Courant Research Centre
'Poverty, Equity and Growth in Developing and Transition Countries: Statistical Methods and Empirical Analysis'

Georg-August-Universität Göttingen
(founded in 1737)

Discussion Papers

No. 205

Re-estimating the Relationship between Inequality and Growth

Nathalie Scholl, Stephan Klasen

September 2016

Wilhelm-Weber-Str. 2 · 37073 Goettingen · Germany
Phone: +49-(0)551-3914066 · Fax: +49-(0)551-3914059

Email: crc-peg@uni-goettingen.de Web: http://www.uni-goettingen.de/crc-peg

Re-estimating the Relationship between Inequality and Growth

Nathalie Scholl[a] and Stephan Klasen[*]

University of Göttingen, Department of Economics

September 21, 2016

a) corresponding author
E-mail: nathalie.scholl@wiwi.uni-goettingen.de

Abstract

In this paper, we revisit the inequality-growth relationship using an enhanced panel data set with improved inequality data. We explicitly take into account the special role of transition (post-Soviet) countries and add an instrumental variable (IV) estimation to add a causal interpretation to our findings. Our analysis is based on the specification of Forbes (2000), but we also address the functional form concerns raised by Banerjee and Duflo (2003). We arrive at three main findings: First, the significant positive association between inequality and economic growth in the full sample is entirely driven by transition countries. Second, this positive relationship in transition countries is not robust to the inclusion of separate time effects. Lastly, it therefore appears that this association is not causal but rather driven by the particular timing of the transition dynamics. In particular, the rise in inequality in the 1990s coincided with a sharp output collapse, leading us to find an association between the large increase in inequality in the early 1990s and a growth recovery in the late 1990s. Results from IV estimation confirm our interpretation of the observed positive relationship in the overall sample as non-causal. In sum, once the transition country dynamics are accounted for, we find no robust, systematic relationship between inequality and subsequent growth, neither for levels nor for changes in inequality. These results hold for different lag structures as well as in the medium- rather than the short term, and the empirical patterns observed are robust to the use of different data sets on inequality.

JEL classification: O11, O15, O40, E25.
Keywords: Inequality; Growth; Transition Countries.

[*]**Acknowledgements:** We are grateful to Sebastian Vollmer, Axel Dreher, and the participants of the 2016 Development Economics and Policy Conference, the 2015 AEL Doctoral Research Seminar, the RTG Globalization and Development workshops, the 11th ISI conference on Economic Growth and Development, and the 2015 GlaD conference for valuable comments and suggestions.

1 Introduction

The possible trade-off between inequality and growth has been investigated theoretically and empirically for decades. In the mid-1990s, the empirical debate was significantly enhanced by the availability of a much broader set of data on inequality across the world. Initially, the workhorse dataset was created by Deininger and Squire (DS1996) and used in a study by Deininger and Squire (1998) to show that, in a cross-section of countries, initial inequality (particularly of assets but, in some specifications, also of income) was associated with subsequent lower growth.

Ensuing debates focused on the one hand on weaknesses in the data, where Atkinson and Brandolini (2001) showed that the comparability and consistency of the (DS1996) data set was open to question. Since then, the World Income and Inequality Database (WIID) was created which significantly enhanced not only the coverage but also the transparency of the inequality data used. Many studies on inequality have since relied on this dataset, where some authors used regression-based adjustment methods to address inconsistency issues (e.g., Gruen and Klasen 2008, 2012; Easterly 2007). Nevertheless, the dataset remains heterogeneous in terms of the underlying monetary concept (covering not only different types of income (net, gross, wage incomes) but also consumption and expenditure), the measurement unit (household vs. individual), and the type of equivalence scale used for adjusting household-level data, amongst other dimensions. As pointed out by Atkinson and Brandolini (2009), it is often not sufficient to account for these differences using dummy variables for each category underlying the data that are being used in a regression, as has been frequently done in the literature. Doing so implicitly assumes that the differences between the types of unit remain constant over time, which has been shown to not be generally true.[1] More recently, Solt (2016) has, based on the WIID, used imputation techniques to also attempt to address data gaps and consistency issues in his Standardized World Income Inequality Database (SWIID). Although this approach has also been criticized (Jenkins 2015), we will rely on these data in our analysis, but also show the robustness of our results to using the WIID data.

A second focus of the debate was the empirical specification of the inequality-growth

[1]See, e.g., the examples provided in Atkinson and Brandolinie (2009) on the increasing difference between pre- and post-tax income.

relationship. In particular, Forbes (2000) moved from the cross-sectional setting used by Deininger and Squire (1998) to a panel setting for two reasons. First, she wanted to address unobserved heterogeneity through fixed effects (and endogeneity through the use of GMM-type methods). Second, a fixed effects specification which exploits the within-variation is also the more policy-relevant question, as policy-makers are interested in whether changes in inequality in a country will promote or hurt subsequent growth. This approach came at the cost of using rather short panel periods of only five years. Essentially, this time span implies examining the short-term impact of changes in inequality on growth. While interesting, it is not so closely related to the theoretical literature which generally focused on longer-term impacts of inequality on growth (e.g., Galor and Zeira 1993; Alesina and Rodrik 1994. Forbes found that rising inequality is associated with higher subsequent growth, although the result is not significant when 10 year periods are used.

The paper by Forbes attracted a lot of debate and commentary. Apart from the abovementioned data issues (her analysis was based on the DS dataset), there was the concern that the use of fixed effects takes out most of the variation in the dataset and that the little within variation might be heavily affected by measurement error (Knowles 2005). Second, there was concern about the functional form. In particular, Banerjee and Duflo (2003) argued that the data are more consistent with the claim that any change in inequality (whether positive or negative) is associated with lower subsequent growth, which is, of course, a rather different interpretation. There have been further debates on this issue (some of which we address below), but the question how inequality affects growth in a panel setting remains open.

There are three further reasons to revisit this debate again. First, we now have an additional 15 years that can be used to study whether the relationship holds in a longer panel. Second, there have been further improvements in coverage and consistency of inequality data so that one can examine this relationship with an improved data set on inequality. And third, it is important to consider to what extent the relationships found by Forbes (2000) relate to the unique experiences of transition countries. This relates to a separate literature that has pointed out that transition countries experienced a large negative output shock at the start of the transition period in the early 1990s from which they recovered in the late 1990s and early 2000s. More importantly, this initial output shock was associated with a large increase in inequality. In fact, as shown by Ivaschenko (2003) and Klasen and Gruen (2001), the size of the output shock in transition countries was positively correlated with the size of the increase in inequality up until the mid-1990s. The changes in inequality in transition countries in the 1990s and 2000s were among the largest to be found anywhere in the world so that this unique experience, causing a concurrent increase in inequality as well as a collapse and subsequent recovery in economic growth, could potentially be driving the results.

In this paper, we therefore revisit the inequality-growth relationship using an enhanced panel data set with improved inequality data and special attention to the role of transition countries. We base our analysis on the specification of Forbes (2000), but also consider other specifications (including those of Banerjee and Duflo 2003). We find that, when using her specification and the full sample, higher inequality is still significantly associated with higher subsequent growth. But we also find that this finding is entirely driven by the experience of transition countries and is not present in the remaining country sample. It also appears that while increases in inequality are associated with higher growth in transition countries, very rapid and very large increases are associated with reduced growth. However, once we introduce separate time effects for transition countries, these associations disappear as well. We corroborate the finding that there is no robust relationship between inequality and growth in the overall sample with a new instrumental variable strategy introduced by Nunn and Qian (2014) in the literature on aid and growth. We also do not find evidence that the Banerjee and Duflo (2003) specification is superior and cannot confirm symmetry in the relationship of changes in inequality with economic growth.

These results point to three conclusions. First, there is no systematic empirical relationship between initial inequality and growth across the world, except in transition countries. Second, our finding is consistent with the claim that the relationship we find for transition countries is due to the particular timing of inequality and growth dynamics during and after the transition. In particular, the rise in inequality in the 1990s coincided with a sharp output collapse, leading us to find an association between the large increase in inequality in the early 1990 and a growth recovery in the late 1990s. Given that this relationship disappears once separate time effects are introduced for transition countries, it could mean that this association is not causal but rather driven by the particular dynamics of the transition. Instrumenting for inequality, we corroborate the interpretation that there is no systematic relationship between inequality and growth in the transition countries either, and find that the coefficient turns negative, but remains insignificant throughout all tested specifications. Lastly, if the findings for the transition countries are not entirely driven by the temporal dynamics of the transition process, our results suggest that the very low inequality in transition countries at the start of the transition process might have been a barrier to higher growth, but rapid increases apparently were detrimental also there. However, as our instrument is, by construction, not able to pick up the transition experience, we cannot definitely rule out that part of the growth acceleration in the late 1990s originated from the increase in inequality during the transition.

2 Literature Review

There is a large theoretical and empirical literature on the relationship between inequality and growth. Because this paper estimates a reduced-form relationship between inequality and growth and does not explicitly test any particular channel through which inequality might affect economic growth, we will not discuss the theoretical literature in detail, but rather give a broad overview of different types of arguments and direct the interested reader to excellent summary papers of the respective field. Following Voitchovsky (2009), theoretical papers can be broadly divided into four types of arguments relating to different parts and aspects of the income distribution.

The first group of papers relates to the circumstances of the poor. One of the arguments most frequently brought forward for why inequality can be bad for growth is that of missed investment opportunities for those at the bottom end of the distribution. Credit market imperfections are the basis for the idea that because the poor are subject to credit constraints, this leads to foregone investment opportunities in physical and human capital, and hence foregone economic growth (e.g., Birdsall 2008, Ghatak and Jiang 2002, Deininger and Squire 1998, Galor and Zeira 1993). Other arguments relating to the bottom end of the income distribution pertain to vicious cycles of poverty and crime (e.g., Chiu and Madden 1998, Josten 2003, and poverty and fertility (e.g., Kremer and Chen 1999).

A second group of arguments, focusing on the size and circumstances of the middle class, argues that domestic demand is a crucial factor determining economic growth, and is typically associated with a (relatively) equal income distribution with few poor (e.g., Foellmi and Zweimüller 2006, Murphy et al. 1989). For a more detailed survey of the demand-side type of arguments, see Erhart (2009). A second well-know channel of how inequality and growth are linked through the circumstances of the middle class is the median voter theorem and related political economy arguments, postulating a negative relationship between inequality and growth Arguably, the higher the inequality in a society, the larger is the gap between mean income and the income of the median voter and the higher his preference for redistribution through taxation, which can reduce incentives and thereby dampen economic growth (e.g., Bertola 1993, Alesina and Rodrik 1994, Perotti 1993). An overview of earlier literature on inequality and public spending can be found in Osberg et al. 2004.

Focusing on the upper part of the income distribution, there are a number of arguments pertaining to the concentration of wealth. One of the most frequently used arguments in favor of having a more unequal distribution of wealth is that the rich can provide the savings necessary for making large investments. This goes back to a model by Kaldor (1955). On the other hand, an unequal distribution of income with high "top" inequality can also be detrimental to growth when it is easier for the elite to capture institutions and extract resources from the economy or to move their capital abroad (see, e.g.,Glaeser et al. 2003.

Finally, the overall distance between individuals in a society also matters for inequality. How far individuals or groups in a society are from each other in economic terms can have important repercussions on growth via the formation of social capital and trust. If very large, the distance between individuals can also have explicit negative consequences for growth via social unrest and the socio-political polarization of society (see, e.g., Keefer and Knack 2002, Easterly 2001).

In sum, while there are arguments in both directions, most of the more recent work favors inequality hurting long-term growth. However, the type and extent of inequality matters. Ferreira et al. (2014), for example, distinguishes between "good" inequality, which rewards effort and leads to better performance (analogous to the "incentive" argument), and "bad" inequality, which wastes human potential (analogous to inequality of opportunity). Besides these conceptual differences, the time frame considered also matters for theoretical predictions of how inequality should affect growth.

In terms of empirical evidence from reduced-form estimations of the effect of inequality on economic growth, we will focus on only the most important contributions given the vast number of empirical studies on the topic. The following overview is based on Neves and Silva (2014). Overall, the evidence on the empirical impact of inequality on growth is mixed and remains controversial. However, a pattern emerges with regards to the results obtained using different empirical specifications. Generally, cross-sectional studies (Alesina and Rodrik 1994, Persson and Tabellini 1994, Clarke 1995, Perotti 1996, and Deininger and Squire 1998 tend to find a negative relationship between inequality and growth, whereas panel analyses yield mostly positive or insignificant results. But Knowles (2005) argues that most evidence on the growth and inequality relationship in cross-sectional studies is derived from inequality data which are not fully comparable. Once the heterogeneity in the underlying income concepts is accounted for, he concludes that there is no remaining relationship between income inequality and growth, but that inequality in expenditure is still negatively correlated with growth. The only cross-sectional study explicitly addressing the endogeneity problem is Easterly (2007), who instruments inequality with a country's "wheat-sugar ratio", which is a function of the fraction of land suitable for growing wheat over the fraction of land suitable for growing sugar cane. The idea is based on the hypothesis by Engerman and Sokoloff (1997) that agricultural endowments predict a country's institutional environment. More specifically, growing sugar cane is more prone to large-scale farming involving slave labor, which leads to higher inequality and extractive institutions, whereas wheat production involves family farming and is associated with the emergence of a middle-class and less inequality. Instead of growth rates,(Easterly, 2007) then shows that higher inequality is associated with lower income levels, as well as worse institutions, and lower education. Most of the cross-sectional results should be viewed with caution because they may contain substantial omitted variable bias, given that any unmeasured factors which are associated with both inequality and growth can be wrongly attributed as an effect of inequality on growth.

Although panel data are not able to perfectly resolve this issue, the possibility of introducing fixed effects allows the removal of at least the time-invariant portion of the omitted variable bias, which is also the main explanation for the divergence in findings between cross-sectional and panel studies. Moreover, it is also more useful from a policy perspective to know what happens to growth if inequality changes within a country, which can be estimated only if the data also contain a time-series dimension. However, apart from the abovementioned data problems which continue to persist in many of the panel data studies using the DS1996 or the WIID data, as well as any remaining concerns about omitted variable bias and endogeneity, panel studies do suffer from another shortcoming: since many of the theoretical effects are likely to have an impact over long periods of time, short-run panels that consider 5 or 10 year periods might be too short to pick up these effects. Nevertheless, we limit the discussion to panel data studies in the following, also because they are more relevant for the empirical set-up of this paper.

The most important study in the context of this paper is Forbes (2000), which we also use as the basis for our own empirical set-up. She finds a small, but positive and significant impact of inequality on subsequent economic growth using 5-year averaged growth rates and the DS1996 dataset. Her sample consists of 45 low- and high-income countries during 1975-95. The application of a difference GMM estimator to deal with the upward bias arising from her dynamic panel structure has, however, been shown to be problematic. Roodman 2009 demonstrates that Forbes' results become insignificant once the econometric issue of overidentification is being addressed, which is something we can confirm in our data as well.

Another widely cited study, Barro 2000 finds, for a samples of 40 to 70 countries and 10-year time periods, that higher inequality leads to lower growth in poor countries and higher growth in rich countries, but there is little overall relationship between income inequality and growth. He refrains from using fixed effects in his preferred specification and points to the exacerbation of measurement error with this approach, but his results from a three-stage least-squares estimation do hold qualitatively in a fixed effects specification, although the latter is only able to capture the contemporaneous relationship between inequality and growth.

Banerjee and Duflo (2003) criticize the functional form assumptions made in previous studies and argue that the growth rate is an inverted U-shaped function of net changes in inequality. They further show how this non-linearity can explain the different findings in previous studies. However, their paper has little to say on the fundamental question of whether inequality is bad for growth. Nevertheless, we test their main empirical specifications on our data as well and find no evidence to support superiority of their empirical (non-linear) set-up over ours.

Deininger and Olinto (2000) focus on asset instead of income inequality in their panel of 60 countries, and find a negative and significant relationship with subsequent growth rates. In addition, they confirm the positive relationship with income inequality as found in previous studies, which continues to hold even when asset inequality is retained in the model.

Ezcurra (2007) looks at annual regional growth across the European Union over the 1993-2002 period and concludes that higher inequality is associated with lower growth, thereby contradicting Barro (2000) wo found that inequality is positively related to growth in rich countries - although the differing results of the two studies could also be due to the different time frames they consider. In sum, results from reduced-form panel studies are heterogeneous and despite the continuous improvement of the inequality data since DS1996, data issues as well as concerns about functional form and appropriate estimation techniques keep being raised in the literature.

3 Data and Empirical Strategy

Our estimations are based on a sample of 122 countries over the 1961-2012 period, with a total of 712 observations for the level, and 577 observations for the difference specifications (115 countries). This is much larger data set both in its cross-country as well as its time dimension than those that have been used in the literature. The IV estimates rely on a smaller sample of 92 countries, which does not, however, affect the point estimates of inequality.[2] Unless indicated otherwise, estimations are using 5-year averages of growth as the dependent variable and the beginning-of-period Gini, lagged by one period, as the variable of interest. That is, the first time period is 1961-1965 and the last one is 2011-

[2] We are able to reproduce table 2 using only the subsample used also in the IV. Results are shown in appendix table A.1.

2012,[3] yielding a total of 12 time periods. Except for the GDP data,[4] which is taken from the Penn World Tables (PWT), Version 8.0 (Feenstra et al. 2015),[5] all control variables are as in Forbes (2000): the price level of investment (also from the PWT) is included, which she uses as a proxy for market distortions, and average years of secondary schooling for the population aged over 25 (taken from the Barro and Lee database, Version 2.0) is added separately for males and females.

To add a causal interpretation to our findings, we use an instrumental variable (IV) estimation employing a recently introduced technique (Nunn and Qian, 2014) which allows us to use Easterly's (2007) wheat-sugar ratio as an instrument for inequality in a panel set-up. We are thereby able to address endogeneity concerns more convincingly with the simultaneous use of both fixed effects and IV-estimation. The idea of the instrument is that the interaction between a cross-sectional variable, varying only between countries, and a time-varying variable, which is the same across all countries, is valid if the level of the respective variable is controlled for. This is generally taken care of for the cross-sectional variable by using a fixed effects estimator, and for the time-varying variable by including year dummies. As mentioned, we use Easterly's (2007) wheat-sugar ratio as the cross-sectional variable and interact it with the oil price, which introduces variation in inequality over time. A higher oil price arguably leads to higher inequality numbers because higher oil prices have a disproportionately larger adverse effect on the poor, who spend a larger share of their budget on staple food items and transport, the prices of both of which increase with the oil price (empirical evidence on the oil price-poverty-inequality link stems mostly from country case studies; see, e.g., Naranpanawa and Bandara (2012) for Sri Lanka, or Essama-Nssah et al. (2007) for South Africa). The estimator then compares the difference in growth in years following a high oil price to years following a low oil price in countries that have a high wheat-to-sugar ratio (low inequality) to countries that have a high wheat-to-sugar ratio (high inequality). The identifying assumption is that the effect of the oil price on growth will not (systematically) differ between countries with a

[3] 2011-12 is the only period with less than five years. More recent data was not available at the time of writing.

[4] Forbes used Gross National Income data from the WDI.

[5] In choosing the accounting concept underlying the GDP data for growth rates and levels, we follow the recommendations of the PWT and use the (real) output-based growth rates derived from the national accounts as the dependent variable and the expenditure-based current-price level of GDP as the initial level to capture convergence effects.

high and a low wheat-to-sugar ratio through channels other than inequality. More intuitively, the resulting estimator is similar to a difference-in-differences approach, but with a continuous treatment variable (inequality). The sugar-wheat ratio is correlated with inequality levels (corresponding to the pre-treatment differences in an important observable variable), whereas the oil price is correlated with inequality differences (corresponding to a common time effect in the variable of interest). Through the country fixed effects and the year fixed effects, we also take out the difference of the "baseline" growth rate between countries with high inequality levels and low inequality levels and the time trend they have in common (changes in growth). Remaining changes in growth, taking the baseline differences in inequality levels as well as in growth, and common trends in inequality as well as growth into account, are then attributable to changes in inequality if the exclusion restriction is valid - that is, if changes in the oil price do not systematically affect growth differently in countries with high and countries with low sugar-wheat ratios in a way that is correlated with a country's sugar-wheat ratio after controlling for a number of other potential influencing factors.

Our main measure of inequality, the Gini coefficient of net income, is taken from the Standardized World Income Inequality Database (SWIID) (Solt 2016). One of the main advantages of the SWIID is that the data are strongly balanced, i.e., all missings in the final dataset stem from other control variables. The SWIID is based on the World Income Inequality Database (WIID) (UNU-WIDER 2015) and standardizes the rather heterogeneous and unbalanced database by drawing on several other data sources and multiply imputing values to make the resulting data comparable across countries and over time. The final dataset contains 100 imputations for each data point, allowing the researcher to explicitly account for the uncertainty associated with imputing values by using multiple imputation (mi) estimation. Unless indicated otherwise, estimations employ the "mi: estimate" command as provided by Stata, which yields a single coefficient estimate and its corresponding corrected standard error applying Rubin's rule (Rubin 2004). As opposed to the regression results which exploit all of the 100 imputations, the descriptive statistics and graphs are based on the mean value of the Gini across the 100 imputations. In addition to the overall sample, descriptives are reported separately for transition- and non-transition countries. Our classification of transition countries is based on Gruen and

Klasen (2012) and includes 22 post-Communist countries, of which the following 15 are part of our sample: Albania, Armenia, Bulgaria, Czech Republic, Estonia, Hungary, Kyrgyz Republic, Latvia, Lithuania, Poland, Romania, Russia, Slovak Republic, Slovenia, and Ukraine. Table 1 contains descriptive statistics for all variables used in the model.

As one can see, most variables do not display major differences between transition- and non-transition countries, notable exceptions being schooling of both males and females, and, very importantly, inequality. The average Gini coefficient in transition countries is a full 8.5 Gini points lower than in non-transition countries, substantiating our belief that the inequality-growth relationship in transition countries is inherently different from that in the rest of the world - or at least the part covered by our sample.

Table 1: Descriptive Statistics

Total sample (712 obs.)	mean	sd	min	max
Gini	38.09	10.52	15.8	75.71
GDP per capita growth	0.023	0.032	-0.199	0.112
Price level of investment[6]	0.65	0.45	0.07	5.93
Initial GDP per capita (in 2005 PPP USD)	11533.3	11414.4	272.8	76523.6
Schooling (female)	2.22	1.56	0.02	6.89
Schooling (male)	2.6	1.52	0.15	7.25
Only transition countries (71 obs.)				
Gini	30.51	6.01	18.87	44.7
GDP per capita growth	0.02	0.054	-0.154	0.112
Price level of investment	0.58	0.21	0.21	1.01
Initial GDP per capita (in 2005 PPP USD)	10936.2	5899.3	1974.7	24519.5
Schooling (female)	3.68	1.15	0.99	6.47
Schooling (male)	3.89	1.03	1.46	6.62
Sample without transition countries (641 obs.)				
Gini	38.98	10.59	15.8	75.71
GDP per capita growth	0.023	0.028	-0.199	0.109
Price level of investment	0.66	0.47	0.07	5.93
Initial GDP per capita (in 2005 PPP USD)	11595.3	11842	272.8	76523.6
Schooling (female)	2.078	1.52	0.02	6.89
Schooling (male)	2.46	1.5	0.15	7.25

All estimations employ country fixed effects to control for unobserved heterogeneity and remove a potential source of (time-invariant) omitted variable bias. While some concerns have been raised in the literature that this approach exacerbates measurement

[6]The Price Level of investment (PI) is defined as the PPP over GDP divided by the exchange rate multiplied by 100.

error and removes a large part of the variation in inequality (e.g., Knowles 2005), the use of the more consistent SWIID data, which combine information from different datasets and thereby minimize measurement error, as well as an increase of the within-country variation in inequality in the past 15 years,[7] lead us to believe that these drawbacks no longer justify not using a within estimator. Because of the use of growth rates as the dependent variable and the initial GDP per capita level variable as a control, the fixed effects specifications suffer from Nickell bias, entailing an upward bias on our variable of interest (Nickell 1981). All significant estimates are therefore furthermore subjected to a difference Generalized Method of Moments (GMM) estimator (Arellano and Bond 1991). The estimator eliminates the bias by using deeper lags of the independent variables as instruments, which are by construction uncorrelated with the error term. Orthogonalizing the instruments mitigates the unbalancedness of the dataset. Using the full instrument set would lead to the problem of too many instruments, which in this case exceeds the number of cross-sections (122) and renders the Hansen test of overidentification invalid. In all our reported GMM estimates, the instrument set has therefore been restricted in different ways.[8] Because the multiple imputation command does not produce test statistics for the relevant GMM misspecification tests (AR1, AR2, and overidentification tests), they have been conducted individually for each of the 100 imputations. We then report the share of incorrectly specified regressions, along with the mean value of each test statistic. The multiply imputed regressions are considered well specified if less than 5% of the individual regressions are misspecified. In line with Forbes (2000), we use the difference GMM estimator. A system GMM (Blundell and Bond 1998) is sometimes suggested in the literature because the use of the level equation implies that the estimator is less prone to measurement error. However, although the system GMM estimator does yield similar estimates, the results are less clear, and, more importantly, the misspecification tests indicate problems in all but a few cases. System GMM is therefore retained as a

[7] The within-country variation of net income inequality has increased from 14% of the overall variation in the 1960-1996 sample to 18% of the overall variation in the sample going until 2012. While this may still seem rather small, within-country variation of market inequality has increased from 24 to 32%, implying that some of the observed lack of within-variation is the result of successful redistribution.

[8] Instruments have been restricted to a maximum of 2 lags, and collapsed in some cases. The restrictions imposed on the individual GMM regressions are reported in the respective table notes. Our results do not depend on the type of instrument restriction used and we report the ones which perform best in terms of the share of misspecified regressions (which is explained just below).

robustness check, but the preferred estimator is a (two-step) difference GMM. Standard errors are robust in all estimations as per Windmeijer's (2005) correction procedure.

4 Results and Discussion

Table 2 displays the first set of results. The first column corresponds to Forbes' (2000) basic specification. Like Forbes, we find a positive coefficient on the inequality variable, although the coefficient is substantially smaller than hers, and, as found by Roodman (2009), this effect does not hold with a non-biased GMM estimator. Appendix table A.2 displays the results for different instrument restrictions, none of which are well specified. Moreover, although the coefficient is now closer to Forbes' estimate of 0.0013, it loses significance in most specifications. Once we include a transition country dummy in column 2 and interact it with the inequality measure, the results become much clearer. The coefficient on the interaction is now substantially larger and highly significant. Moreover, the effect persists in the GMM specification, as shown in column 3. This time, we are also able to find a well-specified regression, which further underpins our belief that the inequality-growth relationship during the transition is inherently different from that in the rest of the sample and that it is incorrect to estimate a common slope parameter for the two processes. Notably, as the transition countries pick up the positive effect of inequality on growth, the non-interacted inequality variable shrinks substantially and turns insignificant. That is, we do not find any effect of inequality on growth in the remaining (non-transition) countries and our findings lead us to conclude that the small positive impact found in the full sample is not robust and is furthermore driven by a small group of transition countries. According to the fixed effects estimate, which is the lowest of our point estimates for the impact of inequality on growth in transition countries, a ten point increase in a country's Gini coefficient - which is roughly equal to the total increase in inequality in transition countries between 1985 and today - would lead to a 4 percent increase in average annual growth over the next five years. However, this result is to be taken with caution. The processes occurring in the 1990s in transition countries after the breakdown of the Soviet Union - political and economic liberalization, the introduction of market economies and opening up of markets to (non-Soviet) external trade - were exogenous events with effects on both inequality and growth.

Figure 1 illustrates the average correlation across all transition countries between inequality and growth as it occurs in the estimation, that is, with the Gini coefficient lagged by one period. A striking image emerges with a sharp increase in both growth and lagged inequality between 1995 and 2000, raising concern that this period might be driving the effect in transition countries. Moreover, it appears as if it is precisely the 5-year lag structure used in our estimations which causes this correlation. Nevertheless, one should be cautious in interpreting the graph since it merely displays the averages across all transition countries, and developments within single countries do not necessarily show the same correlation as depicted here. Indeed, when consulting the individual correlations in each country (as shown in appendix figure A.1), the picture is less clear. An outlier analysis [9] does not yield any clear results pertaining to the issue, either - no single country-year observation is driving the positive impact of inequality on growth in the transition countries.

In order to capture the events occurring in the 1990s which might be driving the observed correlation between inequality and growth at least partially, we introduce separate time effects for the group of transition countries. Indeed, once the separate period dummies are introduced, the positive impact of inequality on growth disappears also for the transition countries, and remains very small and insignificant for the remaining sample (column 4 of table 3). Finally, we re-estimate the model of column 1 (corresponding to Forbes' basic specification) in column 5, and introduce separate year dummies for transition countries without including an interaction between the inequality measure and the transition country dummy. The mere introduction of a separate time effect for the transition countries slashes the positive coefficient of inequality by more than half and wipes out the previously found significant positive effect of inequality on growth. In sum, we cannot confirm that higher inequality enhances economic growth in our sample of countries outside the transition period, at least not in terms of higher levels - as opposed to increases or decreases - of inequality.

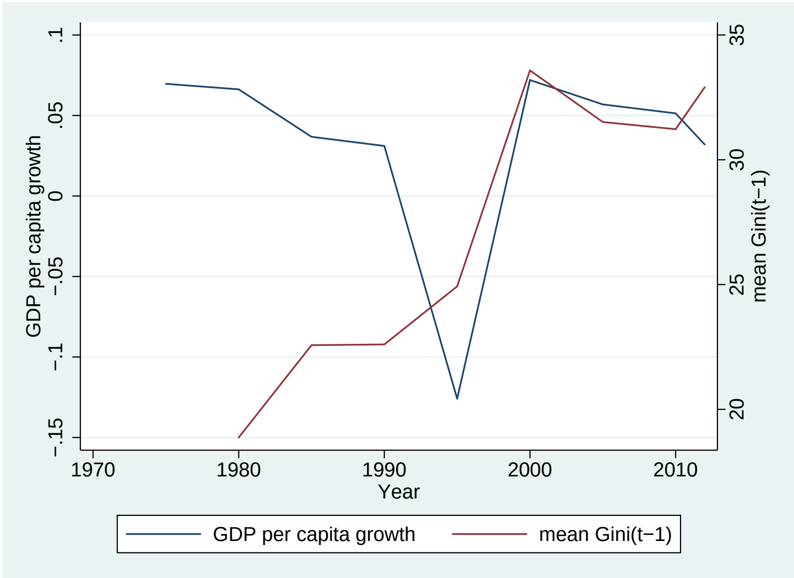
[9] Instruments have been restricted to a maximum of 2 lags, and collapsed in some cases. The restrictions imposed on the individual GMM regressions are reported in the respective table notes. Our results do not depend on the type of instrument restriction used and we report the ones which perform best in terms of the share of misspecified regressions (which is explained just below).

Table 2: Baseline specifications in levels, FE results

Dep. var.: GDP growth	(1) multiple imputation estimation	(2) transition country interaction	(3) transition country interaction, diff. GMM	(4) transition country interaction & transition-year dummies	(5) transition-year dummies
Gini(t-1)	0.000472*	0.000140	-0.000103	0.000149	0.000169
	(0.000269)	(0.000234)	(0.000612)	(0.000228)	(0.000222)
Transition* Gini(t-1)		0.00400***	0.00653**	0.000565	
		(0.00150)	(0.00282)	(0.00129)	
GDP(t-1)	-0.0513***	-0.0469***	-0.0652***	-0.0420***	-0.0423***
	(0.00895)	(0.00897)	(0.0139)	(0.00890)	(0.00881)
PI(t-1)	-0.00834	-0.00766	-0.00322	-0.00902	-0.00906
	(0.00515)	(0.00527)	(0.0104)	(0.00579)	(0.00579)
Schooling_m(t-1)	2.03e-05	0.00260	0.00107	-0.00205	-0.00207
	(0.00852)	(0.00894)	(0.0175)	(0.00775)	(0.00775)
Schooling_f(t-1)	0.00308	-0.000914	0.000752	0.00155	0.00161
	(0.00922)	(0.00979)	(0.0168)	(0.00952)	(0.00949)
Constant	0.415***	0.382***		0.360***	0.362***
	(0.0694)	(0.0714)		(0.0698)	(0.0689)
# of instruments			74		
AR1			0,0013559		
AR2			0,4196453		
Hansen test			0,1833639		
% misspecified			0		
Observations	712	712	590	712	712
# of countries	122	122	116	122	122
Trans-Year FE	NO	NO	NO	YES	YES
Year FE	YES	YES	YES	YES	YES

Notes. Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Instruments in the GMM estimator (column 3) have been orthogonalized and restricted to lags 3 and 4. p-values are reported for the GMM misspecification tests (AR1, AR2, Hansen test). The system GMM estimate can be found in columns 1-3 of appendix table A.2

Figure 1: Correlation between growth and (lagged) inequality in transition countries



Building on Banerjee and Duflo (2003), who focus on the relationship between changes in inequality and growth, we also test Forbes' specification in differences instead of levels of inequality.[10] Neither in the full sample, nor using a transition country interaction - with and without separate time effects - do we find any significant impact of changes in inequality and growth.[11]

We then introduce both levels and differences simultaneously as shown in table 3. In column 1, when both levels and differences are included in the estimation, the positive coefficient of the level of inequality is confirmed, but inequality increases are associated with lower growth. Introducing the transition country interaction in column 2, it becomes clear that these effects are, again, driven by the transition countries. This would, at first glance, suggest that higher inequality levels in transition countries are associated with higher growth, but sharp increases lead to lower growth. However, like in the previous set of regressions, when we proceed to introduce transition-year effects, they eradicate the significance of the coefficients on the transition country-inequality interactions both for levels and differences.[12]

[10] Note that only the inequality measure has been differenced and the specification does not correspond to a model in differences.

[11] Results available upon request.

[12] However, the coefficients are not reduced as much as in the equation containing only the levels. When only the time dummies, but not the interaction for transition countries are introduced in column 4, the level effects for the whole sample are significant at the 10% level - however, when this effect is tested in a GMM framework, both the inequality level and change variables lose significance and decrease in size, in line with the direction of Nickell bias. It appears as if, once the effect of changes in inequality is accounted for

Table 3: Baseline specification, augmented with differences

Dep. var.: GDP growth	(1) FE	(2) FE	(3) FE	(4) FE	(5) diff. GMM
Gini(t-1)	**0.000947****	**0.000518**	**0.000532**	**0.000577***	0.000377
	(0.000384)	(0.000338)	(0.000326)	(0.000316)	(0.000440)
Transition* Gini(t-1)		**0.00426*****	**0.00145**		
		(0.00155)	(0.00118)		
Δ Gini(t-1)	**-0.000653****	**-0.000370**	**-0.000365**	**-0.000442***	-0.000204
	(0.000288)	(0.000262)	(0.000257)	(0.000256)	(0.000430)
Transition* Δ Gini(t-1)		**-0.00236****	**-0.00143**		
		(0.000919)	(0.00119)		
Observations	577	577	577	577	577
#of countries	115	115	115	115	115
Control variables	YES	YES	YES	YES	YES
Trans-Year FE	NO	NO	NO	YES	YES
Year FE	YES	YES	YES	YES	YES

Notes. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Instruments in the GMM estimator (column 7) have been restricted to lag 3, resulting in a total of 97 instruments. Misspecification tests (p-values) of the GMM are: AR1: 0.0058, AR2: 0.3473, Hansen test: 0.3473, misspecified: 0%. The system GMM estimate can be found in columns 4-6 of appendix table A.3

In order to add a causal interpretation to our main findings from table 2, we employ an IV strategy using the interaction of the wheat-sugar ratio and the oil price as described above. We use a one-period lag of the oil price to instrument for inequality,[13] and repeat the specifications from columns 1, 2, and 4. The second stage results are presented in the top panel of table 4, and the first stage is displayed in the bottom panel, along with weak-instrument tests. Our instruments turn out to be valid and are clearly above the cut-off value in the first two specifications, and still provide reasonable estimates in the third one.[14]

separately, higher inequality levels are associated with higher subsequent growth in transition economies. Although the coefficient is insignificant, it drives up the size of the effect in the overall sample and, if no separate time dummies are introduced, may lead to interpretations of a substantive inequality-growth relationship in these countries.

[13]There are two reasons for doing so: Firstly, the oil price cannot be expected to have an immediate impact on inequality given that it needs to work its way through the economy and even if it affects production immediately - which is not always plausible - it will not affect prices, e.g. for food, right away. Second, on statistical grounds, the first lag turns out to be the stronger instrument, although the contemporaneous oil price is also valid and delivers very similar estimates (results available upon request). When both the contemporaneous variable and the first-period lag are included as instruments, only the lagged oil price turns out to be significant. We therefore decided to drop the contemporaneous variable from the instrument set.

[14]This is the F-statistic for the single-instrument case (columns 1 and 2) and the maximum bias in percent according to the Kleibergen-Paap test statistics, which is appropriate when several instruments and robust standard errors are used (column 3). Our instrument turns out to be strong as per the cut-off value of 10 for the first-stage F-statistics, and while the coefficient in column 3 may be biased up to 10%

Table 4: Baseline specifications, IV results

Dep. var.: GDP growth	(1) basic	(2) transition-year effects	(3) transition country interaction
Gini(t-1)	-0,00221	-0,000688	-0,000735
	(-0,0014)	(-0,00135)	(-0,0016)
Transition* Gini(t-1)			-0,00725
			-0,00593
GDP(t-1)	-0.0503***	-0.0415***	-0.0649***
	(-0,0118)	(-0,00922)	(-0,0176)
PI(t-1)	-0,00321	-0,0055	-0,0051
	(-0,00594)	(-0,0056)	(-0,0058)
Schooling_m(t-1)	-0,00851	-0,00443	-0,0129
	(-0,00865)	(-0,00654)	(-0,0106)
Schooling_f(t-1)	0,0076	0,000866	0,0165
	(-0,00911)	(-0,00718)	-(0,0123)
Observations	566	566	566
# of countries	92	92	92
Year FE	YES	YES	YES
Transition-Year FE	NO	YES	NO
FIRST STAGE			
GDP(t-1)	2.505*	3.554***	2.477*
	(-1,318)	(-1,268)	(-1,327)
PI(t-1)	0,774	0,824	0,782
	(-0,701)	(-0,683)	(-0,703)
Schooling_m(t-1)	-2.709*	-2,092	-2.664*
	(-1,401)	(-1,386)	(-1,383)
Schooling_f(t-1)	1,499	0,535	1,464
	(-1,546)	(-1,534)	(-1,53)
SWratio*Oil price(t-2)	0.315***	0.312***	0.303***
	(-0,0835)	(-0,091)	(-0,093)
SWratio*Oil price(t-2)*trans			0,0455
			(-0,153)
R^2	0,127	0,192	0,127
Weak instruments: F-stat	14,2	11,79	
Kleibergen-Paap max. bias			10%

Notes. Robust standard errors in parentheses, ***p<0.01, **p<0.05, *p<0.1. Note that estimation is not based on multiple imputation because the combination of mi:estimate with xtivreg2 is currently not yet implemented in Stata. We use the average Gini across the 100 imputations instead. The point estimate using the average estimate across the 100 imputed data sets yields a coefficient of -0.00224, which is very close to the one estimated above.

Inequality is not only insignificant, but the coefficient has now turned negative, and remains so across all four specifications. This result is in line with the GMM finding from table 3, and indicates that there exists some form of endogeneity introducing a spurious positive correlation between inequality and growth in the OLS estimates. What is more, the instrument appears to work even with the transition country interaction. Although separately instrumenting for transition countries takes out a good deal of variation from the instrument given that they all are Eastern European and Central Asian countries and have positive wheat-sugar ratios, the IV still appears to deliver reasonable estimates. Since the IV is biased towards the OLS estimate in the presence of weak instruments, concern with weak instruments does not threaten the validity of our conclusions from column 3. If anything, the true parameter estimates might be even further from the positive association found in the OLS/FE specifications.

Naturally, the instrument does not pick up the changes in inequality caused by the transition and, even with the introduction of separate transition-period dummies in column 2, remains to be a strong instrument for inequality. Overall, these results substantiate our belief that the positive association between inequality and growth is non-causal, although we cannot definitely rule out that the increase from excessively low inequality levels during the transition did contribute to the subsequent high growth rates in the transition countries as well.

One should keep in mind, however, that even after the transition, inequality in transition countries is still rather low compared to non-transition countries: the maximum inequality value found among the transition countries is still only around half a standard deviation above the non-transition country mean. Our tentative reading of the results on the transition countries thus far could be that the higher inequality levels in these countries after the transition might therefore rather represent "normal" inequality levels, and the previous, excessively low inequality numbers could reflect the fact that inequality was kept at "artificially" low levels due to the income compression during the socialist system. The breakdown of the Soviet Union led to a well-documented (see, e.g., Aristei and Perugini 2012) large drop in output with a recovery over the 1990s, and the transition

towards the OLS estimate, this does not cause a problem for the results here given that the inequality variables are insignificant and very different from the OLS/FE results.

to a market economy was associated with unprecedented increases in inequality. Although we do not specifically test for this, our reading of the literature on the topic is that the two developments in inequality and growth are more likely to have been unrelated events. That is, both are caused by the transition, but one is not causing the other, although the possibility cannot be definitely ruled out on the basis of our estimations.

5 Robustness Tests

As a check on functional form, we test Banerjee and Duflo's proposition that changes in inequality may just be measurement error, and because measurement error is larger in times of economic distress, this would cause a negative relationship between changes in inequality and growth. Despite the fact that their argument would entail a contemporaneous relationship between inequality changes and growth and we are estimating a lagged one, we run a number of different specifications to see whether we find a symmetric effect of changes in inequality on growth. If positive and negative changes in inequality are symmetrically offsetting each other, this would also explain why we do not find any effect in the difference equations. In order to generally account for functional form issues brought up by Banerjee and Duflo, we also test the level equation for such effects. In a first step, we are simply including a quadratic term in both the level and the difference specifications. Table 5 displays the results.

The only significant result is that of the difference specification with the transition country interaction (column 2). An F-test of joint significance indicates that the effect is significant at the 1% level. At a value of 141.7, the maximum is located far from even the highest of the transition country Gini coefficients of 48.5, and even further from the mean of 29 Gini points. The result would hence indicate that the sample values are located on the upward-sloped part of the curve, meaning that positive changes in inequality enhance growth, but at a decreasing rate. However, when subjected to a difference GMM, none of the quadratic terms were jointly significant (as shown in table 2.A.3). We therefore reject the proposition of a quadratic effect of inequality on growth for transition countries as well as non-transition countries, and in both levels and differences.

Piecewise linear regressions

Table 5: Quadratic FE specifications in differences

Dep. var.: GDP growth	(1)	(2)	(3)	(4)
Gini(t-1)	0,00205	-0,000164		
	(-0,00156)	(-0,00123)		
Gini(t-1)^2	-1,76E-05	3,29E-06		
	(-1,56E-05	(-1,24E-05)		
Transition*Gini(t-1)		0,00717		
		(-0,012)		
Transition*Gini(t-1)^2		-5,06E-05		
		(-0,000183)		
ΔGini(t-1)			-0,000209	-0,00013
			(-0,000216)	(-0,000212)
ΔGini(t-1)^2			-1,09E-05	-1,09E-05
			(-1,33E-05)	(-1,17E-05)
Transition*ΔGini(t-1)				-0,00141
				(-0,00119)
Transition*ΔGini(t-1)^2				7,58E-05
				(-8,42E-05)
Observations	712	712	577	577
#of countries	122	122	115	115
Control variables	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Turning point for trans.	58.2 (Max.)	**141.7 (Max.)**	No quadr. effect	9.3 (Min.)
F-test of quadratic terms	0,2363	**0.0393*** **		0,4941

Notes. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The p-value is reported for the F-test. The turning point of the quadratic effect is only calculated for the transition countries, i.e., from the coefficients Transition*l.Gini and Transition*l.Gini^2 (and their respective values in differences. The F-test of quadratic terms test contains all constituent terms of the interactions, i.e., L.Gini, L.Gini^2, Transition*l.Gini and Transition*l.Gini^2

As another test of the functional form concerns, we run a set of piecewise linear regressions. They are based on inequality changes, and employ different margins of change ranging from 3 to 20% change in inequality, as indicated in the top row. Differential slopes are estimated for negative, zero (within the aforementioned margin), and positive changes. This is similar to Banerjee and Duflo's (2003) piecewise linear approach, but instead of using the model in differences, we are basing the inequality change brackets on the changes in levels of inequality since no evidence for any kind of relationship between inequality and growth was found in the differenced specification in the first step of our analysis (appendix table A.5).[15] The FE estimates (table 6, columns 1-4) show that growing inequality is

[15]The relationship is estimated with and without including the level variable of the Gini into the model, but results are almost identical between the two specifications (this is true for all versions of the piecewise linear specification, including the subsequent versions using subsamples and interactions) and we therefore proceed with the model without the level variable. Appendix table A.6 displays the results with the level

related to lower subsequent growth.

variable.

Table 6: Piecewise linear regressions of inequality changes, FE and GMM results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.:	FE	FE	FE	FE	GMM	GMM	GMM	GMM
GDP growth	3	5	10	20	3	5	10	20
Neg. change	-0,000217	-0,000114	5,41E-05	0,000513	0,00111	0,00113	0,00131	0,00194
	(0.000283)	(0.000323)	(0.000443)	(0.000858)	(0.000788)	(0.000813)	(0.000922)	(0.00131)
No change	0.00100*	0,000367	2,92E-05	-0,000126	-0,000221	-8,09E-05	-0,000106	3,92E-05
	(0.000509)	(0.000337)	(0.000200)	(0.000142)	(0.00128)	(0.000712)	(0.000416)	(0.000331)
Pos. change	-0.000995***	-0.00103***	-0.00116***	-0.00141***	-0.00164***	-0.00172***	-0.00197***	-0.00264***
	(0.000213)	(0.000225)	(0.000254)	(0.000319)	(0.000535)	(0.000585)	(0.000600)	(0.000949)
# of Instr.					93	93	93	93
AR1					0,0012974	0,0015662	0,0015634	0,0031463
AR2					0,9706342	0,9785592	0,9778599	0,9689
Hansen test					0,436301	0,4110748	0,3200502	0,266292
% misspcfd.					0	0	0	0
Observations	614	614	614	614	497	497	497	497
# of countries	115	115	115	115	110	110	110	110
Control vars.	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes. Robust standard errors in parentheses, ***p<0.01, **p<0.05, *p<0.1.Instruments in the difference GMM have been restricted to lags 3 and 4. The results with the restricted instead of the collapsed instrument set are reported here due to problems with the misspecification test for the 10%-change specification (column 7). Using collapsed instruments, the results are very similar for the positive changes, but negative changes are also significant (results available upon request). Numbers in the third row represent the knots for defining the "no change"-bracket, i.e., changes between $+/-3$ (5, 10, 20) percent are coded as "no change", and changes above (below) the knot as increases (decreases).

This relationship is confirmed in both the difference- and the system GMM estimations. The association is stronger, but less robust for larger changes in inequality. No robust relationship is found for negative inequality changes, but the coefficients are mostly positive, especially for the larger changes, and are significant in some of the GMM specifications. When the same estimation is repeated with a subsample excluding transition countries, the coefficients on the positive change variable retain their negative sign, but become insignificant. The results can be found in appendix table A.7.[16]

Finally, the specification using the full sample is repeated, but with interactions between the inequality change variables and a transition country dummy (table7). Although some of the positive coefficients are insignificant in the GMM estimations, the results clearly show that the negative and significant effect of positive inequality changes on growth stems from the transition countries only. In line with the results using only the subsample of non-transition countries, the coefficient on the positive change variable remains negative, but it is very small and far from significant. Again, once the transition country dynamics are accounted for separately (columns 5-8), no significant impact of inequality is found for the remaining sample. The results of the piecewise linear regressions indicate that when a separate slope is estimated only for those countries showing positive inequality changes, higher inequality increases are actually associated with lower growth rates. This finding directly contradicts the proposition put forward by Banerjee and Duflo (2003) that changes in inequality could just be measurement error, which would imply a symmetric effect of both positive and negative inequality changes being associated with lower growth. However, the positive effect is, again, driven by the group of transition countries and is not robust to transition-year effects.

[16]Because the FE results are insignificant, they are not further subjected to a GMM.

Table 7: Piecewise linear regressions of inequality changes with transition country interaction, FE results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.: GDP growth	3	5	10	20	3	5	10	20
Decrease	-0,000365	-0,000274	-0,000166	0,00022	-0,000351	-0,000249	-0,000136	0,000218
	(0.000276)	(0.000318)	(0.000442)	(0.000820)	(0.000273)	(0.000314)	(0.000434)	(0.000814)
Decrease*trans	0,00112	0,00122	0,00121	0,00167	-7,35E-05	-0,000431	-0,000775	-0.00629**
	(0.00136)	(0.00150)	(0.00201)	(0.00307)	(0.000999)	(0.00111)	(0.00180)	(0.00296)
No change	0,000317	-1,34E-05	-0,000128	-0,000143	0,000162	-0,000127	-0,000196	-0,000188
	(0.000458)	(0.000309)	(0.000189)	(0.000136)	(0.000457)	(0.000309)	(0.000188)	(0.000135)
No change*trans	-4,33E-05	-0,000101	-6,30E-05	-0,000596	0,000737	0,00094	0,000725	0,000662
	(0.00161)	(0.00109)	(0.000752)	(0.000672)	(0.00137)	(0.000879)	(0.000615)	(0.000426)
Increase	**-0,000151**	**-0,000133**)	**-0,000132**)	**-0,000188**)	**-0,000149**)	**-0,000126**)	**-0,000118**)	**-0,000165**)
	(0.000223)	(0.000240)	(0.000294)	(0.000416)	(0.000220)	(0.000238)	(0.000294)	(0.000414)
Increase*trans	**-0.00141*** **	**-0.00144*** **	**-0.00150*** **	**-0.00134** **	**-0,000172**	**-0,000219**	**-0,000292**	**-0,000272**
	(0.000379)	(0.000391)	(0.000448)	(0.000582)	(0.000380)	(0.000388)	(0.000429)	(0.000541)
Observations	614	614	614	614	497	497	497	497
# of countries	115	115	115	115	110	110	110	110
Control variables	YES	YES	YES	YES	YES	YES	YES	YES
Trans-Year FE	**NO**	**NO**	**NO**	**NO**	**YES**	**YES**	**YES**	**YES**
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes. Robust standard errors in parentheses, ***p<0.01, **p<0.05, *p<0.1. Instruments in the difference GMM have been collapsed. Numbers in the second row represent the knots for defining the "no change"-bracket, i.e., changes between +/−3 (5, 10, 20) percent are coded as "no change", and changes above (below) as increases (decreases).

Alternative time spans and lag structures

We also test our main specification for robustness to the choice of the lag structure as well as the time span chosen. Forbes (2000) also included 10 year averages in her analysis and found in what she called an "informal test" that the positive relationship between inequality and growth diminished over time, but noted that because of the limited degrees of freedom, these results were to be interpreted with caution. Now that we have four new time periods available for estimation, we are repeating the exercise to see whether there are different dynamics for ten- as opposed to five-year periods, and to test whether these effects are equally sensitive to how transition countries are accounted for in the estimation. As shown in table 8, the results using ten-year averages do not only qualitatively resemble the 5-year ones, but also the magnitude of the effects is rather similar. This is in stark contrast to Forbes' results, where the $10-$year coefficient on inequality was only little over one third of the $5-$year one. We can also confirm that the same caveats pertaining to the 5-year results are present in the 10-year averaged data as well: the inclusion of transition countries diminishes the positive impact of inequality on growth and renders the coefficient insignificant. Transition countries appear to have a positive relationship between inequality and growth, but once the transition-year effects are included as well (columns 3 and 4), there is no significant association between inequality and growth in neither the transition countries nor the remaining sample.

A second concern pertaining to timing is the lag structure. The graphical depiction of the inequality and growth variables in transition countries raises concerns that it is merely the choice of a one period lag which generates the correlation between the two variables. We therefore re-run the basic specification of table 2, once with a contemporary time structure and once with a two period lag. The contemporaneous specification, shown in the first panel of table 9, does not yield any significant results - if anything, there appears to be a negative contemporaneous correlation between inequality and growth in transition countries, but the effect is not robust to the inclusion of the transition-year effects (column 3). The coefficient on the remaining sample is very small and insignificant throughout. In sum, there seems to be no systematic contemporaneous relationship between inequality and growth, either. More results emerge with the two period lagged Gini coefficient,

Table 8: 10−year averages

Dep. var.: GDP growth	(1) baseline	(2) transition country interaction	(3) transition country interaction & trans.-year effects	(4) transition-year effects
Gini(t-1)	0.000377*	0,000253	0,000247	0,000268
	(-0,000225)	(-0,000218)	(-0,000216)	(-0,000212)
Gini(t-1)*trans		0.00255***	0,000883	
		(-0,000818)	(-0,000817)	
Observations	296	296	296	296
# of countries	118	118	118	118
Control variables	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Trans-Year FE	NO	NO	YES	YES

Notes. Robust standard errors in parentheses, ***p<0.01, **p<0.05, *p<0.1.

displayed in the second panel of table 9. The estimates are similar to those obtained for the one period lag (including the changes occurring when transition countries and transition-year effects are introduced) but are larger and more significant. Importantly, the coefficient for the overall sample remains positive and significant throughout the fixed effects specifications (columns 4-8).[17] However, when subjected to a GMM,[18] it loses significance as well. We are therefore confident that our results are neither contingent on the choice of a particular lag structure, nor on the use of 5-year averages rather than a longer time span.

[17]We have ruled out that this is simply a sample composition effect. Results using a constant sample from the two period lag specification are available upon request.

[18]Note that the GMM is not based on a multiple imputation estimation due to problems with keeping the sample constant when deeper lags are involved. The corresponding FE estimate (replicating column 7), along with further GMM specifications using other restrictions on the lags can be found in appendix table A.9. Because the non-mi FE estimate is slightly larger and more significant than the one using proper mi estimation, the corresponding GMM estimate is a rather optimistic estimate of the impact of inequality on growth, and the mi estimate can be expected to be slightly lower.

Table 9: Alternative lag structures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lags	0	0	0	2	2	2	2	2
Dep. var.: GDP growth	FE	FE	FE, trans. country interaction & trans.-year effects	FE	FE	FE, trans. country interaction & trans.-year effects	FE, trans.-year dummies	GMM, trans.-year dummies
L.Gini	-5,66E-05	0,000104	4,29E-05	0.000793***	0.000518**	0.000515**	0.000560**	0,0000169
	(0.000305)	(0.000301)	(0.000290)	(0.000278)	(0.000256)	(0.000251)	(0.000245)	(0.000785)
L.Gini*trans		-0.00441*	-0,00145		0.00269***	0,0012		
		(0.00226)	(0.00188)		(0.000854)	(0.00119)		
Observations	721	721	721	625	625	625	625	506
# of countries	122	122	122	119	119	119	119	114
Control vars.	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Trans-Year FE	NO	NO	YES	NO	NO	YES	YES	YES

Notes. Robust standard errors in parentheses, ***p<0.01, **p<0.05, *p<0.1. Instruments in the difference GMM have been collapsed. Numbers in the second row refer to the lag length of the inequality variable.

Alternative inequality data

Although there are some clear advantages to using Solt's 2016 SWIID data, some researchers have expressed concern over the choice of the imputation procedure, and the validity of the resulting data (Jenkins 2015). We therefore repeat our analysis with the WIID data, a previous version of which Forbes' 2000 analysis was also based on. Due to the heterogeneity of the underlying data, most authors use some sort of adjustment to make the Gini coefficients contained in the dataset more comparable (such as adding the average difference of 6.6 Gini points between the expenditure and income based Gini coefficients onto the expenditure one). We use a more sophisticated, regression-based adjustment procedure, based on Gruen and Klasen (2012).[19] Again, as shown in table 10, the results are similar to what we have obtained in our basic specifications in table 2: the positive and significant coefficient of inequality is driven by the transition countries and vanishes when the transition-year effects are introduced in the estimation (columns 3 and 4), although the coefficient on the interaction just misses significance in column 2.

Table 10: WIID (adjusted) Ginis, FE results

Dep. var.: GDP growth	(1)	(2)	(3)	(4)
Gini(t-1)	0.000688**	0,000352	0,000415	0,000322
	(-0,000339)	(-0,000353)	(-0,000345)	(-0,000328)
Gini(t-1)*trans		0,002	-0,000944	
		(-0,00129)	(-0,00106)	
Observations	562	562	562	562
R^2	0,326	0,34	0,483	0,481
# of countries	118	118	118	118
Control variables	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Trans-Year FE	**NO**	**NO**	**YES**	**YES**

Notes. Robust standard errors in parentheses, ***p<0.01, **p<0.05, *p<0.1.

Further IV robustness checks

[19]The adjustment procedure regresses the full sample of Gini coefficients on the different income definitions and reference units used in the dataset to remove the effect of the differential concepts underlying the data, which are added or subtracted from the reported Gini to achieve at a measure equivalent to that based on gross income per person. Because the resulting dataset contains duplicate observations whenever more than one income concept was available in the original data, we report another version of table 9 in the appendix (table A.10), where the duplicates where switched. The results are very similar between the two versions.

To account for the possibility that the exclusion restriction of the IV might be violated due to the differential production structure of agriculture in economies with high and low wheat-sugar ratios (e.g., wheat producing countries might rely more on machinery and hence could be more dependent on fuel as an input factor and also be hit more adversely by increases in the oil price in terms of economic growth), we include a number of control variables capturing countries' agricultural production structure: the size of land under cereal production, the use of machinery in agriculture, and total agricultural land (all from the WDI). As shown in table 11, none of these variables threaten the validity of our IV. If anything, controlling for the share of land under cereal production leads to a more precise estimate of the inequality coefficient. Note that the change in the sign of the coefficient when agricultural machinery is included is entirely attributable to the smaller sample: running the IV without the variable on the same sample (column 4) yields virtually the same coefficient estimate. We also interact the variables with our instrument to model more explicitly that the instrument might affect countries with different agricultural endowments differently. The results (presented in appendix table A.12) do not indicate that this is the case, with the coefficients on the inequality variable again remaining virtually unchanged. As another check on the robustness of the IV, we have included separate time trends for the OPEC countries to account for the fact that the effect of a higher oil price might affect inequality differently in these countries. While the idea for using the oil price as a correlate of inequality was mainly through the adverse effect of a higher oil price on the poor, it might actually affect inequality through the other end of the distribution in oil producing countries and could thereby have a differential impact on growth in these countries. Including separate time trends for the OPEC countries (results shown in appendix table A.11) does not affect the IV estimates much and merely leads to slightly lower (but still valid) F-statistics in the first stage, which is in line with the reasoning of a different transmission mechanism of the oil price on inequality in OPEC countries. We have also tested the impact of separately including a time trend for each continent. Apart from the "Europe and Central Asia" dummy (again, capturing the transition economies), none of these have a major impact on the estimates.[20]

[20] Results available upon request

Table 11: Robustness of the IV result to further control variables

Dep. var.: GDP growth	(1)	(2)	(3)	(4)
Gini(t-1)	-0,00213	0,000553	0,000572	-0.00294*
	(-0,00155)	(-0,00271)	(-0,00267)	(-0,00176)
Agricultural land	9,35E-05			
	(-0,000643)			
Agricultural machinery		1,67E-06		
		(-6,48E-06)		
Land under cereal production				-2.83e-09*
				(-1,66E-09)
Observations	563	327	327	563
# of countries	92	75	75	92
Control variables	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Transition-Year FE	NO	NO	NO	NO

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1.

6 Conclusion

In this paper, we have revisited the inequality-growth relationship using an enhanced panel data set with improved inequality data and special attention to the role of transition countries. We based our analysis on the specification of Forbes (2000), but also addressed the functional form concerns raised by Banerjee and Duflo (2003). Using the SWIID data, which provide an improved and substantially longer panel dataset, we can avoid several of the data concerns brought up by the literature, such as consistency over time and between countries, and a low within-country variation. We also take into account the unique experience of transition countries, which suffered a large negative output shock at the start of the transition period in the early 1990s from which they slowly recovered in the late 1990s and early 2000s. This was coincidental with large increases in inequality, which had been kept at low levels during the Communist rule.

Using robust dynamic panel estimation and multiple imputation estimation, we find no robust, systematic relationship between inequality and subsequent growth, neither for levels nor for changes in inequality. While higher inequality appears to be significantly associated with higher subsequent growth when Forbes' and Banerjee and Duflo's basic specifications are used, we find that this effect is entirely driven by the experience of transition countries and is not present in the remaining country sample. Once we introduce separate time effects for the transition countries, these associations disappear for this group of countries as well. These results hold for different lag structures as well as for the medium- rather than the short term, and the empirical patterns observed emerge not only in the SWIID, but also the WIID data.

Our results point to two conclusions. First, there does not appear to be a trade-off between inequality and growth. Second, because the positive impact of inequality on growth in transition countries is not robust to the inclusion of separate time effects, it appears to be driven by other events. Our findings are hence consistent with the claim that the relationship is due to the particular timing of inequality and growth dynamics in transition countries. In particular, the rise in inequality in the 1990s coincided with a sharp output collapse, leading us to find an association between the large increase in inequality in the early 1990 and a growth recovery in the late 1990s.

Results from an IV estimation confirm our interpretation of the positive association between inequality and growth found in the FE specifications as non-causal, both within as well as outside of the transition countries. Given that our instrument does not pick up the transition experience itself, we cannot, however, infer from our IV estimates that the observed positive association between inequality and growth during and after the transition is entirely spurious. However, while we may not be able to definitely rule out a causal link between the increase in inequality and the subsequent growth spell on the basis of our estimations, research on the dynamics of the transition (see, e.g., Aristei and Perugini 2012, and the references in Sukiassyan 2007) suggests that the breakdown of the Soviet regime and the economic transition to a capitalist system triggered both the increase in inequality as well as the slump and subsequent recovery in growth, rather than one causing the other.

References

Alesina, A. and Rodrik, D. (1994). Distributive politics and economic growth. *The Quarterly Journal of Economics*, 109(2):465–490.

Arellano, M. and Bond, S. (1991). Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. *The Review of Economic Studies*, 58(2):277–297.

Aristei, D. and Perugini, C. (2012). Inequality and reforms in transition countries. *Economic Systems*, 36(1):2–10.

Atkinson, A. B. and Brandolini, A. (2001). Promise and pitfalls in the use of "secondary" data-sets: Income inequality in OECD countries as a case study. *Journal of Economic Literature*, 39(3):771–799.

Atkinson, A. B. and Brandolini, A. (2009). On data: a case study of the evolution of income inequality across time and across countries. *Cambridge Journal of Economics*, 33(3):381–404.

Banerjee, A. V. and Duflo, E. (2003). Inequality and growth: What can the data say? *Journal of Economic Growth*, 8(3):267–299.

Barro, R. J. (2000). Inequality and Growth in a Panel of Countries. *Journal of Economic Growth*, 5(1):5–32.

Barro, R. J. and Lee, J. W. (2013). A new data set of educational attainment in the world, 1950–2010. *Journal of Development Economics*, 104:184–198.

Bertola, G. (1993). Factor shares and savings in endogenous growth. *The American Economic Review*, 83(5):1184–1198.

Birdsall, N. (2008). Income Distribution: Effects on Growth and Development. In Dutt, A. K. and Ros, J., editors, *International Handbook of Development Economics, Volume 2*, chapter 48. Edward Elgar Publishing, Cheltenham, UK.

Blundell, R. and Bond, S. (1998). Initial conditions and moment restrictions in dynamic panel data models. *Journal of Econometrics*, 87(1):115–143.

Chiu, W. H. and Madden, P. (1998). Burglary and income inequality. *Journal of Public Economics*, 69(1):123–141.

Clarke, G. R. G. (1995). More evidence on income distribution and growth. *Journal of Development Economics*, 47(2):403–427.

Deininger, K. and Olinto, P. (2000). Asset distribution, inequality, and growth. World Bank Policy Research Working Paper No. 2375.

Deininger, K. and Squire, L. (1996). A new data set measuring income inequality. *The World Bank Economic Review*, 10(3):565–591.

Deininger, K. and Squire, L. (1998). New ways of looking at old issues: inequality and growth. *Journal of development economics*, 57(2):259–287.

Easterly, W. (2001). The middle class consensus and economic development. *Journal of Economic Growth*, 6(4):317–335.

Easterly, W. (2007). Inequality does cause underdevelopment: Insights from a new instrument. *Journal of Development Economics*, 84(2):755–776.

Engerman, S. L. and Sokoloff, K. L. (1997). Factor endowments, institutions, and differential paths of growth among new world economies. In Haber, S., editor, *How Latin America Fell Behind*, pages 260–304. Stanford University Press, Stanford, CA.

Erhart, C. (2009). The effects of inequality on growth: a survey of the theoretical and empirical literature. *ECINEQ Working Paper No. 107.*

Essama-Nssah, B., Go, D. S., Kearney, M., Korman, V., Robinson, S., and Thierfelder, K. (2007). Economy-wide and distributional impacts of an oil price shock on the South African economy. *World Bank Policy Research Working Paper No. 4354.*

Ezcurra, R. (2007). Is income inequality harmful for regional growth? Evidence from the European Union. *Urban Studies*, 44(10):1953–1971.

Feenstra, R. C., Inklaar, R., and Timmer, M. P. (2015). The next generation of the Penn World Table. *The American Economic Review*, 105(10):3150–3182.

Ferreira, F., Lakner, C., Lugo, M. A., and Özler, B. (2014). Inequality of opportunity and economic growth: A cross-country analysis.

Foellmi, R. and Zweimüller, J. (2006). Income distribution and demand-induced innovations. *The Review of Economic Studies*, 73(4):941–960.

Forbes, K. J. (2000). A reassessment of the relationship between inequality and growth. *American Economic Review*, 90(4):869–887.

Galor, O. and Zeira, J. (1993). Income distribution and macroeconomics. *The review of economic studies*, 60(1):35–52.

Ghatak, M. and Jiang, N. N.-H. (2002). A simple model of inequality, occupational choice, and development. *Journal of Development Economics*, 69(1):205–226.

Glaeser, E., Scheinkman, J., and Shleifer, A. (2003). The injustice of inequality. *Journal of Monetary Economics*, 50(1):199–222.

Gruen, C. and Klasen, S. (2008). Growth, inequality, and welfare: comparisons across space and time. *Oxford Economic Papers*, 60(2):212–236.

Gruen, C. and Klasen, S. (2012). Has transition improved well-being ? *Economic Systems*, 36(1):11–30.

Ivaschenko, O. (2003). Growth and inequality: Evidence from transitional economies in the 1990s. In Eicher, T. and Turnovsky, S., editors, *Inequality and Growth: Theory and Policy Implications*, pages 155–198. MIT Press, Cambride, MA.

Jenkins, S. P. (2015). World income inequality databases: an assessment of WIID and SWIID. *The Journal of Economic Inequality*, 13(4):629–671.

Josten, S. D. (2003). Inequality, crime and economic growth. a classical argument for distributional equality. *International Tax and Public Finance*, 10(4):435–452.

Kaldor, N. (1955). Alternative theories of distribution. *The Review of Economic Studies*, 23(2):83–100.

Keefer, P. and Knack, S. (2002). Polarization, politics and property rights: Links between inequality and growth. *Public Choice*, 111(1-2):127–154.

Klasen, S. and Gruen, C. (2001). Growth, income distribution and well-being in transition countries. *Economics of Transition*, 9(2).

Knowles, S. (2005). Inequality and economic growth: the empirical relationship reconsidered in the light of comparable data. *The Journal of Development Studies*, 41(1):135–159.

Kremer, M. and Chen, D. (1999). Income-distribution dynamics with endogenous fertility. *The American Economic Review*, 89(2):155–160.

Murphy, K. M., Shleifer, A., Vishny, R., et al. (1989). Income distribution, market size, and industrialization. *The Quarterly Journal of Economics*, 104(3):537–564.

Naranpanawa, A. and Bandara, J. S. (2012). Poverty and growth impacts of high oil prices: Evidence from Sri Lanka. *Energy Policy*, 45:102–111.

Neves, P. C. and Silva, S. M. T. (2014). Inequality and growth: Uncovering the main conclusions from the empirics. *Journal of Development Studies*, 50(1):1–21.

Nickell, S. (1981). Biases in dynamic models with fixed effects. *Econometrica*, 49(6):1417–1426.

Nunn, N. and Qian, N. (2014). Us food aid and civil conflict. *The American Economic Review*, 104(6):1630–1666.

Osberg, L., Smeeding, T. M., and Schwabish, J. (2004). Income distribution and public social expenditure: Theories, effects, and evidence. *Social Inequality*, pages 821–59.

Perotti, R. (1993). Political equilibrium, income distribution, and growth. *The Review of Economic Studies*, 60(4):755–776.

Perotti, R. (1996). Growth, income distribution, and democracy: what the data say. *Journal of Economic Growth*, 1(2):149–187.

Persson, T. and Tabellini, G. (1994). Is inequality harmful for growth? *The American Economic Review*, 84(3):600–621.

Roodman, D. (2009). A note on the theme of too many instruments. *Oxford Bulletin of Economics and Statistics*, 71(1):135–158.

Rubin, D. B. (2004). *Multiple imputation for nonresponse in surveys*. John Wiley & Sons, New York.

Solt, F. (2016). The standardized world income inequality database. *Social Science Quarterly*, pages 1–15.

Sukiassyan, G. (2007). Inequality and growth: What does the transition economy data say? *Journal of Comparative Economics*, 35(1):35–56.

UNU-WIDER (2015). *World Income Inequality Database (WIID3.3)*.

Voitchovsky, S. (2009). Inequality and economic growth. In Nolan, B., Salverda, W., and Smeeding, T. M., editors, *The Oxford Handbook of Economic Inequality*. Oxford University Press, Oxford.

Windmeijer, F. (2005). A finite sample correction for the variance of linear efficient two-step GMM estimators. *Journal of Econometrics*, 126(1):25–51.

A Appendix

Figure A.1: Correlation between growth and (lagged) inequality in transition countries

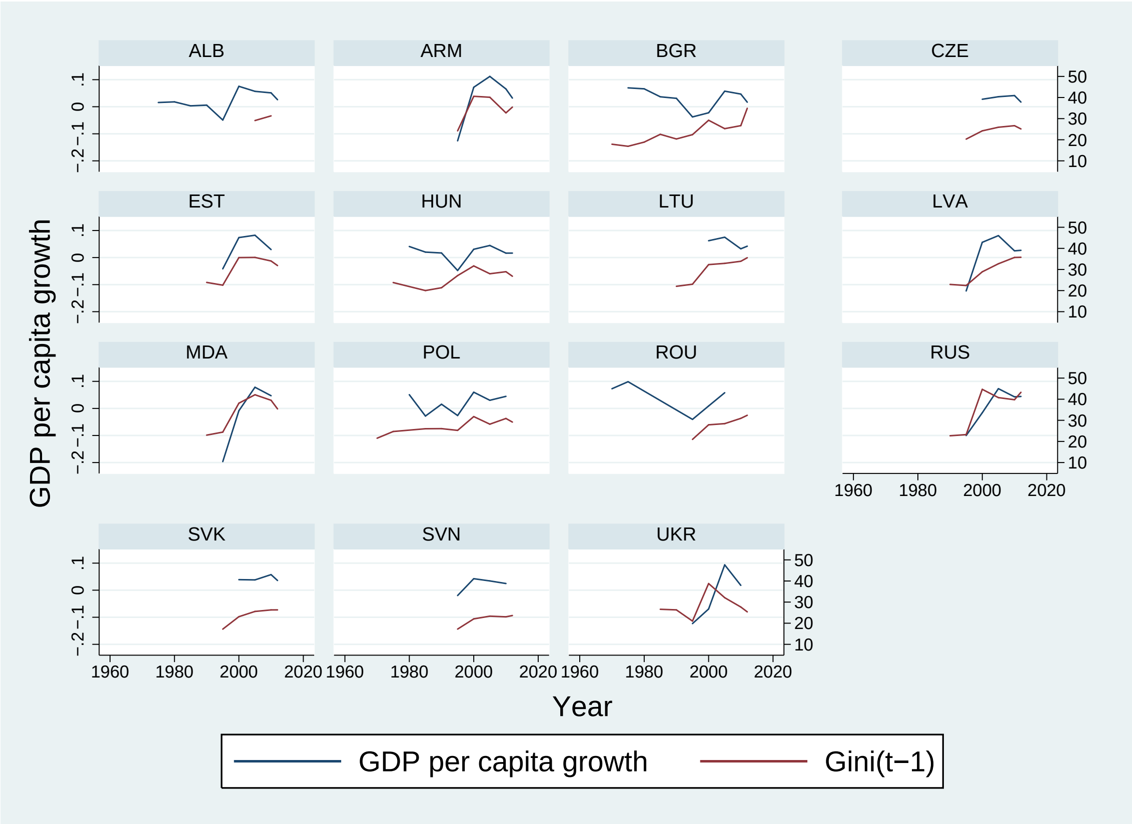


Table A.1: Reproduction of table 2 using the IV sample

Dep. var.: GDP growth	(1) levels	(2) levels with transition countries	(3) levels with transition countries & separate transition-year effects	(4) levels with transition-year effects
Gini(t-1)	0.000802**	0.000318	0.000331	0.000379
	(-0.000327)	(-0.000259)	(-0.000252)	(-0.000249)
Transition*Gini(t-1)		0.00463***	0.00112	
		(-0.00175)	(-0.00168)	
GDP(t-1)	-0.0537***	-0.0464***	-0.0404***	-0.0412***
	(-0.0107)	(-0.00945)	(-0.00847)	(-0.00841)
PI(t-1)	-0.00663	-0.00579	-0.00711	-0.00717
	(-0.0049)	(-0.00499)	(-0.00546)	(-0.00547)
Schooling_m(t-1)	0.00342	0.00697	0.0012	0.00111
	(-0.00763)	(-0.00799)	(-0.00621)	(-0.00625)
Schooling_f(t-1)	-0.0027	-0.00857	-0.00479	-0.00456
	(-0.00822)	(-0.00847)	(-0.00762)	(-0.00761)
Constant	0.429***	0.376***	0.346***	0.353***
	(-0.0808)	(-0.0746)	(-0.0658)	(-0.0648)
Observations	587	587	587	587
# of countries	92	92	92	92
Year FE	YES	YES	YES	YES
Transition-Year FE	NO	NO	YES	YES

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: GMM results, level specification

Dep. var.: GDP growth	(1) restricted	(2) collapsed	(3) col. & res.	(4) ort. & res.	(5) ort. & col.
Gini(t-1)	**0.00163**	**0.00161***	**0.00178**	**0.00169***	**0.00214**
	(-0.00158)	(-0.000957)	(-0.00504)	(-0.000901)	(-0.00141)
PI(t-1)	-0.036	-0.028	-0.0459	-0.0158	-0.0505**
	(-0.0262)	(-0.0181)	(-0.0798)	(-0.0174)	(-0.0243)
GDP(t-1)	-0.131***	-0.111***	-0.208***	-0.0928***	-0.127***
	(-0.0237)	(-0.021)	(-0.0447)	(-0.0181)	(-0.0192)
Schooling_m(t-1)	-0.0308	-0.0402	0.0133	-0.00455	-0.0282
	(-0.03)	(-0.0288)	(-0.123)	(-0.0203)	(-0.0305)
Schooling_f(t-1)	0.0257	0.0392*	0.0334	0.0202	0.04
	(-0.0306)	(-0.0207)	(-0.0777)	(-0.0198)	(-0.0337)
# of instruments	74	44	19	74	44
AR1	0.0198631	0.13865757	0.8283414	0.00442597	0.0572844
AR2	0.65305763	0.71841735	0.8001032	0.64667875	0.650463
Hansen test	0.05399719	0.00620502	0.3936974	0.1044502	0.0204975
% misspecified	100	100	100	49	100
Observations	566	566	566	590	590
# of countries	115	115	115	116	116
Year FE	YES	YES	YES	YES	YES

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1. The top row indicates the type of instrument restriction (res=lags restricted to 3&4, col=collapsed, ort=orthogonalized).

Table A.3: System GMM results of the basic specifications

Dep. var.: GDP growth	(1)	(2)	(3)	(4)	(5)	(6)
Corresponding GMM specification:	Table 2, column 3	Table 2, column 3	Table 2, column 3	Table 3, column 5	Table 3, column 5	Table 3, column 5
Instrument restriction:	res.	ort. & col.	ort. & res	restricted	ort. & col.	ort. & res.
Gini(t-1)	0.000449	0.0007	0.000429	-0.000387	-0.000374	-0.000303
	(0.000391)	(0.000828)	(0.000331)	(0.000309)	(0.000425)	(0.000436)
Transition*Gini(t-1)				0.00241***	0.00181*	0.00191
				(0.000686)	(0.00110)	(0.00132)
Δ Gini(t-1)	9.66E-05	0.000121	4.03E-05	0.000486	-0.000558	-0.000674
	(0.000274)	(0.000418)	(0.000220)	(0.000469)	(0.000486)	(0.000565)
Transition* Δ Gini(t-1)				-0.00154	-0.000125	0.000449
				(0.00158)	(0.00198)	(0.00260)
GDP(t-1)	0.00423	0.00423	0.00228	0.00184	0.00215	0.0072
	(0.00306)	(0.00306)	(0.00215)	(0.00354)	(0.00522)	(0.00637)
PI(t-1)	-0.0112	-0.0112	-0.0102	-0.0156*	-0.0154	-0.0209
	(0.00736)	(0.00736)	(0.00709)	(0.00902)	(0.0132)	(0.0155)
Schooling_m(t-1)	0.000969	0.000969	0.00322	-0.00922	-0.00801	-0.00904
	(0.00664)	(0.00664)	(0.00570)	(0.00705)	(0.00779)	(0.00594)
Schooling_f(t-1)	-0.00182	-0.00182	-0.00267	0.0059	0.00599	0.00422
	(0.00680)	(0.00680)	(0.00537)	(0.00676)	(0.00776)	(0.00673)
Constant	-0.0122	-0.0122	0.00145	0.0606*	0.0486	0.0135
	(0.0350)	(0.0350)	(0.0292)	(0.0353)	(0.0482)	(0.0549)
Observations	712	712	712	577	577	577
# of countries	122	122	122	115	115	115
Year FE	YES	YES	YES	YES	YES	YES
# of instruments				113	89	59
Hansen Test				0.896	0.592	0.943
AR(1)				0.0113	0.0128	0.0169
AR(2)				0.173	0.164	0.476

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1. Instrument restrictions are: res=lags restricted to 3 & 4, col=collapsed, ort=orthogonalized. For the specifications in columns 1-3, the misspecfication tests have not been computed because none of the variables of interest are significant. Results for further alternative instrument restrictions are similar (available upon request). Also note that the results in columns 4-6 do not rely on multiple imputation estimation due to problems with varying omitted terms in the interactions. Instead, the data are averaged across the 100 imputations before instead of after the estimation. While this may affect the resulting point estimate of the coefficient, it is highly unlikely that it is qualitatively different. The "true" standard errors are also smaller, which does not, however, challenge the finding that this effect - significant or not - vanishes once separate transition-year effects are added to the model.

Table A.4: GMM results, quadratic level specification

Dep. var.: GDP growth	(1) res	(2) col	(3) colres	(4) ortres	(5) ortcol	(6) ortcolres
Gini(t-1)	0.000389	-0.00695	0.00943	0.000494	-0.00501	0.00859
	(0.00483)	(0.00717)	(0.0275)	(0.00372)	(0.00686)	(0.0226)
Gini(t-1)^2	-3.92E-06	7.09E-05	-8.83E-05	-3.17E-06	5.43E-05	-5.97E-05
	(5.09e-05)	(7.79e-05)	(0.000289)	(4.25e-05)	(7.67e-05)	(0.000252)
Transition*Gini(t-1)	0.00785	0.0281	0.0291	0.00985	0.0195	0.0265
	(0.0170)	(0.0196)	(0.0452)	(0.0179)	(0.0237)	(0.0373)
Transition*Gini(t-1)^2	-6.49E-05	-0.000308	-0.000473	-8.32E-05	-0.000185	-0.000522
	(0.000268)	(0.000304)	(0.000603)	(0.000289)	(0.000371)	(0.000575)
GDP(t-1)	-0.0922***	-0.0760***	-0.187**	-0.0770***	-0.0855***	-0.188**
	(0.0199)	(0.0208)	(0.0763)	(0.0169)	(0.0200)	(0.0841)
PI(t-1)	-0.0117	-0.0138	-0.0112	-0.00691	-0.0134	-0.0527
	(0.0177)	(0.0174)	(0.0609)	(0.0114)	(0.0196)	(0.0566)
Schooling_m(t-1)	-0.0131	-0.024	0.0594	0.00379	-0.0174	-0.00192
	(0.0320)	(0.0275)	(0.0876)	(0.0187)	(0.0253)	(0.0688)
Schooling_f(t-1)	0.0178	0.0138	-0.00509	0.00335	0.0161	0.0297
	(0.0242)	(0.0267)	(0.0544)	(0.0168)	(0.0256)	(0.0482)
F-test of quadratic terms (p-value)	0.4456	0.1237	0.7727	0.2439	0.2759	0.673
Observations	566	566	566	590	590	590
# of countries	115	115	115	116	116	116
Year FE	YES	YES	YES	YES	YES	YES

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1. The second row indicates the type of instrument restriction imposed on the GMM (res=lags restricted to 3&4, col=collapsed, ort=orthogonalized).

Table A.5: Basic specification in differences

Dep. var.: GDP growth	(1) differences	(2) differences with transition country dummies
Δ Gini(t-1)	-0.000206	-0.000111
	(-0.000214)	(-0.000206)
Transition*Δ Gini(t-1)		-0.00069
		(-0.000836)
GDP(t-1)	-0.0635***	-0.0639***
	(-0.0106)	(-0.0106)
PI(t-1)	-0.0140**	-0.0141**
	(-0.00625)	(-0.00635)
Schooling_m(t-1)	-0.00774	-0.00756
	(-0.0105)	(-0.0104)
Schooling_f(t-1)	0.013	0.0127
	(-0.0115)	(-0.0115)
Constant	0.553***	0.556***
	(-0.0866)	(-0.087)
Observations	577	577
# of countries	115	115
Year FE	YES	YES

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Splines with level Gini, FE results

	(1)	(2)	(3)	(4)
	"change" definition brackets (in percent):			
Dep. var.: GDP growth	3	5	10	20
GDP(t-1)	-0.0505***	-0.0505***	-0.0501***	-0.0495***
	(0.00958)	(0.00967)	(0.00977)	(0.00986)
PI(t-1)	-0.00805	-0.00812	-0.00806	-0.00775
	(0.00897)	(0.00901)	(0.00911)	(0.00878)
Schooling_m(t-1)	-0.00332	-0.00297	-0.00274	-0.00226
	(0.00851)	(0.00850)	(0.00844)	(0.00834)
Schooling_f(t-1)	0.00635	0.00608	0.00575	0.00514
	(0.00945)	(0.00951)	(0.00954)	(0.00943)
Gini_net(t-1)	**-0.000158**	**-0.000137**	**-8.68E-05**	**-2.66E-05**
	(0.000322)	(0.000321)	(0.000320)	(0.000330)
Negative change	**-0.000278**	**-0.000171**	**1.05E-05**	**0.000495**
	(0.000302)	(0.000343)	(0.000469)	(0.000938)
No change	**0.00100***	**0.000358**	**1.59E-05**	**-0.000132**
	(0.000508)	(0.000340)	(0.000211)	(0.000158)
Positive change	**-0.00103*****	**-0.00105*****	**-0.00117*****	**-0.00142*****
	(0.000227)	(0.000238)	(0.000264)	(0.000326)
Constant	0.436***	0.436***	0.432***	0.436***
	(0.0736)	(0.0745)	(0.0762)	(0.0808)
Observations	614	614	614	614
# of countries	115	115	115	115
Year FE	YES	YES	YES	YES

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1. Numbers in the third row represent the knots for defining the "no change"-bracket, i.e., changes between +/-3(5, 10, 20) percent are coded as "no change", and changes above (below) as increases (decreases).

Table A.7: Splines, FE results with sample excluding transition countries

	(1)	(2)	(3)	(4)
	"change" definition brackets (in percent):			
Dep. var.: GDP growth	3	5	10	20
GDP(t-1)	-0.0466***	-0.0468***	-0.0470***	-0.0470***
	(0.0104)	(0.0104)	(0.0105)	(0.0104)
PI(t-1)	-0.00893	-0.00903	-0.00913	-0.00908
	(0.00964)	(0.00959)	(0.00959)	(0.00961)
Schooling_m(t-1)	-0.00948	-0.00979	-0.0101	-0.0102
	(0.00802)	(0.00801)	(0.00806)	(0.00785)
Schooling_f(t-1)	0.0093	0.00974	0.0101	0.0103
	(0.0105)	(0.0105)	(0.0106)	(0.0103)
Negative change	-0.000341	-0.000238	-0.000121	0.000238
	(0.000272)	(0.000313)	(0.000433)	(0.000806)
No change	0.000154	-0.000134	-0.0002	-0.000188
	(0.000452)	(0.000306)	(0.000188)	(0.000134)
Positive change	-0.000154	-0.000129	-0.000119	-0.00017
	(0.000219)	(0.000237)	(0.000291)	(0.000410)
Constant	0.399***	0.402***	0.405***	0.413***
	(0.0842)	(0.0848)	(0.0864)	(0.0888)
Observations	549	549	549	549
# of countries	100	100	100	100
Year FE	YES	YES	YES	YES

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1

Table A.8: 10-year averages, levels and differences

Dep. var.: GDP growth	(1) levels & differences	(2) levels & differences with transition countries	(3) levels & differences with transition-countries & trans.-year effects	(4) levels & differences with transition-year effects
Gini(t-1)	0.000309	0.000369	0.000336	0.000306
	(0.000422)	(0.000471)	(0.000472)	(0.000461)
Transition*Gini(t-1)		0.000224	-0.00335	
		(0.00194)	(0.00258)	
ΔGini(t-1)	-2.26E-05	-0.000179	-0.000152	-0.000102
	(0.000253)	(0.000294)	(0.000302)	(0.000282)
Transition*ΔGini(t-1)		0.000702	0.00186	
		(0.000679)	(0.00119)	
GDP(t-1)	-0.0181**	-0.0191**	-0.0153*	-0.0145*
	(0.00891)	(0.00887)	(0.00901)	(0.00859)
PI(t-1)	-0.0397***	-0.0391***	-0.0361**	-0.0358**
	(0.0144)	(0.0145)	(0.0150)	(0.0149)
Schooling_m(t-1)	-0.00511	-0.00496	-0.00602	-0.00568
	(0.00786)	(0.00886)	(0.00927)	(0.00912)
Schooling_f(t-1)	0.003	0.00345	0.00452	0.00399
	(0.00875)	(0.00978)	(0.0103)	(0.0100)
Constant	0.185**	0.189**	0.160**	0.147**
	(0.0756)	(0.0751)	(0.0755)	(0.0726)
Observations	183	183	183	183
# of countries	91	91	91	91
Year FE	YES	YES	YES	YES
Trans-Year FE	NO	NO	YES	YES

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: Two-year lag FE and alternative GMM specifications

Dep. var.: GDP growth	(1) FE lag2	(2) diffGMM coll	(3) diffGMM coll	(4) sysGMM res	(5) sysGMM coll	(6) sysGMM coll
Gini(t-1)	0.000686**	1.69E-05	0.000892	0.000102	-0.000485	-0.000171
	(0.000267)	(0.000785)	(0.000785)	(0.000482)	(0.000474)	(0.000346)
GDP(t-1)	-0.0489***	-0.1000***	-0.0633***	0.00777	-0.00127	-0.0017
	(0.00998)	(0.0159)	(0.0147)	(0.00547)	(0.00445)	(0.00344)
PI(t-1)	-0.0142**	-0.0344***	-0.0306**	-0.0181	-0.00928	-0.00663
	(0.00565)	(0.0126)	(0.0123)	(0.0136)	(0.00819)	(0.00772)
Schooling_m(t-1)	-0.00597	-0.0535**	-0.0166	-0.0108	-0.00672	-0.00246
	(0.00849)	(0.0225)	(0.0195)	(0.0110)	(0.00726)	(0.00971)
Schooling_f(t-1)	0.00737	0.0410*	0.015	0.00824	0.00663	0.00339
	(0.0108)	(0.0214)	(0.0222)	(0.0109)	(0.00728)	(0.00942)
Constant	0.412***			-0.0106	0.075	0.0602*
	(0.0788)			(0.0503)	(0.0503)	(0.0337)
Observations	625	481	506	625	625	625
R^2	0.438					
# of countries	119	113	114	119	119	119
Year FE	YES	YES	YES	YES	YES	YES
Trans-Year FE	YES	YES	YES	YES	YES	YES
# instruments		89	97	69	103	111
Hansen Test		0.417	0.136	0.145	0.147	0.223
Sargan Test		1.27E-05	1.39E-06	0	0	0
AR(1)		0.0207	0.00492	0.014	0.00845	0.00751
AR(2)		0.925	0.25	0.201	0.324	0.292

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: WIID Ginis (adjusted), Version 2, FE results

Dep. var.: GDP growth	(1)	(2)	(3)	(4)
Gini(t-1)	0.000813**	0.000456	0.0005	0.000368
	(0.000370)	(0.000399)	(0.000386)	(0.000365)
Transition*Gini(t-1)		0.00197	-0.00126	
		(0.00126)	(0.00105)	
GDP(t-1)	-0.0512***	-0.0500***	-0.0410***	-0.0411***
	(0.0103)	(0.0104)	(0.0109)	(0.0110)
PI(t-1)	-0.00801	-0.00783	-0.00952	-0.00951
	(0.00578)	(0.00604)	(0.00730)	(0.00737)
Schooling_m(t-1)	0.00704	0.00874	0.00333	0.00369
	(0.0109)	(0.0116)	(0.00989)	(0.01000)
Schooling_f(t-1)	0.00144	-0.000856	0.000225	-2.30E-06
	(0.0112)	(0.0118)	(0.0116)	(0.0117)
Constant	0.399***	0.398***	0.342***	0.345***
	(0.0846)	(0.0865)	(0.0873)	(0.0878)
Observations	562	562	562	562
R^2	0.33	0.344	0.485	0.481
# of countries	118	118	118	118
Year FE	YES	YES	YES	YES
Trans-Year FE	NO	NO	YES	YES

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1.

Table A.11: IV with OPEC-year effects

Dep. var.: GDP growth	(1) cereal production	(2) agr. land	(3) agr. machinery
Gini(t-1)	-0.00344	-0.00502	0.00135
	(-0.00279)	(-0.00371)	(-0.00334)
GDP(t-1)	-0.0438***	-0.0402***	-0.0683***
	(-0.0137)	(-0.0151)	(-0.0125)
PI(t-1)	-0.00511	-0.00351	-0.0260**
	(-0.00589)	(-0.00662)	(-0.0124)
Schooling_m(t-1)	-0.00869	-0.0129	0.022
	(-0.0118)	(-0.0138)	(-0.02)
Schooling_f(t-1)	0.00465	0.00587	-0.0122
	(-0.0106)	(-0.0119)	(-0.0187)
Cereal production*instr	3.59E-09		
	(2.05E-09)		
Cereal production	-2.35E-09		
	(-1.70E-09)		
Agr. land *instr		8.85E-06	
		(-7.74E-06)	
Agr. land		-3.81E-05	
		(-0.000798)	
Agr. machinery*instr			-2.17E-07
			(-1.89E-07)
Agr. machinery			3.04E-06
			(-4.24E-06)
Observations	584	584	346
# of countries	92	92	75
Year FE	YES	YES	YES
Transition-Year FE	NO	NO	NO

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: IV with transition countries

Dep. var.: GDP growth	(1) basic	(2) trans-year	(3) transinteract	(4) transinteract & trans-year
Gini(t-1)	-0.00222	-0.000594	-0.000558	-0.000886
	(-0.00152)	(-0.00148)	(-0.00176)	(-0.00162)
Transition*Gini(t-1)			-0.0073	0.00417
			(-0.00593)	(-0.0035)
GDP(t-1)	-0.0489***	-0.0413***	-0.0652***	-0.0378***
	(-0.0127)	(-0.0102)	(-0.0188)	(-0.0115)
PI(t-1)	-0.00363	-0.00588	-0.00578	-0.00548
	(-0.00603)	(-0.00567)	(-0.00579)	(-0.00584)
Schooling_m(t-1)	-0.00604	-0.00237	-0.0106	-0.00222
	(-0.00808)	(-0.00614)	(-0.00972)	(-0.00621)
Schooling_f(t-1)	0.00469	-0.00137	0.014	-0.00221
	(-0.00842)	(-0.00661)	(-0.0116)	(-0.00684)
Observations	566	566	566	566
# of countries	92	92	92	92
Year FE	YES	YES	YES	YES
Transition-Year FE	NO	YES	NO	YES
OPEC-Year FE	YES	YES	YES	YES
FIRST STAGE				
GDP(t-1)	3.196**	4.286***	3.165**	4.331***
	(-1.385)	(-1.323)	(-1.392)	(-1.324)
PI(t-1)	0.7	0.752	0.71	0.783
	(-0.705)	(-0.69)	(-0.706)	(-0.692)
Schooling_m(t-1)	-2.258*	-1.603	-2.207	-1.594
	(-1.366)	(-1.376)	(-1.347)	(-1.377)
Schooling_f(t-1)	0.962	-0.0476	0.922	-0.0733
	(-1.537)	(-1.544)	(-1.521)	(-1.546)
SWratio*instr(t-2)	0.294***	0.289***	0.281***	0.278***
	(-0.0856)	(-0.0931)	(-0.0948)	(-0.0951)
Transition*instr(t-2)			0.0517	0.369
			(-0.154)	(-0.277)
Observations	566	566	566	566
R^2	0.16	0.228	0.16	0.228
# of countries	92	92	92	92
Weak instruments: F-stat	11.8	9.64		
Kleibergen-Paap max. bias			15%	15%

Notes. Standard errors in parentheses as indicated in the top column; *** p<0.01, ** p<0.05, * p<0.1.