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Measuring Income Inequality and Implications for Economic Transmission Channels

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Measuring Income Inequality and Implications for Economic Transmission Channels

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Authorized for distribution by Nikolay Gueorguiev

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Abstract

We study the channels that theoretically transmit the effects of inequality to economic growth, unlike much of the existing literature that focuses on the direct linkage. The role of inequality in these transmission channels is difficult to pin down and varies with the particular inequality indicator chosen. We run our analyses with six methodologically distinct inequality measures (Gini coefficients and Top10 income shares). Methodological differences *within* the set of Gini coefficients and the Top10 income shares exert a first-order impact on the estimated relationships, which is generally larger than the effect of switching *between* Gini and Top10 income shares. For a given inequality indicator, we find that the transmission channels can react in opposite directions, with the net effect on growth difficult to determine. Finally, we emphasize two additional but so far underappreciated empirical complications: (i) estimated relationships change over time; and (ii) fragile countries create significant but counterintuitive empirical associations that may obscure structural relationships.

JEL Classification Numbers: D31, P36, P16, O15, O47.

Keywords: economic growth, fragile countries, growth transmission channels, income inequality, inequality measurement, and political stability.

Author's E-Mail Address: RBlotevogel@imf.org; EImamoglu@imf.org; KMoriyama@imf.org; BSarr@imf.org

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I. INTRODUCTION

1. This paper makes the case that measurement of income inequality has a first-order impact on its estimated empirical effects. We specifically explore the empirical association between inequality and the transmission channels identified in the literature through which inequality can affect economic growth—human capital, fertility, capital services (the input of capital into production), total factor productivity (TFP), and political stability. This approach contrasts with much of the existing literature on the inequality-growth nexus that has focused on the direct reduced-form relationship between inequality and growth, without seeking to differentiate between different inequality measures and transmission channels. Our main finding is reminiscent of an earlier contribution by Atkinson and Brandolini (2001) on the reliability of secondary inequality statistics: "the choice of data matters."

2. Indeed, we confirm that the choice of data is paramount. We use three distinct variants of the Gini coefficient and the Top10 income share each as indicators of inequality. We then study the empirical association between these indicators and the growth transmission channels using three different methods: event studies, weighted-average least square regressions (WALS), and Pooled-Mean Group (PMG) regressions. Econometrically, each of three methods method addresses a neglected problem in the empirical inequality literature. Taken together, the methods put into sharp relief the contingent nature of the empirical relationships on the choice of an inequality indicator—relationships found to be statistically significant with one indicator tend not to replicate with another. What is more, the relationships between inequality and the transmission channels change over time and are sensitive to the inclusion of fragile countries.

3. Specifically, empirical results are more sensitive to the variations *within* the two groups of inequality indicators (the methodological choices that make one Gini coefficient different from the next) than to the variations *between* the two groups (the difference between the Gini and the Top10 income share). The same inequality indicator, but compiled using different methodologies, can yield substantially different results about the empirical relationship with the transmission channels.

4. The crucial importance of methodology in the compilation of inequality statistics is quickly moving from obscurity into the limelight. Auten and Splinter (2019), to take just one example, show that the trajectory of the top 1 percent income share in the United States for the past 50 years looks substantially different under different methodological assumptions. Splinter (2019) shows that the fraction of the increase in annual inequality attributable to labor mobility varies between zero and 75 percent depending solely on methodological choices.

5. Why are measurement technicalities and data compilation methods so important when dealing with income inequality statistics? The basic problem arises because the distribution of incomes across individuals is not directly observable; it can only be estimated. The data underlying these estimates are imperfect. Household surveys and administrative tax records both have well-known limitations. A fixed set of assumptions is necessary to estimate the unobserved

components of income and the incidence of taxes and transfers. It turns out that these methodological choices, which typically operate silently in the background, are exerting a first-order influence over the empirical pattern of inequality statistics.

6. The empirical challenges do not end with measurement uncertainty. For a given indicator, inequality may often affect several of the transmission channels simultaneously but in opposite directions. As the strength of each transmission channel is likely to be country-specific, the overall net effect from inequality on economic growth for a specific country is difficult to determine from cross-country regressions. Complicating matters further, within each transmission channel, the effects of inequality can change over time. And finally, the estimated effects of inequality vary across different country samples. We show that social conflict in fragile countries can create significant correlations between inequality and political stability, although we cannot speak with any confidence about the causal nature of this relationship.

7. Our paper contributes to three related strands of the empirical literature on the economic effects of inequality. We emphasize that methodological choices in the compilation of inequality indicators affect empirical patterns and relationships. This aspect builds on the literature that highlights the lack of consistency in international inequality statistics and attempts to improve their comparability for cross-country work (Anand and Segal, 2008; Atkinson and Brandolini, 2001; Deininger and Squire, 1998; Fields, 1994; Knowles, 2005; Perotti, 1996). Our paper is also inspired by the ongoing debate about the evolution of top income shares, especially in the United States, where methodological differences lead to starkly different levels and trends for the same inequality indicator, the top 1 percent income share (Atkinson et al. 2011; Auten and Splinter, 2019; Kopczuk, 2019; Piketty et al., 2018; Piketty and Zucman, 2003; Saez and Zucman, 2019; Smith et al., 2020).

8. Finally, we also extend the vast literature on the effects of inequality on economic growth (Alesina and Perotti, 1996; Alesina and Rodrik, 1994; Banerjee and Duflo, 2003; Barro, 2000; Berg et al., 2018; Brueckner and Lederman, 2018; Dabla-Norris et al., 2015; Deininger and Squire, 1998; Ferreira et al., 2018; Forbes, 2000; Galor and Zeira, 1993; Grigoli et al, 2016; Grigoli and Robles, 2017; Halter et al., 2014; Knowles, 2005; Li and Zou 1998; Marrero and Servén, 2018; Neves et al., 2016; Perotti, 1996; Persson and Tabellini, 1994; Ravallion, 2012; Voitchovsky, 2005), by deepening the understanding of the transmission channels at work (Berg et al., 2018; Neves and Silva, 2014; Perotti, 1996).

9. The rest of the paper flows as follows. The next section describes our data on income inequality and the transmission channels. We outline in greater detail the challenges in measuring income inequality consistently over time and across countries. The third section presents our empirical strategy and discusses the three different estimation methods we employ to study the effect of inequality on growth transmission channels. The estimation results are in Section 4, along with several robustness checks. The final section concludes and offers thoughts for improving our understanding of the economic effects of income inequality.

II. DATA

Inequality Indicators

10. We use six income inequality indicators in our empirical analysis, grouped into two sets. The first set comprises three different Top10 income shares, from: (i) the World Bank's World Development Indicators (WDI); (ii) the World Inequality Database (WID); and (iii) and the Luxemburg Income Study (LIS). The second set comprises three different Gini coefficients, from: (iv) the Standardized World Income Inequality Database (SWIID; version 8.3); (v) the SWIID (version 3.1, used in Berg et al., 2018); (iv) and International Disposable Income Gini Database (Fiscal Affairs Department, 2017). Our indicators generally cover the time period 1970–2017, with some variation in the start and end date across indicators.[1]

11. At the outset, we emphasize that there is no such thing as the ideal inequality statistic, either theoretically or empirically. Inequality indicators attempt to convey information about the shape of countries' underlying income distributions. Reducing complex distributions to a single number for comparisons over time and across countries is fraught with conceptual and practical difficulties. Aggregating information about incomes and their dispersion necessarily loses information about the income earners and their circumstances. Moreover, each inequality statistic embeds, explicitly or implicitly, value judgement about the relative importance of individual income earners along the income distribution. Inequality statistics are, contrary to their promise, "not purely statistical" (McGregor et al., 2019).

12. Any given Top10 income share, for example, can be compatible with starkly different income shares of the lower third of the distribution; focusing on Top10 income is then akin to making a value judgement that the way the economy distributes incomes in bottom 90 percentile of the income distribution does not matter. Similarly, the Gini coefficient weighs more heavily incomes close to the center of the distribution than in the tails (Cowell, 2011). Economists typically focus only on one indicator, either because of practicality (the Gini is the most readily available indicator) or because they are making explicit assumptions about social welfare function (see Jenkins and Van Kerm, 2011; McGregor et al., 2019; and Ravallion, 2014, and 2018, for an introduction).

13. Our choice of the Top10 income shares and the Gini are only based on practical considerations: both indicators have been extensively used in the empirical literature on inequality (see, for example, Atkinson and Piketty, 2010; and Berg et al., 2018) and are available from several different sources with broad country coverage. The second criterion is crucial for our analysis. Differences *within* (as opposed to *between*) the two groups of inequality indicators allow us to zoom in on the importance of methodological choices in the compilation process of the indicators. If two Top10 income shares, for example, give different readings for the same country at the same point in time, they must, *ipso facto*, end up measuring something different.

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[1] In the Online Appendix, we also consider the Bottom20 income share as an additional group of inequality indicators. The sources of the Bottom20 income share indicators are the same as those of the Top10 income shares. The results for the Bottom20 income share are qualitatively very similar to those presented in the paper.

14. It is well known that different indicators of income inequality can send conflicting messages about the evolution of inequality, both within countries and across time (Cowell, 2011; and World Bank, 2016). But even in a given country at one point in time, the same income inequality indicator can suggest significantly different levels of inequality, primarily because of three methodological choices (Anand and Segal, 2008): (i) the definition of income; (ii) the unit of analysis; and (iii) the primary source data. As different methodologies give rise to different levels and trends of the same inequality indicator, it follows that methodologies can, on their own, have a significant impact on empirical relationships (Knowles, 2005).

15. *Income* appears intuitively straightforward but is difficult to measure consistently across countries. Significant complications in the measurement of income involve: (i) the treatment of taxes (pre-tax versus post-tax income and the rules for allocating taxes collected back to individual incomes); and (ii) the income sources considered (e.g., public and private pension income, social transfers, realized capital gains and corporate retained earnings, imputed rental income, and underreported income). Instead of *income*, some household surveys measure *consumption,* which too is rife with measurement challenges: (i) estimating the consumption of long-lived assets that are purchased only once and used over many years; and (ii) valuing and estimating the access to publicly provided in-kind transfers. Empirically and theoretically, *consumption* tends to be less volatile over time than *income*.

16. *The unit of observation* refers to the number of people who control and benefit from the income being measured. Income is typically observed at some aggregate level, often the level of households, tax units, or married couples. The average number of people grouped into the unit of observation can differ systematically across the income distribution (for example, lower-income households often count more members than richer households). The question then arises of how to split income between these individuals. This choice is consequential for measuring inequality (Anand, 1983). The most common options are to split the observed income among the following individuals: (i) adult equivalents (which assigns a weight of less than one to each additional adult and children in the household); (ii) adults only; and (iii) everybody (including children).

17. *Primary source data* are either household surveys or administrative tax records. Household surveys are widely available but suffer from measurement error and selection bias: respondents may not be able to accurately remember their income or consumption expenditure, and rich respondents are typically less likely to reply. In contrast, tax records are generally measured with less error and subject to lower nonresponse bias but they are often confidential and less readily available in detailed micro formats. Tax records also fail to reflect economic activity in the informal sector, which can be a significant component of national income particularly in emerging-market and low-income countries. Deriving income estimates from tax records confronts the significant challenge of ensuring consistency, across countries and over time. Differences and changes in prevailing tax legislation can have large effects on reported incomes for tax purposes.

18. Table 1 provides an overview of the methodological characteristics of our six inequality indicators.

Table 1. Methodological Characteristics of Inequality Indicators

Source	Indicator	Income Concept	Unit of observation	Primary source data	Comment
World Bank World Development Indicators (WDI)	Top10 income share (WDI)	Market, gross, and disposable income, and consumption expenditure	Individuals	Household surveys available in PovcalNet	based on the World Bank's PovcalNet database; mixes different income concepts, depending on national definitions of income or consumption in a nationally representative household survey. See World Bank (2015, 2016) for more details.
World Inequality Database (WID)	Top10 income share (WID)	Pre-tax national income, after pensions	Adults	Administrative tax records and national accounts data	follows Distributional National Accounts guidelines: taxable income reported on fiscal returns is scaled up to match pre-tax national income; requires assumptions for imputing national accounts income (e.g. corporate retained earnings and housing) and taxes (e.g. CIT, VAT) back to observed tax returns. See Alvaredo et al. (2016) for more details.
Luxemburg Income Study (LIS)	Top10 income share (LIS)	Disposable income	Adult equivalents	Household surveys	uses harmonized household surveys designed for cross-country comparability; disposable income is defined as the sum of labor, capital, pension, and monetary social transfers minus income taxes and social security contributions; it ignores non-monetary capital income (imputed rents for owner-occupied housing) and in-kind social transfers. See LIS (2019) for more details.
Standardized World Income Inequality Database	Gini (v8.3)	Disposable income	Adult equivalents	Household surveys	based on a large number of primary source-data Ginis, such as LIS, OECD, Eurostat and National Statistical Agencies; LIS observations serve as anchors; an algorithm predicts 'LIS-like' Ginis for all country-years where LIS data is unavailable, taking into account country and regional-specific correlations between the alternative sources, income definitions, equivalence scales and LIS. See Solt (2020) for more details.
Standardized World Income Inequality Database	Gini (v3.1)	Disposable income	Adult equivalents	Household surveys	same as above; but includes more than 600 country-year observations (clustered in the period 1960–1979) drawn from non-representative surveys of the employed population only.
International Disposable Income Gini Database	Gini (FAD)	Disposable income, consumption	Individuals and adult equivalents	Household surveys	Based on five primary source data Ginis (LIS, Eurostat, OECD, SEDLAC, PovcalNet); mixes disposable income and consumption and per capita and adult-equivalent equivalence scales; LIS observations serve as anchor and basis for inter- and extrapolation when LIS observations are not available. See Fiscal Affairs Department (2017) for more details.

19. Tables 2 and 3 provide summary statistics. Across all countries, the six indicators are positively, but imperfectly, correlated. Imperfect correlations in part reflect systematic differences in country coverage of each indicator. The Top10 income share from WDI and WID mainly cover developing countries (EMs and LICs), with the share of advanced countries accounting for only 26 and 37 percent, respectively, of all observations. In contrast, the LIS mainly covers advanced economies. The corresponding share of advanced economies in the three Gini series is between 29 and 39 percent. To get a feel for these systematic coverage differences, consider the case of China. China has only 12 year and 2 year-observations in the WDI and LIS Top10 income share series, respectively. The FAD Gini database has 13 observations. At the other end of the spectrum, the coverage of China in the SWIID and the WID Top10 income share series amounts to between 37 and 40 observations.

Table 2. Overview of Inequality Indicators

Indicators	Obs.	Mean	St. Dev.	Min	Max	Countries with at least one observation	Share of advanced economies (% of total)
The Top10 income share, WDI	1,459	31	7	19	62	162	26
The Top10 income share, WID	3,592	42	12	15	80	115	37
The Top10 income share, LIS	340	26	7	17	55	50	71
The Gini coefficient, SWIID v8.3	5,297	38	9	18	67	192	29
The Gini coefficient, SWIID v3.1	4,040	38	10	15	71	163	31
The Gini coefficient, FAD	1,667	37	9	19	66	155	39

Table 3. Pairwise Correlations between Inequality Indicators

	The Top10 income share, WDI	The Top10 income share, WID	The Top10 income share, LIS	The Gini coefficient, SWIID, v8.3	The Gini coefficient, SWIID, v3.1	The Gini coefficient, FAD
The Top10 income share, WDI	1.00					
The Top10 income share, WID	0.88	1.00				
The Top10 income share, LIS	0.93	0.80	1.00			
The Gini coefficient, SWIID, v8.3	0.89	0.87	0.97	1.00		
The Gini coefficient, SWIID, v3.1	0.91	0.87	0.96	0.90	1.00	
The Gini coefficient, FAD	0.98	0.87	0.96	0.93	0.96	1.00

20. Imperfect correlation and systematic differences in country coverage combine to give different answers to seemingly simple questions, such as: "What has been the evolution of income inequality in advanced economies since the 1980s?" As a summary measure for advanced countries, we calculate the mean of each indicator for every year from 1980 to 2015.[2]

21. Figure 1 presents the evolution of the mean of advanced countries only, separately for each indicator, over time. Table 4 shows the correlation matrix between the six time-series of means. The

[2] The median and population-weighted mean give qualitatively almost identical results.

correlation between the mean weakens compared to the correlations of the country-year data, especially between the three Top10 income share indicators. This exercise only provides a rough cut at the data and has significant limitations. That said, we note that stylized facts about inequality depend to a great extent on the underlying inequality indicator.

Figure 1. Means of Inequality Indicators

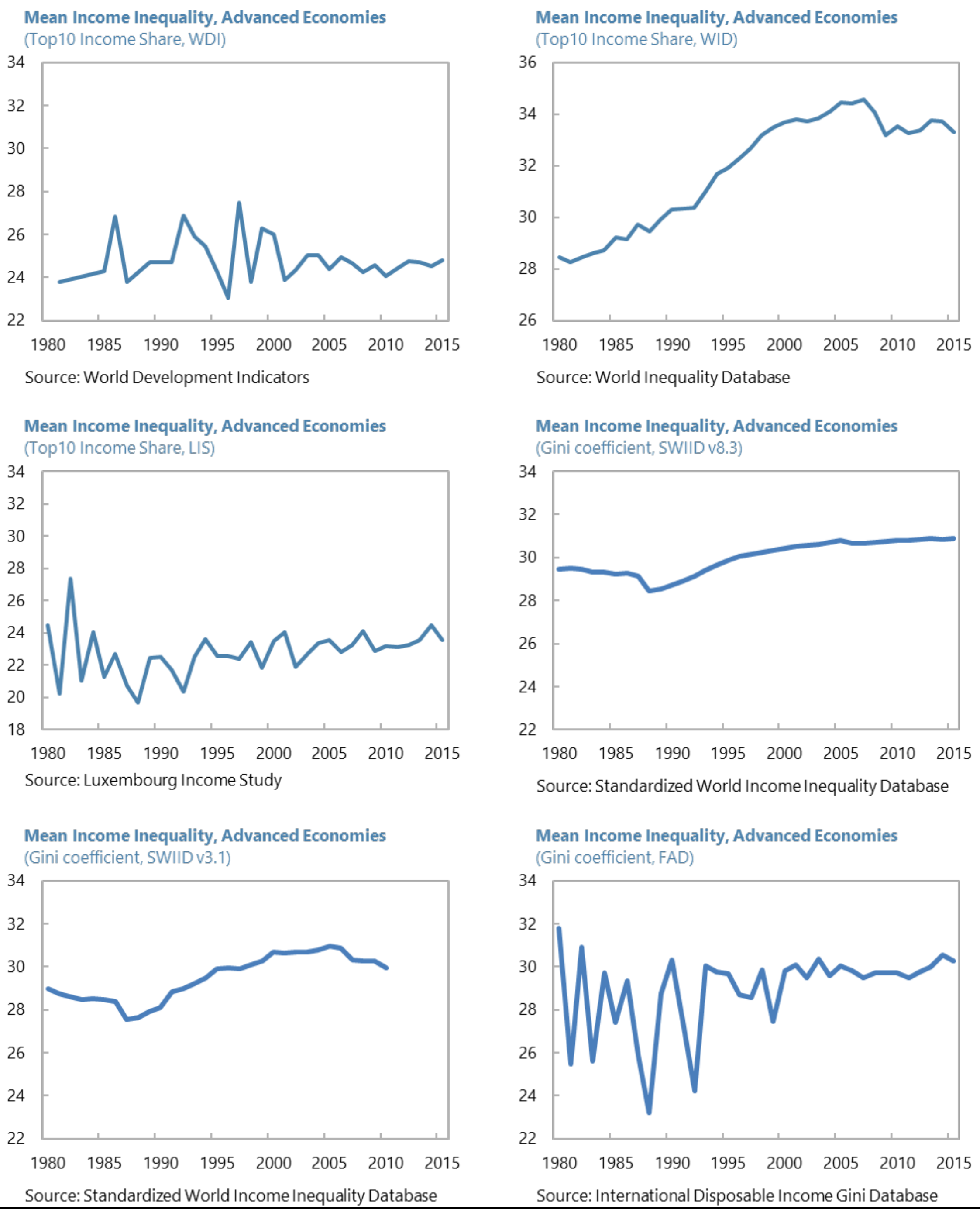

Table 4. Correlations between the Means of the Inequality Indicators for Advanced Economies Only

	Mean, WDI	Mean, WID	Mean, LIS	Mean, SWIID v8.3	Mean, SWIID v3.1	Mean, FAD
Mean, Top10 Income Share, WDI	1.00					
Mean, Top10 Income Share, WID	-0.07	1.00				
Mean, Top10 Income Share, LIS	0.06	0.27	1.00			
Mean, Gini, SWIID, v8.3	-0.18	0.87	0.43	1.00		
Mean, Gini, SWIID, v3.1	-0.02	0.90	0.34	0.94	1.00	
Mean, Gini, FAD	-0.05	0.43	0.85	0.52	0.49	1.00

Transmission Channels

22. We consider five variables to capture the transmission channels that run from inequality to economic growth: human capital, fertility, capital services (investment), TFP, and political stability. Previous attempts at identifying specific transmission channels are relatively scarce, with notable contributions from Perotti (1996) and Berg et al. (2018). Both studies emphasize the role of fertility and human capital accumulation.

23. These five transmission channels are at the heart of the economic theory that invokes an impact of inequality on economic growth (see Banerjee and Duflo, 2003). We analyze the degree to which inequality, as measured by our six inequality indicators, empirically exhibits a systematic relationship with these transmission channels. Note, however, that we do not investigate the second stage of the theoretical transmission process, the link between the channels and economic growth. The following sketches the intuitive workings of the five transmission channels and provides references to authors who have established their theoretical and empirical validity as determinants of growth:

- Income inequality may impede lower-income, liquidity-constrained households from undertaking profitable investments (including in human capital), exerting optimal effort, or choosing productive occupations (Aghion and Bolton, 1997; Bénabou, 1996a; Durlauf, 1996; Galor and Zeira, 1993; Moav, 2002; Piketty, 1997).
- Concentration of income at the top may facilitate more domestic savings and investment (Bhattacharya, 1998; Bourguignon, 1981; and Galor and Moav, 2004) and spur innovation and technological progress (Galor and Tsiddon, 1997; and Zweimüller, 2000).
- Inequality may provoke political instability and social conflict and (Alesina and Perotti, 1996; Bénabou, 1996b; Easterly, 2001; and Keefer and Knack, 2002).

24. Some of these theoretical contributions have implications for the type of income used to define inequality and the time horizon over which inequality's effects would be felt (Knowles, 2005 and Neves and Silva, 2014). Regarding the type of income, disposable income (after redistribution) determines people's ability to save and invest (in human capital and capital services). Market income

(pre-tax, pre-transfer) influence incentives for innovation. Either disposable or market income can generate political instability, depending on the type of instability under examination (for example, dwindling support for existing political institutions or socio-political unrest). In sum, both disposable and market income inequality therefore play a role in transmitting the effects of inequality to economic growth, although the strength of the transmission will differ across the five channels. Regarding the time horizon, the most common reading of the theoretical literature suggests that most of the effects of inequality would pertain to the long term. Intuitively, the process of accumulating human capital and reaping the associated economic benefits can take years and decades. Turning savings into productive investments and finding more efficient means of production, on the other hand, can play out over shorter time horizons. The design of our empirical strategy reflects these theoretical considerations.

25. Table 5 presents summary statistics of the transmission channels. Data on the accumulation of factors of production are from the Penn World Tables (v9.1). Specifically, we consider human capital, capital services, and TFP. These variables (known as the sources of growth) aggregate into real per capita GDP growth, both over time and across countries. We also consider fertility, from the World Bank and measured as births per woman, as an additional variable affecting education and human capital accumulation (De la Croix and Doepke, 2003). Note that economic theory predicts that higher fertility (not lower) is associated with weaker economic growth.

26. Our indicator of the political economy channel is the government stability sub-index from the International Country Risk Guide (Howell, 2011). This indicator comprises three dimensions of political stability, weighted equally: (i) government unity; (ii) legislative strength; and (iii) popular support. In the tradition of Cukierman et al. (1992), this indicator seeks to capture governments' ability to stay in office and implement their policy program.

Table 5. Overview of Transmission Channel Variables

Variable	**Obs.**	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Human Capital (index)	6,593	2.170	0.722	1.007	3.974
Physical Capital Stock (index in constant prices in national currency; 2011=1)	5,728	0.611	0.349	0.016	3.034
TFP (index in constant prices in national currency; 2011=1)	5,039	1.011	0.311	0.289	7.107
Political Stability (index; 12=most stable)	4,484	7.563	2.058	0.667	12.000
Fertility (children per woman)	8,847	3.835	1.968	0.860	8.864

III. EMPIRICAL STRATEGY

27. We employ three different empirical methods to study the relationship between inequality and the growth transmission channels: (i) event studies; (ii) weighted-average least squares (WALS); and (iii) pooled-mean group (PMG) regressions.[3] Each method illuminates a particular neglected spot in the existing empirical literature on the inequality-growth relationship: event studies are less exposed to measurement error and can capture complex non-linearities, WALS regressions address model uncertainty, and PMG regressions are robust to unobserved factors that affect both the transmission channels and inequality.

28. That said, no empirical method is perfect: event studies are not designed to capture country-specific shocks; both event studies and WALS regressions require strong identifying assumptions for causal interpretation; and PMG models cannot be estimated for many inequality time series for lack of sufficient data. In our interpretation, we therefore stay away from making strong claims about causal relationships and instead focus on the sensitivities of estimated empirical relationships arising from differences in: (i) inequality indicators; (ii) control variables; (iii) time and country samples.

29. We estimate the event studies and WALS regressions separately for each transmission channel, using the six inequality indicators. This approach yields thirty different indicator-transmission channel combinations. By considering each transmission channel individually, we abstract from possible interrelationships between the channels. This approach is valid under the identifying assumptions of each method discussed below. The discussion in section II suggests some combinations are theoretically more likely than others to display significant relationships. But we keep an open mind about the validity of the theory and do not restrict the number of combinations a priori. The event studies and WALS cover ten-year time windows. The PMG regressions cover longer time periods, at least 28 years, to distinguish between short- and long-term effects. Out of the two Ginis from the SWIID database that have sufficiently broad country coverage over longer time periods to meet the PMG's data requirements, only the most recent vintage (v8.3) is consistently based on nationally representative household surveys. So, we only use v8.3 for the PMG estimates.

[3] In the Online Appendix, we also consider panel GMM models. The results do not display a clear pattern, being similarly sensitive to changes in the inequality indicator as the main results in this paper. Moreover, we find persuasive recent theoretical and empirical critiques of GMM methods (Kraay, 2015; and Ferreira et al., 2018). They highlight the serious problems for statistical inference arising from weak instruments and under-identification, which are pervasive in GMM models.

Event Studies

30. Our event study design is based on a difference-in-difference model with fixed effects. Consider the following fixed-effect panel model (following Fuest et al., 2018; also see Jaumotte and Osorio Buitron, 2015):

$$tran\_ch_{it} = \alpha_i + \mu_t + \sum_{j=-5}^{5} \gamma_j D_{it}^{j} + \varepsilon_{it} \quad (1)$$

where $tran\_ch_{it}$ is the growth transmission channel at time t in country i, α_i is the country fixed effect, μ_t the year fixed effect (in actual time), and D_{it}^{j} a dummy variable indicating a large increase in inequality having occurred *j* years ago in country i relative to year t. The D_{it}^{j} dummy variable marks the 'event': a large *increase* in inequality, defined as a five-year change in the inequality indicator above the 75th percentile of all observed changes. The coefficients γ_j measure the impact of a large increase in inequality on the transmission channel. The event window comprises ten years: the five years during which inequality changes and the subsequent five years.

31. Every event study measures the impact of an event relative to a control, or baseline, group. In our set-up, the control group consists of countries having experienced large *declines* in inequality, taken to be changes in inequality that fall below the 25th percentile of all observed changes. The country-year observations included in the event study can therefore be thought of as a sample of 'large changes', comprising country episodes during which inequality either significantly increased or declined. Focusing on large changes circumvents the problem that country-level inequality is quite persistent over time, casting doubt on the reliability of empirical estimates that only rely on within-country variation of inequality over time (Halter et al., 2014; Berg et al., 2018). As a matter of fact, studies that rely on cross-sectional variation in inequality tend to find systematically different results than studies that examine the within-variation over time (Neves et al., 2016).

32. Table 6 presents summary statistics of the samples of 'large changes' in inequality, one sample for each of the six inequality indicators. It is important to highlight that the country-year observations included in the 'large changes' sample are specific to each inequality indicator. To illustrate, the WDI Top10 income share may exhibit a large change in Country A in the five years through 2010, whereas the other inequality indicators remain flat in that period. Accordingly, the 2010 episode in Country A will be included in the event study that uses the WDI Top10 income share, but not in the event studies that rely on the other indicators. Changing country compositions across the samples of 'large changes' are therefore a necessary feature (and not a bug) of our empirical strategy. However, differences in country coverage opens up the possibility of selection bias (if, say, 'large changes' of the WID Top10 income share were only occurring in emerging-market economies). Table 6 shows that the share of advanced economies in the sample of 'large changes' generally reflects the underlying country composition of the particular inequality indicator in question. The fact that the six inequality indicators themselves exhibit significant differences in country coverage remains a source of concern, for which the ultimate remedy is better data collection at the national level (World Bank, 2015). Two additional criteria apply when constructing the sample of large changes: (i) the transmission channel variable has to be observed continuously throughout the event window; and (ii) countries have at most one

observation of large increase and large decrease, to avoid biasing the sample with countries with better data coverage. If countries have more than one large increase or decline in any of the six inequality indicators, we only retain the most recent episode for that specific sample of 'large changes'.

Table 6. Overview of 'Large Changes' by Inequality Indicator

Indicators	Obs.	Mean	St. Dev.	Min	Max	Share of advanced economies (% of total)
Large increases						
The Top10 income share, WDI	49	3	3	1	15	27
The Top10 income share, WID	74	6	4	2	24	41
The Top10 income share, LIS	14	2	1	1	3	79
The Gini coefficient, SWIID v8.3	95	2	2	1	11	37
The Gini coefficient, SWIID v3.1	116	7	5	2	26	29
The Gini coefficient, FAD	54	4	2	1	11	37
Large decreases						
The Top10 income share, WDI	49	-5	3	-12	-2	14
The Top10 income share, WID	68	-6	4	-21	-1	37
The Top10 income share, LIS	12	-3	1	-5	-1	67
The Gini coefficient, SWIID v8.3	83	-2	1	-7	0	29
The Gini coefficient, SWIID v3.1	122	-6	5	-25	-2	25
The Gini coefficient, FAD	51	-6	3	-18	-3	25

33. The attractiveness of the event study design lies in its flexibility and parsimony. The right-hand side variables are free of measurement error, as they are either binary dummy variables or unobserved. The model is non-parametric in that it can accommodate complex non-linearities in the relationship between inequality and the transmission channels.

34. The event study identifies the causal effect of inequality on the transmission channels under two conditions: (i) common pre-event trends in both the treatment (countries having experienced large increases in inequality) and the control group (country having experienced large decreases); and (ii) absence of country-specific shocks. These are strong identifying assumptions. The first 'common trends' condition can be empirically tested: the γ_j marking country-year observations from before the large change in inequality (observations with a negative j) should not be different from zero. The second condition is not directly testable in the event study setup. Our alternative empirical methods are better equipped to address the problem of confounding variables.

Weighted-Average Least Squares

35. Weighted-Average Least Squares (WALS) is an example of recently developed computational model-averaging techniques that seek to address model uncertainty (Magnus, Powell, and Prüfer, 2010). To appreciate the role of model uncertainty, consider the following cross-sectional OLS regression set-up (based on Ravallion, 2012):

$$\Delta tran\_ch_{i\tau} = \alpha \quad + \beta \Delta inequa_{i\tau-5} + \boldsymbol{X}_{i\tau-5}\gamma + \varepsilon_i \qquad (2)$$

where $\Delta tran\_ch_{i\tau}$ is the five-year change in the transmission channel between time τ and τ-5 in country i, $\Delta inequa_{i\tau-5}$ is the five-year change in inequality between time τ-5 and τ-10 also in country i, $\boldsymbol{X}_{i\tau-5}$is a vector of control variables at time τ-5, and α is a constant. β is the coefficient of interest, as it measures the impact of inequality on the transmission channels.

36. The coefficient β aims to measure the causal impact of inequality on the transmission channels. To justify a causal interpretation of β, the lagged five-year change in inequality and the five-year lag in the other explanatory variables must not be systematically related to the subsequent five-year change in the transmission channel. This lag structure is common in cross-country regressions, although not without its critics (Durlauf et al., 2004).

37. The sample of country observations included in the WALS regressions follows the same selection criteria as that of the event study.[4] We only consider observations of countries having experienced large changes in inequality (increases and decreases) in which we can observe the subsequent five-year evolution of the transmission channels. As the event study, the regressions pool observations from different points in time: country A's large increase in inequality may have occurred in the five years to 2005, whereas country B's large increase may date from the five years to 1995. The sample of country observations are specific to each inequality indicator.

38. Which control variables should be included? Table 7 illustrates that the estimates of the coefficient β crucially depend on the set of included control variables $\boldsymbol{X}_{i\tau-5}$ (a point also underscored by Torstensson, 1996). The tables show the results of three simple OLS regressions, where we regress the change in political stability on the change in inequality in the preceding five-year period, as measured by the WDI Top10 income share. The main difference between the three specifications is the choice of control variables. The coefficient varies by a factor of three and in statistical significance.

39. Table 7 presents three of many possible specifications. Perotti's (1996) parsimonious and Barro's (2000) extensive specifications are common benchmarks in the literature (Banerjee and Duflo, 2003). However, singling out these two specifications is somewhat arbitrary, for two reasons. First, the choice of included control variables in both benchmark specifications reflects the authors' prior specifications but are not derived from an explicit theoretical model. Second, there are potentially

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[4] The LIS Top10 income share has significantly fewer observations than the other inequality indicators. To obtain a sufficient number of events for the WALS regressions, we widen the definition of events to include all increases in inequality of >P(70) and decreases of <P(30).

many more variables not included in either benchmark that could theoretically mediate the effect of inequality on the transmission channels and economic growth. Dabla-Norris et al. (2015), for example, emphasize the importance of financial development and trade and financial openness, which leads to a third, 'alternative', specification in Table 7.

Table 7. Three Alternative Sets of Control Variables

	Dependent Variable: Capital Stock (t-5 to t)		
	Perotti (1996)	**Barro (2000)**	**Alternative**
	(1)	(2)	(3)
	WDI	WDI	WDI
Inequality change (t-10 to t-5)	0.89*	0.40	1.19**
Observations	91	91	91
R-squared	0.08	0.38	0.14
Control variables:	Price level of investment goods (t-5) and GDP per capita level log (t-5)	GDP per capita level log (t-5), GDP per capita level log squared (t-5), Commodity Terms of Trade Index change (t-10 to t-5), Share of government consumption PPP (t-5), Share of gross capital consumption PPP (t-5), Inflation (t-10 to t-5), Electoral democracy index (t-5), Electoral democracy index, squared, (t-5) and Rule of Law index (t-5)	GDP per capita level log (t-5), Banking Crisis Dummy (t-5 to t), Trade Openness (t-5), Financial Openness (t-5) and Financial Development index (t-5)

Notes: *** p<0.01, ** p<0.05, * p<0.1

40. WALS is a data-dependent approach to identifying those variables in the set of $\boldsymbol{X}_{i\tau-5}$ that are consistently related to the transmission channels across the entire space of possible models (see Furceri and Ostry, 2019, for an example of using WALS to find robust determinants of inequality). Assume that we have k possible determinants (including changes in inequality) of each transmission channel. There are then 2^k possible models (combinations of the explanatory variables) for each channel. The WALS algorithm estimates a probability for each possible model based on a mean-square error criterion. WALS then uses these model-specific probabilities to weigh each estimate of β and γ. The final WALS estimates is equal to the weighted average of the model-specific estimates.

41. An explanatory variable with a WALS t-statistic larger than one is said to be a 'robust determinant.' At this level of statistical significance, adding the explanatory variable increases the model's adjusted R-squared and lowers its mean-square error. Put differently, Raftery (1995) and Masanjala and Papageorgiou (2008) show that a t-statistic greater than one in absolute value corresponds approximately to a posterior inclusion probability of greater than 0.5.

42. We do not use any prior information about whether changes in inequality are robustly related to the transmission channels. We therefore treat changes in inequality as an 'auxiliary' rather than 'focus' variable. 'Focus' variables are always included in the model, regardless of their explanatory power. In fact, the WALS methodology determines robustness of the 'auxiliary variables' by maximizing

the explanatory power of the 'focus' variables. For our purposes, there is only one 'focus' regressor—the constant. Changes in inequality is the first 'auxiliary' regressor. The remaining 'auxiliary regressors' are standard variables used as determinants of economic growth (see, for example, Berg et al., 2012) and can be grouped into four different categories:[5]

- Policy: inflation and price of investment goods (as a measure of distortions).
- Structural: economic development (GDP per capita and GDP per capita squared), financial development (financial development index from Svirydzenka, 2016), trade openness, international financial integration, share of government consumption (at current PPP exchange rates), and share of gross capital formation (at current PPP exchange rates).
- Institutional: democracy, democracy squared, and rule of law (from V-Dem, see Coppedge et al., 2020).
- Shocks: contemporaneous banking crisis (the only variable in the control group that is not lagged, belonging to the same time period as the change in inequality; from Laeven and Valencia, 2018), and change in commodity terms of trade index (weighted by net exports, from Gruss and Kebhaj, 2019).

The resulting 2^k possible versions of (2) stretch from the most parsimonious specification without any 'auxiliary variables' to the most extensive specification with all of them included.

Pooled Mean-Group Regressions

43. The Pooled Mean Group (PMG) estimator improves upon standard fixed- or random-effect panel models by allowing for relationship between inequality and the transmission channels to vary across countries in the short run (Chudik and Pesaran, 2015; Pesaran, 2006; Pesaran, et al., 1999). Only the long-run structural relationship between inequality and the transmission channel, if it exists, is identical across countries. Allowing adjustments to a long-run steady state to be different across countries makes the PMG estimator sufficiently flexible to account for country-specific interactions between structural characteristics and inequality.

44. In addition, the PMG estimator corrects for cross-sectional dependencies that are due to unobserved global factors, affecting all countries at the same time but to varying degrees. Pesaran (2006) shows that standard fixed- and random-effect estimators would be biased and inconsistent in the presence of unobserved common factors.

45. Our baseline PMG specification is as follows:

$$\Delta Tran\_Ch_{it} = \Phi_i\,[Tran\_Ch_{it-5} - \beta_0 - \beta_1 Gini_{it-5}] + \beta_{2i}\Delta Gini_{it} + \gamma_i X_{it-5} + \alpha_i f_{t-5} + \varepsilon_{it} \tag{3}$$

where $\Delta Tran\_Ch_{it}$ represents the change in the transmission channel between t-5 and t, Φ_i is the speed of adjustment to the long-run equilibrium and β_1 is the long-run response of the transmission

[5] Again, for the LIS Top10 income share, we restrict the list of the 'auxiliary variables' to increase degrees of freedom. We use the 'auxiliary variables' that are the most relevant for the other inequality indicators.

channel to inequality, β_{2i} is the short-run effect of inequality $\Delta Gini_{it}$ that varies across countries, and $Gini_{it-5}$ is the five-year lagged level of the Gini. X_{it-5} is the parsimonious set of control variables that includes GDP per capita and the price level of capital formation (as in Perotti, 1996). The vector f_{t-5} contains unobservable common factors, α_i represent the associated factor loadings, and ε_{it} is the error term.

46. The main coefficient of interest is the structural long-run parameter β_1. We are interested to test if the model's long-run structural relationship is stable over time and across country income groups. To this end, we run rolling-window regressions, whereby we estimate equation (3) 20 times over a rolling window of 28 years between 1970 and 2017 (the first estimation window is 1970-1997 and the last one is 1990-2017). We run these rolling-window regressions for the full sample of countries, and separately for advanced and emerging-market and low-income countries.

47. Pesaran (2006) shows that the parameters in (3) can be consistently estimated, and causally interpreted, by adding cross-sectional means of the dependent and independent variables. That way, the model controls for unobserved common factors that affect the transmission channels and inequality at the same time. A second source of endogeneity arises from the possibility of reverse causality. The PMG model mitigates this concern by explaining the change in the transmission channels by the 5-year lagged level of inequality. The empirical model is therefore robust to time-invariant country-specific factors such as institutions that could affect both inequality and the transmission channels. Pesaran and Tosetti (2011) also show that this method is robust to non-stationarity in both observables and non-observables and works well in the presence of weak and/or strong cross-sectionally correlated errors.

48. Table 8 shows results for the cross-sectional dependence test by Pesaran (2004), which tests the null hypothesis of no cross-sectional dependence. The CD test strongly rejects the null hypothesis for the Gini coefficient and the transmission channels. The variables thus appear to be related across countries, reflecting the impact of unobserved common factors. Ignoring this cross-sectional dependence would bias standard panel estimators such as the within-estimator.

Table 8. Cross-Sectional Dependence Test

Variable	Gini	Human Capital	Capital Services	TFP	Political Stability	Fertility
CD Test	277.8***	623.4***	382.5***	17.9***	282.9***	605.1***
P-value	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of Countries	161	126	115	108	114	136

Notes: *** p<0.01, ** p<0.05, * p<0.1

IV. RESULTS

49. In this section, we document that: (i) estimated empirical relationships are sensitive to the choice of inequality indicator, with the sensitivity being greater for two indicators from the same group of indicators (two Top10 income shares or two Ginis) than for two indicators from two different groups (one Top10 indicator and one Gini); (ii) for a given specific indicator, inequality can affect the transmission channels in opposite directions, with the net effect difficult to determine; (iii) the estimated relationships change over time; and (iv) results for political stability, in particular, are sensitive to the inclusion of fragile countries. These results emerge by asking three questions. Do the three Top10 income shares and three Gini coefficients yield the same empirical relationships with the transmission channels? Are differences in estimated relationships predominately due to using different variants of the same indicator (the variation arising from *within* the Top10 income shares) or due to using different indicators (the variation arising from switching *between* Top10 income shares and the Gini coefficient)? Finally, for a given inequality indicator, are the estimated effects stable over time and across different country samples?

Event Study

50. Figures 2.1 and 2.2 show the results of the event study. The bolded symbols and the dotted bars represent the point estimates and confidence intervals, respectively, of the γ_j in equation (1). The point estimates measure the differential impact of increases in inequality (relative to the control group of countries having experienced declines in inequality) on the transmission channel over a ten-year period, the length of the event window. Figure 2.1 (2.2) shows estimates of equation (1), with each series representing one of the three Top10 income shares (Gini coefficients) as inequality indicator. The point estimate at t+5 is especially important, as it measures the cumulative effect on the level of each transmission channels five years after the large increase in inequality. In the following discussion of the results, we group results by transmission channel and refer to them as 'consistent' if at the end of the five-year event window: (i) all three inequality indicators carry the same sign (regardless of statistical significance); or (ii) two indicators carry the same sign and are statistically significant.

51. Let us start with Figure 2.1 that focuses on the Top10 income shares. Only two out of five transmission channels display 'consistent' results when using the Top10 income share indicators: capital services and TFP. For capital services, the three-point estimates of the effect of inequality after five years are positive, bounded between 0 and 10 percent, and statistically insignificant. For TFP, two indicators are (just about) statistically significant and negative (the WDI and WID), whereas the LIS is

positive and insignificant. In case of the other three transmission channels, the estimated relationships after five years carry different signs when switching indicators. Moreover, in the transmission channels of fertility and political stability, the statistical significance differs as well. Curiously, for political stability: the WID suggests a significantly positive impact, whereas the WDI signals a (marginally) significantly negative relationship.

Figure 2.1 Event Study and Transmission—Top10 Income Share

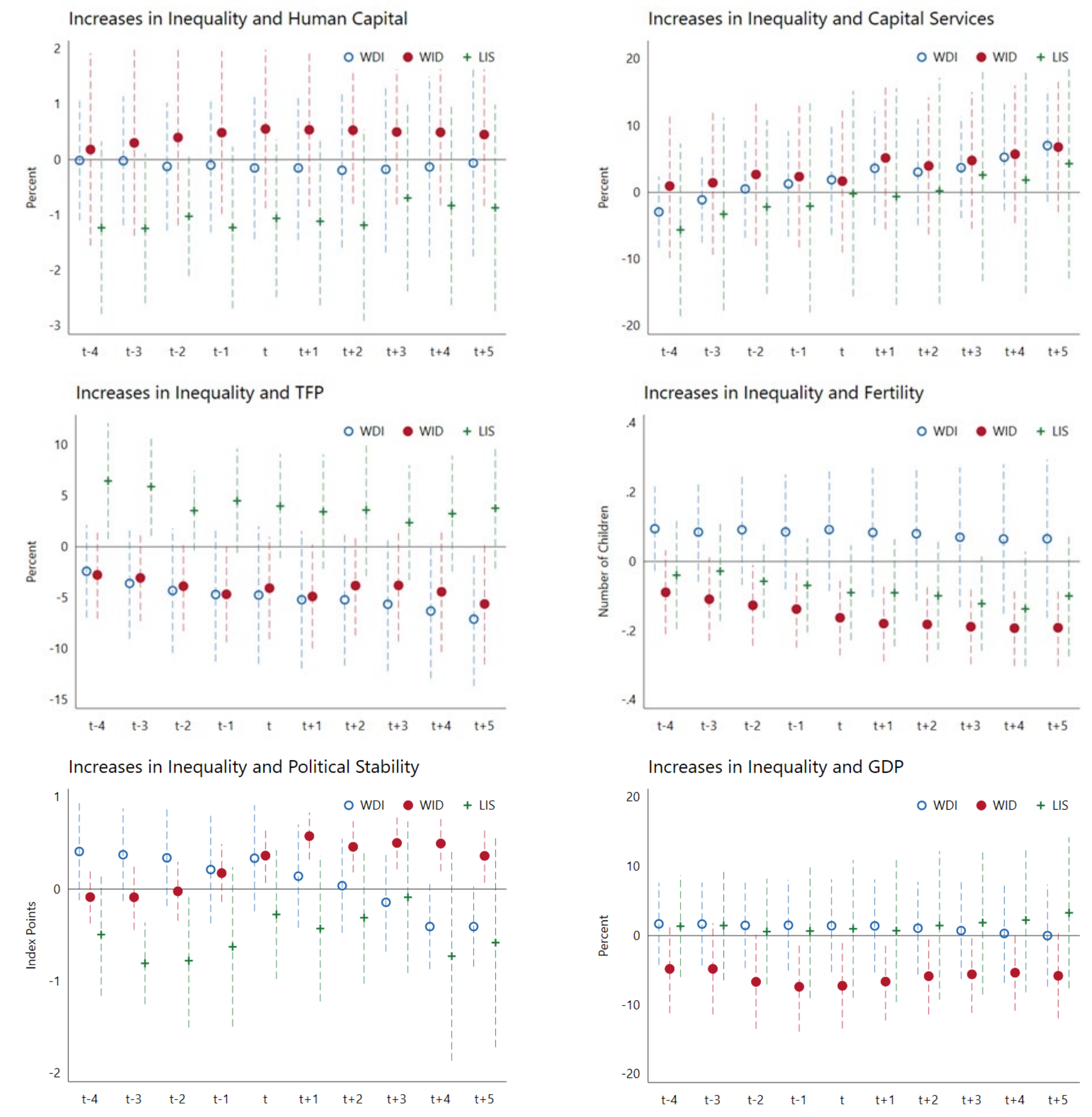
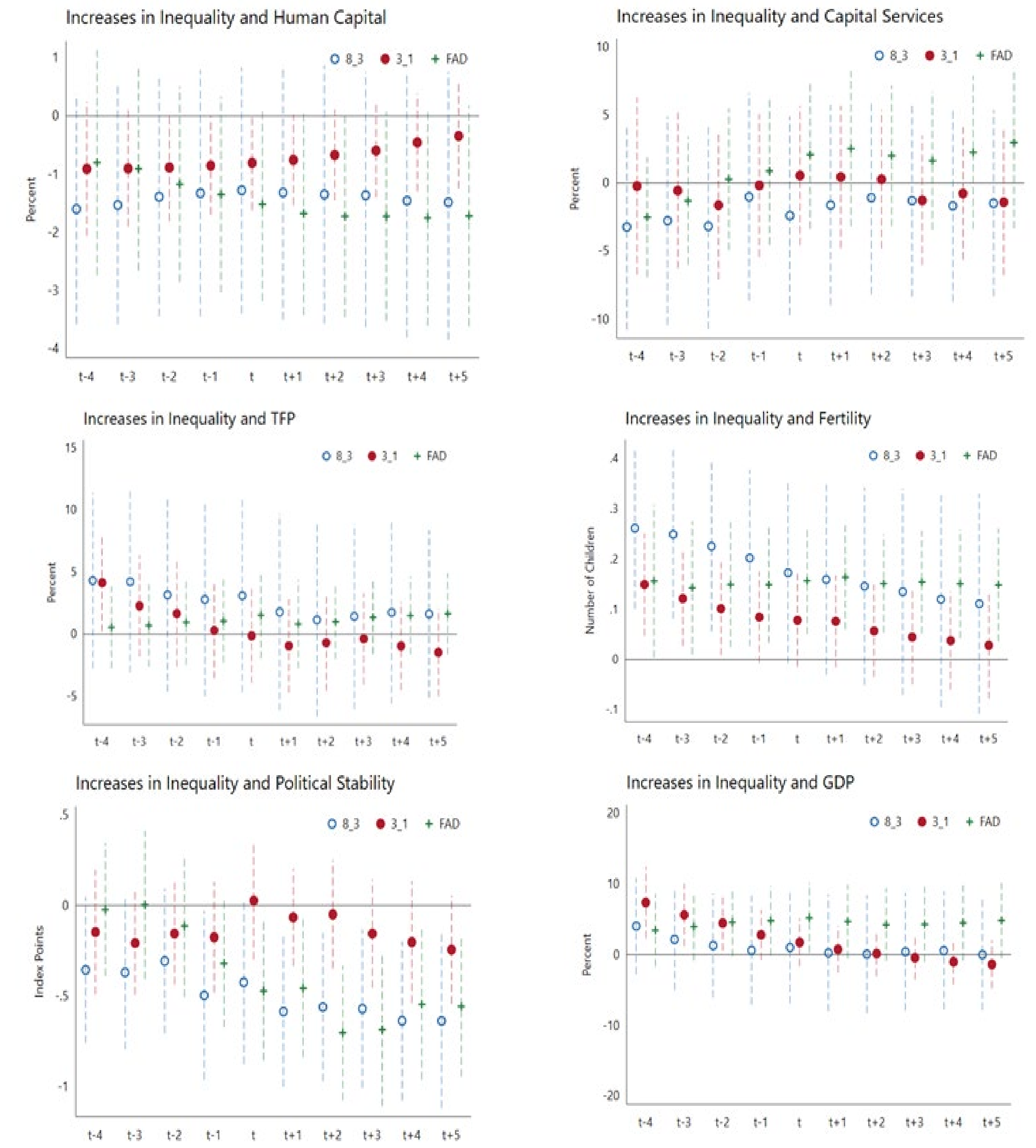


Figure 2.2 Event Study and Transmission Channels—Gini



52. Figure 2.2 repeats the event studies using the three different Gini coefficients. Results are similarly diffuse, with three transmission channels with ‘consistent results’: human capital, fertility, and political stability. For human capital, the estimated relationship at the end of five years after a large increase in inequality is negative (in the range between zero and minus two percent) and insignificant. For fertility and political stability, the point estimates are of the same sign, but their statistical

significance differs. By contrast, the estimated effect on capital services and TFP carry different signs and are all statistically insignificant.

53. A side-by-side inspection of Figures 2.1 and 2.2 reveals the fuzziness of the overall results, the absence of a clear pattern. The estimated relationships vary considerably from one indicator to the next. If pressed, we would summarize the event study as follows: (i) inequality exhibits no strong relationship with human capital (while all coefficients are negative, none of them is statistically significant); (ii) inequality may be negatively related to TFP and political stability (each channel with two significant coefficients), although much depends on the specific indicator chosen and switching indicators would either lead to the vanishing of the significant relationship or even point in the opposite direction; and (iii) the results for capital services and fertility are inconclusive (signs, magnitude, and statistical significance vary across the three Top10 income shares and three Ginis).

54. There are two arguments to be skeptical of taking the results of the event study at face value. The first has to do with the interpretation of the underlying economic theory. Inequality can take many forms: poverty, shrinking middle class, concentrated top incomes, to name a few. Different realizations of inequality could have different economic consequences. It would then be misguided to search for similar effects when using different inequality indicators. Voitchovsky's (2005) finds that income inequality at the top of the income distribution boosts economic growth, whereas bottom inequality acts as a break. Similarly, as discussed in Section II, some of the theory of inequality posits an effect on economic growth conditional on a specific definition of income. Human capital accumulation, for example, should react to people's disposable income, not pre-tax market income (Knowles, 2005).

55. However, Figures 2.1 and 2.2 do not provide strong grounds for these theoretical concerns. We neither see a systematic difference between the Top10 income shares and the Gini, nor between indicators based on different income concepts. To hone in on lack of a systematic difference between the two indicator groups, consider the simple average of the estimated impact after five years for the three Top10 income shares and the three Ginis in each transmission channel. These simple averages are always very close to each other. We do not discern a pattern whereby Top10 income shares would stimulate the transmission channels and the Gini fails to do so. For illustrative purposes only (and being cognizant that the standard assumptions for this exercise may be violated), we conduct an ANOVA exercise to decompose the variation in the five-year coefficients. Comparing all the five-year coefficients from using the Top10 income shares (equal to 15, the number of indicators times the number of transmission channels) to the coefficients from using the Gini, we find that the variation *within* groups of indicators (comparing one Top10 income share to another) trumps the variation *between* them (comparing the Top10 income share to the Gini): *within* group variation accounts for 98 percent of the total variation (the full ANOVA tables are in the Online Appendix).

56. The second argument for skepticism concerns methodology weaknesses of the event study. The results could suffer from reverse causality or omitted variables bias. As noted above, establishing causality in the event study requires the point estimates of the γ_j not to be different from zero in the years prior to the event—this is a visual interpretation of the 'common trends' assumption in difference-in-difference models. Some of the significant point estimates at five years in the models of fertility and political stability appear visually as the continuation of developments that started already

at the beginning of the event window (though statistically insignificant), before the change in inequality. Different evolutions of the transmission channels in the pre-event period indicate that the 'common trends' assumptions may be violated. The statistical significance after five years may really reflect structural difference between countries, and not the effect of rising inequality.

57. As a final remark on the event study, Figures 2.1 and 2.2 illustrate Voitchovsky's (2009) supposition that inequality exerts positive and negative influences on economic activity at the same time. To make this point more formally, we show results of the event study using real GDP per capita as the dependent variable in equation (1) in the bottom right-hand corner of Figures 2.1 and 2.2. The estimated five-year coefficients are insignificant for most indicators. Yet an insignificant relationship between inequality and growth can hide significantly positive and negative effects on individual transmission channels. For example, the point estimate at the end of the event window of the impact of the WDI Top10 income share on real GDP is about zero. But at the same time, Figure 2.1 indicates a significantly positive impact on capital services (top right) and significantly negative impact on TFP (mid-left).

58. Conversely, a (marginally) statistically significant association with real GDP per capita does not necessarily reflect the aggregation of individual effects at the level of the transmission channels. The FAD Gini has a marginally significant positive relationship with real GDP per capita (Figure 2.2), but the statistically significant effects on the transmission channels (positive on fertility and negative on political stability) would suggest that the aggregate effect would be negative. It follows that it is not straightforward to aggregate the individual impact on the transmission channels to arrive at a net impact on overall economic growth for individual countries. Intuitively, the reason why aggregation fails is that importance of the transmission channels varies from country to country and over time (see Neves and Silva, 2014; Neves et al., 2016; and Hsieh and Klenow, 2010). To be meaningful, the aggregation method would have to be country-specific, reflecting the importance of each transmission channel in each country at each point in time. We leave this topic for future research.

Weighted-Average Least Squares

59. Tables 9.1 and 9.2 present the results from the WALS regressions. Overall, the WALS regressions paint a similar picture as the event study: neither the Top10 income shares nor the Gini exhibit a robust empirical relationship with the transmission channels. In fact, only three (out of ten) indicator-transmission channel combinations yield 'consistent' results (in the sense defined in paragraph 50, using the estimated WALS coefficient instead of the five-year coefficient of the event study): the Top10 income shares and TFP (negative and insignificant), the Gini and human capital (negative and one significant coefficient), and the Gini and capital services (negative and one significant coefficient). It follows that differences within each group of indicators are predominant: the one-way ANOVA decomposition attributes 95 percent of the total variation to *within* group variation, using the estimated coefficients of the lagged inequality indicator from the WALS regressions. Moreover, compared to the other possible determinants of the transmission channels (the 'auxiliary regressors'), inequality seems to have relatively weak explanatory power. Other variables in the list of 'auxiliary regressors' appear more frequently as a 'robust determinant', implying greater predictive power for changes in the transmission channels.

Table 9.1 Weighted-Average Least Squares Results—Top10 Income Share

	ΔHuman Capital			ΔCapital Services			ΔTFP		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	WID	WDI	LIS	WID	WDI	LIS	WID	WDI	LIS
Inequality change (t-10 to t-5)	0.06	-0.30	0.40	-2.93	-2.85	1.84	-0.53	-1.10	-2.24
	(0.28)	(-0.69)	(0.36)	(-1.17)	(-0.74)	(0.14)	(-0.65)	(-0.51)	(-0.30)
Inequality change (t-10 to t-5) * GDP per capita level, log (t-5)	-0.00	0.00	-0.00	0.00	0.00	-0.00	0.00	0.00	0.00
	(-0.29)	(0.68)	(-0.34)	(1.05)	(0.78)	(-0.11)	(0.66)	(0.46)	(0.20)
GDP per capita level, log (t-5)	0.03	0.02	-0.01	-0.70**	-0.25	-0.06	0.39***	0.01	0.06*
	(0.93)	(0.39)	(-1.04)	(-2.06)	(-0.59)	(-1.06)	(3.03)	(0.02)	(1.75)
GDP per capita level, log squared (t-5)	-0.13	-0.09		3.86**	0.98		-2.36***	0.10	
	(-0.79)	(-0.34)		(2.00)	(0.40)		(-3.22)	(0.07)	
Price level of investment goods, (t-5)	-0.59	-3.54*	-1.03	-12.99	-27.24	-12.11	-4.06	-4.93	-2.79
	(-0.35)	(-1.77)	(-1.02)	(-0.83)	(-1.63)	(-1.05)	(-0.68)	(-0.49)	(-0.42)
Commodity Terms of Trade Index change, (t-10 to t-5)	-0.01	-0.04	-0.00	-0.15	0.62	0.99	0.19**	0.07	-0.51
	(-0.24)	(-0.43)	(-0.05)	(-0.56)	(0.97)	(1.46)	(2.03)	(0.17)	(-1.36)
Share of government consumption, PPP, (t-5)	-2.12	6.34		85.73*	72.06		7.98	0.74	
	(-0.45)	(1.29)		(1.91)	(1.63)		(0.43)	(0.03)	
Share of gross capital consumption, PPP, (t-5)	-0.60	-5.00		56.72	51.54		25.49	-27.92	
	(-0.15)	(-1.10)		(1.42)	(1.38)		(1.61)	(-1.28)	
Inflation, (t-10 to t-5)	0.00	0.00	0.02	-0.01*	-0.00	-0.07	0.01***	0.00	0.20*
	(0.20)	(1.21)	(1.23)	(-1.78)	(-0.02)	(-0.41)	(3.73)	(0.28)	(2.08)
Electoral democracy index, (t-5)	8.92	-8.70		8.10	-43.93		-27.83	-24.73	
	(1.28)	(-1.29)		(0.14)	(-0.71)		(-1.12)	(-0.72)	
Electoral democracy index, squared, (t-5)	-10.93	2.83		3.35	45.69		22.67	16.82	
	(-1.64)	(0.43)		(0.06)	(0.76)		(0.99)	(0.49)	
Rule of Law index, (t-5)	0.27	5.02**		-25.95	2.78		13.65*	-4.82	
	(0.14)	(2.25)		(-1.39)	(0.18)		(1.84)	(-0.51)	
Trade Openness (t-5)	-0.01	0.03*		0.14	-0.02		0.09	-0.04	
	(-0.72)	(1.79)		(0.78)	(-0.14)		(1.33)	(-0.45)	
Financial Openness (t-5)	0.00	-0.00		-0.00	0.00		-0.00	0.00	
	(1.51)	(-1.58)		(-0.54)	(0.02)		(-1.06)	(0.71)	
Financial Development index (t-5)	-3.73	4.51	-3.58	15.86	-6.40	3.21	-17.35	1.41	1.04
	(-1.00)	(0.94)	(-1.70)	(0.47)	(-0.14)	(0.13)	(-1.49)	(0.06)	(0.07)
Crisis Dummy (t-5)	-0.09	0.40	-0.69	-10.22	-10.61*	-2.58	3.66	-3.47	2.07
	(-0.13)	(0.64)	(-1.55)	(-1.42)	(-1.84)	(-0.52)	(1.38)	(-1.15)	(0.73)
Constant	-9.19	-1.69	8.98*	324.51**	173.20	87.11	-162.38***	10.38	-56.35*
	(-0.74)	(-0.09)	(2.03)	(2.23)	(0.94)	(1.66)	(-2.94)	(0.10)	(-1.87)
Observations	95	87	26	84	88	26	79	83	26

Notes: t-stats in parentheses.

Table 9.1 Weighted-Average Least Squares Results—Top10 Income Share (concluded)

	ΔFertility			ΔPolitical Stability		
	(1)	(2)	(3)	(1)	(2)	(3)
	WID	WDI	LIS	WID	WDI	LIS
Inequality change (t-10 to t-5)	0.01	-0.03	0.05	-0.05	**0.86****	-0.98
	(0.98)	(-0.74)	(0.41)	(-0.35)	**(2.22)**	(-0.43)
Inequality change (t-10 to t-5) * GDP per capita level, log (t-5)	-0.00	0.00	-0.00	0.00	**-0.00****	0.00
	(-0.87)	(0.66)	(-0.34)	(0.33)	**(-2.26)**	(0.35)
GDP per capita level, log (t-5)	-0.00	-0.00	0.00	0.04*	**-0.06***	0.01
	(-0.30)	(-0.06)	(0.10)	(1.71)	**(-1.81)**	(0.68)
GDP per capita level, log squared (t-5)	0.01	0.01		-0.25*	**0.35***	
	(0.57)	(0.39)		(-1.73)	**(1.74)**	
Price level of investment goods, (t-5)	0.00	-0.11	-0.10	**-0.87**	**-2.81****	-0.84
	(0.01)	(-0.73)	(-0.79)	**(-0.75)**	**(-2.25)**	(-0.41)
Commodity Terms of Trade Index change, (t-10 to t-5)	-0.00	-0.00	0.00	**-0.03***	-0.03	**-0.15**
	(-0.59)	(-0.48)	(0.70)	**(-1.68)**	(-0.67)	**(-1.33)**
Share of government consumption, PPP, (t-5)	0.16	**0.49**		-4.03	-1.20	
	(0.60)	**(1.31)**		(-1.16)	(-0.44)	
Share of gross capital consumption, PPP, (t-5)	0.18	**-0.36**		-3.90	**-4.69***	
	(0.69)	**(-1.19)**		(-1.37)	**(-1.94)**	
Inflation, (t-10 to t-5)	**0.00**	-0.00	**-0.00****	0.00	**0.00****	**0.07****
	(0.67)	(-0.49)	**(-2.72)**	(0.14)	**(2.22)**	**(2.31)**
Electoral democracy index, (t-5)	**0.55**	**0.77**		-5.35	1.56	
	(1.35)	**(1.34)**		(-1.21)	(0.37)	
Electoral democracy index, squared, (t-5)	-0.32	**-0.75**		3.91	**-5.04**	
	(-0.80)	**(-1.34)**		(0.91)	**(-1.27)**	
Rule of Law index, (t-5)	**-0.01**	-0.13		1.02	**3.53****	
	(-0.05)	(-0.91)		(0.75)	**(2.45)**	
Trade Openness (t-5)	**-0.00**	-0.00		0.00	0.01	
	(-0.15)	(-0.19)		(0.05)	(0.78)	
Financial Openness (t-5)	**-0.00**	0.00		-0.00	**-0.00**	
	(-0.20)	(0.02)		(-0.41)	**(-1.29)**	
Financial Development index (t-5)	0.17	**0.60**	**0.93*****	**-1.92**	**-3.56**	-2.13
	(0.74)	**(1.57)**	**(3.53)**	**(-0.70)**	**(-1.32)**	(-0.48)
Crisis Dummy (t-5)	0.01	-0.05	**0.08**	0.84	**0.48**	-0.70
	(0.26)	(-0.96)	**(1.34)**	(1.63)	**(1.46)**	(-0.79)
Constant	-0.45	-0.64	-0.10	-15.50	**28.99***	-6.51
	(-0.58)	(-0.42)	(-0.20)	(-1.43)	**(1.93)**	(-0.71)
Observations	103	93	26	93	81	26

Notes: t-stats in parentheses.

Table 9.2 Weighted-Average Least Squares Results—Gini

	ΔHuman Capital			ΔCapital Services			ΔTFP		
	(1)	(2)	(4)	(1)	(2)	(4)	(1)	(2)	(4)
	8_3	3_1	FAD	8_3	3_1	FAD	8_3	3_1	FAD
Inequality change (t-10 to t-5)	-0.30	-0.19	-0.03	-8.88	-1.77	-0.39	-5.15	-0.38	2.62
	(-0.34)	(-1.05)	(-0.06)	(-0.99)	(-1.10)	(-0.11)	(-1.51)	(-0.30)	(1.27)
Inequality change (t-10 to t-5) * GDP per capita level, log (t-5)	0.00	0.00	0.00	0.01	0.00	0.00	0.01	0.00	-0.00
	(0.17)	(0.95)	(0.10)	(0.97)	(0.82)	(0.10)	(1.58)	(0.12)	(-1.22)
GDP per capita level, log (t-5)	0.03	0.12***	0.06	-0.09	0.52**	-0.50	-0.01	-0.51***	-0.13
	(0.74)	(3.54)	(1.08)	(-0.32)	(2.09)	(-1.12)	(-0.06)	(-2.89)	(-0.58)
GDP per capita level, log squared (t-5)	-0.15	-0.75***	-0.35	0.13	-3.30**	2.22	0.03	3.14***	0.71
	(-0.75)	(-3.52)	(-1.12)	(0.07)	(-2.16)	(0.87)	(0.06)	(2.88)	(0.56)
Price level of investment goods, (t-5)	-1.74	2.46**	-0.76	-15.92	-9.91	-12.39	-8.19*	-16.96**	0.40
	(-1.32)	(2.59)	(-0.39)	(-1.32)	(-0.99)	(-0.75)	(-1.92)	(-2.61)	(0.05)
Commodity Terms of Trade Index change, (t-10 to t-5)	-0.07	0.08*	0.04	-0.33	0.28	0.27	0.11	-0.01	-0.02
	(-1.39)	(1.96)	(0.50)	(-0.65)	(0.81)	(0.42)	(0.61)	(-0.04)	(-0.06)
Share of government consumption, PPP, (t-5)	2.13	4.10	2.31	-8.95	11.74	37.83	17.60	-4.15	-21.71
	(0.52)	(1.14)	(0.47)	(-0.28)	(0.47)	(0.92)	(1.35)	(-0.26)	(-1.06)
Share of gross capital consumption, PPP, (t-5)	-0.54	7.17**	-1.02	45.80*	68.43***	66.00	28.74***	-23.24	-35.88*
	(-0.19)	(2.26)	(-0.22)	(1.85)	(2.88)	(1.61)	(2.94)	(-1.36)	(-1.74)
Inflation, (t-10 to t-5)	0.00	-0.00*	0.00	-0.00	-0.01**	0.00	-0.00	0.01***	-0.00
	(1.29)	(-1.91)	(1.07)	(-0.34)	(-2.56)	(0.02)	(-0.10)	(3.79)	(-0.00)
Electoral democracy index, (t-5)	-4.99	-7.35	-2.22	3.42	-38.68	-24.95	-47.51**	5.15	-26.62
	(-0.90)	(-1.56)	(-0.33)	(0.07)	(-1.21)	(-0.44)	(-2.17)	(0.22)	(-0.86)
Electoral democracy index, squared, (t-5)	3.61	1.76	-0.37	1.00	31.80	23.16	51.70**	-10.78	14.63
	(0.67)	(0.36)	(-0.06)	(0.02)	(1.01)	(0.42)	(2.44)	(-0.46)	(0.47)
Rule of Law index, (t-5)	-0.04	1.82	2.00	0.86	1.22	5.46	-16.43***	-2.24	1.99
	(-0.03)	(1.56)	(0.84)	(0.07)	(0.12)	(0.31)	(-2.78)	(-0.32)	(0.20)
Trade Openness (t-5)	0.00	-0.01	0.01	0.02	0.17	0.01	-0.12***	-0.07	-0.06
	(0.22)	(-0.63)	(0.71)	(0.14)	(1.59)	(0.09)	(-3.16)	(-1.15)	(-0.88)
Financial Openness (t-5)	-0.00	0.00	-0.00	0.01	0.02	-0.00	0.00	-0.01	0.01
	(-0.42)	(0.37)	(-0.54)	(0.89)	(1.39)	(-0.17)	(0.56)	(-0.88)	(1.43)
Financial Development index (t-5)	1.27	4.74	2.89	-10.36	9.90	-5.89	5.08	18.57	19.16
	(0.34)	(1.25)	(0.68)	(-0.33)	(0.37)	(-0.16)	(0.43)	(0.95)	(1.01)
Crisis Dummy (t-5)	0.03	-0.14	0.16	-7.31	-2.76	-6.05	-3.03*	-2.42	-1.61
	(0.06)	(-0.26)	(0.25)	(-1.56)	(-0.65)	(-1.10)	(-1.78)	(-0.89)	(-0.60)
Constant	-3.75	-44.51***	-18.79	98.92	-179.49*	282.91	17.59	223.15***	81.04
	(-0.26)	(-3.27)	(-0.81)	(0.79)	(-1.77)	(1.45)	(0.40)	(3.14)	(0.83)
Observations	116	155	95	109	127	96	103	118	90

Notes: t-stats in parentheses.

Table 9.2 Weighted-Average Least Squares Results—Gini (concluded)

	ΔFertility			ΔPolitical Stability		
	(1)	(2)	(4)	(1)	(2)	(4)
	8_3	3_1	FAD	8_3	3_1	FAD
Inequality change (t-10 to t-5)	-0.03	0.01	**-0.04**	-0.18	0.07	0.01
	(-0.62)	(0.87)	**(-1.27)**	(-0.26)	(0.46)	(0.05)
Inequality change (t-10 to t-5) * GDP per capita level, log (t-5)	0.00	**-0.00**	**0.00**	0.00	-0.00	0.00
	(0.48)	**(-1.03)**	**(1.14)**	(0.36)	(-0.37)	(0.02)
GDP per capita level, log (t-5)	**0.00**	**-0.01***	0.00	**0.04**	-0.01	-0.03
	(1.09)	**(-1.88)**	(0.45)	**(1.40)**	(-0.31)	(-0.92)
GDP per capita level, log squared (t-5)	-0.01	**0.04****	-0.00	**-0.21**	0.04	0.19
	(-0.53)	**(2.17)**	(-0.09)	**(-1.20)**	(0.24)	(0.91)
Price level of investment goods, (t-5)	**-0.12**	**-0.10**	-0.13	**-1.34**	0.36	**-2.37***
	(-1.45)	**(-1.19)**	(-0.94)	**(-1.19)**	(0.44)	**(-1.69)**
Commodity Terms of Trade Index change, (t-10 to t-5)	0.00	0.00	-0.00	**-0.04**	**-0.07****	-0.00
	(0.43)	(0.47)	(-0.28)	**(-1.01)**	**(-2.15)**	(-0.00)
Share of government consumption, PPP, (t-5)	0.14	0.21	**0.49**	**-5.63***	**-4.28**	-3.09
	(0.60)	(0.80)	**(1.41)**	**(-1.92)**	**(-1.52)**	(-0.96)
Share of gross capital consumption, PPP, (t-5)	**-0.36***	**-0.70****	**-0.49**	-2.40	**4.36**	**-4.48**
	(-1.97)	**(-2.45)**	**(-1.39)**	(-0.97)	**(1.64)**	**(-1.41)**
Inflation, (t-10 to t-5)	0.00	**0.00**	0.00	-0.00	0.00	0.00
	(0.05)	**(1.24)**	(0.07)	(-0.24)	(0.16)	(0.18)
Electoral democracy index, (t-5)	**-0.70***	**0.89****	0.26	-1.22	-2.08	-2.64
	(-1.88)	**(2.18)**	(0.54)	(-0.28)	(-0.53)	(-0.54)
Electoral democracy index, squared, (t-5)	**0.62***	**-0.73***	-0.17	-2.62	1.07	-0.51
	(1.72)	**(-1.73)**	(-0.36)	(-0.66)	(0.27)	(-0.11)
Rule of Law index, (t-5)	-0.06	0.02	-0.08	**2.05**	0.18	**2.92**
	(-0.70)	(0.18)	(-0.55)	**(1.55)**	(0.20)	**(1.49)**
Trade Openness (t-5)	-0.00	-0.00	0.00	**0.01**	-0.00	0.00
	(-0.51)	(-0.92)	(0.15)	**(1.13)**	(-0.36)	(0.35)
Financial Openness (t-5)	**0.00**	-0.00	0.00	-0.00	-0.00	-0.00
	(1.25)	(-0.06)	(0.07)	(-0.48)	(-0.93)	(-0.87)
Financial Development index (t-5)	0.15	0.30	**0.65****	**-4.33**	-2.73	0.73
	(0.67)	(0.87)	**(2.20)**	**(-1.52)**	(-0.93)	(0.28)
Crisis Dummy (t-5)	-0.01	-0.02	-0.04	**0.52**	**0.67**	0.13
	(-0.24)	(-0.50)	(-0.78)	**(1.26)**	**(1.49)**	(0.33)
Constant	**-1.30**	**1.51**	-1.25	**-17.73**	4.21	**16.64**
	(-1.52)	**(1.28)**	(-0.89)	**(-1.41)**	(0.45)	**(1.06)**
Observations	124	166	101	113	150	90

Notes: t-stats in parentheses.

60. In Table 9.1 that presents results for the Top10 income shares, the point estimates of the effect of inequality do not move above the 'robust' threshold in the regressions of human capital, TFP, and fertility. Interestingly, the WALS results do not replicate any of the significant relationships found in the event study. The (marginally) significant relationships in the event study between: (i) WDI and WID and TFP; (ii) WID and fertility; and (iii) WID and political stability, all become insignificant. The point estimate reaches the 'robust' level of a t-statistic larger than one in the models that link: (i) negatively the WID Top10 income share and capital services; and (ii) positively the WDI Top10 income share and political stability (which was marginally negative in the event study). However, the other two Top10 income shares do not mirror the statistically significant associations with capital services and political stability; in fact, at least one (capital services), if not two (political stability), of the other estimates carry the opposite sign of the 'robust' estimate.

61. Table 9.2 presents the WALS results for the Gini. At least one variant of the Gini enters significantly in four out of the five transmission channels. However, the t-statistics of the 'robust associations' are generally just above one, indicating a probability of just over fifty percent that inequality (as measured by the Gini) belongs into the models of the transmission channels. Political stability is the only transmission channel without a significant association with the Gini, which stands in contrast to Figure 2.2, which displayed two significant associations. Human capital and capital services exhibit 'consistent' results, with all coefficients carrying a negative sign. In both instances, the SWIID Gini (v3.1, whereas v8.3 remains insignificant) moves above the 'robust' threshold. This finding lends some partial support to the event study, which found the same 'consistent' results for the Gini and human capital (negative, albeit without any significant relationship). The results for TFP seem difficult to read at a cursory glance, as the two significant associations carry the opposite signs and suggest opposite effects as countries become richer (more below). The relationship between the Gini and fertility is negative and statistically significant when using the FAD Gini, but close to zero with the other two.

62. Comparing Tables 9.1 and 9.2 reinforces the finding from Figures 2.1 and 2.2: the differences in results *within* the group of Top10 income and Gini indicators is predominant. Each variant of the Top10 income share or the Gini can, on its own, give rise to unique empirical relationships with the transmission channels. By implication, given the wide dispersion in estimated coefficients within both groups of indicators, the WALS regression do not suggest a systematic difference between Top10 income shares and the Gini. Repeating the exercise in the event study of computing the simple average of the three estimated coefficients in each transmission channel, we find again that the averages are relatively close to each other. The capital services regressions are the only instance of a 'robust' association with a variant of both the Gini and the Top10 income share, although the majority of the indicators suggest an insignificant relationship in this transmission channel as well.

63. Tables 9.1 and 9.2 further underscore the importance of accounting for countries' stage of development. The 'robust' associations between inequality and the transmission channels

comprise, with two exceptions, a significant interaction term between inequality and GDP per capital (we note that the interpretation of interaction terms in WALS regressions is not straightforward; Cade, 2015). A significant interaction term suggests that inequality's total effect on the transmission channels changes as countries grow richer. The negative impact of the WID Top10 income share on TFP, for instance, turns positive for richer countries—countries whose per capita income exceeds $3,300 (expressed at current PPP), respectively. However, the inverse relationship holds for the estimated association between the FAD Gini and TFP. In this case, increases in the Gini seem to stimulate TFP growth in poorer countries, but when per capita income exceeds $11,000, the effect turns negative.

64. Looking at the entire set of WALS results together, we do not see how WALS regressions can settle the question of whether and how strongly inequality is related to the transmission channels—despite the method's claim to "let the data speak." The Top10 income shares and the Gini generally have lower explanatory power for changes in the transmission channels than many other variables commonly used in the empirical growth literature. Inequality does emerge as a 'robust determinant' in a few specifications, but it turns out that these specifications are not robust to a change in compilation methodology of the underlying inequality indicator.[6]

Pooled Mean-Group Regressions

65. Table 10 presents the short and long-run estimates of the effect of the Gini (SWIID v83) on the transmission channels. The short-run coefficient is the unweighted mean of the estimated country-specific coefficients, whereas the long-run effect is jointly estimated with information from all countries.

66. Overall, the Pooled Mean-Group Regressions (PMG) results reinforce the conclusion from the previous empirical approaches: inequality does not display a significantly robust long-term relationship with the transmission channels. The second striking result is that short-run and long-run effects are markedly different. For the full sample of countries, the short-run effect is positive for TFP, whereas the long-run effect is never significant. One possible interpretation would be to read these results as supportive of Halter et al. (2014), who emphasize that the positive effects of inequality, to the extent they are present, are clustered in the short term, with the negative effects observed farther away in time. But Table 8 also indicates a more immediate reason why the estimates of the full sample may be problematic—the effects of inequality differ significantly across country groups.

67. Table 10 emphasizes the importance of country-income levels by presenting results for advanced and emerging-market and low-income economies separately (echoing Kuznets, 1955).

[6] In additional results in the Online Appendix, we confirm our conclusions about the predominance of variation *within* groups of inequality indicators and the weak explanatory power of inequality. We perform the following robustness checks: (i) use 10-year horizons to define 'large changes' in inequality (event study and WALS); (ii) include poverty in the list of 'auxiliary variables' (WALS), which significantly reduces the sample size; and (iii) separately consider large increases and large decreases in inequality (WALS).

Compared to the full sample, a new significant negative long-run association emerges between inequality and the level of capital services (investment) in advanced economies. The short-run effects of inequality tend to be positive if they are significant: that is the case for human capital and fertility in advanced economies (AEs) and capital services in emerging-market and developing economies (EMDEs). Overall, while the results support the idea that inequality travels through different transmission channels as countries develop, they also underscore the difficulty of drawing strong conclusions about the relationship between inequality and stages of development, as all estimated short-run effects seem to wane, or even reverse, over longer time horizons.

Table 10. Pooled Mean-Group Regressions Results

	ΔHuman Capital			ΔCapital Services			ΔTFP			ΔPolitical Stability			ΔFertility		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	ALL	AE	EMDE	ALL	AE	EMDE	ALL	AE	EMDE	ALL	AE	EMDE	ALL	AE	EMDE
Gini (t-5), long-run effect	-0.05	-0.02	-0.06	0.00	**-0.035***	-0.01	0.33	0.43	0.47	0.17	-0.10	0.29	-0.42	-1.09	0.06
Gini (t-5), short-run effect	0.00	**0.06****	-0.01	0.03	-0.01	**0.055****	**0.587****	0.18	0.52	-0.04	-0.31	0.15	0.67	**1.628***	0.35
Observations	3616	845	2771	3263	845	2418	3119	845	2274	2562	604	1958	3777	845	2932
R-squared	0.30	0.20	0.33	0.23	0.19	0.22	0.20	0.17	0.22	0.18	0.15	0.21	0.23	0.28	0.26
Number of groups	127	23	104	118	23	95	109	23	86	116	23	93	139	23	116

Control variables: GDP per capita level, log (t-5), Price level of investment goods, (t-5)
Notes: *** p<0.01, ** p<0.05, * p<0.1

68. We next use rolling-window PMG estimates to analyze the stability of the long-run parameter of inequality over time. If the effect of inequality on the transmission channels reflects structural behavior of households and firms, we would expect this relationship to be stable over time. Figure 3 displays the results of the rolling-window regressions. Each point on the solid line represents a point estimate of the long-run relationship in equation (3), estimated from an estimation window of 28 years. The dotted lines in red correspond to the 95 percent confidence interval of the point estimate.

Figure 3. Rolling-Window Regressions: Long-Run Impact on Transmission Channels

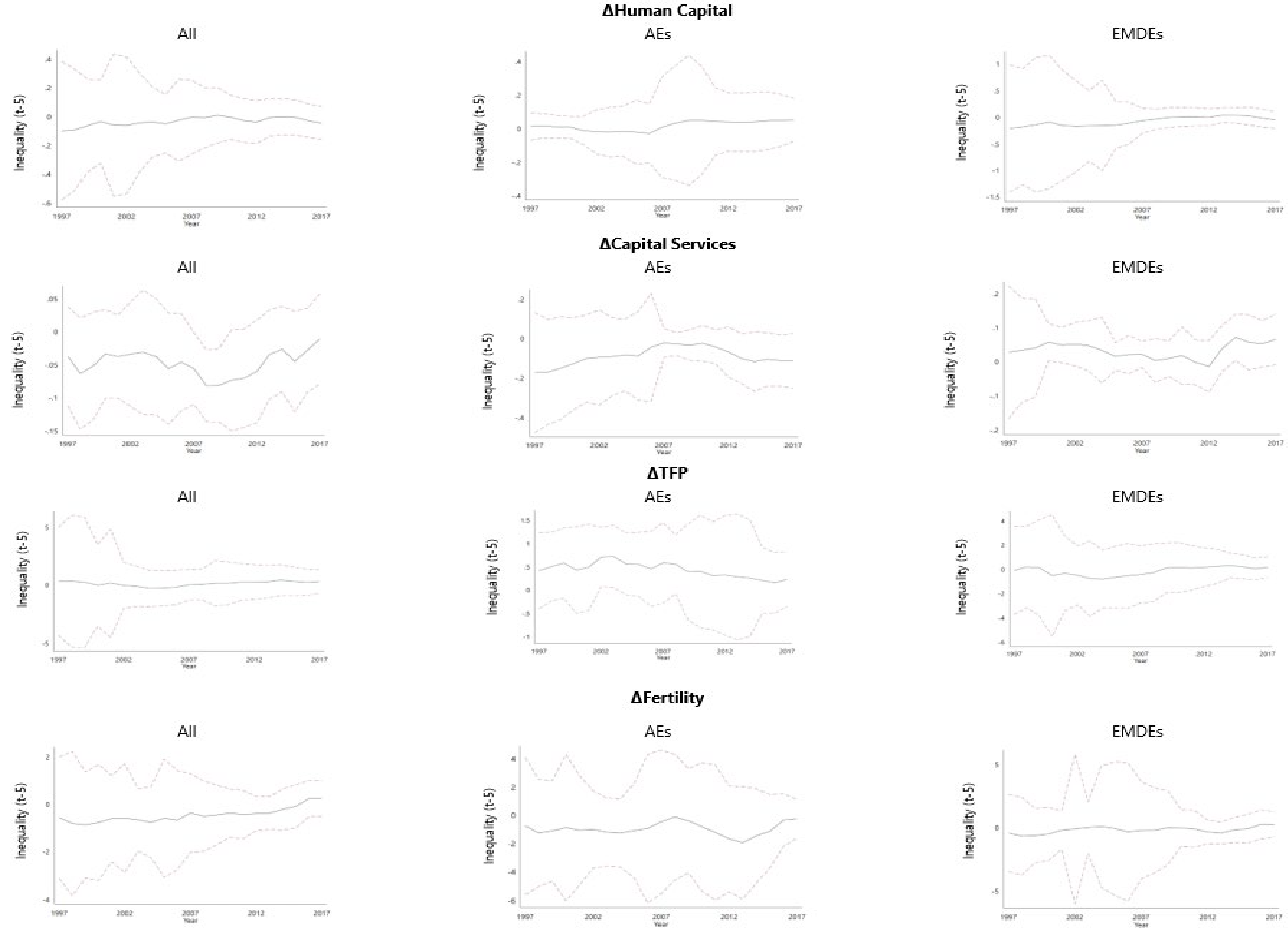
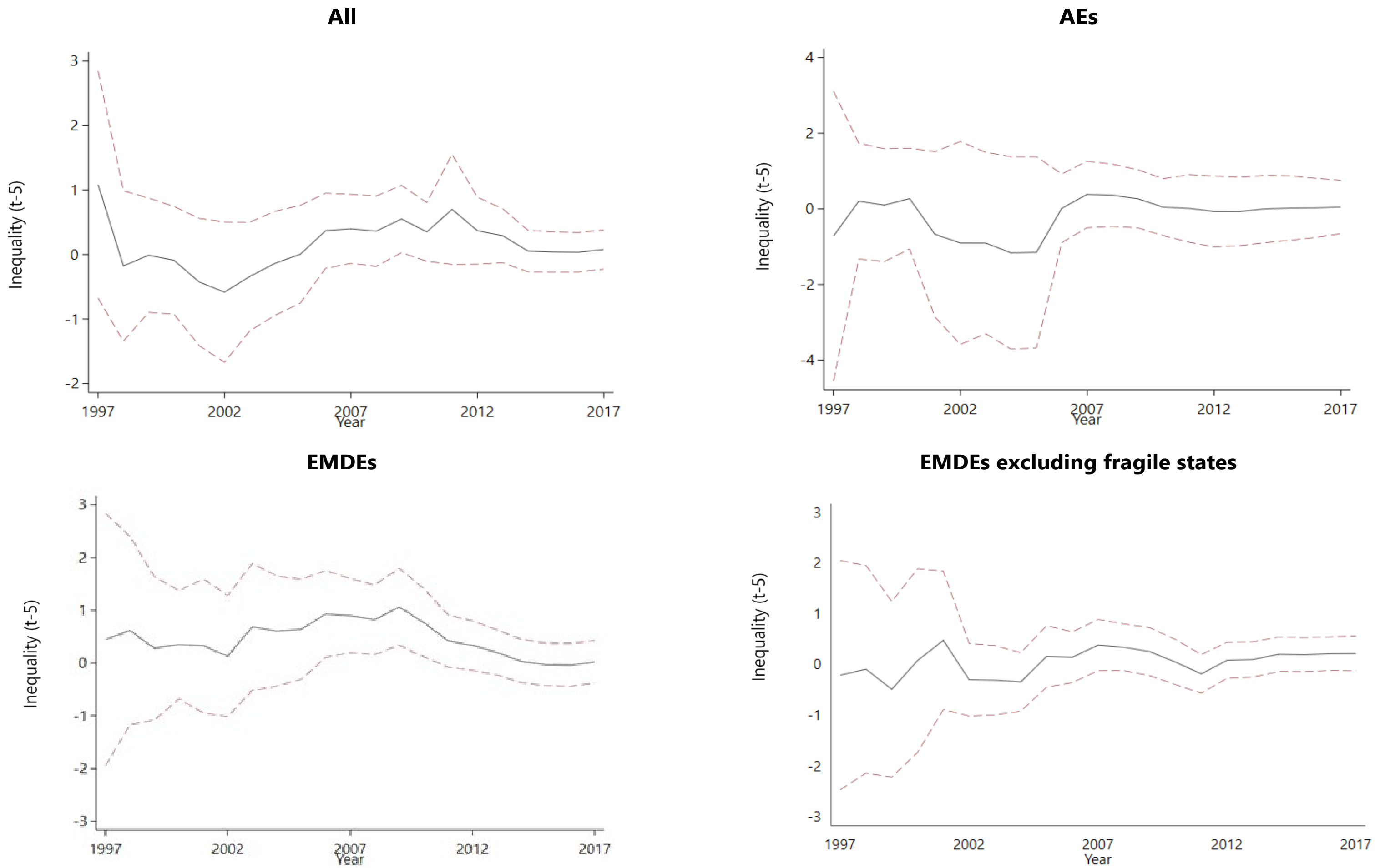


Figure 4. Rolling-Window Regressions: Long-Run Impact on Political Stability



69. Our previous conclusions seem to hold even when using different sample periods. The structural long-run effect of inequality on four transmission channels (i.e., human capital, physical capital, TFP, and fertility) is indistinguishable from zero throughout time. However, there is an interesting twist in Figure 4 showing the results for the impact of inequality on political stability in EMDEs. The estimated effect turns significantly positive in the estimation windows ending in the period of 2006-2011.

70. We dig into this brief positive relationship by identifying the responsible country episodes. We find that the result reflects the co-movement of inequality and political stability in countries that experienced (violent) social conflict in the 2000s. Examples include: DR Congo, Haiti, Côte d'Ivoire, and Zimbabwe. In these countries, inequality worsened amid the social conflict and then persisted at higher levels after the conflict ended. Political stability, however, quickly improves once peace is restored, giving rise to an apparent positive impact of rising inequality on political stability. When we exclude these fragile countries from the estimation sample, the significant relationship between inequality and political stability disappears. This finding underscore possible non-linearities in the relationship between inequality and political stability.

V. Conclusion

71. In this paper, we document that the seemingly innocuous decision of picking an inequality indicator has far-reaching effects on estimated empirical relationships. Significant associations between income inequality and the growth transmission channels that may exist with one particular inequality indicator typically fail to replicate when switching to another one. In fact, we show that empirical relationships are more sensitive to the choice of a particular inequality indicator from a given set of indicators (one particular Gini from the set of available Ginis) than the choice between different inequality indicator sets (Gini versus Top10 income share).

72. One possible reaction is to say that this result is not surprising. All of the indicators in our study measure income inequality differently according to their specific methodological criteria. It could be that only one specific subtype of income inequality (as a random example, inequality in after-tax income between adults only, after adjusting for income from the provision of in-kind public services and owner-occupied housing but excluding monetary unemployment benefits) exerts economic effects, whereas other definitions of income inequality do not.

73. This interpretation would be consistent with the evidence we present, but we do not consider it particularly constructive. We feel that the perils of income inequality need to be rigorously documented, grounded in robust empirical relationships that hold up against changes in indicators, estimation periods, and country samples. Otherwise it will be difficult to calibrate policy responses to inequality with a view to optimize trade-offs between equity and efficiency goals and maximize impact.

74. We also caution against interpreting our results to say that inequality does not matter for economic growth. The adage "absence of evidence is not evidence of absence" holds true. Our paper confronts the same limitations that has beleaguered the empirical economics literature generally, and the inequality-growth literature in particular: low statistical power (Ioannidis et al., 2017). Although we use a multitude of empirical methods and data sources, we cannot be sure to conclude that our specific estimates have sufficient power to discriminate between a non-existing effect and an effect possibly observable only in specific circumstances. Cross-country datasets at annual frequencies pose a binding constraint on statistical power.

75. The way forward passes through more and better data. The line of action should be to expand the country coverage of existing inequality statistics, ensuring that they are based on comparable household surveys and administrative tax records. More ambitiously, the IMF (2020) recommends intensifying the development of comparable distributional indicators within the Systems of National Accounts (SNA) framework. Ongoing research efforts to update the 2008 SNA is the natural place for these efforts. Compared to household surveys and tax records, SNA have comprehensive and standardized income definitions. Embedding income inequality statistics in national accounts will facilitate international comparability and pave the way towards more robust cross-country empirical work on the economic effects of inequality.

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