DISCUSSION PAPER SERIES

DP14013

ELITE VIOLENCE AND ELITE NUMERACY IN EUROPE FROM 500 TO 1900 CE: A CO-EVOLUTION?

Thomas Keywood and Jörg Baten

ECONOMIC HISTORY

ISSN 0265-8003

ELITE VIOLENCE AND ELITE NUMERACY IN EUROPE FROM 500 TO 1900 CE: A CO-EVOLUTION?

Thomas Keywood and Jörg Baten

Discussion Paper DP14013
Published 20 September 2019
Submitted 19 September 2019

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

ELITE VIOLENCE AND ELITE NUMERACY IN EUROPE FROM 500 TO 1900 CE: A CO-EVOLUTION?

Abstract

We develop a new indicator for elite numeracy in order to investigate trends in European elite numeracy since the 6th century CE and describe its co-evolution with elite violence over this period. 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 and conclude that violence had a detrimental impact on human capital formation. The drastic increase in human capital since the High Middle Ages was at least partially due to declining violence.

JEL Classification: N00, N13, N33

Keywords: elite human capital, elite violence, Great Divergence, Europe, middle ages, Early Modern Period

Thomas Keywood - thomas.keywood@uni-tuebingen.de
University of Tuebingen

Jörg Baten - joerg.baten@uni-tuebingen.de
University of Tuebingen and CEPR

Acknowledgements
We thank the German Science Foundation (DFG) for grant BA1503/18-1, and comments by participants at the WEHC Boston, EHES Tübingen, WEast Cracow, and the Tübingen research group.

Elite Violence and Elite Numeracy in Europe from 500 to 1900 CE: A Co-Evolution?

Thomas Keywood* and Jörg Baten**

Abstract

We develop a new indicator for elite numeracy in order to investigate trends in European elite numeracy since the 6th century CE and describe its co-evolution with elite violence over this period. 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 and conclude that violence had a detrimental impact on human capital formation. The drastic increase in human capital since the High Middle Ages was at least partially due to declining violence.

*Univ. Tübingen, **Univ. Tübingen, CEPR and CESifo

* thomas.keywood@uni-tuebingen.de, ** joerg.baten@uni-tuebingen.de

Keywords: elite human capital, elite violence, Great Divergence, Europe, Middle Ages, Early Modern Period

JEL: N00, N13, N33

Acknowledgements: We thank the German Science Foundation (DFG) for grant BA1503/18-1, and comments by participants at the WEHC Boston, EHES Tübingen, WEast Cracow, and the Tübingen research group.

1. Introduction

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 and Perrin 2013 and Broadberry 2013). In this study, we suggest that the role of violence, though inextricably linked to the majority of explanations that have been proposed previously, has been under-researched and largely neglected (exceptions: 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 economic development through human capital formation.

Our study aims to clarify when and why certain parts of Europe developed differently; consequently, assessing the role of violence in the long-term development of human capital will be the core effort of this paper.

Our strategy for approaching this question will rely on proxy indicators, as standard indicators of violence and human capital are not available for early periods of European history. Hence, here, we establish a new indicator that is able to trace the development of elite numeracy over the very long term – we employ 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 elite numeracy indicators in medieval societies for which both metrics were simultaneously available in the same location.

We contribute to a modestly sized but growing literature on elite numeracy. Baten and van Zanden (2008) studied book consumption as an indicator of advanced numeracy in early modern Europe, while Squicciarini and Voigtländer (2015) concluded that it played a key role in building human capital for early French industrial development. Our approach allows us to answer 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 but also less densely populated regions – had an elite numeracy level at least equal to that of Western Europe.

Another main contribution of this study is to explore the correlates and potential determinants of elite numeracy. In particular, we address the issue of whether either military violence or interpersonal violence among the ruling faction was related to elite numeracy. This question will also be examined through proxy indicators. First, we use the share of rulers who were killed in battles to estimate military violence. Other cases of interpersonal violence are measured by the share of murdered rulers outside of battles – if killed, rulers were typically murdered by their own family members or by competing nobility (see Baten 2018 on the Middle East; Eisner 2011 on Europe). Clearly, violence was not the only factor that mattered for elite numeracy; we also include religion, geography, institutional factors such as serfdom and early electoral elements of ruler succession, as well as other potential determinants.

We contribute to current explanations for the divergences in human capital formation between Southern, Eastern and North-Western Europe. This is particularly important because the developmental divergence among European regions during the early Modern Period is not yet fully understood and has implications for European cooperation and coherence today (de

Pleijt and van Zanden 2016). In particular, different hypotheses about institutional development have been proposed, which, given the lack of appropriate indicators, could only be indirectly assessed by qualitative and quantitative evidence until now (for selected studies see, among others, Buringh and Van Zanden 2009; Kelly et al. 2014; Baten and Hippe 2018; Galor 2008; Voigtländer and Voth 2013; Diebolt, Le Chapelain and Menard (2017). For a summary, see Baten 2016). We also discuss whether institutional development might have been partially endogenous to violence. Market institutions work better with trust, and trust can only develop in the absence of violence (Baten and Steckel 2018). Only in such situations can incomplete contracts be used to regulate market exchange and promote human capital formation. 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.

2. Measuring Elite Numeracy

Our indicator for elite numeracy is the share of known birth years among all rulers residing in the capitals of principalities located in a given country. 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, 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.

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 (700 – 1500 CE), using them to construct a per capita indicator. In figure 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 correlations are nevertheless 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 2). Here, the correlation coefficient is ρ=-0.58.

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-term 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. In fact, this method of elite selection was used less often in periods after Central Asian nomads had conquered parts of China, possibly because cultural assimilation into Chinese society only happened gradually. These periods were also characterised by lower elite numeracy rates as measured by the known ruler birth year proxy, as seen in figure 3.[1] In sum, the Chinese evidence allows us to complement our comparisons of European monastery manuscripts, 'birth year heaping' and elite numeracy, measured by the share of known ruler birth years, with another world region.

To estimate elite numeracy via the known birth year rate for medieval Europe, we had to make some 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 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 (in smaller principalities, for example) within modern country borders, we also assigned them to the modern country according to where their capital was located.[2] 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

[1] 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.

[2] 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 15).

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 opposite approach is obviously more problematic and could lead to significant measurement error; for example, large empires that spanned many modern countries, such as Russia and Austria-Hungary. We assigned these Czars and Emperors to modern Russia and Austria, respectively, because their bureaucracies also resided in the capitals located in these countries. The main explanatory variables that we assess below also relate to the same modern geographical units described here.

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

One could imagine 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 they might be substantial or not.

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 a strong research tradition 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 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 taking place even centuries later. However, somewhat surprisingly, many of these countries actually had lower known-birth-year rates in the Middle Ages than, for example, today's Iraq, Turkey or Greece (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 a loss of previously existing records; however, royal chronologies were traditionally copied. Even if one city archive were destroyed, any 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. 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.

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 of their rule. For example, some rulers originated from less numerate, nomadic societies in Central Asia – such as some of the early Bulgarian rulers. Here, we have to distinguish between a truly lower level of elite numeracy among these rulers and their elites, which is what we want to measure, and a bias that stems from a lack of information about their births in foreign and possibly distant lands. The latter might imply less knowledge about the first generation, 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 potential bias that could lead to concern.

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 2).

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 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.[3] We find, in table 2, that rulers who served this governor-type function were not significantly different from their overlords in terms of elite numeracy after controlling for country and century fixed effects. In conclusion, these developments would speak against a potential fame bias if we assume 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' included.

[3] 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.

4. Measuring Potential Determinants of 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.

Cummins (2017) argues that a substantial share of noblemen in the medieval period died through acts of violence, such as on the battlefield. Given that lifespans and the prevalence of violence are negatively correlated – though not perfectly, as other factors importantly also influence lifespans – we argue that part of the underinvestment in elite human capital during this early period was caused by lower lifespans. Individuals did not have an incentive to invest as much into numerical human capital if they expected to die early. If they did invest in human capital, they would probably prefer to invest in military education, exercising their skills to act with sword and axe.

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 are 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 among 4738 skeletons that cover the period 300 to 1900 CE and find 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 Keywood and Baten 2018 for an econometric analysis with a strongly expanded European sample).

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 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 main exception was Italy during the 19th century, where instability before and during unification generated higher regicide rates relative to the evidence from homicide.

Although these subfigures all display strong declines, the panel unit root tests that we run in the appendix 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. This strong relationship also validates our use of regicide as a proxy for elite interpersonal violence, discussed in more depth in Keywood and Baten (2018). Finally, temporal autocorrelation does not play a strong role because our main results also hold in first differences (see appendix, table 11.1).

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 in Europe, they declined, 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 Keywood and Baten 2018 for more details).

5. Data and Regional Trends

When looking at regional trends in elite numeracy (figure 5), we can see that North-Western Europe did not always lead the way; rather, it was South-Western Europe with Iberia and Italy, and South Eastern Europe, led by the East Roman Empire, that had the highest levels of numeracy during the early Middle Ages, although they fell back thereafter. North-Western Europe was on a more stable growth path, however, taking the lead in the 10^{th} – 13^{th} centuries. By the 14^{th} and 15^{th} 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.

North-Eastern Europe began the sixth century with approximately 20% known birth years, or just slightly lower. Their 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 12^{th} century. Only later does Romania exhibit a strong growth rate in elite numeracy. In the period between 1200 and 1800 CE, 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.[4] Admittedly, we have few observations for the East Roman Empire (with its capital located in today's Turkey) in the first period, but our figure shows a clear deterioration of elite numeracy during the decline of the Byzantine Empire, followed by stagnation in the years that followed; also coinciding 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 year 1800, a lag of approximately 400 years. Central European trends are not shown here because they have a very high starting point and quickly reach 100%. However, they are presented as a group in figure 6, which plots elite numeracy for broader regions in a single figure.

In figure 5, 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. 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

[4] 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 the kingdoms neighbouring it at the time.

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 (see table 13 for a robustness check at the regional level), but they are less robust due to the smaller sample size.

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 more 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 a ruler had the freedom to act and set policy autonomously or whether they were under the suzerainty of an overlord. Table 2 shows that neither of the last two factors 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.[5]

The 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 lowered incentives to invest in elite numeracy due to lower life expectancy (Cummins

[5] As a precaution, the full fixed effects specification from this section 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.

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.

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 admit that as urbanisation may be endogenous, there may be a trade-off between including an endogenous control and allowing omitted variable bias. Therefore, we include urbanisation in our regression models to avoid omitted variable bias but exclude it in certain specifications to avoid this bad-control problem.

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 certain preference for the division and limitation of dynastic power.[6] 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).[7] The differences in institutional quality between states, seen through modes of succession, is

[6] Among other elites, since universal suffrage is a relatively recent phenomenon.

[7] 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.

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. 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 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 their human capital and contribute to economic development (Diebolt and Perrin 2013).

Fourth, as a counterweight to the pasture 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 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 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.

We include some additional variables below, such as religion and geographic variables, which are mostly time invariant, in a section estimating random effects models.

6.1 Fixed Effects Specification

We undertake an empirical analysis that consists of four 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 conduct spatial regressions to uncover whether spatial autocorrelation affects our results. Third, 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.

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 employing the wild bootstrap procedure of Cameron et al. (2008, see notes to Table 3).

We immediately see that both the regicide and battle death indicators enter into each regression model significantly and with a negative sign (table 3). These coefficients are also fairly stable across our specifications, implying that our control variables are less important for elite numeracy than violence seems to be. The coefficient for regicide is always 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 known-birth rate. 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, we can also imagine 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 a significant impact 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 of 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 appear 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 raised 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 3 while omitting elite violence and then regressing elite violence on all other explanatory variables.[8] Figure 7 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 and Montenegro and from Lithuania in the 14th century show high residual violence and low residual elite numeracy. On the other hand, there are cases such as the East Roman Empire (with its capital in what is today Turkey) that have low residual violence and high residual elite numeracy in the 6th century. Another interesting aspect of this figure pertains to the cases located north-east of the regression line, e.g., Hungary in the 11th century and Sweden in the 12th century. These regions reached relatively high elite numeracy levels despite remaining fairly violent. This is not true for the examples on the other side of the spectrum, such as Romania in the 14th century. However, in general, we observe a very close relationship between residual violence and residual elite numeracy.

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

6.2 Spatial Regression

While the results from our fixed effects specification provide a solid point of departure for our coevolution 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 higher if spatial autocorrelation is not controlled for. 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 Rules (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 10.1 and 10.2), 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 4.1), the Spatially Lagged X Model (SLX Model; equation 3, table 4.2) and the Spatial Error Model (SEM; equation 4, table 4.1).

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

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

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

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).[9] 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 4.1 and 4.2).

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 6.1 (equation 1, table 3); 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

[9] 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).

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 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.

6.3 Instrumental Variable Specification

Although the fixed effects 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 to assess whether any causal effects exist. Clearly, finding suitable instruments for the medieval period is a substantial challenge, but there were certain events that 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.

The invasions of the Hungarians, Mongols, Huns and other equestrian-driven nomads affected much of Europe, their distinctive style of warfare resulting in imported violence, adding to existing European violence. 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 now European Kingdom of Hungary offered little resistance to Mongol invasions (Sinor 1999).[10]

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 at play (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 called the area home (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, although these tactics had already been developed in earlier times and were known throughout Europe. For example, the "Parthian shot" was a Parthian military tactic of mounted archers firing at their enemies while in actual or staged

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

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 they possessed resulted in geographic constraints that we can 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 more "imports 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 Asian invasions (and added to existing European violence). Clearly, the Hungarians and Mongols were not the only groups that spread violence over such large distances.[11] The Viking raids of the 9^{th} and 10^{th} centuries, the Arab-Berber invasions of Iberia and parts of Italy, as well as the Ottoman invasions in the

[11] 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.

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 they were, 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 is not evident for the early medieval period. We have seen, in figure 6, that elite numeracy was highest in South-Eastern Europe during the period 500 – 700 CE, when the East Roman Empire was the gravitational centre of European development. The second highest 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 occupation of Gaul and Britain did not cause an east-west divergence in the early medieval period, according to our evidence. Figure 11 supports this line of reasoning through the coefficients of regressions of elite numeracy on longitude over time.[12] 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, can serve as such a test. 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, 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.

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

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 do not immediately spring to mind, but one could imagine that the nomads brought diseases with them and 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 Black Death 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, nothing obvious comes to mind. 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 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, circa 800 – 1000 CE, and the Mongolians, approximately 1200 – 1400 CE. 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 possibly 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 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.

6.4 Random Effects Specification with Partially Endogenous and 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 include, first, 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 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 (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 find similar levels of numeracy across Catholicism, Protestantism and Islam, with some evidence of lower levels for Orthodoxy and our 'other' category. Surprisingly, Protestantism did not make much of a difference (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 a positive effect on numeracy, perhaps via competition[13] – stimulating book consumption, for example – or a negative effect via conflict through social fractionalisation (Easterly and Levine 1997). However, we find no real evidence of an effect here.

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%.

[13] Rasul and Roger (2015) find a positive effect of ethnic fractionalisation on project completion rates among bureaucrats.

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 & 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 & 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.

7. Conclusion

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

The south-east was the first region to undergo transformation, led by the East Roman Empire (figure 6). Shortly afterwards, the south-west was slightly superior. All European regions were quite similar 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. A very consistent negative correlation is observable with violence – both violence during battles and "normal", interpersonal violence among the elite. Given the challenges in identifying causality, we prefer to describe European violence and the history of elite numeracy as a co-evolutionary process, but we can nevertheless employ a relatively exogenous import of violence in 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 violence did not follow any east-west pattern at this time (figure 9, panel b). By using the "natural experiment" characteristics of the nomadic invasions, we observe casual effects from violence to elite numeracy.

Other variables also had a modest influence on elite numeracy. Constraining the tyranny of rulers with either partial or full elective systems had a varying effect, while 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, regional units 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 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.

References

A'Hearn, B., Baten, J. & Crayen, D. 2009. Quantifying quantitative literacy: Age heaping and the history of human capital. *The Journal of Economic History*, 69(3): 783–808.

Acemoglu, D., Johnson, S., Robinson, J., 2005. The rise of Europe: Atlantic trade, institutional change, and economic growth. *The American Economic Review*, 95(3): 546–579.

Adshead, S. 2016. *Central Asia in world history*. London: Palgrave Macmillan.

Alesina, A., Giuliano, P., & Nunn, N. 2013. On the origins of gender roles: Women and the plough. *The Quarterly Journal of Economics*, 128(2): 469–530.

Allen, R. 2001. The Great Divergence in European Wages and Prices from the Middle Ages to the First World War, *Explorations in Economic History*, 38(4): 411–447.

Anderson, R., Johnson, N. & Koyama, M. 2017. Jewish persecutions and weather shocks: 1100–1800. *The Economic Journal*, 127(602): 924–958.

Bai, Y. & Kung, J., 2011. Climate shocks and Sino-nomadic conflict. *Review of Economics and Statistics*, 93(3): 970–981.

Baker, J., White, N. & Mengersen, K. 2014. Missing in space: an evaluation of imputation methods for missing data in spatial analysis of risk factors for type II diabetes. *International Journal of Health Geographics*, 13(47): 1–13.

Baten, J. & Hippe, R. 2018. Geography, land inequality and regional numeracy in Europe in historical perspective. *Journal of Economic Growth*, 23(1): 79–109.

Baten, J. & van Zanden, J. 2008. Book Production and the Onset of Early Modern Growth, with Jan Luiten van Zanden, *Journal of Economic Growth*, 13(3): 217–235.

Baten, J. 2016. *A history of the global economy*. Cambridge: Cambridge University Press

Baten, J. 2018. "Elite Violence and Elite Numeracy in the Middle East since the early Middle Ages". Working Paper, Univ. Tübingen.

Baten, J., Steckel, R., Larsen, C. S. & Roberts, C. A. 2018. Multidimensional Patterns of European Health, Work, and Violence over the Past Two Millennia, in Steckel, R., Larsen, C. S., Roberts, C. A. & Baten, J. *The Backbone of Europe: Health, Diet, Work and Violence over Two Millennia*. Cambridge: Cambridge University Press. pp. 381–396.

Baten, J., Szołtysek, M., & Campestrini, M. 2017. "Girl Power" in Eastern Europe? The human capital development of Central-Eastern and Eastern Europe in the seventeenth to nineteenth centuries and its determinants. *European Review of Economic History*, 21(1): 29–63.

Becker, S. & Woessmann, L. 2009. Was Weber wrong? A human capital theory of Protestant economic history. *The Quarterly Journal of Economics*, 124(2): 531–596.

Becker, S. & Woessmann, L. 2010. The effect of Protestantism on education before the industrialization: Evidence from 1816 Prussia. *Economics Letters*, 107(2): 224–228.

Bihrmann, K. & Ersbøll, A. 2015. Estimating range of influence in case of missing spatial data: a simulation study on binary data. *International Journal of Health Geographics*, 14(1): 1–13.

Bohara, A., Mitchell, N. & Nepal, M. 2006. Opportunity, democracy, and the exchange of political violence: A subnational analysis of conflict in Nepal. *Journal of Conflict Resolution*, 50(1): 108–128.

Bosker, M., Buringh, E. & van Zanden, J. 2013. From Baghdad to London: Unravelling Urban Development in Europe, the Middle East, and North Africa, 800–1800. *Review of Economics and Statistics*, 95(4): 1418–1437

Bosworth, C. 1996. *The New Islamic Dynasties*. New York: Columbia University Press.

Botticini, M, & Eckstein, Z. 2012. *The chosen few: How education shaped Jewish history, 70-1492*. Princeton: Princeton University Press.
Bowlus, C. R. 2006. *The battle of Lechfeld and its aftermath, August 955: The end of the age of migrations in the Latin West*. Abingdon-on-Thames: Routledge.
Broadberry, S. 2013. *Accounting for the great divergence.* Economic History Working Papers, 184/13. London School of Economics and Political Science, London, UK.
Buringh, E. & Van Zanden, J. 2009. Charting the "Rise of the West": Manuscripts and Printed Books in Europe, A Long-Term Perspective from the Sixth through Eighteenth Centuries. *The Journal of Economic History*, 69(2): 409–445.
Burkey, M. 2017. Overview of Spatial Econometric Models. Available at: https://www.youtube.com/watch?v=6qZgchGCMds&list=PLlnEW8MeJ4z6Du_cbY6o08KsU6hNDkt4k&index=2 [Accessed 29 Aug. 2019].
Cameron, A., Gelbach, J. & Miller, L. Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, 90(3): 414–427.
Cantoni, D. 2015. The economic effects of the protestant reformation: Testing the weber hypothesis in the German lands. *Journal of the European Economic Association*, 13(4): 561–598.
Cummins, N. 2017. Lifespans of the European Elite, 800–1800. *The Journal of Economic History*, 77(2): 406–439.
De Long, J. & Shleifer, A. 1993. Princes and merchants: European city growth before the industrial revolution. *The Journal of Law and Economics*, 36(2): 671–702.
De Pleijt, A. & van Zanden, J. 2016. Accounting for the "Little Divergence": What drove economic growth in pre-industrial Europe, 1300–1800? *European Review of Economic History*, 20(4): 387–409.
De Pleijt, A., Nuvolari, A., & Weisdorf, J. 2016. Human Capital Formation during the First Industrial Revolution: Evidence from the Use of Steam Engines. CAGE Online Working Paper Series, 1–43.
Deng, G. 1993. *Development versus stagnation: technological continuity and agricultural progress in pre-modern China*. Westport: Greenwood Press.
Diebolt, C., Le Chapelain, C. & Menard, A.R. 2017. Industrialization as a Deskilling Process? Steam Engines and Human Capital in XIXth Century France, Working Papers 07-17, Association Française de Cliométrie (AFC)
Diebolt, C. & Perrin, F. 2013. From stagnation to sustained growth: the role of female empowerment. *American Economic Review*, 103(3): 545–549.
Donnelly, F. 2012. Geographic Centroids for Countries [Data File]. Retrieved from: https://atcoordinates.info/resources/.
Easterly, W. & Levine, R. 1997. Africa's Growth Tragedy: Policies and Ethnic Divisions. *Quarterly Journal of Economics*, 112(4): 1203–1250.
Eisner, M. 2011. Killing kings: patterns of regicide in Europe, AD 600–1800, *British Journal of Criminology*, 51(3): 556–77.
Eisner, M. 2014. From swords to words: does macro-level change in self-control predict long-term variation in levels of homicide? *Crime and Justice*, 43(1): 65–134.
Elias, N. 1939. *Über den Prozess der Zivilisation. Soziogenetische und psychogenetische Untersuchungen*. Verlag Haus zum Falken: Basel.
Findlay, R. & O'Rourke, K. 2009. *Power and plenty: trade, war, and the world economy in the second millennium*. Princeton: Princeton University Press.
Galor, O. 2008. Economic Growth in the Very Long Run. In: Durlauf, S.N. and Blume, L.E., Eds. *The New Palgrave Dictionary of Economics*. New York: Palgrave Macmillan.
Gennaioli, N. & Voth, J. H. 2015. State capacity and military conflict. *Review of Economic Studies*, 82: 1409–1448.

Goldewijk, K. K., Beusen, A., Doelman, J., & Stehfest, E. 2017. Anthropogenic land use estimates for the Holocene–HYDE 3.2. *Earth System Science Data*, 9(1): 927–953.

Griffith, D. & Paelinck. J. 2011. *Non-standard spatial statistics and spatial econometrics*. Berlin: Springer Science & Business Media.

Griffiths, D. 2010. *Vikings of the Irish Sea*. Stroud: The History Press.

Griffiths, D., Bennett, R., & Haining, R. 1989. Statistical Analysis of Spatial Data in the Presence of Missing Observations: A Methodological Guide and an Application to Urban Census Data. *Environment and Planning A: Economy and Space*, 21(11): 1511–1523.

Idrobo, I., Mejía, D. & Tribin, A. M. 2014. Illegal Gold Mining and Violence in Colombia. *Peace Economics, Peace Science and Public Policy*, 20(1): 83–111.

Kelejian, H. & Prucha, I. 1998. A Generalized Spatial Two-Stage Least Squares Procedure for Estimating a Spatial Autoregressive Model with Autoregressive Disturbances. *Journal of Real Estate Finance and Economics*, 17(1): 99–121.

Kelly, M. 2019. The Standard Errors of Persistence. Available at SSRN: https://ssrn.com/abstract=3398303 or http://dx.doi.org/10.2139/ssrn.3398303 [Accessed 29 Jun. 2019].

Kelly, M., Mokyr, J. & Ó Gráda C. 2014. Precocious Albion: A New Interpretation of the British Industrial Revolution. *Annual Review of Economics*, 6(1): 363–389.

Keywood, T. & Baten, J. 2018. "Long Run Correlates of European Violence, A.D. 500–1900". Working Paper, Univ. Tübingen.

LeSage, J. & Pace, R. 2004. Models for Spatially Dependent Missing Data. *The Journal of Real Estate Finance and Economics*, 29(2): 233–254.

LeSage, J. & Pace, R. 2009. *Introduction to Spatial Econometrics*. Boca Raton: Chapman and Hall/CRC Press.

Maddison, A. 1998. Measuring the Performance of a Communist Command Economy: An Assessment of the CIA Estimates for the U.S.S.R. *Review of Income and Wealth*, 44(3): 307–323.

Manski, C. 1993. Identification of endogenous social effects: The reflection problem. *Review of Economic Studies*, 60(3): 531–542.

Mattern-Parkes, S. 2003. The Defeat of Crassus and the Just War. *The Classical World*, 96(4): 387–396.

McEvedy, C. & Jones, R. 1978. *Atlas of World Population History*. London: Penguin Books.

Micro-geographies, Conflict Diffusion, and Clusters of Violence. *Eurasian Geography and Economics*, 51(4): 437–471.

Mitton, T. 2016. The wealth of subnations: Geography, institutions, and within-country development. *Journal of Development Economics*, 118(1): 88–111.

Mokyr, J. 2005. Long-Term Economic Growth and the History of Technology. *Handbook of Economic Growth*, 1(1): 1113–80.

Morby, J. 1989. *Dynasties of the World*. Oxford: Oxford University Press.

Nunn, N. & Puga, D. 2012. Ruggedness: The Blessing of Bad Geography in Africa. The *Review of Economics and Statistics*, 91(4): 20–36.

Nunn, N. & Qian, N. 2011. The potato's contribution to population and urbanization: Evidence from a historical experiment. *The Quarterly Journal of Economics*, 126(2): 593–650.

Nüssli, C. 2010. *Euratlas: History of Europe*. Available: http://euratlas.com/ [Accessed 29 Nov. 2018].

O'Loughlin, J., Witmer, F., & Linke, A. 2010. The Afghanistan-Pakistan wars, 2008-2009: Micro-geographies, conflict diffusion, and clusters of violence. *Eurasian Geography and Economics*, 51(4): 437–471.

Paelinck, J. & Klaassen, L. 1979. *Spatial econometrics*. London: Gower Publishing.

Pérez-Artés, M. and Baten, J. 2018. "Land inequality and Numeracy in Early Modern Spain: The Case of Andalucía". Working Paper, Univ. Tübingen.
Pinker, S. 2011. *The Better Angels of our Nature: The Decline of Violence in History and its Causes*. London: Penguin Books UK.
Rasul, I. & Roger, D. 2015. The Impact of Ethnic Diversity in Bureaucracies: Evidence from the Nigerian Civil Service. *American Economic Review*, 105(5): 457–461.
Rubin, D. 1987. *Multiple Imputation for Nonresponse in Surveys*, New York: John Wiley & Sons, Inc.
Schultz, T. 1997. Testing the Neoclassical Model of Family Labor Supply and Fertility. *The Journal of Human Resources*, 25(4): 599–634.
Sinor, D. 1999. The Mongols in the West. *Journal of Asian History*, 33(1): 1–44.
Squicciarini, M. & Voigtländer, N. 2015. Human capital and industrialization: Evidence from the age of enlightenment. *The Quarterly Journal of Economics*, 130(4): 1825–1883.
Stein, M. 1999. *Interpolation of Spatial Data: Some Theory for Kriging*. Berlin: Springer Science & Business Media.
Victora, C., Adair, L., Fall, C., Hallal, P., Martorell, R. & Richter, L. 2008. Maternal and child undernutrition: consequences for adult health and human capital. *The Lancet*, 371(9609): 340–357.
Voigtländer, N., & Voth, H. J. 2013. How the West" Invented" fertility restriction. *American Economic Review*, 103(6): 2227–2264.
Winroth, A. 2014. *The Age of the Vikings*. Princeton: Princeton University Press.

Figures and Tables[14]

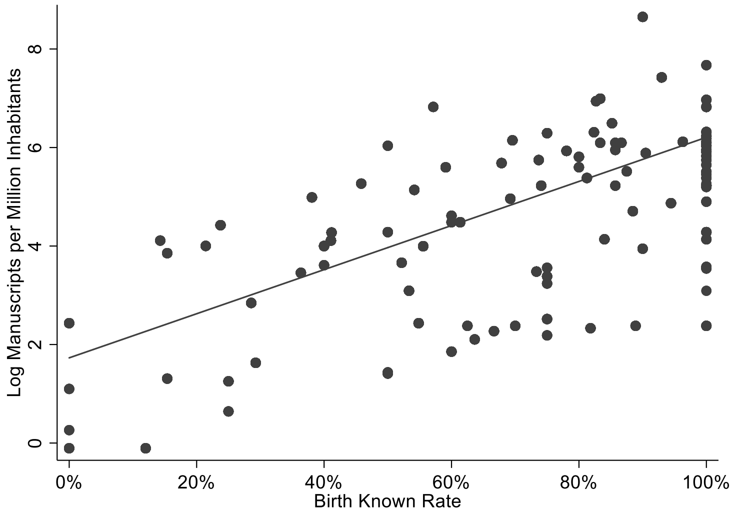


Figure 1: Manuscripts vs Birth Known Rate (11 European countries, 700 – 1500 CE)

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

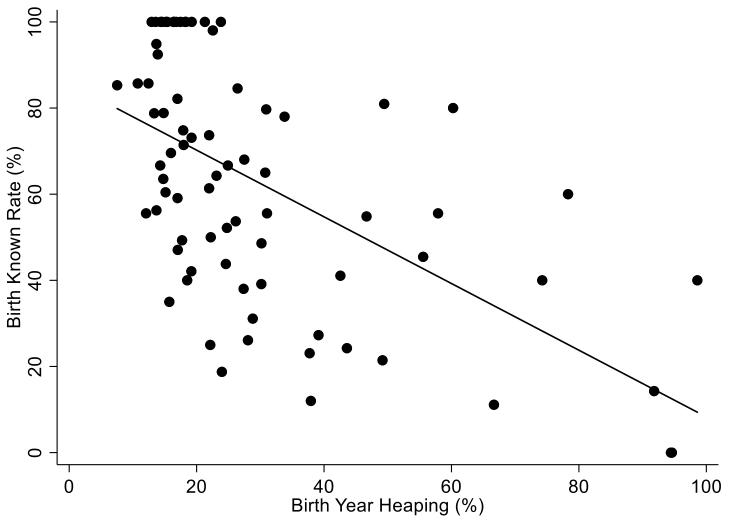


Figure 2: Birth Year Heaping vs Birth Known Rate (7 European Regions, 800 – 1800 CE)

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

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

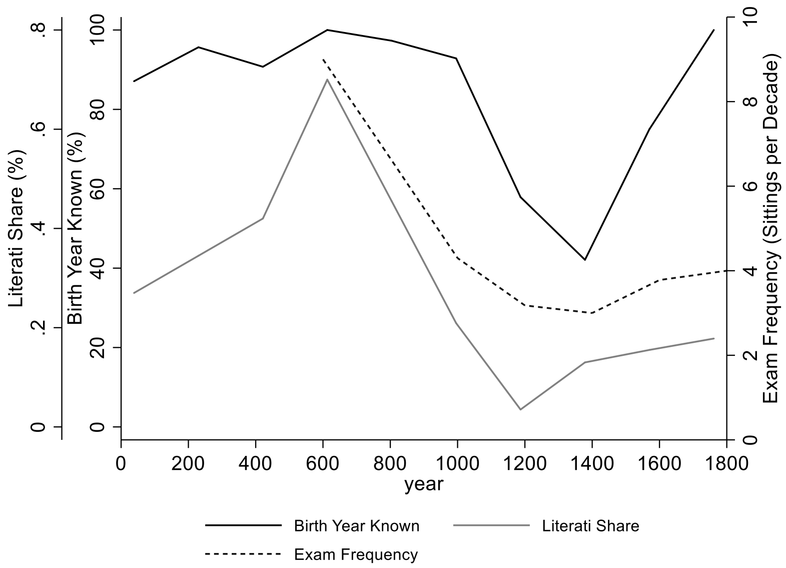


Figure 3: Elite Numeracy and the "Literati" (China, 0 – 1800 CE)

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 surprisingly well under this system during the medieval period (Baten 2016).

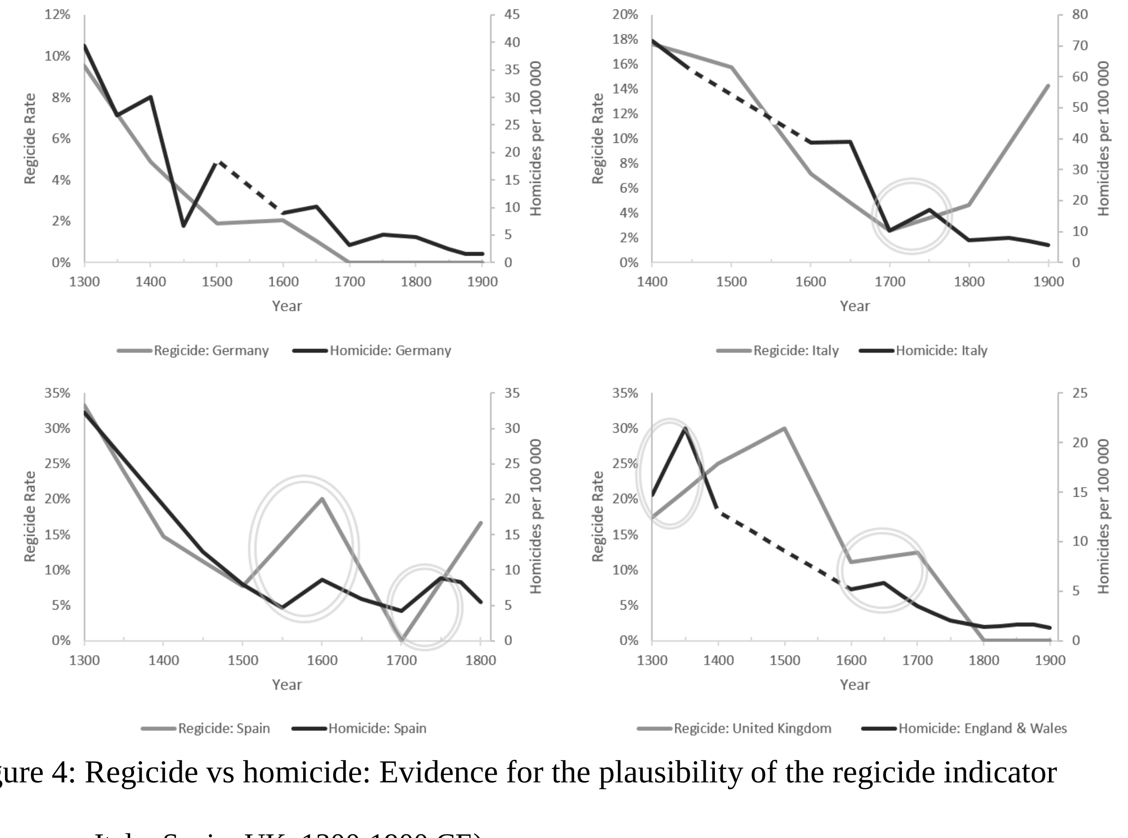


Figure 4: Regicide vs homicide: Evidence for the plausibility of the regicide indicator (Germany, Italy, Spain, UK, 1300-1900 CE)

Note: The figure shows decline 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).

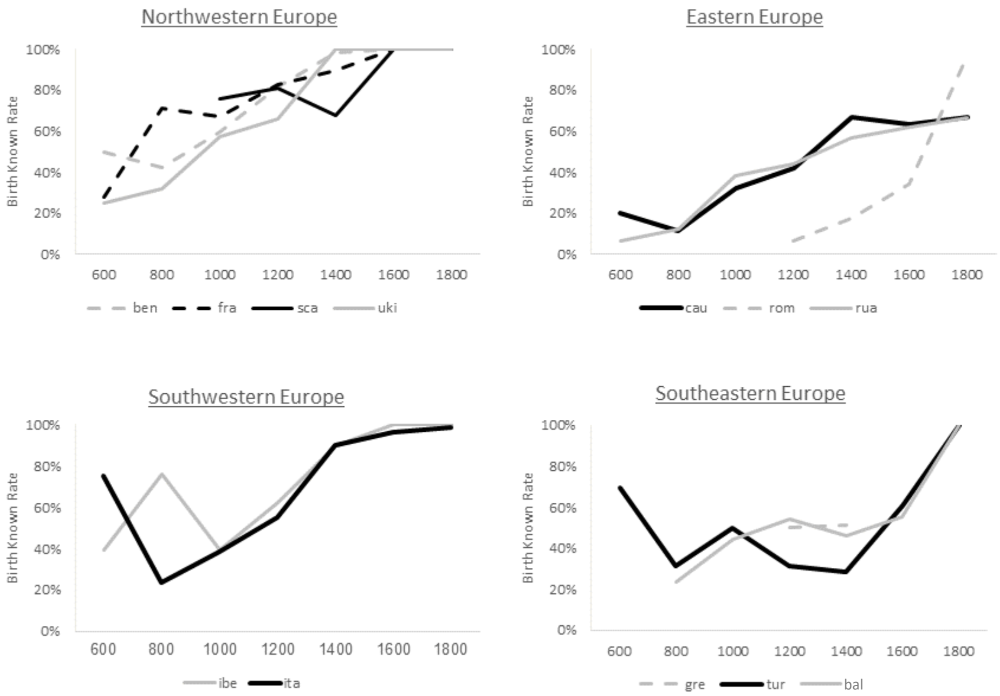


Figure 5: Sub-regional trends in elite numeracy

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).

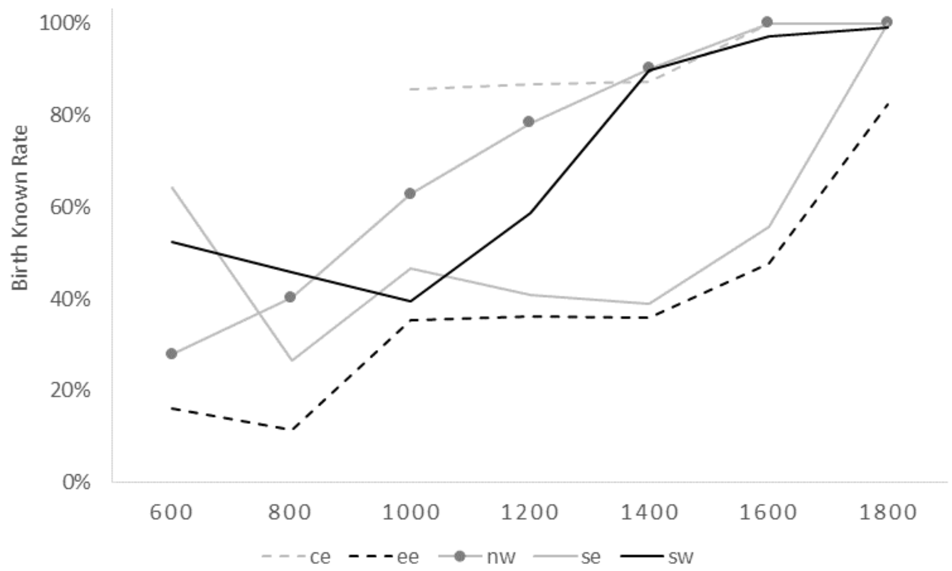


Figure 6: Inter-regional trends in elite numeracy

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

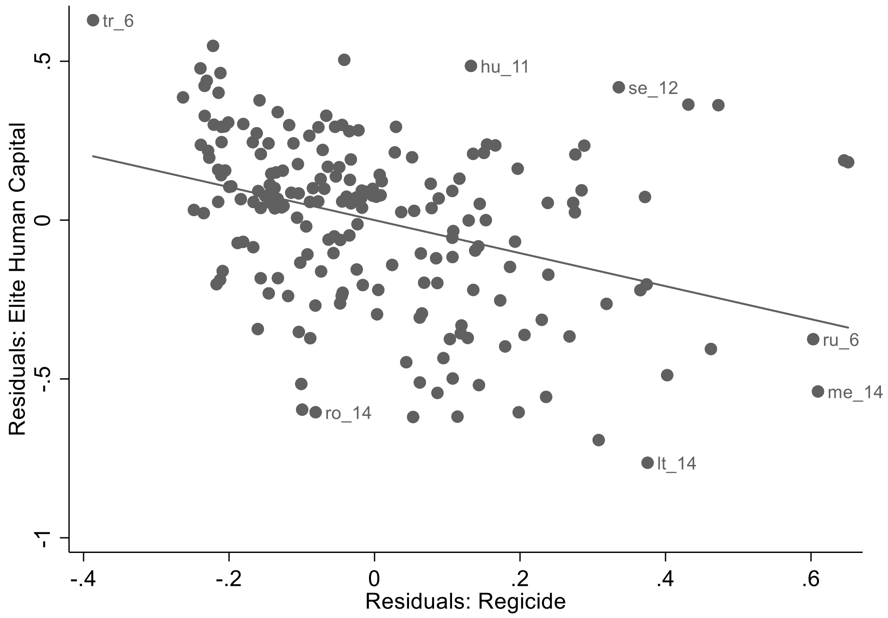


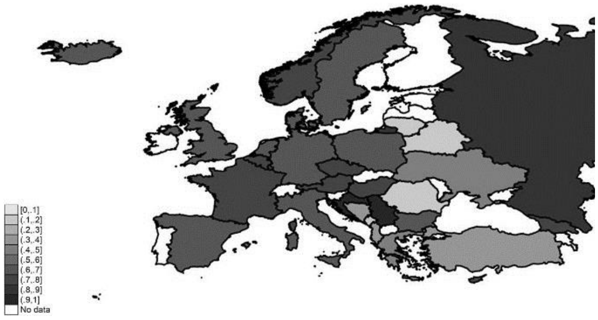
Figure 7: Residual scatterplot (all regressors and controls included)

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 8: Elite numeracy (500 – 1900)


a) 500 – 900


b) 1000 – 1300


c) 1300 – 1500

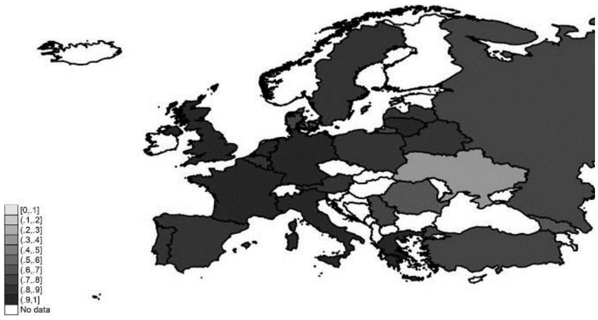

d) 1600 – 1900

Figure 9: Elite numeracy by period

Figure 10: Elite numeracy (1600 – 1900, 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 9, panel d for 1600-1900). This bin width adjustment merely allows for a clearer distinction between countries.

Panel A Panel B

Figure 11: 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.

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 1a: 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 1b: 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 2: Regressions of elite numeracy on potential biasing factors

	(1) Birth Known	(2) Birth Known	(3) Birth Known	(4) Birth Known	(5) Birth Known	(6) Birth Known	(7) Birth Known
Regicide	**-0.416***** (0.139)	**-0.429***** (0.143)	**-0.427***** (0.141)	**-0.436***** (0.152)	**-0.436***** (0.152)	**-0.509***** (0.138)	**-0.474***** (0.125)
Battle	**-0.686***** (0.214)	**-0.698***** (0.218)	**-0.703***** (0.216)	**-0.694***** (0.249)	**-0.686***** (0.247)	**-0.665**** (0.244)	**-0.661**** (0.254)
Urbanisation		-0.216 (0.203)	-0.217 (0.203)	-0.206 (0.220)	-0.197 (0.220)	-0.227 (0.214)	-0.174 (0.178)
Mode of Succession (Base=Hereditary):							
● Partially Elected			-0.0833 (0.0726)	-0.0251 (0.0669)	-0.0269 (0.0666)	-0.0337 (0.0760)	0.00838 (0.0897)
● Fully Elected			0.0247 (0.0875)	0.00864 (0.0899)	0.00866 (0.0897)	-0.0122 (0.0837)	-0.0168 (0.0849)
Pasture Area				0.0151 (0.0105)		**0.342***** (0.0789)	**0.338***** (0.0787)
Crop Area					0.00769 (0.00824)	**-0.362***** (0.0813)	**-0.363***** (0.0831)
Second Serfdom	-0.0277 (0.0759)	-0.0431 (0.0790)	-0.0431 (0.0792)	-0.0364 (0.0853)	-0.0334 (0.0859)	-0.0775 (0.0834)	-0.0620 (0.0841)
Constant	**0.562***** (0.157)	**0.574***** (0.161)	**0.568***** (0.170)	**0.598***** (0.199)	**0.595***** (0.199)	**0.643***** (0.193)	**0.549***** (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 3: 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 3 and regicide and battle always remain 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).

Birth Known	(1) sar	(2) sem	(3) sar	(4) sem	(5) sar	(6) sem	(7) sar	(8) sem	(9) sar	(10) sem	(11) sar	(12) sem	(13) sar	(14) sem	(15) sar	(16) sem	(17) sar	(18) 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 4.1: 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.

Birth Known	(1) slx	(2) Θ	(3) slx	(4) Θ	(5) slx	(6) Θ	(7) slx	(8) Θ	(9) slx	(10) Θ	(11) slx	(12) Θ	(13) slx	(14) Θ	(15) slx	(16) Θ	(17) slx	(18) 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 4.2: 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.

	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 5: Instrumental variable regressions of elite numeracy[15]:

Was there a difference between the two invasion periods (800 – 1000 and 1200 – 1400) and the peaceful period of the High Middle Ages?

[15] See appendix (tables 15 and 16) for first stage regressions.

	(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 6: Instrumental variable regressions (invasions from Central Asia: 800 – 1400 CE)[16]

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

[16] 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 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.

Appendix

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 think that that assigning them to Scandinavia is a good 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	Regional Abbreviation	Region	Countries
Central Europe	deu	German speaking	Austria, Germany
	ece	East-Central Europe	Czech Republic, Hungary, Poland
Eastern Europe	cau	Caucasus	Armenia, Georgia
	rom	Romania	Romania
	rua	Russia	Belarus, Russia, Ukraine
North-Western Europe	ben	Benelux	Belgium, Luxembourg, Netherlands
	fra	France	France, Monaco
	sca	Scandinavia	Denmark, Iceland, Lithuania, Latvia, Norway, Sweden
	uki	United Kingdom and Ireland	Ireland , United Kingdom
South-Eastern Europe	gre	Greece	Cyprus, Greece
	tur*	Turkey	Turkey
	bal	Balkans	Albania, Bosnia and Herzegovina, Bulgaria, Croatia, Montenegro, Serbia
South-Western Europe	ibe	Iberia	Portugal, Spain
	ita	Italy	Italy

Table 8: Aggregation of European countries to broader regions

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

Unit Root Tests

Although all of our regression specifications include time fixed effects, any presence of non-stationary series may mean that our regressions could capture spurious relationships and invalidate our inference. 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 9 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 9: Unit root tests

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 10.1: Spatial regression without interpolation (Cross section: 1300)

Note: Although the regicide coefficients in the first few 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 10.2: Spatial regression without interpolation (Cross section: 1400)

ΔBirth Known	(1) sar	(2) sem	(3) sar	(4) sem	(5) sar	(6) sem	(7) sar	(8) sem	(9) sar	(10) sem	(11) sar	(12) sem	(13) sar	(14) sem	(15) sar	(16) sem	(17) sar	(18) 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 11.1: Spatial fixed effects regressions 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.

ΔBirth Known	(1) slx	(2) Θ	(3) slx	(4) Θ	(5) slx	(6) Θ	(7) slx	(8) Θ	(9) slx	(10) Θ	(11) slx	(12) Θ	(13) slx	(14) Θ	(15) slx	(16) Θ	(17) slx	(18) 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 11.2: Spatial fixed effects regressions 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.

Using Predicted Values

To test whether collinearity between our elite control variables 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 elite control variables 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*** **	**-0.396*** **	**-0.397*** **	**-0.388*** **	**-0.389*** **	**-0.452*** **
	(0.127)	(0.129)	(0.130)	(0.137)	(0.138)	(0.125)
Battle	**-0.690*** **	**-0.700*** **	**-0.698*** **	**-0.711*** **	**-0.704** **	**-0.686** **
	(0.219)	(0.221)	(0.221)	(0.259)	(0.257)	(0.255)
Urbanisation		-0.189	-0.190	-0.177	-0.172	-0.202
		(0.180)	(0.181)	(0.185)	(0.185)	(0.186)
Mode of Succession (Base=Hereditary)						
● Partially Elected			-0.0246	0.0279	0.0276	0.0161
			(0.0878)	(0.0954)	(0.0953)	(0.0972)
● Fully Elected			0.00681	-0.0586	-0.0601	-0.0114
			(0.0821)	(0.0826)	(0.0825)	(0.0855)
Pasture Area				0.0123		**0.328*** **
				(0.00823)		(0.0746)
Crop Area					0.00523	**-0.350*** **
					(0.00588)	(0.0789)
Second Serfdom	-0.0134	-0.0253	-0.0261	-0.0107	-0.00908	-0.0484
	(0.0753)	(0.0767)	(0.0770)	(0.0814)	(0.0818)	(0.0803)
Constant	**0.208***	**0.214***	**0.214***	0.197	0.198	0.226
	(0.115)	(0.116)	(0.124)	(0.139)	(0.139)	(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 12: Fixed effects regressions with predicted values

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 8. 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 13: Regional fixed effects regressions

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 already introduced 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 14: 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 very 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.

Instrumental Variable Regressions

Tables 15 and 16 show the first stage regressions to the IV regressions from tables 5 and 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***** (0.0364)	-0.0507 (0.0349)	**-0.0535*** (0.0306)
Constant	**0.920***** (0.218)	**0.483**** (0.201)	**0.489***** (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

Table 15: First Stage IV Regressions to:

Table 4: Instrumental variable regressions of elite numeracy: Was there a difference between the two invasion periods (800 – 1000 and 1200 – 1400) and the peaceful period of the High Middle Ages?

	(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 16: First Stage IV Regressions to:

Table 5: Instrumental variable regressions (Central Asian Invasions: 800 – 1400 CE)

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 their 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 16th until the 18th 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 & 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 & 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 fertile soil data come from Nunn and Puga (2012).