USING CAPITAL INCOME TO PROXY FAMILY BACKGROUND: AN APPLICATION TO INEQUALITY OF OPPORTUNITY

Hugo del Valle-Inclán*

This version: April 2020

The measurement of opportunity inequality has attracted increasing attention in recent years, despite the fact that its empirical application suffers from stringent data limitations. In this paper we address one of these problems: the scarcity of data on family background. We propose to use a widely available variable as alternative proxy of socioeconomic origin, instead of the traditional and sparse proxies commonly employed. This alternative proxy is capital income. Using data of 31 European countries we first successfully test the accuracy of our approach, and then we apply it to obtain many new inequality of opportunity estimates. These results are useful for the measurement of inequality of opportunity, but also in other areas of research where we want to account for family background. (123 words)

JEL Codes: D63, D31

Keywords: inequality of opportunity, family background, capital income

1. Introduction

Throughout all her life Sophie Germain had to face opposition to study mathematics simply because she was born a woman. Germain was forced to study in secret during her youth, and to hide her gender with a pseudonym—Monsieur Le Blanc—once she grew up. Yet, in spite of the hurdles, she eventually became a great mathematician who impressed prominent figures of her time like Joseph-Louis Lagrange and Carl Friedrich Gauss. Now we know that we owe her a number of contributions, specially in number and elasticity theory, but we do not know what else she could have achieved, had she enjoyed the *opportunity* to receive early formal education and social support. Nonetheless, even though Germain suffered utter discrimination because of her gender, she had something most people did not: a rich family. She never had to work for money, nor had she to fulfill domestic tasks—she had the time, which she used to study.

*University of Vigo. Email address: hvalle-inclan@uvigo.es. I thank Carlos Hervés-Beloso, M. Ali Khan, Pedro Salas-Rojo, Juan Gabriel Rodríguez, Begoña Álvarez-García, Daniel Miles and the participants at the Eighth ECINEQ Meeting, the 2019 Africa Meeting of the Econometric Society, the 2019 Singapore Economic Review Conference, the XXXI SIEP Conference, and the XXVII EEP for invaluable comments and suggestions. The research leading to these results has received funding from the *European Union's Horizon 2020 Research and Innovation Programme* under grant agreement No 730998, *InGRID-2 – Integrating Research Infrastructure for European Expertise on Inclusive Growth from Data to Policy*; and *Consellería de Cultura, Educación e Ordenación Universitaria, Xunta de Galicia, grant code ECOBAS (Agrup15/08)*. The responsibility for all conclusions drawn from the data lies entirely with the author.

We believe that the life of Sophie Germain is interesting in its own right, but we resort to it now to illustrate what (in)equality of opportunity, IOP henceforth, is. This area of research arises from a debate in political philosophy that took place in the 1970s and 1980s, in which the focus of the egalitarian project was shifted from "equality of outcomes" to "equality of resources". Arneson (1989), Cohen (1989), Dworkin (1981a,b), and Sen (1980), building upon the work of Rawls (1971), sought to define a concept of equity that would accommodate concerns regarding advantages acquired through birth and results obtained by means of personal effort. Where political philosophers started, economists soon followed. The seminal contributions of Fleurbaey (1995), Roemer (1993, 1998), and Van de gaer (1993) formalized this ideal of fairness into economics, combining it with distributional analysis. The result of their work is a theory to systematically classify differences as just or unjust.

In short, inequalities with respect to any outcome—be it income, wealth, health status and such—are deemed "fair" or "unfair" depending on where they stem from. Inequality caused by factors individuals can choose—like the degree of effort exerted—is considered fair, while unfair inequality arises from sources individuals cannot control—like gender or race. For instance, Germain suffered discrimination due to her gender, something she cannot be held responsible for. This is, being a woman complicated her path to become a mathematician only because her society had established a role for her gender that did not match such prestige. In the same way, Germain enjoyed time to devote to her passions because she did not have to work, thanks to the comfortable economic position of her family. Of course she had no merit or responsibility on that either. Consequently, the IOP approach would consider the inequalities steaming from her gender and privileged socioeconomic origin as unfair; the former playing a negative influence and the latter furnishing a head start. However, Germain attained greater levels of knowledge and (postmortem) prestige than most people, and some of that inequality is surely due to the greater amount of effort she must had exerted. Such inequalities, stemming from variables individuals can choose, are deemed fair. A "leveled playing field", as the usual metaphor goes, is what we understand by equality of opportunity.[1]

IOP matters for at least three reasons. First, meritocracy[2] is a core value in western societies and beyond (Pignataro 2012). People prefer outcomes to be distributed according to personal effort and responsibility rather than to characteristics outside individual control, as shown by social attitude surveys (Fong 2001), laboratory experiments (Almås, Cappelen, Sørensen, et al. 2010; Konow 2000), or neuroeconomics studies (Cappelen et al. 2014). Moreover, similar behavior has been observed in nonhuman primates (Brosnan and De Waal 2003). In fact, participants in laboratory experiments are willing to redistribute income according to their notions of fairness even at a cost to themselves (Dawes et al. 2007; Fehr and Gachter 2000;

[1]For a recent survey on the philosophical grounds of the IOP approach, see Ferreira and Peragine (2016) or Roemer and Trannoy (2016).

[2]Funnily enough, the author who coined the term "meritocracy", Michael Young, used it to describe an undesirable social order ruling a dystopian United Kingdom (Young 1958). It was later euphemized into a positive term for rewards to individual "merit"; see for example Celarent (2009).

Fehr and Schmidt 1999; Mitchell et al. 1993).[3] Second, normative subjects such as fairness and morals have been part of the economic thinking at least since Aristotle, "who advocated proportionality of rewards to efforts".[4] Adam Smith (1776) famously stated that the pursue of self-interest can inadvertently become the pursue of the common good, but he also warned that unrestrained liberty to seek one's interest can undermine a prosperous nation. In his first major work, *The Theory of Moral Sentiments*, Smith (1759) had already discussed these limits to liberty: that some natural principles of civic ethics are a necessary condition for a nation to thrive.[5] Third, from a positive perspective, dearth of widespread opportunities may constrain economic development. For instance, lack of early investments in education strongly affects productivity of later inputs (Heckman 2006). Therefore, borrowing constraints faced by unwealthy parents might deprive their children from access to education (Lee and Seshadri 2019), potentially harming future economic growth (Berg et al. 2018; Hsieh et al. 2019). In addition, perceptions and beliefs about the importance of effort for personal achievement appear to shape preferences for redistribution and other political outcomes (Alesina et al. 2018; Bénabou and Tirole 2006; Piketty 1995).

The measurement of "fair" and "unfair" inequality is an appealing field that has been growing notably in recent years, despite the fact that its empirical application suffers from stringent data limitations. This article presents a strategy aiming to ease the data requirements to measure IOP. We propose a method to circumvent the restriction imposed by the scarce availability of a key piece of information routinely employed for estimating IOP: family background of individuals.[6] The standard approach for estimating IOP uses data on parental education and/or occupation to proxy socioeconomic origin. Although these are, naturally, not perfect proxies, it is generally assumed that they serve the purpose well—the problem we address here is related to their availability only. The approach presented in this article consists of using capital income as alternative proxy, because it also approximates family background *and* it is widely available.

In order to grasp the magnitude of the constrain imposed by relying on parental education and/or occupation data availability, consider the following two well-known databases for the study of poverty and inequality. The Luxembourg Income Study (LIS), the largest available income database of harmonized microdata collected from about 50 developed and developing countries, has information on parental education (parental occupation is not available) in only 63 of its 364 datasets (about the 18 per cent)[7]. Likewise, the European Union Statistics on Income and Living Conditions (EU-SILC), another extensively researched database on comparable income, poverty and living conditions—although including European countries only—has information on parental education and occupation in 2 of its 14 waves (around the 14 per cent).

[3]For a survey of political psychology and behavioral economics research on economic fairness see Starmans et al. (2017).

[4]Taken from Roemer and Trannoy (2016, p. 1299).

[5]See also Dougherty (2002) and Evensky (2005).

[6]The importance of socioeconomic origin in determining personal outcomes has been explored extensively by the literature on intergenerational mobility. See for example Corak (2013).

[7]As of the time of writing this article.

These are just two examples, but the pattern is general: data on parental features is scarce. The bottom line is that following a standard methodology for estimating IOP we can only obtain a small number of data points.[8] On the contrary, data on household capital income is available in around the 99 per cent of LIS' datasets, and in all waves of the EU-SILC. Therefore, by using capital income to proxy socioeconomic origin we could obtain many new IOP estimates. But why would capital income constitute an adequate proxy? We discuss it in section 2.

The possibility of overcoming the limitation imposed by the scarcity of data on family background has been explored before. Marrero, Rodríguez, and Weide (2016) simply opt to omit any information about individuals' background in their circumstances' set, using data from the US. Ferreira, Gignoux, and Aran (2011) estimate IOP in Turkey using demographic and health surveys data instead of standard household surveys. Teyssier (2017) employs a multiple imputation technique to attribute fatherly education and occupation to individuals in the sample for whom this data is missing, using Brazilian data. We add to these contributions by proposing a method of which accuracy is tested more broadly, considering a group of countries instead of solely one. Note that although our sample is constituted by developed economies, there is considerable heterogeneity in their characteristics and income. In addition, our methodology can be implemented more generally.

The rest of the article is organized as follows: in section 2 we discuss the adequacy of using capital income to proxy family background; section 3 briefly reviews the measurement of IOP; in section 4 we show an empirical application of the approach to measure IOP we propose, describing the methodology we follow to test its accuracy; and in section 5 we take advantage of our method to estimate IOP in a number of countries and periods in which, to the best of our knowledge, it has never been measured before. Section 6 concludes.

2. Why could capital income proxy family background?

This article proposes to use capital income to proxy socioeconomic origin. But why would capital income serve the purpose? The relationship between capital income and family background appears in a large body of literature studying wealth inequality, intergenerational transmission of advantages, and financial returns determinants. We review it in this section, in which we also address the issue that capital income is not (completely) exogenous to individuals.

In his famous book *Capital in the Twenty-First Century*, Piketty (2014) wrote about the return of what he dubbed "patrimonial capitalism", referring to the importance of bequests and inter vivos gifts in the determination of the wealth distribution. In fact, Piketty had already described in a previous article the contemporaneous rising relevance of intergenerational wealth

[8] A practical consequence of this restriction are a number of studies attempting to assess the evolution of IOP in Europe considering only two points in time, like Suárez Álvarez and López Menéndez (2017) or Andreoli and Fusco (2017). Other studies have explored the relationship of IOP with a number of economic and social phenomena by performing regressions with around 50 observations, such as Checchi, Peragine, and Serlenga (2016) and Marrero and Rodríguez (2012). Albeit these are relevant pieces of research that pursued interesting matters, they faced severe constrains due to the small amount of data points they could rely on.

transmission in France, measured as a fraction of either aggregate private wealth or national income (Piketty 2011). He finds the annual inheritance flow to be about 20% of disposable income, a figure much larger than the typical annual flow of new savings, and almost as big as the annual flow of capital income. Later, Alvaredo et al. (2017) extended the same analysis to include also the UK, Germany, Sweden and the USA, reaching similar conclusions. They estimate that in these countries the stock of inherited wealth as a share of total private wealth was about 50–60% (and rising) in 2000–2010.

As a matter of fact, the work by Piketty and coauthors revisit a question that in the 1980s fired up a famous debate, which is commonly known as the Modigliani vs. Kotlikoff-Summers controversy. The discussion started in Kotlikoff and Summers (1981), who argued that "intergenerational transfers account for the vast majority of aggregate U.S. capital formation [and] only a negligible fraction of actual capital accumulation can be traced to life-cycle savings". Specifically, they estimated that the inheritance share in total US' private wealth is up to the 80%. Modigliani (1986) replied by pointing out a number of methodological errors and insisted the share is actually much lower, at around 20% (see also Kotlikoff 1988 and Modigliani 1988). Subsequent studies found support for either one or the other side, or argued for a sort of a point in between. For a summary of the early literature on the role of bequests in the determination of aggregate private wealth go to Davies and Shorrocks (2000). More recently, Piketty, Postel-Vinay, et al. (2014) dismissed all Kotlikoff (1988), Kotlikoff and Summers (1981), and Modigliani (1986, 1988), pinning down the source of their dissent to the choice of the capitalization rate of past inheritance. Nevertheless, Alvaredo et al. (2017), Piketty (2011), and Piketty, Postel-Vinay, et al. (2014)'s results are more in line with the conclusions of Kotlikoff and Summers. A recent overall look at this literature can be found in Piketty and Zucman (2015).

In any case, bequests are only one of the channels driving the transmission of wealth from parents to children. Indeed, the intergenerational mobility literature has identified multiple mechanisms through which wealth persists across generations.[9] An early paper studying the correlation of wealth across generations found that in the US, the age-adjusted elasticity of child wealth with respect to parental wealth is 0.37 *before* the transfer of bequests (Charles and Hurst 2003). The authors argue that the main drivers of this correlation are earnings persistence within families, and that parents and children tend to allocate their portfolios quite similarly (either because children mimic or learn from their parents or because they share preferences such as risk tolerance). In contrast with posterior research, Charles and Hurst (2003) find that education plays a minor role. With a quasi-experimental design based on adoptees data from Norway, Fagereng, Mogstad, et al. (2018) conclude that "family background matters significantly for children's accumulation of wealth and investor behavior as adults, even when removing the genetic connection". The mechanisms they identify are children's education,

[9]Although the transmission of other advantages has been studied as well, such as ability, goals, or family reputation and connections; see Becker and Tomes (1979, 1986) for classical references.

income, financial literacy, and inter vivos transfers of wealth from parents.

In a series of companion papers, Boserup et al. (2016) study Danish wealth records and find that bequests increase wealth of recipients by a 36%, on average. Yet, Boserup et al. (2018), using the same data, find that wealth holdings during childhood, of which main source are (inter vivos) transfers from parents, have a stronger predictive power of future holdings than parental wealth itself. They justify this relationship on the basis that wealth ownership during childhood is a marker for characteristics that are not captured by parental wealth alone, pointing at intergenerational correlation in savings and/or investment behavior. Finally, Boserup et al. (2017), also using the same Danish data, find a U-shaped pattern when looking at the wealth rank correlation as a function of child age. It is very high when children are entering adulthood, it then declines until children reach the age of 30, when it starts to rise again, and finally increases even further upon the receipt of bequests. They find a correlation of 0.27 before bequests, and explain the pre-bequest wealth correlation on the basis of early inter vivos transfers and human capital accumulation. It is worth noting that since Denmark is a (relatively) egalitarian country, it is reasonable to expect wealth correlation across generations to be higher in other economies. Consistent with Boserup et al. (2017), Mudrazija (2014) finds a non-linear pattern of parent-child net transfers across the adult life-cycle in 11 European countries, with positive transfers from parents to adult children decreasing modestly until advanced old age, when the decrease intensifies—hence remarking the importance of inter vivos transfers. Although the pattern is general its intensity differs across countries, what Mudrazija explains on the basis of different welfare regimes. However, this heterogeneity could also respond to different fiscal treatment across countries of inter vivos transfers with respect to bequests (Kopczuk 2007).

Parallelly, the portfolio literature has related returns on investments to education and financial literacy (Bucher-Koenen and Ziegelmeyer 2011; Von Gaudecker 2015), what also suggests the possibility of the indirect link between parental education and children's capital income identified by Fagereng, Mogstad, et al. (2018). Moreover, research in sociology has found that savings and wealth ownership are largely determined by the intergenerational transmission of human capital (Hällsten and Pfeffer 2017; Hansen 2014).

So far we have treated wealth and capital income almost interchangeably, although they are not the same thing. Regrettably, wealth databases are scant and we are forced to consider capital income only. Nevertheless, although capital income does not perfectly follow total wealth holdings, they are closely related. For instance, Saez and Zucman (2016) estimate the long-run evolution of US' wealth inequality using capitalized income tax data. Fagereng, Guiso, et al. (2016) contested Saez and Zucman (2016)'s method and, using Norwegian data, showed that heterogeneity in returns to wealth and potential correlation of returns with wealth (i.e., that richer individuals tend to enjoy disproportionately higher returns) may introduce an upward bias in the estimated inequality. Nevertheless, their critique is not about the existence of a connection between wealth holdings and returns. In fact, Fagereng, Guiso, et al. (2020) show again that capital income is indeed markedly correlated with wealth.

In conclusion, the literature reviewed in this section suggests that capital income may serve as proxy of family background. However, in the context of IOP we must be aware of an important caveat. In Roemer (1998)'s sense, only exogenous variables (exogenous meaning being beyond individual control) may qualify as circumstances. Since capital income is not outside the influence of personal choice, or at least not completely, our proposal of including it in the circumstances' set violates this theoretical principle. We defend our strategy on three grounds: a) capital income should be understood not as an income variable, but as a variable correlated to socioeconomic origin—to the extent that strong intergenerational persistence exists, the concern of it being within individuals' control is lessened; b) to tackle this concern further we follow a procedure to isolate the exogenous component of capital income, which we detail in section 4.2; and most importantly, c) we perform extensive accuracy and robustness tests of the IOP estimates produced, with satisfactory results.

3. The measurement of inequality of opportunity

In the "canonical" model of IOP, as described by Ferreira and Peragine (2016, p. 755), an individual outcome y is determined by a vector of personal circumstances $C = (c^1, \ldots, c^K)$ and a scalar of effort e. The individual outcome is an economic good, i.e., it is universally desired with no satiation. Circumstances are factors that cannot be chosen, and therefore individuals should not be held responsible for them. These include gender, race, geographical origin, family background and the sort. Effort is the intensity with which individuals devote themselves to work, and can, conversely, be decided. We have described it as a scalar, what is common in the literature, but it can be thought of as a vector.

Circumstances and effort belong to the finite sets Ω and θ, respectively. Then, y is a function $\Phi : \Omega \times \theta \to \mathbb{R}$, such that:

$$y = \Phi(C, e). \tag{1}$$

This can be seen as a reduced-form model in which outcomes depend on circumstances and effort only, according to which all individuals sharing the same circumstances and exerting the same degree of effort would enjoy the same amount of outcome. Note that in eq. (1) effort implicitly captures different forms of luck[10] and the effect of circumstances on effort is not explicitly addressed.[11]

In recent years there has been an explosion in the number of methods available to empirically assess the extent of unfair inequality.[12] In this article we will use one of the most popular procedures: the non-parametric *ex-ante between-types inequality* approach, variously proposed

[10]The evaluation of luck is controversial. See Dworkin (1981a,b), Fleurbaey (2008), and Lefranc et al. (2009).

[11]For the constrain of circumstances on effort, see Roemer (1998).

[12]For surveys of the existing approaches to measure IOP see Ramos and Van de gaer (2016), Roemer and Trannoy (2016) or Ferreira and Peragine (2016).

Table 1: Distribution of an outcome that depends on circumstances and effort

	e_1	e_2	$\cdots$	e_m	$\cdots$	e_M
C^1	y_{11}	y_{12}	$\cdots$	y_{1m}	$\cdots$	y_{1M}
C^2	y_{21}	y_{22}	$\cdots$	y_{2m}	$\cdots$	y_{2M}
$\vdots$	$\vdots$	$\vdots$	$\cdots$	$\vdots$	$\cdots$	$\vdots$
C^n	y_{n1}	y_{n2}	$\cdots$	y_{nm}	$\cdots$	y_{nM}
$\vdots$	$\vdots$	$\vdots$	$\cdots$	$\vdots$	$\cdots$	$\vdots$
C^N	y_{N1}	y_{N2}	$\cdots$	y_{Nm}	$\cdots$	y_{NM}

by Van de gaer (1993), Peragine (2002), Checchi and Peragine (2010), and Ferreira and Gignoux (2011). As the name suggests, this method is *ex-ante*, and we will not consider any *ex-post* approach in this text.[13] Also, it produces IOP estimates that are generally interpreted as *lower-bounds*.[14]

We proceed now to briefly describe the measurement approach we will use. Suppose we have a population of individuals denoted by $i \in \{1, \ldots, I\}$, each of whom is fully characterized by the elements (y, C, e). This population can be partitioned in two ways. On the one hand, into types $t_n \in T_n$, within which all individuals share the same combination of circumstances C^n. This partition is such that $t_1 \cup \ldots \cup t_N = \{1, \ldots, I\}$, $t_n \cap t_{n'} = \varnothing$, and $C_i = C_{i'}$ $\forall i | i \in t_n, i' \in t_n$, $\forall n$.[15] Based on the realizations r_k of each circumstance c^k, the number of types is given by $N = \prod_{k=1}^{K} r_k$.[16] On the other hand, we can partition the population into tranches $t_m \in T_m$, in which everyone exerts the same degree of effort e_m. These are such that $t_1 \cup \ldots \cup t_M = \{1, \ldots, I\}$, $t_m \cap t_{m'} = \varnothing$, and $e_i = e_{i'}$ $\forall i | i \in t_m, i' \in t_m$, $\forall m$. Naturally, the number of tranches M is equal to the number of distinct values of e_m. Then, denote y_{nm} the outcome generated by circumstances C^n and effort e_m. Now we can represent the population with a matrix $Y = (y_{nm})_{n=1 \ldots N, m=1 \ldots M}$ of N rows and M columns, as displayed in table 1.

The non-parametric procedure known as ex-ante between-types inequality consists of constructing a smoothed counterfactual of y by replacing each individual outcome with its type-specific mean. This is, we replace y_{nm} with the mean value μ_n of the outcome distribution of

[13]Ex-ante techniques measure IOP considering circumstances, while ex-post methods account for effort. Despite having more normative appeal, ex-post approaches are applied less frequently because they have stricter data requirements. On the differences (and clash) between the ex-ante and ex-post perspectives see Fleurbaey and Peragine (2013).

[14]This means they represent the minimum value of IOP we can expect, since the vector of circumstances C we are able to observe is smaller than the "true" vector C^*, such that $|C| < |C^*|$. For the proof see Ferreira and Gignoux (2011), for a definition of *upper-bound* estimates go to Niehues and Peichl (2014), and for further discussion see Hufe et al. (2017). Nevertheless, recent research has questioned the interpretation as lower-bounds, pointing out that estimates may suffer from an upward bias due to sampling variance (Brunori et al. 2019).

[15]Superscripts of C denote specific combinations of circumstances, while subscripts refer to the circumstances of particular individuals.

[16]In empirical applications of IOP it is common to consider only discrete variables as circumstances c^k, because a continuous variable would dramatically increase the number of types, leading to very few observations, if any, in each type.

Table 2: Removing within-types inequality

	e_1	e_2	$\cdots$	e_m	$\cdots$	e_M
C^1	μ_1	μ_1	$\cdots$	μ_1	$\cdots$	μ_1
C^2	μ_2	μ_2	$\cdots$	μ_2	$\cdots$	μ_2
$\vdots$	$\vdots$	$\vdots$	$\cdots$	$\vdots$	$\cdots$	$\vdots$
C^n	μ_n	μ_n	$\cdots$	μ_n	$\cdots$	μ_n
$\vdots$	$\vdots$	$\vdots$	$\cdots$	$\vdots$	$\cdots$	$\vdots$
C^N	μ_N	μ_N	$\cdots$	μ_N	$\cdots$	μ_N

type t_n; see table 2. By doing so any inequality within types is eliminated—remaining only inequality between types, i.e., inequality due to circumstances.[17] Finally, we can apply an inequality index to $\tilde{\mu}$, the smoothed counterfactual distribution of y, in order to obtain an absolute

$$\text{IOP}_{abs} = I(\tilde{\mu}), \tag{2}$$

and a relative measure of IOP

$$\text{IOP}_{rel} = \frac{I(\tilde{\mu})}{I(y)}. \tag{3}$$

Notice that the criterion to identify the existence of IOP are differences between mean outcome levels of types. This is, $\exists t_n, t_{n'} \in T_n$ such that $\mu_n \neq \mu_{n'}$.

Let us conclude this non-comprehensive review of IOP measurement by discussing the selection of the inequality index to be employed. We would like our measure to be Lorenz-consistent, for which it must satisfy the principles of symmetry, population invariance, scale invariance and transfers (e.g. Foster and Lustig 2019). This limits the choice to the known as summary indexes, of which the most commonly used are the Atkinson, Gini and generalized entropy measures. In addition, we need our index to be additively decomposable, given that we intend to split total inequality into its fair and unfair shares, and moreover, we would like it to be path-independent decomposable (Foster and Shneyerov 2000). The last requirement conveniently narrows the possible choices to just one, the Theil 0, also known as mean log deviation.[18] This measure is defined as follows:

$$\text{MLD} = \frac{1}{N} \sum_{i=1}^{N} \ln \frac{\bar{x}}{x_i}, \tag{4}$$

where N is the size of the sample, x_i is the outcome of observation i, and $\bar{x}$ is the mean of x_i.

[17]Note that assuming all inequality between types is IOP implies regarding as normatively irrelevant possible differences in the amount of *absolute* effort exerted across types. See Roemer (1998).

[18]For further discussion on the axiomatic properties one might wish to require in the IOP setting see e.g. Ferreira and Gignoux (2011).

However, in spite of the foregoing, the Gini index is sometimes employed in the IOP literature, and since we acknowledge that each inequality index implies a normative choice (Atkinson 1970), we will also employ the Gini index when testing for robustness.

4. Empirical application

This article presents a strategy that aims to ease the data requirements to measure IOP. This strategy, which we call the capital income approach, involves the selection of circumstances: instead of proxying family background with information on parental education and/or occupation—which is scarce—, we propose to employ a measure of capital income—which is widely available. Simply put, we suggest that capital income can also be used to proxy socioeconomic origin.

Our project consists of two parts:

- Validating the approach: we first consider datasets that have information on parental features, in order to obtain IOP estimates using a "standard" set of circumstances (including parental education and occupation) and the set we propose (which excludes parental features but includes a measure of capital income). We then compare the two kinds of estimates in order to assess the accuracy of our method, and conclude that the results of the capital income approach are accurate to the extent they are similar to those of a standard methodology.

- Benefiting from the approach: once the reliability of our method has been assessed, we show an application to estimate IOP in datasets that do not have information on parental features.

4.1. Data and methodology

We make use of the cross-sectional files of the European Survey of Income and Living Conditions. This is a well-known and researched database for the study of inequality, poverty, and social exclusion that offers harmonized data on income and circumstances at the individual and household level. It has been conducted yearly since 2003 for up to 31 European countries in its most recent waves. The EU-SILC has information about parental features in two waves only—2004 and 2010—, but capital income of households is available in all waves and countries. Hence, we will consider the two waves of 2004 and 2010 for the validation purpose, and all of them to take advantage of the capital income approach. We include in our analysis all countries available, but the number of them participating in the survey has been increasing since it started being conducted.[19]

[19] In the first wave, referred to 2003, 15 countries took part: Austria, Belgium, Denmark, Estonia, Finland, France, Greece, Iceland, Ireland, Italy, Luxembourg, Norway, Portugal, Spain and Sweden. In 2004, 11 more were added: Cyprus, Czech Republic, Germany, Hungary, Latvia, Lithuania, Netherlands, Poland, Slovakia, Slovenia and the United Kingdom. In 2006, 4 more were included: Bulgaria, Malta, Romania and Switzerland. Finally, in 2009 Croatia joined.

We restrict our sample to people aged 30 to 59 to account for life-cycle effects, which is common in the literature (e.g. Marrero and Rodríguez 2012). We choose 30 years as the lower limit because income at this age is a good predictor of long-term earning potential (Chetty et al. 2014), and 59 as the upper limit because the EU-SILC does not collect information on family background for older individuals. We also remove from our sample all observations with missing values in any circumstance, and in addition, we cap very high values in each income distribution, in particular by replacing all values above the 99th percentile with the value at that percentile.

As the outcome of interest we consider annual gross wage.[20] We choose to consider gross values because it allows to have an account of how the market rewards each type, without the effect of state intervention. Also, we focus on the personal instead of the household level because we want to include gender in the circumstances' set.[21] However, focusing on the individual level overlooks household bargaining processes that affect labor market participation. Yet, abstracting from this issue, as can be done by focusing on the household level, is not satisfactory either, since part of the effect of circumstances on personal achievement works precisely through labor supply decisions—specially that of gender (Bursztyn et al. 2017; Goldin 2014; Kleven, Landais, et al. 2019). Hence, ideally we would account for the problem of different patterns of labor market participation, rather than abstracting from it. To account for this problem we estimate a Heckman two-step selection model into employment (Heckman 1979). The kind of employment modeled includes employees or self-employed who worked full- or part-time during at least 7 months in the reference period.[22] We believe that explicitly modeling the selection into remunerated occupations entails an improvement with respect to limiting our sample to individuals at work only (see for example Andreoli and Fusco 2017). However, since this choice is non-standard (to the best of our knowledge it has only been applied in the IOP setting by Checchi, Peragine, and Serlenga 2016) we also conduct our analyses without a selection model, what we report in section 4.4.

Regarding circumstances, in the "standard" set, which we take as our baseline, we include gender, immigrant status, parental education and parental occupation. These are circumstances

[20] Defined as "Employee cash or near cash income (Gross)" (variable PY010G) plus "Non-Cash employee income (Gross)" (PY020G), setting all non-positive values to one. We exclude "Value of goods produced by own-consumption (Gross)" (PY070G) because it is not available in all countries. Gross means that neither taxes nor social contributions have been deducted at source. From 2006 to 2008, both included, the variable "Non-Cash employee income (Gross)" is not available in the Netherlands' datasets, so we compute wage without it in those years. Also, in the data of France, Greece, Italy, Latvia, Spain and Portugal prior to 2006 the gross value of these variables is not available, so for all waves of these countries we consider the net values (variables PY010N and PY020N).

[21] Considering income at the household level may implicitly nullify the contribution of gender to total IOP (Valle-Inclán 2020).

[22] We used the variables "Number of months spent at full-time work" (PL070) and "Number of months spent at part-time work" (PL072) in waves prior to 2008. In waves from 2008 on these variables were updated in the survey design to "Number of months spent at full-time work as employee" (PL073), "Number of months spent at part-time work as employee" (PL074), "Number of months spent at full-time work as self-employed (including family worker)" (PL075), and "Number of months spent at part-time work as self-employed (including family worker)" (PL076).

that have been frequently used.[23] We use a baseline set of circumstances to compare the IOP estimates it produces to those obtained with our proposed set, which we call the *capital set* of circumstances. The capital set differs from the baseline in the last two circumstances only, which are substituted by a measure of capital income. Hence, the capital set of circumstances includes gender, immigrant status and a measure of capital income.

We will now briefly describe each circumstance. As gender we consider binary gender, since it is the information available in the database.

Regarding immigrant status, we differentiate between individuals born in the country of residence and those born outside. In spite of being frequently used, immigrant status is not considered a circumstance by some researchers. For that reason we would like to justify this choice. Although emigration clearly falls within the control of individuals (except in extreme situations such as famines, wars, political prosecution or natural disasters), we believe it can be considered a circumstance on the basis that the country where we live largely determines our income (Milanovic 2015), and unless we emigrate, the country where we live is the country where we were born, what is outside our control. Moving to a country with a more favorable income distribution may return a gain, but at a cost in terms of effort that those already born in such country do not have to assume. Furthermore, natives do not face possible discrimination due to their national origin.

With respect to paternal education, we group individuals according to the highest educational level attained by any of their parents: pre-primary, primary or lower secondary education (levels 0, 1, and 2 of ISCED-97), upper secondary and post-secondary non-tertiary education (levels 3 and 4 of ISCED-97), and first and second stage of tertiary education (levels 5 and 6 of ISCED-97). This is, we distinguish three levels of parental education.

Finally, regarding parental occupation, we also create three groups of individuals according to the highest job category of their parents, which correspond to elementary occupations (group 9 of ISCO-88), plant and machine operators and assemblers, craft and related trades workers, skilled agricultural and fishery workers, service workers and shop and market sales workers, clerks (groups 8, 7, 6, 5 and 4 of ISCO-88), and technicians and associate professionals, professionals, and legislators, senior officials and managers (groups 3, 2 and 1 of ISCO-88).[24]

In section 4.2 we detail the construction of the measures of capital income that we will include in the capital set, which have also three levels.

Therefore, with the baseline set of circumstances we have up to 36 types, product of 2 genders $\times$ 2 geographical origins $\times$ 3 levels of parental education $\times$ 3 levels of parental occupation. On the other hand, with the capital set we have up to 12 types, product of 2 genders $\times$ 2 geographical origins $\times$ 3 levels of capital income. However, the number of types falls shorter than 36 or 12 in some datasets, because some combinations of circumstances are infrequent and do not appear in the data. To attenuate the possible bias produced by types with very few

[23]See Ferreira and Peragine (2016), table 25.8, for a list of circumstances employed in eight studies.

[24]In the case of Sweden we are forced to exclude the circumstance parental occupation due to the very small number of respondents, what would drive the number of observations per type unacceptably low.

observations that may contain extreme values (Brunori et al. 2019) we retain only those types with at least a minimum number of observations, which we set to 10.

Only 36 types, let aside 12, is clearly less than the *true* number of types. This is a problem when our objective is to measure IOP as truthfully as possible, but it is not a concern here. On the contrary, a small set of circumstances constitutes a tougher test for the capital income approach, provided that, generally, the bigger the number of circumstances, the smaller their relative role will be. Hence, differences between the two kinds of estimates should be easier to spot with small sets of circumstances. In fact, in section 4.4 we manually modify the sets of circumstances, employing both bigger and smaller sets, and find that the accuracy of our approach holds, or even improves as we add more types to the analysis.

With respect to the choice of circumstances, a non-subjective process has been proposed by Brunori et al. (2019). We do not apply their cross validation approach here because we attempt to keep our methodology as standard as possible. However, note that their proposal concerns the selection of circumstances, and hence it is perfectly compatible with the capital income approach.

Finally, regarding the empirical methods to estimate IOP, as we mentioned in section 3 we apply the non-parametric ex-ante between-types inequality approach, and as inequality measure we employ the mean log deviation. In addition, we take mean values of each type in logarithms, a choice with an almost negligible effect on the results.

4.2. Construction of the capital income variables

In this section is detailed how we construct the measures of capital income to be included in the capital set of circumstances. Looking at the research presented in the introduction, using capital income to proxy socioeconomic origin might seem adequate. However, in the IOP setting an important concern arises: the amount of capital income can be, at least to some extent, decided by individuals. Only characteristics over which individuals cannot exert any control are included in the circumstances' set, and that is precisely what justifies the qualification of any inequality stemming from these factors as unfair.

Kotlikoff and Summers (1981) divide wealth into two components:

$$W = L + T, \tag{5}$$

where W is the stock of wealth, and L and T are life-cycle and transfer components, respectively. Adapting their model to our setting, we can think of capital income as determined by

$$K = \Psi(M, D), \tag{6}$$

where K is capital income, a function of M and D, which are, respectively, *meritocratic* and *dynastic* components. The dynastic component is product of advantages acquired through birth such as bequests and access to good education. The meritocratic component comes from effort exerted during our lifetime.

The evidence reviewed in the introduction portraits the dynastic component as larger than the meritocratic, but even if we trust this literature we would be concerned by the influence of the latter if we were to introduce a simple measure of capital income in our circumstances' set. Hence, we would like to consider the dynastic component only. For this purpose we endeavor to isolate D from eq. (6), which we will then include as a measure of dynastic capital income in our circumstances' set. However, for comparison purposes we will also use K, a simple measure of capital income containing both its meritocratic and dynastic components, to proxy family background. We proceed now to detail how these two measures of capital income are constructed.

The procedure to isolate the dynastic component of capital income consists of running an OLS regression of household per capita gross capital income[25] against a number of effort variables—namely personal education, personal occupation, and a dummy on mating—and a variable on the position in the life cycle—namely age. Then, we take the residuals of this regression, which can be seen as the value of capital income once its meritocratic component has been removed, and use them as input to construct a variable to be used as proxy of family background.

This procedure to "remove" the meritocratic component follows the logic behind the parametric approach to measure IOP proposed by Ferreira and Gignoux (2011), which "removes" the effect of effort on a given outcome by means of an OLS regression. Of course, we acknowledge that this technique to isolate the dynastic component of capital income is far from ideal, but we believe it entails a step in the right direction in light of the accuracy and robustness tests performed in section 4.3. We will return to this below.

We will now briefly discuss the variables of effort considered. We include personal education in the regression because it is a popular effort variable in empirical studies (see for example Almås, Cappelen, Lind, et al. 2011). Yet, it is not free of controversy. On the one hand, since primary and secondary school take place before the age of consent, some believe that performance at this stage of life cannot be deemed responsibility of individuals. Success at tertiary schooling, which takes place after the age of consent has been reached, suffers nonetheless from hysteresis—so it bears a problem too. On the other hand, if one does not think that everything before the age of consent should be considered a circumstance, or that hysteresis leaves some room for personal responsibility, it is clear that own effort influences educational attainment. In sum, it is possible that personal education is a bad variable of effort, which in spite of that

[25] Gross household capital income is defined as "the income received less expenses occurring during the income reference period by the owner of a financial asset or a tangible non-produced asset (land) in return for providing funds to, or putting the tangible non-produced asset at the disposal of, another institutional unit". This is, income coming from rental of a property or land (variable HY040G) + interests, dividends and profits from capital investment in an unincorporated business (variable HY090G). Gross means that neither taxes nor social contributions have been deducted at source. This sum is then divided by the number of adults in the household. We use information of capital income at the household level because at personal level it is not widely available. In the data of France, Greece, Italy, Latvia, Spain and Portugal prior to 2006 the gross value of these variables is not available, so for all waves of these countries we use the net values (variables HY090N and HY040N).

has been used frequently as such—the reason is probably the lack of many alternatives.

Personal occupation is commonly employed as effort variable too. Undoubtedly, personal effort plays a role in determining our job. However, past opportunities to receive high-quality education and network effects play a role too. Therefore, personal occupation is not a perfect variable of effort either, but it is however used with less caution than personal education.

Marital status is not frequently employed as effort variable, although this is not the first time (e.g. Ramos and Van de gaer 2020). Nevertheless, we include it here not only for it can be understood as such, but because we want to use it in relation to *household* capital income. Consider the situation in which a person with no capital income mates with someone who receives large amounts of it. In such case we would not be able to distinguish who is the actual owner of the capital, since we can only look at household capital income. By adding a dummy on mating we are able to account for this possibility.[26]

We include age too, despite the fact that it is exogenous, because the position in the life-cycle may influence the process of wealth accumulation. The use of age to account for life cycle effects is common at least since Modigliani (1966).

Personal education and occupation are defined in exactly the same way as the variables parental education and occupation (coded in three levels following ISCED-97 and ISCO-88 classifications, respectively). Mating is defined as couples living in the same household, either with legal or informal bindings. Age is considered at the end of the reference period.

The OLS regression is as follows:

$$pckinc_i = \beta_0 + \beta_1 education_i + \beta_2 occupation_i + \beta_3 couple_i + \beta_4 age_i + \beta_5 age_i^2 + \varepsilon_i \tag{7}$$

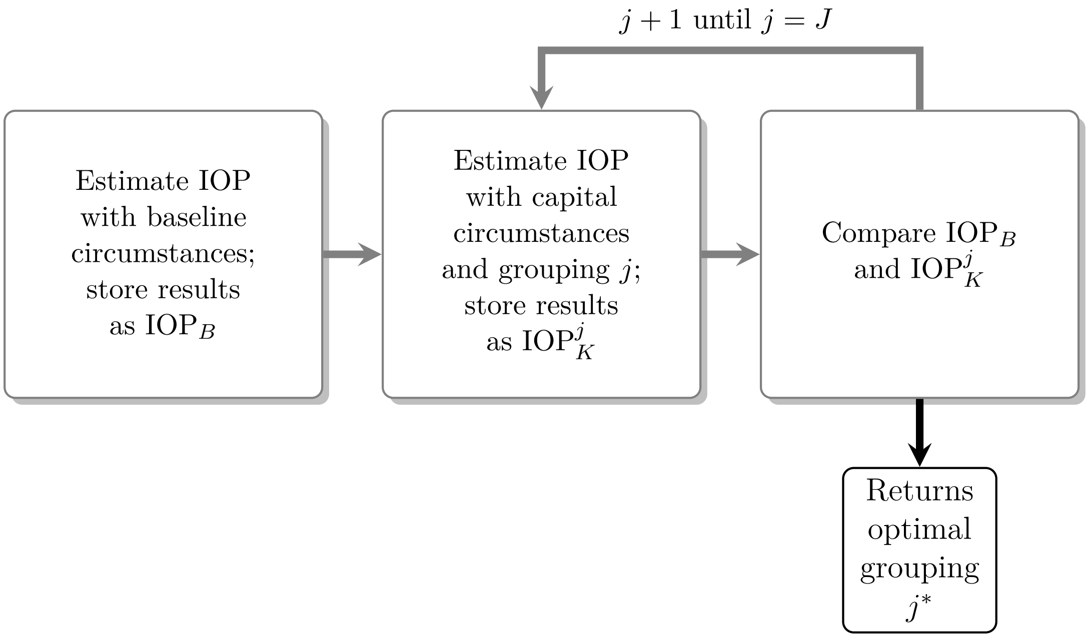
where *pckinc* is the amount of per capita household gross capital income, *education* is the highest educational level attained, *occupation* is the occupational status, *couple* is a dummy on mating, and *age* is self-explanatory.

Now that we have an estimate of the residuals $\hat{\varepsilon}$ from eq. (7), which can be seen as the dynastic component of capital income, we would like to use them to proxy family background. However, in order to include them in our set of circumstances we must first convert this continuous variable into a discrete one. Generally, in empirical applications of IOP only discrete variables are included in the set of circumstances, as we explained in section 3. In consequence we are forced to group individuals according to $\hat{\varepsilon}$ from (7) (this applies also to K from (6), the simple measure of capital income containing both its dynastic and meritocratic components, which we will employ as well for comparison purposes). The grouping could be done in many ways—for instance, purely arbitrary choices can be made, such as splitting the population into quartiles or deciles.

However, we believe that the best solution is to simply test *all* possibilities. To that end we write an algorithm that finds the optimal grouping (optimal according to an accuracy test) by

[26] For more on the importance of accounting for spouses see Peichl and Ungerer 2016.

Figure 1: How the grouping algorithm works



dividing the population into percentiles and testing all possible combinations of groups, up to 100 groups. What this means is the following. The algorithm starts by estimating IOP using the baseline set of circumstances, in which all variables are already discrete. It then stores the results, which we call IOP_B. Next, it makes two groups of individuals according to the residuals $\hat{\varepsilon}$ from (7) (or the variable K from (6)), the first group composed by those in the first percentile, and the second formed by individuals in the top 99 percentiles. Let us call this grouping $j = 1$. It then estimates IOP with a set of circumstances that includes a capital income measure discretized according to grouping $j = 1$, and stores the results. Let us call this results $\text{IOP}_K^{j=1}$. The algorithm finishes this iteration by comparing IOP_B to $\text{IOP}_K^{j=1}$ and assessing their similarity. It repeats this process several times, testing grouping $j + 1$ until $j = J$, and exits by returning the optimal grouping j^* that produces the most similar "capital" estimates IOP_K^{j*} to their baseline IOP_B. The functioning of the algorithm is represented in fig. 1.

Yet, due to computational capacity restrictions we cannot divide the sample into pieces as small as percentiles nor can we test combinations of up to 100 groups. Dividing the sample so finely leads to a number of permutations on the order of billions,[27] and such a program would take a reasonably powerful computer years to complete. Hence, we are forced to cap the analytic capacity of our algorithm. We decided to divide the population into ventiles and test combinations of up to 4 groups only, what implies 1,159 permutations. Bear in mind that since we are interested in standard errors we must employ the bootstrap, what means that with for instance 1,000 replications the algorithm will be estimating IOP for the whole sample of

[27] It is easy to calculate this number using combinatorics.

countries over a million times, what is already computationally challenging.

Nevertheless, it is remarkable how effective the algorithm is, even a capped version of it. In section 4.3 we show the satisfactory results of the accuracy test with the grouping determined as optimal by the algorithm, and in section 4.4 we show the results using arbitrary groupings.

The rules used by the algorithm to assess the similarity between IOP_B and IOP_K^j consists of an analysis of moments, regression coefficients, and correlations of the IOP estimates' distributions at the country level. Specifically, the algorithm finds the grouping j^* that minimizes the sum (in absolute value) of:

a. the deviation from 1 of the ratio of the average value of the estimates IOP_B to the average value of the estimates IOP_K^j

b. the deviation from 1 of the ratio of the average standard error of IOP_B estimates to the average standard error of IOP_K^j

c. the deviation from 1 of the regression coefficient of IOP_B against IOP_K^j estimates

d. the deviation from 1 of the pairwise correlation coefficient between IOP_B and IOP_K^j estimates, and

e. the deviation from 1 of the Spearman's rank correlation coefficient between IOP_B and IOP_K^j estimates

The optimal grouping j^* determined by the algorithm consists of the cumulative distribution function [65, 85, 95, 100] in the case of $\hat{\varepsilon}$ from eq. (7), the dynastic measure of capital income, and [85, 90, 95, 100] in the case of K from (6), the simple measure containing both the dynastic and the meritocratic components. What these numbers mean is, for example in the former case, that the first group is composed by the bottom 65 percentiles, the second group by the following 20, the third are the percentiles 86 to 95, and the fourth group are the top 5 percentiles.

Summing up, in this subsection we have detailed the construction of two measures of capital income: one based on K from eq. (6), which encompasses both the dynastic and meritocratic components of capital income, and another based on $\hat{\varepsilon}$ from (7), which represents the dynastic component only. In the next subsection we estimate IOP using these two measures to subsequently test the accuracy of the results by comparing them to their baseline.

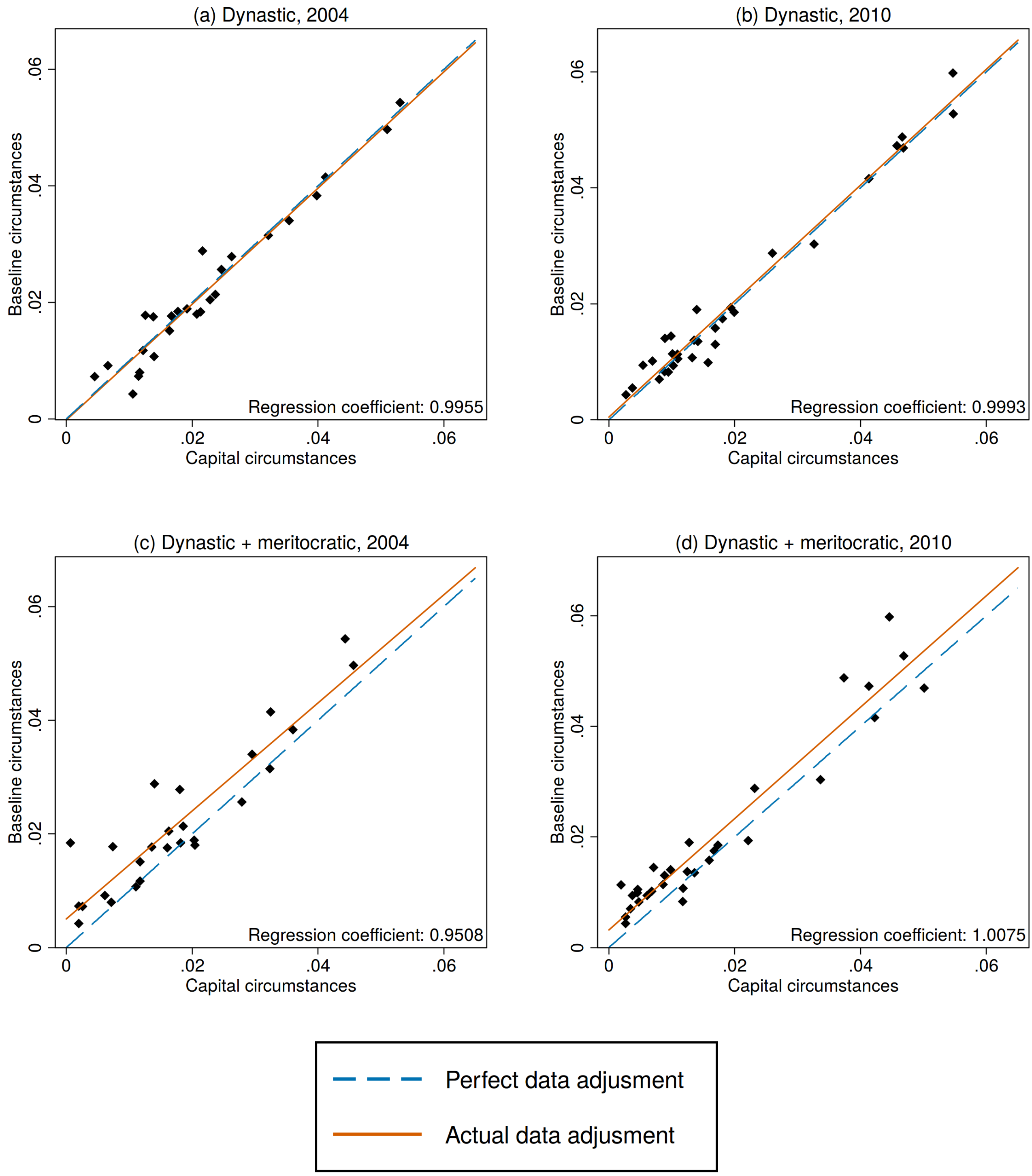
4.3. Results

In this section we compare IOP estimates obtained with our baseline set of circumstances, which includes parental education and occupation, to the estimates obtained with the capital set we propose. We will compare these estimates in a number of ways, in order to assess the reliability of the capital income approach. Following Teyssier (2017), the reliability test will consist of a visual inspection of the fit of the capital IOP estimates against their baseline, a comparison of the IOP estimates distributions' moments, and a correlation analysis. We report results of the accuracy test with each of the two measures of capital income that we have constructed: the dynastic component and the simple measure containing both the dynastic and the meritocratic components.

Estimates of absolute IOP are plotted in fig. 2. On the vertical axes is displayed IOP measured using our baseline set of circumstances, and on the horizontal ones are shown the estimates obtained with the capital circumstances. Panels (a) and (b) show the results of employing the dynastic component of capital income, and panels (c) and (d) show results obtained with the simple measure of both dynastic and meritocratic capital income. Parallelly, panels (a) and (c) present results in 2004's wave, which includes 26 countries, and panels (b) and (d) display the estimates of 2010, including 31 countries. Each black diamond represents a country in the sample, the solid lines are linear fits of the capital estimates against their baseline, and the dashed 45º lines represent the hypothetical ideal data adjustment. In panels (a) and (b) we can see that the linear fits of the IOP estimates obtained with the dynastic measure overlap the ideal 45º line almost completely, with beta coefficients very close to 1 in both years. This suggests that with our method we can obtain IOP estimates that are, on average, very similar to those obtained with a standard methodology, while suffering from a substantially less stringent data limitation. In panels (c) and (d) we observe that the adjustment of the IOP estimates obtained with the simple capital income measure is not as good, although still fairly close to the 45º line. Indeed, the regression coefficients are also close to 1, albeit slightly further from it. We see this as evidence in favor of the procedure to isolate the dynastic component that we followed in section 4.2; although at the same time, these results suggest that using a simple measure of capital income containing both its dynastic and meritocratic components may be accurate enough to constitute an informative approximation.

In addition to the visual analysis of fig. 2, we test the accuracy of the capital income approach by comparing moments of the IOP estimates' distributions at the country level, as well as assessing pairwise and rank correlations. The results of this comparison are shown in table 3. The moments compared are the mean value and the standard error. Standard errors have been computed via bootstrapping stratified by region, following Andreoli and Fusco (2017) (see also Goedemé 2013). The second and third columns of table 3 refer to the results using the dynastic component of capital income, in the years 2004 and 2010 respectively. The fourth and fifth columns show results obtained with the simple measure of capital income, which includes both its dynastic and meritocratic components, also in the two years studied. The distributions in 2004 have 26 observations, in 2010 they have 31. We will start by discussing the second and third columns. The dynastic measure performs satisfactorily. Its average values are similar to those of the baseline, being less than 2% bigger and 3% smaller in 2004 and 2010, respectively. The average standard error is virtually identical in 2004, although a 7% smaller in 2010. Of course, there is no limit to what difference would be acceptable, but a discrepancy of the 7% on such a small standard error (0.0004 out of the estimate 0.0204) is hardly a concern. The last two rows of the table refer to correlations between the baseline and capital IOP estimates. The

Figure 2: Graphical comparison of baseline and capital estimates of IOP



Note: 'Baseline circumstances' refers to the set including parental education and occupation; 'capital circumstances' to the set that includes either one of our two measures of capital income. 'Dynastic' refers to the capital set of circumstances including the dynastic capital income measure, 'dynastic + meritocratic' to the capital set containing the measure of both dynastic and meritocratic capital income. Estimates of absolute IOP. EU-SILC database.

Table 3: Statistical comparison of baseline and capital estimates of IOP

	Dynastic		Dynastic + meritocratic	
	2004	2010	2004	2010
Average baseline IOP	0.02206	0.02037	0.02206	0.02037
Average capital IOP	0.02234	0.01989	0.01792	0.01708
Ratio of averages	0.98736	1.02395	1.23078	1.19244
Average baseline SE	0.00035	0.00035	0.00035	0.00035
Average capital SE	0.00035	0.00033	0.00025	0.00025
Ratio of average SEs	0.99800	1.06725	1.43700	1.38948
Pairwise correlation	0.97304	0.98565	0.92216	0.96225
Rank correlation	0.97128	0.90403	0.87829	0.91452

Note: 'Baseline' refers to IOP estimates obtained with a set of circumstances including parental education and occupation; 'capital' to estimates obtained with circumstances' sets including either one of our two measures of capital income. 'Dynastic' refers to the measure based on $\hat{\varepsilon}$ from eq. (7); 'dynastic + meritocratic' to the one based on K from (6). Standard errors obtained via bootstrap stratified by region (1,000 replications). Estimates of absolute IOP. EU-SILC database.

pairwise correlations are over 0.97 in both years, what is satisfactory. This is specially important if we are interested in assessing the evolution of IOP over time. Lastly, the rank correlations are not as high, specially in 2010—but being both over .9, in any case they indicate that a country ranking high in the capital IOP estimates would be likely to rank high with standard estimates too. The rank correlations point at the fact that the capital income approach can generate accurate estimates *on average*, but that some deviations may appear in a few particular cases. Indeed, notice that some countries are relatively far from the 45º line, meaning that the capital income approach is not accurate in those particular cases. The fact that, taken individually, not all estimates are close to their baseline impels us to be cautious. Let us now look at the fourth and fifth columns. We can readily see that the simple measure of capital income does not perform as good in almost all metrics considered. Again, we see this as support for the procedure followed above to isolate the dynastic component of capital income.

It is worth noting that the capital income approach appears to perform slightly worse in 2010's wave, when the Great Recession had already started. A possible explanation may be that the financial losses produced by the crisis could have distorted the predictive power of capital income over family background. Nevertheless, this distortion is sufficiently small to disregard it as a source of concern.

In sum, the results obtained suggest that the capital income approach does perform reliably. The visual test, the moments comparison and the correlation analysis are all satisfactory. In the next subsection we show that these results hold when we vary a number of methodological choices.

4.4. Robustness

To ease the concern that the results of the reliability test might be a product of chance we perform an exhaustive robustness check. If the IOP estimates obtained with our two sets of circumstances are similar by casualty with a particular methodology, it would be unlikely that they remain similar after modifying a number of meaningful methodological choices. Table 4 shows the same statistics presented in table 3, but referring to estimates obtained with the following methodological variations:

1. Increasing the number of types, by adding "population density of the area of residence" to the sets of circumstances
2. Decreasing the number of types, by removing "immigration status" from the sets of circumstances
3. Reducing the sample size by excluding individuals who declare to be self-employed
4. Reducing the sample size by keeping only individuals who are aged 45 to 59 years old
5. Using a selection model into full-time employment
6. Avoid using any selection model into employment
7. Modifying the definition of capital income to include only property rent
8. Using the Gini index as inequality measure, instead of the MLD
9. Grouping individuals according to the capital income variables by following the cumulative distribution function of the variable parental education
10. Grouping individuals according to the capital income variables by following the arbitrary cumulative distribution function [50, 75, 100]
11. Grouping individuals according to the capital income variables by following the arbitrary cumulative distribution function [50, 100]
12. Lastly, not including a capital income variable at all in the capital circumstances' set

The second and third columns of table 4 display results obtained with the dynastic measure of capital income, while the fourth and fifth correspond to the results using the simple measure that contains both the dynastic and meritocratic components. In the following paragraphs we will only describe the results using the dynastic measure, since it has proven more useful—however, we report all results for comprehensiveness.

The first robustness check, labeled *Five circumstances*, consists of increasing the number of types in our analysis by adding "population density of the area of residence" to the circumstances' sets, distinguishing between rural and non-rural.[28] Whether living in a rural or an urban area can be considered a circumstance is controversial. We argue that living in rural areas may reflect the cost, in terms of effort, that moving to the city represents for individuals born in the countryside.[29] Nonetheless, even upon disagreement over considering population density of the area of residence a circumstance, adding it to the circumstances' sets fulfills the task at hand in any case, which is assessing the sensitivity of the accuracy test to an increase in the number of types. The results obtained seem, if anything, slightly better than those obtained with smaller sets of circumstances. This goes in line with what we expected in section 4.1, i.e., that the capital income approach is likely to perform better as the dimension of the set of circumstances increases.

In the second robustness test we reduce the number of types by removing the circumstance "immigration status", and it is labeled as *Three circumstances* in table 4. The metrics obtained do not change substantially, what is satisfactory.

The third test is named *Employees only* and consists of excluding from our sample all individuals who declare to be self-employed.[30] This robustness test responds to possible concerns over the reliability of reported income by self-employed individuals (Kleven, Knudsen, et al. 2011). We observe again that the results of the accuracy test do not change meaningfully with respect to those shown in table 3.

In the following check we reduce the sample size by keeping only individuals who are aged 45 to 59 years old, to assess the sensibility of the results to life-cycle effects. We name this test *Older cohorts*, and the results show some sensibility to it in the year 2004, specially the standard errors. However, in the year 2010 the accuracy holds. It is interesting to note that if bequests were the main factor behind the predictive capacity capital income has of family background, with this test we should see and increase in accuracy. However, this is not the case, pointing at the importance of other mechanisms.

For the fifth test, labeled *Full-time workers only*, we use a selection model into full-time

[28]The EU-SILC database includes a three-level variable on the population density with values "densely populated area", "intermediate area" and "thinly-populated area". We take thinly-populated areas as rural, which are characterized by being a contiguous group of local areas, not belonging to a densely or intermediate-populated area, each of which has a density equal or inferior to 100 inhabitants per square kilometer, with a total population for the group of less than 50,000 inhabitants, and not adjacent to a densely or intermediate-populated area. The definition of area follows the Labour Force Survey recommendations.

[29]Living in a rural or urban area can be chosen by individuals. Nevertheless, individuals tend to develop ties to the place where they were born, in the form of emotional attachment or social networks, meaning that the necessary effort to move to an urban area—where chances of economic success are higher—is positive for those born in the countryside. On the contrary, for those born in the city the required effort is zero. Also, those already born in the city may enjoy more time to develop their social capital in the area. Note that instead of a variable on where individuals *currently* live, ideally we would include a circumstance on whether individuals were *born* in the city or the country side, what is clearly outside personal choice; however, such variable is not available in the EU-SILC.

[30]To do this we first drop from our sample individuals who declare their "status in employment" (variable PL040) to be 'self-employed', and then we apply a selection model into employment.

Table 4: Robustness analysis

	Dynastic		Dynastic + meritocratic	
	2004	2010	2004	2010
Five circumstances				
Average baseline IOP	0.02501	0.02198	0.02501	0.02198
Average capital IOP	0.02531	0.02193	0.02122	0.01945
Ratio of averages	0.98780	1.00246	1.17840	1.12978
Average baseline SE	0.00029	0.00029	0.00029	0.00029
Average capital SE	0.00041	0.00032	0.00032	0.00028
Ratio of average SEs	0.72155	0.89920	0.91076	1.04067
Pairwise correlation	0.97337	0.98660	0.93552	0.95934
Rank correlation	0.95043	0.91970	0.87217	0.89901
Three circumstances				
Average baseline IOP	0.02031	0.01799	0.02031	0.01799
Average capital IOP	0.02123	0.01813	0.01677	0.01556
Ratio of averages	0.95679	0.99241	1.21076	1.15609
Average baseline SE	0.00023	0.00020	0.00023	0.00020
Average capital SE	0.00027	0.00017	0.00012	0.00014
Ratio of average SEs	0.85484	1.17619	1.90960	1.46360
Pairwise correlation	0.97026	0.97812	0.91220	0.96305
Rank correlation	0.95829	0.88226	0.83521	0.85565
Employees only				
Average baseline IOP	0.02405	0.02266	0.02405	0.02266
Average capital IOP	0.02372	0.02239	0.01988	0.01967
Ratio of averages	1.01406	1.01185	1.20954	1.15170
Average baseline SE	0.00029	0.00034	0.00029	0.00034
Average capital SE	0.00031	0.00037	0.00032	0.00024
Ratio of average SEs	0.95816	0.89869	0.90873	1.42422
Pairwise correlation	0.94173	0.97684	0.89875	0.95558
Rank correlation	0.91521	0.86855	0.89402	0.87258
Older cohorts				
Average baseline IOP	0.02564	0.02432	0.02564	0.02432
Average capital IOP	0.02290	0.02280	0.01997	0.01981
Ratio of averages	1.11977	1.06647	1.28368	1.22749
Average baseline SE	0.00037	0.00068	0.00037	0.00068
Average capital SE	0.00047	0.00047	0.00025	0.00036
Ratio of average SEs	0.79281	1.43935	1.49658	1.91130
Pairwise correlation	0.89459	0.98229	0.89968	0.95376
Rank correlation	0.90017	0.94194	0.87966	0.90081

Note: 'Baseline' refers to IOP estimates obtained with a set of circumstances including parental education and occupation; 'capital' to estimates obtained with circumstances' sets including either one of our two measures of capital income. 'Dynastic' refers to the measure based on $\hat{\varepsilon}$ from eq. (7); 'dynastic + meritocratic' to the one based on K from (6). Standard errors obtained via bootstrap stratified by region (1,000 replications). Estimates of absolute IOP. EU-SILC database.

Continuation of Table 4: Robustness analysis

	Dynastic		Dynastic + meritocratic	
	2004	2010	2004	2010
Full-time workers only				
Average baseline IOP	0.01239	0.01157	0.01239	0.01157
Average capital IOP	0.01200	0.01088	0.00851	0.00853
Ratio of averages	1.03289	1.06314	1.45575	1.35622
Average baseline SE	0.00020	0.00016	0.00020	0.00016
Average capital SE	0.00015	0.00018	0.00014	0.00013
Ratio of average SEs	1.28200	0.92862	1.46390	1.27306
Pairwise correlation	0.97203	0.97346	0.91820	0.95895
Rank correlation	0.87350	0.87823	0.69778	0.62661
No selection model				
Average baseline IOP	0.03092	0.02815	0.03092	0.02815
Average capital IOP	0.02897	0.02577	0.02338	0.02243
Ratio of averages	1.06719	1.09267	1.32271	1.25527
Average baseline SE	0.00034	0.00050	0.00034	0.00050
Average capital SE	0.00064	0.00057	0.00027	0.00034
Ratio of average SEs	0.52649	0.88575	1.23376	1.46542
Pairwise correlation	0.94988	0.96373	0.89406	0.92799
Rank correlation	0.88991	0.85081	0.79897	0.81694
Property rent only				
Average baseline IOP	0.02211	0.02039	0.02211	0.02039
Average capital IOP	0.02363	0.02299	0.01552	0.01440
Ratio of averages	0.93560	0.88699	1.42458	1.41611
Average baseline SE	0.00026	0.00030	0.00026	0.00030
Average capital SE	0.00029	0.00042	0.00011	0.00015
Ratio of average SEs	0.88956	0.71061	2.27500	2.02961
Pairwise correlation	0.96221	0.97301	0.92543	0.96754
Rank correlation	0.95829	0.88750	0.86256	0.91774
Gini				
Average baseline IOP	0.11063	0.10339	0.11063	0.10339
Average capital IOP	0.10862	0.09953	0.09426	0.09046
Ratio of averages	1.01852	1.03880	1.17369	1.14299
Average baseline SE	0.00071	0.00073	0.00071	0.00073
Average capital SE	0.00067	0.00080	0.00059	0.00062
Ratio of average SEs	1.06192	0.91418	1.21418	1.18245
Pairwise correlation	0.95221	0.98054	0.86328	0.93924
Rank correlation	0.95556	0.93750	0.86188	0.90040

Note: 'Baseline' refers to IOP estimates obtained with a set of circumstances including parental education and occupation; 'capital' to estimates obtained with circumstances' sets including either one of our two measures of capital income. 'Dynastic' refers to the measure based on $\hat{\varepsilon}$ from eq. (7); 'dynastic + meritocratic' to the one based on K from (6). Standard errors obtained via bootstrap stratified by region (1,000 replications). Estimates of absolute IOP. EU-SILC database.

Continuation of Table 4: Robustness analysis

	Dynastic		Dynastic + meritocratic	
	2004	2010	2004	2010
Parental education cdf				
Average baseline IOP	0.02211	0.02039	0.02211	0.02039
Average capital IOP	0.02248	0.02091	0.01765	0.01695
Ratio of averages	0.98347	0.97525	1.25282	1.20321
Average baseline SE	0.00026	0.00030	0.00026	0.00030
Average capital SE	0.00025	0.00026	0.00016	0.00021
Ratio of average SEs	1.04503	1.15546	1.65065	1.40303
Pairwise correlation	0.89915	0.96865	0.91613	0.96042
Rank correlation	0.82838	0.87137	0.87145	0.91653
Three arbitrary groups				
Average baseline IOP	0.02211	0.02039	0.02211	0.02039
Average capital IOP	0.02385	0.02143	0.01725	0.01674
Ratio of averages	0.92718	0.95152	1.28162	1.21836
Average baseline SE	0.00026	0.00030	0.00026	0.00030
Average capital SE	0.00040	0.00020	0.00014	0.00017
Ratio of average SEs	0.64899	1.51811	1.91564	1.78272
Pairwise correlation	0.96037	0.97369	0.88741	0.95354
Rank correlation	0.92957	0.87419	0.83316	0.89960
Two arbitrary groups				
Average baseline IOP	0.02211	0.02039	0.02211	0.02039
Average capital IOP	0.02319	0.02090	0.01640	0.01571
Ratio of averages	0.95336	0.97600	1.34792	1.29843
Average baseline SE	0.00026	0.00030	0.00026	0.00030
Average capital SE	0.00026	0.00019	0.00015	0.00013
Ratio of average SEs	0.99038	1.60418	1.73478	2.22040
Pairwise correlation	0.96078	0.97387	0.90243	0.95652
Rank correlation	0.92821	0.86976	0.83658	0.90524
No capital income variable				
Average baseline IOP	0.02211	0.02039	0.02211	0.02039
Average capital IOP	0.01541	0.01417	0.01541	0.01417
Ratio of averages	1.43499	1.43943	1.43499	1.43943
Average baseline SE	0.00026	0.00030	0.00026	0.00030
Average capital SE	0.00012	0.00009	0.00011	0.00011
Ratio of average SEs	2.24576	3.29609	2.44565	2.59794
Pairwise correlation	0.92923	0.96294	0.92923	0.96294
Rank correlation	0.87282	0.91774	0.87282	0.91774

Note: 'Baseline' refers to IOP estimates obtained with a set of circumstances including parental education and occupation; 'capital' to estimates obtained with circumstances' sets including either one of our two measures of capital income. 'Dynastic' refers to the measure based on $\hat{\varepsilon}$ from eq. (7); 'dynastic + meritocratic' to the one based on K from (6). Standard errors obtained via bootstrap stratified by region (1,000 replications). Estimates of absolute IOP. EU-SILC database.

employment during at least 7 months in the reference year. This may be preferred by some researchers since assuming that, once we have accounted for circumstances, all differences in incomes between people who are employed full-time respond to IOP may be weaker than assuming the same thing among people who work both full- and part-time. On the other hand, part of the effect of circumstances works through labor market participation, and hence modeling full-time employment might "leave out" some IOP, especially that related to gender (we have discussed this trade-off in section 4.1). In any case, the level of IOP estimates are very sensible to this sample variations, since in our analysis we consider wages. In fact, observe that the average level of IOP has halved. Table 4 shows that the capital income approach performs slightly worse with this methodological variation. Yet, with average moments reasonably similar and pairwise correlations around .97 we argue that our method responds with satisfactory robustness to such a substantial change in the sample composition.

The sixth variation consists of not using any selection model at all. The way in which we proceed consists simply of using a sample composed only by individuals who declared to have been at work at least 7 months in the reference period, either full- or part-time, employed or self-employed. Results following this methodology suffer, naturally, from a self-selection bias. The average level of IOP increases, and the accuracy of the capital income approach diminishes just slightly.

For the seventh test we defined capital income in a different, more restrictive way. In table 4 this check is referred to as *Property rent only*. Gross household capital income is now defined as gross income coming from rental of a property or land (variable HY040G) (we therefore avoid including interests, dividends and profits from capital investment in an unincorporated business—variable HY090G). Checking results using this even more limited definition of capital income can be fruitful since we can expect the reliability of variable HY090 to be lower than that of HY040. One reason for this is that it is easier for individuals to keep track of rental income (which generally comes from a small number of sources) than it is to keep it of interests, dividends and profits (that usually requires to follow closely one's banks accounts and investments). This test suggests the capital income approach is reasonably robust to an alternative definition of capital income.

With the eighth check we see that using the Gini index as inequality measure returns results as good as when we use the MLD. The Gini index is not so popular in the IOP field as the MLD is, but we include it here because it is employed sometimes.

The following three tests are related to the way in which we group individuals when building our measures of capital income. Instead of using the algorithm described in section 4.2 to decide the groupings, we make arbitrary choices. The different possibilities shown are when our capital income variables' cumulative distribution functions a) mimic that of the variable parental education (which is different in each country and year), b) follow the pattern $[50, 75, 100]$, and c) the pattern $[50, 100]$. It is reassuring to see that the accuracy of the capital income approach is rather robust to the way in which we group individuals.

The last test of the robustness analysis consists of simply not including any measure of capital income in our capital set of circumstances. This is, to omit accounting for family background in any way. We do this by comparing the results of our baseline set composed of gender, immigrant status and parental education and occupation to those of a set that includes only gender and immigrant status. We perform this test because such strategy is an alternative to the capital income approach, followed for instance by Marrero, Rodríguez, and Weide (2016). Interestingly, the correlations diminish only slightly, what can be interpreted as suggesting that the effect of family background on IOP remained, on average, constant between 2004 and 2010. This result speaks good of the strategy followed by Marrero, Rodríguez, and Weide (2016). However, it also shows that the capital income approach entails an improvement over it. Notice that under *No capital income variable* in table 4 the average IOP estimates and their average standard errors are much lower than the baseline, up to three times smaller. Hence, this test remarks that the capital income approach adds to previously applied strategies trying to overcome the limitation imposed by the scarcity of data on family background.

In conclusion, after trying a large number of substantial methodological changes and observed no major differences in the accuracy results, we conclude that the robustness analysis performed provides evidence that the results of our reliability analysis are not spurious, and hence further support the capital income approach.

5. Measuring IOP for the full length of the EU-SILC database

Once we have validated, in the previous section, the capital income approach, we now proceed to take advantage of it and obtain IOP estimates for the full length of the EU-SILC database. We are able to obtain a remarkable number of new IOP estimates. Figures 3 and 4 show, to the best of our knowledge, the most comprehensive estimation of lower-bound IOP in Europe produced so far. Figure 3 shows absolute IOP, fig. 4 displays relative estimates. For both figures we have used the dynastic measure of capital income, since it appears to perform better than the simple measure of capital income. Confidence intervals are shown as gray areas. These figures include as well IOP estimates obtained using the baseline circumstances, with their confidence intervals shown in red brackets, for the only periods in which they can be estimated, 2004 and 2010. This superposition allows to rapidly verify to what extent the baseline and capital estimates are similar, and it also illustrates how large is the number of new data points available thanks to the capital income approach.

The main conclusion we can draw is that in many European countries the level of absolute IOP has not remained stable during the last two decades. Please bear in mind that since we use axes with a common scale it may be difficult to appreciate the extent of the changes occurred in some countries. To asses this in more detail consult tables 5 and 6.

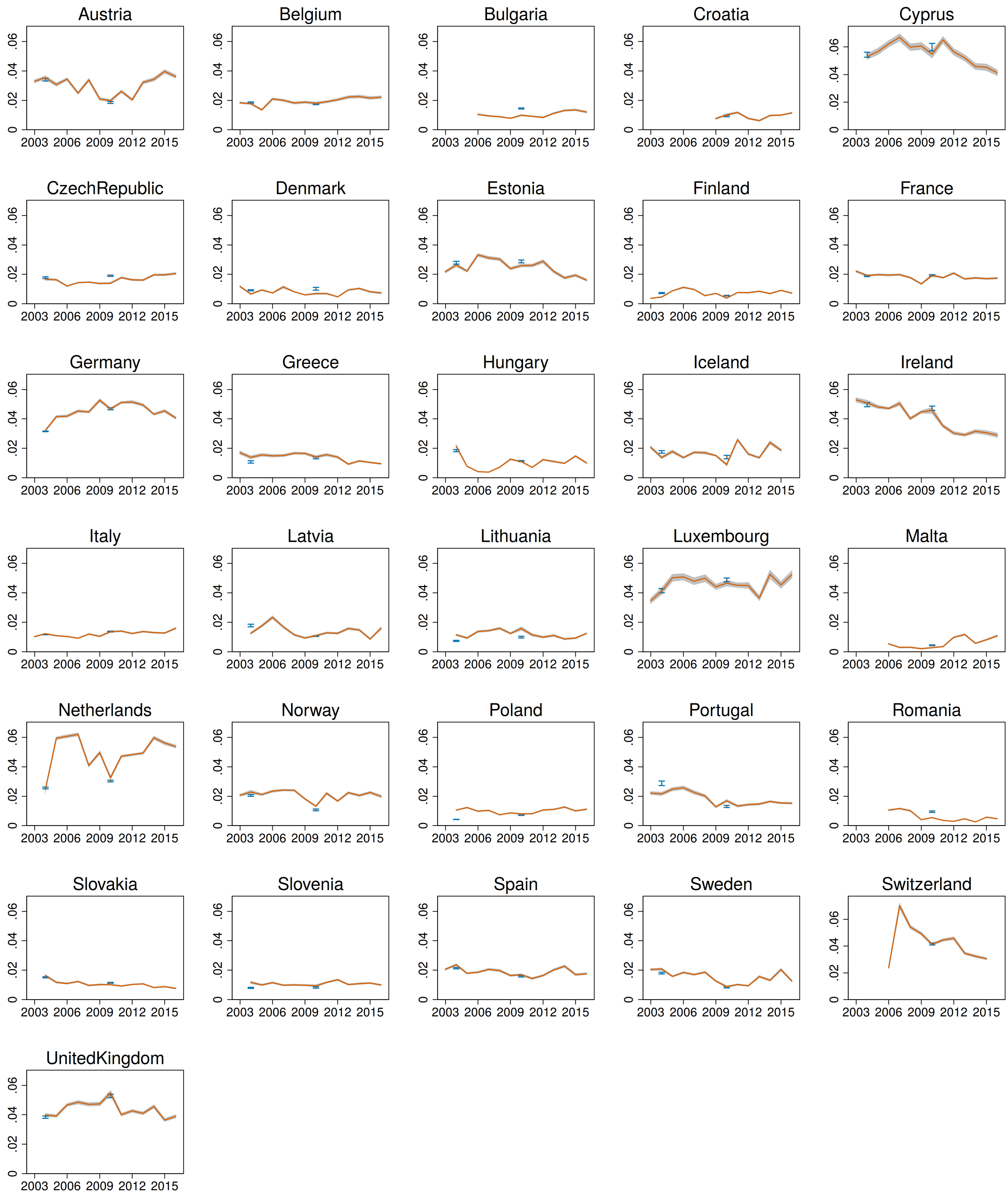
Since the Great Recession the absolute value of IOP has increased substantially in Austria and the Netherlands, and decreased in Cyprus, Estonia, Germany, Ireland, Portugal, Switzerland and the United Kingdom. Looking at relative IOP we observe a similar picture, although we could add Belgium and Italy to the list of countries with a rising level, and Greece and Spain to the ones with diminishing IOP.

This is, we believe, the first time that evidence on this matter can be offered for such a big number of European economies. However, we will not perform an exhaustive analysis of the evolution of IOP in Europe here, since this article is devoted to test the method proposed and to show the possibilities opened up by it—which are not restricted to the use of the EU-SILC database. Possible applications include obtaining historical estimates of IOP, studying the role of institutions, or analyzing the relationship of IOP with economic growth and political outcomes.

6. Concluding remarks

In this article we have proposed a new approach to measure IOP that does not rely on the availability of data on parental features. After testing this method by comparing its results to those of a standard approach, we conclude that it is sufficiently reliable to be used when we lack information on parental background. The results of the method proposed are not equal in all countries and periods to their baseline, what impels us to be cautious—yet, we believe they are similar enough to constitute an informative approximation. Furthermore, when taken on average, the IOP estimates returned by the capital income approach are nearly identical to those of their baseline. The visual test, the moments comparison and the correlation analysis provide strong support for the capital income approach, and in response to the concern that these metrics may be spurious, we have conducted an extensive robustness test that makes this possibility unlikely. We believe that, in light of the tests performed, the capital income approach can help foster the empirical measurement of IOP.

Figure 3: Evolution of absolute lower-bound IOP in Europe



Note: Confidence intervals shown as gray areas for the IOP estimates obtained with the dynastic measure of capital income, and as red brackets for those obtained with the baseline set of circumstances. All have been calculated with standard errors computed via bootstrapping stratified by region (1000 replications). EU-SILC database.

Figure 4: Evolution of relative lower-bound IOP in Europe

Note: Confidence intervals shown as gray areas for the IOP estimates obtained with the dynastic measure of capital income, and as red brackets for those obtained with the baseline set of circumstances. All have been calculated with standard errors computed via bootstrapping stratified by region (1000 replications). EU-SILC database.

Table 5: Absolute lower-bound IOP in Europe

	2003	2004*	2004	2005	2006	2007	2008	2009	2010*	2010	2011	2012	2013	2014	2015	2016
Austria	0.0328	0.0343	0.0357	0.0308	0.0344	0.0252	0.0340	0.0209	0.0188	0.0201	0.0264	0.0210	0.0325	0.0346	0.0404	0.0370
Belgium	0.0183	0.0186	0.0178	0.0138	0.0210	0.0203	0.0189	0.0196	0.0174	0.0182	0.0194	0.0207	0.0229	0.0234	0.0224	0.0228
Bulgaria					0.0105	0.0094	0.0089	0.0078	0.0145	0.0099	0.0092	0.0084	0.0113	0.0131	0.0135	0.0121
Croatia								0.0076	0.0094	0.0102	0.0118	0.0078	0.0063	0.0097	0.0099	0.0115
Cyprus		0.0530	0.0521	0.0553	0.0602	0.0651	0.0589	0.0597	0.0575	0.0530	0.0608	0.0547	0.0508	0.0453	0.0454	0.0410
Czech Rep.		0.0169	0.0160	0.0162	0.0120	0.0144	0.0147	0.0138	0.0190	0.0139	0.0177	0.0159	0.0161	0.0196	0.0197	0.0205
Denmark	0.0119	0.0090	0.0069	0.0095	0.0074	0.0115	0.0083	0.0060	0.0104	0.0070	0.0069	0.0048	0.0095	0.0104	0.0083	0.0075
Estonia	0.0218	0.0278	0.0263	0.0222	0.0332	0.0312	0.0303	0.0241	0.0292	0.0265	0.0263	0.0289	0.0221	0.0176	0.0195	0.0161
Finland	0.0038	0.0072	0.0045	0.0087	0.0111	0.0097	0.0056	0.0071	0.0057	0.0038	0.0077	0.0077	0.0086	0.0070	0.0091	0.0072
France	0.0222	0.0189	0.0193	0.0200	0.0198	0.0199	0.0180	0.0137	0.0195	0.0197	0.0179	0.0210	0.0173	0.0178	0.0173	0.0174
Germany		0.0315	0.0321	0.0410	0.0408	0.0456	0.0447	0.0525	0.0469	0.0468	0.0511	0.0515	0.0494	0.0431	0.0454	0.0407
Greece	0.0156	0.0100	0.0132	0.0147	0.0142	0.0142	0.0162	0.0162	0.0125	0.0131	0.0149	0.0137	0.0092	0.0111	0.0098	0.0090
Hungary		0.0184	0.0213	0.0078	0.0041	0.0038	0.0071	0.0126	0.0113	0.0109	0.0070	0.0123	0.0110	0.0098	0.0147	0.0101
Iceland	0.0207	0.0175	0.0138	0.0178	0.0132	0.0171	0.0170	0.0150	0.0140	0.0088	0.0258	0.0161	0.0136	0.0239	0.0188	
Ireland	0.0529	0.0497	0.0510	0.0482	0.0474	0.0507	0.0407	0.0461	0.0479	0.0464	0.0357	0.0308	0.0293	0.0317	0.0310	0.0295
Italy	0.0102	0.0116	0.0121	0.0108	0.0103	0.0090	0.0119	0.0105	0.0138	0.0136	0.0140	0.0125	0.0140	0.0130	0.0125	0.0158
Latvia		0.0177	0.0125	0.0175	0.0234	0.0169	0.0115	0.0093	0.0106	0.0110	0.0129	0.0125	0.0157	0.0147	0.0087	0.0158
Lithuania		0.0072	0.0114	0.0093	0.0138	0.0143	0.0160	0.0124	0.0099	0.0158	0.0114	0.0099	0.0110	0.0086	0.0093	0.0124
Luxembourg	0.0343	0.0415	0.0411	0.0500	0.0508	0.0478	0.0496	0.0443	0.0494	0.0471	0.0455	0.0458	0.0368	0.0530	0.0456	0.0524
Malta					0.0054	0.0028	0.0028	0.0020	0.0037	0.0021	0.0034	0.0089	0.0112	0.0055	0.0082	0.0105
Netherlands		0.0253	0.0243	0.0595	0.0607	0.0620	0.0408	0.0495	0.0296	0.0320	0.0473	0.0485	0.0494	0.0599	0.0561	0.0538
Norway	0.0204	0.0205	0.0229	0.0212	0.0235	0.0242	0.0240	0.0180	0.0105	0.0129	0.0220	0.0172	0.0225	0.0206	0.0226	0.0198
Poland		0.0042	0.0106	0.0123	0.0098	0.0104	0.0072	0.0086	0.0070	0.0080	0.0081	0.0106	0.0111	0.0126	0.0101	0.0112
Portugal	0.0221	0.0286	0.0216	0.0249	0.0258	0.0228	0.0202	0.0127	0.0130	0.0169	0.0133	0.0142	0.0147	0.0164	0.0154	0.0151
Romania					0.0106	0.0116	0.0102	0.0040	0.0093	0.0054	0.0036	0.0029	0.0047	0.0025	0.0058	0.0048
Slovakia		0.0151	0.0164	0.0117	0.0110	0.0123	0.0096	0.0102	0.0114	0.0101	0.0093	0.0103	0.0107	0.0082	0.0088	0.0077
Slovenia		0.0081	0.0118	0.0100	0.0114	0.0098	0.0098	0.0097	0.0083	0.0095	0.0115	0.0133	0.0102	0.0108	0.0112	0.0100
Spain	0.0206	0.0213	0.0237	0.0179	0.0186	0.0204	0.0198	0.0168	0.0162	0.0173	0.0151	0.0169	0.0206	0.0229	0.0177	0.0183
Sweden	0.0206	0.0181	0.0208	0.0161	0.0184	0.0170	0.0185	0.0126	0.0084	0.0089	0.0107	0.0098	0.0161	0.0131	0.0203	0.0129
Switzerland					0.0238	0.0702	0.0540	0.0488	0.0413	0.0410	0.0442	0.0458	0.0343	0.0323	0.0305	
UK		0.0382	0.0396	0.0388	0.0467	0.0484	0.0464	0.0473	0.0525	0.0547	0.0397	0.0424	0.0410	0.0457	0.0367	0.0387

Note: Years followed by a star (*) indicate values obtained with the baseline set of circumstances, all other estimates were obtained with the set including a dynastic measure of capital income. EU-SILC database.

Table 6: Relative lower-bound IOP in Europe

	2003	2004*	2004	2005	2006	2007	2008	2009	2010*	2010	2011	2012	2013	2014	2015	2016
Austria	0.1922	0.2000	0.2081	0.1667	0.1722	0.1040	0.1350	0.0684	0.0771	0.0826	0.1051	0.0919	0.1366	0.1385	0.1641	0.1650
Belgium	0.1740	0.1758	0.1687	0.1438	0.1956	0.1661	0.1770	0.1831	0.1528	0.1592	0.1923	0.2042	0.2338	0.2385	0.2321	0.2340
Bulgaria					0.0956	0.0707	0.0739	0.0664	0.1465	0.0998	0.0916	0.0816	0.0856	0.1056	0.0662	0.0597
Croatia								0.0916	0.1198	0.1311	0.1622	0.1015	0.0835	0.1116	0.1110	0.1250
Cyprus		0.2939	0.2885	0.2930	0.2904	0.2841	0.2406	0.2364	0.2703	0.2489	0.2603	0.2255	0.2342	0.2217	0.2376	0.2010
Czech Rep.		0.1667	0.1580	0.1662	0.1268	0.1504	0.1591	0.1522	0.1895	0.1391	0.1695	0.1564	0.1599	0.1852	0.1807	0.1909
Denmark	0.1099	0.0881	0.0671	0.0914	0.0687	0.0984	0.0846	0.0663	0.0898	0.0609	0.0764	0.0548	0.1013	0.0942	0.0685	0.0560
Estonia	0.1223	0.1738	0.1641	0.1457	0.2099	0.2033	0.2003	0.1719	0.1961	0.1779	0.1756	0.1876	0.0975	0.0889	0.0958	0.0777
Finland	0.0141	0.0275	0.0173	0.0321	0.0395	0.0373	0.0266	0.0351	0.0307	0.0206	0.0377	0.0383	0.0447	0.0358	0.0447	0.0379
France	0.1715	0.1333	0.1362	0.1364	0.1450	0.1205	0.1030	0.0778	0.1088	0.1099	0.1031	0.1269	0.0967	0.1083	0.1062	0.1046
Germany		0.1672	0.1704	0.1627	0.1486	0.1986	0.2034	0.2389	0.2306	0.2301	0.2269	0.2265	0.2269	0.1995	0.2234	0.2047
Greece	0.1649	0.1001	0.1321	0.1704	0.1760	0.1828	0.1926	0.2054	0.1440	0.1511	0.2250	0.2017	0.1589	0.1484	0.1044	0.1055
Hungary		0.1054	0.1219	0.0588	0.0315	0.0288	0.0562	0.1068	0.0985	0.0950	0.0635	0.1199	0.1017	0.0853	0.1191	0.0982
Iceland	0.0926	0.0767	0.0605	0.0789	0.0561	0.0761	0.0778	0.0770	0.0758	0.0478	0.1320	0.0816	0.0690	0.1158	0.0957	
Ireland	0.3025	0.2948	0.3027	0.2308	0.1854	0.2264	0.2300	0.1965	0.2965	0.2877	0.2080	0.1679	0.1777	0.1762	0.1815	0.1602
Italy	0.1133	0.1570	0.1632	0.1201	0.1079	0.1015	0.1163	0.1042	0.1260	0.1247	0.1419	0.1132	0.1371	0.1224	0.1221	0.1607
Latvia		0.1256	0.0889	0.1084	0.1391	0.0880	0.0632	0.0557	0.0625	0.0649	0.0795	0.0761	0.0954	0.0939	0.0535	0.0946
Lithuania		0.0459	0.0722	0.0571	0.0841	0.0969	0.1050	0.0587	0.0573	0.0913	0.0648	0.0610	0.0694	0.0468	0.0545	0.0743
Luxembourg	0.1798	0.2324	0.2302	0.2547	0.2494	0.2167	0.2432	0.2216	0.2681	0.2557	0.2532	0.2739	0.2272	0.3054	0.2313	0.2488
Malta					0.0722	0.0361	0.0301	0.0175	0.0356	0.0203	0.0273	0.0711	0.0952	0.0432	0.0675	0.0927
Netherlands		0.1187	0.1141	0.3141	0.3287	0.3524	0.2097	0.2713	0.1708	0.1842	0.2682	0.2762	0.2671	0.3216	0.2855	0.2881
Norway	0.1086	0.1093	0.1222	0.1110	0.1160	0.1237	0.1336	0.1027	0.0620	0.0760	0.1233	0.1018	0.1260	0.1194	0.1242	0.1268
Poland		0.0387	0.0965	0.1124	0.0900	0.0920	0.0618	0.0796	0.0658	0.0747	0.0806	0.1036	0.1065	0.1171	0.0898	0.1105
Portugal	0.1417	0.1750	0.1323	0.1531	0.1326	0.1443	0.1375	0.1004	0.1004	0.1307	0.1061	0.1363	0.1370	0.1529	0.1364	0.1365
Romania					0.1085	0.1340	0.1254	0.0591	0.1365	0.0787	0.0553	0.0440	0.0644	0.0431	0.0969	0.0782
Slovakia		0.1525	0.1654	0.1272	0.1248	0.1319	0.1198	0.1213	0.1318	0.1171	0.1201	0.1211	0.1269	0.1464	0.1270	0.1167
Slovenia		0.0496	0.0725	0.0719	0.0806	0.0697	0.0700	0.0630	0.0495	0.0561	0.0767	0.0847	0.0702	0.0741	0.0744	0.0667
Spain	0.1970	0.1941	0.2154	0.1761	0.1687	0.1312	0.1326	0.1172	0.1175	0.1260	0.1086	0.1219	0.1452	0.1493	0.1155	0.1011
Sweden	0.1073	0.1039	0.1198	0.0911	0.0980	0.0961	0.1135	0.0789	0.0487	0.0518	0.0654	0.0640	0.0931	0.0796	0.1295	0.0844
Switzerland					0.1162	0.2427	0.2270	0.2122	0.1821	0.1809	0.1994	0.2127	0.1523	0.1499	0.1414	
UK		0.1803	0.1868	0.1882	0.2119	0.2104	0.2015	0.2165	0.2390	0.2488	0.2108	0.2094	0.2056	0.2243	0.1776	0.1669

Note: Years followed by a star (*) indicate values obtained with the baseline set of circumstances, all other estimates were obtained with the set including a dynastic measure of capital income. EU-SILC database.

References

Alesina, A., S. Stantcheva, and E. Teso, “Intergenerational Mobility and Preferences for Redistribution,” *American Economic Review,* 108, 521–554, 2018.

Almås, I., A.W. Cappelen, J.T. Lind, E. Sørensen, and B. Tungodden, “Measuring unfair (in)equality,” *Journal of Public Economics,* 95, 488–499, 2011.

Almås, I., A.W. Cappelen, E. Sørensen, and B. Tungodden, “Fairness and the Development of Inequality Acceptance,” *Science,* 328, 1176–1178, 2010.

Alvaredo, F., B. Garbinti, and T. Piketty, “On the Share of Inheritance in Aggregate Wealth: Europe and the USA, 1900–2010,” *Economica,* 84, 239–260, 2017.

Andreoli, F. and A. Fusco, “The evolution of inequality of opportunity across Europe: EU-SILC evidence,” in A.B. Atkinson, A.-C. Guio, and E. Marlier, eds., *Monitoring social inclusion in Europe*, 435–448, Publications Office of the European Union, Luxembourg, 2017.

Arneson, R.J., “Equal Opportunity for Welfare,” *Philosophical Studies,* 56, 77–93, 1989.

Atkinson, A.B., “On the Measurement of Income Inequality,” *Journal of Economic Theory,* 2, 244–263, 1970.

Becker, G.S. and N. Tomes, “An Equilibrium Theory of the Distribution of Income and Intergenerational Mobility,” *Journal of Political Economy,* 87, 1153–1189, 1979.

———, “Human capital and the rise and fall of families.” *Journal of labor economics,* 4, 1–47, 1986.

Bénabou, R. and J. Tirole, “Belief in a just world and redistributive politics,” *Quarterly Journal of Economics,* 121, 699–746, 2006.

Berg, A., J.D. Ostry, C.G. Tsangarides, and Y. Yakhshilikov, “Redistribution, inequality, and growth: new evidence,” *Journal of Economic Growth,* 23, 259–305, 2018.

Boserup, S.H., W. Kopczuk, and C.T. Kreiner, “The Role of Bequests in Shaping Wealth Inequality: Evidence from Danish Wealth Records,” *American Economic Review: Papers & Proceedings,* 106, 656–661, 2016.

———, “Intergenerational Wealth Formation over the Life Cycle: Evidence from Danish Wealth Records 1984-2013”, 2017.

———, “Born with a Silver Spoon? Danish Evidence on Wealth Inequality in Childhood,” *Economic Journal,* 128, F514–F544, 2018.

Brosnan, S.F. and F.B. De Waal, “Monkeys reject unequal pay,” *Nature,* 425, 297–299, 2003.

Brunori, P., V. Peragine, and L. Serlenga, “Upward and downward bias when measuring inequality of opportunity,” *Social Choice and Welfare,* 52, 635–661, 2019.

Bucher-Koenen, T. and M. Ziegelmeyer, “Who Lost the Most? Financial Literacy, Cognitive Abilities, and the Financial Crisis”, 2011.

Bursztyn, L., F. Thomas, and A. Pallais, “‘Acting Wife’: Marriage Market Incentives,” *American Economic Review,* 107, 3288–3319, 2017.

Cappelen, A.W., T. Eichele, K. Hugdahl, K. Specht, E. Sørensen, and B. Tungodden, “Equity theory and fair inequality: A neuroeconomic study,” *Proceedings of the National Academy of Sciences of the United States of America,* 111, 15368–15372, 2014.

Celarent, B., “The Rise of the Meritocracy, 1870–2033, by Michael Young,” *American Journal of Sociology,* 115, 322–326, 2009.

Charles, K.K. and E. Hurst, “The Correlation of Wealth across Generations,” *Journal of Political Economy,* 111, 1155–1182, 2003.

Checchi, D. and V. Peragine, “Inequality of opportunity in Italy,” *The Journal of Economic Inequality,* 8, 429–450, 2010.

Checchi, D., V. Peragine, and L. Serlenga, “Inequality of opportunity in Europe: Is there a role for institutions?,” in L. Cappellari, S.W. Polachek, and K. Tatsiramos, eds., *Research in Labor Economics*, vol. 43, 1–44, Emerald Group Publishing Limited, 2016.

Chetty, R., N. Hendren, P. Kline, and E. Saez, “Where is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States,” *Quarterly Journal of Economics,* 129, 1553–1623, 2014.

Cohen, G., “On the Currency of Egalitarian Justice,” *Ethics,* 99, 906–944, 1989.

Corak, M., “Income Inequality, Equality of Opportunity, and Intergenerational Mobility,” *Journal of Economic Perspectives,* 27, 79–102, 2013.

Davies, J.B. and A.F. Shorrocks, “The distribution of wealth,” in A.B. Atkinson and F. Bourguignon, eds., *Handbook of Income Distribution*, vol. 1, chap. 11, 605–675, 2000.

Dawes, C.T., J.H. Fowler, T. Johnson, R. McElreath, and O. Smirnov, “Egalitarian motives in humans,” *Nature,* 446, 794–796, 2007.

Dougherty, P.J., *Who’s afraid of Adam Smith? How the market got its soul*, Wiley, New Jersey, 2002.

Dworkin, R., “What is Equality? Part 1: Equality of Welfare,” *Public Affairs,* 10, 185–246, 1981.

———, “What is Equality? Part 2: Equality of Resources,” *Public Affairs,* 10, 283–345, 1981.

Eurostat, *EU Statistics on Income and Living Conditions*, 2019.

Evensky, J., “Adam Smith’s Theory of Moral Sentiments: On morals and why they matter to a liberal society of free people and free markets,” *Journal of Economic Perspectives,* 19, 109–130, 2005.

Fagereng, A., L. Guiso, D. Malacrino, and L. Pistaferri, “Heterogeneity in Returns to Wealth and the Measurement of Wealth Inequality,” *American Economic Review: Papers & Proceedings,* 106, 651–655, 2016.

———, “Heterogeneity and Persistence in Returns to Wealth,” *Econometrica,* 88, 115–170, 2020.

Fagereng, A., M. Mogstad, and M. Ronning, “Why Do Wealthy Parents Have Wealthy Children?”, 2018.

Fehr, E. and S. Gachter, “Cooperation and punishment in public goods experiments,” *American Economic Review,* 90, 980–994, 2000.

Fehr, E. and K.M. Schmidt, “A Theory of Fairness, Competition and Cooperation,” *The Quarterly Journal of Economics,* 114, 817–868, 1999.

Ferreira, F.H.G. and J. Gignoux, “The Measurement of Inequality of Opportunity: Theory and an Application to Latin America,” *Review of Income and Wealth,* 57, 622–657, 2011.

Ferreira, F.H.G., J. Gignoux, and M. Aran, “Measuring Inequality of Opportunity with Imperfect Data: The Case of Turkey,” *Journal of Economic Inequality,* 9, 651–680, 2011.

Ferreira, F.H.G. and V. Peragine, “Individual Responsibility and Equality of Opportunity,” in M.D. Adler and M. Fleurbaey, eds., *The Oxford Handbook of Well-Being and Public Policy*, 833–881, Oxford University Press, 2016.

Fleurbaey, M., “Three solutions for the compensation problem,” *Journal of Economic Theory,* 65, 505–521, 1995.

———, *Fairness, Responsibility, and Welfare*, Oxford University Press, London, 2008.

Fleurbaey, M. and V. Peragine, “Ex Ante Versus Ex Post Equality of Opportunity,” *Economica,* 80, 118–130, 2013.

Fong, C., “Social preferences, self-interest, and the demand for redistribution,” *Journal of Public Economics,* 82, 225–246, 2001.

Foster, J.E. and N. Lustig, “Spotlight 3.2: Choosing an inequality index,” in P. Conceição, ed., *Human Development Report 2019*, 136–138, United Nations Development Programme, New York, 2019.

Foster, J.E. and A.A. Shneyerov, “Path Independent Inequality Measures,” *Journal of Economic Theory,* 91, 199–222, 2000.

Goedemé, T., “How much Confidence can we have in EU-SILC? Complex Sample Designs and the Standard Error of the Europe 2020 Poverty Indicators,” *Social Indicators Research,* 110, 89–110, 2013.

Goldin, C., “A grand gender convergence: Its last chapter,” *American Economic Review,* 104, 1091–1119, 2014.

Hällsten, M. and F.T. Pfeffer, “Grand Advantage: Family Wealth and Grandchildren’s Educational Achievement in Sweden,” *American Sociological Review,* 82, 328–360, 2017.

Hansen, M.N., “Self-Made Wealth or Family Wealth? Changes in Intergenerational Wealth Mobility,” *Social Forces,* 93, 457–481, 2014.

Heckman, J.J., “Sample Selection Bias as a Specification Error,” *Econometrica,* 47, 153–161, 1979.

———, “Skill Formation and the Economics of Investing in Disadvantaged Children,” *Science,* 312, 1900–1902, 2006.

Hsieh, C.-T., E. Hurst, C.I. Jones, and P.J. Klenow, “The Allocation of Talent and U.S. Economic Growth,” *Econometrica,* 87, 1439–1474, 2019.

Hufe, P., A. Peichl, J.E. Roemer, and M. Ungerer, “Inequality of income acquisition: the role of childhood circumstances,” *Social Choice and Welfare,* 49, 499–544, 2017.

Kleven, H.J., M.B. Knudsen, C.T. Kreiner, S. Pedersen, and E. Saez, “Unwilling or Unable to Cheat? Evidence From a Tax Audit Experiment in Denmark,” *Econometrica,* 79, 651–692, 2011.

Kleven, H.J., C. Landais, and J.E. Søgaard, “Children and Gender Inequality: Evidence from Denmark,” *American Economic Journal: Applied Economics,* 11, 181–209, 2019.

Konow, J., “Fair shares: Accountability and cognitive dissonance in allocation decisions,” *American Economic Review,* 90, 1072–1091, 2000.

Kopczuk, W., “Bequest and tax planning: Evidence from estate tax returns,” *Quarterly Journal of Economics,* 122, 1801–1854, 2007.

Kotlikoff, L.J., “Intergenerational Transfers and Savings Behavior,” *Journal of Economic Perspectives,* 2, 41–58, 1988.

Kotlikoff, L.J. and L.H. Summers, “The Role of Intergenerational Transfers in Aggregate Capital Accumulation,” *Journal of Political Economy,* 89, 706–732, 1981.

Lee, S.Y.T. and A. Seshadri, “On the intergenerational transmission of economic status,” *Journal of Political Economy,* 127, 855–921, 2019.

Lefranc, A., N. Pistolesi, and A. Trannoy, “Equality of opportunity and luck: Definitions and testable conditions, with an application to income in France,” *Journal of Public Economics,* 93, 1189–1207, 2009.

Marrero, G.A. and J.G. Rodríguez, “Inequality of Opportunity in Europe,” *Review of Income and Wealth,* 58, 597–621, 2012.

Marrero, G.A., J.G. Rodríguez, and R. van der Weide, “Unequal Opportunity, Unequal Growth”, 2016.

Milanovic, B., “Global Inequality of Opportunity: How Much of Our Income Is Determined by Where We Live?,” *The Review of Economics and Statistics,* 97, 452–460, 2015.

Mitchell, G., P.E. Tetlock, B.A. Mellers, and L.D. Ordóñez, “Judgments of social justice: Compromises between equality and efficiency.” *Journal of Personality and Social Psychology,* 65, 629–639, 1993.

Modigliani, F., “The Life Cycle Hypothesis of Saving, the Demand for Wealth and the Supply of Capital,” *Social Research,* 33, 160–217, 1966.

———, “Life Cycle, Individual Thrift, and the Wealth of Nations,” *American Economic Review,* 76, 297–313, 1986.

———, “The Role of Intergenerational Transfers and Life Cycle Saving in the Accumulation of Wealth,” *Journal of Economic Perspectives,* 2, 15–40, 1988.

Mudrazija, S., “The balance of intergenerational family transfers: A life-cycle perspective,” *European Journal of Ageing,* 11, 249–259, 2014.

Niehues, J. and A. Peichl, “Upper bounds of inequality of opportunity: Theory and evidence for Germany and the US,” *Social Choice and Welfare,* 43, 73–99, 2014.

Peichl, A. and M. Ungerer, “Accounting for the spouse when measuring inequality of opportunity,” *Social Choice and Welfare,* 47, 607–631, 2016.

Peragine, V., “Opportunity egalitarianism and income inequality,” *Mathematical social sciences,* 44, 45–64, 2002.

Pignataro, G., “Equality Of Opportunity: Policy and measurement paradigms,” *Journal of Economic Surveys,* 26, 800–834, 2012.

Piketty, T., “Social Mobility and Redistributive Politics,” *The Quarterly Journal of Economics,* 110, 551–584, 1995.

———, “On the long-run evolution of inheritance: France 1820-2050,” *Quarterly Journal of Economics,* 126, 1071–1131, 2011.

———, *Capital in the Twenty-First Century*, Harvard University Press, Cambridge, 2014.

Piketty, T., G. Postel-Vinay, and J.L. Rosenthal, “Inherited vs self-made wealth: Theory & evidence from a rentier society (Paris 1872-1927),” *Explorations in Economic History,* 51, 21–40, 2014.

Piketty, T. and G. Zucman, “Wealth and inheritance in the long run,” in, trans., *Handbook of Income Distribution*, 1st ed., vol. 2, 1303–1368, Elsevier B.V., 2015.

Ramos, X. and D. Van de gaer, “Approaches To Inequality of Opportunity: Principles, Measures and Evidence,” *Journal of Economic Surveys,* 30, 855–883, 2016.

———, “Is Inequality of Opportunity Robust to the Measurement Approach?,” *Review of Income and Wealth,* 0, 1–19, 2020.

Rawls, J., *A theory of justice*, Harvard University Press, Cambridge, 1971.

Roemer, J.E., “A Pragmatic Theory of Responsibility for the Egalitarian Planner,” *Public Affairs,* 22, 146–166, 1993.

———, *Equality of Opportunity*, Harvard University Press, Cambridge, 1998.

Roemer, J.E. and A. Trannoy, “Equality of Opportunity: Theory and Measurement,” *Journal of Economic Literature,* 54, 1288–1332, 2016.

Saez, E. and G. Zucman, “Wealth Inequality in the United States since 1913: Evidence from Capitalized Income Tax Data,” *Quarterly Journal of Economics,* 131, 519–578, 2016.

Sen, A., “Equality of what?,” in S. McMurrin, ed., *Tanner Lectures on Human Values*, vol. 1, 195–220, Cambridge University Press, Cambridge, 1980.

Smith, A., *The Theory of Moral Sentiments*, A. Millar, London, 1759.

———, *An Inquiry into the Nature and Causes of the Wealth of Nations*, W. Strahan, London, 1776.

Starmans, C., M. Sheskin, and P. Bloom, “Why people prefer unequal societies,” *Nature Human Behaviour,* 1, 1–7, 2017.

Suárez Álvarez, A. and A.J. López Menéndez, “Assessing Changes Over Time in Inequality of Opportunity: The Case of Spain,” *Social Indicators Research,* 139, 989–1014, 2017.

Teyssier, G., “Inequality of Opportunity: New Measurement Methodology and Impact on Growth”, 2017.

Valle-Inclán, H. del, “Individual Characteristics Relate to Individual Outcomes? The Importance of the Income Aggregate when Measuring Inequality of Opportunity”, 2020.

Van de gaer, D., “Equality of Opportunity and Investment in Human Capital”, PhD thesis, 1993.

Von Gaudecker, H.M., “How Does Household Portfolio Diversification Vary with Financial Literacy and Financial Advice?,” *Journal of Finance,* 70, 489–507, 2015.

Young, M., *The Rise of the Meritocracy, 1870–2033*, Thames and Hudson, London, 1958.