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# IS INEQUALITY OF OPPORTUNITY ROBUST TO THE MEASUREMENT APPROACH?

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Recent literature has suggested many ways of measuring equality of opportunity. We analyze in a systematic manner the various approaches put forth in the literature to show whether and to what extent different choices matter empirically. Drawing on data for most European countries for 2005 and 2011, we find that the choice between ex-ante and ex-post approaches is crucial and has a substantial influence on inequality of opportunity country orderings. Growth regressions also illustrate the potential relevance of conceptual choices.

**JEL Codes**: D3, D63

**Keywords**: direct approach, effort, equality of opportunity, EU-SILC, ex-ante, ex-post, income, indirect approach, measurement, responsibility

## 1. Introduction

Responsibility-sensitive egalitarianism shifts the focus from outcomes to their determinants, when assessing economic inequalities, and advocates offsetting the effect of circumstances, for which individuals are not deemed responsible, while respecting the effects of effort. Since the first contributions by Dworkin (1981), Arneson (1989), and Cohen (1990), the economics literature has laid out the basic principles that ought to guide measurement, following seminal contributions by Roemer (1993, 1998), Fleurbaey (1995) and Bossert (1995) on allocation rules and policy. In a recent paper (Ramos and Van de Gaer, 2016), we bring together the theoretical and the empirical literature and draw attention to the conceptual differences of the empirical measures. This paper takes those lessons as starting point with the intention to investigate whether those important conceptual differences have any bearing in ordering distributions when taken to the data, and bring about systematic differences in orderings. To this end, we estimate a wide range of inequality of opportunity measures to the same set of data, the European

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Union–Statistics on Income and Living Conditions (EU-SILC), an empirical exercise which has not been done so far.[1]

Conceptually, the most frequently used measures of inequality of opportunity can be classified on the basis of three criteria. The first criterion distinguishes between ex-ante and ex-post measures. Ex-ante measures compute the inequality in the values of individuals' opportunity sets while ex-post measures compute the inequality in the incomes of those that have the same efforts. Initially, the theoretical literature treated ex-ante and ex-post approaches as being very similar (Roemer, 2002; Roemer *et al.*, 2003). Recent theoretical contributions stress they are different and often conflict (Ooghe *et al.*, 2007; Roemer, 2012; Fleurbaey and Peragine, 2013). Most of the empirical literature continues to treat them as interchangeable, by motivating their concern with inequality of opportunity from ex-post intuitions and using ex-ante measures of inequality of opportunity. We find that the distinction between ex-ante versus ex-post matters a lot for country orderings. The second criterion, due to Pistolesi (2009), distinguishes between direct and indirect measures. Direct measures calculate the inequality in a counterfactual income distribution where all income inequalities are exclusively due to individuals' circumstances. Indirect measures calculate the difference between the inequality in the actual income distribution and the inequality in a counterfactual income distribution in which there is no inequality of opportunity. Our results suggest that the distinction between direct and indirect measures is of secondary importance. The third criterion focuses on whether a parametric or non-parametric method is used to construct the counterfactual. This choice is relevant when the often-used parsimonious linear specification does not yield a reasonable fit, and it is thus data-dependent.

In the next Section we provide a more detailed description of these criteria, present and formally define the most frequently used measures of inequality of opportunity and classify them. Section 3 describes the EU-SILC data and the circumstances and effort variables used in the empirical analysis, Section 4 presents our empirical strategy, while Section 5 reports our main results. We first examine the incidence of choices on country orderings, and then show estimates from growth regressions to illustrate further their empirical relevance. The concluding section wraps up.

## 2. Measurement Approaches

As responsibility-sensitive egalitarianism distinguishes between efforts and circumstances, the empirical model assumes that for each individual $k$ in the population $N = \{1, \cdots, n\}$, his income, $y_k$, depends on his circumstances, given by a $d^C$ -dimensional vector $a_k^C$, his efforts, given by a $d^R$-dimensional vector $a_k^R$, and a random term $e_k$, such that[2]

[1]Previous papers provide partial (not systematic) comparisons, which do not allow drawing robust conclusions about the importance of conceptual choices. For instance, drawing on the same EU-SILC data, Checchi *et al.* (2016) compare non-parametric ex-ante $I^{c1}$ and ex-post $I^{c4}$ measures, defined in Table 1, using the same two inequality indices we employ, the Gini coefficient and the Mean Log Deviation.

[2]We discussed the consequences of unobserved random variation in Ramos and Van de Gaer (2016), and abstract from that complication here.

$$y_k = g\left(a_k^C, a_k^R, e_k\right) \quad \text{where} \quad g: \mathbb{R}^{d^C} \times \mathbb{R}^{d^R} \times \mathbb{R} \rightarrow \mathbb{R}_{++}.$$

Following Roemer (1993) and Peragine (2004) a type (tranche) is a set of people having the same circumstances (efforts). Measures of inequality of opportunity can be classified on the basis of three criteria: whether they take an ex-ante or ex-post perspective, whether they are direct or indirect measures of inequality of opportunity, and whether the estimation is based on a parametric or non-parametric method.

A first distinction is between ex-ante and ex-post approaches. The ex-ante approach measures the inequality between individuals' opportunity sets, and assumes that these opportunity sets are determined by individuals' circumstances. It attaches the same value to the opportunity set of those that belong to the same type, and measures the inequality in the values of individuals' opportunity sets. The ex-post approach measures the inequality in the incomes of individuals that have the same effort. All inequalities between such individuals must be due to their circumstances, and is, for that reason a measure of inequality of opportunity.[3]

A second distinction is between direct and indirect measures. Direct measures of inequality of opportunity compute the inequality in a $n$-dimensional counterfactual income distribution $y^c$ in which all inequalities due to differences in effort have been eliminated such that only the inequality that is due to differences in circumstances is left:

$$I\left(y^c\right), \tag{1}$$

where $I: \mathbb{R}^n_{++} \rightarrow \mathbb{R}$ is a measure of inequality. Indirect measures of inequality of opportunity compare the inequality in the actual distribution of income, $I(y)$, to the inequality in a counterfactual income distribution where there is no inequality of opportunity $I\left(y^{E0}\right)$. This results in the measure

$$\Theta_I\left(y, y^{EO}\right) = I(y) - I\left(y^{EO}\right), \tag{2}$$

where $\Theta_I\left(y, y^{EO}\right): \mathbb{R}^n_{++} \times \mathbb{R}^n_{++} \rightarrow \mathbb{R}$. Based on a decomposition argument, the idea behind the approach is that the difference between the inequality in the actual distribution and the inequality in the counterfactual income distribution without inequality of opportunity gives the inequality that is due to inequality of opportunity.

A third distinction is based on the estimation method. This method can be parametric or non-parametric. The parametric approach imposes a functional form to estimate individuals' incomes as a function of efforts or circumstances, resulting in specifications with 3 possible domains:

$$\hat{g}\left(a_k^C, a_k^R, e_k\right) \quad \text{where} \quad \hat{g}: \mathbb{R}^{d^C} \times \mathbb{R}^{d^R} \times \mathbb{R} \rightarrow \mathbb{R}_{++},$$

[3]In parametric ex-post approaches the random term is typically set equal to zero in the construction of the counterfactual, such that variation in the counterfactual is due to differences in efforts. A notable exception is Björklund *et al.* (2012) who use the error term to parametrically identify non-observable effort and decompose the parametric error term into two terms: a first one that captures the indirect effect of circumstances (through effort) and a second one that captures effort, net of circumstances. In non-parametric approaches random terms are not usually taken into account, but the averaging procedures make them disappear, at least asymptotically.

$$\widehat{g}^{C}\left(a_k^C, e_k\right) \quad \text{where} \quad \widehat{g}^{C}:\mathbb{R}^{d^C}\times\mathbb{R}\rightarrow\mathbb{R}_{++},$$

$$\widehat{g}^{R}\left(a_k^R, e_k\right) \quad \text{where} \quad \widehat{g}^{R}:\mathbb{R}^{d^R}\times\mathbb{R}\rightarrow\mathbb{R}_{++}.$$

These equations can be used to estimate $y_k$ by setting $e_k$ equal to zero.

Table 1 uses the three distinctions to classify standard measures used in the literature.

Panel A describes direct and indirect measures based on conterfactuals. Consider the direct measures first. Four ways to measure the value of an individual's opportunity set are proposed. Counterfactual $y^{c1}$, proposed by Van de Gaer (1993), measures the value of an individual's opportunity set by the average income of his type; $y^{c2}$, proposed by Lefranc *et al.* (2008) measures it by the normalized surface under the generalised Lorenz curve of his type; $y^{c3}$, proposed by Ferreira and Gignoux (2011) takes the parametric estimate of his income, given his circumstances. Counterfactual $y^{c4}$, proposed by Pistolesi (2009), relies on the choice of $\overline{a}^R$, a reference value for the vector of responsibility characteristics and takes the parametric estimate of an individual's income, given his circumstances and with efforts equal to the reference values. Fleurbaey and Schokkaert (2009)

TABLE 1
Measures of inequality of opportunity

| | Non-parametric | Parametric |
|---|---|---|
| *Panel A: Counterfactuals in measures based on counterfactuals* | | |
| (a) Direct $I(y^c)$ | | |
| Ex-ante[a] | $y_k^{c1} = \frac{1}{\lvert N_{k.}\rvert}\sum_{i\in N_{k.}} y_i$ | $y_k^{c3} = \widehat{g}^{C}\left(a_k^C, 0\right)$ |
| | $y_k^{c2} = \frac{2}{\lvert N_{k.}\rvert\lvert N_{k.}+1\rvert}\sum_{i\in N_{k.}} i\widetilde{y}_i$ | $y_k^{c4}\left(\overline{a}^R\right) = \widehat{g}\left(a_k^C, \overline{a}^R, 0\right)$ |
| Ex-post[a] | $y_k^{c5} = y_k\frac{\mu(y)}{y_k^{EO1}}$ | |
| (b) Indirect $\Theta_I\left(y, y^{EO}\right) = I(y) - I\left(y^{EO}\right)$ | | |
| Ex-ante[a] | $y_k^{EO5} = y_k\frac{\mu(y)}{y_k^{c1}}$ | |
| Ex-post[b] | $y_k^{EO1} = \frac{1}{\lvert N_{.k}\rvert}\sum_{i\in N_{.k}} y_i$ | $y_k^{EO3} = \widehat{g}^{R}\left(a_k^R, 0\right)$ |
| | $y_k^{EO2} = \frac{2}{\lvert N_{.k}\rvert\lvert N_{.k}+1\rvert}\sum_{i\in N_{.k}} i\check{y}_i$ | $y_k^{EO4}\left(\overline{a}^C\right) = \widehat{g}\left(\overline{a}^C, a_k^R, 0\right)$ |
| *Panel B: Alternative inequality of opportunity measures* | | |
| Ex-post, direct, non parametric[c] | $I^P = \frac{1}{n}n^t\sum_{t=1}^{T} I^t(y^t)$ | |
| Norm based, direct, parametric[d] | $I^N = I(y - y^N)$ | |

*Note*: $N_{k.} = \left\{i\in N \mid a_i^C = a_k^C\right\}$, $\widetilde{y}_i$ is the $i$−th largest level of income in the set $N_{k.}$, $\overline{a}^R$ is a reference value for the vector of responsibility variables.

[a] $\mu(y)$ is mean income of vector $y$.

[b] $N_{.k} = \left\{i\in N \mid a_i^R = a_k^R\right\}$, $\check{y}_i$ is the $i$−th largest level of income in the set $N_{.k}$, $\overline{a}^C$ is a reference value for the vector of circumstance variables.

[c] There are $T$ tranches; tranche $t\in\{1,2,\ldots,T\}$, $n^t$ is the number of individuals in tranche $t$ and $y^t$ is the $n^t$- dimensional vector with the incomes of all individuals that belong to tranche $t$. $I(y^t)$ measures the inequality in this vector.

[d] $y^N$ is the $n$-dimensional vector of norm incomes. An individual's claim is the average income he would receive if everyone had his responsibility vector. His norm income is total income multiplied by his claim divided by the sum of everyone's claims (the "generalized proportionality principle").

call this measure "direct unfairness". The counterfactual for ex-post measure $y^{c5}$, proposed by Checchi and Peragine (2010), scales everybody's income up or down by the ratio of the average income in the population and the average income of his tranche. That way, since we use a relative inequality measure, the inequalities between those that belong to the same tranche are preserved, while the differences in average incomes of different tranches are eliminated.

The counterfactuals used in the indirect approach are obtained by switching the role of circumstance and effort variables of the direct approach. This dual relationship is reflected in the number used to label the counterfactuals: for all $i = 1, \ldots, 5$, $y^{EOi}$ is the dual counterfactual of $y^{ci}$. Checchi and Peragine (2010) proposed counterfactual $y^{EO1}$, which assigns to every individual the average income of his tranche; $y^{EO2}$ assigns the value of the normalized surface under the generalised Lorenz curve of the income distribution of his tranche; $y^{EO3}$ the parametric estimate of his income, given his efforts; $y^{EO4}$ the parametric estimate of his income, given his efforts and circumstances equal to the reference values. In all these counterfactuals, those with the same efforts have the same income, such that the corresponding indirect measure becomes a measure of the income inequality that is due to their different circumstance; they are ex-post measures of inequality of opportunity. The only ex-ante measure is $y^{EO5}$, proposed by Checchi and Peragine (2010), where incomes are scaled up or down by the ratio of the average income in the population and the value of the opportunity set measured by $y_k^{c1}$, such that in this counterfactual, the average income of every type equals average income in the population and everyone's opportunity set has the same value. Observe that the duals of direct ex-ante measures are indirect ex-post measures, while the dual of the direct ex-post measure is an indirect ex-ante measure. In the sequel $I^X$ denotes the inequality measure based on counterfactual $y^X$ with $X \in \{c1, \ldots, c5, EO1, \ldots, EO5\}$.

Panel B presents alternative measures. The ex-post measure $I^P$, due to Rodríguez (2008), is a weighted average of the inequalities within each tranche. The norm based measure, proposed by Almås *et al.* (2011), measures the inequalities in the deviations between individuals' actual incomes and their norm income (or fair income). In the norm income distribution all inequalities due to circumstances are eliminated. This is similar to the "unfairness gap" measure proposed by Fleurbaey and Schokkaert (2009).

## 3. Data

We draw on data from the European Union–Statistics on Income and Living Conditions, which collects comparable information on socioeconomic and demographic characteristics of individuals across European countries. In particular we use the 2005 and 2011 waves, which collected information on family background and circumstances when the respondent was young in separate questionnaire modules. EU-SILC data have been commonly used to study equality of opportunity across European countries, see *inter alia* Marrero and Rodríguez (2012) and Checchi *et al.* (2016).

The EU-SILC provides data for a large number of countries, which allows us to compare country orderings by inequality of opportunity when different measures are used. The main limitation of EU-SILC is the reduced sample sizes for some countries, which obliges us to work with a reduced number of circumstances and efforts.

As in previous studies, e.g. Marrero and Rodríguez (2012), we select individuals aged 25 to 59 to avoid the noise associated to the school-job transition for the younger population and to retirement decisions for the older individuals.

Our outcome of interest is individual disposable equivalent income.[4] To check the reliability of our income variable, we compare our Gini coefficient estimates with other estimates coming from different sources, such as the OECD, and obtain correlation coefficients above 0.9, indicating that our estimates are in line with those from the OECD.

Working with a limited amount of circumstances and effort variables, and thus types and tranches, results in partitions that are too coarse and that end up driving the estimates of the various direct and indirect measures. Because of this, we use a set of five circumstance and four effort variables, which translate into 48 types and 24 tranches, thus achieving finer partitions than the majority of recent empirical studies on equality of opportunity.[5] Our circumstances include parental education and occupation, gender, birthplace, and whether the respondent lived with both parents when young, while the set of efforts includes own educational attainment, own occupation, work status, and marital status. All variables have two categories, except the two occupation variables, which have 3 categories each. Description and summary statistics of circumstance and effort variables can be found in Appendix Tables 12 to 15.

Many papers identify effort by means of Roemer's Identification Assumption (RIA), that is, by using the relative position of the individual – often aggregated into deciles—in the type conditional distribution of the outcome variable.[6] As Ramos and Van de Gaer (2016) argue, this strategy is based on two strong assumptions and it is also likely to misidentify effort if relevant circumstances are omitted. However, we do not follow this strategy to identify effort because we include in our analysis inequality of opportunity measures that are based on parametric counterfactuals, such as $y^{c4}$ and $y^{EO4}$, which require observable effort variables. Furthermore, in our analysis we examine the empirical relevance of including effort variables in the econometric specification to estimate the counterfactual distribution of parametric ex-antes

[4]Disposable equivalent individual income is computed by deflating disposable equivalent household income (variable HY020, including the sum for all household members of gross personal income components plus gross income components at household level, minus taxes paid), with the modified OECD equivalent scale, (variable HX050, which assigns a weight of 1 to the first adult, of 0.5 to remaining adults of the family, and of 0.3 to children younger than 14).

[5]The downside of using such a large number of partitions is that our estimates may be vulnerable to upward biases of a size that (negatively) depends on the sample size and on the particular measure estimated (Brunori et al., 2018). However, the following exercise suggests that the concerns about such bias may be limited. For each country and year, $y^{c3}$ and $y^{c4}$ are estimated on the same sample but using different specifications—a similar argument applies to $y^{EO3}$ and $y^{EO4}$. In particular, $y^{c3}$ relies on $\hat{g}^C$, i.e. a specification that considers only circumstance variables, while $y^{c4}$ relies on $(\hat{g})$, which adds effort variables to the previous specification, and thus has lower degrees of freedom. Since $y^{c4}$ does not include additional circumstance variables in the specification, but only incorporates additional effort variables, we expect the possible downward bias due to omitted (or too coarsely defined) circumstances to change very little between $y^{c3}$ and $y^{c4}$. In contrast, the decrease in degrees of freedom that results from adding new variables in the specification should increase the upward bias of $y^{c4}$, especially for countries and years with small sample sizes. Therefore, we expect a negative correlation between sample size and the difference between $y^{c4}$ and $y^{c3}$. Instead we find correlations that are not statistically significant.

[6]Some relevant and influential papers however use observable effort variables instead, e.g. Bourguignon *et al*. (2007), Pistolesi (2009) and Almås *et al*. (2011).

measures—i.e. we examine whether the predictions from specifications $\hat{g}^C$ and $(\hat{g})$ lead to ordering changes between $I^{c3}$ and $I^{c4}$. This of course implies that we are working with effort levels instead of degree of effort, which results from using RIA.

The circumstance variable indicating the presence of both parents at home when the respondent was young is not typically used, and thus deserves some justification.[7] Growing up in non-intact families is found to condition several later-life outcomes, and earnings during adulthood is one of them (Mohanty and Ullah, 2012; Lopoo and DeLeire, 2014; Lerman *et al*., 2017).

Own education has been previously used as effort variable (e.g. Almås *et al*., 2011), but we believe deserves some discussion. Undoubtedly own effort affects educational attainment (De Fraja *et al*., 2010), which in turn determines wages and thus incomes. What may be a bit more controversial is the choice of own education as an effort variable, as some may argue that children cannot be deemed responsible for their own effort before the age of consent, and such effort levels are also determining later educational attainment after the age of consent. As discussed above, however, since there are only few variables in the EU-SILC that can be used as effort variables and most of them provide only very small cell sizes, we decided to use own educational attainment as an effort variable.

Marital status is typically omitted from the analysis. Thus, depending on how error terms are treated, marital status is considered either a circumstance or an effort variable by previous empirical studies.[8] We believe that marital status is largely a choice, and thus include it in the effort set. Notice that our choice is only weakly related to deciding how to treat the spouse's circumstances and effort variables—see Peichl and Ungerer (2016) for an interesting discussion about the various ways spouse's characteristics can be handled.

The other variables included in the set of circumstances or effort are not new and rather uncontroversial, and do not deserve further discussion.[9]

## 4. Empirical strategy

To test whether the main conceptual issues discussed in Section 2 matter in practice, we first estimate all twelve inequality of opportunity measures, and then compute Spearman's rank correlations between all pair of measures to gain a first insight about what measures seem closer to each other in the sense of delivering similar inequality of opportunity orderings across countries.

When using the parametric method we fit the same functional form for all countries and years. In particular, we use a product separable specification that includes the same circumstance and effort variables, defined the same way, for all countries and years. Arguably, since the true data generating process may differ across countries and years, imposing the same product separable functional form,

[7]A notable exception is Björklund *et al*. (2012).

[8]In parametric ex-post approaches, for instance, if the random term is set equal to a constant in the construction of the counterfactual, marital status will be considered as a circumstance. However, in a parametric ex-ante measure setting the random term to a constant entails regarding marital status as an effort.

[9]See Table 25.8 in Ferreira and Peragine (2016) for a list of circumstance variables used in eight papers that cover 41 countries. Roemer and Trannoy (2015) discuss important issues in the use of effort variables often included in empirical analyses.

where circumstances and efforts do not interact to determine the outcome variable, with exactly the same circumstance and effort variables, may bias the estimated counterfactual distributions in ways that depend on the country and year, and also on the inequality of opportunity measure. This bias is the net effect of two sources of bias: a possible upward bias, which results from small sample sizes, and a possible downward bias stemming from the poor explanatory power of models with few circumstances, efforts or interaction terms (Brunori *et al.*, 2018). Because of this, Brunori *et al.* (2018) suggest using specifications that minimize the Mean Squared Error. However, using a different model specification for each country and year would introduce a source of variability across countries and years, that would complicate the interpretation of our results. By fitting the same regression model to all countries and years we hold this constant.[10] Non-parametric procedures typically do not impose a functional form and rely on averaging procedures.

Following the common practice in empirical studies, we employ two inequality indices to estimate the inequality in the twelve counterfactual distributions: the Mean Log Deviation (MLD) and the Gini coefficient. Because of its path independence property, many empirical studies employ the MLD. However, the MLD is more sensitive to extreme values than the Gini coefficient and is not bounded from above (Brunori *et al.*, 2019). Thus, the MLD may underestimate inequality of opportunity when the counterfactual is based on a smoothed distribution, as in $y^{c1}$ and $y^{EO1}$. Furthermore and importantly, the MLD cannot deal with negative values, which may be present in the gap distribution $(y-y^N)$ used in the norm-based measure, $I^N$. Therefore, we employ the Gini coefficient in our baseline estimates reported below, and present the robustness of our findings to using the MLD in Section 5.1.[11]

The inspection of rank correlations in Table 2 suggest that inequality of opportunity measures could be grouped into three sets. A first set would include only ex-ante measures, $(I^{c1}, I^{c2}, I^{c3}, I^{EO5})$, a second set would include only ex-post measures $(I^{EO1}, I^{EO2}, I^{EO3}, I^{EO4})$, while a third set would include the norm-based measure, $I^N$, and the ex-post measure $I^P$ that show very weak correlations with the measures included in the previous two sets. Compared to the strong correlations between the measures included in the first two sets, the ex-ante measure $I^{c4}$ displays weaker correlations with all other measures. However, since it correlates more strongly with the other ex-ante measures than with the ex-post measures, this measure could also be included in the first set with the other ex-ante measures. Finally, the ex-post measure $I^{c5}$ also correlates mildly with the ex-ante measures and shows slightly higher correlations with the ex-post measures included in the second set. The above pattern of rank correlations also holds for 2011 and when the MLD is used—see Appendix Tables 5–7.[12]

[10]A systematic and formal analysis about how different empirical functional forms bias the different measures of inequality of opportunity when the true data generating process differs, would require Monte-Carlo simulations with a purposely-made dataset. This exercise is beyond the scope of this paper, and we leave it for further research.

[11]The norm-based measure is of course excluded from the analysis when using the MLD. Inequality of opportunity estimates are available from the authors upon request.

[12]Section 5.1 discusses in detail the few changes in rank correlations that emerge when the MLD is used instead of the Gini coefficient.

TABLE 2
Rank correlations between inequality of opportunity measures. Gini coefficient, 2005

| | Direct measures | | | | | | Indirect Measures | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | EA | | | | | | EP | | | | EA |
| | $I^{c1}$ | $I^{c2}$ | $I^{c3}$ | $I^{c4}$ | $I^{c5}$ | $I^{P}$ | $I^{EO1}$ | $I^{EO2}$ | $I^{EO3}$ | $I^{EO4}$ | $I^{EO5}$ |
| $I^{c2}$ | 0.957 | | | | | | | | | | |
| | 0.000 | | | | | | | | | | |
| $I^{c3}$ | 0.866 | 0.905 | | | | | | | | | |
| | 0.000 | 0.000 | | | | | | | | | |
| $I^{c4}$ | 0.671 | 0.777 | 0.726 | | | | | | | | |
| | 0.001 | 0.000 | 0.000 | | | | | | | | |
| $I^{c5}$ | 0.661 | 0.710 | 0.703 | 0.710 | | | | | | | |
| | 0.001 | 0.000 | 0.000 | 0.000 | | | | | | | |
| $I^{P}$ | −0.042 | −0.147 | −0.110 | −.0248 | −0.003 | | | | | | |
| | 0.858 | 0.526 | 0.634 | 0.278 | 0.991 | | | | | | |
| $I^{EO1}$ | 0.408 | 0.414 | 0.310 | 0.512 | 0.799 | 0.061 | | | | | |
| | 0.066 | 0.062 | 0.171 | 0.018 | 0.000 | 0.723 | | | | | |
| $I^{EO2}$ | 0.227 | 0.262 | 0.227 | 0.439 | 0.773 | 0.118 | 0.917 | | | | |
| | 0.322 | 0.251 | 0.322 | 0.047 | 0.000 | 0.610 | 0.000 | | | | |
| $I^{EO3}$ | 0.312 | 0.308 | 0.236 | 0.370 | 0.740 | 0.126 | 0.942 | 0.935 | | | |
| | 0.169 | 0.175 | 0.302 | 0.097 | 0.000 | 0.584 | 0.000 | 0.000 | | | |
| $I^{EO4}$ | 0.414 | 0.416 | 0.344 | 0.431 | 0.758 | 0.062 | 0.920 | 0.884 | 0.971 | | |
| | 0.062 | 0.061 | 0.127 | 0.051 | 0.000 | 0.788 | 0.000 | 0.000 | 0.000 | | |
| $I^{EO5}$ | 0.948 | 0.901 | 0.779 | 0.573 | 0.495 | 0.068 | 0.257 | 0.113 | 0.207 | 0.322 | |
| | 0.000 | 0.000 | 0.000 | 0.007 | 0.023 | 0.771 | 0.261 | 0.626 | 0.369 | 0.155 | |
| $I^{N}$ | 0.087 | −0.021 | 0.042 | −0.210 | 0.243 | 0.204 | 0.229 | 0.395 | 0.334 | 0.296 | 0.117 |
| | 0.708 | 0.929 | 0.858 | 0.360 | 0.289 | 0.375 | 0.319 | 0.077 | 0.139 | 0.193 | 0.614 |

*Notes*: Rank correlations are shown in the upper row, while $p$-values are shown in the lower row. EA means Ex-ante, EP means Ex-post.

TABLE 3
Three Sets of Measures that Minimize the Average Spearman Distance. Gini coefficient, 2005 and 2011

| % | Set 1 | Set 2 | Set 3 |
|---|---|---|---|
| *2005* | | | |
| 97 | $I^{c1}$, $I^{c2}$, $I^{c3}$, $I^{c4}$, $I^{EO5}$ | $I^{EO1}$, $I^{EO2}$, $I^{EO3}$, $I^{EO4}$ | $I^{c5}$, $I^{P}$, $I^{N}$ |
| 3 | $I^{c1}$, $I^{c2}$, $I^{c3}$, $I^{c4}$ | $I^{EO1}$, $I^{EO2}$, $I^{EO3}$, $I^{EO4}$, $I^{c5}$ | $I^{EO5}$, $I^{P}$, $I^{N}$ |
| *2011* | | | |
| 98 | $I^{c1}$, $I^{c2}$, $I^{c3}$, $I^{c4}$, $I^{EO5}$ | $I^{EO1}$, $I^{EO2}$, $I^{EO3}$, $I^{EO4}$ | $I^{c5}$, $I^{P}$, $I^{N}$ |
| 2 | $I^{c1}$, $I^{c2}$, $I^{c3}$, $I^{c4}$ | $I^{EO1}$, $I^{EO2}$, $I^{EO3}$, $I^{EO4}$, $I^{c5}$ | $I^{EO5}$, $I^{P}$, $I^{N}$ |

In the light of this suggestive evidence, we devise and implement an algorithm to find out the way of grouping the 12 inequality of opportunity measures in three sets that minimizes a weighted average distance between all pairs of orderings that belong to a given set. The algorithm follows the following steps:

1. Bootstrap each one of the 12 measures for all countries and compute the ensuing orderings. We run 100 iterations.
2. Since the Spearman's rank correlation matrix suggests 3 groups, find all the different ways of grouping the 12 measures into 3 sets, when the order of the sets does not matter. There are 12 such partitions.
3. For each and every one of the 12 different partitions, e.g. the partition where the first set contains 10 measures, while the second and third sets contain one measure each, define all possible compositions, i.e. ways of allocating our 12 measures into these 3 sets, when the order of the measures within each set does not matter.
4. For each and every composition and for all possible partitions compute the weighted average distance between all orderings, as follows:
   - first, calculate the Spearman distance between all pairs of orderings that belong to a given set, and then
   - compute the weighted average of Spearman distances across the 3 sets using "population shares" as weights, (i.e. relative frequency of distances in each set).
5. Finally, choose the composition that minimizes the weighted average distance.
6. Do steps 4 and 5 for each one of the 100 iterations.

We draw our conclusions from the frequencies of compositions that minimize the weighted average distance, displayed in Table 3.

## 5. Results

Table 3 shows the composition of the three sets that minimizes the weighted average of Spearman distances for the 100 replications when we use the Gini coefficient to measure inequality in the twelve counterfactual distributions described in Section 2, for the two years of EU-SILC data available. The results are consistent with the rank correlations estimates reported in Table 2, and show that some conceptual and theoretical differences outlined in Section 2 matter in practice. In particular, one of the main lessons to be drawn is that the difference between ex-ante and ex-post measures comes out as an important empirical divide.[13] Looking at the composition that gathers the largest frequency, the first set includes all ex-ante measures, while the second set includes only ex-post measures. The two measures that showed the lowest rank correlations—i.e. the norm-based measure, $I^N$, and the ex-post measure $I^P$ are included in the third set. Surprisingly, the ex-post measure $I^{c5}$ is also included in the third set. This shows that the composition of each of the three sets that minimizes the weighted average of Spearman distances cannot be inferred in a trivial way from the estimated rank correlations. This conclusion is statistically robust, as the composition of the three sets that minimizes the weighted average of Spearman distances is the same in nearly all 100 replications.[14]

The direct and indirect approaches do not seem to shape the estimated rank correlations as much as the ex-ante/ex-post divide. The rank correlations of Table 2 provide preliminary evidence of this. While rank correlations amongst direct measures based on counterfactuals are reasonably high (>.66), direct ex-ante measures $I^{c1}$, $I^{c2}$, and $I^{c3}$ show higher correlations with the indirect ex-ante measure $I^{EO5}$ than with the direct ex-post measure $I^{c5}$.[15] Indirect measures show a similar pattern, as the indirect ex-ante measure $I^{EO5}$ shows a higher correlation with direct ex-ante measures than with indirect ex-post measures $I^{EO1}$, $I^{EO2}$, $I^{EO3}$, $I^{EO4}$. Likewise, indirect ex-post measures show higher correlations with direct ex-post measures than with the indirect ex-ante measure $I^{EO5}$. That is, conditional on the ex-ante or ex-post approach, measures yield closer rankings within direct and within indirect methods than across them. This insight is corroborated by the "optimal" composition of the three sets shown in Table 3, as all ex-ante measures are grouped together in the same set regardless of them being direct or indirect, while neither all direct nor all indirect measures are.

We turn next to the third distinction between measures based on *parametric and non-parametric counterfactuals*. Non-parametric counterfactuals are equivalent to those obtained from fully saturated parametric models that include all possible interaction effects, when all variables in the model are categorical variables—which is the standard practice in the literature. Most of the literature, however, uses linear specifications for the parametric approach. In the light of this, the relevant question is: To what extent does the importance of interaction effects differ enough across countries as to change the country orderings? Our findings suggest that interaction effects are not so relevant in determining country orderings, as non-parametric and analogous or similar linear parametric approaches yield similar orderings. To start with, the composition that minimizes the weighted average of Spearman distances between country orderings groups ex-ante direct

[13]Results presented in this section are robust to dropping Cyprus from the analysis, as it showed an unreasonable increase in income inequality, according both to the Gini (>20% increase from 2005 to 2011) and to the MLD (>45% increase).

[14]Another way of showing this robustness, is noticing that the difference between the weighted average of Spearman distances of this composition and that of the composition that yields the second lowest weighted average of Spearman distances is statistically significant.

[15]Given the distinct behaviour of the direct ex-post measure $I^P$, we do not include it in our analysis here.

non-parametric $I^{c1}$ and linear parametric $I^{c3}$ in the same set—see Table 3. Likewise, indirect ex-post non-parametric $I^{EO1}$ and its parametric counterpart $I^{EO3}$ are also included in the same set. Rank correlations from Table 2 convey the same message: the rank correlation between $I^{c1}$ and $I^{c3}$ is 0.87, while the rank correlation between $I^{EO1}$ and $I^{EO3}$ is also large (0.94). These high correlations are also found for 2011 and for the MLD. Our findings are in line with Brunori et al., 2018. They use the same EU-SILC dataset for 2011 and the MLD to compare a parsimonious linear specification with another specification that includes all possible interaction terms and where categorical variables are partitioned more finely, and also obtain a strong rank correlation of 0.80 between direct measures $I^{c1}$ and $I^{c3}$.[16]

As we explained above, parametric approaches estimate counterfactuals in two ways: either by using $\hat{g}^C$ ($\hat{g}^R$) in the direct (indirect) approach—i.e. including in the regression only the set of circumstances (efforts)—, or by using the functional form ($\hat{g}$), i.e. including both circumstances and efforts in the specification. The latter allows for a more flexible treatment of the correlation between circumstances and efforts and is thus likely to yield different parameter estimates and counterfactual distributions. To see whether and to what extent including both circumstances and efforts matters empirically we compare the orderings stemming from the indirect ex-post measures $I^{EO3}$ and $I^{EO4}$, on the one hand, and the orderings stemming from the direct ex-ante measures $I^{c3}$ and $I^{c4}$, on the other hand. Both the evidence coming from the more 'global' empirical strategy of finding the composition of the three sets that minimizes the weighted average of Spearman distances and from the more 'local' empirical strategy based on rank correlations consistently suggest that conditioning on circumstances in the indirect ex-post approach or on effort in the direct ex-ante approach is not that relevant. As Table 3 shows, the indirect ex-post measures $I^{EO3}$ and $I^{EO4}$ are grouped in the same set, and the direct ex-ante measures $I^{c3}$ and $I^{c4}$ are also included in the same set. Furthermore, the rank correlation between the relevant measures is also strong: 0.97 between the two indirect ex-post measures and 0.73 between the two direct ex-ante measures.[17]

*Dual counterfactuals* provide a somewhat natural way of making conceptual analogies between views and approaches. The data reveal that they lead to country orderings that are quite different—rank correlation coefficients amongst dual counterfactuals range from 0.24 to 0.49. Moreover, in our grouping of measures on the basis of the weighted average of Spearman distances, it never happens that dual counterfactuals belong to the same group.

Finally we examine whether it matters allowing for *inequality aversion with respect to income differences due to differences in effort*. We compare the non-parametric measures, $I^{c1}$ and $I^{c2}$ for the direct approach, and $I^{EO1}$ and $I^{EO2}$ for the indirect approach. Our results suggest that allowing for inequality aversion does not matter neither for the direct nor for the indirect measures. Table 3 shows that

[16]The rank correlation is our own computation based on the results they present in Table 2.

[17]Some of the evidence differs when we use the MLD to estimate the direct ex-ante measures $I^{c3}$ and $I^{c4}$. As shown in Appendix Table 8, now $I^{c3}$ and $I^{c4}$ do not belong to the same set, as $I^{c4}$ is included in the third set together with $I^P$. This is consistent with the somewhat lower rank correlation (0.70) displayed for year 2005, but not so much for the stronger rank correlation (0.82) displayed for year 2011—see Appendix Tables 6 and 7.

$I^{c1}$ and $I^{c2}$ belong to the same set, and that $I^{EO1}$ and $I^{EO2}$ are also included in the same set. Furthermore, Table 2 shows that rank correlations are larger than 0.92.

### 5.1. *Robustness to using the MLD instead of the Gini*

The main conclusions obtained when using the Gini coefficient, reported in the previous section, are quite robust when we employ the MLD. Now, we cannot include the norm-based index $I^N$ in the analysis, as the distribution of $(y - y^N)$ has negative values. Appendix Table 8 shows that the composition of the three sets is very similar to the composition obtained with the Gini coefficient, as only two indices change sets—see Table 3. One of the differences is induced by the use of the MLD. Due to its path independence property (Foster and Sneyerov, 2000), $I^{EO1}$ and $I^{c5}$, as well as $I^{c1}$ and $I^{EO5}$, yield exactly the same ordering when using the MLD. As a result, now $I^{c5}$ is included in the same group as $I^{EO1}$. The second difference is that now, $I^{c4}$ goes to the third group together with $I^{c5}$. It is worth noting that once again, the obtained composition is statistically robust, as the composition of the three sets that minimizes the weighted average of Spearman distances is the same in all 100 replications.

The high cross-index correlations of Appendix Table 9 provide further evidence about the robustness of our findings to using MLD instead of the Gini coefficient. The correlations between direct ex-ante measures ($I^{c1}$, $I^{c2}$, $I^{c3}$, $I^{c4}$), on the one hand, and indirect ex-post measures ($I^{EO1}$, $I^{EO2}$, $I^{EO3}$, $I^{EO4}$), on the other, are substantially higher for the MLD than for the Gini. This may suggest that a decomposition argument saying that the difference in inequality in the actual distribution and the inequality that is due to efforts—which is what indirect measures capture—equals the inequality that is due to circumstances—which is what direct measures capture—makes more sense for the MLD than for the Gini coefficient—recall that unlike the MLD, the Gini coefficient does not perfectly decompose additively into a within and a between components, but there is a third component that depends on the degree of overlap of subgroup distributions.

### 5.2. *Inequality of opportunity and economic growth*

To illustrate further the empirical relevance of the different conceptual choices, this section explores the relationship between inequality of opportunity and economic growth, which has captured the attention of the recent literature.[18] It is important to note that given the many limitations that the EU-SILC imposes, this empirical exercise is solely illustrative. As outlined in Section 3, the EU-SILC collects data on family background and circumstances when the respondent was young at two points in time, 2005 and 2011. This means that we can only estimate inequality of opportunity for these two years. This data structure imposes two major limitations on our empirical exercise: First, our time series is very short, as we can only exploit variability at two points in time, and second, we can only study

[18]Exploring the correlation between the various measures of inequality of opportunity and other important socioeconomic or institutional indicators, as in Marrero and Rodríguez (2012) and Checchi *et al*. (2016), would be another way of illustrating the relevance of the different conceptual choices.

growth over a time period which is much shorter than the 5 or 10 year period, which is customary.

Recent literature has explored the idea that inequality due to efforts and inequality due to circumstances (inequality of opportunity) have opposite effects on economic growth, which in turn may help explain the inconclusive evidence of the effects of overall inequality on economic growth (see among others Marrero and Rodríguez, 2013; Ferreira *et al.*, 2018). While inequality of opportunity is argued to have a deleterious impact on economic growth, effort inequality is deemed to have an enhancing impact on growth. The empirical papers that test this hypothesis usually use only one of the many options outlined in Section 2 to estimate inequality of opportunity. However, as we have reported above, different opportunity inequality measures give rise to different country orderings. Do the findings reported in the literature crucially hinge on the specific inequality of opportunity measure used? To answer this question, this section runs growth regressions and checks whether results are robust to the way inequality of opportunity and inequality of effort are measured.

Following Forbes (2000), we estimate the following panel regression

$$g_{c,(t,t-s)} = \beta_1 IO_{c,t-s} + \beta_2 IE_{c,t-s} + \beta_3 GDP_{c,t-s} + \beta_4 Ed_{c,t-s} + \beta_5 Inv_{c,t-s} + \alpha_c + \tau_t + \epsilon_{ct},$$

where $g_{ct}$ is the average annual growth rate of per capita real GDP between $t$ and $t-s$, $IO_{c,\,t-s}$ is one of the inequality of opportunity measures outlined in Section 2, $IE_{c,\,t-s}$ is residual inequality, often assumed to be inequality of effort, and computed as the difference between outcome and opportunity inequality, $GDP_{c,\,t-s}$ is the per capita real Gross Domestic Product, $Ed_{c,\,t-s}$ is the population share with upper secondary education or above, $Inv_{c,\,t-s}$ is the business investment to GDP ratio, and $\alpha_c$ and $\tau_t$ capture country and time specific fixed effects. Control variables, namely GDP, education shares, and investment, come from Eurostat, and all regressors refer to the initial period over which growth is estimated to avoid simultaneity issues.

We estimate the model by fixed effects, which control for time-invariant omitted variables. It is a demanding estimation strategy, as we are identifying effects using within-country variation with only two time points, but nonetheless still suffers from endogeneity problems (Bond, 2002). Given the difficulty to find external instruments, system-GMM methods are usually employed to address such endogeneity issues (Bond *et al.*, 2001). These models, however, require three time points, while we only have two, which precludes us from taking due account of the possible endogeneity bias. If we however assume that the possible bias does not change across different measures of inequality of opportunity, it should not invalidate our comparative results, which is what we are mainly concerned with in this empirical exercise.

Table 4 reports the fixed effects estimates of outcome inequality and of the two variables of interest, $IO_{c,\,t-s}$ and $IE_{c,\,t-s}$ for one to three year average annual growth rates and the twelve measures of inequality of opportunity, measured with the Gini coefficient. Outcome inequality regressions simply replace the two inequality of opportunity and effort measures in the specification above with a measure

TABLE 4
GROWTH REGRESSIONS

| | | | | Direct Approach | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | $I$ | | $I^{c1}$ | | $I^{c2}$ | | $I^{c3}$ | | $I^{c4}$ | | $I^{c5}$ | | $I^{P}$ | |
| Period | Inequality | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value |
| ($t$, $t$−1) | Outcome | −0.12 | 0.98 | | | | | | | | | | | | |
| | Opportunity | | | 0.20 | 0.71 | −0.08 | 0.89 | 0.07 | 0.91 | 0.53 | 0.69 | −0.26 | 0.52 | −4.11 | 0.53 |
| | Effort | | | −0.28 | 0.65 | 0.01 | 0.97 | −0.03 | 0.94 | 0.01 | 0.99 | 1.45 | 0.11 | −0.48 | 0.31 |
| ($t$, $t$−2) | Outcome | −0.34 | 0.92 | | | | | | | | | | | | |
| | Opportunity | | | 0.20 | 0.67 | 0.10 | 0.85 | 0.06 | 0.92 | 0.57 | 0.62 | −0.30 | 0.38 | −7.99 | 0.21 |
| | Effort | | | −0.33 | 0.54 | −0.08 | 0.83 | −0.05 | 0.89 | −0.02 | 0.96 | 1.53* | 0.05 | −0.39 | 0.39 |
| ($t$, $t$−3) | Outcome | −0.12 | 0.72 | | | | | | | | | | | | |
| | Opportunity | | | 0.01 | 0.99 | 0.09 | 0.87 | −0.11 | 0.86 | 0.66 | 0.58 | −0.35 | 0.33 | −8.81 | 0.16 |
| | Effort | | | −0.30 | 0.59 | −0.14 | 0.71 | −0.13 | 0.72 | −0.11 | 0.76 | 1.20 | 0.13 | −0.50 | 0.26 |
| | | | | Indirect Approach | | | | | | | | | | Norm-Based | |
| | | | | $I^{EO1}$ | | $I^{EO2}$ | | $I^{EO3}$ | | $I^{EO4}$ | | $I^{EO5}$ | | $I^{N}$ | |
| | | | | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value | $\beta$ | $p$-value |
| ($t$, $t$−1) | Opportunity | | | −0.28 | 0.54 | 0.05 | 0.92 | 0.06 | 0.89 | −0.02 | 0.96 | 2.51 | 0.33 | 0.03 | 0.94 |
| | Effort | | | 0.33 | 0.50 | −0.08 | 0.86 | −0.12 | 0.81 | −0.00 | 0.99 | −0.43 | 0.46 | −0.19 | 0.65 |
| ($t$, $t$−2) | Opportunity | | | −0.35 | 0.37 | −0.05 | 0.90 | −0.04 | 0.91 | −0.08 | 0.84 | −1.83 | 0.42 | −0.01 | 0.97 |
| | Effort | | | 0.37 | 0.37 | −0.01 | 0.98 | −0.02 | 0.97 | 0.04 | 0.93 | −0.34 | 0.51 | −0.12 | 0.74 |
| ($t$, $t$−3) | Opportunity | | | −0.37 | 0.36 | −0.10 | 0.80 | −0.12 | 0.78 | −0.19 | 0.64 | 0.71 | 0.76 | −0.11 | 0.76 |
| | Effort | | | 0.18 | 0.67 | −0.16 | 0.72 | −0.15 | 0.75 | −0.03 | 0.95 | −0.27 | 0.61 | −0.19 | 0.62 |
| $N$ | | 51 | | 51 | | 51 | | 51 | | 51 | | 51 | | 51 | |

*Notes*: Fixed effect estimates of Outcome, Opportunity and Effort Inequality. Inequality measured with the Gini coefficient. All regressions include GDP, population share with upper-secondary school or above, and ratio of business investment to GDP, all measured at the beginning of the period, plus a time dummy.
*$p$-value <0.05.

of outcome inequality. We find non-significant effects of outcome inequality on growth, regardless of whether the latter is measured over one, two or three years.

Inequality of Opportunity is never significant at standard significance levels, and inequality of effort is significant at 5 percent with the expected positive sign only once, for $I^{c5}$ and two-year growth rates. Appendix Table 11 shows that when the MLD is used to measure inequality in the counterfactual distributions, inequality of opportunity shows no significant effect on growth for any of the twelve measures.[19]

Given the difficulty in identifying precise effects using within-country variation with only two time points, we next abstract from the precision of the point estimates, and examine how the conceptual divides relate to the following two hypotheses about the effects of opportunity and effort inequality.

*Strong hypothesis (SH) The effect of inequality of opportunity is negative while the effect of effort inequality is positive (i.e.* $\beta_1 < 0$ *and* $\beta_2 > 0$*).*

*Weak hypothesis (WH) The effect of inequality of opportunity is more negative than the effect of effort inequality (i.e.* $0 > \beta_1 < \beta_2$*).*

The estimates reported in Table 4 show that, ignoring issues of statistical significance, *SH* occurs very few times, and only for ex-post measures $I^{c5}$ and $I^{EO1}$ it occurs for all three time periods over which growth is measured. In addition to the two ex-post measures that satisfy *SH*, another two ex-post measures also satisfy the weak hypothesis: $I^{P}$ and $I^{EO4}$.[20] Finally, it is interesting to note that the growth regression estimates do not follow the taxonomy obtained from the "optimal" grouping of measures, in the sense that the inequality of opportunity coefficient estimates from ex-ante measures do not systematically differ from those coming from ex-post measures. This might be due to the different correlation between the measures and the other covariates.

In sum, this empirical exercise illustrates that the effect of inequality of opportunity (and effort) on growth is not robust to the measure of inequality of opportunity employed. These findings cast doubt on existing evidence, which is exclusively based on the ex-ante parametric measure $I^{c3}$, and highlights the importance of different measurement choices. We hope we provide grounds for the still incipient empirical literature that explores the effects of equality of opportunity on growth to check the sensitivity of its findings to different choices.

[19]As it happens with the correlation between different measures, our growth results are also robust to dropping Cyprus from the analysis.

[20]When we use the MLD as inequality index, our findings are reasonably consistent. Now in addition to $I^{c5}$ and $I^{EO1}$, also $I^{EO4}$ systematically satisfies *SH*—note that when using the Gini, $I^{EO4}$ is very close to systematically satisfying *SH*. Now, in addition to the measures satisfying *SH*, only $I^{EO2}$ satisfies *WH*—Table 11 in the Appendix shows the coefficient estimates.

## 6. Conclusion

Several choices guide the measurement of equality of opportunity. We use EU-SILC data for many European countries to examine whether those choices matter empirically. To this end, we perform two empirical exercises: First we check whether measures that share the same conceptual choices yield similar inequality of opportunity country orderings, and then we analyse whether they yield similar estimates in growth regressions.

Our findings on country orderings identify one crucially important divide, between ex-ante and ex-post views, as it leads to different country orderings. The distinction between direct and indirect approaches matters only conditional on choosing an ex-ante or an ex-post view. Recent theoretical contributions have shown that ex-ante and ex-post approaches to inequality of opportunity are incompatible. Our paper shows that this distinction also matters empirically, and that the norm based measure produces a country ranking that is very different from the rankings produced by the other measures. Hence we have to recognize that inequality of opportunity is a multifaceted concept. As the evaluation of inequality of opportunity is in essence a normative exercise, scholars should provide arguments to support the conceptual choices embedded in the measures they use. Particular attention should be paid to taking an ex-ante or an ex-post view.

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## Supporting Information

Additional supporting information may be found in the online version of this article at the publisher's web site:

**Appendix**

**Table 5** Rank correlations between inequality of opportunity measures. Gini coeffcient, 2011

**Table 6** Rank correlations between inequality of opportunity measures. Mean Log Deviation, 2005
**Table 7** Rank correlations between inequality of opportunity measures. Mean LogDeviation, 2011
**Table 8** Three Sets of Measures that Minimize the Average Spearman Distance. MLD, 2005 and 2011
**Table 9** Rank correlations between Gini- and MLD-based inequality of opportunity measures, 2005
**Table 10** Rank correlations between Gini- and MLD-based inequality of opportunity measures, 2011
**Table 11** Growth regressions. Fixed effect estimates of Outcome, Opportunity and Effort Inequality. Inequality measured with the Mean Log Deviation
**Table 12** Summary Statistics of Circumstance Variables, 2005
**Table 13** Summary Statistics of Circumstance Variables, 2011
**Table 14** Summary Statistics of Effort Variables, 2005
**Table 15** Summary Statistics of Effort Variables, 2011