' Later Outcomes (as measured by the 75-25 ratio)
Regressed on Educational Environment

| dependent variable: | 75-25 ratio of earned income | | | 75-25 ratio of wage | | | | |
|--|-----------------------------------|---------------------------------|---------------------------------|--------------------------------|-----------------------------------|--------------------------------|-------------|-------------|
| measure of diversity: | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio | | |
| | of variation | | | of variation | | | | |
| median income of parents in student's state | 0.031 (0.066) | 0.013 (0.059) | 0.030 (0.061) | 0.001 (0.004) | 0.002 (0.004) | 0.003 (0.004) | | |
| diversity of parents' income in student's state (see column heading for measure) | 4.363 (2.250) | 0.554 (0.269) | 0.837 (0.362) | 0.278 (0.148) | 0.044 (0.018) | 0.047 (0.024) | | |
| median per-pupil spending experienced by student | -0.275 (0.419) | -0.264 (0.404) | -0.283 (0.410) | -0.025 (0.027) | -0.035 (0.025) | -0.038 (0.026) | | |
| diversity of per-pupil spending in student's state (see column heading for measure) | 1.223 (2.340) | 0.277 (0.312) | 1.124 (0.984) | 0.141 (0.106) | 0.004 (0.021) | 0.049 (0.065) | | |
| slope of relationship between per-pupil spending and family income | 12.026 (84.107) | 65.249 (81.672) | 27.117 (78.436) | 9.652 (5.461) | 9.666 (5.476) | 7.984 (5.226) | | |
| gender, race, and Hispanic ethnicity fixed effects | yes | yes | yes | yes | yes | yes | | |
| state fixed effects | yes | yes | yes | yes | yes | yes | | |
| year fixed effects | yes | yes | yes | yes | yes | yes | | |
| dependent variable: | 75-25 ratio of | | | 75-25 ratio of | | | | |
| measure of diversity: | highest grade completed by age 30 | coefficient | 90-10 ratio | 75-25 ratio | highest grade completed by age 20 | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | | | |
| median income of parents in student's state | -0.008 (0.003) | -0.007 (0.002) | -0.006 (0.003) | 0.001 (0.001) | -0.001 (0.001) | -0.001 (0.001) | | |
| diversity of parents' income in student's state (see column heading for measure) | 0.194 (0.091) | 0.040 (0.011) | 0.067 (0.034) | 0.050 (0.021) | 0.004 (0.002) | 0.004 (0.005) | | |
| median per-pupil spending experienced by student | 0.046 (0.017) | 0.036 (0.016) | 0.036 (0.016) | -0.002 (0.004) | -0.001 (0.004) | -0.001 (0.004) | | |
| diversity of per-pupil spending in student's state (see column heading for measure) | 0.139 (0.065) | 0.011 (0.013) | 0.019 (0.040) | -0.018 (0.025) | -0.004 (0.005) | 0.008 (0.015) | | |
| slope of relationship between per-pupil spending and family income | -0.292 (3.344) | -1.334 (3.390) | -1.402 (3.224) | 1.669 (1.213) | 2.133 (1.210) | 2.417 (1.180) | | |
| gender, race, and Hispanic ethnicity fixed effects | yes | yes | yes | yes | yes | yes | | |
| state fixed effects | yes | yes | yes | yes | yes | yes | | |
| year fixed effects | yes | yes | yes | yes | yes | yes | | |

Notes to Table 4: All table notes from Table 3 apply.

Table 5
The Diversity of Students' Later Outcomes (as measured by the coefficient of variation)
Regressed on Educational Environment and *State-Specific Linear Time Trends*

| dependent variable: | coefficient of variation in earned income at age 30 | | | coefficient of variation in highest grade completed at age 20 | | |
|--|--|--------------------------------|--------------------------------|--|--------------------------------|--------------------------------|
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | 0.001 (0.006) | 0.001 (0.006) | 0.003 (0.006) | -0.001 (0.001) | -0.001 (0.001) | -0.001 (0.001) |
| diversity of parents' income in student's state (see column heading for measure) | 0.105 (0.054) | 0.024 (0.013) | 0.028 (0.013) | 0.029 (0.014) | 0.005 (0.002) | 0.005 (0.002) |
| median per-pupil spending experienced by student | -0.009 (0.071) | 0.013 (0.066) | 0.005 (0.066) | 0.003 (0.004) | -0.001 (0.004) | -0.001 (0.004) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.167 (0.200) | -0.006 (0.038) | 0.024 (0.107) | 0.055 (0.023) | 0.005 (0.005) | 0.024 (0.014) |
| slope of relationship between per-pupil spending and family income | 7.337 (12.144) | 9.078 (12.758) | 7.677 (12.131) | 1.1078 (1.1835) | 0.9458 (1.2143) | 0.858 (1.171) |
| state-specific linear time trends | yes | yes | yes | yes | yes | yes |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Table 5: The regressions shown in this table are the same as those shown in Table 2, except that a linear time trend is estimated separately for each state. All notes from Table 2 apply.

Table 6
The Diversity of Students' Later Outcomes (as measured by the coefficient of variation)
Regressed on Educational Environment *with Controls for Policy Endogeneity*

| dependent variable: | coefficient of variation in earned income at age 30 | | | coefficient of variation in highest grade completed at age 20 | | |
|--|--|--------------------------------|--------------------------------|--|---------------------------------|---------------------------------|
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | -0.005 (0.006) | -0.001 (0.006) | -0.001 (0.006) | -0.001 (0.001) | -0.001 (0.001) | -0.001 (0.001) |
| diversity of parents' income in student's state (see column heading for measure) | 0.260 (0.109) | 0.032 (0.015) | 0.028 (0.014) | 0.026 (0.012) | 0.007 (0.003) | 0.006 (0.005) |
| median per-pupil spending experienced by student | -0.003 (0.047) | -0.032 (0.047) | -0.034 (0.046) | 0.006 (0.003) | 0.005 (0.003) | 0.005 (0.003) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.019 (0.193) | 0.002 (0.027) | 0.074 (0.086) | -0.026 (0.021) | -0.003 (0.003) | -0.006 (0.009) |
| slope of relationship between per-pupil spending and family income | 13.508 (7.867) | 13.243 (8.169) | 14.048 (7.885) | 0.794 (0.789) | 1.146 (0.791) | 1.260 (0.772) |
| annual change in family income diversity when student was age 6-17 | -0.003 (0.004) | -0.002 (0.004) | -0.001 (0.004) | -0.002 (0.001) | -0.002 (0.001) | -0.002 (0.001) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Table 6: The regressions shown in this table are the same as those shown in Table 2, except that the annual change in family income diversity (measured when student was age 6-17) is added, as a control for the circumstances that prevailed when school finance policy was set. All notes from Table 2 apply.

Table 7
The Level of Students' Later Outcomes (as measured by their *median* outcome)
Regressed on Educational Environment

| dependent variable: | median | | | median | | |
|--|-------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|--------------------|
| | earned income at age 30 (thousands) | | | wage at age 30 | | |
| measure of diversity: | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.279 (0.052) | 0.090 (0.046) | 0.085 (0.048) | 0.133 (0.042) | 0.076 (0.038) | 0.079 (0.041) |
| diversity of parents' income in student's state (see column heading for measure) | -2.722 (3.896) | 0.486 (0.488) | 4.345 (2.780) | -3.253 (2.171) | -0.069 (0.266) | 0.150 (1.530) |
| median per-pupil spending experienced by student | 1.003 (0.710) | 0.900 (0.689) | 0.795 (0.686) | -0.039 (0.396) | -0.197 (0.375) | -0.187 (0.378) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 6.945 (2.804) | -0.267 (0.562) | -1.384 (1.742) | 0.596 (1.562) | 0.326 (0.306) | 0.981 (0.959) |
| slope of relationship between per-pupil spending and family income | -162.934 (143.864) | -185.502 (148.038) | -159.060 (139.146) | 2.660 (80.157) | 56.142 (80.689) | 38.010 (76.593) |
| gender, race, and Hispanic ethnicity | yes | yes | yes | yes | yes | yes |
| fixed effects | | | | | | |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

| dependent variable: | median | | | median | | |
|--|-----------------------------------|-------------------|-------------------|-----------------------------------|-------------------|-------------------|
| | highest grade completed by age 30 | | | highest grade completed by age 20 | | |
| measure of diversity: | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.005 (0.013) | 0.004 (0.012) | 0.004 (0.013) | 0.003 (0.004) | 0.003 (0.004) | 0.003 (0.004) |
| diversity of parents' income in student's state (see column heading for measure) | 0.140 (0.453) | 0.025 (0.056) | 0.006 (0.320) | -0.202 (0.113) | -0.012 (0.013) | -0.089 (0.082) |
| median per-pupil spending experienced by student | -0.086 (0.082) | -0.049 (0.078) | -0.046 (0.079) | 0.001 (0.024) | 0.020 (0.024) | 0.016 (0.024) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.374 (0.326) | 0.001 (0.064) | -0.020 (0.200) | -0.050 (0.137) | 0.040 (0.026) | 0.016 (0.085) |
| slope of relationship between per-pupil spending and family income | 10.901 (16.711) | 8.144 (16.856) | 9.613 (16.013) | 6.387 (6.610) | 6.247 (6.663) | 8.202 (6.535) |
| gender, race, and Hispanic ethnicity | yes | yes | yes | yes | yes | yes |
| fixed effects | | | | | | |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Table 7: The observations in the regressions shown above at the individual level. Individuals are classified by state of birth and the birth cohort (the cohorts in the data are the 1950, 1960, 1970, and 1980 birth cohorts). Most of the explanatory variables are not at the individual level, but the cohort-by-state level. Therefore, the standard errors shown in parentheses are calculated using Moulton's (1986) procedure for dealing with clustered observations.

Regressions with age 20 outcomes contain 200 cohort-by-state clusters. Regressions with age 30 outcomes contain 150 cohort-by-state clusters. Notes on data sources from Table 2 notes apply.

Table 8
The Level of Students' Later Outcomes (as measured by their *mean* outcome)
Regressed on Educational Environment

| dependent variable: | mean | | | mean | | |
|--|-----------------------------------|--------------------------------|--------------------------------|-----------------------------------|--------------------------------|--------------------------------|
| | earned income at age 30 | | | wage at age 30 | | |
| measure of diversity: | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.415 (0.024) | 0.352 (0.033) | 0.368 (0.032) | 0.211 (0.015) | 0.177 (0.019) | 0.183 (0.020) |
| diversity of parents' income in student's state (see column heading for measure) | -1.608 (0.901) | 0.040 (0.118) | 1.340 (0.721) | -0.637 (0.528) | -0.047 (0.064) | 0.210 (0.428) |
| median per-pupil spending experienced by student | 0.262 (0.208) | 0.439 (0.252) | 0.381 (0.238) | 0.085 (0.139) | -0.215 (0.144) | -0.188 (0.145) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.663 (0.574) | -0.180 (0.117) | -0.187 (0.404) | -0.211 (0.368) | -0.057 (0.049) | -0.076 (0.165) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity | 26.318 (23.445) | 5.874 (38.325) | 6.425 (32.058) | -0.071 (12.456) | -2.070 (17.528) | 8.709 (15.342) |
| fixed effects | yes | yes | yes | yes | yes | yes |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: | mean | | | mean | | |
| | highest grade completed by age 30 | | | highest grade completed by age 20 | | |
| measure of diversity: | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.008 (0.004) | 0.008 (0.004) | 0.006 (0.003) | 0.004 (0.002) | 0.003 (0.004) | 0.003 (0.004) |
| diversity of parents' income in student's state (see column heading for measure) | 0.390 (0.151) | 0.020 (0.020) | -0.057 (0.137) | 0.148 (0.094) | -0.009 (0.013) | -0.130 (0.093) |
| median per-pupil spending experienced by student | 0.074 (0.039) | 0.040 (0.047) | 0.048 (0.045) | -0.046 (0.025) | -0.050 (0.028) | -0.044 (0.028) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 0.127 (0.124) | 0.028 (0.020) | 0.037 (0.060) | 0.023 (0.074) | 0.032 (0.030) | 0.028 (0.030) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity | 4.444 (4.288) | -0.216 (6.820) | -3.919 (5.234) | 2.274 (2.342) | 0.638 (2.730) | 0.672 (2.584) |
| fixed effects | yes | yes | yes | yes | yes | yes |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Table 8: All notes from Table 7 apply.

Table 9
 The Level of Students' Later Outcomes (as measured by their median and mean outcomes)
 Regressed on Educational Environment and *State-Specific Linear Time Trends*

| dependent variable: | median | | | median | | |
|--|-------------------------|-------------------------|-------------------------|-----------------------------------|---------------------------|---------------------------|
| | earned income at age 30 | | | highest grade completed by age 20 | | |
| measure of diversity: | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.260 (0.063) | 0.103 (0.052) | 0.108 (0.053) | 0.002 (0.004) | 0.003 (0.004) | 0.003 (0.005) |
| diversity of parents' income in student's state (see column heading for measure) | -8.060 (4.296) | 6.662 (0.684) | 8.017 (3.864) | -0.138 (0.141) | -0.012 (0.017) | -0.086 (0.112) |
| median per-pupil spending experienced by student | 0.808 (1.246) | -0.285 (1.175) | 0.147 (1.214) | 0.033 (0.042) | 0.025 (0.038) | 0.024 (0.038) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 6.953 (3.511) | 1.298 (0.671) | 1.122 (1.952) | -0.174 (0.223) | -0.032 (0.047) | 0.106 (0.141) |
| slope of relationship between per-pupil spending and family income | 144.211 (212.673) | 232.261 (227.286) | 134.724 (221.903) | 31.506 (11.637) | 29.002 (12.038) | 30.703 (11.888) |
| gender, race, and Hispanic ethnicity fixed effects | yes | yes | yes | yes | yes | yes |
| state-specific linear time trends | yes | yes | yes | yes | yes | yes |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: | mean | | | mean | | |
| | earned income at age 30 | | | highest grade completed by age 20 | | |
| measure of diversity: | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | variation | | | of variation | | |
| median income of parents in student's state | 0.391 (0.026) | 0.299 (0.033) | 0.310 (0.037) | 0.004 (0.002) | 0.003 (0.002) | 0.006 (0.002) |
| diversity of parents' income in student's state (see column heading for measure) | 0.783 (1.161) | -0.269 (0.169) | -0.379 (1.042) | 0.190 (0.112) | -0.008 (0.016) | -0.124 (0.094) |
| median per-pupil spending experienced by student | -0.100 (0.336) | 0.176 (0.404) | -0.008 (0.379) | -0.099 (0.029) | -0.100 (0.028) | -0.109 (0.027) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 0.673 (1.106) | -0.134 (0.152) | -0.160 (0.457) | -0.047 (0.102) | -0.022 (0.015) | -0.190 (0.048) |
| slope of relationship between per-pupil spending and family income | -100.815 (56.329) | -104.064 (75.276) | -70.578 (64.389) | 3.734 (5.072) | 1.088 (6.859) | -1.520 (6.018) |
| gender, race, and Hispanic ethnicity fixed effects | yes | yes | yes | yes | yes | yes |
| state-specific linear time trends | yes | yes | yes | yes | yes | yes |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Table 9: The regressions shown in this table are the same as those shown in Tables 7 and 8, except that a linear time trend is estimated separately for each state. Notes from Tables 7 and 8 apply.

Table 10
The Level of Students' Later Outcomes (as measured by their median and mean outcomes)
Regressed on Educational Environment *with Controls for Policy Endogeneity*

| dependent variable: measure of diversity: | median | | | median | | |
|---|---------------------------|-----------------------------------|----------------------------|--------------------------|-----------------------------------|--------------------------|
| | earned income at age 30 | highest grade completed by age 20 | | earned income at age 30 | highest grade completed by age 20 | |
| | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.391 (0.026) | 0.299 (0.033) | 0.310 (0.037) | 0.003 (0.002) | 0.003 (0.002) | 0.006 (0.002) |
| diversity of parents' income in student's state (see column heading for measure) | -0.783 (1.161) | -0.269 (0.169) | -0.379 (1.042) | -0.190 (0.112) | -0.008 (0.016) | -0.124 (0.094) |
| median per-pupil spending experienced by student | -0.100 (0.336) | 0.176 (0.404) | -0.008 (0.379) | -0.099 (0.029) | -0.100 (0.028) | -0.109 (0.027) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 6.073 (1.106) | -0.234 (0.152) | -0.660 (0.457) | 0.427 (0.102) | -0.029 (0.025) | -0.049 (0.048) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | -100.815 (56.329) | -104.064 (75.276) | -70.578 (64.389) | 13.734 (5.072) | -11.088 (6.859) | -10.520 (6.018) |
| annual change in family income diversity when student was age 6-17 | -0.03 (0.00) | -0.02 (0.04) | -0.01 (0.04) | -0.02 (0.01) | -0.02 (0.01) | -0.02 (0.01) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: measure of diversity: | mean | | | mean | | |
| | earned income at age 30 | highest grade completed by age 20 | | earned income at age 30 | highest grade completed by age 20 | |
| | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.423 (0.025) | 0.355 (0.035) | 0.373 (0.034) | -0.004 (0.003) | -0.001 (0.004) | -0.002 (0.004) |
| diversity of parents' income in student's state (see column heading for measure) | -7.142 (0.888) | 0.075 (0.123) | 1.635 (0.750) | 0.552 (0.099) | -0.010 (0.014) | -0.147 (0.097) |
| median per-pupil spending experienced by student | 0.303 (0.220) | 0.472 (0.279) | 0.414 (0.264) | -0.058 (0.028) | -0.060 (0.031) | -0.054 (0.031) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.358 (0.570) | -0.019 (0.118) | -0.018 (0.401) | 0.164 (0.083) | 0.032 (0.010) | 0.062 (0.030) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | 59.269 (23.827) | 74.692 (38.689) | 105.780 (32.735) | -4.147 (2.364) | -5.259 (2.761) | -8.906 (2.617) |
| annual change in family income diversity when student was age 6-17 | -0.03 (0.04) | -0.02 (0.04) | -0.01 (0.04) | -0.02 (0.01) | -0.02 (0.01) | -0.02 (0.01) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Table 10: The regressions shown in this table are the same as those shown in Tables 7 and 8, except that

the annual change in family income diversity (measured when student was age 6-17) is added, as a control for the circumstances that prevailed when school finance policy was set. Notes from Tables 7 and 8 apply.

Appendix Table 1
The Diversity of *Male* Students' Later Outcomes (as measured by the coefficient of variation)
Regressed on Educational Environment

| dependent variable: | coefficient of variation in earned income at age 30 | | | coefficient of variation in wage at age 30 | | |
|--|--|---------------------------------|---------------------------------|--|----------------------------------|---------------------------------|
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | -0.001 (0.006) | 0.004 (0.006) | 0.003 (0.006) | -0.006 (0.008) | -0.007 (0.008) | -0.007 (0.008) |
| diversity of parents' income in student's state (see column heading for measure) | 0.127 (0.061) | 0.029 (0.014) | 0.035 (0.014) | 0.168 (0.138) | 0.042 (0.029) | 0.049 (0.033) |
| median per-pupil spending experienced by student | -0.035 (0.037) | -0.056 (0.036) | -0.054 (0.036) | 0.041 (0.051) | 0.024 (0.049) | 0.025 (0.049) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 0.003 (0.146) | -0.009 (0.029) | 0.037 (0.092) | 0.127 (0.203) | 0.009 (0.040) | 0.094 (0.124) |
| slope of relationship between per-pupil spending and family income | 19.914 (7.509) | 19.784 (7.705) | 21.407 (7.321) | 16.014 (10.416) | 21.868 (10.496) | 20.886 (9.941) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: | coefficient of variation in highest grade completed by age 30 | | | coefficient of variation in highest grade completed by age 20 | | |
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | 0.001 (0.001) | 0.002 (0.001) | 0.002 (0.001) | 0.001 (0.001) | 0.001 (0.001) | -0.001 (0.001) |
| diversity of parents' income in student's state (see column heading for measure) | 0.097 (0.024) | 0.009 (0.004) | 0.013 (0.003) | 0.024 (0.012) | 0.005 (0.002) | 0.005 (0.004) |
| median per-pupil spending experienced by student | -0.002 (0.004) | -0.007 (0.005) | -0.007 (0.005) | -0.008 (0.004) | -0.007 (0.004) | -0.007 (0.004) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.036 (0.017) | 0.002 (0.004) | 0.012 (0.013) | -0.030 (0.024) | 0.001 (0.005) | 0.018 (0.014) |
| slope of relationship between per-pupil spending and family income | -0.008 (0.874) | 1.100 (1.128) | 1.169 (1.061) | 3.746 (1.146) | 4.266 (1.138) | 4.344 (1.102) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Appendix Table 1: The estimates shown are from regressions that are the same as those in Table 2, except that the dependent variables are computed for males only. All notes to Table 2 apply.

Appendix Table 2
The Diversity of *Male* Students' Later Outcomes (as measured by the 90-10 ratio)
Regressed on Educational Environment

| dependent variable: | 90-10 ratio of earned income (coefficients divided by 1000 for convenience) | | | 90-10 ratio of wage | | |
|---|---|--------------------------------|--------------------------------|---|---------------------------------|---------------------------------|
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | 0.001 (0.001) | 0.001 (0.001) | 0.001 (0.001) | -0.148 (0.045) | -0.098 (0.044) | -0.115 (0.045) |
| diversity of parents' income in student's state (see column heading for measure) | 1.111 (0.542) | 0.253 (0.120) | 1.296 (0.598) | 3.293 (1.721) | 0.579 (0.212) | 2.047 (1.248) |
| median per-pupil spending experienced by student | 0.001 (0.002) | 0.001 (0.002) | 0.001 (0.002) | 0.732 (0.285) | 0.448 (0.285) | 0.512 (0.283) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.006 (0.009) | -0.001 (0.002) | -0.009 (0.005) | 1.553 (1.123) | 0.284 (0.233) | 0.903 (0.719) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | 0.174 (0.451) | 0.458 (0.435) | 0.299 (0.417) | 8.535 (57.636) | 33.986 (61.337) | 17.348 (57.450) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: | 90-10 ratio of highest grade completed by age 30 | | | 90-10 ratio of highest grade completed by age 20 | | |
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | 0.005 (0.004) | 0.003 (0.004) | 0.003 (0.004) | 0.001 (0.003) | -0.001 (0.003) | -0.002 (0.003) |
| diversity of parents' income in student's state (see column heading for measure) | 0.321 (0.150) | 0.037 (0.018) | 0.207 (0.107) | 0.160 (0.078) | 0.011 (0.006) | 0.053 (0.055) |
| median per-pupil spending experienced by student | -0.007 (0.027) | 0.004 (0.026) | 0.001 (0.026) | -0.041 (0.017) | -0.030 (0.016) | -0.033 (0.016) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.204 (0.108) | -0.032 (0.021) | -0.071 (0.067) | -0.172 (0.095) | -0.017 (0.018) | 0.002 (0.057) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | 0.201 (5.521) | 4.109 (5.521) | 4.044 (5.353) | 2.445 (4.589) | 3.499 (4.502) | 4.252 (4.406) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Appendix Table 2: The estimates shown are from regressions that are the same as those in Table 3, except that the dependent variables are computed for males only. All notes to Table 3 apply.

Appendix Table 3
The Diversity of *Male* Students' Later Outcomes (as measured by the 75-25 ratio)
Regressed on Educational Environment

| dependent variable: | 75-25 ratio of earned income (coefficients divided by 1000 for convenience) | | | 75-25 ratio of wage | | |
|---|---|--------------------------------|--------------------------------|---|--------------------------------|--------------------------------|
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | 0.002 (0.017) | -0.003 (0.016) | -0.001 (0.017) | 0.002 (0.008) | 0.004 (0.007) | 0.006 (0.008) |
| diversity of parents' income in student's state (see column heading for measure) | 4.184 (2.308) | 0.551 (0.272) | 0.899 (0.379) | 0.295 (0.156) | 0.044 (0.020) | 0.058 (0.029) |
| median per-pupil spending experienced by student | -0.161 (0.111) | -0.108 (0.102) | -0.123 (0.106) | -0.049 (0.050) | -0.074 (0.048) | -0.076 (0.047) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 0.023 (0.437) | -0.076 (0.083) | -0.028 (0.269) | 0.188 (0.199) | 0.025 (0.039) | 0.225 (0.120) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | -9.542 (22.441) yes | -13.702 (21.909) yes | -18.846 (21.468) yes | 2.298 (10.202) yes | 8.210 (10.264) yes | 5.498 (9.611) yes |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: | 75-25 ratio of highest grade completed by age 30 | | | 75-25 ratio of highest grade completed by age 20 | | |
| measure of diversity: | coefficient of variation | 90-10 ratio | 75-25 ratio | coefficient of variation | 90-10 ratio | 75-25 ratio |
| median income of parents in student's state | -0.006 (0.003) | -0.004 (0.003) | -0.004 (0.003) | -0.001 (0.001) | -0.001 (0.001) | -0.001 (0.001) |
| diversity of parents' income in student's state (see column heading for measure) | 0.205 (0.099) | 0.046 (0.012) | 0.069 (0.070) | 0.059 (0.029) | 0.005 (0.003) | 0.012 (0.007) |
| median per-pupil spending experienced by student | 0.037 (0.018) | 0.022 (0.017) | 0.023 (0.017) | -0.003 (0.006) | -0.002 (0.006) | -0.002 (0.006) |
| diversity of per-pupil spending in student's state (see column heading for measure) | 0.079 (0.071) | 0.012 (0.014) | 0.068 (0.044) | -0.013 (0.035) | -0.006 (0.007) | 0.014 (0.021) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | 1.394 (3.637) yes | 1.606 (3.746) yes | 1.492 (3.524) yes | 0.354 (1.672) yes | 1.081 (1.673) yes | 1.472 (1.630) yes |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Appendix Table 3: The estimates shown are from regressions that are the same as those in Table 4, except that the dependent variables are computed for males only. All notes to Table 4 apply.

Appendix Table 4
The Level of *Male* Students' Later Outcomes (as measured by their *median* outcome)
Regressed on Educational Environment

| dependent variable: | median | | | median | | |
|---|-------------------------------------|--------------------------------|---------------------------------|-----------------------------------|--------------------------------|--------------------------------|
| measure of diversity: | earned income at age 30 (thousands) | | | wage at age 30 | | |
| | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.352 (0.059) | 0.108 (0.048) | 0.107 (0.051) | 0.142 (0.045) | 0.083 (0.038) | 0.086 (0.038) |
| diversity of parents' income in student's state (see column heading for measure) | -6.975 (6.604) | 0.167 (0.817) | 4.842 (4.666) | -3.094 (2.960) | 0.208 (0.368) | 0.422 (2.119) |
| median per-pupil spending experienced by student | 0.721 (1.204) | 0.056 (1.152) | 0.030 (1.151) | 0.249 (0.540) | -0.133 (0.519) | -0.054 (0.523) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.562 (4.751) | 1.419 (0.940) | 4.550 (2.923) | -0.107 (2.130) | 0.824 (0.423) | 2.825 (1.327) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | -190.511 (243.818) | -39.693 (247.663) | -100.060 (233.550) | 45.183 (109.291) | 148.133 (111.631) | 128.204 (106.035) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |
| dependent variable: | median | | | median | | |
| measure of diversity: | highest grade completed by age 30 | | | highest grade completed by age 20 | | |
| | coefficient | 90-10 ratio | 75-25 ratio | coefficient | 90-10 ratio | 75-25 ratio |
| | of variation | | | of variation | | |
| median income of parents in student's state | 0.008 (0.018) | 0.004 (0.017) | 0.005 (0.017) | 0.011 (0.004) | 0.009 (0.004) | 0.008 (0.004) |
| diversity of parents' income in student's state (see column heading for measure) | 0.222 (0.623) | -0.009 (0.077) | -0.095 (0.434) | 0.011 (0.100) | -0.010 (0.011) | -0.065 (0.071) |
| median per-pupil spending experienced by student | -0.031 (0.113) | 0.033 (0.108) | 0.026 (0.107) | 0.033 (0.021) | 0.017 (0.021) | 0.016 (0.021) |
| diversity of per-pupil spending in student's state (see column heading for measure) | -0.499 (0.448) | -0.047 (0.088) | -0.506 (0.272) | 0.071 (0.122) | -0.010 (0.023) | -0.020 (0.074) |
| slope of relationship between per-pupil spending and family income gender, race, and Hispanic ethnicity fixed effects | -7.881 (22.986) | -11.793 (23.308) | -11.761 (21.742) | -5.910 (5.888) | -7.695 (5.821) | -7.523 (5.672) |
| state fixed effects | yes | yes | yes | yes | yes | yes |
| year fixed effects | yes | yes | yes | yes | yes | yes |

Notes to Appendix Table 4: The estimates shown are from regressions that are the same as those in Table 7, except that the dependent variables are computed for males only. All notes to Table 7 apply.