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Abstract: Income differences arise from many sources. While some kinds of inequality, caused by differences in 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 118 income and expenditure household surveys and 134 Demographic and Health Surveys, to revisit the relationship between total inequality and economic growth – particularly 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.

Keywords: inequality, inequality of opportunity, economic growth

JEL codes: D31, D63, O40

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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).

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 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).

[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)

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 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 118 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 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).

However, 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 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 data sets, which make use of 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 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 Unites States (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.[3] These papers 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), 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.[4]

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

[3] See Atkinson (1997).

[4] See Loury (1981) for a precursor.

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 increase 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.[5]

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 optimal, and this may benefit poorer or richer groups, depending on their relative productivity levels in economic and political activities.[6]

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.[7] 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% 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 they compiled, report the same basic finding of a negative effect of initial inequality on growth.

[5] 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).
[6] The theoretical literature on the links between inequality and growth has been extensively reviewed, and we do not attempt to review it comprehensively here again. For some of the best surveys, see Aghion et al. (1999), Bertola (2000) and Voitchovsky (2009).
[7] 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.

This Deininger and Squire (1996) dataset, introduced in the late 1990s, 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 on a panel, rather than a cross-section, of countries – ushering in "Phase 2" of the 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 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 that, over the long term, agricultural endowments predict inequality, and inequality in turn affects institutional development and ultimately growth.[8] 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.

[8] Engerman and Sokoloff (1997) originally formulated this hypothesis in these terms.

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.[9] Ostry et al. (2014) investigate a recent data set – 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.[10] Amongst OECD countries, Cingano (2014) also finds a negative effect of net inequality on growth. 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 come 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 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 mis-specified, and one ought to distinguish between the two kinds of inequality. Marrero and Rodriguez (2013) do this for 26 states of the United States: they decompose a Theil (L) index into a component associated with inequality of opportunity and another, which 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 Rodriguez (2013) is the only published paper that investigates whether inequality of opportunity is the "active ingredient" in the relationship between inequality and growth.[11] Their findings suggest that this component of inequality was negatively associated with economic growth in the United States in the 1970-2000 period. Is this a more general result? Can the same be said of other places and contexts? In particular, can a decomposition of inequality into an opportunity and a residual

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[9] "Phase 3" also saw the emergence of studies using variation in inequality within countries. For example, consistent with the pivotal voter model of Bénabou (2000) and Ferreira (2001), Araujo et al. (2008) finds that more unequal communities in Ecuador are less likely to receive Social Fund investment projects that provide private goods to the poor – with the effect being strongest for expenditure shares at the top of the distribution.

[10] The data set 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 "made-up data" makes the results in this paper suspect, at least until considerable additional validation can be carried out.

[11] But see Teyssier (2013) for an attempt to replicate Marrero and Rodriguez's approach to the case of Brazil, finding opposite results: no effects of inequality of opportunity (or effort) on state-level growth rates.

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 investigate the effects of fiscal systems – broadly the size and incidence of taxes and transfers – on inequality of opportunity in eleven (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 $i \in \{1, \dots, N\}$. 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 *J* elements, all of which are discrete with a finite number of categories, x_j, $j = 1, \dots, J$. Following Roemer (1998), the elements of $\boldsymbol{C_i}$ are referred to as circumstance variables.

Define a partition of the population $\Pi = \{T_1, T_2, \dots, T_K\}$, such that $T_1 \cup T_2 \cup \dots \cup T_K = \{1, \dots, N\}$, $T_l \cap T_k = \varnothing, \forall l, k$, 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 $\overline{K} = \prod_{j=1}^{J} x_j$.[12]

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 type, assigning each individual the value of his or

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

her type's opportunity set, and computing the inequality in that distribution.[13] 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:

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

Using the superscript *k* to indicate the type to which individual *i* belongs, a typical element of the income vector ***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, $\forall i, k$.[14] 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. 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 empirical measure of inequality of opportunity used in the income and expenditure survey sample in Section 5 below.

The mean log deviation is not, however, suitable for use in the Demographic and Health Survey sample. As discussed in the next section, the DHS surveys do not contain credible measures of income or consumption. It does 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 ***C*** as our measure of inequality of opportunity. The essence of the rationale for this choice of measure is as follows.[15] We tend to think of advantage (in this case the wealth index *w*) as a function of circumstances, efforts, and possibly some random factor *u*:

$$w = f(C, E, u) \quad (2)$$

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:

$$E = g(C, v) \quad (3)$$

[13] 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 (2012) for a discussion of both approaches.

[14] See Foster and Shneyerov (2000).

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

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, $\{\widehat{w}\}$ - where $\widehat{w} = C\widehat{\psi}$ - is a parametric equivalent to the smoothed distribution $\{\mu_i^k\}$ previously described. It is a distribution where individual values of the wealth index have been 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: $V(\{\widehat{w}\})$.

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 J, then both the non-parametric index $I(\{\mu_i^k\})$ and the parametric measure $V(\{\widehat{w}\})$ 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 do know the direction of bias – downward – for the overall measures of inequality of opportunity, however, which is why they are lower-bound estimators.

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 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):

$$g_{it} = \beta_1 y_{i,t-5} + \beta_2 I(y)_{i,t-5} + \beta_3 ME_{i,t-5} + \beta_4 FE_{i,t-5} + \beta_5 PPPI_{i,t-5} + \alpha_i + \eta_t + u_{it} \tag{5}$$

We estimate equation (5) (and equation (6), which replaces overall inequality with inequality of opportunity and a residual component, described below) in two panel data sets: one consisting of income and expenditure surveys (IES), and another comprised of DHS surveys. These data sets are described in detail below. In both data sets, the dependent variable, g_{it}, is the average annual growth rate of per capita gross national income in a five-year interval. The data comes from the World Bank's World Development Indicators data set, from which we also obtain the (five-year) lagged national income per capita, $y_{i,t-5}$, expressed in constant 2005 US dollars.[16] $I(y)_{i,t-5}$ – our measure of overall inequality – is

[16] 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.

the key variable that varies between the two samples[17]: in the IES sample, it denotes the mean logarithmic deviation of incomes (or expenditures) at the beginning of the five-year interval. In the DHS sample, it denotes the (overall) variance of the asset index ($V(w)$), also 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-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 1 (Income and Expenditure Surveys) and Table 2 (Demographic and Health Surveys).

Female and male education data ($ME_{i,t-5}$ and $FE_{i,t-5}$) 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.[18] These data are in the spirit of Barro and Lee (2001), although the method used to complete missing data differs slightly.[19] Finally, as in Forbes, market distortions are proxied by the price level of investment from Penn World Tables (version 6.3), defined as the purchasing power parity of investment/exchange rate ($PPPI_{i,t-5}$). α_i denotes country *i*'s fixed effect, η_t is a period dummy, and u_{it} is the error term.

Equation (5) provides estimates for the effect of total inequality on growth à la Forbes (2000). However, we are interested in whether the two components of overall inequality – namely inequality between morally irrelevant groups and inequality within them, interpreted as proxies for inequality of opportunity and inequality due to effort – have heterogeneous effects on growth. Therefore, in equation (6), we re-estimate equation (5) but replacing $I(y)_{i,t-5}$ with our measures of inequality of opportunity: $I(\{\mu_i^k\})$ in the IES sample, and $V(\{\widehat{w}\})$ in the DHS sample. For simplicity, we denote both of these as $IOp_{i,t-5}$ in the generic specification. We also include the residual term, $IR_{i,t-5} = I(y)_{i,t-5} - IOp_{i,t-5}$, and estimate:

$$g_{it} = \beta_1 y_{i,t-5} + \beta_2 IOp_{i,t-5} + \beta_3 IR_{i,t-5} + \beta_4 ME_{i,t-5} + \beta_5 FE_{i,t-5} + \beta_6 PPPI_{i,t-5} + \alpha_i + \eta_t + u_{it} \quad (6)$$

We estimate equations (5) and (6) using a variety of different techniques, which are discussed in the next section before we present the results. All regressions for equation (6) include a quartic in the number of types used to estimate inequality of opportunity. In the remainder of this section, we briefly describe the microdata sets used to compute the inequality and inequality of opportunity variables. Tables 1 and 2 also show the percentage of total inequality accounted for by inequality of opportunity.

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[17] To be precise, we divide the survey years into five-year bins. For example, the value of inequality of opportunity in 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 A1 and A2 for details.

[18] 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.

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

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. 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.[20] For a large proportion of the countries, we use 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 from the World Bank (I2D2) for another 10 developing economies. For the remaining three countries included in the sample, we use the respective national household surveys. The advantage variable used to compute total inequality and inequality of opportunity is always a measure of household wellbeing. For 32 countries, it is net household income per capita, while for another ten, where reliable income data are not available, it is household expenditure per capita. 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.

We use a number of circumstance variables, referring to the characteristics of the household head, to partition the population into types. We classify circumstances 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.[21] In the second set, which is used in our main tables, we add the current region of residence for those 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, which is unavailable in many surveys. Table A1 provides more detailed information on the source and years of the household survey, the welfare and circumstance variables and the number of types in the partition for each country. Once again, the circumstance variables and the number of categories for each variable are unchanged over time within countries.

[20] Note that we treat Germany before and after reunification as two separate countries to avoid any spurious change in inequality of opportunity, so the result tables report 43 country observations.

[21] It is clear that not all of these characteristics satisfy the criteria to be considered 'circumstances'. For example, gender of the head of the household could be a choice or a circumstance. However, the gender of the head of household does explain a non-negligible part of overall inequality in many countries and, hence, presents a trade-off with respect to its exclusion. Given the limited number of circumstance variables available to us and to avoid further underestimation of inequality of opportunity, we chose to include gender among our set of circumstance variables. Immigration status is also clearly a choice variable, but its inclusion has little effect on our empirical analysis, as this information is only available in a few IES data sets (see Table A1).

Unlike most studies of inequality of opportunity undertaken within specific countries, we were unable to draw on a richer set of circumstance variables including father's and/or mother's education and occupation and region of birth, in addition to race or language spoken at home.[22] Since these family background variables have typically been found to account for a substantial share of the between-type inequality in other studies, we anticipate the cost of having to rely on a "lowest common denominator" circumstance vector in our panel cross-country analysis to be non-trivial. Naturally, a higher dimension (J) for the circumstance vector (C) allows the analyst to better capture the possible sources of inequality of opportunity. Although the resulting measure, $I(\{\mu_i^k\})$, is still a lower bound on actual inequality of opportunity, as noted earlier, fewer omitted circumstances is likely to mean a smaller underestimation.

In an attempt to address this problem, we extended our analysis to an additional sample of countries and household surveys, by drawing on the Demographic and Health Surveys (DHS), where additional circumstance variables were available. The DHS sample contains 42 developing countries from Africa, Asia and Latin America (see table A2 in the 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 of household members and 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 on 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's 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, again, remains the same within countries across time). Details of the DHS data set are also reported in Table A2.

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, the OLS can suffer from biased coefficient estimates due to the fact that the lagged outcome variable can be correlated with the fixed effect in the error term, especially when T is small, violating the underlying consistency assumption for OLS. 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 Tables 3-6. For comparison with other studies on inequality and growth, such as Marrero and Rodríguez (2013), we also estimate a ten-year OLS, which regresses growth during the latest

[22] When the advantage variable is individual earnings, rather than household income or expenditure, gender is typically also included. The resulting partitions typically contain larger numbers of types: 72 in Checchi and Peragine (2010) and Belhaj-Hassine (2012), 54 or 108 in Ferreira and Gignoux (2011), and so on.

10-year period we have in each country on initial conditions at the beginning of that period, excluding the time dummies.[23] These estimates are presented in column (3) of each regression table.

However, the FE estimator does not solve the endogeneity problem. Using the within-country variability, the lagged dependent variable and the error term are still correlated, violating the assumption of independence between the regressors and the error term. Whereas the 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).

The obvious way to solve 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. Difference-GMM, the first difference transformation of equations (5) or (6), does exactly this. However, considerable concern has been expressed, for example, that in a 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, in Forbes (2000), and most of the cross-country growth literature - the two-period lagged dependent-variable (in levels) used as instruments for the first-differences in the second stage are weak instruments. 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 (of or 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 T rather than quartic.[24] However, as with any instrumental variable method, the success of the system GMM estimation depends on the strength of the instruments. Following Bazzi and Clemens (2013), we split up the collapsed system GMM into the differenced and levels equations. We estimate

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

[24] The *collapse* option in Stata's *xtabond2* command performs this, 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 still retains more information than limiting the use of only certain lags as instruments rather than the full set of available lags.

these two equations separately using standard instrumental variables techniques and use the Kleibergen-Papp rk-LM test for under-identification.[25]

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), system-GMM with the full set of available instruments and the collapsed set of instruments in Columns (5)-(6). The differenced- and levels-equations of the system GMM with the collapsed instrument set are shown in Columns (7) and (8) respectively.[26] All estimates use the two-step System-GMM estimator with standard errors corrected using the Windmeijer (2005) procedure.[27] As the first-difference transform is affected by gaps in the panel data, orthogonal deviations transformation was used for robustness checks in the DHS data set, which contains gaps in the panel for three countries. This issue does not affect our findings.[28] We report standard errors clustered at the country level that are robust to heteroskedasticity and autocorrelation.[29] 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.

We start by discussing the relationship between total inequality and growth (equation 5), presented in Table 3. This helps place our findings in the preceding literature by allowing comparisons with previous findings before we proceed to examine the same relationship for the two distinct components of overall inequality – namely inequality of opportunity and a residual term (a proxy for inequality due to effort). Our main empirical specification is identical to Forbes (2000), while Table A3 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 significant at the 95% level or confidence or above for two of the six main specifications.[30] 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

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[25] 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.

[26] The differenced-equation of the system GMM (column 7), 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 (7) 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), the Difference-GMM can be reproduced exactly (results not shown).

[27] 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).

[28] Results are available from the authors upon request. The IES data set contains no gaps.

[29] 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.

[30] The only 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) data set. Our findings are not qualitatively different if we use the Gini index instead of Theil (L). 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.

significant at the 90% confidence level in two of the six main specifications. The difference-GMM specification in Forbes (2000) (Table 3, column 4) implies a 1.3% increase in average growth over the next five-year period for a 10-point increase in the initial Gini coefficient, while the same estimate from our study is a statistically insignificant 1.3% decrease associated with a 10-point increase in initial mean log deviation (Table 3, column 4).[31]

Two issues are worth additional discussion regarding the findings presented in Table 3. 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 Hanse J-test which are more reasonable (column 6), while 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 118 observations for 42 countries, whereas Forbes has 135 observations (in the GMM specification) for 45 countries. 24 countries are present in both Forbes's and our IES sample. Periods also differ, with these ranging from 1961-65 to 1986-1990 in Forbes (2000), compared to 1981-1985 to 2001-2005 in our study.[32] In addition, as noted earlier, not only the inequality measures used are different (Gini vs. Theil (L)), 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) relied on Gini coefficients available in the Deininger and Squire (1996) data set. Whatever the reasons for the differences, it is fair to conclude that the relationship between inequality and growth is not robust to changes in either data sources/periods or seemingly small changes in empirical specifications.

As described in the previous section, the IES data set is comprised of 23 high-income countries and 19 low- and middle-income (LMIC) countries. In contrast, our DHS data set is comprised entirely of developing countries from Africa, Asia, and Latin America. Although we constructed our DHS data set because of its perceived advantage in containing more observed circumstance variables, it is still interesting to examine the overall inequality-growth relationship in that data set, which we present in Table 4. The findings here are much more equivocal than those presented in Table 3: 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 & 5). The Kleibergen-Papp test suggests that the system GMM with the collapsed instrument set is under-identified.

[31] It's worth noting that our difference-GMM estimate suffers from low power and the 95% confidence interval does not exclude a 1.3% increase.

[32] Clearly, neither sample of countries is representative of the world, since they are driven entirely by survey availability, which is evidently non-random. Although our sample covers fewer countries, it has slightly broader regional coverage, including two countries from Africa.

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 data set.

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, $IOp_{i,t-5}$ and $IR_{i,t-5}$ respectively, by estimating equation (6). Table 5 reports results from this regression using the IES country sample.[33] 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 all six coefficient estimates for inequality of opportunity are negative, none of them are statistically significant at the 90% level of confidence. The conclusion is qualitatively the same using the DHS data set: one cannot detect any consistent pattern of a relationship between growth and inequality of opportunity (Table 6). As in Tables 3 & 4, 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.[34] These findings are clearly 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 considered the possibility that these findings might be driven by measurement error. As noted in Section 4, the need for (rough) comparability of circumstance sets across countries led us to use a measure of inequality of opportunity based on a very sparse partition of types. Like other examples of this method, the measure used in the regressions reported in Tables 5 and 6 is a lower-bound indicator. But given the paucity of types, it is arguably a very substantial underestimate of true inequality of opportunity: On average across all the countries and years, our circumstances explain 11.6% of total inequality in the IES and 15.7% in the DHS data sets (See Tables 1 and 2 for details). While it is obviously not the only possible cause, 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. The negative coefficient estimates for both the within- and between-type inequality in Table 5 is suggestive of this possibility.

6. Conclusions

In this study, our motivating hypothesis 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 different kinds of inequality into the conventional income inequality measure: 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

[33] We use the sample that includes region of residence as a circumstance for our default data set, and total inequality is defined over the observations that have the set 2 circumstances. While region of residence is not exogenous, region of birth is missing in many data sets, causing significant underestimation of inequality of opportunity by excluding an important circumstance. Given this tradeoff between underestimation and endogeneity, we report the findings using the data set that excludes region of residence and only utilizes region of birth in Table A4.

[34] As Bazzi and Clemens (2013) point out, high p-values for the Kleibergen-Papp statistic (e.g. Table 6) do not point towards a biased or underpowered test, as is the case for the Hansen J-test.

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 of household per capita incomes (or expenditures) in 118 household income and expenditure 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 minimum time dimension of three periods and we relied on OLS, fixed effects, long-run OLS, and various Generalized Method of Moments specifications for estimation.

Our main findings are such that we cannot reject the null hypotheses that there is no relationship between initial inequality and subsequent growth. Using a data set of income and expenditure surveys and the mean log deviation of income (or expenditure) as our measure of overall inequality, there is very weak suggestive evidence of a negative association between overall income inequality and subsequent growth, which is neither robust to changes in specification nor to switching to the DHS data set. These findings do not support the positive association between total inequality and growth found in earlier studies by Forbes (2000) and Li and Zou (1998). Furthermore, we find no evidence for our motivating hypothesis of heterogeneous effects of inequality on growth, which found some support in a data set 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 data sets.

What can we take away from these null results, if anything? It would be hard to argue that the data we use is much more problematic than other available data sets. Both the IES and the DHS data sets are the most comprehensive cross-country data sets put together specifically for this purpose – products of thousands of hours of very meticulous data work.[35] The only 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). As both studies are equally opportunistic in using the best data available at the time, it is hard to argue that the findings in one should be preferred to the other. The best explanation might be that any relationship between inequality and growth is not robust to the set of countries and/or the time period included in the analysis.

It is harder to argue that our data are ideal for the construction of types needed to build a measure of inequality of opportunity. While the numbers of variables we use to construct types in our data sets are large (see Tables A1 and A2 for details), circumstance variables that are consistently available within a country over time are limited. There is little doubt that the resulting estimates of inequality of opportunity must be substantially lower than the actual measures that remain unobserved. This in turn implies that

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

the residual inequality term is contaminated with (unobserved) inequality of opportunity rather than being purely a measure of inequality due to variation in effort within types.

It is again hard to argue that the resulting measure is inferior or superior to that used in other studies. For example, in the only study examining the same question in the United States, Marrero and Rodríguez (2013) use only two circumstances: father's education and race, which explain approximately 5% of overall inequality in their sample of 26 states. Our data sets include these circumstances, along with other circumstances, but not consistently for all countries in all years, causing them to be left out of type definitions in many countries. There are many differences between these studies and all we can say is that the hypothesis of heterogeneous effects of inequality on growth finds support in their study but not here.

Another issue that needs to be highlighted here 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 sets, that GMM methods in particular are very sensitive to the myriad of choices that need to be made by the researcher. Simple 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 under-identified. 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 in 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 same 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 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. One 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 data sets 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 such promising avenue. Every time policymakers target certain interventions to disadvantaged groups, they 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 one region/state/district within a country, implemented a novel policy with program with plausible effects on reducing inequality of opportunity, recent causal inference methods, such as synthetic controls (Abadie, Diamond, and Hainmueller, 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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Table 1. Summary statistics for Income and Expenditure Surveys

Country	Average annual growth rate of per capita GNI in *next 5 years*					GNI per capita in constant 2005 USD (in natural logarithm)					Total inequality (set 2) (MLD)					Ratio of inequality of opportunity (set 2) to total inequality (set 2) (in percent)				
	1985	1990	1995	2000	2005	1985	1990	1995	2000	2005	1985	1990	1995	2000	2005	1985	1990	1995	2000	2005
Australia	0.043	-0.003	0.001			9.9	10.1	10.1			0.2	0.2	0.21			1.8	1.9	1.0		
Austria			-0.028	0.048	0.028			10.4	10.3	10.5			0.27	0.15	0.15			4.0	3.9	4.6
Bangladesh			0.018	0.019	0.058			6.0	6.1	6.2			0.13	0.19	0.18			0.2	0.4	0.4
Belgium			-0.025	0.050				10.4	10.3				0.19	0.24				1.7	1.3	
Belize			0.039	0.024				8.2	8.4				0.74	0.54				9.4	10.9	
Bolivia				-0.019	0.094				7.0	6.9				0.98	0.7				39.5	28.2
Brazil			-0.010	-0.018	0.156			8.4	8.4	8.3			0.71	0.71	0.64			14.8	14.8	14.4
Bulgaria			0.019	0.139				7.4	7.5				0.15	0.19				18.8	34.0	
Canada	0.049	-0.023	0.001	0.057	0.034	10.0	10.2	10.1	10.1	10.4	0.24	0.17	0.17	0.21	0.21	6.0	4.4	3.8	3.1	4.7
Chile				0.025	0.089				8.6	8.7				0.54	0.54				4.1	4.9
Colombia				0.022	0.105				7.9	8.0				0.58	0.59				22.4	17.8
Czech Rep.				0.041	0.021				9.3	9.5				0.12	0.12				4.6	5.9
Denmark		0.034	-0.019	0.061	0.022		10.4	10.6	10.5	10.8		0.18	0.1	0.1	0.11		5.9	7.6	7.7	8.0
Estonia				0.073	0.001				8.9	9.2				0.25	0.21				11.2	12.9
Finland			0.019	0.060	0.021			10.2	10.3	10.6			0.09	0.12	0.13			5.2	4.9	4.3
France			-0.023	0.049	0.020			10.3	10.2	10.5			0.18	0.16	0.17			9.3	7.9	6.3
Germany			-0.041	0.040	0.025			10.5	10.3	10.5			0.15	0.14	0.16			9.2	7.8	7.9
Ghana			-0.039	0.042	0.178			6.1	6.0	6.2			0.26	0.3	0.34			15.8	29.2	25.5
Greece			-0.005	0.085	0.023			9.6	9.6	10.0			0.32	0.24	0.21			5.7	11.9	5.2
Guyana			0.021	0.016				6.8	6.9				0.46	0.51				12.3	14.0	
Hungary			0.000	0.137	0.028			8.6	8.6	9.2			0.21	0.15	0.17			9.5	16.5	14.7
India			0.017	0.076	0.092			6.1	6.2	6.6			0.18	0.17	0.22			20.5	26.3	25.1
Ireland			0.045	0.106	-0.028			10.0	10.3	10.8			0.24	0.19	0.19			4.8	5.1	6.6
Israel		0.028	0.031	0.002	0.034		9.6	9.8	9.9	9.9		0.22	0.21	0.25	0.3		9.1	10.0	10.8	15.1
Italy		-0.004	-0.005	0.054	0.009		10.1	10.1	10.1	10.3		0.18	0.25	0.35	0.25		19.4	19.7	24.0	18.0
Kyrgyzstan				0.072	0.106				5.8	6.1				0.24	0.13				40.3	22.3
Luxembourg			-0.014	0.069	-0.012			10.9	10.8	11.1			0.11	0.14	0.16			4.8	3.5	7.0
Nicaragua				0.015	0.023				7.0	7.1				0.65	0.49				8.1	6.5
Norway				0.089	0.046				10.6	11.1				0.15	0.18				7.0	4.5
Panama				0.019	0.086				8.3	8.4				0.81	0.83				33.5	36.7
Paraguay				-0.045	0.150				7.3	7.1				0.57	0.47				24.4	18.5
Peru				0.032	0.093				7.7	7.9				0.5	0.42				15.8	16.6
Poland				0.068	0.088				8.6	8.9				0.18	0.22				12.1	11.2
Romania			0.011	0.145	0.124			7.5	7.6	8.3			0.3	0.14	0.13			16.9	13.7	8.1
Russia			-0.104	0.169	0.142			8.1	7.6	8.4			0.4	0.44	0.31			2.1	1.2	11.9
Rwanda				0.001	0.116				5.6	5.6				0.71	0.6				36.5	25.4

Spain			-0.005	0.077	0.023			9.8	9.8	10.1			0.27	0.21	0.24			7.5	9.7	6.2
Sweden			0.003	0.052	0.015			10.4	10.4	10.7			0.12	0.14	0.12			4.0	4.2	4.2
Switzerland			-0.026	0.047	0.027			10.9	10.7	11.0			0.23	0.19	0.16			7.7	3.4	4.9
UK			0.037	0.060	-0.024			10.1	10.3	10.6			0.22	0.23	0.24			4.5	6.0	6.0
USA	0.034	0.014	0.026	0.027	-0.008	10.2	10.4	10.5	10.6	10.7	0.25	0.26	0.28	0.32	0.35	10.4	10.4	9.3	8.8	8.7
Vietnam			0.070	0.083	0.106			5.8	6.1	6.5			0.11	0.16	0.17			19.6	26.1	26.1
West Germany	0.124	0.041				9.6	10.3				0.15	0.14				3.8	4.8			
Total	**0.062**	**0.012**	**0.000**	**0.053**	**0.057**	**9.9**	**10.2**	**9.1**	**8.7**	**9.0**	**0.21**	**0.19**	**0.25**	**0.32**	**0.29**	**5.5**	**8.0**	**9.0**	**13.9**	**12.3**

Notes: The summary statistics correspond to the data used in the regressions. In every country, total inequality (set 2) is defined over the observations that have the set 2 circumstances. The last five columns show the ratio of inequality of opportunity (set 2) to total inequality (set 2). Further summary statistics are available from the authors.

Table 2. Summary statistics for Demographic and Health Surveys

Country	Average annual growth rate of per capita GNI in *next 5 years*					GNI per capita in constant 2005 USD (in natural logarithm)					Total inequality (Variance)					Ratio of inequality of opportunity to total inequality (in percent)				
	1985	1990	1995	2000	2005	1985	1990	1995	2000	2005	1985	1990	1995	2000	2005	1985	1990	1995	2000	2005
Armenia				0.178	0.099				6.7	7.6				3.01	2.99				1.6	0.2
Bangladesh		0.004	0.005	0.026	0.074		6.0	6.0	6.0	6.2		3.67	3.66	3.47	3.07		0.3	0.9	0.0	0.1
Benin			-0.006	0.064	0.033			6.0	6.0	6.3			4.97	5.42	5.00			32.3	30.3	32.6
Bolivia			-0.009	0.005	0.100			7.0	6.9	7.0			4.00	4.16	3.76			38.6	35.6	31.0
Brazil	0.061	0.067	-0.078			7.9	8.3	8.6			2.96	3.12	2.41			25.5	26.5	15.4		
Burkina Faso			-0.026	0.096	0.052			5.7	5.6	6.1			4.42	4.06	4.51			7.7	6.1	9.9
Cambodia				0.079	0.069				5.8	6.2				5.35	4.80				0.8	2.8
Cameroon		-0.080		0.076	0.019		7.1		6.5	6.8		4.56		4.27	4.25		28.5		26.5	26.3
Colombia	-0.033	0.125	-0.035			7.6	7.4	8.0			4.49	4.06	4.36			21.4	18.7	25.3		
Cote d'Ivoire			-0.047	0.058	0.022			6.8	6.5	6.8			4.05	4.06	3.84			11.2	15.0	7.4
Dom. Rep.	-0.029	0.125	0.025	0.030	0.066	7.4	7.2	7.8	8.0	8.1	3.09	3.05	2.66	2.52	2.26	9.3	11.9	9.4	5.7	5.1
Egypt	-0.004	0.052	0.054	-0.033	0.110	6.9	6.9	7.1	7.4	7.2	3.97	3.85	3.64	3.48	3.14	41.6	37.1	36.5	32.2	30.5
Ethiopia				0.040	0.110				5.0	5.2				5.88	6.16				6.0	7.7
Ghana		-0.041	-0.065	0.117	0.152		6.3	6.1	5.8	6.4		3.36	3.65	3.67	3.72		16.9	27.3	25.4	23.4
Guatemala		0.061	0.005	0.032			7.2	7.5	7.5			4.13	3.94	3.78			36.3	40.4	38.7	
Guinea				-0.050	0.046				5.9	5.7				4.46	4.46				11.0	8.2
Haiti			-0.002	0.027	0.052			6.1	6.1	6.2			4.43	4.09	3.93			0.6	1.2	0.5
India			0.006	0.091	0.097			6.2	6.2	6.7			5.67	5.32	5.07			12.3	14.9	15.3
Indonesia		0.095	-0.115	0.118	0.132		6.7	7.2	6.6	7.2		2.88	2.78	2.25	2.45		2.1	1.5	2.2	2.1
Jordan		0.038		0.053	0.078		7.3		7.6	7.9		2.40		2.18	1.83		2.0		1.4	0.9
Kazakhstan			-0.016	0.185				7.4	7.3				2.73	3.03				24.3	25.6	
Kenya		-0.027	0.015	0.046	0.053		6.1	6.0	6.1	6.3		3.22	3.44	3.55	3.52		12.7	13.3	18.9	16.5
Madagascar			-0.001	-0.010	0.064			5.7	5.7	5.6			3.64	3.43	3.77			25.2	30.1	29.2
Malawi			-0.056	0.066	0.070			5.3	5.0	5.4			2.55	2.58	2.65			6.4	10.5	13.9
Mali		-0.043	-0.033	0.110	0.054		6.0	5.7	5.6	6.1		3.45	3.77	3.94	3.69		2.4	3.8	2.3	2.2
Mozambique				0.028	0.064				5.5	5.7				4.23	4.63				42.4	38.8
Namibia			-0.046	0.119	0.034			7.9	7.6	8.2			4.83	4.91	4.89			3.4	2.6	3.1
Nepal			0.000	0.051	0.094			5.6	5.6	5.8			3.00	3.27	3.16			13.2	13.9	17.9
Nicaragua			0.004	0.016				7.0	7.0				5.70	5.52				33.6	32.6	
Niger			-0.050	0.068	0.046			5.5	5.2	5.6			4.26	4.56	4.49			19.0	18.8	20.2
Nigeria		-0.053		0.175	0.064		5.9		5.8	6.7		3.73		3.71	3.69		29.3		28.0	25.7
Peru	-0.007	0.109	-0.039	0.056	0.098	7.4	7.3	7.9	7.7	8.0	3.94	3.93	3.51	3.48	3.22	34.5	32.1	34.7	34.9	35.9
Philippines			-0.041	0.020	0.087			7.3	7.0	7.1			3.29	3.48	3.20			13.3	16.4	14.9
Rwanda			-0.036	0.047	0.108			5.6	5.4	5.7			2.86	3.02	2.92			3.6	11.7	6.9

Senegal	0.037	-0.076	-0.037	0.073	0.034	6.7	6.9	6.5	6.3	6.7	3.51	3.33	3.52	3.40	3.30	2.7	7.1	4.8	3.6	6.2
Tanzania		-0.010	0.081	0.021	0.048		5.5	5.4	5.8	5.9		3.22	3.47	3.41	3.77		12.2	17.6	16.8	17.3
Turkey			0.018	0.129	0.050			8.2	8.3	8.9			2.56	2.27	2.14			18.5	21.0	14.1
Uganda		0.002	-0.041	0.026	0.066		5.8	5.8	5.6	5.7		2.92	2.87	2.95	3.13		0.5	0.6	1.1	2.2
Uzbekistan			-0.031	-0.011				6.6	6.4				2.56	2.65				30.3	34.9	
Vietnam			0.048	0.089	0.104			5.9	6.2	6.6			4.01	3.84	3.67			13.5	18.6	19.9
Zambia		-0.037	-0.046	0.108			6.3	6.1	5.9			4.23	4.53	4.60			8.5	5.3	6.2	
Zimbabwe		-0.069	-0.072	-0.064	0.051		7.0	6.7	6.3	6.0		3.86	3.72	3.83	4.00		2.8	2.5	3.1	1.3
Total	0.004	0.013	-0.020	0.059	0.072	7.3	6.7	6.6	6.3	6.6	3.66	3.52	3.69	3.78	3.69	22.5	15.2	16.1	16.2	14.0

Notes: The summary statistics correspond to the data used in the regressions. Further summary statistics are available from the authors.

Table 3. Economic growth on total inequality

Income/expenditure survey sample

					System-GMM			
						Collapse		
	OLS	*FE*	*Long-run-OLS*	*Difference GMM*	*Full*	*System*	*Difference equation*	*Levels equation*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log initial GDP per capita	-0.005	-0.206***	-0.007	-0.153**	0.001	-0.028	-0.188***	0.002
	(0.004)	(0.051)	(0.006)	(0.060)	(0.022)	(0.046)	(0.058)	(0.104)
Total inequality (set 2) (lagged)	-0.037*	-0.174*	0.000	-0.129	-0.056	-0.303	-0.256	-0.092
	(0.021)	(0.092)	(0.020)	(0.168)	(0.064)	(0.249)	(0.193)	(0.421)
Female secondary education (lagged)	0.052	1.138**	-0.005	1.937	0.032	0.408	2.012**	0.563
	(0.049)	(0.516)	(0.060)	(1.503)	(0.127)	(0.478)	(0.898)	(0.719)
Male secondary education (lagged)	-0.021	-0.950	0.071	-1.462	0.013	-0.619	-1.569	-0.957
	(0.056)	(0.579)	(0.068)	(1.608)	(0.151)	(0.688)	(0.988)	(0.939)
Price level of investment (lagged)	-0.001***	0.000	-0.000	-0.001	-0.001*	-0.001	0.000	-0.001
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.009)
Indicator of income data	-0.015	0.000	-0.020		-0.019	0.055		
	(0.010)	(.)	(0.015)		(0.044)	(0.100)		
Constant	0.156***	1.804***	0.112**		0.137	0.498		
	(0.037)	(0.429)	(0.043)		(0.139)	(0.435)		
Observations	118	118	43	75	118	118	75	118
Countries	43	43		43	43	43	43	43
Instruments				37	56	28	21	11
Hansen				0.833	0.914	0.141		
AR1				0.683	0.0854	0.112		
AR2				0.700	0.427	0.754		
Kleibergen-Papp							0.345	0.566

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 (Ho: 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

Table 4. Economic growth on total inequality
Demographic and Health Survey sample

	OLS	*FE*	*Long-run-OLS*	*Difference GMM*	*System-GMM*			
						Collapse		
					Full	*System*	*Difference equation*	*Levels equation*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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

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 (Ho: Underidentification). 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

Table 5. Economic growth on inequality of opportunity and residual inequality
Income/expenditure survey sample

	OLS	*FE*	*Long-run-OLS*	*Difference GMM*	*System-GMM*			
					Full	*Collapse*		
						System	*Difference equation*	*Levels equation*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log initial GDP per capita	-0.003	-0.224***	-0.007	-0.189***	-0.017	-0.023	-0.173***	0.031
	(0.005)	(0.050)	(0.006)	(0.061)	(0.015)	(0.025)	(0.052)	(0.054)
Inequality of Opp. (set 2) (lagged)	-0.070	-0.050	-0.072	-0.144	-0.234	-0.681	-0.400	-0.227
	(0.074)	(0.193)	(0.087)	(0.286)	(0.209)	(0.447)	(0.351)	(2.107)
Residual inequality (set 2) (lagged)	-0.036	-0.210	0.029	0.063	-0.063	-0.175	-0.143	0.054
	(0.035)	(0.145)	(0.075)	(0.353)	(0.095)	(0.240)	(0.251)	(0.856)
Female secondary education (lagged)	0.069	0.991*	-0.008	2.441	0.064	0.255	1.657**	0.588
	(0.046)	(0.497)	(0.081)	(1.501)	(0.110)	(0.424)	(0.805)	(0.667)
Male secondary education (lagged)	-0.052	-0.819	0.080	-2.026	-0.018	-0.506	-1.163	-0.988
	(0.055)	(0.563)	(0.111)	(1.558)	(0.137)	(0.670)	(0.850)	(1.020)
Price level of investment (lagged)	-0.001**	0.000	-0.000	-0.001	-0.000	-0.000	0.000	-0.003
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.005)
Indicator of income data	-0.023*	0.000	-0.026		-0.004	0.011		
	(0.011)	(.)	(0.016)		(0.039)	(0.085)		
Constant	0.143***	1.933***	0.102*		0.243**	0.460		
	(0.041)	(0.445)	(0.058)		(0.108)	(0.293)		
Observations	118	118	43	75	118	118	75	118
Countries	43	43		43	43	43	43	43
Instruments				44	65	35	25	14
Hansen				0.846	0.926	0.205		
AR1				0.909	0.0832	0.190		
AR2				0.979	0.429	0.777		
Kleibergen-Papp							0.607	0.446

Two-step GMM estimation method. Standard errors in parentheses. Period dummies not reported. Quartic polynomial in the number of types included throughout. 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 (Ho: 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.

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

Table 6. Economic growth on inequality of opportunity and residual inequality
Demographic and Health Survey sample

	OLS	*FE*	*Long-run-OLS*	*Difference GMM*	*System-GMM*			
					Full	*Collapse*		
						System	*Difference equation*	*Levels equation*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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

Two-step GMM estimation method. Standard errors in parentheses. Period dummies not reported. Quartic polynomial in the number of types included throughout. 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 (Ho: Underidentification). 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

Table A1. List of countries included in the Income and Expenditure Surveys sample

Country	Source	Welfare Variable	Circumstance variable												Number of Types		Survey years				
			gender	ethnicity	language	religion	citizen	immigrant	country born	disability	father educ	mother educ	birth region	region of residence	Set 1	Set 2	1981-1985	1986-1990	1991-1995	1996-2000	2001-2005
Australia	LIS	Income	1						1					1	10	70	1985	1989	1995		
Austria	LIS	Income	1				1							1	8	44			1995	2000	2004
Bangladesh	I2D2	Expend.	1			1									10	10			1991	2000	2005
Belgium	LIS	Income	1				1							1	11	25			1995	2000	
Belize	I2D2	Income	1	1										1	10	92			1994	1999	
Bolivia	SEDLAC	Income	1	1	1									1	50	325				2000	2005
Brazil	SEDLAC	Income	1	1									1		259	259			1995	1999	2005
Bulgaria	I2D2	Expend.	1	1										1	66	120			1995	2001	
Canada	LIS	Income	1					1		1				1	8	22	1981	1987	1991	2000	2004
Chile	SEDLAC	Income	1	1	1									1	30	427				2000	2003
Colombia	NQLS	Expend.	1								1	1	1		994	994				1997	2003
Czech Rep.	LIS	Income	1				1			1				1	18	182				1996	2004
Denmark	LIS	Income	1					1		1				1	8	213		1987	1995	2000	2004
Estonia	LIS	Income	1	1			1			1				1	43	418				2000	2004
Finland	LIS	Income	1		1					1				1	67	506			1995	2000	2004
France	LIS	Income	1				1							1	10	209			1994	2000	2005
Germany	LIS	Income	1						1	1				1	108	1025			1994	2000	2004
Ghana	I2D2	Expend.	1	1										1	10	85			1991	1998	2005
Greece	LIS	Income	1				1			1				1	12	45			1995	2000	2004
Guyana	I2D2	Income	1	1										1	12	113			1992	1999	
Hungary	LIS	Income	1	1										1	4	16			1994	1999	2005
India	I2D2	Expend.	1	1		1								1	38	1224			1993	1999	2004
Ireland	LIS	Income	1						1	1				1	15	90			1995	2000	2004

Israel	LIS	Income	1	1											8	8		1986	1992	1997	2005
Italy	LIS	Income	1						1	1				1	16	271		1989	1995	2000	2004
Kyrgyzstan	I2D2	Expend.	1						1					1	16	136				1997	2002
Luxembourg	LIS	Income	1				1								6	6			1994	2000	2004
Nicaragua	SEDLAC	Income	1	1	1								1		139	139				2001	2005
Norway	LIS	Income	1					1	1	1				1	83	255				2000	2004
Panama	LSHS	Expend.	1		1				1		1	1			499	499				1997	2003
Paraguay	SEDLAC	Income	1		1								1		109	109				1999	2005
Peru	SEDLAC	Income	1	1									1		246	246				2001	2005
Poland	LIS	Income	1							1				1	4	289				1999	2004
Romania	I2D2	Expend.	1						1					1	10	394			1994	2002	2006
Russia	RLMS	Income	1				1		1					1	42	103			1994	2000	2005
Rwanda	I2D2	Expend.	1			1								1	8	144				2000	2005
Spain	LIS	Income	1				1			1				1	8	50			1995	2000	2004
Sweden	LIS	Income	1				1			1				1	21	113			1995	2000	2005
Switzerland	LIS	Income	1		1		1							1	18	53			1992	2000	2004
UK	LIS	Income	1	1						1				1	24	292			1994	1999	2004
USA	LIS	Income	1	1						1				1	16	64	1986	1991	1994	2000	2004
Vietnam	I2D2	Expend.	1	1										1	20	124			1993	1998	2006
West Germany	LIS	Income	1						1					1	10	211	1984	1989			
Number of observations (Total and per year)																**118**	**4**	**7**	**29**	**41**	**37**
Number of countries																**43**					

Notes: Income refers to per capita household income (net). Expenditure refers to per capita household expenditure. The circumstances birth region and region of residence may contain more than one variable (e.g. administrative region and rural/urban). The number of types is the average across the years for a given country, rounded to the nearest integer. The number of types may differ across years for a given country if some categories are unobserved in a particular year.
LIS: Luxemburg Income Study; SEDLAC: Socioeconomic Database for Latin America and the Caribbean (CEDLAS-World Bank); I2D2: International Income Distribution Database; NQLS: National Quality of Life Survey; LSHS: Living Standard Household Survey; RLMS: Russian Longitudinal Monitoring Survey.

Table A2. List of countries included in the Demographic and Health Survey sample

Country	Welfare variable	Circumstance variables					Number of types	1982-1986	1987-1991	1992-1996	1997-2001	2002-2006
		Region of birth	No. of siblings	Religion	Ethnic group	Mother tongue						
Armenia	Wealth Index					1	3				2000	2005
Bangladesh	Wealth Index			1			4		1993	1996	1999	2004
Benin	Wealth Index	1		1	1		213			1996	2001	2006
Bolivia	Wealth Index	1					4			1994	1998	2003
Brazil	Wealth Index	1		1			16	1986	1991	1996		
Burkina Faso	Wealth Index			1	1		38			1992	1998	2003
Cambodia	Wealth Index	1		1			20				2000	2005
Cameroon	Wealth Index	1		1	1		30		1991		1998	2004
Colombia	Wealth Index	1					3	1986	1990	1995		
Cote d'Ivoire	Wealth Index			1	1		24			1994	1998	2005
Dom. Rep.	Wealth Index	1					2	1986	1991	1996	1999	2002
Egypt	Wealth Index	1					2	1988	1992	1995	2000	2003
Ethiopia	Wealth Index	1		1		1	93				2000	2005
Ghana	Wealth Index	1		1	1		83		1988	1993	1998	2003
Guatemala	Wealth Index	1			1		6		1987	1995	1998	
Guinea	Wealth Index	1		1	1	1	175				1999	2005
Haiti	Wealth Index			1			4			1994	2000	2005
India	Wealth Index			1	1	1	166			1992	1998	2005
Indonesia	Wealth Index			1			6		1991	1994	1997	2002
Jordan	Wealth Index			1			2		1990		1997	2002
Kazakhstan	Wealth Index	1		1	1		65			1995	1999	
Kenya	Wealth Index	1		1	1		105		1989	1993	1998	2003
Madagascar	Wealth Index	1	1	1			63			1992	1997	2003
Malawi	Wealth Index	1	1				15			1992	2000	2004
Mali	Wealth Index			1	1		21		1987	1995	2001	2006
Mozambique	Wealth Index	1	1			1	279				1997	2003
Namibia	Wealth Index	1		1			19			1992	2000	2006
Nepal	Wealth Index			1	1	1	87			1996	2001	2006
Nicaragua	Wealth Index	1					4			1997	2001	
Niger	Wealth Index	1		1	1		29			1992	1998	2006

Nigeria	Wealth Index	1	1			12		1990		1999	2003
Peru	Wealth Index	1				3	1986	1992	1996	2000	2004
Philippines	Wealth Index		1	1	1	148			1993	1998	2003
Rwanda	Wealth Index	1				2			1992	2000	2005
Senegal	Wealth Index			1		6	1986	1992	1997	1999	2005
Tanzania	Wealth Index	1	1			10		1992	1996	1999	2004
Turkey	Wealth Index	1			1	9			1993	1998	2003
Uganda	Wealth Index		1			3		1988	1995	2000	2006
Uzbekistan	Wealth Index	1	1	1	1	55			1996	2002	
Vietnam	Wealth Index		1	1		27			1997	2002	2005
Zambia	Wealth Index		1	1	1	107		1992	1996	2001	
Zimbabwe	Wealth Index		1			3		1988	1994	1999	2005
Number of observations (Total and per year)						**134**	**6**	**19**	**34**	**40**	**35**
Number of countries						**42**					

Notes: The number of types is the average across the years for a given country, rounded to the nearest integer. The number of types may differ across years for a given country if some categories are unobserved in a particular year.

Table A3. Robustness check: Specifications without controls
Only coefficients on inequality are reported

					System-GMM			
						Collapse		
	OLS	*FE*	*Long-run-OLS*	*Difference GMM*	*Full*	*System*	*Difference equation*	*Levels equation*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Total inequality (set 2) - Income and Expenditure Surveys (Table 3)								
Total inequality (lagged)	-0.051**	-0.187*	-0.040**	-0.574**	-0.209	-0.450***	-0.979**	-0.005
	(0.019)	(0.103)	(0.016)	(0.237)	(0.141)	(0.163)	(0.479)	(0.148)
Instruments				24	34	16	12	8
Hansen				0.707	0.608	0.329		
Kleibergen-Papp							0.758	0.0388
B. Total inequality - Demographic and Health Surveys (Table 4)								
Total inequality (lagged)	-0.003	0.014	-0.007	0.046	0.027	0.001	-0.024	-0.030
	(0.004)	(0.017)	(0.006)	(0.033)	(0.036)	(0.048)	(0.033)	(0.048)
Instruments				24	33	15	12	7
Hansen				0.981	0.932	0.886		
Kleibergen-Papp							0.793	0.0918
C. Inequality of opportunity (set 2) - Income and Expenditure Surveys (Table 5)								
Inequality of opp. (lagged)	-0.155*	-0.007	-0.100	-0.706	-0.516**	-0.877***	-0.680	-1.667
	(0.083)	(0.237)	(0.086)	(0.446)	(0.211)	(0.310)	(0.449)	(4.937)
Residual inequ. (lagged)	-0.020	-0.228	-0.034	-0.249	-0.090	-0.090	-0.152	0.455
	(0.032)	(0.156)	(0.039)	(0.263)	(0.126)	(0.131)	(0.257)	(1.397)
Instruments				35	50	24	17	11
Hansen				0.845	0.830	0.547		
Kleibergen-Papp							0.696	0.695
D. Inequality of opportunity - Demographic and Health Surveys (Table 6)								
Inequality of opportunity (lagged)	0.006	0.018	-0.016	0.001	0.027	0.006	-0.033	-0.102
	(0.007)	(0.037)	(0.011)	(0.084)	(0.021)	(0.049)	(0.076)	(0.412)
Residual inequ. (lagged)	-0.003	0.015	-0.004	-0.001	0.011	-0.027	-0.025	0.055
	(0.005)	(0.023)	(0.007)	(0.051)	(0.013)	(0.037)	(0.061)	(0.328)
Instruments				38	51	24	19	11
Hansen				0.887	0.951	0.736		
Kleibergen-Papp							0.768	0.770

Two-step GMM estimation method. Standard errors in parentheses. The regressions drop the controls except log initial GNI per capita, period dummies, the quartic polynomial in the number of types, and the indicator for income data (for the income and expenditure surveys). Reporting p-values for Hansen test of overidentifying restrictions; Kleibergen-Papp rk-LM statistic (Ho: Underidentification).

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

Table A4. Economic growth on inequality of opportunity and residual inequality (set 1)
Income/expenditure survey sample

	OLS	*FE*	*Long-run-OLS*	*Difference GMM*	*System-GMM*			
						Collapse		
					Full	*System*	*Difference equation*	*Levels equation*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log initial GDP per capita	-0.007	-0.216***	-0.004	-0.200***	0.003	-0.025	-0.210***	0.115
	(0.006)	(0.065)	(0.010)	(0.067)	(0.017)	(0.044)	(0.055)	(1.916)
Inequality of Opp. (set 1) (lagged)	-0.136	0.228	-0.128	0.154	-0.057	0.667	0.421	-3.300
	(0.088)	(0.472)	(0.172)	(0.432)	(0.254)	(1.000)	(0.700)	(114.538)
Residual inequ. (set 1) (lagged)	-0.042	-0.271*	0.033	-0.260	-0.039	-0.340	-0.238	0.980
	(0.033)	(0.143)	(0.048)	(0.251)	(0.091)	(0.319)	(0.231)	(16.743)
Female second. educ. (lagged)	0.043	1.226**	-0.034	1.806*	0.023	0.266	2.310***	-0.520
	(0.049)	(0.497)	(0.081)	(1.082)	(0.139)	(0.373)	(0.767)	(30.467)
Male secondary educ. (lagged)	-0.009	-1.059*	0.112	-1.541	0.028	-0.309	-1.869**	0.522
	(0.058)	(0.549)	(0.100)	(1.256)	(0.160)	(0.476)	(0.818)	(36.293)
Price level of inv. (lagged)	-0.001***	0.000	-0.000	-0.001	-0.001**	-0.001	0.000	-0.005
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.079)
Indicator of income data	-0.010	0.000	-0.030		-0.024	0.066		
	(0.016)	(.)	(0.032)		(0.039)	(0.119)		
Constant	0.168***	1.830***	0.083		0.126	0.382		
	(0.049)	(0.555)	(0.069)		(0.113)	(0.331)		
Observations	118	118	43	75	118	118	75	118
Countries	43	43		43	43	43	43	43
Instruments				44	65	35	25	14
Hansen				0.752	0.974	0.350		
AR1				0.637	0.0969	0.110		
AR2				0.616	0.414	0.710		
Kleibergen-Papp							0.617	0.970

Two-step GMM estimation method. Standard errors in parentheses. Period dummies not reported. Quartic polynomial in the number of types included throughout. 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 (Ho: 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. Using set 1 circumstances that exclude region of residence and only utilize region of birth.

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