A Report on "Changing Opportunity: Sociological Mechanisms Underlying Growing Class Gaps and Shrinking Race Gaps in Economic Mobility" by Chetty et al. (2026)

Reviewer 2

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> I am wiser than this person; for it is likely that neither of us knows anything fine and good, but he thinks he knows something when he does not know it, whereas I, just as I do not know, do not think I know, either. I seem, then, to be wiser than him in this small way, at least: that what I do not know, I do not think I know, either.
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> Plato, *The Apology of Socrates*, 21d

> To err is human. All human knowledge is fallible and therefore uncertain. It follows that we must distinguish sharply between truth and certainty. That to err is human means not only that we must constantly struggle against error, but also that, even when we have taken the greatest care, we cannot be completely certain that we have not made a mistake.
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> Karl Popper, 'Knowledge and the Shaping of Reality'

Overview

Citation: Chetty, R., Dobbie, W., Goldman, B., Porter, S. R., and Yang, C. S. (2026). Changing Opportunity: Sociological Mechanisms Underlying Growing Class Gaps and Shrinking Race Gaps in Economic Mobility. *Quarterly Journal of Economics*, pp. 1–74.

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Abstract Summary: Intergenerational economic mobility in the U.S. changed rapidly between the 1978 and 1992 birth cohorts, with class gaps growing for white children and white-Black race gaps shrinking for low-income children. These divergent trends are attributed to differential changes in childhood environments, suggesting social interaction mediates changes in economic mobility.

Key Methodology: Uses deidentified federal income tax returns linked to census and Numident data for 57 million children (1978-1992 birth cohorts), quasi-experimental design, and OLS regressions with fixed effects.

Research Question: Can economic opportunity change in shorter time frames, and what are the causal mechanisms underlying the rapid changes in intergenerational mobility by race and class in recent decades?

Summary

Is It Credible?

Chetty et al. analyze linked tax and census data for 57 million children to document "divergent trends" in intergenerational mobility in the United States between the 1978 and 1992 birth cohorts. The authors report that the "class gap" in earnings between white children from low- and high-income families grew by roughly 30%, while the earnings gap between white and Black children from low-income families shrank by a similar magnitude (p. 1). They argue that these shifts are driven by differential changes in "childhood environments," specifically proxied by community-level parental employment rates. Furthermore, they posit that the mechanism linking these environmental changes to child outcomes is "mediated by social interaction" rather than solely by the allocation of economic resources (p. 2).

The descriptive findings regarding the divergence of mobility trends appear highly credible. The reliance on rank-based measures mitigates concerns regarding inflation adjustments, and the consistency of these trends across non-monetary outcomes such as educational attainment and mortality lends robustness to the observation that "outcomes began to diverge before children entered the labor market" (p. 4). The authors are careful to contextualize the "shrinking" racial gap, noting that despite relative gains, "there are still vast and widespread white-Black race gaps" in absolute terms (p. 29).

The article's causal claims regarding childhood exposure effects are also persuasive, though they rest on specific identification assumptions. Using a "movers" design, the authors estimate that the benefit of moving to an improving community is proportional to the duration of exposure (p. 7). This relies on the assumption of "constant selection by age"—that families moving with younger children do not differ on unobservables from those moving with older children (p. 49). While this is in-

herently untestable, the authors provide extensive validation, including sibling comparisons that net out family-level unobservables (p. 58).

However, the interpretation of the specific mechanisms driving these effects warrants more caution. The central explanatory variable is "parental employment rates," but the authors explicitly frame this as a "proxy for a broader set of community-level factors" rather than a precise causal lever (p. 47). Furthermore, in the baseline analysis, this proxy is measured when children are 27 years old, introducing a temporal mismatch between the child's upbringing and the measurement of the environment (p. 39). While robustness checks using census data from the children's actual youth suggest the measure captures stable long-term trends, this choice blurs the line between cause and effect.

The argument that "social interaction" is the primary mediator is suggestive but faces limitations regarding the unit of analysis. The empirical work is conducted at the county level, a geography that may be too broad to capture the hyperlocal nature of social ties (p. 37). The authors argue that local mechanisms aggregate to the county level in a linear model, but this risks an ecological fallacy where county-wide correlations mask neighborhood-level dynamics. The evidence supporting social interaction—specifically that outcomes track the employment rates of peers in a child's specific birth cohort more closely than adjacent cohorts (p. 62)—is compelling, as economic resources are unlikely to vary sharply by birth year. However, the use of county-level interracial marriage rates as a proxy for cross-racial interaction is potentially confounded by other factors like general racial tolerance, a limitation the authors acknowledge (p. 67). Consequently, while the link between community decline and reduced mobility is established, the precise sociological pathways remain partly opaque.

The Bottom Line

Chetty et al. provide convincing evidence that intergenerational mobility trends have diverged significantly, with widening class gaps among whites and narrowing racial gaps among low-income families. The claim that these shifts are driven by changing childhood environments is well-supported by quasi-experimental designs, though the specific identification of "parental employment" serves more as a statistical proxy for community health than a precise policy lever. While the argument for social interaction as a mechanism is plausible and supported by cohort-specific patterns, the reliance on county-level data limits the precision of this sociological conclusion.

Potential Issues

Mismatch between the unit of analysis and the proposed mechanism: The article's central explanatory claim is that changes in mobility are mediated by social interaction within a child's "social community," which is defined by race, class, and childhood county (p. 6). However, the empirical analysis is conducted at the county level, a broad administrative unit that may not align with the hyperlocal nature of social interaction. Many U.S. counties are large and internally segregated, meaning a child's actual social environment (neighborhood, school) may differ substantially from the county-wide average for their demographic group. This raises the possibility of an ecological fallacy, where county-level correlations may not accurately reflect the micro-level causal processes the article posits. The authors explicitly justify this design choice, stating that estimates of mobility changes at smaller geographies like census tracts are "very noisy" and statistically unreliable (p. 37). They argue that under a linear model, local-level mechanisms will aggregate to the county level. While this is a pragmatic response to data limitations, the validity of the findings rests on the assumption that the county is a meaningful proxy for the "community" in which social influence operates.

Strength of the identifying assumption in the causal design: The article's primary claim to identifying a causal effect of childhood environments relies on a quasi-experimental "movers" design. The validity of this design hinges on the "Constant Selection by Age" assumption, which posits that the unobserved characteristics of families who move to improving areas do not vary with the age of their child at the time of the move (p. 49). This is a strong assumption, as it is plausible that families who move with younger children differ systematically from those who move with older children in unobserved ways, such as motivation or long-term planning, which could independently affect child outcomes. The authors are transparent about this assumption and provide a series of validation exercises to support it. They show

that there is no evidence of selection on a rich set of observable characteristics (p. 57; Table II, columns 6–7, p. 55) and employ a sibling-comparison design which relies on a weaker set of assumptions (p. 58). While these tests lend credibility to the design, the core claim of a "causal exposure effect" ultimately depends on the validity of this untestable assumption about unobservables.

Temporal mismatch in the primary proxy for childhood environment: The article's central explanatory variable is parental employment rate, which is used as a proxy for the economic and social conditions of a child's community during their upbringing. However, in the baseline analysis, this variable is measured when the children in the sample are 27 years old, long after their childhood has ended. For instance, for the 1978 birth cohort, parental employment is measured in 2005 (p. 39). The authors justify this by arguing that this late-life measure captures "labor force attachment and other latent factors whose roots emerged while children were growing up" (p. 39). This introduces a potential for temporal confounding, as the analysis correlates adult children's outcomes with their aging parents' outcomes measured around the same time. The authors acknowledge this measurement choice and provide several pieces of evidence to address the concern. They demonstrate that their findings are similar when using an alternative measure of parental employment from decennial census data collected during the children's actual childhoods (p. 38; Online Appendix Figure A.17, p. 116). They also show that the variation in their primary measure is driven more by stable differences across cohorts than by year-specific shocks, suggesting it captures long-term trends (p. 39; Online Appendix Figure A.26, p. 125).

Ambiguity of the primary causal mechanism: The article identifies parental employment rates as a powerful predictor of changes in children's outcomes but is explicit that this variable should be interpreted as a proxy for a broader, unspecified set of factors. A footnote clarifies that the "analysis does not shed light on whether parental employment rates themselves are the key causal lever" but rather tests whether growing up in an area with changing parental employment

rates—which is associated with many other factors—has a causal effect (p. 7, fn. 5). Later, the authors state they "interpret parental employment rates as a proxy for a broader set of community-level factors that may influence children's outcomes" (p. 47). This framing is transparent about the scope of the findings but also makes the core mechanism difficult to falsify. The study demonstrates that some set of changing community-level conditions, for which parental employment is a strong statistical marker, causally affects children. However, it does not isolate the specific components of that environmental change, leaving the precise policy levers unclear.

Scope of the explanation provided: The analysis convincingly demonstrates that community-level changes in the parental generation propagate to the next generation, but it does not identify the upstream causes of those initial changes. The article documents a strong relationship between trends in parental employment and children's mobility but does not systematically test what caused the parental employment trends to diverge in the first place. The authors acknowledge that factors such as the decline of manufacturing, the opioid epidemic, and changes in incarceration rates could be the root causes of the community-level changes they document (p. 10). They explicitly frame the exploration of these upstream factors as an avenue for future research, defining the scope of the current article as identifying the intergenerational transmission mechanism rather than its ultimate origins (p. 67).

Use of a potentially confounded proxy for social interaction: To distinguish between social interaction and economic resource mechanisms, the article examines whether the effect of white parents' employment on Black children's outcomes is stronger in counties with more cross-racial interaction. The analysis uses the county-level rate of interracial marriage as a proxy for such interaction (p. 66). This proxy is potentially confounded, as counties with higher rates of interracial marriage may differ in many other ways that could independently improve outcomes for Black children, such as having lower levels of systemic racism or more integrated institutions. The authors explicitly acknowledge this limitation, stating that the finding is "not

conclusive because rates of interracial marriage are endogenous and could be correlated with other factors" (p. 67). They present this evidence as suggestive, which appropriately reflects the limitations of the proxy used.

Measurement limitations of key variables: The study's reliance on administrative tax data introduces several measurement limitations, which the authors generally acknowledge. The definition of "household income" is based on tax units and therefore excludes the economic resources of cohabiting partners who are not married to the tax filer (p. 14). This could systematically understate resources, particularly for lower-income families where cohabitation is more common. The authors address this with a robustness check that includes income from other adults in the household and report that their main findings are unchanged (p. 25). Similarly, the data on income and employment are derived from formal tax filings (W-2s and 1040s), meaning they do not capture economic activity in the informal economy, which may be a relevant source of support for some families (p. 15). Finally, the article notes that its primary measure of parental marital status is taken at different child ages for different birth cohorts due to data availability, and therefore "must be interpreted with caution" (p. 14, fn. 11).

Framing of relative versus absolute changes in racial gaps: The article's narrative highlights a "shrinking white-Black race gap" in mobility for children from low-income families, which is a key finding. While this focus on the *change* in the gap is appropriate for a study on trends over time, it is important to contextualize this relative improvement against the backdrop of large and persistent absolute disparities. The authors are careful to provide this context, stating that "despite these gains, there are still vast and widespread white-Black race gaps" and noting that even in 1992, Black children from the highest-mobility counties still had worse outcomes on average than white children from the lowest-mobility counties (p. 29). This ensures that the narrative of relative progress does not obscure the persistence of profound underlying inequality.

Minor clerical error in a reported ratio: There appears to be a minor calculation error in the article. To support the social interaction mechanism, the article compares the estimated effect of white parental employment on white children's outcomes ($\hat{\beta}_w$) to the effect of Black parental employment ($\hat{\beta}_b$). The article reports a ratio of $\hat{\beta}_w/\hat{\beta}_b = 14.5$ (p. 65). The coefficients used for this calculation appear to come from Online Appendix Table A.24, column 1, which reports values of 0.292 and 0.022, respectively. The ratio of these values is approximately 13.27, meaning the figure reported in the main text is imprecise. This minor discrepancy does not alter the article's qualitative conclusion that the estimated ratio is substantially larger than the simple population ratio, but the figure reported in the main text is imprecise.

Future Research

Granular investigation of social mechanisms: Future research could test the social interaction hypothesis using more granular data than the county level. While the authors utilize county aggregates to reduce noise, investigating these dynamics at the census tract or school attendance zone level—perhaps using social network data or more detailed administrative records—would better align the unit of analysis with the theoretical mechanism of interpersonal influence and mitigate the risk of ecological inference errors.

Identification of upstream determinants: Future work could identify the structural causes of the community-level changes in parental employment that this study treats as exogenous inputs. While the article establishes that declining community employment harms future generations, it does not systematically test what caused those employment trends to diverge initially. Research connecting these community-level shifts to specific economic shocks—such as trade exposure, automation, or incarceration policies—would clarify the root causes and offer more concrete policy levers.

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