Wealth Concentration and Economic Growth: A Dynamic Panel Investigation.

Abstract

This paper investigates the impact of wealth concentration on long-run economic growth. We initially build a theoretical model from Alessina and Rodrik (1994) which explains how greater wealth concentration decreases economic through greater political pressure for redistribution using the median voter hypothesis. We estimate a short, dynamic growth model through pooled OLS, fixed effects, random effects, and system GMM estimation. Our model is estimated on a sample of 137 countries between 1995-2020. The wealth data are taken from the World Inequality Database, which have some conceptual benefits over the data used previously in the literature. We find no long-run effect of wealth concentration on economic growth. We find some evidence that income inequality negatively affects growth, which disappears when considering only a higher income sample, supporting existing empirical income inequality findings.

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2.0 – Introduction

The relationship between wealth inequality and economic growth has been of longstanding interest in the economics literature, but the recent global pandemic has brought inequality to the forefront. During 2020 alone, an estimated 100 million (Lakner et al., 2021) up to 400 million (Sumner et al., 2020) people worldwide fell below the \$1.90 per day poverty line while billionaire wealth grew by \$3,917.23bn (Berkhout et al., 2021). Even pre-pandemic, Piketty (2014) found that the world's richest 1% owned around half of global wealth, while the poorest 50% owned less than 5% of wealth. To address this disparity, we first need to understand how wealth concentration affects economic growth. Data limitations mean much of the previous work in inequality and growth has focused on the role of income inequality, but Piketty (2014) suggests that wealth inequality may have more noticeable effects than income inequality. This project hopes to further understand the effect of wealth concentration's on long-run economic growth.

The structure of the paper is outlined as follows: Section 2.1 describes an economic model from Alesina and Rodrik (1994) relating wealth inequality to economic growth, Section 2.2 provides a literature review, Section 3.1 outlines the econometric theory behind estimating the effect of wealth inequality on economic growth, and Section 3.2 presents and discusses our regression results.

2.1 – The Model

Alesina and Rodrik's (1994) model uncovers a mechanism driving the inequality-growth relationship. The model is characterised by a closed economy with perfect competition, profit maximising firms, and a government which spends G_t each period on productive services. The government runs a balanced budget:

$$G_t = \tau K_t \tag{1}$$

Where spending is financed in each period by a tax τ on the aggregate capital stock, K_t. Note that capital letters denote aggregate variables and lower-cased variables are per capita. Alesina and Rodrik call τ a tax on capital income, however since it is defined as a proportion of the capital stock, it is a tax on wealth. Government expenditure positively enters the Cobb-Douglass style production function (2) to represent the constructive role of government in the economy, allowing for growth-enhancing effects of taxation.

$$Y_t = AK_t^{\alpha} G_t^{1-\alpha} L^{1-\alpha} \quad , \quad 0 < \alpha < 1 \tag{2}$$

Where Y_t is aggregate output at time t, A is a constant technology factor, and L is the constant labour force. Differentiating the production function with respect to capital and labour and substituting in the government budget constraint (1), we find their respective marginal products; because we assume perfect competition, these are also the marginal returns to capital (3) and labour (4).

$$\frac{\partial Y_t}{\partial K_t} = r = \alpha A (L\tau)^{1-\alpha} = r(\tau) \tag{3}$$

$$\frac{\partial Y_t}{\partial L} = w = (1-\alpha)A\left(\frac{K_t}{L^{\alpha}}\right)\tau^{1-\alpha} = w(\tau)K_t \tag{4}$$

From (3) we can see that the real rate of interest, r, positively depends on the capital tax rate and has no dependence on the level of capital stock K_t, meaning there are constant returns to capital. This characteristic is not realistic, it implies that any increase in capital (even at very high levels of capital) has the same effect on aggregate output – the implications of this are discussed at the end of this section. From (4), we see that the returns to labour, w, also positively depend on the tax rate. In both cases, this is because of the complementarity between G_t and K_t, L in (2). Higher taxation allows for greater government expenditure on productive services, resulting in higher marginal products of capital and labour. Capital also increases the returns to labour because (1) shows how a higher capital stock increases government expenditure.

2.1.1 – The Growth Rate of the Economy:

All individuals in the economy have the same preferences and time horizon, so the utility maximising problem is generalisable to the entire population. The utility maximisation is given in (5), where individuals maximise a logarithmic utility function subject to their income constraint.

$$\max U_i = \int_0^{\infty} (\log c_i) e^{\rho t} dt \qquad \textit{subject to}: \frac{dk_{it}}{dt} = w(\tau)K_t l_i + [r(\tau) - \tau]k_{it} - c_{it} \tag{5}$$

Assuming a constant tax level (which we will see later to be the case), the result of (5) gives a growth rate of individual consumption, $\dot{c}_{it}/c_{it}$, equal to:

$$\frac{\dot{c}_{it}}{c_{it}} = r(\tau) - \tau - \rho \equiv \gamma(\tau) = \frac{\dot{K}_t}{K_t} = \frac{\dot{k}_{it}}{k_{it}} \tag{6}$$

Since consumption grows at the same rate for everyone, the consumption growth rate defines the growth rate of the economy $\gamma(\tau)$. With the economy growing at rate $\gamma(\tau)$, aggregate capital K_t grows at the same rate. The income constraint in (5) applies to everyone, so individual capital, k_{it}, follows the same growth rate for each i and everyone's relative wealth ownership is constant. (6) shows conflicting effects of taxation on growth. Differentiating (6) with respect to τ shows the non-linear effects of capital taxation on growth:

$$\frac{\partial \gamma}{\partial \tau} = \frac{\partial r}{\partial \tau} - 1 \gtreqless 0 \quad for \quad \tau \lesseqgtr [\alpha(1-\alpha)AL^{1-\alpha}]^{\frac{1}{\alpha}} \tag{7}$$

Where $\frac{\partial r}{\partial \tau}$ is calculated from (3), giving an optimal tax rate $\tau^* = [\alpha(1-\alpha)AL^{1-\alpha}]^{\frac{1}{\alpha}}$. A higher tax rate has two effects on the growth rate: (i) greater spending on productive services by the government increases the returns to capital and labour (see (3), (4)) which facilitates higher growth. (ii) a higher τ also distorts saving incentives since the after-tax rate of return is lower, which limits growth. These 2 conflicting effects result in the concave relationship seen in Figure 1. With $\tau < \tau^*$ the productive spending effect dominates, but as τ increases past τ^*, the incentive distorting effect dominates so growth decreases in the tax rate.

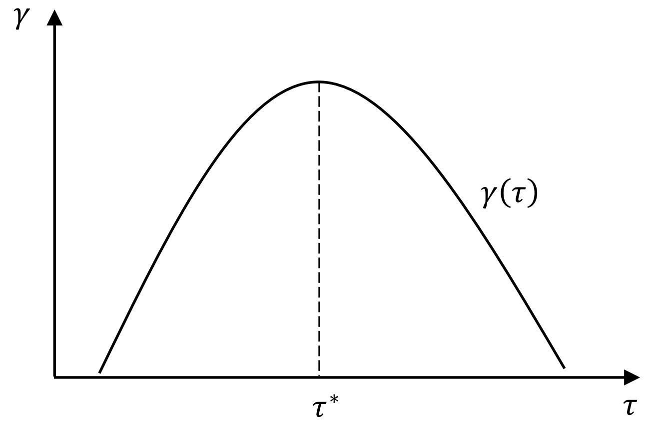

Figure 1 – the effect of capital taxation on the economy's growth rate.

2.1.2 – Voting

The population is heterogenous in initial endowments, where each individual, i, has a relative factor endowment σ_i, which can also be seen as their dependence on labour income:

$$\sigma_i = \frac{l_i/L}{k_{it}/K_t} \tag{8}$$

Since $\sum_i k_{it} = K_t$ and $\sum_i l_i = L$, the numerator in (8) can be interpreted as the individual's relative income level, and the denominator is their relative wealth ownership. With $\sigma_i = 0$, this individual has no labour income, they are a pure capitalist. As σ_i increases above 0, this represents an increasing dependence on labour income and lower relative capital endowment. Alesina and Rodrik assume approximately the same l_i across the population (≈ 1), so σ_i only varies by capital endowment – a higher σ_i means i is relatively wealth poorer. As shown earlier, capital grows at the same rate for everyone (individual wealth grows at the same rate as aggregate wealth) so σ_i is time invariant.

The tax rate is democratically chosen, so each individual votes for their preferred tax rate τ_i. Their chosen rate is the result of (5) which, for each i, yields a unique preferred tax rate. Capital taxation has two effects on individual utility: (i) a positive level effect through wages – the productive government spending financed through a higher τ increases the marginal productivity of labour in (4), hence, labour income increases for all i. (ii) a growth effect – this can be interpreted from Figure 1. At low τ levels, a higher τ is preferred because of its consumption growth enhancing effects until we reach the growth maximising tax rate τ^*. However, in deriving τ_i we find a linear positive effect of σ_i, so a higher dependence on labour income is associated with a higher preferred capital tax rate. Intuitively, everyone benefits from the public service G_t through higher labour income so this effect is constant across σ_i. However, if you are less dependent on labour income (lower σ_i), you are adversely affected by the wealth tax, so prefer a lower rate of tax. In fact, in the limiting case of $\sigma_i = 0$, a pure capitalist prefers $\tau_i = \tau^*$ because this tax rate maximises the growth rate of capital. Therefore, since τ_i monotonically increases in σ_i, for any $\sigma_i > 0$ the individual chooses $\tau_i > \tau^*$.

Voting occurs through pairwise comparisons with a simple majority rule. Since they vote over one issue, individuals have a single preferred tax rate, and there is a monotonic relationship between preferred tax rates and initial factor endowments, the median voter theorem can be employed. Under this, the politically decided tax rate is always that of the median voter, τ^m, who has median dependence on labour income σ^m. Since the wealth distribution remains constant over time, there is no difference between voting for τ once and keeping this forever or voting for τ in each period. In the second case, preferences remain the same in each period, so the tax rate would remain unchanged at τ^m.

2.1.3 – The Effect of Wealth Inequality

Since we are concerned with how wealth inequality affects growth, we should first understand how changes in inequality manifest within the model. To start, we can take a perfect equality case where everyone has the same initial wealth endowment. From (8), assuming everyone has wealth $k_{it} = K_t/L$, their relative dependence on labour income is $\sigma_i \approx 1, \forall i$ (holding the assumption that $l_i \approx 1$). Of course, the median voter also has $\sigma^m = 1$, so the decided tax rate is everyone's preferred rate. However, real world wealth distributions are typically right skewed with large top wealth shares leading to mean wealth ownership above that of the median. In our model, this means the relative labour income dependence of the median voter increases compared to the 'mean', so σ^m rises above one. As previously outlined, a higher σ^m leads to a higher preferred tax rate of the median individual, and a higher politically decided tax rate for the economy. Hence, the extent to which σ^m is above one measures the level of inequality in the model. More concentrated wealth decreases median wealth ownership relative to the mean, so we see higher equilibrium wealth taxation. Since τ^m is necessarily above τ^* (Figure 1), further increases in wealth taxation reduce economic growth because of the distortionary savings effects of the tax. This is the core result of Alesina and Rodrik's model, that higher wealth inequality reduces economic growth.

As previously stated, (3) shows the constant returns to capital. Therefore, the model is AK-style where changes in fundamental variables cause permanent growth effects. If we instead had diminishing marginal returns to capital, the increased taxation above τ^* would only temporarily decrease the growth rate, but the level of output would be permanently lower than where the tax rate never changed (Groth, 2010). Overall, the main finding of the model remains the same with diminishing returns to capital, growth falls following a rise in wealth inequality, but the reduction in growth is not permanent. However, the model is stylised in the role of government and voting mechanism – discussed in more detail in 2.2 – and the constant σ_i implication is unrealistic. Relaxing this and allowing individuals to move up/down the wealth distribution, as in reality, affects their voting decision depending on how their preferences change. If they retain some tax rate bias from their initial endowment, the preferred tax rate is no longer monotonic in σ_i, therefore, the median voter theorem cannot be employed. This is one of many possible critiques of the voting mechanism.

2.2 – Literature Review

A range of mechanisms link inequality and economic growth, but conflicting relationship directions and imperfect markets lead to ambiguous theoretical channels. As covered in section 2.1, Alesina and Rodrik's model finds greater inequality resulting in higher redistribution due to the median voter's preference changing. Alongside Persson and Tabellini (1994), they propose that greater redistribution lowers incentives to invest, with extreme levels of inequality resulting in political instability and social unrest, in all cases reducing growth (Alesina and Perotti, 1996; Keefer and Knack, 2000). However, Benabou (1996) argues that this theorem misses the causes and consequences of redistribution because imperfect markets change the model's mechanisms. Mello and Tiongson (2006) empirically investigate this claim to find that more unequal societies spend less on redistribution, supporting Benabou's imperfect markets hypothesis. Furthermore, according to Stiglitz (2012), the median voter's preference is no longer pivotal because of the disproportionate political influence of the wealthy through lobbying and campaign contributions. Without a pivotal median voter, the key result from Alesina and Rodrik's model would need to be tested with a more sophisticated voting mechanism. Even the theorised fact of greater redistribution hampering growth is unconfirmed, with multiple empirical analyses finding a positive association between redistribution and economic growth (Perotti, 1996; Bergh and Henrekson, 2011).

Imperfect markets appear again in Galor and Zeira's (1993) model, which builds on Loury (1981) in relating wealth inequality to human capital investment. Wealth constrained households have a barrier to investing in human capital. With imperfect credit markets, wealth inequality limits who can borrow, leading to lower long-run growth due to reduced investment. Since poorer parents typically substitute savings in human and physical capital for 'saving' in the form of children, the increased fertility of poorer households compounds this downward effect on average human capital in the economy, further dampening long run growth (De La Croix and Doepke, 2003). Aghion, Caroli and Garcia-Peñalosa (1999) claim that under perfect markets there is no trade-off between equity and efficiency, highlighting the unintuitive nature of perfect market assumptions in inequality and growth models.

Since new technology adoption relies on domestic demand, the consumption loss from higher inequality reduces the level of innovation and long run growth (Murphy et. al. 1989; Kreuger, 2012; Bernstein 2013). However, wealth/income inequality has also been theorised to positively impact growth through i) the aspirational effect: more unequal distributions of wealth incentivise hard work and risk-taking, leading to higher productivity, hence faster growth (Mirrlees, 1971; Rosen, 1981). ii) the savings effect: richer individuals have a higher marginal propensity to save, so more concentrated income distributions increase savings and capital accumulation (Bourguignon, 1981).

2.2.1 – Empirical Strategies

The empirical literature is generally split into 2 categories: those who find a positive effect of inequality on growth, and those who find a negative effect. A variety of econometric techniques have been employed with varying results. The pioneering paper in this literature is by Alesina and Rodrik (1994) who, along with other early works (such as Persson and Tabellini, 1994; Clarke, 1995; Perotti, 1996) use cross-sectional data to find a negative effect of income inequality on consequent economic growth.

Since "Empirical studies in income distribution are often limited by the available data" (Li and Zou, 1998, pp. 322) and the studies above relied on cross-sectional inequality data, their results were naturally based on cross-sectional regressions. However, these regressions do not account for time-invariant country characteristics which influence both inequality and growth. Discussed at length in Forbes (2000), one omitted factor could be the degree of corruption within government which, when omitted, would result in a negative bias on the inequality coefficient. Panel data techniques like Fixed Effects and Random Effects models can solve some of this endogeneity, but we first need a panel of inequality data.

In 1996, Deininger and Squire constructed a dataset containing high-quality data on the income Gini index for 46 countries from 1947-1995, allowing for the creation of a panel dataset covering economic growth and income inequality. Following this, Li and Zou (1998) use fixed and random effects models to find a positive relationship between income inequality and subsequent growth. Forbes (2000) used this dataset to add country and time fixed effects in Perotti's (1996) specification. The positive coefficient on Forbes' inequality variable contrasted Perotti's negative result, highlighting the bias in cross-sectional regressions. However, Atkinson and Brandolini (2001) state that even for OECD countries Deninger and Squire's dataset differs in data coverage, income definitions, and construction methods causing comparability issues across countries. Since panel techniques, such as Forbes' Fixed Effects model, magnify measurement error bias in growth regressions, caution should be taken in the choice of dataset (Hauk and Wacziarg, 2009).

While the Fixed/Random Effects models allow for country and time-period idiosyncrasies, these are not the optimal tool for a dynamic model with a small number of time periods (T). Judson and Owen (1999) show that with a lagged dependent variable and $T = 5$, the bias on the lagged variable's coefficient is over 50%, while the bias on the other dependent variables is around 3%. To address this issue, Arellano and Bond (1991) propose the First Differenced Generalised Method of Moments (FD GMM) estimator, which corrects for the bias of the lagged dependent variable while allowing some endogeneity in other regressors. Forbes used this estimator in her analysis to add robustness to the positive result, however, in growth regressions this estimator still suffers adversely from measurement error (Hauk and Waczjarg, 2009).

Differencing inequality measures within-country, as one does when transforming the model for FD GMM, discards a large amount of information since inequality measures usually have small changes between years (Voitchovsky, 2005). This restricted variation makes it difficult to estimate the coefficients with precision using FD GMM (Dollar and Kraay, 2002). The system GMM estimator proposed by Arellano and Bover (1995) and Blundell and Bond (1998) uses information from the levels and changes of variables, retaining cross-country inequality variation, but its validity requires a larger sample of countries (Islam and McGillivray, 2020). With modern datasets allowing for greater country coverage, system GMM has become the principal econometric tool in the growth and inequality literature.

2.2.2 – Model Specification

Even with a generally standardised estimation technique (system GMM), specification choices change the underlying mechanism being tested. In the early literature, authors commonly used the Gini coefficient of income as a measure of inequality. While this captures inequality across the entire income distribution, the mechanisms affecting growth are specific to different sections of the income distribution (Voitchovsky, 2005). For example, the savings and aspirational effects (positive) primarily concern the top of the distribution while fertility and capital constraints (negative) focus on the bottom of the distribution. Voitchovsky (2005) explicitly tests this, using ratios of income percentiles (Kuznets ratios) at the top and bottom of the income distribution as measures of inequality. They find a negative effect of bottom-inequality and a positive effect of top-inequality on growth. If we measure inequality by the Gini coefficient, we find an average of these effects, rather than identifying a specific channel between inequality on growth (Voitchovsky, 2005; Cingano, 2014; Bagchi and Svejnar, 2015; OECD, 2015).

However, these results are still sensitive to specification changes. Cingano (2014) uses the 80/50 Kuznets ratio (rather than Voitchovsky's 90/75 ratio) to find an insignificant effect of "top inequality" on growth, opposing Voitchovsky's positive result. One conceptual criticism was raised by Litschig and Lombardi (2019): since Voitchovsky controls for the Gini coefficient of income, the coefficient on top inequality measures changes in top inequality while keeping overall inequality constant. This necessarily means the increase in top-inequality is offset by a decrease in inequality elsewhere in the distribution, so it is unclear what the top-inequality coefficient is measuring.

In Alesina and Rodrik's (1994) model, the deciding influence on growth is wealth inequality. Due to the limited availability of wealth data, they use income and land inequality as proxies since both capture some aspect of wealth inequality. Since wealth accumulates and generates income over time, wealth inequality is thought to have greater, more persistent, growth effects compared to income inequality (Piketty, 2014). However, because neither income or land fully capture the accumulating nature of wealth, they are imperfect proxies for the underlying effect (Alesina and Rodrik, 1994; Benabou, 1996; Aghion et. al., 1999). Data limitations mean the literature has departed from wealth-based analysis and put a greater focus on the role of income. However, rising wealth inequality since the Global Financial Crisis (Bogliacino and

Maestri, 2016) and growing wealth to income ratios (Piketty and Zucman, 2014) further highlight the need for empirical analyses, especially with the growing availability of data from sources like the World Inequality Database making wealth-based analyses more feasible.

Limited empirical studies cover the effect of wealth inequality on economic growth. Bagchi and Svejnar (2015) used data from Forbes' billionaires list to create a measure of top wealth concentration normalised by GDP. Their measure of billionaire wealth concentration was negatively associated with economic growth. Interestingly, when splitting their wealth measure into politically and non-politically connected wealth (based on the wealth holder), only the politically connected component had a negative growth effect, the other had no effect. These findings were robust to several specification changes and estimation techniques, including system GMM. This highlights how the source of wealth concentration affects its growth effect. However, since this data only covers the very top of the wealth distribution, its findings cannot be generalised to other wealth inequality measures. Chesters (2016) used direct wealth distribution data for G20 nations from Credit Suisse in a descriptive data analysis of economic growth and wealth inequality between 2001-2013. They concluded that periods of high growth were associated with large increases in wealth inequality, but the direction of causality (or causality itself) were not discussed at length.

Islam and McGillivray (2020) used the Credit Suisse dataset in a more rigorous investigation using a system GMM approach. Their findings suggested that wealth inequality, measured as the top 1% and top 10% wealth shares, has a negative impact on growth. However, the negative growth effects are mitigated with better governance. They highlight the conceptual issue with Bagchi and Svejnar's (2015) inequality measure since it is a ratio of wealth (a stock measure) to GDP (a flow measure). Instead, they use the Forbes and Credit Suisse data to create a billionaire wealth share variable, which was also negatively associated with economic growth, supporting Bagchi and Svejnar's result. Overall, even though high growth periods are associated with increased wealth concentration, wealth inequality has been found to negatively correlate with growth.

3.1 – Econometric Strategy

In this section, a Barro (1991) style endogenous growth regression model is used to empirically estimate the effect of wealth inequality on economic growth. Since we are concerned with the long-term effects of Inequality on economic growth, we estimate a model with 5-year intervals as follows:

$$y_{i,t} - y_{i,t-1} = \alpha y_{i,t-1} + Ineq'_{i,t-1}\varphi + X'_{i,t-1}\beta + \rho_t + u_{it} \tag{9}$$

Where y_{it} is the log of real GDP per capita (p.c.) for country i at time t. Since t and $(t-1)$ are 5 years apart, the left-hand side of (9) approximates a country's 5-year growth rate, henceforth denoted $growth_{it}$. On the right-hand side, $y_{i,t-1}$ is the lagged log of GDP p.c., $Ineq_{i,t-1}$ is a column vector of our inequality variables, $X_{i,t-1}$ is a column vector containing a set of country characteristics at the start of the growth period, ρ_t are period dummies, and u_{it} is an error term such that $u_{it} = \mu_i + v_{it}$. Further, μ_i are unobservable country characteristics, and v_{it} is the idiosyncratic error term. We can re-write (9) as:

$$y_{i,t} = (\alpha + 1)y_{i,t-1} + Ineq'_{i,t-1}\varphi + X'_{i,t-1}\beta + \rho_t + \mu_i + v_{it} \tag{10}$$

Which clearly shows the dynamic nature of the model. The controls are measured in t-1 to prevent reverse causality. Since causality plausibly works in both directions, we want to see how initial changes in the controls affect subsequent growth. We include the lag of GDP p.c. to capture the convergence effect, where poor countries grow faster than richer countries due to diminishing returns to capital thereby 'converging' in GDP p.c. (Barro, 1991). $Ineq_{it}$ contains our measure of wealth inequality (the top one percent or top ten percent wealth share), and the Gini coefficient on income inequality. X_{it} is a vector of controls in line with the existing endogenous growth literature: educational attainment, the investment ratio, institutional quality, trade openness, population growth rate, and the inflation rate. These controls, which are the same as those used by Islam and McGillivray (2020), are discussed in more detail in section 3.2.

We can initially estimate (9) through Pooled OLS (POLS). Here, the data is treated as a large cross-section with $n \times T$ observations - necessarily meaning μ_i, the effect of country-specific unobserved heterogeneity, is not directly considered. The consistency of the POLS estimator requires u_{it}, including the country fixed effects, to be uncorrelated with the regressors. In section 3.2, we calculate cluster-robust standard errors to allow for general patterns of heteroskedasticity and serial correlation, and explain our serial correlation concerns. Even with cluster-robust standard errors, the presence of a lagged dependent variable (LDV) results in dynamic panel bias also known as Nickell bias (Nickell, 1981). If a country has a unique negative shock in output for one period, we observe lower GDP p.c. for that period and a lower country fixed effect. Since GDP p.c. enters as a regressor, we have correlation between the LDV and u_{it}, a clear violation of POLS consistency. The fixed effect's variation is absorbed into the LDV, inflating the coefficient. If T were necessarily large, this would reduce the endogeneity problem, however, since our panel has T=6, POLS is clearly sub-optimal (Roodman, 2009).

The Fixed Effects (FE) estimator can resolve the endogeneity problem arising from μ_i. FE estimation employs a within-groups transformation where variables become country-level mean deviations:

$$growth_{it} - \overline{growth_i} = \alpha\left(y_{i,t-1} - \bar{y}_{i,}\right) + \left(Ineq_{i,t} - \overline{Ineq}_{i,t}\right)'\varphi + \left(X_{i,t-1} - \bar{X}_i\right)'\beta + (u_{it} - \bar{u}_i) \tag{11}$$

As a result, the country fixed effect in u_{it} is differenced out and (11) can be estimated through OLS, relaxing the assumption that country effects μ_i should be uncorrelated with our regressors. However, even though FE estimation allows correlation between the regressors and our error term, we still have Nickell bias. Our error term now includes $v_{i,t-1}$ due the mean-differencing, which is necessarily correlated with $y_{i,t-1}$, causing endogeneity and biased OLS estimates of (11) (Nickell, 1981; Bond, 2002). Judson and Owen (1999) show that even with $T = 30$, we have a negative bias of 20% on the LDV. Since the FE bias is negative and the bias on our POLS estimate was positive, the true coefficient should lie between these estimates, allowing for checks of theoretically superior estimators (Bond, 2002). A key disadvantage of the FE estimator is that mean-differencing leaves only within-country variation, so the effects of time-invariant variables like region dummies cannot be estimated.

The Random Effects (RE) estimator instead uses within and between-country variation, increasing the efficiency of the estimates. This is because the RE estimator can be thought of as a weighted average of the POLS and FE estimates, which is why it is often called the 'between estimator'. Like POLS though, RE estimates are only consistent where the explanatory variables are orthogonal to the country fixed effects.

This assumption can be tested using a Hausman test, which has a null hypothesis of no correlation between the regressors and fixed effects (Hausman, 1978), and is calculated through:

$$H = (\hat{\beta}_{FE} - \hat{\beta}_{RE})' \left[\widehat{var}(\hat{\beta}_{FE}) - \widehat{var}(\hat{\beta}_{FE}) \right]^{-1} (\hat{\beta}_{FE} - \hat{\beta}_{RE}) \tag{12}$$

Where hats denote estimates. Under the null, H is χ^2 distributed and both RE and FE estimators are consistent. With no correlation between regressors and fixed effects, the RE estimator is more efficient than FE, therefore preferred. If the null is rejected, H is large due to a bigger difference between the FE and RE estimates. This only occurs where there is correlation between the fixed effects and our regressors, meaning the RE estimator is inconsistent and the FE estimates are preferred. However, with heteroskedasticity or serial correlation, the Hausman test statistic is no longer χ^2 distributed under the null hypothesis (Wooldridge, 2010). To overcome this issue, Wooldridge (2010) proposes a regression-based 'robust' Hausman test which allows for general heteroskedasticity and serial correlation while testing the same null hypothesis. This is a test of overidentifying restrictions, where (9) is re-estimated through RE, adding the mean-deviated regressors from (11). The test statistic is a Wald test of the significance of the additional regressors which is asymptotically equivalent to the Hausman test statistic, with the same null hypothesis and rejection/acceptance conclusions as above (Wooldridge, 2010; Schaffer and Stillman, 2010).

A consistent issue in our estimation strategies is the collinearity of regressors. Discussed in more detail in section 3.2, we have serious concerns that our wealth inequality measures are highly correlated with another regressor, which makes it difficult to distinguish between their effects. As a result, our coefficients of interest are estimated with less precision, so the standard errors are inflated leading to invalid inference (Wooldridge, 2020). To see the severity of correlation between our regressors, we can calculate the Variance Inflation Factor (VIF) of our variable of interest. VIF gives a measure of how well the other variables explain the regressor of interest, calculated using the R^2 from regressing the variable of interest on all other regressors. Hence, a high VIF means we have more linear dependence between our regressors. While a lower VIF is always better, a general rule is that a VIF above 10 should cause serious concerns. VIF scores are calculated for our regression specifications and discussed in section 3.2.

3.1.2 – GMM Estimation

Across all the above estimators, the Nickell bias is a persistent issue in a small T dataset. The First Difference GMM (FD GMM) estimator developed by Arellano and Bond (1991) eliminates the country specific effects through first-differencing (9) and instrumenting these differenced variables with sufficiently lagged level variables (where 'level' refers to the non-differenced variable). Our instrumented LDV is uncorrelated with the error term, eliminating the Nickell bias. In GMM estimation, we have one moment condition for each instrument which states they are orthogonal to the idiosyncratic error, also known as orthogonality conditions, and the estimated coefficients are chosen to minimise this set of moment conditions. When satisfied, the orthogonality conditions mean our instrumented variables are exogenous to v_{it}. However, instrumenting only eliminates the Nickell bias where v_{it} are not serially correlated. With serially correlated v_{it}, the lagged level variables are not orthogonal to the error term, therefore, our LDV is still endogenous after being instrumented. Crucially, some of our regressors, like inequality, are persistent so first-differencing removes the cross-country variation, and our estimates are based on limited within-country information. Persistent level instruments are weak in this context, because they poorly explain first differences, giving imprecise estimated with a large bias (Blundell and Bond, 1998; Bond et al. 2001).

The system GMM estimator from Arellano and Bover (1995) and Blundell and Bond (1998) sets additional moment conditions on the level equation, using lagged differenced variables as instruments. System GMM uses information from within and cross-country variation, therefore, seeing efficiency gains over FD GMM (much like the RE case earlier). However, these additional moment conditions are only valid where our instruments satisfy further assumptions. We need to satisfy the Blundell-Bond (1998, pp. 124) requirements which state that country deviations from their steady state level of GDP p.c. at $t = 1$ and (Blundell and Bond, 1998, pp. 136) differenced explanatory variables should be uncorrelated with the country fixed effects. To detect possible violations of this, Roodman (2009) suggests reporting difference-in-Hansen tests on the group of instruments for the level equation. This is another test of overidentifying restrictions, testing whether the additional moment conditions are relevant in explaining the endogenous variables. Under the null, this test indicates the joint validity of the set of instruments considered and the test statistic is χ^2

distributed with degrees of freedom equal to the number of suspect instruments. If this test rejects the null, the considered instruments are weak or invalid, implying a violation of the Blundell-Bond requirements.

The Hansen test instead considers the full set of instruments, not just the subset in the difference-in-Hansen case, but is still χ^2 distributed with degrees of freedom equal to the number of suspect instruments under the null. However, the Hansen/difference-in-Hansen tests weaken with a larger instrument set, so p-values close to 1 are a sign of instrument proliferation (Roodman, 2009). Having too many instruments causes wider issues in system GMM because they overfit the endogenous regressors. If we have a set of instruments which perfectly fit the regressor of course the endogeneity remains, so we need to limit the number of instruments. Instrument proliferation is a particular worry in system GMM because the number of instruments is quartic in T. As a rule, Roodman (2009) suggests that the number of instruments is less than the number of countries. To adhere to this, our instrument set is limited to 3 lags per variable (details in Table 3 notes), with the instrument set collapsed to have one instrument per variable and lagged difference, instead of one instrument per variable, lagged difference, and time-period (Roodman, 2009).

Instrument validity further depends on the absence of serial correlation in the idiosyncratic error v_{it}. If we have first order serial correlation in the level equation's error, the instrument $y_{i,t-2}$ is endogenous to the error term in differences $(v_{i,t} - v_{i,t-1})$ making it potentially invalid. We can test for this using AR(1) and AR(2) tests for serial correlation on the errors in the differenced equation (Arellano and Bond, 1991). The test statistic is calculated from estimating the idiosyncratic errors from the differenced equation, which is asymptotically normally distributed under the null hypothesis of no serial correlation. Of course, Δv_{it} is mathematically related to $\Delta v_{i,t-1}$ so evidence of first order serial correlation is uninformative (Roodman, 2009). However, with a significant AR(2) test statistic, this shows serial correlation of order 1 in the errors for the level equation, meaning we should start our instrument set from the second lag to avoid endogeneity. Furthermore, the system GMM estimator has one step and two step versions, where the two-step estimator is asymptotically more efficient than its counterpart (Bond et. al. 2001). In the past, both estimates were reported together because of the downward bias in the two-step estimator's computed standard errors, however, the Windmeijer (2005) correction resolves this bias in the standard errors. In section 3.2, all system GMM estimates are calculated in two-steps with Windmeijer (2005) corrected standard errors. Finally, because the system GMM estimator assumes no contemporaneous correlation between cross-country error terms, time dummies control for this (Sarafidis and Robertson, 2006; Roodman, 2009).

3.2 – Data and Results

To empirically estimate the relationship between wealth inequality and economic growth, we first need a panel covering wealth inequality, GDP, and our control variables for a range of countries. GDP p.c. data (at current prices) are taken from the World Bank Development Indicators database, which uses information from World Bank and OECD national accounts files. Controlling for GDP accounts for the convergence effect and has widely been found to have a negative sign in cross-country endogenous growth models (e.g. Barro 1991). The World Bank database has also provided data for: the investment ratio, measured by gross capital formation as a percentage of GDP; the trade openness, measured as the ratio of imports and exports to GDP; population growth in all age groups; and the inflation rate, given as the GDP deflator. The investment ratio is controlled for because Levine and Renelt (1992) found it to be the most robust determinant of growth among several indicators in their cross-country empirical growth investigation. Trade openness (specifically, trade intensity) has a range of effects on growth such as providing developing countries with access to investment/intermediary goods, typically this is positively associated with economic growth (Yanikkaya, 2003). Inflation captures the effect of macroeconomic instability on investment so should have a negative effect on growth (Fischer, 1993). Populations grow geometrically, while output grows arithmetically therefore higher population growth should depress GDP p.c. (Islam and McGillivray, 2020).

Educational attainment is a proxy for the stock of human capital at the start of the period, our education data are taken from the Barro-Lee educational attainment dataset. In the earlier literature, human capital was measured as the primary/secondary school enrolment ratio, which measures what is being “added”, or the flow into human capital. Since the stock of human capital has the greatest growth effects (for example, through technology use and innovation), years of schooling is a more suitable proxy (Barro, 1991; Perotti, 1996). Furthermore, our proxy for democracy (and institutional quality) is from the Centre for Systemic Peace. The index ranges from -10 (complete autocracy) to 10 (complete democracy) and we assume that more democratic countries have better economic institutions (Islam and McGillivray, 2020). The quality of institutions are controlled for because they have been argued as a fundamental variable to account for cross-country variations in development (Acemoglu et al., 2001; Acemoglu and Johnson, 2005). The degree of democracy, as explained in section 2.1, should partially account for the extent to which changes in wealth distributions affect growth. Both effects work in the same direction, so our polity index should positively affect growth. We further control for the Gini coefficient on (pre-tax) income, taken from the World Inequality Database (WID) to account for general trends in income inequality which may otherwise be absorbed into the effect of wealth.

These variables are similar to those from Islam and McGillivray (2020), who conduct an investigation into the effect of wealth concentration on economic growth. Our studies differ in the wealth data and time period considered. They use one-year growth periods from 2000-2012, estimating the short-term effect of wealth inequality on growth. Here, we measure the long-term effect using 5-year growth intervals between 1995-2020 ($T = 6$) to avoid incorporating short-run growth disturbances (Forbes, 2000). Secondly, Islam and McGillivray use wealth distribution data from Credit Suisse, covering 45 countries with direct wealth distributions observed for 31 countries. The remainder of wealth distributions are constructed from income distribution data. Our wealth inequality data are taken from WID, covering 137 countries with direct wealth distributions for 32 countries and the remainder estimated using other correlates, including income distributions and country clustering (Bajard et al., 2022). A key difference in the wealth data generation is that WID ensures the distribution is in line with Forbes' billionaire dataset, better capturing the very top of the distribution which is typically missed by household surveys (Bajard et al., 2022). The Forbes dataset is used by Islam and McGillivray (2020) as a robustness check and in the main analysis of Bagchi and Svegnar (2015), in both cases having a negative significant effect on growth. The inclusion of this data directly in the wealth distribution should, therefore, more accurately measure the effect of wealth concentration.

Since income inequality is used to estimate wealth distributions, there are concerns of collinearity between the income Gini and the top 10% and 1% wealth shares. There should be a stronger correlation between the income Gini and wealth concentration for countries where wealth data are not directly observed (N=105). We can test the extent of correlation between these variables by calculating the VIF in our full models. The top 1% wealth share and top 10% wealth share have, respectively, VIFs of 2.87 and 2.98 which are well below 10, showing that they do not have strong linear dependence with the other regressors.

3.2.1 – Summary Statistics

Table 1 presents our summary statistics. Immediately, the minimum value of the growth rate is alarming, at -2.8. However, if we calculate an 'average' growth rate over the 5-year period, the implied growth rate is -0.56 per year. This growth rate was seen in Iraq between 1990-1995, a plausible growth rate during a war. Upon further inspection, we see that all three inequality measures, the population growth rate, and

Table 1: Summary Statistics

Variable	Panel	Mean	Std. Dev.	Min.	Max.	Observations
Real GDP p.c. 5-year Growth Rate	Overall	0.1974	0.3612	-2.8093	1.4415	N = 802
	Between		0.1224	-0.2140	0.6021	n = 137
	Within		0.3400	-2.4688	1.5006	T = 5.86
Log of Real GDP per capita	Overall	8.3265	1.6077	4.7343	11.6730	N = 815
	Between		1.5313	5.2543	11.2983	n = 137
	Within		0.5080	6.6170	9.6750	T = 5.95
Average Years of Schooling	Overall	8.2016	2.9325	0.981	13.275	N = 685
	Between		2.8375	1.7404	13.0924	n = 137
	Within		0.7699	6.1344	10.7496	T = 5
Top 10% Wealth Share	Overall	0.6305	0.0784	0.4084	0.9017	N = 822
	Between		0.0731	0.4440	0.8602	n = 137
	Within		0.0289	0.4649	0.7763	T = 6
Top 1% Wealth Share	Overall	0.2983	0.0818	0.1208	0.5729	N = 822
	Between		0.0764	0.1454	0.5261	n = 137
	Within		0.0297	0.1208	0.4515	T = 6
Gini Coef. on Income	Overall	0.5685	0.0908	0.3396	0.7789	N = 822
	Between		0.0880	0.3879	0.7454	n = 137
	Within		0.02341	0.4812	0.6893	T = 6
Investment Ratio	Overall	0.2333	0.0758	0.0109	0.7940	N = 792
	Between		0.0564	0.0993	0.4098	n = 137
	Within		0.0523	0.0104	0.6484	T = 5.78
Population Growth Rate	Overall	0.01369	0.01512	-0.1688	0.09219	N = 822
	Between		0.01185	-0.0117	0.0498	n = 137
	Within		0.0094	-0.1457	0.0685	T = 6
Inflation Rate	Overall	0.1397	0.9726	-0.3019	26.3012	N = 802
	Between		0.4529	-0.0059	5.2498	n = 137
	Within		0.8586	-5.1217	21.1910	T = 5.85
Polity Index	Overall	4.1746	6.1599	-10	10	N = 664
	Between		5.8192	-10	10	n = 134
	Within		2.0614	-7.6253	12.5747	T = 4.95
Trade Openness	Overall	0.8370	0.5527	0.0002	4.2043	N = 792
	Between		0.5283	0.2102	3.6018	n = 137
	Within		0.1681	-0.1942	1.5646	T = 5.78

investment ratio have low within-country standard deviations. These variables are persistent and have small within-country changes over time so we should be cautious in interpreting their effects when using the FE estimator. We can also see that $T \approx 5$ for the polity index and years of schooling since these variables were not available for the year 2020. However, since they are both controls, and all regressors are measured in $(t-1)$ in our regressions, this should not present an issue. On average, 63.1% and 29.8% of household wealth are owned, respectively, by the top 10% and top 1% of wealth owners in each country – a large proportion of household wealth. Even more alarming is that in the countries with the most concentrated wealth, we see that 90.2% and 57.3% of wealth is owned by the top 10% and 1%, respectively.

3.2.2 – Pooled OLS, Fixed Effects, and Random Effects Results

Table 2 presents the results from our POLS, FE and RE regressions in which the 5-year growth rate is the dependent variable. In the baseline POLS specifications, columns (1) and (3), lagged log GDP p.c. is negative and highly significant, in line with the convergence findings. In fact, there is evidence of convergence for every regression in Table 2. There is also a positive and highly significant effect of school attainment, highlighting the importance of human capital. The Gini coefficient on income is positive and insignificant where we measure wealth inequality as the top 10% share, but positive and highly significant measuring wealth inequality as the top 1% share. For our wealth inequality measures, the top 10% share is positive and significant only at the 10% level while the 1% share is negative and significant at the 5% level. When we add to this baseline model, controlling for the investment ratio, population growth, inflation, our polity index, and trade openness the coefficients on the inequality measures change (columns (2) and (4)). The income inequality coefficients increase in magnitude and are significant at the 5% level in both full models. This indicates that an increase in the income Gini of 1 s.d. would increase the 5-year growth rate by 1.2 - 2.6 percentage points (pp). Our coefficient on the top 1% wealth share remains negative which is now significant at the 1% level in column (4), showing its downward effect on growth. This shows that the omitted variables in (3) were causing a positive bias. The estimate implies a 1 s.d. increase in the 1% wealth share reduces 5-year growth by 5.5 pp. For the top 10% wealth share, the coefficient remains positive but is no longer statistically significant, again showing an upward bias in the baseline estimation.

Table 2: Pooled OLS, FE, and RE Regressions

	Pooled OLS				Fixed Effects				Random Effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ln(GDP p.c.)$_{(t-1)}$	-0.0650***	-0.0771***	-0.0595***	-0.0737***	-0.3703***	-0.4206***	-0.3706***	-0.4206***	-0.0949***	-0.1076***	-0.0948***	-0.1079***
	(0.0132)	(0.0136)	(0.0118)	(0.0127)	(0.0452)	(0.0379)	(0.0452)	(0.0378)	(0.0116)	(0.0124)	(0.0116)	(0.0124)
Wealth Share(10%)$_{(t-1)}$	0.4924*	0.2405			-0.3007	-0.1782			0.0265	-0.1974		
	(0.2838)	(0.2730)			(0.5859)	(0.5288)			(0.2181)	(0.2594)		
Wealth Share(1%)$_{(t-1)}$			-0.5307**	-0.6664***			-0.2611	-0.1956			0.0063	-0.1613
			(0.2537)	(0.2550)			(0.6330)	(0.5003)			(0.2162)	(0.2506)
Income Gini$_{(t-1)}$	0.2159	0.5011**	0.9709***	1.1018***	0.8768	0.3368	0.8557	0.3558	-0.2337	-0.0001	-0.2191	-0.0195
	(0.2585)	(0.2547)	(0.1675)	(0.1750)	(0.7429)	(0.6530)	(0.7738)	(0.6529)	(0.2128)	(0.2527)	(0.2187)	(0.2566)
School Attainment$_{(t-1)}$	0.0368***	0.0331***	0.0362***	0.0321***	0.0978***	0.0945***	0.0980***	0.0946***	0.0313***	0.0290***	0.0313***	0.0292***
	(0.0081)	(0.0089)	(0.0082)	(0.0088)	(0.0350)	(0.0232)	(0.0351)	(0.0232)	(0.0059)	(0.0075)	(0.0058)	(0.0075)
Investment Ratio$_{(t-1)}$		0.3220		0.2831		0.0019		-0.0004		0.0732		0.0761
		(0.2315)		(0.2270)		(0.2423)		(0.2425)		(0.1656)		(0.1656)
Population Growth$_{(t-1)}$		-1.9567*		-1.9964*		-0.2313		-0.2273		-1.5749*		-1.5648*
		(1.1383)		(1.0764)		(1.2849)		(1.2848)		(0.9446)		(0.9444)
Inflation$_{(t-1)}$		-0.0255***		-0.0268***		-0.0325***		-0.0326***		-0.0326***		-0.0326***
		(0.0091)		(0.0091)		(0.0121)		(0.0121)		(0.0107)		(0.0107)
Polity$_{(t-1)}$		0.0019		0.0016		0.0189***		0.0189***		0.0016		0.0016
		(0.0026)		(0.0025)		(0.0060)		(0.0060)		(0.0023)		(0.0023)
Trade Openness$_{(t-1)}$		0.0944***		0.0967***		0.3633***		0.3640***		0.0922***		0.0908***
		(0.0273)		(0.0272)		(0.0829)		(0.0830)		(0.0268)		(0.0267)
Observations	680	582	680	582	680	582	680	582	680	582	680	582
R^2	0.3033	0.3758	0.3041	0.3831	0.1911	0.3001	0.1910	0.3002	0.2618	0.3511	0.2616	0.3523
Countries	137	123	137	123	137	123	137	123	137	123	137	123

Dependent Variable is the 5-year growth in real GDP per capita. Robust standard errors are in parentheses, *, **, *** denote, respectively, the 10%, 5%, and 1% significance levels. R^2 is the within-R^2 for fixed effects and the overall-R^2 for random effects. Period dummies are included in the regressions but not reported for brevity.

However, the POLS estimator suffers from the Nickell bias as well as potential further endogeneity from the fixed effects in the error term. Columns (5) - (12) show the Fixed and Random Effects estimates which can give us insights into the characteristics of our model. As outlined in 3.1, the FE estimator is consistent with and without correlation between our regressors and the fixed effects, while the RE estimator is inconsistent when these are correlated. A Hausman test has been used to understand whether this correlation exists in our model. However, since we have used robust standard errors (due to worries of autocorrelation, which are shown later in the section), the Hausman statistic is no longer χ^2 distributed under the null of no correlation. To overcome this issue, we use the regression-based 'robust' Hausman test which allows for general heteroskedasticity and serial correlation while testing the same null hypothesis. This test is implemented by the xtoverid command in Stata (Schaffer and Stillman, 2010). In all regressions, the robust Hausman test rejects the null with a p-value of 0.0000, so there is evidence of correlation between our regressors and the country fixed effects. Therefore, we should trust the FE estimates because they are consistent while the RE and POLS estimates are inconsistent.

From the FE estimates in columns (5) – (8), the significant coefficient of our polity index shows the positive effect of democracy and its associated institutions on growth. Furthermore, the negatively significant coefficient on inflation shows the damaging effect of macroeconomic instability on growth, but the significantly positive coefficient on school attainment shows the positive effect of a larger human capital stock. When considering the coefficients of interest, the top 1% and top 10% wealth shares have negative coefficients signalling that higher wealth concentration is a barrier to growth. However, in all cases, the effect is not significant even at the 10% level, so the regressions imply that there is no effect of top wealth concentration on growth. These results contrast those in (1) - (4), implying a downward bias in POLS.

3.2.2 – System GMM Results

As discussed in 3.1, the FE estimator (and all other estimators used in Table 2) is biased in our regression setting due to the LDV. In Table 3 we present the results of our theoretically superior system GMM estimates, which aren't affected by the Nickell bias, on the same regression specifications. The choices for our system GMM model are outlined in Table 3, these are largely the same as those used by Islam and McGillivray (2020). We can immediately see that our coefficients on GDP per capita are between the FE and POLS estimates, verifying the initial check from Bond (2002) that our estimates are not severely biased. Our AR(1) tests for autocorrelation show evidence of first order serial correlation in the idiosyncratic disturbance term for the equation in differences. As discussed in section 3.1, this is uninformative. In the baseline models, our AR(2) test returns a p-value of 0.017, there is evidence of second order serial correlation with over 95% confidence so the lagged instrument on our LDV is endogenous, hence invalid. When we add our additional controls in columns (2) and (4), we see that this second order serial correlation is accounted for by our additional explanatory variables, implying a misspecification for the baseline regressions. In the full models, there is no evidence of second order serial correlation, so

Table 3: System GMM Regressions

	System GMM				System GMM High Quality Sample	
	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(\text{GDP p.c.})_{(t-1)}$	-0.1493***	-0.1768***	-0.1548***	-0.1785***	-0.1310***	-0.1504***
	(0.0236)	(0.0670)	(0.0248)	(0.0664)	(0.0530)	(0.0612)
Wealth Share $(10\%)_{(t-1)}$	-0.2547	0.4908			-0.1363	
	(0.7120)	(0.8708)			(0.4938)	
Wealth Share $(1\%)_{(t-1)}$			-0.3568	0.7915		0.5843
			(1.2139)	(0.8740)		(1.2520)
Income $\text{Gini}_{(t-1)}$	0.6067	-2.0511**	0.6678	-2.3054**	-0.2359	-1.1153
	(0.7437)	(0.8690)	(1.0196)	(1.1132)	(0.5886)	(1.3464)
School $\text{Attainment}_{(t-1)}$	0.0655***	0.0372	0.0531*	0.0315	0.0557	0.0406
	(0.0217)	(0.0365)	(0.0307)	(0.0337)	(0.0421)	(0.0340)
Investment $\text{Ratio}_{(t-1)}$		0.5357		0.5907*	0.3637	0.1080
		(0.3296)		(0.3402)	(0.8255)	(0.7571)
Population $\text{Growth}_{(t-1)}$		-0.1940		-0.2481	-2.3440	-3.8203
		(1.5663)		(1.4563)	(4.9075)	(4.1325)
$\text{Inflation}_{(t-1)}$		-0.0288***		-0.0269***	-0.5085***	-0.4805***
		(0.0070)		(0.0079)	(0.1407)	(0.0946)
$\text{Polity}_{(t-1)}$		-0.0037		-0.0026	-0.0127	-0.0112
		(0.0081)		(0.0079)	(0.0158)	(0.0224)
Trade $\text{Openness}_{(t-1)}$		0.3309**		0.3466**	0.1277***	0.1257***
		(0.1436)		(0.1414)	(0.0389)	(0.0318)
Period Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	680	582	680	582	155	155
No. Countries	137	123	137	123	32	32
No. Instruments	17	32	17	32	24	24
AR(1) p-value	0.001	0.001	0.002	0.012	0.026	0.034
AR(2) p-value	0.017	0.457	0.017	0.431	0.324	0.343
Hansen p-value	0.620	0.229	0.590	0.267	0.685	0.664
Diff-in-Hansen p-value	0.534	0.657	0.478	0.572	0.472	0.429

Dependent variable is the 5-year growth of real GDP per capita. All explanatory variables are treated as potentially endogenous. Estimated coefficients obtained through 2-step system GMM. First and second lags of the dependent variables are used as instruments for the equation in differences, whereas first differences of the variables are used as instruments for the equation in levels. The instrument matrix is collapsed using the xtabond2 command from Roodman (2009) to avoid instrument proliferation. Windmeijer (2005) corrected standard errors are in parentheses. *, **, *** denote, respectively, the 10%, 5%, and 1% significance levels. Diff-in-Hansen is the p-value examining the validity of instruments for the level equation.

instrumenting our LDV with its first lag is still appropriate. Our Hansen tests of joint validity of the instrument set fails to reject the null hypothesis in all cases, implying joint validity of our instruments.

Further, our difference-in-Hansen tests on the instruments for the level equation also fail to reject the null hypothesis of joint validity, so there is no evidence of the Blundell-Bond (1998) requirement being violated. In contrast to our FE estimates, we see no significance of the polity index. This reflects an argument made in Glaeser et al. (2004) that the role of dictators can be good or bad, so the average effect of polity over a range of countries could be insignificant. Considering this, the polity index is a weaker proxy for institutional quality. Even though it still accurately measures the degree of democracy, a less democratic country does not necessarily imply weaker institutions. The positive and significant coefficient on trade openness is in line with the estimates in Table 2 (showing that trade intensity positively influences economic growth), however, the stock of education has an insignificant effect on growth. The coefficients on top 1% and top 10% wealth shares are highly insignificant in (2) and (4), meaning wealth inequality has no effect on economic growth. This drastically contrasts the robust negative results found by Islam and McGillivray (2020), especially since our measure of wealth inequality includes billionaire wealth which, according to their results, should reinforce the negative effect of wealth concentration on growth. The coefficients on income inequality also stand in contrast to theirs. While they found an insignificant effect of income inequality, we see negative and statistically significant coefficients for our full models. The contrasting results could be because our dataset covers more countries so aggregating the effects can lose their significance, and the data quality could be poorer due to different data collection practises across countries. GDP data quality varies systematically between countries at different stages of development, so Dawson et al. (2001) suggest testing cross-country results on a smaller set of countries as a sensitivity check.

Columns (5) and (6) estimate the full model on a dataset only containing countries with full wealth distributions in the WID dataset (see appendix). Here, the instrument set for the equation in differences is restricted to only the first lag of each variable to avoid instrument proliferation. This serves as a higher quality dataset with reduced measurement error, (assuming full wealth data indicates better data collection practises) while bringing our country coverage closer to that of Islam and McGillivray (2020). The significance of our income inequality coefficient is lost, which is in line with their findings, but we still see an insignificant effect of wealth concentration on economic growth. Since the restricted dataset has a mean real GDP p.c. of $36,598 (and the unrestricted dataset mean is $15,924), this sample of countries is richer, which could explain the different income inequality effects. Multiple studies have found income inequality to positively affect growth in richer countries and negatively affect growth in poorer countries, so the negative effect from lower income countries is likely being captured in columns (2) and (4) (Barro, 2000; Castelló-Climent, 2010; Fawaz et al., 2014). Even with direct wealth data, we see an insignificant effect of both wealth shares on subsequent economic growth; this result is more puzzling. Islam and McGillivray (2020) consider annual growth periods and their results are robust to 3-year growth periods, but both are relatively short and susceptible to business cycle effects. While it could be beneficial to re-estimate our model with a shorter time delta, the Barro-Lee education data is measured every 5-years, so interpolating the educational attainment could lead to inaccurate conclusions. Bagchi and Svejnar (2015) use 5-year growth periods, but their wealth concentration measure is fundamentally different, focusing only on billionaires, so we wouldn't expect to find the same impact of wealth inequality. Independent from the literature, our results imply that wealth concentration has no effect on economic growth; this contrasts the model described in 2.1, however, considering the range of empirical evidence did not support the model (described in 2.2) this is not an alarming result.

Table 3 seems to have a pattern of insignificant coefficients on persistent variables (e.g. wealth inequality). Cingano (2014) notes that by exploiting within-country variation with system GMM, variables which are highly persistent or trend in one direction are estimated with lower precision. System GMM identification relies on lagged differences explaining variation in levels, which might not be the case for wealth inequality. Cingano suggests using annual data with a Pooled Mean Group (PMG) estimator which calculates short and long run coefficients for each growth determinant as Bassanini and Scarpetta (2002) estimate for human capital. However, the PMG estimator requires 'longer T' data, so we would need annual control variables or a much longer panel of wealth inequality data (Tan, 2009), which is beyond the scope of this study.

3.2.3 – Conclusion

Our analysis shows that wealth concentration has no significant long run effect on economic growth across our sample of countries. We found a negative effect of income inequality, measured by the Gini coefficient, likely due to its effect in lower income countries. Considering the limited wealth inequality literature, our results imply that the concentration of wealth into the top 10% and 1% of wealth-holders has a negative short-term effect on growth, but no effect on long-term economic growth. From the theoretical mechanisms in section 2.2, this result is consistent with the idea that wealth concentration reduces domestic demand and investment in physical capital to slow short-run growth, but in the long run, the political-economy, human capital, and aspirational effects balance out. This conclusion is unsatisfying and, largely, unjustified. Overall, the empirical literature would benefit from a longer and more accurate panel of wealth inequality for 2 future studies: (i) so that 5 and 10-year growth periods could be considered to understand how the effect of wealth inequality varies temporally (as Halter et al. (2014) do for the effect of income inequality). (ii) so that a Pooled Mean Group estimator could be employed on an annual dataset to allow the effect of wealth inequality to vary over time, as Bassanini and Scarpetta (2002) use for the effect of human capital.

Policymakers should be cautious in using these findings. Even though our results indicate that wealth concentration has not empirically led to reductions in in GDP per capita, we have not considered the effect of bottom wealth inequality, or wealth poverty, on economic growth. We should also consider why we are using GDP per capita as our outcome of interest, is it purely because of its nature as an economic target, or are we instead concerned with aggregate welfare? If we care more about the latter, then GDP per capita growth does not necessarily improve welfare. The Stiglitz-Sen-Fitoussi report (2009) covers the issues with GDP targeting in detail, suggesting a dashboard of target indicators which are tailored to each country's need. This represents a shift from measuring production to measuring people's wellbeing, specifically the diversity of experiences, not just average wellbeing. There would, therefore, be an innate benefit from reducing inequality over and above its effect on GDP per capita. In conclusion, while it is important to understand the relationship between inequality and growth, policymakers must recognise that reducing inequality is a crucial step towards inclusive and sustainable development.

Appendix:

Data:

Country List:

Countries with generated wealth distribution data (N=105):
Afghanistan, Albania, Algeria, Argentina, Armenia, Australia, Bahrain, Bangladesh, Belize, Benin, Bolivia, Botswana, Brazil, Brunei Darussalam, Bulgaria, Burundi, Cambodia, Cameroon, Canada, Central African Republic, Chile, Hong Kong, Macao, Colombia, Congo, Costa Rica, Cote d'Ivoire, Cuba, Czech Republic, Democratic Republic of the Congo, Dominican Rep., Ecuador, Egypt, El Salvador, Gabon, Gambia, Ghana, Guatemala, Guyana, Haiti, Honduras, Iceland, Indonesia, Iran (Islamic Republic of), Iraq, Israel, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Kuwait, Kyrgyzstan, Lao (People's Democratic Republic), Lesotho, Liberia, Libyan Arab Jamahiriya, Malawi, Malaysia, Maldives, Mali, Mauritania, Mauritius, Mexico, Morocco, Mozambique, Myanmar, Nepal, New Zealand, Nicaragua, Niger, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Qatar, Moldova (Republic of), Romania, Rwanda, Saudi Arabia, Senegal, Serbia, Sierra Leone, Singapore, Slovakia, Slovenia, South Africa, Sri Lanka, Sudan, Swaziland, Sweden, Syrian Arab Republic, Tajikistan, Thailand, Togo, Trinidad and Tobago, Tunisia, Turkey, Uganda, Ukraine, United Arab Emirates, United Republic of Tanzania, Uruguay, Venezuela, Viet Nam, Zimbabwe.

Countries with full wealth distribution data (N=32):
Austria, Belgium, China, Croatia, Cyprus, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, India, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Norway, Poland, Portugal, Republic of Korea, Russian Federation, Slovakia, Slovenia, South Africa, Spain, Switzerland, USA, United Kingdom.

World Inequality Database (WID) provided data for the top 10% wealth shares, top 1% wealth shares, and the Gini coefficient on income. Data were downloaded directly in Stata using the WID Stata package -ssc install wid- which downloads data from wid.world (Blanchet, 2021).

World Bank Development Indicators, 2023 provided data on GDP per capita (in current USD prices), the investment ratio (the ratio of gross capital formation to GDP), trade openness (the sum of total exports and imports over GDP), the inflation rate (the GDP deflator), and the population growth rate (all ages). The online database can be accessed through https://databank.worldbank.org/source/world-development-indicators.

Barro-Lee Education Dataset provided data on educational attainment, specifically the average years of education for the 15–64-year-old population, measured in 5-year intervals. The online database can be accessed from http://www.barrolee.com/.

Centre for Systemic Peace provided our polity variable from their 'Polity 5' project. This variable is measured on a scale from -10 to 10 where -10 is complete autocracy and 10 is complete democracy. The database is available from https://www.systemicpeace.org/inscrdata.html.

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