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]

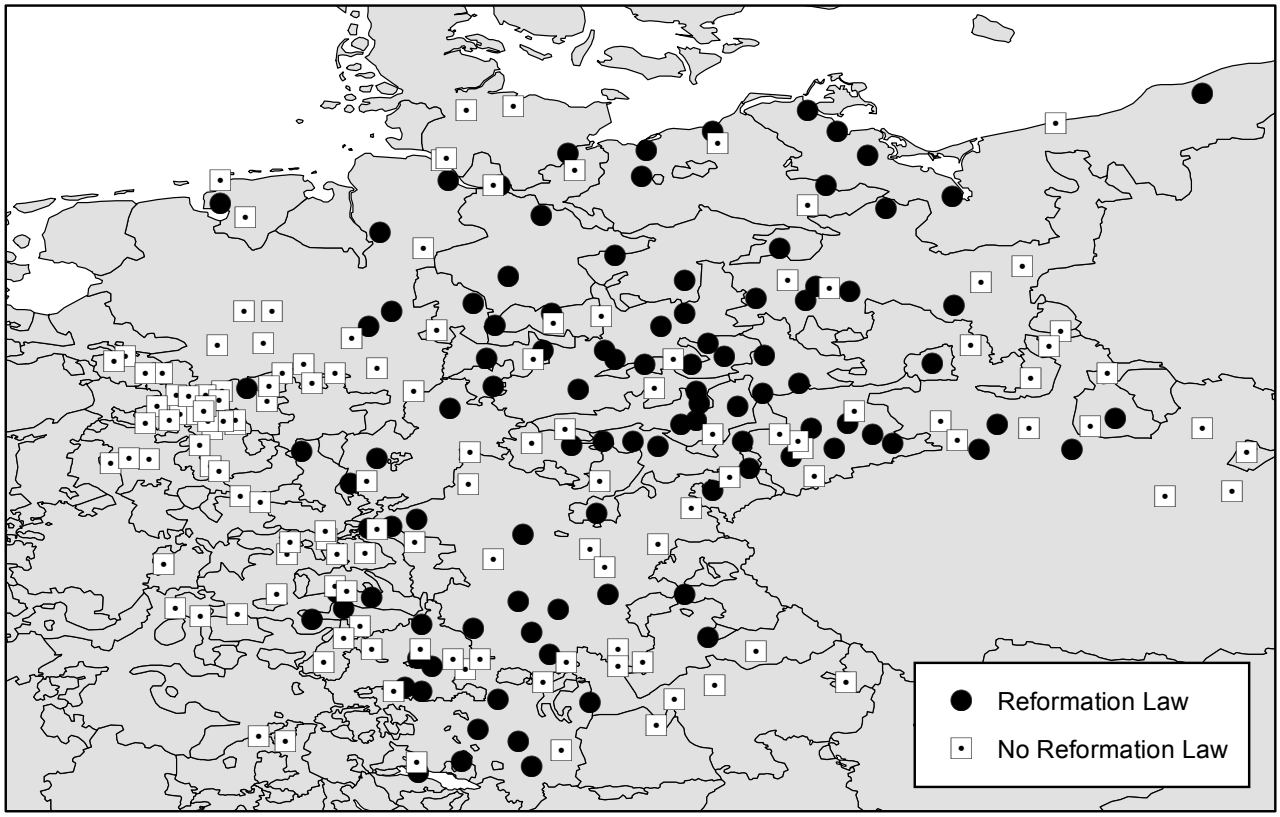
Figure 1 maps the cities in our data and illustrates the local variation in which cities had Reformation laws. Figure 2 shows the cumulative share of cities with Reformation laws in each year. Most cities passed their first law by 1545. In 1546, the Schmalkaldic War broke out between Protestant and Catholic 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).

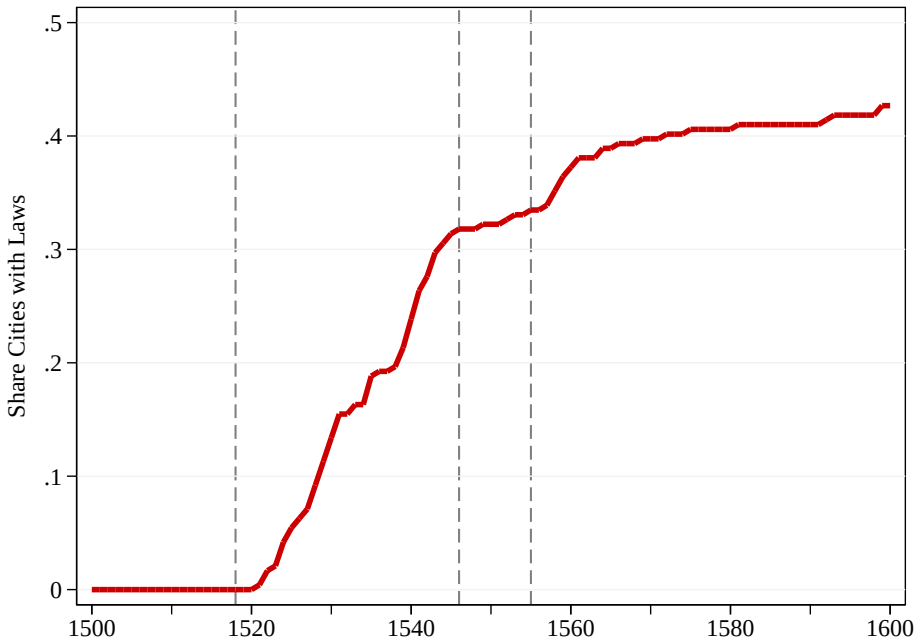
[16] The settlement included a provision, *cuius regio, eius religio,* which allowed local rulers to dictate the religion in their realm, but maintained a complicated set of exceptions for cities where magistracies and offices were to be shared and largely respected facts on the ground (Dittmar and Seabold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

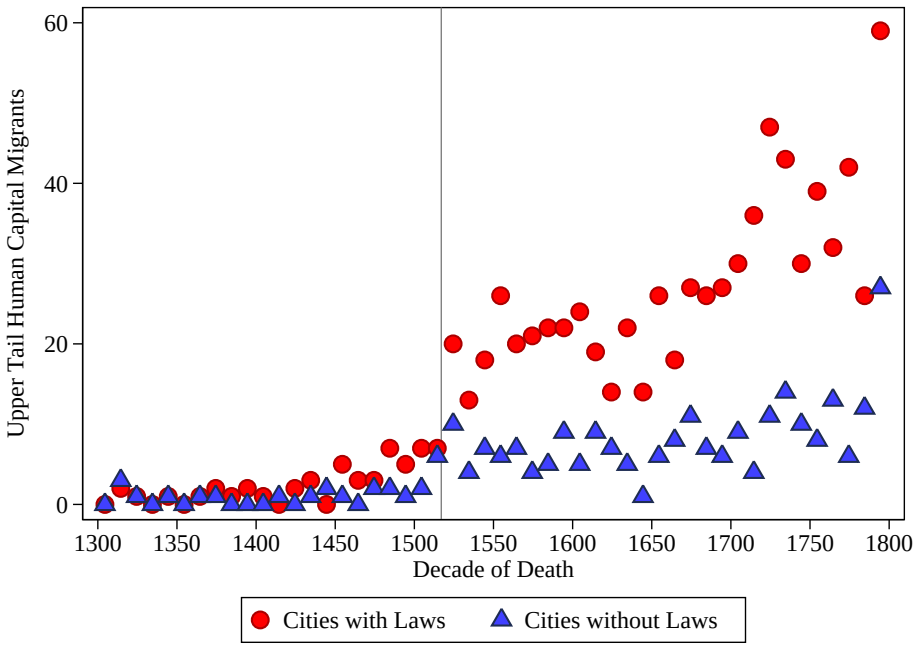
[23] Systematic efforts were made to identify talented children from poor backgrounds (Strauss 1978).

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

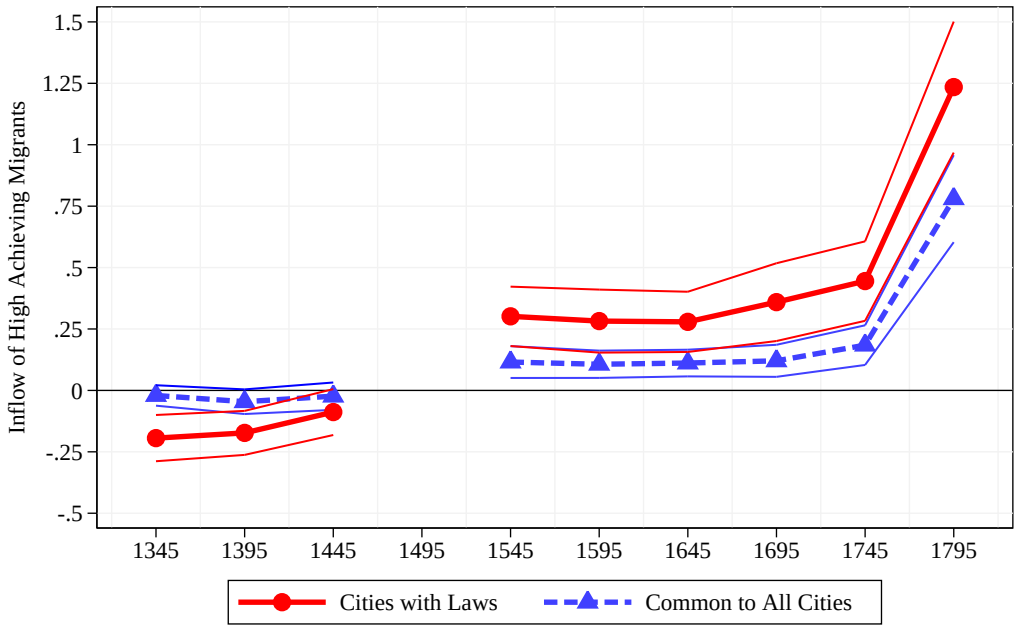
To examine the human capital response to institutional change more flexibly, we study how migration and local human capital formation varied with 'ever-treated' status period-by-period. We estimate regressions of the form:

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

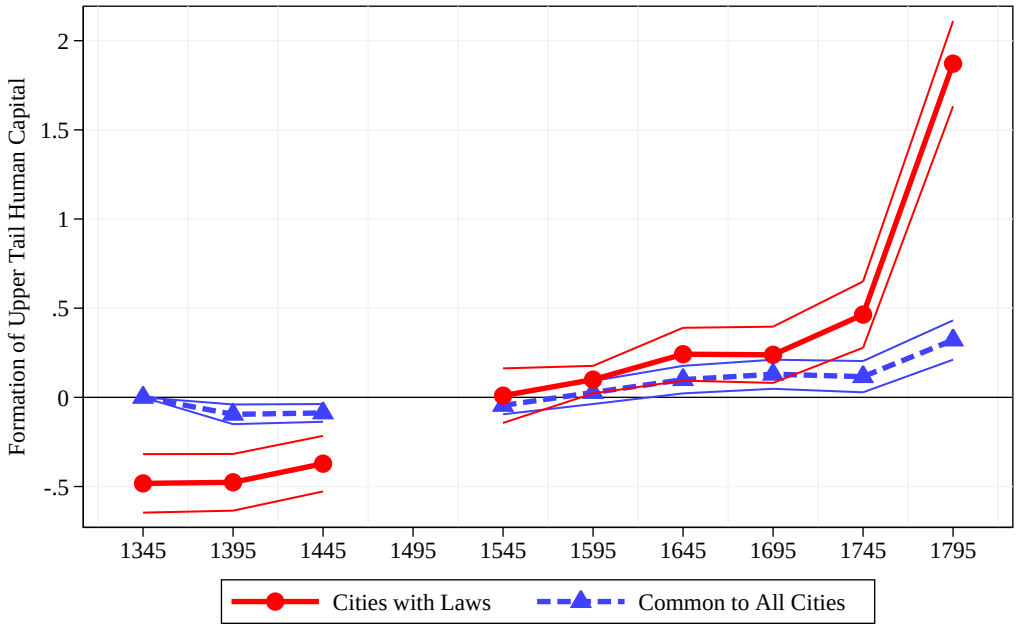
The parameters of interest are the β_s, which capture the period-specific human capital advantage enjoyed by treated cities, controlling for city fixed effects θ_i and time-period fixed

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.

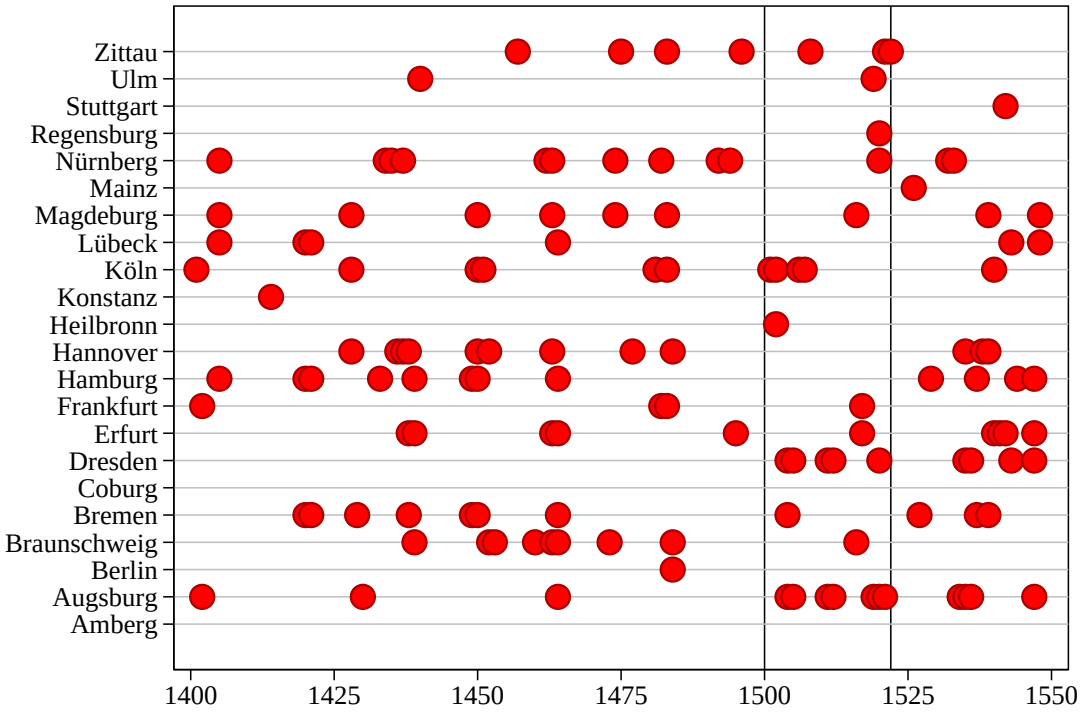
6.1 Why Plague Shocks Provide Exogenous Variation

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.

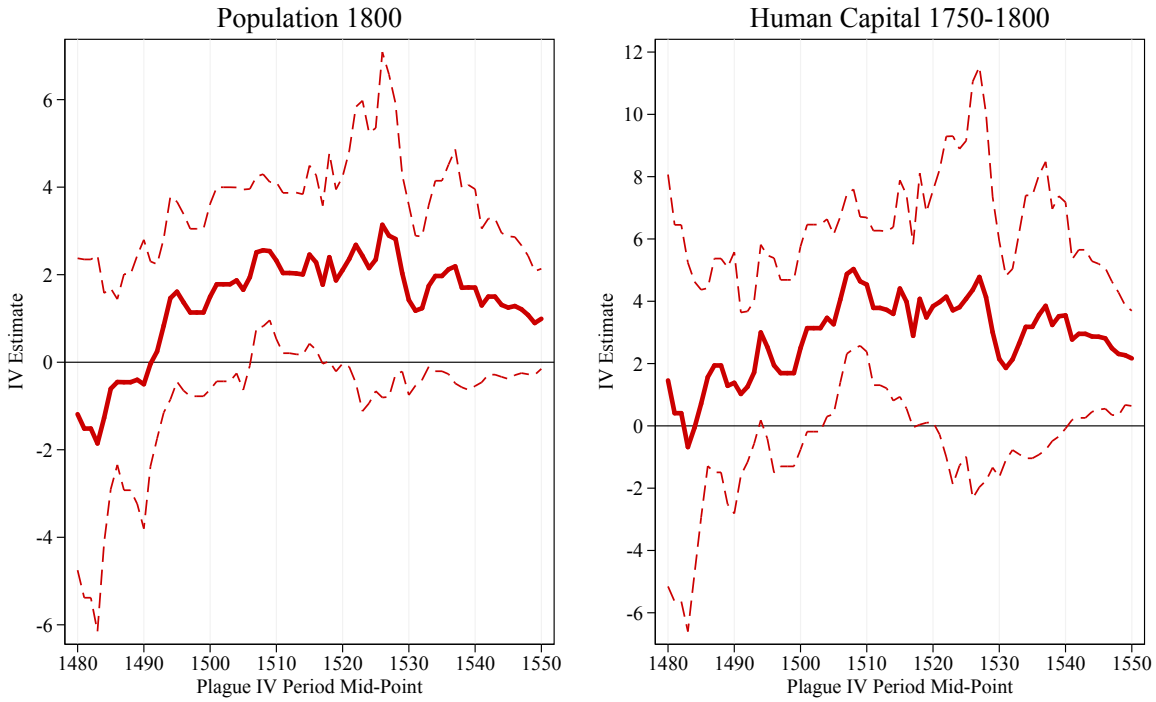
First, we show that only plagues in the early 1500s explain growth through the institutional channel. We document the unique significance of plagues in the early 1500s with comparisons across regressions that use plagues in other narrow periods as candidate IVs using our baseline specification (equation 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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Appendices – Not For Publication

A Data

A.1 Legal Institutions of Reformation

We gather data on the municipal laws (*Kirchenordnungen*) of the Reformation from the multi-volume collection of Protestant church ordinances *Die Evangelischen Kirchenordnungen Des XVI. Jahrhunderts*, originally edited by Emil Sehling. We read the text of laws and code locations. The complete list of volumes we code is as follows.

- Emil Sehling editor, Volume I Sachsen und Thüringen nebst angrenzenden Gebieten (1902) (Leipzig: O.R. Reisland).
- Emil Sehling editor, Volume II Sachsen und Thüringen nebst angrenzenden Gebieten (1904) (Leipzig: O.R. Reisland).
- Emil Sehling editor, Volume III Brandenburg, Ober- und Niederlausitz, Schlesien (1909).
- Emil Sehling editor, Volume IV Preußen, Polen, Pommern (1911).
- Emil Sehling editor, Volume V Baltische Länder, Mecklenburg, Lübeck, Lauenburg, Hamburg (1913).
- Institut für evangelisches Kirchenrecht der EKD, Volume VI/1 Niedersachsen (1955).
- Institut für evangelisches Kirchenrecht der EKD, Volume VI/2 Niedersachsen (1957).
- Institut für evangelisches Kirchenrecht der EKD, Volume VII/1, Niedersachsen (1963).
- Institut für evangelisches Kirchenrecht der EKD, Volume VII/2, Niedersachsen (1980).
- Institut für evangelisches Kirchenrecht der EKD, Volume VIII Hessen I: Landgrafschaft bis 1582 (1965).
- Institut für evangelisches Kirchenrecht der EKD, Volume XI, Franken, (1961).
- Institut für evangelisches Kirchenrecht der EKD, Volume XII, Schwaben (1963).
- Institut für evangelisches Kirchenrecht der EKD, Volume XIII Altbayern (1966).

- Institut für evangelisches Kirchenrecht der EKD, Volume XIV Kurpfalz (1969).
- Institut für evangelisches Kirchenrecht der EKD, Volume XV Baden-Württemberg I: Hohenlohe (1977).
- Heidelberger Akademie der Wissenschaften, Volume XVI Baden-Württemberg II: Württemberg, Baden u. a. (2004).
- Heidelberger Akademie der Wissenschaften, Volume XVII/1; XVII/2 Baden-Württemberg III/IV: Reichsstädte (2007/09).
- Heidelberger Akademie der Wissenschaften, Volume XVIII Rheinland-Pfalz I: Zweibrücken, Veldenz, Sponheim u. a. (2006).
- Heidelberger Akademie der Wissenschaften, Volume XIX Rheinland-Pfalz II: Wild- und Rheingrafschaft, Leiningen, Wied u. a. (2008).
- Heidelberger Akademie der Wissenschaften, Volume XX/1 Elsass I: Straßburg (2011).
- Heidelberger Akademie der Wissenschaften, Volume IX Hessen II: Landgrafschaft ab 1582, Waldeck, Solms, Frankfurt u. a. (2011).
- Heidelberger Akademie der Wissenschaften,Volume X Hessen III: Nassau, Hanau-Münzenberg, Ysenburg (2012).
- Heidelberger Akademie der Wissenschaften, Volume XX/2 Elsass II: Hanau-Lichtenberg, Colmar, Mülhausen, Weißenburg u.a. (2013).

We also review Richter (1846), *Die evangelischen Kirchenordnungen des sechszehnten Jahrhunderts*. For select additional cities we consult additional sources: Thomas A. Brady, Heiko Augustinus Oberman, James D. Tracy, *Handbook of European History 1400 - 1600: Late Middle Ages, Renaissance and Reformation* (Leiden: Brill, 1994); Joseph Guerber, *Haguenau et la Réforme* (Lyon: Le Roux L.F., 1861); Jürgen Sanowsky, "Vorgeschichte und Anfänge der Reformation in der Ballei Brandenburg des Johanniterordens", in Johannes Mol et al. eds., *Military Orders and the Reformation: Choices, State Building, and the Weight of Tradition* (Amersfoort: Uitgeverij Verloren, 2006); Peter Blickle, *Communal Reformation:*

The Quest for Salvation in Sixteenth-Century Germany (Leiden: Brill, 1992); and Cameron (1991).

A.2 Institutional Treatment and Religious Heterogeneity

In this section, we discuss how our classification protocol treats a small number of cities that reverted to Catholicism or experienced more complicated institutional and religious trajectories. We also discuss how we distinguish Protestant cities that adopted the legal institutions of the Reformation from cities where Protestantism became the dominant religion but the legal institutions of the Reformation were not adopted.

Cities where the institutions of the Reformation did not survive to 1600 are classified as untreated in our analysis. In a few cities, ordinances passed in the 1500s did not establish persistent institutions. Münster and Beckum adopted Reformation laws but experienced early institutional reversals due to re-Catholicization. In both cities, Protestant city councils adhering to Anabaptist ideas passed city-level Reformation laws in the mid-1530s. These Anabaptist experiments, which were atypical in fusing Protestant theology with radical egalitarianism, were crushed militarily by 1536. Both cities reverted to Catholicism and their institutional experiments were quickly undone.

Cities that adopted the institutions of the Reformation and in which these institutions survived to 1600 are classified as "treated" in our baseline analysis. Where we observe institutions established by Reformation laws in effect through 1600, these institutions were typically persistent. Our basic findings on the relationship between institutional change and city growth are robust to different classifications of cities with persistent institutions but complicated religious trajectories.

The city of Amberg in Bavaria provides an instructive example showing how Reformation institutions typically persisted in treated cities once long-run benefits of Reformation institutions became clear, even if a city or its territory came under Catholic rule in the 17th century. Amberg passed a Protestant ordinance in the 1540s and eyewitness accounts from 1564 record boys' and girls' schools, "in which the German Catechism, reading, writing, and arithmetic are diligently taught" (Johnson 2009; p. 32). Catholic authorities preserved the institutional legacy that they inherited when Amberg and the surrounding territory of

Upper Palatine (Oberpfalz) was absorbed by Catholic Bavaria in the early 1600s. During the 1620s, the Jesuits took over formerly Calvinist and Lutheran higher schools in Amberg. More broadly, "the educational infrastructure of the territory impressed Counter-Reformation Catholics when they inherited it after 1621; summing up his reflections... after his visitation in 1656, the (Catholic) Regensburg vicar general noted approvingly that 'in nearly every village, schools are to be found, of which in [Catholic] Bavaria there [otherwise] seems to be a great shortage and decline' " (Johnson 2009; p. 35).

The city of Augsburg provides another example of institutional persistence. In Augsburg, the Reformation was formally adopted and municipal social services were reorganized between 1534 and 1537, when Protestant guildsmen gained control of the city council (Broadhead 1979). In 1548, the Holy Roman Emperor Charles V reestablished a form of Catholic rule in Augsburg that allowed Protestant institutions to persist: rule by an elite of Catholic patricians, with reserved control of key government positions, over a Protestant majority city with active public service institutions shaped by the Reformation. Under this arrangement Augsburg experienced peaceful co-existence without institutional reversals into the 1600s. Stein (2009; p. 73) observes: "At no point did the Catholic-dominated patrician council attempt to re-catholicise the city; instead it governed with discretion in order to minimise [sic] tensions with the Protestant community... The admission practices of all the city-run health care institutions reflected this strategy of confessional tolerance."[53]

Our measure of legal change distinguishes between the formal institutionalization and informal diffusion of Protestantism as the dominant city-level religion. The distinction is significant because previous research has documented that the diffusion of Protestants as the dominant religion had no impact on city growth (Cantoni 2015). An example of a city that became predominantly Protestant but did not adopt a Reformation law is Bautzen. The citizens of Bautzen embraced Lutheranism in the 1520s. The Catholic bishop of the time defied Catholic Church doctrine and invited Protestants to begin sharing the Cathedral in 1524. In 1543, Protestants and Catholics signed a formal contract that still governs times of worship and use of Cathedral space today. Despite becoming a predominantly Protestant

[53] In 1620 – during the Thirty Years War (!) – the head nurse of the Hospital of the Holy Ghost in Augsburg was instructed: "every day... to go from bed to bed and to enquire whether the patients are lacking anything in their care...or whether indeed they require the attentions of a Catholic priest or Protestant pastor" (Stein 2009; p. 74).

city, Bautzen did not adopt a Reformation ordinance.[54]

To the best of our knowledge, there is no systematic evidence for analogous city-level Catholic Church ordinances with provisions regarding schooling. The example of the (Catholic) Bavarian territory-level school ordinance of 1569 is discussed in Strauss (1978). See also Lurz (1907) for details on the Bavarian *Schulordnungen*.

An example of the policy environment in a city treated by Catholic institutional innovations is Fulda. Fulda passed a Catholic poor ordinance in 1587 and established a common chest. Roeck (1999; p. 288) observes that Fulda, "reflects a situation that is in many respects typical. Catholic institutions – a seminary, provastaries, a Jesuit college – were faced with a citizenry that was already predominantly Protestant. The Catholic authorities' goal was to force the Protestants back into the Roman church." In Fulda, religion was used as a screening mechanism to select who was worthy and unworthy of access to social services. Conflicts arose over access and whether Protestants were being excluded. Conflict also arose over control of the Fulda common chest, with the Jesuits being widely accused of diverting resources from the common chest to support their own students instead of the needy. These conflicts were resolved only with the complete re-Catholicization of 1603.

A.3 Schooling

Several dimensions of educational institutions are of interest for economists. First, urban schools admitted poor children free of charge (Strauss 1988). More broadly, the institutions were designed to recognize and promote talent. "Everyone held talent to consist of a quick intellect, a tenacious memory, a strong desire to learn, the ability to persevere in hard work, good health, and a tranquil spirit. A boy who possessed these qualities had the makings of a theologian, lawyer, or scholar and should not be left to waste his gifts in a workshop or in the field" (Strauss 1978; p. 179). Second, attendance was compulsory. For example, in Ulm written excuses were required from parents for truant children; in Stralsund, the beadle was sent to locate absent children; and in Hamburg, municipal authorities began paying for school heating when it was discovered that parents were reluctant to have their kids go to

[54] On the absence of a law, Speer (2014; p. 51) observes that catholic officials were still in charge of matters of marriage. Only from 1565 on, the city council passed some resolutions regarding church matters but no church ordinance. Bautzen is an example of a city that became predominantly Protestant and is classified as such in Cantoni (2012) but did not adopt Protestant law.

cold school in winter. Third, basic primary schooling for boys began at age six or seven and typically lasted five or six years. Fourth, school days included morning and afternoon sessions. School days typically began at 7AM (8AM in winter). Students went home for lunch and returned to school 1PM-4PM. Fifth, vacations were relatively short. Classes were held throughout the calendar year, with vacations at Christmas and Easter. In city schools, "Vacations were rare. But it was customary to cancel afternoon classes during the hottest weeks of summer and to take a holiday at the time of the annual fair" (Strauss 1978; p. 187). Wednesday afternoons and Sundays were free.

The institutional changes we study in this paper were associated with subsequent differences in education and service provision, including school construction. Figure A1 compares new school building in cities with and without Protestant church ordinances across the 1500s. Figure A1 shows that legal changes were associated with subsequent differences in fixed educational investments.[55] However, in many Protestant cities new schools were established in former Catholic Church buildings (Ocker 2006). Hence new school construction should be taken as just one indicator of the way institutional change was associated with on-the-ground shifts in education and service provision.

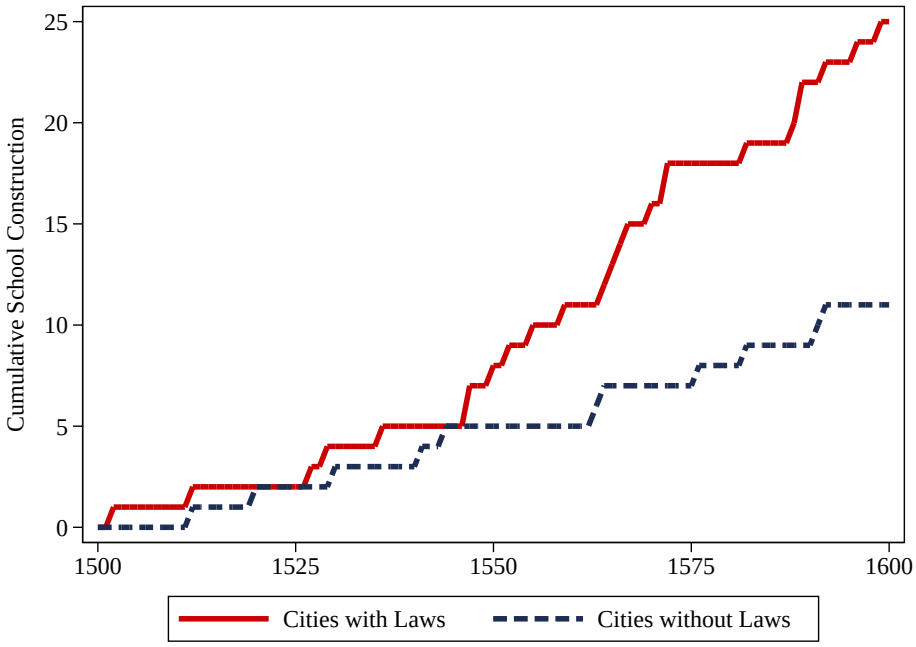
A.4 Data on Upper Tail Human Capital from the *Deutsche Biographie*

We construct data on upper tail human capital from the *Deutsche Biographie*, the authoritative biographical dictionary for all regions of Europe in which German is spoken and German culture is prevalent (Hockerts 2008).

In this section we discuss how the *Deutsche Biographie* was designed to provide a comprehensive, universal, biographical dictionary of important economic, political, and cultural figures within German speaking Europe. We highlight the fact that the *Deutsche Biographie* was constructed with the specific objective to record the biographies of important economic, political, and cultural figures from all time, places, and social and religious groups within German-speaking Europe. We provide a review of the scholarly literature examining

[55]Evidence on school construction is assembled from multi-volume *Deutsche Städtebuch*, which provides detailed histories urban development for of all cities in Germany. These construction data motivate and corroborate our analysis of legal change, upper tail human capital, and city growth which examines data from other sources.

Figure A1: New School Construction in German Cities



This graph shows the cumulative number of new schools constructed in cities that did and did not pass laws formalizing the public goods institutions of the Reformation. Data on school construction are from the *Deutsches Städtebuch* and cover the 239 German cities we study in this paper as described in the text.

the scope and representativeness of the *Deutsche Biographie*. We then describe the nature of the information the *Deutsche Biographie* contains and how we use this information to construct our database on upper tail human capital.

We devote a separate section of the Appendix to documenting that our results are not driven by selective inclusion of "marginal" important individuals. To do this, we restrict the analysis to the most influential people *within* the *Deutsche Biographie* – for whom selective inclusion is not plausible (for details see Appendix C).

The Deutsche Biographie as a Data Source

Deutsche Biographie is the definitive biographical dictionary of political, cultural, and economic figures in German history. The design and objectives of the *Deutsche Biographie*, scholarly assessments of its coverage, and our econometric results all strongly weigh against the possibility that our key findings are driven by a selective inclusion process into the *Deutsche Biographie*.[56]

[56]In addition, we document that the shifts in human capital in cities treated by Reformation laws were in the specific sectors targeted by these laws (government administration and church employment). The fact that the effects were in the targeted sectors and not universal across occupations suggests that our results

The current *Deutsche Biographie* builds on and extends the original *Allgemeine Deutsche Biographie*, a 56 volume reference collection published 1875-1912. The online portal, www.deutsche-biographie.de, which we use to collect biographical data, also includes articles published in *Neue Deutsche Biographie*, the successor of the *Allgemeine Deutsche Biographie*, that was initiated in 1953. The coverage of *Allgemeine Deutsche Biographie* included all German-speaking parts of Europe. It was organized with entries on important Jewish figures frequently written by Jewish scholars, entries on Austrians written by Austrian experts, and entries on Catholics written by experts on Catholic culture.

The *Deutsche Biographie* is regarded as the authoritative biographical dictionary for all regions of Europe in which German is spoken and German culture is prevalent. The *Deutsche Biographie* records individuals who made a "significant impact on developments in politics, economics, social life, scholarship, technology or the arts."[57] A strict structure is imposed on all biographical articles, simplifying the extraction of relevant information. In particular, the available information contained in the articles is structured as follows: 1) the full name, occupation, data and place of birth, date and place of death, tomb, religious denomination, 2) family (genealogy), 3) Career, achievements, critical evaluations, 3) List of selected works, 4) List of selected works, 5) list of sources and secondary literature, 6) references of portraits, 7) name of the author.[58]

The *Deutsche Biographie* was designed to address and eliminate selective inclusion. The original *Allgemeine Deutsche Biographie* was designed to document the "thoroughly pluralistic foundation" of German cultural achievement and to that end explicit effort was made to include religious and social minorities (Hockerts 2008: p. 238).[59] The editorial inclusion criteria targeted everyone, "whose deeds or works contributed to the development of Germany in history, science, art, trade, or business, in short in every corner of political or cultural life" (von Liliencron and Wegele 1875; p. V-VI). However, evidence suggests that

are not picking up a general effect of cities with public goods institutions either producing, attracting, or recording more upper tail human capital in general.

[57] See http://www.ndb.badw-muenchen.de/ndb_aufgaben_e.htm.

[58] See http://www.ndb.badw-muenchen.de/ndb_richtlinien.htm.

[59] In their preface of the first volume the editors of the *Allgemeine Deutsche Biographie*, Rochus Freiherr von Liliencron und Franz Xaver Wegele, describe the purpose of this project as "scientific reference for scholars and the 'educated, interested public'. " To fulfill this purpose, individuals for which an (exclusively or mostly) scientific interest exists were included to help users with their research but the articles were to be written in a way that they were "accessible" for the general public.

the original *Allgemeine Deutsche Biographie* over-represented people active in the humanities and social sciences and under-represented scientists, entrepreneurs, and technicians (Hockerts 2008). The *Neue Deutsche Biographie* was initiated in 1953 to update the *Allgemeine Deutsche Biographie* and was specifically designed to eliminate the prior over-representation of activities in the humanities and social sciences (Hockerts 2008). Hockerts (2008; p. 257-58) further observes that the *Neue Deutsche Biographie* includes, "relatively unknown, but yet important personalities from the back rows....which is the real value of a biographical lexicon." As such, the current edition and digital version, which we use in this paper, attempt to be universal in coverage and contain both new entries as well as updated information on entries in prior editions.

It is unlikely that the selective survival of historical records explains our results. Two principal sources of selection are notable. First, selection would problematic if *subsequent* historical shocks differentially hit and destroyed records of upper tail human capital achievement in cities that did not adopt institutional change. However, if anything, these differentially struck Protestant cities with laws (e.g. during the Thirty Years War). More broadly, on the nature of the documentary evidence, Parker (1997; p. 187) observes, "Seventeenth-century Germans were scrupulous record-keepers, and the Thirty Years' War did little to change their habits of meticulous documentation. Here and there crucial records were destroyed by negligence or acts of war, but enough documents have survived to provide vast amounts of data about local conditions." These record-keeping practices support the integrity of the data. Second, selection would be problematic if cities that adopted institutional change simply kept better records and as result have greater numbers of *marginal* upper tail human capital individuals. We address this question by restricting the data to top super-stars *within* the *Deutsche Biographie* where this type of selection is not plausible.

Construction of Database on Upper Tail Human Capital

Scope of the Data

The entries in the *Deutsche Biographie* record key information on individuals, including name, date and place of birth and death, a list of professional and career activities, and in most cases a biographical essay.

For our study of the formation of human capital, we restrict our study to people born in our baseline set of 239 cities between the 1280s and 1770s with birthdate known. For our study of migration, we restrict our study to people migrating to our 239 cities and dying between the 1320s and 1810s. Our evidence on migrants includes people whose birthplaces are rural towns, cities outside Germany, and in some cases unknown. Our evidence on migrants also includes people whose dates of birth are not known.

Our baseline data measure the number of locally born upper tail human capital individuals from a given city-period and the number of upper tail human capital migrants observed in a given city-period. We examine the locally formation of human capital using birthdates and at age 40, as an approximation for peak productivity years for mature adults. We identify migrants as people born in location i and dying in one of the cities in the data j ($j \neq i$). We examine migrants using dates of death because the timing of migration is not known for most migrants.

Classification of Types of Upper Tail Human Capital

To construct measures of specific types of human capital, we classify individual occupations recorded in the *Deutsche Briographie*. The occupations we classify are recorded in the *Deutsche Biographie* as *beruflebensstellung* – literally "professional life position." The *Deutsche Biographie* records thousands of different individual professions and occupations (e.g. councillor, city councillor, mayor, patrician, teacher, professor, school-master, physicist, merchant, banker, preacher, theologian, etc.).

We classify professions in occupational sectors as follows: (1) the *government* sector comprises all government, public administration, law, and legal services careers; (2) the *church* sector comprises all religious or Church occupations; (3) the *education* sector comprises all research, teaching, and science occupations; (4) the *business* sector comprises all careers in trade, finance, services, crafts, proto-industry, and industry; (5) the *arts* sector comprises all occupations in the visual, performing, and literary arts; (6) the *medicine* sector comprises all medical occupations.[60]

To illustrate how we classify upper tail human capital careers, Table A1 presents the top

[60]In addition to these principal sectors, a number of individuals had military careers or were members of the nobility. We see some evidence suggesting Protestant cities with Reformation laws may have produced more notables with military careers during the Thirty Years War, but the evidence is imprecise and we focus our discussion here on the leading six sectors.

careers by frequency for the principal occupational sectors and the share of careers by sector.

When we construct our baseline data for analysis, we take the career sector as the unit to be classified. Of the individuals observed born between 1280 and 1769 in the *Deutsche Biographie*, approximately 62 percent have one career occupation and fewer than 100 have four recorded occupations. In the data, approximately 78 percent of people born 1280 to 1769 pursued their significant professional and life activities in just one sector and 98 percent pursued their activities in no more than two sectors.[61] Most people with multiple occupations remain in one sector. For example, we observe people who were priests and theologians, both *church* occupations. However, in instances where a given individual worked in two sectors, his or her city is credited as having both upper tail human capital careers. An example of this kind of human capital, career structure, and data classification is Christian Lorentz von Adlershelm (1608-1684) of Leipzig, who was a jurist and merchant (*Kaufmann*) and is classified as *government* and *business*. An example of human capital in both education and government administration is Albert Philipp Frick (1733-1798), who was active as a professor (*Hochschullehrer*) and as a jurist. An example of human capital in *business* and *government* is Detlev Karl Graf von Einsiedel (1737-1810), who was a state functionary in Saxony (*Sächsischer Minister*) and an industrialist in the metal industries (*Eisenindustrieller*).

[61] Approximately 62 percent have one career occupation and fewer than 100 have four recorded occupations.

Table A1: Classification of Upper Tail Human Capital by Occupational Sector – Illustration

[1]	[2]	[3]
Government (20%)	***Church*** (15%)	***Education*** (16%)
Jurist (Jurist)	Theologe (Theologian)	Historiker (Historian)
Politiker (Politician)	Evangelischer Theologe (Evangelical Theologian)	Philosoph (Philosopher)
Diplomat (Diplomat)	Pfarrer (Pastor)	Mathematiker (Mathematician)
Patriziat (Patrician)	Lutherischer Theologe (Lutheran Theologian)	Philologe (Philologist)
Bürgermeister (Mayor)	Jesuit (Jesuit)	Pädagoge (Pedagogue)
Beamter (Official)	Katholischer Theologe (Catholic Theologian)	Humanist (Humanist)
Staatsmann (Statesman)	Prediger (Preacher)	Schulmann (Teacher)
Geheimrat (Privy)	Kardinal (Cardinal)	Astronom (Astronomer)
Ratsherr (City Councillor)	Bischof (Bishop)	Botaniker (Botanist)
Abgeordneter (Deputy)	Reformierter Theologe (Calvinist Theologian)	Physiker (Physicist)
[4]	[5]	[6]
Business (18%)	***Arts*** (26%)	***Medicine*** (5%)
Kupferstecher (Copper Engraver)	Maler (Painter)	Mediziner (Doctor)
Kaufmann (Merchant)	Schriftsteller (Writer)	Arzt (Physician)
Verleger (Publisher)	Künstler (Artist)	Apotheker (Pharmacist)
Buchhändler (Book-seller)	Komponist (Composer)	Chirurg (Surgeon)
Baumeister (Builder)	Architekt (Architect)	Anatom (Anatomist)
Drucker (Printer)	Bildhauer (Sculptor)	Gynäkologe (Gynecologist)
Goldschmied (Goldsmith)	Dichter (Poet)	Pharmazeut (Pharmacist)
Buchdrucker (Book printer)	Ital. Maler ('Italian' Painter)	Wundarzt (Surgeon)
Bankier (Banker)	Zeichner (Draftsman)	Physiologe (Physiologist)
Publizist (Publicist)	Musiker (Musician)	Optiker (Optician)

This table presents the classification of upper tail human capital by occupational sectors for the most frequent careers for individuals in the *Deutsche Biographie*. Careers (occupations) are directly recorded by the *Deutsche Biographie*. We manually classify careers into occupational sectors for analysis in this research. This table presents the top careers for the six leading sectors in our data. We include in parentheses an English language translation of the German language career (occupation) designation. Top careers account for 48% of careers in the data. See text for further details.

A.5 Data on City Population

This section describes the population data on which we rely and additional evidence on city populations.

This paper relies on Bairoch, Batou, and Chèvre (1988) as the principal source for data on city populations. The Bairoch, Batou, and Chèvre (1988) data (the "Bairoch data") are designed to record the populations of all European cities that had populations of at least 5,000 some time between 1000 CE and 1800 CE. The data are recorded at 100 year intervals through 1700 and then at 50 year intervals through 1850. Bairoch, Batou, and Chèvre (1988; p. 289) record data on the populations of urban agglomerations and make a special effort to include, "the 'fauborgs', the 'suburbs', 'communes', 'hamlets', 'quarters', etc. that are directly adjacent" to historic city centers.[62]

The Bairoch, Batou, and Chèvre (1988) database provides an unbalanced panel. Table A2 documents that over 70 percent of cities have population unobserved in one or more periods 1500, 1600, 1700. Over the period 1500 to 1800 only 66 of 239 cities are in the balanced panel. Over the period 1400 to 1800 37 of 239 cities are in the balanced panel. Our baseline analysis examines long-run populations for all 239 cities, including those that were small and for which population was not observed in 1500 in Bairoch, Batou, and Chèvre (1988).

The principal reason city populations are not observed for some city-years in the Bairoch data is because locations were in those periods small towns or villages with low and unrecorded total population figures. However, because we examine long-run (1800) population conditional on initial population *categories* we naturally wonder whether there is unobserved variation in the populations of towns for which data is not recorded in 1500. We wonder first whether unobserved populations varied systematically in 1500: were the unobserved that got treated already larger than the unobserved that did not get treated? We wonder second whether some cities are unobserved because they recently experienced negative shocks (wars, fires, etc.) and as a result might have been expected to grow relatively quickly as they recovered.

[62]To construct the data Bairoch et al. drew on primary and secondary sources. Prior to publication the data were reviewed by 6 research institutes and 31 regional specialists in urban history. The leading alternate source of data on city populations in European history is the database in de Vries (1986), *Urbanization in European History*. However, the data in de Vries (1986) only cover cities with at least 10,000 inhabitants some time between 1500 and 1800 and are only recorded at one-hundred year intervals starting in 1500.

Table A2: Summary of Cities in Balanced Panel for Population Data

	[1]	[2]	[3]	[4]	[5]	[6]
	Cities with Law		Cities without Law		All Cities	
	Number	Share	Number	Share	Number	Share
Balanced 1300-1800	20	0.19	10	0.07	30	0.13
Balanced 1400-1800	24	0.23	13	0.10	37	0.15
Balanced 1500-1800	42	0.41	24	0.18	66	0.28
Total Cities	103	1.00	136	1.00	239	1.00

This table records the number and share of cities with population observed each period in Bairoch, Batou, and Chèvre (1988). The row "Balanced 1300-1800" reports the number and share of cities with population observed every 100 years starting in 1300 and ending in 1800. The row "Balanced 1400-1800" reports the number and share of cities with population observed every 100 years starting in 1400 and ending in 1800. The row "Balanced 1500-1800" reports the number and share of cities with population observed every 100 years starting in 1500 and ending in 1800. "Total Cities" reports the total number of cities in the data examined in this research.

To address these questions and show that this selection does not explain our results, we gather city-by-city data on the historic populations and shocks experienced by locations with population unobserved in 1500. Below we show that our key findings hold for cities where we *do* observe population in 1500 and when we control for prior shocks and for prior evidence on city sizes which we present in this section.

We gather evidence on locations for which population data in 1500 are not observed in Bairoch, Batou, and Chèvre (1988) from the *Deutsches Städtebuch*. The *Deutsches Städtebuch* provides evidence on city populations and city sizes for all periods where these data are available. The *Deutsche Städtebuch* data (i) confirm that the cities that are not observed in Bairoch, Batou, and Chèvre (1988) were small settlements and (ii) document when settlements first appear with population data. Table A3 provides evidence on each individual city for which population data are missing in 1500, which in our baseline cross-sectional regressions we class together as the "population missing" category.

Table A3: Evidence on Cities with Data Missing in 1500

City	Evidence on Population and Size
Amberg	Population of 3,500 in 1460 and 4,000 in 1548 (Vol. 5.2)
Andernach	First record is 1,530 in 1690 (Vol. 4.3)
Anklam	Population of 3,000 in 1350 and 5,000 in 1565 (Vol. 1)
Ansbach	First record is 3,950 in 1713 (Vol. 5.1)

Continued on next page

Table A3 – continued from previous page

City	Evidence on Population and Size
Aschaffenburg	167 hearths in 1470 (Vol. 5.1)
Aschersleben	Population of 3,213 in 1526, however the town was impacted by military conflict during the Peasants' War in 1525 (Vol. 2)
Hersfeld	First record is 557 hearths in 1585 (Vo. 4.1)
Bad Kreuznach	First record is 3,500 in 1601 (Vol. 4.3)
Baden-Baden	First record is 1,900 in 1790 (Vol. 4.2)
Wuppertal-Barmen	No information
Beckum	No information
Bingen	First record is 3,000 in 1666 (Vol. 4.3), fires 1490, 1540, 1634 leading to destruction
Bonn	First record is 12,644 in 1784 (Vol. 3.3)
Brandenburg	First record is 12,000 in 1625 (Vol. 1)
Breisach	First record is 4,600 in 1697 (Vol. 4.2), Black Death, destructions by several floodings
Brieg	First record is 36+ household hearths in 1250, 3,600 in 1675 (Vol. 1)
Burg b.M.	First record is 3,795+ in 1723 (Vol. 2)
Burscheid	No information
Berlin-Charlottenburg	First record is 1,568 in 1722 (Vol. 1)
Berlin-Spandau	First record is 1,100 in 1386, 2,000 in 1573 (Vol. 1)
Clausthal-Zellerfeld	First record is 3,000 in 1637 (Vol. 3.1)
Coesfeld	First record is 2,061 in 1795 (Vol. 3.2)
Cottbus	First record is 2,000 in 1400, 490 in 1599, 3,205 in 1750, many destructions by fire and epidemics during 16th century (Vol. 1)
Darmstadt	No information
Duisburg	First record is 2,983 in 1714, destructions by fire 1473 and 1499 (Vol. 3.3)
Ebersbach	First record is ca 100 in 1510 (Vol. 2)
Eichstaett	First record is 1,450 in 1637 (Vol. 5.1)
Eilenburg	280+ hearths in 1400, 900 people in 1530 (Vol. 2)
Emmerich	First record is 5,660 in 1722 (Vol. 3.3)
Erlangen	First record is 7,939 in 1752 (Vol. 5.1)
Eschweiler	First record is 3,386 in 1750 (Vol. 3.3)
Forchheim	First record is 337 hearths in 1653 (Vol. 5.1)
Frankenthal	First record is 300 in 1580, grows rapidly in the 18th century (Vol. 4.3)
Freising	First record is 4,954 in 1752 (Vol. 5.2)

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Table A3 – continued from previous page

City	Evidence on Population and Size
Fulda	First record is 3,000 in 1648 (Vol. 4.1)
Gelnhausen	Many epidemics (Vol. 4.1)
Giessen	First record is 240 hearths in 1495, 1546: 500 hearths, 1669: 3531 persons (Vol. 4.1)
Glogau	First record is 2,000 in 1648, many epidemics between 1025-1676 (Vol. 1)
Glueckstadt	No information
Goeppingen	First record is ca 2,000 in ca 1600 (Vol. 4.2)
Landsberg	First record is ca 2,500 in 1600 (Vol. 1), even though there are no entries around 1500 the book mentions that there are numerous destructions by fire and war
Goldberg	First record is 3,940 in 1756 (Vol. 1)
Greifswald	First record is 4,611 in 1767 (Vol. 1)
Guben	First record is 4,000 in 1600 (Vol. 1)
Guestrow	First record is 360 hearths in 1536, destructions by fire 1503, 1508, 1512 (Vol. 1)
Halberstadt	First record is 1,464+ hearths in 1531, 11,000 persons in 1589, many epidemics between 1358-1682 (Vol. 2)
Hameln	First record is 1,950 in 1585, multiple destructions by fire and floods - however, none recorded around 1500
Hamm	First record is 7 hearths in 1350, 3,250 persons in 1719, many fires between 1287-1762 (Vol. 3.2)
Harburg	No information
Neviges	No information
Hattingen	First record is 178 in 1584 (many deaths due to epidemic), 1,251 in 1714 (Vol. 3.2)
Helmstedt	First record is 4,687 in 1790, epidemic in 1506 and destruction in 1553 (Vol. 3.1)
Herford	First record is 2,767 in 1719, many epidemics and destructions throughout centuries (Vol. 3.2)
Hof	First record is 2,187 in 1495, 2,400 in 1502, many epidemics and destructions (Vol. 5.1)
Hueckeswagen	First record is 4,364 in 1792 (Vol. 3.3)
Ibbenbüren	No information
Hirschberg	First record is 436 hearths in 1543, 5,819 persons in 1742 (Vol. 1)
Kaiserslautern	First record is 2,120 in 1611 (Vol. 4.3)
Karlsruhe	First record is 1,994 in 1719, town was founded in 1715 (Vol. 4.2)
Kaufbeuren	No information but epidemics
Kempten (Allgaeu)	First record is 3,000 in 1423, 5,000 in 1618 (Vol. 5.2)
Glatz	First record is 1,000+ in 1310, 3,647 in 1742 (Vol. 1)

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Table A3 – continued from previous page

City	Evidence on Population and Size
Koblenz	First record is 674 hearths in 1560, 1,409 persons in 1663, destructions and epidemics (Vol. 4.3)
Koenigswinter	No information
Koethen	First record is 2,000 before 1418, ca 4,000 in 1758 - sheet indicates reformation in 1508 (possible?) (Vol. 2)
Konstanz	First record is ca 5,000 in 1418, 3,714 in 1763, many epidemics and destructions (Vol. 4.2)
Kuestrin	First record is 1,490 in 1623, town nearly completely destroyed by fire in 1490, many floodings throughout time
Krefeld	First record is 350 in 1604, 1,350 in 1650, 4,576 in 1740, many destructions by war (Vol. 3.3)
Krempe	First record is 4,000-5,000 in ca 1600 (Vol. 1)
Landshut	First record is 9,000 in 1450, 8,600 in 1560, many epidemics (Vol. 5.2)
Langensalza	First record is 5,029 in 1414, 3,930 in 1447, 3,145 in 1551, fire in 1506 (Vol. 2)
Leer	First record is 3,500 in 1600 (Vol. 3.1)
Lauban	First record is 3,915 in 1706 (Vol. 1)
Ludwigsburg	No information
Lüneburg	First record is 14,000 in 1618, many epidemics (Vol. 3.1)
Mannheim - Sandhofen	First record is ca 570 in 1450, ca 800 in 1572, 3,000 in 1663 (Vol. 4.2)
Marburg	First record is 3,404 in 1696, many epidemics 16th/17th century (Vol. 4.1)
Marienberg	First record is 1,100 in 1550, many epidemics (Vol. 2)
Merseburg	First record is 5,000 in 1600, many epidemics (Vol. 2)
Muelheim a.d. Ruhr	No information
Nauen	First record is 1,700 in 1705, 1514 severe fire (Vol. 1)
Neubranden-burg	First record is 4,711 in 1797 (Vol. 1)
Neuburg a.d. Donau	No information
Neuss	First record is 3,555 in 1771 (Vol. 3.3)
Neustrelitz	First record is 1,619 in 1745 (Vol. 1)
Norden	No information
Neisse	First record is 4,500 in 1424, 7,344 in 1551 (Vol. 1)
Offenbach	First record is 480 in 1540 (Vol. 4.1)
Oppenheim	First record is 1,000+ between 600-1118, 4,000-5,000 between 1400s and 1600s (Vol. 4.3)
Paderborn	First record is 500+ in 1222, 4,500 in 1551, 1506 severe fire (Vol. 3.2)
Pforzheim	1501 epidemic (Vol. 1)

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Table A3 – continued from previous page

City	Evidence on Population and Size
Pirna	First record is 600+ in 1547 (Vol. 2)
Prenzlau	First record is 130 in 1643, complete destruction during war in 1483 (Vol. 1)
Radevormwald	First record is 2,239 in 1792, complete destruction except church by fire in 1525 (Vol. 3.3)
Ratzeburg	First record is 2,000 in 1693 (Vol. 1)
Reichenbach i. V.	First record is 476 in 1531 (Vol. 2)
Remscheid	No information
Rheine	No information
Rheydt	First record is ca 450 in 15. Jh, 650 in 1532
Rothenburg o.d. Tauber	First record is 6,000 in 1400, 5,800 in 1770, destruction by fire in 1791 and 1501
Rottenburg	First record is 3,768 in 1394, 2,750 in 1581 (Vol. 4.2)
Alt-Saarbruecken	First record is 188 hearths in 1542 (Vol. 4.3)
Saarlouis	First record is 700 hearths in 1683, many epidemics (Vol. 4.3)
Sagan	First record is 850 in 1618, destructions by fire in 1472 and 1486, many epidemics (Vol. 1)
Salzwedel	First record is ca 6,800 in 1418, 4,100 in ca 1448, 3,589 in 1730. Epidemics (Vol. 2)
Schwaebisch Gmuend	Epidemic in 1501 (Vol. 4.2)
Schwaebisch Hall	First record is 1,124 in 1396, 1,223 in 1597 (Vol. 4.2)
Schweinfurt	First record is 3,255 in 1557 (Vol. 5.1)
Schwerin	First record is 293 hearths in 1633, many destructions by fire since 1531 (Vol. 1)
Solingen	1492 and 1521 fatal fires (Vol. 3.3)
Stade	First record is 2,716 in 1675, many sieges since 1628 (Vol. 3.1)
Straubing	First record is 7,531 in 1787 (Vol. 5.2)
Stuttgart	First record is 4,000 in 1400, many severe epidemics (Vol. 4.2)
Velbert	No information
Verden	First record is 4,300 in 16. Jh, many sieges since 1626 (Vol. 3.1)
Warburg	First record is 4,000-5,000 during middle ages (Vol. 3.2)
Weissenfels	First record is 2,500 in 1622 (Vol. 2)
Wesel	First record is 2,200-2,300 in 1241, epidemics (Vol. 3.3)
Wetzlar	First record is 6,000 in 1350, 1,600 in 1567 (Vol. 4.1)
Wiesbaden	First record is 915 in 1629 (Vol. 4.1)
Wolfenbüttel	First record is 200 in 1548, 12,000-14,000 in 1748 (Vol. 3.1)
Wuelfrath	First record is 1,529 in 1792 (Vol. 3.3)

Continued on next page

Table A3 – continued from previous page

City	Evidence on Population and Size
Wuppertal-Elberfeld	First record is 2,500 in 1610 (Vol. 3.3)
Xanten	First record is 1,716 in 1721 (Vol. 3.3)
Zeitz	First record is 630 hearths in 1615 (Vol. 2)
Zittau	First record is 5,000 in 1400, less than 8,000 in 1568, many epidemics during 16th and 17th century with deaths of several thousands in many years (Vol. 2)
Zuellichau	First record is 250 hearths at the beginning town history (1319), 580 hearths around 1700, no information around 1500
Zweibruecken	First record is 5,459 in 1774, fatal fire in 1470 (Vol. 4.3)
Zwickau	First record is ca 3,900 in 1462, 7,677 in 1530, war from 1418 until 1486 (Vol. 2)

Table A3 also shows several cities that had attained populations above 5,000 at previous points in time. Our baseline results are robust to excluding these cities from the analysis or to instead incorporating indicator variables for population size observed in the *Städtebuch* at different moments in time that absorb variation in past population missed in the Bairoch data.

A.6 Territorial and Jurisdictional Status of Cities

Our principal source on the constitutional status of cities is the 1521 tax register (*Reichsmatrikel*) of the Holy Roman Empire. The *Reichsmatrikel* lists the cities constitutionally designated as free and imperial cities (*Freie und Reichsstädte*). For on-line list, see: http://de.wikisource.org/wiki/Reichsmatrikel_von_1521 (downloaded December 2012). In analysis where we use data from Cantoni (2012) we use Cantoni's coding of *Reichstädt* which diverges in a few instances from the 1521 Reichsmatrikel. (These divergences reflect the changing and somewhat ambiguous jurisdictional status of a handful of cities.)

We locate cities in historic territories as of 1500 using GIS maps from Euratlas (2008), *Periodical Historical Atlas of Europe*. These data are: Copyright 2008, Christos Nüssli, Euratlas www.euratlas.com, utilization license of 2009. The geography of these territories is shown in Figure 1 in the main text. In the econometric analysis, we cluster standard errors

at the Euratlas territory ("holder") level. Euratlas territories capture geographic proximity, but are not a direct measure of territorial institutions. Complicated and heterogeneous institutional arrangements in some cases applied even to cities within a given territory and even within cities. See Whaley (2012) and Roeck (1999) for a review.

A.7 *Deutsches Städtebuch* Data

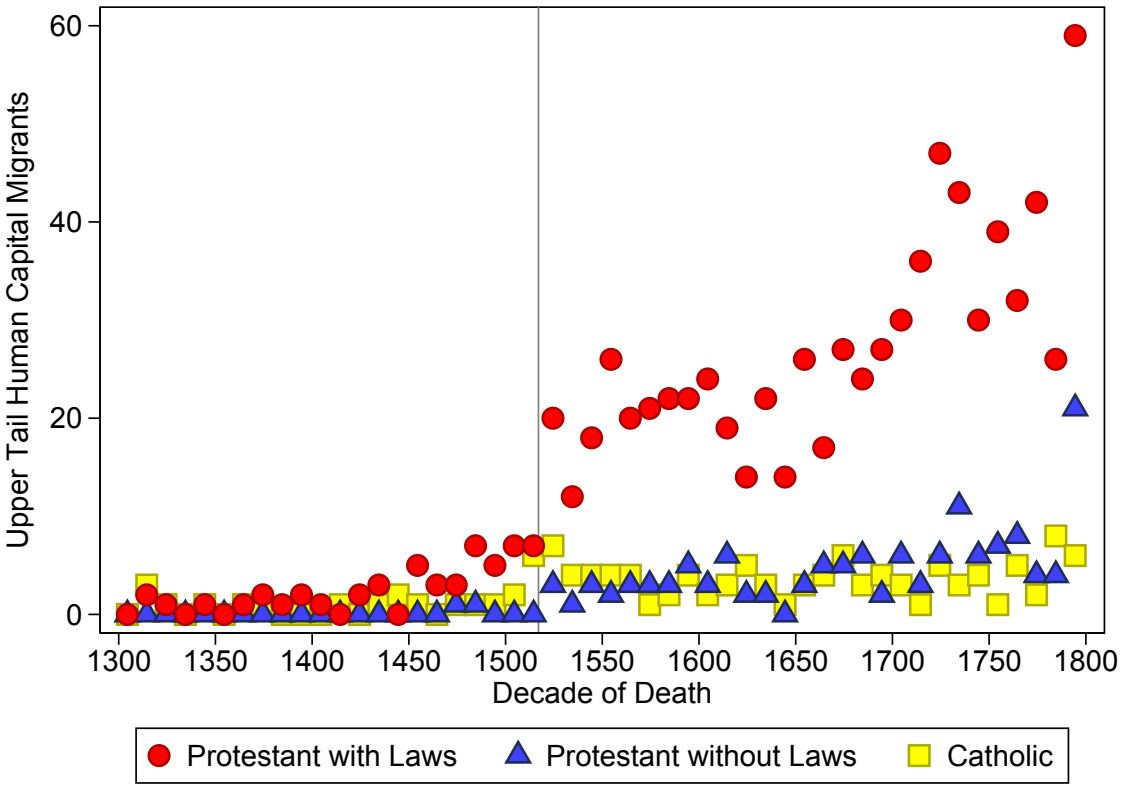
The *Deutsches Städtebuch* provides information on notable historic construction events in German cities (Cantoni, Dittmar, and Yuchtman 2015). We present information on school construction from the *Deutsches Städtebuch*. Volume I: Erich Keyser (editor), *Deutsches Städtebuch, Handbuch städtischer Geschichte. Bd. I Nordostdeutschland* (Stuttgart: Kohlhammer, 1939). Volume II: Erich Keyser (editor), *Deutsches Städtebuch, Handbuch städtischer Geschichte. Bd. II Mitteldeutschland* (Stuttgart: Kohlhammer, 1941). Volume III: Erich Keyser (editor), *Deutsches Städtebuch, Bd. III Nordwest-Deutschland. 2, Westfalen. Westfälisches Städtebuch* (Stuttgart: Kohlhammer, 1954). Erich Keyser (editor), *Deutsches Städtebuch, Bd. III Nordwest-Deutschland. 3, Landschaftsverband Rheinland. Rheinisches Städtebuch* (Stuttgart: Kohlhammer, 1956). Volume IV: Erich Keyser (editor), *Deutsches Städtebuch, Bd. IV Südwest-Deutschland. 1, Land Hessen. Hessisches Städtebuch* (Stuttgart: W. Kohlhammer, 1957). Erich Keyser (editor), *Deutsches Städtebuch: Handbuch städtischer Geschichte, Bd. IV Sudwest-Deutschland. 2, Land Baden-Wurttemberg. Teilbd. Württemberg. Württembergisches Städtebuch* (Stuttgart: Kohlhammer, 1962). Erich Keyser (editor), *Städtebuch Rheinland-Pfalz, Saarland, Mainz, Stadtkreis* (Stuttgart: W. Kohlhammer, 1964). Volume V: Erich Keyser (editor), *Deutsches Städtebuch, Bd. V Bayern. 1, Bayerisches Stadtebuch* (Stuttgart: Kohlhammer, 1971). Erich Keyser (editor), *Deutsches Städtebuch, Bd. V Bayern. 2, Bayerisches Stadtebuch* (Stuttgart: Kohlhammer, 1974).

B Upper Tail Human Capital

In this section we provide additional discussion and evidence on the relationship between the public goods institutions and upper tail human capital. We first provide additional discussion of our baseline results and research design. We then examine how the effects of institutions on upper tail human capital compare to the effects of the plagues we have identified as institutional shifters. We also present additional regressions to characterize the impact of the plague on the level and growth rate of human capital formation.

B.1 Disaggregating Untreated Cities

Figure B1: The Migration of Upper Tail Human Capital by Institutions and Religion



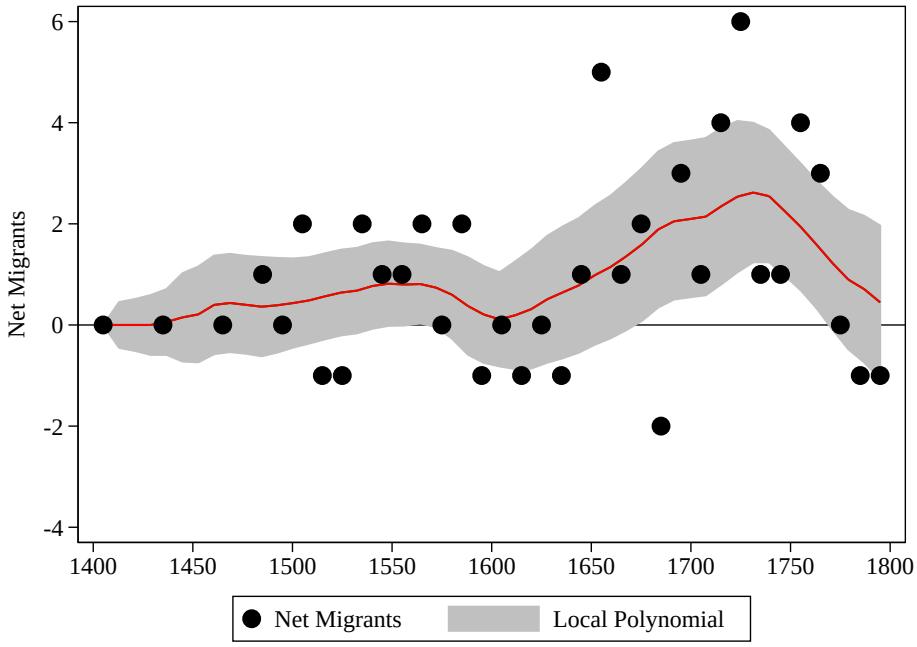
This graph plots the number of migrants observed in the *Deutsche Biographie* at the decade level for cities grouped as (1) Protestant cities with Reformation laws, (2) Protestant cities without Reformation laws, and (3) Catholic cities also without laws. Migrants are identified as people living and dying in town i but born in some other location j. The vertical line is at 1518, the year Luther's theses began circulating widely.

We motivate the analysis in the main body of the paper with a figure showing that cities that got Reformation laws subsequently began differentially attracting upper tail human capital migrants. A first question this analysis raises is whether "untreated" Protestant and "untreated" Catholic cities developed similarly.

Figure B1 confirms that the key variation is explained by the distinction between cities with and without laws by disaggregating the data to show the number of migrants observed in Protestant cities with laws, Protestant cities without law, and Catholic cities.

B.2 Net Migration from Untreated to Treated Cities

Figure B2: Net Migration from Cities Without Laws to Cities With Laws



This graph plots net migration from cities without Reformation laws to cities with Reformation laws. The graph plots the raw data on net migration at the decade level as observed in the *Deutsche Biographie* in the 239 cities in our data. The graph also plots the local polynomial regression estimate and associated 95% confidence interval for the relationship between net migration and time. Net migration is defined as the net flow of migrants into cities "ever treated by Reformation Law" from cities where these laws were not passed.

Our baseline results study total migration. Total migration flows were overwhelmingly driven by migration from small towns into cities, not by migration from untreated cities to treated cities. Figure B2 plots net migration at the decade level and shows that (1) net migration was always small and (2) that net migration into treated cities was essentially

zero until 1700, became positive for a few decades, and returned to zero.

B.3 Defining the Treatment Period

In the main body of the paper, we use regression analysis to quantify differences in upper tail human capital across cities. To interpret our findings, it is is important to understand the research design. In the regression analysis, we study the formation and migration of upper tail human capital over 50 year periods. To study the formation of human capital we assign individuals to the fifty-year period in which they were 40 years old.[63] Fifty-year periods are by design broad "bins" that enable us to consider upper tail human capital formation before and after the Reformation. Our baseline analysis associates people with their town of birth as a summary measure of how human capital was associated with city locations.[64]

There are several considerations that relate to this periodization and how we should interpret the formation of talent and the nature of the institutional treatment. First, a number of individuals became famous due to their activities as early reformers, theologians, and Protestant activists. Most early Reformers were adults prior to the Reformation. Early Reformers were disproportionately born in cities that adopted laws, although some migrated to these cities before the Reformation. Second, the set of highly educated individuals we study include both those who are responsible for the institutional changes of the Reformation and those whose formation reflects these changes. Almost by construction the cities that adopted the laws of the Reformation had slightly larger numbers of people identified in the *Deutsche Biographie* just following or even on the eve of the Reformation. These facts provide motivation for our baseline regression specifications in which the omitted time category is for the generations reaching adulthood just before the Reformation. More generally, our graphs showing changes in upper tail human capital at the Reformation should not be thought of as precise analogues to regression discontinuity designs – since the people observed at the discontinuity were actively involved in shaping which cities became exposed to the new institutions.

[63] The small number of people who died before age 40 are assigned to the period in which they died.

[64] There are relatively few documented instances where people moved between towns as children or adolescents.

Table B1: Formation of Upper Tail Human Capital

	Log People	Binary Outcome: Number of People Crosses Threshold					
		Any	25th Pct.	50th Pct.	75th Pct.	90th Pct.	95th Pct.
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Post	1.79***	0.66***	0.65***	0.41***	0.18***	0.06**	0.02
	(0.12)	(0.06)	(0.05)	(0.06)	(0.05)	(0.02)	(0.02)
Post x Law	0.76***	0.00	0.18**	0.25***	0.22**	0.11*	0.07*
	(0.24)	(0.13)	(0.08)	(0.08)	(0.10)	(0.05)	(0.04)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	478	478	478	478	478	478	478
R^2	0.83	0.76	0.80	0.70	0.61	0.54	0.53

This table presents the results of regression analysis estimating the effect of Reformation laws on the local formation of upper tail human capital, measured by the number of native-born people observed in the *Deutsche Biographie*. "Law" is an indicator for cities that passed Reformation ordinances in the 1500s. The "Post" period is 1520 through 1820. The pre period is 1300 to 1519. In column 1 the outcome is the logarithm of the number of people plus one. In column 2 the outcome is a binary indicator for any native-born people observed in the *Deutsche Biographie*. In columns 3-7 the outcomes are binary indicators for cities above the 25th, 50th, 75th, 90th, and 95th percentiles in the post-period distribution of upper tail human capital. The 25th percentile is 4 people. The 50th percentile is 9 people, the 75th percentile is 22 people. The 90th percentile is 50 people. The 95th percentile is 124 people. Columns 2-7 are estimated as linear probability models (OLS). Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, and *, respectively. Standard errors are clustered at the territory level. Territories are from Euratlas.

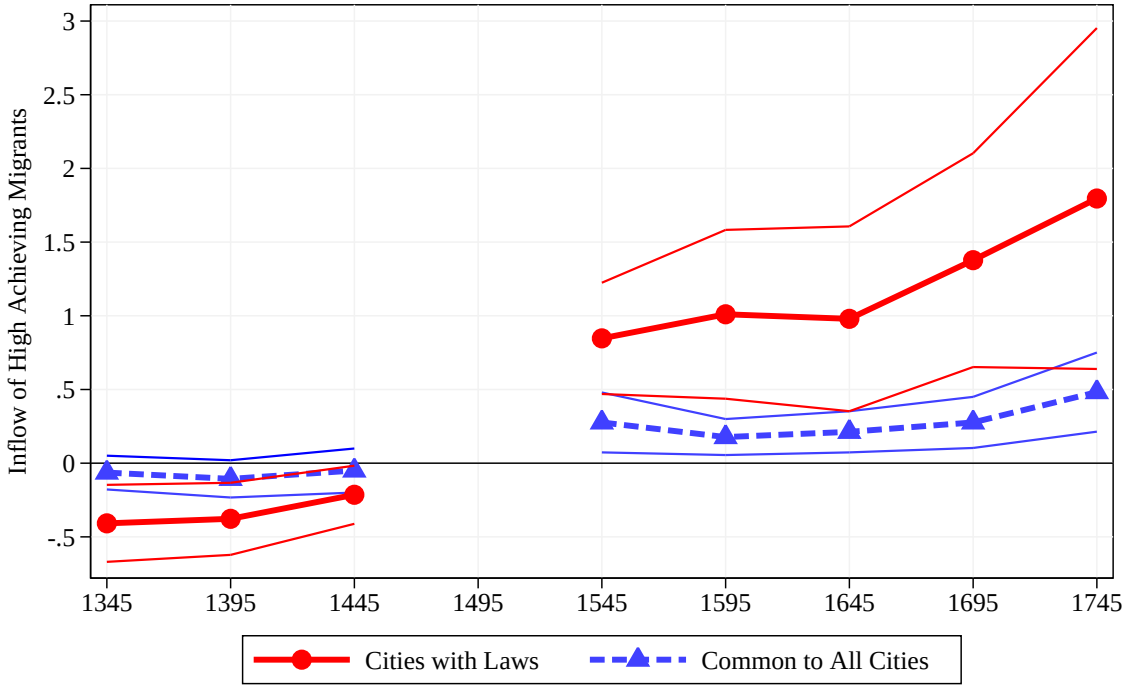
B.4 Count Data Outcome

We also collapse the data into a pre- and post-period. We then measure locally produced upper tail human capital with the number of people in the *Deutsche Biographie* born in city i and estimate the following regression:

$$People_{it} = \theta_i + \alpha \cdot Post_t + \beta \cdot (Post_t \times Law_{i,pre-1600}) + \epsilon_{it}, \tag{7}$$

Table B1 documents the relationship between legal institutions supporting public goods provision and the formation of upper tail human capital using using estimating equation (7). Column 1 studies shows that cities with Reformation laws produced significantly more upper tail human capital formation in the post-Reformation period. Column 1 measures upper tail human capital by the logarithm of the number of people in the *Deutsche Biographie* born in a given city plus one. Columns 2-7 examine where these effects are located in the upper tail

Figure B3: The Migration of Upper Tail Human Capital Using Count Data



This graph plots parameter estimates from regression analysis examining the differential migration of upper tail human capital into cities that adopted Reformation law. The outcome variable is the count of the number of migrants in city i at time t. Migrants are upper tail human capital individuals identified in the *Deutsche Biographie* as having moved to the city in which they died. We assign migrants to the time-period and city in which they died. Time is measured in 50-year periods. We graph the parameter estimates on time-period fixed effects and on 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).

human capital distribution by studying binary outcomes for any upper tail human capital and for upper tail human capital crossing thresholds in the post-1520 distibution. Column 4 indicates that cities with the institutions of the Reformation were 25 percent more likely to be above the 50th percentile in the post-1520 upper tail human capital distribution, controlling for time invariant city characteristics. This advantage is smaller in magnitude but holds in the far upper tail. Cities with public goods institutions were 22 percent more likely to be above the 75th percentile, 11 percent more likely to be above the 90th percentile, and 7 percent more likely to be above the 95th percentile.

Consistent with Table B1, our baseline results in the main text study the logarithm of upper tail human capital as an outcome, but are supported by analysis of the relationship between public goods institutions and the number (count) of upper tail human capital migrants observed in the data. Figure B3 presents parameter estimates of the relationship between public goods institutions and the count of migrants using the specification from equation (4). Figure B3 plots the parameter estimates through 1745 for ease of presentation: The migration advantage for cities with laws becomes very large post-1750 – and when plotted this dominates and obscures the early period advantages highlighted by this graph and our broader analysis.

B.5 Human Capital Pre-Trends

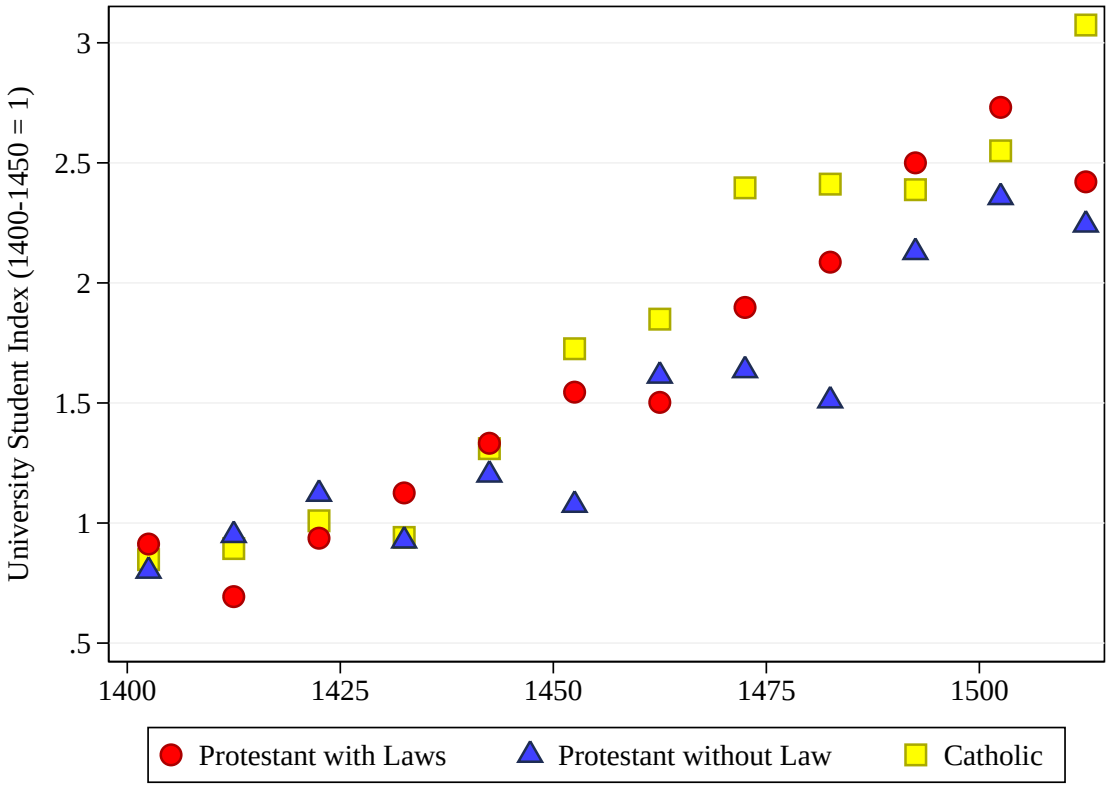
Our baseline analyses control for upper tail human capital before the Reformation as observed in the *Deutsche Biographie*. Here we present additional evidence on the similar human capital pre-trends in cities that did and did not adopt public goods institutions in the 1500s by studying records on individual university degree recipients over the period 1398 through 1517.

To document similar human capital pre-trends, we study microdata on university students *by home town location* from Cantoni, Dittmar, and Yuchtman (2015). The data allow us to match students recorded in university registries as receiving degrees (Bachelor, License, Master, and Doctorate degrees) to the students' home towns.[65] Figure B4 plots an index of the number of students receiving degrees (the average number of degrees between 1400 and 1450 is indexed to 1). We use the index to compare the flow of degrees to students from cities that got Protestant laws, cities that adopted Protestantism in religion but did not get Protestant laws, and always Catholic cities. Figure B4 shows that cities that did and did not get Reformation laws exhibited similar pre-Reformation trends in university degrees earned by hometown students.

We use regression analysis to test for prior differences in university student trends and

[65] Because these data stop in the mid-1500s, we use the home town locations of university degree recipients as evidence on the pre-trends in upper tail human capital across cities before the Reformation and not as an outcome variable.

Figure B4: Pre-Trends in University Degrees



This graph presents data on students from different types of cities who received university degrees each 10-year period before the Reformation. The number of students is indexed such that within-group average 1400-1450 = 1. Source: Microdata on degrees received from German universities from 1400 to 1517 by student home town are from Cantoni, Dittmar, and Yuchtman (2015).

find no significant pre-1520 differences between cities that did and did not adopt institutional change. We estimate regressions of the form:

$$Students_{it} = \alpha_i + \delta_t + \beta(Trend_t \times Law_i) + \epsilon_{it} \tag{8}$$

Here α_i and δ_t are city and time fixed effects and the parameter of interest is β which estimates the differential trend for cities with laws $(Trend_t \times Law_i)$.

We present our results in Table B2. Column 1 shows that there is no significant difference in trends for cities that did and did not adopt, controlling for a common time trend $(Trend)$ and an indicator for cities that adopted institutional change (Law). Column 2 shows there is no significant difference controlling for city fixed effects. Column 3 shows there is no significant difference controlling for city and time fixed effects.

Table B2: University Student Trends Before Institutional Change

	[1]	[2]	[3]
	Outcome: Number of University Students		
Law	-11.177		
	(9.268)		
Trend	0.025***	0.025***	0.021***
	(0.005)	(0.005)	(0.005)
Law × Trend	0.008	0.008	0.008
	(0.007)	(0.007)	(0.007)
City Fixed Effects	No	Yes	Yes
Decade Fixed Effects	No	No	Yes
Observations	2868	2868	2868
R^2	0.048	0.705	0.706

This table presents regression estimates studying trends in university students between 1398 and 1517. The outcome is the number of students from a city granted a university degree in a ten-year period at a German university. Data on university student degrees are from Cantoni, Dittmar, and Yuchtman (2015). Time is measured in ten-year periods starting 1398-1407 through 1508-1517.

B.6 Robustness of Results – Evidence on Upper Tail Super-Stars

In our baseline analysis, we examine all individuals in the *Deutsche Biographie*. In this subsection, we restrict analysis to upper tail "super-star" individuals. We focus on super-stars in order to study individuals (1) who were especially important and (2) for whom selective inclusion into the *Deutsche Biographie* is not plausible. We show that our baseline results are robust in data on super-stars.

We restrict the data to "super-star" individuals who were the most important figures *within* the *Deutsche Biographie*. We do this by restricting analysis to indviduals with biographies with extended biographical essay, which account for slightly more than 25 percent of individuals in the *Deutsche Biographie*. We examine the presence of such upper tail human capital super-stars in a given city-period as an outcome. We use our baseline flexible difference-in-differences design and distinguish between migration and local formation. We present results studying the binary outcome of any presence in a city-period.

We estimate regressions of the form:

$$(Any\ Super\text{-}Stars)_{it} = \theta_i + \delta_t + \sum_{s=1320}^{1770} \beta_s (Law_i \times Time_s) + \epsilon_{it} \tag{9}$$

The outcome $(Any\ Super\text{-}Stars)_{it}$ is a binary variable for any super-stars in a city-period.

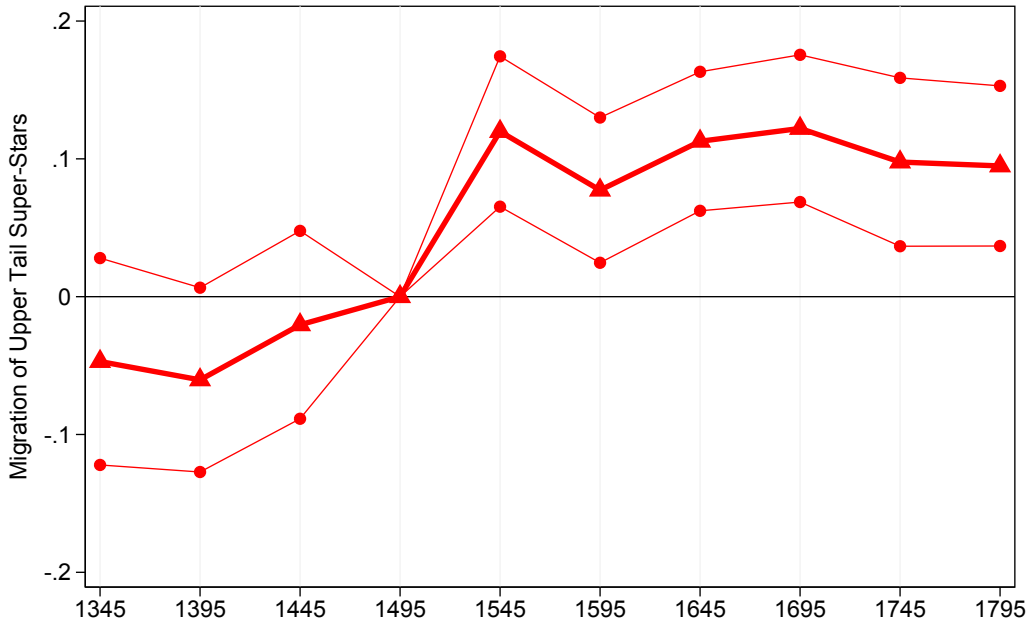
The θ_i and δ_t are city and time fixed effects. We examine whether there was any shift in the probability of observing a super star in cities that passed Reformation laws period-by-period and study the β_s as the parameters of interest. We first study migration and local formation in aggregate and then the sectoral allocation of super-stars.

Figure B5, which shows that total migration of super-stars responded sharply to the passage of Reformation laws while the local formation effects are more muted. These results are broadly consistent with our baseline findings studying the complete data. We find an immediate, persistent, and significant migration response. We find a much more muted response in local formation. We note that unlike in our main results we are here studying a binary outcome for any super-stars, whereas in Figure 4 (in the main text) the outcome is the log of the number of upper tail human capital individuals.[66]

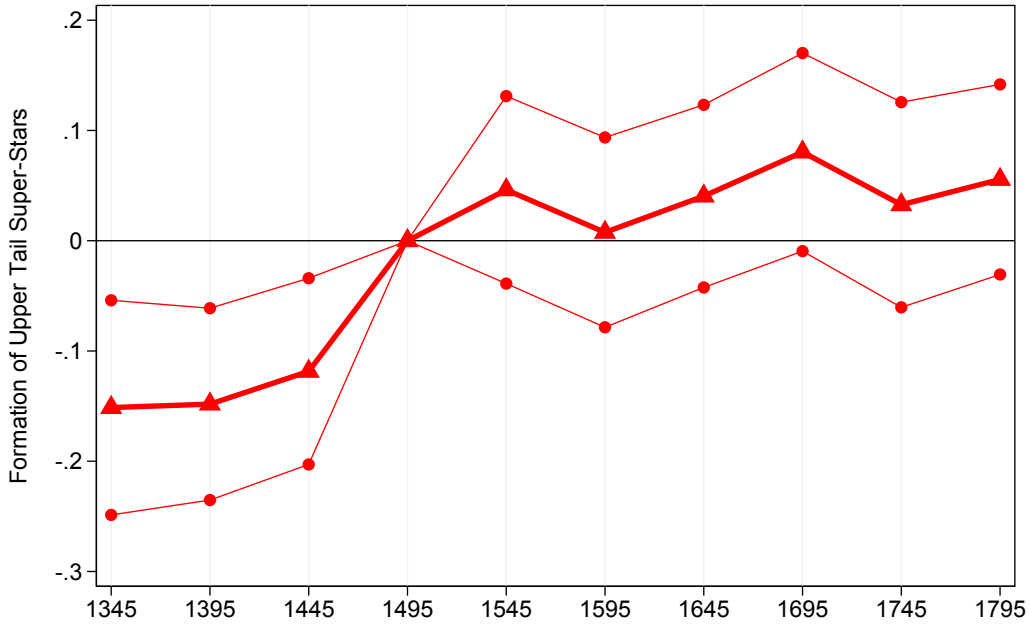
[66]We get similar results when we study the log of the number of super-stars: highly significant shifts in migration and more muted differences in local formation.

Figure B5: Regression Analysis of Super-Star Human Capital

Panel A: Migration

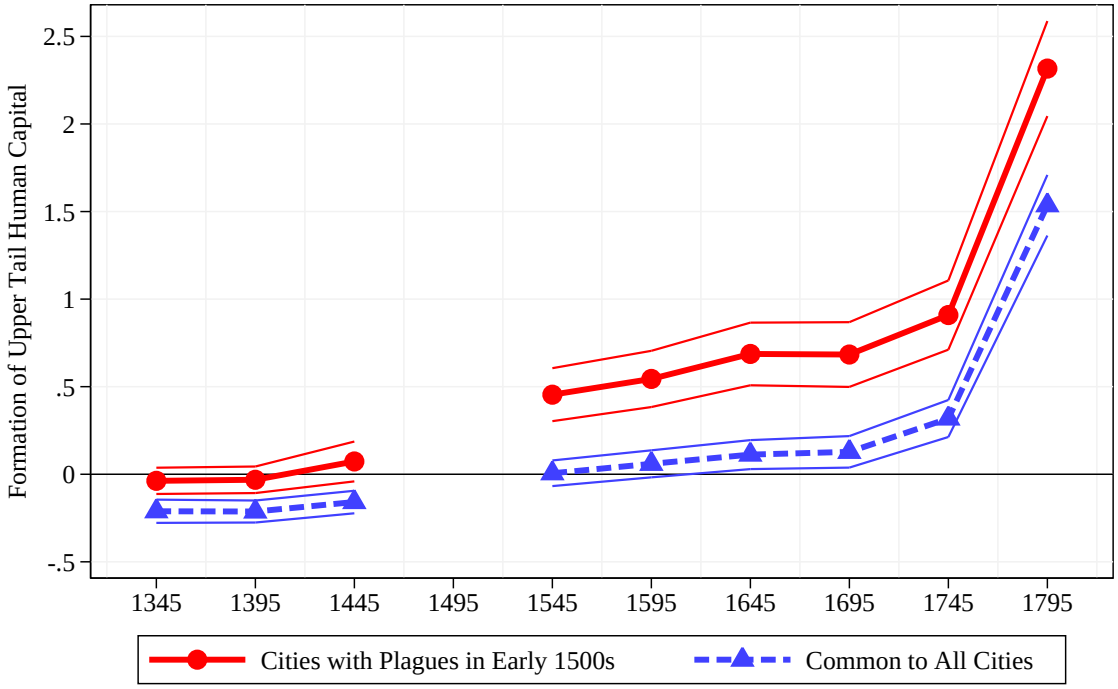


Panel B: Local Formation



This graph plots parameter estimates from regression analysis examining the presence of upper tail human capital "super-stars" in cities that adopted Reformation laws. The outcome is a binary variable for super-star individuals observed in tthe *Deutsche Biographie*. We define super-stars as individuals with biographies in the top 25 percentiles ranked by length. In Panel A, the outcome is the presence of super-star migrants. In Panel B, the outcome is the presence of super-star locals. 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).

Figure B6: Relationship between Upper Tail Human Capital and Early 1500s Plague



This graph plots parameter estimates from regression analysis examining the differential local formation of upper tail human capital in cities that adopted were struck by plague between 1500 and 1522. 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 struck by plague between 1500 and 1522. The regression 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). The variation common to all cities is δ_t in equation (4) in the text. The incremental variation specific to cities with laws is β_s.

B.7 Reduced Form Evidence on Plagues and Human Capital

Given our argument highlighting plague of the early 1500s as an institutional shifter, it is natural to wonder whether exposure to these plagues shifted human capital production. We test the hypothesis that the formation of human capital responded to shocks by re-estimating regressions examining period-by-period the differntial formation of upper tail human capital in cities that were "treated" in the 1500s. Specifically, we re-estimate equation (4) with exposure to plague 1500-1522 as the treatment variable, in the spirit of the reduced form in an instrumental variable design. Figure B6 presents a comparison of regression estimates where "ever treated" cities are cities exposed to plagues in the early 1500s and presents (i)

estimates of time period fixed effects common across all cities and (ii) estimates of the total period-specific variation for cities with Reformation laws. Figure B6 shows that exposure to plagues in the 1500s is associated with differential increases in the formation of upper tail human capital starting in the Reformation era. In contrast to our baseline analysis, we see the post-Reformation differential for "treated" cities is relatively stable – there is no sharp increase in the differential in the late 1700s – and the treated cities also had a modest differential before the Reformation.

C Institutional Change and City Growth

This section provides additional evidence to characterize the relationship between institutional and city growth. We first study whether institutional change interacted with initial city characteristics to predict growth. We then document how institutional change in the 1500s predict which *towns* are observed as *cities* with population in 1800. We study the set of over 2,000 towns in the *Deutsche Städtebuch* and document the fact that towns that institutionalized the Reformation were more likely to become cities than towns that did not formalize the Reformation in law. We then examine the relationship between institutional change and city growth in the panel of cities for which we do observe populations.

C.1 Interactions Between Institutional Change and Prior City Characteristics

Our baseline analysis examines how institutional change predicts city growth. In this section we test whether institutional change interacted with prior city characteristics to predict city growth.

We 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.[67] 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.

To test whether institutional change interacted with initial characteristics, we estimate regressions of the form:

$$\ln(Population_{i,1800}) = \alpha_0 + \alpha_1 Law_i + \alpha_2(Law_i \times x_i) + \gamma X_i + \epsilon_i \tag{10}$$

Table C1 presents our results. Panel A studies upper tail human capital outcomes and shows that the direct effect of Reformation Laws is stable and significant and all the interactions are insignificant. Panel B studies long-run population outcomes and shows that

[67] 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.

the interaction between Reformation law and initial city size is negative and significant when it enters alone and maintains its magnitude but loses statistical significance when other interactions are introduced. The measure of initial size we study here is an indicator for "Large City in 1500" which distinguishes cities with populations of 6,000 or more. Cities with 6,000 or more were in the far upper tail: 80 percent of cities in our data had less than 6,000 inhabitants in 1500.

C.2 Institutional Change and City Status in 1800

In this subsection, we test the hypothesis that Reformation laws were associated with an increased likelihood that a location became a city with population observed in 1800 and hence entered our sample. We test this hypothesis by examining the set of over 2,000 historic German towns identified in the *Deutsches Städtebuch* and studying whether towns with Reformation Laws were more likely to subsequently be observed as cities with population data in Bairoch, Batou, and Chèvre (1988) in 1800.

We are interested in the relationship between institutions and having city population data for several reasons. First, we are interested in the extensive margin of growth. A documented relationship between public goods institutions and becoming a city would strongly suggest that the extensive margin was an important dimension along which institutional change mattered. Second, we are interested in documenting selection into the sample. It is natural to wonder whether we observe institutional change on populations in the panel. However, the inferences we can draw from the panel data are contingent on the process through which locations enter the panel as cities with observed population. If many treated locations are only observed after treatment and if treatment itself is a predictor of entrance into the panel, then identification using panel methods may be comprised. In particular, there will be limited and *selective* within-unit variation available to study.

To test the hypothesis that the adoption of public goods institutions predicts in the 1500s predicts observable population data in 1800, we estimate linear probability model regressions. The binary dependent variable captures whether a historic town appears as a city with population recorded in 1800.[68] We estimate basic linear probability models with

[68] We aggregate a small number of *Deutsches Städtebuch* towns that were initially separate jurisdictions but ultimately became part of larger urban agglomerations in the Bairoch, Batou, and Chèvre (1988) data.

Table C1: Reformation Law Interactions with Pre-Characteristics

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A: Human Capital							
	Outcome: Ln Upper Tail Human Capital 1750-1799						
Reformation Law	0.38***	0.36***	0.41***	0.43***	0.35***	0.31**	0.39**
	(0.11)	(0.11)	(0.13)	(0.15)	(0.12)	(0.15)	(0.16)
Law × Large City 1500		0.15					0.08
		(0.34)					(0.38)
Law × Free-Imperial			-0.17				-0.20
			(0.23)				(0.25)
Law × High Student				-0.15			-0.18
				(0.18)			(0.16)
Law × Printing					0.31		0.34
					(0.42)		(0.46)
Law × Market Rights						0.18	0.17
						(0.24)	(0.29)
Observations	239	239	239	239	239	239	239
R^2	0.40	0.40	0.40	0.40	0.40	0.40	0.40
Panel B: City Population							
	Outcome: Ln Population in 1800						
Reformation Law	0.26**	0.34***	0.29**	0.31**	0.29**	0.21	0.29**
	(0.10)	(0.09)	(0.11)	(0.12)	(0.11)	(0.12)	(0.12)
Law × Large City 1500		-0.55***					-0.54
		(0.18)					(0.33)
Law × Free-Imperial			-0.15				-0.04
			(0.14)				(0.14)
Law × High Student				-0.12			-0.05
				(0.09)			(0.10)
Law × Printing					-0.28		-0.02
					(0.30)		(0.42)
Law × Market Rights						0.16	0.20
						(0.18)	(0.20)
Observations	239	239	239	239	239	239	239
R^2	0.53	0.54	0.53	0.53	0.53	0.53	0.54

This table presents regression estimates that document the relationship city population and human capital outcomes and interactions between legal change and other city characteristics. The outcome in Panel A is the log of human capital measured as the sum of local born and migrant individuals observed in the *Deutsche Biographie* plus one. The outcome in Panel B is log population in 1800. "Law" is an indicator for cities exposed to Reformation laws. "Large City in 1500" is an indicator for cities with at least 6,000 inhabitants. "High Student" is an indicator for cities with above median students receiving degrees from universities 1508-1517 (median is two students). "Printing" is an indicator for cities with printing pre-1517. "Market Rights" is an indicator for cities with market rights. All regressions contain the complete set of direct effects, all controls from Table 7, and territory fixed effects. Standard errors are clustered at the 1500 territory level. Territories are from Euratlas. Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, *, respectively.

the form:

$$City_{i,1800} = c + \alpha Law_i + \beta X_i + \epsilon_i$$

Here $City_{i,1800}$ is a binary outcome recording whether a town appears as city with population in 1800 and Law_i is an indicator for cities with Reformation laws. The controls X indicator variables for cities with market rights or universities by 1517, cities incorporated by that date. The X_i also control for the number books published as of 1517.

Table C2 shows that towns treated by reformation institutions were approximately 10 percent to 13 percent more likely to be cities with population observed in 1800 than observably similar towns that were not treated by laws. This finding should be interpreted as a correlation and not a causal estimate. However, this finding suggests that cities with laws cities are more likely to be enter the Bairoch city data we study above. Because the Bairoch data are designed to capture locations above a size threshold of 5,000, this evidence confirms the positive relationship between the institutions of the Reformation and growth across small places and points to potential selection into the set of cities. The potential selection that we uncover implies that our earlier estimates may be conservative. In the main analysis, the control group we study to assess the impact of legal innovation is truncated. It does not include the most stagnant locations, which were disproportionately untreated locations.

C.3 How Missing Data Undermines Research Designs Using Panel Data on Total City Populations

Our baseline results document the cross sectional relationship between population observed in 1800 and treatment by legal institutions adopted in the 1500s. The key challenge and drawback of city-level panel data is that balanced data covering even the period 1400 to 1800 are available for relatively few cities and only for cities already in the far upper tail of the population distribution as of 1500. Fundamentally, *the panel data needed to test for the impact of Reformation laws on city population in research designs with city fixed effects are not available.*

Several observations about the available population data inform our research strategy.

For example, we treat Düsseldorf, Düsseldorf-Gerresheim, and Düsseldorf-Kaiserswerth as a single location. The results are not sensitive to this treatment of near neighbors.

Table C2: Town-Level Determinants of Becoming a City with Population Observed in 1800

	Binary Dependent Variable: City Population Observed in 1800					
	Sample: All Towns in *Deutsches Städtebuch*			Sample: Towns without Population Observed in 1500		
	[1]	[2]	[3]	[4]	[5]	[6]
Reformation Law	0.12***	0.10***	0.11***	0.15***	0.12***	0.13***
	(0.15)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Market Rights by 1517		0.03***	0.04***		0.04**	0.05**
		(0.01)	(0.01)		(0.01)	(0.02)
Incorporated by 1517		-0.01	-0.01		-0.01	-0.01
		(0.01)	(0.01)		(0.01)	(0.01)
Population Fixed Effects	Yes	Yes	Yes			
Controls	No	Yes	Yes	No	Yes	Yes
Territory Fixed Effects	No	No	Yes	No	No	Yes
Observations	2230	2230	2230	2116	2116	2116
R^2	0.41	0.44	0.46	0.03	0.10	0.13

This table presents results from linear probability model regressions studying the determinants of the binary outcome of being a city with population observed in 1800 in the Bairoch data. We study the complete set of towns from the *Deutsches Städtebuch* as locations that were candidates to become cities. Reformation law is an indicator variable whether a city had any ordinance by 1600. Controls are: four indicators for number of books that were printed in a city by 1517 (0, 1-100, 101-1000, more than 1000), university by 1517 indicator, and Reichsstadt indicator. Population bins are five indicator variables (population in 1500 data missing, 1,000-5,000, 6,000-10,000, 11,000-20,000, and more than 20,000). ***, **, * denotes 1%, 5%, and 10% statistical significance. Standard errors are clustered at the 1500 territory level. Territories are from Euratlas.

First, for many "treated" cities, there is no evidence on outcomes with and without treatment for the simple reason that we have no population observations before the 1600s. This is observed and discussed in Cantoni (2015), which studies the relationship between the non-institutional diffusion of Protestantism and city growth. In our data, there are 239 cities of which 103 adopted public goods institutions in the 1500s. The balanced panel of cities with population observed every 100 years 1400 to 1800 comprises 37 cities, of which 24 are "treated" with public goods institutions and 13 are "untreated." Many of the cities that adopted public goods institutions and became dynamic are only observed after 1500. Second, the set of balanced cities is restricted to cities in the far upper tail where the impact of institutional change was muted, as shown above.

These facts motivate our study of upper tail human capital from the *Deutsche Biographie*, where we can construct a balanced panel covering the period 1300 to 1800 and document that the introduction of the legal institutions of the Reformation was associated with a differential increase in the formation of talent after institutional change occurred. In our analysis of the *Deutsche Biographie*, we include city fixed effects and/or city trends because in those data there is meaningful variation in outcomes pre- and post-treatment.

However, it remains natural to wonder whether and if our baseline results on aggregate city population go through in panel data and/or in other periods. Because of the limited variation in the city population data, we estimate regressions that include territory fixed effects, and territory fixed effects interacted with time fixed effects, but not city fixed effects.

Table C3 presents cross sectional regressions and shows the relationship between Reformation law and city population in 1600 and in 1700. Consistent with our evidence in the microdata we observe popuation differences opening up over time: "treated" cities have a modest and not statistically significant size advantage in 1600 and have a larger and more significant advantage in 1700.

Table C4 presents unbalanced panel regressions with which we document that institutions explain city populations by 1800. We find some evidence for effects of institutions in 1700. However, the significance of the 1700s effect depends on the specification.

Table C3: City Size in 1600 and 1700 and Reformation Laws

	Ln Population in 1600			Ln Population in 1700		
	[1]	[2]	[3]	[4]	[5]	[6]
Reformation Law	0.17	0.11	0.17	0.27	0.21	0.42**
	(0.15)	(0.19)	(0.10)	(0.20)	(0.20)	(0.13)
Population Bin Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	No	Yes	No
Cantoni Controls	No	No	Yes	No	No	Yes
Geographical Controls	No	Yes	No	No	Yes	No
Territory Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	139	139	139	137	137	137
R^2	0.69	0.73	0.74	0.55	0.66	0.64

This table presents the regression results of estimating the effect of reformation laws on log population in 1600 and 1700. Reformation law is an indicator variable whether a city had any ordinance by 1600. Controls are: Market rights by 1517, town incorporated by 1517, four indicators for number of books that were printed in a city by 1517 (0, 1-100, 101-1000, more than 1000), university by 1517 indicator, Reichsstadt indicator, number of university students in each decade from 1398-1508. Cantoni Controls include indicators for Protestantism as the dominant religion, rivers, Hansa membership, and monasteries (Cantoni 2012). Geographical controls are latitude, longitude, and interaction of latitude and longitude. Population bins are five indicator variables (population in 1500 data missing, 1,000-5,000, 6,000-10,000, 11,000-20,000, and more than 20,000). ***, **, * denotes 1%, 5%, and 10% statistical significance. Standard errors are clustered at the 1500 territory level. Territories are from Euratlas.

Table C4: City Size and Reformation Laws in Panel Data

	Ln Population		
	[1]	[2]	[3]
Reformation Law	0.29***		
	(0.13)		
Ever Reformation Law × 1300		0.22	0.07
		(0.14)	(0.20)
Ever Reformation Law × 1400		0.18	0.18
		(0.20)	(0.14)
Ever Reformation Law × 1600		0.22	0.29
		(0.17)	(0.18)
Ever Reformation Law × 1700		0.37*	0.28
		(0.21)	(0.28)
Ever Reformation Law × 1800		0.42***	0.43***
		(0.10)	(0.12)
Controls	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Territory FE	Yes	Yes	Yes
Controls x Time FE	No	Yes	Yes
Territory FE x Time FE	No	No	Yes
Observations	775	775	775
R^2	0.34	0.40	0.46

This table presents the regression results of estimating the effect of reformation laws on log population using an unbalanced panel from 1300 to 1800. "Reformation Law" is an indicator variable whether a city had an ordinance and is currently treated. "Ever Reformation Law" is an indicator for whether a city ever had an ordinance. Controls are: Reichsstadt indicator, river indicator, Hansa indicator, latitude, longitude, and interaction of latitude and longitude. ***, **, * denotes 1%, 5%, and 10% statistical significance. Standard errors are clustered at the 1500 territory level. Territories are from Euratlas.

C.4 Human Capital as a Channel for Growth

City growth in pre-industrial driven by migration, not by demographic growth of resident populations (Bairoch 1991; De Vries 1984). We observe the migration of upper tail human capital, however data on total migration do not exist.[69] We examine observed observed migration of upper tail human capital individuals over the period 1520 through 1770 as a proxy for unobserved total migration flows.

In the data we find the migration of upper tail human capital that followed the institutional changes of the 1500s is a robust predictor of long-run city populations. To document this we study how variations in long-run populations are explained by public goods institutions (Reformation laws), migration flows, and the non-institutionalized diffusion of Protestantism. We estimate regressions of the following form:

$$\ln(Pop_{i,1800}) = \alpha + \beta Law_i + \theta(Migration_{i,1520-1770}) + \gamma X_i + \epsilon_i \tag{11}$$

The outcome is log population in 1800, Law_i is an indicator for Reformation laws institutionalizing public goods, and $Migration_{i,1520-1770}$ is the log of the number of upper tail human capital migrants plus one over the post-Reformation era.

Table C5 presents our estimates and shows (1) that migration of upper tail human capital is a powerful and statistically significant predictor of population in 1800 and (2) that conditional on the migration of upper human capital, there is a very small positive and statistically insignificant relationship between the institutional treatment and long-run populations. In Column 1 we replicate our baseline estimates showing that cities instutionalizing public goods with Reformation laws were approximately 25 percent larger in 1800 than observably similar cities that did not. In Column 2 we show that the Reformation law effect becomes insignificant and almost zero when we introduce migration flows as an exmplantory factor. In Column 3 we show these effects hold when we also control for the non-institutional diffusion of Protestantism. The fact that the effect of observed upper tail human capital migration dominates the institutional effect is striking, given that there is

[69] City records do not systematically record all migrants. At its most detailed and expansive, surviving evidence exists in some cities and some time periods for the migration of individual *bürgher* who obtained citizenship rights. However, most migration consisted of moves by people who do not show up in *bürger* rolls or the *Deutsche Biographie*.

Table C5: Institutions and Migration as Determinants of Long-Run City Population

	[1]	[2]	[3]
	Outcome: Ln Population in 1800		
Reformation Law	0.27**	0.03	0.05
	(0.12)	(0.08)	(0.12)
Ln Migrants 1520-1770		0.40***	0.38***
		(0.06)	(0.06)
Protestant Religion			-0.07
			(0.18)
Territory Fixed Effects	Yes	Yes	Yes
Population Fixed Effects	Yes	Yes	Yes
Main controls	Yes	Yes	
Geographic Controls	Yes	Yes	
Cantoni Controls			Yes
R^2	0.53	0.68	0.67
Observations	239	239	239

This table presents regressions estimating the relationship between the outcome log population in 1800 and independent variables for the institutionalization of public goods provision, migration, and the informal diffusion of Protestantism. "Reformation Law" is an indicator for the adoption of public goods institutions. "Ln Migrants 1520-1770" is the log of upper tail human capital migrants observed in the *Deutsche Biographie* between 1520 and 1770 plus one. "Protestant Religion" is the measure of the non-institutional diffusion of Protestantism as the dominant religion from Cantoni (2012). Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, and *, respectively. Standard errors are clustered at the territory level.

likely to be measurement error in our upper tail human capital variable operating as a proxy for unobserved total migration.

D Plague as Instrumental Variable

This section provides further evidence that supports our instrumental variable research design. First, we provide narrative evidence on the impact of plague outbreaks on local politics. This evidence fleshes out the mechanism through which plague outbreaks increased the likelihood of adopting a Reformation law. Second, we show that there is no increase over time in the likelihood that cities on trade networks were struck by plagues. This supports the view that plagues in the early 1500s were random conditional on long run plague and other observables. Third, we document that plagues in the early 1500s had a unique relationship with long-run city populations and that similar plagues in other periods between 1400 and 1600 had no significant relationship with long-run population. This evidence supports the exclusion restriction for our IV analysis. We also show that plagues in the early 1500s predict upper tail human capital in the time series. Fourth, we examine the dynamics in how recent plague shocks impacted institutional change and long-run growth over the course of the period 1522 to 1555. Fifth, we examine whether plague shocks interacted with other features of cities in shaping institutional change in order to study the nature of the local average treatment effect the IV recovers. We find that plagues mattered less for institutional change in Free-Imperial cities, but that other city characteristics did not interact with plague to predict institutional change. However, we find no evidence that institutional change had any differential correlation with growth in Free-Imperial cities.

D.1 The Mechanism – Plague and City Politics

In this section, we discuss how plague outbreaks impacted local politics. We discuss three interrelated topics: (1) the disruption of political processes and administration, (2) the breakdown of social order in cities during plague outbreaks, and (3) the consequence of migration into cities after plague outbreaks.

Plague outbreaks caused extreme increases in mortality and disrupted municipal politics and administration. Historical records suggests a city may often lose a quarter of its population or more during an outbreak (Slack 2012). Local political elites and swathes of the voting population typically died during outbreaks. Because plague outbreaks caused extreme disruption to civic life, detailed records from plague outbreak periods are often incomplete

or do not exist. Indeed, this motivates Biraben (1975) to collect evidence on the presence of major outbreaks rather than mortality counts. However, surviving evidence indicates remarkable dislocations. During the 1533 plague outbreak in Nürnberg a largee number of craftsmen with voting rights died (Isenmann 2012). During the 1597 plague in Uelzen, plague deaths were recorded for 41 percent of burgher (all city council members) households and 7 percent of other households. While the plague affected the poorer burgher somewhat more still about 30 percent of richer burger houses reported plague death (Woehlkens 1954). In Hamburg, 57 percent of city administrators, 76 percent of city council members, 35 percent of bakers, and 45 percent of butchers died during the Black Death. In Bremen and Lübeck, 36-40 percent of city council members died during the same period. Broadly, the narrative evidence indicates that plague outbreaks caused severe disruption of local political processes and administration, typically reduced the voting population, and frequently killed a substantial number of local political leaders.

Death, sickness, and flight all threatened the breakdown of social order. The fact that elites who did not fall victim to outbreak usually fled their home cities during plagues had significant consequences. The flight of city council members and administrators had a particularly important impact on functioning of cities and could precipitate a collapse of public order (Dinges 1995). For instance, 19 of 39 council members of Nürnberg fled during the plague of 1505 (Isenmann 2012).[70] Perhaps the starkest high-level evidence of the collapse of authority during plague outbreaks was the temporary dissolution of the Reichskammergericht, the highest court of the Holy Roman Empire, in 1540 when Speyer experienced a plague outbreak (Ahl 2004). In addition to the dissolution of the local order, the supply of other public goods frequently collapsed. To avoid contagion, city markets remained closed (Ibs 1993). Doctors, priests, and lawyers were among the first to flee, leaving the remaining population without help or means to care for the sick, bury the dead, write testaments, or settle the care for new orphans. With family solidarity and structures disappearing and local order dissolving, a "brutalization" of society frequently followed (Isenmann 2012). When the plague subsided, surviving populations usually had

[70] In an attempt to uphold some order, the city of Nürnberg subsequently restricted the ability of elites to flee the city. In 1520, the executive council forced some mayors and required some administrators to stay in Nürnberg during a plague outbreak, and in 1521/22 one third of the city council members had to stay in Nürnberg on a rotation basis.

less trust in the local elites that had abandoned the city. Plague outbreaks therefore left behind a fertile ground for new institutions that promised to mitigate suffering, to uphold social order, and to force local elites to do their civic duties during plague outbreaks. It is not a coincidence that Osiander in his famous plague sermon (1533) scolded the elites for fleeing from Nürnberg.

Following plague outbreaks, cities experienced a large inflow of migrants that, at least in part, replaced the victims of disease. Significantly, a considerable number of migrants were wealthy enough to register as burgher and thus obtain formal political rights (city voting rights). The number of Neubürger (new burgher) often rose dramatically after plague outbreaks, indicating considerable mobility of skilled craftsmen, merchants, and wealthy elites. However, migrants usually did not receive the right to vote immediately. New migrants often had to reside in city for 5 to 10 years to be eligible to vote in the city council and therefore acquired a political voice through voting channels only years after plague outbreaks (Isenmann 2012).[71] This provides one reason why the effect of plague outbreaks on institutional change through the political process developed with lags.

D.2 Exogeneity – The Stability of Plague in Well-Connected Cities Over Time

In this section, we provide additional evidence to support our IV strategy using exposure to plagues in the early 1500s as a source of exogenous variation in institutions. An identifying assumption for our IV research design is that plague outbreaks in a narrow window in the 1500s were exogenous conditional on long-run plague prevalence. Here we provide additional evidence for conditional exogeneity. In particular, we show that there were no differential trends in plagues towards well-connected cities and that well-connected cities did not experience any increase in plague outbreaks during the time period when we construct our IV. This evidence enables us to rule out the possibility our IV recovers underlying heterogeneity in locations that might have been the fundamental determinants of growth.

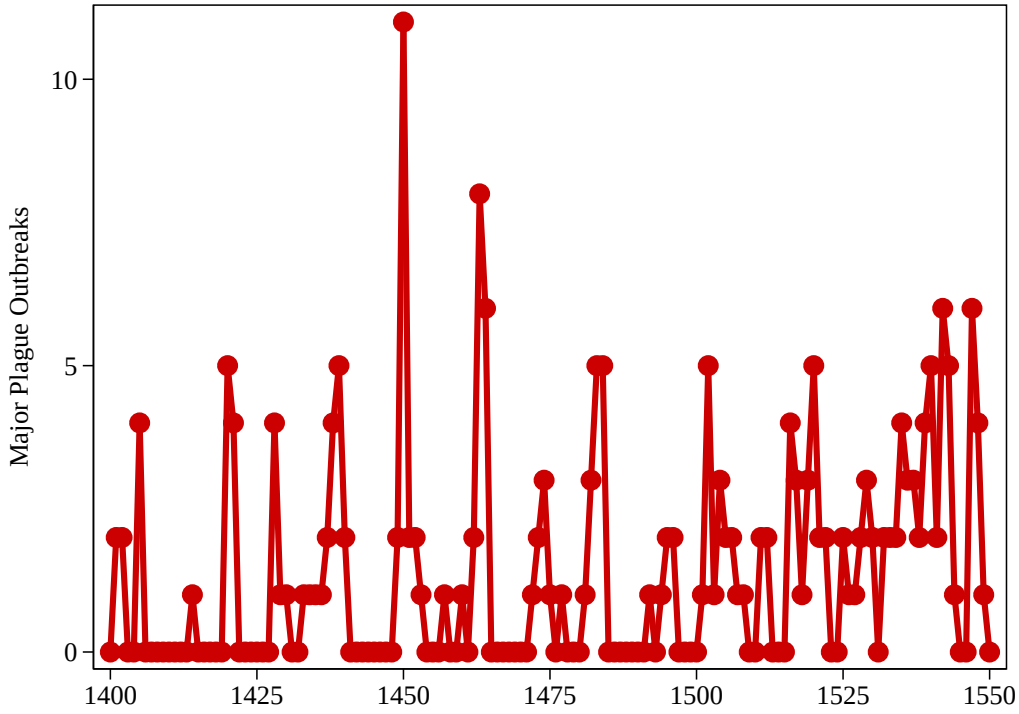
Our IV analysis studies how plague shocks in the early 1500s shifted institutions conditional on long-run city-level plague propensity across the 1400s. We control for long-

[71] For instance, Augsburg required a 10 year residency and being married to be eligible to vote from 1476 on.

run plague propensity because historical evidence suggests that more connected cities were in general more likely to experience plague. In particular, cities on trade routes experienced plague more frequently but were also likely to grow because of their transportation and network advantages. To address this possibility, our primary results control for both long-run, time invariant plague prevalence across the 1400s and independently for plague in each quarter century across the 1400s (Table 8).

Over the period we study the rate of plague outbreaks was stable in aggregate. Figure D1 shows that the aggregate plague time series is stable over the period from 1400 to 1550.

Figure D1: Aggregate Plague Outbreaks



This graph shows the total number of major plagues between 1400 and 1550 in cities in our sample.

However, it is natural to wonder whether the plague shifted over time in other ways that could threaten identification. In particular, it is important to know whether the distribution of plague outbreaks shifted over time towards more "open" and connected locations. Were cities on trade networks *increasingly* experiencing plague shocks, the shocks we study in the early 1500s could reflect the long run evolution in plague trends or other non-linear changes in

how plague transmission correlated with the operation of trade networks themselves, rather than conditionally exogenous shocks.

We test whether the relationship between city connectedness and the frequency of plague shocks was changing over time. We find no evidence of such changes, consistent with the overall stability of plague outbreak rates over this period.

We examine three measures of city connectedness. We observe which cities were located on rivers, members of the Hanseatic League, and located in close proximity to neighboring cities. Navigable rivers provided access to cheap waterborne transport in early modern Europe. The cities of the Hanseatic League were located along the Baltic litoral, had locational advantages due to their access to Atlantic trade, and legal arrangements designed to support trade. More broadly, the density of the urban network varied across space as shown in Figure 1 (main text). To test for time-varying plague dynamics, we examine the relationship between plague outbreaks over 25-year periods and time varying functions of these measures of connectedness.

We first study whether there was any differential trend in outbreaks across more or less connected cities. We estimate the following regression:

$$Plagues_{it} = \alpha_i + \delta_t + \beta(Connected_i \times Trend_t) + u_{it} \tag{12}$$

The dependent variable *Plagues* is the number of years with plagues observed in the city-level in 25-year periods. We estimate this equation using three measures of *Connected*: indicators for rivers (*River*), indicators for Hanseatic cities (*Hansa*), and the count of cities within 100 km (*Proximity*). We test whether there was any differential time trend in plagues in connected cities by regressing plagues on the interaction between measures of connectedness and a time trend ($Trend_t$). We estimate over the period 1400-1575 and include include city and time fixed effects (α_i and δ_t) in all specifications.

Because the assumption of linear time trends may be restrictive, we also estimate a model that allows the relationship between connectedness and plague to vary non-linearly over time:

$$Plagues_{it} = \alpha_i + \delta_t + \sum_{s=1425}^{1550} \beta_s(Connected_i \times Time_s) + u_{it} \tag{13}$$

Table D1 presents regression estimates that document that the relationship between plague and city connectedness was stable between 1400 and 1550. Columns 1 to 4 show there is no evidence of differential plague trends for cities connected to trade networks. Columns 5 to 8 show that there is similarly no evidence of plague increasing in a non-linear fashion in cities connected to trade networks over the long period 1400 to 1574. The outcome unit of analysis is the number of plague shocks at the city level in 25-year periods. All regressions control for city and time-period fixed effects. Standard errors are clustered at the city level. The omitted time category and interaction is 1400-1424.

D.3 Exclusion Restriction – The Importance of Plagues in the Critical Juncture

Our IV analysis rests on an exclusion restriction: plagues in the early 1500s impacted long-run outcomes only through the institutional channel. Consistent with the exclusion restriction, plagues in the 1500s were unique in predicting long-run city growth. After the mega-shock of the Black Death in the mid-1300s, German cities experienced irregular plague outbreaks starting over a period of several hundred years. When we examine the data starting in the late 1300s and running through the 1500s, we find that only plagues in the early 1500s predict long-run city growth.[72]

To document the unique relationship between plagues in the early 1500s and long-run growth, we estimate regressions of the form:

$$\ln(population_{i,1800}) = \alpha + \sum_t \beta_t plagues_{i,t} + \gamma X_i + \epsilon_i \tag{14}$$

We study log population in 1800 as the outcome variable. The parameters of interest are the β_t, which capture the relationship between long-run population and $plagues_{i,t}$, which measures the number of plagues in city i in 25-year intervals starting 1350 and running to 1575. The controls X_i include indicators for cities with market rights by 1300, cities legally incorporated in 1300, region fixed effects, and initial city population in 1300, measured categorically.

[72] Plagues in the 1600s largely reflected the military events of the Thirty Years War and are thus highly correlated with other factors shaping development.

Table D1: Relationship Between Plague Shocks and City Connectedness 1400 to 1574

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Dependent Variable: Plagues per 25-Year Period							
River × Trend	0.00			0.00				
	(0.00)			(0.00)				
Hansa × Trend		0.00		-0.00				
		(0.00)		(0.00)				
Proximity × Trend			-0.00	-0.00				
			(0.00)	(0.00)				
River × 1425-1449					-0.12*			-0.12*
					(0.06)			(0.07)
River × 1450-1474					-0.06			-0.12
					(0.08)			(0.10)
River × 1475-1499					-0.10			-0.07
					(0.07)			(0.06)
River × 1500-1524					-0.03			-0.00
					(0.11)			(0.10)
River × 1525-1549					0.02			0.00
					(0.11)			(0.11)
River × 1550-1574					0.13			0.11
					(0.10)			(0.10)
Hansa × 1425-1449						0.01		0.04
						(0.23)		(0.23)
Hansa × 1450-1474						0.50		0.53
						(0.32)		(0.33)
Hansa × 1475-1499						-0.23		-0.21
						(0.22)		(0.22)
Hansa × 1500-1524						-0.15		-0.14
						(0.25)		(0.24)
Hansa × 1525-1549						0.22		0.22
						(0.22)		(0.22)
Hansa × 1550-1574						0.13		0.10
						(0.16)		(0.17)
Proximity × 1425-1449							0.00	0.00
							(0.00)	(0.00)
Proximity × 1450-1474							0.00	0.00
							(0.00)	(0.00)
Proximity × 1475-1499							0.00	0.00
							(0.00)	(0.00)
Proximity × 1500-1524							0.00	0.00
							(0.00)	(0.00)
Proximity × 1525-1549							-0.00	0.00
							(0.00)	(0.00)
Proximity × 1550-1574							-0.00	-0.00
							(0.00)	(0.00)
Observations	1673	1673	1673	1673	1673	1673	1673	1673

Table D2 presents our results and shows that after the Black Death, plagues in the early 1500s had a unique relationship with long-run city population that is robust across specifications. Table D2 shows that plagues during the Black Death mega-shock (1350-1375) were also positively correlated with long-run growth. Table D2 also shows that plagues in the period 1525-1549 were not correlated with growth conditional on other observed plagues. Because institutional change was concentrated across the first half of the 1500s, this raises a question: Did plagues in the period 1525-1549 operate as institutional shifters for cities that remained candidates for institutional change over this period? In the next section, we show that they did.

Table D2: City Size and Historic Plague Outbreaks

	[1]	[2]	[3]	[4]
	Ln Population in 1800			
Plagues 1350-1374	0.42***	0.29***	0.29***	0.27***
	(0.05)	(0.08)	(0.08)	(0.09)
Plagues 1375-1399	0.07	0.12	0.11	0.12
	(0.10)	(0.12)	(0.12)	(0.15)
Plagues 1400-1424	-0.07	-0.08	-0.08	-0.06
	(0.19)	(0.22)	(0.22)	(0.26)
Plagues 1425-1449	0.11	0.13	0.16*	0.13
	(0.09)	(0.08)	(0.08)	(0.09)
Plagues 1450-1474	0.00	0.01	0.01	0.02
	(0.10)	(0.09)	(0.08)	(0.09)
Plagues 1475-1499	0.20	0.13	0.13	0.10
	(0.22)	(0.20)	(0.20)	(0.19)
Plagues 1500-1524	0.19**	0.20**	0.20**	0.22**
	(0.07)	(0.07)	(0.08)	(0.09)
Plagues 1525-1549	0.03	0.03	0.02	0.02
	(0.07)	(0.06)	(0.07)	(0.08)
Plagues 1550-1574	0.10	0.08	0.10	0.08
	(0.07)	(0.05)	(0.06)	(0.08)
Plagues 1575-1599	0.00	-0.06	-0.06	-0.04
	(0.05)	(0.06)	(0.06)	(0.07)
Observations	239	239	239	239
R^2	0.41	0.46	0.47	0.52
Population in 1300 Fixed Effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Territory Fixed Effects	No	No	No	Yes

This table presents results from regressions estimating the relationship between city population in 1800 and historic plague exposure between 1350 and 1599. "Plagues 1350-1374" is the count of plague outbreaks in that period. Other plague variables are similarly defined. Controls include indicators for city incorporation and for city market rights granted by 1300. Population fixed effects are for categorical variables: population in 1300 data missing; 1,000-5,000; 6,000-10,000; 11,000-20,000; and more than 20,000. Territory fixed effects control for regional territories from the *Deutsches Städtebuch*. Standard errors are clustered at the territory level. ***, **, * denotes 1%, 5%, and 10% statistical significance.

D.4 The IV Period – Plagues Across First Half of the 1500s

This section expands on our baseline analysis by examining how plagues acted as institutional shifters across the first half of the 1500s. Our baseline IV for institutional change is the number of plagues between 1500 and 1522, the year the first Reformation law was passed. In this section we first document how the relationship between plague shocks, institutional change, and growth evolved after 1522. By examining plagues after 1522 we provide evidence on the local average treatment effect the IV recovers and the dynamics of the Reformation. We then study plagues across the early 1500s through 1550 as the IV.[73] Our findings support our baseline estimates, as shown below.

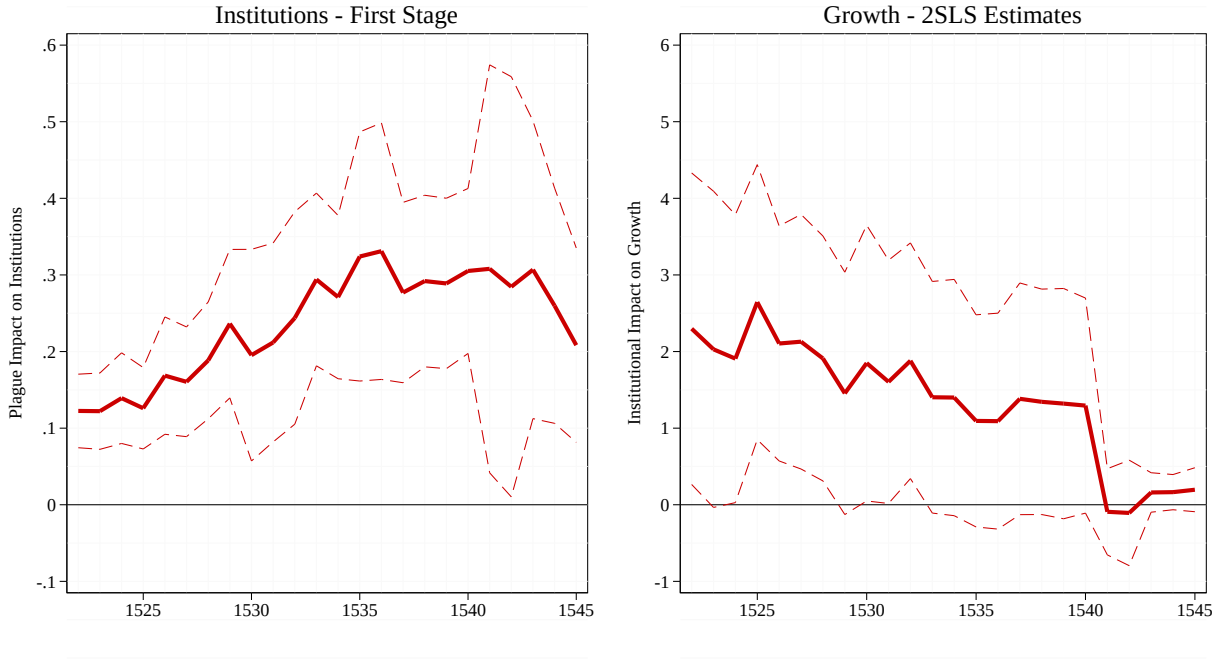
To study the relationship between plague shocks, institutional change, and growth during the Reformation, we estimate instrumental variable regressions year-by-year starting in 1522. In each year, we estimate a first stage regression documenting how the number of plague outbreaks over the past twenty-two years explains institutional change across the set of cities that "survived" to that date as candidates to adopt a Reformation law. In each year, we use changes in institutions induced by recent plagues to study the relationship between institutions and growth across the set of cities surviving to that date. In this analysis, the set of cities surviving as candidates for Reformation changes (cities that get laws drop out). In addition, the value of the instrument changes as we update recent plagues. In 1521, all 239 cities in our data are candidates for institutional change. By 1535, 200 cities survive untreated. By 1545, 168 survive.

These changes in the instrument provide one kind of external validity check on our baseline IV estimates. They allow us to compare how shocks shaped institutional change and how institutions shaped outcomes for cities that had the instrument "turned on" at different times.

To understand how the instrument varies over time, consider the example of the city of Hannover. Hannover had no major plagues from 1500 to 1522. For Hannover the instrument is thus "turned off" in our baseline analysis (Table 8). Hannover survived without a law into the 1530s, but experienced renewed Protestant agitation in the 1530s and a plague outbreak

[73] Our baseline estimates examine plagues before 1522 to avoid potential endogeneity and to restrict attention to institutional changes that occurred after plague shocks. Here we extend the IV period through the mid-1500s, when a new institutional equilibrium was settled, as described in the main text.

Figure D2: IV Estimates for Cities Surviving as Untreated Year by Year



This graph presents estimates from instrumental variable regressions for cities surviving without Reformation laws year-by-year. In each year, we estimate regressions examining the set of "surviving" cities that had not yet adopted a Reformation law as of that year. For each year, the instrument is the number of plague outbreaks in the previous twenty years. The left panel presents first stage regression estimates. The outcome is a binary variable for institutional change (Reformation law). First stage regressions estimate how the probability of institutional change varied with the count of recent plague shocks, defined as outbreaks in the previous twenty years. The right panel presents 2SLS regressions that document how long-run city populations (log population in 1800) responded to induced variation in institutions (Reformation law). In each year, the regressions are estimated using recent city-level plague shocks as of that year. This graph plots how the annual estimates evolve. All regressions include the same control variables as in Table 8, including the mean level of plague in the 1400s and period-by-period plague across the 1400s. Standard errors are clustered at the territory level. The red dashed line represents the 95 percent confidence interval.

in 1535 when the pro-Catholic city council was in disarray, and passed a Reformation law in 1536.[74] When we now use the IV set-up to study the plague and induced institutional change during the Reformation, we keep Hannover in the sample each year through 1536, when it gets a law and is no longer a candidate for institutional change.

Figure D2 plots year-by-year (i) the first-stage estimates of the relationship between institutional change and plagues and (ii) the 2SLS estimates of the population growth impact

[74]Examples can be multiplied. Lüneberg was struck by plague in 1516, then again in 1525 and 1528, and finally passed a law in 1531. Hildesheim experienced a plague in 1516, was again struck in 1538, and passed a law in 1541. Tübingen experienced no plagues in the 1500s until being hit in 1530, 1540, and 1541 – and passed a law in 1559.

of induced variation in institutions. Figure D2 shows that the first stage relationship between plague and institutional change strengthened over the initial years of the Reformation and then declined. In the early 1520s, one additional recent plague raised the probability of institutional change by about 15 percent. By the mid-1530s, the estimate is over 25 percent. Figure D2 also shows that the relationship between institutions and growth is more stable, but slowly declines over time and then collapses to zero in the early 1540s, just before the Schmalkaldic war, which initiated a new era in which relatively few cities adopted institutional change, as discussed above.

Finally, we present regressions that use plagues across the period 1500 to 1550 as the instrumental vavriable for institutional change. Table D3 presents these estimates for specifications that mirror Table 8 in the main text. Using plagues across the first half of the 1500s we observe a strong and highly significant first stage relating plague shocks to institutional change. However, the first stage estimates relating plagues to institutional change are slighly smaller in magnitude and the F-statistics are somewhat smaller, while ranging from 11.2 to 22.7. Similarly the 2SLS estimates are highly significant though marginally smaller than the baseline. For example, for upper tail human capital the baseline estimates range from 2.79 to 4.61. In Table D3 the corresponding estimates range from 2.66 to 4.34 (see Columns 1 and 5).

Table D3: Instrumental Variable Analysis of Long-Run Outcomes

	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: First Stage – Institutional Change						
	First Stage Outcome – Reformation Law					
Plagues 1500-1550	0.08***	0.07***	0.07***	0.06***	0.06***	0.06***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
R^2	0.29	0.29	0.30	0.51	0.51	0.52
F Statistic on IV	22.66	15.20	16.58	21.61	11.06	11.20
Panel B: Instrumental Variable Outcomes – Population and Human Capital						
	Outcome – Ln Population in 1800					
Reformation Law	1.57**	1.96**	1.87**	2.25***	2.79***	2.65***
	(0.77)	(0.90)	(0.80)	(0.83)	(0.88)	(0.77)
	Outcome – Ln Upper Tail Human Capital 1750-1799					
Reformation Law	2.66**	3.52**	3.35**	3.42**	4.34***	4.22***
	(1.33)	(1.54)	(1.39)	(1.34)	(1.67)	(1.46)
	Outcome – Upper Tail Human Capital per 1,000					
Reformation Law	0.71***	0.91***	0.85***	0.82***	0.98***	0.95***
	(0.27)	(0.30)	(0.27)	(0.29)	(0.32)	(0.30)
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 1500-1550" is the number of plagues 1500 to 1550. 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 include the complete set of controls from Table 7, including city population in categorical bins. The upper tail human capital regressions also control for the log of upper tail human capital observed 1370-1420 and 1420-1470. Upper tail human capital is measured by the sum of the number of migrants dying in a city and the number of people born 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.

D.5 Local Average Treament Effects – Interactions between Plague and City Characteristics

The historical evidence indicates that plague shocks shifted local politics and preferences over religion and institutions. It is natural to wonder whether plague shocks interacted with other city characteristics to shift politics and institutional change. This question is natural both given the historical evidence and because our IV estimates of the impact of institutions on growth are larger than our OLS estimates.

We study several features of city life that could potentially shape the way the experience of plague shocks was transmitted into local politics and preferences: cities' constitutional status as free or feudal cities (*Freie und Reichstadt* or *Landstadt*), the local history of printing, the local flow of recent university graduates, and the presence of market rights.

To consider whether these city characteristics shaped how plague shocks explain institutional change, we estimate the following regression model:

$$Law_i = c + \alpha\ Plagues_i + \beta(Plagues_i \times Characteristic_i) + \gamma X_i + \epsilon_i, \tag{15}$$

The outcome *Law* is a binary indicator for cities with Reformation laws. The key explanatory variable *Plagues* is the count of plagues 1500-1522 as in the main text. We include interactions with four characteristics. "Free City" is an indicator for free cities. "Any Printing" is an indicator for any printing pre-1517. "Students" is the count of students from within 10 km of city i who received a university degree 1508-1517. "Market Rights" is an indicator for cities with formal market rights.

Table D4 presents the results and shows that the main effect on "Plagues" is significant and stable across specifications. We also find strong evidence that the plague effect was muted in free cities. In free cities, the net effect of plagues (the sum of the main effect and the interaction) is not different from zero. This suggests that the effect of plagues was most concentrated in cities subject to feudal lords, where the barriers to mobilization and political change were otherwise highest. This is consistent with the finding in Dittmar and Seabold (2015) that variations in media market competition mattered most in cities subject to lords.

Table D4: Plague Shocks and City Characteristics in Institutional Change

	[1]	[2]	[3]	[4]	[5]	[6]
	Dependent Variable: Reformation Law					
Plagues	0.13***	0.18***	0.15***	0.16***	0.11***	0.18***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Plagues × Free City		-0.15**				-0.19**
		(0.05)				(0.08)
Plagues × Any Printing			-0.09*			0.29
			(0.05)			(0.18)
Plagues × Students				-0.00*		0.00
				(0.00)		(0.00)
Plagues × Market Rights					0.05	-0.03
					(0.09)	(0.09)
Free City	0.35***	0.36***	0.35***	0.33***	0.35***	0.36***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Any Printing		0.07	0.09	0.05	0.08	0.02
		(0.15)	(0.15)	(0.14)	(0.15)	(0.13)
Students	-0.01	-0.00	-0.01	0.00	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Market Rights	-0.06	-0.06	-0.06	-0.06	-0.06	-0.06
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	239	239	239	239	239	239
R^2	0.29	0.29	0.29	0.29	0.29	0.29

This table presents results from regressions estimating the relationship between the legal change outcome and historic plague exposure in the early 1500s. The outcome is is an indicator variable whether a city had a Reformation ordinance by 1600. "Plagues" is the count of plagues 1500-1522. "Free City" is an indicator for free cities. "Any Printing" is an indicator for cities with any printing pre-1517. "Students" is the count of students from within 10-km of each city who received a university degree 1508-1517. "Market Rights" is an indicator for cities with market rights before 1517. We includes all direct effects and the complete set of controls discussed in the main text. Column [1] presents the baseline first stage from the IV analysis. In column [1] we control for books printed pre-1517 in bins, following the specifications in the main text. ***, **, * denotes 1%, 5%, and 10% statistical significance. Standard errors are clustered at the territory level. Territories are from Euratlas.

E Historical Evidence on Politics and Institutional Diffusion

This appendix presents additional historical evidence on the diffusion of the Protestant Reformation as a social movement and how institutional change took place.

In the main body of the paper we study the relationship between institutions and subsequent city growth (i) across all of German-speaking Europe and (ii) restricting to within-territory variation. Our focus on cities is motivated by our interest in the institutional variation at the city level and by historical evidence on the importance of local preferences and political mobilization for the diffusion of the Reformation. Because it is natural to wonder about unobserved – or hard to quantify – differences in regional culture and in the ways territorial authorities responded to the Reformation, we present estimates that employ territory fixed effects that will absorb this sort of variation and focus on comparisons between cities in the same territory. In this section, we provide additional historical evidence to characterize the diffusion process.

A key feature of the historical evidence is that the adoption of the Protestant Reformation was driven by popular, or even revolutionary, preferences and political mobilization. Facing civil disobedience and unrest, and in some cases armed citizens, city councils defied their Catholic rulers and passed Reformation laws. The realization of popular mobilization and institutional demands differed across cities even within the same territory. This within-territory and within-region variation is particularly interesting for studying the growth effects of Reformation laws.

The diffusion of the Reformation at the city level involved civil disobedience and a broad range of actions forbidden by or at odds with the policy of city councils. In Frankfurt am Main, the guilds revolted against the city council after the council ordered an end Protestant church services in 1524. A Bürgerausschuss (a burgher council) published 46 articles, which city council was forced to adopt when facing mobilized and armed citizens. With the defeat of the Protestant peasants in the Peasant's War (1525), the burgher council was dissolved and the 46 articles nullified. However, public pressure continued to build. By 1533 the city officially converted, and the legal articles were implemented.[75] Speyer, Worms,

[75] Sehling (1902-2013) Vol. IX, p. 473f.

and Rothenburg also experienced unrest during the 1520s. While these unpheavals had no immediate institutional results, historical evidence suggests they laid groundwork for the later conversion to Protestantism. Similarly, in Augsburg, the city council was forced to drop its policy of religious neutrality following riots in 1524, 1530, and 1534 (Broadhead 1979). In Northern cities, such as Rostock, Stralsund, Greifswald, Lübeck, Braunschweig, Lüneberg, Göttingen and Hanover institutional change was led by citizens excluded from political power (Cameron 1991). In Zwickau, Lutheran publications were printed in 1523; the city council unsuccessfully attempted to suppress protests, street theatre, and civil disobedience in 1524; the Reformation was formally adopted in 1525 (Scribner 1979).

The political power struggle berween citizens and city councils was often a lengthy one (Ozment 1975). The adoption of the Reformation in Hildesheim exemplifies the struggle between the city council and other disenfranchised interest groups. From the beginning, the city council of Hildesheim actively opposed the Reformation. In 1524, the council banned Protestant books. In 1525, the council and guilds urged the Catholic church to fight heretics (i.e. reformers). Nevertheless, clerics integrated Lutheran songs into their masses. These songs were subsequently outlawed in 1528, but in 1530 a Protestant preacher was welcomed with Lutheran songs. The city council responded with passing new law against Lutheran songs the next day, prompting 150 Protestants to go to the vesper in an act of civil disobedience. The city council then outlawed Lutheran or "Wittenberg" teachings and instituted fines for Protestants. While the local Protestants received some forms of support from the *Städtetag* (assembly of the free cities), they remained unable to establish Protestantism in Hildesheim in the 1520s. In 1532, the Hildesheim guild masters first sent for a Protestant preacher and in September of that year 150 burghers stormed the city hall to demand Protestant preaching. In response, the city council asked Catholic duke of Wolfenbüttel for protection. However, in 1534 the city also signed an alliance with Protestant cities ensuring mutual protection. After the death of the staunch anti-Reformation mayor Wildefüers (elected 1526), the city council relented and openly adopted Protestantism in 1542.[76]

Lutheran also ideas received little if any support from lords during the period in which the Reformation initially emerged and diffused across cities. Luther himself was based in

[76] Sehling (1902-2013) Vol. VII/2/1, p. 797-806.

Wittenberg in the principality of Electoral Saxony. The Elector (Prince) of Saxony was a devote and traditional Catholic. The Elector had specifically forbidden religious innovation in his territories and his personal acts of piety involved activities Luther quickly came to denounce, including the maintenance of a collection of precious religious relics. Scribner (1979; p. 53) observes that, "It is undeniable that the Wittenberg movement was borne on a wave of popular enthusiasm. It outran the city magistrates' ability to control it, and finally forced them to act even against the will of the Elector [the territorial ruler of Wittenberg], who had prohibited any innovations in church matters." While the Elector explicitly forbade religious innovation and initially had Luther arrested, Luther was not exiled or executed.

While the local rulers were typically opposed to the early Reformation, the strength of their opposition could vary. Local histories provide insight into how citizens navigated this opposition. In Göttingen, Protestant preaching was initiated in 1518, but the city council was concerned about the reaction of the local Catholic duke and did not adopt the Reformation. The first openly Protestant service was held in Göttingen in 1529. In that year, the burghers revolted against the city council and succeeded in forcing the provision of Protestant services. When the local duke subsequently demanded that the Protestant preachers leave Göttingen, the council obeyed. Shortly thereafter, the city council, fearing a further burgher revolt, sent the duke a formal statement declaring for Protestantism.[77] The Reformation movement in Hannover was similarly resisted by the local duke (Duke Erich d. Ältere) and the city council. In agreement with the duke, the city council passed an ordinance against Luther's teachings. After civil unrest in 1532, Lutheran teaching became dominant in Hannover, but the city council would not allow Protestant services. In 1533, renewed unrest forced the city council to flee. The duke was unable to broker a compromise. On July 31, 1534, the newly elected (Protestant) city council entered into a transaction in which the city provided a payment to compensate the duke for accepting Protestant preaching in the city.[78] Similarly, the city council of Northeim exploited the financial difficulties of its Catholic duke, who accepted the Reformation in Northeim after also receiving a payment.[79] In contrast, the Dukes of Bavaria emerged as particularly ardent defenders of Catholic orthodoxy in the 1520s, but over the

[77] Sehling (1902-2013) Vol. VI/2, p. 902.
[78] Sehling (1902-2013) Vol. VI/2, p. 941.
[79] Sehling (1902-2013) Vol. VI/2, p. 919.

1500s failed to eliminate the diffusion of Protestantism in their territory.[80]

[80]For religious policy in Bavaria see Strauss (1959).

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