the review of income and wealth

Review of Income and Wealth
Series 64, Number 4, December 2018
DOI: 10.1111/roiw.12311

INEQUALITY OF OPPORTUNITY AND ECONOMIC GROWTH: HOW MUCH CAN CROSS-COUNTRY REGRESSIONS REALLY TELL US?

BY FRANCISCO H. G. FERREIRA*

World Bank and Institute for the Study of Labor (IZA)

AND

CHRISTOPH LAKNER, MARIA ANA LUGO AND BERK ÖZLER[1]

World Bank

Income differences arise from many sources. While some kinds of inequality, caused by differential rewards to effort, might be associated with faster economic growth, other kinds, arising from unequal opportunities for investment, might be detrimental to economic progress. This study uses two new datasets, consisting of 117 income and expenditure household surveys and 134 Demographic and Health Surveys, to revisit the relationship between total inequality and economic growth. In particular, we ask whether inequality of opportunity, driven by circumstances at birth, has a negative effect on subsequent growth. Using the income and expenditure micro dataset, we find that while both total income inequality and inequality of opportunity are negatively associated with growth, the coefficient estimates are insignificant. The evidence is similarly equivocal using the Demographic and Health Surveys data. On balance, the data do not provide support for the hypothesis that inequality of opportunity is bad for growth.

JEL Codes: D31, D63, O40

Keywords: economic growth, inequality, inequality of opportunity

1. Introduction

Although the question of whether inequality may have a detrimental effect on subsequent economic growth has been asked many times, there is no consensus answer in the literature. Theory provides ambiguous predictions: whereas higher inequality may lead to faster growth through some channels (such as higher aggregate savings when a greater share of income accrues to the rich), it may have negative effects through other channels (such as lower aggregate rates of investment in human capital if credit constraints prevent the poor from financing an optimal amount of education).

*Correspondence to: Francisco H. G. Ferreira, World Bank and Institute for the Study of Labor (IZA) (fferreira@worldbank.org).

[1]Ferreira is at the World Bank and the Institute for the Study of Labor (IZA). Lakner, Lugo and Özler are at the World Bank. We are grateful to Manuel Fernandez-Sierra, Marina Gindelsky, Ursula Mello, Christelle Sapata and Marc Smitz for excellent research assistance and to Claudio Montenegro for help in assembling our dataset. We are also thankful to Conchita D'Ambrosio and Michael Wolfson (editors); to two anonymous referees; and to Angus Deaton, Marc Fleurbaey, Aart Kraay, Peter Lanjouw, Gustavo Marrero, Leonardo Santos de Oliveira, Juan Gabriel Rodríguez and a number of conference and seminar participants for comments on earlier versions. We are grateful to the UK Department for International Development for support through its Knowledge for Change (KCPII – TF012968) and Strategic Research Programs. We are solely responsible for any remaining errors. The findings and conclusions presented here are those of the authors, and do not necessarily represent the views of the World Bank Group.

The empirical evidence has been correspondingly mixed. The earliest crop of papers including measures of income inequality in growth regressions, in the 1990s, tended to find a negative and statistically significant coefficient, which was widely interpreted to suggest that the theoretical channels through which inequality was bad for growth dominated those through which there might be positive effects. But all of these studies relied on OLS or IV regressions on a single cross-section of countries. Using the "high-quality" subset of the Deininger and Squire (1996) dataset, which permitted panel specifications, Forbes (2000) and Li and Zou (1998) found positive effects of lagged inequality on growth, and suggested that omitted (time-invariant) variables may have biased the OLS coefficients. Banerjee and Duflo (2003) raised further questions about the credibility of the earlier results—whether drawing on single cross-sections or on panel data—by showing that if the true underlying relationship between inequality (or its changes) and growth was non-linear, this would suffice to explain why the previous estimates were so unstable. The prevailing conclusion from these disparate results, as summarized by Voitchovsky (2009), was that "recent empirical efforts to capture the overall effect of inequality on growth using cross-country data have generally proven inconclusive" (p. 549).

And yet, the question continues to motivate researchers and policymakers alike. Asking what might explain the absence of poverty convergence in the developing world, Ravallion (2012) revisits the effects of the initial distribution on subsequent growth, and claims that a higher initial level of poverty—not inequality—is robustly associated with lower economic growth. In remarks delivered at the Center for American Progress in 2012, Alan Krueger, Chairman of the Council of Economic Advisers to the US president, claimed that "the rise in inequality in the United States over the last three decades has reached the point that inequality in incomes is causing an unhealthy division in opportunities, and is a threat to our economic growth" (Krueger, 2012).[2]

The conjecture that an "unhealthy division of opportunities" might be bad for growth is consistent with some of the theory: if production sets are non-convex (as, for example, when production functions are characterized by increasing returns to scale over some range) and credit markets fail, the poor may be prevented from choosing privately optimal levels of investment—in human or physical capital (Galor and Zeira, 1993). Others have suggested that low levels of wealth are associated with reduced returns to entrepreneurial effort as a result of the need to repay creditors. This moral hazard is anticipated by lenders, leading to credit market failures and differences in the entrepreneurial opportunities available to rich and poor agents (Aghion and Bolton, 1997).

Drawing on the recent literature on the formal measurement of inequality of opportunity—as distinct both from income or wealth inequality and from economic mobility—this paper seeks to address that question directly. Is it possible that

[2]Voitchovsky (2009) also suggests that the link between income and wealth inequality and growth might operate through the distribution of opportunities: "... income or asset inequality is considered to reflect inequities of opportunity" (p. 550).

inequality—like cholesterol—comes in many varieties, and that some are worse for the health and dynamism of an economy than others? In particular, is it possible that the two broad categories of sources of inequality suggested by Roemer (1998)—opportunities and efforts—have opposite effects on economic performance? If so, one reason for the ambiguity in past empirical studies of the relationship between inequality and growth might have been the failure to distinguish between the two types of inequality.

Unfortunately, measures of inequality of opportunity were not readily available for a large number of countries, in the way that income inequality measures were in the Deininger and Squire (1996) dataset, or the World Income Inequality Database of WIDER. We therefore constructed original measures of inequality of opportunity from unit-record data from 117 income or expenditure household surveys (IES) for 42 countries, and 134 Demographic and Health Surveys (DHS) for 42 countries. These indices were combined with information on the other explanatory variables used by Forbes (2000), which are illustrative of the set of regressors typically used in the literature. Although we use the same difference-GMM specification as Forbes (2000) for comparison purposes, we also draw on more recent developments in the estimation of Generalized Method of Moments models, including a number of system-GMM specifications which are designed to alleviate the weak instruments problem that plagues difference-GMM with highly persistent data.

Our empirical findings are inconclusive. Using the IES data, the relationship between overall inequality and growth is almost never positive, but the coefficient estimates on total inequality are only weakly significant in two of our six main empirical specifications. Using the DHS dataset, the coefficient estimates are always small and insignificant in all specifications. While the confidence intervals do not rule out a positive relationship, these findings do not provide much support for a positive coefficient on total inequality found in Forbes (2000) and Li and Zou (1998).

Furthermore, decomposing overall inequality into a component associated with inequality of opportunity and a residual component (notionally related to inequality arising from effort differences) is equally unhelpful in resolving the ambiguity in the relationship between inequality and growth. Using the IES sample, the coefficient for the inequality of opportunity component is almost always negative, but never significant. In the DHS sample the coefficient on inequality of opportunity is again small and insignificant in all but one of the six main specifications.

On the whole, the new datasets, which make use of the largest available set of micro data that allow us to construct measures of inequality of opportunity, do not provide support for the hypothesis that inequality of opportunity is bad (or good) for growth. It does not help that the system-GMM models that were developed to improve upon difference-GMM used in earlier studies are under-identified in our study.

The paper is organized as follows. The next section briefly reviews the literature on the relationship between inequality and growth, with a focus on the main empirical papers. Section 3 introduces the concept and measurement of inequality of opportunities. Section 4 describes the econometric specification and the data used in the analysis. Section 5 describes the estimation procedures and presents the results. Section 6 concludes.

2. A Brief Review of the Literature

Speculation that the distribution of incomes at a given point in time might affect the subsequent rate of growth in aggregate income goes back at least to the 1950s, following the empirical finding that the savings rate increased with income, albeit at a decreasing rate, in the U.S. (Kuznets, 1953). Kaldor (1957) incorporated this feature into a growth model, by assuming that the marginal propensity to save out of profits was higher than the propensity to save out of wages. Under that assumption, a higher profit-to-wage ratio—which corresponded to higher income inequality in that model—would lead to a faster equilibrium rate of economic growth. See also Pasinetti (1962).

But it was in the 1990s that a number of papers linking inequality to growth and the process of development appeared, raising the profile of distributional issues not only within development economics, but in the broader discipline as well (see Atkinson, 1997). The theoretical literature on the links between inequality and growth has been extensively reviewed, and we aim to provide only a brief review in this section. For some of the best surveys, see Aghion *et al.* (1999), Bertola (2000) and Voitchovsky (2009). Papers in this literature came in two basic varieties: first, models where the combination of an unequal initial distribution of wealth with imperfections in capital markets led to inefficiencies in investment activities and, second, political economy models where inequality led to taxation or spending decisions that deviated from those a benevolent social planner might make.

The first class of models is perhaps best illustrated by Galor and Zeira (1993), which followed Loury (1981), where agents have a choice between investing in education and working as unskilled workers. An indivisibility in the production function of human capital and the existence of monitoring or tracking costs in the credit markets (as a result of information and enforcement costs) implies that there is a given, positive wealth threshold (f) below which individuals choose not to invest in schooling. Above it, all agents choose to acquire human capital. Wealth is transmitted across generations through bequests which, under certain assumptions, render wealth dynamics a Markov process. The long-run limiting distribution depends on initial conditions, and a higher mass of individuals below f leads to lower aggregate wealth in equilibrium.

Other papers involving capital market imperfections rely on alternative mechanisms, but are essentially variations on the same theme. Banerjee and Newman (1993) model a process of occupational choices where, in the absence of credit markets, initial wealth determines whether individuals prefer to work in self-employment, as employees, or as employers. A nice feature of the model is that the decision also depends on aggregate factor prices, notably the wage rate, which is endogenous to the initial wealth distribution, leading to multiple equilibria. In Aghion and Bolton (1997) borrowers suffer from an effort supply disincentive arising from the need to repay their debts. The strength of this moral hazard effect increases in the size of the loan required, and thus decreases in initial wealth, leading to higher interest rates for the poorest borrowers. A related mechanism is the choice between investing in quantity and "quality" of children: poorer agents experience a lower opportunity cost from having children, and thus a higher fertility rate. However, credit market constraints prevent them from investing as much in each child. In the aggregate, more unequal societies (i.e.

those with greater numbers of poor people for a given mean income level) tend to have a greater relative supply of unskilled workers, and hence a lower unskilled wage rate leading, once again, to the possibility of multiple equilibria, with higher initial inequality possibly causing lower subsequent growth.

The second group of models focuses on the effect of inequality on policy decisions—either through voting or through lobbying. Alesina and Rodrik (1994) and Persson and Tabellini (1994) use standard median voter models to predict that societies with a larger gap between median and mean incomes (a plausible measure of inequality) would choose higher rates of redistributive taxation. If taxes distort private investment decisions, then greater inequality might lead to lower growth rates through higher distortive taxation. Bénabou (2000) proposes an alternative set up where inequality distorts public policy by leading to inefficiently low—rather than high—taxes. This mechanism requires that voting power increases with wealth, so that the pivotal voter has higher than median wealth. It also requires that public investment (e.g. educational subsidies) have positive spillovers, so that taxes finance efficient public expenditures. These conditions are not sufficient for, but may lead to, multiple equilibria that depend on the initial distribution. The mechanism proposed by Bénabou (2000) has the advantage that it is more consistent with the evidence that high inequality countries tend to tax less, rather than more, than less unequal countries. See also Ferreira (2001).

Inequality may also matter for political processes other than elections. Esteban and Ray (2000) suggested that the rich might find it easier to lobby the government, and distort resource allocation from the social optimal towards the kinds of expenditures they prefer. Campante and Ferreira (2007) construct a model where the outcome of lobbying is generally not Pareto efficient: resource allocation can be distorted away from the social optimum, and this may benefit poorer or richer groups, depending on their relative productivity levels in economic and political activities.

These various predictions have been put to the test a number of times, typically by including a measure of initial inequality in the standard cross-country growth regression of Barro (1991). In a first phase of the literature, both Alesina and Rodrik (1994) and Persson and Tabellini (1994) reported results from such an exercise. Alesina and Rodrik (1994) regressed the annual growth rate in per capita GDP on the Gini coefficients (for income or land) in 1960, for different country samples, using both OLS and two-stage least squares (TSLS) regressions.[3] Their inequality data come from secondary sources, namely compilations of income Gini coefficients from Jain (1975) and Fields (1989), and of land coefficients from Taylor and Hudson (1972). Both of these studies found a negative and statistically significant coefficient for initial inequality in the growth regression. Alesina and Rodrik report a particularly robust correlation between land inequality and subsequent growth, significant at the 1 percent level, and implying that an increase of one standard deviation in land inequality would lead to a decline of 0.8 percentage points in annual growth rates. Deininger and Squire (1998), using a larger (and arguably higher-quality) cross-country inequality dataset that they compiled, report the same basic finding of a negative effect of initial inequality on growth.

[3]Literacy rates in 1960, infant mortality rates in 1965, secondary enrollment in 1960, fertility in 1965 and an Africa dummy are used as instruments for inequality in the TSLS first-stage.

The Deininger and Squire (1996) dataset contained inequality data points for many more countries and, most importantly, at various points in time. This allowed Li and Zou (1998) and Forbes (2000) to run the same growth regression as the earlier papers, but on a panel, rather than a cross-section, of countries—ushering in "Phase 2" of the empirical literature on inequality and growth. Forbes (2000) reported fixed effects, random effects, and GMM estimates for a panel of 45 countries where, instead of regressing annualized growth over a long period on a single inequality observation at the beginning of the period, growth rates for five-year intervals were regressed on inequality at the start of each interval. In the difference-GMM estimates, lagged values of the independent variables were used as instruments. The results from these panel specifications were strikingly different from single cross-section results: the coefficient on inequality was generally positive and, in the preferred specifications, statistically significant. Various interpretations were possible: perhaps the short-run effect of inequality on growth was positive, but the long-term effect was negative. But another, equally if not more plausible, interpretation was that the OLS cross-section coefficients were biased downward by omitted variables correlated with inequality. The fixed-effects and difference-GMM estimates correct at least for time-invariant omitted variables, and this correction would appear to invalidate the negative effect of inequality on growth.

Other estimates are also available: Barro (2000) considered the possibility that the effect of inequality on growth might differ between rich and poor countries. While no significant relationship is found for the whole sample, he reports a significant negative relationship for the poorer countries and a positive relationship among richer countries when the sample is split. Voitchovsky (2005) focuses on another kind of heterogeneity: rather than asking whether the effect differs across the sample of countries, she tests whether inequality "at the bottom" of the distribution had a different effect from inequality "at the top," claiming that this would be consistent with some of the theoretical mechanisms discussed above. Indeed she finds that inequality measures more sensitive to the bottom of the distribution appear to have a negative effect on growth, while those more sensitive to the top of distribution are positively associated with growth. By the early to mid-2000s, however, the dominant conclusion that appeared to be drawn from the existing evidence was that the cross-country association between inequality and growth was simply not robust to variations in the data or econometric specification used to investigate it. Banerjee and Duflo (2003), for example, argue that if the true relationship between the two variables were non-linear, it may not be identified by the linear regressions described above.

Such skepticism has not prevented a recent revival in interest in the cross-country association between inequality and growth. In what might be described as "Phase 3" of the literature, a number of recent papers have suggested alternative tests of the same basic idea. Easterly (2007) sets out to test the hypothesis, originally formulated by Engerman and Sokoloff (1997), that, over the long term, agricultural endowments predict inequality, and inequality in turn affects institutional development and ultimately growth. Using a new instrumental variable constructed as the ratio of a country's land endowment suitable for wheat production to the land suitable for growing sugarcane, the author finds strong support for the endowments-inequality-growth link, with higher inequality leading to lower subsequent growth. Berg, Ostry and Zettelmeyer (2012) look at a different feature of growth processes—their sustainability, rather than intensity—and find that inequality is a powerful (inverse) predictor of the duration of future growth spells.

Ravallion (2012) also finds that features of the initial distribution affect future growth, but suggests that poverty—rather than inequality—provides the best distributional predictor of future growth. Ostry *et al.* (2014) investigate a recent dataset—which, they claim, allows them to "calculate redistributive transfers for a large number of country-year observations" (p. 4)—and find that after-tax inequality is robustly associated with lower rates of economic growth.[4] Taken together, this latest, third phase of the empirical literature tends to replace the positive results of the second phase ("inequality is, if anything, good for growth") with the negative results that used to prevail in Phase 1: "inequality is bad for growth, after all". The pendulum would seem to have swung full cycle.

Another possibility raised in this latest phase of research into the link between distribution and economic performance is that scalar measures of income or expenditure inequality may be best seen as composite indicators, the constituent elements of which affect economic performance in different ways. In particular, it has been suggested that inequality of opportunity might have more adverse consequences than the inequality which arises from differential rewards to effort (e.g. Bourguignon *et al.* 2007b). This claim resonates with some of the theoretical mechanisms reviewed above, for example that low wealth leads to forgone productive investment opportunities for part of the population. Such mechanisms operate through differences in the opportunity sets faced by different agents, and are potentially still consistent with differences in earnings that provide incentives for effort being good for growth.

If overall income inequality comprises both inequality of opportunity and inequality due to effort, and these two components have different effects on economic growth, then the relationship that has typically been estimated is misspecified, and one ought to distinguish between the two kinds of inequality. Marrero and Rodríguez (2013) do this for 26 states of the U.S.: they decompose a Theil (L) index into a component associated with inequality of opportunity, and another that they attribute to differences in efforts. When economic growth is regressed on income inequality and the usual control variables in their sample of states, the coefficient on inequality is statistically insignificant. But when the two components of inequality are entered separately, the coefficient on "effort inequality" is generally positive, and that on inequality of opportunity is negative and strongly significant.

To our knowledge, Marrero and Rodríguez (2013) is the only published paper that investigates whether inequality of opportunity is the "active ingredient" in the relationship between inequality and growth.[5] Their findings suggest that this component of inequality was negatively associated with economic growth in the U.S. in the 1970–2000 period. Is this a more general result? Can the same be said of other places and contexts?

[4]The dataset used by Ostry *et al.* (2014) is the Standardized World Income Inequality Database (SWIID)—see Solt (2009). Unfortunately, this database relies on a very large number of imputed inequality entries for country-year cells for which no household surveys were conducted. Reliance on such imputed data makes the results in this paper suspect, at least until considerable additional validation can be carried out. See Jenkins (2015) for a detailed critique of the SWIID. In addition, and as discussed in more detail below, Kraay (2015) shows that the estimators used by Ostry *et al.* (2014) suffer from weak instruments. Confidence intervals consistent with weak instruments are much wider, such that the coefficient on inequality is no longer significantly different from zero.

[5]Although, in a recent manuscript, Teyssier (2015) finds a similar effect across Brazilian municipalities.

In particular, can a decomposition of inequality into an opportunity and a residual component help resolve the inconclusiveness of the cross-country literature on this subject? In order to address this question, the next section briefly reviews the recent empirical literature on the measurement of inequality of opportunity, and defines the indices we use in this paper.

3. Inequality of Opportunity

The concept of equality of opportunity has been widely discussed among philosophers since the seminal papers by Dworkin (1981), Arneson (1989) and Cohen (1989). It is central to the school of thought that believes that meaningful theories of distributive justice should take personal responsibility into account. In essence, these "responsibility-sensitive" egalitarian perspectives propose that those inequalities for which people can be held ethically responsible are normatively acceptable. Other inequalities, presumably driven by factors over which individuals have no control, are unacceptable, and often referred to as inequality of opportunity.

The concept was formalized and introduced to economists by Roemer (1993, 1998) and van de Gaer (1993). Among economists, its usage was initially restricted to social choice theorists. Broader applications in the field of public economics began with Roemer *et al.* (2003), who investigated the effects of fiscal systems—broadly the size and incidence of taxes and transfers—on inequality of opportunity in 11 (developed) countries. Actual empirical measures of inequality of opportunity based on the definitions provided by Roemer (1998) and van de Gaer (1993) are more recent, and include Bourguignon *et al.* (2007a), Lefranc *et al.* (2008), Checchi and Peragine (2010) and Ferreira and Gignoux (2011).

In this paper, we follow the *ex-ante* approach independently proposed by Checchi and Peragine (2010) and Ferreira and Gignoux (2011). Consider a population of agents indexed by $i \in \{1, \ldots, N\}$. Let y_i denote what is known in this literature as the "advantage" of individual i, which, in the present paper, will be a measure of household income, consumption, or wealth. The N-dimensional vector $\boldsymbol{y}$ denotes the distribution of incomes in this population. Let $\boldsymbol{C_i}$ be a vector of characteristics of individual i over which she has no control, such as her gender, race or ethnic group, place of birth, and the education or occupation of her parents. Let $\boldsymbol{C_i}$ have H elements, all of which are discrete with a finite number of categories, x_h, $h=1, \ldots, H$. Following Roemer (1998), the elements of $\boldsymbol{C_i}$ are referred to as circumstance variables.

Define a partition of the population $\Pi = \{T_1, T_2, \ldots, T_K\}$, such that $T_1 \cup T_2 \cup \ldots \cup T_k = \{1, \ldots, N\}$, $T_l \cap T_k = \emptyset, \forall l, k$, and $C_i = C_j, \forall i, j | i \in T_k, j \in T_k, \forall k$. Each element of Π, T_k, is a subset of the population made up of individuals with identical circumstances. Following Roemer (1998), we call these subgroups "types". The maximum possible number of types is given by $\bar{K} = \prod_{h=1}^{H} x_h$.[6]

In simple terms, the ex-ante approach to measuring inequality of opportunity consists of agreeing on a measure of the value of the opportunity set facing each

[6] $K < \bar{K}$ if some cells in the partition are empty in the population.

type, assigning each individual the value of his or her type's opportunity set, and computing the inequality in that distribution.[7] Following van de Gaer (1993) and Ooghe *et al*. (2007), Ferreira and Gignoux (2011) choose the mean income in type k, μ_k, as a measure of the value of the opportunity set faced by people in that type. In other words, a hypothetical situation of equality of opportunity would require that:

$$\mu_k(y)=\mu_l(y), \ \ \forall k,l|T_k \in \Pi, \ \ T_l \in \Pi \tag{1}$$

Using the superscript k to indicate the type to which individual i belongs, a typical element of the income vector $\boldsymbol{y}$ is denoted y_i^k. The counterfactual distribution in which each individual is assigned the value of his or her type's opportunity set is then simply the *smoothed distribution* corresponding to the vector y and the partition Π, *i.e.* the distribution obtained by replacing y_i^k with μ_k, $\forall i,k$. Denoting that distribution as $\{\mu_i^k\}$, Ferreira and Gignoux propose a very simple measure of inequality of opportunity, namely $I(\{\mu_i^k\})$, where $I()$ is the mean logarithmic deviation, also known as the Theil (L) index, or Generalized Entropy index with $\alpha=0$ (GE(0)). Among inequality indices that use the arithmetic mean as the reference income, this measure is the only one that satisfies the symmetry, transfer, scale invariance, population replication, additive decomposability and path-independent decomposability axioms (Foster and Shneyerov, 2000). This is the main empirical measure of inequality of opportunity used on the income and expenditure survey sample in Section 5 below, although we also report results for GE(2), a measure that is more sensitive to higher incomes, in one of our robustness tests.[8]

The mean log deviation is not, however, suitable for use on the Demographic and Health Survey sample. As discussed in the next section, DHS surveys do not contain credible measures of income or consumption. They do, however, contain information on a number of assets and durable goods owned by the household, as well as dwelling and access to service characteristics. Following Filmer and Pritchett (2001), it has become standard practice to use a principal component of these variables as a proxy for household wealth. As a principal component, this wealth index has negative values, and its mean is zero by construction, so that the mean log deviation is not a suitable measure of its dispersion.

In our DHS sample, we therefore follow Ferreira *et al*. (2011) in using the variance of predicted wealth from an OLS regression of the asset index on all observed circumstances in $\boldsymbol{C}$ as our measure of inequality of opportunity. The essence of the rationale for this choice of measure is as follows.[9] We tend to think

[7]The *ex-post* approach to the measurement of inequality of opportunity requires computing the inequality among individuals exerting the same degree of effort which, in turn, requires assumptions about how effort can be measured. See Fleurbaey and Peragine (2013) for a discussion of both approaches.

[8]GE(2) is simply one half of the square of the coefficient of variation. It satisfies the same axioms as GE(0), except for path-independent decomposability.

[9]This discussion draws heavily on Bourguignon *et al*. (2007a) and Ferreira and Gignoux (2011). Readers are referred to those papers for detail.

of advantage (in this case the wealth index w) as a function of circumstances, efforts, and possibly some random factor u:

$$w=f(C,\ E,\ u) \tag{2}$$

Although circumstances are exogenous by definition (i.e. they are factors beyond the control of the individual and are hence determined outside the model), efforts can be influenced by circumstances:

$$E=g(C, v) \tag{3}$$

For the purposes of simply measuring inequality of opportunity (as opposed to identifying individual causal pathways), it suffices to estimate the reduced form of the system (2)–(3). Under the usual linearity assumption, this is given by:

$$w=C\psi+\varepsilon \tag{4}$$

Under this linearity assumption, $\{\hat{w}\}$ — where $\hat{w}=C\hat{\psi}$ — is a parametric equivalent to the smoothed distribution $\{\mu_i^k\}$ previously described. It is a distribution where individual values of the wealth index are replaced by the mean conditional on circumstances, much as before. Whereas a non-parametric approach, using the cell means, is clearly preferable when data permits it, the parametric approach based on estimating the reduced-form equation (4) may be preferable when K is large relative to N, so that many cells are sparsely populated, and their means imprecisely estimated. Given the properties of the distribution of w, we follow Ferreira *et al.* (2011) in measuring its inequality simply by the variance: $V(\{\hat{w}\})$.

An important caveat about these measures is that, in practice, not all relevant circumstance variables may be observed in the data. If the vector of *observed* circumstances has dimension less than H, then both the non-parametric index $I\left(\{\mu_i^k\}\right)$ and the parametric measure $V(\{\hat{w}\})$ are lower-bound estimates of true inequality of opportunity. See Ferreira and Gignoux (2011) for a formal proof. In addition, in the presence of omitted circumstances, clearly neither the non-parametric decomposition nor the reduced-form regression (4) can be used to identify the effect of individual circumstance variables. We know the direction of bias—downward—for the overall measures of inequality of opportunity (which is why they are lower-bound estimators), but the same cannot be said for individual regression coefficients, or their non-parametric analogues.

4. Econometric Specification and Data Sources

Our aim is to investigate whether decomposing inequality into inequality of opportunity and a residual term (comprising inequality arising from efforts, as well as from omitted circumstances) helps resolve the inconclusiveness about the effects of inequality on subsequent growth in the empirical cross-country literature. We first estimate the following equation, which is identical to the specification employed in Forbes (2000):

$$(5) \quad g_{jt} = \beta_1 y_{j,t-5} + \beta_2 I(y)_{j,t-5} + \beta_3 ME_{j,t-5} + \beta_4 FE_{j,t-5} + \beta_5 PPPI_{j,t-5} + \alpha_j + \eta_t + u_{jt}$$

We estimate equation (5) in two panel datasets: one consisting of income and expenditure surveys (IES), and another comprised of DHS surveys. These datasets are described in detail below. In both datasets, the dependent variable, g_{jt}, is the average annual growth rate of per capita gross national income in country j, during a five-year interval to year t. The data comes from the World Bank's World Development Indicators dataset, from which we also obtain the (five-year) lagged national income per capita, $y_{j,t-5}$, expressed in constant 2005 US dollars.[10]

$I(y)_{j,t-5}$—our measure of overall inequality—is the key variable that differs between the two samples: in the main specification run on the IES sample, it denotes the mean logarithmic deviation of incomes (or expenditures) among individuals at the beginning of the five-year interval.[11] In the DHS sample, it denotes the (overall) variance of the asset index ($V(w)$), also across individuals at the beginning of the five-year interval. Unlike in Forbes (2000) or most other studies in this literature, these inequality indices do not come from a compilation of scalar measures from earlier studies, such as the Deininger and Squire (1996) database, or the WIDER World Income Inequality Database. Instead, the inequality indices are computed from the original microdata for all surveys in all countries. Details on the household- and person-level metadata set are provided below. Summary statistics for the growth and income variables, as well as the total inequality variable, are reported in Table A1 (income and expenditure surveys) and Table A2 (Demographic and Health Surveys), in the online appendix.

Male and female education data ($ME_{j,\ t-5}$ and $FE_{j,t-5}$) come from Lutz *et al*. (2007, 2010), and are defined as the proportion of adult (male/female) population that attained at least one year of secondary education. Lutz and co-authors produced estimates for 120 countries from 1970 to 2010, on a quinquennial basis.[12] These data are in the spirit of Barro and Lee (2001), although the method used to complete missing data differs slightly.[13] Finally, as in Forbes, market distortions are proxied by a price level index of investment from the Penn World Tables (version 6.3), which is defined as the ratio of investment good prices at purchasing power parity, to those prices at market exchange rates ($PPPI_{j,t-5}$). α_j denotes country j's fixed effect, η_t is a period dummy, and u_{jt} is the error term.

Equation (5) provides estimates for the effect of total inequality on growth, à la Forbes (2000). However, we are principally interested in whether the two

[10]With the exception of the Czech Republic, Estonia and Ireland in the case of the IES sample and of Haiti for the DHS sample, where GDP is used instead of GNI.

[11]To be precise, we divide the survey years into five-year bins. For example, the measure of inequality for 2005 may come from any year between 2001 and 2005. In a small number of cases, we have stretched the boundaries slightly: in Romania, e.g. we use the 2002 survey for 1996–2000 and the 2006 survey for 2001–2005. We only extend the boundaries forward and not backward (e.g. we do not use a 2000 survey for the 2001–2005 bin). Please see Tables A3 and A4 in the online appendix for a precise listing of the survey years used for each country.

[12]For the IES sample the five-year intervals align with the Lutz data. However, for the DHS sample, the five-year intervals are one year later (e.g. the end-year is 1991 or 1996). Therefore, we move the Lutz data forward by one year when matching to the DHS sample.

[13]While Barro and Lee used the perpetual inventory method to complete their dataset, and transform flows into stocks of education, Lutz *et al*. used backward (2007) and forward (2010) projections from empirical observations given by UNESCO and UN data on population structure.

components of overall inequality, namely inequality between types and inequality within them—interpreted as proxies for inequality of opportunity and inequality due to effort—have heterogeneous associations with growth. Therefore, in equation (6), we re-estimate equation (5) but replacing $I(y)_{j,t-5}$ with our measures of inequality of opportunity: $I(\{\mu_j^k\})$ in the IES sample, and $V(\{\hat{w}\})$ in the DHS sample. For simplicity, we denote both of these as $IOp_{j,t-5}$ in the generic specification. We also include the residual term, $IR_{j,t-5}=I(y)_{j,t-5}-IOp_{j,t-5}$, and estimate:

$$(6) \qquad \begin{aligned} g_{jt} = {} & \beta_1 y_{j,t-5}+\beta_2 IOp_{j,t-5}+\beta_3 IR_{j,t-5}+\beta_4 ME_{j,t-5} \\ & +\beta_5 FE_{j,t-5}+\beta_6 PPPI_{j,t-5}+\alpha_j+\eta_t+u_{jt} \end{aligned}$$

We estimate equations (5) and (6) using a variety of different techniques, which are discussed in the next section before we present the results. Because the number of types used to compute $IOp_{j,t-5}$ varies across countries (but not over time within a country), depending on data availability, all regressions for equation (6) also include a quartic polynomial in the number of types used to estimate inequality of opportunity. This allows us to be very flexible in controlling for the effect of using different partitions across countries.

The availability of household survey micro-data with information on *both* a reliable indicator of well-being (income, consumption, or wealth) *and* circumstance variables—which are required for computing inequality of opportunity measures—is the key factor constraining our sample(s) of countries. The requirement is even more stringent since we need, for each country, at least two comparable surveys five years apart to construct the panel of countries—three when using GMM estimators.[14] As noted earlier, we use two types of household surveys: income or expenditure households surveys (IES) such as labor force surveys, household budget surveys, or Living Standard Measurement Surveys to construct our first sample, and Demographic and Health Surveys (DHS) for the second sample.

The IES sample contains 42 countries, both developed and developing. A large proportion of the surveys comes from three harmonized meta-databases that allow for the construction of comparable measures of household income or consumption. We use the Luxembourg Income Study (LIS) for 23 (mostly developed) countries; the Socioeconomic Database for Latin America and the Caribbean (SEDLAC) for six Latin American countries; and the International Income Distribution Database (I2D2) from the World Bank for another 10 developing economies. For the remaining three countries included in the sample, we use their national household surveys directly. Germany before and after reunification is treated as two separate countries to avoid any spurious change in inequality of opportunity, so the result tables report 43 country observations.

[14]The sample of income and expenditure surveys includes 25 country observations with three periods and 18 countries with only two periods (Table A1). As explained by Roodman (2009a), when running system-GMM without a nonzero instrument for the transformed equation, the estimation is run only on the levels equation. Restricting the difference- and system-GMM estimators to countries with only three observations produces similar results (e.g. negative coefficients on residual inequality), although with implausibly high Hansen statistics (thus pointing towards instrument proliferation).

In each of these surveys, we compute total inequality and inequality of opportunity *among individuals* aged 15 or above, each of whom is assigned the per capita income or expenditure for his or her household. Household-level sampling weights are used to compute the inequality measures. In other words, we compute the inequality of household per capita income (the welfare concept) in a distribution of individuals (the recipient units). For 32 countries, we use household income per capita, while for another 10, where reliable income data are not available, we use household expenditure per capita.[15] Definitions are always consistent across periods within countries and a dummy variable indicating whether the inequality measure is based on expenditure or income is included in the estimation of equations (5) and (6).

In calculating $IOp_{j,t-5}$ for each specific survey, we use a number of circumstance variables to partition the population into types. These circumstance variables refer to individual characteristics, and are classified into two sets. The first set is frequently used in the literature, and it is generally agreed that these variables satisfy the exogeneity requirement for circumstances. They include gender, race or ethnicity, the language spoken at home, religion, caste, nationality of origin, immigration status and region of birth. In the second set, which is used in our main tables, we add the current region of residence for a number of countries where the birth region was unavailable. While migration decisions are obviously very important, region of residence is strongly correlated with birth region, and might thus provide a proxy for the latter. The *IOp* measures used to estimate our main results (in Tables 1 and 3) are based on this second set of circumstances. Results using the first set of circumstances are reported in Table A6 as a robustness check.

Not all circumstance variables are available for all countries, and Table A3 provides more detailed information on the source and date of each household survey, as well as on the welfare and circumstance variables available in each of them, and on the number of types used in the partition for each country. Once again, the circumstance variables and the number of categories for each variable (and consequently the partitions of the population into types) are unchanged over time within countries.[16] Type partitions do vary across countries, however, and, as indicated above, we control flexibly for this by including a quartic polynomial on the number of types in each country in Equation (6). Tables A1 and A2 also show the percentage of total inequality accounted for by inequality of opportunity, i.e. $\frac{IOp_{j,t}}{I(y)_{j,t}}$.

Owing to the difficulties associated with obtaining comparable household income or expenditure surveys at regular five-year intervals for poor nations, rich countries are over-represented in our IES sample. In an attempt to broaden the geographical coverage of the study, we extended our analysis to an additional

[15]Naturally, income and consumption expenditure do not measure the same things. Under certain conditions, consumption expenditures will be nearer a concept of permanent income, for example. However, given the absence of comparable data on incomes—or consumption—exclusively around the world, we follow here the common practice of including inequality measures based on both, and controlling for that in the regressions. For each country-year, we have trimmed the top 0.5% of incomes (or expenditures) to reduce the sensitivity of the GE(2) inequality measure (see Table A5) to such extreme values.

[16]Given that these partitions may contain empty cells, the observed number of types may actually vary slightly over time within countries, but these variations are small in practice.

sample of countries and household surveys, by drawing on the Demographic and Health Surveys (DHS). Our DHS sample contains 42 developing countries from Africa, Asia and Latin America (see Table A4 in the online appendix for details). The earliest survey used is from 1986 and the most recent from 2006. The DHS are designed to provide in-depth information on health, nutrition, and fertility. In addition, the survey includes socioeconomic information on household members and data on access to services. As noted earlier, the DHS does not typically contain estimates of household income or expenditure, so we construct a wealth index as the first principal component of a set of indicators of assets and durable goods owned, dwelling characteristics, and access to basic services. The list of indicators included may vary somewhat from country to country, but we maintain the same set of variables within countries across time.

For all women aged 15 to 49, the DHS collects relatively detailed information on circumstance variables. We define the types based on the following indicators: region of birth, number of siblings, religion, ethnicity, and mother tongue. Mother and father's education are available in some countries for some years, but never for all years, so this variable could not be included in our set of circumstances. Since not all indicators are available in all surveys and the number of categories in each variable also varies, the number of types differs from country to country (but, as for the IES sample, remains the same within countries across time). As for the IES sample, estimates of equation (6) on the DHS sample control flexibly for the number of types used to calculate inequality of opportunity in each country. Details of the DHS dataset are reported in Table A4 in the online appendix.

As noted earlier, our measures of inequality of opportunity are lower-bound estimates of true inequality of opportunity, because not all relevant circumstance variables can be observed. In a recent survey, Brunori *et al*. (2013) report a number of estimates of inequality of opportunity available from various country-level studies. In Figure A1 (in the online appendix) we compare these estimates with our own measures for the IES sample. The range of estimates for the share of inequality of opportunity is very similar, although our estimates tend to be somewhat lower. For example, the cross-country average is 13.1 percent in our sample, compared with 18.0 percent in Brunori *et al*. (2013). Similarly, in 18 of 24 countries that are available in both data sets, the estimate reported in Brunori *et al.* (2013) exceeds our measure. As discussed above, we only include circumstance variables that are available in multiple periods for a given country, while country-level studies need not impose this restriction. This limitation on our set of circumstances likely explains why we obtain somewhat lower levels of inequality of opportunity.

5. Estimation and Results

Equations (5) and (6) can be estimated using a variety of techniques. First, they can be estimated with the classical OLS estimator. However, OLS estimates may be biased due to the fact that the lagged outcome variable can be correlated with the fixed effect in the error term, especially when the number of periods is

TABLE 1

Economic Growth on Total Inequality: Income and expenditure survey sample

	OLS (1)	FE (2)	Long-run-OLS (3)	Difference-GMM (4)	System-GMM (5)	System-GMM Collapsed set of instruments (6)	Difference equation (6a)	Levels equation (6b)
Log initial GDP per capita	−0.005	−0.204***	−0.007	−0.140**	−0.011	−0.035	−0.195***	0.084
	(0.004)	(0.051)	(0.005)	(0.060)	(0.016)	(0.049)	(0.057)	(0.281)
Total inequality (set 2) (lagged)	−0.042*	−0.216**	0.001	−0.156	−0.115	−0.409	−0.305	0.240
	(0.022)	(0.095)	(0.022)	(0.229)	(0.071)	(0.275)	(0.228)	(1.079)
Female secondary education (lagged)	0.050	1.207**	−0.005	1.728	0.040	0.696	1.982**	0.990
	(0.047)	(0.513)	(0.059)	(1.493)	(0.128)	(0.553)	(0.841)	(1.766)
Male secondary education (lagged)	−0.017	−0.990*	0.072	−1.330	0.003	−0.961	−1.501	−1.005
	(0.054)	(0.576)	(0.065)	(1.736)	(0.158)	(0.710)	(0.940)	(1.585)
Price level of investment (lagged)	−0.001***	0.000	−0.000	−0.001	−0.001	−0.001	0.000	−0.009
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.022)
Indicator of income data	−0.015	0.000	−0.020		0.008	0.079		
	(0.010)	(.)	(0.015)		(0.040)	(0.097)		
Constant	0.162***	1.786***	0.111**		0.211*	0.623		
	(0.037)	(0.428)	(0.042)		(0.114)	(0.437)		
Observations	117	117	43	74	117	117	74	117
Countries	43	43		43	43	43	43	43
Instruments				36	55	27	20	11
Hansen				0.842	0.871	0.128		
AR1				0.725	0.0820	0.144		
AR2				0.582	0.604	0.796		
Kleibergen-Papp							0.299	0.663

Notes: Column 5 uses full set of instruments. Columns 6, 6a and 6b use the collapsed instrument set: Column 6 reports the system-GMM estimate, while 6a and 6b estimate the differenced and levels equations separately using two-stage least squares. Two-step GMM estimation method. Standard errors in parentheses. Period dummies not reported. LR-OLS omits period dummies and uses average annual growth over the last decade a particular country is observed for. Education defined as proportion of adult (fe)male population with some secondary education or above. Reporting p-values for Hansen test of overidentifying restrictions, tests for autocorrelation in residuals, and the Kleibergen-Papp rk-LM statistic (H_0: Underidentification).

Sources: Country-specific household surveys, World Development Indicators, Penn World Tables, and Lutz *et al*. (2007, 2010). Inequality indices are constructed using household income or expenditure data.

* $p < 0.1$, ** $p<0.05$, *** $p < 0.01$

small, violating the underlying consistency assumption. Therefore, a second technique to estimate our model is by using a fixed effects (FE) estimator. The OLS and FE estimators are presented in columns (1) and (2) in our main results tables: Tables (1–4). For comparison with other studies on inequality and growth, such

TABLE 2

Economic Growth on Total Inequality: Demographic and Health Survey sample

	OLS (1)	FE (2)	Long-run-OLS (3)	Difference-GMM (4)	System-GMM (5)	System-GMM Collapsed set of instruments (6)	Difference equation (6a)	Levels equation (6b)
Log initial GDP per capita	−0.001	−0.138***	−0.006	−0.172***	−0.007	−0.040	−0.321***	−0.021
	(0.006)	(0.026)	(0.009)	(0.047)	(0.017)	(0.028)	(0.047)	(0.018)
Total inequality (lagged)	−0.001	0.016	−0.006	0.044	0.010	−0.034	0.019	−0.020
	(0.004)	(0.022)	(0.005)	(0.045)	(0.025)	(0.052)	(0.041)	(0.056)
Female secondary education (lagged)	0.053	0.284	−0.178	0.523	0.073	0.433	1.629	0.349
	(0.104)	(0.523)	(0.145)	(0.861)	(0.207)	(0.286)	(1.254)	(0.509)
Male secondary education (lagged)	−0.003	−0.236	0.217*	−0.048	0.018	−0.367	−1.549	−0.306
	(0.083)	(0.468)	(0.118)	(0.911)	(0.160)	(0.368)	(1.404)	(0.601)
Price level of investment (lagged)	−0.000	0.000	0.000	0.000	0.000	−0.000	0.000	−0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Constant	0.011	0.790***	0.085		0.004	0.452		
	(0.057)	(0.202)	(0.061)		(0.200)	(0.425)		
Observations	134	134	42	89	134	134	89	134
Countries	42	42		42	42	42	42	42
Instruments				52	73	29	23	10
Hansen				0.965	0.999	0.359		
AR1				0.421	0.00292	0.0305		
AR2				0.619	0.133	0.515		
Kleibergen-Papp							0.655	0.280

Notes: See Table 1.

Sources: Country-specific household surveys, World Development Indicators, Penn World Tables, and Lutz *et al*. (2007, 2010). Inequality indices are constructed using data from the Demographic and Health Surveys.

* $p < 0.1$, ** $p<0.05$, *** $p < 0.01$

as Marrero and Rodríguez (2013), we also estimate a ten-year OLS, which regresses growth during the latest 10-year period we have in each country on initial conditions at the beginning of that period, excluding the time dummies.[17] These estimates are presented in column (3) of each regression table.

Naturally, the FE estimator does not fully resolve the endogeneity problem. Using within-country variability, the lagged dependent variable and the error term may still be correlated, violating the assumption of independence between the regressors and the error term. Whereas OLS is biased in one direction, the FE estimator is biased in the other direction, meaning that theoretically superior estimates, such as difference- or system-GMM estimators, should lie within or near the range of these estimates (Bond 2002; Roodman 2009a).

[17]We would ideally like to run a long-run OLS, as in Marrero and Rodríguez (2013), examining growth over a longer period of time as a function of initial inequality. However, the durations of long-run periods vary widely in our data. Hence, for consistency, we chose to examine growth during the latest available 10-year period as a function of initial inequality.

TABLE 3

Economic Growth on Inequality of Opportunity and Residual Inequality: Income and expenditure survey sample

	OLS (1)	FE (2)	Long-run-OLS (3)	Difference-GMM (4)	System-GMM (5)	System-GMM Collapsed set of instruments (6)	Difference equation (6a)	Levels equation (6b)
Log initial GDP per capita	−0.003	−0.218***	−0.007	−0.186***	−0.024	−0.020	−0.187***	0.055
	(0.005)	(0.053)	(0.006)	(0.056)	(0.019)	(0.027)	(0.045)	(0.090)
Inequality of Opp. (set 2) (lagged)	−0.089	0.006	−0.103	−0.067	−0.235	−0.638	−0.357	0.849
	(0.071)	(0.210)	(0.096)	(0.353)	(0.236)	(0.556)	(0.394)	(1.956)
Residual inequality (set 2) (lagged)	−0.032	−0.316*	0.058	−0.179	−0.137	−0.231	−0.228	−0.139
	(0.036)	(0.166)	(0.098)	(0.426)	(0.119)	(0.312)	(0.314)	(1.338)
Female secondary education (lagged)	0.062	1.134**	−0.016	2.106	0.078	0.353	1.742**	0.937
	(0.044)	(0.504)	(0.081)	(1.517)	(0.103)	(0.493)	(0.741)	(1.095)
Male secondary education (lagged)	−0.040	−0.933	0.092	−1.922	−0.046	−0.603	−1.262	−1.126
	(0.052)	(0.568)	(0.107)	(1.719)	(0.143)	(0.727)	(0.812)	(1.428)
Price level of investment (lagged)	−0.001**	0.000	−0.000	−0.001	−0.000	−0.001	0.000	−0.007
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)
Indicator of income data	−0.023**	0.000	−0.028*		0.019	0.014		
	(0.011)	(.)	(0.016)		(0.056)	(0.102)		
Constant	0.148***	1.921***	0.094		0.293**	0.466*		
	(0.041)	(0.471)	(0.059)		(0.124)	(0.256)		
Observations	117	117	43	74	117	117	74	117
Countries	43	43		43	43	43	43	43
Instruments				43	64	34	24	14
Hansen				0.773	0.876	0.134		
AR1				0.644	0.0543	0.201		
AR2				0.863	0.543	0.977		
Kleibergen-Papp							0.433	0.238

Notes: See Table 1. Quartic polynomial in the number of types included throughout.

Sources: Country-specific household surveys, World Development Indicators, Penn World Tables, and Lutz *et al.* (2007, 2010). Inequality indices are constructed using household income or expenditure data.

* $p < 0.1$, ** $p<0.05$, *** $p < 0.01$

The obvious way to address the endogeneity problem is to use instrumental variables. To avoid the problem of finding suitable instruments in each case, difference- and system-GMM methods were developed, with which the fixed effects are eliminated and where longer lags of the regressors are available as instruments. The difference-GMM specification, which is based on the first-difference transformation of equations (5) or (6), does exactly this, using twice-lagged level regressors as instruments. However, concern has been expressed, for example, that in a

TABLE 4

Economic Growth on Inequality of Opportunity and Residual Inequality: Demographic and Health Survey sample

						System-GMM Collapsed set of instruments		
	OLS (1)	FE (2)	Long-run-OLS (3)	Difference-GMM (4)	System-GMM (5)	(6)	Difference equation (6a)	Levels equation (6b)
Log initial GDP per capita	−0.003	−0.137***	−0.005	−0.160***	−0.008	−0.021	−0.295***	−0.113
	(0.007)	(0.028)	(0.010)	(0.046)	(0.014)	(0.024)	(0.048)	(0.978)
Inequality of Opp. (lagged)	0.006	0.005	−0.017**	0.051	0.033	0.012	−0.018	−0.013
	(0.007)	(0.040)	(0.008)	(0.060)	(0.027)	(0.051)	(0.081)	(0.296)
Residual inequality (lagged)	−0.001	0.022	−0.001	0.028	0.014	−0.001	0.052	−0.397
	(0.006)	(0.031)	(0.007)	(0.024)	(0.028)	(0.043)	(0.054)	(3.859)
Female secondary education (lagged)	0.047	0.365	−0.176	0.550	−0.086	0.181	1.276	−1.344
	(0.106)	(0.621)	(0.150)	(1.155)	(0.256)	(0.286)	(1.206)	(15.447)
Male secondary education (lagged)	0.001	−0.350	0.232*	−0.250	0.116	−0.128	−1.215	1.703
	(0.098)	(0.546)	(0.133)	(1.108)	(0.226)	(0.340)	(1.235)	(18.137)
Price level of investment (lagged)	−0.000	0.000	0.000	−0.000	−0.000	−0.000	0.000	0.007
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.073)
Constant	0.025	0.808***	0.061		0.015	0.144		
	(0.061)	(0.238)	(0.070)		(0.136)	(0.329)		
Observations	134	134	42	89	134	134	89	134
Countries	42	42		42	42	42	42	42
Instruments				63	88	37	29	14
Hansen				0.978	1.000	0.369		
AR1				0.261	0.00182	0.0125		
AR2				0.384	0.190	0.369		
Kleibergen-Papp							0.532	0.917

Notes: See Table 1. Quartic polynomial in the number of types included throughout.

Sources: Country-specific household surveys, World Development Indicators, Penn World Tables, and Lutz *et al.* (2007, 2010). Inequality indices are constructed using data from the Demographic and Health Surveys.

* $p < 0.1$, ** $p<0.05$, *** $p < 0.01$

context where the time series are persistent and the time dimension is small "the first-differenced GMM estimator is poorly behaved" (Bond *et al.* 2001). In particular, under those circumstances—which evidently apply to the data used in this paper, as well as to the data in Forbes (2000) and most of the cross-country growth literature—the two-period lagged dependent-variable (in levels) used as instrument for the first-differences in the second stage is a weak instrument.

When instruments are weak, large finite sample biases can occur, and these problems have been documented in the context of first-difference GMM models (Blundell and Bond, 1998; Bond *et al.* 2001).

To deal with these issues and increase efficiency, "system-GMM" models, using an additional set of moment restrictions, combine the usual equation in first-differences using lagged levels as instruments, with an additional equation in levels, using lagged first-differences as instruments. According to Blundell and Bond (1998), Blundell *et al.* (2000) and Bond *et al.* (2001), this approach results in substantial reductions in finite-sample biases in Monte-Carlo experiments. Although system-GMM estimation is, for these reasons, now generally preferred to difference-GMMs, it is not problem-free. In particular, Roodman (2009a) urges caution with the effect of instrument proliferation on the Hansen test of joint validity of instruments. Although a significant Hansen statistic suggests that the instrument set is not valid, Roodman points out that implausibly good p-values (very close to 1.0) are telltale signs of the fact that the Hansen test has been weakened to the point of no longer being informative. To limit the number of instruments in GMM estimation, we collapse the instrument set, which makes the instrument count linear in time periods rather than quartic.[18] However, as with any instrumental variable method, the success of system-GMM estimation depends on the strength of the instruments. Following Bazzi and Clemens (2013) and Kraay (2015), we split up the collapsed system-GMM into the differenced and levels equations. We estimate these two equations separately using standard instrumental variables techniques and use the Kleibergen-Papp rk-LM test for under-identification.[19]

To be transparent and thorough in checking the robustness of any finding in our empirical analysis, we present five GMM estimates in each table: Difference-GMM in Column (4), and system-GMM with the full set of available instruments and the collapsed set of instruments in Columns (5)–(6). The (separated) differenced- and levels-equations of the system-GMM with the collapsed instrument set are shown in Columns (6a) and (6b) respectively.[20] All estimates use the two-step system-GMM estimator with standard errors corrected using the Windmeijer (2005) procedure.[21] As the first-difference transformation is affected by gaps in the panel data, orthogonal deviations transformation was used for robustness checks in the DHS dataset, which contains gaps in the panel for three countries. This issue does not affect our findings. We report standard errors clustered at the country level that

[18]The *collapse* option in Stata's *xtabond2* command performs this operation, and the resulting instrument matrix, according to Roodman (2009a), "embodies the same expectation but conveys slightly less information" than the uncollapsed instrument set. Roodman (2009b) suggests that collapsing the instrument set retains more information than merely limiting the use of only certain lags as instruments.

[19]Given that we fail to reject under-identification, we do not test for weak instruments. Confidence intervals which are robust to weak instruments are larger, calling for further caution in interpreting any of the significant coefficients. Kraay (2015) finds that weak instruments are common in cross-country growth regressions using system-GMM estimators.

[20]The differenced-equation of the system-GMM (column 6a), and the difference-GMM (column 4) differ in three ways. Column (4) uses the full set of instruments, the default *xtabond2* specification of the covariance matrix, and a two-step estimator. Column (6a) uses the collapsed instrument set and two-stage least squares. As explained in the help file for *xtabond2*, choosing the one-step estimator and specifying *h(1)* allows for the difference-GMM estimates to be reproduced exactly (results not shown).

[21]The two-step estimator is more efficient asymptotically. The Windmeijer (2005) standard errors lead to more reliable results than the asymptotic standard errors (Bond and Windmeijer, 2002).

are robust to heteroskedasticity and autocorrelation.[22] For each GMM specification, we report the Hansen J-test of instrument validity, and Arellano-Bond (1991) autocorrelation tests. We also report the numbers of observations, countries, instruments, and, when relevant, the p-values of the Kleibergen-Papp rk-LM test.

Turning to our results, we start by discussing the relationship between total inequality and growth (equation 5), presented in Table 1. This helps place our findings in the context of the preceding literature, before we proceed to examine the same relationship for the two distinct components of overall inequality, namely inequality of opportunity and a residual term. Our main empirical specification is identical to Forbes (2000), while Table A5 in the Appendix presents the same regression models without the controls for education and market distortions.

As in Forbes (2000), we find signs of conditional convergence: the sign of the coefficient on initial income is always negative, and it is significant at the 95 percent confidence level or above for two of the six main specifications (columns 1–6).[23] The coefficient estimates for male and female education and the price level of investment are also similar to those in Forbes (2000). When it comes to the conditional correlation between inequality and growth, however, our results diverge: whereas Forbes (2000) reports a coefficient on inequality that is always positive, and significant in four different specifications, our estimates are always negative, and significant at the 90 percent confidence level in two of the six main specifications. The difference-GMM specification in Forbes (2000) (Table 1, column 4) implies a 1.1 percentage-point increase in average growth over the next five-year period for a one standard deviation increase in the initial Gini index (standard deviation of 8.7, own calculations), while the analogous estimate from our study is a statistically insignificant 2.5 percentage-point decrease in growth associated with a one standard deviation increase in the initial mean log deviation (standard deviation of 0.16) (Table 1, column 4).

Two issues are worth additional discussion regarding the findings presented in Table 1. First, regression diagnostics, particularly the Hansen J-test, point towards instrument proliferation when we use the full set of instruments (columns 4 and 5). Collapsing the instrument set produces p-values of the Hansen J-test which are more reasonable (column 6). The Arellano-Bond autocorrelation tests suggest no problems with any of the GMM specifications. However, using the Kleibergen-Papp rk-LM test, we fail to reject the null hypothesis that the system-GMM model is under-identified. In other words, there are not enough instruments to explain the endogenous regressors.

Second, the fact that our findings are not consistent with the findings in Forbes (2000) may reflect differences in the country and period coverage of the two samples: we have 117 observations for 42 countries, whereas Forbes has 135

[22]Of course, for the long-run OLS, which uses a cross-section of countries, one cannot cluster at the country level and we use robust standard errors.

[23]One difference between our empirical specification and Forbes (2000) is the measure of inequality used: we use mean log deviation while Forbes (2000) employs the Gini coefficient available in the Deininger and Squire (1996) dataset. Our findings are not qualitatively different if we use the Gini index instead of mean log deviation. Readers should note, however, that we are not trying to replicate Forbes (2000) here: Since the focus of our paper is as much on inequality of opportunity as it is on overall inequality, the set of countries in our sample is restricted by the availability of data on circumstances.

observations (in the GMM specification) for 45 countries. 24 countries are present in both Forbes's and our IES sample. Periods also differ, ranging from 1961–65 to 1991–95 in Forbes (2000), compared to 1981–85 to 2001–05 in our study.[24] In addition, as noted earlier, not only are the inequality measures used different (Gini vs. mean log deviation), but also our inequality measures arguably satisfy a higher standard of international comparability, since they were all computed under exactly the same criteria and using the same routines directly from the microdata, whereas Forbes (2000) relies on Gini coefficients available in the Deininger and Squire (1996) dataset. Whatever the reasons for the differences, it is fair to conclude that the positive association between inequality and growth found by Forbes (2000) is not robust to changes in country and period coverage, or to seemingly small changes in empirical specifications.

As described in the previous section, the IES dataset is comprised of 23 high-income countries and 19 low- and middle-income (LMIC) countries. In contrast, our DHS dataset is comprised entirely of developing countries from Africa, Asia, and Latin America. Estimates for equation (5) from the DHS sample are presented in Table 2. The findings here are much more equivocal than those presented in Table 1: while there are still signs of conditional convergence, we find no statistically significant coefficient estimates for total inequality (measured by the variance of the wealth index). For the difference-GMM and system-GMM using the full set of instruments, signs of instrument proliferation are apparent: 52 and 73 instruments, respectively, producing unusually high p-values of 0.965 and 0.999 for the Hansen J-test of instrument validity (columns 4 and 5). The Kleibergen-Papp test suggests that the system-GMM with the collapsed instrument set is under-identified. The coefficient estimates, all of which are close to zero and about half of which are negative, suggest no apparent relationship between inequality and growth in this dataset.

Our main interest, however, lies in examining whether and how the association between inequality and growth might change when we decompose overall inequality into the opportunity and residual components, $IOp_{j,\ t-5}$ and $IR_{j,\ t-5}$ respectively, by estimating equation (6). Table 3 reports results from this regression using the IES country sample.[25] We find no consistent relationship between growth and either inequality between types or inequality within types (as proxies for inequality of opportunity and inequality of effort, respectively): While five out of six coefficient estimates for inequality of opportunity are negative, none of them are statistically significant at the 90 percent level of confidence. The conclusion is qualitatively similar using the DHS dataset, although there is one negative and statistically significant coefficient on IOp in column 3 of Table 4. That single significant coefficient, however, is clearly not sufficient to suggest any consistent pattern of a relationship between growth and inequality of opportunity. As in

[24]Clearly, neither sample of countries is representative of the world, since both are driven by survey availability, which is evidently non-random. The difference in the periods covered by the analysis may be material, however. See Scholl and Klasen (2016) for a discussion of how rising inequality and an output collapse during economic transition in Eastern Europe and the former Soviet Union during the late 1980s and early 1990s may drive Forbes's results.

[25]We use the sample that includes region of residence as a circumstance for our default dataset, and total inequality is defined over the observations that have the set 2 circumstances.

Tables 1 and 2, some of the GMM specifications suffer from instrument proliferation which is addressed by using a collapsed set of instruments. However, the collapsed system-GMM is still under-identified.[26] These findings are not supportive of the hypothesis that there might be a negative association between inequality *of opportunity* and growth (and a positive one between the residual inequality and growth), à la Marrero and Rodríguez (2013).

We conducted a series of robustness checks to confirm our finding of null results. First, we estimate the regression models in Equations (5) and (6) without the controls for education and market distortions used in Forbes (2000). This change to our specification, shown in Panels A and B of Table A5 in the online appendix, produces slightly stronger results than those presented in Tables 1 and 3. The negative association between overall inequality and growth is now statistically significant (at 90 percent or higher) across all six main specifications (Panel A). There are also two negative and statistically significant coefficient estimates for inequality of opportunity (in Panel B). Nevertheless, and in light of the results reported elsewhere in the paper, we find it difficult to interpret these unconditional associations as anything other than faintly suggestive.

Second, to address the main concern about the endogeneity of our set of circumstances, we exclude region of residence (and, instead, only utilize region of birth) and rerun the regression models in Equations (5) and (6). The results, shown in Table A6, are broadly similar to those in Table 3. Third, we investigate whether reducing the measurement error that is present in our estimates of inequality of opportunity (because of insufficient numbers of observed circumstance variables) has any effect on results. To this end, we restrict the sample to countries with more circumstance variables, in two ways. We rerun the regression models in Equations (5) and (6) for countries which have (a) at least 100 types (Table A7, Panels A and B); and (b) at least four circumstance variables (Table A7, Panels C and D). While this sample restriction leads to a significant decline in the number of countries included in the analysis, our main findings do not change: some suggestive evidence of a negative correlation between overall inequality and growth persists, but there is no obvious sign of a relationship between inequality of opportunity (or effort) and growth, despite one single statistically significant negative coefficient for the OLS specification in Panel B, column 1.

Finally, we considered the possibility that a top-sensitive measure of inequality, such as GE(2), which is equal to half the squared coefficient of variation, may have a different relationship with growth than the bottom-sensitive GE(0) that we used in our main analysis. The results are shown in Panels C and D of Table A5. The coefficient estimates for total inequality in Panel C should be compared to the original findings in Table 1: there are no significant estimates in this new specification. In Panel D, we present the coefficient estimates for inequality of opportunity and residual inequality, which are analogous to those presented in Table 3 using GE(0): of the six main specifications that we chose to include in our study, we find two statistically significant coefficient estimates for inequality of opportunity (compared with none in Table 3).

[26]As Bazzi and Clemens (2013) point out, high p-values for the Kleibergen-Papp statistic (e.g. Table 4) do not point towards a biased or underpowered test, as is the case for the Hansen J-test.

In two instances, these robustness tests produce results that are slightly more supportive of our initial conjecture. Omitting controls from equations (5) and (6), as shown in Table A5, Panels A and B, suggests a slightly more pervasive negative association between overall inequality and growth, and hints to a negative association between inequality of opportunity and growth. Separately, using a measure of inequality that is more sensitive to high incomes to estimate Equation (6) also generates two negative coefficient estimates for inequality of opportunity which are significant at the 95 percent level of statistical confidence (Table A5, Panel D). It is possible that this might suggest that higher inequality of opportunity may be detrimental for growth, particularly if it is pervasive in the upper tail of the distribution.

Nevertheless, when they are taken together with all of the other results presented in the main Tables (1-4) and in Tables A6 and A7 in the online appendix, these additional specification checks cannot substantively change our overall assessment of null results. On balance, a reading of the econometric evidence presented in this paper does not provide robust support to the hypothesis that inequality of opportunity is negatively associated with subsequent growth in a cross-section of countries.

6. Conclusions

The hypothesis motivating this paper was that the lack of robust conclusions about the association between initial inequality and economic growth in the previous literature might have been driven, at least in part, by the conflation of two substantively different kinds of inequality into conventional income inequality measures: inequality of opportunities and inequality driven by effort. Because effort is notoriously difficult to measure, we have followed the recent literature on the measurement of ex-ante inequality of opportunity, and decomposed overall income inequality into a component associated with opportunities, and a residual component, driven by effort as well as omitted circumstances.

These decompositions were carried out for the mean logarithmic deviation and for the Generalized Entropy ($\alpha = 2$) measure of household per capita incomes or expenditures in 117 household surveys for 42 countries, and for the variance of a wealth index obtained from Demographic and Household Surveys in 134 surveys for 42 countries. The resulting indices of inequality of opportunity and residual inequality were then included as explanatory variables in growth regressions that also included measures of male and female human capital investment and a measure of investment price distortion, following the specification in Forbes (2000). The same regressions were run for the overall income inequality measure (with no decomposition). The two country-level samples were unbalanced panels with a preferred time dimension of three periods and we relied on OLS, fixed effects, long-run OLS, and various Generalized Method of Moments specifications for estimation.

Taken together, our main findings are such that we cannot reject the null hypothesis that there is no relationship between initial inequality and subsequent growth. Using the income and expenditure surveys dataset and the mean log deviation as our measure of overall inequality, there is weak suggestive evidence of a negative association between overall income inequality and subsequent growth, both when controls are included and when they are not. However, this negative association is not robust to changing the inequality measure (to GE(2)), or to estimation in the alternative DHS sample. Although these results are not supportive of the negative association between overall inequality and growth that had been hypothesized by an earlier literature, it is important to note that neither do they support the positive association found by Forbes (2000) and Li and Zou (1998).

Furthermore, we find no stable evidence for our main motivating hypothesis of heterogeneous effects of inequality on growth, which found some support in a dataset of 26 U.S. states (Marrero and Rodríguez, 2013): there is no apparent relationship between either component of inequality and growth in either of our two datasets.

Our study has a number of limitations. *First*, as argued by Banerjee and Duflo (2003), it may be difficult to interpret any of this kind of econometric evidence causally, insofar as variations in inequality are likely to be correlated with various unobservable factors that are also associated with economic growth. Readers are cautioned to interpret any correlations presented in this paper—or indeed, their absence—as only suggestive of causal relationships. *Second*, while we present a large number of specifications used in the literature before us and conduct a number of robustness checks of the regression models, the possibility of specification error remains.

Third, measurement error is an issue that pervades our study. Not only under-reporting and differential non-response in household surveys may be correlated with growth, but they may also be correlated with circumstances, all of which make our inequality measures very noisy. Furthermore, as noted in Section 4, the limited availability of circumstance variables for most countries led us to use measures of inequality of opportunity based on very sparse partitions of types, which arguably produced substantial underestimates of true inequality of opportunity. This kind of measurement error would certainly be consistent with substantial amounts of inequality of opportunity (due to omitted circumstances) contaminating the residual component, leading to biased coefficients. We therefore suggest a reading of our work that is exploratory, i.e. searching for a signal in very noisy data, rather than a reliable test of our hypotheses regarding the effects of inequality of opportunity and effort on subsequent growth.

Perhaps, one conclusion that we can take away is that our findings are inconsistent with a number of similar studies that precede this paper. As noted above, our findings on the relationship between overall inequality and growth differ, for example, from Forbes (2000). Both the IES and the DHS datasets are the most comprehensive cross-country datasets put together specifically for this purpose—products of thousands of hours of meticulous data work.[27] It would be hard to argue that the income inequality data we use are much more problematic than other available datasets. The differences between our analysis and that in Forbes (2000) are in the coverage of countries and time periods (and in the specific inequality measure used as a dependent variable).

[27]In fact, one tangible thing that can be taken away from this endeavor is the public dataset. Our aim is to make these datasets available online as soon as possible, but interested researchers can request these data from the authors in the meantime.

Nor can we provide support with any confidence to the findings in Marrero and Rodríguez (2013) regarding the relationship between inequality of opportunity and growth in the U.S. It is again hard to argue that our inequality of opportunity measure is clearly inferior to that used by Marrero and Rodríguez (2013), who use only two circumstances: father's education and race, which explain approximately 5 percent of overall inequality in their sample of 26 states. In contrast, inequality of opportunity accounts for an average of between 4.0 and 14.8 percent of total inequality in each of the five time periods in our dataset (Table A1, bottom row). The best explanation might be that any relationship between inequality of opportunity and growth is *not robust* to the set of countries and/or the time period included in the analysis.

A methodological issue we would like to highlight is the evident instability of coefficient estimates and regression diagnostics to minor changes in the estimation procedures. It does appear, at least in our data, that GMM methods in particular are very sensitive to the myriad of choices that need to be made by the researcher. Small changes not only move coefficient estimates around, but also render instrument sets invalid or uninformative in many instances. Furthermore, most of our preferred specifications of the system-GMM estimator are underidentified. Although we have diligently combed the latest literature on GMM estimation techniques and closely adhered to the recommendations regarding robustness checks and detailed reporting in Roodman (2009a), examining our results does not suggest that these econometric techniques are reliable strategies for addressing the question at hand.

Similar (or more serious) data and econometric issues have also affected previous studies, and the instability of results between the three "phases" of the empirical cross-country literature reviewed in Section 2 is quite similar to the lack of robustness that we have encountered in our two datasets. A review of that literature suggests that, in retrospect, perhaps each study drew firmer conclusions than was actually warranted. We are not confident that the latest crop of papers—including Ostry *et al.* (2014), that relies on the SWIID data from Solt (2000)—will prove to be immune from this trend. The lack of robustness in our own study may reflect additional factors, such as unusually large measurement error in the inequality of opportunity variable, but it also arises from data and methodological problems that have plagued the literature at large (for a recent discussion of these methodological issues, see Kraay, 2015). Perhaps the main conclusion we draw from our null results is that considerable circumspection is in order when interpreting findings from any single cross-country study of the relationship between inequality and growth.

If the best available cross-country datasets and the best available econometric techniques do not appear suitable to answering this important question that has been, and continues to be, the subject of considerable debate, then what is? Taking advantage of case studies and natural experiments may be one promising avenue. Policymakers often target certain interventions to disadvantaged groups in a deliberate attempt to reduce future inequality of opportunity: anti-discrimination laws against minorities; early childhood interventions for certain ethnic groups; schooling and mentoring programs for adolescent girls; interventions that give voice and increase the participation of oppressed groups are all examples of such interventions. To the extent that such interventions cause strong changes in measurable inequality of opportunity (and satisfy exclusion restrictions), they can be used as instruments to study the relationship between inequality of opportunity and subsequent growth. In cases where one country, or subnational unit, implements a novel program with plausible effects on reducing inequality of opportunity, recent causal inference methods, such as synthetic controls (Abadie *et al.*, 2010), can be utilized. One could even imagine long-term randomized controlled trials. Natural experiments and other causal inference methods relying on interesting cases around the world may end up providing more fruitful avenues for studying this important question than using cross-country regressions.

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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

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