State Capacity and Public Goods: Institutional Change, Human Capital, and Growth in Historic 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. Cities that adopted institutional change subsequently began to differentially produce and attract human capital and grow faster. Institutional change occurred where ideological competition introduced by the Protestant Reformation interacted with local politics. We study plagues that shifted local politics in a narrow period as sources of exogenous variation in institutions, and find support for a causal interpretation of the relationship between institutional change, human capital, and growth.

JEL Codes: I25, N13, O11, O43

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1 Introduction

What are the institutional origins and impacts of the state as a provider of non-defense public goods? One view in the social sciences is that states emerge as stationary bandits that extract resources to support private goods (Tilly 1985). Another view is that historically states have emerged from military and dynastic conflict and to provide defense as a fundamental public good (Besley and Persson 2011). However, states today provide of a broader set of public goods – including education, insurance, and health care. The institutional origins of the state as the provider of such non-defense public goods have not been studied quantitatively.

In this research, we study historic institutional changes that expanded state capacity and non-defense public goods provision at the city-level.[1] The institutional changes we examine involved the reform of public finances, social welfare provision, and the establishment of Europe's first large scale experiments with mass public education. These changes were codified in law by German-speaking cities during the 1500s. In this period, the introduction of institutional competition during the Protestant Reformation interacted with city politics. Institutional change was adopted only in the subset of Protestant cities where citizens mobilized successfully. We study the impact of this institutional change on human capital and city growth. We find that institutional change – and not the informal diffusion of Protestantism – drove differences in outcomes by comparing Protestant cities that did and did not adopt institutional change. We show that plague outbreaks in the critical juncture of the early 1500s acted as institutional shifters. We use the *timing* of plague as a source of exogenous variation in local politics. Our causal interpretation of the positive relationship between institutional change and economic development is also supported by a difference-in-difference strategy.

Results. — The institutional changes we study specifically targeted the formation of upper tail human capital for public administration (Strauss 1978; 1988). We first study shifts in the migration and formation of upper tail human capital across cities following institutional change in the 1500s.[2] We measure institutional change by the presence of city-level Reformation laws, which were passed starting in the 1520s and adopted in only 55

[1]We view "state capacity" as a component of the "infrastructural power" of the state (Acemoglu, García-Jimeno, and Robinson 2015).

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

percent of Protestant cities. To test the impact of these laws, we assemble novel microdata on upper tail human capital between 1320 and 1820 from the *Deutsche Biographie*, which is the definitive biographical dictionary of economic, cultural, and political figures in German history (Hockerts 2008).[3] We examine outcomes across 239 German-speaking cities for which Cantoni (2015) measures the informal diffusion of Protestantism and Bairoch, Batou, and Chèvre (1988) record population. 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 a sharp and persistent positive shift in the level and trend in migration of upper tail human capital towards cities that adopted institutional change. We also observe a level shift and differential positive trend in the local formation of upper tail human capital in cities that adopted institutional change starting in the 1500s. The observed human capital effects persist through later 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*. We find that the largest and most significant shifts in migration towards cities that adopted institutional change 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 then study long-run outcomes: population and human capital intensity at the city level. We show that cities that adopted institutional change grew to become significantly larger and more human capital intensive by 1800.[4] To identify the long-run impact of institutional change 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

[3] 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.

[4] Around 1800, further institutional changes and educational reforms impacted economic development in German cities (Strauss 1978; Acemoglu et al. 2011)

capital intensity, controlling for Protestantism. Supporting the exclusion restriction for the IV strategy, we show that only plagues in the early 1500s were associated with long-run city growth. We also document that plague outbreaks were highly localized and that there were no trends in plagues overall or towards cities with trade network advantages.

The plague became salient as an institutional shifter in the 1500s due to the introduction of ideological competition. Before the 1500s, the Catholic Church enjoyed an ideological monopoly and local public goods provision was limited. The Reformation introduced ideological competition, and was animated by ideas about the common good, public provision, and elite corruption (Dittmar and Seabold 2015; Whaley 2012; Brady 2009). In German cities, institutional change at the municipal level was driven by the interaction between these ideas and local politics (Cameron 1991; Scribner 1979). The plague shifted local politics by threatening civic order, discrediting elites, altering the composition of city populations, and increasing demand for public goods provision (Dinges 1995; Isenmann 2012). Institutional change responded to these shocks.

We present panel estimates that support our cross-sectional IV analysis of city population outcomes. First, we show that institutional change explains which towns *became* cities with population records by constructing panel data on the universe of over 2,200 German towns. Second, we show that the probability that city population is observed in upper quantiles increased after cities adopted institutional change within the balanced panel of 239 cities. We examine these outcomes because it is *not possible* to directly estimate population growth effects of institutional change in the panel: most "treated" cities are only observed after treatment and estimates examining selectively observed cities embody "conditional on positive" measurement bias (Angrist and Pischke 2008). Conditional on positive estimates miss the growth effects of institutional change in initially small towns, where population data are not observed before institutional change.

Placing Our Results in Context. — Our paper relates to several literatures. We 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 document the positive growth impact of institutional change that expanded state capacity to promote non-defense public goods and human capital formation. 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.[5] Our study contrasts with prior research studying the potential growth effects of the non-institutional adoption of Protestantism.[6] We find that institutional change, and not the adoption of non-institutionalized religion, drove human capital accumulation and growth *before* the Industrial Revolution.

The existing economics literature has studied the military origins of state capacity 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. The institutional changes we study embodied religious ideas. Prior economics research has not highlighted the role of religion in the development of state capacity.[7]

A growing literature highlights the importance of upper tail human capital for growth in historical settings (Mokyr 2009; Meisenzahl and Mokyr 2012; Squicciarini and Voigtländer 2015), but has not identified the institutional origins of upper tail human capital. We study institutional innovations that targeted education and were designed to produce an administrative elite. As Strauss (1988; p. 203) observes, "Preparing pupils for high office was always the salient objective." 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.[8]

Another related literature we contribute to studies how political competition shapes institutions and public goods provision. Existing research studies political competition

[5] 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 public goods institutions on human capital and growth in *targeted* municipalities. Related work on public goods includes Banerjee and Iyer (2005) and Martinez-Bravo et al. (2014).

[6] 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.

[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).

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

operating through democratic channels (Fujiwara 2015; Acemoglu et al. 2014; Besley, Persson, and Sturm 2010). We study how the *interaction* between the introduction of political competition in a *non-democratic* setting and public health shocks drove fundamental changes in public goods institutions.

2 Institutional Change During the Reformation

We study the impact of institutional change that supported public goods provision. The new institutions were codified at the city-level in municipal law in the Reformation era. Institutional change was adopted in only in half of the cities that adopted Protestantism as their dominant religion.

The Protestant Reformation introduced new forms of institutional competition. The 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. Some but not all Protestant cities adopted new institutions that set up safeguards against church corruption and promoted public goods provision.[9]

What factors influenced why some cities adopted institutional change 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 Institutional Change

The adoption of institutional change depended on city politics. Local politics reflected the timing of short-run shocks, as well as underlying differences in city characteristics. Institutional change at the city-level was driven by citizens' movements that emerged without initial support from oligarchic city governments or territorial lords (Dickens 1979).[10] Cameron (1991; p. 240) observes, "As a rule neither the city patricians nor the local princes

[9]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).

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

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 of a 'burgher committee'." 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). While territorial princes did exert some influence over the process of institutional change, we focus on variation in institutions and outcomes across cities within the same territory (Sections 4 to 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.

Plague outbreaks in the early 1500s shocked local politics at a critical juncture. We use the timing of plague outbreaks as a source of exogenous variation in politics, given that politics otherwise reflected underlying differences across cities. 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]

Several factors explain why not all Protestant cities adopted institutional change.

[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 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.

City laws were adopted where local politics favored institutional change. In some cities, Protestantism diffused but political compromises between elites and the population prevented institutional change. For example, in Bautzen the Catholic bishop signed a contract agreeing to share the use of the Cathedral (this contract still governs the use of church space as of 2017). Bautzen became a Protestant city but institutional change did not occur. Other cities adopted Protestantism under the influence of a territorial lord, but without popular mobilization for city-level institutional change.

2.2 The Municipal Institutions of the Reformation

The institutional changes we study were formalized in laws that expanded the role of the state and the provision of public goods. The key institutional innovations were city-level laws. These laws transferred control of service provision from the Catholic Church to the secular state authorities 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 institutional changes increased state capacity, understood as the "infrastructural power" of the state (Acemoglu, García-Jimeno, and Robinson 2015). For example, in education, city laws "placed the supervision of all educational institutions firmly in the hands of...magistrates" (Strauss 1988; p.193). Moreover, education reform was explicitly designed to produce graduates who would serve the state and improve administration. In public health, institutional change provided for access to municipal hospitals, physicians, and midwives who were compensated from public resources (Lindemann 2010).[13] Anti-corruption safeguards, including the formal institutionalization of audits for public finances, were designed to reduce corruption. A concrete example of these innovations was the introduction of a "common chest." Wittenberg was a model: institutional change established an audited common chest in 1522, all church income was to be collected under one administration, these resources were to be used to provide care for the poor and sick and financial support to enable children of low-income parents to attend school and university (Sehling 1902-2013).

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

[13] 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). Bugenhagen also worked with Luther translating the Bible into German.

Institutional change directly targeted upper tail human capital, which we can measure in the data. The new institutions established compulsory public schooling and aimed to produce a human capital elite to staff expanding Protestant church and state bureaucracies.[14] Institutional change was associated with subsequent variation in provision, including in investments in school construction as we document in Appendix A. While we highlight the importance of institutional change for education, the consequences of Reformation laws arguably flowed from their interlocking nature.[15]

2.3 Measuring Institutional Change

Our measure of institutional change is the adoption of a Reformation law. Cities that adopted Reformation laws that persisted are considered "treated," including both cities that remained Protestant and cities that experienced later re-Catholicization. Cities that remained Catholic or that became Protestant without legal institutions are "untreated." A small number of cities where institutional change was reversed after a few years are considered untreated in our baseline analysis, which considers cities with new institutions that persisted to 1600 as treated. However, we obtain virtually identical results when we conduct an intent-to-treat analysis including the few cities where institutional change was reversed in the early 1500s.[16]

Figure 1 maps the cities in our data and illustrates the variation in which cities adopted institutional change. Figure 2 shows the cumulative share of cities that had adopted institutional change as of 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) established a new institutional equilibrium.[17] City level institutional change largely ended 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

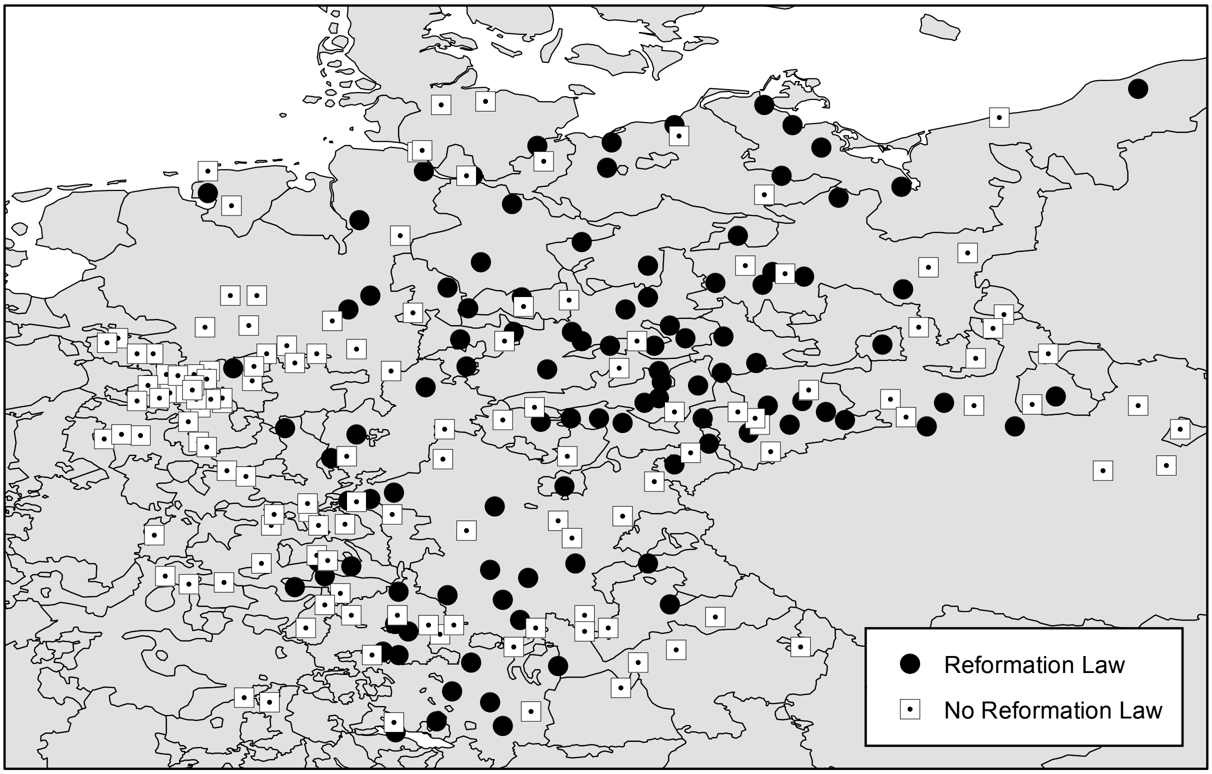
[14]Most school curricula do not mention Bible reading (Strauss 1978). We provide information on school hours, the short length of vacations, and the fact that city schools were free for poor children in Appendix A.

[15]To be clear, several possible channels may explain the effects of institutional change, including their influence on behavior, preferences, and the administration of existing institutions that were not directly targeted, including institutions supporting property rights enforcement.

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

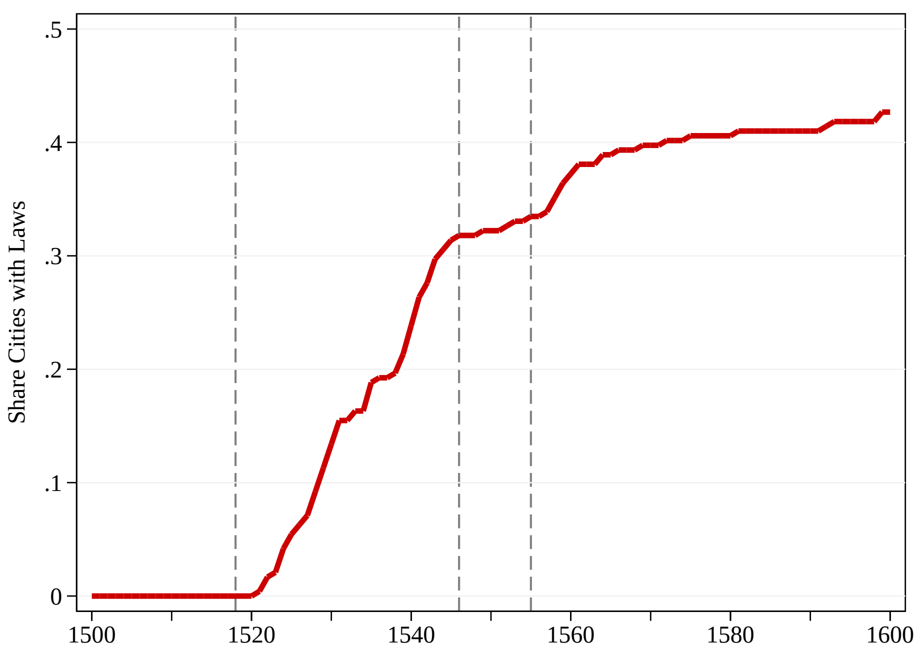
[17]The settlement included a provision, *cuius regio, eius religio,* which allowed local rulers to dictate the religion in their realm, but maintained a complicated set of exceptions for cities where magistracies and offices were to be shared and largely respected facts on the ground (Dittmar and Seabold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

Figure 1: Cities and Institutional Change



This map shows cities with and without institutional change, measured by Reformation Laws. Historic territories from Nüssli (2008).

Figure 2: The Share of Cities Adopting Institutional Change



This graph shows the share of cities with institutional change, measured by Reformation Law. Lines mark the spread of Luther's ideas in 1518, the Schmalkaldic War of 1546, and the Peace of Augsburg in 1555.

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 institutional change (Grell 2002).

3 Data

Definition of Sample – We focus on institutions and outcomes in 239 German-speaking cities with population observed in 1800 in Bairoch, Batou, and Chèvre (1988) and information on the non-institutional diffusion of Protestantism recorded in Cantoni (2012).[18]

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).[19] 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 *Deutsche*

[18]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 and Poland that have consistent evidence on institutional change and appear in the *Deutsches Städtebuch*, a comprehensive encyclopedia of over 2,000 German cities and towns (described below). Due to the nature of the sources, our analysis excludes Austrian cities and Alsatian cites.

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

Biographie is a project of the Historical Commission of the Bavarian Academy of Sciences (Reinert, Schrott, and Ebneth 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 1320 to 1820. We classify individual occupations in six principal sectors: (1) *government*; (2) *church*; (3) *education*; (4) *business*; (5) *arts*; and (6) *medicine*.[20] We provide detailed discussion of the nature and construction of the *Deutsche Biographie*, and our classification of occupations, in Appendix A.[21]

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).[22] 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).

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

[21]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.

[22]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 estimate the causal impact of institutional change on upper tail human capital using a difference-in-differences identification strategy. We document the distinct effects of institutional change on the migration and local formation of human capital. We show that the effects were most immediate in sectors targeted by institutional change designed to increase state capacity – government, education, and church. We find lagged spillover effects on the business sector.

The institutional changes we study were designed to develop human capital elites to staff expanding state and church bureaucracies (Strauss 1988). This operated in two ways. Institutions were designed to *produce* human capital. In his open-letter, *To the City Councillors* (1524), Luther emphasized the need for "men to govern." In 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, "men are needed to serve in preaching offices, governments, temporal posts, administrative offices."[23] Institutions were also designed to support the *migration* of human capital, including the recruitment of talented schoolchildren: "Officials roamed the land looking for 'good minds' in town and village schools" (Strauss 1978; p. 178).[24] 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 1370 and 1820. We classify as a migrant any individual who died in a given city, but was born in some other location. 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 minors. We classify as local formation individuals born in a given city in our data. Table 1 presents summary statistics and shows significant differences in the period after institutional change.

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

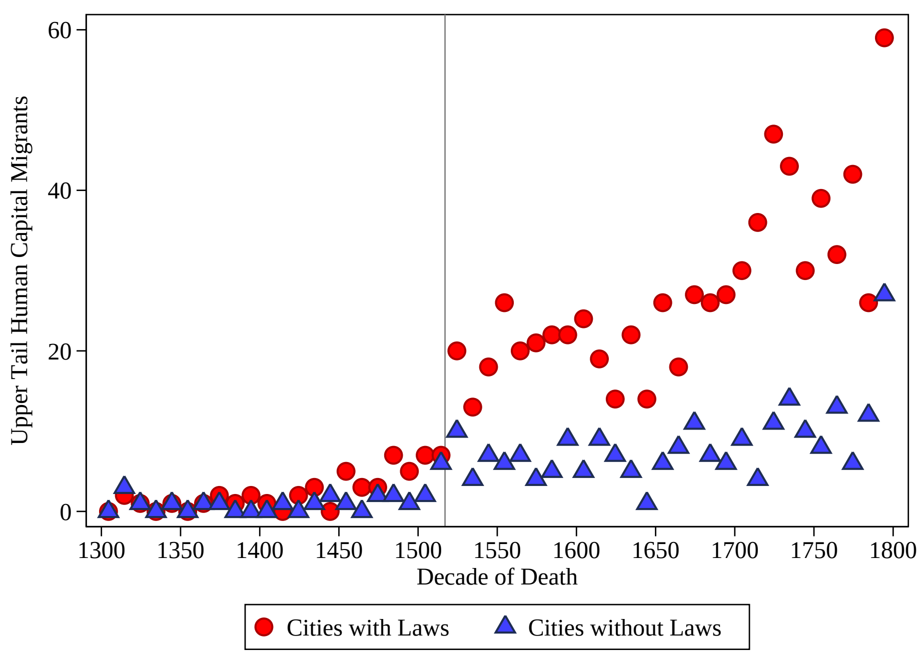
[24]Systematic efforts were made to identify talented children from poor backgrounds (Strauss 1978).

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-Lehmann non-parametric statistic for the difference (median shift) between cities with laws and cities without laws. We use the Hodges-Lehmann 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.

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.[25] 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.[26]

4.2 Results

We study the migration and local formation of upper tail human capital using difference-in-differences research designs. We show that cities that adopted institutional change in the 1500s experienced positive level and trend shifts in migration and in the formation of local (native) human capital. These effects hold relative to time invariant city fixed effects, underlying city-specific trends, and controlling for variation at the territory-year level.

Baseline Estimates – We present regression estimates that document the level and trend shifts in human capital, controlling for differences in underlying city-specific trends. We estimate a model:

$$\begin{aligned} People_{it} = \beta_0 + \beta_1(Post_t \times Law_i) + \beta_2(Post_t \times Trend_t \times Law_i) + \\ \phi_i Trend_{it} + \delta_{t,territory} + \epsilon_{it} \end{aligned} \tag{1}$$

Here the parameters of interest are β_1 and β_2, which captures the level and trend shift, respectively, for cities with institutional change in the post-period. The ϕ_i are city-specific time trends. The $\delta_{t,territory}$ are territory-time fixed effects that absorb variation shared by cities in a given territory and time (e.g. all cities in Saxony in a given period). Territory-time fixed effects control for shared shifts in post-period trends. We normalize the linear trend to be time 0 in the period before treatment (i.e. 1470-1519).[27]

Table 2 reports our estimates. In Panel A, the outcome is the log of migration plus

[25] 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.

[26] An identifying assumption in the empirical work below is that cities were stable treatment units. Consistent with this assumption, we find differential local formation in treated cities. We acknowledge that the absence of cross-city migration effects does not rule out the possibility of some cross-city spillover effects in the rural-to-urban migration data. However, it is unlikely that these drove the larger differences in formation.

[27] 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.

one.[28] Columns 1-4 examine upper tail human capital migration in fifty year periods from 1370 through 1819. The post period begins 1520. We test for and find significant level shifts in Columns 1 and 2: following institutional change the migration of upper tail human capital rises by 0.24-0.29 log points in treated cities. In Columns 3 and 4, we control for city-specific trends and find that institutional change was associated with both a positive level effect of 0.27-0.35 log points and positive shift in the post-trend of 0.04-0.05 log points. Time is measured in 50 year periods, so that after 100 years the trend effect implies an increase in migration of approximately 8-10 percent. These effects hold controlling for territory-year fixed effects (Column 4). Columns 5-8 show these results hold excluding the late 1700s and early 1800s when industrialization began to spread in Germany.

To preview our analysis of the plague as an institutional shifter, we also study how plague shocks in the early 1500s explain human capital outcomes. We measure plague shocks by the number of excess plagues in the early 1500s relative to long-run prevalence, calculated using the city-specific mean of plague outbreaks observed 1400-1499. We focus on early 1500s shocks to the generation in place when institutional change began. We compute excess plagues between 1500 and 1522, the year of the first institutional changes. We replicate our analysis using plague shocks in the early 1500s as the treatment variable in Columns 9-12. We find that early 1500s plague shocks, which in Section 6 examine as an IV for institutional change, were associated with large and significant positive level shifts in human capital. We similarly find that plague shocks drove positive shifts in the migration trend.

In Panel B, we examine the local formation of human capital and find consistent results. We measure human capital formation with the log of the number of local individuals in the *Deutsche Biographie* plus one. We find positive level and trend shifts in cities that adopted institutional change in the post-period. These effects hold controlling for city-specific trends and territory-year fixed effects. We also find positive and significant level effects when we examine plague shocks but weaker and statistically insignificant changes in human capital trends associated with plague shocks.

Flexible Model – We next flexibly study how migration and local human capital formation

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

Table 2: Institutional Change, Plague Shocks, and Upper Tail Human Capital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Log Migration Outcome												
	Treatment: Laws Full Sample 1370-1819				Treatment: Laws Pre-Industrial Sample 1370-1769				Treatment: Plague Shock Full Sample 1370-1819			
Post × Law	0.239	0.288	0.273	0.351	0.236	0.280	0.264	0.359				
	(0.061)	(0.068)	(0.071)	(0.077)	(0.058)	(0.064)	(0.077)	(0.094)				
Post × Trend × Law			0.035	0.049			0.039	0.045				
			(0.019)	(0.020)			(0.022)	(0.027)				
Post × Plague Shock									0.217	0.226	0.263	0.261
									(0.094)	(0.089)	(0.056)	(0.060)
Post × Trend × Plague Shock											0.026	0.032
											(0.018)	(0.018)
Observations	2151	2151	2151	2151	1912	1912	1912	1912	2151	2151	2151	2151
Panel B: Log Formation Outcome												
	Treatment: Laws Full Sample 1370-1819				Treatment: Laws Pre-Industrial Sample 1370-1769				Treatment: Plague Shock Full Sample 1370-1819			
Post × Law	0.344	0.390	0.240	0.203	0.324	0.343	0.222	0.236				
	(0.062)	(0.068)	(0.064)	(0.074)	(0.057)	(0.063)	(0.064)	(0.077)				
Post × Trend × Law			0.077	0.103			0.085	0.089				
			(0.025)	(0.028)			(0.025)	(0.031)				
Post × Plague Shock									0.224	0.232	0.258	0.234
									(0.072)	(0.067)	(0.113)	(0.123)
Post × Trend × Plague Shock											0.012	0.021
											(0.031)	(0.031)
Observations	2151	2151	2151	2151	1912	1912	1912	1912	2151	2151	2151	2151
Controls												
City FE	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No
Trend	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No
City-Specific Trend	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Post × Trend	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Territory × Year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

This table presents regression estimates documenting the relationship between institutional change and plague shocks on the local formation and migration of upper tail human capital. Panel A studies the local migration of upper tail human capital outcome, measured by the log of the number of upper tail human capital migrants plus one. Columns 1 to 4 study the relationship between human capital and institutional change in the complete data 1370-1819. Columns 5 to 8 study human capital and institutional change restricting to the period 1370-1769. Columns 9-12 study the relationship between human capital and plague shocks occurring in the early 1500s. Institutional change is measured by the adoption of a Reformation law. The variable "Post × Law" is the interaction between indicators for the post-1520 period and for ever-treated status. "Post × Trend × Law" is the interaction between treated, post, and the linear time trend. The "Plague Shock" is the number of excess plagues observed 1500-1522 relative to the long-run mean observed for each city 1400-1499. Plague shock interactions are defined similarly. Time periods are 50 year intervals: starting with 1370-1419 and ending with 1770-1819. Panel B studies the formation of upper tail human capital outcome in a city-period, measured by the log of the number of home-grown upper tail human capital individuals plus one. Standard errors in parentheses are clustered at the city level.

varied with 'ever-treated' status period-by-period. We estimate regressions of the form:

$$\ln(People_{it} + 1) = \theta_i + \delta_t + \sum_{s=1320}^{1770} \beta_s (Law_i \times \delta_s) + \epsilon_{it} \tag{2}$$

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

Table 3 presents our estimates. Columns 1 presents estimates for migration and show that that cities adopting institutional change enjoyed a very large *level* increase migration in the post period. The differential shift in migration into cities that adopted institutional change is observed directly after these changes (starting 1520-1569) and persisted through 1800.[29] Column 2 shows that this result is robust when we study variation within territory-year cells. Column 3 presents estimates for the local formation of human capital. The human capital formation results indicate that the strong positive relationship between institutional change and the local formation of human capital formation emerged more gradually over time. We observe local formation effects emerging in the later 1500s and strengthening thereafter. This is consistent with the returns to institutional change interacting with evolving economic opportunities (Becker and Woessmann 2009; Acemoglu, Johnson, and Robinson 2002). Column 4 shows this result again holds when study variation within territory-year cells.[30]

Allocation of Human Capital Across Sectors – To address questions of causality more tightly, we study whether upper tail human capital responded differentially in sectors targeted by the new institutions.

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%).[31] We measure the allocation of human capital by classifying the professions of all

[29] 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.

[30] Our results are not explained by the selective inclusion of marginal individuals into the *Deutsche Biographie*. Our results hold for individuals for whom there is no ambiguity about inclusion, e.g. individuals with extended biographical essays in the *Deutsche Biographie* as discussed and shown below. We note that while cities that adopted institutional change were attracting and producing fewer upper tail human capital individuals in the 1370-1419 period, there is no significant difference between 'treated' and 'untreated' cities over the period 1420-1469 relative to the 1470-1519 baseline. Moreover, when examine 'super-star' upper tail human capital we observe no significant pre-treatment differences (Appendix B).

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

Table 3: Institutions and Upper Tail Human Capital

	(1)	(2)	(3)	(4)
	Outcome: Log Migration		Outcome: Log Formation	
Law × 1370-1419	-0.088	-0.129	-0.265	-0.242
	(0.044)	(0.046)	(0.062)	(0.070)
Law × 1420-1469	-0.057	-0.081	-0.081	-0.057
	(0.049)	(0.049)	(0.062)	(0.070)
Law × 1520-1569	0.176	0.203	0.012	-0.009
	(0.063)	(0.067)	(0.072)	(0.093)
Law × 1570-1619	0.168	0.184	0.100	0.155
	(0.065)	(0.082)	(0.083)	(0.096)
Law × 1620-1669	0.147	0.197	0.161	0.211
	(0.061)	(0.065)	(0.083)	(0.104)
Law × 1670-1719	0.233	0.257	0.116	0.155
	(0.082)	(0.095)	(0.094)	(0.101)
Law × 1720-1769	0.215	0.209	0.218	0.415
	(0.086)	(0.106)	(0.119)	(0.133)
Law × 1770-1819	0.428	0.601	0.405	0.680
	(0.159)	(0.180)	(0.179)	(0.212)
Time FE	Yes	No	Yes	No
City FE	Yes	Yes	Yes	Yes
Territory-Year FE	No	Yes	No	Yes
Observations	2151	2151	2151	2151

This table reports regression estimates examining how human capital migration and formation outcomes varied with institutional change status ("Law") period-by-period. Outcomes are the logarithm of the number of upper tail human capital individuals plus one. Time is measured in 50-year periods from 1370 through 1819. The omitted time period is 1470 through 1519. Standard errors clustered on city in parentheses.

individuals in the *Deutsche Biographie* (see Appendix A). We study the allocation of upper tail human capital using the flexible difference-in-difference regression design of Equation 2, and maintaining the distinction between migration and local formation. We study the presence of upper tail human capital in a sector, measured 1/0. We examine this binary outcome because most of the variation at the city-sector-period level is between having zero or one observed individuals.

Table 4 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.

Discussion and Robustness – We find that institutional change drove increases in the migration and formation of upper tail human capital. These effects appear first in the sectors targeted by the institutional changes – government, church, and education.

It is unlikely that selective inclusion into the *Deutsche Biography* explains our findings. We find similar results when we restrict the analysis to the approximately 25 percent of individuals who were sufficiently important to merit an extended biographical essay in the *Deutsche Biographie*. Individuals with extended biographies were not plausibly subject to selective inclusion into the *Deutsche Biographie*. For these people, we also observe sharp effects for individuals active in business in the immediate post-1520 periods, particularly for migration. For local formation, the results are more muted and point towards spillover effects on sectors that were not directly targeted, notably business. We report these results in Appendix B, where we also discuss how the *Deutsche Biographie* was prepared by the Bavarian Historical Commission with the express aim of capturing unbiased evidence. To test for selective inclusion, we also examine the presence of nobles and find nobles are not more frequently observed in cities with laws except during the Thirty Years War period.

The nature of the migration and formation processes helps explain our findings. While migration flows partly reflected geographic sorting by adults, cities that adopted institutional change also directly promoted the migration of upper tail human capital *during* the educational process. Recruiters compiled dossiers on promising school children from small towns (Strauss 1978). This provides an explanation why strong migration effects are observed in the immediate post-1520 periods.

Table 4: Institutions and Sectors with Upper Tail Human Capital

	[1] Govt	[2] Church	[3] Education	[4] Business	[5] Arts	[6] Medicine
	Outcome: Binary Any Migration					
Law × 1370-1419	-0.041	0.015	-0.027	-0.049	-0.034	-0.008
	(0.025)	(0.040)	(0.022)	(0.030)	(0.021)	(0.009)
Law × 1420-1469	-0.007	0.058	-0.057	-0.057	-0.034	-0.008
	(0.035)	(0.042)	(0.028)	(0.029)	(0.018)	(0.009)
Law × 1520-1569	0.120	0.118	0.136	0.012	0.133	0.032
	(0.043)	(0.058)	(0.049)	(0.041)	(0.059)	(0.028)
Law × 1570-1619	0.098	0.107	-0.018	0.064	0.120	0.006
	(0.059)	(0.068)	(0.047)	(0.054)	(0.059)	(0.028)
Law × 1620-1669	0.131	0.110	0.090	0.069	0.098	0.026
	(0.046)	(0.054)	(0.055)	(0.046)	(0.049)	(0.025)
Law × 1670-1719	0.126	0.128	0.036	0.064	0.092	0.072
	(0.052)	(0.054)	(0.049)	(0.055)	(0.059)	(0.035)
Law × 1720-1769	0.061	0.111	0.092	0.071	0.055	0.067
	(0.057)	(0.048)	(0.048)	(0.055)	(0.059)	(0.038)
Law × 1770-1819	0.197	0.172	0.152	0.156	0.208	0.150
	(0.071)	(0.075)	(0.076)	(0.074)	(0.080)	(0.062)
p-value post-1520	0.000	0.002	0.011	0.044	0.004	0.007
p-value post-1670	0.002	0.002	0.026	0.039	0.022	0.003
	Outcome: Binary Any Local Formation					
Law × 1370-1419	-0.082	-0.036	-0.057	-0.042	-0.130	-0.059
	(0.051)	(0.039)	(0.046)	(0.038)	(0.041)	(0.042)
Law × 1420-1469	0.013	0.071	0.043	-0.058	-0.028	-0.068
	(0.056)	(0.054)	(0.054)	(0.051)	(0.061)	(0.039)
Law × 1520-1569	-0.012	0.069	0.032	-0.026	-0.044	-0.030
	(0.084)	(0.069)	(0.066)	(0.064)	(0.051)	(0.047)
Law × 1570-1619	0.107	0.056	0.028	-0.026	-0.052	0.005
	(0.080)	(0.068)	(0.049)	(0.060)	(0.068)	(0.052)
Law × 1620-1669	0.132	0.191	0.086	0.105	0.020	0.074
	(0.071)	(0.068)	(0.059)	(0.068)	(0.072)	(0.061)
Law × 1670-1719	0.141	0.075	0.077	0.047	0.087	-0.004
	(0.082)	(0.068)	(0.068)	(0.066)	(0.083)	(0.051)
Law × 1720-1769	0.105	0.022	0.291	0.160	0.126	0.050
	(0.082)	(0.073)	(0.080)	(0.072)	(0.088)	(0.066)
Law × 1770-1819	0.167	0.069	0.261	0.177	0.170	0.063
	(0.092)	(0.080)	(0.089)	(0.086)	(0.087)	(0.074)
p-value post-1520	0.081	0.128	0.007	0.123	0.339	0.546
p-value post-1670	0.047	0.352	0.001	0.029	0.071	0.471
Observations	2151	2151	2151	2151	2151	2151
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Territory-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

In the upper panel the outcomes are binary variables for any migrants to a city by sector and time. In the lower panel the outcomes are binaries for individuals born in a city by sector. Time is measured in 50-year periods 1370 through 1819. The reported parameters are interactions between an indicator for institutional change ("Law") and time indicators. The omitted category is 1470-1519. Standard errors clustered at the city level in parentheses. The post-1520 (post-1670) p-value is for the joint significance of post-1520 (post-1670) interactions.

5 Institutions and Long-Run Economic Outcomes

In this section, we test the hypothesis that institutional change drove long-run population growth and upper tail human capital intensity. We study city population as a measure of local economic activity (De Long and Shleifer 1993; Glaeser, Scheinkman, and Shleifer 1995; Acemoglu, Johnson, and Robinson 2005a). We present panel and cross-sectional estimates. We address potential endogeneity with an instrumental variable design in Section 6.

The challenge for panel research designs is that for the majority of treated cities, population data are observed only after treatment. For instance, we do not observe population in 1500 for 129 out of 239 cities because they were too small to be recorded.[32] We therefore consider two outcomes: (1) whether and when towns became cities with observed population and (2) whether and when towns became large enough to be at or above the 75th percentile of the city population distribution. We study these binary outcomes to avoid "conditional on positive" selection bias in a research design that includes unit fixed effects (Angrist and Pischke 2008). This selection bias is a concern because many initially small places only grew large enough to be observed after treatment.[33] We study being in the 75th percentile as a measure of size that we observe for all cities and periods.

We estimate panel regressions studying how institutional change impacted whether and when towns become cities and when cities became relatively large.

$$Outcome_{it} = \theta_i + \delta_t + \beta Law_i \times Post_t + \epsilon_{it} \tag{3}$$

$Outcome_{it}$ is a binary variable for locations (1) being cities with population observed in period t or (2) being cities with population above the 75th percentile. The θ_i and δ_t are city and time fixed effects, and Law_i is an indicator for institutional change. We examine data at the city-century level 1300 to 1800, with the post period running 1600 to 1800.

Table 5 reports our estimates. Columns 1-4 study 2,230 German towns and show that towns that adopted institutional change were approximately 7 percent more likely to be

[32]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 in 1500.

[33]There are only 30 cities in the balanced panel with population observed every century 1300 through 1800. Our study thus contrasts with Cantoni (2015), which (1) studies the relationship between a measure of non-institutionalized religion (Protestant or Catholic) and city outcomes and (2) only studies population outcomes within the set of city-years where population is observed.

observed as cities in the post-treatment period. This result holds controlling for territory-by-year fixed effects (Column 4). Columns 5-8 show that cities were more than 10 percent more likely to be large after adopting institutional change. We find the effects of institutional change dominate the effects of the informal diffusion of Protestantism.

Table 5: Institutional Change and City Population in the Panel

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Outcome: Population Observed				Outcome: Population Above 75%			
	Data on All Towns				Data on Cities			
Post × Law	0.074		0.069	0.067	0.102		0.108	0.184
	(0.018)		(0.018)	(0.020)	(0.042)		(0.042)	(0.055)
Post × Protestant		0.028	0.017	0.017		0.022	-0.025	-0.039
		(0.007)	(0.007)	(0.008)		(0.043)	(0.042)	(0.048)
Observations	13380	13380	13380	13380	1434	1434	1434	1434
R^2	0.65	0.65	0.65	0.66	0.63	0.62	0.63	0.69
p-value difference			0.013	0.030			0.040	0.009
Town FE	Yes	Yes	Yes	Yes	No	No	No	No
City FE	No	No	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Territory-Year FE	No	No	No	Yes	No	No	No	Yes

This table presents regression estimates examining city population outcomes. In Columns 1-4, the outcome is a binary variable that takes the value of 1 if a town has population observed in Bairoch, Batou, and Chèvre (1988). Columns 1-4 examine the 2,230 towns recorded in the *Deutsches Städetebuch*. In columns 5-8, the outcome is a binary variable for cities with population above the 75th percentile, and analysis is restricted to cities observed in Bairoch, Batou, and Chèvre (1988). "Post × Law" interacts an indicator for institutional change and the post-1520 period. "Post × Protestant" interacts an indicator informal Protestantism and the post-1520 period. We measure informal Protestantism using Cantoni (2015) for cities and the *Deutsches Städtebuch* for towns. Populations are observed at 100-year intervals 1300 to 1800. Standard errors in parentheses are clustered at the town or city level.

We next examine long-run growth in the cross-section, because missing population data make panel estimates biased. Table 6 presents three key facts on the relationship between initial city population, institutional change, and population in 1800. First, cities that adopted institutional change 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 (column 8). Third, there is a large and significant positive relationship between institutional change and long-run population for the vast majority of locations that were *small* in 1500, but not for limited set of already-large cities. Table 6 thus shows that relationship between institutional change and population growth was strongly positive in locations that had not

Table 6: Log Population in 1800 by Institutional Change Status and Initial Size

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Population	Cities with Law			Cities without Law			Difference	Share	Share
in 1500	N	Mean	Sd	N	Mean	Sd	in Means	with Law	of Cities
Unobserved	35	1.81	0.43	94	1.59	0.50	0.22 [0.01]	0.27	0.54
1-5 Thousand	32	2.01	0.61	30	1.69	0.57	0.32 [0.04]	0.52	0.26
6-10 Thousand	20	2.37	0.85	8	2.50	0.59	-0.13 [0.66]	0.71	0.12
11-20 Thousand	12	2.94	0.96	2	3.43	0.36	-0.49 [0.26]	0.86	0.06
21+ Thousand	4	3.29	0.13	2	3.90	0.27	-0.61 [0.16]	0.67	0.03
All Cities	103	2.17	0.76	136	1.73	0.65	0.45 [0.00]	0.43	1.00

This table presents the summary statistics for log city population in 1800 by institutional change status and initial pre-Reformation city size. Institutional change is measured by an indicator variable for whether a city had a Reformation law by 1600. Populations are measured in thousands: $\ln(population/1000)$. P-values for the statistical significance of differences in means in square brackets 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.

been particularly dynamic and were initially small. We present complete summary statistics on all variables in the analysis in the Appendix.

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

$$Outcome_i = c + \alpha \cdot Law_i + \gamma \cdot X_i + \epsilon_i, \tag{4}$$

where $Law_i = 1$ if city i had a Reformation law. The control variables (X_i) include territory fixed effects, our measure of upper tail human capital before 1517, and the number of university students over multiple periods prior to the Reformation to absorb pre-trends. We also control for whether cities had market rights in 1517, were incorporated by 1517, indicators for printing, universities, Free-Imperial cities, the average number of plagues from 1400 to 1499, the informal diffusion of Protestantism, and geographic controls. We control for initial population either with categorical fixed effects or for log population in 1500, setting this to 0 for unobserved cities and including an indicator for unobserved status. We present detailed summary statistics in Appendix A.

Table 7 shows the results from estimating equation (4). The outcome in Panel A is 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.

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.[34] 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.[35] 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.[36]

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.[37] We find no differential human capital or growth effect for institutions in Free-Imperial cities,

[34]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.

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

[36]We assign a value of 0 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.

[37]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.

Table 7: Institutional Change and Long-Run Outcomes

	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Human Capital						
	Outcome: Ln Upper Tail Human Capital 1750-1799					
Reformation Law	0.351	0.412	0.397		0.297	0.297
	(0.114)	(0.126)	(0.126)		(0.099)	(0.099)
Protestant				0.204	0.185	0.185
				(0.195)	(0.222)	(0.222)
Observations	239	239	239	239	239	239
Panel B: City Population						
	Outcome: Ln Population in 1800					
Reformation Law	0.237	0.259	0.249		0.278	0.263
	(0.118)	(0.115)	(0.104)		(0.090)	(0.088)
Protestant				-0.085	-0.106	-0.122
				(0.177)	(0.175)	(0.214)
Observations	239	239	239	239	239	239
Panel C: Human Capital Intensity						
	Outcome: Upper Tail Human Capital per 1,000					
Reformation Law	0.088	0.112	0.110		0.078	0.075
	(0.035)	(0.036)	(0.042)		(0.043)	(0.044)
Protestant				0.093	0.087	0.090
				(0.085)	(0.084)	(0.087)
Observations	239	239	239	239	239	239
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

This table presents the regression estimates of the relationship between institutional change, measured by Reformation laws, and long-run outcomes. Outcomes are: 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 territory fixed effects and 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. Standard errors are clustered at the 1500 territory level. Territories from Nüssli (2008).

cities with many university students, cities with printing, or cities with market rights. We report these results in Appendix C.

6 Plague Shocks as a Source of Exogenous Variation

The fact that cities that adopted institutional change 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. Consider the example of two neighboring cities, Altenburg and Eisleben. In 1506, Altenburg experienced a plague outbreak and a breakdown in civil order. In the first years of the Reformation, popular mobilization in Altenburg led to anti-Church riots, the city council eventually bowed to popular demands for a Protestant priest in 1522, and institutional change was formalized in 1533 (Frommelt 1838). In contrast, Eisleben experienced no major plagues in the early 1500s and, while becoming Protestant, did not formalize institutional change. In 1500, Eisleben had a larger population than Altenburg. By 1800, Altenburg had population of 9,000, while Eisleben had population of 5,000.

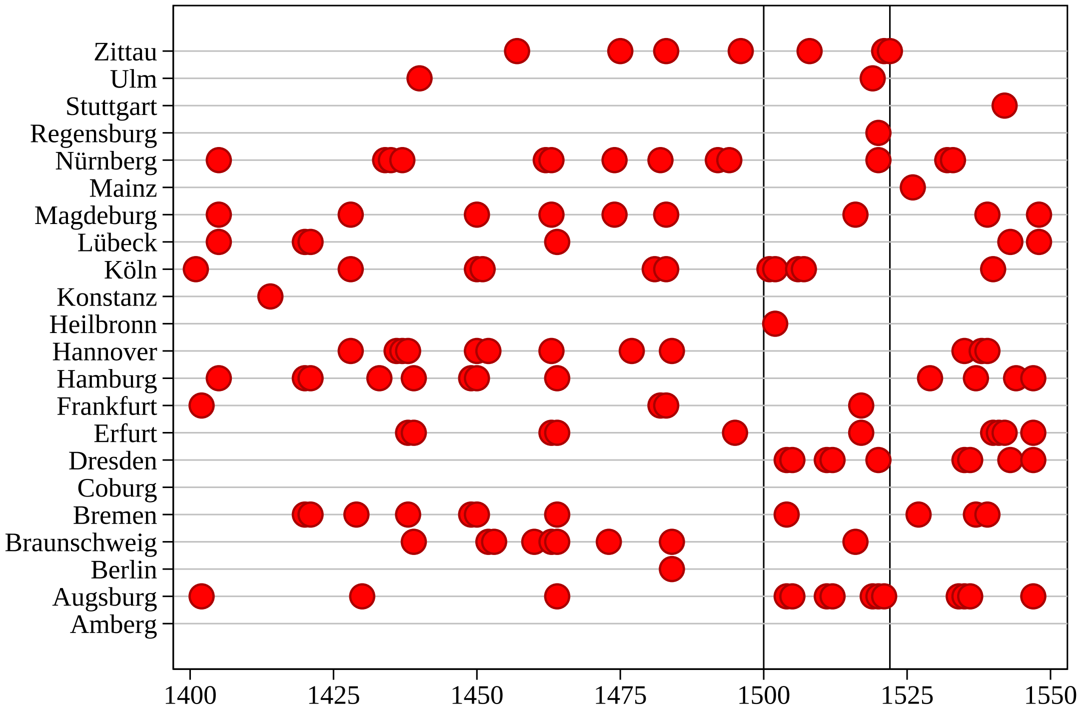
The identifying assumption for a causal interpretation of our IV estimates is that plague outbreaks within a narrow window were exogenous conditional on long-run propensity and other observables. We present evidence for exogeneity and the exclusion restriction that is the other identifying assumption 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

Figure 4: 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.

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 4 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 4 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.

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 pre-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.[38] 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).[39] 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.[40] 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

[38] Almost all Reformation laws contain provisions on directing priests to visit the sick and offer consolation.

[39] Catholic cities outside Germany did develop strategies to address the plague, e.g. in Italy (Cipolla 1992).

[40] 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.

"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:

$$Law_{i,pre-1600} = c + \alpha \cdot \ Plagues_{i,1500-1522} + \beta \cdot \ g(Plagues_{i,1400-1499}) + \gamma \cdot X_i + \epsilon_i \qquad (5)$$

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 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.[41] 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.[42]

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

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

[42]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 – Institutional Change						
	Outcome: Reformation Law					
Plagues 1500-1522	0.137	0.130	0.117	0.129	0.122	0.108
	(0.023)	(0.028)	(0.030)	(0.027)	(0.026)	(0.027)
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	1.618	2.038	2.649	1.931	2.408	3.103
	(0.856)	(0.935)	(0.717)	(1.047)	(0.914)	(0.651)
	Outcome: Ln Upper Tail Human Capital 1750-1799					
Reformation Law	2.789	3.788	4.136	3.202	4.004	4.613
	(1.221)	(1.265)	(1.341)	(1.337)	(1.304)	(1.292)
	Outcome: Upper Tail Human Capital per 1,000					
Reformation Law	0.574	0.747	0.790	0.616	0.706	0.824
	(0.272)	(0.277)	(0.288)	(0.297)	(0.317)	(0.313)
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 use territories recorded by Nüssli (2008). "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.

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 territory 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 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.[43]

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 institutional change was associated with growth, cities positively selected into treatment. However, there is little evidence that the institutional change 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 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

[43] 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.

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.[44] If cities subject to lords had higher returns to institutional change, our IV could recover these returns. However, we find no differential 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

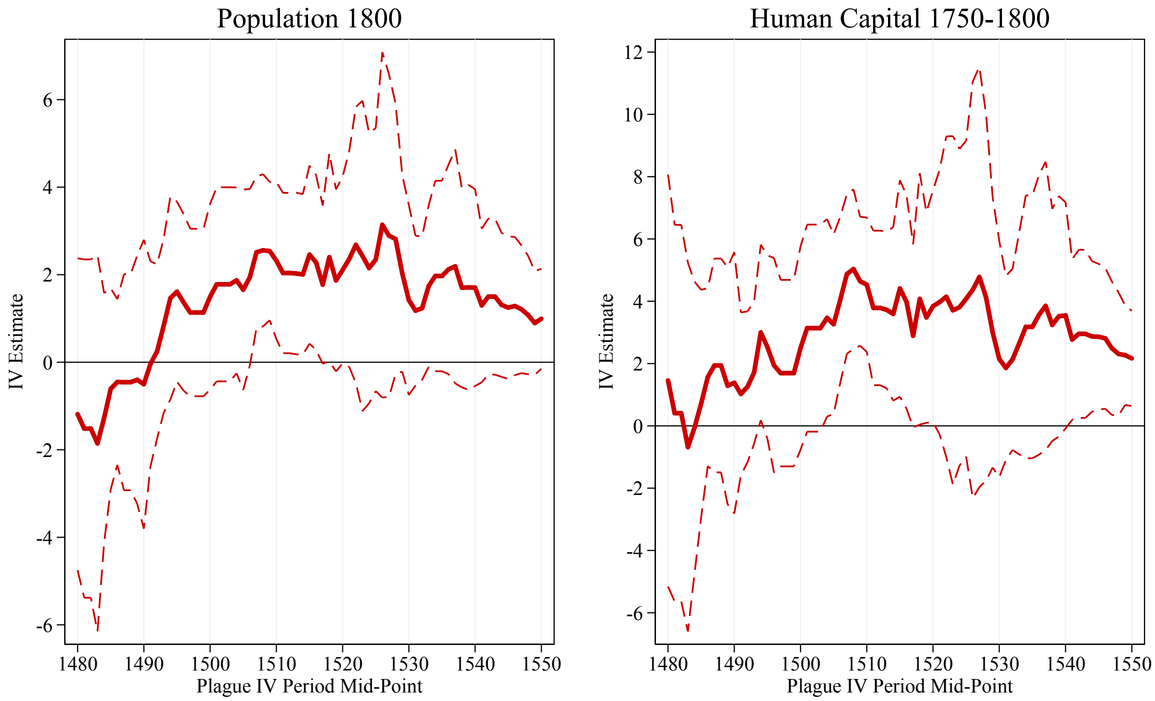
Our identification strategy requires that plague outbreaks in the early 1500s impacted long-run growth only through their impact on institutional change. We present three pieces of evidence that support the exclusion restriction.

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 5). 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 5 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.[45]

[44]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.

[45]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).

Figure 5: 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.

Second, because it is natural to wonder whether plagues in general had a direct effect on long run growth, we study the direct relationship between city population in 1800 and plagues in different periods from the mid-1300s through the late-1500s. We estimate regressions of

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.

the form:

$$\ln(population_{i1800}) = \alpha + \sum_t \beta_t plagues_{it} + \gamma X_i + \epsilon_i \tag{6}$$

The parameters of interest are the β_t, which capture the relationship between long-run population and $plagues_{it}$, which measures the number of plagues in city i in 25-year intervals starting 1350-1374 and running to 1575-1599.[46] 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.

Table 9 presents our results and shows that while early 1500s plagues predict city population in 1800, plagues in other periods across the 1400s and 1500s do not. After the Black Death mega-shock of the mid-1300s, the positive relationship between long-run population and plagues in the early 1500s was unique in its magnitude, precision, and robustness. For example, while plagues in the period 1475-1499 were also positively associated with long run outcomes, the estimated relationship is imprecise and declines in magnitude when we control for initial population in 1300 (column 2), city incorporation and market rights as of 1300 (column 3), and region fixed effects (column 4). In contrast, the relationship between early 1500s plagues and long-run outcomes is robust to controlling for initial observables and studying within-region variation.[47] The fact that plagues in other periods 1400-1599 have no robust relationship with long-run population strongly suggests that long-run outcomes were not driven by the plague's direct economic and demographic impact. These results are consistent with historical evidence indicating that plagues had *long-run* development impacts when outbreaks occurred in critical junctures (Biraben 1975) and with the exclusion restriction required for identification in our IV design.

Third, we also find that plague shocks after 1522 positively predict institutional change and long-run growth for cities that were not early adopters. Table 9 shows that plagues in the period 1525-1549 were not correlated with long-run population growth *conditional* on plague exposure in other periods, including 1500-1524. Given that institutional change was concentrated in the first years of the Reformation era, this raises a question: Did plagues in the period 1525-1549 operate as institutional shifters for cities that remained candidates

[46] We restrict to the period before 1600 because plagues in the 1600s largely reflected the military events of the Thirty Years War and are thus highly correlated with other factors shaping development.

[47] We recognize that we cannot reject the hypothesis that the early 1500s and late 1400s estimates are the same, due to the magnitude of the standard errors on the latter.

Table 9: Historic Plague Outbreaks and City Population in 1800

	[1]	[2]	[3]	[4]
	Outcome: Ln Population in 1800			
Plagues 1350-1374	0.418	0.291	0.293	0.271
	(0.048)	(0.080)	(0.080)	(0.087)
Plagues 1375-1399	0.072	0.115	0.114	0.118
	(0.096)	(0.120)	(0.123)	(0.146)
Plagues 1400-1324	-0.066	-0.080	-0.081	-0.057
	(0.192)	(0.217)	(0.220)	(0.259)
Plagues 1425-1449	0.105	0.132	0.157	0.130
	(0.088)	(0.083)	(0.078)	(0.087)
Plagues 1450-1474	-0.004	0.012	0.009	0.019
	(0.105)	(0.090)	(0.088)	(0.091)
Plagues 1475-1499	0.199	0.132	0.127	0.104
	(0.232)	(0.200)	(0.200)	(0.194)
Plagues 1500-1524	0.193	0.197	0.198	0.221
	(0.069)	(0.073)	(0.075)	(0.089)
Plagues 1525-1549	0.026	0.032	0.022	0.018
	(0.069)	(0.064)	(0.069)	(0.075)
Plagues 1550-1574	0.096	0.088	0.097	0.082
	(0.066)	(0.052)	(0.061)	(0.077)
Plagues 1575-1599	0.001	-0.064	-0.062	-0.038
	(0.051)	(0.057)	(0.058)	(0.070)
Observations	239	239	239	239
R^2	0.41	0.46	0.46	0.52
Population in 1300	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.

for institutional change over this period and if so, with what effects? To fix ideas, consider the city of Hannover which had no plagues from 1500 to 1522, meaning the IV is "turned off" in our baseline analysis. Hannover survived without a law into the 1530s, experienced a plague in 1535, and adopted institutional change in 1536. In the data, we find that plagues in the 1525-1549 period do explain institutional change and population growth for cities like Hannover that were not early adopters. We find that the first stage relationship between recent plagues and institutional change initially strengthened after 1522 and that the relationship between induced institutional change and growth remained relatively stable over the first half of the 1500s. Our analysis comparing the effects of the instrument as it gets "turned on" at different times for different cities provides one external validity check on our baseline estimates and is presented in Appendix D.

7 Conclusion

Institutions that provide non-defense public goods may profoundly shape economic activity. Most economic research on the origins of public goods institutions has studied either the role of defense or institutional changes that 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; Besley and Persson 2011; Gennaioli and Voth 2015). Another literature emphasizes the colonial origins of variation in property rights protections (Acemoglu, Johnson, and Robinson 2001). In contrast, we study historic institutional change that expanded state capacity and occurred when citizens challenged local rulers in non-democratic settings. The resulting institutional changes were designed to produce highly educated administrators and to ensure the functioning of a new social order. We find that institutional change drove increases in upper tail human capital and local growth. More broadly, our research suggests that institutional change during the Reformation era 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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