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Vu, Trung V.

Department of Economics, University of Otago

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Trung V. Vu*

Department of Economics, University of Otago, Dunedin, New Zealand

Abstract

Does state history matter for contemporary income distribution? Employing data for up to 153 countries, this paper examines the extent to which accumulated statehood experience, obtained over six millennia, affects the current level of income inequality. To capture the historical depth of experience with state-level institutions, I use an extended measure of state history, constructed from 3500BCE to 2000CE. The results indicate that the relationship between state history and income inequality exhibits a U-shaped pattern. Specifically, statehood experience up to a point helps reduce income inequality. Nevertheless, an excessive duration of state history is conducive to more unequal income distribution. These findings are largely robust to performing a battery of sensitivity tests.

Keywords: state history, income inequality, deep determinants, comparative development.

JEL Classifications: O11, O15, O43, N00.

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Correspondence: Department of Economics, University of Otago, PO Box 56, Dunedin 9054, New Zealand. Email: trung.vu@postgrad.otago.ac.nz or trungvu.ueb@gmail.com.

1. Introduction

The conventional wisdom of development economics postulates that high and rising levels of income inequality are one of the most serious impediments to achieving sustainable economic development across the world (see, e.g., Persson & Tabellini, 1994; Easterly, 2007; Piketty, 2014; Berg et al., 2018). Income inequality, in particular, may be associated with an unequal distribution of power and rent-seeking within a country (Bartels, 2009; Stiglitz, 2012). More recently, Berg et al. (2018) demonstrate that inequality lowers economic growth via reducing investments in human capital and increasing fertility. Furthermore, an unequal distribution of income is strongly correlated with crime, lower educational outcomes, the prevalence of mental illness, political instability, and social consensus, which ultimately hinder economic growth (Alesina & Perotti, 1996; Easterly, 2007; Pickett & Wilkinson, 2010; Berg et al., 2018). As illustrated in Figure 1, there exist striking variations in income inequality levels across countries, measured by the Gini coefficient of disposable income. For these reasons, it is important to investigate the drivers of income inequality.

There are a large number of studies examining the determinants of income inequality, using cross-country data (see, e.g., Furceri & Ostry, 2019). The seminal article by Kuznets (1955), for instance, demonstrates that economic development at first induces higher income inequality but eventually lowers it. A recent study by Jauch and Watzka (2016) posits that financial development exerts a positive influence on the level of income inequality. In a similar vein, Haan and Sturm (2017) propose that income inequality is attributable to financial development, financial liberalization and banking crises. Furthermore, Furceri and Ostry (2019) provide a thorough and critical review of the development-inequality nexus. Using model-averaging methods, the authors suggest that the robust drivers of income inequality include demographic factors, income levels, and globalization.[1]

A major challenge with drawing causal inference in this literature stems from endogeneity concerns. More specifically, economic development and financial development, for example, may affect the extent to which a country can adopt an egalitarian distribution of income, but reverse causation potentially exists, making it difficult to obtain valid statistical

[1] These factors have been regarded as key determinants of income inequality across the world in many studies (see, e.g., Lam & Levison, 1992; Deaton & Paxson, 1997; Razin et al., 2002; Lee, 2014; Jauch & Watzka, 2016; Haan & Sturm, 2017). Gupta et al. (2002) argue that income inequality is more prevalent in countries with poor institutional quality. See also Förster and Tóth (2015) for a detailed review of related literature.

inference.[2] Achieving causal interpretations, therefore, critically relies on identifying valid instrumental variables, which appears to be challenging. In this respect, Bazzi and Clemens (2013) reveal that the assumptions of instrument validity and relevance are commonly violated in growth regressions. These problems, if not accounted for, may yield a spurious relationship between economic performance and income inequality. From a conceptual perspective, if income inequality is induced by financial development, economic growth and globalization, a key question emerges as "What fundamentally drives the distribution of income?" It follows from this argument that these "proximate" determinants of inequality fail to explain the deep origins of income distribution, essentially because they are interrelated with and jointly determined by the level of income inequality. This necessitates examining the causes of income inequality from a deep or fundamental determinants approach.[3]

Despite the above challenges of identification, the literature on the determinants of income inequality helps advance our understanding of factors affecting the distribution of income. This is essential for forming relevant policies to combat income inequality across countries. Nevertheless, one aspect that remains largely unexplored in previous studies is whether the formation and development of historical states, measured by an index of state history, matters for cross-country differences in income distribution. Therefore, the primary objective of the current study is to address this question through which it delivers a fresh perspective to the following lines of inquiries.

First, this paper contributes to a large and emerging strand of research investigating the causes of income inequality (Furceri & Ostry, 2019). As reviewed earlier, a central focus of previous studies is the contemporary and proximate drivers of income distribution (e.g., economic growth, financial development, trade openness and demographic factors, to name just a few). Much less is known about the extent to which historical factors matter for today's income distribution. Furthermore, it is useful to examine the relationship between history and income inequality because designing effective policies necessitates understanding the deep origins of a country's income inequality. For this reason, this paper goes beyond the exisiting literature by exploring the effect of the early development of historical states on income inequality. Importantly, it is plausible that contemporary degrees of income inequality do not

[2] Specifically, income inequality may impede economic development via reducing investments in human capital, population health and deterring social consensus (Alesina & Perotti, 1996; Easterly, 2007; Berg et al., 2018).

[3] Specifically, the deep determinants literature attempts to investigate the fundamental causes of comparative development across countries, such as institutions, geography, genetic characteristics, statehood experience. See Owen (2017) for a review of related studies.

exert a direct infuence on the formation and development of historical states, dating back several thousands of years ago. This at least in part suggests that state history can provide plausibly exogneous sources of variation in present-day income inequality levels.

Second, this study builds upon a number of studies arguing that the early development and formation of historical states lie at the roots of global income differences (e.g., Bockstette et al., 2002; Chanda & Putterman, 2007; Putterman & Weil, 2010; Hariri, 2012; Ang, 2013b, 2013a; Borcan et al., 2018).[4] In particular, Bockstette et al. (2002) construct a measure of state antiquity that captures the historical depth of experience with state-level institutions across countries. Using this indicator, the authors demonstrate that an early start confers a country with strong bureaucratic capabilities, which helps foster economic prosperity. Subsequent studies document a positive effect of state history on institutional quality and financial development (Ang, 2013b, 2013a). Therefore, the current research complements these studies by focusing on the role of state history in affecting income inequality. To my knowledge, this issue remains poorly understood in the long-run comparative development literature although the drivers of income inequality have been extensively examined in many studies. Later, I will control for income per capita, institutional quality, financial development, and other variables, which helps partial out the effect of state history on economic developmet. Even when I factor out these factors, statehood experience still exerts a statistically significant influence on today's degrees of income inequality (Sections 2 and 5).

Importantly, previous studies in this strand of literature have predominantly focused on a linear relationship between state history and economic performance. For this reason, they fail to explain why very old states such as Turkey, China and Iraq suffer from underdevelopment relative to those with an intermediate length of statehood experience like the UK, Japan and Denmark (Borcan et al., 2018).[5] Using an extended measure of state history constructed by Borcan et al. (2018), this paper offers an explanation for this long-standing fact by documenting that the relationship between accumulated statehood experience across six millenia and income inequality follows a U-shaped pattern. In particular, the historical depth of experience with state-level institutions up to a point helps reduce income inequality. Nevertheless, countries

[4] As explained earlier, these studies are broadly related to the deep determinants literature that explores the deep roots of global income differences (Owen, 2017).

[5] As far as I am aware, Borcan et al. (2018) is the first and only study that documents a hump-shaped relationship between state antiquity and GDP per capita. This provides an explanation for income differences between very old states and those with an intermediate level of state history. The authors, in particular, argue that excessive state experience may be associated with an overcentralization of power, resulting in institutional stagnation and lagged productivity.

with an excessive duration of state experience also come at a cost of higher income inequality. These findings are robust to conducting a series of sensitivity tests. Overall, the results are consistent with Borcan et al. (2018) who show that state history has a hump-shaped effect on GDP per capita across countries rather than a linear relationship as previously established in the literature. By doing so, this paper lends additional support to the non-monotonic relationship between statehood experience and economic performance.

The rest of this paper proceeds as follows. Section 2 presents the analytical framework explaining how statehood experience mattters for the distribution of income. Section 3 discusses econometric methods and variables' descriptions. The main findings are presented in Section 4. Section 5 discusses the results of robustness tests, and the last section conludes.

2. The economic argument

The central idea of this paper postulates that the historical depth of experience with state-level institutions, obtained over six millennia, exerts a persistent and U-shaped effect on today's income inequality levels. More specifically, I propose that accumulated statehood experience, up to a point, helps strengthen a more egalitarian distribution of income within a country. Nevertheless, an excessive duration of state history leads to institutional stagnation and the emergence of powerful entrenched groups, which worsens a country's income distribution.

A key line of inquiry in the comparative development literature examines the role of statehood experience in explaining income differences across countries (see, e.g., Bockstette et al., 2002; Chanda & Putterman, 2007; Hariri, 2012; Ang, 2013a, 2013b; Borcan et al., 2018). Accordingly, an early development of historical states confers a country with better fiscal and legal capabilities, which critically affect investments and economic growth.[6] Furthermore, Bockstette et al. (2002) highlight that long-standing states are endowed with more competent bureaucratic capabilities obtained through learning by doing. Hence, a well-functioning state is of importance for the process of institutional building because good institutions typically proliferate in countries with competent and well-trained bureaucrats (Ang, 2013b).[7] By

[6] This is in line with Dincecco and Katz (2016) who document that state capacity exerts a positive influence on long-run economic performance. These scholars, however, employ national government revenues per capita and non-military spending per capita to capture cross-country differences in state capacity. By contrast, the state history index constructed by Borcan et al. (2018) reflects experience with state-level institutions obtained throughout thousands of years in history. See Johnson and Koyama (2017) for a critical review of the literature on the relationship between state capacity and economic development.

[7] Several East Asian economies have recently experienced robust economic growth, which is mainly attributable to their powerful states. More specifically, a key characteristics of these states lies in a strong ability to enforce

contrast, newly established states tend to suffer from weakened fiscal capacity, thus deterring the efficiency of public administration (Besley & Persson, 2009; Borcan et al., 2018).

The above narrative suggests that countries lacking statehood experience suffer from weaker fiscal and organizational capabilities, leaving them with poorer institutional quality. This may intensify income inequality within a country through hindering progressive redistribution of income. This argument is consistent with the findings that income inequality tends to prevail in countries with inefficient tax administration and poor governance, which reduce the progressiveness of the tax system (Gupta et al., 2002). It is also plausible that strengthened fiscal capabilities allow a country to provide pro-poor social services that contribute to a more egalitarian society. Furthermore, a short duration of statehood experience implies a dearth of legitimate laws and regulations, which increases corrupt practices by public officials (Owen & Vu, 2020).[8] Therefore, government bureaucrats in those countries with limited fiscal capacity to raise taxes tend to misuse public resources at the expense of the rest of society, leading to greater income inequality.[9]

Borcan et al. (2018) argue that newly established states may experience greater political instability. This is mainly attributable to their frequently changing regimes caused by predatory attack and internal conflicts. Meanwhile, conventional wisdom holds that political instability is detrimental to economic prosperity and the distribution of income (see, e.g., Alesina & Perotti, 1996; Barro, 1996). It is widely perceived that a longer history of experience with state-level institutions supports linguistic unity and national identity, thus fostering social trust and interactions (e.g., Diamond, 1997; Temple, 1998; Chanda & Putterman, 2007). A unified society reduces conflicts and political instability, which helps improve its income distribution. Furthermore, Michalopoulos and Papaioannou (2013) find that internally fragmented societies are characterized by widespread poverty, thus exacerbating income differences within a country. A final argument holds that improved fiscal and legal capabilities, reflected in a longer

rules (legal capacity) and collect tax revenues to finance development objectives (see, e.g., Wade, 1990; Studwell, 2013). This suggests that the state plays an essential role in fostering economic growth and development.

[8] Consistent with this view, there exists a hypothesis that greater instability in newly established states leads to more intense corruption because officials tend to expropriate public resources when their window of opportunity is short (see, e.g., Campante et al., 2009; Ferraz & Finan, 2011; Vu, 2020).

[9] Part of the arguments presented here implies that statehood experience may influence income distribution through affecting productivity, income levels, or the quality of institutions, which may be regarded as the proximate determinants of income inequality. Later, I will control for GDP per capita, institutional quality and other drivers of income inequality, which does not alter my findings. Hence, state history still exerts a direct influence on income inequality even when I partial out these proximate factors to reduce obtaining a spurious regression (see more details in Section 5).

history of state institutions, improve investments in human capital formation and health services, which in turn lower income inequality.

However, I postulate that an excessive duration of state experience also comes with a cost of greater income inequality. The basic explanation for this argument is that a very long history of statehood is associated with institutional stagnation, corruption and reduced productivity, caused by an over-centralization of power, as suggested by some recent studies (see, for instance, Hariri, 2012; Lagerlöf, 2016; Borcan et al., 2018; Harish & Paik, 2019; Owen & Vu, 2020). This idea builds upon an early contribution by Olson (1993) who asserts that the first historical state emerged to address collective action problems in large communities and to protect farmers from attacks and expropriation by predators. Accordingly, stable states were developed when "roving bandits" were replaced by "stationary bandits" sustained by tax collecting tax revenues rather than by plundering. Nevertheless, Olson (1982, 1986) contends that very old and autonomous states may be conducive to the emergence of powerful elites. These entrenched groups eventually turned into "roving bandits", maximizing private gains with the cost borne by the rest of the population. This institutional stagnation results in persistent income differences in societies with a very long history of statehood. It follows from these discussions that very long state history is harmful to a country's distribution of income.

In line with the above proposition, subsequent empirical studies find that very old and autonomous states suffer from underdevelopment. Hariri (2012), for instance, demonstrates that former colonies with excessive statehood experience is associated with autocratic institutions. The basic intuition behind this finding is that state experience at the timing of colonization limited the extent to which European colonizers could set up democratic institutions. Lagerlöf (2016) indicates that autocrats in very experienced states obtained large extractive capacity over time, thus becoming resistant to transiting toward democracy. Borcan et al. (2018) propose that very long-standing states lagged behind those with an intermediate length of statehood in terms of income per capita. This is partly attributable to reduced productivity caused by an over-centralization of power of powerful and entrenched groups.

More recently, Owen and Vu (2020) contend that excessive statehood experience is positively associated with corruption levels mainly because powerful interest groups in these societies are more likely to exploit public resources for personal gain. Further, corrupt practices tend to be more widespread in states with an excessive length of state history because stability increases the interaction between government bureaucrats and the private sector (Campante et al., 2009). These findings suggest that income inequality also prevails in older and more

autonomous states besides those lacking statehood experience as presented earlier. Hence, the main hypothesis of this paper posits that the relationship between state history and income inequality follows a U-shaped pattern.[10]

3. Estimation strategies

3.1. Model specification

I hypothesize that state history and income inequality exhibit a U-shaped relationship. To examine this proposition, I estimate the following model:

$$GINI_i = \alpha + \beta Statehist_i + \gamma Statehist\_sqr_i + \delta X_i + \varepsilon_i$$

in which $GINI$ denotes an index of disposable income inequality. $Statehist$ and $Statehist\_sqr$ are a measure of state antiquity and its squared term, respectively. β and γ capture the hypothesized non-monotonic relationship between state history and income inequality. X is a vector of main control variables. ε represents the unobserved error component. Subscript i stands for country i.

I estimate the benchmark model using OLS regression. A key issue of drawing causal interpretations is omitted variable bias. More specifically, this potential bias exists if an omitted factor is correlated with both state history and the dependent variable, thus confounding the OLS estimates. It is worth emphasizing that reverse causation is unlikely to exist in this context. This is because it is difficult to envisage a direct channel of influence running from today's degrees of income inequality to the development of historical states that took place several thousands of years ago. For this reason, it is plausible that accumulated statehood experience may provide exogenous sources of variation in current income inequality. Furthermore, previous studies examining the persistent effect of state history on economic performance typically treat state history as an exogenous variable (e.g., Bockstette et al., 2002; Chanda & Putterman, 2007; Putterman & Weil, 2010; Hariri, 2012; Ang, 2013b, 2013a; Borcan et al., 2018). An additional issue is that the results may be confounded by measurement bias.

To mitigate the above concerns, I first control for geographic endowments, primarily because these factors are widely regarded as the deep determinants of comparative development across countries. Further, a number of the drivers of income inequality are

[10] To my knowledge, Borcan et al. (2018) and Owen and Vu (2020) are two studies documenting a non-monotonic relationship between state history and economic performance, which is in line with the main hypothesis of this paper. Using subnational data for European countries, Harish and Paik (2019) also find an inverted U-shaped relationship between state antiquity and income levels.

incorporated in the regression in Section 5. This helps address the possibility that my results just reflect the effect of a third omitted factor. This approach has been widely applied in the long-run development literature to reduce omitted variable bias (see, e.g., Acemoglu, 2009, Ch. 4; Vu, 2020). As discussed above, state history is plausibly exogenous with respect to current economic performance and is treated as such in the influential studies cited in the previous paragraph. Nevertheless, I also estimate the model using 2SLS to check the sensitivity of my findings, which further mitigates omitted variables bias. Also, alternative measures of state antiquity and income inequality will be used as a robustness check.

3.2. Variables' descriptions and data

Measuring income inequality

I use the Gini coefficient taken from the Standardized World Income Inequality Database (SWIID) to measure the cross-country variation in income inequality (Solt, 2009) (Figure 1).[11] In particular, I utilize the Gini coefficient of net income as the benchmark dependent variable. This indicator reflects the level of income inequality within a country, taking into consideration the government's efforts to redistribute income. There are two major advantages of using data from the SWIID. *First*, it has a wide coverage of both countries and years (Solt, 2009; Ferreira et al., 2015). This provides a general understanding of the relationship between state history and income inequality across the world. *Second*, the SWIID provides an internationally comparable measure of income inequality across countries, which is particularly relevant for exploring the link between income inequality and economic development (see, e.g., Solt, 2009; Berg et al., 2018).[12] For these reasons, the SWIID index of income inequality has been widely used in recent studies to explore the causes and consequences of income inequality from a cross-country perspective (see, e.g., Jauch & Watzka, 2016; Berg et al., 2018; Lee & Vu, 2019). Other measures of income inequality will be employed in a sensitivity test.

[11] To estimate cross-sectional data, I calculate the mean value of the SWIID Gini coefficients from 1960 to 2018. I also check whether my results are driven by this measurement in Section 5.

[12] Measuring income inequality across the world in a consistent manner provides a valid basis for statistical inference because it helps reduce potential measurement bias. This is challenging because the unit of analysis differs between different surveys that are used to measure income inequality (e.g., individual or household). Further, some surveys utilize information on gross income while others use net income (Berg et al., 2018). These issues are partly mitigated by Solt (2009) based on standardization and multiple imputation. See also Ferreira et al. (2015) for more details on the SWIID.

Measuring state history

To capture cross-country differences in statehood experience, I use an index of state history constructed by Borcan et al. (2018), covering the period from 3500BCE to 2000CE (Figure 2). Accordingly, this measure captures the existence of historical states in each country dating back approximately six millennia when the first statehood was recorded (Borcan et al., 2018). The method of construction of this index is mainly based on Bockstette et al. (2002) by measuring the presence of states above the tribal level, the autonomy of the state, and its territorial coverage across 110 50-year periods from 3500BCE to 2000CE. The measurement can be explained by the following equation:

$$Statehist_{i\rho} = \frac{\sum_{t=0}^{\rho}(1+\delta)^{t-\rho} \times s_{it}}{\sum_{t=0}^{\rho}(1+\delta)^{t-\rho} \times 50}$$

in which t stands for a 50-year period. s_{it} is the state history index for each country in a given period of which the maximum achievable value is 50. In particular, s_{it} of each present-day country is calculated by multiplying scores for three dimensions reflecting the existence of the state, its autonomy, and its territorial coverage and 50 as follows:

$$s_{it} = S_{it}^{presene} \times S_{it}^{autonomy} \times S_{it}^{coverage} \times 50$$

where $S_{it}^{presene}$ equals 1 if there was the government above the tribal level in a given period (and 0.75 if the government could be at best described as a paramount chiefdom, and zero otherwise). $S_{it}^{autonomy}$ takes a value of 1 if the rule was locally based (and zero if it was ruled by a foreign government, and 0.75 if the rule was locally based but with considerable foreign oversight. $S_{it}^{coverage}$ equals 1 if the territorial coverage of the state was greater than 50 percent, (and 0.75, 0.5 and 0.3 if the coverage was 25-30 percent, 10-25 percent, and below 10 percent, respectively).

Next, the sum of s_{it} across 110 periods of 50 years is computed using a commonly presumed discount rate (e.g., $\delta = 1\%$) to consider that more distant periods have less important implications for contemporary economic performance (e.g., Bockstette et al., 2002; Borcan et al., 2018). The final step is to divide these values by their maximum possible value of 50 to obtain an overall index of state history for each country, ranging between zero and one. The benchmark measure of state history is calculated across six millennia, which corresponds to 110 50-year periods ($\rho = 0, \ldots, 109$). Hence, other measures of state history covering different periods in history can be computed by adjusting the value of ρ to check for robustness of the

findings. I also assume alternative discount rates to consider the possibility that the results are driven by this measurement.[13]

Control variables

To minimize omitted variables bias, I incorporate a number of control variables in the regression, including geographic endowments and continent dummies. Specifically, the inclusion of geographic controls is largely motivated by several studies documenting the role of geography in shaping comparative economic performance across countries. For instance, some scholars argue that geographic endowments exert a direct influence on international income differences (Sachs, 2003; Carstensen & Gundlach, 2006), while others posit that geography helps shape the quality of institutions, thus affecting income differences in the world (Easterly & Levine, 2003; Rodrik et al., 2004).

While the relative importance of geography versus institutions in fostering economic development remains largely inconclusive, geographic endowments are widely included as control variables in the long-term comparative development literature (see, e.g., Acemoglu et al., 2001; Knowles & Owen, 2010; Ang, 2013a, 2013b; Borcan et al., 2018; Vu, 2019, 2020). Therefore, I control for a number of geographic variables in the baseline model, including absolute latitude, mean elevation, distance to coast, precipitation, temperature, malaria, and a dummy for being landlocked (see the online appendix for a description of these variables and data sources). Importantly, if geographic factors affect income differences across countries through impacting institutional quality, they can also matter for a country's income distribution. Later, I will also control for a number of factors that have been regarded as key determinants of income inequality in previous studies (Section 5).

A key concern is that the baseline findings may be confounded by unobserved region-specific factors.[14] Countries located in the same continent, for instance, may share common cultural, historical or geographic characteristics, which may arguably affect the distribution of income and the development of historical states. Additionally, statehood experience and contemporary economic outcomes may transcend national borders (Borcan et al., 2018). For this reason, they may be subject to common shocks or productivity spillovers. These issues, if

[13] See also Bockstette et al. (2002) and Borcan et al. (2018) for more details on the construction of the state history index. Alternative measures of state history are employed in robustness checks to minimize potential measurement bias as presented in Section 5.

[14] Figures 1 and 2 demonstrate that there exists considerable variation in income inequality and state history across regions in the world. These regional differences suggest that my findings may be confounded by unobserved region-specific characteristics.

not accounted for, may yield a spurious relationship between state history and income inequality. Thus, I first mitigate this problem by including continent dummies in the benchmark model. Later, I also account for spatial dependence by re-calculating conventional standard errors in Section 5, following Conley (1999).

It is important to emphasize that previous studies in the fundamental development literature typically do not control for proximate determinants of economic performance (see, e.g., Ang, 2013b, 2013a; Owen, 2017; Borcan et al., 2018). The basic intuition is that including these factors in the regression may capture some of the effect of state history on today's economic development. Nevertheless, I will include the proximate determinants of income inequality to check the sensitivity of the baseline findings later in Section 5.

4. Main results

The main proposition of this study posits that the relationship between state history and income inequality exhibits a U-shaped pattern. Figure 3 illustrates a non-monotonic relationship between the historical experience with statehood and income inequality. In particular, both long-standing and newly established states suffer from high levels of income inequality. By contrast, those with an intermediate length of statehood are among the most egalitarian societies. This lends support to the arguments presented in Section 2. Nevertheless, achieving causal interpretations requires controlling for a number of confounding factors. For this reason, I first estimate the benchmark model, and present the core findings in Table 1.

The baseline estimates are broadly consistent with the main hypothesis of this study (Table 1). In column (1) of Table 1, I report the unconditional estimates, which are suggestive of a U-shaped relationship between state history and income inequality. In particular, the estimated coefficient of state history is negative and statistically significant at the 1% level. Furthermore, its squared term has a statistically significant and positive effect on the level of income inequality. These findings reveal that statehood experience at first reduces income inequality but eventually worsens income distribution within an economy. This non-monotonic relationship is largely consistent with recent studies arguing that underdevelopment prevails in countries with either a lack of statehood experience or an excessive duration of state antiquity (Borcan et al., 2018; Harish & Paik, 2019; Owen & Vu, 2020).

Geographic endowments are added to column (2) of Table 1. Accordingly, this inclusion does not alter the baseline findings although the magnitude of the estimated coefficients of state history and its squared term reduces significantly. To rule out the possibility that the results

reflect unobserved region-specific factors, I incorporate continent dummies in the benchmark model (column 3, Table 1). Nevertheless, the non-linear relationship between state history and income inequality remains precisely estimated at the 1% level of significance. Overall, I find that the historical depth of experience with state-level institutions, obtained across six millennia, exerts a U-shaped influence on contemporary income inequality, controlling for geographic variables and continent dummies.[15]

5. Sensitivity analysis

Robustness to endogeneity

As explained earlier, reverse causality is unlikely to be a key issue of identification in this context, and previous studies typically treat statehood experience as exogenous sources of variation in economic performance across countries. The baseline findings can still be biased and inconsistent if the main variables of interest are correlated with the error term. Later, I will address this concern by controlling for a number of confounding factors that may be correlated with both state history and income inequality. However, there may still exist some unobserved factors that may be difficult to identify and measure in a world sample. It is also necessary to consider potential measurement bias. To address these concerns, I employ geographic proximity to the regional frontiers in 1000BCE (*Proximity*) as an instrumental variable for state history. Following Ashraf and Galor (2013) and Wooldridge (2010, pp. 265-268), I create an instrument for state history squared using the squared term of the fitted values of state history obtained in the first-stage regression.[16]

[15] To avoid obtaining a spurious relationship between statehood and income inequality, I conduct Ramsey's test of functional form misspecification under the null hypothesis of correct model specification (Ramsey, 1969). This empirical exercise is implemented by obtaining the fitted values from the estimates reported in each column of Table 1. Next, the square, cube and the fourth power of these predicted values are included in the regression to conduct a test of joint significance. Accordingly, we fail to reject the null hypothesis that the model is correctly specified at conventionally accepted levels of significance (except in column 1, Table 1). This lends support to the validity of the baseline estimates. Furthermore, I check for the normal distribution of the error terms, following Doornik and Hansen (2008). The results reported in Table 1 indicate that we fail to reject the null hypothesis that the residuals are normally distributed. These diagnostic tests is partly suggestive that the baseline models are adequately specified, thus lending support to causal inference. See Owen (2017) for detailed discussions on diagnostic testing for model misspecification in the long-term development literature.

[16] In particular, I first regress state history on *Proximity*, including a set of baseline control variables and continent dummies. Next, the square of the fitted values calculated from this regression is used as an instrumental variable for state history squared. See Wooldridge (2010, pp. 265-268) and Ashraf and Galor (2013) for more details.

Data for *Proximity* are obtained from Ang (2015) that examines the determinants of the emergence and development of historical states.[17] More specifically, regional frontiers for each continent in 1000BCE are identified as areas with the largest urban settlements (Modelski, 2003; Ang, 2015). This is largely based on the proposition that only affluent regions could afford high population density. In this regard, Ang (2015) identifies the regional frontiers based on historical data on urban settlements collected from various sources. Following Ashraf and Galor (2013), he constructs geographic proximity to those leaders using the Haversine formula.[18] Higher values correspond to greater geographic proximity to the regional leaders.

There are good reasons to believe that *Proximity* is a valid instrument for state history. *First*, Ang (2015) examines a world sample of countries, and find that geographic proximity to regional leaders in 1000BCE plays an important role in the formation and development of historical states. The explanation rests on the premise that geographic distance acts as a barrier to knowledge diffusion, trade and economic interactions. In addition, countries located near the regional frontiers could benefit from the dissemination of technologies and state knowledge, which are conducive to the formation and development of historical states (Ang, 2015). This lends support to the argument that *Proximity* is not a weak instrument for state history.

Second, the instrumental variable reasonably exerts no direct influence on today's income inequality. Countries located near the regional leaders in 1000BCE could benefit from the dissemination of state experience at that time. It may follow from this argument that geographic distance also impedes the diffusion of today's state-level institutions, which may affect current income inequality. However, most regional leaders in 1000BCE have ceased to exist as contemporary regional leaders (Ang, 2015).[19] This suggests that the advantage of being proximate to regional leaders might matter for economic development in the distant past but not in the present time. To my knowledge, there is also no empirical evidence supporting the hypothesis that income inequality transcends borders, making it difficult to envisage whether geographic distance matters for a country's income distribution. Therefore, *Proximity* arguably has no direct effect on contemporary economic development across the world. This at least

[17] The author thanks James B Ang for kindly sharing data on this variable.

[18] Proximity is calculated by the following formulae:

$$Proximity = 1 - \left(\frac{Dist_{i,RF}}{Dist_{max}}\right)$$

in which $Dist_{i,RF}$ denotes the geographic distance between country i and the regional leader, and $Dist_{max}$ stands for to the maximum distance in the sample. Higher values correspond to greater geographic proximity to the regional frontiers in 1000BCE. See Ang (2015) for more details.

[19] Details are presented in Ang (2015).

partially suggests that I can isolate plausibly exogenous sources of variation in state history using geographic distance to the regional frontiers in 1000BCE.

Nevertheless, it is important to emphasize that the exogeneity assumption cannot be empirically validated mainly because of the unobserved nature of the disturbance terms. The validity of the instrument, therefore, largely relies on theoretical justification (see, e.g., Owen, 2017). To address this concern, I employ the fractionally resampled Anderson-Rubin test (FAR) developed by Berkowitz et al. (2012) to check for the sensitivity of the IV-2SLS estimates under some minor degrees of deviation from the exogeneity condition. The FAR test is based on the Anderson-Rubin test (AR) that allows obtaining valid inference under weak instruments. More specifically, Berkowitz et al. (2012) modify the AR test based on the jackknife histogram estimator of Wu (1990) to account for the possibility that the instrumental variable may be not perfectly exogenous. This test, therefore, allows obtaining causal inference even when the exogeneity assumption is slightly violated.

The IV-2SLS estimates are reported in Table 2. According to the results in columns (2) and (3), the instrumental variables are found to have a statistically significant effect on state history and its squared term. The values of the F-test of excluded instruments are also larger than the rule-of-thumb value of 10. This justifies the relevance of the instrumental variables used in the IV regression. The second-stage estimates reported in column (1) are broadly consistent with the OLS estimates. This reinforces the baseline findings that statehood experience has a U-shaped relationship with income inequality, controlling for potential endogeneity concerns. The FAR test result indicates that we can reject the null of no statistically significant effect of state history on income inequality at the 5% level even when the instruments slightly deviate from the perfect exogeneity condition.

Robustness to additional controls

The above results demonstrate that the baseline findings are largely robust to using an IV approach, which at least partially accounts for omitted variables bias. To rule out the possibility that my findings just reflect a third unobserved factor, I incorporate a number of additional control variables in the regression.[20]

A key line of inquiry in the long-run development literature reveals that other early development indicators are essential for economic prosperity. The timing of Neolithic

[20] The estimated coefficients of these additional control variables are not reported in Table 4 to conserve space. However, they are available on request.

revolution, for instance, matters for income differences (see, e.g., Galor & Moav, 2007; Putterman & Weil, 2010; Ang, 2013b). Furthermore, Ang (2015) finds that countries adopting sedentary agriculture earlier experienced improved productivity, technologies, fiscal capacity and better institutions. These factors acted as a catalyst for the emergence and development of historical states across the globe. Other studies posit that the length of time elapsed since the first human settlement and genetic diversity help explain comparative development across countries (Ahlerup & Olsson, 2012; Ashraf & Galor, 2013). Thus, one may argue that the baseline estimates just reflect the persistent effect of these early development factors on statehood experience and contemporary economic performance. This is because these factors are comparable to state history when it comes to reflecting a country's early development. If statehood experience, as proposed in this study, matters for today's degrees of income inequality, these factors may exert an influence on income distribution, thus confounding the benchmark findings. Therefore, I allow these variables to enter the baseline regression in a quadratic form, which is in line with my arguments in Section 2.[21] The results in Table 3 indicate that including these factors in the benchmark model does not alter the core results.

Another identification strategy is to control for a series of factors that help explain cross-country differences in income inequality (Table 4).[22] These variables are key drivers of income inequality as established in many studies cited below. Following the long-term development literature, I incorporate legal origins, ethnolinguistic fractionalization, religions, and social trust in the benchmark model from columns (2) to (5) of Table 4 (see, e.g., La Porta et al., 1999; Acemoglu et al., 2001; Alesina et al., 2003; Klerman et al., 2011; Naveed & Wang, 2018). The baseline estimates, however, retain their sign and significance levels when I account for these potential confounding factors. An additional concern holds that the abundance of land may reduce the unequal distribution of land within a country, thus hindering income inequality (Naveed & Wang, 2018). Hence, I employ an index of land suitability for agriculture and the fraction of arable land as control variables (column 6, Table 4).

[21] The timing of Neolithic transition, the duration of human settlement are comparable to state history as measures of a country's early development. For this reason, I control for these variables and their squared terms in the regression. Ashraf and Galor (2013) find that predicted genetic diversity has an inverted U-shaped relationship with the cross-country variation in income per capita. This lends support to the non-linear specification as shown in Table 3. The baseline results also prevail when I exclude the square of these variables. See the online appendix for variables' descriptions and data sources.

[22] See the online appendix for variables' descriptions and data sources.

A number of studies also document the effect of age dependence ratio on income inequality levels (e.g., Lam & Levison, 1992; Deaton & Paxson, 1997; Razin et al., 2002; Furceri & Ostry, 2019). Therefore, I account for this factor in column (7) of Table 4. A final set of control variables is added to the regression, including income per capita, institutional quality, trade openness, financial development, and the quality of human capital (column 8, Table 4).[23] The inclusion of GDP per capita is mainly because economic development may affect income distribution. As discussed earlier, state history is a key determinant of cross-country differences in income levels, institutional quality and financial development. Thus, controlling for these factors helps partial out the effect of state history on economic development as found in previous studies. This helps address a concern that the core results merely reflect the correlation between state history and economic development established in previous papers.

Accordingly, the U-shaped relationship between state history and income inequality withstands controlling for these contemporary determinants of income inequality. Even when I include all additional controls in one regression, which reduces the feasible sample size significantly, the effect of state history and its squared term on income inequality remains very precisely estimated at conventionally accepted levels of significance (column 9, Table 4). Overall, I find that statehood experience exerts a statistically significant influence on today's degrees of income inequality, controlling for a range of drivers of inequality.

Robustness to potential measurement bias

Another challenge of identification stems from potential measurement bias. In particular, the baseline measure of statehood experience, obtained from Borcan et al. (2018), is constructed from 3500BCE, the timing when the first statehood was recorded, to 2000CE. According to Borcan et al. (2018), their extended measure of state history covers the period before the Common Era, which was largely ignored by Bockstette et al. (2002). This helps capture the cross-country variation in accumulated state experience better.

To reduce potential measurement bias, I employ alternative measures of state history, calculated across different periods in history (Panel A, Table 5). In column (1), I use an index of state antiquity, measured by excluding the period from 1500CE to 2000CE. This empirical

[23] The baseline findings remain largely insensitive to controlling for the both GDP per capita and its squared term, following the well-known Kuznets hypothesis of Kuznets (1955). Following Haan and Sturm (2017) and Furceri and Ostry (2019), I also control for the level of financial development, trade globalization, and human capital.

experience is partly motivated by a concern that the results may be confounded by statehood experience obtained during the period of European colonization, which is widely documented as a key determinant of global income differences (Acemoglu et al., 2001). Following Putterman and Weil (2010), I use an ancestry-adjusted measure of state history in column (2) of Table 5. The basic intuition of this robustness test is to account for state experience, institutions, and technologies that are attributable to mass migration starting from the sixteenth century.[24] Overall, I find that the non-linear relationship between state history and income inequality remains largely insensitive to accounting for the effect of colonization and migration.

As discussed earlier, Borcan et al. (2018) assume a 1% discount rate when constructing the state history index. This is mainly motivated by the fact that statehood experience obtained in more distant periods is less relevant for current economic performance. For this reason, the authors give more weight to more recent periods. To check whether the baseline findings are confounded by this measurement, I employ alternative measures of state antiquity, calculated by assuming different discount rates (Panel B, Table 5). In particular, the state history indicators used in columns (3) to (8) of Table 5 is computed by assuming a 0.1% and 2% discount rate, following Borcan et al. (2018). The sign and significance levels of the estimated coefficients of state history and its squared terms appear to be largely robust to this exercise. The results presented Table 5 altogether suggest that the baseline findings remain largely insensitive to using alternative measures of state history, and the persistent effect of migration and colonization on comparative prosperity.

As an additional robustness test of measurement bias, I use alternative measures of income inequality as dependent variables. The results are reported in the Appendix Table A1. More specifically, I employ an index of market income inequality instead of relying on the disposable income measure in the baseline model. I also adopt the GINI index taken from the World Income Inequality Database (WIID).[25] Furthermore, I use the average of the GINI coefficient from 1960 to 2018 in the baseline regression, which is mainly dictated by the availability of data. To check for the possibility that my findings are driven by this empirical approach, I re-calculate the main dependent variable using different periods (Table A1). However, the benchmark findings remain largely insensitive to this empirical exercise.

[24] See Putterman and Weil (2010) for details on the constructions of the ancestry-adjusted state history index.
[25] Data are obtained from the UNU-WIDER (https://www.wider.unu.edu/research/Database/en_GB/database). Jenkins (2015) provides a detailed discussion on the SWIID and WIID measures of income inequality.

Robustness to colonization and (historical) migration flows

As presented in Panel A of Table 5, I attempt to address a concern that my findings may be confounded by the persistent effect of European colonization and mass migration, starting from the sixteenth century (Acemoglu et al., 2001; Acemoglu et al., 2002; Putterman & Weil, 2010).[26] Importantly, the set of control variables used in the benchmark regression includes the fraction of the population at risk of contracting malaria, which is widely regarded as an essential determinant of the type of institutions established by European colonial powers (Acemoglu et al., 2001; Acemoglu et al., 2002). Hence, the disease environment, captured by *malaria* at least in part reduces the effect that my findings are driven by colonization and migration.

In this section, I consider this potential bias more thoroughly by estimating alternative samples as reported in Table 6. The baseline estimates are replicated mainly for ease of comparison. Specifically, I employ the World Migration Matrix, constructed by Putterman and Weil (2010), to restrict the sample size based on the proportion of indigenous people (as of 1500) in the current population (columns 2 to 5, Table 6). In the last two columns of Table 6, I break the sample size into 54 countries that were never colonized and 74 former colonies. The sign and significance levels of the baseline findings remain broadly unchanged when estimating different sub-samples. Therefore, the U-shaped relationship between state history and income inequality is insensitive to accounting for the impact of colonization and (historical) migration flows.

Other robustness checks

To achieve causal interpretations from my findings, I conduct several additional sensitivity tests, which are presented in the online appendix. *First*, I rule out the possibility that the baseline results may be confounded by unobserved region-specific factors. Indeed, this concern is partly addressed by incorporating continent dummies in the benchmark regressions. I perform an additional sensitivity test by re-estimating the baseline model using sub-samples with different regional coverage (the Appendix Table A2). In particular, I sequentially drop countries located in each continent to check whether the results are driven by the inclusion of a particular region. The baseline findings, however, retain their sign and statistical significance when I estimate different sub-samples of countries.

[26] A wealth of literature has indicated that the historical event of European colonization exerts a persistent influence on today's differences in institutional quality and income per capita (Acemoglu et al., 2001; Acemoglu et al., 2002). In addition, there has been mass migration across countries since the sixteenth century, which arguably affects institutions and income levels (Putterman & Weil, 2010).

Second, a crucial challenge of identification, as highlighted by Borcan et al. (2018), is that accumulated statehood experience and contemporary economic performance may transcend borders. The basic explanation for this argument posits that there may exist productivity spillovers and common shocks between countries. The presence of such spatial dependence may confound the conventional OLS estimates. To address this concern, I re-estimate the conventional standard errors that account for spatial dependence across countries, following Conley (1999). This empirical exercise has been recently applied in the long-term comparative development literature to obtain valid statistical inference under the presence of spatial spillovers (see, e.g., Ashraf & Galor, 2013; Borcan et al., 2018; Vu, 2020).[27] The estimation results are reported in the Appendix Table A3, which demonstrates that my findings are largely robust to accounting for potential spatial dependence.

Finally, I thoroughly examine whether the baseline results withstand excluding outliers in the Appendix Table A4. Countries are identified as outliers using different methods, including computing the Cook's distance, the standardized residuals, and robust regression weights.[28] In all cases, the U-shaped relationship between state history and income inequality remains statistically significant at conventionally accepted levels. Therefore, the baseline findings are not driven by potential outliers.

Further evidence on the U-shaped relationship

As presented above, I incorporate the squared term of state antiquity in the standard regression model to test the existence of a U-shaped relationship. The core results lend empirical support to the central hypothesis discussed in Section 2. The findings also withstand a battery of sensitivity checks. To draw causal interpretations, I further conduct an additional statistical test and check for the possibility of a higher-order non-linear relationship.

First, I perform the U-shaped test of Lind and Mehlum (2010) that builds upon an early study by Sasabuchi (1980). The basic idea of the U-shaped test is to check whether the hypothesized relationship is decreasing at low values of the state history index and increasing within high values (Lind & Mehlum, 2010). To conduct this empirical exercise, I employ the baseline estimates reported in Table 1. The results are presented at the end of the Appendix

[27] The intuition of this method is to calculate the weighted covariance matrices, in which the weights correspond to the inverse of the distance between countries. Furthermore, the weights equal zero after a specified threshold. Following Borcan et al. (2018), I specify a threshold of twenty coordinate degrees. The results are also largely insensitive to using another threshold of fifty coordinate degrees as shown in Table A3. See also Ashraf & Galor (2013) for discussions on this method.

[28] I present details in Table A4 of the online appendix.

Table A3. Accordingly, we can reject the null hypothesis of that state history and income inequality exhibit a monotonic or inverted U-shaped relationship at conventionally accepted levels of significance. This lends credence to the baseline findings. *Second*, I replicate the baseline estimates by including a cubic term of state history in the regression model to check for the presence of a higher-order relationship in the Appendix Table A5. However, the estimated coefficient of state history cubed is statistically insignificant, which reveals that we can reject the existence of a higher-order correlation. Overall, the results suggest that the U-shaped relationship between state history and income inequality is robust.

6. Conclusion

The formation and development of historical states have been documented as a key determinant of cross-country differences in GDP per capita. Other studies also postulate that accumulated statehood experience helps improve institutional quality and the level of financial development. This paper extends this literature by documenting a robust relationship between state antiquity and contemporary income inequality levels, estimating data for up to 153 countries. I find that the state history and income inequality exhibit a U-shaped pattern. In particular, statehood experience at first helps reduce income inequality but eventually increase it. This suggests that societies endowed with an intermediate length of state history enjoy the most egalitarian distribution of income. My findings are largely insensitive to performing a battery of robustness checks. To my knowledge, this is the first study documenting a robust U-shaped relationship between state antiquity and today's income inequality.

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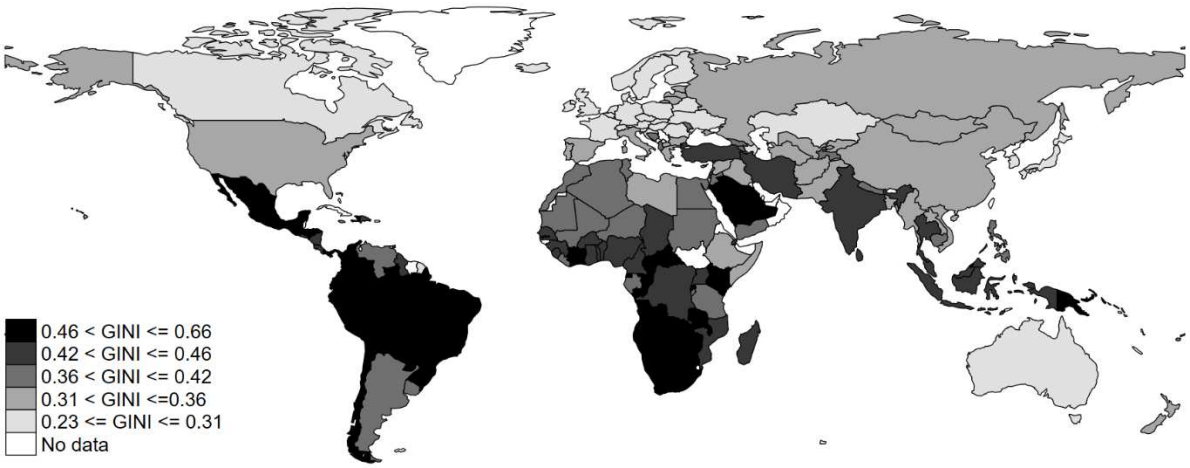

Figure 1. Cross-country differences in income inequality

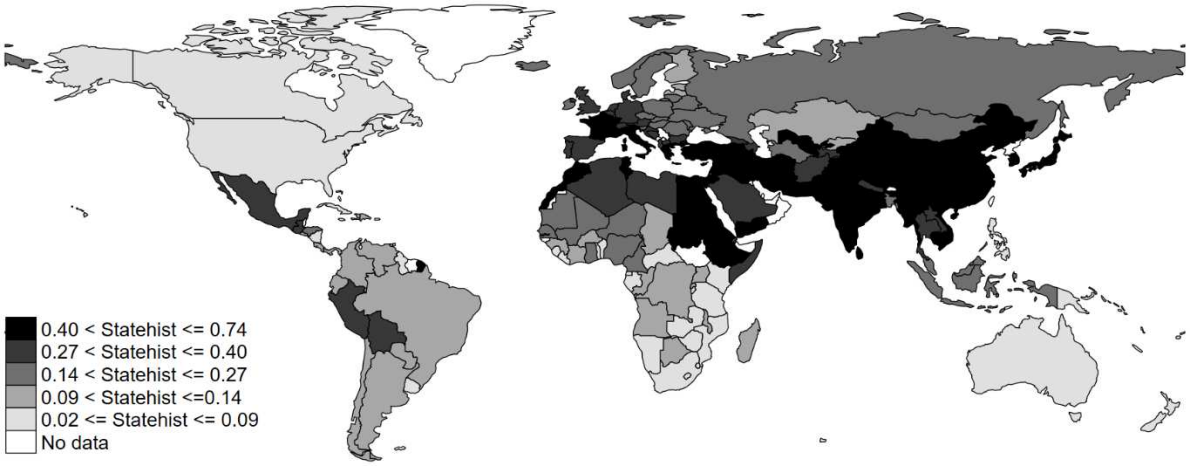

Figure 2. Cross-country differences in state history (3500BCE – 2000CE)

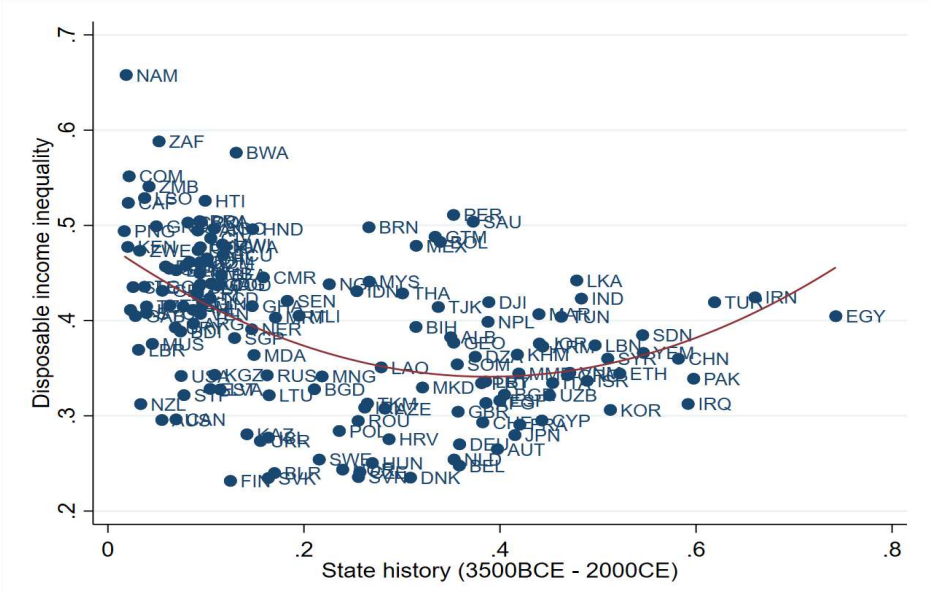

Figure 3. The U-shaped relationship between state history and income inequality

Table 1. Main results

	Unconditional estimates	Adding geographic controls	Adding continent dummies (Full specification)
	(1)	(2)	(3)
Dependent variable is disposable income inequality			
State history	**-0.711*** **	**-0.409*** **	**-0.445*** **
	[0.122]	[0.103]	[0.119]
State history squared	**0.915*** **	**0.348** **	**0.421*** **
	[0.188]	[0.142]	[0.153]
Absolute latitude		-0.107	-0.043
		[0.090]	[0.091]
Mean elevation		0.068***	0.072***
		[0.012]	[0.012]
Landlocked		-0.027*	-0.029*
		[0.014]	[0.015]
Distance to coast		0.000	0.000
		[0.000]	[0.000]
Precipitation		-0.001	0.000
		[0.001]	[0.001]
Temperature		0.006***	0.007***
		[0.002]	[0.002]
Malaria		-0.043**	-0.060***
		[0.017]	[0.020]
Europe dummies			-0.022
			[0.023]
America dummies			-0.023
			[0.020]
Asia dummies			-0.031*
			[0.018]
Oceania dummies			-0.106**
			[0.045]
Observations	153	128	128
R-squared	0.240	0.722	0.753
RESET [p-value]	0.070	0.769	0.934
Normality [p-value]	0.895	0.005	0.221

Notes: Robust standard errors in parentheses. *RESET* is the test for functional form misspecification under the null hypothesis that the model is correctly specified. *Normality* denotes the test for normality of the error terms, and the failure to reject the null hypothesis the disturbance terms follow the normal distribution. *** p<0.01, ** p<0.05, * p<0.1.

Table 2. IV-2SLS estimates

	Second-stage estimates	**First-stage estimates**	
	(1)	(2)	(3)
Dependent variables	Disposable income inequality	State history	State history squared
State history	**-1.190***** [0.426]		
State history squared	**1.331**** [0.585]		
Proximity		**0.209**** [0.097]	0.073 [0.067]
Fitted state history squared		0.358 [0.478]	**0.655**** [0.328]
Baseline controls	✓	✓	✓
Continent dummies	✓	✓	✓
First-stage F-statistic of excluded instruments [p-value]	n/a n/a	11.74 [0.001]	12.12 [0.001]
FAR [p-value]	0.031		
Observations	128	128	128
R-squared	0.635	0.693	0.643

Notes: Baseline controls are geographic control variables included in Table 1. Continent dummies stand for dummy variables for each continent as presented in Table 1. ✓ denotes the inclusion of control variables. FAR stands for the fractionally resampled Anderson-Rubin test. The null hypothesis of the FAR test is that state history has no statistically significant effect on income inequality, allowing for some slight violations of the orthogonality condition. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Robustness to controlling for historical confounders

	Baseline	Adding other measures of early development			
	(1)	(2)	(3)	(4)	(5)
Dependent variable is disposable income inequality					
State history	**-0.445*** **	**-0.367*** **	**-0.446*** **	**-0.469*** **	**-0.384*** **
	[0.119]	[0.113]	[0.120]	[0.126]	[0.116]
State history squared	**0.421*** **	**0.381** **	**0.414*** **	**0.464*** **	**0.404*** **
	[0.153]	[0.145]	[0.150]	[0.164]	[0.147]
Neolithic transition		-0.026*			-0.032**
		[0.013]			[0.015]
Neolithic transition squared		0.002*			0.002**
		[0.001]			[0.001]
Human settlement duration			0.042		0.067
			[0.056]		[0.059]
Human settlement duration squared			-0.023		-0.045
			[0.042]		[0.042]
Predicted genetic diversity				-0.868	-3.662
				[3.074]	[3.488]
Predicted genetic diversity squared				0.416	2.388
				[2.285]	[2.595]
Baseline controls	✓	✓	✓	✓	✓
Continent dummies	✓	✓	✓	✓	✓
Observations	128	127	128	128	127
R-squared	0.753	0.765	0.754	0.756	0.776

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Tables 1 and 2.

Table 4. Robustness to controlling for other drivers of income inequality

	Baseline estimates	Including additional control variables							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable is disposable income inequality								
State history	**-0.445***** [0.119]	**-0.333***** [0.115]	**-0.436***** [0.114]	**-0.394***** [0.110]	**-0.394***** [0.137]	**-0.457***** [0.120]	**-0.445***** [0.120]	**-0.405***** [0.148]	**-0.379**** [0.148]
State history squared	**0.421***** [0.153]	**0.323**** [0.154]	**0.417***** [0.151]	**0.379**** [0.150]	**0.358**** [0.177]	**0.434***** [0.154]	**0.421***** [0.154]	**0.469**** [0.189]	**0.558***** [0.198]
Common law		✓							✓
Mixed law		✓							✓
Fractionalization			✓						✓
Catholics				✓					✓
Muslims				✓					✓
Protestants				✓					✓
Social trust					✓				✓
Land suitability						✓			✓
Arable land						✓			✓
Age dependence ratio							✓		✓
GDP per capita (log)								✓	✓
Institutional quality								✓	✓
Trade openness (log)								✓	✓
Financial development								✓	✓
Human capital index								✓	✓
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Continent dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	128	126	127	127	79	127	128	109	63
R-squared	0.753	0.770	0.758	0.768	0.769	0.752	0.753	0.787	0.889

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Tables 1 and 2.

Table 5. Robustness to using different measures of state history

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable is disposable income inequality							
Panel A. Using alternative statehood periods								
State history in 1500CE	**-0.345***** [0.088]							
State history in 1500CE squared	**0.339***** [0.121]							
Ancestry-adjusted state history		**-0.354***** [0.091]						
Ancestry-adjusted state history squared		**0.337**** [0.130]						
Panel B. Using alternative discount rates								
State history (0.1% discount)			**-0.479***** [0.118]					
State history squared (0.1% discount)			**0.504***** [0.153]					
State history (2% discount)				**-0.391***** [0.127]				
State history squared (2% discount)				**0.318**** [0.157]				
State history in 1500CE (0.1% discount)					**-0.387***** [0.098]			
State history in 1500CE squared (0.1% discount)					**0.418***** [0.130]			
State history in 1500CE (2% discount)						**-0.296***** [0.084]		
State history in 1500CE squared (2% discount)						**0.250**** [0.120]		
Ancestry-adjusted state history (0.1% discount)							**-0.391***** [0.102]	
Ancestry-adjusted state history squared (0.1% discount)							**0.413***** [0.139]	
Ancestry-adjusted state history (2% discount)								**-0.306****

Ancestry-Adjusted state history squared (2% discount)								[0.087] **0.247*** [0.129]
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓
Continent dummies	✓	✓	✓	✓	✓	✓	✓	✓
Observations	128	128	128	128	128	128	128	128
R-squared	0.745	0.746	0.750	0.752	0.742	0.747	0.741	0.749

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Tables 1 and 2.

Table 6. Robustness to colonization and (historical) migration flows

Restricted samples	Baseline	The fraction of indigenous people (as of 1500) in current population				Never colonies	Former colonies
		70%	80%	90%	95%		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
State history	**-0.445*** **	**-0.407*** **	**-0.419*** **	**-0.439*** **	**-0.511*** **	**-0.543** **	**-0.418*** **
	[0.119]	[0.134]	[0.132]	[0.151]	[0.168]	[0.235]	[0.153]
State history squared	**0.421*** **	**0.365** **	**0.405** **	**0.377***	**0.510** **	**0.573***	**0.374** **
	[0.153]	[0.178]	[0.182]	[0.199]	[0.243]	[0.334]	[0.180]
Baseline controls	✓	✓	✓	✓	✓	✓	✓
Continent dummies	✓	✓	✓	✓	✓	✓	✓
Observations	128	91	84	68	52	54	74
R-squared	0.753	0.766	0.781	0.810	0.826	0.716	0.568

Notes: From columns (2) to (5), I restrict the sample size to countries in which the proportion of indigenous people in 1500CE in current population is greater than 70%, 80%, 90% and 95%. I also break the sample into former colonies and countries that were colonized in columns (6) and (7), respectively. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See also notes to Tables 1 and 2.