State Capacity and Public Goods: Institutional Change, Human Capital, and Growth in Early Modern Germany*

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Abstract

What are the origins and consequences of the state as a provider of public goods? We study institutional changes that increased state capacity and public goods provision in German cities during the 1500s, including the establishment of mass public education. We document that cities that institutionalized public goods provision in the 1500s subsequently began to differentially produce and attract upper tail human capital and grew to be significantly larger in the long-run. Institutional change occurred where ideological competition introduced by the Protestant Reformation interacted with local politics. We study plague outbreaks that shifted local politics in a narrow time period as a source of exogenous variation in institutions, and find support for a causal interpretation of the relationship between institutional change, human capital, and growth.

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1 Introduction

What are the origins and impacts of the state as a provider of public goods? Prior to 1500, European states strongly resembled stationary bandits extracting resources to support private goods. In the 1500s, new institutions significantly expanded the administrative state capacities and social welfare bureaucracies of European cities, and established Europe's first large scale experiments with mass public education. These institutional innovations were codified in law by German cities. The laws were passed at the municipal level during the Protestant Reformation. During the Reformation remarkable institutional variation emerged at the local level, including across neighboring Protestant cities.

The economics literature has studied the origins of the state as a rent-seeking organization and the implications of state capacity that emerged for military reasons in European history (Besley and Persson 2009; Gennaioli and Voth 2015; Sanchez de la Sierra 2015; Mayshar et al. 2015). Related research has documented how institutional limits on the prerogatives of rulers and the scope of state power have secured property rights as a fundamental public good (Acemoglu, Johnson, and Robinson 2005a; 2001; North and Weingast 1989). The expansion of state capacity that we study represented a different type of institutional change. Institutional change in German cities in the 1500s increased economic inclusion by expanding public service provision, varied at the local level, and was advocated by popular anti-corruption movements (Witte 2002; Grell 2002; Cameron 1991; Hamm 1994; Ozment 1975; Sehling 1902-2013). A key objective of institutional change was the formation of upper tail human capital for a service elite to administer public goods provision (Strauss 1978; 1988). These features make the institutional changes of the 1500s a unique source of evidence on, and canonical examples of, state institutions supporting public goods. While historical evidence suggests that the institutional changes observed in German cities had profound consequences for education and urban life, prior economic research has not studied these institutionalized expansions in state capacity.[1]

In this research, we document the causal impact of institutional changes in state capacity on upper tail human capital and city growth, using historical evidence from German

[1]Prior research has only studied the relationship between the non-institutionalized diffusion of Protestantism and outcomes (Becker and Woessmann 2009; Cantoni 2015; Becker, Pfaff, and Rubin 2015), as discussed below.

cities. This paper presents the first research to document the causal impact of institutions supporting local public goods on outcomes in targeted municipalities, to the best of our knowledge.[2] We study novel microdata on the formation, migration, and sectoral allocation of upper tail human capital before and after the institutional changes of the 1500s. We use a difference-in-differences strategy to document the large positive impact of the new institutions on human capital, and show that there was no difference in human capital trends between treated and untreated cities before the institutional changes of the 1500s. We then study the causal impact of institutional change on long-run outcomes using an instrumental variable (IV) strategy. We use the timing of plague outbreaks in the revolutionary period of the early 1500s as an IV to isolate exogenous variation in institutional change, and find that the institutional changes drove significant increases in long-run city population growth.

Results. — We first study shifts in the migration and formation of upper tail human capital across cities following institutional change in the 1500s.[3] We measure institutional change by the presence of city-level Reformation laws, which were passed starting in the 1520s and adopted in only 55 percent of Protestant cities. To test the impact of these laws, we assemble microdata on upper tail human capital from the *Deutsche Biographie*, which is the definitive biographical dictionary of economic, cultural, and political figures in German history (Hockerts 2008). We use the data to measure the local formation, migration, and sectoral allocation of upper tail human capital across German cities between 1320 to 1820.[4]

We use a difference-in-differences identification strategy to document the causal impact of institutional change supporting public goods provision on human capital. We find no underlying differences in human capital trends for cities that did and did not adopt before 1520. We find a sharp and persistent shift in the migration of upper tail human capital towards cities that adopted public goods institutions after 1520. We also observe a differential positive trend in the local formation of upper tail human capital in cities that institutionalized public goods provision in the 1500s. The observed human capital effects persist through later

[2] Acemoglu, García-Jimeno, and Robinson (2015) study the *spillover* impacts of state capacity on outcomes across localities in contemporary Colombia. In contrast, we study the impact of the public goods institutions on human capital and growth in directly targeted municipalities.

[3] Data on literacy in Germany is first observed systematically in the mid-1800s at the county level.

[4] The *Deutsche Biographie* was designed to provide universal coverage across regions and religious groups (Bayerischen Akademie der Wissenschaften 2015). We show that our results are not driven by selective inclusion of marginal figures by restricting our sample to the super-stars *within* the *Deutsche Biographie* for whom selective inclusion is not plausible and show our baseline results hold.

shocks such as the Thirty Years War (1618-1648).

To shed light on the precise impact of institutional change, we study how institutions shifted the sectoral allocation of upper tail human capital. We classify the occupations of all individuals in the *Deutsche Biographie* and study six high-level sectors: government, church, education, business, arts, and medicine. We find that the largest and most significant shifts in migration towards cities that adopted public goods institutions in the 1500s were in the targeted sectors: government, church, and education. In the 1600s and 1700s, these cities also began producing more locally born human capital elites active in business and the arts. We also study the subset of the most prominent individuals ("super-stars") for whom potential selection into the *Deutsche Biographie* is not salient. We find consistent results, but also more immediate positive effects on the business sector for super-stars.

We then study long-run city sizes and human capital outcomes. We show that cities that adopted public goods institutions grew to become significantly larger and more human capital intensive by 1800.[5] To identify the long-run impact of public goods institutions on city sizes and human capital intensity, we use plague outbreaks in a narrow period in the early 1500s as an instrumental variable (IV) for institutional change. We use the quasi-experimental short-run variation in plague, which shifted local politics during the critical juncture of the early 1500s, and control for long-run plague prevalence and trends, which could reflect underlying differences in economic activity and locations. We find institutional change drove significant differences in long-run population and human capital intensity. Supporting the exclusion restriction for the IV strategy, we show that while plagues in the early 1500s and in other periods had similar direct demographic effects, only plagues in the early 1500s were associated with long-run city growth. We also document that plague outbreaks were highly localized, that neighboring cities did not experience shocks at the same time, and that there were no trends in plagues overall or towards cities with geographic or trade network advantages.

The introduction of ideological competition in the 1500s explains why the plague became salient as an institutional shifter. Before the Reformation, the Catholic Church enjoyed a monopoly in religion and local public goods provision was limited. The Reformation

[5] Around 1800, further institutional changes and educational reforms impacted economic development in German cities (Strauss 1978; Acemoglu et al. 2011). Due to missing city population data in periods before the Reformation, we are unable to study city populations using panel methods, as discussed below.

introduced religious competition into Western Europe, and was animated by ethical ideas about the common good, public provision, and elite corruption (Dittmar and Seabold 2015; Whaley 2012; Brady 2009). In German cities, institutional changes at the municipal level were driven by the interaction between these ideas and local politics (Cameron 1991; Scribner 1979). The plague shifted local politics towards public provision by threatening civic order, discrediting elites, and altering the composition of city populations (Dinges 1995; Isenmann 2012). Experience with the plague highlighted the differences between Protestant and Catholic ideas in the market for religion on the subject of public goods provision and the advantages of institutionalizing public goods. Catholics suggested that epidemic disease was divine punishment for sin and favored limited public provision. In contrast, Protestant Reformation laws formalized an agenda for a "Christian Commonwealth" in which institutions to promote public health interlock with other public goods (Rittgers 2012; Lindemann 2010; Roeck 1999; Grell 2002; Cameron 1991).

Placing Our Results in Context. — Our paper relates to several literatures. The existing economics literature has studied the military origins of the state and the role of the state as a rent-extracting institution (Besley and Persson 2011; Dincecco and Prado 2012; Gennaioli and Voth 2015; Sanchez de la Sierra 2015; Mayshar et al. 2015). We study the *popular* origins of variations in state capacity at the local level, and document the direct impact of local state capacity on upper tail human capital and growth.

We also contribute to the literature on institutions and growth. Prior research has found that institutions that constrain arbitrary executive authority and protect property rights explain development (Acemoglu, Johnson, and Robinson 2005a; 2001; North and Weingast 1989). We study the growth impact of inclusive institutions that expanded access to improved public goods and supported human capital formation.

The role of human capital in fostering development is subject to debate (Gennaioli et al. 2013; Acemoglu, Gallego, and Robinson 2014; Glaeser et al. 2004). In our setting, institutional innovations targeted education and were designed to produce an upper tail human capital administrative elite. As Strauss (1988; p. 203) observes, "Preparing pupils for high office was always the salient objective." Existing research has documented the relationship between upper tail scientific elites (Squicciarini and Voigtländer 2015) and skilled craftsmen (Mokyr 2009; Meisenzahl and Mokyr 2012) and economic activity during

the industrial revolution, but does not identify the origins of upper tail human capital. Related research documents the relationship between universities and the development of formal market institutions in the middle ages (Cantoni and Yuchtman 2014). We use micro-data and show that institutional change first led to increases in upper tail human capital in occupations that enhanced state capacity and the provision of public goods, and later and more gradually to increases in business and the arts.[6] We further document large effects of human capital supporting institutions on long-run city growth.

Our paper also contributes to the literature on the economics of religion. Starting with Weber (1904), researchers have studied the economic effects of Protestantism. Cantoni (2015) finds that the non-institutional diffusion of Protestantism had no effect on city population growth. Becker and Woessmann (2009) argue that Protestantism led to higher growth across Prussian counties via human capital effects that became salient in the 19th century. In constrast, we find that underlying institutions, and not non-institutionalized religion, drove human capital accumulation and growth *before* the Industrial Revolution. These institutions increased state capacity and embodied religious ideas. Prior economics research has not highlighted the role of religion in the development of state capacity.[7]

Another related literature we contribute to studies how political competition shapes institutions and the provision of state services. Existing research documents the impact of political competition in democracies (Fujiwara 2015; Acemoglu et al. 2014; Besley, Persson, and Sturm 2010). We study a *non-democratic* setting in which political competition combined with citizen action to shape fundamental changes in public goods institutions.

Finally, our study relates to the literature on bourgeois revolutions. A large literature following Marx has framed the institutional changes of the Reformation as an "early bourgeois revolution," but has devoted limited attention to the expansion of public goods (Brady 2009; Dorpalen 1985). Our evidence suggests that changes in public goods provision were highly consequential. The local heterogeneity of the institutional changes in Germany in the 1500s, the emphasis on education, and the centrality of religion distinguish these events from the later classic bourgeois revolutions – England in 1688 and France in 1789.

[6] In related research, Rauch and Evans (2000) find that meritocratic recruitment of government bureaucrats lowers country risk in contemporary settings.

[7] The role of religion in the development of state capacity is documented in an extensive historical literature (Whaley 2012; Brady 2009; Lindemann 2010; Roeck 1999; Gorski 2003).

2 Institutional Change During the Reformation

The Protestant Reformation involved the diffusion of political ideas and institutions, not just new religious beliefs. We study the impact of new institutions that supported public goods provision. These institutions were codified at the city-level in municipal law, but only in half of the cities that adopted Protestantism as their dominant religion.

What factors influenced why some cities adopted the institutions of the Reformation and others did not? We draw on a rich body of historical evidence to characterize the Reformation movement and the political economy processes that led to institutionalization or non-institutionalization, including how the plague operated as an institutional shifter.

2.1 Diffusion of the Reformation and Institutional Change

The Protestant Reformation began as a movement of churchmen calling for the reform of practices and institutions within the Catholic Church and became a broad social movement for religious and social reform (Cameron 1991). Within months of the initial circulation of Martin Luther's famous theses in 1517, Reformation ideas swept across Germany.

The Reformation involved movements for religious renewal and for institutional change. Reformers called for religious renewal within cities, argued that biblical authority was paramount over and above the authority of existing Catholic Church institutions, and were frequently anti-clerical (Moeller 1972; Brady 2009; Dykema and Oberman 1993). These religious ideas were sometimes accompanied by formal institutional change. Some but not all Protestant cities adopted new institutions that set up safeguards against church corruption and promoted economic inclusion – by extending public goods provision.[8]

The adoption of Reformation institutions reflected city politics. Institutional change at the city-level was driven by citizens' movements that emerged without initial support from oligarchic city governments or territorial lords.[9] Cameron (1991; p. 240) observes, "As a rule neither the city patricians nor the local princes showed any sympathy for the Reformation in the crucial period in the late 1520s and early 1530s; they identified themselves with the old Church hierarchy... Popular agitation on a broad social base led to the formation

[8]The reformists moved to eliminate clerical tax exemptions and economic privileges, and frequently raised objections to high prices for essential religious services (Cameron 1991; Ozment 1975).

[9]See Dittmar and Seabold (2015). We discuss princes' preferences and city elites below and in Appendix E.

of a 'burgher committee'." Dickens (1979) confirms that city councils did not advocate for institutional change. The constituency for institutional change came from citizens who were excluded from political power by oligarchic elites, typically lesser merchants and guild members (Ozment 1975; Schilling 1983). Territorial princes did exert some influence over the process of institutional change. In our empirical work we focus on variation in institutions and outcomes across cities in the same territory (Sections 5 and 6).

The popular origins of institutional change can be illustrated with a few examples. In Augsburg, the city council was forced to drop its policy of religious neutrality following riots in 1524, 1530, and 1534 that culminated in legal change (Broadhead 1979). In Northern cities, such as Rostock, Stralsund, Greifswald, Lübeck, Braunschweig, and Hanover institutional change led by citizens excluded from political power had a *coup d'état* quality (Cameron 1991). In Zwickau, Lutheran publications were printed in 1523; the city council unsuccessfully attempted to suppress protests in 1524; the Reformation was adopted in law in 1529 (Scribner 1979). Further discussion is provided in Appendix E.

A key reason why institutional change was not adopted by all Protestant cities is that institutional change was driven by the popular mobilization not local elites. Popular mobilization for institutional change reflected slow-moving city characteristics and contingent short-term events.[10] The determinants of popular mobilization could reflect city characteristics that had direct implications for economic outcomes, raising questions about endogeneity.

Plague outbreaks in the early 1500s shocked local politics at a critical juncture. Plague outbreaks led to the breakdown of civic order, discredited city elites, and changed the composition of the population. Experience with plague also shifted the salience of public goods institutions. Plagues in the early 1500s shifted local politics at a juncture characterized by the introduction of political competition. The probability of institutional change increased for cities exposed to plagues in the early 1500s. We provide detailed discussion of these dynamics in Section 6.[11]

[10]Popular mobilization reflected: (1) the size and self-organization pro-Reform constituencies, including guilds and merchants not on the city council, (2) the nature and extent of local Catholic Church corruption and monopolies, and (3) the persistent nature of local culture (Ozment 1975; Cameron 1991; Voigtländer and Voth 2012).

[11]These variations in demand for institutional change are orthogonal to variations in the supply of Protestant ideas. Historians (Eisenstein 1980; Brady 2009) and economists (Rubin 2014) argue that

2.2 The Municipal Institutions of the Reformation

Protestant reformers designed interlocking institutions with a legal foundation. The objective was to formalize and reinforce the new system of beliefs and to transform the provision of religious and social services. The key institutional innovations were city-level laws that transfered control of service provision from the Catholic Church to the temporal rulers, established binding guidelines for a new society, and initiated fixed investment commitments (Strauss 1978).[12] These laws were called church ordinances (*Kirchenordnungen*). We refer to them as "Reformation laws" or ordinances.

The new institutions supporting public goods had multiple provisions. These provisions cover: (1) the conduct of mass; (2) the provision of and rules governing public education; (3) the provision of health care, including the establishment, staffing, funding, and eligibility for treatment at hospitals; (4) the expansion of social insurance and transfers such as poor relief; (5) the regulation public life and behavior; and (6) compensation and quality control of priests and teachers. In general, the laws institutionalized the redistribution resources towards lower income families, and ensured a measure of equal opportunity by providing public assistance for education. For example, in the law for the city of Braunschweig, Johannes Bugenhagen wrote that it was disgraceful that the poor could not afford the services of professional midwives – and that access to these services must be provided for all (Bugenhagen 1885; p. 31).[13] An innovation that expanded state capacity and secured access to services was the introduction of a "common chest," an audited lock-box for funds used to support poor relief, medical care, and education.

The education provisions are of special interest for economists interested in human capital and development. These provisions established compulsory public schooling and aimed to make the Reformation irreversible – by producing a human capital elite to

the printing press shifted the supply of Reformist ideas. Recent research argues that the diffusion of Protestantism was driven by competition in the use of printing technology (Dittmar and Seabold 2015). Our research is fundamentally differentiated from this work in that it studies a larger set of cities, including more cities without printing, and examines shocks that were orthogonal to the supply-side shocks the research on printing has examined. Every printer death documented in Dittmar and Seabold (2015) occurred outside of plague outbreaks studied here. Similarly, we control for distance from Wittenberg, which Becker and Woessmann (2009) identify as a determinant of the diffusion of Protestant ideas.

[12] For discussion on how the Reformation impacted the law and legal institutions, see Witte (2002).

[13] Bugenhagen also wrote the laws for Lübeck and Hamburg, consulted widely, was Martin Luther's confessor, and was on the team with Luther that translated the Bible into German.

staff expanding Protestant church and state bureaucracies and by producing disciplined Protestant subjects.[14] Institutional changes were associated with subsequent differences in provision, including in investments in school construction as we document in Appendix A.

While we highlight the importance of legal interventions in education, the consequences of Reformation laws arguably flowed from the interlocking nature of these institutional innovations. For example, the city of Wittenberg adopted a Reformation law in 1522. This law established a common chest and stipulated that all church income was to be collected under one administration, and that these resources were to be used to pay for care for the poor and sick and to provide financial support to low-income parents so they could afford to send their children to school or university, among other uses (Sehling 1902-2013).

2.3 Measuring Institutional Change

Our measure of treatment is the formalization of public goods provision in law. Cities that adopted the legal institutions of the Reformation and remained Protestant are considered "treated." Cities where these legal institutions were adopted and persisted despite later re-Catholicization are also considered as treated. Cities that remained Catholic or that became Protestant without legal institutions are "untreated." A small number of cities where Protestant institutions were eliminated after a few years are considered untreated in our baseline analysis, which considers cities with institutions that survived to 1600 as treated. However, we obtain virtually identical results when we include as treated the few cities where Protestant laws were set up but rolled back in the early 1500s.[15]

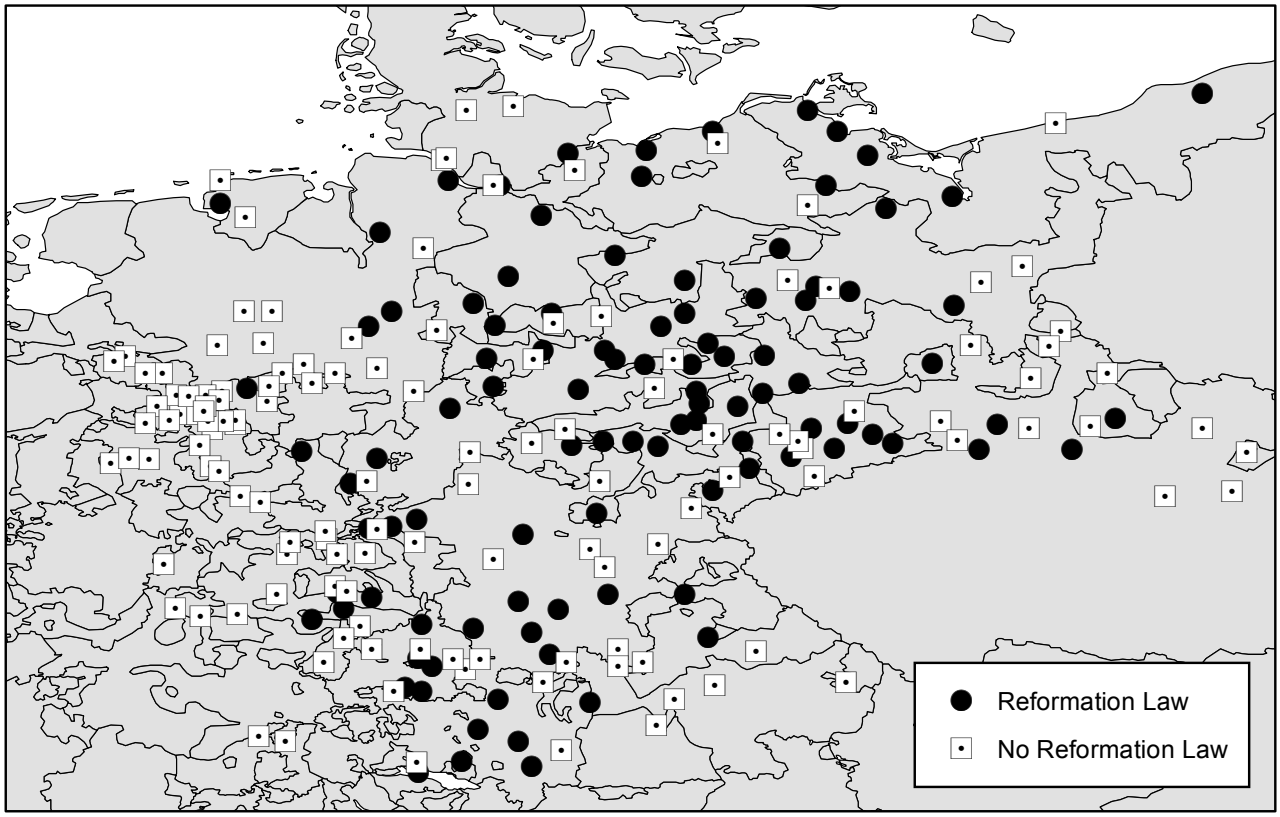
Figure 1 maps the cities in our data and illustrates the local variation in which cities had Reformation laws. Figure 2 shows the cumulative share of cities with Reformation laws in each year. Most cities passed their first law by 1545. In 1546, the Schmalkaldic War broke out between Protestant and Catholic princes, largely arresting city-level diffusion. The Augsburg Settlement (1555) ended hostilities and established a new institutional equilibrium.[16]

[14] Most school curricula do not mention Bible reading (Strauss 1978). We provide information on school hours, the short length of vacations, and the fact that city schools were free for poor children in Appendix A.

[15] In Münster and Beckum institutional change was reversed after a few years (by the mid-1530s).

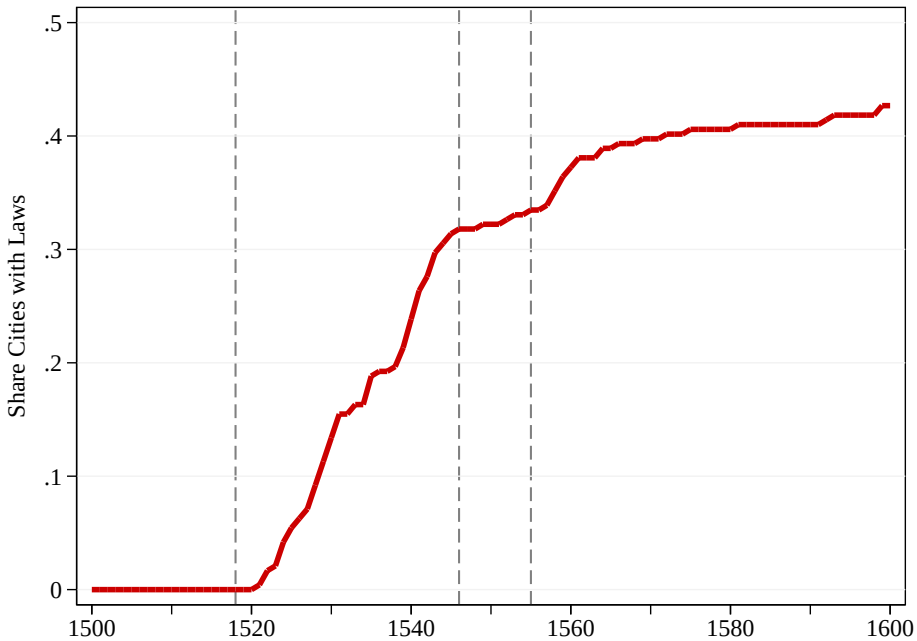
[16] The settlement included a provision, *cuius regio, eius religio,* which allowed local rulers to dictate the religion in their realm, but maintained a complicated set of exceptions for cities where magistracies and offices were to be shared and largely respected facts on the ground (Dittmar and Seabold 2015).

Figure 1: Cities With and Without Reformation Laws



This map shows cities with Reformation Laws (black circles) and without these laws (white squares).

Figure 2: The Share of Cities with Reformation Laws



This graph shows the share of cities with a Reformation Law. Vertical lines mark the mass circulation of Luther's ideas in 1518, the Schmalkaldic War of 1546, and the Peace of Augsburg in 1555.

We provide discussion of the institutions and our classification in Appendix A and illustrative examples here. Bautzen is an example of a Protestant city which did not adopt institutional change. In Bautzen, the Catholic Bishop and Protestants reached a legal compromise and institutional change was arrested (Speer 2014). Augsburg and Amberg are examples of cities where the institutions of the Reformation were established and persisted despite forms of re-Catholicization. Augsburg adopted the institutions of the Reformation 1534-1537, but was assigned a Catholic city council by the emperor in 1548. The council did not attempt to re-Catholicize the population and access to city services remained open to Protestants (Stein 2009). Amberg passed a Reformation law in the 1540s, but was absorbed into Catholic Bavaria in the early 1600s. The Bavarian authorities explicitly worked to preserve the educational infrastructure they inherited in Amberg (Johnson 2009).

While there were some *territorial* Catholic interventions in the counter-reformation that adopted innovations from the Protestant agenda (Strauss 1978), the consensus among historians is that policy ordinances developed "much more clearly and earlier in Protestant than in Catholic Germany" (Roeck 1999; p. 282) and that the presence of Catholic interventions that borrowed from and responded to Protestant innovations will lead us to conservatively underestimate the impact of Protestant institutional change (Grell 2002).

3 Data

Definition of Sample – We focus on institutions and outcomes in 239 cities in Germany with population observed in 1800 in Bairoch, Batou, and Chèvre (1988) and information on the non-institutional diffusion of Protestantism recorded in Cantoni (2012).[17]

Legal institutions of the Reformation – Our principal data source on Protestant church ordinances is the 21 volume collection *Die evangelischen Kirchenordnungen des XVI. Jahrhunderts* (Sehling 1902-2013).[18] We review the text of the laws and manually code which cities adopted institutional change.

Upper Tail Human Capital – Data on individuals with upper tail human capital are from the *Deutsche Biographie* (Bayerischen Akademie der Wissenschaften 2015). The

[17] We do not study ordinances adopted in castles and religious establishments. We emphasize within-territory variation and defer analysis of territorial laws. We restrict to cities in contemporary Germany.

[18] Appendix A provides a complete list of volumes and a description of these and other sources.

Deutsche Biographie is a project of the Historical Commission of the Bavarian Academy of Sciences (Reinert et al. 2015), provides the most definitive record of upper tail human capital individuals in German history, and was designed to provide comprehensive coverage across regions and religions (Hockerts 2008). We identify over 8,000 individuals born in or migrating to our baseline set of cities from 1300 to 1820. We classify individual occupations in six principal sectors: (1) *government*; (2) *church*; (3) *education*; (4) *business*; (5) *arts*; and (6) *medicine*.[19] We provide detailed discussion of the nature and construction of the *Deutsche Biographie*, and our classification of occupations, in Appendix A.[20]

City Populations – City population data are from Bairoch, Batou, and Chèvre (1988), who record populations for urban agglomerations that ever reached 5,000 inhabitants between 1000 and 1800 at 100 year intervals. A number of cities in the Bairoch data have no recorded observation for population in 1500. In Appendix A we collect evidence on each such city from the *Deutsche Städtebuch* to document when city size first appears in the historical record.

Plague Outbreaks – We construct city-year level data on plague outbreaks from Biraben (1975), which provides quantitative data designed to characterize the frequency, duration, and variations in incidence of the plague in European history. Biraben (1975) collects evidence on the presence of major outbreaks (1/0), motivated by the fact that outbreaks were public events that left a mark in the historical record and because the evidence on mortality embodies measurement error and is not available for a large proportion of outbreaks.

City Level Characteristics – Data on books printed in each city pre-Reformation are from Dittmar and Seabold (2015). Data on the hometowns of students receiving university degrees from 1398 to 1517 are from Cantoni, Dittmar, and Yuchtman (2015).[21] Data on market rights and city incorporation are from Cantoni and Yuchtman (2014). Data on navigable rivers, the ecclesiastical status of cities, monasteries and mendicant orders, and the diffusion of Protestantism as the dominant city-level religion are from Cantoni (2012).

[19] In addition to these principal sectors, a number of individuals had military careers or were nobles.

[20] For selective inclusion into the *Deutsche Biographie* to threaten our research design what would be required is that people born in or migrating to cities that adopted institutional change are selectively included. However, our results hold if we restrict analysis to super-star individuals for whom selective inclusion is not plausible, as discussed in Appendix B. Our results are also unlikely to be explained by shocks that destroyed historical records as discussed in Appendix A.

[21] These data are only available through 1550 due to the nature of the underlying sources. Because long-run data on university degree recipients are not available we not study this as an outcome here.

4 The Impact of Institutions on Human Capital

4.1 Motivation

In this section we study how upper tail human capital responded to institutional change. We estimate the causal impact of institutions on the formation of upper tail human capital using a difference-in-differences identification strategy. We document the distinct effects of institutions on the migration and local formation of human capital and show that the effects were most immediate in sectors targeted by institutional change – government, education, and church. The human capital response thus both reflected and worked to increase state capacity – by impacting the location of elites who organized the provision of public goods.

We study upper tail human capital because the institutions of the Reformation were designed to produce and attract human capital elites to staff and improve performance in expanding state and church bureaucracies (Strauss 1988). In his open-letter, *To the City Councillors* (1524), Luther emphasized the need for "men to govern." In the prologue to a 1528 church ordinance, Philip Melanchthon underlined that the institutions were designed, "for raising up people who are skilled to teach in the church and govern in the world," and an ordinance from Württemburg (1546) indicates simply, "men are needed to serve in preaching offices, governments, temporal posts, administrative offices."[22] To achieve this goal: "Officials roamed the land looking for 'good minds' in town and village schools" (Strauss 1978; p. 178).[23] This evidence motivates us to distinguish migration and local formation, and to examine whether the human capital effects of institutional change varied across sectors.

To study the migration and formation of upper tail human capital we collect biographical data on all individuals in the *Deutsche Biographie* who either were born in or migrated to the 239 cities in our data between 1320 and 1820. We classify as a migrant any individual who died in a given city, but was born in some other location, whether a city, a town, or a village. Observed migrants thus comprise both individuals who migrated as adults and those who were identified as promising students and offered school places in cities while still minors. We classify as local formation individuals observed in the *Deutsche Biographie* born in a given city in our data. Table 1 presents summary statistics on upper tail human capital

[22] Cited in Strauss (1988; p. 196). See also Sehling (1902-2013).

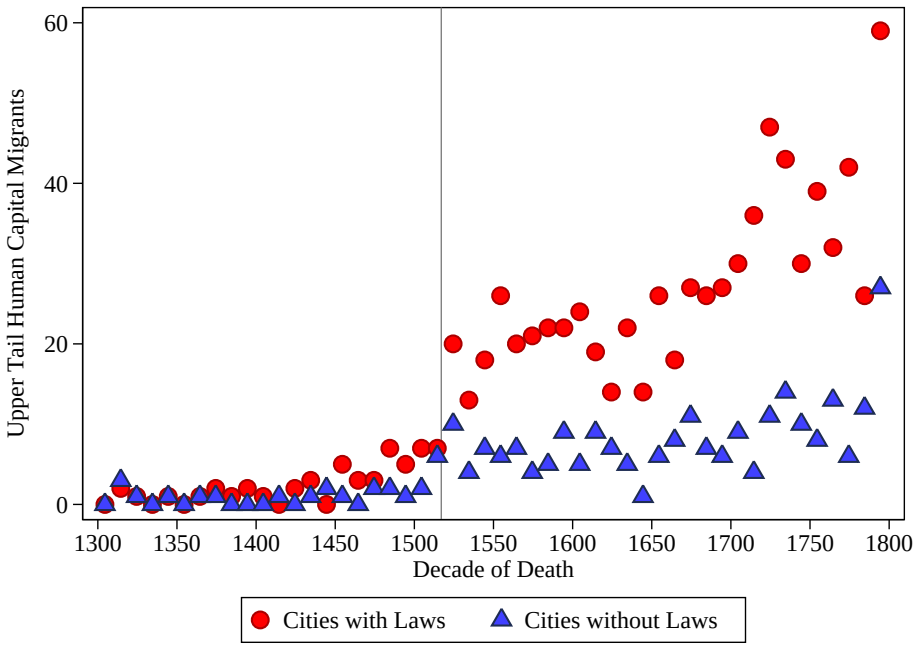
[23] Systematic efforts were made to identify talented children from poor backgrounds (Strauss 1978).

Table 1: Summary Statistics on Upper Tail Human Capital

	Cities with Law			Cities without Law			Difference
Upper Tail Human Capital	N	Mean	Sd	N	Mean	Sd	HL Statistic
Locally Born Pre-1520	103	1.26	3.55	136	0.24	0.77	0.00
Locally Born Post-1520	103	36.95	89.09	136	10.82	23.58	6.00***
Migrants Pre-1520	103	0.63	1.25	136	0.23	0.90	0.00
Migrants Post-1520	103	17.54	50.45	136	4.46	10.51	2.00
Total Pre-1520	103	1.89	4.36	136	0.47	1.49	0.00
Total Post-1520	103	54.50	138.42	136	15.28	33.04	8.00***

Upper tail human capital is measured by the number of people observed in the *Deutsche Biographie*. Locally born are people born in a given city i. Migrants to any given city i are individuals born in some other location j who died in city i. The last column presents the Hodges-Lehman non-parametric statistic for the difference (median shift) between cities with laws and cities without laws. We use the Hodges-Lehman statistic because we are examining non-negative distributions for which the standard deviation is larger than the mean and as a test statistic that is robust to outliers. Statistical significance at the 99%, 95%, and 90% levels denoted ***, **, and *, respectively.

Figure 3: The Migration of Upper Tail Human Capital



This graph plots the number of migrants observed in the *Deutsche Biographie* at the decade level in cities with and without laws. Migrants are identified as people living and dying in town i but born in some other location j. The vertical line is at 1518, the year Luther's theses began circulating.

and shows significant differences in the period after institutional change.

Our econometric analysis is motivated by Figure 3, which plots the raw data and shows a sharp jump in migration into cities that adopted institutional change in the 1520s. Figure 3 shows that cities with and without laws were attracting similar numbers of migrants before

the Reformation, that there is a sharp and persistent increase in migration observed in cities with laws starting in the 1520s, and that the evolution in the number of migrants in cities without laws does not change during the Reformation.[24] Significantly, cities with laws overwhelmingly attracted these migrants from smaller towns, not from cities without laws. Net migration from untreated to treated cities was virtually zero as shown in Appendix B.

4.2 Results

We study the migration and local formation of upper tail human capital using difference-in-differences research designs.[25] We show that cities where institutions changed in the 1500s saw shifts in the level and trend of human capital accumulation, relative to time invariant fixed effects and underlying trends, and that the key shifts date from the era of institutional change. We also document that there was no prior difference in human capital trends for cities that did and did not adopt institutional change in the 1500s.

Baseline Difference-in-Differences

Table 2 reports estimates that test for shifts in the level and linear trend in upper tail human capital accumulation associated with the adoption of a Reformation law institutionalizing public goods. The outcome is the log of the number of upper tail human capital people observed in a fifty year periods from 1370 through 1819.[26] The post period begins 1520: the first treatment period is 1520-1569, the second is 1570-1619, etc.

We first test for level shifts in the local formation and migration of upper tail human capital in response to institutional change measured by Reformation law (Law_i). Columns 1 and 4 present results for local formation and migration, respectively, controlling for city fixed effects (θ_i), time fixed effects (δ_t) and common trends ($Trend_t$). We estimate the following regression specification:

$$\ln(People_{it} + 1) = \theta_i + \delta_t + \beta_0(Trend_t) + \beta_1(Post_t \times Law_i) + \epsilon_{it} \quad (1)$$

[24] In Appendix B we show that "untreated" Protestant and Catholic cities evolve similarly. The observed jump in the data should not be interpreted as a direct measure of the local treatment effect, since some of the migrants we observe in the 1520s became famous due to their role in the institutionalization of the Reformation or migrated in earlier periods.

[25] In the Appendix we collapse the data into single 'pre' and 'post' periods and find large effects of institutions on upper tail human capital in the post period.

[26] The Appendix reports estimates examining the raw count of upper tail human capital individuals that show qualitatively similar results.

Table 2: Upper Tail Human Capital Before and After Institutional Change

	[1]	[2]	[3]	[4]	[5]	[6]
	Outcome: Ln Formation			Outcome: Ln Migration		
Trend	0.25***	0.39***		0.06*	0.07	
	(0.04)	(0.07)		(0.03)	(0.06)	
Post × Law	0.34***		-0.13	0.24***		0.22
	(0.06)		(0.16)	(0.06)		(0.15)
Post × Trend		-0.17***	-0.17***		-0.02	-0.02
		(0.03)	(0.03)		(0.03)	(0.03)
Trend × Law		0.03			-0.01	
		(0.02)			(0.02)	
Post × Trend × Law		0.02**	0.05**		0.03***	-0.01
		(0.01)	(0.02)		(0.01)	(0.02)
Observations	2151	2151	2151	2151	2151	2151
R^2	0.55	0.55	0.67	0.56	0.56	0.68
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes		Yes	Yes	
City-Specific Trends			Yes			Yes
Territory Fixed Effects			Yes			Yes

This table presents the results of regression analysis estimating the effect of Reformation laws on the local formation and migration of upper tail human capital. Columns 1 to 3 study the local formation of human capital, measured by the log of the number of native-born people observed in the *Deutsche Biographie* plus one in a city-period. Time periods are 50 year intervals: starting with 1370-1419 and ending with 1770-1819. Columns 4 to 6 study the migration of human capital, measured by the log of the number of migrants to a given city observed in the *Deutsche Biographie* plus one. Migrants to city i are defined as people dying in city i who were born elsewhere. "Law" is an indicator for cities that passed Reformation ordinances in the 1500s. The "Post" period is 1520 through 1819. The pre period is 1370 to 1519. All Columns 1 and 4 control for time-period fixed effects. Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, and *, respectively. Standard errors are clustered at the city level. Panel B estimates analagous regressions examining the migration of upper tail human capital, measured as the number of people observed in the *Deutsche Biographie* who died in a given city but were not born there.

We find that the local formation of human capital increased by 0.34 log points (42 percent) and that migration increased by 0.24 log points (27 percent) in cities that adopted institutional change in the post period.

We next test for differences and shifts in the human capital trends across cities that did and did not adopt institutional change. To do this we estimate the model:

$$\begin{aligned} \ln(People_{it}+1) = \theta_i + \delta_t + \beta_0(Trend_t) + \beta_2(Post_t \times Trend_t) \\ +\beta_3(Trend_t \times Law_i) + \beta_4(Post_t \times Trend_t \times Law_i) + \epsilon_{it} \end{aligned} \tag{2}$$

The identifying assumption for this difference-in-differences design is that absent institutional change similar human capital trends would have characterized cities that did and did not adopt public goods institutions. Consistent with this assumption, we find no significant difference in human capital pre-trends for cities that adopted these institutions: Columns 2 and 5 show that β_3 is insignificant. While underlying trends were similar, we find that cities that adopted the institutions saw significant increases in their human capital formation and migration trends in the post period, of 2 and 3 percentage points, respectively.

Finally, we test for differences in levels and trends simultaneously. Here we control for city-specific trends (ϕ_{it}) and territory fixed effects (γ_j):

$$\begin{aligned} \ln(People_{it}+1) = \phi_{it} + \delta_t + \gamma_j + \beta_1(Post_t \times Law_i) + \beta_2(Post_t \times Trend_t) \\ +\beta_4(Post_t \times Trend_t \times Law_i) + \epsilon_{it} \end{aligned} \tag{3}$$

Column 3 shows that cities that adopted institutional change in the 1500s experienced a significant positive shift in the trend of local human capital formation. Column 3 also shows a weak negative shift in the level of human capital formation. Column 6 shows that there was no similar change in the trend in migration and that the positive level shift in migration is less precisely estimated when we control for city-specific trends. These results indicate that migration and local formation responded somewhat differently to institutional change and suggest that estimates from linear models may not capture some of the relevant variation associated with treatment.

Flexible Difference-in-Differences Design

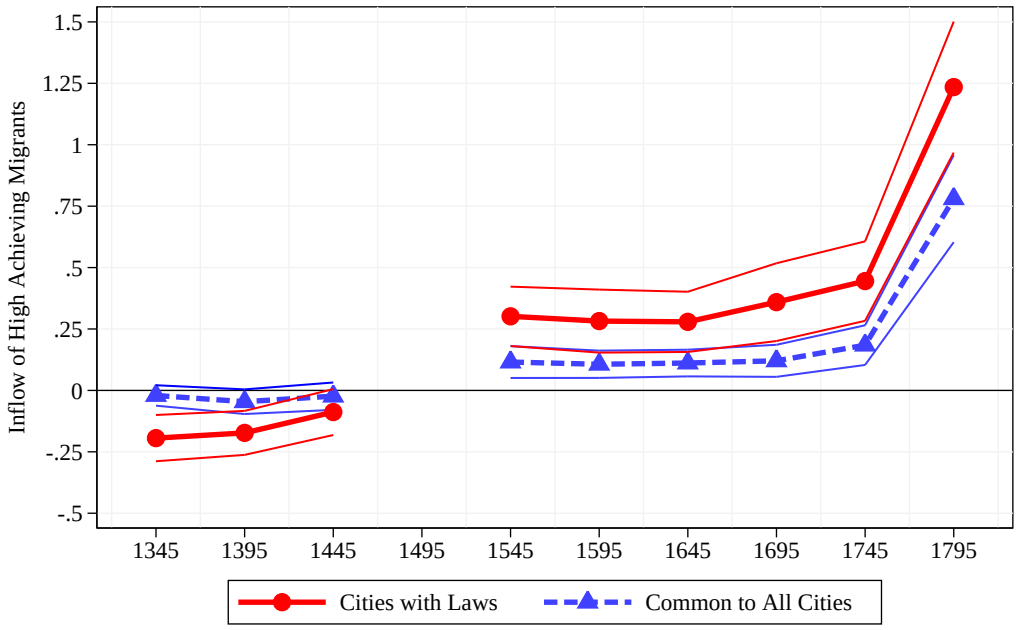
To examine the human capital response to institutional change more flexibly, we study how migration and local human capital formation varied with 'ever-treated' status period-by-period. We estimate regressions of the form:

$$\ln(People_{it}+1) = \theta_i + \delta_t + \sum_{s=1320}^{1770} \beta_s(Law_i \times Time_s) + \epsilon_{it} \tag{4}$$

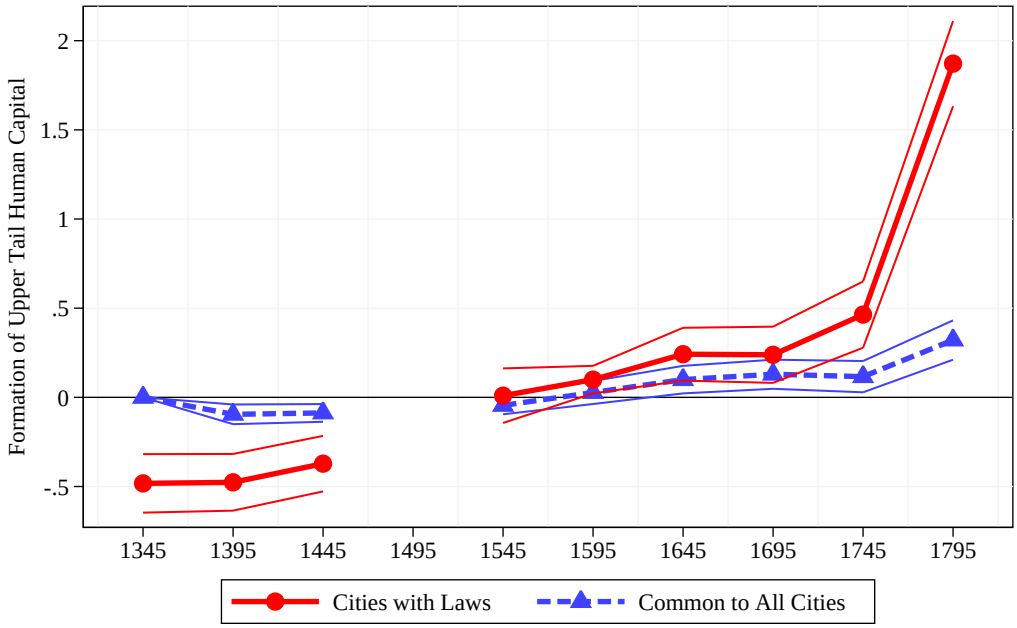
The parameters of interest are the β_s, which capture the period-specific human capital advantage enjoyed by treated cities, controlling for city fixed effects θ_i and time-period fixed

Figure 4: Regression Analysis of Upper Tail Human Capital

Panel A: Migration of Human Capital



Panel B: Local Formation of Human Capital



This graph plots parameter estimates from regression analysis examining the differential local formation of upper tail human capital in cities that adopted Reformation laws. The outcome variable is the logarithm of the number of upper tail human capital individuals plus one who were born in city i in period t. Dates and places of birth are identified in the *Deutsche Biographie*. We assign people to the city and time-period in which they were born. We graph the parameter estimates on time-period fixed effects and the interactions between time-period indicators and an indicator for cities ever adopting Reformation law. The regression includes fixed effects for cities and time periods and is estimated over data from 1320 through 1820. The omitted time category is the period 1470 through 1519 (centered on 1495). The post-Reformation periods begin with the 1520-1570 period (centered on 1545).

effects δ_t.[27]

Figure 4 presents our regression estimates graphically.[28] Panel A presents estimates for migration and shows that all cities enjoyed a modest increase in migration of human capital post-Reformation, but that cities adopting public goods institutions enjoyed a very large increase at this date. Consistent with the results in Table 2, we observe a level shift in migration but not a differential shift in the time trend. Panel A shows that the differential migration of upper tail human capital into cities that reformed their legal institutions in the 1500s is observed only after the Reformation and persisted through 1800. The results from this baseline specification are supported by alternate specifications that directly examine the count of upper tail human capital migrants. We report additional results in Appendix B.

Figure 4, Panel B presents estimates for the local formation of human capital. Panel B suggests that cities with public goods institutions enjoyed an increase in trend in human capital formation, but that the biggest relative gains were in the 1700s. It also shows that cities with public goods institutions experienced a level increase in upper tail human capital in the omitted period just before the Reformation. This reflects the fact that educated human capital elites who participated in the formalization of public goods institutions are more likely to feature in the *Deutsche Biographie*.[29] To address questions of causality more tightly, we study whether upper tail human capital responded differentially in sectors targeted by the new institutions.

Shifts in Allocation of Human Capital Across Sectors

We examine the allocation of upper tail human capital across six occupational sectors: government (20%), church (15%), education (16%), business (18%), arts (26%), and medicine (5%).[30] We measure the allocation of human capital by classifying the professions of all individuals in the *Deutsche Biographie* (see Appendix A). We then study the allocation of upper tail human capital using the flexible difference-in-difference regression design of

[27] We estimate this regression using data starting in 1320 for illustration purposes. Results over the baseline period 1370 through 1770 are virtually identical.

[28] We plot time fixed effects common to all cities (δ_t) and for cities treated by Reformation law ($\delta_t + \beta_t$).

[29] Our results are not explained by the selective inclusion of marginal individuals into the *Deutsche Biographie*. Our results hold in the upper tail of "super-stars" for whom there is no ambiguity around inclusion. For example, our results hold for the top 25 percent of individuals ranked by length of biographical essays in the *Deutsche Biographie* as discussed below.

[30] A limited number of military careers and nobles are do not included in this analysis, as described above.

Equation 4, and maintaining the distinction between migration and local formation.

Table 3 presents our estimates. In Panel A, the outcome is a binary variable for the presence of any upper tail human capital migrants in a given city-period active in a specific occupational sector. Panel A shows that cities that adopted public goods laws in the 1500s were significantly more likely to attract migrants in the goverment and education sectors starting in the 1520s. These cities were also significantly more likely to attract upper tail human capital migrants with church careers across the post-1520 period, although they were also somewhat more likely to attract church human capital in the 1420-1469 period. In contrast, while we observe positive effects in business, arts, and medicine these are not significant in most periods. Panel B presents similar estimates studying the formation of upper tail human capital in different sectors. The outcome is a binary variable for any individuals in a given city-period and sector. We find that there is no discontinuous shift in the local formation of human capital and that the sectors with the biggest effects by the late 1700s are education, business, and arts.

Super-Stars within the Upper Tail of Human Capital

We next examine "super-stars" within the *Deutsche Biographie* in order to study (1) individuals for whom potential selection into the *Deutsche Biographie* is not salient and (2) the effects of institutions on human capital *within* the upper tail. We define "super-stars" as those individuals for whom the *Deutsche Biographie* provides an extended biographical essay.[31] These super-stars account for just over 25 percent of entries. We examine super-stars using our baseline difference in differences designs.

Table 4 presents our results on the sectoral allocation of super-stars and shows strong responses in migration to Reformation laws, as well as some differences in which sectors have the strongest responses when compared to our baseline results. In Panel A, we examine super-star migration. We observe sharp responses to Reformation laws in the education, church, and business sectors. The business sector migration effect we observe for super-stars was not as clear when we examined the complete data (above). For super-stars we also find a muted and less immediate impact on government sector than in the baseline data. In Panel B, we examine super-star local formation and observe more gradual effects as in our

[31]The *Deutsche Biographie* provides summary evidence for all individuals on careers, places of birth and death, and family connections and extended biographical essays for the most prominent individuals.

Table 3: Institutions and Types of Upper Tail Human Capital 1370-1819

	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Migration of Human Capital – Binary Outcome for Specific Human Capital						
	Govt	Church	Education	Business	Arts	Medicine
Law × 1370-1419	-0.03	0.02	-0.02	-0.04	-0.04	-0.01
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.01)
Law × 1420-1469	-0.00	0.06*	-0.04*	-0.05**	-0.04*	-0.01
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)
Law × 1520-1569	0.09**	0.14***	0.11**	0.02	0.08*	0.02
	(0.04)	(0.05)	(0.04)	(0.03)	(0.05)	(0.02)
Law × 1570-1619	0.07	0.10**	0.01	0.06	0.08*	0.03
	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.03)
Law × 1620-1669	0.10**	0.11**	0.07*	0.05	0.05	0.02
	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.02)
Law × 1670-1719	0.11**	0.12**	0.04	0.04	0.06	0.06**
	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.03)
Law × 1720-1769	0.08*	0.12***	0.09**	0.04	0.02	0.03
	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.03)
Law × 1770-1819	0.14**	0.17***	0.09	0.06	0.13**	0.10*
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.05)
Observations	2151	2151	2151	2151	2151	2151
R^2	0.33	0.36	0.34	0.37	0.38	0.24
Panel B: Local Formation of Human Capital – Binary Outcome for Specific Human Capital						
	Govt	Church	Education	Business	Arts	Medicine
Law × 1370-1419	-0.09**	-0.06	-0.06	-0.03	-0.14***	-0.07**
	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)
Law × 1420-1469	0.01	0.04	0.03	-0.03	-0.04	-0.07**
	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.03)
Law × 1520-1569	-0.01	0.09	0.02	-0.02	-0.06	-0.03
	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.04)
Law × 1570-1619	0.09	0.03	0.01	-0.00	-0.03	-0.01
	(0.06)	(0.06)	(0.04)	(0.05)	(0.05)	(0.04)
Law × 1620-1669	0.06	0.11*	0.08*	0.08*	0.01	0.07
	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
Law × 1670-1719	0.10*	0.07	0.05	0.05	0.03	-0.01
	(0.06)	(0.06)	(0.06)	(0.05)	(0.07)	(0.04)
Law × 1720-1769	0.01	0.01	0.22***	0.05	0.10	-0.00
	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)	(0.05)
Law × 1770-1819	0.06	-0.04	0.16**	0.16**	0.13*	0.05
	(0.08)	(0.06)	(0.07)	(0.07)	(0.07)	(0.06)
Observations	2151	2151	2151	2151	2151	2151
R^2	0.37	0.30	0.41	0.40	0.41	0.26

This table presents regression estimates. The outcome in Panel A is a binary variable capturing the presence of migrants in a given sector. The outcome in Panel B is a binary variable for individuals born in a given city. Time is measured in 50-year periods 1370 through 1819. Table 3 reports estimates on interactions between an indicator for cities that adopted Reformation Laws ("Law") and time period indicators. The omitted time category is 1470-1519. Migration and local formation are measured as described in the text. All regressions include separate city and time period fixed effects. Standard errors clustered at the city level. Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, and *, respectively.

Table 4: Institutions and "Super-Star" Human Capital 1370-1819

	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Migration of Human Capital – Binary Outcome for Super-Star Human Capital						
	Govt	Church	Education	Business	Arts	Medicine
Law × 1370-1419	-0.02	-0.00	0.00	-0.01	-0.00	0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	.
Law × 1420-1469	-0.02	0.01	0.00	-0.02	-0.02	-0.01
	(0.02)	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)
Law × 1520-1569	0.03	0.11**	0.06	0.06**	0.02	0.02
	(0.03)	(0.05)	(0.04)	(0.03)	(0.03)	(0.02)
Law × 1570-1619	0.03	0.04	0.04	0.06*	0.11***	0.02
	(0.04)	(0.05)	(0.03)	(0.03)	(0.04)	(0.02)
Law × 1620-1669	0.03	0.07	0.11***	0.05*	0.07*	0.02
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.02)
Law × 1670-1719	0.05	0.06	0.06*	-0.00	0.08	0.00
	(0.04)	(0.05)	(0.04)	(0.03)	(0.05)	(0.01)
Law × 1720-1769	0.11**	0.03	0.12***	0.00	0.04	0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.02)
Law × 1770-1819	0.15**	0.13**	0.14**	0.06	0.10	0.14**
	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.05)
Observations	2151	2151	2151	2151	2151	2151
R^2	0.32	0.36	0.33	0.37	0.33	0.21
Panel B: Local Formation of Human Capital – Binary Outcome for Super-Star Human Capital						
	Govt	Church	Education	Business	Arts	Medicine
Law × 1370-1419	-0.06	0.00	-0.04	-0.01	-0.04	-0.02
	(0.04)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
Law × 1420-1469	-0.01	0.07	-0.04	-0.00	0.00	-0.03
	(0.04)	(0.04)	(0.04)	(0.02)	(0.03)	(0.02)
Law × 1520-1569	-0.03	0.04	0.01	-0.03	0.03	-0.02
	(0.05)	(0.05)	(0.05)	(0.03)	(0.04)	(0.03)
Law × 1570-1619	-0.06	0.03	-0.01	0.03	0.07*	-0.01
	(0.05)	(0.05)	(0.04)	(0.03)	(0.04)	(0.03)
Law × 1620-1669	0.01	0.03	0.07	0.03	0.03	0.07*
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Law × 1670-1719	0.03	0.03	0.05	0.06	0.14**	0.02
	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)	(0.04)
Law × 1720-1769	-0.02	-0.01	0.17**	0.05	0.15**	0.03
	(0.06)	(0.05)	(0.07)	(0.06)	(0.06)	(0.05)
Law × 1770-1819	0.09	0.01	0.16**	0.16**	0.15**	0.09
	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)	(0.06)
Observations	2151	2151	2151	2151	2151	2151
R^2	0.36	0.27	0.42	0.45	0.37	0.26

This table presents regression estimates studying sector-specific super-star human capital. Panel A studies migration, measured by a binary dependent variable capturing the presence of super-star migrants. Panel B studies the formation of human capital, measured a binary dependent variable measuring the presence of super-star individuals born in a given city. Super-stars and sectors are classified as described in the text. All regressions include separate city and time period fixed effects. Standard errors clustered at the city level. Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, and *, respectively.

baseline analysis. We find that by the 1700s, treated cities were producing more super-stars in business and the arts. These results are broadly consistent with our baseline findings.

When we study super-stars in aggregate, as opposed to at the sectoral level, we find shifts in migration and local formation that closely mirror our findings for overall upper tail human capital including non-super-stars. We report these results in Appendix B.

Discussion

We find that institutional change drove increases in the migration and formation of upper tail human capital. In the full sample, these effects are concentrated in the sectors targeted by the institutional changes – government, church, and education. For super-stars, we also observe sharp effects for individuals active in business. These shifts are sharpest for migration. For local formation, the results are more muted and point towards spillover effects on sectors that were not directly targeted, notably the business sector.

The nature of the empirical migration and formation processes help explain these findings. Narrative evidence and theory lead us to hypothesize that migration flows represented Tiebout sorting. However, German cities that institutionalized public goods in law also directly promoted the migration of upper tail human capital *during* the educational process (Strauss 1978). The recruitment of promising school children from small towns may explain why observed migration effects are relatively strong and sharp.

More broadly, our findings contribute to and cut across existing research on upper tail human capital. Cantoni and Yuchtman (2014) find that universities drove the emergence of market institutions in Germany during the Middle Ages. Meisenzahl and Mokyr (2012) and Squicciarini and Voigtländer (2015) document that the upper tail of the human capital distribution in science and the mechanical arts mattered for growth during the Industrial Revolution.[32] In research on general human capital, Becker and Woessmann (2009) show that Protestantism was associated with literacy and development in the 1800s. These studies all rely on cross sectional data on human capital, except Cantoni and Yuchtman (2014), who study the impact of exogenous university foundation on local market institutions.

Unlike previous research, we employ panel data and highlight the importance of institutional change for upper tail human capital centuries before the Industrial Revolution.

[32]These studies examining upper tail human capital are in part motivated by the finding that basic literacy appears to have had little effect on development during the British Industrial Revolution (Mitch 1998).

Table 5: Log Population in 1800 by Reformation Law Status and Initial Size

Population in 1500	[1] Cities with Law N	[2] Cities with Law Mean	[3] Cities with Law Sd	[4] Cities without Law N	[5] Cities without Law Mean	[6] Cities without Law Sd	[7] Difference in Means	[8] Share with Law	[9] Share of Cities
Unobserved	35	1.81	0.43	94	1.59	0.50	0.22**	0.27	0.54
1-5 Thousand	32	2.01	0.61	30	1.69	0.57	0.32**	0.52	0.26
6-10 Thousand	20	2.37	0.85	8	2.50	0.59	-0.13	0.71	0.12
11-20 Thousand	12	2.94	0.96	2	3.43	0.36	-0.49	0.86	0.06
21+ Thousand	4	3.29	0.13	2	3.90	0.27	-0.61	0.67	0.03
All Cities	103	2.17	0.76	136	1.73	0.65	0.45***	0.43	1.00

This table presents the summary statistics for log city population in 1800 by Reformation law status and initial pre-Reformation city size. Reformation law is an indicator variable whether a city had any ordinance by 1600. Populations are measured in thousands: $\ln(population/1000)$. Statistical significance for differences in means at the 1%, 5%, and 10% levels denoted ***, **, *, respectively in column 7. Column 8 reports the share of cities with a Reformation law in each initial size category for population in 1500. Column 9 reports the share of total cities in each initial size category.

We observe significant differences in upper tail human capital in administrative, government, church, and educational occupations – pointing to the early emergence of a service elite.

5 Institutions and Long-Run Economic Outcomes

In this section, we test the hypothesis that cities that institutionalized public goods provision with Reformation laws experienced more rapid long-run population growth and became more upper tail human capital intensive by 1800. We study city population as a measure of local economic activity in historical settings where direct measures of output or revenue are not available, and as a measure of revealed preferences, motivated by the literature on city growth (De Long and Shleifer 1993; Glaeser, Scheinkman, and Shleifer 1995; Acemoglu, Johnson, and Robinson 2005a). The results we present in this section are suggestive correlations. We address potential endogeneity with an instrumental variable design in Section 6.

We find that cities that adopted Reformation laws grew to be significantly larger, have more upper tail human capital, and be upper tail human capital intensive in 1800 compared to observably similar cities without these institutions. We find that non-institutionalized Protestantism was not associated with growth, consistent with Cantoni (2015).[33]

Table 5 presents three motivating facts on the relationship between initial city population,

[33]We examine the cross section because data on city populations are unbalanced and for a large proportion of "treated" cities unobserved in many pre-treatment periods. We provide further discussion in Appendix C.

institutional change in the 1500s, and population in 1800. First, we observe that cities with Reformation laws were 45 log points (57 percent) larger in 1800 than cities that did not adopt. Second, cities that were already large in 1500 were more likely to adopt Reformation laws (column 9). Third, when we compare cities of similar initial sizes we observe a large and significant positive relationship between Reformation laws and long-run population across the vast majority of locations that were *small* in 1500, but not in the far upper tail of already-large cities. Table 5 thus shows that relationship between institutional change and subsequent population was strongly positive in locations that had not been particularly dynamic and were initially small – and statistically insignificant and negative for upper tail cities where adoption was most likely.[34]

Table 6 presents summary statistics for cities that did and did not adopt Reformation laws. In 1500, cities with laws were not significantly different on human capital dimensions, as measured by the number of books printed, the number of students getting university degrees, or the presence of universities. Among cities that were small in 1500, cities that adopted Reformation laws were more likely to be Free-Imperial cities (*Freie und Reichstädte*). We control directly for these factors in our analysis.[35] We also document that Free-Imperial cities had no growth advantages post-1500. In fact, the relationship between institutional change and growth was *weaker* in Free-Imperial cities.[36]

To study the relationship between the city-level Reformation law and long-run population and human capital outcomes, we estimate the following regression:

$$Outcome_i = c + \alpha \cdot Law_i + \gamma \cdot X_i + \epsilon_i, \tag{5}$$

where $Law_i = 1$ if city i had a Reformation law, and X_i contains control variables, including both our measure of upper tail human capital and the number of university students over multiple periods prior to the Reformation to absorb pre-trends.

Table 7 shows the results from estimating equation (5). The outcome in Panel A is

[34] We present detailed evidence on each individual town where population data is unobserved in 1500 in Bairoch, Batou, and Chèvre (1988) in Appendix A, to confirm that they were indeed small cities in 1500.

[35] In Appendix B, we present document that there are no differential pre-trends in the number of university students from treated and untreated cities. We provide evidence that the adoption of public goods institutions predicts which towns transitioned to become cities with population observed in 1800 in Appendix C.

[36] A large literature documents the relative decline of Free-Imperial cities after 1600 (Whaley 2012).

Table 6: Summary Statistics on City Populations and Characteristics

Panel A: Small Towns – Population 5,000 or Less in 1500							
	Cities with Law			Cities without Law			Difference
City Population & Controls	N	Mean	Sd	N	Mean	Sd	in Means
Log ($Population_{1800}$/1000)	67	1.91	0.53	124	1.61	0.52	0.30***
Log ($Population_{1500}$/1000)	32	1.05	0.52	30	0.93	0.46	0.11
Town Incorporation pre-1517	67	0.36	0.48	124	0.40	0.49	-0.05
Market Rights pre-1517	67	0.29	0.46	124	0.40	0.49	-0.10
Books printed pre-1517	67	10.93	90.40	124	2.45	12.38	8.47
Free-Imperial City	67	0.21	0.41	124	0.06	0.25	0.14***
University Pre-1517	67	0.03	0.17	124	0.01	0.09	0.02
University Students pre-1517	67	23.37	26.25	124	18.48	18.73	4.89
Plagues 1400-1499	67	0.19	0.89	124	0.04	0.24	0.15
Plagues 1500-1522	67	0.16	0.75	124	0.00	0.00	0.16*
Panel B: Large Towns – Population Above 5,000 in 1500							
	Cities with Law			Cities without Law			Difference
City Population & Controls	N	Mean	Sd	N	Mean	Sd	in Means
Log ($Population_{1800}$/1000)	36	2.66	0.90	12	2.89	0.77	-0.22
Log ($Population_{1500}$/1000)	36	2.38	0.49	12	2.40	0.62	-0.02
Town Incorporation pre-1517	36	0.53	0.51	12	0.50	0.52	0.03
Market Rights pre-1517	36	0.56	0.50	12	0.50	0.52	0.06
Books printed pre-1517	36	430.83	1177.80	12	530.00	1344.90	-99.12
Free-Imperial City	36	0.44	0.50	12	0.33	0.49	0.11
University pre-1517	36	0.14	0.35	12	0.42	0.51	-0.28
University Students pre-1517	36	56.69	48.99	12	120.00	138.22	-61.31
Plagues 1400-1499	36	2.11	3.18	12	0.92	0.1.78	1.19
Plagues 1500-1522	36	0.61	1.25	12	0.58	1.16	0.03

This table presents summary statistics. Panel A presents statistics for cities with population of 5,000 or less in 1500. Panel B presents statistics for cities with population greater than 5,000 in 1500. "Town Incorporation pre-1517", "Market Rights pre-1517" , and "University pre-1517" are indicators for incorporation, markets rights, and universities established by 1517. "Books printed pre-1517" is the count of books. "Free-Imperial City" is an indicator for Free-Imperial status. "University Students pre-1517" is the number of students receiving degrees from German universities 1398 to 1517. Statistical significance on t-tests for difference in means at the 99%, 95%, and 90% levels denoted ***, **, and *, respectively.

upper tail human capital 1750-1799, measured as the log of the sum of migrants and local formation. The outcome in Panel B is log population in 1800. The outcome in Panel C is the number of upper tail human capital individuals 1750-1799 per 1,000 population in 1800. Across specifications, we find that cities with laws institutionalizing public goods had 35-40 percent more upper tail human capital in the late 1700s, were 24-28 percent larger in 1800, and thus were more upper tail human capital intensive. In Column 1 we control for territory fixed effects, upper tail human capital 1470-1519 and 1420-1469 separately, and population in 1500 with categorical indicator variables.

Table 7: Reformation Laws and Long-Run Outcomes

	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Human Capital						
	Outcome: Ln Upper Tail Human Capital 1750-1799					
Reformation Law	0.35***	0.41***	0.40***		0.30***	0.30***
	(0.11)	(0.13)	(0.13)		(0.10)	(0.10)
Protestant				0.20	0.19	0.19
				(0.20)	(0.22)	(0.22)
R^2	0.29	0.40	0.40	0.34	0.35	0.35
Panel B: City Population						
	Outcome: Ln Population in 1800					
Reformation Law	0.24*	0.26**	0.25**		0.28***	0.26***
	(0.12)	(0.12)	(0.10)		(0.09)	(0.09)
Protestant				-0.08	-0.11	-0.12
				(0.18)	(0.17)	(0.21)
R^2	0.47	0.52	0.53	0.51	0.52	0.50
Panel C: Human Capital Intensity						
	Outcome: Upper Tail Human Capital per 1,000					
Reformation Law	0.09**	0.11***	0.11**		0.08*	0.08*
	(0.04)	(0.04)	(0.04)		(0.04)	(0.04)
Protestant				0.09	0.09	0.09
				(0.08)	(0.08)	(0.09)
R^2	0.25	0.42	0.42	0.30	0.31	0.30
Controls that Vary Across Specifications						
Population Fixed Effects	Yes	Yes	Yes	Yes	Yes	No
Main controls	No	Yes	Yes	No	No	No
Geo Controls	No	No	Yes	No	No	No
Cantoni Controls	No	No	No	Yes	Yes	Yes
Log Population in 1500	No	No	No	No	No	Yes
Observations	239	239	239	239	239	239

This table presents the regression estimates of the relationship between Reformation laws and long-run outcomes. Outcomes are as follows: In Panel A the log of upper tail human capital plus one 1750-1799; in Panel B log population in 1800; and in Panel C upper tail human capital individuals 1750-1799 per 1,000 population in 1800. Upper tail human capital is measured as the sum of locally born individuals and migrants recorded in the *Deutsche Biographie*. "Reformation Law" is an indicator for our main treatment variable. "Protestant" is an indicator for cities where Protestantism became the dominant religion (Cantoni 2012). All regressions control for Ln Upper Tail Human Capital in both 1420-1469 and 1470-1519. "Main Controls" are: Market rights by 1517, town incorporated by 1517, indicators for the number of books printed pre-1517 (0, 1-100, 101-1000, 1001+), university by 1517 indicator, Free-Imperial city indicator, number of university students in each 10-year period starting 1398 through 1508, the log of upper tail human capital in both 1420-1469 and in 1470-1519, and the average number of plagues from 1400 to 1499. "Geo Controls" are longitude, latitude, and their interaction. "Cantoni Controls" are year city founded and year turned Protestant, indicators for rivers, Hansa cities, Free-Imperial status, monasteries, university, and printing. Population fixed effects are indicators for population in 1500 data: missing, 1,000-5,000, 6,000-10,000, 11,000-20,000, and 20,000+. Column 6 controls for log population in 1500, setting log population to 0 for cities with data unobserved, and an indicator for cities with data unobserved. Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, *, respectively. Standard errors are clustered at the 1500 territory level.

Our main result holds when we control for initial conditions, human capital pre-trends, and the non-institutional diffusion of Protestantism. The estimate is slightly stronger and more precise when we control for initial conditions and human capital pre-trends in Column 2.[37] The point estimate is virtually unchanged when we include longitude, latitude, and their interaction as proxies for the potential growth advantages of proximity to Atlantic ports and city age in Column 3. To distinguish the variation explained by Reformation laws from the variation explained by the non-institutional diffusion of Protestantism using Cantoni's (2012) data on the non-institutional diffusion of Protestantism. We also control for distance from Wittenberg (Becker and Woessmann 2009), but most variation in distance is already absorbed in territory fixed effects. In Column 4, we use the same controls as Cantoni (2012) and find that the non-institutional diffusion of Protestantism alone had no significant relationship with outcomes.[38] We find the point estimate on Reformation laws is positive and significant controlling for the non-institutional diffusion of Protestantism in Column 5. In Column 6, we control for log population in 1500 and find the results are robust.[39]

We also find no evidence that institutional change interacted with initial city characteristics to predict outcomes, with one exception. In *ex ante* large cities we find a *negative* differential relationship between Reformation institutions and population growth.[40] We find no differential human capital or growth effect for institutions in Free-Imperial cities, cities with many university students, cities with printing, or cities with market rights. We report these results in Appendix C.

[37] We control flexibly for the number of university students from city i receiving a university degree from any German university in each 10-year period from 1398 to 1508 to proxy for pre-Reformation human capital and tastes for education. We control for formal market rights and town incorporation to proxy for commercial activity. We include categorical indicators for the number of books printed before 1517 (0, 1-100, 101-1000, 1000+), an indicator for universities, and the number of plagues between 1400 and 1499 to control for health shocks potentially affecting population and growth prospects.

[38] The controls include Protestant indicator, river indicator, Hanse indicator, Free-Imperial city indicator, year city founded, university indicator, printing press indicator, and monasteries.

[39] We assign a value of 1,000 for all cities with population unobserved in 1500 and include an indicator for unobserved status. We provide detailed evidence on these cities in the Appendix.

[40] In pre-industrial Europe, the largest cities were constrained by the need to transport food over distance and grew relatively slowly (Dittmar 2015). The institutions we study may not have relaxed this constraint.

6 Plague Shocks as a Source of Exogenous Variation

The fact that cities that adopted Reformation laws that institutionalized public goods subsequently grew more raises a question: Did cities selectively adopt based on unobservable characteristics that are the true underlying drivers of variations in growth?

We use plague outbreaks in the early 1500s as an instrumental variable (IV) to isolate exogenous variation in institutional change. Outbreaks in the early 1500s were shocks to the local political equilibrium at a critical juncture. The intuition for our IV design is that outbreaks within a narrow window are exogenous conditional on long-run propensity and other observables. We present evidence for exogeneity and the exclusion restriction that is the other identifying assumption below.

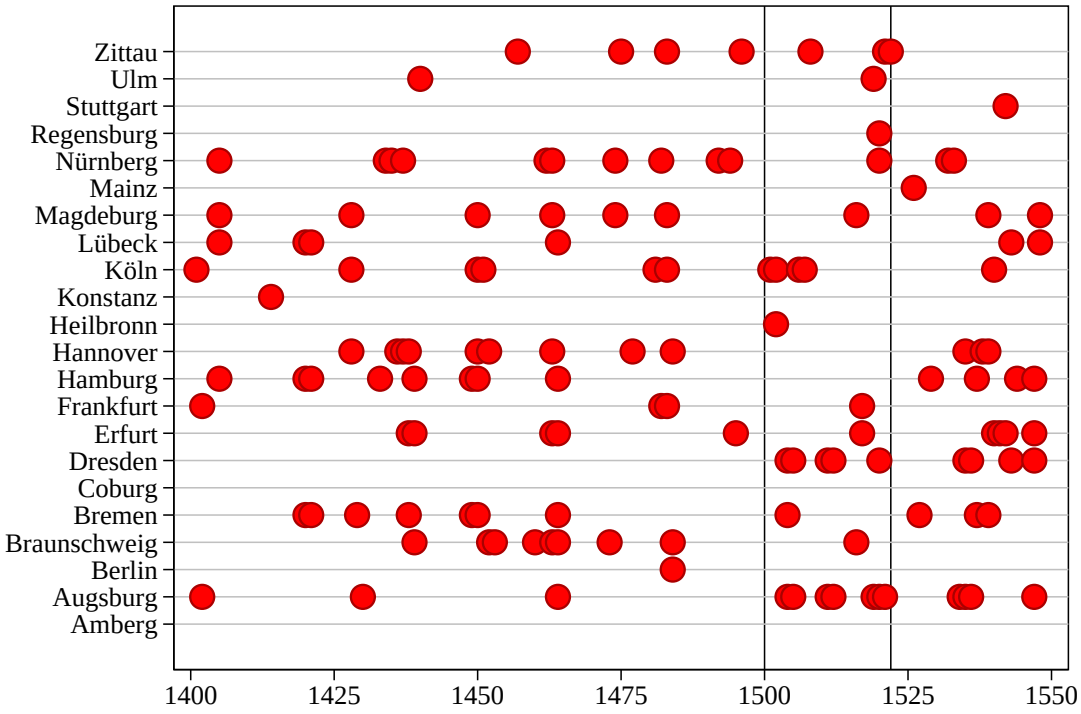
6.1 Why Plague Shocks Provide Exogenous Variation

Plagues in the early 1500s delivered exogenous variation in institutions because the *short-run* timing of outbreaks was random, conditional on long-run prevalence and observables, and because outbreaks in the early 1500s impacted city politics in a critical juncture.

Exogeneity – The historical epidemiology strongly suggests that the *short-run* distribution of plague outbreaks was random, conditional on observables such as cities' long run plague prevalence (Biraben 1975; Slack 1988). Historic plagues outbreaks were characteristically observed in "compartmentalized" locations and *not* spreading neighbor-to-neighbor (Biraben 1975; p. 285). Among the notable "puzzling features in the spread of plague" was that it "missed some towns in its transit along major highways" and was characterized by "irregular timing" (Slack 1988; p. 435).

Figure 5 illustrates the short-run randomness of plague outbreaks and the variation in the IV: outbreaks from the beginning of the century to the passage of the first law in 1522. (We find similar results examining outbreaks across the first half of the 1500s, as discussed below.) Figure 5 presents the data for select cities and shows that some experienced outbreaks frequently but with considerable differences in the timing. Others experienced outbreaks at different times despite being geographically close, for example Mainz and Frankfurt am Main, which are less than 50 kilometers apart. Others experienced few or no major outbreaks despite being important urban centers, like Frankfurt, Ulm, and Regensburg.

Figure 5: City-Level Plague Outbreaks



This graph shows the timing of major plague outbreaks in selected cities between 1400 and 1550. Source: Biraben (1975). The vertical lines at 1500 and 1522 delimit the period used in our baseline instrumental variable analysis to construct the early 1500s plague exposure instrument.

By using variation in plague within a narrow time period as our instrumental variable, we isolate shocks as opposed to variations in plague that might be correlated with city characteristics that could directly shape economic development. We show that there was no aggregate trend or periodicity in plague between 1400 and 1600 (Appendix D). We document further that there were no non-linear increases in plagues in more connected cities in the IV period, and that there were no differential plague trends in cities that were more connected to trade networks (see Appendix D). However, we still control flexibly for long-run differences and trends in plague prevalence that could reflect city characteristics like openness to trade.

Relevance – Plague shocks in the early 1500s delivered variation in institutional change because of how they interacted with politics in the critical juncture of the Reformation.

The Reformation introduced political competition. Political competition centered on radically different institutional agendas for public goods provision. The plague and public health provision figured prominently in Protestant institutional blueprints, which explicitly

formalize the provision of health and pastoral care.[41] Cities with Reformation laws institutionalized the provision of health care (Lindemann 2010; Grell 2002). In contrast, Catholic theologians and statesmen, "rejected public participation entirely or wanted to allow it in only very reduced measure" (Roeck 1999; p. 286).[42] Citizens were faced with health shocks and competition in the market for religion over social service provision. Similar dynamics are observed in contemporary research on AIDS in Africa, which shows that service provision drives conversion to a new religion (Trinitapoli and Weinreb 2012).

In the early 1500s, plague outbreaks shifted local politics and therefore institutions. Outbreaks shifted the institutional preferences of the survivors and changed the composition of the population by attracting a subsequent influx of migrants.[43] In plague outbreaks, it was not unusual for 1/4 of a town's population to die (Slack 2012). Plagues shifted politics towards institutional change by threatening civic order, discrediting elites, and altering the composition of local populations. During outbreaks, elites died and fled, often resulting in a breakdown of civic administration. Protestant reformers criticized this behavior and advocated institutional change. For example, in 1533 Andreas Osiander both scolded the city council of Nürnberg for previously abandoning the city during outbreaks in his famous "Plague Sermon" and authored a Reformation law. During the critical juncture of the early 1500s, plagues shifted the probability of adopting a Reformation law. We provide a more detailed discussion of these dynamics in Appendix D.

6.2 Instrumental Variable Estimates

For our instrumental variable design, we estimate the following first stage regression:

$$Law_{i,pre-1600} = c + \alpha \cdot \ Plagues_{i,1500-1522} + \beta \cdot \ g(Plagues_{i,1400-1499}) + \gamma \cdot X_i + \epsilon_i \qquad (6)$$

In our baseline specification, the instrument shifting institutions is the number of plague outbreaks between 1500 and 1522, the year the first Reformation law was passed. Our instrument recovers how plagues that hit the generation in place when the Reformation began

[41] Almost all Reformation laws contain provisions on directing priests to visit the sick and offer consolation.
[42] Catholic cities outside Germany did develop strategies to address the plague, e.g. in Italy (Cipolla 1992).
[43] Isenmann (2012) observes that typically the number of *new* property owning citizens with voting rights (*Neubürger*) rose dramatically after plagues. The fact that these new burghers only obtained voting rights after a period of 5 to 10 years residency is one reason why political change often occurred with lags.

shifted the probability of institutional change. The impact of plagues across the early 1500s, including through 1545, is similar and is discussed below. We control for long-run variation in plague because over the long-run outbreaks may have been more frequent in cities that were "open" or "good" and already bound to grow. To isolate plausibly exogenous variation in outbreaks we control for: the average annual level of outbreaks 1400 to 1499; higher order polynomials of outbreaks 1400 to 1499; and the number of plague outbreaks in each quarter-century across the 1400s.[44] We denote these controls with $g(Plagues_{i,1400-1499})$. The vector X_i contains the same control variables as in Section 5. The identifying assumptions are that variation in plague in the early 1500s was exogenous conditional on the observables and that the exclusion restriction, which we discuss below, holds.[45]

Table 8 shows our IV results. Column 1 shows that $Plagues_{i,1500-1522}$ is a strong predictor for the adoption of a Reformation law and that each additional plague outbreak between 1500 and 1522 increases the propensity of adopting a Reformation law by 14 percentage points. The F-statistic on the excluded instrument is above 37. The point estimate of the second stage implies that a city with a Reformation law by 1600 was 1.62 log points larger in 1800 than a city without a law. Our second stage results are slightly stronger and more precisely estimated when we control for polynomials in long-run plague prevalence (column 2). The second stage results are even stronger and more precisely estimated when we control for plague in different periods across the 1400s (column 3). The results strengthen further when we introduce state fixed effects and identify off within-state variation (columns 4 to 6). These results all control for upper tail human capital 1420-1469 and 1470-1519 (measured continuously) and population in 1500 (categorically, with one category for unobserved).

To gauge the magnitudes of our IV estimates, we compare our three regression designs. The OLS results imply that cities with Reformation laws had about 0.35 log points more upper tail human capital in the late 1700s than comparable untreated cities (Section 4). The difference-in-difference estimates imply an advantage of 1.2-1.9 log points in late 1700s (Section 5). The IV design estimates a growth advantage of about 2.7 to 4.1 log points. Converted to annual growth rates of upper tail human capital, the OLS estimates imply an advantage of 0.1 percent for the typical treated city. The difference-in-differences estimates

[44] We control for the number of plagues 1400-1424, 1425-1449, 1450-1474, and 1475-1499.

[45] Our results are robust to also controlling for non-institutionalized Protestantism. As shown above, Protestantism *per se* does not predict city growth or upper tail human capital.

Table 8: Instrumental Variable Analysis of Long-Run Outcomes

	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: First Stage – Public Goods Institutions						
	First Stage Outcome – Reformation Law					
Plagues 1500-1522	0.14***	0.13***	0.12***	0.13***	0.12***	0.11***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
R^2	0.29	0.29	0.29	0.51	0.51	0.51
F Statistic on IV	37.01	20.90	15.70	23.50	21.81	16.36
Panel B: Instrumental Variable Outcomes – Population and Human Capital						
	Outcome – Ln Population in 1800					
Reformation Law	1.62*	2.04**	2.65***	1.93*	2.41***	3.10***
	(0.86)	(0.93)	(0.72)	(1.05)	(0.91)	(0.65)
	Outcome – Ln Upper Tail Human Capital 1750-1799					
Reformation Law	2.79**	3.79***	4.14***	3.20**	4.00***	4.61***
	(1.22)	(1.27)	(1.34)	(1.34)	(1.30)	(1.29)
	Outcome – Upper Tail Human Capital per 1,000					
Reformation Law	0.57**	0.75***	0.79***	0.62**	0.71**	0.82***
	(0.27)	(0.28)	(0.29)	(0.30)	(0.32)	(0.31)
Controls that Vary Across Specifications						
Plagues 1400s Level	Yes	Yes	Yes	Yes	Yes	Yes
Plagues 1400s Polynomial	No	Yes	Yes	No	Yes	Yes
Plagues 1400s Non-Linear	No	No	Yes	No	No	Yes
Territory Fixed Effects	No	No	No	Yes	Yes	Yes
Observations	239	239	239	239	239	239

The first stage outcome variable in Panel A is an indicator for Reformation law. "Plagues 1500-1522" is the number of plagues 1500 to 1522. The outcome variables in Panel B are: log population in 1800; log of the number of upper tail human capital individuals observed between 1750 to 1799 plus one; and the number of upper tail human capital individuals per thousand population. In first stage regressions, the dependent variable is an indicator for the passage of a Reformation ordinance by 1600. All regressions control for the log of upper tail human capital observed 1370-1420 and 1420-1470 and include the complete set of controls from Table 7, including city population in categorical bins. Upper tail human capital is measured by the sum of the number of migrants dying in a city-period and the number of people locally born people reaching age forty in a city-period. Territory fixed effects control for city territories. Territories are from Euratlas. "Plagues 1400s Level" is the average number of plagues from 1400 to 1499. "Plagues 1400s Polynomial" indicates inclusion of quadratic and cubic polynomials of the level. "Plagues 1400s Non-Linear" indicates independent controls for the number of years with plague outbreaks in each of the twenty-five year periods: 1400-1424, 1425-1449, 1450-1474, and 1475-1499. Standard errors are clustered at the 1500 territory level. Territories are from Euratlas. Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, and *, respectively.

imply an annual advantage of about 0.5 percent. The IV estimates implies an annual growth advantage of approximately 1.1 percent. For city population, the OLS and IV estimates imply annual growth rate advantages of 0.1 percent and 0.7 percent, respectively.[46]

There are several possible explanations for the fact that the IV estimates are much larger than the OLS estimates. The first is that IV isolates exogenous variation in treatment and that unobserved city characteristics attenuate the OLS estimate. One might assume that because legal change was associated with growth, cities positively selected into treatment. However, there is little evidence that the Reformation was adopted for directly economic reasons. In a few notable wealthy and well-connected cities, the municipal leadership was motivated to take an anti-Reformation position by economic considerations, and was successful in preventing Protestant institutional change. Cologne was Germany's largest city in 1500 and is the classic example of a city in which elites' interest in preserving trade relationships motivated anti-Protestant behavior (Scribner 1976). A second possibility is that the instrumental variable design recovers a cleaner measure of the true nature or intensity of treatment. The legal institutions of the Reformation produced what North (1990) would recognize as local "institutional matrices." Our simple binary classification of institutions is a proxy for more nuanced variation in local rules and arrangements. It is possible that the IV captures underlying variation in institutions that are lost in proxy measurement error implicit in the binary treatment variable on which OLS relies. A third possibility is that the IV recovers underlying heterogeneity in the returns to treatment across cities.

To examine whether the IV recovers underlying heterogeneity in returns, we study whether the interaction between plague shocks and city characteristics shaped institutional change in Appendix D. We find no significant interaction between plagues and prior printing, plagues and university students, or plagues and market rights. We do find evidence that the plague effect on institutional change was muted in free cities. This suggests that the effect of plagues on institutional change was concentrated in cities subject to feudal lords, where the barriers to political change were higher.[47] If cities subject to lords had higher returns to institutional change, our IV could recover these returns. However, we find no differential

[46]For comparison, Acemoglu, Johnson, and Robinson (2005b) study city growth and find that European cities with access to Atlantic trade were 0.8-1.1 log points larger in 1800, controlling for time invariant city characteristics and time fixed effects shared across cities.

[47]This is consistent with the finding in Dittmar and Seabold (2015) that variations in media market competition mattered most for the diffusion of the Reformation ideas in cities subject to lords.

correlation between institutional change and growth in cities subject to lords (Appendix C).

Another possibility is a violation of the exclusion restriction. The next section presents evidence on the unique relationship between long-run growth and plague shocks in the early 1500s as opposed to plagues in other periods that supports the exclusion restriction.

6.3 Evidence in Support of the Exclusion Restriction

Our identification strategy requires that plague outbreaks in the early 1500s impacted long-run growth only through their impact on institutional change. We present three pieces of evidence that support the exclusion restriction.

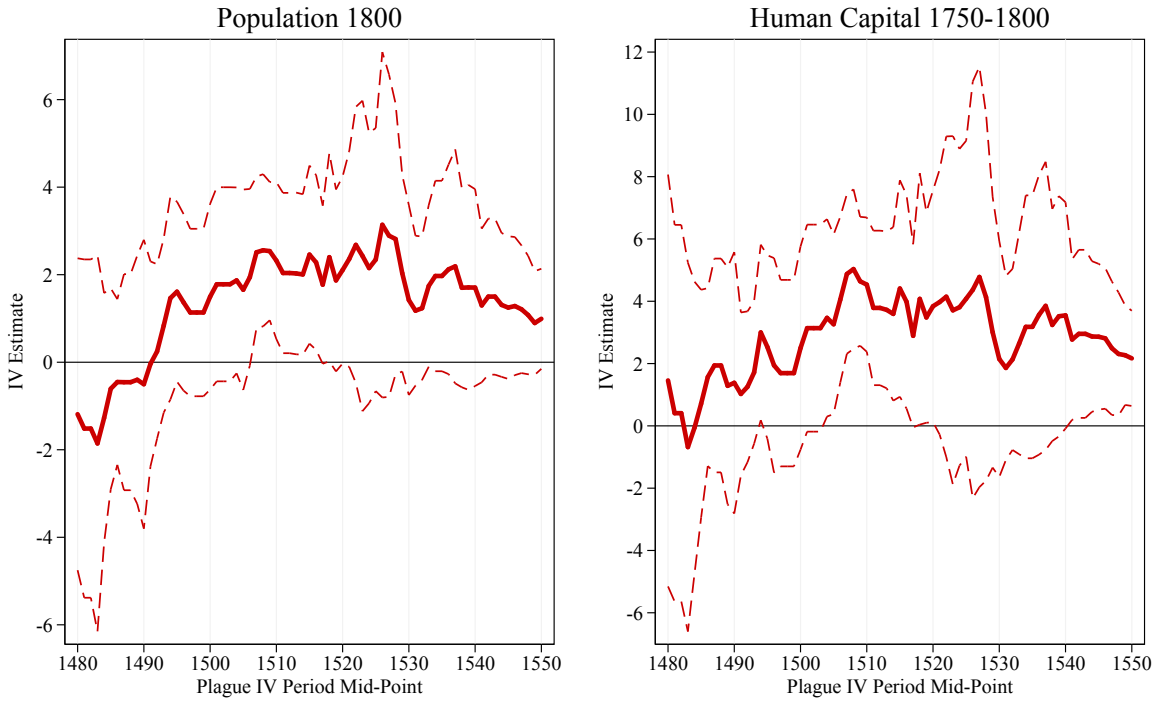
First, we show that only plagues in the early 1500s explain growth through the institutional channel. We document the unique significance of plagues in the early 1500s with comparisons across regressions that use plagues in other narrow periods as candidate IVs using our baseline specification (equation 6). We compare estimates as we shift a window of a fixed size (twenty-three years) over time. Only plagues in the early 1500s have a significant first or second stage. Figure 6 plots the IV estimates and shows that the significant relationship between growth and variation in institutions induced by plagues is only observed in the early 1500s.[48]

Second, using reduced form regressions we document that early 1500s plagues predict city population in 1800 while similar plagues in other periods 1400 to 1600 do not (see Appendix D). The fact that plagues in other periods did not shift long-run population supports the exclusion restriction and is consistent with historical evidence indicating that plagues had *long-run* impacts when outbreaks occurred in critical junctures (Biraben 1975).

Third, we examine how the IV set-up predicts institutional change and growth as some cities get laws and drop out of the sample of candidates for institutional change after 1522. We document that plagues in later periods explain institutional change for cities that were not early adopters of institutional change. For example, Hannover had no plagues from 1500 to 1522 meaning the IV is "turned off" in our baseline analysis. Hannover survived without

[48]To interpret the lingering explanatory power of plagues in the mid-1500s two observations are important. First, the laws we study increased inwards migration and city growth starting in the 1500s (Section 4 above). Because outbreaks were more likely in cities with more migrant arrivals, the distribution of plague in the mid-to-late 1500s may to some degree reflect the institutional changes of the early 1500s. Second, some cities without plagues in the period 1500 to 1522 were subsequently exposed to outbreaks and only after these later outbreaks adopted institutional change.

Figure 6: Instrumental Variable Estimates Varying the Plague Exposure Period



This graph presents estimates from instrumental variable regressions that vary the time-period used to measure the plague outbreak IV. The outcome in the left-hand panel is log population in 1800. The outcome in the right-hand panel is log upper tail human capital 1750 to 1800 plus one. Upper tail human capital is measured as the sum of migration and formation. We estimate our baseline IV regression specification in all regressions, but use as the instrument plagues from different twenty-three year time-periods. The results reported in the main text use the time-period 1500 to 1522 to measure the plague outbreak IV (see Table 8). On this graph that estimate corresponds on the x-axis to the "Plague IV Period" at 1511, the mid-point of the 1500-1522 interval. We estimate similar regressions shifting the plague period year-by-year and present the estimates graphically. All regressions include the same control variables as in Table 8, including log upper tail human capital 1420-1469, log upper tail human capital 1470-1519, and categorical indicators for total population in 1500. All regressions control for long-run plague prevalence 1400 to 1499: linearly in the level, the quadratic, and the cubic transformation of the average level of plague in the 1400s. Standard errors are clustered at the territory level. The red dashed line represents the 95 percent confidence interval.

a law into the 1530s, experienced a plague in 1535, and passed a Reformation law in 1536. We find that the first stage relationship between recent plagues and institutional change strengthened and then declined over the first half of the 1500s, but that the relationship between induced institutional change and growth remained relatively stable. This analysis allows us to compare the effects of the instrument as it gets "turned on" at different times for different cities and provides an external validity check on our baseline estimates. We present these results in Appendix D.

7 Human Capital as a Channel for Growth

Was human capital a channel for city growth driven by the institutional changes of the 1500s? Our results characterize two outcomes – upper tail human capital and city growth. We now consider two channels through which upper tail human capital may have contributed to overall growth.

First, institutional change may have directly increased growth by increasing the number of upper tail human capital producers in the private sector. For example, institutional change may have shifted the supply of skilled craftsmen, mechanics, and merchants. In the data, we observe a sharp and significant shift in the migration of *super-star* human capital in business sector occupations towards cities with Reformation laws following institutional change. Prior to institutional change, there was no significant difference across cities that did and did not adopt institutional change.[49] We also observe that cities that adopted institutional change began producing more upper tail human capital in business in the 1600s and 1700s. The effects on local formation are positive but imprecisely identified until the late 1700s during the start of the Industrial Revolution.[50]

Second, it is possible that upper tail human capital drove growth indirectly through institutional channels. Differences in upper tail human capital across cities may have caused differences in the quality and operation of institutions that were underlying determinants of growth. For instance, we expect that higher quality administrative elites may have enhanced property rights enforcement and limited corruption.[51] Historical research also suggests that the institutional changes we study, and upper tail human capital administrative elites, increased social and behavioral discipline in European society (Gorski 2003). Finally, it is possible that upper tail human capital educators and administrators fostered basic literacy that had productivity-enhancing effects.

The proximate cause of pre-industrial city growth was migration (Bairoch 1991). As

[49] Because cities that adopted institutional change facilitated migration *during* the educational process, some of these migration effects may be direct: talented young students who subsequently went into business.

[50] Meisenzahl and Mokyr (2012) point to the importance of innovators who introduce incremental change. Such innovators are less likely to be included in national biographies such as the *Deutsche Biographie*: only half of the significant "tweakers" identified in Meisenzahl and Mokyr (2012) are in the British *Dictionary of National Biography*. It is thus likely that some actors who raised productivity are not observed in our data.

[51] Contemporary research finds that meritocratic recruitment of bureaucrats is associated with variations in cross-country risk (Rauch and Evans 2000).

De Vries (1984; pp. 200, 213) observes, "Cities before the nineteenth century did not exhibit autonomous and self-reinforcing growth." Migration drove growth and was controlled. Cities had walls and gates, formal application procedures governed access to civic rights, and entrance into many trades was governed by guilds. City governments used these institutions to regulate and limit entrance by undesired poor migrants (Friedrichs 1995; Reith 2008; Hochstadt 1983; Isenmann 2012). The historical evidence suggests that the flows of upper tail human capital migration that were caused by institutional change *led* to unobserved flows of unskilled labour that were the proximate cause of growth.[52]

8 Conclusion

We provide new evidence on the origins and long-run effects of state capacity and public goods provision. We study local variation in institutional change that expanded state capacity and public goods provision in Germany during the Protestant Reformation. We document that the introduction of ideological competition combined with local public health shocks to drive institutional change in this critical juncture. Localized plague outbreaks shifted politics and increased the probability of cities adopting institutions designed to support public goods – despite restrictions on formal political representation. The new institutions bundled religious, educational, anti-corruption, and social welfare interventions and were formalized in law.

We highlight the importance of expansions of local state capacity for upper tail human capital outcomes and city growth. Using new microdata, we document that cities that adopted public goods institutions subsequently produced and attracted more individuals with upper tail human capital over the long period running from the early 1500s through 1800. We also show that cities that adopted these institutions in the early 1500s grew to be much larger by 1800. These large effects on human capital and growth occurred before the Industrial Revolution.

[52]Consistent with the interpretation that upper tail human capital was a channel for growth, in a "horse race" regression we find upper tail human capital migration 1520-1770 is a robust predictor of long-run population and that institutional change ceases to predict long-run population conditional on upper tail human capital migration (see Appendix D). This result is a suggestive correlation which we do not interpret causally. In addition, an unreported 3SLS regression analysis identifies that causal impact of institutional change running through migration, assuming exclusion restrictions hold.

In non-democratic settings, changes in institutions and state functions frequently come from above. For example, a large body of evidence highlights the military origins of state capacity in European history – driven by elites and elite competition for power (Tilly 1975; 1992; Besley and Persson 2011; Gennaioli and Voth 2015). In contrast, we study expansions of state capacity in non-democratic settings that resulted from challenges to local rulers. These expansions in state capacity were supported by new, more inclusive institutions. The institutions were designed to produce highly educated administrators and to ensure the functioning of a new social order. More broadly, our research suggests that the Reformation provides a canonical historical model of the emergence and implications of state capacity driven by political movements that challenge incumbent elites.

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