

Lower Bounds on Inequality of Opportunity and Measurement Error

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Abstract

When measuring inequality of opportunity, researchers usually opt to eliminate within-type variation. Provided that in practice it is impossible to observe all circumstances, this implies that the researcher estimates a lower bound of the true level of inequality of opportunity. By using data drawn from 27 Demographic Household Surveys (circa 2008), it is found that lower bound estimates

can have substantial measurement error, and that measurement error can vary considerably across countries. As a consequence, lower bound estimates of inequality of opportunity can demand too little redistribution to equalize inequalities due to circumstances and can make the “traditional” cross-country comparisons misleading.

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1 Introduction

The literature on Inequality of Opportunity (IOp) follows Roemer's conceptual framework (Roemer, 1998) in which inequalities derived from circumstances beyond the control of individuals are morally objectionable; individuals should be held responsible only for the level of effort they exert in comparison to that exerted by other individuals. Therefore, to measure IOp is to measure the amount of inequality (or variation) that comes from circumstances. In practice, however, this is hard to do. On the one hand, it is virtually impossible to account for all characteristics that constitute individuals' circumstances. On the other hand, the level of effort is usually unobservable. Here I focus on exploring the former problem.

Equality of opportunity embodies two basic principles: i) *The compensation principle*, which demands that inequalities due to circumstances be eliminated, and ii) *The reward principle*, which demands that inequalities due to differences in effort are accepted. These principles are at the core of any empirical application (Ramos and van de Gaer, 2012).

To measure IOp we can take an *ex-ante* or an *ex-post* approach to compensation. The former is concerned with outcome differences among individuals exerting the same effort, that have different circumstances. The latter focuses on prospects, so there is equality whenever all individuals have the same level of outcome regardless of their circumstances.

The *ex-ante* approach requires that we observe effort or that we impose working assumptions about the relationship between effort and outcomes that enable identification. However, this approach is incompatible with the reward principle (Fleurbaey and Peragine, 2012). The *ex-post* approach, on the other hand, is compatible with the reward principle. We can achieve this by partitioning the distribution of outcomes for each group of individuals sharing the same circumstances, under the assumption that individuals at the same percentile of the outcome distribution have exerted the same degree of effort (Cecchi and Peragine, 2010).

Although the literature has proposed several methodologies to measure IOp based on the *ex-ante* and *ex-post* approaches (Ramos and van de Gaer, 2012), researchers still confront several difficulties. One of the most salient is the partial observability of circumstances.

Barros et al. (2010), Ferreira and Gignoux (2011) and Luongo (2011) show that estimates of IOp based on an incomplete list of circumstances are lower bound estimates of true IOp. Even if we make working assumptions about the distribution of an underlying effort variable, we can still face downward bias due to unobservable circumstances and efforts. The problem is that lower bound estimates only pick up part of the observed inequality, hence any alternative number which exceeds the lower bound would be equally valid (Kanbur and Wagstaff, 2014). This has two major implications: i) Any lower bound estimate of IOp could increase by adding one additional circumstance correlated with the outcome variable. In other words, lower bounds can have measurement error. ii) When measuring IOp on different

countries in order to make comparisons, countries could presumably have the same true value of IOp but have different estimated values. This problem implies that lower bound estimates of IOp can have different levels of measurement error, rendering them incomparable.

Recently, Niehues and Peichl (2014) address the problem of partial observability of circumstances by using panel data for Germany and the United States. They use a fixed-effects regression to estimate the amount of inequality that comes from time-invariant circumstances and show that “the existing lower bound estimates substantially underestimate IOp [...]” In this paper I contribute with further evidence on this regard. However, I focus on exploring an outcome for which effort (arguably) plays no role: *height-for-age* for children 0 to 2 years old. This outcome allows me to identify the size of measurement error coming from the empirical impossibility of including all circumstances when estimating IOp since I do not have to make any assumptions about the distribution of effort. By including a comprehensive set of observable circumstances in my analysis I find that measurement error is large and that it varies considerably across countries, implying that making the “traditional” cross-country comparisons using lower bounds estimates of IOp can be misleading.

The paper proceeds as follows. In the next Section I describe the analytical framework. In Section 3 I describe the data and present the results of my analysis. In section 4 I conclude.

2 Analytical framework

Let us consider a society of N individuals. Each individual is described by two classes of variables: circumstances (C) and efforts (e). Let $X = \{x_1, \dots, x_i, \dots, x_N\} \in \mathbb{R}_+^N$ be the vector of outcomes in the population with

$$x = f(C, e),$$

where $f : C \times e \rightarrow \mathbb{R}_+$. I consider, however, the special case where x is completely independent of efforts and where circumstances are partially observable.

Let $C = C^o \cup C^u$ be the full set of circumstances, where the superscripts o and u stand for observable and unobservable. Traditionally, the measurement of IOp is based on a partition of the population into $K = \{1, \dots, k\}$ mutually exclusive types based on observable circumstances (ex-post approach to compensation), so that $\sum_K N_j = N$ with $j \in K$. In this sense, a type is a group of individuals sharing the same observable circumstances.

Once the population is partitioned according to observable circumstances, we can rewrite the overall outcome as follows:

$$X = \{x^1, \dots, x^j, \dots, x^k\} \in \mathbb{R}_+^N$$

where $x^j = \{x_1^j, \dots, x_i^j, \dots, x_{N_j}^j\} \in \mathbb{R}_+^{N_j}$.

Now, X can be partitioned into two vectors:

$$\begin{aligned} X_B &= \{\bar{x}^1, \dots, \bar{x}^j, \dots, \bar{x}^k\}, \\ X_W &= \{\tilde{x}^1, \dots, \tilde{x}^j, \dots, \tilde{x}^k\}. \end{aligned}$$

The first vector is obtained by a smoothing process through which each outcome $x \in x^j$, is substituted by the arithmetic mean of the outcome in vector x^j , denoted by $\bar{x}^j = \frac{\sum_{N_j} x_i^j}{N_j}$. The second distribution is obtained by a standardization process which consists of rescaling the individuals' outcome in each type as follows: $\tilde{x}_i^j = \frac{\bar{x}}{\bar{x}^j} x_i^j$, where $\bar{x} = \frac{\sum_N x_i}{N}$. Through this process X_B eliminates the variation within each type. Analogously, X_W removes the variation between each type, so that we are left only with those inequalities caused by unobservable circumstances.

Now let $I : X \rightarrow \mathbb{R}_+$ be an index of IOp. If $I(X)$ is subgroup decomposable (Shorrocks, 1980), then

$$I(X) = I(X_B) + I(X_W),$$

where $I(X_B)$ accounts for between-type inequality and $I(X_W)$ accounts for within-type inequality. Provided that the Mean Log Deviation (MLD) is additively subgroup decomposable (Foster and Shneyerov, 2000), I use the MLD (without loss of generality) for my empirical exercises.¹

Note that since in our approach the degree of effort exerted does not play any role in the measurement of IOp, by grouping into types using $C^o \subset C$, the partition of the population we obtain is coarser than the one we would obtain from using C . This would cause an underestimation of the level of inequality. This is easy to see since if $I(X_W) > 0$, then $I(X_B) < I(X)$. Thus, $I(X_B)$ represents a *lower bound* estimate of IOp –as it is traditionally defined in the literature.

3 An empirical exercise using Demographic Household Surveys

The data I use come from 27 Demographic Household Surveys (DHS) circa 2008. I focus on kids 0 to 2 years of age because we can confidently assume that children 24 months old or younger have not exerted efforts. Indeed, there is scientific evidence that toddlers start relating their own actions to their surrounding environment after 24 months of age (Rochat, 1998); that is, they become self-aware of their

¹The MLD is defined as

$$MLD(X) = \frac{1}{N} \sum_N \ln \frac{\bar{x}}{x_i}.$$

actions. This provides an empirical advantage, we can confidently assume that individual effort does not play a role on this outcome.

Height-for-age for toddlers is a *human opportunity*. On the one hand, several studies have shown that taller children are more likely to show higher cognitive test scores and, as a result, higher labor earnings.² On the other hand, children's height is a function of both nature and nurture. The height of a child can depend on genetic factors, like the height of his/her parents for example; but it can also depend on nutrition, access to medical services when the mother is pregnant, access to clean water, and many other potential environmental factors that can depend on parents' choice and government interventions –which belong to the set of nurture variables.³

Note that both nature and nurture variables make up our set of circumstances. But, although the DHS provides information on a broad number of these observable characteristics, many of these variables contain a big number of missing values, compromising the representativeness of the samples. Therefore, I include as circumstances variables that do not compromise the representativeness of our samples due to missing values. I include: age of the toddler (in months), gender (as a dummy), birth order (numerical), age of the mother (in years), height of the mother (in centimeters, as proxy for nature), educational attainment of the mother (in years), household wealth (the DHS' index of wealth) and geographical location (urban or rural, as a dummy). These comprise a set of variables not much different than the set of variables that is used to analyze other human opportunities (Molina et al., 2013).⁴

It is important to note that since the height of toddlers increases in variance with age and varies by sex, I use the World Health Organization's growth charts (WHO, 2006) to create a standardized measure for height (x), which corresponds to the equivalent height the child would have had if (s)he were a 24 months old female. More formally

$$x = F_{\bar{a}, \bar{g}}^{-1}(F_{a,g}(h)),$$

where F is the distribution function of heights in the reference population for the age and sex group of an individual of age a and gender g ; h is the actual height of that individual; $\bar{a} = 24$ months; and $\bar{g} = female$.

²See Case and Paxson (2008), for example, for evidence and a discussion.

³See Aturupane et al. (2008) for a discussion.

⁴My full sample consists of 70,788 children. However, after dropping biological implausible values for height of my sample –following the WHO (2006) guidelines to identify biologically implausible values for height- and dropping missing values in my variables of interest, I lose 6% of my full sample. Information on sample sizes can be found at: <https://sites.google.com/site/cfbalcazars/misc>

3.1 Results

Given that x follows a normal distribution (WHO, 2006), I can estimate X_B for each country by means of a linear regression.⁵ Following Ferreira and Gignoux (2011):

$$x_i = \alpha + \beta C_i^o + \varepsilon_i,$$

where x_i is the observed standardized height for individual i , C_i^o is the vector of observable circumstances, and ε_i is an idiosyncratic error term. Note that the distribution of predicted values of this regression (\hat{X}_B), corresponds to the distribution obtained by replacing each individual's outcome with the mean outcome of the type to which she/he belongs. Thus $MLD(\hat{X}_B)$ corresponds to my lower bound estimate of IOp.

Provided that the MLD is path independent, we can compute the relative size of within-type inequality as

$$IR(\hat{X}_W) = \left[1 - \frac{MLD(\hat{X}_B)}{MLD(X)} \right] \times 100,$$

with $IR(\hat{X}_W) \in [0, 100]$. $IR(\hat{X}_W)$ provides us with a comparable cross-country estimate of the extent of measurement error that arises from the impossibility of including all observable circumstances. Note that the higher $IR(\hat{X}_W)$, the higher the level of measurement error.

Table 1 shows the results, but let us focus on the fourth column of results. My computations show that in spite of including a comprehensive set of circumstances all lower bound estimates show a substantial amount of measurement error. Indeed, the value of $IR(\hat{X}_W)$ ranges from 75.79 (Colombia) to 93.23 (Chad). In other words, as little as 73% and as much as 93% of total variation is not being explained by the set of observable circumstances, conditional on the country.

Table 1: Inequality of opportunity decomposed by country

Country	Total inequality $MLD(X) \times 100$	Between-type inequality $MLD(\hat{X}_B) \times 100$	Within-type inequality $[MLD(X) - MLD(\hat{X}_B)] \times 100$	Relative within-type inequality $IR(\hat{X}_W)$
Azerbaijan	0.216	0.027	0.190	87.70
Bangladesh	0.184	0.024	0.160	86.89
Bolivia	0.159	0.030	0.129	81.25
Burkina Faso	0.246	0.024	0.221	90.05
Burundi	0.189	0.024	0.164	87.09
Cambodia	0.181	0.024	0.157	86.97
Cameroon	0.235	0.026	0.208	88.76
Chad	0.341	0.023	0.318	93.23

⁵If we were to use a non-parametric approach, we would face problems of small cell-sizes: With a large number of types, the sample size of each type can be quite small, leading to biased estimations of type-means.

Country	Total inequality $MLD(X) \times 100$	Between-type inequality $MLD(\hat{X}_B) \times 100$	Within-type inequality $[MLD(X) - MLD(\hat{X}_B)] \times 100$	Relative within-type inequality $IR(\hat{X}_W)$
Chad	0.341	0.023	0.318	93.23
Colombia	0.114	0.028	0.086	75.79
Cote d'Ivoire	0.205	0.026	0.179	87.49
Egypt	0.351	0.028	0.323	92.05
Ethiopia	0.259	0.025	0.234	90.38
Guinea	0.271	0.023	0.248	91.45
Haiti	0.171	0.026	0.145	84.61
Honduras	0.127	0.027	0.100	78.69
Jordan	0.134	0.022	0.112	83.46
Kenya	0.261	0.025	0.236	90.48
Lesotho	0.234	0.022	0.212	90.50
Liberia	0.246	0.026	0.220	89.35
Morocco	0.305	0.029	0.276	90.39
Mozambique	0.266	0.024	0.242	90.91
Niger	0.301	0.022	0.279	92.59
Peru	0.132	0.028	0.104	78.84
Rwanda	0.192	0.024	0.167	87.25
Tanzania	0.200	0.024	0.176	87.77
Turkey	0.162	0.026	0.136	83.78
Uganda	0.225	0.026	0.198	88.26

Note: I multiply each number in the first three columns of results by 100 for readability.

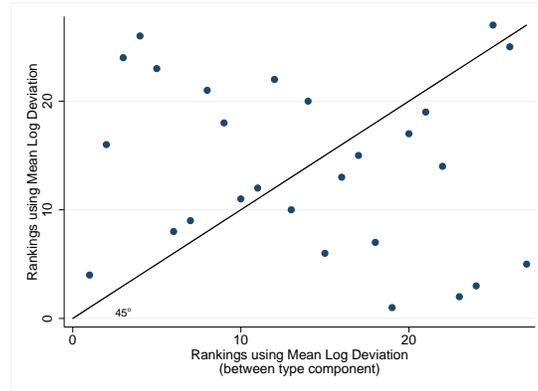
Source: Authors' calculations based on Demographic Household Surveys.

Given that $IR(\hat{X}_W)$ also varies considerably across countries, we can also conclude that lower bound estimates of inequality of opportunity are not comparable. To be comparable we should obtain a high correlation coefficient between $MLD(X)$ and $MLD(\hat{X}_B)$, but the correlation is -0.17. To further show the implications of this, Figure 1 compares the ordinal $MLD(X)$ country-rankings and the ordinal country-rankings for $MLD(\hat{X}_B)$, both from most to least. The results show that there are considerable re-rankings.

4 Conclusion

Several development reports and research papers have incorporated the IOp dimension when addressing development and social issues. Nonetheless, my results suggest that the empirical applications that use lower bound estimates of IOp can be misleading. On the one hand, lower bound estimates can underestimate substantially the actual value of IOp, demanding too little redistribution to equalize inequalities due to circumstances. On the other hand, measurement error can vary considerably across samples, thus making the traditional cross-country comparisons unreliable. These conclusions derive from the empirical advantage of using an outcome variable for which we can confidently assume effort plays no role, so that we are able to identify the size of measurement error that comes from the empirical impossibility of including all circumstances in the analysis.

Figure 1: Rankings of $MLD(X_B)$ vs. $MLD(X)$



Source: Authors' calculations based on Demographic Household Surveys circa 2008.

A promising path to address the problem of partial observability of circumstances is the use of panel and pseudo-panel data. Niehues and Peichl (2014) show that we can estimate an upper bound of inequality of opportunity through the fixed effects component of a fixed effects regression, addressing problems of measurement error. However, since panel data are scarce, we can use pseudo-panels in order to exploit the availability of household surveys (Verbeek and Vella, 2005). By using pseudo-panels we should be able to estimate the long-run upper bound of IOp *à la* Niehues and Peichl addressing (plausibly) both measurement error and comparability across countries. This is a path that has not been explored yet, and it would be worth exploring by the scientific community.

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