Re-estimating the Relationship between Inequality and Growth

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Summary

In this paper, we 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 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 (post-Soviet) 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 dynamics of the transition. Our finding is consistent with the claim that the relationship between inequality and growth emerges 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. 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.

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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 (DS 1996) 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 lower growth.

Subsequent debates focused on the one hand on weaknesses in the data, where Atkinson and Brandolini (2000) showed that the comparability and consistency of the DS 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 that dataset where some authors used regression-based adjustment methods to address inconsistency issues (e.g., Gruen and Klasen 2008, 2012). More recently, Solt (2014) 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 2014). We will rely on these data in our analysis, but also show the robustness of our results to the WIID data.

A second focus of the debate was the empirical specification of the inequality-growth relationship. In particular, Forbes (2000) moved from a cross-section 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 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 1992; 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 also 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. Secondly, there was a 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 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 slowly 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 Ivashenko (2001) and Gruen and Klasen (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 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. Lastly, we find no evidence that the Banerjee and Duflo (2003) specification is superior and see no symmetries in the relationship between increases and decreases in inequality.

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, the finding for transition countries could 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 were detrimental also there. Lastly, 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 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. Given that this relationship disappears once separate time effects are introduced for transition countries, it appears that this association is not causal but rather driven by the particular dynamics of transition countries.

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 go into detail in the theoretical literature, but rather give a broad overview of different types of arguments redirect 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 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, and hence foregone economic growth (e.g., Birdsall 2006, Ghatak and Jiang 2002). Other arguments relating to the bottom end of the income distribution pertain to vicious cycles in economics of crime (e.g., Chiu and Madden 1998, Josten 2003) and fertility (e.g., Kremer and Chen 2002).

A second group of arguments, focusing on the size and the 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, Sleifer and Vishny 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. An overview of earlier literature on inequality and public spending can be found in Osberg, Smeeding and Swabish (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 (1956). 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 the economy in their favor (see e.g. Glaeser Scheinkman and Shleifer 2002).

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 social political polarization of society (see e.g. Keefer and Knack 2002, Easterly 2001).

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. In another cross-sectional analysis, Knowles (2005) argues that most evidence on the growth and inequality relationship 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 no remaining relationship between income inequality and growth, but that inequality in expenditure is still negatively correlated with growth.

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. On another note, 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, 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 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, and – in line with later cross-sectional results from an IV focusing on initial land distribution and income levels (Easterly 2007) – 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. This finding of course casts doubt on the validity of using land inequality as a proxy for income inequality, which seems to operate through a different channel altogether.

Ezcurra (2007) looks at regional growth across the European Union and concludes that higher inequality is associated with lower growth, thereby contradicting Barro’s (2000) result that inequality is positively related to growth in rich countries.

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). Unless indicated otherwise, estimations are using 5-year averages of growth as the dependent variable and the lagged beginning-of-period Gini as the variable of interest. That is, the first time period is 1961-1965 and the last one is 2011-2012,[2] yielding a total of 12 time periods. Except for the GDP data,[3] which is taken from the Penn World Tables (PWT), Version 8.0,[4] all control variables are as in Forbes (2000): the price level of investment (also taken from the PWT) is included as a proxy for market distortions, and the average years of secondary schooling for the population aged over 25 (taken from the Barro and Lee database, Version 2.0) is included separately for males and females.

Our main measure of inequality, the Gini coefficient of net income, is taken from the Standardized World Income Inequality Database (SWIID) (Solt 2014). 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) 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. All 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 1987). 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 Klasen and Gruen (2012) and includes 22 post-Communist countries, of which the following 15 are part of our sample: Albania, Armenia, Bulgaria, Czech Republic,

[2] 2011-12 is the only period with less than five years. More recent data was not available at the time of writing.
[3] Forbes used Gross National Income data from the WDI
[4] 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.

Estonia, Hungary, Kyrgyz Republic, Latvia, Lithuania, Poland, Romania, Russia, Slovak Republic, Slovenia, and Ukraine.

As one can see from Table 1, 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

Note: Descriptive statistics are based on the estimation sample of countries.

	mean	**sd**	**min**	**max**
Total sample (712 obs.)				
Gini	38.09	10.52	15.80	75.71
GDP per capita growth	0.023	0.032	-0.199	0.112
Price level of investment[5]	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_F	2.22	1.56	0.02	6.89
Schooling_M	2.60	1.52	0.15	7.25
Only transition countries (71 obs.)				
Gini	30.51	6.01	18.87	44.70
GDP per capita growth	0.020	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_F	3.68	1.15	0.99	6.47
Schooling_M	3.89	1.03	1.46	6.62
Sample without transition countries (641 obs.)				
Gini	38.98	10.59	15.80	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	11842.0	272.8	76523.6
Schooling_F	2.07	1.52	0.02	6.89
Schooling_M	2.46	1.50	0.15	7.25

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

All estimations employ 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 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,[6] 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.[7] 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 2002) is sometimes suggested in the literature because the use of the level equation implies that the estimator is less prone to measurement error. However, using the System GMM estimator yields less clear results, and, more importantly the misspecification tests indicate problems in all but a few cases. System GMM is therefore employed as a robustness check, but the preferred estimator is a (two-step)

[6] The within-country variation of net income inequality has increased from 14 to 18% of the overall variation. 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.

[7] 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 as well. Our results do not depend on the type of instrument restriction and we report the ones which perform best on the share of misspecified regressions in as described below.

difference GMM. Standard errors are robust in all estimations as per Windmeijer's (2005) correction procedure.

4. Results and Discussion

Table 2: Baseline specifications in levels

VARIABLES	(1) levels, multiple imputation estimation	(2) levels with transition country interaction	(3) levels with transition country interaction, GMM	(4) levels with transition country interaction & separate year dummies	(5) levels with separate transition country year dummies,
L.Gini	**0.000472***	**0.000140**	**-0.000103**	**0.000149**	**0.000169**
	(0.000269)	(0.000234)	(0.000612)	(0.000228)	(0.000222)
Transition*L.Gini		**0.00400*****	**0.00653****	**0.000565**	
		(0.00150)	(0.00282)	(0.00129)	
L.GDP	-0.0513***	-0.0469***	-0.0652***	-0.0420***	-0.0423***
	(0.00895)	(0.00897)	(0.0139)	(0.00890)	(0.00881)
L.PI	-0.00834	-0.00766	-0.00322	-0.00902	-0.00906
	(0.00515)	(0.00527)	(0.0104)	(0.00579)	(0.00579)
L.Schooling_m	2.03e-05	0.00260	0.00107	-0.00205	-0.00207
	(0.00852)	(0.00894)	(0.0175)	(0.00775)	(0.00775)
L.Schooling_f	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)
Number of instruments			74		
AR1			0.0013559		
AR2			0.4196453		
Hansen test			0.1833639		
% misspecified			0		
Observations	712	712	590	712	712
Number of groups	122	122	116	122	122
Transition-Year FE	NO	NO	NO	YES	YES
Year FE	YES	YES	YES	YES	YES

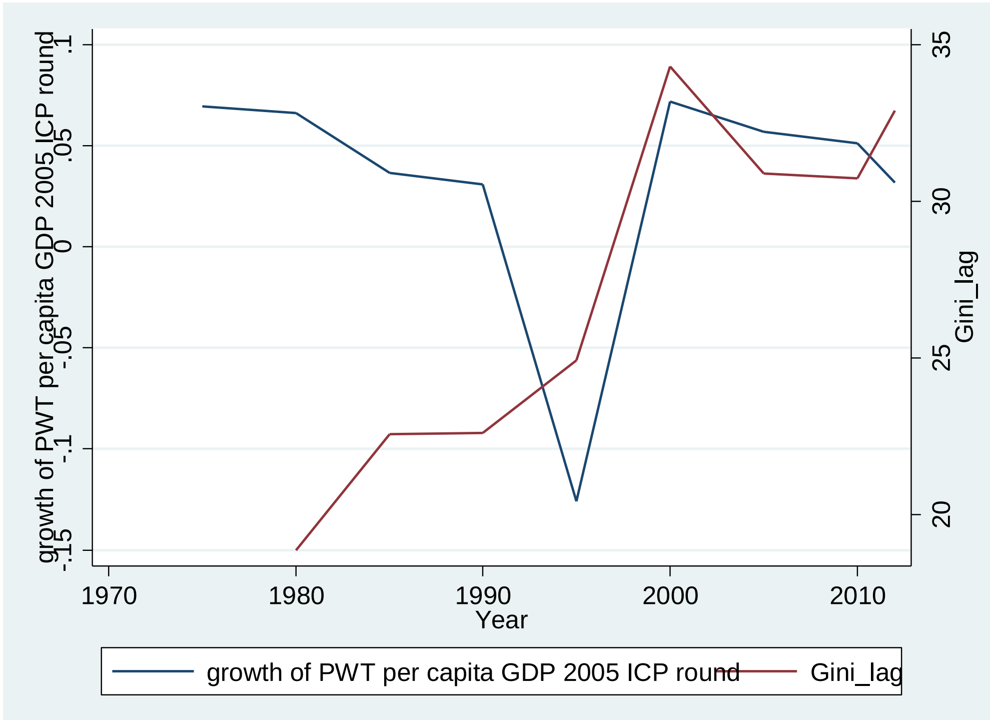
*Notes. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Instruments in the GMM estimator (column 3) are orthogonalized and restricted to lags 3 and 4.*

Table 1 displays the first set of basic 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 like in her original analysis, this effect does not hold with a non-biased GMM estimator. Appendix table A1 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 in transition countries 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 2 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 inequality between 1995 and 2000, raising concern that the 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 might not show the same correlation as depicted here. Indeed, when consulting the individual correlations in each country (as shown in Appendix Figure 1), the picture is less clear. An outlier analysis[8] does not yield any clear results pertaining to the issue, either – no single

[8] Added variable plots, (partial) leverage plots as well as values for Cook's d are available upon request.

country-year observations is driving the positive impact of inequality on growth in the transition countries.

Figure 1: Correlation between growth and (lagged) inequality in 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 year dummies are introduced, the positive impact of inequality on growth disappears also for the group of transition countries, and remains very small and insignificant for the remaining sample (column 4 of table 2). 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 positive effect of inequality on growth. In sum, we cannot confirm that higher inequality enhances economic growth, at least not in terms of higher levels – as opposed to increases or decreases – of inequality.

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.[9] 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.[10] We then introduce both levels and differences simultaneously as shown in table 3.

Table 3: Baseline specification, augmented with differences

VARIABLES	(1) levels and differences, FE	(2) levels & diff. with transition country interaction, FE	(3) levels & diff. with transition countries & transition country year dummies, FE	(4) levels& diff. with transition country year dummies, FE	(5) levels& diff. with transition country year dummies, difference GMM
L.Gini	**0.000947****	**0.000518**	**0.000532**	**0.000577***	0.000377
	(0.000384)	(0.000338)	(0.000326)	(0.000316)	(0.000440)
Transition*L.Gini		**0.00426*****	**0.00145**		
		(0.00155)	(0.00118)		
ΔL.Gini	**-0.000653****	**-0.000370**	**-0.000365**	**-0.000442***	-0.000204
	(0.000288)	(0.000262)	(0.000257)	(0.000256)	(0.000430)
Transition*ΔL.Gini		**-0.00236****	**-0.00143**		
		(0.000919)	(0.00119)		
No. of instruments					97
AR1					0.0057836
AR2					0.225435
Hansen test					0.3473344
% misspecified					0
Observations	577	577	577	577	577
Number of groups	115	115	115	115	115
Control variables	YES	YES	YES	YES	YES
Transition-Year FE	NO	NO	NO	YES	YES
Year FE	YES	YES	YES	YES	YES

*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.*

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 driven by the transition countries. Like in the previous set of regressions, we proceed to introduce transition-year effects. The inclusion of the transition-year effects again eradicates the significance of the coefficients on the transition country-inequality interactions

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

[10] Results available upon request.

both for levels and differences. 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. Overall, it seems that once the effect of changes in inequality is accounted for 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 coefficient in the overall sample and, if no separate time effects are introduced, may lead to misleading interpretations of the inequality-growth relationship in these countries. One should keep in mind that compared to non-transition countries, inequality in transition countries is still rather low – the maximum inequality value among the transition countries is still only around half a standard deviation above the mean inequality value in the sample of non-transition countries. Our reading of this result is that higher inequality levels in transition countries might therefore rather reflect "normal" inequality levels, and inequality levels in the low-inequality transition countries could reflect the fact that inequality was kept at "unnaturally" low levels due to the income compression during the socialist system.

5. Robustness Checks

As a first robustness check, 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 4 displays the results.

Table 4: Quadratic FE specifications in differences

VARIABLES	(1) levels	(2) levels with transition country interaction	(3) differences	(4) differences with transition country interaction
L.Gini	0.00205	-0.000164		
	(0.00156)	(0.00123)		
L.Gini²	-1.76e-05	3.29e-06		
	(1.56e-05)	(1.24e-05)		
Transition*l.Gini		0.00717		
		(0.0120)		
Transition*l.Gini²		-5.06e-05		
		(0.000183)		
∆L.Gini			-0.000209	-0.000130
			(0.000216)	(0.000212)
∆L.Gini²			-1.09e-05	-1.09e-05
			(1.33e-05)	(1.17e-05)
Transition*∆l.Gini				-0.00141
				(0.00119)
Transition*∆l.Gini²				7.58e-05
				(8.42e-05)
L.GDP	-0.0506***	-0.0469***	-0.0636***	-0.0633***
	(0.00881)	(0.00901)	(0.0106)	(0.0106)
L.PI	-0.00817	-0.00764	-0.0140**	-0.0139**
	(0.00517)	(0.00527)	(0.00628)	(0.00624)
L.Schooling_m	0.00115	0.00287	-0.00803	-0.00760
	(0.00858)	(0.00872)	(0.0105)	(0.0105)
L.Schooling_f	0.00194	-0.00128	0.0132	0.0126
	(0.00922)	(0.00962)	(0.0116)	(0.0117)
Constant	0.375***	0.384***	0.554***	0.551***
	(0.0754)	(0.0782)	(0.0869)	(0.0870)
Observations	712	712	577	577
Number of groups	122	122	115	115
Year FE	YES	YES	YES	YES
Turning point for transition countries[11]	58.2 (Max.)	**141.7 (Max.)**	No quadratic effect	9.3 (Min.)
F-test of quadratic terms (p-value)[12]	0.2363	**0.0393*** **		0.4941

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

[11] 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.Gini² (and their respective values in differences)

[12] The test contains all constituent terms of the interactions, i.e., L.Gini, L.Gini², Transition*l.Gini and Transition*l.Gini²

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 A2). 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

As a final check 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 changes on levels 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 A3).[13] The FE estimates of the full sample (table 5) show that a growing inequality is related to lower subsequent growth. 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 A5.[14]

[13] 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[13]) and we therefore proceed with the model without the level variable. Appendix table A4 displays the results with the level variable.

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

Table 5: Piecewise linear regressions of inequality changes, FE and GMM results

	(1) FE	(2) FE	(3) FE	(4) FE	(5) GMM	(6) GMM	(7) GMM	(8) GMM
VARIABLES	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)
No. 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
% misspecified					0	0	0	0
Observations	614	614	614	614	497	497	497	497
# of groups	115	115	115	115	110	110	110	110
Control variables	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

*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.*[15]

Finally, the specification using the full sample is repeated, but with interactions between the inequality change variables and a transition country dummy (table 6). 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.

[15] 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).

Table 6: Piecewise linear regressions of inequality changes with transition country interaction and FE results

	(1) FE	(2) FE	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE	(8) FE
VARIABLES	3	5	10	20	3	5	10	20
Decrease	-0.000365	-0.000274	-0.000166	0.000220	-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	0.000940	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
Number of groups	115	115	115	115	110	110	110	110
Control variables	YES	YES	YES	YES	YES	YES	YES	YES
Transition-Year FE	NO	NO	NO	NO	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

*Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Instruments in the difference GMM have been collapsed.*

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 7, the results using ten-year averages are not only qualitatively very similar to 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 that the same caveats pertaining to the 5-year results are also present in the 10-year averaged data: 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.

Table 7: 10-year averages

VARIABLES	(1) Baseline	(2) Transition countries	(3) Transition countries and year effects	(4) Transition country year effects
L.Gini	0.000377*	0.000253	0.000247	0.000268
	(0.000225)	(0.000218)	(0.000216)	(0.000212)
L.Gini*trans		0.00255***	0.000883	
		(0.000818)	(0.000817)	
Observations	296	296	296	296
Number of groups	118	118	118	118
Control variables	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Transition-Year FE	NO	NO	YES	YES

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

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 and once with a two period lag. The contemporaneous specification, shown in the first panel of table 8, does not yield any significant results – if anything, there is 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. More results emerge with the two period lagged Gini coefficient, displayed in the second panel of table 8. The results 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).[16] However, when subjected to a GMM,[17] 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.

Table 8: Alternative lag structures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LAGS	0	0	0	2	2	2	2	2
VARIABLES	FE	FE	FE, trans & transyear	FE	FE	FE, trans & transyear	FE, transyear	GMM, transyear
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***	0.00120		
		(0.00226)	(0.00188)		(0.000854)	(0.00119)		
Observations	721	721	721	625	625	625	625	506
# of groups	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
Transition-Year FE	NO	NO	YES	NO	NO	YES	YES	YES

*Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Instruments in the difference GMM have been collapsed.*

Alternative inequality data

Although there are some clear advantages to using Solt's (2014) SWIID data, some researchers have expressed concern over the choice of the imputation procedure, and the validity of the resulting data (Jenkins 2014). 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).[18] Again, as shown in table 9, the results are similar to what we have obtained in our basic

[16] 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.

[17] 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 A7. 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.

[18] 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

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 & 4), although the coefficient on the interaction just misses significance in column 2.

Table 9: WIID (adjusted) Ginis

VARIABLES	(1) FE	(2) FE	(3) FE	(4) FE
L.Gini	0.000688**	0.000352	0.000415	0.000322
	(0.000339)	(0.000353)	(0.000345)	(0.000328)
L.Gini*trans		0.00200	-0.000944	
		(0.00129)	(0.00106)	
L.GDP	-0.0508***	-0.0496***	-0.0410***	-0.0409***
	(0.0104)	(0.0105)	(0.0110)	(0.0110)
L.PI	-0.00801	-0.00771	-0.00956	-0.00951
	(0.00569)	(0.00595)	(0.00729)	(0.00733)
L.Schooling_male	0.00642	0.00846	0.00311	0.00346
	(0.0109)	(0.0115)	(0.00996)	(0.0100)
L.Schooling_female	0.00176	-0.000879	0.000369	7.75e-05
	(0.0112)	(0.0118)	(0.0117)	(0.0117)
Constant	0.402***	0.400***	0.346***	0.344***
	(0.0848)	(0.0867)	(0.0878)	(0.0883)
Observations	562	562	562	562
R-squared	0.326	0.340	0.483	0.481
Number of groups	118	118	118	118
Control variables	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Year-Trans FE	NO	NO	YES	YES

*Robust standard errors in parentheses, *** 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 address the functional form concerns raised by Banerjee and Duflo, 2003. Using the SWIID data, providing 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 experiences of transition countries, which

was available in the original data, we report another version of table 9 in the appendix (table A8), where the duplicates where switched. The results are very similar between the two versions.

experienced 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 artificially 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 transition countries, these associations disappear for the transition country subsample 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.

These results point to two conclusions. First, there is no systematic empirical relationship between initial inequality and growth across the world, the positive impact which can be found for the overall sample is entirely driven by transition countries. Thus there does not appear to be a trade-off between inequality and growth. Second, the positive impact of inequality on growth in transition countries is not robust to the inclusion of separate time effects, and hence appears to be driven by other events. Lastly, given that this relationship disappears once separate time effects are introduced for transition countries, it appears that this association is not causal but rather driven by the particular dynamics of transition countries. Our finding is 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.

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Appendix

Figure A1: Correlation between growth and (lagged) inequality in transition countries

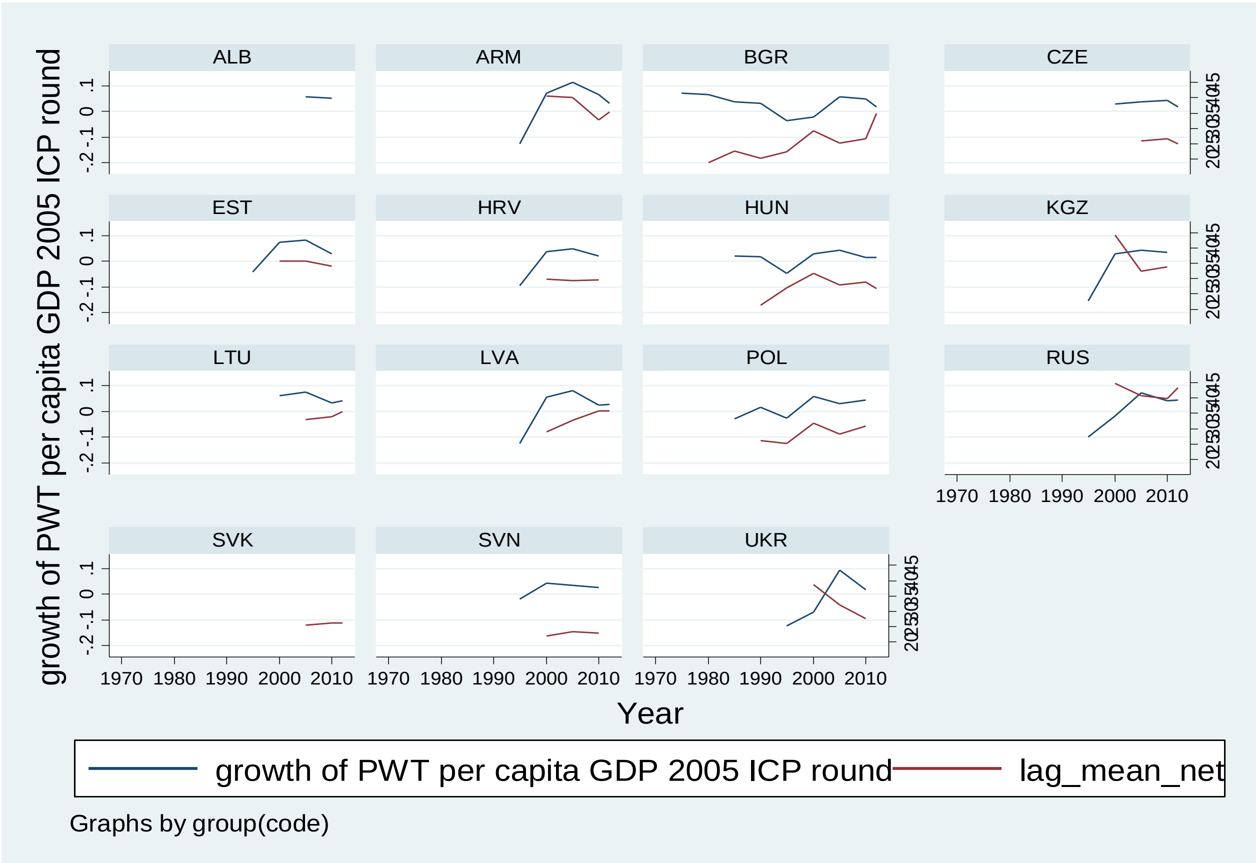

Table A1: GMM results, level specification

VARIABLES	(1) restricted	(2) collapsed	(3) col. & res.	(4) ort. & res.	(5) ort. & col.
L.Gini	**0.00163**	**0.00161***	**0.00178**	**0.00169***	**0.00214**
	(0.00158)	(0.000957)	(0.00504)	(0.000901)	(0.00141)
L.PI	-0.0360	-0.0280	-0.0459	-0.0158	-0.0505**
	(0.0262)	(0.0181)	(0.0798)	(0.0174)	(0.0243)
L.GDP	-0.131***	-0.111***	-0.208***	-0.0928***	-0.127***
	(0.0237)	(0.0210)	(0.0447)	(0.0181)	(0.0192)
L.Schooling_m	-0.0308	-0.0402	0.0133	-0.00455	-0.0282
	(0.0300)	(0.0288)	(0.123)	(0.0203)	(0.0305)
L.Schooling_f	0.0257	0.0392*	0.0334	0.0202	0.0400
	(0.0306)	(0.0207)	(0.0777)	(0.0198)	(0.0337)
Number 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
Number of groups	115	115	115	116	116
Year FE	YES	YES	YES	YES	YES

*Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The top row of the GMM tables indicates the type of restriction which has been imposed: (res=lags restricted to 3&4, col=collapsed, ort=orthogonalized).*

Table A2: GMM results, quadratic level specification

VARIABLES	(1) res	(2) col	(3) colres	(4) ortres	(5) ortcol	(6) ortcolres
L.Gini	0.000389	-0.00695	0.00943	0.000494	-0.00501	0.00859
	(0.00483)	(0.00717)	(0.0275)	(0.00372)	(0.00686)	(0.0226)
L.Gini²	-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*l.Gini	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*l.Gini²	-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)
L.GDP	-0.0922***	-0.0760***	-0.187**	-0.0770***	-0.0855***	-0.188**
	(0.0199)	(0.0208)	(0.0763)	(0.0169)	(0.0200)	(0.0841)
L.PI	-0.0117	-0.0138	-0.0112	-0.00691	-0.0134	-0.0527
	(0.0177)	(0.0174)	(0.0609)	(0.0114)	(0.0196)	(0.0566)
L.Schooling_m	-0.0131	-0.0240	0.0594	0.00379	-0.0174	-0.00192
	(0.0320)	(0.0275)	(0.0876)	(0.0187)	(0.0253)	(0.0688)
L. Schooling_f	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	0.6730
Observations	566	566	566	590	590	590
Number of groups	115	115	115	116	116	116
Year FE	YES	YES	YES	YES	YES	YES

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

Table A3: Differences

VARIABLES	(1) differences	(2) differences with transition country dummy
ΔL.Gini	-0.000206	-0.000111
	(0.000214)	(0.000206)
Transition*ΔL.Gini		-0.000690
		(0.000836)
L.GDP	-0.0635***	-0.0639***
	(0.0106)	(0.0106)
L.PI	-0.0140**	-0.0141**
	(0.00625)	(0.00635)
L.Schooling_m	-0.00774	-0.00756
	(0.0105)	(0.0104)
L. Schooling_f	0.0130	0.0127
	(0.0115)	(0.0115)
Constant	0.553***	0.556***
	(0.0866)	(0.0870)
Observations	577	577
Number of groups	115	115
Year FE	YES	YES

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

Table A4: Splines with level Gini, FE results

VARIABLES	(1) 3	(2) 5	(3) 10	(4) 20
L.GDP	-0.0505***	-0.0505***	-0.0501***	-0.0495***
	(0.00958)	(0.00967)	(0.00977)	(0.00986)
L.PI	-0.00805	-0.00812	-0.00806	-0.00775
	(0.00897)	(0.00901)	(0.00911)	(0.00878)
L.Schooling_m	-0.00332	-0.00297	-0.00274	-0.00226
	(0.00851)	(0.00850)	(0.00844)	(0.00834)
L. Schooling_f	0.00635	0.00608	0.00575	0.00514
	(0.00945)	(0.00951)	(0.00954)	(0.00943)
L.gini_net	**-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
Number of groups	115	115	115	115
Year FE	YES	YES	YES	YES

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

Table A5: Splines, FE results with sample excluding transition countries

VARIABLES	(1) 3	(2) 5	(3) 10	(4) 20
L.GDP	-0.0466***	-0.0468***	-0.0470***	-0.0470***
	(0.0104)	(0.0104)	(0.0105)	(0.0104)
L.PI	-0.00893	-0.00903	-0.00913	-0.00908
	(0.00964)	(0.00959)	(0.00959)	(0.00961)
L.Schooling_m	-0.00948	-0.00979	-0.0101	-0.0102
	(0.00802)	(0.00801)	(0.00806)	(0.00785)
L. Schooling_f	0.00930	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	-0.000200	-0.000188
	(0.000452)	(0.000306)	(0.000188)	(0.000134)
Positive change	-0.000154	-0.000129	-0.000119	-0.000170
	(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
Number of groups	100	100	100	100
Year FE	YES	YES	YES	YES

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

Table A6: 10-year averages, levels and differences

VARIABLES	(1) levels and differences	(2) levels and differences with transition countries	(3) levels and differences with transition countries & year effects	(4) levels and differences with transition country year effects
L.Gini	0.000309	0.000369	0.000336	0.000306
	(0.000422)	(0.000471)	(0.000472)	(0.000461)
Transition*L.Gini		0.000224	-0.00335	
		(0.00194)	(0.00258)	
ΔL.Gini	-2.26e-05	-0.000179	-0.000152	-0.000102
	(0.000253)	(0.000294)	(0.000302)	(0.000282)
Transition*ΔL.Gini		0.000702	0.00186	
		(0.000679)	(0.00119)	
L.GDP	-0.0181**	-0.0191**	-0.0153*	-0.0145*
	(0.00891)	(0.00887)	(0.00901)	(0.00859)
L.PI	-0.0397***	-0.0391***	-0.0361**	-0.0358**
	(0.0144)	(0.0145)	(0.0150)	(0.0149)
L.Schooling_m	-0.00511	-0.00496	-0.00602	-0.00568
	(0.00786)	(0.00886)	(0.00927)	(0.00912)
L.Schooling_f	0.00300	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
Number of groups	91	91	91	91
Year FE	YES	YES	YES	YES
Transition-Year FE	NO	NO	YES	YES

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

Table A7: Two-year lag FE and alternative GMM specifications

VARIABLES	(1) FElag2	(3) diffGMMcoll	(4) diffGMMcoll	(5) sysGMMres	(6) sysGMMcoll	(7) sysGMMcoll
L.Gini	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)
L.GDP	-0.0489***	-0.1000***	-0.0633***	0.00777	-0.00127	-0.00170
	(0.00998)	(0.0159)	(0.0147)	(0.00547)	(0.00445)	(0.00344)
L.PI	-0.0142**	-0.0344***	-0.0306**	-0.0181	-0.00928	-0.00663
	(0.00565)	(0.0126)	(0.0123)	(0.0136)	(0.00819)	(0.00772)
L.Schooling_m	-0.00597	-0.0535**	-0.0166	-0.0108	-0.00672	-0.00246
	(0.00849)	(0.0225)	(0.0195)	(0.0110)	(0.00726)	(0.00971)
L.Schooling_f	0.00737	0.0410*	0.0150	0.00824	0.00663	0.00339
	(0.0108)	(0.0214)	(0.0222)	(0.0109)	(0.00728)	(0.00942)
Constant	0.412***			-0.0106	0.0750	0.0602*
	(0.0788)			(0.0503)	(0.0503)	(0.0337)
Observations	625	481	506	625	625	625
R-squared	0.438					
Number of groups	119	113	114	119	119	119
Year FE	YES	YES	YES	YES	YES	YES
Year-Trans FE	YES	YES	YES	YES	YES	YES
N. of 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	0.0140	0.00845	0.00751
AR(2)		0.925	0.250	0.201	0.324	0.292

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

Table A8:WIID Ginis (adjusted), Version 2

VARIABLES	(1) FE	(2) FE	(3) FE	(4) FE
L.Gini	0.000813**	0.000456	0.000500	0.000368
	(0.000370)	(0.000399)	(0.000386)	(0.000365)
Transition*L.Gini		0.00197	-0.00126	
		(0.00126)	(0.00105)	
L.GDP	-0.0512***	-0.0500***	-0.0410***	-0.0411***
	(0.0103)	(0.0104)	(0.0109)	(0.0110)
L.PI	-0.00801	-0.00783	-0.00952	-0.00951
	(0.00578)	(0.00604)	(0.00730)	(0.00737)
L.Schooling_m	0.00704	0.00874	0.00333	0.00369
	(0.0109)	(0.0116)	(0.00989)	(0.01000)
L.Schooling_f	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-squared	0.330	0.344	0.485	0.481
Number of groups	118	118	118	118
Year FE	YES	YES	YES	YES
Year-Trans FE	NO	NO	YES	YES

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