Income Inequality and Intergenerational Income Mobility in the United States

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Abstract

Is there a relationship between family income inequality and income mobility across generations in the United States? As family income inequality rose in the U.S., parental resources available for improving children's health, education, and care diverged. The amount and rate of divergence also varied across U.S. states. Researchers and policy analysts have expressed concern that relatively high inequality might be accompanied by relatively low mobility, tightening the connection between individuals' incomes during childhood and adulthood. Using data from the Panel Study of Income Dynamics, the National Longitudinal Survey of Youth, and various government sources, this paper exploits state and cohort variation to estimate the relationship between inequality and mobility. Results provide very little support for the hypothesis that inequality shapes mobility in the U.S. The inequality to which children were exposed during youth has no robust association with the mobility they experienced as adults. Formal analysis reveals that offsetting effects could underlie this result. In theory, mobility-enhancing forces may counterbalance mobility-reducing effects. In practice, the results suggest that in the U.S. context, the intergenerational transmission of income may not be very responsive to changes in inequality of the size observed since 1970.

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How tightly linked are economic inequality between families and economic mobility across generations in the United States? Over the last four decades in the U.S., disparities grew in hourly wages, annual earnings, and, most substantially, family incomes (McCall and Percheski 2010; Gottschalk and Danziger 2005). Rising inequality across repeated cross-sections has stimulated concern among both policymakers and academics about how inequality persists across generations. One widely discussed hypothesis maintains that high economic inequality inhibits intergenerational mobility (e.g., Ermish et al. 2012; Smeeding, Erikson, and Jäntti 2011; Wilkinson and Pickett 2010; Beller and Hout 2006; Solon 2004). Alan Krueger (2012), current Chairman of the Council of Economic Advisers, recently stated that "it is hard to . . . not be concerned that rising inequality is jeopardizing our tradition of equality of opportunity. The fortunes of one's parents seem to matter increasingly in American society."

While inequality trends stimulated policy interest, academic interest derives from variation not only over time but also across space. Sociologists have a long history of studying how the transmission of socioeconomic status differs across countries, on the hypothesis that different economic, social, and political contexts may generate different opportunities for socioeconomic mobility (e.g., Lipset and Zetterberg 1956; Grusky and Hauser 1984; Erikson and Goldthorpe 1992; Breen 2004). Theories explaining how inequality might shape mobility apply equally well to differences in inequality across countries, across states within the U.S., or across years. However, the relationship between inequality and mobility remains largely unexplored in the U.S. context (Hout 2004). This paper examines both temporal and geographic variation to assess the relationship between family income inequality and income mobility across generations in the contemporary U.S. It addresses whether children raised in relatively high-inequality areas or during relatively high-inequality eras experienced more, less, or about the same level of mobility as children raised in less unequal areas or eras.

A state-centered analysis provides a U.S.-specific analogy to cross-national research. States differ along many economic and demographic dimensions. Perhaps more impor-

tantly, states also represent stable political jurisdictions that control important social and economic policies. States receive different amounts of federal resources to disburse. They also determine many policies that could influence children's economic prospects. These policies include tax rates and redistributive spending on public assistance, education, health and hospitals. Over the period of rising inequality, states played a major role in determining the level of healthcare spending on children and poor parents, and until 1996 they set the levels of spending for cash assistance on the poor. State governments also largely controlled both educational policy and educational spending during the years under study. Educational spending does vary within states. However, between-state variation in educational spending is much larger than within-state variation and, as inequality rose, states differed in the extent to which they equalized disparities in district funding (Murray, Evans, and Schwab 1998). These decisions were made in state capitals.[1]

By focusing on state differences as well as time trends, this paper more fully exploits the variation available for learning about how inequality shapes mobility than previous research, which has been either purely historical (e.g., Lee and Solon 2009; Mayer and Lopoo 2005) or purely cross-national (e.g., Corak 2012; Treiman and Yip 1989). The family income Gini coefficient rose from .361 to .434 between the 1970 and 2000 censuses (a .073 point difference). Cross-sectional differences across states in 1970 were even larger than the change from 1970 to 2000 (with state Ginis ranging from .317 to .427 in 1970, a .110 point difference). Furthermore, inequality rose at different rates in different states (U.S. Census Bureau 2012). Exploring variation both over time and across states provides a more powerful design and allows greater insight into how closely inequality and mobility are linked in the U.S.

The intuition that high economic inequality undermines intergenerational mobility is widely shared; nevertheless, many contrary arguments have also been articulated. Thoughtful scholars have worried that inequality "will reinforce privilege among the affluent and disadvantage among the poor, reinforcing economic inequality in the next generation" (Neckerman and Torche 2007: 340). A theory of cumulative advantage appears

to underlie these concerns. According to this theory, children's advantages compound at each developmental stage. Differences between high- and low-income parents' investments in their children's health, education, and care create differences in children's early-life achievements. The effects of these resource disparities build over time, as children move through school and into the labor market, thus generating income differences adulthood (for review of cumulative advantage theory, see DiPrete and Eirich 2006). Differences among families' investments in children may be larger in high inequality eras and areas than in more equal times and places. Inequality could thus produce what McLanahan (2004) calls "diverging destinies" for children from different class backgrounds.

However, complex interactions between the family, the state, and the market in shaping children's opportunities make the net relationship between inequality and mobility theoretically uncertain. High inequality may not correspond to low economic mobility if high inequality generates populist political responses. Redistributive public investments in children's development could offset the dispersion of private investments (Solon 2004). Even considering parental investments alone, if the benefits children reap increase non-linearly with the amount of investment, then disparities in families' resources may not associate closely with mobility (Downey 1995). Returns to very high levels of parental investments may be quite low. Mobility and inequality could even increase together, if the era of rising inequality coincides with major projects in collective mobility, such as the reforms institutionalized following the U.S. Civil Rights movement.

Because many share the intuition that high inequality inhibits mobility, studies finding negative associations between inequality and one of the forces promoting mobility may be more widely remembered than studies finding offsetting positive associations. Nevertheless, given the complex interactions among forces affecting both the variability of adult incomes and how much of that variability can be explained by parental income, inequality is likely to have contradictory consequences. The many countervailing forces shaping children's opportunities generate theoretical ambiguity. This ambiguity augments the need for empirical investigation.

This paper both (1) provides a formal theoretical framework for understanding why the net association between economic inequality and intergenerational mobility might not corroborate the common hypothesis that inequality reduces mobility, and (2) estimates the association empirically in the contemporary U.S. The empirical analysis improves on previous research in three ways. First, while previous mobility studies have been either historical or comparative, this paper uses both over-time and across-space comparisons. Using data from the Panel Study of Income Dynamics (PSID) and various government sources, this paper examines how the relationship between parents' and children's family incomes varies with inequality, exploiting differences across both U.S. states and birth cohorts. This design is not only statistically more powerful than previous studies (yielding greater variation in inequality and mobility). It is also the first to examine geographic variation in mobility within the U.S., thus providing new insights into within-country dynamics.

Second, this paper employs multiple longitudinal data sources, providing substantially more information about how inequality and mobility covary than possible from any single source.[2] The PSID has been widely used in mobility studies, and it provides the longest time-series for analyzing trends. However, the sample size is relatively small. This paper supplements the PSID with the National Longitudinal Survey of Youth 1979 cohort (NLSY79), which provides a significantly larger sample and thus greater power for detecting differences.[3] Third, this paper uses modeling techniques not previously applied to comparative mobility studies (including random coefficient models) that substantially reduce the amount of estimation uncertainty. This paper thus improves upon previous estimates and excludes a wider range of values as plausible descriptors of the inequality-mobility relationship. It provides new insight into how inequality may shape mobility in the U.S. context.

How Might Inequality Shape Mobility?

Like many previous empirical studies of intergenerational mobility, this paper aims to provide basic descriptive evidence rather than test specific theories about why inequality and mobility might be linked. Nevertheless, a formal theoretical exploration provides context for this descriptive undertaking.

To understand how income inequality between families might influence intergenerational income mobility, we must understand how economic status persists. Inequality must affect mobility by shaping the process of persistence; greater persistence implies lower mobility. At a very basic level, any skill that is both correlated across generations and is rewarded in the labor market (or marriage market, when considering family economic status) can contribute to the intergenerational persistence of economic status.[4] Variations in economic mobility across time, place, or subpopulation can be explained by (1) different distributions of these skills, (2) different levels of skill transmission across generations, or (3) different rates of return to these skills. Inequality may shape mobility by affecting any of these three factors (defined formally below). Many of the skills correlated across generations are malleable. Consequently, both their distributions and intergenerational transmission rates may shift with changing social circumstances. Even for relatively fixed traits, returns may vary across settings.

The hypothesis that inequality reduces mobility, though often articulated, may not hold empirically because inequality may have multiple, offsetting effects. Offsetting effects can manifest in two ways. First, they can appear in different relationships between inequality and a given skill's distribution, transmission, and returns. Second, they can result from differences across skills in the way inequality shapes intergenerational similarity. Because inequality's effects may not be uniformly negative, basing our intuition about the inequality-mobility relationship on studies of a single source of intergenerational similarity could be misleading.

Duncan's (1966) basic theorem of path analysis provides a formal framework for un-

derstanding importance of offsetting effects (both along a single pathway of intergenerational similarity as well as across multiple pathways). It also elucidates the distinction between transmission and returns. The theorem states that the correlation between two variables k and j (here, individuals' incomes in their adult families of destination and their childhood families of origin), ρ_{kj}, can be decomposed into contributions from the $\{q\}$ different sources of similarity,[5]

$$\rho_{kj} = \sum_q \alpha_{kq}\rho_{qj}.$$

The contribution of each source, in turn, can be decomposed into the product of its "transmission" (ρ_{qj}, the correlation between parental income and children's skill q — for example their educational attainment) and its "returns" (α_{kq}, the standardized coefficient from a multiple regression predicting children's adult income — for example the coefficient used to predict children's income from children's educational attainment). We could consider deeper structural parameters (e.g., parameters indicating how parental skill q correlates with parental income which in turn correlates with children's skill q). However, it is sufficient to recognize that the persistence of economic status across generations varies with the strength of the relationships of skills $\{q\}$ to parental income (transmission) and to children's adult income (returns).[6]

Skill distributions may also affect mobility, in addition to skill transmission and returns. There are two ways to formally understand the role of skill distributions in mobility. First, when considering correlations and standardized regression coefficients (as in the above statement of Duncan's theorem), distributional shifts collapse to changes in transmission. If children's standardized skill distribution becomes less equally dispersed across parental income, then the correlation between skill and parental income rises and, by definition, skill transmission increases. Second, when considering unstandardized regression coefficients, distributional shifts can have unique effects on mobility (separate from transmission). Restating Duncan's theorem in terms of unstandardized coefficients,

we see that $\beta_{kj} = \sum_q \gamma_{kq}\beta_{qj} = \sum_q \alpha_{kq}(\sigma_k/\sigma_q)\rho_{qj}(\sigma_q/\sigma_j)$, where α_{kq} and ρ_{qj} were defined above and σ_k is the standard deviation of variable k (see Wright 1960; Land 1969). Ultimately, this decomposition reduces to the familiar identity that the regression coefficient equals the product of two variables' correlation and the ratio of their standard deviations: $\beta_{kj} = \rho_{kj}(\sigma_k/\sigma_j)$. However, the distribution of skill q (σ_q) has separate effects on unstandardized skill transmission (β_{qj}) and unstandardized skill returns (γ_{kq}). Increasing skill variability raises unstandardized skill transmission and decreases unstandardized skill returns, all else equal. Sociologists have gained analytic purchase by separating such distributional effects from raw associational effects, particularly when studying the role of education in social mobility (e.g., Breen 2011; Hout 1988; Mare 1980).

Income inequality may associate with intergenerational income mobility if it relates to (1) the distribution of children's income-generating skills correlated with parental income, (2) the transmission of these skills, or (3) the economic returns to these skills. For any given skill, the mechanisms linking inequality to its distribution could be mobility-reducing while the mechanisms linking inequality to its transmission could be mobility-enhancing, yielding ambiguous predictions about the net inequality-mobility relationship. Moreover, looking across the range of skills generating intergenerational income persistence, the net inequality-mobility association is uncertain because inequality may affect different skills differently. In short, offsetting effects both along a single intergenerational pathway and across multiple intergenerational pathways generate theoretical ambiguity. Two examples, one for each type of offsetting effect, help clarify how this ambiguity manifests in the contemporary U.S. context.

First, previous research yields ambiguous predictions regarding the net effect of inequality along the mobility pathway connecting parents' and children's incomes through children's education. Higher-income parents have more resources to invest in their children's education than lower-income parents. As inequality rose, budget constraints on these investments fell more among affluent than poor families (Alderson, Beckfield, and Nielsen 2005). Consequently, the affluent may have increased their investments in their

children's education faster than the poor. In turn, these investments may have shifted the distribution of children's academic skills such that affluent children's chances of remaining affluent as adults increased. Consistent with this hypothesis, Reardon (2011) finds that the test score gap between high- and low-income children rose through the period of rising inequality.[7] However, shifts in parental investments that generate changes in the distribution of children's academic skills can be offset by changes in government redistribution, which alter the transmission of these skills (Solon 2004). The inequality-mobility relationship thus depends on the net influence of public and private human capital investments. Highly progressive public spending on children's human capital development can increase mobility, tying children's educational achievements more closely to public provisions and reducing the dependence on parental income.[8] In fact, redistributive state spending on programs benefiting children increased during the early period of rising inequality (Mayer and Lopoo 2008). Offsetting public and private shifts could help explain the relative stability, or even decline, in the association between children's educational attainment and their socioeconomic background (Breen 2011; Hout and Janus 2011).

Second, moving beyond ambiguity along a single pathway of intergenerational persistence, variation across pathways in how inequality affects persistence also generates theoretical uncertainty regarding the net inequality-mobility association. Whether or not changes in the distribution of economically-valuable skills (driven by parental investments) were counterbalanced by shifts in these skills' transmission (due to government expenditures), strong evidence reveals that the economic returns to children's education rose along with inequality (e.g., Autor, Katz, and Kearney 2008). In isolation, these rising returns tend to decrease mobility (since the predictive power of education increases and education itself is a function of parental income; Bloome and Western 2011). Thus, it is not unreasonable to assume that on net, inequality associates positively with the strength of the pathway linking parents' and children's incomes through children's education. However, because many sources of similarity contribute income persistence, changes in one pathway can offset changes in another. In the U.S. context, several im-

portant demographic shifts associated with increasing inequality may have also increased mobility. For example, shared union membership across generations may have provided a path to intergenerational continuity in middle-class incomes. Consequently, declining union prevalence may have increased both inequality and (potentially downward) mobility (Western and Rosenfeld 2011). Likewise, the rising share of single-mother families both contributed to rising family income inequality (Martin 2006) and shifted children into family structures with relatively high (downward) mobility, as parental absence weakens the familial processes that reproduce socioeconomic status (Björklund and Chadwick 2003; Biblarz, Raftery, and Bucur 1997). Children from middle-class backgrounds are more likely to be downwardly mobile if raised by one parent (DeLeire and Lopoo 2010).

In sum, although many have expressed concern regarding the mobility-depressing potential of high inequality (e.g., Esping-Andersen 2004; Wilkinson and Pickett 2010), null or even positive associations are theoretically plausible. Complex interactions between the family, the state, and the market in shaping children's opportunities generate theoretical ambiguity. The preceding formal analysis reveals that this ambiguity stems from the likelihood of offsetting inequality effects, both along any one pathway linking parents' and children's incomes (due to different inequality effects on the distribution, transmission, and returns to the skill along that path), and across multiple pathways. Resolving this uncertainty requires empirical evidence.

Empirical Methods

Most studies of intergenerational income mobility examine some variant of the model

$$\ln Y_i^{child} = \alpha + \beta \ln Y_i^{parent} + \epsilon_i \tag{1}$$

where Y is income (adjusted for age and measurement error) and β is the elasticity of children's income with respect to their parents' income (e.g., Solon 1992; Mayer and Lopoo 2005). An elasticity of 0.5 implies that a 10% difference between two families'

incomes translates into an average difference of roughly 5% between their children's incomes. While the elasticity (β) measures persistence, its complement ($1-\beta$) measures mobility: $1-\beta$ represents the fraction by which children may expect to be closer to the mean than their parents were (Bowles and Gintis 2002).

To examine the relationship between inequality and mobility, I write the income elasticity as a function of the income inequality children experienced in their states when they were young, using three basic model specifications: simple OLS models with interactions, fixed effects models, and random effects models.**[9]** Equation 1 assumes a single income elasticity, implicitly averaging over heterogeneity in income persistence. As a first step toward relaxing equation 1's homogeneity assumption, I introduce an interaction between parents' income and state-year inequality. For family (parent-child pair) i observed at time t living in state s, the simple interaction model is written as

$$\ln Y_{ist+20}^{child} = \alpha + \gamma_a \mathrm{Gini}_{st} + \beta \ln Y_{ist}^{parent} + \gamma_b (\ln Y_{ist}^{parent} * \mathrm{Gini}_{st}) + \epsilon_{ist}. \qquad (2)$$

The interaction coefficient γ_b reveals whether the relationship between parents' and children's incomes depends on the inequality children experienced while growing up.**[10]**

Equation 2 pools all the variation (across individuals growing up in different states in different years) to estimate a single, shared set of parameters. An alternative specification utilizes variation only within states and years. This fixed-effects specification is written

$$\ln Y_{ist+20}^{child} = \alpha + \gamma_a \mathrm{Gini}_{st} + \beta \ln Y_{ist}^{parent} + \gamma_b (\ln Y_{ist}^{parent} * \mathrm{Gini}_{st}) + \mu_s + \mu_t + \epsilon_{ist} \qquad (3)$$

where μ_s (μ_t) represents a state (year) fixed effect and ϵ_{ist} is a family-specific error term.**[11]**

Random effects models present a compromise between the simple interaction model and the fixed effect model, by partially pooling the variation across contexts. In the random coefficient specification, I model mobility with a general, shared component, a component that depends on state-year characteristics such as family income inequality, and a state-year random error component. This error component permits state-years with

equal covariate values to have different predicted income elasticities. Each state-year also has a unique intercept. The random components of the intercept and elasticity are drawn from a multivariate normal distribution. The model is written as

$$\ln Y_{ist+20}^{child} = \alpha_{st} + \beta_{st} \ln Y_{ist}^{parent} + \epsilon_{ist} \tag{4a}$$

$$\alpha_{st} = \alpha_0 + \gamma_a \mathrm{Gini}_{st} + \mu_{st} \tag{4b}$$

$$\beta_{st} = \beta_0 + \gamma_b \mathrm{Gini}_{st} + \nu_{st}. \tag{4c}$$

The intergenerational income elasticity, β_{st}, varies across states and years according to equation 4c.[12] It is a function of an average slope, the Gini coefficient in state s in year t, and a random component ν_{st}, which represents variation in the elasticities not captured by the measures included in the model. Like the elasticity, the intercept α_{st} varies across states and years, depending on both fixed state-year characteristics and a random state-year deviation from the fixed values, μ_{st}. The μ_{st} term represents the state-year 'contribution' to children's adult family incomes.[13] I allow the random components of the intercept and slope to covary. This model allows the relationship between parents' and children's incomes to vary with both observed and unobserved state-year characteristics, including family income inequality. The coefficient of interest is γ_b.

Throughout the paper, I present results from all three model specifications (OLS, fixed effects, and random effects; I also present random effects models that allow random variation only in the intercept, not the slope). The findings do not depend on these modeling choices. However, I focus most closely on the random coefficient model (equations 4a-4c) for several reasons. The β_{st}'s are optimal shrinkage estimators, in that they are weighted averages of the within- and between-state-year estimates. Borrowing strength across states and years improves the raw state-year estimators (reducing their mean squared error).[14] Reductions in MSE are not valued for purely statistical purposes, but rather for the additional information they provide via narrower confidence intervals,

compared to wider, less informative intervals. Since Stein (1956) recognized the dominance of shrinkage estimators over stratified maximum likelihood estimators, statisticians and social scientists have successfully developed random effect models to study situations in which both micro-level and macro-level observations play important roles. Moreover, random components account coherently for clustering of observations within states and years in the likelihood function, rather than requiring post-hoc corrections like the other models. However, for robustness, I explore all these model specifications.

All models described so far have excluded covariates beyond parental income and inequality. This allows the elasticities to capture the full association between parents' and children's incomes and the interactions capture the raw, unadjusted association between mobility and inequality. However, I also explore models that adjust for covariates in an attempt to make the observations more comparable and explain any observed relationship between inequality and mobility. I sometimes adjust for a vector of family-varying covariates at the individual level (including parental education, marital status, and race) and a vector of state covariates at the macro level (some time-varying, like racial and ethnic composition and educational spending per child, and some time-invariant, like region). Appendix A provides more information on these covariates.

Finally, in addition to exploring different specifications of the model's error structure and covariate vector, I also explore different measures of economic mobility. In most models I explore the elasticity, as described above. However, I also model the correlation. In a bivariate regression, the elasticity is $\beta = \rho(\sigma_y^{child}/\sigma_y^{parent})$, where ρ is the intergenerational correlation and $(\sigma_y^{child}/\sigma_y^{parent})$ is the ratio of the standard deviations of children's and parents' logged incomes. In some models I standardize log incomes to mean zero and standard deviation one, equalizing the elasticity and the correlation. Standardized measures are theoretically preferable if we are interested in the inequality-mobility relationship, abstracting from the distributions of parents' and children's incomes observed in these sample data. However, as shown below, accounting for these distributions does not change the conclusions about the relationship between inequality and mobility.

Data and Measures

To study the association between intergenerational mobility and income inequality, I combine parent-child pairs from the Panel Study of Income Dynamics (PSID) with government data on state characteristics. I also use parent-child pairs from the National Longitudinal Survey of Youth 1979 cohort (NLSY79) to replicate the PSID analysis, though differences in survey design necessitate some differences in analyses.

The PSID is the longest-running study providing income data on a national sample of individuals and families. Beginning in 1968 with approximately 5,000 families, surveys continued annually until 1997 and biannually thereafter. The survey followed both children and parents from the initial sample, permitting comparisons between the children's family incomes in childhood and adulthood. I analyze incomes between 1967-2006 (survey years 1968-2007) for children born in 1954-1974 who were living in the U.S. as teenagers during the late 1960s to early 1990s, a period of rising inequality.[15] Results cannot be generalized to populations not present in large numbers when the PSID began, such as recent immigrants. Initially, the PSID's core sample was composed of two sub-samples, the Survey Research Center (SRC) national sample and the Survey of Economic Opportunity (SEO) low-income oversample. Serious irregularities in the sampling of SEO respondents preclude easy generalization to any well-defined population (see Brown 2006), so I study the SRC sample.[16]

Like several recent mobility studies using the PSID, I examine total family income (e.g., Chadwick and Solon 2002; Mayer and Lopoo 2005; Lee and Solon 2009). Although many mobility studies compare fathers' and sons' labor earnings, children's development and adult wellbeing depend more heavily on a family's total resources than on father's earnings alone. Family income also improves inferences for children from non-intact families and for married daughters, whose family income is especially affected by their spouse's income.[17] Further, using family income reduces omitted variable biases that arise when ignoring mothers' attributes, and it makes over-time comparisons more reasonable by

incorporating changing assortative mating patterns (Beller 2009).[18] Family income includes income from labor earnings, assets, and transfers accruing to the head, spouse, and other family members. Because after-tax income is not consistently available in the PSID, I consider pre-tax income, although transfers such as AFDC are included.[19] I adjust for inflation using the CPI-U-RS. To obtain consistent topcodes and eliminate extreme outliers, I exclude individuals with family incomes in the top or bottom two percent within age-gender-year cells (Winship 2009; Gottschalk and Moffitt 1994). (Different top- and bottom-coding schemes do not change my results.)

Since income fluctuates, I average over five years to reduce sensitivity to the measurement year and better capture permanent incomes (Mazumder 2005).[20] Because young adult income is often unstable, the intergenerational income elasticity is best measured when children at least 30 (Haider and Solon 2006). I compare grown children's family incomes when they were between ages 30-34 to their parents' family incomes when the children were between ages 13-17. These ages enable comparisons of grown children's economic resources with their family's resources when they were teenagers. I restrict the age range to ensure that cohort differences are not driven by differences in the ages at which income is observed. Throughout the paper, I report on income both unadjusted and adjusted for need (using the square root of family size). Like Hertz (2007), I find that adjusting for need sometimes affects mobility estimates. Adjusting for need helps capture the resources available for children in their families of origin. However, it also mixes measures of children's adult incomes with their family size choices. Thus, both adjusted and unadjusted measures substantively interesting. Table 1 contains descriptive statistics for parents' and children's incomes. The variance of real income increased across generations (whether or not income is adjusted for family size). Inequality also varied by region, with relatively high income dispersion in Southern states.

[TABLE 1 ABOUT HERE]

The PSID is perhaps the most widely-used dataset for studying intergenerational mobility in the U.S., and it has much to recommend it for the current analysis. Importantly,

the sample includes a wide range of birth cohorts, enabling exploration of how mobility varies across time as well as across states. However, the PSID sample is fairly small. Consequently, the power of statistical tests may be low and a relatively strong relationship between inequality and mobility may be statistically indistinguishable from zero. I therefore complete an analysis similar to my PSID analysis using data from the National Longitudinal Survey of Youth 1979 cohort (NLSY79). Table 1 shows that the NLSY79 analytic sample is about 2.5 times larger than the PSID sample.

The NLSY79 began with a national sample of over 12,000 men and women ages 14 to 22 in 1979. Respondents were reinterviewed annually through 1994 and biannually thereafter. I study income reports for the years 1978-2009 (survey years 1979-2010). In the earlier survey years, when respondents were young and many were living in their parents' household, parents were given a version of the survey in which they reported their income. I only use income as reported by parents in these early years. To avoid over-representing late home-leavers I exclude those older than 19 in 1979. Because the remaining cohorts were born so close together (1960-1964), I focus only on cross-state differences in the NLSY79 analysis. The NLSY79 contains a nationally-representative sample as well as over-samples of African Americans, Hispanics, and poor whites. To maintain as large a sample as possible, I include these over-samples using survey weights, although I also report on models excluding these over-samples and weights.

Just as in the PSID analysis, I focus on total family income in the NLSY79. I adjust for inflation and family size and handle topcoding as described above for the PSID. I also focus on multiyear averages, though the averages cover slightly different age ranges than in the PSID. I measure parental income during the children's teen years using the 1979-1983 NLSY79 surveys, averaging all years in which parents reported their family income for the children age 14-19 in 1979. I measure adult family income when the respondents were age 30 and older, averaging all available observations. Because in the NLSY79 there is no concern about cohort differences in the ages at which income is observed, I do not restrict the observations to ages 30-34 (as in the PSID), but rather attempt to maximize

the sample size by using all observations age 30 and above. Table 1 reports descriptive statistics for parents' and their adult children's family incomes. As in the PSID, the variance in real income increased substantially across generations.

I augment the PSID and NLSY79 mobility data with state-level measures of family income inequality. Using a geographic indicator specifying current state of residence, I assign each child the level of inequality in the state where he lived when he was about 14 years old – around the same time I measure parental income. In the PSID, the calendar year for this measure varies across cohorts. In the NLSY79, all individuals' state of residence comes from the 1979 survey.[21] Measuring inequality at the same time as parental income not only focuses on developmentally-shared income transmission paths. It also maximizes the sample size. In the PSID, earlier measurement would eliminate children from less-recently born cohorts, whose early-life state of residence is not observed (because it occurred before PSID data collection began). Nevertheless, the best age at which to measure inequality depends on the mechanisms linking inequality and mobility. For example, age 14 may be appropriate if inequality affects mobility via public investments in schooling; individuals beginning high school are starting to make decisions affecting their adult educational attainment. However, some research suggests that early-life investments may be more important (e.g., Cunha and Heckman 2009). Consequently, I also explore inequality in respondents' state and year of birth using the NLSY79 (as reported in 1979). In the PSID, I explore inequality in a respondent's state of residence at age 14 but in the year when he was age 4 (imperfectly capturing age 4 state inequality while keeping multiple birth cohorts in the analysis). Analyses examining inequality at different ages generate similar substantive conclusions.

Historical decennial Census data provide the most reliable estimates of state family income inequality due to the very large samples, even in relatively unpopulated states. I use the Gini coefficient for family income, linearly interpolating the values for intercensal years by state. The linear assumption does not exactly follow national inequality trends. Thus, I also employ family income Gini coefficients calculated from March Current Pop-

ulation Survey (CPS) data as a robustness check. Due to small state-year samples, the CPS generates less stable estimates, but it provides an annual series. I also examine 90/50 and 50/10 ratios as well as top 1% income shares, which are more sensitive to changes in the tails of the income distribution, to see whether certain types of inequality are especially influential. I estimate the ratios from decennial Census and March CPS data. Frank (2008) provides the top 1% shares, as derived from IRS records.

Besides the information required to construct mobility and inequality measures, I construct additional variables from the PSID, NLSY79, and various government sources to examine how micro- and macro-level forces shape the inequality-mobility association. At the family level, I measure parental age, education, race, marital status, and the child's sex. At the state level, I measure several attributes that, if left unaccounted for, could drive spurious associations between inequality and mobility. Time-varying attributes include the state's percent African American, percent Hispanic, percent poor, and median income. I also account for the region of the country. Including all of these measures provides a very conservative test of inequality's association with mobility, since some of these measures might reflect endogenous processes. (For example, high inequality could reflect high prevalence of low-wage jobs, which might increase the minority share of the population by drawing African American or Hispanic workers.) Consequently, most model specifications exclude these state-level covariates, but some include them for robustness. I also calculate several state-level measures that might help explain any observed inequality-mobility relationship. These measures capture several macro-level mechanisms potentially linking inequality and mobility. I measure per-capita spending on health, per-capita spending on welfare, per-child spending on education, and residential segregation by income. Appendix A describes these measures in greater detail. Appendix Tables A1 and A2 contain descriptive statistics for the PSID and NLSY79 samples.

Results

Figure 1 shows a large upward shift in the distribution of state inequality over time. Members of more recently-born cohorts experienced substantially more inequality when growing up than members of earlier-born cohorts. Research has focused on national trends, but states also vary considerably in their inequality levels and trends. Both longitudinal variation across years and cross-sectional variation across states provide useful information for estimating the relationship between inequality and mobility.

[FIGURE 1 ABOUT HERE]

Turning first to national-level time trends, U.S. family income inequality rose monotonically (and dramatically, relative to historical standards) between the late 1960s and the early 1990s, the years in which the PSID children were teenagers. If inequality hampered mobility, we would expect the family income elasticity to increase across birth cohorts, while if inequality stimulated mobility we would expect declines. In fact, Figure 2 shows no consistent trend in the family income elasticity for birth cohorts 1954-1974. Across the 20 year span of birth years (corresponding to age 30 income observations between 1984 and 2004), the elasticity ranged between .38 and .59. It exhibited no systematic upward or downward movement to mirror the trend in family income inequality.[22] This finding aligns with previous studies using the PSID, which uncover little evidence of recent shifts in the mobility regime (Lee and Solon 2009; Hertz 2007). However, these results also improve on previous PSID-based estimates of national-level mobility trends. Though my point estimates and those of Lee and Solon (2009) are extremely similar, by optimally pooling the data using random coefficient models, my estimates are much more precise. This additional precision reduces the width of the estimates' confidence intervals by more than half, thus excluding many more extreme values (see Appendix Table A3).[23] The data do not provide evidence of national mobility trends that align with inequality trends.[24]

[FIGURE 2 ABOUT HERE]

However, this examination of national trends obscures important information on the relationship between inequality and mobility by averaging over state-level differences. I improve on previous investigations by examining inequality and mobility by both state and year. The state-year design provides a stronger test for an inequality-mobility relationship by more fully exploiting variation in both inequality and mobility. The percent of the total residual variance in children's income that is shared at the group level is much higher when children are grouped by state and year than when they are grouped only by year. This additional shared variation better enables the detection of group-level trends.

Figure 3 plots the relationship between state-year family income inequality and state-year family income mobility. Examining the horizontal spread, Census data reveal significant variation in the Gini coefficient across states and years. Examining the vertical spread, PSID data show substantial differences in the intergenerational elasticity. These differences are larger than the differences observed with purely cross-cohort comparisons. In Figure 3, the elasticities range between .27 and .74. However, there is no apparent relationship between family income inequality and family income mobility. The inequality to which children were exposed in their state when growing up provides no information about the mobility they experienced as adults. State-years with higher inequality do not exhibit higher levels of income inheritance. The inequality-mobility slope is flat.

[FIGURE 3 ABOUT HERE]

But how sensitive are these results to the analytic choices underlying Figure 3, including data, measurement, and modeling choices? Sensitivity analyses generally focus on protecting against false positives in the context of estimating causal effects (e.g., Young 2009; Leamer 1983). However, it is also important to guard against false negatives in the context of estimating simple correlations. Especially when investigating macro-level questions — where the number of cases is relatively small and sharp designs for estimating causal relationships are seldom available — establishing the credibility of simple associations is crucial for building social scientific understanding. Likewise, although sensitivity

analyses have typically focused on model specification and, particularly, the choice of covariate vectors (e.g., Leamer 1985; Sala-i-Martin 1997), investigating the fragility of conclusions to data and measurement choices is equally important. Replication of results using new datasets, particularly, may provide more validation than testing multiple models on a single dataset (Freedman 1991). Consequently, I next explore different measures, models, and datasets to obtain the best possible estimates for describing the relationship between inequality and mobility in the U.S.

Model 1 in Table 2 reports the intergenerational income elasticity as estimated from PSID data. Likewise, model 1 in Table 3 reports the same quantity as estimated from NLSY79 data. Table 2 (model 1, top panel) shows an elasticity of .482 and Table 3 (model 1, top panel) shows an elasticity of .477. Both estimates are typical of those previously reported in the literature (see reviews by Solon 1999 and Black and Devereux 2011). They can be interpreted as averages over the cohort and state-by-cohort estimates presented above. The bottom panel of Table 2 shows that the estimated elasticity is substantially higher when income is adjusted for family size, at .540. Hertz (2007, Table 2) also finds this pattern in the PSID. In the bottom panel of Table 3, however, we see that adjusting for family size does not substantially alter the income elasticity in the NLSY79. Due to the two surveys' different sampling schemes and demographic compositions, there are many reasons why PSID and NLSY79 estimates might differ. However, these differences are not of primary importance in this paper. Rather, the goal is to assess the relationship between income inequality and income mobility using two independent samples, although they are representative of somewhat different U.S. populations.

[TABLES 2 AND 3 ABOUT HERE]

Models 2-9 in Tables 2 and 3 examine how the average income elasticity reported in model 1 varies with income inequality. Models 2 and 3 explore simple interaction models, using OLS to completely pool the information across individuals from different states and cohorts. Models 4 and 5 explore random intercept models (partially pooling the

between-context information). Models 6 and 7 explore random coefficient models (partially pooling the information, like the random intercept models, but also allowing the income elasticity to vary stochastically across states and years, rather than only varying deterministically with fixed covariate differences). Models 8 and 9 explore fixed effects models (which exploit variation only within states and years, ensuring that neither unmeasured differences in stable state characteristics nor national period effects bias the estimates of the inequality-mobility relationship). Within each of these model specifications, the first models (models 2, 4, 6, and 8) allow the income elasticity to vary only with income inequality. The second models (models 3, 5, 7, and 9) also allow it to vary with a variety of other macro-level characteristics (such as state racial composition) and introduce a host of additional covariates, measured both at the level of the individual family and at the same level as income inequality (state-by-year).

As expected, the income elasticity is substantially smaller when covariates are included in the model than when they are excluded, since the covariates help explain some of the intergenerational income similarity. For example, highly-educated parents tend to have both high incomes themselves and higher-income children compared to less highly-educated parents with the same income. Thus, adjusting for parental education reduces the intergenerational income association. Similarly, the magnitude of the coefficient on the interaction between parental income and income inequality, γ_b, which quantifies how parent-child income elasticities vary with the inequality children experienced when growing up, is often smaller in the models accounting for additional covariates. However, whether or not covariates are included, and no matter the specification of the model's error structure, the magnitude of the γ_b coefficient is always very small and almost always statistically indistinguishable from zero. There is very little evidence of a relationship between the inequality children experienced in their state when growing up and their later mobility away from their parents' economic status.

Table 2 shows that when income is adjusted for family size (bottom panel), the ratio of the interaction coefficient γ_b to its standard error is always less than one. When income is

not adjusted for family size (top panel), the ratio is 1.965 for one model specification, the random coefficient model without additional covariates (model 6); it less than one in both fixed effects specifications, two random effects specifications, and one OLS specification. However, even in the one specification suggesting a marginally significant relationship between inequality and mobility at conventional levels of significance, the magnitude of the relationship is quite small. It is easy to assess this magnitude, because the Gini coefficient is measured in standard deviation units. (I standardize the Gini both to facilitate interpretation and to reduce the correlation between the parental income main effect and its interaction with the Gini; see endnote 29 for further discussion of this point. Inference is not sensitive to this standardization.) The elasticity is estimated to be about .446 in states and years with the average level of inequality and about .497 (.446+.051) in moderately unequal states and years (with inequality one standard deviation above the mean). This difference is much smaller than the difference between the elasticities estimated with incomes adjusted versus unadjusted for family size (see model 1).

The PSID allows us to investigate differences in inequality and mobility both across different state contexts and within different states over time, covering cohorts born over 20 years. Like other recent analyses of the PSID, I find no systematic variation in mobility across cohorts growing up through a period of rising inequality. Moving beyond previous research by incorporating information on state differences in both their inequality levels and growth rates, my results reveal no strong link between inequality and mobility. As Figure 3 illustrates (displaying estimates from Table 2, model 6), differences in income inequality cannot explain the variation in mobility across state-year contexts.[25]

One limitation of the PSID sample, however, is its relatively small size. Consequently, inequality-mobility associations may not be detectable in these data. To address this concern, Table 3 displays NLSY79 results analogous to the PSID results in Table 2. Though the NLSY79 study design only permits investigation of the inequality-mobility relationship across states, rather than both states and time, the sample of children is about 2.5 times larger than the PSID sample. Even with this increased power, however, there is no

evidence of an inequality-mobility association. Regardless of whether the model is specified with a simple interaction term, random effects, or fixed effects, regardless of whether additional covariates are included, and regardless of whether income is adjusted for family size, the coefficient of interest γ_b is quite small and statistically indistinguishable from zero.[26] The estimates are also generally smaller in size than the PSID estimates. The simple average of the estimates presented in Table 3 is about -.004, versus about .028 in Table 2. In short, these NLSY79 results provide little evidence of a relationship between inequality and intergenerational mobility across U.S. states, at least for the cohorts who were teenagers in the late 1970s and early 1980s, who then entered the labor market and formed adult families in the 1990s and 2000s. Combining these cross-state findings from the NLSY79 with the PSID results based on comparisons across both states and years, the best available data cannot confirm the hypothesis that there is a systematic link between inequality and mobility in the U.S.

Figures 4 and 5 investigate whether the dearth of evidence for an association between family income inequality and family income mobility reflects poor measurement of either inequality or mobility. Measurement issues are especially important when effect magnitudes are small, as it is difficult to separate weak signals from noise.[27]

All estimates presented thus far measure inequality by the Gini coefficient. Previous research that has investigated the relationship between inequality and mobility using cross-national data has also focused on the Gini (e.g., Corak 2012; Blanden 2009; Andrews and Leigh 2009). However, mobility might vary across U.S. contexts only in response to changes in particular parts of the income distribution. For example, highly-disproportionate income shares accruing to individuals at the very top of the distribution could give these individuals disproportionate political influence. Consequently, top-tail inequality might influence mobility by limiting public spending on mobility-enhancing policies (Burtless and Jencks 2003). To address this concern, I re-estimate the mobility-inequality relationship using the income share of the top 1% to measure inequality. I also use the ratios of the $90^{\text{th}}/50^{\text{th}}$ and the $50^{\text{th}}/10^{\text{th}}$ percentiles of family incomes. These

measures are more sensitive to distributional shifts in the tails than the Gini.

Figures 4 and 5 also explore different data sources for measuring inequality. I repeat the analyses using the Gini and the 90/50 and 50/10 ratios twice, using summary measures from decennial Census and March CPS data. Tables 2 and 3 use family income Ginis from Census data, linearly interpolated by state for intercensal years. Particularly in the PSID analysis, the benefits of large Census samples for generating state-specific estimates may be outweighed by the errors introduced by linear interpolation. CPS data allow for an annual series of state-specific estimates. (The top 1% shares are calculated from annual IRS data, and are never interpolated.)

Finally, Figures 4 and 5 also explore three different income measures. First, as in Tables 2 and 3, I examine income both with and without adjustments for family size. Adjusting income helps capture the resources available to children during youth (since the same income suggests fewer resources for a family of six than a family of four), but leaving income unadjusted untangles the transmission of income and family size. Second, I examine income that has been standardized to mean zero, standard deviation one in both the children's and parents' generations, thus focusing inference on correlations, rather than elasticities as in Tables 2 and 3. Third, while previous PSID models include imputed income components, Figure 4 examines models excluding individuals with any income component imputed by "major assignment" in the years over which their income is averaged. (The NLSY79 results do not include imputed incomes. Thus, I instead explore sample differences in Figure 5 by excluding the African American, Hispanic, and poor white over-samples, which are included in Table 3.)

[FIGURES 4 AND 5 ABOUT HERE]

Figures 4 and 5 suggest that measurement decisions do not drive the null relationship between inequality and mobility reported in Tables 2 and 3. These figures display the point estimates and 95% confidence intervals for the interaction coefficient γ_b, which quantifies how income mobility varies with a one standard deviation difference in income

inequality. Figure 4 uses PSID data while Figure 5 uses NLSY79 data. Of the 56 models in Figure 4, only four have 95% confidence intervals excluding zero (only about one more than we would expect given a 5% false discovery rate). All four use unstandardized income. In general, the point estimates are larger when income is unstandardized. This result is expected, because unstandardized elasticities rise when income inequality in the children's generation rises compared to inequality in the parents' generation, generating a somewhat mechanical relationship between unstandardized elasticities and inequality. However, even when income is unstandardized there is very little evidence of an inequality-mobility association.[28] Estimates also appear somewhat larger when income is not adjusted for family size and when imputed income observations are excluded, though inference is identical. No strong patterns emerge across inequality measurement choices. No matter the measure of inequality, or its underlying data source, or the treatment of income imputations, or the adjustment for family size, the interaction coefficient quantifying the inequality-mobility association remains small in magnitude and is almost never statistically distinguishable from zero. Figure 5 provides additional support for these conclusions. None of the 56 models estimated on the NLSY79 data generate statistically significant point estimates. Moreover, these estimates are generally smaller in magnitude and more precise than the PSID estimates. The larger sample decreases the widths of the confidence intervals. No matter the measures used, these data provide very little evidence that income inequality shapes intergenerational mobility.

Together, Tables 2 and 3 and Figures 4 and 5 reveal that substantive conclusions regarding the inequality-mobility relationship do not depend on the model's error specification (OLS versus random effects versus fixed effects) nor on the measurement of either inequality or mobility. Appendix Figures A1 and A2 further confirm that results do not depend on the specification of the model's covariate vector. Tables 2 and 3 examine the Gini-by-parental income interaction in two ways: first, conditional on only Gini and parental income main effects, and second, conditional on Gini and parental income main effects plus all other state- and individual-level main effects and interactions. Exploring

only these two covariate vectors raises concerns about both under- and over-controlling.[29] However, as Appendix Figures A1 and A2 reveal, no matter the model's error structure or its covariate vector, there is very little evidence to support the conclusion that income inequality and income mobility are associated in the U.S.

The PSID and NLSY79 suggest that children's income mobility may not be linked to the income inequality they were exposed to during youth. The inequality children experienced in their states as teenagers does not predict their income mobility. Using inequality during the teen years, when parental income is measured, both focuses the analysis on a single developmental stage (when pathways of income transmission may be shaped by individual family resources as well as the wider economic context) and also maximizes the analytic sample size (since early-life state of residence is not observed for earlier-born cohorts, who were very young before PSID data collection began). However, some research suggests that early childhood deprivations and inequality might have larger impacts on mobility prospects (e.g., Cunha and Heckman 2009). Consequently, I also explore inequality earlier in the life course.

The results from these additional analyses also fail to confirm the hypothesis that higher inequality is linked to lower intergenerational mobility. The PSID results are similar across Table 2 (which explores teen inequality) and Appendix Table A4 (which explores early childhood inequality around age 4). Table A4 provides somewhat greater support for the hypothesis, as the coefficients describing how parent-child income elasticities vary with inequality, γ_b, are somewhat larger (e.g., random coefficient models estimate γ_b to be .055 when using early childhood inequality as in Table A4, versus .051 when using teen inequality as in Table 2, or .039 versus .032 when income is adjusted for family size) and more likely to be statistically distinguishable from zero. However, the inference remains fragile, depending on whether random or fixed effects are used and whether or not income is adjusted for family size. More surprisingly, the NLSY79 results suggest that children exposed to higher income inequality in their states at birth experience significantly more intergenerational mobility than children from lower inequality

states.[30] The γ_b's in Appendix Table A5 are all negative (suggesting that lower elasticities accompany higher inequality) and almost all are statistically distinguishable from zero. This contrasts with the findings in Table 3, where the γ_b estimates are smaller, almost never statistically significant, and of mixed sign. Altogether, no strong evidence emerges from the PSID or NLSY79 to confirm that higher inequality is systematically associated with lower mobility in the U.S., whether inequality is measured during children's teen years or much earlier in their lives.

Conclusions

As family income inequality rose over recent decades, parents' resources available for their children's development diverged, both across cohorts and across states. These shifts led to renewed interest in intergenerational mobility amongst both scholars and policymakers. Many speculated that relatively high inequality would undermine economic mobility prospects. This paper sought to clarify the relationship between family income inequality and intergenerational income persistence in the contemporary U.S. It provides the first systematic analysis of how inequality and mobility covary across contexts within the U.S., exploiting variation across cohorts, across states, and within states over time.

Combining data from the PSID with information on state characteristics, I find little evidence of a relationship between individuals' economic mobility and the income inequality they experienced when growing up. Like other recent studies of national-level time trends using the PSID (e.g., Lee and Solon 2009; Hertz 2007; Mayer and Lopoo 2005), I find that over a twenty year period in which income inequality rose continuously, the intergenerational income elasticity showed no consistent trend. My estimates also improve upon previous estimates by reducing the width of the confidence intervals and, thus, excluding previously-plausible trend values. Evidence suggests no major shifts in mobility occurred nationally. Moving beyond previous research, I also find that differences in within-state inequality trends do not predict within-state mobility trends. Neither

do between-state differences in inequality covary with mobility, a result confirmed in my analysis of the NLSY79.

The lack of association between inequality and mobility across political jurisdictions within the U.S. appears to conflict with the negative association across larger political units documented in cross-national studies (e.g., Corak 2012; Björklund and Jäntti 2009; Andrews and Leigh 2009). One explanation of this difference highlights the fact that inequality varies more across countries than within. Perhaps if some U.S. states were as equal as Denmark or as unequal as Brazil we would observe a relationship. However, even restricting comparisons to countries where inequality is within the range observed in the U.S., cross-country analyses reveals a substantial inequality-mobility association. A second explanation suggests that cross-country differences in inequality are more long-standing and that the consequences are therefore more institutionalized. Perhaps the mobility consequences of recent inequality trends will not be evident until some future time, when cohorts born during the height of inequality mature, their permanent incomes are better measured, and institutional responses to rising inequality have had more time to take effect. However, cross-state differences in economic inequality are quite long-standing. In fact, Nunn (2008: 170) reports a very strong relationship between state-level Gini coefficients of land inequality in 1860 and Gini coefficients of income inequality in 2000. Nevertheless, there is very little evidence for a relationship between inequality and mobility at the state level in the contemporary U.S. Consequently, perhaps the best explanation for why the inequality-mobility association evident in cross-country comparisons is not replicated using comparisons within the U.S. is that the inequality-mobility association varies across countries. The forces driving the cross-county association may not be directly applicable within any one country.

Heterogeneity in the roles of the family, the state, and the market may induce different relationships between inequality and mobility in different countries. Previous research on the relationship between mobility and educational investments also suggests that cross-national correlations may not extend to the U.S. context. Looking across de-

veloped nations, Blanden (2009) reports a strong negative correlation between education spending as percent of GDP and father-son elasticities, suggesting that mobility rises with educational investments. However, Grawe (2010) finds that U.S. states with lower student-teacher ratios (meaning higher educational investments) also have lower earnings mobility, and Mulligan (1999) find no significant relationship between earnings elasticities and several measures of education spending (including state education spending per student and state student-teacher ratios). Focusing on Iowa in the early 1900s, Parman (2011) reports that districts with greater school access had lower intergenerational mobility than districts where public school access was more limited. Although cross-national regressions imply that increased public investments in education increase mobility, research using only U.S. data suggest that the cross-national result may not apply in this country. Likewise, this paper suggests that the relationship between inequality and mobility across countries may not extend to the U.S. context. However, my results also cannot definitively rule out a modest association.

The evidence presented in this paper provides very little support for the hypothesis that income inequality between families shapes income mobility across generations in the U.S. However, the nature of scientific inference does not permit the conclusion that there is absolutely no relationship between individuals' mobility away from their parents' incomes and the inequality they experienced when growing up. The imprecision of the PSID estimates, in particular, cannot allow us to rule out a modest relationship, despite the small point estimates.

I work to increase precision in three ways. First, I more fully exploit variation in inequality and mobility to increase the power of the tests (looking across states and years, not only across years like previous studies). Second, I use random coefficient models to optimally pool the information from different state and year contexts and reduce mean squared error (although both OLS interaction models and fixed effects models lead to similar conclusions). Third, I supplement the PSID with the NLSY79, which offers a substantially larger sample. All of these approaches help reduce the uncertainty

associated with our understanding of how inequality associates with mobility in the U.S.

I gather as much evidence about the inequality-mobility relationship as possible by exploring a wide variety of measurement choices and model specifications. Amongst the hundreds of models explored, a few produce associations between inequality and mobility that are statistically distinguishable from zero. However, the results are very fragile. Inferential decisions are easily reversed by changing the model's error specification, its covariate vector, or the way income is adjusted for family size. A reader would have to be very strongly convinced of the truth of a specific generative model to conclude that these data provide strong evidence that mobility depends negatively upon inequality. The data cannot rule out the possibility that mobility is relatively low for individuals who grew up in states and years when inequality was relatively high, nor that mobility is relatively high. However, the results suggest that, even if there is a non-zero association between inequality and mobility in the contemporary U.S. that the best available data cannot detect, the strength of the association is likely to be quite modest.

The lack of association between inequality and mobility may derive from countervailing trends in the sources of income similarity across generations. Economic mobility is shaped by a variety of familial, governmental, and market processes that could change in offsetting directions. For example, rising inequality driven by increasing returns to education might decrease mobility, by more closely linking parents' investments in their children's human capital to their children's earnings (Solon 2004). However, concurrent trends in the progressivity of state spending on children's human capital development might increase mobility (Mayer and Lopoo 2008). Taken together, these shifts could generate a null relationship between inequality and mobility. Counterbalancing effects are especially likely if inequality's influence on mobility is relatively weak. Many forces affecting income similarity across generations may not respond to changing inequality in a timely fashion, and some may not respond at all (for example, genetic resemblance, parenting practices, or state policies that progress through legislative bodies at unpredictable rates). Even along the "susceptible" paths, inequality per se may be much less

influential than other family or environmental characteristics. We may need much larger changes in inequality before we observe significant effects. The results presented here are limited to inequality within the range observed across U.S. states between the late 1960s and the early 1990s. This range is substantial by historical standards; it covers a period of rapidly increasing inequality, and inequality differences across states are just as large. Nevertheless, it is possible that the differences were too small to have much effect on mobility. While extreme fluctuations in inequality might alter the transmission of advantages across generations, the "treatment dosage" may have been too weak to generate marginal effects over the range experienced in the U.S. for the cohorts studied.

Countervailing trends mean that studies of the relationship between inequality and one single path linking parents' and children's incomes can generate misleading intuitions regarding the overall inequality-mobility relationship. Nevertheless, focused studies remain vitally important for the information they provide about opportunity, as distinct from mobility. The data used in this study reflect the distribution of economic outcomes, not opportunities. These data suggest no net association between inequality and income mobility in the contemporary U.S. Nevertheless, we might be concerned if, for example, rising inequality reduced low-income children's ability to complete college, even if this effect were counterbalanced by inequality's incentive effects boosting college entrance rates by convincing more high school graduates that they should try to earn a BA. From an opportunity perspective, the particular obstacles that individuals must overcome to obtain their adult incomes matter. The "means" can be as important as the "ends." To understand the relationship between inequality and economic opportunity, we must examine the mechanisms generating intergenerational income persistence (Swift 2004). Research on different pathways linking parents' and children's incomes should clarify how inequality shapes opportunities for economic success. However, these more focused investigations will continue to benefit from studies, like the current one, that capture overall, net effects and contextualize the wide range of mechanisms within a broader system.

While this analysis finds little evidence of an association between economic inequality

and income mobility in the U.S., it is not definitive. The children covered in this analysis were born between 1954 and 1974. They were teens between the late 1960s and the early 1990s. These findings may not apply to children growing up during the more recent period of persistently high inequality. It will take another 5-15 years for such children to reach the age at which their incomes can be fruitfully compared to their parents' incomes. This study is also limited to children who grew up in the U.S. Investigations of how mobility varies within other countries could provide useful insights into the consequences of inequality, as well as the extent to which the inequality-mobility relationship differs across nations.[31] If different birth cohorts or nations suggest that inequality is associated with intergenerational mobility, then it would be valuable to better account for the mechanisms linking inequality and mobility. The current study quantified several macro-level mechanisms potentially linking inequality and mobility (specifically, spending on education, health, and welfare, and residential segregation by income), although ultimately there was no apparent relationship between inequality and mobility to explain. If future work reveals a stronger association, then exploring these mechanisms and the many others that remain unmeasured will be important for understanding how to improve economic opportunities.

However, currently available U.S. data provide little evidence of a systematic connection between family income inequality and intergenerational income mobility. Intergenerational economic mobility remains lower in the U.S. than in most other developed nations, and there is little evidence to suggest that rising inequality has been accompanied by rising mobility. Nevertheless, inequality may not reproduce itself by tying children more closely to their parents' positions on the economic ladder.

Appendix A: Supplemental Data

Besides the information required to estimate mobility and measure inequality, I use additional data to examine potential mediators and confounders of the mobility-inequality association. From the PSID and NLSY79, I use several demographic variables including parental age, race, education, and marital status. These measures help capture family mechanisms potentially linking inequality and mobility, for example, by shaping parental investment decisions. Tables A1 and A2 provide descriptive statistics for the PSID and NLY79, respectively.[32]

[TABLES A1 AND A2 ABOUT HERE]

I also examine two macro-level mechanisms that may link inequality and mobility: public spending and residential segregation by income. I use the Census Bureau's annual *Statistical Abstract of the United States* to obtain state spending on education. I divide this spending by the number of residents age 5-17 in the relevant year (using Intercensal Estimates of the Resident Population of States by Age from the Census Bureau) to obtain per-child education spending measures. I also use per-capita spending on health and hospitals and on other public welfare by state and year from the *Statistical Abstract* volumes. To measure economic segregation, I use the "neighborhood sorting index" (NSI — the square root of the share of total metro-area income variance that lies between census tracts; Jargowsky 1996) calculated from tract-level Census data by Tara Watson (see Watson 2009 for details). The NSI ranges theoretically between zero and one, when segregation is complete and there is no variance within tracts in the MSA, only between. To generate state-level measures, I average the NSIs for MSAs in the state, weighting by MSA population.[33] Tract-level data are available in Census years (1970-2000); I linearly interpolate for intercensal years by state.[34]

In addition to these potential mediators, I measure four potential inequality confounders, plus geographic region. I include the percent of the state population that is African American, as previous studies have found that effects attributed to economic

inequality were biased due to the exclusion of the fraction African American in the state (e.g., Deaton and Lubotsky 2003). I also include the percent Hispanic. To differentiate inequality from affluence or poverty, I measure state median family income and the portion of the state population whose family income falls below their official poverty threshold. These four measures were created from the Census Integrated Public Use Microdata Series (1960-2000). I linearly interpolate intercensal years by state. Tables A1 and A2 include descriptive statistics for state covariates. Regional differences are apparent, with Southern states housing the largest concentrations of poor and African American residents and generally engaging in lower social spending.

Notes

1 Moreover, the most important redistributive decisions may lie outside school funding. While debate about the educational effectiveness of school spending continues, some prominent researchers claim there is little relationship between resources and achievement (e.g., Hanushek 1996; but see Hedges, Laine, and Greenwald 1994).

2 Previous studies of mobility trends have each used a single data source and often come to conflicting conclusions. For example, Lee and Solon (2009) use the PSID and come to a different conclusion about mobility trends than Levine and Mazumder (2002), who use two National Longitudinal Survey (NLS) cohorts. While both analyses are reasonable, the lack of consistency suggests that small differences in analytic choices may drive conclusions. Consequently, it is crucial to study mobility questions using a standard approach on multiple data sources.

3 The NLSY79 analysis focuses on differences across states, rather than differences across both states and years like the PSID analysis. This limitation stems from the design of the NLSY79; it is a cohort study, so all individuals followed from their parents' homes into adulthood were born very close together in time. Nevertheless, by using multiple data sources to explore variation across both place and time, this paper provides new insights into the relationship between inequality and mobility in the U.S.

4 I use the term "skill" in a broad sociological sense to include not only human capital but also the wide range of cultural competencies and social and psychological orientations that contribute to economic attainment.

5 Note that additivity does not require the sources to be uncorrelated. The additivity of different sources' contributions can be relaxed by including interactions. Interactions can also relax the assumption that returns to a given skill are homogeneous across the population. For example, an interaction could generate different paths linking parents' and children's incomes via children's education for children from college-educated versus non-college educated parents (allowing college-educated parents to provide additional income returns to children's education).

6 Of course, many of the $\{q\}$ paths may be spurious, and even those representing causal effects of parental income may not be identified through structural equation or instrumental variable models without implausible assumptions (Alwin and Hauser 1975; Sobel 2008). Nevertheless, this framework facilitates the interpretation of mobility variation.

7 However, inconsistent with this hypothesis is the fact that this gap was rising even when inequality was not. The affluent may face declining marginal returns on their investments in children, which create a ceiling on the skills produced by additional income (Downey 1995). In this case, greater inequality in parents' investments in their children's education driven by a rising top tail may not generate additional skill inequality.

8 Mobility will increase particularly if the relative increase in private funds available to children from affluent families is offset by increasing public funds, which both raise total investments in children from poor families and partially substitute for private investments

in children from affluent families.

9 As discussed earlier, examining variation across states as well as over time not only increases the power of statistical tests that rely on variation across units to detect associations. It also allows us to capture important macro-level process that may link inequality and mobility, including public spending on mobility-enhancing programs. States also have methodological advantages for studying the inequality-mobility relationship over smaller areas of aggregation, like MSAs. MSA borders shifted over time, preventing clean comparisons of changes in inequality and mobility at this level. State borders did not change. Additionally, not all individuals in the U.S. live in MSAs, and the proportion of the population living in MSAs increased with inequality, meaning trends in inequality and mobility within MSAs cover different population subgroups at different times. In contrast, all U.S. residents live in states (or D.C., included here). Further, small-area inequality reflects selection through local migration, and the smaller the area studied, the more migration found and the more important this concern becomes. If unobserved characteristics driving parents to select one school district or metro area over another also influence children's mobility prospects, estimates of inequality effects will be biased. State-level inequality suffers far less from this selection bias because families are less likely to move between than within states to improve their children's socioeconomic prospects. In the last 60 years in the U.S., only 16 percent of annual moves were across state lines, on average (Frey 2009). For these reasons, this paper studies state-level family income inequality in addition to national-level trends.

10 Though children's adult incomes are indexed by s in equations 2-4, the sample is not restricted to individuals living in the same state during childhood and adulthood. Rather, s indexes the childhood state.

11 In models estimated with NLSY79 data, year fixed effects are not included because all individuals' childhood state is observed in 1979 (see data section). The PSID is not especially conducive to fixed effects specifications, because within-group sample sizes are small, making measurement error a substantial problem. Tests conventionally used to compare random and fixed effects specifications are not appropriate when errors in variables are large (Hausman 1978). In this application, all model specifications support the same substantive conclusions, so choosing between them is unnecessary.

12 The NLSY79 analysis exploits variation only across individuals and states, not years (see data section).

13 For completely non-nested models, the random components of the intercept and slope equations would contain only two terms, one for the state and one for the year (e.g., $\mu_{st} = \mu_s + \mu_t$). To capture the implicit nesting of years within states and allow for differential time trends within states, I also include a third term, analogous to an interaction within the random components (e.g., $\mu_{st} = \mu_s + \mu_t + \eta_{st}$), though the results are not sensitive to this decision (Bates 2005; Gelman and Hill 2007).

14 The random coefficient estimate of β_{st} is $\hat{\beta}_{st} = \omega_{st}\hat{\beta}^{\mathrm{A}}_{st} + (1-\omega_{st})\hat{\beta}^{\mathrm{B}}_{st}$. It is the weighted average of $\hat{\beta}^{\mathrm{A}}_{st}$, the coefficient estimated from data completely pooled across contexts (as in OLS models, using variation both within and between all states and years), and $\hat{\beta}^{\mathrm{B}}_{st}$, the coefficient estimate from data completely stratified by context (using variation only

within states and years). The weight ω_{st} determines the degree of cross-context pooling reflected in $\hat{\beta}_{st}$. Estimates from states and years with fewer observations are more heavily weighted toward the completely pooled estimate. The weights ensure that information is optimally pooled across contexts, drawing information from the full sample as necessary, rather than relying solely on small within-context samples.

15 These cohorts were selected to maximize sample size within the constraints of the PSID data collection. The 1974 cohort was the latest for which multiple years of income data above age 30 were available at the time of analysis. (As discussed below, income below age 30 is a poor proxy for permanent income.) The 1954 cohort was the earliest with information on state of residence at age 14, since the PSID began in 1968. (As discussed below, state contextual variables are measured around children's age 14, about the same time as parental income, though I also explore alternate ages.)

16 I also combined the SEO and SRC samples and completed the analysis with survey weights for each child taken from the latest wave in which he was observed (see Hill 1992). The weighted results do not differ substantively from the results presented here.

17 I combine sons and daughters in the analyses reported here, because I found no differences in their relationships between mobility and inequality, and combining them increases the power of the tests. However, I also completed the analyses stratified by sex; results available upon request.

18 Despite the many advantages of studying family income, one important disadvantage is that adult family income mixes labor market and marriage market effects. If, for example, inequality lowers the association between parental income and children's labor income but increases the association between parental income and children's spouses' income (due to increased assortative marriage), the overall inequality-mobility association might be null despite important offsetting dynamics. To capture these dynamics, I studied children's labor income in addition to family income. I also studied children's spouses' income and, thus, assortative marriage. I found no evidence of offsetting dynamics between spouses' labor incomes. Rather, the results from these supplemental analyses echoed the results from the analyses of family income. Results available upon request.

19 The results remain unchanged when using post-tax income estimated via the NBER's TAXSIM model following Butrica and Burkhauser's (1997) methodology.

20 To maximize sample size, I include respondents who have fewer than five observations averaged (due to survey or item non-response). Missing data on income questions in the PSID is quite low, about 2-3% (Duncan and Peterson 2001). Some observations include imputed income components. I explore the sensitivity to these imputations below. Because of the low rate of missingness and the likely failure of the "missing at random" assumption key to imputation methods (Lillard, Smith, and Welch 1986), individuals are excluded if no information is observed regarding parental and adult incomes.

21 NLSY79 state identifiers were obtained from BLS GEOCODE CDs through confidential agreement.

22 A formal test of the variance of the elasticity σ^2_β by cohort (using Scheipl, Greven, and Kuechenhoff's method [2008], which accounts for the placement of the null on the

boundary of the parameter space) reveals that mobility did not vary significantly. The test fails to reject the null that $\sigma_\beta^2 = 0$ with p = .435.

23 The range of elasticities reported in Table A3 is smaller than the range reported in the text because Table A3 focuses on men only, to facilitate comparison with previous research.

24 Some studies using other data and measures have come to similar conclusions about mobility trends (e.g., Hauser 2010; Harding et al. 2005). But, this is not universally true. For example, studies using National Longitudinal Surveys and decennial Censuses have found decreasing mobility (e.g., Levine and Mazumder 2002; Aaronson and Mazumder 2008). Results appear sensitive to the exact years studied and measurement choices (e.g., in Census studies, parental income is unobserved and must be proxied). The PSID results are especially credible because they reflect the most complete survey history of U.S. family income, in terms of both years covered and income information collected.

25 A formal test of the variance of the elasticity σ_β^2 by state-year reveals that mobility varied significantly across these contexts when income is adjusted for family size. The test rejects the null that $\sigma_\beta^2 = 0$ (p = .039). However, when income is not adjusted for size the null cannot be rejected (p = .213).

26 Of the 16 estimates presented in Table 3, 11 have t-ratios less than one. One estimate is significant at the .05 level: model 3, when income is adjusted for family size. But, when income is not adjusted for family size, the γ_b estimate from model 3 not only becomes statistically indistinguishable from zero but also flips sign, from positive to negative.

27 All estimates shown in Figures 4 and 5 come from random coefficient models (like model 6, Tables 2 and 3). However, as shown in Tables 2 and 3 as well as Appendix Figures A1 and A2, the results do not depend on model specification; the substantive conclusions from Figures 4 and 5 do not depend on the model used.

28 It may appear surprising that the relationship between inequality and unstandardized elasticities is not stronger, given that unstandardized elasticities increase when children's incomes become more unequal relative to their parents' incomes. However, as Hout (2004) notes, there is no necessary connection between inequality and mobility because the former refers to contemporary differences in living standards while the latter refers to differences across generations in living standards. By definition, changes in inequality across *generations* are associated with changes in mobility as measured by unstandardized income elasticities. However, changes in inequality over periods shorter than a generation (which, in fact, is not a well-defined concept temporally, as children from any given birth cohort have parents from many different birth cohorts) or differences in inequality across states may be unrelated to mobility.

29 One specific concern relates to the inclusion of both main effects and interaction effects. Mayer and Lopoo (2008), in their study of how intergenerational mobility varies with state government spending, find high correlations between government spending's interaction with parental income and the main effects, which made it difficult to estimate the interaction precisely. Unlike Mayer and Lopoo (2008), I find low correlations between the inequality-parental income interaction and the parental income main effect. Mayer

and Lopoo (2008: 149) report a correlation of .93. In my analysis the correlation is low by design, because I standardize my inequality measures to mean zero, standard deviation one. Standardizing inequality increases the interpretability of the results, as estimates refer to one standard deviation differences in the Gini (rather than one point differences, which are so large as to be meaningless because they indicate moving from no inequality to complete inequality). Standardizing inequality also reduces the correlation between the parental income main effect and the inequality-parental income interaction. In the PSID sample for parental income (not) adjusted for family size, the correlation is (-.11) -.07 when inequality is standardized and (.53) .59 when it is not. The corresponding numbers for the NLSY79 sample are (-.22) -.23 when inequality is standardized and (.75) .79 when it is not. Standardizing aids model estimation and interpretability without affecting inferential conclusions. However, to ensure that including other main effects do not suppress a significant interaction, I exclude them from some models. Figures A1 and A2 show that in these models, the interaction is not only more precisely estimated but also generally much smaller in size than the interaction from models including main effects. It remains statistically indistinguishable from zero in all but one model specification.

30 State of birth is reported in the 1979 survey; just under 20% of the analytic sample lived in a different state at birth than in 1979.

31 Studies that examine directional mobility relative to parents' economic position (rather than overall intergenerational elasticities or correlations) may also help illuminate the inequality-mobility relationship. Different social processes may be at work if the null relationship is due to offsetting directional trends than if neither upward nor downward moves relative to parents' income rank increase with inequality. However, the PSID and NLSY79 data provide little evidence that offsetting directional trends generated the null inequality-mobility relationship. If offsetting directional trends were important, we would expect different inequality-mobility associations for different groups (e.g., different race or class groups). However, I find no evidence that inequality effects differed across demographic subgroups (results not shown, available upon request).

32 I employ information on both parents if available; otherwise, I use whichever parent is observed. Using only mothers' or only fathers' information leaves the results unchanged. Tables A1 and A2 contain fewer observations than Tables 1 and 2 because statistics are shown only for respondents with fully observed covariates. Because conclusions are robust to many covariate specifications, and because missing data are relatively rare, and to reduce the complexity of the model, I exclude models for the missing data and focus on respondents with observed parental information when using background covariates. Early explorations using multiply-imputed datasets generated similar results.

33 For MSAs straddling state borders, I apportion the NSI to each state according to its share of the population. Other segregation measures, including those which capture non-MSA residents, do not change the results.

34 Because of possible lags in policy or behavioral responses to inequality, I measured these hypothesized mediators (spending and segregation) at the same time as inequality but also 1-5 years after. I found the year of measurement did not influence the results. Reported results use mediators measured in the same year as inequality.

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Table (1) Descriptive statistics for parents' and children's log family incomes (2007 dollars) and state family income inequality. PSID, NLSY79 and Census data.

		Northeast	Midwest	West	South	US
PSID						
Adult Income, Child Age 30-34						
Not Adjusted for Family Size						
	Mean	11.14	11.00	11.03	10.82	10.98
	SD	.59	.61	.58	.69	.64
Adjusted for Family Size						
	Mean	10.64	10.46	10.51	10.33	10.47
	SD	.60	.62	.59	.68	.64
Parental Income, Child Age 13-17						
Not Adjusted for Family Size						
	Mean	11.15	11.05	11.14	10.89	11.04
	SD	.51	.50	.47	.59	.53
Adjusted for Family Size						
	Mean	10.39	10.31	10.42	10.16	10.30
	SD	.51	.50	.47	.63	.55
State Family Income Gini, Child Age 13-17						
	Mean	.360	.351	.366	.388	.366
	SD	.022	.018	.021	.017	.024
N individuals		504	817	376	699	2396
N states		7	12	8	16	43
NLSY79						
Adult Income, Child Age 30-49						
Not Adjusted for Family Size						
	Mean	10.80	10.78	10.72	10.62	10.71
	SD	.90	.83	.83	.81	.84
Adjusted for Family Size						
	Mean	10.34	10.30	10.21	10.15	10.23
	SD	.86	.79	.80	.78	.80
Parental Income, Child Age 13-19 in 1979						
Not Adjusted for Family Size						
	Mean	10.69	10.82	10.67	10.45	10.63
	SD	.69	.62	.63	.72	.69
Adjusted for Family Size						
	Mean	9.93	10.06	9.89	9.66	9.86
	SD	.70	.63	.65	.75	.71
State Family Income Gini, Child Age 13-19 in 1979						
	Mean	.360	.346	.367	.379	.365
	SD	.014	.008	.010	.013	.018
N individuals		1092	1481	1083	2244	5913
N states		6	11	8	16	42

Table (2) Family income elasticity-family income Gini models. Gini standardized to mean 0, standard deviation 1. **PSID** and Census data.

	OLS				Random Effects			Fixed Effects	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Income Not Adjusted for Family Size									
Parental Income	.482	.474	.197	.444	.191	.446	.204	.415	.244
	(.022)	(.035)	(.072)	(.023)	(.079)	(.031)	(.084)	(.033)	(.033)
Parental Income*Gini		.052	.041	.042	.042	.051	.030	.029	-.007
		(.028)	(.047)	(.023)	(.052)	(.026)	(.058)	(.032)	(.049)
Gini		-.570	-.445	-.485	-.464	-.583	-.333	-.393	-.042
		(.320)	(.518)	(.251)	(.578)	(.291)	(.645)	(.376)	(.514)
Intercept	5.664	5.748	8.596	6.070	8.584	6.058	8.557	6.284	7.469
	(.246)	(.388)	(.983)	(.258)	(1.013)	(.348)	(1.078)	(.345)	(.851)
State-year Intercept				✓	✓	✓	✓	✓	✓
State-year Slope						✓	✓		
Additional Covariates			✓		✓		✓		✓
AIC	4208.640	4207.059	3559.994	4089.411	4091.043	4217.472	3750.755	4194.975	3608.294
Income Adjusted for Family Size									
Parental Income	.540	.535	.243	.507	.241	.509	.243	.479	.292
	(.021)	(.033)	(.063)	(.022)	(.074)	(.029)	(.077)	(.034)	(.031)
Parental Income*Gini		.037	.021	.029	.024	.032	.020	.015	-.017
		(.031)	(.039)	(.022)	(.049)	(.025)	(.052)	(.034)	(.039)
Gini		-.368	-.194	-.298	-.227	-.333	-.182	-.200	.110
		(.323)	(.392)	(.222)	(.510)	(.263)	(.538)	(.366)	(.348)
Intercept	4.902	4.958	7.959	5.243	7.859	5.228	7.840	5.483	6.984
	(.219)	(.348)	(.943)	(.231)	(.911)	(.308)	(.953)	(.329)	(.974)
State-year Intercept				✓	✓	✓	✓	✓	✓
State-year Slope						✓	✓		
Additional Covariates			✓		✓		✓		✓
AIC	4086.374	4085.556	3394.976	4089.411	4091.043	4092.390	3590.341	4074.602	3442.602
N individuals	2396	2396	2096	2396	2096	2396	2096	2396	2096
N state-years	689	689	621	689	621	689	621	689	621
N states	43	43	41	43	41	43	41	43	41

Note: Standard errors in parentheses (robust, clustered by state and year). All continuous variables except parental and adult income are transformed to mean 0, standard deviation 1. Random effects models fit with restricted maximum likelihood (REML). Because this method is based on error contrasts, AIC can be compared only across REML models with the same fixed components. Additional covariates included in Models 3, 5, 7, and 9 include all state-level variables listed in Appendix Table A1, plus interactions between parental income and these state-level variables, plus all individual-level parental covariates listed in Appendix Table A1.

Table (3) Family income elasticity-family income Gini models. Gini standardized to mean 0, standard deviation 1. **NLSY79** and Census data.

	OLS			Random Effects				Fixed Effects	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Income Not Adjusted for Family Size									
Parental Income	.477	.470	.259	.486	.333	.486	.334	.468	.230
	(.018)	(.017)	(.026)	(.015)	(.049)	(.015)	(.049)	(.018)	(.034)
Parental Income*Gini		.006	-.001	-.025	-.025	-.025	-.025	.003	-.039
		(.015)	(.028)	(.015)	(.050)	(.015)	(.050)	(.018)	(.022)
Gini		-.095	.045	.256	.294	.258	.294		
		(.167)	(.319)	(.163)	(.528)	(.163)	(.528)		
Intercept	5.701	5.777	8.024	5.548	7.323	5.544	7.316	.055	.007
	(.192)	(.181)	(.280)	(.161)	(.522)	(.161)	(.520)	(.006)	(.003)
State Intercept				✓	✓	✓	✓	✓	✓
State Slope						✓	✓		
Additional Covariates			✓		✓		✓		✓
AIC	14549.445	14546.833	12379.984	13677.673	11827.624	13681.649	11831.746	14481.324	12321.911
Income Adjusted for Family Size									
Parental Income	.472	.465	.247	.485	.306	.479	.306	.462	.220
	(.016)	(.013)	(.023)	(.014)	(.045)	(.018)	(.045)	(.014)	(.030)
Parental Income*Gini		.009	.046	-.020	.027	-.021	.027	.002	.002
		(.011)	(.021)	(.014)	(.045)	(.017)	(.045)	(.013)	(.015)
Gini		-.112	-.433	.197	-.242	.200	-.245		
		(.114)	(.227)	(.140)	(.445)	(.171)	(.444)		
Intercept	5.622	5.688	7.814	5.458	7.299	5.514	7.292	.041	.004
	(.164)	(.125)	(.235)	(.139)	(.452)	(.179)	(.450)	(.004)	(.002)
State Intercept				✓	✓	✓	✓	✓	✓
State Slope						✓	✓		
Additional Covariates			✓		✓		✓		✓
AIC	13760.132	13757.161	11704.017	13012.892	11293.597	13012.403	11297.779	13669.153	11650.342
N individuals	5913	5913	5249	5913	5249	5913	5249	5913	5249
N states	42	42	41	42	41	42	41	42	41

Note: Standard errors in parentheses (robust, clustered by state). All continuous variables except parental and adult income are transformed to mean 0, standard deviation 1. Random effects models fit with restricted maximum likelihood (REML). Because this method is based on error contrasts, AIC can be compared only across REML models with the same fixed components. Additional covariates included in Models 3, 5, and 7 include all state-level variables listed in Appendix Table A2, plus interactions between parental income and these state-level variables, plus all individual-level parental covariates listed in Appendix Table A2. Additional covariates included in Model 9 include all interactions between parental income and state-level variables, plus individual-level parental covariates. However, Model 9 cannot estimate main effects for the state-level variables due to the state fixed effects (unlike Table 3 Model 9, which uses PSID data and thus includes variation within states over time).

Table (A1) Descriptive statistics for state and parental characteristics, states/years in which PSID respondents resided at age 13-17. **PSID**, Census, and Statistical Abstracts data.

	Northeast	Midwest	West	South	US
Parental Characteristics					
Age (years)					
Mean	43.47	41.90	41.90	40.97	41.96
SD	5.75	6.01	5.94	6.27	6.08
Education (years)					
Mean	12.41	12.29	12.60	11.46	12.13
SD	2.11	2.10	2.28	2.83	2.40
Race (%)					
White	94.80	93.98	87.65	77.80	88.45
Black	4.30	4.76	2.71	19.90	8.73
Other	.90	1.26	9.64	2.30	2.81
Marital Status (%)					
Stably Married	71.72	66.39	59.94	65.62	66.27
Both Stably Single	.45	.14	.90	.66	.48
Both Unstably Married	16.74	16.11	21.99	17.27	17.51
Other	11.09	17.37	17.17	16.45	15.74
(e.g., one single, one unstably married)					
Child Male (%)	47.06	51.82	47.59	48.19	49.45
Child's year of birth (mean)	1962.25	1963.42	1963.55	1964.07	1963.55
State Characteristics					
Median Family Income					
Mean	52330.62	50239.56	50451.35	42202.84	48382.80
SD	4981.89	4117.29	2639.93	5997.13	6224.19
Percent Black	9.76	8.28	4.68	19.05	11.14
Percent Hispanic	4.92	1.59	11.89	3.99	4.62
Percent Poor	10.54	10.97	11.28	17.15	12.72
Region (%)					
Northeast	100	0	0	0	21.09
Midwest	0	100		0	34.06
West	0	0	100	0	15.84
South	0	0	0	100	29.01
Economic Segregation (State NSI)					
Mean	.46	.44	.43	.43	.44
SD	.07	.06	.07	.05	.06
Education spending per child age 5-17					
Mean	5387.11	5578.74	6495.15	5023.64	5522.46
SD	1882.43	1773.12	1704.45	1575.84	1794.85
Health spending per capita					
Mean	312.74	274.38	266.86	280.26	282.98
SD	154.50	88.09	86.56	117.68	114.53
Welfare spending per capita					
Mean	605.05	428.35	504.56	292.54	438.29
SD	211.26	176.59	191.86	92.23	202.61
N individuals	442	714	332	608	2096
N states	6	12	7	16	41

Table (A2) Descriptive statistics for state and parental characteristics, states in which NLSY79 respondents resided in 1979. **NLSY79**, Census, and Statistical Abstracts data.

	Northeast	Midwest	West	South	US
Parental Characteristics					
Age (years)					
Mean	45.25	44.84	44.26	44.36	44.63
SD	6.83	6.84	7.13	7.40	7.12
Education (years)					
Mean	11.12	11.57	9.99	10.33	10.73
SD	3.11	2.60	3.97	3.10	3.22
Race (%)					
White	49.58	55.48	32.74	30.65	40.92
Black	24.37	22.37	10.15	47.65	30.02
Hispanic	18.78	4.61	46.34	13.91	18.27
Other	7.28	17.54	10.77	7.79	10.78
Marital Status (%)					
Parent Married, Child Age 14	70.68	81.36	77.93	72.43	75.44
Child Male (%)	51.90	51.68	50.21	47.75	49.91
Child's year of birth (mean)	1962.47	1962.53	1962.65	1962.56	1962.38
State Characteristics					
Median Family Income					
Mean	52178.49	52303.31	49833.70	43338.13	48454.30
SD	3844.38	3253.53	2959.12	3753.23	5375.51
Percent Black	10.91	9.38	5.68	20.19	13.05
Percent Hispanic	5.80	1.84	16.61	6.11	6.85
Percent Poor	10.85	10.27	11.63	15.48	12.59
Region (%)					
Northeast	100	0	0	0	18.06
Midwest	0	100		0	26.06
West	0	0	100	0	18.21
South	0	0	0	100	37.66
Economic Segregation (State NSI)					
Mean	.51	.47	.47	.44	.47
SD	.04	.05	.05	.05	.05
Education spending per child age 5-17					
Mean	5999.69	5944.02	6970.11	5173.41	5850.71
SD	997.33	700.13	334.96	547.76	920.82
Health spending per capita					
Mean	322.96	322.98	321.83	351.40	333.47
SD	111.38	47.30	40.60	82.59	76.71
Welfare spending per capita					
Mean	655.96	495.23	556.50	303.46	463.19
SD	146.05	133.94	193.44	151.88	205.41
N individuals	948	1368	956	1977	5249
N states	6	11	8	16	41

Table (A3) Family income elasticity trends: comparing coefficients and standard errors with previous research. SRC subsample, sons only. PSID data.

	Lee and Solon		Current Analysis	
Cohort	β	se	β	se
1954	0.50	0.15	0.492	0.047
1955	0.48	0.13	0.469	0.047
1956	0.42	0.14	0.480	0.047
1957	0.52	0.12	0.463	0.048
1958	0.46	0.11	0.479	0.047
1959	0.39	0.11	0.454	0.047
1960	0.41	0.12	0.422	0.047
1961	0.47	0.10	0.469	0.047
1962	0.41	0.12	0.457	0.047
1963	0.38	0.09	0.422	0.047
1964	0.42	0.09	0.448	0.047
1965	0.36	0.08	0.431	0.048
1966	0.43	0.08	0.469	0.049
1967	0.45	0.08	0.496	0.046
1968	0.49	0.08	0.467	0.049
1969	0.43	0.07	0.513	0.047
1970	0.40	0.07	0.466	0.048
1971	0.43	0.07	0.454	0.047
1972			0.476	0.047
1973	0.47	0.06	0.431	0.046
1974			0.475	0.047
1975	0.47	0.06		

Note: "Current analysis" estimates from random coefficient models (partially pooled estimates). "Lee and Solon" estimates from Table 1 of their paper (2009), OLS models. In Table 1 they report by year rather than cohort; cohorts reported here are year minus 25. Several differences in the analyses generate some divergence in the results, including the ages studied and the empirical models. See papers for greater detail.

Table (A4) Family income elasticity-Gini models. Gini coefficient measured in early childhood, and standardized to mean 0, standard deviation 1. **PSID** and Census data.

	OLS		Random Effects		Fixed Effects
	Model 1	Model 2	Model 3	Model 4	Model 5
Income Not Adjusted for Family Size					
Parental Income	.481	.456	.434	.432	.412
	(.022)	(.034)	(.023)	(.030)	(.034)
Parental Income*Gini		.057	.051	.055	.044
		(.029)	(.022)	(.026)	(.030)
Gini		−.662	−.603	−.651	−.531
		(.328)	(.240)	(.282)	(.344)
Intercept	5.671	5.957	6.192	6.219	6.370
	(.246)	(.381)	(.260)	(.322)	(.367)
State-year Intercept			✓	✓	✓
State-year Slope				✓	
AIC	4201.478	4188.380	4198.721	4202.876	4185.268
Income Adjusted for Family Size					
Parental Income	.539	.521	.497	.496	.475
	(.021)	(.032)	(.023)	(.029)	(.033)
Parental Income*Gini		.040	.035	.039	.032
		(.030)	(.021)	(.025)	(.033)
Gini		−.432	−.392	−.431	−.346
		(.318)	(.209)	(.256)	(.347)
Intercept	4.913	5.107	5.354	5.366	5.541
	(.219)	(.338)	(.235)	(.303)	(.329)
State-year Intercept			✓	✓	✓
State-year Slope				✓	
AIC	4077.574	4073.751	4076.984	4080.124	4063.679
N individuals	2393	2393	2393	2393	2393
N state-years	686	686	686	686	686
N states	42	42	42	42	42

Note: Standard errors in parentheses (robust, clustered by state and year). Gini coefficient captures inequality in the year a PSID respondent was about 4 years old in the state in which he resided around age 14. Random effects models fit with restricted maximum likelihood (REML). Because this method is based on error contrasts, AIC can be compared only across REML models with the same fixed components.

Table (A5) Family income elasticity-Gini models. Gini measured in state and year of birth, and standardized to mean 0, standard deviation 1. **NLSY79** and Census data.

	OLS		Random Effects				Fixed Effects	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Income Not Adjusted for Family Size								
Parental Income	.473	.456	.482	.483	.474	.484	.458	.456
	(.018)	(.020)	(.016)	(.016)	(.025)	(.016)	(.022)	(.016)
Parental Income*Gini		−.009	−.039	−.040	−.034	−.040	−.016	−.006
		(.018)	(.015)	(.015)	(.023)	(.015)	(.005)	(.010)
Gini		.053	.379	.390	.322	.393		−.081
		(.188)	(.156)	(.156)	(.244)	(.158)		(.151)
Intercept	5.746	5.917	5.586	5.568	5.664	5.564	.055	6.124
	(.198)	(.221)	(.170)	(.171)	(.269)	(.174)	(.006)	(.138)
State Intercept			✓	✓	✓	✓	✓	✓
Year Intercept				✓		✓		✓
State Slope					✓	✓		
Year Slope						✓		
AIC	13082.038	13064.958	12374.229	12374.229	12361.135	12361.135	12989.589	12989.589
Income Adjusted for Family Size								
Parental Income	.469	.454	.483	.486	.485	.486	.455	.457
	(.017)	(.014)	(.015)	(.015)	(.017)	(.021)	(.016)	(.010)
Parental Income*Gini		−.008	−.039	−.040	−.040	−.038	−.021	−.005
		(.012)	(.014)	(.014)	(.015)	(.018)	(.004)	(.004)
Gini		.038	.357	.373	.369	.353		−.113
		(.119)	(.134)	(.134)	(.149)	(.179)		(.073)
Intercept	5.652	5.798	5.467	5.435	5.451	5.439	.041	5.985
	(.170)	(.145)	(.147)	(.149)	(.165)	(.215)	(.004)	(.130)
State Intercept			✓	✓	✓	✓	✓	✓
Year Intercept				✓		✓		✓
State Slope					✓	✓		
Year Slope						✓		
AIC	12347.558	12330.463	11746.236	11746.236	11745.827	11745.827	12238.002	12238.002
N individuals	5354	5354	5354	5354	5354	5354	5354	5354
N state-years	264	264	264	264	264	264	264	264
N states	49	49	49	49	49	49	49	49

Note: Standard errors in parentheses (robust, clustered by state in models 2, 3, 5, and 7; clustered by state and year in models 4, 6, and 8). Gini measured in respondent's state and year of birth. Respondents born 1960-65, allowing random and fixed effects by both state and year (unlike the main NLSY79 analysis, which permits random and fixed effects by state only because the Gini is measured in the same year for all respondents, 1979). Random effects models fit with restricted maximum likelihood (REML). Because this method is based on error contrasts, AIC can be compared only across REML models with the same fixed components.

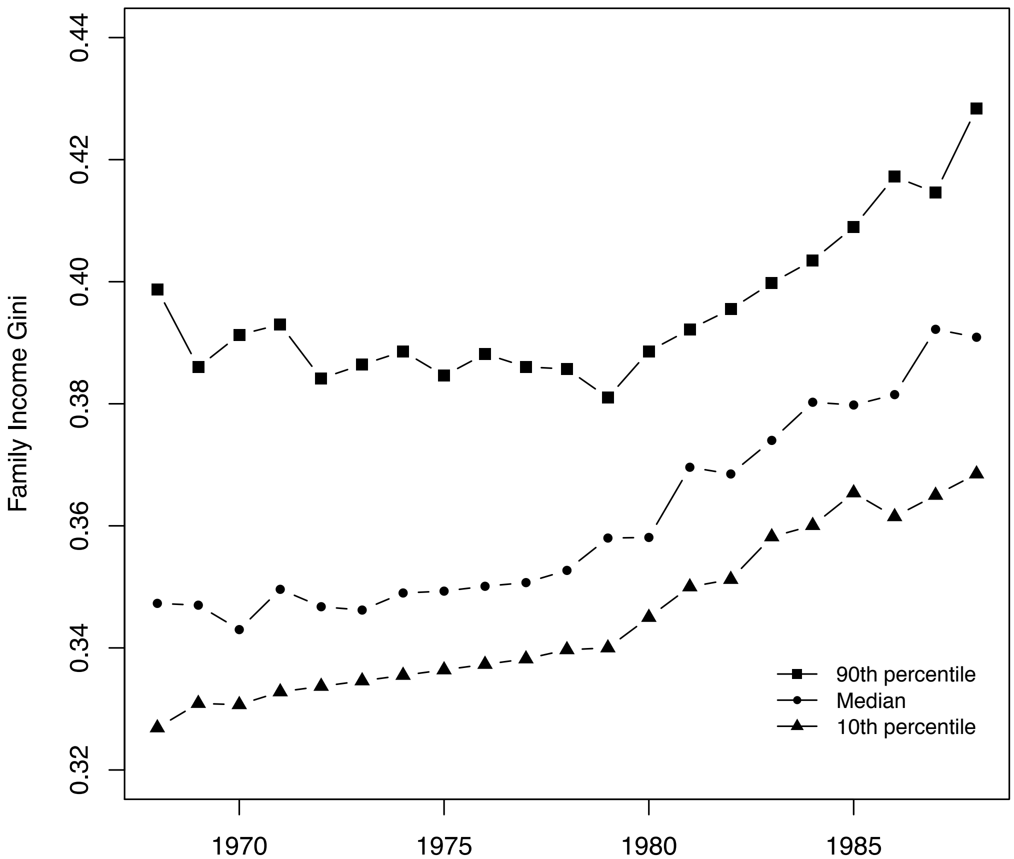

Figure (1) 90^{th}, 50^{th}, and 10^{th} percentiles of the distributions of family income Gini coefficients across states. State-years included are those in which PSID respondents (SRC birth cohorts 1954-1974) are observed in their teen years. Census data.

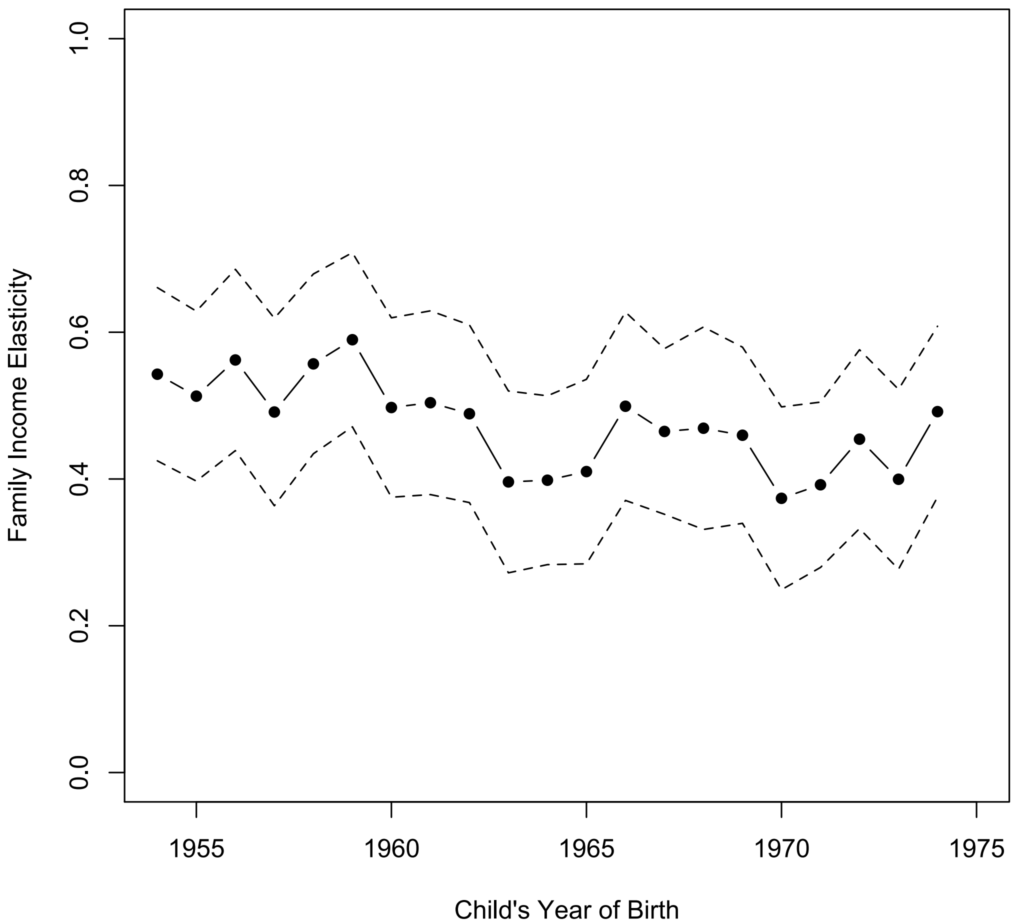

Figure (2) Family income mobility by cohort, random coefficient estimates. Posterior medians of cohort slopes with point-wise 95% confidence intervals. SRC birth cohorts 1954-1974, PSID data.

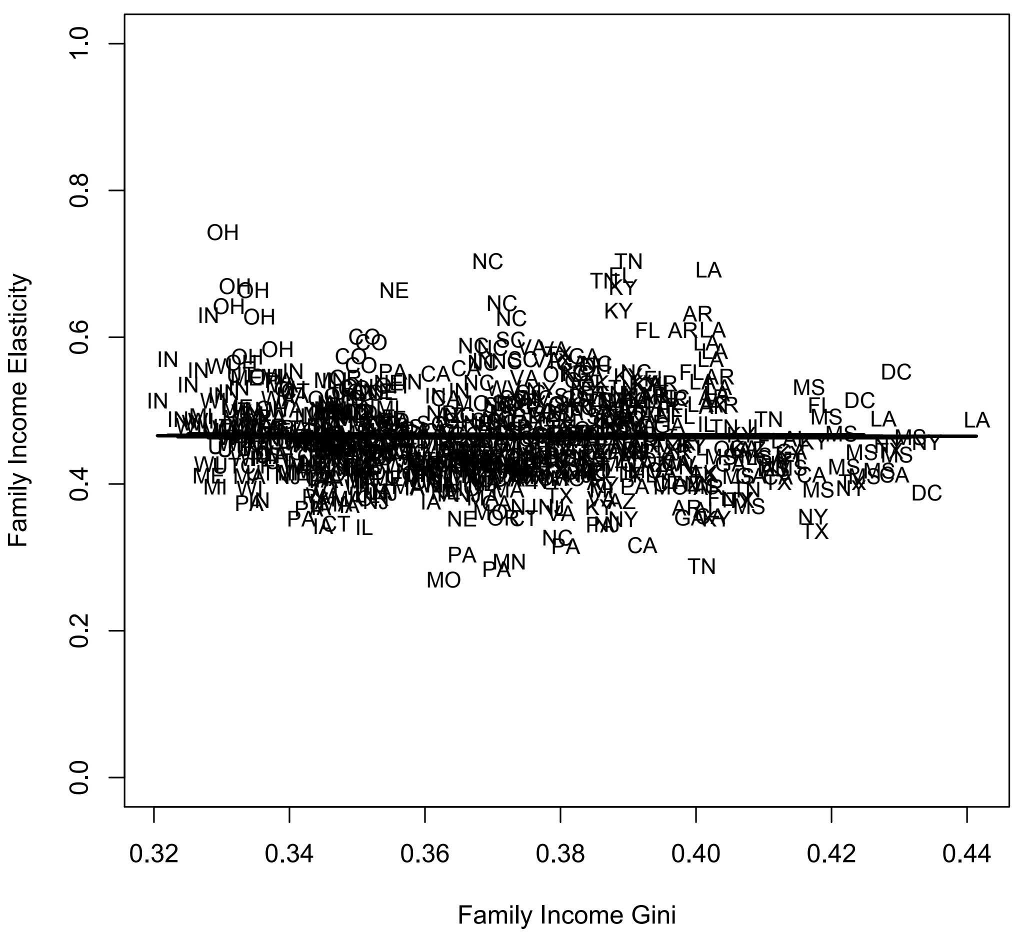

Figure (3) Family income mobility and inequality by U.S. state and year, random coefficient estimates. Posterior medians of state-year slopes versus state-year inequality. SRC birth cohorts 1954-1974, PSID and Census data.

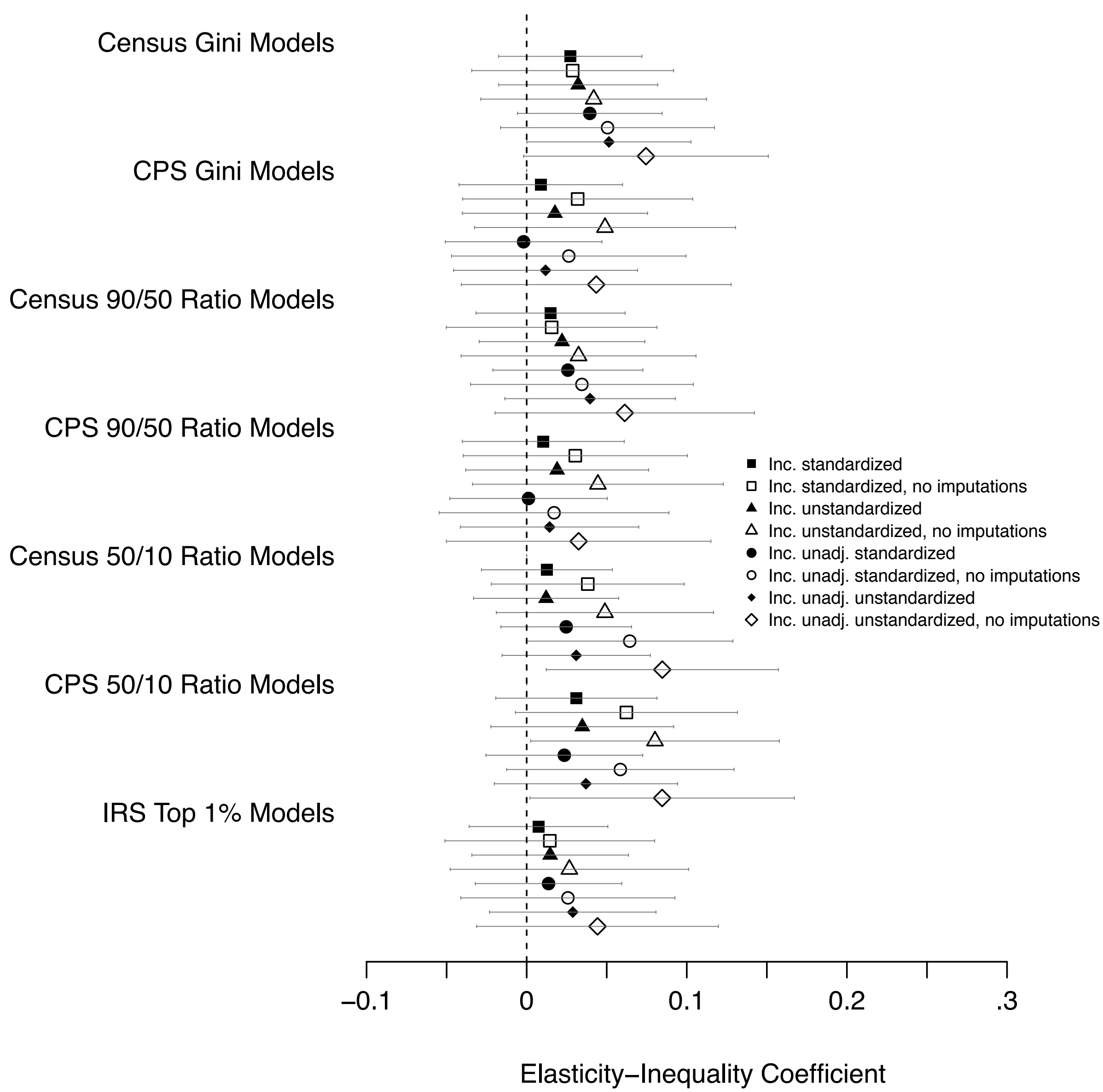

Figure (4) Measurement robustness checks for mobility-inequality relationship. **PSID**, Census, CPS, and IRS data. Estimates (with 95% confidence intervals) of elasticity-inequality coefficient from random coefficient models. Models explore (a) different inequality measures, (b) different sources of inequality data, (c) different income codings (un/standardized, un/adjusted for family size), and (d) different subsamples (including/excluding imputed income). All inequality measures standardized (mean 0, sd 1).

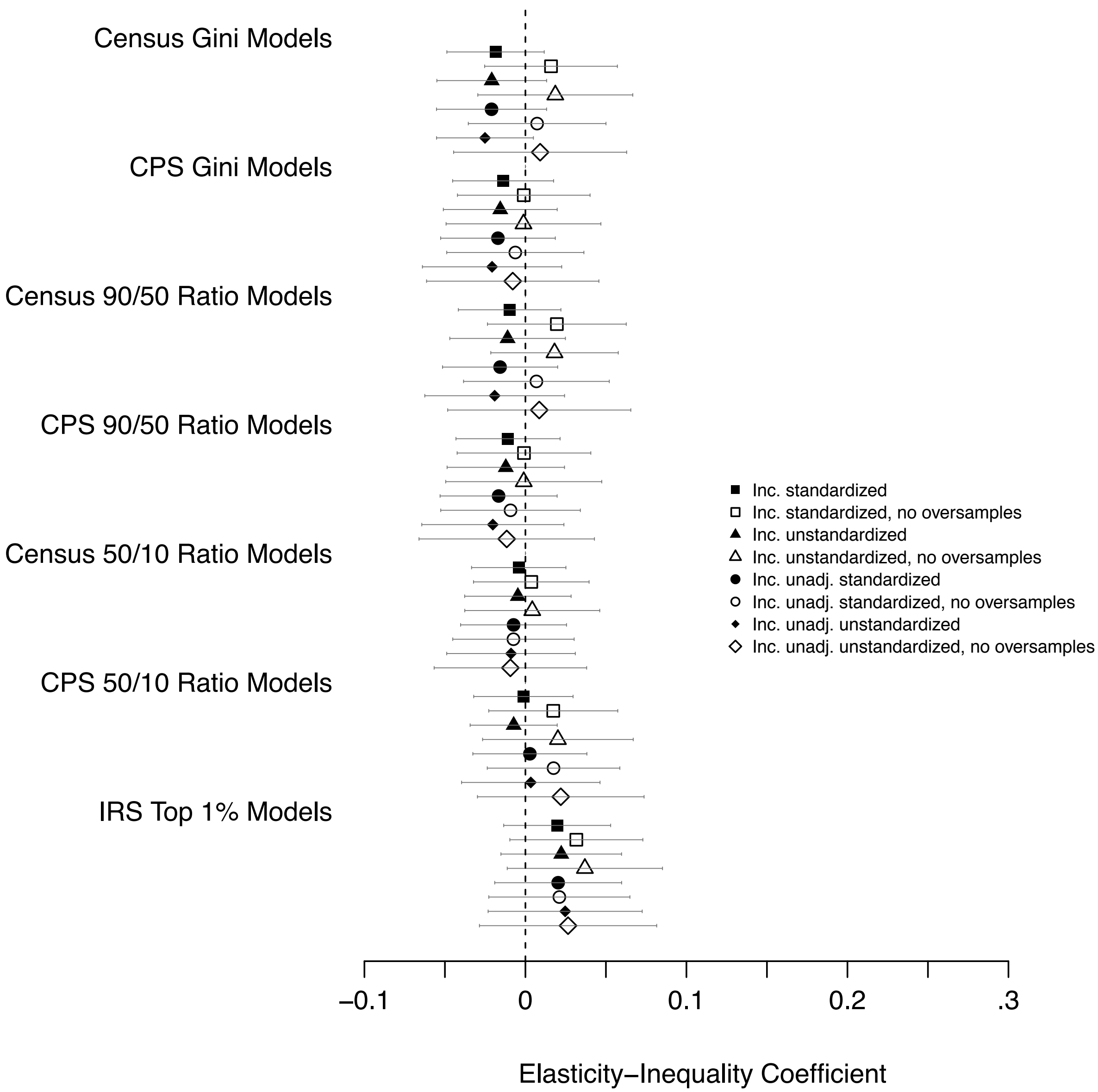

Figure (5) Measurement robustness checks for mobility-inequality relationship. **NLSY79**, Census, CPS, and IRS data. Estimates (with 95% confidence intervals) of elasticity-inequality coefficient from random coefficient models. Models explore (a) different inequality measures, (b) different sources of inequality data, (c) different income codings (un/standardized, un/adjusted for family size), and (d) different subsamples (including/excluding minority oversamples). All inequality measures standardized (mean 0, sd 1).

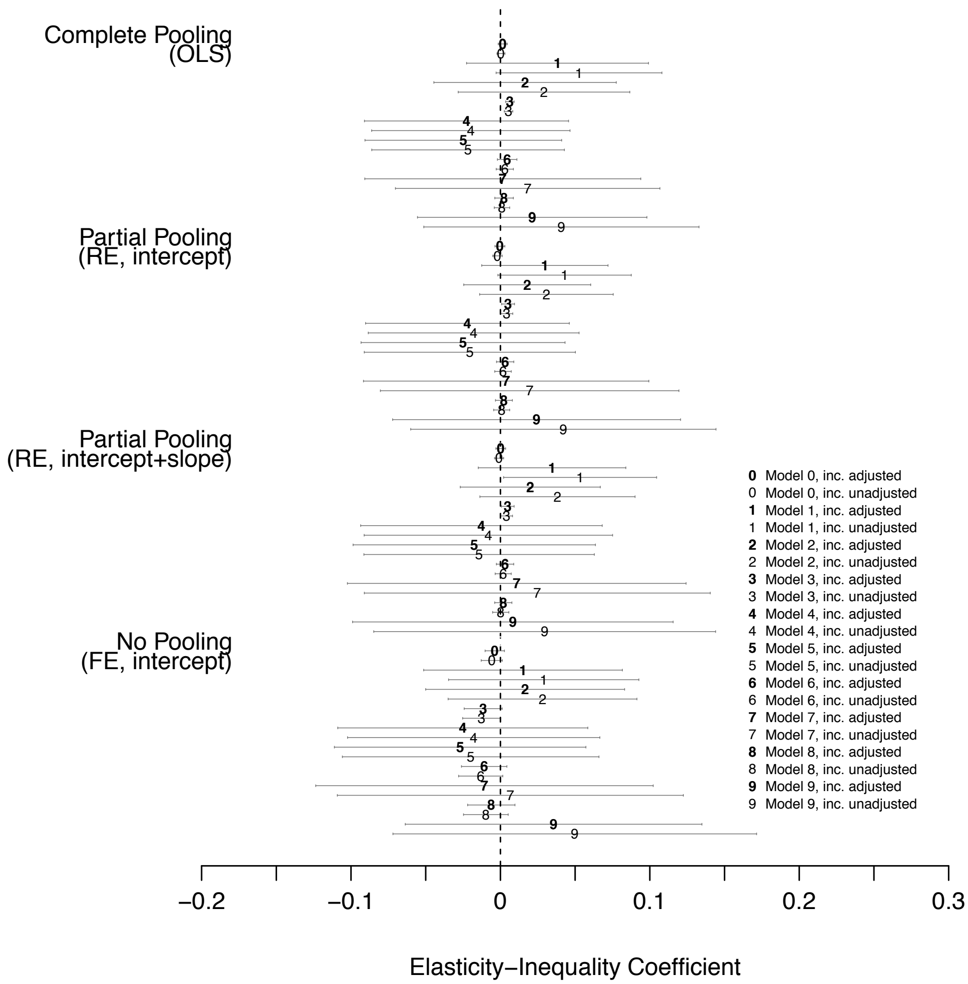

Figure (A1) Model robustness checks for mobility-inequality relationship. **PSID**, Census, and Statistical Abstracts data. Estimates (with 95% confidence intervals) of elasticity-inequality coefficient. Inequality measures standardized (mean 0, sd 1). Models use different income measures (income un/adjusted for family size) to explore (a) different covariate vectors and (b) different error structures (OLS "complete pooling," random effects "partial pooling," and fixed effects "no pooling" of between state/year information). Covariate vectors are as follow:

	Model 0	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Parental Income Main Effect	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Gini*Parental Income	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Gini Main Effect		✓	✓	-	✓	✓	-	✓	-	✓
State-Level Covariates' Main Effects			✓	-	✓	✓	-	✓	-	✓
State-Level Covariates*Parental Income				✓	✓	✓	✓	✓	✓	✓
State-Level Mediators' Main Effects						✓	-	✓	-	✓
State-Level Mediators*Parental Income							✓	✓	✓	✓
Individual-Level Covariates' Main Effects									✓	✓

Note: Within each pair of models (3, 4), (6, 7), and (8, 9) the same predictors are used, except for the state-level main effects, which are included in only the second model of each pair. State-level covariates: % black, % hispanic, median income, % poor, region. State-level mediators: education spending, health spending, welfare spending, residential segregation. Individual-level covariates: parental age, parental education, parental marital status, race, and child's sex.

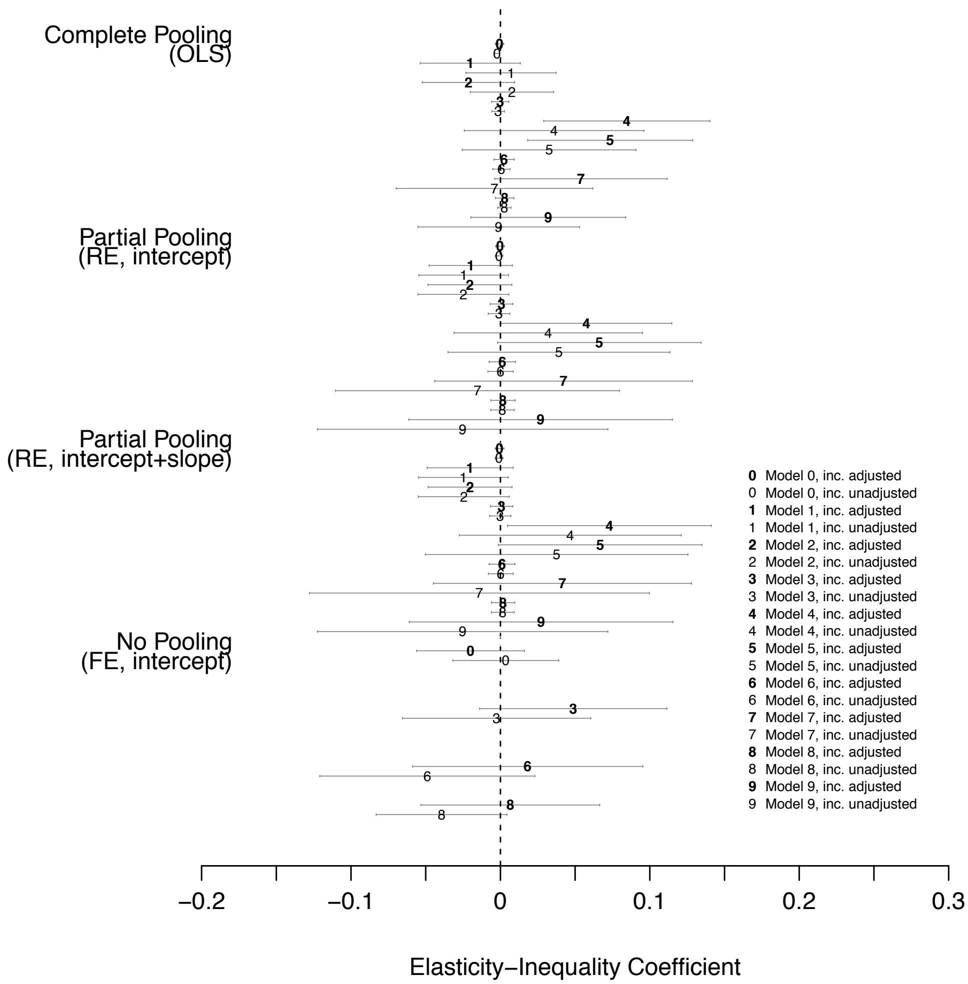

Figure (A2) Model robustness checks for mobility-inequality relationship. **NLSY79**, Census, and Statistical Abstracts data. Estimates (with 95% confidence intervals) of elasticity-inequality coefficient. Inequality measures standardized (mean 0, sd 1). Models use different income measures (income un/adjusted for family size) to explore (a) different covariate vectors and (b) different error structures (OLS "complete pooling," random effects "partial pooling," and fixed effects "no pooling" of between state information). Covariate vectors are as follow:

	Model 0	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Parental Income Main Effect	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Gini*Parental Income	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Gini Main Effect		✓	✓	-	✓	✓	-	✓	-	✓
State-Level Covariates' Main Effects			✓	-	✓	✓	-	✓	-	✓
State-Level Covariates*Parental Income				✓	✓	✓	✓	✓	✓	✓
State-Level Mediators' Main Effects						✓	-	✓	-	✓
State-Level Mediators*Parental Income							✓	✓	✓	✓
Individual-Level Covariates' Main Effects									✓	✓

Note: Within each pair of models (3, 4), (6, 7), and (8, 9) the same predictors are used, except for the state-level main effects, which are included in only the second model of each pair. Some covariate vector/error structure combinations not shown, because in the NLSY79 fixed effects models (which use variation only within states) some models with state-level main effects cannot be estimated. State-level covariates: % black, % hispanic, median income, % poor, region. State-level mediators: education spending, health spending, welfare spending, residential segregation. Individual-level covariates: parental age, parental education, parental marital status, race, and child's sex.