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State history and political instability: The disadvantage of early state development

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State history and political instability: The disadvantage of early state development*

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Abstract

This paper establishes that long-term exposure to statehood is detrimental to building politically stable regimes outside Europe. It argues that accumulated statehood experience impeded the diffusion of European institutions and was conductive to the early emergence of powerful elites, leading to contemporary institutional stagnation. This undermines the provision of public goods and lowers the opportunity cost of engaging in riots, arguably giving rise to socio-political unrest. Using data for 109 non-European societies, the study documents evidence that a long history of statehood is linked to the persistence of political instability. The main findings withstand numerous robustness analyses.

Key words: state history, early development, political instability, institutions.

JEL Classification: D74, N00, O43, P16.

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1. Introduction

The pervasiveness of social and political unrest remains an enduring feature of many societies across the globe, and appears to be one of the most serious barriers to achieving sustainable economic development. A direct negative consequence of riots and violent conflicts is substantial loss of human life. More specifically, over 16 million deaths worldwide have been attributed to civil conflicts since the end of the Second World War (Arbatlı *et al.*, 2020). Additionally, the persistence and pervasiveness of socio-political unrest may impose considerable economic costs through inducing political instability or uncertainty.[1] It is widely acknowledged that politically unstable economies find it difficult to maintain social order, and tend to adopt suboptimal economic policies (Carmignani, 2003; Azzimonti, 2011; Aisen & Veiga, 2013). Previous studies document that political instability transmits to underdevelopment through various channels, including, but not limited to, lower growth rates (Alesina & Perotti, 1996; Jong-A-Pin, 2009; Aisen & Veiga, 2013), reduced firms' investment (Julio & Yook, 2012), lower levels of human capital accumulation (Azzimonti, 2011), environmental deterioration (Fredriksson & Svensson, 2003) and an unequal distribution of income (Dutt & Mitra, 2008).

Against this background, the main objective of this paper is to uncover one of the deepest origins of political instability. To this end, the study exploits international variation in long-term exposure to statehood to explain the persistence and pervasiveness of social and political unrest across the world. Considering this inquiry, I find inspiration in numerous influential contributions to the comparative development literature highlighting the role of accumulated statehood experience in shaping contemporary economic performance (see, for example, Bockstette *et al.*, 2002; Chanda & Putterman, 2007; Putterman & Weil, 2010; Spolaore & Wacziarg, 2013; Borcan *et al.*, 2018).[2] The conventional wisdom of this growing body of research postulates that the early emergence of state-like polities conferred a present-day society with strengthened fiscal and organizational capabilities, leading to higher levels of income per capita (Bockstette *et al.*, 2002). More specifically, the idea that history casts a long shadow on today's economic prosperity builds upon the seminal article by Bockstette *et al.* (2002) that constructs a novel index of state history. These authors attempt to measure cross-country differences in accumulated statehood experience

[1] As defined by the World Bank, political instability refers to the probability that a government can be overthrown or destabilized by unconstitutional methods and/or politically motivated violence (see Section 3 for details).

[2] Spolaore and Wacziarg (2013) provide a comprehensive review of studies investigating the deep historical roots of comparative cross-country economic performance.

based on three aspects of state formation and development, including the early emergence of a state above the tribal level, the territorial coverage of a state and the autonomy of a government.

Subsequent research reveals that state history is conducive to establishing well-functioning financial markets (Ang, 2013a) and inclusive institutions (Ang, 2013b). These findings are consistent with previous studies documenting the beneficial role of early state development (Bockstette *et al.*, 2002; Chanda & Putterman, 2007; Putterman & Weil, 2010). Nevertheless, some scholars provide suggestive evidence that excessive statehood experience is an impediment to achieving higher levels of productivity and GDP per capita (Lagerlöf, 2016; Borcan *et al.*, 2018; Harish & Paik, 2020), building democratic institutions (Hariri, 2012) and creating an egalitarian society (Vu, 2021).[3] Taken altogether, the existing literature offers highly mixed findings when it comes to investigating the economic impacts of state history, making it difficult to reconcile the long-run legacy of early state development. Importantly, earlier contributions to this strand of literature remain largely silent about how state history helps shape international differences in political instability. It is surprising how little attention has been paid to this inquiry given that socio-political unrest is a widespread social concern in many societies across the world.

A key contribution of the study lies in the exploration of the extent to which statehood experience, accumulated over a period of six millennia, matters for a country's ability to establish politically stable regimes in present times. Unfortunately, examining the relationship between state history and political instability faces several challenges arguably due to contradictory evidence provided by existing studies in the comparative development literature.

Therefore, I begin the empirical analysis by analysing a conditional correlation between accumulated statehood experience and the degree of uncertainty associated with political systems. The study exploits two separate world samples of countries, including European and non-European economies. Several interesting patterns are depicted in Figure 1.[4] In particular, there exists a

[3] It is worth mentioning a recent empirical analysis by Olsson and Paik (2020) demonstrating that an early transition to sedentary agriculture has persistent and negative impacts on present-day economic development, measured by income per capita. This piece of work is closely related to the literature exploring the long-term legacy of statehood experience because the timing of Neolithic revolution is a key determinant of the formation and development of historical states (Ang, 2015). The findings of Olsson and Paik (2020) are also in sharp contrast to most previous studies arguing that early development exerts a positive influence on today's economic performance.

[4] The results illustrated in Figure 1 are based on regressing the World Bank's index of political instability on the state history index of Borcan *et al.* (2018). A set of country-level geographic attributes is incorporated in the regression. The results presented in Panel A of Figure 1 also account for unobserved heterogeneity across regions. More details are provided Sections 3 and 4.

positive association between state history and political instability across non-European societies, partialling out the effects of numerous potentially confounding factors (Panel A, Figure 1). Many old civilizations outside the European continent (e.g., Egypt, Iran, Turkey, Iraq, Pakistan, Yemen and Sudan) experience high degrees of political instability. By contrast, the level of political uncertainty is much lower in many newly established states outside Europe (e.g., New Zealand, Costa Rica, Benin and Gabon). These stylized patterns are suggestive of a positive effect of statehood experience on political instability across non-European countries. However, state history is negatively correlated with political uncertainty within Europe, which reveals that socio-political unrest is less likely to proliferate in long-standing European states (Panel B, Figure 1).

It comes as little surprise that state experience is linked to less political uncertainty across European nations. Many scholars argue that the process of state building within the European world exerts a positive influence on present-day economic outcomes. The underlying idea is straightforward: the state-building process in Europe led to the emergence of modern democratic institutions, which play an important role in fostering economic prosperity. Accordingly, the history of state building in Europe reveals that rulers were forced to make political concessions in order to secure economic resources, held by asset-owning citizens (Tilly, 1975; Bates, 1991; Finer, 1997; Hariri, 2012). An unintended result of such concessions is the onset of representative assemblies (Tilly, 1975; Hariri, 2012). In this context, accumulated state experience improved productivity over time by reinforcing institutional innovations. As reviewed by Borcan *et al.* (2018), the onset of innovative institutions in European centralized states resulted in dramatic increases in output per capita by fostering private property rights and the accountability of political institutions. This provides a modern society with the ability to maintain social order, enforce rules and regulations, and allocate vital scare resources efficiently, leading to less political instability. By contrast, the absence of democratic institutions in non-European powerful states, characterized by strong extractive capacity due to early state development, contributed to economic and institutional stagnation (Lagerlöf, 2016; Borcan *et al.*, 2018). Therefore, older and more autonomous states outside Europe are characterized by the prevalence of socio-political unrest (Panel A, Figure 1). This stylized fact stands in stark contrast to many influential studies in the long-term comparative development literature, which document a positive effect of state history on present-day economic outcomes (Bockstette *et al.*, 2002; Chanda & Putterman, 2007; Putterman & Weil, 2010; Ang, 2013a).

Partially motivated by these above stylized patterns, this study attempts to explore the relationship between state history and political instability outside the European world. The central hypothesis is that accumulated statehood experience is associated with poor-quality institutions across non-European societies. The underlying reason builds upon existing studies, which document that state history impeded the diffusion of European institutions, starting around the sixteenth century (Hariri, 2012; Ertan *et al.*, 2016). Additionally, a long history of statehood is linked to the early emergence of powerful elites and entrenched groups within an economy, resulting in institutional stagnation (Lagerlöf, 2016; Borcan *et al.*, 2018; Vu, 2021). This may translate into persistent and high levels of political uncertainty through undermining the provision of public goods and lowering the opportunity cost of engaging in riots and revolts. Exploiting data for up to 109 non-European countries, the current study provides strong and robust evidence of a positive association between state history and political instability outside Europe. Moreover, it finds that non-European societies endowed with a long history of statehood are likely to experience the occurrence of riots and revolts. A mediation analysis reveals that the political legacy of early state development is partially mediated through income per capita, institutional quality and redistribution. In contrast to most previous studies, this paper documents evidence of the negative consequences of state history using a sample of non-European economies. Therefore, the results help reconcile highly inconclusive findings offered by the existing literature.[5]

Furthermore, the current study belongs to an emerging body of research examining the "proximate" determinants of political instability. Conventional causes of socio-political unrest include, among others, income inequality (Alesina & Perotti, 1996), low levels of income per capita, widespread poverty, poor governance (Feng, 1997; Fearon & Laitin, 2003; Collier & Hoeffler, 2004; Blattman & Miguel, 2010), resource wealth (Dutt & Mitra, 2008), demographic characteristics (Goldstone, 2002; Krieger & Meierrieks, 2011; Acemoglu *et al.*, 2020) and trade openness (Martin *et al.*, 2008). Nevertheless, the exploitation of these economic and demographic factors in the investigation of the deep origins of political instability is unsatisfactory from both

[5] An additional motivation for focusing on non-European economies is dictated by the availability of data. This paper, therefore, leaves it open for future research to examine the proposed negative relationship between state history and political instability within the European continent (Panel B, Figure 1). It is noteworthy that the results illustrated in Panel B of Figure 1 do not imply causal inference because they are based on using a limited sample of 34 European countries. In this regard, it is difficult to account for alternative explanations when the inclusion of numerous confounding factors in the regression significantly reduces the feasible number of degrees of freedom. For this reason, this paper exploits a sample of non-European societies with greater data availability.

empirical and theoretical perspectives. Empirically, a major concern of credibly causal inference stems from reverse causality. This problem arises because political instability has a direct influence on its "proximate" determinants.[6] By adopting a historical approach, in which I explore the contribution of accumulated statehood experience, obtained over six millennia, to explaining international differences in political instability, this paper largely circumvents the issue of reverse causation. Because current levels of political uncertainty reasonably exert no direct influence on the formation and development of historical states, as predetermined thousands of years ago, potential endogeneity bias induced by the presence of reverse feedback is broadly ruled out.

It is important to re-emphasize that political instability appears to be a persistent feature of many countries across the world. Figure 2 depicts the evolution of the World Bank's index of political instability within selected countries and regions from 1996 to 2015. It reveals that the prevalence of socio-political unrest exhibits high degrees of persistence over time within an economy (Panel A, Figure 2). A simple average of political instability constructed for each selected region is suggestive of the same time-series pattern (Panel B, Figure 2). It appears that socio-political unrest may be hard to change once it is present within a society. Therefore, curtailing this widespread social issue arguably requires a profound understanding of its fundamental causes (Spolaore & Wacziarg, 2013; Nunn, 2020; Vu, 2021). Given the persistence and pervasiveness of political instability, the deep origins of riots and revolts may stem from country-level fundamental (fixed) characteristics, such as slowly evolving geographic, cultural or historical factors. If political instability is fundamentally driven by the formation and development of historical states, establishing politically stable economies arguably requires attention to the long shadow of histories (Spolaore & Wacziarg, 2013; Nunn, 2020). Unfortunately, few studies exist on the deep-rooted determinants of political instability.[7] Therefore, this paper sheds light on the vast literature on the

[6] For example, an unequal distribution of income can provoke dissatisfaction with the government, possibly leading to greater political uncertainty. However, effective leaders of politically unstable economies are likely to adopt suboptimal policies arguably because their window of opportunity is short and uncertain. This potentially exacerbates within-country income inequality. It is also widely established that political uncertainty exerts a direct influence on productivity or income levels, poverty, institutional quality, population growth and trade openness.

[7] Empirical evidence on the deep determinants of political instability is hard to find. An exception is a recent study by Grechyna (2018) who highlights the role of geographic characteristics in shaping the prevalence of socio-political unrest across countries. Moreover, Depetris-Chauvin (2016) examines the relationship between state history and civil conflicts across Sub-Saharan African economies. However, his analysis focuses on a specific region, making it difficult to obtain a broad understanding of the long-term political legacy of early state development across the world. Importantly, his dependent variable is the onset of civil conflicts, while the main variable of interest of this paper is the World Bank's index of political uncertainty, which reflects the probability of a government collapse. It is

causes of political instability through exploiting variation in statehood experience to trace the deep historical roots of the worldwide distribution of socio-political unrest.

Besides the main findings previewed above, this paper finds significant heterogeneity in the effects of state history on political instability, using three separate dimension of state formation and development. Consistent with the main results, the length of time elapsed since the first statehood was recorded is an impediment to establishing politically stable systems outside Europe. By contrast, the autonomy of a government and the territorial unity help lower uncertainty associated with the political environment. These distinct patterns obtained from decomposing the overall state history index have been largely ignored in the long-term comparative development.

The remainder of the study proceeds as follows. Section 2 explains why non-European countries endowed with greater statehood experience tend to suffer from political instability. Sections 3 and 4 contain detailed descriptions of data and three key methods of identification. The main findings are presented in Section 5, followed by robustness analyses in Section 6. Further evidence is discussed in Section 7, and Section 8 concludes.

2. Statehood experience and the persistence of political instability

This study proposes that non-European countries endowed with a longer history of statehood are more likely to establish politically unstable regimes, compared with newly established states. This argument builds upon an emerging body of research documenting two potential conditions that are conducive to the prevalence of social and political unrest across the world.

A key driver of political instability is the opportunity cost of engaging in riots and revolts, which can be captured by income or productivity levels (Collier & Sambanis, 2002; Collier & Hoeffler, 2004; Blattman & Miguel, 2010; Ang & Gupta, 2018). Therefore, higher income corresponds to a greater opportunity cost of participating in socio-political unrest compared with that of peaceful cooperation or negotiation, which arguably reduces the pervasiveness of political uncertainty. This suggests that low-income countries tend to suffer from greater political instability

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noteworthy that there exist numerous studies on the causes and consequences of political instability, besides ones focusing on the incidence of violent conflicts. Additionally, there are several attempts at estimating the effects of ethnolinguistic fractionalization or polarization on political unrest (see, e.g., Fearon & Laitin, 2003). Nevertheless, the degree of population fragmentation is likely to be interrelated with and jointly determined by voluntary or involuntary cross-border movements of migrants, partly driven by the prevalence of riots and revolts. Hence, these studies suffer from the same theoretical and empirical shortcomings as ones exploring the conventional “proximate” determinants of political instability.

at least partially because people living in the developing world typically experience a lower opportunity cost of participating in violent conflicts. Moreover, widespread poverty and hunger, an enduring feature of many developing economies, can act as a catalyst for the onset of violent conflicts over scare vital resources, thus shaping international differences in political instability (Blattman & Miguel, 2010). It is widely acknowledged that the onset of social and political unrest depends on the provision of public goods and redistributive policies within an economy (Arbatlı *et al.*, 2020). More specifically, an unequal distribution of income (and/or power) and the under-provision of public goods may provoke greater dissatisfaction with the government, possibly resulting in higher levels of political uncertainty. However, the quality of institutions is a major determinant of the provision of public goods and redistributive policies that help lower political instability. Therefore, I postulate that state history exerts persistent and positive effects on contemporary politically instability outside the European world through shaping reduced productivity and poor-quality institutions, as follows.

First, the central hypothesis of this paper builds upon the existing literature suggesting that state history is detrimental to building inclusive institutions across non-European societies, which hinder the provision of public goods and income (re)distribution. As such, long-term exposure to statehood helps explain the persistence and pervasiveness of political instability outside the European world. One of the most influential theories in the long-term comparative development literature posits that the historical event of European colonization, starting around the sixteenth century, lies at the deep roots of cross-country differences in institutional quality (see, for example, Acemoglu *et al.*, 2001; Spolaore & Wacziarg, 2013). It follows from this widely accepted hypothesis that European colonizers established different types of institutions outside Europe depending on the disease environment of former colonies. This provides an explanation for significant inequalities in economic prosperity across non-European nations.[8]

Recent contributions to this line of inquiry reveal that precolonial statehood experience plays a critical role in mediating the historical diffusion of European institutions (Hariri, 2012; Ertan *et*

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[8] Acemoglu *et al.* (2001) document that places where Europeans could settle permanently are relatively wealthier because of well-functioning (inclusive) institutions established by colonial powers. By contrast, Europeans set up extractive institutions in countries of which the disease environment prevented the long-term settlement of colonizers. The historically established institutions persist until today, thus determining the pattern of economic development among European former colonies. Spolaore and Wacziarg (2013) provide a comprehensive survey of related studies examining the deep historical roots of comparative cross-country economic development.

al., 2016). The basic premise is that the early presence of precolonial state institutions of indigenous population constrained the conquest, settlement and institutional transplantation of European colonizers. Statehood experience, in particular, conferred a country with an improved capacity to consolidate power, which arguably acted as a barrier to a conquest by European powers (Ferro, 1997). In precolonial eras, long-standing states were able to mount an organized military defence, and developed strong fiscal and administrative capabilities, which increased their ability to resist European colonization (Ferro, 1997; Hariri, 2012). Consistent with these arguments, Ertan *et al.* (2016) find evidence that long-term exposure to statehood reduced the probability of having been colonized. Additionally, former colonies endowed with a long history of statehood were subject to a significantly shorter duration of colonization (Ertan *et al.*, 2016).

Furthermore, Hariri (2012) establishes that Europeans adopted different strategies of colonization depending on the historical depth of experience with state-like polities. Specifically, former colonies characterized by early state development tend to have been ruled though the existing legal framework and state infrastructure. In places where indigenous state-like institutions and infrastructure were already developed for a long time, European colonizers adopted an indirect colonial rule that incorporated existing legal-administrative institutions into an overall colonial domination (Gerring *et al.*, 2011; Hariri, 2012). Indeed, several influential studies in the long-run development literature highlight that an indirect form of colonial rule left former colonies with poor governance, ineffective administrations and under-provision of public goods, leading to postcolonial underdevelopment (Lange, 2004). However, a direct structure of colonial rule was adopted in places lacking long-term exposure to state-level polities (Gerring *et al.*, 2011; Hariri, 2012). For this reason, statehood experience was an impediment to the diffusion of European ideas and institutions. Employing cross-sectional data for non-European countries, Hariri (2012) finds that state history exerts a negative influence on the quality of democratic institutions.

More recently, Borcan *et al.* (2018), Harish and Paik (2020) and Vu (2021) find that an excessive duration of statehood is associated with poor-quality institutions, reduced productivity and an unequal distribution of income within an economy. This is mainly attributed to the early emergence of powerful elites and entrenched groups with superior economic and political power in very long-standing states, leading to an over-centralization of power (Borcan *et al.*, 2018; Harish & Paik, 2020; Vu, 2021). The existing literature on the link between state development and economic performance reveals that the first state-like polity was established to resolve collective action issues in societies (Olson, 1982, 1986, 1993). In particular, a state above the tribal level emerged when "roving bandits" were replaced by "stationary bandits" sustained by taxation rather than by plundering (Olson, 1993). However, a long history of historical state development may eventually lead to the emergence of powerful elites and entrenched groups within a society (Olson, 1982, 1986). The rise of these groups may translate into persistent institutional stagnation because powerful elites characterized by their economic and political power tend to maximize their privileges (private gain) at a cost borne by the rest of the population. Powerful elites are likely to establish oppressive regimes to reduce possible expropriation of their privileges, and engage in rent-seeking activities (Bentzen *et al.*, 2017; Borcan *et al.*, 2018; Vu, 2021). This also hinders progressive (re)distribution of income because entrenched groups within a country tend to expropriate tax revenue instead of providing it in the form of public goods and services (Borcan *et al.*, 2018; Vu, 2021).

These narratives explain why very long-standing states typically suffer from institutional stagnation and an unequal distribution of income. It is important to re-emphasize that institutional quality and income (re)distribution play an important role in shaping the evolution of political uncertainty. Well-functioning institutions contribute to establishing politically stable regimes through enhancing the provision of public goods, redistributive policies and income levels (Blattman & Miguel, 2010). By contrast, poor governance and higher degrees of income inequality may provoke riots and revolts via inducing dissatisfaction with the government (Alesina & Perotti, 1996). Therefore, accumulated statehood experience may translate into persistent political instability through shaping institutional stagnation outside the European continent.

Second, I propose that an early start outside Europe is linked to greater uncertainty associated with political regimes through lowering the opportunity cost of engaging in riots and revolts. This argument rests upon numerous contributions to the comparative development literature demonstrating that a very long history of statehood is associated with reduced productivity.

Lagerlöf (2016), in particular, develops a theoretical model explaining the global divergence in growth trajectories based on variation in long-term exposure to statehood. It argues that preindustrial state development became an impediment to the rise of democratic institutions and innovation, which are the main drivers of long-term growth. The underlying idea is that very long-standing states obtained large extractive capacity, thus becoming resistant to transiting to democratic statehood. By contrast, rulers of newly established states accumulating less extractive capacity were more inclined to adopt new forms of democratic statehood. Importantly, a transition to democratic institutions was associated with an improved capacity to provide growth-enhancing public goods, which would eventually translate into sustained innovation-led economic growth. Consequently, older and more autonomous states were overcome by younger states, characterized by less extractive capacity and more inclusive institutions. The theoretical model advanced by Lagerlöf (2016) is suggestive of a negative association between early state development and income per capita. More recently, an empirical analysis by Olsson and Paik (2020) indicates that an early start is linked to reduced productivity and low levels of income per capita because old civilizations established autocratic and hierarchical societies.[9] Hence, it is argued that excessive statehood experience is associated with poor-quality institutions, which hinder national innovative capacity and long-run growth (see, e.g., Lagerlöf, 2016; Borcan *et al.*, 2018; Harish & Paik, 2020; Olsson & Paik, 2020; Vu, 2021). For this reason, people living in very long-standing states outside Europe may exhibit a lower opportunity cost of engaging in riots and revolts, leading to greater political instability.

Overall, I propose that long-term exposure to state-level institutions is detrimental to establishing politically stable regimes outside the European continent. Figure 3 illustrates the main hypothesis of the current study.

3. Data and model specification

3.1. Empirical framework

The exploration of the contribution of accumulated statehood experience to explaining the persistence and pervasiveness of socio-political unrest outside Europe is mainly based on regressing a measure of political instability on the extended state history index. Thus, the paper estimates cross-sectional models, following the empirical framework of Bockstette *et al.* (2002) and Borcan *et al.* (2018). The baseline model specification can be expressed below:

$$PIS_i = \alpha + \beta Statehiste_i + \gamma Geo\_Controls_i + \varphi Region\_FE_i + \varepsilon_i \qquad [1]$$

in which PIS is the main dependent variable, capturing cross-country differences in the level of political instability. This indicator is constructed using the World Bank's index of Political

[9] Olsson and Paik (2020) argue that the length of time elapsed since the Neolithic revolution is associated with underdevelopment, measured by GDP per capita. These findings also build upon the intuition that an early start was an impediment to the (historical) emergence of inclusive political institutions, which persist until today and shape comparative cross-country development.

Stability and Absence of Violence/Terrorism. The main variable of interest is $Statehiste$, which stands for the extended state history index of Borcan *et al.* (2018). It reflects the historical depth of experience with statehood, accumulated over a period of six millennia from 3500BCE to 2000CE. β captures the effects of state history on contemporary political instability across 109 non-European societies ($i = 1,2, \ldots ,109$). To avoid omitted variables bias, the benchmark model specification accounts for numerous country-level geographic attributes ($Geo\_Controls$). They include absolute latitude, terrain ruggedness, mean elevation, the range of elevation, mean land suitability, the range of land suitability, distance to the nearest waterway, and a dummy for island nations (see Section 4). Moreover, I incorporate binary variables for the Word Bank's regions to control for unobserved region-specific factors ($Region\_FE$). Variables' descriptions, data sources, and summary statistics are provided in the online Appendix.

3.2. Political instability

To capture international differences in political instability, this paper exploits the World Bank's index of Political Stability and Absence of Violence/Terrorism, following Grechyna (2018). The construction of this index relies on standardized surveys that reflect respondents' perceptions of a government collapse and the presence of violence. The indicator, therefore, measures the probability that a government can be destabilized by contravention of established conventions (unconstitutional methods) and/or politically motivated violence, including terrorism.[10] More broadly, it reflects perceptions of the probability of riots, revolutions and other forms of violence. Higher values of the World Bank's index correspond to a lower likelihood of a government collapse and the absence of violence within an economy. For ease of interpretation, I re-construct this index by calculating the difference between the maximum value of the whole sample and each country-year value (1996-2015), consistent with the approach of Grechyna (2018). This provides a comparable measure of the worldwide distribution of political instability, with higher scores denoting greater political uncertainty (Panel A, Figure 4). To estimate cross-sectional models, I compute a simple average of this index for 109 countries outside Europe from 1996 to 2015. It is noteworthy that the degree of political instability appears to be very stable within an economy over time (Figure 2). The results, therefore, are unlikely to be driven by using a simple average of the data between 1996 and 2015. Section 7 further explores this possibility by employing repeated

[10] The concept of political instability often equates with political uncertainty or political turnover when it refers to the likelihood of major changes in the government (Grechyna, 2018).

cross-country data on the occurrence of riots and revolts, which may serve as an alternative measure of the prevalence of socio-political unrest.

3.3. Statehood experience

The historical depth of experience with statehood of each present-day country is measured by the extended state history index constructed by Borcan *et al.* (2018). The method of construction is similar to one developed by Bockstette *et al.* (2002). More specifically, Borcan *et al.* (2018) exploit archaeological data, covering a period of six millennia (3500BCE to 2000CE), to calculate the state history score, consisting of three main dimensions of statehood experience as follows:

$$s_{it} = z_{it}^{presence} \times z_{it}^{autonomy} \times z_{it}^{coverage} \times 50$$

in which s_{it} is the state history score of each present-day country i in a given 50-year period t between 3500BCE and 2000CE. $z_{it}^{presence}$ captures the early existence of a state, and it is assigned a value of 1 if there existed a government above the tribal level, 0.75 if the government could be at best described as a paramount chiefdom, and 0 if there was no government. $z_{it}^{autonomy}$ reflects the autonomy of a state, taking a value of 1 if a country was ruled by an internal government, 0.5 if it was ruled by a foreign government, and 0.75 if the rule was locally based but with substantial foreign oversight. $z_{it}^{coverage}$ captures the territorial coverage of a government. It equals 1 if the proportion of territories covered by this government was greater than 50%, and 0.75, 0.5, and 0.3 if the territorial coverage of the government was, respectively, 25-50%, 10-25% and below 10%. Next, three different components of state experience are multiplied together and by 50. This yields the state history score of each country covering 110 periods of 50 years between 3500BCE and 2000CE.

The next procedure is to construct the overall state history index as represented below:

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

where $Statehiste_i^{\varphi}$ is the overall state history index and it can be computed over different periods of time by adjusting the number of 50-year periods (φ). I employ the extended state history index covering a period of six millennia (3500BCE – 2000CE), dating back to prehistoric times when the first statehood was recorded ($\varphi = 110$). Alternative periods of statehood can be used for comparison and robustness checks. The construction of the overall state history index is performed through calculating the sum of s_{it} across 110 50-year periods. A discount rate of 1% ($\delta = 0.01$) is applied to account for the possibility that statehood experience obtained in more recent periods has larger effects on contemporary economic development. In other words, smaller weights are given to state experience accumulated in the more distant history. Next, these summary values are normalized by dividing by their maximum achievable value of 50. Hence, the state history index ranges between zero and one, with higher values denoting greater statehood experience. Panel B of Figure 4 depicts the international variation in the extended state history index (3500BCE – 2000CE) of Borcan *et al.* (2018).

4. Identification strategy

A major threat to identifying the causal effects of *Statehiste* on *PIS* is potential endogeneity bias induced by failure to account for a relevant factor and/or measurement errors in the state history index. It is important to note that state history has been typically treated as an exogenous source of international variation in contemporary economic performance in most influential studies in the long-term comparative development, conditional on accounting for a wide range of possibly confounding factors (see, for example, Bockstette *et al.*, 2002; Chanda & Putterman, 2007; Putterman & Weil, 2010; Borcan *et al.*, 2018). The underlying argument is that the adoption of a historical perspective helps avoid the problem of reverse causality. As argued previously, it is implausible to assume that present-day political instability exerts a direct influence on the early emergence of historical states, taking place nearly six millennia ago. Therefore, this paper employs three alternative strategies of identification to rule out the possibility that the positive relationship between state history and political instability outside Europe is exclusively driven by potential confounders and/or measurement errors associated with capturing statehood experience.

4.1. Observed confounding factors

The first empirical strategy relies on accounting for numerous observed confounders, which is consistent with the existing literature (see, e.g., Bockstette *et al.*, 2002; Borcan *et al.*, 2018).

The study incorporates an extensive set of country-level geographic characteristics in the benchmark model. These factors can be correlated with both the historical evolution of states and political uncertainty. This approach arguably addresses a concern that the results are merely proxies for geographic attributes, which are the key drivers of comparative cross-country prosperity. Ang (2015) presents empirical estimates of the impacts of numerous geographic variables on accumulated statehood experience, which partly motivates the choice of main geographic control variables. It is widely acknowledged that absolute latitude and distance to the nearest waterway help explain substantial variation in economic performance across the globe. These two geographic characteristics can exert an influence on the prevalence of socio-political unrest through shaping climate, the quality of institutions, income levels and trade-related mechanisms (Arbatlı *et al.*, 2020). Following Fearon and Laitin (2003), the paper controls for the effects of the ruggedness of terrains. The underlying idea is that rugged terrains may provoke greater political uncertainty through providing safe havens for rebels (Fearon & Laitin, 2003). Additionally, countries characterized by rough terrains may experience higher degrees of population heterogeneity because geographic isolation can be linked to the emergence of numerous subgroups within a population (Michalopoulos, 2012). This may worsen political unrest because it is more difficult for the government to reconcile large heterogeneity in preferences for public goods and redistributive policies (Arbatlı *et al.*, 2020).

Following the same line of argument, I augment the benchmark analysis by controlling for other geographic attributes, which fundamentally drive cross-country differences in political instability through affecting ethnolinguistic fractionalization (Michalopoulos, 2012). They include mean elevation, the dispersion of elevation, mean land suitability and the dispersion of land suitability. An additional concern relates to the possibility that island nations followed different (historical) patterns of state formation and development, and the evolution of political unrest due to their geographic isolation. Moreover, island countries could be subject to relatively higher levels of immunity to cross-border spillovers of violent conflicts (Arbatlı *et al.*, 2020). It is noteworthy that countries with a land connection to other nations could benefit from the international dissemination of state knowledge and early technologies, thus accumulating greater experience with state-like polities (Ang, 2015). Hence, the baseline model specification controls for a binary variable for island countries. Region dummies are included in Eq. [1] to account for unobserved heterogeneity across regions (e.g., common cultures, histories and other geographic characteristics), following the World Bank’s classification (see the notes to Table 1).

4.2. Selection on observables and unobservables

The baseline analysis incorporates a variety of geographic characteristics to avoid obtaining spurious estimates. Nevertheless, it is impossible to identify all potentially confounding variables and control for them in the regression.

To find additional support for causal inference, the study implements the coefficient stability test advanced by Oster (2019). This method permits a quantitative assessment of the relative importance of potentially unobserved confounders required to explain away the observed association between state history and political instability. The basic premise is that the degree of selection bias attributed to unobserved confounding factors can be detected by the decrease in selection bias from incorporating additional observed control variables, as put forward by Altonji *et al.* (2005). As such, it is possible to perform an empirical analysis of the amount of selection on unobservables, relative to that on observables, in order to drive the coefficient on *Statehiste* down to zero (Altonji *et al.*, 2005; Oster, 2019). This method is particularly relevant in this context given that a key threat to obtaining a causal interpretation from the baseline estimates stems from possible failure to incorporate relevant variables in the regression specification.

Following Oster (2019), an important assumption of the data-generating process can be expressed as follows:

$$PIS_i = \alpha + \delta Statehiste_i + \omega X_c^o + \theta X_c^u + \epsilon_i \qquad [2]$$

where X_c^o denotes a vector of observed control variables, which are geographic controls and region dummies included in Eq. [1]. X_c^u represents a vector of unobserved control variables, and ϵ_i stands for the disturbance term. This hypothetical model assumes that the estimate of the treatment variable (*Statehiste*) is determined by both observed confounders (X_c^o) and unobserved confounders (X_c^u). Let $W^o = \omega X_c^o$ and $W^u = \theta X_c^u$. Also define the proportional selection relationship below:

$$\delta \frac{cov(W^o, Statehiste)}{var(W^o)} = \frac{cov(W^u, Statehiste)}{var(W^u)} \qquad [3]$$

where the delta statistic (δ) is the coefficient of proportionality. More specifically, the paper considers the δ value that captures how strong the association between unobserved confounders and the treatment variable, relative to that between observed confounders and the treatment variable, needs to be in order to easily explain away the estimated effects of state history ($\beta = 0$). If the δ value equals 1, observed and unobserved variables are equally correlated with statehood experience, whereas, unobserved confounders are less correlated with state history compared with observed confounders if the δ statistic is less than unity.

Assuming that observed and unobserved variables are equally important in accounting for the observed association between state history and political instability ($\delta = 1$), Oster (2019) demonstrates that

$$\beta^{*} = \tilde{\beta} - \left[\ddot{\beta} - \tilde{\beta}\right] \frac{R_{max} - \tilde{R}}{\tilde{R} - \ddot{R}} \qquad [4]$$

where β^{*} represents the bias-adjusted treatment effect. $\ddot{\beta}$ and $\ddot{R}$ denote, respectively, the coefficient and the *R*-squared obtained from regressing *PIS* on *Statehiste* without controls. $\tilde{\beta}$ and $\tilde{R}$ are, respectively, the coefficient and the *R*-squared of the regression of *PIS* on *Statehiste* with full observed controls (X_c^o). R_{max} is the *R*-squared of a hypothetical regression, expressed in Eq. [2]. β^{*} reflects the estimated value of the coefficient on *Statehiste* if unobserved and observed confounders are equally related to state history. The construction of the bias-adjusted coefficient (β^{*}) relies on exploiting the movements of coefficients and R-squared values when observed control variables are incorporated in the regression. This permits estimating the amount of bias induced by unobservables, assuming proportional selection. Employing simulated and observational data, Oster (2019) provides strong empirical validation for this estimator. Additionally, Oster (2019) recommends using the interval bounded by the estimated coefficient on *Statehiste* and β^{*} to check for robustness to omitted variables bias. If the bounded set safely excludes zero, there is evidence against the null hypothesis that the observed association between state history and political instability is exclusively driven by selection on unobservables.[11]

4.3. Isolating exogenous sources of variation in statehood experience

A final method of reaching a causal interpretation is based on exploiting plausibly exogenous sources of variation in statehood experience that contribute to explaining the worldwide distribution of socio-political unrest. This empirical strategy requires identifying a valid instrumental variable, which exerts no direct influence on a country's political instability except through shaping the formation and development of historical states.

To this end, the paper employs the length of time elapsed since the transition to sedentary agriculture (*Agyears*) as an instrument for *Statehiste*. This approach utilizes the availability of anthropological and archaeological evidence documenting that the early existence of sedentary

[11] See Oster (2019) for more detailed theoretical and empirical discussions of this method. There are numerous empirical studies that adopt the method developed by Oster (2019) as a robustness check for omitted variables bias. See, among others, Arbatlı *et al.* (2020) and Vu (2020).

agricultural settlements led to the emergence of a state above the tribal level in prehistoric times (Ang, 2015). The first Neolithic revolution was recorded 10,500 years ago in several Middle Eastern countries, including Israel, Jordan, Lebanon and Syria. These oldest civilizations, included in the sample of this paper, were typically subject to early state development. As reviewed by Borcan *et al.* (2018), the first state-like polity emerged around 5000 years ago, preceded by the Neolithic revolution throughout the world. There hardly exists any archaeological evidence suggesting the presence of a state before the onset of sedentary agricultural settlements (Diamond, 1997; Ang, 2015). An early contribution by Diamond (1997) suggests that the historical transition to sedentary agriculture was a basis for the abundance of food supply, which in turn translated into the emergence of the institutionalization of power relations. A possible explanation is that the abundance of food supply led to the existence of a non-food producing class that could specialize in other activities, such as designing laws and building military forces. This gave rise to the emergence and development of statehood. Furthermore, substantial increases in agricultural productivity following the Neolithic transition improved the capacity to raise taxes (fiscal capabilities) and maintain social order in historical societies. For these reasons, the timing of Neolithic revolution is positively correlated with the formation and development of states, which points to the relevance of the instrumental variable.[12]

The validity of this excluded instrument partially relies on an observation that the onset of sedentary agricultural settlements took place independently across the world. Additionally, I argue that state history potentially transmits to the persistence of political instability outside Europe through undermining the provision of public goods, redistributive policies, and income (or productivity) levels (Figure 3). These mechanisms largely rest upon the role of the government in providing public goods and designing the institutional framework, or, more broadly, state policies. Hence, the timing of the Neolithic revolution plausibly exerts no direct influence on contemporary degrees of political uncertainty except through fostering the formation and development of historical states. This provides a basis for the plausibility of the exogeneity condition. The IV estimates, therefore, permit a causal interpretation at least for the purpose of an alternative strategy of identification.

[12] Using a cross-country analysis, Ang (2015) shows that the length of time elapsed since the transition to sedentary agriculture exerts a strong and robust positive influence on state history, accumulated from 1 to 1950CE. This, at least partially, provides evidence of a strong instrument.

5. Main findings

5.1. OLS estimates

This section presents OLS estimates of the effects of state history on contemporary political instability, using a sample of 109 non-European countries. Panel A of Figure 1 depicts the partial relationship between statehood experience and political instability outside Europe. It reveals that long-standing states are more likely to suffer from the prevalence of riots and revolts, *ceteris paribus*. This is consistent with the main hypothesis that accumulated statehood experience is a barrier to establishing politically stable regimes across non-European societies.

The positive link between state history and political uncertainty can be illustrated by the data of two countries in sub-Saharan Africa, namely Ethiopia and Namibia. The early presence of sedentary agricultural settlements in Ethiopia, which occurred 4,000 years ago, conferred this very old civilization with large accumulated statehood experience through promoting the formation and development of state-like polities. The value of the state history index of Ethiopia is approximately 0.52, which is much higher than of that of Namibia (0.02). More specifically, the difference in *Statehiste* between these two societies is substantial, and it equates to approximately 2.7 standard deviations of the extended state history index (3500BCE – 2000CE). This possibly translates into significant disparities in the pervasiveness of political instability between these two African economies. In particular, Namibia established more politically stable systems than those set up by Ethiopia. Namibia experiences a relatively low value of *PIS* of 1.27, while the *PIS* score of Ethiopia is 3.35. These two countries are separated by approximately 2.5 standard deviations of the World Bank's index of political instability.

It is important to note that Ethiopia is the only country in sub-Saharan Africa that managed to resist colonization arguably due to its long history of state development (Hariri, 2012). However, a long duration of state history of Ethiopia is linked to institutional stagnation, reduced productivity and the prevalence of socio-political unrest in present times. Consistent with the central argument, very old civilizations can be overcome by newly established states in terms of income per capita and the quality of institutions, which directly matter for the ability to establish politically stable systems. Hence, a long history of statehood appears to be a barrier to maintaining political stability outside the European world. This stands in stark contrast to many studies exploring the contribution of early state development to international variation in economic prosperity (Bockstette *et al.*, 2002; Chanda & Putterman, 2007; Putterman & Weil, 2010; Ang, 2013b).

However, it is difficult to obtain a causal interpretation from these stylized facts. Therefore, this paper estimates the benchmark model expressed in Eq. [1], controlling for a wide range of country-level geographic attributes and unobserved region-specific factors. Table 1 presents the OLS estimates of the causal influence of state history on today's political instability outside Europe. The empirical analysis starts with the adoption of the state history index constructed between 3500BCE and 1CE, excluding statehood experience obtained in the Common Era (column 1). The results in column (2) capture the contribution of state experience, accumulated over a period before the mass migration of Europeans throughout the world (3500BCE – 1500CE), to the global distribution of political instability. In column (3), I employ the extended state history index, reflecting long-term exposure to statehood from 3500BCE to 2000CE.

Following Putterman and Weil (2010), the paper also utilizes an ancestry-adjusted index of state history to account for the possibility that statehood experience of present-day countries is partially mediated by cross-border migration flows of people. The basic premise is that the formation and development of historical states could be affected by movements of people through shaping the international diffusion of technologies, state knowledge and capabilities, and the quality of institutions. The construction of the ancestry-adjusted state history indicator exploits the World Migration Matrix developed by Putterman and Weil (2010). It provides information on the estimated percentages by location of the present-day population's ancestors in 1500CE. For instance, the World Migration Matrix allows tracing where the ancestors of India were living in 1500CE. Accordingly, 97.9%, 1% and 1.1% of the current population of India descended from India, Bangladesh and Pakistan, respectively. The ancestry-adjusted index of statehood experience of India is a weighted average of state history scores of these three source countries, in which the weights correspond to population ancestral proportions. This permits an assessment of whether the observed relationship between state history and political instability outside Europe is exclusively driven by historical migration flows.

It is evident from the benchmark findings that the estimated coefficients of *Statehiste* are positive and statistically significant at the 1% level in all cases (Table 1). The results reveal that non-European societies characterized by a longer history of state experience tend to suffer from higher degrees of political instability, holding everything else constant. The positive association between state history and political uncertainty is robust to accounting for numerous geographic characteristics and unobserved heterogeneity across regions.[13] This lends empirical support to the main hypothesis that a long history of statehood is associated with greater political instability outside Europe. This finding offers novel insights into the strand of literature exploring the contribution of early state development to explaining substantial variation in economic prosperity across the world. The study, in particular, documents evidence of the negative consequences of early state development outside the European world. As argued earlier, a possible explanation is that accumulated statehood experience impeded the diffusion of European (democratic) institutions, thus worsening the quality of institutions of present-day countries (Hariri, 2012). Moreover, long-standing states are characterized by the emergence of powerful elites and entrenched groups, leading to institutional and productivity stagnation (Lagerlöf, 2016; Borcan *et al.*, 2018; Olsson & Paik, 2020; Vu, 2021). Hence, socio-political unrest tends to proliferate in old and autonomous states outside Europe (Figure 3). My results provide some support for recent studies documenting the negative impacts of early development on present-day economic performance (Lagerlöf, 2016; Borcan *et al.*, 2018; Olsson & Paik, 2020; Vu, 2021).

The size of the estimated coefficients decreases marginally from columns (1) to (4) of Table 1. Figure 5 illustrates the variation in the point estimate and the 95% confidence interval of the coefficients on *Statehiste* when exploiting different periods of statehood experience. Accordingly, the largest estimated effect is recorded in the model specification that incorporates statehood experience obtained from 3500BCE to 1CE (column 1). The magnitude of the coefficient reduces when the construction of the state history index considers state experience accumulated in the Common Era, as shown in columns (2) and (3) and Figure 5. The last column of Table 1 adjusts for the persistent effects of (historical) cross-border movements of people. It is important to re-highlight that the oldest state-like polities emerged around 5000 years ago outside the European world (Borcan *et al.*, 2018). These long-standing states also suffer from the persistence and pervasiveness of socio-political unrest in modern times. Therefore, an explicit focus on statehood experience obtained before the Common Era produces quantitatively larger impacts of state history

[13] Drawing reliable inference on the baseline OLS estimates requires some attention to the statistical adequacy of the model. I report the results of Ramsey's *RESET* test of functional form misspecification in Table 1, which are suggestive of correctly specified models. Additionally, the paper checks for the normal distribution of the disturbance terms, using Doornik-Hansen's *Normality* test. The results reveal the normality assumption of the data is not violated. These results, at least to some extent, lend credence to reliable statistical inference. See also the notes to Table 1.

on contemporary political instability. This reinforces the central hypothesis that early emergence of state-like polities is detrimental to building politically stable regimes outside Europe.

Following Borcan *et al.* (2018), I employ the extended state history index (3500BCE – 2000CE) in the main analysis because it reflects state experience obtained over a prolonged period of six millennia (since the first statehood was recorded). However, I obtain broadly similar results in columns (2) to (4). This reveals that the results are insensitive to accounting for (historical) migration flows across the world, starting from the sixteenth century. More specifically, the exclusion of state experience obtained from 1500CE to 200CE fails to alter the core results (column 2). The use of an ancestry-adjusted measure of state history slightly reduces the magnitude of the coefficient on *Statehiste*, but the effects are still precisely estimated at conventional levels of significance (column 4). Therefore, the paper selects the estimated results in columns (3) and (4) as the benchmark findings, which are exploited to conduct numerous robustness checks later.

For ease of comparison, I replicate the main results by using the original state history index of Bockstette *et al.* (2002), which captures statehood experience obtained from 1 to 1950CE. Figure 5 depicts the point estimate and 95% confidence interval of the coefficient of *Statehiste* (1-1950CE*)*.[14] As noted by Borcan *et al.* (2018), the indicator constructed by Bockstette *et al.* (2002) disregards the early presence of many old and autonomous states before the Common Era. As such, the results may suffer from potential bias induced by measurement errors in the state history index. In particular, the coefficient of *Statehiste* reduces by more than a half when statehood experience obtained before the Common Era is not taken into consideration (Figure 5). This is in line the findings of Borcan *et al.* (2018) who propose the use of an extended measure of state history.

It is noteworthy that accumulated statehood experience also exerts an economically significant influence on present-day political instability. More specifically, the results in column (3) indicate that a 0.2-unit increase (approximately an extra standard deviation) in *Statehiste* is associated with a 0.3-unit increase in *PIS*, which equates to just below a half of a standard deviation of the World Bank's index of political instability (0.829). As discussed above, the state history index of Ethiopia and Namibia is 0.52 and 0.02, respectively. The baseline estimates suggest that if Namibia instead experienced a score of the state history index similar to that of Ethiopia, the predicted increase in the political instability index of Namibia would equate to roughly 0.751 units.

[14] The full results, not reported for brevity, are available on request.

This projected increase is slightly smaller than a standard deviation of *PIS*. Overall, this paper documents that statehood experience has strong and robust effects on contemporary political instability outside the European continent.

Furthermore, the paper finds that several country-level geographic characteristics contribute to the worldwide distribution of political instability. As presented in Table 1, societies located further away from the equator tend to establish politically stable systems. The coefficient of *Absolute latitude* is negative and statistically significant at the 1% level. Moreover, *Distance to the nearest waterway* is associated with greater political instability. These findings are consistent with previous studies documenting that riots and revolts tend to proliferate in tropical and isolated countries, which can be explained by climatological, institutional and trade-related mechanisms (see Arbatlı *et al.*, 2020). I also find that rugged terrains exert a positive and statistically significant influence on political instability at the 1% level (Table 1). This lends support to the argument that *Terrain ruggedness* may induce greater fractionalization within a society, making it difficult to build up politically stable regimes. Consistent with these results, the paper documents that the dispersions of land suitability for agriculture and elevation are linked to greater political uncertainty through shaping ethnic fractionalization (Michalopoulos, 2012). In contrast to Michalopoulos (2012), *Mean elevation* is found to lower political instability. A recent study by Vu (2021) reveals that the mean level of elevation is associated with an unequal distribution of income arguably due to its potential effects on linguistic diversity. Nevertheless, my findings are suggestive of the positive effects of mean elevation on economic development, controlling for the intra-country dispersion of elevation and the ruggedness of terrains. As expected, a dummy variable for island nations enters the four main model specifications with a negative coefficient. Hence, island countries may suffer from less political instability compared with those characterized by a land connection with other countries. However, these estimated impacts are highly imprecise and not statistically significant at conventional thresholds (Table 1).

5.2. Addressing possible endogeneity concerns

The OLS estimates provide evidence that non-European countries characterized by a long history of statehood are likely to experience greater political instability. The findings remain insensitive to accounting for a wide range of geographic variables and unobserved region-specific factors. These results, at least partially, suggest that the observed association between state history and political instability may not be purely driven by the conventional fundamental causes of comparative cross-country development.

This sub-section further reduces the possibility of obtaining spurious estimates, induced by possible omitted variables bias, through adopting the coefficient stability test of Oster (2019), discussed in Section 4. I perform this empirical exercise using the baseline OLS estimates, and report the results in Table 2. I calculate the δ statistic using different periods of statehood experience (column 3, Table 2). More specifically, a value of 4.3 of the δ statistic, obtained from using an ancestry-adjusted measure of state experience, indicates that the correlation of unobserved confounders with *Statehiste* is required to be more than four times as large as that of observed confounders in order to result in the baseline estimates being indistinguishable from zero. Moreover, Oster (2019) reveals that the results are not exclusively driven by selection on unobservables if the δ statistic is greater than unity. As shown in column (3) of Table 2, the δ statistic exceeds this threshold in all cases. These results suggest that selection on unobserved factors needs to be unreasonably strong to account for the benchmark findings, thus providing evidence of robustness of the results to omitted variables bias.

Assuming that unobserved and observed variables are equally important in explaining the main hypothesis, I construct the bias-adjusted coefficient β^* (column 2, Table 2). In all cases, the interval bounded by the main coefficient and β^* safely excludes zero. Hence, one can reject the null hypothesis that the political legacy of state history is exclusively driven by the effects of potential unobserved confounders. It is challenging to identify and incorporate all confounding factors in standard regression analysis. However, the paper provides an assessment of the relative importance of unobserved variables, suggesting that they cannot easily explain away the observed association between state history and political uncertainty outside the European world.

An additional attempt at obtaining causal inference relies on using plausibly exogenous sources of variation in the formation and development of historical states. Consistent with my previous arguments, I employ the length of time elapsed since the transition to sedentary agriculture (*Agyears*) to perform IV regressions (Table 3). According to the first-stage estimates, *Agyears* has positive and statistically significant effects on accumulated statehood experience, in line with the findings of Ang (2015). The early emergence of sedentary agricultural settlements helps explain cross-country differences in state experience, thus lending support to relevance of the instrument (Figure 6). The *F*-statistic of excluded instruments of Olea and Pflueger (2013) is also much larger than the conventional threshold of 10. This reveals that *Agyears* is not a weak instrument.[15] As shown in the second-stage regression, the plausibly exogenous component of *Statehiste* exerts positive impacts on contemporary political instability. The effects are precisely estimated at conventional levels of statistical significance in all cases. Overall, the IV results are broadly consistent with the OLS estimates. To check whether the IV estimates are driven by weak instrument bias, I report identification-robust Anderson-Rubin confidence intervals, all of which safely exclude zero. This permits an interpretation of statistically significant effects of *Statehiste* on *PIS*. Andrews *et al.* (2019) demonstrate that these results are efficient irrespective of the strength of the excluded instrument in the first-stage regression.

It is noteworthy that the magnitude of the coefficients of *Statehiste*, reported in Table 3, turns out to be much larger than that of the OLS estimates. This can be attributed to the persistence of socio-political unrest in prehistoric times (Arbatlı *et al.*, 2020). The OLS estimates can suffer from a downward bias if the early presence of riots can reduce the ability to obtain statehood experience. Additionally, I assume earlier that cross-border movements of people starting in 1500CE, especially to the New World, can confound my results. This concern is partly addressed by adjusting for the ancestral composition of the population of present-day countries and excluding statehood experience obtained from 1500CE to 2000CE (columns 2 and 4, Table 1). However, migration flows can affect current economic performance through human capital, socio-political institutions, and other unobserved mechanisms (Arbatlı *et al.*, 2020). The OLS estimates can be attenuated if these channels, which may not be captured by the ancestral composition of the current population, affect the ability to maintain a politically stable environment. However, the IV estimates indicate that the extent to which state history contributes to international variation in present-day political instability is even more economically significant.

There exists no perfect strategy for identifying the causal effects of state history on political uncertainty. This study, therefore, adopts different alternative methods of identification. All of them, reassuringly, provide robust evidence of the economic and statistical significance of the political legacy of early state development outside Europe, thus permitting causal inference. The following section employs both the OLS and IV estimates reported in the last two columns of Tables 1 and 3 to perform a variety of robustness analyses to avoid obtaining spurious estimates.

[15] As suggested by Andrews *et al.* (2019), the effective *F*-statistic of Olea and Pflueger (2013) provides a valid basis for inference on weak instruments even when adopting non-homoscedastic, clustered and autocorrelated data.

6. Sensitivity checks

6.1. Robustness to controlling for other effects

This section replicates the main analysis by controlling for a wide range of potentially confounding factors. This reduces the possibility that the results are exclusively attributed to conventional explanations of international differences in political instability.

First, the central hypothesis suggests that long-term exposure to statehood may translate into the persistence of political uncertainty outside Europe through shaping the opportunity cost of engaging in riots, the quality of institutions, the provision of public goods and redistributive policies. One may well argue that the main findings are merely proxies for these factors. I contend that this assumption is implausible for several reasons. The inclusion of these variables in the regression fails to drive the results down to zero (Table 4).[16] Moreover, these factors are interrelated with and jointly determined by political instability, leading to reverse causality bias. For this reason, they are excluded from the benchmark model specification. Importantly, Acharya *et al.* (2016) reveal that empirical estimates can be biased if the baseline regression incorporates potential channels underlying the relationship between *Statehiste* and *PIS*.[17] However, the results reported in Table 4 suggest that my findings are not purely driven by failure to control for potentially mediating variables.

Second, I account for the confounding effects of population diversity in Table 5. It is widely acknowledged that population diversity, captured by ethnolinguistic fractionalization or polarization, is a barrier to establishing politically stable regimes (Fearon & Laitin, 2003; Blattman & Miguel, 2010). Countries characterized by the presence of numerous subgroups may suffer from mistrust and the under-provision of public goods, leading to greater political uncertainty. More recently, Arbatlı *et al.* (2020) find that heterogeneity in the composition of genetic traits, which captures interpersonal population diversity, is associated with the onset of civil conflicts. Hence, I re-estimate the benchmark model by controlling for different measures of population diversity (Table 5). However, the OLS and IV estimates retain their signs and significance levels in all cases.

[16] As shown in Table 4, I control for the log of income per capita (*Lgdppc*), which reflects the opportunity cost of engaging in socio-political unrest. The World Bank's index of control of corruption, a commonly used proxy for institutional quality (*Institutions*), is also included in the baseline model. I attempt to capture the provision of public goods and income redistribution through using an index of the relative difference between inequality of market and disposable income (*Redist*). The online Appendix contains detailed descriptions of variables. See also Section 7.

[17] Section 7 contains a more rigorous analysis of the role of these variables in mediating the benchmark findings.

Finally, Table 6 replicates the main analysis by accounting for other factors shaping the international variation in political instability.[18] Alesina and Perotti (1996) find that an unequal distribution of income is a key determinant of riots and revolts. This motivates the inclusion of an index of disposable income inequality in column (1). It is argued that countries with lower levels of social capital may suffer from the persistence of conflicts and political uncertainty. Thus, the baseline model incorporates a measure of social capital (column 2).[19] In column (3), I control for the effects of resource wealth through including four indicators of resource endowments (oil, gas, mineral and forest rents) in the regression (see, e.g., Dutt & Mitra, 2008). Following Krieger and Meierrieks (2011), the paper checks for robustness to accounting for democratic institutions. My findings can be confounded by country-level demographic characteristics, as suggested by Acemoglu *et al.* (2020). Hence, I incorporate population density and the size of population in column (5). The last three columns account for the political legacy of trade openness, urbanization and religions (Martin *et al.*, 2008). The positive relationship between *Statehiste* and *PIS* outside Europe retains its statistical significance in all cases. For this reason, the core findings are unlikely to be easily explained away by other effects.

6.2. Robustness to the plausibility of the exogeneity condition

The credibility of the orthogonality requirement provides a valid basis for statistical inference based on the IV estimates. The narrative presented in Section 4 suggests that *Agyears* is unlikely to transmit directly to the persistence of present-day political instability except through its effects on accumulated statehood experience. Unfortunately, attempts at empirical justification of exclusion restrictions are challenging due to the unobserved nature of the error components. My previous results indicate that the IV estimates retain their signs and statistical significance when accounting for a wide range of potentially confounding factors. To the extent that *Agyears* affects contemporary political instability through shaping conventional causes of riots and revolts, the exogeneity requirement is satisfied upon allowing them to enter the model specification. To find additional support for the exogeneity condition, this paper employs an alternative excluded instrument for *Statehiste*, and performs a test of over-identifying restrictions.

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[18] The estimated coefficients of additional controls included in Table 6 are omitted for brevity. However, they are available on request.

[19] It is important to note that data on social capital, constructed using the Word Values Survey, are sparse across countries. This prevents conducting a comparable replication of the core findings. However, the baseline estimates remain statistically significant at conventional levels even when estimating a more restricted sample.

Pursuing this strategy requires isolating at least one additional source of variation in state history because the baseline model is exactly identified. The empirical analysis is motivated by Vu (2021) who identifies the causal influence of state history on within-country income inequality using geographic distance to the regional leaders in 1000BCE (*Proximity*) as a valid instrument for state experience.[20] The basic intuition is that countries located near the regional frontiers in 1000BCE, which were areas with the highest level of development in prehistoric times, could benefit from the spillovers of state knowledge and technologies, and socio-economic interactions. This is conducive to the early emergence of state-like polities. Using a cross-country analysis, Ang (2015) finds that *Proximity* is strongly correlated with the formation and development of states. In line with these results, Vu (2021) documents a positive relationship between *Proximity* and the extended state history index of Borcan *et al.* (2018). Hence, *Proximity* may be a relevant instrument for statehood experience. Furthermore, Vu (2021) highlights that the advantage of being proximate to the regional leaders in 1000BCE has no direct effect on current economic performance. The basic idea is that most regional leaders are no longer frontiers of economic development in modern times. This suggests that *Proximity* may not directly affect current economic development.[21]

Table 7 reports the estimation results when using *Proximity* as an alternative instrument. The signs and significance levels of the IV estimates withstand this empirical exercise. Diagnostic tests and the first-stage estimates lend support to relevance of this additional excluded instrument, in line with the findings of Ang (2015) and Vu (2021) (Table 5). Importantly, it is evident from *p*-values of the test of over-identifying restrictions that the null hypothesis underlying the validity of additional instruments is not rejected at conventionally accepted levels of significance (columns 2 and 4, Table 7). This, at least to some extent, provides suggestive evidence of the validity of the exogeneity condition. Hence, the main IV estimates reasonably provide a valid basis for obtaining a causal interpretation.

[20] As put forward by Ang (2015), the regional leaders for each continent are societies with the largest urban settlements in 1000BCE. The basic premise is that only developed societies in prehistoric times were able to afford dense population. Exploiting historical data, Ang (2015) develops a measure of geographic proximity between a given country and the regional leaders based on the Haversine formula. Countries with higher values of *Proximity* are geographically close to the regional leaders in 1000BCE, thus driving the early emergence of state-like polities.

[21] Most regional leaders in 1000BCE, as identified by Ang (2015), have ceased to exist as the world leaders of technologies in present times. For this reason, the advantage of being proximate to a regional frontier in 1000BCE is relevant for the formation and development of historical states, but *Proximity* is unlikely to affect current economic development through shaping the spillovers of modern technologies and institutions. See also Ang (2015) and Vu (2021) for a more detailed discussion.

6.3. Additional robustness tests

To conserve space, the paper provides a more detailed discussion of other sensitivity analyses in the online Appendix; here I present an overview of the results. Specifically, performing numerous additional robustness checks demonstrates that the contribution of early state development to contemporary political instability across non-European societies is largely insensitive to (1) applying various discount rates to distant periods in the measurement of the state history index; (2) accounting for possible spatial dependence of statehood experience and socio-political unrest; (3) considering the role of potential outliers; (4) excluding different specific groups of countries; and (5) checking for the presence of a non-monotonic relationship.

7. Further analyses

7.1. State history and the occurrence of socio-political unrest

Exploiting repeated cross-country data, this section extends the baseline analysis by examining the relationship between accumulated statehood experience and the incidence of social and political unrest. From a conceptual perspective, this empirical exercise investigates the extent to which long-term exposure to statehood hinders the establishment of politically stable systems by *provoking* the prevalence of socio-political unrest within an economy. To this end, the paper utilizes a dichotomous measure of unrest, constructed by Acemoglu *et al.* (2019). This indicator, in particular, captures the occurrence of riots and revolts on an annual basis between 1960 and 2010 across countries.

Following the empirical framework of Arbatlı *et al.* (2020), I specify the following probit model, which is estimated using annual repeated cross-country data.

$$Unrest_{i,t} = \beta_0 + \beta_1 Statehiste_i + \beta_2 X_i + \beta_3 Unrest_{i,t-1} + \beta_4 \delta_t + \epsilon_{i,t} \qquad [5]$$

where $Unrest_{i,t}$ is binary variable measuring the occurrence of riots and revolts for country *i* in year *t* from 1960 to 2010. The time period is dictated by the availability of data provided by Acemoglu *et al.* (2019). $Unrest_{i,t-1}$ is the lagged occurrence of unrest, which captures the persistence of socio-political unrest. It is assumed that the probability of observing the incidence of unrest in a given year is higher in countries with an experience of riots and revolts in the previous year. The model incorporates numerous time-invariant variables. They include the state history index (*Statehiste*) and a set of geographic controls and region dummies (X_i), which are similar to those included in Eq. [1]. δ_t denotes a vector of year dummies that account for unobserved heterogeneity over time. $\epsilon_{i,t}$ represents the error term.

The paper adopts similar empirical strategies implemented in the main analysis to estimate Eq. [5]. Table 8 reports the probit and IV probit estimates of the effects of state history on the occurrence of riots and revolts. In all regressions, the analysis accounts for country-level geographic attributes, unobserved heterogeneity across regions and over years, and the persistence of socio-political unrest. As represented in Table 8, *Statehiste* enters all probit model specifications with a positive coefficient. The estimated effects of statehood experience on the incidence of unrest remain highly precise at the 1% level of significance (Table 8). It suggests that non-European societies endowed with a long history of statehood are more likely to experience the annual incidence of riots and revolts, leading to greater political instability. These findings extend the main analysis by documenting evidence that state history is associated with the persistence and pervasiveness of political instability outside the European continent via *triggering* the occurrence of riots and revolts.

7.2. Heterogeneity in the political legacy of different statehood components

The benchmark analysis exploits a summary measure of statehood experience, which incorporates three distinct aspects of state formation and development (Section 3). In this sub-section, the study investigates the possibility that three dimensions of state history exert heterogeneous impacts on contemporary political instability outside the European continent. Hence, I re-estimate the benchmark model using those three components of state experience. They include *Stage age* (the length of time elapsed since the first statehood was recorded), *State autonomy* (the degree to which the rule was internally based), and *State coverage* (the state's territorial unity).[22]

Table 9 reports empirical estimates of the heterogeneous political legacy of three components of statehood. It is evident from both the OLS and IV estimates that *State age* is associated with higher levels of political instability, consistent with the main hypothesis (Figure 3). In particular, *State age* enters all model specifications with a positive and statistically significant coefficient (Table 9). This lends credence to the baseline results arguing that the emergence of historical states leads to political instability possibly through giving rise to powerful

[22] See the online Appendix for a detailed description of these variables.

elites and institutional stagnation within a society.[23] Nevertheless, the study provides evidence that *State autonomy* and *Stage coverage* help lower the level of political instability. The estimated coefficients of these variables are negative in all cases, although the statistical significance of the point estimate varies considerably between the OLS and IV estimates (Table 9). When I include all three variable in one single OLS regression, the coefficients on *State autonomy* and *State coverage* turn out to be imprecisely estimated at conventional levels of significance (column 4, Table 9). The robustness of the coefficient on *Stage age* suggests that the main findings are largely driven by the early presence of a state above the tribal level.

Importantly, the results in Table 9 reveal that having experienced the early existence of state-like polities is a barrier to establishing politically stable regimes. By contrast, having experienced the rule of an internally based government (instead of being part of a foreign ruled empire) and having experienced territorial completeness (or unity) help lower uncertainty associated with the political environment of non-European societies in modern times. To my knowledge, possible heterogeneity in the degree to which accumulated statehood experience helps shape the worldwide distribution of economic prosperity has been largely unexplored in most previous studies in the long-term comparative development literature.

7.3. Potentially mediating mechanisms

As depicted in Figure 3, the positive link between *Statehiste* and *PIS* outside Europe can be mediated through several mechanisms. For instance, the study proposes that long-term exposure to statehood is linked to institutional stagnation of present-day countries. Poor-quality institutions eventually hinder establishing politically stable systems through lowering productivity and income levels (Section 2).[24] Additionally, poor governance impedes the provision of public goods or redistributive policies. These potentially mediating variables are included in Table 4, which fails to alter the main findings. Motivated by a recent study by Acharya *et al.* (2016), this sub-section provides a more rigorous analysis of possible mechanisms underlying the benchmark findings, including *Lgdppc*, *Institutions* and *Redist*. The online Appendix provides a detailed description of these variables. It is noteworthy the provision of public goods and/or redistributive policies are

[23] These results offer an additional interpretation of variation in the point estimate of the coefficient on *Statehiste* in Table 1, which is illustrated in Figure 5. They demonstrate that older civilizations outside Europe tend to suffer from the persistence and pervasiveness of political uncertainty.
[24] As discussed previously, income per capita arguably captures the opportunity cost of engaging in riots and revolts, which represents an important mechanism of transmission.

commonly measured by public expenditure on health or education. Such metrics, however, may not reflect progressive redistribution of income if they disproportionately benefit middle- and upper-income groups of society, particularly in the developing world (Milanovic, 2000). Hence, I employ *Redist*, which reflects the relative difference between inequality of net and market income, using the Standardized World Income Inequality database.[25]

A conventional empirical strategy relies on incorporating potentially mediating variables in the baseline analysis to check for the role of proposed mechanisms in explaining the observed relationship. Nonetheless, Acharya *et al.* (2016) argue that the inclusion of channels of transmission (e.g., *Institutions*) and the treatment variable (*Statehiste*) in one regression specification leads to biased estimates arguably due to mediating variables bias.[26] To address this concern, Acharya *et al.* (2016) recommend implementing a two-step regression procedure, which provides reliable inference on potential mechanisms of influence. Following this method, the current study performs a mediation analysis to capture the average controlled direct effects (ACDE) of *Statehiste* on *PIS*. The ACDE estimates reflect the contribution of accumulated statehood experience to contemporary political instability outside Europe after ruling out the effects of possibly mediating mechanisms.

Specifically, the first-step analysis involves regressing the outcome variable (*PIS*) on the treatment variable (*Statehiste*), potential mediators, and main control variables (geographic controls and region dummies). Exploiting the first-step estimates, the paper transforms the outcome variable by subtracting the effect of the mediating variable. In the second-step regression, the *demediated* outcome variable is regressed on the treatment, yielding ACDE estimates.[27] One may plausibly conclude that the impacts of state history on political instability are mainly mediated through the proposed mechanism if the ACDE estimates are indistinguishable from zero. By contrast, it is possible that the degree to which the treatment variable matters for the outcome variable is mediated through other pathways in addition to the proposed mechanism if the ACDE

[25] The adoption of health or education expenditure produces broadly similar findings. These results are not reported for brevity, but are available on request. *Redist* is constructed by the difference between the Gini coefficient of inequality of market income and disposable income (as a proportion of inequality of market income). Higher values reflect the government's greater efforts to provide public goods and redistributive policies.

[26] Acharya *et al.* (2016) present a detailed discussion on the extent of bias induced by simultaneously controlling for the treatment variable and the proposed mediating variables.

[27] A bootstrapping method is used to produce consistent estimates, as shown in the notes to Table 10.

estimates remain highly precise at conventional levels of statistical significance (see Acharya *et al.*, 2016 for more details).

The OLS estimates of the effects of state history on political uncertainty remain statistically significant at the 5% level in most cases after I rule out the effects of possibly mediating variables (Panel A, Table 10). This reflects a reduction in the precision of the point estimate of the coefficients on *Statehiste*, compared with those reported in Table 1. When exploiting a plausibly exogenous source of variation in state experience, the ACDE estimates turn out to be statistically insignificant at conventional thresholds in some cases (Panel B, Table 10). These patterns reveal that the causal influence of statehood experience on political instability is partially mediated through several proposed mechanisms, including institutional quality, income per capita (or productivity) and income redistribution (Acharya *et al.*, 2016). These results provide some suggestive evidence of the main hypothesis articulated in Section 2 (Figure 3).

8. Concluding remarks

This study attempts to identify the deep historical roots of political instability. It highlights the role of statehood experience in shaping the persistence and pervasiveness of socio-political unrest outside the European world. In contrast to numerous influential studies documenting the beneficial effects of state history on economic development, the current paper establishes that long-term exposure to statehood, obtained over six millennia, is a barrier to creating politically stable economies across non-European societies. The results, therefore, provide novel insights into the existing literature by demonstrating the negative consequences of early state development.

The paper also advances a central line of inquiry in economics that attempts to understand the causes of political instability. It goes beyond previous studies by providing suggestive evidence that contemporary political instability can be linked to the formation and development of historical states. By doing so, the current research is the first attempt to identify the deeply rooted historical factors behind the persistence and pervasiveness of riots and revolts. It follows from my findings that the political legacy of historical states should be taken into consideration when formulating policies that help curtail the prevalence of socio-political unrest. Moreover, the main results suggest that riots and revolts tend to persist in long-standing states outside Europe, which is at least partially attributed to a long history of statehood. Hence, policy-makers should take a long-term perspective in understanding the evolution of political instability. Importantly, the efficacy of today's policies arguably requires being compatible with the prevailing historical environment (Spolaore & Wacziarg, 2013; Nunn, 2020).

Employing data for 109 non-European societies, I provide strong and robust evidence of a positive relationship between state history (3500BCE – 2000CE) and present-day political instability. To obtain a causal interpretation from the empirical estimates, the study employs alternative strategies of identification, yielding remarkably similar findings. Moreover, the baseline results withstand numerous sensitivity checks. Further analyses using repeated cross-country data suggest that a long history of statehood is associated with the occurrence of riots and revolts outside the European continent. A replication of the main results that relies on decomposing the overall state history index reveals some novel findings. Specifically, I find that the length time elapsed since the first statehood was recorded is detrimental to establishing politically stable regimes, while the autonomy and territorial unity of historical states contribute to political stability. These heterogeneous patterns have been largely overlooked in previous studies exploring the impacts of early state development on economic performance. It is evident that the early existence of a state above the tribal level has negative long-term consequences on current economic development via inducing greater uncertainty associated with the political environment. By contrast, having been ruled by an independent (internally based) empire and having experienced territorial completeness contribute to economic prosperity through lowering political instability.

The current research proposes several possible mechanisms underlying the positive relationship between state history and political instability outside Europe (Figure 3). In particular, a long history of statehood is linked to institutional stagnation through impeding the diffusion of European institutions starting around the sixteenth century. Moreover, powerful elites and entrenched groups tend to proliferate in old civilizations. Consequently, poor-quality institutions translate into the persistence of political uncertainty via hindering the provision of public goods or redistributive policies, and lowering income or productivity levels (Figure 3). Using a mediation analysis developed by Acharya *et al.* (2016), the study provides some evidence that the degree to which state history matters for current political instability is at least partially mediated through income per capita, the quality of institutions and the government's efforts to provide public goods or redistributive policies.

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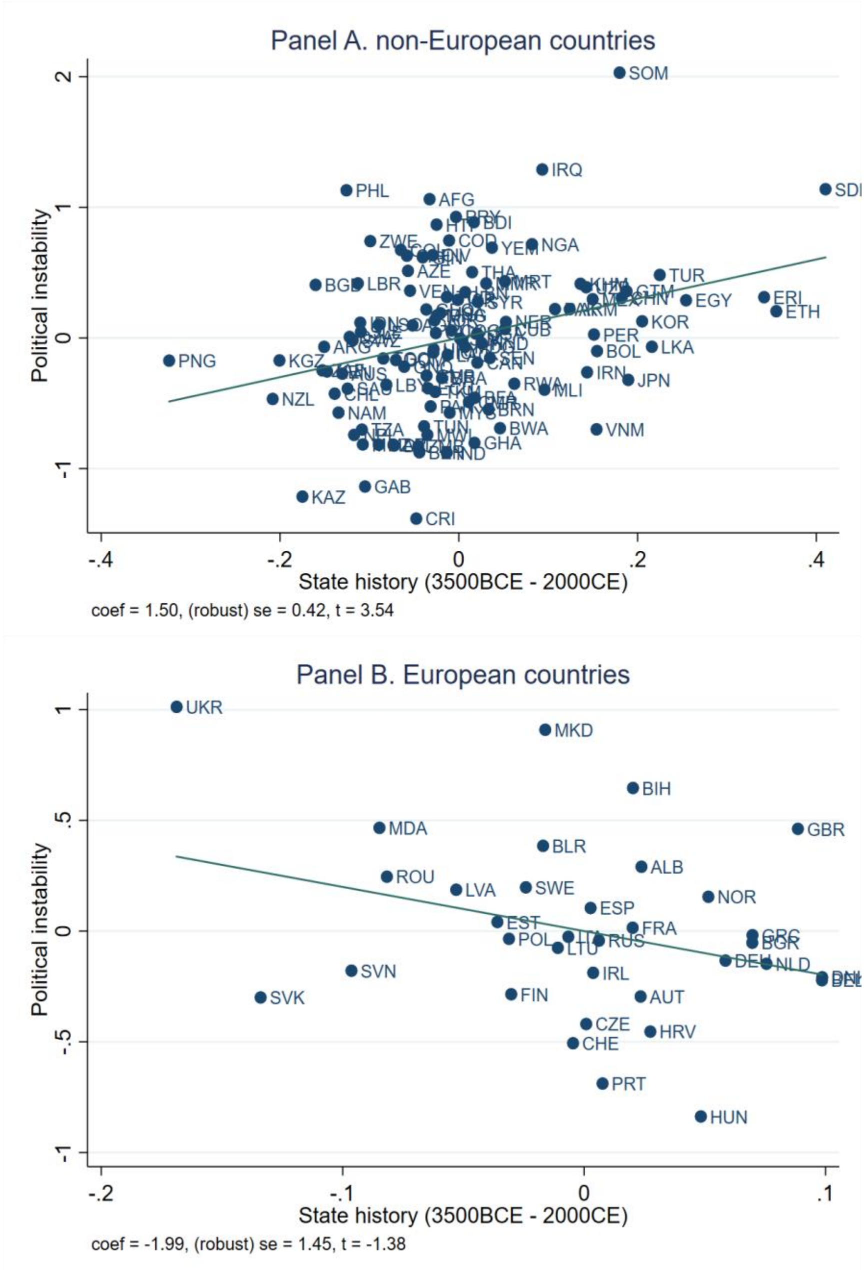

Figure 1. The relationship between state history and political instability

Notes: This figure illustrates the effects of statehood experience on political instability across the world, partialling out the effects of geographic attributes and unobserved region-specific factors. Statehood experience is captured by the extended state history index constructed by Borcan *et al.* (2018), with higher values reflecting greater statehood experience. Political instability is constructed using the World Bank's index of Political Stability and Absence of Violence/Terrorism, with higher values corresponding to greater political uncertainty. See the main text for more details.

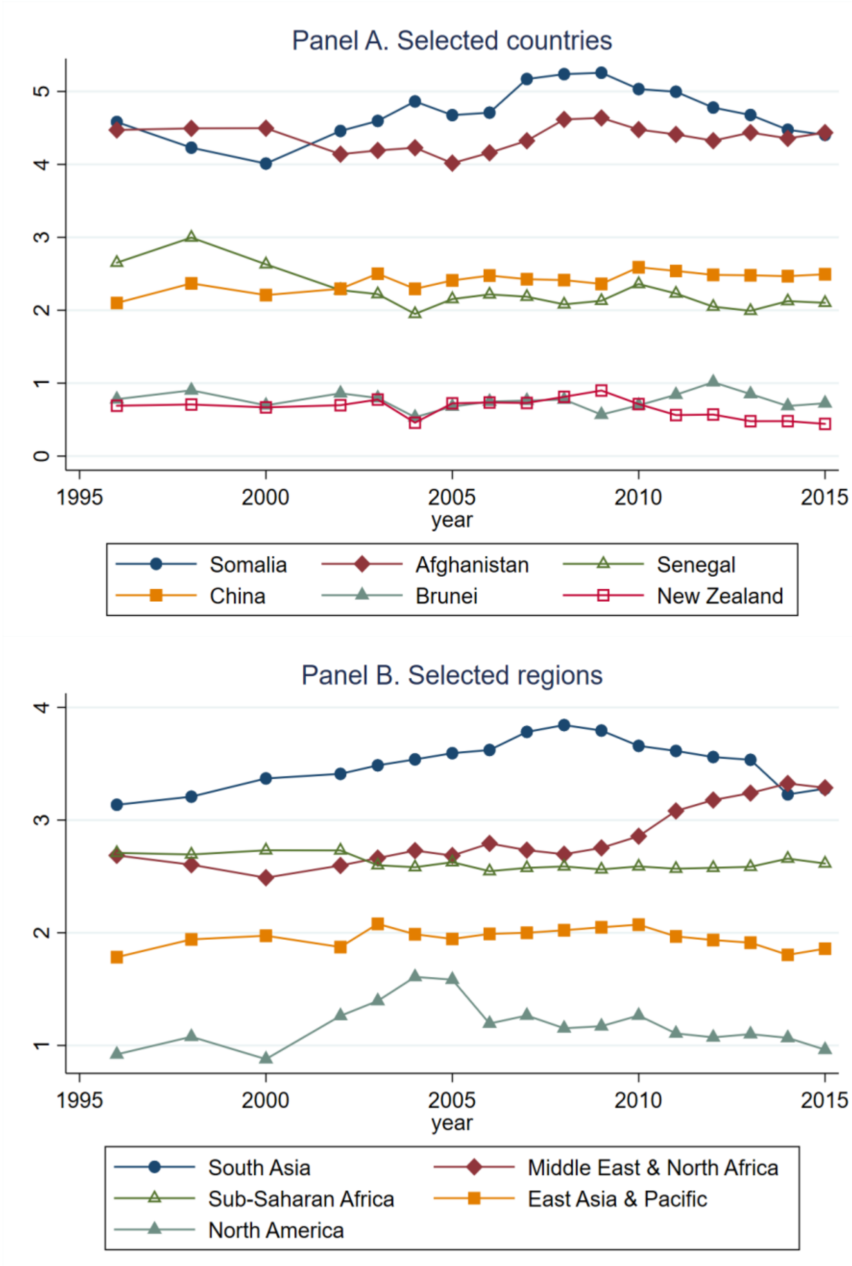

Figure 2. The evolution of political instability within selected countries and regions

Notes: This figure depicts the variation in the World Bank's index of political instability from 1996 to 2015. Panel A presents the data of six selected countries endowed with high, intermediate and low degrees of political uncertainty. Panel B exhibits a simple average of political instability for several regions.

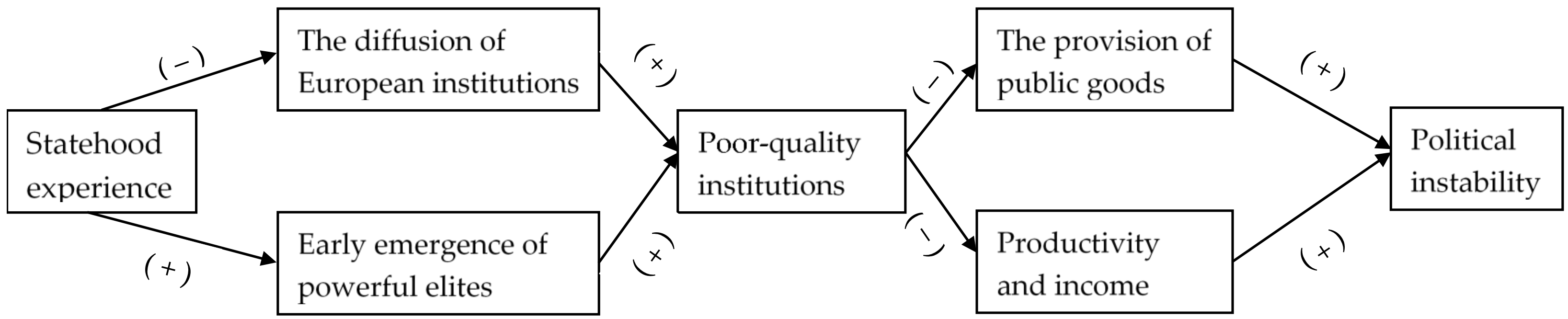

Figure 3. The hypothesized effects of statehood experience on political instability

Notes: This figure depicts the central hypothesis linking accumulated statehood experience and contemporary political instability. More specifically, state history impeded the diffusion of European institutions, and could give rise to powerful elites and entrenched groups within an economy. Thus, a long history of statehood is associated with institutional stagnation. Poor-quality institutions eventually translate into persistent political uncertainty through hindering the provision of public goods (or redistributive policies) and income levels. (–) and (+) denote positive and negative effects, respectively. See the main text for more details.

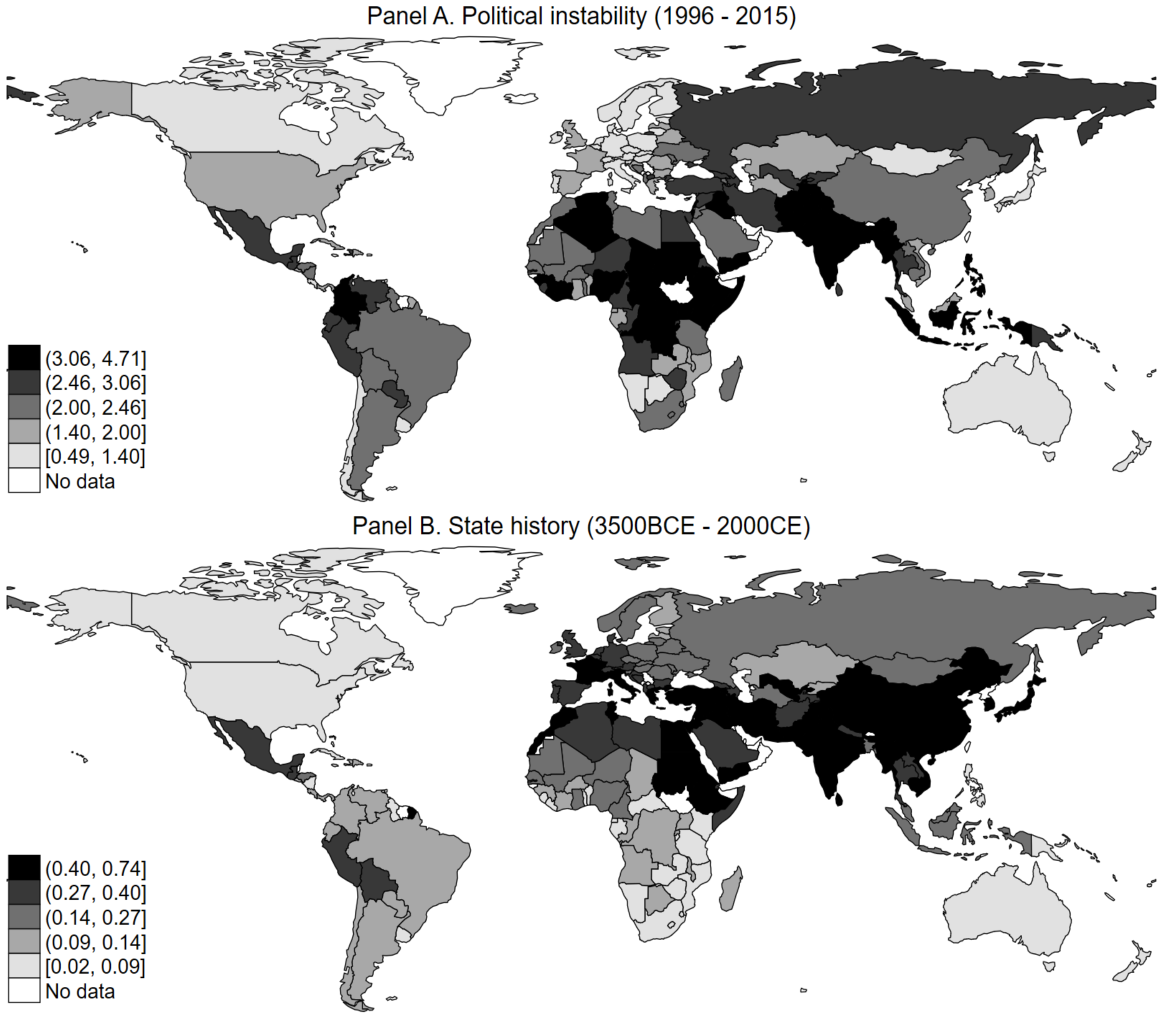

Figure 4. Cross-country differences in political instability and state history

Notes: This figure depicts the worldwide distribution of political instability and statehood experience. Data on political instability are averaged across the period 1996 – 2015. See the notes to Figure 1 and the main text for more details.

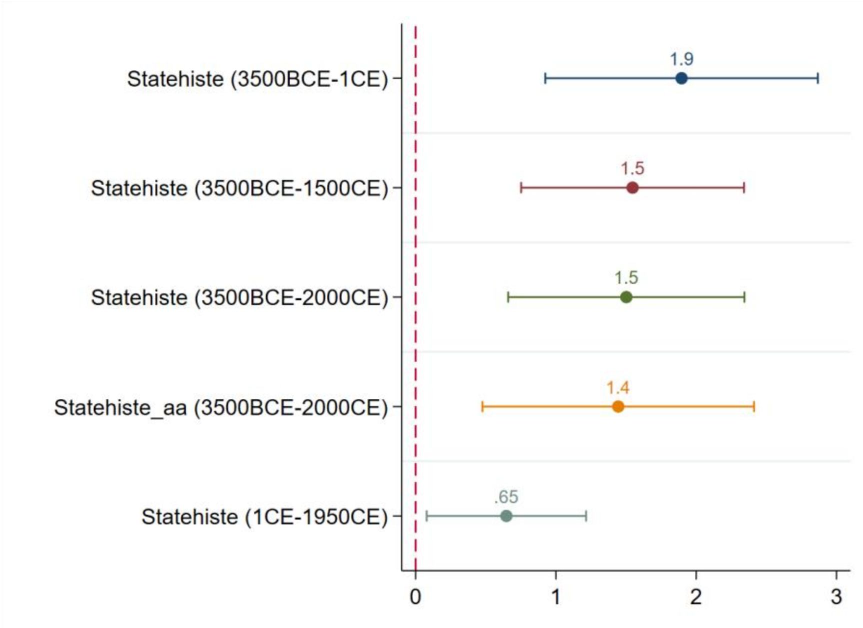

Figure 5. The effects of different periods of statehood experience on political instability

Notes: This figure depicts the point estimate and the 95% confidence interval of the coefficient on *Statehiste*, reported in Table 1. *Statehiste_aa* is an ancestry-adjusted measure of statehood experience. For ease of comparison, I also estimate the benchmark model using the original state history index (1- 1950CE) developed by Bockstette *et al.* (2002).

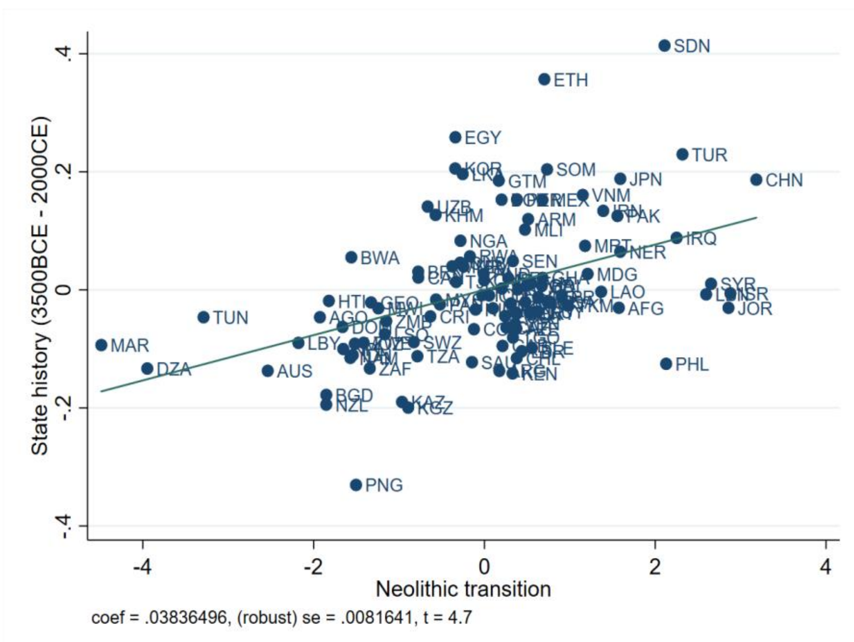

Figure 6. The effects of the Neolithic transition on state history

Notes: This figure depicts the partial effects of the Neolithic transition on accumulated statehood experience (3500BCE – 2000CE). These results are based on the first-stage estimates reported in Table 3.

Table 1. State history and political instability, OLS estimates

Dependent variable is *PIS*	(1)	(2)	(3)	(4)
Periods of statehood	3500BCE – 1CE	3500BCE – 1500CE	3500BCE – 2000CE	3500BCE – 2000CE (Ancestry-adjusted)
Statehiste	**1.896*** **	**1.547*** **	**1.502*** **	**1.444*** **
	[0.489]	[0.400]	[0.424]	[0.487]
Absolute latitude	-3.918***	-3.823***	-3.790***	-4.214***
	[0.570]	[0.604]	[0.604]	[0.630]
Terrain ruggedness	0.003***	0.003***	0.003***	0.003***
	[0.001]	[0.001]	[0.001]	[0.001]
Mean elevation	-0.478**	-0.498**	-0.488**	-0.467**
	[0.210]	[0.209]	[0.211]	[0.215]
Range of Elevation	0.156**	0.138*	0.138*	0.152**
	[0.071]	[0.071]	[0.072]	[0.071]
Mean land suitability	0.307	0.283	0.250	0.294
	[0.341]	[0.331]	[0.336]	[0.326]
Range of land suitability	0.731***	0.691***	0.699***	0.528**
	[0.235]	[0.236]	[0.241]	[0.253]
Distance to the nearest waterway	0.060***	0.064***	0.060***	0.061***
	[0.019]	[0.018]	[0.019]	[0.019]
Island country dummy	-0.250	-0.187	-0.195	-0.296
	[0.225]	[0.257]	[0.252]	[0.250]
Region dummies	Yes	Yes	Yes	Yes
RESET	0.137	0.260	0.223	0.094
Normality	0.067	0.182	0.236	0.192
Observations	109	109	109	107
R-squared	0.586	0.582	0.575	0.565

Notes: This table presents OLS estimates of the effects of statehood experience on political instability, using a sample of non-European countries. Region dummies stand for binary variables for East Asia and Pacific, Latin America and Caribbean, Middle East and North Africa, North America, South Asia, and Sub-Saharan Africa (Central Asia is omitted as the base group). This is based on the World Bank's classification of regions. *RESET* denotes *p*-values of Ramsey's test for functional form misspecification. *Normality* denotes *p*-values of Doornik-Hansen's test for the normal distribution of the error terms. An intercept is included in all regressions, but is omitted to conserve space. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2. The relative importance of selection on unobserved confounders

	(1)	(2)	(3)
Treatment variable	Baseline estimates – $\hat{\beta}$ (std. error) [R]	Oster's identified set ($\hat{\beta}, \beta^*$) ($R_{max} = 1.3 \times R, \delta = 1$)	Delta statistic (δ) for $\hat{\beta} = 0$
Statehiste (3500BCE – 1CE)	1.896*** (0.489) [0.586]	[1.896, 2.157]	**2.119 > 1**
Statehiste (3500BCE – 1500CE)	1.547*** (0.400) [0.582]	[1.547, 1.855]	**1.695 > 1**
Statehiste (3500BCE – 2000CE)	1.502*** (0.424) [0.575]	[1.502, 1.560]	**1.764 > 1**
Statehiste_aa (3500BCE – 2000CE)	1.444*** (0.487) [0.565]	[1.444, 2.124]	**4.283 > 1**
Geographic controls	Yes	Yes	
Region dummies	Yes	Yes	

Notes: This table reports the results of the coefficient stability test developed by Oster (2019). For ease of comparison, column (1) replicates the main results, including the estimated coefficients and standard errors of *Statehiste*, and *R*-squared values. The delta statistic in column (3) corresponds to the degree of selection on unobserved confounders relative to that on observed confounders. In column (2), I report Oster's identified set, bounded by the baseline coefficients ($\hat{\beta}$) and the treatment effects adjusted for possible omitted variables bias (β^*). They are constructed based on two restrictive assumptions, following Oster (2019). First, the delta statistic (δ) equals one, suggesting that the degree of selection on unobservables is proportional to that on observables. Second, the *R*-squared of a hypothetical regression (R_{max}) is assumed to be 30% larger than that of the baseline regression with full controls (*R*). *Statehiste_aa* is an ancestry-adjusted measure of statehood experience. *** p<0.01.

Table 3. State history and political instability, IV estimates

	(1)	(2)	(3)	(4)
Periods of statehood	3500BCE – 1CE	3500BCE – 1500CE	3500BCE – 2000CE	3500BCE – 2000CE (Ancestry-adjusted)
Panel A. First-stage estimates. Dependent variables is Statehiste				
Agyears	**0.043***	**0.040***	**0.038***	**0.033***
	[0.007]	[0.008]	[0.008]	[0.008]
Panel B. Second-stage estimates. Dependent variable is political instability				
Statehiste	**3.552***	**3.826***	**4.010***	**4.601***
	[0.780]	[1.001]	[1.114]	[1.363]
Panel C. Additional information				
Geographic controls	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes
RESET	0.998	0.334	0.329	0.209
Normality	0.195	0.713	0.439	0.518
First-stage F-statistic	38.66	25.55	22.08	17.81
Anderson-Rubin CI	[2.24, 5.33]	[2.14, 6.30]	[2.36, 6.99]	[2.58, 8.51]
Observations	106	106	106	105
R-squared	0.546	0.485	0.461	0.405

Notes: This table presents IV estimates of the effects of state history on political instability. *Agyears* is the length of time elapsed since the transition to sedentary agriculture. The effective first-stage *F*-statistic of excluded instruments is suggestive of the relevance of the instrument (Olea & Pflueger, 2013). Following Andrews *et al.* (2019), I report identification-robust Anderson-Rubin confidence intervals, which are efficient even when *Agyears* is weakly correlated with *Statehiste*. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Controlling for potentially mediating variables

Periods of statehood	3500BCE – 2000CE				3500BCE – 2000CE (Ancestry-adjusted)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. First-stage estimates. Dependent variable is Statehiste								
Agyears	**0.038*** **	**0.038*** **	**0.038*** **	**0.040*** **	**0.031*** **	**0.034*** **	**0.033*** **	**0.034*** **
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]
Panel B. Second-stage estimates. Dependent variable is PIS								
Statehiste	**4.216*** **	**2.898*** **	**3.951*** **	**3.057*** **	**5.088*** **	**3.252*** **	**4.601*** **	**3.566*** **
	[1.138]	[0.973]	[1.105]	[0.884]	[1.517]	[1.135]	[1.378]	[1.107]
Lgdppc	0.060			0.030	0.060			0.036
	[0.041]			[0.036]	[0.046]			[0.039]
Institutions		-0.503***		-0.515***		-0.555***		-0.565***
		[0.090]		[0.091]		[0.090]		[0.100]
Redist			0.114	1.536			-0.775	1.032
			[1.040]	[1.051]			[1.158]	[1.038]
Panel C. OLS estimates. Dependent variable is PIS								
Statehiste	**1.415*** **	**1.261*** **	**1.544*** **	**1.270*** **	**1.419** **	**1.427*** **	**1.538*** **	**1.554*** **
	[0.441]	[0.357]	[0.492]	[0.459]	[0.547]	[0.404]	[0.565]	[0.517]
Lgdppc	0.012			0.010	0.011			0.016
	[0.041]			[0.038]	[0.043]			[0.038]
Institutions		-0.528***		-0.515***		-0.541***		-0.524***
		[0.091]		[0.093]		[0.094]		[0.096]
Redist			-0.690	0.933			-0.991	0.709
			[0.878]	[0.912]			[0.920]	[0.917]
Panel D. Additional information								
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic	23.17	20.55	22.79	24.43	15.41	18.14	17.56	17.88
Anderson-Rubin CI	[2.53, 7.26]	[1.26, 5.30]	[2.31, 6.91]	[1.57, 5.25]	[2.84, 9.74]	[1.57, 6.29]	[2.56, 8.56]	[1.92, 6.52]
Observations	102	108	105	98	100	106	104	97

Notes: This table replicates the baseline estimates by controlling for potential mechanisms underlying the relationship between *Statehiste* and *PIS*, including the log of income per capita (*Lgdppc*), institutional quality (*Institutions*) and income redistribution (*Redist*). A mediation analysis is provided in Table 10 (Section 7). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Robustness to controlling for population diversity

Periods of statehood	(1) 3500BCE – 2000CE	(2) 3500BCE – 2000CE (Ancestry-adjusted)
Panel A. First-stage estimates. Dependent variable is Statehiste		
Agyears	**0.028*** **	**0.028*** **
	[0.007]	**[0.007]**
Panel B. Second-stage estimates. Dependent variable is PIS		
Statehiste	**4.889*** **	**4.897*** **
	[1.711]	[1.728]
Panel C. OLS estimates. Dependent variable is PIS		
Statehiste	**1.150** **	**1.031** **
	[0.515]	[0.501]
Panel D. Additional information		
Predicted genetic diversity	Yes	Yes
Ethnic fractionalization	Yes	Yes
Ethnolinguistic polarization	Yes	Yes
Geographic controls	Yes	Yes
Region dummies	Yes	Yes
First-stage F-statistic	17.15	16.13
Anderson-Rubin CI	[2.35, 9.80]	[2.33, 9.86]
Observations	108	106

Notes: This table replicates the baseline estimates by controlling for three measures of population diversity, including predicted genetic diversity, ethnic fractionalization and ethnolinguistic polarization. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Robustness to controlling for other factors

Including additional controls	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. First-stage estimates. Dependent variable is Statehiste								
Agyears	**0.036*** **	**0.036*** **	**0.038*** **	**0.038*** **	**0.027*** **	**0.037*** **	**0.038*** **	**0.029*** **
	[0.008]	[0.009]	[0.008]	[0.008]	[0.007]	[0.008]	[0.009]	[0.007]
Panel B. Second-stage estimates. Dependent variable is PIS								
Statehiste (3500BCE – 2000CE)	**3.782*** **	**3.134** **	**3.627*** **	**3.900*** **	**4.567** **	**4.160*** **	**4.071*** **	**5.108*** **
	[1.233]	[1.316]	[1.056]	[1.112]	[1.818]	[1.187]	[1.177]	[1.561]
Panel C. OLS estimates. Dependent variable is PIS								
Statehiste (3500BCE – 2000CE)	**1.360*** **	**1.056** **	**1.468*** **	**1.409*** **	**1.023** **	**1.431*** **	**1.580*** **	**1.483*** **
	[0.459]	[0.487]	[0.493]	[0.452]	[0.476]	[0.466]	[0.435]	[0.414]
Panel D. Additional information								
Inequality	Yes							
Trust		Yes						
Resource wealth			Yes					
Democracy				Yes				
Population					Yes			
Trade openness						Yes		
Urbanization							Yes	
Religions								Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic	18.19	15.20	19.51	22.41	15.18	22.41	15.50	18.97
Anderson-Rubin CI	[1.95, 7.08]	[1.18, 6.65]	[1.85, 6.45]	[2.25, 6.87]	[1.87, 10.14]	[2.40, 7.33]	[2.32, 7.45]	[2.79, 9.28]
Observations	106	57	107	106	106	108	90	108

Notes: This table replicates the baseline estimates by controlling for numerous causes of political instability. Additional controls include the Gini coefficient of inequality of post-tax post-transfer household income (*Inequality*) and social capital (*Trust*), as shown in columns 1 and 2. *Resource wealth* denotes four variables of resource endowments, including oil, gas, mineral and forest rents (as a proportion of GDP). I incorporate the Polity2 index of democratic institutions (*Democracy*), population density and the size of population (*Population*), trade openness, and urban population as a proportion of total population (*Urbanization*) in columns (4) to (7). Moreover, I control for three variables, capturing the proportions of the total population practicing major *religions* such as Catholics, Muslims and Protestants (column 8). As reported in the online Appendix, the results remain broadly unchanged when using an ancestry-adjusted index of statehood experience (3500BCE – 2000CE). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. A test of over-identifying restrictions

Periods of statehood	3500BCE – 2000CE		3500BCE – 2000CE (Ancestry-adjusted)	
	(1)	(2)	(3)	(4)
Panel A. First-stage estimates. Dependent variable is Statehiste				
Proximity	**0.290*** **	**0.183** **	**0.262*** **	**0.156** **
	[0.073]	[0.073]	[0.070]	[0.067]
Agyears		**0.027*** **		**0.024*** **
		[0.009]		[0.008]
Panel B. Second-stage estimates. Dependent variable is PIS				
Statehiste	**2.969*** **	**3.697*** **	**3.139*** **	**4.208*** **
	[0.818]	[0.861]	[0.989]	[1.028]
Panel C. Additional information				
Geographic controls	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes
RESET	0.252	0.538	0.159	0.057
Normality	0.291	0.352	0.264	0.517
First-stage F-statistic	15.65	15.38	13.97	12.43
Anderson-Rubin CI	[1.59, 5.15]	[1.93, 6.31]	[1.47, 5.98]	[2.28, 7.77]
Over-ID		**0.574**		**0.589**
Observations	109	106	107	105
R-squared	0.529	0.489	0.509	0.444

Notes: This table replicates the IV estimates by using an alternative excluded instrument. *Proximity* is geographic proximity to regional frontiers in 1000BCE, constructed by Ang (2015). *Over-ID* denotes *p*-values of Hansen's J-test of over-identifying restrictions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8. State history and the incidence of unrest in repeated cross-country data

Periods of statehood	3500BCE – 2000CE		3500BCE – 2000CE (Ancestry-adjusted)	
	(1)	(2)	(4)	(5)
Estimator	Probit	IV Probit	Probit	IV Probit
Panel A. OLS and IV second-stage estimates				
Statehiste	**0.830*** **	**2.486*** **	**0.643*** **	**2.949*** **
	[0.183]	[0.439]	[0.199]	[0.521]
Geographic controls	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Lagged incidence of unrest	Yes	Yes	Yes	Yes
Observations	4,500	4,409	4,436	4,377
# of countries	107	104	105	103
Pseudo R-squared	0.192		0.188	
Average marginal effect of *Statehiste*	0.239	0.151	0.187	0.081
Panel B. IV first-stage estimates. Dependent variable is Statehiste				
Agyears		**0.035*** **		**0.029*** **
		[0.001]		[0.001]
First-stage F-statistic		967.04		768.21

Notes: This table reports Probit and IV Probit estimates of the effects of state history on the incidence of socio-political unrest from 1960 to 2010, using repeated cross-country data. The dependent variable is a dichotomous measure of the occurrence of riots and revolts, constructed by Acemoglu *et al.* (2019). The full model specification is expressed in Eq. [5]. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9. The heterogeneous effects of state history on political instability

	(1)	(2)	(3)	(4)
Panel A. First-stage estimates. Dependent variables are different dimensions of state experience				
Agyears	**0.444***** [0.056]	**-0.021***** [0.007]	**-0.042***** [0.008]	
Panel B. Second-stage estimates. Dependent variable is PIS				
State age	**0.347***** [0.074]			
Average state autonomy ($z^{autonomy}$)		**-7.445***** [2.453]		
Average state coverage ($z^{coverage}$)			**-3.622***** [0.940]	
Panel C. OLS estimates. Dependent variable is PIS				
State age	**0.224***** [0.054]			**0.259***** [0.068]
Average state autonomy ($z^{autonomy}$)		**-0.979*** [0.536]		**-0.319** [0.475]
Average state coverage ($z^{coverage}$)			**-0.824**** [0.407]	**0.405** [0.423]
Panel D. Additional information				
Geographic controls	Yes	Yes	Yes	Yes
Continent dummies	Yes	Yes	Yes	Yes
First-stage *F*-statistic	62.01	7.91	30.09	
Anderson-Rubin CI	[0.21, 0.50]	[-18.86, -4.29]	[-5.95, -2.04]	
Observations	109	109	109	109

Notes: This table explores the contribution of different dimensions of statehood experience to the persistence of political instability. *State age* is the length of time elapsed since the first statehood was recorded (measured in millennia). *Average* $z^{autonomy}$ corresponds to the degree to which the rule was internally based. *Average* $z^{coverage}$ captures the state's territorial unity. As noted by Borcan *et al.* (2018), $z^{autonomy}$ and $z^{coverage}$ are constructed conditional on the presence of a state above the tribal level ($z^{presence}>0$). Section 3 contains more details. Three components of statehood are not included altogether in the IV regression because the model is exactly identified (column 4). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. State history and political instability, ACDE estimates

Periods of statehood	3500BCE – 2000CE			3500BCE – 2000CE (Ancestry-adjusted)		
	(1)	(2)	(3)	(4)	(5)	(6)
Potentially mediating variables	*Lgdppc*	*Institutions*	*Redist*	*Lgdppc*	*Institutions*	*Redist*
Panel A. OLS regressions. Dependent variable is PIS						
Statehiste	**1.406****	**1.167****	**1.466****	**1.460****	**1.492*****	**1.449****
[95% bootstrapped CI]	[0.31, 2.51]	[0.24, 2.10]	[0.37, 2.56]	[0.11, 2.79]	[0.42, 2.56]	[0.15, 2.75]
Panel B. IV regressions. Dependent variable is PIS						
Statehiste	**4.178***	**3.039***	**4.345****	**5.133**	**3.599**	**5.321**
[95% bootstrapped CI]	[-0.08, 8.44]	[-0.05, 6.13]	[0.05, 8.64]	[-36.42, 46.69]	[-9.07, 16.27]	[-34.96, 45.60]
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the averaged controlled direct effects (ACDE) of *Statehiste* on *PIS* once accounting for the effects of potentially mediating variables. The results are estimated following a two-step regression procedure proposed by Acharya *et al.* (2016), and the main text contains a more detailed discussion. Following Acharya *et al.* (2016), consistent estimates are obtained through a bootstrapping procedure with 1000 replications. *** p<0.01, ** p<0.05, * p<0.1.