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Redistribution, inequality, and growth: new evidence

Andrew Berg[1] · Jonathan D. Ostry[2] · Charalambos G. Tsangarides[2] · Yorbol Yakhshilikov[2]

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Abstract We investigate the relationship between inequality, redistribution, and growth using a recently-compiled dataset that distinguishes clearly between market (pre-tax and transfer) and net (post tax and transfer) inequality, and allows us to calculate redistributive transfers for a large number of advanced and developing countries. Across a variety of esti-

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✉ Jonathan D. Ostry
jostry@imf.org

Andrew Berg
aberg@imf.org

Charalambos G. Tsangarides
ctsangarides@imf.org

Yorbol Yakhshilikov
yyakhshilikov@imf.org

1 Institute for Capacity Development, International Monetary Fund, Washington, DC, USA

2 Research Department, International Monetary Fund, Washington, DC, USA

mation methods, data samples, and robustness checks, we find: (1) lower net inequality is robustly correlated with faster and more durable growth, controlling for the level of redistribution; (2) redistribution appears benign in terms of its impact on growth, except when it is extensive; and (3) inequality seems to affect growth through human capital accumulation and fertility channels.

Keywords Growth · Inequality · Redistribution · Cross country analysis

JEL Classification O11 · O15 · O40

1 Introduction

Economists are increasingly focusing on the links between rising inequality and sustainable growth. While the empirical literature seems to have converged toward a tentative consensus that inequality is generally harmful for the pace and sustainability of economic growth over the medium run (e.g. Persson and Tabellini 1994; Easterly 2007; Halter et al. 2014), the policy implications are far from clear. The difficulty is easy to see. Inequality may impede growth at least in part *because* it calls forth efforts to redistribute through the fiscal system that themselves may undermine growth. While the literature on this score remains controversial, the notion of a tradeoff between redistribution and growth seems well-embedded in policy makers' consciousness. In such a situation, even if inequality is bad for growth, taxes and transfers may be precisely the wrong remedy.

The two separate mechanisms—how redistribution affects growth and how inequality affects growth—have been examined empirically.[1] However the literature almost without exception does not examine the role of both redistribution and inequality in growth in a common empirical framework. One reason for the lack of this joint analysis is that usable cross-country data that distinguish between inequality before and after taxes and transfers (market and net inequality) are very scarce and imperfect.

In this paper, we extend the empirical literature in several ways. *First*, we include both redistribution and inequality as drivers of growth, so that we can identify the direct effects of both inequality and redistribution on growth. This allows us to see whether inequality matters for growth independent of its effects on redistribution. With some additional assumptions, we can also make some inferences about how redistribution may affect growth indirectly, through its effects on inequality. *Second*, we take advantage of a recently-compiled cross-country dataset (Solt 2009) that carefully distinguishes net from market inequality and allows us to calculate redistributive transfers—defined as the difference between the Gini coefficient for market and for net inequality—for a large number of country-year observations covering both advanced and developing countries. This method of calculating redistribution has two important advantages. First, it focuses on the actual redistributive outcome of fiscal policy. And second, it produces a measure of redistribution in Gini points, facilitating comparison with the inequality measure itself. *Third*, we analyze both the growth rate over 5 year horizons (panel growth regressions) as well as the duration of growth *spells*, as defined in Berg et al. (2012), which we think is a more salient way of assessing growth experience, especially for emerging and developing economies. We also look for nonlinearities in the relationships, in order to get at the notion that redistributive policies may be benign or harmful depending on

[1] See for example Perotti (1996), Forbes (2000), Barro (2000), Panizza (2002), Banerjee and Duflo (2003) and Voitchovsky (2005).

how extensive they are, and the idea that the growth–inequality relation may itself depend on the extent of inequality. And *finally*, we examine evidence for the key channels linking growth to inequality and redistribution.

Our principal findings can be summarized as follows. *First*, lower net inequality is strongly and robustly correlated with faster and more durable growth, controlling for the effect of redistribution. *Second*, redistribution appears generally benign in terms of its impact on growth; only when redistribution is very large is there some evidence that it may have direct negative effects on the durability of growth. *Third*, we find preliminary evidence that inequality's impact on growth works through lower education and life expectancy, and higher fertility.

Our results are consistent across the variety of estimation methods, data samples, and robustness checks that we employ. Our two main approaches (panel growth regressions and spell analysis using duration models) involve different conceptions of the growth process, functional forms, and approaches to identification. These approaches therefore have different advantages and are subject to different flaws. Our results are also consistent with the basic impression provided by the raw data. In sum, they seem unlikely to be due to specific features of the techniques we apply to the data, with all their possible shortcomings. Insofar as there is overlap, our results, including on the main channels, also seem to be consonant with the tenor of the empirical literature.

The remainder of this paper is organized as follows. Section 2 reviews the literature on growth, inequality and redistribution. Section 3 presents the data and some stylized facts. Section 4 presents the empirical results for the panel growth regressions and Sect. 5 for the growth spell survival analysis. Section 6 assesses the channels of transmission from inequality to growth. Section 7 conducts a variety of robustness tests. Section 8 concludes.

2 A review of the theoretical and empirical literature

To set a frame for a review of this literature, it is worth spending a moment to understand the channels among the different variables (inequality, redistribution, and growth), summarized in Fig. 1, and why theory does not provide strong guidance on these questions.

The classical literature emphasized the channels through which inequality can promote growth (line E in Fig. 1) by fostering incentives for innovation and entrepreneurship (Lazear and Rosen 1981); raising saving and investment if rich people save a higher fraction of their income (Kaldor 1957); and allowing at least a few individuals to accumulate the minimum needed to start businesses and get a good education (Barro 2000). In contrast, more recent work has stressed that inequality may be harmful for growth. Galor and Zeira (1993) emphasize the interaction of credit market imperfections and fixed costs of investment in education, showing that inequality can lead to under-investment in human capital and reduce growth.[2] Galor and Moav (2004) propose a unified theory of inequality and growth which argues that the effect of inequality depends on the relative return to physical and human capital. When the relative return to physical capital is high, characteristic of the earliest phases of industrialization, inequality is beneficial for growth. In the modern growth regime, human capital accumulation becomes the main driver of growth and inequality reduces growth. Along related lines, de la Croix and Doepke (2003) argue that inequality increases the fertility of the poor and hence reduces human capital accumulation and growth.

An unequal distribution of income may also lower growth by increasing social and political instability. This creates disruption and diverts resources from productive activities, leads to

[2] See also Perotti (1996) and Aghion et al. (1999).

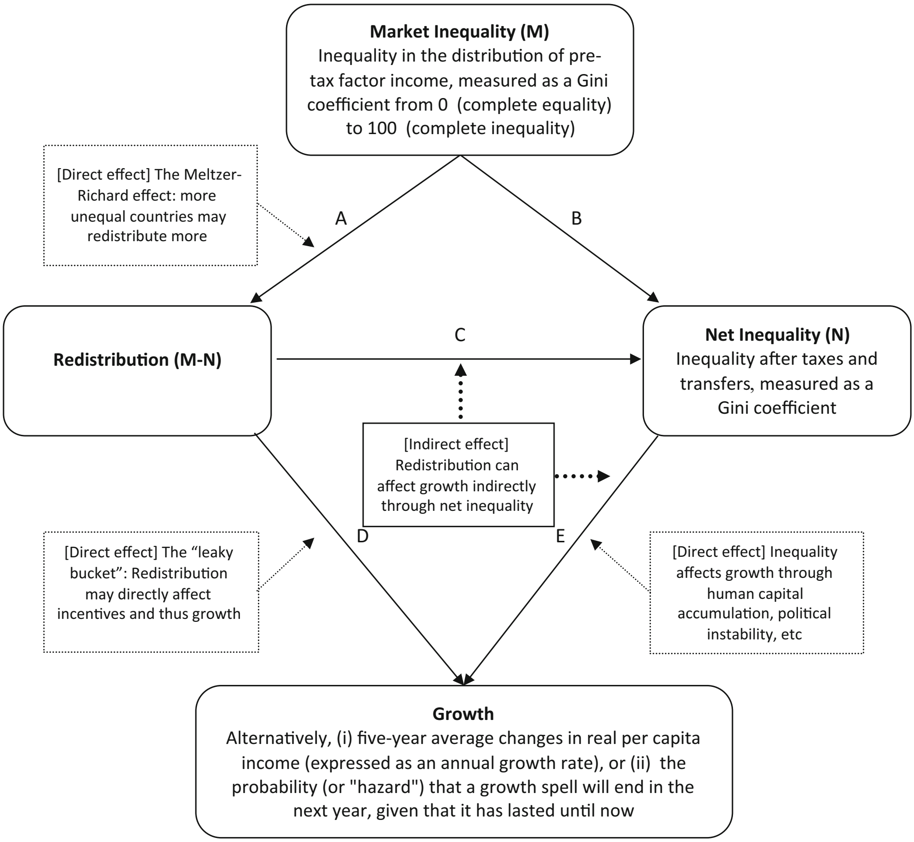

Fig. 1 Interrelationships between inequality, redistribution and growth. *Note* This picture shows the main channels of influence investigated in this paper. We estimate econometrically the direct effects of redistribution (line D) and net inequality (line E), in each case in effect holding the value of the other variable constant. We also calculate the "total effect" of redistribution on growth. We assume that redistribution does not affect market inequality, so redistribution affects net inequality one-for-one. The total effect is thus the sum of the estimated direct effect (line D) and the indirect effect, which is a combination of the effect of redistribution on net inequality (line C) and the estimated direct effect of net inequality on growth (line E). There are many other arrows one could draw in the picture, such as from growth back to inequality and redistribution. In addition, there are possible channels that relate the levels of income, inequality, and redistribution. The paper emphasizes those shown here, as discussed in the text

coordination failure or lower efficiency-enhancing cooperation, and creates uncertainty in the politico-economic environment which creates disincentives to investment and generates instability that reduces investment (Alesina and Perotti 1996). Less equal countries may also experience ethnic tensions and social polarization which reduce the security of property and contract rights and growth (Keefer and Knack 2002); and impede the social consensus required to adjust to shocks and sustain growth (Rodrik 1999).[3]

When considering policy responses to inequality, the political economy channel (Alesina and Rodrik 1994; Persson and Tabellini 1994; Benabou 1996; Perotti 1996), in which high

[3] The relationship between inequality and growth may be nonlinear, as in the theoretical model of Benhabib (2003), in which increases in inequality from low levels provide growth-enhancing incentives, while increases past some point encourage rent-seeking and lower growth.

inequality generates pressures for distortionary fiscal redistribution that itself reduces growth, looms large. The idea that inequality causes redistribution (line A in Fig. 1) is based on the seminal paper by Meltzer and Richard (1981) who argue that, because political power is more evenly distributed than economic power, the median voter will have the power and incentive to vote for redistribution. This political-economy channel has been strongly challenged both theoretically and empirically, along the lines that inequality permits better-endowed agents to block redistribution (Perotti 1996; Benabou 2000; Galor et al. 2009).

On the issue of redistribution and growth, the policy literature has focused on the direct effect (line D in Fig. 1) and generally assumed that redistribution hurts growth (Okun 1975), as higher taxes and subsidies dampen incentives to work and invest. Losses are likely to be an increasing and convex function of the tax or subsidy rate, given the convexity of deadweight costs, with losses from redistribution minimal when tax rates are low but rising steeply with the tax or subsidy rate (e.g. Barro 1990; Jaimovich and Rebelo 2012). However, some have recognized that redistribution need not be inherently detrimental to growth, to the degree that it involves reducing tax expenditures or loopholes that benefit the rich or as part of broader tax reforms (e.g. higher inheritance taxes offset by lower taxes on labor income). More broadly, redistribution can also occur when progressive taxes finance public investment or other pro-poor spending (the total incidence of the tax policy is what matters here), when social insurance spending enhances welfare of the poor (Benabou 2000), or when health and education spending that benefits the poor increases (Saint-Paul and Verdier 1993, 1997). In such cases, redistributive policies could increase both equality and growth. Overall, the literature has ignored the question of how redistribution may affect growth both directly (line D in Fig. 1) and as it acts through inequality (lines C and E in Fig. 1).

Because theory provides a partial guide on these issues, we turn to empirical research to verify and quantify that importance of the various theoretical channels proposed. With respect to inequality and growth, the statistical evidence generally supports the view that inequality impedes growth, at least over the medium term. In a sequence that mirrors intellectual fashions on the empirics of growth, researchers have looked at rates of growth over long periods of time (e.g. Alesina and Rodrik 1994; Persson and Tabellini 1994; Perotti 1996), the level of income across countries (Easterly 2007), and the duration of growth spells (Berg et al. 2012), and have found that inequality is associated with slower and less durable growth. Other studies allow the possibility of non-linearities in the relationship between inequality and growth (Banerjee and Duflo 2003) or use panel data techniques that emphasize short-run correlations (Forbes 2000).[4]

The evidence on the relationship between inequality and redistributive transfers is not clear-cut. This may stem from the fact that most studies have measured neither inequality nor redistribution appropriately (Milanovic 2000, 2010). In earlier work, inequality is generally measured as net inequality, when market inequality would be the appropriate concept for assessing effects on redistribution. And almost all studies use various proxies of redistribution, such as social spending or tax rates (Benabou 1996; Perotti 1996; Bassett et al. 1999). While such spending may seem redistributive (e.g. education or social insurance spending), it need not be in practice: for example, spending on post-secondary education in poor countries or on social protection for formal sector workers in many developing countries.[5]

[4] In particular, as emphasized in Halter et al. (2014), estimates based on time-series variation only (e.g. estimations relying on fixed-effects or first-differences estimators such as those in Forbes 2000) typically pick up only the (positive) short-run effects.

[5] Milanovic (2000) uses data on market and net inequality for a small set of mostly OECD countries that allow direct measurement of redistribution market inequality. He shows that with these data, the evidence is supportive of the Meltzer–Richard hypothesis: more unequal societies do engage in more redistribution.

Empirical studies on the relationship between redistribution and growth are also somewhat divided. Studies that look at presumptive indicators of redistribution (such as taxes or government spending) tend to suggest that more redistribution is detrimental to growth. However, there is perhaps surprisingly little clear evidence that increases in tax rates impede medium-to-long-run economic growth. Overall, it seems hard to improve on the conclusions of Tanzi and Zee (1997), who find some general indication that the relationship between growth and the level of total taxes or of income taxes is negative, but that this relationship is not robust and is sensitive to model specification. With respect to spending, Lindert (2004) sees something of a "free lunch" paradox in that some categories of public spending that are redistributive have no apparent adverse impact on growth (for example, spending on health and education, or tax-financed infrastructure spending).[6]

It bears emphasizing that the literature has found it difficult to disentangle definitively cause and effect in these relationships. One strand has examined variations in inequality that are arguably exogenous and looked at resulting implications for the level of income.[7] Another has relied on the use of lagged information to try to tease out cause and effect. With both approaches, the literature is suggestive that causality runs from inequality to growth, but caution is still warranted in interpreting the correlations.[8]

What does one make, then, of this enormous literature? Three lessons stand out. *First* is the criticality of using data that are appropriate to the question at hand. For instance, the inequality that would affect transfers would seem to be market inequality, since presumably it is on the basis of pre-tax incomes that opinions would be formed about the need for redistribution. Meanwhile in principle the effects on growth generally depend on net (post-tax) inequality, which affects incentives as well as prospects for social stability and consensus. Yet most previous studies paid little attention to this critical distinction, and often combined pre- and post-tax data. *Second* is to be aware that there are complicated interconnections across the different variables of interest: growth, inequality, and redistribution—implying the need for a joint empirical analysis. *Third* is to be open-minded as to what the empirical analysis may show: the theory has multiple possible channels in play—this is especially true for the different proxies of redistribution and their effects on growth.

3 The data

A defining constraint of previous studies on inequality and growth is the lack of data on both net and market inequality measures on a comparable basis for a large number of countries.[9] Solt (2009) represents the only systematic effort so far to address this gap for a large sample of countries.[10] He combines information from available surveys to infer comparable series for net and market inequality. He defines net inequality as that associated with income after direct taxes and subsidies and market inequality as pre-tax and pre-subsidy income. These data can

6 Benabou (1996) finds that growth is positively related to some categories of social spending.

7 Easterly (2007) examines the effects of inequality instrumented with colonial landholding patterns and argues that contemporary inequality is the legacy of plantation-based agricultural systems dictated by geography.

8 Bourguignon (2004) examines the interactions between inequality and growth and how they relate to absolute poverty. In his Poverty–Growth–Inequality "Triangle Model", Bourguignon suggests that changes in poverty depend on growth, income distribution and changes in income distribution, so reducing poverty requires a combination of country-wide policies focused on growth *and* reducing inequality.

9 For surveys, see Atkinson and Brandolini (2001, 2009).

10 Solt labels his database "Standardized World Income Inequality Database" (SWIID). Other sources exist albeit for smaller samples, such as the advanced countries, as discussed below in the robustness section.

be used to compute measures of redistributive transfers as the difference between market and net Ginis.[11] Importantly, Solt (2009) addresses the non-comparability of the various surveys that underlie the data. For some surveys the unit of analysis is the household; for others it is the individual. And some surveys look at pre-tax gross income (including transfers), others at factor or market income, and others still at disposable income or expenditures.[12] Solt (2016) further discusses the methodology and the validity of his data, and also constructs standard errors associated with the inequality estimates. While no cross-country database, perhaps particularly on inequality, can be considered fully reliable, the SWIID appears superior to the alternatives for our purposes.[13]

It is important to underscore that "redistribution" as defined in this paper and as measured in the Solt dataset does *not* attempt to capture all the effects of government on the income distribution. The government influences the distribution of income in a large number of potentially important ways beyond the fiscal redistribution that is the focus of this paper. A few examples include setting government wages, influencing relative prices through tariffs and subsidies, establishing minimum wages and other labor market policies, and providing in-kind goods and services such as health care and education. Rather than being included in redistribution as defined and measured here and in general in the literature, they are reflected in the level of "market" inequality, which is thus something of a misnomer, albeit a well-established one in the literature.[14] An examination of these aspects of the government's role in distribution is well beyond the scope of this paper, however. The focus here is on the growth implications of inequality—among the drivers of which may be these sorts of government policies—and of fiscal redistribution.

Health and education spending, in particular, is both likely to be important for inequality and similar conceptually to the redistribution we are considering. We might want to control for these other effects of government on inequality in isolating the effects of fiscal redistribution. However, essentially no large cross-country inequality databases directly capture the effects of these categories of spending. It is difficult to generalize about the distributional consequences of health and education spending in developing countries, as the incidence is often not particularly pro-poor in many cases (e.g. spending on urban hospitals or tertiary education in low-income countries; IMF 2014). Moreover, the highly variable role of the state in the provision of these services further complicates cross-country comparisons.

An important complication is that the analysis of redistribution, constructed as the difference between two already-noisy series, demands more of the data than does the analysis of inequality itself. We follow Solt (2009) and restrict the sample to the more reliable data. In particular, in our baseline sample we strike out a set of specific observations where Solt con-

[11] Solt follows the definitions of net and market inequality in the Luxembourg Income Study (LIS), a harmonized set of income inequality data for a number of upper and middle-income countries. These definitions do not capture the provision of most in-kind health and education services by the government or of indirect taxes (as discussed below). Other transfers are in principle captured in the difference between market and net income.

[12] Atkinson and Brandolini (2001) discuss inequality data quality and consistency. They point out that differences between the various welfare definitions and equivalence scales vary across countries and over time, and that "definitional differences interact." The SWIID incorporates Atkinson and Brandolini's recommendations to provide the most comparable data available (Solt 2015). For example, the SWIID's adjustments are calculated for each distinct combination of definition and scale.

[13] Jenkins (2014) compares WIID with Solt's SWIID and challenges various aspects of the SWIID database. However, Solt (2015) argues that the SWIID database remains useful for cross-country work. He also shows that Jenkins (2014) even more clearly exposes the great difficulties involved in doing cross-country work with other databases such as the WIID that are not standardized.

[14] That the term "market inequality" is potentially misleading is emphasized, for example, in Stiglitz (2012).

Table 1 Correlation between redistribution and transfers. *Source*: SWIID 3.1, World Bank World Development Indicators (2013), OECD National Accounts database (2012), and authors' calculations

Name of transfers variable	Correlation coefficient
Tax revenue (% of GDP) (WBWDI)	0.51
Subsidies and other transfers (% of expense) (WBWDI)	0.49
Social security benefits paid by general government/GDP (OECD)	0.55
Current transfers received by households/GDP (OECD)	0.52
Subsidies/GDP (OECD)	0.42
Social expenditure in percentage of GDP (OECD)	0.68
Total tax revenue as percentage of GDP (OECD)	0.70

Redistribution is calculated as the difference of market and net income inequality

cludes that the raw surveys are unreliable. In addition, we require that one of two conditions be satisfied in order to include an observation, each designed to ensure that the redistribution measure is informative. In particular, we require either (a) that the country contain at least one survey of some sort of net concept (e.g. disposable income or expenditure) *and* one pre-tax concept (e.g. personal market income), so that there is country-specific information on redistribution itself from the survey data for that country; or (b) that uncertainty associated with estimated redistribution is very small relative to the size of redistribution.[15] In sum, the baseline sample balances the objectives of exploiting all the information available in the data with the advantages of focusing on the highest-quality data. For robustness, we also consider below various restricted samples.

Table 1 presents some correlations between the Gini-based measure of redistributive transfers and a number of proxies used in the earlier literature. The table shows that the correlations are in the range of roughly 0.50–0.75. We find it reassuring both that our measure is highly correlated with many common-sense direct measures of transfers, and that it may also contain unique information. This is consistent with the observation that many presumptively redistributive transfers may not be so in particular cases.

The Gini-based measure of redistribution can also be used to assess its relation with market-based inequality measures. Do countries with more market inequality tend to redistribute more? In Fig. 2, most countries lie below the line, implying some degree of redistribution and, on average, the distance from the line grows with the amount of market inequality, showing that relatively unequal countries do tend to redistribute more. These impressions are supported by regressions reported in "Appendix C", which confirm that more unequal countries do tend to redistribute more, with a stronger effect in the OECD sample.

Finally, before turning to formal statistical analyses, we plot the data on inequality, redistribution, and growth in our sample. Figure 3 shows a strong negative relationship between net inequality and growth in income per capita over the subsequent period (top panel), and a weak indication of a *positive* relation between redistribution and subsequent growth (bottom panel). Figure 4 explores the relationship between the length of growth spells (defined in Sect. 5 below) and inequality and redistribution at the beginning of the spell. It shows that higher inequality is associated with shorter growth spells, while the association between redistribution and the length of growth spells is weak. These are essential facts to bear in mind as they do not depend on specific estimation techniques that are explored below.

[15] Summary statistics are provided in "Appendix A". "Appendix B" presents variables and summary statistics of the market and net inequality data, redistribution, and the rest of the variables used in the analysis.

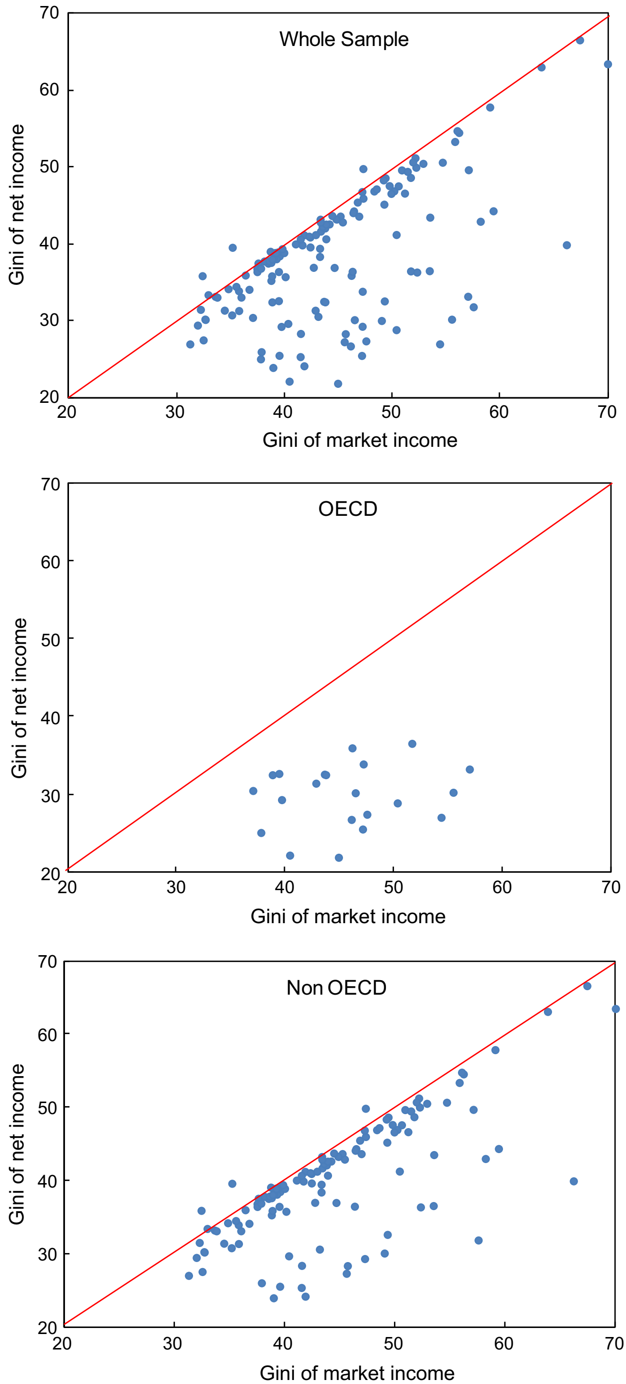

Fig. 2 Market and net inequality by country group sample. *Note* Latest available year of market and net inequality; the line represents the 45 degree line. *Source*: SWIID 3.1 and authors' calculations

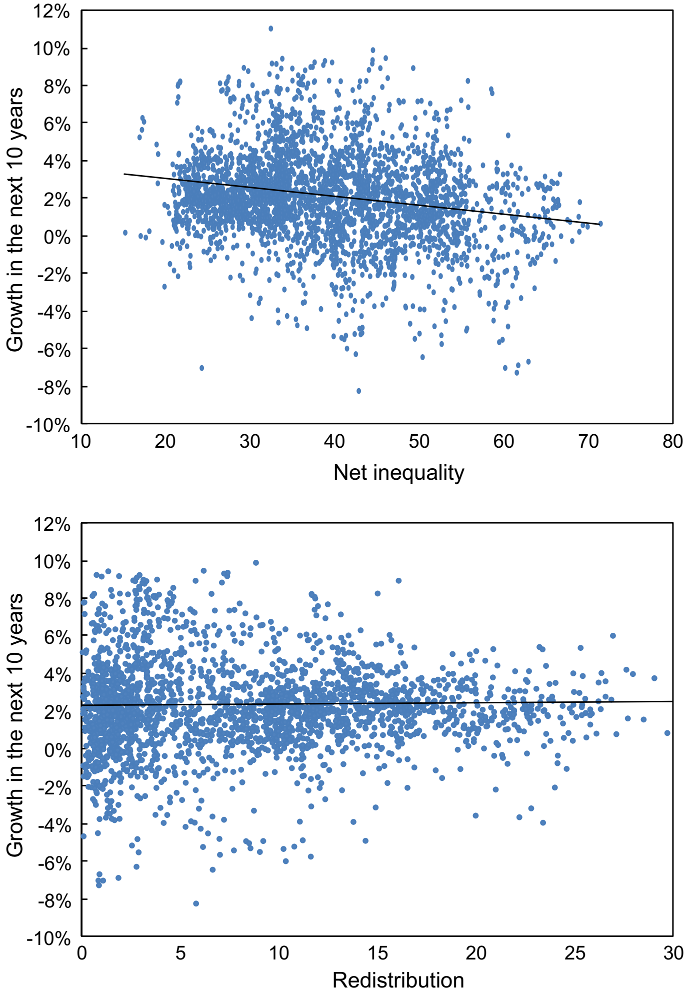

Fig. 3 Growth, inequality, and redistribution. *Note* Simple correlations between growth in the next 10 years, and the average net income inequality and transfers for a sample of countries. *Source*: Penn World Tables version 7.1, SWIID 3.1, and authors' calculations

4 Panel growth regressions

We suppose that growth in per capita GDP for country i at time t, $g_{i,t}$, is described by

$$g_{i,t} = f(Y_{i,t}, \alpha N_{i,t}, \beta R_{i,t}, \gamma' Z_{i,t}) \tag{1}$$

where $Y_{i,t}$ is the initial GDP per capita, $N_{i,t}$ is net inequality and $R_{i,t}$ is redistribution (measured as gross *minus* net inequality Ginis) and $Z_{i,t}$ is a vector of other controls. With this specification (and assuming a linear form of (1) for simplicity), holding the control variables constant, we can ask several questions:

- Does inequality matter for growth, independent of any effect it may have on redistribution?

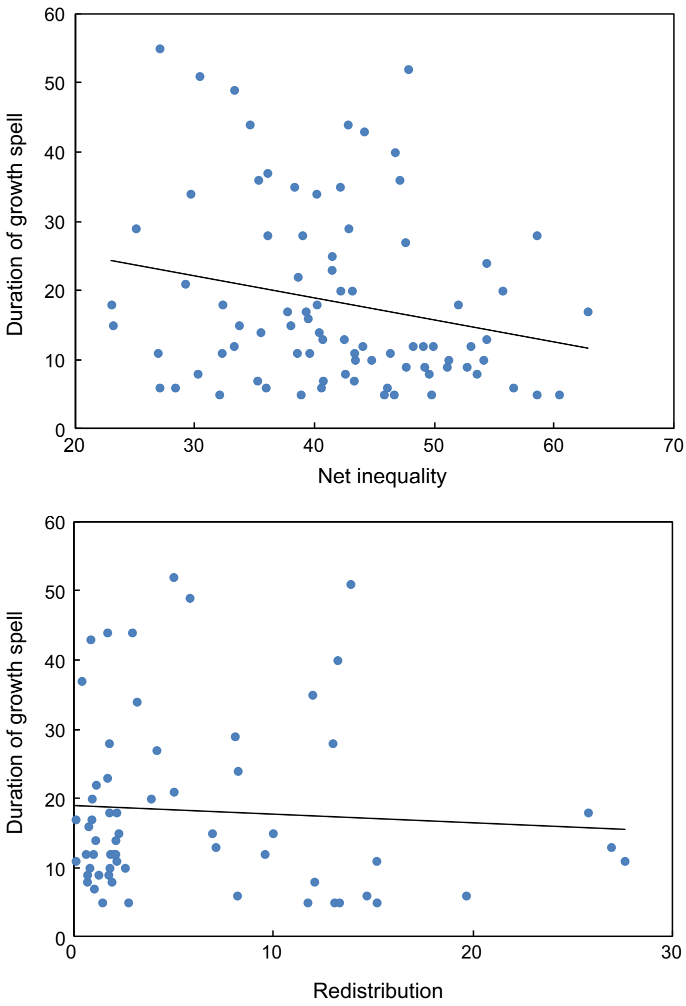

Fig. 4 Duration of growth spells, inequality, and redistribution. *Note* Simple correlation between length of growth spells, and the average net income inequality and transfers during the spell. Spells that end in-sample are included; minimum spell length is 5 years. *Source*: Penn World Tables version 7.1, SWIID 3.1, and authors' calculations

- Does redistribution matter for growth, independent of any effect it may have on inequality?
- Do the indirect effects of redistribution acting through inequality outweigh the direct effects of redistribution ($|\alpha| > |\beta|$?).

To analyze this last question, we define the overall effect of redistribution on growth as the direct growth effect of redistribution plus the growth effect of the resulting reduction in inequality. In making this calculation, we assume that redistribution does not affect market income, in line with Paulus et al. (2009), OECD (2011) and Caminada et al. (2012), who assume that redistributive policies reduce net inequality, given market inequality. While redistribution can influence behavior in ways that may change labor supply and market wages and thus market inequality as well, there is little cross-country evidence to serve as a guide on the sign and overall magnitude of the effects, particularly since there are two potentially off-setting effects: redistribution is likely to reduce the labor supply of both the rich (who are taxed more) and the poor (insofar as they receive means-tested benefits that reduce incentives to work).[16] If there is a trade-off between redistribution and growth, the coefficient on redistribution (β) should be more negative than that on inequality (α).[17]

Consistent with the empirical literature on cross-country comparisons of economic growth (e.g. Mankiw et al. 1992; Caselli et al. 1996), our specification models the level of the steady-state growth path as a function of initial income per capita, population growth, and proxies for the stocks of human and physical capital (average years of schooling, and the average investment ratio to income, respectively). We have the following specification for the ith country in the tth period

$$\ln Y_{i,t} - \ln Y_{i,t-\tau} = \tilde{\gamma}_0 \ln Y_{i,t-\tau} + \alpha N_{i,t} + \beta R_{i,t} + \gamma_1' Z_{i,t-\tau} + u_i + v_t + \varepsilon_{i,t} \tag{2}$$

where $Y_{i,t}$ is per capita GDP in country i,period t; $N_{i,t}$, $R_{i,t}$, and $Z_{i,t}$ consist of proxies for inequality, redistribution, and other determinants of economic growth; u_i is a country-specific effect; v_t are period dummies; and $\lambda_{i,t}$ is an overall error term.[18] Equation (2) can be written as a dynamic equation with the lagged dependent variable on the right-hand-side with $\gamma_0 = \tilde{\gamma}_0 + 1$ and $\ln Y_{i.t} = y_{i,t}$ where $W_{i,t}$ consists of proxies for inequality and redistribution and other determinants of economic growth, as follows

$$y_{i,t} = \gamma_0 y_{i,t-\tau} + \gamma_1' W_{i,t-\tau} + u_i + v_t + \varepsilon_{i,t}. \tag{3}$$

To simultaneously address both omitted variable bias and endogeneity issues, the cross-country growth empirics literature typically uses a generalized method of moments (GMM) estimator where potentially endogenous right-hand side variables are instrumented using appropriate lagged values and differences. One such estimator is the difference GMM (dGMM) estimator which involves taking the first difference of the levels Eq. (3) thus eliminating the individual effect u_i (see Arellano and Bond 1991).

$$\begin{aligned} y_{i,t} - y_{i,t-\tau} = {} & \gamma_0 (y_{i,t-\tau} - y_{i,t-2\tau}) + \gamma_1' (W_{i,t-\tau} - W_{i,t-2\tau}) + (v_t - v_{t-\tau}) \\ & + (\varepsilon_{i,t} - \varepsilon_{i,t-\tau}). \end{aligned} \tag{4}$$

Removing the cross-sectional variation may not be desirable for our investigation, however, particularly because within-country inequality is quite persistent. Therefore, our preferred estimator is the systems GMM (sGMM) estimator proposed by Arellano and Bover (1995) and Blundell and Bond (1998), which estimates the level Eq. (3) and the difference Eq. (4) as a system, and appropriately exploits both time series and cross-sectional variation in the data. Following earlier studies in the literature, we rely on 5-year non-overlapping time intervals ($\tau = 5$).

[16] As discussed in "Appendix C", we find that Granger causality tests reject causality from redistribution in our sample, but not the reverse. We investigate the market inequality–redistribution relationship empirically in the next section.

[17] For the estimation of the direct effects of inequality and redistribution on growth, but not for the overall effects, we can be agnostic about whether there is significant two-way causality between redistribution and market inequality, because our multivariate techniques isolate the effects of each variable holding the other constant.

[18] Our timing convention, which is standard in the literature, implies that for inequality and redistribution, $Z_{i,t-\tau}$ is measured as the average over the period from $t-1$ to $t-\tau$. As a robustness check we experimented with different lag structures and found that lagging inequality generally resulted in similar but attenuated effects.

We are generally agnostic about the drivers of inequality and redistribution. However, we believe that there are both slow-moving factors that are mainly picked up in the cross-country variation, such as historical legacies as discussed in Easterly (2007), and factors that will manifest at 5-year frequencies. For example, terms of trade shocks or policy changes (e.g. input subsidies, tariff structures) can quickly change the rural/urban terms of trade and thus income distribution in the many countries where there are large rural/urban wage differentials: see for example Deininger and Okidi (2003), IMF (2015) and Adams et al. (2018).

To isolate causal effects, sGMM relies on a set of internal instruments, namely the lagged levels and differences of the explanatory variables. We rely on a set of internal instruments, consisting of various lagged levels and differences of right-hand-side variables, in order to identify the parameters of interest. As is typical in the literature, we make the following standard identifying assumptions: (1) the error term is uncorrelated over time; (2) inequality and redistribution are predetermined with respect to growth in the subsequent period; and (3) current and lagged differences of the right-hand side variables in the levels equation are valid instruments in the levels equation. This latter assumption requires in particular the initial deviations of output from steady state are not correlated with the fixed effects—what Roodman (2009) calls a stationarity assumption.

A concern with sGMM estimation is that sensitivity to slight variations in instrumentation strategy, and a proliferation of instruments, may reflect problems of weak or invalid instruments. With respect to validity, we present Hansen tests for overidentifying restrictions and the first and second-order residual autocorrelation tests in order to confirm the validity of the instruments. We examine difference-in-Hansen tests that speak to the stationarity assumption. In the robustness Sect. 7 we also examine directly the sensitivity of our results to small variations in instrumentation strategy. With respect to instrument strength, where the concern is that inference may be unreliable even in large samples if instruments are weak, the literature as it applies to sGMM is rapidly developing, complicated by the fact that tests applied to instrumental variable estimation do not extend to the case of GMM (where internal instruments are used); the application of weak instrument robust methods to sGMM is lacking; and the use of projection methods to construct weak instrument robust confidence intervals costs power, as the number of endogenous variables increases.[19] In our robustness section we apply several approaches to address these concerns and examine the robustness of our results.

Our basic specification is a stripped-down standard model in which growth depends on initial income, inequality, and redistribution (Table 2 [1]). We find that higher inequality seems to lower growth. Redistribution, in contrast, has a slightly positive—and insignificant—effect. To get an idea about the magnitude of the effects, suppose we increase inequality or redistribution from the median value in the sample to the 60th percentile. This 10-percentile increase in net Gini decreases growth on average by 0.48 percentage points per year, holding redistribution and initial income constant. A 10-percentile increase in redistribution, by contrast, increases the growth rate slightly, controlling for inequality and initial income.[20]

[19] See Stock et al. (2002). Kraay (2015) analyzes the implications of weak instruments for Ostry et al. (2014), which is a precursor to this paper with a more limited robustness analysis.

[20] While our preferred specification is one that includes both inequality and redistribution, for completeness, the Online Appendix Table 1.1 presents the results for alternative specifications. When first net and then market inequality enters separately in a specification with initial income as the only control, we find strong negative effects for both; when redistribution enters alone it is still insignificant; and when both net and market inequality are included as regressors, only net inequality is statistically significant (with a negative effect). We thank one of the referees for the suggestion.

Table 2 The effect of inequality and redistribution on growth. *Source*: Income, investment/GDP, population growth and openness (Penn World Tables 7.1); redistribution and Gini (SWIID 3.1); average years of primary and secondary schooling (Barro and Lee 2013); political institutions from − 10 (most autocratic) to 10 (most democratic) (Polity IV); external debt/GDP (data from Lane and Milesi-Ferretti 2007, updated and extended by Milesi-Ferretti in 2011); goods terms-of-trade = 1 when the annual change is in the bottom 3 deciles (WEO). For details see Berg et al. (2012)

	Benchmark	Benchmark + nonlinear effects	Benchmark + controls		
	(1)	(2)	(3)	(4)	(5)
Log(initial income)	− 0.0069**	− 0.0014	− 0.0081**	− 0.0140***	− 0.0135***
	(0.0034)	(0.0033)	(0.0035)	(0.0037)	(0.0046)
Net inequality	− 0.1435***		− 0.0914***	− 0.0739***	− 0.1057**
	(0.0444)		(0.0336)	(0.0266)	(0.0492)
Redistribution	0.0046		0.0258	0.0109	0.0530
	(0.0492)		(0.0516)	(0.0428)	(0.0494)
Net inequality at top 25%		− 0.1258***			
		(0.0406)			
Net inequality at bottom 75%		− 0.1246**			
		(0.0510)			
Redistribution at top 25%		− 0.0668			
		(0.0432)			
Redistribution at bottom 75%		0.0323			
		(0.0548)			
Log(investments)			0.0241***	0.0250***	0.0076
			(0.0078)	(0.0084)	(0.0125)
Log(population growth + 5)			− 0.0159	− 0.0215	− 0.0084
			(0.0182)	(0.0174)	(0.0160)
Log(total education)				0.0206***	0.0164*
				(0.0073)	(0.0099)
Large negative TOT shock					− 0.0424***
					(0.0158)
Political institutions					− 0.0011
					(0.0008)
Openness					0.0091
					(0.0082)
Debt liabilities					− 0.0198***
					(0.0059)
Constant	0.1262***	0.0743*	0.0718	0.0965**	0.1687***
	(0.0389)	(0.0401)	(0.0457)	(0.0389)	(0.0573)
Observations	828	828	828	751	558
Number of groups	130	130	130	110	79

Table 2 continued

	Benchmark	Benchmark + nonlinear effects	Benchmark + controls		
	(1)	(2)	(3)	(4)	(5)
Number of instruments	117	144	133	139	100
AR1 test (*p* values)	0.0000	0.0000	0.0000	0.0000	0.0000
AR2 test (*p* values)	0.1639	0.1350	0.1204	0.1818	0.4780
Hansen test of joint instrument validity (*p* values)	0.3748	0.8366	0.4379	0.9229	0.9244
Difference-in-Hansen tests of instrument subsets (*p* values)					
Instruments for levels	0.9618	0.9994	0.9967	1.0000	0.9996
Instruments for initial income	0.9849	0.9982	0.6980	1.0000	0.9896
Instruments for IV-type	0.8245	0.9975	0.7299	0.6092	1.0000
p values for tests					
Redistribution = inequality	0.0099		0.0158	0.0411	0.0010
Redistribution top 25% = inequality		0.2560			
Redistribution bottom 75% = inequality		0.0304			
Inequality top 25% = inequality bottom 75%		0.9445			
Redistribution top 25% = redistribution bottom 75%		0.0257			

System GMM estimation. Robust standard errors in brackets where *, **, and *** statistical significance at the 10, 5 and 1% levels, respectively

Table 2 columns [2]–[5] represent various plausible specifications. First, we add non-linear effects. As discussed in the literature review, it is plausible that a given increase in inequality may be more harmful for growth if the level of inequality is already high. If we allow the effect of inequality to differ when the level of inequality is already high (say at above the 75th percentile), we find no evidence of such nonlinearities.[21] Similarly, we find no evidence of such nonlinear effects of redistribution on growth. Next, we add physical and human capital in Table 2 [3] and [4], and a number of additional standard growth determinants such as external shocks, the quality of institutions, and measures of openness to trade (Table 2 [5]).

The main lesson from these columns is that the inclusion of these additional determinants does not change our findings about inequality and redistribution. In particular, inequality is

[21] We find no evidence for a break in the inequality–growth or the redistribution–growth relationship from the 50th through the 90th percentile of redistribution.

always significant while redistribution is not. Thus, it does not seem that these additional controls are driving the impacts of either inequality or redistribution on growth. However, the inclusion of these controls seems to (marginally) reduce the estimated direct effect of inequality on growth, suggesting that investment, population growth, and human capital are potential channels through which inequality affects growth.[22] We return to the question of channels below.

We can interpret the coefficient on net inequality [α in Eq. (2)] as the direct effect (line D in Fig. 1) of inequality on growth, for a given level of redistribution. Similarly, the coefficient on redistribution β is a measure of the direct effect of redistribution, holding net inequality constant. If we assume in addition, and consistent with the evidence above, that redistribution does not cause market inequality, then we can interpret $\beta - \alpha$ as capturing the total effect (direct and acting through inequality) of redistribution on growth. Thus, we report in Table 2 the p-values for the hypothesis that $\beta = \alpha$. The results are inconsistent with the notion that there is on average a major trade-off between redistribution and growth: the coefficient on net inequality in all columns of Table 2 is statistically more negative than that on redistribution.

Redistribution, and market and net inequality are perfectly linearly related, from the identity $R \equiv M - N$. We can therefore include any two of these variables in the regression: the residuals would be the same, with the only difference being that the coefficients require different interpretations, and the null hypotheses being tested by the t-statistics are different. In particular, we could replace net with market inequality in Eq. (1):

$$g_{i,t} = \alpha M_{i,t} + (\beta - \alpha) R_{i,t} + \gamma' Z_{i,t} \tag{5}$$

where α is the coefficient on net inequality and β is the coefficient on redistribution in Eq. (1). The coefficient on (market) inequality still measures the direct effect of inequality on growth, holding redistribution constant. The coefficient on redistribution now measures the effect of changing redistribution while holding market (not net) inequality constant. Thus, it measures the *total* effect of redistribution on growth (lines C, D, and E in Fig. 1). In Table 1.2 in the Online Appendix we estimate Eq. (5) for the same specifications as those presented in Table 2. Our results are, of course, preserved: higher market inequality is associated with lower growth, while the total effect of redistribution is positive and statistically significant.[23] While the two different specifications [Eqs. (1), (5)] are statistically equivalent, we use net inequality as our benchmark, since the basic theory relating inequality to growth concerns net inequality.

The combination of the use of econometric methods and a dataset that includes net and market inequality (and thus redistribution) measures on a comparable basis for a large number of countries, helps us to better understand the growth–inequality–redistribution relationship

[22] As discussed earlier, a typical concern with sGMM estimation relates to the strength and validity of instruments as well as robustness to variations in the instrumentation strategy. Our empirical approach was motivated by an effort to maintain consistency in the instruments used across specifications while abiding by the Roodman (2006) "minimally arbitrary rule of thumb" to keep the number of instruments around the same number as the cross sections. While some specifications in Table 2—particularly those with additional controls which inevitably increase the number of instruments—may result in higher p values of for the test of over-identifying restrictions, a battery of robustness tests presented in Sect. 7 (including variations in the instrumentation strategy) support the overall conclusions of our paper.

[23] This presentation sheds light on the issues related to the potential measurement error of redistribution. The lack of significance of the estimated coefficient of redistribution in Table 2 could be interpreted as implying that the redistribution data are just too noisy to generate statistically significant results. Table 1.2 in the Online Appendix shows, however, that there is a statistically significant positive relationship between redistribution and growth, controlling for market inequality. We discuss below additional ways to address the potential measurement error in our robustness tests.

and to reconcile some of the findings in the literature. First, our conclusion that inequality has a negative effect on growth confirms findings in the literature that encompasses both the time series and cross-sectional variation (e.g. Panizza 2002; Halter et al. 2014). As emphasized by Halter et al. (2014), estimates based on time-series variation only (e.g. estimations relying on fixed-effects or first-differences estimators such as those in Li and Zou 1998; Forbes 2000), typically examining 5-year observations, find a positive impact of inequality on growth. On the other hand, estimation methods exploiting the cross-sectional variation in the data tend to find a negative relationship (e.g. Alesina and Rodrik 1994; Persson and Tabellini 1994; Deininger and Squire 1998; Barro 2000).

We demonstrate how the use of alternative estimation methods and data might influence the conclusions about the growth–inequality–redistribution relationship. In "Appendix D" we use the same dataset (thus ensuring an apples-to-apples comparison) and replicate some of the results from previous literature using alternative estimation methods. As shown in Table 9, estimators that rely only on time-series variation—FEw and dGMM in columns [2] and [5], respectively—e.g. Li and Zou (1998) and Forbes (2000), find a positive effect of inequality on growth (albeit insignificant for the case of FEw). Since within-country inequality is quite persistent, these estimators are not likely to capture the full picture. In addition, when the cross sectional variation is considered in the between estimator (BE), the growth–inequality relationship turns negative (but barely insignificant). Combining both cross-sectional and time series variation is indeed important. Estimations using RE (e.g. Barro 2000), and sGMM (e.g. Panizza 2002; Halter et al. 2014; and this paper) better identify the relationship between growth and inequality, which turns out to be statistically significant and negative.[24] Using sGMM has the additional advantage over RE in that potentially endogenous variables are appropriately instrumented, which explains the difference in magnitude of the estimated effect of inequality on growth between columns [3] and [6].

5 Survival analysis and growth spells

Many growth takeoffs fizzle after a few years: critical to successful development is the ability to *sustain* growth rather than (just) ignite it. We follow Berg et al. (2012) in focusing on "growth spells", defined as periods of at least 5 years during which growth is above 2% *and* significantly higher than during preceding years.

The analysis of growth spells and their determinants consists of estimating proportional hazard models to relate the probability that a growth spell will end to a set of time-varying covariates. Let T denote survival time (duration), a random variable with a cumulative distribution function $F(t) = \Pr(T \leq t)$. The survival function $S(t)$ is the complement of the distribution function $S(t) = \Pr(T > t) = 1 - F(t)$. Methods for analyzing survival data often focus on modeling the hazard rate which assesses the instantaneous risk of demise at time t, conditional on survival up to that time

$$h(t) = \lim_{\Delta t \to 0} \frac{\Pr[(t \leq T \leq t + \Delta t)|T \geq t]}{\Delta t} = \frac{f(t)}{S(t)}. \tag{6}$$

Duration is modeled by parametrizing the hazard rate and estimating the relevant parameters. Assuming a proportional hazards model and that the relationship between the hazard and covariate vector X_i is log linear, the "benchmark hazard" takes a particular functional form

24 A negative association between inequality and growth is found using simple cross-country OLS estimates (Column [1] of Table 9) in agreement with the findings presented in earlier studies (e.g. Alesina and Rodrik 1994; Persson and Tabellini 1994; Deininger and Squire 1998).

for subject i with covariate vector as

$$\lambda(t|X_i) = \lambda_0(t)\exp(X_i\beta) \tag{7}$$

where $\lambda_0(t)$is the benchmark hazard at time t and β is a vector of unknown parameters.

One potential problem in estimating (7) arises from the feedback of spell duration to the covariates. For example, a covariate may depend on whether or not a spell has ended or is still ongoing. It is possible to estimate (7) consistently if we assume that the hazard at time t conditional on the covariates at time t depends only on the lagged realizations of those covariates (Wooldridge 2002). Thus, we define the hazard at time t (now in discrete time) as the probability that the spell will end during period $t + 1$ conditional on time-varying covariates up to and including time t, including some variables measured at the beginning of the spell. This does not rule out all sources of endogeneity (for example through expectations that the end of a spell is imminent), but it should prevent bias through standard feedback from the end of a spell to potential determinants.

Our benchmark specification is represented in the first column of Table 3, where we relate the hazard to initial income at the start of the spell and to inequality and redistribution during the spell. The coefficient on each covariate represents the change in the probability that the spell will end in the next year associated with a one-unit change in the given independent variable. As with the growth regressions, there is some a priori reason to think that the effects of inequality and/or redistribution might be nonlinear. Table 3 [1] presents the results with the linear specification—both inequality and redistribution appear to be bad for the duration of growth spells. Turning to potential nonlinearities, there is no evidence of a nonlinear relationship between inequality and spell duration (Table 3 [2]). For redistribution, in contrast, we do find evidence for a nonlinear relationship between redistribution and spell duration. Thus, the specification in Table 3 [3] divides observations into those where the degree of redistribution is very large (the top 25th percentile of all observations) and where it is moderate (the rest of the distribution).[25]

Our first main result is that inequality has a statistically significant negative relationship with the duration of growth spells. A one-Gini-point increase in inequality is associated with a 6 percentage point higher risk that the spell will end the next year (or, equivalently, with a decrease in expected spell length of about 7%).[26] Turning to redistribution, we find that when redistribution is already high (above the upper quartile), there is evidence that further redistribution is indeed harmful to growth. When it is below that level, however, there is no evidence that further redistribution has any effect on growth. Figure 5 shows redistribution for selected countries; the dividing line for when further redistribution seems to start being growth-negative is when it amounts to 13 Gini points.

As with the growth regressions, we can ask about the overall effect of redistribution on growth, taking into account both direct and indirect effects. For very large-scale redistribution, the point estimate of the effect of redistribution on growth is negative and somewhat larger in absolute value than the estimated (positive) effect of inequality on growth. However, this difference is statistically insignificant, implying that even in the case of large-scale redistribution, there is little evidence of an overall adverse effect on growth. For smaller transfers, of

[25] The p value for the test that the two coefficients on redistribution in Table 2 [3] are equal is 0.095. As the specification in column 3 shows, the negative effect of redistribution in the linear case is driven by the high-redistribution cases. As a robustness check, we have examined different definitions of “high redistribution” from above the 50th to above the 90th percentile; the likelihood is maximized around the 75th percentile.

[26] Our chosen specification for the hazard function in (5) allows us to express the effect of a variable either in terms of its effect on the hazard that a spell will end in the next year or, with a transformation of the hazard function, as the expected duration of the spell.

Table 3 The effect of transfers and inequality on the duration of growth spells. *Source*: See notes to Table 2

	Linear	Nonlinear	Benchmark	Benchmark + controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Log(initial income)	1.021	1.046	1.024	1.026	1.077*	1.216***
	(0.0318)	(0.0356)	(0.0318)	(0.0318)	(0.0413)	(0.0844)
Net inequality	1.057**		1.060**	1.050*	1.060**	1.074**
	(0.0254)		(0.0266)	(0.0266)	(0.0291)	(0.0314)
Redistribution	1.097***					
	(0.0334)					
Net inequality at top 25%		1.097***				
		(0.0351)				
Net inequality at bottom 75%		1.125***				
		(0.0459)				
Redistribution at top 25%		1.108***	1.098***	1.099***	1.055	0.990
		(0.0315)	(0.0322)	(0.0329)	(0.0378)	(0.0567)
Redistribution at bottom 75%		0.982	0.987	0.961	0.971	0.938
		(0.0698)	(0.0690)	(0.0735)	(0.0695)	(0.0734)
Log(investment)				3.050**		
				(1.7293)		
Log(population growth)				1.201		
				(1.7085)		
Log(total education)					0.694	0.845
					(0.2705)	(0.4260)
Large negative global interest rate shock					2.719**	3.198**
					(1.1700)	(1.4887)
Large negative terms of trade shock					1.391	1.153
					(0.6620)	(0.5945)
Political institutions						0.924*
						(0.0398)
Openness						0.990
						(0.0066)
Debt liabilities						1.001
						(0.0027)
Observations	640	640	640	640	609	549
Number of total spells/number of complete spells	62/28	62/28	62/28	62/28	55/23	49/20
Log-likelihood	−65.772	−62.306	−64.138	−62.145	−52.989	−40.063

Table 3 continued

	Linear	Nonlinear	Benchmark	Benchmark + controls		
	(1)	(2)	(3)	(4)	(5)	(6)
p values for tests						
Redistribution top 25% = inequality	0.193	0.768	0.195	0.108	0.888	0.175
Redistribution bottom 75% = inequality		0.087	0.312	0.236	0.236	0.113

The table reports results using the baseline sample and estimation of a proportional hazard model with time-varying covariates, which relates the probability that a growth spell will end to a variety of economic and political variables. A hazard ratio of 0.9 means that a unit change in the regressor decreases the expected time of duration by 10%; a hazard ratio of 1 means there is no effect; and a ratio of 1.1 means it increases expected duration by 10%. We test the probability that the true hazard ratio equals 1, and statistical significance at the 10, 5 and 1% level is indicated by *, **, ***, respectively

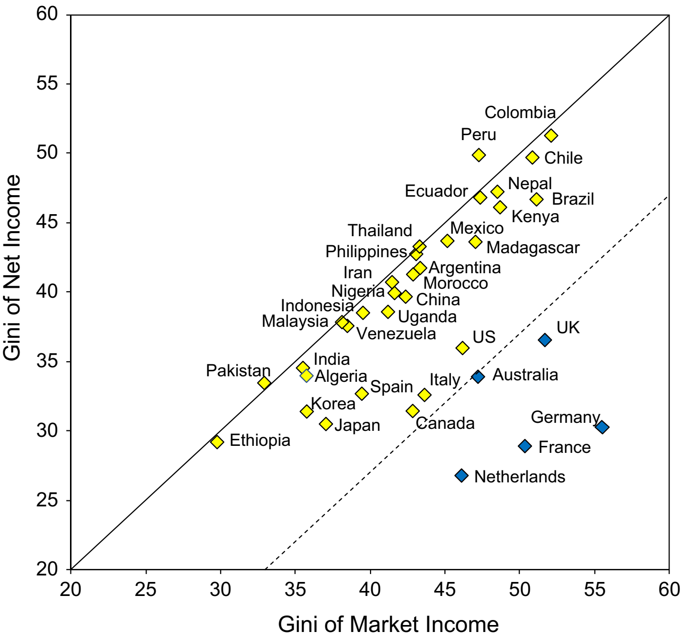

Fig. 5 Redistribution: the top 25% and the bottom 75%. *Note* The Gini coefficient for net income inequality is on the vertical axis, and the Gini for market income inequality is on the horizontal axis (both for the latest year data was available). For clarity, only the top 20% of countries by population are presented. The distance below the solid diagonal line represents the amount of redistribution. Countries below the dashed diagonal line are those in the top 25% of the distribution for redistribution. For a country exactly on that line, the difference between the market and net Gini values (the amount of redistribution) would be about 13 Gini points. *Source*: Penn World Tables version 7.1, SWIID 3.1, and authors' calculations

less than 13 Gini points, the evidence suggests that the overall effect of redistribution would be growth positive: roughly neutral direct effects of redistribution, and a protective effect of the resulting reduction in inequality. Columns [4]–[6] of Table 3 present similar results, but this time controlling for a number of potential determinants (e.g. physical investment, education, and institutions). Again, we find that the inclusion of additional controls preserves the results related to inequality and, to a lesser extent, redistribution.

While sample size constraints limit the amount of possible combinations we can run, we conclude that, as with the panel growth regressions above, the result on inequality's effect on growth duration is fairly robust: it seems to stick more or less irrespective of the covariates included in the model. The results with respect to redistribution are more fragile. In particular, the negative effect of very large transfers seems to disappear when certain other covariates—such as exogenous shocks, institutions, debt liabilities and openness (Table 3 [6])—are included as additional controls.

6 Assessing transmission channels in the panel growth regressions

Higher inequality seems to have a detrimental effect on growth. But why? Some of the control variables in Table 2 capture mechanisms whereby inequality has been thought to influence growth, notably through its possible effects on the accumulation of human and physical capital. Indeed, when proxies for these factors are included (Table 2 [4]), the coefficient on inequality itself falls by half. The implication is that inequality may be acting on growth in part through its effect on these variables.

We now take a more formal and systematic look at the channels through which inequality seems to be influencing growth. We focus on the growth regressions because there is too little data for clear inference about channels in the duration analysis. Following the discussion in Galor (2009) and Galor et al. (2009), we investigate the role of four channels: (1) and (2) physical and human capital (proxied by investment to GDP, population growth, total education years, and life expectancy) (Galor and Moav 2004); (3) socio-political instability (Alesina and Perotti 1996; Acemoglu and Robinson 2000) proxied by a measure of political participation; and (4) fertility (de la Croix and Doepke 2003) proxied by the fertility rate.

We follow a two-stage approach. We first look at the effect of inequality on a candidate mechanism variable (e.g. years of education), then at the effect of inequality on growth insofar as it is acting through this variable.[27] Thus, for each channel Z^k we estimate

$$Z_{i,t}^k = \lambda_0 + \lambda_1^k Y_{i,t-\tau} + \lambda_N^k N_{i,t} + \lambda_R^k R_{i,t} + \mu_i^k + \kappa_t^k + \omega_{i,t}^k \tag{8}$$

where estimated λ_1 describes the effect of income at the beginning of the period, and λ_2 and λ_3 describe the effect of net inequality and redistribution on the channel k, respectively and μ_i^k, κ_t^k, and $\omega_{i,t}^k$ are country-specific, period dummies, and overall error term, respectively. Second, to quantify both the direct and indirect effects of inequality and redistribution on growth, we substitute (8) into (2) in order to get the total effect of inequality (or redistribution) on growth, as follows:

$$\ln Y_{i,t} - \ln Y_{i,t-\tau} = \left(\tilde{\gamma}_0 + \gamma_1' \sum_k \lambda_1^k\right) \ln Y_{i,t-\tau} + \left(\alpha + \gamma_1' \sum_k \lambda_N^k\right) N_{i,t}$$

[27] Pellegrini and Gerlagh (2004) follow a similar approach in investigating the effect of corruption on growth.

$$+\left(\beta+\gamma_1'\sum_k\lambda_R^k\right)R_{i,t}+u_i+v_t+\varepsilon_{i,t} \tag{9}$$

We estimate (9) and the first difference of (9) as a system using sGMM and present the results of the channels' analysis in Table 4. The first panel of the Table (columns [1]–[6]) present specifications of Eq. (2) that include inequality and redistribution and also, sequentially, proxies for physical and human capital (education and life expectancy), fertility, and the political environment. As additional channels are included, the coefficient on inequality decreases and loses statistical significance while the direct effect of redistribution remains insignificant. This suggests that the effect of inequality (and redistribution) on growth is mainly transmitted through these additional variables, whose coefficients reflect the indirect effects of inequality (and redistribution) on growth.

To explore this possibility, the next panel of the Table (columns [7]–[12]) analyzes the first step of the indirect transmission channel, that is, the effect of inequality and redistribution on the channels.[28] We find support for the Galor–Zeira hypothesis that higher inequality is associated with lower human capital (total years of education, and lower life expectancy) and higher fertility. We also find that higher inequality is associated with worse political institutions and higher population growth. Thus, inequality affects almost all the hypothesized channels significantly and in a manner consistent with theory. In contrast, the effect of redistribution on each of the channels is generally insignificant.

Now that we have estimated the effect of inequality and redistribution on the transmission channels we can decompose the effects of inequality and redistribution on growth into the direct and indirect effects, making explicit the channels. The last column of Table 4 reports the coefficient estimates for Eq. (9). First, and not surprisingly given Table 2 [1], we find a strong and statistically significant (total) effect of inequality on growth; redistribution, in contrast, has a small and statistically insignificant (albeit positive) effect on growth. Second, we recover estimates of the direct effects of each channel variable γ^k. The direct effect of inequality α is estimated in column [6] and the effect of inequality on each channel variableλ_N^k comes from columns [7]–[12].[29] The results suggest a negative direct effect of inequality on growth with contributions from the channels to the total effect of inequality on growth.

It is beyond the scope of this paper to resolve questions surrounding the complexities of the fertility–growth interactions, as covered in a rapidly growing literature. This being said, we would interpret our results as being consistent with theoretical channels explored in recent contributions. First, de la Croix and Doepke (2003) argue, based on a well-articulated theoretical and empirical framework, that the nonlinear human capital/fertility relationship implies that an increase in inequality (for a given mean income) would raise fertility.[30] A large literature has linked in turn high fertility to low growth, both empirically and theo-

[28] Presumably because some of the estimated channels have little time-series variation, some of the AR1 tests in Table 4 are larger than desirable. Alternatively, since the channels regressions in (8) do not include a lagged dependent variable, they can be consistently estimated with instrumental variables (IV). When we do so, we find very similar results to those reported in Table 4 (results available on request).

[29] The contribution of the direct effect in the total effect of inequality on growth is $\alpha/(\alpha+\sum_k\gamma^k\lambda_N^k)$, while the contribution of the indirect effect (through all channels) in the total is $\sum_k\gamma^k\lambda_N^k/(\alpha+\sum_k\gamma^k\lambda_N^k)$. The contribution of each channel k in the indirect effect is $\gamma^k\lambda_N^k/\sum_k\gamma^k\lambda_N^k$.

[30] The microeconomic literature on the effect of income (and hence from a macroeconomic perspective inequality) on fertility can help address the question of timing. In particular, recent papers such as Black et al. (2013) and Cohen et al. (2013) find some suggestive evidence of a nearly contemporaneous (albeit weak) relationship between income and fertility.

Table 4 Transmission channels of inequality and redistribution. *Source*: See notes to Table 2

Variables	Effects on growth, adding channels						Effect of inequality and redistribution on the channels						Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7) Investment	(8) Population	(9) Education	(10) Life expectancy	(11) Fertility	(12) Polity	(13) Full specification
Log(initial income)	−0.0070**	−0.0068**	−0.0104**	−0.0130**	−0.0228***	−0.0237***	0.1130**	−0.0615***	0.2623***	0.0694***	−0.2555***	0.0182**	−0.0031
	(0.0035)	(0.0029)	(0.0049)	(0.0055)	(0.0058)	(0.0062)	(0.0495)	(0.0181)	(0.0455)	(0.0059)	(0.0350)	(0.0077)	(0.0034)
Net inequality	−0.0853**	−0.0672**	−0.0680**	−0.0776***	0.0228	0.0213	−0.0363	0.6802***	−2.5477***	−0.2050***	1.7899***	−0.2342***	−0.1135***
	(0.0361)	(0.0335)	(0.0302)	(0.0256)	(0.0279)	(0.0416)	(0.5787)	(0.2348)	(0.6882)	(0.0638)	(0.3389)	(0.0743)	(0.0425)
Redistribution	0.0357	0.0325	0.0447	0.0505	0.0420	0.0255	−0.6635	−0.1044	−1.5543***	−0.1027	0.1271	−0.1333	−0.0276
	(0.0406)	(0.0393)	(0.0541)	(0.0505)	(0.0505)	(0.0608)	(0.6580)	(0.3180)	(0.5039)	(0.0835)	(0.4831)	(0.1110)	(0.0627)
Log(investment)	0.0260*	0.0223**	0.0267***	0.0207**	0.0415***	0.0401***							
	(0.0147)	(0.0087)	(0.0093)	(0.0094)	(0.0110)	(0.0107)							
Log(population growth)		−0.0206	−0.0133	−0.0007	0.0169	0.0019							
		(0.0276)	(0.0212)	(0.0140)	(0.0131)	(0.0062)							
Log(total education years)			0.0122	0.0046	0.0179	0.0185*							
			(0.0096)	(0.0105)	(0.0117)	(0.0105)							
Life expectancy				0.0810	−0.0848	−0.0446							
				(0.0628)	(0.0670)	(0.0913)							
Log(fertility)					−0.0633***	−0.0593***							
					(0.0159)	(0.0175)							
Polity						−0.0405							
						(0.0656)							
Residual of Log(investment)													0.0401***
													(0.0107)

Table 4 continued

Variables	Effects on growth, adding channels						Effect of inequality and redistribution on the channels						Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7) Investment	(8) Population	(9) Education	(10) Life expectancy	(11) Fertility	(12) Polity	(13) Full specification
Residual of Log(population growth)													0.0019
													(0.0062)
Residual of Log(total education years)													0.0185*
													(0.0105)
Residual of life expectancy													− 0.0446
													(0.0913)
Residual of log(fertility)													− 0.0593***
													(0.0175)
Residual of polity													− 0.0405
													(0.0656)
Constant	0.0245	0.0628	0.0485	0.0285	− 0.1413**	− 0.1071**	2.2233***	2.1963***	0.2456	0.1239**	− 1.6924***	0.0064	0.0853**
	(0.0408)	(0.0650)	(0.0589)	(0.0510)	(0.0591)	(0.0487)	(0.4278)	(0.2343)	(0.5435)	(0.0571)	(0.3667)	(0.0779)	(0.0346)
Observations	656	656	656	656	656	656	656	656	656	656	656	656	656
Number of groups	88	88	88	88	88	88	88	88	88	88	88	88	88
Number of instruments	93	102	88	98	98	108	108	66	58	99	99	99	108
AR1 test (*p* values)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.7789	0.2878	0.0008	0.0000	0.0000	0.5046	0.0000
AR2 test (*p* values)	0.5533	0.3761	0.4158	0.5819	0.8151	0.5442	0.0108	0.5049	0.1504	0.6189	0.1977	0.2640	0.5442
Hansen test of joint instrument validity (*p* values)	0.4947	0.8599	0.4867	0.7757	0.7450	0.8113	0.8480	0.2911	0.2027	0.7724	0.7386	0.5887	0.8113

System GMM estimation. Robust standard errors in brackets where *, **, and *** indicate statistical significance at the 10, 5 and 1% levels, respectively retically, mainly through its effects on human capital accumulation.[31] Ashraf et al. (2013) find that higher fertility lowers output per capita, through a variety of mechanisms, some of which are in principle controlled for in some of our regressions (such as education and physical capital accumulation) and some which are not, such as parental time-input into childrearing.

Second, de la Croix and Doepke (2003), as well as Kremer and Chen (2002), emphasize the potential impact of inequality on *differential* fertility: because fertility is correlated with income, higher inequality drives higher *differences* in fertility across income groups. de la Croix and Doepke (2003) go on to argue, with simulations and empirical evidence, that differential fertility itself reduces growth. Unfortunately, there is no large set of panel data on differential fertility available for our growth regressions. However, the de la Croix and Doepke (2003) measure of differential fertility is correlated with the measure of (the log of) average fertility that we use in our regressions (with a correlation coefficient of 0.55). It is thus possible that we are picking up the effect of inequality on differential fertility and of differential fertility on growth, as argued in de la Croix and Doepke (2003).[32]

The precise breakdown of the effect of inequality into direct and indirect effects depends on the exact specification of the growth regression. When the list of potential channels includes only the traditional human capital variables and excludes fertility, the direct effect of inequality accounts for 56% of its total effect and the indirect effect accounts for the remaining 44%. Education and life expectancy contribute about 35% each to the indirect effect of inequality on growth, with population growth and the proxy for institutions accounting for the remaining 19 and 9%, respectively.

Redistribution has direct effects on several of the channels, with some evidence that it lowers education and raises fertility. This implies a negative indirect effect of redistribution on growth, working through these channels. However, in a reduced-form regression of growth on redistribution (Table 2 [1]), the effect of redistribution is negligible. This combination (a negligible total effect and a negative indirect effect) can only be reconciled if the direct effect of redistribution on growth is positive. However, none of these growth effects is statistically significant.

In sum, there is fairly strong evidence that inequality matters for a set of variables predicted by the literature: it seems to lower education and life expectancy and raise population growth and fertility, while it reduces the quality of institutions. Putting these results together with the influence of these variables on growth, we have found some preliminary evidence that inequality works through lowering education and life expectancy and through raising fertility. These results, particularly those on fertility, suggest avenues for further research.

[31] Galor and Zeira (1993) emphasize the role of both high fertility and inequality in lowering growth, through effects on human capital accumulation. Galor and Zang (1997) show that fertility per se is more important than labor force growth in driving growth. De Gregorio and Lee (2004) and Barro (2008) present evidence that inequality affects growth in part through its effect on total fertility. See also Li and Zhang (2007) and Acemoglu and Johnson (2007), who use various external instruments to identify independent negative effects of fertility and population growth on GDP per capita.

[32] Kremer and Chen (2002) argue that the effect of inequality on differential fertility is stronger among developing than advanced countries. Our results on channels, and in particular fertility, are similar when we exclude advanced economies.

7 Robustness

7.1 Sample and data sources

The baseline sample used for all these results thus far represents a compromise between using the full sample and using the strictest criterion to throw out potentially less informative data. We now examine the robustness of our results for growth using alternative samples.[33]

1. Our "restricted" sample follows Solt (2009) and imposes the requirement that a country has at least *three* direct observations on a net and three direct observations on a market inequality concept (in contrast with our baseline sample, where we require a minimum of one each), and excludes *all* developing country observations prior to 1985 and developed country observations prior to 1975. Solt (2009) argues that older observations, particularly for developing countries, are unusually suspect.
2. Our "very restricted" sample adds the additional restriction that *each 5-year period* contains at least one direct observation on a net and one on a market inequality concept.[34]
3. A final subsample is the 24 countries of the OECD.[35] These advanced economies are structurally different, so the question of whether our results hold is of independent interest. More importantly, the data are considered to be of higher quality and we can compare our SWIID-based results with those using the Luxemburg Income Study (LIS) data.[36]

As shown in Table 2.1 in the Online Appendix, we find that our core results are preserved in all these smaller samples. Inequality is generally significantly bad for growth (except in the smallest sample with the full set of controls, where almost nothing is significant), and redistribution is insignificant. For the OECD sample, the LIS data yield similar results to the SWIID data (see Table 2.2 in the Online Appendix).[37] The p value for the hypothesis that the coefficient on inequality is the same as that on redistribution is generally below standard critical values.[38] However, we do lose information in the smaller samples, as it becomes harder to get precise estimates of the coefficients of interest in the presence of a large number of controls.[39]

33 Inference about spells is generally not possible in samples more restricted than the baseline, because in these smaller samples we fail to observe the start of enough spells.

34 We have also investigated a spell-specific restricted sample that requires that there be one net and one market measurement per spell, instead of per 5-year period. This makes no important difference to the results. "Appendix A" provides the sample definitions and stylized facts.

35 We define OECD membership as being the members as of 1975.

36 The LIS has earned a reputation as the best data available for making cross-national comparisons of income inequality. It is based on highly-comparable, high-quality harmonized micro-data from national household income surveys, and allows the calculation of net and market income, and hence redistribution. However, LIS data are available essentially only for OECD countries.

37 Cingano (2014) confirms our results that inequality is bad and redistribution is insignificant for growth using an OECD sample and OECD data, which is similar to but distinct from the LIS data (the underlying micro-data are generally the same but the processing and various assumptions made may be different).

38 Our results for the OECD sample are in contrast with the results of Thewissen (2014), who looks at similar issues for a smaller set of OECD countries. However, he uses a fixed-effects methodology, which does not account for the cross-sectional variation. In addition, using fixed effects to estimate a dynamic panel model with small and fixed time dimension generates biased estimates of the coefficients.

39 We have also examined our results using a subsequently-available vintage of the Solt database (Solt 2016). Reassuringly, the main conclusions remain the same with this updated data (results available on request).

7.2 Explicit consideration of measurement error

One advantage of the SWIID data set is that it provides an explicit estimate of the measurement error associated with the imputation required to generate comparable data. We estimate the growth models explicitly accounting for this error, essentially estimating the corresponding regression from Table 2 100 times, each time randomly selecting values for the inequality and redistribution variables from a distribution that reflects their expected values and measurement error. The result is estimates of the coefficients and, most importantly, of the standard errors associated with those variables that take into account this measurement error.[40] As Table 5 shows, our results are preserved.

7.3 The use of instruments

With respect to validity, all results presented earlier pass the standard Hansen test of overidentifying restrictions and the test for no second-order serial correlation of the error term. As recommended by Roodman (2009), we check—and pass—difference-in-Hansen tests of the validity of subsets of instruments.[41] In Table 6 we test for sensitivity to various alternative instrumentation strategies, in particular the reduction in the number of instruments in the panel growth estimations.[42] We present such estimates for specifications with physical capital (Table 6 [1]–[4]) and human capital (Table 6 [5]–[8]), with columns [1] and [5] repeating Table 2 [3] and [4]), for comparison. We allow the instrument sets to change, first by increasing the number of lags, then collapsing the lags in two different ways. Irrespective of the number of instruments used, our results still remain: higher inequality is associated with lower growth while redistribution is generally insignificant.[43]

Next, we turn to the issue of instrument strength. As discussed earlier, if the instruments in our sGMM regressions are weak, then inference may be unreliable even in large samples. In what follows, we apply several approaches to address these concerns and examine the robustness of our results.

One approach to address the potential weak instrument problem is provided by Newey and Windmeijer (2009). They propose a variance correction for the Continuous Updating Estimator (CUE) which is suitable for "many weak instruments" settings and thus results in more reliable inference even in the presence of potentially weak instruments. Table 7 presents the results when we estimate sGMM using CUE with the NW variance correction.[44] Using the NW CUE estimator preserves our results: higher inequality is associated with lower growth while redistribution is generally insignificant.

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40 Specifically, we use the Stata multiple imputation or "mi" command, as suggested in Solt (2009).

41 In particular, we examine difference-in-Hansen tests for the validity of instrument subsets, particularly those for the full set of instruments for the levels equation and those based on initial income. This provides some comfort that the stationarity restrictions on the initial conditions process required for the validity of sGMM are valid.

42 The number of instruments grows quadratically with T. Bun and Windmeijer (2010) and Windmeijer (2005) discuss that the finite sample properties of GMM estimators are sensitive to the number of moment conditions used. To address the instrument proliferation problem, Roodman (2009) proposes collapsing the instrument matrix and limiting the number of lags used.

43 The Online Appendix Table 3.1 presents similar variations to the number of instruments for the remaining specifications in Table 2.

44 In order to apply the NW CUE to our dynamic panel data setting, we use wxyz_xtabond2 to generate transformed variables, ivreg2 to obtain CUE estimates, and Farbmacher (2012) to obtain appropriate SEs (using the Stata program nwind). Exogenous regressors are "partialled out" before the CUE estimates are calculated.

Table 5 Controlling for the measurement error. *Source*: See notes to Table 2

	Benchmark	Benchmark + controls		
	(1)	(2)	(3)	(4)
Log(initial income)	− 0.0030	− 0.0061*	− 0.0077	− 0.0101*
	(0.0045)	(0.0033)	(0.0050)	(0.0055)
Net inequality	− 0.1132**	− 0.0678**	− 0.0739**	− 0.0975**
	(0.0488)	(0.0342)	(0.0360)	(0.0466)
Redistribution	0.0055	− 0.0018	0.0045	− 0.0009
	(0.0438)	(0.0373)	(0.0424)	(0.0442)
Log(investments)		0.0200**	0.0270***	0.0068
		(0.0085)	(0.0095)	(0.0137)
Log(population growth + 5)		− 0.0333	− 0.0114	− 0.0067
		(0.0237)	(0.0148)	(0.0168)
Log(total education)			0.0070	0.0127
			(0.0101)	(0.0118)
Large negative TOT shock				− 0.0412*
				(0.0220)
Political institutions				− 0.0006
				(0.0009)
Openness				0.0091
				(0.0116)
Debt liabilities				− 0.0167***
				(0.0059)
Constant	0.0835*	0.0941*	0.0367	0.1390**
	(0.0475)	(0.0511)	(0.0441)	(0.0593)
Observations	828	828	751	558
Number of groups	130	130	110	79
Number of instruments	84	129	107	100
AR1 test (*p* values)	0.0000	0.0000	0.0000	0.0000
AR2 test (*p* values)	0.1829	0.0873	0.2090	0.5368
Hansen test of joint instrument validity (*p* values)	0.1188	0.3995	0.2342	0.8953
p value for test Redistribution = inequality	0.0475	0.1232	0.0872	0.0628

As suggested by Solt in SWIID 4.0 dataset documentation we explicitly control the measurement error in the 100 imputed series of net inequality and redistribution. While fixing data points on inequality and redistribution for country-year observations where actual LIS survey exist we treated the other country-year data points as imputed. We then run sGMM regressions on each of the imputed series using "mi" build-in command in STATA

Another approach is taken by Bazzi and Clemens (2013) who try to construct weak instrument robust confidence intervals after the estimation. Lacking an sGMM-specific methodology, BC apply weak instrument-robust confidence intervals separately to each of the two equations (the levels and difference equations) that make up the system in sGMM. This practice seems to be inconsistent with the "raison d'être" of sGMM by removing the efficiency gains realized from stacking the levels and the difference equations together in a system. In the Online Appendix 4 (Tables 4.1, 4.2, and Figure 4.1), we present results from

Table 6 Alternative GMM instrument sets: the effect of inequality and redistribution on growth. *Source*: See notes to Table 2

		Table 2, column 3				Table 2, column 4			
		(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Log(initial income)	$Y_{(i,t-1)}$	−0.0081**	−0.0086**	−0.0130***	−0.0072*	−0.0140***	−0.0133***	−0.0148***	−0.0117**
		(0.0035)	(0.0039)	(0.0031)	(0.0040)	(0.0037)	(0.0047)	(0.0041)	(0.0050)
Net inequality	$G_{(i,t)}$	−0.0914***	−0.1047***	−0.1094***	−0.1016**	−0.0739***	−0.0756**	−0.0900***	−0.0695**
		(0.0336)	(0.0383)	(0.0387)	(0.0400)	(0.0266)	(0.0308)	(0.0337)	(0.0326)
Redistribution	$R_{(i,t)}$	0.0258	−0.0087	0.0485	−0.0015	0.0109	0.0082	0.0556	0.0409
		(0.0516)	(0.0530)	(0.0518)	(0.0387)	(0.0428)	(0.0504)	(0.0605)	(0.0456)
Log(investments)	$I_{(i,t)}$	0.0241***	0.0269***	0.0256***	0.0176**	0.0250***	0.0210**	0.0268***	0.0296***
		(0.0078)	(0.0081)	(0.0099)	(0.0082)	(0.0084)	(0.0091)	(0.0104)	(0.0092)
Log(population growth + 5)	$\Delta N_{(i,t)}$	−0.0159	−0.0233	−0.0311	−0.0292	−0.0215	−0.0207	−0.0238	−0.0110
		(0.0182)	(0.0214)	(0.0250)	(0.0225)	(0.0174)	(0.0191)	(0.0191)	(0.0135)
Log(total education)	$Edu_{(i,t)}$					0.0206***	0.0206**	0.0127	0.0153*
						(0.0073)	(0.0104)	(0.0078)	(0.0090)

Table 6 continued

		Table 2, column 3				Table 2, column 4			
		(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Constant	*Constant*	0.0718	0.0917*	0.1411***	0.1150**	0.0965**	0.1045***	0.1126**	0.0442
		(0.0457)	(0.0556)	(0.0482)	(0.0543)	(0.0389)	(0.0382)	(0.0467)	(0.0411)
Observations		828	828	828	828	751	751	751	751
Number of groups		130	130	130	130	110	110	110	110
Number of instruments		133	114	117	121	139	137	103	107
AR1 test (p values)		0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
AR2 test (p values)		0.1204	0.1118	0.0956	0.0936	0.1818	0.1821	0.1790	0.2159
Hansen test of joint instrument validity (p values)		0.4379	0.2157	0.2010	0.3686	0.9229	0.9071	0.2425	0.3955
p value for test redistribution = inequality		0.0158	0.0701	0.0042	0.0545	0.0411	0.0474	0.0215	0.0270

Table 6 continued

	Table 2, column 3				Table 2, column 4			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Instrument settings								
Instruments in difference equation	$Y_{i,t-2}$	$Y_{i,t-2}, Y_{i,t-3}, Y_{i,t-4}$	$Y_{i,t-2}, Y_{i,t-3}, Y_{i,t-4}, Y_{i,t-5}$	$Y_{i,t-2}, Y_{i,t-3}, Y_{i,t-4}, Y_{i,t-5}$	$Y_{i,t-2}$	$Y_{i,t-2}$	$Y_{i,t-2}$	$Y_{i,t-2}$
	$G_{i,t-1}, G_{i,t-2}$	$G_{i,t-1}$	$G_{i,t-1}$	$G_{i,t-1}, \ldots$ *collapse*	$G_{i,t-1}$	$G_{i,t-1}$	$G_{i,t-1}$	$G_{i,t-1}, \ldots$ *collapse*
	$R_{i,t-1}, R_{i,t-2}$	$R_{i,t-1}$	$R_{i,t-1}$	$R_{i,t-1}, \ldots$ *collapse*	$R_{i,t-1}$	$R_{i,t-1}$	$R_{i,t-1}$	$R_{i,t-1}, \ldots$ *collapse*
	$I_{i,t-1}, I_{i,t-2}$	$I_{i,t-1}$	$I_{i,t-1}$	$I_{i,t-1}, \ldots$ *collapse*	$I_{i,t-1}$	$I_{i,t-2}$	$I_{i,t-1}$	$I_{i,t-1}, \ldots$ *collapse*
	$\Delta N_{i,t-1}, \Delta N_{i,t-2}$	$\Delta N_{i,t-1}$	$\Delta N_{i,t-1}$	$\Delta N_{i,t-1}, \ldots$ *collapse*	$\Delta N_{i,t-1}$	$\Delta N_{i,t-1}$	$\Delta N_{i,t-1}$	$\Delta N_{i,t-1}, \ldots$ *collapse*
					$Edu_{i,t-1}$	$Edu_{i,t-2}$	$Edu_{i,t-1}$	$Edu_{i,t-1}, \ldots$ *collapse*
Instruments in level equation	$\Delta G_{i,t-1}$	$\Delta G_{i,t-1}$	$\Delta G_{i,t-2}$	$\Delta G_{i,t-1}$	$\Delta G_{i,t-1}, \Delta G_{i,t-2}$	$\Delta G_{i,t-1}, \Delta G_{i,t-2}$	$\Delta G_{i,t-2}$	$\Delta G_{i,t-1}$
	$\Delta R_{i,t-1}$	$\Delta R_{i,t-1}$	$\Delta R_{i,t-2}$	$\Delta R_{i,t-1}$	$\Delta R_{i,t-1}, \Delta R_{i,t-2}$	$\Delta R_{i,t-1}, \Delta R_{i,t-2}$	$\Delta R_{i,t-2}$	$\Delta R_{i,t-1}$
	$\Delta I_{i,t-1}$	$\Delta I_{i,t-1}$	$\Delta I_{i,t-2}$	$\Delta I_{i,t-1}$	$\Delta I_{i,t-1}, \Delta I_{i,t-2}$	$\Delta I_{i,t-1}, \Delta I_{i,t-2}$	$\Delta I_{i,t-2}$	$\Delta I_{i,t-1}$
	$\Delta_2 N_{i,t-1}$	$\Delta_2 N_{i,t-1}$	$\Delta_2 N_{i,t-2}$	$\Delta_2 N_{i,t-1}$	$\Delta_2 N_{i,t-1}, \Delta_2 N_{i,t-2}$	$\Delta_2 N_{i,t-1}, \Delta_2 N_{i,t-2}$	$\Delta_2 N_{i,t-2}$	$\Delta_2 N_{i,t-1}$

Table 7 Cue estimation with Newey and Windmeijer (2009) variance correction. *Sources* See notes to Table 2

	Baseline		Specification 1		Specification 2		Specification 3	
	(1) System GMM	(2) GMM level equation	(3) System GMM	(4) GMM level equation	(5) System GMM	(6) GMM level equation	(7) System GMM	(8) GMM level equation
Log(initial income)	0.0013	0.0825	0.0027	0.0825	−0.0015	−0.0255	−0.0029	−0.0255
	(0.0025)	(0.0914)	(0.0036)	(0.0914)	(0.0066)	(0.0210)	(0.0095)	(0.0210)
Net inequality	−0.0882***	−1.4014	−0.1361***	−1.4014	−0.1672***	−0.6646**	−0.1474*	−0.6646**
	(0.0233)	(1.0417)	(0.0352)	(1.0417)	(0.0442)	(0.2870)	(0.0786)	(0.2870)
Redistribution	0.0995	−2.0466	−0.0378	−2.0466	−0.0622	−0.0943	0.0987	−0.0943
	(0.1022)	(1.5512)	(0.3003)	(1.5512)	(0.1022)	(0.1388)	(0.1553)	(0.1388)
Observations	1506	828	1506	828	1506	828	1506	828
Number of groups	130	130	130	130	130	130	130	130
Number of instruments	106	34	82	34	81	16	63	16
Under-identification tests								
Kleibergen–Paap rk LM test	110.48	39.58	92.77	39.58	92.71	17.44	81.14	17.44
p value	0.3133	0.1677	0.1556	0.1677	0.1388	0.2336	0.0434	0.2336
Kleibergen–Paap rk Wald test	4667.00	264.13	2013.08	264.13	472.17	34.92	248.34	34.92
p value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0015	0.0000	0.0015
Weak identification and Hansen tests								
Kleibergen–Paap weak identification test	40.6415	7.4012	23.0498	7.4012	5.4770	2.1264	3.7505	2.1264
Hansen J stat	102.98	34.39	87.86	34.39	89.79	10.88	76.28	10.88
p value	0.4822	0.3087	0.2318	0.3087	0.1703	0.6206	0.0765	0.6206

The table reports the results for our baseline and additional specifications when we estimate sGMM using CUE with the Newey and Windmeijer (2009) variance correction. To estimate we use the STATA program wxyz_xtabond2 to generate transformed variables, ivreg2 to obtain the CUE estimates, and the Stata program nwind based on Farbmacher (2012) to obtain appropriate SEs. Exogenous regressors are “partialled out” before the CUE estimates are calculated various attempts to address weak instrument issues. We first follow BC and report robust intervals for the levels equation.[45] In addition, we then exploit new procedures to report weak instrument-robust intervals in the sGMM context, but the properties of these intervals (e.g. finite-sample properties and power) are not well-established in the literature.

In sum, full consideration of potentially weak instruments makes inference challenging in the sGMM context. The literature is evolving, but tools designed for sGMM and whose properties are well understood do not yet exist. It seems clear from the literature that the power of available methods to reject false null hypotheses is likely to be very weak, particularly when there are multiple endogenous variables. Nonetheless, we continue to find, using a variety of approaches, evidence that inequality is harmful for growth and redistribution insignificant, even with consideration of the implications of potentially weak instruments.[46]

7.4 Various additional robustness tests

In this final section, we summarize results from several additional robustness tests such as using alternative measures for redistribution and using longer horizons for our growth regressions and spells analysis. We have measured redistribution as the difference, in Gini points, between market and net inequality. Alternatively, it may be argued that the appropriate measure is the percentage change in the Gini, that is, (Market Gini − Net Gini)/Market Gini. This definition can be justified as being proportional to the deadweight loss from taxation if all redistribution takes the form of proportional income taxation. We find (results available on request) that, while there are some small differences, our main results are robust to this alternative definition.

The recently-compiled standardized database used in the paper distinguishes between market and net income inequality, and allows the direct calculation of *effective* redistribution as the difference between Gini market and Gini of net income. In addition to allowing more systematic coverage, this measure captures effective redistribution and what shows up in the data, rather than the *effort* for redistribution. In Okun's "leaky bucket" analogy we measure what "comes out" of the bucket, while proxies for the effort of redistribution like tax rates, government spending, or transfers measure what "goes in" the bucket.

As we show in Table 1, our measure of effective redistribution is highly correlated with the direct proxies; the correlation is not 1, however, suggesting that there is still an unexplained part of the direct measures that potentially our measure can capture. Nevertheless, we re-estimate our baseline specification, replacing effective redistribution with tax revenues as a ratio to GDP, and subsidies and other transfers as a ratio to total expenditure. Despite the reduction in the sample size due to the incomplete coverage of these proxies, our results are

[45] Stock and Wright (2000) and Bun and Windmeijer (2010) discuss why weak-instrument diagnostics for linear instrumental variables regression do not carry over to the more general setting of GMM, while Stock et al. (2002) point out that weak identification is a more difficult issue in GMM than in IV regression.

[46] We agree with Kraay (2015) that a full consideration of weak instrumentation issues, such as we have undertaken here, makes inference challenging (if the instruments are indeed weak). However, in our view, and as discussed earlier in this section, the literature on weak instruments in sGMM is rapidly developing and the properties of these intervals, notably implications for power, are not well-established. It would be interesting, however, to apply the weak instrument-robust methods used here to variables other than inequality. Our experience is that inequality seems generally speaking at least as robust as other variables typically considered to belong in the "pantheon" of growth determinants, as discussed for example in Berg et al. (2012).

preserved; inequality has a negative and significant effect while the proxy for redistribution is insignificant.[47]

Finally, we investigate the effect of inequality and redistribution on the average growth rate and spell duration for longer horizons (results available on request). As suggested by theories, inequality may have longer-run effects through human capital development, fertility rates, and improvements in the political environment. For the first part of our analysis, we redefine growth and construct 10-year averages for the explanatory variables; for the second part, we calculate spells that last longer, by imposing a minimum length of 8 years. Lower inequality is still associated with higher per capita growth while redistribution is insignificant. For spells, due to the reduced number of identified spells, lower inequality is still associated with longer spell duration, but it is generally insignificant.

8 Conclusion

We have taken advantage of a new comprehensive data set to look at the relationships among inequality, redistribution, and growth; earlier work on the inequality–growth relationship has generally confounded the effects of redistribution and inequality. Our focus has been on the medium and long term: growth over 5-year periods and the duration of growth spells. Several important conclusions emerge.

First, inequality is a robust and powerful determinant both of the pace of medium-term growth and of the duration of growth spells, even controlling for the size of redistributive transfers: more equal societies grow faster and more sustainably than less equal ones. And second, there is surprisingly little evidence for the "leaky bucket" hypothesis, that is, for the growth-destroying effects of fiscal redistribution at a macroeconomic level; fiscal redistribution, unless it is extreme, may be a win-win policy via its equality-inducing effects. We do find some mixed evidence that very large-scale redistribution may have direct negative effects on growth duration. But for non-extreme redistributions, there is no evidence of any adverse direct effect. Third, we find evidence for many of the other mechanisms posited in the literature. Inequality is associated with lower investment in human and physical capital, higher fertility, and weaker political institutions. Considering in addition our estimates of the impacts of these variables on growth, inequality seems to lower growth mainly through its effects on education, life expectancy, and especially fertility.

For our analysis we have used the best—really the only—available comparable data on both variables that cover a large number of countries and time periods. We have subjected our results to a variety of alternative samples and even, for the much smaller OECD sample, to alternative data sources of the highest possible comparability and quality. And we have looked at alternative estimation methods, explicit consideration of the effects of measurement error in these variables and, for the panel growth regressions, weak-instrument robust confidence intervals. We have looked at two entirely different empirical approaches, in addition to simple graphs. Our main results—the significant negative effect of inequality and the general insignificance of redistribution—are remarkably robust.[48]

47 Following the suggestion of the referee, as a robustness test we re-estimate our baseline specification where we include direct measures of redistribution such as taxes, government spending, transfers, as instruments for our measure of redistribution (results available on request). This instrumentation strategy may help address potential measurement error of the redistribution variable because the measurement errors in the two measures of redistribution are not likely to be correlated. Our results are preserved.

48 In Berg and Ostry (2011) we also found a role for inequality in a narrative (historical) analysis of the ends of growth spells, in the context of growth spell duration analysis.

We need to be cautious about over-interpreting these results, especially for policy purposes. It is hard to go from these sorts of conclusions to firm statements about causality. We nonetheless see an important affirmation from our empirical analysis. On average across countries and over time, there is no evidence that the things that governments have typically done to redistribute have led to bad growth outcomes, unless they were extreme. And insofar as these redistributions have resulted in lower inequality, this has helped support faster and more durable growth.

Appendix A: Various samples of redistribution

Name	Description	Observations	Mean	SD	Min	Max
Baseline	All available data in Solt (2009) as long as either (1) the country has at least one observation based on a net and one based on a market inequality measure; or (2) the ratio of the difference between market and net inequality to the associated standard error is greater than 1.96, but excluding transition countries and a set of specific observations handpicked for deletion by Solt (2009)	3667	7.5	7.2	– 14.2	30.3
Full	Full sample of all available data in Solt (2009)	4396	6.9	6.9	– 14.2	30.4
Restricted	All available data in Solt (2009) as long as the country has at least three observations based on net and three based on market inequality measures, but excluding transition countries, all developing countries prior to 1985, all developed countries prior to 1975, and a set of specific observations handpicked for deletion by Solt (2009)	2158	7.7	7.3	– 11.9	30.3
Very restricted	All available data in Solt (2009) where country-year observation from the actual survey was available	997	14.3	5.7	2.5	30.3
OECD (1975)	Subsample of the baseline for OECD countries who became members prior to 1975; total of 24 countries	806	9.1	7.3	– 11.9	30.3

Appendix B: Data and summary statistics

Description	Source	Unit of measurement	Obs	Mean	SD	Min	Max
Gini of market income	SWIID 3.1	Index of market income inequality (0–100)	828	46.0	8.9	24.3	73.4
Gini of net income	SWIID 3.1	Index of net income inequality (0–100)	828	38.3	10.2	19.8	66.1
Redistribution (full)	SWIID 3.1	Gini market—Gini net for full sample	828	7.6	7.1	− 10.6	27.9
Redistribution (baseline)	SWIID 3.1	Gini market—Gini net for baseline sample	828	7.7	7.1	− 10.6	27.9
Redistribution (restricted)	SWIID 3.1	Gini market—Gini net for Solt restricted sample	462	7.4	7.3	− 10.6	27.9
Redistribution (very restricted)	SWIID 3.1	Gini market—Gini net for very restricted sample	334	8.0	7.2	− 10.6	27.9
Log(initial income)	PWT 7.1	Real GDP per capita, PPP adjusted, chain series, in 2005 prices	828	8.6	1.3	5.7	11.2
Log(investment)	PWT 7.1	Real investment to GDP, in 2005 US dollars at PPP	828	3.1	0.4	1.3	4.3
Log(population growth + 5)	WEO	Population growth	828	1.9	0.2	− 0.5	2.7
Log(education)	Barro and Lee (2013)	Average years of primary and secondary schooling in the total population over 25	751	1.8	0.6	− 0.8	2.6
Terms of trade growth (dummy)	WEO	1 if in the bottom three deciles, and 0 otherwise	683	0.3	0.2	0.0	1.0
Polity 2	Polity IV	Scale from − 10 (autocratic) to 10 (democratic)	705	3.6	6.8	− 10.0	10.0

Description	Source	Unit of measurement	Obs	Mean	SD	Min	Max
Openness	PWT 7.1	Openness at current prices (%)	828	0.7	0.5	0.0	4.2
External debt liabilities	Lane and Milesi-Ferretti (2011)	External debt liabilities from WEO and Global Development Finance database	730	0.9	2.3	0.0	32.9
Summary statistics for the survival sample for h = 5, p = 10							
Gini of net income	SWIID 3.1	Index of net income inequality (1–100)	640	40.4	9.2	21.4	65.5
Redistribution (full)	SWIID 3.1	Gini market—Gini net for full sample	640	4.8	6.0	−6.6	26.8
Redistribution (baseline)	SWIID 3.1	Gini market—Gini net for baseline sample	640	4.8	6.0	−6.6	26.8
Redistribution (restricted)	SWIID 3.1	Gini market—Gini net for Solt restricted sample	364	4.5	5.7	−5.9	25.6
Log(Investment)	PWT 7.1	Real investment to GDP, in 2005 US dollars at PPP	640	3.2	0.4	1.5	4.3
Log(population growth + 5)	WEO	Population growth	640	1.9	0.1	1.6	2.5
Log(education)	Barro and Lee (2013)	Average years of primary and secondary schooling in the total population over 25	633	1.7	0.6	−0.4	2.5

Description	Source	Unit of measurement	Obs	Mean	SD	Min	Max
US interest rate (dummy)	FED 3 month treasury bill	1 if upper three deciles, and 0 otherwise	640	0.4	0.5	0.0	1.0
Terms of trade growth (dummy)	WEO	1 if in the bottom three deciles, and 0 otherwise	616	0.3	0.4	0.0	1.0
Polity 2	Polity IV	Scale from − 10 (autocratic) to 10 (democratic)	614	2.1	6.7	−9.0	10.0
Openness	PWT 7.1	Openness at current prices (%)	640	86.6	74.7	8.5	433.0
External debt liabilities	Lane and Milesi-Ferretti (2011)	External debt liabilities from WEO and Global Development Finance database	597	163.2	490.8	3.0	3468.3

Appendix C: Correlation between market inequality and redistribution

As discussed in Sect. 3 countries with more market inequality tend to redistribute more, with a stronger effect in the OECD sample. This is so in a model with country-specific fixed effects (which focuses on the variation across time within countries) while controlling for unobserved heterogeneity, as well as with IV-GMM estimation, where market inequality is instrumented with its lagged differences.[49] The effect is modest but nontrivial: an increase in market inequality from the 50th to the 75th percentile of the sample is associated with an increase in redistribution by 3–4 Gini units (Table 8).

Appendix D: Reconciling with the literature: alternative panel estimation methods to investigate the growth-inequality-redistribution relationship

In an attempt to reconcile disparate results in the literature, this Appendix, presents results from additional panel estimators that have been used in the literature to investigate the growth–inequality–redistribution relationship. By using the same dataset, we are able to ensure and apples-to-apples comparison and isolate differences between our results and those in the literature using alternative estimation methods.

One of the advantages of panel data estimators is that it allows modeling (or controlling for) the time-invariant, unobserved individual effect ui in Eq. (3). The standard estimator pooled OLS (POLS) ignores the ui which is correlated with the lagged dependent variable in the dynamic panel specification, as it is part of the process that generates the lagged dependent variable yi, $t-1$, and hence, it is biased (upwards). The fixed effects (FE) estimator (obtained by OLS estimation on the time-demeaned variables) allows the individual effect ui to be correlated with the regressors. In a finite dynamic panel model with fixed T, FE will be (downward) biased of order 1/T because the transformed time varying component of the error term vt and the transformed lagged dependent variable are correlated (Nickell 1981). (FE is also called the "within" estimator, because it uses the time variation within each cross-section.) The between estimator (BE) is obtained by OLS estimation on the cross-sectional equation on time averages of the variables, and effectively ignores variables' changes over time. As in the case of the POLS, the between estimator is biased (upwards) since the ui is correlated with the lagged dependent variable. Finally, the random effects (RE) estimator assumes that the individual effect is uncorrelated with the regressors, which is violated in the case of the dynamic panel setting.

To illustrate the effect of these estimators, Table 9 uses a specification with inequality, redistribution and standard growth determinants as controls and applies econometric techniques used in the literature. Columns [1]–[6] present results from POLS [1]; FE-within (FEw, [2]) which is based on time variation; the between (BW, [3]) which looks at the between-country effects and uses the cross section variation in averages to identify the parameters; RE [4] which is effectively a weighted average for the within and between estimators;[50] over random effects the difference GMM estimator (dGMM, [5]) which explores time series

[49] To test the relationship more formally, we supplement the analysis with two sets of Granger causality tests. Estimated panel VARs fail to reject the hypothesis that redistribution does not Granger cause market inequality, while we reject the hypothesis that market inequality does not Granger cause redistribution, at conventional levels of significance. Also, unit root and Granger causality tests for each country at a time suggest that, on aggregate, in the overwhelming majority of countries there is no Granger causality from redistribution to market inequality, while in these countries market inequality also Granger causes redistribution.

[50] The Hausman test confirms empirically the use of fixed effects.

Table 8 Correlation between market inequality and redistribution. *Source*: Income, investment/GDP, population growth and openness (Penn World Tables 7.1); redistribution and Gini

Variables	Full sample						OECD					
	(1) FE	(2) IV-GMM	(3) FE	(4) IV-GMM	(5) FE	(6) IV-GMM	(7) FE	(8) IV-GMM	(9) FE	(10) IV-GMM	(11) FE	(12) IV-GMM
Log(initial income)	1.2566	4.3199***	1.2843	4.2982***	1.1621	2.9828***	0.7543	5.2816**	0.9431	5.4138**	1.4357	8.0343***
	(0.8607)	(0.5291)	(0.8755)	(0.5118)	(0.8161)	(0.6737)	(2.6213)	(2.3464)	(2.5971)	(2.2823)	(2.8511)	(1.6258)
Market inequality	0.4867***	0.6399***			0.4870***	0.6748***	0.6235***	0.8233***			0.6440***	0.8418***
	(0.0525)	(0.1542)			(0.0520)	(0.1470)	(0.0823)	(0.1379)			(0.0814)	(0.1324)
Market inequality at top 25%			0.4906***	0.6415***					0.6300***	0.8721***		
			(0.0543)	(0.1628)					(0.0815)	(0.1220)		
Market inequality at bottom 75%			0.4946***	0.6464***					0.6517***	0.9048***		
			(0.0588)	(0.1737)					(0.0902)	(0.1226)		
Log(investment)					0.6062	0.9363					−4.4355**	−2.1698
					(0.8391)	(1.6084)					(2.1028)	(2.9746)
Log(population growth)					0.0823	−15.3110***					−1.8318	−22.9745***
					(0.8769)	(5.0927)					(3.8710)	(5.4090)
Constant	−23.7246***	−60.7850***	−24.2302***	−60.8031***	−24.9843***	−26.1321**	−21.0068	−76.8192***	−23.8384	−81.4628***	−10.9501	−60.0848**
	(7.1208)	(9.4209)	(7.4296)	(10.1274)	(7.2661)	(12.9723)	(24.6063)	(27.9029)	(24.3775)	(26.7712)	(25.8245)	(26.6726)

Table 8 continued

Variables	Full sample						OECD					
	(1) FE	(2) IV-GMM	(3) FE	(4) IV-GMM	(5) FE	(6) IV-GMM	(7) FE	(8) IV-GMM	(9) FE	(10) IV-GMM	(11) FE	(12) IV-GMM
Observations	828	553	828	553	828	553	220	170	220	170	220	170
R-squared	0.8795	0.1237	0.8796	0.1311	0.8798	0.1815	0.9085	0.5673	0.9100	0.5681	0.9156	0.6732
Underidentification LM (p values)		0.0028		0.0050		0.0018		0.0104		0.0307		0.0102
Weak id stat		7.7140		5.2580		9.7290		13.5300		12.8000		11.6500
Hansen (p values)		0.1130		0.185		0.3670		0.1340		0.2220		0.5930
Endogeneity test (p values)		0.0051		0.0099		0.0041		0.8240		0.3120		0.8900

Variables	Non-OECD					
	(13) FE	(14) IV-GMM	(15) FE	(16) IV-GMM	(17) FE	(18) IV-GMM
Log(initial income)	1.3723	2.0632***	1.3523	2.0282***	1.1663	0.8479
	(0.9318)	(0.5528)	(0.9575)	(0.5297)	(0.9009)	(0.7355)
Market inequality	0.4100***	0.5623***			0.4126***	0.6303***
	(0.0570)	(0.1434)			(0.0559)	(0.1509)
Market inequality at top 25%			0.4065***	0.4922***		
			(0.0584)	(0.1503)		

Table 8 continued

Variables	Non-OECD					
	(13) FE	(14) IV-GMM	(15) FE	(16) IV-GMM	(17) FE	(18) IV-GMM
Market inequality at bottom 75%			0.4033***	0.4785***		
			(0.0632)	(0.1643)		
Log(investment)					1.0675	2.0625
					(0.8861)	(1.5033)
Log(population growth)					− 0.4651	− 12.1395**
					(0.7744)	(4.7503)
Constant	− 20.4836***	− 38.3101***	− 20.0868**	− 34.5573***	− 21.3660***	− 15.6712
	(7.3775)	(8.0048)	(7.7419)	(8.9786)	(7.7257)	(11.7118)
Observations	608	407	608	407	608	407
R-squared	0.8207	− 0.2437	0.8207	− 0.1360	0.8221	− 0.2322
Underidentification LM (*p* values)		0.0067		0.0082		0.0056
Weak id stat		7.0470		4.8330		9.6180
Hansen (*p* values)		0.0322		0.0581		0.2190
Endogeneity test (*p* values)		0.0092		0.0254		0.0045

The table reports results from the fixed effects and IV-GMM regressions over three samples of country groups. Robust standard errors in brackets where *, **, and *** statistical significance at the 10, 5 and 1% levels, respectively

Table 9 Alternative panel estimation methods: the effect of inequality and redistributive transfers on growth. *Source*: See notes to Table 2

	(1) POLS	(2) FEw	(3) BE	(4) RE	(5) dGMM	(6) sGMM
Log(initial income)	− 0.0041***	− 0.0394***	− 0.0051***	− 0.0054***	− 0.0673***	− 0.0081**
	(0.0030)	(0.0000)	(0.0027)	(0.0003)	(0.0011)	(0.0199)
Net inequality	− 0.0394***	0.0145	− 0.0201	− 0.0436***	0.1147**	− 0.0914***
	(0.0092)	(0.5576)	(0.3305)	(0.0045)	(0.0485)	(0.0066)
Redistribution	− 0.0226	0.0415*	− 0.0155	− 0.0076	0.0209	0.0258
	(0.3075)	(0.0797)	(0.6071)	(0.7138)	(0.7816)	(0.6173)
Log(investment)	0.0236***	0.0236***	0.0237***	0.0225***	0.0107	0.0241***
	(0.0000)	(0.0007)	(0.0000)	(0.0000)	(0.3333)	(0.0019)
Log(population growth + 5)	− 0.0237*	− 0.0004	− 0.0365***	− 0.0199	0.0276	− 0.0159
	(0.0541)	(0.9725)	(0.0017)	(0.1444)	(0.2514)	(0.3837)
Constant	0.0410	0.2476***	0.0692	0.0459		0.0718
	(0.1652)	(0.0000)	(0.2679)	(0.1631)		(0.1163)
Observations	828	828	828	828	678	828
R-squared	0.2261	0.2592	0.5223			
F-test	0.8444	0.6350	0.0255	3.7505	1.0668	5.8208
p value for test redistribution = inequality	0.3598	0.4270	0.8733	0.0528	0.3017	0.0158

variation; and our preferred systems GMM estimator which explores both the time-series and cross sectional variation-replicating the specification in Table 2 [3]. (Unlike the rest of the estimators the two GMM estimators allow consistent estimation in the presence of a dynamic panel and potentially endogenous variables.)

Overall, our results confirm findings in the literature and the findings of Halter et al. (2014). Estimates based only on time-series variation such as the dGMM used in Forbes (2000) generally find a positive impact of inequality on growth. On the other hand, estimation methods exploiting the cross-sectional variation in the data tend to find a negative relationship (e.g. Alesina and Rodrik 1994; Persson and Tabellini 1994; Deininger and Squire 1998; Barro 2000). Combining both cross-sectional and time series variation as in sGMM is indeed important.

As an additional illustration, we explore the growth–inequality relationship in the short-run and long-run context graphically. The left column in Fig. 6 plots per capita growth rates against the (lagged) change of inequality or redistribution, thus representing short-run changes. The right column plots the level of per capita GDP against the (lagged) level of inequality or redistribution. Two distinct patterns emerge: in the long-run there is a strong negative relationship between per capita income and inequality; however, the growth–inequality relationship is (slightly) positive in the short-run. The bottom row of the figure suggests that higher transfers may increase long-run growth, but there is no significant relationship in the short-run.

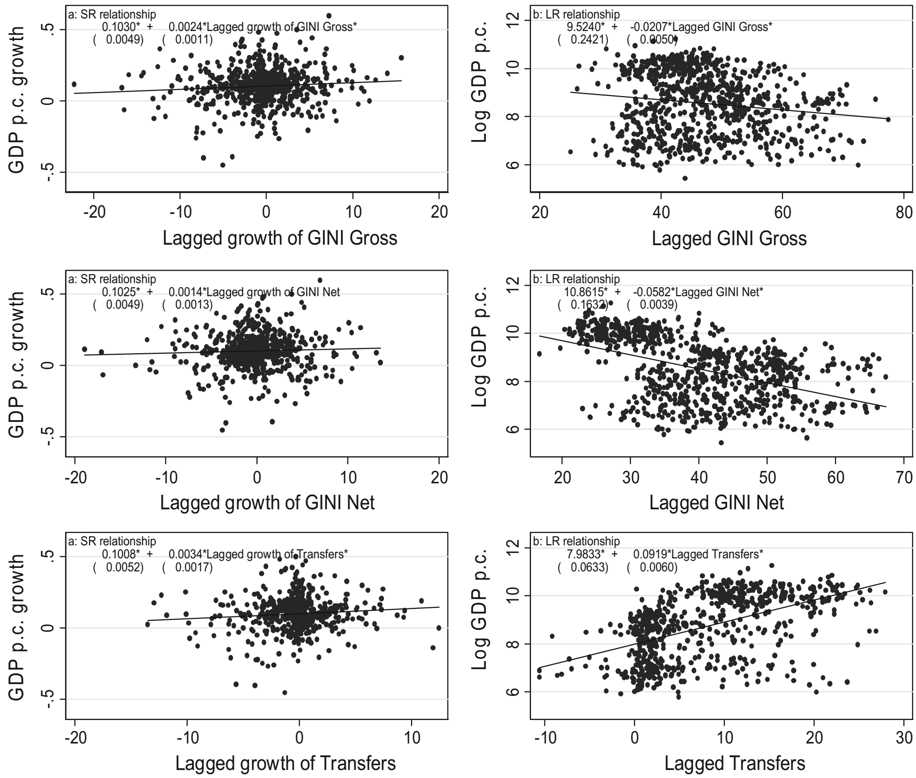

Fig. 6 Market inequality, net inequality, redistribution, and growth: evidence from short-run and long-run effects

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