Of Kings and Criminals: Essays on Elite Violence and Economic Development

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A. Introduction

A.1. Violence and Development

Although undoubtedly linked to developmental themes such as institutional quality, geography, inequality and efficiency, the absence of empirical research into the causes and consequences of violence in the larger development debate is glaring (McIlwaine 1999; Enamorado et al. 2014). Economics and economic history have tended to research the impacts of war on development, with divergent conclusions, or else the opportunity costs of overall criminal activity (both violent and non-violent) for economic growth, rather than the impacts of interpersonal violence. Rogers (1989) comments on the "*assumed link* between violence, crime and societal development" while Stone (2006) refers to "limited knowledge in the field". Likewise, Enamorado et al. (2014) explains how "scholars have often argued that crime deters growth, but the empirical literature assessing such [an] effect is scarce".

In the fields of psychology and anthropology, violence has arguably been researched more thoroughly, but the focus of these studies has tended to investigate evolutionary[1] and

[1] Buss and Shackelford (1997) describe violence as a learnt, evolutionary response used to solving seven human societal problems: "co-opting the resources of others, defending against attack, inflicting costs on same-sex rivals, negotiating status and power hierarchies, deterring rivals from future aggression, deterring mates from sexual infidelity, and reducing resources expended on genetically unrelated children". Evolutionary theories of violence have, however, been criticised as fatalist and that they preclude the role of free will in committing violent acts.

environmental[2] drivers of violence rather than its role in development (Accomazzo 2012; Feshbach 1990; Goetz 2010).

Literature of economics and crime mostly stems from studies that investigate violent crime as a sub-question of their research topic, though these studies do often find negative development impacts (Rogers 1989; McIlwaine 1999). For example, Burnham et al. (2004) investigated the influence of certain criminal activities on suburban income growth in the US from 1982 to 1997, finding that only violent crime had any significant adverse impact. Further, they detected a negative relationship between the strength of this crime-income effect and the distance from urban centres, calculating a cut-off point of between 53km and 68km, after which the relationship was no longer negative[3]. This is not to say that only violent crime in urban areas has a detrimental effect on income growth, but merely that violent urban crime has a wide-reaching sphere of influence. Another example is Bourguignon's (2000) global study which estimates the social cost of crime from 1985 to 1995. He estimates that homicides cause economic inefficiencies that result in foregone GDP growth of up to 2 percentage points per year.

Traditional economics literature of war and development often focuses on the detrimental effects that war has on specific groups. For example, Goldson (1996) and Ibáñez and Moya (2006) study the adverse impacts on displaced children globally throughout the 20th century. Correspondingly, Rosenheck et al. (1994) find that US military veterans have been more likely than the general population to become homeless and unemployed since the war in Vietnam, perhaps due to post-traumatic stress disorder (PTSD) and to difficulties in finding employment. On economies in a broader sense, Smith (1977) found a negative impact for

[2] Latessa and Lowenkamp (2006) suggest that "low-risk" inmates often leave prisons as more-skilled criminals after learning in suboptimal prison environments.

[3] Calculated from table 3's regression results in Burnham et al. (2004).

military expenditure on growth in developed countries, while Deger and Smith (1983) made the same conclusion for developing countries. The UN Committee for Development Planning also stated that “the single and most massive obstacle to development is the worldwide expenditure on national defence activity” (Deger and Smith 1983).

Additionally, Pinker (2011) showed that the frequency and intensity of all forms of violence have declined over the long run, including war. Pinker proposed that increased human capital allowed for alternatives to violence when resolving disputes and that increased cooperation in trade and commerce made “other people become more valuable alive than dead” (Pinker 2011).

However, Charles Tilly’s (1975) hypothesis also argues that violence in the form of international conflict has been development promoting throughout human history. According to Tilly, small social groups expanded in order to consolidate their military capabilities and improve security, also increasing the scope of economic activities conducted by the group. This wider range of economic activities then allowed for occupational specialisation and promoted economic development through centralisation, eventually leading to fully-functioning tax systems. In contrast, Broadberry and Harrison (2005), argued that this war and development relationship is endogenous; that war and development are chiefly related because wealthier countries are able to mobilise greater resources and are therefore more likely to win wars.

A recent branch of literature has revisited the Tilly hypothesis (Dincecco 2015; O’Brien 2011; Hoffman 2015), focusing on the association between war and tax capacity and arguing that societies at war were more willing to accept higher tax rates, resulting in centralised development. Authors such as Kaempffert (1941) and Kellner (1999) argue that this effect took place through spillovers from military innovation to technology that could be used in manufacturing and industry. However, even the role of the state in violence is debated, with studies like Acemoglu et al. (2005) maintaining that strong states pose obstacles to development and that institutions which allowed merchant classes to restrict the power of monarchies – encouraging technological innovation through stable property rights – were key both to curbing conflict and encouraging development.

In sum, links between violence and development have largely been indirect; tending to focus on military conflict and military spending, violent crime as a sub-question to overall criminal activity, or the determinants of crime from psychological and anthropometric backgrounds. One of the reasons for the lack of direct violence research in development contexts is simply the lack of data, especially over the long run. Data is often scarce and unreliable due to underreporting because of the illegal nature of violent activity, and because victims fear further violence if they do report their stories (Mauro and Carmeci 2007). Additionally, long-run homicide series for European countries, which would have facilitated long-run development studies, have only reached back as far as the 19th century until recently. Data from other world regions usually have an even more limited historical reach and are even less convenient for long-run analysis. As such, the lack of violence studies in the development literature may reflect a problem of access rather than relevance.

However, recent advances have been made. First Gurr (1981, for England) and then Eisner (2014, for selected countries across Europe) collected city-level homicide data and then recreated national homicide series for certain European countries, stretching all the way back to the 13th century. Additionally, Eisner (2011) and Cummins (2017) have examined trends in interpersonal elite violence using regicide[4] and nobilicide,[5] respectively, since the Middle Ages.

One of the main contributions of this dissertation is to build on their ideas and introduce a wide-ranging indicator for interpersonal elite violence through which long-run violence

[4] The killing of kings and other rulers.
[5] The killing of noblemen.

research can be conducted. Using the relative dearth of research into long-run violent activity as motivation, this dissertation also aims to contribute towards a number of development-related debates by investigating the role of violence.

A.2. Outline of the Dissertation

This dissertation consists of three distinct chapters that are referred to as studies or papers, following a cumulative thesis approach. Together, they contribute to our understanding of the role that violence has played since the 6th century and of the consequences that it continues to have today.

The first two papers, chapters B and C, concern interpersonal elite violence and employ the regicide indicator to investigate the interplay between it, elite human capital and state capacity between the 6th and 19th centuries. Although chapter C motivates and introduces the regicide indicator, it is positioned after chapter B. This is done because these papers were written simultaneously and chapter B received more attention at conferences, providing motivation to more clearly explain the concepts which overlap the two papers in chapter B. The reader is advised to accommodate the regicide indicator as a valid measure of interpersonal elite violence in chapter B, before putting it to scrutiny in chapter C. Chapter D then moves the analysis of elite violence from this deeply historical period towards the present, analysing the global financial market impacts of assassinations since 1970.

Since the role of violence in economic development has been largely overlooked in favour of factors such as institutions and geography, I, along with Jörg Baten,[6] test for a causal relationship between our new regicide indicator and elite human capital. Human capital is an essential ingredient for economic growth as it drives innovation and technological

[6] Jörg Baten co-authored chapters B and C, contributing approximately 20% of the work to each paper.

development, at least in conjunction with inclusive institutions (see Becker 1962; Mincer 1984; Acemoglu and Dell 2010; and Barro 2001). Therefore, we develop a new proxy for elite human capital and use our regicide indicator in an instrumental variable setting to determine a causal effect of elite violence on elite human capital formation. Since much of the literature on economic growth and development focuses on institutions, geography and natural resources rather than violence, the causal inverse relationship that we derive is an important result. By comparing Eastern and Western Europe, we also find evidence that the Great Divergence – the developmental leap that separated Western Europe from the rest of the world – at least partially had its roots in violence as far back as the 14th century.

In chapter C, also with Jörg Baten, the regicide indicator is formally introduced and motivated by comparing it to alternative measurements of interpersonal violence and elite violence, as well as to the historical narratives of European history between the 6th and 19th centuries. After establishing the indicator, the long-run role of the state in European violence is investigated. The role of the state in shaping violence has been the subject of conflicting hypotheses in the economics literature, with researchers either arguing that states promoted violent activity and used military conflict to develop, or that they helped to restrain violent activity (Pinker 2011; Broadberry and Harrison 2005; Tilly 1975; Dincecco 2015). We find a negative relationship between territorial state capacity and interpersonal elite violence, which begins in the 10th century. This result is interpreted as states having had a largely pacifying role on trends and regional differences in interpersonal elite violence, at least since the High Medieval Period, although the relationship is not necessarily causal.

From the assassinations of kings and other rulers, the third study moves the dissertation to a more modern application of elite violence, the assassinations of modern politicians. Specifically, the chapter considers how murdered politicians have signalled shocks to political risk and how financial markets have reacted in terms of asset allocation since 1970. How investors allocate their holdings influences the levels of financing available to firms or national treasuries, affecting the ability of firms to develop their industries or the ability of governments to provide public services and direct fiscal policy.

Ordinarily, in accordance with modern portfolio theory, investors should reallocate their holdings from risky assets toward the risk-free rate in periods of heightened risk (Markowitz 1952). Generally, much of this reallocation should be a substitution from equity to bonds, and sovereign bonds in particular, since they are underwritten by entire governments instead of banks or smaller financial institutions. Accordingly, this chapter investigates the rationality of investor reactions to political shocks when the government is both the source of the risk and the traditional investment safe haven. Additionally, the paper differentiates between developed and developing states and finds somewhat disparate effects and responses times. Finally, a fifth chapter provides concluding remarks from all three studies as well as an outlook on potential future research.

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B. Elite Violence and Elite Numeracy in Europe from 500 – 1900 CE: Roots of the Divergence.[7]

Abstract

We present new evidence of elite numeracy in Europe since the 6th century CE. During the early medieval period, Western Europe had no advantage over the east, but the development of relative violence levels changed this. After implementing an instrumental variable strategy and a battery of robustness tests, we find a substantial relationship between elite numeracy and elite violence, and conclude that violence had a detrimental impact on human capital formation. For example, the disparities in violence between Western and Eastern or South-Eastern Europe, helped to shape the famous divergence movement.

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[7] Co-authored by Jörg Baten. He contributed approximately 20% of the work to this paper.

B.1. Introduction

In this study, we assess the joint evolution of elite violence and elite numeracy across Europe over 1400 years (including Asia Minor and the Caucasus). New evidence on elite numeracy is presented for the first time and the relationship between elite violence and elite numeracy is examined. The study uses a variety of econometric techniques, from panel regressions to spatial methods, first difference regressions to instrumental variable estimation. We find that declines in violence determined the growth of elite numeracy in certain European countries since the medieval period, such as England and the Netherlands. Similarly, higher levels of elite violence corresponded to lower elite numeracy in Eastern and South-Eastern European countries, for example, leading to Europe's famous divergence movement (van Zanden 2009, 2016, Broadberry 2013). Since war and elite violence might be correlated in early periods, our findings also stimulate the theoretical debate that "war generates states" (and state capacity to tax in particular; Tilly et al. 1975), by providing contrasting results to this widely accepted view. We discuss this question in the following parts of the study.

Additionally, we contribute to a modestly sized but growing literature on elite numeracy. To demonstrate that the upper tail of the knowledge distribution mattered for growth, Squicciarini and Voigtländer (2015) use the example of the industrial revolution in France. Inspired engineers and bold entrepreneurs were able to establish firms using recently developed technologies, and to develop various technologies further. Baten and van Zanden (2008) studied advanced human capital using book consumption, and drew parallels with the 16th century when several European countries managed to set up growth-promoting institutions due to human capital. This resulted in a system of trading cities and merchants who coordinated world trade as far back as the 16th century. In this study, given the clear relevance of human capital, we take an additional step and uncover the medieval roots of the divergence of elite numeracy in Europe.

Our approach allows us to resolve crucial questions in European history, such as why elite numeracy advanced or declined in certain regions and periods rather than others, and why that process took place at disparate rates. For example, there was a strong increase in elite numeracy in Italy and Iberia during the late medieval and renaissance periods, while it stagnated in South-Eastern Europe at the same time. Before this period, the European east – which included Constantinople as well as certain less densely populated regions – had an elite numeracy level at least equal to that of Western Europe.

The debate around explanations for the Great Divergence, which saw Western Europe become the world's chief economic force during the modern era, has produced advocates for geography, institutional design, gender equality, human capital and a host of other explanatory factors as key elements of Western Europe's ascent (Bosker et al. 2013; Allen 2001; Diebolt, Le Chapelain and Menard 2017; Diebolt and Perrin 2013 and Broadberry 2013). In this study, we suggest that the role of violence has been under-researched and largely neglected (aside from certain contributions: Cummins 2017; Findlay and O'Rourke 2009). Therefore, we explore the co-evolution of non-violent behaviour and human capital among elites and conclude that violence played a significant role in shaping economic development through human capital formation.

Our strategy for approaching this question relies on proxy indicators, as standard indicators of violence and human capital are not available for early periods of European history. Hence, we establish a new indicator that is able to trace the development of elite numeracy over the very long term – the share of rulers for whom a birth year is reported in conventional biographical sources. We reason that a ruler's birth year was regularly reported and entered into historical chronologies only if elite bureaucracies around the ruler were capable of processing numerical information with ease; otherwise, it was simply forgotten and left unrecorded. Below, we discuss a number of potential biases and reason that they do not invalidate our proxy indicator for elite numeracy. We also report correlations with other indicators of elite numeracy in medieval societies for which both metrics were simultaneously available in the same location.

As a proxy indicator for interpersonal violence among the elite, we use the share of murdered rulers. If killed, rulers were typically murdered by their own family members or by competing nobility (see chapter C). The kingdom's elite was also affected by the fear of becoming victim to violent death themselves if the ruler was killed – murder, particularly of a central figure, creates an atmosphere of fear in society. This external effect of violence is even supported by 20th century evidence from psychology (OECD 2011, Baten et al. 2014). We have also studied to what degree regicide is correlated with nobilicide, the killing of the nobility, as Cummins (2017) provides valuable data on this (for nobility killed in military conflicts). The correlation is very close, indicating that regicide may also serve as a proxy indicator for the wider elite (see appendix, figure A.B.1.).

Clearly, violence was not the only factor that mattered for elite numeracy. Hence, we also include religion, geography, institutional factors such as serfdom and early electoral elements of ruler succession, as well as other potential determinants.

Our work is also clearly related to the "war generates states" hypothesis, going back to Tilly et al. (1975). While many influential studies traditionally focused on the strong state as an obstacle to development (Acemoglu et al. 2005), a recent strand of the literature picked up the Tilly et al. hypothesis, arguing that the experiences of war and conflict allowed tax capacities to develop – most notably during the Hundred Years' War in France, which stimulated innovations in tax collection and financed standing armies (North 2000, Hoffman 2012). A wider set of related studies focused on war as the basis of a state's capacity to tax (see, for example, Dincecco 2015, O'Brien 2011, Hoffman 2015). In contrast, our study finds that elite violence was rather a development hurdle during the medieval and early modern periods. How can these seemingly contrasting views be reconciled? Can we gain additional theoretical insights from this incongruity? We agree that state capacity had positive effects, in general, as Dincecco and Katz (2014) have shown. However, three facts were crucial: firstly, wars might have been the trigger rather than the underlying reason for developing tax capabilities. The famous example of France's development of tax capacity during the Hundred Years' War first took place in a country that had already developed low elite violence and high elite numeracy in earlier periods, as we show below, preparing a more serviceable environment for state capacity. The trigger of the devastating war with England convinced the French nobility that permanent taxation would be necessary, but this would not have been possible in another setting with a similarly devastating war, in Bulgaria during the 13th and 14th centuries for example. Secondly, tax-financed military expenditure also increased the defensive abilities of states and they became able to avoid military conflicts on their own soil. For example, Britain did not experience many invasions after 1066 and most of its interstate conflicts were executed on foreign territory. France had many military conflicts on German soil and in other countries between the Hundred Years' War, ending in the 15th century, and the late 19th century. The Netherlands mostly initiated maritime wars after building the capacity to tax during the 16th century. Hence, the general population of these states with high tax capacities arguably did not suffer as much from war, nor did the local elites. Whether this was in fact the case is an empirical question that we will study in the following. Thirdly, the changes in military technology that took place during the early modern period required tax capacity – emphasizing gunpowder and the "*trace italienne*" style of city fortification – but they also protected both the general population and elites better than characteristics of the medieval style of warfare ever did (Gennaioli and Voth 2015).

In order to study the relationship between elite violence and elite numeracy, we use a battery of econometric techniques. Since endogeneity, spatial autocorrelation or temporal autocorrelation may affect our estimates, we use two-stage least squares, controls for spatial autocorrelation, unit root tests, time fixed effects and first difference estimates.

B.2. Measuring Elite Numeracy

Our indicator for elite numeracy is the share of known birth years among all rulers residing in the capitals of their principalities. We organise these data by century (and two-century periods for our graphs) based on the end of each ruler's reign. We propose that for the birth year of a ruler to be entered into a kingdom's historical records, a certain level of numerical sophistication is required among the ruling elite. This evidence does not necessarily estimate the numerical ability of the rulers themselves but rather that of the government and bureaucratic elite around them and, by implication, the elites of the polity in general. This indicator shares similarities with A'Hearn et al.'s (2009) ABCC Index, which uses the prevalence of age heaping to estimate numerical proficiency – age heaping being the phenomenon of less numerate individuals rounding their ages when they are unable to report them accurately. Admittedly, one could imagine a situation in which political elites were highly numerate but economic elites were not. However, these social groups were usually highly connected (Mokyr 2005).

As more traditional indicators of education such as literacy rates, school enrolment, or age heaping-based numeracy are not available for most medieval European countries, the 'known ruler birth year' proxy allows us to trace elite numeracy in periods and world regions for which no other indicators currently exist.

We assess the validity of this measurement by using insights from alternative sources, only including cases where information for at least ten rulers is available. Most notably, Buringh and van Zanden (2009) traced elite European education through the number of monastery manuscripts that were kept between 700 and 1500 CE, using them to construct a per capita indicator. In figure B.1., we document the substantial correlation between their proxy measure of elite numeracy and ours for eleven European countries. Although there is naturally a certain amount of variation resulting in some observations deviating from the trend line, the correlation remains highly significant (correlation coefficient ρ=0.67).

Likewise, we compare our indicator to the rate of ‘birth year heaping’ in Cummins’ (2017) database of European noblemen from 800 to 1800 CE and again find a highly significant correlation (figure B.2.; here, the correlation coefficient is ρ=-0.58[8]).

Similar comparisons with another indicator can also be made for China. As another large and fairly stable world region it can also provide broadly applicable insights into long-run development processes. An early indicator of numeracy and human capital used for China concerns the number of “literati” among the population.

During certain phases of Chinese history, most notably after nomadic invasions, the literati system was of reduced importance. These periods were also characterised by lower elite numeracy rates; as measured by the known ruler birth year proxy and seen in figure B.3.[9] In sum, the Chinese evidence allows us to complement our comparisons of European monastery manuscripts and ‘birth year heaping’ with elite human capital in another world region.

To estimate elite numeracy via the known birth year rate for medieval Europe, we had to make certain methodological decisions. For practical reasons, we assign modern country names to the geographic units we study, using the location of historical capitals within modern boundaries as our assignment criterion – as the kingdom’s elite mostly lived in these capitals. A large number of studies in economic history have used modern countries as their cross-sectional units of analysis because this approach allows the tracing of long-run determinants, even if it invites a certain degree of measurement error. For example, Maddison (1998) traced post-Soviet economic growth and populations in former Soviet states back into Soviet times. The Clio-Infra database also allows us to study historical country units using their modern boundaries. If boundaries change, then using modern countries may seem somewhat anachronistic, but the insights gained by analysing the long-term development of these

[8] The relationship is negative because heaping measures *innumeracy*.

[9] Our literati data come from Deng (1993), where the literati indicator is the per capita literati membership rate, and exam frequency is measured by the number of exam sittings held per decade.

territorial units still provide valuable insights. Nevertheless, for most European countries, such as France, the UK and Spain, modern country borders are broadly compatible with historical boundaries.

If there were concurrent rulers within the borders of modern countries (in smaller principalities, for example), we also assigned them to a modern country according to where their capital was located.[10] The alternative, assigning elite numeracy values to grid cells across Europe, also leads to measurement error because we do not have measurements for all grid cells, only for those containing each capital city. Thus, we cannot measure any difference between grid cells containing capitals and those without. In fact, we could more precisely call our unit of observation the average elite numeracy of each capital situated in the territory of each modern country. For simplicity, we abbreviate this with the name of each modern country. The main explanatory variables that we assess below also relate to the same modern geographical units described here.

B.3. Potential Biases of the "Known Birth Year" Indicator

It is conceivable that the 'known birth year' indicator may suffer from potential biases that capture information unrelated to elite numeracy. We discuss these biases below and consider whether or not they are substantial.

1. Ruler biographies, for example, were often only recorded many years after a ruler's death, and the exact sources on which these were based are often unknown. Therefore, factors such as strong research traditions may have contributed to more detailed and complete chronologies of ruler birth years – with chronologists perhaps even calculating them based on significant events that occurred closer to the birth of an earlier ruler. Specifically, countries with

[10] Additionally, several smaller principalities within a modern country frequently allow us to reach our lower-bound constraint of 10 rulers per country and century (though this lower bound is chosen somewhat arbitrarily, our results are not sensitive to it; see table A.B.10.).

strong university traditions such as England, France or Germany, might have boasted scholars who created detailed accounts of the medieval histories of their countries, leading to more accurate approximations of birth years that took place centuries later. However, somewhat surprisingly, many of these countries actually had lower known birth year rates in the Middle Ages than, for example, the regions in today's Iraq, Turkey or Greece did (see below and in Baten 2018). Consequently, this notion is incompatible with the view that the research intensity of the last few centuries might have biased the elite numeracy estimates of medieval times.

2. A second potential source of bias is the destruction of city archives, which might have resulted in the loss of previous records. However, royal chronologies were traditionally copied (Hanawalt and Reyerson 2004: 39). Even if one city archive were destroyed, prominent information such as that concerning a ruler would likely have been preserved in other libraries, books and supplementary written media. Moreover, we observe that the proportion of known ruler births often declined over time (figure B.18.). If the destruction of city archives were a core determinant of this indicator, we would have expected near zero values for the earlier centuries, which would suddenly reach 100% in later centuries. This does not occur in any of our series. Clearly, we should not assume a linear loss, but if some loss occurred due to the destruction of archives, one would expect some downward bias for known birth years to have occurred. However, we argue that since ruler lists were considered highly important pieces of information, they were usually kept by different people in different places and were therefore not lost after the destruction of one or even several city archives. Victorious invaders were also not necessarily interested in burning all written records, because keeping information about their newly conquered territories was vital. Hence, the burning of city archives was usually isolated and accidental. Even during the famously brutal Tamerlan invasions, not all cities and their archives were destroyed, because certain cities surrendered. Gaining power over cities and territories was Tamerlan's main aim, not destroying them, though destruction did occur in several cases to generate terror (Kunt and Woodhead 1995: 857).

3. Third, and more relevantly for South-Eastern Europe, rulers who assumed the throne after an invasion might have been different from rulers born in the countries that they later ruled. For example, some rulers originated from less numerate, nomadic societies in Central Asia – such as the first of the early Bulgarian rulers. Here, we have to distinguish between a truly lower level of elite numeracy among these rulers and their elites, what we want to measure, and a bias that stems from a lack of information about their births in foreign and possibly distant lands. Being born elsewhere might imply less knowledge about the first generation of settlers, but the second generation should have already undergone a catch-up period in which to learn and record the second ruler's birth year. Therefore, using a sufficient number of cases per period should mitigate any degree of bias that could potentially lead to concern. One famous example of a new political entity formed after a migration movement was the Bulgarian Empire (on the following, see Shepherd 2002). Originating on the plains of West Asia, the semi-nomadic Bulgars moved to the Balkans in several stages. Asparuh was the first ruler of the Bulgarian Empire after settling north of the Byzantine Empire. No birth year is known for him and it seems plausible that the human capital of his early imperial elite was modest, consistent with the above hypothesis. Contrastingly, his successor, Tervel, reorganised the empire. He cooperated with the Byzantines at first, before conflict later took place. Correspondingly, for him a birth year is known. These are individual examples and, hence, only have limited representativity, but they aptly illustrate the considerations above.

4. A fourth possible bias could be that rulers who spent more time on the throne could have better established themselves and their policies, giving chronologists more reason and more time to document their birth years. We control for this potentially biasing effect by including the length of the ruler's reign as a control variable, finding no relationship with the proportion of known ruler birth years (table B.3.).

5. Finally, and possibly the most challenging potential bias to alleviate, the birth years of more famous rulers might have been better recorded. It is conceivable that events in the lives

of lesser rulers, who were placed under the suzerainty of an emperor, for example, would be less diligently documented. However, birth years for several of the most famous rulers in world history, such as Charlemagne, were not documented; this is a first hint that 'fame bias' may not have been so crucial. Nevertheless, we can also control for this 'fame bias' to a certain extent by controlling for whether the rulers of each kingdom were under the suzerainty of an overlord. Rulers with a more dependent, governor-type function most likely attracted less attention from chronologists.[11] We find, in table B.3., that rulers who served this governor-type function were not significantly different to their overlords in terms of elite numeracy, after controlling for country and century fixed effects. In conclusion, these developments speak against any fame bias under the assumption that fame and suzerainty are related.

Furthermore, we include the area of each kingdom as a second control variable against more famous or powerful rulers being better documented. Although not all powerful rulers held large territories, rulers of powerful kingdoms such as the Holy Roman Empire, the Ottoman Empire, Poland-Lithuania and the Kievan Rus certainly did. Nevertheless, like our indicator for suzerainty, kingdom area does not exhibit any relationship with the proportion of known ruler birth years. Throughout the paper, we compare our regression specifications both with and without these 'elite controls'.

B.4. Measuring Potential Determinants of Elite Violence

Elite violence could potentially be an important determinant of elite numeracy. Cummins (2017) argues that a substantial share of noblemen in the medieval period died through acts of violence, including kings, and particularly on the battlefield. Given that lifespans and the prevalence of violence are negatively correlated – though not perfectly, as

[11] As we use the location of a kingdom's capital in order to link kingdoms to modern countries, some countries might have had multiple rulers simultaneously. Consequently, we use the 'autonomy' indicator variable to distinguish between the decision-making powers of these rulers.

other factors also influence lifespans – we argue that part of the underinvestment in elite human capital during this early period was caused by lower lifespans. Individuals had had fewer incentives to invest in numerical human capital if they expected to die early. While we measure the murders of rulers, external effects on the kingdom's elite are very likely. The wider elite is also affected by the fear of becoming victims to violence if the ruler is killed – murder, particularly of a central figure, creates an atmosphere of fear in society (on recent evidence of the external effects of murder, see OECD 2011, Baten et al. 2014). Moreover, after the repeated killing of rulers – both in battle or in non-battle situations – specific value systems often developed, typically related to "cultures of revenge" (Pust 2019). While most inhabitants of wealthy modern societies consider 'blood revenge' outdated and unimaginable, the contemporaries of the 14th century, for example, considered it imperative. It was closely related to the 'culture of honour', which led aristocrats to die in duels even as late as in the 19th century, attempting to enact revenge for insults or violence against their relatives. The persistence of these cultures of honour has also been studied for the Southern United States (see Nunn 2012).

Elias (1939) described a long-term process in which societies and elites in particular became less violent over time, adopting and accepting greater state capacities and a culture of increasingly civil, non-violent behaviour. He termed this humankind's "civilising process". In societies of high state capacity – or even a widely accepted monopoly of the state to execute violence – returns to investments in education by meritocratic elites were certainly higher. Eisner (2014) argued that the complex interaction between more education and less violence in a society sets a "swords to words" process in motion, in which potential conflicts were increasingly solved through negotiation rather than violence (Gennaioli and Voth 2015; Pinker 2011). Cummins (2017) finds that increasingly fewer European nobility were killed in battles after 1550 CE. Baten and Steckel (2018) also studied the history of interpersonal violence in Europe by tracing the proportion of cranial traumata cases among 4738 skeletons that cover the period 300 to 1900 CE, finding that interpersonal violence remained very high until the late

Middle Ages before rapidly declining. Eisner (2011) also collected evidence on 45 European kingdoms, documenting a decline in the rate of regicide over time – regicide being the assassination of kings and other rulers. If killed, rulers were usually the victims of their own families or competing nobility. The rates of regicide and of rulers killed in battles declined strongly between the early medieval period and the modern era (see chapter C for an econometric analysis with a strongly expanded European sample and figure B.7. on regional regicide rates).

To crosscheck the plausibility of our own evidence of declining violence over time, as well as the relationship between elite and population-wide violence, we compare evidence on regicide and homicide for a number of European countries for which Eisner (2014) presented early evidence of homicide rates. In figure B.4., we can see that both series showed very similar trends across the countries where data are available. Moreover, deviations from the general downward trend also often occurred at similar times (one exception being Italy during the 19th century). This strong relationship also validates our use of regicide as a proxy for interpersonal elite violence, discussed in more depth in chapter C).

Although these subfigures all display strong declines, the panel unit root tests that we run in the appendix (table A.B.2.) lead us to conclude that regicide, over the whole panel, is a stationary process. Nevertheless, we include time fixed effects as a measure against non-stationarity in our empirical analysis. Finally, temporal autocorrelation does not play a strong role because our main results also hold in first differences (see appendix, tables A.B.7 and A.B.8.).

For the Middle East, Baten (2018) adopted a similar strategy by analysing the number of rulers who were killed in battles and by other forms of regicide, mostly due to conflicts over who should rule. Interestingly, we found that Europe tends to display diametrically opposite trends to the Middle East. For a large portion of the period that Baten (2018) studied, both battle deaths and murder rates within the ruling houses increased, whereas they declined in Europe, as we describe in detail below.

For the remainder of this paper, we use regicide as our indicator of elite violence. Our regicide dataset was initially built using the rulers found in Eisner's (2011) original regicide study, comprising 1513 rulers from across 45 kingdoms. We then strongly expanded this dataset with an array of supplementary sources, chiefly Morby's (1989) 'Dynasties of the World' and Bosworth's (1996) 'The New Islamic Dynasties' as well as many other individual biographies and encyclopaedia entries. The expanded dataset consists of 4066 rulers from 92 kingdoms across the period 500–1900 CE and comprises all of Europe (see chapter C for more details).

We differentiate 'battle death' from killing outside of battle. Admittedly, the two variables are not always perfectly distinguishable, but our definition of battle violence is to be killed in a battle.

Finally, our regicide evidence covers all states, for almost all periods. This is not possible for other indicators such as conflict counts. Pinker (2011) studied conflicts over time, arguing that both overall and interpersonal elite violence declined despite the number of conflicts in some countries seeming to increase over time. Accordingly, Pinker criticised simple conflict counts as uninformative due to three different biases. First, the number of casualties per capita needs to be measured accurately, which is not often done. Secondly, the number of conflict victims per capita needs to be quantified, particularly because simple conflict counts are higher in more densely populated countries with larger populations. Thirdly, and perhaps most importantly, psychologists have identified a strong perception bias – we know much more about minor conflicts in Northern France or Germany than, for example, in Ukraine or in the Balkans during the 15th century. Conflicts between neighbouring Ukrainian cities during the late medieval period would probably not have been documented, whereas similar conflicts between two Western German cities, for example, might have indeed been recorded.

B.5. Regional Patterns of Elite Numeracy

When looking at regional trends in elite numeracy (figure B.5.; see table A.B.1. for regional classifications), we see that North-Western Europe did not always lead the way. Rather South-Western Europe led with Iberia and Italy, while South-Eastern Europe had the highest levels of numeracy during the early Middle Ages, led by the East Roman Empire, although it fell back thereafter. North-Western Europe was on a more stable growth path, however, taking the lead in the 10th – 13th centuries. By the 14th and 15th centuries, Iberia and Italy had caught back up to North-Western Europe, as described by Broadberry (2013). By then, however, the UK had already reached full elite numeracy under our indicator.

Eastern Europe began the sixth century with approximately 20% of its ruler birth years known, or just slightly lower. Its developmental path for numeracy would occur at a much slower rate, particularly in Romania, where the proportion of known ruler birth years was less than 5% when its kingdoms began to emerge in the 12th century. Only later does Romania exhibit a strong growth rate in elite numeracy. In the period between the 12th and 18th centuries, other Eastern European countries lagged significantly behind their North-Western counterparts.

South-Eastern Europe is an interesting case in which we can clearly see the impact of historical developments.[12] Admittedly, we have few observations for the East Roman Empire in the first period (with its capital located in today's Turkey), but our figure (B.5.) shows a clear deterioration of elite numeracy during the decline of the Byzantine Empire, followed by stagnation in the years that followed. This stagnation also coincided with various invasions from Central Asia. Finally, South-Eastern Europe exhibited strong growth in elite numeracy after the Great Plague, catching up to both groups of Western European countries by the 18th century, a lag of approximately 400 years. Central European trends are not shown here because

[12] Additionally, it should also be noted that South-Eastern Europe is heavily influenced by the East Roman Empire in the earlier centuries of our sample. Before its decline, the Byzantine Empire displayed much less violence and higher rates of numeracy than are associated with its neighbouring kingdoms at the time.

they have a very high starting point and quickly reach 100%. However, they are presented as a group in figure B.6., which plots elite numeracy for broader regions in a single figure.

In figure B.6., two clear patterns emerge within Europe's regional development in elite numeracy. Although it is difficult to confidently assert initial positions in the 6th century, it seems that all regions aside from Central Europe had roughly similar levels of elite numeracy – ca. 40% – around the 10th century, before diverging drastically. While Central, North-Western and South-Western Europe (with a small lag) exhibit strong increases from this point onwards, Eastern and South-Eastern Europe display stagnant or even declining series that only begin to increase during the period 1500–1700 CE. Eastern Europe only catches up to Central and Western Europe towards the end of the study period.

Moreover, the similarity in trends of neighbouring regions makes our estimates more plausible. For the remainder of our analysis, we will revert to country-level units instead of the regional level used in the figures above. The advantage of using more aggregated units for figures is that we obtain smoother trends, while this is less important for regression analysis. When using regional units, we find the same overall regression results, but they are less robust due to smaller sample sizes (see table A.B.10. for a robustness check at the regional level).

We study a very long time-frame of elite violence and elite numeracy in this paper and it is quite likely that the relationship between the two variables may have changed, especially as military technology transformed, state organization developed and the intensity of nomadic invasions varied. Hence, we look at a series of scatterplots, first separating the study period by the first three centuries (6th to 8th centuries) and then bicentennial periods thereafter (9th and 10th centuries, 11th and 12th century etc.; figures B.8. to B.12). We invert violence into 'non-violence', as this makes the graphic easier to read. The relationship between elite non-violence and elite numeracy is already clearly visible in the eighth century, with Spain (es) holding one of the highest elite numeracy values when Al-Andalus had reached its peak (figure B.8.). In contrast, Spain had some of the worst values in terms of elite violence and elite numeracy under the west Gothic rulers of the sixth century. The decline of the East Roman Empire (tr) is also apparent here. Russia (ru) and Ukraine (ua) were more extreme, with all rulers in Russia being killed violently.

The following period was characterised by the Hungarian invasions that affected large parts of Europe, as well as more localised conflicts in South-Eastern Europe (figure B.9.; the Arabic and Bulgarian invasions of the East Roman Empire (tr), for example, and the Vikings in the north-west). Muslim Spain (es) was still among the low-violence and high-numeracy cases, as was the Holy Roman Empire (de); although the population suffered terribly from Hungarian invasions, the Emperors were not killed. Ukraine (ua), and the states and principalities on the area of modern Turkey (tr) suffered the most.

The following period of the 11th and 12th centuries had no major nomadic invasions (rather, European states invaded in the Middle East), and European principalities reached a greater stage of feudal development (figure B.10.; also see Hehl 2004). We observe that the relationship between elite violence and elite numeracy was weaker during this 'high medieval peace period' – violence was less detrimental for overall development. This changed during the 13th and 14th centuries with the arrival of nomadic Mongolian invasions. During this period, the impact of violence was larger again, as can be seen by the slope of the regression line (figure B.11.). During this period, state organization continued to develop and France made particularly strong progress in tax institutions during the Hundred Years' War (North 2000).[13]

Finally, during the 15th and 16th centuries, we observe an even stronger east-west disparity. A cluster of Western and Central European countries had almost no elite violence by

[13] Another substantial change that took place during this period was the 'infantry revolution'. How did the infantry revolution affect battle violence of the elite? We might expect that the reduced importance of heavy cavalry and its substitution for infantry reduced battle-related violence among the elite. However, this reduction might have taken time to come into effect. During the 14th and 15th centuries, the number of battle deaths among noblemen might have even increased. The famous battle of Crécy is a good example, as many noblemen were killed by English longbows or other military innovations.

this time, along with near complete rates of elite numeracy. In contrast, Ukraine (ua), Albania (al) and other Eastern and South-Eastern countries lagged far behind during this period. Some outliers combine high violence and low numeracy (see Cyprus [cy], Luxembourg [lu] etc. in figure B.10.), but these were small principalities with lower observational densities. During this period, the new, resource-intensive city protection of the '*Trace Italienne*' began to require increasingly greater tax resources for military success (Gennaioli and Voth 2015). Western powers such as Britain (uk), France (fr) and the Netherlands (nl) were better suited to develop these tax capabilities, and the evidence from regicide and battle deaths suggests that this resulted in a decline of violent deaths among the elite.

B.6. Empirical Analysis

The independent variables used in this analysis fall into two distinct groups: those that control for potential biases that may cause the known ruler birth year indicator to diverge from a 'true' measurement of elite numeracy, and those that constitute explanatory variables – variables that help to assess the potential impact of elite violence on elite numeracy.

Because a longer reign may provide greater opportunity for chronologists to record a ruler's birth year, we control for the average length of reign across each country and century. To control for the power and influence of each kingdom, we use their areas in square kilometres (Nüssli 2010) as well as whether rulers had the freedom to act and set policy autonomously, as opposed to being under the suzerainty of an overlord. Table B.3. shows that neither reign length nor autonomy significantly affects the likelihood of a ruler's birth year being recorded, although kingdom area becomes marginally significant when other explanatory variables and controls are included.[14]

[14] As a precaution, the full fixed effects specification from section B.6.1. is repeated using the predicted values for the known-birth indicator in the appendix. Although some of the coefficients change marginally, all of our conclusions remain the same.

Our first explanatory variable is the 'proportion of rulers killed in battle'. This variable provides information on civil wars and external military pressures on each kingdom, which may have affected elite numeracy through the destruction of educational infrastructure or reduced incentives to invest in elite numeracy due to lower life expectancies (Cummins 2017). Moreover, battle deaths and regicide are correlated, meaning that excluding battle deaths as a control variable could lead to an overstatement of any effect of regicide on elite numeracy.

Urbanisation rates are widely used in the economic history literature and act as a broad control variable for factors that could confound the relationship between elite violence and elite numeracy. They have also been employed as a proxy indicator for income among early societies in which other income proxy data are unavailable (Bosker et al. 2013; De Long and Shleifer 1993; Acemoglu et al. 2005; Nunn and Qian 2011; Cantoni 2015). Bosker et al. (2013) hypothesise that part of this relationship works through agricultural productivity, because a productive agricultural sector is required to support a large urban centre and urban areas cannot produce their own agricultural goods. We admit that, as urbanisation may be endogenous, there may be a trade-off between including an endogenous control and allowing omitted variable bias to enter the model. Therefore, we only include urbanisation in a subset of regression models.

We also introduce a measure of institutional quality as a potential determinant of elite numeracy. Our indicator for this is the mode of ruler succession, as this captures a certain preference for the division and limitation of dynastic power.[15] We use a three-category indicator to describe whether a ruler obtained their position through inheritance, partial election or full election by the aristocracy (as in Venice, for example).[16] The differences in institutional quality between states, seen through modes of succession, are not as large as those between democracy

[15] Division and limitation of power among other elites, since universal suffrage is a relatively recent phenomenon.

[16] Among partial electoral systems, we include ceremonial systems in which a vote took place but the current ruler's heir was consistently elected. For example, a ceremonial system was always in place in the Holy Roman Empire between 1453 and 1740, where a member of the House of Habsburg was consistently elected. We propose that ceremonial elections at least indicate a preference for dividing power over autocracy.

and autocracy, but evidence on democratic structures does not exist for the earlier periods under study here. However, a preference for the division of power reduces the likelihood of unconstrained totalitarianism. Again, we expect this aspect of institutional quality to be positively correlated with elite numeracy.

Next, we use estimates of pastureland area from Goldewijk et al. (2017). We transform the variable to pastureland per square kilometre per capita and then standardise it to a [0, 1] index. Motivation for including this control is that pastureland provides nutritional advantages, and improved nutrition is known to have positive implications for human capital (Schultz 1997; Victoria et al. 2008). Second, numerous studies have used pastureland and pastoral productivity as means of estimating female labour force participation, providing information on female autonomy and gender inequality, and perhaps elite human capital as a result (Alesina et al. 2013; de Pleijt et al. 2016; Voigtländer and Voth 2013; Baten et al. 2017). This mechanism functions through women's comparative physical disadvantage, relative to men, when ploughing fields and performing other tasks required when crop farming. Over time, this tendency developed into a social norm that saw men work in the fields while women took care of 'the home' (Alesina et al. 2013). However, when cattle and other domestic animals were present, their care became the task of women, boosting female labour participation and the contributions of women to household income. With increased income contributions, female autonomy increases and gender inequality is reduced, allowing women to develop their own human capital and contribute to economic development (Diebolt and Perrin 2013).

Fourth, as a counterweight to the pasture variable, we also use cropland. Like pastureland, cropland should describe agricultural and nutritional development but should also emphasise gender inequality for the reasons just mentioned. Therefore, its coefficient should be positive if nutrition, in terms of calories, is more important for elite numeracy, and negative if gender inequality is. The cropland variable is also transformed into per square kilometre per capita terms and then standardised (Goldewijk et al. 2017).

Last, we include a variable for the second serfdom to assess whether the inequality that it wrought had any impact on elite numeracy in Eastern Europe. This is coded as a dummy variable for all of Eastern Europe from the 16th until the 18th century and until the 19th century in Russia, where serfdom was only officially abolished under Tsar Alexander II in 1861.

In a section estimating random effects models (section B.6.3.), we also include religious and geographical factors, which are mostly time-invariant.

B.6.1. Fixed Effects Specification

We undertake an empirical analysis that consists of three parts. We first employ a fixed effects specification to test the existence and robustness of the relationship between elite violence and elite numeracy. Thereafter we implement an instrumental variable strategy and endeavour to find a causal effect of elite violence on elite numeracy and, lastly, we run a random effects specification to add time invariant (or almost invariant) factors.[17]

The fixed effects specification is set up as follows:

$$elite\ human\ capital_{it} = \alpha_i + \gamma_t + \beta_1 regicide_{it} + \beta_2 battle\ deaths_{it} + \beta_k \psi_{it} + \varepsilon_{it} \quad (1)$$

where α_i are country fixed effects, γ_t are two-century fixed effects, ψ_{it} is a vector of the control variables described above and ε_{it} is an error term that captures time-variant unobservables. We also make use of clustering at the country level, as it would be unrealistic to assume that within-country observations are entirely independent of one another, and estimate robust standard errors. We also use bootstrapped standard errors by employing the wild bootstrap procedure of Cameron et al. (2008, see notes to table B.4.).

[17] We also conduct spatial regressions (appendix B.10.3.) to uncover the effects of spatial autocorrelation.

We immediately see that both the regicide and battle death indicators enter into each regression model significantly and with a negative coefficient (table B.4.). These coefficients are also fairly stable across our specifications, implying that our control variables are less important for elite numeracy than violence. The coefficient for regicide remains between approximately -0.42 and -0.51, which can be interpreted as a one percentage point increase in regicide being associated with a 0.42 to 0.51 percentage-point decrease in the rate of known birth years. Alternatively, a one standard deviation increase in elite violence is associated with a 7.4-to-8.9 percentage point decrease in elite numeracy, which is a substantial effect. However, in the same way that violence could have acted as a restraining factor on the growth of elite numeracy over time, it is also possible that causality runs in the other direction.

Like regicide, the battle indicator also yields significant and negative coefficients that are robust to the introduction of control variables. These coefficients are approximately one-third larger than those for regicide (in absolute terms) and fall between approximately -0.66 and -0.70. However, the distribution of battle death frequency is narrower than that for regicide, meaning that a one standard deviation increase in battle deaths is associated with a 5.4 to 5.8 percentage point decline in elite numeracy.

None of the control variables appear to have significant impacts in estimating elite numeracy after including both country and two-century fixed effects, although the results for pastureland and cropland (proportions per square kilometre, per capita) are still interesting. In isolation, neither of these variables enters into any of the regressions significantly; however, together they reveal drastically disparate results. If either the cropland or pastureland variables had significantly and positively entered into regressions four and five, this would have provided evidence for the hypothesis that nutrition improves numeracy and human capital. This is not the case here, but because the coefficient for pastureland is significantly positive while the coefficient for cropland is significantly negative when the variables enter together in regressions six to eight, this may have implications for gender inequality in accordance with the Alesina et al. (2013) and de Pleijt et al. (2016) hypothesis. Consequently, this result also hints that improved gender equality may have improved elite numeracy in Europe.

Residual scatterplots allow us to compare our dependent variable and independent variable of interest more directly. We first run our standard fixed effects regression from table B.4. while omitting elite violence, and then regressing elite violence on all other explanatory variables.[18] Figure B.13. shows the relationship between the residuals of both regressions, allowing us to conclude that the controlled relationship between elite numeracy and elite violence is indeed strongly negative. This also allows us to conclude that the results are not driven by a small number of outliers.

Observations from the 6th century territories of today's Russia (ru) and Montenegro (me), and from Lithuania (lt) in the 14th century, show high residual violence and low residual elite numeracy. Conversely, there are cases such as the East Roman Empire (with its capital in what is today Turkey [tr]) that have low residual violence and high residual elite numeracy in the 6th century. Another interesting aspect of figure B.13. pertains to the cases located north-east of the regression line, e.g., Hungary (hu) in the 11th century and Sweden (se) in the 12th century. These regions reached relatively high levels of elite numeracy despite remaining fairly violent. This is not true for the examples on the other side of the spectrum, such as Romania (ro) in the 14th century. In general, we observe a close relationship between residual violence and residual elite numeracy.

Nevertheless, we must acknowledge the role that spatial autocorrelation may have played. Kelly (2019) recently argued that many results in the economic persistence literature could have arisen from random spatial patterns and that the likelihood of this problem is higher if the effects of spatial autocorrelation are not controlled. In this study, spurious relationships may form due to numeracy or violence spillovers rather than as a result of truly economic

[18] We include our 'elite controls' as explanatory variables in both of these regressions.

interactions. Therefore, we make use of spatial econometric techniques first formalised by Jean Paelinck and Leo Klaasen (1979) in appendix B.10.3. The results from these spatial regressions provide remarkably similar results to those from the fixed effect model (equation 1). Hence, spatial autocorrelation does not seem to be a notable source of endogeneity in this study.

B.6.2. Instrumental Variable Specification

Although the fixed effects regressions (and spatial regressions) provide a robust assessment of the conditional correlations between elite violence and elite numeracy, endogeneity in the form of simultaneity could still exist. Accordingly, we use an instrumental variable analysis to circumvent this endogeneity issue and assess whether any causal effects exist. Clearly, finding suitable instruments for the medieval period is a substantial challenge, but certain events that took place had the characteristics of 'natural experiments'. We use the nomadic invasions from Central Asia because their origins were determined by climatic forces – mainly droughts in Central Asia (Bai and Kung 2011) – and by military capacity.

Pinker (2011) found that the major nomadic invasions represented three of the six most violent and victim-intensive events in all of human history.[19] For European history during our 6th to 19th century timeline, the Hungarian and Mongolian invasions were the most influential. Although other invasions (the Arab-Berber invasions of Spain, the Bulgarians, the Vikings, and the Seljuks/Ottomans and others) were also relevant, they were more localised. Here, we analyse how these invasions affected European elites.

[19] He reanalysed White's (2011) list of "death tolls of wars, massacres, and atrocities" by deflating the number of victims of each event by the population of each respective century. Pinker argued that with a larger population, more victims are likely. Deflating by population, the wars of the 20th century are still among the most terrible atrocities, but are less exceptional. The Mongolian invasions were the most influential of all nomadic invasion-related events (ranked second of all atrocities in human history). Other events related to nomadic invasions included the end of the Ming dynasty in China (and the Manchurian invasion related to it) as well as the end of the West Roman Empire (and the Hunnic and Germanic invasions related to it; see Pinker 2011).

First, some of the nomadic invaders created new vassal states in their newly conquered territories, often leading to additional conflicts because local elites disputed the legitimacy of their regimes (Fennell 1986). For example, the Mongolians set up client rulers and partially dependent rulers in Eastern and South-Eastern Europe. Yury, the prince of Moscow, even received military support from the Mongolians when trying to conquer Tver, Russia, in 1317 (see Fennell 1986 on the following): After being defeated, Yury was called to the 'Golden Horde'[20] to be put on trial for his failure. Before any inquiry could take place, he was killed by Dmitry "the Terrible Eyes", who the son of Mikhail of Tver. Dmitry was later executed by the 'Horde' himself. In sum, the behaviour of the rulers under Mongolian suzerainty was unusually violent (Fennell 1986).

Secondly, after the nomadic invaders had killed several European rulers, the psychological hurdles for Europeans to assassinate their own rulers had been lowered. Previously, particularly during the high Middle Ages, the lives of rulers were accepted as sacrosanct more widely than before or after (there were exceptions, of course, see Hehl 2004). During the 13th and 14th centuries, rulers were often killed by their own knights or other personnel, and not only by competing nobility or neighbouring rulers. For example, Richard Orsini, the count of Cephalonia, was killed in 1303 by one of his own knights (Nicol 1984).

Thirdly, the manner of killing rulers changed dramatically after the nomadic invasions. In the medieval period, death by sword was considered more honourable and appropriate for rulers, whereas many other ways of killing were reserved for criminals. That rulers were subjected to alternative means of killing was initially inconceivable. For example, the Byzantine historian and chronicler Leo the Deacon describes the death of Igor I of the Kievan Rus with some horror: "They [a neighbouring nomadic tribe] had bent down two birch trees to the prince's feet and tied them to his legs; then they let the trees straighten again, thus tearing the

[20] The division of the Mongolian Empire that had offered Yury military aid.

prince's body apart." (Kane, 2019). As another example, Aleksandr of Tver was quartered in Sarai in 1339 (Fennell, 1986).

Fourthly, and with a long run impact, taking revenge rose in cultural value. The traumatic impact of the additional frequency of violence against rulers produced psychological responses from the upper classes, forming a 'culture of revenge' which was applied if they felt that their honour had been violated (Pust 2019). This 'culture of revenge' phenomenon was most persistent in Eastern and South-Eastern Europe. One act of revenge spurred the next, and the increase in the cultural value of taking revenge became a strong hurdle against development. In societies that favour revenge, trust of foreigners also develops at a slower rate (Pust 2019).

In conclusion, this 'natural experiment' of nomadic invasions first increased the existing levels of violence, as many individual examples show. Several mechanisms were at work and not all of these examples took place on the battlefield. Even more effectively, the trauma from violence had a relatively persistent effect via the development of a 'culture of revenge', particularly in Eastern and South-Eastern Europe.

The Hungarians, Mongols, Huns and other equestrian-driven nomads had a distinctive style of warfare. The secret to their success was the combination of horsemanship, mounted archers and the incitement of terror against civilian populations (Adshead 2016). Their military efficacy was often so superior that even Europe's strongest empires were unable to protect their constituents. For example, the Holy Roman Empire was helpless against Hungarian raids for more than a century, and it took them almost two centuries to defeat the Hungarian armies at the Battle of Lechfeld in 955 CE (Bowlus 2006). Likewise, in the 13th century, the powerful and by then European Kingdom of Hungary offered little resistance to Mongol invasions (Sinor 1999).[21]

[21] The Hungarians had already settled in today's Hungary by late 9th century and had, by the beginning of the 11th century, abandoned their nomadic lifestyles in favour of a more settled, somewhat urban lifestyle.

How did these nomadic invaders succeed against Europe's strongest empires? Military historians agree that their equestrian-based military tactics were the most critical factors (Sinor 1999). Central Asia was the world's equine capital at the time. It has been estimated that by approximately 1200 CE, half of the world's horse population was based between what is today Eastern Russia, Mongolia and the Ural mountains, whereas only a tiny fraction of the world's human population resided there (Adshead 2016: 61). Each Central Asian warrior could therefore possess up to 15 or 20 horses (Adshead 2016: 61), providing easy remounts each time a horse was wounded. Complimentarily, these nomads were expert archers and military strategists. For example, they employed the "Parthian shot", which was a Parthian military tactic of mounted archers firing at their enemies while in actual or staged retreat. The manoeuvre became famous when used against the Roman Empire in the first century BCE, a particularly noteworthy example being the defeat of the Romans by the Parthians at the Battle of Carrhae in South-Eastern Turkey – on the border of the Roman and Persian Empires in 53 BCE (Mattern-Parkes 2003).

The innovative equestrian strategies and the bowmanship of the Asian nomads were impressive and could have been emulated by European armies, but the strength of their cavalry, with 15-20 horses per warrior, could not be provided by Europeans at the time.

Inciting terror was also a tactic used by many armies before then, but only in combination with the speed of horses was it so exceptionally effective. On the other hand, the unique military supremacy provided by their horsemanship and the sheer number of horses that they possessed resulted in geographic constraints that we use for our instrumental variable strategy. Short campaigns to Italy, France or North-Central Europe were possible, but Central Asian invaders quickly returned to the sparsely populated regions of Eastern Europe or to Central Asia itself. For example, the Mongols suddenly left for the Russian Steppe in 1242 after conquering most of East-Central Europe (Sinor 1999). As a consequence, the closer a European territory was to Central Asian and Eastern European horse bases, the larger an "import of violence" it experienced. As a reaction to frequent raids and terror, Eastern and Central European societies militarised and favoured power and values such as loyalty over mercantile activities or trade. Hence, we can use the distance to Central Asia as an instrument for the additional violence that was imported through these nomadic invasions.

Clearly, the Hungarians and Mongols were not the only groups that spread violence over such large distances.[22] The Viking raids of the 9th and 10th centuries, the Arab-Berber invasions of Iberia and parts of Italy, as well as the Ottoman invasions in the Balkans – to name just a few – added to European violence too. However, we argue that these activities were more localised, whereas Central Asian nomads affected almost all of Europe. Moreover, it is unclear that the Muslim rulers of Spain were more violent than Spain's earlier Gothic rulers (Pérez Artés and Baten 2018). Likewise, although the Vikings were far more violent than the incumbent inhabitants of the lands that they conquered, historians have explained that their reputation was, to a degree, overstated by monks in Western European monasteries who sought to disseminate propaganda against the "mighty heathens of the north" (Winroth 2014). Winroth (2014) adds that since the victims were from societies more literate than themselves, Viking raids constitute a rare historical case where history was not written by the 'victors'. Additionally, the Vikings began to settle in the United Kingdom and Normandy well before 1050 and ceased their tradition of raiding (Griffiths 2010).

Because we use these nomadic invasions from Central Asia as an instrumental variable, endogeneity could result from heterogeneous levels of economic development along the east-west gradient. However, we observe that this gradient is a feature of the last few hundred years and does not exist for the early medieval period. We have seen, in figure B.6., that elite numeracy was highest in South-Eastern Europe during the 6th to 7th centuries, when the East Roman Empire was the gravitational centre of European development. The second highest

[22] Our period of study does not include the Hunnic invasions but, as nomadic invaders of Europe, their history is still relevant to the discussion of our instrument.

levels at the time were found in South-Western Europe, particularly in Italy. The economic dominance of Europe's north-west only arose later, during the period when Eastern and Central Europe were affected by the Hungarian invasions. Indeed, the East Roman Empire was not overwhelmed by the Hungarian invasions, although much of its economic base in the Balkans was devastated. Furthermore, the Roman occupations of Gaul and Britain did not cause an east-west divergence in the early medieval period, according to our evidence. Figure B.17. supports this line of reasoning through the coefficients from regressions of elite numeracy on longitude over time.[23] Here, we see that being further east was actually associated with higher elite numeracy during the early Middle Ages and that the traditional, negative gradient effect is reduced (and insignificant) during this high medieval peace period.

In sum, a strong east-west gradient did not exist before the period of the Hungarian invasions but developed thereafter. The strongest emergence of an east-west gradient arose after the Mongolian invasions ceased during the 14th century. During this period, our instrument loses its econometric value, as the gradient would have become correlated with factors associated with the stronger economic development of the west. Therefore, we argue that for much of the formative period of Europe's path-dependent processes in the Middle Ages, the nomadic invasions from Central Asia are a suitable instrument for violence.

European history offers a placebo test for studying the exclusion restriction of our instrument. The period between the respective episodes of invasions by the Hungarians and Mongolians, namely, the High Middle Ages of the 11th and 12th centuries. Europe did not experience any major invasions at this time (instead, it acted as an aggressor by invading the Middle East during the crusades). Cummins (2017) provides some initial evidence for the high medieval peace period when analysing his database of noblemen. He shows a small but clear decline in battle deaths as well as a corresponding increase in average lifespans at the time,

[23] Longitude measured by geographic centroids for modern countries from Donnelly (2012).

which sharply reversed as the Mongol invasions begun and again as the Great Plague took effect. Hence, the proximity to Central Asia should be unimportant for violence during this high medieval peace period, given the absence of nomadic invasions, which would also provide additional evidence against any simple east-west effect.

Before we execute our IV regressions, we need to consider other potential factors that could prevent our instrument from meeting the exclusion restriction. Specifically, our instrument becomes invalid if any characteristics of the nomadic invasions that are not associated with military or interpersonal violence affected elite numeracy in Europe. Such characteristics are not immediately apparent, but, for example, any diseases that the nomads brought with them could have influenced numeracy and human capital through demographic channels. However, we find no evidence of this. The Justinian Plague ravaged much of South-Eastern Europe and parts of the Middle East from the sixth to the early eighth century, but this was clearly before the period of the Hungarian invasions. Likewise, the Great Plague erupted in the mid-14th century, approximately 150 years after the Mongols had begun invading Europe. Therefore, the spread of diseases from Central Asia can only have had a very indirect effect on elite numeracy at most. Another potential factor that could violate the exclusion restriction is the transfer of technological ideas from Central Asia to Europe, brought by the nomads. Again, we cannot find any obvious examples. As discussed earlier, the horse and bow were already widely used throughout Europe by the time of the first nomadic invasions, and military tactics such as the "Parthian shot" had already been known in Europe for centuries.

In table B.5., we treat the three periods 800–1000, 1000–1200 and 1200–1400 CE separately and run the following instrumental variable specification, restricting our sample to each of the three periods mentioned above:

First Stage:

$$regicide_{it} = \alpha + \beta_1 proximity_{it} + \varepsilon_{it} \tag{5}$$

Second Stage:

$$elite\ human\ capital_{it} = \alpha + \beta_1 \widehat{regicide}_{it} + \beta_2 battle\ deaths_{it} + \beta_k \psi_{it} + \varepsilon_{it} \tag{6}$$

where $proximity_{it}$ is the logged inverse distance to Central Asia, ψ_{it} is a vector of control variables, α is a constant and ε_{it} is an error term that captures the effects of any unobservables.

Admittedly, the number of cases in each period is small, but this should bias the tests towards insignificance. Instrumented regicide exhibits a significantly negative effect on elite numeracy during the two invasion periods of the Hungarians and Mongolians, circa 800–1000 CE and 1200–1400 CE, respectively. During the High Middle Ages, when no Central Asian invasions occurred, the relationship between elite numeracy and the invasions from Central Asia becomes insignificant. Although the absence of significance does not rule out the existence of a relationship, this result hints that our IV only influences elite numeracy through violence during the invasion periods. Additionally, this result disputes the possible criticism that our IV only captures the east-west development gradient of more modern times. As such, it provides tentative evidence (despite the small N) of a causal impact of elite violence on elite numeracy.

In table B.6., we pool all evidence on nomadic invasions from Central Asia in the periods 800–1000 and 1200–1400 as an instrument, including all explanatory variables that have been identified before, finding negative and significant coefficients for regicide. We again find a positive and significant coefficient for partially democratic political systems as well as our pasture variable, while we find a negative and significant coefficient for our crop variable.

B.6.3. Random Effects Specification with Time-Invariant Factors

As a further robustness test, we also apply a random effects specification because it does not eliminate the confounding effects of omitted time-invariant factors.

These controls first include variables concerning religion. Although religion is not perfectly time invariant, there are not many examples of major religious changes within European kingdoms that occur on a mass scale after the collapse of the Roman Empire. Major religious changes that occurred include the Great Schism between the Catholic and Orthodox Churches in the 11th century, the Protestant Reformation, the spread of Islam under the Ottoman Empire, and the Arab-Berber conquest and Reconquista in Spain. We coded the majority religion using the ruler's religion from our regicide sources and the summaries of historical religion in the Encyclopaedia Britannica (2019).

Our first additional variable for the random effects specification is an indicator of the most prominent religion in each country during each century – Islam, Orthodoxy, Protestantism, Catholicism (the reference group) and an 'other' category; comprising Pagan, tribal and pre-Christian religions. This indicator variable was included to capture the effects of cultural characteristics that are associated with religion. We find similar levels of numeracy across Catholicism, Protestantism and Islam, with some evidence of lower levels for Orthodoxy and our 'other' category. Surprisingly, despite numerous results from previous literature, Protestantism is not associated with higher levels of numeracy (see Becker & Woessmann 2009 and 2010 for an alternative expectation).

We also include a dummy for religious diversity (Baten and van Zanden 2008). This could have either have had a positive effect on numeracy, perhaps via competition – stimulating book consumption, for example – or a negative effect via conflict through social fractionalisation (Easterly and Levine 1997). However, we find no evidence of an effect at all.

Our final religious variable is a dummy for the presence of a substantial Jewish minority, which we include because Jews were, on average, better educated than other religious groups among whom they lived. These data are from a combination of Anderson et al. (2017), Botticini and Eckstein (2012) and the Encyclopaedia Judaica (1972). This dummy provides a positive and significant association with elite numeracy of approximately 7-13%.

The rest of our new controls for the random effects model are geographic and wholly time invariant. We use ruggedness because numerous studies have associated it with violence and lower economic development in a broader sense. For example, Mitton (2016) finds flatter landscapes to be associated with higher GDP per capita, while Bohara et al. (2006), O'Loughlin et al. (2010) and Idrobo et al. (2014) all describe different situations where rugged terrain provides advantages for instigators of violence. In contrast, Nunn and Puga (2012) describe how ruggedness protected parts of Africa from the adverse effect of the slave trade between 1400 and 1900. The ruggedness data that we use come from Nunn and Puga (2012). As spatial controls, we again include latitude and longitude for each country. Next, we use the percentage of each country that is covered by fertile soil and the percentage of each country that lies within 100 km of ice-free coast. Both variables come from Nunn and Puga (2012) and control for any additional agricultural effects or the effects that maritime trade may have had on elite numeracy, respectively.

The random effects regressions also show largely similar results as the initial fixed effects specification, although the sizes of the coefficients differ modestly. The coefficients for elite violence are approximately 10-20% smaller under random effects, whereas those for battle deaths are between 5% and 15% larger. These variables both remain consistently negative and significant across specifications. Likewise, the coefficients for pasture and crop areas are approximately 40% smaller, though this is somewhat due to multicollinearity after the inclusion of the soil fertility variable. The soil fertility variable is frequently significant at the 10% level, though it is negative like the crop area variable. The fertile soils of Southern and Eastern Europe were often used for grain production, whereas the less fertile Northern European soils were more often used for cattle farming. During later periods, higher elite numeracy developed in Northern Europe.

Finally, we further assess the robustness of our results using quantile regressions to ensure that they do not depend on specific clusters of the overall data distribution (see section B.10.6.). We find that the results are robust.

B.7. Conclusion

In this study, we provide a 1400-year overview of elite numeracy in European history, using the share of rulers for whom a birth year was recorded as a new indicator. We carefully evaluate this measure, finding high correlations with other proxies for elite numeracy as well as dramatic shifts in elite numeracy throughout Europe.

The south-east was the first region to undergo transformation, led by the East Roman Empire (figure B.6.). Shortly afterwards, the south-west was slightly superior. All European regions were displayed comparable rates of elite numeracy around the year 1000, while North-Western and Central Europe did not begin to display their divergent patterns before the High Middle Ages. After this period, both the east and south-east entered into decline, and by 1400, a development path was firmly established that divided the east and the west of the continent. Iberia and Italy grew to similarly high levels as the north-west during the renaissance period.

We also assessed a number of potential explanatory variables that might either determine or interact with elite numeracy. For example, the existence of a substantial Jewish minority is associated with greater elite numeracy – what we observe might be external human capital effects from the Jews to the Christian elite. Finally, regions that specialised in cattle farming developed greater elite numeracy than grain-intensive regions, although this variable only becomes significant when both agricultural specialisations (cattle and crops) are included in our estimations simultaneously. A growing body of literature finds a relationship between agricultural specialisation in animal husbandry and the relatively strong position of women economically, which might also have influenced the upper tail of numeracy and human capital.

A consistent negative correlation is observable with violence – both violence during battles and 'ordinary', interpersonal violence among the elite. We also employ a relatively exogenous import of violence from the Central Asian nomadic invasions of ca. 800–1000 and 1200–1400 as an instrumental variable because these invasions acted contagiously and motivated additional intra-European violence. Interestingly, Europe did not experience invasions from Central Asia during the High Middle Ages, and European numeracy did not follow any east-west pattern at this time (figure B.15., panel b). By using the 'natural experiment' characteristics of the nomadic invasions, we observe casual effects from violence to elite numeracy. This is a crucial finding for understanding the divergence movement in Europe's developmental history, because it stands in contrast to Tilly's (1975) hypothesis that "war generated states". However, wars might have been the trigger rather than the underlying reason for developing tax capabilities. For example, France had already developed lower elite violence and higher elite numeracy in the centuries before the Hundred Years' War took place, already having prepared a more conducive environment for state capacity. The trigger of the devastating war with England convinced the French nobility that permanent taxation would be necessary, but this would not have been possible in another setting with a similarly devastating war, in South-Eastern Europe during the late medieval period, for example.

B.8. References

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B.9. Figures and Tables[24]

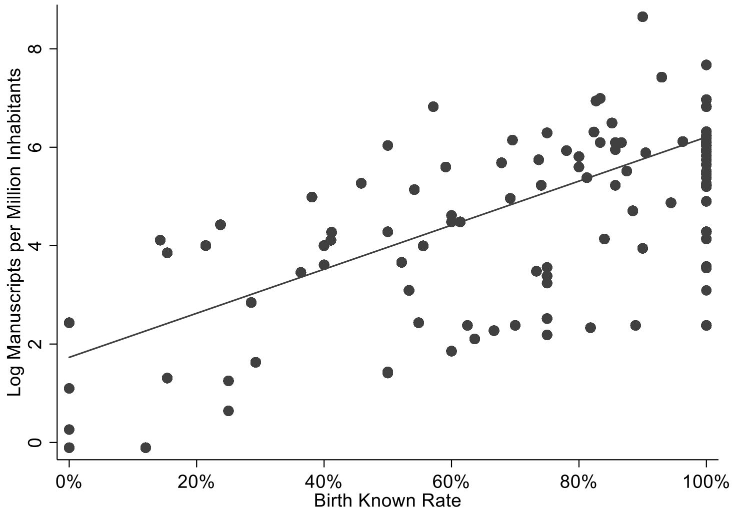

Figure B.1. Manuscripts vs Birth Known Rate (11 European countries, 700–1500 CE)

Note: Number of monastery manuscripts per million inhabitants (correlation coefficient ρ =0.67; or ρ=0.71 where the birth known rate is less than 100%). *Source*: Buringh and van Zanden (2009).

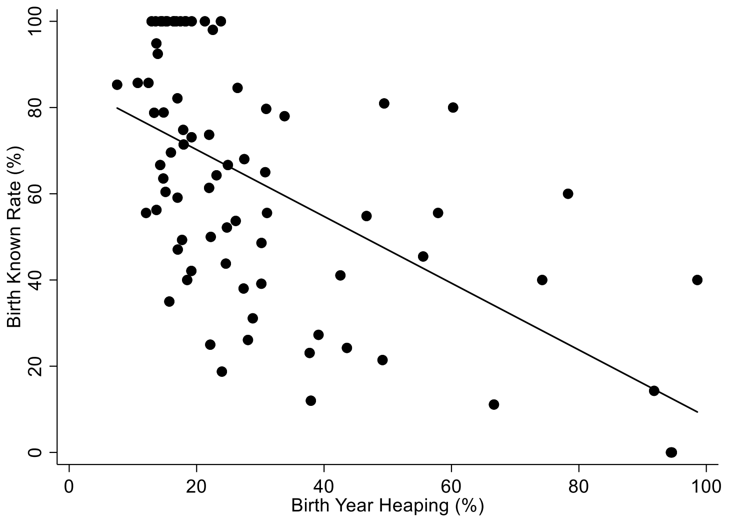

Figure B.2. Birth Year Heaping vs Birth Known Rate (7 European Regions, 800–1800 CE)

Note: Birth year heaping calculated from Cummins' (2017) sample of 115 650 European noblemen (correlation coefficient ρ =-0.58; or ρ=-0.54 where the birth known rate is less than 100%). *Source*: Cummins (2017).

[24] All figures plotted using Stata's graph or spmap functions

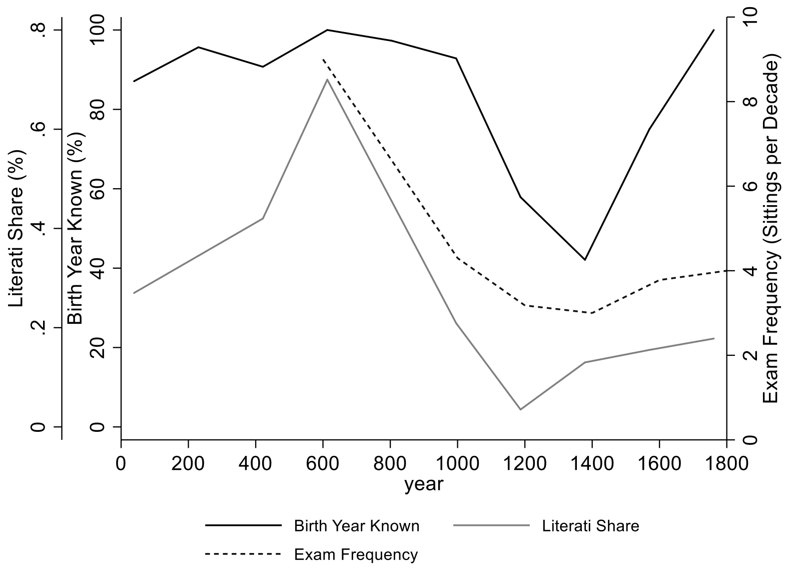

Figure B.3. Elite Numeracy and the "Literati" (China, 0 – 1800 CE)

Note: By 605 CE, China had introduced an unusual system for appointing their bureaucratic elites (Deng, 1993). If a candidate succeeded in passing the exam, they became a member of the educational nobility, the "literati", with considerable social status and a substantial income. Economically, China fared exceptionally well under this system during the medieval period (Baten 2016).

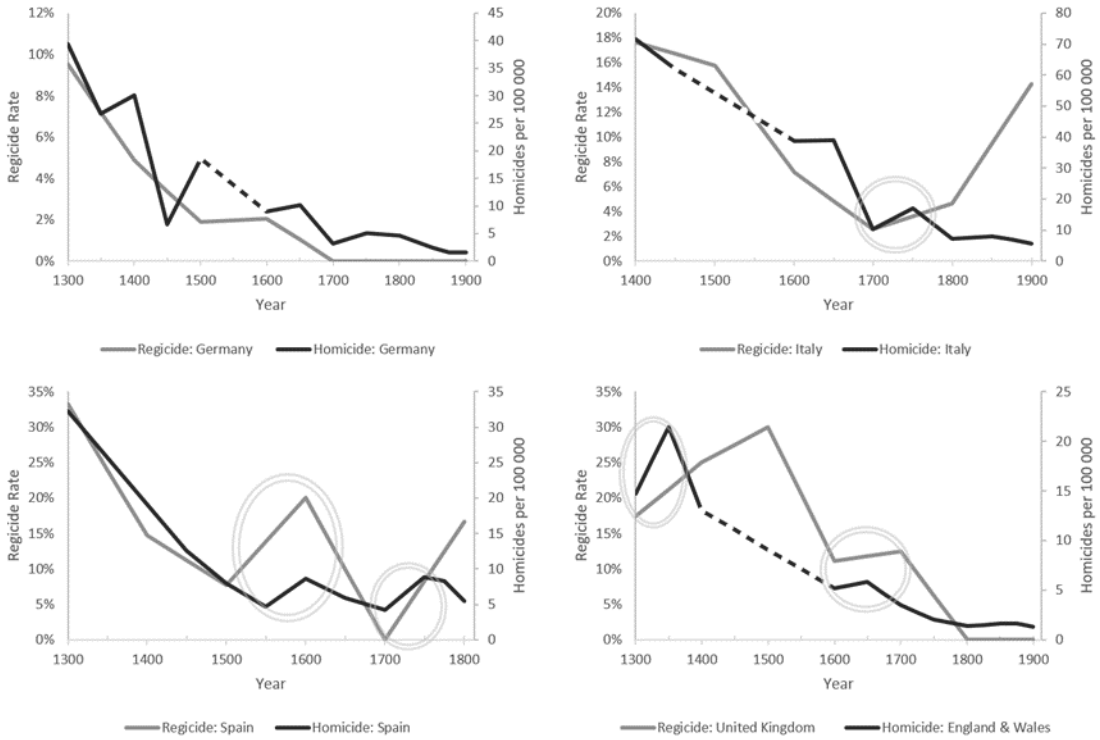

Figure B.4. Regicide vs homicide: Evidence for the Plausibility of the Regicide Indicator (Germany, Italy, Spain, UK, 1300-1900 CE)

Note: The figure shows declines in violence and the relationship between elite violence (regicide, defined as the share of rulers who were killed) and interpersonal violence (homicide per 100,000 population). The grey circles indicate periods during which both homicide and regicide rose simultaneously. *Sources*: Homicide data from Eisner (2014).

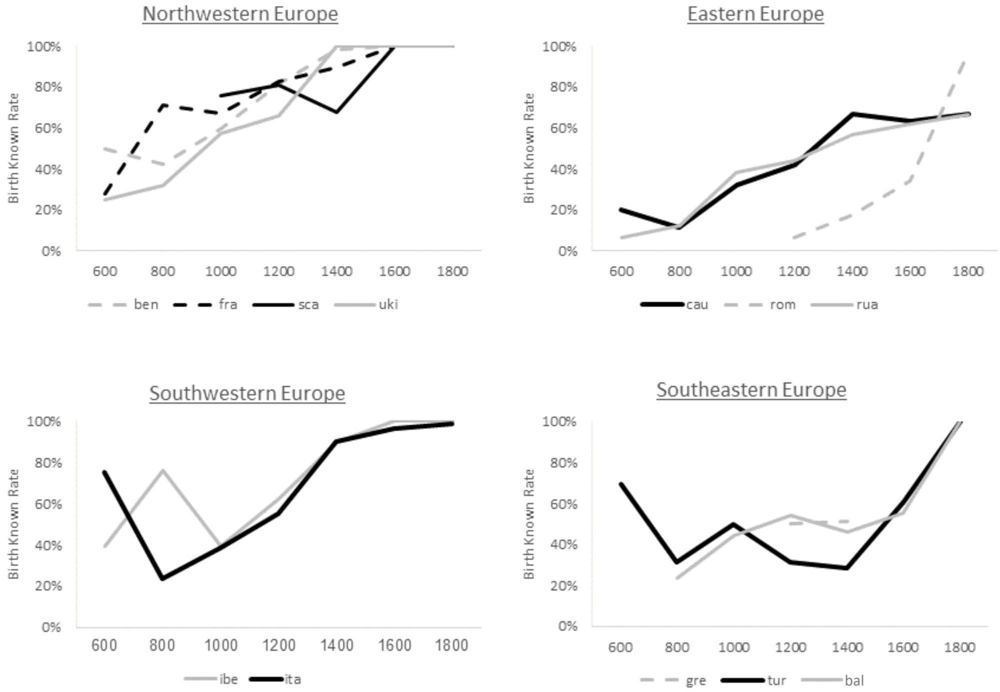

Figure B.5. Sub-regional Trends in Elite Numeracy

Notes: The year is the middle year of each two-century period, 600 for the 6th and 7th century etc. Abbreviations refer to the following: Benelux (ben – Belgium, Netherlands, Luxembourg); France and Monaco (fra); Scandinavia (sca – Denmark, Iceland, Lithuania, Latvia, Norway, Sweden); United Kingdom and Ireland (uki); Caucasus (cau – Armenia, Georgia); Romania (rom); Russia, Belarus and Ukraine (rua); Iberia (ibe – Portugal, Spain); Italy (ita); Greece and Cyprus (gre); Turkey (tur);: Balkans (bal – Albania, Bosnia and Herzegovina, Bulgaria, Croatia, Montenegro, Serbia).

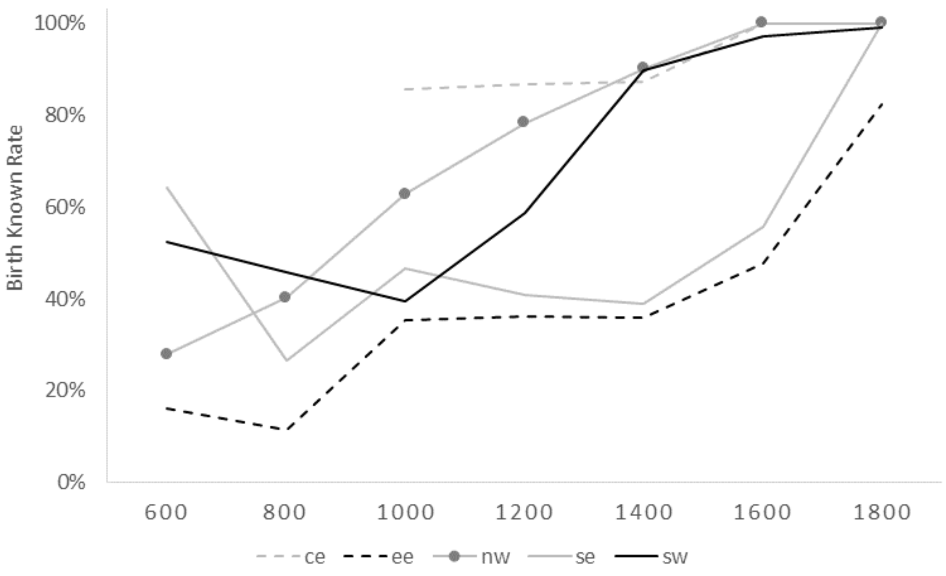

Figure B.6. Inter-Regional Trends in Elite Numeracy

Note: The legend refers to Central Europe (ce), Eastern Europe (ee), North-Western Europe (nw), South-Eastern Europe (se) and South-Western Europe (sw).

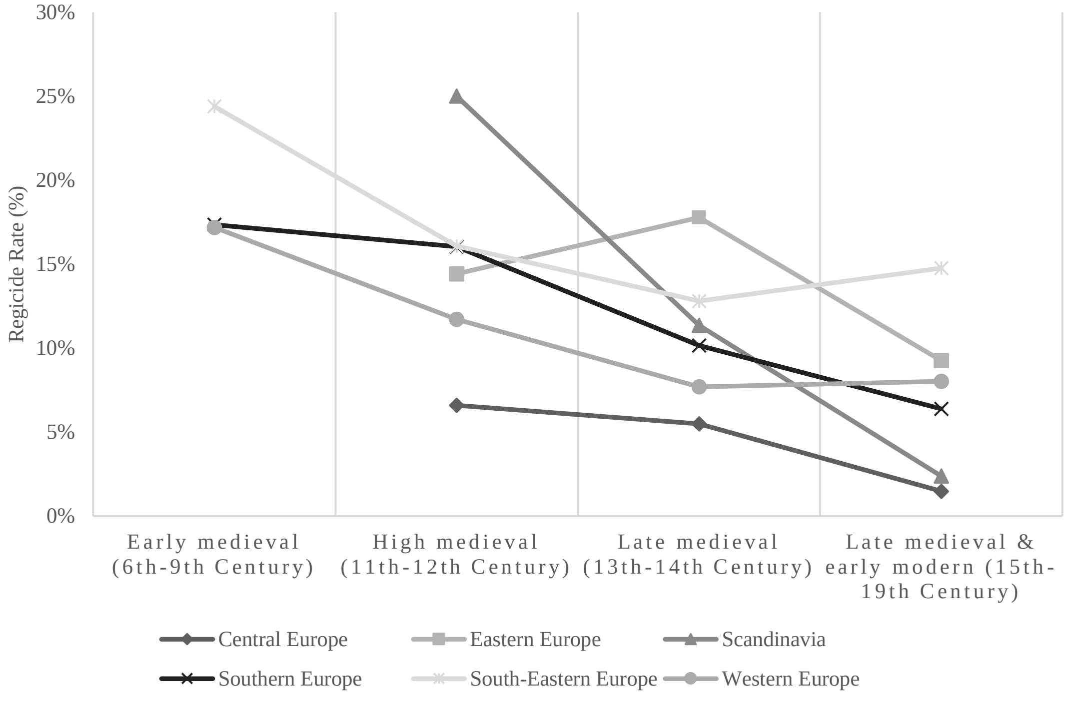

Figure B.7. Inter-Regional Trends in Regicide

Note: Regicide for the early medieval period in Eastern Europe was omitted here, as its 50% regicide rate would have obscured the graphic.

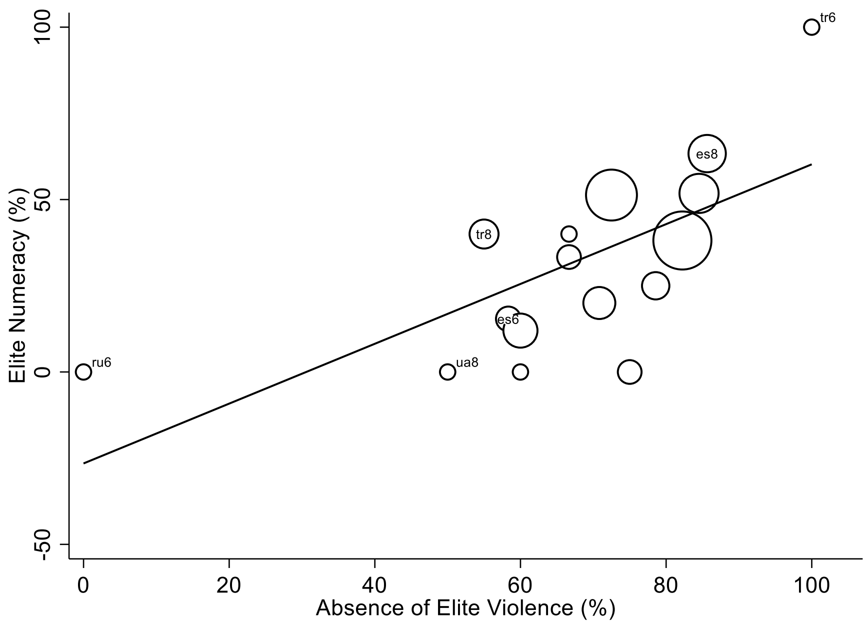

Figure B.8. Elite Numeracy and Non-Violence (6th – 8th Century)

Note: Scatter plot weighted by observations. Labels refer to countries (see table A.B.1. for country codes) and centuries.

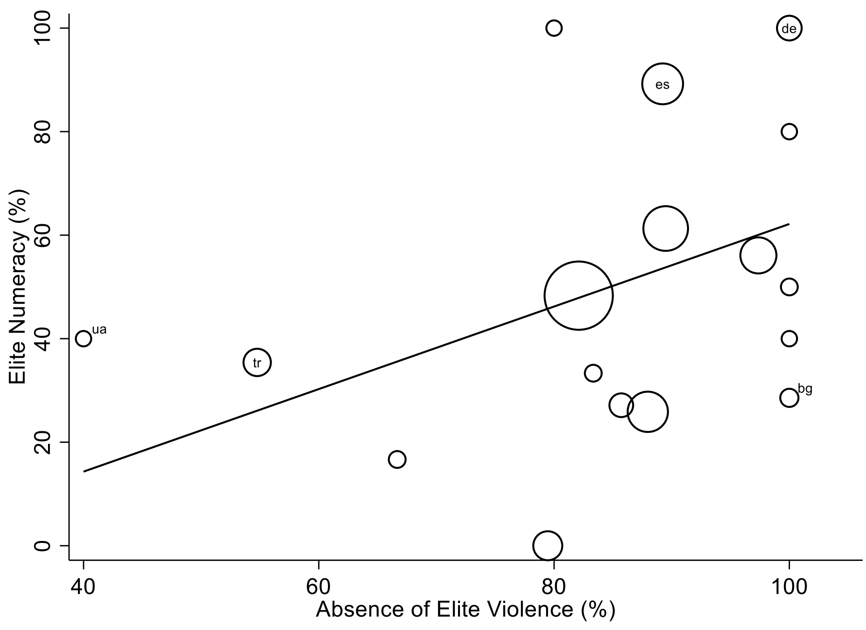

Figure B.9. Elite Numeracy and Non-Violence (9th – 10th Century)

Note: Scatter plot weighted by observations. Labels refer to countries (see table A.B.1. for country codes).

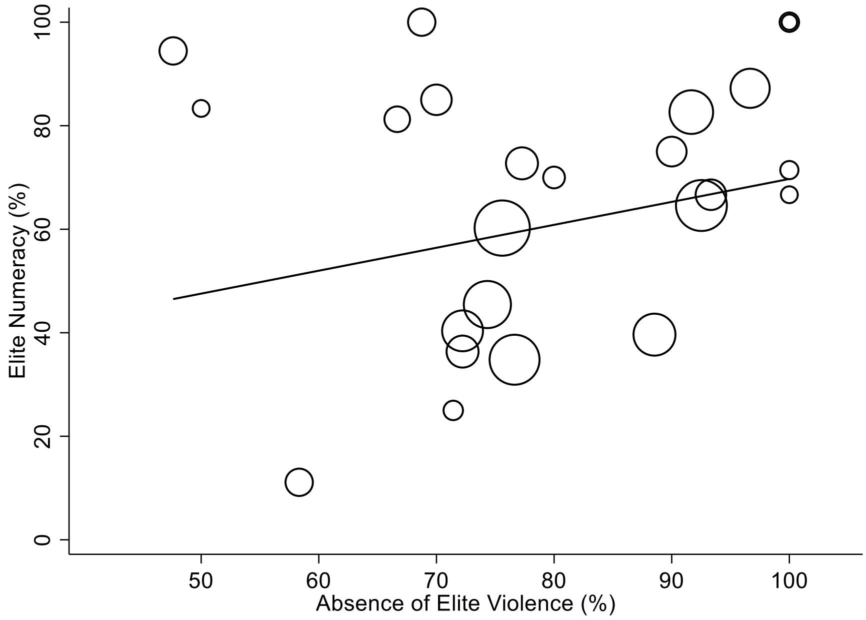

Figure B.10. Elite Numeracy and Non-Violence (11th – 12th Century)

Note: Scatter plot weighted by observations. Labels refer to countries (see table A.B.1. for country codes).

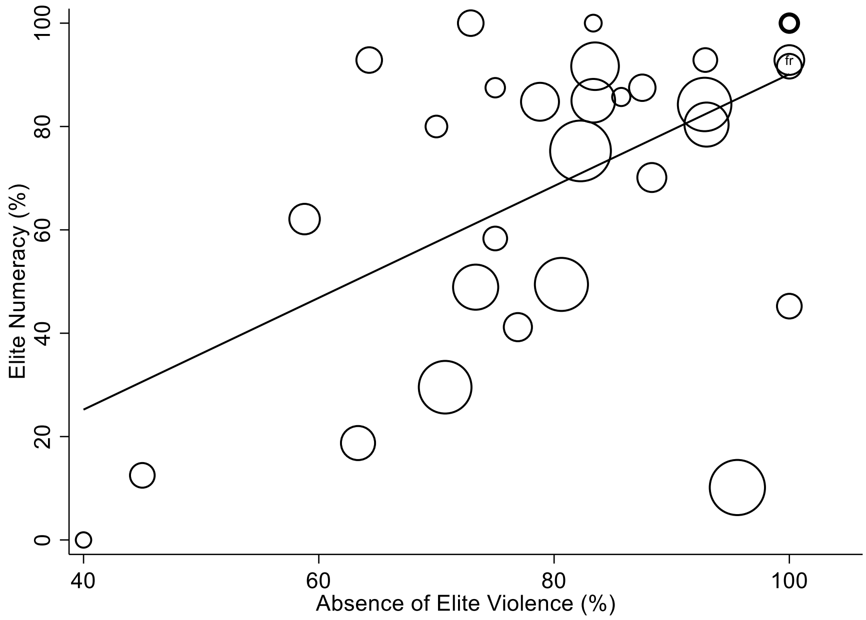

Figure B.11. Elite Numeracy and Non-Violence (13th – 14th Century)

Note: Scatter plot weighted by observations. Labels refer to countries (see table A.B.1. for country codes).

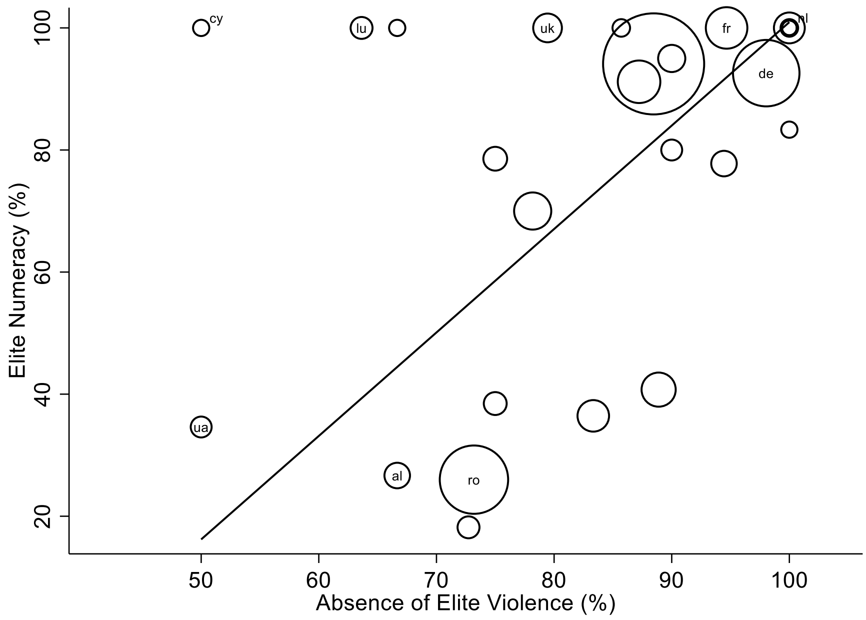

Figure B.12. Elite Numeracy and Non-Violence (15th – 16th Century)

Note: Scatter plot weighted by observations. Labels refer to countries (see table A.B.1. for country codes)

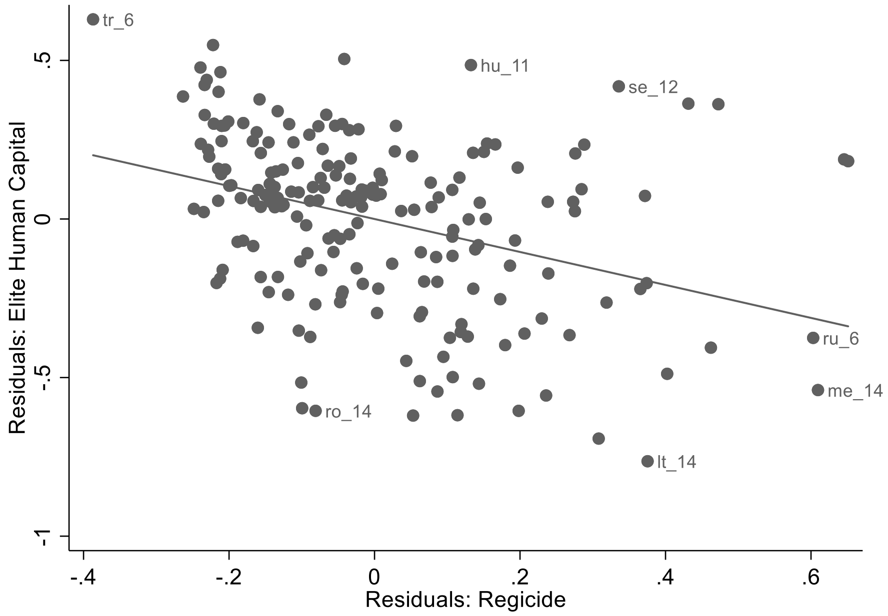

Figure B.13. Residual Scatterplot (All Regressors and Controls Included)

Note: The labels, above, refer to Turkey (tr), Hungary (hu), Sweden (se), Russia (ru), Montenegro (me), Lithuania (lt) and Romania (ro), respectively. The numbers denote the century of each observation e.g. ro_14 refers to 14th century Romania. (ρ = -0.36)

Figure B.14. Elite Numeracy (500–1900)

Note: The darker colours exhibit greater elite human capital.

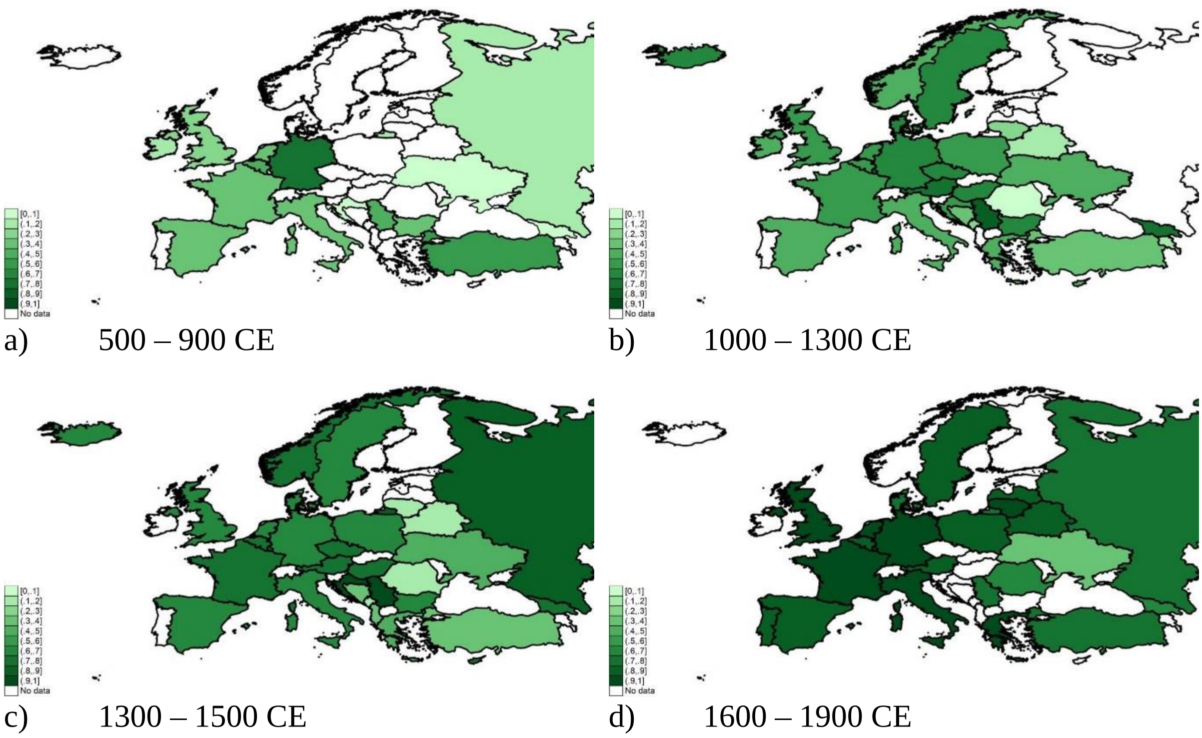

Figure B.15. Elite Numeracy by Period

Note: The darker colours exhibit greater elite human capital.

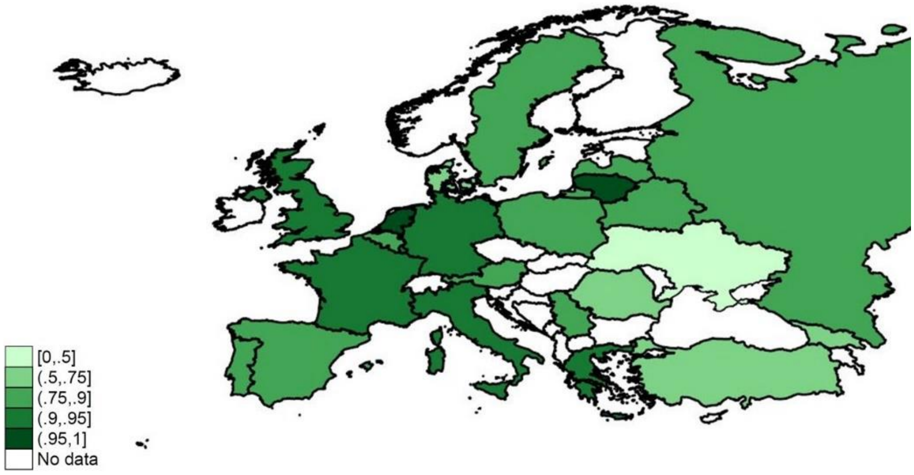

Figure B.16. Elite Numeracy (1600 – 1900 CE, Adjusted Bin Widths)

Note: The known ruler birth year measurement means that elite numeracy was consistently high by the early Modern Period (most countries are dark in figure B.15., panel d for 1600-1900). This bin-width adjustment merely allows for a clearer distinction between countries. The darker colours exhibit greater elite human capital.

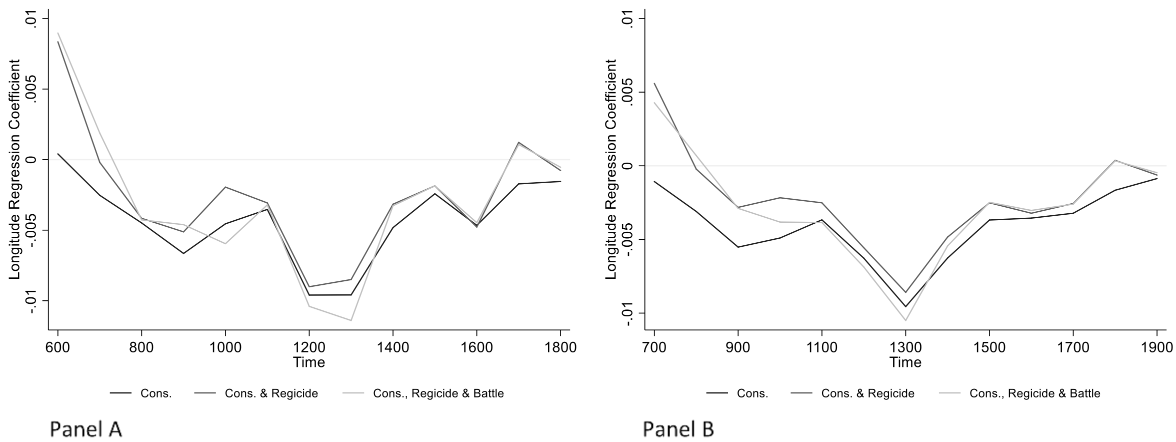

Figure B.17. No Western European advantage before 800: Regression Coefficients of Elite Numeracy on Longitude

Note: A positive coefficient means that longitude shares a positive relationship with elite numeracy; i.e. that being further east was associated with higher levels of numeracy. When the coefficient is negative, being further west was associated with higher levels of numeracy. Panel A refers to regressions for each century whereas Panel B uses two-century time periods for an increased sample size.

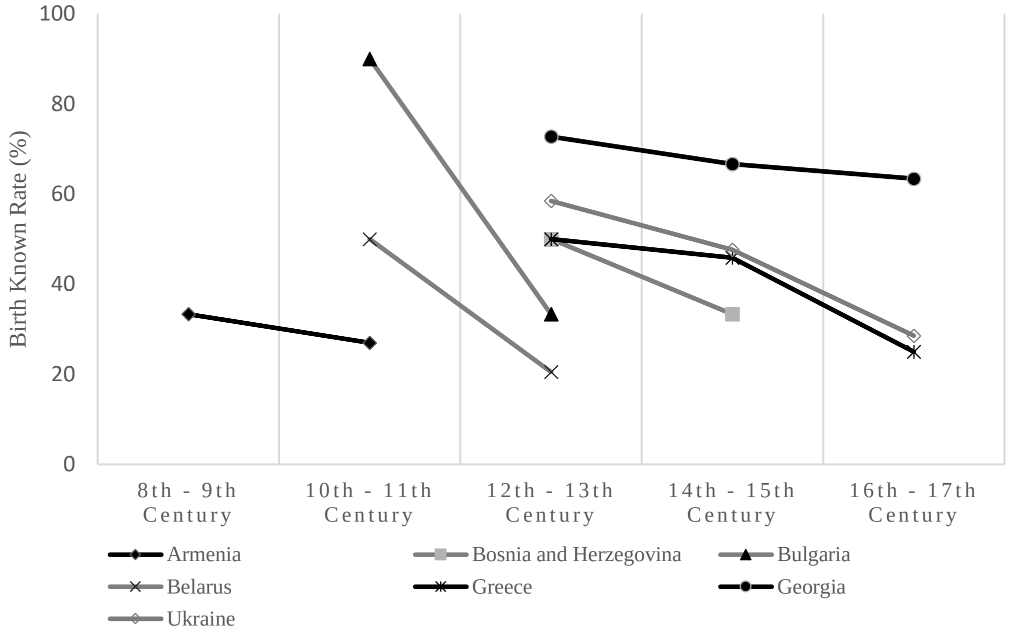

Figure B.18. Examples of Decreasing Elite Human Capital

European Region	600	800	1000	1200	1400	1600	1800
Central	(1)	(6)	69	105	158	120	87
Eastern	56	26	51	155	151	189	108
North-Western	147	162	255	220	150	106	103
South-Eastern	14	53	73	189	331	36	39
South-Western	44	59	145	97	235	233	93

Table B.1. Number of Cases

Note: Central Europe 600 and 800 are not included in the regression analyses.

Variable	Obs	Mean	Std.Dev.	Min	Max
Birth Known	227	0.69	0.32	0	1
Regicide	226	0.18	0.18	0	1
Battle	226	0.06	0.08	0	0.40
Urbanisation	227	0.08	0.10	0	0.63
Pasture Area	202	0.11	1.39	-0.17	16.32
Crop Area	202	0.11	1.36	-0.19	15.58
Mode of Succession					
● Partially Elected	227	0.05	0.22	0	1
● Fully Elected	227	0.11	0.31	0	1
Autonomy	227	0.63	0.48	0	1
Reign Length	227	16.21	5.88	3.67	43.25
Area	227	292958	426134	0	2618188
Second Serfdom	227	0.10	0.30	0	1
Proximity to Central Asia	227	0.18	0.03	0.12	0.28
Religion					
● Catholicism	227	0.53	0.50	0	1
● Islam	227	0.07	0.26	0	1
● Orthodoxy	227	0.27	0.45	0	1
● Protestant	227	0.08	0.27	0	1
● Other	227	0.04	0.20	0	1
Religious Diversity	227	0.34	0.47	0	1
Jewish Minority	227	0.39	0.49	0	1
Ruggedness	227	1.44	1.23	0.04	6.61
Latitude	227	48.21	6.92	35.05	64.99
Longitude	227	17.78	20.24	-18.59	96.71
% Fertile Soil	227	51.98	19.26	0	88.65
% Within 100 km. of Ice-Free Coast	227	41.90	34.05	0	100

Table B.2. Descriptive Statistics

Note: Measured using country-century units. Pasture area and crop area are indices per capita, per square kilometre. Area is set to zero if the kingdom is not autonomous since the ruler does not control it personally.

	(1) Birth Known	(2) Birth Known	(3) Birth Known	(4) Birth Known
Kingdom Area	-1.56e-08	-1.67e-08	-1.72e-08	**-6.73e-08***
	(3.89e-08)	(3.99e-08)	(3.96e-08)	(3.51e-08)
Reign Length		0.00298	0.00298	-0.000405
		(0.00296)	(0.00294)	(0.00370)
Autonomy			0.00715	-0.0332
			(0.0561)	(0.0682)
Constant	**0.326*****	**0.291****	**0.285****	**0.621*****
	(0.116)	(0.112)	(0.132)	(0.193)
Observations	227	227	227	201
Adjusted R^2	0.386	0.386	0.383	0.439
Explanatory Variables	NO	NO	NO	YES
Country Fes	YES	YES	YES	YES
Time Fes	YES	YES	YES	YES

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.3. Regressions of Elite Numeracy on Elite Controls

	(1) Birth Known	(2) Birth Known	(3) Birth Known	(4) Birth Known	(5) Birth Known	(6) Birth Known	(7) Birth Known
Regicide	**-0.416*** **	**-0.429*** **	**-0.427*** **	**-0.436*** **	**-0.436*** **	**-0.509*** **	**-0.474*** **
	(0.139)	(0.143)	(0.141)	(0.152)	(0.152)	(0.138)	(0.125)
Battle	**-0.686*** **	**-0.698*** **	**-0.703*** **	**-0.694*** **	**-0.686*** **	**-0.665** **	**-0.661** **
	(0.214)	(0.218)	(0.216)	(0.249)	(0.247)	(0.244)	(0.254)
Urbanisation		-0.216	-0.217	-0.206	-0.197	-0.227	-0.174
		(0.203)	(0.203)	(0.220)	(0.220)	(0.214)	(0.178)
Mode of Succession (Base=Hereditary)*:*							
● Partially Elected			-0.0833	-0.0251	-0.0269	-0.0337	0.00838
			(0.0726)	(0.0669)	(0.0666)	(0.0760)	(0.0897)
● Fully Elected			0.0247	0.00864	0.00866	-0.0122	-0.0168
			(0.0875)	(0.0899)	(0.0897)	(0.0837)	(0.0849)
Pasture Area				0.0151		**0.342*** **	**0.338*** **
				(0.0105)		(0.0789)	(0.0787)
Crop Area					0.00769	**-0.362*** **	**-0.363*** **
					(0.00824)	(0.0813)	(0.0831)
Second Serfdom	-0.0277	-0.0431	-0.0431	-0.0364	-0.0334	-0.0775	-0.0620
	(0.0759)	(0.0790)	(0.0792)	(0.0853)	(0.0859)	(0.0834)	(0.0841)
Constant	**0.562*** **	**0.574*** **	**0.568*** **	**0.598*** **	**0.595*** **	**0.643*** **	**0.549*** **
	(0.157)	(0.161)	(0.170)	(0.199)	(0.199)	(0.193)	(0.139)
Observations	226	226	226	201	201	201	201
Adjusted R^2	0.458	0.458	0.453	0.419	0.417	0.440	0.439
Elite Controls	YES	YES	YES	YES	YES	YES	NO
Country FEs	YES	YES	YES	YES	YES	YES	YES
Time FEs	YES	YES	YES	YES	YES	YES	YES

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.4. Fixed Effects Regressions

Note: The reference category for institutional factors is hereditary succession; for "second serfdom", it is the regions and periods not affected. Since there are 36 clusters when clustering by country, we also crosschecked our results using Cameron et al.'s (2008) wild bootstrap procedure (using 1000 replications). We find very similar results to table B.4. and regicide and battle always remains significant, at least at a 98% confidence level (t-statistics from -2.58 to -3.47 and corresponding p-values from 0.019 to 0.001).

	Hungarian Invasions (9th and 10th centuries)	High Medieval Peace (11th and 12th centuries)	Mongolian Invasions (13th and 14th centuries)
	(1)	(2)	(3)
	LIML	LIML	LIML
	Birth Known	Birth Known	Birth Known
Regicide	**-1.036*** **	-1.001	**-3.183*** **
	(0.328)	(1.237)	(1.101)
Constant	**0.594*** **	**0.811*** **	**1.225*** **
	(0.105)	(0.233)	(0.174)
Observations	14	23	33
Adjusted (Centred) R^2	0.362	-0.301	-2.364
Uncentred R^2	0.795	0.857	0.392
F-Statistic	6.067	0.0597	15.390

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.5. IV Regressions of Elite Numeracy[25]: Comparing Invasion Periods.

[25] See appendix for first stage regressions (tables A.B.12. and A.B.13.).

	(1) LIML Birth Known	(2) LIML Birth Known	(3) LIML Birth Known	(4) LIML Birth Known	(5) LIML Birth Known	(6) LIML Birth Known	(7) LIML Birth Known
Regicide	**-2.105*** **	**-2.113*** **	**-2.363*** **	**-2.235*** **	**-2.005*** **	**-2.010*** **	**-1.777*** **
	(0.762)	(0.758)	(0.878)	(0.802)	(0.712)	(0.717)	(0.642)
Battle		-0.193	-0.258	-0.168	-0.288	-0.283	-0.371
		(0.425)	(0.455)	(0.437)	(0.427)	(0.428)	(0.391)
Urbanisation			-1.085	-0.705	-0.693	-0.705	-0.619
			(0.829)	(0.776)	(0.744)	(0.746)	(0.679)
Mode of Succession (Base=Hereditary)							
● Partially Elected				**0.340** **	**0.374** **	**0.379** **	**0.352** **
				(0.149)	(0.155)	(0.156)	(0.142)
● Fully Elected				-0.0615	-0.150	-0.152	-0.132
				(0.137)	(0.145)	(0.145)	(0.132)
Pasture Area					0.00542		**0.491***
					(0.0220)		(0.256)
Crop Area						0.00297	**-0.508***
						(0.0229)	(0.266)
Constant	**0.813*** **	**0.822*** **	**0.908*** **	**0.878*** **	**0.848*** **	**0.849*** **	**0.797*** **
	(0.139)	(0.135)	(0.170)	(0.163)	(0.157)	(0.159)	(0.143)
Observations	120	120	120	120	106	106	106
Time FEs	YES	YES	YES	YES	YES	YES	YES
Adj. (Centered) R^2	-0.658	-0.678	-0.947	-0.735	-0.487	-0.492	-0.256
Uncentered R^2	0.701	0.701	0.655	0.698	0.726	0.725	0.771
F-Stat	14.31	14.97	16.07	15.32	12.99	12.94	10.69

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.6. Instrumental Variable Regressions (Central Asian Invasions: 800 – 1400 CE)[26]

Note: The reference category for institutional factors is hereditary succession; for “second serfdom”, it is the regions and periods not affected by the experience.

[26] See appendix for first stage regressions.

	(1) Birth Known	(2) Birth Known	(3) Birth Known	(4) Birth Known	(5) Birth Known	(6) Birth Known	(7) Birth Known
Regicide	**-0.392****	**-0.377*****	**-0.385*****	**-0.375****	**-0.374****	**-0.389*****	**-0.383*****
	(0.159)	(0.143)	(0.146)	(0.160)	(0.161)	(0.150)	(0.130)
Battle		**-0.720*****	**-0.729*****	**-0.742*****	**-0.740*****	**-0.757*****	**-0.745*****
		(0.205)	(0.207)	(0.243)	(0.243)	(0.245)	(0.240)
Urbanisation			-0.165	-0.183	-0.183	-0.239	-0.228
			(0.177)	(0.184)	(0.183)	(0.187)	(0.156)
Pasture Area				0.00310		**0.164***	**0.190****
				(0.0123)		(0.102)	(0.0757)
Crop Area					0.00115	**-0.168***	**-0.196****
					(0.0113)	(0.106)	(0.0807)
Mode of Succession (Base=Hereditary)							
● Partially Elected	0.0146	0.00179	0.00260	0.0863	0.0876	0.113	0.0544
	(0.0789)	(0.0744)	(0.0734)	(0.0910)	(0.0937)	(0.110)	(0.0913)
● Fully Elected	-0.00529	-0.00654	-0.00765	-0.0550	-0.0551	-0.0483	-0.0398
	-0.0867	-0.0837	-0.0839	-0.0822	-0.0827	-0.0863	(0.0878)
Second Serfdom	-0.0935	-0.0794	-0.0892	-0.103	-0.103	-0.112	-0.0846
	(0.0709)	(0.0661)	(0.0674)	(0.0685)	(0.0685)	(0.0689)	(0.0693)
Religion							
● Islam	**-0.137***	-0.0948	-0.0977	-0.112	-0.112	-0.111	-0.118
	(0.0744)	(0.0760)	(0.0775)	(0.0800)	(0.0800)	(0.0815)	(0.0742)
● Orthodoxy	**-0.173****	-0.121	-0.124	**-0.196****	**-0.195****	**-0.186****	**-0.143***
	(0.0805)	(0.0744)	(0.0754)	(0.0810)	(0.0810)	(0.0816)	(0.0784)
● Protestantism	-0.0525	-0.0785	-0.0680	-0.0541	-0.0553	-0.0266	-0.0559
	(0.0586)	(0.0542)	(0.0502)	(0.0698)	(0.0702)	(0.0767)	(0.0738)
● Other	**-0.215****	**-0.161****	**-0.158****	**-0.164****	**-0.165****	**-0.148***	**-0.138***
	(0.0874)	(0.0721)	(0.0721)	(0.0807)	(0.0808)	(0.0809)	(0.0785)
Religious Diversity	-0.0389	-0.0517	-0.0506	-0.0548	-0.0550	-0.0594	-0.0474
	(0.0338)	(0.0356)	(0.0358)	(0.0372)	(0.0372)	(0.0386)	(0.0361)
Jewish Minority	**0.0679***	**0.0804****	**0.0867****	**0.128*****	**0.127*****	**0.125*****	**0.119*****
	(0.0367)	(0.0358)	(0.0377)	(0.0382)	(0.0383)	(0.0385)	(0.0384)
Ruggedness	-0.0284	-0.0232	-0.0260	-0.0386	-0.0385	-0.0436*	-0.0446
	(0.0267)	(0.0248)	(0.0244)	(0.0244)	(0.0243)	(0.0262)	(0.0282)
Latitude	0.00231	0.00589	0.00527	0.000631	0.000689	-0.00108	-8.86e-05
	(0.00680)	(0.00696)	(0.00680)	(0.00722)	(0.00722)	(0.00765)	(0.00738)
Longitude	-0.000511	-0.00234	-0.00226	-0.000955	-0.000992	-0.000689	-0.00135
	(0.00170)	(0.00156)	(0.00158)	(0.00163)	(0.00163)	(0.00159)	(0.00151)
% Fertile soil	-0.00180	-0.00227	-0.00232	**-0.00379***	**-0.00377***	**-0.00392***	**-0.00400***
	(0.00211)	(0.00205)	(0.00204)	(0.00199)	(0.00199)	(0.00209)	(0.00208)
% Within 100 km. of ice-free coast	-0.000280	-0.000422	-0.000338	-0.000169	-0.000166	-9.82e-07	0.000352
	(0.000978)	(0.000949)	(0.000934)	(0.000942)	(0.000943)	(0.000978)	(0.00104)
Constant	0.582	0.512	0.545	**0.816***	**0.813***	**0.901***	**0.803***
	(0.451)	(0.448)	(0.443)	(0.463)	(0.462)	(0.483)	(0.472)
Observations	226	226	226	201	201	201	201
Overall R^2	0.506	0.527	0.525	0.548	0.549	0.552	0.558
Country FEs	NO	NO	NO	NO	NO	NO	NO
Time FEs	YES	YES	YES	YES	YES	YES	YES
Elite Controls	YES	YES	YES	YES	YES	YES	NO

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.7. Random Effects Regressions

Note: The reference category for institutional factors is hereditary succession; for “second serfdom”, it is the regions and periods not affected by the experience.

B.10. Appendices

B.10.1. Regional Classifications

Since there are no universal standards for assigning countries to European sub-regions, some of our classifications may seem unorthodox. However, in these cases their allocations follow historical narratives. For example, some may suggest that Lithuania and Latvia be defined as Eastern European countries because of their shared histories with the Russian Empire and the Soviet Union, or else Central Europe because of their participation in the Kingdom of Prussia or the Polish-Lithuanian Commonwealth. However, being countries that were heavily influenced by Baltic trade and by the Swedish Empire in the 17th and 18th centuries, we assign them to Scandinavia as a compromise. Moreover, they exhibit trends that are more in line with Scandinavia than either Eastern- or Central European countries. These include high rates of regicide in the High and late Middle Ages before exhibiting a sharp decline, as well as early development in elite numeracy.

Greater Region	Narrower Region	Countries and Regional Abbreviations
Central Europe	German speaking	Austria (at), Germany (de)
	East-Central Europe	Czech Republic (cz), Hungary (hu), Poland (pl)
Eastern Europe	Caucasus	Armenia (am), Georgia (ge)
	Romania	Romania (ro)
	Russia	Belarus (by), Russia (ru), Ukraine (ua)
North-Western Europe	Benelux	Belgium (be), Luxembourg (lu), Netherlands (nl)
	France	France (fr), Monaco (mc)
	Scandinavia	Denmark (dk), Iceland (is), Lithuania (lt), Latvia (lv), Norway (no), Sweden (se)
	United Kingdom and Ireland	Ireland (ie), United Kingdom (uk)
South-Eastern Europe	Greece	Cyprus (cy), Greece (el)
	Turkey*	Turkey (tr)
	Balkans	Albania (al), Bosnia and Herzegovina (ba), Bulgaria (bg), Croatia (hr), Montenegro (me), Serbia (rs)
South-Western Europe	Iberia	Portugal (pt), Spain (es)
	Italy	Italy (it)

Table B.8. Aggregation of European Countries to Broader Regions

**Note*: Early Turkey refers to the East Roman (Byzantine) Empire

B.10.2. Unit Root Tests

Although all of our regression specifications include time fixed effects, the presence of non-stationary series may mean that our regressions capture spurious relationships and invalidate our inferences. Since we have an unbalanced panel with gaps in certain individual time series, a unit root meta-analysis, such as a Fisher-type test, needs to be carried out. We use both the Augmented Dickey-Fuller and the Phillips-Perron tests before conducting our Fisher-type meta-analysis.

Table A.B.2. shows that, among our variables of interest, only elite numeracy and battle deaths display any kind of non-stationarity, and only with a 200 year lag or longer. Since we use 200 year fixed effects, unit roots should not have affected our results. Of course, variables like urbanisation rates are non-stationary by nature, but these are only used as control variables in this study.

Test	Lags	Regicide		Elite Numeracy		Battle	
		χ^2 (df)	P-Value	χ^2 (df)	P-Value	χ^2 (df)	P-Value
ADF	0	chi2(70) = 322.36	0.0000	chi2(70) = 95.81	0.0220	chi2(70) = 490.53	0.0000
ADF	1	chi2(64) = 215.55	0.0000	chi2(66) = 155.08	0.0000	chi2(64) = 83.64	0.0503
ADF	2	chi2(62) = 86.09	0.0232	chi2(62) = 33.23	**0.9990**	chi2(62) = 23.37	**1.0000**
ADF	3	chi2(48) = 111.57	0.0000	chi2(48) = 11.69	**1.0000**	chi2(48) = 27.34	**0.9929**
Phillips–Perron	0	chi2(70) = 320.60	0.0000	chi2(70) = 95.81	0.0220	chi2(70) = 490.53	0.0000
Phillips–Perron	1	chi2(70) = 382.91	0.0000	chi2(70) = 91.87	0.0410	chi2(70) = 427.13	0.0000
Phillips–Perron	2	chi2(70) = 292.71	0.0000	chi2(70) = 101.65	0.0080	chi2(70) = 447.94	0.0000
Phillips–Perron	3	chi2(70) = 330.39	0.0000	chi2(70) = 115.35	0.0005	chi2(70) = 470.56	0.0000

H_0: Series contains a unit-root

Table B.9. Unit Root Tests

B.10.3. Spatial Regressions

As mentioned in the main text, while the results from our fixed effects specification provide a solid point of departure for our co-evolution hypothesis, we must acknowledge the role that spatial autocorrelation may have played. Kelly (2019) recently argued that many results in the persistence literature could have arisen from random spatial patterns and that the likelihood of this problem is greater if the effects of spatial autocorrelation are not controlled. Our study is less affected by this issue because our explanatory and dependent variables are coded for contemporaneous time units, but we still need to control for spatial autocorrelation. Spurious relationships may form due to numeracy or violence spillovers rather than as a result of truly economic interactions. Here, we make use of spatial econometric techniques, first formalised by Jean Paelinck and Leo Klaasen (1979), to combat these effects, which may be particularly important in our study because disparities in levels of development between Eastern and Western Europe could conceivably have driven our earlier results.

We first constructed an inverse distance weighting matrix based on the coordinates of the geographic centroids of our geographical units from Donnelly (2012). In this way, our models control for spatial effects in a linear manner – with neighbouring countries having a greater weight than those further away – as opposed to only capturing the effects of immediate neighbours or using an alternative system with an unequal weighting mechanism that reflects historical characteristics, for example.

Because spatial methods require a weighting matrix to link each observation of the dependent variable to every contemporaneous observation from a different geographical unit's dependent and independent variables, they require strongly balanced panels. Unfortunately, as with most studies in social science, we do not have a perfectly balanced panel and must resort to an alternative strategy. This is a common problem in the spatial econometrics literature, with researchers either having to drop all panels with any missing data whatsoever or having to revert to imputation (for sources on multiple imputation in spatial econometrics, see Griffith and

Paelinck 2011; Griffith et al. 1989; Bihrmann and Ersbøll 2015; Stein 1999; LeSage and Pace 2004; and Baker et al. 2014, among others).

To perform our imputation, we used Stata's *mi* command with its multivariate regression option, using this statistical simulation technique to effectively create 50 new datasets of predicted values for each panel. The following analysis is then performed on each simulated dataset separately before the results are pooled using Rubin's Rule (Rubin 1987).

According to Rubin (1987), these estimates afford valid inferences despite the increased sample size of the underlying analysis, provided that data are missing at random. Because the availability of our data improves over time and is itself associated with development in numeracy, as discussed above, we cannot make this claim. Therefore, before proceeding with our imputed spatial analysis, we first run the following models on the two panels where we have the most observations, 1300 and 1400 (tables A.B.5. and A.B.6.), observing results that are remarkably analogous and lead us to believe in the validity of our imputed spatial results.

Our spatial analysis utilises the three most simple spatial econometric models, the Spatial Autoregressive Model (SAR Model; equation 2, table A.B.3.), the Spatially Lagged X Model (SLX Model; equation 3, table A.B.4.) and the Spatial Error Model (SEM; equation 4, table A.B.3.).

$$y_{it} = \rho \boldsymbol{W} y_{it} + \boldsymbol{X_{it}} \beta + a_i + \varepsilon_{it} \tag{2}$$

$$y_{it} = \boldsymbol{X_{it}} \beta + \boldsymbol{W X_{it}} \Theta + a_i + \varepsilon_{it} \tag{3}$$

$$y_{it} = \boldsymbol{X_{it}} \beta + a_i + u_{it}, u_{it} = \lambda \boldsymbol{W} u_{it} + \varepsilon_{it}, where\ \varepsilon_{it} \sim i.i.d. \tag{4}$$

where y_{it} is a vector for the elite numeracy variable in time period t; $\boldsymbol{X_{it}}$ is a matrix of all time-varying regressors for time period t; a_i is a vector of country fixed effects; ε_{it} is a vector of spatially lagged errors; u_{it} is a stochastic error term; $\boldsymbol{W}$ is an inverse distance weighting matrix constructed using the coordinates of modern geographic country centroids; β is a vector of ordinary regression coefficients; and ρ, Θ and λ are coefficients of the spatial characteristics described below.

The SAR model controls for the direct effect that variation in the dependent variable of other countries may have on country i (measured by ρ) i.e. the effect of elite numeracy spillovers from neighbours. Likewise, the SLX model controls for spillover effects from the independent variables of other countries (measured by Θ), such as the effect of neighbouring elite violence on elite numeracy in country i. Last, the SEM model controls for any effect that unexplained variation from other countries may have on elite numeracy in country i (measured by λ), such as the effect of an omitted variable. While more complex models can be estimated, these often suffer from multicollinearity, or else fail to converge (Burkey 2017).[27] Additionally, our estimates of ρ, Θ and λ from each of these simpler specifications indicate that spatial correlation is not very influential in our analysis (tables A.B.3. and A.B.4.).

Our results show similar coefficients for regicide and battles, although these are surprisingly somewhat larger (in absolute terms) than those from the fixed effects specification in section B.6.1. (equation 1, table B.4.); between approximately -0.6 and -0.8 for regicide, and -0.75 to -0.9 for battles. Further, the coefficient for urbanisation is positive and significant, between 0.5 and 1.0, and while no other coefficients are significant in the SAR and SEM models, additional coefficients in the SLX model turn out significant. The SLX model shows a positive and significant coefficient of approximately 0.05 for more participative succession systems, while the coefficients for pasture and crop areas fall in line with the fixed effects results, although they are only approximately half as large. The regicide and battle coefficients

[27] For example: The Spatial Durbin Model (SDM; LeSage and Pace, 2009) simultaneously captures spillover effects from neighbouring dependent and independent variables, the Kelejian-Prucha Model (Kelejian and Prucha, 1998) considers spillovers from the dependent variable and error term, while all three spatial terms are included in the Manski Model (Manski, 1993).

may be larger, partially because none of the spatial models converged when time fixed effects were also included, leading to their unfortunate omission. However, in order to ensure that the omission of time dummies is not driving our results, we run all three spatial models in first differences, bringing our results more in line with those from the fixed effects specification from equation 1. Under first differences, each of the models yield regicide and battle coefficients that are approximately 30-40% smaller than under equation 1, while pasture and crop areas provide similar trends. In addition, the SLX model shows a negative and significant coefficient of approximately -0.15 for the second serfdom dummy.

Although the results from these spatial regressions provide undoubtedly interesting interpretations, they are remarkably similar to those from the fixed effect model (equation 1). Additionally, the Θ parameter is never significant, and the ρ and λ parameters are insignificant in all but a few specifications. This leads us to believe that despite limited evidence of dependent variable and error term spillovers across countries, spatial autocorrelation is not a notable source of endogeneity in this study.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Birth Known	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem
Regicide	**-0.782*** **	**-0.778*** **	**-0.691*** **	**-0.689*** **	**-0.661*** **	**-0.661*** **	**-0.604*** **	**-0.604*** **	**-0.577*** **	**-0.577*** **	**-0.580*** **	**-0.581*** **	**-0.579*** **	**-0.579*** **	**-0.585*** **	**-0.585*** **	**-0.614*** **	**-0.614*** **
	(0.130)	(0.130)	(0.128)	(0.129)	(0.124)	(0.124)	(0.128)	(0.128)	(0.124)	(0.124)	(0.122)	(0.122)	(0.122)	(0.122)	(0.121)	(0.121)	(0.124)	(0.123)
Battle					**-0.905*** **	**-0.913*** **	**-0.814*** **	**-0.823*** **	**-0.807*** **	**-0.816*** **	**-0.814*** **	**-0.823*** **	**-0.811*** **	**-0.820*** **	**-0.836*** **	**-0.844*** **	**-0.771*** **	**-0.784*** **
					(0.254)	(0.258)	(0.253)	(0.256)	(0.247)	(0.250)	(0.249)	(0.251)	(0.248)	(0.251)	(0.244)	(0.247)	(0.248)	(0.251)
Urbanisation							**0.516** **	**0.525** **	**0.549** **	**0.560** **	**0.552** **	**0.562** **	**0.550** **	**0.560** **	**0.537** **	**0.547** **	**0.738*** **	**0.743*** **
							(0.236)	(0.237)	(0.240)	(0.241)	(0.238)	(0.239)	(0.239)	(0.240)	(0.241)	(0.242)	(0.233)	(0.235)
Mode of Succession									0.0582	0.0575	0.0572	0.0564	0.0573	0.0566	0.0578	0.0569	0.0483	0.0474
									(0.0356)	(0.0356)	(0.0353)	(0.0354)	(0.0354)	(0.0355)	(0.0353)	(0.0354)	(0.0359)	(0.0358)
Pasture Area											0.00510	0.00506			0.112	0.109	0.0792	0.0766
											(0.0178)	(0.0179)			(0.121)	(0.121)	(0.120)	(0.120)
Crop Area													0.00375	0.00374	-0.110	-0.107	-0.0750	-0.0725
													(0.0181)	(0.0181)	(0.123)	(0.123)	(0.122)	(0.122)
Second Serfdom	0.0197	0.0190	0.0305	0.0342	0.0279	0.0325	0.0294	0.0353	0.0301	0.0372	0.0308	0.0378	0.0308	0.0378	0.0287	0.0356	0.0174	0.0224
	(0.0637)	(0.0707)	(0.0613)	(0.0660)	(0.0574)	(0.0607)	(0.0560)	(0.0582)	(0.0562)	(0.0582)	(0.0567)	(0.0586)	(0.0567)	(0.0585)	(0.0574)	(0.0593)	(0.0616)	(0.0647)
Observations	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504
Country FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Elite Controls	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	NO	NO
Rho	**0.274** **		**0.237* **		**0.206* **		0.176		0.171		0.170		0.170		0.170		0.193	
	(0.127)		(0.125)		(0.124)		(0.125)		(0.125)		(0.126)		(0.125)		(0.126)		(0.129)	
Lambda		**0.268* **		0.225		0.201		0.173		0.154		0.153		0.154		0.149		0.192
		(0.141)		(0.139)		(0.139)		(0.146)		(0.150)		(0.154)		(0.153)		(0.154)		(0.152)
Sigma2_e	**0.0798*** **	**0.0800*** **	**0.0736*** **	**0.0738*** **	**0.0682*** **	**0.0684*** **	**0.0663*** **	**0.0664*** **	**0.0649*** **	**0.0651*** **	**0.0646*** **	**0.0647*** **	**0.0646*** **	**0.0648*** **	**0.0641*** **	**0.0643*** **	**0.0687*** **	**0.0688*** **
	(0.00896)	(0.00897)	(0.00830)	(0.00828)	(0.00789)	(0.00789)	(0.00783)	(0.00784)	(0.00770)	(0.00770)	(0.00766)	(0.00767)	(0.00768)	(0.00768)	(0.00758)	(0.00758)	(0.00804)	(0.00804)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.10. Spatial Fixed Effects Regressions: Spatial Autoregressive (SAR) and Spatial Error (SEM) Models

Note: The reference category for institutional factors is hereditary succession; for “second serfdom”, it is the regions and periods not affected by the experience.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Birth Known	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ
Regicide	**-0.743*** **	-0.31	**-0.663*** **	-0.375	**-0.639*** **	-0.267	**-0.583*** **	-0.13	**-0.555*** **	-0.133	**-0.559*** **	-0.082	**-0.558*** **	-0.094	**-0.563*** **	-0.053	**-0.59*** **	0.012
	(0.074)	(0.394)	(0.076)	(0.428)	(0.074)	(0.428)	(0.074)	(0.446)	(0.074)	(0.451)	(0.075)	(0.456)	(0.075)	(0.456)	(0.074)	(0.460)	(0.074)	(0.431)
Battle					**-0.815*** **	0.068	**-0.727*** **	0.518	**-0.727*** **	0.557	**-0.733*** **	0.538	**-0.73*** **	0.534	**-0.759*** **	0.654	**-0.715*** **	0.766
					(0.156)	(0.713)	(0.155)	(0.767)	(0.154)	(0.767)	(0.154)	(0.779)	(0.154)	(0.780)	(0.154)	(0.785)	(0.158)	(0.771)
Urbanisation							**0.539*** **	0.499	**0.576*** **	0.515	**0.583*** **	0.64	**0.58*** **	0.621	**0.575*** **	0.658	**0.766*** **	**0.982** **
							(0.146)	(0.653)	(0.146)	(0.657)	(0.146)	(0.677)	(0.146)	(0.677)	(0.146)	(0.683)	(0.141)	(0.625)
Mode of Succession									**0.052*** **	-0.009	**0.052*** **	0.025	**0.052*** **	-0.002	**0.054*** **	0.026	**0.043*** **	-0.002
									(0.020)	(0.118)	(0.020)	(0.120)	(0.020)	(0.120)	(0.020)	(0.122)	(0.020)	(0.119)
Pasture Area											0.006	0.025			**0.163*** **	0.527	**0.134*** **	0.345
											(0.009)	(0.040)			(0.078)	(0.44)	(0.080)	(0.415)
Crop Area													0.004	0.022	**-0.162*** **	-0.528	**-0.132** **	-0.326
													(0.009)	(0.042)	(0.081)	(0.457)	(0.082)	(0.431)
Second Serfdom	0.027	0.156	0.052	0.099	0.054	0.074	0.068	-0.004	0.073	-0.022	0.072	-0.017	0.072	-0.021	0.063	0.006	0.051	0.004
	(0.073)	(0.164)	(0.071)	(0.162)	(0.069)	(0.159)	(0.068)	(0.166)	(0.068)	(0.169)	(0.067)	(0.171)	(0.067)	(0.171)	(0.067)	(0.174)	(0.069)	(0.175)
Observations	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504
Country FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Elite Controls	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	NO	NO
Sigma2_e	**0.291*** **		**0.278*** **		**0.269*** **		**0.264*** **		**0.261*** **		**0.26*** **		**0.261*** **		**0.259*** **		**0.268*** **	
	(0.010)		(0.009)		(0.009)		(0.009)		(0.009)		(0.009)		(0.009)		(0.008)		(0.009)	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.11. Spatial Fixed Effects Regressions: Spatially Lagged X Model (SLX)

Note: The theta (Θ) columns indicate the coefficients for each spatially lagged independent variable. This shows that the spatial independent variable spillovers from other countries are insignificant, while the direct effect of the regressors from within countries can be interpreted as usual from the columns labelled *slx*.

The reference category for institutional factors is hereditary succession; for “second serfdom”, it is the regions and periods not affected by the experience.

Birth Known	(1) sar	(2) sar	(3) sar	(4) sar	(5) sar	(6) sar	(7) sar	(8) sar	(9) sar
Regicide	-0.491	-0.334	-0.331	-0.328	-0.303	**-1.200*** **	**-1.038** **	**-1.160*** **	**-1.000*** **
	(0.304)	(0.307)	(0.307)	(0.307)	(0.313)	(0.455)	(0.474)	(0.434)	(0.299)
Battle			0.115	0.0847	0.104	-0.239	0.0565	-0.803	-0.614
			(0.692)	(0.706)	(0.706)	(0.648)	(0.659)	(0.709)	(0.702)
Urbanisation				-0.118	-0.0666	0.275	0.239	0.217	0.133
				(0.689)	(0.701)	(0.627)	(0.657)	(0.597)	(0.601)
Mode of Succession					0.0368	-0.119	-0.0909	-0.114	-0.0813
					(0.0949)	(0.104)	(0.108)	(0.0990)	(0.0828)
Pasture Area						**0.263*** **		**0.845** **	**0.843** **
						(0.0927)		(0.374)	(0.387)
Crop Area							**0.242** **	-0.653	-0.694
							(0.106)	(0.407)	(0.424)
Second Serfdom	**0.637*** **	0.332	0.315	0.334	0.306	**0.909*** **	**0.763** **	**0.998*** **	**0.943*** **
	(0.200)	(0.234)	(0.260)	(0.271)	(0.282)	(0.326)	(0.327)	(0.315)	(0.204)
Observations	26	26	26	26	26	24	24	24	24
Country FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO
Elite Controls	NO	YES	YES	YES	YES	YES	YES	YES	NO
Rho	0.212	0.0850	0.0947	0.0788	0.0685	-0.171	-0.0954	-0.280	-0.121
	(0.285)	(0.298)	(0.313)	(0.315)	(0.313)	(0.296)	(0.306)	(0.290)	(0.256)

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.12. Spatial Regression without Interpolation (Cross Section: 1300)

Note: Although the regicide coefficients in the first five specifications are imprecisely measured due to a very small sample, the sign remains negative and the coefficient is nevertheless quite substantial.

Birth Known	(1) sar	(2) sar	(3) sar	(4) sar	(5) sar	(6) sar	(7) sar	(8) sar	(9) sar
Regicide	**-0.923*** **	**-1.112*** **	**-1.031*** **	**-0.818** **	**-0.868** **	**-1.183*** **	**-1.170*** **	**-1.222*** **	**-1.030*** **
	(0.317)	(0.323)	(0.316)	(0.326)	(0.346)	(0.367)	(0.366)	(0.365)	(0.345)
Battle			-0.932	-0.926	-0.945	-1.037	-1.008	**-1.171***	-0.848
			(0.616)	(0.587)	(0.589)	(0.656)	(0.653)	(0.668)	(0.575)
Urbanisation				**1.817***	1.704	0.787	0.808	0.549	0.955
				(1.085)	(1.113)	(1.163)	(1.167)	(1.185)	(1.138)
Mode of Succession					-0.0337	-0.167	-0.164	**-0.177***	**-0.176***
					(0.0831)	(0.102)	(0.102)	(0.101)	(0.103)
Pasture Area						0.0301		0.320	0.105
						(0.0409)		(0.362)	(0.337)
Crop Area							0.0245	-0.271	-0.0875
							(0.0382)	(0.337)	(0.317)
Second Serfdom	**0.899*** **	**0.664** **	**0.559***	0.363	0.411	**0.851** **	**0.849** **	**0.897** **	**1.027*** **
	(0.181)	(0.307)	(0.304)	(0.312)	(0.325)	(0.377)	(0.378)	(0.376)	(0.227)
Observations	27	27	27	27	27	24	24	24	24
Country FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO
Elite Controls	NO	YES	YES	YES	YES	YES	YES	YES	NO
Rho	-0.0465	-0.128	0.0575	0.134	0.138	-0.207	-0.193	-0.273	-0.178
	(0.270)	(0.281)	(0.299)	(0.289)	(0.290)	(0.359)	(0.358)	(0.363)	(0.292)

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.13. Spatial Regression without Interpolation (Cross Section: 1400)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
ΔBirth Known	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem	sar	sem
ΔRegicide	**-0.295****	**-0.295****	**-0.301****	**-0.301****	**-0.299*****	**-0.299*****	**-0.298*****	**-0.298****	**-0.297****	**-0.297****	**-0.296*****	**-0.296*****	**-0.308*****	**-0.307*****	**-0.342*****	**-0.342*****	**-0.334*****	**-0.334*****
	(0.118)	(0.118)	(0.118)	(0.119)	(0.114)	(0.115)	(0.116)	(0.116)	(0.116)	(0.117)	(0.114)	(0.115)	(0.112)	(0.113)	(0.113)	(0.113)	(0.114)	(0.114)
ΔBattle					**-0.422****	**-0.420****	**-0.421****	**-0.418****	**-0.421****	**-0.418****	**-0.392***	**-0.389***	**-0.377***	**-0.374***	**-0.382***	**-0.379***	**-0.381***	**-0.378***
					(0.197)	(0.198)	(0.198)	(0.198)	(0.197)	(0.198)	(0.201)	(0.202)	(0.201)	(0.202)	(0.199)	(0.200)	(0.195)	(0.196)
ΔUrbanisation							0.0102	0.00776	0.00258	0.000392	0.0214	0.0192	0.0310	0.0289	0.0317	0.0298	0.0131	0.0107
							(0.268)	(0.269)	(0.264)	(0.265)	(0.262)	(0.263)	(0.259)	(0.259)	(0.257)	(0.258)	(0.251)	(0.252)
ΔMode of Succession									-0.0150	-0.0147	-0.0152	-0.0150	-0.0149	-0.0146	-0.0132	-0.0130	-0.0176	-0.0174
									(0.0305)	(0.0306)	(0.0300)	(0.0301)	(0.0295)	(0.0296)	(0.0288)	(0.0288)	(0.0291)	(0.0291)
ΔPasture Area											-0.111	-0.111			0.507	0.507	0.504	0.505
											(0.0906)	(0.0911)			(0.342)	(0.343)	(0.350)	(0.352)
ΔCrop Area													**-0.138***	**-0.138***	**-0.549***	**-0.548***	**-0.542***	**-0.543***
													(0.0787)	(0.0794)	(0.295)	(0.296)	(0.303)	(0.304)
ΔSecond Serfdom	0.0124	0.0110	0.0120	0.0107	0.00356	0.00294	0.00335	0.00261	0.00324	0.00258	0.00312	0.00251	0.00337	0.00279	0.00474	0.00428	0.00493	0.00445
	(0.0586)	(0.0561)	(0.0581)	(0.0557)	(0.0569)	(0.0546)	(0.0568)	(0.0545)	(0.0563)	(0.0540)	(0.0558)	(0.0538)	(0.0555)	(0.0535)	(0.0550)	(0.0530)	(0.0553)	(0.0532)
Observations	468	468	468	468	468	468	468	468	468	468	468	468	468	468	468	468	468	468
Country FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Elite Controls	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	NO	NO
Rho	-0.139		-0.141		-0.150		-0.150		-0.150		-0.148		-0.148		-0.147		-0.145	
	(0.193)		(0.189)		(0.190)		(0.189)		(0.189)		(0.187)		(0.186)		(0.187)		(0.189)	
Lambda		-0.134		-0.139		-0.134		-0.135		-0.137		-0.131		-0.128		-0.126		-0.124
		(0.198)		(0.195)		(0.196)		(0.193)		(0.194)		(0.193)		(0.191)		(0.193)		(0.191)
Sigma2_e	**0.0465*****	**0.0465*****	**0.0457*****	**0.0457*****	**0.0434*****	**0.0435*****	**0.0432*****	**0.0433*****	**0.0430*****	**0.0431*****	**0.0426*****	**0.0426*****	**0.0422*****	**0.0423*****	**0.0415*****	**0.0415*****	**0.0421*****	**0.0421*****
	(0.00632)	(0.00630)	(0.00603)	(0.00602)	(0.00529)	(0.00529)	(0.00522)	(0.00522)	(0.00518)	(0.00518)	(0.00519)	(0.00519)	(0.00520)	(0.00520)	(0.00516)	(0.00516)	(0.00527)	(0.00527)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.14. Spatial Fixed Effects in First Differences: Spatial Autoregressive (SAR) and Spatial Error (SEM) Models

Note: The reference category for institutional factors is hereditary succession; for "second serfdom", it is the regions and periods not affected by the experience.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
ΔBirth Known	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ	slx	Θ
ΔRegicide	**-0.254*** **	0.033	**-0.261*** **	0.032	**-0.271*** **	0.083	**-0.278*** **	0.084	**-0.28*** **	0.093	**-0.298*** **	0.104	**-0.322*** **	0.117	**-0.355*** **	0.137	**-0.343*** **	0.113
	(0.052)	(0.248)	(0.052)	(0.267)	(0.051)	(0.263)	(0.052)	(0.274)	(0.052)	(0.276)	(0.052)	(0.284)	(0.052)	(0.288)	(0.053)	(0.3)	(0.053)	(0.28)
ΔBattle					**-0.456*** **	0.373	**-0.455*** **	0.381	**-0.453*** **	0.402	**-0.395*** **	0.382	**-0.383*** **	0.381	**-0.395*** **	0.376	**-0.397*** **	0.342
					(0.097)	(0.464)	(0.098)	(0.483)	(0.098)	(0.488)	(0.098)	(0.522)	(0.097)	(0.517)	(0.097)	(0.522)	(0.097)	(0.5)
ΔUrbanisation							-0.09	0.085	-0.089	0.046	-0.058	0.055	-0.044	0.049	-0.05	0.02	-0.049	-0.155
							(0.162)	(0.911)	(0.161)	(0.922)	(0.16)	(0.922)	(0.159)	(0.915)	(0.158)	(0.931)	(0.153)	(0.87)
ΔMode of Succession									0.007	0.032	0.004	0.033	0.003	0.037	0.003	0.033	-0.003	0.007
									(0.02)	(0.104)	(0.019)	(0.105)	(0.019)	(0.104)	(0.019)	(0.106)	(0.018)	(0.092)
ΔPasture Area											**-0.179*** **	0.090			**0.409** **	-0.248	**0.408** **	-0.232
											(0.055)	(0.275)			(0.188)	(1.131)	(0.187)	(1.054)
ΔCrop Area													**-0.187*** **	0.093	**-0.512*** **	0.301	**-0.499*** **	0.303
													(0.045)	(0.221)	(0.155)	(0.915)	(0.154)	(0.862)
ΔSecond Serfdom	**-0.164*** **	0.086	**-0.158*** **	0.079	**-0.14*** **	0.066	**-0.141*** **	0.071	**-0.137*** **	0.079	**-0.134*** **	0.078	**-0.137*** **	0.082	**-0.145*** **	0.086	**-0.154*** **	0.086
	(0.046)	(0.193)	(0.047)	(0.202)	(0.046)	(0.199)	(0.046)	(0.202)	(0.047)	(0.207)	(0.046)	(0.208)	(0.046)	(0.206)	(0.046)	(0.209)	(0.045)	(0.199)
Observations	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504	504
Country FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Elite Controls	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	NO	NO
Sigma2_e	**0.238*** **		**0.235*** **		**0.229*** **		**0.228*** **		**0.226*** **		**0.222*** **		**0.220*** **		**0.218*** **		**0.221*** **	
	(0.008)		(0.008)		(0.008)		(0.008)		(0.008)		(0.008)		(0.007)		(0.007)		(0.008)	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.15. Spatial Fixed Effects in First Differences: Spatially Lagged X Model (SLX)

Note: The theta (Θ) columns indicate the coefficients for each spatially lagged independent variable. This shows that the spatial independent variable spillovers from other countries are insignificant, while the direct effect of the regressors from within countries can be interpreted as usual from the columns labelled slx.

Note: The reference category for institutional factors is hereditary succession; for “second serfdom”, it is the regions and periods not affected by the experience.

B.10.4. Using Predicted Values

To test whether collinearity between our variables that could potentially alleviate bias (from table B.3.) and variables of interest has any effect on the relationships we obtained, we run a regression specification using predicted values for elite numeracy. We first regress elite numeracy on our variables that could potentially alleviate bias before regressing the predicted values from this regression on our variables of interest. Here, we see that our core results concerning elite violence, battle deaths, crop area and pasture area remain intact, and that no changes in signs or significance occur.

	(1) Birth Known	(2) Birth Known	(3) Birth Known	(4) Birth Known	(5) Birth Known	(6) Birth Known
Regicide	**-0.386***** (0.127)	**-0.396***** (0.129)	**-0.397***** (0.130)	**-0.388***** (0.137)	**-0.389***** (0.138)	**-0.452***** (0.125)
Battle	**-0.690***** (0.219)	**-0.700***** (0.221)	**-0.698***** (0.221)	**-0.711***** (0.259)	**-0.704**** (0.257)	**-0.686**** (0.255)
Urbanisation		-0.189 (0.180)	-0.190 (0.181)	-0.177 (0.185)	-0.172 (0.185)	-0.202 (0.186)
Mode of Succession (Base=Hereditary)						
● Partially Elected			-0.0246 (0.0878)	0.0279 (0.0954)	0.0276 (0.0953)	0.0161 (0.0972)
● Fully Elected			0.00681 (0.0821)	-0.0586 (0.0826)	-0.0601 (0.0825)	-0.0114 (0.0855)
Pasture Area				0.0123 (0.00823)		**0.328***** (0.0746)
Crop Area					0.00523 (0.00588)	**-0.350***** (0.0789)
Second Serfdom	-0.0134 (0.0753)	-0.0253 (0.0767)	-0.0261 (0.0770)	-0.0107 (0.0814)	-0.00908 (0.0818)	-0.0484 (0.0803)
Constant	**0.208*** (0.115)	**0.214*** (0.116)	**0.214*** (0.124)	0.197 (0.139)	0.198 (0.139)	0.226 (0.136)
Observations	226	226	226	201	201	201
Adjusted R^2	0.107	0.107	0.099	0.084	0.082	0.115
Country FEs	YES	YES	YES	YES	YES	YES
Time FEs	YES	YES	YES	YES	YES	YES

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.16. Fixed Effects Regressions with Predicted Values

B.10.5. Changing the Spatial Unit of Observation

Next, we implement another robustness test by changing our spatial unit of observation from modern countries to the broader regions specified in table A.B.1. Again, our key findings are largely unaffected, although neither the pasture nor crop variables become at all significant; while the second serfdom now has a negative and significant impact.

	(1) Birth Known	(2) Birth Known	(3) Birth Known	(4) Birth Known	(5) Birth Known	(6) Birth Known	(8) Birth Known
Regicide	**-0.284***	**-0.289***	**-0.295***	**-0.326****	**-0.326****	**-0.326****	**-0.296****
	(0.151)	(0.152)	(0.154)	(0.150)	(0.147)	(0.151)	(0.132)
Battle	**-0.603****	**-0.615****	**-0.597****	**-0.532***	**-0.529***	**-0.527***	**-0.518***
	(0.271)	(0.273)	(0.270)	(0.255)	(0.254)	(0.263)	(0.261)
Urbanisation		-0.137	-0.144	-0.127	-0.124	-0.123	-0.107
		(0.162)	(0.161)	(0.157)	(0.157)	(0.157)	(0.180)
Mode of Succession (Base=Hereditary)							
● Partially Elected			-0.0322	-0.0390	-0.0390	-0.0374	-0.0374
			(0.0649)	(0.0683)	(0.0687)	(0.0693)	(0.0624)
● Fully Elected			-0.0352	-0.0429	-0.0427	-0.0425	-0.0465
			(0.0874)	(0.0872)	(0.0872)	(0.0887)	(0.0830)
Pasture Area				0.0115		-0.00528	-0.0152
				(0.00950)		(0.0684)	(0.0587)
Crop Area					0.0127	0.0179	0.0268
					(0.00848)	(0.0678)	(0.0596)
Second Serfdom	**-0.110****	**-0.120*****	**-0.129*****	**-0.135****	**-0.133****	**-0.133****	**-0.122****
	(0.0384)	(0.0381)	(0.0423)	(0.0490)	(0.0483)	(0.0496)	(0.0397)
Constant	**0.446****	**0.448****	**0.461****	**0.488****	**0.487****	**0.486****	**0.429****
	(0.183)	(0.184)	(0.193)	(0.195)	(0.195)	(0.200)	(0.146)
Observations	155	155	155	149	149	149	149
Adjusted R^2	0.656	0.655	0.651	0.621	0.621	0.618	0.621
Elite Controls	YES	YES	YES	YES	YES	YES	NO
Country FEs	YES	YES	YES	YES	YES	YES	YES
Time FEs	YES	YES	YES	YES	YES	YES	YES

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.17. Regional Fixed Effects Regressions

B.10.6. Quantile Regression

Next, we use quantile regression to detect whether using median responses rather than mean responses in our regressions yields contrasting outcomes. Another advantage of quantile regression is that it is less sensitive to outliers than ordinary linear models and is therefore better equipped to face any noise that we may have introduced to the data by summarising individuals as countries and centuries. We did introduce a minimum requirement of ten rulers per country-century unit as a precaution against potential measurement error and outliers, but quantile regression offers this additional advantage in the presence of noisy variables. It should also be noted that Keywood and Baten (2018) use binary choice models, namely linear probability models and logistic regression, as robustness tests to inspect whether summarising our data affects our results in the context of regicide and our elite numeracy proxy. They find comparable results.

The conclusions drawn from our quantile regression at the median are largely the same as those of the fixed effects specification. The only real difference between the two estimators is that model five of the quantile regression shows none of our regressors to be significant. However, the remarkable similarity of the other results leads us to believe that this is an anomaly and that it does not invalidate any of our previous results.

	(1) Birth Known	(2) Birth Known	(3) Birth Known	(4) Birth Known	(5) Birth Known	(6) Birth Known	(7) Birth Known
Regicide	**-0.416*** **	**-0.429*** **	**-0.429*** **	**-0.436*** **	-0.434	**-0.503*** **	**-0.473*** **
	(0.108)	(0.112)	(0.112)	(0.167)	(0.351)	(0.173)	(0.130)
Battle	**-0.686*** **	**-0.698*** **	**-0.697*** **	**-0.700** **	-0.695	**-0.675* **	**-0.662*** **
	(0.202)	(0.206)	(0.206)	(0.343)	(0.720)	(0.354)	(0.283)
Urbanisation		-0.217	-0.215	-0.192	-0.172	-0.194	-0.171
		(0.192)	(0.192)	(0.277)	(0.570)	(0.286)	(0.211)
Mode of Succession (Base=Hereditary)							
● Partially Elected			-0.0177	0.0501	0.0503	0.0384	0.00884
			(0.0853)	(0.132)	(0.277)	(0.138)	(0.121)
● Fully Elected			0.00462	-0.00256	-0.00247	-0.00590	-0.00838
			(0.0303)	(0.0433)	(0.0906)	(0.0452)	(0.0368)
Pasture Area				0.0154		**0.344*** **	**0.339*** **
				(0.0128)		(0.117)	(0.0937)
Crop Area					0.00818	**-0.365*** **	**-0.363*** **
					(0.0272)	(0.129)	(0.103)
Second Serfdom	-0.0292	-0.0429	-0.0435	-0.0383	-0.0364	-0.0839	-0.0626
	(0.0721)	(0.0748)	(0.0750)	(0.111)	(0.232)	(0.116)	(0.0891)
Observations	226	226	226	201	201	201	201
Elite Controls	YES	YES	YES	YES	YES	YES	NO
Country FEs	YES	YES	YES	YES	YES	YES	YES
Time FEs	YES	YES	YES	YES	YES	YES	YES

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.18. Quantile Regressions (Median)

The robustness tests that we conduct in this appendix show that our fixed effects regression may slightly overstate the effect of regicide on elite numeracy and cast doubt on the effect of battle deaths; but that the remaining variables, especially crop and pasture areas, seem to be consistent across model specifications. In sum, our fixed effects results seem robust and provide clear evidence for our key conclusions; particularly that elite violence does seem to have a causal impact on elite numeracy.

B.10.7. Instrumental Variable Regressions

Tables A.B.12. and A.B.13. show the first stage regressions to the IV regressions from tables B.5. and B.6. respectively.

	Hungarian Invasions (9th and 10th centuries)	High Medieval Peace (11th and 12th centuries)	Mongolian Invasions (13th and 14th centuries)
	(1) LIML Regicide	(2) LIML Regicide	(3) LIML Regicide
Invasion Proximity	**-0.120*** **	-0.0507	**-0.0535***
	(0.0364)	(0.0349)	(0.0306)
Constant	**0.920*** **	**0.483** **	**0.489*** **
	(0.218)	(0.201)	(0.178)
Observations	14	23	33
R-squared	0.474	0.091	0.090
Adjusted R^2	0.431	0.048	0.060

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

First Stage IV Regressions to: Table B.5.

	(1) LIML Regicide	(2) LIML Regicide	(3) LIML Regicide	(4) LIML Regicide	(5) LIML Regicide	(6) LIML Regicide	(7) LIML Regicide
Invasion Proximity	**2.049*** **	**2.064*** **	**1.955*** **	**2.007*** **	**2.220*** **	**2.207*** **	**2.290*** **
	(0.692)	(0.692)	(0.710)	(0.706)	(0.760)	(0.760)	(0.776)
Battle		0.180	0.157	0.199	0.139	0.141	0.132
		(0.179)	(0.183)	(0.183)	(0.201)	(0.202)	(0.203)
Urbanisation			-0.228	-0.173	-0.145	-0.147	-0.139
			(0.315)	(0.322)	(0.342)	(0.342)	(0.344)
Mode of Succession (Base=Hereditary)							
● Partially Elected				0.0774	0.0622	0.0632	0.0611
				(0.0611)	(0.0731)	(0.0731)	(0.0734)
● Fully Elected				-0.0709	-0.0861	-0.0862	-0.0862
				(0.0546)	(0.0621)	(0.0621)	(0.0623)
Pasture Area					0.0125		0.0802
					(0.00981)		(0.138)
Crop Area						0.0126	-0.0703
						(0.0102)	(0.143)
Constant	-0.186	-0.204	-0.169	-0.184	-0.221	-0.219	-0.233
	(0.129)	(0.130)	(0.139)	(0.139)	(0.152)	(0.152)	(0.154)
Observations	120	120	120	120	106	106	106
R-squared	0.084	0.092	0.096	0.124	0.155	0.154	0.157
Adjusted R^2	0.060	0.060	0.057	0.069	0.085	0.084	0.078
Time FEs	YES	YES	YES	YES	YES	YES	YES

Standard errors clustered by country
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B.19. First Stage IV Regressions to: Table B.6.

B.10.8. Regicide and Nobilicide

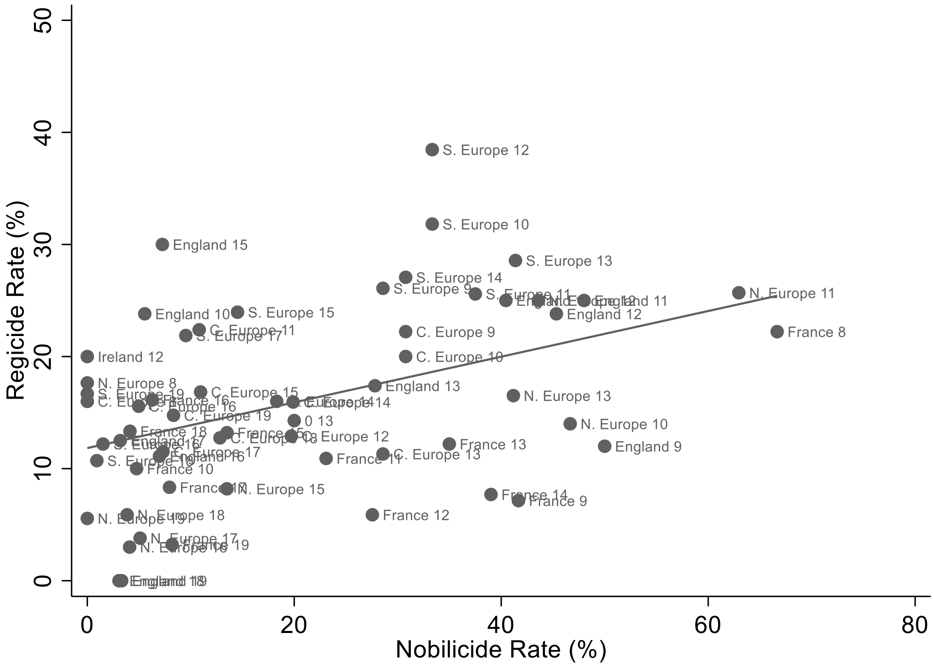

Figure B.19. Regicide versus Nobilicide (Nobilicide from Battles)

Note: centuries are rounded up and abbreviated, i.e. 15 refers to the 15th century. Regional disaggregation follows Cummins (2017) where S. Europe refers to Southern Europe, C. Europe refers to Central Europe and N. Europe refers to Nothern Europe. *Source*: Nobilicide data from Cummins (2017).

B.10.9. Description of Variables

1. Elite Numeracy

In order to estimate elite numeracy, we employ the share of rulers for whom a birth year is reported in conventional biographical sources. We propose that for the birth year of a ruler to be entered into a kingdom's historical records, a certain level of numerical sophistication is required among the ruling elite. This evidence does not necessarily estimate the numerical ability of the rulers themselves but rather that of the government and bureaucratic elite around them and, by implication, the elites of the polity in general.

As more traditional indicators of education such as literacy rates, school enrolment, or age heaping-based numeracy are not available for most medieval European countries, only the 'known ruler birth year' proxy allows us to trace elite numeracy in periods and world regions for which no other indicators are available.

The data for the elite numeracy measure come from our regicide dataset, which was initially built using the rulers found in Eisner's (2011) original regicide study, comprising 1513 rulers from across 45 kingdoms. We then strongly expanded this dataset with an array of supplementary sources, chiefly Morby's (1989) "Dynasties of the World" and Bosworth's (1996) "The New Islamic Dynasties" as well as many other individual biographies and encyclopaedia entries. The expanded dataset consists of 4066 rulers from 92 kingdoms across the period 500 – 1900 CE and comprises all of Europe (see Keywood and Baten 2018 for more details).

2. Elite Violence

Elite violence could potentially be an important determinant of elite numeracy. If the risk of being killed were high, elite families would likely have substituted some of their children's education for military training or instruction in self-defence. Similarly, elites surrounding the ruler would have been selected based on criteria concerning strategic combat and defence rather than on sophisticated skills in negotiation and trade. Additionally, violence may have prevented students from travelling to educational facilities, and these institutions may even have been destroyed through violent acts.

We use the regicide rate as our indicator for elite violence after comparing evidence on regicide and homicide for a number of European countries for which Eisner (2014) presented early evidence of homicide. The data for the elite violence variable come from our regicide dataset.

3. Battle Violence

Battle violence provides information on civil wars and external military pressures on each kingdom, which may have affected elite numeracy through the destruction of educational infrastructure or lowered incentives to invest in elite numeracy due to lower life expectancy (Cummins 2017). Moreover, battle deaths and regicide are correlated, meaning that not including them as a control variable could lead to an overstatement of any effect of regicide on elite numeracy. Consequently, because we aim to use regicide as a proxy for interpersonal violence, we must differentiate between it and violence stemming from external sources. The data for the battle violence variable come from our regicide dataset.

4. Urbanisation

Urbanisation rates are widely used in economic history literature, and act as a broad control variable for factors that could confound the relationship between elite violence and elite numeracy. They have also been employed as a proxy indicator for income among early societies in which other income proxy data are unavailable (Bosker et al. 2013; De Long and Shleifer 1993; Acemoglu et al. 2005; Nunn and Qian 2011; Cantoni 2015). Bosker et al. (2013) hypothesise that part of this relationship works through agricultural productivity because a productive agricultural sector is required to support a large urban centre, and urban areas cannot produce their own agricultural goods. We constructed our urbanisation variable using Bosker et al.'s (2013) estimates of urban populations and calculated urbanisation rates using McEvedy and Jones' (1978) measurements of country populations by century.

5. Institutional Quality

We also introduce a measure of institutional quality as a potential determinant of elite numeracy. Our indicator is the mode of succession of rulers, as this captures a preference for the division of power and the willingness to forego executive decision-making in the interests of democracy. We use a three-category indicator to describe whether a ruler obtained their position through inheritance, partial election or full election by the nobility or a business aristocracy (as in Venice, for example). The differences in institutional quality between states, seen through modes of succession, is not as large as those between democracy and autocracy, of course, but evidence on democratic structures does not exist for the first centuries under study here. However, a preference for the division of power reduces the likelihood of unconstrained totalitarianism. We expect institutional quality to be positively correlated with elite numeracy. The data for the institutional quality variable come from our regicide dataset.

6. Pastureland

Next, we use estimates of pastureland area from Goldewijk et al. (2017). We transform the variable to pastureland per square kilometre per capita. Motivation for including this control is that pastureland provides nutritional advantages, and improved nutrition is known to have positive implications for human capital (Schultz 1997; Victoria et al. 2008). Second, numerous studies have used pastureland and pastoral productivity as means of estimating female labour force participation, which is lined to female autonomy gender inequality, human capital and numeracy as a result (Alesina et al. 2013; de Pleijt et al. 2016; Voigtländer and Voth 2013; Baten et al. 2017). This mechanism functions through women's comparative physical disadvantage relative to men when ploughing fields and performing other tasks required when crop farming. Over time, this tendency developed into a social norm that saw men work in the fields while women took care of 'the home' (Alesina et al. 2013). However, when cattle and other domestic animals were present, their care became the task of women – boosting female labour participation and their contributions to household income, thereby increasing female autonomy and reducing gender inequality – allowing women to develop skills in human capital and contribute to economic development (Diebolt and Perrin 2013).

7. Cropland

As a counterweight to the pastureland variable, we use cropland as a comparative indicator. Like pastureland, cropland should describe agricultural and nutritional development but should also emphasise gender inequality for the reasons above. Therefore, its coefficient should be positive if nutrition, in terms of calories, is more important for elite numeracy, and negative if gender inequality is. The cropland variable is also transformed into per square kilometre per capita terms; and comes from Goldewijk et al. (2017).

8. Second Serfdom

We include a variable for the second serfdom to assess whether the inequality that it wrought had any impact on elite numeracy in Eastern Europe. This is coded as a dummy variable for all of Eastern Europe from the 16^{th} until the 18^{th} century and until the 19th century in Russia, where serfdom was only officially abolished under Tsar Alexander II in 1861.

9. Nomadic Invasions

We use the nomadic invasions of Europe from Central Asia as an instrument for elite violence because they resulted in an external import of violence to Europe. Additionally, nomadic invasions meet the exclusion restriction their origins were determined by climatic forces, such as droughts in Central Asia (Bai and Kung 2011), and by military capacity. To estimate the impact of these invasions, we use the logged inverse distance of each kingdom's capital to Avarga, Mongolia, the location of the first capital of the Mongolian Empire.

10. Length of Reign

The next three variables are used to control for ruler specific characteristics, labelled "elite controls" in the text. First, rulers who spent more time on the throne could have better established themselves and their policies, giving chronologists more reason and more time to document their birth years. We control for this potentially biasing effect by including the length of the ruler's reign as a control variable. The data for the reign length variable come from our regicide dataset.

11. Fame of Ruler

Second, the birth years of more famous rulers might have been better recorded. It is conceivable that events in the lives of lesser rulers, who were placed under the suzerainty of an emperor, for example, would be less diligently documented. We can also control for this "fame bias" to a certain extent by controlling for whether the rulers of each kingdom were always under the suzerainty of an overlord, whether this applies to a part of each period, or whether it was never the case. Rulers with a more dependent, governor-type function most likely attracted less attention from chronologists than those who had the freedom to act and set policy autonomously. The data for the ruler fame variable come from our regicide dataset.

12. Power of Ruler

We include the area of each kingdom in square kilometres as a third control variable against more famous or powerful rulers being better documented. Although not all powerful rulers held large territories, rulers of powerful kingdoms such as the Holy Roman Empire, the Ottoman Empire, Poland-Lithuania and the Kievan Rus certainly did. The data for the ruler power variable come from Nüssli (2010).

13. Religion

As an additional variable for the random effects specification we use the most prominent religion in each country during each century – Islam, Orthodoxy, Protestantism, Catholicism (our reference group) and an 'other' category; comprising Pagan, tribal or pre-Christian religions. This indicator variable was included to capture the effects of cultural characteristics that are associated with religion. We coded the majority religion by using the ruler's religion from our regicide sources and the summaries of historical religion in the Encyclopaedia Britannica (2019).

14. Religious Diversity

We also include a dummy for religious diversity from Baten and van Zanden (2008). This could have either a positive effect on numeracy, perhaps via competition – stimulating book consumption, for example – or a negative effect via conflict through social fractionalisation (Easterly and Levine 1997).

15. Jewish Minority

Our final religious variable is a dummy for the presence of a substantial Jewish minority, which we include because Jews were, on average, better educated than other religious groups among whom they lived. These data are from a combination of Anderson et al. (2017), Botticini and Eckstein (2012) and the Encyclopaedia Judaica (1972).

16. Ruggedness

We use ruggedness because numerous studies have associated it with violence and lower economic development in a broader sense. For example, Mitton (2016) finds flatter landscapes to be associated with higher GDP per capita, while Bohara et al. (2006), O'Loughlin et al.

(2010) and Idrobo et al. (2014) all describe different situations where rugged terrain provides advantages for instigators of violence. In contrast, Nunn and Puga (2012) describe how ruggedness protected parts of Africa from the adverse effect of the slave trade between 1400 and 1900. The ruggedness data that we use come from Nunn and Puga (2012).

17. Coordinates

Latitude and longitude are used as general spatial controls, and are measured by the geographic centroids for modern countries from Donnelly (2012).

18. Percentage Fertile Soil

We use the percentage of each country that is covered by fertile soil as an additional control for any agricultural impact on elite numeracy. The fertile soil data come from Nunn and Puga (2012).

19. Percentage within 100 km of ice-free coast

We use the percentage of each country that that lies within 100 km of ice-free coast as an additional control for the effects that maritime trade may have had on elite numeracy. The within 100 km of ice-free coast data come from Nunn and Puga (2012).

C. Territorial State Capacity and Violence, 500 – 1900 CE.[28]

Abstract

We present new evidence for elite violence using regicide, the killing of kings, and investigate the role of the state in European violence between the 6th and 19th centuries. First, regicide is critically assessed as a proxy for interpersonal elite violence, but it survives this scrutiny. We present a close relationship between regicide and homicide, and compare qualitative trends in regicide with events that take place throughout Europe's economic history. We also investigate territorial state capacity by studying which states could keep or even expand their territories. A series of empirical tests are then conducted to illustrate that territorial state capacity likely had a largely pacifying role on trends and regional differences in interpersonal elite violence, at least since the High Middle Ages.

[28] Co-authored by Jörg Baten. He contributed approximately 20% of the work to this paper.

C.1. Introduction

Tilly (1975), Dincecco (2015), and Hoffman (2015) assert that war is a crucial determinant of state capacity, arguing that a high degree of tax capacity developed in France as a direct result of the Hundred Years' War. In contrast, Pinker (2011) and Fearon and Laitin (2003) maintain that state capacity and the policing function associated with it helps to contain interpersonal violence. We will assess, in the following, which of these two views is supported by long run European data from 500 to 1900 CE.

Interpersonal violence is a challenging topic to study over the very long run, particularly as homicide rates have traditionally been used as its standard unit of measurement among economic historians. Homicide data does not often extend further back in time than the 19th century, even in countries with the most rigorous traditions of record-keeping. However, by building on an approach by Eisner (2011) and using regicide – the killing of kings and other rulers – as a proxy indicator for interpersonal elite violence, we are able to study more than a millennium of the history of violence. After exploring these trends in elite violence, we assess its relationship with state capacity.

How can state capacity be measured for the Middle Ages? During the medieval period, states aimed to keep their territory or expand it if possible; there was strong competition between the existing principalities and many of them disappeared from the landscape, whereas others kept their territories or even expanded. Hence, our proxy indicator for state capacity is the retention or even expansion of territory, which we will name 'territorial state capacity' in the following. We do not necessarily see expansion as positive, but it is a side effect of territorial state capacity. Correspondingly, territorial state capacity would also have been determined by marriage patterns, with ruling families of more successful states being able to arrange marriages strategically, in order to acquire valuable territories. This strategy allows us to assess the correlation between violence and state capacity, two crucial components of development.

Although we cannot claim causality from our estimates, we expand the literature on violence and state capacity by investigating which of the hypotheses – that expanding kingdoms or principalities were violence-promoting or violence-restricting – is supported by European data since 500 CE.

Evidence about the relationship between violence and state capacity, both for earlier and more recent periods, but especially for the 20th century, is very mixed. The most famous definition of 'the state' was coined by Max Weber (1919) a century ago; he characterised it as an organisation holding a "monopoly of legitimate violence" – that states curb interpersonal violence through a policing-type function but are still able to enact violence themselves through legal systems or warfare. However, as mentioned previously, Tilly (1975) proposed that only states with the capability to raise taxes and support armies could protect their constituents, and that the threat of war was necessary to make the nobility comply with plans to generate tax capacity. More recently, Dincecco (2015), O'Brien (2011) and Hoffman (2012, 2015) studied cases where war is associated with tax capacity and conclude that war-induced state capability was one of the core reasons for Europe's economic rise.

Conversely, another branch of the literature finds that state capacity has a significant conflict-restraining function. As mentioned, Pinker (2011) proposed that states developed policing capabilities to restrict violence, but he also suggested that high capacity states promoted certain value systems which favoured cooperation and negotiation, helping to stem violence further.

Correspondingly, Fearon and Laitin (2003) study the origins of global civil war between the Second World War and the turn of the millennium. They find that weak state capacity, along with poverty and large populations, has been a stronger predictor of violence than more intuitive drivers of civil violence, such as inequality, state discrimination against minorities, colonial legacies and ethnic or religious fractionalisation. Further, Richani (2010) also agrees with

Pinker's (2011) findings, reasoning that eroded state capacities cause interpersonal violence through increased opportunities for corruption, weakening service provision and potentially causing heightened financial and macroeconomic instability, as well as fluctuations in goods prices.

Bell et al. (2013) largely agree that high state capacity tends to limit violence, but also contend that strong, centralised states can incite and contribute to violence through human rights violations and political imprisonment, for example. However, they maintain that the relationship is not simply due to institutions that allow for political freedom, asserting that restrictions on citizen coordination such as enforcing curfews or banning gatherings is likely to reduce violence.

Heldring (2019) examines a particularly interesting example, the impact of state capacity on the intensity of the Rwandan genocide in 1994. He finds a positive relationship using subnational units before implementing an instrumental variable strategy and concludes that state capacity caused genocide.[29]

Similarly, Acemoglu et al. (2010) discuss how certain investments in state capacity can lead to civil wars. They use a game-theoretical model to outline a situation in which the elites in command of a polity with weak institutions must choose whether or not to strengthen their army in order to acquire a monopoly on violence and quell a hypothetical rebellion. Doing so, however, could grant the army implicit political influence or even endanger the personal safety and political positions of the elite if the army decides to execute a coup.[30]

[29] Heldring used geographical cattle suitability as an instrument. Cattle constituted the foremost store of value during precolonial times and acquiring cattle was the main goal of conquering neighbouring principalities. Modern state capacity was then simpler to develop in regions where precolonial state capacity already existed. This causal effect also fits one of the main findings of the International Criminal Tribunal for Rwanda, that the genocide was planned and that the perpetrators leveraged political positions to enact genocide.

[30] Acemoglu and Robinson (2012) provide an example of this by outlining how Siaka Stevens, president of Sierra Leone from 1971 to 1985, crippled his own country's army in favour of individual paramilitaries under his direct control.

Clearly, evidence on the relationship between state capacity and violence is mixed. Our strategy for studying the relationship between state capacity and violence is to study the last 1400 years of European history, which provide a new arena in which to explore this relationship.

C.2. Assessing the Regicide Indicator

C.2.1. Regicide and Homicide

Although homicide is the academic standard when measuring interpersonal violence, historical evidence of it only begins to be recorded from the 14th century. Even then, it is only available for major cities – chiefly in Western Europe – and a small number of countries, while most European states begin to document homicides from the early 19th century. Initially, homicide statistics began to be recorded because they were considered significant and unusual events, unlike more frequent experiences such as births and weather patterns or metrics like education standards or trade volumes; data for which usually appear later or are inferred by economic historians, perhaps never being used in a contemporaneous setting.

The use of regicide as a proxy for interpersonal violence was first explored by Manuel Eisner (2011), who noticed a strong association between European homicide and regicide rates in Western Europe as far back as the 13th century. Like homicide, regicide records were collected because of their value as both significant and unusual events within societies, though to an appreciably greater extent. Accordingly, accounts of regicide were amassed from much earlier times, providing far lengthier data series. Unlike early homicide records, which would have been confounded by poor base rate estimates, since formal population censuses only became widespread during the 19th century, regicide rates are calculated from comprehensive dynastic lists. Documenting rulers was always deemed important regardless of whether they were killed.

Although studying regicide clearly offers far fewer observations and consequently requires greater periodicity than homicide, the habit of recording the lives and deaths of rulers permits an analysis that begins far earlier than alternative indicators of interpersonal violence have traditionally allowed.

C.2.2. Comparisons with other Indicators

Elias (1939) studied descriptions of medieval brutality and put forward a hypothesis that violence has declined over multiple centuries. Later, he characterised this as part of humankind's 'civilising process' – an appreciation for elevated speech, table manners and the rise of chivalry – a process which steadily evolved into a distaste for violence in favour of negotiation as the preferred tool for resolving conflicts. Many scholars have declared this process decisive in altering universal levels of welfare over time (Eisner 2001, 2011; Meeus and Raaijmakers 1986, Steinert 2003).

In order to motivate regicide as a proxy indicator for interpersonal violence, we compare it to homicide statistics from Eisner (2014) in the context of the civilising process. Figure C.1., below, compares our estimates of regicide with homicide records in Germany, Italy, Spain and the United Kingdom. Evidence of the relationship between the two series as well as the civilising process is immediately evident, as high rates of interpersonal violence are visible from the 13th century before gradually declining toward modern levels and flattening during the early modern period. At 65-70 homicides per 100 000 people, Italy's 14th century homicide rate was comparable to that of El Salvador and Honduras today, while Germany, Spain and the UK were comparable to Columbia, Brazil or South Africa at about 30 homicides per 100 000 people (UNODC 2019).

In Germany, we see a strong decline in violence from the 13th century which takes on a steady concave pattern, despite overcorrections between 1400 and 1500. The relationship is also clear in Italy until the 18th century, when regicide diverges from homicide and increases strongly in the last century – due to the assassinations which took place in the build up to Italy's unification. In Spain, although fluctuations in regicide appear larger than in homicide, their correlation over time is extraordinary. Finally, the two series also display a largely common trend in the United Kingdom, despite missing values in the homicide series between 1400 and 1600.

One criticism of these simple comparisons could be that both regicide and homicide follow a common declining trend which may expose a spurious relationship. However, the circles indicated in figure C.1. provide evidence that the correlation is not only dependent on time by illustrating cases where the two series simultaneously increase. Indeed, every instance of increasing violence in these four countries is followed by both indicators, aside from Italy in the 19th century and in Germany, where the discrepancies reflect differences in periodicity.

Additional evidence on the relationship between regicide and elite violence can also be obtained through comparisons with the rates of nobilicide (the killing of noblemen) calculated by Cummins (2017; using the proportion of battlefield deaths among noblemen). Figure C.2. illustrates how both series, aggregated at the European level, decline from the 6th and 8th centuries, respectively. However nobilicide also deviates from this downward trend in the 14th century, which coincides with the Mongolian invasions and the beginning of the Hundred Years' War in Western Europe. Further, figure C.3. provides evidence of this relationship disaggregated to the regional levels that were used by Cummins (2017).

C.2.3. A Timeline of Regicide

To assess the plausibility of our regicide indicator further, we also compare trends in regicide with the major economic developments that took place since 500 CE, such as major invasions, episodes of plague and the 'second serfdom'. These major economic developments are often mentioned in the economic history literature and are therefore discussed here.

When we disaggregate European violence into six regions, we observe interesting deviations in each series (figure C.4.). For example, at the end of the Viking Age, during the High Middle Ages (11^{th} – 12^{th} century), Scandinavia introduced more centralised monarchies, but they initially lacked common acceptance, and regicide very often took place.[31]

Likewise, North Eastern Europe deviated towards increased regicide in the period of the Mongolian invasions (13^{th} – 14^{th} century), though with more persistent consequences. Keywood and Baten (2019) explain why this invasion period resulted in interpersonal violence and regicide, even without considering battlefield violence. In the following period, the 15^{th} – 19^{th} centuries, North-Eastern Europe remained the second most violent European region while South-Eastern Europe became the most violent.

Another potential determinant could be the second serfdom in Eastern Europe. The second serfdom was an event through which feudal systems were reintroduced into Eastern Europe after increased state centralisation had dismantled earlier feudal systems in order to better organise labour in the aftermath of the Great Plague (Ogilvie and Edwards 2000). This period, lasting approximately between the 16^{th} and late 18^{th} centuries (although serfdom in Russia was only abolished in 1861 under Tsar Alexander II) is commonly thought to have been the result of the low agricultural output and a high land-labour ratio in Eastern Europe that was caused by The Plague (Acemoglu and Wolitzky 2011). Alternatively, the combination of scarce labour and abundant land in the aftermath of the Great Plague resulted in substantially higher wages as a result of increased labour demand, attracting labourers from Western Europe. From the 16^{th} to the 18^{th} century, working conditions increasingly deteriorated and it became difficult

[31] This was the setting for Shakespeare's *Hamlet*.

for serfs to leave since they had no movable assets. Landlords subsequently grew powerful due, in part, to Western Europe-bound exports (Kula 1976; Blum 1957), amassing militias and gaining bargaining power over rulers and peasants alike. Keeping peasants as serfs would have increased inequality (Ogilvie and Edwards 2000) and potentially led to further interpersonal violence. From figure C.4., we also gain evidence that the second serfdom might be associated with persistently high rates of regicide. While most European series seem to steadily drop toward the low levels of modern violence experienced after the Plague, regicide stagnates in Eastern Europe. Eastern European regicide rates settled well above 10% as opposed to the steady declines towards 5% that were experienced in Western and Central Europe by the 19th century.

C.2.4. Regional Differences in Violence by Period

In addition to European trends, country-specific trends in regicide also allow for the detection of certain events throughout Europe's history. The maps in figures C.6. and C.7. show the distribution of violence over time, grouped by countries as opposed to principalities for the purposes of the mapping software. The figures that follow describe the respective states of regicide during four periods of European development.

We compare these general trends to key events that took place within each region – often referring to battles with foreign powers as any spillover effects from organised conflict may have led to interpersonal violence. Figure C.6. describes the average state of regicide for each country over our entire sample period.

Bulgaria, Armenia, Turkey and Cyprus – in that order – exhibit the highest rates of regicide over our entire period of study, all above 30%. Conversely, the central European countries of Germany, Austria and Poland – along with Portugal – display the lowest rates, all under 8%. Broadly, Europe seems to have had a peaceful centre with violent frontiers. Until the end of the High Middle Ages, Ireland and parts of Scandinavia all saw comparatively higher levels of regicide with considerable numbers of deaths in battle. As such, the notion of a peaceful centre with violent frontiers becomes even clearer when including battle deaths and in the periodic maps that follow (figures A.C.2. and A.C.3.).

During the early Middle Ages (figure C.7., panel a), violence was extreme and nine of the eighteen countries for which we have data exhibit regicide rates of over 25%. From the beginning of this period until the 8th century, the Justinian Plague may have had some effect in inducing the violence that we see in Turkey and Italy as it ravaged the Mediterranean states, killing up 50 million people or an estimated 15% of the world's population (Caspermeyer 2016). At the same time, principalities within Germany, the Czech Republic and Serbia had low regicide rates.

The 10th to 13th centuries are probably the closest match to our map encompassing the entire period; as northern European regicide appears to gain prominence due the pacification of the Vikings and the subsequent generation of non-yet fully-accepted monarchies. This shift becomes particularly clear when examining the map including battle deaths, as a disproportionate number of Scandinavian rulers died in battle (figure A.C.3., panel b). Indeed, the ratio of battle deaths to regicide is 1.75 for Norway and 2 for Iceland as opposed to the average ratio of 0.49 across all countries and periods.

Denmark, the Netherlands, Sweden and the United Kingdom all experience surges in regicide between the 10th and 13th centuries, relative to the previous period. At this time, the Second Bulgarian Empire was the main power within South-Eastern Europe, although it was under constant pressure due to ceaseless invasion attempts by the Mongols, Byzantines, Hungarians and Serbs (Wolff 1949). Meanwhile, their neighbours in Romania experienced a particularly low-regicide period in comparison to their country average (3.1% as opposed to 19.3%), driven by the Kingdom of Transylvania. Additionally, Georgia transitioned from one of the most violent regions in the early Middle Ages to one of the most peaceful in the High Middle Ages (from 42.9% to 5%). This period coincides with the so-called 'Golden Age' of Georgia which followed the earlier conflicts that the Kingdom of Iberia had fought against the Persians and Byzantines (de Waal 2011). This so-called 'Golden Age' saw Georgia control the entire south Caucasus region before much of it was conquered by the Mongols in the late 13th century.

The 13th to 15th centuries are characterised by near universal trends away from regicide in Europe's west and centre while Eastern Europe's violence levels persist or even strengthen in the cases of Romania, Georgia and Hungary. Here, a strong case can be made for divergence between east and west. Indeed, the only Western European country that still exhibits a 'very high' level of regicide in this period is Denmark which, along with the United Kingdom, is the only western country to sustain a regicide rate above 20%. Conversely, Bulgarian regicide remains fairly high during the Ottoman expansion while Lithuania, Belarus and Ukraine constitute a region of substantial conflict as the rulers of Poland and Lithuania first fought off the Mongols during the early 14th century before the Ottomans conquered much of Ukraine's Black Sea coastline during the 1470s, including Crimea.

The early modern period (panel d) then saw drastic declines in regicide, with only Ukraine (considered Eastern Europe) and Romania (South-Eastern Europe) displaying rates comparable to those in earlier periods. However, despite these widespread declines in violence we can still identify a clear east-west divide, as regicide in Spain and Luxembourg become the only western countries with regicide rates over 10%

After comparing regicide to estimates of homicide and nobilicide, we conclude that there is substantial evidence that regicide measures interpersonal elite violence. This was also confirmed by Baten and Steckel's (2018) comparison of regicide with bio-archaeological evidence; using the share of violent cranial traumata and weapon wounds in Europe.

Additionally, we can see some evidence of Europe's historical narrative reflected in the series as well as the civilising process, encouraging us to proceed with our analysis by using regicide as a proxy for interpersonal elite violence and investigating the role of the state.

C.3. Data

C.3.1. State Capacity

The literature on state capacity and development is well established, but research of its role in violence, as outlined above, is multi-facetted. Throughout the literature, state capacity is estimated in a multitude of ways, attempting to capture the effects of military capacity, bureaucratic or administrative capacity, and the quality of political institutions collectively (Hendrix 2010). As such, previous measurements range from military personnel per capita (Hendrix 2010; Kocher 2010) to territorial variation (Soifer 2008), corruption (Fortin 2010) state fragility (Besley and Persson 2011), tax compliance (Ottervik, 2013), road network density (Hanson and Sigman 2011) and the ease of doing business (Cardenas 2010).

The key right-hand-side variable in this paper is the territorial retention or expansion of principalities, measured by the percentage changes in their areas. This idea stems from the role that military capacity plays as a core component of state capacity as well as the assumption that it is always in the interest of states to keep their territories. The development of state sizes describes a competitive situation between states, as in the 9th to 12th centuries, when some smaller principalities disappeared at the expense of others. Additionally, we use the percentage change in territory in order to show a relationship between elite violence and the development of state capacity instead of absolute changes, which would cause our results to be driven solely by large territories such as the Holy Roman or Ottoman Empires.

Although state capacity has been estimated using the array of indicators listed above, we are hesitant to refer to state capacity in its entirety and prefer to name our variable an indicator of 'territorial state capacity', placing emphasis on the capacity to defend territory and expand as opposed to other features of state capacity such as its bureaucratic or administrative capabilities. However, Lake and O'Mahoney (2004) propose that state sizes are determined by a balancing act between military capabilities (required both for defence or conquest) and certain economies of scale in bureaucratic tasks and service provision (geographical limits to tax collection, transportation, communication and state infrastructure, for example).

Additionally, there is precedent for using territorial expansion to approximate state capacity. Archaeologist and anthropologist Charles S. Spencer (2010) proposed a simultaneously causal relationship between state capacity and territorial expansion, arguing that bureaucratic capacity is required for states to grow and that larger states cause greater bureaucratic capacity by providing a larger tax base and access to additional natural resources. Although this causal claim is heavily criticised (Claessen & Hagesteijn 2012), the correlation itself seems to be robust.

Further, Rotberg (2002) discusses the interplay between state capacity, territorial changes and interpersonal violence, using global examples from throughout the 20th century. He describes how low capacity states are more likely to lose territory and that this is associated with increased criminal, interpersonal violence. Diehl and Goertz (1988) empirically assess global territorial changes between 1816 and 1980 and find that international conflicts are more common if the territory of the belligerents is contiguous (shares a land border) and if the difference in state capacity between them is large.[32]

[32] Congruently, Kocs (1995) observes that wars are more frequent if the existing boundary is not recognised by international law. This is more importantly for the 20th and 21st centuries, when international law was used for legitimisation, or legal disregard resulted in a loss of state reputation.

Our variable for territorial state capacity comes from digitised and georeferenced data that was created using Nüssli's (2010) maps of European principalities since the first century CE. When matched to our regicide data, this leaves 34 principalities over the timespan 500 – 1900 CE. When principalities died and formed new principalities, these were matched whenever there was internal continuity within the region, as opposed to conquests. For example, West Francia was matched with the Kingdom of France with the rise of the Capetian Dynasty in 987 CE. Dying principalities are unrecorded as opposed to assigning them -100% changes in territory. It may be argued that this decision introduces certain selectivity biases, but we decided to focus on gradual changes in territories rather than extreme cases. Likewise, we exclude cases where principalities grew by over 500%, such as 14th century Lithuania; which, according to our calculations, grew by 1055% over the century after merging with Poland. Similarly, no record is provided for emerging principalities. When in doubt, the historical record provided enough information to justify matching principalities.

Figures C.8. to C.13. outline the simple relationship between territorial expansion and non-violence (measured as $1 - violence$) over time, showing an overall positive relationship and indicating that greater state capacity is associated with non-violence, following Pinker (2011), Fearon and Laitin (2003), Rotberg (2002) and Richani (2010). For example, Aragon and Venice grew in state capacity over the 12th and 13th centuries and had low regicide rates, as did Denmark, Austria and Venice in the 14th and 15th centuries. In contrast, Denmark and Bulgaria failed on both accounts in the 13th century, and Granada in the 14th century. However, investigating sub-periods reveals no relationship before the 10th century and even a negative although weaker relationship after 1500. The latter negative relationship was mostly caused by the two Austrian outliers of the 16th and 18th centuries, when the Habsburgs were particularly successful in consolidating territory.

C.3.2. The Regicide Dataset

We built our regicide dataset on the foundations of Eisner's (2011) study[33] and then expanded it using a variety of sources; namely, Morby's (1898) "Dynasties of the World" and Bosworth's (1996) "The New Islamic Dynasties" as well as other individual biographies and encyclopaedia entries. This compilation finally resulted in a dataset of 4066 rulers, spanning the period 500-1900 CE and covering all European countries. Where conflicts arose between our sources, we included all rulers that were mentioned; taking care to exclude any duplicates which often arose due to translated names or alternative naming conventions.

We included all rulers with the title of King or Queen and any equivalent or higher-ranking position such as Emperor, Tsar or Sultan; as well as any Dukes, Doges or Prince-Bishops that we could find. We believe that our dataset is near complete for all high-ranking rulers and although the same level of completeness was not possible for lesser rulers, in part due to less thorough recordkeeping, they are widely distributed across both space and time, making us confident that ruler ranks do not affect our trends in regicide systemically. Additionally, several controls for ruler status and ability to set policy are discussed and employed in section C.3.3.

Since Europe and Asia form one contiguous land mass, there is still some debate about its definition, but the most widely accepted view is that the border is formed by the Ural Mountains and the Caspian Sea.[34] Accordingly, we include Turkey, Georgia, Armenia and

[33] Eisner's study included 1513 rulers.

[34] Europe and Asia form one contiguous land mass with intricately interlinked histories, making delimiting our geographical definition of Europe challenging. The centuries' long debate over the continental border began because ancient Greek geographers had little knowledge of any regions north of the Black Sea, assuming that the Sea of Azov or the river Don led to some kind of ocean beyond (Bassin 1991). This classical view of cartography was undisputed well into the Middle Ages, long after it had become clear to Europeans that a vast landscape existed between the Sea of Azov and the Arctic Ocean, which are over 2000 kilometres apart. Thereafter, the debate took on a largely political nature as European noblemen formed and propagated the idea of a civilised, Christian Europe which was superior to the Asian territories further east (Bassin 1991).

western Russia in our sample. Turkey was included because its capital, Istanbul (Constantinople, Byzantium) lies mostly in Europe; its inclusion is also justified by the influence that Turkish societies have had on the Balkan states and on Greece from the Byzantine era to the Ottoman Empire. Additionally, all of Russia's monarchs that we include in our dataset ruled well within Russia's classically European territories and our country level regressors also refer to the traditionally European domain of Russia. Armenia and Georgia are also included, partially because of the presence of Christianity.[35]

The reasons for choosing our timeline are also straightforward; we wished to make use of as large a period as possible without skewing our results and eroding the integrity of the relationship that regicide shares with interpersonal violence. Consequently, we begin our analysis in 500 CE to eliminate the (Western) Roman Empire and end in the year 1900, before the two World Wars. We propose that including the Roman Empire would have led to numerous complications as it encompassed a high share of Europe and exhibited famously high rates of regicide. We then end our analysis in 1900, because the 20th century has undergone drastically shifting borders while European principalities tended to transition either into democracies or dictatorships. Both of these periods would have suffered from a far lower cross-sectional density in observations, as few monarchies remained.

Using these regional and chronological delimiters, we assembled our dataset by accumulating general information such as dates of birth and death, reign dates and the causes

Consequently, continental borders were creatively drawn using combinations of many rivers such as the Volga, Kama or Ob, as well as the Caspian Sea and the Ural Mountains. These are still used today.

[35] Christianity was present in Armenia from the 1st century and became Armenia's official state religion in 302 AD under Tiridates III (Parry 2010). The Armenian language also has Indo-European roots. The Kingdom of Greater Armenia also stretched into the Kingdom of Cilicia (now in southern Turkey) and into the Russian Caucasus territories of today. Lastly, we included Georgia because of its historical links to Christianity (Parry 2010) and because of the strong self-determination of modern Georgians to be classified as Europeans; as seen in surveys conducted since the collapse of the Soviet Union (Gogolashvili 2009).

of death for 4066 rulers from across 34 European principalities. Conquest meant that the borders of principalities continually shifted over this period – consequently, we have some degree of disparity between our regicide indicator and the other variables that make up our empirical analysis in section C.4., which are disaggregated to the modern country level. Therefore, we allocated principalities to countries based on the location of their capitals.

Rulers died in a number of different ways, blurring the distinction between regicide and natural or accidental death. Throughout the data collection process, we made use of three classifications and three definitions before deciding which was the most theoretically appealing. Our most narrow definition is made up of cases where the ruler was clearly assassinated, such as King Canute IV of Denmark who was killed by rebels following a tax revolt, after fleeing from Vendsyssel and hiding in a church in Odense. The rebel group was led by Canute's brother, who succeeded him and became King Olaf I. Narrowly defined; we have 442 cases of regicide, or 11.89% of all rulers.[36]

The intermediate definition consists of these clear assassinations as well as deaths described as dubious. We label cases as dubious when historical accounts imply or strongly conject that a ruler was killed, or if the ruler was poisoned or imprisoned at the time of their death. Deaths during imprisonment seem to make up a small but consistent and widespread proportion of cases and are almost unanimously accompanied by reports of starvation or unlikely 'accidents'. Likewise, poisonings are also included here, despite most cases seeming to be clear cut assassinations. In addition to the 442 narrowly defined cases of regicide, we have another 182 that are labelled dubious, meaning that 624, or 16.78%, of all rulers fall under this intermediate definition.

[36] Percentages are calculated after subtracting the 348 cases where we have no evidence concerning a ruler's cause of death.

Finally, we have documented a further 218 cases of death in battle and add these to our intermediate indicator in order to classify what we term broad regicide. Consequently, 842 rulers fall under this definition, making up 22.65% of the total.

From figure C.5., these definitions seem to reflect similar trends at the European level, suggesting that they reflect somewhat consistent proportions of broadly defined regicide. The only deviations seem to be that the proportion of deaths in battle is unusually high whenever violent peaks form in any of the three series.

For the remainder of this paper we refer to intermediate regicide and simply name it 'regicide'. The reason for including dubious cases in our variable of interest is that, in our opinion, cases of poisoning or death in imprisonment still reflect interpersonal violence and that the benefits of expanding the variability of our dataset by 182 regicide cases outweighs any noise that may be introduced by the possibility of a few false positives – keeping in mind the consistent ratio of dubious regicide to narrow regicide. However, the rationale for our proxy becomes less clear when including deaths in battle. Though civil war accounts for the vast majority of battle deaths and may reflect interpersonal violence in certain instances, international conflicts often simply stemmed from the whim of a foreign power and would have reflected external causes rather than interpersonal violence. We take the more cautious approach of dropping battle deaths from the regicide indicator entirely instead of attempting to separate civil and international conflicts, as any influence that civil conflicts may have on interpersonal violence is not entirely clear.

However, this more conservative approach also comes with an advantage – it allows us to use the proportion of rulers killed in battle as a control variable for more organised violence. This may be important due to the possibility of contagion from civil or external conflict to interpersonal violence. Although the presence of rulers killed in battle does not encompass all battles from across all countries and centuries under study, it provides a convenient metric for this purpose; particularly as we have recorded a not insignificant 218 deaths in battle.

C.3.3. Other Right-Hand-Side Variables

While individual psychology is undoubtedly the key component in understanding singular acts of violence – with the psychological condition of a particularly charismatic leader perhaps even causing large deviations in short term trends – strong states should be able to create a “monopoly of legitimate violence” and thus restrict the extent of interpersonal violence using their police forces or militaries, according to Weber (1919). Conversely, predatory leaders could stimulate violent conduct and trigger a positive correlation between regicide and territorial state capacity.

Additionally, we assess whether certain economic, environmental and social factors affect long term interpersonal violence by generating social unrest and political instability. We test the relationships between regicide and territorial state capacity, income, agricultural productivity and certain measurements of institutional quality on the right-hand side; controlling for several factors such as battles and principality fractionalisation. We also include certain elite controls that may be important in estimating regicide but not necessarily important determinants of elite violence.

The impact of nomadic invasions from Central Asia is also investigated here. The invasions of the Hungarians, Mongols, Huns and other nomadic groups had enormous effects on Europe’s violence environment, possibly causing spillovers into interpersonal violence (Keywood and Baten 2019). Their superior equestrian-based tactics allowed them to gain large territories very quickly, providing shocks to the territorial state capacities of even the strongest of Europe’s principalities (Adshead 2016). For example, the Holy Roman Empire could not defeat the Hungarians for nearly two centuries before the Battle of Lechfeld in 955 CE (Bowlus

2006). Likewise, in the 13th century, the powerful and now European Kingdom of Hungary offered little resistance to Mongol invasions (Sinor 1999).[37]

In an attempt to capture some of the effects of these invasions on elite violence and territorial state capacity, we use the distance to Central Asia as another right-hand-side variable. Of course, not all of the nomadic invasions that Europe experienced originated in the same place, so we use the inverse distance from each principality to Avarga, Mongolia, the location of the first capital of the Mongolian Empire.

Since distance is invariant and fixed effects regressions cannot be run with time-invariant regressors, we only include this proximity variable in a random effect specification (table C.3.). However, using a Hausman test and comparing the results to those from an alternative random effects specification which mirrors the fixed effects model in table C.1., we contend that no biases are introduced by failing to include individual fixed effects.

Our next variable of interest is income, as higher income has been hypothesised as reducing violence as well as elite violence (Baten et al. 2014). Many recent economic history studies use urbanisation rates as a reliable proxy of income among early societies where alternative GDP measurements are unavailable (Bosker et al. 2013; De Long and Shleifer 1993; Acemoglu et al. 2005; Nunn and Qian 2011; Cantoni 2015; Cantoni and Yuchtman 2014). We expect increased income to be negatively associated with interpersonal violence, as outside options to violent conduct arise with financial freedom. Individuals and societies with greater incomes will have faced fewer problems of scarcity and would therefore have experienced less social unrest.

Additionally, in their study of violence based on cranial traumata and weapon wounds, Baten and Steckel (2018) found evidence that rates of interpersonal violence first declined in

[37] The Hungarians had already settled in today's Hungary by late 9th century and had, by the beginning of the 11th century, abandoned their nomadic lifestyles in favour of a more settled, somewhat urban lifestyle.

urban centres. This lends support for the hypothesis that income is negatively associated with violence, provided that the income-urbanisation relationship holds, or that city walls and local government prevented violence. Bosker et al. (2013) theorise that one of the reasons for this widely researched income-urbanisation relationship is due to agricultural productivity. Their hypothesis suggests that productive agricultural sectors are required in order to support large urban centres, as these are unable to produce their own agricultural products; making agricultural productivity particularly important in the absence of today's efficient trading systems and without technologies such as refrigeration. Throughout our timeline, agriculture would have contributed to a very large share of each economy, as is characteristic among developing states. Relative decline in the importance of the agricultural sector only began to change with the industrial revolution, after which sectors such as manufacturing began to grow disproportionately. However, most of Europe only began to industrialise well after the inception of the industrial revolution in late 18th century England, meaning that this income-urbanisation relationship should have held throughout our period of study (Baten 2016).

However, many studies have also found that levels of violence were higher in urban centres over the 20th century, chiefly citing the losses of personal networks and societal support structures that are associated with living in small rural villages (Baten et al. 2014). The lack of communal support may have put pressure on resource acquisition and failed to prevent individuals from falling into poverty, thereby increasing both theft and violence. Additionally, the impersonal structure of cities may have increased incentives to appropriate resources from others and may have diminished any sense of community security that may have existed in rural villages, also potentially leading to violence. Through our analysis, we hope to gain some insight into which effect is dominant among early societies.

We constructed our urbanisation variable using Bosker et al.'s (2013) estimates of urban populations – urban centres defined as cities with a population of at least 5000 inhabitants –

and calculated urbanisation rates using McEvedy and Jones' (1978) measurements of country populations by century. As Bosker et al.'s (2013) urban population estimates end in 1800; these were then augmented with urbanisation rates from the Clio Infra database for the 19th century.

Next, we also make use of temperature in order to proxy for agricultural output. Agricultural output is a dimension of income that is less reflected by urban growth, but it could still determine the opportunity costs of violence for elites. This is particularly important in the context of the 'Little Ice Age'. The 'Little Ice Age' has come to be known as a period of general cooling throughout the Northern Hemisphere and particularly in Europe between about 1300 and 1850, with its most severe period in the 16th and 17th centuries (Mann 2002a). Alternatively, it refers to the period between what is known as the 'Medieval Climatic Optimum' – a relatively warmer period from about 900 to 1300 CE – and the warmer modern period that began around the time of the industrial revolution (Mann 2002b). The 'Little Ice Age' was characterised by exceedingly cold winters during which rivers were said to have frozen while crop yields were decimated, even in relatively temperate European regions such as the Mediterranean states (Mann 2002a). The sources for these events have been mainly anecdotal in nature until fairly recently. However, more recent studies in historical climatology have provided economic historians with a plethora of long run temperature series from a variety of sources. These include evidence from tree rings, corals, ice-core isotopes and pollen assemblages, comparing them to the existing anecdotal evidence where possible (Guiot and Corona 2010). These sources also tend to be exceptionally consistent regardless of which indicators are used (Guiot and Corona 2010).

To estimate agricultural output, we employ temperature reconstructions from Guiot and Corona (2010), who consider all of the above methods to reconstruct annual summer temperatures for all of Europe in a 5x5 degree grid pattern over the last 1400 years. These are then applied to each of our principality units based on the grid nodes closest to their historical capitals. These temperature series are measured as the deviation in degrees Celsius from the 1961–1990 mean at each node (Guiot and Corona 2010).[38]

So far, we have described the potential ways in which environmental and social factors could be associated with rates of violence, but institutional factors could also potentially play a role. To that end, we use variables geared specifically towards regicide, namely autonomy and the mode of succession, and various religious variables which may reflect some degree of institutional quality for the society as a whole. We define autonomy as a ruler's unhindered ability to make decisions and to dictate policy – for example, Transylvania would not be considered a completely autonomous state while it was subject to tributes to the Ottoman Empire. We control for autonomy under the hypothesis that a ruler is more likely to be killed if their successor is able to act autonomously. Alternatively, rulers of subservient principalities may have been more likely to be killed by their overlords who would then be able to install more cooperative leaders. Further, a lack of autonomy may have created conflict over how to resolve the problem of an external state dictating local laws – possibly even in the context of an extractive tribute. This was famously the case in Wallachia, where Vlad III Dracul ended the tradition of tribute to the Ottoman Empire after his father's assassination, and was later killed in battle against the Ottomans (Wright 2018). Both he and his father were killed amidst a complicated and fluctuating system of alliances, treaties and tributes between Wallachia, Hungary, Transylvania (under Hungarian administration) and the Ottoman Empire (Wright 2018).

Since the majority of rulers were killed by family members hoping to take the throne, we also control for mode of succession. Under electoral systems, these power-hungry relatives would have had a lower chance of being elected, decreasing the probability of regicide. We split this indicator into three levels: hereditary systems, ceremonial electoral systems and de

[38] See appendix for a note on smoothing.

facto electoral systems. The reason for this is because many principalities held elections among a group of the elite but then simply voted for the direct heir of the previous ruler – possibly out of fear of retribution from the ruling family, due to political ties or for continuance in policy. For example, this was the case in the Holy Roman Empire between 1453 and 1740, where a member of the House of Habsburg was always elected. However, even the ceremonial existence of elections reveals some kind of preference for shared decision making, which may have been associated with more inclusive institutions than under completely hereditary systems of succession. Consequently, we use a three part indicator variable rather than a dummy.

Like the mode of succession, we anticipate that religion could have played a role in determining long term violence through possible cultural differences or differences in institutional quality. Therefore, we use an indicator variable for the majority religion in each principality, under the categories: Catholicism, Orthodoxy, Protestantism, Islam and Other. The 'Other' category includes Paganism and tribal religions from times before each principality adopted one of Europe's largest four modern religions. Additionally, we include dummy variables for religious diversity and religious transition. Religious diversity may have led to conflict and transition may have caused violence due to opposing forces trying to preserve old orders or instil new ones. Furthermore, we introduce a dummy variable for whether a country had a significant Jewish minority, as Jews often held above average income and human capital, despite being the targets of numerous forms of persecution throughout Europe over our timeline.

We also control for fractionalisation, measured as three or more principalities overlapping with a particular modern country. Borcan et al. (2018) suggested using modern boundaries as a benchmark for historical principality size. In this manner, we also aim to control for conflict between principalities that may be driven by fractionalisation that is not explained by the other independent variables.

Since some studies describe a relationship between geographical factors and violence, we include certain geographical controls here. For example, Bohara et al. (2006) describes how more rugged terrain protects instigators of violent insurgencies, while Nunn and Puga (2012) assert that ruggedness protected certain West African regions from the Atlantic slave trade. Pinker (2011) also argued that mountainous terrain inhibits policing functions. Therefore, we include Nunn and Puga's (2012) ruggedness measure. As discussed, access to agricultural resources could have an impact on violence, so we also include Nunn and Puga's (2012) measures of fertile soil distribution as an additional control for agricultural productivity. Further, access to agricultural trade via sea could also have been important, so we also include their measure of the percentage of each country that lies within 100 km of ice-free coast. Since these geographical variables are time-invariant, they are only included in the random effects specification (table C.3.).

Lastly, we use three dummy variables in order to capture the effects of periods in which major societal transformations took place; the Justinian Plague, the Great Plague and the second serfdom. The Great Plague and its devastation of Europe's population in the 14th century has been thoroughly researched, and the subsequent societal upheaval could have played a role in impacting interpersonal violence through societal fear and resource scarcity. Scarcity would also have been compounded in cities, as they would have received limited imports, particularly as agricultural industries collapsed from a depleted labour force. The Justinian Plague could also have had a similar impact as it killed approximately 50 million people – an estimated 15% of the world's population – in what is now Turkey and throughout the Mediterranean states between the 6th and 8th centuries (Caspermeyer 2016). Finally, we use the second serfdom as a case study in order to test whether inequality has had a significant impact on regicide and interpersonal violence. We assess the second serfdom using a dummy variable for Eastern European countries in the 16th, 17th and 18th centuries, and in Russia for the 19th century; as serfdom in Russia was only abolished under Tsar Alexander II in 1861.

C.4. Methodology and Results

Having presented a variety of hypotheses, we test whether these potential correlates share significant relationships with regicide. We first employ a standard fixed effects specification in order to account for any omitted variable bias that may be caused by the absence of any relevant time-invariant variables. As a robustness measure against time trends within each variable, we also use time fixed effects throughout our analysis in addition to individual fixed effects, although the issue of stationarity does not seem affect regicide across panels (table A.C.7.). Our fixed effects specification is as follows:

$$regicide_{it} = \alpha_i + \gamma_t + \beta_1 territrial\ state\ capacity_{it} + \beta_2 temperature_{it} + \beta_3 urbanisation_{it} + \beta_4 battle_{it} + \beta_k \psi_{it} + \varepsilon_{it} \quad (1)$$

Where α_i are country fixed effects, γ_t are two-century fixed effects, ψ_{it} is a vector of control variables and ε_{it} is an error term. We also employ cluster-robust standard errors as we do not expect within-principality observations to be entirely independent of one another; potentially due to common cultural, climatic or geographic features which may influence within-region rates of interpersonal violence.

Table C.1. shows the results of the fixed effects regression. Immediately we can see that territorial state capacity enters all regressions both significantly and negatively, with a stable coefficient of around -0.08. This can be interpreted as a one percentage point increase in a state's growth rate being associated with a 0.08 percentage point decrease in regicide. Alternatively, a one standard deviation increase in the state growth rate is associated with a 5.5 percentage point decrease in regicide. Since this interpretation is somewhat unorthodox, the first difference specification, used below, allows for a more natural interpretation. Broadly, this result confirms the Pinker (2011), Fearon and Laitin (2003) and Richani (2010) hypotheses that strong state capacities have a violence-reducing effect.

The fixed effects specification offers two additional conclusions. First, principalities where Orthodoxy is the majority religion seem to be about 20 percentage points more violent, on average, even after implementing principality fixed effects and clustering by principality. Secondly, the regression provides some evidence that fractionalisation is negatively related to regicide, which contradicts the theory of competing groups enacting violence against one another, although this it is only significant at the 10% level of significance.

For a more natural interpretation of the expansion variable and as a robustness check against spurious correlation that may arise from variables that follow a unit root process, we also perform first difference regressions in table C.2. First difference regressions, like fixed effects, have the advantage of eliminating omitted variable bias caused by absent time invariant variables, but are also effective in eliminating spurious correlations from time trends (Wooldridge 2012). However, this comes at a cost as differencing removes much of the variation in the variables, attenuating standard errors and potentially leading to type-2 errors (Wooldridge 2012). As such, it is a somewhat harsh robustness test in a panel setting.

$$\Delta regicide = y_{it} + \beta_1 territrial\ state\ capacity_{it} + \beta_2 \Delta temperature_{it} + \beta_3 \Delta urbanisation_{it} + \beta_4 \Delta battle_{it} + \beta_k \psi_{it} + \varepsilon_{it} \qquad (2)$$

Again, we see that the coefficients for territorial state capacity are negative and significant, and slightly larger than under the fixed effects specification, approximately -0.095 on average. Therefore, a one percentage point increase in state capacity is associated with a 0.095 percentage point decrease in interpersonal violence. Alternately, a one standard deviation increase in territorial state capacity is associated with approximately a 6.6 percentage point decrease in regicide.

Aside from territorial state capacity, the only other variable to enter the first difference model significantly is religious transition. On average, changes in majority religion are associated with an 11-percentage point higher regicide rate. Some of the strongest examples of these periods occurred during the protestant reformation, where our data shows France, Austria and England to have undergone the strongest transition effects.

In order to include the proximity to invasion term, we also run a random effects model (table C.3.). Initially, there is a significant positive relationship between invasion proximity and regicide, but this disappears once territorial state capacity enters the model. This either suggests that territorial state capacity was more important for elite violence than the invasions, or that the invasions affected elite violence through territorial state capacity. Although the coefficient for territorial state capacity remains stable at between -0.06 and -0.07, only weak evidence of relationships between other right-hand-side variables and regicide exist.

Overall, the evidence from tables C.1. to C.3. points towards a robust inverse relationship between territorial expansion and regicide, and consequently provide evidence that state capacity may have had a restraining effect on elite violence. However, despite the strong and stable coefficients the relationship is not necessarily causal, since reverse causality from regicide to state capacity may apply.

C.5. Conclusion

We provide new evidence on the history of elite violence by using Eisner's (2011) method of measuring regicide and identify relationships between European homicide and regicide between the 6th and 19th centuries CE. This link is motivated by the close relationship that it appears to share with patterns in homicide and because the impacts of many of Europe's historical events can be seen within the regicide series.

When comparing Eastern and Western Europe, we see that South-Eastern and North-Eastern Europe clearly exhibited higher rates of regicide than Western and Central Europe did over the initial and latter parts of our period of study period. We then see a clear divergence of North-Eastern Europe during the Mongolian invasion period whereas the south-east diverged during the 15th to 19th centuries.

Fixed effects and first difference strategies are then employed in order to analyse the relationship between territorial state capacity and long run interpersonal elite violence in Europe between the 6th and 19th centuries, finding a robust negative association. Although we cannot claim that the results are causal, this implies that state capacity, reflected by territorial state capacity, likely had a restraining effect on interpersonal violence. The relationship appears to be driven by the period between the 10th and 15th centuries.

This result contributes to the literature about the emergence of modern tax states, and Tilly's (1975) hypothesis that "war generated states". Many authors argue that the state's capacity to tax developed as a result of military conflict, such as France during the Hundred Years' war. The hypothesis suggests a positive correlation between state capacity and elite violence in the early period. We interpret the fact that we find a negative relationship as support of the Pinker (2011) hypothesis that higher state capacity, and the value systems it introduced, resulted in lower interpersonal violence. One potential explanation for Tilly's findings for France, since it was a low-violence kingdom at the time of the Hundred Years' War, could be that the war acted as a catalyst in exposing potential weaknesses, and that taxing its citizens was necessary in order for France to remain a military power.

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C.7. Tables and Figures

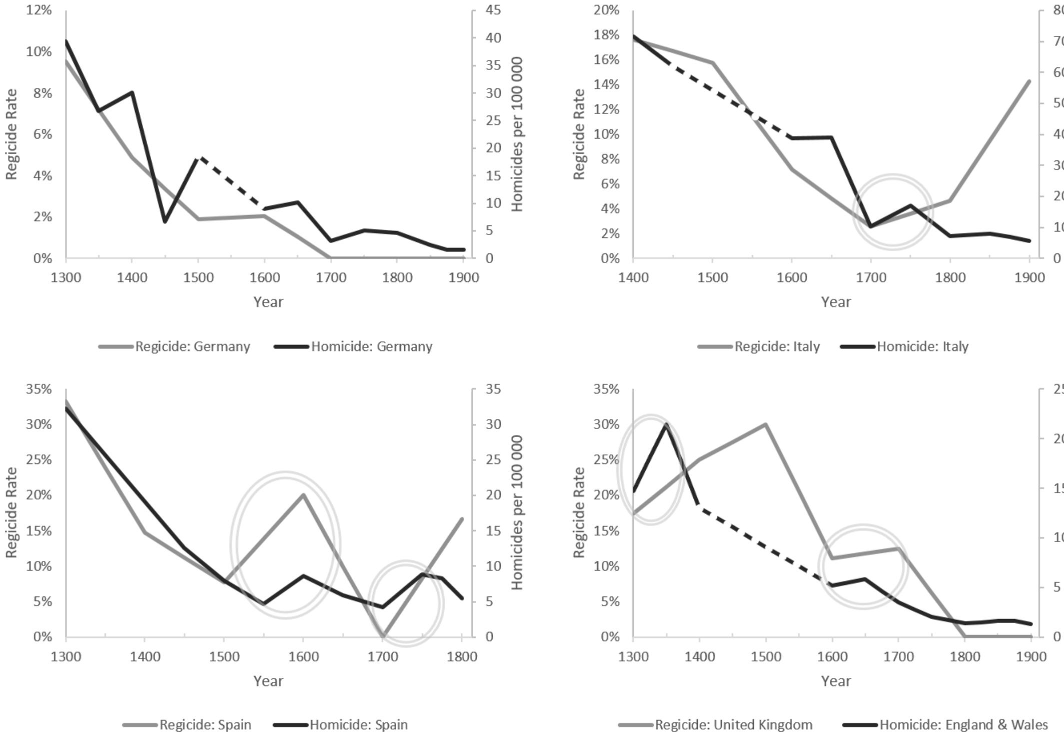


Figure C.1. Regicide vs Homicide

Note: Centuries are rounded up, i.e. 1500 refers to the 15th century. Dashed lines indicate interpolations where homicide data is unrecorded. Grey circles indicate simultaneous increases. *Sources*: Homicide data from Eisner (2014).

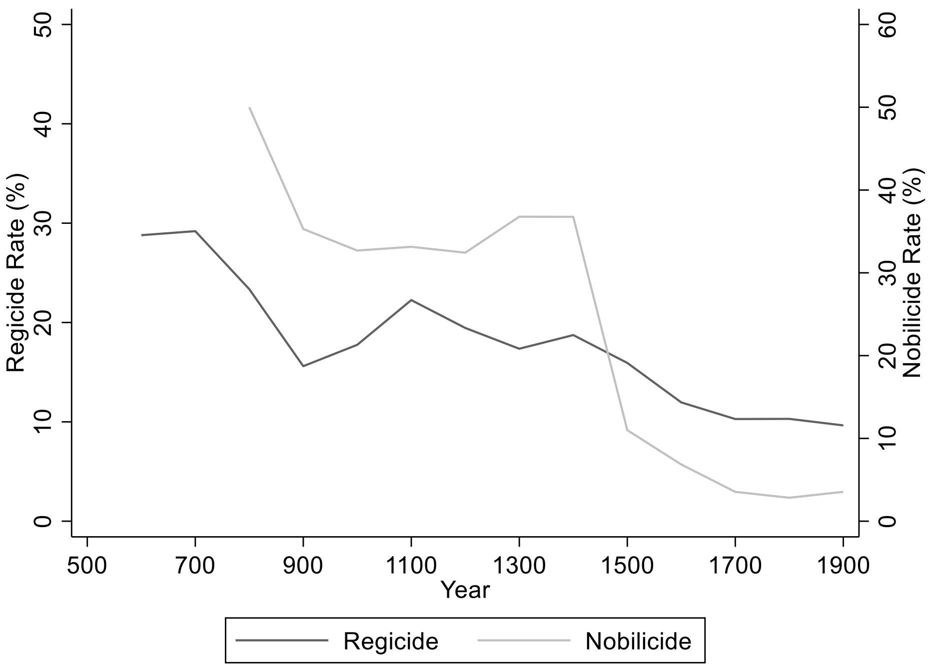


Figure C.2. Timeline of Regicide and Nobilicide (Nobilicide from Battles)

Note: Centuries are rounded up, i.e. 1500 refers to the 15th century.

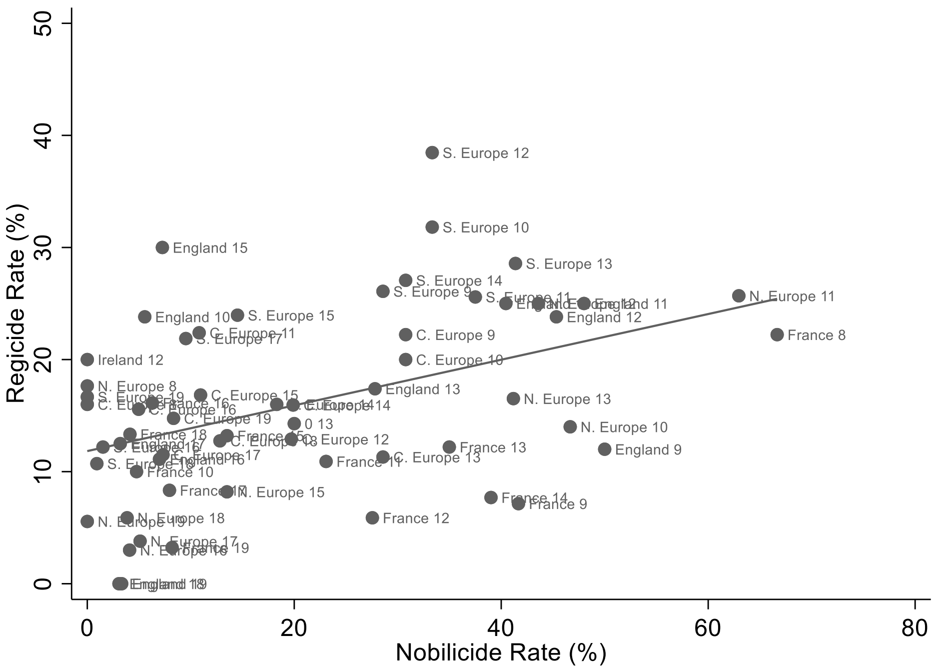


Figure C.3. Regicide versus Nobilicide (Nobilicide from Battles)

Note: Centuries are rounded up and abbreviated, i.e. 15 refers to the 15th century. Regional disaggregation follows Cummins (2017) where S. Europe refers to Southern Europe, C. Europe refers to Central Europe and N. Europe refers to Northern Europe. *Source*: Nobilicide data from Cummins (2017).

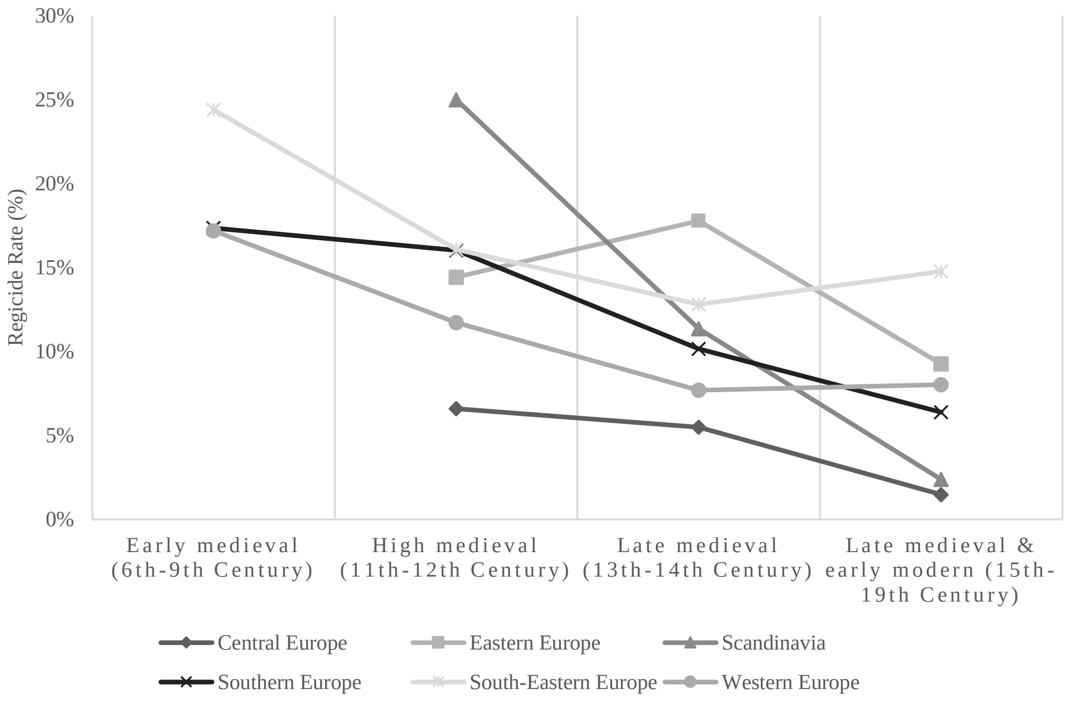


Figure C.4. Regicide and the Second Serfdom

Note: Regicide for the early medieval period in Eastern Europe was omitted here, as its 50% regicide rate relies on small N and would have obscured the graphic.

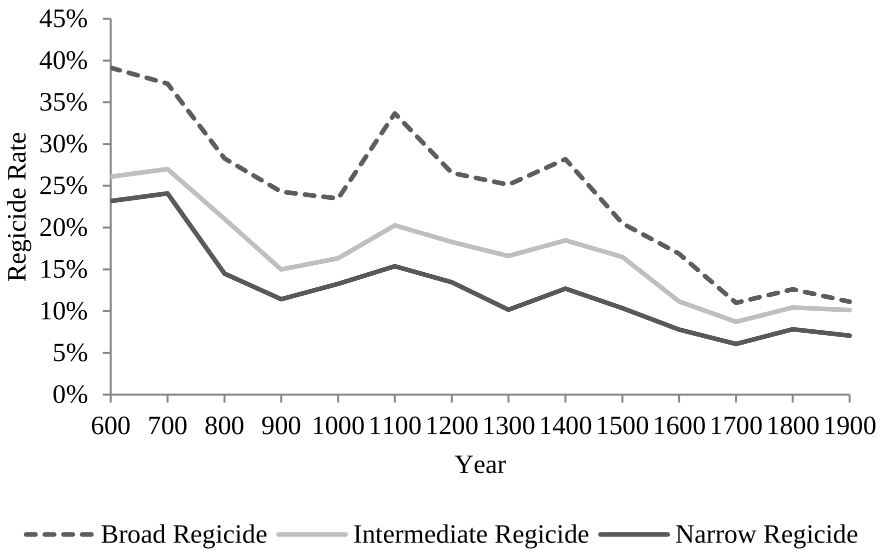


Figure C.5. Defining Regicide

Note: centuries are rounded up, i.e. 1500 refers to the 15th century.

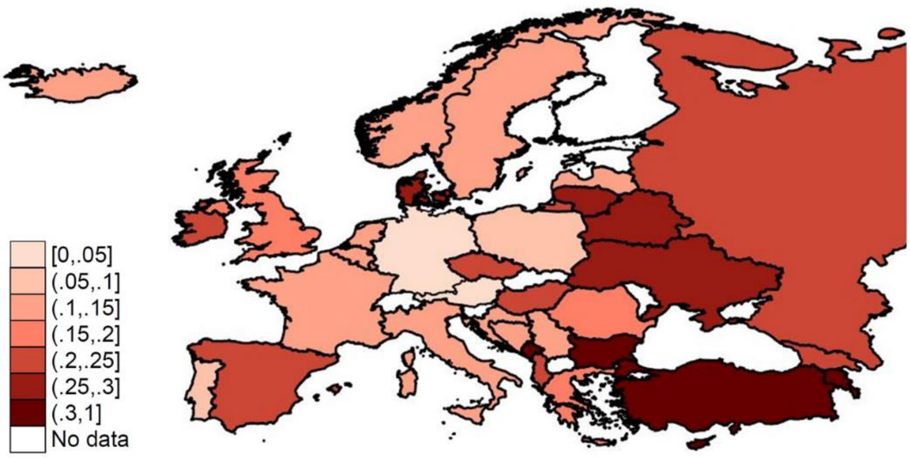
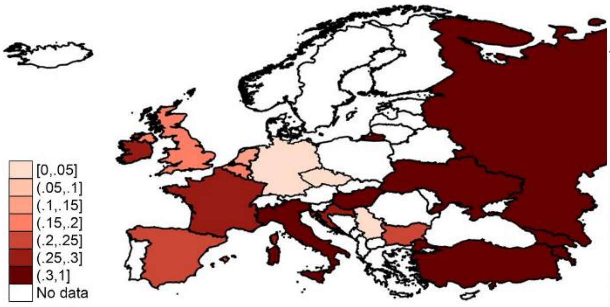
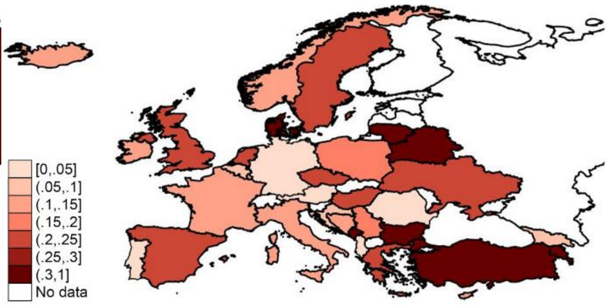


Figure C.6. European Regicide: 6th – 19th Century

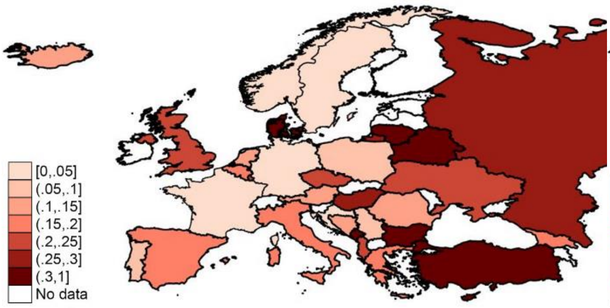
Note: The darker colours demonstrate greater elite violence.



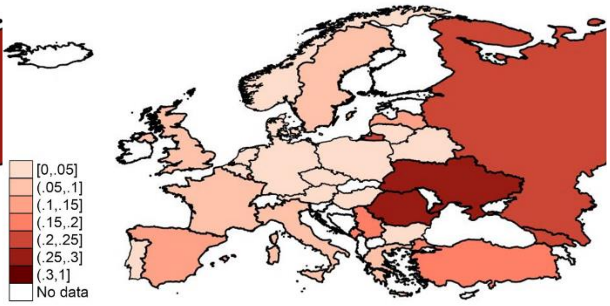
a) 6th – 9th Century



b) 10th – 13th Century



c) 13th – 15th Century



d) 15th – 19th Century

Figure C.7. European Regicide by Period

Note: The darker colours demonstrate greater elite violence.

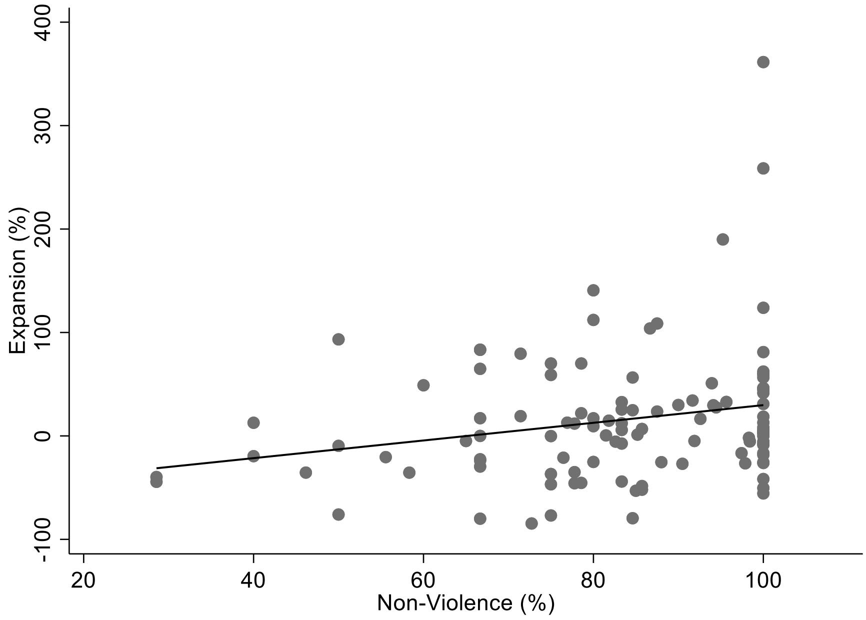


Figure C.8. Non-Violence and Territorial State Capacity (6th to 19th Century CE)

Note: Non–violence is measured by *1-proportion of killed rulers*

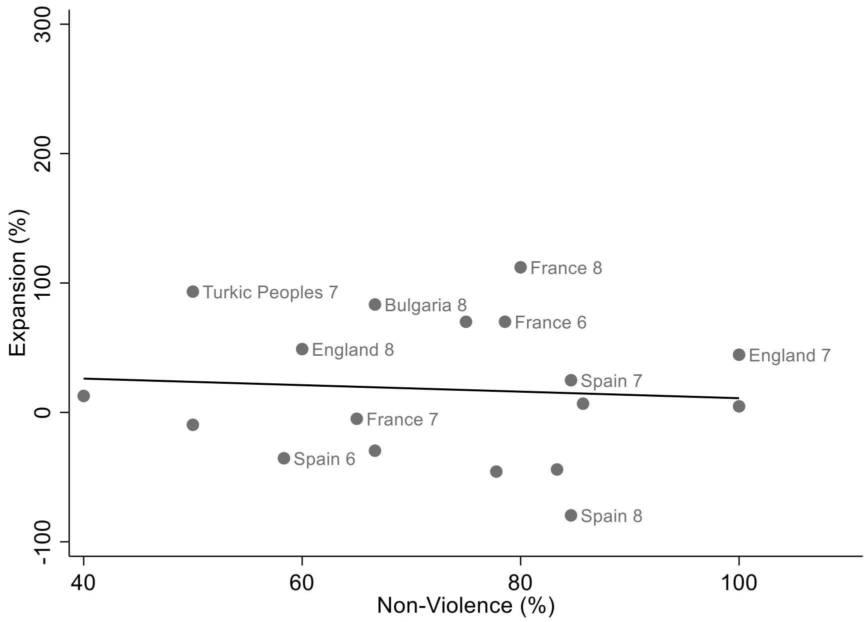


Figure C.9. Non-Violence and Territorial State Capacity (6th to 9th Century CE)

Note: Centuries are rounded up, i.e. 1500 refers to the 15th century. Non–violence is measured by *1-proportion of killed rulers.*

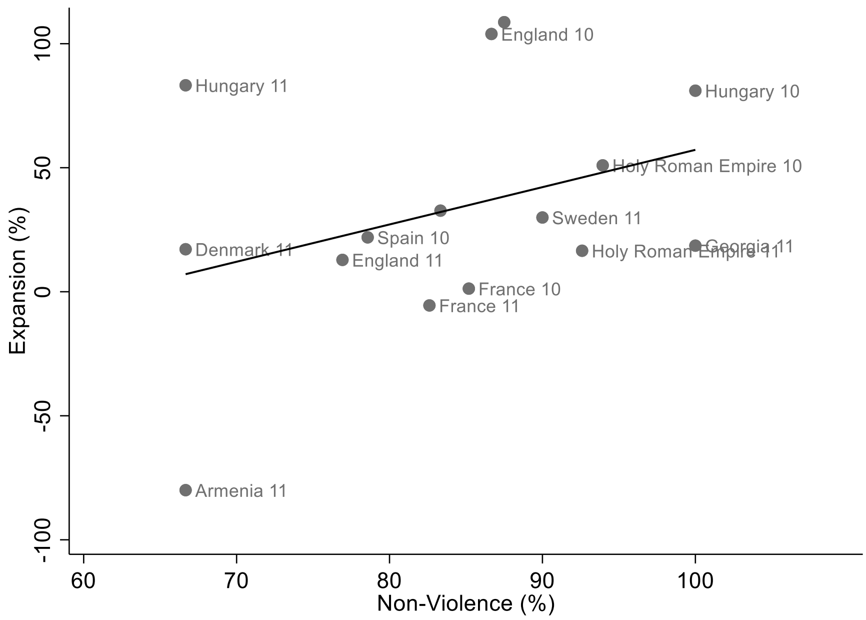


Figure C.10. Non-Violence and Territorial State Capacity (10th to 11th Century CE)

Note: Centuries are rounded up, i.e. 1500 refers to the 15th century. Non–violence is measured by *1-proportion of killed rulers.*

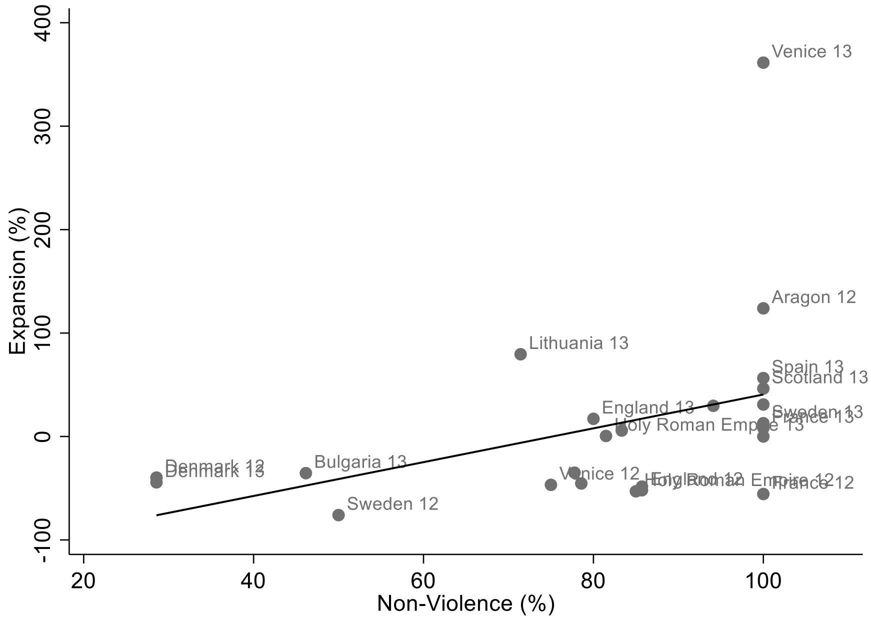


Figure C.11. Non-Violence and Territorial State Capacity (12th to 13th Century CE)

Note: centuries are rounded up, i.e. 1500 refers to the 15th century. Non–violence is measured by *1-proportion of killed rulers.*

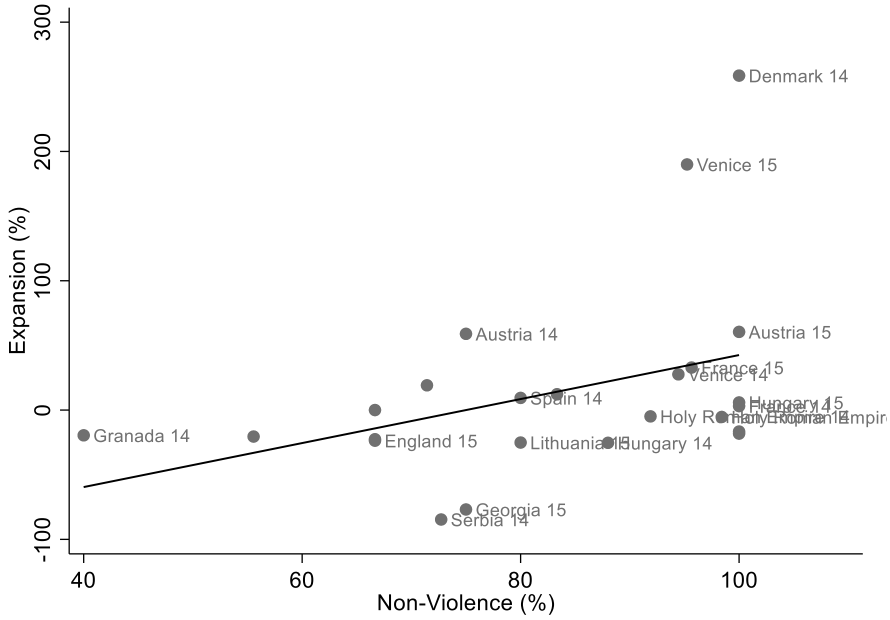


Figure C.12. Non-Violence and Territorial State Capacity (14th to 15th Century CE)

Note: centuries are rounded up, i.e. 1500 refers to the 15th century. Non–violence is measured by *1-proportion of killed rulers*.

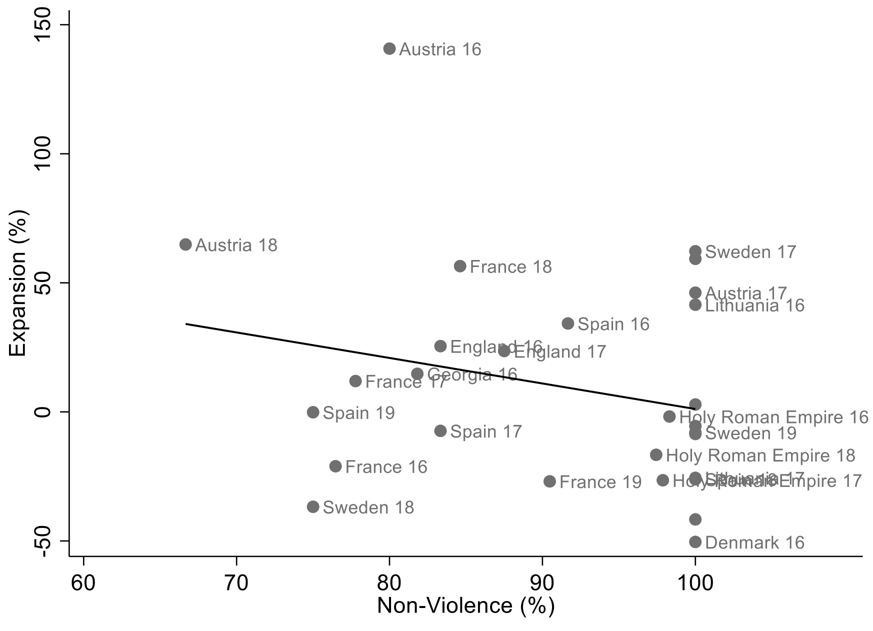


Figure C.13. Non-Violence and Territorial State Capacity (16th to 19th Century CE)

Note: centuries are rounded up, i.e. 1500 refers to the 15th century. Non–violence is measured by *1-proportion of killed rulers*.

	(1) Regicide	(2) Regicide	(3) Regicide	(4) Regicide	(5) Regicide	(6) Regicide	(7) Regicide	(8) Regicide	(9) Regicide	(10) Regicide
Territorial State Capacity	**-0.0781*** **	**-0.0772*** **	**-0.0752** **	**-0.0786** **	**-0.0754** **	**-0.0744***	**-0.0746***	**-0.0803** **	**-0.0857*** **	**-0.0803** **
	(0.0266)	(0.0251)	(0.0285)	(0.0319)	(0.0340)	(0.0366)	(0.0369)	(0.0347)	(0.0270)	(0.0347)
Temperature		-0.0722	-0.0721	-0.0622	-0.0824	-0.118	-0.118	-0.0774		-0.0774
		(0.132)	(0.133)	(0.137)	(0.139)	(0.136)	(0.136)	(0.134)		(0.134)
Urbanisation			-0.0663	-0.0406	-0.0282	-0.00776	-0.00634	-0.0497		-0.0497
			(0.201)	(0.281)	(0.283)	(0.253)	(0.249)	(0.327)		(0.327)
Mode of Succession (Base=Hereditary)										
● Partially Elected				0.0281	0.0200	0.00787	0.00803	-0.00244		-0.00244
				(0.0587)	(0.0621)	(0.0488)	(0.0503)	(0.0700)		(0.0700)
● Fully Elected				-0.0523	-0.0672	-0.0506	-0.0502	-0.0471		-0.0471
				(0.0510)	(0.0526)	(0.0539)	(0.0556)	(0.0747)		(0.0747)
Battle						0.434	0.436	0.192		0.192
						(0.338)	(0.344)	(0.321)		(0.321)
Autonomy							-0.00594	-0.0430		-0.0430
							(0.0472)	(0.0354)		(0.0354)
Fractionalisation					-0.0634	**-0.0613***	**-0.0609***	-0.0616		-0.0616
					(0.0377)	(0.0341)	(0.0351)	(0.0529)		(0.0529)
Religion (Base=Catholic)										
● Islamic								-0.0285	-0.0263	-0.0285
								(0.0824)	(0.0541)	(0.0824)
● Orthodox								**-0.178** **	**-0.200*** **	**-0.178** **
								(0.0780)	(0.0610)	(0.0780)
● Protestant								-0.0912	-0.107	-0.0912
								(0.0774)	(0.0719)	(0.0774)
● Other								-0.0708	-0.0902	-0.0708
								(0.111)	(0.0717)	(0.111)
Jewish Minority								0.0162	-0.00807	0.0162
								(0.0612)	(0.0552)	(0.0612)
Religious Diversity								0.0207	0.0254	0.0207
								(0.0500)	(0.0491)	(0.0500)
Religious Transition								0.0101	0.0194	0.0101
								(0.0578)	(0.0543)	(0.0578)
Black Plague	0.00245	0.00289	0.00119	-0.00543	-0.00328	-0.0166	-0.0160	-0.0289	-0.0157	-0.0289
	(0.0541)	(0.0535)	(0.0536)	(0.0553)	(0.0553)	(0.0548)	(0.0555)	(0.0612)	(0.0620)	(0.0612)
Justinian Plague	0.0837								0.102	
	(0.108)								(0.0995)	
Second Serfdom	0.0386	0.0399	0.0367	0.0893	0.0987	0.0670	0.0662	0.0541	0.0388	0.0541
	(0.111)	(0.111)	(0.113)	(0.116)	(0.114)	(0.110)	(0.110)	(0.0865)	(0.0810)	(0.0865)
Constant	**0.279*** **	**0.293*** **	**0.294*** **	**0.316*** **	**0.354*** **	**0.330*** **	**0.335*** **	**0.386*** **	**0.307*** **	**0.386*** **
	(0.0834)	(0.0952)	(0.0962)	(0.0905)	(0.0750)	(0.0714)	(0.0884)	(0.0952)	(0.107)	(0.0952)
Observations	109	106	106	106	106	106	106	106	109	106
Within R^2	0.231	0.209	0.210	0.222	0.231	0.260	0.260	0.308	0.313	0.308
Number of Principalities	34	34	34	34	34	34	34	34	34	34
Principality FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Cluster robust standard errors in parentheses (clustered by principalities)
*** p<0.01, ** p<0.05, * p<0.1

Table C.1. Fixed Effects Regressions

	(1) ΔRegicide	(2) ΔRegicide	(3) ΔRegicide	(4) ΔRegicide	(5) ΔRegicide	(6) ΔRegicide	(7) ΔRegicide	(8) ΔRegicide	(9) ΔRegicide
Territorial State Capacity	**-0.097****	**-0.093****	**-0.085***	**-0.082***	**-0.085****	**-0.090****	**-0.089****	**-0.107****	**-0.103****
	(0.043)	(0.046)	(0.047)	(0.045)	(0.041)	(0.044)	(0.043)	(0.044)	(0.045)
ΔTemperature		-0.0129	-0.0254	-0.0202	-0.0212	-0.0461	-0.0465		-0.0829
		(0.117)	(0.121)	(0.115)	(0.112)	(0.113)	(0.114)		(0.125)
ΔUrbanisation			-0.317	-0.322	-0.380	-0.342	-0.347		-0.336
			(0.238)	(0.244)	(0.241)	(0.241)	(0.245)		(0.342)
Mode of Succession (Base=Hereditary)									
● Partially Elected				-0.056	-0.069	-0.083	-0.084		-0.083
				(0.120)	(0.125)	(0.130)	(0.131)		(0.144)
● Fully Elected				-0.0212	-0.033	-0.027	-0.027		-0.003
				(0.056)	(0.061)	(0.061)	(0.061)		(0.070)
Fractionalisation					0.068	0.0712	0.0703		0.0688
					(0.060)	(0.062)	(0.064)		(0.072)
ΔBattle						0.346	0.343		0.363
						(0.294)	(0.294)		(0.294)
Autonomy							0.011		-0.004
							(0.106)		(0.098)
Black Plague	-0.018	-0.016	-0.041	-0.039	-0.055	-0.064	-0.065	-0.001	-0.045
	(0.084)	(0.094)	(0.106)	(0.098)	(0.099)	(0.099)	(0.100)	(0.088)	(0.101)
Justinian Plague	-	-	-	-	-	-	-	-	-
Religion (Base=Catholic)									
● Islamic								-0.081	-0.125
								(0.099)	(0.104)
● Orthodox								-0.046	-0.066
								(0.057)	(0.075)
● Protestant								-0.109	-0.065
								(0.100)	(0.125)
Jewish Minority								0.008	0.034
								(0.057)	(0.062)
Religious Diversity								0.017	0.012
								(0.050)	(0.054)
Religious Transition								**0.110***	**0.119***
								(0.065)	(0.060)
Constant	0.011	0.006	0.015	0.027	0.017	0.022	0.012	0.002	-0.001
	(0.028)	(0.028)	(0.032)	(0.041)	(0.041)	(0.040)	(0.103)	(0.044)	(0.103)
Observations	62	59	59	59	59	59	59	62	59
Number of Principalities	16	16	16	16	16	16	16	16	16
R-squared	0.113	0.112	0.122	0.128	0.147	0.169	0.169	0.159	0.220
Adjusted R^2	-0.052	0.064	0.057	0.027	0.030	0.036	0.016	0.032	-0.051
Principality FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO
Time FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO

Cluster Robust standard errors in parentheses (clustered by principalities)
*** p<0.01, ** p<0.05, * p<0.1

Table C.2. First Differences Regressions

	(1) Regicide	(2) Regicide	(3) Regicide	(4) Regicide	(5) Regicide	(6) Regicide	(7) Regicide	(8) Regicide	(9) Regicide	(10) Regicide	(11) Regicide	(12) Regicide
Invasion Proximity	**8.599****	6.070	5.798	5.538	4.212	3.384	3.553	3.733	4.996	6.921	**16.85****	12.14
	(5.018)	(5.315)	(5.232)	(5.246)	(5.359)	(5.633)	(5.915)	(5.994)	(7.092)	(7.173)	(7.581)	(8.304)
Territorial State Capacity		**-0.070*****	**-0.068*****	**-0.063****	**-0.062****	**-0.061****	**-0.062****	**-0.062****	**-0.064****	**-0.062****	**-0.062****	**-0.066****
		(0.025)	(0.024)	(0.026)	(0.029)	(0.030)	(0.031)	(0.031)	(0.031)	(0.032)	(0.025)	(0.033)
Temperature			-0.0538	-0.0547	-0.0417	-0.0527	-0.0602	-0.0611	-0.0424	-0.0650		-0.0437
			(0.132)	(0.132)	(0.136)	(0.136)	(0.134)	(0.135)	(0.131)	(0.136)		(0.131)
Urbanisation				-0.201	-0.236	-0.186	-0.163	-0.157	-0.205	-0.217		-0.250
				(0.155)	(0.194)	(0.198)	(0.192)	(0.191)	(0.246)	(0.194)		(0.239)
Mode of Succession (Base=Hereditary)												
● Partially Elected					**0.0997***	0.0932	0.0880	0.0884	0.105	0.0929		0.0891
					(0.0567)	(0.0587)	(0.0622)	(0.0668)	(0.0652)	(0.0653)		(0.0583)
● Fully Elected					-0.0278	-0.0331	-0.0326	-0.0318	-0.0212	-0.00820		0.00509
					(0.0347)	(0.0378)	(0.0390)	(0.0411)	(0.0512)	(0.0417)		(0.0511)
Fractionalisation						-0.0317	-0.0323	-0.0320	-0.0358	-0.0333		-0.0389
						(0.0428)	(0.0432)	(0.0443)	(0.0413)	(0.0433)		(0.0425)
Battle							0.108	0.124	0.0450	0.190		0.136
							(0.268)	(0.270)	(0.256)	(0.280)		(0.272)
Autonomy								-0.0169	-0.0191	-0.0269		-0.0313
								(0.0526)	(0.0541)	(0.0561)		(0.0566)
Religion (Base=Catholic)												
● Islamic									0.0241		-0.0311	-0.00364
									(0.0662)		(0.0722)	(0.0651)
● Orthodox									-0.0514		-0.0766	-0.0813
									(0.0697)		(0.0611)	(0.0613)
● Protestant									-0.0399		0.0343	-0.0312
									(0.0677)		(0.0570)	(0.0675)
● Other									0.0456		-0.0247	0.0234
									(0.125)		(0.113)	(0.119)
Jewish Minority									-0.00934		-0.0421	-0.0142
									(0.0534)		(0.0402)	(0.0523)
Religious Diversity									0.0125		0.0373	0.0287
									(0.0405)		(0.0431)	(0.0448)
Religious Transition									0.00742		-0.0209	-0.00741
									(0.0561)		(0.0581)	(0.0581)
% Within 100 km. of ice-free coast										0.0007	0.0010	0.0009
										(0.0006)	(0.0007)	(0.0007)
% Fertile soil										0.0027	0.0023	0.0031
										(0.0017)	(0.0017)	(0.0022)
Ruggedness										0.0009	0.0002	0.0006
										(0.0011)	(0.0009)	(0.0011)
Black Plague	0.0441	0.0392	0.0414	0.0390	0.0400	0.0433	0.0402	0.0376	0.0491	0.0378	0.0703	0.0421
	(0.0622)	(0.0549)	(0.0552)	(0.0553)	(0.0573)	(0.0556)	(0.0554)	(0.0548)	(0.0582)	(0.0559)	(0.0640)	(0.0608)
Justinian Plague	0.0277	0.0373									0.0773	
	(0.113)	(0.0929)									(0.0899)	
Second Serfdom	-0.00387	0.0181	0.0190	0.00650	0.0454	0.0490	0.0468	0.0457	0.0368	0.0191	0.0351	0.00640
	(0.0936)	(0.104)	(0.104)	(0.100)	(0.0966)	(0.0989)	(0.101)	(0.1000)	(0.0757)	(0.0986)	(0.0850)	(0.0775)
Constant	**0.291*****	**0.308*****	**0.325*****	**0.327*****	**0.333*****	**0.345*****	**0.340*****	**0.355*****	**0.351*****	0.134	0.0743	0.0960
	(0.0656)	(0.0679)	(0.0796)	(0.0795)	(0.0773)	(0.0737)	(0.0789)	(0.0982)	(0.100)	(0.153)	(0.161)	(0.179)
Observations	109	109	106	106	106	106	106	106	106	106	109	106
Between R-squared	0.3455	0.3025	0.3218	0.3355	0.3545	0.3318	0.3062	0.3001	0.3217	0.3453	0.3644	0.3489
No. Principalities	34	34	34	34	34	34	34	34	34	34	34	34
Principality FEs	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Time FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Cluster robust standard errors in parentheses (clustered by principalities)
*** p<0.01, ** p<0.05, * p<0.1

Table C.3. Random Effects Regressions

C.8. Appendix

The following descriptive statistics provide an overview of the distribution of our data and aid in the interpretations of regressions.

Variable	Observations	Mean	Standard Deviation	Min	Max
Autonomy	110	0.8818	0.3243	0	1
Battle	109	0.0562	0.0759	0	0.3333
Territorial State Capacity	110	0.1787	0.6921	-0.8464	3.6140
Fractionalisation	110	0.2545	0.4376	0	1
Great Plague	110	0.1364	0.3447	0	1
Invasion Proximity	110	0.00019	0.00001	0.00017	0.00021
Jewish Minority	110	0.3364	0.4746	0	1
Justinian Plague	110	0.0182	0.1342	0	1
Mode of Succession	110	0.6545	0.9030	0	2
Regicide	109	0.1689	0.1692	0	0.7143
Religion	110	1.8364	1.3377	1	5
Religious Diversity	110	0.3818	0.4881	0	1
Religious Transition	110	0.1727	0.3797	0	1
Second Serfdom	110	0.0364	0.1881	0	1
Temperature	107	0.0136	0.2216	-0.5894	0.5834
Urbanisation	110	0.0854	0.0973	0	0.4708

Table C.4. Descriptive Statistics

C.8.1. Sampling and Proxy Measurement Error

One of the advantages of using regicide as an indicator of interpersonal violence over homicide is that we have access to near complete dynastic lists and are not subject to sampling biases from overlooked information. However, small sample sizes may induce strong deviations in regicide and misrepresent the relationship between it and interpersonal violence.

Figure A.C.1. indicates the total number of rulers per century and shows that there were hundreds of rulers across all time periods. The lowest numbers are available for the early Middle Ages in Eastern Europe (ca. 30 per century). This means that low observation density is unlikely to have caused spurious conclusions when conducting analyses on European or on regional levels. Since the trends that we study are disaggregated to the regional or European level, and since our regressions take all of Europe into account, we see no reason why this potential for error in approximating interpersonal violence would lead to systematic biases. Nevertheless, as a precaution against this kind of measurement error, we require a minimum of five rulers per principality, per century in all of our analyses.

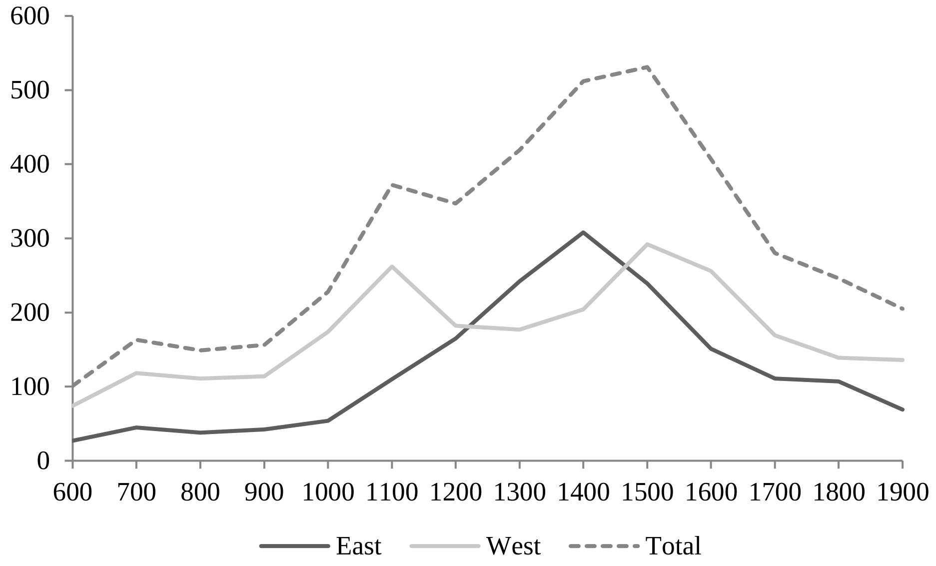


Figure C.14. Rulers per Century

Note: centuries are rounded up, i.e. 1500 refers to the 15th century.

C.8.2. Female Rulers

Considerable research has shown that women display lesser violent tendencies than men (Lussier et al. 2012; Steffensmeier and Allan, 1996). Consequently, female rulers may have provoked fewer rivals looking to obtain the throne, as they may have caused fewer disputes leading to regicide. As a result, we also considered investigating whether female rulers were killed as often as their male counterparts. Unfortunately, our entire dataset only contains 138 female rulers from across all countries and periods, so we are reluctant to construct a female regicide rate or attempt to use the proportion of female rulers as a regressor. Further, the patriarchal organisation of most early societies might mean that the presence of female rulers reflects the effect of improved institutional quality rather than any gender specific effect on violence. In our sample, only ten female rulers (7.25%) were killed, five of which fall under the dubious regicide classification. Although this figure is less than half the overall regicide rate of 16.78%, the number of observations provides limited statistical evidence that violence levels were lower under the authority of female rulers. Table A.C.2. lists all female rulers in our dataset that were killed.

Ruler	Principality	Regicide	Dubious	End of Reign
Amalasuintha	Ostrogoths	1	0	534
Joanna	Duchy of Durazzo	0	1	1368
Joanna I	Naples	1	0	1382
Maria I	Hungary	0	1	1385
Margaret I	Denmark	0	1	1412
Chiara Zorzi	Duchy of Athens	1	0	1454
Blanche II	Navarre	0	1	1464
Lady Jane Grey	England	1	0	1553
Bona Sforza d'Aragona	Milan	0	1	1557
Mary I	Scotland	1	0	1567

Table C.5. Regicide among Female Rulers

C.8.3. Smoothing Temperature Data

To convert the annual temperature records into centennial estimates in order to suit the periodicity of our data, we apply a Hodrick-Prescott filter with a lambda value of 500 000. This extracts the longer run trends from each series, removing any noise which is due to the relatively high frequency of the data. Though $\lambda = 500\,000$ is a much higher value than that recommended by Ravn and Uhlig (2002) for annual periodicity, we argue that a 1400 year series is exceptionally long and that it consequently displays characteristics of higher frequency data; requiring more smoothing than is usual for time series estimates. Additionally, the trends obtained using this parameter provide a balance between the noisy estimates of the annual data and what could be identified as over-smoothing. Finally, we take a simple average of this long run trend for each century.

C.8.4. Regicide Maps with Battle Deaths

In order to show that the discussion of regional trends in section C.2.4. is not biased by using our intermediate definition of regicide (unambiguous and dubious assassinations) as opposed to our broad definition (the intermediate definition plus battle deaths), we compare the intermediate- and broad regicide maps here.

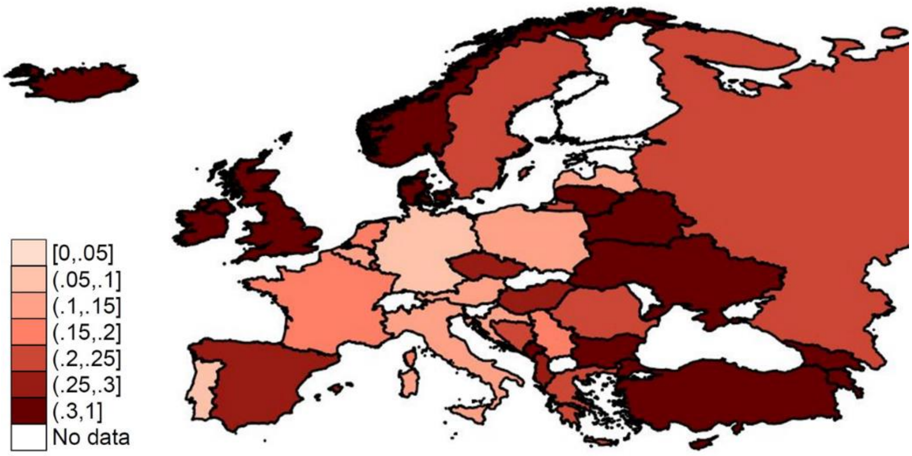
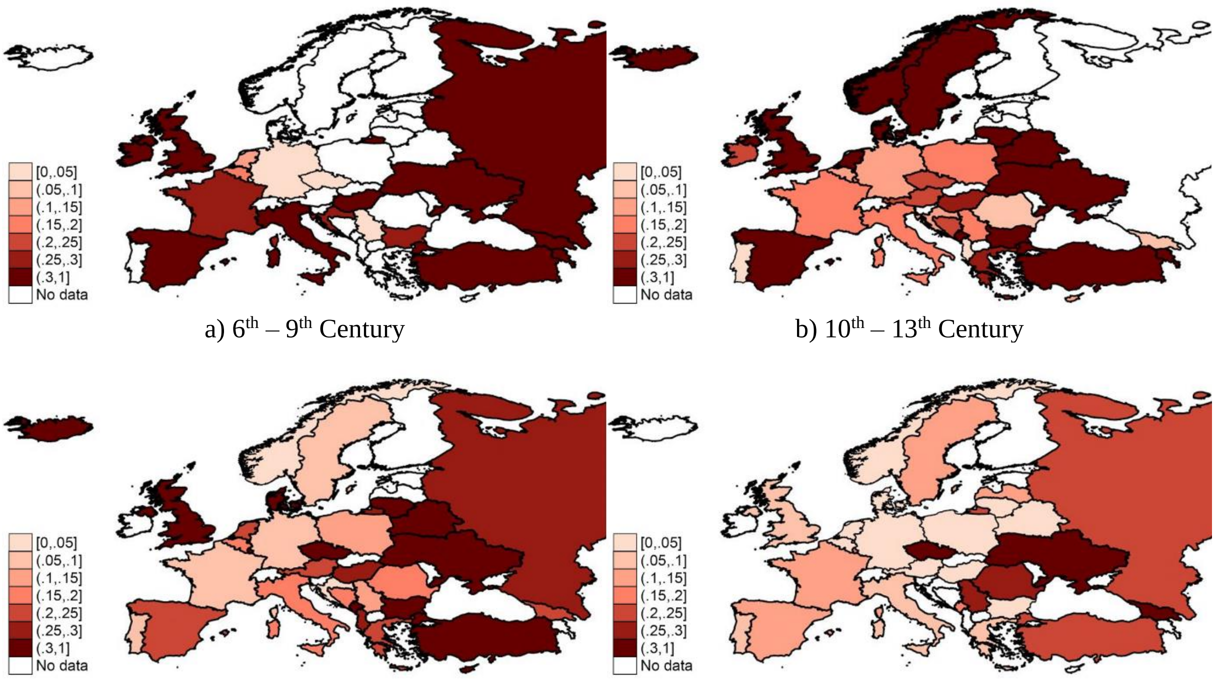


Figure C.15. European Regicide and Battle Deaths: 6th – 19th Century

The broadly defined map of the entire sample period is almost identical to the intermediate case. Aside from many countries increasing by one level of regicidal intensity, the only striking difference is that Scandinavia, the United Kingdom and Ireland become vastly more violent. Likewise, Austria, Germany and Poland seem somewhat more violent when battle deaths are included, but these countries are still among the least violent that we study.

In the period 500–900 (figure A.C.3., panel a), the differences are also only very slight. When battle deaths are included, the United Kingdom and Spain increase in regicidal intensity by one level, whereas Croatia and Turkey experience decreases of one level each.

Battle deaths in the High Middle Ages (panel b) are the root cause of the differences in the maps that cover the entire sample period, with northern Europe becoming far more violent. As mentioned, the Vikings and Norsemen had a disproportionately high ratio of battle deaths to intermediate regicide, resulting in much higher levels of broad regicide and distorting the northern countries in our map. Aside from the northern countries – and Austria, Germany and Poland – the increases in regicidal intensity that occur after including battle deaths appear quite uniform.



a) 6th – 9th Century

b) 10th – 13th Century

c) 13th – 15th Century

d) 15th – 19th Century

Figure C.16. European Regicide and Battle Deaths by Period

In the late Middle Ages (panel c), declines in Norwegian and Swedish battle deaths during the formation of the Kalmar Union (1397) largely reduced the disparities between the regicide maps under our two definitions. However, battle deaths in the United Kingdom and Iceland remain disproportionately high, as they do in Germany. These deviations probably affect our discussion of regicide through European history the most of our four periods.

During the early modern period (panel d), the inclusion of battle deaths seems to have increased regicidal intensity in the Czech Republic, Georgia and Serbia, while decreasing it in

Romania and particularly in Ukraine. Other than these geographically diverse examples, the maps under the two definitions are markedly similar.

C.8.5. Hausman Test

To motivate the random effects specification with the proximity-to-invasion variable in table C.3., we compare table C.1.'s results to an equivalent random effects specification (table A.C.4.) using Hausman tests. These tests conclude that the random effect assumption – the individual specific effects being uncorrelated to the independent variables – holds in all ten cases. Therefore, the results which include the new proximity indicator in table C.3.'s random effects specification should not be subject to omitted variable bias from omitted, time-invariant factors. Additionally, the remaining results from tables C.3. and A.C.4. are also nearly identical, suggesting that no other right-hand-side variables (other than Orthodox Christianity) are correlated to invasion proximity

		Hausman Tests		
Model	Degs. Freedom	χ^2	P-Value	Conclusion
1	10	3.45	0.9688	RE
2	10	3.95	0.9496	RE
3	11	4.50	0.9530	RE
34	13	4.62	0.9826	RE
5	14	4.41	0.9924	RE
6	15	6.95	0.9590	RE
7	16	6.48	0.9820	RE
8	23	28.11	0.2115	RE
9	9	6.52	0.6868	RE
10	23	13.88	0.9301	RE

Table C.6. Hausman Tests

	(1) Regicide	(2) Regicide	(3) Regicide	(4) Regicide	(5) Regicide	(6) Regicide	(7) Regicide	(8) Regicide	(9) Regicide	(10) Regicide
Territorial State Capacity	**-0.074*****	**-0.072*****	**-0.067*****	**-0.066****	**-0.065****	**-0.065****	**-0.065****	**-0.064****	**-0.066****	**-0.069****
	(0.024)	(0.023)	(0.026)	(0.028)	(0.029)	(0.031)	(0.031)	(0.030)	(0.019)	(0.031)
Temperature		-0.0565	-0.0571	-0.0429	-0.0558	-0.0645	-0.0651	-0.0511	-0.0744	-0.0601
		(0.129)	(0.130)	(0.135)	(0.135)	(0.133)	(0.134)	(0.130)	(0.136)	(0.132)
Urbanisation			-0.190	-0.212	-0.156	-0.140	-0.136	-0.223	-0.174	-0.235
			(0.162)	(0.202)	(0.202)	(0.193)	(0.192)	(0.250)	(0.196)	(0.257)
Mode of Succession (Base=Hereditary)										
● Partially Elected				**0.093***	0.084	0.080	0.081	**0.115***	0.090	0.107
				(0.055)	(0.058)	(0.061)	(0.065)	(0.064)	(0.067)	(0.066)
● Fully Elected				-0.0349	-0.0407	-0.0390	-0.0384	-0.0252	-0.0239	-0.0122
				(0.0337)	(0.0363)	(0.0371)	(0.0388)	(0.0471)	(0.0416)	(0.0513)
Fractionalisation					-0.0374	-0.0378	-0.0377	-0.0398	-0.0405	-0.0464
					(0.0407)	(0.0410)	(0.0415)	(0.0381)	(0.0429)	(0.0424)
Battle						0.126	0.138	0.0392	0.207	0.129
						(0.264)	(0.266)	(0.251)	(0.280)	(0.267)
Autonomy							-0.0135	-0.0200	-0.0192	-0.0232
							(0.0525)	(0.0543)	(0.0555)	(0.0571)
Religion (Base=Catholic)										
● Islamic								0.0401		0.0206
								(0.0683)		(0.0678)
● Orthodox								-0.0271		-0.0433
								(0.0697)		(0.0658)
● Protestant								-0.0358		-0.0320
								(0.0667)		(0.0686)
● Other								0.0630		0.0548
								(0.126)		(0.127)
Jewish Minority								-0.00323		-0.000188
								(0.0500)		(0.0523)
Religious Diversity								0.00958		0.0191
								(0.0395)		(0.0421)
Religious Transition								0.0112		0.00334
								(0.0570)		(0.0577)
Black Plague	0.0302	0.0323	0.0301	0.0302	0.0344	0.0323	0.0307	0.0505	0.0271	0.0373
	(0.0533)	(0.0535)	(0.0535)	(0.0548)	(0.0531)	(0.0531)	(0.0528)	(0.0582)	(0.0546)	(0.0593)
Justinian Plague	0.0430									
	(0.0912)									
Second Serfdom	0.0401	0.0412	0.0291	0.0695	0.0701	0.0652	0.0642	0.0481	0.0509	0.0433
	(0.0964)	(0.0958)	(0.0937)	(0.0867)	(0.0891)	(0.0897)	(0.0894)	(0.0693)	(0.0882)	(0.0688)
% Within 100 km. of ice-free coast									0.000342	0.000408
									(0.000602)	(0.000629)
% Fertile soil									0.00235	0.00215
									(0.00157)	(0.00185)
Ruggedness									0.00133	0.00102
									(0.00119)	(0.00107)
Constant	**0.325*****	**0.342*****	**0.342*****	**0.346*****	**0.359*****	**0.353*****	**0.365*****	**0.361*****	0.188	0.195
	(0.0693)	(0.0803)	(0.0804)	(0.0781)	(0.0734)	(0.0766)	(0.0957)	(0.0990)	(0.131)	(0.155)
Observations	109	106	106	106	106	106	106	106	106	106
Between R-squared	0.266	0.285	0.299	0.320	0.300	0.273	0.268	0.324	0.300	0.322
No. Principalities	34	34	34	34	34	34	34	34	34	34
Principality FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.7. Comparative Random Effects Regressions for the Hausman Test

C.8.6. Spatial Models

To control for spatial spillovers, we also use Spatial Autoregressive (SAR) and Spatially Lagged X (SLX) models, in tables A.C.5. and A.C.6., below. A full description of the methodology is provided in section B.10.3. This SAR model includes principality and time fixed effects and only differs from the fixed effects specification in section C.4. through the inclusion of the spatially lagged regicide variable, rho, to control for spillovers of interpersonal violence. The results found under the model are comparable to those from the fixed effects specification, although with a few key differences. First, the coefficients for expansion are slightly smaller than under fixed effects, though still significant. Second, the coefficients for urbanisation become significant with a negative sign, indicating that income and urbanisation are correlated to interpersonal violence in neighbouring regions, although the spatially lagged regicide variable remains insignificant. Ceremonial systems of succession then share a positive relationship with regicide, which is unexpected. Lastly, there is some evidence that coastal regions are more violent, which is contrary to the theory that coastal regions have better access to traded goods, although coastal regions may also suffer more from military spillovers.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Regicide	Regicide	Regicide	Regicide	Regicide	Regicide	Regicide	Regicide	Regicide	Regicide
	sar	sar	sar	sar	sar	sar	sar	sar	sar	sar
Territorial State Capacity	**-0.0535****	**-0.0555****	**-0.0516****	**-0.0515****	**-0.0507****	**-0.0512****	**-0.0512****	**-0.0514****	**-0.0584****	**-0.0594****
	(0.0249)	(0.0249)	(0.0244)	(0.0244)	(0.0239)	(0.0237)	(0.0239)	(0.0245)	(0.0233)	(0.0232)
Temperature		0.0663	-0.00858	-0.00678	-0.00785	-0.00658	-0.0107	-0.0194	-0.00558	-0.0139
		(0.0805)	(0.0800)	(0.0800)	(0.0794)	(0.0777)	(0.0809)	(0.0836)	(0.0797)	(0.0800)
Urbanisation			**-0.563*****	**-0.566*****	**-0.535*****	**-0.532****	**-0.540****	**-0.549****	**-0.467****	**-0.478****
			(0.202)	(0.202)	(0.206)	(0.212)	(0.211)	(0.213)	(0.198)	(0.206)
Mode of Succession										
(Base=Hereditary)										
● Partially Elected				**0.113****	**0.111***	**0.111***	**0.109***	**0.119***	**0.0868***	0.0893
				(0.0577)	(0.0578)	(0.0579)	(0.0578)	(0.0621)	(0.0508)	(0.0545)
● Fully Elected				-0.00728	-0.00875	-0.00914	-0.00968	-0.000468	0.00358	0.0126
				(0.0384)	(0.0388)	(0.0390)	(0.0389)	(0.0401)	(0.0380)	(0.0398)
Fractionalisation					-0.0181	-0.0174	-0.0167	-0.0198	-0.0373	-0.0436
					(0.0435)	(0.0437)	(0.0434)	(0.0434)	(0.0452)	(0.0454)
Battle						-0.00168	-0.00674	-0.00565	0.0990	0.119
						(0.245)	(0.246)	(0.244)	(0.257)	(0.258)
Autonomy							0.0125	0.00939	0.00649	0.00210
							(0.0566)	(0.0561)	(0.0546)	(0.0549)
Religion								-0.0166		
(Base=Catholic)										
● Islamic								0.0110		-0.00555
								(0.0620)		(0.0638)
● Orthodox								-0.0300		-0.0296
								(0.0610)		(0.0597)
● Protestant								-0.0677		-0.0520
								(0.0615)		(0.0625)
● Other								0.0287		0.0189
								(0.103)		(0.102)
Jewish Minority								0.0129		0.0178
								(0.0412)		(0.0420)
Religious Diversity								0.0115		0.0327
								(0.0433)		(0.0467)
Religious Transition								-0.0257		-0.0370
								(0.0469)		(0.0464)
% Within 100 km. of ice-free coast									**0.000972***	**0.00114****
									(0.000536)	(0.000552)
% Fertile soil									0.00206	0.00220
									(0.00147)	(0.00156)
Ruggedness									0.000752	0.000607
									(0.00107)	(0.00110)
Black Plague	0.0250	0.0264	0.0247	0.0248	0.0257	0.0264	0.0278	0.0227	0.0211	0.0161
	(0.0564)	(0.0556)	(0.0527)	(0.0530)	(0.0528)	(0.0522)	(0.0527)	(0.0526)	(0.0516)	(0.0518)
Justinian Plague	-0.0442	-0.0434	-0.0295	-0.0318	-0.0321	-0.0324	-0.0325	-0.0315	-0.0200	-0.0188
	(0.0814)	(0.0812)	(0.0771)	(0.0772)	(0.0770)	(0.0761)	(0.0764)	(0.0745)	(0.0759)	(0.0734)
Second Serfdom	0.00306	0.000706	-0.00859	-0.00389	-0.00438	-0.00423	-0.00371	-0.0130	-0.00416	-0.0144
	(0.0752)	(0.0736)	(0.0688)	(0.0686)	(0.0681)	(0.0680)	(0.0678)	(0.0666)	(0.0629)	(0.0632)
rho	0.00425	0.00510	0.00819	0.00632	0.00708	0.00663	0.00624	0.00793	0.0120	0.0126
	(0.0644)	(0.0645)	(0.0634)	(0.0640)	(0.0642)	(0.0639)	(0.0639)	(0.0650)	(0.0645)	(0.0660)
Sigma2_e	**0.0323*****	**0.0318*****	**0.0284*****	**0.0282*****	**0.0279*****	**0.0276*****	**0.0273*****	**0.0262*****	**0.0250*****	**0.0238*****
	(0.00584)	(0.00584)	(0.00511)	(0.00509)	(0.00486)	(0.00487)	(0.00480)	(0.00442)	(0.00431)	(0.00397)
Observations	476	476	476	476	476	476	476	476	476	476
Number of Principalities	34	34	34	34	34	34	34	34	34	34
Principality FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Cluster robust standard errors in parentheses (clustered by principalities)
*** p<0.01, ** p<0.05, * p<0.1

Table C.8. Spatial Autoregressive Model (SAR) with Time and Principality Fixed Effects.

	(1) Regicide slx	(2) Regicide slx	(3) Regicide slx	(4) Regicide slx	(5) Regicide slx	(6) Regicide slx	(7) Regicide slx	(8) Regicide slx	(9) Regicide slx
Territorial State Capacity	**-0.055*** **	**-0.056*** **	**-0.051*** **	**-0.051*** **	**-0.051*** **	**-0.052*** **	**-0.052*** **	**-0.052*** **	**-0.060*** **
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Temperature		**0.075** **	0.015	0.017	0.017	0.019	0.019	0.017	0.023
		(0.035)	(0.035)	(0.035)	(0.035)	(0.036)	(0.036)	(0.036)	(0.035)
Urbanisation			**-0.507*** **	**-0.509*** **	**-0.509*** **	**-0.524*** **	**-0.524*** **	**-0.520*** **	**-0.508*** **
			(0.186)	(0.188)	(0.194)	(0.188)	(0.193)	(0.186)	(0.184)
Mode of Succession (Base=Hereditary)									
● Partially Elected				**0.106***	**0.105***	**0.105***	**0.102***	0.085	0.071
				(0.076)	(0.076)	(0.076)	(0.076)	(0.080)	(0.078)
● Fully Elected				-0.010	-0.007	-0.007	-0.009	-0.010	0.009
				(0.027)	(0.033)	(0.033)	(0.034)	(0.039)	(0.038)
Fractionalisation					-0.005	-0.006	-0.007	-0.019	-0.032
					(0.033)	(0.033)	(0.033)	(0.042)	(0.041)
Battle						-0.057	-0.058	-0.054	0.035
						(0.103)	(0.103)	(0.103)	(0.104)
Autonomy							0.017	0.009	0.002
							(0.058)	(0.059)	(0.057)
Religion (Base=Catholic)									
● Islamic								-0.089	-0.096
								(0.085)	(0.082)
● Orthodox								-0.049	-0.049
								(0.059)	(0.057)
● Protestant								-0.061	-0.050
								(0.06)	(0.057)
● Other								-0.027	-0.022
								(0.097)	(0.093)
Jewish Minority								0.024	0.026
								(0.045)	(0.044)
Religious Diversity								0.019	0.032
								(0.044)	(0.042)
Religious Transition								-0.017	-0.027
								(0.043)	(0.041)
% Within 100 km. of ice-free coast									**0.001*** **
									(0.000)
% Fertile soil									**0.002*** **
									(0.001)
Ruggedness									**0.001***
									0.000
Black Plague	0.016	0.018	0.014	0.013	0.013	0.014	0.016	0.005	0.004
	(0.045)	(0.044)	(0.042)	(0.043)	(0.043)	(0.042)	(0.043)	(0.044)	(0.032)
Justinian Plague	-0.021	-0.022	-0.013	-0.014	-0.014	-0.014	-0.013	-0.012	0.007
	(0.059)	(0.058)	(0.056)	(0.056)	(0.056)	(0.055)	(0.055)	(0.055)	(0.049)
Second Serfdom	0.006	0.003	-0.005	0.000	-0.001	-0.001	0.000	-0.007	-0.013
	(0.053)	(0.052)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.046)
Theta	-0.001	-0.001	0.000	0.000	0.000	-0.001)	-0.001	-0.001	0.000
	(0.032)	(0.031)	(0.025)	(0.026)	(0.027)	(0.028)	(0.029)	(0.030)	(0.028)
Constant	**0.184*** **	**0.182*** **	**0.174*** **	**0.173*** **	**0.173*** **	**0.172*** **	**0.172*** **	**0.171*** **	**0.167*** **
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	476	476	476	476	476	476	476	476	476
Number of Principalities	34	34	34	34	34	34	34	34	34
Principality FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES

Cluster robust standard errors in parentheses (clustered by principalities)
*** p<0.01, ** p<0.05, * p<0.1

Table C.9. Spatially Lagged X Model (SLX) with Time and Principality Fixed Effects.

Instead of controlling for regicide spillovers, our SLX model controls for territorial state capacity in neighbouring regions (using theta). The idea is that increased territorial state capacity elsewhere may either cause interpersonal violence to move to our example principality, or decrease violence due to better a control of violence there. However, theta is never significant, and the results are nearly identical to those from the SAR model, the only difference being the small but significant and positive coefficients for fertile soil and ruggedness. Overall, despite the spatial terms never being significant themselves, changes in the coefficients of certain control variables provide some evidence that spatial correlations play a small role at a principality level.

C.8.7. Unit Root Tests

To ensure that our results are not reliant on common trends, we run panel unit root tests. We use the Phillips–Perron test since it is one of the few panel tests that is able to circumvent the duel problems of unbalanced panels and gaps in the time-series; which arise where principalities were dissolved and later resurfaced, e.g. Norway before and after the Kalmar Union. Table A.C.7. outlines the results, showing that only the urbanisation variable with zero lags follows a unit-root process. Therefore, our inclusion of time fixed effects and the first difference model should rule out any adverse effects of unit roots.

Lags	Regicide	Territorial State Capacity	Temperature	Urbanisation	Battle
	P-Value	P-Value	P-Value	P-Value	P-Value
0	0.0000	0.0000	0.0124	**0.2393**	0.0000
1	0.0000	0.0000	0.0000	0.0005	0.0000
2	0.0000	0.0000	0.0000	0.0000	0.0000
3	0.0000	0.0000	0.0000	0.0000	0.0000

Phillips–Perron Panel Unit Root Test

Table C.10. Panel Unit Root Tests

D. To Fly to Quality or Disinvest? The Dilemma of Political Violence and Investor Sentiment.

Abstract

In times of uncertainty and heightened risk, conventional wisdom dictates that investors amend their portfolios to increase their holdings in short term bonds and reduce their exposures to equity, aiming to earn real returns or restrict losses. Political uncertainty, however, provides an interesting conundrum since the safest short-term bonds are issued in the form of Treasury bills – by government – the source of the risk. In this paper, event study methodology is employed using assassinations in 56 countries since 1970. While investors do tend to disinvest from equity, no significant effect is detected for short-term bonds. However, exchange rate depreciations are also found, implying that investors tend either to favour cash holdings over equity or else move their investments abroad.

D.1. Introduction

Traditional financial literature recommends that investors substitute stocks for defensive assets such as bonds and Treasury bills when they perceive greater risk environments, following the Capital Asset Pricing Model (CAPM), as investors aspire to earn returns that are more predictable or to restrict losses (Sharpe 1964, Lintner 1965, and Mossin 1966). Asset managers largely adhere to this principle, despite empirical literature yielding mixed evidence on its practical validity (Fama and French 2004, Dempsey 2013).

When the source of risk is political, however, do investors still blindly follow convention and purchase Treasury bills, in search of the fabled 'risk-free rate'? If not, do they reallocate their holdings offshore or do they prefer to disinvest and wait for political uncertainty to subside? This problem is tackled through a series of event studies, using the sudden nature of assassinations to detect sharp escalations in political uncertainty and their impacts on financial markets in 56 countries since 1970.

Political uncertainty is a fundamental factor for investors to consider when constructing their portfolios. In this paper, since assassinations are used to capture the effects of political risk, investors are likely to respond to future policy uncertainty or to the threat of potential violence from the group or individual that carried out the assassination. Of course, this risk of violence could apply to personal safety, to any tangible investments or to the institutions that administer them. These fears may have particularly large impacts since the downside risk to an investment is potentially unlimited, depending on the extent of political or macroeconomic fallout from an assassination; as opposed to impacts from minor threats comparable to increased inflation forecasts, for example.

One problem that arises when studying the impact of political risk on investor behaviour is that it is difficult to measure because investors account for uncertainty, at least partially, in accordance with the efficient market hypothesis. Under the strong form of the hypothesis, prices reflect all available information and are updated immediately when new information becomes available, meaning that anticipated changes in political risk will already be ‘priced in’ and have no discernible impact on investor behaviour (Fama 1971). For example, an election outcome should have no effect if the polls had already accurately forecasted the result and there had been no reason to believe that they would be wrong. In reality, different markets seem to experience varying degrees of efficiency, but a weak or semi-strong form of the hypothesis seems to be most prevalent in political contexts, meaning that most publicly available information is priced in (Jacque 1981).

As assassinations are unexpected events that are largely exogenous by nature, since governments expecting threats are able to implement vast security measures to avoid them, this paper uses assassinations to assess the rationality of investor reactions to political risk. Additionally, how investors allocate their holdings influences the levels of financing that are available to firms or national treasuries, affecting the ability of firms to grow and develop certain industries or the ability of governments to provide public services and direct fiscal policy.

The extent to which governments protect their officials is the subject of countless books and films, which, although often exaggerated and based on presidents or other higher-ranking figures, highlight the protective resources that can be executed in response to perceived threats. This is not only the case in wealthy countries that can afford large security units but rather a universal trait, since governments of poorer countries have tended to prioritise security to a greater degree, often using their militaries or police forces for personal security (see Ball et al. 2003: 264, on personal and political security in post-colonial Sub-Saharan Africa). Therefore, the advantage of using assassinations to detect sharp escalations in political risk is that they are unexpected and can help to isolate the impact of political risk on investor behaviour.

D.2. Literature review

Literature on political risk and financial markets has tended to focus on events that signal political risk and analysed how they have affected exchange rate movements and equity markets in terms of both return and volatility. Indeed, Kim and Mei (2001) suggest that political events are the most important, or at least the most common factors that influence stocks in Hong Kong. They employ a GARCH 'components jump volatility filter' to identify dates where the Hang Seng Index undergoes sharp adjustments before relating these 'jumps' to prevailing events, observing a close relationship between these 'jump dates' and political news. Additionally, Voth (2002) finds that between half and two-thirds of stock market volatility during the Great Depression in Western Europe was due to political uncertainty, emphasising that the Russian Revolution occurred just over a decade beforehand and that investors were particularly sensitive to the potential consequences of political risk, following the impacts of communism for private ownership.

Although small, a body of literature that specifically studies the relationship between political risk and financial markets through unanticipated acts of violence does exist. For example, Zussman and Zussman (2006) indirectly assess the effectiveness of Israeli counterterrorism policy through an event study by considering stock market responses to the targeted assassinations of Palestinian terrorist leaders since 2000. They observe no effect until separating the targets into military and political leaders, finding that the assassinations of political leaders resulted in a 1 percent loss for investors in equity, on average, while assassinating military leaders led to gains of between 0.5 and 1 percent in the immediate aftermath. Zussman and Zussman (2006) therefore conclude that political assassinations are perceived as counterproductive for counterterrorism by investors, but military assassinations do seem to be effective. This distinction is probably because military assassinations impede military capabilities while political assassinations may provoke retaliation.

More recently, Incerti and Incerti (2019) examined the stock market impacts of ‘irregular’ regime changes – namely coups, assassinations and resignations – in a global event-study setting since 1901. Using an event study approach, they detect positive impacts from resignations of about 4%, which persist for approximately two months, on average; but negative returns after coups and assassinations of about 2%, lasting approximately six months.

The cases of Pakistan and the Arab Spring have also received some attention in the contexts of stock markets. Mahmood et al. (2014) and Nazir et al. (2014) both used event studies and found that a variety of political events and acts of violence from 1998 to 2013 and 1999 to 2011, respectively, had negative impacts on the Karachi Stock Exchange and resulted in increased volatility. The magnitude of these impacts also depended on the types of events that occurred. Likewise, Chau et al. (2014) and Abdelbaki (2013) found negative return and increased volatility effects on Middle Eastern and North African stock markets following uprisings and protests during the Arab spring. They used GARCH methods and VECM models with impulse response functions, respectively. However, Chau et al. (2014) only found large impacts for Islamic stock indices (“Shariah-compliant Islamic financial assets”) and little or no effect for conventional stock market indices, emphasising the roles of investor expectations and ideology.

Departing from the literature on the stock market impacts of political risk, Fatehi (1994), recognising the challenges in quantifying political risk, proposed that capital flight could be used as a simple but reliable proxy. In his paper, he uses regression analysis with lags to detect the effects of various sources of political instability, including political assassinations, on capital flight from 17 Latin American countries to the US between 1954 and 1982. Although these results were rather heterogeneous, many indicators – including political assassinations – had robust impacts with response times in excess of a year. Similarly, as part of a study to determine whether exchange rate uncertainty and socio-political instability, both individually and jointly, affect private investment in eight Latin American countries from 1975 to 1999, Escaleras and Thomakos (2008) also find that socio-political factors have lasting impacts on exchange rate uncertainty, persisting for a year or longer. However, the effect of assassinations is insignificant once riots and violent demonstrations are controlled for. More directly, Bouraoui and Hammami (2017) examine exchange rate volatility using a political instability index derived from events such as terrorist attacks, labour strikes, violent protests and political assassinations in five Middle Eastern countries during the Arab Spring. They use autoregressive distributed lag (ARDL) and vector auto-regressive (VAR) models with impulse response functions, finding elasticities of between 0.13 and 0.36, depending on the country. These impulse response functions indicated that the effects last between 12 and 18 months.

There has not been much research on the impacts of political risk on bond markets, but Oosterlinck (2003) finds that only political factors caused structural breaks in French bond yield series during World War II. Additionally, Ferguson (2006) found that European bond yields were sensitive to political events between 1843 and 1880, but this was no longer the case between 1881 and 1914.

Overall, literature on the effect of political risk on financial markets seems to show that investors respond by disinvesting in equity and then either by moving their holdings to bonds or else more stable destinations offshore.

D.3. Data

The assassination data for this paper come from the Global Terrorism Database. The database provides detailed information of over 190,000 violent events, including 20,000 assassinations from around the world since 1970. The Oxford Dictionary (2019) defines an assassination as the “murder [of] (an important person) for political or religious reasons”, necessitating that this paper use a subset of the database in order to adhere to the problem of political risk. As such, only the assassinations of members of 'general government' and 'diplomatic government' are included, while categories such as religious, business, police, journalistic or military assassinations are excluded. After matching the assassinations with the financial data, 1,767 cases across 107 countries remain.[39]

The financial data come from the Global Financial Database. The database affords centuries of financial and macroeconomic data at various geographical and administrative levels and at various periodicities. For example, United Kingdom's FTSE All-Share Return Index extends back to 1694 on a monthly basis, with daily data available since 1964. However, since most series are not available at daily periodicities globally and since 1970, monthly data is used. Additionally, all financial series under study are inflation adjusted, in order to examine the real impacts of political risk.

The first of the financial series utilised to investigate the impact of political risk is the total return on equity holdings from the largest stock exchange within each country. This is defined as the monthly equity return after all dividends are assumed to be reinvested, as calculated in equation 1. Since the research question here is whether investors move their holdings from equity to short term bonds in uncertain times, despite the political nature of the risk, this is a natural point of departure. Regardless of how investors choose to reallocate their holdings in response to political risk, a negative impact on equity return is expected.

$$Total\ Stock\ Return_{it} = \frac{\left(P_{it} - P_{i,t-1}\right) + D_{it}}{P_{i,t-1}} \quad (1)$$

Where *P* refers to stock price, *D* to dividends, *i* to each stock exchange and *t* to each month. After attempting to detect a departure from equity holdings, I examine the impact of assassinations on proxies of the risk-free rate. The risk-free rate is best approximated by short-

[39] Note, the final sample sizes are greatly reduced, since only assassinations with valid ('exogenous') estimation windows are analysed.

term bond yields; and by yields on Treasury bills in particular (Dimson et al. 2002). Therefore, the preferred risk-free rate proxy for this paper is the 3-month Treasury bill yield, since this is the shortest-term Treasury bill yield within the financial database. When unavailable, the risk-free rate series were supplemented by alternative short- or long term government bonds (these have also been widely used as risk-free rate proxies, e.g. Hodges et al. 1997; Mukherji 2011), or else central bank discount rates or interbank rates (used by Dimson et al. 2002) when they are closely related to Treasury bill yields in overlapping periods (e.g. figure A.D.1.). A negative impact of assassinations on bond yields implies an increase in demand for short-term government bonds, which would provide evidence of investors ignoring the political nature of risk in seeking the risk-free rate. Conversely, a positive impact on bond yields is associated with a decline in demand for short-term government bonds.

The next series under study is the equity risk premium, the theoretical 'reward' for incurring risk. This is a particularly interesting metric since it reflects the willingness of investors to maintain their level of exposure to risk, at least with regard to equities. The equity risk premium is also the best predictor of the prospective risk-premium, an important metric for estimating future returns, the cost of equity capital and company valuations (Dimson et al. 2002). This means that a negative impact on the equity risk premium following an assassination could signal a long run distaste for risk. Conversely, no significant impact on risk premium could indicate either that investors see the period of heightened risk as temporary, or that they are convinced of the persistence of the equity premium puzzle – that equity outperforms even its risk adjusted return – possibly as a result of habit persistence (Mehra and Prescott 1985; Benartzi and Thaler 1995; Constantinides 1990).

The geometric risk premium is calculated here (equation 2), in accordance with Dimson et al. (2002). This has become standard in financial literature as using the arithmetic risk premium implicitly requires the assumption that the observations in the series are independent of one another (Lütolf-Carroll & Pirnes 2009). Since Fama and French (1986), Lo and MacKinlay (1988), and Porterba and Summers (1988) detect negative autocorrelation in stock returns – due to short term overcorrections – this is not necessarily the case and the geometric method should be used.

$$Equity\ Risk\ Premium_{it} = \left(\frac{1 + r_{equity,it}}{1 + r_{risk-free\ rate,it}}\right) - 1 \quad (2)$$

Then, as an alternative to the return on equity holdings, the impact of assassinations on the growth rate of market capitalisation is examined. In this way, a more precise impression of how investors change the proportions of equity in their portfolios can be obtained. Additionally, since the total equity return indicator from before includes dividend pay-outs and assumes that all dividends are reinvested, market capitalisation acts as an ideal robustness measure, as only dividends that are actually reinvested are appraised.

Lastly, the impact of assassinations on the real US dollar exchange rate for each country is investigated. The US dollar is used for comparison as it is the most traded currency globally and the most widely held reserve currency, while the United States is the world's largest economy and the home of the world's two largest stock exchanges by market capitalisation.[40] Real exchange rate responses to assassinations would provide a first clue as to whether investors have incentives to move their holdings abroad or else make local investments even more attractive than before. A full answer to this question would require a complete analysis of spillover effects, but simple changes in exchange rates guide intuition. A real depreciation relative to the dollar would indicate that international investors earn reduced returns, *ceteris paribus*, and make flights to safety more likely. Conversely, a real exchange rate appreciation increases returns for international investors, providing incentives to increase their holdings in local assets, ignoring the impact of political risk on those assets.

[40] The New York Stock Exchange (NYSE) and the NASDAQ

D.4. Methodology

Event studies are now common in academic research, particularly within financial literature at the firm or even stock level. James Dolley (1933) is widely credited for conducting the first event study in an academic setting, in his paper which considered the impact of stock splits on equity prices, largely finding a common upward effect. However, the aim of Dolley's paper was to outline the corporate procedures involved in stock splits rather than develop an event study methodology, meaning that the methodology he used was very straightforward, simply comparing the changes in price per share with the ratio of each stock split before and after the event.

Event study methodology then evolved somewhat. Ball and Brown (1968) and Fama et al. (1969) effectively provided the procedure that is the academic standard today, in their respective studies of how stock prices are affected by the release of accounting reports and by stock splits that have been adjusted for increased dividend pay-out ratios. Since then, most refinements have been introduced to deal with study-specific challenges; such as including controls for common market trends when studying firm-level impacts (Ashley 1962), as well as the development of robustness tests such non-parametric techniques (Brown and Warner, 1980). Additionally, Brown and Warner (1980, 1985) studied the differences in event study characteristics under monthly and daily periodicities while Fama and French (1993, 2015) introduced their three and then five-factor asset pricing models, adjusting for certain risk factors and thereby developing more sophisticated baselines which can be used as fairer comparisons for post-event outcomes.

The basic setup is as follows: first, an estimation window is established, from which the processes that we are interested in can be observed under ordinary conditions. The period immediately preceding the event is used, since it provides the most recent information without being biased by the event itself. This paper uses the 24 months prior to, and not including, the event as the estimation window. Although Brown and Warner (1980) use a 38-month window, in this paper, excluding events with 'endogenous estimation windows' reduces sample sizes significantly. Endogenous estimation windows are defined as estimation windows that contain at least part of the event window for a previous event, thereby invalidating the assumption that they represent returns under normal circumstances, which can be used as baselines from which to estimate abnormal returns during the subsequent event window. Therefore, the 24-month estimation window, here, is a compromise between sample size and the stability of the pre-event benchmarks. However, a sensitivity analysis described in the appendix shows that estimation windows of up to 48 months yield highly consistent results.

Next, the event window is determined, the period beginning with the event and either lasting the duration that the effect of the event is expected to continue to have an impact, or else a specific duration dictated by the study. The response functions from figure D.3., discussed below, guided decision-making here, indicating that an event window of 12 months is appropriate for this study. Again, a sensitivity analysis is conducted and comparable results are found for event windows as small as 3 months, although a more detailed analysis is left to a later discussion.

The setup can be summarised by figure D.1., below, where t_1 is the event itself, t_0 to t_1 is the estimation window, t_1 to t_2 is the event window, and the periods before t_0 and after t_2 are not considered.

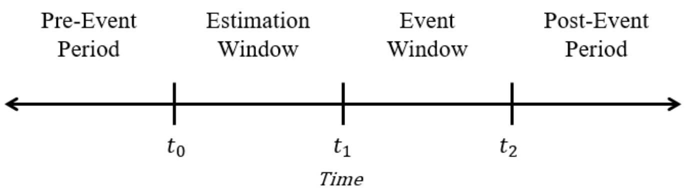


Figure D.1. Timing of an Event

Once the estimation and event windows have been set, baseline returns are calculated over the estimation window and used as counterfactuals with which to calculate ‘abnormal’ returns in the event window. Many methods of estimating ‘normal’ returns exist, although the ‘constant mean return model’ from equation 3 is used here.

$$R_{it} = \mu_i + u_{it} \tag{3}$$

Where R_{it} are the actual returns in the event window, μ_i refers to the mean return over the estimation window and u_{it} is an error term. Despite its simplicity, the constant mean return model typically provides similar outcomes to those from more sophisticated models (Brown and Warner 1980, 1985). This is potentially because future returns are inherently difficult to predict, in accordance with the efficient market hypothesis.

As a robustness test, a ‘market return model’ is also used; the main results of which are reported in tables D.4. to D.6. This model controls for common market trends.

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it} \tag{4}$$

Where R_{mt} is the market return and α_i is the constant mean return after controlling for the market return.

Since this study is conducted at an international level, the market return is defined as a global median return. The median is used here rather than the mean, because a global market is potentially susceptible to many unknowns, as well as outliers and measurement error, which could produce improbable results if economic or financial crises took place elsewhere, for example. However, there is a precedent for using world markets, set by authors such as Solnik (1974), Harvey (1991), Grauer et al. (1976) and Du and Hu (2015). Nevertheless, in order to make it a more suitable baseline, the market return is separated into three groups based on income categories (United Nations 2014). Although increasingly sophisticated baselines could be calculated using factor models (Fama and French 1993, 2015), the benefits of doing so are limited at best, and imprecisely estimated factors could even bias the results (MacKinlay 1997). Therefore, only the constant mean return and market models are used in this study.

After estimating these 'normal' return series for the event window, abnormal returns are simply calculated by subtracting the expected normal return from the actual, realised return.

$$\widehat{AR}_{i\tau} = R_{i\tau} - E(R_{i\tau}) \tag{5}$$

Where $\widehat{AR_{it}}$ are the estimated abnormal returns, R_{it} are the actual returns and $E(R_{it})$ are the expected 'normal returns'. Note that τ differs from *t*; *t* refers to the date whereas τ refers to the number of periods (months) from the event.

Next, the abnormal returns are summed together to form cumulative abnormal returns, the total effect of the event on the returns over the event window.

$$\widehat{CAR}_{iT} = \sum_{t=t_1}^{t_2} \widehat{AR}_{i\tau} \tag{6}$$

Where *T* refers to the duration of the event window.

Finally, the impact of the events is tested for significance. Each individual event can be tested by using a t-test.

$$t = \frac{\sum \widehat{AR}_{\tau}}{N} \bigg/ \frac{\sigma_{\widehat{AR}_{\tau}}}{\sqrt{N}} \tag{7}$$

Where N is the number of months in the event window and $\sigma_{\widehat{AR}_{\tau}}$ is the standard deviation of the abnormal return series.

Alternatively, to test for the significance of all events simultaneously and, therefore, to estimate the impact of assassinations on each of the series of interest, the cumulative abnormal returns are simply regressed on a constant.

$$\widehat{CAR}_{iT} = \varphi_{iT} + v_{iT} \tag{8}$$

Where φ_{iT} is a constant term and v_{iT} is a stochastic error term.

As a gauge of robustness, regressions following equation 8 are run with robust standard errors, bootstrapped standard errors (with 50 replications) and quantile regression.

D.5. Results and Discussion

D.5.1. Constant Mean Return and Market Models

The results are derived from a sample of 198 assassinations across 99 countries, although data availability for each event analysis differs by financial indicator. Figure D.2. shows that the following results describe the financial responses to assassinations worldwide, across both developed and developing countries. Additionally, of countries that could be matched to the financial data with exogenous estimation windows, Kenya, Israel, Lebanon, Pakistan, Mexico and South Africa experienced the most political assassinations between 1970 and 2017[41].

From table D.1., which uses the constant mean return model, we already obtain a clear outline of this paper's overall findings. Investors who hold instruments that track the local stock market lose, on average, 0.88% of their holdings over the 12 months after an assassination. This is corroborated by the change in stock market capitalisation, which loses 0.95% over the same investment horizon. Together, these two results provide clear initial evidence that investors do tend to adapt their portfolios and reallocate their holdings away from equity, in line with traditional financial theory, although the coefficients are perhaps smaller than expected; approximately half of those obtained by Incerti and Incerti (2019). Figure D.3. illustrates that both equity return and market capitalisation require 14 to 16 months to return to equilibrium,

[41] For a description of assassinations across countries, independent of financial data and/or exogeneity, see figures AD2 and AD3.

on average, although market capitalisation seems to make a near complete recovery after six months before experiencing a second negative shock. This is explained in section D.5.2.

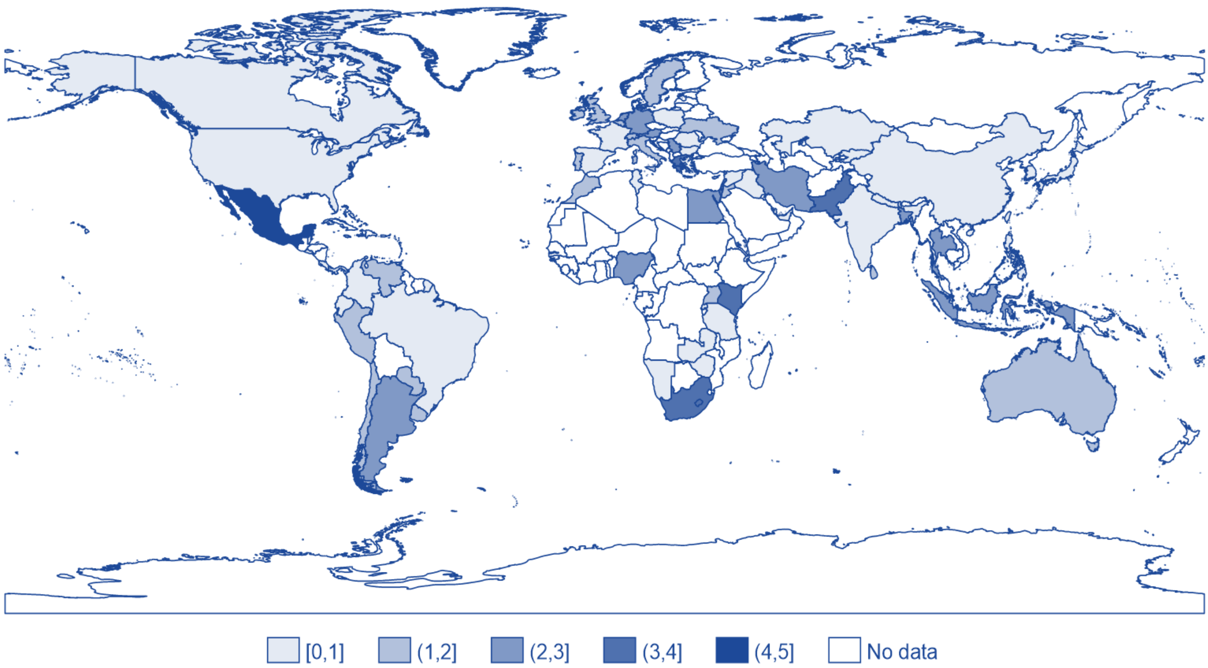


Figure D.2. Matched Assassinations with Exogenous Estimation Windows

That investors withdraw from equity holdings was rather predictable, since they were always bound to act in response to heightened political risk; the more interesting question is, to where do they, on average, reallocate their funds? Table D.1. shows no significant impact on either the risk-free rate or the equity risk premium, although the signs of their coefficients are positive and negative, respectively. The small positive coefficient from the risk-free rate would have signalled an increased yield on Treasury bills, had it been significant, which would have been associated with a decline in the demand for government bonds. In addition to indicating that the return on equity holdings is negative, the insignificance of the negative coefficient for the risk premium indicates that, in these risky periods, the return on equity effect is made unclear by the risk-free rate effect. Consequently, evidence from government bond yields is inconclusive as to the destination of investor holdings, following periods of heightened political risk.

Nonetheless, the coefficient from the real US dollar exchange rate may provide the solution to this destination problem. The exchange rate variable is the real US dollar exchange rate, meaning that, on average, countries experience a real exchange rate depreciation of 6.61%, relative to the US dollar. This depreciation provides evidence that investors withdraw from the country entirely, perhaps signalling flight to quality, rather than reallocating their holdings from stocks to government bonds. This exchange rate depreciation seems to require a full two years to return to pre-assassination levels, on average, emphasising its severity (figure D.3.). Although two years is a very long time when considering exchange rate movements, Escaleras and Thomakos (2008), Bouraoui and Hammami (2017) and Fatehi (1994) all find response times between 12 and 24 months in their studies of the impacts that assassinations and other indicators of political risk have on exchange rates.

Continuing to the results from the regressions that used bootstrapped standard errors in table D.2., the above results are reinforced. However, the results from the quantile regression model, though largely similar, present an insignificant coefficient for the exchange rate and cast doubt on the flight to quality conclusion from before.

Turning to the market return model, the previous results are largely upheld in the robust standard error and bootstrapped standard error specifications (tables D.4. and D.5.)[42]. The only differences from the conclusions of the constant mean model are that the negative impacts of political assassinations on both equity return and stock market capitalisation are far smaller – though still significant – and that the 12-month exchange rate depreciation is about twice as large as before. Here, the return on equity holdings is -0.38% and stock market capitalisation

[42] Note: In order not to compare vastly difference countries, market baselines were calculated using the income groups from table A.D.25.

loses -0.32% of its value, while the exchange rate undergoes a depreciation of 11.55% on average. The response functions from the market model indicate similar durations for the financial indicators to return to pre-assassination levels. However, there appear to be certain outliers in the risk-free rate series, which cause strange fluctuations in the response function for the risk-premium, casting doubt on the stability of the market model results for the risk-free rate and risk premium.

The quantile regression specification in table D.6., though supportive of all previous evidence on return to equity and stock market capitalisation, now produces a 5.68 percentage point exchange rate depreciation – approximately the same as under the constant mean return model – as well as a significant coefficient for the risk-free rate of 1.58%. Though only significant at a 90% level of confidence, it would be tempting to infer that these results provide some evidence of increased bond yields and thus decreased demand for government bonds, but the response functions from figures D.3. and D.4. again undermine this conclusion.

Overall, this analysis provides strong evidence of withdrawal from equity as well as tentative evidence of reallocation overseas, while bond holdings remain unaffected.

D.5.2. Results by Income Group

After assessing the impact of assassinations and political risk on investor reallocations from equity, a natural question to ask is whether the results are consistent across cases. If exchange rate depreciations take place and signal flights to quality, how do economically and financially developed – ‘quality’ – investment destinations differ from those that generally offer higher returns at the cost of greater risk? To investigate, the previous results are split by income groups (United Nations 2014) and estimated by using the constant mean return model. In order to detect varying response times between income groups; these results are also reported using 12, 6 and 3-month event windows.

Over a 12-month horizon, the effect of assassinations on equity returns is clearly driven by lower middle- and low-income countries (table D.7.) with an average return of -1.35%, while high-income countries undergo no significant impact. This is reinforced by a -2.32% decline in market capitalisation for lower- and middle-income countries (table D.9.), while no impact is experienced by upper middle- and high-income countries. There is no significant impact on risk premia or the risk-free rate for any income group. Additionally, lower middle- and low-income countries experience a 14.42% exchange rate depreciation, while upper middle-income countries saw depreciations of 10.75%, on average.

Additionally, the response functions from figure D.5. reveal quicker recoveries for higher-income countries, especially for the exchange rate and market capitalisation indicators. The exchange rate reverts to the pre-assassination level after two years for lower middle and low-income countries, on average, while it only requires about 10 months for high-income countries. Additionally, the two-phase response function for market capitalisation is explained here: lower and low-income countries experience the strongest impact of assassinations after about a year and return to pre-assassination levels after 20 months. Conversely, upper middle-income countries experience the strongest impact after 4 months and already undergo a partial recovery after 8 months, recovering fully after 16 months. This disparity in response times creates the two-phase pattern. Additionally, the recovery period for equity return remains at about 12 to 16 months, although the magnitude of the impact is much smaller for higher-income countries, as discussed above.

Over a 6-month event horizon, at -0.78%, the impact on equity return is smaller for lower middle- and low-income countries (table D.12.). Upper middle-income countries experience an impact of -0.9% whereas, for high-income countries, it is -0.45%. Some evidence of decreased market capitalisation and an exchange rate depreciation for lower- and low-income countries persists, but no other indicators show any level of significance after 6 months.

Finally, over a 3-month event window, no significant impact on equity return is found for lower- and low-income countries, but upper middle-income countries yield a significant coefficient of -0.69% while high-income countries experience a return of -0.37%. No other significant results are obtained over a three-month window.

In sum, this analysis of income categories is largely consistent with the previous results, as lower-income countries seem to experience greater impacts of assassinations, especially when considering equity return and exchange rates. However, these effects are more persistent for lower-income countries than for economies that are more developed. Since high-income countries are often associated with better institutions, investors could see assassinations there as isolated events, with future policy largely being set by the same organising bodies and remaining consistent in future. Conversely, assassinations in lower-income countries could be perceived as greater risks, since the successors of the assassinated individuals may wish to change policy, especially if institutions are weak.

D.6. Conclusion

This study investigates the responses of investors to political risk. In uncertain times when investors perceive a higher risk environment, traditional financial theory suggests that they should withdraw their holdings in equity and instead favour short-term bonds, seeking the risk-free rate. However, political uncertainty provides an interesting dilemma, since the safest short term bonds are issued in the form of Treasury bills – by government – which is the source of the risk.

The paper uses a series of event studies in an effort to resolve this dilemma and assess the rationality of investor decisions, using the assassinations of political leaders as examples of largely exogenous political shocks. The key findings of this study indicate that investors do indeed withdraw their holdings in equity, on average depleting stock market capitalisation just over 1% and, resulting in losses of nearly 1% for those who remain invested in equity. But to where they reallocate their holdings remains unclear. Since no significant impact on bond yields for risk-free rate proxies are found, no conclusions about any change in the demand for government bonds can be made. However, there is consistent evidence of exchange rate depreciations of about 6%, on average, following an assassination; thereby suggesting that investors may react by withdrawing their holdings from the affected country entirely.

Finally, to test the consistency of these results among developed and developing countries, the effects of assassinations are investigated by income classification, revealing that the impacts of political assassinations are larger and more persistent for developing countries. The reasons for these disparities could have their origins in investor perceptions of local institutional quality and the possible implications of political assassinations for the setting of future policy. If institutions are weak and future policy uncertain, then investors may prefer to reallocate their holdings to traditionally safer markets. Therefore, setting clear and long-term policy agendas may help to alleviate the volatility of future capital flows in developing countries.

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D.8. Figures and Tables

D.8.1. Event Study Regressions

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	**-0.880*** **	-1.297	**-0.945** **	0.377	**6.607***
	(0.307)	(1.226)	(0.412)	(1.130)	(3.502)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.1. OLS with Robust Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	**-0.880*** **	-1.297	**-0.945*** **	0.377	**6.607** **
	(0.322)	(1.210)	(0.327)	(1.308)	(3.351)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

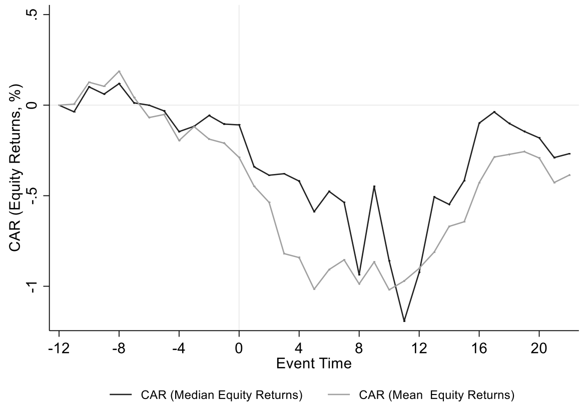
Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.2. OLS with Bootstrapped Standard Errors

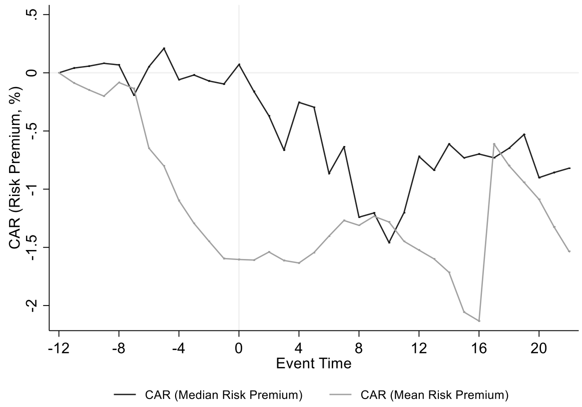
	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	**-0.888** **	-1.083	**-0.685** **	-0.248	2.008
	(0.385)	(0.797)	(0.322)	(0.417)	(2.073)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.3. Quantile Regression

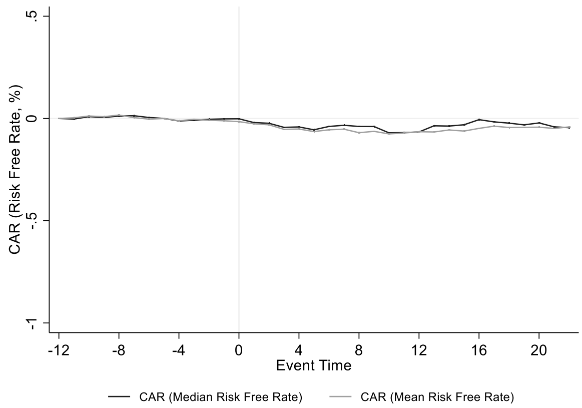


Equity Return

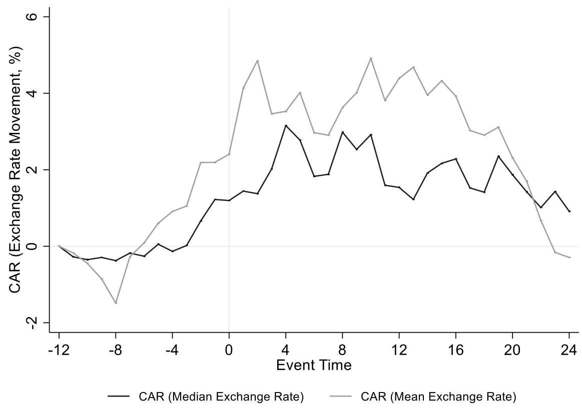


Risk Premium

Market Capitalisation



Risk-Free Rate



Real Exchange Rates

Figure D.3. Response Functions to Assassinations (Constant Mean Return Model)

D.8.2. Market Baseline[43]

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	**-0.384*** **	0.717	**-0.315*** **	-0.715	**11.55*** **
	(0.118)	(1.606)	(0.117)	(1.789)	(3.373)
Observations	109	89	79	98	85
Countries	56	45	40	48	43

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.4. OLS with Robust Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	**-0.384*** **	0.717	**-0.315*** **	-0.715	**11.55*** **
	(0.127)	(1.510)	(0.122)	(1.741)	(3.691)
Observations	109	89	79	98	85
Countries	56	45	40	48	43

Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

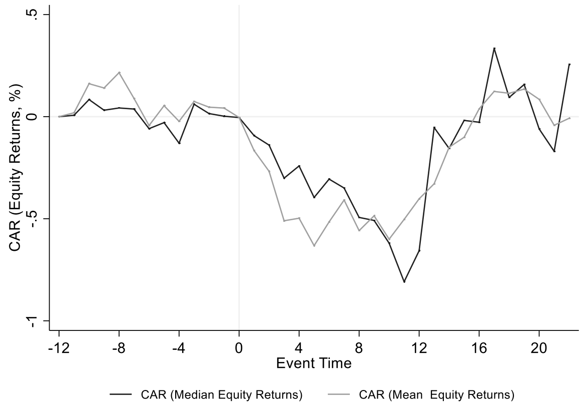
Table D.5. OLS with Bootstrapped Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	**-0.451*** **	-1.323	**-0.368** **	**1.583* **	**5.682** **
	(0.133)	(0.863)	(0.150)	(0.912)	(2.438)
Observations	109	89	79	98	85
Countries	56	45	40	48	43

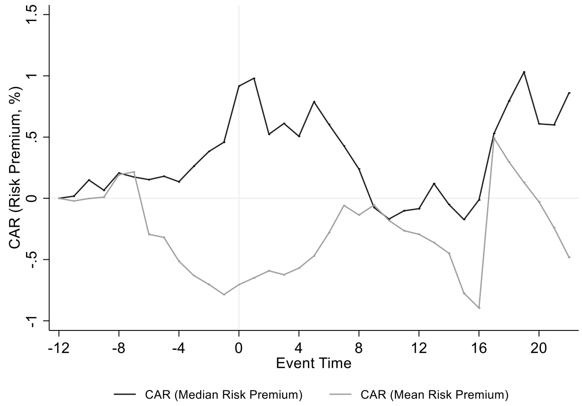
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.6. Quantile Regression

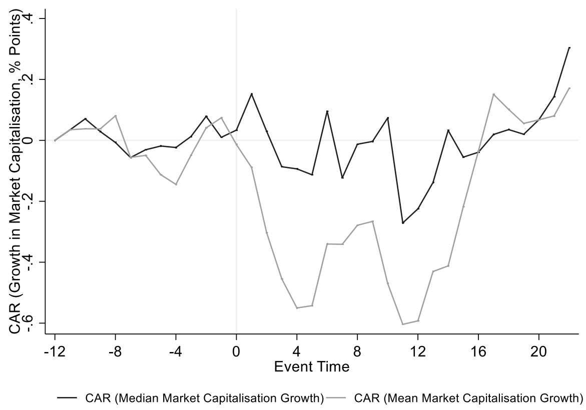
[43] Income groups were used to calculate market baselines (United Nations 2014).



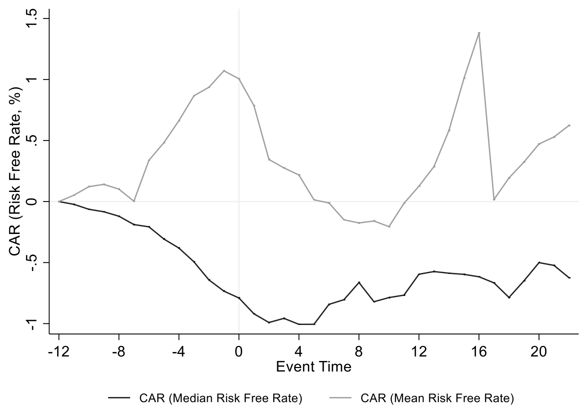
Equity Return



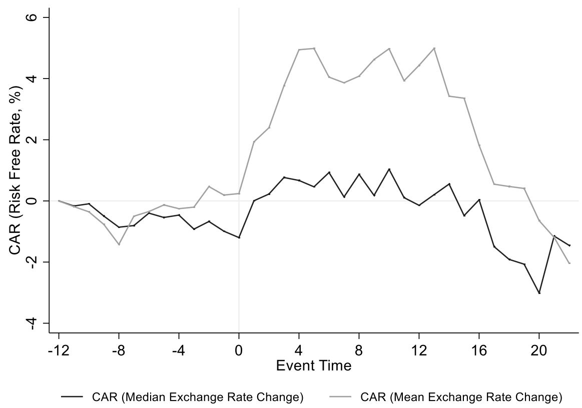
Risk Premium



Market Capitalisation



Risk-Free Rate



Real Exchange Rates

Figure D.4. Response Functions to Assassinations (Market Model)

D.8.3. Event Analysis by Income Category (12 Month Event Window)

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	**-1.208*** **	**-1.349** **	-1.100	-0.469
	(0.450)	(0.533)	(0.688)	(0.398)
Observations	60	26	34	48
Countries	32	16	16	24

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.7. Equity Return

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	-2.305	0.357	-4.251	-0.242
	(2.274)	(2.160)	(3.597)	(0.805)
Observations	45	19	26	43
Countries	21	10	11	22

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.8. Risk Premium

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	**-1.480** **	**-2.322** **	-0.941	-0.352
	(0.653)	(1.078)	(0.817)	(0.472)
Observations	41	16	25	37
Countries	19	7	12	20

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.9. Market Capitalisation

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	0.604	-1.454	2.181	0.104
	(1.983)	(1.995)	(3.153)	(0.747)
Observations	53	23	30	44
Countries	25	11	14	23

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.10. Risk-Free Rate

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	**12.137****	**14.424****	**10.749****	-0.119
	(6.017)	(6.766)	(8.843)	(0.747)
Observations	45	17	28	37
Countries	21	7	14	20

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.11. Exchange Rate

D.8.4. Event Analysis by Income Category (6 Month Event Window)

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	**-0.846*****	**-0.775***	**-0.900****	**-0.449***
	(0.299)	(0.420)	(0.425)	(0.263)
Observations	60	26	34	48
Countries	32	16	16	24

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.12. Equity Return

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	-0.275	0.396	-0.764	-0.414
	(1.341)	(1.181)	(2.173)	(0.520)
Observations	45	19	26	43
Countries	21	10	11	22

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.13. Risk Premium

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	**-0.996***	-1.596	-0.612	-0.246
	(0.520)	(0.925)	(0.618)	(0.292)
Observations	41	16	25	37
Countries	19	7	12	20

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.14. Market Capitalisation

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	-0.853	-1.038	-0.711	0.139
	(1.013)	(1.047)	(1.616)	(0.431)
Observations	53	23	30	44
Countries	25	11	14	23

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.15. Risk-Free Rate

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	8.648	**10.259***	7.669	0.168
	(5.582)	(5.577)	(8.385)	(1.472)
Observations	45	17	28	37
Countries	21	7	14	20

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.16. Exchange Rate

D.8.5. Event Analysis by Income Category (3 Month Event Window)

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	**-0.579****	-0.435	**-0.689****	**-0.366***
	(0.232)	(0.319)	(0.332)	(0.183)
Observations	60	26	34	48
Countries	32	16	16	24

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.17. Equity Return

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	-0.0215	0.125	-0.129	-0.423
	(0.878)	(0.567)	(1.475)	(0.320)
Observations	45	19	26	43
Countries	21	10	11	22

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.18. Risk Premium

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	**-0.877***	-1.303	-0.604	-0.182
	(0.476)	(0.919)	(0.522)	(0.164)
Observations	41	16	25	37
Countries	19	7	12	20

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.19. Market Capitalisation

	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	-0.712	-0.551	-0.837	0.137
	(0.627)	(0.486)	(1.052)	(0.253)
Observations	53	23	30	44
Countries	25	11	14	23

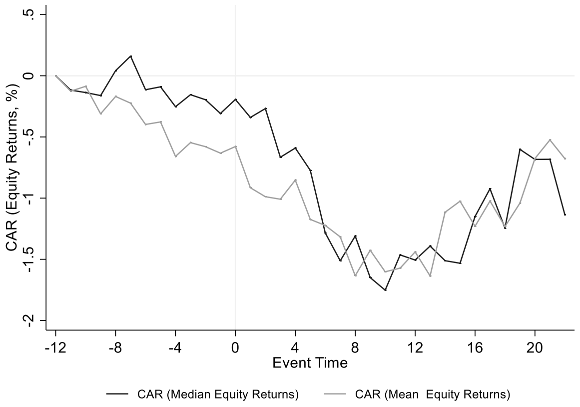
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.20. Risk-Free Rate

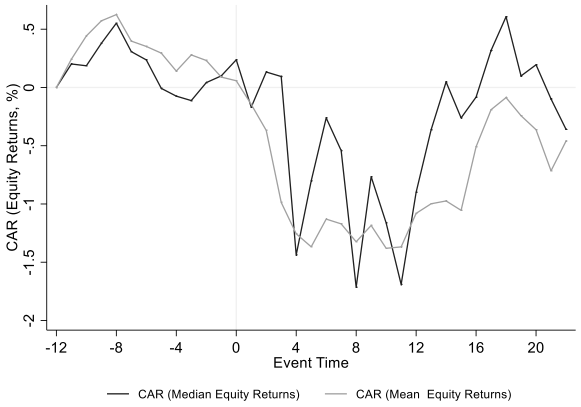
	(1) Middle & Lower Income	(2) Lower Middle & Low Income	(3) Upper Middle Income	(4) High-Income
Assassination	7.892	9.335	7.016	0.120
	(4.511)	(5.717)	(6.438)	(1.124)
Observations	45	17	28	37
Countries	21	7	14	20

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.21. Exchange Rate



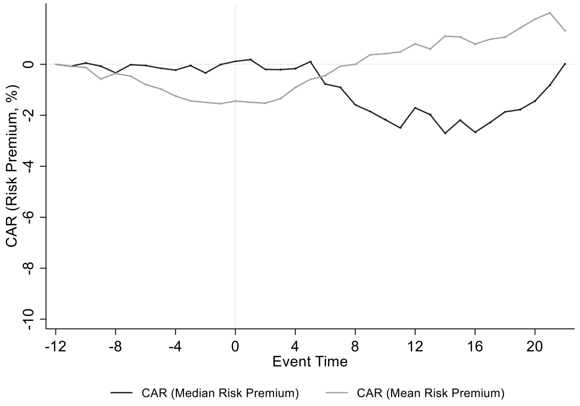
Lower Middle & Low Income Countries



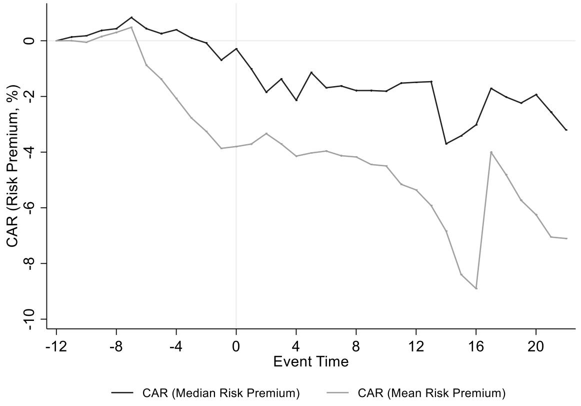
Upper Middle Income Countries

High-Income Countries

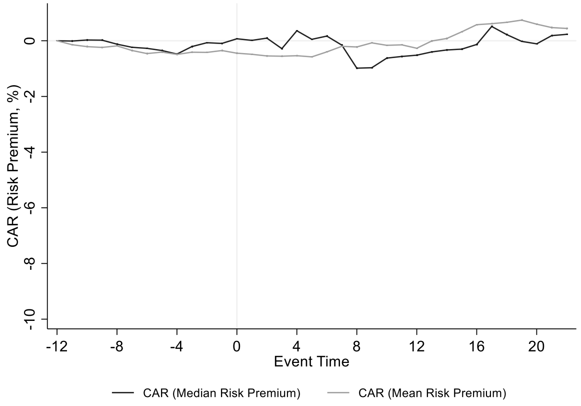
<u>Equity Return</u>



Lower Middle & Low Income Countries

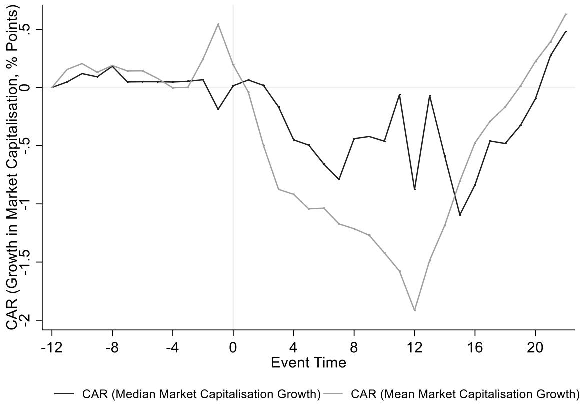


Upper Middle Income Countries



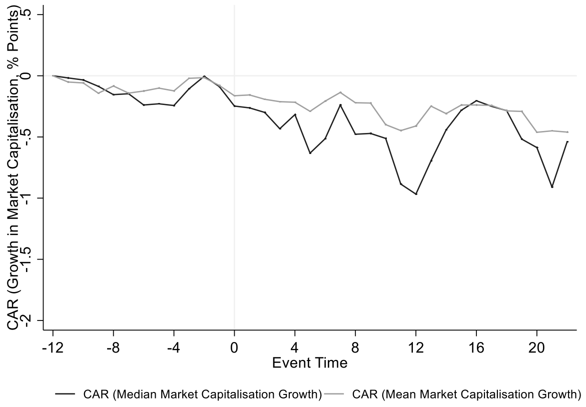
High-Income Countries

<u>Risk Premia</u>



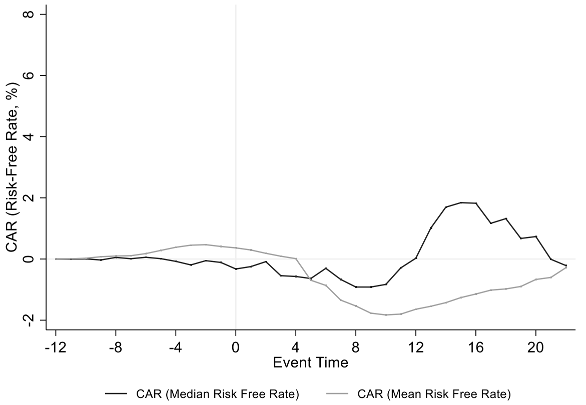
Lower Middle & Low Income Countries

Upper Middle Income Countries

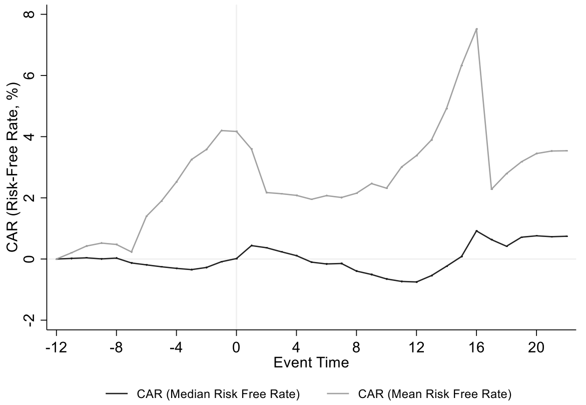


High-Income Countries

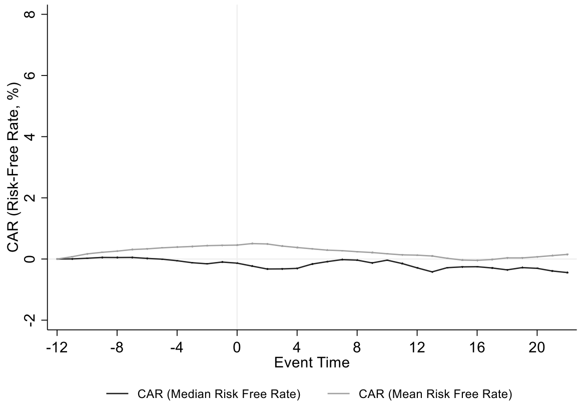
Market Capitalisation Growth Rate



Lower Middle & Low Income Countries

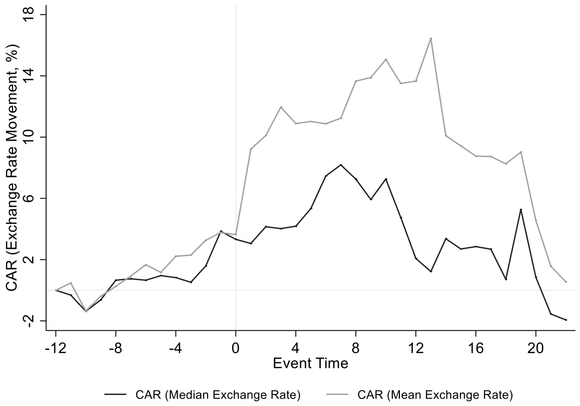


Upper Middle Income Countries

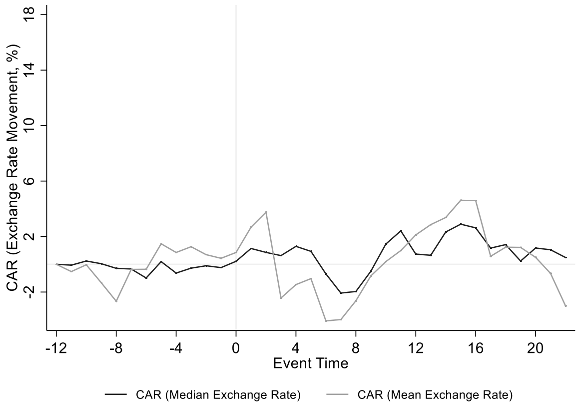


High-Income Countries

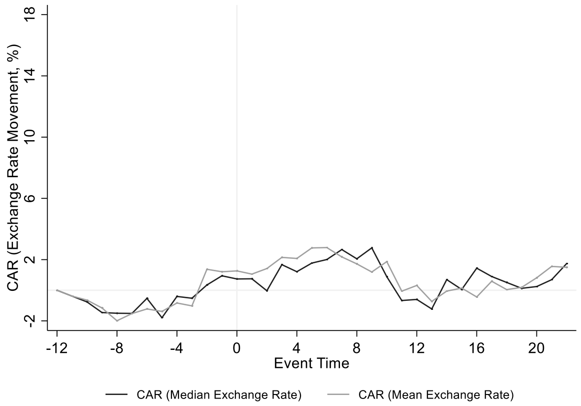
Risk-Free Rate



Lower Middle & Low Income Countries



Upper Middle Income Countries



High-Income Countries

<u>Real Exchange Rate Changes</u>

Figure D.5. Response Functions to Assassinations by Income Categories

D.9. Appendix

D.9.1. Time Decompositions

Despite the earlier result that more developed, higher-income countries experience shorter and less severe effects after an assassination, the decades of the 1990s and 2000s seem to drive the results, as in Girardi (2018) on elections and financial markets. This is counterintuitive, as global development has changed the world radically over the last half century and, intuitively, we would expect the effects to have decreased over time.

In table A.D.1., the main results before and after 1998 are shown, since 1998 is the mean year in the database.

	Equity Return		Risk Premium		Market Capitalisation		Risk-Free Rate		Exchange Rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Pre 1998	Post 1998	Pre 1998	Post 1998	Pre 1998	Post 1998	Pre 1998	Post 1998	Pre 1998	Post 1998
Assassination	**-0.786****	**-1.091***	-1.512	-0.516	-0.450	**-2.594****	0.795	-1.338	7.275	4.231
	(0.372)	(0.549)	(1.509)	(1.526)	(0.421)	(1.040)	(1.359)	(1.471)	(4.403)	(3.219)
Observations	75	33	69	19	60	18	78	19	64	18
Countries	43	25	40	15	35	14	46	15	37	14

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.D.1. 1998 Cut-off Date

Next, to emphasise that the main results are driven by the 1990s and 2000s, regressions are run for each decade separately.

	(1)	(2)	(3)	(4)	(5)
	1970	1980	1990	2000	2010
Assassination	-0.613	-0.358	**-1.119***	**-2.155****	-0.831
	(0.544)	(0.732)	(0.582)	(0.901)	(0.688)
Observations	25	29	25	13	16
Countries	23	23	23	12	15

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.22. Equity Return

	(1) 1970	(2) 1980	(3) 1990	(4) 2000	(5) 2010
Assassination	-0.0290	-3.308	-0.539	-3.667	2.521
	(1.462)	(3.099)	(2.550)	(2.110)	(2.311)
Observations	23	27	21	9	8
Countries	21	22	19	9	8

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.23. Risk Premium

	(1) 1970	(2) 1980	(3) 1990	(4) 2000	(5) 2010
Assassination	-0.0967	-0.190	-0.785	**-4.196****	-1.723
	(0.326)	(0.766)	(0.833)	(1.383)	(1.596)
Observations	17	22	23	9	7
Countries	16	19	21	9	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, *
p<0.1

Table D.24. Market Capitalisation

	(1) 1970	(2) 1980	(3) 1990	(4) 2000	(5) 2010
Assassination	-0.491	1.984	0.581	0.363	-3.602
	(1.345)	(2.854)	(2.111)	(1.975)	(2.602)
Observations	26	31	23	9	8
Countries	24	26	21	9	8

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.25. Risk-Free Rate

	(1) 1970	(2) 1980	(3) 1990	(4) 2000	(5) 2010
Assassination	0.798	4.481	14.37	5.998	3.010
	(4.874)	(3.514)	(10.62)	(6.147)	(2.188)
Observations	18	24	24	9	7
Countries	17	21	22	9	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.26. Exchange Rate

D.9.2. Endogenous estimation results

Throughout the paper, only assassinations with ‘exogenous’ estimation windows are included. Since event studies require stable baselines, cases with previous events in their estimation windows provide instable baselines and therefore yield biased estimates of abnormal returns. The following estimations with ‘endogenous’ estimations windows show no significant results except for an impact on stock market capitalisation about 3 times larger than under the constant mean return model and nearly 10 times that of the market model.

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	-0.148 (0.111)	-0.420 (0.430)	**-2.927*** (1.664)	-0.108 (0.462)	-1.180 (1.531)
Observations	557	481	443	508	443
Countries	56	41	40	45	42

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table D.27. OLS with Robust Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Cap Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	-0.148 (0.103)	-0.420 (0.449)	**-2.927*** (1.624)	-0.108 (0.400)	-1.180 (1.522)
Observations	557	481	443	508	443
Countries	56	41	40	45	42

Bootstrapped standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table D.28. OLS with Bootstrapped Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Cap Capitalisation	(4) Risk-Free Rate	(5) Exchange Rate
Assassination	-0.0532	-0.140	-0.165	0.0409	-0.0136
	(0.0614)	(0.0861)	(0.419)	(0.0273)	(0.217)
Observations	557	481	443	508	443
Countries	56	41	40	45	42

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.29. Quantile Regression

D.9.3. Sensitivity Analysis

The following section emphasises the consistency of the results from the main paper, despite the chosen estimation and event windows often being improbable. Overall, this sensitivity analysis reinforces the finding that investors withdraw from stock markets following an assassination, but the effect on exchange rate depreciation is less clear.

<u>36 Month Estimation Window with a 12 Month Event Window</u>

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.701****	-1.200	**-0.997****	0.472	5.136
	(0.309)	(1.302)	(0.431)	(1.238)	(3.481)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.30. Robust Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.701****	-1.200	**-0.997*****	0.472	5.136
	(0.304)	(1.131)	(0.307)	(1.186)	(3.987)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.31. Bootstrapped Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.815****	-1.177	-0.514	-0.0361	0.895
	(0.405)	(0.758)	(0.393)	(0.432)	(2.249)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.32. Quantile Regression

48 Month Estimation Window with a 12 Month Event Window

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.742****	-1.368	**-1.204*****	0.715	5.153
	(0.310)	(1.701)	(0.430)	(1.635)	(3.742)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.33. Robust Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.742****	-1.368	**-1.204*****	0.715	5.153
	(0.326)	(1.613)	(0.368)	(1.418)	(4.001)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.34. Bootstrapped Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.750****	**-1.225***	**-0.728***	-0.0246	0.732
	(0.357)	(0.700)	(0.373)	(0.527)	(2.128)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.35. Quantile Regression

24 Month Estimation Window with a 3 Month Event Window

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.485*** **	-0.218	**-0.547** **	-0.327	**4.385***
	(0.152)	(0.473)	(0.263)	(0.362)	(2.550)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.36. Robust Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.485*** **	-0.218	**-0.547** **	-0.327	4.385
	(0.150)	(0.418)	(0.237)	(0.361)	(2.730)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.37. Bootstrapped Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.439** **	**-0.431***	-0.224	-0.132	0.638
	(0.190)	(0.238)	(0.144)	(0.130)	(0.986)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.38. Quantile Regression

<u>24 Month Estimation Window with a 6 Month Event Window</u>

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.670*** **	-0.343	**-0.640** **	-0.403	4.822
	(0.203)	(0.727)	(0.308)	(0.587)	(3.153)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.39. Robust Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.670*** **	-0.343	**-0.640* **	-0.403	4.822
	(0.213)	(0.649)	(0.341)	(0.582)	(3.555)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.40. Bootstrapped Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.575** **	-0.727	**-0.356** **	-0.150	1.121
	(0.252)	(0.512)	(0.165)	(0.229)	(1.155)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.41. Quantile Regression

48 Month Estimation Window with 3 Month Event Window

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.441***** (0.154)	-0.241 (0.496)	**-0.633**** (0.263)	-0.202 (0.409)	3.910 (2.585)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.42. Robust Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	**-0.441***** (0.159)	-0.241 (0.498)	**-0.633***** (0.235)	-0.202 (0.429)	3.910 (2.698)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.43. Bootstrapped Standard Errors

	(1) Equity Return	(2) Risk Premium	(3) Market Capitalisation	(4) Risk Free-Rate	(5) Exchange Rate
Assassination	-0.235 (0.190)	-0.208 (0.236)	**-0.289*** (0.148)	-0.0893 (0.134)	0.180 (1.051)
Observations	108	88	78	97	82
Countries	56	43	39	48	41

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.44. Quantile Regression

D.9.4. Risk-Free Rate

Since 3-month Treasury bill yields were not available for the entire duration of the study for each country, alternative risk-free rate proxies were often required, as outlined above. 3-months Treasury bills were always selected, when available, and spliced with the alternative risk-free rate series that most accurately resembled it in overlapping periods. Figure A.D.1. outlines how similar these yields were over time and further motivates the use of alternative risk-free rate proxies.

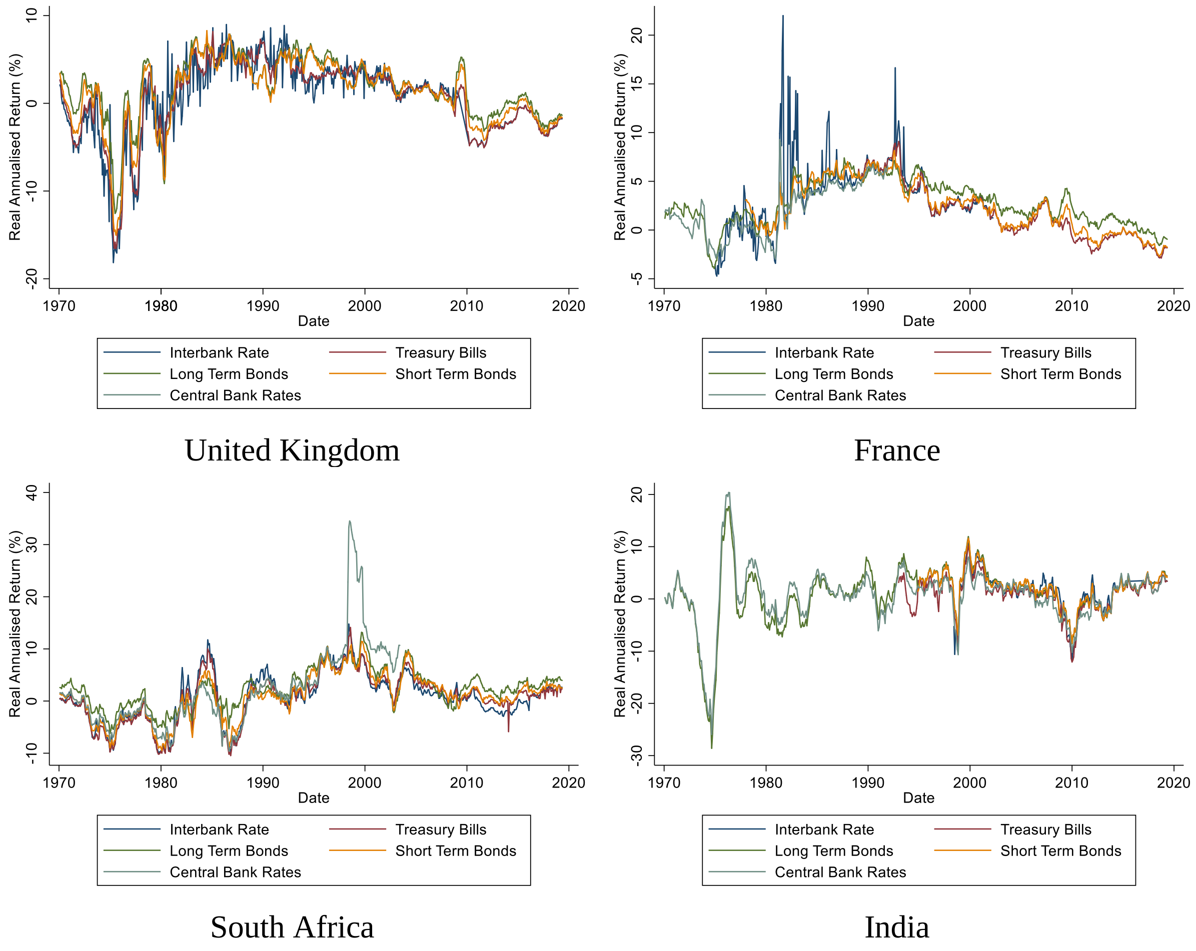


Figure A.D.1. Selected Risk-Free Rate Comparisons

D.9.5. Income Classifications

ISO 2 Code	Country	UN Classification
ar	Argentina	upper middle-income
au	Australia	high-income
at	Austria	high-income
bd	Bangladesh	lower middle and low-income
be	Belgium	high-income
br	Brazil	upper middle-income
ca	Canada	high-income
cl	Chile	high-income
cn	China	upper middle-income
co	Colombia	upper middle-income
cy	Cyprus	high-income
eg	Egypt	lower middle and low-income
fr	France	high-income
de	Germany	high-income
el	Greece	high-income
in	India	lower middle and low-income
id	Indonesia	lower middle and low-income
ir	Iran	upper middle-income
ie	Ireland	high-income
il	Israel	high-income
it	Italy	high-income
jm	Jamaica	upper middle-income
jp	Japan	high-income
jo	Jordan	upper middle-income
ke	Kenya	lower middle and low-income
kr	Korea, Republic of	high-income
kg	Kyrgyzstan	lower middle and low-income
lb	Lebanon	upper middle-income
mx	Mexico	upper middle-income
ma	Morocco	lower middle and low-income
np	Nepal	lower middle and low-income
nl	Netherlands	high-income
ng	Nigeria	lower middle and low-income
pk	Pakistan	lower middle and low-income
pa	Panama	upper middle-income
py	Paraguay	lower middle and low-income
pe	Peru	upper middle-income
ph	Philippines	lower middle and low-income
pt	Portugal	high-income
rs	Serbia	upper middle-income
za	South Africa	upper middle-income
es	Spain	high-income
lk	Sri Lanka	lower middle and low-income
se	Sweden	high-income
ch	Switzerland	high-income
sy	Syria	lower middle and low-income
tw	Taiwan	high-income
th	Thailand	upper middle-income
tt	Trinidad and Tobago	high-income
tn	Tunisia	upper middle-income
ug	Uganda	lower middle and low-income
ua	Ukraine	lower middle and low-income
uk	United Kingdom	high-income
us	United States	high-income
uy	Uruguay	high-income
ve	Venezuela	upper middle-income

Table D.45. Country List and UN Development Classifications

Note: These are the countries for which assassinations took place, according to the Global Terrorism Database, for which financial data could be matched.

D.9.6. Assassination Maps

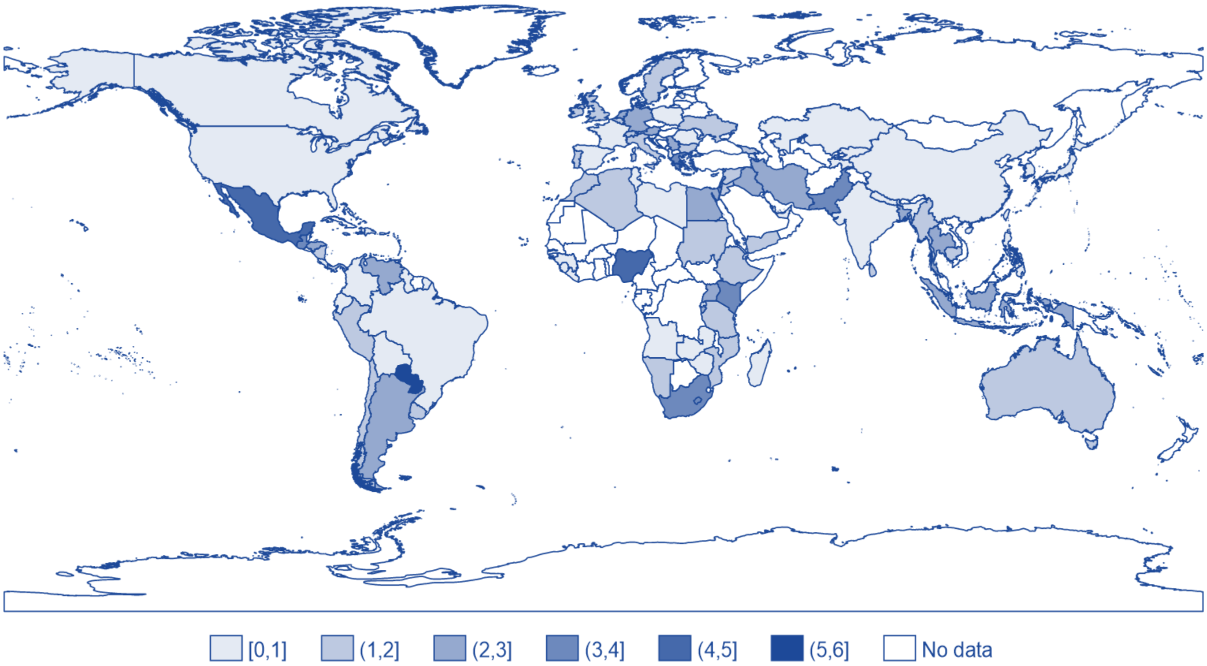


Figure D.6. Matched Assassinations with Endogenous Estimation Windows

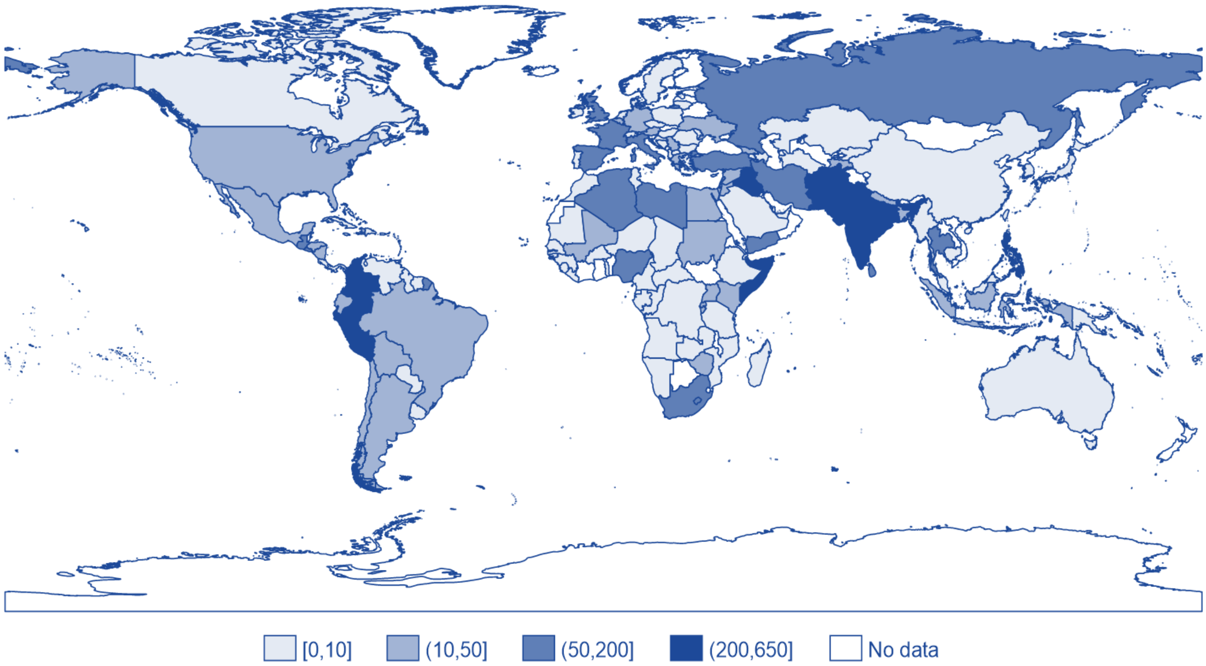


Figure D.7. All Political Assassinations (1970 – 2017)

E. Summary and Outlook

Literature on the processes that induce economic development has yielded theories emphasising the roles of institutional design, geography, gender equality and human capital. Violence, however, has largely been treated as an outcome of development rather than a contributing factor (McIlwaine 1999; Enamorado et al. 2014). On an individual level, violence is largely driven by psychological factors, but these cannot explain regional or societal disparities. Therefore, the first of three studies in this dissertation contributes to the literature by finding a causal effect of elite violence on elite human capital. The inverse relationship that is derived is an important result, since human capital encourages technological innovation and is an important driver of economic growth (see Becker 1962; Mincer 1984; Acemoglu and Dell 2010; and Barro 2001). Further, chapter B also contributes to the Great Divergence debate and shows that at its origins were rooted in violence, at least to a certain degree, from as far back as the 14th century.

The second chapter's chief contribution is that of the regicide indicator. Eisner's (2011) idea was heavily expanded upon in this paper as we include more than 4000 rulers from across Europe between the 6th and 19th centuries and provide a wider-ranging and longer-term indicator for violence than has been available previously. Since empirical evidence of violence from before the 19th century is only available sporadically and for parts of Western Europe, the regicide indicator opens up entirely new avenues of violence research. Europe undoubtedly has the most complete and far-reaching dynastic lists of all world regions, as well as the most detailed biographical accounts of rulers from which the regicide indicator is constructed. Nevertheless, documenting rulers has been a universal phenomenon throughout history, meaning that this chapter sets a precedent which could also be followed in future studies of other world regions and perhaps even help to explain patterns of development elsewhere. This chapter then goes on to study the role of state capacity in elite violence, using ‘territorial state capacity’, following the divergent hypotheses of researchers such as Pinker (2011) and Tilly (1975). The empirical evidence shows a negative relationship between territorial state capacity and regicide, illustrating that state capacity likely had a largely pacifying role on trends and regional differences in interpersonal elite violence, at least since the High Middle Ages.

Finally, as a means of providing a more rounded impression of the consequences of elite violence, and of assassinations in particular, the dissertation proceeded to investigate how shocks to political risk have caused investors to react in terms of global asset allocation since 1970. How investors allocate their holdings influences the levels of financing available to firms or national treasuries; affecting the ability of firms to grow and develop their own industries as well as the ability of governments to provide public services and to direct fiscal policy.

The setting of political risk and assassinations provides an interesting conundrum, as financial doctrine dictates that investors reallocate their holdings from stocks to sovereign bonds, while governmental institutions are the source of increased risk in the aftermath of political assassinations (Markowitz 1952). The results obtained from an event analysis illustrate that investors tend to disinvest from equity following political assassinations but refrain from pursuing traditional risk-free instruments. Subsequent exchange rate depreciations likely indicate that investors prefer to move their holdings abroad, signalling that, on average, investors do not act blindly and simply follow financial doctrine. In order to make a stronger conclusion about the destination of financial flows subsequent to political assassinations, an ideal extension to this study would employ spatial methods to examine financial spillovers in conjunction with exchange rate impacts.

The study also reveals that developing countries tend to experience more severe and persistent financial market effects in the aftermath of political assassinations. Because developing countries are associated with weaker institutions, investors may believe that existing political and macroeconomic policies could be threatened by the successor of an assassinated individual. Conversely, due to stronger political institutions, investors may see political assassinations in developed countries as isolated events with the continuance of existing policies guaranteed. This suggests that setting clear and long-term policy agendas may be beneficial for governments of developing countries and help to alleviate the volatility of future capital flows.

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