*Kyklos*

# Does Economic Freedom Boost Growth for Everyone?

Andreas Bergh and Christian Bjørnskov

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**Abstract:** While the association between economic freedom and long-term economic growth has been well documented, the parallel research literature on the distributional consequences of economic freedom is full of conflicting findings. In this paper, we take a step toward reconciling these two bodies of literature by exploring the within-quintile growth consequences of changes in three separate elements of economic freedom: the size of government, institutional quality and and policy quality. Although the distributional consequences of increases in economic freedom are theoretically ambiguous, we find evidence that economic freedom affects all parts of the income distribution equally, in addition to indications that the growth effects are largest for the poorest and richest quintiles.

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## I. INTRODUCTION

The positive association between economic freedom and economic growth is well-documented (Ayal and Karras 1998; Heckelman and Stroup 2000; Compton et al. 2011; Doucouliagos and Ulubasoglu 2006; Gwartney et al. 1999; Hall and Lawson 2014; Rode and Coll 2012; Williamson et al. 2011; Xu and Li 2008), as is the relation between economic freedom and overall productivity (Klein and Luu 2003; Zhang et al. 2018). Approaches based on Granger causality and using instruments for economic freedom have also suggested that the association at least partly reflects a causal effect of economic freedom on growth (Dawson 2003; Heckelman 2000; Justesen 2008; however, see also Sturm et al. 2002). In addition, there is now a growing body of micro-level evidence of the effects of specific reforms that increase economic freedom, suggesting that such reforms can causally affect firm-level productivity (e.g., Bjuggren 2018; Schivardi and Viviano 2011).

In contrast, the research on the distributional consequences of economic freedom is full of conflicting findings. After research in this field was begun by Berggren (1999), several studies examined the relationship between economic freedom and income equality (Bergh and Nilsson 2010; Carter 2006; Scully 2002). These studies typically reported results that differ depending on the type of economic freedom, the level of development, or other types of heterogeneity (cf. Bennett and Nikolaev 2017). The heterogeneity between democracies and autocracies is of particular interest, given recent political debates. For example, Reich (2009) claims that capitalist institutions and policies that are consistent with economic freedom undermine democracy and equates democratic institutions with substantial redistribution. Similarly, Piketty (2014) claims that capitalism without substantial redistribution and regulatory activity will lead to increased inequality by allowing the richest segments of society to grow economically and politically apart from the rest.

This paper contributes to the field by testing the standard assertion that reforms towards economic freedom only or mainly benefit the rich. As will be shown, this assertion lacks a theoretical foundation. Moreover, a robust association between economic freedom and a unidimensional metric of income inequality such as the Gini coefficient is unlikely to exist if economic freedom relates differently with income growth at different parts of the income distribution. For example, economic freedom could plausibly lead to disproportionally higher incomes at the top of the distribution by increasing the returns to capital or certain skills, while simultaneously favoring low-income earners by lowering barriers to entry and promoting competition. If that is the case, increased economic freedom will change the shape

of the Lorenz curve, as illustrated in Figure 1, with ambiguous consequences for the Gini coefficient (as the Gini coefficient is by definition double the area between the 45 degree line and the Lorenz curve; see e.g. Lambert 1993).

Figure 1. Lorenz curve before (full) and after (dashed) a disproportional income increase at the bottom and at the top.
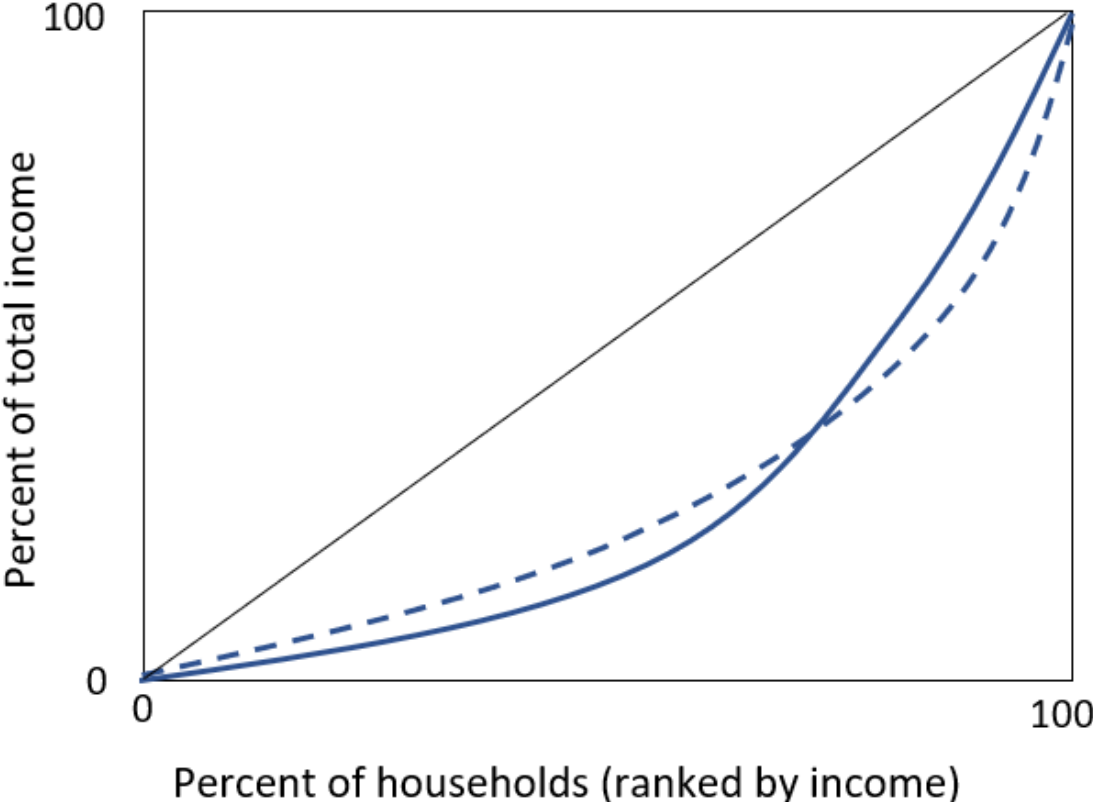


Until recently, reliable global cross-country data on income growth at different points in the income distribution was unavailable. A few studies examined how the Economic Freedom of North America (EFNA) was associated with income growth at different points in the U.S. income distribution. Compton et al. (2014) found that increases in the EFNA index exert a positive and significant impact on the growth of mean household income for the top four quintiles, and a positive but insignificant impact on the bottom income quintile. However, Wiseman (2017) found that increases in economic freedom are associated with larger income growth rates for the bottom 90% of income earners relative to the top 10% (and that the relationship between economic freedom and income inequality is negative and statistically significant). Also analyzing U.S. states, Ashby and Sobel (2008) found that increases in

economic freedom are associated with significantly higher levels of income growth, for the lowest, middle, and highest income quintiles. In general, they found no robust effect of economic freedom on income inequality. The potentially more precise state-level literature thus appears to be almost as mixed as the cross-country studies.

In this paper, we shed new light on these conflicting findings by examining the association between economic freedom and income growth at different points in the income distribution using (for the first time in this context) new data compiled by Lahoti et al. (2016) in the Global Consumption and Income Project (GCIP). The project uses available data from a large set of credible sources on the relative distribution and mean income for a country in each year since the 1960s and fills in the gaps using interpolation. The resulting database allows us to calculate within-quintile national income, and thus also growth rates of within-quintile income for 145 countries since the early 1970s. We match these data with data on economic freedom to estimate whether changes in economic freedom give rise to systematically different income growth rates for individuals within each of the five income quintiles. In general, we find that increases in economic freedom are associated with higher income growth for all quintiles, although there are some signs that the effect for the highest quintile is smaller in autocracies—particularly in autocracies with limited veto institutions.

The paper proceeds as follows: Section 2 provides a set of theoretical considerations, based on previous studies. Section 3 describes the data used in Section 4, where we estimate the growth effects of economic freedom for each quintile. Section 5 concludes the paper.


II. THEORETICAL EXPECTATIONS AND RELATED LITERATURE

A great deal of the research on economic freedom relies on the Economic Freedom of the World Index, now published yearly by the Fraser Institute (Gwartney et al. 2017) and first published in 1995. As indicated by surveys such as those by Berggren (2003) and Hall and Lawson (2014), the consequences of economic freedom have become a rapidly growing research field that includes non-economic outcomes such as tolerance (Berggren and Nilsson 2013), social trust (Berggren and Jordahl 2006), and life satisfaction (Gehring 2013).

While early papers often treated economic freedom as a unidimensional concept, several scholars have discussed the differences between different types of economic freedom (Bergh 2019; Ott 2018; Rode and Coll 2012; Heckelman and Stroup 2005). Perhaps the most important lesson from that line of research is that economic freedom in the sense of having a limited government (area 1) correlates only weakly—and typically negatively—with other

types of economic freedom. There is also an important conceptual difference between the economic freedom of institutions (area 2) and the economic freedom of policies (areas 3 to 5). For these reasons, results based on the aggregate index are often difficult to interpret. In the empirical section, we therefore choose to follow the path taken by more recent studies and distinguish between government size (area 1), institutional quality (area 2), and policy quality (areas 3–5).

A second challenge is that the causal association between a numerical index of economic freedom and economic growth is admittedly difficult to establish beyond the Granger causality tests employed in studies such as those of Dawson (2003) and Justesen (2008), and instrumental variables (IV) estimations such as those in the work of Faria and Montesinos (2009). There are, however, several examples of specific reforms that have been evaluated using sophisticated identification strategies. Although the main purpose of the present paper is to examine whether the association between economic freedom and income growth differs across the income distribution, a discussion of credibly identified evaluations of reforms is a useful starting point—both because such evaluations illustrate what kind of reforms higher economic freedom entails, and because they feed into the discussion on the distributional impact of such reforms. In doing so, we follow the conceptual distinction between components of economic freedom by organizing the discussion around 1) government size, 2) institutional quality, and 3) policy quality.


*1. Area 1—Government size*

Who benefits from limiting or expanding government size? The effects of changes to overall government expenditure depend on the distributional profile of the funds allocated by the public sector, and on the dynamic effects of the changes. One might expect public expenditure to be targeted toward low-income earners; however, the largest welfare states tend to be universal rather than targeted (Korpi and Palme 1998). Such states engage in a great deal of intra-individual redistribution (Bergh 2005) and are influenced by political economy mechanisms such that funds tend to benefit the median or pivotal voter (e.g., Goodin et al. 1987).

Upon examining the relationship between government size and income inequality in 35 African countries, Odedokun and Round (2004) found that a smaller government need not increase income inequality. Yet if the size and scope of the government affect growth, the indirect effects of changes in government size may well be substantial. Bergh and Henrekson

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(2011) surveyed studies of the relationship between government size and growth and concluded that there are theoretical reasons to expect a positive association in poor countries and a negative one in rich countries.

While empirical studies are roughly in line with these expectations, identifying the causal impacts of changes in government size on real income growth is complicated by endogeneity problems. Nevertheless, studies with credible identification strategies do exist for changes to specific taxes. For example, Ljungqvist and Smolyansky (2014) exploited variation in corporate income tax rates across the U.S. states to compare contiguous counties straddling state borders. The spatial-discontinuity approach permitted a causal interpretation of their findings, which were that increases in corporate tax rates lead to significant reductions in employment and income. Interestingly, the results were not entirely symmetrical: The authors only found evidence that corporate tax cuts boost employment and income only if implemented during recessions. Rosholm and Skipper (2009) instead focused on the effects of active labor market policies, which are implemented by almost all large welfare states as a way to reduce unemployment among relatively low-skilled groups. They nonetheless found evidence from Denmark that such policies actively increase individual unemployment rates and reduce the employability of those enrolled in such programs. As such, many similar welfare state policies may result in adverse distributional consequences by, among other effects, undermining individual economic incentives.

*2. Area 2—Institutional quality*

From a theoretical standpoint, there are several reasons to expect large inequalities to be associated with inferior institutions. Lacking a rule of law is typically assumed to favor asset stripping over value building (Hoff and Stiglitz 2004). You and Khagram (2005) have noted that rich interest groups and firms may use bribery or connections to influence both law-implementing processes and interpretations of the law. Sonin (2003) has suggested that poor protection of property rights may actually be relatively more beneficial to those who are already rich, resulting in greater inequality.

A key feature of well-functioning institutions is the lowering of transaction costs (Davis and North 1970; North 1990). If transaction costs are approximately constant for all transactions (or at least do not increase in proportion to the value of the transaction), they are arguably more problematic for low-income earners. A well-known example is De Soto's (2000) description of how dysfunctional (or entirely absent) property rights institutions often create unsurmountable problems for the poorest population segments in developing countries.

There is also evidence that low-income earners are more likely to have to pay bribes (Justesen and Bjørnskov 2014).

The causal effects of rule of law and property rights are difficult to pin down, and the survey by Lambsdorf (2006) mentions inequality as both a cause and a consequence of corruption. Still, some informative studies exist. The causal effect of successfully fighting corruption has been studied by Svensson and Reinikka (2005) using a (successful) newspaper anti-corruption campaign in Uganda. These scholars identified a positive effect from lower corruption on school enrollment and student learning.

A natural experiment regarding property rights occurred in 1984 when some—but not all—of the squatters in Buenos Aires were given formal property rights to their land. As shown by Galiani and Schargrodsky (2010), these property rights had positive effects on housing investment and child education.


*3. Areas 3–5—Policy quality*

Area 3, known as 'sound money,' is another area in which improvements should theoretically benefit low-income earners relatively more. Inflation is typically regarded as more harmful to the relatively poorer segments of society, as the value of land and physical property is not as affected by inflation as income from other sources and wealth held in other forms. The value of human capital, for example, is likely to be affected in the same way as the marginal product of labor once inflation becomes unpredictable. Theoretically, inflation can therefore be seen as a regressive consumption tax (Erosa and Ventura 2002), and empirical cross-country evidence has confirmed the existence of a positive association between inflation and income inequality (Albanesi 2007).

For area 4, freedom to trade internationally, the distributional consequences are more complex, both theoretically and empirically. According to standard trade theory, as reflected in the Stolper-Samuelson theorem, economic openness may lead to lower income inequality in developing countries where production is typically labor intensive and capital scarce. The same logic suggests that in rich countries, trade openness will increase the returns to capital

and to high-skilled labor. In new trade theory, however, theoretical implications are less clear and the empirical evidence is mixed (Harrison et al. 2011; Marsh 2016)[1].

Several studies have suggested that economic openness impacts prices and productivity (Alcala and Ciccone 2004; Auer et al. 2013; Auer and Fischer 2008). Improved labor market matching has been identified as an important mechanism in the link from openness to productivity (Davidson et al. 2014). In rich countries, trade-induced changes to consumer prices typically favor the poor because they spend relatively more in trade-intensive sectors (Fajgelbaum and Khandelwal 2016). However, most studies (including this one) will fail to capture such effects because real incomes in official statistics are calculated using the same price index for all income earners (as discussed by Deaton and Muellbauer 1980).

Finally, a negative association between area 5, regulation, and productivity has been documented several times, for example by Gray (1987), who studied manufacturing industries in the United States between 1958 and 1978. A review of both theory and evidence on how regulation affects productivity has been provided by Crafts (2006). More recent studies have made progress in identifying causal effects: Using a minor reform of the Swedish labor market, Bjuggren (2018) showed that more flexible rules regarding the hiring and firing of workers increase labor productivity (through factor productivity and capital intensity).

In summary, both theory and empirical evidence indicate that increases in economic freedom have a causal effect on income growth, but do not suggest a clear distributional profile due to the many countervailing effects that at least partly compensate each other.


III. DATA AND EMPIRICAL STRATEGY

In order to test whether economic freedom *dynamically* affects income groups differently, our dependent variable is income growth rate over five-year periods in each quintile of the income distribution from 1975 to 2015, which we calculate by combining two sources. We employ the recently available data on income distributions from the GCIP (Lahoti et al. 2016). Using data from the Luxembourg Income Study (LIS), the Socio-Economic Database for Latin America and the Caribbean (SEDLAC), the European Union Statistics on Income and Living Conditions (EU-SILC), the World Bank's Povcalnet, Branko Milanovic's data on the World

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1. As the survey in the work by Harrison et al. (2011) notes, the effects of trade liberalization theoretically depend on a number of factors, including the flexibility of labor market institutions, specific firm dynamics, and geographical mobility.

Income Distribution (WYD), the UNU-WIDER World Income Inequality Database (WIID), and primary sources (preferred in the order mentioned) on the relative distribution and mean income for each country in a given year, the GCIP project calculates disposable income in each decile of the distribution. For years with missing data, the income profile is interpolated or extrapolated using *per capita* growth rates from the World Development Indicators[2].

Although the GCIP data allow several interesting research questions to be studied, the process of interpolation and extrapolation adds uncertainty. This is particularly true in the many cases in which data are based on household surveys, and especially in the extremes of the income distribution, where measurement errors are larger due to the difficulty of properly capturing the incomes of the very poor and to the incentive for and ability of the wealthiest to hide—legally or illegally—part of their actual income. We therefore aggregate the income shares of each decile to the quintile level. The quintile shares are combined with data on purchasing-power adjusted GDP *per capita* from the Penn World Tables, mark 9 (Feenstra et al. 2015), in order to arrive at average incomes in each quintile. Finally, we calculate the cumulative growth for each quintile over all five-year periods from 1975 to 2015.

The main independent variables are derived from the Fraser Institute's annual report, *Economic Freedom of the World* (Gwartney et al. 2017). Economic freedom is therein defined as a state in which individuals 'are permitted to choose for themselves and engage in voluntary transactions as long as they do not harm the person or property of others.' (p. 1). Gwartney et al. measure the degree to which policies and institutions are consistent with the concept of economic freedom as an aggregate index of five elements: 1) the size of the government, 2) the quality of the legal system and property rights, 3) sound money, 4) the freedom to trade internationally, and 5) a regulation component. All of these indices are themselves composed of a number of sub-indices, such that the full dataset allows any researcher to aggregate or disaggregate according to the relevant situation.

To be specific, area 1 measures government size using indicators such as public consumption and transfers relative to the GDP, top marginal tax rates, and state-owned enterprises. Area 2 quantifies the quality and integrity of the legal system and the protection

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2. For more details on the GCIP data, see Lahoti et al. (2016). Although interpolated data may be problematic, we argue that it is not a major problem in our application. The main problems with using interpolated data occur in annual panel or time series data, while we employ five-year periods. Thus, changes are already smoothed out across five-year periods, and interpolating one or two years in between surveys is therefore unlikely to cause any practical problems.

of property rights, and can be thought of as an attempt to quantify the rule of law. This index is known to correlate substantially with alternative measures of the rule of law. Area 3, which is called 'sound money,' captures the effect of high and unpredictable changes in inflation and money supply. Area 4 combines measures of trade taxes, tariff rates, non-tariff trade barriers, and capital market controls to create a composite measure of the freedom to trade. Finally, area 5 consists of three indices on the regulation of credit, labor, and business, which quantify the regulation of credit markets, labor markets, and business in general. This area consists of measures of bank ownership, interest rate controls, hiring and firing regulations, and the administrative and bureaucracy costs associated with starting and running businesses.

In addition, the most recent editions include a correction for gender differences in the access to proper legal protection, which we apply in the following (Fikes 2017). As described above, results based on the aggregate index are difficult to interpret, as they may consist of different and potentially opposite effects of separate elements of the overall index of economic freedom. We therefore disaggregate the full index of economic freedom into the conceptually coherent indices of government size, institutional quality, and policy quality. Our interpretation of the indices is that area 2 is basically an indicator of institutional quality and is also the area that has consistently been shown to be the most robustly associated with growth (Berggren and Jordahl 2005; Rode and Coll 2012; Hall and Lawson 2014)[3]. This pattern has been further confirmed by the literature on institutions as fundamental causes of growth (Acemoglu et al. 2005; Rodrik et al. 2004). Finally, areas 3, 4, and 5 are the policy areas that capture the economic freedom of monetary policy, trade policy, and regulatory freedom, respectively. We follow arguments in the literature that policies within these three areas can be substitutes with approximately equal consequences in aggregating them into one index (Gwartney et al. 2017). They are nevertheless *a priori* easier to change than basic property rights institutions and thus should be treated separately.

Because institutional quality and policy quality are highly correlated with each other (*r=.60*), we do not include them simultaneously in order to avoid multicollinearity problems. Instead, we think of the results using policy quality as a form of robustness test for the main results using institutional quality, as slight changes in measurement should not change the

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3. Typically, area 2 of the EFW index is heavily correlated with alternatives such as the rule of law component of the Worldwide Governance Indicators (Kaufmann et al. 1999)—or indeed other components of these indicators, or those of the Heritage Foundation Index of Economic Freedom (2017). Within our sample, the correlations between institutional quality and these measures are .8 or higher.

main findings. On the other hand, the government size area is almost uncorrelated with institutional quality and policy quality ($r = -.13$ and $r = -.19$), and is therefore always included in the specification.

We keep the rest of the specification relatively simple in order to avoid including so-called bad controls or effectively controlling for the relevant transmission mechanisms of economic freedom[4]. Our specification therefore follows the parsimonious standard in the growth literature by including only the logarithm of the initial quintile income, trade volumes (export plus imports as a share of total GDP), investment rates as a share of total GDP, and a dummy for democracy. Trade volumes and investment shares are derived from Feenstra et al. (2015), while the democracy measure is the dichotomous minimalist measure developed by Cheibub et al. (2010), as updated by Bjørnskov and Rode (in press). In a set of separate tests, we replace the democracy measure with the PolCon III indicator of political constraints from Henisz (2000). That indicator captures the strength of effective institutional veto players and thus works as a proxy measure for the likely stability of changes in economic freedom.

The inclusion of democracy allows us to include an interaction between democracy and the economic freedom variables, and thus to account for the theoretical expectation that policies and institutions may have different consequences in democratic societies. By doing so, we contribute to the debate regarding democracy and capitalism cited in the introduction[5]. We also provide tests in which we have the political constraints measure interact with the economic variables. In both cases, we provide conditional marginal point estimates with conditional standard errors, as calculated by the Delta method (Brambor et al. 2006).

We run regressions as in (1), where $\Delta Y_{i, j, t}$ denotes growth in income in quintile $i$ in country $j$ at time $t$, $X_{j, t}$ is a vector of country-specific variables, $EF_{j, t}$ is economic freedom in country $j$ at time $t$, $DE_{j, t}$ is initial democracy, and $EF_{j, t} DE_{j, t}$ denotes the interaction between economic freedom and initial democracy. All regressions also include a full set of period and

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4. While trade policy is part of area 4 of the EFW and thus part of our policy quality index, we include actual trade flows in the baseline specification. In an additional test (not shown), we found no differences from the results reported in the following when we excluded trade flows.

5. In addition, we include an interaction between democracy and initial income. This interaction is necessary, as convergence may differ between democracies and autocracies when democratic economic performance is similar (cf. Giavazzi and Tabellini, 2005). In addition, the interaction between institutional quality and democracy may arguably proxy for an interaction between income and democracy, which we effectively control for by including the latter.

country fixed effects, which means that the findings are identified by the within-country variation over time.

$$\Delta Y_{i,j,t} = \alpha + \delta\, Y_{i,j,t\text{-}1} + \beta\, X_{j,t} + \mu\, EF_{j,t} + \eta\, DE_{j,t} + \chi\, EF_{j,t}\, DE_{j,t} + \varepsilon_{i,j,t} \qquad (1)$$

The full dataset, which consists of up to 977 observations with full data in nine five-year periods between 1975 and 2015 from 145 countries, is summarized in Table 1. Like all economic freedom variables, democracy and the political constraints indicator are measured at the beginning of each five-year period; the inclusion of country fixed effects implies that the effect of economic freedom on the growth of quintile income in each period is identified by changes in economic freedom that occurred at sometime within the *preceding* five-year period. While this is not a perfect solution to a potential causality problem, we argue that the timing of changes here alleviates the endogeneity problem and enables us to identify medium-run effects of reforms that change elements of economic freedom.

Table 1. Descriptive statistics

| Variable (q1 indicates first quintile etc.) | Mean | Standard deviation | $N$ | Definition |
|---|---|---|---|---|
| Growth (q1) | .079 | .292 | 1219 | Growth rate of real GDP *per capita* at constant prices (PPP), *y*, multiplied by quintile share of total income, $s_1$: $\ln(y_t\, s_{t\,1}) - \ln(y_{t-1}\, s_{t-1\,1})$ |
| Log initial income (q1) | 7.081 | 1.583 | 1220 | Logarithm to real GDP *per capita* at constant prices |
| Growth (q2) | .086 | .227 | 1219 | As growth (q1) |
| Log initial income (q2) | 7.783 | 1.453 | 1220 | As log initial income (q1) |
| Growth (q3) | .088 | .205 | 1219 | As growth (q1) |
| Log initial income (q3) | 8.234 | .1365 | 1220 | As log initial income (q1) |
| Growth (q4) | .089 | .192 | 1219 | As growth (q1) |
| Log initial income (q4) | 8.680 | 1.276 | 1220 | As log initial income (q1) |
| Growth (q5) | .092 | .185 | 1219 | As growth (q1) |
| Log initial income (q5) | 9.674 | 6.226 | 1220 | As log initial income (q1) |
| Trade volume | .498 | .469 | 1424 | Share of merchandise exports and imports in total GDP |
| Investment rate | .211 | .101 | 1424 | Share of gross capital formation in total GDP |
| Democracy | .443 | 4.97 | 1590 | Dummy for democracy, defined as institutions in which legislative and executive offices are *de facto* contestable, and where at least one peaceful turnover of such offices has occurred following elections |
| Government size index | 5.883 | 1.589 | 1223 | Defined in text |
| Institutional quality | 5.198 | 1.915 | 1111 | Defined in text |
| Policy quality | 6.493 | 1.642 | 1266 | Defined in text |
| Political constraints | .219 | .216 | 1351 | Normalized index counting branches of government with veto power over policy decisions not politically aligned with the executive, and modified to capture within-branch preference heterogeneity |

In addition, although we do not directly deal with potential endogeneity problems except through the specific timing of the variables, the established consensus from previous research (as summarized in Section 2) suggests that the association between reforms that increase economic freedom and the growth of average income reflects a causal relationship

(e.g., Dawson 2003; Faria and Montesinos 2009; Heckelman 2000; Justesen 2008; Bjuggren 2018; Schivardi and Viviano 2011). We can therefore be rather certain that the *average* association between economic freedom and growth is causal, and that the additional effect identified by differences among the five quintiles allows causal inference (cf. Nizalova and Murtazashvili, 2016). We thus interpret the associations in the following as causal.

IV. ANALYSIS

Table 2 provides our main estimates of the effects for government size and institutional quality across the five quintiles. The results indicate positive effects of trade and investments, which are in line with the literature, but these estimates vary across the quintiles. We find relatively larger estimates of trade for the low quintiles and larger estimates of investments for higher quintiles, although the differences are not significantly different across quintiles. Hence, we cannot say with any certainty that trade or investments in general cause unbalanced growth. Similarly, we find no effects of democracy, whereas initial quintile income—that is, the convergence term—is significantly larger for the first quintile relative to the rest[6]. As such, countries with a lower average income and countries with comparatively smaller income shares in the first quintile tend to become more equal over time. These results are thus consistent with most of our sample being on the downward sloping part of the Kuznets curve (Kuznets 1955; Chong 2004).

Turning to the main variables of interest, our estimates in Tables 2 and 3 suggest that both institutional quality and policy quality are significantly and positively associated with growth. We plot these estimates in Figures 2 (government size) and 3 (institutional quality), and note that the estimates from Table 3 for policy quality match those for institutional quality quite closely.

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6. The point estimates of democracy in the tables must be interpreted as democracy at values of government size, institutional quality, and policy quality of zero, which we never observe in the sample. However, it is worth noting that there are no values at which the marginal point estimate of democracy even approaches significance at any conventional levels. It appears that democracy is always insignificant with a point estimate close to zero.

Figure 2. Government size and quintile growth
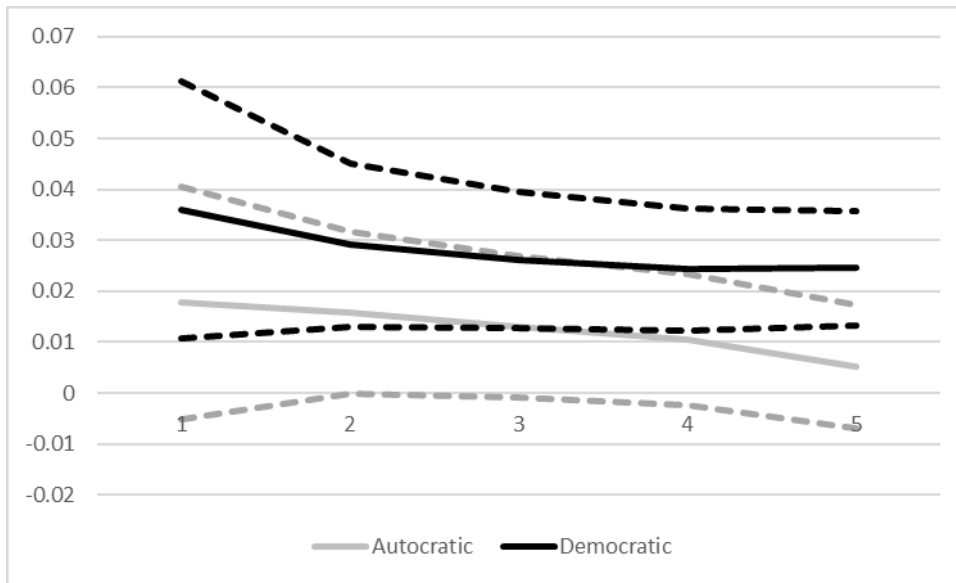


Note: This figure plots the growth effect of a one-point change in government size (measured on a 0-10 scale) for each of the five quintiles, 1–5; the black (grey) line denotes effects in democracies (autocracies) and the dotted lines illustrate the 95% confidence interval around effects.

Figure 3. Institutional quality and quintile growth
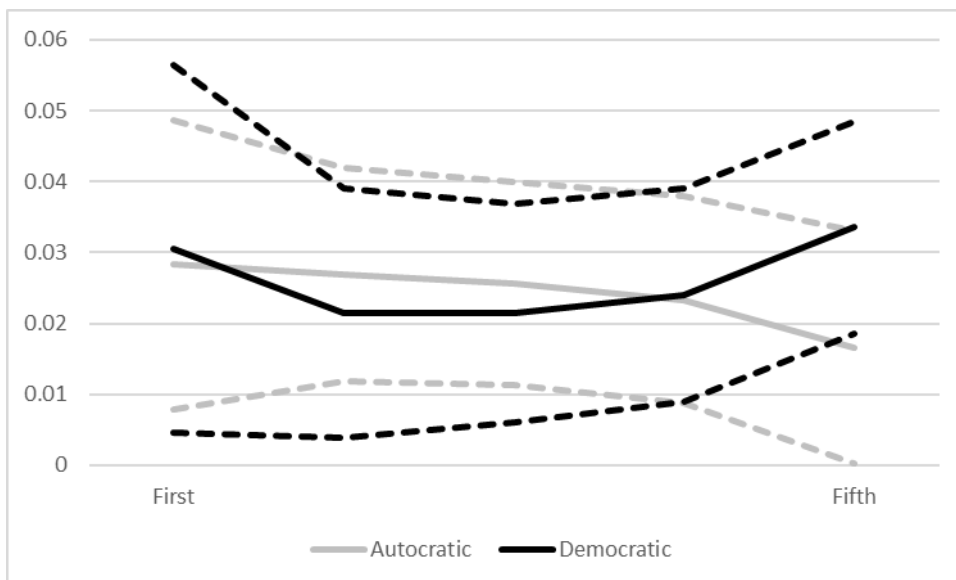


Note: This figure plots the growth effect of a one-point change in institutional quality (measured on a 0-10 scale) for each of the five quintiles, 1–5; the black (grey) line denotes effects in democracies (autocracies) and the dotted lines illustrate the 95% confidence interval around effects.

The point estimates suggest that the effect of institutional quality is largest for the lowest quintile and smallest for the highest quintile; however, these differences are again not statistically significant, as the confidence intervals clearly overlap the point estimates. Conversely, we find that although the point estimates of the government index do not differ significantly, these estimates are subject to substantially more noise in autocracies. As we report in the conditional point estimates in the lower panel of the tables, the index of government size is significantly associated with growth across all quintiles in democratic countries and always positively so. We find a similar pattern for both institutional quality and policy quality, where the estimates are always significant. Although institutional quality appears to be marginally more important in autocracies and policy quality more so in democracies, none of these differences are near significance, nor are any of the differences across the five quintiles.

Table 2. Main results, government size and institutional quality

| Quintile | First | Second | Third | Fourth | Fifth |
|---|---|---|---|---|---|
| Lagged quintile av. income | -.414*** | -.314*** | -.276*** | -.262*** | -.274*** |
| | (.036) | (.033) | (.029) | (.026) | (.024) |
| Trade volume | .079* | .065** | .059** | .058** | .059** |
| | (.042) | (.029) | (.025) | (.024) | (.025) |
| Investment rate | .439*** | .459*** | .466*** | .482*** | .538*** |
| | (.169) | (.129) | (.115) | (.106) | (.099) |
| Democracy | -.011 | -.092 | -.124 | -.142 | -.106 |
| | (.165) | (.118) | (.112) | (.115) | (.157) |
| Government size index | .018 | .016* | .013* | .010 | .005 |
| | (.012) | (.008) | (.007) | (.007) | (.006) |
| Institutional quality | .028*** | .027*** | .026*** | .023*** | .017** |
| | (.010) | (.008) | (.007) | (.007) | (.008) |
| Democracy * lagged income | -.015 | .007 | .009 | .008 | -.008 |
| | (.019) | (.015) | (.014) | (.015) | (.018) |
| Democracy * government | .018 | .013 | .013 | .014* | .019** |
| | (.015) | (.010) | (.009) | (.008) | (.008) |
| Democracy * quality | .002 | -.006 | -.004 | .001 | .017* |
| | (.015) | (.009) | (.009) | (.009) | (.009) |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 977 | 977 | 977 | 977 | 977 |
| Countries | 145 | 145 | 145 | 145 | 145 |
| Within R squared | .346 | .333 | .333 | .336 | .342 |
| F statistic | 18.48 | 16.19 | 15.18 | 14.83 | 17.54 |
| *Marginal effect in democracies* | | | | | |
| Lagged quintile av. income | -.429*** | -.306*** | -.266*** | -.254*** | -.282*** |
| | (.039) | (.033) | (.028) | (.025) | (.023) |
| Government size index | .036*** | .029*** | .026*** | .024*** | .025*** |
| | (.013) | (.008) | (.007) | (.006) | (.006) |
| Institutional quality | .030** | .021** | .021** | .024*** | .034*** |
| | (.013) | (.009) | (.008) | (.009) | (.008) |

Note: The dependent variable is growth rate of real GDP per capita over a five year period at constant prices (PPP). Standard errors clustered at the country level in parentheses *** (**) [*] denote significance at $p<.01$ ($p<.05$) [$p<.10$]. All estimates are obtained by OLS with a constant term and two-way (period and country) fixed effects. All conditional standard errors accompanying the estimates conditional on democracy in the lower panel are calculated by the Delta method (Brambor et al. 2006).

Table 3. Main results, government size and policy quality

| Quintile | First | Second | Third | Fourth | Fifth |
|---|---|---|---|---|---|
| Lagged quintile av. income | -.366*** | -.279*** | -.248*** | -.234*** | -.235*** |
| | (.033) | (.029) | (.026) | (.024) | (.023) |
| Trade volume | .059 | .051** | .048** | .049** | .051** |
| | (.038) | (.025) | (.022) | (.020) | (.020) |
| Investment rate | .315** | .369*** | .391*** | .416*** | .472*** |
| | (.159) | (.113) | (.098) | (.089) | (.087) |
| Democracy | -.047 | -.064 | -.065 | -.063 | -.023 |
| | (.151) | (.115) | (.113) | (.117) | (.146) |
| Government size index | .008 | .007 | .006 | .004 | .000 |
| | (.011) | (.008) | (.007) | (.007) | (.007) |
| Policy quality | .028** | .035*** | .037*** | .036*** | .037*** |
| | (.012) | (.009) | (.009) | (.009) | (.008) |
| Democracy * lagged income | -.024 | -.008 | -.007 | -.008 | -.013 |
| | (.019) | (.015) | (.014) | (.014) | (.016) |
| Democracy * government | .019 | .012 | .009 | .007 | .011 |
| | (.016) | (.011) | (.009) | (.009) | (.008) |
| Democracy * quality | .016 | .010 | .012 | .014 | .012 |
| | (.015) | (.012) | (.012) | (.012) | (.010) |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 1052 | 1052 | 1052 | 1052 | 1052 |
| Countries | 145 | 145 | 145 | 145 | 145 |
| Within R squared | .333 | .347 | .360 | .364 | .345 |
| F statistic | 19.38 | 21.39 | 20.56 | 20.18 | 19.00 |

*Marginal effect in democracies*

| | First | Second | Third | Fourth | Fifth |
|---|---|---|---|---|---|
| Lagged quintile av. income | -.390*** | -.287*** | -.254*** | -.242*** | -.248*** |
| | (.039) | (.027) | (.024) | (.022) | (.021) |
| Government size index | .027** | .019** | .015** | .012* | .012** |
| | (.013) | (.008) | (.007) | (.0076 | (.006) |
| Policy quality | .044*** | .045*** | .048*** | .051*** | .049*** |
| | (.013) | (.009) | (.009) | (.009) | (.008) |

Note: The dependent variable is growth rate of real GDP per capita over a five-year period at constant prices (PPP). Standard errors clustered at the country level in parentheses *** (**) [*] denote significance at p<.01 (p<.05) [p<.10]. All estimates are obtained by OLS with a constant term and two-way (period and country) fixed effects. All conditional standard errors accompanying the estimates conditional on democracy in the lower panel are calculated by the Delta method (Brambor et al. 2006).

To illustrate the size of our estimated associations, we note first that (as shown in Table 1) incomes grow on average between 8% and 9% over a five-year period in our sample. Based on the estimates in democracies, a one standard-deviation increase in institutional quality is associated with roughly 6 percentage units of higher income growth in quintiles one and five,

and roughly 4 percentage units of higher growth in quintiles two, three, and four. While these differences are in line with the idea that economic freedom reforms benefit mainly the top and bottom of the income distribution, the differences between quintiles are far from statistically significant. In summary, we thus find no evidence that the positive effects of economic freedom differ across the income distribution, at least when separating the medium-run growth effects in the five quintiles of the initial distribution. For changes to government size in autocracies, estimates are too noisy to yield a significant pattern.

In Table 4, we explore one of several reasons for these differences between democracies and autocracies. Previous studies have argued that the potential effects of policy and institutional changes mainly materialize in countries in which strong veto institutions make reforms credibly stable (Henisz 2000; Justesen and Kurrild-Klitgaard 2013; Justesen 2014). We therefore replace democracy and its interactions with a direct measure of the strength of effective veto players in the policy process. We report the results in Table 4, where the lower panel contains conditional point estimates of government size and institutional quality evaluated at the median and 90th percentile of veto player strength. The estimates *per se* in the table can therefore be interpreted as marginal effects at the 25th percentile, which is essentially *no* veto players and is thus a subset of the most autocratic autocracies.

The results first indicate that while government size affects the growth rate in all quintiles and the effects do not differ significantly in democracies, the effect is insignificant in the fifth quintile in autocracies and the much larger estimate for the first quintile is significantly different from that in the fifth quintile. Put differently, we find that increases in government size (i.e., reductions in the economic freedom index, area 1) are significantly associated with lower growth for the first to fourth quintiles, but not for the richest quintile when there are no effective veto institutions.

Table 4. Veto results, government size and institutional quality

| Quintile | First | Second | Third | Fourth | Fifth |
|---|---|---|---|---|---|
| Lagged quintile av. income | -.417*** | -.313*** | -.276*** | -.263*** | -.271*** |
| | (.039) | (.035) | (.031) | (.027) | (.024) |
| Trade volume | .076* | .060** | .054** | .053** | .058** |
| | (.042) | (.027) | (.024) | (.022) | (.024) |
| Investment rate | .452*** | .459*** | .464*** | .479*** | .526*** |
| | (.160) | (.123) | (.109) | (.102) | (.097) |
| Political constraints | -.147 | -.292 | -.384 | -.449* | -.206 |
| | (.350) | (.279) | (.252) | (.248) | (.298) |
| Government size index | .022* | .018** | .014** | .011* | .005 |
| | (.012) | (.008) | (.007) | (.006) | (.006) |
| Institutional quality | .019 | .020** | .019** | .019** | .012 |
| | (.012) | (.009) | (.008) | (.008) | (.009) |
| Constraints * lagged income | -.018 | .021 | .029 | .029 | -.027 |
| | (.046) | (.036) | (.032) | (.030) | (.035) |
| Constraints * government | .022 | .022 | .024 | .027 | .043*** |
| | (.031) | (.020) | (.018) | (.017) | (.015) |
| Constraints * quality | .045 | .016 | .015 | .022 | .055*** |
| | (.035) | (.024) | (.021) | (.020) | (.021) |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 977 | 977 | 977 | 977 | 977 |
| Countries | 145 | 145 | 145 | 145 | 145 |
| Within R squared | .347 | .336 | .339 | .343 | .348 |
| F statistic | 16.77 | 15.29 | 15.12 | 15.19 | 17.83 |

*Marginal effect at median veto strength*

| | First | Second | Third | Fourth | Fifth |
|---|---|---|---|---|---|
| Lagged quintile av. income | -.423*** | -.307*** | -.267*** | -.254*** | -.279*** |
| | (.037) | (.032) | (.028) | (.025) | (.023) |
| Government size index | .029*** | .025*** | .022*** | .020*** | .019*** |
| | (.009) | (.007) | (.006) | (.005) | (.005) |
| Institutional quality | .034*** | .025*** | .025*** | .026*** | .031*** |
| | (.009) | (.007) | (.006) | (.006) | (.006) |

*Marginal effect at 90$^{th}$ percentile veto strength*

| | First | Second | Third | Fourth | Fifth |
|---|---|---|---|---|---|
| Lagged quintile av. income | -.427*** | -.302*** | -.261*** | -.248*** | -.285*** |
| | (.039) | (.033 | (.028) | (.025) | (.025) |
| Government size index | .034*** | .029*** | .027*** | .026*** | .028*** |
| | (.013) | (.009) | (.007) | (.007) | (.006) |
| Institutional quality | .043*** | .029*** | .028*** | .030*** | .042*** |
| | (.014) | (.009) | (.008) | (.008) | (.008) |

Note: The dependent variable is growth rate of real GDP per capita over a five-year period at constant prices (PPP). Standard errors clustered at the country level in parentheses *** (**) [*] denote significance at $p<.01$ ($p<.05$) [$p<.10$]. All estimates are obtained by OLS with a constant term and two-way (period and country) fixed effects. All conditional standard errors accompanying the estimates conditional on veto strength in the lower panel are calculated by the Delta method (Brambor et al. 2006).

We find a similar difference for institutional quality in countries without veto institutions, where the association fails to be significant in both the first and fifth quintiles. Conversely, we cannot exclude the possibility that the effects of government size and institutional quality are identical in countries with at least a minimum number of veto players with *de facto* influence. As such, we find that economic freedom only has non-inclusive growth consequences in countries with no effective veto institutions. It is important to note that these countries are always autocratic; yet within the present sample, a third of all autocratic societies have governments that are subject to veto institutions with some power. The rather weak indications of heterogeneous effects in Tables 2 and 3 between democracies and autocracies, and the somewhat stronger heterogeneity in Table 4 across degrees of veto player strength, thus indicate that the restricting features are not democracy or veto institutions *per se*, but are likely to be associated with particular types of autocracies.

We performed a set of further analyses (some of which are reported in full in the appendix) that indicated that the main findings are robust to several additional tests. These tests consisted of excluding post-communist countries and replacing the conditioning effect of democracy with the conditions that observations are either above the sample median average income or above the sample median level of institutional quality.

For example, we find that they are not driven by the inclusion of post-communist countries in which both economic freedom and the shape of the income distribution have changed dramatically since the collapse of communism; these results are reported in the appendix in Table A1. Tests in which we exclude the 10% poorest or 10% richest observations in our sample also yield qualitatively identical (and quantitatively very similar) estimates of government size and institutional quality. Neither does the exclusion of observations with substantially negative economic growth—that is, from the financial crisis after 2008 or the Latin American debt crisis in the early 1980s—change the main findings. Before proceeding to a discussion of the overall findings, therefore, it can be noted that these findings are very robust to most standard tests.

A final question remains: To what extent can we establish that there are—or are not— effect differences due to democracy, or determine whether alternative factors are more likely? In the appendix, we explore two such factors, which we use to form a dummy similar to the democracy measure: real GDP *per capita*, and good institutional quality. In both cases, we form dummies that take the value 1 if the observation is above the sample median and 0 otherwise.

As expected, the overlap between the subsamples characterized by having democracy, high incomes, and high institutional quality is substantial, but far from perfect. For example, our sample includes 181 observations in which countries are democratic but have below-median institutional quality; 101 observations from autocratic countries with above-median institutional quality; 151 observations from relatively poor democracies; and 136 observations from relatively rich autocracies. Finally, the sample includes 158 observations from countries with above-median real income that nonetheless have institutional quality below the median, and 84 observations from countries that were characterized in a five-year period as having relatively low incomes but above-median institutions. As such, the variation across these ways of dividing the sample ought to be sufficient to identify differences between the splits.

Nevertheless, we find very similar results when we replace the interaction with democracy with an interaction with whether or not a country was below or above the sample median real income in a given five-year period; the results are reported in the appendix in Table A2. Again, we observe that the main difference is in the precision of the estimate, and not in the size of the effect, as government size is only significant in countries above the median income. Conversely, when government size, policy quality, and institutional quality interact with a dummy indicating whether institutional quality is above the sample median (appendix Table A3), we observe that the effects of changes in government size are always larger in societies with relatively good institutional quality, and that the difference is most precisely measured for the fifth quintile. We also see indications that convergence is substantially faster in countries with relatively high institutional quality, implying that the long-run effects of policy and institutional changes in these countries are substantially larger (cf. Knack and Keefer 1995). As such, these findings, although much more general, are similar to Freund and Bolaky's (2008) finding that trade policy has larger income effects in countries with good institutions.

In a final test (not shown), we performed a 'beauty contest' between the interactions by simultaneously including each combination of two of the three interactions in the same regression. Although this inevitably creates multicollinearity problems, we can take the results as first indications of the strength of each separate conditional effect. The results of the beauty contest suggest that the precisely estimated effects of government size on the first quintile are strongest when we condition on democracy, while the estimated effects of policy quality and institutional quality are strongest when conditioning on whether or not institutional quality is above the median. As such, although this type of test can never be definitive, we find some evidence that political institutions are more likely to mediate the effects of government size,

while judicial institutions are more likely to mediate the effects of policy and institutional differences. With this final indication, we proceed to discuss the findings.

V. CONCLUSIONS

Numerous studies have documented the effects of economic freedom on growth and long-run development (Hall and Lawson 2014). However, the literature on the relation between economic freedom and income inequality is mixed, and claims of its pernicious consequences regularly appear in political debate. In this paper, we have therefore explored whether economic freedom really 'raises all boats,' or if political and institutional changes toward more economic freedom lead to unbalanced growth across the initial income distribution. To do so, we combined standard data on economic freedom from the Fraser Institute with new and extensive data on income distribution from the Global Consumption and Income Project. We used the resulting panel dataset of 145 countries observed in up to nine five-year periods to test the effects of economic freedom on income growth within the five quintiles of the distribution.

Overall, our results suggest that institutional quality and policy quality are positively associated with income growth across the income distribution. These effects appear in both democracies and autocracies, although limited government is significantly associated with income growth across all quintiles only in democracies. In societies with no effective veto institutions, which form a subset of autocratic societies within our sample, we find that limited government is associated with higher income growth for the lowest quintile, but has no significant effect for the top quintile.

In all other cases, we find no significant differences in the effects of economic freedom on quintile income growth. Nevertheless, although the differences in how economic freedom is associated with income growth at different points in the income distribution are not significant, they may still help to explain why studies using inequality as the dependent variable are so mixed. First, the strongly significant differences in the convergence term between the first quintile and the rest of the distribution—and thus the substantially larger long-run multiplier of changes for income in the first quintile—suggest that there are large differences between short-run and long-run distributional effects, and the long-run equilibrium consequences of economic freedom may actually imply a more equal distribution of income. Second, when focusing on the medium-run effects, i.e. the effects of changes on economic freedom on growth over the next five-year period, our findings suggest that the sample composition may strongly affect the overall results. In particular, findings in samples

dominated by poor, autocratic countries with limited veto institutions are likely to be quite different from findings in samples dominated by modern democracies. Because most existing studies with conflicting findings arguably capture medium-run effects and differ in sample composition, our result suggests that the conflicting findings in previous studies are less puzzling than they might seem.

Finally, our findings highlight the importance of seriously considering how features of political institutions may moderate the distributional consequences of economic freedom. In any case, our findings clearly suggest that the consequences of reforms that increase economic freedom will boost medium-run growth for all five income quintiles. In other words, economic freedom does seem to lift all boats.

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