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THE IMPACT OF INEQUALITY AND REDISTRIBUTION ON GROWTH

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In this paper, we reassess the impact of inequality on growth. The majority of previous papers have employed (system) GMM estimation. However, recent simulation studies indicate that the problems of GMM when using non-stationary data such as GDP have been grossly underestimated in applied research. Concerning predetermined regressors such as inequality, GMM is outperformed by a simple least-squares dummy variable estimator. Additionally, new data have recently become available that not only double the sample size compared to most previous studies, but also address the substantial measurement issues that have plagued past research. Using these new data and an LSDV estimator, we provide an analysis that both accounts for the conditions where inequality is beneficial or detrimental to growth and distinguishes between market-driven inequality and redistribution. We show that there are situations where market inequality affects growth positively while redistribution is simultaneously beneficial.

JEL Codes: O4, I3

Keywords: inequality, growth, redistribution, education

1. Introduction

For decades, the question of whether and how inequality affects growth has been the subject of open debate in academia. Both the theoretical and empirical literature are inconclusive as to whether the effect of inequality on growth is predominantly positive or negative. In this paper, we revisit this question using a new dataset that has been carefully treated to minimize measurement error and inconsistency, and we perform robust inference using fixed effects, thus addressing a major criticism of earlier studies that have used a within-country framework.

The early theoretical literature propagated the idea that inequality could foster growth because the rich have a higher propensity to save (Kuznets, 1955; Kaldor, 1957). Particularly when assuming imperfect capital markets, this can be necessary to increase investment and thus economic growth. In the early 1990s, a political economy–based literature challenged this earlier finding, partly driven by cross-country evidence supporting a negative relation between inequality and growth. Persson and Tabellini (1994) argue that inequality promotes institutions that prevent the proper protection of property rights. In a similar vein, Alesina and Rodrik (1994) propose a median voter model where inequality drives taxation

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in a way that reduces growth. Alesina and Perotti (1996) strengthen this general idea for the case of non-democratic settings, suggesting that rising inequality drives political instability, which then induces higher social costs that in turn reduce growth. However, this interpretation was quickly challenged again, for example, by Forbes (2000), who argues that the results of those models are far more conditional on the setting than is often portrayed, and by Li and Zou (1998), who demonstrate that a generalization of both Alesina and Rodrik (1994) and Persson and Tabellini (1994) reverses the results. Galor and Moav (2004) create a unified theory, building a development model where inequality is helpful in the early stage of development, when capital must be accumulated as the primary engine to drive growth, but is detrimental later on, when human capital accumulation is relatively more critical for growth. In general, this third generation of theoretical and empirical papers emphasizes the ambiguity of the inequality–growth nexus, thereby mirroring the huge variation in empirical findings.

Empirical evidence is similarly controversial: it ranges from finding negative to positive relationships, to finding no relationship at all, or suggesting diverse types of non-linearities. Ever since the seminal papers by Deininger and Squire (1996, 1998), who collected a wide array of distributional data that allowed panel inference, most studies have employed a dynamic panel setting with country-specific effects. Nevertheless, the empirical literature of recent years is subject to a few substantial problems.

First, the bulk of the literature in recent years has relied on system GMM. At first glance, this seems like an obvious choice. Due to the importance of economic convergence, which necessitates the use of lagged GDP as regressor, growth regressions are typically dynamic panels with short T and large N, lending them to GMM estimation. Because system GMM is more efficient than first-difference GMM, particularly with persistent variables, it seems perfectly suited. What is often ignored, however, are the fairly severe mean stationarity assumptions required for system GMM. Recent studies show that system GMM is greatly biased when estimating the impact of predetermined regressors in dynamic panels and is greatly outperformed by a simple least-squares dummy variable (LSDV) approach (Moral-Benito, 2013).

Second, employing panel data, most studies have some type of fixed-effects setup (usually estimated via GMM, as noted above) and thereby estimate the within-country effect of changing inequality (e.g. Li and Zou, 1998; Forbes, 2000). However, this has been strongly criticized by several authors, most notably Barro (2000). These authors argue that the degree of measurement error, mostly caused by varying definitions of inequality, is so large that within-country variation is often driven by chance rather than change. Therefore, they suggest using random effects models, thus exploiting the cross-country variation more strongly. While we agree with the Barro critique, we do have some objections to this solution. The assumptions behind the random effects specification—in particular, the lack of correlation between the fixed effects and regressors—seem hardly credible in this context. Yet, a new dataset recently compiled by Solt (2014) and Solt (2016) makes careful adjustments to account for structural breaks in measurement, thereby alleviating these concerns.

In this paper, we revisit and extend the research on the relationship of growth and inequality using, despite its problems, the preferable LSDV estimator on the new dataset, the Standardized World Income Inequality Database—SWIID5.1 (Solt, 2014, 2016). This allows us to use an unbalanced panel of 123 countries from 1960 to 2010 (using non-overlapping 5-year growth periods as observations).

Our empirical contribution is twofold.

First, we show that the impact of inequality strongly depends on both education and income. While generally beneficial, inequality can only fully manifest its benefits if general educational attainment is high, and it can even turn negative in extreme cases, such as in fairly rich countries with relatively low levels of education.[1] While some previous papers have looked into potential interactions of inequality with other indicators, we are not only the first to use the most current data, but we also refine the treatment of interactions in the presence of non-stationarity, which has been largely ignored by the previous literature. Second, we are one of the first papers to look into the relation of redistribution and growth. To our knowledge, the only other paper to proceed a similar direction is the recent work by Ostry *et al.* (2014), who use a shorter sample, apply GMM (which is problematic in this context), and have a specification that allows far fewer non-linearities, which we find to be extraordinarily relevant to drawing the correct conclusions. We propose a new way to decompose net inequality into a market-driven component and redistribution, alleviating the multi-collinearity issues that have also plagued previous studies in that field. To our knowledge, we are the first to embed redistribution in an econometric framework that accounts for a wide range of interactions. Our results on redistribution are more ambiguous than our results on inequality itself. While inequality in itself only seems to have a negative effect in the most extreme situations, there are more situations—particularly those involving low educational attainment—where redistribution is effective at promoting growth. Our results thus show that analysis of inequality may be of limited value in regard to assessing the benefits and dangers of redistribution.

Previously, empirical evidence on inequality was often viewed to implicitly contain empirical evidence concerning redistribution. However, our results suggest that equating empirical evidence regarding the two concepts is to jump to conclusions prematurely. Even in situations where the marginal effect of more market-based inequality on growth is positive, redistribution might be desirable to promote growth under specific conditions. Drawing policy implications on redistribution based on empirical outcomes that address inequality might therefore be highly misleading. While we generally support the previous finding that inequality might be helpful to promote growth since it reflects market-based institutions, some degree of redistribution might be necessary, particularly if access to education is low and social mobility is correspondingly restricted.

[1]The most closely related paper in this respect seems to be Brueckner and Lederman (2015), who also consider some interaction. However, they ignore most of the standard drivers of growth, focus on system GMM, and it is unclear whether their dataset is actually suitable for fixed-effects settings, or might still be subject to the Barro critique.

The remainder of this paper is structured as follows. Section 2 discusses some of the key problems in the empirical literature on inequality and growth, particularly with respect to inequality measurement and estimation techniques. Section 3 traces the evolution of the empirical literature on inequality and growth and replicates selected specifications using our method of choice and our data. In Sections 4 and 5, we discuss our own specifications and results regarding inequality and redistribution, respectively, and Section 6 concludes.

2. Difficulties in Assessing the Inequality–Growth Nexus

2.1. *Measurement of Inequality*

Coverage versus Homogeneity and the Multiple Imputation Frameworks

Finding appropriate data has traditionally been one of the key issues cited in the literature on inequality. For almost two decades, the literature has been dominated by the excellent and extensive dataset collected by Deininger and Squire (1996) and later by its successor, the World Income Inequality Database (WIID), provided by the World Institute for Development Economic Research at the United Nations University (UNU-WIDER, 2015). However, the size of these datasets came at the cost of including more different measurements, different definitions of the underlying income concept, various units of analysis, surveys with different coverage, and, of course, different agencies collecting the data. In the past, the treatment of those differences has often been problematic. In particular, the early literature often adjusted consumption-based Gini measures by merely adding a constant based on the mean values of income- and consumption-based Gini coefficients (Deininger and Squire, 1998; Li and Zou, 1998; Forbes, 2000; Keefer and Knack, 2002; Banerjee and Duflo, 2003). Yet, the data suggest that the difference between consumption- and income-based measures, person- and household-based measures, and so on, differ greatly across countries and periods. While the difference between an income- and a consumption-based Gini index was 12 percentage points in Armenia in 1996, the difference in Indonesia in 2012 between similar measures was only 2 percentage points, to cite just one example. The frequently performed level shift of consumption-based Gini coefficients is thus far from sufficient to truly allow comparison across countries. Recently, authors have become more careful when using inequality data. While some authors simply focus on more-homogenous datasets, paying the price for a greatly decreased sample size (see, e.g., Herzer and Vollmer, 2012; Cingano, 2014), others have attempted more detailed data adjustments to allow comparability and yet maintain large samples (Easterly, 2007; Chambers and Krause, 2010; Brueckner and Lederman, 2015; Brueckner *et al.*, 2015). Yet, by far the most extensive treatment of this issue is that of Solt (2014) and Solt (2016) in his Standardized World Income Inequality Database (SWIID), currently available in its fifth edition. The original version (Solt, 2009) merely considered the then-current edition of the WIID, while the latest SWIID 5.1 merges all available datasets, carefully adjusting the data to time series for each country for both net and market income-based Gini coefficients. The adjustment varies across countries and over time. When no

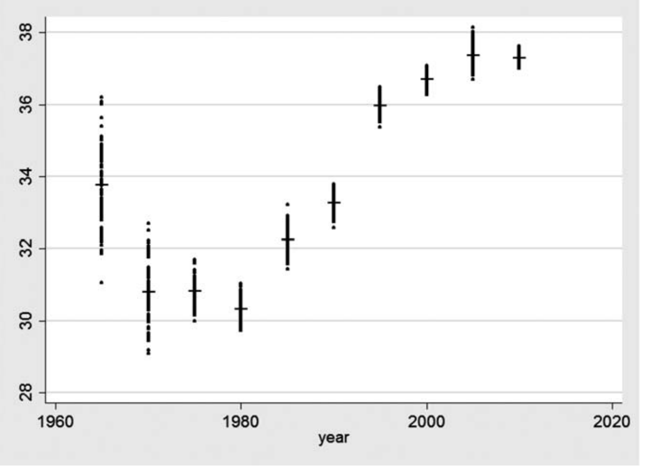


Figure 1. Uncertainty Concerning the Measurement of Inequality in the U.S.

Note: The sign "+" signifies the mean estimate of the net Gini coefficient for the U.S. in the respective year. Each dot represents one imputation for the same year.

within-country information on different measures is available, regional information is used. To account for the resulting uncertainty, the SWIID does not report a single imputation of the missing standardized data but instead reports 100 plausible, simulated imputations, thereby allowing the use of multiple imputation methods (Rubin, 1976, 1996). The SWIID has recently drawn attention from economists and has been used by both Dabla-Norris *et al*. (2015) and Ostry *et al*. (2014). To our knowledge, our paper is the first to make use of the newly updated fifth edition of the SWIID and its multiple imputation framework. The importance of these imputations is highlighted in Figure 1, where we show all imputations of net inequality for the United States (U.S.) over the sample period, thus demonstrating the uncertainty concerning the true level of inequality.

In this multiple imputation framework, the generated regressor problem is accounted for by reporting averaged regression results over different regressions using one out of a large set of plausible imputations rather than by just using the results of a single imputation. The standard errors reported account for both the standard errors estimated for individual regressions using a single imputation and the variation in point estimates.

The Role of Measurement Error for Model Specification

Both Barro (2000) and Banerjee and Duflo (2003), among others, argue that the degree of mismeasurement in inequality data makes it problematic to exploit the time series dimension using a within-country estimator. This is why they opt to use a random effects estimator that also exploits the cross-sectional dimension of the data (rather than a fixed-effects estimator). Their reason is that a fixed-effects estimator would increase the importance of fluctuations in measured inequality driven by variation in the measurement error. This is, of course, a particularly relevant problem when using merged datasets, as inequality studies aiming for broad coverage typically do. Yet, we would not agree with Barro's conclusion. Sharp differences between fixed- and random-effects model frameworks usually indicate inconsistent random-effects estimates due to a violation of the underlying assumptions. Past evidence on inequality thus strongly suggests that we choose between fixed effects—which we employ here—and cross-sectional frameworks. In addition, for datasets before the SWIID—including the Deininger and Squire (1996) data used by Barro—measurement issues in the cross-sectional dimension are much more prevalent than they are over time. It has proven extremely difficult to make the scales between two countries comparable, because measurement is rarely performed by the same institutions. Therefore, differences in detail are nearly unavoidable. The huge variety of adjustments that had to be performed for the SWIID5.1 highlights the magnitude of shifts induced by different measures. However, it is fairly easy to identify and omit implausibly large changes within a country. In addition to the between-country adjustment, this is also done as part of the data adjustment in the SWIID. We thus believe that the inequality measures we use are reasonably precise in measuring both cross-country and within-country variation. Particularly because uncertainty in the imputation is accounted for by our estimation, we are convinced that any significant results we find are not driven by measurement problems. Yet, accounting for uncertainty in this rather conservative way reduces the chance of identifying correlations that exist in the true data-generating process. Thus, we must emphasize the importance of keeping in mind the general rule that absence of evidence does not represent evidence of absence.

The Gini Coefficient versus Alternative Measures

While the Gini coefficient has been the most frequently used measurement of relative income inequality, plenty of alternatives have been introduced, such as the indices proposed by Atkinson (1970) and Theil (1972). Nevertheless, the Gini coefficient is still widely accepted in the empirical literature for a reason. While it does have some minor flaws, such as not meeting the transfer sensitivity axiom (Shorrocks and Foster, 1987), its benefits outweigh its costs. First, under a wide range of assumptions, the Gini coefficient does indeed offer many theoretically desirable features. For example, Aaberge (1992) shows that the Gini coefficient can be rationalizable under a fairly plausible set of preference relation axioms. Additionally, applying the positional version of the principle of transfer sensitivity, the Gini coefficient can be decisive in determining the welfare ordering of intersecting Lorenz curves (Zoli, 1999). More importantly, we know that the Gini index is most sensitive to income changes around the median income; thus, the Gini coefficient performs comparatively best, although not perfectly, in explaining the shape of Lorenz curves compared to, for example, any single quantile income share. Finally, the large datasets currently available typically focus on the Gini coefficient. Because most papers assessing the robustness of results over different measures find no major difference in the implications (see, e.g., Clarke, 1995; Cingano, 2014; Dabla-Norris *et al*., 2015), the huge loss of observations that results from the use of alternative choices seems too high a cost.

Sample

The SWIID5.1 is a set of unbalanced panel data including 4,627 annual observations of the Gini coefficient covering 172 countries from 1960 to 2013. For our work, we use 5-year averages. After removing observations where some of our control variables required for our various specifications are not available, we end up with a sample of 846 observations for 123 countries from 1960 to 2010. Since some variables are required as lags in our model and the dataset has a few gaps, a sample size of up to 694 observations (or less) is actually used depending on the precise specification we choose. Real GDP and the investment share are taken from the current edition of Penn World Tables (PWT8.1). The education attainment data are from the dataset of Barro and Lee (2013). The institutional data are from Gwartney *et al*. (2015). A complete list of countries in our sample (including the time span where data are available for those countries) can be found in the Appendix (in the Online Supporting Information), in Table A.1.

2.2. *How to Treat Predetermined Regressors in Growth Regressions*

We follow the common setup in the literature, which aims to estimate an augmented catch-up growth regression given by the following:

$$\frac{y_{i,t+a}-y_{i,t}}{a}=\beta_0 y_{i,t}+BX_t+\eta_i+\varepsilon_{i,t}, \tag{1}$$

where y_t is the log of (per capita) real GDP at time t, and X_t is a vector of regressors that includes a set of predetermined drivers of growth. That is, contrary to cross-country studies of long-run growth (now often referred to as development rather than growth regression), most of those setups do not rely heavily on contemporaneous regressors. The growth period considered usually ranges from $a = 5$ to 10 years, whereas we focus on the 5-year period. B and β_0 hold our coefficients, η_i is the country-specific component of the error term, and $\varepsilon_{i,t}$ the idiosyncratic component of the error term.

While this equation has occasionally been estimated in past literature using a simple fixed-effects model, it has frequently been noted—by, for example, Forbes (2000), Banerjee and Duflo (2003), and Frank (2009), to name just a few—that it can easily be rearranged into a standard dynamic panel form. Isolating the lead of $y_{i,t}$ at the right-hand side, we can write

$$y_{i,t+a}=\lambda_0 y_{i,t}+\Lambda X_t+\tilde{\eta}_i+\tilde{\varepsilon}_{i,t}, \tag{2}$$

where $\lambda_0=1+a\beta_0$ and $\Lambda=aB$. The well-known problem of this type of regression is the correlation of the fixed effect with both future and current GDP. This gives rise to a bias that does not disappear for small time dimensions, even if the cross-sectional dimension goes to infinity (Nickell, 1981). Even when removing the country-specific effects through differencing, the Nickell bias persists. The Nickel bias becomes smaller for both fixed-effects and random-effects models if the time dimension T approaches infinity. Simulation studies suggest that a simple fixed-effects or least-squares dummy variable (LSDV) estimator typically yields more-precise results if T is approximately 30.[2] However, given the common choices for a, even large datasets in the growth literature usually have a fairly small time dimension, and thus this carries all the problems typically associated with this type of model.

Thus, the vast majority of the empirical growth literature that considers panel data has used techniques designed to cope with this issue. Typically, the predetermined variables in the first difference version of equation (2)—including the lagged dependent variable—are instrumented by lagged levels, as originally suggested by Anderson and Hsiao (1982), and then extended to the now standard GMM estimators (using all available lags rather than just one) by Arellano and Bond (1991). The first generation of panel analyses of the inequality–growth nexus (such as Li and Zou, 1998; Forbes, 2000) usually used this simple type of GMM estimator. Yet, when the data are persistent, it can be shown that lagged levels become extremely poor instruments. Thus, many more recent papers (e.g. Ostry *et al.*, 2014; Brueckner and Lederman, 2015; Dabla-Norris *et al.*, 2015) have used more-efficient system GMM estimators, where an additional level equation with first differences as instruments is estimated, as proposed by Arellano and Bover and Blundell and Bond (1998).

While our objective is to estimate the impact of inequality on growth, equation (2) shows that we have to estimate a dynamic panel using the level of log(GDP) to get there. Yet, these system GMM estimators rely heavily on mean stationarity assumptions that are obviously violated by the level of log(GDP). Particularly because system GMM is actually designed for persistent data, the importance of this assumption is often ignored.[3]

However, in a recent paper, Moral-Benito (2013) compares the performance of a battery of estimators in small samples with small T and (comparably) large N for a broad range of settings.[4] An evaluation of his results yields two main conclusions. First, the Nickel bias seems to be less severe for control variables and mostly matters for the lagged dependent variable. Second, non-stationarity affects the LSDV estimator much less than it does the two GMM approaches. For every single non-stationary setting considered, the author finds that difference GMM is less biased than system GMM and produces smaller mean absolute errors. This holds for both the coefficient on the lagged dependent variable and the coefficients of predetermined regressors, which matter far more in our context. More importantly, for the said coefficients of predetermined regressors, Moral-Benito finds in 14 out of 16 non-stationary settings that a simple LSDV estimator outperforms difference and system GMM estimators in terms of mean absolute error (MAE) and is on par with difference GMM in terms of bias. Because we are not

[2]Fixed effects usually refer to within estimators where the data is demeaned by country, which are equivalent to LSDV estimators, where the country-specific effects are included through country dummies. In this paper, we use fixed effects and LSDV interchangably. The estimation is done using the dummy variable approach.

[3]For a detailed discussion of the importance of stationarity in this context, see Bun and Sarafidis (2015).

[4]The paper by Moral-Benito (2013) is not primarily a simulation paper, however. The author actually introduces a new maximum likelihood estimator for dynamic panels. While technically highly attractive, this estimator requires balanced panels, which would force us to drop far too many observations to be applicable for our paper.

TABLE 1
Summary of Simulation Results Comparing LSDV, GMM, and System GMM in Non-stationary Settings

	Bias	
	$\phi > 0$	$\phi < 0$
ρ=0.95	$LSDV < GMM \ll sGMM$	$LSDV < GMM \ll sGMM$
ρ=0.75		$LSDV < GMM \ll sGMM$
ρ=0.50	$GMM \simeq LSDV \ll sGMM$	$LSDV < GMM \ll sGMM$
	MAE	
	$\phi > 0$	$\phi < 0$
ρ=0.95	$LSDV < GMM \ll sGMM$	$LSDV < GMM \ll sGMM$
ρ=0.75		$LSDV < GMM \ll sGMM$
ρ=0.50	$LSDV \simeq GMM \ll sGMM$	$LSDV < GMM \ll sGMM$

Note: The table summarizes the relative performance of the least-squares dummy variable (LSDV), first difference GMM (GMM), and system GMM (sGMM) methods in terms of bias and mean absolute error (MAE) concerning the coefficient of a predetermined variable in a non-stationary dynamic panel setup with a lagged endogenous variable. All 16 non-stationary models from Moral-Benito are included. We only distinguish by the sign of the coefficient of the feedback effect from the endogenous variable on the predetermined variable (ϕ) and the autoregressive parameter of the predetermined variable (ρ). The autoregressive parameter of the endogenous variable is 0.75 in all models considered. For the full information on the setups, please refer to Moral-Benito (2013).

seeking the catch-up coefficients (where difference GMM would still be preferable) but are instead seeking the impact of lagged inequality on growth, it seems that the often-shunned LSDV is the method of choice, outperforming difference GMM and, in particular, the widespread system GMM. The results of the simulation study are summarized in Table 1.[5]

3. Revisiting Empirical Evidence

3.1. *A Brief History of the Literature on Inequality and Growth*

While the understanding during the 1950s and 1960s was that inequality could be beneficial to economic growth because it fosters savings and thus investment, the mid-1990s saw a surge of literature aiming to disprove this original consensus. Galor and Zeira (1993)find that inequality hinders development under credit imperfections and the indivisibility of human capital formation. Alesina and Rodrik (1994), Persson and Tabellini (1994), Perotti (1996), and Alesina and Perotti (1996) all build political economy models—highlighting the risks of inequality for growth—to support their empirical finding that inequality is negatively related to growth in a cross-sectional setting. Alesina and Rodrik (1994) build a simple endogenous growth model with distributive conflicts and find consistent empirical evidence with cross-country regression that wealth inequality

[5]Since these simulation results are only available for straightforward LSDV, we do not consider further alternatives such as random effects models, or bias corrected LSDV (Bruno, 2005).

enhances growth by affecting fiscal policy. Later, Alesina and Perotti (1996) extended this finding to non-democratic settings, arguing that income inequality increases sociopolitical instability, which in turn reduces investment and growth. Persson and Tabellini (1994) make a similar argument about institutional channels and suggest that inequality promotes institutions that do not protect property rights.[6] The contemporary empirical literature seems to support these findings. Birdsall *et al*. (1995) conduct an in-depth analysis of East Asia, yielding essentially the same results. Clarke (1995) reports that the negative effects are robust across different inequality measurements and regression specifications, including interactions between inequality and regime type; and Perotti (1996) lends empirical support to the social instability channel promoted in Alesina and Perotti (1996).[7]

The discussion changed with the seminal paper by Deininger and Squire (1996), which introduced a new dataset on inequality that allowed for panel analysis of the inequality–growth nexus. The subsequent literature is essentially split into more or less separate branches, one focusing on the cross-sectional dimension and taking a very long-run perspective on growth (often with 20 or more years), and the other being a panel literature, to which this paper contributes, that considers country-specific effects.

A key issue in the cross-sectional literature seems to be instrumentation. The cross-sectional literature often has to use contemporaneous inequality rather than lags, giving rise to massive endogeneity problems. Yet there are very few alternatives. Historical data on inequality that would allow us to consider initial inequality rather than contemporaneous inequality is barely available, and due to the long growth periods considered, it is questionable whether initial inequality would provide much information even if it were available. A large part of that literature therefore aims to find the best instrumentation. Knowles (2005) shows that cross-sectional studies in this field suffer from inconsistency of inequality measurements, and he finds that a robust negative relation exists between consistently measured inequality of expenditure data and economic growth in developing countries. Easterly (2007) suggests an attractive and truly exogenous new instrument using agriculture endowments to instrument income inequality, and finds that high inequality shows statistically significant and negative effects on prosperity, the quality of institutions, and higher education. Due to our own findings on the importance of education for the relationship between inequality and growth, the most interesting contributions in this field, from our perspective,

[6]Persson and Tabellini (1994) is one of the few papers that also includes some very limited panel evidence. In addition to the then standard cross-country regression with about 50 countries, this paper also includes a very small historical panel covering nine countries.

[7]There have been a few papers that analyze the postulated relation of inequality and institutions empirically rather than focusing on the growth effect. Scully (2002) shows that economic freedom promotes both growth and equality, the tradeoff between inequality and growth being positive and relatively small. Carter (2007) demonstrates that inequality is significantly and positively correlated with economic freedom in country-fixed-effects models. Apergis *et al*. (2014) use panel error correction model to analyze the Granger causality between economic freedom and income inequality, and find that economic freedom creates equal access to property rights for the poor and enhances growth, which in turn affects income distribution; and it also limits redistribution from the rich to the poor, which affects growth as well.

might be Sylwester (2000), Castelló and Doménech (2002), and Castelló-Climent and Doménech (2008), who focus on the role of education in this context. Sylwester (2000) reports that income inequality raises public expenditure on education and thus reduces contemporaneous growth, even though future growth may benefit from this cost. Castelló and Doménech (2002) and Castelló-Climent and Doménech (2008) provide a cross-country analysis of human capital inequality. They find that initial human capital inequality affects economic growth more robustly than income inequality.

While the results in the cross-sectional literatures are primarily negative, the panel results are much more ambiguous. In particular, the first wave of panel studies (Li and Zou, 1998; Forbes, 2000) challenges the original cross-country results. Li and Zou (1998) demonstrate that the theoretical results brought forward by Alesina and Rodrik (1994) can be reversed with a minor generalization of the model. Similarly, Forbes (2000) argues that the above-mentioned political economy models have much less clear-cut implications than is often portrayed. Using the panel data provided by Deininger and Squire (1996), both show that inequality has a positive impact on growth in the panel context when accounting for country-specific effects. Panizza (2002) supports this finding for a sample of U.S. states, using standard fixed-effects and system GMM estimators. This new generation of literature was sharply criticized by Barro (2000). While using panel data, he questions the strong focus on within-country effects, as is done with the fixed-effects and difference GMM estimators used in those papers. He claims that the measurement error is far too large to allow proper within-country inference, because variation in inequality measures over time is often driven by variation in measurement error rather than true changes in inequality. Therefore, Barro (2000) and Barro (2008) focus on a random effects setup that still exploits the cross-sectional information, using a three-stage least-squared method. Their approach is a variation of the Chamberlain (1984) estimator that dominated the literature on dynamic panels before GMM estimation of those models with a slightly different instrumentation. Both papers conclude that the effect of inequality is rather weak; income inequality generally tends to impede economic growth in poor countries, but less so in rich countries. However, this approach is questionable. It is difficult to believe that the country-specific effects—which might, for example, represent institutions and culture—only affect growth but not inequality, thereby invalidating one of the key assumptions behind random effects. Banerjee and Duflo (2003), who also focus on random effects, assess potential non-linearities in the impact of inequality (or, in their case more specifically, changes in inequality) on growth, finding that changes in inequality affect growth very strongly and over-proportionally.

In recent years, the new political interest in inequality and the increasing availability of data have spurred a new generation of empirical papers, particularly by researchers of the major political actors such as the IMF and the OECD, that mostly use system GMM estimation (Halter *et al*., 2014; Ostry *et al*., 2014; Brueckner and Lederman, 2015; Dabla-Norris *et al*., 2015). Among them, our paper is most closely related to the work by Ostry *et al*. (2014), which provides one of the very few attempts to distinguish between the effects of market-driven inequality and redistribution.

Of course, this literature review is far from complete and focuses strongly on the strand of literature giving rise to our own approach. Other papers focus on alternative measures of income distributions (Voitchovsky, 2005), different types of inequality, such as asset and land inequality (Deininger and Olinto, 2000), inequality of human capital (Cingano, 2014; Castelló-Climent, 2013), or the inequality of opportunity (Marrero and Rodríguez, 2013), including a more micro-oriented perspective on the dynamics of inequality and growth by van der Weide and Milanovic (2014). Finally, there is a strand of literature trying to tackle the non-stationarity of GDP explicitly using cointegration models (Frank, 2009; Herzer and Vollmer, 2012). However, the latter papers are more closely related to the development literature, despite their use of panel methods, because they are essentially trying to identify a long-term relationship.

3.2. *Selected Specifications from the Literature Revisited Using SWIID5.1*

In this section, we reproduce the settings proposed in a few of the seminal papers in the literature but using SWIID data and—for the reasons discussed above—LSDV. Of course, like most empirical works, the papers we cite here include a whole battery of specifications, sensitivity analyses, and robustness tests. To review all of them would mean exceeding the length limitations of a single paper by far. Therefore, we focus on the preferred specification or the specification that provided the greatest innovation to the literature from each paper. The results on all specifications are summarized in Table 2. More details on all setups included, and the reasoning behind why we consider them, follow below. The results of all these replications are jointly discussed at the end of the section.

Forbes (2000)/Perotti (1996)

Forbes (2000) was one of the first papers to use panel econometrics to challenge the general view that inequality is detrimental to growth; it was primarily based on cross-country studies. Using the set of covariates suggested by Perotti (1996) and a panel setup with country-specific effects, she finds that inequality boosts growth. In her study, this finding is robust over a large range of alternative specifications of the basic growth regression. What makes the Forbes (2000)/Perotti (1996) specification particularly interesting is the very parsimonious specification. They only include education (individually measured for the male and female population as average years of secondary education) and the relative real price of investment (compared to the U.S.). While this does, of course, omit a large range of potential determinants of growth that have been identified in the literature, it also prevents the impact of inequality from being obfuscated by the inclusion of the channels through which inequality affects growth, such as investment or political pressure.

Barro (2000)/Barro (2008)

Barro (2000, 2008) emphasizes the conditionality of the impact of inequality on the original level of (per capita) GDP, which is now one of the most widely used specifications in the literature. He finds that inequality is detrimental in poor countries but

TABLE 2

Specifications Suggested by Barro (2000), Forbes (2000), and Banerjee and Duflo (2003)

	(1)	(2)	(3)	(4)	(5)
$gini_{t-1}$	0.00715***	0.00591***	0.00720***		
	(0.00273)	(0.00208)	(0.00268)		
$gini_{t-1}y_{t-1}$		0.00129	0.000402		
		(0.00160)	(0.00189)		
$\Delta gini^2_{t-1}$				0.000484	−0.000924
				(0.00161)	(0.00166)
$\Delta gini_{t-1}$				3.27e–05	−1.40e–05
				(0.000132)	(0.000135)
Observations	694	572	694	499	549
Number of countries	123	117	123	112	118
Controls	Forbes	Barro	Forbes	Barro	Forbes
F-test (p)	0.00867	0.0155	0.0180	0.878	0.808

Notes: Robust standard errors in parentheses.

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

The dependent variable is the difference of log real GDP per capita over 5 years, based on real GDP as reported in the current edition of Penn's world tables (PWT8.1). All estimations include the initial level of logged GDP per capita. The first growth period considered is 1960–5; the last 2005–10. We use non-overlapping periods. Time fixed effects are included in all models. *gini* refers to the Gini coefficient based on net income.

Forbes: This list of controls refers to the control variables used in Forbes (2000). The variables included are the average years of secondary schooling in the male population over 25, the average years of secondary schooling in the female population over 25 (both taken from the Barro and Lee (2013) data), and the relative real price of investment as reported in PWT8.1.

Barro: This list of controls refers to the control variables used in Barro (2008). The variables included are the average inverse of life expectancy at birth, the log of fertility (both taken from the World Development Indicators, WDI), and legal institutions measured (taken from the Freedom of the World data collected by Gwartney *et al.* (2015)) at the beginning of the period. Openness (adjusted for area and population size effects), investment share and terms of trade growth—weighted with the openness ratio—are included as contemporary averages over the growth period considered. All these variables are computed by the authors based on PWT8.1 data, and using area and population as reported in WDI. In addition to the different estimation techniques, the main difference compared to Barro's paper are that we use 5- rather than 10-year periods and that we have to use legal institutions at the beginning of the period rather than period averages, because our institutional variable is only measured every 5 years until approximately 2010. Thus, a 5-year average would otherwise represent the end-of-period value for a substantial part of the sample.

can augment growth in (very) rich countries. While Barro accounts for the inclusion of the lagged endogenous variable by using a 3SLS estimator, he does not include country fixed effects, arguing that the majority of variance comes from the cross-sectional dimension, and he uses random effects instead. For our analysis, we consider the same explanatory variables as Barro (2008), but we use our standard fixed-effects estimator. Barro uses a wider selection of potential drivers of growth than Perotti (1996) and Forbes (2000); in particular, he explicitly considers investment. Due to the above-mentioned problem, that investment might be the channel through which inequality affects growth. We also test the general idea that the impact of inequality depends on the level of development using the smaller set of controls discussed in subsection 3.2. Except for investment, this setup considers institutions (more specifically legal rule), demographic indicators (in particular, the fertility rate and the inverse of life expectancy), and trade-related indicators (openness and change in the terms of trade). Rather than distinguishing between schooling for the male and female populations, the population average of years in (higher) education is used to proxy human capital.

Banerjee and Duflo (2003)

Banerjee and Duflo (2003) deviate from the literature in two ways. First, they argue that it is the change in inequality rather than the level of inequality that affects growth. Second, they stress the idea of potential non-linearities in the impact of (changes in) inequality on growth. Using different specifications with the controls suggested by Perotti (1996) and Barro (2000), they add non-linear terms to a random effects specification of these growth models. Rather than choosing a specific non-linear transformation of non-linearity, they take the residuals of a growth regression without inequality (estimated through GMM) and use a kernel estimator to assess the non-linear relationship between inequality and this residual. Because the relation between inequality and growth they identify is a simple hump shape, it can easily be approximated by a quadratic term (which they do as a robustness test). For simplicity and comparability with our other specifications, we opt to estimate this linearized version with a squared term of the inequality indicator in our LSDV setup rather than apply a kernel estimator.

Results

The only result from the previous literature that is robust to our estimation method and large dataset is the strongly positive relationship between inequality and growth that has already been found by Forbes (2000); however, the magnitude of the effect we find is considerably larger (0.0072 instead of 0.0036). We find neither a robust non-linearity of the impact of inequality conditional on the level of income, nor the non-linearity regarding the impact of the changes in inequality that has been postulated by Banerjee and Duflo (2003). While insignificance does not provide evidence of the absence of an effect, the confidence bounds we find are fairly narrow, so a potential non-linear effect in changes of inequality measured by the Gini index would be moderate at best, even if it does exist. Compared with the results of Barro (2008) and Banerjee and Duflo (2003) (see Table 2), our results are distinctly opposite. While we have insignificant coefficients concerning the interaction term, and a strongly significant effect on the level of inequality itself, indicating that the marginal effect for the typical country is positive, Barro's results indicate clearly negative marginal effects in developing countries that turn positive in extremely rich countries.

4. When Does Inequality Affect Growth?

4.1. *Specification*

While our replication studies strongly suggest a positive relation between inequality and growth, they say little about the conditions under which inequality is truly beneficial to economic development. The theory proposed by Galor and Moav (2004) suggests that the impacts of inequality on development depend on the relative rates of return on investment in physical capital and human capital, and thus depend implicitly on factor endowments. In this paper, we use interactions between the Gini index and GDP and between human capital and investment[8] to account for those potential non-linearities in the effect of inequality. These interactions allow us to observe many possible variations in the impact of inequality suggested by modern theory. Specifically, we explore a variety of interactions between (lagged) inequality and (lagged) macroeconomic conditions to assess where we can safely say that inequality is not harmful. While using a different dataset and technique, we also deviate from the previous literature that explored interactions. In particular, in regard to development and education, it seems hardly plausible to consider development in 1960 the same as development in 2010. For variables with a strong time trend (or drift component in a stochastic trend), we thus allow interactions after adjusting for time fixed effects. This does, of course, slightly change the interpretation. For example, we technically no longer assess whether countries in the later stages of development benefit more or less than countries at an early stage of development, but whether relatively rich or poor countries benefit or suffer.

Our baseline specification is a compromise between the controls used by Forbes (2000) and Barro (2008). While keeping our list of controls short to maintain a parsimonious specification that avoids multi-collinearity, we add investment. However—as opposed to Barro—we only use predetermined (i.e. in this case, lagged) explanatory variables. Thus, even if inequality works through increasing or decreasing investment during the growth period considered, this does not overshadow the impact of inequality but helps to alleviate potential omitted variable bias. All variables where we do not remove the time-specific mean as described above, such as investment share, are demeaned using the sample mean before applying interactions. The coefficient reported for inequality itself thus corresponds to the marginal effect at the mean. Nevertheless, our fixed-effects estimator implicitly demeans all variables, including interaction terms, by country. As is standard in the literature, the reports in this section focus on the inequality in net income.

4.2. *Results*

The results are summarized in Table 3. The generally positive impact of inequality that we revisited previously (refer to Table 2) in the replication of existing literature persists in the more refined specification. That is, our results of our extended specifications qualitatively confirm Li and Zou (1998) and Forbes (2000), while contradicting the results obtained by Barro (2000), Banerjee and Duflo (2003), and Barro (2008). The average size of the estimation on the effects of inequality is 0.00731 for our specifications, and it is 0.00675 in the replications in the previous subsection. While the difference between these estimates is economically insubstantial and statistically insignificant, the effects we find both in our own specifications and the replication with the new data and the appropriate method are consistently larger than the effects found by Forbes (2000), which is probably the most widely recognized paper finding a positive growth effect. In economic terms, evaluated at the mean of the variables considered as interactions,

[8]While investment is merely a rough proxy of capital endowment, its availabilitity and the reliability of measurement are far superior.

TABLE 3
THE IMPACT OF NET INEQUALITY ON GROWTH

	(1)	(2)	(3)	(4)	(5)	(6)
Interactions	None	y	edu	I	Gini	All
$gini_{t-1}$	0.00695***	0.00698***	0.00683***	0.00718***	0.00854***	0.00739***
	(0.00266)	(0.00262)	(0.00255)	(0.00262)	(0.00286)	(0.00264)
$gini_{t-1}y_{t-1}$		0.000349				−0.00528**
		(0.00191)				(0.00265)
$gini_{t-1}edu_{t-1}$			0.00502***			0.00603***
			(0.00162)			(0.00179)
$gini_{t-1}I_{t-1}$				0.0158		0.0163
				(0.0146)		(0.0152)
$gini^2_{t-1}y_{t-1}$					−0.000267**	−0.000132
					(0.000122)	(0.000125)
Observations	694	694	694	694	694	694
Number of countries	123	123	123	123	123	123
F-test (*p*)	0.00906	0.0202	8.93e–05	0.0162	0.00996	0.000469

Notes: Robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
The dependent variable is the difference of logged real GDP per capita over 5 years, based on real GDP as reported in the current edition of Penn's world tables (PWT8.1). All estimations include the initial level of logged GDP per capita. The first growth period considered is 1960–5; the last 2005–10. We use non-overlapping periods. Time fixed effects are included in all models. *gini* refers to the Gini coefficient based on net income.
Controls: All equations control for the investment share (*I*), average years of secondary schooling of the population over 25 (*edu*), and the relative price of investment at the beginning of the period.

a change of 1 percentage point in the Gini coefficient creates approximately 0.74 percentage points of growth over 5 years. While this is not much compared to the volatility of growth over time, it can still make a meaningful difference over longer horizons. However, the impact of inequality differs strongly across countries. In particular, we find a strongly positive correlation between education and the impact of inequality. The higher the educational attainment of a society, the more the economy can benefit from inequality. Yet, the marginal effect of inequality only becomes negative for the least educated economies in our sample, more precisely the bottom 14 percent, roughly. This is in stark contrast to Brueckner and Lederman (2015), who find a negative interaction of educational attainment and the Gini index in their growth regression. Contrary to our approach, they use contemporary (rather than lagged) inequality, an instrumental variables approach, and they always interact it with initial educational attainment. While we initially find an insignificantly positive interaction of inequality and (relative) income per capita, this interaction turns significantly negative once we control for the interaction of inequality and education. This strongly suggests that the original weak effect is an omitted variable bias arising from mistaking the effect of education (which is driving development) as an effect of development itself, because of the positive correlation between educational attainment and GDP that, in turn, gives rise to a strong correlation between the interaction terms.

TABLE 4

The Impact of Market Inequality and Redistribution on Growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Interactions	None	y	edu	I	Gini	redistribution	All
$gini_{t-1}$	0.00702***	0.00717***	0.00692***	0.00725***	0.00765***	0.00701***	0.00677***
	(0.00267)	(0.00265)	(0.00257)	(0.00263)	(0.00267)	(0.00265)	(0.00254)
red_{t-1}	−0.00525	−0.00346	−0.00507	−0.00543	−0.00546	−0.00447	−0.00213
	(0.00359)	(0.00404)	(0.00337)	(0.00363)	(0.00395)	(0.00410)	(0.00387)
$gini_{t-1}y_{t-1}$		0.000247					−0.00468*
		(0.00189)					(0.00275)
$red_{t-1}y_{t-1}$		−0.00449					−0.00114
		(0.00306)					(0.00428)
$gini_{t-1}edu_{t-1}$			0.00485***				0.00591***
			(0.00168)				(0.00184)
$red_{t-1}edu_{t-1}$			−0.00570***				−0.00583**
			(0.00215)				(0.00264)
$gini_{t-1}I_{t-1}$				0.0144			0.0154
				(0.0184)			(0.0205)
$red_{t-1}I_{t-1}$				−0.0194			−0.0179
				(0.0189)			(0.0175)
$gini^2_{t-1}$					−0.000303**		−0.000174
					(0.000150)		(0.000146)
$gini_{t-1}red_{t-1}$					9.70e–05	−0.000113	−1.91e–05
					(0.000264)	(0.000288)	(0.000312)
red^2_{t-1}						−9.38e–05	0.000236
						(0.000203)	(0.000274)
Observations	694	694	694	694	694	694	694
Number of crossid	123	123	123	123	123	123	123
F-test (*p*)		0.193	0.0146	0.474	0.153	0.618	0.0784

Notes: Robust standard errors in parentheses.

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

The dependent variable is the log difference of real GDP per capita over 5 years, based on real GDP as reported in the current edition of Penn's world tables (PWT8.1). All estimations include the initial level of logged GDP per capita. The first growth period considered is 1960–5; the last 2005–10. We use non-overlapping periods. Time fixed effects are included in all models. Contrary to the previous tables, *gini* refers to the Gini coefficient based on market income in this table.

Controls: All equations control for the investment share (*I*), average years of secondary schooling of the population over 25 (*edu*), and the relative price of investment at the beginning of the period.

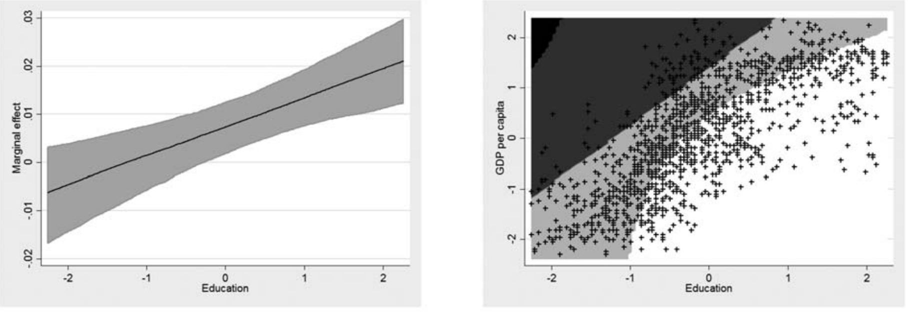
Our findings correspond nicely to the new line of micro-data literature that distinguishes between inequality of opportunity and inequality of outcome. In this literature, income is regressed of factors that are unchangeable for the individual (such as gender, ethnicity, and place of birth) and factors that are affected by the individual's choice. This allows identification of the share of inequality that is not due to differences in effort or skill, but to mere luck and initial endowments. Due to the lack of data, this literature primarily focuses on country studies in highly developed countries such as the U.S. (Marrero and Rodríguez, 2013).[9] While we cannot truly test the hypothesis of different impacts of inequality of outcome and inequality of opportunity, our results point in a direction that is explicable by the hypothesis originally derived in this context. Secondary schooling has a natural upper bound per person. Thus, an increase in the years of higher education typically represents more people attending school rather than the most educated group attending school for an even longer period of time. Implicitly, the level of schooling thus captures the access to education for a broad public, which increases social participation and, of course, the access to higher-income jobs for the "masses." Only if those conditions are met can the economy benefit from inequality. While macro data, available for growth regressions, do not allow us to view inequality under the microscope, it seems plausible that inequality that accompanies broad access to education actually mirrors economic institutions that allow individuals to reap the benefits of their economic efforts rather than exploit the weak, which cannot be ruled out in societies where a wider public has poor access to education.

Figure (2) visualizes the marginal effect of inequality conditional on (a) education and (b) education and income. While the effect of higher education is compensated somewhat by the high GDP that is typically associated with higher education, it is still clearly visible that the positive effects usually prevail. For most countries, particularly most countries with higher education, there is a positive marginal effect, often significantly so. Especially in countries with atypically high levels of education and relatively low levels of development, there can be benefits from inequality in terms of economic growth.

4.3. *Robustness*

Our results are fairly, yet not perfectly, robust. We assess robustness in terms of additional explanatory variables and the chosen method. Our results are robust to the inclusion of (current) population growth. We also assess a version of our model including institutions measured according to the legal systems rating reported by Gwartney *et al*. (2015). This reduces our sample quite substantially. Thus, while the signs and the order of magnitude of the effects we found previously do persist, we do not find significance (with p values typically between 0.1 and 0.2). Some auxiliary regressions indicate an auto-regressive coefficient of approximately 0.5 for the Gini coefficient, and we find an insignificant impact of development on inequality. This means that our setting

[9]The notable exception is Ferreira *et al*. (forthcoming), who gather data for a cross-country analysis of 42 countries and attempt a macro study on this issue. Yet—and potentially driven by the small sample size—they do not find significant and robust results.



(a) (b)

Figure 2. Marginal Effect of Inequality on Growth. (a) Marginal effect of inequality conditional on education (b) Marginal effect of inequality conditional on education and income

Note: Both education (measured as average years of schooling of the population over 25) and the GDP per capital (more precisely, the log of real GDP per capita) are demeaned by year. In (a), the gray shaded area represents the 95 percent confidence bound. In (b), different shades of gray refer to the magnitude of the effect of inequality on growth given education and GDP per capital, ranging from significantly negative (black), to insignificantly negative (dark gray) and insignificantly positive (light gray), to significantly positive (white). Every "+" sign denotes one of our observations in terms of education and GDP. It can be clearly seen that the expected marginal effect is positive for most of our observations, and significantly so for many of them. There are no observations where we expect a significantly negative effect covered by our data.

might be close to the ρ=0.5, $\phi > 0$ settings discussed in Moral-Benito (2013), where GMM and LSDV are roughly on a par, with GMM being less biased and LSDV being more precise. We therefore run a robustness test using first difference GMM. Again, we find the same quantitative results but lose significance concerning our key results discussed above. However, because positive coefficients seem to be typically downward biased in LSDV, it is extremely unlikely that LSDV creates the positive significant effects that we find due to bias. Moreover, GMM loses substantial information by taking first differences. Thus, to some degree, wider confidence bounds are only natural (and correspond to the higher MAE found in the simulation study). Therefore, we consider the results that are extremely close to our key findings in terms of magnitude to be a confirmation of our previous findings.

5. Redistribution and Growth

5.1. *Measuring Redistribution*

By providing both market income-based Gini coefficients (henceforth, market Gini) and net income-based Gini coefficients (henceforth, net Gini), the SWIID is the first large-scale dataset that allows us to assess not only inequality but also redistribution, which—from a policy perspective—might be the most relevant aspect of the inequality–growth nexus. To the best of our knowledge, there is so far only one paper exploring this dimension of the data using a previous version of SWIID (3.1), which is Ostry *et al*. (2014). Following Solt (2016) and Ostry *et al*. (2014), we define redistribution as the difference between market Gini and net Gini. Using much smaller datasets, several authors have previously used similar measures; that is, the difference between an inequality measure based on market income and the same measure based on net income (e.g. Milanovic, 2000; Thewissen, 2013).

Yet, by construction, this method of redistribution measurement creates multi-collinearity issues when simultaneously controlling for either market Gini or net Gini. Defining $redistribution_{i,t}=gini_{i,t}^{\text{market}}-gini_{i,t}^{\text{net}}$ creates a strongly positive correlation between market Gini and redistribution and a correspondingly negative correlation between net Gini and redistribution. To avoid the resulting identification issues, we perform a robustness test with a slightly modified decomposition of net Gini in a market Gini-based component and a redistribution-based component through an auxiliary regression that orthogonalizes redistribution and market Gini and thereby alleviates the multi-collinearity problems. That is, we estimate

$$gini_{i,t}^{\text{net}}=\alpha_0+\alpha_1 gini_{i,t}^{\text{market}}+u_{i,t}, \tag{3}$$

and treat the negative of the resulting residuals, $-\hat{u}_{i,t}$, as our measurement of redistribution. By considering the negative rather than the residuals themselves, we ensure that high values of redistribution still correspond to unusually strong redistribution, in the sense that they correspond to an unusually low net inequality. Both measures, the simple difference and the regression-based measure, are highly correlated. This alternative approach, therefore, only slightly changes the economic interpretation of our redistribution measure. Rather than looking at absolute redistribution, we essentially look at an unusual degree of redistribution compared to the market inequality found in the country. We would like to emphasize that this does not require us to assume a unidirectional causality from market inequality to net inequality. Feedback effects created by the incentive effects of redistribution might affect market Gini to some degree. However, this is rather unlikely because incentives to provide labor are decreased across all income levels; different income levels are not affected in different directions, as noted by Ostry *et al.* (2014). Yet, this definition merely looks at unusually strong or weak redistribution compared to market inequality without making any assumptions about whether this is in any way caused by market inequality.

Our models include both market inequality and redistribution, essentially decomposing net inequality in a market-related component and (non-standard) redistribution efforts. Because we find that inequality has strongly varying impacts given other conditions, we include both market inequality and redistribution in the same interaction framework discussed in the previous section. The results reported in the following paragraph are based on the simple difference-based measure of redistribution. However, both measures considered produce very similar coefficient estimates.

Figure 3 summarizes market inequality and net inequality in 1980 and 2010. It is clearly visible that—unsurprisingly—net inequality is usually lower than market inequality, often considerably so. Yet, the distribution of both net inequality and market inequality in 1980 (+) and 2010 (diamonds) is similar. That is, while

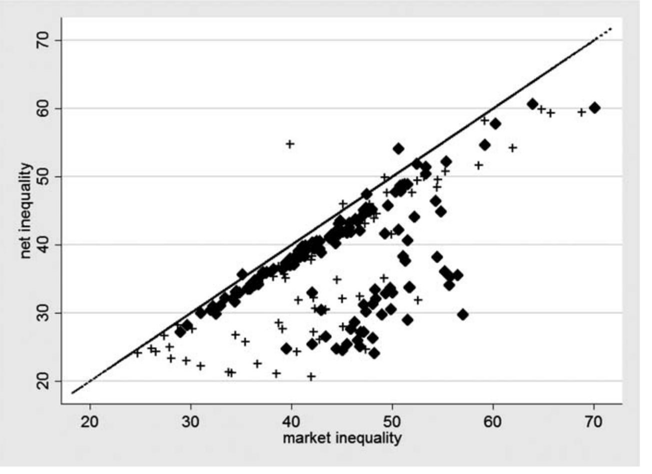


Figure 3. Net and Market Inequality in 1980 and 2010

Note: Observations from 1980 are marked with a "+" sign; observations from 2010 are marked with a diamond.

individual countries did change their positions, there does not seem to be a major structural break in terms of global inequality.

5.2. *Results*

The results from our baseline specification regarding inequality itself are robust in terms of both magnitude and sign.

While the impact of redistribution is negative (see Table 3), the impact is insignificant and quantitatively small. This is in strong contrast to the positive results obtained by Ostry *et al*. (2014) using GMM and a previous version of SWIID. However, we find a strong negative interaction with education. That is, the source of net inequality does not truly matter where the interaction with education is concerned. Meanwhile, the distinctly different coefficients for inequality and redistribution imply clearly different marginal effects. For market inequality, this confirms our results found for net inequality in the previous section. That is, inequality is generally beneficial to growth for most countries, with the marginal effect only becoming negative for those countries with extremely poor education, roughly when the level of education is approximately one standard deviation below the mean or lower. The relatively small coefficient of redistribution that—unlike the coefficient of inequality—is close to zero, in combination with the much larger coefficient of its interaction term, does, however, imply that redistribution can indeed be helpful if education is just below the mean but is clearly detrimental when the average level of educational attainment is high. Moreover, we do not find the significant interaction between the income level and redistribution that we find for the income level and inequality. As mentioned before, due to the correlation of education and income, the effects of income and education on the marginal effect of inequality often compensate each other. This is not true for redistribution, again, making it even more likely to actually find clearly positive marginal effects of redistribution in situations that are empirically relevant.

Economically, this result is plausible. Institutions that incentivize effort and thus produce high market inequality are usually beneficial to growth. Yet, there is some room for redistribution to enhance growth under certain circumstances, particularly if educational attainment is low. This is in line with inequality theories of the early and mid-1990s, which highlighted the risks of social unrest. If educational attainment is high, this implies broad social participation and—possibly more important—labor market chances for a large fraction of the population. Under those conditions, it seems unlikely that high inequality reflects widespread destitution. If, however, educational attainment is low, thereby restricting the access of the poor to the labor market, and making upward mobility almost impossible, stronger policies that alleviate inequality seem to be effective for growth.

Our results strongly point to the pitfalls of taking empirical evidence on inequality to support policy advice concerning redistribution. Market and net inequality (i.e. the total effect, including redistribution) have very different marginal effects. More importantly, the positive effects of inequality and redistribution on growth may occur simultaneously. For instance, a developing country might benefit from institutions promoting more competition and thus market inequality while at the same time requiring a wider social net with more redistribution to foster growth.

6. Conclusions

Through the use of the Gini coefficients from 1960 to 2010 of 123 countries from the novel dataset SWIID5.1 (which carefully treats all currently available income inequality datasets to minimize measurement error and inconsistency) and the simple but appropriate LSDV estimator, and accounting for measurement uncertainty through a multiple imputation approach, we find that inequality is generally beneficial to growth in a medium horizon. Yet, despite inequality being helpful, there is some room for redistribution. The positive effect of inequality is strongly driven by market-based inequality, which most likely corresponds to institutions that incentivize effort. However, we find that redistribution can be beneficial if the average educational attainment is low. In contrast to the previous literature, which either did not find any effect of redistribution or only considered the benefits of inequality as proof of the disadvantages of redistribution, we provide a more nuanced view. While the dangers of redistribution should not be underestimated, particularly in countries where equality of opportunity is otherwise guaranteed through good education, they also should not be uniformly demonized.

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Supporting Information

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

Appendix
Table A.1: Summary Statistics