Journals of Gerontology: Social Sciences
cite as: J Gerontol B Psychol Sci Soc Sci, 2022, Vol. 77, No. S2, S199–S208
https://doi.org/10.1093/geronb/gbab240
Advance Access publication February 1, 2022

Supplement Article: Why Does Health in the US Continue to Lag Behind?

Early Exposure to County Income Mobility and Adult Individual Health in the United States

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Received: June 10, 2021; Editorial Decision Date: December 18, 2021

Decision Editor: Mikko Myrskylä, PhD

Abstract

Objectives: Previous research in the United States suggests contextual income mobility may play a role in explaining the disparities between life expectancy in the United States and peer countries. This article aims to extend previous research by estimating the consequences of average individual exposure to mobility regimes during childhood and adolescence on adult health.

Methods: This study draws its data from two longitudinal datasets that track the county of residence of respondents during childhood and adolescence, the Panel Study of Income Dynamics and the National Longitudinal Survey of Youth 1997. We implement marginal structural models to assess the association of the average exposure to county income mobility on five health outcomes and behaviors.

Results: The results are only partially consistent with a systematic association between exposure to income mobility and health outcomes. Evidence obtained from the National Longitudinal Survey of Youth suggests less income mobility might increase the probability of smoking by age 30.

Discussion: The paper provides a precise assessment of the hypothesis that childhood exposure to income mobility regimes may influence health status through behavior later in life and contribute to longevity gaps. Only partial evidence on smoking suggests an association between income mobility and health, so we discuss potential reasons for the disparities in results with previous research.

Keywords: Health disparities, Income mobility, NLSY, PSID

In the last 20 years, two sets of apparently disconnected traits were characteristic of the U.S. mortality and health profile. The first is a persistent and growing within-country health and mortality disparities by geography, race, education, and income (Chetty et al., 2014). The second is an equally persistent and perhaps also growing gap between the U.S. health and mortality levels and those experienced in peer countries (National Academy of Sciences, 2015). Although, for the most part, these two regularities have been considered in separate literature, there are important areas of overlap. Just as disparities in income and income inequality levels are considered central determinants of U.S. *intercounty* mortality differentials (Ezzati et al., 2008), so are intercountry differentials in national income and income inequality identified as candidate explanatory factors accounting for life expectancy differences across countries. Smoking behavior, for instance, is a proximate determinant of between-group contrasts in chronic illness within single populations and so could intercountry disparities in smoking prevalence be responsible for differences in chronic illnesses and mortality risks across countries.

Although income inequality has been considered an important distal determinant of within and between population mortality disparities, levels of income and social mobility have been largely ignored. And yet, although correlated (Krueger, 2012), income and social mobility and income inequality can act independently and through separate pathways to affect individual health and mortality risks. In addition, previous research suggests that the United States fares poorly relative to peer countries when using standard measures of social and economic mobility and that its relative mobility ranking is similar to its life expectancy ranking (Corak, 2016; Manduca et al., 2020). The above considerations justify the inclusion of social and economic mobility as a plausible distal mortality determinant both within and among populations.

Our study extends previous research by estimating the effect of average exposure during childhood and adolescence on health outcomes and behaviors measured during young adulthood (early 30s and 40s) using longitudinal data. We employ both the National Longitudinal Survey of Youth 1997 (NLSY97) and Panel Study of Income Dynamics (PSID) with geocode data to assess the link between county-level income mobility and health outcomes and behaviors such as self-reported health, body mass index (BMI), depression, and smoking. Also, we use data that match better the cohorts used by Chetty to estimate income mobility in the United States at the county level (i.e., children born between 1980 and 1982), account for selection associated with residential mobility over time, and adjust for time-varying confounders using marginal structural models. Thus, we provide a more precise assessment of the hypothesis that exposure to income mobility may determine health later in life and explain the longevity gap. Although this article is about pathways through which local mobility regimes can influence local health and mortality disparities in the United States, these have counterparts at the population levels and should also be considered in explanations of intercountry disparities.

International Disparities in Social and Economic Mobility

How plausible is social and economic mobility as a distal mortality determinant both between and within populations? The first condition of plausibility is that disparities in social and economic mobility regimes across countries ought to be as large as they seem to be within the United States. A body of recent empirical research highlights that intergenerational income and educational mobility in the United States have declined continuously since 1940 (Chetty et al., 2018). Furthermore, there is robust evidence that the United States compares poorly to peer countries on measures of intergenerational income and educational mobility. The probability of moving from the bottom to the top quintile in the earnings distribution is 0.075 in the United States versus 0.110 in Sweden (Alesina et al., 2018) and, furthermore, the United States is one of four high-income countries out of 50 that has the lowest rates of upward income mobility. In a recent report (World Economic Forum, 2020), the United States places 20th in a ranking of relative intergenerational *educational mobility*, a ranking remarkably similar to that of life expectancy in the same pool of countries (22nd).

This empirical evidence suggests that the United States does not compare well relative to peer countries when using standard measures of social and economic mobility and that its relative ranking is similar to its ranking in life expectancy. What needs to be established is whether or not this regularity is a result of causal links, rather than the product data artifacts. It would, of course, be highly desirable to simultaneously investigate within- and between-country mobility as determinants of within- and between-country disparities in life expectancy. However, we are threading in a new research area and the data required for hypotheses testing available to us are limited. Thus, in what follows, we discuss pathways linking local mobility regimes population setting (counties) in one country (the United States) and health behaviors responsible for individual health and mortality differentials within U.S. counties. Some of these pathways have counterparts at the level of entire populations and could be useful as a reference for the study of cross-country mortality differentials. Similarly, we single out identification problems standing in the way of inferences about within-country relations that also apply to the study of differentials across countries.

U.S. Disparities in Social and Income Mobility

Life expectancy gaps by income in the United States are very large. Recent work by Chetty et al. (2016) shows that the difference in life expectancy (at age 40) between the richest 1% and poorest 1% in the U.S. counties is 14.6 years for men and 10.1 years for women. On average, this is about twice the difference in life expectancy at birth between the United States (78.5) and Bangladesh (72.5).

Previous evidence also demonstrates that neither access to medical care nor socioeconomic factors fully explain observed geographic or income disparities in longevity. The search for drivers of the longevity gap has led scholars to suggest that contextual income mobility—defined as the ability of individuals to exceed their parents' income—may play an essential role in explaining health disparities (Daza & Palloni, 2018; Gaydosh et al., 2017; Venkataramani et al., 2020; Zang & Kim, 2021). For instance, low-income mobility may harm health by raising despair and diminishing the motivation to engage in healthy behaviors. These effects would be distinct to the consequences of income inequality for health. Individuals living in areas characterized by similar high degrees of income inequality may experience different probabilities of income mobility—and therefore may have different impacts on health outcomes.

While the association between income inequality and health has been studied over the last 20 years, recent work states that its contribution to disparities in longevity may be small (Chetty et al., 2016). In contrast, the health consequences of economic mobility remain understudied. This gap in the literature is particularly salient given the emerging evidence of falling income mobility in the United States, especially among the same birth cohorts currently experiencing divergence in their life expectancy.

Recent studies on the link between income mobility and health use mostly aggregate data and individual cross-sectional surveys. We identify at least three main limitations of this research. First, associations observed at the aggregate (e.g., county-level data) might not be kept at the individual level when most of the mechanisms proposed in the literature consist of individual processes (i.e., ecology fallacy), not aggregate ones. Second, as the neighborhood effects literature has pointed out (Sampson et al., 2002), residential mobility might produce spurious associations between contextual variables and individual outcomes. Lastly, previous research has not defined clearly when exposure to a place's income mobility during the life course would have significant consequences for health.

Individual Mechanisms

The relationships of interest in this article are those between a *place's income mobility* (i.e., an aggregate property of the stratification system) and health outcomes (i.e., an individual trait), not the relationship between individuals' lifetime income or socioeconomic status (SES) mobility and individuals' adult health. The latter is relevant to us only if individuals' experiences of SES mobility are themselves influenced by the prevailing aggregate regime of income mobility.

We argue that a link between places' income mobility and mortality could exist if communities with higher income mobility host social and economic environments that reduce mortality risks relative to communities with lower income mobility, *independently of the income level and income inequality*. Individuals who occupy the most vulnerable social positions within unequal communities may be comparatively better off when facing advantageous income mobility prospects than when they do not.

Three pathways can be identified on the link between income mobility and health. First, the association may be the outcome of a composition effect. Namely, places with higher income mobility have a population composition biased toward individuals who experience socioeconomic mobility. In this case, the association between a stratification trait and individual experiences of health and mortality reflects the influence of individual residential mobility patterns and selection.

Second, exposure to income mobility during childhood and adolescence may be a fundamental pathway. There is growing evidence that individuals' early conditions and upbringing matter greatly for adult health and mortality disparities (Palloni et al., 2009). Thus, some of the health differentials between men in low- and high-ranking positions initially attributable to chronic stress among those in subordinate positions (Marmot, 2004) may be rooted in antecedent health conditions sculpted early in life (Case & Paxson, 2011).

Lastly, communities with low-income mobility may distort opportunities and incentives, reinforce unequal allocation of favorable traits, undervalue public institutions that contribute to the formation of skills with a high wage premium, and many of them support nonmeritocratic reward allocation strategies. These community properties directly influence the suite of opportunities available to individuals and regulate how parental socialization favors (discourages) the adoption of positive outlooks and the value of skill acquisition (Browman, Destin, et al., 2019; Browman, Svoboda, et al., 2019). Rigid or weak income mobility fosters individual hopelessness, despair, mistrust, disbelief in a level playing field for all, weakens aspirations, and, more generally, diminishes the value of adopting attitudes and behaviors that promote good health. This is in line with the hypothesis proposed by Case and Deaton (2020), who reported the fastest-rising death rates of causes such as suicides, drug overdoses, and alcoholic liver disease in the United States, especially among those without a bachelor's degree (i.e., *deaths of despair*). In what follows we extend previous research and carry out a more precise assessment of the link between contextual income mobility and health. In particular, we assess whether exposure to a given mobility regime during childhood and adolescence affects health indicators later in life, after adjusting for residential mobility selection and time-variant confounders.

Data

Our data result from combining different sources. The first is the Health Inequality Project Data (HIPD) created by Chetty et al. (2016).[1] The HIPD includes statistics of income distribution and two indicators of income mobility derived from measures of the association between income of children born between 1980 and 1982 and their parents' income. First, we use the index of relative mobility (*rank–rank slope*) at the county level that is the slope of a regression model between children's national income rank—within a birth cohort—and their parents' national income rank. For the relative income mobility indicator, larger values correspond to lower income mobility (i.e., higher rank–rank slope between parents' and child's income). We also use an absolute upward mobility score or "the mean rank (in the national income distribution) of children whose parents are at the 25th percentile of the national parent income distribution" (Chetty et al., 2014, p. 7). Absolute upward income mobility ranges from 0 to 1, and higher values correspond to larger income mobility. To facilitate interpretation, we multiply the upward mobility score by −1 so that the meaning and expected association of relative and absolute income mobility with health are the same. Finally, we use the Gini coefficient as an indicator of income inequality.

The second database is the NLSY97, a nationally representative sample of 8,984 American youth born between 1980 and 1984. The NLSY97 sample matches the cohorts of the core sample used by Chetty et al. (2014; 1980–1982), so we can align the timing of early exposure with the place's income mobility. We use the restricted NLSY97 geocoded data file with information on the geographic residence of each respondent since age 12, to merge Chetty's county-level income mobility measures. After merging the two databases, we kept 8,810 NLSY97 respondents. The total number of counties matched was 1,607.

The third database is the PSID, a nationally representative sample of U.S. men, women, children, and their families followed for more than 40 years. Similarly to the NLSY, restricted geographic data allow us to merge individual records with county income mobility measures. Unlike the NLSY97, the PSID data permit us to estimate the effect of exposure to contextual mobility from birth to age 20. However, we lose statistical power because the number of respondents who match the Chetty et al. (2014)'s cohort is smaller. For instance, between 1975 and 1985, the PSID panel had 4,771 newborns. Of these, 2,358 were the *reference person* or *spouse/partner* of the household at any time during their participation in the panel. Although that cohort does not match exactly the cohort used by Chetty et al. (2014), it offers a reasonable approximation to the mobility regime exposure of that generation, provided income mobility does not change dramatically before 1980–1982. After merging PSID and HIPD databases, we obtained 2,273 respondents. The total of counties matched was 1,120.

Analytical Strategy

This article aims to estimate the effect of average exposure to county income mobility during childhood and adolescence on health outcomes such as smoking, BMI, self-reported health, and mental health during young adulthood. The key independent variable is the average income mobility exposure between ages 12 and 20 in the case of the NLSY97 and ages 1 to 20 for the PSID. Outcomes, in contrast, were measured during the last NLSY97 and PSID waves when respondents were in their 30s or 40s. As a benchmark, we used both relative and absolute income mobility and estimated the effect of average county income inequality exposure (i.e., Gini coefficient) to compare the magnitude and direction of the associations. We used residualized income mobility and inequality scores from a county-level regression model that adjusted for characteristics such as population size, proportion of African Americans, average household income, and income inequality (or income mobility). For completeness, we also show the results with nonresidualized exposure treatments in Supplementary Material. Thus, we adopt a conservative strategy when estimating the consequences of county's income mobility on health outcomes. County's income mobility estimates, for instance, are associated with the county's median income. It might be misleading to use income mobility as exposure directly. Income differences across counties could drive the association between income mobility and health behavior at the aggregate level. Instead, we want to estimate the income mobility effect, independently of the income level: Would counties with the same income have the same health outcomes if they differ in their level of socioeconomic flexibility (i.e., income mobility)? The associations from nonresidualized mobility estimates include a much broader set of determinants that are not necessarily properly part of mobility regimes which individuals might *perceive*. It is certainly expected that economic mobility may influence onward outcomes through shaping SES and the distribution of income. But, as the literature examining the influence of income inequality on health, we are trying to estimate the net association between income mobility on health, arguing that changes in expectation of economic flexibility might have an impact on individual health investment.

We modeled five health outcomes or behaviors as a function of duration-weighted exposures to different levels of county mobility regimes: self-reported health status, BMI, depression, smoking, days smoking/number of cigarettes. By using the inverse probability of treatment (IPT) weighting, we emulated a counterfactual scenario in which we compared children with the same combination of *observed* covariate values during the exposure time, who did not select systematically into different county mobility regimes. Thus, we adjusted for confounding by time-varying covariates that might be affected by past treatment (Hernan & Robins, 2006) and generated a pseudo-population, in which treatment was no longer confounded with measured covariates. Weights balance treatment assignment across prior confounders and give more or less weight to children with covariates histories that are underrepresented (or overrepresented) in their current treatment group. To reduce the variability of weights, we used stabilized IPT weights (van der Wal & Geskus, 2011).

As a sensitivity analysis, and because IPT weights using a continuous treatment are more sensitive to misspecification and outliers (Thoemmes & Ong, 2016), we also estimated weights for both continuous and categorical scores of income mobility and inequality. While we used linear regression in the first case, we ran ordinal logistic regressions to estimate the probability of exposure to county income mobility quintiles. Finally, following the strategy suggested by Dugoff et al. (2014), we included the sampling weights when computing IPT weights, multiplied them, and considered survey design variables and compound weights when estimating exposure models.

We implemented different outcome models depending on the nature of the dependent variable (e.g., ordinal logistic regression for the effect of income mobility and inequality on *self-reported health status*). Outcome models adjusted only for baseline and time-invariant covariates and took into account sampling design variables (strata, clusters) and weights. We used multiple imputation with multilevel models to address both item-specific nonresponse and attrition (20 multiple imputed data sets). A complete description of the analytical steps, imputation, and covariates used is available in Supplementary Material.

Results

NLYS97 Estimates

We estimated four sets of models based on the following categories: unadjusted or adjusted, continuous or categorical exposure. Unadjusted models provide *naive* estimates by regressing exposure on outcomes without adjustments and IPT weighting, except for sampling weighting. Within each set of models, we ran independent models for each exposure variable: relative income mobility, absolute income mobility, and income inequality.

Table 1 presents the coefficients of average residualized exposure on health outcomes for NLSY97. The first three rows in Table 1 show the *naive* association of average county exposure from age 12 to 20 with five health outcomes. All the exposure treatments represent a negative trait, so we expect adverse consequences for health. To keep consistency with that interpretation, we multiplied *upward mobility* by −1, so that any increase in exposure would consist of a negative condition (i.e., less income mobility, more rigidity of the stratification system, more inequality).

The rank–rank score coefficient (relative income mobility) for self-reported health status is 0.02 (*SE* = 0.04). Because that coefficient comes from an ordinal logistic regression, an increase in 1 *SD*[2] on the average exposure to a rigid stratification environment implies an increment of 2% (exp (0.02) = 1.02) in the odds of reporting excellent health (vs. bad health).[3] Similar coefficients are observed regarding upward mobility (i.e., absolute income mobility) and income inequality (Gini coefficient). However, those estimates are very imprecise and noisy. Coefficients regarding BMI and depression symptoms in Table 1 are easier to interpret. An increase of 1 *SD* in the rank–rank score rises BMI by 0.04 points, and the depression scale by 0.02 points. Again, these estimates are very imprecise and switch their sign in a nonsystematic fashion.

Smoking models show more systematic associations. Both increases in the rank–rank slope or lack of upward mobility raise the odds ratio of smoking later in life by 25% and 23%, respectively (β_{rank} = 0.22, SE_{rank} = 0.04, β_{upward} = 0.21, SE_{upward} = 0.05). Surprisingly, the naive association between the Gini coefficient and current smoking is negative. Similar associations are observed when modeling the number of days smoking during the last day. The quasi-Poisson coefficients suggest that increasing the rank–rank slope or reducing upward mobility raises the incidence rate ratio by 23% and 22%. Again, and contrary to our expectations, the naive Gini coefficient suggests a negative relationship between income inequality and days smoked during the last month.

In summary, unadjusted point estimates for income mobility were relatively small and not systematic across health outcomes. Only smoking behavior seems to have a systematic association with exposure to income mobility in the expected direction (the higher the income mobility, the better the health outcome). Depression, self-report health status, and BMI estimates, instead, are small and uncertain. As a sensitivity analysis, we estimated unadjusted models using nonresidualized exposure variables. In that case, the association between exposure and health outcomes can be spurious, as income mobility and inequality might relate to counties' characteristics that also affect health. Supplementary Table 9 presents unadjusted models with nonresidualized exposure variables. The patterns are relatively similar to the residualized exposure variable models.

Table 1. Estimates of Average Residualized Continuous Exposure on Health Indicators, NLSY97

	Health status	BMI	Depression	Smoking	Days smoking last month
Unadjusted models					
Rank–rank	0.02 (0.04)	0.04 (0.13)	0.02* (0.01)	022*** (0.04)	0.21*** (0.04)
Upward mobility × −1	0.04 (0.04)	−0.04 (0.16)	0.02* (0.01)	0.21*** (0.05)	0.20*** (0.05)
Gini	0.01 (0.03)	−0.08 (0.11)	−0.00 (0.01)	−0.15*** (0.03)	−0.16*** (0.03)
Adjusted models					
Rank–rank	−0.03 (0.04)	0.33* (0.16)	0.01 (0.01)	0.12** (0.05)	0.11** (0.04)
Upward mobility × −1	0.02 (0.05)	0.11 (0.15)	0.01 (0.01)	0.07 (0.05)	0.05 (0.04)
Gini	0.02 (0.04)	−0.07 (0.11)	−0.01 (0.01)	−0.05 (0.04)	−0.07* (0.03)
Individuals	8,810	8,810	8,810	8,810	8,810

Notes: NLSY97 = National Longitudinal Survey of Youth 1997; BMI = body mass index. Each coefficient represents a model. Coefficients and standard errors are combined estimates from 20 multiple imputed data sets. Analyses based on exposure from 12 to 20 years old. We estimate different models depending on the outcome: ordinal regression (self-reported health), general linear model (BMI, depression), logistic regression (smoking), quasi-Poisson regression (days smoking last month).

***p < .001, **p < .01, *p < .05.

Most of the systematic associations are observed between income mobility and smoking, but also BMI.

The estimates discussed above might be biased because of nonrandom selection into counties (residential mobility) and confounding. The next set of results comes from models using a weighted pseudo-population in which county exposure at each wave is independent of prior timevarying covariates. The second section of Table 1 (*adjusted models*) shows IPT-weighted estimates of the effect of standardized income mobility and inequality exposure on health outcomes. Under strong assumptions like no unmeasured confounders, no model misspecification, and positivity—there is a nonzero probability of treatment for every level and combination of confounders (Hernan & Robins, 2006)—stabilized IPT weighting provides unbiased estimates of average causal effects.

Most of the estimates are small and very imprecise to claim they are systematically positive or negative. Relative income mobility estimates on smoking and BMI seem slightly more precise, even after IPT weighting reduces them in about half with respect to the *unadjusted estimates*. We should note, though, that the standard errors in Table 1 are underestimated as we are using several outcomes and making multiple comparisons. Consequently, there is a higher chance of finding false positives. Moreover, given the small and noisy BMI estimate in the *unadjusted* model, the positive coefficient ($\beta_{\mathrm{BMI}} = 0.33$, $SE = 0.16$) in the *adjusted* model, still imprecise, must be interpreted with caution.

Finally, IPT weights using continuous exposure are sensitive and unstable due to parametric misspecification and outliers. Thus, we also estimated the effect of income mobility and inequality using a categorical version of exposure (quintile). Table 2 presents both unadjusted and adjusted models by health outcome. Although the pattern of the coefficients is similar to Table 1, estimates tend to be—as expected—more precise and smaller due to the change in the scale of exposure (1–5). In some cases, coefficients even switch their sign. For instance, the unadjusted smoking coefficients for upward mobility and depression symptoms were, as expected, positive, but they become negative when using the categorical version of income upward mobility. Similar to Table 1, smoking estimates and BMI reveal a systematic relationship with *relative income mobility*. For instance, exposure to the most rigid stratification level (5) compared to counties in the third quintile (average), increases the odds of smoking by about 17% ($\exp((5-3) \times 0.08) = 1.17$). Again, although BMI has a positive and relatively precise coefficient, it only appears in the adjusted models what suggests it is not systematic.[4]

PSID Estimates

The same set of models was estimated with the PSID sample. Although the period of exposure, in this case, is longer (from birth to age 20), the sample size of the 1975–1980 cohort is substantially smaller (2,273 respondents vs. 8,810 in the NLSY97). Table 3 presents unadjusted and adjusted models for residualized income mobility and inequality on health outcomes. In contrast to the NLSY97 results, most of the nonadjusted coefficients are very imprecise and noisy. The only stable coefficients are those related to current smoking and number of cigarettes. The adjusted results in the bottom section of Table 3, in turn, show associations that, although most of the time in the expected direction, are so imprecise to suggest either positive or negative consequences for health.[5] Using a categorical version of exposure does not change results or improve estimates (Table 4). Similar to the NLSY97 analysis, we estimated the model using nonresidualized exposure variables to examine how sensitive our results were to aggregate adjustments of income inequality and mobility indexes. Supplementary Tables 12 and 13 show that the estimates of exposure to a rigid or unequal stratification environment are nonsystematic and noisy, even in the nonadjusted models.

Table 2. Estimates of Average Residualized Categorical (Quintile) Exposure on Health Indicators, NLSY97

	Health status	BMI	Depression	Smoking	Days smoking last month
Unadjusted models					
Rank–rank	0.01 (0.02)	0.07 (0.07)	0.01 (0.01)	0.11*** (0.02)	0.11*** (0.02)
Upward mobility × −1	−0.01 (0.02)	0.00 (0.08)	−0.01* (0.01)	−0.08*** (0.02)	−0.08*** (0.02)
Gini	0.02 (0.02)	−0.04 (0.07)	−0.00 (0.00)	−0.10*** (0.02)	−0.10*** (0.02)
Adjusted models					
Rank–rank	−0.01 (0.02)	0.18* (0.07)	0.00 (0.01)	0.08*** (0.02)	0.07*** (0.02)
Upward mobility × −1	−0.01 (0.02)	−0.05 (0.07)	−0.01 (0.01)	−0.03 (0.02)	−0.02 (0.02)
Gini	0.03 (0.03)	−0.04 (0.08)	−0.00 (0.00)	−0.04 (0.03)	−0.05** (0.02)
Individuals	8,810	8,810	8,810	8,810	8,810

Notes: NLSY97 = National Longitudinal Survey of Youth 1997; BMI = body mass index. Each coefficient represents a model. Coefficients and standard errors are combined estimates from 20 multiple imputed data sets. Analyses based on exposure from 12 to 20 years old. We estimate different models depending on the outcome: ordinal regression (self-reported health), general linear model (BMI, depression), logistic regression (smoking), quasi-Poisson regression (days smoking last month).

***$p < .001$, **$p < .01$, *$p < .05$.

Table 3. Estimates of Average Residualized Continuous Exposure on Health Indicators, PSID

	Health status	BMI	Depression	Smoking	Cigarettes smoked
Unadjusted models					
Rank–rank	−0.03 (0.08)	−0.10 (0.32)	0.03 (0.03)	0.26* (0.13)	0.26* (0.13)
Upward mobility × −1	−0.10 (0.10)	0.40 (0.36)	0.04 (0.04)	0.31 (0.20)	0.37* (0.18)
Gini	0.11 (0.08)	−0.34 (0.29)	−0.01 (0.02)	−0.15 (0.09)	−0.16* (0.08)
Adjusted models					
Rank–rank	−0.01 (0.09)	−0.15 (0.26)	0.03 (0.04)	0.10 (0.15)	0.18 (0.14)
Upward mobility × −1	0.01 (0.10)	0.09 (0.32)	0.03 (0.04)	0.05 (0.19)	0.14 (0.17)
Gini	0.13 (0.09)	−0.12 (0.28)	−0.00 (0.02)	−0.04 (0.11)	−0.08 (0.08)
Individuals	2,273	2,273	2,273	2,273	2,273

Notes: PSID = Panel Study of Income Dynamics; BMI = body mass index. Each coefficient represents a model. Coefficients and standard errors are combined estimates from 20 multiple imputed data sets. Analyses based on exposure from 1 to 20 years old. We estimate different models depending on the outcome: ordinal regression (self-reported health), general linear model (BMI, depression), logistic regression (smoking), quasi-Poisson regression (cigarettes smoked).
$^*p < .05$.

Table 4. Estimates of Average Residualized Categorical (Quintile) Exposure on Health Indicators, PSID

	Health status	BMI	Depression	Smoking	Cigarettes smoked
Unadjusted models					
Rank–rank	−0.00 (0.05)	−0.01 (0.17)	0.01 (0.02)	0.09 (0.07)	0.09 (0.07)
Upward mobility × −1	0.04 (0.05)	−0.34 (0.18)	−0.01 (0.02)	−0.16* (0.08)	−0.20** (0.07)
Gini	0.08 (0.05)	−0.32* (0.15)	−0.00 (0.01)	−0.08 (0.06)	−0.06 (0.05)
Adjusted models					
Rank–rank	0.03 (0.05)	−0.09 (0.13)	0.01 (0.02)	0.03 (0.08)	0.03 (0.07)
Upward mobility × −1	−0.01 (0.05)	−0.13 (0.18)	−0.01 (0.02)	−0.06 (0.08)	−0.10 (0.07)
Gini	0.09 (0.05)	−0.23 (0.14)	−0.00 (0.01)	0.00 (0.06)	0.00 (0.05)
Individuals	2,273	2,273	2,273	2,273	2,273

Notes: PSID = Panel Study of Income Dynamics; BMI = body mass index. Each coefficient represents a model. Coefficients and standard errors are combined estimates from 20 multiple imputed data sets. Analyses based on exposure from 1 to 20 years old. We estimate different models depending on the outcome: ordinal regression (self-reported health), general linear model (BMI, depression), logistic regression (smoking), quasi-Poisson regression (cigarettes smoked).
$^{**}p < .01$, $^*p < .05$.

Conclusion and Discussion

This article aims to estimate the effect of average exposure during childhood and adolescence to a rigid and unequal stratification environment on health outcomes and behaviors during adulthood. Our analysis suggests that the connection between income mobility and health is not as systematic as previous research shows. Our most robust estimates are related to smoking behavior, although only for NLSY97 and *relative income mobility*. This particular finding implies income mobility might directly influence behaviors critically associated with modern chronic illnesses, such as smoking uptake and desistance, alcohol consumption, substance abuse, choices of diet, and physical activity, as the theory indicates. However, we were not able to replicate these findings using the PSID sample that comprehensively measures exposure from birth to age 20, and where selection bias could be better reduced by using IPT weights. Surprisingly, not even our naive coefficients—without adjustments—were systematic enough. The association between income inequality and health outcomes did not hold, either.

The relative consistency between unadjusted and adjusted models suggests that selection is not the only reason there might be inconsistencies between previous aggregate and cross-sectional results and our findings. Although adjustment does reduce the NLSY97 smoking coefficients, unadjusted models do not seem to reveal a systematic association between average exposure to economic opportunities and health outcomes as previous research shows. There might be several explanations for these findings.

First, our results might be affected by measurement error. As Mogstad et al. (2020) have pointed out recently, income mobility measures and rankings computed by Chetty et al. (2014) are *estimates* rather than *true values*, so they might carry considerable uncertainty as population size varies considerably across counties. Supplementary Figure 1 provides some evidence on how measurement error might affect our results. Supplementary Figure 1 shows that most of the counties in the NLSY97 sample have a larger population than those counties excluded. This is expected given the usual sampling design of nationally representative samples. Given the population size of counties, we would expect higher uncertainty of estimates in the small counties. Supplementary Figure 1 shows smaller counties do actually have the most extreme income mobility values, likely due to higher uncertainty. This seems to be especially the case of absolute income mobility (the correlation between population and upward mobility is around −0.37), although relative income mobility has also the most extreme values among smaller counties except that they are evenly distributed across positive and negative values. Thus, by mostly including bigger counties, the NLSY97 and PSID sampling scheme is excluding uncertain income mobility estimates from smaller counties. This is far from an optimal strategy to account for measurement error when estimating the effect of the county economic opportunity environment on health, but unfortunately, we do not have access to the standard errors of the income mobility estimates at the county level.

In comparison, Venkataramani et al. (2016) pool several cross-sectional samples from the Behavioral Risk Factor Surveillance Survey, reaching nearly 147,000 individuals of ages 25–35. That sample covers 2,242 counties, which represents about 78% of all the counties used by Chetty et al. (2014) to estimate income mobility. The same is true concerning previous aggregate analyses, where almost all the counties used by Chetty et al. (2014) were included. In contrast, the NLSY97 and PSID samples cover only 55% and 39% of the counties, respectively. Although we do not know what would happen to previous research results if only the counties of NLSY97 and PSID samples were considered, or better, if the measurement error were considered in the analysis, our findings and the work by Mogstad et al. (2020) suggest that measurement error might exaggerate estimates. Future research will be needed to assess the consequences of measurement error thoroughly, provided the standard errors of income mobility estimates are available.

A second way to interpret our findings stresses the complexity of the effects being estimated. The mechanisms we outlined when discussing the reasons why we should expect a causal relationship between the stratification system and health are not simple. The size of these effects is probably small, and they might be relevant to specific groups of the population and not others. For instance, an individual's family mobility experiences might be equally influential and may offset harmful effects stemming from a place's income mobility. Thus, estimating the population's average effect would not necessarily provide unbiased and robust coefficients. In addition, the effect of exposure to high (or low) mobility environments might exert different effects on health at different life stages. Chetty and Hendren (2015), for instance, when examining the effect of neighborhood exposure, show that the timing of exposure matters a great deal (the younger the more important, with exposures in later teen years showing much smaller effects). It is likely that the window of time when income mobility effects become apparent is a complex function of the mechanisms through which it affects individuals' assessments about advancement, fairness, and social reward allocation. If the impact is only due to socioeconomic conditions—income, nutrition, education, parental practices, stress—most of the impact will concentrate early in life or in utero. On the other hand, if the mechanism operates via expectations about the flexibility of the stratification system, adolescence might be the critical time for forming beliefs about one's position in society and, in this case at least, we should expect that the nature of the social mobility regime will be more salient during this life cycle stage. Critical and sensitive windows is a complex topic in the social sciences and developmental biology literature, as is the associated theme of resilience and inversion of effects later in life. However, due to limitations associated with the nature and size of the sample (NLYS cohorts start at age 12), we cannot conduct properly powered statistical testing to discriminate between signal and noise in estimates of age-specific interaction effects, especially considering the magnitude of standard errors of our main effects.

Finally, we are assuming—due to data limitations—that the aggregate level that matters is county, although it is perfectly possible that the adequate level is neighborhood. There might also be significant differences between the actual place's flexibility of the stratification system and the economic mobility *perceived* by individuals. As Gugushvili and Prag (2021) suggest, perceived mobility seems to map more strongly to health than actual mobility. These factors, in addition to measurement error, make it difficult to estimate long-term implications of society's opportunity system for health. Our results do not necessarily indicate no causal link between income mobility and health, but that our data and analytical strategies are not strong enough to show they are systematic and in the expected direction.

Some additional limitations of our specific analysis should be noted. First, although IPT-weighted estimates avoid some problems associated with conditioning on observed time-varying confounders, selection bias may still occur if unobserved factors simultaneously affect decisions about where to live and health behavior. Second, although we used different specifications and the results were relatively stable, models may still be misspecified. We also need to assume a positive probability of treatment for every level and combination of prior confounders. Theoretically, there is no reason to expect zero treatment probabilities across a set of covariates over time subgroups, except for the inherent limitations of sampling. Third, we assume that measuring income mobility in a cohort—a measure that necessarily realizes in the future—accounts for the latent socioeconomic rigidity to which people were exposed early in life. It is possible that what really matters is the income mobility of the previous generation, as those experiences would determine the socialization and investments of the next generation. Finally, we imputed missing values and adjusted attrition using multiple imputations. Even though we obtained reasonable values and distributions, it is still possible that our imputation models are misspecified, and assumptions such as *missing at random* do not hold.

Overall, our article provides individual estimates of the effect of income mobility on health using a precise definition of exposure and accounting for selection and time-varying confounders. Thus, by focusing on individual outcomes during adulthood and influences during early formative years, we assess more directly the hypothesis that growing up in a community with a rigid stratification system may discourage the adoption of behaviors that provide immediate rewards but are highly noxious, difficult to abandon, and bearers of large effects on health that take a long time to manifest. Future research should focus on finding new indicators of socioeconomic mobility, both at the individual and appropriate aggregate levels, to assess the magnitude of the consequences of the society system of opportunities for health and whether it is an important distal mortality determinant both between and within populations.

Supplementary Material

Supplementary data are available at *The Journals of Gerontology, Series B: Psychological Sciences and Social Sciences* online.

Author Notes

1. A complete description of the data is available in the *Data* section of Supplementary Material.
2. The standard deviation of the rank–rank slope at the county level is 0.086.
3. Proportional odds assumption in original logistic models is not simply that the odds are the same, but that odds ratios are the same across categories.
4. Supplementary Table 10 displays unadjusted models with nonresidualized and categorical exposure variables. They show a similar pattern to the models already discussed.
5. Similar results were obtained when redefining the cohort of respondents (e.g., those born between 1970 and 1985) to increase the sample size, at the cost of adding imprecision to the exposure measures.

Funding

Funding for this research was provided by the National Institute on Aging (P30AG012846, R01AG016209, R03AG015673, R01AG018016, R37AG025216, P01AG029409), the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation program (grant agreement No 788582), and the Fogarty International Center (D43TW001586). This paper was published as part of a supplement sponsored by the University of Michigan with support from the National Institute on Aging (P30AG012846).

Conflict of Interest

None declared.

Acknowledgments

An earlier version of this paper was presented at the 2021 Annual Meeting of the TRENDS Network, May 13–14. This publication reflects only the authors' view and the Research Executive Agency and the Commission are not responsible for any use that may be made of the information it contains.

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