INEQUALITY OF OPPORTUNITY FOR INCOME IN DENMARK AND THE UNITED STATES: A COMPARISON BASED ON ADMINISTRATIVE DATA*

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Abstract

We carry out a comparative analysis of inequality of opportunity (IOp) for long-run income in Denmark and the United States. We adopt a luck-egalitarian understanding of IOp, use high-quality administrative data, and rely on highly improved methods. These include novel identification assumptions that allow us to produce set estimates of IOp in the United States relative to Denmark rather than just lower-bound estimates of IOp in the two countries. There are five main results. First, with types based only on gender and parental income rank as the circumstances beyond people's control, measured IOp for income is high in the United States and far from negligible in Denmark: before taxes and transfers, the lower-bound Gini coefficients for individual earnings and family income opportunities are in the 0.21-024 range in the United States and in the 0.08-0.12 range in Denmark. Second, the tax system and the welfare state reduce measured IOp in both countries, but they do so by more than twice as much in Denmark. Third, our analyses in terms of disposable family income per adult—which factor in taxes and transfers and purge the effect of the association between parental income and the probability of marriage—entail that there is more IOp for income in the United States than overall income inequality in Denmark. Fourth, IOp for income is substantially higher in the United States than in Denmark. With opportunities defined in terms of disposable family income per adult, IOp is at the very least 68 percent higher in the United States, and this result is very robust to the inequality index employed in the analysis. Fifth, our lower-bound estimates of the unfair inequality as a share of overall inequality are much larger in both countries than typically reported for advanced economies. When we account for race and ethnicity as circumstances beyond people's control (in addition to gender and parental income), our lower-bound estimate of that share for the U.S. reaches almost 58 percent. We conclude that the distribution of economic opportunities—and not just of economic outcomes—is substantially less unequal in Denmark than in the U.S., and that a very large share of U.S. income inequality may be tracked back to circumstances beyond people's control.

Introduction

For a long time, social scientists only studied inequality in economic opportunities within countries and its variation across countries indirectly, by focusing on economic mobility across generations (for reviews, see Blanden 2013; Corak 2006; Mitnik et al. 2018; Solon 1999). The study of economic mobility—in particular, the estimation of intergenerational income and earnings elasticities (IGEs)—was motivated, explicitly or implicitly, by the notion that (im)mobility rates provide information on how (un)equal opportunities are, something that is still the case today.[1] At least in part, this focus on mobility resulted from the greater conceptual, methodological and practical difficulties involved in theorizing and measuring inequality of opportunity compared to mobility.

Over the last 15 years, however, things have changed significantly, as a large, sophisticated, and influential empirical literature on inequality of opportunity (IOp) has developed quite independently from the mobility literature. In terms of philosophical foundations, this new literature has mostly adopted the "luck egalitarian" understanding of IOp (e.g., Dworkin1981a, 1981b; Arneson 1989; Cohen 1989; Roemer 1998), which puts individual responsibility front and center in the normative assessment of inequality. The various theories of justice in the luck-egalitarian family stress the ethical imperative of counteracting the distributive effects of luck on people's incomes and other outcomes (e.g., health status, educational attainment) that have a large impact on their chances of achieving their life plans. As luck is often interpreted as the opposite of what individuals are responsible for (e.g., Cohen 2006:442), luck egalitarianism has also been called "responsibility-sensitive egalitarianism."[2]

Luck egalitarians argue that income and other consequential outcomes are determined by factors that are beyond individuals' responsibility, usually referred as "circumstances" (e.g., gender, race, socioeconomic background), and by factors for which individuals should be held responsible, often referred as "effort" (e.g., number of hours worked, educational attainment, occupational choice). Inequalities due to differences in circumstances are deemed ethically unacceptable or unfair whereas those arising from differences in effort are considered just, but only as long as the differences in effort cannot be traced back to differences in circumstances.[3] Thus, for any outcome of interest, the luck-egalitarian normative ideal is an outcome distribution that satisfies two principles: the *reward principle*, that is, the principle that efforts should be rewarded adequately; and the *compensation principle*, that is, the principle that the effects of circumstances should be fully compensated for. In this ideal context, all existing disparities are due to effort

[1] An exact account of the relationship between those two pairs of complementary concepts in the context of IGE-based research, however, has only been provided very recently (Mitnik, Bryant and Weber 2019:387-388).

[2] This characterization glosses over important qualifications regarding the notion of luck that is relevant here. For a detailed analysis of this notion and its role in luck-egalitarianism, see Lippert-Rasmussen (2018).

[3] This position, which requires that effort be "cleaned from any contamination coming from circumstances" (Jusot, Tubeuf and Trannoy 2013), is due to Roemer (e.g., 1998) and is the one dominant in empirical work. For alternative philosophical positions on this matter, see Barry (2005) and Swift (2005).

differentials not accounted by circumstances. As we explain next, the empirical literature has focused on the latter principle.

There are two different interpretations of the compensation principle. In the *ex-post view*, the principle requires equalizing outcomes among people exerting the same level of effort but subject to different circumstances. In the *ex-ante view*, it requires equalizing people's opportunity sets. IOp has typically been measured by establishing how far a society is from satisfying the compensation principle. This has involved (a) measuring and suitably aggregating into one index of overall IOp the inequalities that exist across people with the same levels of effort, or (b) measuring the inequality in the value of the opportunity sets of people with different circumstances. Most studies in the empirical literature on IOp for income (IOpI) have implemented a specific version of the ex-ante view, first proposed in the theoretical literature by Van de gaer (1993), in which the value of an individual's opportunity set is measured by the average income of all individuals with the same circumstances. Using this approach, the IOpI literature has produced estimates for many countries and—like the economic mobility literature—has made comparing countries in terms of their IOpI levels a central goal (e.g., Björklund, Jäntti and Roemer 2012; Brunori, Ferreira and Peragine 2013; Checchi, Peragine, and Serlenga 2010; Ferreyra and Gignoux 2011; Hufe et al. 2017; Marrero and Rodríguez 2012; Pistolesi 2009; Suárez Álvarez and López Menéndez 2019).

How much variation in IOpI exists across highly advanced economies that differ in terms of their labor market, education, job training, welfare, and tax policies? Do IOpI levels vary systematically across countries representing different "varieties of capitalism" (Hall and Soskice 2001; Amable 2003) and different "worlds of welfare" (Esping-Anderson 1990, 1999), in the same way in which income inequality does? And more specifically, how do social-democratic countries like Denmark, Finland, Norway, and Sweden compare with the U.S., which is the prototypical case of a country with a liberal market economy and a liberal welfare regime? Most comparative research on economic mobility suggests that the former countries have achieved substantially lower levels of IOpI in addition to substantially lower levels of inequality in economic outcomes (e.g., Björklund and Jäntti 1997; Bratsberg et al. 2007; Corak 2006; Esping-Anderson 2015; Helsø 2018, forthcoming; Mitnik, Bryant and Grusky 2018).[4] However, the *direct* evidence that is available, especially on the question of the magnitude (and not just the sign) of the differences in IOpI between the U.S. and the social-democratic countries, is far from compelling.

Indeed, although existing IOpI estimates and international comparisons provide information on the questions posed above, the literature has been affected by a series of suboptimal methodological decisions, the widespread use of an incorrect estimation approach, and the fundamental methodological difficulty generated by the all-encompassing nature of the theoretical notion of circumstances (which includes *all*

[4] In opposition to a very extensive literature, Landersø and Heckman (2017) recently claimed that the low level of intergenerational persistence in Denmark is a "fantasy," mainly because (a) intergenerational educational mobility in Denmark and the United States are similar, and (b) the difference in intergenerational economic persistence across the two countries is very sensitive to the income measure used to compute it (and, in particular, is small when computed with a measure of pre-tax-and-transfers income). Andrade and Thomsen (2018) and Helsø (2018, forthcoming) have shown that these arguments are seriously flawed (see also Mitnik, Bryant and Grusky 2018).

factors beyond people's control). Equally important, the studies on which a comparison between the U.S. and the social-democratic countries could be based have important empirical limitations.

Suboptimal methodological decisions. IOpI scholars have not focused on the IOp in *long-run* income (for a notable exception, see Björklund, Jäntti and Roemer 2012), which is the income of interest from a normative point of view.[5] In addition, when selecting the inequality measure to be used in their analyses, IOp scholars have tended to prioritize attractive theoretical properties over pragmatic properties like interpretability. Thus, the mean logarithmic deviation (MLD), the inequality index most often used in the literature, does not have an upper bound; this makes it difficult to attach an interpretation to absolute levels of IOpI (AIOpI) measured with that index. Perhaps due to those interpretative difficulties, IOp scholars have given at least as much attention in their analyses (and often much more) to results pertaining to relative IOpI (RIOpI)—that is, AIOpI as a share of overall income inequality—as to their AIOpI results. However, AIOpI is the relevant quantity for cross-country normative assessments. Moreover, as discussed in detail by Mitnik (2020b), once the focus is on long-run income, the denominator of RIOpI, overall long-run income inequality, is affected by an upward bias when estimated with the same data that produce consistent estimates of AIOpI. And, as we explain later, one additional and rather serious methodological difficulty will most likely arise when trying to estimate overall income inequality if the inequality measure is the MLD.

Incorrect estimation approach. In direct analogy to what has been the case with the estimation of the IGE in the economic mobility literature (Mitnik and Grusky 2020), the approach typically used to produce IOpI estimates is inconsistent with the interpretations attached to those estimates. Indeed, the so-called "parametric approach" (e.g., Ferreira and Gignou 2011), which can be more precisely characterized as a "parametric log-linear approach" (Mitnik 2020c), does not do what it is supposed to do even if its functional form assumptions hold. Although the approach is supposed to index opportunity sets by the expected income of people with the same circumstances, it indexes them by the geometric mean of those people's incomes; as a result, it estimates something other than what it is trying to estimate (Mitnik 2020c). A corollary of this "wrong-estimand problem" is that estimates based on the parametric log-linear approach are not really comparable to estimates produced with the more sparingly used "nonparametric approach" for IOpI estimation (e.g., Checchi and Peragine 2010).

Partially observed circumstances. The fundamental methodological difficulty, well understood in the literature, is that in empirical studies AIOpI is always measured with respect to an incomplete set of circumstances, which entails that AIOpI estimates are always lower-bound estimates (for a general formal proof that applies to any Lorenz-consistent inequality measure, see Luongo 2011: Prop. 1). Comparisons of AIOpI estimates across countries are therefore very complicated affairs, and not nearly enough attention has been paid to the conditions under which those comparisons may be informative. These difficulties are compounded if the goal is to conduct comparisons of RIOp for long-run income, as in this case the estimates are pulled down not only by the underestimation of AIOpI but also by the overestimation of long-run income inequality mentioned above.

[5] This has long been well understood in the mobility literature (e.g., Jenkins 1987; Black and Devereux 2011; Solon 1992; Solon 1999).

Empirical limitations. Beyond these general methodological problems, IOpI in the U.S. and the social-democratic countries has most often been measured using different circumstances and income concepts, relying on different estimation approaches, or using samples representing very different cohorts, periods and (sometimes, quite selective) populations; all of this reduces our confidence on the conclusions about cross-country differences that may be obtained. Also problematic, some estimates are based on survey data that do not cover well the upper tail of income distributions; this can be expected to distort, perhaps greatly, the measurement of IOpI. When, further, country results based on these survey data are compared with country results based on register or other administrative data (which cover well the full income distributions), there is a good chance that any conclusions that are drawn will be misleading.

In this article, we make both substantive and methodological contributions to the literature on IOpI. We start to address the all-important empirical questions posed above by focusing on Denmark and the U.S. while simultaneously tackling all the data issues and methodological problems just discussed. We avoid the pitfalls involved in comparing estimates based on survey and administrative data by using high-quality administrative data for both countries: register data for Denmark and the Statistics of Income Mobility (SOI-M) Panel for the U.S. Further, in our analyses we focus on the same birth cohorts (1972-1975) and time period (2010) and use the same sample selection rules, income concepts (individual earnings, total and disposable family income) and nonparametric estimation method for both countries. To the best of our knowledge, this is the first cross-country IOpI comparison based on administrative data—and where, in addition, methods, cohorts, period, and income notions are all well aligned. And because we use the nonparametric approach for IOpI estimation, our results are immune to the wrong-estimand problem affecting the many estimates generated with the parametric log-linear approach.

Unlike almost all previous estimates in the literature, our estimates pertain to long-run income; we provide a clear justification—based on an empirically-validated measurement-error model developed by Mitnik (2020b)—for why this is the case despite the fact that we use annual income measures to compute the mean income of people with the same circumstances. We report both AIOpI and RIOpI estimates but we dedicate much more attention to the former. We compute AIOpI estimates based on the MLD, in part because it is a theoretically attractive measure (e.g., Ferreira and Gignoux 2011) but mostly because this allows direct comparison with a large share of the estimates previously reported in the literature. But we use the Gini coefficient much more extensively. The Gini coefficient is double bounded and therefore much easier to interpret than the MLD and other indices with no upper bound and, unlike the MLD, it can always be unproblematically estimated. To assess how robust our comparative results are to the inequality measure employed, we carry out complementary analyses with several other inequality measures.

In our main analyses, we use a very sparse set of variables, gender and parental-income rank, to define circumstances. Nevertheless, with our data and methodological approach, the computation of AIOpI with respect to this very incomplete set of circumstances produces highly informative lower-bound estimates. We also discuss, for the first time in the literature, explicit identification assumptions under which point and set estimates of AIOpI *ratios* between countries—that is, ratios of AIOpI in one country relative to another—can be obtained with an incomplete set of circumstances. We show that an assumption that leads to the point identification of this ratio—and which, arguably, has been implicitly relied upon in the empirical research on cross-country comparisons of AIOpI—is very difficult to justify. We further argue that two identification assumptions that lead to large improvements in the set estimation of ratios of AIOpI in the U.S. relative to Denmark—compared to the situation in which such set estimation is based on "the data alone" (Manski 2003)—are most likely correct. By relying on these identification assumptions, we are able to generate highly informative lower-bound estimates of how much AIOpI there is in the U.S. compared to Denmark (i.e., of AIOpI ratios between the U.S. and Denmark). Thus, we provide a comparison of inequality of opportunity between the U.S. and a social-democratic country that is valid even though it is based on an incomplete set of circumstances. In an extension of our analyses, only for the U.S., we report approximate estimates of how much larger AIOpI and RIOpI are when race and ethnicity are added to the set of considered circumstances.

What are our main findings? With respect to AIOpI, we will show that (a) measured IOp for long-run income is large in the U.S. and far from negligible in Denmark; (b) although tax and transfers reduce this inequality in both countries, they do so by more than twice as much in Denmark; (c) in terms of disposable family income per adult—which factors in taxes and transfers and purges the effect of the association between parental income and the probability of marriage— there is more IOp for income in the U.S. than overall income inequality in Denmark; and (d) with the same income notion, IOp for long-run income is at the very least 68 percent higher in the U.S. than in Denmark. These results indicate that the distribution of economic opportunities—and not just of economic outcomes—is substantially less unequal in Denmark than in the U.S. With respect to RIOpI, we will show that, even with types based only on gender and parental income rank as the circumstances beyond people's control, the lower-bound shares of the unfair inequality are much larger in both countries than typically reported for advanced economies. And that when race and ethnicity are also accounted for in the analysis, the lower-bound estimate of that share for the U.S. reaches almost 58 percent.

The structure of the rest of the article is as follows. We lead off with a detailed analysis of the many methodological difficulties involved in transforming a sophisticated philosophical understanding of inequality of opportunity into a solid empirical research program, what the previous literature has done in this regard, and the various improvements introduced in this article. Next, we address the thorny problem of how to compare lower-bound estimates across countries (as well as other issues related to the interpretation of results) and stress the need for explicit identification assumptions. This is followed by a description of our data, variables, and approach to estimation and statistical inference, and by the presentation of our empirical results. The last section discusses those results and distills the article's main conclusions.

From theorical principles to measurement

Most empirical studies in the literature have adopted a notion of IOpI that is based on a specific variant of the ex-ante interpretation of the compensation principle. This notion posits that (a) *circumstances* are all the things that account for people's incomes and are beyond their control (and for which, therefore, they cannot be held responsible), (b) *types* are groups of individuals who share the same circumstances, (c) the individuals belonging to a type share a common *opportunity set*, i.e., a set of income prospects, (d) the *value* of each opportunity set is measured by the mean of the realized incomes of those belonging to the type, and (e) *inequality of opportunity* is the inequality in opportunity-set values across individuals. This is the understanding of IOpI to which we subscribe in this article.

Transforming this understanding into empirical measures of IOpI requires making several consequential methodological decisions; the quality and relevance of the resulting measures is affected by these decisions and by the data used to produce the estimates. We examine in this section the methodological approaches and data used in the previous literature, paying special attention to the studies that have produced IOpI estimates for Denmark and the U.S. We also explain how we improve on those data and approaches in this article.

Previous results for Denmark and the United States

Table 1 summarizes the nine studies we know about that have produced AIOpI and RIOpI estimates for Denmark or the U.S. using the MLD as inequality measure. Putting aside studies based on pre-2000 income data, AIOpI estimates for Denmark are in the 0.001-0.020 range while those for the U.S. are in the 0.01-0.329 range (and in the 0.01-0.07 range if we exclude the estimates from Hufe et al. 2017, on which more later). Although the U.S. estimates tend to be larger, given the wide diversity of periods, cohorts, income concepts, measured circumstances, methods and represented populations across studies, there is no pair of estimates in this table that could reliably be used as the basis for a comparative assessment of AIOpI in Denmark and the U.S.

Table 2 summarizes the three studies we know about that have produced estimates for Denmark or the U.S. and relied on the Gini coefficient instead or in addition to the MLD. The AIOpI estimates for Denmark are in the 0.03-0.10 range while those for the U.S. are in the 0.12-0.17 range. The disposable-income estimates for Denmark and the U.S. due to the Equalchances Project were produced with the explicit goal of allowing cross-national comparisons, and therefore standardized procedures were used to obtain them.[6] Focusing on the most recent estimates that can be used for a comparison, the AIOp for household equivalent disposable income is put by this project at 0.03 and 0.13 for Denmark in 2010 and the U.S. in 2008, respectively.[7] If we ignore for now that these are lower-bound estimates, these results suggest substantially higher AIOpI in the U.S. than in Denmark but are not inconsistent with the existence of very low (Denmark) or relatively low (U.S.) levels of AIOpI in both countries. Moreover, as we explain below, although these estimates are not affected by several of the methodological problems impacting the other studies listed in Tables 1 and 2, they are still affected by some important methodological shortcomings and by the limitations of the data on which they are based.

Data limitations

All estimates in Tables 1 and 2 were produced with survey data: the European Union Survey on Income, Social Inclusion and Living conditions (EU-SILC), on which all estimates for Denmark are based; the Panel Study of Income Dynamics (PSID), on which nearly all U.S. estimates are based; and the National Longitudinal Survey of Youth 1979 (NLSY79) and its Child and Young Adults supplement, which Hufe et

[6] For the Equalchances Project, see equalchances.org. Several other studies included in Tables 1 and 2 used standardized procedures for the same reason, but they only included European countries in their analyses.

[7] It could be argued that the 2008 estimate for the U.S. is affected by the income compression and very high unemployment rates of the Great Recession, and therefore may not reflect the typical level of AIOpI in the country. The fact that the estimates for 2002-2006 are very similar provides reassurance.

al. (2017) used to produce their estimates. None of these surveys covers the institutionalized population (e.g., people in prison or in residences for the disabled or mentally ill), the homeless, or the geographically mobile; given the evidence (e.g., Pettit 2012; Western, 2006) about U.S. statistics on related topics (e.g., educational attainment, labor force participation, earnings), the biasing effects of excluding people in prison can be expected to be particularly consequential for the measurement of IOpI in the U.S. In addition, it is well-known that in surveys like the PSID and the NLSY79, where income information is provided by respondents, the income questions are affected by high nonresponse rates, deliberate underreporting, and inaccurate reporting due to recall failures and other problems (e.g., Moore et al. 1997). It is also well-known that surveys of this type do not cover well the upper tail of income distributions (e.g., Fixler and Johnson 2014; Törmälehto 2017).[8] Another limitation, exclusive to the PSID data, is that this survey only makes available full income information for household heads and their spouses (or cohabiting partners) rather than for the full adult non-institutionalized population. Lastly, the PSID and NLSY79 are long-running longitudinal surveys affected by substantial attrition, while the EU-SILC data are affected by high unit nonresponse rates (e.g., 44.4 percent for Denmark in the 2011 wave used to produce the 2010 estimates shown in Tables 1 and 2 [Hlasny and Verme 2018:Table 2]); neither of these two problems can be fully countered by adjusting sampling weights.[9]

The administrative data we use in this article are essentially immune to all the limitations just discussed. Although they have other limitations (which we discuss below), they nevertheless provide a better foundation for carrying out a comparative assessment of IOpI in Denmark and the U.S. than the data used before.

The parametric log-linear approach: Wrong estimand and represented populations

The nonparametric approach for estimating AIOpI (e.g., Chechi and Peragine 2010) is very simple. Once the types are defined in terms of measured categorical circumstances, the mean income of each type is estimated and assigned to all individuals belonging to that type, and the chosen inequality measure (e.g., the MLD or the Gini coefficient) is computed over this "smoothed distribution" (Foster and Shneyerov 2000). Unfortunately, even with a few circumstances and a few categories in each circumstance, most often the data demands of this strategy cannot be satisfied by the available samples. To address this problem, the literature has relied on what it has been referred as the "parametric approach." For reasons we explain next, following Mitnik (2020c) we refer to this approach as the "parametric log-linear approach." It involves (a) running a linear regression of log income on indicator variables for the categorical circumstances defining the types (e.g., gender, race, parental education), often without any interactions, (b) computing predicted values for all

[8] The EU-SILC income information for Denmark comes from administrative sources, and there is some evidence suggesting that it represents the full income distribution well (Bartels and Metzing 2019:138).

[9] Attrition, for instance, is addressed by adjusting the weights of the remaining respondents. When these adjusted weights are used to compute IOpI measures, the implicit (and strong) assumption is that attrition is independent of people's earnings or income (after controlling for the variables on which the weights are based). Against this assumption, Shoeni and Wiemers (2015) have shown that, in intergenerational analyses, the PSID is affected by selective attrition.

individuals, (c) exponentiating these predicted values to obtain (what is interpreted as) opportunity-set values, and (d) computing the chosen inequality measure over the resulting smoothed distribution.

Using Z and D to denote income and the inequality measure, respectively, this means that, if the functional form of the regression model is correct, D is computed over the distribution of $\exp\big(E(\ln Z\,|C)\big) \equiv GM(Z|C)$, where C is a variable indexing the types under consideration and GM is the geometric mean operator. In the general case, however, $D\big(GM(Z|C)\big) \neq D\big(E(Z|C)\big)$ because, within types, the geometric mean of income is smaller than expected income and the proportional difference between the two varies across types. It then follows (Mitnik 2020c) that (a) the estimates do not pertain to IOpI as defined in the literature but, rather, to a different notion of IOpI (where the value of an opportunity set is measured by the geometric mean of the realized incomes of those belonging to the corresponding type), and (b) the estimates produced by the parametric log-linear approach are not directly comparable to those produced by the nonparametric approach. Moreover, because the geometric mean is undefined when a variable includes zero in its support, explicitly or implicitly the reference populations in studies using the parametric log-linear approach get restricted to people with positive incomes. These selected populations are very unlikely to be the populations of interest. This is a particularly serious problem if the goal is to estimate AIOp for individual earnings since many people do not receive any earnings (due to unemployment and other forms of nonemployment). But focusing instead on family- or household-based income measures—as sometimes has been suggested precisely to address the problem of a substantial number of people with zero earnings in countries with high unemployment rates (e.g., Suárez Álvarez and López Menéndez 2019:152)—does not necessarily provide a solution. For the U.S., for instance, Chetty et al. (2014: Online Appendix Table IV) report that, in 2011-2012, 5.4 to 8.0 percent of 29 to 32 year-olds had zero family income (depending on the data set), while 9.2 to 12.6 percent had zero earned family income (again depending on the data set).[10]

As in the IOpI literature more generally, most estimates in Tables 1 and 2 are based on the parametric log-linear approach, either in its original formulation (e.g., Ferreira and Grignou 2011) or an extension of it proposed by Björklund, Jäntti and Roemer (2012) that aims to account not just for mean effort heterogeneity but for heterogeneity in effort distributions between types (Hufe and Peichl 2015). The foregoing entails that most available estimates for Denmark and the U.S. do not pertain to the right estimand and populations of interest.

Better parametric approaches exist. An obvious alternative is to substitute income for log income in the left hand of the estimated model, which is how the estimates in Table 2 that we highlighted above (Equalchances Project 2018) were generated. But this has the shortcoming that the predicted values may be negative. For this reason, Mitnik (2020b) argues that a better solution is to estimate an exponential regression model using the Poisson Pseudo-Maximum Likelihood estimator (Santos Silva and Tenreyro 2006). Here we circumvent the problems just discussed, and the substantial risk of not getting the functional form (at least approximately) right with any parametric model, by relying on the straightforward nonparametric approach to produce our IOpI estimates.

[10] The datasets in question are the Current Population Survey and the American Community Survey.

More on selected samples

Besides the unjustified exclusion of people with zero income or earnings from analyses, IOpI studies have often used samples where, for separate reasons, the represented populations are not as relevant as they would ideally be for assessments of IOpI (in many cases, markedly so). Sometimes sample restrictions are plausibly justified and involve trading off relevance for precision. For instance, Suárez Álvarez and López Menéndez (2019) excluded the self-employed because, they argued, income from self-employment is not well measured in the EU-SILC. Sometimes the restrictions are imposed by the data or the methods used. Thus, as explained earlier, any study of IOp for individual earning based on PSID data can only cover household heads and their spouses. As being a household head (or their spouse) is endogenous to own income, the resulting sample does not represent well the full adult population of interest. Similarly, in order to take advantage of a dataset with very rich information on circumstances, Hufe et al. (2017) worked with a sample representing individuals (a) aged 25-30 and with positive earnings in 2010-2012, and (b) born to mothers aged 14–21 in 1978. It follows that the people in the sample were born between 1980 and 1987. But their outcomes in 2010-2012 are not likely to represent well the outcomes of the full 1980-1987 cohorts in their late twenties, because those in the sample were born when their mothers were quite young compared to what is the case for the full cohorts.[11] Lastly, the unusual populations represented in the PSID samples used by Niehues and Peichl (2014; see our Table 1) are a byproduct of the stringent requirements of the novel methods for the (upper-bond) estimation of IOpI that they introduced in their study.

In other cases, however, the reduced relevance of the populations represented by the samples employed seems completely self-inflicted. For instance, several studies in Table 1 that estimate IOp for household equivalent disposable income (or for household disposable income per adult) only include household heads in their samples when they could have also included their spouses (or all adults, depending on the survey). Of course, if all household heads were married, excluding spouses from the sample would not make any difference for estimates given that (a) the income measure is based on household income and (b) all standard inequality measures, including the MLD, satisfy the axiom of "population independence".[12] But many household heads are not married and therefore we can expect that including spouses in the analysis would in fact make a (possibly substantial) difference.

By contrast, the samples for Denmark and the U.S. we use in this article are hardly affected by the type of issues just discussed. As it will be clear after we describe those samples, they represent well the birth cohorts 1972-1975.

The unsettled status of age

Nearly all previous studies of IOpI have computed IOpI measures by taking as outcome variable the annual income (or some other short-run income measure) of a large number of cohorts, e.g., people 25-55

[11] In the sample, those from the 1980 cohort were born to mothers 16-23 years old, those from the 1981 cohort to mothers 17-24 years old . . . and those from the 1987 cohort to mothers 23-30 years old.

[12] This axiom allows comparing inequality in societies of different sizes. It requires that "replicating a society" *X* times so that it becomes *X* times as large, does not change its level of inequality (e.g., Cowell 2011).

years old in the one year (or in the few years) their incomes are measured (see Tables 1 and 2 for examples). This is a very problematic practice. If one could legitimately assume away the existence of age-income profiles, then the fact that different cohorts are observed at very different ages could be simply ignored and the results would pertain to the average IOpI across all cohorts in the population represented by the sample. Assuming away age-income profiles, however, is indefensible, even as a first-order approximation, and the problem arises of how to treat age in the analysis.

Indeed, age is clearly beyond people's control but there are good reasons for not treating it as a circumstance whose effects ought to be compensated for. The reason is that most people experience all ages in question in their lives, so that the effects of age tend to be automatically compensated for over time (more on this below). Of course, with year fixed, age may also be interpreted as indexing cohorts, or groups of cohorts, which have been shown to differ in terms of their opportunities (e.g., Carlson 2008). However, taking the full inequality due to age as reflecting these cohort effects clearly overestimates what a society may need to compensate for. Nevertheless, IOpI scholars have often chosen to include people's age in defining types (e.g. Checchi, Peragine and Sarlenga 2010, 2015; Pistolesi 2009; Suárez Álvarez and López Menéndez 2019), which should tend to overstate IOpI. Unfortunately, the alternative of just ignoring age is also unsatisfactory, as this would still tend to overestimate IOpI if age is correlated with circumstances included in the analyses. This is very likely. For instance, immigration status, an oft-included circumstance, is typically correlated with age, as immigrants are younger than the native population in many countries.

Our solution here is very simple: we make age inconsequential by focusing on four contiguous cohorts observed in the same year (when they are in their late 30s) rather than on a broad population (in terms of cohorts), as most previous studies have done.

Long-run income and lifecycle biases in the estimation of absolute inequality of opportunity

What is the "temporal scope" of the income relevant for empirical analyses of IOpI? In the philosophical literature, it is typically held that "the subject of an egalitarian principle is not the distribution of particular rewards to individuals at some time, but the prospective quality of their lives as a whole, from birth to death" (Nagel 1991:69). Consistent with this position, mobility scholars have long focused on obtaining estimates of long-run economic mobility (e.g., Jenkins 1987; Black and Devereux 2011; Solon 1992; Solon 1999). For this reason, they have put a lot of effort into developing empirical strategies aimed at making this possible given that long-run income measures are typically unavailable and need to be replaced by proxy short-run measures (e.g., Haider and Solon 2006; Nybom and Stuhler 2016; Mitnik 2019, 2020a). In stark contrast, in the vast majority of empirical studies of IOpI, scholars have simply used short-run (e.g., annual) income measures in their analyses, without worrying about the relationship between their estimates and those that would be obtained with long-run (e.g., lifetime) income measures or, alternatively, advancing a positive argument to justify the intrinsic interest of their "short-run estimates."

Our premise here is that empirical studies of IOpI should primarily aim at assessing IOp for long-run income. This requires paying careful attention to the difficulties involved in achieving this goal with the short-run income measures typically available. The methodological research on how to measure economic mobility of the last 30 years offers important clues in this regard. Indeed, this research has developed and empirically validated models of nonclassical measurement error in the short-run income variable with respect to the long-run variable (e.g., Haider and Solon 2006; Nybom and Stuhler 2016; Mitnik 2019, 2020a). In these models, IGE estimates based on short-run income measures taken when children are young are affected by a downward bias whereas those based on measures taken when children are old are affected by an upward bias. The models entail, however, that these "lifecycle biases" tend to disappear when the short-run income measures pertain to specific ages; in addition, the available empirical evidence indicates that this is indeed the case when short-run income is measured close to age 40.

Mitnik (2020b) shows that using a short-run income measure to proxy for long-run income when producing AIOpI estimates leads to similar lifecycle biases. He advances the following nonclassical measurement-error model. Let $Z = \pi_0 Y^{\pi_1} + V$ be a "multiplicative projection" of Z on Y, where Z and Y are short- and long-run income, respectively.[13] As before, let D be the inequality measure used to compute AIOpI, which is assumed to satisfy the very basic axiom of scale independence (which requires it to be invariant to equi-proportional changes of the income variable). The quantity of interest is $D(E(Y|C))$, where C is as defined earlier. Mitnik (2020b) shows that, under the empirical assumption $E(V|c) = 0$, for all c, $D\big(E(Z|C)\big) = D\big(E(Y|C)\big)$ when $\pi_1 = 1$. It follows that AIOpI estimated with the short-run income variable is not affected by lifecycle bias when that is the case. Further, using PSID family-income data for men and women pooled and the same circumstances and estimation method we use in our analyses here, Mitnik (2020b) provides evidence that (a) $\pi_1 \approx 1$ close to age 40, (b) $E(V|c)$ is not much different from zero at all values of C when $\pi_1 \approx 1$, consistent with the model's empirical assumption, (c) measures of AIOpI based on various inequality measures (including those we use here) and short-run income obtained around age 40 are very close to the corresponding AIOpI measures computed with long-run income, and (d) long-run AIOpI is underestimated when income is measured at younger ages and overestimated when measured at older ages.

Our focus in this article on a few contiguous cohorts observed in their late 30s is not due to an intrinsic interest on what happens at those ages. Rather, it is motivated by the analysis of measurement error just summarized, which suggests that our estimates of AIOpI, based on income measures obtained close to age 40, should not be much affected by lifecycle bias.

Lifecycle biases and other issues in the estimation of relative inequality of opportunity

Inequality in long-run income is much lower than what the standard cross-sectional estimates of inequality suggest (e.g., Aaberge and Mogstad 2015; Bjorklund, 1993; Lillard 1977). As the bias varies with the age at which income is measured, following Aaberge and Mogstad (2015) we may also refer to these age-specific biases as lifecycle biases. Using Norwegian register data for men, Aaberge and Mogstad (2015) provide evidence on these biases. They show that computing the Gini coefficient and two other related inequality measures with income measured at younger ages (between 24 and 35) approximates well or overestimates somewhat the corresponding long-run inequality measures (i.e., the same measures but computed with long-run income). However, at older ages the bias is positive and increases monotonically with the measurement age. Similarly, using the same PSID data mentioned above, Mitnik (2020b) finds that, with a large array of inequality measures (although not all he considers), lifecycle bias starts off somewhat negative in the mid-20s, crosses zero soon after that (between ages 27 and 33, depending on the measure),

[13] In direct analogy with a linear projection, the parameters π_0 and π_1 of a multiplicative projection are such that $E(V) = 0$ and $Cov(Y, V) = 0$ (Mitnik 2020b).

and is then positive and increases monotonically with age. He reports, in particular, that the bias crosses zero around ages 32-33 with the Gini coefficient and three Gini-type indices, and around ages 28-29 with the MLD. With other inequality measures the bias is positive at all ages.

The foregoing entails that the estimation of RIOp for long-run income with a short-run income measure is impacted by not fewer than two, and possibly three, biases. The first is the negative bias affecting estimation of AIOpI with partially observed circumstances. The second is the negative (positive) bias affecting estimation of AIOpI when the income measure is obtained earlier (later) than age 37 (age 43), approximately. And the third is the positive (negative) bias affecting estimation of overall income inequality when the income measure is obtained too late (too early) in the lifecycle; in this case, the cutoff age at which the bias becomes positive varies across inequality measures but appears to be always earlier than the range of ages at which lifecycle bias in the estimation of AIOpI can be (mostly) avoided. Therefore, it is not possible to eliminate the last two biases simultaneously by carefully selecting the ages at which income is measured. For instance, estimating RIOpI close to age 40, as we do here, would still produce lower-bound estimates of long-run RIOpI, but the estimates would be pushed down by two biases rather than one: the downward bias affecting the estimation of AIOpI (the numerator of RIOpI) due to the fact that circumstances are partially observed, and the upward lifecycle bias affecting the estimation of overall income inequality (the denominator of RIOpI) too late in the lifecycle (i.e., close to age 40). As we explain later, in our empirical analyses we address the latter bias by introducing post-estimation corrections to our estimates of overall inequality (where the correction factors are computed from an auxiliary dataset).

Producing RIOpI estimates based on the MLD is also affected by an additional, and rather serious, methodological problem. If inequality is measured with this index and the available (i.e., short-run) income measure includes a nonnegligible share of zeros, then (a) overall income inequality, the denominator of RIOpI, cannot be consistently estimated by direct computation on the sample even in the absence of any lifecycle bias (due to the selection bias that results if those with zero income are dropped), and (b) the estimand simply does not exist if the long-run income variable also includes zeros.[14] Moreover, replacing zeros by a "small amount"—e.g., by the value 1, as Checchi et al. (2015) do in some of their analyses—is not a good strategy, as the MLD is very sensitive to the exact amount that is substituted.[15] As our income measures all include a nonnegligible share of zeros, we do not use the MLD to estimate RIOpI.

[14] The MLD is equal to the difference between the logarithm of the expectation and the logarithm of the geometric mean of a distribution. Given that the latter is undefined in the presence of zeros, the MLD is undefined as well when this is the case. Importantly, this means that this problem is relevant even when the goal is to estimate RIOp for income at some particular age rather than RIOp for long-run income. At the same time, the MLD may be unproblematically used to estimate AIOp for either short-run or long-run income because, empirically, mean income within types can be expected to be positive for all types.

[15] Empirical evidence that this is the case, based on the data used in this article, is available from the authors.

From measurement to interpretation

Absolute versus relative inequality of opportunity

As we pointed out in the introduction, IOp scholars have given too much attention in their analyses to results pertaining to RIOpI—sometimes, and most surprisingly, even when conducting cross-country comparisons. Indeed, AIOpI estimates are the ones that are relevant for comparative normative assessments. Let's say we want to compare how countries A and B are doing in terms of IOp for income, and we are shown the MLD-based figures in Table 3 (which we assume are true values rather than estimates). The distribution of income opportunity-set values, or income opportunities for short, in country A is much more egalitarian than in country B (the MLD for the latter country is five times larger). According to the theory of distribute justice motivating the analyses conducted in the IOpI literature, this inequality, the ethically unacceptable or unfair inequality, is the one that needs to be minimized. It immediately follows that country A is doing substantially better (i.e., five times better) than country B in this regard. The fact that RIOpI is twice as large in country A is literally irrelevant for this assessment.

Of course, RIOpI, which tells us what share of overall income inequality in a country is unjust, is an interesting and important descriptive quantity, even if it is not that relevant for cross-country normative assessments. In large part, its importance comes from the fact that arguments positing that a high level of income inequality is normatively unproblematic often imply that this is the case because such inequality is (mainly) the result of differences in effort. Thus, in any given country, RIOpI estimates provide evidence that is relevant for such an argument.[16] For this reason, we do report RIOpI results in this article, although more briefly.

We suspect, however, that RIOpI's intrinsic interest has not always been the main reason for its popularity in the literature. Rather, its appeal has often been that, for analyses relying on the MLD, they add an intuitive way of interpreting individual estimates. The problem is that this interpretation does not pertain to what the normative theory motivating the empirical research is all about but to something else. If providing individual estimates of AIOpI that are easy to interpret is important—and we do think that this is the case—that can be immediately achieved by using the Gini coefficient or other double-bounded measures. Indeed, all double-bounded measures provide an intuitive way of assessing how much unfair inequality there is.[17]

[16] A high RIOpI estimate should count as evidence against such an argument. However, given that RIOpI estimates are lower-bound estimates, the converse is not (necessarily) true, that is, a low RIOpI estimate would not support the argument unless the low estimate is based on a very rich set of very precisely measured circumstances.

[17] With the Gini coefficient, AIOpI can be further interpreted. For instance, it can be interpreted (a) graphically, in terms of the underlying Lorentz curve, and (b) as half the ratio between the mean absolute difference in income opportunities between pairs of randomly chosen people and the average income opportunity in the population.

Inequality of opportunity and cross-country comparisons: Identification assumptions

We have stressed that the fundamental methodological problem of the empirical research on IOp is that AIOp is measured with respect to an incomplete set of circumstances; and that, as a result, empirical estimates of AIOp are lower-bound estimates. Although individual AIOpI estimates may still be informative, comparing estimates across countries and times becomes very challenging. Studies conducting cross-country comparisons and trend analyses have all explained that their estimates are lower-bound estimates. They have not tried, however, to advance explicit identification assumptions that would justify stronger conclusions than that fact alone allows—even though they typically have at the very least flirted with such stronger conclusions.

As in Mitnik et al. (2019), let's stipulate that "modulo *W*" means "computed with respect to the incomplete set of circumstances *W*." Denoting AIOpI modulo *W* by AIOpI (*W*), an identification assumption that would justify the type of analyses found and the conclusions often suggested in empirical studies is this:

> *Fixed-ratio assumption (FRA):* The ratio of AIOpI to AIOpI (*W*) is (approximately) the same across the countries or periods under consideration.

FRA is a very powerful identification assumption. To start, it makes it possible to construe any ranking of countries in terms of AIOpI (*W*) as a ranking in terms of AIOpI. But it allows for much more than this. For instance, it makes it possible to conclude that in country A, AIOpI in 2010 is (approximately) *p* percent higher than in 2004, or that the ratio of AIOpI between countries A and B is (approximately) *q*, where *p* and *q* are computed directly from the AIOpI (*W*) estimates. FRA also allows for unproblematically interpreting linear correlations and nonlinear associations between AIOpI (*W*) and other variables (e.g., overall inequality, GDP per capita) as if they pertained to AIOpI. Lastly, it justifies using AIOpI (*W*) measures in regression models—for instance, models aimed at explaining variation in inequality of opportunity across countries and/or times in terms of institutional factors and fixed effects—and interpreting the qualitative results as pertaining to AIOpI tout court. Unfortunately, FRA is very unlikely to be correct (more on this below).

A weaker identification assumption, meant to be used when AIOpI (*W*) is larger in country or period A than in country or period B, is this:

> *Ratio-inequality assumption (RIA)*. The ratio of AIOpI to AIOpI (*W*) in A is not smaller than the ratio of AIOp to AIOp (*W*) in *B.*

Although less powerful than FRA, RIA is still quite powerful. Let's say that it holds for countries A and B. Then, the AIOpI (*W*) ratio between A and B is a lower-bound estimate of the AIOpI ratio between A and B.[18] Because AIOpI (*W*) is larger in A ex hypothesi, it follows that AIOpI is also larger in A. Moreover, if the estimated AIOpI (*W*) ratio is *q*, RIA allows us to conclude that AIOpI in country A is at least *q* times AIOpI in country B (rather than *q*, as is the case under FRA). Although RIA might seem prima facie plausible in

[18] With subscripts indexing countries, from $AIOpI_A/AIOpI_A(W) \geq AIOpI_B/AIOpI_B(W)$ it immediately follows that $AIOpI_A/AIOpI_B \geq AIOpI_A(W)/AIOpI_B(W)$.

some contexts, it will generally be very difficult to make a case that it applies based on empirical evidence (more on this below).[19]

A still weaker identification assumption, which nevertheless makes it possible to rank countries or periods and to carry out comparative quantitative assessments, is the following:

> *Inequality assumption for the lower bound (IALB)*.[20] The difference between AIOpI and AIOpI (*W*) in country or period A is not smaller than the difference between AIOpI and AIOpI (*W*) in country or period *B*.

Let's say that IALB holds for countries A and B. If AIOpI (*W*) is larger in A, then AIOpI is larger in A as well, that is, like the previous identification assumptions, IALB also allows to rank countries; moreover, the difference in AIOpI (*W*) between A and B is a lower-bound estimate of the AIOpI difference between those countries.[21]

Although it is useful to know the lower bound of the difference in AIOpI between two countries or times, the meaning of such a difference varies depending on the AIOpI levels in those countries. Whereas an AIOpI difference of 0.1 may be unimpressive if AIOpI is 0.9 in country A and 0.8 in country B, it may be quite impressive if AIOpI is 0.1 and 0.2, respectively. Fortunately, IALB also allows researchers to generate a lower bound estimate of the proportional difference in AIOpI between the two countries. In fact, in contexts in which overall income inequality and AIOpI (*W*) are both larger in country or period A than in country or period B, IALB may be combined with a second identification assumption to generate a proper (i.e., finite-length) set estimate of the proportional difference in AIOpI—or, equivalently, of the AIOpI ratio—between A and B. So, rather than explaining how AIOpI may be used to generate a lower-bound estimate of such proportional difference, we will subsume that explanation in our explanation of how to generate a proper set estimate of it.

The identification assumption just mentioned is the following:

> *Inequality assumption for the upper bound (IAUB)*. The difference in overall income inequality between countries or periods A and B is not smaller than the corresponding difference in AIOpI.

If IAUB holds for countries A and B, the difference in overall income inequality between A and B is an upper-bound estimate of the AIOpI difference between those countries. So, if IALB and IAUB both hold, we may use estimates of income inequality and AIOpI (*W*) in A and B to generate a set estimate of the additive difference in AIOpI between A and B. But this will not do if what we are after is an estimate of a proportional difference, that is, an estimate of the AIOpI ratio between two countries.

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[19] In some cases, it may be possible to show that RIA does not apply (more on this below as well).

[20] The reason why we chose this name for the assumption becomes fully clear below.

[21] From $AIOpI_A - AIOpI_A(W) \geq AIOpI_B - AIOpI_B(W)$ it immediately follows that $AIOpI_A - AIOpI_B \geq AIOpI_A(W) - AIOpI_B(W)$. IALB is weaker than RIA because RIA entails $(AIOpI_A - AIOpI_B)$ / $AIOpI_B >$ $(AIOpI_A(W) - AIOpI_B(W))$ / $AIOpI_B(W))$ and $AIOpI_B \geq AIOpI_B(W)$. So, RIA entails IALB.

Without any identification assumption, point estimation of the AIOpI ratio between two countries is impossible. Nevertheless, the ratio is partially identified even in this context. We may set estimate it by (a) using the AIOpI (W) estimate in each country as its lower-bound estimate of AIOpI, (b) using the estimate of overall income inequality in each country as its upper-bound estimate of AIOpI, and (c) computing a set estimate of the AIOpI ratio between the two countries by combining the previous estimates in an appropriate way. However, even with large samples, this set estimate based "on the data alone" (e.g., Manski 2003) may be very wide and even fully uninformative (if it covers the value 1). When applicable, IALB and IAUB will allow IOpI scholars to produce not just an informative but, typically, also a much tighter set estimate of the ratio of interest.

Figure 1 shows why this is the case. In the example represented in the figure, the true overall income inequality and AIOpI (W) are, respectively, 0.5 and 0.2 in country A and 0.3 and 0.1 in country B. The left panel of the figure shows what is identified without any identification assumption. For the AIOpI of each country, the set of values consistent with the population data, or "identified set" (e.g., Manski 2003), is indicated by the thicker line segment in the corresponding country axis. The light-gray rectangle represents all pairs of AIOpI values (one for country A and one for country B) consistent with the identified sets for both countries. As each such pair maps into one AIOpI ratio, the same area also represents the identified set of AIOpI ratios. The minimum and maximum values of this latter set are at the upper-left and bottom-right vertices of the rectangle. Set estimating the AIOpI ratio without making any identification assumption is straightforward. The upper-bound estimate is the upper-bound AIOpI estimate for country A divided by the lower-bound AIOpI estimate for country B, whereas the lower-bound estimate is the lower-bound AIOpI estimate for country A divided by the upper-bound AIOpI estimate for country B. In the example, and assuming that the four underlying estimates are based on consistent estimators, the probability limit of the set estimator is [0.67, 5]. In this context, and regardless of sample size, set estimates will be fully uninformative in the sense that they will not tell us even whether AIOpI is larger in country A or B. Empirically, this is a very common situation.

The right panel of Figure 1 incorporates the information provided by IALB by adding an "IALB line": the locus of the pairs of AIOpI values (for countries A and B) whose difference is equal to the AIOpI (W) in country A minus the AIOpI (W) in country B. It also incorporates the information provided by IAUB by adding an "IAUB line": the locus of the pairs of AIOpI values (for countries A and B) whose difference is equal to the overall income inequality in country A minus the overall income inequality in country B. Under IALB and IAUB, the pairs of AIOpI values below the IALB line and the pairs of AIOpI values above the IAUB line are not part of the identified set because they do not satisfy IALB and IAUB, respectively. The AIOpI ratio remains only partially identified. But the identified set of AIOpI ratios is substantially smaller than (and a proper subset of) the corresponding identified set in the left panel. In the example, the new identified set is still a parallelogram (although no longer a rectangle) and its minimum and maximum values are again at the upper-left and bottom-right vertices.[22] The probability limit of the set estimator is now [1.33, 3], compared to [0.67, 5] with no identification assumption. This means that, asymptotically, a set estimate

[22] Under IALB and IAUB, the identified set may not be a parallelogram. This happens, for instance, if the IALB and IAUB lines cross the top horizontal dashed line between the two vertical dashed lines.

will be highly informative: it will indicate that AIOpI is no less than 33 percent higher in country A than in country B, and 200 percent higher at most.[23]

Our analysis of how to generate proper set estimates has assumed consistent estimation of overall income inequality. This is unproblematic if the focus is on the AIOp in short-run income (as it has implicitly been the case in almost all the literature). In this article, however, our focus is on the AIOp in long-run income. As we pointed out earlier, the estimation of long-run inequality will be typically subject to an upward lifecycle bias. When this is the case, the analysis above is still correct if the proportional asymptotic bias of the estimator of inequality is not smaller for country A than for country B. This follows directly from the fact that the bias is upward. Nevertheless, there is a cost to substituting upward-biased estimators for consistent estimators of long-run inequality: the probability limit of the set estimator of the AIOpI ratio is wider than what would be the case with consistent estimators. If information about the size of the biases is available from previous empirical studies or can be obtained from auxiliary data, it becomes possible to correct the estimates of long-run inequality so as to approximate the (maximally informative) set estimates that would be obtained with consistent estimators of overall inequality.

A two-circumstance model of partial-observability bias

We contend that, given the circumstances we include in our analyses, IALB and IAUB apply with the U.S. as country A and Denmark as country B. The simple two-circumstance model we introduce next identifies the factors generating asymptotic bias in the estimation of AIOpI due to the partial observability of circumstances and provides a decomposition of overall income inequality into AIOpI and ethically acceptable inequality. We use this analysis in the next section to articulate our empirical argument for the applicability of IALB and IAUB. The model also helps understand why RIA is very difficult to assess empirically and, further, why FRA is very likely to be false (regardless of context).

Let the true income generating process be:

$$Q_i = \gamma_0 + \gamma_1 X_{1i} + \gamma_2 X_{2i} + \varepsilon_i \qquad (1)$$

$$Y_i = \omega\, Q_i, \qquad (2)$$

where X_1 is an indicator variable for high parental income, X_2 is an indicator variable for high parental education, ε is effort, Y is individual earnings or family income (before or after taxes and transfers), and Q is an univariate index of people's capitals, that is, an index of their human, social, and cultural capitals as well as any other personal attribute or relationship that is valued or provides an advantage in the labor market (when Y is earnings) or in both the labor and marriage markets (when Y is family income). The parameters γ_1 and γ_2 are the partial effects of the circumstances on people's capitals, ω is the return to people's capitals

[23] The fact that the set estimate is informative (or, equivalently, that the difference in AIOpI across countries A and B is positive) is, as we explained earlier, entailed by IALB itself. In other words, IALB guarantees that the set estimate will be informative. How informative it is, however, is still an empirical matter).

(in terms of individual earnings or family income), ε has mean zero and is mean independent of X_1 and X_2 (jointly), and we assume without loss of generality that $E(Y) = 1$.[24]

Substituting equation (1) into equation (2) and relying on the squared coefficient of variation to measure AIOpI, the true AIOpI is:

$$AIOpI = \frac{Var\big(E(Y|X_1,X_2)\big)}{\big[E\big(E(Y|X_1,X_2)\big)\big]^2} = (\omega\,\gamma_1)^2 Var(X_1) + (\omega\,\gamma_2)^2 Var(X_2) + 2\,(\omega)^2\,\gamma_1\gamma_2 Cov(X_1,X_2), \qquad (3)$$

where we have used that $E\big(E(Y|.)\big) = E(Y) = 1$ (here the dot represents any set of conditioning variables). Parental education is not observed. The expected income of the two observed types (people with high and low parental income) may be written as:

$$E(Y|x_1) = \theta_0 + \theta_1 x_1, \qquad (4)$$

where $x_1 = 0$ or 1. The measured AIOpI, AIOpI (W), with $W = \{X_1\}$, is then:

$$AIOpI\ (W) = \frac{Var\big(E(Y|X_1)\big)}{\big[E\big(E(Y|X_1)\big)\big]^2} = (\theta_1)^2 Var(X_1). \qquad (5)$$

Using the omitted-variable formula, equations (1), (2) and (4) entail $\theta_1 = \omega\gamma_1 + \omega\gamma_2\ \frac{Cov(X_1,X_2)}{Var(X_1)}$. Substituting this expression into equation (5) yields:

$$AIOpI\ (W) = (\omega\gamma_1)^2 Var(X_1) + (\omega\gamma_2)^2\big(\ Corr(X_1,X_2)\big)^2 Var(X_2) + 2\,(\omega)^2\gamma_1\gamma_2\ Cov(X_1,X_2). \quad (6)$$

Subtracting equation (6) from equation (3), we obtain:

$$AIOpI - AIOpI\ (W) = (\omega)^2(\gamma_2)^2\ Var(X_2)\left[1 - \big(Corr(X_1,X_2)\big)^2\right]. \qquad (7)$$

Equation (7) provides a very simple expression for the difference between $AIOpI$ and $AIOpI\ (W)$, that is, the additive asymptotic bias in the estimation of AIOpI due to the partial observability of circumstances. The equation shows that this bias is larger when (a) the return to people's capitals, ω, is larger, (b) the partial effect of the unobserved circumstance on people's capitals, γ_2, is larger, (c) the dispersion of the unobserved circumstance, $Var(X_2)$, is larger, and (d) the correlation between the observed and unobserved circumstances, $Corr(X_1, X_2)$, is smaller. Importantly, with indicator variables the variance is largest when the prevalence of the indicated attribute in the population is 50 percent (i.e., the indicator variable has mean 0.5), and falls monotonically for both larger and smaller prevalence rates. In the next section we use these results heuristically, to help us make the case for why IALB applies to the comparison between the U.S. and Denmark.

Dividing now equation (6) by equation (3) gives an expression for the proportional asymptotic bias in the estimation of AIOpI due to the partial observability of circumstances:

$$\frac{AIOpI}{AIOpI\ (W)} = \frac{F + G\ Corr(X_1,X_2) + H}{F + G\ Corr(X_1,X_2) + H\ \big(Corr(X_1,X_2)\big)^2}, \qquad (8)$$

[24] This entails no loss of generality because it can always be achieved by changing the monetary units used to measure income.

where $F = (\gamma_1)^2 Var(X_1)$, $G = 2\,\gamma_1\gamma_2\, SD(X_1)\, SD(X_2)$, $H = (\gamma_2)^2 Var(X_2)$ and SD is the standard-deviation operator. FRA assumes that the ratio on the right hand of equation (8) is (approximately) the same for the countries or periods A and B being compared, which seems exceedingly unlikely regardless of context. RIA assumes the ratio is larger in country or period A than in country or period B. Equation (8) suggests that making an empirical case that RIA applies in any specific comparison will be, at best, a daunting task. In addition to the fact that the equation does not lend itself to any straightforward analysis (because of its structure), the return to people's capitals (ω) plays no role in it; this means that well-known differences in returns across countries cannot be part of the case for RIA.

Finally, from (1), (2) and (3):

$$\frac{Var(Y)}{[E(Y)]^2} = (\omega\,\gamma_1)^2 Var(X_1) + (\omega\,\gamma_2)^2 Var(X_2) + 2\,(\omega)^2\,\gamma_1\gamma_2 Cov(X_1, X_2) + (\omega)^2 Var(\varepsilon)$$

$$= AIOpI + (\omega)^2 Var(\varepsilon). \qquad (9)$$

Equation (9) shows that total income inequality may be additively decomposed into two terms: (a) AIOpI, and (b) the product of the dispersion of effort and the square of people's returns to capitals. The second term is the inequality deemed ethically acceptable by the luck-egalitarian approach when the inequality measure is the squared coefficient of variation. In the next section we use this result heuristically, to help us make the case for why IAUB applies to the comparison between the U.S. and Denmark.

The case for the applicability of IALB and IAUB to the comparison between the U.S. and Denmark

Equation (7) motivates a general and crucial argument for the applicability of IALB to the comparison at hand: the additive bias will be larger in the U.S. than in Denmark because the returns to people's capitals, both in terms of individual earnings and family income, are substantially larger in the U.S.

Earnings. It is well known that Denmark's labor-market institutions compress the earnings distribution—in particular, by propping up pay in low-skill and mid-skill jobs—much more than their U.S. counterparts (e.g., Jaumotte and Osorio Buitron 2015; Pontusson et al. 2002; Rueda 2015). The fact that public-sector employment as a share of all employment is nearly twice as large in Denmark than in the U.S.—28 percent compared to 15 percent in 2017 (OECD 2019a:85)—further enhances this effect of labor-market institutions (e.g., Oesch 2014; Pontusson et al. 2002). Cross-country differentials in the returns to people's capitals are the micro-level concomitants of the dispersion differential at the level of overall earnings distributions. Thus, human-capital premiums are substantially larger in the U.S. than in Denmark (Broecke 2016: Figure 2; OECD 2017: Chart A6.1; Peracchi 2006: Table 6; Hanushek and Woessmann 2011: Figure 2.10). The earnings returns to social and cultural capital may also be expected to be larger in the U.S. because Denmark's compressed earnings distribution and much lower between-workplace inequality (Tomaskovic-Devey et al. 2020: Fig. 1) strongly suggest that the monetary rewards for the winners in the competition for better jobs at different skill levels are lower in Denmark.[25]

[25] On average across the economy, social and cultural capital are also likely to be somewhat less consequential for the competition for jobs itself in Denmark. This is so because of (a) Denmark's larger share of public-sector employment, and (b) the fact that hiring, compensation and promotion are governed to a

Family income. Earnings are, by far, the main source of income for our populations of interest, and people with similar capitals tend to marry each other. Unless there is much less association between spouses' capitals in the U.S. than in Denmark, the family-income returns, before taxes and transfers, to people's capitals should also be substantially larger in the U.S. Theoretical arguments and empirical evidence show that spouses' similarity increases with wage and income inequality (see Fernández et al. 2005 and the literature reviewed by Schwartz 2013:455-456), and there is direct evidence that educational homogamy is higher in the U.S. than in Denmark (Eika et al. 2014: Figure 2; Fernández et al. 2005: Table 1; Monaghan 2015: Table 2).[26] We may then conclude that the returns to people's capitals in terms of pre-tax-and-transfers family income are indeed larger in the U.S.[27] Moreover, these cross-country differentials will be much greater with after-tax-and-transfers family income. This is the case because the welfare state is substantially more expansive and generous in Denmark than in the U.S. (e.g., Kenworthy 2020d) and taxes and transfers reduce household income inequality by 66 percent more in Denmark than in the U.S. (see Gornick and Milanovic 2015: Figure 1).

Equation (7) and the much larger U.S. earnings and family-income returns to people's capitals justifies our general presumption that IALB applies in the comparison between the U.S. and Denmark. As we explain next, other differences across the two countries further strengthen the case for the applicability of IALB.

The literature has considered four main types of circumstances: (a) gender; (b) race, ethnicity, immigration status and other "origin circumstances"; (c) circumstances concerning the characteristics of the families where people are raised, like parental income, parental education, parental class, and family structure; and (d) circumstances concerning the place of residence when growing up. Let's call these circumstances "primary." There are, of course, many circumstances that do not belong to any of those four groups. But they may be expected to have quite small partial effects, to vary little within types defined by all primary circumstances (the conceptual equivalent, when there are many circumstances, to the squared

larger extent in this sector than in the private sector by bureaucratic provisions—formal rules, regulations, and standards—that reduce managerial autonomy and constrain discretionary decision-making (compared to what is the case in private organizations).

[26] Note that what is relevant for our argument is the actual similarity across spouses rather than underlying preference parameters (e.g., Logan et al. 2008) or assortative-mating measures that "net out" differences in marginal distributions (e.g., Eika et al. 2014).

[27] Other, less central, factors to consider are differences across the two countries in (a) the association between the probability of marriage and people's capitals, and (b) the probability that married women will drop out of the labor force when spousal earnings are high. Analyses by Helsø (forthcoming) and Mitnik et al. (2015) suggest a larger association between capitals and marriage probability in the U.S., which reinforces our argument. They also suggest that although a larger share of U.S. married women drop out of the labor force when spousal earnings are high, this is largely driven by much higher spousal earnings at the top of the U.S. distribution, so our conclusion should not be affected.

correlation being close to 1 in the two-circumstance model), or both. For this reason, in what follows we focus on the primary circumstances alone.

In our empirical analyses, we only use gender and one family-level circumstance, parental-income rank, to define types. We advance three arguments regarding the remaining primary circumstances. The first argument is that e*xcluding place of residence (when growing up) from the analysis biases down AIOpI estimates for the U.S. more than for Denmark.* Measures of intergenerational income mobility and persistence, defined in terms of national-level income ranks, vary substantially more across places in the U.S. than in Denmark (Eriksen and Munk 2020).[28] For instance, the average income rank of people raised by families at the 25th percentile of the parental income distribution is much more geographically heterogenous in the U.S. One key reason is that the importance of place of residence for adult outcomes increases with socioeconomic residential segregation (e.g., Durlauf 1996), which is lower in Denmark.[29] Also important for our argument, not only does average income rank (conditional on parental-income rank) vary more across places in the U.S. but, in agreement with our earlier analyses of the returns to people's capitals, the differences are more consequential for monetary economic outcomes. This is the case because the income distribution is more unequal in the U.S., so that monetary differences between any two ranks are larger in the U.S. than in Denmark.

The second argument is that e*xcluding origin circumstances from the analysis biases down AIOpI estimates for the U.S. more than for Denmark.* Race (e.g., Alexander 2010) as well as immigration and ethnicity (e.g., National Academies 2017: Ch. 2) have played an extraordinarily central role throughout U.S. history, and there are extensive present-day differences in economic outcomes across racial and ethnic groups and between natives and immigrants within the country (e.g., Chetty et al. 2020a; Duleep and Dowhan 2008; National Academies 2017:Ch. 3; Villareal and Tamborini 2018). Moreover, substantial differences in average income ranks between these groups are observed even after conditioning on parental-income rank (Chetty et al. 2020a). By contrast, despite increased immigration into Denmark in more recent times, the country is still highly homogenous in terms of its racial/ethnic composition compared to the U.S., a fact reflected in various indices of ethnic fractionalization (Alesina et al. 2003; Drazanova 2019; Fearon 2003; Patsiurko et al. 2012).[30] Therefore, even though in Denmark there also are important differences in economic outcomes between natives and immigrants (e.g., Brodmann and Polavieja 2011; Felbo-Kolding et al. 2019), it seems safe to conclude that omitting origin circumstances has a larger impact on the measured AIOpI in the U.S. In terms of the analysis in the two-circumstance model, this conclusion relies on the fact that both the prevalence (and therefore the variance) of "minority origin" and the returns to capitals are much larger in the U.S. while there is no reason to believe that differences in the partial effect of minority origin or

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[28] The same is true for the U.S compared to Sweden (Heidrich 2017).

[29] Income segregation among schools, which is a good proxy for socioeconomic residential segregation, is much lower in Denmark (Chmielewski and Reardon 2016: Figure 4).

[30] For instance, the U.S. has a value of 0.53 whereas Denmark has a value of 0.18 in the Historical Index of Ethnic Fractionalization (HIEF) for 2013 (Drazanova 2019).

the correlation of the latter with parental-income rank across countries is an important counteracting factor (or even a counteracting factor at all).

The third argument is that *excluding all family circumstances other than parental-income rank from the analysis biases down AIOpI estimates for the U.S. more than for Denmark.* The extent to which family circumstances contribute to the generation of inequality in people's economic outcomes depends on a country's institutions and policies. As we already discussed, some of these institutions and policies affect the earnings and family-income returns to people's capitals. Others affect inequalities in expected capitals across people with different circumstances.[31] Compared to the U.S., Denmark reduces inequalities in people's expected capitals because (a) it invests much more heavily on early education and on job training, job placement and other active labor-market policies, and (b) as already pointed out, it has put in place more generous and more expansive public-insurance programs (e.g., Kenworthy 2020d). These policy differences between the two countries make a large array of family-of-origin disadvantages less consequential for people's capitals in Denmark, their prevalence (or the prevalence of severe forms thereof) less common in Denmark, or both. To keep the discussion manageable, we will exemplify our argument with the role played by investments on early education and active labor-market policies with respect to two important family circumstances: parental education and being raised by a single parent ("single parenthood" for now on).

The partial effects of parental education and single parenthood arise largely from the fact that these circumstances affect the development during childhood of cognitive and noncognitive skills (in a broad sense of the term) that are highly consequential for people's school performance and adult economic outcomes (e.g., Bloome 2017; Heckman and Mosso 2014; McLanahan and Percheski 2008). Although parenting differences across families are unavoidable, they become less consequential the more other agents are involved in the process of early skill formation. In Denmark, high-quality pre-school care and education are publicly provided and there is near-universal participation; in the U.S., the system is mostly based on market-based providers of very heterogenous quality, most care is of low or moderate quality, and participation is far from universal and picks up speed at a later age (e.g., Esping-Andersen 2004; Esping-Anderson et al. 2012; Kenworthy 2020a). As a result, parental education and single parenthood have larger effects on the early formation of skills in the U.S. than in Denmark.[32]

Similarly, the extent to which single parenthood and low parental education negatively affect the amount of human capital in adulthood is not only determined by their effects on people's early skills and formal educational attainment but also by how likely it is that those with low educational attainment will

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[31] From equations (2) and (3), we may write $AIOpI = \omega^2\ Var\big(E(Q|X_1, X_2)\big)$. Specific policies and institutions may affect ω, $Var\big(E(Q|X_1, X_2)\big)$ or both.

[32] The early-education policies implemented by Denmark and other social-democratic countries appear to dampen parenting effects mainly by improving the skills of children from disadvantaged homes (e.g., Kenworthy 2020a). For direct evidence for Denmark and the U.S. based on 2003 PISA scores (for students age 15), see OECD (2004). Information on "single-parenthood score penalties" in math in the two countries is available in Table 4.2e; similar information on "low-maternal-education score penalties" in math, reading and science can be read off from the figures reported in Table 4.2b. Penalties are markedly lower in Denmark in all cases.

have the resources and the incentives to acquire new in-high-demand skills over their working lives (and especially when technological and industrial changes lead to extensive job destruction and creation). The more likely this is, the smaller the negative impacts of single parenthood and low parental education on adult human capital will be. In 2000-2017, average public spending on job training, job placement and other active labor-market policies as a share of GDP was 13 times larger in Denmark than in the U.S., 1.7 percent versus 0.13 percent (OECD 2020).[33] This reinforces the conclusion that those two circumstances may be expected to have lower partial effects in the former country than in the latter.

The variances of the two circumstances are not a counteracting factor. If anything, the opposite is the case: when parental education is measured categorically, its distribution is very similar in the two countries (OECD 2014: Table A4.1a), while years of parental education is more dispersed (Hertz et al. 2007) and the prevalence of single parenthood is higher in the U.S. than in Denmark (Case and Maldonado 2012; Pew Research Center 2019).[34] Finally, there is no reason to expect cross-country differences in the correlation between parental income rank and parental education or single parenthood to be an important countervailing factor.

So far, we have made the case that IALB applies with the U.S. as country A and Denmark as country B. We now make the case for why IAUB also applies to the comparison between these countries. The argument boils down to the fact that we expect "within-type inequality"—the inequality due to differences in effort—to be larger in the U.S than in Denmark simply because people's returns to capitals are larger in the U.S. The argument is easily formalized when the inequality measure is the squared coefficient of variation. Using equation (9), we may write:

$$I_{US} - I_{DK} = AIOpI_{US} - AIOpI_{DK} + (\omega_{US})^2 Var(\varepsilon_{US}) - (\omega_{DK})^2 Var(\varepsilon_{DK}), \qquad (10)$$

where I is overall income inequality and the subscripts denote countries. IALB implies $AIOpI_{US} \geq AIOpI_{DK}$. As the returns to people's capital are substantially larger in the U.S. and there is no reason to expect the dispersion of effort to be larger in Denmark (in fact, Landersø and Heckman [2017] have advanced arguments entailing the opposite), we expect $(\omega_{US})^2 Var(\varepsilon_{US}) > (\omega_{DK})^2 Var(\varepsilon_{DK})$. Therefore, $I_{US} - I_{DK} > AIOpI_{US} - AIOpI_{DK}$, as posited.

[33] These figures are based on spending on training, employment incentives, public employment services and administration, and sheltered and supported employment and rehabilitation. In 2017, this spending was 1.97 percent in Denmark and 0.09 percent in the U.S., i.e., 22 times higher in Denmark.

[34] For people ages 20-34 in 1998 (Denmark) and 1994-2000 (U.S.), grouped in five-year cohort groups, Hertz et al. (2007: Supplement Dataset) report standard deviations for years of parental education in the 1.82-2.01 range in Denmark and in the 2.53-2.69 range in the U.S. The share of children under age 18 living with one parent and no other adult has been reported as 18 percent in Denmark in 2007 and 27 percent in the U.S. in 2011 (Casey and Maldonado 2012) and 17 percent in Denmark in 2014 and 23 percent in the U.S. in 2010-2016 (Pew Research Center 2019).

Equation (10) assumes consistent estimation of overall income inequality. When the focus is on AIOp in long-run rather than short-run income, we need to account for the upward lifecycle biases discussed earlier. This may be achieved by modifying equation (10) as follows:

$$(1 + r_{US})I_{US} - (1 + r_{DK})I_{DK} = AIOpI_{US} - AIOpI_{DK} + (\omega_{US})^2 Var(\varepsilon_{US}) - (\omega_{DK})^2 Var(\varepsilon_{DK}) + r_{US}I_{US} - r_{DK}I_{DK}, \quad (11)$$

where r_{US} and r_{DK} capture the proportional asymptotic biases in the estimators of inequality for the U.S. and Denmark, respectively, and the left-side of the equation reflects what is actually being estimated by those estimators. Our argument is still valid if $r_{US} \geq r_{DK} \geq 0$. Here we assume that $r_{US} \approx r_{DK} > 0$. This assumption is based on the fact that our income measures pertain to exactly the same ages in both countries, and that we expect the bias to be positive at those ages.

Data and variables

For Denmark, our analyses are based on administrative register data, which cover the full Danish population in 1980-2015. For the U.S., our analyses are based on the Statistics of Income Mobility (SOI-M) Panel (Mitnik et al. 2015), which represents all people born between 1972 and 1975 who were living in the U.S. in 1987. The SOI-M Panel was built from U.S. tax returns, W-2 forms, and other administrative sources. For both countries, the samples employed pertain to people who were 35-38 years old in 2010.[35] We use information on their (a) gender, (b) total and disposable family income, (c) total and disposable family income per adult, and (d) individual earnings, all measured in 2010. We also use information on (e) the average disposable family income of people's parents when those in the sample were 15-23 years old.[36]

In the U.S. data, due to differences in data availability, the income concepts are not measured identically for the people in the sample and their parents, but the differences are only minor. The measure of annual parental total income in the SOI-M Panel is the sum of (a) pre-tax "total income" in Form 1040 (which includes labor earnings, capital income, unemployment insurance income, and the taxable portion of pensions, annuities, and social security income), and (b) nontaxable interest. For people who filed taxes in 2010, total income is very similar; the only difference is that it also includes nontaxable earnings. For nonfilers in that year, total income is the sum of earnings from the W-2 form and unemployment insurance (UI) income from the 1099-G form, as long as at least one of them were available (see Chetty et al. 2014 for a further discussion of this approach). For those for whom both W-2 and UI information were unavailable, the SOI-M Panel includes a set of imputed income variables, which we use here; these variables are based on data from the Annual Social and Economic Supplement of the Current Population Survey (CPS-ASEC) on

[35] The sample for Denmark replicates as much as possible the approach used in the SOI-M Panel to assign parents to people and to define families (for that approach, see Mitnik et al. 2015:16-19) as well as the represented population (i.e., the sample for Denmark also excludes those not living in the country in 1987).

[36] For the sake of readability, in what follows we will often refer to our income concepts as "total income," "disposable income," "total income per adult," "disposable income per adult," and "earnings."

likely nonfilers without UI income or earnings (see Mitnik et al. 2015:28-31).[37] After-tax income is computed by subtracting out net federal taxes (which include refundable credits) from total income, and this is what we use as our measure of disposable income; as state taxes are not excised from this measure, and some non-taxable transfers are not included (e.g., Temporary Assistance for Needy Families), it follows that this is an approximation to true disposable income.[38] Total (disposable) income per adult is equal to total (disposable) income when the person is single and total (disposable) income divided by 2 when the person is married (where marital status is based on filing status).[39] Earnings are the sum of W-2 wages and 65 percent of self-employment income when positive (the other 35 percent is assumed to be the return to capital).

In the Danish data, total income is the sum of labor earnings (including 100 percent of self-employment income), capital income, and unemployment insurance. Disposable income is the sum of total income, public transfers and other third-party reported income, minus taxes paid on income. As in the U.S. data, earnings include 65 percent of self-employment income. Total and disposable income per adult are computed as in the U.S. data.

The samples used for our analyses exclude people with (a) more than 3 years of missing parental information, and (b) nonpositive average parental income. In analyses where total or disposable income is the outcome of interest, the samples also exclude people with negative total or disposable income. In Table 4, we provide demographic and income statistics for the samples. We express all income variables in 2010 dollars using the Consumer Price Index for Denmark and the Consumer Price Index for Urban Consumers - Research Series (CPI-U-RS) for the U.S.; we further transform Danish krones into U.S. dollars using a purchasing power parity exchange rate computed by the Eurostat-OECD PPP Programme (see OECD/Eurostat 2012).

For auxiliary purposes, we also use aggregate tax-based U.S. statistics that Chetty et al. (2020b) have made publicly available. The microdata underlying those statistics represent the birth cohorts 1978-1983. Here, people's income is their average income in 2014-2015, when they were between 31 and 37 years old, and their parental income is measured by averaging five years of information when they were between 11 and 22 years old. In both cases, income refers to pretax family income, which is very similar to our notion of total income. We use information on people's income rank in 2014-2015 by gender, parental income percentile bin and race/ethnicity. We combine this information with a "crosswalk" provided by Chetty et al. (2020b) that maps income rank into real income in 2015 dollars and employ the resulting values to compute some auxiliary quantities we use in one section of our article. The race/ethnicity categories are Hispanic,

[37] In some contexts, the imputed income variables included in the dataset can be used to compute point estimates and confidence intervals using the standard approach for multiple imputation (e.g., Little and Rubin 2002). Here, however, we only use those variables and the standard approach to compute point estimates. For statistical inference, see below.

[38] As explained in detail later, in our empirical analyses we introduce adjustments aimed at accounting for the approximate nature of our U.S. measure of disposable income.

[39] For nonfilers (both with and without other administrative information), the SOI-M Panel includes a set of imputed marriage-status variables that are also based on data from the CPS-ASEC.

White, Black, Asian, American Indian or Alaskan Native, and Other (the last five only include non-Hispanics).

Estimation and statistical inference

We use the nonparametric approach (e.g., Checchi and Peragine 2010) to estimate IOpI. This simply involves (a) defining types, (b) computing mean income within types, (c) assigning type-specific means to people, and (d) computing an inequality measure (e.g., Gini, MLD) over the resulting distribution. We define 100 types by combining gender and parental income "fiftiles" (fiftiles are like quintiles but each includes 2 percent of the population instead of 20 percent); we compute fiftiles of parental disposable income. When we estimate IOpI separately for men and women, types are defined exclusively in terms of parental income fiftiles.

We use only gender and parental income to define types both because there is not much additional information in the SOI-M Panel that could be used for this purpose, and because it is unlikely that we could include additional variables and still rely on the nonparametric approach given the size of our SOI-M Panel sample. With this sample, we use sampling weights to produce all estimates.

Statistical inference is based on the nonparametric bootstrap with 2,000 repetitions. With the U.S. data, we carry out multiple imputation—of the income variables for nonfilers without any administrative information and of marriage status for all nonfilers—within the bootstrap procedure. We use the same source of data and approach employed in the SOI-M Panel to generate imputed variables (see Mitnik et al 2015: 28-31) to re-impute them within each bootstrap sample. This way the reported uncertainty reflects not only sampling variability but also the additional variability generated by multiple imputation.

When we provide point estimates, including point estimates of quantities representing lower bounds, we report bias-corrected bootstrap confidence intervals (e.g., Efron 1987). When we provide set estimates of partially identified quantities, we report confidence intervals for the actual quantities of interest, not for the identified sets. To this end, we use the approach advanced by Imbens and Manski (2004).

Results

Absolute inequality of opportunity for income in the U.S. and Denmark

We present our main results about AIOpI in Denmark and the U.S. in Tables 5 and 6 and Figures 2, 3 and 4. Table 5 shows our lower-bound estimates of AIOp for individual earnings and for total and disposable family income (overall and per adult), both for the full population and for men and women separately. In these analyses, we use the Gini coefficient and the MLD as inequality measures. Table 6 reports Gini-based adjusted estimates of AIOpI for disposable income in the U.S., whose discussion we will postpone to the next subsection.

In Figures 2, 3 and 4, taking advantage of the fact that our types are defined by only two variables, we offer graphical representations of how income opportunities (that is, the value of income opportunity sets) vary across types in Denmark (left panel of each figure) and the U.S. (right panel of each figure).[40] In Figure

[40] More precisely, the figures offer graphical representations of how *mean* income opportunities (across true types) vary across *the types distinguished in our research* (each of which includes many true types) in

2, the 100 dots in each panel represent the earnings opportunities of men and women at different fiftiles of parental income. To facilitate the interpretation of the figure, we have superimposed nonparametrically estimated curves summarizing the relationship between opportunities and parental fiftiles by gender. Figures 3 and 4 each have information pertaining to two income notions rather than one. So, to avoid cluttering, we have only included the summary nonparametric curves in those figures.

Figure 2 indicates that, in both countries, mean earnings (that is, the value of earnings opportunity sets) increase with parental fiftile, regardless of gender. This association is more marked in the U.S. than in Denmark, especially for men at the top and women at the bottom of the parental income distribution. In the U.S., the difference between men's and women's mean earnings is very small at the bottom of the parental income distribution but increases rapidly and becomes quite large in the top parental decile and extraordinarily large at the very top of the distribution. By contrast, although in Denmark the curves for men and women also diverge more at the top of the parental income distribution, the distance between them is much closer to being constant across parental fiftiles. A useful way of summarizing how earnings opportunities vary across types in each country is by computing the average "opportunity return" to parental income fiftile, which comes at about $800 and $400 for Danish men and women, respectively, but is as much as $2,000 and $600 for U.S. men and women, even when mean earnings are about 16 percent larger in Denmark (see Table 4).[41] This means that the annual-earnings opportunity set of U.S. men born at the top fiftile is worth about $100,000 more than the opportunity set of men born at the first fiftile, while the corresponding difference for Danish men is about $40,000.

As shown in Table 5, the patterns we just described translate into very large cross-country differences in AIOp for earnings—or, rather, in its lower-bound estimates, a qualification that needs to be kept in mind but we will not repeat in the rest of this subsection. As measured by the Gini coefficient, which is less sensitive to the tails of a distribution, the AIOp for earnings is close to 0.12 in Denmark and above 0.23, or nearly twice as high, in the U.S. (for men and women pooled). Moreover, while the proportional difference between women's Gini values for the two countries (about 0.07 and 0.15 for Denmark and the U.S. respectively) is similar to that for the full population, that difference is substantially larger among men, for whom the estimates are close to 0.09 and 0.25, respectively.

The MLD is more sensitive to the tails of a distribution than the Gini coefficient. As expected from the shapes of the curves in Figure 2, this results in cross-country differences in the estimates of AIOp for earnings that are markedly larger than with the Gini coefficient. Thus, the earnings AIOp for U.S. men is as much as seven times larger than for Danish men, whereas for men and women pooled, and for the latter alone, it is between four and five times larger.

Denmark and the U.S. For simplicity, we use the less precise language everywhere. In this we follow the practice of IOpI scholars, who do not explicitly distinguish between true types and those defined for research purposes given the information available and other pragmatic considerations (e.g., sample size).

[41] Estimates based on within-type mean income values or the smoothed values on the nonparametric curves give very similar results. Here and later the figures we report are always based on the former.

Figure 3 focuses on family income. Each panel includes four curves representing men's and women's opportunities in terms of total and disposable family income. In contrast to what is the case with individual earnings, within countries, and keeping the income notion fixed, there is little difference in the shapes of the income opportunity curves across genders (although now the women's curves are, for the most part, a little bit above the corresponding men's curves). This reflects a deep asymmetry across genders in how economic advantages are transmitted from parents to their offspring. As Mitnik et al. (2015:64-68) have shown for the U.S., whereas for men about 61 percent of that transmission "goes through" the labor market (i.e., own earnings) and 39 percent "goes through" the marriage market (i.e., spouse's earnings), for women those shares are about 29 and 71 percent, respectively. The left panels of Figures 2 and 3 make apparent that a similar, but smaller, gender asymmetry is also present in Denmark.

If the only source of income were the labor market, and if everyone were married to somebody of the opposite sex also born in 1972-1975 and with parents in the same fiftile, the total income curves of Figure 3 would simply be the horizontal sum of the earnings curves for men and women shown in Figure 2. And because men tend to have larger earnings than women, we would expect the total income curves to resemble those for men's earnings. Although these conditions obtain very imperfectly, Figure 3 shows that the total income curves, for men and women alike, behave similarly to the men's earnings curves in Figure 2. This leads to very large cross-country differences in the AIOp for total income (for men and women pooled); in fact, this AIOpI is, in both countries and regardless of inequality measure, extremely close to the AIOp for men's earnings. For instance, the Gini-based estimates are 0.243 (total income) and 0.234 (men's earnings) in the U.S. and 0.092 (total income) and 0.093 (men's earnings) in Denmark. Also, as suggested by the shapes of the curves, in both countries the AIOp for total income is somewhat larger for men than for women, and more so with the MLD than with the Gini (see Table 5).

Due to the impact of income taxes (which dominate those of transfers at all levels of parental income), the disposable income curves are below their total income counterparts in both countries. Because of higher tax rates in Denmark, the proportional downward shift when moving from total income to disposable income opportunities is larger in that country at all parental fiftiles; disposable income opportunities are, on average across fiftiles and genders, 18 percent lower in Denmark compared to 13 percent lower in the U.S. Also reflecting the cross-country differences in tax-and-transfer systems, the disposable income curves in Denmark are much flatter than their total income counterparts, whereas in the U.S. the shapes of the disposable and total income curves are more similar. [42] Thus, while in Denmark the average income return to parental income fiftile falls from about $1,700 to $1,100 for men and from about $1,100 to $650 for women once we move from total to disposable income—that is, it falls about 35 and 41 percent, respectively—in the U.S. it falls from about $3,800 to $2,900 for men and from about $3,600 to $2,700 for women—that is, it falls close to 24 percent in both cases. As a result, whereas in the U.S. the AIOp for disposable income, measured with the Gini coefficient, is only 8.4 percent smaller than with total income, in Denmark the disposable-income Gini is 22.1 percent smaller. The mechanical effects of the countries' choices regarding taxation and redistribution policies are even larger when AIOpI is measured

[42] For standardized tax statistics for the two countries, see OECD (2019b). For comparative analyses of taxes and transfers in the U.S. and various countries, including Denmark, see Kenworthy (2020b, 2020c).

with the MLD. With this inequality measure, AIOp for disposable income is 38 percent lower than AIOp for total income in Denmark compared to 15.5 percent lower in the U.S.

So far, we have measured people's income (meaning here the sum of their earned and unearned income) at the family level and without giving any consideration to family structure, which has been the conventional approach in the intergenerational mobility literature (for an exception, see Hertz 2005). The conventional approach in the IOpI literature has been different. Usually, IOpI scholars have also measured income at the family level. However, they have typically divided family income by the number of adults in the family, by the root of family size, or by some more elaborate factor that reflects both family size and composition in terms of adults and children of different ages.

Adjusting family income so that it reflects people's childbearing decisions does not seem an attractive strategy. The following toy example shows why. Assume that there are only two types, DI (disadvantaged) and AD (advantaged), which are equally frequent in the population. In type AD, people's family income is $60,000 and everyone is married and has two children (which is their ideal number of children). In type DI, people's family income is $30,000 and everyone is married. But although their preferred number of children is the same as in type AD, they only have one child because this is what they can afford with their income. Using the Gini coefficient, AIOpI is 0.17 when computed with unadjusted family income but falls to 0.13 when computed with family income divided by the root of family size. However, computing IOpI with overall income is the right approach here, as this is the income that determines the inequality in people's capacity to pursue life plans (regardless of their actual life-plan choices), which is what matters in the luck-egalitarian perspective (rather than, for instance, inequality in achieved welfare or material well-being).

In addition to showing why a family income measure that reflects people's childbearing decisions should be deemed unattractive in the IOpI context, the foregoing provides a good illustration of the type of concerns with endogeneity that have led mobility scholars to ignore family structure. At the same time, mobility scholars' approach also seems conceptually unsatisfactory, at least when applied in that context. One key issue is this: if what we care about is people's ex-ante capacity to pursue life plans that income makes possible, it follows that we should take into account that a given level of family income cannot lead to the same level of such capacity for each of two adults that are married with each other than for one adult that is single.[43] For this reason, the approach used by some IOpI scholars (e.g., Marrero and Rodríguez 2011) of dividing income by the number of adults in the family has a lot to recommend it. Although it is not free of conceptual problems either, it appears as an eminently reasonable and, arguably, theoretically superior strategy.

Figure 4 is like Figure 3 but is based on family income per adult. The shapes of the curves are similar to those shown in Figure 3 but the normalization of family income further reduces gender differences in

[43] This is the case even if we consider the economies of scale of living together (for these economies, see, e.g., Bütikofer and Gerfin 2017), which is not at all clear we should do. After all, single people may also enjoy such economies of scale if they choose to do so (and they often do) and, although much less common, sometimes married people do not enjoy them.

income opportunities, which are now of note only at the top of the parental income distribution in Denmark and at the bottom of that distribution in the U.S. Table 5 shows that the normalization of income also leads to an across-the-board reduction in the estimates of AIOp for family income. For instance, the U.S. Gini coefficients for men and women pooled are now 0.213 (total income) and 0.191 (disposable income) compared to 0.243 and 0.223, respectively, with overall income, whereas the corresponding Danish figures are now 0.082 (total income) and 0.056 (disposable income) compared to 0.092 and 0.072, respectively. This general reduction of the AIOpI estimates is a direct product of the fact that, in both countries, the prevalence of marriage increases steadily with parental income (see Helsø, forthcoming; Mitnik et al. 2015); after normalizing income, this marriage-probability gradient is no longer reflected in the AIOpI measures.

At the same time, the mechanical effects of tax and transfers on AIOpI become larger with the per-adult income measures in both countries. When switching from total to disposable income, the U.S. AIOpI now falls 10.2 percent (Gini) and 18.5 percent (MLD) compared to 8.4 and 15.5 percent, respectively, with overall family income (for men and women pooled). These percentage-point differences are even larger in Denmark, where the AIOpI now falls as much as 31.3 percent (Gini) and 49.8 percent (MLD) compared to 21.1 and 38.4 percent, respectively, with overall family income.

Importantly, the average family income returns, per adult in the family, to parental income fiftile are still very substantial, and much more so in the U.S. For men, these returns are close to $2,200 (total income) and $1,600 (disposable income) in the U.S. whereas they are about $1,000 (total income) and $600 (disposable income) in Denmark. For women, they are about $1,800 (total income) and $1,400 (disposable income) in the U.S and $600 (total income) and $350 (disposable income) in Denmark. These returns entail, for instance, that the expected difference between the disposable family incomes of married men born in the top and first fiftiles is about $160,000 in the U.S. compared to $60,000 in Denmark.

Absolute inequality of opportunity for disposable income in the U.S.: Addressing data limitations

A limitation of our previous family-income analyses is that, as we indicated when we described our data, our U.S. measure of disposable income is only approximate because it excludes state taxes and some transfers. State taxes make little difference for inequality compared to federal taxes (Cooper et al. 2011; Sammartino and Francis 2016). And our measure does include income from unemployment insurance and the earned income tax credit, which is the largest cash transfer program for low-income families in the country. Nevertheless, it could be argued that, because our measure of disposable income omits other transfers as well as state taxes, our analyses have offered a misleading picture of (a) AIOp for disposable income in the U.S., and (b) the magnitude of the mechanical effects of taxes and transfers on AIOp for family income in the U.S compared to Denmark. Although we cannot fix the limitations of the U.S. micro data on disposable income, we do make ex-post adjustments to our estimates to address these concerns.

We present the results in Table 6. The left panel reproduces all Gini-based estimates of disposable-income AIOpI for the U.S. reported in Table 5 and shows adjusted estimates that account for the exclusion of state taxes and some transfers from our analyses. We compute approximate adjustment factors with CPS-ASEC data (Flood et al. 2020). These factors reflect the effects of state taxes as well as cash transfers from all potentially important sources: Transitory Assistance for Needy Families, Social Security (including disability insurance), Supplemental Security Income, the Veterans' Administration, and various disability programs. Our key assumption is that AIOp estimated with our measure of disposable income overstates AIOp in true disposable income by the same proportional factor that the estimation of overall inequality with that measure overstates overall inequality in true disposable income.[44] Because the MLD cannot be computed when an income variable includes zeros, and both the SOI-M Panel sample and the CPS-ASEC sample include a nonnegligible share of people with zero disposable income, we only compute adjusted AIOpI estimates based on the Gini coefficient. On average across populations and disposable income notions (i.e., overall and per adult), the adjustments reduce the U.S. AIOpI estimates by 3 percent. The largest proportional reduction, 3.9 percent, is for women's AIOp for disposable income per adult, which falls from 0.180 to 0.173.

The right panel of Table 6 shows the absolute proportional difference between the AIOp for family income computed before and after taxes and transfers for Denmark and the U.S., in the latter case using both the unadjusted and the adjusted AIOpI estimates. On average across populations and disposable income notions, the fall in measured AIOpI in the U.S. due to the effect of taxes and transfers is 2.7 percentage points larger with the adjusted estimates; the larger difference, 3.5 percentage points, pertains to the AIOp for women's income per adult. These differences, however, do not alter our finding of substantially larger impacts of taxes and transfers in Denmark than in the U.S. For instance, once the AIOpI estimate for the U.S. is adjusted, the fall in women's AIOp for family income per adult due to taxes and transfers is still three times larger in Denmark than in the U.S.

Although the effects of omitting state taxes and some transfers from our U.S. measure of disposable income are minor, they are not negligible. For this reason, we use the adjusted disposable-income estimates in all our subsequent analyses.

Absolute inequality of opportunity for income in the U.S. relative to Denmark

So far, we have compared lower-bound AIOpI estimates for Denmark and the U.S. and have shown that they are all substantially larger in the latter country, regardless of income notion and population. However, we would like to say substantially more than this. Is (true) AIOpI larger in the U.S.? Combining our results in the previous subsections and our evidence that IALB applies to the comparison between the U.S. and Denmark, we can conclude that (true) AIOpI is indeed larger in the U.S. than in Denmark. But how much larger?

Figure 5 starts to address this question. For men and women pooled, we present in that figure set estimates of the ratios between AIOpI in the U.S. and Denmark, together with the corresponding confidence intervals for the partially identified ratios. A value larger than 1 indicates how much more inequality of opportunity there is in the U.S. compared to Denmark; for instance, a ratio of 1.4 indicates 40 percent more inequality. The figure includes Gini-based but not MLD-based estimates. To generate set estimates of AIOpI ratios we need both lower and upper-bound AIOpI estimates, but the latter—that is, the estimates of overall inequality—cannot be computed with the MLD because, in both countries, all our income measures have a nonnegligible—and in the case of earnings, very substantial—share of zeros. Importantly, here and in all subsequent analyses we correct the estimates of overall inequality to address lifecycle bias. As explained in

[44] See the note to Table 6 for additional information on the computation of the adjustment factors.

some detail earlier, this upward bias results from estimating inequality in long-run income with a proxy short-run income measure obtained when people are "too old." Our correction relies on data from the PSID (PSID 2019). We estimate overall income inequality with an approximate measure of long-run family income and with a measure of short-run family income obtained at ages 35-38 (the ages of the people in our samples), compute the magnitude of the upward lifecycle bias, and use this result to correct our estimates of overall inequality in Denmark and the U.S. (see the caption of Figure 5 for more details).

The left panel of Figure 5 shows that, without any identification assumption—that is, relying only on the data and our knowledge of the sampling process—we would not be able to make any progress with the questions posed above. The set estimates of AIOpI ratios based on the data alone all cover the value 1. Therefore, they cannot even tell us whether AIOpI is larger in the U.S. or in Denmark, let alone by how much, with any of our five income measures.

In the right panel of Figure 5, we present our set estimates of AIOpI ratios under our identification assumptions, IALB and IAUB. Now all set estimates are not just informative but substantially tighter—on average, 63 percent tighter. The set estimate of the earnings AIOp ratio indicates that the U.S. AIOp for long-run earnings is at the very least 37 percent larger than its Danish counterpart, and no more than 2.8 times as large. AIOp for long-run family income is a better normative yardstick to compare countries because it takes into account income sources other than the labor market as well as the crucial role that marriage plays in the intergenerational transmission of advantages. Before taxes and transfers, the AIOp for family income is at least 47 percent larger in the U.S. than in Denmark with overall income and at least 49 percent larger with income per adult, and the upper bounds for the percent differences are similar to that for the earnings AIOp. Once we include taxes and transfers in the analysis, the effects of the differences between the policy choices of the two countries manifest fully. With overall disposable income, the U.S. excess AIOp for long-run family income compared to Denmark is at the very least 55 percent; with disposable income per adult—the best of our income measures for carrying out a comparison of inequality of opportunity between countries—it is at the very least 68 percent. The upper bounds of the ratios cap the differences at about 250 and 350 percent, respectively.[45]

We next examine whether our results are robust to the choice of inequality measure, that is, whether our comparative findings still stand when we use inequality measures other than the Gini coefficient to compute AIOpI ratios. For this purpose, we can only use inequality measures that may be computed in the presence of zeros; we resort to Gini-type indices that give more weight to low incomes (Kakwani and Mehran) and to high incomes (Piesch), and to the relative mean deviation (RMD).[46] For simplicity, and to

[45] The upper bounds of our set estimates of AIOpI ratios are all larger than the corresponding ratios of lower-bound estimates from Tables 5 and 6. This means that the results here are not inconsistent with RIA. It is possible, however, for results under IALB and IAUB to be inconsistent with RIA—and in fact, this is the case with one of the inequality indices we introduce next (although only for total income estimates). This is the reason why in note 19 we said that, in some cases, it may be possible to show that RIA does not apply.

[46] The computability-with-zeros constraint excludes not only the MLD but also the Theil index, another well-known member of the class of generalized entropy (GE) indices. A third well-known member of this class, which is not excluded by that constraint, is the GE index with sensitivity parameter equal to 2, or half the

focus on our most important conclusions, in Figure 6 we present lower-bound estimates of AIOpI ratios rather than (finite-length) set estimates. Therefore, here we rely on IALB alone for identification and the uncertainty of our estimates is assessed with standard bootstrap bias-corrected confidence intervals.

The figure shows that our results are very robust across inequality measures. The lower-bound estimates of the AIOpI ratios that rely on the four new inequality indices are very similar to those obtained with the Gini coefficient. The average excess AIOpI in the U.S across these four indices is 38 percent with earnings, 48 percent with total income, 56 percent with disposable income, 50 percent with total income per adult, and 71 percent with disposable income per adult; the corresponding figures with the Gini coefficient are very similar, that is, 37, 47, 55, 49 and 68 percent. The estimate of the ratio for disposable income per adult is somewhat larger with the Kakwani index than with the Gini coefficient and essentially the same as the latter with the other three indices. Moreover, the null hypothesis that the excess AIOp for disposable income per adult in the U.S. compared to Denmark is less than 60 percent with any (i.e., at least one) of the five inequality indices is easily rejected (p-value=0.005).[47]

Relative inequality of opportunity for income in the U.S. and Denmark

How much of a country's income inequality is accounted for by circumstances beyond people's control? Or, equivalently, what share of a country's income inequality can be deemed unjust? We have argued that these questions are not relevant for comparative assessments of how countries are doing in terms of inequality of opportunity, which need to focus on AIOpI levels and not on what shares they represent of overall inequality. Nevertheless, we have also argued that RIOpI is an interesting and important descriptive quantity, and for this reason we estimate it here. We present all our Gini-based RIOpI results in Table 7 and summarize our key results for men and women pooled in Figure 7. For reasons we have already discussed, we do not report MLD-based estimates. Our RIOpI estimates need to be interpreted as lower-bound estimates of the share of long-run income inequality that is accounted for by circumstances beyond people's control.

Figure 7 makes apparent that although income inequality is substantially larger in the U.S. than in Denmark (on average across income variables, 71 percent larger), the lower bound estimates of RIOpI, the ethically unacceptable share, are also substantially larger in the U.S. (on average, 59 percent larger). In Denmark, RIOpI estimates are close to 29 percent for family income—across the four family-income measures—and 38 percent for earnings. In the U.S., the family income estimates are close to 49 percent while the earnings estimate is close to 45 percent. These are very striking figures given that we only use gender and parental rank to define types.

The results in Table 7 indicate that, for family income, the U.S. estimates are somewhat larger for men than for women, but the differences are minor and the estimates are similar to those for men and women pooled. By contrast, although earnings inequality is the same among men as among women, RIOpI is almost

square of the coefficient of variation. Unfortunately, the AIOpI estimates we obtain with this index are very imprecise. The constraint also prevents us from using another popular index, the standard deviation of log incomes.

[47] This is a "type-2 p-value," which is computed as the proportion of bootstrap samples in which the null hypothesis is true (Singh and Berk 1994). Type-2 p-values can be interpreted as standard p-values.

20 percentage points larger for men, 48 percent compared to 28.5 percent. This is likely driven by the fact that, in the U.S., married women with advantaged parental backgrounds often drop out of the labor force—and therefore are included in the analysis with zero earnings—when spousal earnings are high enough (see Mitnik et al. 2015). In Denmark, all RIOpI estimates are somewhat larger for men than for women (i.e., between 2.1 and 5.6 percentage points larger) but, unlike in the U.S., the earnings estimates for men and women separately are much lower than for men and women pooled (29.8 and 25.6 percent compared to 37.9 percent).

An extension: Accounting for race and ethnicity in the United States

It will never be possible to estimate AIOpI with empirical types that even approach true types. But perhaps we can aspire to obtain tight lower bounds by including in our analyses all or most circumstances that play major roles in generating inequality in opportunities (or, at least, all circumstances that we believe play such roles). Some such circumstances we would like to include when studying the U.S., in addition to those we have so far considered in this article, are parental education, place of residence when growing up, and race and ethnicity. In this section we focus on the latter. We do not have measures of race or ethnicity in the SOI-M Panel, so we cannot carry out an analysis in which we define types in terms of gender, parental fiftile, and race and ethnicity. Instead, we compute adjustment factors that are meant to reflect how much larger the estimates of AIOpI for total income would be with race and ethnicity included in the analysis. These are the "auxiliary quantities" we mentioned when describing our data and variables.

To compute the adjustment factors we need (for use with Gini-based and MLD-based AIOpI measures, respectively), we rely on three pieces of information that Chetty et al. (2020b) have made publicly available: (a) data on people's average family income rank in 2014-2015, by gender and parental-income centile bin, and by the same variables plus race/ethnicity, (b) data on the number of people in the cells defined by gender and parental-income centile bin, and by the same variables plus race/ethnicity, and (c) a crosswalk that maps income rank into real income in 2015 dollars. We use these data to produce a rough approximation to within-type family income means for types defined both in terms of gender and parental income fiftile, and in terms of gender, parental income fiftile, and race and ethnicity. Combining this information with the information in (b), we obtain the distributions of within-type means under both definitions of types. Figure 8 shows nonparametric estimates of the densities of these distributions. As expected, the distribution of within-type family income means based on more disaggregated types (because of the inclusion of race and ethnicity in defining them) is substantially more dispersed.

Our adjustment factors are simply the ratios between the Gini coefficient or MLD of the distribution where types account for race and ethnicity and the Gini coefficient or MLD of the distribution where types do not account for race and ethnicity. When using these adjustment factors, we make a key assumption: that the effects of replacing true within-type income means by rough approximations in computing inequality measures tend to cancel out, e.g., that if inequality in one approximate distribution is X percent lower than in the corresponding true distribution, more or less the same is the case with the other approximate distribution.

As we mentioned earlier, the notion of family income used by Chetty et al. (2020b) is very similar to our notion of total family income, so here we focus on our AIOpI estimates based on this notion. The results of adjusting our lower-bound IOpI estimates to account for race and ethnicity in the U.S. are shown in Table

8. An adjustment factor of almost 14 percent in the Gini-based measure of AIOp for total family income puts it at about 0.28 (compared to about 0.24 without race and ethnicity). In the case of the MLD, the adjustment factor is much larger, almost 52 percent, most likely reflecting (a) the higher sensitivity of this inequality measure to the tails of a distribution, and (b) the substantial elongation of the left tail of the distribution of within-type income means once race and ethnicity are also considered in defining types (see Figure 8). This puts the lower bound MLD-based estimate of AIOpI at as high as 0.15.

For reasons we have already explained, we do not report RIOpI estimates based on the MLD. But adjusting our Gini-based estimate of near 51 percent to account for race and ethnicity suggests that, in the U.S., at the very least 58 percent of the country's high inequality in long-run total family income is due to circumstances beyond people's control.

Discussion and conclusions

In this article, we have carried out the first cross-country comparative analysis of inequality of opportunity for income based on administrative data. Our focus on Denmark and the U.S. is of great interest given that these countries are often portrayed as quasi-ideal types in literatures dealing with the various configurations that political economies, welfare state regimes, production systems and so forth take in highly industrialized capitalist democracies.

While most comparative research on intergenerational economic mobility suggests that Denmark and other social-democratic countries are able to limit inequality of opportunity for income to a larger extent than the U.S. does, the direct evidence produced by the burgeoning empirical literature on inequality of opportunity is far from compelling. This has partly been the result of data limitations. But it has also been the result of conceptual and methodological shortcomings in the way in which that literature, despite its many contributions and achievements, has transformed the luck-egalitarian understanding of inequality of opportunity into an empirical research program.

Our empirical analyses in this article have relied on improved data and methods. We have used data that cover the full populations of interest and are unaffected by attrition, recall problems and other factors that reduce the confidence one may place on empirical findings or curtail their pragmatic relevance. We have made our evidence as informative as possible not only by using administrative data for both Denmark and the U.S. but also by focusing on the same cohorts and time period, using the same circumstances to define types, employing the same estimation method, and aligning as much as possible the income notions across the two countries. We have avoided the conceptual inconsistency involved in the use of the parametric log-linear approach, as well as the selected populations that result. Unlike nearly all the previous literature, our aim here has been to produce estimates of inequality of opportunity for long-run income—which is the normatively relevant notion of income for empirical analyses—and to this end we have resorted to a new, empirically validated, nonclassical measurement-error model similar to those used by mobility scholars. Finally, we have advanced and extensively justified two identification assumptions. These have allowed us to go beyond the generation of lower-bound estimates of inequality of opportunity in the U.S. and Denmark to produce the first set estimates of inequality of opportunity in the U.S. relative to Denmark.

What have we found? We will not attempt to review all our findings on absolute inequality of opportunity here, but it is nonetheless useful to briefly discuss a few of them. Our results indicate that absolute inequality of opportunity for long-run individual earnings and for long-run family income (before taxes and transfers) are both high in the U.S. Even when we only consider two circumstances in our main analyses, gender and parental income rank, the lower-bound Gini coefficients for earnings and family-income opportunities are in the 0.21-024 range.[48] Further, the extension of our analysis to account for race and ethnicity suggests a Gini for family-income opportunities of at least 0.28; notably, this lower-bound estimate of inequality of opportunity in the U.S. is only somewhat smaller than our point estimate of overall inequality in Denmark with the same income concept, which puts the latter at 0.32.

We also find that inequality of opportunity for long-run earnings and family income (again, before taxes and transfers) is far from negligible in Denmark. Indeed, even with the very minimum set of circumstances considered in our analysis, the Danish lower-bound Gini coefficients for earnings and family-income opportunities are in the 0.08-0.12 range. These values are, nonetheless, much lower than the corresponding values for the U.S. (and, in proportional terms, this is even more with the MLD as the inequality index). The cross-country differences in our earnings and total income estimates are likely the result of three main factors. We have already referred to two of them: (a) Denmark's labor-market institutions compress the earnings distribution much more than their U.S. counterparts do, and (b) Denmark invests much more heavily on early education and job training and has put in place more generous and more expansive public-insurance programs, all of which makes various family-of-origin disadvantages less consequential or less prevalent in Denmark than in the U.S. The third factor is that the "welfare regimes" of the two countries lead to employment structures with quite different job mixes, with the U.S. employment structure including, in particular, a much larger share of low-skill personal-service jobs (Esping-Andersen 1990, 1993, 1999; Oesch 2015). Disentangling the contributions that these related factors make to the cross-country differences in inequality of opportunity for earnings and for pre-tax-and-transfers family income is an important topic for future research.

Taxes and transfers reduce inequality of opportunity for long-run family income in both countries. However, due to the countries' markedly different policies in those two domains, the effects on our lower-bound estimates are very different in each case. In the U.S., taxes and transfers reduce measured inequality of opportunity for family income by almost 11 percent and for family income per adult by 13 percent. In Denmark, they reduce it by 22 percent in the case of family income and by 31 percent in the case of family income per adult. As a result, measured inequality of opportunity with our preferred income notion, disposable family income per adult, is less than 0.06 in Denmark but better than 0.18 in the U.S. Remarkably, the latter lower-bound estimate of inequality of opportunity is nearly the same as our point estimate of overall inequality in Denmark (with the same income notion). As (true) inequality of opportunity can be expected to be significantly higher than our lower-bound estimate, it follows that, after taxes and transfers, there is more inequality of opportunity for long-run income in the U.S. than (overall) inequality in long-run income in Denmark.

An important contribution of our research is that it provides direct quantitative evidence that (true) inequality of opportunity for long-run income is substantially higher in the United States than in Denmark. Focusing again on disposable family income per adult, which takes into account taxes and transfers and

[48] Here and in what follows, all results we discuss pertain to men and women pooled.

purges the effect of the association between circumstances and the probability of marriage, absolute inequality of opportunity in the U.S. is at the very least 68 percent higher than in Denmark (the upper bound of our set estimate caps this difference at 355 percent). This gives a very clear response to the main question we set out to answer in this article: the distribution of economic opportunities—not just of economic outcomes—is substantially less unequal in Denmark than in the U.S. Moreover, it is typically argued that results for Denmark apply more generally to the Scandinavian social-democratic countries (e.g., Landersø and Heckman 2017:179). If this is correct—and we believe it is—then we may conclude that, compared to the U.S., these countries have achieved a considerable measure of success in reducing the effects of circumstances beyond people's control on their economic opportunities.

It is also useful to provide some comments about the significance of our results on relative inequality of opportunity. The literature has been both deeply interested in measuring the share of income inequality that may be deemed unjust and surprised by the results. Hufe et al. (2017:499-500) explained it well: "Many studies have estimated the effect of circumstances on income acquisition. Perhaps surprisingly, the fraction of inequality attributable to circumstances is usually quite small—in the advanced democracies, approximately 20%. … Since it is this part of inequality that is ethically troubling, the conclusion might be drawn that any existing income inequality is ethically acceptable, being largely dependent on differential effort".[49] Hufe et al.'s response to this state of affairs was to expand considerably the set of circumstances to be taken into account (compared to those previously considered in the literature) under the argument that "all measurable achievements and behaviors of children, before an age of consent is attained, are the result of their circumstances" so "children should not be held responsible for any of their accomplishments before that age" (Hufe et al. 2017:501). Using a rich U.S. dataset and a very long list of circumstances, they reported that their estimate of relative inequality of opportunity for gross individual income increased from 27 to 43 percent after accounting for childhood circumstances.[50]

In this context, the magnitude of our lower-bound estimates of relative inequality of opportunity is very striking. Although we only consider gender and parental rank in defining types, our U.S. estimates are in the 48 to 51 percent range with our four family-income measures whereas the corresponding figures for Denmark are in the 27 to 31 percent range (with individual earnings, the estimates are 45 and 38 percent,

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[49] Of course, as Hufe et al. (2017) fully understand, drawing this conclusion would be a non sequitur, given that all estimates are lower-bound estimates. At the same time, Kanbur and Wagstaff (2016:138) have persuasively argued that "the fact that [a lower-bound estimate] is the number that is produced in front of the policy makers will make it akin to a point estimate in the policy discourse, no matter how much the analyst caveats it as a lower bound."

[50] The dataset is very rich, but it does not represent well the population of interest (see Table 1 and our earlier discussion). The larger of the two estimates is based on the following circumstances: gender, family income at age 16, country of birth, ethnicity, cohort, mother education and occupation, height, rural/urban residence, five indicators of the child-parent relationship at age 16, indicators of the mother's and the individual's health during the individual's gestation and at age 16, indicators of the individual's ability at age 16, whether the individual attended private or public school, the education of people in the household, psychological test scores at age 16, and whether the mother was convicted of a crime.

respectively). This indicates, roughly, that at least half of family income inequality in the U.S., and at least 30 percent in Denmark, are “ethically troubling,” that is, can be traced back to circumstances beyond people’s control. Moreover, adjusting our total family income estimate for the U.S. to account for race and ethnicity suggests that at least 58 percent of the country’s high inequality in long-run income is due to such circumstances. Given that other circumstances that may be expected to have a noticeable impact are still excluded from the analysis (e.g., parental education, place of residence when growing up), it is quite possible that this lower-bound estimate still understates significantly the share of U.S. income inequality that is ethically unacceptable from a luck-egalitarian perspective.

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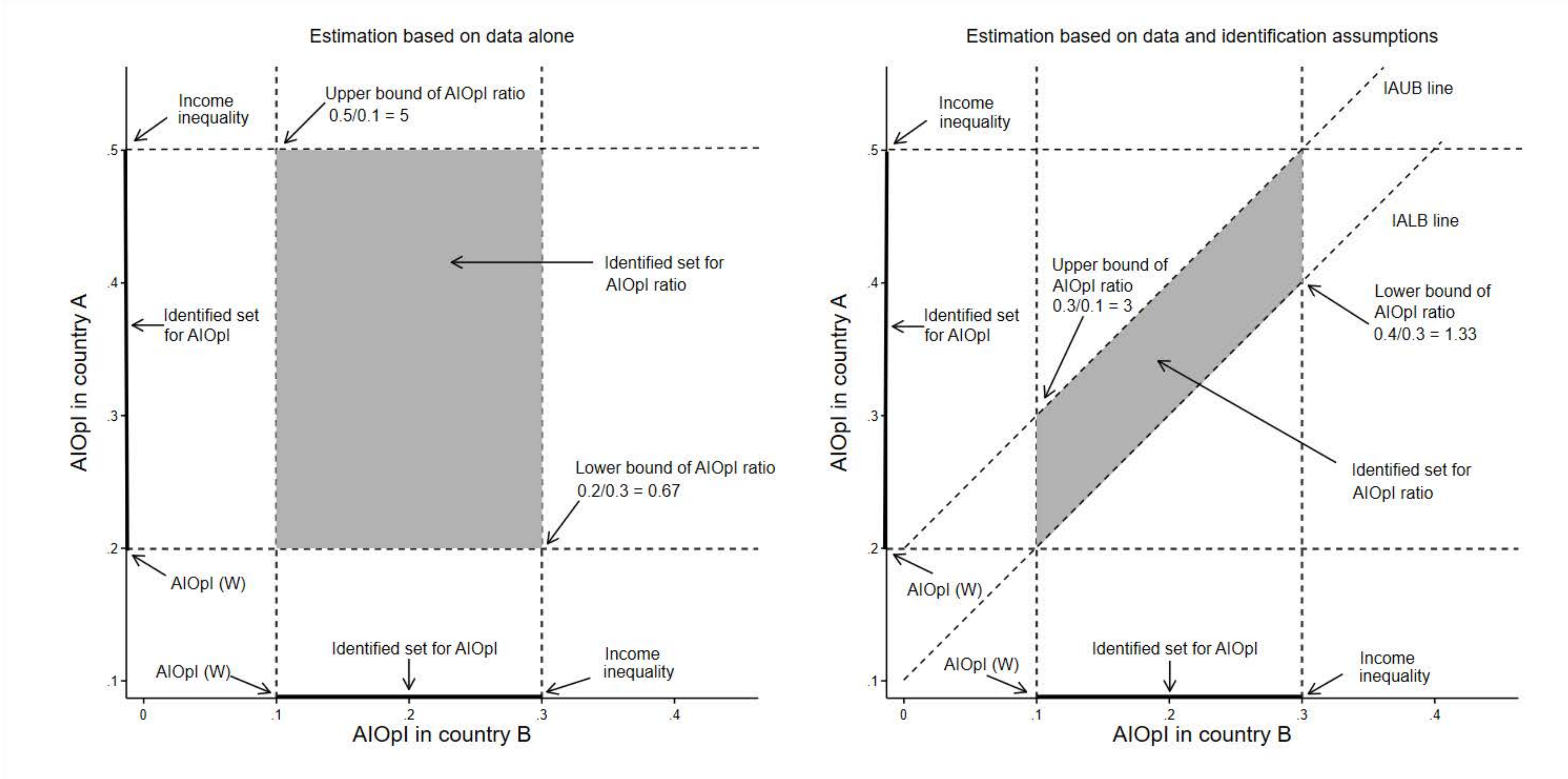

Figure 1. Set estimation of ratios of absolute inequality of opportunity for income between two countries

AIOpI = absolute inequality of opportunity for income. On the left panel, estimation is based only on the data and on knowledge of the sampling process. The identified set for the AIOpI ratio between the two countries includes all the ratios consistent with the identified sets for the AIOpI of each country. The upper bound of this identified set is the upper-bound AIOpI for country A divided by the lower-bound AIOpI for country B, and its lower bound is the lower-bound AIOpI for country A divided by the upper-bound AIOpI for country B. On the right panel, estimation incorporates the information provided by two identification assumptions: the inequality assumption for the lower bound (IALB) and the inequality assumption for the upper bound (IAUB. This leads to a much smaller identified set for the AIOpI ratio. The upper bound of this smaller set is the largest ratio on the IAUB line, and the lower bound is the smallest ratio on the IALB line.

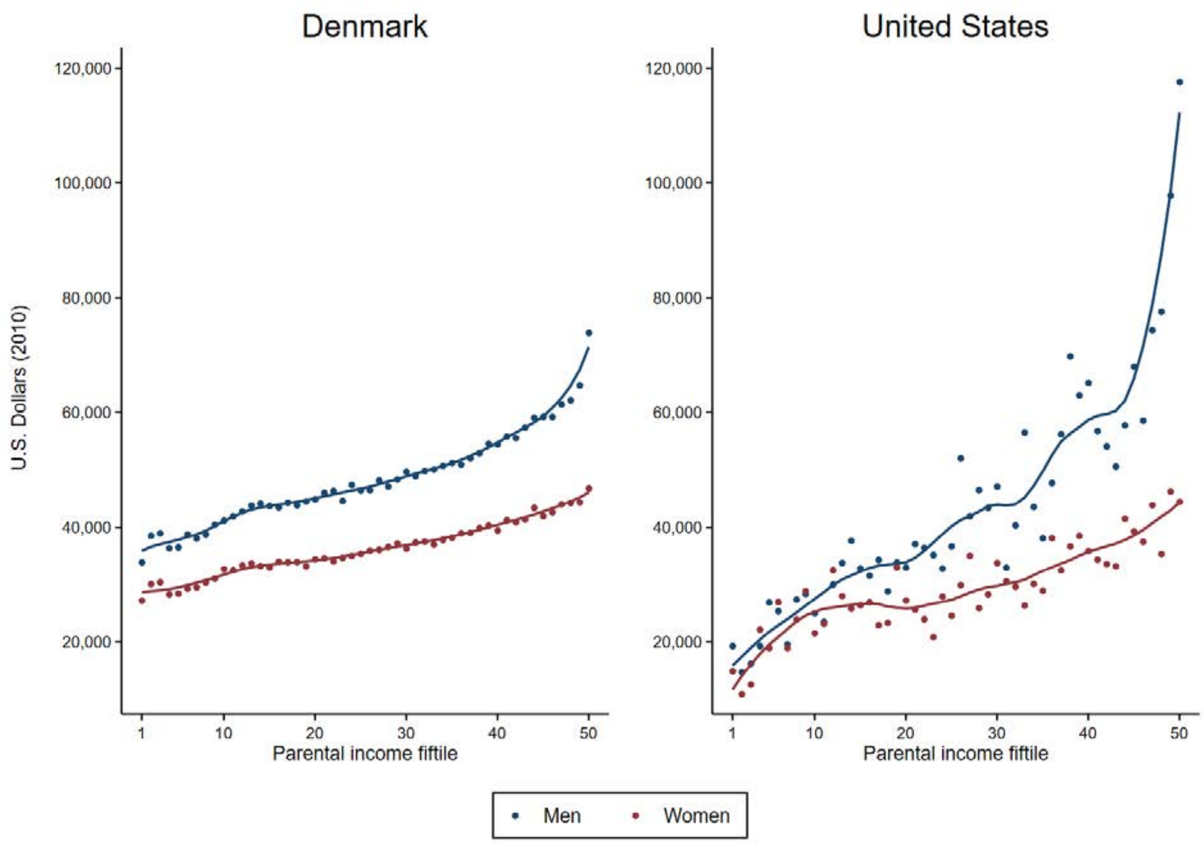

Figure 2. Individual earnings opportunities by parental income fiftile and gender

The 100 dots in each panel represent the earnings opportunities of men and women at different fiftiles of parental income. They are within-type mean earnings, with types defined by gender and parental income fiftile. The superimposed nonparametric curves are based on local polynomial regressions of degree 1, using an Epanechnikov kernel function and a bandwidth selected automatically by a rule-of-thumb estimator. They are estimated from the already-computed mean values shown in the figure, not from the microdata.

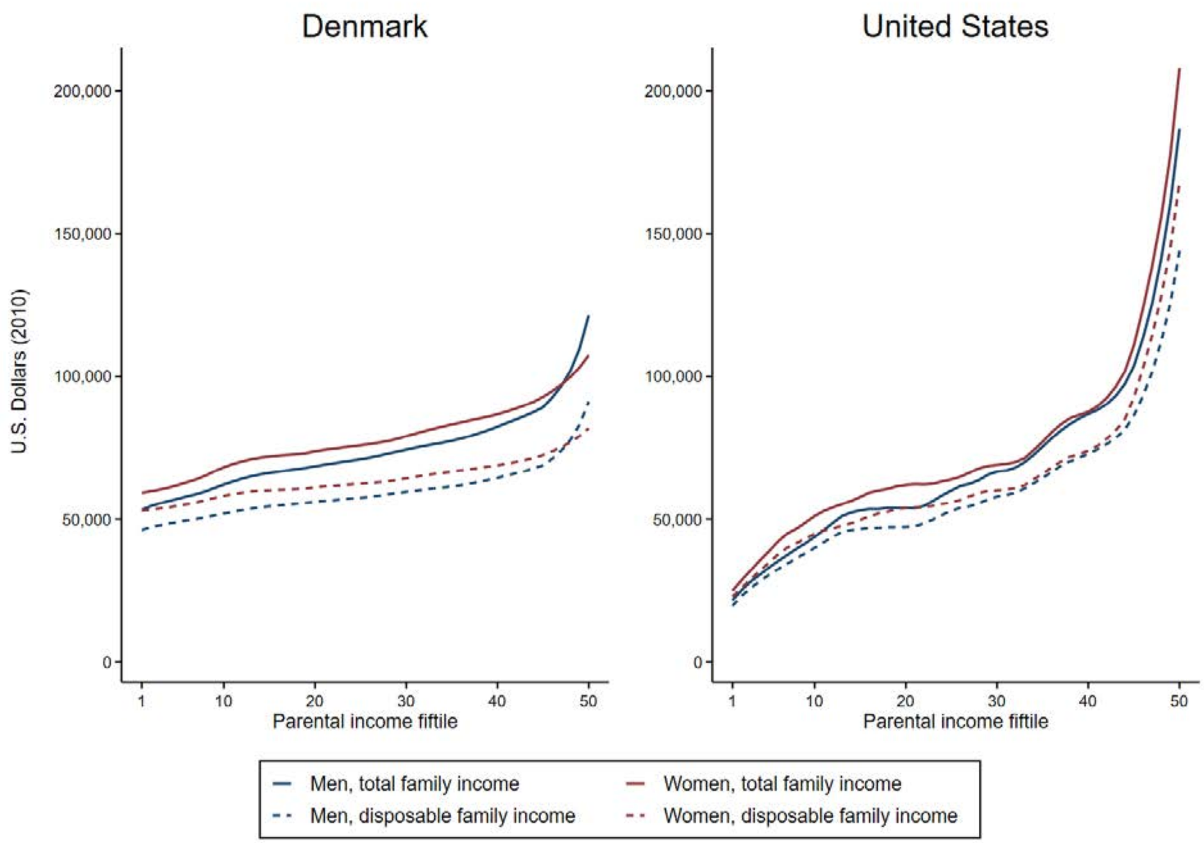

Figure 3. Family income opportunities by parental income fiftile and gender

For men and women separately, the curves represent the relationship between opportunities for total or disposable family income and fiftiles of parental income. The opportunities are within-type means of total and disposable family income, with types defined by gender and parental income fiftile. The curves are nonparametrically estimated using local polynomial regressions of degree 1, with an Epanechnikov kernel function and a bandwidth selected automatically by a rule-of-thumb estimator. They are estimated from the pre-computed within-type mean values, not from the microdata.

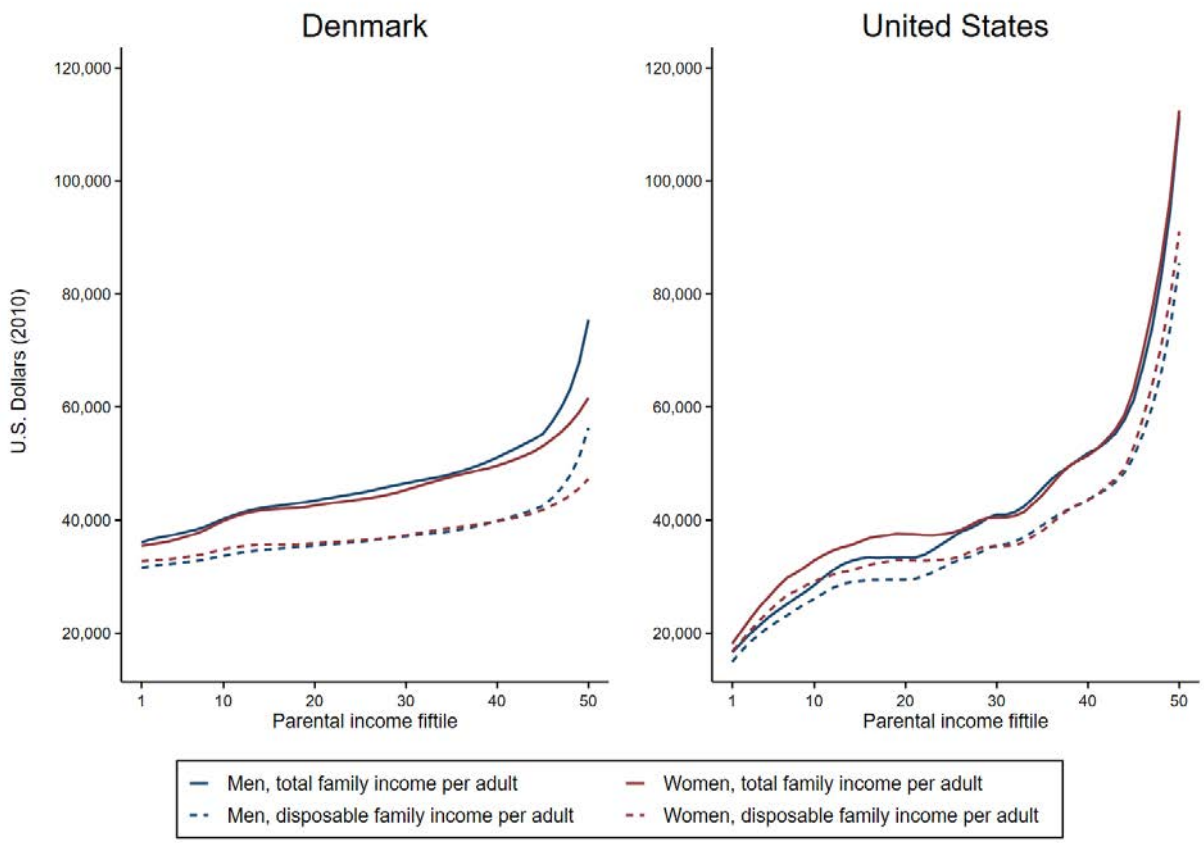

Figure 4. Per-adult family income opportunities by parental income fiftile and gender

For men and women separately, the curves represent the relationship between opportunities for total or disposable family income per adult and fiftiles of parental income. The opportunities are within-type means of total and disposable family income per adult, with types defined by gender and parental income fiftile. The curves are nonparametrically estimated using local polynomial regressions of degree 1, with an Epanechnikov kernel function and a bandwidth selected automatically by a rule-of-thumb estimator. They are estimated from the pre-computed within-type mean values, not from the microdata.

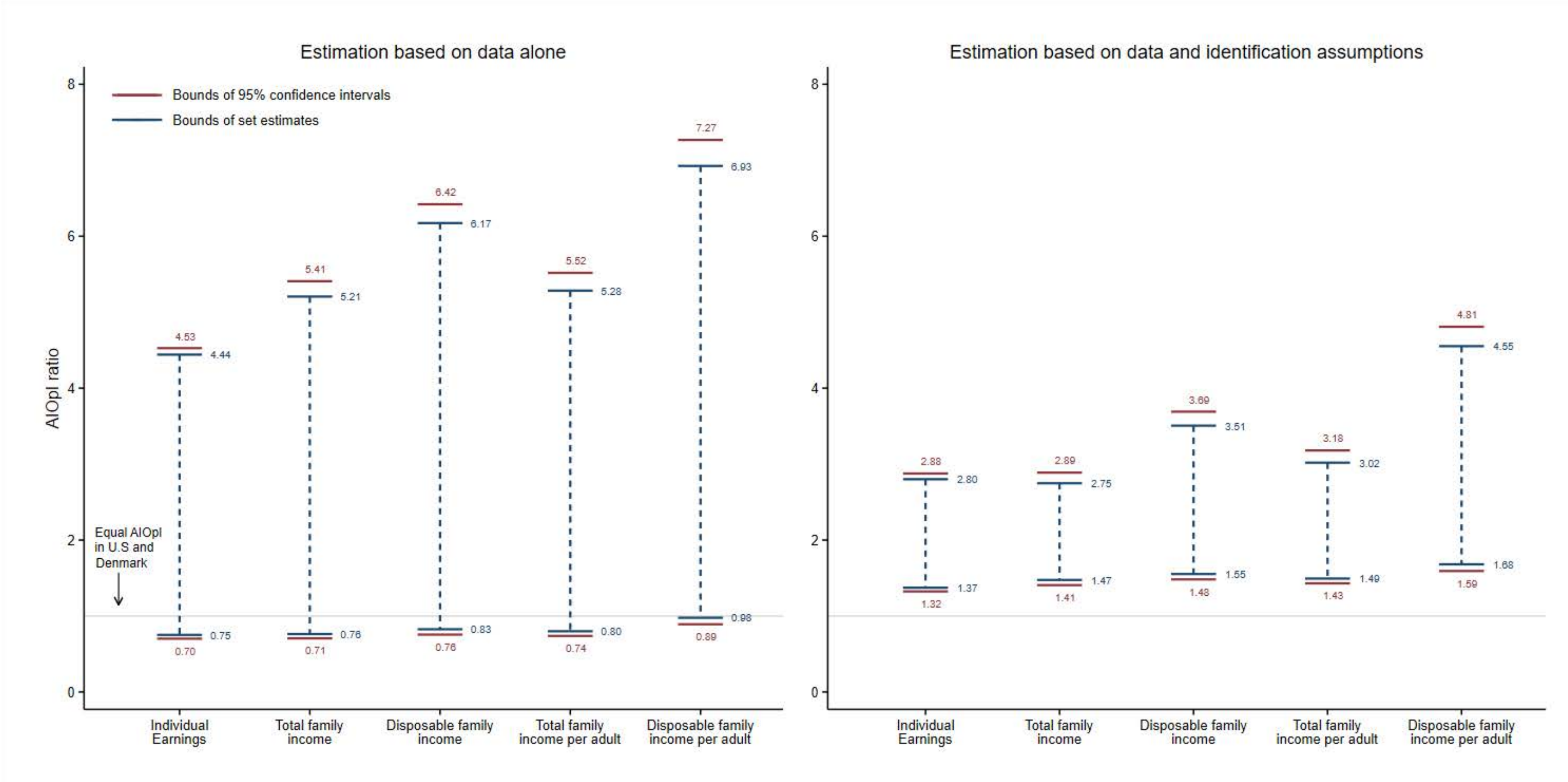

Figure 5. Set estimates of ratios of absolute inequality of opportunity for income in the United States and Denmark (Gini coefficient, full population)

AIOpI = absolute inequality of opportunity for income; AIOpI ratio = ratio of AIOpI in the United States and Denmark. AIOpI is computed with the Gini coefficient and for men and women pooled. A ratio larger than 1 indicates how much more inequality of opportunity there is in the U.S. compared to Denmark; for instance, a ratio of 1.4 indicates 40 percent more inequality. Estimates of overall income inequality are used as inputs in the set estimation of AIOpI ratios (see the text and Figure 1 for the approaches to estimation used in the left and right panels); those inequality estimates are divided by an adjustment factor to account for the upward lifecycle bias generated by the estimation of inequality in long-run income with an annual measure of income obtained at ages 35-38. The adjustment factor is computed with a sample from the Panel Study of Income Dynamics, "Sample I" from Mitnik (2019). A Gini coefficient estimate for family income at ages 35-38 is divided by a Gini coefficient estimate for average income at ages 24-56. The resulting adjustment factor is 1.11. Confidence intervals are for the partially identified AIOpI ratios, not for the identified sets. They are computed using the approach of Imbens and Manski (2004), with bootstrap-based standard errors.

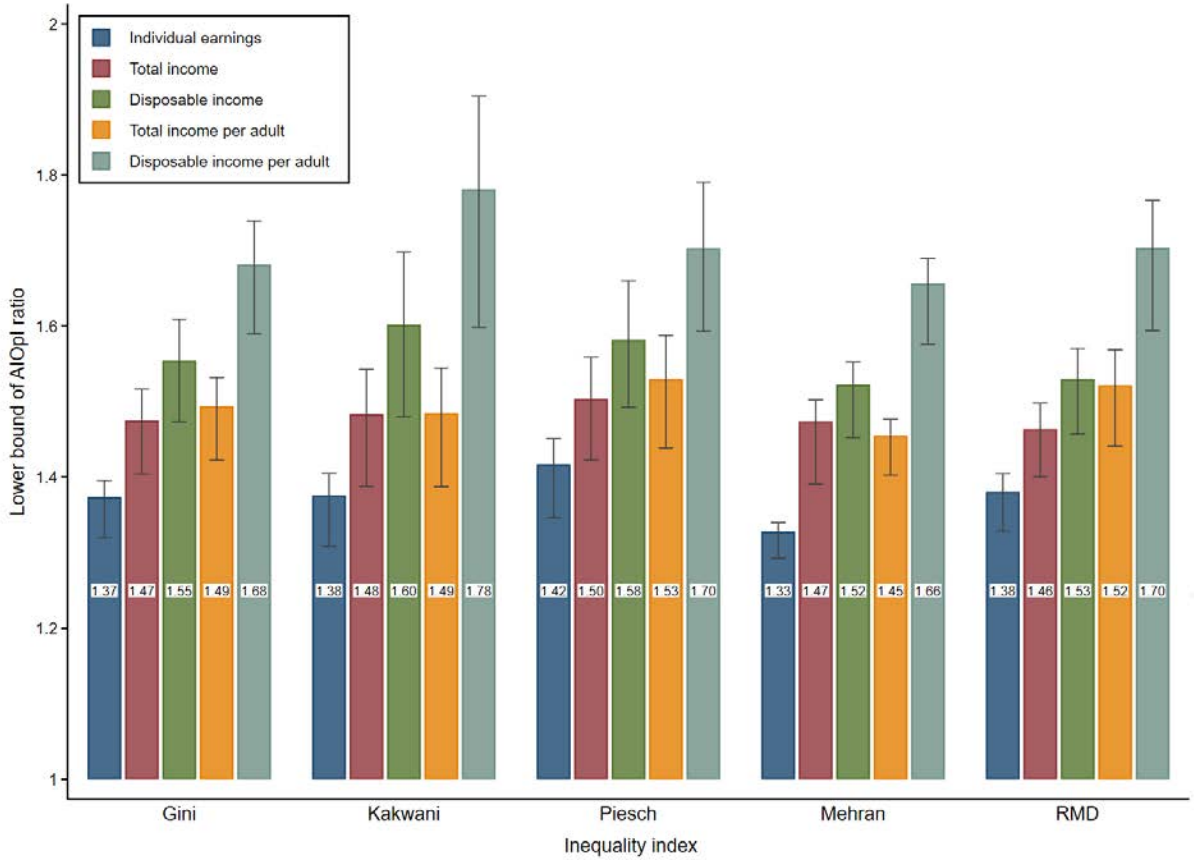

Figure 6. Lower-bound estimates of ratios of absolute inequality of opportunity for income in the United States and Denmark (Five inequality indices, full population)

AIOpI = absolute inequality of opportunity for income; AIOpI ratio = ratio of AIOpI in the United States and Denmark. AIOpI is computed for men and women pooled, with the five inequality indices shown in the figure. A ratio larger than 1 indicates how much more inequality of opportunity there is in the U.S. compared to Denmark; for instance, a ratio of 1.4 indicates 40 percent more inequality. Estimation relies on IALB, the inequality assumption for the lower bound, so estimates of overall income inequality are used as inputs for generating the lower-bound estimates of AIOpI ratios (see the text and Figure 1). The estimates of overall income inequality are divided by an adjustment factor to account for the upward lifecycle bias generated by the estimation of inequality in long-run income with an annual measure of income obtained at ages 35-38. The adjustment factors are computed as explained in the caption to Figure 5. They are 1.11 (Gini), 1.22 (Kakwani), 1.12 (Piesch), 1.10 (Mehran) and 1.11 (RMD). Confidence intervals, represented by the line segments with capped spikes, are 95 % bootstrap bias-corrected confidence intervals.

Figure 7. Estimates of income inequality and lower-bound estimates of relative inequality of opportunity for income (Gini coefficient, full population)

RIOpI = relative inequality of opportunity for income. RIOpI is computed by dividing absolute inequality of opportunity for income (AIOpI) by overall income inequality. Overall inequality and AIOpI are computed with the Gini coefficient and for men and women pooled. RIOpI estimates are lower-bound estimates because circumstances are partially observed. The estimates of overall income inequality are divided by an adjustment factor to account for the upward lifecycle bias generated by the estimation of inequality in long-run income with an annual measure of income obtained at ages 35-38. The adjustment factor, 1.11, is computed as explained in the caption to Figure 5. Confidence intervals, represented by the line segments with capped spikes, are 95 % bootstrap bias-corrected confidence intervals.

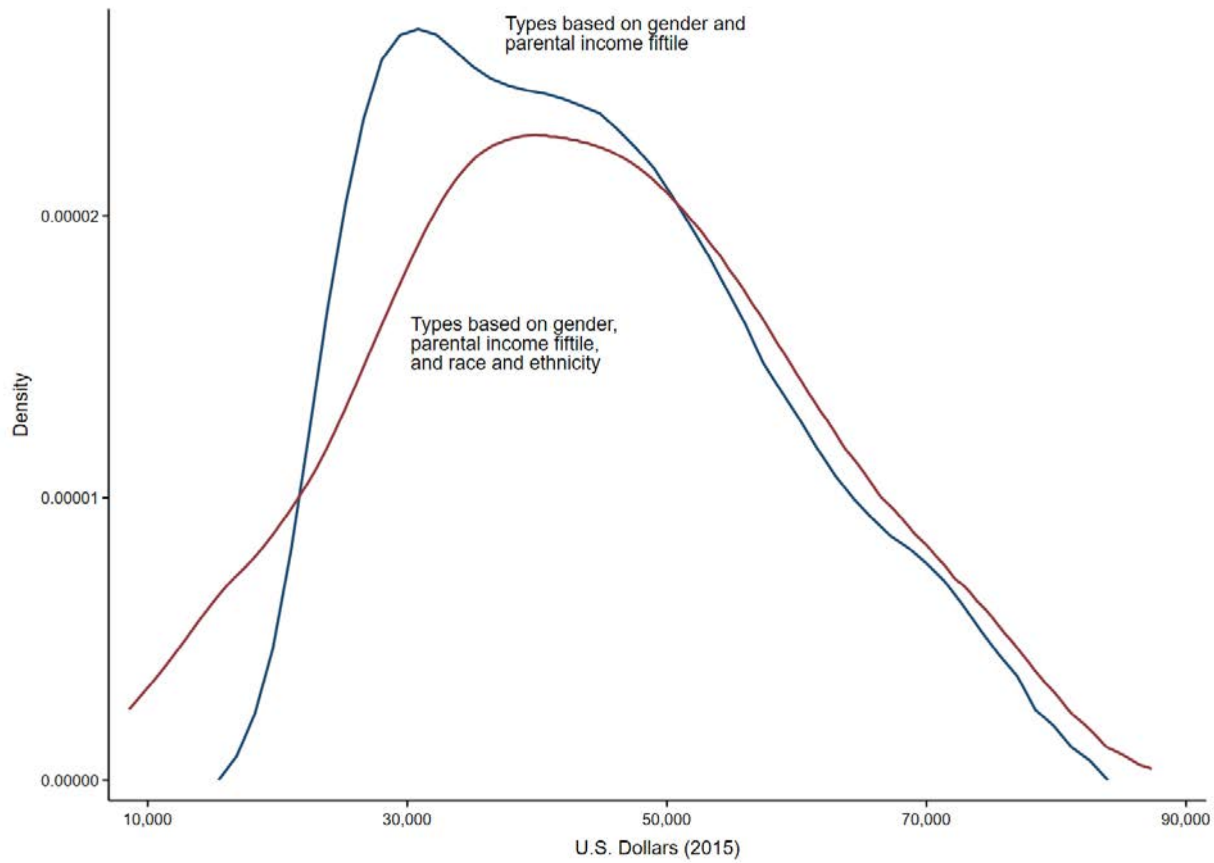

Figure 8. Probability density function of the distribution of within-type means excluding and including race and ethnicity in the definition of types

The figure shows the densities of two approximate distributions of within-type mean family incomes constructed with data from Chetty et al. (2020). In one distribution, types are based on gender and parental income fiftiles. In the other distribution, types are based on the same circumstances plus seven race-ethnicity categories (including one for when information on race and ethnicity is missing). See the text for how the distributions are constructed from Chetty et al.'s (2020) data. The densities are estimated nonparametrically using an Epanechnikov kernel function and an automatic procedure to select the width of the kernel.

Table 1. Previous estimates of absolute and relative inequality of opportunity for income in Denmark and the United States based on the mean logarithmic deviation as inequality measure

Country	Study	Period	Inequality	AIOpI	RIOpI (%)	Income measure	Population	Circumstances	Estimation	Data
DK	Checchi, Peragine and Serlenga (2010)	2004	0.083	0.012	14.2	After-tax individual earnings	Individuals 25-60 year old with positive earnings	Gender, immigrant (yes/no), parents education and occupation, and population density of place of residency	Parametric log linear	EU-SILC
			0.083	0.013	14.9				Param. log linear (EDH)	
DK	Marrero and Rodríguez (2012)	2004	0.069	0.001	1.9	Household equivalent disposable income	Household heads 26-50 years old	Parents education, father occupation, country of birth, economic difficulties during chilhood	Parametric log linear	EU-SILC
DK	Brzezinski (2015)	2004	0.068	0.001	2.1	Household equivalent disposable income	Household heads 26-50 years old and people 25-50 years old in the same households	Parents education, father occupation, country of birth	Parametric log linear	EU-SILC
		2010	0.120	0.004	3.0					
DK	Suárez Álvarez and López Menéndez (2019)	2004	0.060	0.006	9.6	Household equivalent disposable income	Individuals 25-59 years old (excluding self-employed)	Gender, immigrant (yes/no), parents education and occupation, age, and population density of place of residency	Parametric log linear	EU-SILC
		2010	0.140	0.020	14.7					
US	Pistolesi (2009)	2000	0.220	0.041	18.6	Individual gross earnings	Working male household heads 30-50 years old with positive earnings	Age, parents education, father occupation, race (black, nonblack), born in the south (yes/no)	Semiparametric	PSID
US	Marrero and Rodríguez (2011)	2007	0.429	0.013	3.1	Household total income per adult in the household	Household heads 25-50 years old	Race, father education	Parametric log linear	PSID
			0.429	0.015	3.4			Race, father education, and interactions		
			0.429	0.022	5.2			Race, father education	Param. log linear (EDH)	
			0.429	0.025	5.9			Eight race-father education groups	Non-parametric	
US	Marrero and Rodríguez (2013)	1969-1970	0.092	0.006	6.1	Potential-experience adjusted household total income per adult in the household	Household heads 18-65 years old	Race, father education	Non-parametric	PSID
		1979-1980	0.106	0.005	4.4					
		1989-1990	0.175	0.010	5.7					
US	Niehues and Peichl (2014)	2006	0.35 / 0.29	0.06 / 0.05	17.1/17.2	Individual gross / net earnings	Household heads/spouses 25-55 years old w/ at least 5 consecutive years of positive earnings in 1981-2006 and positive earnings in 2006	Gender, race, foreign born (yes/no), born in the south (yes/no), cohort, father education and occupation, height, degree of urbanization of place of birth	Parametric log linear (with correction)	PSID
			0.31 / 0.26	0.03 / 0.03	9.7 / 11.5	Men gross / net earnings				
			0.32 / 0.26	0.03 / 0.02	9.4 / 7.7	Women gross / net earnings				
		At least five consecutive years in 1981-2006	0.25 / 0.19	0.07 / 0.05	28.0 / 26.3	Aver. ind. gross / net earnings	Household heads/spouses 25-55 years old with at least five consecutive years of positive earnings in 1981-2006			
			0.19 / 0.14	0.03 / 0.02	15.8 / 14.3	Aver. men gross / net earnings				
			0.20 / 0.15	0.02 / 0.01	10.0 / 6.7	Aver. women gross / net earnings				
US	Hufe et al. (2017)	2010-2012	0.597	0.162	27.1	Individual gross income (average across 2010-2012)	Individuals 25-30 years old with positive earnings in 2010-2012, and born to mothers aged 14–21 on December 31, 1978.	Gender, country of birth, ethnicity, cohort, mother educ. and occ., height, rural/urban residence and fam. income at age 16	Parametric log linear	NLSY79
			0.597	0.168	28.1			*Plus* indicators of child-parent relationship at age 16.		
			0.597	0.209	35.0			*Plus* indicators of mother and individual health during individual's gestation and at age 16.		
			0.597	0.232	38.8			*Plus* indicators of individual's ability at age 16.		
			0.597	0.259	43.5			*Plus* priv./pub. school, educ. of people in household and psychological test scores at age 16, mother convicted crime.		
			0.559	0.109	19.5			Circumstances in most comprehensive set above that are measured at birth		
			0.559	0.25	44.6			Circumstances in most comprehensive set above measured at birth or age 12		
			0.559	0.329	58.8			Circumstances in most comprehensive set above measured at birth, age 12 or/and age 16 (all available)		

Note: DK = Denmark; US = United States; Inequality = income inequality; AIOpI = absolute inequality of opportunity for income; RIOpI = relative inequality of opportunity for income; EU-SILC = European Union Survey on Income, Social Inclusion and Living Conditions; PSID = Panel Study of Income Dynamics; NLSY79 = National Longitudinal Survey of Youth 1979. Parametric log linear estimation is as in Ferreira and Gignoux (2011). Parametric log linear (EDH) estimation is similar but adjusts estimates to account for effort distribution heterogeneity (EDH) across types, as proposed by Björklund et al. (2012). Parametric log linear (with correction) estimation attempts to correct for modeling log income instead of income; the correction assumes the error is homoskedastic and normally distributed. Nonparametric estimation computes mean income within types. For semiparametric estimation, see Pistolesi (2009). Household equivalent disposable income in studies using the EU-SILC data is household disposable income adjusted to account for household size and composition (in terms of adults and children of different ages). Pistolesi (2009) computed MLD-based annual estimates for 1967-2000, but only reported the mean, minimum and maximum values over the period. The estimates reported in this table are from Brunori et al. (2013), who attributed them to Pistolesi (2009). Niehues and Peichl (2014) define the population in their sample as "individuals." As in most years covered by their study the PSID only provides individual earnings information for household heads and their spouses (including cohabiting partners), this is reflected in the corresponding cell in the table. RIOpI values reported for this study are approximate, as they are based on rounded values of AIOpI and inequality. Marrero and Rodríguez (2011) also report annual estimates for 1969-2006 and nonparametric estimates with types based on race or father education alone. Hufe et al. (2017) also report estimates in which ability is dropped from the sets of circumstances.

Table 2. Previous estimates of absolute and relative inequality of opportunity for income in Denmark and the United States based on the Gini coefficient as inequality measure

Country	Study	Period	Inequality	AIOpI	RIOpI (%)	Income measure	Population	Circumstances	Estimation	Data
DK	Equalchances Project (2018)	2004	0.25	0.03	0.12	Household equivalent disposable income	Working-age individuals	Indicators of parents education and occupation, and origin (i.e., race, ethnic origin, parental culture, parental religion, or area of birth) selected by crossvalidation in each country-year.	Parametric linear	EU-SILC
		2010	0.24	0.03	0.12					
DK	Suárez Álvarez and López Menéndez (2019)	2004	0.18	0.06	0.34	Household equivalent disposable income	Individuals 25-59 years old (excluding self-employed)	Gender, immigrant (yes/no), parents education and occupation, age, and population density of place of residency.	Parametric log linear	EU-SILC
		2010	0.24	0.10	0.43					
US	Equalchances Project (2018)	2002	0.38	0.12	0.32	Household equivalent disposable income	Working-age individuals	Indicators of parents education and occupation, and origin (i.e., race, ethnic origin, parental culture, parental religion, or area of birth) selected by crossvalidation in each country-year.	Parametric linear	PSID
		2004	0.40	0.12	0.30					
		2006	0.40	0.12	0.30					
		2008	0.40	0.13	0.33					
		2010	0.39	0.17	0.43	Total gross hous. equiv. income				

Note: DK = Denmark; US = United States; Inequality = income inequality; AIOpI = absolute inequality of opportunity for income; RIOpI = relative inequality of opportunity for income; EU-SILC = European Union Survey on Income, Social Inclusion and Living Conditions; PSID = Panel Study of Income Dynamics. Parametric log linear estimation is as in Ferreira and Gignoux (2011). Parametric linear estimation relies on a model of income rather than log income. In studies using the EU-SILC data, household equivalent disposable income is household disposable income adjusted to account for household size and composition (in terms of adults and children of different ages); in the study using PSID data (Equalchances Project 2018), the adjustment consists of dividing household disposable income by the root of household size. In this study, disposable income is computed using a simulation model. The Equalchances Project 2010 estimate for the U.S. based on total gross household equivalent income was provided by Paolo Brunori.

Table 3. Income inequality and inequality of opportunity in two hypothetical countries (mean logarithmic deviation)

	Country A	Country B
Income inequality	0.06	0.6
Absolute IOp for income	0.006	0.03
Relative IOp for income (%)	10	5

Note : IOp = inequality of opportunity.

Table 4. Demographic and income statistics

Variables	Denmark: Income analyses	Denmark: Earnings analyses	United States: Income analyses	United States: Earnings analyses
Gender (% female)	49.5	49.4	49.0	48.9
Age				
35	25.2	25.1	24.8	24.8
36	24.7	24.7	23.8	23.7
37	24.6	24.6	25.5	25.6
38	25.5	25.6	25.9	25.9
Marriage status (% married)	60.9	NA	52.9	NA
Individual earnings				
Mean	NA	42,273	NA	36,573
Standard deviation	NA	30,594	NA	58,453
Total family income				
Mean	75,581	NA	70,418	NA
Standard deviation	100,681	NA	145,278	NA
Disposable family income				
Mean	61,362	NA	60,218	NA
Standard deviation	61,831	NA	120,056	NA
Total family income per adult				
Mean	45,667	NA	42,475	NA
Standard deviation	59,957	NA	78,176	NA
Disposable family income per adult				
Mean	37,336	NA	36,508	NA
Standard deviation	35,840	NA	63,899	NA
Average parental disposable income				
Mean	50,461	50,500	64,304	64,838
Standard deviation	73,611	73,587	150,849	157,827
Sample size	262,201	263,254	12,805	13,107

Note : Monetary values are in 2010 dollars. Monetary values for Denmark were transformed into dollars using a purchasing power parity exchange rate of 758.6 krones to 100 dollars. For the U.S., values are weighted and total and disposable income statistics (but not parental income statistics) are means across multiple-imputed income variables. NA = not applicable (variable not relevant).

Table 5. Lower bound of absolute inequality of opportunity for income

Population and income variable	Lower bound AIOpI			
	Gini coefficient		Mean Logarithmic Deviation	
	Denmark	U.S.	Denmark	U.S.
All				
Individual earnings	**0.118**	**0.234**	**0.021**	**0.090**
	(0.116 - 0.119)	(0.217 - 0.240)	(0.021 - 0.022)	(0.078 - 0.093)
Total family income	**0.092**	**0.243**	**0.014**	**0.099**
	(0.089 - 0.095)	(0.218 - 0.254)	(0.012 - 0.015)	(0.081 - 0.107)
Disposable family income	**0.072**	**0.223**	**0.008**	**0.084**
	(0.069 - 0.074)	(0.199 - 0.236)	(0.008 - 0.009)	(0.067 - 0.093)
Total family income per adult	**0.082**	**0.213**	**0.011**	**0.076**
	(0.078 - 0.085)	(0.195 - 0.222)	(0.010 - 0.013)	(0.064 - 0.082)
Disposable family income per adult	**0.056**	**0.191**	**0.006**	**0.062**
	(0.053 - 0.059)	(0.174 - 0.202)	(0.005 - 0.007)	(0.050 - 0.069)
Men				
Individual earnings	**0.093**	**0.248**	**0.014**	**0.100**
	(0.090 - 0.095)	(0.217 - 0.263)	(0.013 - 0.014)	(0.074 - 0.112)
Total family income	**0.097**	**0.252**	**0.016**	**0.105**
	(0.092 - 0.104)	(0.224 - 0.268)	(0.014 - 0.019)	(0.083 - 0.118)
Disposable family income	**0.076**	**0.227**	**0.010**	**0.086**
	(0.073 - 0.081)	(0.200 - 0.242)	(0.009 - 0.012)	(0.067 - 0.098)
Total family income per adult	**0.086**	**0.225**	**0.013**	**0.083**
	(0.080 - 0.093)	(0.201 - 0.241)	(0.010 - 0.017)	(0.064 - 0.095)
Disposable family income per adult	**0.063**	**0.200**	**0.007**	**0.066**
	(0.059 - 0.069)	(0.177 - 0.213)	(0.006 - 0.010)	(0.049 - 0.076)
Women				
Individual earnings	**0.074**	**0.147**	**0.008**	**0.039**
	(0.072 - 0.076)	(0.128 - 0.148)	(0.008 - 0.009)	(0.032 - 0.039)
Total family income	**0.083**	**0.231**	**0.011**	**0.091**
	(0.079 - 0.085)	(0.201 - 0.255)	(0.010 - 0.011)	(0.070 - 0.112)
Disposable family income	**0.059**	**0.216**	**0.005**	**0.080**
	(0.056 - 0.061)	(0.184 - 0.248)	(0.005 - 0.006)	(0.060 - 0.109)
Total family income per adult	**0.077**	**0.198**	**0.009**	**0.067**
	(0.073 - 0.079)	(0.172 - 0.218)	(0.008 - 0.010)	(0.052 - 0.084)
Disposable family income per adult	**0.049**	**0.180**	**0.004**	**0.057**
	(0.046 - 0.050)	(0.155 - 0.208)	(0.003 - 0.004)	(0.042 - 0.079)

Note : AIOpI = absolute inequality of opportunity for income. AIOpI estimates for the full population ("all") are basec on 100 types defined by gender and fiftiles of parental disposable income whereas estimates for men and women are based on types defined by fiftiles of parental disposable income. AIOpI estimates are lower-bound estimates because circumstances are partially observed. Estimates are in bold, confidence intervals are in parentheses.

Table 6. Estimates of absolute inequality of opportunity for disposable income in the United States adjusted to take into account state taxes and omitted transfers (Gini coefficient)

Population and income variable	Lower-bound AIOpI			Population and income notion	Absolute difference in lower-bound AIOpI before and after taxes and transfers (%)		
	U.S.	U.S., adjusted	Difference (%)		Denmark	U.S.	U.S., adjusted
All				All			
Disposable family income	**0.223**	**0.217**	**2.5**	Family income	**22.1**	**8.4**	**10.6**
	(0.199 - 0.236)	(0.194 - 0.230)			(21.6 - 22.5)	(6.3 - 9.7)	(8.5 - 11.9)
Disposable family income per adult	**0.191**	**0.185**	**3.5**	Family income per adult	**31.3**	**10.2**	**13.3**
	(0.174 - 0.202)	(0.167 - 0.195)			(30.7 - 32.1)	(8.1 - 11.6)	(11.3 - 14.7)
Men				Men			
Disposable family income	**0.227**	**0.222**	**2.2**	Family income	**21.5**	**9.9**	**11.8**
	(0.200 - 0.242)	(0.195 - 0.237)			(20.9 - 22.2)	(8.9 - 11.2)	(10.9 - 13.1)
Disposable family income per adult	**0.200**	**0.194**	**3.1**	Family income per adult	**26.8**	**11.3**	**14.1**
	(0.177 - 0.213)	(0.172 - 0.206)			(26.1 - 27.5)	(10.3 - 12.9)	(13.1 - 15.7)
Women				Women			
Disposable family income	**0.216**	**0.210**	**2.7**	Family income	**28.9**	**6.7**	**9.2**
	(0.184 - 0.248)	(0.179 - 0.242)			(28.3 - 29.7)	(2.9 - 9.0)	(5.5 - 11.4)
Disposable family income per adult	**0.180**	**0.173**	**3.9**	Family income per adult	**36.4**	**8.8**	**12.3**
	(0.155 - 0.208)	(0.149 - 0.200)			(35.6 - 37.3)	(4.7 - 11.2)	(8.4 - 14.7)

Note : AIOpI = absolute inequality of opportunity for income. AIOpI estimates for the full population ("all") are based on 100 types defined by gender and fiftiles of parental disposable income whereas estimates for men and women are based on types defined by fiftiles of parental disposable income. AIOpI estimates are lower-bound estimates because circumstances are partially observed. Estimates are in bold, confidence intervals are in parentheses. The second column reproduces results from Table 5. The adjusted AIOpI estimates in the third column are obtained by multiplying the figures in the second column by adjustment factors estimated with a sample of people ages 35-38 in 2010 (when their income is measured) from the Annual Social and Economic Supplement of the 2011 Current Population Survey (CPS-ASEC). The factors are computed by generating approximations to the notion of disposable family income used in the Statistiscs of Income Mobility (SOI-M) panel and to true disposable family income (overall and per adult). These approximations use as inputs information on income from all sources and various transfers collected by the CPS-ASEC as well as information on federal and state liabilities (after credits) computed by the Census Bureau with a tax model (and included in the CPS-ASEC). The transfers include income from Transitory Assistance for Needy Families, Social Security (including disability insurance), Supplemental Security Income, the Veterans' Administration, and various small disability programs. The Gini coefficient is computed with the two disposable income measures (separately for men, women and men and women pooled, and with overall and per-adult income). The adjustment factors are ratios of the Gini estimates based on the approximations to the SOI-M notion of disposable income and true disposable income. The computation of the adjustment factors assumes that AIOpI estimates based on the SOI-M panel's measure of disposable income overstate AIOpI estimates based on true disposable income by the same proportional factor that the estimation of overall inequality with that measure overstates overall inequality in true disposable income.

Table 7. Income inequality and lower-bound of absolute and relative inequality of opportunity for income (Gini coefficient, adjusted U.S. disposable-income estimates of absolute inequality of opportunity)

Population and income variable	Denmark			United States		
	Lower-bound AIOpI	Inequality	Lower-bound RIOpI (%)	Lower-bound AIOpI	Inequality	Lower-bound RIOpI (%)
All						
Individual earnings	**0.118**	**0.311**	**37.9**	**0.234**	**0.523**	**44.7**
	(0.116 - 0.119)	(0.309 - 0.312)	(37.3 - 38.3)	(0.217 - 0.240)	(0.514 - 0.534)	(41.7 - 45.4)
Total family income	**0.092**	**0.318**	**28.9**	**0.243**	**0.479**	**50.7**
	(0.089 - 0.095)	(0.316 - 0.321)	(28.0 - 29.7)	(0.218 - 0.254)	(0.468 - 0.494)	(47.2 - 51.7)
Disposable family income	**0.072**	**0.263**	**27.3**	**0.217**	**0.443**	**49.1**
	(0.069 - 0.074)	(0.261 - 0.265)	(26.4 - 28.0)	(0.194 - 0.230)	(0.432 - 0.459)	(45.4 - 50.5)
Total family income per adult	**0.082**	**0.266**	**30.6**	**0.213**	**0.431**	**49.4**
	(0.078 - 0.085)	(0.264 - 0.270)	(29.6 - 31.6)	(0.195 - 0.222)	(0.420 - 0.446)	(46.3 - 50.2)
Disposable family income per adult	**0.056**	**0.189**	**29.6**	**0.185**	**0.388**	**47.6**
	(0.053 - 0.059)	(0.187 - 0.192)	(28.5 - 30.7)	(0.167 - 0.195)	(0.378 - 0.404)	(44.5 - 48.6)
Men						
Individual earnings	**0.093**	**0.312**	**29.8**	**0.248**	**0.516**	**48.0**
	(0.090 - 0.095)	(0.310 - 0.314)	(28.9 - 30.4)	(0.217 - 0.263)	(0.503 - 0.532)	(43.1 - 49.9)
Total family income	**0.097**	**0.315**	**30.8**	**0.252**	**0.483**	**52.1**
	(0.092 - 0.104)	(0.312 - 0.320)	(29.5 - 32.5)	(0.224 - 0.268)	(0.469 - 0.501)	(47.5 - 54.2)
Disposable family income	**0.076**	**0.268**	**28.5**	**0.222**	**0.448**	**49.5**
	(0.073 - 0.081)	(0.265 - 0.271)	(27.2 - 30.0)	(0.195 - 0.237)	(0.435 - 0.464)	(45.3 - 51.5)
Total family income per adult	**0.086**	**0.272**	**31.5**	**0.225**	**0.442**	**51.0**
	(0.080 - 0.093)	(0.268 - 0.278)	(29.9 - 33.7)	(0.201 - 0.241)	(0.428 - 0.459)	(46.7 - 53.0)
Disposable family income per adult	**0.063**	**0.200**	**31.3**	**0.194**	**0.4**	**48.4**
	(0.059 - 0.069)	(0.197 - 0.205)	(29.6 - 33.5)	(0.172 - 0.206)	(0.387 - 0.416)	(44.4 - 50.2)
Women						
Individual earnings	**0.074**	**0.289**	**25.6**	**0.147**	**0.517**	**28.5**
	(0.072 - 0.076)	(0.287 - 0.290)	(24.8 - 26.2)	(0.128 - 0.148)	(0.505 - 0.529)	(25.0 - 28.6)
Total family income	**0.083**	**0.320**	**25.9**	**0.231**	**0.474**	**48.8**
	(0.079 - 0.085)	(0.317 - 0.324)	(24.7 - 26.5)	(0.201 - 0.255)	(0.458 - 0.499)	(43.8 - 51.6)
Disposable family income	**0.059**	**0.257**	**22.9**	**0.210**	**0.437**	**48.1**
	(0.056 - 0.061)	(0.254 - 0.260)	(21.8 - 23.5)	(0.179 - 0.242)	(0.420 - 0.467)	(42.6 - 52.2)
Total family income per adult	**0.077**	**0.260**	**29.4**	**0.198**	**0.419**	**47.2**
	(0.073 - 0.079)	(0.257 - 0.264)	(28.2 - 30.1)	(0.172 - 0.218)	(0.403 - 0.444)	(42.4 - 49.7)
Disposable family income per adult	**0.049**	**0.177**	**27.5**	**0.173**	**0.375**	**46.3**
	(0.046 - 0.050)	(0.175 - 0.180)	(26.2 - 28.2)	(0.149 - 0.200)	(0.359 - 0.402)	(40.9 - 49.8)

Note : AIOpI = absolute inequality of opportunity for income; inequality = income inequality; RIOpI = relative inequality of opportunity for income. Estimates for the full population ("all") are based on 100 types defined by gender and fiftiles of parental disposable income whereas estimates for men and women are based on types defined by fiftiles of parental disposable income. AIOpI and RIOpI estimates are lower-bound estimates because circumstances are partially observed. Estimates are in bold, confidence intervals are in parentheses. AIOpI estimates for Denmark are from Table 5. AIOpI estimate for the United States are from Table 5 (individual earnings, total family income, and total family income per adult) and Table 6 (disposable income and disposable income per adult). The estimates of overall income inequality are divided by an adjustment factor to account for the upward lifecycle bias generated by the estimation of inequality in long-run income with an annual measure of income obtained at ages 35-38. The adjustment factor, 1.11, is computed as explained in the caption to Figure 5.

Table 8. Inequality of opportunity for income in the United States: Accounting for race and ethnicity (Total family income, full population)

Inequality index	Without race and ethnicity		With race and ethnicity	
	Lower-bound AIOpI	Lower-bound RIOpI (%)	Lower-bound AIOpI	Lower-bound RIOpI (%)
Gini coefficient	0.243	50.7	0.276	57.6
MLD	0.099	NA	0.150	NA

Note : AIOpI = absolute inequality of opportunity for income; RIOpI = relative inequality of opportunity for income; NA = not applicable (estimate not available). AIOpI and RIOpI estimates are lower-bound estimates because circumstances are partially observed. Estimates in the left panel are from Table 7. They are based on 100 types defined by gender and fiftiles of parental income. Estimates in the right panel are obtained by multiplying the estimates in the left panel by adjustment factors specific to each inequality measure. The adjustment factors are estimates of how much larger the AIOpI estimates would be if the types were based on gender, fiftiles of parental income, and the race/ethnicity categories used by Chetty et al. (2020). See the text for details about their computation.