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

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Abstract

What are the origins and consequences of the state as a provider of public goods? We study legal reforms that established mass public education and increased state capacity in German cities during the 1500s. These fundamental changes in public goods provision occurred where ideological competition during the Protestant Reformation interacted with popular politics at the local level. We document that cities that formalized public goods provision in the 1500s began differentially producing and attracting upper tail human capital and grew to be significantly larger in the long-run. We study plague outbreaks in a narrow time period as exogenous shocks to local politics and find support for a causal interpretation of the relationship between public goods institutions, human capital, and growth. More broadly, we provide evidence on the origins of state capacity directly targeting welfare improvement.

JEL Codes: I25, N13, O11, O43

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"With respect to schools and schooling, the purview of territorial and urban governments in Germany was fixed at an early juncture in the chain of events leading to the established Reformation."

– Strauss (1988), "The Social Function of Schools in the Lutheran Reformation in Germany"

"The revolutionary program came to rest in an established religion; the pamphlet became a church ordinance. . . The new Protestant institutions persisted."

– Ozment (1975), *The Reformation in the Cities*

1 Introduction

What are the origins and impacts of the state as a provider of public goods? Prior to 1500, European states strongly resembled stationary bandits extracting resources to support private goods – consumption for elites, the construction of palaces, and dynastic warfare. In the 1500s, new legal institutions significantly expanded the social welfare bureaucracies and administrative state capacities of cities. These new municipal legal institutions established Europe's first large scale experiments with mass public education. The innovations appeared in what is now Germany during the Protestant Reformation. During the Reformation remarkable institutional variation emerged at the local level, including across neighboring cities within common territories in German-speaking Europe. The institutional variation included fundamental differences across Protestant cities: less than 55 percent of cities that adopted Protestantism as their dominant religion established legal institutions governing the provision of public goods.

In this research, we study the causal impact of the public goods institutions of the Reformation on city growth and human capital. We find that cities that established public goods institutions in the 1500s enjoyed large subsequent advantages in long-run population growth. We use plague outbreaks in a narrow period in the early 1500s as an instrumental variable for institutional change. In the instrumental variable analysis, we control for long-run plague prevalence, which could reflect underlying differences in economic activity and locations, and focus on the quasi-experimental variation during the critical juncture of the early 1500s. To explore channels and develop alternate panel data identification strategies, we examine novel microdata on upper tail human capital. We use the microdata to document that public goods institutions positively impacted the formation and migration of upper tail human capital in the short- and long-run. We find that the upper tail human capital advantages enjoyed by cities with public goods institutions appear following the reforms of the 1500s and persist period-by-period over multiple centuries.

We study the Protestant Reformation as a unique source of evidence on the origins, nature, and consequences of the state as a provider of public goods. A prior literature has emphasized the role of military conflict in the emergence of state capacity, with a focus on fiscal capacities that came to support economic development (Tilly, 1975, 1992; Besley and Persson, 2009; Dincecco and Prado, 2012; Gennaioli and Voth, 2015). A related literature has studied historical institutions that supported economic growth by restricting arbitrary executive authority and establishing inclusive property rights protection (Acemoglu, Johnson, and Robinson, 2005a, 2001; North and Weingast, 1989). Research on the economic impacts of institutions and state capacity has emphasized sources of exogenous variation coming from the supply side or "from above" (Acemoglu, Johnson, and Robinson, 2001; Acemoglu et al., 2011; Besley and Persson, 2011).[1] These literatures focus on the welfare effects of social arrangements whose primary goals were *not* welfare improvement.

The existing literature on the Reformation is extensive, but has not studied the economic effects of institutional change during this social transformation.[2] Our research documents that the Reformation provides a canonical example of the emergence of state capacity and public goods institutions. The new institutions promoted economic inclusion and directly targeted welfare: they supported the provision of education and social services and set up anti-corruption safeguards (Strauss, 1988, 1978; Grell, 2002; Cameron, 1991). Popular politics drove the institutional changes. During the Protestant Reformation citizens' movements pushed for the adoption of public goods institutions (Cameron, 1991; Hamm, 1994; Schilling, 1983; Ozment, 1975; Sehling, 1911).

The reason why these changes occurred in the 1500s relates to the introduction of ideological competition. Before the Reformation, the Catholic Church enjoyed a monopoly in religion and local public goods provision was limited. The Reformation not only introduced religious competition, but was specifically animated by ethical ideas about the common good and corruption among secular

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[1]For example, Acemoglu, Johnson, and Robinson (2001) study the consequences of institutional differences across countries that arose in the colonial era and Tilly (1992), Acemoglu et al. (2011), and Besley and Persson (2009) examine variations in state capacity that emerged due to the exigencies of war or quasi-random military events, all of which can be conceptually thought of as supply-side sources of variation.

[2]For example, using a measure of non-institutional diffusion, Cantoni (2015) documents that the diffusion of Protestantism as the dominant religion at the city-level in the 1500s had no impact on city growth in Germany. In contrast, Becker and Woessmann (2009) find that religion was was strongly associated with education and economic development across counties in early 1800s Prussia, studying as their measure of religion the Protestant share of county population. We discuss how our research supports, reconciles, and extends these arguments below. In other recent research, Cantoni, Dittmar, and Yuchtman (2015) find that locations where Catholic monasteries were closed saw human and physical capital reallocated away from church use (1517-1600) and Basten and Betz (2013) find differences in preferences in Switzerland along the border between historically Protestant and Catholic territories. For a review of the literature see Becker, Pfaff, and Rubin (2015). A larger literature looks at how religion explains differences in performance across countries (Barro and McCleary, 2003) and differences in preferences across individuals in contemporary settings (Guiso, Sapienza, and Zingales, 2003). We focus our discussion here on the economics literature, however a rich body of non-quantitative historical scholarship studies the nature and consequences of Protestant institutions (Strauss, 1978; Witte, 2002; Rittgers, 2012; Grell, 2002; Ozment, 1975, 1980; Weber, 1904).

and Church authorities (Dittmar and Seabold, 2015; Whaley, 2012; Brady, 2009). When the ideas of the Reformation swept across German-speaking Europe, institutional changes in public goods provision at the city-level were driven by the interaction between these ideas and local determinants of demand and political mobilization (Cameron, 1991; Scribner, 1979).

In this research we study both the causes and consequences of these changes in state capacity and public goods institutions. This paper presents the first research that we are aware of to document: (1) the long-run causal impact of the creation of municipal public goods institutions targeting education and welfare; (2) the impact of state capacity on local growth in municipalities directly subject to reform; and (3) the institutional determinants of upper tail human capital as a channel for growth before the industrial revolution.[3]

Our key findings are that after establishing legal institutions to support public goods provision, cities with these institutions grew faster and produced and attracted more upper tail human capital. We examine the relationship between institutions and city growth as a measure of economic dynamism at the local level (Acemoglu, Johnson, and Robinson, 2005b; Glaeser, Scheinkman, and Shleifer, 1995; De Long and Shleifer, 1993). Cities that adopted legal change grew at least 0.1 percent faster per year than both Protestant cities that did not formalize social change in municipal law and Catholic cities. This advantage lasted over several centuries. We also assemble individual-level microdata on the most significant economic and cultural figures in Germany 1300-1800 from the *Deutsche Biographie* (Bayerischen Akademie der Wissenschaften, 2015). We use the microdata to document that the advantages enjoyed by cities with public goods institutions supporting education and economic inclusion date from the era of reform in the 1500s and persist period-by-period through subsequent centuries and later shocks. We find no growth effect from the non-institutionalized diffusion of Protestantism, consistent with Cantoni (2015). Instead we find the relevant variation in formal institutional change and evidence consistent with the view that the "interdependent web of an institutional matrix...produces massive increasing returns" (North, 1990, p. 27).

Our findings naturally raise a question about cause and effect: Did cities selectively adopt different institutional arrangements and are there sources of exogenous variation? More broadly,

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[3]In related research, Acemoglu, García-Jimeno, and Robinson (2015) document the *spillover* impacts of state capacity on outcomes across localities in contemporary Colombia. Duflo (2001) studies the labor market returns in the 1990s associated with a school building program in Indonesia that was initiated in the mid-1970s. Goldin and Katz (2011) study the impact of compulsory schooling and child labor laws from the period 1910 to 1939 on state-level high school enrollments in the US in 1960. Squicciarini and Voigtländer (2015) document the relationship between upper tail human capital and growth as the industrial revolution diffused in France in the late 1700s. Becker, Pfaff, and Rubin (2015) provide a survey of economics research on the Protestant Reformation. We discuss this literature further below.

what was the political process that led to institutional change?

We present an instrumental variable (IV) analysis using plague outbreaks in a narrow window in the early 1500s as an instrument for institutional change. We control for long-run differences in plague prevalence across cities, which may reflect differences in openness and locations, and identify off of variations in plague outbreaks during the critical juncture of the early 1500s. The identifying assumption is that variations in outbreaks in the early 1500s were random, conditional on long-run prevalence. We control for long-run prevalence linearly and with higher-order polynomials. We further control independently for plague in different sub-periods across the 1400s to absorb any long-run trends or shifts in plague prior to the 1500s and improve precision.[4] We also present evidence that supports the exclusion restriction necessary for identification. The early 1500s were a critical juncture in which plagues were associated with institutional change. Viable models of institutional change did not exist in the 1400s; after 1555, the Peace of Augsburg restricted institutional change by establishing a new religious equilibrium in law. We show that plagues in the early 1500s predict long-run growth, but plagues in other periods 1400 to 1600 do not. Because plagues in other periods had the same direct demographic effects as those in the early 1500s, this evidence supports the view that the early 1500s plagues had their effects through the institutional channel.[5]

Plagues mattered for institutional change only after the Reformation introduced political competition. During outbreaks incumbent wealthy elites died or temporarily fled their home cities, reducing their political power (Dinges, 1995). Following outbreaks, migration into cities increased, changing the composition and politics of the population (Isenmann, 2012). The institutional implications of these dynamics were magnified when religious and political competition appeared in the early 1500s. In the market for religion Protestant and Catholic ideas explicitly diverged on the plague. Catholics suggested that epidemic disease was divine punishment for sin. Protestants tended to argue that Jesus had died to absolve human sin. Protestants developed an agenda for a "Christian Commonwealth" in which pastoral care and institutions to promote public health and

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[4]We find no evidence of pre-trends or shifts in plague. Introducing these controls simply improves precision.

[5]Our identification strategy thus uses a source of variation (plagues) to study a causal factor (public goods institutions) neither of which have been examined in the previous literature on the Reformation. In contrast, Becker and Woessmann (2009) study the relationship between the Protestant share of population and economic development across counties in the 1800s using distance to Luther as a source of variation in Protestantism. Cantoni (2015) studies the relationship between the the non-institutionalized diffusion of Protestantism and city growth in a difference and differences set up. Dittmar and Seabold (2015) study shocks to local media markets with printing as a determinant of the diffusion of ideas in the media, but restrict their focus to the smaller set of historical printing cities in German-speaking Europe, including cities in Switzerland, Poland, and other territories not covered in the current research. Our research is very different, in being able to examine the diffusion of the Reformation across all cities in Germany, because the plague shocks we study here are orthogonal to the firm-level shocks previously studied in Dittmar and Seabold (2015), and because not one of those firm-level shocks was caused by the city-level plagues we study. Rubin (2014) documents that cities with printing in 1500 were more likely to adopt Protestantism, studying a measure of Protestantism that includes non-institutionalized diffusion, similar to Cantoni, Dittmar, and Yuchtman (2015).

alleviate suffering figure prominently and interlock with other public goods provisions (Rittgers, 2012; Lindemann, 2010; Roeck, 1999; Grell, 2002; Cameron, 1991).

To address the question about the channels through which institutions impacted growth, and to present identification strategies that use panel data techniques, we study individual-level microdata on upper tail human capital (Mokyr, 2009; Squicciarini and Voigtländer, 2015; Meisenzahl and Mokyr, 2012). The public goods institutions we study contain educational provisions that established Europe's first large scale experiments with mass public education.[6] These provisions shifted schooling from the private sector to the public sector and from being voluntary to compulsory (Strauss, 1978). Historical evidence suggests these changes in education were particularly consequential for the subsequent development of cities. We show that cities that adopted the legal institutions of the Reformation subsequently produced more people with upper-tail human capital, using novel microdata on over 8,000 of the most important economic and historical cultural figures in German-speaking Europe from 1300 to 1820 from the *Deutsche Biographie* (Bayerischen Akademie der Wissenschaften, 2015). We similarly show that cities with the legal institutions of the Reformation began differentially attracting upper tail human capital migrants after institutions were changed. These effects pre-date and continue after later shocks such as the Thirty Years War (1618-1648). The migration effects are driven by differential migration from towns into cities – not by migrants moving from cities without Reformation laws to those with these instititutions. While we cannot rule out growth effects running through other channels, the microdata and narrative evidence strongly suggest that the new legal institutions shaped the trajectory of city development through their effects on education, the acquisition of skill, and incentives to migration.

Our paper relates to several literatures. First, we contribute to the literatures on state capacity, institutions, and economic development. We break new ground by studying the emergence of institutions and state capacities targeting education and welfare. We also contribute by documenting the role of ethical and religious movements in the emergence of state capacity in Europe.[7] Second, we build on and extend the literature on political competition and public goods provision. We study how political competition combined with citizen action in *non-democratic*

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[6]For example, in Venice only 5 of 245 schools observed in 1587 were public and throughout Renaissance Italy education was principally privately organized (Grendler, 1989, p. 43). In France, a considerable number of city and independent secondary schools emerged piecemeal over the later 1500s, to a considerable extent as a response to the perceived failures of the clerical school system. See Huppert (1984).

[7]The religious and popular origins of state capacity are not highlighted in the economics research, to the best of our knowledge. We do, however, build on and extend research on the role of religion in the development of European states in the historical literature (Whaley, 2012; Brady, 2009; Lindemann, 2010; Roeck, 1999; Gorski, 2003).

settings to shape fundamental changes in governance in European history.[8] Third, we contribute to the literature on persistence (Acemoglu, Johnson, and Robinson, 2001, 2005a). Specifically, we study how localized public health shocks in a critical juncture shifted institutions with long-run growth consequences.[9] Fourth, we contribute to the literature on human capital and development. We study institutions that set up Europe's first large scale experiments with mass public education. We also present novel evidence on the formation and migration of upper tail human capital across several centuries before the industrial revolution. Fifth, we contribute to the literature on bourgeois revolutions. The historical literature following Marx has framed the institutional changes of the Reformation as an "early bourgeois revolution," but has devoted relatively limited attention to the expansion of public goods and education provision.[10] We present unique evidence on middle class political mobilization that aimed to constrain local elites and secure the provision of public goods. The evidence we provide suggests that these public goods provisions at the *local level* were highly consequential. The local heterogeneity of the institutional changes in Germany in the 1500s, the emphasis on education, and the centrality of religion distinguish these events from the later classic bourgeois revolutions – England in 1688 and France in 1789.

2   History

The Protestant Reformation involved the diffusion of political ideas and policy programs, not just new religious beliefs in a narrow sense. In this research, we study the spread and impact of an institutional program of public goods provision that emerged during the Reformation. The public goods program was institutionalized at the municipal level by Protestant laws called Church ordinances, but only in half of the cities that adopted Protestantism as their dominant religion.

What factors influenced how the ideas of the Reformation diffused and why some cities adopted the institutions of the Reformation and others did not? To answer this question we draw on a rich body of social, cultural, religious, and economic history evidence. We use this research to characterize the Reformation movement, ideas, and policy goals – and the political economy processes that led to institutionalization or non-institutionalization. The historical characterization

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[8]A large literature studies how political competition shapes institutions and the provision of state services in electoral democracies (Fujiwara, 2015; Acemoglu et al., 2014; Besley, Persson, and Sturm, 2010; Acemoglu and Robinson, 2006).

[9]In a research design that is structurally related to ours, Dell (2012) studies how weather shocks shifted rural insurgency during the Mexican revolution of the early 1900s, leading to persistent differences in rural land tenure institutions and outcomes. In contrast, we study how plague outbreaks in the 1500s shifted local politics and urban public goods institutions that notably supported path-breaking innovations in mass public education.

[10]See Brady (2009) and Dorpalen (1985) for surveys.

we present here motivates the empirical work in the following sections.

2.1 Diffusion of the Reformation and Institutional Change

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

The Reformation was simultaneously a movement for religious renewal and an institutional reform program that set up safeguards against church corruption, extended public goods provision, and transformed the provision of education. The reformists moved to eliminate clerical tax exemptions and economic privileges, promoting economic inclusion at the local level.[11] More generally, reformers called for moral renewal within cities (Moeller, 1972), argued that biblical authority was paramount over and above the authority of existing Catholic Church institutions (Brady, 2009), and were frequently anti-clerical (Dykema and Oberman, 1993).

The adoption of Reformation institutions reflected significant shifts in city politics. The political shifts that led to institutional change were driven by popular mobilization not elites. We focus attention on how institutions were related to growth across cities in the same territory, and hence subject to the same territorial policy environment and laws, in our empirical work (Sections 5 and 6). However, it is important to observe that the city Reformations were citizens' movements that emerged without initial support from oligarchic city governments or territorial lords.[12] Cameron (1991, p. 240) observes, "As a rule neither the city patricians nor the local princes showed any sympathy for the Reformation in the crucial period in the late 1520s and early 1530s; they identified themselves with the old Church hierarchy and accordingly shared its unpopularity. Popular agitation on a broad social base led to the formation of a 'burgher committee'." (Dickens, 1979, p. 20) confirms that city councils did not initiate local Reformations. The constituency for reform came from citizens who were excluded from political power by oligarchic elites, typically lesser merchants and guild members (Ozment, 1975; Schilling, 1983). In Augsburg, the city council was forced to drop its policy of religious neutrality following riots in 1524, 1530, and 1534 (Broadhead, 1979). In Northern cities, such as Rostock, Stralsund, Greifswald, Lübeck, Braunschweig, Lüneberg, Göttingen and Hanover institutional change led by citizens excluded from

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[11]Besides exemption from taxes and civic duties, religious orders enjoyed monopolies on priced religious services (e.g. funeral services) and on the production of products like beer. Reformist arguments and agitational literature frequently raised objections to high prices for essential religious services and to religious tax exemptions (Cameron, 1991; Ozment, 1975).

[12]The discussion in this section draws on Dittmar and Seabold (2015).

political power had a *coup d'état* quality (Cameron, 1991). In Zwickau, Lutheran publications were printed in 1523; the city council attempted to suppress protests in 1524; the Reformation was formally adopted in 1525 (Scribner, 1979). More detailed discussion of individual towns is provided in Appendix E.

There are two key reasons why Protestant institutions were not adopted by all Protestant cities, despite their long-run advantages. First, legal change was driven by the political mobilization of non-elites and popular movements for institutional change faced barriers even in Protestant cities. Second, it unclear whether political actors in cities without laws had the evidence to make clear cross-city comparisons and adjust local institutions accordingly. We observe increases in upper tail human capital migration into cities with laws starting in the 1500s, but whether and how contemporaries observed these differences is very unclear. Moreover, in the city population data the advantages associated with the institutions we study appear to have emerged only over time.

The question of why some cities adopted Reformation institutions while others did not is a political economy question and a question about supply and demand, broadly conceived.

Several features of social landscape shaped how reformist ideas were translated into institutional demands. Notable shifters of demand included: (1) the size and self-organization pro-Reform constituencies, including guilds and merchants not on the city council, (2) the salience of local public health concerns and challenges, (3) the nature and extent of local Catholic Church corruption and monopolies, and (4) the nature of local culture, including Germanic humanism (Ozment, 1975; Cameron, 1991; Dittmar and Seabold, 2015). We examine a rich set of measures and proxies for these determinants of demand in our empirical work below, but we share the fundamental question of whether cities where Reformation ordinances were adopted were different on unobservable dimensions that may directly account for their subsequent superior growth. This question motivates the instrumental variable strategy we develop below using the timing of plague shocks to isolate exogenous variation in institutional change.

Cities were also exposed to variation in the supply of reformist ideas. Historians (Eisenstein, 1980; Brady, 2009) and economists (Rubin, 2014) argue that the printing press technology shifted the supply of Reformist ideas. Recent research argues that the diffusion of Protestantism in the media was driven by competition in the use of printing technology rather than technology *per se* (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.[13] Another supply-side

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[13]We study shocks to cities from plagues which shifted demand. In contrast, Dittmar and Seabold (2015) study

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factor was distance, as Lutheran ideas diffused to other parts of Germany from Wittenberg (Becker and Woessmann, 2009). We study these determinants of supply in our empirical work below.

2.2 The Municipal Institutions of the Reformation

Protestant reformers designed interlocking institutions with a legal foundation. The objective was to formalize and reinforce the new system of beliefs and to transform the provision of religious and social services. The key institutional innovation was the church ordinance (*Kirchenordnung*). The name "church ordinances," if taken literally, is somewhat misleading. These ordinances became the civil and religious law of cities. These laws changed municipal institutions by transferring control of service provision from the Catholic Church to the temporal rulers, establishing binding guidelines for a new society, and initiating fixed investment commitments.[14]

Church ordinances were laws that established the Protestant institutional agenda. These laws contain provisions covering: (1) how to conduct mass; (2) the provision of and rules governing public education; (3) the provision of health care, including the establishment, staffing, funding, and eligibility requirements for treatment at hospitals and hospices; (4) the expansion of social insurance and transfers such as poor relief, as well as revenues; (5) the regulation public life and behavior; (6) compensation for priests, teachers, and their assistants; and (7) quality control of priests and teachers. In general, the ordinances institutionalized the redistribution resources towards lower income families, and ensured some measure of equal opportunity by providing public assistance for education. A key policy innovation that expanded state capacity was the introduction of a "common chest." The common chest was a literal lock-box for funds used to support education, scholarships, poor relief, and other public goods provision. The common chest was established by laws that also set out regulations concerning audits and other anti-corruption measures.

The education provisions are of special interest because of the channels through which Protestantism shaped long-run outcomes. The most fundamental provisions established compulsory public schooling.[15] The educational provisions aimed to make the Reformation irreversible – by producing high-quality public servants to staff expanding Protestant church and state bureaucracies and by producing disciplined Protestant subjects. The principal motivation for the educational

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shocks to firms and the local competitive environment which shifted supply. Because these shocks are deaths of individual printers it is natural to wonder whether their deaths were in fact caused by the plagues we study in this research, however every printer death documented in Dittmar and Seabold (2015) occurred outside of plague outbreaks studied here.

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

[15]In Ulm written excuses were required from parents for truant children. In Stralsund, the beadle (a church official) was sent to locate absent children. In Hamburg, municipal authorities began paying for school heating when it was discovered that parents were reluctant to have their kids go to cold school in winter. See Strauss (1978).

provisions was *not* the view that all citizens or even all men should read the Bible. Bible reading was not a center-piece in Lutheran education. Most elementary school curricula make no mention of Bible reading and instead focus on catechism lessons (Strauss, 1978).[16]

These institutional changes were associated with subsequent differences in education and service provision, including school construction. Figure 1 compares new school building in cities with and without Protestant church ordinances across the 1500s. Figure 1 shows that legal changes were associated with subsequent differences in fixed educational investments.[17] However, in many Protestant cities new schools were established in former Catholic Church buildings (Ocker, 2006). Hence new school construction should be taken as just one indicator of the way institutional change was associated with on-the-ground shifts in education and service provision.

While we highlight the importance of legal interventions in education, the consequences of Reformation ordinances arguably flowed from the interlocking nature of these institutional innovations. The church ordinance of Wittenberg provides an example of how specific institutional innovations addressing potentially distinct challenges were related and made mutually reinforcing in law. In 1522, the town council of Wittenberg adopted a Reformation ordinance establishing a common chest. The ordinance stipulates that all church income is to be collected under one administration, equally shared by the town council, community, and pastors. The ordinance indicates that these resources will be used to pay for the care for the poor and sick. The ordinance also indicates that low-income parents are to be given financial support so they can afford to send their children to school or university.

Historical evidence indicates these institutional changes transformed public services and economic life where they were adopted (Strauss, 1978; Witte, 2002; Grell, 2002). Legally, the Catholic church was displaced and temporal rulers became the relevant authority. Spiritually, suffering was no longer seen as necessary for redemption. Intellectually, education was emphasized and made accessible to all. Financially, resources were redistributed to benefit society at large. Given the scope of the institutional change, we study whether church ordinances had effects on human capital and long-run economic growth.

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[16]The evidence suggests that Lutherans were concerned that the complexity of the Bible and independent reading might yield unorthodox views and practices. "This is why so little encouragement was given in the pupil's formal education to individual Bible reading. Most school plans make no mention of it at all" (Strauss, 1988, p. 202).

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

3 Definition of Institutional Treatment

Our baseline measure of institutional treatment is exposure to city-level ordinances that established institutions that survived until at least 1600. We study institutional innovations that had not been reversed by 1600 to focus on the effects of changes that were not short-lived. Cities that adopted the institutions of the Reformation and remained Protestant are considered "treated." Cities that remained Catholic or that became Protestant without formalizing public goods in legal institutions are "untreated." Cities where where the institutions of the Reformation were adopted and persisted despite later re-Catholicization are considered as treated. A small number of cities where Protestant institutions were eliminated after a few years are considered untreated.

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 formalize Protestant public goods provision in law (Speer, 2014). Augsburg and Amberg are examples of cities where the public goods 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 Protestant public goods 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). Münster and Beckum are examples of cities where Protestant institutions were eliminated after a few years.

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

4 Data

Definition of Sample

We focus on institutions and outcomes in cities and towns in contemporary Germany.[18] These

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[18]We do not study ordinances adopted in castles, religious establishments, and small villages. We also defer analysis of laws passed at the territorial level and emphasize within-territory variation.

cities form the overwhelming majority of historically German-speaking cities and for these cities we are able to construct evidence on a rich set of characteristics from the *Deutsches Städtebuch*, as described below. We study the 239 German-speaking cities with population observed in 1800 in Bairoch, Batou, and Chèvre (1988) and non-institutionalized diffusion or non-diffusion of Protestantism recorded in Cantoni (2012) in our principal analysis.

Legal institutions of the Reformation

Our key measure of persistent institutional change is the presence of a Protestant church ordinance in 1600. Our principal data source on Protestant church ordinances is the 21 volume collection *Die evangelischen Kirchenordnungen des XVI. Jahrhunderts* edited by Sehling (1911).[19] We also examine Richter (1846a,b) and other sources described in Appendix A.[20]

Figure 2 maps the cities in our data and illustrates the local variation in which cities had Reformation laws. Figure 3 shows the cumulative share of cities with Reformation ordinances in each year. Most cities passed their first ordinance by 1545. In 1546, the Schmalkaldic War broke out between Protestant and Catholic princes and largely arrested the city-level diffusion of the Reformation. The Augsburg Settlement (1555) ended hostilities and established a new religious and institutional equilibrium. 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 (Dittmar and Seabold, 2015).

City Level Characteristics

Data on market rights and dates of city foundation and incorporation are from Cantoni and Yuchtman (2014), based on the *Deutsches Städtebuch*. Data on books printed in each city pre-Reformation are from Dittmar and Seabold (2015). Evidence on students from each city receiving a university degree in Germany from 1398 to 1517 are from Cantoni, Dittmar, and Yuchtman (2015). Data on navigable rivers, the ecclesiastical status of cities, monasteries and mendicant orders at the city level, and the adoption of Protestantism as the dominant city-level religion are from Cantoni (2012).

City Populations and Upper Tail Human Capital

City population data 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.

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[19]These volumes were published from 1902 to 2015. We refer to them in the text as Sehling (1911) as a shorthand. A complete list these volumes is provided in Appendix A, which provides for description of the data.

[20]We restrict our sample to cities in the Holy Roman Empire as of 1500, following Cantoni, Dittmar, and Yuchtman (2015).

Data on individuals with upper tail human capital are from the *Deutsche Biographie* (Bayerischen Akademie der Wissenschaften, 2015). We identify over 8,000 individuals born in or migrating to our baseline set of cities from 1300 to 1820.

Plague Outbreaks

We construct city-year level data on major plagues outbreaks from Biraben (1975), *Les Hommes et La Peste*, 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 major outbreaks, motivated by the fact that the plague outbreaks were highly 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. The Biraben (1975) data record over 500 major outbreaks of the plague in German cities from 1350 to 1600. These outbreaks are recorded because they were large enough to leave a trace in the historical record.

5   Correlations Between Institutions and Long-Run City Growth

In this section, we document the relationship between the municipal public goods institutions of the Protestant Reformation and city population growth. We study city population as a measure of local economic activity in European history, where direct measures of output or municipal tax revenues are not available, and as a measure of revealed preferences as in Tiebout sorting. Our study of city population is motivated by the literature on city growth (De Long and Shleifer, 1993; Glaeser, Scheinkman, and Shleifer, 1995; Acemoglu, Johnson, and Robinson, 2005a).[21]

We test the hypothesis that cities with city-level Reformation laws by 1600 experienced more rapid population growth and were larger by 1800. Our key finding is that cities exposed to the reformation institutions grew to be significantly larger in 1800 than cities that were observably similar in 1500 but were not treated by these institutions. In contrast, we find that non-institutionalized Protestantism was not associated with growth, consistent with Cantoni (2015).[22]

Table 1 shows the summary statistics of our key regression variables and shows that 43 percent

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[21]De Long and Shleifer (1993) and Acemoglu, Johnson, and Robinson (2005a) study city population as a measure of long run economic dynamism in history. Glaeser, Scheinkman, and Shleifer (1995) document that U.S. cities with more schooling in 1960 experienced a faster income and population growth rate over the next 30 years.

[22]We report cross sectional results because panel data on city populations is unbalanced and populations for a large proportion of cities "treated" by institutional change are only observed in the post-Reformation period. For this reason, our set-up differs from Cantoni (2015), which studies the unbalanced panel of German cities, however we report similar results examining panel data in Appendix B. In addition, our analysis of individual-level microdata allows us to construct balanced panel data on upper tail human capital in which we document large and significant effects of Reformation legal institutions in the post-Reformation period. See Section 7 below.

of cities in our sample have a Reformation law. Table 2 splits the sample by law status. In 1500, cities with laws were not significantly different on human capital dimensions, as measured by the number of books printed, the number of students getting university degrees, or the presence of universities. Among cities with population observed in 1500, cities with laws were larger in 1500. Cities with laws were more likely to be Reichstädte (free cities) and also had higher long run propensity to experience plague shocks. We control directly for these factors in our analysis below. In addition, we provide evidence that the adoption of public goods institutions predicts which towns transitioned to become cities with population observed in 1800 (see Appendix B for discussion).

To document the relationship between the city-level Reformation law and long-run city growth, we estimate the following regression.

$$Log\ Population_{i,1800} = c + \alpha \cdot Law_{i,pre-1600} + \gamma \cdot X_i + \epsilon_i, \tag{1}$$

where $Law_{i,pre-1600} = 1$ if city i had a city-level church ordinance by 1600, and X_i contains control variables.

We first document the baseline correlation between city size in 1800 and the institutions of the Protestant Reformation. We then introduce a series of controls to show that the significance and magnitude of the estimated relationship between institutions and subsequent city size is robust to controlling for a rich array of observables, including time invariant city characteristics, and time-varying measures of human capital production in the run-up to the Reformation.

Table 3 shows the results from estimating equation (1). In column 1 we control only for territory fixed effects and population in 1500. We control for initial population using five indicator variables: population in 1500 unobserved; 1,000-5,000; 6,000-10,000; 11,000-20,000; and more than 20,000. We control for the territory to address the possibility that our city-level measure of institutions may be correlated with, and pick up variation in urban growth driven by, territory-level policy or institutions. The within-territory estimate implies that cities with Reformation law by 1600 were 26 percent larger in 1800 – that is, cities with a Reformation law grew 0.1 percent faster annually. Our baseline estimates cluster standard errors at the territory level. Because the 31 territories in our analysis provide a small of clusters, we also report 95 percent confidence intervals estimated using the Wild bootstrap methodology of Cameron, Gelbach, and Miller (2008) and find that the significance of our key parameter estimates is robust. The within-territory variation we observe is consistent with a large body of narrative evidence indicating that territorial lords had limited state capacity and that important institutional and development heterogeneity is observed at the city

level (Whaley, 2012).

A natural question is whether the baseline relation between growth and institutions reflects observable pre-Reformation differences between cities that subsequently did and did not adopt Reformation laws. To address the question of selection on observables, we present regressions documenting the robustness of our results to a rich array of controls. We also show that cities that adopted Reformation laws did not have a population advantage before the Reformation.[23]

Given the importance of human capital in the Reformation, it is particularly notable that we do not observe systematic differences in human capital before the Reformation across (i) cities that turned Protestant and adopted a Reformation law, (ii) cities that turned Protestant but did not adopt a Reformation law, and (iii) cities that remained Catholic during the Reformation. Table 2 shows that there are no significant differences between treated and untreated cities in having a university, the number of books printed on the eve of the Reformation, or in the number of locals obtaining university degrees at any German university.[24]

To show that our main result holds controlling for initial conditions, we add large set of control variables. We use market rights by 1517 and town incorporated by 1517 to proxy for commercial activity, four categorical indicators for the number of books that were printed in a city by 1517 (0, 1-100, 101-1000, more than 1000), an indicator for the presence of a university by 1517, and the average number of plagues between 1400 and 1499 to control for health shocks potentially affecting city population and growth prospects in 1500. We also control flexibly for the number of university students from city i enrolling in any German university in each 10-year period from 1398 to 1508 to proxy for pre-Reformation human capital and tastes for education at the local level. The point estimate, shown in column 2, remains the same as in column 1. The point estimate of Reformation law also remains unchanged when we use longitude, latitude, and their interaction as proxies to control for potential growth advantages of proximity to Atlantic ports and city age in column 3.[25]

It remains natural to wonder whether Reformation laws capture variation in the general but not strictly institutional diffusion of Protestantism. To disentangle variations in growth explained by institutional change from variations explained by the diffusion of non-institutionalized Protestantism we do two things. First, given evidence that Protestantism spread in concentric circles around Wittenberg (Becker and Woessmann, 2009), we control for distance to Wittenberg.

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[23]In Section 7, we study microdata on upper tail human capital and document period-by-period growth effects.

[24]In Appendix D, we present microdata on university degrees 1390-1517 to document that there are no differential pre-trends in the total number of university students coming from (i) Protestant cities with laws, (ii) Protestant cities without laws, or (iii) Catholic cities.

[25]Our inclusion of this variables is motivated by Cantoni's (2015) argument that longitude proxies for city age because of the historic Eastward movement of Germanic settlers and that latitude proxies for distance to Atlantic ports, which fostered growth from 1600 to 1800 (Acemoglu, Johnson, and Robinson, 2005b).

Second, given that Cantoni (2012, 2015) constructs data on which cities adopted Protestantism as the dominant religion and finds these cities did not grow faster, we study the relationship between Reformation law and growth conditional on inclusion of Cantoni's measure of which cities had Protestantism as the dominant religion. Here we also use the same controls as Cantoni (2012).[26] Column 4 shows the results of estimating equation (1) with measures of diffusion of Protestantism as explanatory variables. Consistent with Cantoni (2015), we do not find that diffusion of Protestantism effected city growth.[27] In column 5, we also add Reformation laws. Controlling for the diffusion of Protestantism, the point estimate on Reformation laws remains positive and significant. In this subsample with Cantoni (2012) controls, the point estimate implies that cities with Reformation law by 1600 were 31 percent larger in 1800.

One potential concern our baseline set up raises is whether we introduce measurement error or sample selection effects that increase our estimates by controlling for population in 1500 categorically in bins, one of which comprises cities with unobserved population. To explore this possibility, we restrict our sample to cities for which population is known and control for log population in 1500. In this subsample we find a larger point estimate on Reformation laws that is significant when we construct standard errors using the Wild bootstrap but not when we cluster at the territory level.

To ensure that Reformation laws do not just proxy for city size, picking up residual unobserved variation in city size not captured by our control variables, we regress Reformation laws on population in 1500. We find that Reformation laws have a smaller and statistically insignificant relationship with city size in 1500 (column 7).[28] However, because our estimates in the sample with 1500 population are imprecise we cannot claim that the estimate for 1800 is significantly different than the estimate for 1500. To provide more definitive evidence that significant differences in outcomes are observed only after institutional change we present an instrumental variable analysis and panel data estimates using microdata on upper tail human capital in Sections 6 and 7.

The identifying assumption for a causal interpretation of our estimates is that laws were uncorrelated with unobserved characteristics that predict growth. Questions about unobserved factors motivate our instrumental variable and difference-in-differences analyses below.

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[26]The controls include Protestant indicator, river indicator, Hanse indicator, Reichsstadt indicator, year city founded, university indicator, printing press indicator, and monasteries.

[27]We examine the cross section because population for many treated cities is only observed after the Reformation. In contrast, Cantoni (2015) studies the unbalanced panel. We provide discussion and analysis of the unbalanced panel Appendix B and examine a balanced panel data on city-level upper tail human capital below.

[28]Appendix B examines the data as a panel in greater detail. Section 7 uses panel microdata on the formation and migration of upper tail human capital from 1300 to 1800 to show another margin along which differences in populations respond to the timing of Reformation laws.

6 Plague Shocks as a Source of Exogenous Variation

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

In this section, we use data on major plague outbreaks to isolate plausibly exogenous variation in institutions. We use variation in major outbreaks in the early 1500s, conditional on long-run propensity to experience the plague, as a measure of short-run shocks. The intuition is that cities had different underlying likelihoods to get plague over the long-run, but that outbreaks within a narrow window are exogenous conditional on long-run propensity.

We present an instrumental variable (IV) analysis in which the number of major plagues at the city-level 1500 to 1522 is the instrument for institutional change, conditional on plague outbreaks 1400 to 1499. We provide evidence that exposure to outbreaks in the early 1500s shifted the probability of adopting a Reformation law, conditional on long-run prevalence. We control for plagues in the 1400s to isolate plausibly random variation in early 1500s outbreaks that does not reflect differences in record keeping, openness to trade, or other sources of endogeneity. We control for the level and for higher order polynomials of plagues across the 1400s. We also control for independently for the number of plagues in different sub-periods within the 1400s. We use variation in laws induced by early 1500s plagues to study the impact of institutions on growth. This IV set up enables us to test and provide evidence that supports a key hypothesis in the literature on the plague: The long-run impacts of historic outbreaks operated through indirect (e.g. political or institutional) rather than direct demographic channels and are observed when the timing of outbreaks interacted with other factors that made social change more likely (Biraben, 1975).[29]

6.1 Why Plague Shocks Provide Exogenous Variation

Major plague shocks in the early 1500s delivered exogenous variation in institutions for two reasons. The first relates to the randomness in the timing of the plague and bears directly on the conditional exogeneity requirement for instrumental variable identification. The second relates the magnitude of these shocks, how plague impacted politics in the early 1500s, and thus relevance.

The epidemiological and historical literatures strongly indicate that the *short-run* distribution of plague outbreaks was random, conditional on observables such as cities' long run plague prevalence (Biraben, 1975; Slack, 1988). Historians observe that historic plagues outbreaks are

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[29]Biraben (1975, p. 189) specifically observes of the plague: "Son influence lointaine n'apparaît que par les conséquences indirectes...en particulier, losque'lle intervient à une moment difficile."

characteristically observed in "compartmentalized" locations and not as spreading neighbor-to-neighbor.[30] 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 its "irregular timing" (Slack, 1988, p. 435). Figure 4 illustrates these facts by showing the dates of plague outbreaks for select cities 1400 to 1550 and by highlighting the variation that provides the IV (1500 to 1522). Consistent with historical observation, there is no clear pattern at the city-level. Some cities experienced outbreaks frequently but with considerable differences in the timing. Other cities experienced outbreaks at different times despite being geographically close, for example Mainz and Frankfurt am Main, which are less than 50 kilometers apart. Some cities experienced few or no major outbreaks despite being important urban centers, like Frankfurt, Ulm, and Regensburg. Figure 5 presents the total number of plague outbreaks for the cities in our data and shows that there is no obvious trend or periodicity in the time series from 1400 through 1600.[31]

By using variation in plague within a narrow time period as our instrumental variable, we are able to control for long-run differences and non-linear trends in city plague prevalence. This allows us to isolate shocks as against variations in plague that might be correlated with city characteristics and locations – e.g. openness to trade – that could directly shape economic development.

Plague shocks in the early 1500s delivered variation in institutions because of the scale of suffering they precipitated and because of how these shocks interacted with religion and politics in the critical juncture of the Reformation. Plague outbreaks caused extreme increases in mortality. It was not unusual for 1/4 of a town's population to die (Slack, 2012). "The critical nature of the phenomenon to which early modern Europeans had to respond can be summarized briefly. The mortality levels reached during outbreaks of the plague were unparalleled" (Slack, 1988, p. 434).[32] During the critical juncture of the early 1500s, plagues shifted the probability of adopting a Reformation law. We present evidence suggesting that these effects worked through political and demand channels.

Plague outbreaks shifted local politics. During outbreaks, elites died and fled, often resulting in a breakdown of civic administration. Plague outbreaks were also followed by an influx of migrants

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[30]Biraben (1975, p. 285): "si l'on porte sur une carte tous les lieux touché par l'epidemie...loin de s'étendre par voisinage, elle est compartimentée en foyers, presque tous isolés les uns des autres."

[31]We document that there were no differential trends in outbreaks in cities that were more connected to trade networks between 1400 and the late 1500s in Appendix C. We also show that there are no non-linear increases in plagues in more connected cities in Appendix C. The Black Death is reflected in the spike in outbreaks at 1350.

[32]Deaths due to plague were in addition especially painful. Typical symptoms included delirium, fevers, painful swelling of the lymphnodes, and the expulsion of putrid matter. Towards the end of the 15th century, the plague was a central feature of life and German culture. Plague saints became popular. Saint Rochus, previously unknown in historical Germany, become popular as protector against the plague. Newly commissioned altars often featured the plague, as so-called Pestaltare (Dormeier, 1989).

to cities, changing the composition and politics of cities.[33] We provide a detailed discussion of these dynamics in Appendix C.

Plague outbreaks also shifted preferences over institutions and religions. The plague and public health provision figured prominently in the Protestant institutional agenda. Protestants explicitly endorsed municipal public health interventions and developed institutions to provide health and pastoral care. Almost all church ordinances contain provisions on directing priests to visit the sick and offer consolation. The Catholic position on the provision of health care was different: Most Catholic theologians "rejected public participation entirely or wanted to allow it in only very reduced measure" (Roeck, 1999, p. 286).[34]

Health shocks shifted local politics and religious preferences and thus drove institutional innovations. Due to the plague, "the towns of early modern Europe first developed sophisticated mechanisms intended to control the spread of infectious disease and to mitigate its effects... this necessitated the growth of local administrative machines and an *expansion of state power*" (Slack, 1988, p. 433 – emphasis added). Protestant reformers delivered blueprints for these institutions that were more likely to be adopted in places that experienced shocks.

6.2 Instrumental Variable Estimates

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

$$Law_{i,pre-1600} = c + \alpha \cdot Plagues_{i,1500-1522} + \beta \cdot g(Plagues_{i,1400-1499}) + \gamma \cdot X_i + \epsilon_i \qquad (2)$$

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. We thus begin by studying how plagues that hit the generation in place when the Reformation began shifted the probability of institutional change (the impact of plagues in other periods is discussed below). We flexibly control for plague prevalence between 1400 and 1499 to isolate plausibly exogenous variation in outbreaks in the Reformation era. We control for the average annual level of outbreaks and for

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[33]Isenmann (2012) observes that typically the number of new burghers (*Neubürger*) rose dramatically after plagues. Burghers were property owning citizens with voting rights. The fact that 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.

[34]Municipalities in Catholic regions of Europe outside Germany did develop strategies to address the plague, notably in Italy (Cipolla, 1992). The divergence in the response to plague outbreaks between the Reformers and the Catholic Church in Germany is highlighted by the debate between Andreas Osiander, a Protestant theologian, jurist, and academic, and Johannes Eck, arguably the most prominent Catholic theologian, inquisitor, and academic of the Reformation era in Germany (Rittgers, 2012). In 1533, Osiander preached a famous "Plague Sermon" and authored the influential Nürnberg Reformation ordinance. Osiander's message in the sermon and the provisions he wrote into the law emphasized consolation, the reduction of suffering, and the argument that humans do not need to suffer as god suffered for us. In contrast, Eck argued that suffering is necessary penance for our sins.

higher order polynomials of prevalence using quadratic and cubic transformations. We also include separate controls for the number of plague outbreaks in each quarter-century across the 1400s in some specifications: the number of plagues 1400-1424; the number of plagues 1425-1449; the number of plagues 1450-1474; and the number of plagues 1475-1499.[35] 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 assumption is that variation in plague in the early 1500s was exogenous conditional on the observables. 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.[36]

Table 4 shows the results from estimating equation (2) and then equation (1) with the variation in $Law_{i,pre-1600}$ induced by plague outbreaks. Column 1 shows that $Plagues_{i,1500-1522}$ is a strong predictor for the adoption of a Reformation law. Each additional plague outbreak between 1500 and 1522 increases the propensity of adopting a Reformation law by 13 percentage points. The F-statistic on the excluded instrument is above 39 indicating a strong first stage. The point estimate of the second stage implies that a city with a Reformation law by 1600 was 1.64 log points larger in 1800 than a city without a law. Our second stage results are slightly stronger and more precisely estimated when we control for polynomials in long-run plague prevalence (column 2). The second stage results are even stronger and more precisely estimated when we also control for plague in different periods across the 1400s (column 3). The results are slightly stronger when we introduce state fixed effects and identify off of variation across cities within states (columns 4 to 6). These results all control for population in 1500 categorically, with one category for unobserved population.

We also estimate instrumental variable regressions studying the subset of cities for which population in 1500 is known and control for log population in 1500 (columns 7 to 9). This reduces the sample to 110 cities. In this sample, $Plagues_{i,1500-1522}$ remains a strong predictor for Reformation law. The first stage F-statistic is 11.14 conditional on the long-run level of plague (column 7) and falls to just over 7 when we control for higher order polynomials (columns 8 and 9). The IV estimate suggests that having a Reformation law increased city size by at least 2 log points over a 200+ year period (Panel B, columns 7 to 9). In this subsample, there is limited within-territory variation so we present results without fixed effects. Including fixed effects significantly reduces the strength of the first stage. However, the point estimate of having a Reformation law

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[35]These controls allow us to condition on shifts or trends in city-level plague over time. In Appendix C we present additional results showing that there is no evidence of systematic trends or non-linear movements in plague towards cities with economically advantageous locations.

[36]Our results are robust to also controlling for non-institutionalized Protestantism. As shown above, Protestantism *per se* does not predict city growth. We similarly show that laws and not non-institutionalized Protestantism are what matter for upper tail human capital in analysis below.

increases even further.

The instrumental variable estimates are larger than the ordinary least squares (OLS) estimates reported in Section 5. The OLS results imply that cities with Reformation law by 1600 were about 0.25 log points larger in 1800 than comparable untreated cities. The instrumental variable design estimates a growth advantage of about 1.64 to 2.55 log points. The OLS estimates imply an annual growth rate advantage of 0.1 percent for the typical treated city. The IV estimates implies an annual growth rate advantage of approximately 0.6 to 0.9 percent. Strikingly, we find similar size effects in our study of upper tail human capital using difference-in-difference designs with city fixed effects and relying on individual-level data from independent sources (Section 7 below).[37]

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

We study whether the interaction between plague shocks and city characteristics shaped institutional change in Appendix C. We examine how plague explains the institutional outcome and 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 mobilization and political

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[37]For comparison, Acemoglu, Johnson, and Robinson (2005b) 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.

change were otherwise highest. 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. If cities subject to lords also had higher returns to innovations in public goods provision, our IV could recover these returns. However, we find no evidence that public goods institutions had a differential growth effect in cities in cities subject to lords.[38]

Another possibility is a violation of the exclusion restriction. In the next section we present evidence to document the unique relationship between long-run growth and plague shocks in the early 1500s as opposed to plagues in other periods. This evidence supports the exogeneity and exclusion restrictions on which the research design rests.

6.3 Evidence in Support of the Exclusion Restriction and Identification

Our identification strategy requires that plague outbreaks in the early 1500s impacted long-run growth only through their impact on institutions. It is natural to wonder whether plague impacted city growth through direct economic and demographic channels. In this section, we document the unique relationship between early 1500s plagues and long-run growth. We show that early 1500s plagues predict long-run city populations while similar plagues in other periods 1400 to 1600 do not. The fact that similar plagues in other periods did not shift long-run outcomes supports the view that plague did not operate through direct channels and instead through the political and institutional process of the Reformation, consistent with the social history (Biraben, 1975).

We first document the unique relationship between early 1500s plague and growth by flexibly estimating the period-by-period relationship between plague in all periods from 1350 to 1600 and city population in 1800. We estimate the following regression:

$$Log\ Population_{i,1800} = c + \sum_{t=1350}^{1575} \alpha_t \cdot Plagues_{i,t} + \beta \cdot X_{i,1300} + \epsilon_i, \tag{3}$$

where $Plagues_{i,t}$ is the number of outbreaks in city i during 25-year period starting in year t. The parameters of interest are the α_t. Our hypothesis is that α_{1500} is significant and positive. The X_i contains control variables: market rights by 1300; incorporation by 1300; and categorical indicators for population in 1300 (unobserved; 1,000-5,000; 6,000-10,000; 11,000-20,000; more than 20,000).

Table 5 presents regression estimates for equation (3) and shows that plagues between 1500 and 1524 have unique explanatory power in predicting long-run city populations.[39] Because outbreaks

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[38]For example, when we augment our baseline regressions to include the interaction between institutions and lord rule, this interaction does not explain growth conditional on the other covariates including the direct effects.

[39]Plague largely vanishes by the second half of the 17th century. The first half of the 17th century was characterized

in the early 1500s were no more frequent or severe than similar outbreaks in the 1400s or the late 1500s, this evidence strongly suggests that the long-run impact of plagues in the early 1500s ran through the institutional channel and not through the direct impact of plague on city population, relative factor scarcities, and the returns to labor. The only other period in which plague outbreaks predict long-run city growth was the Black Death mega-shock of the mid-1300s, when many more cities experienced outbreaks, mortality rates are believed to have been higher, and the unique nature of the demographic shock arguably shifted the European economy as a whole onto a new growth path (Voigtländer and Voth, 2013).[40]

The unique significance of plagues in the early 1500s for growth through the institutional channel is also supported by comparisons across regressions that use plagues in other narrow periods besides the early 1500s as candidate IVs. We estimate these regressions using our baseline IV specification (equation 2), but vary the time-period window in which we observe the plague IV. We compare IV estimates as we shift a window of a fixed size (twenty-three years) over time. We find no significant first stage relationship between plagues and institutions, and no significant reduced form relationship between plagues and growth, in any period across the 1400s. In the early 1500s, this changes. Figure 6 plots the IV estimates and shows that the significant relationship between growth and variation in institutions induced by plagues is only observed in the early 1500s. 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 7 below). 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.

We further examine how the IV analysis predicts institutional change and growth driven by plagues that occurred during the Reformation in Appendix C. Specifically, we document how plagues in later periods such as the 1530s explain institutional change and growth for cities that were not early adopters of the public goods institutions of the Reformation.[41] This analysis enables

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by a large number of plague outbreaks during the Thirty Years War (1618-1648). The outbreaks observed during the Thirty Years War were associated with large scale movement of troops and refugees. These outbreaks were arguably endogenous to the Reformation as the conflict was to a considerable extent motivated by religious objectives.

[40]Voigtländer and Voth (2013) argue that by reducing labor supply the Black Death increased wages including for female workers, that this shock led to a reallocation of labor within agriculture and to fertility restrictions that supported increased investments in human capital and ultimately faster economic and demographic growth. Our evidence suggests the positive relationship between the Black Death and subsequent growth is observed even at the local level, in the within-Europe variation.

[41]There are a considerable number of cities with no or few plagues 1500 to 1522 which survive without laws into the Reformation, experience later outbreaks, and only then change institutions.

us to compare the effects of the instrument as it gets "turned on" at different times for different cities. These comparisons provide one external validity check on our baseline IV estimates. We find that the first stage relationship between recent plagues and institutional change strengthened and then declined somewhat over the course of the Reformation. We find that the relationship between induced institutional change and city growth remained relatively stable across the 1500s.

7   Upper Tail Human Capital

In this section we examine the migration and formation of upper tail human capital. Our focus on the migration and formation of highly skilled individuals is motivated by the literatures on upper tail human capital and the industrial revolution (Mokyr, 2009; Meisenzahl and Mokyr, 2012; Squicciarini and Voigtländer, 2015), the central role of migration in European city growth (Bairoch, 1991; De Vries, 2006), and human capital as a channel for the impact of the Protestant Reformation in the 19th century (Becker and Woessmann, 2009). Here we are able to use panel data methods to account for different trends and unobserved time invariant heterogeneity across locations.

Our key finding is that public goods institutions explain subsequent variations in the migration and formation of upper tail human capital. This evidence is consistent with Becker and Woessmann's (2009) finding that Protestantism was associated with literacy and development in the 1800s. But unlike previous research, our evidence highlights the importance of human capital for local growth starting in the 1500s, centuries before the Industrial Revolution and persisting through the Thirty Years War of the 1600s.[42] Also unlike previous research, we examine panel data at the local level and so are able to test for pre-trends. In addition, we are able to document that migration was from towns to treated cities – not from untreated cities to treated cities.

To study the migration and formation of upper tail human capital we collect biographical data on all individuals recorded in the *Deutsche Biographie* who either were born or died in the 239 cities in our sample between 1300 and 1820. The *Deutsche Biographie* provides the most comprehensive record of upper tail human capital individuals in German history, to the best of our knowledge.[43]

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[42]Meisenzahl and Mokyr (2012) and Squicciarini and Voigtländer (2015) document that the upper tail of the human capital distribution mattered for growth during the Industrial Revolution. These recent studies examining upper tail human capital are in part motivated by the finding that basic literacy appears to have had little effect on development during the British Industrial Revolution (Mitch, 1998).

[43]The *Deutsche Biographie* was originally produced by the historical commission of the Bayerischen Akademie der Wissenschaften (i.e. in Bavaria). There is no evidence that *Deutsche Biographie* selected on religion. Moreover, for selection to threaten our research design what would be required is that the *Deutsche Biographie* differentially included people born in Protestant cities with laws. On the nature of the documentary evidence, Parker (1997, p. 187) observes, "Seventeenth-century Germans were scrupulous record-keepers, and the Thirty Years' War did little to change their habits of meticulous documentation. Here and there crucial records were destroyed by negligence or acts of war, but enough documents have survived to provide vast amounts of data about local conditions."

In the *Deutsche Biographie* we observe where people were born and died. We use the records on where people were born to measure the local formation of human capital. We classify as a migrant any individual observed in the *Deutsche Biographie* who died in a given city in our data but was born in some other location, whether a city, a town, or a village.

7.1 Migration of Upper Tail Human Capital

Most pre-industrial city growth was driven by migration (De Vries, 2006; Bairoch, 1991). Early modern Germany was characterized by extensive labor mobility (Reith, 2008). Hence it is natural to ask whether cities with public goods institutions attracted more migrants in general or only after the establishment of these legal institutions. To study how the migration of upper tail human capital responded to these institutions we examine data from the *Deutsche Biographie*.

Figure 7 shows that there is a sharp and persistent increase in the total number of migrants observed in cities with laws in the 1520s, whereas the evolution in the number of migrants in cities without laws does not change during the Reformation. The jump at 1520 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 and/or migrated in earlier periods. What Figure 7 shows is that cities with and without laws were evolving similarly until the Reformation. Afterwards cities with laws began attracting more upper tail human capital migrants.[44] Overwhelmingly cities with laws attracted new talent from towns – not by drawing migrants from other cities without laws in a sort of "brain drain." Figure 8 documents this fact by graphing the net migrant flow from cities without laws to those with laws.

We more flexibly examine the migration of upper tail human capital by studying the differential into cities ever-treated by laws period-by-period. We assign migrants to the 50 year period and city in which they died (the "city-period"). We then estimate regressions of the form:

$$\ln(Migrants_{it} + 1) = \theta_i + \delta_t + \sum_{s=1320}^{1770} \beta_s(Law_{i,pre-1600} \times Time_s) + \epsilon_{it}, \tag{4}$$

Figure 9 documents that the differential migration of upper tail human capital into cities that reformed their legal institutions in the 1500s emerged only after the Reformation and persisted through 1800. This is shown by plotting the time fixed effects common to all cities (δ_t) and the time fixed effects for cities that were ever treated by Reformation law ($\delta + \beta_s$). The results from this baseline specification are supported by alternate specifications that directly examine the count

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[44]In Appendix D we show that "untreated" Protestant cities and "untreated" Catholic cities evolve similarly.

of upper tail human capital migrants. We report additional results in Appendix D.

7.2 Formation of Upper Tail Human Capital

The public goods institutions we study emphasized education. In this section, we test whether cities with Reformation laws by 1600 produced more upper tail human capital in two ways. We first estimate how the production of upper tail human capital evolved over time. We estimate the following regression specification:

$$\ln(People_{it} + 1) = \theta_i + \delta_t + \sum_{s=1320}^{1770} \beta_s(Law_{i,pre-1600} \times Time_s) + \epsilon_{it}, \tag{5}$$

Our baseline specification examines the natural logarithm of upper tail human capital, measured by entries in the *Deutsche Biographie* ($People_{it}$) in city-time-periods. We assign people to the city in which they were born and the time-period in which they turned forty years old. Figure 10 presents the parameter estimates graphically and shows that after the Reformation cities the time trend on upper tail human capital formation is stronger for cities with laws and a very large advantage in the 1700s. In Appendix D, we document statistically significant differences in time trends after the Reformation for cities with laws. Figure 10 also shows that cities with laws experienced a level increase in upper tail human capital in the omitted period just before the Reformation. As discussed above, this reflects the fact that educated human capital elites who participated in the formalization of public goods institutions are more likely to feature in the *Deutsche Biographie*.[45]

In Appendix D, we present parallel regression estimates, but with exposure to plague in the period 1500 to 1522 as the "treatment" whose time-varying effects we wish to explore. We find that cities with a plague in the early 1500s began differentially producing more upper tail human capital starting in the Reformation, conditional on city and time fixed effects. These reduced form estimates also connect our difference-in-difference designs to the IV analysis above.

We next collapse the data into a pre- and a post-period. The pre-period runs 1300 to 1519 and the post-period 1520 to 1820. We measure locally produced upper tail human capital with the number of people in the *Deutsche Biographie* born in city i and estimate the following regression:

$$People_{it} = \theta_i + \alpha \cdot Post_t + \beta \cdot (Post_t \ \times \ Law_{i,pre-1600}) + \epsilon_{it}, \tag{6}$$

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[45]Our fundamental results are not explained by the selective inclusion of marginal individuals into the *Deutsche Biographie*. Our results hold in the upper tail of "super-stars" for whom there is no ambiguity around inclusion. For example, our results hold for the 28 percent of individuals with extended biographical essays in the *Deutsche Biographie* and even if we restrict to individuals with biographical essays that are above average length.

Table 6 documents the relationship between legal institutions supporting public goods provision and the formation of upper tail human capital using using estimating equation (6). Column 1 studies shows that cities with Reformation laws produced significantly more upper tail human capital formation in the post-Reformation period. Column 1 measures upper tail human capital by the logarithm of the number of people in the *Deutsche Biographie* born in a given city plus one. Columns 2-7 examine where these effects are located in the upper tail human capital distribution by studying binary outcomes for any upper tail human capital and for upper tail human capital crossing thresholds in the post-1520 distibution. Column 4 indicates that cities with the institutions of the Reformation were 25 percent more likely to be above the 50th percentile in the post-1520 upper tail human capital distribution, controlling for time invariant city characteristics. This advantage is smaller in magnitude but holds in the far upper tail. Cities with public goods institutions were 22 percent more likely to be above the 75th percentile, 11 percent more likely to be above the 90th percentile, and 7 percent more likely to be above the 95th percentile.

8   Conclusion

We provide new evidence on the origins and long-run effects of state capacity and public goods provision. We study local variation in public goods institutions that originated in Germany during the Protestant Reformation. We document that the introduction of ideological competition combined with citizen action to drive fundamental institutional changes – despite restrictions on formal political representation. During the critical juncture of the early 1500s, localized plague outbreaks increased the probability of cities adopting institutions designed to support public goods. The new institutions bundled religious, educational, anti-corruption, and social welfare interventions and were formalized in law. Cities that adopted these institutions enjoyed a persistent population growth advantage of between 0.1 percent and 0.9 percent per year over two centuries.

We highlight the importance of innovations in state capacity for human capital outcomes. Using new microdata, we document that cities that adopted public goods institutions subsequently produced and attracted more individuals with upper tail human capital over the long period running from the early 1500s through 1800. No previous research has documented the impact of these institutions on upper tail human capital quantitatively or indeed the dynamic relationship between human capital and local growth before the Industrial Revolution. More broadly, our research suggests that the Reformation provides a canonical historical model of the emergence and implications of state capacity driven by political movements that challenge elites.

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Figure 1: New School Construction in German Cities
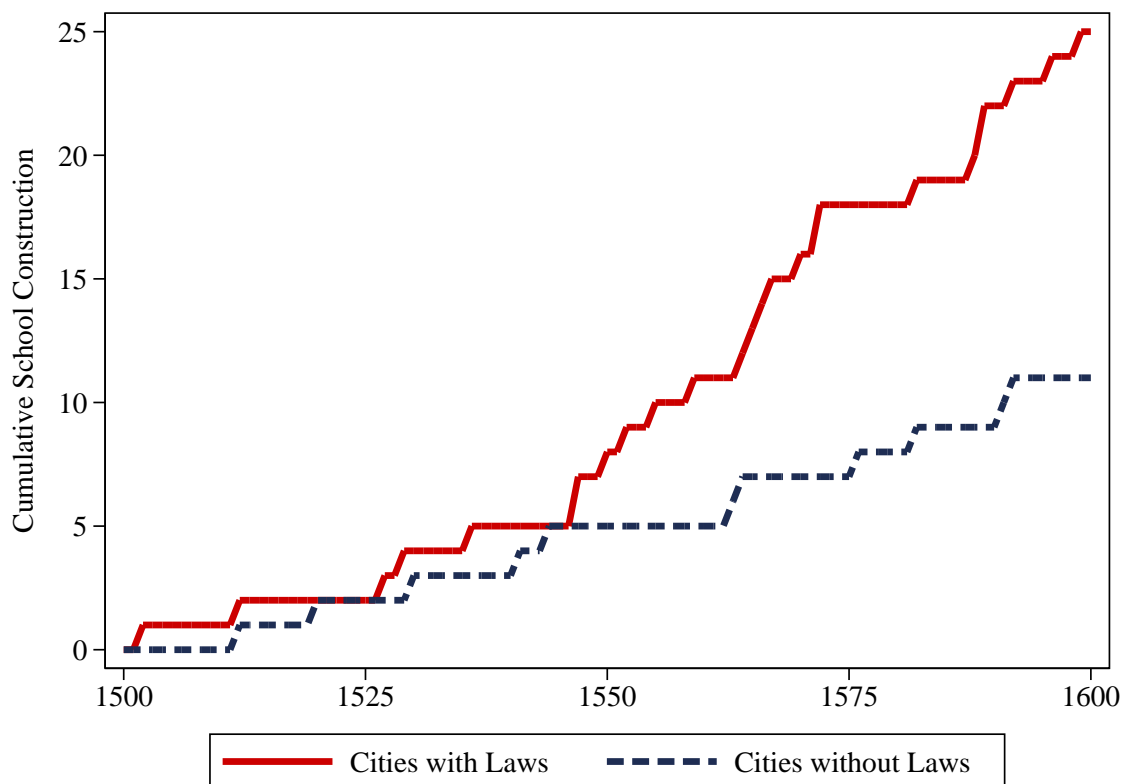
This graph shows the cumulative number of new schools constructed in cities that did and did not pass laws formalizing the public goods institutions of the Reformation. Data on school construction are from the *Deutsches Städtebuch*. Data on laws and from the *Deutsches Städtebuch* are described below and in Appendix A. The graph presents data for the 239 German cities we study in this paper as described in the text.

Figure 2: Cities With and Without Reformation Laws
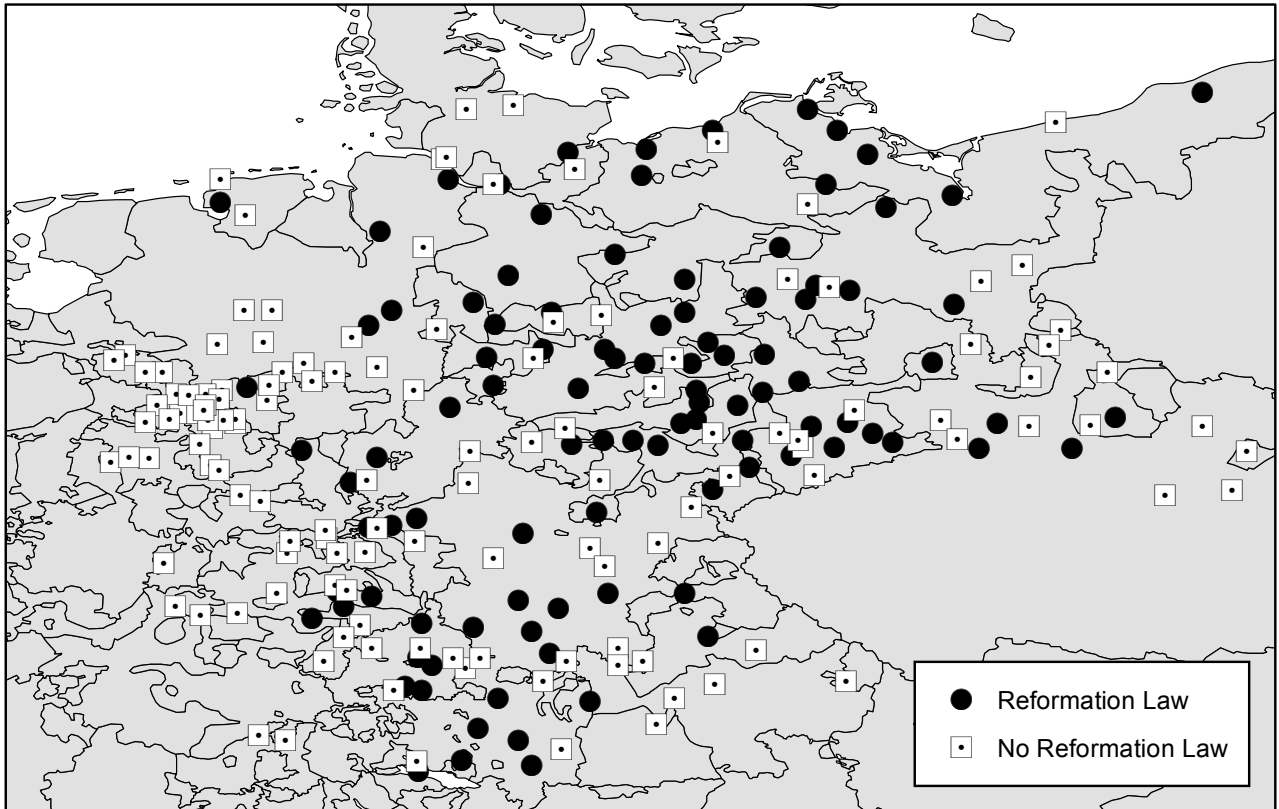
This map shows cities with Reformation Laws (black circles) and without these laws (white squares). Sources are described in the text.

Figure 3: The Share of Cities with Protestant Laws
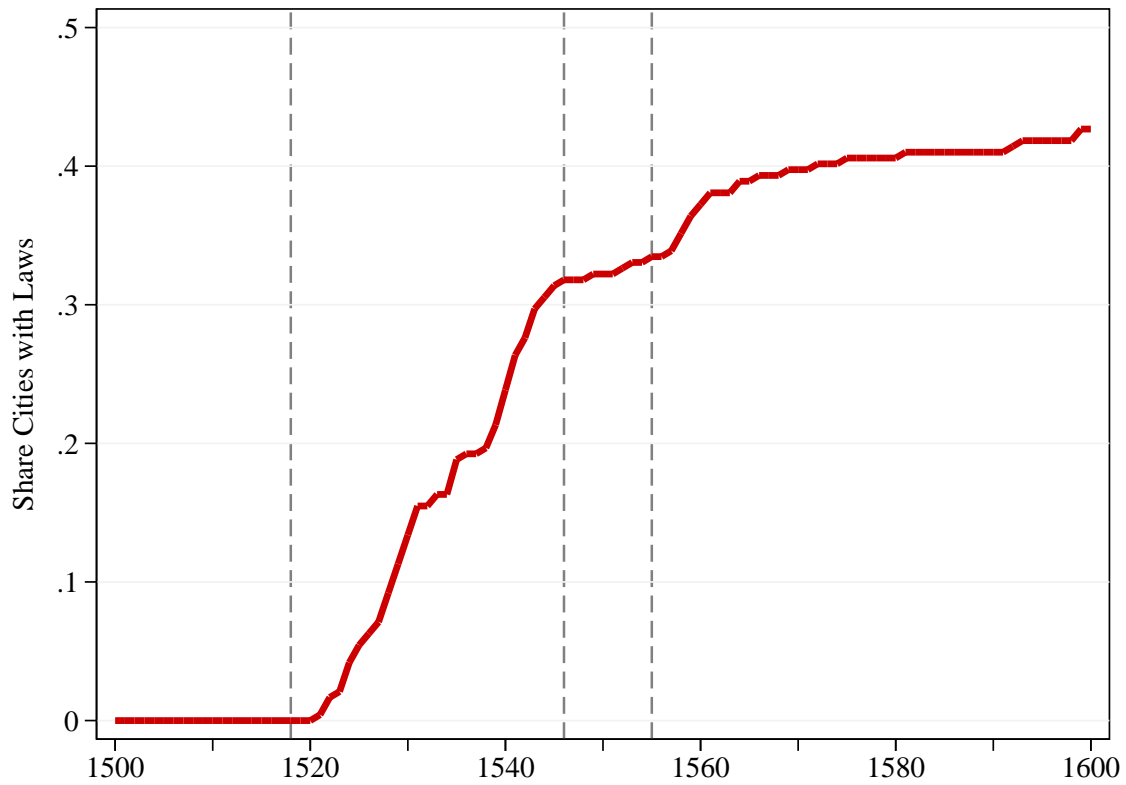
This graph shows the share of cities having passed a Reformation Law. The vertical line at 1518 marks the initial mass circulation of Luther's ideas. The line at 1546 marks the date of the Schmalkaldic War between Catholic and Protestant princes. The line at 1555 marks the Peace of Augsburg which established a new religious equilibrium in law, with provisions for lords' religious prerogatives in their territories and for cities with mixed control over churches and magistracies. Sources are described in the text.

Figure 4: City-Level Plague Outbreaks
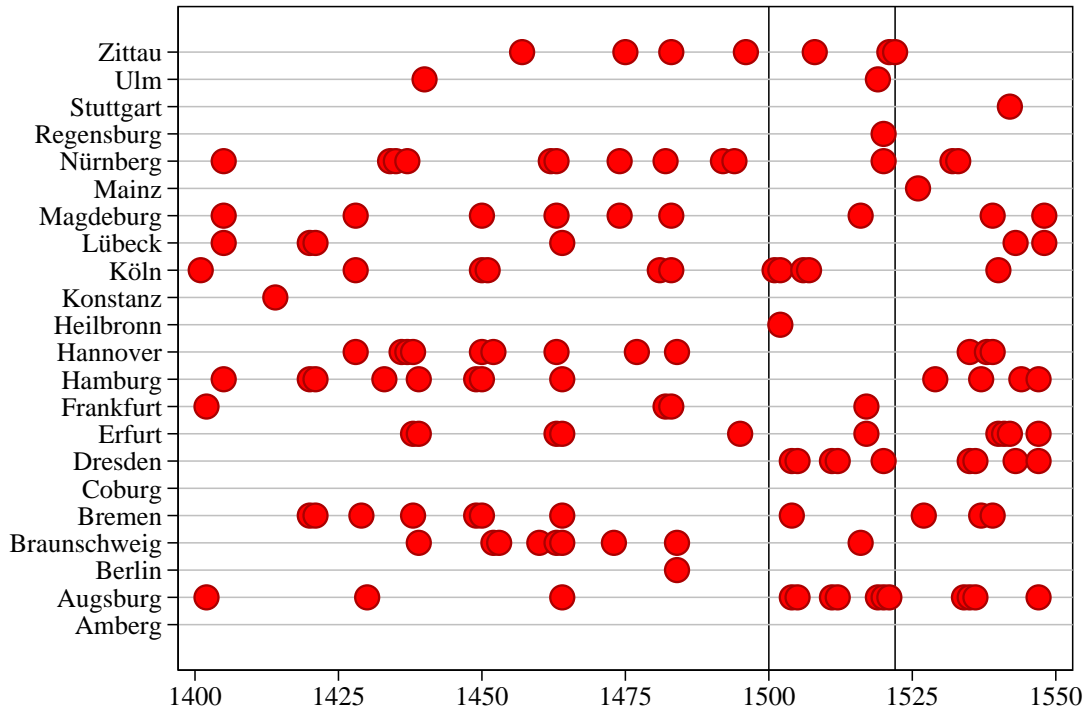
This graph shows the timing of major plague outbreaks in selected cities between 1400 and 1550. Source: Biraben (1975). The vertical lines at 1500 and 1522 delimit the period used in our baseline instrumental variable analysis to construct the early 1500s plague exposure instrument.

Figure 5: Aggregate Plague Outbreaks
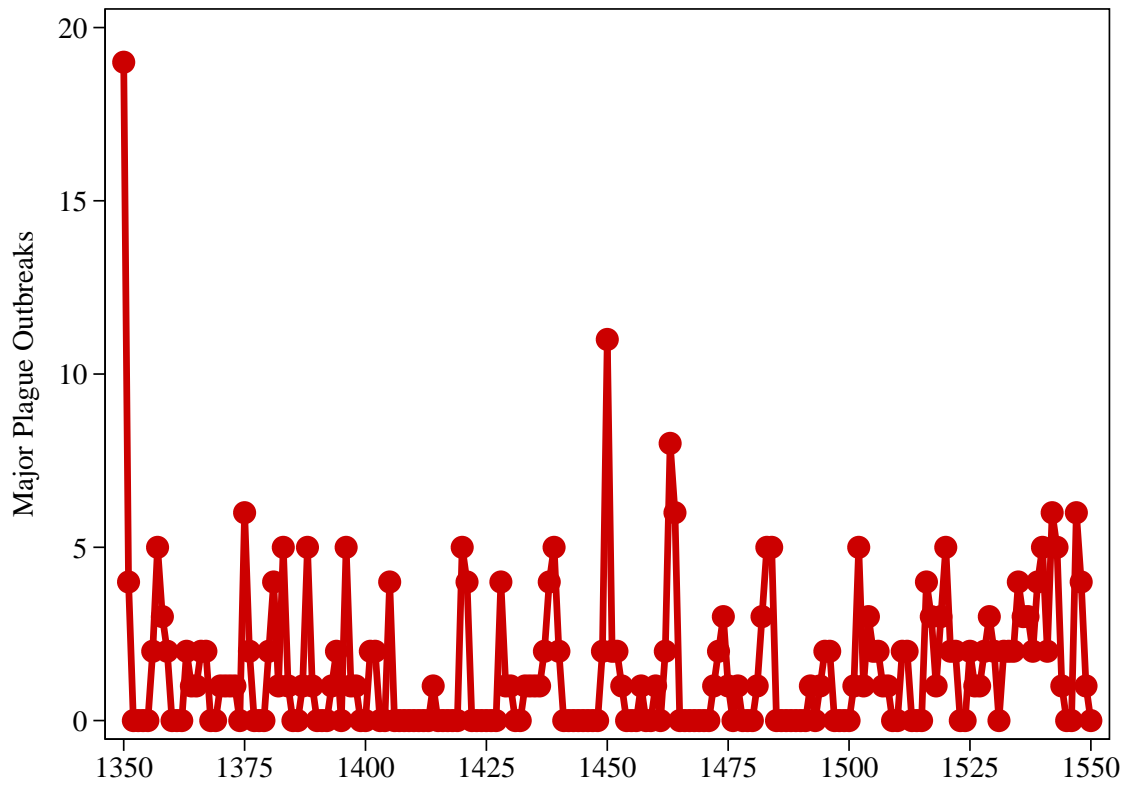
This graph shows the total number of major plagues between 1350 and 1550 in cities in our sample. The underlying unit of observation is the city-year. We code whether there was a major outbreak in a given city-year. Source: Biraben (1975).

Figure 6: Instrumental Variable Estimates Varying the Plague Exposure Period
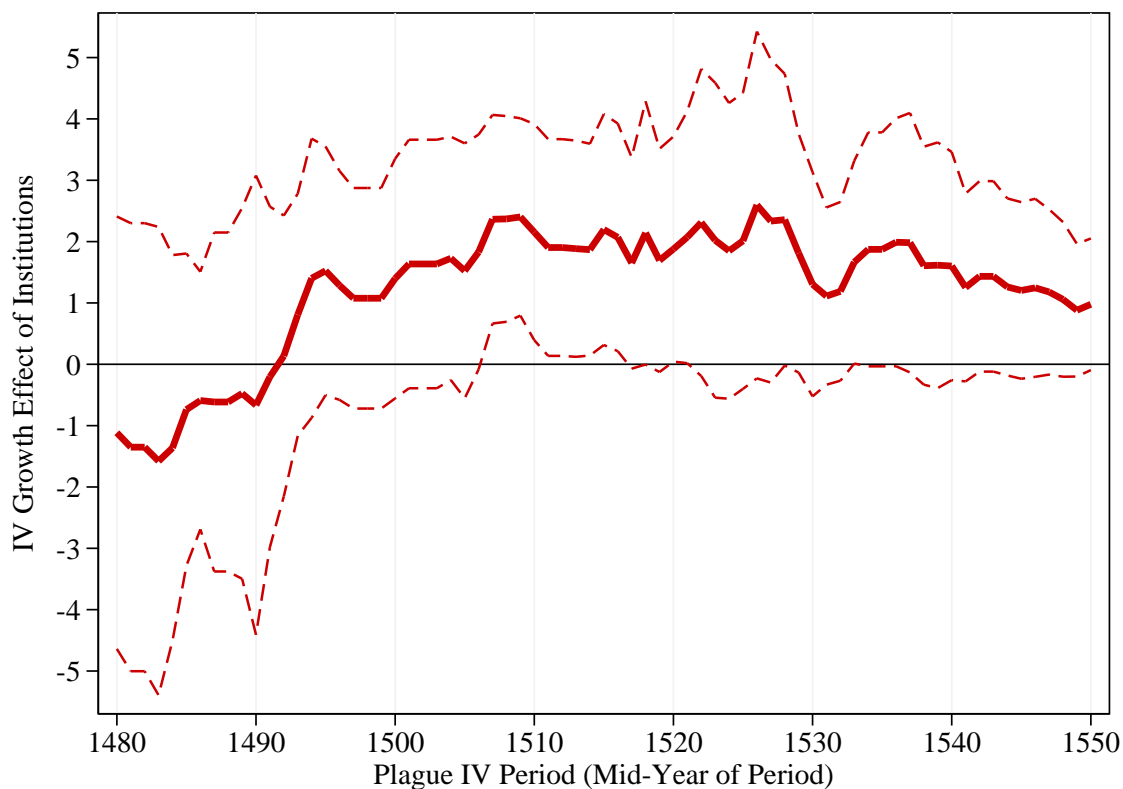
This graph presents estimates from instrumental variable regressions that vary the time-period used to measure the plague outbreak IV. We estimate our baseline IV regression specification in all regressions, but use as the instrument plagues from different twenty-three year time-periods. The results reported in the main text use the time-period 1500 to 1522 to measure the plague outbreak IV (see Table 4). On this graph that estimate corresponds on the x-axis to the "Plague IV Period (Mid-Year of Period)" at 1511. 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 4. 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.

Figure 7: The Migration of Upper Tail Human Capital
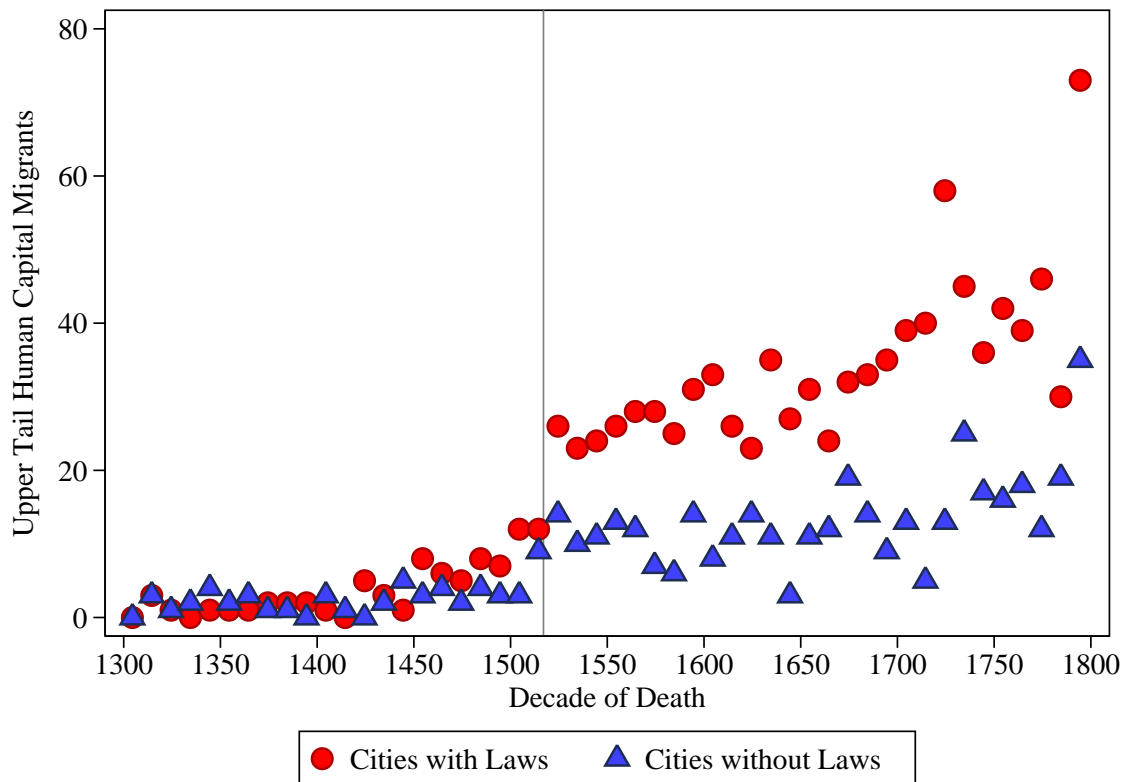
This graph plots the number of migrants observed in the *Deutsche Biographie* at the decade level in cities with and without laws. Migrants are identified as people living and dying in town i but born in some other location j. The vertical line is at 1518, the year Luther's theses began circulating widely.

Figure 8: Net Migration from Cities Without Laws to Cities With Laws
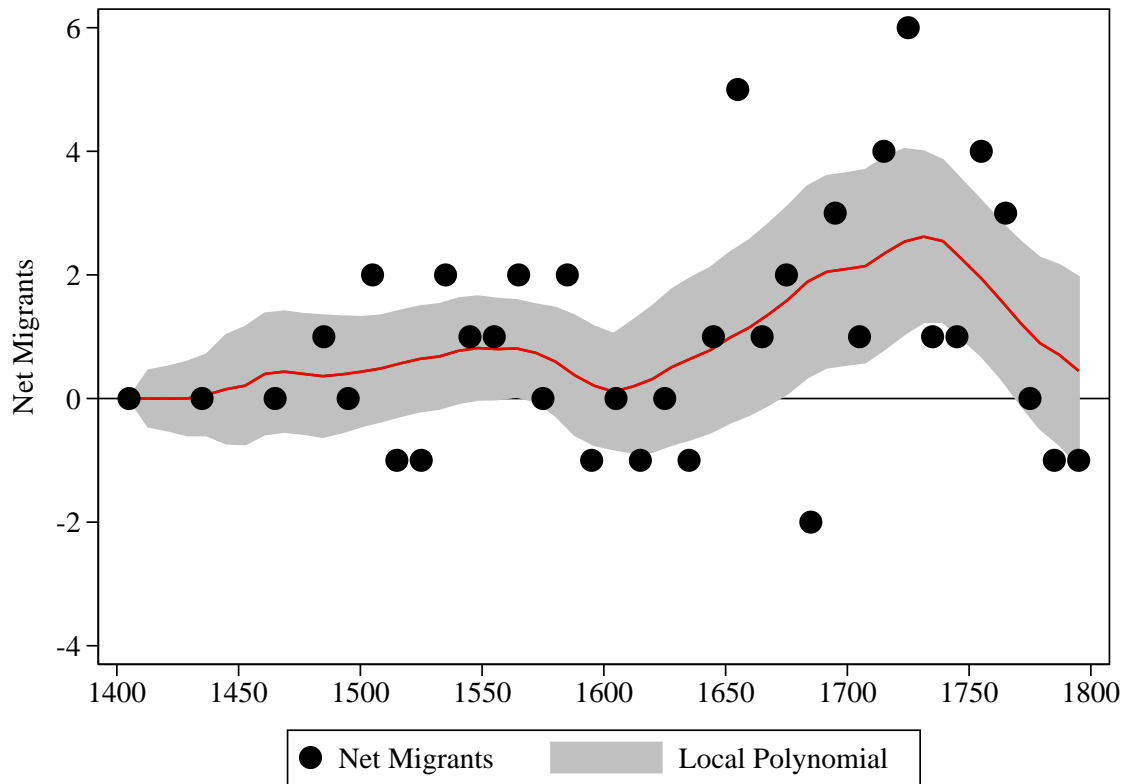
This graph plots net migration from cities without Reformation laws to cities with Reformation laws. The graph plots the raw data on net migration at the decade level as observed in the *Deutsche Biographie* in the 239 cities in our data. The graph also plots the local polynomial regression estimate and associated 95% confidence interval for the relationship between net migration and time. Net migration is defined as the net flow of migrants into cities "ever treated by Reformation Law" from cities where these laws were not passed.

Figure 9: Regression Analysis of the Migration of Upper Tail Human Capital
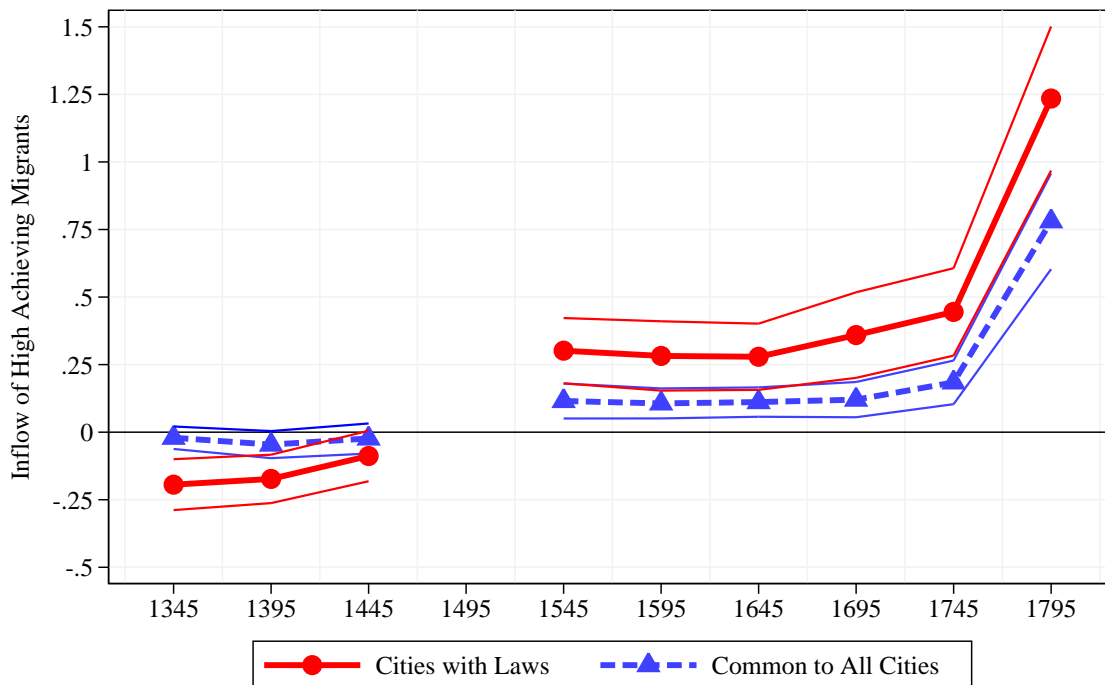
This graph plots parameter estimates from regression analysis examining the differential migration of upper tail human capital into cities that adopted Reformation law. The outcome variable is the logarithm of the number of migrants plus one in city i at time t. Migrants are upper tail human capital individuals identified in the *Deutsche Biographie* as having moved to the city in which they died. We assign migrants to the time-period and city in which they died. Time is measured in 50-year periods. We graph the parameter estimates on time-period fixed effects and on the interactions between time-period indicators and an indicator for cities ever adopting Reformation law. The regression includes fixed effects for cities and time periods and is estimated over data from 1320 through 1820. The omitted time category is the period 1470 through 1519 (centered on 1495). The post-Reformation periods begin with the 1520-1570 period (centered on 1545).

Figure 10: Regression Analysis of the Formation of Upper Tail Human Capital
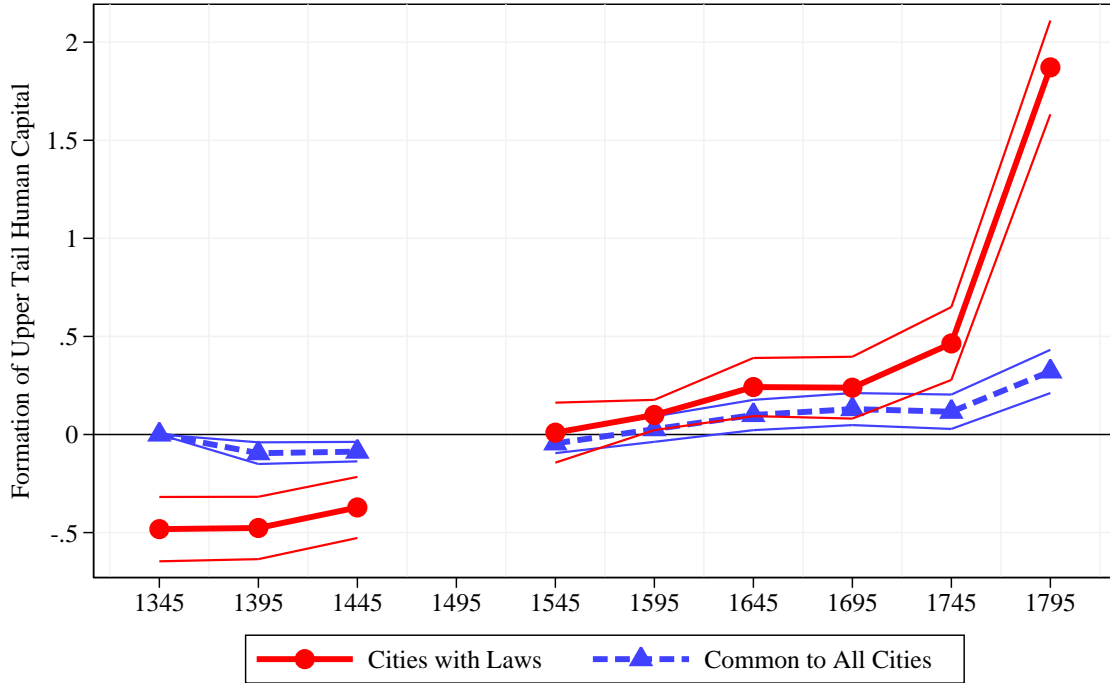
This graph plots parameter estimates from regression analysis examining the differential local formation of upper tail human capital in cities that adopted Reformation laws. The outcome variable is the logarithm of the number of upper tail human capital individuals plus one who were born in city i in period t. Dates and places of birth are identified in the *Deutsche Biographie*. We assign people to the city and time-period in which they were born. We graph the parameter estimates on time-period fixed effects and the interactions between time-period indicators and an indicator for cities ever adopting Reformation law. The regression includes fixed effects for cities and time periods and is estimated over data from 1320 through 1820. The omitted time category is the period 1470 through 1519 (centered on 1495). The post-Reformation periods begin with the 1520-1570 period (centered on 1545).

Table 1: Summary Statistics

	N	Mean	Sd	25%	50%	75%
Reformation Law Indicator	239	0.43	0.50	0	0	1
Log (Population$_{1800}$/1000)	239	1.92	0.73	1.61	1.79	2.20
Log (Population$_{1500}$/1000)	110	1.60	0.86	1.10	1.61	2.20
Town Incorporation pre-1517 Indicator	239	0.41	0.49	0	0	1
Market Rights pre-1517 Indicator	239	0.40	0.49	0	0	1
Number of Books printed pre-1517	239	95.81	567.91	0	0	0
Reichsstadt Indicator	239	0.18	0.38	0	0	0
University Pre-1517 Indicator	239	0.05	0.22	0	0	0
Number of University Students 1398-1508	239	31.01	47.13	6	18	39
Number of Plagues 1400-1499	239	0.44	1.55	0	0	0
Number of Plagues 1500-1522	239	0.17	0.71	0	0	0

This table presents the summary statistics for our main regression variables. Reformation law is an indicator variable whether a city had any ordinance by 1600. Number of university students is the number of students coming from each city.

Table 2: Summary Statistics by Reformation Law Status

	Cities with Law			Cities without Law			Difference
	N	Mean	Sd	N	Mean	Sd	
Log (Population$_{1800}$/1000)	103	2.17	0.76	136	1.73	0.65	0.45***
Log (Population$_{1500}$/1000)	68	1.75	0.84	42	1.35	0.84	0.40***
Town Incorporation pre-1517 Indicator	103	0.42	0.50	136	0.41	0.49	0.01
Market Rights pre-1517 Indicator	103	0.39	0.49	136	0.41	0.49	0.02
Number of Books printed pre-1517	103	157.69	721.57	136	49.00	412.40	108.69
Reichsstadt Indicator	103	0.29	0.46	136	0.09	0.28	0.20***
University Pre-1517 Indicator	103	0.07	0.25	136	0.04	0.21	0.03
Number of University Students 1398-1508	103	35.72	39.44	136	27.44	52.07	8.27
Number of Plagues 1400-1499	103	0.86	2.20	136	0.12	0.61	0.75***
Number of Plagues 1500-1522	103	0.32	0.97	136	0.05	0.37	0.27***

This table presents the summary statistics for our main regression variables by Reformation law status. Reformation law is an indicator variable whether a city had any ordinance by 1600. Number of university students is the number of students coming from each city.

Table 3: City Size and Reformation Laws

	Main Specification Ln Population in 1800						Placebo Ln Population in 1500
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Reformation Law	0.27**	0.27**	0.26**		0.32***	0.34	0.18
Clustered SE	(0.11)	(0.12)	(0.10)		(0.09)	(0.22)	(0.18)
Wild Bootstrap 95% CI	[0.08, 0.48]	[0.08, 0.46]	[0.07, 0.39]		[0.17, 0.48]	[0.06, 0.63]	[-0.04, 0.40]
Distance from Wittenberg				-0.05	-0.02		
Clustered SE				(0.09)	(0.08)		
Wild Bootstrap 95% CI				[-0.21, 0.09]	[-0.14, 0.10]		
Protestant				-0.00	-0.05		
Clustered SE				(0.16)	(0.16)		
Wild Bootstrap 95% CI				[-0.28, 0.27]	[-0.22, 0.39]		
Population Bin FE	Yes	Yes	Yes	Yes	Yes	No	Yes
Controls	No	Yes	Yes	No	No	Yes	Yes
Geo Controls	No	No	Yes	No	No	No	Yes
Cantoni Controls	No	No	No	Yes	Yes	No	No
Log Population in 1500	No	No	No	No	No	Yes	No
Territory FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	239	239	239	239	239	110	110
R²	0.46	0.52	0.53	0.49	0.51	0.64	0.79

This table presents the regression results of estimating the effect of reformation laws on log population in 1800. "Reformation Law" is an indicator variable whether a city had any ordinance by 1600. Distance from Wittenberg is measured in kilometers. "Protestant" is an indicator for cities where Protestantism became the dominant religion and is from Cantoni (2012). Controls are: Market rights by 1517, town incorporated by 1517, four indicators for number of books that were printed in a city by 1517 (0, 1-100, 101-1000, more than 1000), university by 1517 indicator, Reichsstadt indicator, number of university students in each 10-year period starting 1398 through 1508, and the average number of plagues from 1400 to 1499. Geographical controls are longitude, latitude, and the interaction of longitude and latitude. Cantoni controls are year city turned Protestant, river indicator, Hanse indicator, Reichsstadt indicator, year city founded, monastery indicator, university indicator, and printing press indicator. Population bins are 5 indicator variables (population in 1500 data missing, 1,000-5,000, 6,000-10,000, 11,000-20,000, and more than 20,000). Population bins in column 8 are for population in 1400. Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, *, respectively. Standard errors are clustered at the 1500 territory level. Territories are from Euratlas. Wild bootstrap confidence intervals estimated using the methodology of Cameron, Gelbach, and Miller (2008).

Table 4: Instrumental Variable Analysis of City Size and Reformation Laws

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
				Panel A: First Stage Outcome – Reformation Law					
Plagues 1500-1522	0.13***	0.14***	0.13***	0.12***	0.14***	0.13***	0.14***	0.15**	0.15**
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.06)	(0.06)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population Bin Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Log Population in 1500	No	No	No	No	No	No	Yes	No	No
Plagues 1400-1499 Level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plagues 1400-1499 Polynomial	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Plagues 1400-1499 Non-Linear	No	No	Yes	No	No	Yes	No	No	Yes
Territory Fixed Effects	No	No	No	Yes	Yes	Yes	No	No	No
R²	0.28	0.28	0.28	0.49	0.49	0.49	0.32	0.32	0.33
Observations	239	239	239	239	239	239	110	110	110
				Panel B: Instrumental Variable Outcome – Ln Population in 1800					
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Reformation Law	1.64*	1.90**	2.41***	1.99*	2.04**	2.55***	2.46***	2.13***	2.37***
	(0.88)	(0.90)	(0.72)	(1.07)	(0.91)	(0.74)	(0.72)	(0.60)	(0.61)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population Bin Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Log Population in 1500	No	No	No	No	No	No	Yes	No	No
Plagues 1400-1499 Level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plagues 1400-1499 Polynomial	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Plagues 1400-1499 Non-Linear	No	No	Yes	No	No	Yes	No	No	Yes
Territory Fixed Effects	No	No	No	Yes	Yes	Yes	No	No	No
Observations	239	239	239	239	239	239	110	110	110
F Statistic on First Stage IV	39.39	21.47	21.97	26.54	19.82	23.01	11.14	7.27	7.17

This table presents the IV estimates of the effect of Reformation laws on log population in 1800. Panel A presents first stage regression estimates where the dependent variable is an indicator for the passage of a Reformation ordinance by 1600. "Plagues 1500-1522" is the number of plagues 1500 to 1522. Controls are: Market rights by 1517, town incorporated by 1517, four indicators for number of books that were printed in a city by 1517 (0, 1-100, 101-1000, more than 1000), university by 1517 indicator, Reichsstadt indicator, number of university students from within 10 km of the city receiving university degrees in each 10-year period starting 1398 through 1508. Population bins fixed effects are indicator variables categories: population in 1500 data missing; 1,000-5,000; 6,000-10,000; 11,000-20,000; and more than 20,000. "Log Population in 1500" is the natural logarithm of population in 1500. Territory fixed effects control for city territories. Territories are from Euratlas. "Plagues 1400-1499 Level" is the average number of plagues from 1400 to 1499. "Plagues 1400-1499 Polynomial" indicates inclusion of quadratic and cubic polynomials of the level. "Plagues 1400-1499 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. Columns 5 and 6 examine the subset of cities with population observed in 1500. Panel B presents instrumental variable estimates of the relationship between the natural logarithm of city population in 1800 and the presence of Reformation Law institutions, instrumenting for these institutions with plagues 1500-1522. Standard errors are clustered at the 1500 territory level. Territories are from Euratlas. Statistical significance at the 1%, 5%, and 10% levels denoted ***, **, and *, respectively.

Table 5: City Size and Historic Plague Outbreaks

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

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

Table 6: Formation of Upper Tail Human Capital

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

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

Appendices

A Data

A.1 Legal Institutions of Reformation

Our principle source on the municipal laws (*Kirchenordnungen*) of the Reformation is the multi-volume collection of Protestant church ordinances *Die Evangelischen Kirchenordnungen Des XVI. Jahrhunderts*, originally edited by Emil Sehling. The complete list of volumes we code is as follows.

- Emil Sehling editor, Volume I Sachsen und Thüringen nebst angrenzenden Gebieten (1902) (Leipzig: O.R. Reisland).

- Emil Sehling editor, Volume II Sachsen und Thüringen nebst angrenzenden Gebieten (1904) (Leipzig: O.R. Reisland).

- Emil Sehling editor, Volume III Brandenburg, Ober- und Niederlausitz, Schlesien (1909).

- Emil Sehling editor, Volume IV Preußen, Polen, Pommern (1911).

- Emil Sehling editor, Volume V Baltische Länder, Mecklenburg, Lübeck, Lauenburg, Hamburg (1913).

- Institut für evangelisches Kirchenrecht der EKD, Volume VI/1 Niedersachsen (1955).

- Institut für evangelisches Kirchenrecht der EKD, Volume VI/2 Niedersachsen (1957).

- Institut für evangelisches Kirchenrecht der EKD, Volume VII/1, Niedersachsen (1963).

- Institut für evangelisches Kirchenrecht der EKD, Volume VII/2, Niedersachsen (1980).

- Institut für evangelisches Kirchenrecht der EKD, Volume VIII Hessen I: Landgrafschaft bis 1582 (1965).

- Institut für evangelisches Kirchenrecht der EKD, Volume XI, Franken, (1961).

- Institut für evangelisches Kirchenrecht der EKD, Volume XII, Schwaben (1963).

- Institut für evangelisches Kirchenrecht der EKD, Volume XIII Altbayern (1966).

- Institut für evangelisches Kirchenrecht der EKD, Volume XIV Kurpfalz (1969).

- Institut für evangelisches Kirchenrecht der EKD, Volume XV Baden-Württemberg I: Hohenlohe (1977).

- Heidelberger Akademie der Wissenschaften, Volume XVI Baden-Württemberg II: Württemberg, Baden u. a. (2004).

- Heidelberger Akademie der Wissenschaften, Volume XVII/1; XVII/2 Baden-Württemberg III/IV: Reichsstädte (2007/09).

- Heidelberger Akademie der Wissenschaften, Volume XVIII Rheinland-Pfalz I: Zweibrücken, Veldenz, Sponheim u. a. (2006).

- Heidelberger Akademie der Wissenschaften, Volume XIX Rheinland-Pfalz II: Wild- und Rheingrafschaft, Leiningen, Wied u. a. (2008).

- Heidelberger Akademie der Wissenschaften, Volume XX/1 Elsass I: Straßburg (2011).

- Heidelberger Akademie der Wissenschaften, Volume IX Hessen II: Landgrafschaft ab 1582, Waldeck, Solms, Frankfurt u. a. (2011).

- Heidelberger Akademie der Wissenschaften,Volume X Hessen III: Nassau, Hanau-Münzenberg, Ysenburg (2012).

- Heidelberger Akademie der Wissenschaften, Volume XX/2 Elsass II: Hanau-Lichtenberg, Colmar, Mülhausen, Weißenburg u.a. (2013).

For cities not covered in the *Die Evangelischen Kirchenordnungen Des XVI. Jahrhunderts* collection, we identify ordinances from additional historical sources. We review Richter (1846a,b), *Die evangelischen Kirchenordnungen des sechszehnten Jahrhunderts* for all such cities. For select additional cities we consult additional sources. For Brno (Brünn): Thomas A. Brady, Heiko Augustinus Oberman, James D. Tracy, *Handbook of European History 1400 - 1600: Late Middle Ages, Renaissance and Reformation* (Leiden: Brill, 1994). Haguenau: Joseph Guerber, *Haguenau et la Réforme* (Lyon: Le Roux L.F., 1861). Kostrzyn (Kostschin): Jürgen Sanowsky, "Vorgeschichte und Anfänge der Reformation in der Ballei Brandenburg des Johanniterordens", in Johannes Mol et al. eds., *Military Orders and the Reformation: Choices, State Building, and the Weight of Tradition* (Amersfoort: Uitgeverij Verloren, 2006). Mulhouse: Peter Blickle, *Communal Reformation: The Quest for Salvation in Sixteenth-Century Germany* (Leiden: Brill, 1992). Schaffhausen: Cameron (1991).

A.2   Institutional Treatment and Religious Heterogeneity

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

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

Cities that adopted the institutions of the Reformation and in which these institutions survived to 1600 are classified as "treated" in our baseline analysis. Where we observe institutions established by Reformation

laws in effect through 1600, these institutions were typically persistent. Our basic findings on the relationship between institutional change and city growth are robust to different classifications of cities with persistent institutions but complicated religious trajectories.

The city of Amberg in Bavaria provides an instructive example showing how Reformation institutions typically persisted in treated cities once long-run benefits of Reformation institutions became clear, even if a city or its territory came under Catholic rule in the 17th century. Amberg passed a Protestant ordinance in the 1540s and eyewitness accounts from 1564 record boys' and girls' schools, "in which the German Catechism, reading, writing, and arithmetic are diligently taught" (Johnson, 2009, p. 32). Catholic authorities preserved the institutional legacy that they inherited when Amberg and the surrounding territory of Upper Palatine (Oberpfalz) was absorbed by Catholic Bavaria in the early 1600s. During the 1620s, the Jesuits took over formerly Calvinist and Lutheran higher schools in Amberg. More broadly, "the educational infrastructure of the territory impressed Counter-Reformation Catholics when they inherited it after 1621; summing up his reflections. . . after his visitation in 1656, the (Catholic) Regensburg vicar general noted approvingly that 'in nearly every village, schools are to be found, of which in [Catholic] Bavaria there [otherwise] seems to be a great shortage and decline' " (Johnson, 2009, p. 35).

The city of Augsburg provides another example of institutional persistence in a city our classification designates as treated. In Augsburg, the Reformation was formally adopted and municipal social services were reorganized between 1534 and 1537, when Protestant guildsmen gained control of the city council (Broadhead, 1979). In 1548, the Holy Roman Emperor Charles V reestablished a form of Catholic rule in Augsburg that allowed Protestant institutions to persist: rule by an elite of Catholic patricians, with reserved control of key government positions, over a Protestant majority city with active public service institutions shaped by the Reformation. Under this arrangement Augsburg experienced peaceful co-existence without institutional reversals into the 1600s. Stein (2009, p. 73) observes: "At no point did the Catholic-dominated patrician council attempt to re-catholicise the city; instead it governed with discretion in order to minimise [sic] tensions with the Protestant community. . . The admission practices of all the city-run health care institutions reflected this strategy of confessional tolerance."[46]

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

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

a Reformation ordinance.[47] Bautzen is an example of a city that became predominantly Protestant and is classified as such in Cantoni (2012) but did not adopt Protestant law.

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

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

A.3 Territorial and Jurisdictional Status of Cities

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

We locate cities in historic territories as of 1500 using GIS maps from Euratlas (2008), *Periodical Historical Atlas of Europe.* These data are: Copyright 2008, Christos Nüssli, Euratlas www.euratlas.com, utilization license of 2009. The geography of these territories is shown in Figure 2 in the main text. In the econometric analysis, we cluster standard errors at the Euratlas territory ("holder") level. Euratlas territories capture geographic proximity, but are not a direct measure of territorial institutions. Complicated and heterogeneous institutional arrangements in some cases applied even to cities within a given territory and even within cities. See Whaley (2012) and Roeck (1999) for a review.

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[47]On the absence of a law, Speer (2014, p. 51) observes: "wird allein daran deutlich, dass er sich in Sachen Ehegerichtsbarkeit noch dem (katholischen) Offizial in Bautzen unterstellte und keine Kirchenordnung erließ. Erst seit 1565 finden sich in den Stadtstatuten vereinzelt Ratsbeschlüsse zu Kirchenangelegenheiten, eine Kirchenordnung im eigentlichen Sinne konnte vor 1617 bisher nicht nachgewiesen werden."

A.4 *Deutsche Biographie* Data

We gather data on notable cultural figures from the *Deutsche Biographie*. The current *Deutsche Biographie* builds on and extends the original *Allgemeine Deutsche Biographie*, a 56 volume reference collection published 1875-1912. The *Deutsche Biographie* is regarded as the authoritative biographical dictionary for all regions of Europe in which German is spoken and German culture is prevalent. The *Deutsche Biographie* records individuals who made a "significant impact on developments in politics, economics, social life, scholarship, technology or the arts."[48] For our study of the formation of human capital, we restict our study to people born in our baseline set of 239 cities between 1200CE and 1800CE with birthdate known. For our study of migration, we restrict our study to people migrating to our 239 cities and dying between 1200CE and 1800CE. Our evidence on migrants includes people whose birthplaces are rural towns, cities outside Germany, and in some cases unknown. As described in the main text, for some exercises we restrict to the subset of migrants where both place of birth and place of death is known. Our evidence on migrants also includes people whose dates of birth are not known.

Table 7: Summary Statistics on Upper Tail Human Capital

	Cities with Laws			Cities without Laws			Difference	Median
	Mean	SD	Median	Mean	SD	Median	in Means	Shift
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Migrants pre-1520	0.97	2.28	0.00	0.48	1.80	0.00	0.49*	0.00
Migrants post-1519	21.60	53.13	6.00	7.35	22.48	1.00	14.25***	3.00**
Native pre-1520	2.22	8.81	1.00	0.43	1.10	0.00	1.80**	0.00
Native post-1519	45.31	98.82	16.00	15.64	33.44	6.00	29.67***	8.00**

This table presents summary statistics on upper tail human capital as observed from entries in the *Deutsche Biographie*. "Migrants" are defined as people observed born in a given location i and dying in a different city j. We include migrants who relocate to the set of 239 cities we study from other cities, towns, and villages and measure migrants at the year of death. "Native" are all those born in a given city. Native born people are measured at age forty (or age of death if before forty years old). Column [7] reports difference in means between cities with laws and cities without laws. Statistical significance in the difference of means is estimated from t-tests and denoted ***, **, and * for the 99%, 95%, and 90% confidence levels. Column [8] reports the Hodges-Lehman non-parametric estimate of median shift between the set of cities with laws and the set of cities without laws. Hodges-Lehman shifts with 95% confidence intervals that exclude zero are denoted **.

A.5 *Deutsches Städtebuch* Data

The *Deutsches Städtebuch* provides information on notable historic construction events in German cities (Cantoni and Yuchtman, 2014). We present information on school construction from the *Deutsches Städtebuch*. Volume I: Erich Keyser (editor), *Deutsches Städtebuch, Handbuch städtischer Geschichte. Bd. I Nordostdeutschland* (Stuttgart: Kohlhammer, 1939). Volume II: Erich Keyser (editor), *Deutsches*

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

Städtebuch, Handbuch städtischer Geschichte. Bd. II Mitteldeutschland (Stuttgart: Kohlhammer, 1941). Volume III: Erich Keyser (editor), *Deutsches Städtebuch, Bd. III Nordwest-Deutschland. 2, Westfalen. Westfälisches Städtebuch* (Stuttgart: Kohlhammer, 1954). Erich Keyser (editor), *Deutsches Städtebuch, Bd. III Nordwest-Deutschland. 3, Landschaftsverband Rheinland. Rheinisches Städtebuch* (Stuttgart: Kohlhammer, 1956). Volume IV: Erich Keyser (editor), *Deutsches Städtebuch, Bd. IV Südwest-Deutschland. 1, Land Hessen. Hessisches Städtebuch* (Stuttgart: W. Kohlhammer, 1957). Erich Keyser (editor), *Deutsches Städtebuch: Handbuch städtischer Geschichte, Bd. IV Sudwest-Deutschland. 2, Land Baden-Wurttemberg. Teilbd. Württemberg. Württembergisches Städtebuch* (Stuttgart: Kohlhammer, 1962). Erich Keyser (editor), *Städtebuch Rheinland-Pfalz, Saarland, Mainz, Stadtkreis* (Stuttgart: W. Kohlhammer, 1964). Volume V: Erich Keyser (editor), *Deutsches Städtebuch, Bd. V Bayern. 1, Bayerisches Stadtebuch* (Stuttgart: Kohlhammer, 1971). Erich Keyser (editor), *Deutsches Städtebuch, Bd. V Bayern. 2, Bayerisches Stadtebuch* (Stuttgart: Kohlhammer, 1974).

B   Institutional Change and City Growth

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

B.1   Public Goods Institutions and City Status in 1800

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

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

To test the hypothesis that the adoption of public goods institutions predicts in the 1500s predicts observable population data in 1800, we estimate linear probability model regressions. The binary dependent variable captures whether a historic town appears as a city with population recorded in 1800.[49] We estimate basic linear probability models with the form:

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

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

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[49]We aggregate a small number of *Deutsches Städtebuch* towns that were initially separate jurisdictions but ultimately became part of larger urban agglomerations in the Bairoch, Batou, and Chèvre (1988) data. For example, we treat Düsseldorf, Düsseldorf-Gerresheim, and Düsseldorf-Kaiserswerth as a single location. The results are not sensitive to this treatment of near neighbors.

books published as of 1517.

Table 8 shows that towns treated by reformation institutions were approximately 10 percent to 13 percent more likely to be cities with population observed in 1800 than observably similar towns that were not treated by laws. This finding should be interpreted as a correlation and not a causal estimate. However, this finding suggests that cities with laws cities are more likely to be enter the Bairoch city data we study above. Because the Bairoch data are designed to capture locations above a size threshold of 5,000, this evidence confirms the positive relationship between the institutions of the Reformation and growth across small places and points to potential selection into the set of cities. The potential selection that we uncover implies that our earlier estimates may be conservative. In the main analysis, the control group we study to assess the impact of legal innovation is truncated. It does not include the most stagnant locations, which were disproportionately untreated locations.

Table 8: Town-Level Determinants of Becoming a City with Population Observed in 1800

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

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

B.2 Panel Data on City Populations

Our baseline results document the cross sectional relationship between population observed in 1800 and treatment by legal institutions adopted in the 1500s. The key challenge and drawback of city-level panel

data is that balanced data covering even the period 1400 to 1800 are available for relatively few cities. For many "treated" cities, there is no evidence on outcomes with and without treatment for the simple reason that we have no population observations before the 1600s. This is observed and discussed in Cantoni, Dittmar, and Yuchtman (2015), which studies the relationship between the non-institutional diffusion of Protestantism and city growth. In our data, there are 239 cities of which 103 adopted public goods institutions in the 1500s. The balanced panel of cities with population observed every 100 years 1400 to 1800 comprises 37 cities, of which 24 are "treated" with public goods institutions and 13 are "untreated." Many of the cities that adopted public goods institutions and became dynamic are only observed after 1500. These facts motivate our study of upper tail human capital from the *Deutsche Biographie*, where we can construct a balanced panel covering the period 1300 to 1800 and document that the introduction of the legal institutions of the Reformation was associated with a differential increase in the formation of talent after institutional change occurred. In our analysis of the *Deutsche Biographie*, we include city fixed effects and/or city trends because in those data there is meaningful variation in outcomes pre- and post-treatment. However, it remains natural to wonder whether and if our baseline results on aggregate city population go through in panel data and/or in other periods. Because of the limited variation in the city population data, we estimate regressions that include territory fixed effects, and territory fixed effects interacted with time fixed effects, but not city fixed effects.

Table 9 presents cross sectional regressions and shows the relationship between Reformation law and city population in 1600 and in 1700. Consistent with our evidence in the microdata we observe popuation differences opening up over time: "treated" cities have a modest and not statistically significant size advantage in 1600 and have a larger and more significant advantage in 1700.

Table 10 presents unbalanced panel regressions with which we document that institutions explain city populations by 1800. We find some evidence for effects of institutions in 1700. However, the significance of the 1700s effect depends on the specification.

Table 9: City Size in 1600 and 1700 and Reformation Laws

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

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

Table 10: City Size and Reformation Laws in Panel Data

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

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

C   Plague as Instrumental Variable

This section provides further evidence on plague shocks as an instrumental variable. We provide narrative evidence on the impact of plague outbreaks on local politics. This evidence fleshes out the mechanism through which plague outbreaks increased the likelihood of adopting a Reformation law. We then show that there is no increase over time in the likelihood that cities on trade networks were struck by plagues. This supports the view that plagues in the early 1500s were random conditional on long run plague and other observables. We then examine whether plague shocks interacted with other features of cities in shaping institutional change. Finally, we examine the dynamics in how recent plague shocks impacted institutional change and long-run growth over the course of the period 1522 to 1555.

C.1   Plague and City Politics

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

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

Death, sickness, and flight all threatened the breakdown of social order. The fact that elites who did not fall victim to outbreak usually fled their home cities during plagues had significant consequences. The flight of city council members and administrators had a particularly important impact on functioning of cities and could precipitate a collapse of public order (Dinges, 1995). For instance, 19 of 39 council members of Nürnberg fled during the plague of 1505 (Isenmann, 2012).[50] Perhaps the starkest high-level evidence of the

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

collapse of authority during plague outbreaks was the temporary dissolution of the Reichskammergericht, the highest court of the Holy Roman Empire, in 1540 when Speyer experienced a plague outbreak (Ahl, 2004). In addition to the dissolution of the local order, the supply of other public goods frequently collapsed. To avoid contagion, city markets remained closed (Ibs, 1993). Doctors, priests, and lawyers were among the first to flee, leaving the remaining population without help or means to care for the sick, bury the dead, write testaments, or settle the care for new orphans. With family solidarity and structures disappearing and local order dissolving, a "brutalization" of society frequently followed (Isenmann, 2012). When the plague subsided, surviving populations usually had less trust in the local elites that had abandoned the city. Plague outbreaks therefore left behind a fertile ground for new institutions that promised to mitigate suffering, to uphold social order, and to force local elites to do their civic duties during plague outbreaks. It is not a coincidence that Osiander in his famous plague sermon (1533) scolded the elites for fleeing from Nürnberg.

Following plague outbreaks, cities experienced a large inflow of migrants that, at least in part, replaced the victims of disease. Significantly, a considerable number of migrants were wealthy enough to register as burgher and thus obtain formal political rights (city voting rights). The number of Neubürger (new burgher) often rose dramatically after plague outbreaks, indicating considerable mobility of skilled craftsmen, merchants, and wealthy elites. However, migrants usually did not receive the right to vote immediately. New migrants often had to reside in city for 5 to 10 years to be eligible to vote in the city council and therefore acquired a political voice through voting channels only years after plague outbreaks (Isenmann, 2012).[51] This provides one reason why the effect of plague outbreaks on institutional change through the political process developed with lags.

C.2   Plague and City Connectedness Over Time

In this section, we provide additional evidence to support our baseline strategy using exposure to plagues in the early 1500s as a source of exogenous variation in institutions, conditional on long-run plague propensity.

Our baseline analysis examines how plague shocks in the early 1500s shifted institutions conditional on long-run city-level plague propensity across the 1400s because historical evidence suggests that more connected cities were in general more likely to experience plague. Our identifying assumption is that plague outbreaks in a narrow window in the 1500s were exogenous conditional on long-run plague prevalence. Our primary results control for both long-run, time invariant plague prevalence across the 1400s and independently for plague in each quarter century across the 1400s (Table 4). Figure 5 (in the main text) further shows that the aggregate plague time series is stable over the period from 1400 to 1550. However, it is natural to wonder whether the endogeneity of the plague shifted over time in other ways that could threaten identification. In particular, it is important to know whether the distribution of plague outbreaks shifted over time towards more "open" and connected locations. Were cities on trade networks increasingly experiencing plague shocks, the shocks we study in the early 1500s could reflect the long run evolution in plague trends or other non-linear changes in how plague transmission correlated with the operation of trade

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

networks themselves, rather than conditionally exogenous shocks.

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

We examine three measures of city connectedness. We observe which cities were located on rivers, members of the Hanseatic League, and located in close proximity to neighboring cities. To test for time-varying plague dynamics, we examine the relationship between plague outbreaks over 25-year periods and time varying functions of these measures of connectedness. To study whether there was any differential trend in outbreaks across more or less connected cities we estimate the following regression:

$$Plagues_{it} = \alpha_i + \delta_t + \beta(Connected_i \times Trend_t) + u_{it} \tag{7}$$

The dependent variable *Plagues* is the number of years with plagues observed in the city-level in 25-year periods. We estimate this equation using three measures of *Connected*: indicators for rivers (*River*), indicators for Hanseatic cities (*Hansa*), and the count of cities within 100 km (*Proximity*). We estimate over the period 1400-1575 and include include city and time fixed effects (α_i and δ_t) in all specifications. Because the imposition of linear time trends may be restrictive, we also estimate a model that allows the relationship between connectedness and plague to vary flexibly over time:

$$Plagues_{it} = \alpha_i + \delta_t + \sum_{s=1425}^{1550} \beta_s(Connected_i \times Time_s) + u_{it} \tag{8}$$

Table 11 presents regression estimates that document that the relationship between plague and city connectedness was stable between 1400 and 1550. Columns 1 to 4 show there is no evidence of differential plague trends for cities connected to trade networks. Columns 5 to 8 show that there is similarly no evidence of plague increasing in a non-linear fashion in cities connected to trade networks over the long period 1400 to 1574. The outcome unit of analysis is the number of plague shocks at the city level in 25-year periods. All regressions controll for city and time fixed effects, as well as the the complete set of controls in our baseline regression specifications in the main body of the text. Standard errors are clustered at the city level. The omitted time category and interaction is 1400-1424.

C.3 Interactions between Plague and City Characteristics

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

We study several features of city life that could potentially shape the way the experience of plague shocks was transmitted into local politics and preferences: cities' constitutional status as free or feudal cities (*Freie*

Table 11: Relationship Between Plague Shocks and City Connectedness 1400 to 1574

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

Note: For complete details on specification and estimates see text.

und Reichstadt or *Landstadt*), the local history of printing, the local flow of recent university graduates, and the presence of market rights.

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

$$Law_i = c + \alpha\ Plagues_i + \beta(Plagues_i \times Characteristic_i) + \gamma X_i + \epsilon_i, \tag{9}$$

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

Table 12 presents the results and shows that the main effect on "Plagues" is significant and stable across specifications. We also find strong evidence that the plague effect was muted in free cities. In free cities, the net effect of plagues (the sum of the main effect and the interaction) is not different from zero. This suggests that the effect of plagues was most concentrated in cities subject to feudal lords, where the barriers to mobilization and political change were otherwise highest. This is consistent with the finding in Dittmar and Seabold (2015) that variations in media market competition mattered most in cities subject to lords.

C.4   Plagues, Institutional Change, and Growth Over Time

To consider both the local average treatment effect the IV recovers and the dynamics of the Reformation, we document how the relationship between plague shocks, institutional change, and growth evolved once the Reformation began. To do this we estimate instrumental variable regressions year-by-year starting in 1522. In each year, we estimate a first stage regression documenting how the number of plague outbreaks over the past twenty-two years explains institutional change across the set of cities that "survived" to that date as candidates to adopt a Reformation law. In each year, we use changes in institutions induced by recent plagues to study the relationship between institutions and growth across the set of cities surviving to that date. In this analysis, the set of cities surviving as candidates for Reformation changes (cities that get laws drop out). In addition, the value of the instrument changes as we update recent plagues.

These changes in the instrument provide one kind of external validity check on our baseline IV estimates. They allow us to compare how shocks shaped institutional change and how institutions shaped outcomes for cities that had the instrument "turned on" at different times. To fix ideas, consider the example of Hannover, a city which had no major plagues from 1500 to 1522. For Hannover the instrument is thus "turned off" in our baseline analysis (Table 4). Hannover survived without a law into the 1530s, but experienced renewed Protestant agitation in the 1530s and a plague outbreak in 1535 when the pro-Catholic city council was in disarray, and passed a Reformation law in 1536.[52] When we now use the IV set-up to study the plague and

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[52]Examples can be multiplied. Lüneberg was struck by plague in 1516, then again in 1525 and 1528, and finally

Table 12: Plague Shocks and City Characteristics in Institutional Change

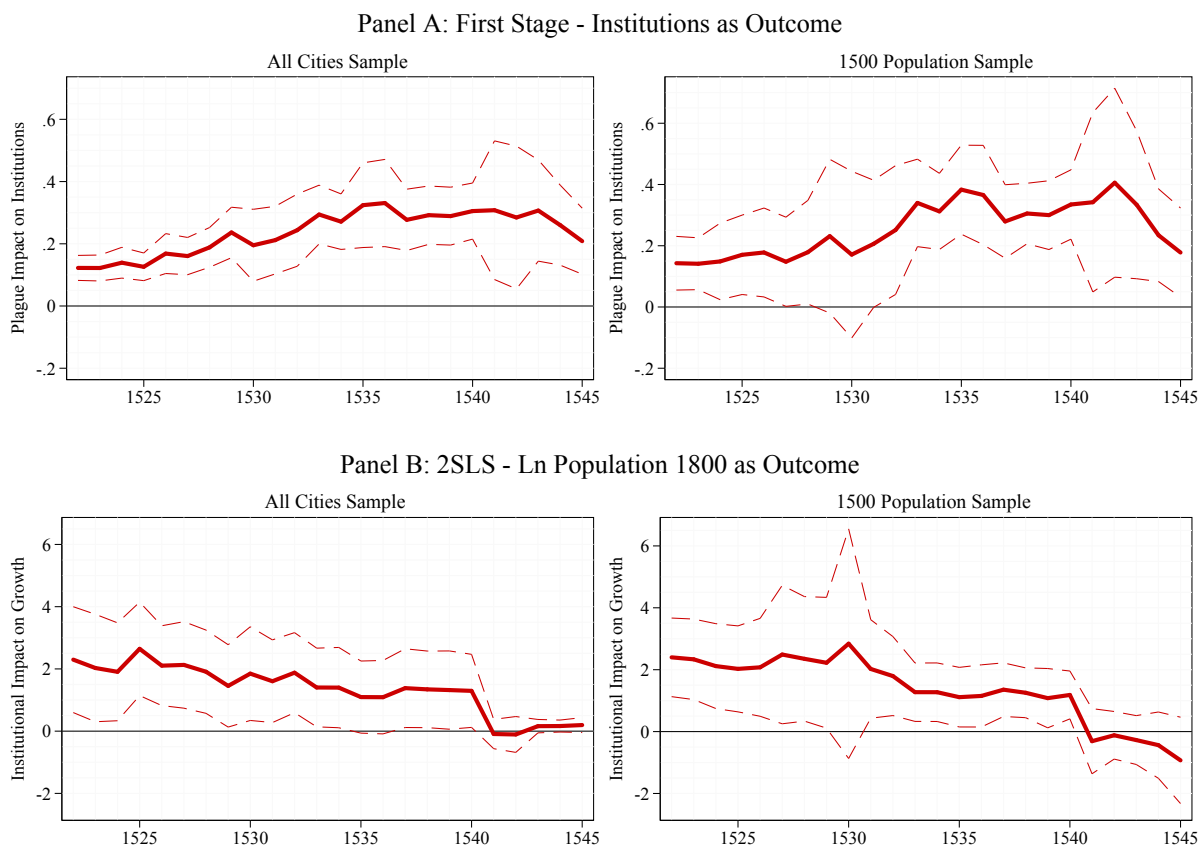|	Dependent Variable: Reformation Law					
	[1]	[2]	[3]	[4]	[5]	[6]
Plagues	0.13***	0.18***	0.15***	0.16***	0.11***	0.18***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Plagues × Free City		-0.15**				-0.19**
		(0.05)				(0.08)
Plagues × Any Printing			-0.09*			0.29
			(0.05)			(0.18)
Plagues × Students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

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

induced institutional change during the Reformation, we keep Hannover in the sample each year through 1536, when it gets a law and is no longer a candidate for institutional change.

Figure 11 plots year-by-year (i) the first-stage estimates of the relationship between institutional change and plagues and (ii) the 2SLS estimates of the population growth impact of induced variation in institutions. Figure 11 shows that the first stage relationship between plague and institutional change strengthened over the initial years of the Reformation and then declined. In the early 1520s, one additional recent plague raised the probability of institutional change by about 15 percent. By the mid-1530s, the estimate is over 25 percent. Figure 11 also shows that the relationship between institutions and growth is more stable, but slowly declines over time and then collapses to zero in the early 1540s, just before the Schmalkaldic war, which initiated a new era in which relatively few cities adopted institutional change, as discussed above.

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passed a law in 1531. Hildesheim experienced a plague in 1516, was again struck in 1538, and passed a law in 1541. Tübingen experienced no plagues in the 1500s until being hit in 1530, 1540, and 1541 – and passed a law in 1559.

Figure 11: Instrumental Variable Regression Estimates Year by Year

Panel A: First Stage - Institutions as Outcome

Panel B: 2SLS - Ln Population 1800 as Outcome

This graph presents estimates from instrumental variable regressions. Panel A presents first stage regression estimates. The outcome is a binary variable for institutional change (Reformation law). First stage regressions estimate how the probability of institutional change varied with the count of recent plague shocks, defined as outbreaks in the previous twenty years. Panel B presents 2SLS regressions that document how long-run city populations (log population in 1800) responded to induced variation in institutions (Reformation law). In each year, we estimate regressions examining the set of "surviving" cities that had not yet adopted a Reformation law as of that year. In each year, the regressions are estimated using recent city-level plague shocks as of that year. This graph plots how the annual estimates evolve. The left panel presents estimates examining all cities in the data from regressions that include territory fixed effects and categorical controls for 1500 population ("All Cities Sample"). The right panel presents estimates examining cities with population in 1500 observed from regressions that control for log population in 1500 and do not include territory fixed effects ("1500 Population Sample"). All regressions include the same control variables as in Table 4, including the mean level of plague in the 1400s and period-by-period plague across the 1400s. Standard errors are clustered at the territory level. The red dashed line represents the 90 percent confidence interval for the "All Cities" specifications and the 95 percent confidence interval for the "1500 Population" specifications.

D Upper Tail Human Capital

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

We motivate the analysis in the main body of the paper with a figure showing that cities that got Reformation laws subsequently began differentially attracting upper tail human capital migrants. A first question this analysis raises is whether "untreated" Protestant and "untreated" Catholic cities developed similarly. Figure 12 confirms that the key variation is explained by the distinction between cities with and without laws by disaggregating the data to show the number of migrants observed in Protestant cities with laws, Protestant cities without law, and Catholic cities.

Figure 12: The Migration of Upper Tail Human Capital by Institutions and Religion
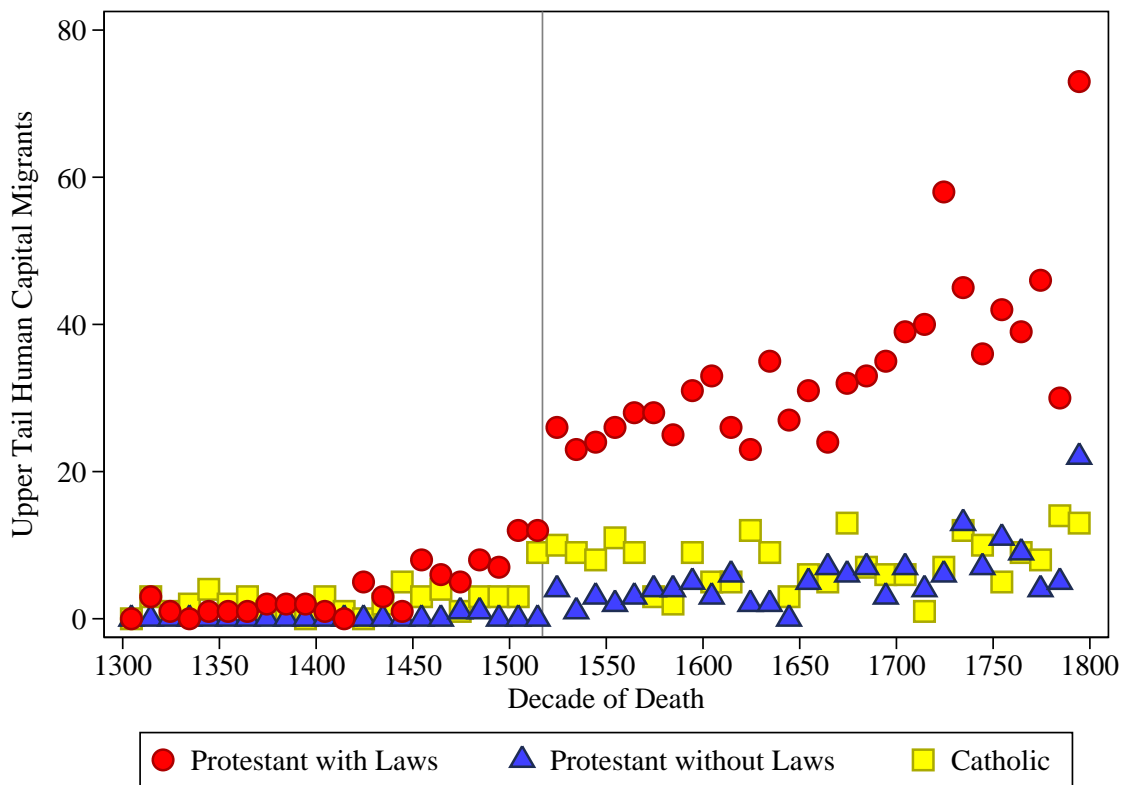
This graph plots the number of migrants observed in the *Deutsche Biographie* at the decade level for cities grouped as (1) Protestant cities with Reformation laws, (2) Protestant cities without Reformation laws, and (3) Catholic cities also without laws. Migrants are identified as people living and dying in town i but born in some other location j. The vertical line is at 1518, the year Luther's theses began circulating widely.

In the main body of the paper, we use regression analysis to quantify differences in upper tail human capital across cities. To interpret our findings, it is is important to understand the research design. In the regression analysis, we study the formation and migration of upper tail human capital over 50 year periods. To study the formation of human capital we assign individuals to the fifty-year period in which they were 40 years old.[53] Fifty-year periods are by design broad "bins" that enable us to consider upper tail human capital formation before and after the Reformation. Our baseline analysis associates people with their town of birth as a summary measure of how human capital was associated with city locations.[54]

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

Given our argument highlighting plague of the early 1500s as an institutional shifter, it is natural to wonder whether exposure to these plagues shifted human capital production. We test the hypothesis that the formation of human capital responded to shocks by re-estimating regressions examining period-by-period the differntial formation of upper tail human capital in cities that were "treated" in the 1500s. Specifically, we re-estimate equation (5) with exposure to plague 1500-1522 as the treatment variable, in the spirit of the reduced form in an instrumental variable design. Figure 13 presents a comparison of regression estimates where "ever treated" cities are cities exposed to plagues in the early 1500s and presents (i) estimates of time period fixed effects common across all cities and (ii) estimates of the total period-specific variation for cities with Reformation laws. Figure 13 shows that exposure to plagues in the 1500s is associated with differential increases in the formation of upper tail human capital starting in the Reformation era. In contrast to our baseline analysis, we see the post-Reformation differential for "treated" cities is relatively stable – there is no sharp increase in the differential in the late 1700s – and the treated cities also had a modest differential before the Reformation.

We also use difference-in-differences methods to test whether upper tail human capital formation differentially increased in cities that institutionalized the Reformation after the passage of laws. We estimate

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

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

the following regression:

$$People_{i,t} = \theta_i + \delta_t + \beta_1 \cdot Trend_t + \beta_2 \cdot Post_t + \beta_3 \cdot Trend_t \times Post_t +$$
$$\beta_4 \cdot Trend_t \times Law_{i,pre-1600} +$$
$$\beta_5 \cdot Post_t \times Law_{i,pre-1600} +$$
$$\beta_6 \cdot Trend_t \times Post_t \times Law_{i,pre-1600} + \epsilon_i$$

(10)

where $Law_{i,pre-1600}$ is equal to 1 in all periods if a city adopted a Reformation law by 1600. The variable $Post_t$ is an indicator for the treatment period, which begins in 1520 in our baseline set up.

Figure 13: Relationship between Upper Tail Human Capital and Early 1500s Plague
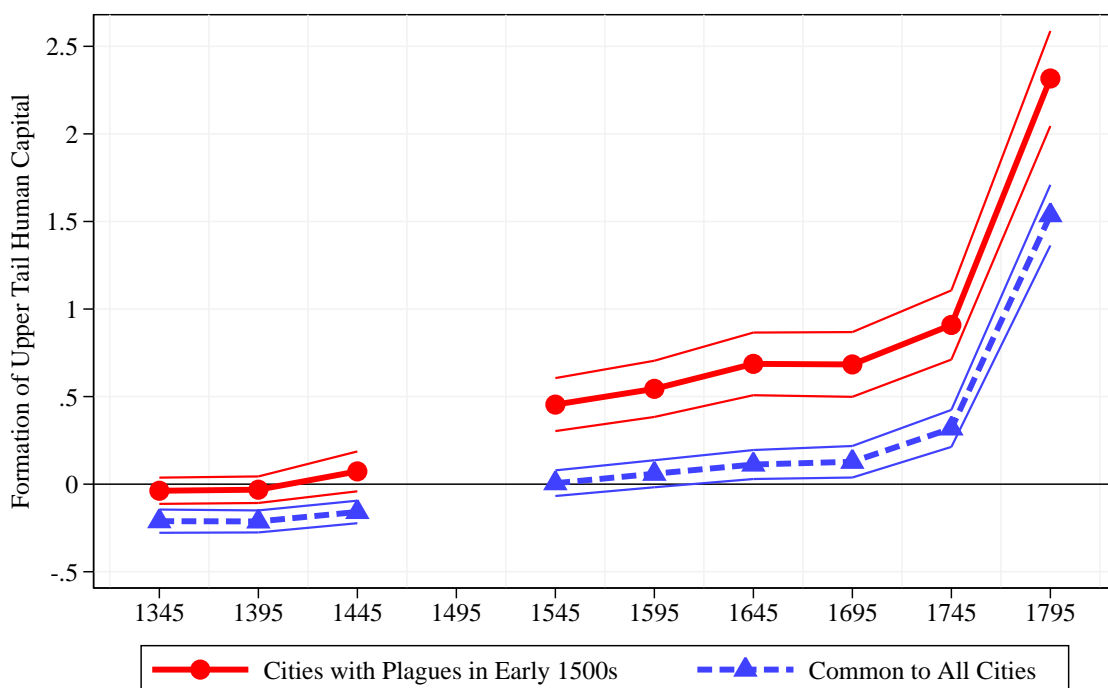
This graph plots parameter estimates from regression analysis examining the differential local formation of upper tail human capital in cities that adopted were struck by plague between 1500 and 1522. The outcome variable is the logarithm of the number of upper tail human capital individuals plus one who were born in city i in period t. Dates and places of birth are identified in the *Deutsche Biographie*. We assign people to the city and time-period in which they were born. We graph the parameter estimates on time-period fixed effects and the interactions between time-period indicators and an indicator for cities struck by plague between 1500 and 1522. The regression is estimated over data from 1320 through 1820. The omitted time category is the period 1470 through 1519 (centered on 1495). The post-Reformation periods begin with the 1520-1570 period (centered on 1545). The variation common to all cities is δ_t in equation (5) in the text. The incremental variation specific to cities with laws is β_s.

Table 13 shows the results of estimating equation (10). In columns 1-3 we examine the logarithm of

Table 13: Formation of Upper Tail Human Capital

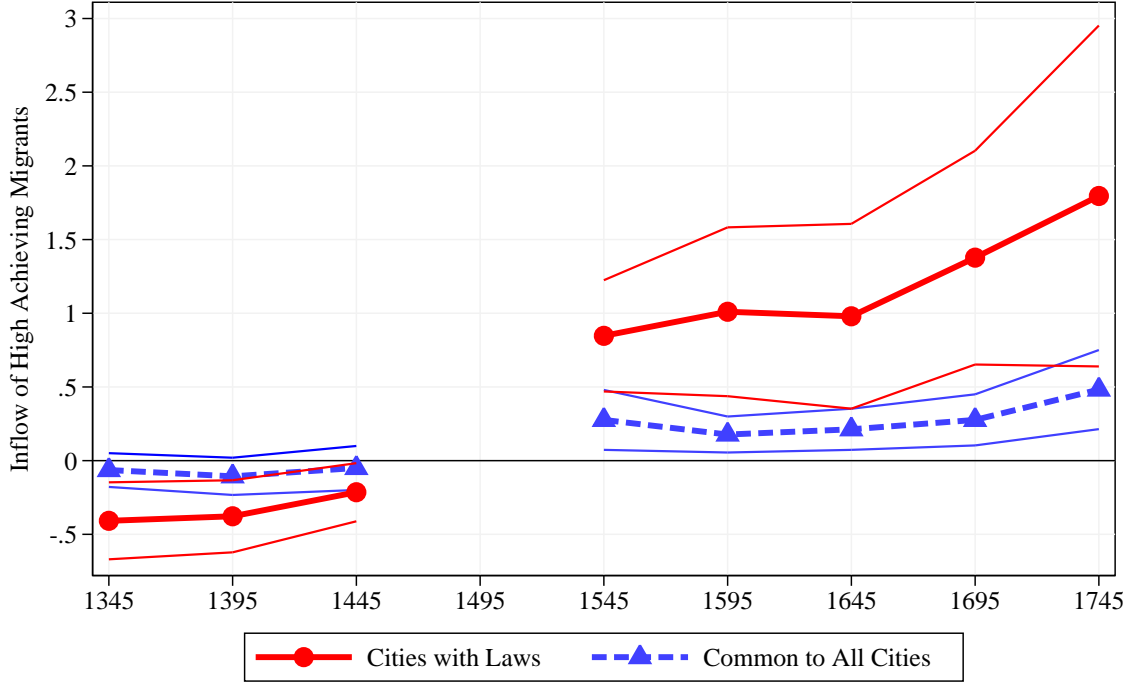|	Ln Upper Tail Human Capital			Count Upper Tail Human Capital		
	[1]	[2]	[3]	[4]	[5]	[6]
Trend	1.59***	2.83***		21.04***	36.86***	
	(0.08)	(0.14)		(3.85)	(6.47)	
Post × Law	0.48***	-0.24	-0.30	4.96***	-21.93**	-22.58**
	(0.08)	(0.24)	(0.23)	(1.81)	(11.01)	(10.82)
Post × Trend		-1.28***	0.13***		-17.00***	1.38***
		(0.06)	(0.01)		(3.03)	(0.23)
Trend × Law		0.03***			0.09*	
		(0.01)			(0.05)	
Law × Post × Trend		0.06*	0.07**		2.77**	2.92**
		(0.03)	(0.03)		(1.33)	(1.28)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	No	Yes	Yes	No
City-specific Trend	No	No	Yes	No	No	Yes
Observations	2,868	2,868	2,868	2,868	2,868	2,868
Number of cities	239	239	239	239	239	239
R^2	0.65	0.66	0.76	0.22	0.23	0.37

This table presents the regression results of estimating the effect of reformation laws on the formation upper tail human capital. Upper tail human capital is measured by locally born people observed in the *Deutsche Biographie* (Bayerischen Akademie der Wissenschaften, 2015). People are assigned to the time period in with they reached the age of 40. "Trend" is the time trend. Time periods are 50 years bins from 1220-1820. "Post" is an indicator for the period starting 1520. "Law" is a time invariant indicator for whether a city had a Reformation ordinance by 1600. The outcome in columns 1-3 is the logarithm of the number of people plus one. The outcome in columns 4-6 is the number of people. ***, **, * denotes 1%, 5%, and 10% statistical significance. Standard errors are clustered at the city level.

the upper tail human capital formation. We find that cities that adopted a public goods institutions of the Reformation produced on average 48 percent more upper tail human capital (column 1). Next, instead of estimating the level, we estimate the growth rate. Column 2 shows that cities with Reformation laws exhibit a 3 percentage points higher growth rate in the production of upper tail human capital across all periods and an additional 6 percentage point growth advantage after the Reformation. Because there is some evidence for a pre-trend specific to cities with laws, we control for city-specific trends and confirm that cities with Reformation laws exhibit 7 percentage points higher growth rate in the production of upper tail human capital after 1520, conditional on city specific trends and post-Reformation trends common to all cities (column 3). In columns 4-6, we examine the raw count of upper tail human capital formation. We find that cities with public goods institutions produced approximately five additional people with upper tail human capital post-1520 in the baseline specification which does not control for differential trends by city type (column 4). When we control for trends shared by cities which adopted these institutions or for city-specific trends, we find that cities with public goods institutions produced approximately three additional upper tail human capital natives in each period after 1520.

Consistent with Table 13, our baseline results in the main text study the logarithm of upper tail human capital as an outcome, but are supported by analysis of the relationship between public goods institutions and the number (count) of upper tail human capital migrants observed in the data. Figure 14 presents parameter estimates of the relationship between public goods institutions and the count of migrants using the specification from equation (4). Figure 14 plots the parameter estimates through 1745 for ease of presentation: The migration advantage for cities with laws becomes very large post-1750 – and when plotted this dominates and obscures the early period advantages highlighted by this graph and our broader analysis.

Additional corroborating evidence on the pre-Reformation similarity of cities that did and did not adopt public goods institutions in the 1500s can be obtained from records on individual university degree recipients in the run-up to the Reformation. Microdata on university students *by home town location* is available from the late 1300s and through the early to mid-1500s from Cantoni, Dittmar, and Yuchtman (2015). The data allow us to match students recorded in university registries as receiving degrees (Bachelor, License, Master, and Doctorate degrees) to the students' home towns. Because these data stop in the mid-1500s, we use the home town locations of university degree recipients as evidence on the pre-trends in upper tail human capital across cities before the Reformation and not as an outcome variable. Figure 15 plots an index of the number of students receiving degrees (the average number of degrees between 1400 and 1450 is indexed to 1). We use the index to compare the flow of degrees to students from cities that got Protestant laws, cities that adopted Protestantism in religion but did not get Protestant laws, and always Catholic cities. Figure 15 shows that cities that did and did not get Reformation laws exhibited similar pre-Reformation trends in university degrees earned by hometown students.

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Figure 14: The Migration of Upper Tail Human Capital Using Count Data

This graph plots parameter estimates from regression analysis examining the differential migration of upper tail human capital into cities that adopted Reformation law. The outcome variable is the count of the number of migrants in city i at time t. Migrants are upper tail human capital individuals identified in the *Deutsche Biographie* as having moved to the city in which they died. We assign migrants to the time-period and city in which they died. Time is measured in 50-year periods. We graph the parameter estimates on time-period fixed effects and on the interactions between time-period indicators and an indicator for cities ever adopting Reformation law. The regression includes fixed effects for cities and time periods and is estimated over data from 1320 through 1820. The omitted time category is the period 1470 through 1519 (centered on 1495). The post-Reformation periods begin with the 1520-1570 period (centered on 1545).

Figure 15: Pre-Trends in University Degrees
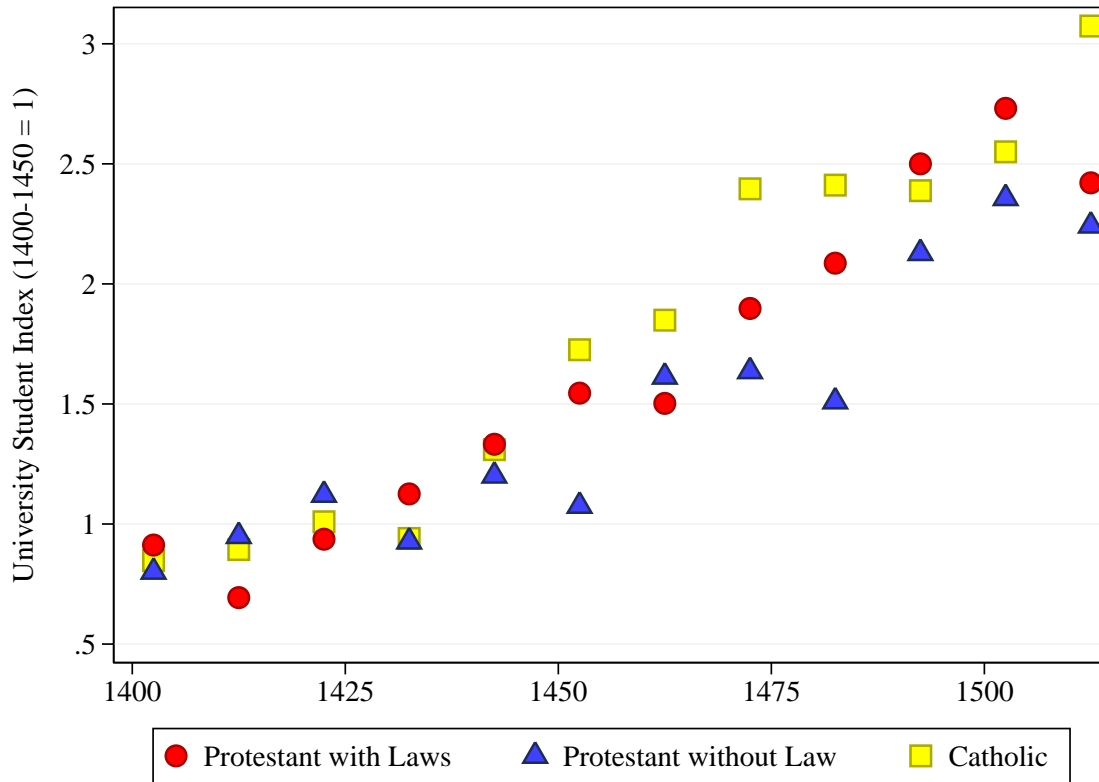
This graph presents data on students from different types of cities who received university degrees each 10-year period before the Reformation. The number of students is indexed such that within-group average 1400-1450 = 1. Source: Microdata on degrees received from German universities from 1400 to 1517 by student home town are from Cantoni, Dittmar, and Yuchtman (2015).

E   Historical Evidence on Politics, Preferences, and Diffusion

This appendix presents additional historical evidence on the diffusion of the Protestant Reformation as a social movement and in law.

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

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

The diffusion of the Reformation at the city level involved civil disobedience and a broad range of actions forbidden by or at odds with the policy of city councils. In Frankfurt am Main, the guilds revolted against the city council after the council ordered an end Protestant church services in 1524. A Bürgerausschuss (a burgher council) published 46 articles, which city council was forced to adopt when facing mobilized and armed citizens. With the defeat of the Protestant peasants in the Peasant's War (1525), the burgher council was dissolved and the 46 articles nullified. However, public pressure continued to build. By 1533 the city officially converted, and the legal articles were implemented.[55] Speyer, Worms, and Rothenburg also experienced unrest during the 1520s. While these unpheavals had no immediate institutional results, historical evidence suggests they laid groundwork for the later conversion to Protestantism. Similarly, in Augsburg, the city council was forced to drop its policy of religious neutrality following riots in 1524, 1530, and 1534 (Broadhead, 1979). In Northern cities, such as Rostock, Stralsund, Greifswald, Lübeck, Braunschweig, Lüneberg, Göttingen and Hanover institutional change was led by citizens excluded from political power (Cameron, 1991). In Zwickau, Lutheran publications were printed in 1523; the city council unsuccessfully attempted to suppress protests, street theatre, and civil disobedience in 1524; the Reformation was formally adopted in 1525 (Scribner, 1979).

The political power struggle berween citizens and city councils was often a lengthy one (Ozment, 1975). The adoption of the Reformation in Hildesheim exemplifies the struggle between city council and other disenfranchised interest groups. From the beginning, the city council of Hildesheim actively opposed the

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[55]Sehling (1911) Vol. IX, p. 473f.

Reformation. In 1524, the council banned Protestant books. In 1525, the council and guilds urged the Catholic church to fight heretics (i.e. reformers). Nevertheless, clerics integrated Lutheran songs into their masses. These songs were subsequently outlawed in 1528, but in 1530 a Protestant preacher was welcomed with Lutheran songs. The city council responded with passing new law against Lutheran songs the next day, prompting 150 Protestants went to the vesper in an act of civil disobedience. The city council then outlawed Lutheran or "Wittenberg" teachings and instituted fines for Protestants. While the local Protestants received some forms of support from the *Städtetag* (assembly of the free cities), they remained unable to establish Protestantism in Hildesheim in the 1520s. In 1532, the Hildesheim guild masters first sent for a Protestant preacher and in September of that year 150 burghers stormed the city hall to demand Protestant preaching. In response, the city council asked Catholic duke of Wolfenbüttel for protection. However, in 1534 the city also signed an alliance with Protestant cities ensuring mutual protection. After the death of the staunch anti-Reformation mayor Wildefüers (elected 1526), the city council relented and openly adopted Protestantism in 1542.[56]

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

While the local rulers were typically opposed to the early Reformation, the strength of their opposition could vary. Local histories provide insight into how citizens navigated this opposition. In Göttingen, Protestant preaching was initiated in 1518, but the city council was concerned about the reaction of the local Catholic duke and did not adopt the Reformation. The first openly Protestant service was held in Göttingen in 1529. In that year, the burghers revolted against the city council and forced the provision of Protestant services. When the local duke subsequently demanded that the Protestant preachers leave Göttingen, the council obeyed. Shortly thereafter, the city council, fearing a further burgher revolt, sent the duke a formal statement declaring for Protestantism.[57] The Reformation movement in Hannover was similarly resisted by the local duke (Duke Erich d. Ältere) and the city council. In agreement with the duke, the city council passed an ordinance against Luther's teachings. After civil unrest in 1532, Lutheran teaching became dominant in Hannover, but the city council would not allow Protestant services. In 1533, renewed unrest forced the city council to flee. The duke was unable to broker a compromise. On July 31, 1534, the

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[56]Sehling (1911) Vol. VII/2/1, p. 797-806.
[57]Sehling (1911) Vol. VI/2, p. 902.

newly elected (Protestant) city council entered into a transaction in which the city provided a payment to compensate the duke for accepting Protestant preaching in the city.[58] Similarly, the city council of Northeim exploited the financial difficulties of its Catholic duke, who accepted the Reformation in Northeim after also receiving a payment.[59] In contrast, the Dukes of Bavaria emerged as particularly ardent defenders of Catholic orthodoxy in the 1520s, but over the 1500s failed to eliminate the diffusion of Protestantism in their territory.[60]

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[58]Sehling (1911) Vol. VI/2, p. 941.
[59]Sehling (1911) Vol. VI/2, p. 919.
[60]For religious policy in Bavaria see Strauss (1959).