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

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Abstract

This article carries out a comparative analysis of inequality of opportunity for long-run income in Denmark and the United States. It uses high-quality administrative data for both countries as well as samples that represent the full populations of interest. In addition, it relies on improved methods and advances a plausible identification assumption that allows to legitimately compare lower-bound estimates of inequality of opportunity across the two countries. There are three main findings. First, inequality of opportunity for long-run income is very high in the United States. With types based only on gender and parental income rank as circumstances outside of people's control, the lower-bound Gini coefficients for individual earnings and family income opportunities are around 0.24. An extension of the analysis to also account for race and ethnicity suggests an upward adjustment of the Gini coefficient for family income opportunities of 14 percent; this implies that no less than 52 percent of long-run family income inequality is due to circumstances outside of people's control. Second, inequality of opportunity for long-run income is far from negligible in Denmark. The lower-bound Gini coefficients for individual earnings and family income opportunities are around 0.10. Lastly, inequality of opportunity for long-run income is radically higher in the United States than in Denmark, and this result is very robust to the inequality measure employed in the analysis. For long-run disposable income opportunities, with factor in taxes and public transfers (including refundable tax credits), the Gini coefficient is no less than 2.8 times higher in the United States whereas the mean logarithmic deviation is no less than 8.2 times higher.

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Introduction

For a long time, researchers interested in studying inequality in economic opportunities within countries, as well as its variation across countries, only did it indirectly, by focusing on economic mobility across generations (see, e.g., Corak 2006; Mitnik et al. 2018; Solon 1999). The study of economic mobility—in particular, the estimation of intergenerational income and earnings elasticities, or IGEs—was explicitly or implicitly motivated by the notion that (im)mobility levels provide information on how (un)equal opportunities are.[1] At least in part, this focus on mobility was a response to 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). As luck is often interpreted as the opposite of what individuals are responsible for (e.g., Cohen 2006:442), luck egalitarianism has been also called "responsibility-sensitive egalitarianism."[2]

Luck egalitarians argue that income and other 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 whereas those arising from differences in effort are considered just, but only as long as they cannot be traced back to differences in circumstances.[3] Therefore, for any outcome of interest, the luck-egalitarian normative ideal is an outcome distribution where efforts are rewarded adequately (the "reward principle") and the effect of circumstances is fully compensated for (the "compensation principle"). In this ideal context, all existing disparities are due to effort differentials not accounted by circumstances.

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

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[1] An exact account of the relationship between these two pairs of complementary concepts, 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).

measured by establishing how far a society is from satisfying the compensation principle, that is, by measuring the inequalities that exists across people with the same levels of effort or the inequality in the value of the opportunity sets of people with different circumstances. Moreover, 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 literature has produced estimates of IOpI for many countries and—as in the economic-mobility literature, and for mostly similar reasons—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)? More specifically, how do social-democratic countries like Denmark, Norway, Finland 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 (e.g., Björklund and Jäntti 1997; Bratsberg et al. 2007; Corak 2006; Esping-Anderson 2015; Helsø 2018; Mitnik, Bryant and Grusky 2018). However, the *direct* evidence that is available 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) a series of suboptimal methodological decisions, (b) the widespread use of an incorrect estimation approach, and (c) the fundamental methodological difficulty generated by the all-encompassing nature of the theoretical notion of circumstances (which includes *all* factors outside of people's control). In addition, the studies on which a comparison between the U.S. and the social-democratic countries could be based have important empirical limitations. We discuss these four problems in turn.

The IOp literature is affected by three suboptimal methodological decisions. First, IOpI scholars have not focused on the IOp in *long-run* income (for a notable exception, see Björklund, Jäntti and Roemer 2012). However, as it has long been well understood in the mobility literature (e.g., Jenkins 1987; Black and Devereux 2011; Solon 1992; Solon 1999), this is the income of most interest from a normative point of view. Second, in selecting the inequality measure to be used in their analyses, researchers 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, which makes the interpretation of absolute levels of IOpI (AIOpI) measured with the MLD difficult. Third, perhaps due to those interpretative difficulties, IOp scholars have given at least as much attention in their analyses (and often much more, e.g., Brunori et al. 2013) 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 normative assessments, especially when comparing countries. Moreover, as discussed in detail by Mitnik (2020a), once the focus is on long-run income, the denominator of RIOpI, overall long-run income

inequality, cannot be consistently estimated with the same data that produce the optimal estimates of AIOpI (although informative upper-bound estimates may still be generated with some inequality measures). And, as explained later, still other serious problems will most likely emerge when trying to estimate overall income inequality if the inequality measure employed is the MLD.

In addition to these suboptimal decisions, and in direct analogy to what has been the case with the estimation of the income IGE in the economic mobility literature (see Mitnik and Grusky Forthcoming), the approach typically used to produce estimates of IOpI is flawed. 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 2020b), does not do what 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 and, as a result, it estimates something other than what it is trying to estimate (Mitnik 2020b). As a corollary, estimates produced with this approach cannot be properly compared to estimates produced with the more sparingly used "nonparametric approach" for IOpI estimation (e.g., Checchi and Peragine 2010).

The fundamental methodological difficulty, very 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, see Luongo 2011). Comparisons of AIOpI estimates across countries are therefore complicated affairs, and not nearly enough attention has been paid to the conditions under which those comparisons may be informative. These difficulties are compounded when the goal is to conduct RAIOpI comparisons with a focus on 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. This provides another reason to privilege AIOpI over RAIOpI in cross-country comparisons.

Beyond these general problems, IOpI in the U.S. and the social-democratic countries has been measured using different circumstances and income concepts, relying on different estimation approaches, and 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. In addition, some estimates are based on survey data that do not cover well the upper tail of income distributions, which 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), any conclusions drawn are likely to 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, to boot, methods, cohorts, periods, and income notions are all well aligned. And, because we use the nonparametric approach for IOpI estimation, our results are immune to the problems affecting the many estimates generated with the parametric log-linear approach.

Unlike almost all previous estimates in the literature, ours pertain to long-run income; we provide a clear justification—based on an empirically-validated measurement-error model developed by Mitnik (2020a)—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, both because it is a theoretically attractive measure (e.g., Ferreira and Gignoux 2011) and because this has the virtue of allowing direct comparison with a large share of the estimates previously reported in the literature. But we also use the Gini index (the second inequality measure most used in the literature), which is double bounded and therefore much easier to interpret than the MLD and other indices with no upper bound. In addition, 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 rely on a very sparse set of variables, gender and parental-income rank (and parental-income rank alone for within-gender analyses), to define circumstances. We show that, with our data and methodological approach, AIOpI computed with respect to this highly incomplete set of circumstances generates highly informative lower-bound estimates. We also discuss two explicit identification assumptions under which point and set (i.e., lower or upper bound) estimates of AIOpI *ratios* between countries—that is, ratios of AIOpI in one country relative to another—are obtained with an incomplete set of circumstances. We further argue that the weaker of these assumptions is most likely correct when the goal is to estimate ratios of AIOpI in the U.S. relative to AIOpI in Denmark using gender and parental-income rank to define circumstances. This entails that our estimates of these ratios—which reflect how much AIOpI there is in the U.S. compared to Denmark—are lower-bound estimates. In an extension of our analyses, only for the U.S., we provide approximate estimates of how much larger AIOpI is when race and ethnicity are added to the set of considered circumstances. The main conclusion of the article is that both AIOp for long-run earnings and AIOp for long-run family income (total and disposable) are radically higher in the United States than in Denmark

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 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 and variables, approach to estimation and statistical inference, and empirical results. The last two sections discuss the main implications of our empirical results and distill the article's key conclusions.

From theorical principles to measurement

Most empirical studies in the IOpI literature have adopted a notion of IOpI 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) (absolute) *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 that have produced AIOpI and RIOpI estimates for Denmark and 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 0.01-0.020 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 IOpI in Denmark and the U.S.

Table 2 summarizes the three studies that have produced estimates for Denmark and 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.[4] 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.[5] This suggests substantially higher AIOpI in the U.S. than in Denmark but is not inconsistent with there being low levels of AIOpI in both countries. Moreover, as we explain below, although these estimates are not affected

[4] 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.

[5] 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 AIOp in the country. The fact that the estimates for 2002-2006 are very similar provides reassurance.

by several of the methodological problems impacting all 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 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, and 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 IOpI measurement 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).[6] Another limitation, exclusive to the PSID data, is that this survey only makes available full income information—in particular, individual earnings information—for household heads and their spouses (or cohabiting partners) rather than for the full adult 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.[7]

The administrative data we use in this article are essentially immune to all the limitations just discussed. This does not mean that they do not have their own limitations, which we discuss below. Nevertheless, in many dimensions they provide a better foundation for carrying out a comparative assessment of IOpI in Denmark and the U.S. than the data used before.

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

[7] 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.

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

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, very often the data demands of this strategy cannot be satisfied by the samples available. What the literature has referred to as the parametric approach, and we prefer to call, following Mitnik (2020b), the parametric log-linear approach, was introduced to address this problem. It involves (a) running a linear regression of log income on dummies for the categorical circumstances—e.g., gender, race, parental education—defining the types (often without any interactions), (b) computing predicted values for all individuals, (c) exponentiating these predicted values, which are interpreted as opportunity-set values, and (d) computing the chosen inequality measure over the resulting smoothed distribution.

Using Z an 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. As in the general case $D\big(GM(Z|C)\big) \neq D\big(E(Z|C)\big)$, it follows, first, that these estimates do not pertain to IOpI as defined 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, second, that the estimates produced by the parametric log-linear approach are not directly comparable to those produced by the nonparametric approach (Mitnik 2020b). 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. But focusing instead on family- or household-based income measures—as sometimes is 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).[8]

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, we contend, populations of interest.

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

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) has argued 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.

More on selected samples

In addition to the unjustified exclusion of people with zero income or earnings from analyses, IOpI studies have often used samples were, 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, their incomes are 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 data from the PSID can only cover household heads and their spouses. As being a household head (or her/his spouse) is endogenous to own income and characteristics other than measured circumstances, the resulting sample does not represent 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, let's say, 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.[9] 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 estimation of upper bounds for IOpI measures that they introduced in their study.

In other cases, however, the reduced relevance of the populations represented by the samples 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".[10] But many household heads

[9] 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.

[10] 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).

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 them, they represent well the birth cohorts 1972-1975 (in their late 30s).

The unsettled status of age

Nearly all previous studies of IOpI have computed IOpI measures by taking as outcome or advantage variable the annual income (or some other short-run income measure) of a large number of cohorts, e.g., people 25-55 years old in the one year (or in the few years) their incomes are measured (see Tables 1 and 2 for many 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 not under 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. Although this seems obvious, 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 would tend to overstate IOpI. Unfortunately, the alternative of just ignoring age is also unsatisfactory, as it tends to unnecessarily underestimate IOpI by fully ignoring cohort effects, which are likely to be nonnegligible given the broad populations (in terms of cohorts) used in the empirical analyses.

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 the vast majority of previous studies have done in their analyses.

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

What's 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). In addition, 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,

Forthcoming). 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 at all about the relationship between their estimates and those that would be obtained with long-run (e.g., lifetime) income measures if they were available (for a notable exception, see Björklund, Jäntti and Roemer 2012) 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 (Haider and Solon 2006; Nybom and Stuhler 2016; Mitnik 2019, 2020c). 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 (2020a) showed that using a short-run income measure to proxy for long-run income when producing AIOpI estimates leads to similar lifecycle biases. He advanced 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.[11] 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 (2020a) showed 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 exactly the same circumstances and estimation method we use in our analysis here, he provided 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.

[11] 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 2020a).

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 Norway register data for men, Aaberge and Mogstad (2015) provided evidence on these biases. They showed 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 a little bit 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 measurement age. Similarly, using the same PSID data mentioned above, Mitnik (2020a) found that, with a large array of inequality measures (although not all he considered), lifecycle bias starts somewhat negative in the mid-20s, crosses zero soon after that (between ages 27 and 33, depending on the measure), and is then positive and increases monotonically with age. In particular, he reported that the bias disappears (i.e., 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 (e.g., the standard deviation of log incomes) the bias is positive at all ages.

The foregoing entails that estimation of the RIOp for long-run income with a short-run income variable is affected by not fewer than two, and possibly three, biases. The first one is the negative bias affecting estimation of the AIOpI with partially observed circumstances. The second one is the negative (positive) bias affecting estimation of the AIOpI when the income measure is obtained earlier (later) than, let's say, age 37 (age 43). And, simplifying things a little bit in interpreting the evidence just discussed, the third one 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. Computing RIOpI close to age 40, as we do here, still produces lower-bound estimates of long-run RIOpI, but these estimates are affected by two downward biases, not just the one due to the fact that circumstances are partially observed (as was the case with the AIOpI estimates).

Mitnik (2020a) has shown that, when estimating RIOpI with short-income variables obtained around age 40, the third bias is substantially smaller with the Gini coefficient than with the MLD. This is one of the reasons why in this article we only report RIOpI estimates based on the Gini coefficient. The second reason is that if inequality is measured with the MLD and the available (i.e., short-run) income variable includes zeros, then 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 simply dropped), whereas the estimand simply does not exist if the long-run income variable also includes zeros.[12] Moreover, replacing zeros by a "small amount"—e.g., by the value 1,

[12] The MLD is equal to the difference between the logarithm of the expectation and the logarithm of the geometric mean of a distribution. Therefore, given that the latter is undefined in the presence of zeros, the MLD is undefined as well when this is the case.

as Checchi et al. (2015) did in some of their analyses—is not a good strategy, as the MLD is very sensitive to the exact amount that is substituted.[13] Importantly, most of this argument against relying on the MLD (a) would apply even if the focus were on the RIOp for income at some particular age rather than the RIOp for long-run income, and (b) does not apply to the AIOpI of either short-run or long-run income because in empirical studies mean income within types can be expected to be positive for all types.

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 (e.g., Brunori et al. 2013). However, AIOpI estimates are the ones that are relevant for normative assessments. Imagine, for instance, that we want to compare how countries A and B are doing in terms of IOp for income, and we are shown the results in Table 3 (which, for simplicity, we assume now are point estimates rather that lower-bound estimates). The distribution of income opportunities 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 accounted for by circumstances outside of people's control, is an interesting quantity, even if it is not that relevant for normative assessments (in particular when these involve country comparisons). For this reason, we do report RIOpI results here, although more briefly. Moreover, we suspect that RIOpI's intrinsic interest is not what explains its popularity in the literature. Rather, it is that for analyses relying on the MLD, they add an intuitive way of interpreting individual estimates. The problem is that these intuitive interpretations concern something else, not what the normative theory motivating the empirical research is about. If providing individual estimates of AIOpI that are easy to interpret is important—and we do think that this is the case—that is immediately achieved if one uses 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. In addition, with the Gini coefficient AIOpI can be further interpreted. For instance, it can be interpreted (a) graphically, in terms of the Lorentz curve, and (b) as the ratio between the mean absolute difference in income opportunities between pairs of randomly chosen people and the population's mean income.[14]

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

[14] In contrast, the best that can be done with the MLD, and only for small values (let's say, values smaller than 0.20), is to interpret it as the approximate percent difference between the mean and the geometric mean of people's income opportunities.

Lower-bound estimation of inequality of opportunity: The problem of cross-country comparisons

The fundamental methodological problem of the empirical research on IOp is that AIOp is always measured with respect to an incomplete set of circumstances. Indeed, Luongo (2011) provided a general formal proof showing that, as long as the inequality index used to measure AIOp is Lorenz-consistent, any IOp estimate based on incomplete circumstances will be a lower-bound estimate.[15] Individual AIOp estimates may still be informative but comparing AIOp estimates across countries and times becomes very challenging. IOpI studies conducting cross-country comparisons and trend analyses have all explained that their estimates are lower-bound estimates but have not tried to advance explicit identification assumptions that would justify stronger conclusions that that fact alone allows—even if, very often, they at the very least have flirted with those stronger interpretations.

As in Mitnik et al. (2019), let's stipulate that "modulo *W*" means "computed with respect to the incomplete set of circumstances *W*." Then, one identification assumption that allows for the type of analyses and conclusions one sometimes find in empirical studies, is this:

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

FRA, which might be more plausible for within-country trend analyses than for cross-country comparisons, allows to make many sorts of comparative statements (e.g., in country A, IOpI in 2010 is *X* percent higher than in 2004, or the ratio of IOpI between countries A and B is *Y*) as well as statements about linear correlations or nonlinear dependencies between AIOpI and other variables (e.g., overall inequality, GDP per capita). It also allows to use AIOpI-modulo-*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 interpret the qualitative results (although not the magnitude of the coefficients) as pertaining to AIOp *tout court*.

A second, much weaker, identification assumption, which nevertheless allows to make some important comparative statements, is the following:

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

Let's say that RIA holds for countries A and B. Then, the ratio of AIOpI modulo *W* between A and B is a lower-bound estimate of the AIOpI ratio between A and B. So, if the estimated ratio is, let's say, 3, this RIA allows us to say that AIOpI in country A is *at least* 3 times AIOpI in country B.[16]

In this article, we posit that, given the circumstances we consider in our analyses, i.e., gender and parental-income rank, RIA applies with the U.S. as country A and Denmark as country B. Our argument is

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[15] See also Ferreira and Gignoux (2011). Their better-known proof applies to one such Lorenz-consistent measure, the MLD.

[16] Of course, in some circumstances it may be more convenient to use as identification assumption a not-larger-than version of RIA, for which a similar analysis applies.

the following. Roughly speaking, there are four main types of circumstances: gender; race, ethnicity, immigration status and other "origin" circumstances; circumstances pertaining to characteristics of the family of origin (e.g., parental income, parental education, parental class, family structure), and circumstances pertaining to the place of residence when growing up. Let's call the circumstances in these four groups primary circumstances. There are, of course, many circumstances not included in these groups (e.g., school quality). Let's call them secondary circumstances. In our empirical analyses we only include gender and one family-level circumstance, parental-income rank. We make four assumptions:

(a) AOpI modulo all circumstances (primary and secondary) is larger but not much different than AOpI module primary circumstances in both Denmark and the U.S; as a first approximation, the difference may be ignored.
(b) Parental-income rank is a reasonable proxy for the circumstances pertaining to the family of origin, in the sense that types defined in terms of parental-income rank are decent predictors of the types defined in terms of all family-of-origin circumstances; the quality of this approximation is similar in Denmark and the U.S.
(c) Race, ethnicity, immigration status and other origin circumstances are not less consequential for economic outcomes in the U.S. than in Denmark.
(d) Place of residence when growing up is not less consequential for economic outcomes in the U.S. than in Denmark.

The first two assumptions are untestable, they are akin to, let's say, exclusion restrictions in sample selection models. Nevertheless, the first assumption is quite plausible while the first part of the second assumption, and a more general version of its second part, is implicit in a large body of research that uses parental income to index people's socioeconomic background and carry out cross-country comparisons. Lastly, there is good evidence for the other two assumptions. First, the more socioeconomic residential segregation there is, the more place of residence when growing up matters for people's income as adults; and the level of income segregation among schools, which is a good proxy for socioeconomic residential segregation, is much lower in Denmark (Chmielewski and Reardon 2016, Figure 4). Second, given the extraordinary role of race in U.S. history and the well documented present-day differences in economic outcomes across races (e.g., Chetty et al. Forthcoming), it defies credibility to think that race could be more consequential in Denmark. Less conclusive but still strong versions of this argument also apply to ethnicity and immigration status.

Based on assumptions (a) – (d), we expect RIA to hold. Under RIA, the estimates we will be reporting of ratios between AIOpI modulo gender and parental income for the U.S. and Denmark are lower-bound estimates of the corresponding AIOpI ratios.

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.[17] We use information on their (a) gender, (b) total family income, disposable family income and individual earnings in 2010, and (c) parents' average disposable family income when those in the sample were 15-23 years old.[18]

In the U.S. data, due to differences in data availability, the income concepts are not measured identically for people 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 also includes nontaxable earnings. For nonfilers in that year, total income is the sum of earnings from the W-2 form and 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 Current Population Survey Annual Social and Economic Supplement on likely nonfilers without UI income or earnings.[19] 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 (e.g., Temporary Assistance for Needy Families) are not included, it follows that this is an approximation to true disposable income. 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.

The samples used for our analyses exclude children 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 children 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 Urban Consumers - Research Series (CPI-U-RS) for the U.S. and the Consumer Price Index for Denmark; we further transform values in Danish krones into U.S. dollars using a purchasing power parity exchange rate of 758.6 krones to 100 dollars.

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[17] 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).

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

[19] 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.

For auxiliary purposes, we also use aggregate tax-based U.S. statistics that Chetty et al. (2020) 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 average 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. (2020) 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, 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 the type-specific means to people, and (d) computing a selected 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 use only these variables 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, multiple imputation for nonfilers without any administrative information is nested within the bootstrap procedure: we use the same source of data and approach employed in the SOI-M Panel to generate imputed income variables and re-impute them within each bootstrap sample. This way the resulting variability reflects not only sampling variability across bootstrap samples but also the additional variability generated by the multiple imputation that occurs within each bootstrap sample. We report bias-corrected bootstrap confidence intervals (e.g., Efron 1987).

Results

Absolute inequality of opportunity for income

We start by presenting our AIOpI earnings results. Taking advantage of the fact that our types are defined by only two variables, we offer in Figure 1 a graphical representation of how mean earnings vary across types—represented by the blue and red dots—in Denmark (left panel) and the U.S. (right panel). To facilitate the interpretation of the U.S. results and the visualization of how within-type mean earnings behave in the two countries, we have superimposed nonparametrically estimated curves showing the relationship

between those earnings and parental fiftiles by gender.[20] In both countries, mean earnings increase with parental fiftile, regardless of gender. But this association is more marked in the U.S. than in Denmark, especially for men at the top of the parental income distribution and for women at the bottom. It is also notable that while in the U.S. there is hardly any difference between men's and women's expected earnings at the bottom of the parental income distribution, this difference increases rapidly and becomes quite large in the top parental decile and extraordinarily large at the very top of the distribution. In contrast, the differences across genders are much closer to constant across parental fiftiles in Denmark, although the curves for men and women also diverge more widely (although much less than in the U.S.) at the top of the parental income distribution.

As shown in the upper-left area of each of the figure's panels, the patterns we just described result in very large cross-country differences in AIOp for earnings (or, rather, in their lower-bound estimates, a qualification we will repeat sparingly in what follows). As measured by the Gini coefficient, which is more sensitive to inequality at the center of a distribution, the earnings AIOp for men and women pooled ("all" in the figure) is close to 0.12 in Denmark and above 0.23, nearly twice as much, in the U.S. Moreover, while the proportional difference between women's Gini values for the two countries (about 0.07 and 0.15, respectively) is similar to that for the whole population, that difference is substantially larger among men, for whom the estimates are close to 0.09 and 0.25, respectively.

In contrast to the Gini coefficient, the MLD is more sensitive to inequality in the upper part of a distribution. Not surprisingly given the patterns shown in Figure 1, this translates in that the proportional differences in AIOp for earnings across countries are markedly larger than with the Gini. Thus, the earnings AIOp for men in the U.S. is as much as seven times larger than for men in Denmark, whereas for men and women pooled, and for the latter alone, it is about four times larger.

Figure 2 is similar to Figure 1, but now the panels include four curves each; these represent the total and disposable family income (solid and dashed lines) of men and women (as before, blue and red lines). To avoid cluttering, we have excluded from this figure the dots representing the computed within-type means but, as can be seen in Figures A1 and A2 in the Appendix, the curves summarize them well. Within countries and keeping the income notion fixed, and unlike what was the case with individual earnings, there is very little difference in the shapes of the income curves across genders (although now the women's curves tend to be a little bit above the men's curves). This reflects that there is 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" marriage (i.e., spouse's earnings), for women those shares are about 29 and 71 percent, respectively. The left panels of Figures 1 and 2 indicate that a similar, but smaller asymmetry, can be found also in Denmark.

If the only source of income were the labor market and everyone were married (and with somebody of the opposite sex also born in 1972-1975), the total income curves of Figure 2 would simply be the

[20] Curves shown in these and other figures were obtained with local polynomial regressions of degree 1, using the Epanechnikov kernel function and a bandwidth selected automatically by a rule-of-thumb estimator. Regressions were run on the already-computed mean values, not on the microdata.

horizontal sum of the earnings curves for men and women shown in the previous figure. And, as men tend to have larger earnings than women, we would expect the total income curves to resemble those for men's earnings. Of course, these conditions do not fully obtain. Nevertheless, Figure 2 shows that, in each country, the total income curves, for men and women alike, behave similarly to the men's earnings curves of Figure 1. This results in very large differences in AIOp for total income (for men and women pooled); in fact, the estimates of AIOp for total income are, both for Denmark and the U.S., extremely close to the estimates of AIOp for men's earnings shown in the previous figure.

Due to the impact of taxes, the disposable income curves are in all cases below their total income counterparts. Because of higher tax rates in Denmark, the proportional downward shifts of these curves are larger in that country at all parental fiftiles. For men and women pooled, the actual within-type disposable income means (rather than the values defining the disposable income curves) are on average 18 percent lower than the within-type total income means in Denmark compared to 13 percent lower in the U.S. Crucially for our AIOpI comparison, and reflecting the combined effects of a more progressive tax system and a more generous welfare state (e.g., Kenworthy 2020), 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 only marginally different. As a result, whereas in the U.S. AIOp for disposable income, measured with the Gini coefficient, is only 8.4 percent smaller than with total income (0.22 compared to 0.25), in Denmark the disposable-income Gini is 22.1 percent smaller (0.072 compared to 0.092). This difference across the countries is even more marked when AIOpI is measured with the MLD. With this inequality measure, the AIOp for disposable income is a whooping 38 percent lower than the AIOp for total income in Denmark compared to 15.5 percent lower in the U.S.

Figure 3 summarizes our key comparative findings on AIOpI by presenting them in the form of AIOpI ratios (only for men and women pooled). Such ratios represent how much more inequality of opportunity there is in the U.S. compared to Denmark. The two sets of bars in the figure pertain to AIOpI measured with the Gini coefficient and the MLD, respectively. Each set shows our estimates of AIOpI ratios for individual earnings and total and disposable family income, as well as the corresponding confidence intervals. As we explained in some detail above, because place of residence in childhood, on the one hand, and race, ethnicity, immigration status and other similar circumstances, on the other, can be expected to be much more consequential for people's economic outcomes in the U.S. than in Denmark, the fact that we did not include them when defining our types entails that our estimates of AIOpI ratios are lower-bound estimates of how much more inequality of opportunity for income there is the U.S. than in Denmark. (In the case of the disposable income ratios, however, the reported lower-bound estimates can be assumed to be somewhat larger than they would have been with a true rather than an approximate measure of disposable income for the U.S., a caveat that should be kept in mind.)

For ratios based on the Gini coefficient, which is more sensitive to inequality at the center of a distribution, our estimates indicate that AIOp is at least two times higher in the U.S., in the case of long-run individual earnings, and at least 2.6 times higher in the case of long-run total income (with the estimated ratio for disposable income even larger). The MLD is more sensitive to inequality in the upper part of a distribution and our estimates of AIOpI ratios based on this inequality measure are substantially larger. The point estimates indicate that the AIOp for long-run earnings is at least four times higher in the U.S. than in

Denmark whereas the AIOp for total income is at least seven times higher. And although the disposable income estimate is more imprecise and is affected by the approximate nature of the U.S. income measure, the figure suggests that the AIOp for disposable income is very unlikely to be less than eight times higher in the U.S. than in Denmark.

Relative inequality of opportunity for income

How much of a country's income inequality is accounted for by circumstances outside of people's control or, in other words, what share of income inequality can be deemed unfair? This may not be relevant for comparative assessments of how countries are doing in terms of inequality of opportunity but is an interesting and important descriptive quantity nonetheless—and one the previous literature has extensively reported. Figure 4 summarizes our results. For reasons we already discussed, we only report results based on the Gini coefficient. These estimates need to be interpreted as lower-bound estimates of the share of long-run income inequality that is accounted for by circumstances outside of people's control. As we explained, this is the case not only because AIOpI estimates are lower-bound estimates but also because estimates of long-run income inequality based on short-run income measured in the late 30s can be expected to overestimate such inequality.

The figure shows that, in Denmark, at least one third of long-run earnings inequality is accounted for circumstances outside of people's control, while that is the case for at least one quarter of long-run family income inequality (both total and disposable income). In the U.S., the estimated shares of unfair inequality are all larger, even though overall inequality is substantially higher than in Denmark, regardless of income measure.[21] Those estimates indicate a lower bound of no less than two fifths of unfair inequality in long-run earnings, and somewhat more than that in the case of long-run family income.

Robustness check: Other inequality measures

Are our results robust to the choice of inequality measure? We address this issue now. We examine whether our qualitative comparative conclusions stand when we use inequality measures other than the Gini coefficient and the MLD to compute AIOpI.

Figure 5 shows estimates of AIOpI ratios between the U.S. and Denmark based on eight inequality measures in addition to the Gini coefficient and the MLD. Some of these added measures are Gini-type indices that give more weight to low incomes (Kakwani and Mehran) or to high incomes (Piesch). Some belong to the class of generalized entropy (GE) indices, which are a function of a "sensitivity parameter."[22] In fact, the MLD is GE (0), the GE index with sensitivity parameter 0. The added indices are GE (1), also known as the Theil index; GE (2), also known as half the square of the coefficient of variation; and GE (-1), which does not seem to be known by other name. The last two added indices are the standard deviation of log incomes (SDL) and the relative mean deviation (RMD). The figure shows very clearly that our conclusions

[21] The Gini coefficients for overall income inequality in the U.S. and Denmark are respectively 0.58 and 0.35 (earnings), 0.53 and 0.35 (total income), and 0.50 and 0.29 (disposable income).

[22] The more positive (negative) the parameter is, the more sensitive the index is to income differences at the top (bottom) of the distribution.

are very robust to the inequality index employed. In fact, the new indices all give results very similar to either the Gini coefficient (Piesch, Mehran, SDL, and RMD indices) or to the MLD (all other indices).

An extension: Adjusting estimates to account 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 at producing tight lower bounds by including in our analyses all or most circumstances we know (or, at least, have strong reasons to believe) play major roles in generating inequality in opportunities. Some key such circumstances we would like to include when studying the U.S., in addition to those we 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/ethnicity in the SOI-M Panel, so we can't carry out an analysis in which we define types in terms of gender, parental fiftile and race/ethnicity. Instead, we compute adjustment factors that are meant to reflect how much larger our estimates of AIOpI for total income would be with race and ethnicity included in our analysis. These are the "auxiliary quantities" we mentioned when describing our data and variables.

To compute the two adjustment factors we need (for use with Gini-based and MLD-based AIOpI measures, respectively), we combine three pieces of information that Chetty et al. (2020) have made publicly available: (a) data on people's average 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. The result is a rough approximation to within-type mean incomes for types defined both in terms of gender and parental centile bin and in terms of gender, parental centile bin and race/ethnicity.

Figure 6 shows histograms for the two distributions that resulted from this exercise, over which we have overlaid nonparametrically estimated densities. As expected, the distribution of within-type income means based on more disaggregated types (because of the inclusion of race/ethnicity in defining them) is substantially more dispersed. Our adjustment factors are simply the ratios between the Gini coefficient (or MLD) of this distribution and the Gini coefficient (or MLD) of the distribution where types are based on gender and parental centile bin alone. When using these adjustment factors we make two key assumptions. First, we assume 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. Second, we assume that the adjustment factors are the same when using 50 or 100 parental income groups to define types.

The results of adjusting our estimates are shown in Figure 7. An adjustment factor of almost 14 percent in the Gini-based measure of long-run AIOp for total family income puts it at 0.28. As explained above, given the combination of biases affecting the corresponding RIOp measure, our adjusted estimate suggests that at the very least 52 percent of the overall inequality in long-run total income is accounted by circumstances outside of people's control. Moreover, as circumstances expected to have a large impact are still excluded from the analysis (e.g., parental education, place of residence) it is highly likely that this estimate is still a rather loose lower bound.

The adjustment factor for the MLD-based AIOpI is 52 percent, which puts it at as high as 0.15; such a large adjustment likely reflects the expanded inequality among within-type means in the upper part of their distribution when race/ethnicity is also considered in defining types (see Figure 6). This large increase suggests that the already very large lower-bound estimate of the U.S/Denmark ratio in AIOpI based on the MLD shown in Figure 3—which indicated that AIOpI in long-run family total income is at least seven times higher in the U.S.—may still be a very substantial underestimate.

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 much larger extent than the U.S. does, the direct evidence produced by the burgeoning empirical literature on inequality of opportunity has been far from compelling. This has been partly 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 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 period, using the same circumstances to define types, employing the same estimation method, and aligning as much as possible income notions across 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 have resorted to this end to a new, empirically validated, nonclassical measurement-error model similar to those used by mobility scholars. Lastly, we have advanced a plausible identification assumption that allows to legitimately compare the results for Denmark and the U.S., even though they are lower-bound estimates.

What have we found? We will not attempt to review all of our findings here, but it is nonetheless useful to briefly discuss a few of them. Our results indicate, first, that inequality of opportunity for long-run income is very high in the United States. Even when we only considered two circumstances in our main analyses, gender and parental income rank, the lower-bound Gini coefficients for earnings and total-income opportunities came at around 0.24.[23] Further, our extension to account for race and ethnicity suggests a total-

[23] Here and in what follows, any earnings results we discuss pertain to men and women pooled.

income Gini of at least 0.28, which entails that no less than half of long-run income inequality—and, likely, substantially more, given that key circumstances are still excluded—is due to circumstances outside of people's control. In addition, our evidence indicates that federal taxes and transfers (via the earned income and other refundable tax credits) reduce inequality of opportunity for income somewhat but not nearly enough to change any of our previous qualitative conclusions.

Second, inequality of opportunity for long-run income is far from negligible in Denmark. Indeed, even with the very minimum set of circumstances considered in our analysis, the lower-bound earnings and total-income Gini coefficients are 0.12 and 0.09. Our evidence suggests, however, that taxes and public transfers do produce a robust proportional reduction in inequality of opportunity for income.

Lastly, inequality of opportunity for income is radically higher in the U.S. than in Denmark. With our measures of earnings and total income, inequality of opportunity in the U.S. is no less than two and two and half times higher, respectively, with the more conservative Gini coefficient, and no less than four and seven times higher when we instead rely on the MLD. In the case of disposable income, the estimated U.S/Denmark ratios are not truly lower bounds, mainly because our measure of disposable income in the U.S. does not subtract state taxes. However, it seems reasonable to assume that any additional reduction in absolute inequality of opportunity that would accrue from including the missing items in our computation of disposable income would not be larger than the one generated from switching from our measure of total income to our measure of disposable income. Under this plausible assumption, inequality of opportunity for disposable income is estimated to be no less than 2.8 times higher in the U.S. than in Denmark with the Gini coefficient, and no less than 8.6 times higher with the MLD.

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Table 1: AIOpI and RIOpI estimates for 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 and 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, minumum 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. In Hufe et al. (2017), the indicators of child-parent relationship are childcare, play with parents, perceived quantity of time with mother, parents split, schoolwork support from parents. Hufe et al. (2017) also report estimates in which ability is dropped from the sets of circumstances.

Table 2: AIOpI and RIOpI estimates for 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: Hypothetical MLD-based results for two countries

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

Table 4. Demographic and Income Statistics

Variables	Denmark		United States	
	Income analyses	Earnings analyses	Income analyses	Earnings analyses
Gender (% female)	49.4	49.4	49.0	48.9
Age				
35	25.1	25.2	24.8	24.8
36	24.7	24.7	23.8	23.7
37	24.6	24.6	25.5	25.6
38	26.6	25.5	25.9	25.9
Total income				
Mean	80,505	NA	70,418	NA
Standard deviation	108,091	NA	145,278	NA
Disposable income				
Mean	65,349	NA	60,218	NA
Standard deviation	66,721	NA	120,056	NA
Earnings				
Mean	NA	45,201	NA	36,573
Standard deviation	NA	32,745	NA	58,453
Average parental disposable income				
Mean	54,099	53,867	64,304	64,838
Standard deviation	78,833	78,587	150,849	157,827
Sample size	263,261	264,443	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 for people (but not for their parents) are means across multiple-imputed income variables. NA = Not Applicable (variable not relevant).

Figure 1: Absolute inequality of opportunity for earnings

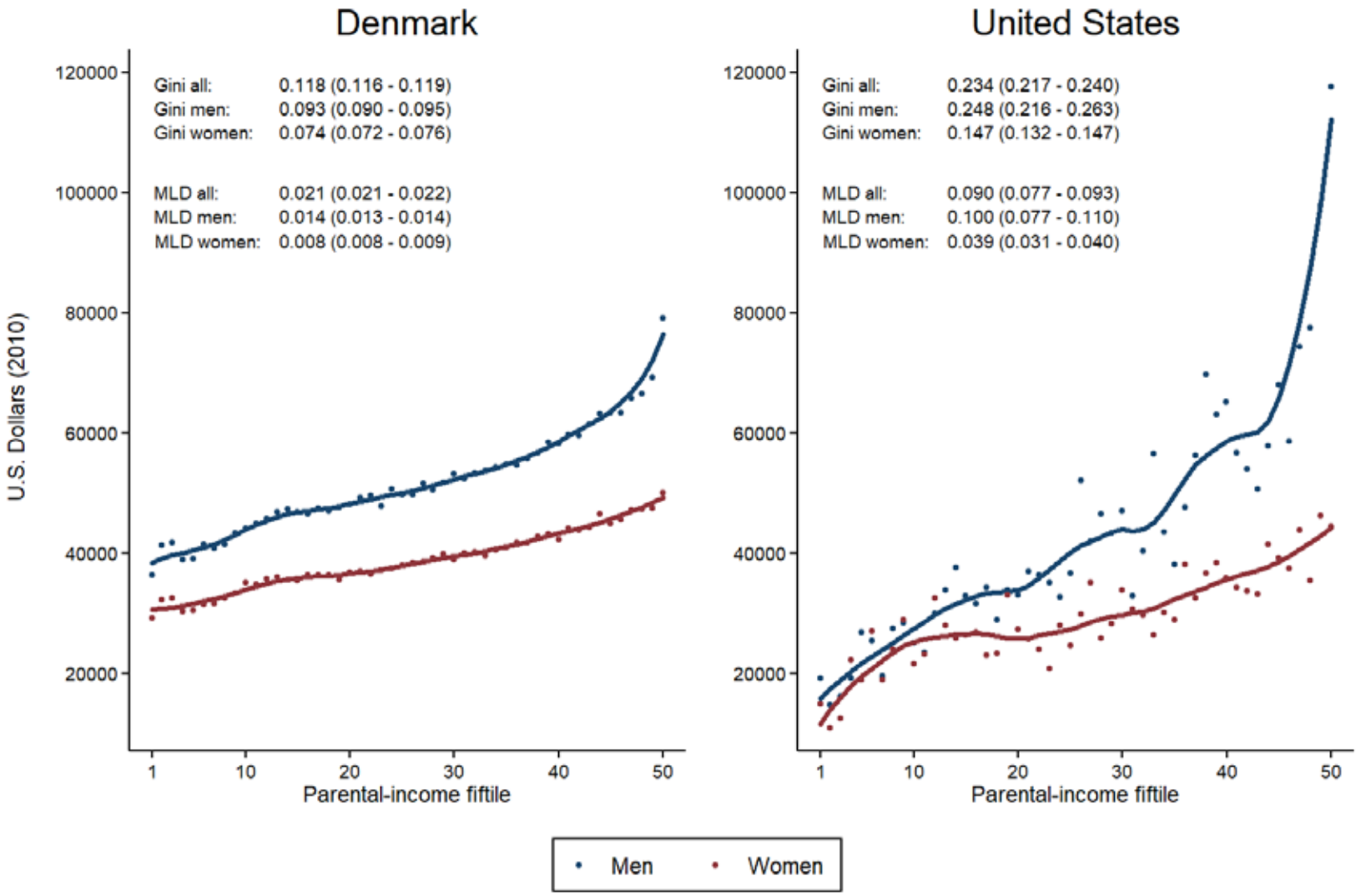


Figure 2: Absolute inequality of opportunity for family income

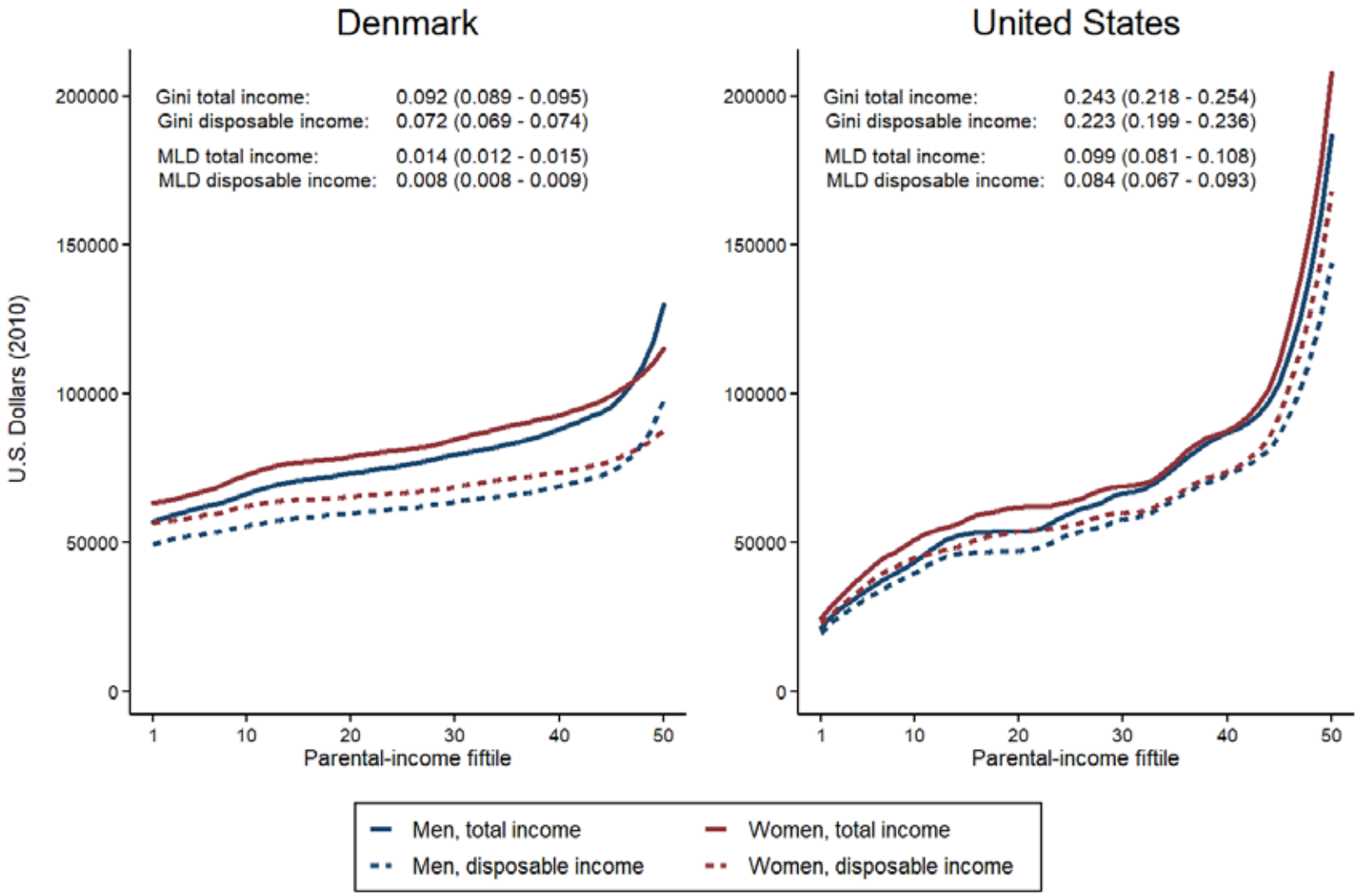


Figure 3. AIOpI ratios between the U.S. and Denmark

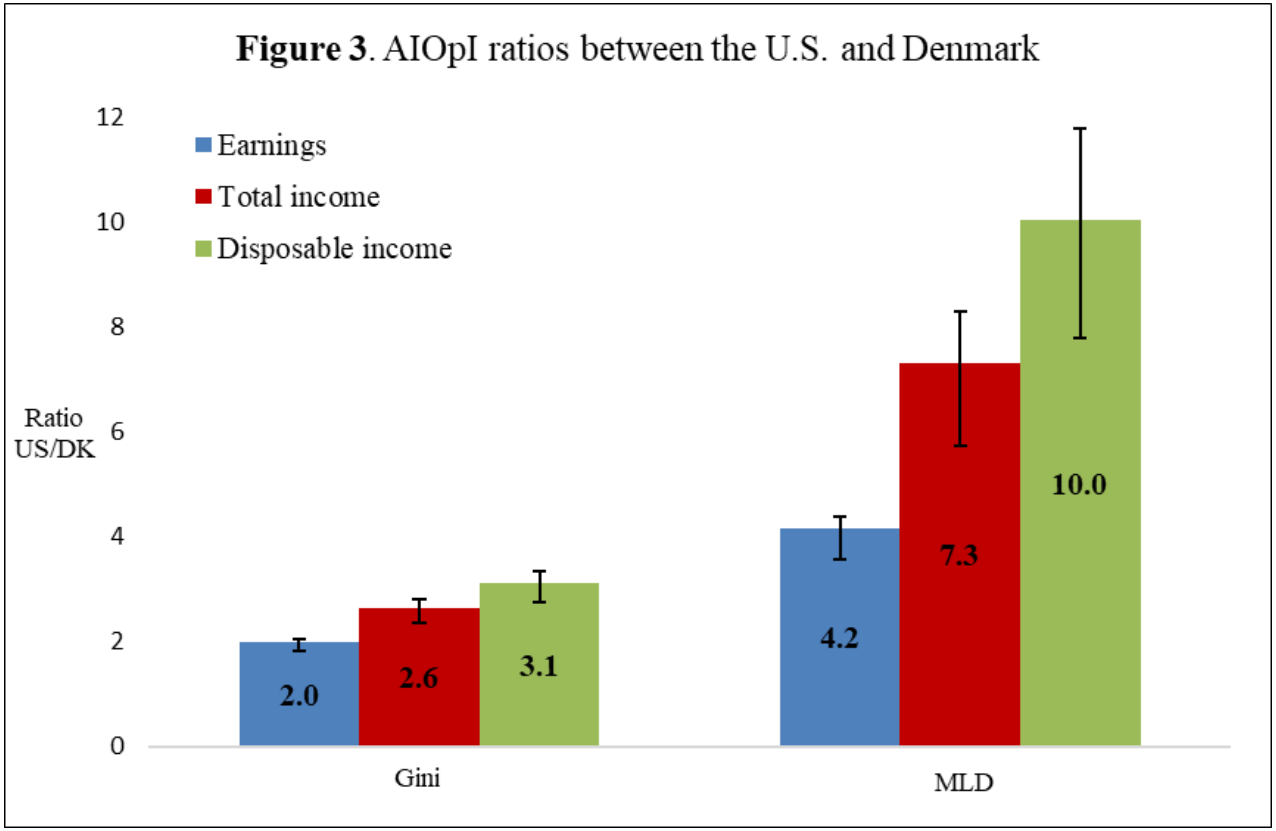
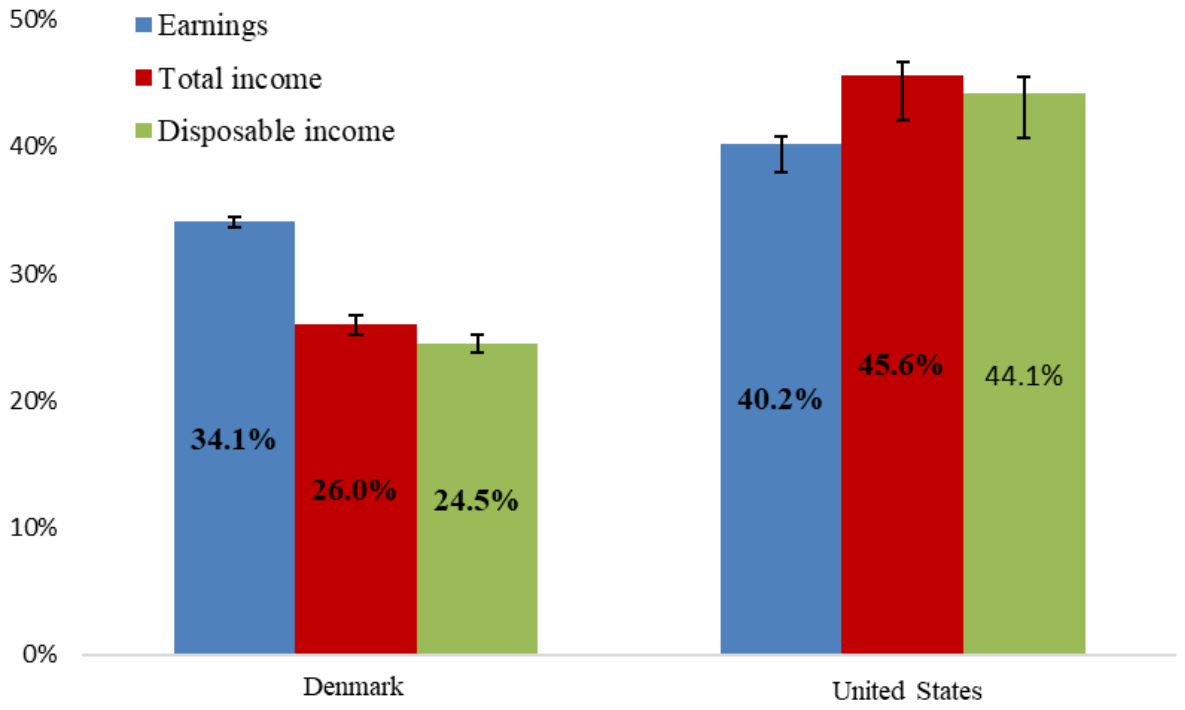


Figure 4. Gini-based RIOpI in the U.S. and Denmark



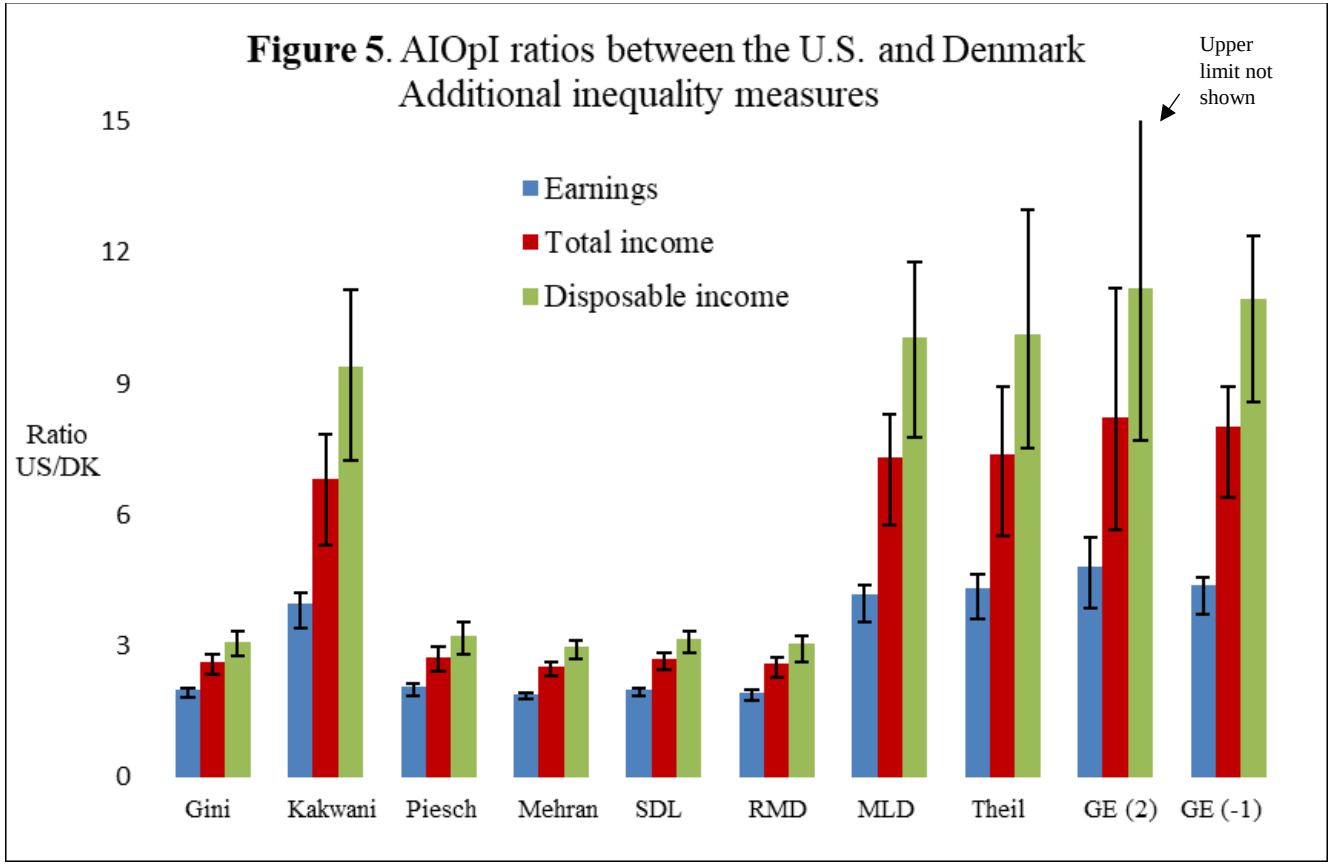
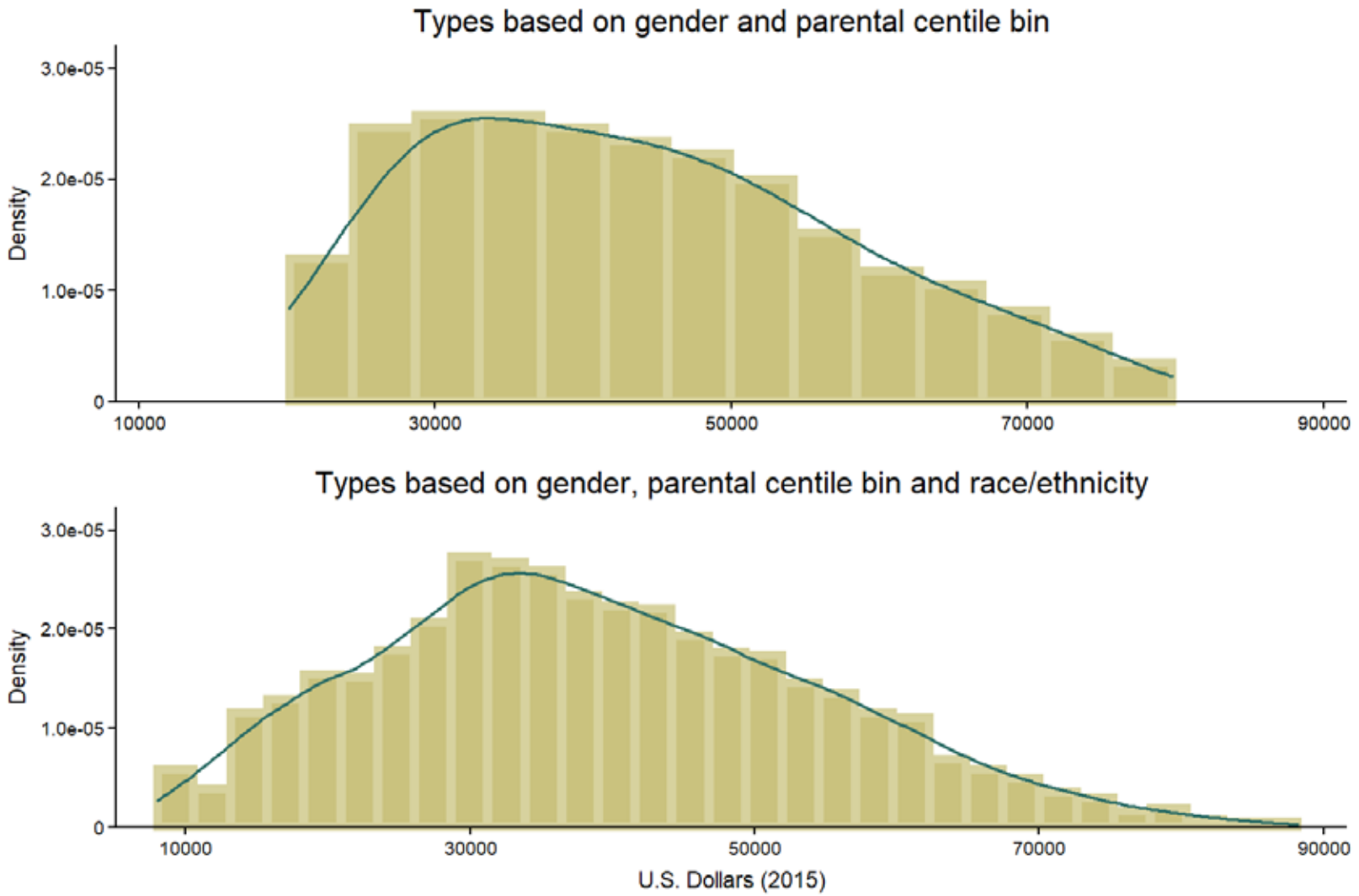

Figure 5. AIOpI ratios between the U.S. and Denmark
Additional inequality measures
Upper limit not shown
Earnings
Total income
Disposable income
Ratio US/DK
15
12
9
6
3
0
Gini
Kakwani
Piesch
Mehran
SDL
RMD
MLD
Theil
GE (2)
GE (-1)

Figure 6: Approximate distributions of within-type family-income means, U.S.



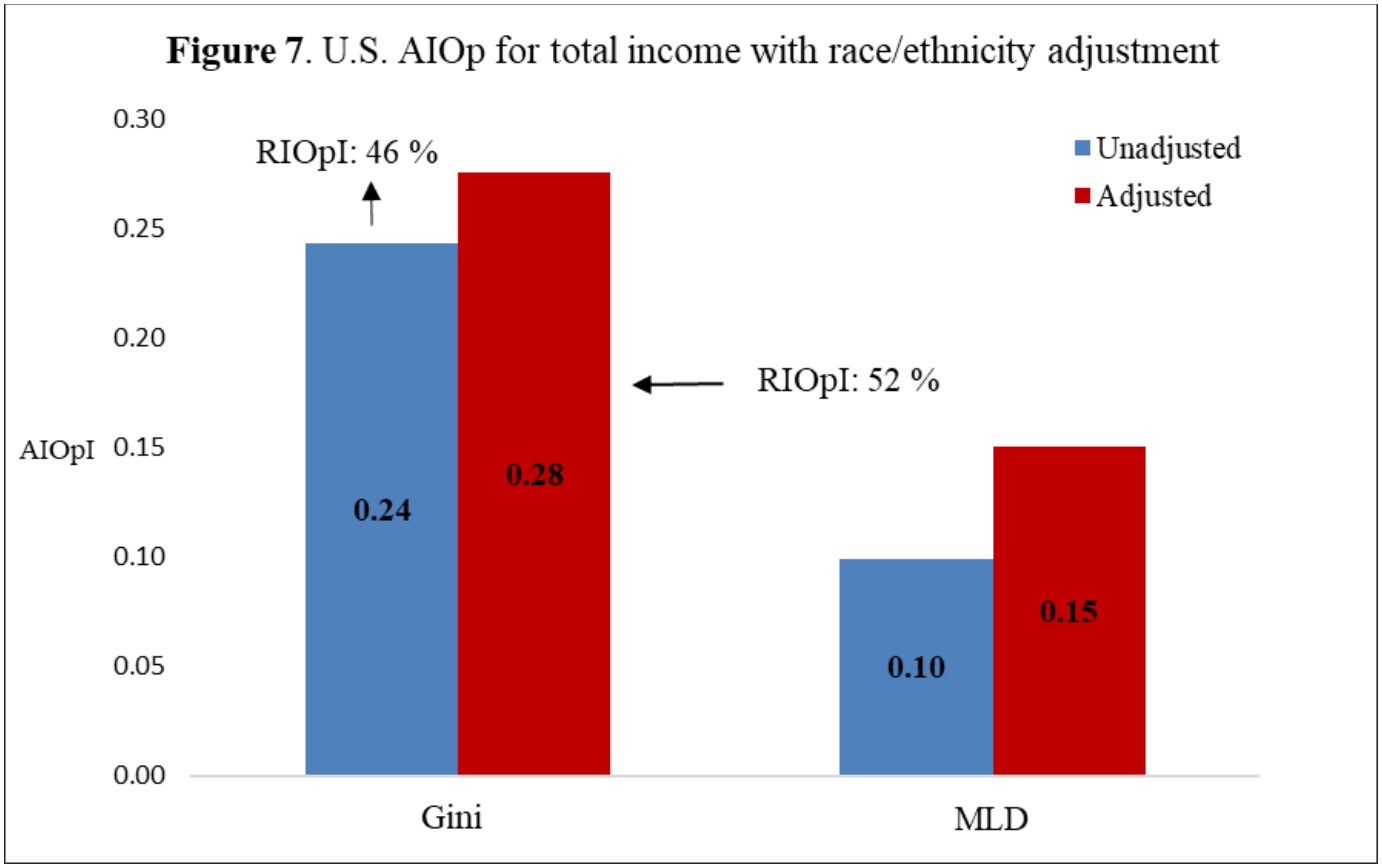

Figure 7. U.S. AIOp for total income with race/ethnicity adjustment
0.30
0.25
0.20
0.15
0.10
0.05
0.00
AIOpI
RIOpI: 46 %
RIOpI: 52 %
Unadjusted
Adjusted
0.24
0.28
0.10
0.15
Gini
MLD

Figure A1: Absolute inequality of opportunity for family income (total income)

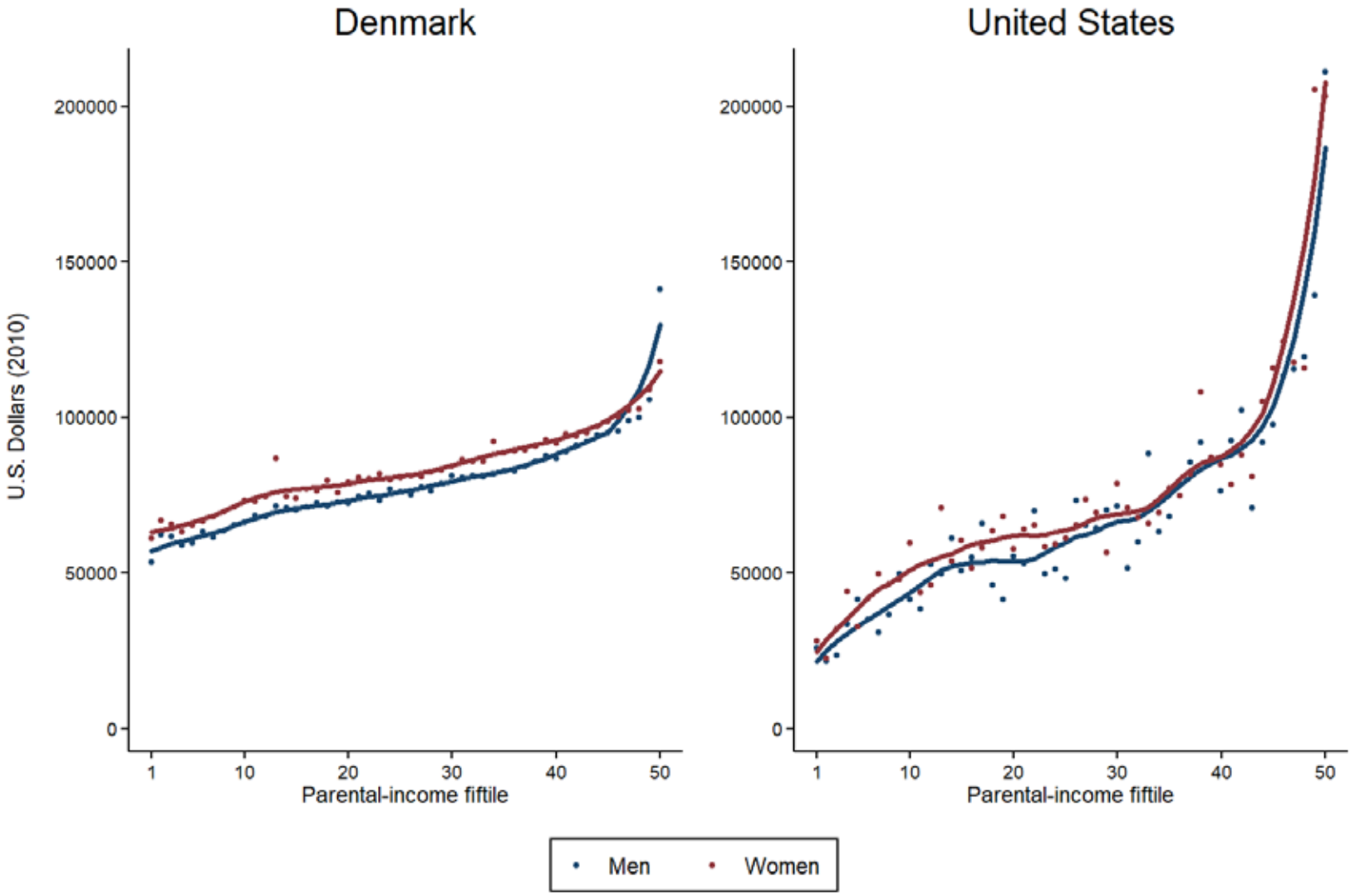


Figure A2: Absolute inequality of opportunity for family income (disposable income)

