

Still no compelling evidence that Americans overestimate upward socio-economic mobility rates: Reply to Davidai & Gilovich (2018)

Sondre S. Nero* Lawton K. Swan[†] John R. Chambers[‡] Martin Heesacker[§]

Abstract

Davidai and Gilovich (2018) contend that (a) Americans tend to think about their nation's income distribution in terms of quintiles (fifths), and (b) when Americans' perceptions of socio-economic mobility rates are measured properly (e.g., by asking online survey respondents to guess upward-mobility rates across quintiles), a trend of overestimation (too much optimism concerning the number of people who manage to transcend poverty) will emerge. In this reply, we hail Davidai and Gilovich's new data as novel, important, and relevant to the former (a), but we doubt that they can support the latter (b) claim about population-level (in)accuracy. Namely, we note that even if mobility-rate perceptions could be measured perfectly, inferences about the accuracy of those perceptions still depend on a *particular* comparator—a point-estimate of the "true" rate of upward social mobility in the U.S. against which survey respondents' guesses are evaluated—that is *itself* an error-prone estimate. Applying different established comparators to survey respondents' guesses changes both the direction and magnitude of previously observed overestimation effects. We conclude with a challenge: researchers who wish to compute the average distance between socio-economic perceptions and socio-economic reality must first select and justify a fair comparator.

Keywords: social mobility, inequality, political ideology, lay beliefs.

Three years ago, in the January 2015 issue of *Perspectives on Psychological Science*, Davidai and Gilovich argued that Americans tend to overestimate socio-economic prospects for the poor. Their evidence: when asked to estimate the likelihood that a child born into a low-income family in the United States would secure a higher social class ranking in adulthood, participants in Davidai and Gilovich's study ($N = 3,324$ online survey respondents) exaggerated the *actual* likelihood of ascent by an average of ten percentage points. Four weeks later, in the February 2015 issue of *Psychological Science*, we (Chambers, Swan & Heesacker, 2015) argued *precisely the opposite* — our research participants ($N = 865$) had apparently *underestimated* the very same upward-mobility statistic.

Seven months ago, here in *Judgment and Decision Making*, we (Swan, Chambers, Heesacker & Nero, 2017) recanted — after (a) cataloging the slew of ostensibly superficial differences between the two teams' methodologies and (b) presenting evidence that *just one* of those differences could account for the discrepancy between the two teams' findings, we concluded that the probability of a false positive result produced by systematic measurement error, such as question-wording artifacts, was high enough to under-

mine *both* teams' claims about *population-level perceptual (in)accuracy*.¹

Now, Davidai and Gilovich (2018; this issue) suggest that the error was endemic exclusively to *our* questionnaire wording. In brief: Davidai and Gilovich (2015) had asked their participants to estimate socio-economic mobility rates by dividing the population into *quintiles*, whereas we (Chambers et al., 2015) had asked had our participants to divide into *tertiles*. When we inverted the quintile-tertile factor in a new set of experiments (Swan et al., 2017; $N = 1,142$) *while holding all other differences in wording constant*, the overestimation and underestimation effects observed respectively by Davidai and Gilovich (2015) and Chambers et al. (2015) either vanished or reversed (see Table 3 in Swan et al., 2017). Our quintile-tertile-inversion results could not bear on the question of which format is more likely to capture respondents' *true* perceptions, but we did speculate that the tertile format may better reflect how people naturally think about income distributions (in terms of the *upper*, *middle*, and *lower classes*).

We may stand corrected. In three new studies ($N = 392$), Davidai and Gilovich (2018) found that most people sampled divided the income distribution pie into *five* (or more, not three) slices. Consequently, on Davidai and Gilovich's account, when we (Chambers et al., 2015) asked our study

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*University of Chicago Booth School of Business.

[†]Department of Psychology, University of Florida. Email: lkswan@ufl.edu.

[‡]Independent scholar

[§]University of Florida.

¹Note that we were not advocating for the retraction of either the Davidai and Gilovich (2015) or Chambers et al. (2015) papers — both addressed a wide range of additional questions related to perceptions of socio-economic mobility (e.g., the relationship between political ideology and mobility perceptions) that withstood our (2017) inquisition.

participants to estimate upward mobility from the *bottom third* — a non-intuitive category label — we exposed ourselves to a set of tertile-specific measurement vulnerabilities. Namely, in two additional studies ($N = 304$), Davidai and Gilovich (2018) found that people tend to (a) assume that a three-rung ladder is *ipso facto* harder to climb than a five-rung ladder; and (b) estimate less upward social mobility when a questionnaire first asks about (primes the concept of) *immobility* (as ours did).² Citing additional evidence that people normally conceptualize movement in rankings as the likelihood of rising up rather than as the likelihood of staying put (Davidai & Gilovich, 2016), Davidai and Gilovich (2018) concluded that their original (2015) quintile prompts elicited reliable and valid responses (whereas our original tertile prompts did not).

Although we find Davidai and Gilovich's (2018) new data to be interesting, we do not believe that they support their central claim: that the average American overestimates the actual number of people who manage to transcend childhood poverty in the United States. We dedicated most of the text in our (2017) critique to an experimental demonstration of the quintile-tertile effect, but, as we noted in our (2017) introduction, the *key* distinction between the Davidai and Gilovich (2015) and Chambers et al. (2015) investigations was simpler: *the two teams had applied different true-mobility rate comparators*. Whereas Davidai and Gilovich (2015) had weighed their participants' guesses against data from the Panel Study of Income Dynamics (PSID) as reported by the Pew Economic Mobility Project (2012), we (Chambers et al., 2015) had relied on tax-record data curated by Chetty, Hendren, Kline, Saez and Turner (2014).³ The distance between the two comparators matters: when one contrasts Chetty et al.'s true-mobility-rate-point-estimate (rather than Pew's) with Davidai and Gilovich's (2015) participant-guess-averages, the overestimation-of-mobility effect disappears entirely. To wit, if Davidai and Gilovich had used Chetty et al.'s (2014) data to compute true-mobility-rate benchmarks in their original (2015) study, they would have discovered that their participants perceived social mobility trends with dead-on accuracy.⁴ Thus, even if one accepts that quintile measures yield psychometrically superior data, Davidai and Gilovich's (2018) overestimation claim still depends on the belief that Pew's (2012) point estimates are more accurate than Chetty et al.'s (2014).

²In our original study (Chambers et al., 2015), we first asked participants to estimate "the percentage of children born to parents in the BOTTOM 3rd of the income distribution who stayed in the bottom third of the income distribution (i.e., lower class), like their parents."

³Note that the Pew Research Center in 2015 updated its sampling methods to bring them more into line with those used by Chetty et al.

⁴Pew (2012) reported that 43% of children born into the bottom quintile of the U.S. income distribution remained there as adults. Chetty et al. (2014) estimated the same quantity to be 33%. Davidai and Gilovich's (2015) participants' guesses were, on average, right on the money ($M = 33\%$).

Our (2017) discovery of elicitation-method sensitivity (the quintile-tertile effect) alone would not have led us to conclude that the quest to quantify population-level perceptual (in)accuracy is hopeless. But reckoning with *both* of these problems — (a) comparators with unknown error margins weighed against participants' responses to (b) estimation prompts with a high potential for subtle systematic bias — did move us to emphasize the formidable methodological challenges that face researchers who approach this topic. The first problem (selecting the correct comparator) does not appear to have a single econometric solution (e.g., see Bloome, 2015; and Warren, 2014). Comparators should therefore be accompanied by clear justifications and statements of limitations.

Davidai and Gilovich (2018) seem to believe that they have solved the second problem, that of ensuring that mobility-perception prompts avoid hidden question-wording confounds. We respectfully disagree — even if the evidence continues to favor the use of quintile response formats, other possible complications remain. For instance, Davidai and Gilovich's (2015) questionnaire apparently asked participants to forecast mobility rates *moving forward* — the likelihood that a person born today *would* end up in a given income group *in the future*. Both Pew (2012) and Chetty et al. (2014) approximated upward social mobility trends *in the past*. Thus, unless one assumes that intergenerational elasticity in earnings will remain stable over the next several decades (this seems highly unlikely to us; see Chetty et al., 2014; and Mishel, Bivens, Gould & Shierholz, 2012), arguably there is no valid comparator for Davidai and Gilovich's (2015) data. It may be that most participants in Davidai and Gilovich's (2015) study ignored these temporal cues and estimated mobility rates in the past, but without a direct empirical test, in our view it remains prudent to doubt Davidai and Gilovich's (2018) broad conclusion that "Americans indeed overestimate the [current] degree of mobility in the United States."

Of course, there are other ways to assess trends in population-level perceptual (in)accuracy. Consider, for instance, Kraus and Tan (2015) — two weeks *after* Davidai and Gilovich (2015) and two weeks *before* us (Chambers et al., 2015), Kraus and Tan *also* published a paper (in the *Journal of Experimental Social Psychology*) on the topic of how Americans perceive the degree of socio-economic mobility in the United States. Like Davidai and Gilovich (2015), Kraus and Tan asked their online survey respondents (Study 1 $N = 751$) to guess the future social class rankings of people born into the bottom income quintile, and, like Davidai and Gilovich, Kraus and Tan observed undue optimism. (See also Kraus, 2015.) But Kraus and Tan (2015) also asked their study participants to estimate several *more specific* social mobility statistics. For instance: how many people out of 100 during a ten-year time period from 1997–2006 would move up from the bottom 20% of income (a) by working

1000 extra hours or (b) with some kind of college degree? People overestimated on both counts.

Should we treat Kraus and Tan's (2015) findings as a line of converging independent evidence that, despite the methodological concerns we have raised about Davidai and Gilovich's (2015) and Chambers et al.'s (2015) studies, Americans really do overestimate social class mobility? Again, the comparator problem rears its head — Kraus and Tan drew their benchmark values from yet another data source (Mishel, Bivens, Gould & Shierholz, 2012). Kraus and Tan's real-mobility-rate estimates were more pessimistic: citing data from the PSID, they claim that fewer than 1% of people born into the bottom 20% will rise to the top 20% in adulthood. (Chetty et al. estimated the number closer to 8%.) The consistency of Kraus and Tan's participants' responses across a diverse array of questions *may well signify an underlying socio-cognitive phenomenon*,⁵ but, in our view, without a clear justification for the use of these data as the comparator, the title of Kraus and Tan's (2015) paper, *Americans Overestimate Social Class Mobility* (see also Kraus' 2015 pre-registered replication report, *Americans Still Overestimate Social Class Mobility*) remains debatable.⁶

Of course, there are still *other* ways to assess trends in population-level perceptual (in)accuracy. In a final pair of studies ($N = 206$), Davidai and Gilovich (2018) observed that Americans tend to rank the United States too favorably among a group of fifteen countries with varied social class mobility rates. Does the preponderance of evidence *now* support the broad claim that Americans' overestimate social class mobility? Alas, although we may update our priors, our skepticism endures. In this case, we note that people's estimates of social mobility rates *within* a country may be orthogonal to their estimates of social mobility rankings *between* countries. One can imagine, for instance, that while most Americans believe their country boasts more socio-economic mobility than most others (which is demonstrably not the case), opportunities for advancement are still unfairly constrained (which is demonstrably the case). Moreover, in our 2017 critique we developed our own novel measure of beliefs about economic mobility — when we asked our participants ($N = 722$) to pick from between three graphical representations of (pictures of ladders overlaid with) different social-mobility-rate distributions, the results could not have been clearer: the vast majority of our participants

⁵However, subtle framing effects may have influenced Kraus and Tan's (2015) participants' responses — for instance, asking participants solely about movement from the very bottom to the very top could have primed the concept of mobility. (Davidai & Gilovich, 2018, documented a related framing effect.)

⁶Though, as we noted in our (2017) article, "Kraus and Tan's 2015 contribution focused chiefly on comparing mobility perception averages between groups (e.g., between people of high versus low subjective socioeconomic status). The question-wording measurement error that we identified in [our] studies need not bear on most of Kraus and Tan's results, which do not require that the dependent measure be externally valid" (p. 513).

(72%) *underestimated* even when they were (a) presented with quintiles and (b) could have selected a distribution that corresponds exactly to the average participant guesses (overestimates) from Davidai and Gilovich's (2015) original study. Davidai and Gilovich (2018) did not mention or discuss this finding. Yet another team of researchers (Alesina, Stantcheva & Teso, 2018) asked a slightly different question, used Chetty et al.'s (2014) data as their comparator, and documented a trend of *accuracy*.

When we advised our judgment-and-decision making colleagues to shift away from the question of population-level (in)accuracy, we were admitting our own discouragement in the face of so many granular complexities. Davidai and Gilovich respectfully disagreed, and we are glad they did. Indeed, effective policy development depends on our ability to discern these trends. And so, in the end, we concur — the quest to measure distortions in Americans' socio-economic-mobility perceptions deserves scientific attention.⁷ Our step-one proposal is to select and justify a fair comparator (with appropriate error bars).

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⁷Clearly many people do overestimate social mobility rates (even if a majority of Americans do not), and those perceptions likely have democratic consequences. It would be useful to know who those people are (see Kraus & Tan, 2015 for some answers). It would also be useful to know why, if a majority of Americans really do perceive mobility rates accurately (Alesina et al., 2018), they seem to care relatively little about the fact that opportunities for social advancement are so unevenly (unfairly) distributed (Gallup, 2016).

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